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Leadership, Hegemony, and the International Economy: Naked Emperor or Tattered Monarch with Potential?

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The so-called theory of hegemonic stability is a research program composed of two distinct theories. Leadership theory builds upon public goods models and seeks to explain the production of the international economic infrastructure. The theory is extended here by identifying its necessary and sufficient conditions and explicating when leadership is likely to be benevolent or coercive. Hegemony theory, subsuming three independent analytic traditions, focuses on the different structurally derived trade policy preferences of states and attempts to explain international economic openness. The core logic of each variant and questions for future research are examined. Neither leadership nor hegemony theory has been tested adequately by existing empirical studies. While theorists have generally failed to present their arguments in an appropriate fashion, empiricists have not been sufficiently sensitive to variations in the theory and have produced studies that suffer from inadequate theoretical and operational specification and theoretical "over-extension." At this stage, formal tests should not seek decisive disconfirmation of the research program but should aim to provide guidance for further theoretical refinement.

For over twenty years, scholars have been actively pursuing the link between international political structures and the international economy. Earlier analysts had posited a relationship between predominance and peace (e.g., Organski, 1958), and E. H. Carr (1964[1939]) had noted the relationship between British economic hegemony and the rise of free trade in the nineteenth century. But, it was only in 1971 that Robert Gilpin—in reaction to the emerging literature on transnational relations—first began to examine systematically the relationship between political power and international economic openness, and in 1973 that Charles Kindleberger drew a connection between British economic decline,

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American ascendancy, and the Great Depression. By the late 1970s, this research program had helped define the nascent field of international political economy. It had also received a catchy appellation: the theory of hegemonic stability.[1] The program's rise to prominence was based, in my view, on both its simple causal structure and its seeming fit with empirical reality, explaining in concise terms the two greatest periods of international economic openness. It was (and remains) analytically and empirically seductive.

Much discussed, debated, and criticized, the research program's popularity now appears to have peaked. At a gathering of West Coast international relationists to examine the state of theory in our field, my attempt to put the theory of hegemonic stability on the agenda produced catcalls—a sure sign that our collective attention span is waning.

A half-life of two decades is not bad in a discipline more prone to fads than the fashion industry—I say "half-life" because our theories are never discarded, just relegated to our graduate syllabi. Nonetheless, consigning the research program to the academic museum to be observed only by incredulous graduate students dressed in more modern garb is, I believe, premature. I too am disappointed in the extent of both theoretical and empirical progress within the research program. Yet, I believe the emperor is clothed—even if his raiment is unstylish, incomplete, and perhaps abused. This article aims to clarify and reinvigorate the research agenda. With major retailoring, some of it already under way, the monarch's ragged garment can be made more elegant and given a better fit.

In this essay, I develop the following arguments. The theory of hegemonic stability is not a single theory, but a research program composed of two, analytically distinct theories. *Leadership theory* builds upon the theory of public goods and focuses on the production of international stability, redefined below as the international economic infrastructure. The theory is extended here by identifying its necessary and sufficient conditions and explicating when leadership is likely to be benevolent or coercive. *Hegemony theory* seeks to explain patterns of international economic openness. There are three distinct variants here, united by a common focus on national trade policy preferences but deriving these preferences from very different analytic roots. The core logic of these variants and questions for future research are discussed.

In the final section, I argue that neither leadership nor hegemony theory has been tested adequately by existing empirical studies. Some responsibility lies with the theorists, who have generally failed to present their arguments in a clear, logical, and falsifiable fashion. Empiricists, however, have not been sufficiently sensitive to variations in the theory and have produced studies that suffer from the twin ills of underspecification and theoretical "over-extension." Given the state of existing theory, I conclude, our ambition at this stage should not be to seek decisive disconfirmation of the research program but to provide guidance for further theoretical refinement.

The Research Program

The so-called theory of hegemonic stability is composed of at least two distinct variants, which I shall refer to as leadership theory and hegemony theory. These two theories are united by a methodological commitment to positivism, a concern with the functioning of the international economy, an independent variable—

[1] As with many others, this appellation was coined by Robert Keohane (1980). As will become clear below, it is actually a misnomer. Given its widespread acceptance, however, the label is unlikely to be superseded by another. Thus, I defer to common practice.

the structure of the international economic system, and the assumption that states, at least in their conduct of foreign affairs, are rational, unitary actors. Their differences, however, are equally fundamental.

Leadership theory is based upon the theory of public goods and seeks to explain the provision of international stability, reconceptualized below as the production of an international economic infrastructure. Hegemony theory focuses on the different structurally derived trade policy preferences of states and attempts to explain international economic openness, defined as the sum of the free trade and protectionist elements in the foreign economic policies of, at least, the largest states within the system.[2] There is considerable variation within each approach, noted where appropriate below.

Practitioners often blur their theories by mixing elements from the two traditions.[3] This tendency is understandable, as the theories are not necessarily incompatible; according to at least some variants, countries can be leaders and hegemons at the same time. Nonetheless, the failure to recognize the distinct nature of these research traditions is a major source of the conceptual muddle we now face. Progress cannot occur unless we understand and explicate the internal logic of each theory.

This focus on leadership and hegemony theory is self-consciously narrow. It excludes related research programs on hegemony and war, such as the long cycle (Modelski, 1987; J. S. Goldstein, 1988) and power transition (Organski, 1958; Organski and Kugler, 1980; Doran, 1991) schools. While similar in structure, and plagued by many of the same problems, comparing the international political economy and international security research programs would unduly burden the present essay.

Even though they are often insightful and commonly build upon the early contributions of leadership and hegemony theory, I also exclude "interpretative" accounts of hegemony and international economic leadership (e.g., Wallerstein, 1974, 1980; Cox, 1987; Gill, 1990; Gill and Law, 1988; Rupert, 1990). Comparing this research program to the self-declared positivist program reviewed here would also broaden the essay beyond reasonable limits. More important, and reflecting deep methodological and epistemological differences, I question the utility of interpretations of the international political economy that lack a clear, deductive foundation. While hegemony may provide a convenient and, by this point, somewhat uncontroversial tool in a narrative of nineteenth and twentieth century international economic history, the ongoing difficulties of specifying concrete causal statements suggest that the interpretative tool is flawed and the account biased. Although the remainder of this essay is critical of leadership and hegemony theory, these criticisms do not, to my mind, constitute or justify an assault on positivist and generally rationalist theories of international political economy. Rather, the criticisms below are intended to identify issues and potential avenues for further theoretical development. Improving the quality of leadership and hegemony theory is an end in itself. It may also help us assess better the utility of the concept of hegemony in other research programs.

Finally, I do not compare the explanatory success of the theory of hegemonic stability to other research programs in international political economy—espe-

[2] Webb and Krasner (1989) draw a similar distinction but imply that the "security version of the hegemonic stability theory," what I call hegemony theory, subsumes the collective goods argument. In my view, the logics are substantially different (but not incompatible) and should be treated separately.

[3] For instance, Robert Gilpin works within a public goods approach, but assumes that states pursue multiple objectives—including political power. As I define the approaches here, Gilpin mixes elements of leadership and hegemony theory. For a recent statement of his views see Gilpin (1987: especially 87–88). Robert Pahre (1989, 1990) also straddles the fence I have constructed between these theories.

cially those, such as endogenous tariff theory, that focus on strictly unit-level attributes (see Nelson, 1988; Magee, Brock, and Young, 1989). The ultimate test of a research program is in the marketplace of ideas—and success, such as it is, is only relative to some other theory (Lakatos, 1978). Yet, the overarching theme of this essay is that the theory of hegemonic stability—despite its prominence over the last two decades—has not put its best possible "model" into the competition. Progress has been made, but significant theoretical problems and issues remain. Rather than compare the theory of hegemonic stability against its prominent rivals, I want to focus the debate here on ways of improving the research program itself.

Leadership Theory

For Kindleberger (1973, 1981, 1986a, 1986b), the exemplar of leadership theory, states are rational egoists who seek to maximize their own welfare, implicitly defined in materialist terms. Because of the free rider problem, he argues, a single leader is necessary for the provision of the public good of international stability.[4]

The Dependent Variable

While leaving the term undefined, Kindleberger (1973:292; 1981:247) identifies five functions which must be performed if the international economy is to be "stable": providing a market for distress goods, producing a steady if not countercyclical flow of capital, maintaining a rediscount mechanism for providing liquidity when the monetary system is frozen in panic, managing the structure of foreign exchange rates, and providing a degree of coordination of domestic monetary policies. For analytic purposes, it is best to eschew the amorphous concept of stability and redefine the dependent variable in terms of these necessary functions—which I have summed elsewhere under the heading of the "international economic infrastructure" (Lake, 1988:33–35). These functions can also be reformulated in simpler and more familiar terms.

A stable international economy requires a medium of exchange and, secondarily, a store of value. Whether this medium is gold, dollars, or seashells is, in many ways, irrelevant as long as there is agreement among the participants in the market. At its root, this function incorporates Kindleberger's concern with exchange rate management and domestic monetary policy coordination—both of which influence fluctuations in and the value of the agreed medium.

A stable international economy must have sufficient liquidity in the long term to allow for economic growth, in the medium term to counter business cycles, and in the short term to manage panics. This function not only subsumes Kindleberger's second and third requirements, but also his first, for, as he notes, "Maintaining a market for distress goods can be regarded as another form of financing" (1973:293).

To this pairing I would add another, equally fundamental function: *a stable*

[4] Although Kindleberger does allude to altruism (1973:302, n.13), subsequent interpreters have largely assumed that states are egoistic. Kindleberger (1986a:841–842) explicitly disavows the term *hegemony* to describe leadership. For Kindleberger, leadership is a function of both material capabilities and willingness. Since the latter is inferred from behavior, this is a tautology, but it nonetheless implies that a single leader may also be a sufficient condition for the provision of international stability. Finally, if we assume, as I believe Kindleberger does in his writings on this topic, that the law of comparative advantage holds and countries will adopt free trade if an adequate infrastructure exists, then openness will follow from the provision of the public good. But this is an implication and not the primary hypothesis of the theory.

international economy must define and protect basic property rights for goods in transit and overseas (portfolio and direct) assets. The more fully specified and secure property rights are, the more easily the wheels of commerce and finance can turn.

Readers will no doubt recognize these functions as necessary attributes of all economies at or above the stage of primitive accumulation. Kindleberger's contribution is merely to extend these functions to the international economy and to remind us that none of the attributes of international stability exist naturally or of their own accord. Rather, they must be created and actively maintained.

Kindleberger correctly posits that this international economic infrastructure, like its domestic counterpart, is a public good, or at least produces sufficient positive externalities so that for purposes of analysis we can treat it as if it were a public good. A medium of exchange, liquidity, and a system of property rights, once provided, can be extended to others at little or no cost. In addition, a country can be excluded from using a medium of exchange, and thus from being affected by the degree of liquidity in the system, only if all participants in the market collude and enforce a perfect foreign exchange embargo against that state; any leakage will allow the excluded country to enjoy (at some appropriate exchange rate) the benefits provided by others. As the history of economic sanctions suggests, such perfect embargoes are extremely costly to impose and practically impossible.[5]

Critics have often assaulted leadership theory for assuming that free trade is a public good (Conybeare, 1984:8–9; Snidal, 1985:590–593). This criticism is generally correct but misdirected. Free trade is rival and excludable and therefore not public, although the enforcement of trade rules—such as the unconditional most-favored-nation principle—is a public good prone to collective action problems.[6] This criticism and subsequent debate merely highlights the problem common to many social science theories of specifying clearly what is to be explained. Based upon the exemplar and properly conceived, the dependent variable of leadership theory is not free trade or international economic openness but stability, in Kindleberger's terms, or the international economic infrastructure in mine.

The Causal Argument

The fallacy in Kindleberger's original statement of the theory is not in treating the international economic infrastructure as a public good but in positing "that for the world economy to be stabilized, there has to be a stabilizer, *one* stabilizer" (1973:305, emphasis added). Defined non-tautologically and independently of the behavior we wish to explain, however, subsequent work suggests that a single leader is neither a necessary nor sufficient condition for the provision of an international public good.

Kindleberger's position is that the difficulties and costs of bargaining between two or more states are likely to thwart international cooperation. "With a duumvirate, a troika, or slightly wider forms of collective responsibility," he writes, "the buck has no place to stop" (1973:299–300).

Yet, as Duncan Snidal (1985) has most clearly pointed out, there is nothing in public goods theory that limits "privileged" groups, in Mancur Olson's ter-

[5] Baldwin (1985) presents the most optimistic view of the efficacy of sanctions. He does not discuss foreign exchange embargoes at length.

[6] Gilpin(1987:34) raises this point; for an extensive treatment see Gowa (1989a:314–316). On the enforcement of rules and norms see Axelrod (1986). The severity of the collective action problem is rooted in the infinite regress of who enforces the rule of enforcement.

minology, to a single state.[7] Using Thomas Schelling's notion of a "k" group, Snidal has demonstrated that it is *possible* for two or more states to reap sufficient net benefits for them to produce international collective goods. In Figure 1a (under the initial conditions of payoff function C), state 1 forms a sufficient k group to provide the international economic infrastructure. Since it contributes, the leader receives a net payoff of s_i (measured along the vertical axis). Other countries, which do not contribute, enjoy a net benefit of s_m. In Figure 1b, two countries are required to form a sufficient k group.

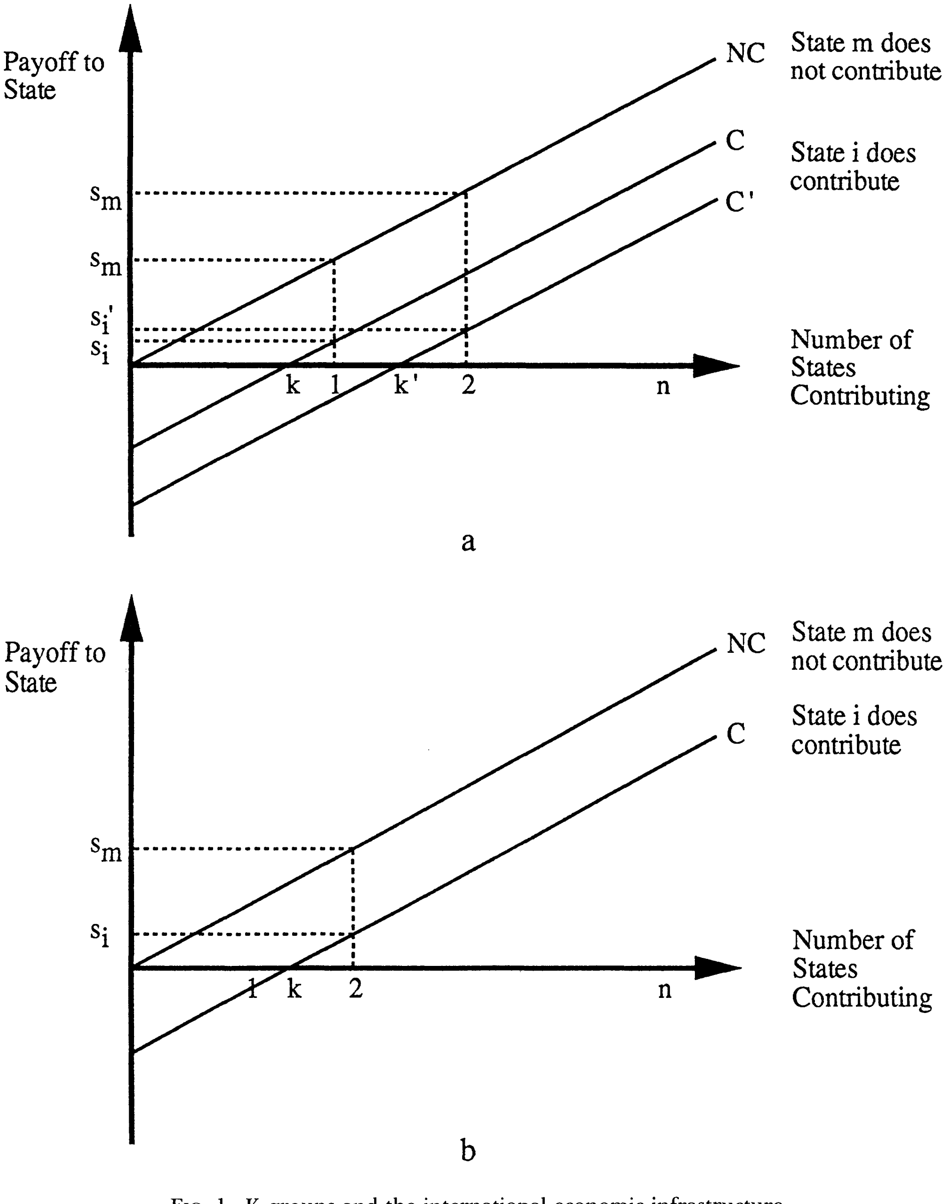
Payoff to State
NC
State m does not contribute
C
State i does contribute
C'
s_m
s_m
s_i'
s_i
k
1
k'
2
n
Number of States Contributing
a
Payoff to State
NC
State m does not contribute
C
State i does contribute
s_m
s_i
1
k
2
n
Number of States Contributing
b

Fig. 1. K groups and the international economic infrastructure. (Adapted from Snidal, 1985, p. 601.)

[7] See also McKeown (1983), Lake (1983, 1984), and Stein (1984).

Recent work on international regimes reinforces this conclusion, at least for "post"–hegemonic systems if not more generally (see Krasner, 1983). While different regimes can have substantial distributional implications, for this theory the substantive content of the international economic infrastructure is somewhat arbitrary in that the exact nature of the medium of exchange or set of property rights is less important than having some agreed-upon medium or rights.[8] As a result, hegemonically created regimes may solve the problem of agreement on a subsequent focal point, thereby easing the bargaining problem which would exist in a "regime-free" environment. Just as the clock in Grand Central Station provides a natural meeting place, so too does the dollar provide a "natural" reserve asset in our post-hegemonic world.[9] Keohane (1984) goes further to suggest that states may be able to construct regimes to facilitate cooperation even in the absence of a single leader.

In short, both public goods and regime theory clearly imply that a single leader is not a necessary condition for the production of an adequate international economic infrastructure. In theory, at least, leadership can be provided by more than one state.

Producing an international economic infrastructure is costly, whether because artificially manipulating an economy to provide liquidity in the short run undermines a country's long run economic health or protecting property rights requires an ability to project force overseas. Thus, in the absence of coercion (see below), the payoffs to countries that contribute are less than those for countries that do not (see Figure 1, where $s_i < s_m$; the cost of providing the public good is equal to the vertical distance between NC and C). Moreover, given business cycles, technological innovations, and the like, the costs of producing the international economic infrastructure are likely to vary over time. As costs increase (NC-C′ > NC-C), the size of the k group necessary to produce the public good also increases. This is illustrated in Figure 1a as a downward shift in the payoff function of contributors. As drawn, the increasing costs of providing the international economic infrastructure now require, holding size constant, that the two largest countries participate. Under such circumstances a single leader is insufficient.

The late nineteenth century may provide an example of this process at work. Given the relative constancy in British policy during this period, it would appear that the first Great Depression (1873–1896) occurred despite England's continued desire and willingness to provide international leadership. What changed, in this instance, was not Britain's role, which may have actually reached its peak around this time with the near universal acceptance of the Gold Standard, but the state of the international economy; even with its financial and monetary resources (and policy of free trade), Britain could not absorb a sufficient quantity of agricultural products (the "distress good" of the period) to avert the deep economic dislocations created in Europe by the opening of the American West.[10] Similarly, the economic trauma of WWI may have done more to thwart the effective provision of an international economic infrastructure in the postwar period than the absence of a single leader. Exchange rate fluctuations, for instance, increased by more than a factor of 100 between 1913 and 1919–1922. Although this reflects the failure of early stabilization attempts, it also highlights

[8] From these distributional implications it might be possible to predict the choice of a particular regime, but this is not the task of this article or the literature it surveys.

[9] On focal points see Schelling (1960).

[10] For a discussion of the first Great Depression and its consequences see Gourevitch (1986), Imlah (1958), and Hobsbawm (1968).

the overwhelming task that confronted Britain and the United States after the war (Lake, 1988:163–182).

In conclusion, a single leader appears neither necessary nor sufficient for the provision of an international economic infrastructure. It is always possible to define any state that effectively produces stability as a "leader" and any state that does not as a "non-leader." Indeed, it is a fairly common tendency in the literature. But in the end, of course, this sleight-of-hand produces not an explanation but a tautology. The task before us is to move beyond behavioral definitions of leadership and to define the necessary and sufficient conditions for the production of the international economic infrastructure.

Necessary and Sufficient Conditions

The literature summarized above can be captured in a simple inequality which helps clarify the necessary and sufficient conditions for the effective production of the international economic infrastructure.[11] Important, of course, is the cost of producing the infrastructure (C_I) and the benefit obtained by any individual country (B_i). When ranked according to absolute size, the benefit of the international economic infrastructure enjoyed by each successive country declines; if smaller countries benefit disproportionately from international exchange, as neoclassical economics maintains, this benefit will also decline at a declining rate.

In addition, the transactions costs incurred by each country in negotiating and enforcing an agreement among the k group (T_i) and the efficiency or efficacy of the agreement so reached (A_k) are also important. Transactions costs represent the resources diverted from other possible uses in order to negotiate and enforce an (explicit or implicit) agreement among the k group. Such costs arise from many diverse activities, including the time and energy of diplomats used in searching for and consulting with other possible providers of the public good, the resources devoted to monitoring participants, and the welfare foregone when it is necessary to sanction violators of the agreement. Transactions costs are likely to rise at an accelerating rate with each new member of the k group. Given that "perfect" contracts covering all eventualities and safeguards against all possible forms of opportunism are virtually impossible to negotiate, the efficiency of agreements is also likely to vary, both with the underlying uncertainty within the system and the number of parties to the accord.[12] In this sense, $1\text{-}A_k$ represents the expected "failure" rate of an international agreement or the probability that countries will defect from or otherwise cheat on the best agreement countries can negotiate. As the incentive to free ride in the provision of public goods increases with the number of providers—and the greater the incentive, the greater the likelihood that countries will find ways around the best or most "airtight" agreements—the efficiency of an agreement will decrease at an accelerating rate with each additional participant.

Combining these factors, the international economic infrastructure will be produced only when some group of countries k satisfies the inequality

[11] Alt, Calvert, and Humes (1988) present a similar and, in some ways, more sophisticated game-theoretic model. Yet their model, focused on the question of the benevolent or coercive nature of hegemony, is developed only for the case of a single leader and not for the general case of an n-country k group. While decision theoretic in structure, the formulation presented here allows for variations in (1) the size of the k group, (2) the costs of providing the international economic infrastructure, (3) the transactions costs of forming an agreement among members of the k group, and (4) the efficacy of the agreement. As demonstrated below, it can also be extended to duplicate some of Alt, Calvert, and Humes's insights about benevolent and coercive hegemony.

[12] On contracting see Williamson (1985) and Eggertsson (1990).

$$C_I \leq A_k \sum_{0-k}^{n} (B_i - T_i).$$

When the individual country benefits, minus the transactions costs of negotiating and enforcing the agreement, weighted by the efficacy of the group's provision, are greater than the cost of producing the international economic infrastructure, the k group is both necessary and sufficient for the provision of effective international leadership.

A single leader constitutes a special case of a k group, as there is no international agreement to be negotiated and enforced (T = 0) and no reduction in the efficacy of the state's actions (A = 1). Whether the infrastructure is provided in this case depends solely upon the leader's benefits relative to the costs of producing the infrastructure. Whether a k group of more than one state exists, on the other hand, depends not only on the absolute costs and benefits of the infrastructure but also on the transactions costs and effectiveness of the agreement undertaken by the group. An effective k group of more than one state is favored the smaller the (a) fall in per state benefits (or the absolute size of the countries), (b) rise in transactions costs, and (c) decline in efficacy.

Benevolent and Coercive Leadership

There has been a lively debate in this literature on whether the leader acts benevolently or coercively.[13] Indeed, Beth Yarbrough and Robert Yarbrough (1992:50) have recently written that "the main source of disagreement among analysts in the hegemonic stability tradition concerns the extent of benevolence or exploitation by the hegemon." When benevolent, the leader provides the international economic infrastructure unilaterally, or at least bears a disproportionate cost of providing the public good, and thereby gains relatively less than others. When coercive, the leader forces other, smaller states to contribute to the international economic infrastructure and, at an extreme, to bear the entire burden. This theoretical divide is often used to distinguish between the literatures reviewed in this and the next section. This is a plausible reading of the arguments, but it does not, I believe, capture fully the difference between existing theories. Despite the prominence of this distinction in the literature, it is actually a second-order question within leadership theory.

Non–k group members have no incentive to contribute voluntarily to the production of the international economic infrastructure. While they benefit from its provision, they expect to free ride on the contributions of others. They will contribute only if coerced by others, with the coercers logically limited to k group members. *Ceteris paribus,* their contributions are a function of the quantity of resources devoted to coercion by the k group and the effectiveness with which such resources are wielded; or,

[13] Snidal (1985) draws the basic distinction. Alt, Calvert, and Humes (1988) develop a game-theoretic approach to this issue that makes several of the points below. For continuity I extend the decision-theoretic model above rather than reiterate their contribution. The central insight captured in Alt, Calvert, and Humes, but not below, is that in equilibrium all states must play mixed strategies. As a result, non-hegemonic states may contribute to the public good out of fear of punishment even when actual sanctions (L_i below) are zero and hegemons may fail to punish even though actual contributions of others (c_m below) are zero. This insight can be partly, but not wholly, incorporated into the model here by reinterpreting the variables below as average contributions and sanctions over multiple periods. Alt, Calvert, and Humes also find that there is nothing qualitatively distinct between benevolent and coercive leadership and that the choice of one or the other depends upon the efficacy of coercion.

$$c_m = a \sum_{0-k}^{n} L_i$$

where c_m is the contribution of a non–k group state, L_i are the resources devoted to coercing state m by each k group member, and a is an efficiency parameter.

This function can be integrated into the inequality above. The international economic infrastructure will be provided when

$$C_I \leq A_k \left(\sum_{0-k}^{n} (B_i - T_i - L_i) + \sum_{l-n}^{n} c_m \right).$$

If $L_i = 0$, and by implication $c_m = 0$, leadership is benevolent. If $L_i > 0$, coercion—however effective—is being employed.

Beyond this purely definitional exercise, two relevant conclusions emerge. First, the basic form of the first inequality above remains intact. There is nothing qualitatively different about the provision of public goods with and without coercion. Most important, k groups greater than one are theoretically possible even when coercion is exercised.

Second, whether or not coercion is used depends on the efficiency with which resources are employed. If $a = 1$, then the k group uses one unit of resources, *ceteris paribus*, to elicit one unit of contribution. Coercion "pays" only when the k group's leverage is particularly effective ($a > 1$). The efficiency of coercion is likely to vary across time, across k groups (depending on the nature of the resources they control), and across target states (as a result of their dependence upon the k group). The answer to the question of whether leadership is benevolent or coercive lies not in the distribution of benefits from the public good but—at least in part—in the efficacy of international leverage.

Research Agenda

As we shall see, leadership theory has enjoyed greater progress than its cousin, hegemony theory. This is due, perhaps, to the intellectual arbitrage that has frequently occurred between international relationists and public goods theorists. Nonetheless, significant theoretical and operational problems remain.

Future research should focus on the (1) components of the international economic infrastructure and their costs of production; (2) cross-national distribution of the benefits of this infrastructure; (3) transactions costs of negotiating and maintaining agreements among groups of various sizes; (4) effectiveness of different agreements; and (5) efficacy of international leverage.

Given the difficulty of defining precisely and measuring these variables, direct tests of the necessary and sufficient conditions identified here are not likely. Through the use of comparative statistics, however, analysts can predict behavior based on changes in the direction (rather than magnitude) of the variables. Hypotheses can then be assessed against the historical record; Barry Eichengreen's (1989) study of international monetary leadership since the late nineteenth century is exemplary in this regard. Moreover, since international structures change slowly and single leaders willing to bear the cost of unilaterally producing the international economic infrastructure are relatively rare, analysts seeking to test leadership theory should look beyond monetary and financial issues to other public goods. There is an immediate overlap, which should be more fully exploited, between leadership theory as described here and emerging work on the international environment.

Hegemony Theory

In hegemony theory, states are presumed to possess different trade policy preferences derived from their varying positions within the international economic structure. By emphasizing differences in preferences, hegemony theory breaks with the law of comparative advantage—which posits that all countries benefit from and share preferences for unilateral free trade (Frey, 1984:15–20). Here, states may prefer free trade or protection and their preferences may be in harmony or conflict. In all cases, the openness or closure of the international economy, the dependent variable, is the result of the strategic interaction and bargaining of self-interested states.

Although free trade or economic openness is not necessarily preferred by all countries, it is strongly desired by the hegemon. Openness arises, in this view, when the hegemon—or, in some versions, a coalition of middle-sized powers—alters the trade policies of other states. Thus, hegemony is necessarily coercive and based on the exercise of power; the hegemon must effectively change the policies of others to satisfy its own goals. Whereas *leaders* may use coercion to overcome the free rider-problem, *hegemons* must use it to reconcile conflicting national trade policy preferences. The leverage exerted by the hegemon may take many different forms, including negative sanctions (threats), positive sanctions (rewards), the restructuring of market incentives, ideological leadership, or simply success worthy of emulation.[14] The issue of how the hegemon alters the policies of others is a second-order question. The instruments of power are less important here than the role of power in creating and maintaining international economic openness.

Thus, in hegemony theory, international economic openness is not stymied only or even principally by collective action problems. As noted above, free trade is inherently excludable and rival and, therefore, not a public good. Although it is not necessarily inconsistent with leadership theory, hegemony theory, in turn, is not built upon a public goods model and free riding is not the principal political problem. Rather, in this theoretical view, the politics of international economic relations arise from the different structurally derived preferences over trade policy possessed by competing states.

Unlike leadership theory where a single, unified theoretical approach has gradually emerged, hegemony theory is characterized by three distinct variants. The first assumes that states possess multiple objectives, at least one of which is political power or security. I refer to this as neorealist hegemony theory. It follows that free trade may not be optimal when states pursue goals different from those assumed in neoclassical economics. The second variant draws upon strategic trade theory to suggest that free trade may not be the optimal policy for all countries under all circumstances. Specifically, this variant focuses on market imperfections—a misnomer widely used in economics to describe departures from the model of pure competition—and relaxes the assumptions of small size and constant returns to scale typically used in the "old" trade theory. The third variant examines the link between international position and domestic politics and builds a "second image reversed" logic for trade policy preferences. Here, the concepts of the state and the "national interest" are unpacked and decomposed into domestic actors and their interests. National trade policy preferences are then derived from the endogenous equilibrium of competing social groups. The remainder of this section surveys two of several possible examples of each variant, examines their strengths and weaknesses, and poses issues for further research.

[14] On the forms of power see James and Lake (1989), Ikenberry (1989), and Ikenberry and Kupchan (1990).

Neorealist Theory

Neorealist hegemony theory assumes that states possess complex utility functions that include such relative goals as power or security. How states maximize across their multiple and possibly conflicting objectives, in turn, depends upon their positions within the international system.

Stephen Krasner's "State Power and the Structure of International Trade" (1976) is the exemplar of this approach.[15] He begins by identifying the effects of international economic openness on four state objectives across two dimensions: the relative size and level of development of the country (see Table 1).[16] Openness enhances the political power and, at least compared to others, the social stability of large, developed countries, while it reduces—again, compared to others—the national income of large states. The other effects are ambiguous and depend upon the structure of the international system. Systems of small, highly developed states, he argues, will tend to be open, for while the effects of openness on political power are detrimental they are symmetrical, safely ignored, and offset by income and growth considerations. Structures of large, unequally developed states will be closed, as the less developed actors will prefer protection. Hegemonic systems are most likely to be open as the single advanced country will prefer free trade and the others will either recognize that competing for political power is useless or succumb to the hegemon's blandishments.

Without addressing the specific theoretical or empirical claims made by Krasner, which have already been widely dissected in the literature, there are, in my opinion, two general impediments to theoretical progress inherent in his approach. One difficulty inheres in aggregating multiple objectives when international economic openness exerts conflicting pressures. Depending upon the weight attached to these four objectives, the behavioral predictions of the theory can change dramatically (see Table 1): if large, developed states weight each objective equally they will incline toward free trade, but if they have a strong preference for, say, national income they will prefer more protectionist policies. If the weights attached to these goals vary over time and across countries, and we have no reason to believe they do not, general predictions will be elusive.

To raise this difficulty, however, is not to argue that the *presence* of conflicting objectives is a problem in neorealist hegemony theory or, more generally, a rationalist approach to politics. Indeed, the microeconomic apparatus that un-

TABLE 1. State power and the structure of international trade.

	Predicted effects of openness according to (direction of relationship)	
Goals	*Larger relative size of country*	*Higher level of development of country*
Political power	+	+
National income	−	system
Economic growth	system	system
Social stability	+	+

A schematic representation of Krasner, 1976. Table should be read: openness enhances (detracts from) the political power (national income) of relatively large countries. "System" indicates that the relationship depends upon system structure.

[15] See also Krasner (1978, 1979, 1981, 1982).

[16] I am indebted to Cheryl Schonhardt-Bailey for the following discussion.

derlies the theory was developed specifically to explain constrained choice across multiple objectives. Rather, the point is simply that we must take the *process* of aggregating multiple preferences seriously and be sensitive to the ways this process is likely to affect our predictions.[17]

A second difficulty lies in the relative and absolute nature of these national objectives. Political power, defined by the opportunity costs of closure, is inherently relative or zero-sum; it can be determined only by comparing the attributes of two or more countries. As noted, however, Krasner also assesses the effects of openness on national income and social stability in relative terms. Free trade benefits all states, according to the law of comparative advantage, but improves the welfare of small states proportionately more; therefore, large countries will prefer closure. Likewise, openness reduces the social stability of all states, but has greater effects on small, less developed countries; as a result, large, developed states will benefit from openness. But to the extent that states value national income or social stability as goals in and of themselves, then the variation across countries is eliminated and, indeed, the "signs" are reversed for large, developed states. In practice, states are likely to pursue both absolute and relative gains.[18] How they weight these conflicting objectives is again a question of considerable importance. If a state assesses its payoffs only in relative terms, Krasner's specification of preferences follows. If states weight absolute and relative gains equally, however, the conflicting pressures will largely cancel each other out and the model will fail to yield any determinant predictions.

The difficulties of aggregating multiple preferences and absolute and relative gains have proven to be significant barriers to theoretical progress. In the absence of a fully specified model, Krasner's behavioral predictions appear and are ad hoc. With a model attentive to the question of weights, on the other hand, no general predictions are possible without addressing the "causes" of these weights.

Recently, several second-generation neorealist models have been developed. All restrict the national utility function to wealth and security, thereby rendering the model more tractable, and posit that it is the tension between these two, sometimes conflicting objectives that produces variations in national trade policy preferences. Joanne Gowa's recent work is indicative of this genre (1989b, forthcoming; Gowa and Mansfield, 1993).[19]

According to Gowa, trade creates security externalities. These spillovers "arise because the source of gains from trade is the increased efficiency with which domestic resources can be employed. This increased efficiency itself frees economic resources for military uses" (Gowa and Mansfield, 1993:408). To the extent that they are concerned more with relative than absolute gains, countries prefer to trade with friends rather than foes, as the efficiency gains enjoyed by a country's allies increase the overall strength of the coalition. In other words, two countries are more likely to adopt free trade when the joint gains are internalized through some long-term security relationship.

While this is true for all states, Gowa (1989b:1251–1253) asserts, it is even more true for hegemons, which possess a unique status within the international economy. Where most states forgo absolute welfare by blocking trade with their

[17] A more serious problem is the presence of socially intransitive preferences, made famous in the Condorcet voting paradox (see Schwartz, 1987, for a review of this literature). I do not address this problem here, except to note that domestic political systems are likely to develop aggregation procedures ("institutions") that guard against this possibility lest they decline into "chaos" in any number of policy areas (see Shepsle, 1979, for an original statement of the structure-induced equilibrium approach and Krehbiel, 1987, for a less technical review).

[18] On the relative vs. absolute gains debate, see the essays collected in Baldwin (1993).

[19] See also Pahre (1989, 1990).

enemies, she argues, hegemons reduce their welfare by trading freely with their friends. Hegemony is costly. Large states possess a positive optimal tariff and they are likely to behave "altruistically" and forgo such benefits only when the foreign gains from trade are limited to their allies.

Since security externalities are largely unobservable, Gowa turns to alliance patterns to derive testable propositions. As alliances are more likely to be stable in bipolar international structures, she hypothesizes, all states should be more likely to adopt liberal trade policies toward members of their own bloc when such a system exists. Conversely, multipolar international systems should be characterized by higher, more universalistic patterns of protection.

The point about security externalities is, I think, a largely correct and important insight, but there are two problems with the current formulation. First, the argument is ultimately based on optimal tariff theory, which Gowa (1989a) herself has criticized in a related context (see below) as inadequate. For reasons explained shortly, I am also skeptical of optimal tariffs as a foundation for hegemony theory. Second, Snidal (1991a, 1991b) has recently demonstrated that even with constant weights attached to the value states place on relative and absolute gains, the larger the number of countries within the system, the less impact relative gains concerns exert on cooperation. In contrast to Gowa's model, this implies that—at least for the great powers—security externalities will constrain free trade less in multipolar than in bipolar systems and that, *ceteris paribus,* the former should be more open.

The unanswered questions facing neorealist hegemony theory are quite basic and fundamental, reflecting what must still be considered at this time—seventeen years after Krasner's original essay—a promising but unfulfilled approach. First, what are the minimum number of goals necessary to generate theoretically interesting and empirically useful patterns of trade policy preferences? Krasner posed four; Gowa and others have restricted their models to power and plenty. As a general rule, more complex utility functions will be more accurate empirically but more intractable theoretically. The art of theory-building will eventually guide the choice.

Second, when and where do the multiple goals possessed by states conflict? Gowa argues that power considerations are not salient between allies in bipolar systems. Joseph Grieco (1990) claims that such concerns remain central inhibitions to cooperation. This is not simply an empirical problem. We must know what to look for as well as where and when to look for it. Empirical observations in the absence of clearly articulated and specified theories will not solve this riddle.

Third, how does a state's position within the international system influence its choice and the nature of the trade-off between its goals? For hegemony theory, this is the most direct question, but it cannot be answered without greater progress on the first and second. Assuming this occurs, theorists must then ask, Are hegemons qualitatively different from other states? Are they less constrained than others from following their narrow self-interests, as suggested by Krasner, or more constrained, as implied by Gowa? Are there variations in the constraints faced by non-hegemonic states? Do these variations matter?

Finally, and relatedly, what defines position within the international system? What are the salient international characteristics of states that exert the predicted effects on trade-offs and choice? What is the appropriate dimension of variation? What is the appropriate metric? Neorealists must answer these questions before a full theory can exist.

Strategic Trade Theory

The second variant of hegemony theory seeks to build upon the emerging models of strategic trade policy in international economics (see Grossman and Richardson, 1985; Krugman, 1986, 1987; Helpman and Krugman, 1989; Richardson, 1990). Central to this approach is the hypothesis that national trade policy preferences do not vary because countries pursue power as a goal but because free trade does not always maximize the absolute gains from trade. In this way, it shares the utility maximization assumptions of traditional economic theory. But by challenging the law of comparative advantage this strategic variant renders the pursuit of free trade problematic.[20]

One approach begins with optimal tariff theory but turns the traditional argument on its head. Both neoclassical and strategic trade theory recognize that tariffs may be welfare-improving when countries possess sufficient market power. In such cases, countries can impose tariffs, reduce imports, turn the terms of trade to their advantage (lower import prices more than the tariff), and improve national welfare. Given appropriate elasticities of demand and supply, it is possible that optimal tariffs may be greater than zero even after foreign producers retaliate against the protectionist country (Johnson, 1954; Riezman, 1982). It is important to note, however, that for nearly all countries nearly all the time optimal tariffs are zero; most countries simply lack the requisite market power at existing elasticities. In addition, optimal tariffs must be assessed on a product-by-product basis, a point often overlooked by political scientists. Even if a country enjoys an optimal tariff greater than zero on some commodities it is unlikely to have sufficient market power on all or most commodities.[21] As a result, optimal tariff theory provides a poor foundation for systemic theories of trade policy. Nonetheless, critics of hegemony theory have argued that if positive optimal tariffs exist, they are most likely to exist for hegemonic states (see particularly Conybeare, 1984).

This proposition is at variance with both the core of the theory and the historical practice of the United States and Britain—although American trade policy in the early 1930s is a debatable exception (see Conybeare, 1985). In another recent article, Gowa (1989a) reconciles this conflict and suggests that forward-looking hegemons should and do waive whatever optimal tariffs they might have. Drawing upon limit-pricing models in economics, she identifies what might be called the "hegemon's paradox." Just as a local monopolist might lower its prices and forgo some rents to deter competitors from entering its market, hegemons have an interest in forgoing optimal tariffs in order to deter foreign competitors from "upscaling" their economies to compete in the hegemon's area of comparative advantage. The logic is that the hegemon's optimal tariffs reduce the relative prices of its imports, thereby creating incentives for the exporters of those commodities to shift production into areas that have enjoyed a relative price increase—presumably commodities exported by the hegemon. Conversely, by forgoing optimal tariffs the hegemon raises (or maintains) the relative prices of its imports and induces foreign countries to specialize further in these commodities.[22]

[20] In addition to the two summarized below, Yarbrough and Yarbrough (1992) present a third approach that focuses on the degree of relationally specific assets and the possibility of third party enforcement of agreements to explain the form of international economic openness (unilateral, bilateral, minilateral, multilateral). The principal problems here, in my view, lie in discerning the degree of relational specificity over time and in integrating private alternatives to international regimes (e.g., foreign direct investment).

[21] These qualifiers, if carefully applied, would allow for a rigorous test of this theory.

[22] For an illustration of how this process works see James and Lake (1989).

Whether hegemons pursue a short-term strategy of optimal tariffs or a long-term strategy of free trade obviously depends upon their discount rates. The more highly they value future trade advantages, the more likely they are to forgo optimal tariffs. This implies, however, that declining hegemons should engage in a strategy of preemptive protection, switching from a long-term to a short-term strategy sooner rather than later. In other words, they should impose optimal tariffs before not after they join the ranks of mere great powers. Yet, historical experience suggests that hegemons typically remain wedded to free trade long after it serves their interests. This is the problem of hegemonic "afterglow" first identified by Krasner (1976).

I develop the second strategic approach in *Power, Protection, and Free Trade* (Lake, 1988). Neoclassical trade theories assume that all production is characterized by constant returns to scale which are fully internalized by the firm. New or strategic trade theory allows that at least some industries might possess internal (limited to the firm) or external (extending to the sector or country) economies of scale. With perfectly functioning capital markets (a strong assumption), internal economies will always be exploited and captured by some entrepreneur. In the absence of government intervention, on the other hand, external economies or positive externalities will always be "underproduced" in national welfare terms. Thus, governments can improve welfare by subsidizing or, in import-competing industries, protecting firms and industries that produce such externalities. If the economies of scale are large enough, government support might well be justified for an extended period of time—a form of semi-permanent, infant-industry protection. The possibility of welfare-improving government intervention, however, creates a problem of international strategy. An expansion of one national import-competing industry necessarily contracts its foreign exporting equivalent and reduces the externalities enjoyed in the foreign country. As a result, countries will clash over who captures such industries and the external economies they generate.

In *Power, Protection, and Free Trade* I postulate that (a) countries use trade protection to "compete" for industries with significant external economies; (b) capital-intensive industries and modes of production (proxied by relative labor productivity) produce greater externalities and, as a corollary, capital-abundant countries (i.e., those with high relative labor productivity) can most effectively compete for these industries; and (c) such competition is more acute in middle- and small-sized countries, as large states are cross-pressured by the hegemon's paradox identified by Gowa (1989a), but moderated when the number of key players is relatively small and discount rates are low. Thus, I predict that countries will develop different trade policy preferences on the basis of their structurally defined ability to compete for industries with significant economies of scale.

This argument rests upon an intuition that—admittedly—must be taken on faith, however difficult that might be for social scientists.[23] Specifically, I assume that externalities increase with the capital-intensity of production and array countries by their national average as proxied by relative labor productivity. The intuition here is that capital-intensive production creates greater spin-offs than labor-intensive production. More productive machines and technologies stimulate technological innovations and, perhaps more important, basic science. Greater human capital has much the same effect. These innovations and sci-

[23] By focusing on the "international" sources of trade policy, I also implicitly assume that economies of scale are prevalent and widespread. While I do not claim that the theory can account for all protection, and agree that domestic rent-seeking does play an important role, the explanatory power of the argument is positively correlated with the breadth and magnitude of the external economies.

entific breakthroughs, in turn, facilitate the emergence of new products and production methods in other areas of the economy, thus creating a virtuous cycle of rising economic progress. Nonetheless, I cannot yet measure or demonstrate these basic linkages.

This intuition is supported indirectly by the historic desire of states and statespeople to move "up" in the international division of labor by shifting into more capital-intensive industries and modes of production.[24] This is, I claim, an almost universal goal of economic statecraft. In the absence of external economies, the rationale for such a policy of economic restructuring disappears, for in a world of constant returns to scale a country's area of comparative advantage is unrelated to its gains from trade. We can test the theory built upon this assumption by comparing its predictions to the real world of trade policy. If it is true that countries with higher relative labor productivity do tend to compete for capital-intensive industries through trade protection and other subsidies, we can have greater confidence in the approach. But, given the lack of appropriate measures of and data on external economies, direct evidence for or against this intuition will not be forthcoming soon.

Like the neorealist variant, the strategic trade approach faces a series of fundamental questions which must define its future research agenda. First, how important are the market imperfections that lead countries to depart from free trade? If optimal tariffs and external economies are small, their effect on trade policy is also likely to be small—unless states are locked into "dilemmas of common interest" in which small imperfections can nonetheless prompt countries into adopting collectively sub-optimal policies (Stein, 1983). If they are large, they may explain the widespread patterns of protection observed historically.

Second, how and in what ways are these market imperfections related to the positions of states within the international structure? What is the appropriate dimension and metric of variation? Do large, middle-sized, or small states face greater market imperfections? Do some types of states confront consistently greater or smaller optimal tariffs and external economies?

Given the complexity of strategic trade theory and its apparent sensitivity to initial assumptions, many of the answers to these questions are likely to come from economists. Political scientists interested in questions of international structure and economic openness must wait for further progress in the underlying theory. Nonetheless, no more potentially fruitful ground for interdisciplinary collaboration exists. Harnessing economic models of strategic trade to the political, policy, and historical concerns of international relationists promises tremendous insights.

"Second Image" Theory

The third and final variant of hegemony theory locates the different trade policy preferences of countries in their varying domestic coalitions and institutions, referred to here as structures. Closely related to traditional societal explanations of trade policy, this approach parallels and is occasionally informed by the burgeoning school of endogenous tariff theory.[25] It differs from this school,

[24] For a related review of the development literature see Krugman (1993).

[25] For a book that cites all of the relevant literature and develops a state-of-the-art endogenous policy model see Magee, Brock, and Young (1989). While most of the work in this area has focused on the contemporary United States, several recent studies have focused on nineteenth-century Britain; e.g., McKeown (1989), Schonhardt (1991), and Schonhardt-Bailey (1991a, 1991b).

however, by explicitly linking international dominance, domestic structures, and international economic openness. In this "second image reversed" view, hegemons prefer free trade not because of security or strategic trade considerations but because they are characterized by certain domestic coalitions and institutions. At least two studies make the link between specific domestic structures and hegemony directly.[26]

First, drawing upon the earlier insights of Gilpin (especially 1975) and international economists, Richard Rosecrance and Jennifer Taw (1990) argue that net creditor countries must eventually run trade deficits if foreign debtors are to repay their loans. This creates domestic political pressure from the country's own financial community and other large holders of overseas assets to adopt policies of greater free trade (see also Frieden, 1988b). Such pressure not only characterized Britain and the United States during their respective periods of hegemony, these authors argue, but will increasingly come to the fore in Japan as its creditor position is solidified.

Yet, how and why the international financial community should dominate national politics in hegemonic states is a question these authors do not sufficiently address. Even if hegemons are relatively capital-abundant, this does not mean that capitalists will necessarily carry the day.[27]

Second, Mark Brawley (1989, 1993) offers a related but alternative factor based explanation.[28] Brawley argues that factors of production have two choices: they can seek profits through production for the international market or rents through domestic and, necessarily, international market restrictions. Following the Stolper-Samuelson theorem, he suggests that abundant factors of production will generally benefit from the first option and scarce factors of production from the second. In addition, abundant capital has a stronger interest in free trade than abundant labor because organizing the international economy—or providing the international economic infrastructure in the language above—is a capital-intensive task which will raise the relative return to capital even further. Given the emphasis on financial transactions in Kindleberger's original list of responsibilities, this is not implausible. A focus only on factor endowments, however, is insufficient as even abundant factors of production can gain from monopoly and trade restrictions if the government is sufficiently responsive. Brawley goes on to argue that because lobbying representative governments is more costly, the level of domestic rent-seeking will be relatively lower in democracies than autocracies. Thus, he concludes, the states with the strongest domestically generated interest in free trade will be capital-abundant democracies. If they are also internationally powerful, such states will be interested in and able to open the international economy. In a word, they will be hegemonic.

This argument is provocative. Yet, it neglects the important role that the domestic elasticities of demand and supply play in determining the relative attractiveness of rent-seeking. If demand is elastic, an increase in price created

[26] Several other studies, which could also be grouped here, posit a less axiomatic relationship between hegemony and particular domestic structures; see Maier (1978) and Ruggie (1983). Peet (1992) develops a more statist version of this basic variant.

[27] On the struggle between national and international capital, see Frieden (1988a). As Rogowski (1989) correctly notes, a focus on factor endowments will only reveal the pattern of political cleavages within a society, not who triumphs politically.

[28] Brawley bills his approach as essentially systemic. Two of the three variables he examines are defined at the international level: factor endowments and international power. Regime type or democracy/autocracy is a unit-level variable. While, of course, factor endowments are inherently relative, I take a certain poetic license here by shifting the focus from endowments to groups defining their interests by their endowments. With this—I think, appropriate—shift, Brawley's approach more clearly displays its natural affinity with endogenous tariff theory.

by monopoly restrictions will lead to an offsetting reduction in the quantity of the good purchased and the level of potential rents. If supply is elastic, a price increase will elicit greater production—again, reducing the available rents. Elasticities will, of course, vary on a product-by-product basis and render any general trade policy predictions problematic (see Ray, 1981).

I also question whether the overall level of rent-seeking is actually lower in democracies than autocracies. The higher cost of lobbying, generated by the sheer number of representatives who must be influenced and the lack of binding contracts on future legislatures, must be offset by the greater access enjoyed in a democracy. Stephen Magee, William Brock, and Leslie Young (1989) demonstrate that democracies can, in fact, fall into "black holes" of competitive rent-seeking. I suspect that democracies and autocracies redistribute similar levels of rents over greater and smaller numbers of groups, respectively.

Most important, however, Brawley's is an important attempt to provide a solid, microeconomic foundation for hegemony theory. There is nothing inconsistent in arguing that hegemony begins at home; Gilpin has reminded us of this from the earliest formulations of his argument (see especially 1977). Indeed, one of the most pressing problems in international relations theory, in my view, is the need to ground adequately the systemic-level motivations attributed to states in the desires of individuals acting as a collectivity. What we need, in other words, is a macro-level theory of international behavior with solid microfoundations. For this reason, I believe that the "second image" variant of hegemony theory, despite its near-embryonic state, is one of the most promising arenas in the overall research program.

Future research in this approach must focus on clarifying the domestic structures that support free trade or protection and, in turn, how these structures are influenced by the position of the country within the international economic structure. Present analysts have failed to exploit fully the existing theoretical and empirical work on endogenous trade policy. Both Rosecrance and Taw and Brawley, for instance, rely on a model of international trade in which factors of production are assumed to be perfectly mobile across sectors and uses within a single country and, as a result, tend to identify and ally with themselves—creating a class-based politics. If assets are specific to particular uses, on the other hand, factors of production will tend to coalesce around industries. Rather than observing a capital–labor divide, for instance, one might find a steel v. electronics industry split. Magee (1980) demonstrates that a specific factors approach is more accurate, at least for the United States around the time of its hegemonic zenith. In reality, factors are neither perfectly mobile nor perfectly immobile, but more or less mobile depending on their attributes and circumstances (Frieden, 1991). A fully developed second image model will have to reflect and build upon this variation.

The prime questions confronting this approach then become, Why do hegemons have domestic structures oriented toward free trade? Are their factors of production more mobile and their abundant factors politically dominant? Or are they simply blessed with relatively few import-competing industries? How do institutional factors affect the degree of factor mobility and the comparative political strength of protectionist and free trade forces? How does the international structure exert its purported effects? Does international position affect political institutions or the pattern of domestic interests? What is the dimension underlying the concept of international structure? What is the appropriate measure of variation for this dimension?

Necessary and Sufficient Conditions

As leadership theory matured, its focus shifted from the existence of a single leader to the existence of a k group of one or more members. The presence or absence of a single dominant state has retained a more central place in hegemony theory—and thus I use this term to describe the approach.

Nonetheless, several theorists have suggested that openness can arise or be maintained even in the absence of a hegemon. As noted, Krasner argues that openness can be created under either hegemony or a system of many small, highly developed states. While positing that hegemons have unique, structurally derived preferences, I also argue that systems of two or more middle-sized and highly productive states may produce relative openness (Lake, 1988). In the absence of the theoretical progress and refinement called for above, these arguments must be regarded as tentative and subject to revision. Yet, it would appear that hegemony may not be necessary for international economic openness. As hegemony theory is developed further, it is likely to experience a considerable broadening of its theoretical focus to include non-hegemonic systems.

The question of how the hegemon alters the behavior of others raises the issue of whether hegemony is sufficient to create or maintain openness. This has obviously been a "hot" topic of debate. The finding that Britain did not actively threaten or reward other states has been taken as an important empirical critique of the entire research program (McKeown, 1983). Several recent works suggest that, due to specific characteristics of the nineteenth century international economy, Britain may have been able to accomplish its objectives by unilateral actions, and that the absence of an activist foreign economic policy is not surprising (Yarbrough and Yarbrough, 1992; James and Lake, 1989; Lake, 1991). Until we understand what states are seeking to accomplish and how they vary in their relevant capabilities, this particular debate will remain unresolved.

The important point, however, is that if hegemony theory is on the right track and states do possess different preferences, the resources required to alter behavior will depend upon (a) the efficiency with which resources are used, (b) the structure of foreign preferences, and (c) the range and number of states holding different preferences. In this case, openness will arise only when the increase in utility obtained by the hegemon is greater than the resources expended to influence the behavior of others. Again, unless we define hegemony tautologically as only those states that can successfully alter the behavior of others, states with identical structural positions may vary in their ability to create openness depending upon the composition of the rest of the system and the degree and direction of dependence between states. We must allow for the possibility that hegemony, defined in terms of position within the structure, is insufficient to produce openness.

As in leadership theory, hegemony may be neither necessary nor sufficient. Openness may exist without hegemony and hegemony may exist without openness. Again, I must stress that significant theoretical work needs to be done before accepting or rejecting any causal statements. Nonetheless, my hunch is that all variants of hegemony theory are likely to evolve in this direction. Future theoretical work will and should focus increasingly on the preferences of and strategic interaction among states in non-hegemonic international systems.

Leadership theory can claim to be something of a success. The core theoretical structure has been gradually refined. Work in this tradition has even been somewhat cumulative, as subsequent scholars have added bits and pieces to the theoretical edifice so that it now stands as a relatively coherent whole.

Hegemony theory has been less successful. Any approach that challenges the intellectual "hegemony" of the law of comparative advantage faces a considerable hurdle; claims that free trade is not in the interests of all countries will necessarily be greeted with suspicion. There also appears to be a higher priority on theoretical innovation in this variant. Although all are in their early stages of development and the jury is still out, work is progressing in all three directions surveyed above. Yet, far more needs to be done along the lines suggested and, in general, toward developing logical, deductive arguments that go beyond simple intuition.

Testing the Research Program

The current wisdom, as I read it, is that tests of the theory of hegemonic stability are (perhaps fatally) impeded by the small number of possible cases and that close examination of the British and American examples fails to corroborate the central propositions of the research program. If true, these conclusions would significantly call into question the explanatory power of leadership and hegemony theory and the utility of additional work in this area.

The first conclusion does present an important constraint. An "n" of two or possibly three cases, if we include the Netherlands in the seventeenth century, is clearly limiting. If hundreds of cases existed, the theories could be treated probabilistically, and tests, whether quantitative or not, could be designed to control for other independent or intervening variables. In the absence of a large number of cases, however, even greater theoretical clarity than normal is required. This highlights the need for theorists to specify carefully necessary and sufficient conditions for the core propositions. While decisive tests will have to await greater theoretical rigor, ongoing assessments can nonetheless be undertaken and potentially enhanced by two research strategies: process-tracing at the international and domestic levels and, for hegemony theory, disaggregating international economic openness into the national foreign economic policies of individual states.

The second conclusion, in turn, is premature. Some well-designed research has raised grounds for concern; however, the evidence is, at worst, mixed. More important, few appropriate "tests" have actually been conducted. The remainder of this section develops these two arguments in reverse order.

Existing Empirical Studies

Existing empirical studies on leadership and hegemony typically take one of four forms. Some have focused on definitions and sources, essentially asking the question of whether or not the United States remains a leader or hegemon (Russett, 1985; Strange, 1987). Others use history to clarify the logic of the research program and to raise larger analytical issues (e.g., Stein, 1984).

Still other scholars, typically stating the research program in its most simplistic form, treat international structure as a "backdrop," or context for other theories (e.g., Odell, 1982; Lipson, 1983; Milner, 1988). The majority of these studies do not find evidence that the program is wrong, only that it is too broad and general to answer the question(s) asked by the author. Rather than attempt to refine the core logic of one or more theoretical variants, as the authors cited in the last sections do, these analysts typically posit new variables and causal relationships essential to understanding the outcome they observe. While they occasionally point to apparent anomalies, these works do not constitute formal tests of the research program.[29]

[29] Given the lack of clear, falsifiable propositions generated by the theory of hegemonic stability, it is difficult

Explicit tests of the research program, the fourth genre, are usually of two types: longitudinal studies of structural and economic trends and detailed studies of specific cases. Although usually critical of the research program, these studies have actually produced ambiguous results. Focusing only on longitudinal studies for the moment, Timothy McKeown (1991) has found little evidence that hegemony produces greater openness, although the concentration of international trade in the five largest trading nations is positively associated with greater free trade. Even though based on trade flows, rather than trade policies where the theory might be expected to apply more directly, this finding is the strongest evidence against hegemony theory to date.[30] Conversely, John Conybeare's (1983) test of an international structural model finds that it performs well in explaining 1902 tariff levels but poorly in accounting for 1971 tariffs; William Thompson and Lawrence Vescera (1992), working within a distinct but related theoretical approach, demonstrate a strong positive link between technological leadership and openness; and Edward Mansfield (1992) concludes that hegemony is positively related to openness under Gilpin's but not under Wallerstein's periodization—suggesting that empirical assessments are very sensitive to definitions. General empirical conclusions remain elusive. Neither opponents nor proponents of the research program should claim success.

Extant tests also suffer from two important failings. First, as Mansfield's results indicate, in the absence of prior theoretical and operational specification, the results are difficult to interpret, at best, or unintelligible, at worst. This problem of underspecification is more self-evident in longitudinal studies, but applies equally to both types. Second, in the quest for richness, case studies typically narrow their empirical focus to specific regions or sectors. This research strategy, however, tends either to produce mis-specified dependent variables or extend the research program in new directions of unknown relevance to the variants discussed above. I refer to this as the problem of theoretical over-extension. As in previous sections, I review examples of each approach to reveal common tendencies.

In "Hegemonic Stability Theory: An Empirical Assessment," Michael Webb and Stephen Krasner (1989) examine trends in the position of the United States within the international economic structure and international economic openness since World War II. Given this construction, their results are applicable only to hegemony theory and do not bear directly on leadership theory. As is correct when the appropriate theoretical dimension and metric is unclear, they use a variety of indicators for American position and openness. Nearly all reveal the same pattern, increasing our confidence that their results are capturing the

to determine just which empirical "facts" are indeed anomalous; those that seem to have been consensually accepted by the field are drawn disproportionately from periods of declining hegemony and derive from the problem of afterglow. See J. Goldstein (1988) for an example.

[30] Also important are the "revisionist" tariff historians, who claim either that Britain did not follow a policy of free trade in the nineteenth century or that it was not exceptional in this regard (see McCloskey, 1980; Irwin, 1988, 1989; Nye, 1990). The first claim, if true, would count as strong evidence against the theory—and the intuition that supports the research program. The second may or may not be consistent with the theory depending upon which variant is at issue, the process of hegemonic free trade expansion posited, and how the evidence is interpreted. On the first claim, Nye contends that (a) average trade-weighted tariff revenues were actually high in Britain (specifically, higher than in France until the 1880s), but declined over the nineteenth century, and (b) the distinction between revenue and protective tariffs is meaningless in practice due to substitution effects and the domestic production inevitably stimulated by a tariff. Although Nye does not appear to integrate the offsetting role of domestic excise taxes into his analysis, there was, undoubtedly, some protective effect from British tariffs; it is virtually impossible to design a revenue system with no distributional implications. It nonetheless remains true that Britain adopted tariffs almost exclusively for revenue purposes, levying duties only on products with inelastic demand and lacking substantial domestic production (e.g., coffee, chocolate, tobacco) or on "sin" goods (e.g., playing cards). France, on the other hand, protected broad-based industry. As always, these are questions of degree, but the difference remains important.

underlying concepts. There was a significant "decline in US power resources from the early 1950s to the early 1970s, but since the early 1970s, the overall US position has changed little, and it still remains by far the world's largest economy" (p. 189). While trends in trade policy are more ambiguous, economic flows suggest continued liberalization.

> International trade has continued to grow faster than output throughout the period since 1945. Furthermore, there has been no significant decline in the ratio between the growth of merchandise exports and the growth of merchandise production since the 1960s. International trade has continued to grow as a proportion of output for the major developed countries. Trade flows (including trade in manufactures) are now at the highest level in proportion to national output ever recorded for most developed market economy countries. Regional trade patterns eroded during the 1950s and 1960s and have not revived. (pp. 193–194; references to tables omitted)

In short, Webb and Krasner conclude that "the international economic system has not fallen apart over the last two decades . . ." (p. 195).

Interpreting this evidence is a difficult task, a problem that this article, like the best of the genre, is sensitive to but does not successfully overcome. Specifically, the evidence is consistent with at least four distinct propositions derived from hegemony theory.

1. *The United States is still a hegemon.* The continued openness of the international economy would then tend to confirm hegemony theory.
2. *The United States is no longer a hegemon but the international economy remains open through "afterglow."* Given its grounding in prior theory (Katzenstein, 1978; Krasner, 1983) and Krasner's (1976) original extension of hegemony theory in this direction, an afterglow of some (as yet unspecified) duration would not decisively disconfirm the theory. Disconfirmation will occur only if a "major" shock to the system occurs and, in the absence of any other change, the international economy does not move rapidly toward closure.
3. *The United States is no longer a hegemon but the international economy remains open through multilateral cooperation.* While this tends to disconfirm early versions of the theory, it does not count against and may possibly support variants of hegemony theory that allow for small-group cooperation (e.g., Keohane, 1984; Lake, 1988).
4. *The United States is no longer a hegemon.* Continued openness thus tends to disconfirm the theory. As implied, this conclusion holds when hegemony is a necessary condition for openness.

Clearly, the interpretation placed upon observed patterns depends upon one's definition of structure and position. How much power is necessary to be hegemonic? A middle-sized power? Is there a clear threshold, or is position more continuous? In the absence of such a definition, we cannot distinguish between interpretations 1 and 2–4; the evidence clearly indicates that the United States has declined, but has it declined far enough? The correct interpretation also depends upon the prior theoretical specification of necessary and sufficient conditions for openness. Depending upon which variant of the research program is at issue, the same evidence can tend to be confirming or disconfirming—as demonstrated by the contrast between interpretations 2, 3, and 4. As always, data do not stand alone.[31]

[31] McKeown (1991) avoids several of these problems by regressing continuous independent variables, trade and income concentrations of the largest and five largest states against trade openness, defined by import values over national income. The coefficients thus reflect the direction and degree of relationship between these variables.

Case studies suffer from identical problems of underspecification. Using words rather than numbers and in-depth analyses of singular issues or events rather than longer historical trends does not avoid the problems of defining structure and position and identifying necessary and sufficient conditions.

In addition, many case studies suffer from the problem of theoretical overextension. Fred Lawson (1983), for instance, attempts to extend hegemony theory to the regional level by analyzing the relationship between local British dominance and trade practices at three Arab ports. He concludes that the structure of economic affairs around the Arabian peninsula during the nineteenth century cannot be explained by hegemony theory. In fact, in Muscat and Aden, British dominance is correlated with trade restrictions (Muscat 1800–1825 and 1890–1905; Aden 1839–1850) and British decline with trade openness (Muscat 1825–1885; Aden 1850–1890); in Mocha, the third case, international political competition is accompanied by trade restrictions (1845–1880).

However, hegemony theory offers no clear predictions of regional behavior; in extending the theory, Lawson distorts the dependent variable.[32] All countries are embedded in the international economy, whether they like it or not; it is at this level that hegemony theory is intentionally stated and at which its propositions are believed to hold. Some countries are also closely linked in a regional economy and may define their economic interests partly in this context. At the regional level, however, there is no presumption that the predictions of the theory will carry through as countries will define their interests with reference to both the local and international arenas.[33] A country may be "large" relative to others in the region, but "small" in comparison to the international economy. Hegemony theory would not necessarily expect this country to pursue policies of free trade. Nor would leadership theory, if it were applied in a similar manner, expect the "large" state to provide public goods unless they were strictly limited to the region. Nesting a regional into an international economy requires more than a simple transference of systemic theories. In the language of earlier systems theory, an international economy is a closed while a regional economy is an open system; this difference must be taken explicitly into account in the process of theory-building. It might be possible to develop yet more variants of leadership and hegemony theory that are sensitive to this problem of nesting, but this has not been done and their logical structures and practical implications are by no means intuitive.

Although intended as a critique, Lawson's evidence actually tends to support the prevailing systemic versions of hegemony theory. Whereas Britain was dominant in Arabia during the early and late nineteenth century, it was globally dominant in the second and third quartiles. Mid-century was the period of greatest international economic openness in Europe. According to Lawson's description, the same appears to have been true for the Arabian peninsula.

Peter Cowhey and Edward Long (1983) examine two alternative explanations of change in the international automobile regime—or, more precisely, the fit

Yet, thresholds could still exist. It is possible that imports or income could become more concentrated in a single power that nonetheless fails to be "hegemonic," suggesting that prior theoretical specification is still required for a complete test of the hypothesis. Nonetheless, since his study covers the periods of British and American hegemony, the absence of a relationship between concentration in the single largest power and openness must count as evidence against the theory.

[32] For another regional application of the theory of hegemonic stability, which avoids the problem of "embeddedness" discussed here, see Laitin (1982). For an important integration of systemic and regional approaches see Krasner (1981).

[33] I deal here only with the consequences of integrating regionally derived interests *into* the systemic theory. To the extent that such regionally derived interests are at variance with systemically motivated ones and are consequential, then hegemony theory in its current guise will also be empirically faulty.

between United States automobile policy and the liberal trade regime. They find that the absence of hegemony is a necessary but insufficient condition for regime alteration; they recommend a synthesis of the theories of hegemonic stability and surplus capacity.

This finding, of course, is not inconsistent with several variants of the research program identified above, only those that claim hegemony or leadership is both necessary and sufficient. More critically, however, hegemony theory, with the possible exception of the strategic trade variant, does not produce predictions about specific sectors. Similarly, the predictions of leadership theory can be expected to hold only if the public good being provided—if, in fact, there is a public good in this case—is limited to the automotive industry. Hegemony and leadership theory predict only the broad contours of national foreign economic policy. While the automobile industry is clearly an important sector, a variety of policies in this specific arena would be consistent with a national policy of economic liberalism/protectionism or providing/not providing public goods. In its present form, the generally positive evidence presented by Cowhey and Long is of unknown utility. Either it must be complemented by a series of sectoral studies that when summed together can be taken as a description of, say, United States foreign economic policy or the authors must identify a second level of necessary and sufficient conditions explicitly stating when and why the predictions of the systemic theory should hold in this particular sector.

Even after over a decade of empirical work, we lack adequate empirical studies of the principal variants of the research program. This is especially true for periods of leadership or hegemony. As a field we have been more intrigued by decline.[34] Although I recognize that the theory has not been confirmed by the available evidence, it would nonetheless be premature to conclude that the theory has been disconfirmed.

Much of the responsibility for the generally inadequate tests of the theory, of course, rests with the theorists, not the empiricists. The former have typically failed to state their arguments in a clear, logically consistent, and falsifiable manner. Given this ambiguity, it is not surprising that arguments get taken out of context or misapplied; indeed, it is a wonder that they get taken seriously at all. Given the small number of cases, real empirical progress will not occur until necessary and sufficient conditions are developed and more clearly articulated.

Empirically oriented researchers, on the other hand, have generally not been sensitive to variations in the research program. Indeed, much of the phrasing in this section has been awkward. The idea of testing a research program borders on the nonsensical. Hypotheses can be tested. Theories can be disconfirmed. Research programs can be more or less successful—but they are not themselves open to direct empirical validation. Nonetheless, authors normally treat the "theory of hegemonic stability" as, in fact, a single, homogenous entity. They do not discuss whether their results apply to leadership theory, hegemony theory, or possibly both. Greater attention to theoretical variations will generate not only better research designs, but more valuable evidence as well.

Research Strategies

In the social sciences, theoretical and empirical progress almost always proceed in tandem. Real-world events stimulate theoretical innovation, new theories are

[34] For an exception see Kapstein (1990). Similarly, despite the now voluminous literature on British and American financial and exchange rate policy during the periods of their respective hegemonies, there have been virtually no attempts to use this historical record to systematically assess leadership theory. Gowa (1984) and Eichengreen (1989) are noteworthy exceptions.

confronted with historical evidence, anomalies generate refinements in the theory, better theories are assessed against additional evidence, and so on. The hegemonic stability research program exemplifies this pattern. As I have argued above, significant theoretical work needs to be done. Empirical research can best aid this process not by seeking decisive disconfirmation but by clarifying questions, describing patterns of behavior in need of explanation, assessing the plausibility of new theoretical predictions, and identifying apparent anomalies. Better data-sets that measure key theoretical concepts more accurately are also necessary. Theory-building cannot proceed without this multifaceted support from empirically minded researchers. In conducting their investigations, however, such scholars should continue following the scientific method. The attempt to falsify carefully constructed "if–then" hypotheses remains the best way to proceed. It is not our methods, but our aspirations and the conclusions that we draw from our empirical research that should be scaled more appropriately.

Two promising research strategies have also been underexploited in the present literature. First, in addition to identifying the correlation (if any) between structure and outcomes, scholars should trace, at both the international and domestic levels, the process by which the independent variable influences the dependent variable.[35] For instance, do countries seek to free ride on the contributions of others? Are there negotiations over economic "burden sharing"? Do conflicts over relative burdens increase as the leader declines? Are countries more forthcoming with their allies?

Equally important, theories of international structure should leave "tracks" on the domestic political landscape as national political leaders seek to understand and cope with the problems and trade-offs identified by each. This understanding, of course, need not be articulated in modern theoretical terms. Nor, given the gap between rhetoric and reality, does it need to be the dominant theme in the political debate. But at the domestic process level key political leaders should show an awareness of the issues central to the theory. Are political leaders aware of the need for an international economic infrastructure, how their actions affect this infrastructure, and their motivations for or against supporting the collective effort? Do political leaders understand that they are in competition with other countries to secure their externality producing industries? If the answer to these and other questions is "No," I would be extremely skeptical of the variants from which they are drawn. If the processes central to a systemic theory have no apparent counterpart in domestic politics, the theory most likely fails to capture meaningful political realities.

Second, hegemony theory can be assessed by disaggregating the systemic outcome into its component parts. This research strategy is obviously less appropriate for leadership theory, as public goods are inherently less decomposable. As noted above, economic openness is the sum of the national foreign economic policies of at least the largest states within the international system. According to hegemony theory, not only should there be variation in policy over time, the subject of most empirical investigation in this area, but there should also be variation across countries. By examining this variation, analysts can greatly expand the number of cases.[36]

Our ability to pursue this research strategy, however, depends upon the careful specification of national trade preferences and modeling of the strategic interaction of states. While progress is being made, these requirements have not

[35] On "process-tracing," see George (1979) and King, Keohane, and Verba (forthcoming). As the latter suggest, process-tracing is a strategy for making many cases from few.

[36] For examples of this research strategy see Lake (1988) for trade policy and Loriaux (1991) for financial issues.

been met by any of the variants discussed above. This underscores the need for additional theoretical refinements and disciplinary patience as these insights are developed. In the meantime, however, identifying national preferences and strategies in particular cases can help to suggest the range of possible variation that must be predicted by any model.

Conclusion

After two decades of research, the field has every right to be disappointed with the hegemonic stability research program. Work in this area provides an object lesson in how not to conduct social science. Leadership and hegemony theory remain poorly articulated. Causal propositions still rest on tenuous logic. Central variables are left undefined. Empirical tests are seldom undertaken and are prone to either underspecification or over-extension. The list of complaints could go on. Critics who suggest that the emperor is, in fact, naked and that it is time to abandon the enterprise have ample evidence to support their claim—even if they fail to offer a persuasive alternative.

Yet, such an action, in my opinion, would be premature. The theoretical and empirical failings identified here are not unique to the hegemonic stability research program. The society of, at best, partly clothed rulers is large indeed. More important, despite the obvious shortcomings, there has been significant progress. Leadership theory has made great strides, as the ability to pose, even in a preliminary fashion, necessary and sufficient conditions demonstrates. Interesting and potentially progressive work is being done within hegemony theory; as some of these variants now draw explicitly on economic theory, analysts may finally benefit from the intellectual arbitrage that has facilitated progress in leadership theory. Economic historians and political scientists, who were in some cases stimulated by early work in this area, have amassed a wealth of relevant data. If mined systematically and with a careful eye toward theoretical relevance, far more valid assessments of the individual theories could be performed. The monarch's suit may be incomplete and tattered, but it continues to possess considerable potential.

Although I am disappointed with the current status of the research program, I remain optimistic about its future. But we must move beyond the superficial and instinctive and begin the hard and occasionally tiresome tasks of carefully crafting and testing theory.

The research program skyrocketed to prominence in the 1970s largely on its intuitive appeal. We must resist the equal error of rejecting the theory because it is boring, bloodied by a series of ill-placed but nonetheless highly visible blows, or, now, with continued liberalism evident in the international economy, intuitively unappealing. Perhaps more than anything else, the hegemonic stability research program has sensitized the current generation of scholars to the international political underpinnings of the international economy. This insight should be preserved and built upon, not ignored and abandoned.

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