

Introduction

By Henry E. Brady

“Two Paths to a Science of Politics” presents two strongly argued points of view about where we should be going in our discipline. The authors by and large agree on the nature of science and the variety of ways to do it, but they disagree on the most fruitful way to proceed at this moment. Granato and Scioli believe that we need to do more to test theories with data and to formulate theories that are based on data. We need, in their phrase, to consider the empirical implications of theoretical models (EITM). Rogers Smith thinks we need to take history, context, and meaning more seriously in order to understand phenomena such as political identity.

To get some perspective on these two approaches to political science, it is useful to think about the nature of scientific argument, and it is very useful to move beyond using physics (especially physics as reconstructed by the logical positivists) as the model for science. Biology, I believe, provides a better model for social science, partly for the very reasons that logical positivists and their allies had difficulty accepting it as a science. Once we develop a more subtle conception of science that avoids the snare of trying to imitate physics, it turns out that linking theories to data almost inevitably leads to more concern with history, context, and meaning. So, although these authors take different routes, their two paths may lead in the same direction, if not to the same place.

What is Science?

When Charles Darwin wrote *The Origin of Species* in 1859, scientific explanations for the diversity of life competed, often unsuccessfully, with creationist views about the immutability of species and the presumed inability of science to explain organic phenomena. In his book, Darwin made a powerful case for how descent from common ancestors coupled with slow but continuous modifications from natural selection could

account for the vast array of species in the fossil record and on the earth today. In this way, as well as bringing science into the study of natural history, he brought history back into science. Although he referred to evolution just once in *The Origin of Species*, and only as the last word in his last sentence where he concluded that “from so simple a beginning endless forms most beautiful and most wonderful have been, and are being, evolved,”¹ Darwin’s theory of evolution replaced stasis and immutability with history, context, and change. Darwin called his book “one long argument,”² and it is perhaps, along with Galileo’s *Dialogue Concerning the Two Chief World Systems*, the best scientific argument ever put forth—more so because it showed how science could attack problems that were thought beyond its ken. Good social science, it seems to me, should look more like the arguments in these books than the stilted and overly mannered explication of science foisted upon us by the logical positivists and by Milton Friedman in his essay on “The Methodology of Positive Economics.”³

Although the logical positivists usefully made us think more carefully about the nature of theories, laws, explanations, verification, and observation, they also developed a reconstruction of science that was overly confining.⁴ The heart of their approach was a schema for the axiomatization of scientific theories leading to universal covering laws that are true, have empirical content, and are not accidental. Thus, a “law,” such as the clearly flawed (but empirically true) assertion that “men who take birth control pills will not get pregnant” must be rejected because it is only accidentally true. One way to show that it is an accidental generalization and not a law is to demonstrate that its counterfactual—“if some men did not take birth control pills they would get pregnant”—is false. Another tenet of the logical positivists, argued eloquently and forcefully by Milton Friedman, was that the test of a law is its ability to predict events.

The emphasis upon empirical content, truth, and counterfactuals is clearly useful. But thinking of theories as axiom systems leading to universal covering laws and the emphasis on prediction as the test of a theory was very limiting. In addition, as pointed out by Thomas Kuhn and others, the logical positivists said very little about the formation of concepts and the development of new scientific theories.⁵ Perhaps the biggest failure of the logical positivists was that their reconstruction did not seem to have much to do with science as it was actually practiced.

What is Darwin’s idea of science? Science requires a number of interrelated steps. It requires clarifying concepts and offering

Henry E. Brady is the Class of 1941 Monroe Deutsch Professor of Political Science and Public Policy, and Director of the Survey Research Center and UC DATA at the University of California, Berkeley (hbrady@csm.berkeley.edu). His co-edited book with David Collier, *Rethinking Social Inquiry: Diverse Tools, Shared Standards*, will be published this year from Rowman and Littlefield and Berkeley Public Policy Press. His thanks to David Collier, Gary King, Paul Sniderman, and the editorial staff of *Perspectives on Politics* for their helpful comments.

causal mechanisms that may be probabilistic as well as deterministic.⁶ To develop a coherent theory of the origin of species, Darwin clarified concepts such as variation, species, and variety; he also described a causal mechanism—the struggle for existence leading to the natural selection of the more fit varieties of a species—that could, over very long periods of time, lead to the diversity and complexity found in nature. Unlike the deterministic physical theories of Isaac Newton and Pierre Laplace, Darwin proposed a probabilistic causal process in which the generation of hereditary variants and the fate of any particular organism is a matter of chance, although the ultimate fate of the variant is determined by its overall fitness.⁷

Science also requires considering alternative hypotheses such as creationism. Time and again in *The Origin of Species*, Darwin asked whether the evidence better accords “with the common view of the immutability of species, or with that of their slow and gradual modification, through descent and natural selection.”⁸ In addition, science demands testing theories by logical argument *and* empirical evidence wherever possible. Darwin showed no sign of believing that one particular method of testing is superior to another. To elaborate and test his theory, he used detailed cases studies on the breeding of pigeons, on fossils found in geological strata, and on the natural history and ecology of such isolated areas as the Galapagos Islands. He also employed simple mathematical calculations akin to formal models to demonstrate Thomas Malthus’s principle that the geometric increase of any population would soon outstrip its food supply unless something kept the population in check. He produced quantitative data on the relationship between the geographic range of a species and the number of varieties, finding, as he predicted, more varieties when a species covered a wider geographic area. He described experimental data on breeding programs, and he did simple experiments himself. Perhaps most importantly, Darwin brought history into the study of the similarity and the classification of organisms.

I have long thought that Darwin’s scientific method is much closer to what political scientists should embrace than the physical science model that formed much of the basis for the logical positivists’ reconstruction of the scientific method, as exemplified in the works of Carl Hempel, Ernest Nagel, and Abraham Kaplan.⁹ Rather than look for universal covering laws¹⁰ that are true in all times and all places, political scientists should be cognizant of history and context, and they should cultivate the systematic study of particular places and times. They must also take chance and contingency seriously. And they must employ whatever method they can muster to check their theories against reality.

Evolutionary theory takes history, context, and contingency seriously. It takes them so seriously that some philosophers have worried that the theory’s reliance upon probabilistic mechanisms unduly limits its ability to make predictions—which are considered by some to be the hallmark of real science. Even more damningly, in this view, evolutionary theory’s dependence upon history and context means that it is not scientific by the standards of the hypothetico-deductive (H-D) approach of Hempel and Nagel, which requires uni-

versal covering laws. The probabilistic nature of evolutionary theory requires, at the very least, that the Laplacean ideal of exact theoretical predictions—which is embedded in the “deductive-nomological” theory of Hempel and Peter Oppenheim¹¹ that was the first complete version of the H-D approach—be abandoned in favor of statistical tests of evolutionary theory as proposed in the revised inductive-statistical (I-S) theory of Hempel.¹²

Although moving from a deterministic to a statistical formulation of theories is a big step, the importance of history and context in evolutionary theory requires an even bigger step if evolution is to be considered a scientific theory. Several decades ago, many philosophers refused to take this step, and they argued that evolution is not a scientific theory. For the philosopher J.J. Smart “there are no laws in the strict sense”¹³ in biology because “the theory of evolution and ecology are not, in the logician’s sense, typically ‘scientific’ in nature. They are quite obviously ‘historical’ subjects. They are concerned with a particular and very important strand of terrestrial history.”¹⁴ Karl Popper agreed with Smart.¹⁵ Although other philosophers have tried to rescue evolutionary theory by proposing that it is an “immature” H-D approach, these attempts—such as treating the Hardy-Weinberg equation for the frequencies of genotypes after selection as “laws of nature”—seem tortured, and the real problem seems to lie with the requirements of the H-D approach itself. After all, it seems presumptuous and just plain wrong for philosophers to claim that evolutionary theory is not scientific.

Philosophers themselves have recognized the folly of this position, and today none would claim that evolutionary theory is not a science. As one alternative, philosophers such as Bas van Fraassen, Patrick Suppes, and Frederick Suppe have gone beyond the logical positivist’s approach and put forth a “semantic” conception of theories as ideal systems, models, or concepts which tell us something about reality but which are more akin to tools for probing reality than to laws such as $F = ma$ or $E = mc^2$.¹⁶ These tools propose possible structures for phenomena such as suggesting that gases are composed of large number of swiftly moving molecules or that light propagates like the waves on a pond. These structures make it possible to ask questions about phenomena and to provide some explanation of them—but they will inevitably have limits such as when molecules in gases are slowed to the point where quantum-mechanical effects take over or when light waves act like particles (quanta) to produce electricity in discrete amounts in the photo-electric effect.

Elisabeth Lloyd and other philosophers who embrace a semantic perspective happily concede that there are no universal, physically necessary laws in modern evolutionary theory, but they argue that the theory is scientific because there are concepts such as variation within a species and models such as natural selection which guide biologists’ thinking about the development of species.¹⁷ Moreover, within genetics and ecology there is a profusion of concepts and models which can be used to explore various aspects of evolution. These concepts and models are devices for understanding reality. Sometimes they are merely abstractions or simplifications or even “fictions” used as conceptual

devices for illuminating what might or might not be true. Sometimes they are predictive models. But unlike the logical positivist and related perspectives¹⁸ that view prediction or falsification as the crucial activity in science, the semantic perspective allows for the possibility that even very approximate theories may tell us something and that theories can be confirmed or disconfirmed through strategies that go beyond their predictive power. These confirmation strategies include the fit between the model and data, support for various assumptions embedded in the model, or the wide range of evidence that accords with the model. These are precisely the strategies used by Darwin in *The Origin of Species*. The semantic approach suggests that science is about creating a workshop of tools, all constantly being used and redesigned to fashion an understanding of reality. There is no master tool, but there is constant attention to improving the relationship between the tools and the projects at hand. The result is a growing capacity to use the tools to explain the world and to shape it to our purposes.

This perspective turns us away from seeking universal covering laws and towards finding useful models for understanding the world. In political science, Mancur Olson's theory of group membership, which introduced the "free rider problem," might be considered a powerful fiction that raised questions about group formation.¹⁹ James C. Scott's notions of the differences between the public performances and the hidden transcripts of those in subordinate positions raise questions about how power is experienced and deflected and how visible evidence should be understood and interpreted.²⁰ Gary Cox's extension of Duverger's law on how the number of parties varies with the form of the electoral system provides some mechanisms, namely strategic voting and strategic entry by politicians, to explain the law's assertion that there are typically no more than two parties in single-member plurality systems.²¹ These models are used in different ways, but they do not claim to produce universal laws. Their results can be subject to historical contingency, and they can even contradict one another because they have different purposes—as in models of the motion of fluids which assume a continuous medium versus models of the chemical behavior of fluids which assume individual molecules.

Should Political Science be Scientific?

The essays by James Granato and Frank Scioli and by Rogers Smith suggest that Darwin, and the biological sciences in general, do, indeed, provide an excellent template for political science. Both essays agree that we should be committed to what Gary King, Robert Keohane, and Sidney Verba have called a unified logic of scientific inference.²² Smith says that:

If we are to judge, for example, to what conceptions of their identities and interests particular political actors are giving priority, we need to form some hypotheses based on what we think we know about those actors. Then, we define the different implications of alternative hypotheses. Finally, we look for observable data about their lives that we can use to falsify some of the hypotheses. That logic is constant, though the techniques of falsification will vary with the types of problems particular data present and with the tools currently at our disposal.

Granato and Scioli make a similar point:

In an ideal world, where there is unification in approach, political science research should have the following components: (1) theory (informed by case study, field work, or a "puzzle"); (2) a model identifying causal linkages; (3) deductions and hypotheses; (4) measurement and research design; and, (5) data collection and analysis.

Granato and Scioli go on to argue that "the three most common current research practices—formal modeling, case study analysis, and applied statistical modeling—deviate from this ideal. They therefore limit the possibilities for substantial enhancement of knowledge." They argue that formal models sometimes fail to respect the facts; that case studies sometimes theorize by proverb; and that statistical analysis sometimes amounts to data mining, garbage can regressions, and statistical patches. They believe that the result is a series of unreliable findings and the "proliferation of non-cumulative studies" that do not provide useful knowledge, especially knowledge that can be used by policymakers. Their solution is to connect formal and empirical analyses so that concepts are clarified and causal linkages specified. That is the goal of a new National Science Foundation program called Empirical Implications of Theoretical Models which will "encourage political scientists to build formal models that are connected to an empirical test" and "lay the groundwork for (social) scientific cumulation." In short, Granato and Scioli want more research to accomplish what Darwin did in *The Origin of Species*.

What Special Problems Does Political Science Face?

In principle, I suspect that Rogers Smith does not disagree with what Granato and Scioli want to do, but in practice, he probably worries that it will lead to an undue emphasis upon universal covering laws. "We cannot," he says, "fully understand the sources and character of changes in political identities by looking only at timeless behavioral constants." Much of his article, in fact, is devoted to two examples, both involving the study of political identities, where he feels that formal theory and a-historical behavioralism have grasped for and failed to produce "timeless behavioral constants." For Smith, "[I]f political identities are socially, indeed politically, constructed, we need to attend to the historical and contextual processes through which identities are constructed differently among different groups in different times and places."

To get at these processes, he recommends "richly interpretive methods that involve discursively grasping the consciousnesses and senses of value and meaning that identities give to people." For Smith:

We are likely to gain more insight on these topics through interpretive textual analyses; ethnographic fieldwork; biographical studies; in-depth interviews; individual and comparative case studies, both historical and contemporary; participant observation research; narrative historical institutional analyses; and other methods rather crudely termed "qualitative."

Smith brings up four distinct issues in these comments. First, he questions the wisdom of searching for universal covering

laws producing “timeless behavioral constants.” Second, he suggests that we must attend to historical and contextual processes, especially political processes. Third, he notes that we must take meaning and values seriously. Finally, he proposes that the way to do this is through “contextual immersion, psychological empathy, and creative imagination.” Each issue requires some discussion.

I have already suggested that Smith is right in thinking that science does not require universal covering laws. Just as evolutionary theory takes history and context seriously, so must social science. Smith presents two examples of research that he believes rely too much on trying to identify “abstract, ahistorical, and enduring regularities in political behavior.” David Laitin’s *Identity in Formation* uses a rational choice “tipping model” to explain the conditions under which Russian speakers in those states that were formerly part of the Soviet Union will decide to learn the non-Russian language of the dominant group (the “titular nationality”) in those states. Henry Brady and Cynthia Kaplan’s “Categorically Wrong” uses psychological models and data from Estonia to explain when nominal measures of ethnic identity will suffice for predicting attitudes and when gradations of ethnic identity will be necessary to explain variations in attitudes.

Smith argues that Laitin’s tipping model explains little compared to measures of social distance, and these measures are inherently the result of “the particularities of the different languages and religions in question” and the degree to which “they resemble each other in decisive respects.” But social distance, says Smith, is the result of “different historically shaped patterns of political institutions and state policies.” Similarly, Brady and Kaplan argue that nominal measures of identity suffice when an identity is highly salient, whereas graded measures (based upon social distance) will be necessary when an identity is less salient, but “they do not go on to explore the logical next question: what determines whether ethnic identity is salient to certain groups?” Yet, Smith notes, it seems likely that salience is the result of political factors, some of which are even described by Brady and Kaplan in their discussion of Estonia’s history and institutions.

As the co-author of one of these two works, my inclination is to tread somewhat lightly here. But Smith is certainly right in saying that historical factors matter in determining social distance and salience, just as historical factors matter in determining the genetic constitution of an organism or the environment that it faces. But he seems to glide too quickly over the contributions of these two pieces of research—namely, the identification of the major factors, social distance and the salience of identity, that explain why Russians choose to learn the titular language or why members of ethnic groups can be treated categorically in some circumstances. The identification of these factors makes it clear what history must explain. This, it seems to me, is no small feat. To put it in perspective, Darwin’s achievement was not just to say that history mattered, it was to say that history had to explain how natural selection and survival of the fittest led to the evolution of an organism. Furthermore, beyond identifying what history must explain, the trick is to say how it matters. Smith seems to recognize

this, and he ends with his own proto-theory of the crafting of political identity through coercive force and persuasive stories. He also suggests that we need “to study processes of political identity formation largely through interpretive, ethnographic, and historical methods.” I certainly agree that we need to study these processes through a variety of methods, but I wonder why Smith limits himself to these particular ones. Why not quantitative and survey methods as well? And why does Smith do so little to explain the comparative advantages of the methods he favors over those he does not mention?

Perhaps the advantage of the methods he mentions is that at least some of them, such as interpretive methods, take meaning and values seriously. Certainly two of the most important things about human society are that we use language and we construct cultures. Meaning is very important to human beings. Social science theories that ignore meaning, such as the stimulus-response behavioralism of the psychologists B.F. Skinner and Clark Hull or the revealed preference utility theory of the economists Paul Samuelson and John Hicks, seem crippled from the start. Yet interpretive methods are by no means the only techniques that take meaning and values seriously. Survey researchers have puzzled over the meaning of survey questions; conversed with people to get their responses; considered their values; and measured their religious, ethnic, political, class, and social identities for decades. In fact, perhaps the best known identity concept in political science is party identification, which was introduced in the early 1950s by Michigan survey researchers. As a survey researcher myself, I’m a bit bemused by Smith’s clarion call for political scientists to take identities seriously.

Smith tells us that “the most crucial work in analyzing political identities must often be done by immersing ourselves in information about the actors in question, and using both empathy and imagination to construct credible accounts of identities and interests.” I have no quarrel with the rhetoric of this assertion, but what does it really mean? Good survey researchers become immersed in the cultures that they study; they use focus groups; they contemplate what questions mean to those who are asked them; they analyze pre-tests; they train interviewers using hundred-page instruction booklets; and they employ experimental variations in question wording to explore people’s identities, attitudes, and values. Empathy and imagination are certainly part of this method, but there is also a great deal of technique that has been built-up over the course of fifty years of survey research. It is a simple fact that the literature on survey research is much more extensive than the literature on interpretive methods. Indeed, the literature on interpretive methods is very thin, and it often eschews technique as detrimental to the method. One reads Clifford Geertz with wonder, but also with questions about his technique. Does he use methods that others might learn? If not, then how can we be sure that his interpretation of a Balinese cockfight is something more than brilliant literary invention? The problem here is not any doubt about the need for interpretation and for taking meaning seriously. Meaning matters—that is why survey researchers ask people what they think on surveys and why they go to great lengths to interpret people’s responses.

The problem is that we know some things about how to teach students to formulate and interpret survey questions, but I am not sure that we know much about how to teach them to do interpretive work. Others have some ideas about how to do this, and I am associated with a growing movement to improve the explication and teaching of qualitative methods. I have also co-edited a book with David Collier on what quantitative methodologists have to learn from qualitative methods (as well as the reverse).²³ But, and I lay this down as a friendly challenge, interpretivists have a ways to go before they can claim a body of methods as well defined and articulated as those available to survey researchers.

Of course, one response to my call for better codification of interpretive methods is to simply reject the notion that interpretive methods should be scientific. This is not Smith's response. He wants theories that that are, "if not universal, at least falsifiable, replicable, and generalizable over some middle range of cases. They should not be simply thickly descriptive case studies or more literary narrative accounts of unique historical events." And I do not think it is the response that any social scientist should offer. Just as modern biology proves that the organic world can be studied scientifically, the lessons of modern psychology, linguistics, and social science are that human behavior can be studied scientifically as well. Let's not give up on the attempt before we have really tried.

Linking Theories with Data and with History, Context, and Meaning

Rogers Smith provides one direction that we could take to improve political science. I think he is right in believing that we should take political identity even more seriously (even though survey researchers have been taking it seriously for a long time), and I agree with his assertion that history, context, and meaning must be part of any serious explanation of social behavior (even though I think that philosophers of science long ago gave up the idea that there should be universal covering laws that ignore them). The EITM style research suggested by Granato and Scioli could, in my view, help move us in this direction for the simple reason that the dialectic they endorse between models and reality will inevitably lead us to the important factors that explain political behavior. Indeed, their four examples of EITM-Type research suggest that this is so. Their first example, Philip Converse's model of adjustments to partisan identification, shows how history explains weak partisan attachment in France compared to America. Their second example, the Phillips curve, shows how people's rational desire to understand the inflation-unemployment trade-off leads to their formation of rational expectations that destroy the relationship. Their third example, quantal response equilibrium, shows how the consideration of people's strategic calculations in international relations lead to markedly different conclusions about the role of military forces. In both of these last two cases, acceptable models of human behavior require assumptions about how people think, interpret, and act strategically. The fourth EITM example shows how public concern, or at least discussion, of a disease affects the Food and

Drug Administration's approval of drugs. In each case, history, context, and meaning play important roles.

The Granato-Scioli article is notable for its eclecticism. It finds virtue in qualitative and quantitative research, case-studies and large-n studies, and formal models formulated verbally or mathematically. I view this as a great strength and in the tradition of Darwin's eclecticism. And, like Darwin, Granato and Scioli want to use these methods in concert to submit theory to empirical tests and to structure reality with theoretical models. This EITM quest leads us back to a fundamental question: What is a theory? Just as the question is not easily answered for biology, it is not easily answered for social science. Granato and Scioli's examples suggest that Markov chains, rational expectations models, quantal response equilibrium, and optimal stopping models are suitable theories. That seems right to me, but what about James C. Scott's theory of domination and the arts of resistance? What about Roger Smith's theory of the crafting of political identity through coercive force and persuasive stories? I called it a "proto-theory" earlier, but mostly because Smith himself calls it a "sketch." It would be useful if Granato and Scioli could tell us what would be required to turn Smith's sketch into a full-blown theory. More generally, we probably need work that identifies a broad range of theoretical exemplars. It would be a great contribution if EITM could produce this kind of guidance.

Will the path suggested by Granato and Scioli and the path suggested by Rogers Smith lead us to exactly the same place? The papers have different emphases. At first blush, Smith is more substantive, with a focus on the study of identity through attention to history, context, and meaning; Granato and Scioli are more methodological, with their focus on linking theories and data. But upon reflection, Smith is also deeply methodological with his concern for finding ways to get at meaning and context, and Granato and Scioli are deeply substantive with their desire to make political science more capable of advising policymakers about political systems and public policy. I view the two papers as complementary. Both endorse a scientific approach that requires theory-building and theory-testing. Both endorse the use of different methods for studying politics. Like Darwin, they believe that good science consists of good arguments constructed from carefully conceived theories that are tested in multiple ways using a variety of methods. Once we embrace this view of shared standards, it seems to me that different starting points and diverse methods are a virtue because they help ensure that we have not missed any of the important terrain, and our shared standards will lead us in the same general direction—towards a better understanding of politics.

Notes

- 1 Darwin 1999 (1859), 400.
- 2 *Ibid.*, 375.
- 3 Friedman 1953, 3–46.
- 4 Classic statements of the logical positivists position are in Hemel 1965 and Nagel 1961. Suppe 1977, 1988 and Salmon 1989 provide extensive summaries of the position and detailed critiques of it.

- 5 Kuhn 1962.
- 6 Sekhon 2004.
- 7 Hodge 1987.
- 8 Darwin 1999 (1859), 254.
- 9 Hempel 1965; Nagel 1961; and Kaplan 1964.
- 10 Hempel and Oppenheim 1948.
- 11 Ibid.
- 12 Hempel 1962.
- 13 Smart 1963, 53.
- 14 Ibid., 59.
- 15 Karl Popper 1979.
- 16 Van Fraassen 1980; Suppes 1967; and Suppe 1977, 1988.
- 17 Lloyd 1988.
- 18 Hempel 1965; Friedman 1953; and Popper 1959.
- 19 Olson 1971.
- 20 Scott 1990.
- 21 Cox 1997.
- 22 King, Keohane, and Verba 1994.
- 23 Brady and Collier (forthcoming).

References

- Brady, Henry E., and David Collier, eds. Forthcoming. *Rethinking Social Inquiry: Diverse Tools, Shared Standards*. Lanham, Md.: Rowan & Littlefield; Berkeley, Calif.: Berkeley Public Policy Press.
- Brady, Henry E., and Cynthia S. Kaplan. 2001. Categorically wrong? Nominal versus graded measures of ethnic identity. *Studies in Comparative International Development* 35:3, 56–91.
- Cox, Gary. 1997. *Making Votes Count: Strategic Coordination in the World's Electoral Systems*. Cambridge: Cambridge University Press.
- Darwin, Charles. 1999 (1859). *The Origin of Species*. New York: Bantam Books.
- Friedman, Milton. 1953. *Essays in Positive Economics*. Chicago: University of Chicago Press.
- Hempel, Carl G. 1962. Deductive-nomological vs. statistical explanation. In *Minnesota Studies in the Philosophy of Science*, vol. 3, ed. Herbert Feigl and G. Maxwell. Minneapolis: University of Minnesota Press, 98–169.
- . 1965. *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*. New York: Free Press.
- Hempel, Carl G., and Peter Oppenheim. 1948. Studies in the logic of explanation. *Philosophy of Science*, 15, 135–75.
- Hodge, M. J. S. 1987. Natural selection as a causal, empirical, and probabilistic theory. In *The Probabilistic Revolution: Ideas in the Sciences*, vol. 2, ed. Lorenz Kruger, Gerd Gigerenzer, and Mary S. Morgan. Cambridge: MIT Press.
- Kaplan, Abraham. 1964. *The Conduct of Inquiry: Methodology for Behavioral Science*. San Francisco: Chandler Publishing.
- King, Gary, Robert Keohane, and Sidney Verba. 1994. *Designing Social Inquiry: Scientific Inference in Qualitative Research*. Princeton: Princeton University Press.
- Kuhn, Thomas. 1962. *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.
- Laitin, David. 1998. *Identity in Formation: The Russian Speaking Populations in the Near Abroad*. Ithaca: Cornell University Press.
- Lloyd, Elisabeth. 1994. *The Structure and Confirmation of Evolutionary Theory*. Princeton: Princeton University Press.
- Nagel, Ernest. 1961. *The Structure of Science: Problems in the Logic of Scientific Explanation*. New York: Harcourt, Brace and World.
- Olson, Mancur. 1971. *The Logic of Collective Action, Public Goods and the Theory of Groups*. Cambridge: Harvard University Press.
- Popper, Karl. 1959. *The Logic of Scientific Discovery*. New York: Basic Books.
- . 1979. *Objective Knowledge: An Evolutionary Approach*, 2nd ed. Oxford: Clarendon Press.
- Salmon, Wesley C. 1989. *Four Decades of Scientific Explanation*. Minneapolis: University of Minnesota Press.
- Sekhon, Jasjeet. 2004. Quality meets quantity: Case studies, conditional probability, and counterfactuals. *Perspectives on Politics* 2:2, 281–93.
- Scott, James C. 1990. *Domination and the Arts of Resistance: Hidden Transcripts*. New Haven: Yale University Press.
- Smart, J. C. C. 1963. *Philosophy and Scientific Realism*. London: Routledge & Kegan Paul.
- Suppe, Frederick. 1977. *The Structure of Scientific Theories*, 2nd ed. Urbana: University of Illinois Press.
- . 1988. *The Semantic Conception of Theories and Scientific Realism*. Urbana: University of Illinois Press.
- Suppes, Patrick. 1967. What is a scientific theory? In *Philosophy of Science Today*, ed. S. Morgenbesser. New York: Meridian, 55–67.
- Van Fraassen, Bas. 1980. *The Scientific Image*. Oxford: Clarendon Press.