NEW VISIONS IN SECURITY

THEORY AND EVIDENCE IN COMPARATIVE POLITICS AND INTERNATIONAL RELATIONS

EDITED BY RICHARD NED LEBOW AND MARK IRVING LICHBACH

Theory and Evidence in Comparative Politics and International Relations

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Printed in the United States of America.

To Kate, Eli, David and Andrew

And to Sammi Jo and Yossi

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What Can We Know? How Do We Know?

Richard Ned Lebow

This book was conceived in the course of a long, wet afternoon in Columbus, Ohio. Inside, in a small, brightly lit auditorium, enthusiastic graduate students took turns presenting papers that were the product of an year-long seminar intended to help them develop dissertation proposals. Their words fell on the ears of their fellow students and six professors in international relations. Their presentations, although diverse in subject, were remarkably uniform in structure. They began by laying out a few propositions, went on to describe the data sets or cases that would be used to test these propositions and ended with a discussion of preliminary research findings. The professor who had taught the student participants exuded an avuncular aura throughout the proceedings, and my colleagues, who were encouraged to interrogate the students, largely queried them about their research design and choice of data. For the most part, the students provided competent answers to these questions.

Another colleague and I raised the tension in the room by asking each of the students in turn why they had been drawn to their subject matter. What puzzle or policy concern animated them? What light might their preliminary findings throw on that puzzle or problem? Their responses were largely unsatisfactory. Two students were flummoxed. One insisted he was "filling a gap in the literature." Two more defended their choices in terms of the availability of data sets. Another noted that his subject was a "hot topic," and that a dissertation on it would increase his chances of landing a good job. Only one student justified her research with reference to her sense of urgency about a real world problem: regional conflict.

When pushed, she nevertheless found it difficult to describe what implications her propositions might have for the trajectory of these conflicts or the efforts to ameliorate them. Another colleague, also dissatisfied, questioned the choice of two of the data sets, suggesting that they lumped together cases that had played out in quite different political-historical contexts. After the session, two of my colleagues, including the professor in charge of the seminar, told me I had been too hard on the students.

Two other colleagues were supportive, one of whom, from another field, had heard reports about what had transpired. The three of us agreed that our students, beginning in their introductory scope and methods class, were encouraged to privilege quantitative over qualitative research and choose dissertation topics based more on their feasibility than on their theoretical or substantive importance. They had a sophisticated understanding of research design—but only in so far as it pertained to the strictures of statistical inference. Despite—or perhaps, because—of three years of graduate training, they were correspondingly uninformed about the more general problems concerning evidence. Most gave the impression that it was just "out there" waiting for them to harvest, and failed to realize the extent to which it is an artifact of their theories. They were largely insensitive to context and the understandings of the actors, and how they might determine the meaning of whatever observations they as researchers made. All their proposals conveyed a narrow understanding of science as a form of inference whose ultimate goal is predictive theories. They were not particularly interested in causal mechanisms, let alone in other forms of political understanding such as the constitution of actors.

We agreed that epistemological and methodological narrowness, although pronounced at Ohio State University, was common enough in the discipline to arouse general concern. In our view, the use of King, Keohane, and Verba, *Designing Social Inquiry* (KKV), as a core reading in so many scope and methods courses could only make the situation worse. My colleague, whose reputation was based on "mainstream" quantitative research—a shorthand term I use to describe those who more or less accept the unity of the sciences—felt just as strongly as I did. He considered many of KKV's recommendations for collecting and evaluating data quite sensible, but he rejected its epistemological foundations as seriously flawed, its characterization of science as ill-informed, relegation of qualitative research to second-class status as unacceptable, and its almost exclusive focus on the construction and analysis of data sets as regrettably narrow. Conversations with a few other dissatisfied colleagues at Ohio

State and other institutions led us to consider a book to address some of these concerns.

We did not want to produce another text, nor a study that sanctioned a particular approach. Our goal was to encourage dialogue in the discipline, and among our students, to transcend epistemological and methodological differences. We must pursue our quest for political knowledge as equals because none of our preferred epistemologies are problem free—quite the reverse. Despite inflated claims by partisans of particular approaches, none of them can point to a string of unalloyed theoretical and empirical triumphs that rightfully leave adherents of other approaches frustrated and envious. We can all benefit from a more thorough understanding of each other's assumptions, strategies, practices, successes and failures, and reasons for pride and self-doubt. Such comparison reveals that many of the epistemological and methodological problems we face cut across approaches and fields of study.

With this end in mind, we commissioned representatives of three different epistemologies to write papers on how evidence matters or should matter in the social sciences. These papers were presented and discussed at a conference at Ohio State, hosted by its Mershon Center, on May 12–13, 2000.[1] Some of the papers were revised and presented, with additional ones, at the September 2000 annual scientific meeting of the American Political Science Association (APSA). Our book includes some of these papers as well as others that were subsequently commissioned. The conference and APSA panel were characterized by sharp disagreements among people from different research traditions. They also witnessed—as do the succeeding chapters—serious efforts at mutual engagement in the context of addressing problems of common concern. We hope readers will find this tension refreshing and informative.

Our choice of evidence as the initial focus of our papers reflected our commitment to dialogue. Most of us take evidence seriously, recognize that it comes in many forms, and want to develop and apply good procedures for its selection and evaluation. We recognize that our procedures and protocols are far from being problem free and that our treatment of evidence in practice never quite measures up to our ideals. While the papers and subsequent chapters all address the question of evidence, they also speak to problems of epistemology and ontology because evidence cannot satisfactorily be addressed in a philosophical vacuum. The purposes for which we seek and use evidence influence—if not determine—the kind of evidence we seek and the procedures we use to collect, evaluate, and analyze it. Our purposes, in turn, reflect our understandings of the nature of knowledge and how it is obtained. Such assumptions are often left implicit; they may be only partially formulated. All the more reason then to foreground these choices and some of their most important implications for research.

Essays of this kind are messier, make more demands on readers, and inevitably raise more questions than they answer. This is a fair price to pay because the alternative—an effort to "get on with the job" by focusing exclusively, or nearly exclusively, on research methods—clearly the message of KKV—risks missing the forest for the trees. Like KKV, it is likely to conceive of research design in a manner that, though inadequate, is not counterproductive to the ends it seeks. More fundamentally, by endorsing an arbitrary or inadequately theorized telos, it may sponsor a project that by its very nature is unrealistic.

King, Keohane, and Verba

Our volume is not conceived of as a critique of *Designing Social Inquiry*, but all our authors play off of it, and many use their criticisms as the jumping off point for their own arguments. KKV is the obvious foil because it is the most widely used text in graduate courses in method. It exudes a neopositivist confidence, shared by the many mainstream social scientists, that evidence is relatively unproblematic and can be decisive in resolving theoretical controversies. It emphasizes the existence of a single scientific method, the search for regularities, the issue of replication, the primacy of causal inference, the importance of "observable" implications that are impartial to competing theories, and the significance of falsifiable hypotheses that are neutral between warring value commitments. It is regarded by its advocates as an important rejoinder to interpretivists, culturalists who flirt with postmodern relativisms, structuralists who have or have not found a haven in the now-dominant realist philosophy of science, and even rationalists (e.g., Hausman in the philosophy of economics literature) who have expressed doubts about the evidentiary basis of economics.[2]

KKV is also an easy target. It makes what many see as unwarranted claims for the rigor and success of quantitative research in the social sciences, unfairly deprecates qualitative research, and insists that qualitative researchers have much to learn from their quantitative colleagues.[3] Still others feel uncomfortable about the way in which KKV represent their protocols as hard-and-fast rules when, as is often the case, they are violated for good reason. A case in point is their injunction against selecting on the dependent variable. In his chapter, David Waldner provides a stunning example of how this strategy has been used successfully. Critics of neopositivism—including some of our contributors—contend that KKV misrepresents philosophical debates concerning falsification and science; it also fails to recognize that science is a practice based on conventions, not deductively established warrants, and that prediction is only one form of knowledge.

KKV is the appropriate starting point for this introduction. By describing what our contributors find valuable and objectionable in the book, we can compare their positions on important questions of method, epistemology, and ontology. When we do this, an interesting pattern emerges. Those closest to KKV in their orientation are equally keen to disassociate themselves from its epistemology and ontology. They do so to salvage methods and procedures they think valuable, but also to broaden the methodological menu and to confront problems with statistical inference to which KKV are oblivious. These contributors—Pollins, Waldner, and to a lesser extent, Chernoff—advocate an understanding of science that shows remarkable similarities to that advanced by more radical critics of KKV's project.

King, Keohane, and Verba explicitly acknowledge the importance of solid philosophical foundations. This makes it all the more surprising that they anchor their project in a version of logical positivism developed by the so-called Vienna Circle, a version that has long since been rejected by some of its key formulators and philosophers of science. Their choice is indefensible, but perhaps explicable in light of their belief in the unity of sciences and its corollary that the goals and methods of inquiry into the physical and social worlds are fundamentally the same. It is therefore appropriate to begin with a discussion of foundational claims and the reasons why the search for them is bound to fail.

Foundational Claims

Logical positivism was an attempt to provide a logical foundation for science. Its early propagators included Moritz Schlick, Otto Neurath, Rudolph Carnap, Herbert Feigl, and Kurt Gödel. They assumed a unity among the sciences, physical and social, and sought to provide warrants for establishing knowledge. Toward this end, they established the "verification principle," which held that statements of fact had to be analytic (formally true or false in a mathematical sense) or empirically testable. It was soon supplanted by the principle of "falsification" when Karl Popper, a close associate of the Circle, demonstrated that verification suffered from Hume's "Problem of Induction." For Popper, a scientific theory had to be formulated in a way that made it subject to refutation by empirical evidence. Scientists had to resist the temptation to save theories by the addition of ad hoc hypotheses that made them compatible with otherwise disconfirming observations. By this means, Popper asserted, a theory that was initially genuinely scientific—he had Marxism in mind—could degenerate into pseudoscientific dogma.

The Vienna Circle and Karl Popper had relatively little influence on the hard sciences but provided the ideological underpinning of the so-called behavioral revolution, of the1960s. As Brian Pollins notes, their influence grew among social scientists, just as their ideas came under serious challenge by philosophers of science. One important reason for this challenge was the logical distinction that "falsificationism" made between theory and observation. Carl Hempel demonstrated that no such distinction exists; tests cannot be independent of theory because all observations presuppose and depend on categories derived from theory. Unity of science was also questioned as the several sciences confronted different degrees of contingency in their subject matter. They worked out diverse sets of practices to deal with this and other problems and to collect and evaluate evidence. As Bernstein et al. point out, thoughtful social scientists, among them Max Weber, had come to recognize that regularities in human behavior and the physical world are fundamentally different. Social science laws, Weber argued, have a short half-life because they disappear or change as human goals and strategies evolve, in part because people come to understand these regularities and take them into account in their deliberations and strategies.[4] By the 1950s, Popper had come to understand "covering laws" as limited in scope, and perhaps as even unrealistic.[5] If he were alive today, he might well agree with Pollins that the social sciences are "the '*really* hard' sciences."[6]

KKV claim that "falsifiability" lies at the heart of the scientific project and insist that they draw their understanding of it from Popper's 1935 book, *The Logic of Scientific Discovery*. This is the version, Pollins reminds us, that Popper later disavowed when he realized the problematic nature of evidence. For the same reasons, it calls KKV's project into question; at the very least it demands a thoroughgoing reformulation. The logical positivism on which KKV draws assumes a "real world" (i.e., an objective reality) that yields the same evidence to investigators who search for it in the proscribed manner. This world is also expected to yield "warrants" that validate theories on the basis of evidence and statistical tests. Knowledge is accordingly a function of good research design and good data.

The notion of a "real world" is very difficult to defend; and among our contributors, only Fred Chernoff makes the cases for a limited kind of "naturalism." Without a "real world," warrants for knowledge cannot be deduced logically, and efforts by philosophers to establish foundational claims, by either substantive (metaphysical) or epistemological (Kantian) means, must, of necessity, end in failure. If "unity of science" is indefensible, there are no universal procedures for determining what constitutes evidence or how it is to be collected and evaluated.

Alfred Schutz observed that all facts are created by cognitive processes.[7] John Searle distinguished between "brute," or observable facts (e.g., a mountain), and "social," or intentional and institutional facts (e.g., a balance of power).[8] Every social scientist deals primarily in social facts and must accordingly import meaning to identify and organize evidence. This is just as true of statistical evidence as it is of case studies. James Coleman has shown that every measurement procedure that assigns a numerical value to a phenomenon has to be preceded by a qualitative comparison. While the assignment of numbers may permit powerful mathematical transformations, it is illicit to make such assignments if the antecedent qualitative comparison has not or cannot be completed.[9] Many mainstream social scientists who acknowledge this problem nevertheless contend that even when the preconditions for successful measurements or causal modeling are not present, the "scientific method" should still serve as a regulative idea. Such a statement has no obvious meaning.

The foundational claims of logical positivism have been used by social scientists to serve political as well as intellectual ends. In the 1950s and 1960s, they were used to justify the behavioral revolution and its claims for institutional dominance and funding. Today, they defend orthodoxy against challenge while obscuring relations of power. Science and pluralism—and the former is impossible without the latter—demand that they be jettisoned.

What are we to do in the absence of a real world, unity of science, and foundational claims that could supply warrants? Does anything go, as some postmodernists joyously proclaim and some mainstream social scientists lament? None of our contributors believe that the baby of science has to be thrown with the bathwater of positivism. They advocate an understanding of science that has become widespread among philosophers and scientists: science as a set of shared practices within a professionally trained community.[10] Those sciences diverge in many ways, including in their relative concern for historical explanation versus prediction. Geology, pathology, and evolutionary biology are focused on the historical explanation of how the earth, dead people, and species came to be the way they are. Physics and chemistry use prediction as the gold standard and, unlike the sciences noted above, understand explanation and prediction to be opposite sides of the same coin.

The competent speaker, not the grammarian, is the model scientist, and each practitioner of discipline, like each speaker of a language, is the arbiter of its own practice. All insights and practices, no matter how well established, are to be considered provisional and almost certain to be supercede. Debates are expected to scrutinize tests and warrants as much as research designs and data. Consensus, not demonstration, determines what theories and propositions have standing. In his last decades, Popper came around to this position. He spoke of relative working truths—"situational certainty" was the term he coined—and emphasized the critical role of debate and radical dissent among scientists.[11]

Kratochwil suggests, and Pollins concurs, that the court is an appropriate metaphor for science as practice. As in court, difficult questions must be decided on the basis of evidence and rebuttal, not on the basis of proofs. Such contests are also quasi-judicial because they are subject to constraints that govern the nature of information and tests that can be presented to the jury. Those scientists who play formal roles in such proceedings (e.g., journal editors, conference chairs), are, like judges, expected to adhere to well-established procedures such as blind peer review to promote fairness and to avoid conflicts of interest. Courts allow appeals that can be made on the basis of new evidence or improper treatment of the existing evidence or the disputing claimants. Science does the same and, in addition, also allows claims to be reopened on the basis of new insights concerning causal mechanisms. David Waldner provides a striking example of how this worked in the case of plate tectonics. The theory of continental drift was proposed by Alfred Wegener in the 1930s, but it was rejected by the scientific community because it ran counter to the prevailing orthodoxy that the continents were fixed. Wegener also hurt his case by failing to offer any plausible mechanism to explain continental drift. The debate was reopened in the 1960s, partially as a result additional evidence, but primarily in response to the appearance of a credible causal mechanism: thermodynamic processes deep within the earth that create convection currents that move the plates on which the continents rest.

Scientists recognize that the ethics of practice is at least as important as the logic of inquiry. Individual scientists must exercise care and honesty in developing frameworks and in collecting, coding, and evaluating data and communicating results to other members of the community. They must be explicit about the normative concerns and financial interests, if any, that motivate their work. Those who control funds, publications, appointments, tenure, promotions, honors, and the like must be open to diverse approaches, supportive of the best work in any research tradition, and committed to the full and open exchange of ideas. In the words of Rom Harré, science is "a cluster of material and cognitive practices, carried on within a distinctive moral order, whose characteristic is the trust that obtains among its members and should obtain between that community and the larger lay community with which it is interdependent."[12]

The Product of Inquiry

A common understanding of the nature of science does not necessarily promote a shared understanding of what is possible to discover. The hypothetical-deductive (H-D) method and mainstream social science in general assume that a self-correcting process of conjectures and refutations will lead us to the truth. Fred Chernoff, who is the most sympathetic among our authors to this understanding, argues that such a process will bring us closer to some truth. If progress is not possible, he asks, why would scholars continue to do research and engage in debate?

Brian Pollins recognizes that visions of the truth will always be multiple because different research communities will reach different conclusions about the nature of knowledge, how it is established, and how it is presented. He is nevertheless convinced that adherence to the principles of falsifiability and reproducibility could foster more meaningful communication across these traditions and improve their respective "tool kits." This would make truth claims more difficult to establish and easier to refute. Hopf shares this vision to a degree. He accepts Popper's notion of working truths and argues that both mainstream and interpretivist approaches could make more convincing, if still modest, truth claims if they engaged in extensive mutual borrowing. To deliver on its promises, the mainstream needs to adopt a more reflexivist epistemology. Interpretivists, who have the potential to deliver on their promises can do so only by incorporating many mainstream research methods.

Mark Lichbach offers a parallel vision. In his view, theory consists of research programs that invoke different causal mechanisms to build theories that describe lawful regularities. Evidence establishes the applicability of these models of a theory for the models of data that exist in particular domains; the elaboration of a theory thus delimits the theory's scope. Evaluation grapples with the problem that the science that results from following the first two principles is prone to nonfalsifiability and to self-serving confirmations. Confrontations between theory and evidence are thus evaluated in the context of larger structures of knowledge, so rationalist, culturalist, and structuralist approaches in practice forge ahead on their own terms.

Kratochwil adopts a more radical position. If truth is no longer a predicate of the world—that is, not out there waiting to be discovered—then neither the H-D nor any other kind of research method can discover it. Truth is a misleading telos. We must rethink our goals and metaphors. Positivists conceived of truth as a chain that justifies beliefs by other beliefs, which ultimately must be anchored in some foundation. The mainstream, and some of our contributors, envisages truth to be more like a circle, whose area can be estimated with increasingly greater accuracy by approximating its circumference by use of successive polygons. This metaphor, Kratochwil suggests, is inappropriate because a circle is bounded by a perimeter, while the physical and social worlds have no knowable limits. If we need a metaphor, the game of Scrabble may be a more useful one. We begin with concepts and rules that make many outcomes possible. We can criss-cross or add letters to existing combinations, but all these entries must be supportive and must at least partially build on existing words and the concepts that underlie them. When we are stymied, we must play elsewhere but might by a circuitous route link up with all other structures. A modified game of Scrabble in which the board had no boundaries and new words could be placed anywhere might capture the idea even more effectively. According to this metaphor—in its original or modified form—progress in the social sciences is measured in terms of questions, not answers.

Bernstein, Lebow, Stein, and Weber share Kratochwil's ontology. They contend that all social theories are indeterminate because of the open nature of the social world. They offer an analogy between social science and evolutionary biology. Outside of certain "red states," evolution is widely regarded as a wonderfully robust scientific theory. Yet, it makes few predictions because its adherents recognize that almost everything that shapes the biological future is outside of the theory. It is the result of such things as random mutations and matings, continental draft, changes in the earth's precession and orbit, variations in the output of the sun—and how they interact in complex, nonlinear ways. Evolution is the quintessential example of a process where small changes can lead to very large divergences over time. The late Stephen Jay Gould suggested that if the tape of evolution could be rewound and played again and again, no two runs would come out the same.[13] Bernstein and his coauthors contend that this is also true of international relations, where personality, accidents, confluence and nonlinear interactions—all of which are, by definition, outside any theory of international relations—have a decisive influence on the course of events. Predictive theory is impossible, and so are even probabilistic theories—if they were possible, they would tell us nothing about single cases.[14]

Bernstein et al. recognize that human beings at every level of social interaction must nevertheless make important decisions about the future. They make the case for forward "tracking" of international relations on the basis of local and general knowledge as a constructive response to the problems they, and other authors in this volume, identify in backward-looking attempts to build deductive, nomothetic theory. They regard this kind of scenario construction, evaluation, and updating as a first step toward the possible restructuring of social science as a set of case-based diagnostic tools.

None of our contributors rally in support of KKV, but Chernoff offers a limited defense for the unity of science, contending that many of the methods used in the physical sciences are applicable to the social world. Despite the many problems involved in bridging the physical and social worlds, outright rejection of unity of science, he warns, involves even greater logical and methodological difficulties. To circumvent the problem of foundational claims, he draws on the understanding of the truth developed by American pragmatists. Following James, he suggests that to describe a statement as true is nothing more than saying that "it works." The concept of something working is treated at length by Peirce and James, and defined as something that helps us navigate the sensible world. This is not a correspondence theory because facts for James are nothing more than mental constructs that are maintained because of their demonstrable utility. In his understanding, there is no useful belief that does not accord with the "facts." Even traditional correspondence theories, Chernoff suggests, frame truth as a relationship between a statement and external reality, as opposed to a feature of reality itself. They are accordingly testable against our observations, as these observations in turn constitute the "effects" of reality. Unlike Platonism, which views the truth as a form, correspondence theories, Chernoff insists, are not vulnerable to Kratochwil's argument that truth is not a predicate of the world.

The previous discussion makes clear the division among our contributors concerning the nature of knowledge. Some, such as Pollins and Chernoff, believe that good questions, methods, and evidence can lead us to some kind of knowledge. Others, such as Kratochwil and this editor, believe that all but the most banal propositions can ultimately be falsified, but the process of falsification requires us to develop new research tools and questions. Falsification can lead us to more sophisticated propositions and methods.[15]

The Purpose of Inquiry

Mainstream social science envisages the goal of inquiry as knowledge, and many of its proponents believe that knowledge requires fact to be separated from values. "Value neutrality" is often described as one of the attributes of true science. It follows that research questions should grow out of prior research or empirical discoveries. The "fact-value" distinction dates back to David Hume, who insisted that statements of fact can never be derived from statements of value, and vice versa. His argument and its implications have been debated ever since. They were a central feature of the *Methodenstreit* that began in Vienna in the late nineteenth century.

Max Weber, one of its most distinguished participants, made the case for the social sciences being fundamentally different from their natural counterparts. Values neither could nor should be separated from social inquiry. This would represent an attitude of moral indifference, which he insisted, "has no connection with scientific 'objectivity.' "[16]

All of our contributors side with Weber on the fact-value distinction. Jack Levy and Andrew Lawrence, who hold quite different views about the value of the democratic peace research program, agree that its ultimate justification must be the insights and guidance it offers us about reducing the frequency of violent conflict. It is possible to emphasize either facts or values in research, but problems arise when either is pursued at the expense of the other. Value neutrality is impossible for there is no way we can divorce our normative assumptions and commitments from our research, and attempts to do so are damaging to discipline and society alike. Efforts to segregate research from values have ironically encouraged and allowed scholars to smuggle norms into their research through the back door. According to John Gerring, the adoption of a Pareto optimality, is a case in point. It is not a scientific choice but a partisan and highly consequential moral choice.[17]

Normative theorizing must deal with facts just as empirical research must address norms. They do no inhabit separate worlds. Nor should they, because the purpose of social science is practical knowledge. The choice of subjects and methods presume judgments of moral importance. It is incumbent upon researchers to make their values or telos explicit and fair game for analysis and critique. In the broadest sense, political science can be described as the application of reason to politics. It is practiced by people with the requisite expertise, which includes the ability to separate reason from values in their analysis—although not in their choice of topics. Hume's "fact-value" distinction can be distorted at either extreme: either by denying values or by denying facts. We need to maintain the distinction but bring norms into the foreground, not only in research, but in our training of graduate students.

A more serious problem arises from the failure of Hume's dichotomy to capture what John Searle has called "institutional facts." These are neither facts nor values, but "performatives"—like the "I do" of a marriage ceremony—that establish actors and their relationships. It is not far-fetched to argue that the most interesting questions of the social and political world are "outside" the Humean dichotomy, and that social science must also go beyond it. Weber, for one, recognized that values are not just the preferences of researchers but are also constitutive of their identities and interests. For John Searle, they are the glue that holds society and its projects together. If we want to understand society, we need to adopt methods that confront values and their importance, not rule them out a priori as much of mainstream has tried to do.[18]

In large part, differences over the role of values reflect differences in the purposes of inquiry. Neopositivists who envisage theory as an end product of social science sometimes see values as a distraction and embarrassment. They would believe, like physical scientists, that their research is driven by puzzles and anomalies that arise from their research. This ignores the well-documented extent to which research agendas of physical scientists are equally driven by normative commitments. More thoughtful neopositivists, including the contributors to this volume, see nothing wrong with acknowledging the normative and subjective nature of research agendas. What makes their research scientific is not their motives but the rigor of their methods. Further along the spectrum are nonpositivists, at least some of whom regard theory as a means to an end and as valuable only in so far as it helps us understand and work through contemporary political, economic, and social problems. For them, social science begins and ends with values.

The Method of Inquiry

Contributors who are generally sympathetic to the goals of the mainstream—Pollins, Chernoff, Waldner, and Levy—consider KKV's depiction of research as a misguided attempt to put the scientific method into a statistical straitjacket. KKV equate good research design with inference and define it in a way that makes it all but synonymous with statistical inference.

For KKV and others who subscribe to their narrow framing of the H-D method, the only ways to challenge a theory are by disputing its internal logic or by adding additional observations. Kratochwil, Hopf, and Waldner all recognize that adding observations addresses the first problem of induction raised by Hume: "How much is enough?" It says nothing about the second problem: causality. The discovery of laws requires leaps of imagination; laws are not simply statements of regularities, but creative formulations that order those regularities or make their discovery possible. Both theory formation and testing frequently require and certainly benefit from the use of counterfactual thought experiments.[19]

The core principle of mainstream social science is the H-D model. KKV's good scientist "uses theory to generate observable implications, then systematically applies publicly known procedures to infer from evidence whether what the theory implied is correct."[20] Valid observations are all that is required to test a theory, and a single, critical experiment can refute a law. In practice, David Waldner observes, a variety of criteria are used to confirm and disconfirm theories, of which evidence is only one.

This is evident from the solution of the mystery of dinosaur extinction, the very example that KKV improperly cite as an outstanding success of the H-D method. They claim that the hypothesis of a meteor impact led to the search for iridium, whose discovery at the K/T boundary confirmed the hypothesis. In fact, researchers reasoned backwards, from the discovery of the iridium layer to its probable cause, and focused on causal mechanisms—what it would take to kill dinosaurs and produce iridium—rather than on research design considerations. Meteor impact is now generally accepted by the wider scientific community—because of the causal mechanism and logic that connects it to an otherwise anomalous outcome. Dinosaur extinction is also an interesting case because it violates KKV's supreme injunction against coding on the dependent variable. Walter Alvarez and the Berkeley group did just this; they never examined other instances of mass extinction and failed to study epochs of nonextinction when extraterrestrial impacts were common. They also ignored far more numerous subextinctions.

Drawing on work in analytical philosophy, Waldner distinguishes between inferences and explanations. He suggests that we evaluate hypotheses in terms of their evidentiary support and theoretical logics. A confirmed hypothesis is one that has survived scrutiny against its closest rivals—given the current state of theory and evidence. It is more reasonable than disbelief but still subject to revision or refutation. We explain by using confirmed hypotheses to answer questions about why or how phenomena occur. All explanations require confirmed inferences, but not all inferences constitute explanations or embody them. Causal mechanisms can impeach or enhance hypotheses with otherwise impeccable research-design credentials. They promote inferential goodness via theory, not via research design. Waldner offers seven ways in which causal mechanisms can be used to reject hypotheses. His major point is that there are many ways to confirm and reject hypotheses, only one of which is statistical inference. He agrees with Hopf that underdetermination is not resolved by collecting more evidence, but by better understanding the evidence we already have. Good social science seeks contextualized explanations based on causal mechanisms, not just law-like regularities.

Theories are also rejected because better theories come along. The Ptolemaic model of planetary motion successfully accounted for the motions of the sun, moon, and five known planets. It was rejected in favor of Copernicus's heliocentric model because the latter was simpler; Ptolemy's model required eighty epicycles to explain these motions. His system was nevertheless *more* accurate than that of Copernicus and remained so until Kepler's Laws could augment the latter.

In practice, most refutations are not accepted, but understood as problems of measurement, experimental error, "put right" through manipulation of data or explained away as anomalies. The hole in the ozone layer over the south pole offers a nice example. The British Antarctic Survey began taking measurements of the density of the ozone layer in 1957, and—for the first twenty years—variation followed a regular seasonal pattern. Beginning in 1977, deviation from this pattern was noted, and at first attributed to instrument error. Every spring, the layer was measured as weaker than the previous spring, and by 1984, scientists reluctantly concluded that change was occurring. This conclusion met considerable resistance until experiments and observations revealed that industrial chemicals, particularly chlorofluorocarbons (CFCs) containing chlorine, could destroy ozone. Refutations are taken seriously only when reasons are provided for why the observed deviations were systematic and not due to random errors or disturbances, and ozone depletion was no exception. Even then, as research on deterrence indicates, refutations can encounter serious resistance when the theories in question serve important political or psychological ends.[21]

There may be good reasons for ignoring refutations. Paul Diesing reminds us that every theory is refuted, as they all are at least somewhat false. If we give up theories because they are refuted, we can no longer profit from their heuristic potential to produce better theories.[22] It may be, as Imre Lakatos suggests, that occasional, if partial, verifications of theories are what keep research programs going, and they are all the more necessary when their theories have been exposed by repeated refutations.[23]

Pollins, Hopf, Waldner, and Chernoff, all offer suggestions for overcoming methodological and epistemological narrowness. Pollins insists that there is no logical reason why the rules of scholarship cannot be pluralistic. Many of the practices described by KKV can be incorporated into a "new and broader based social science epistemology." For Pollins, the two defining criteria of such a science are falsifiability and reproducibility. Falsifiability assumes that we do the best we can to be clear, and that "more correct" can be distinguished from "less correct." Observations should be classified as consistent or inconsistent with a claim, and decisive tests ruled out because of the theory-laden nature of observation. Falsifiability is a communicative concept that allows challenges to and changes in conceptual categories. So is reproducibility. It requires research to be described in ways that allows duplication so others can try to obtain the same results from the same evidence, or same kind of evidence. Such an approach, Pollins acknowledges, shifts the emphasis from the interaction between theory and observation to that between claimant and professional audience.

Hopf plays variations on this theme. He stresses how much the mainstream and interpretivist traditions actually share, and he identifies seven key methodological conventions in this regard: differentiate premises from conclusions and correlations from causes, respect the canons of inference, establish standards of validation for data and other source materials, address problems of spuriousness that arise from correlations, rely on syllogistic and deductive logic, and accept the contestability of all beliefs and findings. Hopf suggests that differences within the reflexivist community on these issues are more serious than those between it and the mainstream. The deepest cleavage runs between phenomenological, interpretivist, and hermeneutic approaches on the one hand, and some postmodern or critical approaches on the other. Some representatives of the latter maintain that narration constitutes its own truth and has no need of argument or proof. So-called mainstream reflexivists are interested above all in the ways in which social order reproduces itself through the behavior of actors. To do so they must consider the context and meaning in which these interactions take place and the various ways in which observers can come to understand them. They have the same need as mainstream scholars to consider the nature of facts, evidence, truth, and theory.

The Practice of Inquiry

Science consists of hard-fought bull sessions with students and colleagues, applications for funding, the conduct of research, management of research facilities and teams, writing up research results, and the presentation of findings. Findings may be circulated as draft papers, posted on the Web as preprints, or submitted to journals or publishers as would-be articles or books. Contested claims are adjudicated at many of these steps by researchers themselves, in informal discussions among colleagues, the more formal proceedings associated with peer review, panel presentations, and debates on Web sites and in professional publications. Such a process is quite distinct from rarefied debates—such as those in this volume—about the nature and purpose of inquiry and the methods appropriate to it.

To understand the practice of science, we need to adopt a microperspective; and with that end in mind, we asked two contributors to look into why some research programs are successful. Jack Levy—guilty of coding on the dependent variable here—examines three successful paradigms in international relations: rational choice, territory-war and power balance, and democratic peace (DP). He evaluates KKV's contention that there is little tension between normative and descriptive research programs, and that the most successful programs are those with the most empirical support. Andrew Lawrence devotes his chapter entirely to the democratic peace.

Levy finds that research programs in international relations are sustained by different combinations of incentives. Rational choice is largely theory driven, while territory-war and the power balance, and DP are more evidence driven. Research programs propelled by a powerful, or at least, intellectually appealing, theory can become self-sustaining even in the absence of evidence. This is true of general equilibrium theory in economics and rational choice in political science, although the latter's influence expanded considerably when it was linked to the quantitative research tradition and received, in Levy's judgment, considerable empirical confirmation. All three research programs indicate that any assessment of the relative importance of theory and evidence in sustaining a research program will depend on the level of theory at which we focus. A paradigm may be theory driven (e.g., liberalism), but a theory within it may be evidence driven (e.g., DP). Research programs can also be motivated in sequence by theory and evidence.

Scholarship in the war/territory and DP research programs has responded to the discovery of striking empirical regularities: that a disproportionately high number of wars involve territorial disputes, that territorial disputes are more likely to lead to wars than to any other kind of dispute, and that democracies appear never to go to war with one another. Levy acknowledges that normative concerns have also influenced the prominence and evolution of the DP research program. He nevertheless questions what he describes as the widely held view that policy agendas account for the appeal and popularity of DP. Although many liberals were drawn into the research, they have not allowed their values or political preferences to stand in the way of or distort the evidence. One reason for this is the engagement in this research of scholars from other political perspectives. In Levy's judgment, the DP is a quintessential example of a "progressive" and responsive research program.

Lawrence is critical of the DP program. In his view, it has distorted its Kantian origins and yielded diminishing returns theoretically. The prevailing norms of mainstream social science—especially those of quantitative social science—have restricted the debate, led to a fetish with numbers and acceptance of "common sense" definitions of key variables such as democracy and war. These definitions obscure the meaning of these variables and how these meanings have evolved over time. Statistical tests are largely useless because "generous fudge factors" are used to code borderline cases of both war and democracy. Quantitative researchers on the whole emphasize external validity (comparison across cases) over internal validity—the application, or fit, of measures to individual cases. Causal inference is supposed to permit communication across the discipline, but the DP research program narrows it. In Lakatosian definition it is a "degenerate paradigm."

Lawrence contends that unarticulated but critical normative presuppositions and commitments often drive research. He is struck by the political bias of the DP literature, the enthusiasm the research program has generated among liberals, and the claims by some that the DP is one of the most robust research findings in international relations. In his view, its focus on nonwar among democracies, conceptions of democracy, and codings of war and democracy reflect, at best, parochial, and at worst, self-serving, perspectives that make the research program a justification for America's foreign policy and way of life. Democratic peace researchers have framed their inquiry in a way that excludes cases where the United States has resorted to force. The program reflects the general tendency of American social scientists to employ positivism as a means of evading reflexive self-knowledge. In this sense, DP, like deterrence theory during the cold war, is best understood as part of the phenomena these theories seek to explain.

"Social" Knowledge

Social understanding is inherently subjective. Research agendas, theories, and methods are conditioned by culture, beliefs, and life experiences. So too is receptivity to research findings. Recognition of this truth has led some postmodernists to interpret science as a political process and cloak for individual and group claims to privilege. This view of science is one-sided because it ignores the barriers erected by the scientific method against theories and propositions that either cannot be falsified or are demonstrably false.

The scientific method does not always prevail over politics and prejudice. The problem is sometimes the scientists themselves. Nineteenth-century biological and anthropological studies of cranial capacity "proved" the superiority of the Caucasian "race." Some contemporary researchers are still trying to do this with data from intelligence tests. Well-founded scientific claims also encounter resistance from the wider community. The theory of evolution continues to provoke widespread opposition from fundamentalist Christians. Claims by medical researchers that smoking is harmful, and more recently, by environmental scientists that the waste products of industrial society threaten to produce an irreversible transformation of the environment, have encountered predictable opposition from industries with profits at stake. The tobacco companies and some major polluters support scientists who dispute these claims.

Our contributors make it clear that there is no such thing as a "scientific method." Researchers and philosophers of science argue over what constitutes adequate specification and testing, the extent to which it is possible, and, more fundamentally, about the nature and goals of science. Attempts to provide definitive answers to these questions, as Karl Popper recognized, inevitably fail and risk substituting dogma for the ongoing questioning, inquiry, and debate that constitute the core commitment of science. These controversies render scientific truth uncertain, but working scientists, invoking the techniques and skills they have learned, generally have little difficulty in distinguishing good from bad science.

The scientific method in many ways resembles the Bill of Rights of the American Constitution. Its meaning is also interpreted through practice. And, like the scientific method, it has not always been interpreted or applied fairly. The Bill of Rights has sometimes failed to protect political, religious, and so-called racial minorities from the ravages of prejudice. In 1898, *Plessy* v. *Ferguson* established the principle of separate and equal education for African Americans that endured until *Brown* v. *Board of Education* in 1954. De facto segregated education continues to this day in some locales. *Brown* v. *Board of Education* reflected changing attitudes toward African Americans and the Constitution itself. Another impetus was extensive social science research that demonstrated that separate education was inherently unequal. Despite continuous controversy about the meaning of the constitution and despite periodic failures to apply its principles in practice, there is an overwhelming consensus that the Bill of Rights—and even more importantly, the American public's commitment to tolerance—remains the most important guarantee of individual freedoms. The scientific method is an imperfect but essential bulwark against many of the same kinds of passions. Like the Constitution, it ultimately depends on the ethical standards and commitments of the community it serves.

As many of our contributors have suggested, there is an important distinction to be made between the questions we ask and the ways in which we answer them. What distinguishes us from ideologues is our commitment to finding and evaluating answers by means of a scientific method. Social scientific research agendas are shaped by political beliefs, life experiences, and desires for professional recognition. There is nothing wrong with these motives. Good social science should be motivated by deep personal involvement in the burning issues of the day. Research can clarify these issues, put new issues on the agenda, and propose and evaluate the consequences of different responses. It can also influence the way people conceive of themselves, frame problems, and relate to the social order.

Logical-neopositivism and other "unity of science" approaches risk making social science sterile in its search for passionless, abstract truths. Some forms of postmodernism would make social science irrelevant by its rejection of the scientific method and insistence that all "readings" of texts and the world at large have equal standing. Social scientists need to confront both these dangers by reaffirming and explaining their twin commitments to social progress and the scientific method. The links between ourselves and our research do not undercut our claim to be practicing science, they make us better scientists and human beings.

The Structure of the Book

Our volume contains eleven chapters divided into four sections. This introduction is followed by chapters by Fritz Kratochwil and Ted Hopf on foundational claims. They develop ontological and epistemological criticisms of the unity of science. Kratochwil shows why warrants can neither be taken for granted nor derived from theories of science. Hopf argues that social science must become reflexivist in its epistemology.

The next section, on the product of inquiry, consists of chapters by Brian Pollins and Fred Chernoff. Pollins accepts the gist of the Kratochwil-Hopf criticisms and believes they point the way toward the possibility of a broader-based, pluralist epistemology that would permit and encourage diverse forms of research and knowledge building. Fred Chernoff reviews and assesses the epistemological and metaphysical claims of Kratochwil, Hopf, and Pollins as well as their take on naturalism. He makes a case for a pragmatic theory of knowledge and for a modified conventionalist account of social science as the best way of accounting for the successes and frequent failures of social science research during the past fifty years.

We then turn to the purpose and methods of research. David Waldner examines the role of causal logic in his explanation of science in general. He contends that they are the distinguishing characteristic of all explanations, and are routinely used to enhance or undermine theories. The next two chapters attempt to evaluate ongoing and well-regarded research programs in light of earlier discussions. As noted earlier, Jack Levy analyzes three such programs, including the democratic peace, and provides a positive take on their accomplishments. Andrew Lawrence focuses on the democratic peace and finds it crippled by epistemological, methodological, and normative problems.

In lieu of a conclusion, we offer two contrasting visions for the future of social science. Steven Bernstein, Ned Lebow, Janice Stein, and Steve Weber contend that our goal should be practical knowledge relevant to individual cases, as this would make our profession relevant to the policy world. They describe the benefits and procedures of scenario generation and updating as powerful forecasting tools that, at best, would provide useful guidance in addressing complex real-world problems and at least provide useful early warning of impending policy failure. Mark Lichbach draws on the arguments of this book and on observations gleaned from observing the practice of science to offer the outlines of an epistemology that would incorporate mainstream and interpretivist practices and encourage progress toward better theories in both traditions.

Notes

1. In attendance were Steven Bernstein, Stephen Hanson, Rick Herrmann, Ted Hopf, Andrew Lawrence, Jack Levy, Mark Lichbach, Brian Pollins, Bert Rockman, Janice Stein, and Steve Weber.
2. Hausman, *Inexact and Separate Science of Economics*.
3. Review Symposium: The Qualitative-Quantitative Disputation: Gary King, Robert O. Keohane, and Sidney Verbas's *Designing Social Inquiry*; Brady and Collier, *Rethinking Social Inquiry*.
4. Weber, " 'Objectivity' in Social Science and Social Policy."
5. Covering laws describe a model of explanation in which an event is explained by reference to another through an appeal to laws or general propositions correlating events of the type to be explained (*explananda*) with events of the type cited as its causes or conditions (*explanantia*). It was developed by Carl Hempel in 1942 and derives from Hume's doctrine that, when two events are said to be causally related, all that is meant is that they instantiate certain regularities of succession that have been repeatedly observed to hold between such events in the past.
6. Pollins, "Beyond Logical Positivism: Reframing King, Keohane, and Verba's *Designing Social Inquiry*," pp. XX.
7. Schutz, "Common-Sense and Scientific Interpretation of Human Action," p. 5.
8. Searle, *Construction of Social Reality*.
9. Coleman, *Introduction to Mathematical Sociology*.
10. Kuhn, *Structure of Scientific Revolutions*; Rouse, *Knowledge and Power*; Kratochwil, "Regimes, Interpretation, and the 'Science' of Politics."
11. Popper, *Objective Knowledge*, pp. 78–81.
12. Harré, *Varieties of Realism*, p. 6.
13. Gould, *Wonderful Life*.
14. For a thoughtful rebuttal of this argument, see Waldner, "Anti-Determinism."
15. Maher, *Betting on Theories*, p. 218, makes the same assertion about the sciences, whose history, he claims, "is a history of false theories."
16. Weber, *Methodology of the Social Sciences*, p. 60.
17. Gerring, "A Normative Turn in Political Science?"
18. Searle, *Construction of Social Reality*, pp. 27–28.

19. Weber, "Counterfactuals, Past and Future"; Lebow, "What's So Different about a Counterfactual?"
20. King, Keohane, and Verba, "The Importance of Research Designs in Political Science," p. 476.
21. Lebow and Stein, *We All Lost the Cold War*, chs. 4 and 13; Kull, *Minds at War*.
22. Diesing, *How Does Social Science Work?* p. 45.
23. Lakatos, "Falsification and the Methodology of Scientific Research Programmes," p. 137.

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Part I

Foundational Claims

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2

Evidence, Inference, and Truth as Problems of Theory Building in the Social Sciences

Friedrich V. Kratochwil

Introduction

The issue of evidence and its role in generating warranted knowledge presents us with a variety of problems. There are those who believe that warranted knowledge is the result of following a particular method. In that case progress in the social sciences consists in applying this scientific method to a subject matter at hand. To the extent that the unity of science is based on this method, which produces causal explanations by inferences from limited observations, statistical techniques such as sampling, establishing correlations, T-tests, and the like are appropriate. Consequently, in order to arrive at warranted knowledge we have to "prime" our students with these techniques.

However, only a brief reflection suggests that things are a bit more complicated. Thus, one could adhere for instance to the first tenet (the unity of science) without necessarily accepting the second part of the argument. After all, even the established sciences use differing techniques; and contrary to the hopes of epistemologists or philosopher kings, criteria for the appropriateness of particular methods are not necessarily field-independent. Consequently, we are hardly in a position to argue, as the salesman in the Midas Muffler commercial does, that "one size fits all."[1] Besides, one could also maintain that not all interesting questions in the natural as well as the social sciences are of a causal nature, as some concern "what" questions, that is, problems of constitution rather than of causality.[2] Furthermore, in order to probe heuristically fruitful connections, we often have to engage implicitly or explicitly in counterfactual reasoning, which is of particular importance when we have only few cases[3] or even just one.[4] In this case, we also cannot simply assume that the sole issue in producing warranted knowledge involves increasing the frequency of observations and/or the reduction in "variance." Finally, depending on whether we proceed largely inductively or are after "causal mechanisms"[5]or hope to identify universal covering laws, differing logical and epistemological problems arise concerning the warrants tests deliver, as the sections below will show in detail.

For the moment it is sufficient to point out that the traditional gambits of either putting ontology first and method second or opting for the opposite procedure are likely to work only by avoiding several important dilemmas. After all, the levels of ontology and epistemology are not entirely independent of each other but tightly linked through the mediating lenses of a conceptual structure dominating a discourse. Thus, even if we believe in method and follow a strict method in the production of warranted knowledge, the belief that we will arrive at the truth through a self-correcting process of conjectures and refutations, or come at least "closer" to it, might be based on a problematic metaphor. What does it mean to come closer to the truth when truth is no longer a predicate of the world but of our assertions derived from fallible and most likely false theories concerning the world?

These last remarks lead us to several wider lessons. *First*, it is for this reason (and quite contrary to the usual understanding) that burdens of proof protect orthodoxies from being challenged and that they often hide the relations of power that are part of a "discipline." However, we should also realize that these burdens of proof also prevent us from always having to start all over again when something did not turn out as predicted, or when we are challenged by alternatives. *Second*, given the failure of foundational claims of philosophy in either its substantive version (metaphysics) or in its epistemological variant after the Kantian turn, the unity of science position does not seem to be adequate, as each discipline is the competent judge of (certainly subject to critical debates and considerable controversies involving philosophical issues) its own practice. The competent speaker rather than the grammarian is, after all, the model according to which we decide which sentences are well formed and make sense. In the same vein it is the relevant community of scientists and not the "methods" person or philosopher who can pass judgment on the fruitfulness of particular avenues of research. Such a stance not only corrects the textbook version of science that has been attacked by Kuhn and historians of science for good reasons, it also corrects in important ways the idea that scientists have to share certain cognitive structures (paradigms) in order to "do" science successfully.

This leads me to the *third* wider lesson. Perhaps science is best conceived not as a theory-driven enterprise but as a practice among a set of persons who share certain techniques, such as measurement procedures, methodological commitments, and presumptions of what constitutes "good practice" in a given field.[6] In that case, neither a substantive paradigm nor the monological procedures characteristic of Popper's first cut[7] at characterizing this enterprise, that is, as a virtually automatic progress by means of self-correction, are particularly helpful descriptions. The evolution of Popper's own thinking is significant in this context. Over the years, he moved from a cognitive definition of the problem, which he inherited from the Vienna Circle (how do we distinguish sentences that make sense from those that do not?), to one that lays more stress on the social aspect of knowledge creation.[8] In his last years, Popper emphasized more and more the role of debate and radical dissent among scientists. The conception of science as a set of true atemporal and universal statements located in a (Platonic) "third world,"[9] was supplanted by the notion of science as *a practice*. In this conception, all insights are preliminary and scientific debates concern the *meaning* of tests and the allocation of the burdens of proof, rather than demonstration and "crucial experiments." Principles of ethics rather than those of logic govern this process of knowledge production.

Oddly enough, for the late Popper, *social theory* becomes a better template for understanding science than his former covering-law model of explanation, which originally was supposed to provide the criteria for social science! In this way Popper could preserve the unity of science position, although it had now an entirely different meaning from before. Scientists were not thought to require a common framework in order to communicate effectively since their communication was no longer seen as a logical demonstration or as some deictic procedure in which reality provides the Archimedean point by telling us "like it is." Rather, scientific inquiry was governed by the observance of certain ethical principles, such as fairness in the allocation of the burden of proof, and not primarily by cognitive or logical warrants.

These changes in Popper's outlook would be of minor significance—particularly in a discipline that does not seem to have noticed these changes—were they not in tune with similar arguments made by other philosophers of science on the other, and were they not of decisive importance for our conception of science and the production of warranted knowledge in general. Given the hypertrophic concern in our field with issues of method and epistemology on the one hand, and the scant recognition of the emergence of important new themes and issues in the philosophy of science on the other, it seems reasonable to devote some attention to these debates. In a way, such an investigation becomes all the more necessary as most political scientists happily take parts from Popper and Hempel, fit them with some Kuhn, enhance them with some elements of Lakatos, and take perhaps a sprinkle from Friedman, while holding on to the idea of testing against reality, an idea that is incompatible with any of these elements. The result is not only a confused debate, but it also explains the popularity of primers that circumvent all these important questions by getting down to business and allegedly training students in doing scientific research.

In this paper, I want to follow up on the idea of science as an enterprise governed by a certain ethic and draw out some of the implications of this position. I want to do this both by addressing these issues abstractly and by criticizing the conventional wisdom of doing "science," in particular political science. In this context, I shall also raise the issue of the recursivity that characterizes social relations and examine the implications of the unavailability of "social kinds" for social theory. Finally, because of my antifoundationalist stance—since I believe that neither method nor ontology, nor even the world, can be invoked as ultimate justifications—I, of course, have to answer to the charge of "relativism" and the allegedly deleterious consequences for the discipline as such, and for the young who are entering the profession. I shall do so by showing that both arguments are deeply flawed. I think that despite my antifoundationalist position, I am able to demonstrate that the production of warranted knowledge is neither an irrational process nor an enterprise where anything goes.

In order to make good on these claims, I address in the next section issues of scientific explanation and some of the criticism that have been voiced against positivism, that is, inductive empiricism and logical positivism (covering-law model) alike. I shall do so by analysis and by telling an exemplary (even if fictitious) story. In this context I also revisit some elements of the debate concerning the (alleged) differences between natural and social science and the issues of qualitative versus quantitative research. While I think that the discussion on the whole is not very illuminating, I still believe that the debate concerns real issues—not just some "muddy arcana produced by philosophers and methodologist."[10]

Section 3 is devoted to a discussion of the problem of science as a practice. Since scientific problems seldom lend themselves to unequivocal demonstration, the issue of the warrants for the assertions made turns on appraisal, that is, on *weighing* the evidence. This notion has procedural as well as qualitative elements, since it involves different interpretations and a choice of criteria that belong to various dimensions. To that extent, the procedures of courts, which have to deal with such issues, prove more instructive than the image of "progress" resulting from procedures governed by deductive or inductive logic. Furthermore, I argue that the metaphor of a court rather than that of a mere debate is appropriate as it drives home the fact that, logically speaking, undecidable questions have to be decided, on the basis of offering and rebutting "evidence," not conclusive proof. Such a strategy however raises questions of fairness and ethical standards concerning the production and utilization of evidence.

Section 4 deals with the problem of relativism and its alleged effects on the discipline. I want to show that the charge of relativism is based on a misunderstanding of the problem of truth, which is commonly identified as a search for a Cartesian *fundamentum inconcussum*. Thus even if truth can no longer be conceptualized as a property of the world but is construed as one of sentences *about* the world, it does not follow that "anything goes." Therefore, the charges of relativism—conceived as some form of irrationalism as well as its concomitant charge of the corruption of the young—turns out to be irrelevant and can be recognized for what it is: as a disciplining and largely self-serving move by which subversive or unorthodox questions are to be silenced. Precisely because the shift from a conception of science as the sum of all true sentences, to one of science as a practice, implies that the *social* aspects in the production of knowledge are of decisive importance, questions of evenhandedness in presenting the evidence and fairness in the procedures of the profession attain paramount importance. A brief summary (section 5) concludes this paper.

Logic and Explanation

Why can we not simply test our theories "against reality," letting, so to speak, nature answer our pointed questions in an experiment? The following story that, if not true, is at least well invented, as the Italians say, illustrates some of the difficulties that arise in this context.

Imagine being a spectator at one of the great controversies of the day in seventeenth century Italy concerning the nature of knowledge. There are on the one hand the Aristotelians, who can point to the tradition of their great books and the mutual support that knowledge and revelation seem to provide for each other in the dominant scholastic synthesis. On the other hand there is a *homo novus*, with some adherents, called Galileo who, far from eschewing all parts of the tradition, nevertheless insists that some of the time-honored truths, such as the perfection of heavenly bodies, the nature of the elements and the like, are mistaken. He does so on the basis of some rather odd demonstrations such as throwing things from the Leaning Tower of Pisa, alleging that such procedures are buttressing his revolutionary theses.

The format of his alleged proofs raises already serious objections from the scholars to whom the transmission and administration of knowledge is entrusted. Galileo's procedures do not fit the established canons worked out for "disputations" and thus violate in important respects the accepted practices for resolving questions of truth and falsehood. After all, if we want to arrive at warranted knowledge we have to examine the assertions carefully and in this respect our senses are not always reliable guides, as the puzzles of the skeptics in classical antiquity have already demonstrated. The oar put into water seems broken, but it would involve a belief in magic if we were to argue that it gets broken when we put it into the water but that withdrawing it makes it whole again. Is there some strange power in this element or are we justified in our belief that our senses deceive us? Whatever it is, we obviously need something going well beyond nature (*physis*) in order to adjudicate these questions. Thus, our questions of physics quickly become those of metaphysics, as it is by resort to these "first principles" that we hope to receive guidance about what to do in cases of doubt. To that extent, Galileo's calling into question these first principles is not only an act of *hybris* but is also tantamount to subverting the orderly pursuit in the production and transmission of knowledge and the legitimacy of the institutions entrusted with it.

Imagine that on the basis of the mutually shared perception of the high stakes in this controversy, both parties agree to a public demonstration. It shall take place as a proper disputation in the *Aula* of the university in which Galileo will face the protagonist of the Aristotelians. Both parties also settle on a "problem" and on the way it is supposed to be attacked, so that the necessary evidence for adjudicating the conflicting claims can be generated. The subject of the dispute shall concern the nature of elements, as here the parties are most clearly at issue. Given these facts, the weightlessness of air seems a good enough problem, as it is crucial to Aristotle's teachings concerning the *entelechy* of nature.

Galileo, certain of his new method and somewhat befuddled by the excitement that such a spectacle promises, accepts—somewhat hastily as it turns out—these conditions. The Aristotelians, equally certain of their case, agree then in turn—despite their serious misgivings—that observations (experiments) can be part of the disputation. Furthermore, both parties reach agreement on two things, which are supposed to be brought to the event in the university: the best scales one can find and a pig's bladder.

The latter shall be weighed first in an uninflated and then fully inflated state. The evidence provided by the scales could then be the long-sought proof, not only for the proposition of the Aristotelians that air has no weight, that is, that Aristotle's teachings on the elements are right, but also for the wider claim that the authority of Aristotle and his teachings can be buttressed by this demonstration.

On the set date, the disputation is in full swing and to the chagrin of Galileo and the hardly disguised satisfaction and relief of the Aristotelians the bladder weighs virtually the same before and after inflation. Dumbfounded, Galileo asks first for repetitions of the weighing, and then he repeatedly examines the scales. Unfortunately, all seems in order and the measurements virtually coincide at the various trials. But then, Galileo has a brilliant insight. The experiment *could not* lead to any different result, but the conclusion concerning the correctness of the Aristotelian theory about the weightlessness of air is nevertheless wrong. Galileo attempts to argue his case citing the phenomenon of displacement known since Archimedes. The air inside the bladder would displace precisely the same amount outside and thus receive a "lift" in direct proportion to the weight of the displaced air.

Consequently, the test, far from establishing the truth of the Aristotelian argument, actually refutes it. The Aristotelians get impatient as this argument seems to demonstrate more than a cavalier attitude toward evidence. They charge Galileo with dishonesty. What is particularly galling is that, having first argued for the experiment to establish the truth of propositions, Galileo now tries to deny its capacity to adjudicate questions of truth and falsehood. Instead he seems now simply to discard the evidence if it does not fit with his preconceived ideas. Second, bringing in the argument of displacement is in the eyes of the schoolmen doubly faulty since it was the property of another element of whose weight we are aware and that had been part of the established Aristotelian canon. Besides, tampering with the evidentiary character of the present demonstration by claiming that properties of another element also are operative in this case obviously depends on the assumption that phenomena in vastly different realms are the same. Such a metaphysical assumption, however, is question-begging since such "sameness" has not been established in the first place. On the contrary, if an analogy was possible at all, so the Aristotelians maintain, it would have to be to fire rather than to earth or water. Consequently, displacement as an explanation is irrelevant for the problem at hand. Dragging such irrelevant arguments into the present dispute is therefore not evidence for the truth of the espoused theory but rather a sign of Galileo's desperation, and it conclusively proves the charlatanry of his so-called experiments.

We, familiar with the various epistemological debates of the past few decades, have hopefully gotten the picture and the wider lessons contained in this little story. There are several points worth reflecting upon. The first is an epistemological point concerning the nature of tests. We never test against nature pure and simple because our data and the evidence we gather are always informed by theories—the famous Hempel paradox that we test, therefore, always against other theories that (might) provide competing explanations. Secondly, even the evidence available is seldom in a position to show the correctness of one or the other theory; instead, we have to transcend the problem at hand and discuss criteria at a metalevel that contains logical and metaphysical problems (natural kinds, the justifiability of logical procedures and their role in inferences). Finally, the bivalence principle of logic—either something is, or is not, the case—is frequently of little help since the issue is "undecidable" in a strict sense. Thus, if the task of science is to arrive at warranted knowledge, then the question arises on what basis can we attach such warrants as necessary, sufficient, or general, to our observational statements.

For empiricists these problems concern issues of inference, that is, our assertions are warranted if they satisfy certain logical procedures by which we can, on the basis of some limited observations, conclude that we are warranted in our beliefs that observed properties or relationships hold also in general. Of course, several important epistemological problems are raised thereby, notably the problem of induction articulated by Hume and by Kant's critique of such a solution to the puzzles of causality.

Addressing the first problem, which focuses on the question of "how much is enough," powerful statistical techniques are available to increase our confidence that sample and general populations will exhibit the same properties. The second problem raises more difficult problems, as it involves conceptual issues, which are the crucial part in generating the necessary warrants. If our problems were simply those of the first kind, theory-building could be reduced to sampling, observation, and issues of inference, even though the "normalization" of data (assigning them to certain classes) does raise questions similar to those of the second category (interpretation and the problem of natural and social kinds). These points deserve some closer examination.

As is well-known, Kant's solution to the Humean problem of induction (how many observations are enough?) was to suggest that the "constant conjunction" of causality is not an empirical problem. On the contrary, the necessity of causal laws stems from a transcendental a priori. For Kant this principle precedes any actual experience by which the senses apprehend reality. Consequently, the necessity of the causal laws we discover is neither derived from a deductive demonstration (as we would then deal only with the elaboration of conceptual tautologies), nor is it an inductive generalization based on observations. Rather it is a product of the mind organizing the empirical sense data within the categorical framework of necessity. Thus it is an inductivist illusion to believe that scientific explanations are derived from the accumulation of more and more data. To that extent, the discovery of causal laws is not simply an inference of the first kind mentioned above, it rather requires a substantial leap. Similarly, laws are also not simple summary statements concerning empirical regularities but are characterized by a counterfactual claim, that is, that something is ruled out. Even if the law is only stateable in probabilistic terms, the predicted and received values of the law have to fall within a certain range, and repeatedly obtained values outside of these parameters ought to be taken seriously, that is, as an indication that the proposed law does not hold or has to be restricted. Different from a mere generalization—for example, that "all coins in my pocket are American currency," which allows me to infer from limited observations that the same will be true for most of the other people I happen to meet in the US—a genuine law, such as that of gravity, tells me not only what happens, but also what *cannot* happen. Furthermore, although laws and explanations utilizing them cannot predict single instances—that is, they predict not what *will* happen, but *what is bound to happen*, all other things being equal—a single experiment can refute a general law, given the counterfactual dimension of laws. Here, the single experiment is not the "single case" that we encounter when we are interested in tracing a particular historical conjuncture, or when we want to know what will happen tomorrow when we are in the midst of a crisis. Rather, the experiment via the *modus tollens*[11] of logic refutes the asserted general validity of the laws, which serves as the major premise in the explanation.

These remarks also explain why dramatic consequences such as wholesale refutations are seldom associated with experiments in the actual *practice of science* and why the perspectives of the epistemologist and that of the practicing scientists diverge considerably. Since laws are valid *ceteris paribus*, the normal instinct of the practicing scientist is to assume the occurance of some interfering factor or a measurement error. Measurement techniques that cut across various theories or explanation sketches attain thereby crucial importance, as they alleviate to a certain extent the Hempel paradox.[12] They cannot, however, by themselves solve the problem of interpreting the evidence. Is, for example, the low reading for solar neutrinos a refutation of the theory concerning the nuclear reactions in the core of the sun, or are the instruments not refined enough to capture the emitted masses of particles? For scientists trained in certain procedures and the bags of tricks of good science, getting it right means often manipulating the data or explaining the failure as an anomaly rather than taking the evidence as a refutation. This practice is common and not as irrational an answer as it appears. Here, the authority of others in the field, the trust in the procedures of "science as usual," and the coherence of the explanation compared with previous work in the field, makes it understandable that failed experiments will not immediately raise eyebrows. Seen from this pragmatic angle, it is also clear that "crucial" experiments become crucial only *ex post*, that is, when we discover, on the basis of some further theoretical development, *the reason* for the observed deviation.

For the *philosopher of science* the problem is different. His/her project is not so much the way in which one does science, as it concerns the nature of the warrants attached to scientific statements. Thus, if the facts do not speak for themselves, and/or immediate sense perception cannot disclose how things really are, what is the nature of the warrants we attach to our assertions? What do we mean by "truth"? What constitutes progress and thus explains the miracle of science?

While scientists are often interested in such speculations, it is certainly not part of their professional training, nor their main concern. To that extent, the actual investigation of scientific practice, disclosed by detailed studies in the development of scientific thought in different fields, is always considerably at odds with the ideals of science and its account of progress. Apparently, warranted knowledge is generated in a way quite different from the rationalist reconstructions that underlie our textbook histories of science. The studies in the history of science by Kuhn,[13] Agassi,[14] and many others have provided evidence that progress was neither as smooth and self-correcting as assumed, nor did it develop on the basis of some paradigmatic method, which becomes intrinsic to all the existing sciences. The toolkits of the various sciences are, on the one hand, much richer and more field-dependent, and, on the other hand, it is even plausible to argue that different sciences developed only because they did not take the belief in *the* scientific method too seriously. While the processes emerging from these techniques, hunches, and actual activities can be described in terms of trial and error—and thus as following in a way a certain method—it is clear that here the concept of method is rather flexible and little more than a metaphor imparting coherence to a story that otherwise would be considerably messier.

Since the account of scientific progress based on empiricism had definitely failed, the vexing problem now consisted in finding a new demarcation criterion of science, as it could no longer be observation or confirming evidence as inductivists had originally argued. The ingenious solution suggested by Popper solved both problems.[15] In recognizing the failure of empiricism and in taking Hume's problem of induction seriously, Popper held that science does not proceed by increasing the degree of confidence that more confirming observations are held to provide. Furthermore, given that crucial theoretical terms are often unobservable, the notion that science produced the warrants for knowledge on the basis of observation became untenable. Popper argued—rather counterintuitively—that science consisted in the connection of formal logical operations with empirical data generated in experiments. To that extent, theories were not proven but had as their most important formal feature "refutability" rather than (degrees of) confirmation. Conversely, it was not simple counterevidence that proved a theory wrong, but rather the crucial experiment that—via the logical form of the *modus tollens*—attached the warrant of necessity to an assertion based on the outcome of a test.

It is for this reason that Popper emphasized the logical equivalence of explanation and prediction. If general laws and initial conditions were known, a conditional prediction could be made, while inferences as to the presumed generality of the law could be made from initial conditions and the result of the experiment. Similarly, the bounds of sense could now be demarcated by a logical criterion, while at the same time allowing for irrational elements in the actual practice of science—as when, for instance, some scientists are reported to take their inspirations from some questionable sources. Thus, although virtually no standards characterize the *psychology of discovery*, science as warranted knowledge (*explanation*) was said to be dependent on a clearly articulated logic, valid for all branches of knowledge.

One of the strongest challenges to the criteria of science came from practitioners of the social sciences, and later also from epistemologists, who emphasized the realist over the logical dimension of science. Critics, particularly in history, argued that good explanations in history needed no general laws.[16] Besides, the very ideal of an explanation scheme based on covering laws might actually be misleading. As Wendt suggests,

> Subsuming "a case" under a general law . . . is not really an explanation at all in the sense of answering why something occurred, but simply a way of saying that it is an instance of regularity. The general problem here is failing to distinguish the grounds for expecting an event to occur (being an instance of regularity) with explaining why it occurs. Causation is a relation in nature, not in logic.[17]

This realist critique of logical positivism offers little improvement because the problem of undecidability in quantum mechanics—popularized by Schrödinger's cat problem—makes it questionable as to whether placing causality back into nature again is justifiable. Scientific realists must believe that there are "natural kinds," and social scientists, who invoke scientific realism, have to maintain, in addition, that no significant distinctions between natural and social kinds exist. A brief elaboration on natural and social kinds and their role in a proper explanation seems, therefore, in order. I shall provide it in the rest of this section by criticizing Wendt's and King, Keohane, and Verba's solution for these problems. In my view, both solutions lead to a new kind of positivism, "authoritative positivism," because it is based on a "never mind argument," systematically refusing to deal with the inconsistencies of their own positions while "resolving" the issues by simple declarations, that is, "positing" it.

Consider the case of realism first. Realists not only believe that things exist independently from the observers' mind, but also suggest that things can be properly characterized by one description. Even if this does not equate with strict essentialism, there is the assumption that objects have to fall under one description that is fitting and correct, as opposed to others.[18] Thus, in somewhat ironically contrasting his own realist account with that of some postmodernists, for whom not even entities such as "dog or cat exist independent of discourse,"[19] Alex Wendt retorts, "human descriptions and or social relationships to other natural kinds have nothing to do with what makes dogs dogs."[20]

Common sense this may be, but a moment's reflection shows that the matter is a bit more complicated. After all, the commonsense object called "table" is different, depending on whether I bring it under the description of a physicist, a chemist, that of a carpenter and/or that of a user. While the latter two may be close or more or less overlap, the same is not true of the other descriptions. Similarly, what does it mean to say "there is a broom in the corner"? Why do we not see a long piece of wood and some bristles on top, held together by a wire? How do we decide which of these materials is its essence and can serve as an appropriate description? In other words, we cannot talk about the "things in themselves" but need descriptions, and these descriptions are not objective (even though they possess intersubjective validity) but embrace all types of social practices and interests that make the things into what they are or referred to. This is why we see a broom and not some agglomeration of material components. Similarly, while dog might be a name for an animal with a specific genetic code; this is not a description that is appropriate in all circumstances. Part of what defines a dog for us is not only its zoological characteristics but also a socially significant property, such as tameness, that brings dog under the description of pet. When we encounter an animal that lacks this property, we are entirely justified in calling it something different, such as dingo, despite its genetic code. The talk about natural essences is thus pretty sterile. Indeed, it is certainly no accident that the growth of science occurred only after we had given up on the idea that the world could somehow speak for itself, or that it could be apprehended from a Platonic perspective.

Wendt's treatment of social kinds raises even more difficulties. Having granted that social kinds are not natural, Wendt nevertheless argues that the same methods are applicable to produce warranted knowledge. This, however, contradicts his explicit acknowledgment that causal inferences are of a different kind when we deal with actors who reflexively understand themselves as the authors of their actions and of the structures that evolve from them. In the latter case, the theories the actors hold are part of the world, and "*thus the causal theory of reference is therefore reversed: reality is being caused by theory rather than vice versa*" (emphasis added).[21] Thus, it seems to follow that any procedure that is geared only toward observable behavior and that does not take this reflexive loop explicitly into account would fail to provide an adequate explanation of the phenomena under investigation.

In particular, institutional rules are impossible to fit in this model of causal explanation. By declaring a particular token to be currency, the effect is not describable in terms of a causal sequence of an antecedent event and a subsequent effect. Here, questions of validity, not of causality are at issue. The declaration "this is legal tender" did not cause the currency and it still does not become a causal account even though the declaration that this "x" (a piece of paper) shall be a "y" (money) brought the latter into existence.[22] Rather, the concept of money is plainly self-referential: in order to satisfy the definition of money there must be a shared code and an agreement on a token instantiating this code.

All attempts to reduce intentional accounts (which the explanations of actions and of social life require) to mere causal and/or observational statements must fail. So too does the attempt to establish inclusionary control because admitting the differences between the social and the natural world while not attending to their consequences for theory building is hardly a convincing stance. Inclusionary control is exercised when a previously excluded phenomenon is admitted into the set of puzzles of a field, provided it does not disturb the coherence of the established epistemological beliefs and its theoretical core assumptions. The main goal is then to demonstrate the derivative character of the recalcitrant phenomenon by reducing it to some other "more basic" factors that are susceptible to conventional methods of investigation. For example, King, Keohane, and Verba, in dealing with actions and their interpretation, explicitly acknowledge the distinction between a twitch (a bodily movement) and a wink (a signal), which allegedly is best researched as a causal hypothesis, but they maintain that no adjustment in the methodology used has to be made if one is interested in explaining winks.

> If the eyelid contraction were a wink, the causal effect would be positive; if it were only a twitch, the causal effect would be zero. If we decided to estimate this causal effect (and thus find out whether it was a wink or a twitch), all the problems of inference discussed at length in the rest of this book would need to be understood, if we were to arrive at the best inference with respect to the interpretation of the observed behaviour.
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> If what we interpret as winks were actually involuntary twitches, our attempts to derive causal inferences about eyelid contraction on the basis of a theory of voluntary social interaction would be routinely unsuccessful: we would not be able to generalize, and we would know it.[23]

In this way, KKV argue that "the logic of scientific inference is unsurpassed"[24] even for evaluating the hypothesis that winking instead of twitching has taken place. But since this is simply stated and no further evidence or proof is provided, we have to take this assertion apparently on the basis of the standing of the authors within the field. In actuality, however, the problem is more complicated. Even the observational world of KKV cannot be reduced to only two exclusive states of the world since other possibilities exist. For example, person A might twitch, while B thinks this is a wink, or B might deliberately ignore the winking, pretending to see a twitch. Precisely because performances can misfire or may be misperceived, or the misperception might even be cynically manipulated, no overall pattern might emerge. But even if we came up with some correlation, it is hard to imagine what they would mean. Besides, what are we able to derive from mere observation if we do not also take the interpretations of the parties themselves, their explanations and excuses into account? We will never know what was the case, whether something might have been a misfire, a manipulation, a twitch or an intentional wink.

Even in the still rather uncomplicated world of signaling, the causal links between the sign and the meaning or message can be broken and thus causal hypotheses without taking the state of mind of the actors themselves into account are not particularly useful. As soon as we leave this rather simple world of signals and engage in the explanation of communicative acts, our analysis will become more complicated and the simple bivalence principle of logic used for causal imputations is of little help in deciding. Indeed, it is one of the surprising phenomena in the social sciences, political science in particular, that they seem indebted to a conception of science that is rather dramatically at odds with science as an actual practice, and to epistemological convictions that are hardly tenable any more. It will be the task of the next two sections to elaborate on these points. The next section is devoted to the conception of science as a social activity in which undecidable questions get vetted according to some procedures for the fair allocation of proofs. Because of these elements, the metaphor of a court is more illuminating than the conception of a logical demonstration. The fourth section addresses then the problem of relativism allegedly flowing from an antifoundationalist stance that makes truth not a property of the world, but of assertions about the world.

Science as Method, as Debate, as Market, or as Court

In our discussion above, we distinguished several meanings of science. At the most general level we conceived of it as some form of warranted knowledge. But in examining the particular warrants, we saw that at least two types of warrants seem to exist: one satisfying an epistemological ideal that makes patterns of inference or the logic of explanation the crucial element, the other is more pragmatically oriented and common among practicing scientists. Usually, they readily acknowledge not only that the epistemological ideal cannot be reached but also that even such an ideal as a regulative idea does not serve its purposes well. The reason is that most scientific statements cannot be subjected to the test by bivalence principle of logic (*tertium non datur*), as concrete problems frequently remain undecidable. Consequently, other criteria have to be adduced to provide the warrants. This circumstance not only explains why most scientists do not feel much affinity to the epistemological discussions that are supposedly reconstructing their work, it also has important implications for a more adequate understanding of science as a social activity by which this type of knowledge is produced. Both problems deserve a brief discussion.

The scientific enterprise even in a field such as physics is badly represented by the textbook view of science in which scientists work on neatly decomposed sets of puzzles that are all part of the theory that is being tested. True, people work on different problems, and their work and the best evidence available for corroborating their conjectures might bear out theoretical constructs. But when a particular theory fails to explain a phenomenon such as superconductivity (as is the case with Schrödinger's wave mechanics), it hardly leads to debates that are so heated that most practitioners feel they cannot continue with their work. The hope is rather that in ploughing ahead some link with certain theories can be established at some future point that, in turn, might give further indications as to what to attack next.

It is the network character of interrelated models, techniques, concepts rather than an overall design that neatly fits every scientific investigation into a whole, that buffers disagreements and allows for "science as usual." To that extent, even the proposed extension of the positivist logic of discovery to a position of "sophisticated falsificationism," as suggested by

Lakatos,[25] appears too limited. As Diesing has pointed out in examining the debates of the Popperian school,[26] not only have scientists continued to use refuted theories, even more disturbing for a logical positivist point of view is the fact that *already refuted theories turned out to be right later*, and that they provided fresh and heuristically fruitful new starts. The upshot is that neither can the progress of science be mapped as a simple linear process characterized by near automatic self-correction, nor can the individual scientists take a simple rule such as refutation as their yardstick for deciding whether or not to abandon a given theory or even research program.

Should disagreement arise among the practitioners, however, the procedures by which scientists try to persuade one another and attempt to decide what constitutes an anomaly, and what is supposed to be taken as a refutation, are far from the ideal type of a compelling demonstration. Not only will there be considerable debate, there will also be several criteria by which various protagonists will attempt to eliminate alternatives and show that the weight of the evidence favors one theory while at the same time discrediting those that supported one or several other alternative explanations. To that extent, the *assignment of burdens of proof and of presumptions* provides better metaphors for capturing the process by which warranted knowledge is produced, than the idea of a demonstration by experiment.

Another point deserves to be borne in mind. Ever since the debates about the nature of paradigms and the historians' challenge to aprioristic conceptions of science, the social dimension of knowledge production has come into sharp focus as of late.[27] The focus of this work is entirely different from traditional sociology of knowledge, which mapped the correlation of ideas with certain social strata, or even as an instantiation of the social phenomenon of "false consciousness," or a Gramscian "hegemony." Rather the issue here is one of the communications among scientists and their respective roles within the process of knowledge generation. This problem, in turn, raises questions about the proper division of labor among theoretical and experimental efforts and of the influence of the experimental apparatus on the results.

With regards to the first issue, the division of tasks between theory and experiment, even Popper had to recognize the importance of the "communal aspect" of knowledge production. Not the individual scientist but rather other members of the scientific community were supposed to keep the enterprise honest by doing the testing that individual scientists might forsake for understandable psychological reasons. If the demonstrative ideal is recognized as inappropriate, it is necessary to explain how else the enterprise of scientific practice can be conceptualized. For this, two analogies offer themselves as an answer. On the one hand, such an explanation entails either an appeal to the metaphor of the market, or to some more authoritative decision process, in which the competing claims can be adjudicated. Confronted with potentially interminable debates, there has to be a way to come to some conclusions that are legitimate. Therefore, the second possibility is that of a "court," meaning that judicial proceedings offer themselves as templates for explaining disagreements and progress. Both metaphors stress the social character of the enterprise rather than the logic of demonstrations. They each provide for the skeptic's objections to the "rational" development of scientific learning, while offering a coherent account of the development of knowledge and blunting the force of his argument. Also both come to more or less determinate outcomes, although the selection mechanism is different in each case. In the market, the selection amounts to a simple elimination of unwanted theories, which fall by the wayside due to lack of demand. In the court metaphor, authoritative decisions mediate the competing claims—and the finality of the judgment is legitimate largely because of procedural criteria (*pro veritate habetur*)—while leaving open the option of reexamining a case if new evidence justifies such an action.

The market metaphor is popular in that it links the scientific enterprise to liberal convictions of the benign effect of competition. In addition, when coupled with the notion of evolution, such an explanation takes care of the paradox that truth can then be seen as "absolute," while at the same time as a revisable step in an evolutionary process driven by the same mechanics as conjectures and refutations suggest. Nevertheless, I think the court metaphor might be more apt for conceptual as well as empirical reasons. The *conceptual reasons* have to do with the dubious argument of evolution that in this version has a definite *telos:* the arrival at the truth. But nature and its evolutionary processes have no goal, there is no preordained destiny analogous to the notion of coming nearer to the truth that provides the persuasive force to the above argument. The best illustration of the conceptual problems is provided by Popper's own attempt to elaborate a theory of *verisimilitude*, attempting to mediate the tension between a scientific conception of evolution as a process of various trials and errors, and evolution as the embodiment of the hope of the enlightenment project to arrive at the truth.[28] It is ironic that he did not see that these two concepts are not the same and that a similar teleology was at work in his conception of coming closer and closer to the truth, as in those theories of history that he so aptly criticized.

The more empirical reasons have to do with the treatment of the activities of the scientific community as an analogon to a competitive market. It miscasts the problem of modern science in important respects. Theoretical and empirical scientists are supposed to behave like buyers and sellers and thereby produce beneficial results. As in microeconomic theory, this assumes all scientists too to be essentially alike in that they all possess the same resources and tools (technology being external to the firm). But as we have seen, scientific activity is not necessarily mediated by some market mechanism, as experimental data may be accumulated without much guidance of a theory and models might proliferate without much data.

The last point leads back to the second problem mentioned above: the role of the scientific apparatus in modern science that makes its relegation to some unproblematic background knowledge a doubtful characterization. Not only might there be only a weak nexus that unites the experimentalist with his theoretical counterpart, in addition the parties may possess essentially different skills and endowments that no longer resemble those of a period even a few generations ago when the required skills both in mathematics and in the handling of laboratory equipment was *relatively simple and available to virtually all members of the community*. Modern science has become an industry dependent on teams with a highly specialized division of labour. As Ziman points out for contemporary particle research physics,

> The gargantuan scale of such an instrument has two consequences for scientific epistemology. In the first place, the physical complexity of the apparatus, involving the harmonious interaction of many separate elements—beam magnets, vacuum systems, accelerating voltages, target assemblies, spark chambers, pulse height analysers etc., etc.—demands elaborate rationality of design, beyond the grasp of *any one person*. The results of the experiment are irretrievably embedded in the design theory of the systems and all its parts, whose correct working must be taken for granted. In the end, we cannot say whether the data are derived primarily from the "external world" or from the theories they are supposed to be validating or falsifying.
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> The other epistemological effect of the trend towards Big Science is in the reproducibility of experimental results, which is their ultimate guarantee of consensuality [sic!] and reliability. The number of such instruments in the world is very limited, and each is heavily committed to what is hoped to be an interesting scientific program with novel results. There is thus a tendency to avoid what is called "wasteful duplication of research" and many experiments must wait long before they can be confirmed by an independent repetition.[29]

It is clear that under such circumstances decisions about experiments need to be justified; and since such justifications involve bets on the future in the light of weighing evidence for and against certain alternatives, the procedure of selecting among the experiments has to be fair. Under such circumstances the decision-making processes take on very quickly the form of *quasi-judicial* proceedings instead of mere debates. Evidence is provided and rebutted according to certain procedural constraints, and decisions have usually to be justified to the scientific community if not to the public at large. That these changes in scientific practices are not merely a result of the resource-constraint connected with "big science" is evident in their exemplary force under much less demanding circumstances. External evaluations, blind reviews, prohibitions to judge when there is a potential for a conflict of interest and all such instances are all part of the scientific enterprise, all designed to ensure the fairness of the procedures by which we arrive at warranted knowledge. Already in the 1970s, U.S. scientists considered the possibility of establishing a formal court that would render judgments in cases of scientific controversies that had public policy implications. This, it was hoped, would bring some finality and legitimacy to scientific arguments about which no consensus could be reached.[30]

This is indeed a far cry from the notion of demonstration by a test that establishes the truth of a proposition through some tests or logical inferences, or of a "market" of ideas in which truth emerges from untrammeled competition! In rethinking science as a practice and the problem of its success, Rom Harré, therefore, speaks of the need for a new approach that is no longer indebted to the idea of progress or to the naive notion that we arrive at truth by testing against the world:

> The account of science to be set out . . . is based on the thought that science is not a logically coherent body of knowledge in the strict, unforgiving sense of the philosopher's high definition, but a cluster of material and cognitive practices, carried on within a distinctive moral order, whose characteristic is the trust that obtains among its members and should obtain between that community and the larger lay community with which it is interdependent . . .
>
> The idea of a philosophical study of the moral order that obtains in the scientific community is not new. But the significance of admitting it to the center of our interest has rarely been acknowledged. I hope to show that science has a special status, not because it is a sure way of producing truths and avoiding falsehood, but it is a communal practice of a community with a remarkable and rigid morality . . . Science is not just a cluster of material and cognitive practices but it is a moral achievement as well.[31]

Interestingly enough, such a perspective in a way dethrones science as a paradigm of warranted knowledge since it undermines the foundational claims of the scientific method that it is able to solve problems on the basis of a demonstration of how the world really is. It also suggests a deep-seated change, both in the practice of science and in the acceptance of scientific statements as self-justifying instruments. The judicialization of the scientific enterprise might be an attractive alternative to the wrangling among competing schools of thought and provide a (temporary) solution for the two competing ideals inherent in the process of producing warranted knowledge: certainty and legitimacy. Given the uncertainties intrinsic to scientific theories, it might be better to lose a round in such a court, while preserving the right to reopen the case later. In a way, both conflicting goals can be achieved, since finality as well as the open-ended nature of scientific inquiry are equally preserved.

While such a gambit might be preferable to a situation in which authority is diffused among concrete communities that either ignore or deadlock one another, it certainly is no *panacea* against either polemics or even abuses, which the Galileo example seemed to suggest. Given the absence of ultimate foundations legitimizing the verdict by the deictic gesture beyond all appeals, "this is how it is" is simply not available to us. As a matter of fact, shifting the metaphor from that of a foundation to one of a process of approximation mediates the tensions and allows us to go on, while at the same time symbolizing the fact that we will never arrive. But even here the image of progress and approximation is paralleled by the sneaking suspicion that we might know more and more about less and less, and that the "world out there" might not provide us with the Archimedean point from which we can gather how things really are, that our procedures are not neutral and unproblematic instruments disclosing reality but are part of a disciplinary structure in which default positions not only entail possibilities for resolving uncertainties and conflicts but also, *mirabile dictu*, enshrine power. At this point, the charge of relativism or even nihilism will be made, and it is to this problematic that I want to turn in the next section.

Truth, Nihilism, and the Power of Metaphors

If the above discussion has shown anything then it is that controversies concerning warranted knowledge raise existential issues. They might start out as simple design questions for inquiry, but (barring some naive empiricism) the question of how we can know what we know leads to several paradoxes. On the one hand, the project seems to undermine itself, as no ultimate foundations for ending all controversies can be discovered. Thus, truth is not the universal silencer of questions about what the world is like and where we are going. Instead, these answers seem to depend in turn on a variety of other criteria, including pragmatic ones (it works!). However, such a criterion is not foundational either, since the attendant validity claims can be defeated: "Yes, it works, but is it true?" What would we do if "it" worked no longer? How could we even conceive of a solution if we do not "know"? Does then utility presupposes truth? This is the logical paradox within the first experiential paradox.

On the other hand—facing now a second experiential paradox—we seem to be able to go on, even when we have no ultimate answers to these existential questions. Whether we sink into desperation because we no longer know how the world is, or whether we celebrate this predicament à la Nietzsche, seems to depend on other factors than "the world out there." In any case, it follows neither conceptually nor empirically that those who are critical of foundationalist projects are nihilists in the sense that they argue that there are no standards, or that anything goes. To that extent, the visceral responses to the criticism of foundationalist accounts and of truth as the mirror of the world show what they are: existential anxieties engendered by the loss of orientation. The cure, however, for such exaggerated fears is not to hold on to truths when confronted with the evidence that they have lost their indubitable character. Instead, we should, first of all, clarify what is at stake. Secondly, in coming to terms with our loss of "rooted-ness," we could inquire into the reasons why we felt such comfort in the first place, *despite* the fact that we have known for a long time that the proposed foundations could not hold and that our hopes would have to be disappointed. The first task requires a conceptual clarification of the problem of truth and its ontological foundations, the second an inquiry into foundational metaphors. Why have metaphors such as the ground, the circle, the chain, or the goal played such an important role in foundational accounts?

In attacking the first issue it should be clear that truth cannot be a property of the world. Things or entities cannot be true, only assertion about them can! Much confusion could be avoided if we were clear about this distinction between the existence of something and its description. Barring again a lapse into naive empiricism, it should also be obvious that questions of truth raise, therefore, issues of justification concerning the descriptions we use. This justification cannot be reduced to some deictic procedures (like pointing to it) but is internal to the system of symbols by which we try to orient ourselves within the world. After all, encountering the world without any descriptions would put us in a quandary since we would not know how to name what we encounter. Going to the thing directly, apprehending it without any description seems an impossible task. Furthermore, in staying within the symbolic universe we notice that the conceptual distinction between "appearance" and "reality" usually organizes our justificatory efforts. But we should be clear that this involves no simple matching operation in which the adequacy of our concepts is assessed in terms of their correspondence to the facts. Consider in this context two examples provided by Toulmin. The first, concerning the ascertainment whether an object is "brown", and the second whether a table is "really" solid or not.

On the face of it, brown as a color and property of objects is on the same footing as red, blue, or black. However, given the principles on which physicists base the classification of color (the wavelength of electromagnetic radiation), only red, yellow, orange, green, blue, indigo, and violet "exist." They designate different parts of the visible spectrum. Thus, it makes sense in both everyday language and in physics to ask whether something is really red, but since "there is no brown part of the spectrum, over brown the corresponding question (in everyday language) cannot arise,"[32] and appeals to reality, independent of the framework, do not make much sense. Similarly, there is no point in asking whether a table is really solid or not, unless one specifies the framework within which the reality/appearance distinction provides the conceptual tools for an explanation. The physicist might deny the solidity of the object since

> that might lead him to suppose, mistakenly, that nothing, not even a beam of a-rays, will go through . . . But it is wrong, or at best, whimsical of him to say "That table is not solid at all; as rays go through it, so it must be full of holes"—if he imagines that the result of his experiments discredit the everyday concept of "solidity."[33]
>
> Reality, in any particular mode of reasoning, must be understood as what (for the purposes of this kind of argument) is relevant and "mere appearance," and what (for these purposes) is irrelevant.

Since the purposes for which we elicit explanations differ, there can be no universal standard of relevance and thus no universal explanation and thus no universal truth that could tell us how things really are. But this is a far cry from asserting that this leads to "relativism" in the sense of a nihilistic "anything goes."

Given that we are dealing here with issues in which we have to move constantly between different languages and frameworks, it is not surprising that our vocabulary often gets mixed up and we no longer know what we are arguing. Thus we might see that truth claims have nothing to do with reality but still hold on to the idea that our concepts somehow have to match reality and that we can find this out by tests.[34] That this tension leads occasionally to rather confused arguments concerning issues of explanation, representation, and the nature of truth claims is not surprising, although it also proves that *dadaist* performances are not the exclusive preserve of postmodernists or those who do not believe in the world "out there."

> A model is a simplification of and approximation to some aspects of the world. Models are never literally "true" or "false," although good models abstract only the "right" features of reality they represent.
>
> For example, consider a six-inch toy model of an airplane made of plastic and glue. This model is a small fraction of the size of the real airplane, has no moving parts, cannot fly and has no contents. None of us would confuse this model with the real thing; asking whether any aspect of the model is

> true is like asking whether the model who sat for Leonardo Da Vinci's Mona Lisa really had such a beguiling smile. Even if she did, we would not expect Leonardo's picture to be an exact representation of anyone, whether the actual model or the Virgin Mary, any more than we would expect an airplane model fully to reflect all features of an aircraft.[35]

The criticisms above should have driven home the point why truth is neither a property of nature nor of the world, and why we cannot test it against reality. In other words, we cannot ask nature a question that it could answer without using a language. But it is equally senseless to think that we are dealing with different "realities" that are created by our different interest and modes of inquiry. As Toulmin reminds us:

> When visualizing the results of science we find it useful to have a mental picture of reality as a kind of a vast box. The nature of whose "contents" it is the calling of the scientist to identify; and in its place this picture may be helpful enough. But what are we to do if we are presented with several "separate realities"? To demand to visualize them all the way in which we do physical reality is to ask us to imagine several separate boxes all occupying exactly the same space—that is, several separate, unseparate boxes—and this demand is a self-contradictory one before which the mind can only boggle.

What becomes obvious here is that these perplexities were created by the extensions of metaphors, in this case a simple spatial one that originally made sense, as it allowed us to orient ourselves. The temptation of falling back into the framework of common sense is difficult to resist, even when the extensions fail to provide meaning as the process ends in self-contradiction. In addition, since some metaphors are so basic to our understanding, we feel particular discomfort when we have to abandon them. After all, people thought that it could not be true that the earth is a sphere instead of a disc because they feared this meant they could fall off. Similarly, we feel that we have to lose our bearing when we are no longer on solid ground, when the world is no longer out there.

One way of dealing with this loss of the metaphysical comfort is to inquire into the reasons why we felt such comfort in the first place, why certain metaphors provided such persuasive power, despite the realization that the hopes built upon them will be dashed. Consider in this context the notion of coming "nearer" to the truth while never quite arriving there. The example of using successive polygons for determining the content of a circle gives some plausibility to this stance. The problem is, however, that in the case of the circle we do have the perimeter given, while the entire problem of scientific knowledge is that there is no way in which we can know its limits. Progress in the latter case consists in being able to ask questions, which were previously unthinkable, and there is no fixed perimeter we can use to determine whether we have come closer. To that extent, the notion of knowledge as a procedure analogous to exhausting the interior of the circle no longer makes much sense.

Furthermore, the metaphor of the circle seems doubly problematic. When we focus on the perimeter instead of the interior, it gives rise to the fear that we might get caught in a circle that turns vicious. Consequently, recursivity raises suspicions and the generative capacity of the circle metaphor has to give way to that of a *chain*, or that of a *foundation*. The chain metaphor and the ground metaphor suggest an absolute beginning or (end). Both rely on rudimentary spatial experiences for their suggestive power. The ground metaphor implies that our knowledge is structured like a building, in which the upper levels receive support from lower ones. To that extent the image of justified belief (or warranted knowledge) resembles the structure of a pyramid shown in figure 2.1.

The fact that we need ever more foundations cuts, however, decisively against the idea of parsimony and shows that there is something fishy about the need for independent support of each justified belief. Besides, we would have to know virtually the entire world in order to support just one belief! This cannot be right and at some point we have to accept some basic beliefs that are not justified by further beliefs, but by some immediate perceptions. Besides, without changing the architectural metaphor, we suddenly realize that *not all beliefs need independent support*: the image of an arch already suggests that it is the mutual support of the parts that often carries the weight.

The metaphor of the chain, on the other hand, supports our naive version of scientific progress as a sequence of discoveries of cause and effect that is

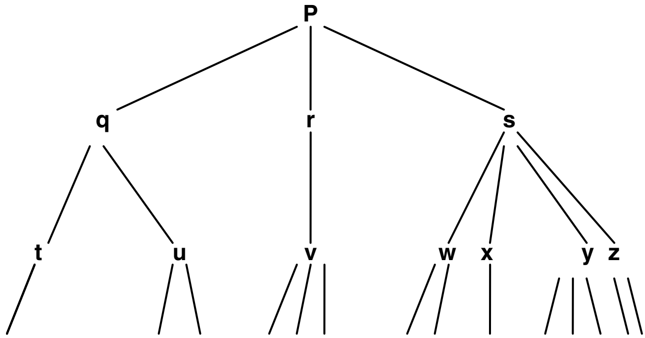

Figure 2.1 Structure of Independently Supported Beliefs.[36]

somehow anchored in reality. This image accords well with some textbook version of the path of science. But as the physicist John Ziman pointed out, this metaphor is seriously misleading as an account of scientific progress:

> This point is of the greatest importance, since it explains so much of the strange sense of unreality that scientists feel when they read books on the philosophy of science. It is abundantly obvious that the overall structure of scientific knowledge is of many, many dimensions . . . The initial path to a new discovery may be apparently one-dimensional with no more reliable authority than a simple causal chain. But the strategy of research is to seek alternative routes, from other starting-points, to the same spot until the discovery has been incorporated unequivocally into the scientific map.[37]

Note that by choosing the metaphor of a map rather than that of a circle, or that of a pyramid, the fears engendered by the previous metaphor vanish. In addition, former fears are actually turned into strengths! Mutual support is no longer seen as vicious, but *as proof for superiority*! The map metaphor, however, might not be the most fruitful either. After all, it presupposes a land, virtually pre-given and unchanging, that has to be discovered and mapped. It is, therefore, still beholden to the notion of representation. But if in at least some part of science the aim is not the discovery of something preexisting, as some experiments in high-energy physics suggest, but an endeavor in which "the process to be observed has never occurred before in the history of the Universe: God himself is waiting to see what will happen!"[38] then notions of representational accuracy quickly lose their usefulness. Similarly, when we accept the thought that we cannot get in between the things and our description of them, but that, in true constructivist fashion, their ontology depends on the purposes and practices embedded in our concepts, then we need no longer hold on to the "thing in itself" as an anchor. We are finally free to address the question of how we do go about our inquiries.

Perhaps the metaphor of a game of Scrabble provides a better image. We begin with a concept that makes certain combinations possible. In crisscrossing, we can "go on," and our additions are justified by the mutual support of the already existing words and concepts. Sometimes, we cannot proceed as our attempts to continue are stymied. Then we begin somewhere else and might, by circuitous routes, reach again some known terrain. Potentially there are innumerable moves, and no two games are the same, since moves at different times will have different consequences. On the other hand, none of them is free in the sense that anything can happen. But none of them could have been predicted by the "view from nowhere" as everything depends on the words that are put in place, the site that is chosen for extending the game, and the time. Without making too much of this metaphor, because like all metaphors it eventually breaks down, I think, this one captures our predicament in generating knowledge and lets us continue our searches and get on with our lives without false hopes and without despair.

Conclusion

This paper, concerned with the role of evidence in the explanation of natural and social phenomena, necessarily had to touch on a whole host of issues informing the present debate on inference and research design in the discipline. I forsook the dogmatic conception of science that serves as the background for a set of recommendations in various primers. Instead, this article critically examined the presuppositions of this concept of science and used conceptual as well as empirical evidence to rebut the often erroneous conclusion that warranted knowledge can be generated on the basis of applying some method or epistemological principles alone to a range of phenomena. This task involved not only an argument against the primacy of epistemology, it also made it necessary to show that science cannot be adequately described as a sum of true statements about the world. Particularly in the social sciences, where the issue of the recursivity creates difficult problems of appraisal, the belief in the unity of method is unjustified. Furthermore, as the epistemological discussions of the last two decades have shown, science is best characterized as a communal practice. To that extent the reduction of the scientific enterprise to problems of demonstration and inference is not useful. Aside from the issue of interpretation that precedes those of inference and proofs, the social element plays a significant part in the communication among practitioners, particularly when the criteria of logic are not helpful in solving the puzzles that scientists address.

As I argued above, turning away from seemingly foundational principles that depend on a clear criterion for deciding true and false statements to an interpretation of decidability as a problem of fair procedure provides a more fitting approach. This amounts to a recognition that the finality and legitimacy of a judgment coincide only in logic but these criteria point into different directions not only in practical matters but also in science. Allocating the burdens of proof fairly is, therefore, not as simple as sometimes suggested by distinguishing between naive and sophisticated falsificationism, or progressive and degenerative problem shifts in a research program.

To that extent, apodictic statements that scientific study depends on "publicly available evidence and some possibility of falsification"[39] is true but trivial, as it does not address the issues of judgment and of the criteria that shall govern the weighing of the evidence and of the discharge of responsibility either in sticking to one's (possibly refuted) theory, or in abandoning it. It is here that the metaphor of a court has a certain heuristic power since debates among the practitioners cannot go on forever but decisions about research and funding have to be made in the absence of conclusive proofs. The core controversy is virtually never about tests but about what they tell us, and about assigning strategic burdens of proof. As we all know, it is usually the default position that invites controversies of a quite different and serious kind rather than problems of evidence or of the formal adequacy of some model or theory.[40] The emphasis on ethics in research points precisely to these difficulties. It brings to the fore the silent presuppositions and invites us to reflect critically upon them, establishing the importance of practical reason and judgment. In a way, these considerations also provide the strongest possible rationale for pluralism, not as the second best but as the most promising strategy for producing warranted knowledge.

Notes

1. This point was eloquently made years ago by an adherent of the "behavioural revolution"; see Abraham Kaplan, *The Conduct of Inquiry*: *Methodology for Behavioral Science* (Scranton, PA: Chandler, 1964).
2. On this point see Alexander Wendt, *Social Theory of International Politics* (Cambridge, England: Cambridge University Press, 1999), particularly chap. 2.
3. For an illuminating discussion of how different understandings of what a case is influences the conduct and results of our inquiry see Charles Ragin and Howard Becker, eds., *What is a Case*?: *Exploring the Foundations of Social Inquiry* (Cambridge, England: Cambridge University Press, 1992).
4. On counterfactual reasoning and its status in the social sciences see Philip Tetlock and Aaron Belkin, eds., *Counterfactual Thought Experiments in World Politics* (Princeton, NJ: Princeton University Press, 1996); see also Geoffrey Hawthorn, *Plausible Worlds*: *Possibility and Understanding in History and the Social Sciences* (Cambridge, England: Cambridge University Press, 1991).
5. On causal mechanisms see Mario Bunge, "Mechanism and Explanation," *Philosophy of the Social Sciences*, Vol. 27 (December 1997): 410–65.
6. See Lichbach's contribution to this volume.
7. See Karl Popper, *The Logic of Scientific Discovery* (New York: Harper, 1968).
8. The first modification occurred when Popper had to realize that scientists did not proceed as he had argued in his "historical reconstruction" on the basis of conjectures and refutations requiring correction as soon as a hypothesis had

been falsified. Given the tendency that scientist, as everybody else, do not want to be proven wrong—and thus tend to attribute the falsification of a hypothetical prediction to violations of the conditions contained in the *ceteris paribus* clause, to measurement errors, or they even try to fix the results through ad hoc adjustments in scope definitions of the underlying law—in the second version of his theory of science it became the task of *other scientists* to keep the individual investigator honest. The second change resulted from Popper's realization of the plasticity of the world and from his acknowledgement of the importance of language and of conceptual structures. See Karl Popper, "Of Clouds and Clocks" in Karl Popper, *Objective Knowledge* (New York: Oxford University Press, 1972), chap. 6. For an assessment of the importance of this shift for the social sciences see Gabriel Almond, "Clouds, Clocks, and the Study of World Politics," *World Politics*, Vol. 29 (1977): 496–522. Although Popper, somewhat inconsistent with some of his tenets, never gave up on the notion of a mirror image theory of truth, he attempted to elaborate a theory of verisimilitude in order to fix the problems of his epistemology. See his essays "Two Faces of Common Sense" and "Philosophical Comments on Tarski´s Theory of Truth," both in Karl Popper, *Objective Knowledge* (Oxford: Clarendon, 1972) chaps. 2 and 9 respectively.

9. See Popper's essay "Epistemology without a Knowing Subject," in Karl Popper, *Objective Knowledge*, op. cit., chap. 3.
10. This is the supposedly apt characterization of the issues underlying the debate by one international relations specialist who proudly disclaims any real acquaintance with the relevant literature but who nevertheless has produced a primer in order save his students from perplexity. See Stephen van Evera, *Guide to Methods for Students of Political Science* (Ithaca, NY: Cornell University Press, 1997).
11. If p then not q; q, therefore not p.
12. The Hempel paradox consists in the insight that data in a way are not theory-independent; while at the same time they are used to test theories. Here the link is empirically of course loose as otherwise we would be involved in tautologies. Similar problems arise in the context of "incommensurability," i.e., claims that theories state different facts. Rather than assuming that therefore no debate and proof is possible, data might mean different things in different theoretical frames but as with different languages, we usually are able to "translate" or establish some ways of getting on with our research and conversations, even if there is not a close match between our concepts and their supporting evidence.
13. See Thomas Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. (Chicago: University of Chicago Press, 1962). See also his *The Essential Tension* (Chicago: University of Chicago Press, 1977).
14. Joseph Agassi, *Science in Flux*, "Boston Studies in the Philosophy of Science," No. 28 (Dordrecht, Netherlands: Reidel, 1975).
15. See Karl Popper, *The Logic of Scientific Discovery* (New York: Harper, 1968); Karl Popper, *Conjectures and Refutations* (New York: Harper, 1965).

16. To name only a few, William Dray, *Laws and Explanations in History* (London: Oxford University Press, 1957); Maurice Mandelbaum, *The Anatomy of Historical Knowledge* (Baltimore, MD: Johns Hopkins University Press, 1977); Fredrick Olafson, *The Dialectic of Action* (Chicago: University of Chicago Press, 1979); Rex Martin, *Historical Explanation* (Ithaca, NY: Cornell University Press, 1977).
17. Alexander Wendt, *Social Theory of International Politics* (Cambridge, England: Cambridge University Press, 1999), p. 81.
18. For such a definition of essence see for example Andrew Sayer, "Essentialism, Social Constructionism and Beyond," *Sociological Review,* Vol. 45, No. 3 (1997): 453–87.
19. Wendt, *Social Theory*, op. cit., p. 49.
20. Ibid., p. 73.
21. Ibid., p. 76.
22. For a further discussion of this problem see John Searle, *The Construction of Social Reality* (London: Penguin, 1995), particularly chap. 2.
23. Gary King, Robert Keohane, and Sidney Verba, *Designing Social Inquiry*: *Scientific Inference in Qualitative Research* (Princeton, NJ: Princeton University Press, 1994), p. 40.
24. Ibid., p. 39.
25. See Imre Lakatos, "Falsificationism and the Methodology of Scientific Research Programs" in Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge, England: Cambridge University Press, 1970), pp. 91–196.
26. Paul Diesing, *How Social Science Works* (Pittsburgh: Pittsburgh University Press, 1991), chap. 2.
27. See Steve Fuller, *Social Epistemology* (Bloomington-Indianapolis: Indiana University Press, 1991).
28. See Karl Popper, *The Poverty of Historicism* (New York: Harper, 1957).
29. John Ziman, *Reliable Knowledge: An Exploration of the Grounds for Belief in Science* (Cambridge, England: Cambridge University Press, 1991), pp. 62–63.
30. See the discussion in Richard Gaskins, *Burdens of Proof in Modern Discourse* (New Haven, CT: Yale University Press, 1992), particularly chap. 5.
31. Rom Harré, *Varieties of Realism*; *A Rationale for the Natural Sciences* (Oxford: Blackwell, 1986), p. 6.
32. Stephen Toulmin, *Reason in Ethics* (Cambridge, England: Cambridge University Press, 1970), p. 107.
33. Ibid., p. 113.
34. See, e.g., Wendt's argument that since theories are always tested against other theories, not the world, while holding elsewhere that "science is successful because it gradually brings our theoretical understanding into conformity with the deep structure of the world out there" (pp. 58, 65, respectively).
35. King, Keohane, and Verba, *Designing Social Inquiry*, p. 49.

36. For a further discussion of the problems of such a “foundationalist” account see Susan Haack, *Evidence and Inquiry*: *Towards Reconstruction in Epistemology* (Oxford: Basil Blackwell, 1993), pp. 24–52.
37. Ziman, *Reliable Knowledge*, p. 84.
38. Ibid., p. 62.
39. Wendt, *Social Theory*, p. 373.
40. For an interesting treatment of this problem in law and science see Richard Gaskins, *Burdens of Proof in Modern Discourse* (New Haven, CT: Yale University Press, 1992).

3

The Limits of Interpreting Evidence

Ted Hopf

"Do not interpretations belong to God?"

Genesis 40:8

Mainstream political science and interpretivism have little to do with each other, intellectually and professionally speaking. It is thought that the concern of the mainstream for causal inferences from a large sample of a representative population in order to assess the comparative merits of hypotheses deduced from competitive theories has no room for the interpretivist concern with the ethnographic and discursive recovery of intersubjective realities. One could ask, what would have happened in a conversation about political science at a cocktail party between Clifford Geertz, Pierre Bourdieu, and Michel Foucault and Robert Keohane, Gary King, and Sidney Verba? Mutual incomprehension, at best? Or a retreat to a more innocuous topic, at worst?

This paper offers to start a conversation at the party. The argument that follows is quite simple, if perhaps not uncontroversial. First, interpretivist scholarship imports mainstream conventions when it makes its arguments and should make these positivistic methods more explicitly salient to make its own work more compelling, and more relevant to the mainstream. Second, the mainstream cannot make the epistemological claims it does by adopting the methodological techniques it offers to fix the problems of making causal inferences. It needs to recognize that interpretivist epistemology has it right, and therefore, the mainstream must dramatically reduce the truth claims it makes based on the kind of work it does. In sum,

1. Interpretivism borrows methodological conventions from the mainstream but does so without acknowledging it, or denies it is doing so, or, still worse, differentiates itself from the mainstream on the very grounds of not using the methods it in fact uses.
2. The mainstream cannot make the truth claims it makes because it shares enough interpretivist epistemology to end up in fundamental contradiction with itself. Since, positivistic methodological devices cannot fill the holes left by interpretivist epistemological insights, the mainstream must confess that the knowledge it produces is far more relative than often admitted.[1]

This chapter begins by noting some fundamental building blocks that both interpretivism and the mainstream share. I then turn to the different ways in which the two approaches find and consider evidence. The two approaches part company over what evidence can actually say about the world, that is, epistemological claims. Throughout the ensuing discussion, I point out where interprevists use commonplace social science techniques and how such techniques actually improve their work, whether they like it, or acknowledge it, or deny it, or not. At the same time, I indicate the inability of mainstream techniques to resolve the dilemmas they themselves honestly recognize. I conclude with an argument about interpretivist generalization.

Common Ground

Hans-Georg Gadamer has written that the natural and human sciences are separated *not* "by a difference in method, but a difference in the aims of knowledge."[2] In this section, I want to concentrate on that "technical commonsense" as a critical shared foundation for both approaches. What Gadamer and I are stressing here is a certain methodological unity between the mainstream and interpretivism, separated by epistemological difference.

Despite the obvious differences, many scholars have pointed out that nonpositivist, even antipositivist approaches to the human sciences share important features with their avowed Other. Craig Calhoun, for example, has criticized those who conflate positivism in general with the nineteenth-century school and the Vienna Circle, while ignoring the inside-out critiques offered by those such as Karl Popper.[3] Others have pointed out that many postpositivists in fact borrow ideas from positivism.[4]

Among the shared methodological conventions that constitute the common ground are

a. clear differentiation of premises from conclusions
b. acknowledgment that sampling strategies matter
c. recognition that some standards of validation must be established for the sources of evidence used
d. differentiation of causes from correlations
e. recognition that the specter of spuriousness haunts all correlations
f. acceptance of syllogistic and deductive logic
g. belief in the need for the contestability of findings

Without belaboring the obvious, let me elaborate shortly on each of these seven principles. The first entails a commitment to avoid tautologies: do not make circular arguments, whereby one's conclusions are in fact also the basis for the stipulation upon which the conclusion is based. While the mainstream and interpretivism have very different ideas about what a sample should be, and how to constitute one, they do agree that sampling matters, that is, that the composition of a sample affects the truth claims coming out the other end. A similar gloss applies to the issue of sources. While interpretivists might entertain a far wider array of sources than the mainstream, they would acknowledge that sources, like samples, have effects on the kinds of knowledge claims one can make at the end of the exercise.

Some interpretivists have a problem with even using the word causality, since it violates an antifoundationalist epistemology and smacks of Humean positivism. Instead, they prefer to say that social phenomena of interest are "constituted" by the copresence of each other. But this is not germane to the shared belief that correlations of phenomena are merely suggestive of some possible causal/constitutive relationship, the latter that can either *never* be demonstrated or at the very least requires a great deal more evidence than simple covariation in a temporal sequence.

Closely related to the causation-correlation distinction is the common awareness that failure to unpack the causal story risks spuriousness, the attribution of causal power to a correlated variable whose presence itself is the product of some other "master" variable or is incidentally present while another, still unobserved, variable is doing all the causal work.

It almost goes without saying that both approaches accept the rudiments of deductive logic, such that, for example, saying women are more likely to do the milking in this village means that men are less likely, that women of any age are included in the category, unless explicitly excluded. In other words, the rules of rhetoric regulate conversation.

Finally, the mainstream believes in falsifiability, that is, the statement of hypotheses such that evidence can be, in principle, gathered to disconfirm the affirmative claim; so, too, does interpretivism, but they are loath to call it that, as again it smacks of positivism. But interpretivists are unaware of how ironic this position is: they, of all people, believe in the *intrinsic* contestability of all their truth claims because of their antifoundationalist skeptical epistemology. Ergo, interpretivism believes in falsifiability, in spades.

All this common ground can be found in virtually any interpretivist work, if only implicitly. But let me give some examples from two of the most important interpretivist social theorists of the last 50 years, the late Pierre Bourdieu and Clifford Geertz.

The use of crucial cases is well established in the mainstream as a way of demonstrating the validity of one's findings in the dreaded small-n environment.[5] The capacity of one's explanation, theory, or variable to explain variance in some outcome independently situated from the dependent variable of interest is also understood by the mainstream as a powerful way to show the validity of that variable's causal power. Process-tracing has been established as perhaps the predominant mainstream way of showing that one's causal claims are in fact causal, rather than correlative.[6] Another predominant mainstream technique is John Stuart Mills's method of difference/similarity, essentially holding constant some variables of interest so as to investigate the possible causal power of those variables one does allow to vary.

Bourdieu's work exemplifies each of these techniques. In arguing that religious power could be best measured by the outcomes it creates in nonreligious areas of economic and political life,[7] Bourdieu employed both the logic of crucial cases and the maxim that the farther a theory's predicted outcomes are from its source, the more powerful that theory. He also suggested a technique that looks like process-tracing to guard against potential spurious associations between his associated variables of social practice and habitus. He warns against permitting superficial correlation to supplant close causal reconstruction and advises that one must reconstruct the causal interrelationships among the multiple possible origins of a social practice.[8]

Clifford Geertz, consistent with the mainstream belief that validity increases with the variety of outcomes explicable with a single variable, writes that interpretivism wants better explanations and an explanation that can explain multiple behaviors.[9] He, in effect, is acknowledging that there are criteria for comparing one explanation to another and for deciding which is superior. Moreover, one of those criteria is that a single explanation should be able to explain a variety of outcomes over a range of domains. And Geertz uses the mainstream method of difference to demonstrate the superiority of his own interpretation.[10] For example, he claims that the Balinese cockfight is a metaphor for status hierarchies in the village. To support the claim, he points out that people from the same faction never bet against a cock from that faction; they bet for cocks in related kinship groups; and they support cocks from their village against those from other villages.[11] What he is showing here is that despite the probable outcome of a cockfight, across different types of competition, villagers still behave the same way. It is a mainstream technique to establish covariation between one's causal variables, while controlling for alternative explanations.

In sum, many mainstream methods unite interpretivism with its putative defining Other. This is important to keep in mind while sifting through their yawning epistemological differences, because this common ground is the first set of organizing principles upon which the ensuing conversation can progress.

Epistemological Difference

In picking up a random collegiate dictionary and turning to the entry for "evidence," one finds two very different definitions. The first refers to it as "an outward sign" or indication, while the second calls evidence "something that furnishes proof" or testimony. One could roughly say that the first definition nicely captures interpretivism's epistemological skepticism, and indeed modesty, about the capacity of evidence to demonstrate more than a relative working truth, or "situational certainty," as so beautifully put by Karl Popper.[12] The second definition, on the other hand, speaks to the mainstream's confidence in the capacity of evidence to prove some hypothesis correct.

In the three sections that follow, I consider what the two approaches consider to be evidence, how and where they find it, and what they think they can do with it, what it means.

What is Evidence?

Many interpretivists take seriously the injunction that all theorizing is destruction. While there have been periodic attempts by mainstream political scientists[13] to remind their colleagues of this epistemological truth, it is a systematic matter of concern to interpretivists. When George Marcus, for example, describes his ethnographic ideal, it is a reproduction of evidence from the field that demands "hermeneutic sensitivity," or the rendering of the "actual uncertainty of life," rather than a "false unity." In other words, we observers must not impose an artificial order on the observed, regardless of how much more easily our theories may work as a consequence.[14]

The problem, from the interpretivist perspective, is how to "translate" what is observed into the language of the scholarly research without changing the meaning of what is experienced by the subjects themselves. Translation, too, is an act of destruction. It is the production of evidence at the expense of the subjects' lived reality. The ideal form of interpretivist evidence would be the overheard conversation, published without, ironically, any interpretation.[15]

Alfred Schutz offered a clear phenomenological standpoint on this issue, arguing that "there are no such thing as facts, pure and simple. All facts are from the outset facts selected from a universal context by the activities of our mind. They are, therefore, always interpreted facts, either looked at as detached from their context by an artificial abstraction [as in the mainstream] or facts considered in their particular setting [as by interpretivists.]"[16] Neither the mainstream nor interpretivists can even describe, let alone explain and theorize, without simultaneously importing meaning. The difference is that the latter accepts this as an epistemological given, not a corrupting intrusion to be ignored, explained away, or controlled for.

Interpretivist understandings of what evidence is relates directly to the relationship between the observer and her subject. The mainstream, of course, unless there is out and out bias or prejudice, ignores this issue entirely, assuming that any observer can be sufficiently objective and distant so as to not affect the meaning of the evidence. As Timothy McKeown observed in his review of *Designing Social Inquiry*, "the question of assessing the adequacy of operationalizations—the defining of empirical referents to theoretical concepts—seems to fall outside the scope of their inquiry."[17]

In figure 3.1, I illustrate the relationships between the observer and her subject, the subject's prevailing social structure, and the observer's reflexivity, her relationship to herself.

Beginning with the mainstream, we see that the scholar gathers evidence by directly observing the subject and the structuralist does the same by observing the social structure that presumably acts on the subject. These two approaches share both an absence of reflexivity and an interest in the subject's own characterizations of his reality.[18] Phenomenology, interpretivism, and hermeneutics, on the other hand, all share an interest in the subject's own story. But then they treat evidence differently from that point. First, intepretivists may or may not situate themselves in their own account of the evidence, whereas hermeneuticians are consistently reflexivist. Moreover, hermeneuticians further may treat the social structure as a

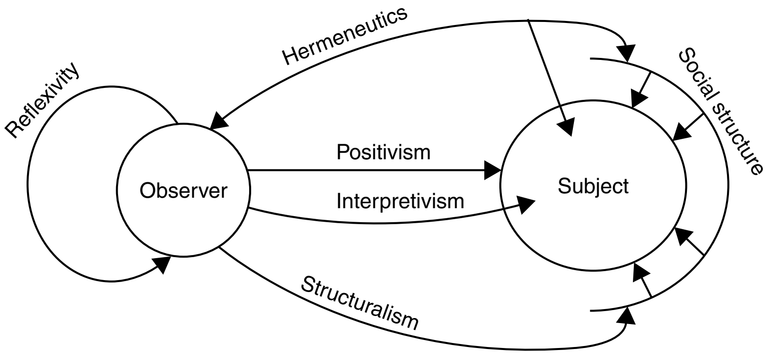

Figure 3.1 Epistemological Positions.

Table 3.1 Interpretivist Positions

		Actor-Observer Interaction Effects or Reflexivity?	
		No	***Yes***
	Observer	Positivism/ Objectivism	Hermeneutics of Suspicion
Whose Perspective?			
	Subject	Phenomenology Psychology Decision Making	Hermeneutics of Trust

more or less autonomous piece of evidence and may or may not become evidence themselves, in terms of changing their own selves through the act of interpreting the subject.

Interpretivist positions on the relationship between the observer, or the theorist, and the subject, or the evidence, reveal whose reality matters, the theorist's or the subject's. For both the mainstream and structuralists the answer is most often the theorist's. For phenomenologists and mainstream scholars who use various modes of decision analysis, the answer is the subject's. But only interpretivists and hermeneuticians answer unequivocally both.

In table 3.1, I reconfigure the taxonomy from a different perspective.

The first move of the interpretivist is phenomenological. He "must inductively gather evidence that allows [him] to evaluate the problem as the agent saw it."[19] This is not significantly different from applying a garden variety decision-making approach to a social science problem. One might ask, why do we not just rely on asking people what they mean when they say and do what they do? The answer is the assumption that unobserved/unobservable social structures exist in all social contexts, such that the meaning of an individual's actions and words are not his to control or interpret. Once spoken or done, a social practice becomes the property of the audience.[20] The observer's task is to reproduce as evidence both the subject's actions and the encompassing social structure. As Charles Taylor wrote, "we can never . . . have a clear view of the implications of what we say at any moment."[21] Imagine, for example, asking a white woman why she moved her handbag from one side of her body to the other when a group of black men were approaching on the other side of the sidewalk. She, of course, would deny her action was racist, and yet her action unintentionally reproduced a particular racial identity for those young black men: they are potentially dangerous felons.

There remains the inductive reconstruction of the social structure that provides meanings for the actions and words of the actor. Unlike structuralists, who collect evidence for the existence of such structures from patterns of behavioral outcomes in the field, interpretivists rely on reconstructing the intersubjective meaning of that structure for the subjects of interest. In other words, evidence of social structure is gathered through the actions of subjects, but not without their own understandings of those actions. A subject's intentions or preferences or interests, for example, are never understood by interpretivists as emerging directly from the subject. There must always be an accompanying account of the relevant sociohistorical context. Evidence does not consist of the actor's words alone.[22]

Finally, the author must include himself in any account of the subjects.[23] This does not mean trying to accomplish the unachievable aim of bracketing the meaning of one's own presence or interpretation. A scholar cannot understand the historical horizon of a subject by trying to abandon her own. Instead, she must comprehend the evidence through her own conceptions, while simultaneously realizing the perspective of her subject.[24] This is not merely the interpreter grasping the subjective intention of the actor, but it is a fusion of the author's horizon with that of the interpreter. Gadamer described the process as placing yourself in the position of the other not to generate empathy or to better apply one's own criteria, but rather to "attain a higher universality that overcomes, not only our own particularity, but also that of the other." This is the fusion of horizons of observer and subject.[25] The interpretivist assumption of intertexuality calls for an understanding of an actor's story in its complete relationship to all other stories available in a site, as well as in its relationship to the observer.

Pierre Bourdieu nicely summarizes both the complexity of the hermeneutic demand and its compromise position between objectivism and relativism: Unlike the mainstream, "objects of knowledge are constructed, not passively recorded . . . ." One must not abandon "the active aspect of apprehending the world by reducing knowledge to a mere recording. To do this, one has to situate oneself within 'the real activity as such' . . . One has to escape from the realism of structure, to which objectivism necessarily leads when it hypostasizes those relations by treating them as realities already constituted outside of the history of the group—without falling back into subjectivism . . . To do this, one has to return to practice, to the site . . . of structures and habitus."[26]

If we were to redraw figure 3.1 as a hierarchy, rather than a circular space, we could specify the mainstream and structuralism first, with their uncomplicated direct access to the evidence of the subject and structure, respectively. See table 3.2.

Table 3.2 What is Evidence?

a.	The Mainstream	What is observed
b.	Structuralists	Observed social structures
c.	Phenomenologists	Subjects' observations of self and structures
d.	Interpretivists	**c.** + observations of structures and subjects via each
e.	Hermeneuticians	**d.** + observer's relationship with subjects and structures

Clearly, that which a scholar must find in order to count as evidence becomes increasingly demanding as one moves away from the mainstream. Indeed, if one achieves the hermeneutic ideal, the hermeneutic circle, the observer will revise as well her own horizon, her own collection of theoretical priors or expectations in light of exposure to, and dialogue with, the subject.[27] Evidence, from this perspective, is gathered only when one's own perspective has been changed in some perspectival fusion with the subject. There is a deep ontological assumption underlying the interpretivist insistence on listening to the subject and reconstructing her intersubjective social structures. This is the belief that meaningful evidence can rarely be found at the surfaces of speech and action, but rather it must be surfaced by the observer, with the help of the subject.

How and Where to Find Evidence

The mainstream wishes to test theories, interpretivism to generate understandings. In other words, the mainstream tries to find evidence that would allow it to differentiate between the truth claims of competing statements about some class of outcomes, while interpretivism is looking for evidence that would allow it to provide a satisfying understanding of some social phenomenon. I will return below to the issue of standards for judging the quality of understandings, but for the moment I focus on issues of research design.

The mainstream has elaborated rules for case selection. These include random, stratified random, or strategic sampling of some population. Cases should be chosen so as to ensure variation in the value of the independent or dependent variables, à la John Stuart Mill's method of difference. The greater the number of cases analyzed, the better. The more variety across time, space, and culture in which the cases are situated, the better. These, and many more, may be found in *Designing Social Inquiry*. But all these rules are largely irrelevant to interpretivist concerns. In fact, they suspect the mainstream has already operated on a case if it is able to identify it as a case. In other words, while the mainstream treats the knowledge of what constitutes a case of something as self-evident, the interpretivist would argue that case-selection already prejudges facts not yet in evidence.

To bring the point home, let us take the example of crucially hard, or crucially easy, cases that assume that some cases of a phenomenon produce more valid inferences than others. This convention is recommended especially if the number of cases available for analysis is small.[28] Interpretivists would find this mainstream position to be especially jarring because it so openly demands that the scholar substitute his own ideas, whether or not derived from the theory being tested, for the meaning of the case for its subjects.

This kind of thinking is apparent in James Caporaso's cogent critique of *Designing Social Inquiry*.[29] Caporaso suggests that case selection can be used to enhance the a priori validity of evidence if it is designed so that whatever confirmatory results emerge are "counterintuitive." The interpretivist observation is that what or who constitutes counterintuitiveness then becomes the unobserved measure of validity. Caporaso cites the work of Lisa Martin and Kathryn Sikkink in which they demonstrated "counterintuitively," and so with greater a priori validity, that Guatemala resisted international pressure to adhere to human rights norms, but Argentina did not. Why is this possibly counterintuititive? Because it violates "our" presumption that bigger, stronger countries are, well, bigger and stronger. So, weak Guatemala's resistance and strong Argentina's acquiescence is surprising. But does this not really only show that the a priori theory of power used by Caporaso, and ascribed to us, is inadequate, that a theory of power that included domestic institutions and social movements would not have been so surprised with Martin and Sikkink's results, and so the theory they were testing would not have been adjudged to have survived a particularly onerous test?[30]

What the mainstream considers to be a trivial exercise in specifying what constitutes a case—in this case, a crucially hard case—in which a theory's implications can be tested, interpretivists see as an already executed act of theorization. That case has become a case of something through an act of interpretation by the scholar; its meaning was not pre-given or self-evident. Since mere description requires interpretation, understanding the meaning of actions and words in some context only trebles that demand. This is why evidence is not self-evident but must be made meaningful through interpretation.[31]

What Evidence Can and Cannot Mean

Gadamer distinguished between "being able to explain a fact completely through deriving all its conditions; through calculating it from the givenness of all its conditions," these being the ideal of natural scientific knowledge, and the far more modest claims of "interpretation, which we always presume to be no more than an approximation: only an attempt, plausible and fruitful, but clearly never definitive."[32]

The mainstream interest in testing theories means that even "problematic data," or evidence, should be made the best of by the scholar.[33] In other words, the observer must try to make the evidence meaningful to his theory, rather than allowing the evidence to have the meaning it has within the social context within which it is situated. If the theory cannot be tested on that data, so be it, the interpretivist would say. One wonders just how "bad," or refractory, or disobedient evidence has to be before the mainstream scholar would abandon his efforts to make that data matter for his theory.

In what follows, I establish what interpretivism believes to be the limits of evidence, the maximum possibilities that can follow legitimately from scholarly observation. On a significant number of crucial epistemological issues, interpretivism does not share the mainstream's confidence.

a. Interpretivists, unlike the mainstream, do not believe it is possible to establish complete analytical or theoretical control over the subject being studied.

b. Interpretivists further reverse the default drive of the mainstream, the latter believing that until a subject is revealed as meaningfully different from other like-labeled subjects, they are theoretically identical and may be treated as such. Interpretivists, quite to the contrary, assume meaningful difference among subjects until demonstrated otherwise.[34]

c. Interpretivists treat anomalies as evidence to inform a truer account, not an aberration to be explained away within the existing theory being tested.

d. Interpretivists, unlike the mainstream, do not expect to gather evidence that would allow them to make predictions about the future behavior of subjects, even within a case, let alone about a class of subjects across the universe. An interpretivist prediction would be very narrow, confined within a case to a limited period of time, and involving very few actions and actors. Needless to say, interpretivists do not understand how evidence can ever be used to establish universalisms or covering laws that are subsequently used to ground some blanket generalization about any social phenomena of nontrivial proportions.

Evidence Is Out of Control

The kind of control the mainstream feels it must establish for its evidence to do its work is considered impossible by interpretivism. It is both logically and practically impossible to control for all alternative accounts of some outcome. It is illogical because of the inability of any observer to imagine all possible causes of some event. It is impractical because even if one could imagine all possible causes, it would be impossible to police the borders of one's evidence so effectively as to guarantee that outcomes were not seeping in from outside the perimeter.[35] That is, spuriousness can never be eliminated, no matter what the controls imposed are. These are external threats to control. But even within the defense perimeter there are unavoidable challenges to control, because it is impossible to guarantee that the evidence whose meaning is established at T is in fact the same evidence at T+1. Unless the field is frozen in time and space, the meaning of one's evidence may have altered sufficiently to make the previous confident assertion of a causal relation nonsensical. Finally, it is obvious that establishing control over even one case is sufficiently difficult. Imagine what a strain it places on the validity of evidence across time and space and cases. This is one critical reason why interpretivists are most hesitant to ever generalize across cases and see even within-case generalizations to be problematic.

Interpretivists recognize control as a problem that is very hard to solve, and so they reduce their claims for evidence accordingly. It mostly seems as if the mainstream sees control as a technical problem and so does not adjust its aspirations and expectations for its evidence accordingly.[36] The mainstream approach to this problem does not satisfy interpretivist standards. One aspect of mainstream control is to assume that a given social phenomenon, say democracy, has only one significant meaning. This allows the observer to treat cases that are dramatically different on a host of dimensions, both seemingly unrelated to democracy and constitutive of an alternative definition of the same, as if they do not matter, as if they are controlled for. We usually think of control as ensuring the absence of some competing cause from the field; in the case of democracy, it is done definitionally, by making democracy mean only one authorized thing. What this does is reverse the logic accepted in analytical philosophy: instead of meaning preceding what is a fact; facts precede and often imply meaning.[37]

The Interpretivist Default of Difference

The secret to mainstream confidence in using evidence to test theories and to develop generalizations for use in other places and times is its implicit assumption of identicality. It assumes that things, concepts, and individuals are meaningfully identical across time and space. Interpretivists believe that gathering evidence from a social context necessarily involves navigating through a thick and complex array of social actions, practices, and identities. The mainstream, on the contrary, sees complexity as something to control or manage, rather than as something to accept or accommodate.[38] The mainstream argues that theory elides complexity. They are right. But interpretivists decry this result; they do not celebrate it.

Far from accepting the default of identicality, interpretivism assumes the principle of difference. The mainstream, to make its epistemological claims stick, would have to defend the logically impossible assumption that there is nothing meaningfully unique about any of the subjects and actions about which they theorize. Votes are votes. Wars are wars. Power is power. Everywhere and for all time.[39] Mainstream scholars have offered a fix to this problem, suggesting that while attaining "unit homogeneity" is often impossible, understanding the degree of unavoidable heterogeneity helps us estimate the degree of uncertainty or likely biases to be attributed to our inferences.[40]

From an interpretivist point of view, this misstates the problem and so offers a misguided solution. The theoretically meaningful heterogeneity that so flummoxes the mainstream does not often present itself as such. If one knew which untheorized features of the field were relevant to understanding social outcomes, one presumably would have accounted for them in the a priori theory. Instead, an interpretivist expects to find heterogeneity in her evidence, but this heterogeneity is not a problem to be solved, but rather evidence to be interpreted.[41]

The mainstream counsels that scholars, "where possible," should homogenize their subjects only after "attaining an understanding of the richness of the history and culture."[42] Interpretivism advises precisely the opposite and shifts the burden of proof accordingly. Until an observer demonstrates that these subjects are indeed meaningfully the same, in the same context, let alone across cases, they should not be treated as meaningfully identical.[43] Interpretivism and the mainstream split on this foundational assumption. The former want to include potentially meaningful difference; the mainstream wants to exclude as much of this as it can feasibly get away with.

As Hilary Putnam has observed, the objective is to discover whether natural kinds, that is, a set of entities that shares a common causal structure and whose behavior therefore can be predicted on the basis of the laws that the govern the behavior of such entities, can be found in the social world. In other words, do social kinds exist? Can revolutions, wars, and democracies, for instance, be treated for theoretical purposes as social kinds?[44] The answer depends both on whether all main causal effects are captured within the definition or model of the concept being employed and on whether those captured effects are properly specified. The interpretivist solution to this problem is that there is no solution, since it is impossible, logically, empirically, practically, and theoretically, to specify a causal model that could possibly account for everything that is excluded outside its boundaries, and everything that is purportedly causally effective within those boundaries.[45]

But this does not mean that interpretevists reject the existence of evidence that can be treated as equivalent or identical within some boundaries. In fact, it is ironic that interpretivism objects to mainstream homogenization without often explicitly acknowledging that interpretivism itself uses a form of homogenization to make its theory work. Intersubjectivity is really a way of homogenizing various parts of society through common webs of meaning. If people understand the same social practices in the same way, is this not an example of sameness, of homogeneity? If so, then does interpretivism not assume that homogeneity exists, at least in some social domains? And does this not demand from interpretivism a methodological technique to deal with both difference and similarity?

The Interpretivist Treatment of Anomalies: Meaningful Difference

The separation between the mainstream and interpretivism on homogenization and difference is easily observed when we consider how each approach handles the anomaly, a piece of evidence that confounds its theory or understanding of some phenomenon. Mainstream theories are sticky, in the Lakatosian or Kuhnian sense. Single pieces of contrary evidence do not and must not be used to change the observer's account of the situation.[46] But, ironically, and perhaps only in this particular manner, interpretivism is truer to Popperian positivism than the mainstream that adopts the practical and sociological defenses of Lakatos and Kuhn. Popper offers an account of falsification that resembles the workings of an open market or a Darwinian evolutionary system. Explanations fail when they are no longer competitive, according to the falsification criteria established above, relative to the other available explanations. In other words, one's understandings are far more responsive to new evidence, no matter how anomalous, than would be the case in either a Lakatosian or Kuhnian world. The product could be called a relative working truth, or, as Popper called it, a "situational certainty."[47] It is a relative truth, in that its validity exists only in comparison to other possible accounts. It is a "working truth" in the sense that its validity is acknowledged to be a pragmatic convention, rather than an absolute fact, but one that can be accepted in daily practice until something more satisfactory comes along. It is a "truth" in the sense that it is believed to be contextually valid, within the prior two constraints.

Interpretivism is doubly Popperian. It resists both theoretical priors, sociological and institutional consensus, and motivated and cognitive impulsion toward premature closure. It treats every anomaly as if it is, at least potentially, meaningful.[48] The mainstream, on the contrary, rejects treating every disconfirming instance as a serious challenge and, instead, appears to don the protective belts offered by a Lakatosian research program, or the Kuhnian mantle of normal science toiling on within a settled paradigm.

Hypothetically, let us assume we test the democratic peace in a large-N comparative case-study format. The results confirm that institutional constraints matter, but the glaring empirical anomaly is that citizens rarely, if ever, express their opinions on foreign and national security policy issues in democracies. This clearly undermines the theory, falsifying one of its empirical links. But one possible mainstream response is to be even more strongly convinced of the theory's validity. After all, even without strong and active participation by citizens on the issue of concern, democracy still works! An interpretivist would never let such an anomaly pass. The question would then ask why do the states we call democracies not fight each other, despite the fact that they are not meaningful democracies. What counts as confirmatory evidence for the mainstream only signals the need for a broader and deeper understanding from the interpretivist.

McKeown's critical review of King, Keohane, and Verba used the work of Arend Lijphart to make a similar point. Lijphart, in a single case study of the Netherlands, developed a seemingly aberrant account of how social cleavages operate within a pluralist society, an anomaly whose serious acknowledgment led to the retheorization of settled truths in comparative democratic theory.[49]

Interpretivist Generalization: Meaningful Identicality

Perhaps the most powerful attack on generalization has come from Theodoro Adorno and Max Horkheimer. They argued that generalization was impossible and "all that is possible is purely destructive resistance to any attempt to confine the world within a single principle that purports to endow it with identi[cali]ty." Homogenization "makes the dissimilar comparable by reducing it to abstract quantities. That which does not reduce to numbers becomes an illusion; modern positivism writes it off as literature. Abstraction creates a herd, rather than unique beings."[50]

Given its skepticism about the ability to control any domain of theorizing about the social world, and its presumptively high regard for the unique and the contextual, interpretivism's position on the ability to make predictions about other cases, or to generalize its local understanding to more remote locales, should be manifest. Since domains vary in unknowable ways, it is futile to make statements that are expected to apply in other "identical" cases. In other words, differences that may not even be discoverable by an external observer may make variables incommensurable across contexts and so render efforts at generalization hopeless.[51] As Brian Fay has pointed out, to the extent an explanation is nomological in character, it explains kinds of events by showing that they are instances of generally recurring patterns of a lawful type. This position renders illegitimate those theories that assert that social phenomena are uniquely related to the culture in which they occur.[52]

Related to discovering that there are differences in social phenomena that make meaningful identicality, and hence, universal and general truth claims and covering laws, impossible, is the issue of incommensurability. Barbara Herrnstein Smith lays out the parameters of this problem very nicely.

> The question is whether, as is traditionally maintained, rival theories are always ultimately measurable against a common standard . . . so that . . . their divergent claims may be compared and the superior ones chosen accordingly; or if, . . . there are conditions under which . . . conflicting theories cannot be measured or compared that way: when, for example, they assume radically divergent but equally credible conceptions of the universe [not so important to our concerns], or, . . . when part of what divides the parties is how to understand the standards (*truth, rationality, evidence*) . . . by which the merits of their divergent theories could be measured . . . .[53]

The latter is our concern.[54]

So, for example, are Russian and German democracy commensurable? Sure, each has elections, legislatures, a federal system, among other things. But the argument here is that looking for what these two countries share already prejudges the evidence that will be gathered and the meaning that will be given to it and ensures the calculated elision of all difference that does not rotate along the axis given by the theory's priors. The mainstream's search for the common and the uncommon only along the common's dimensions, according to interpretivism, destroys the very meaning of that which is being observed. An interpretivist observation is impossible for the mainstream because any observation must serve as support for, or disconfirmation of, a theory. It cannot become an observation until and unless it can be used within a relevant theoretical context.[55]

Perhaps the sensible position here between the twin tyrannies of identicality and difference is a deep skepticism of replicability accompanied by the intuition that some social phenomena are more replicable than others.[56] The capacity to predict should be treated as a variable that never approaches unity, may be zero, but should always be regarded with great doubt. With respect to prediction in particular, it should be stressed that interpretivism accepts bounded predictions, predictions about future outcomes that are deeply contextualized in understandings of the social milieux that produced the present. The logic of prediction remains the same in the mainstream and interpretivism. The critical difference is in the expectations of the range, scope, durability, and ambition of such predictions. Interpretivists will always defend more modest claims than the mainstream. If one believes that boundaries are hard to specify, and that the social world is hard to control, then one must be careful in making claims about future outcomes.

The distinction could be drawn between open and closed systems. Interpretivists believe that all systems or fields of inquiry are necessarily open; one cannot pretend, or devise methodological tools, to reverse or manage this reality.[57] The mainstream commits a two-step error by stipulating a closed context within which to theorize. First, they improbably claim the capacity to control for all possible factors that might be affecting the subjects in which they are interested, and then, second, they take the results from that presumably closed environment and claim that they can be obtained in myriad other environments, presumably no more closed than the originating first.[58]

But is this interpretivist commitment to difference sustainable? Is every social actor and action in fact *meaningfully* unique? Logic itself, and certainly the collection of shared methodological conventions identified earlier would not be possible in the world of the unique, as one would need a separate account for every context, relationship, and interaction, of which there would be infinite variety. One can see that the uniqueness celebrated by interpretivism is just as logically vulnerable as the identicality of the mainstream.[59] Interpretivism, however, raises an issue of fundamental importance here that methodological expediency cannot address. Its interrogation of the mainstream's homogenizing impulse compels us to think about just how to guard against adducing "unit homogeneity" where meaningful difference exists. How much of the world is really noncomparable?

How is it possible to determine whether evidence is the same, or not? There appear to be three mainstream answers to this question: available applicable theory, best empirical observation, and logical deduction from an unrelated model. But notice that all of these choices, in themselves, entail the most consequential assumption, that meaningful identicality already exists. If not, one could not conclude that a theory already exists that is applicable, that the empirical observations already performed are sufficient, and that a model operating effectively in another domain is relevant in this one. In other words, the mainstream necessarily prejudges evidence as similar. Interpretivism, most significantly, has precisely the opposite default. It assumes that identicality must be demonstrated to exist, and that the search for evidence must first entail the establishment, in this case, of the relevant meanings of that which is being assessed. But if interpretivists were to be consistent with their views of the unique, they would not be able to say anything general even within their cases. They nevertheless regularly do make such claims within their cases, suggesting an implicit assumption that generalization is somehow possible within a case. And if this is true, the tyranny of the unique does not operate at all times, in all contexts. In other words, interpretivists themselves are not consistent followers of their own creed.

Clifford Geertz, for example, when discussing his methods in understanding the meanings of the Balinese cockfight, stressed that he frequently generalized within the case, that is, he observed actions by some, and subsequently attributed the same meaning to their behavior and to others who engaged in the "same" behavior.[60] Foucault has written that his methodology of "archaeology provides the principle of the discourse's articulation over a chain of successive events."[61] Bourdieu, not unlike Foucault, sees the habitus as a formation that "produces practices which tend to reproduce regularities."[62] According to Bourdieu, "I believe it is possible to enter into the singularity of an object without renouncing the ambition of drawing out universal propositions."[63]

The fact that Geertz, Foucault, and Bourdieu do generalize within cases establishes that they do in fact consider evidence to be identical, at least within a case. But how do they determine this if they are deprived of the mainstream techniques? The answer is they establish boundaries for meaning, such that evidence means the same thing within the specified boundaries. But their answer, as I argue below, would be immeasurably strengthened if they would explicitly use some mainstream methodology, as well.

Meaningful evidence for interpretivists is intersubjective, intertextual evidence. That social theorists recognize the boundaries of meaningful action to be a major issue may be inferred with just how many different terms are used by so many different theorists to describe this space: Bourdieu's habitus, Foucault's discursive formation, Althusser's *problematique*, Wittgenstein's lifeworld, Heidegger's clearing, Benjamin's imaginary, to name but a few. These are the spaces within which interpretivists of all stripes implicitly assume that meaningful generalizations about human actions and speech may be defensibly made. A feature common to all is the fact that it is understood that whatever is happening within those boundaries is potential evidence, and what is outside of each of them is not.

Rabinow and Sullivan, for example, claim that the intelligibility of any action requires reference to its larger context, "a cultural world," as they call it.[64] But just where are the boundaries to this injunction? How would anyone know when to stop looking for relevant connections and interrelationships with others beyond the last established boundary? On what basis are boundaries to be established?[65] As Alexander rightly observed, reconstruction of a "total sociohistorical context is a chimera."[66] One can neither observe all that one "should' observe as meaningful within any context, nor can one specify precisely where that context "should" stop. Gadamer stipulates that the first principle of hermeneutics is to "admit the endlessness of the task. To imagine that one might ever attain full illumination as to motives or interests in questions is to imagine something impossible." So, what to do? One can "clarify what lies at the basis of our interests as far as possible. Only then are we in a position to understand the statements with which we are concerned, precisely insofar as we recognize our own questions in them."[67] So, intersubjective boundaries, such as they are, only softly delimit the terrain between observer and subject, but they do not even touch on the boundaries of the subject herself.

One of the more ironic differences between the mainstream and interpretivism is their opposite reactions to more evidence. More evidence for the mainstream scholar is a means to generate higher confidence in his arguments because it is more data to test against the competing theories or permits him to increase the number of in/validating observations. On the contrary, more evidence for an interpretivist reduces her confidence because it expands the boundaries of intertextual meaning that must be accounted for in any account of the subject. As Hayden White put it, the more we know about the past, the more difficult it is to generalize about it.[68]

While the mainstream claims that its choice of boundaries is defended ultimately through the act of competing successfully against alternative explanations for the phenomena being studied, interpretivists wish to claim that their boundary is justified by the fact that the understanding being offered has accounted for all possible interpretations within the field. Neither position is defensible. The mainstream can never account for alternative explanations left necessarily unconsidered; interpretivism is equally incapable of accounting for meanings and understandings beyond the essentially arbitrary domain of the "cultural world." What is unavoidable here is convention.

Conclusions: Ten Mainstream Methods in Service of Interpretivist Epistemology

By way of concluding remarks, I want to identify ten mainstream social science conventions whose explicit use by interpretivists would not violate its epistemological principles, including their assumption of an open social system, a default of meaningful difference, an aversion to ambitious generalizations, or still less, predictions, across cases, and a commitment to a relative working truth.

An interpretivist claim that some phenomenon has a particular meaning, that this meaning is associated with the presence of other "variables," that it is plausible to expect its continuation into the future and into other contexts is strengthened

1. the greater the number of times it has been observed in the past.
2. the greater the span of time over which it has occurred.
3. the broader, deeper, and more distinctive the cultural contexts in which it appears.
4. the more independent observers agree that it has occurred.
5. the stronger the evidence of causal/constitutive connections revealed through process-tracing.
6. the more available alternative explanations have been compared to the available evidence and judged inferior.
7. the more exhaustively the available empirical record has been treated.
8. the more likely that other scholars are already at work extending and refining, and falsifying, one's own account.

The last two require some additional elaboration.

9. the more satisfied one can be that intersubjective reality's boundaries have been demarcated.

In Linda Alcoff's interpretation of Foucault's epistemology, the boundary of a domain is established by exhausting the meaningful relationships among the pieces of evidence As he puts it, if there "is no discernible connection or relation" to any other element, the element being observed "is without meaning," at least to the subject being analyzed, and hence, to the observer.[69]

10. the more certain one is that the naturalized, taken-for-granted evidence has been exhausted.

This solution to meaningful boundaries relies on the work of Alfred Schutz and Harold Garfinkel. In describing intersubjectivity, Schutz identified that space as the site wherein "what is taken for granted by me is also taken for granted by you . . . But this We . . . includes everyone who is one of us, that is, everyone whose system of relevances is substantially (sufficiently) in conformity with yours and mine."[70] In other words, the boundaries of an intersubjective world encompass actions and words considered natural to all participants. While Schutz establishes the criteria for inclusion, Garfinkel spent most of his career conducting experiments to uncover the boundaries of exclusion. By probing what people thought, during interactions with others, to be unacceptable, strange, incomprehensible, and surprising, Garfinkel mapped the domain of the intersubjectively naturalized. What he called breaching background expectancies define the edges of intersubjective understanding.[71] Such methods could be used to establish an interpretivist's domain of meaningful identicality and difference, as well.

For example, asking a sample of citizens in a collection of different countries adjudged democratic about what democracy actually means to them could be a way of sketching the preliminary boundaries of the meaning of the normative account of the democratic peace. One might find, for example, that Americans, when asked if the death penalty is compatible with democracy, respond with bewilderment or hostility to the question, just as hundreds of Garfinkel's subjects did to questions about their mundane background expectancies. Note here how the method used to gather relevant evidence is not necessarily anthropological. Garfinkel himself used medium to large-N experiments, but a large-N survey could be employed, or a number of focus groups, or documentary analysis, or in-depth interviews, or participant-observation. As I have suggested, it is not so much methodological technique that separates interpretivist treatment of evidence from the mainstream, as it is the meaning and purposes of evidence, or epistemology.

In sum, interpretivist epistemology would be far more convincing if it would explicitly acknowledge and employ mainstream methodological techniques. On the other hand, mainstream epistemological claims to control open social systems are impossible to sustain by any combination of methodological devices.

Notes

1. Very importantly, I define the "mainstream" exclusively as it appears in King, Keohane, and Verba. This means, on the one hand, that many qualitative scholars who think of themselves as part of the mainstream should remember they are not targets of the indictment offered here. On the other hand, it means that if interpretivist epistemology can render this mainstream more convincing, then it is surely still more applicable to qualitative work.
2. Hans-Georg Gadamer, "Foreword to the Second German Edition of Truth and Method," in Baynes, Bohman, and McCarthy, *After Philosophy*, p. 340. Alfred Schutz, the phenomenolgist who inspired much future interpretivist theorizing, argued that it is a mistake to "disregard the fact that certain procedural rules . . . are common to all empirical sciences," and should not be ignored by interpretivists. Alfred Schutz, "Common-Sense and Scientific Interpretation of Human Action," in Maurice Natanson, ed., *Collected Papers. The Problem of Social Reality*, Vol. 1 (The Hague: Martinus Nijhoff, 1973), p. 6.
3. Craig Calhoun, *Critical Social Theory. Culture, History, and the Challenge of Difference* (Cambridge, MA: Blackwell, 1995), p. 40, note 40 and 64. On the possibility of a "third way" between universalism and particularism, rationalism and relativism, modernism and postmodernism, see pp. 133–34. See also Daniel Little, *Varieties of Social Explanation: An Introduction to the Philosophy of Social Science* (Boulder, CO: Westview Press, 1991), p. 232.
4. See, for example, Richard A. Shweder, *Thinking through Cultures* (Cambridge, MA: Harvard University Press, 1991), p. 59 and David Dessler, "Scientific Realism is Just Positivism Reconstructed," paper presented at the annual meeting of the International Studies Association, Washington, DC, March 28–April 1, 1994.
5. Harry Eckstein, "Case Study and Theory in Political Science," in Fred I. Greenstein and Nelson W. Polsby, eds., *Handbook of Political Science* (Reading, MA: Addison-Wesley, 1975), pp. 79–137.
6. Alexander L. George, "The Causal Nexus between Cognitive Beliefs and Decision-Making Behavior: The 'Operational Code' Belief System," in Leonard Falkowski, ed., *Psychological Models in International Politics* (Boulder, CO: Westview Press, 1979), esp. pp. 104–24, and Alexander L. George and Timothy J. McKeown, "Case Studies and Theories of Organizational Decision Making," in *Advances in Information Processing in Organizations*, Vol. 2 (1985), pp. 21–58. For a brilliant execution of the method, see Yuen Foong Khong, *Analogies at War: Korea, Munich, Dien Bien Phu, and the Vietnam Decisions of 1965* (Princeton, NJ: Princeton University Press, 1992).

7. Pierre Bourdieu and Jean-Claude Passeron, *Reproduction in Education, Society and Culture*, trans. Richard Nice (London: Sage Publications, 1977), p. 34.
8. Pierre Bourdieu, *The Logic of Practice* (Stanford, CA: Stanford University Press, 1990), p. 56.
9. Clifford Geertz, *The Interpretation of Cultures* (New York: Basic Books, 1973), p. 27.
10. On Geertz's use of positivistic methods, see also Little, *Varieties of Social Explanation*, p. 238, n4.
11. Geertz, "Deep Play," in Rabinow and Sullivan, *Interpretive Social Science*, p. 223.
12. Karl Popper, *Objective Knowledge* (Oxford: Oxford University Press, 1972), pp. 78–81.
13. Albert O. Hirschman, "The Search for Paradigms as a Hindrance to Understanding," *World Politics*, Vol. 22, No. 2 (April 1970): 329–43 and Giovanni Sartori, "Concept Misformation in Comparative Politics," *American Political Science Review*, Vol. 64, No.4 (December 1970): 1033–53.
14. George E. Marcus, "Contemporary Problems of Ethnography in the Modern World System," in James Clifford and George E. Marcus, eds., *Writing Culture. The Poetics and Politics of Ethnography* (Berkeley, CA: University of California Press, 1986), pp. 183–84.
15. James Clifford, "On Ethnographic Allegory," in Clifford and Marcus, *Writing Culture*, pp. 106–10.
16. Alfred Schutz, "Common-Sense and Scientific Interpretation," p. 5.
17. Timothy J. McKeown, "Case Studies and the Statistical Worldview: Review of King, Keohane, and Verba's *Designing Social Inquiry: Scientific Inference in Qualitative Research, International Organization*, Vol. 53, No. 1 (Winter 1999): 166.
18. This is not precisely the case, as the mainstream might be interested in psychological approaches to decision making, and so assess the individual's perception of her reality. But this too involves the importing of a priori theories of experimental cognitive and social psychology to be applied to the subject of interest. And of course, the mainstream can observe the manifest meaning of social structure, too, but not as mediated by the interpretation of the subject, unless as a decision-making variable of interest.
19. Paul Ricoeur, *Time and Narrative*, Vol. 1 (Chicago: University of Chicago Press, 1984), pp. 129–30. This is also the first move of Peter L. Berger and Thomas Luckmann in their path-breaking book, *The Social Construction of Reality. A Treatise in the Sociology of Knowledge* (New York: Anchor, 1966), p. 15.
20. Roland Barthes, "The Death of the Author," in CITE and Paul Ricoeur, "The Model of the Text: Meaningful Action Considered as a Text," *Social Research*, Vol. 38, No. 4 (1971): 534.
21. Charles Taylor, *Human Agency and Language. Philosophical Papers*, Vol. I (Cambridge, England: Cambridge University Press, 1985), p. 231.
22. Susan Hekman, *Hermeneutics and the Sociology of Knowledge* (Cambridge, MA: Polity Press, 1986), p. 82.

23. That this is a big step away from and beyond phenomenology is obvious in Schutz's uncritical treatment of "The social scientist as disinterested observer." Schutz, "Common-Sense and Scientific Interpretation," pp. 36–38.
24. Schutz, "Some Leading Concepts of Phenomenology," in Schutz, *Collected Papers*, pp. 104–11.
25. Quoted in Jurgen Habermas, *On the Logic of the Social Sciences* (Cambridge, MA: MIT Press, 1994), p. 151.
26. Pierre Bourdieu, *Outline of a Theory of Practice* (Cambridge, England: Cambridge University Press, 1977), p. 52.
27. For this in the work of Hans-Georg Gadamer, see Richard Bernstein, *Beyond Objectivism and Relativism: Science, Hermeneutics and Praxis* (Philadelphia: University of Pennsylvania Press, 1983), pp. 137–50.
28. For case study methodology in general, and the utility of crucial cases in particular, Eckstein, "Case study and theory in political science." See also Charles C. Ragin, *Fuzzy-Set Social Science* (Chicago: University of Chicago Press, 2000) and Charles C. Ragin and Howard S. Becker, eds., *What is a Case?: Exploring the Foundations of Social Inquiry* (Cambridge, England: Cambridge University Press, 1992).
29. James Caporaso, "Research Design, Falsification, and the Qualitative-Quantitative Divide," *American Political Science Review*, Vol. 89, No. 2 (June 1995): 458. This selection is only part of a wide-ranging special forum devoted to *Designing Social Inquiry* in which many mainstream critiques of the book are raised.
30. To be fair to Martin and Sikkink, they do understand power as domestically constituted. The next essay in the symposium replicates the problem of claiming that a theory is more valid if it meets our own standards of plausibility. So, Ronald Rogowski cites the work of Peter Katzenstein and Robert Bates as "powerful" because both their accounts are underpinned by "universally accepted economic theory," and neither "contravenes the received wisdom . . . " Ronald Rogowski, "The Role of Theory and Anomaly in Social Scientific Inference," *American Political Science Review*, Vol. 89, No. 2 (June 1995): 469–70. An interpretivist would not consider appealing to the power of conventional wisdom about some other theory to be a way of demonstrating the validity of one's interpretation of any particular case, or collection of cases.
31. On the relations between description, understanding, interpretation, and explanation see: Paul Ricoeur, "Explanation and Understanding," in Charles E. Reagan, ed., *The Philosophy of Paul Ricoeur: An Anthology of His Work* (Boston, MA: Beacon Press, 1978), p. 165 and Gadamer, "Foreword," p. 342.
32. Hans-Georg Gadamer, "Hermeneutics as Practical Philosophy," in Baynes, Bohman, and McCarthy, *After Philosophy*, pp. 331–32.
33. Gary King, Robert O. Keohane, and Sidney Verba, *Designing Social Inquiry* (Princeton, NJ: Princeton University Press, 1994), p. 27.
34. As McKeown puts it, "if a phenomenon were invariant, a single observation would be equivalent to many observations." "Case Studies," p. 168.

35. Besides conventional social science concerns about degrees of freedom, see Charles Taylor, "Interpretation and the Sciences of Man," in Rabinow and Sullivan, *Interpretive Social Science*, p. 78.
36. See, for example, the high level of attention "omitted variable bias" receives in *Designing Social Inquiry*, but how easily it is expected to succumb to simple techniques, esp. pp. 168–82. On p. 172, for instance, we are reassured that it is possible to account for omitted variables because "[f]ortunately, in most cases, researchers have considerable information about variables outside their analysis." Interpretivists assume such information is often impossible to obtain.
37. On the relationship between meaning and fact in analytical philosophy, see Sollace Mitchell, "Post-Structuralism, Empiricism, and Interpretation," in Sollace Mitchell and Michael Rosen, eds., *The Need for Interpretation. Contemporary Conceptions of the Philosopher's Task* (London: Athlone Press, 1983), pp. 54–89. On the problematic nature of measuring democracy in particular, see Gerardo L. Munck, *Disaggregating Political Regime: Conceptual Issues in the Study of Democratization* (Notre Dame, IN: Kellogg Institute for International Studies, 1996).
38. King, Keohane, and Verba, *Designing Social Inquiry*, p. 10.
39. This problem has long been recognized in political science. Giovanni Sartori, for example, pointed out three decades ago the perils of doing comparative research. He warned against "stretching" the meanings of variables across contexts in order to achieve some contrived generalization. See Giovanni Sartori, "Concept Misformation in Comparative Politics," *American Political Science Review*, Vol. 64, No. 4 (December 1970): 1033–53.
40. King, Keohane, and Verba, *Designing Social Inquiry*, pp. 93–94.
41. This reach for the methodological fix to what is a deeply ontological problem resonates with McKeown's list of six quandaries that no statistical method is going to resolve: sampling the tail of a distribution; bad variable operationalization; missing the impact of previously ignored variables; misspecification of the relationship among variables already included in a theory; the overreaching assumption of the identicality of cases; and a misconceived explanatory strategy. While McKeown rightly sees the comparative case study method as a solution to these problems, I suggest interpretivist ones. "Case Studies," p. 168.
42. *Designing Social Inquiry*, p. 43. The same problem exists for their advice to ignore irrelevant implications of a theory when searching for evidence (p. 48). The interpretivists ask how can we know what is irrelevant in advance of looking at the evidence from the field? For example, until the National Black Election Study was revised in the last decade, "theories" of democratic participation predicted voting and campaign contributions were "evidence" of such participation. Only after "interpretivist" scholars went to the field and found nonwhite political activity at rallies, church suppers, and at home, did the "theory" widen to treat these social practices as evidence, too. If theory, and its deduced implications, are allowed to drive the search for evidence, one will find what one searches for most of the time.

43. Bourdieu clearly recognizes the need to theorize as if a working identicality is possible. He writes of constructing "objective classes" out of "agents placed in homogenous conditions" who "generate similar practices, possessing a common set of objectified properties." Bourdieu, *Distinction*, p. 101.
44. Little, *Varieties of Social Explanation*, pp. 190, 198–99.
45. McKeown goes farther, saying that "sorting events into particular types is an act of judgment, not statistics." "Case Studies," p. 171.
46. See Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1970).
47. Popper, *Objective Knowledge*, pp. 78–81.
48. Of course, Popper was never the naive falsificationist who argued that a single disconfirmation meant the abandonment of the imperfect theory. No, the disconfirmation had to be accompanied by an alternative theory that could account for all that the failed theory had accomplished, plus some outcome/s unaccounted for by, but entailed within, the theory's predictions.
49. McKeown, "Case Studies," pp. 172–73.
50. Max Horkheimer and Theodor W. Adorno, *Dialectic of Enlightenment* (New York: Continuum Publishing, 1993), esp. pp. 3–42.
51. See, for example, Charles Taylor, "Interpretation and the Sciences of Man," 79. See also Ira J. Cohen's comprehensive review of Giddens's structurationism. He, perhaps too deftly, differentiates between structurationism's rejection of "uniformitarianism," but warm embrace of "reproducibility." "Structuration Theory and Social Praxis," in Anthony Giddens and Jonathan H. Turner, eds., *Social Theory Today* (Stanford, CA: Stanford University Press, 1987), esp. pp. 280–302.
52. Fay, *Critical Social Science*, p. 46.
53. Barbara Herrnstein Smith, *Belief and Resistance. Dynamics of Contemporary Intellectual Controversy* (Cambridge, MA: Harvard University Press, 1997), pp. 125–26.
54. This is of course an important theme in Thomas Kuhn's and Paul Feyerabend's work. It should be pointed out that Gadamer rejects the relativism of Kuhn, Feyerabend, work Winch, Rorty, and Geertz, arguing that a hermeneutic circle is always possible between observer and subject. Bernstein, *Beyond Objectivism and Relativism*, pp. 141–44.
55. Alcoff, "Foucault as Epistemologist," p. 105.
56. Jonathan Turner has urged that we not confuse law with empirical generalization in Turner, "Analytical Reasoning," in Giddens and Turner, *Social Theory Today*, p. 160.
57. For example, King, Keohane, and Verba recommend addressing the problem of uncertainty with an "uncertainty estimate." Interpretivists just recognize and live with it; they don't believe it is something to be resolved. *Designing Social Inquiry*, pp. 9, 32, and 94. In contrast, see Paul Ricoeur, *Time and Narrative*, p. 135.
58. Charles Taylor, "Overcoming Epistemology," in Baynes, Bohman, and McCarthy, *After Philosophy*, p. 474. Something I learned while studying nuclear exchange scenarios was that when one multiplies even very high probabilities (.9) of accuracy, it does not take very long to approach zero.

59. Bourdieu recognizes the extreme futility of each: "The only way of completely escaping from the intuitionism which inevitably accompanies positivistic faith in the nominal identity of the indicators would be to carry out—a strictly interminable—analysis of the social value of each of the properties or practices considered . . . " Pierre Bourdieu, *Distinction*, p. 20. Michael Shapiro appears to appeal for the interminable when he declaims empiricism for not being able to "develop an exhaustive and noncontroversial empirical specification for . . . terms . . . or conceptual systems." See Michael J. Shapiro, *Language and Political Understanding. The Politics of Discursive Practices* (New Haven, CT: Yale University Press, 1981), p. 40.
60. Geertz, "Deep Play," in Rabinow and Sullivan, *Interpretive Social Science*, pp. 195–240. On Geertz as a generalizer, see Alexander, *Fin de Siècle Social Theory*, pp. 99–119.
61. Foucault, *Archaeology of Knowledge*, p. 167.
62. Bourdieu, *Outline of a Theory of Practice*, p. 78.
63. Bourdieu, *Distinction*, p. xi.
64. Rabinow and Sullivan, "The Interpretive Turn," p. 14.
65. Foucault observed that there are a "plethora of intelligibilities, a deficit of necessities," implying the absence of any limit to intersubjectivity. Foucault, "Questions of Method," p. 106.
66. Alexander, "Centrality of the Classics," p. 48.
67. Gadamer, "Hermeneutics as Practical Philosophy," p. 334.
68. Hayden White, *Tropics of Discourse. Essays in Cultural Criticism* (Baltimore, MD: Johns Hopkins University Press, 1978), p. 89.
69. Linda Alcoff, "Foucault as Epistemologist," *The Philosophical Forum*, Vol. 25, No. 2 (Winter 1993): 95–124.
70. Schutz, "Common-Sense and Scientific Interpretation," pp. 12–13.
71. Haorld Garfinkel, *Studies in Ethnomethodology* (Englewood Cliffs, NJ: Prentice-Hall, 1967), esp. chap. 2.

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Part II

The Product of Inquiry

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Beyond Logical Positivism: Reframing King, Keohane, and Verba[1]

Brian M. Pollins

My purpose here is in the spirit of the essays by Friedrich Kratochwil and Ted Hopf. That is, I hope to contribute to a new dialogue that can move the social sciences in general and political science in particular beyond the shopworn debate between "quantitative *versus* qualitative" methods or "neopositivist *versus* interpretivist" epistemologies. In my view, misunderstandings on both sides of this conflict are slowing us down by revisiting controversies that should have been abandoned long ago.

I differ with Kratochwil and Hopf in an evaluation of King, Keohane, and Verba, in that I argue that a more pluralistic and tolerant social science could benefit from a reinterpretation and reframing of *Designing Social Inquiry* rather than from the rejection of it. I agree with many of the central points made by Friedrich Kratochwil and Ted Hopf. They are correct in arguing that a social science epistemology that incorporates interpretivist research interests and practices will be richer and more productive than one that excludes them. My position differs with Kratochwil and Hopf in that I view the position taken in *Designing Social Inquiry* not as wrong or misguided but as incomplete. If we are to build a new social science, I argue, *Designing Social Inquiry* is a stone we can well use.

The heart of my argument is that practices advocated in *Designing Social Inquiry* should be central to a new, broader-based social science epistemology. This will surprise both the ardent advocates of King, Keohane, and Verba's prescriptions as well as their toughest critics. Many "true believers" in KKV will likely question my call for methodological tolerance and diversity, while their harshest opponents will be wary of an epistemology that draws from the neopositivist agenda that they see embedded in *Designing Social Inquiry*. The task I set for myself in this chapter is to overcome the initial reservations of both camps in the hope that we may all begin to develop social science practices that are inclusive and facilitate communication between scholars with different methodological preferences.

The main pillars of my argument are simple. I will argue that many of our epistemological debates are focused wrongly and it is time for us work toward social science practices that permit true communication across different methodological traditions. My position is developed in three parts. First, too many social scientists debate the virtues and vices of the Vienna Circle's logical positivism as though it were something that could still be debated. Too many still view logical positivism as a dominant force in our legitimation of certain methods and delegitimation of others, when in fact it never had that power. There is little point in launching an attack on logical positivism here, for that task has been completed some time ago by much more capable scholars than me. Instead, it is my intention to decouple the practices in *Designing Social Inquiry* from the "positivism" that many associate with those practices.

Second, I reject the radical relativism of King, Keohane, and Verba's harshest critics. The demise of logical positivism does not imply that relativism rules. I will argue that a tolerant and pluralistic social science requires true communication among scholars who employ a variety of methods. My vision is quite consistent with Friedrich Kratochwil's metaphor of a court, as described in this volume. I wish to expand upon this notion, and begin by noting that courts have a clear idea of shared communicative practices. More specifically, all courts operate by making clear to all parties the sort of information that may be counted as evidence, the form that claims upon truth may and may not take, and the means of deciding between competing interpretations of the evidence. I claim that the central practices of social science advocated by King, Keohane, and Verba do not presume a commitment to logical positivism. Indeed, they serve as a starting point for writing the methodological and epistemological rules of practice needed in our epistemologically pluralist court.

Third, I argue that the line between science and nonscience is drawn not by choice of method, but by practices that permit effective communication among members of a research community. Such practices emphasize clarity and openness among practitioners regarding the claims they are making, the evidence they have assembled in support of those claims, and the logic they employed in linking that evidence to their claim. I contend that King, Keohane, and Verba do indeed make a contribution on this front, and we should build on it. I will argue that methodological practices advocated in *Designing Social Inquiry* reduce readily to two important principles of scientific communication: falsifiability and reproducibility. While these principles are not original to *Designing Social Inquiry*, the book helps us see how contemporary mainstream social science is built upon these principles. It is my position—very much like Ted Hopf's—that these principles can and should underpin interpretivist approaches in order to permit greater methodological pluralism in the social sciences.

Laying out this argument in greater detail, subsequent sections will briefly review core tenets of logical positivism and explain why they are untenable; discuss practices that are central to social science; and show that there is *no* necessary connection between positivism and KKV-advocated methods. Finally, I will elaborate my position that a postpositivist, pluralistic social science can be based on specific, communicative practices.

Logical Positivism Has Come and Gone

It is now over eighty years since the formation of the Vienna Circle by Moritz Schlick, Otto Neurath, Hans Hahn, and others. Luminaries such as Rudolph Carnap, Herbert Feigl, and Kurt Gödel joined not long after the Circle's founding. As all members were trained extensively in mathematics and the physical sciences, these disciplines received most attention in their early discussions. But the tenets that the Circle would come to advocate were considered by many members to be equally applicable to the social sciences—Neurath himself was a sociologist. Thus the notion of a "unity of sciences"(physical and social) was born. The keystone in the edifice of logical positivism was the "verification principle" that held that claims of fact must be either purely analytic (i.e., formally true or false in a mathematical sense) or empirically testable to have any meaning.

The influence of the Vienna Circle expanded greatly beginning in the 1930s, just as the original Circle was itself breaking up. Hitler's rise caused several members to flee Vienna for safer havens, and this spread the group's influence more widely. Feigl, for one, established a leading department in the philosophy of science at the University of Minnesota. Carnap moved to the University of Chicago. Gödel joined the Institute for Advanced Study at Princeton. And in addition to Karl Popper (who moved from Vienna to London during this same exodus), others such as A. J. Ayer and Ernst Nagel were attracted to the banner.[2] Under their collective influence, science became a search for immutable laws—positive claims about the workings of the world that were analytically sound and tested empirically.

The Circle had a profound effect on the social sciences, culminating with the 1960s "behavioral revolution" that reshaped the fields of psychology, sociology, and political science—especially in the United States. Ironically, just as the influence of logical positivism on the physical as well as the social sciences was reaching its zenith, its very foundations were being called into question by philosophers of science. Karl Popper showed that the verification principle suffered fatally from Hume's Problem of Induction and therefore could never serve as the arbiter of a theory's truth or falsity. He substituted his own principle of falsification in its stead, and most members of the Circle accepted this without difficulty (Edmonds and Eidinow 2001: p. 171). Further challenges to logical positivism gained significant ground. The attack came on multiple fronts. For one, the "falsificationism" that now substituted for the verification principle in the minds of many followers of the Vienna Circle presumes a logical distinction between theory (the knowledge claim) and observation (the act of testing the theory). Carl Hempel, to name one, famously argued that no such distinction exists—observation presumes theoretically derived frameworks and categories, hence our tests cannot be independent from our theories.[3] Similarly, logical problems with "critical experiments" were found.[4] There are very good reasons why we do not allow one contradictory observation to destroy a theory. But if we tolerate such anomalies (and *all* sciences do), what can "verification" and "falsification" mean?

The "unity of science" was also being rightly questioned. Allow me to illustrate: In a number of fields from evolutionary biology to human history the role of *contingency* is central, while in others such as physics it is largely irrelevant.[5] And where contingency matters, universal or "covering" laws are obviated. Where contingency matters, our explanations for particular events—such as the appearance of homo sapiens on the evolutionary time line, or the ascent of Caesar Augustus to the imperial throne—will emphasize path dependence and invite the exploration of counterfactual conditions. Indeed, the exploration of counterfactuals, whether by thought exercise or more formally via gaming and simulation, is itself a type of evidence important to sciences that explore contingent events (Lebow and Tetlock 2001).

In addition to its often contingent nature, human behavior, unlike that of physical objects, is often purposive and self-conscious. This goal orientation in humans, *inter alia*, means that regularities in behavior—stable patterns that may appear to some to be "laws"—may change as human goals and strategies evolve, perhaps even as a result of rising consciousness of the existing pattern itself. The key point is that our capacity to adapt our behavior to new circumstances ensures that we are not subject to "laws" in the way that physical objects are. Any regularities we find will be *bounded* in space and time—utterly the opposite condition from that studied by our colleagues in physics departments. Thus, due to the contingent and bounded nature of any patterns we find in social phenomena and human tendencies, the methods of social scientists must be more flexible and our ways of understanding our world more pluralistic than the physical sciences.

In sum, efforts in philosophy of science over the latter half of the twentieth century established the existence of fatal flaws in logical positivism. The doctrine of falsificationism that developed from the verification principle was shown to have severe shortcomings—Popper himself made a point of distinguishing his position from the "naive falsificationism" of the logical positivists in his later work. The search for immutable laws of nature, whether in Carnap's purely deductive-nomological form or Hempel's inductive-probabilistic statements, was found to suffer from the same difficulties in proving causation that have been shown to be just as reliant on the psychologic of Kant's constant conjunction.[6] Thus, the notion of science as a quest for universal laws, independently tested by observation, was shown to be deeply problematic even for the physical sciences.[7] Finally, the mutability, historicity, and boundedness of human behavior, the contingencies that can deflect the human story down countless different paths at any given moment, all create qualitative differences between our subject matter and many of the physical sciences in ways that make the search for a "unity of science" completely futile.

It would be salutary if social scientists would admit that we are all post-positivists now. Logical positivism has come and gone, and it is time for us to move on. But let us not begin this journey with a misstep. The passing of logical positivism does not logically imply the ascendance of relativism (Laudan 1990). Relativism has its own set of deep limitations and logical conundrums.

Our journey must begin with the recognition of an obvious point: Scholarship is a communal enterprise, and any community of scholars must share basic rules of communication. One of the lessons of twentieth-century philosophy is that such rules will have linguistic and practical roots (rather than deductively logical foundations). If such rules are not to be derived by unassailable, deductive logic (the program of the Vienna Circle), then they will themselves be conventions that we choose to practice. To say that we, as a community, are free to choose the rules of our research practices does not mean "anything goes." A radical antifoundationalist position will leave us with a Tower of Babel of incommensurable knowledge claims. No "community," no discipline will be possible. Yet there is no logical reason why such practices cannot be pluralistic. Indeed, if we cannot find this pathway, our remaining alternatives will be to see certain epistemologies driven from the field, or to agree to a divorce into ever more diverse departments incapable of interdisciplinary communication. I believe a pluralistic pathway is there to be found, and *Designing Social Inquiry* contains a good starting point. It is to this claim that I now turn.

Core Social Science Practices Advocated in *Designing Social Inquiry* Are not "Positivist"

King, Keohane, and Verba's *Designing Social Inquiry* provokes strong reactions in many readers. Passionate advocates and critics alike view the book as an arrantly positivist line drawn in the sand, intended to bring a narrow methodological orthodoxy upon unruly scholarly communities. Supporters welcome the book as a flail to use upon "undisciplined" scholarship, while the harshest critics see it instead as a feeble rear-guard action, attempting vainly to slow the advance of postmodernism against an untenable, positivist past.

I believe *Designing Social Inquiry* is none of these things. I read it simply as an initial attempt to find common ground among widely different methodologies and social science epistemologies. I believe its purpose is to serve as a first chapter rather than a final word in contemporary dialogues about social science methods. I argue that the research practices prescribed in *Designing Social Inquiry* do not presume a commitment to positivist epistemology and in fact provide a basis for a social science that is methodologically and epistemologically open and tolerant toward a variety of approaches.

The style adopted in *Designing Social Inquiry* reflects an overdrawn distinction between the *Verstehen* and *Erklären* traditions in social science as they have come to be understood by us, now roughly a century since J. G. Droysen, Wilhelm Dilthey, Max Weber, and others distinguished them (von Wright 1971:5). "Understanding" and "Explanation" are often taught to our graduate students not merely as *different* approaches to building knowledge, but as *opposed* epistemologies. We draw stark lines and lead them to embrace one and reject the other, denying the possibility of dialogue in a community of scholars who may follow different approaches to "knowing" while maintaining shared communicative practices. Perpetuation of this dubious dichotomization also leads us to caricature the meaning of these two knowledge-building approaches in order to make the distinction more "clear" to our students. In so doing, I suspect that we ignore some of the important findings of twentieth-century philosophy of science—in particular the untenability of central positions taken by the Vienna Circle, as well as difficulties within the interpretivist program.

As it is read and understood by most researchers in our field, *Designing Social Inquiry* reinforces this false division by concentrating on, and appearing to claim exclusive legitimacy for, the *Erklären* tradition. In an early section entitled "Defining Scientific Research in the Social Sciences," the first of four characteristics states that "The goal is inference" (p. 7). Inferences, I submit, are wrongly associated with positivist epistemology by many of us, and thus King, Keohane, and Verba are quickly classified as positivists. In fact, they go on in chapters 2 and 3 to outline two traditions of social science inference: "descriptive" and "causal." These two forms of inference are broadly consistent with the *Verstehen* tradition as portrayed in Weber and the *Erklären* tradition that underpins "mainstream" political science today.

Nevertheless, despite these shortcomings it is important to note that "descriptive" and "causal" modes of inference are depicted in the book as *complementary* rather than opposing. This is helpful for three reasons. First, it breaks the false dichotomy between two grand traditions in social science epistemology. Second, it forces followers of these traditions to reconsider key epistemological and methodological assumptions. And third, it provides a starting point for those of us interested in genuine epistemological pluralism.

The imbalance in the book toward the *Erklären* tradition over *Verstehen* forces us to reconstruct key arguments in a way that privileges neither tradition, while still preserving core claims made in *Designing Social Inquiry*. To begin this task, I propose to reduce the book to its most essential points, reposition other claims upon those central points, and then see where we stand regarding the possibilities for a pluralistic social science.

There are only two fundamental ideas underlying the practices prescribed by King, Keohane, and Verba. I will refer to these as *falsifiability* and *reproducibility*. Beyond avuncular pointers regarding the choice of a research topic, all other prescriptions and proscriptions found in the book can be either subsumed under or derived from these principles. Importantly, neither principle presumes a commitment to logical positivism. Instead, they serve simply as cornerstones of the communication among researchers in the process of doing their work—something that is inescapably a social enterprise. I consider these two principles in turn.

Falsifiability

It is of central importance that we not confuse "falsifiability" with "falsificationism." King, Keohane, and Verba tell us that we must state our claims about the world in a way that we and our readers could imagine finding evidence that would be inconsistent with that claim (1994: 19).[8] In other words, if we are to play the "knowledge creation" game fairly, we must allow for the possibility that our hunch or claim could be wrong. Fellow scholars—those whom we are trying to persuade in the "court" of academia—must have a fair chance of demonstrating that our claim about the world is subject to their opposition. If our claims are "nonfalsifiable," then the game is over before it has begun, and we "win" only because we made up our own rules as we went along. This notion of falsifiability requires that claims be stated in a way that observations of our world be classifiable as "consistent" or "inconsistent" with that claim. It recognizes the problematic and theory-laden nature of the act of observation; it recognizes that there is never a decisive test of a theory. Falsifiability simply holds that we must do the best we can and be sufficiently clear that "more correct" can be distinguished from "less correct." This notion of fallibilism abjures the claim of independence between theory and observation in favor of a communicative convention that permits give and take within a research community. And it is important to note that the main interaction envisioned by the concept of falsifiability is between the claimant and her audience as much as it is between theory and observation.

Falsificationism, in contrast, calls for a two-cornered test between "theory" (our claim) and "evidence" (our observations). It does not consider the question of "evidence" to be problematic because evidence, in this naive view, is simply made up of observations of a "real world" independent of you and me. That is, this "real world" yields exactly the same information whether it is you or it is me who is observing it. In short, this is the view found in the classical works of logical positivism, and refuted by the great majority of philosophers of science (including Popper as well as several members of the Vienna Circle itself) long ago. It is not necessary to subscribe to this doctrine in order to employ social science practices prescribed in *Designing Social Inquiry*.

The distinction between falsifiability and falsificationism described here may seem subtle at first, but it is crucial.[9] The latter considers the employment of concepts, the categories they contain, and the observations (evidence) placed into those containers to be unproblematic. Falsifiability, meanwhile, is much more open-ended because it recognizes the theory-laden nature of observation and allows for any number of differences between you and me—in our definition of concepts, our creation of categories, the sort of observations we will permit to be used as evidence—provided we describe all these with sufficient clarity that a dialogue on these issues can be engaged between us and by fellow scholars. Debate under the rules of falsificationism can only say "I find the following error in the logic of your argument" (i.e., the claim is flawed analytically) or

"I offer additional observations which are not consistent with your claim" (i.e., the claim is found wanting through observation). What I here term "falsifiability" allows challenges not only on these terms but also challenges on the basis of our chosen conceptual categories and the instances that may or may not serve as evidence to test our claim. Falsificationism is simply a *methodological* concept (whose fatal, logical flaws have long been known) while falsifiability is a *communicative* concept—a necessary characteristic for productive dialogue in a community of scholars.

Reproducibility

The concept of reproducibility is also essentially a communicative concept. Simply put, it requires that research be described to others in a way that would allow them to retrace the researcher's footsteps and find that they had obtained the same evidence, and to arrive from that evidence at the same conclusions. It in no way precludes debate about either the steps taken or the conclusions reached. Quite the contrary, it enables such challenges. Reproducibility simply demands that the researcher describe the process whereby she moved from conjecture to knowledge claim. While enabling scholarly dialogue and challenge across a range of research choices, reproducibility contributes to satisfying the condition of falsifiability. Satisfying the condition of reproducibility, we might say, provides the clarity of concept definition, categorical arrangement, definition of evidence and rules of inference that falsifiability requires, and meaningful debate needs.

This requirement also allows for pluralism in research approaches. Interpretivist epistemologies are not merely allowed but welcomed. Under this requirement it is *not* the case that you must agree with my choice of concepts, or my definition of those concepts, my construction of categories out of them, my selection of evidence or the way in which I marshal that evidence to buttress my claims. The requirement of reproducibility is satisfied when it is clear that *if you did agree* with all the choices I made, you would gather the same body of evidence as I did and you would agree with the claims that I made at the end of the process. Satisfaction of this requirement enables and facilitates communication because the degree of communicative clarity it brings allows the questioning of concepts, categories, evidence, and inference and clarifies where the precise points of contention exist. It allows us to distinguish, for example, a challenge that states "I have a different set of observations which suggest that Democracy does not bring Peace" from a challenge that states "I have different conceptualization of Democracy than you. This alternative definition entails a different classification of evidence, and my results indicate that Democracy does not bring

Peace." If conditions of reproducibility are not satisfied, such debates cannot be engaged.[10] Instead, scholars talk past one another.[11]

Here too, the emphasis is on our ability to communicate with fellow researchers—to communicate not only our results and knowledge claims but the concepts, assumptions, and procedures we employed to reach those claims. By requiring that I communicate the definition of each concept and each research decision with "reproducible" clarity, this principle exposes every part of my work to scrutiny. Satisfying this principle in reported research also sharpens points of contention between author and audience. It allows the larger community to see more clearly whether the debate is focused on the definition of concepts, the choice of cases, the logic whereby one side or the other links evidence to claims, or other possibilities. It helps ensure that contending ideas engage rather than pass each other in the night.

All Specific Practices Advised in Designing Social Inquiry *Derive from Two Principles*

If we accept falsifiability and reproducibility as core tenets, we will find that all research practices advocated by King, Keohane, and Verba can be either subsumed under or derived from these two principles.

The first chapter of *Designing Social Inquiry* describes basic practices of King Keohane and Verba's preferred social science. Many are simply elaborations of the concept of reproducibility, especially as it is viewed in the *Erklären* tradition. They urge explicit definition of the concepts central to our research and clear definition of the rules of inference that will move us from observation to knowledge claim. They tell us to delineate our domain of observation, and to ensure that all our observations are replicable. All these are necessary if we are to imagine a fellow researcher retracing our steps and arriving at the same conclusions and knowledge claims—the very meaning of reproducibility.

Other prescriptions given by the authors in their first chapter simply reflect their commitment to falsifiability. These would include their advice to proliferate the number of observable implications that are implied by a particular theory or conjecture (which they also refer to as "maximizing leverage"), avoidance of polemics, self-critical awareness of the limitations of our research, reporting uncertainty, and consideration of alternative hypotheses. The relevance of each of these to falsifiability is clear. "Maximizing leverage," or increasing the number of observable implications of a knowledge claim increases the number of opportunities we have to assess whether our observations are consistent with those claims.[12] And remaining aware of the limitations of our work, reporting our uncertainty and consciousness of alternatives also serves this fallibilist interest.

The next two chapters expand on these points within approaches they distinguish as “Descriptive Inference” and “Causal Inference.” Chapters 4 and 5 detail specific pitfalls in research designs centered on Causal Inference, and how to avoid these problems. Although the authors do not state this explicitly, the root issue in each instance is falsifiability. The problems they address in these two chapters focus on indeterminate research designs and problems of model specification. Each of these research problems, when left unaddressed, either erodes or completely destroys the falsifiability of research results. To sketch them *ad seriatim*, “insufficient observations”—holding more claims than cases—makes it logically impossible to distinguish one set of claims from any number of contenders. Hence none can be shown to be inconsistent with the cases observed. Collinearity among explanatory factors makes it logically impossible to identify one, the other, or both factors as consequential for the phenomenon Y. Again, contending claims cannot be distinguished under these conditions. Selection bias can skew our results in any direction, in principle, but it is often in the direction that we “find” no effect of X on Y when in fact one is present. Broadly similar biases result from omitted variable problems and endogeneity. In all cases, our ability to ascertain whether or not our observations seem to conform to our claims is undermined. All put the falsifiability of our claims at risk in one way or another.

Again, regardless of the epistemologies that may be employed within a particular research community, it is communication that is at the heart of the research enterprise. Hence, we might begin to build a truly pluralist social science if we consider research as a *rhetorical exercise*. It is to this notion that I now turn.

Social Science as Rhetorical Practice

Social science is a communal enterprise. Nothing can be more basic or essential to our work than communicating our findings and claims to our colleagues for their consideration, evaluation, criticism, denial, or acceptance. The documents we produce to communicate our findings and claims—conference papers, journal articles, books—are attempts to persuade a specific audience. And the means of persuasion we employ takes a particular form; that is, *reasoned argument supported by evidence*. In short, *rhetoric*—in the sense intended by Aristotle or Augustine, not as “empty speech”—is at the very heart of what we do as social scientists.

I speak here of social science less as we conduct it—interviewing subjects, collecting observations, analyzing texts and discourses or aggregate data—but more as we communicate it to the broader audience of our colleagues. Abraham Kaplan long ago made the distinction between research as it is actually carried on day by day and our description of that research upon completion. Both activities follow ". . . a cognitive style which is more or less logical . . ." (Kaplan 1964: 8) but they remain distinct nevertheless. We should expect the correspondence between the two to be less than perfect. The logic of everyday research practice was termed "logic-in-use" by Kaplan while he called the more rationalized, post hoc description of that work "reconstructed logic."

Logic-in-use is a messy enterprise. It is here that we meet dead ends, play hunches, and finally take a stand based upon what we can justify to ourselves and the colleagues who will ultimately vet our claims and make use of our work. At this point in the process, our chief aim is to communicate our findings and claims to that peer community. Clearly, we hope to communicate with them in a way they will find persuasive if not compelling. Hence, as we shift from the logic-in-use of our work to the reconstructed logic of it, we shift from day-to-day social science (however we choose to practice it) to the rhetorical exercise of communicating the results of our work persuasively. This parallels Deirdre McCloskey's key insight that she states more succinctly: "Science is an instance of writing with intent, the intent to persuade other scientists. . . ." (1998: 4).

While intrigued by the insights of Kaplan and McCloskey, I would add that the first social scientists we always seek to persuade is ourselves. We know that once we go public with a result, a finding, or a claim, our reputation will be on the line. Hence we wish to be as sure as we can of our position, and the wiser among us will challenge ourselves or seek counsel and comments from our closest colleagues before we publish. Thus rhetoric plays a part in the "logic in use" stage of research as well as in our reporting of it in the "reconstructed logic" phase. To consider how rhetoric might serve as a basis for a broader and truly pluralistic social science, I will briefly sketch its origins. I will then consider commonalities in the forms of persuasion used by scholars in both the *Erklären* and *Verstehen* traditions. These commonalities, I argue, permit us to adopt falsifiability and reproducibility as the currency of communication within and between different social science epistemologies.

Communicative Practice: The Study of Rhetoric

The systematic study of speaking and writing with intent was thought of as both art and science by the classical Greeks. Rhetoric was a central subject in Greek education, notwithstanding Plato's hostility to the enterprise.[13] Athenian democracy—with its popular election of representatives, citizen assemblies to consider policy, and courts that employed very large juries—created a need for ". . . the cultivation of the elusive skill of winning over mass audiences" (Lawson-Tancred 1991: 11). Gorgias is considered by most to be the founder of the systematic study of rhetoric. He, along with the Sophists, became the object of Plato's withering attack on the subject that shapes some attitudes regarding "mere rhetoric" or "empty rhetoric" to this day. The best known treatment of the subject surviving from classical times is Aristotle's *The Art of Rhetoric*—a work clearly influenced by Gorgias, Isocrates, and the Sophists. Later writings on the subject by scholars such as Cicero and Augustine rest solidly on Aristotle's foundation.

Aristotle notes three broad bases of persuasion that correspond to what he would delimit as three genres of rhetoric: the deliberative, the epideictic, and the forensic. Lawson-Tancred describes deliberative rhetoric as concerned with planning and theorizing, while epideictic (or demonstrative) rhetoric focuses on debate and ceremony, and forensic rhetoric with reconstructing and interpreting past events for judicial purposes. To simplify, the three genres might be seen as corresponding broadly to the three arenas of Athenian political life—policy, leadership, and the courts.

Aristotle tells us that we are persuaded essentially by three things: logic (facts and reason), character (the impression made by the speaker, the moral force of the argument), and emotion (anger, sympathy, pride, etc.). Clearly, elements of both the logical and the psychological are intertwined inextricably in rhetoric. Much as we might like to have all exchanges based only on the former, the latter is part of our makeup. Are we not ahead if we consider our scholarly communications to employ both (as they certainly will since science is a social enterprise) and base our mutual understandings of each others' work on rhetoric—a discipline that takes explicit account of this?

"Positivist" and Interpretivist Epistemologies Share Common Rhetorical Practices

Consider both statistical inference and the hermeneutic circle. While both serve as the basis for knowledge claims in the social sciences, they rest upon different epistemologies that many consider to be opposed and irreconcilable. Must we therefore choose sides, write for different journals, and ridicule the other camp as narrow-minded and backward? Can anything at all be shared between these camps?

In statistical inference, a hunch is played (the hypothesis), observations are gathered on the relevant variables, and techniques based on probability theory are employed to discern whether patterns in observed data conform to the hypothesized relationships. Competing hypotheses are often formed (at the very least, the null hypothesis is always in the game) and a contest between these competing hypotheses—an interplay between the various hunches and the set of observations—leads us to finally take a stand, to make a knowledge claim.

Meanwhile, the hermeneuticist begins with a "text" to be interpreted. The text could be sacred scripture, the script of a play, the evidence from a court case, a sculpture by Henry Moore—indeed Dilthey considered "all manifestations of the human spirit" to be fair game for hermeneutics. Fragments of the text—symbols, words, phrases, characters in the play, and the like—are placed in their larger context, understood and given meaning through their location in that context, which itself comes to be reinterpreted as new meanings and understandings are attached to its component fragments. This is the hermeneutic circle—the continual reinterpretation of parts and whole in terms of each other.

Given the differences in these approaches to knowledge creation—even their view of the underlying ontology of social "things" is different—it is all the more interesting that they employ the same basic technique for weighing their hunches against evidence: the hypothetico-deductive method. Regardless of prior methodological or epistemic commitments, these researchers will observe and form hunches (or begin with hunches and then observe—the sequence is inconsequential to both camps). They then assess whether the bits of information they have gathered fit the interpretation they have posited, or they consider the fit of competing interpretations with the same basic set of "facts" they have gathered on their subject. In other words, whether the researcher starts with observations or hunches before moving to the other, she will at some point ponder, "if my interpretation or hypothesis is correct, then I should observe . . . ." And when those information fragments conform to the clause following "should observe," the *inference* that the interpretation/hypothesis is strengthened is made. Simply put, they all practice the hypothetico-deductive method.

It will surprise no one at all that I make this claim about researchers who employ data analytic techniques. To deduce observable implications from hypotheses and weigh those hypotheses against observed data is a defining characteristic of the method itself. But it will surprise at least a few who read here that the hypothetico-deductive method is just as central to interpretivist approaches. In fact, Dagfinn Føllesdal (1979: 319) noted "*. . . the hermeneutic method is the hypothetico-deductive method applied to meaningful texts*" [emphasis in original].

The truth in this claim is seen most clearly when we consider how contending interpretations of a single "text" battle for broader acceptance from their relevant audience. In the same article just cited, Føllesdal follows five contending interpretations of the character of The Stranger in

Henrik Ibsen's *Peer Gynt*. Drawing from different writers in the field of literary criticism, we find that The Stranger has been alternatively viewed as representing Anxiety, Death, Ibsen himself, the Devil, and the ghost of Lord Byron. How is it possible to establish whether any one interpretation is reasonable? Can some interpretations be seen as more powerful than others? Føllesdal notes that proponents of each of these five contending interpretations justifies the claim by reference to fragments of the play's text. Even more striking is the fact that contests between these interpretations had a particular form: those that seemed to fit more aspects of the play (i.e., more in number or more central to the play's meaning) were judged superior. And such contests are not deflected by the fact that the very meaning of the play itself can shift in some ways depending upon whether we accept one interpretation over another.[14] The hermeneutic circle is indeed at work, parts and wholes are continually reinterpreted by reference to the other. But judgments regarding the accuracy or acceptability of any interpretation in this process are based upon the fit between our evolving understanding of the pieces and the whole. Contending interpretations gain or lose ground depending on how well fragments and wholes piece together with one another. And this "fit" is assessed by no means but the hypothetico-deductive method.

Falsifiability, Reproducibility, and Shared Rhetorical Practice for Social Science

Is the hypothetico-deductive method a "first principle" of research or simply a shared convention? If we consider social science as rhetorical practice, then it is quite sufficient to take it as shared convention. We can be even more optimistic when we realize that additional practices are shared between "mainstream" researchers such as King, Keohane, and Verba and those working in phenomenological, interpretivist, and hermeneutic traditions. Hopf, Kratochwil; and Lebow (2001) point to the following norms shared by these approaches:

> ". . . a clear differentiation of premises from conclusions, respect for the canons of inference, recognitions that some standards of validation must be established for data and source materials, differentiation of correlations from cause; the recognition of spuriousness problems when we rely on correlations, reliance on logic in the establishing warrants for our assertions, and the belief that all findings can be contested."[15]

Would we not be better served, then, to build upon commonly shared practices than to dwell upon differences?

None of this means that the hypothetico-deductive method is the only method employed by researchers in either tradition. Føllesdal points out that an argument that knowledge *must* be hypothetico-deductive would contradict the very point of the method itself—that all our insights are tentative and knowledge is never certain (1979: 325). Its common use among us serves as a bridge, not a limit. A tolerant and pluralistic social science does not require perfect communication, it simply requires that effective communication is possible across approaches, methods, and epistemologies. Hence, what role do falsifiability and reproducibility play in enabling this communication?

If effective communication is our basic need, then it serves us to consider social science as rhetorical practice, for effective communication is the very purpose of the art of rhetoric. As "reasoned argument supported by evidence," the techniques of rhetoric are meant to aid in our persuasion of others to the claims we are making. And, according to Aristotle, the *summum bonum* of effective communication is clarity (Lawson-Tancred 1991: 34). To achieve clarity, the speaker must narrate the facts and prove the case; ". . . to state the subject matter, and to demonstrate it," as Aristotle himself put it.[16] In contemporary social science, the act of "stating the subject matter" involves stating the specific questions that motivate our research and the knowledge claims that arise from the results of that research. If these hunches and claims cannot be stated in a *falsifiable* manner; if they are presented as "truths" through appeal to ideology or through casuistry, then our scholarly discourse has indeed devolved into "empty rhetoric." Meanwhile, fair and persuasive demonstration of the case presumes that the audience can follow how the evidence was gathered and how the speaker moved from this evidence to the claim being weighed. In other words, the speaker presents the reconstructed logic of her proof in a way that the audience can *reproduce* it. Falsifiability and reproducibility are central elements in clear rhetoric. They are essential to clear communication. And they can serve, therefore, as cornerstones for a pluralistic social science.

Conclusion

Designing Social Inquiry offers a starting point for those interested in the construction of a social science that is tolerant and pluralistic in the methods and epistemologies its members practice and hold. The larger project I describe is incomplete. Still, *Designing Social Inquiry* can be used to begin our discussions. King, Keohane, and Verba have it right when they say that "the goal is inference," *provided* that our own understanding of the term is broader than that implied in their book. Rather than limiting ourselves to forms of statistical inference alone, we must consider inference broadly writ as "the upgrading or adjustment of belief in the light of the play of new information upon current beliefs" (Honderich 1995). In this light we see that "mainstream" and interpretivist scholars alike weigh their hunches against evidence, and they do so in ways that it is often difficult to say which came first: the hunch or the observation? Furthermore, the interplay of hunches and evidence for researchers in both camps relies centrally upon a number of norms and practices already shared, including the hypothetico-deductive method. Moreover, all of us accept that the nature of that method dictates that our hunches always remain tentative. Knowledge is never absolute.

When we look beyond the more specific methodological practices prescribed in the book, we see that all can be subsumed under the principles of falsifiability and reproducibility. Falsifiability—quite distinct from falsificationism—and reproducibility presume no epistemological commitment to logical positivism. Instead, they underpin the value of clarity in good rhetorical practice that follows Aristotle's dictum to "state the subject matter and demonstrate it." More specific techniques for doing so in social science's *Erklären* tradition can be found in *Designing Social Inquiry*. More specific techniques for doing so in the *Verstehen* tradition can be seen by example in a number of good interpretive works already published. They deserve to be elaborated and codified in future discussions within our community.

In the end, is my position simply foundationalism in disguise? No. Seeking common ground to enable communication is not the same quest as the search for methodological foundations derived from first principles independent of human thought and language. I seek this common ground because it is inescapably necessary to any pluralist social science. Consider the alternatives. Methodological dogmatism is, by definition, the opposite of pluralism. And radical relativism obviates pluralism by making all claims equally legitimate. It is a mockery of pluralism. The failure of the logical positivist program shows us that methodological rules cannot be derived from first principles. Hence I cannot call myself a "foundationalist." But in rejecting the radical relativism of, say, Paul Feyerabend, I am no more inclined to claim a position as an "antifoundationalist." So perhaps my position might be characterized as a "nonfoundationalist," and the methodological substructure beneath my researcher's feet is actually the deck of Neurath's Boat. As early as the 1930s, Otto Neurath, one of the earliest members of the Vienna Circle, recognized the profound problems embedded within logical positivism and indeed the whole foundationalist program. "We are like sailors . . . ," said Neurath of our quest for epistemological underpinnings, ". . . sailors who have to rebuild their ship on the open sea, without ever being able to dismantle it in dry dock and reconstruct it from the best components."[17]

A tolerant and pluralistic social science can embrace the *Erklären* as well as the *Verstehen* traditions. There should be no further controversy about abandonment of methodological monism, falsificationism, the quest for covering laws, or the substitution of statistical significance for human judgment. Even those of us who work within the *Erklären* tradition rejected these long ago. Similarly, we can all readily recognize the place of contingency in human behavior and social relations; the historical and geographic boundedness of the patterns we observe; the socially constructed, mutable, and evolutionary character of what we study. At the same time, it would serve us to recognize that the general means by which we weigh evidence against our hunches—the hypothetico-deductive method—is already shared, and thereby it serves as a channel of communication. Further communication within and between traditions would be enhanced if we recognized that falsifiability in our claims and reproducibility of our results both contribute to the clarity needed in good rhetorical practice. The "pluralist project" is very much a work in progress and always will be so.

Notes

1. I thank Richard Ned Lebow, Fritz Kratochwil, and Ted Hopf for many insightful comments on earlier drafts. I also gratefully acknowledge the continuing support of the Mershon Center of the Ohio State University for the work represented here.
2. To be precise, Popper, Ayer, and Nagel were not members of the Circle, but were attracted to the *project* the Circle had laid down. Popper's work would become central to the overthrow of the verification principle (Edmonds and Eidinow 2001).
3. It is important to note that Popper always recognized this problem and was never as doctrinaire about the power of falsifying evidence as his critics claimed. The reader will find that many of the criticisms of falsificationism raised by critics were described (and already accepted!) by Popper by the mid-1930s. Popper takes pains to point this out in his 1983 *Postscript* including specific citations to his 1935 *Logik der Forschung* (see esp. Popper, 1983: xix–xxv).
4. This is one facet of a set of logical problems known as "Underdetermination."
5. Readers may be familiar with the late Stephen J. Gould's extensive discussion of the role of contingency in evolution, found in his book *Wonderful Life*, or with the counterfactual thought experiments beloved by historians such as "Cleopatra's Nose"; i.e., how different Roman history could have been if only Cleopatra had been born with a large, unattractive nose.
6. That is, it is only the human mind, not any logical necessity, which bestows "causation" upon some factor X moving some phenomenon Y.

7. Once logical positivism's star had traversed the sky, A.J. Ayer was asked to explain its passing. "Well," he averred, "I suppose that the most important of the defects was that nearly all of it was false." (Edmonds and Eidinow 2001: 157).
8. The authors note that their notion of "falsifiability" is taken straight from Karl Popper's *Logic of Scientific Discovery*. But Popper would later elaborate the position he took in that 1935 book, and, as written, *Designing Social Inquiry* seems to rely on the early Popper. Still, I consider Popper's later, more sophisticated fallibilist position to be perfectly consistent with King, Keohane, and Verba's meaning and with their objectives.
9. Popper took pains to distinguish the two—essentially describing "falsifiability" a logical necessity for science, and "falsificationism" as a doomed methodological quest. See Popper (2002 [1959] [1935]: 66 and more broadly, chaps. 4 and 10). Supporters and critics of Popper alike often seem to miss this distinction.
10. Regarding concept formation, the criterion of reproducibility may have special importance in that the concepts we employ (e.g., "democracy," or "power") can never be as free of ambiguity as we would like. "Contested concepts," the differences in the understanding that various scholars have of a given concept and inescapable imprecisions in their formation, will always inhabit our theories and knowledge claims. The principle of reproducibility requires that we communicate what we mean when we invoke a particular concept as clearly as possible.
11. In fact, as a researcher in the field of International Relations for over twenty-five years, I must say that I find published research in IR often fails to satisfy conditions of reproducibility. This is true across the full range of methods: case study, large-N data analysis, formal models or simulation. Along with my students, I regularly find many examples of nonreproducible research in case study, data analytic as well as formal model traditions. I have no doubt that this is a central reason why so many of our debates seem never to engage in any productive way.
12. Even recognizing the theory-ladenness of our observations and highly problematic nature of "tests" of our theories, a fallibilist notion of conformity between conjecture and observation transcends different epistemological traditions (Laudan 1990: 35, 112–13).
13. Plato's criticisms of rhetoric as a subject worthy of study and practice are laid out in the *Gorgias* and in the *Phaedrus*. The trial and suicide/execution of Socrates left Plato deeply suspicious of democracy including techniques of mass persuasion by speech. To put it perhaps too simply, the fact that rhetoric could contain more artfulness than art corrupted its value as a subject worthy of study in Plato's view. See Lawson-Tancred (1991).
14. Multiple interpretations are also possible. The Stranger might best be interpreted, for example, as both the Devil and the ghost of Lord Byron. But this position would be justified in its own right and against contending interpretations in exactly the same way as any other—using the hypothetico-deductive method.

15. They do point to important divisions within the diverse reflexivist community. Importantly, some postmodern and critical approaches do not accept these norms (Hopf, Kratochwil, and Lebow 2001).
16. See Lawson-Tancred (1991: 34–45) and Aristotle's *The Art of Rhetoric,* chap. 3.13 (page 245 in the cited edition).
17. Quoted in Edmonds and Eidinow (2001: 163).

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5

Methodological Pluralism and the Limits of Naturalism in the Study of Politics

Fred Chernoff

In presenting their alternatives to King, Keohane, and Verba (KKV), Friedrich Kratochwil, Ted Hopf, and Brian Pollins bring out several variations on the interpretivist and scientific approaches to the study of politics. They consider questions in the theory of knowledge (what constitutes and justifies knowledge claims and valid forms of inference), metaphysics (the nature of truth, causality, and the real world), methodology (especially the appropriate degree of pluralism), and the merits of naturalism, that is, the scientific approach to the study of politics.[1] These topics are discussed in the sections that follow.

Kratochwil focuses on the nature of scientific inquiry and on the sort of truth that is thus attained. The first is largely an epistemological undertaking, the latter squarely metaphysical. Hopf argues that the interpretivist approaches, as typically presented, exaggerate the differences between themselves and the mainstream, and that mainstream approaches similarly exaggerate the differences. He says that mainstream theorists overstate knowledge claims, while interpretivists overstate assertions of purity and make use of some mainstream methods without acknowledging them. And Pollins argues that interpretivism and the mainstream are often incorrectly portrayed as competitors, whereas they are more properly understood as complimentary methods of analysis.

The authors of the three preceding chapters generally agree on a rejection of the naturalist approach to social science theory, such as that adopted by KKV, who do not probe questions in the philosophy of science.

As Ned Lebow points out in the chapter 1, KKV aim primarily at producing a "how-to" manual but they make use of important views in the philosophy of social science. Their book draws on presuppositions of the latter and carries implications for the former.

The first two sections of this chapter examine some of the views developed by Kratochwil, Hopf, and Pollins on the theory of knowledge and metaphysics, respectively. The third section contrasts the views of Hopf and Pollins on methodological pluralism. The fourth section raises the question of acceptability of naturalism and the importation of natural science methods into the social sciences. This chapter part company with those of Kratochwil, Hopf, and Pollins by arguing that the rejection of naturalism is logically more difficult and methodologically more costly than they seem to recognize.

The Theory of Knowledge

Kratochwil

Kratochwil argues that a nonfoundational theory of knowledge does not lead inevitably to a dangerous relativism. Kratochwil is quite right here but the point is well known. The past century and a half has seen many nonfoundational theories of knowledge, most of which are not vulnerable to charges of the sort of dangerous relativism of which Kratochwil speaks. It is possible to be a realist with respect to ontology, and a nonfoudationalist with respect to knowledge, as demonstrated by the position taken by Peirce and his followers.

By the late nineteenth century, Charles Sanders Peirce was arguing powerfully against all foundationalist theories of knowledge, especially against Cartesianism. Peirce had immense influence, chiefly in the United States, though many of his views were transmitted through followers (particularly James and Dewey) who, during his lifetime, had much larger international audiences than Peirce himself. For Peirce, foundationalism is unacceptable because scientific knowledge is fallible and provisional while continually moving toward knowing an external reality. Peirce is thus clearly a philosophical realist in terms of ontology, but his theory of knowledge is antifoundationalist.[2]

Peirce believes there is a reality out there, but scientific knowledge deals only in what is observable, which is what confers "cash value" on scientific statements. That is, the cash value derives from the specifiable observations that are made under specifiable conditions. The totality of the set of observations constitutes the meaning of the concept and, according to

Peirce, the meaning of a "clear concept" is the entire set of its practical consequences.

For Peirce, in contrast to Kratochwil, the essence of science is that the reproducible method[3] and the proper application of the method by different investigators will, over time, converge toward some truth. In the years after Peirce wrote, his view of science became quite widespread without invoking foundationalist theories of knowledge. Since Peirce has been described (e.g., by Popper) as one of the greatest philosophers and one of the greatest logicians who has ever lived, it is reasonable to accept for present purposes here that the combination of Peirce's theory of knowledge (antifoundationalist) and metaphysics (realist, and certainly not relativist) is at least prima facie coherent. Peirce's many followers over the past century and a half have held both ontological realism and epistemic antifoundationalism. Thus there is nothing particularly surprising or helpful in Kratochwil's claim that antifoundationalism need not lead one to relativism. And his antifoundationalist argument does not require us to abandon mainstream philosophers' theories of knowledge.

Kratochwil makes references at one point (e.g., p. 44) to "pragmatism," though without citing Peirce, James, or others. But Kratochwil moves on after offering one apparent paradox in the form of a rhetorical question. Kratochwil (p. 18) says that answers "seem to depend in turn on a variety of criteria, including pragmatic ones (it works!). However, such a criterion is not foundational either, since the attendant validity claims can be defeated: 'Yes, it works, but is it true?' What would we do if 'it' worked no longer? . . . ." Presumably Kratochwil here implies that there is no good answer to such questions and, therefore, pragmatism is fatally flawed. But pragmatists have straightforward answers to these questions. On a pragmatic theory of truth like that of James (1907/1975), what it is for a statement to be true just *is* what is the case when we correctly say "it works." The relationship of a statement to our experience and to our ability to navigate the sensible world is what we properly mean by a statement being true. The usefulness of the belief over the long run (the belief, in this sense, "working") is that by virtue of which the belief is true, which is explicated in detail by Peirce, James, and others. The truth will correspond with the facts, though this is not a correspondence theory given by James's theory of being, because if the facts are themselves just mental constructs that have proved useful, then there cannot be a case of a useful belief that does not agree with the facts (cf. Kirkham 1992: ch. 10). For James, reality consists of useful mental constructs, those that aid us as we manipulate objects, communicate, explain, and predict. Our notion of "true" helps us think well, just as our notion of "right" helps us behave well (James 1907/1975: 106).

Kratochwil overlooks the pragmatist option when drawing one of his major conclusions. He argues that "it neither follows conceptually nor empirically that those who are critical of foundationalist projects are 'nihilists' in the sense that they argue that there are no standards, or that anything goes" (p. 29). This is entirely true and many naturalists would agree. The problem here is that a rejection of foundationalism does not allow us, contrary to Kratochwil's comments, to infer that his interpretivist project is the appropriate alternative. For well over a century, noninterpretivist philosophers of science have been producing pragmatic theories of knowledge and of truth that endorse many traditional notions of scientific method and inference but that vehemently oppose foundationalist theories of knowledge.

Hopf

Ted Hopf's chapter attempts to correct errors in both "mainstream" and "interpretetivst" methodology and to develop an alternative. He says that the two would, at best, suffer from "mutual incomprehension." Hopf hopes to give interpretivist epistemology a methodological foundation that will permit it to yield justifiable inferences. He offers a sort of synthesis that seeks to avoid violating the fundamentals of that interpretivist tradition by focusing on intersubjectivity, open social systems, and meaningful difference.

Hopf identifies several central epistemological problems in the comparison between the mainstream and interpretivism. The first is "unit homogeneity," where, Hopf (67) says that the mainstream assumes that all is meaningfully identical, until proven otherwise, while "Far from accepting the default of identicality, interpretivism assumes the principle of difference" (Hopf p. 67). Hopf suggests the mainstream's account, unlike that of interpretivism, is untenable. He says, the interpretivist would "suspect the mainstream has already operated on a case if it is able to identify it as a case. In other words, while the mainstream treats the knowledge of what constitutes a case of something as self-evident, the interpretivist would argue that case-selection already prejudges facts not yet in evidence" (p. 64). This statement gives the impression that mainstream theorists have a single, rigid set of rules for identifying each social observation into a specific "case-kind." That is, Hopf sees the mainstream, but not interpretivists, as treating the set of observations that constitute our knowledge of a case (such as World War I) as only a case of a single, specific kind, and as necessarily so. But in fact mainstream theorists may use the set of observations of a case (such as those producing knowledge of World War I) to constitute examples of different kinds of cases. A theorist in that tradition may regard World War I as one sort of war in one study, e.g., as a case of a war of democracies against non-democracies, and as another sort in another study (e.g., as a case of a great power war) and in still another as a case of war involving unconventional (poison gas) weapons.

Hopf later says, "The mainstream, to make its epistemological claims stick, would have to defend the logically impossible assumption that there is nothing meaningfully unique about any of the subjects and actions about which they theorize. Votes are votes. Wars are wars. Power is power. Everywhere and for all time" (p. 67). But the naturalism of KKV's mainstream view is defensible on this point. What mainstream authors do is reasonable, since they view votes or wars as identical *in a particular respect for a particular inquiry*. In a similar way, we may focus our attention on one aspect of a war for a particular inquiry, such as that one-third of belligerents in the war were democracies or that four of the belligerents were great powers. But this does not preclude those scholars from thinking about different aspects of the same wars for the purpose of answering a different question. Just as mainstream authors may consider a factor, such as revolutionary state, as a dependent variable in one study and an independent variable in another study, they may look at one aspect of the War of 1812 or the Crimean War for a study of democratic peace and another aspect of those wars to treat them as cases for a study of great power rivalry.

It is important to note a general logical-rhetorical difficulty involved in any appraisal of Hopf's chapter, which arises from his use of terms. Hopf offers a contrast between "interpretivism" and "the mainstream." He argues that both overstate some aspects of their methodology. He offers a synthesis that is very close to interpretivism but surrenders some points in favor of mainstream views. In his generally positive and sympathetic characterization of "interpretivism" he cites a variety of major figures in that tradition, including Gadamer, Taylor, and Bourdieu. But in his generally critical characterization of "the mainstream," he refers only to a single work, KKV's *Designing Social Inquiry*. So long as Hopf is explicit in defining his terms (which he does in his note 1) and consistent in using them, it is possible to appraise his argument. This seems reasonable, since the present volume offers an evaluation of KKV's text. However, Hopf's terminology does create some confusion and it sets up an apparent straw person on the mainstream side. If Hopf shows that KKV err on a point, then it misleadingly appears that he has found a flaw in the broader mainstream tradition. It also seems to suggest that on this point KKV err and interpretivism offers a superior account. But this does not follow, since we must bear in mind that a successful attack by Hopf on "the mainstream" shows only errors in *Designing Social Inquiry* and still leaves wide open the possibility that many other mainstream authors (i.e., quantitative scholars and others in the naturalist camp) are entirely capable of presenting satisfactory responses.

Pollins

Pollins attempts to extend the notion of "inference" used by KKV. But by citing a faulty definition, he ultimately overextends it in one direction while unacceptably shrinking it in another. Pollins says, "We can begin with KKV's chief defining characteristic of scientific research: 'The goal is inference' " (p. 93). Pollins regards KKV's notion of "inference" as too narrowly focused on statistical inference. However, in making his case, Pollins (p. 103) cites a source that, while adequate for his immediate purpose, nevertheless offers a definition that is so narrow as to micharacterize the meaning in philosophical contexts. Pollins quotes the definition in the *Oxford Companion to Philosophy*: ". . . the upgrading or adjustment of belief in light of the play of new information upon current beliefs" (Honderich 1995).

While Ted Honderich is a very able philosopher, this particular definition is mistaken and does not capture the standard philosophical meaning of the term. It discounts, for example, important rules of classical deductive logic such as "simplification" (according to which one may deduce "P" from the premise "P and Q"). In moving from the premise "P and Q" to the conclusion "P," no new information is involved. Thus while it is one of the paradigmatic forms of valid inference, it adjusts belief in the absence of any "new information." We must also be careful about the term "information" when applying this definition to nonempirical inference; and such inference is certainly required in political science and the other social sciences, especially in dealing with a theory's policy implications. Some questions will turn on strict equity or moral issues, and the inference from one set of propositions to a conclusion may not be the result of "information" but rather of moral judgments. Someone could presumably conceptualize "information" in such a way as to include moral judgments. But that would, at best, be a debatable move.

Contestibility, Fallibilism, and Falsifiability

Both Hopf and Pollins talk about "contestability," "falisfiability," and "fallibilism." Their use of theseterms needs some clarification. Hopf discusses this in the context of his survey of the ground shared by both the mainstream and interpretivism, as they are ordinarily conceived. Hopf identifies seven of them, the last of which is a shared "belief in the need for the contestability of findings" (p. 57). He is correct on this point. However, Hopf's explanation of this common ground is somewhat unclear. In his explanation of the last of the seven points he says that interpretivists "of all people, believe in the *intrinsic* contestability of all their truth claims because of their antifoundationalist skeptical epistemology. Ergo, interpretivism believes in falsifiability, in spades" (p. 58).

Hopf's comment about "intrinsic constestability" that results from the "antifoundationalist, skeptical epistemology" sounds very much like the principle that any knowledge-claims arising from theoretical or other empirical investigations do not purport to have the status of "final truth of the matter." But this sort of contestability principle is ordinary referred to as "falliblism." We must be careful to distinguish these two notions. *Fallibilism* is a doctrine about the status of knowledge claims with respect to certainty; it holds that there is always a possibility (indeed likelihood) that future inquiry will overturn our knowledge. Fallibilists hold that, while scientific knowledge has great value, our best scientific theories may be, indeed probably are, wrong. *Falsifiability* is a doctrine about the empirical content of scientific knowledge-claims. It is consistent with a foundationalist account of knowledge, and with the claim that the existing scientific theory in a particular domain is the final truth of the matter. In other words, it is possible to accept falsifiability while rejecting fallibilism.[4]

To clarify the distinction further, fallibilism holds that some of our accepted scientific principles and theories may, in fact, be true. But it is not likely that they are. A statement or theory is falsifiable if it is capable of being shown inconsistent with a logically possible observation. Obviously, any accepted principle or theory is thus-far-unfalsified, since Popper's view (1965, 1968) is that once it has been falsified, then investigators will seek to replace it with a rival that has not been falsified. Many of the primary authors of classical physics, quantum mechanics, and other major innovations in physical science believed that their theories were the final answers to the questions they asked. They believed that they achieved a level of certainty that social scientists—and as Hopf points out, especially interpretivist social scientists—would never claim for their researches. But these physical scientists also believed that their theories were falsifiable. That is, in a different physical universe, their theories would be shown to be inconsistent with experimental observations.

One final point is in order in connection with the important difference between fallisbilism and falisifiabilty that is related to the interpretivist-mainstream divide. Interpretivists are clearly fallibilists. But they also hold that there are multiple possible interpretations of any observation or purported fact. And they typically do not believe that there are better and worse interpretations of a fact or observation, certainly none that could be said to be better or worse on "objective grounds." This is particularly true of poststructuralists. Other interpretivists are more open to the idea of better or worse interpretations, but they avoid the claim that one interpretation is the "true" interpretation. If there is such a wide array of choice as to which interpretation should be accepted, then Hopf's claim that interpretivists share with mainstream authors the view that all (social scientific) knowledge is falsifiable stands in need of some clarification.

Pollins uses the term "fallibilism" several times in his chapter, but he does not go far enough in explaining how widely accepted the view has been in displacing not only interwar logical positivism but also other foundationalist theories of knowledge.[5] As noted, most mainstream philosophers endorse some version of fallibilism. To be sure, twenty-first-century philosophy is replete with variations, competing theories and approaches, some radically different from others. However, most share a fallibilist orientation. The idea that empirical knowledge can be established on a base of absolute certainty is defended by a minority, albeit a formidable minority, of philosophers (Chisholm 1957, 1966, 1989, Firth 1998, Bonjour 1985).

The rejection of foundationalism by many philosophers and the advance of fallibilism do not, by any means, imply relativism. Nor do they leave us with only the versions of intepretivism that Hopf and Kratochwil endorse. Pollins correctly points out that "the demise of logical positivism does not imply that relativism rules" (p. 88). The arguments of poststructuralists and hermeneuticists against logical positivism commit the fallacy of the straw person because many of those in the philosophy of the social sciences who endorse naturalism and the scientific approach to social theory and political science are also fallibilists.

Metaphysics

Causality

One of the most notoriously difficult metaphysical problems philosophers have faced is that of causality. In his discussion of causality, Kratochwil generally remains quite aware of the dictum that there are no "ultimate" or "highest level" questions (and theories to answer them) in IR to which other questions (and theories) are subservient. This is evident, for example, in his denunciations of the unity of science thesis. Nevertheless, Kratochwil fails to account for the wide divergence in the sorts of questions raised in IR and for the diversity of theories IR proposed to answer, some of which are properly causal. IR may legitimately ask causal questions. Does democracy cause peace? Does peace cause prosperity? The problems of self-referentiality that Kratochwil identifies do not provide good reasons to abandon, for example, the very successful study of democratic peace hypotheses or the role of hegemony on international stability. Kratochwil is correct in holding that "all attempts to reduce intentional accounts . . . to mere causal and/or observational statements must fail" (37). Nevertheless, there are some questions (such as those suggested in the discussion of methodological pluralism below) that are best pursued by means of the positing of cause-and-effect relationships.

Kratochwil says, "Besides, one could also maintain that not all interesting questions in the natural as well as the social sciences are of a causal nature as some concern 'what' questions, that is, problems of constitution rather than causality" (pp. 25–26). Kratochwil seems to go further in his critique of causal reasoning by condemning all causal theorizing. If he is only claiming that some questions are not amenable to causal theorizing and the methods advocated by KKV, then he is making a very modest claim. It is, moreover, a correct claim, which paves the way for the sort of methodological pluralist position that Pollins advocates—a position that allows both interpretivism and the mainstream or scientific methods developed by KKV. The problems that Kratochwil may encounter with such a line of argument arise from the question of precisely where he would draw the line between questions that are and are not amenable to causal reasoning and KKV's sort of analysis. Since he does not do this, then in that context the attack appears aimed at all causal claims.

Hopf says that both mainstream and interpretivist theorists recognize the difference between correlations and causation but notes that some interpretivists want to avoid the use of "causation." He says, "Some interpretivists have a problem with even using the word causality" (p. 57). The parallel is a bit stronger than Hopf observes, as many empiricists have wanted to avoid "causal" claims. Russell (1918) said that talk of causality in physics is doubly like attitudes toward the monarchy: both are thought to be harmless and are mistakenly so regarded.

Hopf correctly identifies a central problem in the social sciences as to whether there are "social kinds" akin to "natural kinds" in the natural world. This is central to Hopf's treatment of "meaningful identicality," according to which, as he sees it (p. 67), the mainstream assumes and interpretivists deny and rather assume uniqueness or "the principle of difference." Hopf's claim about the superiority of interpretivist approach seems rather overstated. The problem he notes is not inherent in the nature of social inquiry. It affects some but not all investigations. This follows because there are straightforward instances where the position that units are meaningfully identical is justified. Consider the hypothesis that large increases in home ownership in North America are caused by large increases in the marriage rate. There are palaces and there are hovels; there are happy marriages and there are miserable ones. But there does seem to be a sort of social analogue to "natural kinds" when we contrast home-owning people from non-home owners. (The existence of troublesome borderline cases does not discredit the claim that there is a meaningful and useful distinction.) And there appears to be a similarly reasonable distinction between married and unmarried people. This is a legitimate social science hypothesis that may be investigated without a belief that home owners are (perniciously) meaningfully identical in this regard and without a belief that the concept "married people" engenders any logical difficulty. We may pose questions about the accuracy of the asserted causal connection between home ownership and marriage rates. But there does not seem to be a serious problem over whether there is a social-causal structure that connects married people to one another or home owners to one another, by virtue of which the classifications "home owner" or "married person" lack legitimacy.[6] Hopf does later acknowledge that interpretivism overstates its conception of "difference." Nevertheless, whether a researcher may assume that units are meaningfully identical or are unique by their nature is a practical and theoretical problem that must be treated case by case in the social sciences; "difference" is not inherent in all social inquiry.

Pollins says that both hermeneutic and causal arguments make use of the hypothetico-deductive (h-d) method. Hopf and Kratochwil have more reservations. Hopf rejects any generalization across cases. And Kratochwil says that "causal hypotheses [stated] without taking the state of mind of the actors themselves into account are not particularly useful" (p. 38). It is thus interesting that Hempel, who is most associated with the development of the h-d method, would have been reluctant to go along with the view Pollins (pp. 99–101) expresses about the acceptance of the circularity of hermeneutics. But those of us who endorse a pragmatic theory of knowledge (or almost any form of nonfoundationalism) have no difficulty agreeing with Pollins here. Hempel, in contrast, supports such generalization and argues that in causal reasoning, general propositions are developed prior to singular statements. Perhaps the subtlest and most careful analysis of the notion of "cause" in international relations, at least with respect to "causes of war," is that of Hidemi Suganami (1996), who argues for the reverse, that is, that singular statements about the causes of specific wars must be prior to general statements (1996: 151). On a pragmatic theory of knowledge, the two types of statements are cogenerated; each new proposed causal statement, whether general or singular, is combined with others. One must then determine if the overall body produces a better or worse overall account.

The cogeneration does engender a circularity, but it is a virtuous rather than a vicious circle because it is presented here in the context of a metatheory that eschews foundationalism in favor of fallibilism (as most twenty-first-century metatheories do). If there were an identifiable stopping point of analysis, this sort of circularity could present an epistemological problem. But in this context, the discovery of causes of particular wars and of war in general mutually reinforce one another (or discredit previous hypotheses about war(s) in particular or in general) and in so doing make way for new analyses. The cogeneration relationship may present difficulties for supporters of foundational theories of knowledge. But there is no such problem for those who endorse pragmatic or other nonfoundational theories, since there is always a reevaluation of existing relationships of support or confirmation as new evidence emerges. The cogenerative thesis only implies that new individual associations provide grounds for reassessing the support of general hypotheses and vice versa.

Kratochwil on Truth and "The World"

Kratochwil frequently contrasts his view of "truth" to that of others who hold that "truth is a predicate of the world." Once Kratochwil has disposed of that view, he sees the way open for progress in producing a more adequate interpretive/hermeneutic account of the social sciences. Kratochwil's characterization of past philosophers seeing "truth" as a property of "the world" is fully intended, as he states it a number of times.[7] It is not clear who Kratochwil sees as the target of his criticism. Because it is so difficult to find philosophers or scientists who hold that truth is a predicate of the world, it appears that the argument commits the fallacy of the straw person. Who has argued that the world, or things that are part of the world, have "truth" as a predicate of them? Who could make the argument that this tree, this rock, or that dog is "true"? Some philosophers have gone to extremes in the opposite direction, contending that truth is not a predicate at all. F.P. Ramsey, in his historic 1927 essay, argued that the statement "s is true" is equivalent to and says no more than "s" (Ramsey, 1990). But those who do regard it as a predicate regard it as a predicate of statements, propositions, or sentences, and not as a predicate of the world.

Perhaps Platonism, viewing truth as a Form, comes as close as any position to what Kratochwil has in mind. But Plato is generally interpreted as endorsing a correspondence theory of truth, according to which there is a correspondence between a true proposition and the world. The relation of "corresponds to" is very different from that of "predicate of." In general, though, even traditional correspondence theories view truth as a *relationship* between a statement and external reality. Correspondence theories are not vulnerable to Kratochwil's argument against truth as a predicate of the world. And even if they were, contemporary metaphysics is replete with noncorrespondence theories of truth. Thus Kratochwil's conclusion that truth is not a predicate of the world does not "clear the way" or resolve any confusion and *a fortiori* does not pave the way for his alternative, as many philosophers of the past century have eschewed correspondence theories of truth and foundational theories of knowledge, and none (that I am aware of) suffered from confusion about truth being a predicate of the world. Moreover, epistemological antifoundationalist philosophers come in all ontological varieties, ranging from instrumentalists to empiricists to scientific realists (such as Peirce, noted above), without needing to turn to any severe form of skepticism.

The import of Kratochwil's argument about truth as a predicate of the world can perhaps be regarded as his conclusion that scientists do not test "against reality." This would be a much more reasonable conclusion and one that (in contrast to "truth and the world") has at least some adherents. Just what is a theory tested against, if not reality or some "effects" of reality, such as our observations, however theory-laden they may be? Even if observation is theory-laden, there are different philosophical views about how problematic this is for theory-testing. Dretske (1969), for example, argues that there are different senses of "to observe" (or, to use his specific example, "to see"). In any case, what is it that serves as evidence for or against one theory when it is compared to its rivals? Even the "court of judgment" Kratochwil suggests (which Pollins endorses, p. 88) seems to need recourse to evidence that ultimately comes from observation. Such observations, however filtered or theory-laden they may be, are still effects of (caused by) the intersubjective world.

Pollins on Communication, Rhetoric, and Objectivity

Pollins says, "Nothing can be more basic or essential to our work than communicating our findings and claims to our colleagues for their consideration, evaluation, criticism, denial or acceptance" (p. 97). Most researchers would probably disagree with this claim and answer, instead, that finding out the way the world works or discovering the truth is more fundamental to the social sciences, at least as a goal. (Poststructural theorists would offer yet another answer.) Communication is a means to achieve that goal and is a necessary condition of science, given humans' limited lifespans, intellects, and sensory organs. Communication is important because most researchers accept some version of the traditional goal of "finding out how the world works" in a way that somehow embodies, even if to a limited extent, the "objectivity" of science. Objectivity is enhanced the more the peculiar characteristics of the individual researcher are removed from the knowledge-building process. If different researchers come to the same conclusion, then there is less reason to believe conclusions arise purely subjectivity. Communication is an important tool in assuring that this is possible.

Pollins says also that the central elements of KKV are falsifiability and reproducibility and that both of these are essentially communicative concepts. Why does anyone endorse either reproducibility or falsifiability? For Pollins, presumably, it is because they are seen as means of reaching the truth or learning how the world works. Whether one does or does not believe in an unseen world or theoretical entia, the observable world of nature and human behavior can be made sense of and explained—with statements that are true. Any notion of "truth" other than the most radically subjective ones requires some means of overcoming the danger of subjectivity that arises from the fact that observations are made and theories proposed by individuals or small teams rather than by the entire community of enquirers in a collective manner.

In his summary of the failings of logical positivism and methodological falsificationism, Pollins avoids taking a position on the claim that there is a world "out there," but he does not deal in a direct way with "social science objectivity" elsewhere in the chapter. If "my results" are as "valid for me" or as "true for me" as yours are "valid or true for you," and if there is nothing more to be said on the matter, then we have a purely subjective field of inquiry. Numerous and obvious limitations of applicability follow from this sort of conceptualization of the discipline. If research is falsifiable and, even more importantly, reproducible, then other investigators are capable of making the same observations, using the same methods of analysis and if they do so, they should arrive at the same conclusions. In this context, the aim of falsifiability, and especially reproducibility, is to overcome the limits of a purely subjective field of inquiry.

Finally, it is worth noting that Pollins's attempt to examine the needs of a pluralist methodology leaves a gap, failing as it does to attempt to discern the nature of the reality that diverse individual researchers observe. In contrast, Kratochwil's argument, which presumably Pollins would not accept, is a serious attempt to fill that gap.[8]

Facts, Values, Truth and Progress

While neither Kratochwil, Hopf nor Pollins deals with the fact-value distinction directly, Pollins's position is the one that requires a separation between them. Hume in the eighteenth century and Weber in the nineteenth century each gave a good deal of consideration to the relationship between statements of fact and of value. Hume argues that one of the key errors philosophers have made throughout the ages stems from their failure to recognize that statements of fact can never be derived from statements of value, and vice versa. Pollins's treatment of the *Erklären* tradition in social science proceeds under the assumption that facts and values can be neatly and fully separated.

The philosophy literature has many counterarguments and counterexamples that have led to a general rejection of a hard-and-fast fact-value distinction.[9] Good social science should be applicable to the real world; it should, at least to some degree, aid policymakers achieve their goals. Lebow is right in saying that in the social sciences "normative theorizing must deal in facts just as empirical work must deal in values; they do not inhabit different worlds" (p. 12). He points out also that although distinguishable, political scientists must deal with both; it is possible to produce research that emphasizes one or the other. The problem arises when researchers pursue one while losing sight of the other and of the goals of social science.

It is helpful to stress the difficulty of denying "truth" or even "progress" in social inquiry and in the philosophy of the social sciences. If one argues for a metatheoretical position with the expectation that such an effort moves the debate forward in a genuine way (closer to the truth of the matter), then it becomes awkward to assert that the social sciences are incapable of genuine substantive progress (see Chernoff 2004). Those who engage in the debate and hope for progress (that is, uncovering the truth of the matter) are accepting some powerful presuppositions, such as that there *is* a truth of the matter. Those who do so should acknowledge them, as they have a bearing on the sort of the position that one may justifiably advance. Thus, if some authors of interpretivist, poststructural and/or constructivist theories doubt the possibility of progress in empirical inquiry and of discovering truths in any but the most subjective sense, then they must consider how that skepticism affects the goal of moving metatheoretical debates forward. If the latter is impossible, then what exactly is the purpose of debating or publishing one's views on metatheory?

Kratochwil on Theory Choice, the "Best Metaphor" and the Relation to Truth

One of the most crucial questions in the philosophy of science is how theories should be chosen. In this connection, Kratochwil raises the important problem of disagreement among practitioners. He says, "the procedures by which scientists try to persuade each other and attempt to decide what constitutes an anomaly (to be disregarded), and what is supposed to be taken as a refutation (requiring more or less serious repair of analytical apparatus), are far from the ideal type of a compelling demonstration" (p. 40). He advances his interpretivist solution by telling us where to find better metaphors. He supports metaphors of court proceedings and the assignment of burdens of proof and rejects evolutionary and market metaphors.

Kratochwil says, "[T]here will be several criteria by which various protagonists will attempt to eliminate alternatives and show that the weight of evidence favors one theory while . . . discrediting" others (p. 40). He concludes that "burdens of proof and of presumptions provide better metaphors for capturing the process by which warranted knowledge is produced, than the idea of demonstration by experiment" (p. 40).

The most serious problem with Kratochwil's solution here is that, unlike Popper, whom he cites, Kratochwil is attempting to solve problems in social science metatheory. His solution, which is to use the metaphor of a formal court proceeding or the assignments of burdens of proof, risks circularity. Social phenomena are to be understood in terms of the notions of judicial proceedings and the assignments of burdens of proof. But since judicial proceedings and the assignments of burdens of proof are squarely in the domain of social phenomena, the analysis is circular. He analyzes social processes using the analogy of a social process. If we choose one social process as *analysans* to illuminate as *analysanda* social processes in general, including the one chosen as *analysans*, we are in no way getting past or learning *about* social analysis. If Kratochwil were trying to argue for the use of the judicial metaphor in IR specifically, there might be a way out of the problem. But since he seeks to solve "problems of theory building in the social sciences," as the title of his chapter indicates, the judicial metaphor engenders a circularity.

We might also note that it is unnecessary to resort to interpretivism or use of the judicial metaphor in order to advance the idea that the "weight of evidence," rather than a specific falsifying observation or datum, should drive theory choice. A century ago, Duhem argued for this quite explicitly and forcefully in his conventionalist account of theory choice in physics; the scientific community's good sense could recognize when one theory should be replaced by a rival as a result of the latter's claims being undermined by the "weight of evidence" against it.[10]

Kratochwil (p. 41) further supports the courtroom metaphor by arguing that it is superior to competing metaphors, especially those just noted of "evolution" and "the market." The courtroom metaphor is conceptually superior to the evolution metaphor in part because the latter has to do with "the dubious argument that evolution . . . has a definite telos, the arrival at the truth. But nature and its evolutionary processes have no goal, there is no preordained destiny, analogous to the notion of coming nearer to the truth that provides the persuasive force of the argument." Kratochwil adds, "It is ironic that [Popper] did not see that these two concepts [scientific progress of increasing verisimilitude and the enlightenment notion of moving toward the truth] are not the same and that a similar teleology was at work in his conception of coming closer and closer to the truth, as in those theories of history, which he so aptly criticized" (p. 41). But Kratochwil commits a basic fallacy in his reasoning. If history is not moving toward any particular telos (such as the Hegelian "Unfolding of the Absolute Spirit"), then a theory of history should not impute to it such a characteristic. The theory should accurately capture the important aspects of the subject matter with which it deals. Still, the theory about history has a purpose; and a series of theories—whether about history, biological evolution, plate tectonics, gravitational forces, or the causes of war—*does* have a telos. If Kratochwil denies that theories seek the truth—even if defined in pragmatic rather than correspondence terms—in any respect (even in the minimal way of producing true laws), then Kratochwil is not claiming that his metatheory of the social sciences should be taken as true, or as closer to the truth than any other metatheory (see Kratochwil pp. 45 ff.).[11] How then should it be taken?

Furthermore, Kratochwil criticizes the evolutionary metaphor (p. 41) because it does not have a telos, but the theories of science do, namely, that of coming nearer to the truth. Kratochwil clearly attacks the idea that it makes sense to view the history of science as one of "coming closer to the truth." His principle reason is that, on occasion, previously rejected theories may later be accepted. So, if science is not properly viewed as having a telos, then the parallel of the evolutionary metaphor is restored. In other words, Kratochwil's argument that scientific theorizing does not "bring us closer to the truth" would seem to militate *in favor* of the evolutionary metaphor on teleological considerations—since neither has a telos. Secondly, one might argue that theorizing does bring us closer to the truth, which is still not an insuperable objection to the evolutionary metaphor, since two distinct parallel things need not be parallel in every respect. Indeed, they never are, since they would in that case not be two distinct things. For the metaphor to fail on these grounds, the role of the telos must be shown to be central to its value, and that is not what Kratochwil does. It would be peculiar for Kratochwil to attempt to produce a metatheory and persuade others of its merits if he cannot show that his metatheory is true—or at least more true or less erroneous than those of his rivals.

Methodological Pluralism

Social scientists often announce support for methodological pluralism while denigrating methods other than those that they specifically endorse. This pair of characteristics appears in varying degrees in the chapters by Kratochwil, Hopf, and Pollins. Kratochwil is clearly the most focused on the advantages of interpretivism. Hopf, as noted, argues that each approach misrepresents itself to some extent. Pollins makes a case for the merits of both interpretivist and scientific approaches to the social sciences. Pollins correctly points out that the approaches of *Erklären* and *Verstehen* are generally presented to students as mutually exclusive.[12] And he is right in arguing that the two approaches are in fact complimentary. Social scientists ask many sorts of questions that require different sorts of approaches. Consider a natural science analogy. Suppose Dr. Acula, a medical researcher, has a particular fascination with and desire to understand human blood. That curiosity could lead to questions, all of which are about the one and the same substance and all undertaken within the purview of the natural sciences, but they make use of concepts and methods of biochemistry, anatomy, molecular biology, cell biology, evolutionary theory, hydraulics, and other such disciplines.

Human behavior is at least as mysterious and complex and provokes as wide a variety of questions as Acula's vis-à-vis human blood. Answers to both sorts of questions will require different theories, concepts, and methods. IR theorists, likewise, ask a variety of different sorts of questions, such as (1) Do democracies fight fewer wars than nondemocracies? (2) Is a bipolar system more stable than a multipolar system? (3) Why did Prime Minister Begin order air strikes on Iraq? (4) Was the U.S. nuclear alert in 1973 intended as a warning signal to Brezhnev? (5) Is the use of nuclear weapons ever justified? (6) Are preventive wars immoral? (7) Should leaders ever follow policies that do not advance national interest? The appropriate methods to answer each of these questions differ. Some of these questions (1 and 2) are clearly best answered by methods that would include an examination of many cases and an attempt to find associations among variables; some (3 and 4) are most effectively answered by means of interpretive methods; and others (5, 6, and 7) are best answered by the analysis of concepts and the application of moral theory. There is, moreover, no single, unified IR or social science "field equation" into which all must ultimately be made to fit, though there are some specific forms of reasoning that are applicable to all. Because the social world is complex and multifaceted, even an investigation into a possible war between the United States and North Korea yields questions that, though related, sometimes require orthogonal and crosscutting theoretical approaches and methods.

Hopf describes the interpretivist approach and defends a social science method that makes use of interpretivism but offers some modifications limiting the way interpretivism distances itself from some mainstream methods. Three points are in order here. First, Hopf's description does not apply to the way scholars often theorize about agent-based reasons. There are multiple and varied methods of analysis in the social sciences that work in different ways. For example, scholars sometimes work backward, inferring motives or reasons from observable outcomes, which may not have been intended by any party. In some crisis situations, the outcomes were in fact intended by none of the parties.Second, there may be reasons for action, of which agents may be unaware, that have effects on agents' decisions (see, e.g., Williams 1979). Third, Hopf captures the recursive nature of social theory, which includes self-fulfilling and self-defeating prophecies. Theory should and does have an effect on the social world, the ideas of citizens and political leaders (though perhaps less than some scholars seem to think). Still, this is only a part of what political science has (rightly) attempted to do. Hopf argues for the interpretivist approach over the alternatives, but it does not suffice as a comprehensive account, since it is difficult to see how interpretivism alone helps political scientists who study problems such as the first two listed in the previous paragraph." Hopf seeks a method that emphasizes the "epistemology of interpretivsm," which Hopf unqualifiedly endorses ("interpretivist epistemology has it right," p. 55) but which combines with some elements of the mainstream to form a slightly different method. Hopf advances a single method and does not deal with the possibility of a plurality of methods appropriate to the plurality of different sorts of enquiries.

By his attempt to bring mainstream and interpretivist theorists together in a single approach, and thereby rule out various methods that have served to produce insights into large-scale or long-term phenomena and trends, Hopf limits what could otherwise be discovered by IR theorists. Because my own view is methodologically pluralistic, I hold that the choice of the appropriate methods, including the use of statistical modeling and rational choice theory, stems from a consideration of the type of question at hand. Anthropologists, much more than economists, ask questions like 3 and 4 above, answers to which are most appropriately understood by interpretive analysis.

One way that Hopf is able to argue for the superiority of interpretivism is by the selective set of social science questions he chooses as examples. While Hopf cites important interpretive and hermeneutic philosophers of social science, his empirical examples are taken from scholars such as James Clifford, James Marcus, Clifford Geertz and are drawn from ethnography and anthropology. Observed behavior in world politics may have to do with the decisions of states or the patterns found in systems, for example, the instability of the interwar European system. A philosopher of social science who wants to offer a comprehensive account of all social science theorizing should include case studies of ethnomethodology along with those requiring statistical modeling and rational choice theory. Because anthropologists ask different sorts of questions than economists or geographers, the appeal of interpretivism will vary with the sorts of questions it is asked to answer.

Naturalism

Knowledge of human behavior and institutions has struck many investigators as vastly different from knowledge of the natural world. However, both are forms of systematic empirical inquiry and, as such, share a myriad features, such as application of the rules of formal logic, statistical methods, descriptive categorization and observation of phenomena, the hypothetico-deductive method, and the like.[13] The idea that the methods of the natural sciences are applicable to the social sciences is customarily referred to as "naturalism" (see note 1). While many reject the analogy, especially interpretivists and poststructuralists, there are various "degrees" of naturalism between outright rejection and adoption of an identification of the two spheres of knowledge. There are indeed some parallels with the natural sciences; the difficult question is how far they extend.[14] Kratochwil, Hopf, and Pollins all have a good deal to say, mostly negative, about the parallel.

Kratochwil on Social Kinds and Descriptive versus Normative Social Science

Kratochwil (pp. 36–37) criticizes KKV on the treatment of "natural kinds and social kinds." Kratochwil's account, however, also has difficulties. He approvingly quotes (16–17) the scientific realist (SR) views of Rom Harré (1986), who argues that the demarcation of science consists in its standing as a unique "moral community." Kratochwil, referring to a passage from the "Introduction" to Harré's *Varieties of Scientific Realism*, says,

> Interestingly enough, such a [SR] perspective in a way dethrones science as a paradigm of warranted knowledge since it undermines the foundational claims of the scientific method that it is able to solve problems on the basis of a demonstration of how the world really is. It also suggests a deep-seated change, both in the practice of science and in the acceptance of scientific statements as self-justifying instruments, by the public at large. (p. 43)

There are several major problems with the conclusion Kratochwil draws from Harré's comments.

First, Kratochwil holds that science is dethroned as a paradigm of warranted knowledge because Harré's view undermines the claim that science can demonstrate its conclusions based on "how the world really is." There are many empiricist philosophers of science (such as Russell, Bridgman, Mach) who do not interpret scientific theories (because of their views of theoretical terms) as giving us any handle on "how the world really is." While there are serious difficulties with the operationalist and instrumentalist views of these philosophers, they nevertheless correctly argue that their empiricist position does not undermine science's unique standing in providing secure empirical knowledge (e.g., these non-SR theories of science could, unlike in the case of pseudosciences, still offer highly accurate predictions over the long run).

Second, Harré does not think that science is unique by virtue of its method. In that passage Harré is arguing that method is not the *only* thing that distinguishes scientific inquiry from other endeavors, the moral aspect is another. In the quotation Kratochwil cites, Harré says that the moral dimension is an important one "as well."

And third, Kratochwil assumes that Harré is right in embracing SR, which is highly dubious.[15]

In presenting his metatheory, Kratochwil says that he opposes inductive empiricists and logical positivists; he refers specifically to the Vienna Circle. Thus his target seems to be the logical positivists of the Vienna Circle (Carnap, Schlick, Neurath), Popper (at least in his early and middle works), and logical empiricists (such as Reichenbach 1936, 1938). He endorses a view according to which there is no objective notion of "truth" and he denies that reality can be used as a basis for testing theories. So there does not seem, according to Kratochwil, to be an objective record of what has happened in IR, sociology, Chinese politics, and other such areas—a record that could form an important element of theory-testing. In light of this, it is interesting that Kratochwil criticizes the philosophers who offer accounts of the sciences because their accounts do not square with the actual history of the natural sciences (p. 34).[16] Ordinarily the criterion of "squaring or not squaring with the actual history of . . ." would seem to be a reasonable one on which to base a criticism of a theory of science, particularly if there is a wide divergence between the philosophical norms and historical scientific practice. But it is a serious problem with Kratochwil's position that it denies anything approaching an objective record, since it then becomes impossible to make use of this criterion to criticize a theory of science. This would seem to call to mind the following rule of thumb of prima facie plausibility: "subject to further philosophical scrutiny in each case, if a claim or principle (like a nonsubjectivist concept of truth) is so difficult to deny in a consistent fashion that it is used in the midst of denying it, it may very well be true." This rule appears to be violated in that Kratochwil argues against naturalism in the social sciences, but he presents a very naturalist argument in the course of developing his account of the social sciences.

To advance a criticism of this sort requires that there be an identifiable history of science about which one can utter truths ("Einstein showed the superiority of physical theory using non-Euclidean geometry") and falsehoods ("Aristotle drew accurate conclusions from the Michelson-Morley experiment"). Kratochwil's argument that the account of science needs to "square with" the actual history requires that he be able to claim that there is an objectively true (or at least intersubjectively valid) set of propositions about events. However, this is precisely what Kratochwil and many interpretive, hermeneutic, and poststructuralist theorists deny. There are no objectively true or intersubjectively valid propositions about history, whether it is the history of Anglo-German relations or the outcome of physical science experiments, and thus there is no basis for assertions about the historical events on which Kratochwil's criticism may rely.

Hopf on Prediction

One of the most striking features of most of the natural sciences is their ability, under appropriate conditions, to predict. With regard to Hopf's discussion, three main points should be made. First, prediction is essential to human beings' ability to control reality, whether natural or social. Many social scientists enter their respective fields, especially in political science, because they believe it is possible that with the right kind of knowledge, the world can be changed for (whatever they may see it as) the better. That is, many social scientists believe that by developing better theories and making them available to policymakers; the latter could, if they chose to, make use of those theories (along with factual information and goals to pursue) to help guide their decisions. Social science theories are sometimes tested even by means of policymakers making explicit use of them in formulating strategies to solve various problems (unemployment, civil strife, health care, national security, etc.). In international relations, one might argue that Woodrow Wilson implemented liberal theoretical ideas, or that Henry Kissinger implemented realist balance of power ideas; both shaped U.S. foreign policy. It is hard to see how anyone who has no belief in predictive power of social science theories could imagine that social science might be used in any rationally grounded way to change the world.

Prediction is essential for any practical application of theory to the world of policymaking.[17] Policymaking can be either random or nonrandom. Random policymaking would include choosing a preferred outcome, considering all the policies that might possibly lead to that outcome, and then spinning a roulette wheel to choose the course of action. If we are to avoid this, then policymaking would make use of some sort of rational body of beliefs that link specific courses of action to probable outcomes. Examples of predictions that could guide policymaking include (1) If the United States should attack Iraq with 150,000 troops, the United States is likely to defeat of the Iraqi regime and the overthrow of Saddam Hussein, (2) Attacking Iraq with 2,000 special forces is not likely to lead to the defeat of Saddam Hussein and is likely to provoke anger at the United States, (3) Launching no attack would be likely to result in no near-term change in conditions. Random policymaking would ignore the evidence and the theories that link actions to election outcomes and would instead advocate a policy based on the roulette wheel spin. While many interpretivist social scientists are very hostile to the notion of "prediction," they are hesitant to admit that some form of it is needed for any rational policymaking.[18] What is the alternative to any (probabilistic) link between policy and outcomes? All policymaking is an attempt to affect conditions in the future, whether it is long-term, near-term, or immediate future.

Second, we observe that Hopf clearly states that interpretivists endorse the view that social inquiry can produce warranted predictive statements. Hopf takes a very modest and reasonable view of what any political scientist can legitimately predict. He says,

> With respect to prediction in particular, it should be stressed that interpretivism accepts bounded predictions, predictions about future outcomes that are deeply contextualized in understandings of the social milieu that produced the present. The logic of prediction remains the same in the mainstream and interpretivism. The critical difference is in the expectations of the range, scope, durability, and ambition of such predictions. Interpretivists will always defend more modest claims than the mainstream. If one believes that boundaries are hard to specify, and the social world is hard to control, then one must be careful in making claims about future outcomes. (p. 71)

The third point is that, in his argument supporting an interpretivist account of social science "prediction," Hopf makes the claim that he speaks for interpretivists in general when he endorses the notion of social science prediction—but it is hard to see which interpretivists he has in mind. He does not cite any interpretivist authors. While Hopf's argument should be evaluated on its own merits, it is not clear that it represents interpretivist social scientists. In fact, many interpretivists in international relations, political science, and the philosophy of the social sciences are intensely hostile to the idea that social inquiry can justify predictions. So when Hopf endorses interpretivist prediction in the social sciences, it is not clear for which interpretivists he speaks. Many of the prominent figures in political science, certainly in his main subfield of international relations, flatly reject prediction.

First of all, the continental poststructuralists whom Hopf cites, such as Bordieu (1977) and Foucault (1972), are strongly opposed to social science prediction. The subjective character of the social world does not allow any justification of prediction. There are interpretivist British social scientists whom Hopf cites, such as Peter Winch (1990) and Charles Taylor (1985). But they are vigorously opposed to prediction, in part because of the circular nature of interpretative reasoning. We might move closer to North America in seeking interpretivists cited by Hopf who endorse prediction, two of whom are: Alex Wendt (1999) and the philosopher of science social Daniel Little (e.g., 1998). These two authors are about as far from poststructuralists as one might find in the interpretivist camp. However, both oppose prediction in the social sciences. (Little's argument is based on the comparison with the natural sciences, namely, that social science prediction is impossible because the social sciences lack natural science-like "governing regularities," and the "phenomenal regularities" are too weak to support prediction.)

In Hopf's effort to defend prediction, and in a number of other places, he seeks to develop an understanding of methodology that bridges various gaps between interpretivism and the mainstream. But in the discussion of prediction, as elsewhere, Hopf's endorsement of the principle of the incommensurability of paradigms raises some questions.[19] Interpretivist and mainstream scholars offer very different images of what the social world is and what sorts of theories about it are justifiable. He says that the two schools of thought "have little to do with each other" (p. 55). This would seem to be about as clear an example as we might have in the social sciences of "incommensurability." So, if they offer distinct paradigms, then it seems that they deal in concepts that are incommensurable and thereby not amenable to the sort of synthesis Hopf seeks to develop. My point here is meant more to express reservations about the incommensurability thesis, and Hopf's endorsement of it, than about Hopf's attempt to develop a position that tends toward synthesis.

Pollins on Hypothetico-Deductive Reasoning and Prediction

Pollins sees some continuity between the methods of interpretivist and mainstream social science. One of the greatest strengths of his chapter is the lucid way in which he illustrates the need to understand this commonality and the need for communication between the two approaches. His argument should be taken a step further to point out the commonalities between interpretivist and especially mainstream social science and the natural sciences. The largest point of difference I would register with Pollins regards his hesitancy to acknowledge the parallels between the social and natural sciences. He does not move far enough from Hopf and especially from Kratochwil, both of whom reject the idea that the natural sciences can serve as a model for the social sciences. While all three reject naturalism, Pollins, like the other two, says many things that would seem to support at least a partial form of naturalism.

Pollins argues that "falsifiability" and "reproducibility" play a central role in the position developed by KKV. These concepts were developed by philosophers of natural (rather than social) science. Similarly, the hypothetico-deductive method, which Pollins highlights significantly as a link between the *Erklären* and *Verstehen* approaches, was developed by philosophers of natural science. Both the natural and social sciences also use statistical analysis and deductive logic (modus ponens, addition, simplification, contraposition, modus tollens, nonontradiction, etc.). The parallels and connections Pollins stresses between mainstream and interpretivist social science also hold for connections between the natural and social sciences. There is considerable overlap between what the two sets of disciplines do in terms of both methods and goals.[20] Pollins says, "Our journey must begin with the recognition of an obvious point: Scholarship is a communal enterprise and any community of scholars must share basic rules of communication (p. 91).[21] But these claims are not distinctive of the social sciences; exactly the same may be said of the natural sciences. Most of Pollins's descriptions of the social sciences are true of the natural sciences precisely as he formulates them. Thus the parallel with the natural sciences goes much farther than Pollins acknowledges and there is, as many philosophers of the social sciences have long contended, much to be learned from the natural sciences.

Pollins, as just noted, makes a persuasive case for the need for hypothetico-deductive (h-d) method as a common link between the interpretivist and mainstream approaches. (Perhaps it would be more accurate to include inductive reasoning along with deductive when considering the sort of method that links *Erklären* and *Verstehen* approaches.) As just noted, prediction is essential to any attempt to apply social science theory to policymaking or to change the world. So the relationship of h-d reasoning to prediction is an important one; Pollins's strong arguments in favor of the h-d method also justify, in at least broad terms, the sort of inference necessary for prediction.

The form of inference involved in h-d reasoning is the sort of reasoning needed for predictive inference. Problems of open systems and weak probabilistic laws must be overcome in order to have any "expectations" whatsoever (see Bohman 1993, Doran 1999, and Little 1991). If a policymaker accepts a theory, whether it is interpretive or not, she must consider which outcomes follow from which already-accepted claims. Again, it is important to stress that the notion of "prediction" advocated here is an extremely broad and flexible one. In order to have rational justification for the policy of invasion of Iraq in 2003, one must believe that invasion will (or is at least likely to) lead to the ouster of Saddam Hussein. The belief is based on some articulated or unarticulated theories or principles.

The sort of h-d inference that Pollins regards as justifiable moves from known instances to unknown instances; the temporal frame of the unknown cases (whether past, present, or future) neither increases nor decreases the validity or justifiability of the inference. In terms of the metaphysics and theory of knowledge that Hopf and Pollins endorse, prediction remains a possibility and KKV's work is helpful here as it provides guidelines for drawing such inferences. Interpretivists and antipositivists are, after forty years, still reacting so vehemently against logical positivism that all too often they reject whatever they see as associated with it—and prediction has always been central to logical positivism.

Pollins and other opponents of positivism are right to point out that logical positivism ceased to be a metatheoretical option decades ago. Many elements have long been rejected by philosophers, elements such as the verifiability principle of meaning, explanation-prediction symmetry, and even, at least to a degree, foundational theories of knowledge. But in their zeal to reject anything associated with logical positivism, social scientists tend to dismiss even what is justifiable on nonlogical positivist grounds—such as the h-d method and certain forms of forecasting or prediction, even in the broadest, most fallibilistic, and probabilistic sense.[22]

Conventionalism

One important feature shared by the natural and social sciences is that they both require a certain element of conventional choice. As I have argued elsewhere (e.g., Chernoff 2005: 100–06; 155–56; 203), these choices are conventional because they are not always based on pure logic; but they are not thereby rendered arbitrary; they may still be guided by rational considerations. Hopf notes (p. 55) that interpretivists should admit that they adopt mainstream methodological conventions, arguing (passim) that a set of methodological conventions is what connects interpretivism and the mainstream. He does not go as far as some conventionalist philosophers of science do by arguing that the conventions are rationally grounded and nonarbitrary. And Pollins's emphasis on the communal nature of social science (pp. 91, 97)[23] makes it very clear that there is a conventional element to scientific theorizing. He says, in reaching a conclusion as researchers, we must "finally take a stand based upon what we can justify to ourselves and the colleagues who will ultimately vet our claims and make use of our work. At this point in the process our chief aim is to communicate our findings to that peer community" (p. 98). The possibility of success in such communication rests upon the existence of a shared set of rules of theory choice shared by members of the community. These rules are nonlogical (that is, they cannot be derived from the rules of formal logic) but they are, nevertheless, rationally based and thus nonarbitrary.

Pollins says that rules of communication are "conventions that we choose to practice. To say that we, as a community, are free to choose the rules of our research practices does not mean 'anything goes'" (p. 91). Duhem's fundamental insights about physical theory illuminate the way on this point. As he argues in the context of physics, there are always conventional choices, for example, about the axioms of the geometry one selects on which to base a physical theory. We may choose a simpler geometry that is Euclidean, or a more complex non-Euclidean alternative geometry.[24] However, the simplicity of Euclidean geometry does not mean that the resulting physical theory-plus-geometry will be simpler. Duhem foresaw in 1904 (before special relativity) that it might be preferable, from the point of view of simplicity, to eschew Euclidean geometry. It may be that one of the various possible choices among systems of theory-plus-geometry is able to account for all known observations. So the choice is ultimately conventional, even though it is not arbitrary; rational considerations (e.g., of overall simplicity) guide the choice (for an elaboration, see Kyburg 1990). This yields the Duhemian principle of the conventionality of all science.[25]

Conventionalism adds a great deal to our understanding of the social sciences and their relationship to the natural sciences. For example, the success of physicists in reaching decisions on the conventions to be employed in physical theory has opened the path to discipline-wide agreement, and thus to progress, on many fundamental questions; though, of course, there is dispute in some areas of current exploration and research.

In stark contrast, many disciplines in the social sciences have not had this sort of consensus on conventions and there has been a discernible lack of progress in many disciplines and subfields.

Kuhnian-oriented political scientists explain the failure by noting that, for example, in IR, political realists and liberals study IR using incommensurable frameworks and terms that may appear the same but have different theory-derived meanings. However, there have been some areas in IR where theorists see progress, such as in the study of democratic peace hypotheses. (The same can be said for some other social science fields, such neoclassical economics.) Progress may be understood as disputants in a field moving closer both to the truth and to one another, allowing cumulation.

Kuhnians would have trouble explaining how political realists and liberals could agree on important democratic peace claims. But conventionalists may note that there has been widespread agreement on conventions (so-called measure stipulations) in those areas, especially in the definition of "war" (see Small and Singer 1982). The conventionalist account is epistemically optimistic in that it sees the possibility, at least in principle, of progress in other areas of the social sciences, while Kuhnians see advocates of competing paradigms (or disciplinary matrices) only continuing to talk past one another. Moreover, only the conventionalist view is able to offer an account of both the failures and the successes in the social sciences.

Conclusion

Kratochwil, Hopf, and Pollins disagree with one another on some important points and agree on others. Some of the agreed-upon points have been shown in this chapter to be questionable, particularly their rejection of naturalism. Their responses to KKV offer the possibility of a fuller understanding of the structure of social and political argument and theorizing. They create the opportunity for a deeper plumbing of these questions as the debate is continued by the authors of *Designing Social Inquiry* and the supporters of its methodological program.

Progress

The authors engage one another on each others' terms, which is essential if progress in debates of this sort is to occur. I have argued elsewhere (Chernoff 2004) that Kuhnians are wrong and that genuine scientific progress is possible in substantive areas of political science and IR, at least when certain sorts of question are posed, when rational measure-stipulation conventions are widely adopted, and when each of the disputants genuinely engages the others' terms. Debates of this sort are not themselves subject to the same problems, circularities, and dilemmas of reflexivity that are found in substantive or first-order explanations of political behavior. There is no problematical "additional level" of meaning in metatheory comparable to what we encounter in studying behavior such as the bombing of Kosovo, where we must search for the meaning arising from the reflexivity of the conscious actors. In the case of metatheory, the objects of study are the published arguments of scholars and the concepts they invoke, wherefore, the meanings of conscious "actors," namely the metatheoreticians, are part of the primary level at which the study is carried out. The contributions of Kratochwil, Hopf, and Pollins in this volume are very good example of the sort of engagement that can move the debate forward.

Theory of Knowledge

A number of criticisms of KKV and the mainstream follow from their failure to recognize that in the twenty-first century, many mainstream methodologists do not accept foundationalist theories of knowledge; most contemporary theorists are fallibilists of one form or another. Kratochwil's chapter offers some excellent insights into the nature of social scientific inquiry. But in several instances, the conclusions either do not follow strictly from the premises or are overstated and not really as novel as they appear to be presented (as with the implications of a nonfoundational theory of knowledge). For example, while Kratochwil is entirely correct in his attack on logical positivism and logical empiricism, the attacks do not offer us conclusive results because (1) there are few if any adherents to these 1930s doctrines at the present time and (2) the only alternative is not the poststructuralist or interpretivist line of argument that he suggests. Another alternative is the position that I have developed—causal conventionalism—which incorporates the principle of the conventionality of all science, viz., that all scientific inquiry requires certain conventional choices that are, stricltly speaking, nonlogical but are nevertheless nonarbitrary (Chernoff 2005, 2007).

Metaphysics

For those of us who adopt a pragmatic orientation toward the epistemological notion "knowledge" and the metaphysical notion "truth," it is possible to have a more pluralistic view of proper methods. What we should accept are theories and hypotheses that help us accomplish our chosen goals in the world. There are many cases where generalizations are useful for this end. If mainstream statistical methods help accomplish this, then we should be all for them, even if they do not produce "certainty," which, in a fallibilist theory of knowledge, is unavailable in the world of empirical inquiry. The rule of thumb of prima facie plausibility militates against Hopf's rejection of intersubjectively understood "truth" resulting from experience of an external world or Kratochwil's attack on naturalism.

Naturalism

Hopf argues against mainstream naturalism but argues that interpretivism makes use of some methods. His endorsement of the incommensurability thesis creates some doubt as to whether the two views are compatible. Kratochwil vigorously criticizes naturalism. But his argument relies very extensively on analogies drawn from the natural sciences, both in his evidentiary example (Galileo's experiment) and in the literature, especially the work of Popper and Harré, from which he draws.

Pollins states that he seeks a social science methodology rooted in the social and not the natural sciences. But he overstates the degree to which he achieves this. Virtually all of what he says about social science methodology, especially the *Erklären* tradition, applies *mutatis mutandis* to the natural sciences. One is hard-pressed to find anything in his description of the *Erklären* tradition that could not be said in identical fashion for the natural sciences (communication, reproducibility, falsifiability, hypothetico-deductive reasoning, rejection of Vienna Circle positivism). Even Pollins's discussion of rhetoric and communication, emphasizing the communicative and community nature of social science, fits with the natural sciences as well as with the social sciences, despite his suggestions to the contrary.[26]

Pollins argues (p. 93) also that KKV's emphasis on social science "inference" is wrongly associated with *Erklären*. Logical inference is just as important in *Verstehen* reasoning as in *Erklären* reasoning. As Hopf reminds us (pp. 57–58), the applicability of the rules of formal logic and many of the rules of rational inquiry will be the same. (The latter will not invoke the statistical rules developed in the bulk of KKV.) Pollins is right to point out that logically valid inference is as crucial in all approaches used within the social sciences, interpretivist or mainstream, as in the natural sciences. The natural sciences and social sciences overlap here, too. Pollins's conclusions critical of naturalism exceed the evidence he has presented against it.

The contributions of Kratochwil, Hopf, and Pollins form part of a dialogue that helps to move metatheory forward by developing clearer, more well-grounded, and more useful ideas about the relationship of theory to evidence. As I have indicated, a pragmatic theory of knowledge and a modified conventionalist account of social science method do the best job of explaining both the successes and the more frequent failures of social science in the past. Some other account may in the future be found to be more adequate.[27] If so, it must share with conventionalism the possibility and hope of genuine progress in social science disciplines and subfields, such as political science and international relations, at least once standards and conventions (e.g., measure-stipulations) are established.

Notes

1. "Naturalism" is the term most widely used in the philosophy of science to refer to the natural science approach to social science; it is the view that social science inquiry should be construed as parallel to natural science inquiry in terms of the methods, types of evidence, forms of inference and structure of theories.
2. The big difference between realism in metaphysics and foundationalism in the theory of knowledge is evident also in the position developed by Bhaskar (1975, 1998).
3. Pollins regards reproducibility as one of two key features of KKV's approach.
4. It is interesting to note that the modern version of this now-widely held doctrine, was developed by Peirce (discussed above) in the nineteenth century, *before* two important developments came about, each of which lent support to Peirce's view. One was Pierre Duhem's powerful argument that however extensive our data for a theory, as long as the data set is finite (as it must be), it will be consistent with many other possible—some not-yet-thought-of—theories. If this is so, then it is much more likely than previously believed that other theories will prove to be superior to our best current theory. And both Peirce and Duhem presented their arguments before the dramatic change that added weight to both, which was, of course, relativistic physics, proving superior to Newtonian mechanics, even though the latter had been regarded for centuries as certain.
5. Pollins (p. 105, note 8) says that KKV approach is derived from Popper's *The Logic of Scientific Discovery* (1968) but is inconsistent with the later Popper. Pollins says (p. 103) "Moreover, all of us accept that the nature of that [h-d] method indicates that our hunches always remain tentative; knowledge is never absolute."

6. Hopf concludes that section of his chapter by pointing out that interpretivists make use of the assumption of unit homogeneity but in a more limited way than that noted here.
7. For example, "What does it mean to come closer to the truth when truth is no longer a predicate of the world but of our assertions derived from fallible and probably false theories?" (p. 26). And, "In attacking the first issue it should be clear that truth couldn't be a property of the world . . . much confusion could be avoided if we were clear about this distinction between the existence of something and its description" (p. 45). "Thus even if truth can no longer be conceptualized as a property of the world but is construed as one of sentences *about* the world, it does not follow that 'anything goes'" (p. 29). And, referring to the final section of his paper, he says that it "addresses then the problem of 'relativism' allegedly flowing from an antifoundationalist stance that makes truth not a property of the world, but of assertions about the world" (p. 39).
8. Elsewhere I argue that the form of the pragmatic theory of "truth" offers the soundest philosophical answers to the key questions about "truth" and its role in defining "knowledge"; see Chernoff 2005.
9. While the arguments against a hard-and-fast fact-value distinction are difficult to rebut, there are certainly vastly different levels of factual or evaluative content in statements such as "water flows downhill" and "slave-owners are evil." So even if a qualitative distinction is out of reach, a quantitative or scaler understanding of the fact/value content of a statement will do most of the philosophical work that a qualitative distinction does.
10. See Duhem 1954: section 10 and the discussion of "naturalism" below.
11. Kratochwil (p. 34) says that how scientists make progress is very different from how philosophers of science account for it.
12. For all its merits in clarifying and exploring the implications of the two approaches, Hollis and Smith's (1991) excellent textbook *Explaining and Understanding International Relations* seems to reinforce this false dichotomy.
13. This chapter lists some shared features below. Hopf presents a list on p. 57 of his chapter. However, Hopf's list seems to go too far in two areas. One is the injunction against circular argument and the other is prediction. On the former, Hopf says that both mainstream and interpretivist methodologies eschew circular argument. However, hermeneuticists, who are a subclass of interpretivists (see Hopf's table 3.1), endorse or accept circular argument. On their theory, premise and conclusion are part of a circle, since one must use the context as premises in an argument concluding the most plausible understanding of the meaning of an action. But one must use the best interpretation of the meaning of each action as a basis for conclusions about the best understanding of what the context of the action is (as there are always many possible contexts in which an action may be placed). Later (p. 60), when he discusses the relationship of observer to subject, Hopf recognizes the circularity of hermeneutic argument.

14. Elsewhere I have identified eight features of the natural sciences that are typically seen as characteristic (Chernoff 2005: chapter 2). Any combination of these may be applied to the social sciences.
15. I have argued elsewhere that SR is not capable of supplying an adequate accounting of IR metatheory (Chernoff 2002).
16. Kratochwil adds to this criticism that social scientists use an epistemology that is no longer acceptable to philosophers (p. 28). While not strictly contradictory, it seems inconsistent for Kratochwil to argue that there is no truth to the historical proposition "Caesar crossed the Rubicon" but there is to "Plato defended a hierarchy among Forms."
17. Prediction is necessary, whether we call it by that or by some other name, as long as it captures the idea of systematic, rationally based expectation about the future. I have defined "prediction" elsewhere as follows. DEF: A prediction, in the context of the natural sciences or social sciences, is a singular or general proposition that (i) is indexed to the future relative to the moment of its utterance, (ii) may be based on imperfect evidence, (iii) is based on a rationally justifiable body of theory, broadly construed, (iv) may be either deterministic or probabilistic, and (v) involves the sort of phenomenon that serves as a dependent variable in the particular field. A statement such as "NATO expansion to include Russia is more likely to create a stable, peaceful, prosperous Russia than expanding NATO without including Russia" would constitute a prediction (Chernoff 2005, p. 8). Because of the unreasonable interpretivist horror over "prediction," maybe some other term, such as prognostication, would be preferable.
18. Some of these authors reject "prediction" because they take a strangely narrow view of the concept, treating it as synonymous with what is sometimes called "point prediction." Bernstein et al. use "scenario writing" in an attempt to get around "prediction."
19. The principle is generally attributed to Thomas Kuhn, 1962. See also Hanson 1958 and Sellars 1954, 1972. Many of Kuhn's ideas come from the work of Fleck; see Redman 1991.
20. Pollins (p. 100) cites Føllesdal 1979 on this very important point.
21. Pollins adds (p. 97) "Social science is a communal enterprise. Nothing can be more basic or essential to our work than communicating our findings and claims to our colleagues for their consideration, evaluation, criticism, denial or acceptance."
22. For the argument that prediction skeptics tend to define "prediction" in an inaccurately narrow manner, see Chernoff 2005: chapter 5.
23. The communal character, like other characteristics of the social sciences that Pollins notes, could equally be predicated of the natural sciences. That is, the term "natural science" could be substituted for "social science" in every sentence Pollins writes about the way in which the social science researcher seeks to communicate findings to an audience of peers.
24. There are systems of physical laws that use Euclidean geometry that are consistent with all observations but the laws are far more complex than those currently accepted by physicists.

25. The conventionality of all science thesis (Duhem 1954) holds that there is a conventional element to all scientific knowledge as evidenced in physics and chemistry; they require conventional choice between competing postulates (such as the measure-stipulation in physics; see Chernoff 2005: chapter 6). But the scientific process of making this choice is subject to dispute, a process that will ultimately produce clear grounds for choosing one postulate over the others. That is, there is no reason to assume that, because a conventional extratheoretic choice must occasionally be made, such a choice is arbitrary or must be based on ambiguous or open-ended considerations such that it makes no difference, for the investigator's purposes, which convention is chosen.
26. Even in his discussion of rhetoric and communication, Pollins (p. 98) cites McClosky's claim that "science is an instance of writing with intent, the intent to persuade other scientists" (McClosky 1998: 4).
27. There are various advantages to a pragmatic definition of "truth," the term may be defined in a variety of ways and remain consistent with the conventionalist approach.

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Part III

The Purpose and Methods of Research

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Transforming Inferences into Explanations: Lessons from the Study of Mass Extinctions

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Introduction: From Inferences to Explanations

Defenders of methodological singularism have traditionally sought to unify the natural and human sciences. Their traditional opponents insist that while explanation is appropriate to the natural sciences, interpretation should be the goal of the social sciences. Breaking from this tradition, the volume *Designing Social Inquiry* (henceforth KKV) largely ignores the natural sciences and interpretivism. The authors instead advocate a unified approach to explanation within the social sciences, arguing powerfully and provocatively that qualitative analysis should adopt the context-independent rules of valid inferences represented by quantitative analysis.

I defend methodological pluralism, not by defending interpretivism as a contrast to explanation, but by highlighting problems in KKV's analysis of explanation. This defense rests on the distinction between inferences and explanations. We express inferences as hypotheses that are confirmed through contested appraisal of their evidentiary grounds and theoretical logics. To confirm a hypothesis is to claim that it has weathered sufficient scrutiny relative to its rivals and to the current state of theorizing and data gathering that belief in its approximate truth is more reasonable than disbelief but is also subject to revision in the face of future data gathering or theorizing.[1] We explain, on the other hand, by using confirmed hypotheses to answer questions about why or how phenomena occurred.[2] To inquire into explanation is to ask whether a confirmed hypothesis adequately explains a phenomenon. Many do not. All explanations thus require (confirmed) inferences, but not all inferences constitute explanations. Explanatory propositions are distinguished from nonexplanatory propositions by the inclusion of causal mechanisms.[3] But, finally, all explanations are not good explanations: we achieve explanatory goodness by identifying chains of causal mechanisms that were, under the specific circumstances, sufficient to produce the outcome. To not identify the relevant mechanisms is to not explain; to identify them only partially is to gesture at without completing the explanation.

The distinction between inference and explanation explicates why best scientific practices take heterogeneous forms not reducible to context-independent algorithms for constructing valid research designs. Specifically, causal mechanisms discharge a dual function; alongside of their role in establishing explanatory adequacy, causal mechanisms can help resolve the problem of theoretical underdetermination by adjudicating rivalries between two or more theories that are consistent with existing evidence. Causal mechanisms, in other words, can enhance or impeach the credibility of hypotheses whose research-design credentials are otherwise impeccable. Causal mechanisms promote inferential goodness via theory, not via research design; they thus expand our repertoire for making valid inferences. Indeed, under specific conditions, fair causal comparison allows us to discount or even disregard procedures and rules central to the case championed by KKV. Far from resulting in inferential errors, these heterodox strategies support major scientific achievements.

Research designs based on the logic of quantitative analysis undoubtedly should play a large role in achieving valid inferences. But many valid inferences simply do not count as adequate explanations, and the causal mechanisms that render propositions explanatory also play a role in confirming hypotheses. KKV miss this crucial point because the authors tacitly conflate inferences and explanations. The conclusion I draw is that the techniques celebrated by KKV may be useful, significant, important, central, or even crucial components of many social science practices; but they are neither necessary nor sufficient for generating adequate explanations. This conclusion vindicates methodological pluralism: social science inquiry is not methodologically monochromatic.

In the spirit of methodological pluralism that animates this chapter, I make this argument in two ways: through conceptual and logical analysis of the evaluative criteria for confirmation and explanation and through a case study of scientific progress, one which has been erroneously recruited to the cause of methodological unity. I begin with the case study, followed by sections on inferences and explanations, causal mechanisms, and the dual use of causal mechanisms as explanatory devises and as sources of (dis)confirmation.

Causal Mechanisms at the K/T Boundary

KKV's case for methodological unity rests on the hypothetico-deductive method (H-D method), which is composed of

- one or more hypotheses, or statements whose truth value is to be evaluated in terms of their consequences,
- one or more statements of initial conditions, and
- one or more observable predictions, or states of the world that can be deductively implied by the conjoined hypotheses and initial conditions and that therefore must be observed for the theory to be true.

While all valid research follows this method, the authors continue, not all research follows it equally well. Qualitative or case-study research yields valid knowledge, according to KKV, if and only if it adheres to the logic of inference embodied in quantitative research designs. Researchers who ignore this lesson will likely produce results that are indeterminate or biased. Responding preemptively to the obvious retort that qualitative analysis often focuses on highly complex and unique events that cannot be studied statistically, KKV argue that even "unambiguously unique events" can be studied using the scientific methods they champion. To support this important claim, they briefly consider one such unique event, the extinction of the dinosaurs. The authors neatly assemble this position as follows:

> One hypothesis to account for dinosaur extinction . . . posits a cosmic collision: a meteorite crashed into the earth at about 72,000 kilometers an hour, creating a blast greater than that from a full-scale nuclear war. If this hypothesis is correct, it would have the observable implication that iridium (an element common in meteorites but rare on earth) should be found in the particular layer of the earth's crust that corresponds to sediment laid down sixty-five million years ago; indeed, the discovery of iridium at predicted layers in the earth in the earth has been taken as partial confirming evidence for the theory. (KKV, 11)

It is true that the iridium anomaly supports the meteorite (bolide-impact) hypothesis; and it is true that the team of Berkeley scientists responsible for this hypothesis treated the iridium anomaly as the observable implication of an ultimate cause that could not be directly observed. But the scientific study of the dinosaur extinction differs from the summary contained in KKV.[4] Rather than looking for iridium to test a prior hypothesis, researchers stumbled on the iridium anomaly. They then reasoned backward from this improbable finding to its probable cause, and they focused their reasoning exclusively on causal mechanisms.[5] These two facts set the stage for the philosophical discussion to follow.

Causal mechanisms dominated the study of the dinosaur extinction because a key member of the Berkeley team, the geologist Walter Alvarez, discovered the iridium anomaly as a by-product of his work on plate tectonics. Commonly accepted today, the theory of plate tectonics acquired scientific credibility in the 1960s only after scientists identified a plausible causal mechanism.[6] Boldly challenging the prevailing view of fixed continental position, the German meteorologist Alfred Wegener had proposed the theory of continental drift in the 1930s. Wegener inferred from continental morphology that the continents must once have been joined; he then inferred that the continents must be drifting apart. Scientists rejected Wegener's hypothesis because his proposed causal mechanism—that drifting continents cut through solid earth as a ship plows through the ocean—defied basic physical laws. The theory was subsequently accepted three decades later, in part due to new observations, but more importantly because researchers proposed a credible causal mechanism: the continents rest on tectonic plates carried on convection currents generated by the earth's internal thermodynamic processes. The absence of a plausible causal mechanism induced the initial rejection of the theory, not a fallacious inference caused by a faulty research design; the subsequent description of a plausible causal mechanism led to the theory's acceptance, not a research-design-based set of inferences.

A paleomagnetist who specialized in the subcontinental "microplates" of the Mediterranean, Alvarez was working near the medieval city of Gubbio, north of Rome, in the Apennine Mountains. In a canyon just outside the city, stands an outcrop of pink limestone called the *Scaglia rossa* whose exposed face spans the Cretaceous period, running into the more recent Tertiary period. The K/T boundary separates these two periods. For his research, Alvarez sampled rocks crossing the K/T boundary. He dated his samples in part by working with a specialist in foraminifera, "forams" for short—single-celled marine organisms whose microfossils can be identified and dated precisely. Forams were plentiful below the boundary, and scarce above it; upon learning that the dinosaur and foram extinctions coincided, Alvarez decided to study the mass extinction at the K/T boundary.

Motivated by a debate between contrasting geologic paradigms, gradualism and catastrophism, Alvarez first asked whether the extinction had been relatively abrupt or gradual. Gubbio limestone was composed of 95 percent calcium carbonate (composed overwhelmingly of the fossilized remains of forams) and 5 percent clay. The K/T boundary, however, is a physical boundary of almost pure clay. Alvarez asked how long it took to deposit those clay sediments. The two scenarios Alvarez posited predicted different levels of iridium, 0.1 parts per billion for the relatively slow scenario (short-term increase in clay deposits with constant level of fossilized foram deposits) and virtually none for the relatively fast scenario (abrupt cessation of foram deposits with constant level of clay deposits). Both scenarios assumed a constant rate of iridium accumulation.[7] Thus, while Alvarez did use the H-D method, he did not use it as a test of the meteorite hypothesis (or any other specific catastrophic hypothesis).

The results were astounding: iridium was found at the rate of nine parts per billion, roughly ninety times higher than the amount expected if the rate of sedimentation had been relatively slow, on the scale of thousands of years. Far from supporting either of Alvarez' initial scenarios, the data undermined the assumption of a constant rate of iridium accumulation. This find raised new questions without answering old ones, for now Alvarez had to figure out an explanation for "all that iridium." Numerous answers could be proposed for this question—a meteorite impact was one possibility, but so too were massive volcanic eruptions and even noncatastrophic hypotheses, such as an encounter with a cloud of interstellar dust and gas. Any credible explanation for the elevated levels of iridium, moreover, had to do double duty, to also answer the question "what caused the extinction of the dinosaurs?"

The Berkeley team did not answer these questions by devising new research designs based on the H-D method. Instead, they focused on causal mechanisms—or, in the context of this debate, killing mechanisms—much as the theory of continental drift was established only after plausible causal mechanisms were identified. For over a year, Alvarez and his colleagues regularly returned to the impact hypothesis but continuously rejected it, not because it was inconsistent with the evidence, for it *was* consistent with the evidence, but rather because they

> could not understand why an impact would cause worldwide extinction . . . A supernova had seemed more reasonable because it would have bathed the entire Earth in lethal radiation, thus explaining the global character of the extinction. But a supernova was out, and impact seemed to provide no global killing mechanism. For over a year we had searching discussions that always ended in frustration, and I would lie awake at night thinking, "There *has* to be a connection between the extinction and the iridium. What can it possibly be?"[8]

Thus, contrary to the report of KKV, the Berkeley team did not propose elevated levels of iridium as a test of the meteorite hypothesis. Instead, a long process of serendipity and trial and error led to the discovery of elevated iridium levels, a finding that itself begged explanation.

The Berkeley team resolved the iridium anomaly and made great progress in explaining the dinosaur extinction only after they identified a plausible causal mechanism.[9] By late 1979, the physicist Luis Alvarez (father of the geologist Walter Alvarez) believed that he had found the appropriate mechanism: a large impact created a global dust cloud causing the collapse of the entire food chain and mass extinction. No research supported this hypothesis; rather, when initial calculations of the quantity of dust and its impact were approved by a Berkeley astronomer, Luis Alvarez exclaimed, “We’ve got the answer.” Within weeks, the meteorite impact hypothesis was presented at a conference and within a year, the seminal report appeared in the journal *Science*.[10]

Credible causal mechanisms validated the impact hypothesis as the cause of the mass extinction at the K/T boundary. The iridium anomaly was consistent with multiple hypotheses. Some of those hypotheses could not be connected to mass extinction by way of a credible causal mechanism; they were discredited. The identification of a plausible causal mechanism that could explain the iridium anomaly *and* the dinosaur extinction, on the other hand, powerfully supported the bolide-impact hypothesis whose evidentiary warrant was not otherwise superior to its rivals. Causal mechanisms thus acted doubly: as explanatory devices and as instruments for evaluating the relative merits of rival hypotheses. Yet both of these functions of causal mechanisms are overlooked by KKV.

Inferences and Explanations

Consider two questions common to the philosophy of science: “When does a body of evidence confirm a hypothesis?” and “When does a hypothesis adequately explain an outcome?” The first question is about inferences, about how well we reason about the relationship between evidence and premises or between premises and conclusions. The second question is about explanations, about how well inferences account for the causes of an outcome. Each question is associated with a particular method. We confirm (or disconfirm) our inferential hypotheses through the H-D method, which is near universally considered to be *the* scientific method.[11] The H-D method deduces an observational prediction from the conjunction of one or more hypotheses and one or more statements of initial conditions. If the observational statement is true, then the hypothesis has passed the test: it has not been falsified and it has received some degree of inductive confirmation. But hypotheses confirmed by the H-D method do not automatically serve as explanations or as good explanations. Traditionally, the model of explanatory adequacy has been the deductive-nomological model of Carl Hempel. The D-N model of explanation deduces an outcome to be explained from the conjunction of one or more general laws and one or more statements of initial conditions. The outcome is explained by subsuming it under the general law, showing it to be a specific instance of a more general phenomenon known to be true.

Because their logical structures are similar, it is easy to confuse the H-D method of confirmation with the D-N model of explanation; both work by combining statements of particular initial conditions with statements of some general law. Here is an example. We might test the hypothesis "All celestial bodies follow elliptical orbits" by adding the initial condition "The earth is a celestial body" and then deducing the observational prediction "If it is true that all celestial bodies follow elliptical orbits, then the earth must follow an elliptical orbit." Observing that the earth does have an elliptical orbit does not make the hypothesis true: having deduced the prediction from the purported general law, the now validated prediction provides in return only limited inductive support for the law. If we accept the hypothesis as true, conferring on it the status of general law, then the D-N model would explain the earth's elliptical orbit: all celestial bodies have elliptical orbits, the earth is a celestial body, therefore (we logically deduce), the earth must have an elliptical orbit. Thus, whereas the D-N model uses a well-confirmed law to explain an observational statement, the H-D method uses an observational statement to provide inductive confirmation of a hypothesis that might be used in future D-N-type explanations.

It thus might seem that only a short step separates the H-D method of confirmation from the D-N model of explanation: this might be one reason that confirmation and explanation are so often conflated. But in fact the step dividing the two equals the gap between a partially confirmed hypothesis and a well-confirmed general law. Furthermore, since confirmed hypotheses do not automatically serve as explanations, it is absolutely crucial to understand what distinguishes them.

Explanations are inferential, but not all inferences explain. This must be true, because symptoms are inferentially relevant but explanatorily irrelevant. For example, cosmologists believe that the universe is expanding. They *infer* this hypothesis from the Doppler Effect; light from distant galaxies shifts toward the red end of the spectrum, implying that those galaxies must be moving away from us. But nobody believes that the redshift *explains* why galaxies recede. The consensus explanation, rather, is that the "big bang" that originated the universe sent its parts speeding off in different directions. The Doppler Effect is a symptom that comports with this explanation.

The problem of temporal asymmetry also distinguishes explanations from inferences. For example, from having knowledge of initial conditions of the earth, sun, and the moon, and using the general laws of celestial mechanics, we can predict—infer an unobserved event—a future eclipse. We can even claim that the antecedent conditions and general laws *explain* the eclipse. But consider a slightly modified scenario: from the knowledge of the initial conditions of the earth, sun, and moon, combined with the general laws of celestial mechanics, we can infer that an eclipse occurred ten thousand years ago. But nobody would claim that the present positions of celestial bodies cause the past eclipse, as this violates a basic law of causality: causes cannot follow their effects. We can use the present position of planets to retrodict past eclipses, but since explanations must contain statements connecting causes to effects, we cannot claim that present positions explain past eclipses. While inferences and explanations may overlap, they are not equivalent.

KKV tend to conflate inferences and explanations: this is why their framework cannot account for the scientific study of the dinosaur extinction. True, KKV give definitional grounds for distinguishing inferences and explanations; inferences involve reasoning from the known to the unknown; explanations, which are obtained by "connecting causes and effects," on the other hand, are "always based on causal inferences" (KKV, 34, 75 at footnote 1). But KKV frequently appear to conflate causal inferences and explanations, such as when the authors write "a hypothesis is not considered a reasonably certain explanation until it has been evaluated empirically and passed a number of demanding tests" (KKV, 12). Explanations differ from causal inferences, in this and other formulations, only to the degree of confidence our research licenses us to invest in them.[12] No surprise, then, that most of the volume is about making valid inferences; the topic of explanation receives no explicit attention. Important consequences follow.

Causal Mechanisms and Explanations

If not all inferences are explanations, what is the distinguishing characteristic of explanatory adequacy? Consider two examples of incompetent explanations that demonstrate why the D-N model does not establish explanatory adequacy.[13] Mumbling an ancient incantation, I pour salt into a beaker of water and the salt dissolves. I observe that every time someone mumbles the same incantation while pouring salt into water, the salt dissolves. Shall we claim that incantations explain the salt's dissolution? The explanation certainly fits the formal structure of the D-N model, for the statement "Salt dissolves when poured into water and accompanied by ritual incantations" is a true general law. Yet ritual incantations are surely irrelevant. Similarly, by the logic of the D-N model, Mr. Jones makes no error when he explains his failure to get pregnant by pointing to his taking contraceptives. Again, the explanation is absurd, but it fits the logical structure of the D-N model, for it contains the general law "Men who take contraceptives will not become pregnant," a statement that is no less absurd for being falsifiable and well corroborated.

The D-N model fails as a model of explanation because many arguments faithfully follow its prescribed logical form but manifestly fail to explain. We know that water causes salt to dissolve; we know that men cannot become pregnant; we know that both the hex and the contraceptives are explanatorily irrelevant. But the D-N model, with its Humean suspicion of causality and its accompanying preference for statements of regularity to statements of causality, has no immunity from such counter examples.

In the wake of the repeated failure to resuscitate the D-N model of explanation, philosophers have converged on a new understanding of explanations: we explain an event or a phenomenon by identifying the causal mechanisms that produced it. Causal mechanisms are structures and entities that have the capacity to generate observed associations between macrophenomena. Causal mechanisms tell us not only that something occurs (with regularity p), but also *why or how* it occurs. This is knowledge of how the world works. Two types of causal mechanisms appear in valid explanations: etiological mechanisms explain the occurrence of an event; and constitutive mechanisms provide causal analysis of phenomena, usually via microlevel reduction such as explaining pressure within a container of gas by way of the momentum exchanged by colliding molecules. An adequate explanation combines *credible* causal mechanisms that were *present* in the relevant circumstances and that were *jointly sufficient* in those circumstances to produce the event or phenomenon in question.[14]

The explanatory function of causal mechanisms is absent in KKV, which is surprising given that the authors define explanation as "connecting causes and effects" (34). In its place, they instead define causality in terms of "causal effects" and insist their definition of causal effects is "logically prior to the identification of causal mechanisms."[15] By stressing the inferential nature of causal effects and neglecting the explanatory function of causal mechanisms, KKV implicitly adhere to a regularity conception of causality, and thus, as we have seen above, illegitimately equates inferences and explanations, to the detriment of an explanatory social science.

Indeed, KKV discuss causal mechanisms only in the book's final pages, reintroducing them as a means of increasing the number of observable implications of a theory. "By providing more observations relevant to the implications of a theory," they write about the qualitative method of process tracing and other means of elaborating causal mechanisms: "such a method can help to overcome the dilemmas of small-*n* research and enable investigators and their readers to increase their confidence in the findings of social science" (226–28). In other words, causal mechanisms are given a legitimate role in social science only as servants of inferences: qualitative researchers should identify and test causal mechanisms not because this is what adequate explanations demand but because this is what adequate hypothesis testing demands in the absence of the statistical manipulation of data. Once again, we find KKV conflating the confirmation of hypotheses with the elaboration of explanations.

Causal mechanisms certainly perform a confirmatory function. They are especially useful tools for rejecting rival hypotheses, in part by distinguishing between well-confirmed accidental correlations and well-confirmed causal relationships. Consider how falling barometers are empirically associated with the arrival of storm systems, but we immediately reject any claim that the relationship is causal, not because of research-design considerations or the results of a statistical study, but because we know that there is no mechanism by which falling barometers cause storms. In addition, James Johnson assigns to causal mechanisms the definitional function of making theory T "more credible in the sense that [the mechanism] renders the explanations that T generates more fine-grained."[16] The more detailed our understanding of a phenomenon, the more credibility we invest in the theory. Yet we need not conclude that the function of credibility enhancement exhausts the role of mechanisms; mechanisms have the capacity to both confirm and explain. Nor need we subscribe to the claim of KKV that mechanisms make theories more credible only by increasing the number of observations. Thinking in terms of causal mechanisms is not simply a matter of research design; as the case study of dinosaur extinction establishes, we can facilitate confirmation and obtain valid explanations by thinking theoretically about causal capacities and processes.

Research Designs and Causal Mechanisms as Nonrival Instruments of Confirmation

Causal mechanisms, not inferences, constitute explanatory adequacy. Because KKV focus exclusively on the logic of research-designs that yield valid inferences, they underestimate the significance of causal mechanisms. Therefore, their methodological arguments are not sufficient for achieving explanatory adequacy. Their framework would be considered necessary (but not sufficient) for achieving explanatory adequacy, on the other hand, if and only if research designs are the only instruments for achieving inferential validity. There are good reasons to reject categorical versions of that claim. When more than one hypothesis is consistent with the best available evidence, a hypothesis can be vindicated by analyzing causal mechanisms. Causal mechanisms, in other words, are instruments of confirmation and of explanatory adequacy. KKV's framework, we must conclude, is neither necessary nor sufficient for achieving explanatory adequacy.

There are compelling reasons to nominate research designs as the superior means to confirm hypotheses. "In a world in which almost everything is influenced by many different factors," Robin Dunbar reminds us, "confounding variables are the bane of a scientist's life."[17] Take an event, any event: it is preceded by and coincident to an astonishingly large number of other events, all of which are enveloped by a vast coterie of environmental characteristics, qualities, and conditions. Any standing feature or any episode of discrete temporal change could, in principle, be the cause of anything else. And as we attempt to measure these covariations, we find many, many things undergoing simultaneous change. The problem with qualitative analysis, from the perspective of quantitative analysis, is the perceived absence of reliable means to control these confounding variables. The quantity of reasonable causes of a phenomenon may quickly outnumber the cases being studied—too many variables, too few cases is the bumper-sticker sized statement of this inescapable, existential condition more formally called the problem of indeterminate research designs or the "small-n" problem. KKV address this issue by counseling case-study researchers to emulate the techniques of statistical analysis. Following this advise would help qualitative researchers to maximize the concreteness and thus the quantity of implications of their theories; to minimize selection bias when selecting cases; to increase the number of observations, especially by "making many observations from few"; and to avoid fatal errors of endogeneity, measurement error, and bias in the exclusion of relevant variables.

The logic of research design expounded by KKV is a powerful tool of causal analysis.[18] It does not, however, exercise a monopoly over that function: The major breakthroughs in the study of the K/T extinction were not the result of well-crafted research designs.

The Berkeley team understood the need to seek observational implications of their theories; it is precisely this element of their study that set it off from the dozens of earlier speculative enterprises.[19] Yet they spent little time worrying about problems of indeterminate research designs even though the number of existing hypotheses dwarfed their number of observations. Indeed, the core methodological precepts of KKV played virtually no role in confirming the hypothesis linking an extraterrestrial impact to the K/T extinction.

Recall from the case study above that only *after* the discovery of the iridium anomaly did the Berkeley team begin to consider the possibility of an extraterrestrial event as the cause of the mass extinction. The deviations from KKV's regulatory model continue, as the Berkeley team neglected many of the more specific principles of KKV. Take, for example, the proposition that qualitative researchers should increase the number of observations: research designs become determinate and then increasingly reliable as the number of observations grows larger than the number of variables. Following this advice, we would expect the Berkeley team to examine other instances of mass extinction and their relationship to extraterrestrial impacts. This research strategy is not ruled out by the uniqueness of the dinosaur extinction: indeed, no scientific researcher even refers to this event as one of dinosaur extinction, for it involved the extinction of over 40 percent of all genera, making it one of *five* mass extinctions.[20] The Berkeley group explicitly recognized the nonuniqueness of the K/T extinction in the first sentence of their initial publication, stating, "In the 570-million year period for which abundant fossil remains are available, there have been five great biological crises, during which many groups of organisms died out."[21] Yet far from worrying about the problem of indeterminate research design, the Berkeley group ignored these other four instances of mass extinction and concentrated solely on explaining the K/T extinction. They also omitted study of the far more numerous instances of submass extinctions. As the paleontologist J.J. Sepowski meticulously demonstrated, a histogram of all extinctions evinces a highly skewed distribution associated with power laws, suggesting not only that there is no sharp discontinuity between small and large extinctions, but also that the mass extinctions may in fact be random events without specific causes.[22] Note as well that the Berkeley team did not look at other instances of bolide impacts to gauge their correlation with genera extinctions.[23] Neither did they study nonextinction periods (during which time extraterrestrial impacts have been in fact quite common).[24] Finally, note that prior to publication when the Berkeley team looked for additional observations supporting the iridium anomaly, they searched only for iridium anomalies at the K/T boundary itself. In short, while the Berkeley team could state with confidence that the K/T extinction coincided with an iridium anomaly, they made no effort to ascertain whether bolide impacts preceded other instances of mass extinction or whether impact-induced iridium anomalies exist in the absence of mass extinctions. This research strategy explicitly violates one of the most important lessons of KKV: avoid selection bias, about which the authors warn in stark terms: "When observations are selected on the basis of a particular value of the dependent variable, nothing whatsoever can be learned about the causes of the dependent variable without taking into account other instances when the dependent variable takes on other value" (KKV, 129). Yet far from committing the sin of selection bias, the Berkeley research strategy followed a logic that comported well with the research situation and the claims they advanced.[25]

KKV might make three objections to my account of the vindication of the bolide-impact hypothesis:

- *Objection #1*: The story I have told refers to "the irrational nature of discovery," to the process by which theories are generated. The framework of KKV, on the other hand, refers to the evaluation of existing theories.[26]

Response #1: The Berkeley team did not discover the meteorite hypothesis that pre-dates the discovery of the iridium anomaly by two centuries. The Berkeley group repeatedly discussed and rejected the hypothesis until they came up with a plausible causal mechanism. With that mechanism in place, they immediately claimed to have provided "direct physical evidence" for a "satisfactory explanation."

- *Objection #2*: The meteorite hypothesis was only "partially" confirmed by the iridium anomaly (11).

Response #2: This objection raises the question of when confirmation occurs. While KKV treat the original research as exemplary by their own standards, they do not consider the hypothesis to be confirmed, writing (and again conflating confirmation with explanation) that "a hypothesis is not considered to be a reasonably certain explanation until it has been evaluated and passed a number of demanding tests. At minimum, its implications must be consistent with out knowledge of the external world; at best, it should predict what Imre Lakatos refers to as "new facts," that is, those formerly unobserved" (KKV, 12). By these criteria, however, the hypothesis was indeed confirmed. The impact hypothesis *was* consistent with knowledge of the external world; moreover, it *had* predicted what Lakatos calls "novel, excess information," predicting, for example, the presence of an impact crater as well as the existence of extraterrestrial events coincident with other mass extinctions. Lakatos, moreover, always recognized that a theory was to be evaluated *relative to rival theories*. Although KKV note the existence of a rival theory—that the K/T extinction was a product of massive volcanic eruptions—they do not reference this rival theory or the problem of theoretical rivals in their discussion of confirmation. Thus, by the abbreviated sketch of confirmation that KKV offer, the hypothesis was confirmed.[27]

- *Objection #3*: The meteorite hypothesis was not confirmed by the original report; it was confirmed by subsequent research (including the discovery of geological formations believed to be unique to bolide impacts and the impact crater itself) that hewed more closely to the strictures of KKV. The hypothesis was confirmed, in other words, by a retrospectively valid research design produced by an entire scientific community, not just a single research team. The work of any individual author, even work that manifestly selects on the dependent variable, can be considered valid if the author is "contributing to a large scholarly literature."[28]

  Response #3: The Berkeley group was not contributing to an existing literature; they were trying to estimate a causal effect from a single observation, they did so by selecting on the dependent variable; and they claimed to have validated their hypothesis. If retrospective and collective research designs validate research, moreover, then no research can ever be discredited without full knowledge of future ideas.

All of these objections, whatever their individual merits, ignore the research strategy that governed the reasoning of the Berkeley team. They explicitly justified reasoning backward from the iridium anomaly to the meteorite hypothesis and accepting the latter as provisionally warranted belief prior to the gradual accumulation of new supporting evidence. In their 1980 *Science* article, they claim to

> present direct physical evidence for an unusual event at exactly the same time of the extinctions in the planktonic realm. None of the current hypotheses adequately accounts for this evidence, but we have developed a hypothesis that appears to offer a satisfactory explanation for nearly all the available paleontological and physical evidence.[29]

The impact hypothesis counted as warranted belief consequent to its fulfilling all of the following three conditions:

- It accounts for an incredibly significant fact—the iridium anomaly—whose importance outweighs almost every other available piece of data.[30]
- It accounts for that fact better than its rivals such as the supernova hypothesis, because it can be linked to the mass extinction.
- It accounts for the mass extinction with a credible causal mechanism.

This criterial list demarcates the outlines of an alternative methodology based explicitly on the search for causal mechanisms as agents of confirmation and explanation. Confirmation, according to Richard Miller, is a process of fair causal comparison. Formally, "A hypothesis is confirmed just in case its approximate truth, and the basic falsehood of its rivals, is entailed by the best causal account of the history of data-gathering and theorizing out of which the data arose." More colloquially, "These are the facts. This is how they are explained assuming the approximate truth of the favored hypothesis. This is why they are not explained as well on the rival hypotheses which are the current competitors."[31] Because confirmation is based on the relationship of a hypothesis to evidence and to rivals, confirmation *must* always be tentative, contingent on the availability of data and the state of rivals. Acceptance of a hypothesis thus means only that "acceptance is taken to be more reasonable than rejection, but suspended judgment is not excluded."[32]

Note that not just any type of hypothesis will do: fair causal comparison deals with hypotheses that can lay claim to explanatory adequacy because they include causal mechanisms. As the Berkeley team realized, the search for causal mechanisms is an integral element of the processes of confirmation and explanation. Exploiting the twin functions of causal mechanisms—agents of confirmation and explanatory devices—expands our repertoire for engaging in fair, causal comparison. In the following list, to say that a theory is rejected is to render its acceptance less reasonable than a competitor that does not suffer an analogous problem.

- A hypothesis can be rejected because its posited causal mechanism is considered inconsistent with generally accepted principles and thus implausible. This was the case, to give just one example, with the rejection of the initial formulation of plate-tectonics theory. It is also the reason given for the rejection of functionalist models of social change.
- A hypothesis can be rejected because it does not logically imply the outcome attributed to it. The observed association between advanced levels of economic modernity and democracy might plausibly explain why democracies in wealthy countries survive without explaining why wealthy countries became democratic.[33]
- A hypothesis can be rejected because its posited causal mechanism is considered so conceptually inadequate that it provides no insight into "how things work." This form of rejection is much stronger when the hypothesis in question is the latest version of a research program that has long suffered this problem. James Johnson levels this charge against political-culture research, for example.[34]
- A hypothesis can be rejected because it lacks causal depth: it may be definitionally sufficient for the outcome in question (candidate x won because more voters cast ballots for her); it may be part of the normal

course of affairs (bridges may collapse while cars drive over them, but the cause is presumably a structural defect, not the cars that the bridge was designed to support); or it may be subsumed by a hypothesis that lies further back on a causal chain (the East Asian financial crisis of the late 1990s was triggered by a run on local currencies; it was caused by a syndrome of structural imbalances).[35]

- A hypothesis can be rejected because its emphasis on large-scale causes of large-scale effects is demonstrably invalid in circumstances in which small-scale causes can have large effects or in circumstances in which stochastic processes follow power laws, usually producing small effects but occasionally producing large effects whose magnitude is inversely related to their frequency.[36]
- A hypothesis can be rejected because its truth implies observations that cannot be made by credible techniques. This was the means by which the Berkeley team rejected the otherwise plausible inference that the iridium anomaly was produced by a supernova explosion. The absence of long-term economic convergence on a global scale similarly discredits many simple models of economic growth, while the East Asian economic tigers discredit dependency theory.
- A hypothesis can be rejected, finally (but perhaps not conclusively), because it is shown to rest on an invalid inference stemming from a faulty research design.

As this list demonstrates, there are many ways to engage in fair, causal comparison, most of which are not contained within KKV's regulatory framework. The list demarcates a core working model, one that permits us to believe in some ideas because they appear, by current standards and knowledge, superior to their rivals. Fair, causal comparison definitionally implies the simultaneous occurrence of the confirmation of one hypothesis and the elimination of rival hypotheses.[37] It is thus reasonable to commit to a vindicated hypothesis, even tentatively and in full knowledge that superior alternatives might yet emerge. We reach this judgment in diverse ways. Statistical studies and qualitative studies striving to mimic statistical exactitude are powerful members of our methodological ensemble, but they have valuable accomplices whose contributions should be neither overlooked nor slighted. Those allies are largely based on the consideration of causal mechanisms: our evaluative criteria thus deal not only with evidentiary warrants, but also centrally with conceptual coherence and theoretical logic.

Consider, as an illustration, Robert Putnam's, *Making Democracy Work*, a book that has won wide acclaim (KKV consider it emblematic of high methodological standards) for its methodological sophistication and its attention to causal mechanisms.[38] Putnam argues that the quality of political institutions reflects the level of civic culture. In northern Italy, citizens display high levels of civic culture and enjoy efficient political institutions; however, in southern Italy, citizens lack civic culture and political institutions are correspondingly debilitated. Putnam then turns to historical analysis to explain why civic culture flourishes in the north but not in the south of Italy. In the nineteenth century, for example, mutual aid societies and other forms of voluntary organization proliferated in the north but were starkly absent in the south, where, according to an historian quoted by Putnam, "The peasants were in constant competition with each other for the best strips of land on the *latifondo*, and for what meager resources were available. Vertical relationships between landlord and client, and obsequiousness to the landlord, were more important than fixed solidarities."[39]

Process tracing thus seems to confirm Putnam's account: long-standing cultural differences are associated with different patterns of behavior and different institutional outcomes. Yet Putnam does not consider any alternative hypothesis, even though his evidence supports clear and credible rivals. While discussing his culturalist perspective, he tells us that, as aristocratic rule in northern Italy was gradually declining, "From 1504 until 1860, all of Italy south of the Papal States was ruled by the Hapsburgs and the Bourbons, who . . . systematically destroyed horizontal ties of solidarity in order to maintain the primacy of vertical ties of dependence and exploitation."[40] The use of power to prevent peasants from achieving social solidarity that might be used to challenge upper-class hegemony did not end with unification, for in postunification Italy, "The southern feudal nobility . . . used private violence, as well as their privileged access to state resources, to reinforce vertical relations of dominion and personal dependency and to discourage horizontal solidarity."[41]

Putnam's historical sketch thus provides two very different casual mechanisms, one cultural and the other rooted in power relations. In one depiction, peasants mistrust one another and anxiously seek patronage from local elites, thereby creating relations of dependency inconsistent with and injurious to civic culture: this is the genuine cultural interpretation that Putnam wishes to vindicate. But in the second, power-laden image, southern elites deploy their power strategically to create and recreate these vertical ties of dependence and to actively discourage horizontal solidarity among lower classes. The two scenarios are hard to reconcile: if southern peasants were culturally hostile to horizontal norms of solidarity and engagement, why did elites so persistently feel the need to maintain vertical ties and destroy alternative horizontal ones? That power relations and not culture values are responsible for political behavior in southern Italy is an alternative and very plausible interpretation of Putnam's own sources, and Putnam makes no efforts to reject this alternative reading. Absent explicit engagement with this alternative thesis—absent fair causal comparison—there is no reason to vindicate Putnam's hypothesis that it is culturally deprived peasants who intentionally create the relations of their own exploitation.[42]

The differences between the work of the Berkeley team and of Putnam's research team, with the first engaging in fair causal comparison while violating many norms of KKV and the second adhering to KKV while avoiding fair causal comparison, illustrate why fair, causal comparison must render methodological pluralism more credible than methodological unity: there are multiple ways to engage in fair, causal comparison, only some of which are captured by the rules and regulations contained in KKV. The point is not that research design considerations are dispensable, the point, rather, is that efforts to confirm propositions and use them in explanations need not be based solely on the logic of statistical inference. Paying attention to causal mechanisms gives us diverse means to engage in fair causal comparison while attending to the demands of explanatory adequacy.

Sometimes scientists will formulate research designs in ways consistent with KKV's advice; at other times, scholars will approach existing data with a disciplined plan to adjudicate debates, focusing on the conceptual validity and theoretical coherence of the purported causal mechanisms. The two strategies have their respective strengths and weaknesses. Instead of abstractly judging one strategy superior to the other, researchers should pragmatically adapt strategies to the demands of particular research questions. The basic rule is this: if existing data can be interpreted to vindicate a hypothesis against its rivals in a process of fair, causal comparison, then no further data gathering is necessary. If existing data cannot accomplish this task because the existing data is consistent with multiple hypotheses and because thinking about causal mechanisms yields no gains, then new research oriented explicitly toward determining a victor from among the current contributors to the theoretical controversy is required. The appropriate research strategy is always a function of the existing state of knowledge—the data that is available, the controversies on behalf of whose adjudication it was gathered, and the state of theoretical contestation. Under this understanding, the Berkeley team made a reasonable claim to have explained the mass extinction at the K/T boundary.

Causal mechanisms thus play a role in both explanation and confirmation. Analysis of causal mechanisms assists confirmation via strategies of inference that do not follow in the footsteps of statistical reasoning but that do contribute to explanatory adequacy. The lessons of KKV are thus neither necessary nor sufficient for good social science research that takes explanatory adequacy as its goal. This conclusion does not deny that those lessons are highly valuable; rather it locates their value within a more pluralistic field of methodological strategies.

Explanatory Adequacy at the K/T Boundary . . . and Beyond

We have seen how causal mechanisms play a valuable role in confirming hypotheses and an indispensable role in explaining outcomes. But explanatory adequacy requires more than identifying and substantiating causal mechanisms: they must be linked together to span the gap between cause and effect, explaining most of the major causal relationships along the way. We construct adequate explanations by identifying chains of linked causal mechanisms that were, under the specific circumstances, sufficient to produce the outcome. By this standard, many hypotheses, including the bolide-impact hypothesis, can be confirmed hypotheses but offer inadequate explanation.

That a large meteor struck the earth at the K/T boundary is beyond reasonable dispute. Without entering the full debate between the impact hypothesis and its volcanic rival, let us concede the point to the impact side and ignore the issue of rivals.[43] This does not mean that the vindicated impact hypothesis counts as an adequate explanation of the subsequent mass extinction marking the end of the Cretaceous period. To make that claim, we must not only rule out alternative hypotheses but also connect cause and effect through a chain of causal mechanisms. How well does the impact hypothesis explain according to this standard? This question breaks down into subquestions: What are the geological and environmental effects of the impact? By what biological mechanisms did changes in the physical environment result in the mass extinction? And what explains the actual pattern of extinctions? Why, in other words, did some genera become extinct while others did not?[44]

The Berkeley team identified their original causal mechanism by extrapolating from the 1883 explosion of the Krakatoa volcano. That explosion had kicked up enough dust and ash to alter global atmospheric conditions for months; expand the scale of those effects to the size of catastrophic impact, they reasoned, and dust in the air would kill plant life, leading to a collapse of the food chain and mass extinction. Observations made in 1994 when the comet Shoemaker-Levy 9 struck Jupiter support calculations that, given its tremendous speed, the meteorite would have carried with it tremendous energy—far greater than that contained in today's global supply of nuclear weapons—energy that would have to be dissipated postimpact. The predicted effects include "shock waves, tsunamis (tidal waves), acid rain, forest fires, darkness caused by atmospheric dust and soot, and global heating or global cooling."[45] There is evidence for a variety of these postimpact scenarios, including global wildfires, acid rain, and a decade-long "impact" winter.[46] But even book-length, enthusiastic defenses of the impact hypothesis devote startlingly little space to fleshing out these scenarios, more typically concluding that "we know that [the impact] must have had some combination of the effects described. What we do not know is just how the many lethal possibilities would have interacted with each other and with living organisms."[47] There remain, in other words, large gaps in our knowledge of cause and effect. Indeed, recent evidence indicates that the size and distribution of the impact ejecta do not meet the levels required for the shutdown of photosynthesis; thus, the initial mechanism posited by the Berkeley team is quite possibly invalid.[48]

Let us turn next to the variable biotic responses to the K/T environmental disturbances. Given catastrophic environmental changes, we might expect uniform rates of extinction across all genera, but this is not what is said to have occurred in this or any of the other major mass extinctions. Thus, we have two specific questions concerning biological mechanisms of extinction: what mechanism led to the mass killing, and why did it kill some genera but not others? These questions have prompted even firm supporters of the impact hypothesis to conclude that the theory contains some "puzzling features," in the words of Richard Fortey. "There are many animals and plants that *did* survive," Fortey continues, "and somehow it does not seem satisfying to call them 'lucky ones' and leave it at that. Their survival should chime in with the fatal scenario."[49] Responding to what has been called the "Dante's Inferno" scenario of a broad cluster of environmental catastrophes subsequent to the impact, William Clemens states,

> I think the results of studies of patterns of survival and extinction of terrestrial vertebrates fully falsify the hypothesis that an impact caused the terminal Cretaceous extinctions of terrestrial vertebrates *through the series of environmental catastrophes embodied in the "Dante's Inferno" scenario.* Ancestors of groups that are today known to be unable to tolerate major climatic change, such as frogs, salamanders, lizards, turtles, and birds, survived whatever caused the extinction of the other dinosaurs.[50]

Neither Fortey nor Clemens rejects the impact hypothesis *in toto*. Rather, they insist that extinction is ultimately a biological phenomenon, and that along with verifying the geological and environmental consequences of the impact, we need to specify carefully the biological causal mechanisms that produced distinct patterns of extinction before we can claim to have achieved explanatory adequacy.[51]

Defenders of the impact hypothesis treat the call for more fine-grained mechanisms capable of distinguishing heterogeneous intergenera responses to the impact as an unreasonable assault on the impact hypothesis itself, which they insist has met many tests and failed none of them and so deserves to be considered corroborated.[52] This response is based on the conflation of confirmation and explanation. We can agree that the impact hypothesis has been confirmed—the impact itself occurred and we have good reason to believe it was the ultimate cause of extinction—and still maintain that we do not yet possess an adequate explanation of how the dinosaurs and other genera became extinct. Writing recently in the journal *Paleobiology*, Norman MacLeod characterizes the physical evidence for the bolide impact hypothesiss as "overwhelming" and the hypothesis itself as "fully proven, though a number of interesting subsidiary controversies still exist." MacLeod rightly insists that the issue at hand is one of standards of explanatory adequacy, as

> the specification of precise extinction mechanisms is an indivisible part of explaining *any* mass extinction event. Just as geologists remained skeptical about continental drift until a precise causal mechanism . . . was proposed . . . paleontologists will remain skeptical about the connection between impacts and extinctions until precise biological/ecological mechanisms are proposed that uniquely account for observed taxic patterns and the stratigraphic timing of K/T extinction and survivorship.[53]

Inferential goodness, we must conclude, is not equivalent to explanatory goodness. While the specification of causal mechanisms can support inferential goodness, for reasons discussed at length above, identifying causal mechanisms is not sufficient for adequate explanations. Instead, when it comes to explanations, we should become greedy: we should expect a full and complete causal chain, tightly linking cause and effect. Let us consider an example from contemporary social science, turning again to Robert Putnam's *Making Democracy Work*. Since we want to focus on the question of explanatory goodness, let us ignore the issues raised above and concede the inferential validity of the work: northern Italy enjoys high civic culture and well-performing institutions while southern Italy lacks civic culture and suffers ill-performing institutions. Why is this? What is the causal link between civic culture and high-performing institutions? To his credit, Putnam addresses this issue head on, and his provision of a credible causal mechanism has led reviewers to praise the book highly. In a phrase, the answer is social capital: northern Italians have developed norms of reciprocity and mutual collaboration for the collective good, permitting them to engage in collective action that in turn reinforces the norms of reciprocity; southern Italians, mired in distrust, cannot engage in collective action. These two outcomes are both stable equilibria, but one produces fortunate outcomes, the other gross misfortune.

For Putnam, the lesson is clear: "These contrasting social contexts plainly affected how the new institutions worked."[54] But just as the paleontologist MacLeod asked for biological mechanisms to go along with geological ones, we might ask Putnam for social and institutional mechanisms to complement and complete the causal chain that begins with social capital. How exactly does social capital produce variations in institutional performance? Putnam depicts northern Italians confronting poor institutional performance as people who draw on their reserves of social capital, band together in collective action, demand better performance, and monitor and enforce compliance with their demands. Southern Italians, with no social capital to invest, respond to poor performance lethargically, if at all. But Putnam provides no evidence of this differential propensity for collective action on behalf of better institutions. Indeed, Putnam writes only two paragraphs at the very end of the book discussing the causal link between effective democratic governance and a vigorous civil society:

> On the demand side, citizens in civic communities expect better government and (in part through their own efforts), they get it. They demand more effective public service, and they are prepared to act collectively to achieve their shared goals. Their counterparts in less civic regions more commonly assume the role of alienated and cynical supplicants.
>
> On the supply side, the performance of representative government is facilitated by the social infrastructure of civic communities and by the democratic values of both officials and citizens. Most fundamental to the civic community is the social ability to collaborate for shared interests. Generalized reciprocity . . . generates high social capital and underpins collaboration.[55]

These causal mechanisms present some problems. First, citizens in the north of Italy not only expect better government, but they get it "in part through their own efforts." There is no evidence that they make those efforts, only a theoretical argument that they should be favorably disposed to making them. Moreover, by parenthetically claiming that citizen's efforts are only "part" of the story, Putnam raises the possibility of causal incompleteness: some other causal mechanism, perhaps unrelated to social capital, might be needed to complete the explanation. Second, in the following sentence we are told that northern Italians "are prepared to act collectively to achieve their shared goals." Everything about the argument to that point prepares us to anticipate that citizens act, even if this is only "part" of the overall story: now we find that they have a latent predisposition to act, but they do not necessarily make that predisposition manifest. But if northern citizens do not in fact act, what causes institutional effectiveness? Here, Putnam introduces a "supply side" to complement the "demand side." The supply side implies that public officials in northern Italy are, relative to their southern counterparts, either more inclined to provide good public service, or more capable of providing good public service, or both. But if northern officials are disposed and equipped to provide good public service independent of citizens' collective action, what role does the demand side play? Why must northern citizens (be prepared to) act collectively on behalf of good public service if their public officials are independently prepared to provide those services? One might think that the demand side, which now looks far more anemic than earlier parts of the book implied, would be necessary only if the supply side did not reliably exist. It is quite possible that these logical relations could be sorted out and that evidence could be provided for the final version of the causal chain; until that is accomplished, we should be skeptical of the connections between civic culture and institutional performance.[56] Even if we accept as confirmed the inference that it is civic culture that most centrally distinguishes the two halves of Italy (and there are independent reasons to not accept it), we still must conclude that inferential goodness is not accompanied here by explanatory goodness.

Conclusion

Philosophers and scientists have long recognized that causal explanations are based on inferences. Not all inferences are explanations, however, and many explanatory propositions—ones that contain at least a sketch of the relevant causal mechanisms—perform poorly as explanations. This essay has sought to demarcate boundaries between inferences, explanations, and adequate explanations. Doing so, I have argued, raises serious doubts about the validity of methodological unity. Despite the powerful arguments contained in KKV, I have argued that a framework for inferences does not function doubly as a framework for explanations; the KKV framework thus cannot be considered sufficient for generating good explanations. And in addition to using causal mechanisms as explanatory instruments, they are also highly valuable tools in the often difficult practice of confirmation. There are thus multiple and diverse ways to infirm or confirm hypotheses, many of which are not even implicit in the framework of KKV: that framework thus cannot be considered as necessary for explanatory adequacy either.

It might be objected that the explanatory adequacy I advocate here is neither feasible nor desirable. This seems to be the position of the authors of many statistical textbooks, who apparently agree that

> Social science explanations are, at best, partial theories that indicate only the few important potential influences. Even if one could establish the list of all influences, this list would be sufficiently long to prohibit most analysis. Generally, researchers are trying to estimate accurately the importance of the influences that are central to policy making or to theoretical developments.[57]

Paul Humphreys, a philosopher who has written widely on the philosophy of explanations, offers the characterization of such incompletely specified explanations as "explanatorily informative," arguing that incomplete explanations are not necessarily untrue ones, and that explanatory incompleteness has in no way hindered the progress of important scientific and public-policies.[58]

These objections, I think, only vindicate my point. Different communities of scholars may in fact have very different yet noncompetitive standards of explanatory adequacy.[59] Ironically, KKV recognize this point. On the book's first page, they write of two "styles" of research. Quantitative research uses numbers to measure "specific aspects of phenomena [and] it abstracts from particular instances to seek general description or to test causal hypotheses . . . ." Qualitative research, on the other hand, tends "to focus on one or a small number of cases . . . and to be concerned with a rounded or comprehensive account of some event or unit" (KKV, 3–4).

KKV claim that these matters of style are "methodologically and substantively unimportant." But if qualitativists are licensed to desire "rounded or comprehensive accounts," then the standards to which I hold them, with all of their methodological implications, are commensurately legitimate. Quantitativists may deny the centrality of causal mechanisms and thus reject the implications I draw from consideration of those mechanisms, but that may simply be a function of their predisposition toward abstracting from particular instances—a disposition that leads them to discount the value or the feasibility of such comprehensive accounts. Style, *pace* KKV, may in fact matter greatly.

True, giving comprehensive causal accounts may be desirable but not feasible. It may be the case that phenomena are *sequelae* to enormous numbers of intercorrelated causal influences, so that comprehensive and fully specified lists are unattainable. And it might be the case that the sort of definitive tests of hypotheses represented by the Berkeley team's confirmation of the bolide-impact hypothesis are unattainable in the social sciences. Even Stephen Van Evera, a defender of case-study methods, claims that "Most predictions have low uniqueness and low certitude." Passing a test does not rule out rivals (low uniqueness), but failing a test leaves relatively unharmed propositions that make only probabilistic predictions (low certitude). Most social science, Van Evera avers, consists of "straw-in-the-wind tests."[60]

It is perfectly legitimate to accept any one or all of these wagers, to settle on an ontological and epistemological position contrary to the one this chapter advances. But given that social sciences are enveloped in uncertainty so extensive that we cannot be sure of any causal relationships let alone the underlying causal structure of the world, we might also defer judgment, embrace some agnosticism, or accept the rationality of a diversified methodological portfolio. Until it can be proven that explanatory adequacy as discussed here is a chimera, the philosophical position outlined here is valid and the methodological implications follow. Methodological pluralism cannot be defeated by assuming a world that would deliver that defeat.

Notes

1. Richard W. Miller, *Fact and Method: Explanation*, *Confirmation*, *and Reality in the Natural and Social Sciences* (Princeton, NJ, 1987), esp. chap. 4.
2. There are other types of (noncausal) explanations; their existence does not alter the arguments made here. For review, see W.H. Newton-Smith, "Explanation," in W.H. Newton-Smith, ed., *A Companion to the Philosophy of Science* (Malden, MA: Blackwell Publishers, 2000), pp. 127–33.
3. For pioneering statements of the importance of causal mechanisms to social-science explanations, see David Dessler, "Beyond Correlations: Toward a Causal Theory of War." *International Studies Quarterly*, Vol. 35 (1991): 337–55; and Timothy Mckeown, "Case Studies and the Statistical Worldview: Review of King, Keohane, and Verba's *Designing Social Inquiry: Scientific Inference in Qualitative Methods*," *International Organization*, Vol. 53 (Winter 1999): 161–90.
4. My account is based entirely on materials that became available only after the publication of KKV.
5. On the relationship between an ontology of causal mechanisms and reasoning via abduction or via inference to the best explanation, see Ian Shapiro and Alexander Wendt, "The Difference that Realism Makes: Social Science and the Politics of Consent," *Politics & Society*, Vol. 20 (June 1992): 197–223.
6. The main source for what follows is Walter Alvarez, *T. Rex and the Crater of Doom* (Vintage Books, 1997).
7. Iridium exists on earth in the same proportion that it exists on meteorites: but like other heavy elements, iridium concentrates in the earth's core. Most iridium in the earth's crust has been deposited by meteorites. Meteorite dust accumulates slowly: if there is a measurable accumulation, the rate of sedimentation must also have been relatively slow, suggesting that the rising proportion of clay at the K/T boundary was caused by an abrupt extinction of foram. If there were virtually no iridium accumulations, then powerful support would be given the hypothesis that the K/T boundary was due to rising rates of clay deposition and not an abrupt extinction.

8. *T. Rex and the Crater of Doom*, p. 76, emphasis in original.
9. The H-D method played an ancillary role in this procedure. For example, as a test of the hypothesis that a supernova explosion deposited the iridium, they searched for the presence of plutonium-244; its complete absence falsified the supernova hypothesis.
10. Luis W. Alvarez, Walter Alvarez, Frank Asaro, and Helen V. Michel, "Extraterrestrial Cause for the Cretaceous-Tertiary Extinction," *Science*, Vol. 208 (June 6, 1980): 1095–108.
11. See, for example, John Earman and Wesley C. Salmon, "The Confirmation of Scientific Hypotheses," in Wesley C. Salmon, et al ., *Introduction to the Philosophy of Science* (New York: Prentice-Hall, Inc., 1992), p. 44.
12. Henry Brady has previously observed that KKV implicitly equates explanation with causal inference, a conceptual disposition shared by most statistical work and exegesis. Henry Brady, "Doing Good and Doing Better: Symposium on *Designing Social Inquiry*, Part 2," *The Political Methodologist*, Vol. 6 (Spring 1995), 13 at footnote 6, and 14.
13. Examples courtesy of Wesley Salmon, "Four Decades of Scientific Research," in Philip Kitcher and Wesley C. Salmon, eds., *Minnesota Studies in the Philosophy of Science*, *XIII: Scientific Explanation* (University of Minnesota Press, 1989), pp. 46–50.
14. The second and third features are discussed in Richard Miller, *Fact and Method*, chap. 4.
15. A causal effect, they argue, is "the difference between the systematic component of observations made when the explanatory variable takes one value and the systematic component of comparable observations when the explanatory variable takes on another value." (81–82). This definition has some virtues, in that it manifests keen awareness of the distinction between random and systematic causes and is clearly rooted in a counterfactual approach to causality. On the other hand, it substitutes an operational definition—how do we measure causality—for a semantic definition—What does it mean to say that X causes Y?—that one typically finds in the philosophical literature. See also McKeown, "Case Studies and the Statistical World View," pp. 162–64.
16. James Johnson, "Conceptual Problems as Obstacles to Progress in Political Science," *Journal of Theoretical Politics*, Vol.15 (2003): 94.
17. Robin Dunbar, *The Trouble with Science* (Harvard University Press, 1995), p. 14.
18. For important critical evaluation, see Henry E. Brady and David Collier, eds., *Rethinking Social Inquiry: Diverse Tools*, *Shared Standards* (Lanham, MD: Rowman & Littlefield Publishers, Inc., 2004).
19. Michael J. Benton, "Scientific Methodologies in Collision: The History of the Study of the Extinction of the Dinosaurs," *Evolutionary Biology*, Vol.24 (1990): 371–400.
20. The best introduction is Stephen M. Stanley, *Extinction* (New York: Scientific American Library, 1987).
21. Alvarez et al ., "Extraterrestrial Cause," p. 1095.
22. Power laws inversely relate the magnitude of an event to its frequency: small earthquakes are far more common than powerful ones, and small numbers

of species and genera will go extinct far more often than mass extinctions will occur. There are various ways to model this relationship so that mass extinctions do not have causes distinct from small-scale extinctions; if this were true, there would be no reason to look for a unique or rare cause of the K/T extinction. David Raup provides powerful evidence against this line of reasoning in his *Extinction: Bad Genes or Bad Luck*? (New York: W.W. Norton and Company, 1991).

23. A massive bolide impact 35 million years ago on the eastern shore of Virginia created an impact crater roughly the size of Rhode Island. While the impact "obliterated" local flora and fauna, it did not result in a mass extinction. For details, see http://woodshole.er.usgs.gov/epubs/bolide/. Last accessed July 12, 2005.
24. See, for example, K.A. Farley, "Geochemical evidence for a comet shower in the late Eocene," *Science*, Vol.280 (May 22, 1998), pp. 1250–54.
25. A global distribution of the iridium anomaly is a necessary condition for there to have been a large bolide impact coincident to the K/T boundary. Selecting on the dependent variable is in fact obligatory under these circumstances. See Brian Skyrms, *Choice & Chance: An Introduction to Inductive Logic*, 3rd ed. (Wadsworth Publishing Company, 1986), pp. 90–94.
26. KKV explicitly make this defense in their "Importance of Research Design," p. 476.
27. It is true, of course, that the Berkeley team did not consider the matter settled: they concluded their article with two potential tests of their hypothesis. First, their hypothesis *could* be in the future tested against the other four major instances of mass extinction; second, they could find the crater associated with the meteor impact (and whose size they were able to forecast). Note, by the way, that there was no reason to expect to find that crater: the greater probability was that it would be an oceanic impact whose traces would have been eliminated by subduction of the pre-tertiary ocean floor.
28. Gary King, Robert O. Keohane, and Sidney Verba. 1995. "The Importance of Research Design in Political Science," *American Political Science Review*, Vol. 89 (June 1995): 477.
29. Alvarez et al ., "Extraterrestrial Cause," p. 1095.
30. This condition implies a Bayesian perspective. For an excellent discussion, see Jack A. Goldstone, "Comparative Historical Analysis and Knowledge Accumulation in the Study of Revolutions," in James Mahoney and Dietrich Rueschemeyer, eds., *Comparative Historical Analysis in the Social Sciences* (Cambridge, England: Cambridge University Press, 2003).
31. Miller,*Fact and Method*, pp. 155, 163.
32. Ibid., p. 158.
33. Adam Przeworski, et al ., *Democracy and Development: Political Institutions and Well-Being in the World, 1950–1990* (Cambridge, England: Cambridge University Press, 2000).
34. Johnson, "Conceptual Problems."
35. Particularly good on the pragmatics of explanation is John Gerring, *Social Science Methodology: A Criterial Framework* (Cambridge, England: Cambridge University Press, 2001), pp. 90–99.

36. For the argument that nonlinear models of change discredit a diverse array of existing approaches to political science, see Alan S. Zuckerman, "Reformulating Explanatory Standards and Advancing Theory in Comparative Politics," in Mark Irving Lichbach and Alan S. Zuckerman, eds., *Comparative Politics: Rationality, Culture, and Structure* (Cambridge, England: Cambridge University Press, 1997), pp. 277–310.
37. For elaboration of this point, see McKeown's perceptive explication of the logic of process tracing in his "Case Studies and the Statistical Worldview."
38. Robert Putnam, *Making Democracy Work: Civic Traditions in Modern Italy* (Princeton University Press, 1993).
39. *Making Democracy Work*, p. 143.
40. Ibid., p. 136.
41. Ibid., p. 145.
42. Ibid., pp. 177–78.
43. For the full set of reasons to discount heavily the hypothesis that the K/T extinction was caused by huge volcanic outpourings on the Indian subcontinent, see James Lawrence Powell, *Night Comes to the Cretaceous: Dinosaur Extinction and the Transformation of Modern Geology* (New York: W.H. Freeman and Company, 1998), pp. 85–95.
44. We know than answering this last question requires moving beyond geologic and environmental mechanisms to investigate genera-specific biological factors. The two dinosaur groups suffered extinction of all twenty-two of their genera. Given an overall extinction rate of 43 percent of all genera, the probability that every genus of the dinosaurs would go extinct by chance alone is virtually nil. For the ingenious reasoning, see Raup, *Extinction: Bad Genes or Bad Luck*? pp. 88–105.
45. Raup, *Extinction: Bad Genes or Bad Luck*? p. 161.
46. See the brief discussion in Powell, *Night Comes to the Cretaceous*, pp. 176–79.
47. Powell, *Night Comes to the Cretaceous*, p. 179.
48. Kevin O. Pope, "Impact Dust not the Cause of the Cretaceous-Tertiary Mass Extinction," *Geology*, Vol. 30 (February 2002): 99–102. Pope's findings do not challenge the impact hypothesis itself; rather, they suggest the need for more research into explanatory mechanisms. See his perceptive commentary on explanatory adequacy in David Braun, "Researchers Rethink Dinosaur Die off Scenario," *National Geographic News,* February 26, 2002 at http://news.nationalgeographic.com/news/2002/02/0222_020222_dinodust.html. Last accessed July 8, 2005.
49. Richard Fortey, *Life: A Natural History of the First Four Billion Years of Life on Earth* (Alfred A. Knopf, 1998), p. 253. Fortey stresses the anomalous survival of insects that have an annual life cycle and rely on live plants for food and shelter and so should not have survived even a decade-long environmental catastrophe. Other puzzling survivors include bony fish, coral, and birds, which are now widely recognized to be direct descendants of dinosaurs.
50. "On the Mass Extinction Debates: An Interview with William A. Clemens," in William Glen, ed., *The Mass-Extinction Debates: How Science Works in a Crisis* (Stanford University Press, 1994), pp. 245–46.

51. This is by no means an unreasonable demand. As descendants of the genera that survived the impact catastrophe, we should be interested in what made us possible. And answering that question is not technically infeasible. For an introduction to the emerging field of "Astrobiology," see Charles S. Cockell, *Impossible Extinction: Natural Catastrophes and the Supremacy of the Microbial World* (Cambridge, England: Cambridge University Press, 2003).
52. Powell, *Night Comes to the Cretaceous*, pp. 179–80.
53. Norman MacLeod, "K/T Redux," *Paleobiology*, Vol. 22 (1996), p. 315.
54. Putnam, *Making Democracy Work*, p. 182.
55. Ibid., pp. 182–83.
56. Gabriel Almond and Sidney Verba explored this issue some four decades ago, arguing that civic culture did not motivate citizens to act, but rather motivated officials to anticipate and preempt citizen action; through the law of anticipated reactions, then, citizens could exercise influence over elites without being conscious of doing so. The authors also acknowledge, however, that their data cannot demonstrate this point. What we have, then, is a plausible but still uncorroborated sketch of the relevant causal mechanism. See their *The Civic Culture: Political Attitudes and Democracy in Five Nations* (Princeton University Press, 1963), pp. 486–87.
57. Eric A. Hanushek and John E. Jackson, *Statistical Methods for Social Scientists* (Academic Press, 1977), 12, as cited in Lieberson, *Making it Count*, p. 186.
58. Paul Humphreys, *The Chances of Explanation: Causal Explanation in the Social, Medical, and Physical Sciences* (Princeton University Press, 1989).
59. Thus, physicists might feel satisfied that the bolide impact explains the extinction; paleontologists can still legitimately call for more fine-grained analysis. Likewise, I might decide to stop smoking because I believe that smoking causes cancer; epidemiologists can still explore why cigarettes kill some smokers and leave others unharmed.
60. Stephen Van Evera, *Guide to Methods for Students of Political Science* (Cornell University Press, 1997), pp. 30–34.

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7

Theory, Evidence, and Politics in the Evolution of International Relations Research Programs

Jack S. Levy

The field of international relations has always been diverse in its metatheoretical and methodological orientations, perhaps more so than any other field in political science, and intrafield debates about the proper way to study world politics has made it a richer, more interesting, and stronger field. The contentious nature of the discipline is reflected in the fact that the history of the field is often told in terms of a sequence of "great debates." These include debates between interwar idealists and postwar realists (Carr 1939; Morgenthau 1948), between "traditionalists" and "behavioralists" in the 1960s (Bull 1966; Kaplan 1966), and among realists, liberals, and Marxists beginning in the 1970s (Gilpin 1975; Guzzini 1998).

Until recently, these debates were conducted within certain limits (Holsti 1985). Despite their differences, most traditionalists and behavioralists adopted a realist world view (Vasquez 1983; Schmidt 2000). Similarly, the "paradigm wars," particularly between neoliberalism and neorealism, were conducted within an underlying rationalist consensus (Waever 1998; Ruggie 1998). In the last decade, however, that consensus came under sharp attack by various forms of postpositivism, including postmodernism, poststructuralism, feminism, and constructivism. This so-called third debate (Lapid 1989) is in many respects more profound than earlier ones, because underlying ontological and epistemological issues are at the core of the debate.

One theme in these ongoing debates concerns the criteria by which scholars evaluate progress in the cumulation of knowledge. In the last decade or so international relations scholars have been more explicit in grounding their conceptions of scientific progress in particular approaches in the philosophy of science. Many have used Imre Lakatos's (1970) methodology of scientific research programs (Vasquez and Elman 2003; Elman and Elman 2003), while others have criticized Lakatosian metatheory and turned instead to Popper (1957, 1962) or Laudan (1978).[1] Still, each of these metatheoretical frameworks falls within a positivistic conception of social science. This has led others, including many of the contributors to this volume (Bernstein et al., Hopf, Kratochwil, and Lebow), to adopt more critical perspectives. They deal with questions of ontology and epistemology as well as method, and they attempt to broaden the conception of science and thus of what constitutes scientific progress.

While these debates focus on the normative questions of what constitutes scientific progress and the proper criteria for evaluating progress, and thus on how research ought to evolve, my own concern in this chapter is with the more descriptive question of how scientific research programs actually evolve. That is, I am concerned more with the history of research programs than with the prescriptive methodology for evaluating them.[2]

More specifically, I ask the related questions of what factors influence the evolution of research programs and why some programs or traditions are more "successful" than others, defined in terms of their impact on and endurance in the field.[3] I give particular attention to the relationship between theory and evidence. Is the research process dominated by theory, so that research programs endure because they are characterized by theoretical elegance, deductive fertility, and wide-ranging explanatory power? Or is the research process driven by evidence, with the most successful research programs characterized by the extensive support they draw from the accumulation of empirical evidence? Alternatively, do research traditions endure because they respond to current events and/or reflect the policy agendas of the governments or perhaps competing elites?

These questions are more descriptive than normative, and they lead me to direct my primary attention to the level of research design. This stands in contrast to most of the chapters in this volume, which give more attention to questions of ontology and epistemology. Admittedly, questions of method cannot be entirely separated from more fundamental metatheoretical questions. At the same time, however, a useful prescriptive methodology for how a research program ought to develop cannot be entirely divorced from an understanding of how research programs actually develop, and this chapter on the history of research programs provides a useful perspective for the more metatheoretically oriented essays in the rest of the volume.

I organize this chapter around a simple typology of the primary factors influencing the evolution of research programs: theory, evidence, and politics. I argue that different research programs follow different paths to success (and to failure), and that these different paths involve different sequences of theory and evidence. Some research programs are primarily theory driven, others primarily evidence driven, and still others are driven by an alternating sequence of theoretical conjectures and empirical refutations.[4] I illustrate these different paths with examples from a number of research programs in international relations and in political science more generally. I then consider the impact of current events and policy agendas on research programs. I argue that some research programs evolve independently of specific normative values or policy agendas and are driven primarily by autonomous analytical developments or by evidentiary support.

Space constraints preclude a fully systematic empirical analysis of a variety of research programs and their historical evolution. Ideally, such a study would incorporate research programs characterized by variation across a number of dimensions. They would include non-American as well as American scholarship,[5] qualitative as well as quantitative and formal research, and work that falls outside as well as inside positivistic social science. Given the goals of understanding why research programs succeed, it would also be important to include failed research programs. This is not at all possible in a short essay, but our coverage will be broad enough to demonstrate the multiple paths through which international relations research programs develop.

It is useful to acknowledge at this point that the task of assessing the relative impact of theory, evidence, and policy on the evolution of research is complicated by the fact that research programs are generally macrolevel phenomena that involve many scholars and that represent the aggregation of many individual decisions as to where to focus their scholarly efforts. Different scholars may choose to work within a given research program for different, even diametrically opposed reasons. In addition, one set of factors may influence the initiation of a research program while other factors may help to sustain or expand it. These complications make it difficult to identify a single pattern underlying a particular research program.

Theory-Driven Research Programs

Some research programs are driven primarily by theoretical considerations. The strength or quality of a theory is a function of a number of criteria

(Hempel 1966), including its degree of falsifiability and internal consistency,[6] its deductive power, its elegance and parsimony,[7] the plausibility and completeness of its hypothesized causal mechanisms, the range and number of its testable implications, and its consistency with existing laws and theories that have themselves received substantial degrees of empirical support. These are scientifically normative criteria, and are best distinguished from substantive normative values, which I treat in a separate category. Note that many of these criteria are matters of scholarly convention, as Chernoff argues in his contribution to this volume.

While empirical validation of the theory's key propositions clearly enhances its scholarly impact, a research program propelled by a powerful theory can be self-sustaining, even in the absence of a significant amount of supporting evidence. The best example comes from economics, where microeconomics is dominated by general equilibrium theory and a commitment to mathematical formalism and has little empirical content (Weintraub 1985; Backhouse, 1994). The best examples in political science are associated with the rational choice paradigm. Arrow's (1953) general impossibility theorem, Down's (1957) median voter theorem, and Olson's (1965) theory of collective action; each generated enormously influential research programs quite independently of empirical validation, although empirical work on the latter two topics subsequently reinforced those programs.[8]

In international relations, one of the best examples of a theory driven research program is the "bargaining model of war."[9] This rationalist model is based on Fearon's (1995) formalization of an idea suggested by Blainey (1973) and familiar to most economists: war is an inefficient means of settling disputes because it destroys resources that could have been shared by the contending parties. The question that needs to be answered, then, is what precludes parties with conflicting interests from reaching a negotiated settlement that avoids the mutual costs of violent conflict.

This fundamental idea generated a significant line of theoretical research that focuses on the role of "commitment problems," "private information" and incentives to misrepresent that information, and the divisibility or indivisibility of issues (Gartzke 1999; Powell 2002; Filson and Werner 2002; Wagner, 2000). The model has been applied to the study of ethnonational conflict (Fearon and Laitin 1996; Lake and Rothchild, 1998) as well as to interstate conflict. The conception of war as an information-revealing mechanism has also led to hypotheses about the termination of war (Slantchev 2003), some of which have recently been tested empirically. One measure of the influence of the bargaining model is the extent to which the concepts of private information, commitment, and issue indivisibility have become prominent in the qualitative as well as formal literature on international conflict, quite independently of any empirical confirmation of key propositions.

The bargaining model of war emerged quite independently of any obvious normative assumptions or policy issues. It was the product of certain analytic developments in game theory, particularly the incorporation of "incomplete information" into game-theoretic models beginning in the late 1980s. The bargaining model and in fact most of the contemporary game-theoretic models of economics and political science were not possible until economists invented certain analytic techniques that permitted the analysis of games with incomplete information.[10]

We could also include broader paradigmatic approaches such as realist, liberal, and Marxist-Leninist international theories as examples of theory-driven research programs, but difficulties quickly arise. These paradigms contain multiple theories that are not necessarily based on the same set of hard-core assumptions and that consequently may contain contradictory propositions. This means that evidence falsifying one theory might validate another, always leaving some theory within the paradigm consistent with any empirical observation, and thus leaving the paradigm itself immune to falsification.[11] More important, the analytic assumptions underlying each of these paradigms are much more normatively loaded than those for the rational choice paradigm (though not necessarily more than it is for specific substantive theories within rational choice, such as deterrence theory), and it is consequently much more difficult to differentiate the influence of abstract theory from the influence of policy agendas.

Evidence-Driven Research Programs

Some research programs are driven more by evidence than by theory. The strength of evidence refers to the overall quality of the research design; its effectiveness in controlling for extraneous variables and in dealing with endogeneity problems; the validity of empirical indicators for key theoretical concepts; the quality of the data; the appropriateness of any statistical methods used in the analysis of the data, including the fit between the assumptions of the statistical model and those of the theoretical proposition to which it is applied; the replicability of the data analysis and the extent and variety of replications of the analysis; the appropriateness of case selection, given the theory to be tested, including the sensitivity to possible selection bias; and the extent to which the findings can be generalized to other spatial and temporal domains beyond the immediate data.

These criteria are fairly standard in books on research methods, the most influential of which is King, Keohane, and Verba (1994). Their

Designing Social Inquiry gives more emphasis to empirical criteria than to theoretical criteria for scientific progress,[12] and it strongly suggests that successful theories are those with the greatest levels of empirical support—points that many of the book's critics have noted (Brady and Collier 2004).

One example of an empirically driven research program is the one on territory and war (Vasquez 1993; Vasquez and Senese 2004; Huth 1996; Hensel 2000; Huth and Alee 2002). This scholarship has been propelled by the repeated demonstration that a disproportionately high number of wars involve territorial disputes and that territorial disputes are more likely to lead to war than are other kinds of disputes. Thus far, however, there is little agreement on the precise causal mechanisms leading from territoriality to militarized conflict. This is clearly the case of a strong empirical finding coming first and stimulating theoretical efforts to explain that finding and associated empirical relationships.[13]

A more influential research program that is primarily evidence driven, at least in its early stages, but that involves a complex mix of factors, is the democratic peace.[14] It is undoubtedly true, as Lawrence argues (this volume; see also Oren 1995), that many joined in the study of the democratic peace because of a normative commitment to liberal democracy and to an American foreign policy agenda of actively promoting democratic values abroad. Yet perhaps just as many engaged the debate because of their realist worldviews, a determination to demonstrate the fallacy of early empirical work, save realism from one of its most glaring empirical anomalies, and steer American foreign policy away from a misguided liberal interventionism. While conceding the impact of policy agendas (liberal and antiliberal) on the democratic peace research program, I want to emphasize the primacy of another factor, particularly in the early stages of the research program—the unprecedented level of empirical support for the dyadic-level finding that democracies rarely if ever fight each other. It was the strength of this correlation, along with the absence of any unambiguous anomalies, that generated both the intellectual curiosity and professional incentives for realists, liberals, and others to redirect their research energies toward the democratic peace.

After Doyle (1983) and later a special issue of the *Journal of Conflict Resolution* (December 1984) emphasized that democracies almost never go to war with each other, many of the scholars who initiated research on this question were skeptics who were convinced that the findings were based on flawed research designs and who were determined to introduce greater rigor into democratic peace research. This certainly applies to Singer, who coauthored the first systematic study of democracy and peace (Small and Singer 1976) and who remains a skeptic. It also applies to Weede (1984), Bremer (1992), Maoz (1992), Bueno de Mesquita and

Lalman (1992), and Russett (1993), each of whom is now a strong believer in the dyadic democratic peace.[15] Many of these scholars may have wanted to believe that democracies rarely if ever fought each other, but most were skeptical of the validity of the finding and expected that it would wash out once scholars controlled for other key variables such as trade, distance, alliances, and the like. It was the near law-like character of the interdemocratic peace proposition, in a field in which relatively few empirical regularities of even modest strength had been uncovered, that energized scholars to engage in further studies in an attempt to validate or invalidate the early findings, to explore potential anomalies in more detail, to consider the possible extension of the findings to earlier temporal domains and to other international systems, and to generate and test additional theoretical implications of the democratic peace proposition. As the consensus grew that the dyadic democratic peace was real, so did the professional incentives for individuals to attempt to demonstrate that the proclaimed absence of war between democracies was the artifact of misspecified theoretical arguments and flawed research designs.

Some will disagree with my emphasis on the primacy of evidence in the evolution of the democratic peace research program, and a more thorough and systematic analysis is necessary to resolve the debate. One thing that everyone agrees has had little impact on the study of the democratic peace, at least until recently, is a strong theory. The empirical finding clearly came first, followed by attempts to validate it and to explore possible anomalies, and finally by theoretical conjectures to explain it, none of which has generated overwhelming support. The relative absence of war between democracies remains a strong empirical regularity in search of a theory to explain it.[16]

The Dialectic of Theory and Evidence

The theoretical and empirical dimensions of a research program are not analytically distinct, of course. One cannot analyze the evidentiary support for a theory apart from its fundamental assumptions and propositions or from alternative explanations for evidence consistent with the theory, all of which affect case selection, the operationalization of key variables, and all other aspects of research design (Merton 1974). A research program driven entirely by evidence, without any prior theoretical assumptions, is inconceivable. Few contemporary scholars would embrace the epistemology underlying Sgt. Joe Friday's (of "Dragnet" fame) request for "Just the facts, ma'am, just the facts." As Goethe wrote, "Every fact is already a theory" (cited in Waltz 1997: 913).

To say that all research is guided by theory does not imply that theory necessarily plays a greater role than does evidence in research programs, any more than the fact that most theories are influenced by some prior empirical observations implies that evidence plays a greater role. It is not clear, however, exactly how we should assign weights to theory and evidence. The problem is compounded by the multiple ways in which scholars use the term theory—to refer to everything from axiomatic deductive theory to broader conceptual frameworks or paradigms with contested and conflicting theoretical assumptions and only vaguely specified causal mechanisms.

One example is the ongoing debate over whether a preponderance of power or a parity of power is more likely to lead to war. This dyadic-level debate grew out of the power parity hypothesis of balance of power theory and the power preponderance hypothesis of power transition theory (Organski 1968). Neither theory carefully specified the causal mechanisms leading from structure to outcome,[17] but the debate was dominated by a series of empirical studies beginning in the 1970s.[18] There is now strong empirical evidence in support of the power preponderance hypothesis (Kugler and Lemke 1996), but the precise causal mechanisms remain poorly developed, in part because power transition theory has yet to incorporate a theory of bargaining (DiCicco and Levy 1999). I see this pattern as reflecting the dominance of evidence over theory in the evolution of power parity/power preponderance debate, though theory probably plays a greater role in the debate between balance of power theory and power transition theory.[19]

It is also possible that a research program can combine theoretical and empirical elements in an alternating sequence of theory and evidence: a reasonably well-specified theory leads to empirical tests that contradict some of the testable implications of the theory, which then leads to the modification of the theory or perhaps to its replacement by an alternative theory. Or the process may begin with robust empirical findings that lead to the construction of a theory to explain them, which leads to new predictions that guide subsequent empirical research. An alternating sequence of theory and evidence fits Popper's (1962) model of conjectures and refutations.

We start with a hypothesis, whether derived from a theory or induced from observation, test it against the evidence, and use the evidence to refine, revise, or reject the theory. This idea is explicit in the methodology of structured, focused comparison (George and Bennett 2005) and in the methodology of the analytic narrative research program (Bates et al. 1998).

This strategy for the cumulation of knowledge is also influential in historiography. Carr (1964: 20–21, 26–30) criticized both Rankean historiography

(Iggers 1984) for its "fetishism of facts" and historical idealism for its argument that empirical observations are entirely determined by theoretical preconceptions. Carr argued that "the historian is neither the humble slave nor the tyrannical master of his facts," and that history is "a continuous process of interaction between the historian and his facts, an unending dialogue between the present and the past." Similarly, many of the essays in this volume explicitly or implicitly accept as a normative ideal the model of an unending dialogue between theory and evidence, recognizing that theories with different ontological and epistemological foundations call for different kinds of evidence.

Still, there is a less-than-perfect fit between the conjectures and refutations ideal and the reality of political science research programs. In contrast to physics, which in many respects provides the paradigmatic case for Popper's model,[20] the social sciences provide fewer clear-cut rejections of a given theory. A possible exception are the experimental social sciences, where highly controlled experiments generate greater consensus on the refutation of theoretical conjectures.

A good example here is decision theory. If we define this research program broadly to include both formal (normative) decision theory and more descriptive research in social psychology and behavioral economics on how people actually make choices under conditions of risk,[21] then we can interpret a long line of work on decision theory in terms of an alternating sequence of conjectures and refutations.

We can trace the initial conjecture of decision theory to Pascal's proposal of the expected value criterion in the seventeenth century (Hacking 1975: 62). Bernoulli used the St. Petersburg paradox (1738) to refute the expected value concept and then to propose an alternative measure of value based on diminishing marginal returns.[22] This was the first formulation of expected utility, and the concept remained essentially unchanged until it was fully formalized by Von Neumann and Morgenstern (1944). By the 1950s, expected utility theory had gained dominance in economics, but questions about the descriptive accuracy of the theory's axioms and predictions led social psychologists to engage in a series of experiments to see if individuals did in fact behave according to the predictions of expected utility. By the late 1970s, there was growing evidence, primarily from experiments in the laboratory but also from empirical studies of consumer and investment behavior, regarding a number of systematic deviations from expected utility theory.

These were discrete, inductively generated findings generated by dissatisfaction with the descriptive accuracy of expected utility theory, with no apparent connection between those findings. What propelled the research program forward was a series of new conjectures, as economists and social psychologists proposed alternative theories of risky choice by relaxing one or more of axioms of expected utility theory. This lead to a variety of formulations of generalized utility theory (Machina 1982; Camerer 1992). One of the most influential of the alternative theories was prospect theory (Kahneman and Tversky 1979), which emphasized the importance of reference points, the asymmetry of gains and losses around a reference point, and nonlinear responses to probabilities.[23] Prospect theory has been applied in a number of disciplines and has attracted particular attention in international relations (Farnham 1994; McDermott 1998; Levy 2000). It began as a theoretical conjecture in response to a series of apparent experimental and empirical refutations of a prior conjecture about the nature of choice.

The results of ongoing experimental work are mixed. Most analysts agree that there are a number of robust descriptive violations of expected utility, but no single alternative conjecture has replaced it, leaving expected utility theory and prospect theory among a handful of leading contenders to a behavioral theory of choice (Camerer 1992: 239–42). It is important to note that a major reason for the persistence of expected utility theory, in addition to the limitations of competing theories, is its normative appeal as a theory of how people ought to maximize value, even among scholars who are convinced of the descriptive inadequacy of the theory.[24]

The Impact of Policy and Politics[25]

Few would deny that policy and politics often shape the development and persistence of scholarly research programs.[26] Scholars from a variety of metatheoretical orientations have argued that some of the leading research programs in the field are driven by current events and by the policy agendas of states and of individual scholars, that the study of international relations in different countries reflects and therefore varies with their country's distinctive historical circumstances and their government's different policy agendas, and that the relatively new field of international relations reflects a strong American thrust in both policy orientation and academic style (Hoffmann 1977; Krippendorf 1987; Ross 1991; Oren 1995; Waever 1998; Jervis 1998, 2003; Wendt 1999).

In terms of paradigmatic debates, for example, scholars have argued that the interwar period led to idealist and liberal approaches,[27] World War II to realism, the Vietnam War to critical orientations, and the uncertainty of the post–cold war period to multiple paradigms. In terms of substantive focus, the cold war gave rise to an emphasis on nuclear weapons, deterrence theory, and the East-West divide in general. The increase in civil wars and armed insurgencies after the end of the cold war led to a significant expansion of research on ethnonationalism, civil wars, genocide, humanitarian intervention, and, after the September-11 attacks, terrorism.

Similar arguments can be applied to the study of history, where interpretations of the past are often shaped by contemporary values and policy. Combs (1983) argued that changing interpretations of American foreign policy over time reflect ever-changing American foreign policy agendas. The idea that contemporary values, norms, issues, and agendas shape the interpretation of the past is reflected in Croce's famous statement that "all history is contemporary history" (cited in Carr 1964: 20–21), and in Kierkegaard's idea that "life is lived forward but written backwards" (cited in Jervis 2003:100).

While government policy agendas often shape academic research programs—through government or foundation support for academic research or through more diffuse mechanisms—the diversity of the academy reflects a wide range of values and policy agendas, and leading scholarly research programs may reflect agendas and values that are far from the dominant ones in state and society. U.S. government policy agendas shaped traditional histories of the origins of the cold war (Feis 1970), but at the same time competing policy preferences shaped revisionist interpretations of American foreign policy in the 1960s and 1970s (Williams 1972). The influence of countercultural values is also clear in postmodern and cultural history, values that examine the past from the perspective of the powerless and the voiceless and that are currently dominant (and not without power or voice) within many history departments.[28] Perhaps not coincidently, diplomatic and particularly military history, especially in the United States, have been marginalized (Lynn 1997, Black 2004).

Although it is undeniable that politics and policy affect the initiation and evolution of many research programs, it is important to recognize that some influential research programs in the field are driven primarily by autonomous theoretical or analytical developments or by evidentiary support, rather than by recent events or policy agendas. As argued in the last section, the bargaining model of war, rational choice theory in general, and behavioral decision theory have no obvious connection to world events or policy agendas.[29]

Conclusion

I have focused on the descriptive question of what influences the historical evolution of research programs rather than on the more normative questions of how research programs should develop and how they should be evaluated. I have distinguished between theoretical, empirical, and political criteria but conceded that the relationships among them are complex and sometimes difficult to disentangle. My argument is that social science research programs follow multiple trajectories, and that there is no single path for a research program's "success," defined in terms of the program's impact on and endurance in the field. Most rational choice models of international relations are more theory-driven than evidence-driven; though in some cases (Bueno de Mesquita 1981 comes to mind), the ability of some of these models to outperform their rivals in terms of degree of empirical support significantly enhances their influence. Research on the relationship between territory and war and between the dyadic balance of power and the outbreak of war has been primarily evidence-driven. While behavioral decision theory itself has in many respects been evidence-driven, if it is conceived more broadly as part of a broader research program on choice under conditions of risk that goes back to Pascal and Bernoulli, it is a classic case of Popper's model of an alternating sequence of conjectures and refutations.[30]

Policy agendas and normative concerns have had a much greater impact on the evolution of the democratic peace research program, but I question the common view that these factors have been the dominant force behind the scholarly popularity of the democratic peace. I argue instead that the unprecedented levels of empirical support for the dyadic democratic peace proposition, in a field notoriously lacking in law-like behavior, were the primary driving force behind the evolution of the research program, particularly in its early stages.

The question of the relative impact of policy agendas and values on academic research programs is both descriptively interesting and normatively complex.[31] The important question, from the perspective of a normative theory of science, is not *whether* normative and policy concerns influence research programs—since they inevitably do—but *how*. It makes a difference where in the research process normative and policy concerns have an impact. Popper (1965) distinguished between the logic of discovery and the logic of confirmation. The integrity of science is not undermined if values or policy concerns help shape the questions that scholars ask or even the initial theoretical conjectures constructed to explain them. Indeed, social science is a social enterprise as well as a scientific one, and social scientists should be social critics as well as social scientists.[32] As social critics, they should identify and explore important social questions, however out of fashion or contrary to governmental policy they may be.

It is a more serious threat to the integrity of scientific inquiry if values and policy concerns have a significant impact on how scholars define their concepts, translate their conjectures into rigorously formulated theories, construct research designs to test those theories, interpret the evidence, and decide—in the face of disconfirming evidence—whether or not to abandon the research program. This is not to say that the influence of these factors can be entirely eliminated from these stages of research, but rather that this influence and its negative consequences can be minimized if a scholar acknowledges her underlying normative assumptions and attempts to compensate for them in the construction of her research design.

We should also remember that the inseparability of facts and values is a reciprocal relationship. It means both that normative values infuse all empirical inquiry and that normative arguments have empirical components. Social scientists should be sensitive to the normative assumptions and implications of various theoretical arguments and of the research designs constructed to test them. At the same time, scholars should make a serious effort to identify the empirical components of normative arguments and to test those implied empirical propositions with rigorous social science methods (Snyder 2003).

Notes

1. As Lichbach notes in his concluding essay in this volume, Lakatos has declined in favor in the current literature in the philosophy of science. See also Blaug (1994: 109–11).
2. For the purposes of this study I define research programs broadly to include either 1) a body of scholarship that is built around a well-defined set of theoretical assumptions, which is inherent in Lakatos's (1970) conception, or 2) a body of scholarship that focuses on a well-defined substantive problem. Thus I classify the vast literature on the democratic peace as a research program for the purposes of this study, though Lakatosian criteria would lead us to exclude it because of the variety of theoretical explanations that have been advanced for the democratic peace and the different assumptions on which they are based.
3. Similarly, Jervis (1998: 972) states that "a research program succeeds when many scholars adopt it." The impact of a research program could also be measured in terms of the number of articles in prestigious journals and presses, readings on graduate syllabi, convention panels, doctoral dissertations, and hiring patterns.
4. This typology of theory-driven, evidence driven, and alternating sequence of theory and evidence mirrors Lakatos's (1970: 151–52) conception of three "typical variants" in the evolution of research programs: a "Popperian alternation of conjectures and refutations," a "period of relative autonomy of theoretical progress," and one in which all the empirical evidence is in place prior to

theoretical development. Lakatos suggests that "which pattern is actually realized depends only on historical accident" (p. 151). I thank Mark Lichbach for pointing out that my categories were similar to those of Lakatos.

5. Given the enormous differences in the study of international relations and international history across national boundaries (Smith 1985; Waever 1998; Levy 2001), the inclusion of non-American scholarship would be particularly valuable in isolating the role of politics and policy agendas, which vary across states in a way that theory and evidence presumably do not.
6. Falsifiability is a logical criterion that refers to whether the theory or hypothesis is constructed in such a way that there is a nonempty set of empirical observations that would lead researchers to conclude that the theory was incorrect, or at least that it needed to be rejected. Whether a theory is actually falsified is an empirical question, though one that involves some difficult issues in the philosophy of science. See the Lichbach chapter in this volume.
7. A theory is parsimonious if it explains as much as possible with as little theoretical apparatus as possible. A theory is not parsimonious in the abstract but only relative to other theories that purport to explain the same phenomenon. In this view parsimony relates to theories that one constructs to explain the world, not to beliefs about the simplicity of the world itself. King, Keohane, and Verba (1994) adopt this second definition of parsimony, and refer to the first as "maximizing leverage."
8. One of the problems with Lakatos's (1970) conception of research programs is the ambiguity surrounding the "unit of appraisal," or how broadly one should define research programs (DiCicco and Levy 2003). Should we focus, for example, on the rational choice paradigm as a whole; on a particular analytic framework within that paradigm, such as games of incomplete information or, more narrowly, signaling game models; or on applications of rational choice to a particular substantive area, such as bargaining?
9. This discussion builds on Levy (2003b). For a broader analysis of bargaining, one that includes nonrational factors, see Lebow (1996).
10. The key analytic developments were the treatment of games of incomplete information (about adversary preferences) as games of imperfect information (about prior moves in the game) (Harsanyi 1967–68), and the refinement of key equilibrium concepts that permitted the solution of these games. The key equilibrium concepts include perfect equilibrium (Selton 1975) and sequential equilibrium (Kreps and Wilson 1982). Rubenstein (1982) first applied perfect equilibrium to bargaining problems.
11. Within realism, for example, one can identify classical realism and structural realism, offensive realism and defensive realism (Walt 2002), balance of power realism and hegemonic realism (Levy 2002). This leads us back to the question of the appropriate "unit of appraisal," and the question of whether these broad paradigms are usually conceived as a single integrated research program.
12. This is somewhat ironic, because many of the scholarly contributions of King, and especially of Keohane and Verba in their individual work have been more theoretical than empirical.

13. Another example of a research program—or perhaps a paradigmatic approach—that is driven more by evidence than by theory is cognitive psychology. I thank Ned Lebow for suggesting this example.
14. I define the "democratic peace" research program broadly here to include not only research related to the dyadic-level proposition that democracies rarely if ever fight each other, but also monadic-level propositions about the relative war-proneness of democratic states. For an intellectual history of the research program see Ray (1995).
15. On the early skepticism of these scholars toward the democratic peace see Ray (1995: 44).
16. On the progressive nature of the democratic peace research program, as judged by several alternative metatheoretical criteria, see Chernoff (2005).
17. The problem is compounded by confusion over levels of analysis. Balance of power theory and power transition theories are system level, while the power parity and power preponderance hypotheses are dyadic level.
18. For an early review see Siverson and Sullivan (1983).
19. The dominant role of theory in the debate between balance of power theory and power transition theory derives in part from a certain amount of incomensurability between the two. Most balance of power theories focus on land-based military power and are applied to continental systems, especially Europe, while most hegemonic theories, including power transition theory, emphasize economic foundations of power and are applied to global maritime systems (Levy 2003a).
20. On the limitations of physics as a model for the social sciences, and for the possible relevance of other natural sciences, including biology, see Bernstein et al. (2000, and in this volume). On the relevance of other disciplines for the study of history, see Gaddis (2002).
21. Some of the key studies in behavioral decision theory can be found in Edwards and Tversky (1967), Kahneman and Tversky (2000), Kagel and Roth (1995), and Camerer, Loewenstein, and Rabin (2004).
22. This was a theoretical refutation based on the identification of a theoretical anomaly in the expected value concept.
23. Prospect theory itself has been significantly revised (Tversky and Kahneman 1992) in response to an important theoretical (as opposed to empirical) problem relating to the mathematical intractability of the original formulation of the probability weighting function.
24. The normative appeal of expected utility theory implies no distinctive substantive commitment, other than the maximization of individual value, independently of how the individual defines value. Rational choice theory includes "analytical Marxists" such as Przeworski as well as free market economists. Prospect theory, on the other hand, makes no normative claims, and Kahneman and Tversky (1979) and others argue that it is impossible to reconcile normative and descriptive theories of choice.
25. This section builds on Levy (2003b).

26. "Policy and politics" is a very broad category, and includes the impact of current events, the policy agendas of the government and of political oppositions or other groups. It might also include the professional or financial self-interest of individual scholars or research teams, but I exclude this latter consideration from this discussion.
27. Carr (1939) argued that the ascendance of idealist international theory, with its vision of a natural harmony of interests in the world, was basically a rationalization for British and American dominance in a liberal world order.
28. For an argument on why the study of the "voiceless" lends itself to a postmodern orientation, see Haber, Kennedy, and Krasner (1997): 38–40.
29. The rational choice paradigm does not specify actors values or preferences, which are exogenous. It specifies how actors should behave, and perhaps how they do behave, given their values, their beliefs, and the structure of their structural and informational environments. Moreover, the actors themselves are unspecified. They can be individuals, organizations, classes, states, empires, intergovernmental organizations, or any group whose preferences satisfy the axioms of expected utility theory, or perhaps even less demanding criteria in "softer" versions of rational choice. Particular rational choice theories (the signaling model of economic interdependence and peace, for example) specify actors, preferences, and other parameters.
30. This suggests that the temporal boundaries we ascribe to a research program may affect how we classify it. Behavioral decision research from the 1950s to the late 1970s was primarily empirically driven, but if we focus on decision theory more broadly to include the antecedents of behavioral decision research and the theories it generated, it fits nicely into a model of alternating conjectures and refutations.
31. For a discussion of the tension between prescriptive and descriptive theories of research programs, between the methodology of science and the history of science, see Blaug (1994).
32. This does not imply that all researchers need to devote equal time and energy to social criticism and scientific analysis, only that both tasks are appropriate ones.

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8

Imperial Peace or Imperial Method? Skeptical Inquiries into Ambiguous Evidence for the "Democratic Peace"

Andrew Lawrence

Rather than comprehensively review the immense Democratic Peace (DP) literature, this chapter seeks to link the weaknesses of the theory's dominant epistemological foundations to broader tendencies in social science research.[1] It contends that a positivist correlation of static definitions of democracy, statehood, and war cannot explain transformations of these phenomena over time. Moreover, DP theory naturalizes a landscape of power relations and interests that deserve critical scrutiny. DP theory's most familiar and putatively positivist formulation argues that objective measurements of interaction among the variables "democracy," "state," and "war" clearly confirm the proposition that "democratic states do not go to war with one another." A fetish with numbers coupled with an uncritical acceptance of the "commonsense" meaning of these variables, however, can obscure the extent to which each variable has transformed the others in modern history. This seemingly positivist formulation then becomes tautological, weakening comparison from one period to another. The DP debate in particular is representative of research that is naive of U.S. power relations and is thus potentially complicit with apologists of American abuses of power, a theme discussed in greater detail in the chapter's conclusion.

Viewed as a whole, the DP debate suffers the consequences of a divorce of method from theory.[2] Remaining within the confines of an overly delimited research program, DP scholars have relied upon a "sophistication of techniques of observation and proof" that can, if unaccompanied by "a redoubling of theoretical vigilance, lead us to see better and better fewer and fewer things" (Bourdieu et al. 1991: 88). This essay suggests that enhanced theory building cannot be divorced from normative considerations in examining the relationships between democracy and peace. Discarding false assurances about the ontology of democracy, states, and peace complicates yet expands investigations of their interrelatedness.

The tension between theory and method is evident in Doyle's claims about a Kantian lineage for the DP debate (Doyle [1983a and b] 1996) and in his conclusion (in the core volume of essays on this debate) that the debate provides, in the language of positivist philosopher of science Imre Lakatos, "remarkable evidence for the progressive development of the liberal research program" (Doyle 1996: 58). The first section of this chapter interrogates the Kantian lineage through an examination of Kant's essay, because this heritage has been mined selectively. The second section challenges Doyle's conclusion by arguing instead that, from such a deductivist perspective, the debate as it now stands is more exemplary of Lakatosian degeneration: its trajectory is the opposite of *theory building*—in Lakatos's terms—since newer interventions restrict, rather than add to, the existing data set and lead to no "novel facts" or corroborated "excess empirical content" (Lakatos and Musgrave 1970: 116–20). The third section of the chapter introduces an alternative hypothesis for the absence of war that deserves to be considered and examines how different methods and bodies of evidence can reinterpret between democracy and peace. This is important because so little effort has been expended in examining alternative explanations.[3] Further research areas are suggested in the chapter's conclusion.

I. Democratic Peace v. Kantian Peace

Within the terms of the DP debate, the methods used in studies concluding that democracies do not fight each other can be separated from this conclusion; other statistical and empirical analyses draw conclusions skeptical of, or contradictory to, the DP thesis. As discussed in section II, statistical methods can show that democracies rarely fight, but also that they occasionally do fight, or that their bellicose propensity is not unusual among regime types. These ancillary studies, however, evade the central problematic of such approaches. The terms "democracy," "state," and "war" are (like all key social science terms) fundamentally socially contestable and their ontology is mired in dissensus and historically varying meaning. Analogous to the Heisenberg principle of uncertainty, any apparent clarity gained in the definition of one term comes at the inevitable expense of arbitrarily narrowing, distorting, or ignoring the plausible range of meanings of the other terms. The matter of defining democracy and peace, therefore, is not persuasively accomplished in one or two throat-clearing footnotes.

Although drawing inspiration from Kant, proponents of DP theory have transformed Kant's means-ends relation, where practice must strive after the highest ideals of theory (most famously articulated in Kant's 1793 essay, "On the Common Saying, 'While This may be True in Theory, It Does not Apply in Practice' "). By contrast, many DP theorists have trimmed the theoretical ideal to fit actual historical practice. Kant's 1795 essay "On Perpetual Peace: A Philosophical Sketch" (in Reiss 1991) is thus quite alien in emphasis and tone from Doyle's version of this thesis, arguing that (at best) only very stringent and specific practices will bear out the theory of the democratic peace; its closest contemporary counterparts are those theorists that are most skeptical of DP theory. Even (indeed, especially) in the context of low levels of democratic participation of his day, Kant was highly skeptical, if not pessimistic, about the chances of a sustainable peace among democratic states.[4] While DP proponents continue to claim Kant's foundational influence (*cf.* Russett and Oneal 2001: ch. 2, "From Democratic Peace to Kantian Peace"), not only Kant's true theoretical foundation, but more important, his finely honed skepticism, arguably remain vital means of evaluating a given polity's shortcomings during the past two centuries of actual democratic practice.

The vast majority of the democratic peace literature, however, following Doyle's original emphasis (see Doyle 1983a and 1983b), confines itself to the first two of Kant's three Definite Articles elaborated in "Perpetual Peace": II.i., "The Civil Constitution of Every State Shall be Republican" and "II.ii. The Right of Nations Shall be Based on a Federation of Free States." But a limitation by democratic peace theorists to these two articles alone amounts to Wilsonian practice masquerading as Kantian principle. Kant did not understand his variables as separable, in theory or practice, but, on the contrary, as mutually conditioning. Kantian democratic practice, for example, is precisely measurable to the extent that a polity avoids war: the "monadic" variant of DP theory (see discussion below).[5] Similarly, DP theory proponents have overlooked Kant's preliminary, "prohibitive" articles, including Article I.iii—"Standing Armies Will Be Gradually Abolished Altogether" (but see Adler and Barnett 1998; Deutsch 1953; on the financing of war, see Schultz and Weingast 2003); Articles I.ii and I.iv on the relation between indebtedness and war; and also Kant's third Definitive Article: II.iii, "Cosmopolitan Right Shall be Limited to Conditions of Universal Hospitality."[6]

Within this larger constellation of requisite norms, it is clear that democracy is but one element in the constitution of peace, and not necessarily the most important for Kant. His emphasis upon universal hospitality as the most important of human traits continues a lineage of political thought more ancient than democracy's (see e.g. Leviticus 19:33–34). The other Kantian elements take us outside of democracy per se. While Kant may have despaired of ever personally witnessing the transformation of cosmopolitan right into "a universal right of humanity"—he chooses the grave as the most appropriate emblem of "perpetual peace"—his despair does not entirely temper his sense of the necessity of this ideal (Kant, in Reiss 1991: 105–8).

Certainly, the growth of global media in the twentieth century and progress toward writing the unwritten code of political and international right in the past half century have at least partly vindicated Kant's assessment for the longer term. Yet he cautions that "only under this condition" of universal cosmopolitan right "can we flatter ourselves that we are continually advancing towards a perpetual peace" (ibid.: 107–8). The question remains how we are to find and assess evidence that this condition is being satisfied, and that this advancement is in fact progressing, without adopting Kant's recourse to observations of "nature."[7]

II. Problems of Definition, Measurement, and Testing

In this section, I identify three failings of the mainstream DP project. First, there is insufficient interrogation of conventional definitions of the categories of state, democracy, and war, and thus little explicit consideration of alternative definitions. Not only are these received definitions arbitrary, but they do not allow for substantive change over time. Second, even within the boundaries of conventional definitions, statistical demonstrations of DP theory are ambiguous and inconclusive. Faced with such results, DP researchers have resorted to an arbitrary coding of cases. Finally, given these weaknesses in category definition and case coding, DP researchers should explicitly test alternative hypotheses, such as the Imperial Peace theory discussed in the conclusion, as a means of refining and potentially strengthening their own. Their failure to do so is particularly indicative of Lakatosian degeneration.

Significantly, the only paradigm foils with whom DP proponents have extensively engaged are realists. It is no coincidence that (as Levy observes in this volume) initially skeptical realists such as Bremer, Maoz, and Bueno de Mesquita who have become strong DP theory proponents are also partisans of the statistical demonstration of the theory. Oren (see discussion below) is a prominent counterexample. His movement away from

DP theory not only did not lead him to embrace realism; his methods, presuppositions, and evidence were also qualitatively different.

The original, statistically based terms in which DP theory was cast led both DP proponents and opponents to depend uncritically upon quantifiable definitions of the key variables of democracy and war. Employing a Cartesian separation of subject and object, DP theorists define democracy in Dahlian procedural terms of competitive, free, and regular elections and borrow their definition of war from the earlier Correlates of War (COW) data bases as the threshold of at least 1,000 combat deaths (*cf.* Small and Singer 1982; Hagan 1994: 185); the absence of such war ipso facto constitutes "peace".

The strong "monadic" claim that democracies are inherently more pacific than other regime types was shown to be statistically dubious early on in the literature, while the Vietnam War was at its height (Singer and Small 1972).[8] Subsequent quantitative studies (e.g., Maoz and Abdolali 1989), responding in part to the challenge the Vietnam War posed, have compared warring dyads since the Congress of Vienna. This choice of cases, and the temporal restrictions accompanying them, however, is indefensibly arbitrary. The true large-N sampling is *all* state dyads at *all* times. Within *this* sample, wars are rare (even if geographical contiguity is taken into account) and democracies even rarer; as one proponent of the DP thesis (Ray 1995: 204) allows, "the rate of war involvements for pairs of states in general is so low that the absolute difference between the number of wars involving states in general and those involving democratic states will be very small even if the latter number is zero." Any exception above zero "would reduce that difference to insignificance" making it "impossible to deny that the number of wars eliminated by the pacifying impact of democracy has been very small" at best (see also Solingen 1998: 113). There is, further, little effort to distinguish empires from other states in the modern era, a problem implicating definitions of democracy and peace (discussed further in the conclusion). From even a (sufficiently skeptical) positivist perspective, then, there is insufficient basis for affirming DP theory.[9]

But even granting DP such debatable definitions of democracy and peace and allowing for probabilistic standards in correlating them, the distinctiveness of the dyadic finding remains difficult to demonstrate conclusively. Etel Solingen (1998), for example, shows that equally quantifiable economic liberalization is a better predictor of regional cooperation, and thus she argues that it is a better explanation for regional peace. Joanne Gowa (1999: 4) finds that the only period in which militarized disputes between democratic states were relatively rare was the Cold War period. Between 1816 and 1914, by contrast, "members of pairs of democratic states are no less likely to engage each other in war or in other militarized disputes than are their nondemocratic counterparts." This temporal bounding weakens the transhistorical claims of the theory.

Since the DP hypothesis seems to hold best (or only) during the Cold War, it would be better termed "Democratic Peace during the Cold War Era" theory. In fact, democratic polities—even during the Cold War—are more varied than DP tests usually allow. Auerswald (1999: 498), for example, contends that some types of democracies are consistently more prone to go to war than others, a pattern that can be understood along the following most-to-least-likely regime continuum: strong presidential, weak presidential, majority parliamentary, coalition parliamentary. DP theory is thus obliged to go beyond taking "democracy" at face value, and investigate variations within its rubric.

Similarly, the definition of war is in need of further articulation. While "militarized interstate disputes" (MIDs) are not the same thing as 1,000+ combat deaths (the COW definition), it is not clear why one standard is a priori preferable over another. Paul Huth and Todd Allee (2002: 20–6) have identified several additional and interrelated weaknesses with the dyad-year approach. A simple coding that records a conflict between a given dyad in a given year cannot describe its specific pattern of military initiation, response, resolution, or escalation or identify which country initiated military aggression. It cannot account for various stages in a given international dispute, much less explain why they arise. Finally, when a dispute is recorded as occurring in a given year, important differences regarding the length of a dispute (from a few days to several years) are obfuscated, and the discrete character of these events is obscured.

There are two possible responses to this impasse among those who seek to demonstrate the perpetual peace theory statistically. They may explain anomalies first as insignificant or even (seen from the proper angle, with sufficient qualitative context) as only apparent and not "true" anomalies.[10] Yet as an already small N of true democracies diminishes in size, statistical methods' efficacy correspondingly diminishes.[11]

The second response to the large-N problem, increasingly adopted in a more mature phase of this debate, is to examine more closely "borderline cases"—(quasi-) democracies that go to war or threaten to do so.[12] Closer inspection can reveal weaknesses of democratic practice in one or more parties that diminish their criteria as democratic. Indeed, some of the most ardent adherents of the DP thesis concede that, in important respects, the content of democracy has been a moving target—without, however, drawing the relevant conclusion that this concession undermines the basis of their statistical "proofs."

Instead, such theorists employ generous fudge factors by redefining their key variables. For example, Maoz and Abdolali (1989) conclude that the changing nature of democracy, with the enfranchisement of women and decolonization after the world wars, has increased the likelihood of a democratic peace. However, Maoz (1996: chapter 2 and Appendix) later distances himself from the statistical results of this earlier study due to "a modified coding that allows for the changing nature of democracy over time" (Maoz 1997: 166, fn. 10).[13] Without elaboration, such justificatory language is symptomatic of researchers' violations of Popperian standards of "falsifiability" or of even more liberal Lakatosian standards for "novel facts." If these works used changing definitions of democracy to refine or challenge those of statehood or peace, for example, they could add to the research program's progressive research content. Without doing so, there remains a growing attenuation between DP theory's monadic and dyadic versions. A recent statistical reexamination of Bremer's (1992) testing of the dyadic thesis concludes, for example, that while his results confirm the relevance of such factors as geographic contiguity, his finding of a monadic democratic peace using the Chan freedom variable rather than Polity's democracy indicator is "not robust to various operationalizations of democracy. His choice of democracy indicator thus seems to have had a significant influence on his conclusions, demonstrating that data selection can be as crucial as the choice of statistical model" (Buhaug 2005: 95).

While it is eminently plausible to observe that democracies rarely go to war with each other, the argument that among regime types democracies (monadically or dyadically) correlate *uniquely* with peace or even to an *unusual* extent is clearly disproved. The only point of consensus in the literature, regardless of period, is that democracy is *not* a necessary condition for peace. Some (such as Solingen) argue as well that it is not sufficient. Others point to other regime types whose dyads correlate with peace as well as democracies or better: oligarchies (Weart), developed socialist states (Oren and Hayes, 1997), or internationalist trade coalitions (Solingen).

Furthermore, even if there were complete consensus about the validity of the "interdemocratic peace" proposition, no equivalent consensus yet exists about the causal mechanisms explaining this phenomenon. Such explanations require measurements of both democratic norms as well as institutions, in addition to war. The meaningful measurement of democratic institutions is difficult enough, but "there is no easy way to quantify the slippery variable of 'democratic norms' and no widely accepted database for this variable" (Bennett and George, 2005: 43).

This is but one example of the larger problem with data sets of quantified or dichotomized variables: they can "achieve reproducible results across many cases (external validity) but only at the cost of losing some of the ability to devise measures that faithfully represent the variables that they are designed to capture (internal validity)" (ibid.: 44). Thus, statistically driven studies defending the DP hypothesis—including those that build new data sets from scratch (as do Huth and Allee 2002)—need to squarely address the central normative issue: what is the value of representing democratic norms or the transhistorical ontology of war in statistical or commensurate terms? Is the appeal in doing so central to the appeal of the core DP thesis? Why *should* the essence of democracy be homogenized across space and time?

For some DP proponents, war apparently exists only in the eyes of the beholding electorate. For example, Russett (1993: 12) initially defines wars as acts of "large-scale institutionally organized lethal violence" of 1,000 or more combat deaths.[14] Yet he later claims that multiple instances of U.S. organized covert "action" were "not wars, openly fought by military units of the United States. They were low-cost operations designed to minimize public attention."[15] Although "the United States maintained and supplied the contras in a nine-year civil war that could never have been sustained—nor probably even begun—without deep U.S. involvement" including the mining of "Nicaraguan ports—a formal act of war by the standards of international law," such activities "were covert, and American participation could be denied, with varying degrees of plausibility" (p. 123).

However, if the war was both initiated and sustained by the United States and entailed several thousand combat deaths (including those of soldiers on a U.S. payroll), then it should amply satisfy the criterion of "large-scale institutionally organized lethal violence." This view would be strengthened by the open support that Ronald Reagan (the then-U.S. president) gave for the Contra war, extolling the Contras as "freedom fighters" and taking pride in his formal acts of war, that in no way diminishes the extent to which the war was "openly fought." That those fighting a U.S.-planned and sustained war on the U.S. side were not for the most part U.S. citizens seems a nice distinction from a purely statistical perspective, one that weakens the parsimonious elegance of the original theory.

From the normative perspective of Kant's Definite Article II.i, shared by qualitative-minded DP theorists, this distinction is, of course, highly relevant. But in order to avoid complicity with those realists and security studies scholars who condone such covert action in the name of democracy, DP theorists need to focus on the inadequacies and failures of democratic practice that allowed or enabled these acts of aggression. The "variable slippage" entailed in questions of coding is by no means a recent phenomenon. Jawaharlal Nehru ([1934], 1942: 941) observed in 1932 that the then-recent Chaco War between Bolivia and Paraguay and the Leticia War between Colombia and Peru were

> not officially called wars. Ever since the League of Nations covenant and the Kellogg Peace Pact and other pacts, wars hardly occur. When one nation invades another and kills its citizens, this is called a "conflict," and as a conflict

> is not prohibited by the pacts, everybody is happy! These little wars have no world importance, [. . .] but they serve to prove how weak and futile the whole much vaunted peace machinery of the world is, from the League of Nations to the numerous pacts and agreements.

Such "variable slippage" might indicate violations of independent testability; yet the mechanisms for independent testing that King, Keohane, and Verba (1994) suggest as an antidote to variable slippage do not address the possibility that this act of categorical and methodological rationalization is part of a larger pattern.

On the one hand, the boundary between conflict and war has arguably been progressively blurred during the latter half of the twentieth century, especially with the end of the Cold War, fundamentally challenging received notions of war's definition and boundaries (Kaldor 1999). On the other, as Nehru implies, war has ideological connotations denoting geostrategic importance to which many ardent democratic peace proponents are beholden. Russett and others who make distinctions between "appearance of war" and "quantifiable war," argue implicitly in favor of a relaxation of positivist standards of evidence gathering, while maintaining the overall framework of a statistically based argument. "Variable slippage" in this context is symptomatic as well of the more pervasive phenomenon of self-rationalizing power. As Nehru's observation suggests, this phenomenon is not unique to the Cold War period. What has changed since the Cold War is in fact the manner in which the relation of democracy to hegemony is posed.

Part of the DP puzzle lies in the dominant *Erklären* tradition's overly rigid insistence upon varieties of causal inference over the *Verstehen* (or descriptive) tradition of inference. This insistence serves to narrow, rather than permit, this type of effective communication among members of a research community, and indeed, to veto some methodological preferences entirely.

Just as the IMF gravely prescribes rules that no self-respecting great power has ever felt compelled to obey, Russett's perspective typifies the DP literature in another more fundamental respect: it rationalizes the power prerogatives of sitting governments, above all, that of the United States. He codes democracies as those states not engaging in war, and wars as something that only nondemocracies—or at most only one half of a warring dyad—engage in. He thus argues that the U.S. covert actions in Nicaragua and elsewhere were waged against governments "not fully democratic according to the criteria that have been applied here for late twentieth-century regimes; rather, all were anocracies" (p. 121–2).

Given the strong divergence in democratic practice Russett assumes between the United States and these "anocracies," it seems fair to ask whether the United States—with consistently one of the lowest electoral participation rates of any OECD country (with socio-economic class serving the best predictor of participation in both elections and elective office; *cf*. Piven and Cloward, 2000), the highest rate of incarceration, a death penalty (almost alone among OECD countries, and at a rate behind only China, Iran, and Vietnam worldwide), severely delimited and often nonexistent rights of workers to organize, and among the lowest proportions of women among elective officials—remains the world's stellar exemplar of democracy, or whether such glaringly anocratic elements do not in fact preclude bragging rights or, for that matter, unilateral power in drawing a line between democracies and anocracies.

As with domestic practice, so with multinational commitments: the United States lags far behind its putative fellow democracies in the ratification and enforcement of human rights treaties, such as the Ottawa Land Mine Accord, the Torture and Genocide Conventions, and the American Convention of Human Rights. Where it has engaged multilaterally—as with the Friendship, Commerce, and Navigation treaties and increasingly, with bilateral investment treaties—its co-signatories have more often been non-liberal states (see Alvarez 2001: 194–8; Koskenniemi 2002: concl.)

Certainly, the subject position Russett and others adopt would support Oren's (1996: 294) contentions that DP theory is "not about democracies per se; it is better understood as a claim about peace among countries conforming to a subjective ideal that is cast, not surprisingly, in America's self-image"; and that democracy "is not a determinant as much as a *product* of America's foreign relations. The reason we appear not to fight 'our kind' is not that objective likeness substantially affects war propensity, but rather that we subtly redefine 'our kind.' "[16]

Oren's conclusion—that "it is only from the perspective of a secure and overwhelmingly victorious country that [this] time-bound illusion can so easily be taken for a universal truth" (ibid.: 295)—raises two issues, however. The first concerns historical context: whether the "democratic peace" can be rescued from the status of "time-bound" illusion and placed on a firmer epistemological foundation. The second concerns an individual's subject position: whether its significance—from the position of a member of a "victorious" country, a defeated country, or one outside a national framework altogether—may be substantially diminished in the context of a different research program. If U.S. hegemony didn't exist, then (how) would DP theory have to be re-invented?

If "democracy"—however defined—is a social kind, then it should be understood (following Roy Bhaskar 1979: 48–49) as having spatial and temporal boundaries and a foundation of mutually constitutive (and discursively based) ideas, beliefs, and practices. While not all forms of participation are democratic, all forms of democratic practice surely involve participation. The question of dyadic mutual recognition is always assumed to refer only to (elected) political elites, an assumption ancillary to the prior (and increasingly contested and problematic) assumption of prevailing state sovereignty. This precludes the possibility that part of democratic participation entails the mutual recognition of democratic participation. It raises the question of whether participants of democratic practices, in their fullest array worldwide, can mutually recognize a common dimension among such practices and, in so doing, promote lasting peace. Or, less ambitiously, whether a global democratic peace requires only a minimal consensus (in the form of treaties and multilateral agreements) on those practices that are antagonistic to such peace.

An understanding of democracy as a social kind poses particular conundrums for DP theory. The "members of the research community" include important practitioners, as well, who have a practical as well as ideological interest in either demonstrating or refuting the correlation between democracy and peace—often to the detriment of both. To be sure, several critics of the "democratic peace" theory are equally guilty of using reductive and ahistorical definitions of variables such as democracy and peace.[17] And practitioners, even when critical of current U.S. policy such as the Iraq War and occupation, remain unreflexively optimistic about exporting democracy.[18] In the absence of reflexive phronesis, DP (and all other) theorists face the danger of becoming part of the phenomenon they seek to explain (*cf.* Lebow and Stein 1990).

This only underscores the importance of placing the DP debate in a larger historical context. First, we can note a marked correspondence between current events and the publications curve this debate has taken since Dean Babst (1964) published what is widely (albeit retrospectively) recognized as the pioneer DP article. On the eve of the U.S. escalation of the Vietnam War, scholarship pointing to a positive correlation between elective governments and peace was likely to fall on deaf ears; the emphasis Huntington (1968) laid on stable one-party regimes, bureaucratic union structures, and, above all, armies in the Third World proved more popular among political scientists throughout the 1960s and early 1970s, until discredited by defeat of this policy and of the United States in 1975.

Consequently, only a small handful of publications addressed this broad topic until R.J. Rummel's multivolume study of the causes of war began to appear in the late 1970s (Rummel 1975–1981); even then, the emphasis was on the correlates of war rather than the relationship between democracy and peace. It was only after the onset of the "second Cold War" that the DP literature garnered widespread notice, reaching a fever pitch after the fall of the Berlin War. The early 1990s' "Third Wave of Democracy" momentarily appeared to vindicate the DP hypothesis, and the flurry of publication grew further apace. Rather than developing an autonomous dynamism of its own, the DP debates were driven as much or more by world events as by empirical merit or internal logic.

Ironically, although most of these statistical studies strive after Popperian norms of evidence and verification, Karl Popper himself had already articulated a critique of the literature's prevailing assumptions, on three grounds. He first makes the moral importance of the connection between democracy and peace explicit in his contention that humanity escapes the necessity of social Darwinism when it achieves the possibility of "being *critical of its own tentative trials, of its own theories*" through "the evolution of a descriptive and argumentative language." Second, he advocates not the affirmation of theory, but its critique—on grounds, moreover, of descriptive, rather than causal inference.

Finally, and most radically, he maintained that "If the method of rational critical discussion should establish itself, then this should make the use of violence obsolete: *critical reason is the only alternative to violence so far discovered*" (Popper 1970: 292; emphasis in original). If democracies are to be the best vehicles of critical reason, in other words, then it is the actual, lived experience of reason that continually confirms the "democratic peace" by forestalling or subverting violence *at all levels of society*; where violence exists, critical reason (as well as the institutions in which it is formed and finds expression) is lacking or faulty. Form, in other words, should follow content, in politics as in poetry. The democratic peace literature makes a fetish of proving or disproving whether democratic states promote war or peace. From a normative perspective, the point should be rather to discover whether, and in what ways, the world's institutions—individually and taken as a whole—contribute to or undermine the substitution of critical reason for violence (understood broadly to include not just combat casualties of 1,000+ but also inter alia civilian deaths, ethnic cleansing, rape, forced relocation, protracted industrial disputes, and immiseration). Such a normative enrichment of peace requires that the received, independent variable definitions of "democracy" and "state" be transformed.

By contrast, the U.S. government has neglected reflective knowledge connecting domestic and international politics.[19] It has largely ignored the imperative, emphasized by John MacMillan in his eloquent rearticulation of the DP hypothesis, to subordinate "the spread of liberal democratic domestic political systems abroad" to an abiding awareness that "liberalism as an evolving ethical tradition requires critical reflection upon the implications of liberal principles and values for the foreign policies of existing liberal states themselves" (MacMillan 1998: ix; see also Alvarez 2001).

III. An Imperial-Democratic Peace?

The current U.S. doctrine of "perpetual war" against terrorism seems unwittingly to evoke the naive grandeur of Wilson's "war to end all wars" and suggests a jarring discontinuity from the previous decade's celebration of a dawning era of "perpetual peace." This transformation coincides, however, with Democratic Peace (DP) theory's elevation from scholarly controversy to guiding rationale of U.S. foreign policy during the Clinton presidency. President George W. Bush (Bush 2004) succinctly expressed a continuation of this rationale in his 2004 State of the Union address:

> America is a nation with a mission, and that mission comes from our most basic beliefs. We have no desire to dominate, no ambitions of empire. Our aim is a democratic peace—a peace founded upon the dignity and rights of every man and woman. America acts in this cause with friends and allies at our side, yet we understand our special calling: This great republic will lead the cause of freedom.

Doubtless most DP theorists neither recognize nor accept their theoretical framework in a policy that relies on war to achieve peace, employs shifting criteria and inscrutable evidence in its argument for war, and subordinates multilateral practice to military objectives—exemplifying what Charles Tilly (1985: 170–171) has called state-sponsored racketeering.[20] Yet none of these elements of administrative fiat is novel; arguably, they represent the presidential norm over the past century, rather than the exception.[21] In this context, a flurry of publications now addresses the question of the relationship between U.S. democracy and imperialism. The twentieth century appears to have ended as it began: the relationship between democracy and imperialism has been analyzed in the past decade to a greater extent than since World War I.

In the work that signaled the crisis of the old DP debate, Barkawi and Laffey (1999: 404; 2001) argue that the debate's emphasis upon the sovereign territorial state as a unit of analysis fundamentally obscures the transnational "mutually constitutive relations between so-called zones of war and peace" whose division is "internal to the processes of global social change." They follow Shaw (2000) in identifying an emerging transnational, or "global," state that is successfully promulgating and at the same time redefining democratic norms across the world. From this perspective, peace is not inherent to democratic states, but rather, zones of peace and democracy share common causes. Democracies tend not to go to war with each other

> not because they are inherently peaceful in their relations or because of the nature of their domestic political systems or the spread of liberal norms. The use of force between these states is unlikely because they are embedded in geostrategic and political economic relations that buttress international and capitalist power in hegemonic, i.e. non-violent ways. Beginning with a set of liberal democratic *states* rather than an emergent Western or transnational *state* means that the democratic peace debates remain caught in a territorial trap. (Barkawi and Laffy 1999: 419; emphasis in original)

The "imperial peace" hypothesis, thus conceived, understands the variables of democracy and war to be historically variable, it can account for differences across periods while retaining explanatory power for the entire modern era; it explains why regime stability and aggression co-vary, as well as why democracies are particularly bellicose toward nondemocracies; and it theorizes the phenomenon that both "democratic peace" interlocutors at best only acknowledge, namely the reasons for the particular affinity between the United States and the global democratizing project. Indeed, the findings in both the skeptical-realist and affirming-liberal literature cited above appear largely congruent with this theory.

If so, we might inquire why it is that prior to America's latest military engagements, the "imperial peace" theory merited such scant attention. The absence of any sustained theorizing is particularly strange in the case of Michael Doyle, a major early proponent of DP theory (Doyle 1983a, 1983b, 1996). He rightly concludes at one point that the fate of "the debate will turn on the alternatives. Liberal theory should not be compared to the statistical residual or to a richly described case study, but to the comparative validity of other theories of similar scope" (in Brown et al. 1996: 362). But he finds Marxism "confusing" in its focus either on "the mode of production (ownership)" or "the mode of exchange (market)" (ibid.: 363), without considering empire as a relation of production in which the asymmetries of a metropole's corporate power are extended through the forcible integration of the periphery into its markets. He earlier cites Raymond Aron's identification of empire as one of three types of interstate peace, which "generally succeeds in creating an internal peace," but concludes that "this is not an explanation of peace among independent liberal states" (1996; [1983]: 19) without considering that internal peace could characterize trade blocs whose members include semi-sovereign "illiberal democracies" in the imperial periphery (Zakaria 1997).

It is all the more surprising that Doyle in particular should *not* consider empire part of "an explanation of peace among independent liberal states" since he concludes a long and careful study of that question—titled *Empires*, no less—by observing that a process of integration of the periphery "plays a necessary and active role in determining the outcome of empire. . . . so the newly independent United States once struggled out of Britain's imperial grip and set itself on a course of continental, and some say, global empire" (1986: 372). Why mention the imperial hypothesis if it has no merit? If it does, why not engage with it more deeply?

Although Shaw (2000) emphasizes how "unfinished" the process of cosmopolitan democratization is, he remains confident of the global state's existence. This formulation, however, begs the question of the nature and extent of United States commitments in this process, which in turn depend on the extent and quality of reflexive learning within the U.S. and especially by its political actors. In the face of ambiguity on this point, this chapter argues that redoubled focus upon process is the most fruitful approach—in researching democracies, and in engaging democratically. This applies as well to researching the DP debate. It may be that despite the fact that the "imperial peace" hypothesis appears to satisfy the "additional and true" Lakatosian criteria, it requires further "testing" and elaboration to become more accepted. If so, this should be a matter of skeptics, not of proponents of the theory. Or it may be that mainstream democratic peace proponents and skeptics have discovered the "recipe" Feyerabend decries (in Lakatos and Musgrave 1970: 198) for upgrading social science into a "science": "The recipe, according to these people, is to restrict criticism, to reduce the number of comprehensive theories to one, and to create a normal science that has this one theory as its paradigm. Students must be prevented from speculating along different lines and the more restless colleagues must be made to conform and 'to do serious work.' "[22]

Yet academic hierarchies do not fully explain the phenomenon at hand. While liberal theorists predominated over their realist, structuralist, Marxist, and interpretivist counterparts in the 1990s, the issue goes deeper than that. The recent outpouring of works analyzing the United States as empire could be seen as mitigating this theoretical deficit, or even heralding a new paradigm shift. As an intellectual-historical phenomenon, however, it raises more questions than it answers. Empires are not to be confused, in their growth and articulation, with mere changes in leadership or with engagement in war. If the United States can usefully be described as imperial, this characterization must extend back before the current (or indeed before the previous) Bush presidency. This was the era of "globalization," whose discourse temporarily seemed to supplant an older one of imperialism, with an emphasis upon increasingly integrated markets and interdependent economic and political actors.

Neither, however, was this imperial paradigm (regardless of variant) entirely satisfactory; arguably, for similar reasons. As Prabhat Patnaik observed in the early 1990s, whereas in the 1970s "Marxists everywhere looked to the United States for literature on imperialism," which was more widely discussed there than anywhere else, after the fall of the Berlin Wall, "the topic has virtually disappeared from Marxist journals." Furthermore, this was

> not because anyone has theorized against the concept. The silence over imperialism is not the aftermath of some intense debate where the scales tilted decisively in favor of one side; it is not a theoretically self-conscious silence. Nor can it be held that the world has so changed in the last decade and a half that to talk of imperialism has become an obvious anachronism.

On the contrary, he argues, "viewed as a fundamental set of economic relations characterizing the world," imperialism "is stronger today than ever before, at least in the postwar period," with the G-77 in shambles, commodity prices plunging to new lows, and standards of living and life chances decreasing absolutely in many cases. The reason for this silence, Patnaik suggests, "lies precisely in the very strengthening and consolidation of imperialism" that has learned that "half a million troops do not have to be dispatched everywhere; and unless there are half a million troops dispatched somewhere, moral indignation is not widespread, and the reality of imperialism goes unrecognized" (Patnaik 1990: 1–2). The legitimation crisis that the Vietnam War provoked within the United States, in the context of the oil crisis, suddenly revealed to a wide scholarly audience not so much the workings of empire as its weaknesses. Indeed, as with states, the workings of empire become most visible only when they falter. While Hardt and Negri (2000: xi) proclaim, "Empire is materializing before our very eyes," Patnaik might rejoin that on the contrary, it was materializing behind our backs; it now falters before our very eyes.

If this is an adequate explanation for the resurgence of popularity for the empire paradigm (regardless of whether Patnaik's definition is satisfactory), proponents of the empire thesis should have a ready explanation for why a focus on military mobilization proved so tempting and so misleading to the American Left. Such an explanation should also account for the intervening resurgence of the DP hypothesis. In other words, it should account for both the material and ideational (as distinct from ideological) bases of actually existing democracy and peace.[23]

Conclusion: Directions for Further Research

When the variables of state, war, and democracy are truly allowed to vary, many possibilities for further research open up that have been at best inadequately explored within the mainstream DP debate thus far. While Barkawi and Laffey (1999: 412–5; 2001) are right to highlight the weaknesses of DP theory's "territorial trap" among its liberal proponents, they overlook the (complementary) weaknesses inherent in positivist and presentist social science; weaknesses that reinforce a dominant "American subject position" and inform as well some critical perspectives on the Left. Fresh approaches paying greater attention to temporal, political, and cultural variation in political processes are needed to counteract political science's prevailing "presentist and whiggish" bias (Gunnell 2002: 339).[24]

Jack Levy's authoritative conclusion (this volume) that until recently, strong theory "has had little impact on the study of the democratic peace" and that the dynamic of DP research was driven by the initial dyadic findings is surely correct in the most obvious sense of *consciously deployed* theory. The contextual influence of tacitly *assumed* theory, however, has been decisive. As a leading comparativist of democracy (Schmitter 2001: 25) has observed,

> It is precisely the protracted stability, the sheer "taken-for-grantedness" of American political institutions when compared to virtually every other polity in the world, that allows its students of politics to exclude so programmatically the unavoidably complex patterns embedded in any historically specified notion of causality.

Indeed, as I have argued above, a pervasive *lack* of theorizing about the ontology of stateness, about conditions of war violence and conflict other than 1,000 MID-related deaths, and about definitions of democracy beyond Dahlian proceduralism has been DP theory's major shortcoming.

A monadic focus on the quality of democratic practice and experience can no longer assume the sufficiency of Dahlian proceduralism, normatively or historically. Normatively, the heroic claims of democratic culture's inherent pacifism need to confront unflinchingly democracy's weaknesses and regressions, including its implication in ethnic cleansing, democide, or genocide (Mann 2001). In Henry Kissinger's oft-quoted formulation, "history is the memory of states," and memory, John Locke argues in *An Essay*

Concerning Toleration, constitutes identity and accompanies recognition of responsibility for the things one has done.[25] If so, the health of a state may be measured by the extent to which states confront their repressed memories.[26] For Locke as for most U.S. Founding Fathers, there was little contradiction between proclaiming complete freedom and equality for "all," and accepting or advocating slavery for Africans and extermination of Native Americans.[27] Neither practice continues today; yet neither has enjoyed much official recognition and few would dispute the centrality of both to American political development.[28] A related question is whether democratic institutions in general produce particular incentives to go to war or incline democracies toward a special type of war; or alternatively, whether a strong bifurcation exists among democracies regarding bellicose inclinations of these kinds (Brock, Geis, and Müller 2006).

By extension, the current modification of constitutional and territorial claims of states in the face of indigenous peoples' challenges in international organizations should be scrutinized as possible evidence of "denationalization" (Sassen 2003), as can examples of how transnational security communities form the requisite to—rather than result from the domestic effects of—democracy. Such an investigation could entail a distinction between a state's democratic institutions and domestic democratic norms and culture. The latter could either follow from, or precede the former, depending on context (Cumings 2001).

More fundamentally, a focus on process should lead to a consideration of democratization, rather than simply democracy, in relation to peace. Snyder (2000; Mansfield and Snyder 1995) argues provocatively that democratizing states are more, rather than less, bellicose. The larger question remains how—or whether—one delimits "democratization" from "democracy." For example, if the U.S. is better characterized as "democratizing," then from Snyder's perspective, its war engagements seem less unusual. Far from its democratic character precluding war, this argument might further suggest that it is war engagements that trigger further waves of democratization, with the American Revolutionary War spurring the vote for most white men, U.S. Civil War leading to the enfranchisement of African American men, World War I giving women the vote, and the Vietnam War lowering the voting age to eighteen. Clearly, this is not the case for all wars; further research needs to establish the conditions under which this does or does not occur (and relatedly—from Popper's perspective emphasizing an overall reduction of violence—under what conditions troop demobilization reimports violence domestically).

Of course, these questions only beg the larger one of the effective burden of proof, which has a fundamentally political character (see

Kratochwil, this volume). For temporal reasons alone, the DP theory proponents' capacity to ascribe burden of proof is not substantial. Maoz's criticism of another statistical study's periodization as "an exercise in slicing [that] is devoid of theoretical content and strictly ad hoc" dangerously redounds not just on his work, but on the work of all those who do not adequately theorize their historical focus.[29]

If the era of the democratic peace only began to emerge in the latter part of the twentieth century, it is doubtless too soon to insist upon its ontology, let alone describe its dynamics and scope. My contention is that policies reducing violence at different levels of society coupled with the meaningful recognition of responsibility for past acts of violence at home and abroad crucially buttress strong democracy in ways that can durably realize peace among a multitude of monads. However, the relevant court of appeal, *zur lätzten Instanz*, is that of the future.

Notes

1. Steven Bernstein, Jürgen Dedring, Ted Hopf, Ned Lebow, Howard Lentner, John Owen IV, Bernd Reiter, and two anonymous reviewers all made helpful suggestions to earlier drafts, while sharing no responsibility for the final version.
2. As advocated by King, Keohane, and Verba: "Our concern is less with the development of theory than *theory evaluation*—how to use the hard facts of empirical reality to form scientific opinions about the theories and generalizations that are the hoped for outcomes of our efforts" (King et al., 1995: 24; emphasis in original).
3. For a significant exception, see Thompson (1996), arguing that peace is at least as much a cause as a consequence of democracy: as both geographic and temporal contexts, "zones of peace" are neither necessary nor sufficient for democracy's emergence, but "may have facilitated the development of liberal republican institutions and democratization" (p. 142).
4. Arendt (1992 [1970]: 7) may be understating the extent to which Kant's irony is tinged with melancholy indictment and despair when she writes, "the ironical tone of *Perpetual Peace*, by far the most important of [Kant's historical and political essays], shows clearly that Kant himself did not take them too seriously." She is surely right, however, to suggest that the essay is not merely a programmatic manifesto.
5. With reference to republican constitutions, for example, Kant writes that

   > if the consent of the citizens is required in order to decide that war should be declared (and in this constitution it cannot but be the case), nothing is more natural than that they would be very cautious in commencing such a poor game, decreeing for themselves all the calamities of war. . . . But, on the other hand, in a constitution which is not republican, and under which the subjects are not citizens, a declaration of war is the easiest thing in the world to decide upon, because war does not require of the ruler . . . the

> least sacrifice of the pleasure of his table, the chase, his country houses, his court functions, and the like
>
> (Kant, "Perpetual Peace," in Doyle 1996 [1983]: 24–25)

6. Additionally, Article I.ii. stipulates, "No independently existing state, whether it be large or small, may be acquired by another state by inheritance, exchange, purchase or gift"; I.iv. states, "No national debt shall be contracted in connection with the external affairs of the state."
7. For example, there is little consensus over whether the recent wars against terrorism represent continuity or discontinuity in world politics. The centrist former German chancellor, Helmut Schmidt (2002), for example, provocatively declared, "September 11 changed the world—at least according to most Americans," arguing that American foreign policy did not change suddenly in the year after September 11, 2001, but instead had continued along an increasingly imperialist trajectory over the past two decades. He suggests that this tendency became pronounced during the Reagan and, especially, Clinton presidencies.
8. More recent treatments of the monadic thesis are contradictory or inconclusive. Benoit, 1996 defends it but with reference only to the period of 1960–80; Rousseau et al., 1996 find a mixed record for 1918–88, with democracies less likely to initiate crises against all states; Gelpi, 1997, examining 1948–82, finds to the contrary (despite coauthoring Rousseau et al., 1996!) that democracies are more prone to initiate international conflict in order to distract domestic populaces, while Rummel (1995) finds democracies least prone to commit "democide" from 1900 to 1987 (but sidesteps an examination of the nineteenth century); MacMillan, 2003: 241—although ignoring critical literature such as Barkawi and Laffy, 1999—offers a cautious, nuanced review supporting the monadic thesis, while allowing that it remains "an interim, underspecified position" with extant "debate surrounding the causal relationships at work" and terms the dyadic thesis "a phantom" that should be abandoned.
9. See the debate between Russett and Spiro on this statistical evaluation: Russett in M. Brown et al., eds., 1996: 337–50; and Spiro in ibid., 351–54. For a more pluralistic statistical approach emphasizing trade and systemic factors, see Rasler and Thompson (2005).
10. Weart (1998: 2, 4, 176, and 293) exemplifies the hard-positivist claim that "democracies never fight each other." He thereby abandons the probabilistic turn that most similarly minded authors have increasingly taken in the literature, replacing it with "a truly general 'law' of history" (p. 8). As Elman (1999: 96–97) contends, Weart thereby constructs an essentially nonfalsifiable hypothesis in the guise of falsifiability. It may come as no surprise that a theorist so bent on describing an "iron law" of history is by training a physicist.
11. Even less do such authors reflect upon the plausible corollary that at the same time, the standards of democratic practice—including those used to evaluate their own country—correspondingly increase: to the point where (hubris and solipsism notwithstanding) potentially all cases fall short. They disregard the imperative Charles Tilly (2000: 13) reminds us of, namely to "never forget how

far short of theoretically possible maximum values" for the achievement of broad and equal citizenship, binding consultation, and protection "all really existing democracies have always fallen; by these demanding criteria, no near-democracy has ever existed on a large scale."

12. Cf. Russett in M. Brown et al. eds., 1996: 337–50; and Elman, ed., 1998. Ray (1995: 86–87) counts at least twenty borderline cases of interdemocratic peace.
13. Incongruously, Maoz (1997: 166) later complains that an analysis that distinguishes five periods—from the Treaty of Vienna to World War I, W.W. I, the interwar period, W.W. II, and the post–W.W. II period—is "an exercise in slicing [that] is devoid of theoretical content and strictly ad hoc."
14. He includes in this category those fought against Iran (1953), Guatemala (1954), Indonesia (1957), Brazil (1961), Chile (1973), and Nicaragua (1981) (pp. 120–24); remaining within the Cold War period, this list could be expanded to include Angola, the Congo, El Salvador, and Mozambique, among others.
15. That is, minimize U.S. public attention. The whole point of the covert operation was surely to "maximize" Nicaraguan public attention to turn the populace against the Sandinista government. By the same token, "low-cost" may be a matter of opinion, but conservative estimates of costs to Nicaragua run over ten billion dollars in productivity loss and infrastructural destruction, and 30,000 deaths. For a balance sheet of the costs of U.S. interference in the Western Hemisphere, see LaFeber (1986); in the East, see Johnson (2000).
16. There are numerous other examples: Russett retorts to Layne and Spiro (1996: 341)—in the face of evidence that French and British war preparations at the turn of the century were no more democratic than Wilhelmine Germany's—that "one might conclude . . . that virtually no countries had democratically-controlled foreign policy. (Would the United States pass this test in most of the twentieth century?) If so, there would not have been much opportunity for any wars between 'democracies,' and hence there could be no democratic peace!" Russett's unwillingness to answer this parenthetical question directly rather than rhetorically is suggestive of what Oren (ibid.: 295) terms "a *post*-hoc *ideological justification of Anglo-American mastery.*"
17. The coding and periodization of cases by Gowa (1999), for example, has been criticized on various grounds. In another case, Elman (1997, concl.) reductively defines democracy as "norms" or "political ideology" (p. 485), or "public opinion" (p. 487), which none of her interlocutors do. Theoretical and practical issues, *pace* Elman (505), extend beyond *both* matters of "abstract concern" *and* sources of "guidance for policy makers" that challenge "traditional ways of explaining international relations." Elman concludes that "since there are several paths to war and alternative routes to peace, democracy may not be the answer we are looking for" (ibid.: 506). In order to challenge this gloomy conclusion, with its implicit neorealist trade-off of democracy and peace, "we" might do worse than attempt a renovation of these terms within a positive-sum relationship.
18. For example, Larry Diamond, who opposed the war at its outset, calls the American occupation a "bungled effort to bring democracy " informed by an

"imperial hubris that had landed the United States in Iraq with a democratizing mission but no real sense of how to accomplish it." Nonetheless he served as a high-level consultant in this endeavor and credulously concludes in his analysis of this attempt that "if we learn from our mistakes, our next engagement to help rebuild a collapsed state might have a more successful outcome." See Diamond (2005: concl.).

19. Walter LaFeber detailed the successive episodes of U.S. unilateralism during the Cold War, up to and including the buildup of military bases in East Africa and the Middle East central to a "Carter Doctrine" that "committed the United States to protect—unilaterally if necessary—the Middle East and its oil from Soviet aggression. The Nixon Doctrine was dead, and Carter returned to the approach announced by Truman in March 1947." He concludes that "the problem for Americans is to recognize that the post-1945 era was an aberration in their history, and to recognize the causes for this relative decline in United States power." Walter LaFeber, 1981: 303.
20. "Since governments themselves often simulate, stimulate or even fabricate threats of external war and since the repressive and extractive activities of governments often constitute the largest current threats to the livelihoods of their own citizens, many governments operate in essentially the same way as racketeers." (Tilly 1985: 170–71). Giovanni Arrighi has recently extended this metaphor to explain the transformation of U.S. foreign policy from effective hegemony as "plausible protection" to an imperial resort to force without persuasion. See Arrighi (2005).
21. Echoing James Madison in the *Federalist Papers*, Charles Beard warned sixty years ago that in an attempt to bring peace to "the whole world," an empowered Presidency would undermine the U.S. Constitution, since the president would possess "limitless authority publicly to misrepresent and secretly to control foreign policy, foreign affairs, and the war power." See Beard (1948).
22. Although Russett (1996) wishes to cast himself in the mould of Galileo, the power and prominence that his position within the debate (and the academy) typifies lend themselves better to analogy with the established Church. Perhaps Kuhnian "normal" science is more normal in the social sciences than the physical sciences, in the manner that Feyerabend implies. In this case, Jack Levy's contention that DP theory "comes as close as anything we have to an empirical law in international relations" (Levy 1989: 88), if true, does not bode well for the search for laws in international relations. Indeed, "imperial peace" proponents might claim (against Cassius): "The fault, dear scholar, lies not in ourselves/ but in our star system, that we are underlings."
23. For one perspective addressing these aims, see Strange (1989). For a useful discussion of the inadequacies of several critical theories of empire, see Nitzan and Bichler (2004).
24. While future historians will determine whether this presentism is a recent historical development, its best exemplars are typically those with the grandest claims for a continuity of tradition. For example, Gabriel Almond—trained, not coincidentally, at Chicago and a major proponent of the postwar positivist and behavioral school—claims that the scientific tradition of political science,

"beginning with the Greeks and continuing up to the creative scholars of our generation, is the historically correct version of our disciplinary history [. . .]. We need to have a deep-rooted and unshakeable firmness in our commitment to the search for objectivity" (Almond, 1990: 29). More recently, he attacks those whose commitment to objectivity is dangerously shakable: Straussians, neo-Marxists, and "post-positivist, post-behavioral" political scientists—the lattermost holding the "perhaps predominant view of the history" of political science (Almond, 1996: 81).

25. H. Kissinger, 1964. *A World Restored* (New York: Grosset & Dunlap, 1964), chap. 1; Locke, *An Essay Concerning Toleration*, in D. Wootton, ed., *Political Writings of John Locke*. New York: Mentor, 1993: 186–209.
26. On this process of confrontation, with reference to seven countries, see Claudio Fogu, Wulf Kansteiner, and Richard Ned Lebow, eds., *The Politics of Memory in Postwar Europe* (Durham: Duke University Press, 2006).
27. Locke famously argues in the *Second Treatise* that "all men are naturally in a state of perfect freedom to order their actions and dispose of their possessions and persons as they think fit [. . .] and a state also of equality, wherein all the power and jurisdiction is reciprocal, and no one having more than another" (*Second Treatise*, chap. 2, "Of the State of Nature" in Wootton, ed., ibid.: 262–63).
28. For example, no museum of U.S. slavery or of Native American genocide exists on the Mall in Washington, DC, or in any state capital. See Ward Churchill, 1997. *A Little Matter of Genocide*: Holocaust and Denial in the Americas, 1492 to the Present. *San Francisco: City Lights; and David E. Stannard, 1992.* American Holocaust*: Columbus and the Conquest of the New World*. New York: Oxford University Press.
29. See fn. 12, above. While Benoit's (1996) claim that "democracies really are more pacific"—within the period of 1960–80 (!)—may be an extreme example of this tendency, it differs only in degree, not kind. As Doyle (1996: 372) concurs, "Until we have an alternative model, segmenting the data does not produce meaningful results."

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Part IV

New Directions

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9

Social Science as Case-Based Diagnostics

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A deep irony is embedded in the history of the scientific study of political science, but especially of international relations. Recent generations of scholars separated policy from theory to gain an intellectual distance from decision making to enhance the "scientific" quality of their work. But five decades of well-funded efforts to develop theories of international relations have produced precious little in the way of useful, high confidence results. Theories abound, but few meet the most relaxed "scientific" tests of validity. Even the most robust generalizations or laws we can state—war is more likely between neighboring states, weaker states are less likely to attack stronger states—are close to trivial, have important exceptions, and for the most part stand outside any consistent body of theory.

A generation ago, we might have excused our performance on the grounds that we were a young science still in the process of defining problems, developing analytical tools and collecting data. This excuse is neither credible nor sufficient; there is no reason to suppose that another fifty years of well-funded research would produce valid theory in the Popperian sense. We suggest that the nature, goals, and criteria for judging social science theory should be rethought, if theory is to be more helpful in understanding the real world.

We begin by justifying our pessimism, both conceptually and empirically, and argue that the quest for predictive theory rests on a mistaken analogy between physical and social phenomena. Evolutionary biology is a more productive analogy for social science.[1] We explore this analogy in its "hard" and "soft" versions and examine the implications of both for theory and research in international relations. We develop the case for forward "tracking" of international relations on the basis of local and general knowledge as an alternative for backward-looking attempts to build deductive, nomothetic theory.

This chapter is not a broadside against "modern" conceptions of social science. Rather, it is a plea for constructive humility in the current context of fascination with deductive logic, falsifiable hypothesis, and large-N statistical "tests" of propositions. We propose a practical alternative for social scientists to pursue in addition, and in a complementary fashion, to "scientific" theory-testing as traditionally conceived.

Overcoming Physics Envy

The conception of causality on which deductive-nomological models are based, in classical physics as well as in social science, requires empirical invariance under specified boundary conditions. The standard form of such a statement is this: given A, B, and C, if X then (not) Y.[2] This kind of bounded invariance can be found in closed, linear systems. Open systems can be influenced by external stimuli, and their structure and causal mechanisms evolve as a result. Rules that describe the functioning of an open system at time T do not necessarily do so at T + 1 or T + 2. The boundary conditions may have changed, rendering the statement irrelevant. Another axiomatic condition may have been added, and the outcome subject to multiple conjunctural causation. There is no way to know this a priori from the causal statement itself. Nor will complete knowledge (if it were possible) about the system at time T necessarily allow us to project its future course of development.

In a practical sense, all social systems (and many physical and biological systems) are open. Empirical invariance does not exist in such systems, and seemingly probabilistic invariances may be causally unrelated (Bhaskar, 1979; Harré and Secord, 1973; Collier, 1994; Patomäki, 1996; Jervis, 1997). As physicists are the first to admit, prediction in open systems, especially nonlinear ones, is difficult, and often impossible.

The risk in saying that social scientists can "predict" the value of variables in past history is that the value of these variables are already known to us, and thus we are not really making predictions. Rather, we are trying to convince each other of the logic that connects a statement of theory to an expectation about the value of a variable that derives from that theory.

As long as we can establish the parameters within which the theoretical statement is valid, which is a prerequisite of generating expectations in any case, this "theory-testing" or "evaluating" activity is not different in a logical sense when done in past or future time.[3]

Consider how this plays out in evolutionary biology, the quintessential open system. Evolution is the result of biological change and natural selection. The former is a function of random genetic mutation and mating. The latter depends on the nature and variety of ecological "niches" and the competition for them. These are in turn shaped by such factors as continental drift, the varying output of the sun, changes in the earth's orbit, and local conditions that are hard to specify. Biologists recognize that all the primary causes of evolution are random, or if not, they interact in complex, nonlinear ways and make prediction impossible. Certain kinds of outcomes can be "ruled out" in a probabilistic sense, but almost never absolutely. Biologists have attempted to document the course of evolution and explain the ways in which natural selection works. Historical and theoretical work has resulted in a robust theory of evolution that permits scientific reconstruction of the past in the context of a logic that explains why things turned out the way they did.

One of the big controversies within this research community is about the contingency of that past. Stephen Jay Gould (1989) makes the case for determining the role of accident in evolution. He insists that if you could rewind the tape of life and run the program over again you would end up each time with a radically different set of organisms. Some of his colleagues find his claim extreme. Ever since Darwin, it has been recognized that evolution produces morphological similitude because there is something like a "best" set of physical characteristics and strategy for grappling with the challenges of life. Diverse species have converged independently on body plans and life styles that are suited to avoiding predators and to exploiting food resources.[4] What is at stake in this controversy is how close the system has come to optimality; and the extent to which factors outside the system (Gould, 1989) or the system itself (Morris, 1998) are most important in shaping the course of evolution. Both sides acknowledge that the primary causes of evolution are independent of and outside any theory of evolution.

The study of evolution has been approached from scientific and heuristic perspectives. The scientific approach should be of particular interest to political scientists because it eschews prediction in favor of explanation. Working on the assumption that the course of evolution is determined by chance and context, Charles Darwin and his successors developed a theory of process to understand the past. That theory and its extensions fully meet the accepted criteria of scientific theories; they consist of a set of linked propositions with well-specified terms and domain and are thus empirically falsifiable. Darwinian theory, widely regarded as one of the seminal scientific advances of the modern era, challenges those political scientists who assert that prediction is the principal, or even only, goal and test of a scientific theory.

The heuristic approach to evolution consists of narratives intended to influence our thinking about ourselves and our environment. These stories and the homilies associated with them have been extremely influential. What has sometimes been called the "Darwinian revolution" recast human conceptions of species "uniqueness," its relationship to other life forms, and hastened the trend toward secularization by providing an eminently plausible substitute for a deity-centered account of creation. More recent work on mitochondrial DNA, which suggests that Africa was the birthplace of *homo sapiens sapiens* and that "Lucy" was our common ancestor, also have profound political and social implications that neither scientists nor journalists have been shy to draw. These examples stand in sharp contrast to the nineteenth-century use of evolution to justify war and imperialism and prop up Western claims of racial superiority. Gould (1996) has shown how many textbook treatments of evolution are still "species centric" and contain illustrations that show humanity as the apex of evolutionary development.

There is a nice correspondence between the heuristic forms of evolutionary biology and international relations. Narratives of international relations also encapsulate so-called lessons of the past—the more recent past, to be sure—to influence thinking about the present and future. Like homilies about evolution, scholars, journalists, and policymakers cite history as a general guide to action (e.g., realism, deterrence, the dangers (or benefits) of armaments), or as justification for specific foreign policies. Proponents and opponents of intervention in Bosnia, Kosovo, and Iraq have attempted to legitimize their respective positions with reference to 1914, the failure of the League of Nations, the Holocaust, and Vietnam.

The scientific study of international relations fits best, if partially, with evolutionary biology. For fundamentally similar reasons, international relations theory will not be able to predict events, trends, or system transformations in a useful way. But international relations theory, like its Darwinian counterpart, can attempt—as many scholars do—to develop theories of process to organize our thinking about the past. Like paleontologists reading the evidence of fossil beds, these scholars use documents and interviews with former policymakers to evaluate competing theories, qualitatively and quantitatively. Using theories as starting points, they can also reconstruct the origins of revolutions, wars, accommodations, and other international phenomena in cases where there is adequate contextual evidence about the goals, understandings, and calculations of relevant actors and the political environment in which they functioned. Explanatory theories that pass the same tests as evolution have a serious claim to scientific status. International relations differ in at least one major respect from biology. A robust theory of evolution is possible because the actors in this drama—plants, animals, and other forms of life—know nothing about the theory. Human beings devote enormous resources, individually and collectively, to understanding the nature of their environment. That understanding has led them to interfere with biological evolution in important ways. People started to domesticate and selectively breed animals at least 10,000 years ago. Intensive experimentation with crops started not long afterward. In the twentieth century, we have utilized antibiotics and other medical techniques to interfere with natural selection, and knowledge of molecular biology to alter genetically a wide range of plants and animals. The current century will almost certainly bring more radical forms of bioengineering, including gene substitution and more general manipulation of the human genome.

Human intervention in the processes that govern social and political relations has been even more striking. As a general rule, the more people think that they understand the environment in which they operate, the more they attempt to manipulate it to their advantage. Such behavior can relatively quickly change the environment and its governing rules. The Asian financial crisis of the 1990s offers a good example. Rapid growth allowed some Asian countries to attract hundreds of billions of dollars of short-term international loans in the early 1990s. When short-term money managers began to lose faith in the Thai and South Korean economies, the IMF pressured their governments to maintain exchange rates by raising interest rates to restore investor confidence. Such a strategy had often worked in the past, yet the more the Asian governments tried to defend their currencies, the more panic they incited. Money managers hastened to withdraw their funds before local currencies collapsed. Urged by the IMF and Washington, the Russian, South African, and Brazilian economies subsequently pursued the same policy with similar disastrous results. In the aftermath, the IMF and many prominent economists came to recognize that greater sophistication on the part of investors and the greater mobility of capital had changed the rules of the game. They needed different strategies to cope with the problem of investor confidence (Sachs, 1998; Radelet and Sachs, 1999).

Knowledge of structure and process also allows conscious and far-reaching transformations of social systems. Smith, Malthus, and Marx described what they believed to be the inescapable "laws" that shaped human destiny. Their predictions were not fulfilled, at least in part, because their analyses of economics and population dynamics prompted state and corporate intervention designed to prevent their predictions from coming to pass. Human prophecies—which are a form of prediction—are often self-negating.

A similar process has occurred in international relations. Prodded by two destructive world wars and the possibility of a third that might be fought with nuclear weapons, leaders sought ways to escape from some of the deadly consequences of international anarchy and the self-help systems it seemed to engender. They developed and nurtured supranational institutions, norms, and rules that mitigated anarchy and provided incentives for close cooperation among developed states. Gradually, the industrial democracies bound themselves in a pluralistic security community. The same concerns ultimately played a significant role in bringing the cold war to a peaceful end. Influential figures in both camps came to recognize the dangerous and counterproductive consequences of arms races and the sustained competition for unilateral advantage. With Gorbachev acting as a catalyst, the superpowers transformed their relationship and, by extension, the character of the international system.

To the extent that actors can, wittingly or unwittingly, change the "rules of the game," and even the nature of the political and economic systems in which they operate, general theories of process in international relations will have restricted validity. Unlike theories of evolution, they will not apply to all of history, but only to discrete portions. It seems self-evident but needs to be emphasized: scholars need to specify carefully the temporal and geographic domains to which their theories are applicable. We suspect those domains are often narrower and more constrained than is generally accepted.

A second big difference between international relations and evolutionary biology is the purpose of the endeavor. International relations scholars cannot predict the future, but neither can we ignore it. People need to make decisions in the face of uncertainty about the future, and consequently they need appropriate concepts and foci for information to maximize the quality of those decisions. As deductive-nomothetic theory is of very limited utility for this purpose—something policymakers have known for a long time—scholars need to develop some other, more useful method if we are to have any influence as a profession on important policy dilemmas.[5]

Policy-relevant social science considers the general *and* the particular and goes back-and-forth between them to make sense of social reality.[6] At the general level, we have numerous (if fundamentally untestable) propositions and less formal understandings of some of the conditions in which war and peace may be more likely to occur. With regard to war, historians and social scientists alike have distinguished between need- and opportunity-based resorts to force and have identified different sets of conditions associated with each. These include but are not limited to general power capabilities, the military balance between states and alliances, expected shifts in any of these balances, and domestic problems that threaten leaders, regimes, or states themselves. More broadly, decisions to use force also appear to be influenced by the general state of regional and international affairs, dominant moral and intellectual conceptions, and salient historical analogies. We need to treat all these factors as defining possibilities in particular circumstances; but no combination of them can predict what choices real actors will make.

Take the example of the post-"victory" conflict in Iraq, which one reviewer of this chapter objected was quite predictable. What this objection ignores is how senior administration officials involved in war planning systematically sidelined such predictions and the implications of that choice for how events would unfold. The specific decision on troop deployments nicely illustrates the problem for social research. Extensive debate and analysis within the Pentagon, CIA, and National Security Council produced widely varying estimates of the deployment needed in postwar Iraq depending on what peacekeeping "model" of troops to population was used as a baseline. According to a confidential NSC briefing for Condaleeza Rice, "Force Security in Seven Recent Stability Operations," the Kosovo model "predicted" the need for 480,000 troops in postwar Iraq, compared to 364,000 for the Bosnia model and only 13,900 for the Afghanistan model (Gordon 2004). No combination of factors could have predicted that Defense Secretary Donald Rumsfeld would later dismiss the higher estimates. Even if existing explanations for postconflict conditions had been sufficient to demonstrate Rumsfeld's poor judgment, the actual decision to ignore those estimates produced significant and unanticipated outcomes as events unfolded on the ground. The chaos in many cities following the fall of Baghdad, for example, created a hospitable environment for the nascent insurgency to establish a foothold in unsecured areas, organize itself, and gain public support. Even in this "predictable" case, a research strategy that identifies early indicators of which model (if any) of postconflict peacekeeping is playing out or provides warnings of the need to revise troop estimates as events unfold is of greater utility than one that promises predictable outcomes.

Put more generally, concreteness requires culturally local knowledge, because states, ruling elites, and individual leaders respond differently to similar combinations of threats and opportunities. Incentives ultimately are in the eye of the beholder. Leaders may also respond differently to similar stimuli before and after experiences that transform their identities or their understanding of ongoing strategic interactions in which they participate. We need better tools to wed general knowledge about international relations and foreign policy to the more specialized knowledge that area and country experts have about actors in specific conflicts and contexts.

Forward Reasoning

The logic of our argument suggests that point prediction in international relations is impossible. Evolutionary biology is not a tool for explaining current "trends." It is at best a limited tool for identifying relevant trends, but not until fairly long after the fact, because such a multitude of forces and random interactions determine the course of evolution. As we have argued, social scientists cannot afford the luxury of only examining the past, they are deeply engaged in the attempt to explain the present and think analytically about the future. Our interest is in the identification and connection of chains of contingencies that could shape the future.

One useful approach is the development of scenarios, or narratives with plotlines that map a set of causes and trends in future time. This forward-reasoning strategy is based on a notion of contingent causal mechanisms, in opposition to the standard, neopositivist focus on efficient causes, but with no clear parallel in evolutionary biology. It should not be confused with efforts by some to develop social scientific concepts directly analogous to evolutionary mechanisms (such as variation or selection) in biology to explain, for example, transformations in the international system or institutions, or conditions for optimum performance in the international political economy.[7]

Scenarios are not predictions or forecasts, where probabilities are assigned to outcomes; rather, they start with the assumption that the future is unpredictable and tell alternative stories of how the future may unfold. Scenarios are generally constructed by distinguishing what we believe is relatively certain from what we think is uncertain. The most important "certainties" are common to all scenarios that address the same problem or trend, while the most important perceived uncertainties differentiate one scenario from another.

This approach differs significantly from a forecasting tournament or competition, where advocates of different theoretical perspectives generate differential perspectives on a single outcome in the hope of subsequently identifying the "best" or most accurate performer. Rather, by constructing scenarios, or plausible stories of paths to the future, we can identify different driving forces (a term that we prefer to independent variable, since it implies a force pushing in a certain direction rather than what is known on one side of an “equals” sign) and then attempt to combine these forces in logical chains that generate a range of outcomes, rather than single futures.

Scenarios make contingent claims rather than point predictions. They reinsert a sensible notion of contingency into theoretical arguments that would otherwise tend toward determinism. Scholars in international relations tend to privilege arguments that reach back into the past and parse out one or two causal variables that are then posited to be the major driving forces of past and future outcomes. The field also favors variables that are structural or otherwise parametric, thus downplaying the role of both agency and accident. Forward reasoning undercuts structural determinism by raising the possibility and plausibility of multiple futures.

Scenarios are impressionistic pictures that build on different combinations of causal variables that may also take on different values in different scenarios. Thus it is possible to construct scenarios without preexisting firm proof of theoretical claims that meet strict positivist standards. The foundation for scenarios is made up of provisional assumptions and causal claims. These become the subject of revision and updation more than testing. A set of scenarios often contains competing or at least contrasting assumptions. It is less important where people start than where they are through frequent revisions, and how they got there.

A good scenario is an internally consistent hypothesis about how the future might unfold; it is a chain of logic that connects “drivers” to outcomes (Rosell, 1999: 126). Consider as an example one plausible scenario at the level of a “global future” where power continues to shift away from the state and toward international institutions, transnational actors, and local communities. The state loses its monopoly on the provision of security, and basic characteristics of the Westphalian system as we have known it are fundamentally altered. In this setting, key decisions about security, economics, and culture will be made by nonstate actors. Security may become a commodity that can be bought like other commodities in the global marketplace. A detailed scenario about this transformation would specify the range of changes that are expected to occur and how they are connected to one another. It would also identify what kinds of evidence might support the scenario as these or other processes unfold over the next decade, and what kind of evidence would count against the scenario or indicate a branching off point. Moreover, evidence that counts against one scenario might count for another. For example, whereas a plotline that included September 11 might not have been anticipated, alternative scenarios that led to futures where the state reasserted its security function might have been constructed. Evaluations of evidence as events unfolded would then determine which scenario appeared to be playing out, or whether the same scenario had started to evolve in unanticipated directions. The same drivers could be at play in multiple scenarios, but how changes in technology, human agency, and transnational networks interact is less certain and these interactions can lead to outcomes along very different trajectories.

This method is simply a form of process tracing, or of increasing the number of observable implications of an argument, in future rather than past time. Eventually, as in the heuristics of evolutionary biology, future history becomes data. But instead of thinking of data as something that can falsify any particular hypothesis, think of it as something capable of distinguishing or selecting the story that was from the stories that might have been. Such storylines should not be thought of as linear, but as contingent in a way our scenario methodology tries to capture.

The scenario methodology has seven steps: identification of driving forces, specification of predetermined elements, identification of critical uncertainties; development of scenarios with clear "plotlines," extraction of early indicators for each scenario; consideration of the implications of each scenario, and development of "wild cards" that are not integral to any of the scenarios but could change the situation dramatically if they were to happen.

Driving forces are the causal elements that surround a problem, event, or decision. While some driving forces are likely to derive from standard causal arguments in major social science theories (e.g., the diffusion of power and the growth of commodities markets), others are not. In developing explanations for past events it is common to identify only a few, even two, driving forces. We call them "independent variables," which implies, of course, that they are somehow independent (of each other and of other causes). In generating scenarios the starting point is to put on the table multiple driving forces that can be the basis, in different combinations, for diverse chains of connections and outcomes. Parsimony comes after, not before, an analysis of complex causal possibilities.

Predetermined elements appear relatively certain. They are parameters that can safely be assumed for the scope and span of the scenario exercise. One goal of a scenario is to separate what appears certain, or very close to it, from what people simply think or believe is likely, without engaging in well-established psychological processes of treating routine events, "causes" of "effects," and "structural" causes as immutable.[8]

There are no easy experiments and control situations in world politics, but we can still assert with confidence that some developments appear nearly certain. Examples include slowly changing phenomena, such as demographics, and constraints such as geography and physical resources. We nevertheless need to be very careful in categorizing elements as certain.

In the 1970s, the experts assumed that oil reserves were rapidly becoming depleted, only to be surprised by new discoveries. It seems reasonably safe, however, to assume that new water will not be discovered in the Middle East, and that limited supplies constitute a real source of friction between Turkey and Syria, and between Israel and Palestine. We must be even more cautious about political "certainties" and "social facts."[9] In the 1970s many theorists treated as given intense and ongoing conflict between Egypt and Israel, and between the United States and the Soviet Union. In both cases, scholars were profoundly surprised by the termination of these conflicts and the reshaping of the regional and international environments that resulted.

Critical uncertainties describe important determinants of events whose character, magnitude or consequences are unknown. This uncertainty can also be the result of unknown interaction effects among combinations of the predetermined elements. Scenarios highlight the critical uncertainties; the plotlines confront these uncertainties directly as connecting principles that pull the story together.

Standard social science theory "testing" treats as mutable the "independent variables" suggested by connecting principles that we already know well. In scenario thinking, plotlines have to work with the critical uncertainties rather than the other way around. This is often a serious challenge, because it is impossible to know in advance of the empirical data what combinations of driving forces might come together in a setting of multiple conjunctural causality to yield particular outcomes. Of course, it is precisely that challenge that makes the scenario method a valuable tool. The goal is to learn from the future (as it unfolds), not predict it. No set of scenarios captures a comprehensive picture of all possible causal combinations—and it is not necessary to do so. What are necessary are clear causal relationships, even if complex. These can be evaluated, and modified, in response to emerging data.

A *scenario plotline* is a compelling story about how things happen. It describes how driving forces might plausibly behave as they interact with predetermined elements and different combinations of critical uncertainties. Plots have their own logic—sometimes more than one logic—that drive the story forward and suggest the directions in which the uncertainties may resolve. The logic(s) may be drawn from standard international relations theories. For example, balance of power theory emphasizes the way in which a strong driving force (states' desire for independence and autonomy) interacts with predetermined elements (power configurations) and critical uncertainties (who will ally with whom) in an international system to produce outcomes. But this is not the only logic applicable to international relations.

Competing theories or approaches identify different drivers and may lead to different behavioral expectations. Moreover, all these approaches acknowledge the importance—sometimes determining—of elements outside their theory, such as processes of diplomacy and personalities and preferences of individual leaders. The advantage of the scenario method is that stochastic events, equifinality, multifinality, and complex, conjunctural causation are no longer stubborn inconveniences that need to be minimized or simply ignored. They can be treated as natural and fundamental aspects of reality. This can be done by developing multiple scenarios, or scenarios with branching points, that capture the probabilistic nature of the arguments at play, without, however, having to attach essentially arbitrary probability estimates to the strength of particular "variables" or different outcomes.

Plotlines draw on and ultimately depend upon the existence of regularities in social interaction, in world politics as elsewhere.[10] But they consciously place these regularities in a contextualized setting and thus make no claim to identify invariant ontological structures or laws.

Early indicators are observable and measurable attributes of the political situation that allow researchers to assess, as events unfold, the extent to which a scenario (or which part of a scenario) is coming to pass. Developing early indicators is an exercise in "process-tracing," extrapolated into the future. If a particular set of driving forces were to become most important and lead to a given scenario, what would be some of the early indications that events were indeed unfolding along that particular path and not along another? The strategy is a modified version of the simple idea of increasing the number of observables that differentiates one set of explanations from another in a verifiable way.[11] By doing so in future time, we reduce post-hoc determinism and force ourselves to confront historical contingency in a creative manner.

Implications of scenarios are aimed explicitly at decision making and choice. One of the valuable consequences of thinking about historical contingency in a disciplined way is that it forces people who are going to make decisions to ask what they would do if they found themselves in—or heading toward—a world different from the one they expect. Theory-based prediction compels decision makers to make or justify a decision or strategy on the basis of a single-point forecast (at best, with a range of uncertainty around it) whose accuracy cannot be known until after the outcome is known. With scenarios, actors can evaluate decisions against the most plausible scenarios in the current set and then evaluate the likelihood of these scenarios as their strategy unfolds.

Considering at once the behavioral implications of more than one scenario helps to clarify the stakes, risks, and uncertainties connected with any single course of action that an individual or a state might choose. In some situations policymakers may be able to adjust their strategies in response to information that indicates their expectations are not being fulfilled. In others they may be able to hedge effectively against several different scenarios. Tracking through the use of early indicators might also help leaders to recognize that their actions could be an important pivot or determinant of the kind of future that was likely to evolve. Obviously, a process like this that included early consideration of several plausible scenarios, and the different ways the critical uncertainties might combine, could have been very helpful to U.S. political and military authorities before they chose to launch a war to change the regime in Iraq in 2003.

Finally, designers of scenarios need to consider *wild cards*. These are conceivable, if low-probability, events or actions that might undermine or modify radically the chains of logic or narrative plotlines. They might include assassinations, dramatic economic changes, and famines and natural disasters. Some wild cards could constitute extreme values on a familiar independent variable; others might be outside the realm of standard social science arguments. In either case, doing this prospectively could change our views on what variables should be a part of theory, or what an "extreme" value actually is—since it avoids the possibility of post-hoc certainty. It would also be revealing if we were to miss entirely a wild card type cause, or if wild cards happened but were "dampened out" in their effects by other kinds of causes.

A central choice in developing scenarios is whether to begin with drivers—the "causal forces" or the plotline in the story—or the outcomes or resolution of the stories. There are several reasons to start with drivers. From the perspective of traditional social science, it is cleaner in principle to reason from cause to effect when possible. Pragmatically, scenario thinkers are more likely to generate results that contain surprises or challenging combinations of events when they begin from beliefs or ideas about fundamental causes, rather than from preconceived notions of the most likely outcome states. People who work on particular problems and have done so for a long time typically carry around in their heads a set of plausible outcomes, or "official futures," that they believe are likely and relevant to their concerns. One of the purposes of constructing scenarios is to encourage scholars and experts to think outside of these confines about plausible, different futures.

In summary, scenario thinking is disciplined by beginning with the identification of the several factors (causes) that scholars believe are most important to the future of a political relationship. They can then distinguish between what is most certain and what is most uncertain. Uncertainty in this context can mean that scholars are uncertain about the "value" of the variable, or about the causal impact of the variable, or both. The three or four most important uncertain causes can then be identified, as well as a narrative explication of the key uncertainties at play and the nature of their possible interactions. These critical uncertainties become the basis of different plotlines. By assigning different "values" to these variables, and combining them in different ways, scholars can reason to a set of plausible end-states. These end-states should be plausible within existing conceptual frameworks, but, when possible, challenging to "official futures." Scholars can then develop the narrative pathways that could generate the outcomes by moving from a highly abstract framework toward increasingly precise—and compelling—causal stories that specify assumptions, major drivers, limiting conditions, and implications. As part of these narratives, scholars must specify the trends that weave through their stories and can be monitored as time passes.[12]

A Forward-Looking Research Agenda

The novelty of this approach in political research means we cannot draw from existing scholarship to support the fruitfulness of our approach. Nonetheless, we gain some confidence from the wide application of similar approaches in the policy sciences (e.g., health policy research and public health economics) and biological and physical sciences that share some of the epistemological challenges we have identified (e.g., climatology). Such approaches are especially ubiquitous when research fields have direct public policy implications. Moreover, empirical scholarship on expert political judgment suggests that decision making that is more consistent with the underlying logic of scenario-based strategies will produce better "predictions" than deductive research strategies (Tetlock 2005). Specifically, longitudinal studies of expert predictions demonstrate that those with extensive specific knowledge, who draw on research from many fields, and are able to improvise and revise in response to changing events outperform those wedded to one tradition or who impose ready-made solutions.

In lieu of surveying existing research, this section applies the abstract understanding of a forward-looking research strategy to major trends in international relations. We do not elaborate full scenarios here.[13] Instead, we identify what we believe to be three of the most important developments likely to affect international relations in the coming decades: the continued increase in intrastate conflict, further proliferation of weapons of mass destruction, and an increasing privatization of security. We should note at the outset that we originally developed these plotlines in 2000, and have not changed our initial formulations so as to stay true to our methodology. Our purpose is to show how a forward-based method can be used to track, study, and understand these trends in a disciplined way. We make no claim that fundamental controversies in social science can be thus resolved, although we are confident that constructive forward-based thinking can help to clarify some of the parameters surrounding those controversies and the nature of the disagreements at hand.

Distinguishing trends, drivers, and outcomes can be conceptually difficult. The trends we identify may be outcomes caused by previous drivers, and also by drivers of other outcomes, most notably fundamental changes in the international system. Indeed, we chose the three trends because we thought they were likely to contribute to important changes. The methodology of scenario construction has allowed us to monitor and revise our expectations. If indicators that we have specified with any one of these trends do not become apparent, we then reexamine underlying theoretical assumptions and reformulate the scenario. In this sense, the method is rather like an antiaircraft system, responding to feedback and readjusting its trajectory as history flies by.

Using scenarios as a research method, the goals of research expand to include not only the development of better explanations, but also identification of points of intervention, ongoing revisions of scenarios as events unfold, and the consideration and reevaluation of salient causal pathways. Scenario methodology also highlights how learning and feedback may change possible futures in dynamic ways difficult to anticipate. This research strategy could easily be applied to particular regions—South Asia, the Middle East—or to particular relationships. We chose instead to focus on trends that cut across regions to show the most general application of the research strategy.

Intensified Ethnic Conflict

For the most part, the most violent and pervasive conflicts in the post–cold war period are within states, not between them. They nonetheless often become international when they spread across borders or draw in third parties as participants, would-be mediators, or peacekeepers. While a great deal has been written on specific intrastate wars and the general trend away from interstate violence, deductive theory has made relatively little headway in explaining within-state conflict or in understanding how to prevent its eruption. In part, the problem stems from inattention. During the Cold War, theories of international politics developed concepts and categories centered on states and strategic relationships, which said little or nothing about ethnic and civil conflict. Despite this inattention, however, these conflicts were frequently an important foreign policy concern, a central contributor to superpower conflict, and a prominent item on the agendas of consumers of the resources of international institutions. A complicating factor is that the latest round of ethnonationalist conflict is occurring in an historical, strategic, and institutional context markedly different not only from that of the last fifty years, but also from the context of previous historical periods when such conflict was more common.

Research outside of international relations has uncovered a wide range of causes of intergroup conflict and violence.[14] These usually focus on local conditions that may cascade toward or trigger conflict: ancient hatreds, manipulation by belligerent leaders, or fear-driven local security dilemmas between ethnic groups in the same territory.[15] Despite recent attempts by international relations scholars to incorporate these causes into their theories, complex interactions among a changing international institutional environment, relationships among major powers, and evolving local conditions create a formidable challenge. For reasons we have made clear, deductive theories are unlikely to capture the complexity of the interactions among the relevant factors at this stage in our understanding. Nor are they likely to predict communal conflict and, consequently, deductive theories will contribute little to prevention and to the limitation of human suffering produced by such conflicts. One recent review of ethnic and intrastate conflict literature termed monocausal arguments as theoretical "culs-de sac" that have pushed the "study of social violence into the same paradigm-level debates that have characterized the American study of international relations" (King, 2004: 432).

A more modest and useful strategy would be to draw on past cases—Rwanda, Bosnia, Somalia, Sudan—to map the multiple paths to ethnonationalist conflict, identify the contingencies and wild cards that played out, and construct several scenarios of communal conflict, each highlighting a different critical uncertainty.

Generalizing on the basis of the past is not enough. Conditions change, and belligerents may learn lessons, confounding the expectation that strategies that succeeded in the past will work in future conflicts. The lessons learned from Bosnia did not provide an adequate map for anticipating or responding to the crisis in Kosovo. Unanticipated responses, "wild cards" such as the accidental bombing of the Chinese embassy, and the complex interactions of local and external events require the consideration of new branches and new paths. Through scenario construction, analysts recognize that "causes" may interact in unexpected ways and are sensitized to cues when events begin to track down alternative paths.

The scenario-building strategy begins with driving forces and traces through causal pathways as these drivers interact in specific circumstances.

Causal drivers of ethnonationalist conflict might include the breakdown of empires, the proliferation, evolution, and fragmentation of identities, and/or underlying demographic or environmental stresses caused by population growth and resource scarcity. The breakdown of empire is an example of a driving force derived from social science theory (Emerson, 1960; Lasswell, 1935; Kupchan, 1994; Henderson and Lebow, 1974).

When empires decay or collapse they can provoke intense conflicts by former minority groups attempting to create successor states. The competition of two or more groups for the same territory has led in this well-known dynamic to some of the most intractable struggles of the twentieth century. The most acute variants involve successor states that have arisen from partition or have been subsequently partitioned. The end of the British Empire half a century ago left in its wake ongoing conflicts that include Northern Ireland, India and Pakistan, Greeks and Turks in Cyprus, and the Israeli-Palestinian conflict. The collapse of the Soviet Union has generated similar conflicts along its former periphery—Armenia-Azerbaijan, Moldava—conflicts that give every indication of becoming intractable. The disintegration of Yugoslavia might also be considered a by-product of the Soviet collapse, with a smaller but intense set of conflicts associated with the breakup of a central state. Scenarios might be constructed that take early cues from postcolonial conflicts and the presence or absence of various local causes, but then consider additional general and local drivers, in different combinations, to sketch out different plausible trajectories of conflict.

International norms are a more mutable driver that fall under the "critical uncertainty" category and thus need to be tracked. Sometimes they evolve slowly enough so that they can be treated as givens. However, they also may change rapidly, as many have following the cold war. Norms of humanitarian intervention are undergoing a particularly rapid period of evolution. Although sovereignty has never been absolute, the evolution of norms of intervention appears particularly uncertain as spheres of influence have disintegrated, global civil society has increased pressure on the international community to intervene when gross violations of human rights occur, and fear of mass migrations and spillovers of conflicts have increased. Since these norms remain uncertain and not deeply embedded in international institutions and structures, it is impossible to predict on the basis of those norms which crisis will evoke an international humanitarian response, or whether that response will appear justified or convincing enough to be successful or sustainable. Alternative scenarios would weigh this humanitarian impulse differently and explore different catalysts.

The interaction of leaders and domestic politics with changing international norms is even more contingent. The "Somalia Syndrome," for example, "taught" U.S. leaders not to commit ground troops in an unstable local environment and thus significantly affected subsequent decisions in Haiti, Rwanda, Bosnia, and Kosovo. In May 1994 the Clinton administration issued new restrictive guidelines on humanitarian intervention; when the most intense genocide of the twentieth century began in April 1994, the United States stood aside and discouraged states and international organizations from timely and active intervention. One senior state department official, highlighting the problem of feedback and agency in paths to conflict, noted "It was almost as if the Hutus had read it [the guidelines]" (Weiss, 1995: 172).

Recent work on the micropolitics of social violence reinforces the importance of reflexivity. Commenting on the central finding of Mark Beissinger's (2002) study of social mobilization and collective violence in the collapse of the Soviet Union, a recent review notes that violence "cannot be understood, much less modeled, without taking account of the reflexive power of mobilization itself" (King, 2004: 441). Calling Beissinger's book "an elegantly theorized account of the power of contingency," King (2004: 444) notes that "[leaders'] calculus of costs and benefits, such as it was, was demonstrably influenced by their assessment of what had succeeded and failed in other circumstances . . . . It was the very context in which individual events took place that accounts for how over time the impossible came to be seen as inevitable." In other words, the clustering of protests and social mobilization themselves shaped a future of action against the state, the eventual collapse of the Soviet Union, and the social mobilization and solidarity that subsequently, in some local contexts, led to civil war (King, 2004: 441).

The metaphor of disease, illness, and decline, initially suggested by Thucydides, and more recently by Bobrow for analyzing insecurity, fits nicely with our approach to forward reasoning. As Bobrow (1996: 446) puts it, "Implicit or explicit strategy recommendations should then carry warning labels. They also should be subject to continuing monitoring for adverse consequences." They may have adverse side effects, and their use can sometimes produce immunities that make them ineffective in the future. For example, humanitarian efforts, peacekeeping, and safe havens in Bosnia may have prolonged conflict, and, by creating ethnic enclaves, even assisted Serbs in ethnic cleansing.[16] That is not to say that more forceful intervention might have had different results. Forward tracking and careful monitoring can help to expose where and why policies veer from anticipated trajectories and can highlight critical points of intervention as new "branches" emerge.

Work on resource scarcity and acute conflict could also easily become the basis for a scenario-based approach to intrastate conflict (Homer-Dixon, 1999). Homer-Dixon maps the relationship between apparently unalterable trends such as demographic pressures or depletion of natural resources to their impact on local social and political conditions to produce potential conflict. He argues that environmental scarcity constitutes an understudied set of causal variables that may be increasingly important as an underlying cause of acute violent conflict, although, he cautions,

> The relationship between environmental factors and violence is complex. Environmental scarcity interacts with factors such as the character of the economic system, levels of education, ethnic cleavages, class divisions, technological and infrastructural capacity and the legitimacy of the political regime. These factors, varying according to context, determine if environmental stress will produce the intermediate social effects [poverty, intergroup tensions, population movements, and institutional stress and breakdowns]. Contextual factors also influence the ultimate potential for conflict or instability in a society. (Homer-Dixon, 1996: 45)

Homer-Dixon's candid assessment of the limits of the causal claims of his research identifies many of the problems of research informed by the ideal of the covering law: uncertain relationships between underlying and immediate causes, open systems, complexity, negative degrees of freedom, and feedback and learning. The benefit of continually modifying and sharpening hypotheses in an effort to demonstrate that their validity is unclear when "the causes of specific instances of violence are always interacting sets of factors, and the particular combination of factors can vary greatly from case to case" and are "often unique to the society in question" (Homer-Dixon, 1999: 7, 178).

A more pragmatic and effective approach would be to begin with the same causal variables Homer-Dixon identifies, but work with the assumption that these multiple possible causes of environmental scarcity, including constrained agricultural productivity, migrations, and social segmentation, can interact in unanticipated ways with unexpected contingencies to complicate the paths to conflict and create new branches. What is critical is a well-specified set of indicators that can track "evolution."

These putative causal linkages are "emplotted" storylines that can be analyzed in particular cases[17] but require sensitivity to feedback, interventions, surprises, or "wild cards," and the recognition that other drivers are equally plausible. Alternative scenarios should even consider different causal logics that stem from the same drivers. For example, a growing body of empirical work suggests that resource abundance in poor countries, not scarcity, can lead to conflict because it can "motivate rapacious behavior and allow the finance for civil war. . . . It has been common knowledge that many of today's most durable conflicts, such as Angola, Liberia, the Democratic republic of Congo, Sierra Leone, etc. are fuelled by the struggle for control of oil, diamond mines, timber and other resources" (de Soysa, 2002: 7). Refinement and validation of hypotheses is unlikely, for reasons that we have made clear, to produce a definitive causal story that can be stated as a deductive explanation of a law. Similarly, no generic or off-the-shelf strategies of intervention or assistance are likely to prevent trajectories that appear to be moving down the path toward conflict. But a forward-reasoning approach could assist leaders. Context-specific scenarios could provide early warning of dangerous trends and sensitize analysts to local contingencies. Leaders could become aware of the plausibility of more than one future, design strategies of intervention and test these strategies for robustness and adaptability against different scenarios.

Nuclear Proliferation

The unexpected nuclear tests in India and Pakistan in the spring of 1998 quickly altered the security environment in South Asia and beyond. While proliferation of nuclear weapons—or weapons of mass destruction, more broadly—has neither been as uncontrolled or as limited as pessimists or optimists predicted, the explosions in South Asia highlight the importance of contemplating multiple causal pathways and multiple implications in the face of uncertainties and "wild cards."

The nuclear tests pose serious conceptual and policy challenges (Stein, 2001). Many causes of proliferation have been suggested. While strategic environments matter—no state without serious enemies has proliferated—many states with enemies have not (Argentina, Brazil) while others have relinquished nuclear weapons (South Africa, some former Soviet Republics). There is a diverse set of explanations of why states choose not to develop weapons: the effective use of carrots and sticks by major powers; the power of "taboos" on weapons of mass destruction; and decision-makers' specific calculations of whether the risks from increased security dilemmas or being a possible target of preventive war outweigh the possible gains of nuclear weapons. In addition to explanations that focus on external calculations, domestic political factors increasingly appear important as well. For example, Etel Solingen (1994, 1995) argues that liberalizing domestic coalitions, as opposed to more nationalistic or fundamentalist coalitions, are more likely to favor nuclear disarmament in order to strengthen the international economic ties on which they rely.

Separate from the puzzle of proliferation itself are the competing analyses of the consequences of proliferation, both regionally and globally. A number of broad-brush scenarios of possible futures already exist in the literature. In the 1960s, Herz (1968) wrote of "neoterritoriality," a future in which sovereign states recognize not only their interests in mutual respect for each other's independence but also the need for extensive cooperation. Herz argued this kind of cooperation would become possible when the danger of nuclear destruction made all people and societies on the globe recognize their interdependence and their common fate. Interestingly, this scenario was a revision of his earlier argument that the nation-state would decline with the advent of nuclear weapons technology (Herz, 1957). The evolution of Herz's thinking is very much consistent with the forward-reasoning approach we propose: he recognized that the causal driver of nuclear technology produced unanticipated consequences and interacted with nationalism and state legitimacy in unanticipated ways. The outcome was retrenchment rather than demise of territoriality.

Following a similar logic that may resonate even more as proliferation progresses, Deudney (1995a, 1995b, 2000) has presented a functional theory of how the international system might evolve into a global "Philadelphia System," similar to the governance arrangement that he argues prevailed in pre–civil war United States, 1787–1865. He calls this "negarchical" republicanism, residing between anarchy and hierarchy, where certain functions such as the control of nuclear arsenals might be embedded in cooperative institutions or multiple actor command systems while territorial units might maintain authority over other functions.[18] Although Herz's and Deudney's scenarios appear far distant in the future, creative and disciplined thinking of this kind pushes forward the conceptualization of causal drivers that might lead to these outcomes and helps us to assess if and when we are on such a path. It would be worthwhile for Deudney to build in additional drivers and uncertainties to assess factors that open up or close off ways to get to preferred futures. Deudney might be able to argue that such a "negarchical" republican response is functional (and rational) for human survival, but that does not mean it will occur. What are the links between our world and Deudney's future? Are international organizations such as the International Atomic Energy Agency developing the capacity or legitimacy to play the role that would be necessary for such a future to unfold? What would have to occur for established nuclear powers to give up full independent control?

There are also nuclear optimists such as Waltz (1981) and Mearsheimer (1990) who argue from neorealist premises that proliferation will produce greater stability in a multipolar world. More nuanced studies also propose starkly different scenarios, as the debate on the pages of *International Security* between "optimists" and "pessimists" on the effects of proliferation attests.[19] It is worth noting that with the exception of the apparent certainty displayed by Waltz and Mearsheimer, both optimistic and pessimistic scholars recognize that "nuclear strategic logic is occasionally indeterminate or at least multi-faceted, and . . . that many factors determine nuclear behavior" (Feaver, Sagan, and Karl, 1997: 186). Pessimists and optimists seem to agree that deductive theories based on the cold war experience are unlikely to apply as proliferation—or nonproliferation— proceeds. Sagan, a "pessimist" about the effects of proliferation, harshly criticizes early post–cold war scholarship because it was dominated by "purely deductive arguments based on the logic of rational deterrence theory [that] eschewed the kind of historical research that is necessary to test theoretical arguments about the strategic effects of nuclear weapons" (Feaver, Sagan, and Karl, 1997, 193). Similarly, Feaver notes the need to supplement theoretical reasoning about U.S. or Soviet strategic behavior during the cold war, with "attention to causal relationships that drive the real-world behavior underlying observed outcomes." Careful attention to contingency in context allows the drivers of Soviet and American behavior during the cold war—domestic politics, cognitive traps, trade-offs inherent in command-and-control—to be embedded in scenarios of future proliferation, but "in some cases, revised as new data becomes available" (Feaver, Sagan, and Karl, 1997: 186). Karl, a critic of Sagan and Feaver, stresses the need "to go beyond rote arguments over whether proliferation is good or bad and undertake empirical investigations into the actual behavior of new nuclear powers" (Karl, 1996/97: 119).

Scenarios of the consequences of proliferation can make use of and build in a very helpful tool—competing game theoretic models—to identify cryptic and possibly critical dynamics of an important real world problem. Multiple nuclear powers with potentially opaque nuclear strategies and uncertain command-and-control systems are unlikely to operate as the simplest classical models predict. Scenarios might also highlight the different factors on the path to preventive war or military strikes in potentially unstable regions such as the Middle East. Would an Israeli air strike against nuclear facilities in Iran today produce the same muted response as did the strike on Iraq in 1981? This is not a question that could be answered by analogy or inference from any general theoretical understanding of international relations. Playing out different scenarios through different game-theoretic models might highlight dangers of alternative strategies, unexpected consequences under different contingencies, and opportunities to reduce tensions.

Privatization of Security

Plotting trends is not simply a matter of identifying multiple drivers. At times, it may involve detecting a shift in the conceptual terrain itself. When the conceptual terrain does shift, new understandings of international relations make nomothetic deductive theories all the more problematic. A growing body of international relations scholarship has pointed to the shifting ground of sovereignty (e.g., Biersteker and Weber, 1996; Strange, 1996; Ruggie, 1993; Kratochwil, 1995; Caporaso, 2000). Identification of this conceptual shift has, however, had very little impact on mainstream "scientific" theories, largely because analysis of "international conflict" rests on a Weberian conception of the state as the monopolizer of force.

The capacity to provide security as a public good to citizens has been both constitutive and defining for the modern state.[20] It has been constitutive insofar as war-making by the state directly and indirectly expanded its capacity to provide other public goods at home, and it has been defining in the sense that citizens gave their loyalty to the state as their most important shield (Tilly, 1975). The most far-reaching implications for the future of international relations stem from the possibility that this understanding of the state no longer applies. This reformulation of the role of the state suggests that private security, supplied by the market, grows in relative importance, to public security supplied by the state.

Drivers of such a trend can already be identified. For example, in many parts of the world, fear of nuclear and even conventional war has declined precipitously. Citizens, no longer seized with the fear of nuclear war, have begun to think beyond physical security and to shift their agendas from the public to the private. Herz's and Deudney's propositions also drive in the direction of disaggregation of the security function of the state. Additional drivers include the effects of global markets, which put pressures on states to disengage as providers of other public goods; the state becomes instead a regulator of the rules of the game or a supplier of competitive advantage.

New identities proliferate in such an environment. As security from attack abroad becomes less of a preoccupation than it has been at any time in recent historical memory, the situational triggers that traditionally activate and affirm identification with the state are likely to decline in frequency. The disaggregation of security, when combined with the rise of an elaborate set of supranational institutions, may further disengage people from their connection to the state. If the state is not the only supplier of security, its command of the loyalties of its citizens that separate them from the "other" in different countries may diminish.

Evidence already exists that drivers along this path to privatization are active. For example, the capacity of the state to protect its citizens at home has also declined, although it has declined in different regional spaces for quite different reasons. In the United States, the rise of "gated" communities with private security systems contained behind walls is quite remarkable. Many large institutions—banks, schools, hospitals, and universities—now use private security forces to secure their local populations. It is not the state that secures its more privileged citizens from violent attack, but privately organized and financed security systems that are available in the market. Even public security providers are being contracted to the private sector to augment budgets. At the extreme, in Moscow, for example, private suppliers of security serve organized crime even as the capacity of the state to protect its citizens crumbles. Such trends have wide implications. Private markets for security over the long term will advantage the affluent and diminish identification with the state across social boundaries. While borders of states become less important, divisions within society may deepen if markets rather than states provide security. Political identities will be reshaped over time by the declining importance of state borders, and the growing importance of boundaries for private security markets.

The privatization of security is not restricted to the emergence of markets to supply the needs of the affluent within postindustrialized societies. In the wake of the end of the Cold War and the decline of empire, the major powers have progressively disengaged from regions they no longer consider of central strategic interest. Caught in a security vacuum, weak states have fragmented, not only in parts of Africa, but also in the former Soviet Union and Latin America as well. In Colombia, the state military, private paramilitary forces, and several guerrilla organizations compete to provide contracted protection to multinational corporations. Some of these fragmenting states no longer have the capacity to provide security for their populations; on the contrary, civilian populations are deliberately targeted by competing militias that supplant the forces of the state.

As state capacity to provide security declines, and international institutions retreat from the challenge, private suppliers of security increasingly fill the gap. At times they are contracted by international institutions, more often by states who seek to augment their capacity to coerce their own populations, and at times by nongovernmental organizations who seek access to insecure and desperate populations that are being systematically victimized by predatory militias. Private security markets are expanding in the shadow of fragmenting states and unwillingness by the major powers and international institutions to supply security as a collective good (Stein, 2000).

The privatization of security, if it continues to widen and deepen, is likely to reshape the role of the state and shift political identities in global political space. The state, no longer the exclusive supplier of security, becomes one among several focal points of political identity. Borders, no longer the only or even the most important shield against attack, are likely to become increasingly less important as anything but a juridical divide between states, while boundaries—cultural and social divisions among spaces—drawn by private security markets are likely to become more important. These boundaries will not be as stable as state borders were in the twentieth century, because they are constructed out of market allocation not political authority. Nor will private purveyors of security be the focus of the kind of political loyalty that states were able to command.

We are not suggesting that this future will come to pass, or that it is the only plausible future. We must consider, for example, that the events of September 11, 2001 may represent a branching point. Borders appear again to be increasingly important, states are reasserting their security function at home and abroad, and international institutions in the security realm appear under strain. Yet, the underlying driving forces identified above have not disappeared. Even in the post–September 11 context, state militaries continue to "contract out" a number of functions, and supranational organizations appear increasingly necessary in conflict zones such as Afghanistan and postwar Iraq, the frontlines of the "war on terrorism." The "wild card" of September 11 provides an opportunity to construct alternative plotlines. Will its effects simply be "dampened out?" How will the interaction of a predetermined element not considered in our original formulation—the networking of private transnational terrorism and its link to radical religious movements—interact with technological change as described above? These interactions can be the basis of alternative plotlines.

Rather than prediction, laying out such a scenario and its alternatives encourages students of international affairs to consider a range of drivers, to identify the critical uncertainties, to develop different plotlines by varying these uncertainties, and to develop indicators of different paths to monitor trends as they unfold. Just as counterfactual analysis is a useful tool for evaluating the strength of competing explanations and recognizing the contingency of outcomes that actually occurred, forward reasoning opens our analyses to the possibilities of alternative futures but forces discipline in tracing likely paths created by important drivers in combination with significant uncertainties.

This analysis of plausible futures suggests that the nature of the units, identities, and key characteristics of the system can change. At least two of the trends we have identified—continued increases in intrastate conflict and privatization of security—suggest the need to reconceptualize the basic units of analysis as identities and the nature of human agency change. Some scholars have begun this reconceptualization of political actors. Ferguson and Mansbach,[21] for example, note that polities command loyalties of individuals and groups, but they hasten to add that the sovereign state is only one of the many forms and identities polities have taken over the ages. Multiple identities— whether ethnic, national, religious, professional, class, or ideologically based—and competing pressures for people's loyalties are nearly always present. This kind of reconceptualization of political actors and identities provides another starting point for analysis otherwise closed off by deductive theories that posit relationships between given (usually state or national) actors. Different scenarios can be developed using particular conceptualizations of polities or actors as starting points, with analysis of critical uncertainties folded into different paths and plausible outcomes. These scenarios can be monitored along with more "conventional" scenarios to assess where unfolding events fit best and where the "storyline" needs to be adjusted. Using feedback from unfolding events, we can develop better and more compelling narratives of the future as we proceed through the present.

Conclusion

Newtonian physics conceived of a world of clock-like regularities that could be discovered through deductive theory and empirical research. Prediction was a reasonable goal because many of the phenomena studied by 18th- and 19th-century physicists were the result of a few easily measurable forces or of interactions among an extraordinary large number of units that gave rise to normal distributions. Neither of these conditions prevail in international relations—or in much of modern physics.

Evolutionary biology is shaped by a multitude of forces and by quasi to fully random events whose interaction cannot be modeled. Evolutionary biologists do not aim at prediction but instead have focused their efforts on developing theories that explain the process and history of evolution. They have met with considerable success.

We believe that international relations is closer in its basic nature and amenability to scientific study, to evolution than it is to mechanics or fluid dynamics. Like evolutionary biology, most kinds of prediction in international relations are impossible. Theories of structure and process—if we had robust theories—would fail to capture some of the most critical factors responsible for political outcomes because, as in evolution, they would lie outside any of the theories.

Scenario-based forward thinking is a promising method for tracking the policies of actors and the evolution of the international system. Scenarios allow researchers to combine general knowledge of politics with expert knowledge of individual actors and situations, to build in context, complexity, variation, and uncertainty in the form of multiple narratives with numerous branching points, and to revise their expectations as events unfold. Repeated iterations of this process can reasonably be expected to improve the quality of our general knowledge of international relations, our ability to track specific developments and the outcomes that result, and our capacity to address the problems that these evolutionary tracks create.

Why scenarios? First and foremost because theorists and policymakers both need constructive ways to think about the future and parse out the uncertainties in an inherently unpredictable setting. This is necessary for intelligent action, but also for progressive improvements in theory-based understanding of world politics. The future is not predictable. Acknowledging the limits on prediction forces a theorist concerned with the biggest questions in social science to deal first with the boundary conditions around any argument with efficient causes. As the recognized boundary conditions become more restrictive, which they are likely to rapidly do, contingent and complex causality comes to the fore.

Second, we propose scenarios because econometric models and logic cannot accommodate sharp discontinuities. Qualitative uncertainties—particularly uncertainties about the fundamental rules of the game or institutional structures—require a different type of thought process and evidence collection. Certainly there are theorists of international relations who maintain that there have been few sharp discontinuities in world politics over the last 300 years, but that position seems increasingly untenable to most. There are huge risks, theoretical and practical, in attempts to fit incoming evidence to existing theoretical paradigms when qualitative discontinuities may be present. Scenarios are one way to balance that risk.

A third reason to use scenarios is to provide a common vocabulary that helps to clarify the nature of disagreements. We have found in our work that a group of theorists generating scenarios about the future of the Middle East peace process divided along two dimensions of disagreement: contingent disagreements and fundamental disagreements.[22] Fundamental disagreements are the result of basic, almost primordial beliefs about the world and the nature of politics. These are probably irreconcilable by evidence. Contingent disagreements are the result of differences in beliefs about the boundary conditions under which certain relationships hold. Contingent disagreements can be gently pushed toward resolution with careful quasi-experimental research designs, but they first need to be identified. One of the key findings of our scenario process was that disagreements that theorists took at the start to be fundamental, were often revealed later in the process as contingent. This is an important, if small, step on the road to cumulation.

Finally, scenarios are useful because the theoretical study of international relations needs new ideas and arguments just as much as it needs to test existing ones. We are not opposed to the disciplined, precise evaluation of hypotheses and theories that are adequately developed so that they are ready for this kind of treatment. We are concerned about a search for false certainty and about the relatively trivial nature, and lack of policy relevance, of many "big" generalizations. Scenario thinking, obviously, is not a panacea for this problem. It is a complementary toolkit that has promise for generating new ideas and arguments, broadening the range of causal relationships that we study, and tracking the evolution of world politics through periods of discontinuous change, in ways that promise to better over time both understanding and action.

Notes

1. We use evolutionary biology as an analogy for modes of reasoning, not as a model of politics per se.
2. We state the rule in this way to avoid the confusion of "affirming the consequent" (as in if X then Y) and thus to emphasize falsifiability.
3. See Weber, "Counterfactuals Past and Future," in Tetlock and Belkin, eds., 1996.
4. For elaboration, see Morris, 1998.
5. George, 1993 makes a similar point.
6. Carlsnaes, 1992, 1993 has made a similar argument.
7. See, for example, Modelski and Poznanski, 1996, and other contributions to the September 1996 special issue of *International Studies Quarterly*.
8. See "Introduction" in Tetlock and Belkin, 1996.
9. Searle, 1995, defines social facts as those facts produced by virtue of relevant actors agreeing that they exist. See also Ruggie 1998.
10. For an effort to save the "scientific" explanation while doubting the usefulness of general laws for explaining social phenomena see Elster, 1989. See also Brown, 1984.
11. See, for example, King et al., 1994: 19, 28–29.
12. For a similar discussion of "causality" embedded in a narrative explanatory protocol, see Ruggie, 1998: 89–94.
13. For how to construct scenarios, see Weber, 1997 and Stein et al., 1998.
14. For a classic treatment of ethnic conflict see Horowitz, 1985.
15. For applications of various causal theories to the post–Cold War wave of ethnic violence by international relations scholars, see the series of articles in the Fall 1996 issue of *International Security* (Snyder and Ballentine, 1997; Lake and Rothchild, 1997; Ganguly, 1997; Kaufman, 1997).
16. For a discussion of feedbacks and unintended consequences of interventions in the former Yugoslavia see Pasic and Weiss, 1997.
17. Polkinghorne (1988: 19–20) uses the literary term "emplotment" to describe causation embedded in narrative: "It is not the imposition of a ready-made plot structure on an independent set of events; instead, it is a dialectic process

that takes place between the events themselves and a theme which discloses their significance and allows them to be grasped together as parts of one story." Cited in Ruggie, 1998: 94.

18. Wendt (2003) goes further, proposing a teleological explanation for an inevitable world state. His logic follows in part on the same technological argument concerning the increasing capacity for devastation of military technology, but he also introduces an argument that the logic of anarchy will channel struggles for recognition. While such a well developed plotline fits nicely with our scenario methodology, the functional and teleological logics on which Deudney's and Wendt's arguments are based run counter to our approach.
19. Feaver, Sagan, and Karl, 1997. On the general debate between optimists and pessimists, see Sagan and Waltz, 1995.
20. For an analysis of the privatization of security and its consequences, see Stein, 2000.
21. Ferguson and Mansbach, 1996. See also Hall, 1998.
22. See Stein et al., 1998.

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10

Theory and Evidence[1]

Mark Irving Lichbach

In their debate on neopositivism, while Kratocwil danced with relativism, Pollins stressed the value of positivism to qualitative researchers, and Hopf recognized the importance of making interpretations more rigorous. Listening to their stimulating debate, Chernoff tried to clarify the nature of naturalism, and Waldner probed the idea of causal mechanisms. Trying to understand the implications of the debate for social science practice, Lawrence subjected the empirical claims of the democratic peace literature, and Levy international relations research programs more broadly, to scrutiny.

Bernstein, Lebow, Stein, and Weber summarize and extend these analyses by making a plea for case-based reasoning. We need to understand these debates in terms of three principles of the traditional positivist philosophy of science.

1. *Theory* is deductive-nomological: it begins as abstract, axiomatic, and foundational; it becomes subsuming, integrating, and unifying; and it ends as organized, comprehensive, and encyclopedic.
2. *Evidence* is oriented toward falsification: scientists attempt to reject a hypothesis; after one possible explanan is discarded, they investigate another to see if it can account for the explanandum.
3. *Evaluation* is therefore based on deductive and nomological laws that resist falsification: these laws establish the ever-expanding domain of a theory; science therefore succeeds when it discovers universal laws that are true.

This philosophy of science might have suited social scientists a few decades ago. Today's more modest philosophy of science that consists instead of three different principles.

1. *Theory* consists of research programs that contain nuts and bolts; these causal mechanisms are combined into models of a theory that suggest lawful regularities.
2. *Evidence* establishes the applicability of these models of a theory for the models of data that exist in particular domains; the elaboration of a theory thus delimits the theory's scope.
3. *Evaluation* grapples with the problem that the science that results from following the first two principles is prone to nonfalsifiability and to self-serving confirmations. Confrontations between theory and evidence are thus evaluated in the context of larger structures of knowledge.

This final chapter moves the debate (Lichbach 2004) forward by dealing with the problem of evaluation. For pragmatists who work with a thin version of one paradigm, Lakatos's (1970) "additional and true" standard, which lets them explore rationalist, culturalist, and structuralist approaches on their own terms, is applied. For competitors who employ alternative paradigms, Popper's (1968) "different and better" standard, which lets them conduct competitive evaluations among alternative rationalist, culturalist, and structuralist explanations, is employed. And for hegemons who synthesize the different paradigms into one thick paradigm, "nested models" that combine the two standards, and thus lets them compare syntheses to their components (models and foils), is used.

Evaluation

Hirschman (1977: 117) recounts the following: "In an old and well-known Jewish story, the rabbi of Krakow interrupted his prayers one day with a wail to announce that he had just seen the death of the rabbi of Warsaw two hundred miles away. The Krakow congregation, though saddened was of course much impressed with the visionary powers of their rabbi. A few days later some Jews from Krakow traveled to Warsaw and, to their surprise, saw the old rabbi there officiating in what seemed to be tolerable health. Upon their return they confided the news to the faithful and there was incipient snickering. Then a few undaunted disciples came to the defense of their rabbi; admitting that he may be wrong on the specifics, they exclaimed 'Nevertheless, what vision!' "

Are scientists different? Physical and biological scientists who cling to the heuristic power of their "vision" of the world often fit models of their theories to the world so that they might "save" the phenomenon in question. Curve-fitting—aligning theory and observation—is easy, and Davis and Hersh (1981: 75) describe the classic scientific parallel to the disciples of the rabbi of Krakow:

> In the Ptolemaic system, the earth is fixed in position while the sun moves, and all the planets revolve around it. Fixing our attention, say, on Mars one assumes that Mars circled about the earth in a certain eccentric circle and with a certain fixed period. Compare this theory now with the observations. It fits, but only partially. There are times when the orbit of Mars exhibits a retrograde movement which is unexplainable by a simple circular motion. To overcome this limitation, Ptolemy added to the basic motion a second eccentric circular motion with its own smaller radius and its own frequency. This science can now exhibit retrograde motion, and by careful adjustment of the radii and the eccentric periods, he can fit the motion of Mars quite well. If we require more precision, then a third circle of smaller radii still and a yet different period may be added.

The Duhem-Quine thesis explains why such things happen in science and hence why science can be no different than religion: when a theory is found wanting, it is not clear what has gone wrong. Scientists can thus use ad hoc explanations, post hoc adjustments, and tautologizing alterations to immunize their theory from falsification by inaccurate predictions. Analysts, eager to prove their pet theories correct, ignore the facts and instead turn to these fudged factors: arbitrary domain restrictions, empty prevarications, face-saving linguistic tricks, and exception barring. Scientists who claim to know the cases before they see them eventually interpret cases in terms of theory, conflate evidence and generalizations, and equate the empirical and the analytical. Even when they consider plausible rival hypotheses, such scientists often engage those other theories on their own terms, carefully privileging their own theory by setting up their opponents as a straw figure.

Rapoport (in Weintraub 1985: 35) thus argues that

> mathematically, you can cook up anything. You can imagine any sort of situation and represent it by a mathematical model. The problem becomes that of finding something in the real world to fit the model . . .
>
> There was a man who liked to fix things around the house, but the only tools he could use were a screwdriver and a file. When he saw a screw that wasn't tight, he tightened it with his screwdriver. Finally, there were no more screws to tighten. But he saw some protruding nails. So he took his file and made grooves in the caps of the nails. Then he took his screwdriver and screwed them in. To paraphrase Marshall McLuhan's famous remark, "the medium is the message," the mathematician could well say, "the tool is the theory."

Rule (1988: 86) agrees: “For many thinkers, seeing one’s theory ‘fit’ (any slice of reality that catches his/her fancy) is reason enough for acceptance of the theory, indeed for preferring it to others. But . . . we need a more rigorous standard. If one embraces a theory on the grounds that it ‘fits’ evidence that might as well support a version of other theories, the choice is more a statement of one’s own inner world than about a shared exterior one.” A determined theorist can always locate supporting illustrations and rationalize belief in the face of contrary evidence by reinterpreting an appropriate set of stylized facts. As Tolstoy (1968: 771) caricatured the point: “He was one of those theoreticians who so love their theory that they lose sight of the theory’s object—its practical application. His passion for theory made him despair all practical considerations and he would not hear of them. He positively rejoiced in failure, for failures resulting from the theory only proved to him the accuracy of his theory.”

If physical and biological scientists are eerily like the disciples of the rabbi of Krakow, social scientists may be worse. Consider rational choice theory. Some economic theories are comprehensive, unified, and elegant: Arrow-Debreu general equilibrium theory is the most prominent example. Other economic theories are frameworks or toolboxes that organize our thinking by housing many different models for analyzing various problems. Dixit (1996: 35) suggests that oligopoly theory is such an example:

> There are so many different issues that arise in the study of competition among a small number of firms that there is no hope of constructing a single analytical model of oligopoly on par with the standard elegant model of perfect competition. However, most people would agree that oligopoly remains a useful conceptual umbrella for sheltering the large variety of models that examine specific issues such as tacit collusion, strategic commitments, and preemption.

Arrow (1987: 226)—the general equilibrium theorist—identifies a problem with this perspective:

> I think there is a tendency in . . . this methodology to say, “here is a particular problem, I will make a set of assumptions, and here are the consequences; ah! yes in this case they worked out well.” But I say, if these assumptions are true, they should be true for the next problem. In other words, there is a tendency to look only at the consequences that one happens to be studying at that moment, and not asking whether these assumptions can imply something quite different, whether they can be used in another field. In other words, it is not enough to test the assumptions in one field, one has to test them in others as well—something that Popper, for instance, would insist on.

A related foible in economic methodology is what Schumpeter (1954: 472) refers to as the Ricardian vice: strong exemplar cases and self-evident truths lead to a theory that assumes too much and produces tautologies, yet is applied to the world to make theoretical and policy inferences (Pheby 1988: 17). Green and Shapiro (1994; 1996) locate another related problem: rational choicers use a style of theorizing that advocates arbitrary a priori and a posteriori domain restrictions, so when their theory does not fit a case, the case is not treated as a disconfirming instance. Rational choice theorists, in other words, treat ceteris paribus conditions as "open ended escape clauses" (Kincaid 1996: 63).

The twin dangers of self-serving confirmation and nonfalsifiability bedevil rational choice theorists for yet another reason: their models are often deliberately heuristic rather than realistic. They may have, that is, instrumental value in probing the world rather than intrinsic value in mirroring it. Suárez (1999: 174) draws a valuable distinction between the two approaches to approximating theory to the world:

> They are, broadly speaking, two methods for approximating theory to the world. One is the approximation of the theory to the problem situation brought about by introducing corrections into the theoretical description—the theory is refined to bring it closer to the problem-situation . . . this is a form of approximation toward the real case: the corrections introduced into the theoretical descriptions are intended to account for the imperfections that occur in the problem situation.
>
> The other is the approximation of the problem-situation to the theory by means of simplifications of the problem situation itself . . . We idealize the description of the problem-situation, while leaving the theoretical construction unaffected . . . this process can come in either of two forms. It can come first in the form of conceptual redescriptions of the problem-situation, performed only in thought, and not in reality. In such "thought-experiments" complications are idealized away and the result is a simplified description of the problem-situation. Secondly, there is also the possibility of physical "shielding" of the experimental apparatus.
>
> In the latter case the theory is left untouched, while the problem-situation is altered; in the former case the converse is true: the problem-situation is left untouched, while the theoretical description is corrected.

Such approximations to a problem-situation may have great heuristic value but at the cost of realism.

In other words, a rationalist model might not approximate the world at all. Indeed, it might be counterfactual to the average occurrence of reality (Reuten 1999: 198). Gibbard and Varian (1978: 667, 676) suggest that economic models often deliberately distort in order to emphasize some part of reality. They (1978: 665, 673) thus speak of caricature models that present "even to the point of distorting—certain selected aspects of the economic situation . . . Often the assumptions of a model are chosen not to approximate reality, but to exaggerate or isolate some feature of reality." Mayer (1993: 126) comments that "the value of such a model is that it brings out an important feature of the economy that was previously not given enough attention, and that it is robust with respect to the exaggerated assumptions." Modeling in economics is thus often heavily dependent on interpretive context, emphasizing significant features of the world and not describing it as a whole.

Other social scientific research communities also face the twin dangers of self-serving confirmation and nonfalsifiability. With respect to the culturalists, Durkheim often said "the facts are wrong" when confronted with evidence that contradicted his theories (Lukes 1985: 33, 52). With respect to the structuralists, the explanatory modesty of structuralists regarding the scope of their "historically concrete" arguments comes down to the assertion "that the theory should apply only where the evidence happens to fit, while instances of discordant evidence should simply be ignored" (Rule 1988: 71). While a theory with a limited domain is common in science, one that "holds only for certain special cases is not very exciting *unless* we can specify in advance what those cases will be. Perhaps what Weber said of the materialist theory of history holds for theories in general: They are not conveyances to be taken and alighted from at will" (Rule 1988: 89, emphasis in original). Hence, the Marxist theory of revolution is often saved by lengthening the time span: the revolution is *always* coming.[2]

Thus social science—whether practiced by rationalists, culturalists, or structuralists—often displays "built-in justification" (Boumans 1999) that produces the ex post validation of ad hoc modifications of failed theories. In other words, if a social scientist acts as if he or she can neither accept nor reject a theory, but rather acts as if the boundary, scope, or domain of the theory is defined in its application and elaboration, there is a danger of self-serving confirmations and nonfalsifiability. Unless scientists have a way to evaluate the application and elaboration of a theory, a science consisting of models of a theory (nuts and bolts) that mesh with models of data to explain particular problem domains is no science at all.

Why is one story preferable to another? How do we know when we have improved an existing story? What does a set of stories generated from one or more approaches tell us about the approaches?[3] Unless social scientists can separate real knowledge from mere opinion, every social scientist can claim that his or her story is guided by data and evidence and that his or her opponents' stories, too invested in misguided theories or ideas, are driven by dogmatic beliefs. Social science therefore needs criteria of theory appraisal that stand somewhere between positivism and relativism.

To clarify the problem of induction: empirical inquiry faces two fundamental and interrelated logical difficulties. First, facts, data, or observations are overdetermined by theories, hypotheses, or propositions and the supply of plausible rival hypotheses that can fit the same body of evidence is in principle infinite—social scientists, after all, can readily invent different theories to explain the same piece of reality (e.g., lots of causes of capitalism in the West or of economic success—and now failure!—in Japan). Hence, there are several alternative and incompatible ways to account for the facts: one can explain 100 percent of the variance in more than one way, divide the sample space with more than one approach, and locate multiple paths to the same outcome.

Second, theories are underdetermined by empirical evidence. On the one hand, we cannot conclusively verify theories or prove them true. The fallacy of affirming the consequence means that there will always be the possibility of committing a Type I error—rejecting a true null hypothesis. On the other hand, we cannot conclusively falsify theories either. The Duhem-Quine problem of auxiliary hypotheses means that we test theories as whole; if a test fails we do not know whether the test hypothesis or an auxiliary hypothesis is false. Hence, the probability of committing a Type II error—accepting a false null hypothesis—also cannot be reduced to zero. Any theory thus can be reconciled with some evidence.

The implications of these twin problems of induction run deep. Hume ([1739] 1984: 189, emphasis in original) writes that "*we have no reason to draw any inference concerning any object beyond those of which we have had experience*." King, Keohane, and Verba (1994: 79) offer a modern restatement: "We can never hope to know a causal effect for certain." In other words, extrapolation from experience, from the present case to another case, is never completely justified. Probabilistic and nonprobabilistic theories of confirmation and falsification (Howson 2000) and the very notion of verisimilitude (Brink 2000) have deep philosophical problems. And there are even empirical grounds for this skepticism about empiricism. The pessimistic induction (Newton-Smith 1981: 14)—"any theory will be discovered to be false within, say, 200 years of being propounded"—or alternatively, science is "one damn theory after another" (Rouse 1987: 4), seems to fit the history of science quite nicely.

We are left with Maher (1993: 218): "The history of science is a history of false theories, and yet we want to say that science is making progress." Nagel (1953: 700), hoping to overcome Hume's problem, thus writes that the "basic trouble" with the philosophy of science is that "we do not possess at present a generally accepted, explicitly formulated, and fully comprehensive scheme for weighing the evidence for any arbitrarily given hypothesis so that the logical worth of alternative conclusions relative to the evidence available can be compared." The twin problems of induction tell us that just like there can be no methodical routine for doing creative work (i.e., a master science of discovery), there can be no inductive science of justification either. Nagel's (1953) hope for a unique scientific method for making the uniquely rational choice among the limitless number of contending theories, and hence a method that could explain and justify the change in scientific theories, is an impossible dream: The search for a computer program that can conclusively decide between competing theories is a chimera; the search for Algorithor, the philosopher of science who discovers the one true method, cannot succeed (Newton-Smith 2000: 4); and the search for "the methodologist's stone" (Newton-Smith 1981: 77) is fruitless. Scientists cannot quantify "degrees of confirmation," in effect "adding the weight" of many studies so that propositions are established "more or less." There is no algorithmic way to assess verisimilitude: how one hypothesis is close to the truth or closer to the truth than is another hypothesis.

This then is the fundamental indeterminacy of empirical work: important questions can not be entirely arbitrated by the sciences of deductive and inductive logic.[4] Logical empiricism, the positivist, received, or syntactic view favored by rationalist hegemons, overestimated the power of formal logic and measurement strategies to clarify the nature of theoretical claims.

So we must ask: How can observational data give us reasons for accepting or rejecting[5] a hypothesis that transcends the data? What principles can scientists use to weight evidence and make inferences that allow them to accept hypotheses that are true and to reject hypotheses that are false? If we cannot answer these questions, the disciples of social science are no better than the disciples of the Rabbi of Krakow.

Following van Fraasen (1980), "constructive empiricism" does not aim at "true" theories but only aims at empirically adequate theories—everything it says about observables is "true." As best as they can, scientists rely on judgment to establish that a model of a theory is consistent with a model of the data. Newton-Smith (1981: 232) thus writes that

> a practicing scientist is continually making judgments for which he can provide no justification beyond saying that is how things strike him. This should come as no surprise in a post-Wittgensteinian era. Wittgenstein repeatedly drew attention to the fact that we cannot specify usable, logically necessary and sufficient conditions for the application of many commonly employed predicates.
>
> The time has come to model at least some aspects of the scientific enterprise not on the multiplication tables but on the exercise of the skills of, say, the master chef who produces new dishes, or the wine blender who does deliver the goods but who is notoriously unable to give a usable description of how

> it is that he does selection the particular portions of the wines that add up taste-wise to more than the sum of their parts.

Since it is so difficult to assess the epistemic value of a theory, scientific judgment involves "beauty" and "justice" in addition to "truth" (Lave and March 1975). Pragmatic or aesthetic values, including consistency, parsimony or simplicity, and fruitfulness, fertility, scope, or unifying power, thereby enter science.

Let us focus here, however, on empirical criteria of theory evaluation.[6] Since facts are overdetermined by theories and theories underdetermined by facts, something else must determine our choice of explanatory theories. Absolute standards of theory evaluation are not available, so relative ones must be found. I therefore offer a broader approach to evidence that focuses on larger structures of knowledge. For those pragmatists who work within one paradigm to fit its models of theory (nuts and bolts) to models of data in a new problem domain, Lakatos's (1970) additional and true standard should be used; for those competitors who use the nuts and bolts that come from alternative paradigms, Popper's (1968) different and better standard is appropriate; and for those who synthesize different paradigms into one favored paradigm, nested models that combine the two standards are relevant.

Lakatos

The philosopher of science Imre Lakatos (1970) proposes a standard for evaluating a single research program. He suggests that scientists characterize each modification of a research program (i.e., an attempt to apply a program's nuts and bolts to a new problem domain) as "progressive" (1) if it can account for previous findings; (2) if it can predict "novel content" or some hitherto unexpected or counterintuitive observations; and (3) if some of these excess predictions resist falsification. A modification of a research program is "degenerative" if it is merely patchwork to explain an internally generated anomaly of the program and offers no new substantive insights into the new problem domain. Degenerative programs are, accordingly, autonomous and self-perpetuating, farther and farther removed from reality.

Consider an example from the rationalist approach to collective action. Lakatos's additional and true criteria ask the following: What, besides that protest groups do form and that their participants are rational, does the collective action research program tell us about a new case of collective dissent? Each new application of an existing solution to the free-rider problem must tell us something additional and true about the protest.

Solutions are potentially rich in their implications, focusing as they do on the group's actions (e.g., rhetoric, deeds), internal organization (e.g., membership characteristics, entrepreneurs), and external relations (e.g., competition with enemies such as the regime, cooperation with allies such as patrons). Showing that the collective action research program can tell us more about a new conflict than simply that rational people rebel demonstrates the heuristic value of the approach. It reveals the range of observations or the multiple outcroppings (Webb et al. 1981: 66–68) about conflict that the approach can explain. And it enables us to take a fresh look at existing theoretical arguments and empirical evidence.

Consider, for example, the selective incentives idea. The application of this solution to the Rebel's Dilemma could reveal many stylized facts about a new instance of protest or rebellion:

1. Rioters typically loot stores.
2. Voluntary members of a dissident group often attempt to become paid staff and make a career out of their participation (i.e., over time protest is professionalized).
3. Long-lived dissident organizations usually become oligarchical, with leaders receiving the majority of the benefits.
4. Government commonly co-opts leaders and "buys off" followers, and thus long-lived dissident organizations regularly become deradicalized.
5. Organizing manuals written by protest leaders frequently stress appeals to self-interest, and hence to immediate, specific, and concrete issues, rather than to altruism, and hence to ideology, programs, and self-sacrifice.
6. Organizational meetings and protest demonstrations routinely include food, drink, and entertainment.

These ideas tell us more than that participation in the new instance of collective dissent is rational. The existence of selective incentives determines what the various actors (e.g., participants, opposition leaders, government, patrons) do and how opposition groups become corrupt and change over time. While the selective incentives solution to the free-rider problem was initially designed to explain why rational people participate in rebellion, it explains much more—why protest and rebellion take particular courses and have particular consequences. The focus upon additional and true statements about protest is thus a particularly useful perspective for evaluating the new application of this old nut and bolt.

The Lakatosian approach is not without its critics, however. Many would argue that the deductive fertility of a research program—the variety of propositions that it can yield about a new problem domain—is a necessary but not sufficient condition for its value.[7] While valuable and important, there are four reasons why one should not overestimate the significance of any research program's ability to produce additional and true observations about a new domain of inquiry.[8]

First, a research program is only one of many research programs. Each has a more or less fertile agenda of topics for study. Some parts of the agendas of different research programs do not coincide. "Breakdown theories" of protest, for example, tell us that protest will occur during periods of personal pathology and antisocial behavior; the concomitants of protest will therefore be suicide, divorce, alcoholism, drug abuse, and vagrancy. Nothing in the collective action research program leads one to study these phenomena as covariates of protest. Other parts of the agendas of different research programs do coincide. Both collective action and grievance theories, for example, have been used to explain the same observations about the impact of economic inequality on collective dissent (Lichbach 1989, 1990).

Second, almost all the proponents of a given research program claim that the program can explain much of the empirical world by subsuming the important parts of competing research programs. Consider, once again, the case of collective dissent. Gurr (1970: 321) is obviously correct, from one philosophy of science perspective, when he argues that "one determinant of the adequacy of theoretical generalization is the degree to which it integrates more specific explanations and observed regularities." But claims about the deductive fertility and integrative capacity of the core ideas of research programs in conflict studies have been heard too many times. Gurr (1970), for example, too easily integrates status discrepancy, cognitive dissonance, value disequilibrium, and relative deprivation ideas under the frustration-aggression rubric. Tilly (1971: 416) thus likens *Why Men Rebel* to a sponge and maintains that "the sponge-like character of the work comes out in Gurr's enormous effort to subsume—to make every other argument, hypothesis, and finding support his scheme, and to contradict none of them." Students of conflict are thus justifiably suspicious about claims by supporters of the latest research program that the program is the key that "unlocks all conceivable doors" (Hirschman 1970: 330). Exaggerated claims succeed "only in provoking the readers' resistance and incredulity" (Hirschman 1970: 331). The derivation of innumerable "true" propositions from a research program is thus seen as a breathless search for cognitive consistency between new information and old perspectives, with all the inevitable elements of gimmickry and gadgetry. Hirschman (1970) understandably counsels modesty in the difficult search for truth and understanding. In fact, only a simple-minded positivism would lead one to try to subsume all theories under a single favorite theory (Lloyd 1986: 216).

Third, it is always easy to make deductions that support theories. Hence, accounts of the beginning of protest always seem to confirm culturalist theories and accounts of the end of protest always seem to confirm rationalist theories. If, for example, collective dissent occurs, culturalist theories conduct an ex post facto search for grievances while rationalist theories look for collective action solutions. If instead collective dissent does not occur, culturalist theories conduct an ex post facto search for the weakness of grievances and rationalist theories look for the Rebel's Dilemma. An example from Thompson (1966: 572) illustrates the point: "Yorkshire Luddism petered out amidst arrests, betrayals, threats, and disillusionment." Collective action theorists are trained to read "selective disincentives" and the "improbability of making a difference" into Thompson's diagnosis of why Yorkshire Luddism failed. Runciman's (1989: 367) warnings against "self-confirming illustrations pre-emptively immunized against awkward evidence" are quite relevant here.

Finally, it is easy to produce numerous deductions by adding numerous assumptions. Much "like a conjurer putting a rabbit in a hat, taking it out again and expecting a round of applause" (Barry, cited in Hechter 1990: 243), it is an approach that deserves no honors. Research programs in the social sciences often appear deductively fertile only because of an inelegant eclecticism: their assumptions are hedged so as to be able to account for much of the empirical world. But unless the assumptions behind a research program are parsimonious and precise, nothing of value has been accomplished, for anything can be derived from everything.

The consequence of eclectic theories is therefore that testing becomes impossible. Eckstein (1980) discovered this truth in conflict studies when he tried but failed (not *his* fault) to separate two important research programs, Gurr's (1970) version of culturalist theories and Tilly's (1978) version of rationalist theories, by determining which theory better explains the known facts about how social cleavages, the economy, repression, urbanization, and so on influence collective dissent. Eckstein points out that Gurr and Tilly surrounded their core assumptions with a "protective belt" by arguing that grievances and mobilizable resources are required for collective dissent. Both theories thus turned out to be eclectic.

Popper

Given these difficulties with the additional and true criterion, scientists often try to answer a second question about models of theories derived from their pet research program: Compared with other approaches, does my approach tell us things that are unique and more valid about the new problem domain under investigation? Scientists must show, in other words, that the implications of their pet theories are (1) original and pioneering and hence unexpected and counterintuitive, given other traditional wisdom in the field and (2) more valid than that traditional wisdom. The additional and true propositions about the new conflict derived from rational choice theories, for example, must also be different and better than those offered by alternative theories of conflict.

Truth, Popper (1968) tells us, comes out of the confrontation of ideas. A research program's models must therefore be tested against those of the competition. A scientist is consequently less interested in finding the best hypothesis from his or her pet program than in comparing the competing theories from different programs in a subject domain, a point well recognized by both philosophers of science[9] and practicing social scientists.[10]

The different and better criterion is particularly relevant because of the imperialistic tendencies of research programs. Many rational choice theorists have tried to push back the limits of their explanations (Bates and Bianco 1990: 351; Miller 1990: 343). Rapoport (1970: 300) thus comments on Riker's minimum-winning coalition prediction of the election of 1824: "How serious are we to take these calculations? Are not the conventional political interpretation of the Clay-Adams alliance more convincing? I do not know. If we could find situations where the predictions of 'conventional' political theory and the behavioral scientist's interpretation of n-person games seem incompatible, we could pit one against the other. In the above instance they are compatible and the question remains open."

In attempting to be integrative and eclectic, scholars often miss the value of Popperian-type crucial tests among paradigms in advancing middle-range theories and concrete explanations (Lichbach 1995: sect. 9.3). In their widely cited studies, however, Eckstein (1980) and McAdam (1982: chap. 4) develop "explanation sketches" of two or three alternative models of contentious politics and then explore several substantive domains to discover competing test implications. How do the paradigms differ? Do they yield competing predictions? Can we develop and test the predictions in a new problem domain, whether a broad sample, a carefully chosen set of comparisons, or a crucial case study?

Nonetheless, there is a problem with the different and better criteria. Researchers who consider themselves "problem driven," "puzzle directed," or "question oriented" often argue that synergisms of research traditions are valuable. Since this type of social scientist is interested in developing middle-range theories in some substantive domain (e.g., protest cycles) or historically concrete explanations of empirical happenings (e.g., fascism in Germany and Italy), he or she wants to draw freely upon rationalist, culturalist, and structuralist approaches to develop a single comprehensive theory or explanation.

Nested Models

As such scientists mine different approaches to construct explanations that address concrete problems and puzzles, they create the possibility for substantive syntheses. Similarly to how Weber used ideal types, scientists can draw on the nuts of bolts available in different schools to explain a historical puzzle. The combination can, to use a chemical metaphor, be a compound, mixture, or something in between like a colloidal suspension; or the combination, to use a biological metaphor, can range from true symbiosis to mutual coexistence. However intellectually cohesive the result, creativity comes from the reconciliation of differences and the attempt at synthesis. Modest rational choice theory, for example, moves cautiously from thin to thick rationality—incorporating cultural and structural alternatives into an individualistic decision-calculus—in an attempt to establish baselines and boundaries.

There is a research methodology that allows scientists to evaluate the results: nested models. In this approach, splitters develop a set of competing predictions that complement the set of predictions produced by the synthesizers. For example (Lichbach and Seligman, 2000: chap. 4): What do rationalist theories predict about regime transition? Or culturalist theories? Or a rationalist-culturalist consortium? Nested models enable scientists to evaluate the limitations of the pure theories and the value added of the combined one. This approach therefore allows creative competitions in new problem domains. Some combinations might work better in some of these domains than others.

At the level of paradigm, middle-range theory, or empirical explanation, such creative confrontations are to be preferred to flabby and facile syntheses. Rationalist and culturalist paradigms, for instance, can be used to generate ideal-type theories about regime transition that can serve as the models and foils that make theoretical and empirical work interesting and worthwhile. The dialogue between paradigms should therefore stress struggle over synthesis and competition over consortium; even syntheses and consortiums, via the nested models, can enter the struggle and the competition. Contending theories should always guide our research.[11]

The critical assumption here is that metastandards of evaluation exist and hence transparadigmatic connections can be fashioned. Popperians and those who use nested models thus search for this neutral language—an Archimedean point of the ideal observer, a transparadigmatic norm, and a theory-independent standard of comparison— that does not privilege one tradition over the other. Partisans counter, however, that there are often real differences among research schools, that competing theories do not share meanings, and that different theories cannot be translated into one another. The incommensurability or otherness of theories, that is, dooms interparadigmatic translations and transparadigmatic syntheses and researchers are trapped in their particular self-contained discourses. Since there is no metaframework, higher order language, first philosophy, foundations, or independent tribunal that can facilitate comparison of the separate local languages, the only standards are within-paradigm standards. Hence there can be no conversations among traditions, competitions among communities, and rational choice among paradigms.[12]

Davidson (1973–4) challenges this dogma of the separation of conceptual schemes and maintains that a conceptual scheme can be made intelligible to someone else. Wittgenstein (cited in Bhaskar 1997: 8) adds that one can see the fly in the fly-bottle only if one's perspective is different from that of the fly. Following Weber's point that one does not have to be Caesar to understand Caesar, one does not need to speak with Caesar to understand Caesar. More generally, there are analytical and empirical arguments against relativism and for the kind of transparadigmatic comparisons advocated by the synthesizers.

The analytical argument is that conflict implies mutual understanding or a common language within which disagreement can occur—a metastandard of comparability and translatability. As MacIntyre (1988: 370) puts it: "A precondition of the adherents of two different traditions understanding those traditions as rival and competing is of course that in some significant measure they understand each other." MacIntyre (1990: 5) continues: "To be able to recognize some alien system of belief and practice as in contention with one's own always requires a capacity to translate its terms and idioms into one's own. The adherents of every standpoint in recognizing the existence of rival standpoints recognizes also, implicitly if not explicitly, that those standpoints are formulated within and in terms of common norms of intelligibility and evaluation." Implied in incommensurability and incompatibility, or in disagreement and conflict, is some mutual understanding.[13]

The empirical argument against relativism is also straightforward: where is the evidence that scientists on opposite sides of a theoretical fence fail to comprehend one another? Common sense observation implies the exact opposite: Scientists often understand their disagreements and conflicts quite well. To write a history of science, moreover, is to assume that conceptual frameworks different from one's own can be made understandable.

Gellner (1998: 187) thus maintains that traditions or "cultures are not terminal. The possibility of transcendence of cultural limits is a fact; it is the single most important fact about human life." He (p. 191) continues: "Organic, self-contained social and conceptual cocoons cannot cope with either their internal or external conflicts. The notion of a culture-transcending truth emerges partly to cope with the resulting problems, partly to help explain the culture-transcending achievements of science." [14]

Summary

A common complaint about theories is that what is new is wrong and what is right is old; we therefore want models of theories (nuts and bolts) that grow out of a single research program to be additional and true (i.e., new and right) explanations of a new problem domain. Another common complaint about theories is that what is different is wrong and what is right is the same; we therefore want our models of theories to confront alternatives and to be different and better (i.e., different and right) explanations of a new problem domain. A model of a theory thus must ideally satisfy two criteria: it must account for some additional and true observations about a subject matter; and it must explain these observations differently and better than competing models of theories. If the theory is synthetic, both criteria should be applied to its component parts and the resulting consortium.

Modest positivists therefore should elaborate their favorite research program to discover its utility in explaining new problem domains; they should also compare its deductions to a stylized version of an alternative research program and to a synthesis of the two programs.

Foils matter. We social scientists can begin with our research interests and then turn to our colleagues who can further those interests: "When I start a new piece of research, the first thing I ask myself is, 'Who should I take to lunch?' " (Bates, as cited in Shafer 1994: 4). We can also begin with our colleagues who can help us define our research identities: "He who walks with wise men becomes wise" (*Proverbs* 13: 20). Whether our research interests/identities are the goals and our colleagues the means, or our colleagues are the goals and our research interests/identities the means, we need wise colleagues to serve as our models *and* foils. They are the foundation of a modest philosophy of social science.

Notes

1. This chapter derives from Lichbach (2003), and appears by permission.
2. Similarly, the rational choice theory of protest is saved by shortening the time span: almost no one is rebelling *now*.

3. Blaug (1992: 110) writes that "storytelling makes use of the method of what historians call colligation, the binding together of facts, low-level generalizations, high-level theories, and value judgments in a coherent narrative, held together by a glue of an implicit set of beliefs and attitudes that the author shares with his readers. In able hands, it can be extremely persuasive, and yet it is never easy to explain afterwards why it has persuaded." Blaug thus wonders how one validate[s] a particular piece of storytelling. One asks, of course, if the facts are correctly stated; if other facts are omitted; if the lower-level generalizations are subject to counterexamples; and if we can find competing stories that will fit the facts. In short, we go through a process that is identical to the one that we regularly employ to validate the hypothetico-deductive explanations of orthodox economics. However, because storytelling lacks rigor, lacks a definite logical structure, it is all too easy to verify and virtually impossible to falsify. It is or can be persuasive precisely because it never runs the risk of being wrong.
4. Related problems in logic are that one can deduce identical conclusions from different assumptions and that one can deduce true sentences from false premises.
5. I do not have the space to discuss the problems with falsification. See, for example, Lakatos's (1970) critique. Since many traditional positivists see it as a panacea for empirical work, I will, however, mention three interrelated difficulties. First, Laudan (1996: 218–19) writes that "it leaves ambiguous the scientific status of virtually every singular existential statement, however well supported (e.g., the claim that there are atoms, that there is a planet closer to the Sun than the Earth, that there is a missing link." Second, "it has the untoward consequence of counting as scientific every crank claim which makes ascertainably false assertions. Thus flat Earthers, biblical creationists, proponents of laetrile or orgone boes, Uri Geller devotees, Bermuda Triangulators, circle squarers, Lysenkoists, charioteers of the gods, perpetum mobile builders, Big Foot searchers, Loch Nessians, faith healers, polywater dabblers, Rosicrucians, the world-is-about-to-enders, primal screamers, water diviners, magicians, and astrologers all turn out to be scientific on Popper's criterion—just as long as they are prepared to indicate some observation, however improbable, which (if it came to pass) would cause them to change their minds." Third, falsifications can be endless whereas we must ultimately believe in the truth our theories: "In the old story, the peasant goes to the priest for advice on saving his dying chickens. The priest recommends prayer, but the chicks continue to die. The priest then recommends music for the chicken coop, but the deaths continue unabated. Pondering again, the priest recommends repainting the chicken coop in bright colors. Finally, all the chickens die. 'What a shame,' the priest tells the peasant. 'I had so may more good ideas" (*Economist*, June 29, 1996: 19–21, cited in Saffran 1997: 208).
6. Indeed, "judgment" with respect to "truth" can be enhanced. Research methodologists accept the trade-off of Type I and Type II errors, recognize that neither error can be reduced to zero, and try to develop valid research designs that move the curve closer to the origin. Campbell and Stanley (1963) and Cook and Campbell (1979) thus develop checklists of challenges to internal and external validity in experimental designs, and King, Keohane, and Verba (1994) propose valuable methods for small-n studies. While these research design issues are an essential part of the evaluation to follow (one element in Lakatos's approach is

that theories resist falsification and one element of Popper's approach is that a theory provides a better fit to evidence than another theory), questions of research design are not explored here.

7. For example, a sufficient condition for the collective action research program to be progressive in Lakatos's sense is that it meet the above tests for, say, collective dissent. Such tests, however, are not necessary. There are many substantive areas, such as interest-group activity and voting behavior, where the collective action research program may yield insights. Whether the program is valuable for protest and rebellion says nothing about whether or not it is valuable for these other fields. A Lakatosian analysis of the collective action research program therefore cannot be limited to a single domain of study because limiting the empirical focus deprives the analyst of the most novel implications of the program. Focusing on a single field does not yield the full picture of the progressivity of a research program. A Lakatosian evaluation would, on the contrary, determine the impact of the program on a number of different fields: collective action theories are thus progressive if they yield many diverse implications in many different substantive domains. The question, "Is the collective action research program progressive or degenerative?" cannot be addressed with respect to a single substantive domain such as collective dissent.
8. Conciliation with old facts is even less desirable (Feyerabend 1988?):

> Why should an ideology be constrained by older problems which, at any rate, make sense only in the abandoned context and which look silly and unnatural now? Why should it even consider the "facts" that give rise to problem of this kind or played a role in their solutions? Why should it not rather proceed in its own way, devising its own task and assembling its own domain of "facts"? A comprehensive theory, after all, is supposed to contain also an ontology that determines what exists and thus delimits the domain of possible facts and possible questions . . . New views soon strike out in new directions and from upon the older problems.

9. Given that theories are underdetermined by the facts, no amount of accumulated facts can lead to acceptance or rejection of a theory (Giddens 1979: 243). Only a better theory beats a theory. Feyerabend (1988: 24) thus advises scientists to "proliferate" inconsistent theories rather than eliminate rivals. He counsels pluralism and competition rather than authoritarianism and monopoly. Miller (1987: 140) offers the most extensive arguments here: "A theory is tested by comparing it with relevant current rivals. Very abstractly put, the question is which theory is a better basis for explaining phenomena . . . One confirms a theory by showing the best explanations of relevant phenomena appeal to instances of mechanisms in the repertoire of the theory rather than relying on rival theories." In other words, Miller stresses that each competing theory has a repertoire of causal mechanisms that can be applied to the relevant phenomena or subject domains it purports to cover. For each phenomenon or domain under investigation, the question is to find the theory that supplies the best causal mechanisms. And Most (1990) draws on Platt (1966) and offers a positivist

research-design to address the question of competing theories:

1. consider a phenomena or an existing result
2. devise as many alternative hypotheses as possible that might be capable of explaining it
3. for each hypothesis, specify additional predictive expectations that should hold if it is valid
4. devise a crucial experiment (or several of them) that will as nearly as possible exclude one or more of the hypotheses
5. move quickly to carry out the experiment to get a clear result
6. exclude the falsified hypotheses
7. recycle the procedure, making subhypotheses or sequential hypotheses to refine the possibilities, and so on.

10. Rule (1988: 43) argues that "rational choice models only move from the provocative and intriguing to the convincing by identifying sets of data for which the models provide better accounts than do alternative possibilities. We need more serious efforts to confront the models with such pertinent evidence." Mueller (1989: 193) maintains that "unless public choice-derived models can outperform the 'traditional, ad hoc' models against which they compete, the practical relevance of public choice theories must remain somewhat in doubt." Eckstein (1980) offers a classic test of rational actor versus deprived actor theories of protest and rebellion. Arrow's (1974: 65) comments about competition and authority systems also apply to competition and research programs: "The owl of Minerva flies not in the dusk but in the storm." Hence, social scientists should test their pet predictions of empirical regularities to see whether they are different from our existing understanding (i.e., preexisting theory) of the phenomena in question (Shapiro and Wendt 1992: 217): "This conformity with preexisting theories is important because in the realist view, all observation is theory-laden to a degree. Scientists compare theories not with 'the evidence' as empiricists claim but with alternative theories and background understandings of how the world works. Confirmation of a theory with those understandings is never a sufficient reason to accept it, but theory wildly at odds with them will inevitably bear a heavier burden of proof."
11. For an example of this approach in the field of domestic political conflict, see Lichbach (1997, 1998a, 1998b).
12. Pascal expressed this relativism or perspectivism in a way that is deceptively appealing to comparativists: "Truth is different on the other side of the Pyrenees." Or, as Norman (1983: 9), citing Protagoras, the most famous of the Sophists, put it "man is the measure of all things, of what is, that it is, and of what is not, that it is not.' Whatever seems to me to be the case, is true for me, and whatever seems to you to be the case, is true for you. No belief can be said to be true or false in itself, for there is no objective truth."

13. Kincaid (1996: 30–31) thus argues that Kuhn's (1970) relativism is self-defeating and self-referentially incoherent:

> If paradigms speak in entirely different languages, then they really never disagree. Since they share no meanings, they cannot assert what the other denies. Moreover, if meaning depends entirely on the overarching theory, then every difference in theory produces differences in meaning. So when any two individuals have different beliefs about the world, meanings will differ as well. According to Kuhn, however, differences in meaning preclude successful translation. Those who did not share Kuhn's theory of science should be unable to understand him.

The problem with pure relativism, the position that truth is bound in space and time (i.e., to cultures), is indeed self-referential incoherence: the only social scientific law is that there are no social scientific laws. This historicist doctrine has the same universalist pretensions as the social scientific doctrine. It is, however, self-contradictory and thrives only by exempting itself from its own conclusions. As Quine (1975: 238) puts it, "But if it were, then he, within his own culture, ought to see his own culture-bound truth as absolute. He cannot proclaim cultural relativism without rising above it, and he cannot rise above it without giving it up." One therefore cannot argue that all beliefs are unfounded, except this belief itself, or that all statements are biased, except this statement itself. Nihilism, relativism, perspectivism, and skepticism, goes the counterargument, are illusory and thus only lead to sophistry, casuistry, and historicism.

14. Since the incentives to strategically reveal their results and the collective action problem of monitoring their actions bedevil scientists, Moore's (1966: 356) "immunizing stratagem" is to be judicious and careful: "As one tries to grapple with the details of contradictory and fragmentary evidence, either of two things may happen. The certainty may evaporate into a chaos of ill-assorted facts, or else the evidence may be selected to produce an argument that runs too smoothly to be true." Moore (1978: xvi) thus promises:

> I had no intention of forcing the facts of German history though a conceptual sieve in order to "test" hypotheses. Historical facts have a certain patterned relationship to each other than such a procedure would obliterate and destroy. It is the task of the investigator to elicit this pattern through careful and critical attention to the evidence. It is necessary to proceed dialectically, patiently, listening for contradictory clues and signals, much as a skilled diagnostician tries to understand the set of organs and issues in a live human patient while searching for patterns that will reveal a state of health or a specific disease. Dissection and hypotheses are necessary in both forms of inquiry at certain points. But they are nowhere near enough.

Moore thus rejects relativism, or the idea that we are trapped in research communities.

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