

Chapter 2

What Can We Know? How Do We Know?

Richard Ned Lebow

2.1 Introduction

This book¹ 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 a 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

¹This text was first published as: “What Can We Know? How Do We Know,” in Richard Ned Lebow and Mark Lichbach, eds., *Political Knowledge and Social Inquiry* (New York: Palgrave, 2007), pp. 1–22, ISBN 9781403974563. The permission to republish this text was granted on 18 June 2015 by Claire Smith, Senior Rights Assistant, Nature Publishing Group & Palgrave Macmillan, London, UK.

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 (KKV), *Designing Social Inquiry*, 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, 2000.² 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

²In 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.

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.

2.2 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.³

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.⁴ 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

³Hausman, *Inexact and Separate Science of Economics*.

⁴Review Symposium: The Qualitative-Quantitative Disputation: Gary King, Robert O. Keohane, and Sidney Verbas’s *Designing Social Inquiry*; Brady and Collier, *Rethinking Social Inquiry*.

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.

2.3 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 Godel. 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 the 1960s. 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 scientists, 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.⁵ By the 1950s, Popper had come to understand “covering laws” as limited in scope, and perhaps as even unrealistic.⁶ If he were alive today, he might well agree with Pollins that the social sciences are “the ‘really hard’ sciences.”⁷

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 even 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.⁸ 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).⁹ 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

⁵Weber, “‘Objectivity’ in Social Science and Social Policy.”

⁶Covering 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.

⁷Pollins, “Beyond Logical Positivism: Reframing King, Keohane, and Verba’s *Designing Social Inquiry*,” pp. xx.

⁸Schutz, “Common-Sense and Scientific Interpretation of Human Action,” p. 5.

⁹Searle, *Construction of Social Reality*.

transformations, it is illicit to make such assignments if the antecedent qualitative comparison has not or cannot be completed.¹⁰ 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.¹¹ 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 superseded. 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.¹²

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

¹⁰Coleman, *Introduction to Mathematical Sociology*.

¹¹Kuhn, *Structure of Scientific Revolutions*; Rouse, *Knowledge and Power*; Kratochwil, “Regimes, Interpretation, and the ‘Science’ of Politics.”

¹²Popper, *Objective Knowledge*, pp. 78–81.

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 Harre, 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.”¹³

2.4 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

¹³Harre, *Varieties of Realism*, p. 6.

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 drift, 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.¹⁴ 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.¹⁵

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 backwardlooking 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 is 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.

¹⁴Gould, *Wonderful Life*.

¹⁵For a thoughtful rebuttal of this argument, see Waldner, 'Anti-Determinism.'

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.¹⁶

2.5 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’”¹⁷

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.¹⁸

Normative theorizing must deal with facts just as empirical research must address norms. They do not 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

¹⁶Maher, *Betting on Theories*, p. 218, makes the same assertion about the sciences, whose history, he claims, “is a history of false theories.”

¹⁷Weber, *Methodology of the Social Sciences*, p. 60.

¹⁸Gerring, “A Normative Turn in Political Science?”

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.¹⁹

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.

2.6 The Method of Inquiry

Contributors who are generally sympathetic to the goals of 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.

¹⁹Searle, *Construction of Social Reality*, pp. 27–28.

For KKV and others who subscribe to their narrow framing of the Hypothetical-Deductive (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.²⁰

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.”²¹ 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 non-extinction when extraterrestrial impacts were common. They also ignored far more numerous sub-extinctions.

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.

²⁰Weber, “Counterfactuals, Past and Future”; Lebow, “What’s So Different about a Counterfactual?”

²¹King, Keohane, and Verba, “The Importance of Research Designs in Political Science,” p. 476.

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.²²

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.²³ 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.²⁴

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

²²Lebow and Stein, *We All Lost the Cold War*, chs. 4 and 13; Kull, *Minds at War*.

²³Diesing, *How Does Social Science Work?* p. 45.

²⁴Lakatos, "Falsification and the Methodology of Scientific Research Programmes," p. 137.

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.

2.7 The Practice of Inquiry

Science consists of hard-fought battles 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 the 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 democ and war. These definitions obscure the meaning of these variables and how these meanings have evolved over time. Statistical tests are largely use 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 sup 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 presuppo 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 evad 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.

2.8 '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 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 preju. 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 scien claims also encounter resistance from the wider community. The 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 transform 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 "scien method." Researchers and philosophers of science argue over what constitutes adequate specification and testing, the extent to which it is pos, 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 versus Ferguson* established the principle of separate and equal education for African Americans that endured until *Brown versus Board of Education* in 1954. De facto segregated education continues to this day in some locales. *Brown versus Board of Education* reflected changing attitudes toward African Americans and the constitution itself. Another impetus was extensive social science research that demonstrated that separate edu was inherently unequal. Despite continuous controversy about the meaning of the constitution and despite periodic failures to apply its prin 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 free. 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 dis to be made between the questions we ask and the ways in which we answer them. What distinguishes us from ideologues is our commit to finding and evaluating answers by means of a scientific method. Social scientific research agendas are shaped by political beliefs, life experi, and desires for professional recognition. There is nothing wrong with these motives. Good social science should be motivated by deep per 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.

2.9 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 criti 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 natural. 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 explanation, and are routinely used to enhance or undermine theories. The next two chapters attempt to evaluate ongoing and well-regarded research in light of earlier discussions. As noted earlier, Jack Levy analyzes three such programs, including the democratic peace, and provides a position 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.

Bibliography

- Coleman, James. 1964. *Introduction to Mathematical Sociology*. New York: Free Press.
- Diesing, Paul. 1991. *How Does Social Science Work?* Pittsburgh: University of Pittsburgh Press.
- Gerring, John; Joshua Yesnowitz. 2006. "A Normative Turn in Political Science?" *Polity* 38, 1, pp. 101-33.
- Gould, Stephen J. 1990. *Wonderful Life: The Burgess Shale and the Nature of History*. New York: Norton.
- Harré, Rom. 1987. *Varieties of Realism: A Rationale for the Sciences*. Oxford: Blackwell.
- Hausman, Daniel M. 1992. *Inexact and Separate Science of Economics*. Cambridge: Cambridge University Press

- Hempel, Carl. 1942. "The Function of General Laws in History." *Journal of Philosophy* 39, pp. 35-48.
- King, Gary; Keohane, Robert O.; Verba, Sidney. 1994. *Designing Social Inquiry*; Brady and Collier, *Rethinking Social Inquiry*.
- King, Gary; Keohane, Robert O.; Verba, Sidney. 1995. "The Importance of Research Designs in Political Science." *American Political Science Review* 89 pp. 454-81.
- Kratochwil, Friedrich V. 1988. "Regimes, Interpretation, and the 'Science' of Politics." *Millennium* 17 (Summer): 263-84.
- Kuhn, Thomas S. 1962. *Structure of Scientific Revolutions*. Chicago: University of Chicago Press.
- Kull, Steven. 1988. *Minds at War*. New York: Basic Books.
- Lakatos, Imre. 1970. "Falsification and the Methodology of Scientific Research Programmes," in Imre Lakatos and Alan Musgrave (eds.), *Criticism and the Growth of Knowledge*. Cambridge: Cambridge University Press, pp. 91-195.
- Lebow, Richard Ned. 2000. "What's So Different About a Counterfactual?" *World Politics* 52 (July): 550-585.
- Lebow, Richard Ned; Stein Gross, Janice. 1994. *We All Lost the Cold War*, chs. 4 and 13;
- Maher, Patrick. 1993. *Betting On Theories*. Cambridge: Cambridge University Press.
- Pollins, Brian. 2007. "Beyond Logical Positivism: Reframing King, Keohane, and Verba's *Designing Social Inquiry*," in Lebow and Lichbach, *Theory and Evidence*, pp. 87-106.
- Popper, Karl. 1966. *Objective Knowledge*. Oxford: Oxford University Press, pp. 78-81.
- Review Symposium: The Qualitative-Quantitative Disputation
- Rouse, Joseph. 2005. *Knowledge and Power: Toward a Political Philosophy of Science*. Ithaca: Cornell University Press.
- Schutz. 1954. "Common-Sense and Scientific Interpretation of Human Action." *Philosophy and Phenomenological Research*. 14: 1-38.
- Searle, John R. Searle. 1995. *Construction of Social Reality*. New York: The Free Press.
- Waldner, 2008. "Anti-Determinism: Or What Happens When Schrodinger's Cat and Lorenz's Butterfly Meet Laplace's Demon in the Study of Political and Economic Development," Paper presented at the annual meeting of the American Political Science Association, Boston, Ma. 2009
- Weber, Max. 1968. *Gesammelte Aufsätze zur Wissenschaftslehre*, ed. Johannes Winckelmann, 3rd ed. Tübingen: J. C. B. Mohr (Paul Siebeck)
- Weber, Max. 2012. in Hans Henrik Brunn and Sam Whimster, eds., *Max Weber: Collected Methodological Writings*. London: Routledge.
- Weber, Steven. 1996. "Counterfactuals, Past and Future," in P.E. Tetlock and A. Belkin, eds., *Counterfactual Experiments in World Politics*. Princeton: Princeton University Press., pp. 272, 278.

Richard Ned Lebow: Major Texts on Methods and
Philosophy of Science

Lebow, R.N. (Ed.)

2016, X, 133 p. 11 illus., 8 illus. in color., Hardcover

ISBN: 978-3-319-40026-6