INTERNATIONAL POLITICS

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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 *machtpolitik* 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 reconfirm 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 Mansbach 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 relations," or "issue phases" (Keohane & Nye, 1972, esp. pp. 379-386; Mansbach & Vasquez, 1981, Chapter 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. 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

*I am grateful to Raymond Hopkins for inviting me to prepare the original version of this 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 graduate international relations field seminar at Brandeis University during the spring semester, 1982, which I taught with my colleague, Robert J. Art. I have also received extremely valuable comments from a number of friends and colleagues on an earlier draft of this paper, in particular from Vinod Aggarwal, David Baldwin, Seyom Brown, Ben Dickinson, Alexander George, Robert Gilpin, Ernst Haas, Thomas Ilgen, Robert Jervis, Peter Katzenstein, Stephen Krasner, Timothy McKeown, Helen Milner, Joseph Nye, and Kenneth Waltz.

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 Realism 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 twentieth century. Doing this requires constructing a rather elaborate argument of my own, precluding a comprehensive review of the whole literature of international relations. I have therefore selected for discussion 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, p. 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 & Nye, 1977, pp. 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 & Nye, 1977, pp. 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).[5]

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.[6] 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 observational, 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). Progressive research programs display "continuous growth": their auxiliary hypotheses increase our capacity to understand reality (Lakatos, 1970, pp. 116-122, 132-138, 173-180).

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 relied upon by 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' 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![7]

The stringency of Lakatos' 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, p. 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 obfuscatory 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 arguments 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 multi-dimensional 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.

I. 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.[8] 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."[9]

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 decision-makers. 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 said. (Thucydides, Book I, paragraph 23 [Chapter I, Modern Library edition, p. 14])

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, p. 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. 15])[10]

Morgenthau is even more blunt: "International politics, like all politics, is a struggle for power" (1966, p. 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, p. 5)

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

(1) The *state-centric 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;[11]

(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)[12]

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, pp. 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, pp. 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."[14] 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, p. 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, pp. 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" (Wolfers, 1962, p. 105).

In its strong form, the Structural Realist research program is similar to that of micro-economics. 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, pp. 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.[17] 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, p. 221). It is self-reflective and non-deterministic. 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, p. 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 precedessors. By its own standards, Structural Realism is, in Ashley's words, "a progressive scientific redemption of classical realism" (Ashley, 1982, p. 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, pp. 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 REALIST 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 book, *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 crisis 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, p. 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, p. 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, pp. 66-72). Balance of power theory applies to "anarchic" 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 "devise a number of distinct and demanding tests" (1979, p. 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 implication 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.[18] 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 posssible 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 work 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, pp. 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 & Diesing, 1977, Chapters IV and V).

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 so 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 (1981, pp. 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 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 the 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 the 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, 1981, p. 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 Diesing'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:

> Disequilibrium 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 leading 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.[20] 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 affiars, in the hope of ameliorating the security dilemma.[21] Morgenthau put his hopes in diplomacy (1966, chapter 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, 1982). 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, p. 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—"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, p. 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, pp. 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 non-logical manner (1978, p. 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 & Sprout, 1971, pp. 73-77). This limitation makes it both less powerful as a theory, and less dangerous as an ideology.[22] 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% (1981, especially p. 151; Organski & Kugler, 1980, Chapter 2).

The question of the fungibility of power poses a more troublesome issue. As I have noted earlier (see footnote 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, pp. 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, pp. 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 & 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 as often favored the Canadian as the American position, although this was not true for relations between Australia and the United States (Keohane & Nye, 1977, Chapter 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, p. 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, pp. 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, p. 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.[23] 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 needs 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 sub-set 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 & Nye, 1977, Chapter 4, especially pp. 64-65; Simon, 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; Tollison & 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 national 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' 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 only be properly understood 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 multi-dimensional 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, decision-making, 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 decision-maker, 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 decision-makers operate.[24] 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).[25]

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, p. 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, structural theory is important because it provides an irreplaceable *component* for a thorough analysis of action, by states or non-state 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 neo-positivist 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.[26]

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 non-state actors, intergovernmental organizations, and transnational and transgovernmental relations than is the case in Realist analysis (Keohane & 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 & Diesing, 1977, p. 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 it 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 malign or benign can alter the standard operating procedures and sense of identity of the actors themselves.[27]

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, 1982; 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.[28]

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 modelled 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, 1982).

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 & 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.[29] I raise it, however, to clarify the nature of the multi-dimensional network of theories and research programs that I advocate for the study of world politics. We need both spare, 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 differentiations 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.[30] 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.[31] 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 policy-maker's framework, much less his normative values or blinders about the range of available alternatives. On the contrary, independent 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 seeking to 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 policy concern.[32] 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, p. 227).[33] 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 aware of the odds against us. What we need to do now is to understand peaceful change by combining multi-dimensional scholarly analysis with more visionary ways of seeing the future.

NOTES

1. An unfortunate limitation of this chapter is that its scope is restricted to work published in English, principally in the United States. I recognize that this reflects the Americanocentrism of scholarship in the United States, and I regret it. But I am not sufficiently well-read in works published elsewhere to comment intelligently on them. For recent discussions of the distinctively American stamp that has been placed on the international relations field see Hoffmann (1977) and Lyons (1982).

2. Nye and I, in effect, conceded this in our later work, which was more cautious about the drawbacks of conventional "state-centric" theory. (See Keohane & Nye, 1977.)
3. For a discussion of "theory as a set of questions," see Hoffmann (1960, pp. 1-12).
4. Bruce Russett has written a parallel essay in this volume on "International Interactions and Processes: The Internal vs. External Debate Revisited." Professor Russett discusses the extensive literature on arms control and on dependency, neither of which I consider here.
5. Stanley J. Michalak, Jr. pointed out correctly that our characterization of Realism in *Power and Interdependence* was unfair when taken literally, although he also seems to me to have missed the Realist basis of our structural models. (See Michalak, 1979.)
6. It has often been noted that Kuhn's definition of a paradigm was vague: one sympathetic critic identified 21 distinct meanings of the term in Kuhn's relatively brief book (Masterman, 1970). But Lakatos particularly objected to what he regarded as Kuhn's relativism, which in his view interpreted major changes in science as the result of essentially irrational forces. (See Lakatos, 1970, p. 178.)
7. Lakatos' comments on Marxism and psychology were biting, and a colleague of his reports that he doubted the applicability of the methodology of scientific research programs to the social sciences. (See Latsis, 1976, p. 2.)
8. Robert Jervis and Ann Tickner have both reminded me that Morgenthau and John H. Herz, another major proponent of Realist views in the 1950s, later severely qualified their adherence to what has generally been taken as Realist doctrine. (See Herz, 1981, and Boyle, 1980,p. 218.) I am particularly grateful to Dr. Tickner for obtaining a copy of the relevant pages of the latter article for me.
9. For commentary on this assumption, see Keohane and Nye (1972), and Mansbach, Ferguson, and Lampert (1976). In *Power and Interdependence,* Nye and I were less critical than we had been earlier of the state-centric assumption. In view of the continued importance of governments in world affairs, for many purposes it seems justified on grounds of parsimony. Waltz's rather acerbic critique of our earlier position seems to me essentially correct. (See Waltz, 1979, p. 7.)
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, pp. 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, pp. 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, p. 279).

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 micro-economics, see Latsis (1976, especially 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 Krasner (1982).
18. This is the commonsense view of power, as discussed, for example, by Arnold Wolfers (1962, p. 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 in turn determines governance of the international system (p. 33). This 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. 51).
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. 508-509.
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 a priori) were at work. Waltz's discussion of power (1979, pp. 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, Chapter 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 VI on "Crises and In-

ternational 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, p. 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. Latsis (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 (1982). 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 & Mills, 1958, p. 128). For a visionary, value-laden discourse on future international politics by a scholar "reaching out for the impossible," see North (1976, Chapter 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, pp. 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.

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17

International Interactions and Processes: The Internal vs. External Debate Revisited*

Bruce Russett

Interactions may be part of a cooperative process, or part of a conflict. International trade and finance are usually characterized as cooperative interactions, involving a mutually beneficial exchange. Dependence theorists, however, emphasize the unequal distribution of many of the benefits of exchange, the existence of elements of exploitation, and asymmetric penetration of poor, weak societies. "Arms races" seem to exemplify conflict, as seen in the vicious spiral of some types of Richardson (1960) processes, though as understood in the prisoners' dilemma they may be held in check by certain kinds of cooperative interactions. Many of the processes central to dependence theory are the result of decisions by non-state actors such as multinational corporations and national or transnational social classes; they reflect less the power of states—though state actors are important—than of these non-state actors; and they usually do not result in interstate war. As such they typically have been ignored in the standard "realist" perspective. Arms race processes, by contrast, are typified by state decisions to arm; they reflect and affect the international distribution of power; and they may result in war. The overt manifestations of arms races (threat, hostility, perhaps war) have long been at the heart of realist concerns. Nevertheless, the internal bureaucratic and organizational processes now widely regarded as central to understanding arms acquisitions have been neglected in realist analyses (Keohane, 1983).

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*I am grateful to the National Science Foundation and the German Marshall Fund for their support of much of the research on which this essay is based, and especially to Paul Huth, Jim Lindsay, and Steve Silvia for their excellent research assistance in gathering and evaluating much of the material cited here. I am also grateful to Bruce Bueno de Mesquita, James Caporaso, Raymond Duvall, Alexander George, Robert North, and Dina Zinnes for comments. Of course, no individual or organization is responsible for the opinions I have expressed here.

It is one thing to recognize that subnational and transnational actors contribute essentially to the processes that are part of the phenomena we characterize as dependence or arms races; it is another to regard them as introducing sufficient variation to affect significantly the dependent variable of interest. Their impact may be fairly uniform in most times and places, allowing us to expect universal patterns. Richardson seemed to have imagined that when estimated the coefficients would in fact turn out to be essentially the same in all circumstances. If internal and other processes are reasonably uniform, then we might hope to explain large portions of variance with law-like propositions such as those that appear to operate in many areas of physics.

Most analysts would now conclude that such uniformity does not exist, and that contextual variation, introduced by the effect of additional variables, will be important. The simplest version of this effect would allow us to apply the same model to all states and circumstances by expecting that the same *elements* or variables will always be present but that the *form* of their relationship will vary. The values of the parameters may change, and be of different importance. With this expectation, the strength of bureaucratic inertia may vary substantially in different circumstances, or different regimes may have very different perceptions of threat and hostility ("grievance coefficients" in the classic Richardson formulation). But we would nevertheless be asking the same questions about all states. This view, consistent with a perspective sympathetic to social science, would emphasize the complex multivariate and interactive nature of political phenomena. It would not despair of generalization, but only of the prospect of finding simple, powerful, universally-applicable generalizations.

A more fundamental critique, however, would hold that the models themselves must be different: for example, in capitalist countries depressed demand might stimulate increased military expenditures for the purpose of expanding aggregate economic activity; for socialist countries the economic capacity might be seen rather as a constraint, holding back levels of military spending that would otherwise be desired by the leadership. Not only would we expect a different sign for the "economic" variable, but the specification of the variable would be different. This conclusion still would not necessarily lead to a rejection of science and generalization, but it would complicate still further the investigation, and require careful and detailed model specification.

Similar problems apply to any expectations of making broad generalizations about dependence. Theorists like Cardoso (1977) deplore North American "totalizing" efforts that allegedly neglect contextual factors like the size of a country's domestic market or its particular history of class relations as far back as the colonial era. In its extreme form, this argument asserts that the conditions of each country are so different, and the results of "dependence" so different in each case, that the search not only for uniform coefficients, but even for a uniform model, is doomed. The latter *is* an extreme position, but it is encountered.

Investigation of these relationships has major implications both for arms races and dependence. Early efforts to investigate these topics seemed to assume that the prospects for making strong and simple generalizations were good. They worked with only a few variables, and made little acknowledgment of possible interactions. This is common in the first stages of most investigations, and the failure to uncover strong, simple relationships more usual than not. Later examinations in these central topics of political economy have become much more sophisticated, though they have not necessarily produced clear-cut or widely accepted results. In this chapter we shall look at the experience of these endeavors, and try to draw some conclusions to guide further analyses. Whereas the substantive topics of the two literatures may seem far apart, we shall see that they share many similarities of investigation, difficulty, and promise.

ARMS ACQUISITION MODELS

Models to explain states' military expenditures or weapons acquisitions typically are referred to as arms race models. That label, however, is deceptive, because it assumes what should instead be a question for investigation: the role of international competitive processes in affecting decisions to buy arms. While this is indeed a widely hypothesized phenomenon, often correctly, it should not simply be assumed. Critics of the true "arms race" school maintain rather that arms purchase decisions are determined little if at all by what goes on in the international system, but rather by internal pressures of bureaucratic politics or domestic political processes (e.g., "military-industrial complex" theories). The latter emphasize the virtually "autistic" character of arms purchases. Still other models incorporate both external and internal influences. The relative importance of each should not be pre-judged.

The best-known arms acquisition model is aptly characterized as an arms race model. Its formulation is the most familiar of the Richardson models:

$$\Delta X = kY - aX + g$$
$$\Delta Y = lX - bY + h$$

In this two-nation arms race, the changes (Δ) in the military expenditures (or arms stocks—they are not the same) of the two nations (X and Y) are influenced by three major factors: (1) the military expenditures of the other state (k and l represent coefficients); (2) the economic burden of paying for previous decisions to purchase military goods ($-a$ and $-b$ represent "fatigue" coefficients to indicate the weight of this burden); and (3) the underlying "grievance" held by each state against the other (g and h).

The first efforts to investigate these equations either met with little success or, if initially promising, were found to suffer from crippling methodological flaws. The more prominent ones were reviewed by Busch (1970). At that time, the majority of arms race studies had been theoretical rather than empirical; Busch concluded that these theories were too simple and/or too apolitical (for example, taking models directly from physics without specifying the political processes that might make them applicable). The more precise formulations called for by Busch were appearing by the time of the reviews by Luterbacher (1975) and Rattinger (1976). Luterbacher concentrated on theoretical problems, with a critique of the assumptions of linear relationships and the neglect of cost and resource constraints on arms expenditure. For example, though the "fatigue coefficient" was an integral part of the Richardson model, even to this date it has received only sporadic attention. Rattinger devoted most of his attention to methodological problems of estimation. He highlighted the tradeoff arising between the reality of short time series—requiring models to be as parsimonious as possible—and the demands of complex modelling. The former risks specification error by leaving out essential explanatory variables, and the latter risks having too few degrees of freedom to produce any significant results (see, for example, Hamblin, 1977). He also criticized those specifications that proceeded to complex non-linear relations without first employing linear models.

Despite the familiarity and apparent plausibility of the arms race hypothesis of external determinants, it has not always held up in empirical examinations of the United States-Soviet Union relationship. The reasons for the mixed results appear complex, but they include questions of conceptualization as well as the broader theoretical question as to whether international (action-reaction) explanations are even in principal adequate.

Levels, Changes, and Stocks

One major problem lies in the realm of an adequate conceptualization of precisely what it means to postulate that one country reacts to another's military expenditure. The greatest number of early works assumed that the change in one state's spending depends on the *level* of its rival's spending in the preceding period. (This remained true for many of the studies of the 1970s as well; see, for example, Strauss, 1972, 1978; Gregory, 1974; Ostrom, 1977; Gillespie *et al.*, 1977, 1978.) Some earlier studies assumed that the reaction was to the rival's current spending, but that specification has long been regarded as unrealistic in modern complex governmental organizations. More recently, many studies have shifted to focus on the change in one state's spending as depending on the *change* in its rival's spending during the preceding period (Hollist, 1977a, 1977b; Hollist & Guetzkow, 1978; Cusack & Ward, 1981).[1] This seems to offer some improvement on the first formulation, since a marked increment in a rival's effort may seem threatening even if the level of the rival's effort is low. More plausibly, perhaps, a halt in annual increases, or even more a decrement from the previous year, may constitute a strong signal to call forth comparable restraint from a rival. Unfortunately, neither of these specifications regularly produces convincing results when applied to the action-reaction process of most interest, that between the United States and the Soviet Union. An alternative formulation is the distributed lag model employed by Strauss (1972, 1978), Gillespie, Zinnes, and Rubinson (1978), and most recently by Majeski and Jones (1981). Such a model effectively considers a country's current arms expenditures as affected by its antagonist's expenditures over a long period of previous years, with the earlier years producing progressively smaller effects as one moves back in time.

More plausible are the recent studies that give attention to the existing *stock* of weapons to which any addition may be made. For example, in periods when the United States maintained a stock of strategic weapons far superior to that of the Soviet Union, American leaders could—perhaps mistakenly—be relaxed about substantial year-to-year increases in Soviet spending so long as the initial stock of Soviet strategic weapons was fairly low; it might take many years of increases and even years of subsequent high spending levels to bring the Soviet weapons stock to "essential equivalence" with that of the United States. Only as the United States' substantial lead over the Soviet Union in stock of weapons—not just the level of spending—diminished would some decision-makers become fully alerted.

Examination of military spending patterns is itself subject to several well-recognized difficulties. The data available are highly aggregated; usually one must deal with total military spending rather than, for example, spending for strategic arms, which might be the most relevant to an arms-race analysis. (Reasonable data on United States strategic weapons spending are now available for most of the period but estimates for the Soviet Union are very crude.) Kugler and Organski (1980) nevertheless have estimated equations for military expenditure on strategic nuclear weapons systems, compiling a measure of strategic weapons and including a parameter to measure depreciation. This is conceptually attractive, but the basis for deriving the measures is very unclear.

The quality of all the data on Soviet military spending is necessarily poor, subject to wide differences in estimation (Holzman, 1980, 1982). Much of the controversy concerns the different estimates of the level of Soviet spending rather than of year-to-year changes; this leads to questions about how American and Soviet spending are to be measured in a common currency. Nevertheless, even some of the data on changes are dubious, with suggestions that estimates of Soviet spending are sometimes made by extrapolating from previous apparent trends—a procedure guaranteed to "support" hypotheses like those of bureaucratic inertia. Although the serial correlation between the major sources, like I.I.S.S. and S.I.P.R.I., is reasonably high, we now know that arms race analyses are very sensitive to any differences, and can produce results strikingly at variance from one data base to another (Cusack & Ward, 1981).

These difficulties are compounded when the theoretical model is concerned with stocks of weapons, rather than with spending. Whereas the individual data items are probably more reliable than are those for expenditures (but see Ostrich & Green, 1981, and Albrecht *et al.*, 1978, on problems even with I.I.S.S. estimates of strategic forces, and the criticism by Cordesman, 1982, of U.S. Defense Department data), serious validity problems can arise, associated with the familiar controversies about what should be counted. Some use numbers of bombers and/or warheads weighted by yield (e.g., Lambelet, 1973; Luterbacher, 1976), or just missiles (Saris & Middendorf, 1980). Some scholars have experimented with various combinations. Decisions about which measure to use can greatly affect the results, without pointing to any one particular measure as necessarily the "right" one (McGuire, 1977; see also Richelson, 1982).

From a theoretical point of view, however, the case for inclusion of some measure of weapons stocks, not just current or recent acquisitions or spending, is persuasive. A theoretical model incorporating both budgets and stockpiles, and with stockpiles adjusted for depreciation, is presented by Taagepera (1979/80), though Taagepera despaired of the prospect for measurement in any empirical estimation. A more promising approach—but a very complex one requiring some strong assumptions about data quality and the ability to estimate depreciation rates effectively—can be seen in the important work of Ward (1982). Ward devises an index of the stockpile of strategic and conventional weapons, with their value depreciated over time, and then is able to focus on increments to those stockpiles. Ward's index for the stock of strategic weapons employs the measure of lethality created by Tsipis (1975)—by no means an unchallengeable measure, but arguably the best available.

Reaction To What?

A second kind of theoretical refinement requires a broader specification of what it is that a state may be reacting to. States react not merely to the weapons that other states possess or acquire, but to the level of hostility being generated by other states which includes estimates of what they do with their weapons. For example, arguably the boost in U.S. military spending that began in 1980, and was magnified by the Reagan administration, was attributable less to increased Soviet military spending than to Soviet military activities in Africa and Afghanistan. Richardson's grievance coefficient may be seen as an attempt to incorporate this kind of influence, but most empirical arms race models have neglected to specify such a term. Ashley (1980) did incorporate a term for "intersections," which he usually found to have insignificant effects. But his measure, one for commercial intersections, leaves something to be desired in a model where two (China and the USSR) are not capitalist states. Ward (1982) employed a measure of international tensions taken from Azar's COPBAB data base (Azar, 1980), which seems more valid. Zimmerman and Palmer (1983) find that Soviet military spending reacted to Soviet verbalized evaluations of American behavior.

Incorporating the effects of what is happening in the international environment, of course, involves building in not just international tensions in general, but specifically the effects of any wars a state may actually be fighting. Many of the efforts to investigate the post-World War II Soviet-American relationship, for instance, have fallen afoul of the effect of American wars in Korea and Indochina. Part of the initial spurt in American military spending that started in 1950 can plausibly be attributed to a generalized sense of threat from the Soviet Union (stimulating a broad rearmament effort) but a great part of the increment during 1950-1953 was due simply to the costs of fighting the Korean War, as manifested by a drop in spending when the war was over. Even more clearly, American spending increases during the Vietnam War years were due solely to the costs of fighting that war, not to more general increments in international tension. (Some critics in fact pointed to a neglect in maintaining or modernizing forces in Europe and strategic deterrent forces in order to divert funds to fight the war.) The work of Nincic and Cusack (1979) represents one effort to incorporate a war-mobilization term; Ward's (1982) use of Department of Defense estimates of the cost of war involvement may be more satisfactory.

Finally, any effort to model the international environment adequately must show some recognition of the fact that more than two states are involved. Sometimes this recognition is reflected in models that incorporate alliance partners rather than just the superpowers; for example, the results for aggregated NATO/Warsaw-Pact interactions can be quite different from those for the United States and the Soviet Union alone. More important is the triangular Soviet-American-Chinese relationship. Prior to the early 1970s it probably would have made sense to combine Soviet and Chinese military forces as a single "actor" for purposes of American perceptions (e.g. as reflected in American strategic military plans calling for nuclear "retaliation" against China as well as against the USSR in case of Soviet attack). For the last decade, of course, this is no longer an appropriate assumption. As for Soviet perceptions, surely by the early 1960s Soviet military planning had begun to reflect a perception of China as an enemy rather than an ally; about 20-25 percent of current Soviet military spending is now estimated to be directed toward China. Wallace (1980) seems to find that the United States and the USSR react to changes in each other's spending and to the international political climate, and in addition, that the USSR reacts to Chinese spending. This effect of China on the Soviet Union also shows up in Ashley's (1980) triangular analysis, but not in Cusack and Ward's (1981).

Internal Influences

Of course not all the influences on "arms races" stem from the international (external) environment. Richardson attempted to capture the constraining effects of economic burdens with his fatigue coefficient. That coefficient, though it too has often been neglected in contemporary arms race models, is important because it is, in Richardson's formulation, the only constraint on arms races short of war. That is, without it, international tensions and the mutual stimulation of competitive arms purchases would drive an arms race into an ever-upward spiral. Richardson considered that this fatigue coefficient represented the economic burden of the arms race, and Hollist and Johnson (1982) found evidence for it in the case of American spending. Other efforts to model the contemporary arms race with a simple and stable fatigue coefficient have, however, not had good results. Currently the United States is spending about 6 percent of its GNP for military purposes, and there is talk about the inability of the economy to sustain such exertions for long. But the United States spent about 13 percent of its GNP for military purposes during the Korean War, and over 40 percent at the peak of World War II. States under the direct threat may persuade their citizens to spend even more. Britain and the Soviet Union spent as much as 60 percent of their total product on defense during the worst of World War II; by that standard, the current figure of around 14 percent for the USSR may not seem high. Israel has for quite a few years even in "peacetime" carried a defense burden above 25 percent of its GNP—although a good deal of foreign assistance contributes to its defense budget. Thus it is not clear how effective such constraints may be, particularly if the national sense of threat, or grievance, is high or can be made to appear high.

Nincic and Cusack (1979) tried a different approach to internal influences. Rather than considering domestic economic influences a burden, they developed a model of a "political business cycle," whereby military spending was seen as an anti-cyclical fiscal tool, and *increased* according to political perceptions of the need to stimulate macro-economic activity. This effect is keyed to the political cycle of elections, being strongest in presidential elections when an incumbent president is seeking reelection. Their hypothesis was that presidents would seek or accept higher levels of military spending in those years to encourage popular perceptions of prosperity, and hence popular approval of the incumbent administration. The results supported this hypothesis.

A very different approach to internal determinants is represented by the organizational and bureaucratic politics schools. The inertia of large bureaucratic organizations in maintaining and expanding their scope has been well-recognized in case studies (e.g., for the Department of Defense, Halperin, 1974). Attempts to model these effects stem from the work of Davis, Dempster, and Wildavsky (1966), and have been carried furthest with reference to the Defense Department by Crecine (1971; see also Fischer & Crecine, 1979). These efforts have met with some success.

Other studies have produced some important variations on the original insight. Cusack and Ward (1981), for example, began by testing an arms race (action-reaction) model for the three-nation Soviet-American-Chinese relationship. After finding arms race explanations wanting, they turned to internal models. They began with the Nincic and Cusack model for the United States, and they found it valuable. The "domestic political economy" models they developed for the Soviet Union and China, however, had to be quite different. For those countries, of course, an electoral cycle is hardly appropriate; rather Cusack and Ward keyed their model to the cycle of economic planning. It assumes that arms spending is felt as an economic burden, especially in the beginning and final years of government economic plans; hence they hypothesized that military spending would be least constrained in the middle years of the economic plans. This version of the arms spending as burden hypothesis met with much less success for the Soviet Union, and really none at all for China. Nincic (1983) had more positive results with a subsequent study of the USSR. But the theoretical perspective is attractive and deserves further development, especially if adequate data can be generated.

Internal and External Influences

It should now be apparent that a satisfactory understanding of arms acquisition processes can be derived only by testing for both internal and external influences. Such an effort can be seen in Ostrom's (1978) work, and in more recent works by Lambelet, Luterbacher, and Allan (1980) and Hollist and Johnson (1982). The most sophisticated and potentially satisfying effort to date is almost certainly that of Michael Ward (1982). Ward builds on previous theoretical and empirical works, notably those of Taagepera (1979/80), Luterbacher, Allan, and Imhoff (1979), Lambelet, Luterbacher, and Allan (1979), and the work by Nincic and Cusack. Ward's model includes measures of stockpiles and depreciation as well as spending, variables for the "exogenous" effects of the Korean and Vietnam Wars, and indicators of international tension.[2]

Ward's results are impressive. In what he presents as an arms race model, he finds that the United States and the Soviet Union do react to each other's spending decisions, and that the United States, at least, also reacts positively (i.e., reducing defense spending) to decreases in the level of international tension. His model is the first to produce statistically significant estimates of all the parameters, parameter signs consistent with a priori expectations, and large coefficients of multiple determination *all at the same time.*

Ward seems also to confirm the existence of a budgetary constraint, with a negative coefficient for military expenditures in the previous period. His choice of indicator, however, leaves substantial doubt as to just what he has in fact found. A better indicator for budgetary constraint would be a relative measure of military expenditures as a proportion of the total national budget, or GNP, telling us about the proportion of a society's resources being absorbed by the military. Instead, total expenditures would appear to be a measure of organizational momentum, and have been used as such by other researchers (e.g., Kugler & Organski, 1980; Lucier, 1979; Fischer & Crecine, 1979). If we so reinterpret the indicator, we have not the budgetary constraint of a pure arms race model, but the organizational momentum of a model that *combines* internal and external influences.[3]

This interpretation is puzzling, however, because the sign of the coefficient is negative, indicating that the American government bureaucracy has operated as a force to decrease military spending. This is not the standard bureaucratic-organizational politics interpretation (last year's budget plus a little more). Nevertheless it has some plausibility for the United States during the Eisenhower administration ("more bang for the buck") and later administrations—until Reagan—when for much of the time the perceived need was to find budgetary space to maintain or expand big civilian programs. A recent sophisticated analysis by Fischer and Kamlet (1983) confirms the existence both of a small action-reaction effect of Soviet arms and of a fiscal constraint on American military expenditures. Similar results emerge in work by Domke, Eichenberg, and Kelleher (1983), who find both a reaction to international tension and a fiscal constraint. More work on these and similar models is required.

Even as they stand, these findings fit comfortably with those of other studies emphasizing that internal determinants of arms races are important, and that they may be at least as important for the Soviet Union as for the United States (see Ashley, 1980; Nincic, 1982, 1983; Rattinger, 1975; Luterbacher, Lambelet, & Allan, 1980). This testimony to the importance of internal determinants demands further investigation and specification of the nature of those determinants, especially distinguishing between narrow bureaucratic determinants and broader societal ("military-industrial complex") ones, and between expansionary pressures and constraints.

With our reinterpretation of Ward's expenditure coefficient, his work seems to confirm the facts both of Soviet-American interaction and of organizational-bureaucratic inertia. Most previous efforts had been unable satisfactorily to distinguish the two. Indeed some observers have declared that quantitative arms race models probably never would be able to do so, given the crudeness of the assumptions and the data with which they must inevitably work. For example, it is a serious handicap to have to work with annual expenditure data. Given the phases of decision-making on military budgets—in the United States from requests of individual military services, through the Joint Chiefs and the Department of Defense, through the White House and the OMB, and then through the authorization and appropriation process in Congress—the notion of discrete annual increments, even lagging a year or two behind Soviet actions, seems a gross over-simplification. If we add to that a knowledge of the long leadtimes from the conception of a weapon through its research, development, construction, and deployment phases, and the recognition that weapons may be developed in *anticipation* of the other state's development as well as in response to such development (Allison, 1974), we might well have despaired of finding any interpretable results.[4]

Ward also has some important observations about variations in these processes. He writes that the United States did react to Soviet narrowing of the military capability gap between the two countries, but that budgetary constraints weakened the American response—a finding consistent with studies cited above. The Soviet Union, rather differently, also reacted to the gap, but seems not to have felt a significant budgetary constraint—thus allowing the USSR effectively to close the gap. Furthermore, in a methodologically important new analysis Freeman (1983), reanalyzing Majeski and Jones (1981) data with a sophisticated "Granger causality" procedure, finds, in addition to what may be interpreted as a bureaucratic inertia effect, significant effects of American spending on Soviet spending—although not vice versa. It also is important to note, in several analyses, the reaction of the United States to changes in international tension rather than just to Soviet spending. Together, these studies do confirm the existence of something that can appropriately be termed an arms race. Nevertheless it is not entirely clear who is reacting to whom, or for that matter to what.

This research still leaves many unanswered questions. Reservations must persist about the data base. Ward uses I.I.S.S. estimates of Soviet and American military effort; this is odd since presumably the United States is responding to its perception of Soviet activity, which perhaps is more directly captured by American government (i.e., CIA) estimates. Furthermore, we know, from Ward's earlier work (Cusack & Ward, 1981), that the results of arms race modelling efforts are not very robust across different data sources, yet he uses just the one here. More important, it is impossible, at least from the material presented in Ward's paper, to distinguish the relative importance of domestic vs. international influences. Ward's depreciated weapons stock index is necessarily a construct without an obvious empirical counterpart; from the information available we cannot tell the relative impact of the action-reaction effect.

Finally, it is essential to emphasize a point that Ward himself makes clearly: his results apply to an historical epoch when the United States had a consistent—though steadily declining—military advantage over the Soviet Union. His findings about the relative importance of various influences—

especially about the auto-centric nature of Soviet spending, and the insensitivity of the USSR to changes in either American spending or the level of international tension—cannot readily be transferred from the past to the future. In a period of "essential equivalence" we may well see a significant shift in the importance of various determinants (for example, a greater sensitivity of the United States to any apparent Soviet effort to surpass American military strength, or even a greater Soviet recognition of the nature of international action-reaction processes).

Changing Parameters

The most recent review article is that of Moll and Luebbert (1980), which focuses on some other—and equally serious—complexities in arms race analyses. Many of the earlier empirical analyses were marred by serious methodological problems that have now come to be fully recognized. Small samples and few degrees of freedom meant that several different functional relationships could be fitted to the data. This problem may now be easing, however, as we approach 40 years of experience with the Soviet-American "arms race." Another problem is multi-collinearity among explanatory variables, making it virtually impossible to weight their individual effect. In models that combine organizational inertia and international reaction elements both may give predictions of ever-rising expenditures, which in fact can be observed historically for rather long periods (Ostrom, 1977). This is particularly serious in those models that specify the level of a country's expenditures in one period as dependent on its own level of previous expenditures (bureaucratic-organizational) or on its opponents' previous level (action-reaction).

A major conclusion of the Moll and Luebbert review, moreover, is of central importance: the necessity to recognize, and to build into any model, not only the likelihood that key parameters will be different for different countries, but that they will be different for the same country at different times. Ostrom begins to lay some groundwork for such efforts by his willingness to descend within the "black box" of the state. In his 1977 work, for example, Ostrom used several models (action-reaction, organizational politics, and a "naive" model that the best indicator of this year's expenditure is last year's expenditure). He also devised a reactive linkage model (Ostrom, 1978). Moll and Luebbert (1980) point out—without being able to explain it—that the reactive linkage model is more successful for some years than for others; namely the naive model is more successful for the periods 1955-62 and 1969-73, with the reactive linkage model more successful for the middle years. Moll (1974) himself had found significant changes in parameters in analyzing British naval expenditures prior to World War I, and it is a major component of Choucri and North's (1975) work on all the great powers before World War I. Not only did Choucri and North find different coefficients (at widely varying levels of significance, and even opposite signs) for different countries, but they found the coefficients for individual countries very different in the period 1870-1890 than during 1890-1914. Similar variations in results have been obtained by Bishop and Sorenson (1982), Hollist (1977a, 1977b) and Rattinger (1975), though the first analysis is quite simple and in the last three cases methodological deficiencies (such as autocorrelation, a frequent problem in many such studies) cast doubts on the utility of the findings.

Recent methodologically sophisticated work nevertheless confirms the necessity of investigating the possibility of finding very different coefficients for different countries and/or times. For instance, in applying the basic Choucri-North model to American-Soviet-Chinese interactions in the post-World War II period, Ashley (1980) concluded that the United States partly responded to Soviet spending, but he also found a large measure of inertia sustained by the domestic military establishment and national security bureaucracy. The Soviet Union's patterns, by contrast, seemed dominated by domestic forces, and did not respond in any regular way at all to American military spending. Ashley did, however, find some Soviet response to *Chinese* military spending after the two great communist states broke off relations. This seems to confirm Rattinger's conclusions that action-reaction processes were significantly more discernible in NATO countries' expenditures than in those of Warsaw Pact members. Similarly, in analyzing some pre-World War II data, Lucier (1979) found clear parameter shifts associated with some changes in political administrations within the same country. Gillespie *et al.* (1980) indeed suggest the extreme sensitivity of American and Soviet perceptions of each other's constraints and intentions; slight changes in parameters have the potential to set off an explosive arms race.

It is reasonable, after all, to expect that domestic politics do matter. Certain leaders or administrations may be more tolerant of military spending by the other side, or less willing to divert funds from domestic civilian needs, than are others. We have seen it clearly with the advent of the Reagan administration, which has taken decisions to shift spending from civilian to military purposes to a degree that is unprecedented for the United States (Russett, 1982). This was a deliberate political choice, though observers can disagree as to the degree to which it reflects the change in the administration per se, or the degree to which it reflects a broader shift in domestic political preferences —of which the administration would be only the agent—that in turn may be rooted in the higher level of international political tensions. Some might argue, furthermore, that high international tensions themselves have resulted from the change in the administration. Cause and effect are not readily distinguishable here. Moreover, similar sharp shifts in either Soviet or American military spending are not discernible for previous changes in political administrations of either country. One must beware, therefore, of ad hoc "political" explanations and dig carefully to find the driving components.

We need, therefore, more careful specification of what is meant by domestic or internal influence. Is it basically just a matter of bureaucratic politics or organizational inertia, in effect captured by the autoregressive term in our equations, predicting that this year's budget will be like last year's, only a little more? Despite the apparent plausibility of this explanation for some of the cold war period, it does not hold for all years, especially in the post-Vietnam years of the early and mid-1970s in the United States, when the force of bureaucratic inertia was substantially if temporarily reduced. Nor did the actual drop in American military spending produce comparable restraint by the Soviet Union, as the action-reaction proponents would have us expect. (We would not, however, expect prompt Soviet restraint if weapon stocks were the key variable.) At the beginning of the 1980s, fiscal constraints seemed to be lifted, and a new organizational momentum established. If the auto-regressive expectation is to be modified—damped, as after Vietnam, or perhaps magnified, as under Reagan—how do we explain these political phenomena? Moreover, can we generalize about internal political phenomena as themselves reflecting or responding to regularly changing political or economic conditions?

We can now say, with confidence, that internal processes do matter, and matter a great deal. All levels of analysis (Russett & Starr, 1982) are engaged. This conclusion has important implications not just for academic study, but for political efforts to achieve arms control or disarmament. The most sophisticated negotiations to bring international action-reaction processes under control are doomed if they do not also take into consideration the realities of bureaucratic inertia and wider domestic political processes. Individual actors within governments will change, bringing new motivations and different responses to various stimuli. Individuals may change their motivations. Force planners and political leaders typically develop a sense of strategy. Rather than merely mechanically act and react to their opposite numbers in another state, they will anticipate reactions in ways like those suggested by game theory.[5]

All this requires some retreat from hopes of deriving general and relatively simple propositions about the universal relative importance of internal vs. external influences on arms races. The relatively simple expectations of the early researchers have given way, with the slow accretion of knowledge, to very complex expressions of understanding. The phenomena sometimes called "arms races" are more varied than we might like, and will resist the parsimonious explanations that we would surely like. The implications of this will not necessarily please "realists," and they may be more satisfying to students of comparative foreign policy than to students of international systems.

ECONOMIC AND SOCIAL EFFECTS OF DEPENDENCE

The body of work known as dependency literature[6] has been concerned with the effects of foreign penetration on third world countries. Specifically, proponents of one version or another of this concept contend that economic and cultural penetration of third world countries, incorporating them into the global capitalist system, creates distortions in economic, political, and social conditions. Operating over a substantial historical period, the agents of foreign capital interact with local classes under conditions that vary in different times and in different types of countries. Thus the structure of the world economy, combined with the structure of class relations in third world states, in varying forms and degrees leads to new forms of state organization, intense class conflict, and harsh state repression in those states. External and internal influences are held to interact. Like the arms acquisition literature, the dependency literature is rife with controversy over their relative importance.

The more recent contributions to dependency writing have focused chiefly on the intensification of class conflict and the growth of a state coercive apparatus. Class conflict in dependent societies is seen as resulting from great and frequently growing inequalities within and between classes, that in turn are viewed as a consequence of distorted patterns of economic development. Earlier contributions to the dependency perspective, however, were concerned also with the degree of economic growth. Frank (1972), for example, argued that penetration by foreign capital had the overall effect of depressing economic growth; while it of course might promote growth in certain sectors of the economy, overall it permitted the expropriation of surplus capital and discouraged the formation of local capital in those sectors of the economy that had promise of self-sustained, autonomous growth. This argument was in part supported by economists working with the Economic Commission for Latin America (ECLA) (see Prebisch, 1963) who, though not strictly dependency theorists, contributed to the tradition. They saw the economic stagnation typical of Latin American economies during the late 1950s as a result of excessive concentration on producing primary commodities for which the terms of trade had declined drastically after the Korean War. The solution, according to these observers, was to develop manufacturing industries for local markets, thus encouraging the growth of a dynamic sector by import substitution and some insulation from the interests of foreign capital. Later dependency writers (e.g., Cardoso, 1973) have shifted both the argument and the conclusion. They have argued that the penetration of foreign capital may well promote economic growth per se, in the sense of a more rapid rate of growth in GNP per capita, but nevertheless contributes to distortions in development, inequalities, and the loss of "popular" control over the pattern of that development.

We cannot review all these arguments here, nor consider the body of research that now bears on these questions. Some of the most interesting involve the role of the state and the interaction of insurgency and state coercion under the influence of foreign penetration, a topic on which little conclusive research has yet been done. (But see Duvall *et al.*, 1982; Duvall *et al.*, 1981; Freeman & Duvall, 1983; *et al.*, 1983; and Russett *et al.*, forthcoming.) Instead, we shall focus on two particular questions which probably have been the subject of the largest body of systematic empirical research to date: the effect of penetration by foreign capital on rates of growth in peripheral countries, and the effect of that penetration on patterns of inequality. Both of these questions have been investigated extensively, especially by North American scholars heavily influenced by the "positivist" quantitative research tradition common on this continent.

The attempt to estimate equations for propositions from the positivist perspective, using cross-national data from the last two decades, bears many intellectual resemblances to the arms race modelling tradition we have just reviewed. As we shall see, it also shares many of the same difficulties. It must be recognized that much of the writing in the dependency tradition treats dependence as a contextual condition, rather than as a ready set of propositions for cross-national quantitative analyses (Caporaso, 1978; Duvall, 1978).

Nevertheless it still is fair to treat the dependency perspective as a source of ideas from which testable hypotheses can be generated.

Dependence and Growth

The relationship between foreign penetration and economic growth is not one of the central concerns of dependence theory as it can now be understood, but it has nevertheless been important in the development of that body of literature. Moreover, it is the aspect of that theory on which the largest body of cross-national aggregate-data analysis exists in sociology and political science.

By now there is nearly unanimous agreement that, at least in the short term, penetration by foreign capital is associated with higher rates of overall economic growth as measured by GNP per capita or, occasionally, by per capita energy consumption. A positive relationship between the inflow of foreign capital and economic growth has been found in almost every cross-national investigation of less developed countries (LDCs): Bornschier, 1981b; Bornschier, 1980; Bornschier *et al.*, 1978; Dolan and Tomlin, 1980; Jackman, 1982; Kaufman *et al.*, 1975; Mahler, 1980; Papanek, 1972/73; Ray and Webster, 1978; Stoneman, 1975; Szymanski, 1976. This relationship holds for various measures of financial penetration, though the most common is net foreign investment. The only exceptions seem to be early studies by Stevenson (1972) and Alschuler (1976) limited to Latin American countries only, and a study by Rubinson (1977) which included *all* non-socialist countries in the sample (i.e., developed capitalist industrial economies as well as LDCs). While the latter may provide some useful information, it is inadmissable as a test of conditions in LDCs. Dependence theory clearly is concerned only with the latter—and, indeed, only with capitalist, market economies, not with socialist economies like Cuba or North Korea.

It should be noted, however, that the data themselves merely point to the fact of a positive relationship, not necessarily to a causal relationship. That is, one may contend that the inflow of foreign capital contributes to or makes possible the subsequent economic growth, or that the foreign capital is attracted precisely to those countries that would have grown most rapidly in any case. It is common in these studies to include GNP per capita in the equation as a control variable in order to see whether the effects of foreign penetration are different in poor LDCs than in middle-income countries, but it is very rare to see previous growth rate entered as a control. Nonetheless, there is reasonable theoretical agreement that the short-run effect of an inflow of capital may well be a spurt in growth.

The consensus on the short-term effects of foreign penetration dissolves when we move to the long-term effects. In the arms acquisition literature we found that the relationships that held for flow could be very different from those for stocks. Here too, it is one thing to talk about the flow of capital, another to discuss the effects of an accumulated stock of foreign capital. The majority of studies hold that the long-term effect of capital penetration is to retard economic growth to a rate below that which would apply in the absence of foreign capital. But the situation is theoretically as well as methodologically complex, eluding full agreement.

The argument that a large accumulated stock of foreign capital depresses subsequent growth has been supported most extensively by Volker Bornschier and his colleagues (Bornschier, 1981b; Bornschier, 1980; Bornschier & Ballmer-Cao, 1979; Bornschier *et al.*, 1978; Bornschier, 1975). Their findings are reinforced by similar findings reported by Dolan and Tomlin (1980), and Gobalet and Diamond (1979), as well as earlier and less sophisticated work by Stoneman (1975) and Evans (1972) on Latin America alone.

Bornschier contends that the fundamental mechanism is penetration by multinational corporations (MNCs), attracted by intervention by the state in less developed countries to provide infrastructure, subsidies, protective tariffs, tax exemptions, and so on. This investment will be concentrated in industries where MNCs have already gained an advantage with their advanced technology in developed countries (e.g., manufacturing). But because of the small absolute size of LDC markets and the highly skewed distribution of personal income in those countries, MNCs are unable to create indigenous mass markets for their products. Sometimes foreign investment will move into industries directed toward external sales, often to developed countries—the export platform phenomenon (see, for example, Caporaso, 1981). But if the appropriate conditions, such as a skilled, well-disciplined local labor force and state incentives, do not exist, initial investment in all but the largest LDCs is not likely to spread into other sectors. Overall foreign investment will slacken, MNCs will divert their new investment to other countries, and repatriation of earnings will become substantial. Meanwhile, MNC penetration into what had been the dynamic sectors will have discouraged investment by local capitalists who cannot effectively compete with the advanced technology, ample capital, and vertically integrated markets of the modern MNC, and diverted state capital formation into infrastructure supporting the MNCs' investments. Once MNC investment slackens, relative stagnation results.

At first Bornschier *et al.* (1978) maintained, supported by Dolan and Tomlin, that this effect was stronger in the relatively well-to-do LDCs, a conclusion disputed by Gobalet and Diamond (1979). Reanalysis by Bornschier (1981b), Bornschier (1980) and Bornschier and Chase-Dunn (forthcoming) suggests that the income effect is spurious because of the substantial correlation, among LDCs, of income with market size. Once the smallest—and usually poorest—LDCs are excluded from the analysis, writes Bornschier, the interaction of MNC penetration with wealth on growth rates essentially vanishes. Bornschier also reports that the growth-reducing efforts of an accumulated stock of MNC capital are greatest in countries where that investment has been concentrated in manufacturing and mining, where MNCs' technological advantage is likely to have been greatest.

As we have indicated, these results are not free from controversy. Among those with seemingly contradictory findings, we may dismiss the early studies (such as Papanek, 1972/73) and especially those with samples limited to particular geographical areas (Kaufman *et al.*, 1975; McGowan & Smith, 1978; Ray & Webster, 1978; Szymanski, 1976). All of these suffer from methodological limitations whose details need not concern us here. A more serious challenge appears, however, in Jackman (1982), and in a critique of Bornschier's most recent article by Szymanski (1984). Jackman concludes not only that the flow of investment is positively related to growth, but that among the poorer LDCs even the stock of foreign investment is positively related to growth. He argues that the apparent negative relation of foreign investment stocks to growth washes out when one controls for total population. Population may here be seen as a surrogate for market size (total GNP would have been better) since Jackman argues that the possibility of economies of scale is what promotes growth. He also argues that it is unchecked population growth, not MNC penetration, that restrains per capita growth, and that one must control for birth rates. But as Bornschier (1984) points out, Jackman's results are untrustworthy because his equation is mis-specified. Jackman's dependent variable is per capita income growth 1960-78, yet his data for stock of foreign investment are for 1967—the latter could hardly be responsible for growth between 1960 and 1967.

Szymanski (1984) contends that by "controlling" for the "spurious effects" of investment in mining and manufacturing, and for the volume of total *domestic* capital formation, Bornschier in fact eliminated the mechanism by which economic growth in most LDCs now is driven—the *reinvestment* of local earnings and the mobilization of local capital. Szymanski nevertheless accepts Bornschier's basic empirical finding while disputing the process which purportedly explains it: Szymanski declares that whereas *among* countries dependent on foreign technology and entrepreneurship those with more MNC investment grow faster; the fastest growing LDC market economies are those with relatively little MNC investment, but with locally funded accumulation and a technologically advanced domestic private sector (e.g., the Asian MICs). Bornschier (1984) nevertheless holds that a significant relationship remains even without the "controls," though it is weaker. It should be noted furthermore that many results (e.g., Bornschier, 1981) that are statistically significant nevertheless seem to have coefficients that are quite small relative to the size of the phenomena in question. That is, the actual "impact" of a variable like MNC penetration may be very small relative to the amount of growth actually occurring.[7]

A further critique (Weede & Tiefenbach, 1981c) maintained that Bornschier's findings virtually washed out with different specifications of the model, notably with inclusion of the relative size of a state's military establishment. Bornschier (1982) nevertheless replied that Weede and Tiefenbach mis-specified the model by using a growth rate initiated before the investment, and by failing to control for the short-term positive effect of a continuing inflow of investment. With these corrections, says Bornschier, his results hold whether or not there is a control for the size of the military. A further reply of Weede and Tiefenbach (1982) seems to me not to be compelling, though it does emphasize the sensitivity of all these results to methodological and theoretical decisions in the analysis.

The controversy doubtless hangs in part on the vagaries of cross-national aggregate data analysis with fairly small and sometimes varying samples. Such statistical analyses are very sensitive to what seems to be minor specification error in the equations, and to the effects of outliers or the exclusion of particular cases. There also is the possibility that analyses will show different results for different periods of time. Recall our earlier conclusion about the arms race studies; here the cause might be, not the change of regime in a single national actor, but fundamental shifts in the mechanisms operating in the world economy. For example, Dos Santos (1970) among many others, identified a period of "new dependence" when the most potent mechanisms of foreign penetration had shifted first from trade to direct private investment, and then from direct investment to the sale of core countries' technology and the accumulation of vast public debt to commercial banks. It would therefore not be surprising if one measure (e.g., direct investment by MNCs) were to show different results at different times. However, that does *not* appear to be the case in any regular way with these studies.

The debate can correctly be seen as one requiring more careful specification of the theoretical model underlying empirical analyses. This is not just a matter of the need for sophisticated measures of concepts or of incorporating appropriate controls into the analysis, but of recognizing the importance of different contexts, and the mediating effects of key variables. Form and structure of explanation will vary. That is, foreign capital affects growth rates not in some mechanical accounting manner, but by provoking, channeling, or stifling state initiative, by stimulating or repressing entrepreneurial and savings decisions by classes within the LDC, and by enriching certain classes and groups at the relative or absolute expense of others. Only if these processes are recognized—allowed for in the model specification, and the empirical results interpreted in light of reasonable interpretations of such processes—can we hope to obtain robust and intellectually compelling findings.

Our own empirical work on this topic illustrates both the need and the difficulty.[8] We did not investigate the short-term effects of capitalist penetration at all, but instead concentrated on the effects of an accumulated stock of capital, as in the Bornschier formulation. Then, instead of relating stock to economic growth in a simple manner, we employed a distributed lag formulation on the principle that dependence theory typically was concerned with the effects of the history of penetration over time, and that the progressively decaying effect captured by the distributed lag most nearly reflected that history. Furthermore, our concern was with capitalist penetration, broadly conceived, from the industrialized countries; hence our measure was a complex one incorporating not just direct foreign investment, but all foreign debt, the stock of imported capital goods, and relative reliance on the stock of "disembodied" foreign capital: licenses, patents, and trademarks (see Jackson, 1979).

Our results were not the same as those of earlier analyses. For example, we found that among most LDCs (all but the most prosperous ones) capitalist penetration seemed to result in higher rates of GNP per capita growth during the period 1970-75. Moreover, this effect was not only a direct one, but part of a process whereby capitalist penetration typically increased the concentration of commodity exports during this period, and the concentration of commodity exports in turn was associated with higher average income levels. This seemed to indicate that capitalist penetration strove to promote greater commodity exports in response to the boom in commodity prices during this period, and that swelled export earnings and thus raised average incomes in these countries. This interpretation seems statistically robust (not subject to distortion by outliers or modest variations in the functional forms of specified relationships), and it is supported by partial results elsewhere in the literature. For example, the direct relationship of capitalist penetration to growth holds for the poorer countries, but not the richer ones, where it is negative in our data base. (But this applies to a group of only seven countries, and we are not inclined to make much of it.) Bornschier *et al.* (1978) and Dolan and Tomlin (1980) found the negative effects of foreign investment significantly stronger in the richer LDCs than in the poor ones, though Bornschier later dismissed that as spurious. We do not feel confident that we have fully understood the processes by which this happened. Clearly resource base, local conditions, and "history" do matter.

Dependence and Inequality

We can turn from the puzzle over the effects of capitalist penetration on growth to a seemingly more consensual set of findings on the effect of that penetration on the distribution of income within LDCs. Save for an early and flawed study (Tyler & Wogart, 1973) that found no significant relationship, until recently there was unanimous agreement that penetration resulted in greater inequality. Most of the relevant studies concerned the effects of financial penetration, i.e. investment (Bornschier & Chase-Dunn, 1984; Bornschier, 1981; Bornschier & Ballmer-Cao, 1979; Bornschier, 1978; Bornschier *et al.*, 1978; Bornschier, 1975; Chase-Dunn, 1975; Evans & Timberlake, 1980; Kaufman *et al.*, 1975; Mahler, 1980; and Rubinson, 1976), but others used as independent variables trade concentration (Galtung, 1971; Dolan & Tomlin, 1980; Jackman, 1975; Ahn, 1981), exports as a percentage of GDP (Stack, 1978), and a measure of position in the global hierarchy (Ward, 1978). No study has found foreign penetration having the effect of reducing inequality.

Recently there has been some challenge to this consensus. Dolan and Tomlin (1980) reported no relationship, and Weede (1980) has reported some reanalyses of others' results with a different specification. All of the more substantial studies have included a linear control for income level when analyzing the effect of penetration on inequality, in consideration of the well-known negative relationship between income level and inequality among poor and not-so-poor LDCs. Weede insists that when this control is properly specified (that is, with a polynomial function, as derived by Ahluwalia, 1976) the control then becomes stronger, and takes up much of the effect otherwise attributed to penetration. He reports (Weede, 1980; Weede & Tiefenbach, 1981b) the positive relationship turns into an insignificant one in Rubinson's work on trade, and in Bornschier's (1981b) work on foreign investment. Bornschier retorts (1981a) that his relationship still holds for LDCs—though not for a sample of countries at all levels of development—and even more clearly when an improved measure of foreign penetration is used. Weede and Tiefenbach (1981b) reply with a caution about changing indicators in midstream, the need for examining multiple indicators, and the need for careful conceptualization before measurement. Bornschier's finding of a positive relationship even with the Weede polynomial specification is supported by Timberlake and Williams (1982) and by recent work on the effects of concentration of export commodity markets (Stack & Zimmerman, 1982).

We also found a positive impact of capitalist penetration on inequality, but the relationship is not a simple one. One route is through the interaction of capitalist penetration with what we call, from the dependence literature, uneven development (differences in wage rates by productive sector). Among the poorest LDCs this interactive term has almost no effect on inequality, but in the richer ones the positive impact on inequality is substantial. This is essentially what we originally hypothesized.

More puzzling, however, is the evidence for a chain of influence that runs through marginalization. Marginalization is defined as "the extent to which groups are incapable of maintaining their economic position in society." By this we mean both the stagnation of living standards over time (measured by real wages) and the creation of a "reserve army of the unemployed," especially the urban unemployed (measured by the share of economic activity accounted for by wages). We found that capitalist penetration, interacting with what we termed economic heterogeneity (differences in output per worker in different economic sectors) usually had the consequence of reducing marginalization. On examining the cases, we inferred that capitalist penetration did this in two ways. When concentrated in the rural areas, especially for industry or extractive enterprises, it had some job-creating effect that slowed the migration of surplus agricultural workers to the cities where they often joined the ranks of urban unemployed in the shantytowns. When concentrated in the urban areas, it did have employment-creating effects in those areas that had already attracted rural migrants—although a fairly modest effect, as is rather typical of capital-intensive invetment. More surprising is a subsequent negative link from marginalization to inequality. This seems counter-intuitive, and certainly is not what most dependence theorists—or we—would expect. It is interpretable, however, especially if we recognize that our measure of inequality is, unlike that of virtually all the studies reviewed above except Ward's (1978), concerned with welfare instead of simply income. It reflects equality in access to sanitation and health care (see Russett *et al.*, 1981). Effectively the relationship captures the fact that marginalized people, driven from rural areas to urban shantytowns, have not necessarily worsened their material conditions broadly defined. Shantytowns, however miserable, are at least part of urban areas where some public health care *may* be available—it may simply be non-existent in the countryside—and where, especially in the last decade or so, many LDC governments have made efforts to provide *some* of the basics for decent existence, such as piped water.

Hence the two negative links may combine to create a complex positive relationship. That is, penetration reduces marginalization, but because marginalization is negatively related to inequality, less marginalization means greater inequality: thus increased penetration increases inequality! This is surprising to us, and we do not want to over-emphasize its importance. It does not rule out the likelihood that the more direct positive relationship usually hypothesized in the dependence literature (capitalist penetration creating a rich bourgeoisie and small labor aristocracy while exploiting masses of poorly- or sporadically-employed workers) also applies. But it does require anyone writing on this topic to reexamine her or his ideas about the processes and class relations underlying these aggregate statistical relationships. It demands that the largely cross-sectional aggregate data analyses be enriched and interpreted by intensive case studies of changes over time in individual countries. The aggregate data analysis, in its turn, provides some hints for new hypotheses to apply in carrying out those case studies.

CONCLUSIONS

Three conclusions need to be stated. First, in the analysis of situations of dependence the importance of varying conditions within LDCs (size, colonial history, nature and time of foreign penetration, resources, etc.) is apparent; the process is not simply one of external penetration producing reliable regularities everywhere. Clearly this shows the importance of detailed country-specific knowledge, and the country-specialist's familiarity with the details of a particular economy, society, and polity. This is chastening for those of us—international relations specialists or students of the world system—who saw ourselves as striking a blow for the importance of international processes in what had often previously been treated as the largely intra-country analysis of political development. It also emphasizes the role of transnational actors (particularly MNCs) also traditionally neglected in a "realist" perspective. These complexities are reminiscent of those that emerged in the arms acquisition analyses, where we found "realist" assumptions equally naive.

To be sure, we find patterns of behavior that are complex, interactive, and heavily conditioned; nevertheless they do yield generalizations and regularities. It is *not* the ideographic extreme of "every country so different that no general regularities can be derived." The theoretical and methodological sophistication now built into more than a decade of systematic empirical investigation is beginning to produce some results. As we understand the relationships we can hold the results with greater confidence.

Second, some of the empirical findings reviewed in this chapter, both in the analysis of dependence and that of arms acquisitions, emerge as counter-intuitive. Thus they require a continual re-examination of our hypotheses in a way that is sensitive to these contextual differences. We may still refer to our efforts as hypothesis-testing, but it is not quite a matter of always rigorously deriving our hypotheses from a full deductive system. It is more a matter of working in a tentative, continual "re-modelling" fashion, where we revise our hypotheses to take into account varying contexts. The acceptance or rejection of hypotheses is rarely clear-cut. In one sense the experience reminds me of the caution sounded quite a while ago by Hayward Alker (1966) in countering critics of early mathematical applications to international relations: a good theorist does not expect most relationships to be simple (linear, bivariate, without exception). The world is complex—but the mathematics can handle that complexity if we will allow it to do so. In another sense the experience reminds me of what goes on in most other disciplines, from the practice of a historian to that of a biochemist who must take into account genetics and environment as conditioning factors in the processes (s)he would study. A carcinogen may produce cancer only in a particular patient who has an inherited predisposition and an aggravating environmental influence. Being systematic means expecting complexity.

Finally, the inability of "realist" assumptions to capture reality accurately suggests fundamental consequences. In the realm of theory, it requires us to give more attention to perspectives that can explicate the behavior of thinking, perceiving, choosing actors; actors who can behave strategically, anticipating the choices of other actors. In the realm of norms, it requires us to surmount the determinism often associated with "realism," and provides us with opportunities to specify patterns of behavior that are different from, and alternatives to, those of a global system composed predominately of nation-state actors.

NOTES

1. This review is based on a search of what we judged to be the 14 most relevant journals for the past five years, plus review articles, books, unpublished papers, and other articles known to us. Also helpful was a review article by Intriligator (1982). Our review is perhaps not exhaustive, nor have we cited here everything that we examined. The goal is not to provide a complete set of bibliographical citations, but to cover the major contributions and improvements that have been made to the systematic empirical study of arms races. We concentrate on the Soviet-American arms acquisition process because of its importance and the intense study it has received. Other pairs of states' relations are in some ways simpler, in others (for example, their penetration by super-power influence) more complex.
2. The computing algorithm is also much more sophisticated than those usually employed, and better than ordinary least squares, but in this review I am choosing to concentrate on matters of concept and measure rather than computational technique. The choice of the latter does matter greatly, but involves yet another set of issues.
3. I am grateful to Jim Lindsay for this observation. Stoll (1982) notes that a negative coefficient is likely to occur in cases where one country attempts to maintain some particular percentage ratio over an opponent.
4. One element in Ward's success is his return to the continuous framework of Richardson's differential equations, whereas most other arms race modelers have instead employed the discrete version of difference equations.
5. There have been efforts to derive models based on rational and optimizing assumptions (for example, Simaan & Cruz, 1975, and Schrodt, 1976), but few empirical tests save for Gillespie *et al.* (1977) where the optimal-control model seems to add little in the Soviet-American case, and Wallace and Wilson (1978) who do not give us the estimated coefficients for their equations.
6. To maintain clarity between the various conceptualizations of this literature, I will distinguish between its main sections by calling one "the dependency perspective (outlook or approach)" and the other "dependence theory." Dependency analysis precedes dependence theory chronologically and includes such authors as Cardoso, Dos Santos, and Gunder Frank. It is written predominantly by Latin Americans and uses a dialectical or "structural-historical" method of analysis. In contrast, dependence theorists use quantitative statistical methods to create rigorous theories

and sub-theories that are derived from the conceptualizations of the dependency perspective. Dependence theorists include Bornschier, Chase-Dunn, Mahler, and Rubinson. When speaking of both sections of this work, I will call it "dependency literature." This chapter focuses on dependence theory.

7. I am grateful to Steve Silvia for this observation.
8. I will here report only briefly on some of the findings emerging from the collective project on which a group of us has been engaged for some years, to be reported in Russett, Duvall, Jackson, Snidal, and Sylvan, forthcoming. It is not possible to report on a complex data analysis sufficiently here, nor would it be appropriate for me to do so, in an essay signed by me alone, on a project that has been so truly collaborative.

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ADDRESSES FROM THE 1982 LASSWELL SYMPOSIUM: THE USES OF SOCIAL SCIENCE

18

Politics and the Uses of Social Science Research

Donna E. Shalala

When I received this invitation, I knew I could not turn it down, but I have to admit I was terrified. I have been away from my own research for a number of years—something bound to raise the anxiety level of one addressing a gathering of colleagues.

The letter of invitation did not explain: "why me?" I assume it was not my gender because there are very able women who write in this area. No matter. Who could resist the opportunity to join a symposium honoring the author of works like the aptly titled *Politics: Who Gets What, When, How?* Indeed, I assume I was invited as the practitioner and because I am on that short list of political scientists who have survived a tour in Washington—a city dominated by lawyers, economists, and businessmen who prize nothing so much as pragmatism.

I recommend for your amusement Bruce Adams' article in the *Public Administration Review* titled, "The Limitations of Muddling Through: Does Anyone in Washington Really Think Anymore?" In the article Adams suggests that the political decision-making process is characterized by individuals who are "running themselves ragged on a series of marginal, short run issues and problems."[1] And I must confess, "I'm being nibbled to death by ducks" was an all-too-telling decision-maker's refrain.

Indeed, many have argued that Washington is a city where success is measured by long hours and tremendous personal sacrifice. They say the easiest way to identify assistant secretaries at Washington dinner parties is that they start yawning at 10 p.m.

But it was worth every sacrifice, every bit of stress. I am glad I went and even happier I survived. If I have one regret, it is that I was born too late to be an assistant secretary in the Johnson administration or any administration in which budgets and the role of government were expanding, because, quite frankly, I arrived in Jimmy Carter's brave new zero-based world a bit unprepared. No one at the Maxwell School in the late sixties had taught me the politics of scarcity. I honestly did not know how to solve our massive urban problems without spending, or at least leveraging, significant public monies.

And I was not enough of an academic to take much comfort in the fact that no one else did either!

Today, "retrenchment" management is still pretty much a virgin field and, paradoxically, if Reagan's budget slashers have their way with social science research, it will remain one—in theory, if not in practice. It is in the context of this challenge that I want to share some observations on the politics of the uses of social science research.

While some current woes can be pinned on passing political phenomena, many of our difficulties spring from deeper sources. Part of the problem stems from the perception of a vague status and amorphous mission of social science research and social scientists in our society.

In some sense, social science researchers have become the whipping boys (persons) of both the research and political communities. At no time has this been more apparent than in the Reagan administration's recent frontal attack on the budget of the Division of Social and Economic Science of the National Science Foundation. According to the Director of that Division, "1981 may be remembered as the year that social scientists paused from their ancient wars with each other to engage an external threat that became defined as a larger enemy."[2]

This threat triggered a "train of events . . . perhaps unprecedented in the history of the social sciences"[3] that strengthened our view of where and how secure we are on the ". . . wider continuum of scientific and scholarly pursuits."[4]

It has also forced us to rethink the role and nature of federal funding for basic as well as policy research in the social sciences. As one supporter observed: "The slap in the face administered by OMB may contribute more than anything else to social scientists shaping up a compelling case for the value and utility of their field."[5]

THE ATTACK

The official rationale provided by the Office of Management and Budget (OMB) for the recommended 1981 funding reductions was that "the support of these sciences is considered of relatively lesser importance to the economy than the support of the natural sciences."[6]

But Irving Louis Horowitz provided a more honest explanation. Pointing out how Ronald Reagan had made great use of social scientists both as candidate and as President, Horowitz then said that he believed NSF's lack of a clear policy orientation to its research led to its downfall. But in fact, as Horowitz correctly explains, this has always been a conscious policy. The NSF social science group has consistently rebuffed any efforts by Congress and cabinet agencies to make its work more applied. Applied work was left to the cabinet agencies' research budgets.

Then Horowitz addresses a more basic question: Is there really even such a thing as a social science?

> The present situation in the social sciences can be described as one of Balkanization, a fragmentation so severe that no unified paradigm prevails. In place of

> a uniform sense of social science research are a series of belief systems of little concern at the level of presidential decision-making. The inner resolve of social scientists has been sapped not only at the periphery, but at the core. Indeed, a core may no longer exist. Any primary journal within the major social sciences exhibits a range of articles of such diversity and such varied levels of methodological sophistication that it would tax the mind of a schizophrenic to read such a publication from start to finish. This is not to argue that we should determine a priori who is right and who is wrong, or which theoretical framework is inferior or superior, only to assert that the social sciences are being assaulted, not only from the edges inward, but from the innermost core outward.[7]

Prewitt and Sills, acknowledging the lack of support in the past from the leaders of science, add two additional explanations: (1) ". . . social scientists, in comparison with engineers and with physical and life scientists, have been politically naive—not even seeing the need to be a political presence in Washington"; (2) ". . . social scientists have been indifferent toward their own intellectual and practical accomplishments, and correspondingly timid about telling their own story."[8]

There are, of course, additional explanations. OMB clearly perceives this vulnerability. But even more important is the social scientist's apparent lack of political clout. Unlike the so-called "hard" sciences, social science has never enjoyed a broad and active political constituency. No top military brass sings social science research's praises. No tough right-to-life (or choice)-like lobbies work Capitol Hill on our behalf. Indeed, until the recent NSF attack, it was hard to think of a more politically disorganized—and so, politically impotent—group. Small wonder then, that OMB simply regards our research as a nonessential, non-entitlement program. In short, it is easy to cut.

Our political vulnerability is nothing new. Explaining the opposition to social science being included in the National Science Foundation legislation earlier in this century, Roberta Balstad Miller noted ". . . the social science community did not make a strong bid for inclusion with the natural sciences in the Foundation." Again, according to Miller ". . . the opposition of key scientists to NSF support for social science research" was augmented by ". . . conservative fears that social science research would emphasize such potential political problems as racial inequality in the United States; [these fears] undermined Congressional support for the social sciences. . . ."[9]

Thus, although the Reagan Administration is unsupportive, we should keep current attacks in historical perspective. Remembering that while there has been scattered support, no powerful member of Congress has come forward to champion social science research in the last decade. Today's funding cuts are just the culmination of a longstanding federal attitude that can best be termed "benign neglect"—a phrase whose own controversial origin bespeaks the basis of policy makers' not so far-fetched fear that social science research can sometimes become political dynamite because they can't control the results.

Yes, social science research is that favorite political target: the easy mark. The classic example of this is Senator William Proxmire's now somewhat curtailed "Golden Fleece Awards." But I believe it is also something more than that. Indeed, as Roberta Miller's foregoing explanation suggests, such mean-spirited slurs hark back to what I consider social science research's most insidious political problem: not just a lack of well organized support for, but an actual hidden bias against these disciplines.

Much like the rabid Male Chauvinist who dismisses the "Ladies" claim to equality for fear she just may prove superior, the very critics who viciously mock the social sciences' "usefulness" are often those who most secretly dread its untapped power.

THE IMPACT

The implications of the proposed NSF cuts were very serious. Professor Philip Converse told Congress:

> I do not know whether the Subcommittee fully appreciates the enormous impact the social science programs in the National Science Foundation have had on the several social science disciplines in this country in the past twenty or thirty years, despite the tiny fraction of Foundation resources committed in this direction. I am not talking merely about dollars dispensed, although they are obviously important. I am talking more broadly about an interaction between the Foundation and serious social scientists which has progressively drawn the mainstream of social science away from loose speculation and various social advocacies, in the direction of true basic science, by which I mean the confrontation of verifiable theories of social and economic process with hard empirical data collected specifically to illuminate them.[10]

James G. March, a Stanford political scientist, also warned of the serious implications of an NSF cut:

> . . . federal budgets probably do affect the mix of research that is done, the quality of the research, the kinds of research that will come to be valued and the continuity of research and development.
>
> Although the precise effects of the proposed changes cannot be predicted, it is not hard to make some plausible guesses. Reductions in federal support would make social and behavioral sciences somewhat more theoretical, somewhat less empirical, somewhat more case-specific, somewhat less general, somewhat more expressive, somewhat less of a science. In the context of current and future national needs, it is not obvious that such changes are sensible.
>
> These shifts in emphases are important, but they are arguably of less importance than the extent to which the distinctively harsh treatment of social and behavioral science in the current budget has the unintended and more general consequence of weakening those individuals within social science who argue for systematic, empirical testing of social speculation and strengthening those who see social science as essentially social advocacy. Over the past few decades the major national scientific agencies have been allies of serious social scientists in their efforts to improve the capabilities of social science to address fundamental questions of individual, group and institutional behavior in a carefully scientific way.[11]

RALLYING THE TROOPS

There are a number of ways to build political support for the social sciences. The first effective technique used in the NSF case was lobbying and constituency building. This was achieved by identifying the most distinguished social scientists available and bringing them before congressional committees or other public gatherings; by encouraging social scientists throughout the country to write and visit their congressmen; and by soliciting strong support from the scientific community. A most effective coordinating organization, the Consortium of Social Science Associations, led the charge.

Second, the leaders of sciences, represented by the National Academy of Sciences, the American Association for the Advancement of Science, and the National Science Board, all took strong stands in support of basic research. For example, at its April 1981 meeting, the National Academy of Science passed a resolution expressing ". . . its deep concern over the proposed severe reductions in federal support for basic research in the behavioral and social sciences."[12]

More important, the natural sciences community was thoughtful and articulate in defense of its colleagues. Harvard's David Hamburg, the new President of the Carnegie Foundation, wrote:

> It is certainly true that the benefits of social science research may be harder to quantify than in some other fields, nor do social science findings capture the imagination like some of the recent spectacular advances in the physical, biological, and geological sciences. But science is a seamless web to a considerable extent, with many invisible connections between apparently unrelated parts.[13]

Scientists also pointed out that social scientists were being asked to meet a standard that natural scientists were never asked to meet. In calling for support from the scientific community, *Science* publisher William Carey wrote: ". . . the same act of public faith that legitimizes theoretical and applied research in the physical and life sciences has been withheld from the social and economic sciences. . . ."[14] Carey's statement was a significant breakthrough for, as I have noted, traditionally, the natural science community has not been an ally.

Third, supporters carefully identified examples of the utility of social science, and particularly of basic research, which was under challenge. Some of the testimony was extraordinarily perceptive. For example, the economist Lawrence Klein reported on the development of the $100 million information industry associated with econometrics.

> For nearly forty years I have been laboring on development of statistical models of the economy as a whole. Ideas that were developed in research institutes and in academic institutions grew from very modest beginnings, with support from the Rockefeller Foundation, the Carnegie Corporation, the Ford Foundation, and later from the National Science Foundation, to become systems that are now used on a practical basis in private enterprises, banks, international agencies, federal government offices, and state and local government offices. . . . It is still a growth sector of the economy. Fortunately, my associates and I can now

carry on research without support from the National Science Foundation, but such support was instrumental in getting us to this position, in providing the seed money. Ours is a success story of self-financing. . . .

Klein also argued that ". . . some significant part of our productivity slowdown and general loss of competitiveness is due to the disappointing program of federal research support in the 1970s."[15]

Other social scientists also scoured their research for examples of immediate utility, sometimes with unintended humor. The American Psychological Association trotted out its hierarchy—president, president-elect, and executive officer—to tell OMB Director David Stockman, "We are Americans first and scientists second." They then proceeded to give examples of basic work which had been applied. One such example linked studies of the psychophysiological mechanisms underlying taste with the control of coyotes and wolves around the ranches of California.

Ranchers in California are using the conditioning of taste to control predators without harming valuable stock. Coyotes and wolves are fed mutton laced with lithium chloride to produce an unpleasant reaction. As a result the predators are conditioned in one step to cease attacks on sheep, even though sheep have been preyed upon for generations. Estimates of savings in lost stock run in the millions of dollars.

These conditioning techniques were developed in the basic behavioral research laboratory by researchers who began with no intention of having their results applied in this way. Rather, they were interested in the basic psychophysiological mechanisms underlying taste. The same techniques now are being modified for application to two additional problems that have both economic and social costs: drug abuse and anorexia.[16]

There are numerous examples of specific defenses. But what is most troublesome is why we have to make the case at all. Why is it that the social sciences—so well accepted and institutionalized in our universities and in the private sector—are so often ridiculed by public officials? Why is their present plight ignored by a private sector that couldn't survive or progress without us? How did we drift into this unenviable position?

Some have suggested that white coats would help and wonder whether it isn't just a style and attitude question, a sad case of "cosmetics is fate." But, of course, as social scientists we have already spotted the common slight implicit in the phrase "just a style and attitude question." And this bias crops up again and again. It is not easy to convince a world overawed by engineers, high-tech experts, and "hard" science that work in games theory and human behavior may ultimately contribute much to our defense and health and safety.

I believe social science research is at a turning point in its history. How it decides to position itself in the next few years may determine its ability to attract substantial public and private monies and consequently the nature of the research to be conducted.

PLAYING HARD BALL

I say all of this with some understanding of that sometimes brutal world of public policy-making. Perhaps the most chilling, yet fundamental, truth I learned in government was how incredibly tough it is to keep your integrity, maintain the highest research standards, and continue to be influential. My colleagues and I were in a constant tension-filled struggle to make certain that our research effort dealing with critical public policy issues was absolutely first class, elegant, clear, and useful.

All the classic pressures that Washington could deliver—competition from the parade of fast-pen hired guns and messy policy processes—worked against us. While it is hard to keep your head and play your own game when under fire, never is it more essential. I came to the conclusion that the only way to survive in the sometimes vicious, always tough policy-making politics of Washington is not to compromise on excellence, but instead to strive for a growing sophistication in the initiation, funding, and use of research.

It is just this kind of sophistication and toughness that I believe the social science research community must bring to bear in support of its work. This approach will require a new aggressiveness and, most important, much reflection and perhaps a reorientation of stereotypical ideas of the nature of social science. Indeed, we may be talking about a born-again social science. By that I mean a social science which, after some serious soul-searching, emerges with a clearer sense of its own potential impacts and utility. A born-again social science neither compromises on excellence nor backs away from the necessity or centrality of basic research or the appropriateness and challenges of applied research.

MAKING THE NEW UTILITY CASE

Research for research's sake is not an argument without merit, but it does lack political clout. Indeed, we do well to remember that the value judgments of those who determine social science funding in the private and public sectors have a more utilitarian than idealistic base with "that which is good is that which is useful" as their motto. Harold Lasswell understood this when he wrote:

> It is at once apparent, if it were not obvious before, that the social environment is uninterested in knowledge as an end in itself. The inference is that support for the pursuit of knowledge must be obtained by presenting science and scholarship as means, as base values with which to pursue safety and health, wealth, power, prestige, and similar major outcomes.[17]

In the 1980s we need to make an even bolder point: We need to make the case that, like the natural sciences, this nation—indeed the world—cannot and does not survive without us.

We have had our first experience (in the NSF effort) at making this point. When we reached into our heads to evaluate what contributions to "real world" problems we have made, the lists were impressive. You would think there was not an aspect of American life that had not been improved through social scientists' efforts—to hear us tell it. We have aided American business; we have given politicians vital polls and political analyses; we have informed American journalism and reshaped the judicial process—these just for starters.

THE MISSING LINK: ORGANIZING THE USERS

Social science has never fully come to terms with the utility issue. There are reasons for this. For while it is possible to argue that our work has life or death consequences—as John Kemeny discovered when he wrote that human behavior, not technology, explained the Three Mile Island disaster—such assertions can have a Pandora's box effect. We should be cautious about pushing the utility argument to extremes lest we argue ourselves into that tight spot between Washington's proverbial rock and a hard place. If you convince policy-makers your work is important, all too often their thoughts will run: "If it's important, it must have an impact. And if it has an impact, how then should it be regulated?"

But whatever one's justification, the recent NSF campaign did open our eyes to the need for serious reflection on our aims and justifications. And we have just begun. We need a healthy and productive future, not just in Washington, but with the trustees of our colleges, universities and foundations. To insure that future we must understand our limitations and articulate them.

Ken Prewitt has suggested that we must face up to the fact that "the complexities of the problems for which the social and behavioral sciences might be helpful are always going to be one step ahead of the problem-solving abilities of those sciences."[18] I would put it differently; it is the old "linear answers for a non-linear world" conundrum. But while social science research will never be able to analyze all aspects of a problem or sate supply-siders' seemingly insatiable hunger for easy answers, we have a lot to give to those not hooked on hokey, all-inclusive, "voodoo," quick fixes. I know there is considerable controversy over the effectiveness of policy analysis and analysts. But while I must confess that I went into the government with low expectations for the possibilities of applying social science research to policy issues, I left with considerable enthusiasm for the sheer fun of trying to bring research to bear on massive, real life problems.

If my colleagues and I came to any conclusions it was that there is no model, no single approach to increasing the utility of applied research or justifying funds for basic research. Resisting the reductive bottom line, let me suggest instead a number of semi-conclusions:

- One cannot draw hard lines about the value and applicability of short vs. long term research, basic or applied research. Indeed, we took pains to organize ourselves so that we had significant resources for a variety of syntheses: overnight turn-around, one week to three or four months; or one year to ten years.

- We continually searched for ways to integrate users into the research. This was easiest on evaluations. The $12 million University of Pennsylvania effort to study the Community Development Block Grant program had a local official users' panel as well as a researchers' panel built into the research design from almost the beginning.
- We thought about users before we launched the research—primary users at least. And we defined theorists and methodologists as users.
- We also spent a huge percentage—almost 20% of our discretionary funds—on dissemination and related user activities.
- We continually searched for ways to literally shove the latest research findings into policy-makers ears. From speeches—the only thing a cabinet officer reads more than once—to options papers to press leaks. Getting the attention of busy people takes good humor, imagination, and persistence.
- Sometimes it also required fast, dramatic gear changes. When Patricia Roberts Harris left the Department of Housing and Urban Development to go to the Department of Health and Human Services, former New Orleans Mayor Moon Landrieu arrived to replace her. Mrs. Harris was a "paper person." She loved well written, fancy memos, solid briefing books, and options papers. Secretary Landrieu called me in on his first day to inform me "I don't read." By that startling statement, Secretary Landrieu meant that, like most politicians, he preferred oral briefings. After spending two years building a paper-oriented staff, we reoriented the entire department to a new policy process overnight. I can't say that the quality of discussions changed, but the style was certainly different.

Another Washington experience led me to conclude that it is time that researchers—hungry and as ambitious as we may be—gather the courage to tell members of Congress, cabinet officers, and even assistant secretaries the truth about what research can and cannot do. While we must be flexible, we should not be facile.

One of the things that troubles me about the "utility testimony" is that it plays into the hands of those who think—or pretend—everything is researchable now. Every national problem should not necessarily be subject to scrutiny. There really are some questions that cannot be answered by three-month, or five-year, or even $10 million studies. There are times when we should tell policy-makers, program managers, and other officials that it will take years to answer certain impact questions, and that often our instruments are not sophisticated enough to isolate all causes.

These are just a few insights. I hope we will see a re-energized social science that succumbs neither to the pragmatists nor to the isolationists; a social science that marshals our skills, energy, and enthusiasm for the new battles in Washington; a social science that finds comfortable and powerful allies in the private sector—among the captains of industry, the legal community, and the media; but most important, a social science that remembers that healthy disciplines do not need to be reshaped under utilitarian pressures. Indeed, they cannot, without violating their integrity.

Thank you very much.

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19
Basic Inquiry and Applied Use in the Social Sciences[1]

Donald E. Stokes[2]

It is difficult to probe at all deeply into the relationship of basic to applied research without sensing a remarkable tension between the common understanding of this relationship and the motives that actually underlie these types of research. The dominant view holds that basic and applied research are mutually exclusive categories. But this perceived antithesis is false to the goals that actually drive research in a number of fields.

A CONTRAST OF TWO CAREERS

To suggest the nature of this tension I will begin, concretely, with the way two working investigators perceive the goals that have guided their own research, choosing for this heuristic purpose two of my colleagues in the Woodrow Wilson School, at Princeton. This is a very deanly thing to do, but the contrasting research goals of these two gifted scholars raise questions of much wider significance, as we shall see.

My first case is Frank von Hippel, a physical scientist trained at M.I.T. and Oxford in theoretical particle physics. In the early stages of his subsequent research career he used his theoretical gifts to help push back the frontiers of knowledge in particle physics, pressing especially the insights to be gained by observing the regularities in the interactive behavior of fundamental particles. But later, in the 1960s and 70s, he became increasingly concerned about the societal implications of science. As a result, he substantially refocused his research interests and devoted much of his later work to a series of policy issues with critically important scientific and technological aspects, especially the control of nuclear weapons, the development of energy sources, and the protection of the environment.

This shift did not divorce him from research that required his skills as a physical scientist. On the contrary, he made his greatest contribution by penetrating the scientific structure of policy issues and suggesting options that would have gone unnoticed if the scientific or technological black box had remained closed—and the problem of policy choice was seen purely as optimizing or satisfying across a set of received policy options. As he pursued these policy issues he remained an active research scientist, vigorously employing his scientific insight and technical skills.

But it is also true that he saw this work as far removed from the frontiers of basic research in physical science. He was engaged in a type of applied research that sought to meet important societal needs, rather than to advance what was known of the underlying structures or processes of a scientific field. The satisfactions were enormous in view of the importance of the problems he took on. But von Hippel experienced basic and applied research as mutually exclusive categories. He could do one or he could do the other. But he could not, in the same line of research, do both.

My second, contrasting case is W. Arthur Lewis, an economist trained at the London School of Economics, whose fundamental contributions to the field have been recognized by a Nobel Prize. Arthur Lewis was born on the Island of St. Lucia, in what were then called the British West Indies. As a young man he was awarded a scholarship by St. Lucia's colonial government to study at the L.S.E., where he afterward was invited to stay and teach. He rapidly earned a reputation as a brilliant general economist, although much of his early work was in industrial economics. In 1948 he became Professor of Economics at the University of Manchester.

At Manchester his work was led in a new direction, toward economic development and growth, by a confluence of motives. Important among these was his desire to help with the economic problems that confronted many of the peoples of the earth in a post-colonial era. As a distinguished economist with a keen interest in the Third World his counsel was frequently sought by the governments of the newly independent nations as well as by international agencies attempting to foster economic development. This applied motive was strongly reinforced by his teaching. Students from the new nations of Africa and Asia crowded into his classrooms seeking the intellectual tools that would allow them to better economic conditions in their own countries when they returned home. Arthur Lewis felt almost bound to create and supply to his classes the elements of a new field.

But he was also drawn to his new subject by some of the deepest intellectual puzzles in economics, as he himself would write:

> From my undergraduate days I had sought a solution to the question what determines the relative prices of steel and coffee? The approach through marginal utility made no sense to me. And the Heckscher-Ohlin framework could not be used, since that assumes that trading partners have the same production functions, whereas coffee cannot be grown in most of the steel producing countries.
>
> Another problem that troubled me was historical. Apparently, during the first fifty years of the industrial revolution real wages in Britain remained more or less constant while profits and savings soared. This could not be squared with the neoclassical framework, in which a rise in investment should raise wages and depress the rate of return on capital.
>
> One day in August 1952, walking down the road in Bangkok, it came to me suddenly that both problems have the same solution. Throw away the neoclassical assumption that the quantity of labor is fixed. An "unlimited supply of labor" will keep wages down, producing cheap coffee in the first case and high

> profits in the second case. The result is a dual (national or world) economy, where one part is a reservoir of cheap labor for the other.[3]

His resulting two-sector model of economic development was the principal research cited by the Nobel Committee.

Therefore, Arthur Lewis' research career, far from requiring a trade-off between basic and applied goals, strongly fused the motives of basic understanding and applied use. He saw economic growth as a basic *process,* one that challenged understanding on the most fundamental level. But he also saw it as a basic *problem,* one of critical importance for the countries that were trying to raise themselves above the poverty line.

BROADENING THE CANVAS

In view of the clear overlay of research motives in our second example we may wonder how the premise that basic and applied research are mutually exclusive types—rather than just conceptually or analytically distinct—could ever be widely accepted. But this premise is part of the most widely held view of the relationship between these types of research. Indeed, the idea that basic and applied research are exclusive categories continues to influence the organization of the research community and the framing of public policy on the support of research.

It is an idea with a long and interesting history. Plainly it owes a great deal to the outlook of the natural philosophers who made the great scientific discoveries of the eighteenth and nineteenth centuries. This Scientific Revolution was primarily the work of gentlemen who were passionately committed to the goal of advancing knowledge for its own sake.[4] By contrast, the First Industrial Revolution was almost wholly the work of practical men who were intensely interested in the uses to which their inventions could be put.[5] It is no longer so firmly believed that these industrial pioneers had little interest in science and minimal association with the natural philosophers of their day.[6] But from the eighteenth century onward the ideology of science in Western Europe enshrined pure discovery without thought of practical use as the proper motive for scientific research.

This belief did not exclude the possibility that scientific discoveries might later on have worthwhile uses. This companion idea is as old as Bacon, and bore some resemblance to reality, probably for the first time, by the onset of the Second Industrial Revolution toward the end of the nineteenth century.[7] The technological advances that were then achieved in chemical dyes and electric power, for example, clearly depended on prior advances in chemistry and physics. But pure scientific research, on the one hand, and applied research and industrial development, on the other, continued to be seen as radically separate enterprises, carried on by distinct sets of people, who were driven by different goals. The belief that to engage in one was not to engage in the other was as deeply held by the Midlands industrialist who remained in business only until he amassed the wealth that would allow him to become a gentleman scientist, pursuing his experiments without the slightest thought of practical use, as it was by a brilliant applied scientist such as Thomas Edison, who never allowed himself or anyone to whom he paid good money at Menlo Park the slightest leeway to pursue the pure scientific implications of their laboratory findings.

This outlook was heavily reinforced in the nineteenth century by the Germans' success in institutionalizing the separation of basic science from applied science and technology—to the apparent benefit of each. Under the German system, pure science was reserved to the universities and scientific institutes, while applied science and technology were the preserve of industry and the *Technische Hochschulen.*[8] The spectacular German advances in both science and technology made this system widely influential. One consequence, as the German plan of doctoral education spread to this country and large numbers of Americans went to the German universities to be trained in science, was to widen the presence of pure science and its ideology in our own universities, where the earlier "scientific schools" created by Harvard, Yale and other universities, were mainly institutions of applied science and engineering.

The survival of this polarized vision of basic and applied research down to our own time is remarkable. In this country it helped to shape the decisions on public support of science as the federal government became the leading patron of scientific research. It was an inherent part of the conceptual outlook of Vannevar Bush, the director of the Office of Scientific Research and Development during the Second World War, when he wrote the report *Science, the Endless Frontier* that launched the brilliantly successful campaign to commit the federal government to the support of basic research in peacetime.[9] Reading the political situation well, Bush wasted no time arguing the importance of scientific knowledge for its own sake. Rather, he adopted Bacon's premise on the grand scale. By heavily investing in basic scientific knowledge, he said, the nation could reach a variety of its future economic, medical, social, and international goals, just as the basic prewar research in nuclear physics had allowed it to build an atomic bomb and reach its desperately important national security goals during the war. But Bush was equally clear on the radical separation of basic and applied research and the impossibility of applied goals motivating basic inquiry in any direct sense. In the tradition of German science and the natural philosophers he wrote that "basic research is performed without thought of practical ends."[10]

The traditional view is intact today. A simple, two-valued logic—if it's basic it's not applied and if it's applied it's not basic—echoes through congressional hearings on research and development budgets. It helps to sort out responsibilities for the support of R & D by the executive agencies of the government. It partially shapes the organization of the research community itself. It is a staple of media comment on scientific research. It is even equipped with a one-dimensional spatial image that places basic research at one end of a spectrum and applied research at the other. The view of the expounders of the traditional basic/applied distinction is indeed wholly confined by Euclidean one-space, although their spatial imagery in fact typically degenerates still further to a model of only two points.

MODIFYING THE DOMINANT VIEW

Our two heuristic cases would be enough to suggest that the traditional view is incomplete. A different reading of the history of science yields a rich store of scientific advances that were driven by the desire both to extend basic knowledge and to reach applied goals. A clear case is the rise of microbiology toward the end of the nineteenth century. Louis Pasteur and his allies sought a fundamental understanding of the nature of disease. But they also wanted to lessen the ravages of disease in beasts and men. The desire to reach this goal *by the means of* a fundamental extension of knowledge is, in Pasteur's work, almost palpable.

The biological sciences today are equally difficult to bring within the traditional, polarized view. The revolution in molecular biology continues to pose questions, such as how interferon works, that are enormously important both for the advance of fundamental knowledge on recombinant DNA and for major applications—some of which may be immensely profitable. But a similar observation can be made about the non-molecular parts of modern biology. Some of the most fundamental problems in the population dynamics of plants and animals have major applications, which help guide the course of the most innovative research in the field.

Certainly the social sciences offer striking cases of advances that were driven by the desire both to extend basic knowledge and to reach applied goals. A conspicuous example is the unfolding of macroeconomic theory in the hands of John Maynard Keynes and his heirs. Keynes wanted to understand the economy at a fundamental level. But he also wanted to abolish the recurring misery of economic depression. Although the understanding remains unfinished—and sustained growth partially realized—we could not miss the fusion of motives in this distinguished line of social science research.

A further clear example from the social sciences is furnished by demography. Although I will return to the detailed interplay of motives in this field, the importance of both the thrust toward understanding and the concern for social implications is unmistakable. Those who laid the foundations of demography believed that population change could be understood only by the most rigorous scientific work. But they also believed such change was of immense significance for mankind. It was in fact their most sophisticated work on population replacement that revealed a generation ago the staggering potential force of the explosion in the world's population.[11]

But the motives of physical scientists too are more varied than Vannevar Bush led us to believe. There are occasions on which pure physicists and chemists pursue research that is, to some degree, both basic and applied. There is indeed something almost perverse in the claim of the physicists involved in the wartime effort to build the atomic bomb that theirs was only a gigantic exercise in applied science and development. It may be true that the Manhattan Project produced no scientific breakthroughs as fundamental as those of Niels Bohr and others in the prewar decades. But the project reached its goal only by compressing an extraordinary amount of normal science into a brief interval and acquiring a great deal of fundamental knowledge in nuclear physics about such things as the probability of neutrons being captured by very heavy nuclei at various neutron energies.

We will therefore be far more faithful to the actual flow of research in many fields if we free ourselves from the traditional framework. Basic and applied research are indeed conceptually or analytically distinct, but they are not disjoint empirical categories. Whether one or the other or both—or, for that matter, neither—of the basic and applied motives drives particular research is simply an open empirical question.

Having freed ourselves from Euclidean one-space we may indeed find it useful to visualize a two-dimensional image of the relationship of basic and applied research. There is, of course, not the slightest reason why we need think in dichotomous terms, since the presence of the basic and applied motives may be matters of degree. But if we do so it is clear that we have not one dichotomy but two, which now require the cells of a four-fold table where the rows are defined by whether or not the research seeks to extend basic knowledge—the top row yes, the bottom row no—and the columns are defined by whether or not the research seeks to reach some applied goal—the left-hand column yes, the right-hand column no.

Under these definitions the upper right-hand cell of the table is for research that is purely basic in Vannevar Bush's sense, a cell that includes Niels Bohr and the natural philosophers. Let us call this quadrant I. The upper left-hand cell of the table is for research inspired both by basic and applied motives and includes such diverse investigators as Louis Pasteur and Arthur Lewis. Let us call this quadrant II. The lower left-hand cell of the table is for research that is purely applied, work that is undertaken to reach some applied goal and not to extend basic knowledge—although it may of course use the knowledge gained by prior basic research. Let us call this quadrant III. Finally, we have in the lower right-hand corner of the table a cell for research that is *neither* basic *nor* applied. Let us call this quadrant IV. The presence of this only apparently uninteresting category makes the point that we really do have two dimensions and that quadrants I to III are not a slightly more complex version of the familiar one-dimensional basic/applied continuum. Anyone with a taste for the annals of research will, on reflection, know that quite a lot of research is neither basic nor applied and deserves to be assimilated to quadrant IV. Let me offer as an example that is not currently sensitive the very extensive descriptive studies of migration that were mounted from the country's agricultural experiment stations during the Great Depression. This research sought neither to answer questions of general scientific interest about migration nor to clarify any policy issues. Its whole motivation was relief—for the unemployed research personnel it put to work in hard times.[12]

I have worked through this conceptual revision here not for the greater clarity it gives the annals of research, although I believe it does have substantial uses of this kind. We can, for example, gain a far richer insight into the dynamics of research traditions when we are no longer constrained to think of movements across only the traditional model's single threshold between basic and applied research and are instead free to trace the extraordinarily interesting movements in each direction between quadrants I and II and between quadrants II and III.

I would also note as an aside that this conceptual revision helps to clarify issues of science policy and the organization of public support of research. The familiar premise—if it's basic it's not applied and if it's applied it's not basic—has lent too narrow a focus both to the free-standing federal R&D agencies—especially the National Science Foundation—that support basic research, and to the R&D programs of the mission agencies of the government that support applied research. Although the Science Foundation is broadening its outlook, it has traditionally been far more comfortable with Vannevar Bush's quadrant I than it has been with basic research in quadrant II, which is also directly motivated by applied goals. Likewise, the mission agencies of the government have missed a number of opportunities for supporting quadrant II research that is basic as well as applied—and thereby advancing their missions—because they felt their mandate did not extend to basic inquiry—that they were, in effect, limited to quadrant III.[13]

In this symposium on the uses of social science I want to finish my analysis with some observations about institutional and intellectual initiatives that could encourage research that seeks a joint product of basic knowledge and applied use. The experience with such initiatives in other fields raises interesting questions for the social sciences.

STRATEGIES FOR QUADRANT II

Despite the strength of the polarized view of basic and applied research there have been important efforts in a variety of fields to institutionalize quadrant II research. In the biological sciences, the legacy of Pasteur and other pioneers who were partly motivated by applied use is such that a good deal of basic-cum-applied biomedical research enjoys a strong institutional base in the medical schools. Although some biologists, infected by the German tradition, have wanted to draw the distinction between pure and applied biological science in terms of the distinction between biology and medicine, the biomedical portfolio has within it substantial quadrant II and quadrant I research, along with a good deal of quadrant III work, as we would expect. In line with this, the National Institutes of Health have consistently been far more relaxed than the NSF about supporting research in which the basic and applied motives are strongly mixed. Thus local and national institutionalized backing has clearly encouraged biological research that brings together the goals of basic inquiry and applied use.

Institutional encouragement of such work in the physical sciences has followed a different course, in which the rise of the engineering fields has played a critical role. So strong is the hold of the traditional basic/applied distinction on physical science that a number of pure scientists are ready to see all of engineering as a purely applied science. But this would be a misreading of history and research substance.[14] In fact, such fields as chemical and electrical engineering grew in power in an applied sense only as they were able to solve increasingly fundamental research problems and add to more general scientific knowledge. It is indeed fascinating to trace the stages by which research in chemical engineering moved from a low-level concern for engineering problems in particular chemical industries to the more generic "unit operations" of distillation, filtration, absorption, and the like and ultimately to fundamental problems of thermodynamics, material and thermal balances, turbulent flow, and the reaction kinetics of continuous flow systems.[15] But the strongly institutionalized presumption of use allowed applied goals to influence the selection of problems and the interpretation of results at every stage.

The social sciences have not mounted initiatives to institutionalize such research on a scale comparable to the biological and physical sciences. We have, of course, helped to spawn a vast new industry of largely applied research, housed partly on university campuses and partly along the Connecticut Avenues of this and other countries, but this work lies much more in quadrant III than quadrant II. Beyond this, many social scientists who work the quadrant I disciplinary agendas have, with Francis Bacon, an abiding faith that pure research will find practical use, although few who hold this faith have stopped to consider the linking role played by the biomedical and engineering fields in biological and physical science.[16]

But the academic landscape is dotted with forms that could provide an institutional base for social research seeking a joint product of basic knowledge and applied use. Some of the free-standing university research institutes in social science—Michigan's Institute for Social Research and what used to be called at Columbia the Bureau of Applied Social Research (the name was revealing)—are such a form. So are the management schools and schools of public policy. From time to time there are calls for further institutional initiatives, such as the Behavioral and Social Science (BASS) Survey's brief for the creation of schools of applied behavioral science.[17]

I do not want to gloss over the importance of purely institutional factors—and could scarcely do so as the manager of a school of basic-cum-applied social science. But my own view is that the most critical initiatives toward quadrant II research are intellectual rather than institutional. Indeed, the most formative past initiatives have been taken by those who found it intellectually stimulating to define research problems from the standpoint of actors who are required to make decisions.[18] This has happened fairly widely in economics, where a great deal of the intellectual structure of microeconomics is laid out from the perspective of the consumer or firm and a great deal of macroeconomics, as my examples from the work of Keynes and Arthur Lewis suggest, from the perspective of a government or other agency that is required to make decisions of macroeconomic policy. My economics colleagues are not fond of hearing their discipline described as an engineering field, but the fraction of quadrant II research with strongly institutionalized channels of use is almost certainly higher in economics than in any of the other social disciplines.

The nascent research agendas of today include a number of problems of extraordinary intellectual interest that have also a high potential for use in a wide range of applications. Let me cite as one example the systematic study of risk. The essentials of this subject will be laid bare only by the most rigorous intellectual analysis. This analysis will then allow us to cope more effectively with risk across an extraordinary range of problems of which it is an inherent part.[19] A strong ability to define research problems of this kind is our most essential requirement if we are more widely to marry understanding to use in the social sciences.

This is quite a different prescription from the oft-canvassed idea of enhancing our usefulness to the policy world by creating a research paradigm specifically for housing, another for criminal justice, another for health services delivery, and so on. That course seems to me a formula for limiting our selves to quadrant III research of very modest power that will fail over time to attract creative research talent. This fate very nearly befell chemical engineering in the 1920s, at its very citadel of M.I.T., before it ruthlessly pulled back and dealt with a more fundamental research agenda, one that was in the longer run far more important to the industrial constituency of the field.[20]

We have parallels from the social sciences. There was a point in the early development of demography when its research agendas were under intense pressure from those who wanted studies that would support immediate action programs. At this stage a small core of research demographers consciously pulled back and began to pursue a more fundamental research agenda—but one that would yield knowledge with a high potential for applied use. This research led both to the extremely sophisticated methods of estimating population change where demographic data are desperately incomplete and to the understanding of the sources of fertility differentials that undergirds much of the present effort to control population growth.[21] In broad terms, the parallel with chemical engineering is striking.

CONCLUSION

For reasons that are easily traced, the belief that basic and applied research are not just analytically distinct but are in a logical or empirical sense discrete categories has dominated the view of the relationship between these types of research. The belief that if research is basic it's not applied and if it's applied it's not basic, which owes so much to the outlook of the natural philosophers of the 18th century and the institutionalization of German science in the 19th century, has continued to shape the organization of the research community and the development of science policy in the 20th century.

But a revisionist reading of the history of science yields a rich store of examples from the physical, biological, and social sciences of research that is driven both by the goal of basic inquiry and by the goal of applied use. Indeed, a view of the relationship of basic and applied research that is more faithful to the actual interplay of these motives would see research as driven at times by the desire both to extend knowledge and to meet applied needs, at times by one or the other of these goals but not both, and at times by neither of these goals.

This revisionist view of the relationship between basic and applied research provides a deeper understanding of the development of science and clarifies some important issues of science policy. But my principal focus here has been the influences that encourage significant research that is both basic and applied. What factors yield research that seeks to meet applied goals *by extending* fundamental knowledge?

One group of factors has to do with the institutional base of research that combines these goals. Such a base is often provided in the biological sciences by the biomedical research programs in the medical schools and in the physical sciences by certain of the research programs in the engineering schools. The degree of institutionalization in the social sciences is far less, although a sufficient base exists in the free-standing research institutes and the professional schools to suggest that institutional factors are not of pre-eminent importance.

The most important factors are in my own view intellectual. What has allowed certain of the social research fields to address major problems as they explored fundamental processes are analytic frameworks of high generality that nevertheless yield formal or empirical knowledge of high relevance to social choice. These frameworks cannot be narrowly focused on particular problems. In this respect the experience of the most problem-relevant of the social sciences, such as demography, parallels the experience of the engineering fields: they have enhanced their power in an applied sense by pursuing a research agenda that is more fundamental in a scientific sense—indicating once more that the goals of basic inquiry and applied use should by no means be seen as antithetical.

FOOTNOTES

1. This paper was originally prepared for the Harold D. Lasswell Symposium at the September 1982 meetings of the American Political Science Association. For a programmatic statement of Lasswell's view of the policy sciences, see his "The Policy Orientation." In *The policy sciences.* Daniel Lerner and Harold D. Lasswell (Editors). Stanford: Stanford University Press, 1951, pp. 1-15.
2. It is a pleasure to record my debt to a series of colleagues—Harold A. Feiveson, Gerald L. Geison, W. Arthur Lewis, Michael S. Mahoney, Robert M. May, Robert F. Rich, John W. Servos, and Frank von Hippel—who served as generous tutors to an unlikely pupil.
3. Sir Arthur Lewis. *The Nobel prizes 1979.* Stockholm: Almquist and Wiksell International, 1980, p. 257.
4. A wonderfully concise summary of the interaction of science and technology over recent centuries appears in Kuhn, Thomas S. *The essential tension: Selected studies in scientific tradition and change.* Chicago: The University of Chicago Press, 1977, pp. 141-147.
5. See Multhauf, R. P. The scientist and the 'improver' of technology, *Technology and Culture, 1,* 1959, pp. 38-47.
6. See Musson, A. E. and Robinson, Eric. *Science and technology in the industrial revolution.* Manchester: University of Manchester Press, 1969.
7. See Kuhn, *op. cit.*, pp. 145-146.
8. See Lewis, W. *Das Unterrichtswesen im deutschen Reich.* Berlin: A. Asher, 1904.
9. Bush, Vannevar. *Science, the endless frontier.* Washington, D.C.: National Science Foundation, reprinted 1960.
10. *Ibid.*, p. 18.
11. See Notestein, Frank W. Demography in the United States: A partial view of the development of the field. *Population and Development Review,* 8, December 1982, pp. 651-687.
12. The National Research Council's recent Social Research and Development Study Project documented a number of cases in which the real motive for launching

research on social problems was the sponsors' desire to block the creation of government programs to deal with the problems. See National Research Council. *The federal investment in knowledge of social problems.* Washington, D.C.: National Academy of Sciences, 1978.

13. I have dealt with these questions in greater detail in a paper on "Perceptions of the Nature of Basic and Applied Science in the United States," in *Science policy perspectives: U.S.A./Japan.* Arthur W. Gerstenfeld (Editor). New York: Academic Press, 1982, pp. 1-18.
14. C. P. Snow included in his famous lecture on the "two cultures" this partly autobiographical account of the attitudes of pure scientists toward engineering: "Pure scientists have by and large been dimwitted about engineers. . . . They wouldn't recognize that many of the solutions [in engineering] were as satisfying and beautiful. Their instinct—perhaps sharpened [in Britain] by the passion to find a new snobbism wherever possible, and to invent one if it doesn't exist—was to take it for granted that applied science was an occupation for second-rate minds. . . . We prided ourselves that the science which we were doing could not, in any conceivable circumstances, have any practical use. The more firmly one could make the claim the more superior one felt!" (Snow, C. P. *The two cultures and the scientific revolution.* Cambridge: The University Press, 1959.)
15. See Davies, John T. Chemical engineering: How did it begin and develop? In *History of chemical engineering.* William F. Furter (Editor). Washington, D.C.: American Chemical Society, 1980, pp. 15-43.
16. My emphasis here is on what could be learned from the biomedical and engineering fields about developing research agendas that have a high probability both of extending knowledge and finding practical use. I do not envisage a medical or engineering model for the diffusion or application of social knowledge.
17. See The Behavioral and Social Sciences Survey Committee. *The behavioral and social sciences: Outlook and needs.* Englewood Cliffs, N.J., 1969, pp. 200-210.
18. I do not mean to imply that research problems will have a high potential utility only if they are defined in terms of an individual or unit that will need to act. Nonetheless, this perspective explains why significant applications have more often come from social research that utilizes rational-choice or cybernetic, information-processing models than they have from research that utilizes, for example, models of social structure.
19. The concept of risk, for several centuries the focus of systematic analysis in probability and statistics, is by now also the subject of a vast and spreading literature in economics, psychology, and the other social sciences. For a recent work combining the perspectives of anthropology and political science, see Douglas, Mary and Wildavsky, Aaron. *Risk and culture.* Berkeley: University of California Press, 1982.
20. See Servos, John W. The industrial relations of science: Chemical engineering at M.I.T., 1900-1939. *ISIS, 71,* 1980, pp. 531-549.
21. Notestein, *loc. cit.*