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dated 1913, was simply a forgery by his son Ludwig Mach.

We may expect soon to hear much more about this whole field from others. The publication of several new works on imagined experiments by philosophers of science has been announced. To a degree unexpected even some half a century ago, when the Einstein-Podolsky-Rosen (EPR) "experiment" was first announced, much of modern science has become a matter of experimenting in the laboratories of the mind. Some branches are so abstracted from the level of bench-top experiments that the search for consensus has become an intense research site for sociologists of science. Among historians of science, too, there has been a growing fascination with the concept and its various historic expressions. Brown's volume at least serves as a reminder that there is now a real need for a book that would deal with the subject in an historically informed way.

GERALD HOLTON

David Locke. *Science as Writing*. xii + 237 pp., index. New Haven, Conn./London: Yale University Press, 1992. \$27.20.

Excessive modesty is not usually a problem with books on the rhetoric of science. But a historian of science could read the opening of David Locke's book and put it down, thinking there would be little new in it. That would be a mistake, because the book contains a number of valuable readings of texts, from a well-defined position in literary criticism, presented in careful, readable, and personal prose.

The problem is that Locke begins with a very general overview of what he calls the countertraditional strand in science studies. A leaf through suggests that he deals mainly with such authors as Galileo, Darwin, Einstein, and Watson and Crick, who (as he notes) have already been discussed exhaustively elsewhere. But to be fair, Locke says (p. ix) that the book is not aimed at science studies specialists, but at the unconvinced, both the scientists who do not see the central role of writing in their work and the literary scholars who exclude scientific texts from their field.

It is probably more successfully directed at the literary readers. Locke takes for granted a range of critical allusion that might put off some readers. More important, the strength

of the book is in the way it is organized around the commonplaces of literary theory—not the latest innovations, but the approaches that might be taken for granted in literature courses, the baggage of graduate students, however traditional or trendy. Thus he starts with mimesis and representation; though he notes that this topic is not currently popular in literary criticism, it has a lineage running from Aristotle to Erich Auerbach, and it won't go away.

The organization (which I could not guess from the table of contents) loosely follows Roman Jakobson's functions of language (p. 22), with chapters devoted to scientific writing as representation, expression, evocation, art object, artifact in social context, and signifying system. This heuristic leads in almost every chapter to some surprising turns on familiar material. The one chapter for which it flops is that on social context ("The Putative Purity of Science"). Perhaps this is because, as Locke notes, a contextual theory cannot be exemplified by a striking reading of one text.

Locke's justification for sticking to better-known works is that their revolutionary science called for innovations in rhetoric. But I learned more from his occasional uses of ordinary texts: for instance, his brief analysis of the methods from a typical article on chemistry, an article Locke himself coauthored, or his reminiscence of the chemist R. B. Woodward, as seen from afar, delivering a conference paper. In general, Locke's more interesting points are made about eminent but less discussed scientists, about Hermann Muller rather than Darwin, or Jacques Loeb rather than Einstein.

Locke is cautious about epistemological controversies. The one issue on which he does become polemical is the straitjacket of some contemporary scientific journal styles. He makes a good case for loosening editorial strictures, by showing the kinds of pleasure that can be gotten from reading scientific prose.

GREG MYERS

Alexander Rosenberg. *Economics: Mathematical Politics or Science of Diminishing Returns?* (Science and Its Conceptual Foundations.) xviii + 266 pp., bibl., index. Chi-

icago/London: University of Chicago Press, 1992. \$32.50.

Alexander Rosenberg does not mind being described as old fashioned in epistemology and the philosophy of science. By his own account he has for twenty years been worrying over the “cognitive status” of economics, that is, over the question whether economics is a Science. Science, big “S,” is to be defined as in logical empiricism circa 1940. If economics does not fit such a Procrustean bed, then we economists cannot properly use it for policy.

Rosenberg concludes that economics as it has developed since Adam Smith does not have the same “cognitive status” as physics. Economics is not the Science of diminishing returns; on the contrary, it is mathematical politics. Its present version belongs in the department of mathematics, leaving slots in proper economics for economists who fit better the categories of philosophy of science circa 1940.

Rosenberg argues that philosophy can dictate to economics. He points out that scientists themselves impose philosophical constraints on their arguments. Many economists, for example, would agree with Rosenberg that the standard is prediction. (But the economists agree, I report, because they have heard tell that philosophy of Science demands it.)

The book is sharply controversial, quarreling for many pages with Marxists like Martin Hollis and Edward Nell and with neo-classicals like E. Roy Weintraub, Gary Becker, Armen “Alchain,” and me. Its main point—that many economists spend too much time at the blackboard—is argued lucidly and well.

One difficulty, however, is that like many philosophers of science—but few historians of science—Rosenberg identifies the science with its core theory. The progress in applications of economics below the level of grand but empty “theory” does not attract his attention. The “theory” he has in mind was adumbrated by Adam Smith in one or two passages. Rosenberg sees no progress in “predictions of the theory” between Smith and the latest issue of the *American Economic Review*. The theory is merely one argument from selfishness after another. Similarly, Darwinism could be viewed as unprogressive because it still depends on evolution by natural selection.

A second difficulty, then, is Rosenberg’s

undefended affection for prediction as the one demarcation between Science and nonsense. He evinces the worshipful attitude usual among philosophers toward the more prediction-oriented branches of physics. Yet sciences like geology and biology depend on prediction hardly at all. All right, retorts Rosenberg, so much for their Scientific status.

A third difficulty is that Rosenberg has not read history of science, though presuming to tell scientists how it has gone. This occupational hazard among philosophers of science is of course familiar.

Economics needs a history, which writers like Alon Kadish, John Maloney, Margaret Schabas, and Yuval Yoney are beginning to supply, beyond the whiggish tales by economists such as Mark Blaug. It needs a rhetoric and a sociology, which can inform the history, too. Does it need a philosophy? Perhaps not, at least not a philosophy depending on epistemology circa 1940.

DONALD N. McCLOSKEY

Susan E. Cozzens; Thomas F. Gieryn (Editors). *Theories of Science in Society*. (Science, Technology, and Society.) 264 pp., illus., figs., bibls., index. Bloomington/Indianapolis: Indiana University Press, 1990. \$25.

This volume is useful for historians of science in two significant ways: it provides discussions of a variety of sociological approaches to studying science, and it provides a glimpse at the variety of theoretical approaches currently used in sociology.

According to the editors, this book was intended to “bring the news” of sociology of science to sociology. However, the articles instead bring a range of theoretical frameworks currently engaging American sociologists to the study of science. Each article builds continuities between itself and frameworks used in sociological studies of other areas of social life.

In her fine lead article, Adele Clarke brings the symbolic interactionist framework of “social worlds” developed by Anselm Strauss, Howard S. Becker, Tamotsu Shibutani, and Everett Hughes to the study of the early twentieth-century emergence of reproductive science. She constructs a compelling history of the different social worlds involved in negotiating the emergence of that new scientific field. Rob Hagendijk argues the advantages