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Power Shifts and Problem Shifts

THE EVOLUTION OF THE POWER TRANSITION RESEARCH PROGRAM

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This article analyzes the evolution of power transition theory from the perspective of Lakatos's methodology of scientific research programs. The authors reconstruct the development of the power transition research program by analyzing its hard core of irrefutable assumptions, its negative and positive heuristics, and exemplary works contributing to its protective belt of testable auxiliary hypotheses. It is argued that some developments (e.g., Lemke's multiple hierarchy model) constitute progressive problemshifts, but other areas of the research program exhibit signs of degeneration. These include the treatment of the timing and initiation of wars associated with power transitions and causal mechanisms driving such wars. Findings show that the evolution of the power transition research program has generally been progressive in Lakatosian terms, but its future vitality will require continued efforts to explain the above-mentioned theoretical and empirical anomalies in a way that is consistent with the hard core of the research program and that generates new testable propositions.

In a discipline where the half-life of new theoretical ideas is rather short, power transition theory continues to be a major focus of research in international relations four decades after its initial formulation by Organski (1958) in *World Politics*. If anything, interest in the theory has grown, as evidenced by the number of journal articles, convention papers, and doctoral dissertations in recent years. The theory has evolved in significant ways since Organski first presented it, but three generations of scholars have identified with this research enterprise and have continued to refine the theory, extend it, and construct new research designs for empirical tests. This is one of the most enduring research programs in the field, and its 40-year mark and the passing of its founder constitute a good time to take stock and evaluate the evolution of power transition theory and its contribution to our understanding of international conflict.

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We approach this task through the application of Lakatos's (1970) methodology of scientific research programs (MSRP), which provides a framework to describe a body of scientific research and assess whether its development has been progressive. Although there are alternative metatheoretical frameworks to guide and evaluate social science research (Popper 1959; Kuhn 1970; Laudan 1977), international relations scholars have begun to gravitate toward Lakatos over the last decade or so, to the point that when scholars in the field attempt to justify their work in terms of the philosophy of science, Lakatos is probably now the metatheorist of choice.¹

Scholars in various disciplines have used Lakatos's MSRP to appraise research in their respective fields, and there is a lively literature on the interpretation and utility of Lakatosian metatheory, particularly in economics (Latsis 1976; de Marchi 1991; Blaug 1992; Backhouse 1994). International relations scholars have invoked Lakatos for the primary purpose of promoting and legitimizing their own work and criticizing the work of others, but until recently (Elman and Elman 1999b) no one has made a systematic effort to build his or her analysis around the key Lakatosian concepts of the hard core, negative and positive heuristics, and protective belt of a research program. Similarly, although there are some good reviews of power transition theory in the literature (Kugler and Organski 1989; Kugler and Lemke 1996, 2000), and although some of them make occasional references to Lakatosian concepts, none is deeply grounded in Lakatosian metatheory.

Our aim is to use a Lakatosian framework to organize an analysis of the evolution of power transition research over the last four decades. We begin with a brief summary of Lakatos's MSRP and follow with a general overview of the evolution of the power transition research program. We attempt to specify the elements of the research program in terms of its hard core of irrefutable assumptions, its negative and positive heuristics, its protective belt of auxiliary assumptions, and the extent to which it has been "progressive." Our primary aim is to use Lakatos as an aid to evaluate the body of international relations research and not to make an original contribution to the philosophy of science. Therefore, our overview of Lakatos makes no attempt to develop the nuances of the MSRP but only to highlight those elements that are essential for a reconstruction of the power transition research program.²

1. Evidence of the increasing influence of Lakatos in the international relations field includes the number of prominent scholars who have used Lakatos's MSRP as an organizing device (Keohane 1983; Bueno de Mesquita 1985; Kugler and Organski 1989), the fact that a recent symposium in the *American Political Science Review* (December 1997) focused on whether realist theories of balancing were progressive or degenerative in Lakatosian terms, and the convening of a conference on "Progress in International Relations Theory: A Collaborative Assessment of Imre Lakatos' Methodology of Scientific Research Programs," sponsored by Arizona State University in January 1999.

2. For a thorough treatment of Lakatos's MSRP and its application to international relations, see Elman and Elman (1999a). In an unpublished paper, DiCicco and Levy (1999) discuss the lessons of their present analysis for the application of Lakatosian metatheory to international relations theory and the social sciences more generally.

LAKATOS'S METHODOLOGY OF SCIENTIFIC RESEARCH PROGRAMS: A BRIEF OVERVIEW

Building on and reacting to Popper (1959) and Kuhn (1970), Lakatos (1970) envisions science neither as a set of distinct and unconnected theories nor a monolithic paradigm focused on increasingly narrow puzzles, accumulating increasingly minor and unspectacular findings until an intellectual revolution generates a shift to a new paradigm. Rather, scientific inquiry encompasses a number of distinct and often competing series of theories or "research programs." Theories within each research program constitute a coherent entity because they derive from a common set of assumptions (the "hard core" of the research program). The hard core itself is immunized from direct empirical test by a set of methodological prohibitions—the "negative heuristic." Instead, scholars derive from the hard core a set of operational hypotheses, and it is these hypotheses that are subject to empirical test. Thus, the "protective belt of auxiliary hypotheses" surrounds and protects the hard core of irrefutable assumptions. Researchers are aided in the construction of the protective belt by the "positive heuristic," a set of suggestions and hints for theory development and hypothesis testing. Lakatos's MSRP directs us to describe scientific research programs in terms of the hard core, the negative and positive heuristics, and the protective belt (pp. 133-38).

Lakatos (1970) refers to theoretical development or emendation as a "problemshift." If such a problemshift is consistent with the hard core of the existing research program, it is an *intraprogram* problemshift. If the theoretical emendation involves a violation of the assumptions of the hard core, then it marks a break from the first research program and the initiation of a new research program and, thus, constitutes an *interprogram* problemshift (Elman and Elman 1999a).

Lakatos (1970) provides criteria for assessing whether a theoretical development (i.e., whether an intraprogram problemshift or an interprogram problemshift) contributes to scientific progress or whether it is degenerating, no longer producing new and accurate knowledge. To be progressive, a research program must consist of successive theories or emendations that incorporate both the theoretical content and empirical content of the previous theory and, in addition, generate new predictions or "novel facts." If a series of theories fails to produce novel facts and fails to provide empirical corroboration for at least some of those facts,³ these theoretical developments are "ad hoc" and the research program is "degenerating" (pp. 116-19).

More specifically, a theoretical development or problemshift can be ad hoc in three distinct ways (Lakatos 1970). If a problemshift fails to yield predictions of novel facts, it is ad hoc₁. If it predicts novel facts but those novel facts are not empirically corroborated, it is classified ad hoc₂. To be progressive, a theoretical development cannot be ad

3. Lakatos (1970, 116-32) is not perfectly clear on what a novel fact is. Various interpretations have been advanced in the literature and are nicely summarized by Elman and Elman (1999a). 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. This reflects the common methodological injunction that a theory cannot be tested on the same data that were used to construct the theory (King, Keohane, and Verba 1994). This criterion can be quite demanding, however, because in principle it requires information about how the theory was developed.

hoc in either sense. Ad hoc theories merely patch up holes in existing theories without providing new theoretical or empirical content (Elman and Elman 1999a).

There is more ambiguity with respect to ad hoc₃. Although Lakatos (1970, 175-76, 182; see also Elman and Elman 1999a) sometimes suggests that a problemshift that is not fully in accord with the positive heuristic is ad hoc₃, 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 hoc₃ only if they are inconsistent with the positive heuristic. This is a weaker condition for the classification of a new development as an intraprogram shift, for a new emendation need not be explicitly part of the positive heuristic, just not inconsistent with it. This is Zahar's (1973, 101 ff) interpretation of Lakatos, and we accept it.

This concept of scientific progress is reflected in the following frequently quoted passage from Lakatos (1970, 116):

A theory is "acceptable" or "scientific" only if it has corroborated excess empirical content over its predecessor (or rival), that is, only if it leads to the discovery of novel facts . . . a scientific theory *T* is falsified if and only if another theory *T'* has been proposed with the following characteristics: (1) *T'* has excess empirical content over *T*: that is, it predicts novel facts, that is, facts improbable in the light of, or even forbidden, by *T*; (2) *T'* explains the previous success of *T*, that is, all the unrefuted content of *T* is included (within the limits of observational error) in the content of *T'*; and (3) some of the excess content of *T'* is corroborated.⁴

This is a rather demanding conception of scientific progress, for the only theories that explain all of the existing empirical content are those that fully subsume the first. Such theories are relatively rare, particularly if they involve new assumptions and thus constitute a break from the hard core of an earlier research program. This means that Lakatosian metatheory is difficult to use in a comparative evaluation of incommensurable research programs or paradigms.⁵ Here, however, we use Lakatos's MSRP for the purpose of evaluating the development and progressiveness of a single research program. Thus, incommensurability is not an issue, and the commitment to a common set of hard-core assumptions and guidelines for theory development within the positive heuristic allows for more objective standards for the assessment of theoretical progress.

4. Organski's (1958) own conception of what constitutes a good theory, articulated over a decade before Lakatos's (1970) famous essay, is strikingly similar to Lakatos's MSRP in its emphasis on superiority to alternative theories and consistency with the empirical evidence and logical coherence: "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" (p. 283).

5. Because incommensurable theories are based on different assumptions and concepts, it is often quite difficult to construct critical empirical tests that demonstrate the superiority of one over the other.

POWER TRANSITION THEORY: AN OVERVIEW

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, a new split between “offensive” realism and “defensive” realism, and a variety of other efforts to recast realism on a more solid theoretical and empirical foundation (Frankel 1996; Doyle 1997; Schweller and Priess 1997; Brooks 1997; Zakaria 1998; Rose 1998).

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 that 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. They also generate a number of common propositions: the distribution of power in the system or within a dyad is the primary factor shaping international outcomes; high concentrations of power in the system are destabilizing in the sense that they generally give rise to blocking coalitions and often lead to war; and these blocking coalitions, or the anticipation of them, generally work to maintain the sovereign state system and prevent hegemonies from being established and maintained. That is, classical realism, Waltzian neorealism, offensive and defensive realism, and neoclassical realism are all balance of power theories, even though they vary in their specification of exactly who balances, under what conditions, and with what effects.⁶

There are other schools of thought that share basic realist assumptions and the determining role of power in world politics but reject balance of power theory. Most prominent among these are Organski’s (1958) power transition theory, Gilpin’s (1981, 1988) hegemonic transition theory,⁷ and Modelski and Thompson’s (1989) theory of long cycles of global leadership and decline. Each of these approaches posits, contrary to balance of power theories, that 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” (Levy 1994, 725-26; 1998, 146-49). Because the two share some basic realist assumptions but generate mutually contradictory propositions, we treat them as different research programs within the realist tradition.⁸ More specifically, we will argue that power transition the-

6. 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 a Lakatosian assessment of the treatment of the role of balancing in realist theories, see Vasquez (1997).

7. Although clearly a hegemonic realist, Gilpin (1981, 147) argues that balancing of power has at times limited states’ attempts to expand in the modern European state system.

8. Gilpin (1996, 6) clearly self-identifies as a realist. Organski (1958) and his associates contrast power transition theory with balance of power theory, rarely mention realism, and, in fact, explicitly reject the realist label (Kugler and Organski 1989, 172-73; Tammen et al. 2000; Kugler and Lemke 2000).

ory and other forms of hegemonic theory constitute a break from the hard core of balance of power realism.⁹

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 (1970) MSRP, it is more useful to focus our attention on power transition theory as developed by Organski (1958) and refined by Organski and Kugler (1980), Kugler and Lemke (1996), and their colleagues. Three generations of scholars have self-consciously identified with this research program¹⁰ and continue to refine the theory and test it empirically. This has led to a cumulating body of research, which now constitutes a major research program in the field. If Lakatos's MSRP 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.

We exclude from our analysis a discussion of Gilpin's (1981) hegemonic transition theory, Modelski's (1978) and Thompson's (1988; Rasler and Thompson 1994) leadership long cycle theory, and Doran's (1989a; Doran and Parsons 1980) theory of relative power cycles. Although Gilpin's hegemonic transition theory is a theoretically rich and important contribution and shares with Organski's power transition theory many of the same assumptions and arguments, Gilpin does not self-identify with Organski's power transition theory.¹¹ Moreover, the paucity of subsequent efforts to test Gilpin's theory empirically (Spiezio 1990) makes it difficult to apply Lakatosian metatheory, which emphasizes the empirical corroboration of novel facts.¹² Long cycle theory's exclusively systemic orientation (in contrast to Organski's combination of systemic and dyadic elements), explicit assumption of cycles, and focus on sea power and leading economic sectors make this line of research sufficiently distinct that it is best examined as a self-contained research program. Finally, although Doran's power cycle theory shares common elements with power transition theory,

9. This does not imply that these research programs are necessarily incommensurable or mutually exclusive. Bueno de Mesquita and Lalman (1992) and Kadera (1995, 1996) each try to integrate balance of power theory and power transition theory and specify the conditions under which the propositions of each are valid. Levy (1994, 1998) argues that most applications of balance of power theory focus, however implicitly, on the European system and land-based military power. Most applications of power transition theory focus on the global system and measure power in terms of sea power, air power, or wealth, so that the propositions generated by these theories are not necessarily contradictory. Great powers can simultaneously balance against an aspiring European hegemon and align with a dominant global power, and this contradicts neither balance of power theory nor power transition theory. This is also an important theme in Rasler and Thompson (1994).

10. 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. It introduces a Kuhnian element of the sociology of science in contrast to Lakatos's stated emphasis on the rational reconstruction of scientific development. This issue relates to debates over the balance of descriptive and prescriptive elements in Lakatosian metatheory (Latsis 1976; Blaug 1992; Backhouse 1994).

11. Gilpin (1981, 94n. 11) cites Organski only once.

12. We also exclude hegemonic stability theory (Keohane 1984). Although the theory generally assumes that a stable, liberal world economy promotes peace as well as prosperity, few of its proponents other than Gilpin (1981) develop those theoretical linkages. They focus instead on explaining stability in the international political economy.

we exclude it because it includes some nonrationalist elements, which are at odds with the hard core of the power transition research program.¹³

**THE FOUNDATIONS OF THE RESEARCH PROGRAM:
WORLD POLITICS (1958) AND THE WAR LEDGER (1980)**

The most significant scholarly contributions of Organski's (1958) *World Politics* were his critique of balance of power theories (Morgenthau 1948; Gulick 1955; Claude 1962) 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 power parity hypothesis that an equality of capabilities between adversaries contributes to peace and argued that such a condition of parity is more likely to lead to war.¹⁴ He also argued that there is usually a dominant power that sits atop an 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 between states are stable and follow certain patterns and even rules of behavior promoted by the dominant power (Organski 1958, 313-16, 326-30). 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, making the new distribution of power 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 through the use of 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.

13. Doran (1991, 2000) maintains that power transition theory is misspecified and fails to provide as complete an explanation of major power war as power cycle theory. He tested the two theories against the same data set (Doran 1989b) and found that power transitions are associated with major war only when coinciding with "critical points" in states' relative power trajectories, whereas critical points do predict wars even in the absence of transitions. Houweling and Siccama (1991, 1996) investigate the interaction effects of power transitions and critical points in their attempts to merge the two theories; additional *competitive* tests of the two theories would be of even greater interest from a Lakatosian perspective.

14. Although Organski (1958) associated the power parity hypothesis with balance of power theory, many balance of power theorists reject this formulation. This disagreement derives from the existence of multiple versions of balance of power theory and the fact that many of them are poorly specified. They often fail to distinguish clearly between dyadic- and systemic-level propositions and between individual and coalitional units of analysis.

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 on two key explanatory variables, relative power and the degree of satisfaction with the international order (or 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 the accompanying ability to shape the international order. Only the powerful and dissatisfied pose a threat (Organski 1958, chap. 12; Organski and Kugler 1980, 19-23, 39; Lemke 1995, 145; Lemke and Kugler 1996, 21).¹⁵

Organski and Kugler (1980) published the first statistical tests of power transition hypotheses in *The War Ledger*, focusing on the hypothesis that the combination of parity and transition is conducive to major war. They found that among those states capable of contending for global leadership, no wars take place without a transition; in addition, half of the observed transitions were followed by the outbreak of war.¹⁶ Based on these findings, the authors claimed that a power transition between contenders is a necessary but not sufficient condition for major war (Organski and Kugler 1980, 50-52; Kugler and Organski 1989, 179).¹⁷ Although critics have questioned various aspects of *The War Ledger's* research design (Bueno de Mesquita 1980; Thompson 1983, 1996; Levy 1991; Vasquez 1993, 1996; Siverson and Miller 1996; DiCicco 1998), the book stands as the foundation for the empirical development of the power transition research program.

A LAKATOSIAN RECONSTRUCTION OF THE POWER TRANSITION RESEARCH PROGRAM

POWER TRANSITION'S HARD CORE OF IRREFUTABLE ASSUMPTIONS

The central concept of Lakatosian MSRP 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 (Lakatos 1970, 133). Instead, researchers use

15. Recent treatments use Most and Starr's (1989) language, suggesting that parity provides the *opportunity* for war and dissatisfaction with the status quo the *willingness* to engage in war with the dominant state.

16. The findings hinge on the separation of "contenders" from the remainder of major powers in the system (see also footnote 41). Contenders include the dominant state and those states possessing at least 80% of its capabilities. If the dominant state is grossly preponderant, the three most powerful states are classified as contenders.

17. In *The War Ledger* (1980), Organski and Kugler also presented a measure of political development to generate an indicator of power that more accurately reflects a state's ability to extract internal resources (chap. 2), investigated the consequences of war for combatant states (chap. 3), and extended power transition logic to nuclear deterrence (chap. 4) (see also Kugler and Zagare 1987, 1990).

these assumptions to construct a theoretical system, derive auxiliary hypotheses that constitute 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" (Lakatos 1970, 133).¹⁸

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 (1958, 287) 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."¹⁹ 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 between states. Internal growth and development has supplanted the constant shifting of alliances as the primary mechanism for reconfiguring international political relationships (Organski 1958, 287-90, 306-9, 337-38; Organski and Kugler 1980, 24-27; Kugler and Organski 1989, 173; Lemke and Kugler 1996, 5-10).²⁰

Organski (1958, chap. 11) also criticizes balance of power theory's emphasis on alliance formation and dissolution as the primary mechanism for power redistribution and on the ease of making and breaking alliances. He argues that ties between 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 more inflexible (p. 314). Second, alliance ties in the modern era require heavy investments, including arms transfers, building and maintenance of bases abroad, and equipment standardization; consequently, alliances are less transitory. Third, the growth of democracy and leaders' appeals to constituents for support of their alignment policies make alliances much harder to reverse for democratic states. Economically interdependent, militarily tied, and sentimentally bound nations cannot "switch sides" as easily as the dynastic states of the 16th, 17th, and early 18th centuries (pp. 313-16). Consequently, alliances are not a primary means of enhancing national power.²¹

18. Thus, Lakatosian metatheory provides a justification for Friedman's (1953) famous "as if" assumption.

19. 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 that Waltz (1979, 119) makes this critique of Organski, although in non-Lakatosian language.

20. Although balance of power realists have long acknowledged the role of internal sources of military power and potential (Brooks 1997; Rose 1998), the emphasis on the internal mobilization of resources as a serious alternative to alliances as a balancing strategy (particularly in bipolar systems) is generally traced to Waltz (1979).

21. 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.

This discussion and more explicit statements in subsequent work (Organski and Kugler 1980, chap. 1; Kugler and Organski 1989, 172-75; Tammen et al. 2000) suggest the following set of hard-core (HC) assumptions in power transition theory:

HC1: States are the primary actors in international politics.²²

HC2: State leaders are rational in their foreign policy choices.²³

HC3: The international order is hierarchically organized under the leadership of a dominant power.

HC4: The rules governing the international political system are fundamentally similar to those governing domestic political systems.

HC5: Internal growth and development of states is the primary source of international change.

HC6: Alliance ties between states are relatively inflexible, and consequently alliances are not a primary means of enhancing national power.

HC6 is the most problematic in terms of the identification of the hard core. We could conceivably treat the diminished role of alliances not as a hard-core assumption but as a testable hypothesis and, thus, a part of the positive heuristic and protective belt of the power transition research program.²⁴ Although we think that this is how the alliance question ought to be treated, this is not how power transition scholars have treated alliances, since there has been virtually no effort to empirically examine 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 (1989, 1991, 1992, 1996) work on alliance transition theory, but it is significant that Kim's research has not led other power transition researchers to incorporate alliances into their models. 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.²⁵

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 contrast to the standard neorealist assumption that anarchy is the key ordering principle of international relations (Waltz 1979; Keohane 1983; Milner 1991; Buzan, Jones, and Little 1993), power transition theory posits a hierarchically organized international order defined by both the distribution of power and the set of rules and com-

22. Organski (1958, 15) continually refers to "nations" as "the dominant form of political organization" and "the major actors in the drama of world politics," but is clear that he means states.

23. The rationality assumption is clear in Organski and Kugler (1980, 39-40) and Kugler and Organski (1989, 172-73) and particularly explicit in Tammen et al. (2000).

24. Lakatos (1970) provides very little guidance on this issue.

25. Admittedly, Organski (1958, 331-32) 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.

mon practices imposed by a dominant state. In some respects, this distinction is rather thin and reflects semantic differences with regard to the meanings that neorealists and power transition theorists attach to the key concepts of anarchy, hierarchy, and authority. Waltz (1979) 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 (e.g., see Lemke and Kugler 1996, 21).

For Waltz (1979), 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” (p. 91; see also Jervis 1997, 132). In power transition theory, by contrast, order is the intended result of actions taken by a dominant state, which attempts to shape the international system in such a way that advances stability and enhances its own interests (Organski 1958, 326; Kugler and Lemke 2000). In balance of power theory, a single dominant state almost never arises because the balancing mechanism works to deter potential hegemons or to defeat them if deterrence fails (Levy 1998).²⁶

In contrast to the Waltzian assumption that states are functionally undifferentiated and have similar goals, Organski (1958, 53-57) argues that because states occupy different positions in the international hierarchy, they may have different goals. Moreover, in contrast to the view often associated with classical realists such as Morgenthau (1948) and contemporary “offensive realists” such as Mearsheimer (1999), Organski rejects the argument that all national goals reduce to the maximization of power (although he concedes that every state needs to maintain some minimum level of power to survive as a political entity). 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.

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 rules of domestic political systems “despite the absence of an enforceable code of international law” (Kugler and Organski 1989, 172; Lemke and Kugler 1996, 8).²⁷ 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.²⁸ For these reasons, we treat the power transition research program as a break in the hard core of balance of power realism.

26. Similarly, Doran (1991, chap. 5; 2000) argues that the historical record offers little support for the assumption of a single, dominant state powerful enough to impose an international order.

27. Gilpin (1981, 28) asserts that interstate relationships are ordered with an anarchic international system, and that although domestic and international politics are dissimilar, they share commonalities in their control mechanisms.

28. The assumption that international and domestic politics are fundamentally different goes back to Rousseau, which leads Walker (1999) to treat Rousseau as the first modern realist.

POWER TRANSITION'S NEGATIVE HEURISTIC

Lakatos's negative heuristic delineates the types of variables and/or models that ought to be shunned by researchers within a research program because they deviate from the assumptions of the hard core.²⁹ Power transition's hard core implies that researchers should not develop models that posit the importance of nonstate actors, nonrational 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 (1970, 134-38) argued that programmatic research is further guided by the positive heuristic, "a partially articulated set of suggestions or hints" with regard to the development of increasingly sophisticated models. These models generate hypotheses that constitute 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 speculates on the types of emendations and changes that will prepare the research program to best handle likely refutations and anomalies.

Organski (1958) acknowledged that the theory of the power transition would evolve over time. His text *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 such steps does knowledge grow. Beginnings must be big and breezy; refinements follow later" (p. 6). Organski (1958, 307) cautioned that power transition theory is not timeless but instead is limited to the period since the Industrial Revolution. He stated 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, we will "require new theories."³⁰

Organski (1958) 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,

29. This suggests that the negative heuristic is redundant because it follows directly from the hard core and provides no additional information (Sean Lynn-Jones, personal correspondence, cited by Elman and Elman 1999a).

30. Organski (1958) 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" (p. 307).

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 (Organski 1958, chap. 8, esp. 203-10; Organski and Kugler 1980, 33-38).

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 (1958) conceded that a good measure of the effectiveness of political institutions had yet to be developed and argued that “its creation is one of the major tasks that remains for political scientists to accomplish in the years ahead” (p. 203).

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 (1958) 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” (pp. 325-37). Organski never fully developed or measured the degree of satisfaction with the status quo, and clearly the conceptualization and operationalization of this variable is a key element in the positive heuristic of the power transition research program.

Finally, Organski (1958, 334-37) identified other factors affecting the likelihood that a power transition will result in war:

1. *The challenger's power potential when beginning its ascent.* If a rising state is too small to ever challenge the dominant state or “so large that its dominance, once it becomes industrial, is virtually guaranteed,” war becomes very unlikely.³¹
2. *The speed of the challenger's rise.* The more rapid the challenger's ascent, the greater the probability of war, for several reasons. The leaders of the dominant state have trouble adjusting to rapid changes; the challenger's leaders 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.³² Political leaders, to promote extraordinarily rapid growth, may make excessive demands on the populace, which can lead to internal strain and possibly incentives for the diversionary use of force.³³

31. Copeland's (1996) dynamic differentials theory explicitly incorporates concerns about potential power (see also Levy 1987; Van Evera 1999), but more from the viewpoint of the dominant power anticipating its own decline than anticipating the extent of the challenger's growth.

32. This explanation of the effects of a rapid rise of the challenger incorporates certain nonrational psychological processes (cf. Doran and Parsons 1980; Doran 1989a) and, in doing so, 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 (Abdollahian 1996) (see footnote 39). In any case, propositions about the speed of the challenger's rise have not been central to the research program.

33. In contrast to Organski's (1958) argument, Kim and Morrow (1992) argue that it is equally plausible that the challenger's leaders will be pessimistic about the ability to sustain extraordinarily rapid growth

3. *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 can mitigate the likelihood of war.³⁴ This is related to the next factor.
4. *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,³⁵ may reduce the probability of war associated with transitions.³⁶

The preceding discussion leaves us with the following characterization of power transition's positive heuristic (PH):

PH1: Construct models explaining major war onset during the industrializing era using the interaction of power transitions and dissatisfaction with the status quo.

PH2: Construct quantitative indicators of national power that reflect the intrastate sources of interstate dynamics.

PH3: Develop a conceptual and operational definition of political capacity.

PH4: Develop a conceptual and operational definition of dissatisfaction with the status quo.

PH5: Where the combination of relative power and (dis)satisfaction with the status quo fails 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.³⁷

Three examples from *The War Ledger*, each developed in later work, illustrate how the positive heuristic rooted in Organski's (1958) original formulation of power transition theory has guided subsequent inquiry. First, Organski and Kugler (1980, 33-38) provide a lengthy discussion of the research design tasks necessary for testing the power preponderance hypothesis (PH1 and PH2). Second, Organski and Kugler begin to develop a quantitative index to measure the effectiveness of political institutions or

and underestimate, not overestimate, their state's eventual position in the international order. On the diversionary motivation, see Levy (1989).

34. Organski (1958) noted that a "search for the determinants of flexibility of this sort would make an interesting study in itself" (p. 336).

35. Organski (1958, 324, 336-37) implied but did not explicitly state that similar domestic institutions facilitate interstate "friendship," which partially anticipates the interdemocratic peace proposition (Russett 1993). Others argue that satisfaction with the status quo could have the same result, and, in fact, Lemke and Reed (1996) argue that power transition theory subsumes the interdemocratic peace.

36. With the exception of the rapidity of the challenger's rise, Organski (1958, 323-25) 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.

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

political capacity (PH3).³⁸ Third, Organski and Kugler (1980, 56) test a statistical model that incorporates both relative power and the speed of the power transition to explain the likelihood of a peaceful transition (PH5).³⁹

The impact of the positive heuristic is also clear in the development or employment of new measures for key variables.⁴⁰ Following Organski (1958, chaps. 5-8, esp. 201-10), there has been considerable debate, both within and beyond the power transition research program, over the measurement of national power and satisfaction with the status quo. After comparing the Singer-Bremer-Stuckey (1972) measure of national capabilities and GNP, Organski and Kugler (1980, chap. 1) settled on GNP as a parsimonious indicator of political and economic power for testing power transition theory. Houweling and Siccama (1988) and subsequently Lemke and Werner (1996) test power transition hypotheses with Doran and Parsons's (1980) relative capabilities index. Recent replications and tests replace GNP with gross domestic product (GDP) and include the Correlates of War composite index of national capabilities as an alternative indicator (de Soysa, Oneal, and Park 1997; Lemke and Werner 1996). Such tests typically demonstrate the robustness of the association between parity and war among the most powerful states in the system over the past two centuries.⁴¹ More recent improvements and applications of a reliable measure of political capacity are summarized in *Political Capacity and Economic Behavior* (Kugler and Arbetman 1997), which builds on Kugler and Domke (1986) and Kugler and Arbetman (1989).

Power transition theorists have also made a number of efforts to operationalize the concept of the degree of satisfaction with the status quo and incorporate it into their

38. Organski and Kugler (1980) 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 plane. This rationale raises questions about the role of political capacity in a theory that is explicitly limited to great power behavior.

39. Power transition theorists have recently directed more attention to the speed of transitions. Abdollahian (1996, 64, 88), for example, takes issue with Organski's (1958) argument that the faster the transition, the higher the probability of war (PH5). 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 generate a higher probability of war than rapid transitions.

40. The development of improved empirical indicators is an important component of scientific progress, but Lakatos fails to give it adequate emphasis or specify how it should be evaluated. We treat the operationalization of theoretical concepts as part of the positive heuristic, where it plays a central role in the power transition research program.

41. Organski and Kugler (1980), using GNP as an indicator of power, find that power transitions and the outbreak of war are significantly associated during the period from 1860 to 1975, at least for contender states (see footnote 16). Houweling and Siccama (1988) define power in terms of Doran and Parsons's (1980) relative capabilities index and demonstrate that the finding extends to all great powers from 1816 to 1975. de Soysa, Oneal, and Park (1997) replicate Houweling and Siccama's analysis during the period from 1820 to 1989 using GDP and the COW Composite Index of National Capabilities (CINC). They find that power transitions are associated with war (although this is significant only when using the CINC indicator of power). Defining power in terms of GDP, CINC, and Doran and Parsons's index, Lemke and Werner (1996) find a consistent association between power parity in dyads consisting of the dominant power and another major power contender and a strong tendency toward war when such a dyad is characterized by parity and a challenger's "commitment to change." See Lemke and Kugler (1996) and Kugler and Lemke (2000) for more complete summaries of the empirical support for power transition theory.

models.⁴² To take one example, Kim (1991, 1992) operationalizes satisfaction in terms of the similarity of the alliance portfolios of the state with that of the dominant power, as indicated by the tau-b coefficient.⁴³ 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 (dis)satisfaction. Although no single indicator has gained overwhelming scholarly support, healthy debates over the proper measurement of satisfaction (Gibler 1998; Benson 1998; DiCicco 1998) have been propelling power transition research forward rather than obstructing its advancement.

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 which mechanisms does a rising challenger's dissatisfaction with the status quo lead to an increase in the likelihood of a violent confrontation (Organski 1958, 328)? As Oneal, de Soysa, and Park (1998) argue, power transition theorists need to specify exactly "what benefits the international system provides to states and over which they may fight" (p. 518). 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 might also refer to dyadic (Maoz and Mor 1998) or even regional structures or relationships (Lemke and Werner 1996, 245n. 11). Not only do 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' (dis)satisfaction with the status quo is necessary for continued progress within the power transition research program.

42. These include Werner and Kugler's (1996) indicator of extraordinary military buildups, Kim's (1991, esp. 843n. 14) alliance profile similarity measure (based on Bueno de Mesquita 1981), Bueno de Mesquita's money market discount rate measure (1990), and Lemke and Reed's (1996) assessment of the similarity of domestic structures.

43. Signorino and Ritter (1999) note that Kendall's tau-b measures *association*, not similarity, and demonstrates how the two can be quite different. They show that dissimilar alliance policies do not necessarily follow from a perfectly negative association, that identical alliance policies might not be reflected by the tau-b measure of association, and that changes in the value of tau-b do not necessarily reflect commensurate changes in the similarity of states' alliance portfolios. This work calls into question the utility of Kim's operational indicator of satisfaction and raises the possibility that power transition researchers should consider using Signorino and Ritter's spatial measure of foreign policy similarity.

SUBSEQUENT DEVELOPMENT OF THE POWER TRANSITION RESEARCH PROGRAM

The decade since Kugler and Organski's (1989) "retrospective and prospective evaluation" of power transition theory has seen a flurry of activity, both theoretical and empirical (for reviews, see Lemke and Kugler 1996; Vasquez 1996; Siverson and Miller 1996; DiCicco 1998; Kugler and Lemke 2000). The *Parity and War* anthology (Kugler and Lemke 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. 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 (1970, 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. Consequently, we focus on three distinct problem-shifts: Lemke's multiple hierarchy model, Kim's alliance transitions model, and research with regard to the timing and initiation of wars associated with power transitions. We reserve our evaluation of the progress of the power transition research program for the concluding section.

LEMKE'S MULTIPLE HIERARCHY MODEL

Organski's (1958) power transition theory focuses almost exclusively on the dyadic interactions between the dominant state and its potential challengers (Organski and Kugler 1980, chap. 1, esp. 42-45; Houweling and Siccama 1988; Lemke and Werner 1996). Scholars have recently moved beyond Organski's exclusive focus on power transitions at the very top of the international hierarchy and 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 Lemke's (1993, 1995, 1996, 1997) multiple hierarchy model, which extends power transition logic to regional subsystems nested within the overarching international order.⁴⁴

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, as well as smaller

44. There has also been a substantial amount of empirical research on power transitions involving dyads that include nonmajor powers by scholars who are not closely identified with the power transition research program (Weede 1976; Garnham 1976; Bremer 1992). Although these findings are interesting in themselves, the theoretical propositions and research designs that generate them are not explicitly linked to the logic of power transition theory. Consequently, the implications of these findings for power transition theory are unclear. The common finding that dyads characterized by an overwhelming preponderance of power are more likely to be peaceful than more symmetric dyads (Weede 1976; Garnham 1976), for example, does not necessarily reinforce power transition theory's propositions about the implications of changing power for the structure of the international system and the rules that govern the behavior of states.

subregional hierarchies.⁴⁵ A minor power might be satisfied with the global status quo (or unable to challenge the globally dominant state) but will 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 intraprogram or interprogram problemshift is difficult to assess because of the ambiguous status of the multiple hierarchy model with respect to the positive heuristic and the ambiguity of Lakatosian metatheory on this issue. We have argued that the proper criterion is the weaker one of not being inconsistent with the positive heuristic rather than the stronger one 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 *ad hoc*₃. We argue that it represents an intraprogram problemshift that generalizes the logic of power transition theory and contributes the following element to the positive heuristic of the research program:

PH6: 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*₁. Empirical tests of Lemke's hypotheses in some regional contexts have demonstrated preliminary support for the multiple hierarchy model. Most striking, Lemke's (1995, 1996) tests demonstrate that in South American regional hierarchies, parity approximates a necessary condition for minor power war. Applications of the model to the Middle East and Far East (Lemke 1997) yield weaker but still promising results. Thus, some of Lemke's predicted novel facts are empirically corroborated, so that the multiple hierarchy problemshift is not *ad hoc*₂. Finally, Lemke and Werner (1996) show that the multiple hierarchy model is able to postdict 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.

Given that 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 given that some of this excess empirical content is empirically corroborated, we argue that the multiple hierarchy model constitutes a progressive, intraprogram problemshift.

KIM'S ALLIANCE TRANSITIONS MODEL

Kim (1991, 1992, 1996) developed a theory of alliance transitions, which he describes as "revised power transition theory," and tested it for the period since 1648.

45. Lemke's (1993, 1995, 1996, 1997) theoretical justification for identifying regional and subregional hierarchies hinges on the power projection capacity of minor powers, which is operationalized in terms of a loss-of-strength gradient (Boulding 1962; Bueno de Mesquita 1981, 103-9).

Kim hypothesized 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, whereas traditional power transition hypotheses concerning dyadic parity, dyadic transitions, and speed of transition are not empirically supported by the evidence.⁴⁶

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.⁴⁷

More important, Kim (1989, 256; 1991, 835-36; 1992, 155-56) relaxes the assumption that internal economic development is the primary means of augmenting national power and argues that alliance formation is a viable alternative. We interpret Kim's argument for relaxing the internal development assumption as a break—albeit a modest one—with power transition's hard core. The argument 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 interprogram problemshift, a new theoretical and empirical line of inquiry rooted in, but not fully accepting, the assumptions of the prior research program (Elman and Elman 1999a, 9-10).

Like intraprogram shifts, interprogram problemshifts are judged on their ability to generate novel predictions. Kim's extension of the model to the preindustrial era generates predictions that were clearly not within the purview of power transition theory; therefore, Kim's extension of the model is not *ad hoc*₁. Statistical testing reveals empirical corroboration of some of the novel predictions; thus, Kim's alliance transitions model is not *ad hoc*₂. We conclude that Kim's interprogram problemshift is progressive and represents a theoretically and empirically productive offshoot of the power transition research program.

46. Kim (1989, 1991, 1992) includes alliance effects in national capability scores by including expected contributions from other great powers. He adjusts those expected contributions by weighting the third party's capability by a tau-b score reflecting the similarity of preferences between the two states in question. This approach has advantages and disadvantages. As Kim points out, it is more realistic than a strict capability-aggregation formula because it acknowledges that states do not always honor alliance commitments and sometimes expect help from others with whom they do not have formal alliance ties. However, this approach also relies on tau-b (see footnote 43), which is roundly criticized by Signorino and Ritter (1999). Moreover, it allows the researcher to generate adjusted capability scores for opposing states that include assistance from the same third party or even from each other.

47. Thompson (1983, 101) makes a similar argument, contending that uneven growth of states and transitions does not require industrialization. Similarly, Gilpin's (1981, 1988) hegemonic transition theory incorporates different sources of uneven economic growth in different eras.

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 prior to transition, at the point of transition, or after transition. Organski (1958, 333) originally argued that major wars were initiated by challengers prior to overtaking the dominant state.⁴⁸ Organski's subsequent empirical work with Kugler, however, indicates that challengers initiate war *after* overtaking the dominant state (Organski and Kugler 1980, 59-61).⁴⁹

Organski and Kugler's (1980, 59-61) 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:

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.

This argument is troubling from a Lakatosian perspective. It violates power transition's hard core, since the explanation for the challenger's decision to wait relies on the flexibility of alliance ties, which is explicitly rejected elsewhere (HC6).⁵⁰ In addition, the argument that the challenger has an incentive to wait because there is "always hope" incorporates an element of wishful thinking, a nonrational factor that runs counter to the assumption (HC2) that decision makers behave rationally.

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, since the outbreak of war is a question of strategic interaction between two or more states, and any analysis of the timing and initiation of

48. 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 (Levy 1987, 83-84).

49. Geller (1996) provides supporting evidence, but Thompson (1983, 110-11) finds that global wars since 1750 have broken out before the transition point, whereas Kadera (1995, 185-86) gets mixed results. Clearly, more work needs to be done on the timing of war onsets during power transitions.

50. Kadera (1995, 176) criticizes Organski and Kugler (1980) for their inclusion of alliances and argues that her dynamic power-conflict model generates more accurate predictions about the timing of high levels of conflict without alliances. Her brief case studies, however, suggest that alliances have historically played a greater role in the timing of wars, and this has led her to a study of the spatial diffusion of war (Kadera 1998).

war must focus not only on the challenger but also the dominant power and the strategic interaction between the two. We are not the first to observe this gap in the literature. In their study of the conditions under which power shifts precipitate war, Kim and Morrow (1992, 897n. 1) 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."⁵¹ This issue, however, is too important to dismiss so easily.

Kugler and Organski (1989, 187-88) 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 for the benefit of the dominant power." This argument ignores the fact that the very rise of the challenger constitutes a potential threat to the status quo. It ignores that the declining leader may have an incentive to use force not so much 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 (Levy 1987; Van Evera 1999, chap. 4).⁵² The hypothesized role of the preventive motivation draws some support from the empirical literature. Geller (1992, 14) 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.⁵³ Copeland (1996), 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" (p. 54).

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. In their attempt to address the unexpected finding of the posttransition war onset, and after rejecting explanations based on coalitional models and the possibility of faulty measurement of national power, Kugler and Organski (1989, 183-84) emphasize an alternative explanation based on Kugler and Zagare's (1987, 1990) work on nuclear deterrence, which extends power transition logic by combining it with a game-theoretic framework based on Brams's (1994) theory of moves.

Kugler and Zagare's (1990) 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

51. Morrow (1996, 314) makes a similar point.

52. Organski (1958, 309, 333) mentions preventive war but questions its employment on both moral grounds and the basis of historical accuracy, even though he cites only anecdotal evidence to support the latter claim. On the problematic nature of the "preventive war" concept, see Levy and Gochal (1999).

53. Because Geller's (1992) dependent variable includes disputes short of war, his analysis, although 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 (1996) 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" (p. 138). See also Wayman (1996).

is risk acceptant and if the challenger is either risk acceptant or risk neutral.⁵⁴ Thus, the Kugler and Zagare model can account for the anomalous empirical finding of posttransition 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, since 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. A fuller explanation of this puzzle would be helpful.⁵⁵

Kugler and Zagare argue further that this explanation accounts for the absence of a superpower nuclear war since 1945, presumably because no challenger has overtaken the United States (*ipso facto*, preserving the conditions of stable deterrence) (see also Kugler and Organski 1989, 186-88). In this way, they ostensibly account for the anomaly of posttransition war onset and generate a novel fact, stable nuclear deterrence, that is consistent with the evidence.⁵⁶

Recent dissertations by Alsharabati (1997) and Abdollahian (1996) 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. 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.⁵⁷ In Alsharabati's (1997, chap. 2) 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, 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 (chap. 3).

Abdollahian (1996, 63, 84-85) argues that more attention ought to be paid to the dominant power's satisfaction with its dyadic relationship with the challenger. His dynamic differential equations model identifies the structural conditions conducive to

54. 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.

55. The Kugler and Zagare prediction is consistent with the fact that in most game-theoretic models involving military actors, war is not an equilibrium outcome under the condition of complete information if actors are risk neutral or risk averse (Bueno de Mesquita and Lalman 1992; Fearon 1995). Most game-theoretic models of international conflict now incorporate incomplete information. We suspect that efforts by power transition theorists to subsume their models within a game-theoretic framework will have to move in this direction if they are to be successful (Alsharabati 1997).

56. Although the prediction of novel facts means that the Kugler and Zagare (1987) 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 and Zagare's (1987) extension of power transition theory, it is also consistent with many other theories and, therefore, provides rather weak evidentiary support. Kugler and Zagare (1990) concede that "the absence of a superpower war since [1945] . . . makes it impossible to test directly the theory of deterrence" (p. 256).

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

stability and instability, and hence can in principle predict the timing of war but cannot deal with the question of which specific state initiates war.⁵⁸

These recent game-theoretic or dynamic models of the strategic interaction between declining leader and challenger constitute important efforts to put power transition theory into a dynamic and interactive context and explain the anomalous empirical finding that the onset of war occurs after the point of power transition (but see footnote 49). Further development of this line of work could help overcome earlier degenerating elements in the research program and potentially contribute to a progressive intraprogram 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 the question of the timing of war. This suggests some ambiguity in the direction of the research program. Werner and Kugler (1996, 204n. 12), for example, argue that wars could erupt either prior to or following a transition and the condition of parity, not overtake, is the important correlate of war proposed by power transition theory.⁵⁹ Similarly, Lemke and Kugler (1996, 12) 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” (p. 12).

Arguments for the relative importance of parity rather than transitions are puzzling in light of Organski and Kugler’s (1980, 49-52) 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. Moreover, an emphasis on the condition of parity rather than the process of transition means that the question of timing of war onset is moot, since the classification of the independent variable as parity or nonparity 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.

58. 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 to secure the military advantages of striking first, at least under certain conditions (preemption may also involve domestic political or diplomatic 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. Kad-
era (1995) reaches a similar conclusion via a different logical argument.

On the other hand, the situation is not entirely symmetric. Both the domestic politics and political psychology of decline may be different than those of ascent (in part because of loss aversion and related prospect theory hypotheses—see Levy 1997), and these may influence the likelihood of preemption (but note that these variables go beyond the hard core of the power transition research program). Moreover, although the infinite regress of preemption is theoretically plausible, on the empirical level there is little evidence that preemption commonly occurs (Reiter 1995), which undercuts the above-mentioned arguments that the identification of the initiator is meaningless. This question clearly needs much more attention at both the conceptual and empirical levels.

59. Conventional acceptance of parity as a correlate of war is reflected in its use as a control variable in other studies (Kugler and Lemke 2000).

Thus, the emphasis on parity over transitions would redirect our attention away from some important questions with regard to the causes of war that have interested scholars for years. It would discourage power transition theorists from pursuing important puzzles with regard to the timing and initiation of war. It would constitute a major step back from Organski's (1958) 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 questions of who initiates war and when also raise 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.

Questions of intervening causal mechanisms lead directly to the question of bargaining between adversarial states, but this intervening process is generally neglected by power transition theorists.⁶⁰ As Levy (1987, 96) argues:

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 exceeded 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.

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 the commitment problem (Fearon 1995). Along with private information about relative capabilities and resolve and incentives to misrepresent that information, which generate different expectations by the two adversaries with regard to their relative bargaining strength and, consequently, different incentives to settle, this 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

60. This is in spite of Organski's (1958, 336) hypothesis that the flexibility of the dominant state is an additional variable determining the violent or peaceful character of power transitions (PH5).

task, as illustrated by the complexity of Powell's (1999) game-theoretic model of bargaining under conditions of shifting relative power. 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 the degree of (dis)satisfaction with the status quo contribute to the outbreak of war.

The lack of attention to the dominant power and its possible incentives to initiate war, the problem of strategic interaction and preemption, and bargaining between leading power and challenger continues to be a troubling theoretical anomaly. On the other hand, preliminary efforts to cast power transition theory in a game-theoretic framework (Alsharabati 1997) and, thus, subsume its propositions within an axiomatically based system attentive to the decisions and incentives of both dominant state and challenger, are important steps in a more progressive direction.

CONCLUSION

Among efforts to explain the onset of interstate wars, power transition theory stands out as one that has developed into a rich, expanding research program spanning several generations of scholars. 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 paid particular attention to the question of whether these extensions are intraprogram or interprogram 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 has generated predictions of novel facts. These predictions contradict neither the hard core nor the negative and positive heuristics of the research program and have received some degree of empirical corroboration. Because Lemke's multiple hierarchy theory (T') subsumes the empirical content of Organski's theory (T), it constitutes a progressive intraprogram problemshift within the power transition research program. We have argued that Kim's (1991, 1992, 1996) alliance 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. We conclude that Kim's project represents a progressive interprogram 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 yet to be published. Therefore, it would be premature to make a definitive judgment as to 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 (1970, 154-59) would be the first to urge patience, since he recognized that problemshifts can occur slowly and might be discernible as degenerating only with the benefit of hindsight.

Because some areas of inquiry within the power transition research program are progressive whereas 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 MSRP 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 predictions, many of which are empirically corroborated. 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 afford to 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' (dis)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.

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