H-Diplo | ISSF

Authors' Response to Article Review 118

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The Importance of Being Earnest

We find it somewhat unusual to have a book review essay reviewed by another scholar. But we are pleased that H-Diplo chose to accord our piece "Is Grand Strategy a Research Program? A Review Essay," this honor. We are also pleased that Dr. Nina Silove chose to devote the time and effort to this task.

Unfortunately, we believe that her review misrepresents the nature of our original essay and mistakes our central concerns. We neither claimed nor intended to offer a full-fledged Lakatosian analysis (or for that matter alternative positions on IR and the philosophy of science) of a grand strategy research program.

Instead, we used four books, carefully selected in conjunction with the editors of *Security Studies* for their topicality and prominence, to raise questions about the burgeoning literature on grand strategy. Specifically, we asked whether these works share a common understanding of grand strategy; whether they attempt to describe, explain and predict, or detour into normative prescription; and to what extent they only focus on the single example of the United States and ignore the broader, comparative application of the concept to other countries, from great powers like Russia and China to smaller (albeit often pivotal) states.

Our central concern in raising these questions is whether the study of grand strategy constitutes a 'field' or coheres into a research program. A field is characterized more by a pluralistic, possibly even eclectic approach to scholarship in which there is little attention paid to reaching a consensus regarding issues such as a definition, purpose, forms of evidence, or appropriate methodologies. In contrast, participants in a research program consciously attempt to do so. They may debate questions such as causal relationships but do so within the constraints of accepted conventions.

The four books that we examine have dominated the study of grand strategy in the last five years.[1] Each one has justifiably been recognized as an important piece of work, is widely cited and debated, and offers major contributions in terms of the questions we raise. Characteristically, however, their collective features are more reflective of a field than a research program. They differ, for example, on the definition they use and their interpretations of the purpose of a grand strategy, using varied forms of evidence. Two books in particular, Barry Posen's *Restraint*, and Stephen Brooks and William Wohlforth's *America Abroad*, are explicitly prescriptive. None offer propositions in a testable form, although, as we point out in the interest of fairness, Hal Brands's *What Good Is Grand Strategy?* is self-consciously more descriptive and analytic, and Lukas Milevski's intention in *The Evolution of Modern Grand Strategic Thought* is to provide an intellectual history of the term 'grand strategy.'

Viewed through our optic, we conclude that these works collectively suggest that the propensity in the study of grand strategy remains consistent with the characteristics of a field. To the extent that we ourselves are prescriptive, we argue that it would be beneficial if scholars focused on developing a research program, one in which they debate definitions in an attempt to establish a shared baseline, examine cases with a view towards formulating a comparative framework, shift from predominantly idiographic studies of a few great powers to a broader universe of cases, and are more methodologically pluralistic.

To take us to task for not identifying and evaluating the key elements of a mature research program therefore seems unjustified. A review essay, no matter how sophisticated, is not the place to launch such an undertaking. We do, however, begin to address the issues we raise in a series of forthcoming works, including our new edited volume in July entitled *Comparative Grand Strategy: A Framework and Cases* (Oxford University Press, 2019).

Silove, rather, focuses on an issue in one footnote, stating that, "It is only in a footnote that they label their approach explicitly, where they state it is 'Lakatosian,' in reference to the work of philosopher Imre Lakatos, and explain that the application of 'Lakatosian criteria' is 'warranted' (3, note 4)." We make a more modest claim when we state that, "By long-standing agreement (at least according to positivist approaches), a research program should include central questions, core assumptions, and debated theories about cause-and-effect relationships, and their hypotheses should ultimately be subject to trial by evidence" (3). The footnote itself then references several other important articles that debate this issue, including those who argue against the use of Lakatos in IR. The footnote is therefore an aside, representing a nominal percent of our essay, and we used a footnote because we regard the issue as a digressive point. The only other reference to Lakatos in our article is a brief citation in footnote 76. In her review, however, Silove devotes a full ten paragraphs to critiquing a position we effectively do not take.

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[1] Hal Brands, *What Good Is Grand Strategy? Power and Purpose in American Statecraft from Harry S. Truman to George W. Bush* (Ithaca: Cornell University Press, 2015); Stephen Brooks and William Wohlforth, *America Abroad: The United States' Global Role in the 21st Century* (Oxford: Oxford University Press, 2016); Lukas Milevski, *The Evolution of Modern Grand Strategic Thought* (Oxford: Oxford University Press, 2016); and Barry R. Posen, *Restraint: A New Foundation for U.S. Grand Strategy* (Ithaca: Cornell University Press, 2014).

Perhaps more importantly, we think our review essay demonstrates that the recent crop of some of the most important books on grand strategy are wanting in terms of the conventional attributes and aspirations of a research program.

Every essay is an act of communication. But the effectiveness of such communication is contingent upon common language and shared metrics for adjudicating research results. On this basis, we hope our essay provides a strong anchorage for our understanding of grand strategy and how it should be studied.

Further evaluating work on grand strategy in metatheoretical terms remains a worthy and important undertaking. In this respect, a concern of our essay was to identify and characterize the main problems that have prevented such a task from being plausibly debated. We doubt, however, that tacking – without adapting – Lakatos onto the field would solve its most pressing problems. Our review essay thus invites scholars to join us in this venture, so that those working on related issues can debate with and not past each other.

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Simon Reich is a Professor in the Division of Global Affairs and Department of Political Science at Rutgers University-Newark. He is the author and editor of a dozen books, most recently, *The End of Grand Strategy* (Cornell, 2018) with Peter Dombrowski.