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

https://doi.org/10.5281/zenodo.937446

**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 Grynaviski, 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 many for me to exhaustively deal with here. But in general let me point out that the issues that they raise signal *precisely* the kinds of broad conversations that I hope that the book provokes and continues to provoke: conversations about what we are doing when we engage in social-scientific inquiry, conversations about what we ought to be doing, and conversations about how we can do it better. Conversations that do not take as their starting-point a specious notion of "*the* scientific method" or "*the* scientific way to study social life," but instead recognize that there are multiple ways of proceeding, multiple ways that are not reducible to one another. Conversations that do not start off with the common-practice fallacy—"this is how lots of people do things, therefore this is how we should do things"—but instead seek to provide positive warrants for diverse approaches to research design and the evaluation of substantive claims. If people read and react to the book in the way that my interlocutors have, then I will count the book a successful contribution to a richer discussion of these and related questions.

Of course, one can't focus on everything at once, so in the remainder of this response I'm going to engage three issues raised by my interlocutors: why I distinguish between the argument presented in *C of I* and an argument about the sociology of our scholarly field; why I think a reconstruction of diverse commitments in philosophical ontology and the methodologies to which they give rise at the present time is a useful exercise; and why I am opposed to efforts to combine methodologies.

**Locating the Text**

I am delighted to hear that Banks and O'Mahoney found my discussion of methodology in *C of I* helpful for their efforts to think through their own projects; that is the primary use that I hope individual scholars will make of the book. Their concluding reflection that "[a]lthough this book does not help us... 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" is one that I take not as a criticism, but as simple observation that one cannot do everything at once. Had I spent more time in the book in the crevices and crannies of contemporary scholarship, it might have been more difficult to achieve the kind of broad-brush depiction of different methodologies that forms the core of the book's argument. The kind of pressures that more or less compel scholars to emphasize their differences from one another (brilliantly discussed in Abbott 2001) makes it extremely difficult to get a clear view of the whole scholarly landscape and the implicit conceptual scaffolding undergirding it. Faced with a choice between writing a detailed account of what people are saying at the moment, and advancing a broader account of the basic categories with and within which they are operating, I chose the latter course.

Besides which, all too much contemporary discussion in the field about logics of scientific inquiry is sometimes so confused that the best way to "navigate" it is probably to steer clear of it as much as possible. For example, as Daniel Nexon and I have argued (2009), the use of terms like "paradigm" and "research programme" within International Relations and Political Science often bears little resemblance to the actual use of those terms by philosophers of science to assess scientific progress in fields like physics. For another example, the broad use of a phrase like "hypothesis-testing" to refer, as Banks and O'Mahoney observe, to "specifying what data might be relevant to one's research question before doing the research, or simply being clear about the claims that one is making" strikes me as obfuscation. We have less philosophically freighted terms for these operations; to my mind, "specifying relevant data" and "being clear about claims" seem like perfectly reasonable pieces of advice on their own, so I'm unsure what good attaching a phrase like "hypothesis-testing" to that advice would do.[1] Hypothesis-testing *does* carry philosophical baggage with it, and it should *not* be used simply as a generic term for making clear claims and using evidence to evaluate them, because there are *other ways* of using evidence to evaluate claims—ways that I endeavor to elucidate in the book. In such situations, the best way to deal with conventional use is, I think, just to walk away from it.

In some ways I have a very similar reaction to Gunnell's entirely accurate observation that I downplay sociology of science considerations about the political context of philosophical claims as my discussion moves into the heart of 20th century scholarship on world politics. While part of the reason that I do this is because of the fact that there is already some extremely good work—including his own—on these considerations, perhaps a larger part of the reason why I downplay sociological factors is because my target is not to explain the present shape of the field, but to intervene in the field and hopefully disclose some of the ongoing tensions and strategic misunderstandings within our conversations about methodology. While "how we got here," like "precisely what people are saying at the moment," can be a helpful way of getting at those bigger issues, I regard them (at least for the purpose of this book) as means to an end, to be pursued only as far as they help both author and reader to make sense of the conceptual issues tacitly in play.

It is for this reason that I privilege the philosophy of science (and not the sociology of science) in the book: if one wants to mount an internal critique of conversations in a field where the notion of "science" enjoys widespread currency, then taking "science" seriously seems like the basic ante for the game. As it happens, even a cursory examination of debates in the philosophy of science quickly reveals the poverty of the vocabulary current in Political Science and International Relations for discussing methodological questions. Sociology of science, which seeks to historicize and contextualize such vocabulary along with the concrete research practices to which it is linked, is not necessarily the best tool for improving our discussions. This is especially true insofar as "science" in a self-proclaimed scientific field functions as a *foundational* claim, meaning that it declares itself capable of grounding or warranting concrete research practices; while I am not persuaded that one need therefore be a foundational*ist* about science (see Jackson 2009 and Chernoff 2009 for an elaboration of this distinction), I do think that it is important to engage that foundation on its own terms if one wants to open some "thinking space" (as in George and Campbell 1990) within it.

In this way, I have endeavored to position *C of I* in about the same place that Max Weber was standing when formulating and delivering his famous lectures on science and politics (Weber 2004). Weber sought to work through some contemporary debates in a way that would give him a clearer view of the basic conceptual issues involved, particularly the distinctions and transactions between the areas of science, politics, and religious faith. It would, I think, be inaccurate to read Weber's empirical statements in those lectures as the point of the exercise; rather, the lectures were intended to serve a *hortatory* function, and to advise his listeners about the boundaries of realms of practice by developing a conceptual apparatus adequate to the task.[2] Similarly, if nowhere near as sweeping in its scope, *C of I* is intended to take a critical look at our contemporary discussions of how to do research and then to propose ways to improve those discussions. The book thus stands—deliberately—at the border of those discussions, taking no specific position within them so that it can address the shape of the discussion as a whole.

**Reconstruction and Diversity**

The non-standard terminology that Grynaviski and Gunnell note in the book is, as Banks and O'Mahoney rightly point out, an integral part of my argumentative strategy. Because certain words (like "epistemology" or "positivism") have acquired conventional meanings in our field that prevent them from being particularly useful tools for conducting a broad and pluralist debate, I chose to avoid them, and to formulate my central 2x2 map with the lesser-known axes of "mind-world dualism vs. mind-world monism" and "phenomenalism vs. transfactualism." I prefer these terms in part because they do not have the baggage associated with our conventional terminology—terminology which obscures and devalues both mind-world monism and transfactualism, and as such biases the debate toward the combination of mind-world dualism and phenomenalism familiar to us in "neopositivist" methodology. (Actually, it's probably more familiar to us as "positivism" or "the scientific method," but those are even less useful labels.) By introducing novel terminology, even at the cost of forcing the reader to work through it in the first couple of chapters, I seek to avoid simply saying what has already been said, and instead focus on organizing what has already been said (and what is currently being said) into a more useful set of categories.

Thus, the point of the exercise is to reconstruct diverse logics of inquiry in a way that allows us to think more systematically about what it might mean to do research in different ways. In practice, given the rather unreflective prevalence of neopositivism in the official pronouncements both of our leading research design textbook (King, Keohane, and Verba 1994) and its erstwhile critics (Brady and Collier 2004; George and Bennett 2005), this means thinking more systematically about what it might mean to do non-neopositivist research. My set of distinctions, which I call a "metamethodological lexicon" in the last chapter of the book,[3] is designed to do just this, both by foregrounding the commitments that tacitly support neopositivist research practices and by exploring the methodological entailments of other commitments. The reason that I call this work "metamethodological"—dealing with the philosophical foundations of methodology—and not, say, "epistemological," is because my target here is broader than traditional questions about how subjects acquire valid knowledge of objects, and is instead concerned with the more general question of how factual knowledge is produced. Gunnell quite rightly notes that methodology is usually thought to follow from epistemology, but I would say that precisely this "following from" supports the traditional epistemological project, and biases the whole conversation in favor of mind-world dualism and its concern with validly crossing the gap between the mind and the world. Admittedly, I could have generated a neologism here too, but the distinction between method and methodology already had some presence in the existing conversation (e.g., Sartori 1970: 1033; Waltz 1979: 12–13; Schwartz-Shea and Yanow 2002: 459–460), so I elected to retain it.

The distinctions that I draw in order to help us get a handle on the relevant methodological issues—a distinction between different ways of conceptualizing the mind-world interface, and a distinction between different ways of relating knowledge to experience—are ideal-typical distinctions in philosophical ontology. Both of those aspects of my typology require brief elaboration, in the light of some of my interlocutors' comments. Philosophical ontology, a notion I adapted from Mario Bunge via Heikki Patomäki and Colin Wight (2000), refers to that which is logically prior to particular substantive claims and theories—that is, issues pertaining to our "hook-up" to the world (Shotter 1993: 73–79). Methodology, understood as distinct from and prior to "method," operationalizes or enacts philosophical ontology, standing on a set of often-tacit philosophical commitments as it delineates concrete strategies of inquiry. I am thus suggesting, along with many other methodologists, that we should think about issues of research design and knowledge-production *separate from* any particular substantive account of the world and the things in it; to modify the critical realist catchphrase, we should indeed put ontology first, as long as it's philosophical ontology we're talking about. But I am simultaneously suggesting that we should think about these issues as *irresolvable* on purely logical or philosophical grounds; there is no definitive argument for or against any particular commitment of (or combination of commitments of) philosophical ontology, and so—like the skeptical humanists treasured by Stephen Toulmin (1992)—we have no defensible alternative but tolerance, both of alternate methodologies and of the commitments of philosophical ontology that underpin them.

It is important to keep in mind the ideal-typical character of the distinctions I am drawing. Because of this character, the categories that I flesh out in the book's central chapters are, *by definition*, artificially pure; they perfectly describe neither concrete actual authors nor concrete actual research strategies. But it is their abstract logical purity that constitutes their value as conceptual devices for helping us to think through the implications of our commitments, thereby clarifying our methodological stances. Each of the four central chapters in *C of I* takes one combination of philosophical-ontological commitments and discusses some of the methodological implications of those commitments, and I have endeavored to do so in such a way that the result is not an "unraveling" (*contra* Banks and O'Mahoney) of any position but rather a delineation of what that position logically implies. To respond to Banks and O'Mahoney's concern about my treatment of critical realism: you can't coherently be a critical realist unless you take pains to vet posited causal powers of objects either in a laboratory or via transcendental argument, because otherwise you'd just be positing things and regarding them as true without any evidence. But this is not a *problem* with critical realism, but is instead the basic *point* of a critical realist approach to science. Whether self-proclaimed critical realists in our field actually do either of these two things is another matter; my point is that their very methodology and the commitments of philosophical ontology on which it stands directs them to do so.

That said, I would never claim that any of the authors discussed in the central chapters of the book are somehow perfectly located within a particular box in my typology. Indeed, it would be surprising if they were, since the typology itself inhabits an idealized conceptual realm, and like all ideal-types would be quickly falsified in practice if it were treated as a description (see Weber 1999: 192–194). The point is not just that no actual work perfectly matches all of the standards logically entailed in a particular quadrant of the typology. Rather, the point is that actual authors and their works are a good deal more ambiguous than *any* abstract delineation of their main points. This is precisely why the purpose of closely reading a text is not to generate a definitive and incontrovertible summary of its argument, but instead to generate a defensible account of the work as a whole—an ineluctably interpretive process. Hence, locating an author with respect to her or his methodology and philosophical ontology can never be a simple matter of proving beyond a reasonable doubt that author X belongs in box Y; instead, the relevant questions are: Is this reading sustainable, especially for the text or the author as a whole? and does reading author X through category Y help to illuminate the point at issue, whether that point pertains to the text in particular or whether it pertains to the broader argument?

Applying those standards to Grynaviski's argument that I have misread both the American pragmatists and Kenneth Waltz in explicating the analyticist methodological stance, I must admit to being somewhat puzzled. There are certainly lines in Dewey and Pierce that can be read as consistent with the neopositivist procedures of hypothesis-testing and the quest for nomothetic generalizations, but *sustaining* that reading is more difficult in the face of both authors' pronounced reluctance to make either hypothesis-testing or nomothetic generalization the key warrant for valid knowledge. Indeed, Dewey argued (1920: 169) that the point of abstract systematization through scientific inquiry was to create analytically general claims that could serve as "tools of insight; their value is in promoting an individualized response to the individual situation." There is a world of difference between treating a claim about whether states pursue security as this kind of analytically general claim, and (as Grynaviski suggests) treating such a claim as an empirically general proposition: the former is a model that can be useful or not useful and can also be calibrated or updated, while the latter is a hypothetical conjecture that can only be falsified.[4]

Similarly, although one might read Waltz as a neopositivist interested in the testing of hypothetical generalizations—and I freely admit that this is the usual way that Waltz is read in the field—this reading can only be sustained if one downplays or ignores both Waltz's structural-functionalist roots (Goddard and Nexon 2005, 17–18) and his "theory of theory" (Wæver 2009: 206–208). Both of these aspects, by highlighting the importance of conceptualization and imagery, sharply differentiate Waltz's own efforts from those of scholars articulating general, falsifiable empirical propositions. Indeed, the pages of *Theory of International Politics* that Grynaviski cites (Waltz 1979: 124–125) do not, when read in context, unequivocally support a reading of Waltz as a neo-positivist, for at least two reasons:

> (1) Waltz speaks of "confirming" a theory and designing evaluations that, if passed, will help a theory begin to "command belief," but these are operations that a neopositivist can never consistently perform. For a neopositivist, knowledge is only ever an unfalsified conjecture, liable to falsification at any time; belief in confirmation, as Popper (1970; 1996) might have said, provides an obstacle to scientific progress by immunizing certain propositions from testing.[5] So Waltz, by deploying language of this sort, sounds less like a neopositivist and more like something quite different.[6]
>
> (2) Waltz suggests that a theory demonstrates its worth by helping us make sense of events "within a given area and over a number of years," which does not sound like the kind of covering-law explanation sought by neopositivists. Admittedly, to a neopositivist this might initially sound very much like "scope conditions" or some other kind of a call for middle-range theorizing, but note that Waltz never proposes that the *theory* be modified by introducing such empirical boundaries; rather, he suggests that the theory—itself analytically general, and unmodified—helps us make sense of a particular case (since a case is, of course, a unit of observation and not a concrete entity like a state).

Admittedly, Waltz's "penchant for ambiguity" (Goddard and Nexon 2005: 22), or at any rate his ambiguous use of "terminology about hypotheses…allowed easy assimilation" to prevalent neopositivist understandings (Wæver 2009: 211). Part of the point of my reconstruction of Waltz, like those of Wæver and Goddard and Nexon, is to clear up some of that ambiguity. By linking together the tantalizing hints in Waltz's seminal book—and some of his very disparaging comments about prediction and hypothesis-testing in subsequent publications (e.g. Waltz 1996; 1997)—a picture of Waltz as an analyticist emerges, despite Waltz's sometimes unclear use of methodological terms.

**Beyond the Semblance of Pluralism**

But given the ambiguities of textual interpretation and the alternative rules that might frame interpretative strategies (favoring charity versus suspicion, consistency versus contradictions, and so on), a debate about whether to read Waltz as an analyticist or a neopositivist soon hits diminishing intellectual returns. What is more important to recognize is that, even if Waltz is read as an analyticist, there is nothing to stop any neopositivist from reading Waltz, extracting a claim, converting it into a falsifiable proposition, and proceeding to test it. This is, in fact, precisely how neopositivism is supposed to work. But to then turn around and claim that the results of that test should have some bearing on what Waltz was doing in the first place, or to claim that the testability of that proposition would somehow "prove" that the original source was a neopositivist, is to overstep the boundaries of a pluralist approach to methodology and to at least implicitly legislate one methodology as exhausting the boundaries of "science" per se.

This is the flaw in the Empirical Implications of Theoretical Models approach to the use of formal models, in the effort to evaluate feminist claims about patriarchy by gathering evidence about gender discrimination, and in the attempt to read social mechanisms as intervening variables: the problem is not that advocates of one methodology take insight and inspiration from others, but that the advocates of one methodology (neopositivism, for the most part) claim exclusive rights to evaluate all empirical claims on their philosophical-ontological terms. Testing a hypothesis derived from a formal or an informal analytical model (for example) tells us precisely *nothing* about the worth of the model because the epistemic standards appropriate to an analytical model are distinct from those appropriate to falsifiable empirical generalizations. So my opposition to combining methodologies, simply stated, is that I think that it is impossible to combine methodologies, because every logically coherent piece of social science will end up having a dominant epistemic warrant for its claims even if it derives some of those claims from other sources. A neopositivist testing a hypothesis derived from a formal model is not engaging in "mixed methodology" or "multiple methodology" research; she or he is engaging in *neopositivist* research while testing that hypothesis.[7]

In addition to this logical and conceptual barrier to combining methodologies, there is also a practical reason to refrain from doing so: in a field marked by the dominance of neopositivism, a "mixed methodology" is likely to be neopositivist. Although virtually no one is *officially* against pluralism nowadays, many scholars are in effect against methodological pluralism in their research practices and in their engagement with other scholars. The clearest example of this that I know of is David Laitin's (2003) "tripartite methodology," in which formal models pass claims to the neopositivists who use both large-n and small-n hypothesis-testing to evaluate them.[8] Declarations of tolerance for practitioners of reflexivist fieldwork as long as they provide systematic data that can be used to code variables of interest (e.g., King, Keohane, and Verba 1994: 37–43) also provide the same kind of specious pluralism. And an exchange in the journal *International Theory* (2:1, 2010) between Andrew Moravcsik and Beate Jahn about the character of liberal theory was unproductive precisely to the extent that the fundamental methodological differences between the authors were not even acknowledged by Moravcsik; instead, calls for the systematic testing of empirical propositions stood in as a substitute for genuine methodological discussion.

All of this leads me to conclude that even though "pluralism" is the sort of thing that everyone claims to be for, it is in fact not the kind of thing that everyone actually practices. It is, as Colin Elman (2009) stressed in the last issue of this newsletter, "a hard choice" that calls for greater learning about and tolerance of other approaches, and greater care in our claims about the character of our field, than has usually been recognized.[9] A pluralism of mere method within a single methodological framework is not the same thing as a genuine methodological pluralism that would embrace and celebrate fundamentally different ways of producing knowledge. The starting point for any such pluralism, I think, has to be a richer vocabulary for discussing methodological issues, and a vocabulary that begins from the position of important and consequential differences obtaining between methodologies. *The Conduct of Inquiry in International Relations* is intended to contribute to the crafting and to the refinement of such a vocabulary, so that we can continue to do our work—in all of its varied forms—without constantly having to defend the legitimacy of what we are doing against dismissive critics operating with an overly narrow view of science. It is against such critics that the book is directed; it is for the rest of us that the book was written.

**Notes**

[1] Of course, a usage like this might be deployed strategically—even cynically—to help a dissertation prospectus or grant proposal pass muster with neopositivist referees. But any gains to the individual researcher here come at the collective expense of helping to prolong the fiction that something called "hypothesis-testing" is at the heart of all social science—comforting to neopositivists, perhaps, but not especially helpful to the rest of the field.

[2] Whether Weber's broader goal in his discussions of the politik/wissenschaft divide was to preserve the epistemic authority of science in the face of partisan considerations, as Gunnell maintains in his contribution, seems to me to be a slightly different issue. I think it is equally plausible to read Weber as seeking not to preserve science as a non-partisan force in politics, but as seeking to free science and particularly social science for fulfilling the very different social role of formalizing cultural values—but this is a subtle matter of "Weber studies" that we don't have to get into here.

[3] Gunnell is quite right that my use of the term "lexicon" diverges somewhat from Thomas Kuhn's, even though I borrow the term and some of the sensibility from Kuhn's later work (collected in Kuhn 2000). Kuhn remained focused on *substantive* vocabularies throughout his career, while my concern here is with *methodological* issues; however, the emphasis on the historicity and indexicality of key terms, plus the logical untranslatability of certain claims between lexicons, is common to us both.

[4] At the risk of turning this into a discussion about subtle points of Dewey interpretation, I should point out that the section of Dewey's *Logic* that Grynaviski cites is contained in a discussion about why the idea of falsification has to be replaced with "the institution of a contradictory negation" as part of a process of revising a general claim to account for seemingly discrepant evidence (Dewey 1938: 196–

198). The sequence that Grynaviski describes—general claim about state behavior, discrepant evidence provided by Mearsheimer and Walt, reformulated general claim that takes discrepant evidence into account—is a pragmatic procedure rather than a neopositivist one, precisely because neopositivist hypothesis-testing provides no logical way to link a falsified proposition with a successor proposition (except, perhaps, through Lakatosian retrospective reconstruction, and that raises a whole different set of concerns). Pragmatic analyticism, which never treated the general claim as a falsifiable proposition in the first place, has no such problem.

[5] Lakatosian language about "hard cores" is no help here, since Lakatos is very explicit that his philosophical procedure gives no advice to the practicing scientist about which propositions to believe (see, in particular, Lakatos 1970: 178–179).

[6] In fact, Waltz's language here sounds quite strikingly like the language characteristic of pre-Popperian, old-school Vienna Circle logical positivism—which inclined in a decidedly *monistic* direction when it came to the mind-world interface, and was accordingly very much at odds with Popperian notions of falsification (see the discussion in Jackson 2010: chap. 3). Systems theorists prior to Waltz—Talcott Parsons, Morton Kaplan, *et cetera*—had similarly monistic/logical positivist inclinations.

[7] If she or he was also responsible for building the model in the first place, then during model-construction and calibration she or he was engaged in analyticist research; the unit of analysis here is the *argument*, not the *person*. Grynaviski seems to be reading Waltz as doing precisely this: operating as an analyticist when developing his model of the international system, and then operating as a neopositivist when evaluating the model. As I've said, I disagree with this as an interpretation of Waltz, but even if I agreed with it, the fact would remain that what Grynaviski would read as the two different parts of Waltz's argument would be *logically* distinct, and we'd have two methodologies, not a single "mixed methodology."

[8] For my criticisms of Laitin's tripartite methodology, see Jackson (2006); for a variety of views see the symposium on Laitin in the Spring 2006, Vol. 4, No. 1 issue of this Newsletter.

[9] Although, when push comes to shove, I'm considerably more skeptical than Elman seems to be about what he calls "the limits of association and commensurability between several equally valid epistemes," since I don't think that there's any meaningful kind of "commensurability" to be had between different commitments of philosophical ontology and the methodologies that they entail; in this I agree with some of the authors skeptical of "multi-method" research in that issue of the newsletter, such as Ahmed and Sil (2009), and Chatterjee (2009). I also fear that searching for such "commensurability" would end up leading us back into methodological univocality in tacit support of neopositivism.

## References


Abbott, Andrew. 2001. *Chaos of Disciplines*. Chicago: University of Chicago Press.

Ahmed, Amel, and Rudra Sil. 2009. "Is Multi-Method Research Really 'Better'?" *Qualitative and Multi-Method Research* 7:2, 2–6.

Brady, Henry E. and David Collier, eds. 2004. *Rethinking Social Inquiry: Diverse Tools, Shared Standards*. Lanham, MD: Rowman & Littlefield Publishers.

Chatterjee, Abhishek. 2009. "Ontology, Epistemology, and Multi-Methods." *Qualitative and Multi-Method Research* 7:2, 11–15.

Chernoff, Fred. 2009. "Defending Foundations for International Relations Theory." *International Theory* 1:3, 466–477.

Dewey, John. 1920. *Reconstruction In Philosophy*. New York: Kessinger Publishing, LLC.

Dewey, John. 1938. *Logic: The Theory of Inquiry*. New York: Henry Holt and Company, Inc.

Elman, Colin. 2009. "Letter from the Section President: Pluralism as a Hard Choice." *Qualitative and Multi-Method Research* 7:2, 1–2.

George, Alexander L. and Andrew Bennett, eds. 2005. *Case Studies and Theory Development in the Social Sciences*. Cambridge, MA: MIT Press.

George, Jim and David Campbell. 1990. "Patterns of Dissent and the Celebration of Difference: Critical Social Theory and International Relations." *International Studies Quarterly* 34:3, 269–293.

Goddard, Stacie E. and Daniel H. Nexon. 2005. "Paradigm Lost? Reassessing Theory of International Politics." *European Journal of International Relations* 11:1, 9–61.

Jackson, Patrick Thaddeus. 2006. "A Statistician Strikes Out: In Defense of Genuine Methodological Diversity." In *Making Political Science Matter: Debating Knowledge, Research, and Method*. Sanford Schram and Brian Caterino, eds. (New York: New York University Press), 86–97.

Jackson, Patrick Thaddeus. 2009. "A Faulty Solution to a False(ly Characterized) Problem: A Comment on Monteiro and Ruby." *International Theory* 1:3, 455–465.

Jackson, Patrick Thaddeus. 2010. *The Conduct of Inquiry in International Relations*. London: Routledge.

Jackson, Patrick Thaddeus and Daniel H. Nexon. 2009. "Paradigmatic Faults in International-Relations Theory." *International Studies Quarterly* 53:4, 907–930.

King, Gary, Robert O. Keohane, and Sidney Verba. 1994. *Designing Social Inquiry: Scientific Inference in Qualitative Research*. Princeton: Princeton University Press.

Kuhn, Thomas S. 2000. *The Road Since Structure: Philosophical Essays, 1970–1993*. James Conant and John Haugeland, eds. Chicago: University of Chicago Press.

Laitin, David. 2003. "The Perestroikan Challenge to Social Science." *Politics and Society* 31:1, 163–184.

Lakatos, Imre. 1970. "Replies to Critics." PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association 1970, 174–182.

Patomäki, Heikki and Colin Wight. 2000. "After Postpositivism? The Promises of Critical Realism." *International Studies Quarterly* 44:2, 213–237.

Popper, Karl. 1970. "Normal Science and its Dangers." In *Criticism and the Growth of Knowledge*. Imre Lakatos and Alan Musgrave, eds. (Cambridge: Cambridge University Press), 51–58.

Popper, Karl. 1996. *Myth of the Framework: In Defence of Science and Rationality*. London: Routledge.

Sartori, Giovanni. 1970. "Concept Misinformation in Comparative Politics." *American Political Science Review* 64:4, 1033–1053.

Schwartz-Shea, Peri and Dvora Yanow. 2002. "'Reading' 'Methods' 'Texts': How Research Methods Texts Construct Political Science." *Political Research Quarterly* 55:2, 457–486.

Shotter, John. 1993. *Cultural Politics of Everyday Life*. Toronto: University of Toronto Press.

Taylor, Charles. 1995. *Philosophical Arguments*. Cambridge: Harvard University Press.

Toulmin, Stephen. 1992. *Cosmopolis: The Hidden Agenda of Modernity*. Chicago: University of Chicago Press.

Waltz, Kenneth N. 1979. *Theory of International Politics*. New York: McGraw-Hill.

Waltz, Kenneth N. 1996. "International Politics is not Foreign Policy." *Security Studies* 6:1, 54–57.

Waltz, Kenneth N. 1997. "Evaluating Theories." *American Political Science Review* 91:4, 913–917.

Wæver, Ole. 2009. "Waltz's Theory of Theory." *International Relations* 23:2, 201–222.

Weber, Max. 1999. "Die 'Objektivität' Sozialwissenschaftlicher und Sozialpolitischer Erkenntnis." In *Gesammelte Aufsätze zur Wissenschaftslehre*. Elizabeth Flitner ed., Potsdam: Internet-Ausgabe, http://www.unipotsdam.de/u/paed/Flitner/Flitner/Weber.

Weber, Max. 2004. *The Vocation Lectures*. Indianapolis: Hackett Press.

# *Two Cultures: Hume's Two Definitions of Cause*

**Gary Goertz**
University of Arizona
*ggoertz@u.arizona.edu*

**James Mahoney**
Northwestern University
*james-mahoney@northwestern.edu*

> That and no other is to be called cause, at the presence of which the effect always follows, and at whose removal the effect disappears.
>
> *Galileo*

A famous quote from David Hume provides a useful way to introduce two different approaches to causation in the social sciences:

> We may define a cause to be *an object followed by another, and where all the objects, similar to the first, are followed by objects similar to the second*. [definition 1]...Or, in other words, *where, if the first object had not been, the second never would have existed*. [definition 2] (David Hume in *Enquiries Concerning Human Understanding, and Concerning the Principles of Morals* 1975 [1777])

As many philosophers have suggested, Hume's phrase "in other words" is misleading, if not completely incorrect.[1] The phrase makes it appear as if definition 1 and definition 2 are equivalent, when in fact they represent quite different approaches. Lewis writes that, "Hume's 'other words'—that if the cause had not been, the effect never had existed—are no mere restatement of his first definition. They propose something altogether different: a counterfactual analysis of causation" (Lewis 1986a: 160).

Following Lewis, we shall call Hume's definition 2 the "counterfactual definition." By contrast, we shall call definition 1 the "constant conjunction definition," to highlight Hume's idea that causes are always followed by their effects.[2] In this short essay, we consider how these two definitions have informed understandings of causation in the qualitative and quantitative research traditions in political science. Following our earlier work, we characterize these traditions as representing contrasting cultures marked by diverse beliefs, norms, and values (Mahoney and Goertz 2006).

It bears emphasizing that we are not arguing that our interpretations should be attributed to Hume himself. Hume's views on causation have been the source of enormous debate among philosophers, and we make no claim to resolving that debate. Rather, our purpose is to use Hume's definitions, which are widely reproduced in discussions of causation, as a device for discussing the different ways in which political scientists understand the concept of a cause.

### The Quantitative Tradition

Before the rise of the Rubin approach (see Morgan and Winship [2007] for a good survey), statistical discussions of causation focused on Hume's constant conjunction definition (definition 1) within a probabilistic framework. For example, Suppes, in an early and prominent analysis, wrote that, "Roughly speaking, the modification of Hume's analysis I propose is to say that one event is the cause of another if the appearance of the first event is followed with a high probability by the appearance of the second" (Suppes 1970: 10).[3] Under this probabilistic approach, it seems natural to understand the constant conjunction definition in terms of correlation. Thus, definition 1 suggests that causation occurs when there is a strong, or at least statistically significant, correlation between $X$ and $Y$. While all know the mantra "correlation is not causation," in practice statistically significant correlations are very central in identifying causal relationships.

One can also develop a statistical interpretation of Hume's counterfactual definition (definition 2). Doing this requires some work, however, because Hume's counterfactual definition implies a single case. Unlike definition 1, which states "all objects [plural] are followed...," definition 2 states "if the first object [singular] had...." To interpret definition 2 in a constant conjunction fashion, therefore, requires expanding Hume's idea to multiple cases.

The quantitative tradition accomplishes this move by interpreting both definitions 1 and 2 in terms of constant conjunction across many cases. A correlation of 1.00 means that there is a constant conjunction of $X = 1$, $Y = 1$ and $X = 0$, $Y = 0$. Definitions 1 and 2 can thus be fused together into one statistical interpretation. Definition 1 holds that when the cause is present, the outcome will be present (probabilistically). Definition 2 holds that when the cause is absent, the outcome will be absent (probabilistically). Since it makes no statistical sense to just look at cases of $X = 1$ without cases of $X = 0$ (or vice versa), the two definitions become joined as one. Neither definition can stand alone and make statistical sense. But when fused together, they offer a coherent symmetrical understanding of causation, one in which the emphasis is on what follows different values on the independent variable.

As of 2010, it seems safe to say that the dominant statistical view on causation in political science and sociology is the Rubin model.[4] Perhaps its most important innovation within statistical circles was the emphasis on the counterfactual basis of causation. For example, Morgan and Winship's excellent summary is called *Counterfactuals and Causal Inference*. Earlier statistical and probabilistic accounts are understood to have ignored or underappreciated this crucial aspect of causation.

The Rubin approach starts with the individual case and then builds a full-blown statistical model of causation. Using