

Critical Dialogue

International Relations' Last Synthesis? Decoupling Constructivist and Critical Approaches. By J. Samuel Barkin and Laura Sjoberg. New York: Oxford University Press, 2019. 232p. \$81.00 cloth.
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J. Samuel Barkin and Laura Sjoberg are committed to making international relations as open a field of inquiry as possible. Their commitment is ethical but also practical as they argue—and I concur—that openness and tolerance promote more diverse and better research. Fields narrow for many reasons, and the one they focus on is the phenomenon of an intellectual “synthesis.” In theory, it is intended to transcend differences between approaches and encourage a “theoretical peace.” The authors argue that the attempted synthesis of neorealism and neoliberalism in the 1980s *reduced* richness and diversity by excluding, or at least penalizing, scholars who rejected or worked outside the synthesis. The same narrowing is happening again today, they contend, as a result of the more recent synthesis of constructivism and critical theory. Such syntheses, they argue, are intellectually questionable as well as negative in their implications. Barkin and Sjoberg acknowledge that constructivism and critical theory share some important characteristics but insist that they represent two different approaches to IR rather than components of a single theory.

To make their case, Barkin and Sjoberg devote two chapters to constructivism. They describe multiple forms of constructivism, many of which share ontological assumptions and methods. There are also important differences within each of these paradigms or research programs. Theories that qualify as constructivist, they argue, incorporate an ontology of co-constitution and intersubjectivity. Researchers rely on diverse methods for teasing out these relationships and how they affect international politics. Barkin and Sjoberg describe three kinds of constructivism: theories that build on norms, rules, and identities. In contrast to critical theory, Barkin and Sjoberg argue that constructivism does not embody a morality of politics. They might add that this absence also distinguishes constructivism from most forms of realism, liberalism, and Marxism.

Constructivism, they note, can be associated with different political approaches, and they make the case for a “politically promiscuous” paradigm (p. 159).

Two following chapters make similar arguments about critical theory. There is much diversity among theories of IR that qualify as critical. They include those with explicit emancipatory goals, with many kinds of feminist theory, as well as with poststructuralist and postmodernist theories. These theories and approaches share a common ontology, belief that politics matters, and identify similar mechanisms by which politics is said to work. Efforts to build a synthesis of constructivist and critical theories involve a misreading of both research traditions and a rejection of their diversity in favor of one characterization. Such an effort “is intellectually bankrupt, normatively problematic, politically ineffective, and just plain wrong” (p. 17).

In their conclusion, Barkin and Sjoberg elaborate on their argument that efforts at synthesis are counterproductive to the dialogue they seek to foster. They maintain that constructivism need not be critical, and that critical theories need not be constructivist. However, the two kinds of theories can also be combined, but not in the form of a synthesis. Constructivism is a social theory that can be used as a method by critical theorists who are anchored in a political theory. Critical theories can also adopt constructivist social theories for their research. However, the important differences between constructivism and critical theory should not be ignored. Exploration of these differences and tensions, and foregrounding, not glossing over, them has the potential to promote a useful dialogue and richer and more diverse research programs.

I find the book’s argument compelling and on the whole well laid out and developed. In physics, syntheses work well, and progress in the field can be measured by its integration in the nineteenth century of electricity and magnetism, and in the postwar era, of both with the weak and strong forces. Only gravity remains outside the standard model and may someday be incorporated. Some physicists have raised concerns about the model but not about the idea of a synthesis that unifies all of nature’s forces. The social world, and international relations in particular, is different. We have no general laws to which phenomena can be subsumed, nor can we ever expect to develop them. It makes no sense for social science to ape

physics. General theories are fine, but the idea of a general theory is impractical and dangerous. It dramatically distorts and narrows research in the process. We benefit from multiple theories in multiple traditions that respond to the intellectual and policy needs of the moment. As Barkin and Sjoberg recognize, comparisons and competition among them are beneficial to the field, especially when they highlight different assumptions and goals and their implications. These comparisons can also be useful when focused on the problems these different theories or approaches share, as most do, for example, in confronting data (see for example Richard Ned Lebow and Mark I. Lichbach, eds., *Theory and Evidence in Comparative Politics and International Relations*, 2017).

To the extent I have any differences with the authors, it is with respect to Barkin and Sjoberg's single-minded focus on ontology as the principal conceptual basis for comparison of theories. This leads them to too expansive and too narrow a definition of constructivism. They identify co-constitution and intersubjectivity as defining ontological commitments of the paradigm. So-called thin constructivism of the sort advocated by Alexander Wendt does not really meet their definition yet they describe him as a constructivist. He ascribes identities to actors before they interact with others, and claims that they have strong incentives—even little choice—but to maintain the order they have created. Yet he also believes the liberal world order is inevitable (see Alexander Wendt, *Social Theory of International Politics*, 1999, pp. 326-36). By their criteria, Wendt is best characterized as a structural liberal.

Barkin and Sjoberg make no mention of my *Cultural Theory of International Relations* (2008), although it is to my knowledge the only general constructivist theory of IR. Perhaps they do not consider me a constructivist because I do not foreground co-constitution and intersubjectivity, although both feature in my theory. Instead, I build on human motives, the emphasis societies put on different motives, and how these motives are socially channeled. I theorize that the motives of appetite and thumos give rise to different principles of justice, generate different kinds of hierarchies, and have different implications for cooperation, conflict, and risk-taking. The emotion of fear can become dominant when reason loses control of appetites and thumos. It rests on no principle of justice and gives rise to its own kind of hierarchy and propensities for cooperation, conflict and risk-taking.

The explanation for this improper admission and exclusion from constructivism has to do, I believe, with the authors' downplaying of epistemology. The concept gets considerable mention but is not at all central to their analysis and categorization of theories. They rely instead on ontology and political commitments, if any. Yet

epistemology is critical to any scheme of classification. Even casual reference to it would reveal someone like Wendt as a positivist, in contrast to thick constructivists, all of whom to my knowledge are interpretivists. It would also characterize my work as unambiguously constructivist given its emphasis on reflexivity and the social nature of politics.

I want to be clear that I am not invoking epistemology or citing Wendt to exclude anyone. Rather, I want to show how the fit between a scholar and a paradigm very much depends on the criteria that are used for this purpose. Ironically, Barkin and Sjoberg, who wish to be inclusive, exclude me, and perhaps other self-avowed constructivists, by using the criteria that they do. They should acknowledge the sensitivity of their criteria, and the need, or at least the possibility, of using multiple criteria for categorization. Multiple criteria are another way of exploring relationships between and among research programs and theories. We can ask which fit together—or not—depending on the criteria chosen. This process can also be extended across research programs.

Through the lens of epistemology it becomes immediately apparent that most forms of constructivism are interpretivist. This is also true of critical theory, although not for many kinds of Marxism. In *The Quest for Knowledge in International Relations* (2022) I argue that the big divide in IR is between positivism and interpretivism, between those who believe in objective, cumulative knowledge and those who stress reflexivity and the subjective nature of knowledge. Epistemological differences are far more important than methodological ones; they generally determine the kind of method thought appropriate. Epistemology, like ontology, cuts across paradigms. We find positivist and interpretivist realists, liberals, Marxists, and constructivists—if we count thin constructivists among them.

Barkin and Sjoberg make an admirable case for diversity and tolerance and identify theoretical syntheses as a barrier to them. No doubt they are correct. However, syntheses are, I believe, a minor part of the problem. Every discipline contains people with authoritarian tendencies who wish to wield power. They use their personal status to exclude as far as possible those who do not conform to their ideology or acknowledge their authority. Their success rests with the control of journals, search and promotion committees, and funding agencies. We all know who these people are in IR and periodically read their indefensible—even risible—attacks on those who adopt a different approach to their research. The creation of syntheses is just one of the strategies they invoke in their search for control. This was particularly evident in the efforts to integrate neoliberalism with neorealism. It is somewhat less evident in the newer synthesis of constructivism and critical theory. Exclusion in this instance may be more of an unintended consequence.

Response to Richard Ned Lebow's Review of *International Relations' Last Synthesis? Decoupling Constructivist and Critical Approaches*

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— J. Samuel Barkin 

Thanks to Professor Lebow for an engaging review of *International Relations' Last Synthesis*. He highlights the core argument of the book, one in favor of theoretical diversity and openness. In doing so, though, he posits ontology as having a more central role in the argument than is our intent. As he notes, the book does indeed claim that the common feature across self-described constructivisms is ontological, the assumption that political reality is socially constructed rather than materially given. However, this is in no way a claim that ontology is the principal conceptual basis for comparison of theories more broadly. The term “theory” is sufficiently broad, meaning so many different things in different contexts and to different scholars, that any claim that there is one way to compare across theories is misplaced, whether the basis for that comparison is ontological, epistemological, or something else.

Different understandings of theory are often simply orthogonal to each other. This is in a way our point in arguing against a constructivism-critical theory synthesis; the two conceptual categories are orthogonal to each other, and therefore can reasonably interact or not within specific research projects as appropriate to those projects. In fact, we argue at length that critical theories do *not* have a common ontology. What they have in common is the idea that theory in the social sciences is inherently political. This idea is compatible with but in no way inherent to an ontology of social construction.

Nor do we make this claim about constructivist ontology as an exercise in inclusion and exclusion, an argument about who is or is not legitimately a constructivist. Our claim is that self-proclaimed constructivists' descriptions of what they do are generally consistent with an ontology of intersubjectivity and co-constitution. We are not in the business of saying who is or is not a constructivist (or critical theorist, or any other kind of “-ist”); anyone who chooses to describe themselves as a constructivist (or critical theorist, etc.) is, from our perspective, welcome to the moniker.

More broadly, we speak in terms of various theories of and approaches to the study of global politics as each offering its own affordances, understood as the potential uses that are latent in and can be expressed through specific intellectual tools. The key affordance of constructivism writ large is tools for thinking about the social basis of political activity; of critical theory, the political basis of social theory. Thinking in terms of the affordances of intellectual tools, rather than in terms of competition among theories or paradigms, helps to highlight how various tools can be used in tandem in a specific

intellectual project as appropriate to that project. This claim is intended specifically as an argument against the disciplinary tendency in IR to think of theories and paradigms as either mutually exclusive or as interrelated in any fixed and necessary way. It is intended to help enable scholars to think more creatively about how to use the various intellectual tools available to use in the study of global politics. We agree with Lebow that disciplinary gatekeeping hinders such creativity. But we argue that making the intellectual case for such creativity is worthwhile, nonetheless.

The Quest for Knowledge in International Relations:

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In *The Quest for Knowledge in International Relations*, Richard Ned Lebow addresses the question of what counts as good theory in IR by focusing on the epistemological foundations of the discipline. He does this by constructing a binary distinction between positivism and interpretivism, looking at their different—and in ways opposite—assumptions about how knowledge is claimed and acquired. The book is a wide-ranging trip through the history both of IR as an academic discipline and of twentieth-century debates about the philosophy of science and social science. Its ultimate conclusion is twofold. First, that interpretivism, understood in a more or less Weberian way, provides a better basis for knowledge in the social sciences generally, and IR specifically, than positivism, although practitioners of both could learn from each other. And second, that IR scholarship should be thought of as an ethical practice rather than a search for epistemological warrants for truth claims.

The arguments supporting this conclusion are structured around the four sections of the book. In the first Lebow introduces the discussion of positivism and interpretivism in the context of a broader discussion of what knowledge is in the social sciences, drawing largely on the Vienna Circle for positivism and Weber for interpretivism. The second reviews a set of specific methodologies used in IR scholarship, ranging from correlational research to rationalism to practice theory, as examples of his two epistemological categories. The third addresses two specific grounds of epistemological difference between the two categories, that between verification and falsification of theory and that between causal and non-causal narratives. The fourth looks at the role of reason, cause, and mechanisms in IR theory.

The book fits into a broader literature, both in IR specifically and in the social sciences more generally, of dichotomizing scholarship into two categories, one involving quantitative methods and an affinity for the natural sciences (or at minimum with what social scientists imagine the natural sciences to be) and the other involving qualitative methods and an affinity for the humanities and history (e.g., Andrew Abbott, *Chaos of Disciplines*, 2001). In IR this literature can be traced back to the second “great debate” in the discipline’s telling of its history, between science and history. Lebow’s addition to this literature is to ground the dichotomy more explicitly in questions of epistemology and in more recent metatheoretical debates in the field. This grounding, however, begs the question of whether the dichotomy presents a productive framing of questions of epistemology in IR. Lebow notes (on p. 24) that by subsuming all non-positivist approaches under the banner of interpretivism he is “likely to annoy representatives of these approaches” for not addressing the specifics of their positions. He goes on to quote a reviewer of the project at manuscript stage as saying that “only to a neopositivist would ‘interpretivism’ appear to be anything like a coherent category.” But Lebow, in what is the key gambit of the book, stakes out the position that it is coherent.

Lebow’s arguments about the need to separate questions of method from questions of epistemology and to think of causes in IR in broader ways are well taken. At the same time, there are two categories of reasons that reifying a positivist/interpretivist distinction may not be the best way forward in encouraging both thinking and communication within the discipline about questions of epistemology. The first is that it is not clear, even from a Weberian perspective, that the dichotomy is able to carry the explanatory weight that Lebow wants it to, because too much of the research published in the discipline does not fit neatly into it. The second is that dichotomies both reify categories and exclude those that do not fit into them, and this process of exclusion strikes me as problematic. The remainder of this review fleshes out these claims.

Beginning with the concern about reification, Lebow claims at the outset to be talking about positivism and interpretivism as epistemologies, and at various points in the book he grounds his discussions of them in intellectual histories drawing on the works of scholars such as Karl Popper and Max Weber. But when making the transition to discussions of IR theory the connection with epistemology per se becomes attenuated in places. At some points in the book he seems to be discussing the dichotomy as one of methodology, at others as one of disciplinary sociology. Positivism then becomes a description of a group of scholars who use numbers and gatekeep aggressively rather than a description of a type of knowledge claim that they supposedly make. He also introduces as definitional aspects of positivism elements that are difficult

to substantiate either in intellectual history or in current IR scholarship, such as the idea that positivist theorizing is necessarily top-down and interpretivist bottom-up.

In fact, at times positivism ends up sounding less like a clear epistemological stance than a list of research programs Lebow doesn’t like. Two examples can be found with Waltzian neorealism and rational choice theory, both of which he categorizes as positivist. Lebow associates positivism with, among other things, empiricism and reductionism. Waltz makes a structural-functionalist argument against reductionism in the development of a theory that he claims is not testable. It is not clear how these contradictions can be reconciled in a way that makes Waltz an epistemological positivist, even though he is often associated with the science side of IR’s second debate.

Similarly, Lebow includes rational choice theory in the category of epistemologically positivist approaches to IR, and argues at significant length that it is not a useful approach to IR. But the argument is undermined in two ways. First, he does not make clear in what way rational choice theory is positivist. Practitioners generally interpret rational choice assumptions as “as-if” statements (in which case they are Weberian ideal types), or as claims about human nature that are ontologically prior to observational evidence (in which case they are philosophically realist claims). Neither fits into Lebow’s definition of positivism. Furthermore, much of his critique of rational choice theory is normative (it leads to bad outcomes and legitimizes neoliberalism) and empirical, rather than epistemological. With both rational choice theory and Waltzian neorealism the association with positivism seems much more about disciplinary sociology and arguments about who is a legitimate social scientist than about epistemology per se.

Which links with the second category of reason that a positivist/interpretivist dichotomy is not clearly a more useful way to conceptualize epistemology in IR than a more nuanced categorization. It encourages people to take disciplinary sides and raises questions about who is left within the boundaries of legitimate social science and who is excluded. Lebow tries to finesse this issue in the conclusion of the book by calling for better communication across epistemologies and for finding some methodological common ground between them. But it remains unclear what the philosophical basis can be for common ground between two mutually incompatible sets of arguments about what counts as a legitimate knowledge claim in IR.

The dichotomy, by suggesting that there are only two sets of legitimate epistemological claims upon which to base theorizing and research in IR, effectively excludes work that is not rooted in either of those claims (e.g., J. Ann Tickner, “What Is Your Research Program,” *International Studies Quarterly* 49[1], 2005). Lebow in fact explicitly states that some kinds of critical theory do not fit into his interpretivist category, although he does not

make clear either where the line is between included and excluded or, in any real detail, what the logic of exclusion is. In a sense, he is using the concept of interpretivism as an exercise in disciplinary gatekeeping in a way analogous to how Gary King, Robert Keohane, and Sidney Verba (*Designing Social Inquiry*, 1994) use positivism. This is in contrast to the way someone like Patrick Thaddeus Jackson (*The Conduct of Inquiry in International Relations*, 2011) uses epistemological categorization, to understand and enable rather than to exclude.

Lebow's argument for where the new boundaries of IR should be may resonate with committed Weberians but is less likely to be convincing for readers coming from other epistemological starting points. His argument also ends up suggesting that interpretive questions (e.g., how did outcome x come to be) are better or more appropriate questions in IR than inferential questions (e.g., what are the general effects of x on y), illustrated primarily with discussions about the causes of war. But ideally epistemology should be helping us to think about how to answer questions we have about the social world rather than telling us what kinds of questions we should not be asking. Whether one agrees or disagrees with the conclusions, though, *The Quest for Knowledge* provides a broad and historically informed overview of a debate that has been going on in IR for over half a century and that shows no sign of going away. It raises key issues in how we think about social science epistemology and, perhaps even more importantly, forces readers to reflect on the epistemological starting points of their research.

Response to J. Samuel Barkin's Review of *The Quest for Knowledge in International Relations: How Do We Know?*

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— Richard Ned Lebow

Samuel Barkin offers a succinct and fair account of my book. He then challenges its fundamental premise: that we should focus more on epistemology than method in organizing the field of international relations. He has two principal objections. Any dichotomy – or other classification scheme – narrows the field by excluding research that does not neatly fit in its categories. My dichotomy includes approaches that do not really fit into either of my epistemologies.

To understand any field, we must introduce categories to define research programs, their goals, and methods. Categorization is valuable to the extent it tells us something useful about the field. My focus on positivism and interpretivism encourages practitioners to step back and look at their own research in comparison to others in their own and the other epistemology. How is it similar? Where does it differ? What can we learn about ourselves and our

research by making these comparisons? Reflection of this kind can also improve our research by making us aware of our epistemological foundations and their problematic nature.

Some people – I hope Barkin does not count himself among them – assert that categories and comparisons are by their nature exclusionary. On the most superficial level this is true: something fits in a category, does not fit, or fits only in part. We cannot negotiate the world, let alone our research, in the absence of comparisons and the categories that make them possible. The same holds true for ethical judgments, which are equally dependent on categorization. Exclusionary or negative depictions are sometimes justified, as is true with research that does not meet the standards of its own tradition. When possible, we should try to use analytical categories in positive or at least neutral ways. We can do this by affirming value across categories. This is what I do with positivism and interpretivism. I gave historically rooted and, I hope, accurate accounts of their epistemological and substantive assumptions, reasons why people are drawn to them, and how they seek and claim to have produced knowledge. I am more favorable to interpretivism, but offer critiques of both epistemologies and approaches all or in part nested within them. I contend that scholars who work in either epistemological tradition often face similar problems and can learn from one another.

Barkin accuses me of including research approaches or programs that do not fit, or totally fit, in either epistemology and of excluding some that are outside of them. Barkin offers the examples of neorealism and rational choice. I contend that Waltz was unabashedly positivist in his search for a universal, parsimonious theory with predictive value. Rational choice is more complex. It is positivist in the sense that it seeks to explain and predict behavior and assumes that politics can be studied as a science. As biologists would be the first to acknowledge, typologies never map perfectly on to the world. They can tell us much about what we are studying. The discovery in the field of IR that approaches and research programs sometimes fit an epistemology only in part, or bridge epistemologies, says something important about them, the diversity of our field, and the nature of the epistemologies.

Barkin alleged that I delegitimize research – feminism, for example – that does not fit into either epistemology. I do nothing of the kind. I note that at the outset that my typology does not encompass everything and that some research traditions or programs fit my two epistemologies only in part. Much feminism is interpretivist, some less so. I do not use any feminist examples, because it is not associated with a distinctive research method. It is unfair of Barkin to criticize me for casting my epistemological nets broadly and at the same time criticizing me for including approaches that show only a partial fit. I offer broad readings of positivism and interpretivism to make

them as inclusive as possible, something he should regard favorably.

Barkin maintains that my account of positivism is so broad that it ends up “a list of research programs.” I am very clear about the defining characteristics of positivism, devoting two chapters and parts of others to the subject. I describe and evaluate research programs in both epistemologies to illustrate the kinds of problems they encounter in practice. I show that interpretivism has more diverse intellectual roots than positivism; they include German Romanticism and historicism, neo-Kantianism, Max Weber, and Wittgenstein and the linguistic turn. Not surprisingly, interpretivism has generated more diverse research programs. By narrowing my definition of what fits as interpretivist I would be committing the very sin Barkin is so keen to avoid.

The comparison to King, Keohane, and Verba is unwarranted. They offer a parody of constructivism, and of positivism. They provide a very broad definition of the

former and a remarkably narrow one of the latter, both with the goal of discrediting any approach that differs from theirs. My account is broad to make it inclusive. When I offer judgments, they are not made on the basis of some claim to know the right way to do things, but rather with reference to what the two epistemologies and associated approaches claim they can deliver.

Barkin also disapproves of my jumping from epistemological to empirical evaluations. However, they are closely connected. We can evaluate an epistemology on the basis of its internal logic and structure or how it enables useful discoveries about the world. I do both, and it is a legitimate exercise because the purpose of positivism and interpretivism is to generate knowledge. They also incorporate substantive assumptions about the social world.

I regret that Barkin does not engage my final chapters on reason, cause, mechanisms, and social science as an ethical practice. They are the most interesting, important, and original parts of the book.