

Chapter 1

Introduction

Appraising Progress in International Relations Theory

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This book investigates how international relations (IR) theorists can equip themselves to determine whether the subfield's work is getting any better; that is, whether it is progressive in the sense of providing cumulative knowledge about hitherto unexplained phenomena. To answer this question, we make use of some well known theories of scientific change. These might seem far removed from the concerns of working practitioners in the subfield, but in science, including political science, there is no "just doing it." Even those working political scientists who loudly declare their indifference to philosophy of science are inevitably using methodological toolkits based on prior, if unconscious, choices about what it means to achieve and to measure progress.¹ Understanding those toolkits gives IR practitioners a better

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1. For a convincing argument, "written from a philosopher's point of view," that we also need to pay close attention to ontology, see Alexander Wendt, *Social Theory of International Politics* (Cambridge, UK: Cambridge University Press, 1999), pp. 32, 37. Although Wendt suggests that "IR scholars have been too worried about epistemology," he uses language and evaluative criteria that are consistent with those presented by philosopher of science Imre Lakatos. See *ibid.*, pp. 20, 29, 40, 48, 58, 158, 159.

grasp of the potential and the limits of their selected methodologies, and a greater appreciation of the alternatives.² Quite apart from concerns for the accumulation of knowledge, these matters can have surprisingly concrete practical consequences for a scholar's career: professional reputations, research grants, book contracts, and the ability to attract students and followers all hinge on whether one's work is judged positively by others.

We agree with economist Richard Bradley that a refusal to engage in and benefit from methodological debate is to abandon the terrain to intuition, and to the prejudices of whoever has the authority to decide the standards that should be applied.³ Thus our main interest in this volume is in providing information for IR theorists who perform and assess appraisals within the discipline. We are not suggesting that such evaluations should become the profession's main preoccupation: if everyone spent their time describing and assessing previous scholarship, political science would grind to a halt. However, such appraisals are important and have a long and useful track record in the discipline; when they are done, they should be done well. Meaningful stock-taking requires making explicit and informed selections from among alternative ways to describe and evaluate theories.

While political scientists have often shown an interest in evaluating the state of their discipline,⁴ most have relied on partial and subterranean criteria. The international relations subfield is no exception.

2. For similar discussions of the importance of philosophical studies to practicing historians, and of the mutual benefits of cross-disciplinary dialogue between historians and philosophers, see Peter Achinstein, "History and Philosophy of Science: A Reply to Cohen," in Frederick Suppe, ed., *The Structure of Scientific Theories*, 2d ed. (Chicago: University of Illinois Press, 1977), pp. 350–360; and I. Bernard Cohen, "History and the Philosopher of Science," in *ibid.*, pp. 308–349.

3. Richard Bradley, "Review of Explorations in Economic Methodology," *British Journal for the Philosophy of Science*, Vol. 50, No. 2 (June 1999), p. 316.

4. See, for example, Terence Ball, "Introduction," in Terence Ball, ed., *Idioms of Inquiry: Critique and Renewal in Political Science* (Albany: State University of New York Press, 1987), p. 1; James Farr, John S. Dryzek, and Stephen T. Leonard, *Political Science in History: Research Programs and Political Traditions* (New York: Cambridge University Press, 1995).

Its practitioners have produced a steady stream of research appraisals. The end of the Cold War and the close of the millennium brought a marked expansion in the number of stock-taking analyses of the subfield.⁵ This trend is well represented by Frank Harvey's Call for Papers for the 2000 annual convention of the International Studies Association: titled "Reflection, Integration, Cumulation: International Studies Past and Future," it invited "self-critical, state-of-the-art 'reflection' within epistemologies, perspectives and subfields" and suggested that without such reflection, "the promise of International Studies cannot be fulfilled."⁶

Recent assessments identify theoretical developments in a variety of research areas, and rate those that have proved most and least useful to the study of international relations. They also question why some theoretical orientations—notably neorealism, dependency, and world systems theory—have become less popular, while others—such as rational choice, historical institutionalism, and constructivism—have received increased support. However, in identifying "better" theories, and describing the successes and failures of IR research programs, these field surveys rarely address whether there is a pattern to the fate of specific research agendas, or explain why particular theories of international relations have waxed or waned. More importantly, almost

5. See, for example, Richard Ned Lebow and Thomas Risse-Kappen, *International Relations Theory and the End of the Cold War* (New York: Columbia University Press, 1995); Michael W. Doyle and G. John Ikenberry, *New Thinking in International Relations Theory* (Boulder, Colo.: Westview Press, 1997); Tim Dunne, Michael Cox, and Ken Booth, eds., "The Eighty Years Crisis 1919–1999," *Review of International Studies* (special issue), Vol. 24, No. 5 (December 1998); Peter J. Katzenstein, Robert O. Keohane, and Stephen D. Krasner, eds., "International Organization at Fifty: Exploration and Contestation in the Study of World Politics," *International Organization* (special issue), Vol. 52, No. 4 (Autumn 1998); and Davis B. Bobrow, ed., "Prospects for International Relations: Conjectures About the Next Millennium," *International Studies Review* (special issue), Vol. 1, No. 2 (January 1999).

6. Similarly, Ada Finifter notes that the millennium makes "critical evaluations and assessments of research traditions and literatures" particularly timely. Ada Finifter, "Editor's Notes," *American Political Science Review*, Vol. 91, No. 4 (December 1997), pp. viii–ix.

none of the recent appraisals adequately engage the question of what measures should be used to determine whether various theoretical moves are progressive.

There is a strong tendency in the subfield to engage in metatheoretic exercises without metatheory; to evaluate theoretical aggregates without using suitable or even necessary toolkits. A good example is the assessment by Jeffrey W. Legro and Andrew Moravcsik of contemporary theoretical developments in realism, an analysis that, although it is cloaked in metatheoretic terms of art, overlooks pertinent epistemology. Legro and Moravcsik argue that “the specification of well-developed paradigms around sets of core assumptions remains central to the study of world politics,” and accordingly they describe and evaluate realism as a metatheoretic unit.⁷ But they decouple this theoretical aggregate from any underlying metatheory, noting that they “do not mean to imply more with the term ‘paradigm’” than they state, and suggesting that the term is interchangeable with “‘basic theory,’ ‘research program,’ ‘school,’ or ‘approach.’”⁸ Legro and Moravcsik insist that their evaluation of realism (and, by implication, of any other body of IR work) does not depend on holding to a specific philosophy of science. Although their statement is more unequivocal than most, this is not an isolated instance. Almost all disciplinary appraisals in the subfield similarly neglect to state the standards by which research is to be judged.

In organizing this volume as an examination of progress in IR theory, we were motivated by the belief that it is impossible to engage in disciplinary appraisals without making explicit selections from among a menu of competing epistemologies. The contributors to this volume follow recent stock-taking efforts by focusing on theoretical approaches that have had significant influence and staying power:

7. Jeffrey W. Legro and Andrew Moravcsik, “Is Anybody Still a Realist?” *International Security*, Vol. 24, No. 2 (Fall 1999), p. 8.

8. *Ibid.*, p. 9, note 5. In note 6, Legro and Moravcsik direct readers’ attention to “a fuller account of the desirable criteria” in a working paper that predates their *International Security* essay, but that paper does not address the ambiguity noted above either, and is no more connected to relevant literature on theory appraisal.

realism, liberalism, institutionalism, power transition theory, the democratic peace, and psychological decision making. However, by explicitly grounding these evaluations in metatheory, they go well beyond previous assessments. We asked them to use an influential theory of scientific change—Imre Lakatos’s methodology of scientific research programs (MSRP)—as a basis for exploring how we should appraise progress in international relations.⁹ Taking Lakatos’s metatheory as its starting point, the chapters in this book use his methodology to organize an analysis of major research programs of the last several decades, and make a systematic effort to evaluate them using its criteria for measuring theoretical progress.

This volume has three central goals. First, it lays out a received description of Lakatos’s framework for evaluating theoretical and empirical progress. We believe this is necessary and useful because, while many IR scholars have used Lakatos’s metric as an “organizing device” and as a means of defending or undermining scholarly contributions, and while Lakatos’s 1970 essay is probably one of the most cited methodology texts in the subfield, the great majority of citations appear in boilerplate footnotes, and most of the substantive applications or discussions of the methodology of scientific research programs in IR have proceeded on the basis of popularized, misleading, and incomplete accounts of the metatheory. We argue that decisions whether to employ Lakatos’s methodology should be based on what Lakatos and his most thoughtful followers and critics actually say, not on what distant users in the IR subfield have come to believe that they say.

9. 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), pp. 91–196. See also Imre Lakatos, “History of Science and its Rational Reconstructions,” in Roger C. Buck and Robert S. Cohen, *Boston Studies in the Philosophy of Science, PSA 1970*, Vol. 8 (Dordrecht: D. Reidel, 1971), pp. 91–136; Imre Lakatos, “Replies to Critics,” in *ibid.*, pp. 174–182; and Imre Lakatos, “The Role of Crucial Experiments in Science,” *Studies in History and Philosophy of Science*, Vol. 4, No. 4 (1974), pp. 309–325.

Our second goal in this volume is to ask whether Lakatos's methodology is a usable one for evaluating IR theory. Although Lakatos is frequently cited, and there have been some applications of his methodology to particular research programs, there have been no serious attempts to investigate whether the subfield's theoretical developments actually reflect Lakatos's theory of scientific change. In this volume, Lakatos's metric is assessed against an extensive empirical record. The contributors evaluate whether theoretical developments in IR correspond to Lakatos's methodology, and whether his framework offers any useful recommendations as to how we can best promote the growth of knowledge in the subfield. The book thus addresses both descriptive and prescriptive questions: does the methodology of scientific research programs portray how IR research actually develops, and does Lakatos provide the right criteria for assessing the merit of IR theories?¹⁰

Lastly, this volume has the broader goal of developing debate on the nature of scientific change in the social sciences in general, and in the study of international relations in particular. The methodology of scientific research programs is a useful point of departure, but it is not a philosophical straitjacket that we are committed to advocate or employ. This volume is not produced by a "closed shop of committed Lakatosians" (to borrow Mark Blaug's phrase).¹¹ Science can progress in more than one way, and none of the authors argue for the unquestioning acceptance or universal application of Lakatos's methodology. Although several of the contributors find that his

10. While this book covers some of the major theoretical gambits in the IR subfield, due to obvious space constraints not everything could be included in one volume, and we are aware that we have left out some important areas of research. In particular, our choice of topics reflects research agendas that have had high profiles among scholars in North America, and moreover is heavily weighted toward security issues. A more complete appraisal of recent IR scholarship would also include research agendas that reflect interests from other regions, as well as more on international political economy.

11. See Mark Blaug, "Afterword," in Neil de Marchi and Mark Blaug, eds., *Appraising Economic Theories: Studies in the Methodology of Research Programs* (Brookfield, Vt.: Edward Elgar, 1991), p. 499.

methodology has some utility for the IR subfield, none are Lakatos boosters. Some contributors are quite critical of Lakatos's methodology, and find his metric wanting when compared to competing theories of scientific change offered by other leading philosophers such as Thomas Kuhn, Larry Laudan, and Deborah Mayo.

Why Lakatos?

We argued above that disciplinary appraisals require metatheory: a way to describe and evaluate the trajectory of different theoretical aggregates. In this volume we use as our point of departure Lakatos's methodology of scientific research programs (see "A Brief Guide to Imre Lakatos's Methodology of Scientific Research Programs," pp. 19–20). We do not claim that Lakatos's approach is the best of the alternative philosophies of science. Since IR theorists have largely ignored metatheory, the volume could have broken new ground by beginning with virtually any epistemology. Nonetheless, there are at least four reasons why a more careful study of Lakatos's methodology of scientific research programs is worth the effort.

First, on a variety of grounds, it is an intuitively appealing and powerful candidate metatheory for describing and evaluating research. As we suggest in Chapter 2, it provides a rationalist, pluralist, and tolerant metric that rewards creativity, innovation, and inventiveness. Its descriptions of intellectual trajectories, together with its battery of standards for research programs, are those that many IR scholars acknowledge as logical and consistent with the way they and their colleagues work, or should work. Standards that are consistent with Lakatos's already figure prominently in IR methodology texts, for example, that empirical evidence should be the final arbiter among competing theories, that facts employed in constructing a theory should not be the only ones used to test it, and that good theories should be able to explain facts outside their initial domain of

application.¹² Lakatos's central claims transfer well to IR, particularly his advocacy of tolerance and tenacity. IR theorists acknowledge, and tolerate, the existence of competing research programs. Stephen M. Walt, for example, argues that while "scholarship is a competitive enterprise ... the competition that drives progress should be tempered with the recognition that different research traditions can and should coexist."¹³ IR scholars also recognize the importance of guiding assumptions and theoretical commitments, and expect scientists who encounter evidentiary discrepancies to fight with tenacity to save their theories.

A second reason for using Lakatos's methodology is that, because it views science not as individual theories but as a series of theories connected by a common core, recent theoretical developments in realism, liberalism, and constructivism are now amenable to Lakatosian appraisal. Several research areas in the subfield have each accumulated a series of theoretical reformulations, and accordingly the metric may now be useful to assess them. By providing a set of rules that enable us to judge what we have learned from developments in IR theory, Lakatos's methodology of scientific research programs helps us to determine whether such iterations offer added value.¹⁴

Third, consideration of Lakatos's methodology may be particularly timely, given criticisms against much research activity in diverse areas—from the democratic peace to the balancing of power—claiming that it consists of illegitimate theoretical revisions

12. See, for example, Gary King, Robert O. Keohane, and Sidney Verba, *Designing Social Inquiry: Scientific Inference in Qualitative Research* (Princeton, N.J.: Princeton University Press, 1994).

13. Stephen M. Walt, "A Model Disagreement," *International Security*, Vol. 24, No. 2 (Fall 1999), p. 130. Although Walt advocates competition and pluralism, he is not a fan of Lakatos. See Stephen M. Walt, "The Progressive Power of Realism," *American Political Science Review*, Vol. 91, No. 4 (December 1997), pp. 931–935.

14. Whether we should view IR research as a series of theoretical aggregates, however, is open to debate. For example, Randall Schweller (Chapter 9 in this volume) insists that Kenneth Waltz's neorealist theory has not been amended, and so neorealism cannot be considered a Lakatosian research program.

accommodating empirical discrepancies. For example, realists typically argue that democratic peace theorists stubbornly shield liberal claims from awkward facts by mere semantic changes, such as reformulating conceptual definitions and causal mechanisms.¹⁵ Critics of realism make similar charges: John A. Vasquez, for instance, recently claimed that contemporary realist theories of balancing are suspect because proponents have reconstructed realism in ways that explain anomalies, but not much else.¹⁶ Similarly, Legro and Moravcsik argue that today's realists are explaining an increasing number of empirical anomalies in a "trivially easy" fashion by softening realism into a loose rationalism indistinguishable from existing liberal and institutionalist theory.¹⁷ Since the methodology of scientific research programs provides explicit guidelines about how scientists should approach empirical counterexamples, and offers conjectures regarding how in practice they do go about dealing with anomalies, a better understanding of Lakatos's ideas might shed light on whether these criticisms are justified. As political scientist Hillard Pouncy puts it, "Lakatosian methodology can be usefully applied in situations in which an evaluator wants to sort out how well a program has defended itself."¹⁸

15. See, for example, David E. Spiro, "The Insignificance of the Liberal Peace," in Michael E. Brown, Sean M. Lynn-Jones, and Steven E. Miller, eds., *Debating the Democratic Peace* (Cambridge, Mass.: MIT Press, 1996), pp. 206–214; and Stephen M. Walt, "Never Say Never: Wishful Thinking on Democracy and War," *Foreign Affairs*, Vol. 78, No. 1 (January/February 1999), pp. 149–150. For an extended discussion of how democratic peace theorists handle anomalous evidence, see Miriam Fendius Elman, "The Never-Ending Story: Democracy and Peace," *International Studies Review*, Vol. 1, No. 3 (Fall 1999), pp. 87–103.

16. John A. Vasquez, "The Realist Paradigm and Degenerative Versus Progressive Research Programs: An Appraisal of Neotraditional Research on Waltz's Balancing Proposition," *American Political Science Review*, Vol. 91, No. 4 (December 1997), pp. 899–912.

17. Legro and Moravcsik, "Is Anybody Still a Realist?" See also Chapter 5 by Andrew Moravcsik in this volume.

18. Hillard Pouncy, "Terms of Agreement: Evaluating the Theory of Symbolic Politics' Impact on the Pluralist Research Program," *American Journal of Political Science*, Vol. 32, No. 3 (August 1988), p. 784.

Finally, a better understanding of Lakatos's methodology is warranted because IR theorists have long noted the utility of his approach. In 1985, Stephen D. Krasner observed that "Lakatos's sophisticated methodological falsification offers a reasonable set of criteria for assessing research.... [It] is an admirable analytic prescription."¹⁹ Over a decade later, Thomas J. Christensen and Jack Snyder similarly noted that "students of international politics should justify their theories in terms of Imre Lakatos's criteria for distinguishing progressive research programs from degenerative ones."²⁰ Despite these and similar endorsements, most IR theorists have proceeded with only a partial account of the methodology, and without making the predicate choices necessary for its use. Nor have they referred to the voluminous body of work on Lakatos's methodology of scientific research programs that has sought to extend and clarify the method.²¹ We aim to provide a more comprehensive and inclusive account.

19. Stephen D. Krasner, "Toward Understanding in International Relations," *International Studies Quarterly*, Vol. 29, No. 2 (June 1985), p. 137.

20. Thomas J. Christensen and Jack Snyder, "Progressive Research on Degenerate Alliances," *American Political Science Review*, Vol. 91, No. 4 (December 1997), p. 919.

21. See, for example, Thomas S. Kuhn, "Reflections on my Critics," in Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (New York: Cambridge University Press, 1970), pp. 231–278; Thomas Kuhn, "Notes on Lakatos," in Buck and Cohen, *Boston Studies in the Philosophy of Science*, Vol. VIII, pp. 137–146; Lakatos, "Replies to Critics"; Lakatos, "History of Science and its Rational Reconstructions"; Alan Musgrave, "Logical versus Historical Theories of Confirmation," *British Journal for the Philosophy of Science*, Vol. 25 (1974), pp. 1–23; Alan Musgrave, "Method or Madness? Can the Methodology of Research Programmes be Rescued from Epistemological Anarchism?" in R.S. Cohen, P.K. Feyerabend and M.W. Wartofsky, eds., *Essays in Memory of Imre Lakatos* (Dordrecht: D. Reidel, 1976), pp. 457–491; John Worrall, "Imre Lakatos (1922–1974): Philosopher of Mathematics and Philosopher of Science," in *ibid.*, pp. 1–8; Alan Musgrave, "Evidential Support, Falsification, Heuristics, and Anarchism," in Gerard Radnitzky and Gunnar Andersson, eds., *Progress and Rationality in Science* (Dordrecht: D. Reidel, 1978), pp. 181–201; John Worrall, "The Ways in Which the Methodology of Scientific Research Programmes Improves on Popper's Methodology," in *ibid.*, pp. 45–70; and John

Organization of the Book

In Chapter 2, we describe Lakatos's metric for theory appraisal; discuss and debunk some myths and misconceptions about the methodology of scientific research programs that have become prevalent in the field of international relations; and identify some of the metatheory's weaknesses.

The remainder of the volume is organized into two parts. The first part identifies and evaluates several research programs in the IR subfield. The first five chapters in Part I employ Lakatos's methodology of scientific research programs to judge theoretical and empirical progress in research on institutionalism, power transition theory, liberalism, the democratic peace, and operational code analysis. While applying the methodology, these chapters also discuss its limitations, and suggest alternative ways to evaluate scientific growth. Additional chapters in Part I revisit theoretical developments in realism, neoliberalism, and normative research from a variety of perspectives, not just those of Lakatos. These essays highlight some difficulties with identifying research programs in IR, and demonstrate how such descriptions come to be contested.

In Part II, contributors offer commentaries on the previous chapters, and on the volume as a whole: they assess the applications of Lakatos and the appraisals; discuss the advantages and disadvantages of using Lakatos's methodology; and suggest how IR scholars might move beyond Lakatos's account of scientific development.

In the rest of this introductory chapter, we describe the other chapters of the book in more detail.

Worrall, "Research Programmes, Empirical Support and the Duhem Problem: Replies to Criticism," in *ibid.*, pp. 321–338. For more recent discussions, see Neil de Marchi and Mark Blaug, eds., *Appraising Economic Theories: Studies in the Methodology of Research Programs* (Brookfield, Vt.: Edward Elgar, 1991); Jarrett Leplin, *A Novel Defense of Scientific Realism* (New York: Oxford University Press, 1997); and Roger E. Backhouse, *Explorations in Economic Methodology: From Lakatos to Empirical Philosophy of Science* (New York: Routledge, 1998), pp. 1–71.

PART I: APPLYING LAKATOS: JUDGING THEORETICAL AND EMPIRICAL PROGRESS IN INTERNATIONAL RELATIONS

In Chapter 3, Robert O. Keohane and Lisa L. Martin describe, in Lakatosian terms, realist theory and institutional theory, and empirical developments within the institutional theory research program. Keohane and Martin argue that institutional theory is a “half-sibling of realism”: it has adopted almost all of the realist hard core, except that it treats information as a variable. The authors argue that much of the institutional theory research program has been progressive: supporters have easily turned challenges such as the relative gains problem into confirmations of the program, rather than refutations of it. A more fundamental problem for institutional theory proponents, however, is how to handle the realist challenge that institutions are endogenous to structure and are thus epiphenomenal. According to Keohane and Martin, agency theory may provide institutional theory with the means to deal with this challenge. They conclude that “although Lakatos’s criteria are ambiguous and his own formulations often contradictory, thinking about whether research programs are ‘progressive’ remains, in our view, a useful way to help us evaluate their relative merits.”

In Chapter 4, Jonathan M. DiCicco and Jack S. Levy describe the power transition research program, and identify the theoretical amendments that have been degenerative and progressive according to Lakatosian criteria. DiCicco and Levy argue that power transition theory incorporates two ideas that differentiate it from balance of power realism: the importance of changes in power distributions that result from industrialization, and the stabilizing effects of power concentrations. The peripheral role of alliances in power transition theory is also a major point of difference: in balance of power realism, alliances and alignment behavior have a more integral explanatory role. According to DiCicco and Levy, a better understanding of power transition theory’s hard core makes it easier to see which contemporary studies aimed at handling refutations and anomalies have constituted progressive developments within the research program, and which work is better viewed as a break with the power transition research program. They conclude that “most theoretical extensions of power transition principles have generated novel predictions, many of which

have been empirically corroborated.” Thus they find that, overall, the power transition research program has many progressive elements.

In Chapter 5, Andrew Moravcsik specifies the elements that distinguish the liberal research program from its realist, institutionalist, and constructivist competitors. Moravcsik argues that liberalism has been, and continues to be, a progressive research program, because it has predicted new facts that have been empirically corroborated, meeting criteria set by Lakatos. In particular, liberalism explains many recent major developments in world politics, even though they were not prevalent during the Enlightenment when liberal theories were initially formulated. In addition, liberalism explains phenomena that contradict, or cannot be predicted by, rival realist theories. Moravcsik argues that, in contrast to liberalism, realism is degenerating. In accounting for empirical anomalies, he asserts, contemporary realists have constructed new versions of realist theory that are blatantly inconsistent with its hard core assumptions and are virtually indistinguishable from competing “background theories,” especially liberalism and institutionalism. While Moravcsik concludes that the discipline imposed by Lakatos’s approach on theory construction and development offers some benefits, he argues that excessive “Lakatosian thinking” would inhibit scientific progress in the subfield. Moravcsik argues that Lakatos’s methodology of scientific research programs fosters a zero-sum competition between all encompassing approaches and would divert attention from rigorous and useful theory synthesis.

In Chapter 6, James Lee Ray reconstructs the democratic peace research program according to Lakatosian guidelines. Ray argues that proponents of the democratic peace proposition have “proven capable of turning anomalies or apparently disconfirming evidence into strengths and corroborating instances.” Moreover, Ray argues, research on the democratic peace phenomenon continues to expand the number of dependent variables explained by the theory. These include explanations for why democracies are more likely to trade with each other, form lasting leagues and alliances, obey international laws, and win the wars in which they participate. This expansion attests to progress in a Lakatosian sense, says Ray: a set of scholars is using the theory to predict new facts that they are then empirically corroborating.

Ray provocatively concludes that the democratic peace research program might be said to falsify realism, because it not only explains outcomes that realism successfully explains, but also has “excess empirical power over realism,” and is “able to plug a significant gap left by realism ... in a logical, axiomatically-based manner.”

In the book’s last application of Lakatos’s framework, Stephen G. Walker argues in Chapter 7 that, over the past four decades, theoretical emendations and empirical testing in operational code analysis have addressed important anomalies and generated novel facts. For example, an emphasis on self-schemata and self-scripts rather than images of other states accounted for anomalies in previous applications of cognitive theory to foreign policy choices, but only some of these theoretical amendments predicted novel facts that were subsequently corroborated empirically. Walker concludes that while Lakatos’s model of scientific change is consistent with the development of research on operational codes, philosopher of science Larry Laudan’s criteria for theory appraisal lead to a more accurate description of the evolution of this scientific research program. In particular, the fact that cognitive theory, game theory, and personality theory all combine in operational code analysis is consistent with Laudan’s notion of “theory complexes”—sets of theories that complement each other in the solution of common empirical problems.

While the initial chapters in Part I offer descriptions of what applications of the methodology of scientific research programs would look like for particular research programs in IR, the remaining three chapters in Part I focus primarily on better identifying IR research programs and their rivals. In Chapter 8, Robert Jervis discusses the differences between realism and neoliberalism. Like Keohane and Martin, Jervis argues that realism and neoliberalism have much in common. For example, Jervis notes that for both approaches, the differences among leaders have little effect; he further points out that several defensive realist arguments for how to reduce international conflict are compatible with neoliberal prescriptions. He also suggests that many of the factors commonly used to distinguish these two research programs from each other are either “false or exaggerated.”

While Jervis's chapter shows how difficult it is to delineate research programs, it also highlights the value of such an exercise.

Of all the chapters in this volume, Chapter 9 by Randall L. Schweller is the most critical of the methodology of scientific research programs. Schweller's "commonsense criteria" for judging progress in IR include aspects that are consistent with Lakatosian metatheory: for example, hypotheses should be supported by evidence, and knowledge should accumulate. Nevertheless, Schweller insists, the determination of which research programs thrive and which ones die "has more to do with what kinds of theories we find intellectually and politically appealing" than the extent to which they are empirically accurate. Using his own set of appraisal criteria, Schweller identifies and defends a new school of political realism that he calls neoclassical or neotraditional realism.

In Chapter 10, Jack Snyder questions how progress in IR should be assessed when theories include normative elements. He argues that Lakatos's framework, and positivist methods more generally, can be used to evaluate the empirical as well as the logical aspects of normative research programs. He also suggests that the intellectual trajectory of several programs he identifies conforms to the description of scientific change laid out in Lakatos's methodology of scientific research programs. For example, Snyder argues that, consistent with Lakatos, the logical structures of many normative research programs in the subfield strive to keep propositions consistent with their hard cores. When scholars who make normative arguments about standards of appropriate international behavior confront empirical anomalies, they have typically fashioned theoretical emendations in order to defend their research programs from falsification. Snyder argues that, "precisely because of the practical stakes in having a sound empirical theory of ethnic peace, the rigorous application of social science standards of falsifiability is especially important in this type of normative research," and that "empirical social science has a great deal to contribute to contemporary debates about multiculturalism, human rights, and virtually every other normative question of international relations."

PART II: COMMENTARIES ON LAKATOS, AND BEYOND

Part II of the volume offers commentaries on the previous applications of Lakatos, and on the book's central question of how we can know whether the international relations subfield of political science is making progress. David Dessler opens this section in Chapter 11 with a discussion of the advantages and disadvantages of employing Lakatos's methodology to appraise theory developments in IR. Dessler argues that "Lakatos's methodology of scientific research programs remains a useful departure point for discussions of progress in international relations" because it helps us to appreciate that research agendas are best considered in "dynamic profile, rather than in static snapshots." He also suggests that Lakatos's notion of a positive heuristic is particularly helpful because it directs scientists to increase the explanatory power of simple models by making them increasingly more complex and realistic. However, Dessler also argues that, if a sufficient condition for scientific progress is explanatory progress, then Lakatos's metric does not provide the means for assessing research programs that depend on historical research. According to Dessler, many of the debates in IR are not theoretical; that is, they do not involve pitting one series of theories against another. Rather, programs such as those investigating the democratic peace, the end of the Cold War, and ethnic conflict involve the development of more accurate descriptions of the historical record. Here progress is measured not just in terms of theory building, but on the "historical side of the ledger." He concludes that although IR research often conforms to the Lakatosian "verificationist" strategy of theory building, applying Lakatos in practice might increase the tendency to continue with research programs that have little or no potential for successful development. Dessler says that Lakatos's work should not be ignored, but we should not downplay the significance of putting hypotheses to "severe" tests, as urged by Karl Popper.

In Chapter 12, Roslyn Simowitz discusses the lessons to be learned from the applications of Lakatos's methodology of scientific research programs, such as those offered by DiCicco and Levy in Chapter 4 and by Snyder in Chapter 10. Simowitz notes that DiCicco and Levy's efforts to apply Lakatosian appraisal criteria to power transition theory

reveal a significant drawback to the use of his framework in IR: Lakatos provides no guidance for choosing between competing programs when they each contain progressive and degenerative problemshifts. Net assessments of research programs are likely to prove especially difficult in IR because few of them exhibit evidence of progress across the board. She also disagrees with Snyder that Lakatos's metatheory can be applied to normative arguments: she argues that it is impossible to corroborate or refute normative predictions empirically, and since such corroboration or refutation is essential to Lakatos's theory of confirmation, it is impossible to use his framework when assessing normative research programs. Simowitz concludes that, despite these problems in applying Lakatosian metatheory, its use is justified. Its requirement of a precise statement of hard core assumptions, for example, makes it easier to identify conflicting assumptions and inconsistent predictions in rival programs.

In Chapter 13, John Vasquez argues that Lakatos provides helpful rules for distinguishing legitimate adjustments to theories from ad-hoc reformulations, as when contradictions between a theory and evidence are "resolved" in merely semantic ways. Vasquez notes, however, that the methodology of scientific research programs is one among many standards, and argues that these need not be mutually exclusive. For example, he suggests that scholars interested in making "systematic and rigorous" disciplinary appraisals can usefully combine Kuhnian and Lakatosian perspectives. Vasquez notes, however, that Lakatos's metric is only applicable to IR research that has produced a series of theoretical emendations. To use Lakatosian criteria there must be "a considerable body of research" resulting in anomalies that need to be explained. According to Vasquez, offense-defense theory is an example of an area where, because there has not been a great deal of theoretical innovation in response to discrepant evidence, appraisal criteria other than Lakatos's are more appropriate.

In the final chapter, Andrew Bennett concludes that, while Lakatos's methodology is useful for judging theoretical progress, it provides an imperfect standard that cannot be relied upon exclusively. Reviewing the chapters by Elman and Elman (Chapter 2) and by Dessler (Chapter 11), Bennett contrasts the methodology of scientific research programs

with three post-Lakatosian schools of thought: the Bayesian approach associated with John Earman, Colin Howson, and others; the error-statistical school articulated by Deborah Mayo; and the focus on puzzle-solving research traditions advocated by Larry Laudan. He suggests that each of these approaches, along with Lakatos's metatheory, are useful to "guide us toward an answer, or rather many answers, to the nagging question of whether IR has progressed and how we would know if it has."

While this volume does not aspire to convert anyone to a single or dominant method of evaluation, we sympathize with James Lee Ray's observation: "The broader the base of agreement in the field regarding the issue of how we know what we feel we know, the larger and more accommodating will be the platform accessible to all of us for fruitful dialogue." We did not expect, and as the following pages demonstrate, we did not find, consensus on the merits of Lakatos's methodology of scientific research programs. But by using Lakatos's metatheory as a point of departure, the contributors highlight a variety of difficulties common to all theory evaluations, and suggest some preliminary answers to them. Collectively, the chapters corroborate the intuition that was part of the initial impetus for this study: before we can measure progress, we need to decide how.

A Brief Guide to Imre Lakatos's Methodology of Scientific Research Programs

When should one scientific theory replace another? How do we know when one theory or group of theories is superior to another? These questions are addressed by metatheories, which have other theories as their subject matter. A **metatheory** is thus an inquiry at one remove: it is a view of our knowledge of things, as distinct from that knowledge.

One of the most influential statements on the advancement of knowledge is Karl Popper's argument that since science is by definition disprovable, "good" science consists of theories that we attempt to disprove (falsify) but cannot: theories that survive severe tests. (Popper's famous example points out that one could observe any number of white swans, but this would not prove the hypothesis that all swans are white. However, a single black swan can disprove the hypothesis. This concept is known as **falsifiability**.) If empirical data refutes a theory, the theory must be rejected and a replacement sought.

However, Popper, and the philosopher of science who extended his views—**Imre Lakatos**—recognized that scientists are often reluctant to discard their theories. Instead, when confronted by disconfirming evidence, scientists typically use a variety of strategies to save them, including rewriting theories to "cover" discrepancies. Popper and Lakatos both sought to devise rules for deciding when such defensive moves were legitimate.

Lakatos's model of scientific change goes beyond Popper's by shifting the unit of appraisal from individual theories to sequences of theories. He labeled these **scientific research programs** (SRPs); they comprise a series of theories linked by a set of constitutive and guiding assumptions. The **hard core** (or hard core assumptions) comprises the fundamental premises of a scientific research program. (For example, one of the hard core assumptions of the neorealist research program is that states, not sub-state or supra-state actors, are the primary actors in international politics.) The hard core is protected by a **negative heuristic**, which is the rule that forbids scholars within this scientific research program from contradicting its fundamental premises or hard core (e.g., in response to newly discovered evidence that seems to disconfirm the theory). Alteration of the hard core would result in the creation of a new SRP, because the hard core essentially defines the SRP; if it changes, the SRP changes.

A scientific research program also has a **protective belt of auxiliary hypotheses**. These are propositions that are tested, adjusted and readjusted, and replaced as new evidence comes to bear. (For example, in the neorealist research program, scholars typically distinguish two versions of the protective belt: defensive realism, in which states maximize security by defending the status quo; offensive realism, in which they do so by maximizing power.)

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The replacement of one set of auxiliary hypotheses with another constitutes an **intra-program problemshift**—it is “intra” or within the program because only the protective belt, not the hard core, is changed. Intra-program problemshifts should be undertaken in accordance with the program’s **positive heuristic**, a set of suggestions or hints that guide the development of specific theories within the program. (For example, the positive heuristic of the neorealist research program would include the suggestion that scholars make predictions about international political outcomes, e.g., that balances tend to form in the international system, or that multipolar systems will be more war-prone than bipolar systems.)

Despite the negative heuristic, scholars sometimes develop new theories which interfere with the hard core, thus creating a new research program through an **inter-program problemshift**. Both inter-program and intra-program problemshifts face the same problem identified by Popper and Lakatos: how can we tell if these theoretical emendations are just defensive moves designed to cover up discrepant evidence, rather than true progress?

Lakatos argued that to be judged **progressive** new theories must predict **novel facts**. If not, the new theories are merely **ad hoc**, and the research program is **degenerative**. As we detail in Chapter 2, philosophers of science disagree about exactly what this novelty criterion requires: “new” compared to what? One definition—which we prefer—is **heuristic novelty**: the new theory must predict something beyond the anomalous facts used in its construction. (For example, Stephen Walt developed balance of threat theory in part to solve a puzzle for Kenneth Waltz’s balance of power theory: western European countries chose to ally with the United States, rather than the Soviet Union, the weaker of the two superpowers. Because balance of threat theory was designed to accommodate this anomaly, solving it cannot count in favor of the new theory. However, Walt’s subsequent application of balance of threat theory to explain the connection between domestic revolution and war does count, because that hypothesized relationship was not used to construct the original theory.)

Lakatos’s approach is often contrasted with that of **Thomas Kuhn**, whose theory of scientific development sees scientific change as being revolutionary and non-rational, consisting of the wholesale replacement of one dominant view of how the world works (a **paradigm**) by a different one. In contrast to Kuhn, Lakatos rejected the view that a single research program controls a scientific discipline at any given time, or that the decision to reject an old research program and accept a new one was akin to a non-rational “conversion” involving a leap of faith or a “gestalt switch.” He argued instead that research programs should be judged on the basis of rational criteria: their ability to successfully generate predictions of novel facts that are subsequently corroborated with empirical evidence.