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**Thierry Balzacq, Peter Dombrowski, and Simon Reich. "Is Grand Strategy a Research Program? A Review Essay."** *Security Studies* 28:1 (2019): 58-86. DOI: https://doi.org/10.1080/09636412.2018.1508631.

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## Review by **Nina Silove, ETH Zurich and Harvard University**


Thierry Balzacq, Peter Dombrowski, and Simon Reich (BDR) enquire whether grand strategy is a field of study or a mature research program. This is an important question, answers to which would be of tremendous interest to scholars of grand strategy and pivotal for the future of this ever-expanding field of study.

BDR's article is a valuable contribution to a much-needed discussion about the success of research on grand strategy, which should provide a basis for further inquiry on this topic. As a standalone contribution, the article is constrained by its structure. It is not a research article, but a review of four books: *What Good Is Grand Strategy? Power and Purpose in American Statecraft from Harry S. Truman to George W. Bush*, by Hal Brands, *America Abroad: The United States' Global Role in the 21st Century*, by Stephen Brooks and William Wohlforth, *The Evolution of Modern Grand Strategic Thought*, by Lukas Milevski, and *Restraint: A New Foundation for U.S. Grand Strategy*, by Barry Posen.[1] I am thus, by request, undertaking the somewhat unusual task of reviewing a book review essay.

BDR do not provide much discussion of metatheory, methodology, or their method of assessing the state of the study of grand strategy. 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). Their intent to provide a Lakatosian analysis is further evidenced by their reliance on the concept of a "research program" throughout the article, a concept so closely associated with Lakatos that it would be odd to use it in an alternative manner without further explanation.

[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, 2014); Stephen G. Brooks and William C. 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); Barry R. Posen, *Restraint: A New Foundational for U.S. Grand Strategy* (Ithaca: Cornell University Press, 2014).

Thus, although BDR's discussion of Lakatos is brief, it seems fair to read the article as intending to provide a Lakatosian appraisal of research on grand strategy. BDR do not, however, consistently apply Lakatos's work in their analysis. In their departures from Lakatos, they effectively construct and employ an alternative but unfortunately inferior analytic framework.

BDR's alternative framework emerges in the following steps, each of which are discussed in more detail below. First, they do not use Lakatos's definition of a research program, instead providing their own, which is insufficiently specific. Second, the four books they review self-evidently neither constitute nor belong in a common Lakatosian research program. Third, they do not address directly the question central to a Lakatosian inquiry, that of whether a research program is progressive or degenerating, or apply Lakatosian tests to appraise programs as such. Finally, they suggest four alternative tests for whether a research program should be deemed 'mature,' which are inferior to Lakatosian standards of progress because they do not mandate the production of new, corroborated knowledge about grand strategy.

In lieu of what one might expect in an appraisal of a research program, much of the substantive content of BDR's article, in addition to summaries of the books they review, focuses on discussing the following three topics: (1) the definition of grand strategy, specifically whether it should be narrowly or broadly focused (for example on the military sphere of statecraft or inclusive also of other non-military spheres); (2) the temporal and geographical nature of a "case" of grand strategy; and (3) the relative balance between explanation and prescription in the study of grand strategy. On the first topic, the authors do not seem to reach conclusions. Instead, they recommend greater conceptual clarity in the study of grand strategy (18-19). With regard to the second and third topics, BDR recommend more focus on explanation than prescription (25, 27, 28) and more cross-national research (28). The authors also, in their conclusion, make a fourth recommendation, proposing that grand strategy scholars use a "greater variety of methods" (27). These are good recommendations.

But what about the search for the "mature research program?"

As defined by Lakatos, a research program refers to a series of theories. In one explanation of this concept, Lakatos describes "a series of theories [...] where each subsequent theory results from adding auxiliary clauses to (or semantic reinterpretations of) the previous theory in order to accommodate some anomaly, each theory having at least as much content as the unrefuted content of its predecessor."[2] The "continuity" that connects theories into a series is a "hard core" set of claims (or "laws" or "conjectures,"[3] or "postulates,"[4] etc.).[5]

Contrary to Lakatos's definition of a research program as a series of theories, BDR formulate the following definition: "By long-standing agreement […] 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). Rather than a compelling alternative definition of a research program, this statement constitutes a general conceptualization of science that leaves unspecified many core philosophical issues, such as which of the elements of their definition constitutes the relevant unit for appraisal or the standards to be applied in the "trial."

A reasonable starting point in the search for a research program would be to identify a theory, as the first step toward finding a series that share a "hard core." The obvious place to look for a theory would be in the explanatory literature on grand strategy that aims to develop and test theory. Instead, the article is focused on reviewing a work of "applied history" (4) (Brands), two works by political scientists that are policy-prescriptive in their orientations (Brooks and Wohlforth, and Posen), and an intellectual history (Milevski); an inauspicious basis for locating a series of theories.

Unless the theory that BDR seek concerns the intellectual history of grand strategy, Milevski's book can be eliminated as a contender for inclusion in a common research program with the other books. Brands's book does not offer a "cause and effect" theory of grand strategy. In their review, BDR extract a theory from Brands, or what they call "a generalizable analytic and policy lesson," along the lines of: "Crafting and implementing a grand strategy is contingent as much on domestic politics as on the structural forces of international relations" (4). This theory, about the relative significance of domestic versus international structural factors on the formulation and implementation of grand strategy, is one that has little relationship to the theories at stake in the debate between Brooks and Wohlforth, and Posen. In those two books, the theories in question relate to the relative costs of U.S. grand strategies of restraint and deep engagement. Thus, the selection of books under inquiry is weighted unnecessarily against the possibility of finding continuity and thereby identifying a research program.

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[2] Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes [1970 version]," in *Criticism and the Growth of Knowledge*, ed. Imre Lakatos and Alan Musgrave (Cambridge: Cambridge University Press, 1970), 183.

[3] Imre Lakatos, "Introduction: Science and Pseudoscience," in *The Methodology of Scientific Research Programmes*, ed. John Worrall and Gregory Curric (Cambridge: Cambridge University Press, 1978), 4.

[4] Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes [1978 version]," in *The Methodology of Scientific Research Programmes*, ed. John Worrall and Gregory Curric (Cambridge: Cambridge University Press, 1978), 55.

[5] A research program also consists of "negative" and "positive" "heuristics," the details of which are unnecessary for present purposes, but would be central to any analysis of a research program. Lakatos, "Falsification and the Methodology of Scientific Research Programmes [1970 version]," 191-194.

The purpose of identifying a research program is to assess whether it is progressive or degenerating. BDR do not address this question directly. Rather, they ask whether grand strategy is a "mature" program. For Lakatos, maturity is not an attribute of a research program, but rather one of science, the units of which are research programs that can be appraised as either progressive or degenerating.[6] The terms for appraising a research program are set by Lakatos as follows: "Let us say that […] a series of theories is *theoretically progressive* […] if each new theory has some excess empirical content over its predecessor; that is, if it predicts some novel, hitherto unexpected fact. Let us say that a theoretically progressive series of theories is also *empirically progressive* […] if some of this excess empirical content is also corroborated, that is, if each new theory leads us to the actual discovery of some *new fact*. Finally, let us call a [research program] progressive if it is both theoretically and empirically progressive, and degenerating if it is not."[7] In short, the two key issues in determining whether a research program is progressive are whether, in a series of theories, each new theory "anticipate[s] new facts,"[8] and whether these new facts are corroborated by evidence.

Instead of using Lakatos's tests of progress, BDR suggest that what they call a more mature research program would demonstrate greater conceptual clarity, the use of a wider range of methods, an emphasis on explanation rather than prescription, and more cross-national research. If applied as tests to appraise a research program, however, these four criteria would be individually and collectively insufficient to reveal whether a program has been successful. Simply put, research on grand strategy could improve in all respects recommended by BDR, but still fail to generate new knowledge about grand strategy. Thus, BDR's indicia of maturity are inferior to Lakatos's tests of progress, which demand that programs reveal new, corroborated facts. The standards we use to appraise research are also, implicitly, guides to researchers about what we should be doing with our time. Although there are many underspecified and/or contentious elements of Lakatos's work, relative to other metatheories it is a compelling explanation of, and thereby indirectly a prescription for, knowledge creation.

Despite some limitations, BDR's article provides an excellent basis for further appraisals of research on grand strategy.[9] Extending BDR's work could involve assembling into series theories about domestic explanations of grand strategy and – separately – the costs/benefits of deep engagement and restraint, and assessing those series for whether they are progressive or degenerating. This is possible because, contrary to one claim made

[6] Imre Lakatos and Elie Zahar, "Why Did Copernicus's Research Programme Supersede Ptolemy's?," in *The Methodology of Scientific Research Programmes*, ed. John Worrall and Gregory Curric (Cambridge: Cambridge University Press, 1978), 179.

[7] Lakatos, "Falsification and the Methodology of Scientific Research Programmes [1970 version]," 183. Emphasis in original.

[8] Lakatos, "Falsification and the Methodology of Scientific Research Programmes [1970 version]," 184.

[9] This article is one of a spate of very recent works, the others of which also make some contribution toward appraising the field. See Paul C. Avey, Jonathan N. Markowitz, and Robert J. Reardon, "Disentangling Grand Strategy: International Relations Theory and U.S. Grand Strategy," *Texas National Security Review* 2:1 (November 2018); Rebecca Friedman Lissner, "What is Grand Strategy? Sweeping a Conceptual Minefield," *Texas National Security Review* 2:1 (November 2018); Nina Silove, "Beyond the Buzzword: The Three Meanings of Grand Strategy," *Security Studies* 27:1 (2018): 27-57; and Sara Plana, "Making Sense of Grand Strategy: A Review of a Divisive and Often Elusive Literature" (International Studies Association Annual Convention, San Francisco, April 4 2018).

by BDR (4) and to the conventional wisdom among International Relations scholars, there exists a good deal of literature on grand strategy that does identify domestic factors as causes of states' grand strategies.[10] Similarly, there is a large quantity of research from which Brooks and Wohlforth, and Posen, draw as well as further research that their respective contributions have inspired. These bodies of research on domestic factors and the costs/benefits of restraint and deep engagement, respectively, are each sufficiently voluminous and plausibly continuous to constitute reasonable bases for identifying and assessing research programs.

A comprehensive analysis applying the work of Lakatos (or an alternative, well-specified methodology) to appraise research on grand strategy would helpfully organize the literature in the field and probably also elaborate a positive agenda for future research. I hope that such an analysis is on its way, either at the hands of BDR (unshackled by the constraints of the structure of a book review essay) or others currently toiling away.

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[10] See, for example, in addition to the two works cited by BDR and described as rare, Jeffrey W. Taliaferro, Norrin M. Ripsman, and Steven E. Lobell, eds., *The Challenge of Grand Strategy: The Great Powers and the Broken Balance between the World Wars* (Cambridge: Cambridge University Press, 2013); Nicholas Kitchen, "Systemic Pressures and Domestic Ideas: A Neoclassical Realist Model of Grand Strategy Formation," *Review of International Studies* 36 (2010): 117-143; Mark R. Brawley, *Political Economy and Grand Strategy: A Neoclassical Realist View* (Abingdon: Routledge, 2009); Colin Dueck, *Reluctant Crusaders: Power, Culture, and Change in American Grand Strategy* (Princeton: Princeton University Press, 2006); Christopher Layne, *The Peace of Illusions: American Grand Strategy From 1940 to the Present* (Ithaca: Cornell University Press, 2006); Steven E. Lobell, *The Challenge of Hegemony: Grand Strategy, Trade, and Domestic Politics* (Ann Arbor: University of Michigan Press, 2003); Aaron L. Friedberg, *In the Shadow of the Garrison State: America's Anti-Statism and Its Cold War Grand Strategy* (Princeton: Princeton University Press, 2000); Peter Trubowitz, Emily O. Goldman, and Edward Joseph Rhodes, eds., *The Politics of Strategic Adjustment: Ideas, Institutions, and Interests* (New York: Columbia University Press, 1999); Etel Solingen, *Regional Orders at Century's Dawn: Global and Domestic Influences on Grand Strategy* (Princeton: Princeton University Press, 1998); and Thomas J. Christensen, *Useful Adversaries: Grand Strategy, Domestic Mobilization, and Sino-American Conflict, 1947-1958* (Princeton: Princeton University Press, 1996). The foregoing list includes only a selection of the publications that are cast explicitly in terms of "grand strategy" or "strategy." There are many others that potentially could be included, which do not emphasize these terms but may be categorized as being about grand strategy. Note that many of the above-cited publications are works of neoclassical realism and collectively have been subject to appraisal as such, rather than as contributions to a research program on grand strategy. For an appraisal of neoclassical realism, see, for example, Kevin Narizny, "On Systemic Paradigms and Domestic Politics: A Critique of the Newest Realism," *International Security* 42:2 (Fall 2017): 155-190.