Annu. Rev. Polit. Sci. 1998. 1:199–210

SOCIAL SCIENCE AND SCIENTIFIC CHANGE: A Note on Thomas S. Kuhn's Contribution

Nelson W. Polsby
Institute of Governmental Studies, University of California at Berkeley, Berkeley, California 94720; e-mail: nwpolsby@socrates.berkeley.edu

KEY WORDS: paradigm, Kuhn, social science

ABSTRACT

A new reading is proposed for *The Structure of Scientific Revolutions* by the late Thomas S. Kuhn, in which the sort of resistance to change he describes as integral to paradigm shifts is found to be less common in the contemporary hard sciences but useful and applicable in understanding political and social science.

INTRODUCTION

On June 17, 1996, Thomas S. Kuhn, historian and philosopher of science, died, aged 73. His famous book, *The Structure of Scientific Revolutions* (1970),[1] was expressly a study of the natural sciences chiefly before 1910, but it made a fascinating general argument about the process of scientific discovery that has proved both attractive and stimulating to a wide scholarly community.[2] The

[1]First ed., 1962; second ed., 1970, third ed., 1996. (The third edition differs from the second only by adding an index.) All citations refer to the second edition.

[2]In the last 25 years, according to the *Social Science Citation Index* (1972–1997), there have been 6009 references to it. Since 1976, 148 references have been made in political science journals. I suppose the most influential collection devoted to the book is *Criticism and the Growth of Knowledge* (Lakatos & Musgrave 1970). This was published after I had completed the first draft of this essay in 1968. The cogency and broad coverage of this book made me hesitate to publish my own thoughts for nearly three decades. Later contributions to the critical literature, many of them less persuasive, and a desire to mark Tom Kuhn's passing with an appreciation of the *(continued)*

1094-2939/98/0616-0199$08.00

purpose of this essay is to explore the usefulness of his argument in this book to our understanding of political science, considered as a social scientific enterprise.[3]

The Structure of Scientific Revolutions has figured prominently in the footnotes—albeit mostly in early, ceremonial footnotes—of a large number of works in social science, including in such honorific locations as pastoral messages of presidents of the American Political Science Association (Truman 1965, Almond 1966, Deutsch 1971. See also Wolin 1968, Landau 1972, Ball 1976, Wade 1977). So, to borrow the words of Mrs. Willy Loman, attention must be paid. It is a welcome bonus that the book states its arresting thesis in vigorous and graceful if not altogether unambiguous prose.

THE PROBLEM

The Structure of Scientific Revolutions is an attempt to give a general historical account of the growth of scientific knowledge. In doing so, Kuhn offers an alternative radically at odds with a conception of science "as the one enterprise that draws constantly nearer to some goal set by nature in advance" (p. 170), which he supposes to be the belief shared by most of his readers. For Kuhn, science has a more uneven history, in which distinct stages can be observed. Of these the most consequential are periods of scientific revolution, in which "extraordinary investigations...lead the profession at last to a new set of commitments, a new basis for the practice of science," producing a "shift in the problems available for scientific scrutiny and in the standards by which the profession determine[s] what should count as an admissible problem or as a legitimate problem-solution" (p. 6). These shifts are characteristically resisted: "A...new theory implies a change in the rules governing the prior practice of...science. Inevitably, therefore, it reflects upon much scientific work...already...completed. That is why a new theory, however special its range of application, is seldom or never just an increment to what is already known. Its assimilation requires the reconstruction of prior theory and the reevaluation of prior fact, an intrinsically revolutionary process" (p. 7).

Kuhn's argument is empirically grounded. He finds a basis for his account in "the major turning points in scientific development associated with the

potential of his ideas for understanding processes of cumulation in social science have prompted me to exhume and update this essay. Other interesting commentaries by Hall, Polayni, Glass, Toulmin, and Caldin (1963) were published in a symposium, "The Functions of Dogma in Scientific Research," devoted to a paper of Kuhn's that immediately preceded his book's publication and included many of his leading ideas (see also Gutting 1980).

[3]A more thorough treatment would at a minimum take account of Kuhn's other works, e.g. *The Copernican Revolution* (1957), *The Essential Tension* (1977), and *Black Body Theory and the Quantum Discontinuity 1894–1912* (1978), as well as his many articles.

names of Copernicus, Newton, Lavoisier, and Einstein.... [T]hese characteristics...can also be retrieved from the study of many other episodes that were not so obviously revolutionary. For the far smaller professional group affected by them, Maxwell's equations were as revolutionary as Einstein's, and they were resisted accordingly" (pp. 6–7).

As Kuhn sees the history of science, its most important feature—important, that is, in its consequences for subsequent work—is not an inexorable process of self-purification by which incrementally more adequate theories replace their slightly less adequate siblings after the new is scientifically demonstrated and then automatically and universally acknowledged to be superior to the old. What really happens, says Kuhn, is more likely to be a disagreement in which adherents to rival theories stand their ground for a while. He claims that because the theories entail different conceptions of what is problematic and what is taken for granted, it is impossible to show conclusively that one theory subsumes another. How does one theory eventually win out? By persuasion, not proof. What constitutes persuasion? First, a new theory, while perhaps in general not obviously more adequate than the old, is commonly addressed initially to problems that the old theory cannot solve easily. Thus in a narrow sense old and new may be compared. Second, Kuhn invokes the phenomenon of "conversion." By this he apparently means something akin to a psychological phenomenon called a Gestalt switch (pp. 110 and following), in which participants in psychological experiments suddenly see familiar things in a new light, as when a set of equivocal stimuli that a moment ago looked like a duck are newly perceived as a rabbit. By experiencing this switch of perception, scientists are held to adopt new theory.

PARADIGMS

The central event of a scientific revolution is thus the replacement of one scientific theory by another. The magnitude of the revolution is determined by the scientific importance of the theories involved, hence is determined by such things as the range of empirical phenomena to which the theories refer and the numbers of lawlike propositions produced. Kuhn attaches such significance to the sociological importance of scientific theories, however, that he adopts a special term, paradigm, to denote what is contested during scientific revolutions. The exact meaning of this term has become a matter of intense and protracted controversy, but without some attempt to come to grips with it, no fully intelligible reading of Kuhn is possible. [4]

[4]Masterman (1970, pp. 59–89) counts at least 21 different senses of the term in Kuhn, "possibly more, not less" (p. 61). Later on, she reconciles most of these definitions into a smaller number, as I also try to do.

By the term paradigm, I take Kuhn to mean that a scientific theory proffers a description or explanation of a range of events, and by so doing suggests an agenda of further work. Kuhn's "paradigms," are the psychological phenomena related to believing that the description or explanation is correct, and the sociological phenomena surrounding the coordinated enterprise of instrumentation, graduate education, textbook writing and reading, and "problem-solving" according to the suggestions of the theory's agenda (called by Kuhn "normal science"). These two fellow travelers of scientific theories, normally operating jointly, are what I believe Kuhn means to call paradigms.

Several things more should be said about Kuhn's paradigms. First, they are not, I think, synonymous with "philosophical world view." They do not exist apart from the scientific theories to which they are attached. They are, rather, world views in a restricted sense; they entail belief in the adequacy of specific scientific descriptions or explanations and acceptance of the agenda of normal science that these descriptions suggest. The changes in "world view" that Kuhn describes as resulting from paradigm shifts are changes in scientific theories, which in turn require changes in the agenda of normal science. Second, paradigms are not merely vocabularies. While a view of reality may be weakly implied by a comprehensive set of recommendations as to the use of terms, I do not see how an agenda of scientific research can be so implied, unless we take normal science to mean something as atheoretical as the activity (once common in political science) of translating English into, let us say, "systems theory" jargon.

A third misreading of Kuhn is to assert a close connection in general between a single paradigm and an entire academic discipline. Whether academic disciplines are congruent and coextensive with single scientific theories is an empirical question. Sometimes scientific theories are so powerfully persuasive that the agendas they inspire do cause new academic disciplines to split off from the old; but after a while special theories take hold, the division of labor leads to borrowings at different intellectual boundaries, and the overall coherence of the enterprise breaks down so that for most purposes there is no longer a single paradigm but several in peaceful coexistence—perhaps, as Kuhn suggests, for a mature scientific community, a closely related set (Kuhn 1970, p. 161). This state differs from the "preparadigm" situation, in which (*a*) there may be an identifiable subject matter, but (*b*) few empirical propositions, and (*c*) little agreement either about the shape of reality or about an agenda of priorities for future work.

Fourth, the limitations that paradigms place on the advance of knowledge are recognizable only by comparison between their associated theory and a rival scientific description, with its rival empirical claims and its rival agenda. In the first instance the recognition of anomaly provokes remedial action rather

than immediate paradigm rejection; for paradigm rejection to occur, a rival theory must be invented (Lightman & Gingerich 1991).

Fifth, the magnitude of a scientific revolution is recognizable in part by the size of the paradigm shift that it entails. Kuhn contributes the valuable insight that the importance of a scientific theory is discussable in terms of the changes it creates in the agendas, texts, and instrumentation of scientists (i.e. in scientific "practice,") and in scientists' beliefs about the contours of that part of the "real world" comprehended by the theory to which the paradigm refers.

EMPIRICAL DIFFICULTIES

There are, in scientific discourse, agreed-on criteria by which scientists can judge when one scientific theory ought to replace another. These are commonly accepted standards that scientists invoke when they compare the adequacy of rival explanatory theories. Better theories are more parsimonious, covering the same ground as less good theories more succinctly, precisely, or elegantly; they are more capacious, covering more observed regularities than less good theories; and they are richer, predicting regularities not yet observed and bringing into focus items that previously were blurred, unseen, or thought to be unrelated (Kuhn 1970, pp. 151–55; see also Cohen & Nagel 1934, Nagel 1961). Kuhn gives a number of examples of the application of these standards. For example, "to most chemists Dalton's new paradigm proved convincing where Proust's had not been, for it had implications far wider and more important than a new criterion for distinguishing a mixture from a compound" (pp. 133–34).

To state such criteria is by no means to assert that they are self-applying. However, contemporary case studies from the highly formalized "hard" sciences suggest that there are a number—perhaps a large and growing number—of instances of widespread and virtually instantaneous agreement within scientific communities about when scientific revolutions take place, based on widely understood applications of such criteria as these. For examples, see Watson's (1969) account in *The Double Helix* of the discovery of the crystal structure of the DNA molecule, or Bernstein's (1967) discussion in *A Comprehensible World* of the events surrounding Yang & Lee's formulation of their theory concerning the conservation of parity in weak intra-atomic interactions.

These may simply be inaccurate reports. But if not, these cases seem to record instances in which scientific communities received and assimilated substantial scientific advances by adhering to agreed-on standards for choosing between rival theories, standards that are often explicitly formulated and commonly accepted among scientists. Kuhn describes the application of these standards as exercises in persuasion, not proof. These cases suggest, however, that persuasion can come quickly and carry the day overwhelmingly. The holdouts are far fewer and more isolated than one would predict from Kuhn's

account. We must therefore consider the possibility that "revolutions," in Kuhn's terminology, are a sign of the immaturity of a scientific discipline (Gillies 1992, Kolata 1983).

Yet it is evident that the replacement of one scientific theory by another has not always followed a smooth path. There have been occasions, Kuhn asserts, drawing his examples almost entirely from pre–20th century science, when standards like those proposed above promoted less quick and universal agreement in their application, and so did not fully govern the behavior of scientists. Sensitized by Kuhn's argument, we can find more modern examples (e.g. Morell[5] 1997, pp. 699–702; Fleck 1979; McKenzie 1977, pp. 97–124; Stacey 1997, pp. 25–29). Thus, *The Structure of Scientific Revolutions* can be read as an attempt to account for the behavior of scientific communities, not under all conditions, but rather under conditions where important new ideas that later prevail are not immediately accepted.

Even here, Kuhn does not always seem in full control of his argument. This is understandable because of both its complexity and its originality. But it forces the reader to hesitate between mutually incompatible readings of his text, as can be seen in the juxtaposition of the following quotations from his argument:

1. "Crucial experiments—those able to discriminate particularly sharply between the two paradigms—have been recognized and attested before the new paradigm was even invented" (p. 153).
2. "The competition between paradigms is not the sort of battle that can be resolved by proofs" (p. 148).
3. "If...there can be no scientifically or empirically neutral system of language, or concepts, then the proposed construction of alternate tests and theories must proceed from within one or another paradigm-based tradition" (p. 146).
4. "It makes a great deal of sense to ask which of two actual and competing theories fits the facts *better*" (p. 147, emphasis in original).

These statements sit uncomfortably side by side. Either differences between theories can in principle be resolved in an orderly fashion or not. It is hard, without evidence, to believe that the relevant mechanism is in fact Gestalt shifts, if scientists have pertinent experiments or other empirical means at their disposal for discriminating between rival theories. If Kuhn's discussion of crisis and anomaly in normal science is to make sense, his claims about the significant role of Gestalt shifts and conversion phenomena in the acceptance

[5]Morell describes Carl Woese as having "revised the tree of life and started a new age in microbial biology by recognizing a third domain of life."

of scientific revolutions will have to be better supported or sharply modified, and, as a consequence, so will some of his claims about the power of paradigms to constrict the understanding of scientists.

This reading of Kuhn scales down a few of his general claims, but only in the interests, first, of harmonizing his account with instances of relatively painless or unturbulent scientific revolution, and second, of achieving greater internal consistency with respect to the sticky problem of anomaly and crisis in "normal"—non-revolutionary—science. Neither of these occasions of change, strictly speaking, would be possible unless at least some of the scientists operating within a given paradigm could get far enough outside alleged paradigmatic perceptual constraints to appreciate the weaknesses of the regnant paradigm. Kuhn is at his least satisfactory in grappling with this problem; he fades away at some points into psychological mystifications, at another point into a tautology that turns on the meaning of the undefined term "fundamental," as in the phrase "when scientists change their minds about fundamental matters..." (p. 121). At several points, in his enthusiasm for Gestalt psychology, he comes close to denying that normal science is capable of creating and recognizing anomalies and hence paradigmatic crises.

What remains of Kuhn's argument? Quite a lot. Here is a brief restatement: The adequacy of most scientific theories has, for most of human history, been much more difficult to assess than it is today in hard science. Thus, although standards like parsimony, richness, and capaciousness may have been around a long time, in practice a principal economizing device that scientists have used in assessing and comparing theories has had less to do with their convictions about the fulfillment of these three criteria than with each theory's strategic capacity to account for unpredicted or unexpected (but reliably observed) behavioral regularities that scientists, for reasons not given, consider particularly important. Thus, Kuhn says, one of the main methods by which science has grown has been through the identification of significant anomalies and in attempts to bring them under covering laws. The general superiority of theories accounting for anomalies—their superior parsimony, capaciousness, and/or richness—has not always been immediately apparent to scientific communities; thus rival theories accounting for an overlapping range of phenomena have, Kuhn suggests, frequently coexisted.

Kuhn's method of demonstration is anecdotal, and so seemingly contrary anecdotes are apposite. We now know, or at least think we know, of some cases where scientists' conceptions of the world and their work agendas have been drastically altered without the conflict and resistance that seem to have characterized earlier periods of science. I think we can assume that the innate stubbornness of scientists has not changed much over the centuries. So the differences between Kuhn's examples and more modern instances not convincingly covered by his description must depend, not on the psychological charac-

teristics of scientists, but rather on a sociological comparison of organizational properties of modern and older, harder and softer, scientific communities. I conjecture that the most important differences between the two are in the speed and efficiency of their communication, as a consequence of modern communications technology, laboratory equipment, divisions of labor and, in particular, the increased formalization and elaboration of scientific language.[6] All of these—especially the last—have led to a more widely shared, explicitly articulated set of expectations about the results of normal science; therefore, scientific anomalies have stood out more readily and have been reobserved more speedily. The movement from anomaly to crisis has quickened. The movement from crisis to paradigm replacement likewise may have changed in character, at least in the hard sciences, accounting for those occasions when resistance to scientific revolutions seems much less intense or frequent.

SOCIAL SCIENCE APPLICATIONS

For the social sciences, however, which have by no means reached a stage of development along these dimensions comparable with the hard sciences, Kuhn's account of conflict-ridden scientific revolutions seems quite helpful. At the grandest level, we can think of the still-controversial intellectual reorientations associated with the names of Marx, Freud, and Keynes.[7] But even in a more modest way, as befits the scope of most social science theories, social science can provide analogies to Kuhn's description of the roots of scientific controversies. Three political science examples are the controversies surrounding the study of community power (Polsby 1980), the economic interpretation of the American constitution (Beard 1941, Brown 1956, McDonald 1958), and the synoptic versus incremental views of administrative behavior (Lindblom 1959, Cyert & March 1963, Lindblom 1965, Smithies 1955, Wildavsky 1964). In all three cases, coherent but contrary explanations have been offered for an important range of events. Different weights have been assigned to similar variables, different constructions have been placed on similar and overlapping empirical propositions, and different agendas have been advocated and pursued. Attacks have been met with counterattacks. It is not easy to tell, over the short run, who wins these academic battles. This is consistent with Kuhn's description. Adjudication of rival claims through swift application of the criteria used by the communities that welcomed the revolutions of

[6]This conjecture is compatible with Kuhn's later view that incommensurability of paradigms is analogous to untranslatability between two languages (see the interview with Kuhn in Borradori 1994, pp. 161and following; see also Garvey et al 1970, pp. 1166–73).

[7]So pervasive has been the influence of these three thinkers that it seems impractical to give sources. A reader might, however, enjoy Crews 1989, 1996; see also Hoffman 1977, Coats 1969, Kuhn 1970.

Watson & Crick and Yang & Lee still seems a long way off. Nevertheless, the social sciences are constituted—however loosely—as communities, and in this social fact is embedded the potential for cumulation of knowledge.

CONCLUSION

What we might think of as a modern, hard-science epilogue to Kuhn's book would suggest that scientific communities vary in their capacities to assimilate scientific revolutions. The more formal their language and the more precise their empirical expectations, the more readily they can identify normal-science anomalies and stimulate the replacement of scientific paradigms.

Thus my reading of Kuhn leads to at least the following conclusions:

1. Theories and paradigms are not the same thing, but they are associated phenomena; a theory is necessary in order for there to be a paradigm. Obversely, no theory, no paradigm.
2. In what sense are alternative paradigms incommensurable? Only, but not trivially, in the sense that they embody a psychological dimension (beliefs about currently acceptable agreed-on knowledge) and a sociological dimension (texts, curricula, instrumentation, research programs), where scarce resources or the sunk costs of prior intellectual investment preclude or discourage the pursuit of alternative views. I think we must read Kuhn as concluding, despite some hesitancy and lack of clarity, that there are in fact ways of adjudicating between scientific explanations. We can stay within his historical account by saying that theories are commensurable through application of agreed-on standards, but that the sociopsychological paradigms adhering to them may not be.
3. Does acceptance of Kuhn's account require a belief that the body of scientific knowledge is a social construction? Yes, in the sense that all information held in common by social groups is socially constructed, as are the criteria that groups (for example, scientific communities) use to come to conclusions about what they believe. As physicist John Ziman said, "The deep message of *The Structure of Scientific Revolutions* was that...a scientific theory can only be grasped metascientifically as an entity with intertwined philosophical, historical and sociological characteristics" (Ziman 1983, p. 24; see also Ziman 1992, Ben-David & Sullivan 1975). This view does not, however, entail a belief that all social constructions claiming scientific standing have equal validity or that there is no "real world." To the contrary, as the maverick anthropologist Lesley A. White (1954, p. 2363) wrote—about mathematics—half a century ago,

> Mathematical concepts are man-made just as ethical values, traffic rules and bird cages are man-made. But this does not invalidate the belief that mathe-

> matical propositions lie outside us and have an objective reality. They do lie outside us. They existed before we were born. As we grow up we find them in the world about us. But this objectivity exists only for the individual. The locus of mathematical reality is cultural tradition." (1954, p. 2363)

The importance of this point runs beyond the salient reminder that the empirical claims of scientific disciplines are not arbitrary. Indeed, they are backed by social organizations and norms of practice that help practitioners maintain their mutual attention to problems, to ideas, and to one another. In the absence of this mutuality of attention, a scientific community is not possible (Ben-David 1991, Hagstrom 1965), and both continuity in the growth of knowledge and meaningful conflict among scientists are precluded. Thus Kuhn's contribution is to propose an empirically grounded alternative description of the operations of scientific communities, rather than, as is occasionally claimed, a rationale for the abandonment of confidence in the scientific enterprise.

After all, some propositions are much better supported than others, and there are widely understood rules and practices that permit members of a scholarly community to tell the difference. In later iterations of his argument, Kuhn went out of his way to say that the strong warrants commonly employed in conventional science gave entirely appropriate grounds for belief. So Kuhn supplies no comfort to the view that all empirical claims are equal, or that science is no different from any other set of social constructs proffering empirical claims (see also Barnes 1982). Rather, Kuhn wrote that *The Structure of Scientific Revolutions* "was intended to suggest that the status of knowledge is in no way reduced when knowledge is seen as social" (1983, p. 30).[8]

No doubt to a social scientist the unproblematic application of agreed-on criteria for judging between rival theories and their agendas seems a long way off. I think in this respect our community resembles that of 18th century chemists more than that of contemporary hard scientists. If that seems too optimistic a conclusion, perhaps we can settle for this: In Kuhn's hands, the history of science becomes a pageant of achievement so thoroughly grounded in human fallibility and human cooperation that students of the social sciences might easily notice a kinship between the scientific enterprise and our own.[9]

[8]The Rutgers anthropologist Robin Fox, who knew Kuhn, describes the "hapless Tom Kuhn" as "horrified" by the "mangling" of his theory "to justify an ultimately totally relativistic epistemology" (Gross et al 1996, p. 335).

[9]Gutting (1980, pp. 12–15, 19) draws precisely the opposite conclusion.

Literature Cited

Almond GA. 1966. Political theory and political science. *Am. Polit. Sci. Rev.* 60: 865–79

Ball T. 1976. From paradigms to research programs: toward a post-Kuhnian political science. *Am. J. Polit. Sci.* 20:151–77

Barnes B. 1982. *T.S. Kuhn and Social Science*. New York: Columbia Univ. Press. 135 pp.

Beard CA. 1941. *An Economic Interpretation of the Constitution of the United States.* New York: Macmillan. 330 pp.

Ben-David J. 1991. *Scientific Growth: Essays on the Social Organizational and Ethos of Science*. Berkeley: Univ. Calif. Press. 591 pp.

Ben-David J, Sullivan TA. 1975. Sociology of science. *Annu. Rev. Sociol.* 1:203–21

Bernstein J. 1967. *A Comprehensible World: On Modern Science and its Origins*, pp. 35–73. New York: Random House

Borradori G. 1994. *The American Philosopher*, pp. 161ff. Chicago: Univ. Chicago Press

Brown RE. 1956. *Charles Beard and the Constitution, a Critical Analysis of* An Economic Interpretation of the Constitution. Princeton, NJ: Princeton Univ. Press. 219 pp.

Coats AW. 1969. Is there a structure of scientific revolutions in economics? *Kyklos* 22: 289–96

Cohen MR, Nagel E. 1934. *An Introduction to Logic and Scientific Method.* New York: Harcourt Brace. 467 pp.

Crews F. 1996. See Gross et al 1996, pp. 470–82

Crews F. 1989. *The Sins of the Fathers: Hawthorne's Psychological Themes*. Berkeley: Univ. Calif. Press. 293 pp. 2nd ed.

Cyert RM, March JG. 1963. *A Behavioral Theory of the Firm*. Englewood Cliffs, NJ: Prentice-Hall. 332 pp.

Deutsch KW. 1971. On political theory and political action. *Am. Polit. Sci. Rev.* 65:11–27

Fleck L. 1935. *Genesis and Development of a Scientific Fact*. Transl. F Bradley, T Trenn, 1979. Chicago: Univ. Chicago Press. 203 pp.

Fox R. 1996. State of the art/science in anthropology. See Gross et al 1996, pp. 327–45

Garvey WD, Lin N, Nelson CE. 1970. Communication in the physical and the social sciences. *Science* 170:1166–73

Gillies D, ed. 1992. *Revolutions in Mathematics*. New York: Oxford Univ. Press. 353 pp.

Gross PR, Levitt N, Lewis MW, eds. 1996. *The Flight from Science and Reason*. *Ann. NY Acad. Sci.*, vol. 775. New York: Ann. NY Acad. Sci. 593 pp.

Gutting G, ed. 1980. *Paradigms and Revolutions*. Notre Dame, IN: Univ. Notre Dame Press. 339 pp.

Hagstrom W. 1965. *The Scientific Community*. New York: Basic Books. 304 pp.

Hall AR, Polayni M, Glass B, Toulmin S, Caldin EF. 1963. The functions of dogma in scientific research. In *Scientific Change*, ed. AC Crombie, pp. 347–95. New York: Basic Books

Hoffman S. 1977. An American social science: international relations. *Daedalus* 133:41–60

Kolata G. 1983. Number theory problem is solved. *Science* 22:349–50

Kuhn TS. 1957. *The Copernican Revolution*. Cambridge, MA: Harvard Univ. Press. 297 pp.

Kuhn TS. 1970 (1962). *The Structure of Scientific Revolutions*. Chicago: Univ. Chicago Press. 172 pp.

Kuhn TS. 1970. Logic of discovery or psychology of research? See Lakatos & Musgrave 1970, pp. 1–23

Kuhn TS. 1977. *The Essential Tension*. Chicago: Univ. Chicago Press. 366 pp.

Kuhn TS. 1978. *Black Body Theory and the Quantum Discontinuity 1894–1912.* Oxford: Clarendon. 356 pp.

Kuhn TS. 1983. Reflections on receiving the John Desmond Bernal award. *4S Rev.* 1. Winter: 30

Lakatos I, Musgrave A, eds. 1970. *Criticism and the Growth of Knowledge*. Cambridge, UK: Cambridge Univ. Press. 282 pp.

Landau M. 1972. *Political Theory and Political Science*, pp. 56–77. New York: MacMillan. 244 pp.

Lightman A, Gingerich O. 1991. When do anomalies begin? *Science* 255:690–95

Lindblom CE. 1965. *The Intelligence of Democracy: Decision Making Through Mutual Adjustment*. New York: Free. 352 pp.

Lindblom CE. 1959. The science of muddling through. *Public Admin. Rev.* 19:79–88

Masterman M. 1970. The nature of a paradigm. See Lakatos & Musgrave 1970, pp. 59–89

McDonald F. 1958. *We the People: The Economic Origins of the Constitution*. Chicago: Univ. Chicago Press. 436 pp.

McKenzie DP. 1977. Plate tectonics and its relationship to the evolution of ideas in the geological sciences. *Daedalus* 133: 97–124

Morell V. 1997. Microbiology's scarred revolutionary. *Science* 276:699–702
Nagel E. 1961. *The Structure of Science*. New York: Harcourt Brace. 618 pp.
Polsby NW. 1980. *Community Power and Political Theory*. New Haven, CT: Yale Univ. Press. 245 pp. 2nd ed.
Smithies A. 1955. *The Budgetary Process in the US*. New York: McGraw-Hill. 486 pp.
Stacey M. 1997. The fall and rise of Kilmer McCully. *NY Times Mag*. Aug. 10:25–29
Truman DB. 1965. Disillusion and regeneration: the quest for a discipline. *Am. Polit. Sci. Rev.* 59:865–73
Wade N. 1977. Thomas S Kuhn: revolutionary theorist of science. *Science* 197:143–45
Watson JA. 1969. *The Double Helix: A Personal Account of the Discovery of DNA*, pp. 138–40. New York: New Am. Libr.
White LA. 1954. The locus of mathematical reality: an anthropological footnote. In *The World of Mathematics*, ed. JR Newman, 4:2363. New York: Simon & Schuster
Wildavsky A. 1964. *The Politics of the Budgetary Process*. Boston: Little Brown. 216 pp.
Wolin SS. 1968. Paradigms and political theories. In *Politics and Experience*, ed. P King, BC Parekh, pp. 125–52. Cambridge, UK: Cambridge Univ. Press
Ziman J. 1983. Introduction of the 1983 recipient of the John Desmond Bernal award—Thomas S Kuhn. *4S Rev.* 1. Winter, 24
Ziman J. 1992. Subversive scholar. *Times Higher Ed. Suppl*., Nov. 27

Annual Review of Political Science
Volume 1, 1998

CONTENTS