

Sal Restivo
RENSSELAER POLYTECHNIC INSTITUTE

The widespread adoption of the rhetoric of *normal science*, *revolutionary science*, *paradigms*, and *exemplars* by sociologists of science and sociologists in general suggests the strong influence that Thomas S. Kuhn's *The Structure of Scientific Revolutions* (1962, 1970) has had on sociological inquiry. But claims that Kuhn's work is anti-Mertonian, nonnormative, relativistic, an alternative to positivism and logical empiricism, and even compatible with Marxism are all part of the myth of the Kuhnian revolution in the sociology of science.⁽¹⁾ The central dogma in the Kuhnian mythology is that Kuhn's paradigm is a significant, indeed a *radical*, alternative to Merton's. Some Mertonians (see Gaston, 1979, pp. 118-119) have been more perceptive about the Kuhnian mythology than have critics of Mertonian sociology of science. Gaston suggests (correctly I think) that Kuhn's reception is

*Published in Randall Collins (ed.), *Sociological Theory* 1983 (Jossey-Bass, San Francisco 1983), pp.293-305. Due to formatting problems, footnotes are indicated in parentheses instead of by superscripts. I have also added some explanatory notes in brackets, updated the references, and added an appendix.

Sociological Theory 1983 294

accounted for by the simplicity and vividness of his discussion of science. But Gaston's remark that there is a consensus about the importance of Kuhn's work ignores the resistance of historians of science to Kuhn's views on science and scientific change (Beaver, 1979. p. 140; Reingold, 1980). The important point is that Gaston recognizes the compatibility of Merton and Kuhn.

Ben-David (1979, pp. 204-205) notes that Kuhn and Merton were not opposed for at least a decade following the publication of Kuhn's work on scientific revolutions. British sociologists of science were the chief creators of the anti-Mertonian Kuhn.(2) Ben-David's assessment of the British and American reactions as consequences of different (not to mention competing) intellectual traditions may be accurate. But even the British tended to adopt Kuhn as the standard-bearer for an analysis of science that has been generally far less radical than proclaimed in the advertisements for British anti-Mertonian sociology of science. Kuhn may very well be a man for all sociology of science seasons, but he seems peculiarly suited for neo-Mertonian revisionism.(3)

The Myth of Kuhn

Merton (1977, pp. 106-107) has commented on the anomaly of acolytes who fail to distinguish the substance from the fate of Kuhn's work.(4) He has also pointed out numerous convergences between his work and Kuhn's. Merton criticizes Kuhn's restrictive sociology but insists that he and Kuhn are at one regarding the significance of the institutions and values of science as contexts for cognitive decisions. This is interesting because according to some interpreters Kuhn has helped shift the focus of attention in the sociology of science to cognitive

factors. But Kuhn has done little more than what Merton has done: drawn attention to the social context for cognitive decisions. This is far from revolutionary.

One of the curious features of Kuhn's sociology of science is that it does not treat sociological factors as problematic. Kuhn (1970, p. 166) notes, for example, that normal science is educationally narrow, rigid, and ill-designed to foster creativity. This characterization is not incompatible with scientific progress because rigidity accrues to individuals in Kuhn's theory. But he ignores the fact that rigidity accrues to organizations and institutions too. Bureaucratization, professionaliza-

Sociological Theory 1983 295

tion, and social controls imposed by political authorities all contribute to rigidity in collectivities (Restivo, 1975, pp. 158-162). This sort of sociological myopia is further reflected in Kuhn's assumption that the supply of innovators-young scientists and those who have entered new areas of research, according to Kuhn-is independent of social conditions within and outside of science.(5)

The Kuhnian mythology probably stems from the fact that Kuhn discusses scientific change in terms of a political model, uses the term *revolution* to describe a certain type of innovative period in science, and underscores the persuasive aspects of scientific discourse. But Kuhn's model is based on the modern political revolution, a form of social change generally characterized by the circulation of elites. Kuhn (1970, pp. 92-110) argues that political and scientific revolutions are both associated with the perception among members of a small element of the political or scientific community that institutions are malfunctioning. This sense of malfunction leads to a crisis and

fosters revolution. The aim of the revolutionaries is to change institutions; their claim is that the changes are incompatible with the mechanisms for change offered by the institutions themselves. This viewpoint, with its emphasis on sensing malfunctions, obscures the unintended consequences of social actions preceding the crisis and the dialectical sources of change inherent in social structural contradictions.

According to Kuhn, the period of crisis polarizes competing parties in both political revolutions and scientific revolutions. In both cases, defenders of the old order confront advocates of a new order. At this stage, differences between the contending groups can no longer be resolved by recourse to normal political strategies or, in the case of science, normal methods and logic. Institutional governance breaks down. The competing groups cannot agree on means for achieving and evaluating change within existing institutional contexts, and they reject suprainstitutional modes of conflict resolution. The result is recourse to mass persuasion and then force. In scientific revolutions, Kuhn argues, the process of paradigm choice is eventually accomplished on the basis of the highest standard: the assent of the scientific community. The extrainstitutional nature of revolutions in politics and in science, Kuhn contends, is a key aspect of the *evolution* of institutions.

296 Sociological Theory 1983

Kuhn has responded to his critics by diluting the political content (Paretan [Pareto] in essence) of his theory. Like the Mertonians, he expresses satisfaction with what science is as a human enterprise, supports elitism in science, and interprets the pursuit of individual and cliquish interests as compatible with scientific progress. Resistance to

new ideas and discoveries is, for Kuhn as for Merton, not a violation of the commitment to the pursuit of knowledge but evidence for consensus maintenance, a necessary condition for "normal science." The Mertonian Kuhn believes that science-as-it-is is a well-functioning institution; it is the paradigmatic mode of inquiry. discovery; prediction, and explanation. Kuhn joins Merton in giving a prescriptive account of science thinly disguised as objective history and sociology of science; he binds science to a Grand Paradigm of methods, logic, and rationality that remains unperturbed as science goes through its sinusoidal dance of normal and revolutionary periods. The final irony is that while the true believers in the sociology and history of science have been congratulating each other on the Kuhnian revolution, Kuhn himself (1978) has been busy doing internalist history of science unencumbered by notions of paradigms, exemplars, and scientific revolutions.

Kuhn's conception of science is being challenged on a number of fronts. His view of scientific revolutions as the result of conflicts between old and new guards is at odds with current research on the comparative history of mathematics. Major innovations in mathematics appear to be primarily the result of conflicts among rival innovators (see Collins and Restivo, 1983). And the physicist David Bohm (1973) has countered Kuhn's account of science with an argument for science in permanent revolution. Let me turn now to some recent developments in the sociology of science that are carrying us beyond Merton and Kuhn.

Beyond Merton and Kuhn

Marxism is an obvious alternative to the Mertonian-Kuhnian perspective. Marxists have made substantial contributions to the analysis of scientific knowledge as a product of social and political development, commerce, and industry; and they have pioneered in the study of

the social roots of mathematics, an area sociologists of knowledge and science influenced by Mannheim (1936, p. 79) and Merton have virtu-

297 Sociological Theory 1983

ally ignored.(6) But with the possible exception of Marx's ambiguous and aborted criticism of science-as-it-is, Marxists have generally accepted the Grand Paradigm of modern science along with Mertonians and Kuhnians. Alfred Sohn-Rethel's (1978) attempt to provide a materialist account of pure mathematics, for example, fails in the end because he is unable to eliminate transcendental and idealist assumptions about science and mathematics from his analysis. Attempts by Marx (1974) and Marxists (B., 1978) to show that science, and in particular mathematics, obeys dialectical laws have been singularly unsuccessful. Nonetheless, the renaissance in Marxist science studies during the past decade has produced an important alternative to the Mertonian-Kuhnian agenda.

The traditional opposition between functionalism and conflict theory in general sociology has become an increasingly prominent feature of the sociology of science as the Mertonian hegemony has weakened. There is an affinity between conflict theory and Marxism, but conflict theories tend to be less reductionist than Marxist theories. Political and economic factors are prominent but not exclusive features of conflict models in the sociology of science (Collins. 1975, pp. 170-523). The conflict perspective on scientific facts as social constructions conditioned by social contingencies converges with the constructivist perspective emerging from the laboratory life studies.

The laboratory life studies bring the tools and techniques of ethnography to bear upon the description and interpretation of scientific

work. They reflect a variety of substantive concerns, methodologies, and theories; but they all tend to stress inquiry as a constructive rather than a descriptive activity, reject the distinction between social and cognitive aspects of science, emphasize situation in the social construction of scientific facts (and thus challenge conventional ideas about the organization of scientific activities in scientific communities), and describe scientific facts as "discursive accomplishments" (Knorr, 1981; Woolgar, 1981). These studies raise fundamental questions about facticity and present new problems regarding the relationship between science and the sociology of science—questions and problems that are at the center of the controversy among advocates of the strong, moderate, and weak programs in the sociology of knowledge and science.

David Bloor (1976) argues that the best way to study science and knowledge is to proceed as the other sciences proceed. His **strong *pro***

Sociological Theory 1983 298

gram opens the way for sociological studies of all forms of knowledge including mathematical knowledge. But the scientific premise of the strong program is at odds with Bloor's argument against the idea that mathematics has a life of its own. This argument should apply to science in general; but if it is so applied, the strong program is unhinged. In its present form, the strong program is an adjunct to the Mertonian-Kuhnian program.

I refer to Campbell's (1974) descriptive evolutionary epistemology as the ***mild program*** in the sociology of science because it shares a demarcationist perspective with the strong program (science is viewed as superior to other modes of inquiry) but is more flexible about scientific method. Campbell contends that facts are not neutral, self-evident

entities. But he argues that it is essential to preserve the ideology of facts that speak for themselves because it has a functional truth. While this argument sets Campbell down on the side of the Mertonians and Kuhnians, his cautious affection for Feyerabend and his defense of "wildness" and "unjustified variation" in science align him with anarchist (or, to use Feyerabend's term, dadaist) inquirers.

The *weak program* in the sociology of science involves a worldview approach to science studies (Restivo, 1980; Chubin, 1981). Briefly, the weak program is based on the following ideas: (1) theories, systems of knowledge, and facts are embedded in and reflect world views; (2) following Bohm (1973), theories are considered insights, and insights are neither true nor false—they are only more or less successful strategies in certain contexts of human thought and action; (3) no insight can ever be final or absolute; (4) no system for arriving at insights can ever be universally valid and eternally stable; (5) there is always a broader context for establishing an insight than the context of any given system of inquiry; (6) the focus of the sociology of knowledge and science is the general epistemic activity of human beings; (7) there is no a priori reason for granting special status to the logic, methods, and theories of the Grand Paradigm of modern science in the realm of inquiry; (8) ontological nihilism and philosophical relativism are rejected, but Feyerabend's (1978) Protagorean relativism is affirmed - implying that decisions about the nature of science and the sociology of science are political decisions.

The emphasis in the weak program on taking a critical position regarding the privileged status of science reflects the political (and,

Sociological Theory 1983 299

more generally, sociological) awareness that science refers to a world

view, a mode of organization, and a system of power; science is not a disembodied set of logics, methods, theories, and facts. This is the rationale for the Rashomon theorem (based on the famous Japanese tale of the many truths about an event) formulated by Zenzen and Restivo (1982): "There are numerous, if not an infinity of, ways of describing and interpreting any given phenomenon; the status of any given account is not determined by whether it is 'true' or 'real' in some absolute sense, but by how useful it is in the competitive realm of knowledge production and utilization."

The weak program is based on the assumption that democratic values and organizations are necessary conditions for the development of epistemic strategies that can lead to critical understanding of our individual and collective experiences and to progressive (or better, perhaps, unfettered and continuous) inquiry. It is characterized by a Spenglerian capacity for conceptualizing an alternative science, an alternative to science, and thus represents a radical departure from Merton and Kuhn

Toward a New Notion of Inquiry

When contemporary sociologists reflect on the foundations of their discipline, they tend to draw uncritically on orthodox philosophies, histories, and sociologies of science. This tendency applies as much to those sociologists who are wary of scientism or opposed to the idea of a social science as it does to those intent on establishing sociology as a science. Some sociologists have sought to update their reflexive sociologies by uncritically adopting a Kuhnian sociology of science. I have argued that the Kuhnian revolution is a myth. The more important point is that in the last decade or so, the field of science studies has become more diversified, more deeply rooted in the experiences of working scientists, and more concerned with the comparative and

cross-cultural study of knowledge, belief systems, and values. These developments have not simply provided new choices for reflective sociologists; they have also made it imperative for sociologists of sociology and sociologists in general to become more critical students of knowledge and science.

Sociological Theory 1983 300

Marxist and conflict sociologies of science, laboratory life studies, and the conflicts among the strong, moderate, and weak programs promise to carry us beyond orthodox and revisionist Mertonian-Kuhnian paradigms toward critical sociological theories of inquiry. There is a major problem confronting alternatives to sociologies of science that are ideologies of science-as-it-is: the problem of self-exemplification. The resolution of this problem-how to study science *critically* without accepting a priori that science is the paradigmatic mode of inquiry-promises to revolutionize science as we know it and to reshape our conception of inquiry.

Notes

1. The myth of the Kuhnian revolution in the sociology of science and in science studies generally is expressed, for example, in Sklair (1973, p. 69); Blume (1974, p. 7); Barnes (1972a, p. 11); Dolby (1972, p. 315); Weingart (1974, p. 45); Whitley (1974, p. 2); and Suppe (1977, p. 127 11.). On the alleged convergence of Kuhn and Marx see Sklair (1973, p. 150) and Harrington (1976, pp. 18-19). It should be noted that as early as 1971, King (1971, p. 30) pointed out convergences between Merton and Kuhn; see also Bourdieu's (1975) critical remarks on Kuhn.
2. See, for example, Barnes (1972a, p. 11; 1972b, p. 277; 1977, p. 23); MacKenzie and Barnes (1979, p. 207); Barnes (1982). Pinch (1981)

is, in a sense, a simultaneous discoverer of my interpretation of Kuhn; but I do not agree with him that Kuhn can be given a radical interpretation without destroying what is central to the Kuhnian paradigm.

3. On Kuhn's chameleon reception, see Merton (1973, p. 554).

For an example of how Kuhn is called on to support revisions of the customary view in the sociology of science, see Mulkay (1979; 1972, p. 134). It should be noted that Kuhn's ideas have not been adopted wholesale and uncritically outside of North America (see Mulkay, 1979, pp. 39, 48; Elias, 1974, p. 35).

4. The anomaly is self-conscious in Sklair (1973, p. 267), who disembodies the Kuhnian approach from what Kuhn actually writes.

5. Hesse (1980, p. 32) is one of the few students of science who recognizes that Kuhn has not explicitly encouraged the sociological study of science; indeed, she argues that if anything Kuhn has discouraged it.

Sociological Theory 1983 301

6. See, for example, the seminal paper by Hessen (1931); for an example of recent Marxist scholarship see Dickson (1979). Marxist studies in mathematics include Struik (1967), Needham (1956). and Restivo (1981a).

7. Perhaps the most successful application of dialectical thought to the study of mathematics is in Suzanne Bachelard's (1958) study of mathematical physics, by far the best study of science in the phenomenological tradition that I know of.

8. For an extended discussion of the strong, moderate, and weak programs, see Restivo (1981b) and Chubin and Restivo (1982).

References

B., MIKE

1978 "Dialectics in Mathematics." *Proletariat* 4 (Winter):33-37.

BACHELARD, SUZANNE

1958 *La Conscience de la Rationalité*. Paris: Presses Universitaires de France.

BARNES, BARRY

1972a "Introduction." In B. Barnes (Ed.), *Sociology of Science*.

Baltimore: Penguin Books.

1972b "On the Reception of Scientific Beliefs." In B. Barnes

(Ed.), *Sociology of Science*. Baltimore: Penguin Books.

1977 *Interests and the Growth of Knowledge*. London: Routledge & Kegan Paul.

1982 *T. S. Kuhn and Social Science*. London: Macmillan.

BEAVER, D.

1979 "Possible Relationships Between the History and Sociology

of Science." In J. Gaston (Ed.), *The Sociology of*

Science. San Francisco: Jossey-Bass.

BEN-DAVID, JOSEPH

1979 "The Emergence of National Traditions in the Sociology

of Science: The United States and Great Britain." In J.

Gaston (Ed.). *The Sociology of Science*. San Francisco:

Jossey-Bass.

BLOOR, DAVID

1976 *Knowledge and Social Imagery*. London: Routledge &

Kegan Paul.

Sociological Theory 1983 302

BLUME, STUART

1974 *Toward a Political Sociology of Science*. New York: Free Press.

BOHM, DAVID

1973 "Quantum Theory as an Indication of a New Order in Physics. Part B: Implicate and Explicate Order in Physical Law." *Foundations of Physics* 3(2): 139- 168.

BOURDIEU, PIERRE

1975 "The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason." *Social Science information* 14(6): 19-47.

CAMPBELL, DONALD T.

1974 "Evolutionary Epistemology." In P. A. Schilpp (Ed.), *The Philosophy of Karl Popper*. Vol. 1. LaSalle. 111.: Open Court.

CHUBIN, DARRYL

1981 "Constructing and Reconstructing Scientific Reality: A Meta-Analysis." *International Society for the Sociology of Knowledge Newsletter* 7(May):22-28.

CHUBIN, DARRYL, AND RESTIVO, SAL

1982 "The Mooting of Science Studies." In Michael Mulkey and Karin Knorr-Cetina (Eds.), *Science Observed: Analytical Perspectives on Social Studies of Science*. London: Sage:

COLLINS, RANDALL

1975 *Conflict Sociology*. New York: Academic Press.

COLLINS, RANDALL, AND RESTIVO, SAL

1983 "Robber Barons and Politicians in Mathematics: A Conflict Model of Science." *Canadian Journal of Sociology* 8, 2 (Spring), pp. 199-227.

DICKSON, D.

1979 "Science and Political Hegemony in the 17th Century." *Radical Science Journal* 8:7-37.

DOLBY, R.C.A.

1972 "The Sociology of Knowledge in Natural Science." In Barry Barnes (Ed.), *Sociology of Science*. Baltimore: Penguin Books.

ELIAS, NORBERT

1974 "The Sciences: Towards a Theory." In R. Whitley (Ed.),

Sociological Theory 1983 303

Social Processes of Scientific Development. London: Routledge & Kegan Paul.

FEYERABEND, PAUL.

1978 *Science in a Free Society*. London: New Left Books.

GASTON, JERRY

1979 "Different Approaches." In J. Gaston (Ed.), *The Sociology of Science*. San Francisco: Jossey-Bass

HARKINGTON, M.

1976 *The Twilight of Capitalism*. New York: Simon & Schuster.

HESSE, M.

1980 *Revolutions and Reconstructions in the Philosophy of Science*. Bloomington: Indiana University Press.

HESSEN, BORIS

1931 "The Social and Economic Roots of Newton's 'Principia'" In N. Bukharin and others (Eds.), *Science at the Crossroads*. London: F. Cass.

KING, M. D.

1971 "Reason, Tradition, and the Progressiveness of Science." *History and Theory* 103-32.

KNORR, KARIN

- 1981 "The Ethnography of Laboratory Life: Empirical Results
And Theoretical Challenges." *International Society for the Sociology of Knowledge
Newsletter* 7 (May):.1-9.
- KUHN , THOMAS S.
1962 *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.
1970 *The Structure of Scientific Revolutions*, 2nd Ed. Chicago: University of Chicago Press.
1978 *Black-Body Theory and the Quantum Discontinuity 1904-1912*. New York: Oxford University Press.
- MACKENZIE, D., AND BARNES, B.
1979 "Scientific Judgment: The Biometry-Mendelism Controversy." Pp. 191-210 in Barry
Barnes and Steven Shapin (eds.), *Natural Order*. London: Sage.
- MANNHEIM, KARL
1936 *Ideology and Utopia*. New York: Harcourt Brace Jovanovich..

Sociological Theory 1983 304

- MARX, KARL
1974 *Mathematische Manuskripte*. (Edited by W. Endemann.)
Kronberg Taunus, East Germany: Scriptor Verlag.
- MERTON, ROBERT K.
1973 *The Sociology of Science*. Chicago: University of Chicago
Press.
1977 "The Sociology of Science: An Episodic Memoir." Pp. 3-141 in
Robert K. Merton and Jerry Gaston (Eds.), *The Sociology
of Science in Europe*. Carbondale: Southern Illinois University
Press.
- MULKAY, MICHAEL
1972 "Cultural Growth in Science." Pp. 126142 in Barry Barnes (Ed.),
Sociology of Science. Baltimore: Penguin Books.
1979 *Science and the Sociology of Knowledge*. Winchester,
Mass.: Allen & Unwin.
- NEEDHAM, JOSEPH

1956 "Mathematics and Science in China and the West."

Science and Society 20:320-343.

PINCH, TREVOR

1981 "Kuhn-The Conservative and Radical Interpretations."

Paper presented at the sixth annual meeting of the Society for Social Studies of Science, Atlanta, GA.

REINCOLD, N.

1980 "Through Paradigm-Land to a Normal History of Science." *Social Studies of Science* IO (November): 475-496.

RESTIVO, SAL

1975 "Towards a Sociology of Objectivity." *Sociological Analysis and Theory* 5(June):155-183.

1980 "Multiple Realities, Scientific Objectivity, and the Sociology of Knowledge." *Reflections* I (Summer): 61-76.

1981a "A Marxist Sociology of Mathematics in Ancient Greece and Pre-Modern Europe." Unpublished manuscript. Note 10/2006: eventually published as Section VIII, "Sociological Materialism and History of Mathematics: An Exploratory Case Study," in my *The Social Relations of Physics, Mysticism, and Mathematics* (1983). D. Reidel Publishing Co., Dordrecht, Holland.

1981b "Perspectives in Contemporary Sociology of Science."

Science, Technology, and Human Values 6 (Spring): 22-30.

RESTIVO, SAL AND ZENZEN, MIKE

1978 "A Humanistic Perspective on Science and Society."

Humanity and Society 2(November):211-236.

Sociological Theory 1983 305

SKLAIR, LESLIE

1973 *Organized Knowledge*. St. Albans, Herts: Paladin.

SOHN-RETHEL, ALFRED

1978 *Intellectual and Manual Labor*. London: Macmillan.

SPENCLER, OSWALD

1926 *The Decline of the West*. 2 vols. New York: International Publishers.

STRUIK, DIRK

1967 *A Concise History of Mathematics*. New York: Dover.

SUPPE, F.

1977 "The Search for Philosophic Understanding of Scientific Theories." In F. Suppe (Ed.), *The Structure of Scientific*

Theories. (2nd ed.) Urbana: University of Illinois Press.

WEINCART, P.

1974 "On a Sociological Theory of Scientific Change." Pp. 45-68 in

R. Whitley (Ed.), *Social Processes of Scientific Development*.

London: Routledge & Kegan Paul.

WHITLEY, R.

1974 "Introduction." Pp. 1-12 in R. Whitley (Ed.), *Social Processes of/*

Scientific Development. London: Routledge & Kegan

Paul.

WOOLCAR, STEVE

1981 "Science and Ethnomethodology: A Prefatory Statement."

International Society for the Sociology of Knowledge

Newsletter 7(May): 10- 15.

ZENZEN, MIKE, AND RESTIVO, SAL

1982 "The Mysterious Morphology of Immiscible Liquids: A Study of Scientific Practice." *Social Science Information*. 21, 3, pp. 447-473

Appendix 2006

Tom Kuhn received the Bernal Award at the 11th annual meeting of the Society for Social Studies of Science (4S) in 1986. He seemed to be slightly embarrassed (or nonplussed?) by the award, uncertain that he was an appropriate recipient.

His acceptance speech was, like the book that had made him an icon for the new histories, sociologies, and philosophies of science that began to crystallize in the late 1960s and early 1970s, an homage to his teacher Alexandre Koyré. The point of this homage was to emphasize that *The Structure* was in conception and execution a contribution to internalist history of science. Koyré was a formidable internalist, but perhaps one can catch a glimpse of the cautious rebelliousness one occasionally saw in Kuhn in Koyré's critical views on the relationship between experiments and scientific truth. He questioned whether certain experiments in the history of science had in fact been carried out and claimed that experiments proved complicated sets of underlying premises rather than truths per se. The point here is that Kuhn deliberately and passionately divorced himself from any sociological objectives or achievements, heralding his later criticisms of the social studies of science and social constructionism. It was an eye-opening performance for those fortunate to be in the room that evening, and visibly upset then 4S president Bernard Barber. Barber felt that Kuhn had gone too far in distancing himself from the perspectives grounding the society's decision to present him with its most prestigious award. Ironically, when Barber was awarded the Bernal Award in 1994, he too stepped back from what he considered views of science that were too sociologically radical and undermined the notions of objectivity and rationality. As a direct consequence of Barber's intervention, the published version of Kuhn's acceptance speech does not capture the full power and content of the oral presentation. Given the fact that Kuhn helped to bring Ludwik Fleck's *Genesis and Development of a Scientific Fact 1979/1935* (Chicago: University of Chicago Press) to attention of the new generation of science studies researchers (and independently of how and to what extent Fleck influenced Kuhn), it is important to note Fleck is more profoundly and radically sociological than Kuhn.