Bohr's own idiom), for its aversion to formalism, and for a few more specific errors (like taking wave-particle duality to exemplify complementarity). But the book makes some important contributions, not least of which is reminding us that the case is not closed on Bohr, that there are new avenues of approach to his ideas that might yet lead to fuller understanding.

Folse's main thesis is persuasively demonstrated. He shows that Bohr never doubted the reality of the atoms, electrons, and other microparticles that physics investigates. If the book accomplishes nothing else, it should at least finally dispel the myth that Bohr was a simpleminded instrumentalist, positivist, idealist, or subjectivist.

Less convincing is Folse's characterization of the specific content of Bohr's realism and of its differences from "classical" realism. Folse's classical realist holds that the classical "phenomenal" properties a system displays under observation, such as sharply defined position and momentum, correspond to "real" physical properties, all simultaneously possessed by a system. Bohr is interpreted, by contrast, as holding that a reality underlies observed phenomena but that a system's phenomenal properties need not correspond to its real ones. The distinction between real and phenomenal properties makes it possible to reconcile complementarity with realism. The classical realist objects to the complementarity interpretation's assertion that incompatible observables cannot be simultaneously well defined because, assuming a correspondence between phenomenal properties (observables) and real properties, he sees this either as an unwarranted restriction on reality or as the admission that quantum mechanics is incomplete. Folse's Bohr, however, is untroubled, because by his standards restrictions on phenomenal properties entail no corresponding restrictions on reality, and theories are obligated to describe completely only phenomenal properties.

Let me raise just two questions concerning Folse's interpretation of Bohr. First, is it correct to ascribe to Bohr the distinction between real property and phenomenal property? Bohr gave special significance to the concept of a "phenomenon," arguing that physics refers only to "phenomena," whose descriptions must include the associated experimental contexts. It is not clear, however, that Bohr's talk of phenomena is equivalent to the philosopher's talk of "phenomenal properties," despite the verbal similarity, since Bohr took phenomena themselves to be real. Second, is there an enduring set of real properties attaching to every physical system, sufficient to ground its separate identity, even during interactions? Folse's emphasis on "the" reality behind the different phenomenal aspects presented by the "same" system in different contexts suggests that he would answer yes. But Bohr's remarks require a contrary interpretation. Consider this famous passage from the 1927 "Como" paper: "The quantum postulate implies that any observation of atomic phenomena will involve an interaction with the agency of observation not to be neglected. Accordingly, an independent reality in the ordinary physical sense can neither be ascribed to the phenomena nor to the agencies of observation." Folse would emphasize the words "in the ordinary physical sense." But Bohr intended equal emphasis on the word "independent"—object and instrument do not possess *independent* realities. Folse's slighting of this important theme results partly, no doubt, from his paying insufficient attention to the debate between Bohr and Einstein, a topic that must be prominent in any complete account of Bohr's views.

DON HOWARD

Ian Hacking. *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science.* xv + 275 pp., bibl., index. Cambridge: Cambridge University Press, 1983. $39.50 (cloth); $11.95 (paper).

During the Astronomical Revolution realism was a problem: did planets actually course through the heavens, or was it merely convenient to represent them that way in aid of calculation? Later, in the nineteenth-century debate over atomism, the issue resurfaced to challenge scientists and philosophers. In its current revival the realism debate hinges on a philosophical problem: how can competing, seemingly incommensurable theories relate to the world? In his lucidly written account of the modern philosophical realism-antirealism debate, Ian Hacking brings the reader to feel the force of both sides of this central and ever-recurring philosophical issue.

"For my part," Hacking recounts, "I never thought twice about scientific realism until a friend told me about an ongoing experiment to detect the existence of fractional electric charges [quarks]." To alter the charge on a test ball of niobium, Hacking's colleague continued, " 'we spray it with positrons to increase the charge or with electrons to decrease the charge.' From that day forth I've been a scientific realist. *So far as I'm concerned, if you can spray them then they are real*" (pp. 22–23).

Hacking distinguishes different kinds of realism. One can be a realist about theories: "The question about theories is whether they are true, or are true-or-false, or are candidates for truth, or aim at the truth." Or one can be a realist about entities: "The question about entities is whether they exist" (p. 27). Throughout the first part of Hacking's book ("Representing"), he follows the first set of queries, reviewing larger issues linked to theoretical realism, for example, positivism and pragmatism. Hacking traces some of the debate by taking T. S. Kuhn's incommensurability thesis and analyzing it into three better-individuated kinds of incommensurability.

Topic incommensurability exists when the subject matter of one theory is largely disjoint from topics addressed by its successor theory. This makes comparison problematic. By *dissociation incommensurability* Hacking has in mind changes in the groupings of phenomena. Thus Renaissance naturalists associated stars, plants, and states of the human body when they posed questions and admitted solutions. Later natural philosophers bundled things differently, making comparison worse than awkward. *Meaning incommensurability* is the worst. Before Copernicus, the sun was a planet and the earth was not; afterward the earth was a planet and the sun was not. Thus no proposition about planets in the older theory could refer to the same things as similar-sounding sentences in the new theory. Hope for a realistic interpretation of theories would appear shattered. Some forms of realism therefore depend on theories of reference and meaning. So Hacking conducts a similar analysis and review of ideas on reference, including Hilary Putnam's four-component theory of meaning. Hacking's clarity justifies the book's subtitle, but his goal is not just pedagogical.

As the quotation in my first paragraph reveals, Hacking finds his realism in experiment, and in the second half of this volume ("Intervening") comes the excitement. Experiment occupies a singular place in the history and philosophy of science. It is endlessly hailed as one of the two pillars—along with mathematics—of the scientific edifice. But when the praises are over, the philosophical literature quickly changes track to ignore experiment. Even when philosophers do discuss experiments, they frequently reduce laboratory activity to observation and observation to the recording of meter readings. If experiments are construed this way, only two questions make sense. What is the psychology of perception? How do meter readings connect to full-blown scientific theories?

We need a better way if either history or philosophy of science is to depict modern science adequately. My sympathy is entirely with Hacking when he insists that

> noting and reporting of dials—Oxford philosophy's picture of experiment—is nothing. Another kind of observation is what counts: the uncanny ability to pick out what is odd, wrong, instructive or distorted in the antics of one's equipment. The experimenter is not the "observer" of traditional philosophy of science, but rather the alert and observant person. Only when one has got the equipment running right is one in a position to make and record observations. That is a picnic. [P. 230]

Before the picnic comes everything: instrument design, reliability tests, data protocols, data-reduction procedures. Hacking focuses primarily on one criterion for the establishment of the reality of an entity. If one can intervene and manipulate the entity for specific purposes (other than merely establishing the entity's existence), then it is reasonable to take that object as real: "*We are completely convinced of the reality of electrons when we regularly set out to build—and often enough succeed in building—new kinds of device that use various well-understood causal properties of electrons to interfere in other more hypothetical parts of nature*" (p. 265). It is this manipulation that Hacking judges to be robust under changes of theory, allowing talk of electrons to continue even when new properties are ascribed to the electron.

Hacking has taken us a long way from the traditional view, and I am with him for most of that journey. I part company when he elevates the criterion of manipulation far above other criteria demarcating the hypothetical from the real. Thus he professes skepticism toward black holes (pp. 274ff.). If we are setting out to provide a naturalistic depiction of what experimenters do, we need to recognize that they use many techniques other than manipulation to establish the existence of entities. Take the positron. In 1932 Carl Anderson first photographed a cosmic-ray particle passing through a lead plate inside his cloud chamber. The particle's track made it clear to him that the particle was positive in charge and of roughly the same mass as an electron. Within a very short time his experiment was repeated in dozens of other laboratories. Though these efforts were not passive, in a sense they were "observations"—no one could call up a positron at will. Certainly it was many years before positrons could be used for the exploration of more hypothetical entities. Thus, according to Hacking's criterion, Anderson's evidence must remain less than fully persuasive because for many years positrons were not exploited for probing other, less certain processes. A more modern example: the Z particle which mediates weak interactions, commands as close to a unanimous endorsement by the physics community as that disputatious group is ever likely to give. Yet the 1983 demonstration of the Z particle's existence relied not on *using* Z particles but on computer-aided data analysis, electronic signal sorting, and computer simulation to pull a few events from the background.

I suspect that we need a more open notion of demonstration strategies, for the changing technology of twentieth-century experimentation has provided new means to explore the small, fast, large, and distant inhabitants of the physical world. The classic particle detectors such as film, cloud chambers, bubble chambers, and Geiger counters made observations on cosmic-ray particles a persuasive way to argue for new entities. The computer warrants a new kind of observation in which data can be sorted and signals extracted from phenomena previously so complex as to have been opaque to observation. With these advances the sharp distinction between observing and intervening has blurred—not just analytically, but in the day-to-day work of the astrophysicist and physicist. Is there that much difference between the particle physicist's detector picking out one track in ten million and the astrophysicist's electronic equipment extracting the faint pulse of a digitized pulsar signal from the noise? Keeping the spirit of Hacking's work, let us expand the notion of intervention to include the manipulation and simulation of data. Amidst the big machines lie an array of demonstration techniques, some old and some new—a fascinating, largely uncharted land for the historian and philosopher of science.

PETER GALISON

Jarrett Leplin (Editor). *Scientific Realism.* vii + 266 pp., indexes. Berkeley/Los Angeles/London: University of California Press, 1984. $25.

"Like the Equal Rights Movement, scientific realism is a majority position whose advocates are so divided as to appear a minority" (p. 1). So writes Jarrett Leplin, the editor of this important new anthology exploring the epistemological issues and arguments that divide the scientific realists from each other and from their nonrealist and antirealist critics.

This collection of papers grew out of a conference on scientific realism held at the University of North Carolina in 1982. Weighing in for the realists are Ernan McMullin, Richard Boyd, Ronald Laymon, Michael Levin, Hilary Putnam, Ian Hacking, Clark Glymour, and Jarrett Leplin. Outnumbered, but not outmatched, the nonrealists are represented by Arthur Fine, Larry Laudan, and Bas van Fraassen. (The editor wisely omitted material from Karl Popper, Thomas Kuhn, and Paul Feyerabend; though their seminal work forms much of the backdrop for the present discussion, it is widely available elsewhere.) The papers by McMullin, Boyd, Fine, Levin, and Glymour appear here for the first time. Some readers may already be familiar with the second lecture from Putnam's *Meaning and the Moral Sciences* (London: Routledge & Kegan Paul, 1978) and with Laudan's "A Confutation of Convergent Realism" (*Philosophy of Science,* 1981, *48*:19–49). The pieces by Hacking and by van Fraassen are good introductions to the ideas in their recent books: Hacking, *Representing and Intervening* (Cambridge, 1983); and van Fraassen, *The Scientific Image* (Oxford, 1980). Fine's contribution is destined to become a modern classic. It is the centerpiece of a group of related articles that Fine has recently published defending his nonrealist (but not antirealist)