

ence, to be interested in his book! This is a shame, as Jackson probably only seeks here to encourage critical realists to avoid the temptation of playing the science card themselves. Such a goal may have been better achieved by analyzing their position a bit more sympathetically while putting a little more edge into his account of neopositivism.

Conclusion

Jackson certainly shows that philosophy of science should be treated seriously, and this clarifying and inspiring book should be read by all students in the discipline. Yet it is not clear that it will be. As Jackson notes, there is an absence of sincere philosophy of science training in IR. Given the prominence of graduate students' concerns with making their work acceptable to key constituencies, Jackson displays a surprising reticence toward the practical feasibility of adopting his scheme for IR graduate students writing their dissertations. Success in a prospectus defense, conference presentation, or job talk relies upon the extent to which the audience will accept, or at least take seriously, the claims being made. It may, however, be quite difficult to stare down a dissertation committee or a job talk audience member and say, "Well, you are ignoring the philosophical-ontological contradictions implicit in your criticism."

Yet, unless held to such a standard—one, it should be noted, he does not set for himself—Jackson ultimately cannot be held responsible for how seriously the discipline will treat the very substantive issues this book raises. Although this book does not help us as graduate students to navigate the waters of the discipline *as a discipline* as much as we might hope, it has certainly helped us to steer our own thoughts more steadily. Being able to understand that many debates in IR already presuppose the same philosophical wager, and that others often mix and match from different underlying understandings of the hook-up between theory and the world, has helped us as scholars become more clear and confident about the standards which would establish if our own research counts as "science."

References

- Clarke, Kevin A. and David M. Primo. 2007. "Modernizing Political Science: A Model-Based Approach." *Perspectives on Politics* 5:4 (December), 741–753.
- Fearon, James. 1995. "Rationalist Explanations for War." *International Organization* 49:3 (Summer), 379–414.
- Finnemore, Martha and Kathryn Sikkink. 1998. "International Norm Dynamics and Political Change." *International Organization* 52:4 (October), 887–917.
- Jackson, Patrick Thaddeus. 2010. *The Conduct of Inquiry in International Relations: Philosophy of Science and its Implications for the Study of World Politics*. London: Routledge.
- King, Gary, Robert Keohane, and Sidney Verba. 1994. *Designing Social Inquiry: Scientific Inference in Qualitative Research*. Princeton: Princeton University Press.
- Rogowski, Ronald. 2004. "How Inference in the Social (But Not the Physical) Sciences Neglects Theoretical Anomaly." In *Rethinking Social Inquiry: Diverse Tools, Shared Standards*. Henry Brady and David Collier, eds. (Lanham, MD: Rowman and Littlefield), 75–83.

Making Sense of the Study of International Relations: Seeking a Guide for the Perplexed

<https://doi.org/10.5281/zenodo.937388>

John G. Gunnell

State University of New York, Albany
jgg@albany.edu

Patrick Jackson's book on *The Conduct of Inquiry in International Relations* offers graduate students, younger scholars, and, indeed, many specialists a useful map for charting the often inhospitable terrain of scholarship in the field of International Relations (IR). This is particularly the case as far as illuminating the awakening to issues in the philosophy of science that has taken place in IR during the last two decades. Jackson presents a typology for sorting the debates about the nature and demands of scientific inquiry, which have often been conducted, either explicitly or implicitly, in terms of diverse and complicated philosophical arguments. As opposed to many previous analyses, which have tended to be couched in terms of dichotomies and biased toward a particular philosophical persuasion, Jackson's scheme is remarkably neutral, but, in some respects, maybe too neutral.

In exploring these matters, Jackson sometimes becomes entangled in the puzzles he seeks to resolve, such as the relationship between philosophy and social science, and, at certain points in his presentation, the historical context and genealogy of this relationship, as well as that between natural science and the philosophy of science, seems obscured.¹ My purpose is not to quarrel with his attempt to sort out what is going on in IR, but rather to add a little historical and critical gloss in the hope of joining in the kind of constructive contention that he so strongly advocates as essential to the conduct of inquiry.

Jackson's basic destination, after a long journey through a wide range of philosophers, reaching from Descartes to Roy Bhaskar, as well as a representative number of scholars in IR, is a condition of "healthy pluralism" in matters methodological. This general stance is certainly not novel. There is no doubt that pluralism has once again become the dominant ethic in political science, as well as democratic theory, and it now seems nearly as awkward to find fault with pluralism as it is to criticize the norm of eating a balanced meal. By securing objectivity while allowing variety, they seem both to solve the problem of relativism and to secure authority, but even some balanced meals are not easily digested.

The Genealogy of the "Science Card"

To put Jackson's work into perspective, it is helpful to consider briefly the intellectual genealogy from which his argument has emerged. Despite the fact that, from its earliest stages, and especially after the end of the nineteenth century,

what Jackson refers to as “playing the science card” was the pivot of political science’s rhetoric of inquiry, the image of science was both inchoate and largely unchallenged. There was little in the work of individuals such as Charles Merriam that resembled an articulate meta-science, and even among philosophers such as John Dewey, the concept of science had little distinct content beyond a purported affiliation with liberalism. This was, in part, because scientists did not tend to give an account of their own practices; science as a basic value was seldom questioned; and there was yet no disciplined field of the philosophy of science upon which to draw. During the creation of the American Political Science Association, the science card primarily functioned as a basis for claiming the kind of cognitive authority that would sustain the perennial hope for practical purchase in matters of liberal reform.

Although the reform concern would persist through mid-century in a latent manner, and sometimes explicitly in the work of individuals such as Harold Lasswell, the meaning of “science” increasingly for the most part surfaced in disputes internal to the discipline. By the early 1950s, the issue of what constituted science had become central to the behavioral movement and the controversy surrounding it. Although behavioralism is often viewed today as initiating a program for emulating the natural sciences, it was to a large extent a reaffirmation of the discipline’s traditional commitments to the value of science and an image of pluralist liberal democracy, which together had been forged a generation earlier. As early as 1950, these commitments were, for the first time, confronted with a substantial philosophical challenge. What was particularly egregious was the fact that this challenge was mounted from within political theory which heretofore had been the source of the discipline’s self-understanding of both science and liberalism and of the assumed integral relationship between them. An ideologically and philosophically diverse group of émigré scholars, ranging from Leo Strauss to members of the Frankfurt School, focused on science and liberalism as both causes and manifestations of a fundamental “crisis of the West” in which, they claimed, contemporary political science was deeply implicated.²

American political scientists had little in the way of indigenous resources on which to draw for a defense of science, but, serendipitously, another group of émigrés also arrived in the United States and brought with them a complex but systematic account of the logic and epistemology of scientific explanation, which became, in the American context, the basis of the philosophy of science as a distinct field of study. The logical positivists, and their intellectual evolution as the founders of logical empiricism, represented by figures such as Carl Hempel, formulated a meta-science which, in its secondary and tertiary renditions, was appropriated by social scientists. The formulations of logical empiricism were not intended as a guide to the practice of science, but its normative character was the residue of its historical origins and particularly of its ideological purpose of propagating a “scientific view of the world” in the European context. American social scientists were largely unaware of this patrimony, and although Hempel famously noted that if his account of scientific explanation did

not conform to the practice of science, so much the worse for science, social scientists viewed it not only as an account and justification of science but as the basis of a technique for creating and deploying scientific theories and empirically verifying claims to knowledge.

While admirably attentive to the political context in which logical positivism took shape in Vienna, Jackson’s account of mid-century shifts in the concept of science among scholars of international relations loses sight of such contexts. The notions of science deployed by Hans Morgenthau and E.H. Carr were, he notes, philosophically vague, but he overlooks their parallels, respectively, with Strauss and the Frankfurt School’s critical interrogations of science, liberalism, and the purported affinity between them. Seen against this backdrop, the 1960s “great debate” to which Jackson traces the linking of “science” with quantification in IR might be interpreted as a defensive effort by liberals to reclaim ownership over the mantle of science by yoking it to new techniques they were mastering. It might, in turn, then appear as no accident that the democratic peace finding with which Jackson introduces his chapter on neo-positivism is not only the proudest empirical finding of the quantification agenda, but a cornerstone of contemporary liberal theory in IR.

While missing such more politically-charged resonances, Jackson is well attuned to nuanced philosophical contrasts between the view of science at play in IR’s second “great debate” and the views deployed in more recent decades, especially the impact here of Kuhn and Lakatos. As early as the 1960s, Kuhn had begun to be enlisted by both critics and defenders of behavioralism, and subsequently mainstream political scientists increasingly became aware of the implications of his and Lakatos’s arguments for their mantra of science. By the mid-1970s, debates about behavioralism had to a large extent become conducted in the surrogate language of opposing philosophies of science.³ The critique of the positivist image of science was also complemented by a growing literature derived from the philosophy of social science advocating what was often referred to as an interpretive or hermeneutic approach. All of this discussion shared an assumption that the practice of science was grounded in philosophy.

Jackson correctly indicates that these debates did not significantly spillover into IR until after Martin Hollis and Steve Smith (1990) embraced what had become a common attempt at resolution among certain political theorists, that is, the idea that scientific and interpretive approaches were more complementary than mutually exclusive (Moon 1976). Hollis and Smith also voiced another concern that had gained a foothold in political theory. Although there were few critics of logical empiricism who did not find support in the work of Kuhn and although arguments such as that of Peter Winch provided new grounds for a neo-Weberian image of social inquiry as understanding meaningful action, this literature seemed to many to have relativistic implications and undercut the congenial idea of social science as a critical discourse (Bernstein 1976). One line of argument that evolved in political theory was that the attack on positivism signaled a need to reconstitute the image of science on a sounder philosophical basis, which would not

“Methodology” and the Philosophy of Science

only underwrite empirical inquiry but also provide a foundation for a critical social science. This was to be accomplished, it came to be claimed in the 1980s,⁴ by turning to one of the principal successors to logical empiricism in the philosophy of science. This was scientific realism, which in IR, during the subsequent two decades, has become the most enthusiastically pursued meta-theory of science, even though, as Jackson makes clear, it is only one among a number of philosophical options.

Can Engaging the Philosophy of Science Help IR?

Jackson persuasively maintains that, in the end, all of these philosophical claims about science have not achieved a great deal and that science remains largely a rhetorical concept to which diverse content is attributed. After also exploring the philosophical literature devoted to the issues of how to demarcate science and how to determine what kinds of claims are authoritatively scientific, Jackson argues that this work, as well, does not provide much traction for getting on with the job of determining how a science of politics should be conceived and practiced. Jackson’s first suggestion is that rather than seeking answers to these questions from philosophy, we should look within the tradition of social science itself and particularly at the work of Max Weber, which, among other things, indicated that science should be defined more by its goal than by a particular methodology or approach. As opposed to partisan politics, science should be devoted to the search for credible empirical knowledge, and this search should accommodate both (what came to be called in the 1970s) “explanation and understanding” or what today is often parsed as quantitative and qualitative research.

In some respects, Weber’s argument seems an odd choice as a model. The work to which Jackson refers was a masterpiece of rhetoric in the service of reconciling academic ideological and philosophical polarities and demonstrating to political actors the epistemic authority of science.⁵ One might reasonably argue that what prompted Weber’s brand of ecumenicism regarding both the relationship between science and interpretation and the acceptance of many forms of ideal typification, ranging from marginal utility to the Protestant ethic, was precisely his conclusion that there was no authoritative resolution to these conflicts. And although Jackson ends his first chapter suggesting that we release ourselves from remaining intellectually mortgaged to the vagaries of the philosophy of science and, instead, seek social scientific autonomy, he opens the very next chapter arguing that philosophy of science can in fact aid us in clarifying and improving our research practices. Jackson leaves no philosopher’s stone unturned, but some terminological problems emerge, in part because everyone from Descartes to the present is treated somewhat as if they belonged to the same club. In taking up this line of argument, he employs a somewhat odd terminology. The principal problem in this case revolves around Jackson’s use of the term “methodology,” and at this point, it is important to say something about the history of the philosophy of science.

Although the philosophy of science is today often considered a branch of philosophy, it did not emerge as simply part of the evolution and differentiation of philosophical specialties. Departments and subfields in this area are often designated as some combination of the methodology and philosophy of science. Methodology typically refers to the study of the principles underlying a practice of knowledge such as science, and it is often considered a branch of logic. The origins of methodology, however, were largely within the practice of science itself, and the field of the philosophy of science evolved from those origins. Although today we may be inclined to think of someone such as Descartes as a philosopher, a strong case can be made for classifying him as a scientist who made “methodological” arguments to justify his substantive scientific claims at a time when “science” was neither paradigmatic nor socially authoritative. And the same could be said for a number of other people, including Newton and Darwin. Eventually, however, scientists, increasingly institutionalized, did not feel the need to defend themselves against rival authorities, and the theories of individuals such as Newton became internally dominant.

Methodological claims about such matters as induction tended to drop out of the discourse of science and to be taken up by individuals in philosophy or on the cusp of philosophy and science, who were in many cases concerned with raising the status of social science. John Stuart Mill was a typical methodologist who attempted to extend a logical account of scientific procedures to clarify and promote the practice of social inquiry. In the case of the Vienna Circle, most of the members were not easily defined as either scientists or logicians, but their principal arguments were about the underlying logic of science and about how to apply that logic to many areas of human practice. By the time that logical positivism had been re-established in the United States and lodged in departments of philosophy, the methodological emphasis, with its normative focus on the principles of scientific practice, began to give way to claims about the general transcendental structure of scientific explanation such as that represented in the “covering-law” model. Others, such as Popper, and later Imre Lakatos, never relinquished the normative methodological concern, which was at the center of what estranged Popper from Kuhn and, later, Feyerabend. When Feyerabend turned “against method,” he might have more felicitously said “against methodology.” “Method” typically referred more to particular techniques of research than to verification, falsification, induction, deduction, and the like, which was what Feyerabend rejected as specifying the essence of science.

Jackson argues that although we should not turn to the philosophy of science to find out what science is, it can nevertheless help us clarify our research practices, and he devotes much attention to the philosophy of science as a source for clarifying what he refers to as “methodological” questions. He claims that methodology is neither “method” nor “epistemology” and that it is more important than “ontology,” in the sense that critical realists in IR use that term. Jackson equates methodology with what he labels “philosophical ontology,”

and he argues that the core methodological principle that we should adopt is to engage in “wagers” or “provisional commitments” about general background matters such as the nature of the “world” and the relationship between mind and world. Jackson, along the way, seeks to provide a philosophical lineage for such assumptions as well as an account of how they feature in contemporary philosophy. A basic problem with Jackson’s discussion at this point is his use of and distinctions among terms such as “methodology,” “epistemology,” and “ontology.” Although he distinguishes method from methodology, he oddly opposes methodology and epistemology which have historically been closely related. It was largely the traditional epistemological project that Feyerabend was arguing against. What the term “philosophical ontology” would typically evoke would be something such as metaphysical realism, which in some version is central to most accounts of scientific realism in IR. Jackson might have been better off if he had simply spoken of something such as “strategies of inquiry” rather than to define methodology in what seems to be a somewhat odd manner. In any event, he isolates a set of commitments that define such strategies and in terms of which he constructs a matrix of ideal typifications for specifying and comparing them and which are, in various respects, reflected in four general categories of approaches in IR: neopositivism, critical realism, analyticism, and reflexivity. He examines these approaches successively in chapters three to six.

One difficulty with the structure of the book is that this scheme is not fully elaborated until the concluding chapter, and it seems that it might have been more straightforward, and generated less potential misunderstanding, if Jackson had presented the typology more completely before proceeding to examine the similarities and differences among these philosophical premises and their implications for analyzing approaches to IR. In doing so, he might also have made clearer the extent to which the “methodological” assumptions manifest in these approaches simply resembled claims in philosophy, were the actual historical residue of such claims, or, as in the case of critical realism, were explicit attempts to draw directly upon the philosophical literature.

Conclusion: Some Payoffs and Problems

One of the most helpful sections in Jackson’s book is his conclusion’s discussion of the use of the term “constructivism” in IR. This usage has been very ambiguous, and even scholars who subscribe to the label or ascribe it to others often seem unclear about exactly what is entailed. Jackson recognizes that what is behind constructivism is, although often not well articulated, a theory, or what Jackson refers to as a scientific ontology regarding social facts. It is thus, in his terms, neither a method nor methodology even though it may entail both. I am, however, less sanguine about Jackson’s account of particular aspects of the work of certain philosophers such as Kuhn;⁶ his discussion of the relationship between IR and the philosophy of science; and his avowal of ecumenicism as the ethic of inquiry.

The model that Jackson proposes for thinking about disparate approaches in IR is derived from Abraham Heschel,

who argued that although no one religion could demonstrate that it possessed special access to religious truth, they all, despite their differences, shared a number of basic concerns and beliefs. Jackson suggests that we extend this attitude to the study of IR and abandon the “holy war” among approaches based on different methodologies. Jackson argues that each approach believes that science should be systematic, open to public discussion, and devoted to the search for empirical knowledge and that this should be sufficient for finding common ground and constituting a scientific community. His message is that an examination of the philosophy of science reveals that accounts of science are irreducibly pluralist, so instead of chasing the meaning of “science,” scholars, each in their own way, should get on with the business of producing knowledge about world politics. But although he insists that there is no general philosophical answer to the foundations of science, this does not mean that any approach can proceed without philosophical foundations, which are specific to a particular line of inquiry. And we must, as Weber claimed, be explicit about these basic commitments that inevitably inform inquiry.

The first problem with this formulation is that it tends to mix up two senses of “philosophy.” To say that there is plurality in the philosophy of science would be like saying that there is plurality in the philosophy of religion and that because there is no answer to the “religion card,” we should instead pay attention to what might be generically referred to as the philosophical (that is, theoretical) assumptions indigenous to particular religions. The second problem is that while Heschel was talking about the relationship among religions, the problem that Jackson confronts is not relationships among sciences but approaches within a science. He argues that in order to engage in an ecumenical dialogue, it is necessary to have a common vocabulary which allows “translation” and a “mediated” form of contestation.

Jackson notes that Kuhn’s later work relinquishes his prior focus on paradigms in favor of speaking of a “lexicon” which defines a specific scientific community, and Jackson claims that his typology could be the basis of a vocabulary for discussion and mediation within the field of IR. The difficulty with this analogy, however, is that Kuhn’s lexicons reflected the scientific theories or ontologies that bound a community together but which, despite some limited possibility for translation, ultimately not only distinguished one science from another and one historical form of a particular science from earlier and subsequent forms but rendered theories and their lexicons incommensurable. Kuhn never backed off from this claim of incommensurability. Jackson’s vision of the state of contemporary IR is far from that of a scientific community bound together by common theoretical commitments. Jackson’s stresses that his goal is not to urge a “synthesis” of approaches but rather “agonism” without “antagonism” or an “*engaged pluralist attitude*” in the context of “contentious conventions” which respect all approaches as valid but yet rejects “relativism.” Nothing could be further from Kuhn’s image of a scientific community.

It is significant that of all the approaches that Jackson examines, it is only in the literature of critical realism that there

is much focus on the philosophy of science. While Jackson may detect in other approaches indirect connections and certain generic “philosophical” ideas such as “mind-world dualism,” it is only in the case of realism that there is an explicit attempt to constitute inquiry in IR on the basis of a distinct literature in philosophy. What is characteristic of the work of Alexander Wendt, Colin Wight, Heikki Patomaki, and others of this persuasion is that not only are quite diverse varieties of philosophical realism enlisted as a foundation of inquiry, but they are usually combined with elements of other philosophies such as hermeneutics and constructivism. “Scientific” and “critical” realism are intellectual conglomerates of derivations from diverse philosophical positions, and the primary assumption is that philosophy, in the formal sense of the term, is the foundation of social scientific inquiry and normative judgment. Even someone such as Fred Chernoff who opposes realism in favor of a Duhemian theoretical conventionalism agrees with this basic premise. The crucial point here is that although Jackson begins by rejecting the philosophy of science as the key to defining science, he continues to assume in many respects that we must turn to the philosophy of science in order to both understand inquiry and learn how to conduct it. But the pluralism of philosophy with respect to these issues is as great as it is in the case of seeking to isolate the essence of science. This is not the place to engage this issue, but suffice it to say that (historically, conceptually, and logically) it is a dubious proposition and assumption.

When Feyerabend suggested that “anything goes” in science, he was not arguing that this had been characteristic of the history and current practice of science but only that no philosophical account of science could either capture an underlying secret to its evolution or provide a guide to its success. This was primarily directed against his former mentor Popper, as well as his friend Lakatos, and even though by this point he had largely come to agree with Kuhn, he still worried that there was a normative methodological message built into Kuhn’s narrative. What Jackson’s ecumenicism fails to note is that science is not grounded so much in what he calls “philosophical ontology”—general background assumptions about such matters as the relationship between mind and world—but in what he calls “scientific ontology”—substantive theoretical claims about what constitutes the “world.” The poverty of social science resides less in a failure to be tolerant of diverse methodologies than in the liberal assumption that truth will emerge in the marketplace of ideas, just as Heschel assumed that the major religions were all in some basic way on the same page. It is doubtful Heschel’s recommendation has much relevance to either the past history of religions or the basic character of most religious practices—except maybe for some aspects of contemporary Unitarianism. It certainly has little application to natural science. Heschel was confronting neither the kind of cognitive dissonance that Darwin experienced in choosing *between* science and religion nor, to use my favorite example, which Kuhn’s work was too early to acknowledge, the mid-twentieth century conflict within geology between plate tectonics and the geosyncline theory with respect to issues such as the nature of mountains. What lies explicitly

at the core of natural science, and what is more unreflectively manifest in social science, are theoretical claims and assumptions about what kinds of things exist and the manner of their behavior.

The “ontological” claims typically defended in critical realism are quite different from ontologies in natural science. The former involve, first, a philosophical faith in a transcendental but unrepresented reality which somehow provides the basis for adjudicating specific empirical propositions and, second, the assumption that positing categories of social entities such as states, structures, and agents is comparable to theoretical claims in natural science. The actual ontological issue in social science, however, is, on the contrary, the theoretical issue of the basic nature of social phenomena, and this issue, like all theoretical issues, does not allow ecumenicism. From the theories advanced by science flow the epistemologies and in turn the methodologies and the methods, and when the theories change these principles and techniques of practice are often transformed as well. And there is no general philosophical explanation for those transformations.

If there is any common insight into science that can be derived from the work of Kuhn, Popper, Feyerabend, and the like, it is that the dynamics of science depend on competition among theories which in the end is settled by persuasion, based on the application of various means, followed by a period of consensus before conflict arises once more. Where these philosophers differed was with respect to how that competition was, and should be, conducted and resolved. Despite what some at times have viewed as the hegemonic designs of approaches such as systems analysis, structural functionalism, and rational choice analysis, the dominant ethic in political science has typically been tolerance and the proliferation of models, conceptual frameworks, and strategies of inquiry, which in the field are often passed off as “theories.” This ethic has presupposed the pragmatic liberal assumption that pluralism in ethics, politics, and science is necessary because of the methodological principal of “fallibilism” which is the secret of progress and dictates the logic of never “blocking the road to inquiry.”⁷ It seems that in the end Jackson’s argument is very much a reflection of that spirit.

Notes

¹ For a fuller discussion of some of these issues, see Gunnell (1998).

² For a fuller discussion of this period, see Gunnell (1993).

³ See, for example, Gregor (1971), Gunnell (1975), Ball (1976).

⁴ For an example, see Isaac (1987); for a fuller discussion of the evolution of realism in social and political science, see Gunnell (1998: Ch 4).

⁵ For a detailed discussion of Weber in this respect, see Gunnell (2007).

⁶ See Gunnell (2009).

⁷ For a classic statement, see Smith (1957).

References

- Ball, Terence. 1976. “From Paradigms to Research Programs: Toward a Post-Kuhnian Political Science.” *American Journal of Political Science* 20:1 (February), 151–177.

- Bernstein, Richard. 1976. *The Restructuring of Social and Political Theory*. New York: Harcourt, Brace Jovanovich.
- Gregor, A. James. 1971. *An Introduction to Metapolitics*. New York: Free Press.
- Gunnell, John G. 1975. *Philosophy, Science, and Political Inquiry*. Morristown: General Learning Press.
- Gunnell, John G. 1993. *The Descent of Political Theory: The Genealogy of an American Vocation*. Chicago: University of Chicago Press.
- Gunnell, John G. 1998. *The Orders of Discourse: Philosophy, Social Science, and Politics*. Lanham, MD: Rowman and Littlefield.
- Gunnell, John G. 2007. "The Paradoxes of Social Science: Weber, Winch, and Wittgenstein." In *Max Weber's "Objectivity" Revisited*. Laurence McFalls, ed. (Toronto: University of Toronto Press).
- Gunnell, John G. 2009. "Ideology and the Philosophy of Science: An American Misunderstanding." *Journal of Political Ideologies* 14:3 (October), 317–337.
- Hollis, Martin and Steve Smith. 1990. *Explaining and Understanding International Relations*. Oxford: Oxford University Press.
- Isaac, Jeffrey C. 1987. "After Empiricism: The Realist Alternative." In *Idioms of Inquiry*. Terence Ball, ed. (Albany: State University of New York Press).
- Jackson, Patrick Thaddeus. 2010. *The Conduct of Inquiry in International Relations: Philosophy of Science and its Implications for the Study of World Politics*. London: Routledge.
- Moon, J. Donald. 1976. "The Logic of Political Inquiry: A Synthesis of Opposed Perspectives." In *Handbook of Political Science, Vol. 1*. Fred I. Greenstein and Nelson S. Polsby, eds. (Reading MA: Addison-Wesley).
- Smith, James Ward. 1957. *Theme for Reason*. Princeton: Princeton University Press.

Pluralizing Social Science

Patrick Thaddeus Jackson
American University
ptjack@american.edu

The Conduct of Inquiry in International Relations (C of I) was not a book that I had any long-standing plans to write. The manuscript did, however, grow out of two related and long-standing frustrations that I had with discussions in Political Science in general and International Relations in particular about research design, causation, and the basic contours of knowledge-production. First of all, people seemed to invariably conflate questions of *method* or technique with questions of *methodology* or strategy of inquiry. Thus we had and continue to have rather problematic contrasts between "qualitative" and "quantitative" ways of doing social research as though the decision to use or not to use numbers had any determinate bearing whatsoever on the epistemic status of particular empirical claims. But whether or not one uses numbers is a question of technique, not a question of strategy, and as such *cannot* have any such profound impact; this means that in conducting these debates about how to do our work, we are working with impoverished and misleading terminology. Second, and related, people drew on extremely thin and partial conceptions of "science" as a way of warranting their positions; this was equally true of scholars contrasting "explaining" and "understanding" as ways of knowing, and of

scholars reducing the entire panoply of the philosophy of science to the triumvirate Popper-Kuhn-Lakatos as though those were the only three people to have ever intervened in the debate about how science worked. When I taught my Ph.D. seminar on the production of valid empirical knowledge—entitled "The Conduct of Inquiry in International Relations"—I tried to allay both of these frustrations by equipping my students with a broader set of conceptual tools for thinking about these fundamental issues and articulating a defensible position with which they felt comfortable. This book derives from that seminar and from the frustrations that animated my pedagogy in that seminar.

In responding to the excellent critical engagements with my book provided by John Gunnell, Eric Grynawski, and David Banks and Joseph O'Mahoney, I felt it appropriate to begin with this bit of context so as to clarify the book's aims and social location with respect to ongoing discussions. Because the book grew out of my frustrations with the narrowness of existing terminology and conceptual vocabulary, an important goal of the book is to broaden the discussion by casting a wider net and bringing in authors and notions that do not yet have as much currency in our field as they do elsewhere. Because the book grew out of a seminar in which I invited students to develop their own position on certain fundamental issues, an important goal of the book is not to take a strong stand for or against any particular articulation of how knowledge is to be produced scientifically. And because the book grew out of my extreme dissatisfaction with dichotomies like quantitative/qualitative and explaining/understanding, an important goal of the book is to replace those dichotomies with a more nuanced vocabulary that is still concise enough to be useful.

As such, *C of I* is neither directed against nor advocating for any particular kind of social-scientific methodology; it is instead inveighing against the narrow and biased ways that we have been talking about these issues in Political Science and International Relations over the past few decades. Narrow ways, in that the starting-point for many of our discussions seems to be a fairly unreflective commitment to a deductive-nomothetic hypothesis-testing model of "science," and accordingly the discussions descend all too quickly to the technical level of particular tools that can help to advance that unquestioned epistemic goal. Biased ways, in that the very terms that we use to frame and characterize the logic of social-scientific inquiry incline toward one way of proceeding—neopositivism—and generate an uphill battle for anyone wishing to advocate a different variety of social science. Chief among these biased terms, in fact, is the term "epistemology," since the traditional project of epistemology was almost entirely wrapped up with a particular way of conceptualizing the relationship between the mind and the world or between the knower and the known (Taylor 1995: 3–5, 14–17); that is why I am at such pains in the book to redirect the discussion towards methodology *broadly* understood, and away from a more or less exclusive focus on ways of increasing our confidence in general claims about cross-case covariation.

My interlocutors raise a variety of trenchant points, too