

VI. THE HISTORY OF SCIENCE AND PROGRESS IN POLITICAL SCIENCE: AssignmentsNovember 1:

Kuhn, The Structure of Scientific Revolutions. Chapters I through XII.

Comments: Kuhn's book is of major significance in the philosophy of science, and perhaps some of you have read it already. Kuhn argues that the accepted view of science as an open, inquiring search for knowledge in a series of incremental and linear progressions simply does not square with the real practice of science. Compare Kuhn's position with this passage from Hempel, Philosophy of Natural Science: "Science . . . is not interested in defending certain pet conceptions against all possible adverse evidence. It aims, rather, at a comprehensive body of sound empirical knowledge, represented by a well-confirmed system of empirical statements, and it is accordingly prepared to give up or to modify whatever hypotheses it may have previously accepted." (p. 40)

November 6:

Kuhn, The Structure of Scientific Revolutions, Chapters ~~X~~^{XIII} to Postscript.
Bernstein, The Restructuring of Social and Political Theory, 57-114.
Part II, "Language, Analysis, and Theory"

Comments: Bernstein launches his critique of positivism in mainstream social science by questioning the prevailing view of the history of science reflected by Hempel and other committed positivists. He pays particular attention to the turmoil stirred up by Kuhn's work. Understand Bernstein's evaluation of Kuhn's book for the social sciences.

November 8:

Baum, William C. et al. "American Political Science Before the Mirror: What Our Journals Reveal about the Profession," Journal of Politics 38 (November 1976): 895-915. (On Reserve)
Ball, Terrence. "From Paradigms to Research Programs: Toward a Post-Kuhnian Political Science," American Journal of Political Science 20 (February 1976): 151-177. (On Reserve)

Comments: Kuhn contends that the literature of a field in some respects its development as a science. For example, he holds that textbook literature has a greater significance in history and the social sciences than in the natural sciences. Why should that be so, and what does Baum's analysis reveal about the scientific status of political science? Of course Kuhn is not the only historian of science whose work has had impact on the social sciences, as Ball makes clear in his analysis of the Lakatos "alternative" to Kuhn, which was introduced in the reading earlier in Moon's article. As Ball describes it, what is the difference between dogmatic falsification, methodological falsification, and sophisticated methodological falsification?

VI. THE HISTORY OF SCIENCE AND PROGRESS IN POLITICAL SCIENCE: Other Readings

Two types of readings are noted here: those on the natural and "unnatural" sciences. You will also find that the selections vary according to their scholarly and popular character.

Hall, A.R. The Scientific Revolution 1500-1800: The Formation of the Modern Scientific Attitude. Boston: The Beacon Press, 1954.

This is a standard historical treatment of the development of the physical sciences. It contains some fascinating insights, such as the pervasive grip of ancient Greek science upon the activities and thought of scholars more than a thousand years later. For example, Galen, the Greek medical scientist, was not only the principal authority in anatomy, physiology, and therapy until the 17th century, but men risked their reputation and even imprisonment for disagreeing with his writings. "Admiration for Galen was so extravagant that anatomists were more apt to attribute their failure to confirm his descriptions to their own want of skill, than to his." (p. 38)

Bronowski, Jacob. The Ascent of Man. Boston: Little, Brown, 1973.

This book issued from the extraordinary television series by the same name. Bronowski had the capacity to enthrall one with his narrations of scientific development. The book lacks the impact of the program, but it is interesting reading nonetheless.

Watson, James D. The Double Helix. New York: Signet Books, 1969.

Watson and Crick won the Nobel Prize for their work in determining the structure of the DNA molecule. Watson describes that story in this absorbing little book about the personalities involved in the race for the solution. You will see much of Kuhn's view of science in this account.

Walter, Oliver. Political Scientists at Work. Belmont, California: Duxbury, 1971.

There are no success stories about Nobel Prize Winners in this book, but there are some informative accounts of the trials and tribulations of research. Donald Matthews talks about his career path, Leroy Rieselbach explains how his dissertation originated and grew into a book, Harold Guetzkow provides an autobiographical essay about a decade of research with inter-nation simulation and so on. Again, the message is that research is often quite messy and ends up looking quite different than originally imagined.

Golden, Patricia M. (ed.). The Research Experience. Itasca, Illinois: Peacock, 1976.

At 500 pages, this book is more than twice the size of Political Scientists at Work, and it pertains primarily to sociologists at work. It features reprinting the article which reported the original research and then following it with "A Personal Journal" account of what it was like to do the research. Another "inside story" book, it is highly informative.

VI. THE HISTORY OF SCIENCE AND PROGRESS IN POLITICAL SCIENCE: Other Readings (cont)

Bennett, W. Lance. "The Growth of Knowledge in Mass Belief Studies: An Epistemological Critique," AMERICAN JOURNAL OF POLITICAL SCIENCE, 21 (August, 1977), 465-500.

Findings of the AMERICAN VOTER and studies by Converse, among others, established some years ago that the mass public exhibited little constraint or corresponding stability in its political beliefs. But within the past ten years, a number of anomalies have begun to arise about these findings. A strain has been placed on the community of scholars, and some communications pathologies have begun to emerge. Evidence of disarray can be seen in the growth in writings that are critical of paradigmatic understandings. The absence of clear criteria for conducting these disputes has led to fundamental redefinition of some of the basic concepts and findings in the field. But the debate has not been conducted in a way that allows for mutual comparison of positions or findings. We need to acquire an understanding of the sources of problems of knowledge in the field and the formulation of a self-conscious epistemological strategy to correct these problems.

In conceptualizing mass belief studies, we need to work more closely to the theory that is being presented, and criticisms of concepts that are not theory-constrained should examine the unspecified assumptions in the concept. Perhaps there should be a moratorium of research using narrowly defined concepts until the development of more systematic general theory.

At the measurement level, there should be more careful experimental work to resolve the measurement criticisms. For example, there should be concentrated research on the difference between "symbolic" and "concrete" versions of issue items being presented to respondents, with symbolic items "setting up" the item for an unfavorable or favorable response by the respondent as opposed to the non-symbolic items.

Concerning the analytical methods used, this is probably the least source of difficulty, for methods of data analysis do not operate directly at the interface between reality and our scientific reconstructions of it, as concepts and measures do. Still, there may be a need for a "return to basics" and more displays of raw data.

Problems of knowledge in the field of mass belief studies tend to diminish consensus on grounds for accepting and rejecting competing inferences from the same data.

(This article provides a very good review of the literature on the controversy surrounding the mass belief studies. There are some 70 citations to those studies and to works on the philosophy and history of science, especially involving the Kuhnian framework. Although the article does not refer specifically to a "crisis" in mass belief studies, that is the thrust of the argument. It backs off, however, from using the Kuhn model too literally for this debate within the social sciences.)

Lakatos, Imre and Alan Musgrave (eds.), CRITICISM AND THE GROWTH OF KNOWLEDGE. Cambridge: Cambridge University Press, 1970.

This collection of essays arose from a 1965 symposium about Kuhn's views of science. It includes an introductory essay by Kuhn and discussion papers by Watkins, Toulmin, Williams, Popper, Masterman, Lakatos, and Feyerabend. Kuhn concludes with some reflections on his critics. This volume is very important in the discussion of Kuhn's contributions and should be read by the serious student of the subject.

Stephens, Jerome. "The Kuhnian Paradigm and Political Inquiry: An Appraisal," AMERICAN JOURNAL OF POLITICAL SCIENCE, 17 (August, 1973), 467-488.

This appraisal is critical of Kuhn, arguing that it contributes little to the study of politics. Most importantly, Stephens warns that we must not take Kuhn seriously when he states that scientists apply values to judge their research not rules. The article is a good review of the early impact of Kuhn on political science.