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Fred Chernoff. *Explanation and Progress in Security Studies: Bridging Theoretical Divides in International Relations*. Stanford: Stanford University Press, 2014. ISBN: 9780804790956 (cloth, \$90.00); 9780804792264 (paper, \$27.95); 9780804792295 (e-book, \$27.95).

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Introduction by Jack S. Levy, Rutgers University

Defining scientific progress in terms of the cumulation of knowledge, predictive power, and an “approach-to-consensus” regarding the best explanation when intellectual disputes arise, Fred Chernoff raises the critically important questions of why is there relatively little progress in the field of security studies as compared to the natural sciences, and why is there more progress in some areas of security studies than in others. He argues that one important answer to these questions is that scholars in security studies, unlike those in the natural sciences, use different philosophy of science criteria of evaluation and are rarely explicit about what those criteria are. Chernoff finds support for his argument in an empirical examination of how security studies scholars make judgments about the quality of competing explanations regarding three important research questions in the field—nuclear proliferation, balance of power and alliance formation, and the democratic peace. With respect to the latter, he argues that scholars have explicitly stated their criteria, reached agreement about the appropriate criteria, and moved towards consensus on the validity of a liberal explanation (though which particular liberal explanation is still contested). Chernoff includes a discussion of alternative explanations for the lack of scientific progress in security studies, including the fact that some scholars are answering different questions rather than providing different answers to the same question. He concludes with some useful reflections on the role of metatheory in international relations research programs.

The reviewers—each of whom has made important individual contributions at the intersection of philosophy of science and international relations—all emphasize the importance of Chernoff’s research goals of identifying the impediments to the cumulation of knowledge in international relations and security studies, and of suggesting the most promising ways of overcoming those impediments. They each acknowledge the difficulty of the task that Chernoff undertakes—both in terms of the complexity of the philosophy of science issues involved and the severity of the theoretical, methodological, political, and ideological divisions in the international relations field.¹ Each of the reviewers praises Chernoff’s earlier work in this area, and each argues that *Explanation and Progress in Security Studies* is an important new addition to the literature on evaluating progress in the field.²

Ewan Harrison begins his review by claiming that *Explanation and Progress in Security Studies* is “the most important book in the positivist tradition written on the philosophy of science and International Relations

¹ As James Wirtz notes in his review, the difficulty of the task is increased by the fact that Chernoff is writing for two audiences, philosophers of science and security studies scholars. There is also a practical issue. As Chernoff notes and as Tuomas Forsberg highlights in his review, space constraints in journals and other publication outlets often prevent scholars from engaging in extensive discussions of criteria for evaluating theories.

² Particularly notable existing works are Colin Elman and Miriam Fendius Elman, eds., *Progress in International Relations Theory: Appraising the Field* (Cambridge: MIT Press, 2003), which adopts a Lakatosian framework; Patrick Thaddeus Jackson, *The Conduct of Inquiry in International Relations*, 2nd ed. (London: Routledge, 2016), which presents a non-positivist perspective; and Annette Freyberg-Inan, Ewan Harrison, and Patrick James, eds., *Evaluating Progress in International Relations* (New York: Routledge, 2016), which provides a useful collection of different perspectives. Focusing on structural realism is Patrick James, *International Relations and Scientific Progress: Structural Realism Reconsidered* (Columbus: Ohio State University Press, 2002). See also Imre Lakatos, “Falsification and the Methodology of Scientific Research Programmes,” in Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (New York: Cambridge University Press, 1970), 91-196.

(IR) to date.” Harrison praises Chernoff’s depth of knowledge of philosophy of science debates, his engagement with methodological pluralism, and the tenacity with which he conducts his research. However, Harrison questions Chernoff’s conclusions about the progress of democratic peace research. He argues that after many years of generating important findings and insights, democratic peace researchers have begun to produce diminishing marginal returns, in part because of their primarily dyadic focus. Harrison suggests that a more systemic orientation would be a more fruitful path for future research. He concludes by emphasizing that the application of criteria of evaluation from the philosophy of science is important not only for assessing the past progress of a research program, but also for providing a guide for productive directions for future research.

Tuomas Forsberg shares the other contributors’ praise for *Explanation and Progress in Security Studies*. He raises additional questions about some standard criteria of evaluation. Noting that empirical adequacy is the most widely shared criterion among contributors to the research communities that Chernoff investigates, he argues that there may be some variation in how scholars understand and apply that basic concept. Developing a point Chernoff raises in his concluding chapter, Forsberg emphasizes that being explicit about the precise research question under investigation is just as important as being explicit about the criteria for evaluating an explanation. Forsberg gives particular attention to differences in explanations of generalized patterns of behavior as compared to explanations of individual events, arguing that consensus arises more readily in the former than in the latter. Forsberg illustrates his argument by briefly examining Russia’s behavior in the Ukraine since 2014. He argues that the most influential interpretations are those involving “a simple explanation associated with an established theory, preferably with a big name behind it.” Forsberg argues that debates about Russian behavior in the Ukraine, and other debates as well, are driven more by arguments about theory and a commitment to simplicity than by concerns about empirical adequacy, despite participants’ claims about the latter.³

Jérémie Cornut commends Chernoff for focusing on philosophy-of-science issues, particularly in the context of security studies and international relations fields that are often dominated by more narrow methodological debates. He also praises Chernoff for examining actual research practice, and for supporting his prescriptive recommendations with evidence of what researchers actually do and what works. However, Cornut’s sociological perspective leads him to further develop a point raised briefly by Forsberg and criticize Chernoff for his failure to adequately recognize the “politics of knowledge production.” This includes the “psychological, social, and institutional factors” that impede conversation between paradigms, make it harder for new entrants into the field to be heard, and consequently limit the accumulation of knowledge. The problem, Cornut argues, derives in part from the influence (on the IR field and on Chernoff) of Thomas Kuhn’s emphasis on normal science, and in part from Chernoff’s naturalist approach, with the assumption that the model of the philosophy of science adopted by the natural sciences is appropriate for the social sciences.⁴ Unlike the Kuhnian world of the natural sciences, where one paradigm can supersede another based on widely accepted criteria, in the social sciences there are “no paradigms or explanations universally accepted

³ Forsberg adds that the problem is compounded by “the strong politicized habit of reading research outputs backwards from policy recommendations.”

⁴ Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1970).

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as better than others.” This, he adds, is “for the best.” Scientific progress is more likely in contexts that “put scholars in competition rather than in consensus and normal science.”

This line of argument needs to be taken seriously by all scholars in security studies, in the broader field of international relations, in the social sciences more generally, and in historiography as well. Cornut is probably correct about differences in the study of international relations across national boundaries and the lack of interactions across those boundaries.⁵ He may also be right about the limited nature of the interactions between paradigms, though I think that depends on exactly how one defines a paradigm in international relations. There has certainly been ongoing interaction and engagement between scholars committed to realist and liberal paradigms, though some would say that both operate within a broader rationalist paradigm.⁶ Still, the emergence of constructivism as one of the two or three dominant paradigms in the field, along with the growing influence of postmodernist, poststructuralist, and feminist approaches, runs contrary to Cornut’s argument about the psychological, social, and institutional impediments to change in the IR field and the difficulties facing new entrants into the field. The IR field is arguably the most diverse in the discipline of political science. That may have been a within-paradigm diversity during early debates between traditionalists and behaviorists and between realists and liberals, but the ‘third debate’ has been inter-paradigmatic and focused on underlying ontological and epistemological issues.⁷ Cornut may be right that scholars often become psychologically committed to a certain approach, but there are a large number of exceptions. Consider, for example, the democratic peace research program, which some regard as driven by policy preferences and theoretical preconceptions. It is notable that some of the leading proponents of the democratic peace school started out as critics of the proposition that democratic states rarely if ever fight each other, but were eventually persuaded by the evidence and became some of its leading supporters. These scholars include Bruce Bueno de Mesquita, Stuart Bremer, and Zeev Maoz.⁸

Cornut also exaggerates when he claims that “even when scholars have a similar epistemology and the same broad audience, be it positivist or not, it is uncommon to see genuine engagement.” The emphasis on multi-

⁵ Arguments about national differences in the study of international relations have been advanced by Stanley Hoffmann, “An American Social Science: International Relations,” *Daedalus* 106:3 (Summer, 1977): 41-60; Ole Wæver, “The Sociology of a Not So International Discipline: American and European Developments in International Relations,” *International Organization* 52:4 (October 1998): 687-727, DOI: <https://doi.org/10.1162/002081898550725>.

⁶ John Gerard Ruggie, *Constructing the World Polity: Essays on International Institutionalization* (New York: Routledge, 1998).

⁷ On the intellectual history of debates within the International Relations field see Brian C. Schmidt, “On the History and Historiography of International Relations,” in Walter Carlsnaes, Thomas Risse, and Beth A. Simmons, eds., *Handbook of International Relations* (Thousand Oaks: Sage, 2014): 3-28; Yosef Lapid, “The Third Debate: On the Prospects of International Theory in a Post-Positivist Era,” *International Studies Quarterly* 33:3 (September 1989): 235-254.

⁸ James Lee Ray, *Democracy and International Conflict: An Evaluation of the Democratic Peace Proposition* (Columbia, South Carolina: University of South Carolina Press, 1995), 44. For an argument regarding the primarily evidence-driven nature of the democratic peace research program see Jack S. Levy, “Theory, Evidence, and Politics in the Evolution of Research Programs,” in Richard Ned Lebow and Mark Lichbach, eds., *Theory and Evidence in Comparative Politics and International Relations* (New York: Palgrave Macmillan, 2007), 177-197.

method research designs—involving some combination of large-N statistical methods, formal modeling, historical case studies, and, increasingly, experimental methods—is now standard in the field, and increasingly expected in doctoral dissertations, at least in the United States.⁹ The democratic peace research program has certainly been multi-method, and one can point to many other leading research programs in security studies that have been distinguished by their multi-method character: the diversionary theory of war, audience costs, economic interdependence and conflict, and alliances and war, to name a few. It is worth noting, with respect to Cornut's comments about "consensus and normal science," that although each of these research programs is in the "normal science" stage, each is characterized by an enormous amount of competition between scholars.¹⁰

Forsberg's claim that debates in the field are often driven more by arguments over theory and a commitment to simplicity than by concerns about empirical adequacy is sometimes valid, though I think there is considerable variation across research programs. As I suggested earlier, I do not think it is valid for the democratic peace. It is worth noting that Forsberg's argument parallels one commonly made by historians—that international relations scholarship is often driven more by rigid theoretical preconceptions than by the evidence. Historians' conceptions of the dogmatic use of theory in political science is reflected in Isaiah Berlin's comment that an "addiction to theory—being doctrinaire—is a term of abuse when applied to historians; yet it is not an insult if applied to a natural scientist."¹¹ It is interesting to note in this context that the interpretations of Russian behavior that Forsberg mentions all come from political scientists. It would be interesting to see if the most influential interpretations advanced by historians—of this case and of other cases—are also characterized by "simple explanations." I think that is unlikely, given the lesser weight historians generally give to the criterion of parsimony.¹²

Now let me return to the democratic peace. I agree with Harrison that system-level approaches to the study of the democratic peace should and will be increasingly important and influential in the future.¹³ I also agree with his statement that "For thirty years, dyadic research on the democratic peace regularly produced large and important insights." I would add that dyadic democratic peace research contributed not only to our understanding of the nature of democratic foreign policies and the relationships between democracies, but

⁹ The emphasis on multi-method research designs goes back as least as far as David Laitin, "Comparative Politics: The State of the Subdiscipline" in Ira Katznelson and Helen V. Milner, eds., *Political Science: The State of the Discipline* (New York: Norton, 2002), 630-659.

¹⁰ One of many examples is the heated debate between proponents of the democratic peace and the "capitalist peace." Gerald Schneider and Nils Petter Gleditsch, *Assessing the Capitalist Peace* (London: Routledge, 2012).

¹¹ Isaiah Berlin, "History and Theory: The Concept of Scientific History," *History and Theory* 1:1 (1960), 9.

¹² On differences between diplomatic history and international relations scholarship see Jack S. Levy, "Explaining Events and Testing Theories: History, Political Science, and the Analysis of International Relations," in Colin Elman and Miriam Fendius Elman, eds., *Bridges and Boundaries: Historians, Political Scientists, and the Study of International Relations* (Cambridge: MIT Press, 2001), 39-83.

¹³ Harrison has already made contributions in this area, in Ewan Harrison, "The Democratic Peace Research Program and System Level Analysis," *Journal of Peace Research* 47:2 (2010): 155-165. DOI: <https://doi.org/10.1177/0022343309356490>.

also to the research practices of the broader IR field. The robustness of the descriptive finding of the near absence of wars between democratic states played a significant role in leading many scholars to shift away from system-level analyses to the dyadic level in their studies of other kinds of international behavior, after concluding that many system-level analyses were both theoretically incomplete and empirically incapable of accounting for much of the variance in the outbreak or expansion of international conflict. This shift in focus contributed significantly to the emergence of research programs on international rivalries, economic interdependence and conflict, interstate bargaining, and audience costs, among others, and also to new methodologies of analysis.¹⁴ Harrison is probably right that the pendulum has swung too far in the dyadic direction, and others have reinforced this point in various research areas,¹⁵ but this should not lead us to minimize the past contributions of dyadic analysis.

Harrison's call for more system-level research on the democratic peace, along with my argument about shifting levels of analysis over time in other research programs, remind us that research programs go through phases. This applies to the theoretical or empirical emphasis in a particular research program at a particular time as well as to the predominant level of analysis. Elsewhere I have suggested a classification of research programs as primarily evidence-driven, primarily theory-driven, or characterized by an alternating sequence of theory and evidence—of conjectures and refutations, to use Karl Popper's concept.¹⁶ Democratic peace research was driven primarily by evidence in its early stages. For the last decade and a half, theory has played a much greater role, in the form of an intense theoretical competition to explain a near law-like regularity. Other research programs are driven primarily by theory in their early stages, and perhaps longer. This is true of the bargaining model of war and of rational models of international relations more generally.¹⁷ It is probably also true of Waltzian structural realism. Still other research programs are characterized by an alternating sequence of conjectures and refutations. Going beyond International Relations, an example might be decision theory, defined in terms of both formal (normative) decision theory (centered around expected utility theory) and work in behavioral economics on how people actually make choices under conditions of risk (for example, prospect theory).

Many would argue that ideal research programs follow Popper's model of conjectures and refutations, for which criteria of evaluation ought to reflect some combination of theory and evidence. This raises the question, however, of whether different criteria might be appropriate for the evaluation of different stages of a research program. A system of evaluation that gave too much weight to theoretical criteria, applied to democratic peace research in the late 1990s, or to research on territory and conflict at about the same time,¹⁸

¹⁴ Stuart A. Bremer, "Dangerous Dyads: Conditions Affecting the Likelihood of Interstate War, 1816-1965," *Journal of Conflict Resolution* 36:2 (June 1992): 309-341.

¹⁵ Benjamin Fordham and Paul Poast, "All Alliances are Multilateral: Rethinking Alliance Formation," *Journal of Conflict Resolution* 60:5 (August 2016): 840-865. DOI: <https://doi.org/10.1177/0022002714553108>.

¹⁶ Levy, "Theory, Evidence, and Politics"; Karl R. Popper, *Conjectures and Refutations* (New York: Basic Books, 1962).

¹⁷ On the bargaining model of war see James D. Fearon, "Rationalist Explanations for War," *International Organization* 49:3 (Summer 1995): 379-414.

¹⁸ John A. Vasquez and Marie T. Henehan, *Territory, War, and Peace* (London: Routledge, 2011).

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would have significantly underestimated the cumulation of knowledge and scientific progress in each of those research communities. Similarly, a system of evaluation that gave too much weight to empirical criteria, applied to the bargaining model of war, say, in 2010, would have significantly underestimated the scientific progress of that research program and its influence in security studies.

This line of argument suggests an additional layer of difficulty in assessing scientific progress in security studies and in the social sciences more generally. There are multiple paths to the cumulation of knowledge; those paths are not necessarily linear, and a single set of criteria and single set of weights applied to those criteria might not be appropriate. Still, the place to begin any evaluation of competing theories or research program is the specification and conceptual clarification of the criteria for evaluation, as Chernoff has so persuasively demonstrated.

Participants:

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Tuomas Forsberg is Professor of International Politics at the University of Tampere. He is also deputy director of the Centre of Excellence on Choices of Russian Modernisation at the Aleksanteri Institute of the University of Helsinki. Previously he has worked at the University of Helsinki, at the George C. Marshall European Center for Security Studies, Garmisch-Partenkirchen, Germany and at the Finnish Institute of International Affairs. He gained his PhD at the University of Wales, Aberystwyth in 1998. His research has dealt primarily with European security issues, focusing on the EU, Germany, Russia and Northern Europe. His publications include *Divided West: European Security and the Transatlantic Relationship* (co-authored with Graeme Herd, Blackwell 2006), *The European Union and Russia* (co-authored with Hiski Haukkala, Palgrave