SEVEN

Theory of World Politics:
Structural Realism and Beyond

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FOR OVER 2000 years, what Hans J. Morgenthau dubbed “Political
Realism” has constituted the principal tradition for the analysis of
international relations in Europe and its offshoots in the New World
(Morgenthau 1966). Writers of the Italian Renaissance, balance of power
theorists, and later adherents of the school of Machtspolitik all fit under a
loose version of the Realist rubric. Periodic attacks on Realism have taken
place; yet the very focus of these critiques seems only to reinforce the
centrality of Realist thinking in the international political thought of the
West.¹

Realism has been criticized frequently during the last few years, and
demands for a “new paradigm” have been made. Joseph S. Nye and I
called for a “world politics paradigm” a decade ago, and Richard Mans-
bach and John A. Vasquez have recently proposed a “new paradigm for
global politics.” In both these works, the new paradigm that was envisaged
entailed adopting additional concepts—for instance, “transnational re-
lations,” or “issue phases” (Keohane and Nye 1972, esp. 379–386; Mans-
bach and Vasquez 1981, ch. 4). Yet for these concepts to be useful as
part of a satisfactory general theory of world politics, a theory of state
action—which is what Realism purports to provide—is necessary.

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paper for the American Political Science Association Annual Meeting in Denver, September
1982. A number of ideas presented here were developed with the help of discussions in the
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Understanding the general principles of state action and the practices of
governments is a necessary basis for attempts to refine theory or to
extend the analysis to non-state actors. Approaches using new concepts
may be able to supplement, enrich, or extend a basic theory of state
action, but they cannot substitute for it.²

The fixation of critics and reformers on the Realist theory of state
action reflects the importance of this research tradition. In my view,
there is good reason for this. Realism is a necessary component in a
coherent analysis of world politics because its focus on power, interests,
and rationality is crucial to any understanding of the subject. Thus any
approach to international relations has to incorporate, or at least come
to grips with, key elements of Realist thinking. Even writers who are
concerned principally with international institutions and rules, or analysts
in the Marxist tradition, make use of some Realist premises. Since Re-
alism builds on fundamental insights about world politics and state action,
progress in the study of international relations requires that we seek to
build on this core.

Yet as we shall see, Realism does not provide a satisfactory theory of
world politics, if we require of an adequate theory that it provide a set
of plausible and testable answers to questions about state behavior under
specified conditions. Realism is particularly weak in accounting for
change, especially where the sources of that change lie in the world
political economy or in the domestic structures of states. Realism, viewed
dogmatically as a set of answers, would be worse than useless. As a
sophisticated framework of questions and initial hypotheses, however, it
is extremely valuable.³

Since Realism constitutes the central tradition in the study of world
politics, an analysis, like this one, of the current state of the field must
evaluate the viability of Realism in the penultimate decade of the twen-
tieth century. Doing this requires constructing a rather elaborate argu-
ment of my own, precluding a comprehensive review of the whole
literature of international relations. I have therefore selected for discus-
sion a relatively small number of works that fit my theme, ignoring entire
areas of research, much of it innovative.⁴ Within the sphere of work
dealing with Realism and its limitations, I have focused attention on
several especially interesting and valuable contributions. My intention is
to point out promising lines of research rather than to engage in what
Stanley Hoffmann once called a "wrecking operation" (Hoffmann 1960:171).

Since I have written on the subject of Realism in the past, I owe the reader an explanation of where I think my views have changed, and where I am only restating, in different ways, opinions that I have expressed before. This chapter deals more systematically and more sympathetically with Realism than does my previous work. Yet its fundamental argument is consistent with that of Power and Interdependence. In that book Nye and I relied on Realist theory as a basis for our structural models of international regime change (Keohane and Nye 1977:42–46). We viewed our structural models as attempts to improve the ability of Realist or neo-Realist analysis to account for international regime change: we saw ourselves as adapting Realism, and attempting to go beyond it, rather than rejecting it.

Admittedly, chapter 2 of Power and Interdependence characterized Realism as a descriptive ideal type rather than a research program in which explanatory theories could be embedded. Realist and Complex Interdependence ideal types were used to help specify the conditions under which overall structure explanations of change would or would not be valid; the term "Realist" was used to refer to conditions under which states are the dominant actors, hierarchies of issues exist, and force is usable as an instrument of policy (Keohane and Nye 1977:23–29). Taken as a full characterization of the Realist tradition this would have been unfair, and it seems to have led readers concerned with our view of Realism to focus excessively on chapter 2 and too little on the attempt, which draws on what I here call structural realism, to account for regime change (chapters 3–6).

To provide criteria for the evaluation of theoretical work in international politics—Structural Realism, in particular—I employ the conception of a "scientific research programme" explicated in 1970 by the philosopher of science Imre Lakatos (1970). Lakatos developed this concept as a tool for the comparative evaluation of scientific theories, and in response to what he regarded as the absence of standards for evaluation in Thomas Kuhn’s (1962) notion of a paradigm. Theories are embedded in research programs. These programs contain inviolable assumptions (the "hard core") and initial conditions, defining their scope. For Lakatos, they also include two other very important elements: auxiliary, or ob-

s ervational, hypotheses, and a "positive heuristic," which tells the scientist what sorts of additional hypotheses to entertain and how to go about conducting research. In short, a research program is a set of methodological rules telling us what paths of research to avoid and what paths to follow.

"Consider a research program, with a set of observational hypotheses, a "hard core" of irrefutable assumptions, and a set of scope conditions. In the course of research, anomalies are bound to appear sooner or later: predictions of the theory will seem to be falsified. For Lakatos, the reaction of scientists developing the research program is to protect the hard core by constructing auxiliary hypotheses that will explain the anomalies. Yet any research program, good or bad, can invent such auxiliary hypotheses on an ad hoc basis. The key test for Lakatos of the value of a research program is whether these auxiliary hypotheses are "progressive," that is, whether their invention leads to the discovery of new facts (other than the anomalous facts that they were designed to explain).


Lakatos developed this conception to assess developments in the natural sciences, particularly physics. If we took literally the requirements that he laid down for "progressive" research programs, all actual theories of international politics—and perhaps all conceivable theories—would fail the test. Indeed, it has been argued that much of economics, including oligopoly theory (heavily relying on Structural Realists), fails to meet this standard (Latsis 1976). Nevertheless, Lakatos’s conception has the great merit of providing clear and sensible criteria for the evaluation of scientific traditions, and of asking penetrating questions that may help us to see Realism in a revealing light. Lakatos’s questions are relevant, even if applying them without modification could lead to premature rejection not only of Realism, but of our whole field, or even the entire discipline of political science?

The stringency of Lakatos’s standards suggests that we should supplement this test with a "softer," more interpretive one. That is, how much insight does Realism provide into contemporary world politics?

For this line of evaluation we can draw inspiration from Clifford Geertz’s discussion of the role of theory in anthropology. Geertz argues
that culture “is not a power, something to which social events, behaviors, institutions, or processes can be causally attributed; it is a context—something within which they can be intelligibly—that is, thickly—described” (1973:14). The role of theory, he claims, is “not to codify abstract regularities but to make thick description possible, not to generalize across cases but to generalize within them” (ibid., p. 26). This conception is the virtual antithesis of the standards erected by Lakatos, and could all too easily serve as a rationalization for the proliferation of atheoretical case studies. Nevertheless, culture as discussed by Geertz has something in common with the international system as discussed by students of world politics. It is difficult to generalize across systems. We are continually bedeviled by the paucity of comparable cases, particularly when making systemic statements—for example, about the operation of balances of power. Much of what students of world politics do, and what Classical Realism in particular aspires to, is to make the actions of states understandable (despite obscurantist statements by their spokesmen): that is, in Geertz’s words, to provide “a context within which they can be intelligibly described.” For example, Morgenthau’s discussion of the concept of interest defined in terms of power, quoted at length below, reflects this objective more than the goal of arriving at testable generalizations.

This essay is divided into four major sections. The first of these seeks to establish the basis for a dual evaluation of Realism: as a source of interpretive insights into the operation of world politics, and as a scientific research program that enables the investigator to discover new facts. I examine the argument of Thucydides and Morgenthau to extract the key assumptions of Classical Realism. Then I discuss recent work by Kenneth N. Waltz, whom I regard as the most systematic spokesman for contemporary Structural Realism.

Section II addresses the question of interpretation and puzzle-solving within the Realist tradition. How successful are Realist thinkers in making new contributions to our understanding of world politics? In Section III, I consider the shortcomings of Realism when judged by the standards that Lakatos establishes, or even when evaluated by less rigorous criteria, and begin to ask whether a modified version of Structural Realism could correct some of these faults. Section IV carries this theme further by attempting to outline how a multidimensional research program, including a modified structural theory, might be devised; what its limitations would be; and how it could be relevant, in particular, to problems of peaceful change.

The conclusion emphasizes the issue of peaceful change as both a theoretical and a practical problem. Realism raises the question of how peaceful change could be achieved, but does not resolve it. Understanding the conditions under which peaceful change would be facilitated remains, in my view, the most urgent task facing students of world politics.

1. STRUCTURAL REALISM AS RESEARCH PROGRAM

To explicate the research program of Realism, I begin with two classic works, one ancient, the other modern: The Peloponnesian War, by Thucydides, and Politics Among Nations, by Morgenthau. The three most fundamental Realist assumptions are evident in these books: that the most important actors in world politics are territorially organized entities (city-states or modern states); that state behavior can be explained rationally; and that states seek power and calculate their interests in terms of power, relative to the nature of the international system that they face.

The Peloponnesian War was written in an attempt to explain the causes of the great war of the Fifth Century B.C. between the coalition led by Athens and its adversaries, led by Sparta. Thucydides assumes that to achieve this purpose, he must explain the behavior of the major city-states involved in the conflict. Likewise, Morgenthau assumes that the subject of a science of international politics is the behavior of states. Realism is “state-centric.”

Both authors also believed that observers of world politics could understand events by imagining themselves, as rational individuals, in authoritative positions, and reflecting on what they would do if faced with the problems encountered by the actual decisionmakers. They both, therefore, employ the method of rational reconstruction. Thucydides admits that he does not have transcripts of all the major speeches given during the war, but he is undaunted:

It was in all cases difficult to carry [the speeches] word for word in one’s memory, so my habit has been to make the speakers say what was in my opinion demanded of them by the various occasions, of course adhering as closely as possible to the general sense of what they really
Morgenthau argues that in trying to understand foreign policy,

We put ourselves in the position of a statesman who must meet a certain problem of foreign policy under certain circumstances, and we ask ourselves what the rational alternatives are from which a statesman may choose . . . and which of these rational alternatives this particular statesman, acting under these circumstances, is likely to choose. It is the testing of this rational hypothesis against the actual facts and their consequences that gives meaning to the facts of international politics and makes a theory of politics possible. (Morgenthau 1966:5)

In reconstructing state calculations, Thucydides and Morgenthau both assume that states will act to protect their power positions, perhaps even to the point of seeking to maximize their power. Thucydides seeks to go beneath the surface of events to the power realities that are fundamental to state action:

The real cause [of the war] I consider to be the one which was formally most kept out of sight. The growth in the power of Athens, and the alarm which this inspired in Lacedemon, made war inevitable (Thucydides, Book I, paragraph 24 [Chapter I, Modern Library edition, p. 13]).

Morgenthau is even more blunt: “International politics, like all politics, is a struggle for power” (1966:25; see also Morgenthau 1946). Political Realism, he argues, understands international politics through the concept of “interest defined as power”:

We assume that statesmen think and act in terms of interest defined as power, and the evidence of history bears that assumption out. That assumption allows us to retrace and anticipate, as it were, the steps a statesman—past, present, or future—has taken or will take on the political scene. We look over his shoulder when he writes his dispatches; we listen in on his conversation with other statesmen; we read and anticipate his very thoughts. (1966:5)

The three assumptions just reviewed define the hard core of the Classical Realist research program:

(1) The state-centered assumption: states are the most important actors in world politics;

(2) The rationality assumption: world politics can be analyzed as if states were unitary rational actors, carefully calculating costs of alternative courses of action and seeking to maximize their expected utility, although doing so under conditions of uncertainty and without necessarily having sufficient information about alternatives or resources (time or otherwise) to conduct a full review of all possible courses of action;

(3) The power assumption: states seek power (both the ability to influence others and resources that can be used to exercise influence); and they calculate their interests in terms of power, whether as end or as necessary means to a variety of other ends.

More recently, Kenneth N. Waltz (1959) has attempted to reformulate and systematize Realism on the basis of what he called, in Man, the State and War, a “third image” perspective. This form of Realism does not rest on the presumed iniquity of the human race—original sin in one form or another—but on the nature of world politics as an anarchic realm:

Each state pursues its own interests, however defined, in ways it judges best. Force is a means of achieving the external ends of states because there exists no consistent, reliable process of reconciling the conflicts of interests that inevitably arise among similar units in a condition of anarchy. (p. 238)\footnote{11}

Even well-intentioned statesmen find that they must use or threaten force to attain their objectives.

Since the actions of states are conceived of as resulting from the nature of international politics, the paramount theoretical task for Realists is to create a systemic explanation of international politics. In a systemic theory, as Waltz explains it, the propositions of the theory specify relationships between certain aspects of the system and actor behavior (1979:67–73). Waltz’s third-image Realism, for instance, draws connections between the distribution of power in a system and the actions of states: small countries will behave differently than large ones, and in a balance of power system, alliances can be expected to shift in response to changes in power relationships. Any theory will, of course, take into account the attributes of actors, as well as features of the system itself. But the key distinguishing characteristic of a systemic theory is that the internal attributes of actors are given by assumption rather than treated as variables. Changes in actor behavior, and system outcomes, are explained not on
the basis of variations in these actor characteristics, but on the basis of changes in the attributes of the system itself. A good example of such a systemic theory is microeconomic theory in its standard form. It posits the existence of business firms, with given utility functions (such as profit maximization), and attempts to explain their behavior on the basis of environmental factors such as the competitiveness of markets. It is systemic because its propositions about variations in behavior depend on variations in characteristics of the system, not of the units (Waltz 1979:89–91, 93–95, 98).

To develop a systemic analysis, abstraction is necessary: one has to avoid being distracted by the details and vagaries of domestic politics and other variables at the level of the acting unit. To reconstruct a systemic research program, therefore, Structural Realists must devise a way to explain state behavior on the basis of systemic characteristics, and to account for outcomes in the same manner. This needs to be a coherent explanation, although it need not tell us everything we would like to know about world politics.

Waltz's formulation of Structural Realism as a systemic theory seeks to do this by developing a concept not explicitly used by Morgenthau or Thucydides: the *structure* of the international system. Two elements of international structure are constants: (1) the international system is anarchic rather than hierarchic, and (2) it is characterized by interaction among units with similar functions. These are such enduring background characteristics that they are constitutive of what we mean by "international politics." The third element of structure, the distribution of capabilities across the states in the system, varies from system to system, and over time. Since it is a variable, this element—the distribution of "power"—takes on particular importance in the theory. The most significant capabilities are those of the most powerful actors. Structures "are defined not by all of the actors that flourish within them but by the major ones" (Waltz 1979:93).

According to Waltz, structure is the principal determinant of outcomes at the systems level: structure encourages certain actions and discourages others. It may also lead to unintended consequences, as the ability of states to obtain their objectives is constrained by the power of others (1979:104–111).

For Waltz, understanding the structure of an international system allows us to explain patterns of state behavior, since states determine their interests and strategies on the basis of calculations about their own positions in the system. The link between system structure and actor behavior is forged by the rationality assumption, which enables the theorist to predict that leaders will respond to the incentives and constraints imposed by their environments. Taking rationality as a constant permits one to attribute variations in state behavior to variations in characteristics of the international system. Otherwise, state behavior might have to be accounted for by variations in the calculating ability of states; in that case, the systemic focus of Structural Realism (and much of its explanatory power) would be lost. Thus the rationality assumption—as we will see in examining Waltz's balance of power theory—is essential to the theoretical claims of Structural Realism.15

The most parsimonious version of a structural theory would hold that any international system has a single structure of power. In such a conceptualization, power resources are homogeneous and fungible: they can be used to achieve results on any of a variety of issues without significant loss of efficacy. Power in politics becomes like money in economics: "in many respects, power and influence play the same role in international politics as money does in a market economy" (Wollers 1962:105).

In its strong form, the Structural Realist research program is similar to that of microeconomics. Both use the rationality assumption to permit inferences about actor behavior to be made from system structure. The Realist definition of interests in terms of power and position is like the economist's assumption that firms seek to maximize profits: it provides the utility function of the actor. Through these assumptions, actor characteristics become constant rather than variable, and systemic theory becomes possible.16 The additional assumption of power fungibility simplifies the theory further: on the basis of a single characteristic of the international system (overall power capabilities), multiple inferences can be drawn about actor behavior and outcomes. "Foreknowledge"—that aspiration of all theory—is thereby attained (Eckstein 1975:88–89). As we will see below, pure Structural Realism provides an insufficient basis for explaining state interests and behavior, even when the rationality assumption is accepted; and the fungibility assumption is highly questionable. Yet the Structural Realist research program is an impressive intellectual achievement: an elegant, parsimonious, deductively rigorous
instrument for scientific discovery. The anomalies that it generates are more interesting than its own predictions; but as Lakatos emphasizes, it is the exploration of anomalies that moves science forward.

Richard K. Ashley has recently argued that Structural Realism—which he calls “technical realism”—actually represents a regression from the classical Realism of Herz or Morgenthau. In his view, contemporary Realist thinkers have forgotten the importance of subjective self-reflection, and the dialectic between subjectivity and objectivity, which are so important in the writings of “practical,” or “classical” realists such as Thucydides and Morgenthau. Classical Realism for Ashley is interpretive: “a practical tradition of statesmen is the real subject whose language of experience the interpreter tries to make his own” (1981:221). It is self-reflective and nondeterministic. It treats the concept of balance of power as a dialectical relation: not merely as an objective characterization of the international system but also as a collectively recognized orienting scheme for strategic action. Classical Realism encompasses the unity of opposites, and draws interpretive insight from recognizing the dialectical quality of human experience. Thus its proponents understand that the state system is problematic, and that “strategic artistry” is required to keep it in existence (Ashley 1982:22).

The problem with Classical Realism is that it is difficult to distinguish what Ashley praises as dialectical insight from a refusal to define concepts clearly and consistently, or to develop a systematic set of propositions that could be subjected to empirical tests. Structural Realism seeks to correct these flaws, and thus to construct a more rigorous theoretical framework for the study of world politics, while drawing on the concepts and insights of the older Realism. Structural Realism, as embodied particularly in the work of Waltz, is more systematic and logically more coherent than that of its Classical Realist predecessors. By its own standards, Structural Realism is, in Ashley’s words, “a progressive scientific redemption of classical realism” (Ashley 1982:25). That is, it sees itself, and Classical Realism, as elements of a continuous research tradition.

Ashley complains that this form of Realism objectifies reality, and that in particular it regards the state as unproblematic. This leads, in his view, to some pernicious implications: that the interests expressed by dominant elites must be viewed as legitimate, that economic rationality is the highest form of thought, and that individuals are not responsible for the production of insecurity (1982:34–41). But Structural Realists need not make any of these claims. It is true that Structural Realism seeks to understand the limits of, and constraints on, human action in world politics. It emphasizes the strength of these constraints, and in that sense could be considered “conservative.” But an analysis of constraints, far from implying an acceptance of the status quo, should be seen as a precondition to sensible attempts to change the world. To be self-reflective, human action must take place with an understanding of the context within which it occurs. Structural Realists can be criticized, as we will see, for paying insufficient attention to norms, institutions, and change. But this represents less a fault of Structural Realism as such than a failure of some of its advocates to transcend its categories. Structural Realism’s focus on systemic constraints does not contradict classical Realism’s concern with action and choice. On the contrary, Classical Realism’s emphasis on praxis helps us to understand the origins of Structural Realism’s search for systematic understanding, and—far from negating the importance of this search—makes it seem all the more important.

I have argued thus far that Structural Realism is at the center of contemporary international relations theory in the United States; that it constitutes an attempt to systematize Classical Realism; and that its degree of success as a theory can be legitimately evaluated in part according to standards such as those laid down by Lakatos, and in part through evaluation of its capacity to generate insightful interpretations of international political behavior. Two distinct tests, each reflecting one aspect of this dualistic evaluative standard, can be devised to evaluate Structural Realism as a research program for international relations:

1. How “fruitful” is the Realist paradigm for puzzle-solving and interpretation of world politics (Toulmin 1963)? That is, does current work in the Realist tradition make us see issues more clearly, or provide answers to formerly unsolved puzzles? Realism was designed to provide insights into such issues, and if it remains a live tradition, should continue to do so.

2. Does Realism meet the standards of a scientific research program as enunciated by Lakatos? To answer this question, it is important to remind ourselves that the hard core of a research program is irrefutable within the terms of the paradigm. When anomalies arise that appear to challenge Realist assumptions, the task of Realist analysts is to create
auxiliary theories that defend them. These theories permit explanation of anomalies consistent with Realist assumptions. For Lakatos, the key question about a research program concerns whether the auxiliary hypotheses of Realism are “progressive.” That is, do they generate new insights, or predict new facts? If not, they are merely exercises in “patching up” gaps or errors on an ad hoc basis, and the research program is degenerative.

Realism cannot be judged fairly on the basis of only one set of standards. Section II addresses the question of fruitfulness by examining works in the central area of Realist theory: the study of conflict, bargaining, and war. Section II then judges Realism by the more difficult test of Lakatos, which (as noted above) is better at asking trenchant questions than at defining a set of standards appropriate to social science. We will see that in one sense Realism survives these tests, since it still appears as a good starting point for analysis. But it does not emerge either as a comprehensive theory or as a progressive research program in the sense employed by Lakatos. Furthermore, it has difficulty interpreting issues, and linkages among issues, outside of the security sphere: it can even be misleading when applied to these issues without sufficient qualification. It also has little to say about the crucially important question of peaceful change. The achievements of Realism, and the prospect that it can be modified further to make it even more useful, should help students of world politics to avoid unnecessary self-deprecation. Yet they certainly do not justify complacency.

II. PROGRESS WITHIN THE REALISM PARADIGM:
THREE ACHIEVEMENTS

The fruitfulness of contemporary Realist analysis is best evaluated by considering some of the finest work in the genre. Poor scholarship can derive from even the best research program; only the most insightful work reveals the strengths as well as the limits of a theoretical approach. In this section I will consider three outstanding examples of works that begin, at least, from Realist concerns and assumptions: Waltz’s construction of balance of power theory in *Theory of International Politics* (1979); the attempt by Glenn Snyder and Paul Diesing in *Conflict Among Nations* (1977) to apply formal game-theoretic models of bargaining to sixteen case studies of major-power crises during the seventy-five years between Fashoda and the Yom Kippur “alert crisis” of 1973; and Robert Gilpin’s fine recent work, *War and Change in World Politics* (1981). These works are chosen to provide us with one systematic attempt to develop structural Realist theory, one study of bargaining in specific cases, and one effort to understand broad patterns of international political change. Other recent works could have been chosen instead, such as three books on international conflict and crises published in 1980 or 1981 (Brecher 1980; Bueno de Mesquita 1981; Lebow 1981), or the well-known works by Nazli Choucri and Robert C. North (1975) or by Alexander George and Richard Smoke (1974). But there are limits on what can be done in a single chapter of limited size.

**Balance of Power Theory: Waltz**

Waltz has explicated balance of power theory as a central element in his Structural Realist synthesis: “If there is any distinctively political theory of international politics, balance of power theory is it” (1979:117). The realization that balances of power periodically form in world politics, is an old one, as are attempts to theorize about it. The puzzle that Waltz addresses is how to “cut through such confusion” as has existed about it: that is, in Kuhn’s words, how to “achieve the anticipated in a new way” (1962:36).

Waltz attacks this problem by using the concept of structure, which he has carefully developed earlier in the book, and which he also employs to account for the dreary persistence of patterns of international action (1979:66–72). Balance of power theory applies to “anarchical” realms, which are formally unorganized and in which, therefore, units have to worry about their survival: “Self-help is necessarily the principle of action in an anarchic order” (p.111). In Waltz’s system, states (which are similar to one another in function) are the relevant actors; they use external as well as internal means to achieve their goals. Relative capabilities are (as we saw above) the variable element of structure; as they change, we expect coalitional patterns or patterns of internal effort to be altered as well. From his assumptions, given the condition for the theory’s operation (self-help), Waltz deduces “the expected outcome; namely, the formation
of balances of power” (p. 118). His solution to the puzzle that he has set for himself is carefully formulated and ingenious.

Nevertheless, Waltz’s theory of the balance of power encounters some difficulties. First, it is difficult for him to state precisely the conditions under which coalitions will change. He only forecasts that balances of power will periodically recur. Indeed, his theory is so general that it hardly meets the difficult tests that he himself establishes for theory. In chapter 1 we are told that to test a theory, one must “device a number of distinct and demanding tests” (1979:13). But such tests are not proposed for balance of power theory: “Because only a loosely defined and inconstant condition of balance is predicted, it is difficult to say that any given distribution of power falsifies the theory” (p. 124). Thus rather than applying demanding tests, Waltz advises that we “should seek confirmation through observation of difficult cases” (p. 125, emphasis added). In other words, he counsels that we should search through history to find examples that conform to the predictions of the theory; he then proclaims that “these examples tend to confirm the theory” (p. 125). Two pages later, Waltz appears to change his view, admitting that “we can almost always find confirming cases if we look hard.” We should correct for this by looking “for instances of states conforming to common international practices even though for internal reasons they would prefer not to” (p. 127). But Waltz is again making an error against which he warns us. He is not examining a universe of cases, in all of which states would prefer not to conform to “international practice,” and asking how often they nevertheless do conform. Instead, he is looking only at the latter cases, chosen because they are consistent with his theory. Building grand theory that meets Popperian standards of scientific practice is inherently difficult; even the best scholars, such as Waltz, have trouble simultaneously saying what they want to say and abiding by their canons of scientific practice.

Waltz’s theory is also ambiguous with respect to the status of three assumptions that are necessary to a strong form of Structural Realism. I have already mentioned the difficult problem of whether a structural theory must (implausibly) assume fungibility of power resources. Since this problem is less serious with respect to balance of power theory than in a broader context, I will not pursue it here, but will return to it in

Section III. Yet Waltz is also, in his discussion of balances of power, unclear on the questions of rationality and interests.

Waltz argues that his assumptions do not include the rationality postulate: “The theory says simply that if some do relatively well, others will emulate them or fall by the wayside” (p. 118). This evolutionary principle, however, can hold only for systems with many actors, experiencing such severe pressure on resources that many will disappear over time. Waltz undermines this argument by pointing out later (p. 137) that “the death rate for states is remarkably low.” Furthermore, he relies explicitly on the rationality principle to show that bipolar balances must be stable. “Internal balancing,” he says, “is more reliable and precise than external balancing. States are less likely to misjudge their relative strengths than they are to misjudge the strength and reliability of opposing coalitions” (p. 168). I conclude that Waltz does rely on the rationality argument, despite his earlier statement to the contrary.

The other ambiguity in Waltz’s balance of power theory has to do with the interests, or motivations, of states. Waltz recognizes that any theory of state behavior must ascribe (by assumption) some motivations to states, just as microeconomic theory ascribes motivations to firms. It is not reductionist to do so as long as these motivations are not taken as varying from state to state as a result of their internal characteristics. Waltz specifies such motivations: states “at a minimum, seek their own preservation, and at a maximum, drive for universal domination” (p. 118).

For his balance of power theory to work, Waltz needs to assume that states seek self-preservation, since if at least some major states did not do so, there would be no reason to expect that roughly equivalent coalitions (i.e., “balances of power”) would regularly form. The desire for self-preservation makes states that are behind in a struggle for power try harder, according to Waltz and leads states allied to a potential hegemon to switch coalitions in order to construct balances of power. Neither of these processes on which Waltz relies to maintain a balance—intensified effort by the weaker country in a bipolar system and coalition formation against potentially dominant states in a multipolar system—could operate reliably without this motivation.

The other aspect of Waltz’s motivational assumption—that states “at a maximum, drive for universal domination,” is reminiscent of the im-
plication of Realists such as Morgenthau that states seek to “maximize power.” For a third-image Realist theory such as Waltz’s, such an assumption is unnecessary. Waltz’s defense of it is that the balance of power depends on the possibility that force may be used. But this possibility is an attribute of the self-help international system, for Waltz, rather than a reflection of the actors’ characteristics. That some states seek universal domination is not a necessary condition for force to be used.

This ambiguity in Waltz’s analysis points toward a broader ambiguity in Realist thinking: Balance of power theory is inconsistent with the assumption frequently made by Realists that states “maximize power” if power is taken to refer to tangible resources that can be used to induce other actors to do what they would not otherwise do, through the threat or infliction of deprivations. States concerned with self-preservation do not seek to maximize their power when they are not in danger. On the contrary, they recognize a trade-off between aggrandizement and self-preservation; they realize that a relentless search for universal domination may jeopardize their own autonomy. Thus they moderate their efforts when their positions are secure. Conversely, they intensify their efforts when danger arises, which assumes that they were not maximizing them under more benign conditions.

One might have thought that Realists would readily recognize this point, yet they seem drawn against their better judgment to the “power maximization” or “universal domination” hypotheses. In part, this may be due to their anxiety to emphasize the significance of force in world politics. Yet there may be theoretical as well as rhetorical reasons for their ambivalence. The assumption of power maximization makes possible strong inferences about behavior that would be impossible if we assumed only that states “sometimes” or “often” sought to aggrandize themselves. In that case, we would have to ask about competing goals, some of which would be generated by the internal social, political, and economic characteristics of the countries concerned. Taking into account these competing goals relegates Structural Realism to the status of partial, incomplete theory.

Waltz’s contribution to the study of world politics is conceptual. He helps us think more clearly about the role of systemic theory, the explanatory power of structural models, and how to account deductively for the recurrent formation of balances of power. He shows that the international system shapes state behavior as well as vice versa. These are major contributions. But Waltz does not point out “new ways of seeing” international relations that point toward major novelties. He reformulates and systematizes Realism, and thus develops what I have called Structural Realism, consistently with the fundamental assumptions of his classical predecessors.

Game Theory, Structure, and Bargaining: Snyder and Diesing

Game theory has yielded some insights into issues of negotiations, crises, and limited war, most notably in the early works of Thomas Schelling (1960). Snyder and Diesing’s contribution to this line of analysis, as they put it, is to “distinguish and analyze nine different kinds of bargaining situations, each one a unique combination of power and interest relations between the bargainers, each therefore having its own dynamics and problems” (1977:181–182). They employ their game-theoretic formulations of these nine situations, within an explicit structural context, to analyze sixteen historical cases.

This research design is consistent with the hard core of Realism. Attention is concentrated on the behavior of states. In the initial statement of the problem, the rationality assumption, in suitably modest form, is retained: each actor attempts “to maximize expected value across a given set of consistently ordered objectives, given the information actually available to the actor or which he could reasonably acquire in the time available for decision” (p.181). Interests are defined to a considerable extent in terms of power: that is, power factors are built into the game structure. In the game of “Protector,” for instance, the more powerful state can afford to “go it alone” without its ally, and thus has an interest in doing so under certain conditions, whereas its weaker partner cannot (pp.145–147). Faced with the game matrix, states, as rational actors, calculate their interests and act accordingly. The structure of world politics, as Waltz defines it, is reflected in the matrices and becomes the basis for action.

If structural Realism formed a sufficient basis for the understanding of international crises, we could fill in the entries in the matrices solely
on the basis of states’ positions in the international system, given our knowledge of the fact that they perform “similar functions,” including the need to survive as autonomous entities. Interests would indeed be defined in terms of power. This would make game theory a powerful analytic tool, which could even help us predict certain outcomes. Where the game had no unique solution (because of strategic indeterminacy), complete predictability of outcomes could not be achieved, but our expectations about the range of likely action would have been narrowed.

Yet Snyder and Diesing find that even knowledge of the values and goals of top leaders could not permit them to determine the interests of about half the decision-making units in their cases. In the other cases, one needed to understand intragovernmental politics, even when one ignored the impact of wider domestic political factors (pp. 510–511). The “internal-external interaction” is a key to the understanding of crisis bargaining.

As Snyder and Diesing make their analytical framework more complex and move into detailed investigation of their cases, their focus shifts toward concern with cognition and with the effects on policy of ignorance, misperception, and misinformation. In my view, the most creative and insightful of their chapters use ideas developed largely by Robert Jervis (1976) to analyze information processing and decision-making. These chapters shift the focus of attention away from the systemic-level factors reflected in the game-theoretic matrices, toward problems of perception, personal bias, and group decision-making (Snyder and Diesing 1977, chapters 4 and 5).

Thus Snyder and Diesing begin with the hard core of Realism, but their most important contributions depend on their willingness to depart from these assumptions. They are dissatisfied with their initial game-theoretic classificatory scheme. They prefer to explore information processing and decision-making, without a firm deductive theory on which to base their arguments, rather than merely to elucidate neat logical typologies.

Is the work of Snyder and Diesing a triumph of Realism or a defeat? At this point in the argument, perhaps the most that can be said is that it indicates that work in the Realist tradition, analyzing conflict and bargaining with the concepts of interests and power, continues to be fruitful, but it does not give reason for much confidence that adhering

strictly to Realist assumptions will lead to important advances in the field.

Cycles of Hegemony and War: Gilpin

In War and Change in World Politics, Gilpin uses Realist assumptions to reinterpret the last 2400 years of Western history. Gilpin assumes that states, as the principal actors in world politics, make cost-benefit calculations about alternative courses of action. For instance, states attempt to change the international system as the expected benefits of doing exceed the costs. Thus, the rationality assumption is applied explicitly, in a strong form, although it is relaxed toward the end of the book (1981b:77, 202). Furthermore, considerations of power, relative to the structure of the international system, are at the core of the calculations made by Gilpin’s states: “the distribution of power among states constitutes the principal form of control in every international system” (p. 29). Thus Gilpin accepts the entire hard core of the classical Realist research program as I have defined it.¹⁹

Gilpin sees world history as an unending series of cycles: “The conclusion of one hegemonic war is the beginning of another cycle of growth, expansion, and eventual decline” (p. 210). As power is redistributed, power relations become inconsistent with the rules governing the system and, in particular, the hierarchy of prestige; war establishes the new hierarchy of prestige and “thereby determines which states will in effect govern the international system” (p. 33).

The view that the rules of a system, and the hierarchy of prestige, must be consistent with underlying power realities is a fundamental proposition of Realism, which follows from its three core assumptions. If states, as the central actors of international relations, calculate their interests in terms of power, they will seek international rules and institutions that are consistent with these interests by maintaining their power. Waltz’s conception of structure helps to systematize this argument, but it is essentially static. What Gilpin adds is a proposed solution to the anomalies (for static Realism) that institutions and rules can become inconsistent with power realities over time, and that hegemonic states eventually decline. If, as Realists argue, “the strong do what they can and the weak suffer what they must” (Thucydides, Book V,
paragraph 90 [Chapter XVII, Modern Library Edition, p. 331]), why should hegemons ever lose their power? We know that rules do not always reinforce the power of the strong and that hegemons do sometimes lose their hold, but static Realist theory cannot explain this.

In his attempt to explain hegemonic decline, Gilpin formulates a “law of uneven growth”:

According to Realism, the fundamental cause of wars among states and changes in international systems is the uneven growth of power among states. Realist writers from Thucydides and MacKinder to present-day scholars have attributed the dynamics of international relations to the fact that the distribution of power in an international system shifts over a period of time; this shift results in profound changes in the relationships among the states and eventually changes in the nature of the international system itself. (p. 94)

This law, however, restates the problem without resolving it. In accounting for this pattern, Gilpin relies on three sets of processes. One has to do with increasing, and then diminishing, marginal returns from empire. As empires grew, “the economic surplus had to increase faster than cost of war” (p. 115). Yet sooner or later, diminishing returns set in: “the law of diminishing returns has universal applicability and causes the growth of every society to describe an S-shaped curve” (p. 159). Secondly, hegemonic states tend increasingly to consume more and invest less; Gilpin follows the lead of Carlo Cipolla in viewing this as a general pattern in history (Cipolla 1970). Finally, hegemonic states decline because of a process of diffusion of technology to others. In U.S. Power and Multinational Corporation (1975), Gilpin emphasized this process as contributing first to the decline of Britain, then in the 1970s to that of the United States. In War and Change he makes the argument more general:

Through a process of diffusion to other states, the dominant power loses the advantage on which its political, military, or economic success has been based. Thus, by example, and frequently in more direct fashion, the dominant power helps to create challenging powers. (p. 176)

This third argument is systemic, and, therefore, fully consistent with Waltz’s Structural Realism. The other two processes, however, reflect the operation of forces within the society as well as international forces. A hegemonic power may suffer diminishing returns as a result of the expansion of its defense perimeter and the increased military costs that result (Gilpin 1981b:191; Luttwak 1976). But whether diminishing returns set in also depends on internal factors such as technological inventiveness of members of the society and the institutions that affect incentives for innovation (North 1981). The tendency of hegemonic states to consume more and invest less is also, in part, a function of their dominant positions in the world system: they can force costs of adjustment to change onto others, at least for some time. But it would be hard to deny that the character of the society affects popular tastes for luxury, and, therefore, the tradeoffs between guns and butter that are made. Eighteenth-century Saxony and Prussia were different in this regard; so are contemporary America and Japan. In Gilpin’s argument as in Snyder and Diezeng’s, the “external-internal interaction” becomes a crucial factor in explaining state action, and change.

Gilpin explicitly acknowledges his debt to Classical Realism: “In honesty, one must inquire whether or not twentieth-century students of international relations know anything that Thucydides and his fifth-century compatriots did not know about the behavior of states” (p. 227). For Gilpin as for Thucydides, changes in power lead to changes in relations among states: the real cause of the Peloponnesian War, for Thucydides, was the rise of the power of Athens and the fear this evoked in the Spartans and their allies. Gilpin has generalized the theory put forward by Thucydides to explain the Peloponnesian War, and has applied it to the whole course of world history:

Dis-equilibrium replaces equilibrium, and the world moves toward a new round of hegemonic conflict. It has always been thus and always will be, until men either destroy themselves or learn to develop an effective mechanism of peaceful change. (p. 210)

This Thucydides-Gilpin theory is a systemic theory of change only in a limited sense. It explains the reaction to change systematically, in a rationalistic, equilibrium model. Yet at a more fundamental level, it does not account fully for the sources of change. As we saw above, although it is insightful about systemic factors to hegemonic decline, it also has to rely on internal processes to explain the observed effects. Furthermore, it does not account well for the rise of hegemons in the first place, or for the fact that certain contenders emerge rather than others. Gilpin’s
systemic theory does not account for the extraordinary bursts of energy that occasionally catapult particular countries into dominant positions on the world scene. Why were the Athenians, in words that Thucydides attributes to Corinthian envoys to Sparta, “addicted to innovation,” whereas the Spartans were allegedly characterized by a “total want of invention” (Thucydides, Book I, paragraph 70 [Chapter III, Modern Library edition, p. 40])? Like other structural theories, Gilpin’s theory underpredicts outcomes. It contributes to our understanding but (as its author recognizes) does not explain change.

This is particularly true of peaceful change, which Gilpin identifies as a crucial issue: “The fundamental problem of international relations in the contemporary world is the problem of peaceful adjustment to the consequences of the uneven growth of power among states, just as it was in the past” (p. 230).

Gilpin’s book, like much contemporary American work on international politics, is informed and propelled by concern with peaceful change under conditions of declining hegemony. Gilpin sympathetically discusses E. H. Carr’s “defense of peaceful change as the solution to the problem of hegemonic war,” written just before World War II (Gilpin, p. 206; Carr 1939/1946). Yet peaceful change does not fit easily into Gilpin’s analytical framework, since it falls, by and large, into the category of “interactions change,” which does not entail alteration in the overall hierarchy of power and prestige in a system, and Gilpin deliberately avoids focusing on interactions change (p. 44). Yet after one puts down War and Change, the question of how institutions and rules can be developed within a given international system, to reduce the probability of war and promote peaceful change, looms even larger than it did before.

Thus Gilpin’s sophisticated adaptation of Classical Realism turns us away from Realism. Classical Realism, with its philosophical roots in a tragic conception of the human condition, directs our attention in the twentieth century to the existential situation of modern humanity, doomed apparently to recurrent conflict in a world with weapons that could destroy life on our planet. But Realism, whether classical or structural, has little to say about how to deal with that situation, since it offers few insights into the international rules and institutions that people invent to reduce risk and uncertainty in world affairs, in the hope of ameliorating the security dilemma.25 Morgenthau put his hopes in diplo-

macy (1966 ch. 32). This is a practical art, far removed from the abstractions of structural Realism. But diplomacy takes place within a context of international rules, institutions, and practices, which affect the incentives of the actors (Keohane 1982b). Gilpin realizes this, and his gloomy argument—hardly alleviated by a more optimistic epilogue—helps us to understand their importance, although it does not contribute to an explanation of their creation or demise.

Conclusions

Realism, as developed through a long tradition dating from Thucydides, continues to provide the basis for valuable research in international relations. This point has been made by looking at writers who explicitly draw on the Realist tradition, and it can be reinforced by briefly examining some works of Marxist scholars. If they incorporate elements of Realism despite their general antipathy to its viewpoint, our conclusion that Realism reflects enduring realities of world politics will be reinforced.

For Marxists, the fundamental forces affecting world politics are those of class struggle and uneven development. International history is dynamic and dialectical rather than cyclical. The maneuvers of states, on which Realism focuses, reflect the stages of capitalist development and the contradictions of that development. Nevertheless, in analyzing the surface manifestations of world politics under capitalism, Marxists adopt similar categories to those of Realists. Power is crucial; world systems are periodically dominated by hegemonic powers wielding both economic and military resources.

Lenin defined imperialism differently than do the Realists, but he analyzed its operation in part as a Realist would, arguing that “there can be no other conceivable basis under capitalism for the division of spheres of influence, of interests, of colonies, etc. than a calculation of the strength of the participants in the division.” (Lenin 1916/1939:119).

Immanuel Wallerstein provides another example of my point. He goes to some effort to stress that modern world history should be seen as the history of capitalism as a world system. Apart from “relatively minor accidents” provided by geography, peculiarities of history, or luck—which give one country an edge over others at crucial historical junctures—
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"it is the operations of the world-market forces which accentuate the differences, institutionalize them, and make them impossible to surmount over the long run" (1979:21). Nevertheless, when his attention turns to particular epochs, Wallerstein emphasizes hegemony and the role of military force. Dutch economic hegemony in the seventeenth century was destroyed, in quintessential Realist fashion, not by the operation of the world-market system, but by the force of British and French arms (Wallerstein 1980:38–39).

The insights of Realism are enduring. They cross ideological lines. Its best contemporary exponents use Realism in insightful ways. Waltz has systematized the basic assumptions of Classical Realism in what I have called Structural Realism. Snyder and Diesing have employed this framework for the analysis of bargaining; Gilpin has used the classical arguments of Thucydides to explore problems of international change. For all of these writers, Realism fruitfully focuses attention on fundamental issues of power, interests, and rationality. But as we have seen, many of the most interesting questions raised by these authors cannot be answered within the Realist framework.

III. EXPLANATIONS OF OUTCOMES FROM POWER: HYPOTHESES AND ANOMALIES

A Structural Realist theory of interests could be used both for explanation and for prescription. If we could deduce a state’s interests from its position in the system, via the rationality assumption, its behavior could be explained on the basis of systemic analysis. Efforts to define the national interest on an a priori basis, however, or to use the concept for prediction and explanation, have been unsuccessful. We saw above that the inability to define interests independently of observed state behavior robbed Snyder and Diesing’s game-theoretical matrices of predictive power. More generally, efforts to show that external considerations of power and position play a dominant role in determining the “national interest” have failed. Even an analyst as sympathetic to Realism as Stephen D. Krasner has concluded, in studying American foreign economic policy, that the United States was “capable of defining its own autonomous goals” in a nonlogical manner (1978a:333). That is, the systemic constraints emphasized by Structural Realism were not binding on the American government during the first thirty years after the Second World War.

Sophisticated contemporary thinkers in the Realist tradition, such as Gilpin, Krasner, and Waltz, understand that interests cannot be derived, simply on the basis of rational calculation, from the external positions of states, and that this is particularly true for great powers, on which, ironically, Structural Realism focuses its principal attentions (Gilpin 1975; Waltz 1967). Realist analysis has to retreat to a “fall-back position”: that, given state interests, whose origins are not predicted by the theory, patterns of outcomes in world politics will be determined by the overall distribution of power among states. This represents a major concession for systemically oriented analysts, which it is important not to forget. Sensible Realists are highly cognizant of the role of domestic politics and of actor choices within the constraints and incentives provided by the system. Since systemic theory cannot predict state interests, it cannot support deterministic conclusions (Sprout and Sprout 1971:73–77). This limitation makes it both less powerful as a theory, and less dangerous as an ideology. Despite its importance, it cannot stand alone.

When realist theorists say that, given interests, patterns of outcomes will be determined by the overall distribution of power among states, they are using “power” to refer to resources that can be used to induce other actors to do what they would not otherwise do, in accordance with the desires of the power-wielder. “Outcomes” refer principally to two sets of patterns: (1) the results of conflicts, diplomatic or military, that take place between states; and (2) changes in the rules and institutions that regulate relations among governments in world politics. This section focuses on conflicts, since they pose the central puzzles that Realism seeks to explain. Section IV and the Conclusion consider explanations of changes in rules and institutions.

Recent quantitative work seems to confirm that power capabilities (measured not only in terms of economic resources but with political variables added) are rather good predictors of the outcomes of wars. Bueno de Mesquita finds, for example, that countries with what he calls positive “expected utility” (a measure that uses composite capabilities but adjusts them for distance, alliance relationships, and uncertainty) won 179 conflicts while losing only 54 between 1816 and 1974, for a
success ratio of over 75 percent (1981, especially p. 151; Organski and Kugler 1980, ch. 2).

The question of the fungibility of power poses a more troublesome issue. As I have noted earlier (see note 19), Structural Realism is ambiguous on this point; the desire for parsimonious theory impels Realists toward a unitary notion of power as homogeneous and usable for a variety of purposes, but close examination of the complexities of world politics induces caution about such an approach. In his discussion of system structure, for instance, Waltz holds that “the units of an anarchic system are distinguished primarily by their greater or lesser capabilities for performing similar tasks,” and that the distribution of capabilities across a system is the principal characteristic differentiating international-political structures from one another (1979:97, 99). Thus each international political system has one structure. Yet in emphasizing the continued role of military power, Waltz admits that military power is not perfectly fungible: “Differences in strength do matter, although not for every conceivable purpose”; “military power no longer brings political control, but then it never did” (1979:189, 191, emphasis added). This seems to imply that any given international system is likely to have several structures, differing by issue-areas and according to the resources that can be used to affect outcomes. Different sets of capabilities will qualify as “power resources” under different conditions. This leads to a much less parsimonious theory and a much more highly differentiated view of the world, in which what Nye and I called “issue-structure” theories play a major role, and in which military force, although still important, is no longer assumed to be at the top of a hierarchy of power resources (Keohane and Nye 1977, chs. 3 and 6).

The status in a Structural Realist theory of the fungibility assumption affects both its power and the incidence of anomalies. A strong version of Structural Realism that assumed full fungibility of power across issues would predict that when issues arise between great powers and smaller states, the great powers should prevail. This has the advantage of generating a clear prediction and the liability of being wrong much of the time. Certainly it does not fit the American experience of the last two decades. The United States lost a war in Vietnam and was for more than a year unable to secure the return of its diplomats held hostage in Iran.

Small allies such as Israel, heavily dependent on the United States, have displayed considerable freedom of action. In the U.S.-Canadian relationship of the 1950s and 1960s, which was virtually free of threats of force, outcomes of conflicts often favored the Canadian as the American position, although this was not true for relations between Australia and the United States (Keohane and Nye 1977, ch. 7).

In view of power theory in social science, the existence of these anomalies is not surprising. As James G. March observes, “there appears to be general consensus that either potential power is different from actually exerted power or that actually exerted power is variable” (1966:57). That is, what March calls “basic force models,” which rely like Realist theory, on measurable indices of power, are inadequate tools for either prediction or explanation. They are often valuable in suggesting long-term trends and patterns, but they do not account well for specific outcomes: the more that is demanded of them, the less well they are likely to perform.

Lakatos’s discussion of scientific research programs leads us to expect that, when confronted with anomalies, theorists will create auxiliary theories that preserve the credibility of their fundamental assumptions. Thus it is not surprising that Realists committed to the fungibility assumption have devised auxiliary hypotheses to protect its “hard core” against challenge. One of these is what David Baldwin calls the “conversion-process explanation” of unanticipated outcomes: “The would-be wielder of power is described as lacking in skill and/or the ‘will’ to use his power resources effectively: ‘The Arabs had the tanks but didn’t know how to use them.’ ‘The Americans had the bombs but lacked the will to use them.’” (1979:163–164)

The conversion-process explanation is a classic auxiliary hypothesis, since it is designed to protect the assumption that power resources are homogeneous and fungible. If we were to accept the conversion-process account, we could continue to believe in a single structure of power, even if outcomes do not favor the “stronger” party. This line of argument encounters serious problems, however, when it tries to account for the discrepancy between anticipated and actual outcomes by the impact of intangible resources (such as intelligence, training, organization, foresight) not recognized until after the fact. The problem with this argument lies
in its post hoc quality. It is theoretically degenerate in Lakatos's sense, since it does not add any explanatory power to structural Realist theory, but merely "explains away" uncomfortable facts.

Thus what March says about "force activation models" applies to Structural Realist theories when the conversion-process explanation relies upon sources of power that can be observed only after the events to be explained have taken place:

If we observe that power exists and is stable and if we observe that sometimes weak people seem to triumph over strong people, we are tempted to rely on an activation hypothesis to explain the discrepancy. But if we then try to use the activation hypothesis to predict the results of social-choice procedures, we discover that the data requirements of 'plausible' activation models are quite substantial. As a result, we retreat to what are essentially degenerate forms of the activation model—retaining some of the form but little of the substance. This puts us back where we started, looking for some device to explain our failures in prediction. (1966:61).

A second auxiliary hypothesis designed to protect the fungibility assumption must be taken more seriously: that discrepancies between power resources and outcomes are explained by an asymmetry of motivation in favor of the objectively weaker party. Following this logic, John Harsanyi has proposed the notion of power "in a schedule sense," describing how various resources can be translated into social power. An actor with intense preferences on an issue may be willing to use more resources to attain a high probability of a favorable result, than an actor with more resources but lower intensity. As a result, outcomes may not accurately reflect underlying power resources (Harsanyi 1962).

To use this insight progressively rather than in a degenerate way, Realist theory needs to develop indices of intensity of motivation that can be measured independently of the behavior that theorists are trying to explain. Russett, George, and Bueno de Mesquita are among the authors who have attempted, with some success, to do this (Russett 1963; George et al. 1971; Bueno de Mesquita 1981). Insofar as motivation is taken simply as a control, allowing us to test the impact of varying power configurations more successfully, Harsanyi's insights can be incorporated into structural Realist theory. If it became a key variable, however, the effect could be to transform a systemic theory into a decision-making one.

An alternative approach to relying on such auxiliary hypotheses is to relax the fungibility assumption itself. Failures of great powers to control smaller ones could be explained on the basis of independent evidence that in the relevant issue-areas, the states that are weaker on an overall basis have more power resources than their stronger partners, and that the use of power derived from one area of activity to affect outcomes in other areas (through "linkages") is difficult. Thus Saudi Arabia can be expected to have more impact on world energy issues than on questions of strategic arms control; Israel more influence over the creation of a Palestinian state than on the reconstruction of the international financial and debt regime.

Emphasizing the problematic nature of power fungibility might help to create more discriminating power models, but it will not resolve the inherent problems of power models, as identified by March and others. Furthermore, at the limit, to deny fungibility entirely risks a complete disintegration of predictive power. Baldwin comes close to this when he argues that what he calls the "policy-contingency framework" of an influence attempt must be specified before power explanations are employed. If we defined each issue as existing within a unique "policy-contingency framework," no generalizations would be possible. Waltz could reply, if he accepted Baldwin's view of power, that all of world politics should be considered a single policy-contingency framework, characterized by anarchy and self-help. According to this argument, the parsimony gained by assuming the fungibility of power would compensate for the marginal mispredictions of such a theory.

This is a crucial theoretical issue, which should be addressed more explicitly by theorists of world politics. In my view, the dispute cannot be resolved a priori. The degree to which power resources have to be disaggregated in a structural theory depends both on the purposes of the theory and on the degree to which behavior on distinct issues is linked together through the exercise of influence by actors. The larger the domain of a theory, the less accuracy of detail we expect. Since balance of power theory seeks to explain large-scale patterns of state
action over long periods of time, we could hardly expect the precision from it that we demand from theories whose domains have been narrowed.

This assertion suggests that grand systemic theory can be very useful as a basis for further theoretical development in international relations, even if the theory is lacking in precision, and it therefore comprises part of my defense of the Realist research program as a foundation on which scholars should build. Yet this argument requires immediate qualification.

Even if a large-scale theory can be developed and appropriately tested, its predictions will be rather gross. To achieve a more finely tuned understanding of how resources affect behavior in particular situations, one needs to specify the policy-contingency framework more precisely. The domain of theory is narrowed to achieve greater precision. Thus the debate between advocates of parsimony and proponents of contextual subtlety resolves itself into a question of stages, rather than an either/or choice. We should seek parsimony first, then add complexity while monitoring the adverse effects that this has on the predictive power of our theory: its ability to make significant inferences on the basis of limited information.

To introduce greater complexity into an initially spare theoretical structure, the conception of an issue-area, developed many years ago by Robert A. Dahl (1961) and adapted for use in international relations by James N. Rosenau (1966), is a useful device. Having tentatively selected an area of activity to investigate, the analyst needs to delineate issue-areas at various levels of aggregation. Initial explanations should seek to account for the main features of behavior at a high level of aggregation—such as the international system as a whole—while subsequent hypotheses are designed to apply only to certain issue-areas.

In some cases, more specific issue-areas are “nested” within larger ones (Aggarwal 1981; Snidal 1981). For instance, North Atlantic fisheries issues constitute a subset of fisheries issues in general, which comprise part of the whole area of oceans policy, or “law of the sea.” In other cases, specific issues may belong to two or more broader issues: the question of passage through straits, for example, involves questions of military security as well as the law of the sea.

Definitions of issue-areas depend on the beliefs of participants, as well as on the purposes of the investigator. In general, however, definitions of issue-areas should be made on the basis of empirical judgments about the extent to which governments regard sets of issues as closely interdependent and treat them collectively. Decisions made on one issue must affect others in the issue-area, either through functional links or through regular patterns of bargaining. These relationships of interdependence among issues may change. Some issue-areas, such as international financial relations, have remained fairly closely linked for decades; others, such as oceans, have changed drastically over the past 35 years (Keohane and Nye 1977, ch. 4, especially pp. 64–65; Simons 1969; Haas 1980).

When a hierarchy of issue-areas has been identified, power-structure models employing more highly aggregated measures of power resources can be compared with models that disaggregate resources by issue-areas. How much accuracy is gained, and how much parsimony lost, by each step in the disaggregation process? In my view, a variegated analysis, which takes some specific “snapshots” by issue-area as well as looking at the broader picture, is superior to either monistic strategy, whether assuming perfect fungibility or none at all.

This approach represents an adaptation of Realism. It preserves the basic emphasis on power resources as a source of outcomes in general, but it unambiguously jettisons the assumption that power is fungible across all of world politics. Disaggregated power models are less parsimonious than more aggregated ones, and they remain open to the objections to power models articulated by March and others. But in one important sense disaggregation is progressive rather than degenerative. Disaggregated models call attention to linkages among issue-areas, and raise the question: under what conditions, and with what effects, will such linkages arise? Current research suggests that understanding linkages systematically, rather than merely describing them on an ad hoc basis, will add significantly to our comprehension of world politics (Oye 1979, 1983; Stein 1980; Töllsen and Willett 1979). It would seem worthwhile, in addition, for more empirical work to be done on this subject, since we know so little about when, and how, linkages are made.

Conclusions

Structural Realism is a good starting-point for explaining the outcomes of conflicts, since it directs attention to fundamental questions of interest and power within a logically coherent and parsimonious theoretical
framework. Yet the ambitious attempt of Structural Realist theory to deduce rational interests from system structure via the rationality postulate has been unsuccessful. Even if interests are taken as given, the attempt to predict outcomes from interests and power leads to ambiguities and incorrect predictions. The auxiliary theory attributing this failure to conversion-processes often entails unfalsifiable tautology rather than genuine explanation. Ambiguity prevails on the question of the fungibility of power: whether there is a single structure of the international system or several. Thus the research program of Realism reveals signs of degeneration. It certainly does not meet Lakatos’s tough standards for progressiveness.

More attention to developing independent measures of intensity of motivation, and greater precision about the concept of power and its relationship to the context of action, may help to correct some of these faults. Careful disaggregation of power-resources by issue-area may help to improve the predictive capability of structural models, at the risk of reducing theoretical parsimony. As I argue in the next section, modified structural models, indebted to Realism although perhaps too different to be considered Realist themselves, may be valuable elements in a multi-level framework for understanding world politics.

Yet to some extent the difficulties encountered by Structural Realism reflect the inherent limitations of structural models, which will not be corrected by mere modifications or the relaxation of assumptions. Domestic politics and decision-making, Snyder and Diesing’s “internal-external interactions,” and the workings of international institutions all play a role, along with international political structure, in affecting state behavior and outcomes. Merely to catalog these factors, however, is not to contribute to theory but rather to compound the descriptive anarchy that already afflicts the field, with too many independent variables, exogenously determined, chasing too few cases. As Waltz emphasizes, the role of unit-level forces can be properly understood only if we comprehend the structure of the international system within which they operate.

IV. BEYOND STRUCTURAL REALISM

Structural Realism helps us to understand world politics as in part a systemic phenomenon, and provides us with a logically coherent theory that establishes the context for state action. This theory, because it is relatively simple and clear, can be modified progressively to attain closer correspondence with reality. Realism’s focus on interests and power is central to an understanding of how nations deal with each other. Its adherents have understood that a systemic theory of international relations must account for state behavior by examining the constraints and incentives provided by the system; for this purpose to be accomplished, an assumption of rationality (although not of perfect information) must be made. The rationality assumption allows inferences about state behavior to be drawn solely from knowledge of the structure of the system.

Unfortunately, such predictions are often wrong. The concept of power is difficult to measure validly a priori; interests are underspecified by examining the nature of the international system and the position of various states in it; the view of power resources implied by overall structure theories is overaggregated, exaggerating the extent to which power is like money. The problem that students of international politics face is how to construct theories that draw on Realism’s strengths without partaking fully of its weaknesses.

To do this we need a multidimensional approach to world politics that incorporates several analytical frameworks or research programs. One of these should be that of Structural Realism, which has the virtues of parsimony and clarity, although the range of phenomena that it encompasses is limited. Another, in my view, should be a modified structural research program, which relaxes some of the assumptions of Structural Realism but retains enough of the hard core to generate a priori predictions on the basis of information about the international environment. Finally, we need better theories of domestic politics, decisionmaking, and information processing, so that the gap between the external and internal environments can be bridged in a systematic way, rather than by simply adding catalogs of exogenously determined foreign policy facts to theoretically more rigorous structural models. That is, we need more attention to the “internal-external interactions” discussed by Snyder and Diesing.

Too much work in this last category is being done for me to review it in detail here. Mention should be made, however, of some highlights. Peter J. Katzenstein, Peter Gourevitch, and others have done pioneering work on the relationship between domestic political structure and political coalitions, on the one hand, and foreign economic policies, on the
other (Katzenstein 1978; Gourevitch 1978). This line of analysis, which draws heavily on the work of Alexander Gerschenkron (1962) and Barrington Moore (1966), argues that the different domestic structures characteristic of various advanced industrialized countries result from different historical patterns of development; in particular, whether development came early or late, and what the position of the country was in the international political system at the time of its economic development (Kurth 1979). Thus it attempts to draw connections both between international and domestic levels of analysis, and across historical time. This research does not provide deductive explanatory models, and it does not account systematically for changes in established structures after the formative developmental period, but its concept of domestic structure brings order into the cacophony of domestic political and economic variables that could affect foreign policy, and therefore suggests the possibility of eventual integration of theories relying on international structure with those focusing on domestic structure.

Katzenstein and his associates focus on broad political, economic, and social patterns within countries, and their relationship to the international division of labor and the world political structure. Fruitful analysis can also be done at the more narrowly intragovernmental level, as Snyder and Diesing show. An emphasis on bureaucratic politics was particularly evident in the 1960s and early 1970s, although Robert J. Art has pointed out in detail a number of difficulties, weaknesses, and contradictions in this literature (1973). At the level of the individual decisionmaker, insights can be gained by combining theories of cognitive psychology with a rich knowledge of diplomatic history, as in Jervis's work, as long as the investigator understands the systemic and domestic-structural context within which decisionmakers operate. This research program has made decided progress, from the simple-minded notions criticized by Waltz (1959) to the work of Alexander and Juliette George (1964), Alexander George (1980), Ole Holsti (1976) and Jervis (1976).

Despite the importance of this work at the levels of domestic structure, intragovernmental politics, and individual cognition, the rest of my analysis will continue to focus on the concept of international political structure and its relevance to the study of world politics. I will argue that progress could be made by constructing a modified structural research program, retaining some of the parsimony characteristic of Structural

Realism and its emphasis on the incentives and constraints of the world system, while adapting it to fit contemporary reality better. Like Realism, this research program would be based on microeconomic theory, particularly oligopoly theory. It would seek to explain actor behavior by specifying a priori utility functions for actors, using the rationality principle as a "trivial animating law" in Popper’s sense (Latsis 1976:21), and deducing behavior from the constraints of the system as modeled in the theory.

Developing such a theory would only be worthwhile if there were something particularly satisfactory both about systemic explanations and about the structural forms of such explanations. I believe that this is the case, for two sets of reasons.

First, systemic theory is important because we must understand the context of action before we can understand the action itself. As Waltz (1979) has emphasized, theories of world politics that fail to incorporate a sophisticated understanding of the operation of the system—that is, how systemic attributes affect behavior—are bad theories. Theoretical analysis of the characteristics of an international system is as important for understanding foreign policy as understanding European history is for understanding the history of Germany.

Second, structure theory is important because it provides an irre- placeable component for a thorough analysis of action, by states or nonstate actors, in world politics. A good structural theory generates testable implications about behavior on an a priori basis, and, therefore, comes closer than interpretive description to meeting the requirements for scientific knowledge of neopositivists philosophers of science such as Lakatos. This does not mean, of course, that explanation and rich interpretation—Geertz’s “thick description” (1973)—are in any way antithetical to one another. A good analysis of a given problem will include both.

The assumptions of a modified structural research program can be compared to Realist assumptions as follows:

1. The assumption that the principal actors in world politics are states would remain the same, although more emphasis would be placed on nonstate actors, intergovernmental organizations, and transnational and transgovernmental relations than is the case in Realist analysis (Keohane and Nye 1972).

2. The rationality assumption would be retained, since without it, as
we have seen, inferences from structure to behavior become impossible without heroic assumptions about evolutionary processes or other forces that compel actors to adapt their behavior to their environments. It should be kept in mind, however, as is made clear by sophisticated Realists, that the rationality postulate only assumes that actors make calculations “so as to maximize expected value across a given set of consistently ordered objectives” (Snyder and Diesing 1977:81). It does not assume perfect information, consideration of all possible alternatives, or unchanging actor preferences.

(3) The assumption that states seek power, and calculate their interests, accordingly, would be qualified severely. Power and influence would still be regarded as important state interests (as ends or necessary means), but the implication that the search for power constitutes an overriding interest in all cases, or that is always takes the same form, would be rejected. Under different systemic conditions states will define their self-interests differently. For instance, where survival is at stake efforts to maintain autonomy may take precedence over all other activities, but where the environment is relatively benign energies will also be directed to fulfilling other goals. Indeed, over the long run, whether an environment is benign or benign can alter the standard operating procedures and sense of identity of the actors themselves.77

In addition, this modified structural approach would explicitly modify the assumption of fungibility lurking behind unitary conceptions of “international structure.” It would be assumed that the value of power resources for influencing behavior in world politics depends on the goals sought. Power resources that are well-suited to achieve certain purposes are less effective when used for other objectives. Thus power resources are differentially effective across issue-areas, and the usability of a given set of power resources depends on the “policy-contingency frameworks” within which it must be employed.

This research program would pay much more attention to the roles of institutions and rules than does Structural Realism. Indeed, a structural interpretation of the emergence of international rules and procedures, and of obedience to them by states, is one of the rewards that could be expected from this modified structural research program (Krasner 1982; Keohane 1982b; Stein 1982).

This research program would contain a valuable positive heuristic—a set of suggestions about what research should be done and what questions should initially be asked—which would include the following pieces of advice:

(1) When trying to explain a set of outcomes in world politics, always consider the hypothesis that the outcomes reflect underlying power resources, without being limited to it;

(2) When considering different patterns of outcomes in different relationships, or issue-areas, entertain the hypothesis that power resources are differently distributed in these issue-areas, and investigate ways in which these differences promote or constrain actor attempts to link issue-areas in order to use power-resources from one area to affect results in another;

(3) When considering how states define their self-interests, explore the effects of international structure on self-interests, as well as the effects of other international factors and of domestic structure.

Such a modified structural research program could begin to help generate theories that are more discriminating, with respect to the sources of power, than is Structural Realism. It would be less oriented toward reaffirming the orthodox verities of world politics and more inclined to explain variations in patterns of rules and institutions. Its concern with international institutions would facilitate insights into processes of peaceful change. This research program would not solve all of the problems of Realist theory, but it would be a valuable basis for interpreting contemporary world politics.

Yet this form of structural theory still has the weaknesses associated with power analysis. The essential problem is that from a purely systemic point of view, situations of strategic interdependence do not have determinate solutions. No matter how carefully power resources are defined, no power model will be able accurately to predict outcomes under such conditions.78

One way to alleviate this problem without moving immediately to the domestic level of analysis (and thus sacrificing the advantages of systemic theory), is to recognize that what it is rational for states to do, and what states’ interests are, depend on the institutional context of action as well as on the underlying power realities and state position upon which Realist thought concentrates. Structural approaches should be seen as only a basis for further systemic analysis. They vary the power condition in the
system, but they are silent on variations in the frequency of mutual interactions in the system or in the level of information.

The importance of these non-power factors is demonstrated by some recent work on cooperation. In particular, Robert Axelrod has shown that cooperation can emerge among egoists under conditions of strategic interdependence as modeled by the game of prisoners’ dilemma. Such a result requires, however, that these egoists expect to continue to interact with each other for the indefinite future, and that these expectations of future interactions be given sufficient weight in their calculations (Axelrod 1981). This argument reinforces the practical wisdom of diplomats and arms controllers, who assume that state strategies, and the degree of eventual cooperation, will depend significantly on expectations about the future. The “double-cross” strategy, for instance, is more attractive when it is expected to lead to a final, winning move, than when a continuing series of actions and reactions is anticipated.

High levels of uncertainty reduce the confidence with which expectations are held, and may therefore lead governments to discount the future heavily. As Axelrod shows, this can inhibit the evolution of cooperation through reciprocity. It can also reduce the ability of actors to make mutually beneficial agreements at any given time, quite apart from their expectations about whether future interactions will occur. That is, it can lead to a form of “political market failure” (Keohane 1982b).

Information that reduces uncertainty is therefore an important factor in world politics. But information is not a systemic constant. Some international systems are rich in institutions and processes that provide information to governments and other actors; in other systems, information is scarce or of low quality. Given a certain distribution of power (Waltz’s “international structure”), variations in information may be important in influencing state behavior. If international institutions can evolve that improve the quality of information and reduce uncertainty, they may profoundly affect international political behavior even in the absence of changes either in international structure (defined in terms of the distribution of power) or in the preference functions of actors.

Taking information seriously at the systemic level could stimulate a new look at theories of information-processing within governments, such as those of Axelrod (1976), George (1980), Jervis (1976), and Holsti (1976). It could also help us, however, to understand a dimension of the concept of complex interdependence (Keohane and Nye 1977) that has been largely ignored. Complex interdependence can be seen as a condition under which it is not only difficult to use conventional power resources for certain purposes, but under which information levels are relatively high due to the existence of multiple channels of contact among states. If we focus exclusively on questions of power, the most important feature of complex interdependence—almost its only important feature—is the ineffectiveness of military force and the constraints that this implies on fungibility of power across issue-areas. Sensitizing ourselves to the role of information, and information-provision, at the international level brings another aspect of complex interdependence—the presence of multiple channels of contact among societies—back into the picture. Actors behave differently in information-rich environments than in information-poor ones where uncertainty prevails.

This is not a subject that can be explored in depth here. I raise it, however, to clarify the nature of the multidimensional network of theories and research programs that I advocate for the study of world politics. We need both sparse, logically tight theories, such as Structural Realism, and rich interpretations, such as those of the historically oriented students of domestic structure and foreign policy. But we also need something in-between: systemic theories that retain some of the parsimony of Structural Realism, but that are able to deal better with differences between issue-areas, with institutions, and with change. Such theories could be developed on the basis of variations in power (as in Structural Realism), but they could also focus on variations in other systemic characteristics, such as levels and quality of information.

CONCLUSION:
WORLD POLITICS AND PEACEFUL CHANGE

As Gilpin points out, the problem of peaceful change is fundamental to world politics. Thermonuclear weapons have made it even more urgent than it was in the past. Realism demonstrates that peaceful change is more difficult to achieve in international politics than within well-ordered domestic societies, but it does not offer a theory of peaceful change. Nor is such a theory available from other research traditions. The question
remains for us to grapple with: Under what conditions will adaptations to shifts in power, in available technologies, or in fundamental economic relationships take place without severe economic disruption or warfare?

Recent work on "international regimes" has been addressed to this question, which is part of the broader issue of order in world politics (International Organization, Spring 1982). Structural Realist approaches to understanding the origins and maintenance of international regimes are useful (Krasner 1982), but since they ignore cognitive issues and questions of information, they comprise only part of the story (Haas 1982).

Realism, furthermore, is better at telling us why we are in such trouble than how to get out of it. It argues that order can be created from anarchy by the exercise of superordinate power: periods of peace follow establishment of dominance in Gilpin's "hegemonic wars." Realism sometimes seems to imply, pessimistically, that order can only be created by hegemony. If the latter conclusion were correct, not only would the world economy soon become chaotic (barring a sudden resurgence of American power), but at some time in the foreseeable future, global nuclear war would ensue.

Complacency in the face of this prospect is morally unacceptable. No serious thinker could, therefore, be satisfied with Realism as the correct theory of world politics, even if the scientific status of the theory were stronger than it is. Our concern for humanity requires us to do what Gilpin does in the epilogue to War and Change (1981), where he holds out the hope of a "new and more stable international order" in the final decades of the twentieth century, despite his theory's contention that such a benign outcome is highly unlikely. Although Gilpin could be criticized for inconsistency, this would be beside the point: the conditions of terror under which we live compel us to search for a way out of the trap.

The need to find a way out of the trap means that international relations must be a policy science as well as a theoretical activity. We should be seeking to link theory with practice, bringing insights from Structural Realism, modified structural theories, other systemic approaches, and actor-level analyses to bear on contemporary issues in a sophisticated way. This does not mean that the social scientist should adopt the policymaker's framework, much less his normative values or blinders about the range of available alternatives. On the contrary, in-dependent observers often do their most valuable work when they reject the normative or analytic framework of those in power, and the best theorists may be those who maintain their distance from those at the center of events. Nevertheless, foreign policy and world politics are too important to be left to bureaucrats, generals, and lawyers—or even to journalists and clergymen.

Realism helps us determine the strength of the trap, but does not give us much assistance in seeing it escape. If we are to promote peaceful change, we need to focus not only on basic long-term forces that determine the shape of world politics independently of the actions of particular decision-makers, but also on variables that to some extent can be manipulated by human action. Since international institutions, rules, and patterns of cooperation can affect calculations of interest, and can also be affected incrementally by contemporary political action, they provide a natural focus for scholarly attention as well as for policy concern. Unlike Realism, theories that attempt to explain rules, norms, and institutions help us to understand how to create patterns of cooperation that could be essential to our survival. We need to respond to the questions that Realism poses but fails to answer: How can order be created out of anarchy without superordinate power; how can peaceful change occur?

To be reminded of the significance of international relations as policy analysis, and the pressing problem of order, is to recall the tradition of Classical Realism. Classical Realism, as epitomized by the work of John Herz (1981), has recognized that no matter how deterministic our theoretical aspirations may be, there remains a human interest in autonomy and self-reflection. As Ashley puts it, the Realism of a thinker such as Herz is committed to an "emancipatory cognitive interest—an interest in securing freedom from unacknowledged constraints, relations of domination, and conditions of distorted communication and understanding that deny humans the capacity to make their future with full will and consciousness" (1981:227). We think about world politics not because it is aesthetically beautiful, because we believe that it is governed by simple, knowable laws, or because it provides rich, easily accessible data for the testing of empirical hypotheses. Were those concerns paramount, we would look elsewhere. We study world politics because we think it will determine the fate of the earth (Schell 1982). Realism makes us
Theodore of World Politics

10. Emphasis added. Thucydides also follows this "positive heuristic" of looking for underlying power realities in discussions of the Athenian-Corcyrean alliance (chapter II), the decision of the Lacedemonians to vote that Athens had broken the treaty between them (chapter III), and Pericles' Funeral Oration (chapter IV). In the Modern Library edition, the passages in question are on pp. 28, 49-50, and 83.

11. Bruce Bueno de Mesquita (1981:29-33) has an excellent discussion of the rationality assumption as used in the study of world politics.

12. As Waltz points out, Morgenthau's writings reflect the "first image" Realist view that the evil inherent in man is at the root of war and conflict.

13. Sustained earlier critiques of the fungibility assumption can be found in Keohane and Nye (1977:49-52) and in Baldwin (1979).

14. In an illuminating recent review essay, John Gerard Ruggie has criticized Waltz's assumption that the second dimension of structure, referring to the degree of differentiation of units, can be regarded as a constant (undifferentiated units with similar functions) in world politics. Ruggie argues that "when the concept 'differentiation' is properly defined, the second structural level of Waltz's model... serves to depict the kind of institutional transformation illustrated by the shift from the medieval to the modern international system." See Ruggie (1983:279), this volume, p. 146.

15. Waltz denies that he relies on the rationality assumption; but I argue in section II that he requires it for his theory of the balance of power to hold.

16. For a brilliant discussion of this theoretical strategy in microeconomics, see Latvis (1976 esp. pp. 16-23).

17. Since the principal purpose of Realist analysis in the hands of Waltz and others is to develop an explanation of international political reality, rather than to offer specific advice to those in power, the label "technical realism" seems too narrow. It also carries a pejorative intent that I do not share. "Structural Realism" captures the focus on explanation through an examination of the structure of the international system. Capitalization is used to indicate that Realism is a specific school, and that it would be possible to be a Realist—in the sense of examining reality as it really is—without subscribing to Realist assumptions. For a good discussion, see Krassner (1982).

18. This is the commonsense view of power, as discussed, for example, by Arnold Wolfers (1962:103). As indicated in section III, any such definition conceals a large number of conceptual problems.

19. My reading of Gilpin's argument on pp. 29-34 led me originally to believe that he also accepted the notion that power is fungible, since he argues that hegemonic war creates a hierarchy of prestige in an international system, which is based on the hegemon's "demonstrated ability to enforce its will on other states" (p. 34), and which appears to imply that a single structure of power resources exists, usable for a wide variety of issues. But in letters sent to the author commenting on an earlier draft of this paper, both Gilpin and Waltz explicitly disavowed the assumption that power resources are necessarily fungible. In War and Change, Gilpin is very careful to disclaim the notion, which he ascribes to Political Realists but which I have not included in the hard core of Realism, that states seek to maximize their power: "Acquisition of power entails an opportunity cost to a society; some other desired good must be abandoned" (p. 5).
20. A similar issue is posed in chapter 3 of part II of Lineages of the Absolutist State (1974). Its author, Perry Anderson, addresses the puzzle of why it was Prussia, rather than Bavaria or Saxony, that eventually gained predominance in Germany. Despite his inclinations, Anderson has to rely on a variety of conjunctural, if not accidental, factors to account for the observed result.

21. For a lucid discussion of the security dilemma, see Jervis (1978).

22. The fact that sensitive Realists are aware of the limitations of Realism makes me less worried than Ashley about the policy consequences of Realist analysis. (See above, pp. 168–169.)

23. Waltz does not accept Baldwin’s (and Dahl’s) definition of power in terms of causality, arguing that “power is one cause among others, from which it cannot be isolated.” But this makes it impossible to falsify any power theory; one can always claim that other factors (not specified or prior) were at work. Waltz’s discussion of power (1979:191–192) does not separate power-as-outcome properly from power-as-resources; it does not distinguish between resources that the observer can assess a priori from those only assessable post hoc; it does not relate probabilistic thinking properly to power theory; and it takes refuge in a notion of power as “affecting others more than they affect him,” which would result (if taken literally) in the absurdity of attributing maximum power to the person or government that is least responsive to outside stimuli, regardless of its ability to achieve its purposes.

24. Jervis (1976, ch. 1) has an excellent discussion of levels of analysis and the relationship between perceptual theories and other theories of international relations. Snyder and Diesing discuss similar issues in chapter 6 on “Crises and International Systems” (1977).

25. Waltz commented perceptively in Man, the State and War that contributions of behavioral scientists had often been “rendered ineffective by a failure to comprehend the significance of the political framework of international action” (1959:78).

26. Thorough description—what Alexander George has called “process-tracing”—may be necessary to evaluate a structural explanation, since correlations are not reliable where only a small number of comparable cases is involved. (See George 1979).

27. I am indebted for this point to a conversation with Hayward Alker.

28. Latané (1976) discusses the difference between “single-exit” and “multiple-exit” situations in his critique of oligopoly theory. What he calls the research program of “situational determinism”—structural theory, in my terms—works well for single-exit situations, where only one sensible course of action is possible. (The building is burning down and there is only one way out: regardless of my personal characteristics, one can expect that I will leave through that exit.) It does not apply to multiple-exit situations, where more than one plausible choice can be made. (The building is burning, but I have to choose between trying the smoky stairs or jumping into a fireman’s net: my choice may depend on deep-seated personal fears.) In foreign policy, the prevalence of multiple-exit situations reinforces the importance of decision-making analysis at the national level.

29. For a more detailed discussion of some aspects of this notion, and for citations to some of the literature in economics on which my thinking is based, see Keohane (1982b). Discussions with Vinod Aggarwal have been important in formulating some of the points in the previous two paragraphs.

30. Morgenthau devotes a chapter of Politics Among Nations to peaceful change, but after a review of the reasons why legalistic approaches will not succeed, he eschews general statements for descriptions of a number of United Nations actions affecting peace and security. No theory of peaceful change is put forward. In Politics Among Nations Morgenthau put whatever faith he had in diplomacy. The chapter on peaceful change is chapter 26 of the fourth edition (1966).

31. For a suggestive discussion of international relations as policy science, see George and Smoke (1974), Appendix, “Theory for Policy in International Relations,” pp. 616–642.

32. Recall Weber’s aphorism in “Politics as a Vocation”: “Politics is the strong and slow boring of hard boards.” Although much of Weber’s work analyzed broad historical forces beyond the control of single individuals or groups, he remained acutely aware of “the truth that man would not have attained the possible unless time and again he had reached out for the impossible” (Gerth and Mills 1958:128). For a visionary, value-laden discourse on future international politics by a scholar “reaching out for the impossible,” see North (1976, ch. 7).

33. Ernst B. Haas, who has studied how political actors learn throughout his distinguished career, makes a similar point in a recent essay, where he espouses a “cognitive-evolutionary view” of change and argues that such a view “cannot settle for a concept of hegemony imposed by the analyst. . . . It makes fewer claims about basic directions, purposes, laws and trends than do other lines of thought. It is agnostic about the finality of social laws” (1982:242–243). The difference between Haas and me is that he seems to reject structural analysis in favor of an emphasis on cognitive evolution and learning, whereas I believe that modified structural analysis (more modest in its claims than Structural Realism) can provide a context within which analysis of cognition is politically more meaningful.