each revolution, who write the textbooks, present the past as if scientists had always striven for the objective embodied in today's paradigms.

It is probably still too early to assess the impact of Kuhn's potent thesis because its intellectual history is still in the making. Kuhn has made various modifications to the thesis, which are described in a postscript to the 1970 edition. But he has never written the expanded version that was originally promised. "I came to realize that I didn't have anything more to say in the same general vein," he said in a recent interview.

The response to Kuhn's thesis varies with discipline, being perhaps most reserved among his fellow historians of science. Some quibble with his interpretation of the case studies used to support the thesis. "Historians of science assent to it in a general way but not in specific examples," says a West Coast historian. "There has been surprisingly little work by historians of the natural sciences seeking either to vindicate or disprove his thesis," an East Coast colleague observes.

Historians of science value the book for its insights and as a heuristic tool, but seem not to use it as a guide for writing history. Historians tend to be skeptical of general theories of history, but there seem to have been rather few attempts even to put Kuhn's theory to test by applying it to particular historical episodes. John Greene, of the University of Connecticut, used the thesis in describing the development of Darwin's theory of evolution. "On the whole the paradigm doesn't work very well," Greene says, although it could with effort be made to fit some aspects.

There is in fact no Kuhnian school of history, and a general observation in the field is that Kuhn himself is not using the thesis in his present work, the early history of quantum theory. ("He spawned a monster, and doesn't know what to do with it," a colleague suggests.) Kuhn confirms that when writing history, "I try to forget the categories of that book." The theory outlined "is highly schematic and it is not going to apply in any detailed way to any particular episode," Kuhn says. As for his history of quantum theory, "I think there is going to be some grist for my mill, but I have tried very hard to tell this story for its own sake." Kuhn hopes to complete the book by the end of the year.

Philosophers of science have taken a much more active interest in *The Structure of Scientific Revolutions* than have historians. At least two symposia have been held to discuss Kuhn's thesis, and its influence on the philosophy of science is generally acknowledged. His ideas, although not his terminology, have permeated the middle ground between the logical empiricist position, where human psychology is allowed no place in scientific discovery, and the extreme relativism of the Feyerabendian school.

Kuhn's thesis has been welcomed as an antidote to the falsificationist theory of science propounded by Karl Popper of London University, which holds that theories cannot be confirmed, only refuted, and when refuted in any serious instance are abandoned. Kuhn's version, which has it that theories are only discarded when a better alternative is available, is psychologically more satisfying. "Kuhn believes that doing science is a deeply human activity," remarks philosopher Hilary Putnam of Harvard: "He is attacking the idea that the scientific method is a rule which factors out human psychology." Putnam rejects as jejune the relativism of Kuhn's notion that science cannot be said to progress toward truth, but does not consider the argument central to the thesis. In his view, the book is "certainly one of the major events of the period," at least in the English-speaking world.

Kuhn's emphasis on human factors naturally disturbed the logical empiricists and others. He was accused of portraying science as an irrational and subjective process, a charge he denies. The critics' reaction was understandable, because the emphasis of Kuhn's thesis is that logic alone cannot be decisive in a choice between theories. But, as Kuhn explained later, that does not mean that logic and experiment are not of great importance. Philosopher Dudley Shapere of the University of Maryland, one of Kuhn's severest critics on this point, concedes that Kuhn's aim was probably to show that science, "far from being a routine mechanical cranking out of results according to a prescribed method, without interesting intellectual content, was really creative, like art." But the implication of the argument is profoundly antiscientific, Shapere says, because it implies that what one accepts in science, as in art, is only a matter of taste. Yet even Shapere describes the *Structure of Scientific Revolutions* as "without question the most widely influential book on the interpretation of science in the past quarter century."

The appeal of Kuhn's thesis has not been confined to philosophers of science: it has had profound resonances in the social sciences and in fields as distant as general history and economics. "Not since the publication of R. G. Collingwood's *Idea of History* [in 1946] has a work of 'theory' won from historians the amount of interest recently accorded Thomas S. Kuhn's *The Structure of Scientific Revolutions*," commented David A. Hollinger in a 1973 article in the *American Historical Review*. The book "excites the imagination of working historians chiefly because much of what it says about scientific communities seems to apply so strikingly to other kinds of communities. . . . Historians are moved by Kuhn's sense of what a tradition is, what conditions sustain it, and what the relation is between tradition and innovation." But Hollinger acknowledged that some historians had found incongruous uses for the thesis, such as the comparison with a Kuhnian scientific revolution of the American decision to withdraw from Vietnam under the pressure of antiwar demonstrations.

Social scientists seized on the book for different reasons, and in part because it seemed to deflate the aura of the hard sciences in implying that they were not really so different in structure from the softer sciences. "I think this book doubtless seemed to have relieved them of an albatross," comments Kuhn.

Between 1970 and 1975, there occurred a "veritable explosion of interest in Kuhnian thought among social scientists," historian John D. Heyl observed in a review in *Society*. The thesis, says Heyl, "was bound to strike a responsive chord among scholars who were questioning the philosophical basis and the future direction of their disciplines." But amid the excitement over Kuhn's analysis of the physical sciences, few paused to examine Kuhn's brief remarks on the social sciences, in which he strongly suggests that they are in a preparadigmatic state. The "Kuhnian interlude" may not leave much of permanence in some of these disciplines, Heyl implies, but it has nevertheless occasioned a lively debate among thousands of scholars: "This experience has been enormously invigorating to the individuals involved and to the intellectual environment of which they are a part. Such an achievement, all too rare in our day, should be appreciated for its own sake."

Since Kuhn does not permit truth to be a criterion of scientific theories, he would presumably not claim his own theory to be true. But if causing a revolution is the hallmark of a superior paradigm, the *Structure of Scientific Revolutions* has been a resounding success.

—NICHOLAS WADE

Erratum: The name of the director of the New Orleans Sewage and Water Board was misspelled (24 June, page 1421). The correct spelling is Stuart H. Brehm.