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## Chapter 4

### The Power Transition Research Program

#### A Lakatosian Analysis

Jonathan M. DiCicco and Jack S. Levy

Although the “paradigm wars” between realism and liberalism have framed much of the discourse in international relations theory over the past two or three decades, realists have recently begun to devote more attention to systematic divisions within their own ranks. Growing dissatisfaction with neorealism has led to a resurgence of interest in classical realism, to a new split between “offensive” realism and “defensive” realism, and to a variety of other efforts to recast realism on a more solid theoretical and empirical foundation.<sup>1</sup>

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1. On varieties of realism see Benjamin Frankel, ed., *Realism: Restatements and Renewal* (London: Frank Cass Publishers, 1997); Michael W. Doyle, *Ways of War and Peace* (New York: Norton, 1997); Randall L. Schweller and David Priess, “A Tale of Two Realisms: Expanding the Institutions Debate,” *Mershon International Studies Review*, Vol. 41, No. 1 (May 1997), pp. 1–32; Stephen G. Brooks, “Dueling Realisms,” *International Organization*, Vol. 51, No. 3 (Summer 1997), pp. 445–477; Gideon Rose, “Neoclassical Realism and Theories of Foreign Policy,” *World Politics*, Vol. 51, No. 1 (October 1998), pp. 144–172.

In spite of their differences, these distinct theories within the realist tradition share a common set of assumptions: the key actors in world politics are sovereign states who act rationally to advance their security, power, and wealth in a conflictual international system that lacks a legitimate governmental authority to regulate conflicts or enforce agreements. Realist theories also generate a number of common propositions: that the distribution of power, in the system or within a dyad, is the primary variable that shapes international outcomes; that high concentrations of power in the system generally lead to blocking coalitions and often to war; and that these blocking coalitions, or the anticipation of them, generally work to maintain the sovereign state system and prevent hegemonies from being established and maintained. Thus, classical realism, Waltzian neorealism, offensive and defensive realism, and neoclassical realism are all balance of power theories, although they vary in their specification of exactly who balances, under what conditions, and with what effects.<sup>2</sup>

There are other schools of thought that share basic realist assumptions about the determining role of power in world politics, but reject balance of power theory. Most prominent among these are Organski's power transition theory, Gilpin's hegemonic transition theory, and Modelski and Thompson's theory of long cycles of global leadership and decline.<sup>3</sup> Each of these approaches posits, contrary to balance of power theories, that

2. There are many variations of balance of power theory, and whether these constitute a single research program by Lakatosian standards is an interesting question that we do not explore here. For an assessment of the common elements in balance of power theories, see Jack S. Levy, "Balances and Balancing: Concepts, Propositions, and Research Design," in John A. Vasquez and Colin Elman, eds., *Realism and the Balancing of Power: A New Debate* (Englewood Cliffs, N.J.: Prentice-Hall, forthcoming). For a Lakatosian analysis of the realist research program on balancing, see other essays in the Vasquez and Elman volume.

3. A.F.K. Organski, *World Politics* (New York: Knopf, 1958); Robert Gilpin, *War and Change in World Politics* (Cambridge, UK: Cambridge University Press, 1981); Robert Gilpin, "The Theory of Hegemonic War," in Robert I. Rotberg and Theodore K. Rabb, eds., *The Origin and Prevention of Major Wars* (Cambridge, UK: Cambridge University Press, 1988), pp. 15–37; George Modelski and William R. Thompson, "Long Cycles and Global War," in Manus I. Midlarsky, ed., *Handbook of War Studies* (Boston: Unwin Hyman, 1989), pp. 23–54.

hegemonies frequently form, that these extreme concentrations of power are stabilizing rather than destabilizing and contribute to peace rather than to war, and that blocking coalitions do not generally form against dominant states. This variant of realist theory has been referred to as "hegemonic realism," as distinct from "balance of power realism."<sup>4</sup> Because the two approaches share some basic realist assumptions but generate mutually contradictory propositions, we treat them as different research programs within the realist tradition.<sup>5</sup> More specifically, we will argue that power transition theory and other forms of hegemonic theory constitute a break with the "hard core" of assumptions of balance of power realism.<sup>6</sup>

4. Jack S. Levy, "The Theoretical Foundations of Paul W. Schroeder's International System," *International History Review*, Vol. 16, No. 4 (November 1994), pp. 725–726.

5. Robert Gilpin, "No One Loves a Political Realist," *Security Studies*, Vol. 5, No. 3 (Spring 1996), p. 6, clearly identifies himself as a realist. Organski and his associates contrast power transition theory with balance of power theory, rarely mention realism, and explicitly reject the realist label. See Organski, *World Politics*; Jacek Kugler and A.F.K. Organski, "The Power Transition: A Retrospective and Prospective Evaluation," in Midlarsky, *Handbook of War Studies*, pp. 172–173; and Jacek Kugler and Douglas Lemke, "The Power Transition Research Program: Assessing Theoretical and Empirical Advances," in Manus I. Midlarsky, ed., *Handbook of War Studies II* (Ann Arbor: University of Michigan Press, 2000), pp. 129–163.

6. This does not imply that the balance of power and hegemonic realist research programs are necessarily incommensurable or mutually exclusive. Bruce Bueno de Mesquita and David Lalman, and Kelly M. Kadera, each try to integrate balance of power theory and power transition theory and to specify the conditions under which the propositions of each are valid. Bruce Bueno de Mesquita and David Lalman, *War and Reason* (New Haven, Conn.: Yale University Press, 1992); Kelly M. Kadera, *The Power-Conflict Story: A Dynamic Model of Interstate Rivalry* (Ann Arbor: University of Michigan Press, 2001). Jack Levy argues that most applications of balance of power theory focus, explicitly or implicitly, on the European system and land-based military power, whereas most applications of power transition theory focus on the global system and measure power in terms of seapower, airpower, or wealth, so that the propositions generated by these theories are not necessarily contradictory. Levy, "Balances and Balancing: Concepts, Propositions, and Research Design." Great powers can simultaneously balance against an aspiring European hegemon and align with a dominant global

One important issue in the application of Lakatosian metatheory—and one on which Lakatos provides inadequate guidance—is how inclusively to define a research program. Although it would be possible to treat hegemonic theory as a single program for the purposes of an assessment based on Lakatos's methodology of scientific research programs,<sup>7</sup> it is more useful to focus our attention on power transition theory as developed by Organski and refined by Organski and Kugler, Kugler and Lemke, and their colleagues.<sup>8</sup> Three generations of scholars have explicitly identified themselves with this research program and continue to refine the theory and test it empirically.<sup>9</sup> This has led to a cumulating body of research, which now constitutes a major research program in the field. If Lakatos's methodology of scientific research programs is not useful for evaluating the evolution of this relatively coherent research program, it is not clear where in the international relations field Lakatosian metatheory could be applied.

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power, and this contradicts neither balance of power theory nor power transition theory. This is also an important theme in Karen A. Rasler and William R. Thompson, *The Great Powers and Global Struggle, 1490–1990* (Lexington: University Press of Kentucky, 1994).

7. Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in I. Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (New York: Cambridge University Press, 1970), pp. 91–196. Subsequent citations of Lakatos refer to this chapter; we supply page references in parentheses in the text.

8. Organski, *World Politics*; A.F.K. Organski and Jacek Kugler, *The War Ledger* (Chicago: University of Chicago Press, 1980); Jacek Kugler and Douglas Lemke, eds., *Parity and War: Evaluations and Extensions of "The War Ledger"* (Ann Arbor: University of Michigan Press, 1996); Kugler and Lemke, "The Power Transition Research Program"; Ronald Tammen, Jacek Kugler, Douglas Lemke, Alan Stam III, Mark Abdollahian, Carole Alsharabati, Brian Buford-Efird, and A.F.K. Organski, *Power Transitions* (Chatham, N.J.: Chatham House, 2000).

9. Although we think that self-identification with a line of research is one of several useful criteria for the specification and delimitation of a research program, this criterion may not be perfectly compatible with Lakatosian metatheory. Lakatos aimed to provide objective criteria for the identification and evaluation of research programs. He was less interested in the more subjective criterion of whether a scholar self-consciously identifies with the research program.

We exclude from our analysis a discussion of Gilpin's hegemonic transition theory, Modelski and Thompson's leadership long cycle theory, and Doran's theory of relative power cycles. Although Gilpin's hegemonic transition theory is a theoretically rich and important contribution, and although it shares with Organski's power transition theory many of the same assumptions and arguments, Gilpin does not identify himself with Organski's power transition theory.<sup>10</sup> Moreover, the paucity of subsequent efforts to test Gilpin's theory empirically<sup>11</sup> makes it difficult to apply Lakatosian metatheory, which emphasizes the empirical corroboration of novel facts.<sup>12</sup> Long cycle theory's exclusively systemic orientation (in contrast to Organski's combination of systemic and dyadic elements), its explicit assumption of cycles, and its focus on seapower and leading economic sectors make this line of research sufficiently distinct that it is best examined as a self-contained research program.<sup>13</sup> Finally, although Doran's power cycle theory shares common elements with power transition theory, we exclude it because it includes some nonrationalist elements that are at odds with the rationalist hard core of the power transition research program.<sup>14</sup>

In the pages that follow, we apply Lakatos's methodology of scientific research programs in an effort to evaluate the power transition research

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10. Gilpin, *War and Change in World Politics*, p. 94, note 11, cites Organski only once.

11. An exception is K. Edward Spiezio, "British Hegemony and Major Power War, 1815–1939: An Empirical Test of Gilpin's Model of Hegemonic Governance," *International Studies Quarterly*, Vol. 34, No. 2 (June 1990), pp. 165–181.

12. We also exclude "hegemonic stability theory," which seeks to explain stability in the international political economy rather than war. Robert O. Keohane, *After Hegemony: Cooperation and Discord in the World Political Economy* (Princeton, N.J.: Princeton University Press, 1984).

13. William R. Thompson, *On Global War: Historical-Structural Approaches to World Politics* (Columbia, S.C.: University of South Carolina Press, 1988); Rasler and Thompson, *The Great Powers and Global Struggle*.

14. Charles F. Doran and Wes Parsons, "War and the Cycle of Relative Power," *American Political Science Review*, Vol. 74, No. 4 (December 1980), pp. 947–965; Charles F. Doran, "Confronting the Principles of the Power Cycle: Changing

program. First, we describe the theoretical foundations of the research program based on its essential texts. Then we reconstruct the program in Lakatosian terms, identifying its hard core assumptions, its negative heuristic, and its positive heuristic. We then analyze recent developments in power transition research, with particular attention to Douglas Lemke's multiple hierarchy model, Woosang Kim's alliance transition model, and research regarding the timing and initiation of wars associated with power transitions, and we assess whether each of these strands in the research program is progressive or degenerating. We end by reflecting on the preceding analytical exercise, evaluating the utility of Lakatosian metatheory for international relations and for the social sciences in general. We conclude that Lakatos's methodology is useful, but that its application presents some difficult challenges for social scientists. We attribute much of the difficulty to Lakatos's insufficient attention to operational tasks such as identifying the boundaries of a research program, specifying the research program's hard core and whether it evolves over time, evaluating research programs that simultaneously exhibit signs of progress and degeneration, weighing the relative importance of the construction of valid empirical indicators or of multiple corroborations of a hypothesis, and deciding how long to tolerate a struggling research program.

### *The Foundations of the Research Program: World Politics and The War Ledger*

The most significant scholarly contribution of A.F.K. Organski's 1958 book *World Politics* was his critique of balance of power theories and his outline of power transition theory as an alternative explanation for the dynamics of international politics and the onset of major war. Organski rejected the argument that an equal balance or distribution of capabilities between adversaries contributes to peace, and argued that balances are more likely

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Systems Structure, Expectations, and War," in Midlarsky, *Handbook of War Studies II*, pp. 83–110.

to lead to war.<sup>15</sup> He also argued that there is usually a dominant power that sits atop the international hierarchy, positioned above several lesser great powers, other medium and smaller states, and dependencies. The dominant power shapes the "international order" in which relations among states are stable and follow certain patterns and even rules of behavior promoted by the dominant power.<sup>16</sup> Finally, Organski criticized the excessively static character of balance of power theory and its failure to incorporate the changing nature of state power and its implications for the international system. He argued that uneven patterns of growth due to industrialization lead not only to the emergence of a dominant power in the international arena, but also to subsequent challenges to the dominant state's global leadership by great powers undergoing dramatic internal development.

The dominant power achieves its preeminent position in the international hierarchy through a process of rapid economic development that is driven by industrialization. As the boost from industrialization wanes in the dominant state, other contending states industrialize, grow rapidly, and catch up, so that the new distribution of power is no longer commensurate with the existing international order. If a rising power is dissatisfied with its own place in the international hierarchy, it may wish to challenge the existing international order, perhaps using its newly developed military power. Thus the probability of war between the rising challenger and the dominant state peaks near the point of power transition between them. This contrasts with the power parity hypothesis that an equality of power is conducive to peace.

Power transition theory thus incorporates two ideas that have become central in later theories of hegemonic change and war: the importance of changing power distributions in the international system arising from industrialization, and the stabilizing effects of concentrations of power. The theory is centered around two key explanatory variables: relative power, and the degree of satisfaction with the international order (or

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15. Organski, like many others, often fails to distinguish between dyadic and systemic-level balances or preponderance. The dyadic-level power parity hypothesis is not equivalent to systemic-level balance of power theory.

16. Organski, *World Politics*, pp. 313–16, 326–30.

status quo). The interaction effect between them is the primary determinant of war and peace. States that have insufficient capabilities, no matter how dissatisfied with the status quo, will be fundamentally unable to challenge the dominant power. States that are powerful but satisfied will have little motivation to challenge the dominant state for its preeminent position and for the accompanying ability to shape the international order. Only the powerful and dissatisfied pose a threat.<sup>17</sup>

Organski and Kugler published the first statistical tests of power transition hypotheses in their 1980 book, *The War Ledger*, which focused on the hypothesis that the combination of parity and transition is conducive to major war. Organski and Kugler found that among those states capable of contending for global leadership, no wars take place without a power transition, and that half of the observed transitions were followed by the outbreak of war.<sup>18</sup> Based on these findings, the authors claimed that a power transition among contenders is a necessary but not sufficient condition for major war.<sup>19</sup> Although critics have questioned various aspects of *The War Ledger's* research design, the book stands as the foundation for the empirical development of the power transition research program.<sup>20</sup>

17. Organski, *World Politics*, chap. 12; Organski and Kugler, *The War Ledger*, pp. 19–23, 39; Douglas Lemke, “Toward a General Understanding of Parity and War,” *Conflict Management and Peace Science*, Vol. 14, No. 2 (Winter 1995), p. 145; Douglas Lemke and Jacek Kugler, “The Evolution of the Power Transition Perspective,” in Kugler and Lemke, *Parity and War*, p. 21. Recent treatments use Most and Starr’s language, suggesting that parity provides the *opportunity* for war, and that dissatisfaction with the status quo provides the *willingness* to engage in war with the dominant state. Benjamin A. Most and Harvey Starr, *Inquiry, Logic, and International Politics* (Columbia: University of South Carolina Press, 1989).

18. The findings hinge on the separation of “contenders” from other major powers in the system. Contenders include the dominant state and those states possessing at least 80 percent of the capabilities of the dominant state. If the dominant state is grossly preponderant, the three most powerful states are classified as contenders. Organski and Kugler, *The War Ledger*, pp. 42–45.

19. Organski and Kugler, *The War Ledger*, pp. 50–52; Kugler and Organski, “The Power Transition,” p. 179.

20. For recent critiques see John A. Vasquez, “When are Power Transitions Dangerous? An Appraisal and Reformulation of Power Transition Theory,” in

### *A Lakatosian Reconstruction of the Power Transition Research Program*

In this section, we reconstruct the power transition research program in Lakatos’s terms, identifying the hard core and the negative and positive heuristics.

#### POWER TRANSITION’S HARD CORE OF IRREFUTABLE ASSUMPTIONS

The central concept of Lakatos’s methodology of scientific research programs is the “hard core,” which is a set of assumptions considered “‘irrefutable’ by the methodological decision of its protagonists” and not appropriate for empirical testing (p. 133).<sup>21</sup> Researchers use these assumptions to construct a theoretical system, derive “auxiliary hypotheses” that comprise the “protective belt” around the hard core of the research program, and test those hypotheses empirically. “It is this protective belt of auxiliary hypotheses which has to bear the brunt of tests and get adjusted and re-adjusted” (p. 133). A major contribution of Lakatosian metatheory is to provide criteria for assessing whether the addition of auxiliary hypotheses is “progressive” and enhances scientific knowledge, or instead is degenerating.<sup>22</sup>

Kugler and Lemke, *Parity and War*, pp. 35–56; Randolph M. Siverson and Ross A. Miller, “The Power Transition: Problems and Prospects,” in Kugler and Lemke, *Parity and War*, pp. 57–76.

21. Thus Lakatosian metatheory provides a justification for Friedman’s famous “as if” assumption. Milton Friedman, “The Methodology of Positive Economics,” in Milton Friedman, ed., *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953), pp. 3–43.

22. To be progressive, a theoretical development cannot be ad hoc in either of the following respects: if a problemshift fails to yield predictions of novel facts (“ad hoc<sub>1</sub>”) or if it predicts novel facts but those novel facts are not empirically corroborated (“ad hoc<sub>2</sub>”). Theories that are ad hoc in these senses merely patch up holes in existing theories without providing new theoretical or empirical content. However, it is not perfectly clear what Lakatos (pp. 116–132) means by a “novel fact.” For a summary of different interpretations see Elman and Elman’s Chapter 2 in this volume. We prefer the concept of “heuristic novelty,” which requires that a theory must account for facts that were not used in the construction of the theory.

Although Organski's original statement of power transition theory does not contain an explicit list of assumptions that allows us to specify an unambiguous hard core of the research program, his critique of the assumptions of balance of power theory gives us some leverage for that task. Organski charges balance of power theorists with making two misguided assumptions: "nations are fundamentally static units whose power is not changed from within, and . . . nations have no permanent ties to each other but move about freely, motivated primarily by considerations of power."<sup>23</sup> Organski emphasizes repeatedly that the first assumption fails to hold for the period since 1750. Rather, he argues, the impulses of nationalism and industrialization have transformed international politics such that changes in national power from within drive changes in the relations among states. Internal growth and development has supplanted the constant shifting of alliances as the primary mechanism for reconfiguring international political relationships.<sup>24</sup>

Organski also criticizes the emphasis of balance of power theory on alliance formation and dissolution as the primary mechanism for power redistribution, and on the ease of making and breaking alliances.<sup>25</sup> He

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This reflects the common methodological injunction that a theory ought not be tested on the same data that were used to construct the theory. As Elman and Elman (Chapter 2, this volume) aptly note, however, this standard can be quite demanding, because in principle it requires information about how the theory was developed. A problemshift is ad hoc in a third sense ("ad hoc<sub>3</sub>") if it is not in accord with the positive heuristic, in which case it marks a break from the existing research program.

23. Organski uses the term "nations," but plainly he was referring to states. Organski, *World Politics*, p. 287. Note that Organski's critique of the *assumptions* of balance of power theory is inappropriate from the perspective of Lakatosian metatheory, which directs us toward the protective belt. Note too that Waltz makes this critique of Organski, although in non-Lakatosian language. Kenneth N. Waltz, *Theory of International Politics* (Reading, Mass.: Addison-Wesley, 1979), p. 119.

24. Organski, *World Politics*, pp. 287–90, 306–309, 337–38; Organski and Kugler, *The War Ledger*, pp. 24–27; Kugler and Organski, "The Power Transition," p. 173; Lemke and Kugler, "The Evolution of the Power Transition Perspective," pp. 5–10.

25. Organski, *World Politics*, chap. 11.

argues that ties among states in the industrializing period are far less flexible than during the preindustrial era, for three reasons. First, industrialization and the development of a more liberal, free-trade order increased the interdependence of nations, making ties firmer. Second, alliance ties in the modern era require heavy investments—including arms transfers, building and maintenance of bases abroad, and equipment standardization—and consequently alliances are less transitory. Third, the growth of democracy and leaders' appeals to constituents for support of their alignment policies makes it much harder for democratic states to reverse alliance commitments. Economically interdependent, militarily tied, and sentimentally bound nations cannot "switch sides" as easily as the dynastic states of the sixteenth, seventeenth, and early eighteenth centuries, and consequently alliances are not a primary means of enhancing national power.<sup>26</sup>

This discussion, along with more explicit statements in subsequent work,<sup>27</sup> suggests the following set of hard core (HC) assumptions in power transition theory:

(HC-1): states are the primary actors in international politics.<sup>28</sup>

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26. Organski, *World Politics*, pp. 313–316. Organski implies that the satisfaction of both the dominant state and some other great powers with the status quo may add to the inflexibility of their alliance ties, which reinforce their mutual interests and power.

27. Organski and Kugler, *The War Ledger*, chap. 1; Kugler and Organski, "The Power Transition," pp. 172–175; Tammen, et al., *Power Transitions*.

28. One could imagine the extension of power transition theory to the relations among communal groups within or across state boundaries. In fact, analyses of ethnic conflict that emphasize the impact on the likelihood of conflict or cooperation of changes in the relative power of communal groups, whether driven by demographic change or by other variables, have some striking parallels with power transition theory. See James Fearon, "Commitment Problems and the Spread of Ethnic Conflict," in David A. Lake and Donald Rothchild, eds., *The International Spread of Ethnic Conflict* (Princeton, N.J.: Princeton University Press, 1998), pp. 107–126. The extension of realist theory from states to other actors has an important precedent in the application of the concept of the security dilemma between states to the relations among ethnic groups. Barry R. Posen, "The Security Dilemma and Ethnic Conflict," in Michael E. Brown, ed., *Ethnic Conflict and*

(HC-2): state leaders are rational in their foreign policy choices.<sup>29</sup>

(HC-3): the international order is hierarchically organized under the leadership of a dominant power.

(HC-4): the rules governing the international political system are fundamentally similar to those governing domestic political systems.

(HC-5): internal growth and development of states is the primary source of international change.

(HC-6): alliance ties among states are relatively inflexible, and consequently alliances are not a primary means of enhancing national power.

HC-6 is the most problematic in terms of the identification of the hard core.<sup>30</sup> We could conceivably treat the diminished role of alliances not as a hard core assumption, but rather as a testable hypothesis, and thus as part of the positive heuristic and protective belt of the power transition research program. Although we think that this is how the alliance question *ought* to be treated, this is not how power transition scholars have treated alliances, as there has been virtually no effort to examine empirically the relative impact of alliance ties and internal growth on states' position and influence in the system. The one exception, as we will see, is Kim's work on alliance transition theory, but it is significant that Kim's research thus far has not led other power transition researchers to

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*International Security* (Princeton, N.J.: Princeton University Press, 1993), pp. 103–124. Whether such extensions of realist theory in general or power transition theory in particular would constitute intra-program shifts or inter-program shifts raises some difficult issues.

29. The rationality assumption is clear in Organski and Kugler, *The War Ledger*, pp. 39–40; and Kugler and Organski, "The Power Transition," pp. 172–173; and particularly explicit in Tammen et al., *Power Transitions*.

30. Lakatos, "Falsification and the Methodology of Scientific Research Programmes," provides very little guidance on the specific composition of the hard core.

incorporate alliances into their models.<sup>31</sup> This leads us to treat the assumption of the inflexibility of alliances as part of the hard core of the power transition research program, and Kim's work as a break from the hard core.<sup>32</sup>

It is instructive to compare power transition theory's hard core assumptions with those of realist balance of power theories. Although both assume that the key actors in the system are unitary and rational states, they differ in other important respects. Whereas balance of power theories treat both internal growth and alliances as sources of international change, power transition theory excludes alliances and treats internal growth as the only source of power and international change. The peripheral role of alliances in power transition theory is a major point of difference with balance of power realism, where alliances play an indispensable role. In addition, power transition theory is much more explicit about the role of changing power than are balance of power theories.

In contrast to the standard neorealist assumption that anarchy is the key ordering principle of international relations,<sup>33</sup> power transition theory posits a hierarchically organized international order defined both by the

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31. Woosang Kim, "Power, Alliance, and Major Wars, 1816–1975," *Journal of Conflict Resolution*, Vol. 33, No. 2 (June 1989), pp. 255–273; Woosang Kim, "Alliance Transitions and Great Power War," *American Journal of Political Science*, Vol. 35, No. 4 (1991), pp. 833–50; Woosang Kim, "Power Transitions and Great Power War from Westphalia to Waterloo," *World Politics*, Vol. 45, No. 1 (October 1992), pp. 153–172; Woosang Kim, "Power Parity, Alliance, and War from 1648 to 1975," in Kugler and Lemke, *Parity and War*, pp. 93–106.

32. Admittedly, Organski (*World Politics*, pp. 331–332) occasionally argued that the power parity of international coalitions, or "teams," ought to be associated with a greater danger of major war. But Organski did not sustain this argument, and later power transition theorists eliminated it from their models.

33. Waltz, *Theory of International Politics*; Robert O. Keohane, "Theory of World Politics: Structural Realism and Beyond," in Ada W. Finifter, ed., *Political Science: The State of the Discipline* (Washington, D.C.: American Political Science Association, 1983), pp. 503–540; Helen Milner, "The Assumption of Anarchy in International Relations Theory: A Critique," *Review of International Studies*, Vol. 17, No. 1 (January 1991), pp. 67–85; Barry Buzan, Charles Jones, and Richard Little, *The Logic of Anarchy* (New York: Columbia University Press, 1993).

distribution of power and by the set of rules and common practices imposed by a dominant state. In some respects this distinction is rather thin and reflects merely semantic differences in the meanings that neorealists and power transition theorists attach to the key concepts of anarchy, hierarchy, and authority. Waltz concedes that international politics is characterized by some semblance of order, and power transition and other hegemonic theorists concede that order exists within a nominally anarchic system.<sup>34</sup>

For Waltz, however, order is a systemic effect, not a national strategy. It is a by-product of the "coaction of self-regarding units [i.e., states].... No state intends to participate in the formation of a structure by which it and others will be constrained. International-political systems, like economic markets, are individualist in origin, spontaneously generated, and unintended."<sup>35</sup> In power transition theory, by contrast, order is the intended result of actions taken by a dominant state, which attempts to shape the international order in such ways that advance stability and enhance its own interests.<sup>36</sup> In balance of power theory, a single dominant state almost never arises because the balancing mechanism works to deter potential hegemonies or to defeat them if deterrence fails.<sup>37</sup>

In contrast with the Waltzian assumption that states are functionally undifferentiated and have similar goals, Organski argues that because states occupy different positions in the international hierarchy, they may have different goals.<sup>38</sup> Moreover, in contrast to the view often associated with classical realists such as Morgenthau and contemporary "offensive realists" such as Mearsheimer, Organski rejects the argument that all national goals reduce to the maximization of power (though he concedes that every state needs to maintain some minimum level of power to

34. Waltz, *Theory of International Politics*; Lemke and Kugler, "The Evolution of the Power Transition Perspective," p. 21.

35. Waltz, *Theory of International Politics*, p. 91; Robert Jervis, *System Effects* (Princeton, N.J.: Princeton University Press, 1997), p. 132.

36. Organski, *World Politics*, p. 326; Kugler and Lemke, "The Power Transition Research Program."

37. Levy, "Balances and Balancing: Concepts, Propositions, and Research Design."

38. Organski, *World Politics*, pp. 53–57.

survive as a political entity).<sup>39</sup> The assumption of heterogeneous state goals is consistent with Organski's argument that some but not all potential challengers may be satisfied with the existing international order and have no incentive to overturn the hierarchy even if they have the power to do so.<sup>40</sup>

The anarchy/hierarchy distinction is closely related to the question of the similarity of international and domestic political systems. Power transition theory's hard core assumes that the hierarchically-organized international order contains rules similar to those of domestic political systems, "despite the absence of an enforceable code of international law."<sup>41</sup> This breaks from the explicit neorealist assumption that international politics and domestic politics are fundamentally dissimilar because the former is anarchic and the latter is hierarchical.<sup>42</sup> For these reasons we treat the power transition research program as a break with the hard core of balance of power realism.<sup>43</sup>

39. Morgenthau, *Politics Among Nations* (New York: Knopf, 1948); and John J. Mearsheimer, *The Tragedy of Great Power Politics* (New York: Norton, 2001).

40. The dominant power often makes a deliberate effort to coopt some potential challengers and win their acceptance of the existing order, often through the construction of institutions that both reinforce the existing order and impose some limits on the leading state. See G. John Ikenberry, *After Victory: Institutions, Strategic Restraint, and the Rebuilding of Order after Major Wars* (Princeton, N.J.: Princeton University Press, 2001).

41. Kugler and Organski, "The Power Transition," p. 172; Lemke and Kugler, "The Evolution of the Power Transition Perspective," p. 8. Gilpin asserts that interstate relationships are ordered within an anarchic international system, and that while domestic and international politics are dissimilar, they share commonalities in their control mechanisms. Gilpin, *War and Change in World Politics*, p. 28.

42. The assumption that international and domestic politics are fundamentally different goes back to Rousseau, which leads Walker to treat Rousseau as the first modern realist. Thomas C. Walker, "Peace, Rivalry, and War" (Ph.D. dissertation, Rutgers University, 2000).

43. Similarly, Keohane argues that Gilpin's hegemonic transition theory represents a break from the hard core of classical realism. Gilpin, *War and Change in World Politics*; Keohane, "Theory of World Politics: Structural Realism and Beyond," pp. 517–520.

## POWER TRANSITION'S NEGATIVE HEURISTIC

Lakatos's "negative heuristic" delineates the types of variables and models that ought to be shunned by researchers within a research program because they deviate from the assumptions of the hard core.<sup>44</sup> Power transition's hard core implies that researchers should not develop models that posit the importance of non-state actors, non-rational decision-making, the absence of order or rules in the international system, a sharp distinction between domestic politics and international politics, a static conception of national power, or the significance of alliances as sources of national power. In addition, Organski implies that researchers should avoid explanations that posit homogeneous motivations (including power maximization) across states.

## POWER TRANSITION'S POSITIVE HEURISTIC

Lakatos argued that programmatic research is further guided by the positive heuristic, "a partially articulated set of suggestions or hints" regarding the development of increasingly sophisticated models (pp. 134–138). These models generate hypotheses that comprise the protective belt and that should be empirically tested. Lakatos suggested that pioneers of particular research programs anticipate future refutations of some hypotheses derived from the initial model. Although incapable of refining the model at that moment, the researcher should speculate on the types of emendations and changes that would prepare the research program to handle likely refutations and anomalies.

Recognizing that the theory of the power transition would evolve over time, Organski acknowledged that his book, *World Politics*,

contains few "laws" but a great many generalizations and hypotheses which are the first step in the formation of theory. Some of the generalizations are crude and need refinement. Some of the hypotheses are probably downright wrong. The reader is invited to refine and correct wherever he can, for only by

44. This suggests that the negative heuristic is redundant because it follows directly from the hard core and provides no additional information.

such steps does knowledge grow. Beginnings must be big and breezy; refinements follow later.<sup>45</sup>

Organski cautioned that power transition theory is not timeless but instead is limited to the period since the Industrial Revolution, stating that, "differential industrialization is the key to understanding the shifts in power in the 19th and 20th centuries, but it was not the key in the years before 1750 or so, and it will not always be the key in the future." Once all states are fully industrialized, he wrote, we will "require new theories."<sup>46</sup>

Organski also provided a detailed discussion of the measurement of national power, which, he argued, comprises six components (ranked in decreasing order of importance): population size, efficacy of political structure, economic development, national morale, resources, and geography. For measurement purposes, Organski collapsed the last two together with population size and economic development, arguing that highly developed and heavily populated states tend to enjoy adequate access to resources and favorable geographic circumstances. He also omitted national morale, which is "virtually impossible to measure objectively," and suggested national income (effectively, gross national product [GNP]) as a quantifiable indicator summarizing population size and economic development.<sup>47</sup>

State political capacity is a key component of national power that was articulated in the formative statement of the power transition research program as part of the positive heuristic. Organski conceded that a good measure of the effectiveness of political institutions had yet to be developed, and he argued that creation of such a measure would be "one

45. Organski, *World Politics*, p. 6.

46. Organski, *World Politics*, p. 307. Organski also suggested that all theories are bound by culture and experience, and that theories appropriate to one context are not always applicable to another context. Accordingly, theories require revision, and "one of the most serious criticisms that can be made of the balance of power theory is that it has not been so revised." *Ibid.*, p. 307.

47. *Ibid.*, chap. 8, esp. pp. 203–210; Organski and Kugler, *The War Ledger*, pp. 33–38.

of the major tasks that remains for political scientists to accomplish in the years ahead."<sup>48</sup>

The other key variable in power transition theory, but one that until recently has received less attention than national power, is the degree of satisfaction with the international order or existing status quo. Organski argued that "peaceful adjustment is possible in the case of the challenger who is willing to continue the existing international order and abide by its rules, but is much more difficult, if not impossible, in the case of a challenger who wishes to destroy the existing order."<sup>49</sup> Organski never fully developed the concept or a measure for the degree of satisfaction with the status quo, but clearly the conceptualization and operationalization of this variable is a key element in the positive heuristic of the power transition research program.

Finally, Organski identified four other factors affecting the likelihood that a power transition will result in war. First, the challenger's power potential when beginning its ascent: if a rising state is too small ever to challenge the dominant state, or "so large that its dominance, once it becomes industrial, is virtually guaranteed," war becomes very unlikely.<sup>50</sup> The second factor is the speed of the challenger's rise. The more rapid the challenger's ascent, the greater is the probability of war, for several reasons. The leaders of the dominant state have trouble adjusting to rapid changes; the leaders of the challenger have trouble adapting to a new role in the international order; and a rapid rise "may go to the challenger's head," leaving leaders impatient with the unresponsiveness of the international order to the changing distribution of power.<sup>51</sup> Political

48. Organski, *World Politics*, p. 203. For an initial effort to measure political capacity, see Organski and Kugler, *The War Ledger*, chap. 2.

49. *Ibid.*, pp. 325–337.

50. *Ibid.*, pp. 334–337.

51. *Ibid.*, p. 335. This explanation of the effects of a rapid rise of the challenger incorporates certain non-rational psychological processes (see also Doran and Parsons, "War and the Cycle of Relative Power"), and as such it is not fully consistent with the rationalist assumption of the hard core of the research program. Subsequent power transition researchers have incorporated this variable into a rationalist model. Mark A. Abdollahian, "In Search of Structure: The Nonlinear Dynamics of International Politics" (Ph.D. dissertation, Claremont

leaders, seeking to promote extraordinarily rapid growth, might make excessive demands on the populace, which could lead to internal strain and possibly create incentives for the diversionary use of force.<sup>52</sup>

The third factor is the dominant state's flexibility in adjusting to changes in the distribution of power. Especially in conjunction with the rise of a challenger so large as to be assured of dominance in the long run, the ability of the now-dominant state to accommodate the rising challenger through moderate concessions could mitigate the likelihood of war. This is related to the fourth factor, the degree of amity between the dominant power and the challenger. The absence of hostility between the dominant state and the challenger, which may be a function of the similarity of economic or domestic political systems,<sup>53</sup> may reduce the probability of war associated with transitions.<sup>54</sup>

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Graduate School, 1996). In any case, propositions about the speed of the challenger's rise have not been central to the research program.

52. In contrast to Organski's argument, Kim and Morrow argue that it is equally plausible that the challenger's leaders will be pessimistic about the ability to sustain extraordinarily rapid growth and will underestimate, not overestimate, their state's eventual position in the international order. Woosang Kim and James D. Morrow, "When Do Power Shifts Lead to War?" *American Journal of Political Science*, Vol. 36, No. 4 (November 1992), pp. 896–922.

53. Organski implied but did not explicitly state that similar domestic institutions facilitate interstate "friendship," which partially anticipates the interdemocratic peace proposition. Organski, *World Politics*, pp. 324, 336–337. Others argue that satisfaction with the status quo could have the same result, and in fact Lemke and Reed argue that power transition theory subsumes the interdemocratic peace. Douglas Lemke and William Reed, "Regime Types and Status Quo Evaluations: Power Transition Theory and the Democratic Peace," *International Interactions*, Vol. 22, No. 2 (1996), pp. 143–164.

54. With the exception of the rapidity of the challenger's rise, Organski explicitly linked these conditions to the peaceful transition between Great Britain and the United States, which he acknowledged as the "one major exception" to the proposed relationship between the rise of a challenger and major war. He discussed a number of possible explanations, but emphasized that "the major reason why England has allowed the United States to take her place without a struggle is because the United States has accepted the Anglo-French international order"—in other words, American satisfaction with the status quo. Organski, *World Politics*, pp. 323–325.

This gives us a characterization of power transition's positive heuristic:

(PH-1): Construct models explaining major war onset during the industrializing era using the interaction of power transitions and degree of satisfaction with the status quo.

(PH-2): Construct quantitative indicators of national power that reflect the intrastate sources of interstate dynamics.

(PH-3): Develop a conceptual and operational definition of political capacity.

(PH-4): Develop a conceptual and operational definition of degree of satisfaction with the status quo.

(PH-5): Where the combination of relative power and degree of satisfaction with the status quo fail to explain the violent or peaceful character of power transitions, incorporate mitigating factors such as the challenger's potential, the speed of the challenger's rise, the dominant power's flexibility, and friendly relations between the dominant power and the challenger.<sup>55</sup>

Three examples from *The War Ledger*, each developed in later work, illustrate how the positive heuristic, rooted in Organski's original formulation of power transition theory, has guided subsequent inquiry. First, Organski and Kugler provided a lengthy discussion of the research design tasks necessary for testing the power preponderance hypothesis (contained in PH-1 and PH-2).<sup>56</sup> Second, they began to develop a quantitative index to measure the effectiveness of political institutions or political capacity (PH-3).<sup>57</sup> Third, Organski and Kugler test a statistical

55. The concepts of "friendly relations" and especially "flexibility" are quite vague, and unless they are rigorously defined and operationalized independently of the predicted behavior, they open the way for the introduction of an element of non-falsifiability into power transition hypotheses. In practice, however, power transition theorists have carefully avoided this trap.

56. Organski and Kugler, *The War Ledger*, pp. 33–38.

57. Organski and Kugler do not incorporate their relative political capacity index into the tests of power transition hypotheses on the grounds that the major powers included in the tests are politically developed enough to be roughly on the same

model that incorporates both relative power and the speed of the power transition, in order to explain the likelihood of a peaceful transition (PH-5).<sup>58</sup>

The impact of the positive heuristic is also clear in the development or employment of new measures for key variables. Following Organski, there has been considerable debate, both within and beyond the power transition research program, over the measurement of national power and degree of satisfaction with the status quo.<sup>59</sup> After comparing the Singer-Bremer-Stuckey measure of national capabilities and GNP, Organski and Kugler settled on GNP as a parsimonious indicator of political and economic power for testing power transition theory.<sup>60</sup> Houweling and Siccama, and Lemke and Werner, test power transition hypotheses with Doran and Parsons's relative capabilities index.<sup>61</sup> Recent replications and tests replace GNP with gross domestic product (GDP), and include the Correlates of War composite index of national capabilities as an

plane. This rationale raises questions about the utility of the political capacity variable in a theory that is explicitly limited to great power behavior.

58. Organski and Kugler, *The War Ledger*, p. 56. Power transition theorists have recently directed more attention to the speed of transitions. Abdollahian, for example, takes issue with Organski's argument that the faster the transition, the higher the probability of war (PH-5). Abdollahian, "In Search of Structure," pp. 64, 88. Unlike Organski, who suggested that rapid change is dangerous because it disrupts role-perception and generates impatience, or because rapid development might incur domestic unrest, Abdollahian argues that faster transitions are less likely to result in war because the window of opportunity opens and closes more quickly. Slow, prolonged transitions then generate a higher probability of war than do rapid transitions.

59. Organski, *World Politics*, chaps. 5–8, esp. pp. 201–210.

60. J. David Singer, Stuart A. Bremer, and John Stuckey, "Capability Distribution, Uncertainty, and Major Power War, 1820–1965," in Bruce M. Russett, ed., *Peace, War, and Numbers* (Beverly Hills, Calif.: Sage, 1972), pp. 19–48; Organski and Kugler, *The War Ledger*, chap. 1.

61. Henk Houweling and Jan G. Siccama, "Power Transitions as a Cause of War," *Journal of Conflict Resolution*, Vol. 32, No. 1 (March 1988), pp. 87–102; and Douglas Lemke and Suzanne Werner, "Power Parity, Commitment to Change, and War," *International Studies Quarterly*, Vol. 40, No. 2 (June 1996), pp. 235–260; Doran and Parsons, "War and the Cycle of Relative Power."

alternative indicator.<sup>62</sup> Such tests typically demonstrate the robustness of the association between parity and war among the most powerful states in the system over the last two centuries.<sup>63</sup> More recent improvements and applications of a reliable measure of political capacity are summarized in Kugler and Arbetman's *Political Capacity and Economic Behavior*.<sup>64</sup>

Power transition theorists have also made a number of efforts to operationalize the concept of the degree of satisfaction with the status quo and to incorporate it into their models.<sup>65</sup> Kim, for example, operationalizes

62. Indra deSoysa, John R. Oneal, and Yong-hee Park, "Testing Power-Transition Theory Using Alternative Measures of National Capabilities," *Journal of Conflict Resolution*, Vol. 41, No. 4 (August 1997), pp. 509–528; Lemke and Werner, "Power Parity, Commitment to Change, and War."

63. See the findings in, among others, Organski and Kugler, *The War Ledger*; Houweling and Siccama, "Power Transitions as a Cause of War"; Doran and Parsons, "War and the Cycle of Relative Power"; deSoysa et al., "Testing Power-Transition Theory Using Alternative Measures of National Capabilities"; Lemke and Werner, "Power Parity, Commitment to Change, and War." For more complete summaries of the empirical support for power transition theory see Lemke and Kugler, "The Evolution of the Power Transition Perspective," pp. 3–33; and Kugler and Lemke, "The Power Transition Research Program."

64. Jacek Kugler and Marina Arbetman, eds., *Political Capacity and Economic Behavior* (Boulder, Colo.: Westview Press, 1997), building on Jacek Kugler and William Domke, "Comparing the Strength of Nations," *Comparative Political Studies*, Vol. 19, No. 1 (April 1986), pp. 39–69; and Jacek Kugler and Marina Arbetman, "Choosing Among Measures of Power," in Richard J. Stoll and Michael D. Ward, eds., *Power in World Politics* (Boulder, Colo.: Lynne Rienner, 1989), pp. 49–77.

65. These include Werner's and Kugler's indicator of extraordinary military buildups, Kim's alliance profile similarity measure (based on Bruce Bueno de Mesquita, *The War Trap* [New Haven, Conn.: Yale University Press, 1981]), Bueno de Mesquita's money market discount rate measure, and Lemke's and Reed's assessment of the similarity of domestic structures. Suzanne Werner and Jacek Kugler, "Power Transitions and Military Buildups: Resolving the Relationship Between Arms Buildups and War," in Kugler and Lemke, *Parity and War*, pp. 187–210; Kim, "Alliance Transitions and Great Power War," esp. p. 843, note 14; Bruce Bueno de Mesquita, "Pride of Place: The Origins of German Hegemony," *World Politics*, Vol. 43, No. 1 (October 1990), pp. 28–52; Lemke and Reed, "Regime Types and Status Quo Evaluations." For a helpful survey of indicators of status quo evaluations, see Douglas Lemke, *Regions of War and Peace* (Cambridge, UK:

satisfaction in terms of the similarity of the alliance portfolios of the state with that of the dominant power.<sup>66</sup> Although Kim finds little empirical support for the impact of dissatisfaction, others have subsequently used his measure in tests of traditional power transition hypotheses, and this has sparked further debate and concerted efforts to generate a better indicator of degree of satisfaction. Still, no single indicator has gained overwhelming scholarly support. Unsuccessful efforts to improve the measurement of satisfaction have not obstructed the advancement of the power transition research program, but neither have they helped move the program forward.

Some critics might argue that in their haste to construct indicators measuring status quo satisfaction, power transition researchers have neglected several conceptual issues concerning the nature of the status quo. What, exactly, is the status quo, and through what mechanisms does a rising challenger's dissatisfaction with the status quo lead to an increase in the likelihood of a violent confrontation? As Oneal, deSoysa, and Park argue, power transition theorists need to specify exactly "what benefits the international system provides to states and over which they may fight."<sup>67</sup> In the absence of such conceptual refinement, power transition theorists cannot convincingly identify dissatisfied states or demonstrate that the dominant power has constructed an international order that gives it a disproportionate advantage.

There is also a levels-of-analysis question. The status quo can refer to the distribution of benefits in the international system, but also might refer to dyadic or even regional structures or relationships.<sup>68</sup> Not only do

Cambridge University Press, 2002), pp. 99–109; for a comparative evaluation of several indicators, see Douglas M. Gibler, "An Assessment of the Validity of Empirical Measures of State Satisfaction with the Status Quo" (unpublished manuscript, University of Kentucky, 2002).

66. Kim, "Alliance Transitions and Great Power War"; Kim, "Power Transitions and Great Power War from Westphalia to Waterloo."

67. John Oneal, Indra deSoysa, and Yong-hee Park, "But Power and Wealth are Satisfying: A Reply to Lemke and Reed," *Journal of Conflict Resolution*, Vol. 42, No. 4 (August 1998), p. 518.

68. On dyadic relationships, see Zeev Maoz and Ben D. Mor, "Satisfaction, Capabilities, and the Evolution of Enduring Rivalries, 1816–1990: A Statistical

these analytically distinct categories require different indicators; they also require separate conceptualization. The nature of a distribution of benefits at the level of the international system is hardly equivalent to the nature of the benefits at stake between two states in a dyad, and in fact it is likely that some states will be satisfied with the systemic status quo but dissatisfied with the status quo of a particular bilateral relationship. Greater conceptual refinement of the status quo and states' degree of satisfaction with the status quo is necessary for continued progress within the power transition research program.

### *Subsequent Development of the Power Transition Research Program*

Since Kugler and Organski's 1989 "retrospective and prospective evaluation" of power transition theory, there has been a flurry of activity, both theoretical and empirical.<sup>69</sup> The *Parity and War* anthology, published in 1996, reflects not only new operationalizations of key variables but also extensions of the temporal and spatial domains of power transition theory, continued attempts to merge power transition and other research programs, and formal models of power transition processes.<sup>70</sup> As a result of these developments, the protective belt surrounding power transition's hard core is continually expanding and changing. Although such flux is a normal phenomenon anticipated by Lakatos (p. 137), it makes characterization of the protective belt difficult.

Space limitations prevent us from presenting a comprehensive summary of power transition theory's protective belt, and consequently we focus on three distinct problemshifts: Lemke's multiple hierarchy

Analysis of a Game Theoretic Model," in Frank P. Harvey and Ben D. Mor, eds., *Conflict in World Politics* (New York: St. Martin's Press, 1998), pp. 33–61; on regional patterns, see Lemke and Werner, "Power Parity, Commitment to Change, and War," p. 245, note 11.

69. Kugler and Organski, "The Power Transition"; for reviews see Lemke and Kugler, "The Evolution of the Power Transition Perspective"; Vasquez, "When are Power Transitions Dangerous?"; Siverson and Miller, "The Power Transition"; Kugler and Lemke, "The Power Transition Research Program."

70. Kugler and Lemke, *Parity and War*.

model, Kim's alliance transitions model, and research regarding the timing and initiation of wars associated with power transitions. We also address the lack of attention to the causal mechanisms that lead from power transitions to the outbreak of war, and the role of bargaining in these processes. We present our evaluation of the progress of the power transition research program at the end of this section.

### LEMKE'S MULTIPLE HIERARCHY MODEL

Organski's power transition theory focuses almost exclusively on the dyadic interactions among the dominant state and its potential challengers. Scholars have recently moved beyond Organski's exclusive focus on power transitions at the very top of the international hierarchy and have empirically tested power transition hypotheses on data sets that include minor power dyads as well as major powers. The most important of these efforts in terms of power transition theory is Douglas Lemke's multiple hierarchy model, which extends power transition logic to regional subsystems within the overarching international order.<sup>71</sup>

Lemke argues that Organski's international hierarchy is but one of many hierarchies in the global political arena. Nested within it are a number of regional hierarchies, complete with dominant regional powers and regional status quos, and smaller sub-regional hierarchies as well.<sup>72</sup> A minor power might be satisfied with the global status quo (or unable to challenge the globally dominant state), but may nevertheless challenge a locally dominant power for the ability to reshape the regional order.

By extending the basic logic of power transition theory to regional systems, Lemke's problemshift generalizes the theory in important ways. Whether it is an intra-program or inter-program problemshift is difficult to assess because of the ambiguous status of the multiple hierarchy model

71. Douglas Lemke, "Small States and War: An Expansion of Power Transition Theory," in Kugler and Lemke, *Parity and War*, pp. 77–92; Lemke, *Regions of War and Peace*.

72. Lemke's theoretical justification for identifying regional and sub-regional hierarchies hinges on the power projection capacity of minor powers, which is operationalized in terms of a loss-of-strength gradient. Lemke, *Regions of War and*

with respect to the positive heuristic and because of the ambiguity of Lakatosian metatheory on this issue. We believe that the proper criterion is that the problemshift not be inconsistent with the positive heuristic, which is less demanding than the alternative criterion of being explicitly specified in the positive heuristic. Because Lemke's extension of power transition theory is not inconsistent with the positive heuristic, it is not degenerating by this criterion.<sup>73</sup> We argue that it represents an intra-program problemshift that generalizes the logic of power transition theory and contributes the sixth element to the positive heuristic of the research program:

(PH-6): Construct models that extend the logic of power transition theory to subsystems of states (including dyadic relationships) that are nested within the international order.

Because Lemke's multiple hierarchy model generates hypotheses about the behavior of small states, which were neglected in Organski's original formulation, it clearly yields predictions of novel facts and is consequently not ad hoc.<sub>1</sub> Empirical tests of Lemke's hypotheses in some regional contexts have demonstrated significant support for the multiple hierarchy model. Perhaps most strikingly, Lemke's tests demonstrate that in South American regional hierarchies, parity approximates a necessary condition for minor power war.<sup>74</sup> In addition, applications of the model to

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*Peace*, chap. 4; Lemke, "Toward a General Understanding of Parity and War"; Lemke, "Small States and War."

73. Although Lakatos (pp. 175–176, 182) sometimes suggests that a problemshift that is not fully in accord with the positive heuristic is ad hoc,<sub>2</sub> elsewhere (p. 137) he allows for the positive heuristic to change over time in response to new developments, providing that this change does not violate any hard core assumptions. This suggests that auxiliary hypotheses are ad hoc,<sub>3</sub> only if they are inconsistent with the positive heuristic. This is a weaker condition for the classification of a new development as an intra-program shift, for a new emendation need not be explicitly part of the positive heuristic, just not inconsistent with it. This is Zahar's interpretation of Lakatos, and we accept it. Elie Zahar, "Why Did Einstein's Programme Supersede Lorentz's? (I)," *British Journal for the Philosophy of Science*, Vol. 24 (1973), 101ff.

74. Lemke, "Toward a General Understanding of Parity and War"; Lemke, "Small States and War."

the Middle East and Far East show that conditions of power parity and dissatisfaction with the status quo together markedly increase the probability of war onset in these regions.<sup>75</sup> Thus, many of Lemke's predicted novel facts are empirically corroborated, so the multiple hierarchy problemshift is not ad hoc.<sub>2</sub> Finally, Lemke and Werner show that the multiple hierarchy model is able to "postdict" (or retrospectively predict) the major wars cited in support of the original power transition theory, satisfying the Lakatosian criterion that the new theory explain not only novel facts but also those predicted by the old theory.<sup>76</sup>

Because Lemke's multiple hierarchy model accounts for the existing empirical content of power transition theory and contains excess content that is not inconsistent with the hard core, and because some of this excess empirical content is empirically corroborated, we argue that the multiple hierarchy model constitutes a progressive, intra-program problemshift.

#### KIM'S ALLIANCE TRANSITIONS MODEL

Woosang Kim develops a theory of alliance transitions, which he describes as "revised power transition theory," and tests it over the period since 1648. Kim hypothesizes that alliance parity—a balance of capabilities between opposing alliance coalitions—is associated with an increased probability of major war. His statistical tests show that alliance parity is indeed associated with an appreciably higher probability of war, while traditional power transition hypotheses concerning dyadic parity, dyadic transitions, and speed of transition are not empirically supported by the evidence.<sup>77</sup>

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75. When parity and dissatisfaction are present, the probability of war in Middle Eastern and Far Eastern dyads is at least five times greater than their baseline probability of war (when neither parity nor dissatisfaction is present). Lemke, *Regions of War and Peace*, p. 135.

76. Lemke and Werner, "Power Parity, Commitment to Change, and War."

77. Kim, "Alliance Transitions and Great Power War"; Kim, "Power Transitions and Great Power War from Westphalia to Waterloo"; Kim, "Power Parity, Alliance, and War from 1648 to 1975." Kim includes alliance effects in national capability scores by including expected contributions from other great powers, which he adjusts by weighting the third party's capability by an indicator that reflects the similarity of alliance preferences between the two states in question.

Kim breaks from the main orientation of the power transition research program in two important respects. First, he extends the temporal domain back to 1648. Although Organski asserted that power transition theory does not apply to the preindustrial period, his reliance on industrialization as the primary mechanism for economic growth and consequently international change is unnecessarily restrictive. If we focus on the more general concept of internal growth and uneven rates of growth, extensions of the temporal domain to periods before the onset of industrialization need not violate the hard core assumptions of power transition theory.

More important, Kim relaxes the assumption that internal economic development is the primary means of augmenting national power and argues that alliance formation is a viable alternative.<sup>79</sup> We interpret Kim's argument for relaxing the internal development assumption as a break—albeit a modest one—with power transition's hard core, because it violates the assumption that economic growth within the state, not external affiliations, is the primary means of increasing national power. Thus Kim's alliance transitions model represents an inter-program problemshift, a new theoretical and empirical line of inquiry rooted in, but not fully accepting, the assumptions of the prior research program.

Like intra-program shifts, inter-program problemshifts are judged on their ability to generate novel predictions. Kim's extension of the model to the pre-industrial era generates predictions that were clearly not within the purview of power transition theory, and therefore is not ad hoc. Statistical testing reveals empirical corroboration of some of the novel predictions; thus Kim's alliance transitions model is not ad hoc. We conclude that Kim's inter-program problemshift is progressive, and

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He uses the tau-b statistic to measure alliance similarity, the limitations of which are noted by Signorino and Ritter, who suggest an alternative measure of similarity as a possible corrective for future analyses. Curtis S. Signorino and Jeffrey M. Ritter, "Tau-b or Not Tau-b: Measuring the Similarity of Foreign Policy Positions," *International Studies Quarterly*, Vol. 43, No. 1 (March 1999), pp. 115–144.

78. Kim, "Power, Alliance, and Major Wars, 1816–1975," p. 256; Kim, "Alliance Transitions and Great Power War," pp. 835–836; Kim, "Power Transitions and Great Power War from Westphalia to Waterloo," pp. 155–156.

represents a theoretically and empirically productive offshoot of the power transition research program.

#### WHO INITIATES WAR, WHEN, AND WHY?

Power transition theory and its associated hypotheses enjoy a substantial record of empirical corroboration. The confluence of a dissatisfied challenger's rise and a dominant state's decline or stagnation is correlated with the onset of major wars. Lemke shows that a similar relationship obtains for minor power wars. One thing that is missing, however, is a specification of which state initiates war, when, and why.

The power transition research program has not fully resolved the question of *timing*: whether war is initiated before transition, at the point of transition, or after transition. Organski originally argued that major wars were initiated by challengers prior to overtaking the dominant state.<sup>79</sup> Organski's subsequent empirical work with Kugler, however, indicates that challengers initiate war *after* overtaking the dominant state, and other studies have generated mixed results.<sup>80</sup>

Organski's and Kugler's first response to the unexpected finding about the timing of war was to argue, based on their empirical findings, that although the challenger initiates war after the *dyadic* transition, it does so before the strength of the challenger's *coalition* has surpassed the dominant state's coalition. They suggest the following tentative explanation:

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79. Organski, *World Politics*, p. 333. This is theoretically problematic because it implies that the challenger initiates a war while it is still the weaker party and consequently likely to lose the war. See Levy, "Declining Power and the Preventive Motivation for War," *World Politics*, Vol. 40, No. 1 (1987), pp. 82–107, at 83–84.

80. Organski and Kugler, *The War Ledger*, pp. 59–61. Geller provides supporting evidence, but Thompson finds that global wars since 1750 have broken out before the transition point, and Kadera's findings are mixed. Daniel S. Geller, "Relative Power, Rationality, and International Conflict," in Kugler and Lemke, *Parity and War*, pp. 127–44; Thompson, "Succession Crises in the Global System: A Test of the Transition Model," in Albert Bergesen, ed., *Crises in the World System* (Beverly Hills, Calif.: Sage, 1983), pp. 93–116 (see pp. 110–111); Kadera, *The Power-Conflict Story*. Clearly, more work needs to be done on the timing of war onsets during power transitions.

When two nations fight alone, there can be little doubt in the defender's and attacker's minds what their respective positions are and what will be the prospects for each if things are left to drift. On the other hand, when alliances are present the challenger may be in a position to afford to hesitate longer, for there is always hope that some important country will be separated from the rest of the defending coalition, thus tipping the balance. The dominant nation, secure in the support of the stronger coalition, also may tend to procrastinate before it faces up to the necessity of trying to turn back the foe.<sup>81</sup>

This argument is troubling from a Lakatosian perspective. It violates power transition's hard core, for the explanation of the challenger's decision to wait relies on the flexibility of alliance ties, which is explicitly rejected elsewhere (HC-6). In addition, the argument that the dominant state has an incentive to wait because there is "always hope" incorporates an element of wishful thinking, a non-rational factor that runs counter to the assumption (HC-2) that leaders make rational choices.

Another limitation of these analyses of the timing and initiation of war is that they focus only on the behavior of the challenger and ignore the declining dominant power. This is theoretically problematic, for the outbreak of war is a question of strategic interaction between two or more states, and any analysis of the timing and initiation of war must focus not only on the challenger but also on the dominant power and on the strategic interaction between the two. Similarly, in a study of the conditions under which power shifts precipitate war, Kim and Morrow explain, "we do not ask the question of why dominant states do not crush nascent challengers far in advance of their rise to power. The literature, to our knowledge, has never addressed this question, so we do not."<sup>82</sup> This issue is too important to dismiss so easily.

Kugler and Organski anticipate the argument that the dominant power may have an incentive to initiate hostilities, but claim that because the dominant power is satisfied, it "has little incentive" to alter the status quo. "After all, the prevailing international order is controlled by and designed

81. Organski and Kugler, *The War Ledger*, pp. 59–61.

82. Kim and Morrow, "When Do Power Shifts Lead to War?" p. 897, note 1. James D. Morrow, "The Logic of Overtaking," in Kugler and Lemke, *Parity and War*, p. 314, makes a similar point.

for the benefit of the dominant power."<sup>83</sup> This argument ignores the fact that the very rise of the challenger constitutes a potential threat to the status quo, and that the declining leader may have an incentive to use force not in order to alter the status quo but to maintain it, by initiating or provoking a "preventive war" to block the rising challenger while the opportunity is still available.<sup>84</sup>

The hypothesized role of the preventive motivation in power transitions draws some support from the empirical literature. Geller finds that wars that break out during power shifts are initiated either by the dominant power before the transition or by the rising challenger after the transition.<sup>85</sup> Copeland, whose dynamic differentials theory merges power transitions with polarity, finds in several cases that "in both multipolarity and bipolarity, it is the dominant and declining state that initiates war."<sup>86</sup>

83. Kugler and Organski, "The Power Transition," pp. 187–188.

84. Levy, "Declining Power and the Preventive Motivation for War"; Stephen Van Evera, *Causes of War* (Ithaca, N.Y.: Cornell University Press, 1999), chap. 4. Organski, *World Politics*, pp. 309 and 333, mentions preventive war but questions its employment both on moral grounds and on the basis of historical accuracy, although he cites only anecdotal evidence to support the latter claim. On the problematic nature of the "preventive war" concept, see Jack S. Levy and Joseph R. Gochal, "Democracy and 'Preventive War': Israel in the 1956 Sinai Campaign," *Security Studies*, Vol. 11, No. 2 (Winter 2001–02), pp. 1–49.

85. Daniel S. Geller, "Power Transition and Conflict Initiation," *Conflict Management and Peace Science*, Vol. 12, No. 1 (Fall 1992), pp. 1–16, at p. 14. Because Geller's dependent variable includes disputes short of war, his analysis, while suggestive, does not contradict the finding that war occurs after the power transition. In other work that includes initiation of both war and serious disputes, Geller shows that "among contender states, war and dispute initiators are as likely to be inferior to their opponents as they are to be superior in the static balance of relative capabilities." Geller, "Relative Power, Rationality, and International Conflict," p. 138. See also Frank Whelon Wayman, "Power Shifts and the Onset of War," in Kugler and Lemke, *Parity and War*, pp. 145–162.

86. Dale C. Copeland, "Neorealism and the Myth of Bipolar Stability: Toward a New Dynamic Realist Theory of War," *Security Studies*, Vol. 5, No. 3 (Spring 1996), pp. 29–89 (quotation on p. 54); see also Dale C. Copeland, *The Origins of Major War* (Ithaca, N.Y.: Cornell University Press, 2000).

Although power transition theorists continue to reject the hypothesis that under some conditions the declining leader will initiate war for primarily preventive reasons, they have begun to develop some models that incorporate strategic interaction into the power transition research program. After acknowledging the unexpected finding of the post-transition war onset, and after rejecting explanations based on coalitional models and the possibility of faulty measurement of national power, Kugler and Organski emphasize an alternative explanation based on Kugler and Zagare's work on nuclear deterrence, which extends power transition logic by combining it with a game-theoretic framework based on Brams's theory of moves.<sup>87</sup>

Kugler and Zagare's model implies that, given a transition between a dominant state and a dissatisfied challenger, war will not occur prior to the point of transition. War can occur soon after the point of parity or transition only if the declining state is risk-acceptant and if the challenger is either risk-acceptant or risk-neutral.<sup>88</sup> Thus the Kugler and Zagare model can account for the anomalous empirical finding of post-transition war, but only by adding an additional assumption about the risk propensities of states. This is not problematic per se, although for this move to be progressive, additional predictions based on risk orientation would have to be generated and empirically confirmed, as the resolution of existing anomalies is not by itself sufficient. It is puzzling, however, why risk-acceptant actors might go to war after a transition but never before, and a fuller explanation of this puzzle would be helpful.<sup>89</sup>

87. Kugler and Organski, "The Power Transition," pp. 183–84; Kugler and Zagare, eds., *Exploring the Stability of Deterrence* (Boulder, Colo.: Lynne Rienner, 1987); Kugler and Zagare, "The Long-Term Stability of Deterrence," *International Interactions*, Vol. 15 (1990), pp. 255–278; Steven J. Brams, *Theory of Moves* (New York: Cambridge University Press, 1994).

88. War can also occur at the exact point of parity, if both states are risk acceptant or if one state is risk acceptant and the other is risk neutral. Kugler and Zagare, "The Long-Term Stability of Deterrence."

89. The Kugler and Zagare prediction is consistent with the fact that in most game-theoretic models, war is not an equilibrium outcome under complete information if actors are risk neutral or risk averse. Bueno de Mesquita and Lalman, *War and Reason*; James D. Fearon, "Rationalist Explanations for War,"

Kugler and Zagare argue further that this explanation accounts for the absence of a superpower nuclear war since 1945, presumably since no challenger has overtaken the United States (*ipso facto*, preserving the conditions of stable deterrence).<sup>90</sup> Thus, they claim to account for the anomaly of post-transition war onset and generate a novel fact—stable nuclear deterrence—that is consistent with the evidence.<sup>91</sup>

Recent dissertations by Alsharabati and Abdollahian deal with the question of the timing and initiation of war, but because this research is unpublished our assessment of it is preliminary and tentative.<sup>92</sup> Although each of these studies is an important step forward in understanding the dynamics of strategic interaction during power transitions, neither goes quite far enough with respect to the question of the role of the declining power.<sup>93</sup> In Alsharabati's game-theoretic model of the strategic interaction between dominant power and challenger, for example, the challenger makes the initial move, leaving the dominant power with a choice between resisting or capitulating. The model does not allow the dominant state to take preventive action to incapacitate the rising power before it has grown powerful enough to challenge the defender. In addition,

*International Organization*, Vol. 49, No. 3 (Summer 1995), pp. 379–414. Most game-theoretic models of international conflict now incorporate the element of incomplete information, and efforts by power transition theorists to subsume their models within a game-theoretic framework probably will have to move in this direction if they are to be successful.

90. See also Kugler and Organski, "The Power Transition," pp. 186–188.

91. Although the prediction of novel facts means that the Kugler and Zagare model is not ad hoc, whether the absence of a U.S.-Soviet war since 1945 constitutes an empirically corroborated novel fact is problematic. Although the long great power peace is consistent with the predictions of Kugler's and Zagare's extension of power transition theory, it is also consistent with many other theories as well, and therefore provides rather weak evidentiary support. Kugler and Zagare, *Exploring the Stability of Deterrence*. Kugler and Zagare concede that "The absence of a superpower war since [1945] ... makes it impossible to test directly the theory of deterrence." Kugler and Zagare, "The Long-Term Stability of Deterrence," p. 256.

92. Carole Alsharabati, "Dynamics of War Initiation" (Ph.D. dissertation, Claremont Graduate School, 1997); Abdollahian, "In Search of Structure."

93. This, of course, is not the only question they are trying to answer.

preliminary empirical tests of the model include variables representing the value of the status quo and the costs of war to the challenger, but not to the defender.<sup>94</sup>

Abdollahian argues that more attention ought to be paid to the dominant power's satisfaction with its dyadic relationship with the challenger.<sup>95</sup> His dynamic differential equations model identifies the structural conditions conducive to stability and instability, and hence in principle can predict the timing of war, but it cannot deal with the question of which specific state initiates war.

Although the question of who initiates war is commonly addressed by scholars both inside and outside the power transition research program, it is actually quite problematic in the context of strategic interaction. If one party has an incentive to initiate a war, its adversary might anticipate this, and act preemptively in order to secure the military advantages of striking first, at least under certain conditions (preemption may also involve diplomatic or domestic political costs). If so, the first state may have an incentive to preempt the preemptor, and so on. This implies that the attempt to identify the initiator may not be analytically useful.<sup>96</sup> On the other hand, the situation is not entirely symmetrical, for both the domestic politics and political psychology of decline may be different from those of ascent (in part because of the overweighting of losses), and these may influence the likelihood of preemption.<sup>97</sup> Moreover, although the infinite regress of preemption is theoretically plausible, on the empirical level there is some evidence that preemption rarely occurs, which undercuts

94. Alsharabati, "Dynamics of War Initiation," chaps. 2 and 3.

95. Abdollahian, "In Search of Structure," pp. 63, 84–85.

96. Kadera reaches a similar conclusion via a different logical argument. Kadera, *The Power-Conflict Story*.

97. There is substantial experimental evidence that people respond more strongly to losses than to comparable gains. This suggests another interesting line of research for power transition—to incorporate loss aversion and risk acceptance in the domain of losses. This would be compatible with the hard core of the power transition research program if the actors' reference points were always equated with the current status quo. Jack S. Levy, "Prospect Theory, Rational Choice, and International Relations," *International Studies Quarterly*, Vol. 41, No. 1 (March 1997), pp. 87–112.

the abovementioned arguments that the identification of the initiator is meaningless.<sup>98</sup>

These game-theoretic and dynamic models of the strategic interaction between declining leader and challenger constitute important efforts to put power transition theory in a dynamic and interactive context and to explain the anomalous empirical finding that the onset of war occurs after the point of power transition. Further development of this line of work could help overcome earlier degenerating elements in the research program and might contribute to a progressive intra-program problemshift. At the same time, however, other power transition theorists seem to be moving away from an emphasis on the dynamic nature of power transitions and from the question of the timing of war, and this suggests some ambiguity in the direction of the research program. Werner and Kugler, for example, argue that wars could erupt either prior to or following a transition, and that the condition of parity, not overtake, is the important correlate of war proposed by power transition theory.<sup>99</sup> Similarly, Lemke and Kugler argue emphatically that:

theoretically, it is parity that is important to war initiation. The closer to parity a dyad is, the greater the threat of war. Parity, not actual transitions, is of theoretical importance. For this reason it would have been better if Power Transition Theory had been named Power Parity Theory.<sup>100</sup>

Arguments for the relative importance of parity, rather than transitions, are puzzling in light of Organski's and Kugler's argument that the onset of war is more consistently associated with the process of transition and overtake than with the condition of parity, and with their finding of exactly zero cases of wars under conditions of parity without transition.<sup>101</sup> Moreover, an emphasis on the condition of parity rather than on the process of transition means that the question of timing of war onset

98. Dan Reiter, "Exploding the Powder Keg Myth: Preemptive Wars Almost Never Happen," *International Security*, Vol. 20, No. 2 (Fall 1995), pp. 5–34.

99. Werner and Kugler, "Power Transitions and Military Buildups," p. 204, n. 12.

100. Lemke and Kugler, "The Evolution of the Power Transition Perspective," p. 12 (emphasis in original).

101. Organski and Kugler, *The War Ledger*, pp. 49–52.

is moot, for the classification of the independent variable as parity or non-parity would not be affected by whether war occurred slightly before or slightly after the point of transition. Similarly, whether the dominant power initiates or provokes war would no longer be a central question.<sup>102</sup>

Thus the emphasis on parity over transitions would redirect our attention away from some important questions regarding the causes of war that have interested power transition scholars for years. It would discourage power transition theorists from pursuing important puzzles regarding the timing and initiation of war, and would constitute a major step back from Organski's attempt to construct a dynamic alternative to static balance of power models. We would regard such a shift in the orientation of the power transition research program, if it continued, as degenerating from a Lakatosian perspective.

#### QUESTIONS OF CAUSAL MECHANISMS AND BARGAINING

The question of who initiates war and when also raises the question of the causal mechanisms through which war occurs. The power transition research program has done a better job of specifying the structural conditions conducive to war than of explaining the causal mechanisms that drive this process. We have ample evidence of a fairly robust correlation between power parity and war, particularly among contenders vying for control of the international or regional order, but we still lack a complete theoretical explanation for this phenomenon.

The central question here concerns bargaining between adversarial states, and particularly why the two adversaries cannot reach a mutually agreeable settlement that avoids war. Unfortunately, power transition

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102. Geller's recent study is an exception in that he deliberately revisits both Organski's original emphasis on process and the question of which state initiates war. Among enduring rivals engaging in war between 1816 and 1996, dissatisfied challengers are more likely to initiate war than are status quo defenders, but in those cases where defenders do initiate wars with challengers, the dyad in question is almost always experiencing a shift in relative power when war is initiated. Daniel S. Geller, "Status Quo Orientation, Capabilities, and Patterns of War Initiation in Dyadic Rivalries," *Conflict Management and Peace Science*, Vol. 18, No. 1 (Fall 2000), pp. 73-96.

theorists generally neglect this intervening process,<sup>103</sup> although others have addressed it. As Levy has argued,

There is some level of concessions that the challenger would prefer to grant rather than to fight, particularly since he can always hope to regain those concessions later when he is stronger. Similarly, there is some level of concessions that the declining state would prefer to accept from the challenger rather than to initiate a war. If the challenger's offer matched the declining state's demands, war would not occur. Moreover, if both states agreed on the likely outcome of the war, they would be better off accepting that outcome without incurring the actual costs of war ... [but] the very fact that the declining state knows that the rising adversary will probably be able to regain any concession later makes the former less likely to accept those concessions.<sup>104</sup>

Moreover, the kind of concessions acceptable to the declining state would be those that impeded the further increases in the military power and potential of the rising adversary, but these are often based on internal economic changes that cannot easily be bartered away.

This is generally known as the commitment problem.<sup>105</sup> Along with private information about relative capabilities and resolve, and incentives to misrepresent that information (which generate different expectations by the two adversaries regarding their relative bargaining strength and consequently different incentives to settle), the inability of rising states to commit to refraining from using their power when they achieve a dominant position often makes it difficult for dyads undergoing a shift in relative power to reach a negotiated settlement. Specifying the conditions under which bargaining between a rising state and a declining state leads to a satisfactory settlement and peaceful transition is no easy task, as illustrated by the complexity of Powell's game-theoretic model of

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103. This is in spite of Organski's hypothesis that the flexibility of the dominant state is an additional variable determining the violent or peaceful character of power transitions (PH-5). Organski, *World Politics*, p. 336.

104. Levy, "Declining Power and the Preventive Motivation for War," p. 96.

105. Fearon, "Rationalist Explanations for War."

bargaining under conditions of shifting relative power.<sup>106</sup> Nevertheless, this is one question to which power transition theorists must devote more attention if they are to construct a more fully developed explanation of the causal paths through which power transitions, combined with status quo evaluations, contribute to the outbreak of war.

Troubling theoretical anomalies are left by the lack of attention to the dominant power and its possible incentives to initiate war, to the problem of strategic interaction and preemption, and to bargaining between leading power and challenger. On the other hand, preliminary efforts to cast power transition theory in a strategic interaction framework and to model the process formally,<sup>107</sup> and thus to incorporate the decisions and incentives of both dominant state and challenger, are useful steps in a more progressive direction.

### *Summary Evaluation of the Power Transition Program*

We have examined several distinct streams of research within the power transition research program, including the initial formulation of the theory by Organski, the refinement of the theory and empirical tests of some of its key propositions by Organski and Kugler, and important extensions of the theory by Kugler and Organski, Lemke, Kim, and others. We have given particular attention to the question of whether these extensions are intra-program or inter-program problemshifts, and whether these problemshifts are progressive or degenerating in a Lakatosian sense. We have argued that in extending power transition theory to regional subsystems, Lemke's multiple hierarchy model subsumes the empirical content of Organski's theory and generates predictions of novel facts that contradict neither the hard core nor the negative and positive heuristics of the research program, and have received some degree of empirical corroboration. Consequently, it constitutes a progressive intra-program problemshift within the power transition research program. We have argued that Kim's alliance

106. Robert Powell, *In the Shadow of Power* (Princeton, N.J.: Princeton University Press, 1999).

107. Alsharabati, "Dynamics of War Initiation."

transition theory also builds on the foundations of power transition theory and generates predictions of numerous novel facts that have been empirically confirmed. The focus on alliances, however, breaks from the power transition research program's hard core of assumptions, and we conclude that Kim's project represents a progressive inter-program problemshift.

Finally, with regard to the questions of timing and initiation, we have argued that the power transition research program has exhibited both signs of degeneration and signs of promise. Some attempts to explain anomalies in Organski's initial formulation depart from the program's hard core in significant ways, and are consequently degenerating. Recent efforts to explain the timing and initiation of war in terms of formal models—particularly game-theoretic models capturing strategic interaction—offer considerable promise. This outgrowth of the research program is still at an early stage, however, and several potentially important works have not yet been published. It would therefore be premature to make a definitive judgment whether this work will reverse the earlier trend toward degeneration on the questions of the timing and initiation of war and lead to a progressive problemshift. Lakatos would be the first to urge patience, for he recognized that problemshifts may occur only slowly, and might be discernible as degenerating only with the benefit of hindsight (pp. 154–159).<sup>108</sup>

Because some areas of inquiry within the power transition research program are progressive while others are degenerating, it is difficult to make a summary appraisal of the power transition research program from the perspective of Lakatosian metatheory. One of the limitations of Lakatos's methodology of scientific research programs is its failure to address the problem of how to aggregate judgments about the progressive or degenerating nature of individual projects into an integrated net assessment of the research program as a whole. Nonetheless, we are strongly inclined to argue that the power transition research program is, on balance, progressive. It is a lively and expanding research program that has moved forward in several important substantive directions. Most theoretical extensions of power transition principles have generated novel

108. See also Elman and Elman, Chapter 2 in this volume.

predictions, many of which are empirically corroborated, and proponents of the research program have been particularly good at developing improved operational measures of key theoretical concepts.

Scholars working within progressive research programs cannot simply sit back and admire their handiwork, however, for the research program that stops progressing begins to degenerate. We have argued that among the most important tasks for power transition theorists are the conceptual development and operationalization of states' degree of satisfaction with the status quo, the construction of an explanation for the timing of war that is fully consistent with the hard core of the research program, and the better specification of the causal mechanisms leading to war, including the role of bargaining between the dominant state and the rising challenger. Attention to these tasks is essential if the power transition research program is to continue on a progressive trajectory.

### *Conclusion: The Utility of Lakatos's Methodology for Assessing Progress in International Relations*

Having used Lakatos's methodology of scientific research programs as a framework for an analysis of the evolution of the power transition research program, we now reverse gears and consider the lessons we have learned from this exercise for the question of the utility of Lakatosian metatheory for evaluating research in international relations and in social science more generally. Lakatos's framework has been useful in many ways. It shifts our attention from individual theories to a series of theories that make up a research program; it forces us to think critically about both the generation of novel theoretical predictions and their empirical corroboration; and it provides guidelines for assessing whether efforts to resolve anomalies within a research program are scientifically legitimate. Because of space limitations, here we focus primarily on the problems we encountered in applying Lakatosian metatheory.

One very basic problem, for which Lakatos provides little guidance, concerns the "unit of appraisal": how broadly or how narrowly should we define a research program? In our case, should we focus on realism, structural realism, dynamic power theories broadly defined (to include not only Organski's power transition theory but also Gilpin's hegemonic

transition theory, Doran's power cycle theory, Thompson's leadership long cycle theory, and Copeland's dynamic realism), or just one particular model of power and change?

In choosing to focus on the power transition research program as developed by Organski, Kugler, Lemke, and their colleagues, we have opted for a somewhat restrictive view of the boundaries of a research program.<sup>109</sup> We have done so deliberately, out of concern that defining research programs too broadly brings with it two related risks. First, by grouping distinct theories together, we run the risk of including internally contradictory assumptions and hypotheses under the same heading. This can generate inconsistencies in the hard core, increase the ambiguity of the testable propositions derived from the hard core assumptions, and consequently diminish the falsifiability of these theories.<sup>110</sup>

A second danger is that the difficulties involved in testing such large, inclusive groups of theories can distract researchers' attention from the empirical evaluation of key hypotheses, and encourage them to engage in more abstract theoretical debates. An example is the "paradigm wars" that have dominated discourse in the international relations field for decades. To minimize these risks, we chose to delimit the unit of appraisal in a restrictive manner, considering only those theories sharing identical or nearly identical assumptions with the works of Organski and his colleagues. The result is a clearly defined research program, but one that is somewhat limited in scope.

109. Such a narrow delimitation is consistent with de Marchi's conception of a research program as a connected sequence of models. Neil de Marchi, "Introduction: Rethinking Lakatos," in Neil de Marchi and Mark Blaug, eds., *Appraising Economic Theories: Studies in the Methodology of Scientific Research Programs* (Aldershot, UK: Edward Elgar, 1991), pp. 12–13. A more expansive conception of research programs is reflected in Keohane's argument that classical realism and structural realism are part of the same research program (Keohane, "Theory of World Politics," pp. 506–518), or in Andrew Moravcsik's treatment in this volume (Chapter 5) of liberalism as a single research program.

110. On the importance of constructing theories that are both falsifiable and internally consistent, see Gary King, Robert O. Keohane, and Sidney Verba, *Designing Social Inquiry* (Princeton, N.J.: Princeton University Press, 1994), pp. 100–107.

Because the power transition research program is relatively well-defined, and because power transition theorists have used Lakatosian language and identified their work explicitly with the research program, in principle it should be relatively easy to analyze the program through the lenses of Lakatos's methodology of scientific research programs.<sup>111</sup> In fact, delineating the elements of the power transition research program has been very difficult. The most critical task involves the specification of the hard core, which is a prerequisite for all subsequent tasks in Lakatosian metatheory. Lakatos provides few guidelines, however, as to exactly what constitutes the hard core and whether it changes over time. At one point he emphasizes the fact that most Newtonian puzzles were foreseeable and even foreseen in the early phases of Newton's research program, and argues that it is useful to "separate the 'hard core' from the more flexible metaphysical principles expressing the positive heuristic" (pp. 136–137). Elsewhere, however, Lakatos argues that the hard core only emerges over time: the "hard core of a research programme does not actually emerge fully armed like Athene from the head of Zeus. It develops slowly, by a long, preliminary process of trial and error" (p. 133, note 4), and "dramatic spectacular results become visible only with hindsight and rational reconstruction" (p. 179). Most interpreters of Lakatos emphasize this view of the evolving hard core. Neil de Marchi, for example, argues that "the hard core and heuristics evolve and mutually inform each other."<sup>112</sup>

111. It is also interesting to note that Organski's own beliefs about what constitutes theoretical progress in international relations are similar to the principles of Lakatos' methodology of scientific research programs. Writing before Lakatos published his famous essay, Organski argued that, "A good theory must be clearly formulated and logically sound, and it must be consistent with the data it seeks to explain. Furthermore, it must explain something about the data that one would not otherwise know, and it must provide a more satisfactory explanation than any other rival theory can offer." Organski, *World Politics*, p. 283. Although not written in the language of Lakatosian metatheory, Organski's remarks are remarkably similar in spirit, emphasizing the issues of empirical testability or falsifiability, prediction of novel facts or excess empirical content, and explanatory capacity exceeding that of competing theories.

112. De Marchi, "Introduction," p. 16. See also Alan Musgrave, "Method or Madness? Can the Methodology of Research Programmes be Rescued from

If the hard core emerges gradually, however, by what criteria do we distinguish between a situation in which a problemshift breaks from the hard core and one in which that same problemshift helps to redefine the hard core? In terms of power transition theory, for example, does Kim's alliance transition theory break with the hard core, as we have argued, or does it broaden the content of the hard core to include alternative elements of national power? The concept of an evolving hard core blurs the distinction between inter-program problemshifts and intra-program shifts.

If the hard core evolves over time, the negative and positive heuristics also evolve and become more difficult to specify with any degree of precision. But even if the hard core is fixed at a certain point in the evolution of a research program, does this mean that the positive heuristic is also fixed, precluding further changes? This strikes us as unrealistic. Research programs evolve in response to theoretical and empirical anomalies that emerge at various stages in the evolution of a research program but that are not necessarily anticipated at the time of the consolidation of the hard core. This leads us to argue that the proper criterion for a progressive problemshift is that it not be inconsistent with the assumptions of the hard core, rather than the more stringent criterion that it be explicitly anticipated by the original positive heuristic. With respect to power transition theory, for example, we treat Lemke's multiple hierarchy model as a progressive intra-program problemshift because it is not inconsistent with the hard core, despite the fact that Organski says nothing about nested multiple hierarchies in his initial formulation of power transition theory.

Although through his discussion of ad hoc emendations Lakatos provides criteria for the classification of research programs as degenerative, his emphasis on tolerance of struggling research programs raises several practical questions, including how long we should wait for the empirical corroboration of novel theoretical predictions and thus for a definitive judgment that a problemshift is or is not degenerating. How

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; Backhouse, "The Lakatosian Legacy in Economic Methodology," p. 176.

much "trial and error" should we tolerate before we conclude that scholars should abandon the research program and shift their efforts in more productive directions? This is similar to the questions that we always ask of our theories: At what point do we conclude that we are wrong? What evidence would convince us to abandon the theory? Lakatos's emphasis on the need to tolerate struggling research programs, if taken to the extreme, runs the risk of undermining objective standards of scientific progress and of failing to discourage attempts to prop up unproductive research programs. One of Lakatos's most important contributions is to provide criteria for assessing what modifications of a research program are scientifically acceptable; excessive tolerance of struggling research programs threatens to undercut this achievement.

Another issue on which Lakatos provides little guidance concerns the fact that most research programs are multidimensional in the sense that they involve several anomalies and several independent efforts to resolve those anomalies. Some of these efforts may be progressive while others may be degenerating, as we found with respect to the power transition research program, and this raises the question of the criteria by which we aggregate these discrete problemshifts and reach an overall judgment of the progressiveness of the research program as a whole. Presumably we should consider the number of novel facts, their centrality to the research program, how many of these have been corroborated and how convincingly, but we have found little discussion of this. To the contrary, much of the secondary literature (though not necessarily Lakatos himself) treats the progressive or degenerative character of a research program as dichotomous categories rather than as a continuum.

Our evaluation of the power transition research program raises several other issues that are related to larger debates over whether Lakatos gives much more emphasis to the generation of novel theoretical predictions than to their empirical corroboration.<sup>113</sup> First, a major strength of the power transition research program—and indeed of many research programs in international relations, particularly those based on large-*n* research designs—is its constant attention to the development of improved empirical indicators for key theoretical concepts, including

113. Elman and Elman, Chapter 2 in this volume.

national power, political capacity, and satisfaction with the status quo. This is an important component of scientific progress because it facilitates empirical testing, but Lakatos fails to give it adequate emphasis or to specify how it should be evaluated. The operationalization of theoretical concepts can presumably be subsumed within the positive heuristic (as we have done here), but it deserves more emphasis than Lakatos gives it.

Another important element of scientific progress to which Lakatos appears to give insufficient attention is the role of multiple corroborations of a single fact through repeated tests based on different empirical domains, different operational indicators, and different research designs. This derives largely from the difficulty of "corroborating" novel facts in international relations and the social sciences more generally, a difficulty that Lakatos appears to underestimate. The recent growth of multi-method research designs is a direct result of the recognition of the difficulty of testing theoretical propositions and the advantages of "triangulation" through the application of different methodologies, each compensating for the weaknesses of others.

Lakatos's emphasis on theoretical progress over empirical progress is also reflected by the tendency to define the hard core exclusively in terms of theoretical assumptions, not methodological assumptions. While our own belief in the utility of multiple methods for testing most theoretical propositions makes us sympathetic toward this perspective, there are a number of important research programs that are closely associated with a specific methodological commitment in addition to certain substantive assumptions. Microeconomics, for example, is dominated by general equilibrium theory and a commitment to mathematical formalism, and has little empirical content.<sup>114</sup> In international relations, some argue that the research program of neoclassical realism includes a commitment to

114. E. Roy Weintraub, *General Equilibrium Analysis: Studies in Appraisal* (New York: Cambridge University Press, 1985); Backhouse, "The Lakatosian Legacy in Economic Methodology," p. 176; Mark Blaug, "Why I am Not a Constructivist: Confessions of an Unrepentant Popperian," in Roger E. Backhouse, ed., *New Directions in Economic Methodology* (New York: Routledge, 1994), pp. 115–121.

historical or case study methodologies.<sup>115</sup> Almost every study that we have associated with the power transition research program (beyond Organski's initial formulation of the theory) involves statistical methods. The recent research program on "analytic narratives" is defined by both its theoretical assumptions and its commitment to a particular methodology.<sup>116</sup> These characterizations are not consistent with the common practice of excluding methodological assumptions from the specification of the hard core of a research program.

Despite these problems, we have been able to use Lakatosian concepts as a guide to analyze whether certain problems shift are progressive or degenerating and whether they remain within a research program or initiate a new research program. We have focused on a single research program. Although our aim has not been to assess the relative merits of competing research programs, we expect that such a task would be far more difficult and that we would receive far less guidance from Lakatos.

Although Lakatos emphasizes the importance of "three-cornered fights between rival theories and experiment," he provides few criteria to evaluate the outcomes of such tests. His well-known injunction—that one theory is falsified if and only if there is a second theory that explains "all the unrefuted content" of the first and in addition predicts novel facts, some of which are corroborated (p. 116)—is of only limited utility in social science. First of all, the only theory that would explain everything another theory does is one that deductively subsumes the first. This generally involves axiomatically-based theories, but these are relatively rare in most of the social sciences outside of economics. More commonly, theory A explains only some of the content of theory B but also explains content that B does not explain, and Lakatos provides no criteria by which we might establish a preference between the two theories. This, of course, resembles the problem that Kuhn raises with his concept of

115. Rose, "Neoclassical Realism and Theories of Foreign Policy," p. 166; Schweller, Chapter 9 in this volume.

116. Robert H. Bates, Avner Greif, Margaret Levi, Jean-Laurent Rosenthal, and Barry R. Weingast, *Analytic Narratives* (Princeton, N.J.: Princeton University Press, 1998).

"incommensurable" paradigms.<sup>117</sup> The greater the differences in the assumptions in the hard cores of two different research programs, the less useful is Lakatosian metatheory as a guide for evaluating the respective merits of those research programs.

Let us end with a few remarks about the utility of the collective effort expressed in this volume. Elman and Elman are absolutely correct that most previous applications of Lakatos's methodology of scientific research programs have been based on incomplete understandings of the metatheory, that the assessment of international relations research could be improved by a more informed understanding of Lakatosian metatheory, and that the collection of essays in this volume, which is the first of its kind in political science, makes an important contribution toward that goal. It is necessary to point out, however, that the lessons we can learn from our collective efforts are limited in two respects, each imposed by practical constraints, and that these limitations suggest two potentially useful directions for future investigation.

First, the research programs selected for analysis in this volume are generally those that have endured over time.<sup>118</sup> To the extent that one criterion for a successful research program is that it endures over time, this constitutes a form of "selecting on the dependent variable," in that successful research programs are oversampled relative to unsuccessful research programs. If one of our questions is whether successful research programs tend to evolve in ways that are consistent with Lakatosian criteria for progressive research programs, and another question is whether progressive research programs (in the Lakatosian sense) tend to endure, this selection bias means that we are on more solid ground in answering the first question than the second. It is quite conceivable that there may be other research programs that have generated progressive

117. Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. (Chicago: University of Chicago Press, 1970).

118. This is also true of similar anthologies in economics, e.g., Spiro J. Latsis, ed., *Method and Appraisal in Economics* (Cambridge, UK: Cambridge University Press, 1976).

Lakatosian problemshiffts but nevertheless die out for other reasons.<sup>119</sup> It would be useful to examine research programs that did not endure and ask whether this was because they failed to satisfy Lakatosian criteria or for some other reason. Possible examples of such “failed” research programs might be applications of Kaplan’s balance of power theory or Rummel’s framework for the analysis of the relationship between internal conflict and external conflict.<sup>120</sup> Future applications of Lakatos’s methodology of scientific research programs should include failed research programs and attempt to explain the sources of failure.

Second, we all recognize that any metatheory has its limitations, and that perhaps the proper question is not the utility of Lakatos’s methodology of scientific research programs in any absolute sense, but rather its utility relative to that of alternative metatheories. Ironically, in focusing only on Lakatos’s methodology of scientific research programs, this volume as a whole has implicitly adopted a naive falsificationist approach to Lakatosian metatheory, pitting Lakatos in a two-cornered fight against the “data” of the various research programs in this volume rather than a three-cornered fight against these data and rival metatheories. That is, we have applied Popperian criteria, not Lakatosian criteria, to the evaluation of Lakatosian metatheory.<sup>121</sup>

As a first stage in the exploration of the utility of Lakatos’s methodology, this is reasonable, because for practical reasons the effort required to gain an adequate understanding of Lakatos could not have been repeated for each of several other metatheories, perhaps including Popper, Kuhn, and Laudan.<sup>122</sup> This suggests, however, that a useful task

119. Robert Jervis, “Realism in the Study of World Politics,” *International Organization*, Vol. 52, No. 4 (Autumn 1998), pp. 972–975.

120. Morton A. Kaplan, *System and Process in International Politics* (New York: John Wiley, 1957); Rudolph Rummel, “Dimensions of Conflict Behavior Within and Between Nations,” *General Systems: Yearbook of the Society for General Systems*, Vol. 8 (1963), pp. 1–50.

121. Karl R. Popper, *The Logic of Scientific Discovery* (New York: Harper and Row, 1959).

122. Popper, *The Logic of Scientific Discovery*; Kuhn, *The Structure of Scientific Revolutions*; and Larry Laudan, *Progress and Its Problems: Towards a Theory of Scientific Growth* (Berkeley: University of California Press, 1977).

for future research would be to examine historical research programs through the conceptual lenses of several distinct metatheories simultaneously in order to evaluate which metatheory most accurately describes how research traditions evolve and provides the best guide as to how they ought to evolve.<sup>123</sup>

123. On the tension between descriptive and prescriptive criteria—that is, between criteria for describing the evolution of scientific research and normative criteria for evaluating and perhaps guiding scientific research—see for example Blaug, “Why I am Not a Constructivist”; Mark Blaug, *The Methodology of Economics*, 2nd ed. (Cambridge, UK: Cambridge University Press, 1992); Latsis, *Method and Appraisal in Economics*; and Roger E. Backhouse, “The Lakatosian Legacy in Economic Methodology,” in Backhouse, *New Directions in Economic Methodology*, pp. 173–191.