Book Reviews

The Kuhnian Perspective

The Essential Tension. Selected Studies in Scientific Tradition and Change. THOMAS S. KUHN. University of Chicago Press, Chicago, 1978. xxiv, 366 pp. $18.50.

The Essential Tension is a collection of previously published essays by Thomas Kuhn to which the author has added two hitherto unpublished articles and a splendid, partly autobiographical preface. These assembled studies shed much light on the early evolution and subsequent reformulations of Kuhn's provocative *The Structure of Scientific Revolutions* (1962; revised edition 1970). Read as a group, they reveal a profound and subtle mind struggling to articulate and to resolve tensions between different modes of knowing and experiencing.

Public forms of expression frequently obscure the personal sources of creative work. Kuhn's introduction to this volume helps us to see that the connection between the context of his own discoveries and the arguments of his writings is a close one. Trained as a physicist (a "normal scientist"?), Kuhn was asked to put together a course of lectures on the history of 17th-century mechanics. As he began to wrestle with Aristotelian texts, he was confronted with a major difficulty: how could someone as intelligent as Aristotle have entertained so many absurd physical theories? Why Kuhn was discontent with his Whiggish science-textbook view of Aristotle is not entirely clear; another person might not have been so troubled. The solution to his frustrations came to him suddenly in the summer of 1947. To understand the meaning of an out-of-date text, one needed to recapture an out-of-date way of reading it; Newtonian vocabulary and concepts were an obstacle to understanding Aristotle's questions and the answers he gave to them. Once this insight had been achieved, writes Kuhn,

> Strained metaphors often became naturalistic reports, and much apparent absurdity vanished. I did not become an Aristotelian physicist as a result, but I had to some extent learned to think like one [p. xii].

The experience was formative and liberating, the prelude to other "revolutions." Once the integrity of thought modes different from his own had been acknowledged, the problem was to try to understand apparently untenable beliefs on their own terms.

For Kuhn, understanding a discipline other than one's own is like comprehending a new paradigm: one does not gain access to a new domain of knowledge through the mechanical manipulation of a set of algorithmic "correspondence rules." The point is nicely illustrated in his 1968 Isenberg Lecture at Michigan State University entitled "The relations between the history and the philosophy of science (p. 3)." He writes:

> During my days as a philosophically inclined physicist, my view of history resembled that of the covering law theorists, and the philosophers in my seminars usually begin by viewing it in a similar way. What changed my mind and often changes theirs is the experience of putting together a historical narrative [p. 16].

Kuhn's account of writing in three disciplines captures some of the crucial experiential differences. In characterizing the doing of history, he suggests that a basic element, as in the physical sciences, is the recognition of similarity relations. The historian's task is like putting together a puzzle in which certain limiting rules must be observed (no empty spaces in the middle, human limbs cannot be united with trees, and the like) but in which the outcome is something previously unseen. The sorting out of the pieces of the puzzle is a painful procedure. As Kuhn reports,

> There are almost always key points at which [the historian's] pen or typewriter refuses to function and his undertaking comes to a dead stop [p. 8].

By contrast, the writing of a physics paper is portrayed as a straightforward, relatively conflict-free experience. The methods, assumptions, and conclusions are all contained in one's notes, and the form and style are standardized. The philosopher's "research" is unlike either the physicist's or the historian's:

> One worries it—on paper, in one's head, in discussions with colleagues—waiting for the point at which it will feel ready to be written down. More often than not that feeling proves mistaken, and the worrying process begins again, until finally the article is born [p. 9].

These are valuable and unusually candid reflections. They are not set forth as general accounts of the way in which all physicists, historians, and philosophers work, but they reveal much about what it is like for one man to give birth to different kinds of knowledge. In so doing Kuhn provides us with a deeply human perspective which helps us better to understand the sources of his own fruitful ideas.

Many of the most famous elements of Kuhn's concept of scientific revolutions appear to be rooted in those personal experiences within the several disciplines and in his travels across them. Take the concept of "normal science." The early version of this notion is contained in Kuhn's "The function of measurement in modern physical science" (p. 178; first written in 1956). The physical sciences are seen as the paragon of "sound knowledge"; in what do their alleged soundness and their prestige reside? Kuhn argues that quantitative techniques are, as they have been taken to be, central to the success of science, but not because they bring about revolutions, an impression often conveyed by that ideological standard-bearer, the science textbook.

> To discover quantitative regularity, one must normally know what regularity one is seeking and one's instruments must be designed accordingly; even then nature may not yield consistent or generalizable results without a struggle [p. 219].

The function of measurement is to refine and mop up what is presupposed and established on other grounds; by itself it does not produce scientific laws, and very rarely (if ever) is it sufficient to overthrow theories. This recurring thesis is driven home in Kuhn's more recent article "Mathematical versus experimental traditions in the development of physical science" (p. 31; 1976), where he argues historically for the conservative role of experimentation and for the development of the classical physical sciences (astronomy, statics, optics, mathematics, harmonics) and what he calls the "Baconian sciences" (for example magnetism, electricity, chemistry) as two separate traditions. In the Middle Ages, experiments usually demonstrated a conclusion already established in advance; in the 17th century, there developed the concept of the experiment-by-constraint—nature forced by intervention to reveal features of itself. But even such interventions, though much lauded, were few in number, and

> All owe their special effectiveness to the closeness with which they could confront the evolving theories of classical [mathematical-

physical] science which had called them forth [pp. 45–46].

That Kuhn had described something fundamental is clear from the loud noise generated by critics and admirers alike. The 1965 London Colloquium in the Philosophy of Science (published in 1970 as *Criticism and the Growth of Knowledge*) devoted to his views and those of Karl Popper gave vent to feelings of tragedy among many philosophers and to battle cries for pedagogical reform. As Popper wrote,

In my view the "normal" scientist, as Kuhn describes him, is a person one ought to be sorry for. . . . The "normal" scientist, in my view, has been taught badly . . . all teaching on the University level (and if possible below) should be training and encouragement in critical thinking [*Criticism*, pp. 52–53].

Another critic, John Watkins, picked up some of the more dramatic phrasing in *The Structure of Scientific Revolutions*, in which normal scientific education is compared to orthodox theology in its narrowness and rigidity, and suggested that, by analogy, the relatively rare periods of extraordinary science must correspond to "a period of crisis and schism, confusion and despair, to a spiritual catastrophe" (*Criticism*, p. 33). Nothing expresses better what distinguishes Kuhn's sociopsychological approach from the approach of the Popperians than Watkins's comment that

From a methodological point of view, something rare in science—a path-breaking new idea or a crucial experiment between two major theories—may be far more important than something going on all the time [*Criticism*, p. 32].

With such an assumption, one wonders how philosophers of science could ever see the history of science as relevant to their concerns. To Margaret Masterman, on the other hand, normal science exists as "the crashingly obvious fact which confronts and hits any philosophers of science who set out, in a practical or technological manner, to do any actual scientific research" (*Criticism*, p. 60).

Whatever the disagreements among Kuhn's supporters and critics and whatever the ambiguities in his early formulations of the paradigm concept, his conceptualization of science has clearly urged serious attention to the following question: What place do shared elements—elements such as language and meaning, values, educational training, metaphysical presuppositions, theories, methodologies, and attitudes toward criticism—have in scientific inquiry? Kuhn's position is clear. From early on, as the essays in this volume reveal, he has tried to characterize science (as demarcated from other kinds of knowledge) by its high degree of consensus. Its strength and novelty notwithstanding, this perspective is not without difficulties. Many philosophers have pointed to the relativism implied by Kuhn's account of paradigms (now replaced by "disciplinary matrices") as closed systems of premises with no transparadigmatic juries to adjudicate disputes. Kuhn now concedes that the problem of translation and incommensurability between paradigms cannot be handled in the way he has attempted to handle it in the past, and he promises future thoughts on the matter (p. xxiii). Even descriptively, however, it is not clear that consensus-bound research is quite as pervasive as Kuhn has supposed. Consider a favorite case of Kuhn's, the "Copernican Revolution." Early reactions to the Copernican theory showed a strong methodological consensus among its *opponents*, who nevertheless borrowed heavily from Copernicus's planetary models. The scientific community of the time did not act as though it were choosing between different ways of doing science, nor did its members live in totally different universes of meanings. The first "textbook" of Copernican astronomy, written by Kepler and appearing some 75 years after Copernicus's *De revolutionibus*, was read and understood by opponents and supporters alike. One cannot help wondering whether Kuhn's concern, manifested as early as 1959 in the essay that gives the present collection its title, with epitomizing "*the nature* of education in the natural sciences" (p. 228, my italics) is itself time- and culture-bound—characteristic, that is, of his own education in postwar America. The description of his own "conversion" to the history of science and his account of the difficulties encountered by his philosophy graduate students in reading a text historically and by history students in dealing sympathetically with the analytic clumsiness of early thinkers reveal Kuhn's rich sensitivity to the problem of how we make contact with different frames of meaning and experience. That problem is a universal one, no more specific to scientists than to parents trying to understand children or men struggling to understand the experience of women or Americans the life-style of Europeans or anthropologists a foreign culture. If Kuhn has failed thus far to solve the problem of what demarcates science from other forms of knowledge, that is more than compensated for by the increase he has brought about in our hermeneutic sensibilities. No one who studies his writings will come away unchanged.

ROBERT S. WESTMAN
Department of History, University of California, Los Angeles 90024

Atmospheric Phenomena

Environmental Aerodynamics. R. S. SCORER. Horwood, Chichester, England, and Halsted (Wiley), New York, 1978. 488 pp., illus. $49.50. Mathematics and Its Applications.

Richard Scorer is a keen observer. He has greatly distinguished himself by explaining many visible phenomena by ingenious uses of physical principles. About 20 years ago, Scorer produced a well-received book called *Natural Aerodynamics*. In it he explained, in fairly qualitative terms, such atmospheric structures as mountain waves, plumes, clouds, and thermals.

In *Environmental Aerodynamics*, Scorer has retained much of the material of the first book, added some sections, and made his argument considerably more rigorous and mathematical. His introduction implies that "mesoscale" phenomena—meteorologists define mesoscale as those scales of motion that are too small to be seen on weather maps but yet contribute to weather—are emphasized more than in the first book.

Scorer deals primarily with those mesoscale phenomena that interest him and that he or his colleagues and students have investigated. The book includes chapters on secondary vorticity, the rotating earth, waves in a stratified fluid, billow mechanics, turbulence, clouds and fallout, and dispersion of pollution. Scorer does not discuss mechanisms and models of such important phenomena as sea breeze and thermally driven mountain-valley circulations.

The types of phenomena of interest to Scorer are discussed with clarity and ingenuity. But he limits himself to analytical solutions of linearized equations and does not mention the considerable progress achieved by the use of numerical models. He has a general bias against such models, which, in the reviewer's opinion, is only partly justified.

Scorer is a scientist of strong opinions, which makes the book provocative yet irritating. Some of what he states categorically is really controversial. He also dislikes much of the work of other mete-