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Distinguishing a minimalist role for grand theorizing

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Abstract

A major theoretical shortcoming in international relations (IR) is the lack of any semblance of a common understanding regarding the role of grand theory. This article argues that explicit theoretical work of this sort is useful, but that a search for a single overarching grand theory to guide inquiry is misguided and that the primary business of IR is to form and evaluate middle-range theories.

Keywords

grand theory, middle-range theory, Robert Merton, theoretical pluralism

International relations (IR) scholars have long differentiated between a general form of theorizing and a specific one. The so-called ‘grand theories’ in IR offer ‘organizing devices for the entire field, not only portions of it’.¹ In contrast, the ‘middle-range theories’ in IR have a more limited range; they aim to ‘explore specific problems, to form hypotheses or generalizations explaining limited ranges of phenomena’.² A hallmark of the IR field during the post-WWII period is how much attention it devoted to grand theorizing – that is, the development, refinement, and evaluation of grand theories. In this respect, IR stood in marked contrast to comparative politics, where the formative leaders of the field emphasized ‘middle-range theoretical propositions based on a keen awareness of contingencies and contextual effects within different national settings’.³

During the 2000s, however, the salience of grand theorizing declined greatly within the IR field.⁴ This raises an important question: how much emphasis should be placed on grand theorizing in the years ahead? My read of Justin Rosenberg’s stimulating, provocative, and welcome recent article is that his answer to this question is, ‘much more

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attention'. But behind this initial question lies a deeper one: what is the proper function of grand theorizing within IR? To help further dialogue, this article aims to sketch a succinct answer to this question.

A major theoretical shortcoming in IR is the lack of any semblance of a common understanding regarding the role of grand theory. Exaggerating for emphasis, it often seems as though IR scholars can essentially be divided into two general groups: one viewing the grand theories as being incredibly important and therefore worthy of investigation, the other seeing them essentially as a distraction from what they regard as the real business of IR: creating and evaluating middle-range theories. In my view, both of these positions are wrong: I favor a stance which mediates between them. With the partisans of grand theorizing, I believe that explicit theoretical work of this sort is useful; our understanding of international relations will be weaker if we neglect grand theorizing. But with the partisans of middle-range theory, I concur that a search for a single overarching grand theory to guide inquiry is misguided and that the primary business of IR is, in fact, to form and evaluate middle-range theories.

The role of (grand) theory

Theories can obviously be helpful in a number of ways; of the various tasks they can perform, there are five I will briefly highlight which are particularly relevant for understanding the role of grand theorizing in the IR field. Drawing on an explicit theory (1) helps us to select certain variables for investigation out of an endless number of potential causal factors, (2) pushes us to state our underlying assumptions directly, (3) gives us clues as to the type of causal connections to establish and directs us toward certain kinds of phenomena to investigate, (4) encourages us to identify variables and develop hypotheses that are consistent with each other, and (5) makes it easier to distinguish our hypotheses from competing ones – including competing hypotheses that feature the same independent variable – that are drawn from different underlying assumptions. These five tasks are obviously not the only roles that theories can play, but a theory can be incredibly valuable merely if it performs them and does nothing else.

At the level of grand theory in IR, I maintain that these five tasks are all that such theories should try to accomplish. Viewed this way, grand theory ultimately just tells us 'where to look' for explanations of international relations and to be explicit about the choices we make in this regard. This is a much more limited function for grand theorizing than has typically been propounded by its proponents in the discipline. In particular, many advocates of grand theorizing either implicitly or explicitly subscribe to the view that it can explain behavior on its own; this becomes clearest when IR scholars try to use the assumptions of their preferred grand theory to explain state behavior.

Pushing grand theory to accomplish more than the five tasks specified above does great harm: it ineluctably leads to debates over which grand theory would be best off adopting as the standard, or, alternatively, over which grand theory in the aggregate is 'most useful' at explaining international behavior. But if the purpose of grand theory is merely to accomplish the above five tasks, then there would be no point in seeking to establish one particular grand theory as 'King of the Hill'.

The value of Robert Merton's perspective

Robert Merton comes closest to specifying the minimalist role for grand theorizing that I have in mind. That I turn to Merton in this regard will strike many readers as highly ironic, since he is arguably the most ardent defender of middle-range theory in the social sciences (he, in fact, coined the term).⁵ And yet, in making his argument for emphasizing middle-range theory, Merton outlines a key caveat:

it is theories of the middle-range which hold the largest promise, *provided that*, underlying this modest search for social uniformities, there is an enduring and pervasive concern with consolidating the special theories into a more general set of concepts and mutually consistent propositions.⁶

But what prompts such concerns for consolidation? And what pushes analysts to choose certain variables for investigation as well as to formulate propositions so as to be mutually consistent with each other? Here, Merton underscores the vital importance of a different form of theorizing – what he calls ‘general theoretical orientations’ – which he argues

have great propaedeutic value. For one thing, they bring out into the open air for all to see the array of assumptions, concepts and basic propositions employed ... If true art consists in concealing all signs of art, true science consists in revealing its scaffolding as well as its finished structure.⁷

Moreover, Merton outlines a number of reasons why general theoretical orientations facilitate the creation and evaluation of middle-range theories, notably (1) they point to certain types of variables to be taken into account; (2) they provide a compact arrangement of the central concepts and assumptions that are used – thereby making it possible for these concepts and assumptions that are used in the analysis to be evaluated all at once, both by the analyst and by potential critics;⁸ (3) they ‘direct the attention of research workers to different kinds of phenomena through which each array of problems can be investigated to good advantage’;⁹ (4) they facilitate ‘the systematic cross-tabulation of presumably significant concepts’;¹⁰ and (5) they promote internal consistency and cumulation, since they make it easier to detect when a middle-range theory is developed from the original set of assumptions and, alternatively, when a middle-range theory is developed only by extending or changing the original set of assumptions.¹¹

While not identical, the functions that Merton outlines for what he calls general theoretical orientations are very similar to the five roles I laid out above for grand theorizing in IR. This is no accident, of course, since I was influenced by Merton's arguments to this effect. In Merton's view – and mine – what in IR we call grand theories are best used ‘tentatively, as a point of departure’.¹² As Merton notes,

[i]ndispensable though these [general theoretical] orientations are, they provide only the broadest framework for empirical inquiry ... The chief function of these orientations is to provide a general context for inquiry; they facilitate the progress of arriving at determinate hypotheses ... [T]hey constitute only the point of departure for the theorist. It is his task to

develop specific, interrelated hypotheses by reformulating empirical generalization in light of these generic orientations.¹³

Consistent with Merton's perspective, grand theorizing has value in our quest to understand international relations, but it should have a highly constrained role: performing the five tasks specified above, which collectively help to promote the formation and evaluation of middle-range theories. Expecting grand theories to do more than simply accomplish the above tasks distracts us from examining the explanatory power of middle-range theories and toward 'King of the Hill' consolidation exercises. At the level of middle-range theory, debate is necessary for progress. But the necessity of this kind of debate at the level of middle-range theory by no means requires searching for a particular grand theory which can best organize the field.

Theoretical pluralism

In many respects, a minimalist approach to grand theorizing automatically feeds into a respect for theoretical pluralism; it is thus not surprising that Merton strongly endorses its value.¹⁴ If grand theorizing is merely a tool in the development and assessment of middle-range theories, then having different alternatives will actually be helpful for scientific progress.

Karl Popper arguably underscores this point better than anyone else, maintaining that it would be a grave mistake to have all scientists in a field working within the same theoretical framework since this would impair creativity, which is the essence of science in his understanding. In his writings, Popper emphasizes the overriding importance of permanent open critical discussion, stressing that a researcher should always seek out discussion with those in disagreement with them and be prepared to change their views following these conversations; in his view, having a range of contrasting general theoretical frameworks both encourages and facilitates such discussions. As Popper maintains,

It is often asserted that discussion is only possible between people who have a common language and accept common basic assumptions. I think this is a mistake. All that is needed is a readiness to learn from one's partner in the discussion, which includes a genuine wish to understand what he intends to say. If this readiness is there, the discussion will be the more fruitful the more the partners' backgrounds differ. Thus the value of a discussion depends largely on the variety of the competing views. Had there been no Tower of Babel, we should invent it.¹⁵

Conclusion

The great value of Justin Rosenberg's recent article is that it bravely seeks to strengthen the theoretical foundations of the IR discipline. We need more such attempts at bold, sweeping theorizing. Provided that Rosenberg's theoretical effort can be made compatible with the minimalist understanding of the role of grand theorizing outlined here, then I think his contribution will have great value indeed.

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Notes

1. Kalevi Holsti, 'Retreat from Utopia: International Relations Theory, 1945–1970', *Canadian Journal of Political Science*, 4, 1970, p. 170.
2. Holsti, 'Retreat from Utopia', p. 171.
3. Richard Gunther, 'Reflections on the Golden Age of Comparative Politics', *International Studies Review*, 42(2), 1998, pp. 322–4.
4. This is the case across the International Relations (IR) field, but is especially true regarding international political economy; see Benjamin Cohen, *International Political Economy: An Intellectual History* (Princeton, NJ: Princeton University Press, 2008).
5. In *Social Theory and Social Structure*, Merton argues that

sociology will advance in the degree that its major concern is with developing theories of the middle-range and will be frustrated if attention centers on theory in the large. I believe our major task today is to develop special theories applicable to limited ranges of data.

Robert K. Merton, *Social Theory and Social Structure* (New York: The Free Press, 1957), pp. 6–7, 9.

6. Merton, *Social Theory and Social Structure*, p. 10 (emphasis in original).
7. Merton, *Social Theory and Social Structure*, p. 13.
8. Merton, *Social Theory and Social Structure*, pp. 14–5.
9. Robert K. Merton, *Sociological Ambivalence and Other Essays* (New York: The Free Press, 1976), p. 140.
10. Merton, *Social Theory and Social Structure*, p. 15.
11. Merton, *Social Theory and Social Structure*, p. 15.
12. Merton, *Social Theory and Social Structure*, p. 16.
13. Merton, *Social Theory and Social Structure*, p. 88.
14. Robert Merton, 'Theoretical Pluralism', in Robert Merton and Piotr Sztompka (eds) *On Social Structure and Science* (Chicago, IL: Chicago University Press, 1996), pp. 34–40.
15. Karl Popper, *Conjectures and Refutations: The Growth of Scientific Knowledge* (New York: Routledge & Kegan Paul, 1963), p. 352.

Author biography

Stephen G Brooks is an Associate Professor of Government at Dartmouth College and has previously held fellowships at Harvard and Princeton. He is the author of four books: *Producing Security: Multinational Corporations, Globalization, and the Changing Calculus of Conflict* (Princeton, 2005), *World out of Balance: International Relations and the Challenge of American Primacy* (Princeton, 2008), with William Wohlforth, *America Abroad: The United States' Global Role in the 21st Century* (Oxford, 2016), with William Wohlforth, and *Political Economy of International Security* (Princeton, forthcoming).