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Benjamin A. Most

Harvey Starr

University of South Carolina, starr-harvey@sc.edu

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# INTERNATIONAL RELATIONS THEORY, FOREIGN POLICY SUBSTITUTABILITY, AND “NICE” LAWS

By BENJAMIN A. MOST and HARVEY STARR\*

## INTRODUCTION

A pair of basic problems appear to have impeded the development of an integrative understanding of international and foreign policy phenomena. The first has to do with the potential for foreign policy substitutability: through time and across space, similar factors could plausibly be expected to trigger different foreign policy acts. The second problem concerns the potential existence of “sometimes true,” domain-specific laws. It is the logical opposite of the substitution problem: different processes could plausibly be expected to lead to similar results. Neither problem appears to be well understood in the current literature; if anything, standard research practices suggest that both are ignored entirely. Nevertheless, they are potentially important. Taken together, they suggest that scholars who are interested in developing a cumulative base of integrative knowledge about foreign policy and international relations phenomena should recognize that:

1. If writers on the foreign policy behaviors of governments define “islands of theory” in terms of concrete phenomena, their work will not yield broad understanding. If governments behave differently to pursue their (perhaps heterogeneous) national goals and, *under at least certain conditions* nations may *substitute* one means for another, then all of the behaviors that tend to be studied in fragmented fashion need to be conceived from the outset—*not* as separate and distinct phenomena, the understanding of which will eventually be integrated—but rather as commensurable behaviors or component parts of abstract conceptual puzzles.
2. Applications of the standard approaches for testing models and hypotheses are likely to produce misleading results and lead analysts to reject theories and models that are “good,” “nice,” and “useful,” even

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if they are not general, or universally “true.” If it is plausible to argue that states may pursue different goals, for different reasons and with different degrees of effectiveness, then it may be useful to reconsider the efforts to search for a “true,” “general,” or universally applicable explanation of what they do. It might instead be more sensible to search for models or theories that operate, hold, or are valid only under certain explicitly prescribed conditions.

As a consequence of our analysis, we conclude that there is a need to reexamine some of the “grand” theoretical approaches found in the “traditional” literature. A new synthesis of tradition and science and of grand, middle, and narrow approaches seems to be needed. Finally, in contrast to arguments such as those presented by Waltz and Singer, we find that the most fruitful avenues for theorizing and research are at the microlevel, in which the focus is on decision making, expected utility calculations, and foreign policy interaction processes.<sup>1</sup>

#### COMMON RESEARCH PRACTICES

Several basic research practices and procedures have increasingly come to characterize much of the current quantitative research on foreign policy and international phenomena.<sup>2</sup>

*Empirical “puzzles” and empirically defined islands.* Recent theoretical and quantitative empirical research on foreign policy and international relations has tended to focus on the “middle level” in an apparent effort to develop “islands of theory,” or solutions to middle-range “puzzles.” While scholars argue that they are ultimately concerned with understanding why states do what they do, they have eschewed efforts at “grand theory.” They have tended to specialize their research—becoming arms race experts, alliance theorists, war analysts, students of arms transfers, specialists on the uses of foreign economic policy, experts on processes of international negotiation and conflict resolution, and so on.<sup>3</sup>

<sup>1</sup> Kenneth N. Waltz, *Theory of International Politics* (Reading, Mass.: Addison-Wesley, 1979); J. David Singer, “Accounting for International War: The State of the Discipline,” *Journal of Peace Research* 18 (No. 1, 1981), 1-18.

<sup>2</sup> Exceptions could readily be cited. Our contention is only that the practices outlined below are typical of much of what is done, and that they seem to reflect how scholars approach their research problems.

<sup>3</sup> Analysts who work with the WEIS, CREON, and COPDAB events data sets adopt a different approach, of course. Rosenau’s “pre-theory” of foreign policy and its various extensions and tests also quite clearly depart from the pattern by hypothesizing that different explanations may hold in different types of states. See James N. Rosenau, “Pre-Theories and Theories of Foreign Policy,” in Rosenau, ed., *The Scientific Study of Foreign Policy* (New York: Free Press, 1971), 95-149; James N. Rosenau and Gary Hoggard, “Foreign Policy Behavior in Dyadic Relationships: Testing a Pre-Theoretical Extension,” in Rosenau, ed., *Comparing Foreign Policies* (New York: John Wiley & Sons, 1974), 117-49; Maurice A. East,

Some have asked why nations go to war. Others have dealt with why alliances form or dissolve. Possible explanations and effects of arms transfers have been analyzed by a third group of researchers. Still others have focused on arms races, and so on.<sup>4</sup>

Some scholars specialize and focus on one phenomenon or type of event because the subject seems to them to have an intrinsic appeal. They regard the phenomenon, in and of itself, as a concept; their typical goals are to understand its causes or consequences. Other analysts, viewing their chosen empirical behavior or event as an indicator of some more overarching concept, reason that an understanding of the phenomenon will eventually inform them about the concept; examples are scholars who focus on war from the perspective of Galtung's structural theory of aggression.<sup>5</sup> They argue that progress toward an understanding of conceptual relationships will come most rapidly if they focus narrowly, concentrate on a given empirical phenomenon, and study it in isolation. At a minimum, an island of theory will emerge; an area of understanding will be established. If, in the meantime, other analysts have been successful in investigating other concrete behaviors and have produced islands of their own, it should eventually be possible to go further: the

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"Size and Foreign Policy Behavior," *World Politics* 25 (July 1973), 556-76. Our contention is only that such researchers tend to be the exception rather than the rule. It should also be said that a number of analysts report nongeneral findings; e.g., results that hold in the 19th century but not in the 20th, or that apply to major powers but not to minor ones. The point to note, however, is that the majority of these analysts initially look for—and apparently expect to find—general relationships. They have no *initial* theoretical expectation that their model *should* be applicable to some limited domain or that it should be useful only under certain conditions.

<sup>4</sup> For research that adopts a narrow focus on war, see Benjamin A. Most and Harvey Starr, "Diffusion, Reinforcement, Geopolitics, and the Spread of War," *American Political Science Review* 74 (December 1980), 932-46, and Harvey Starr and Benjamin A. Most, "Contagion and Border Effects on Contemporary African Conflict," *Comparative Political Studies* 16 (April 1983), 92-117. For a study dealing exclusively with alliances, see Brian L. Job, "Membership in Inter-Nation Alliances, 1815-1965: An Exploration Utilizing Mathematical Models," in Dina A. Zinnes and John V. Gillespie, eds., *Mathematical Models in International Relations* (New York: Praeger, 1976), 74-109. For a study focusing exclusively on arms races, see Philip A. Schrodt, "Richardson's Model as a Markov Process," *ibid.*, 156-75. Research dealing exclusively with international arms transfers is reported by Ilan Peleg, "Military Production in Third World Countries," in Pat McGowan and Charles W. Kegley, eds., *Threats, Weapons and Foreign Policy* (Beverly Hills, Calif.: Sage Publications, 1980), 209-30. For an analysis focused exclusively on international negotiations, see Glenn H. Snyder and Paul Diesing, *Conflict Among Nations* (Princeton: Princeton University Press, 1977). Analysts who have written about "islands of theory" in international relations include Harold Guetzkow, "Long Range Research in International Relations," *American Perspective* 4 (Fall 1950), 421-40, and Nigel Forward, *The Field of Nations* (Boston: Little, Brown, 1971).

<sup>5</sup> See, for example, Maurice A. East, "Status Discrepancy and Violence in the International System: An Empirical Analysis," in James N. Rosenau, Vincent Davis, and Maurice A. East, eds., *The Analysis of International Politics* (New York: Free Press, 1972), 299-319, and Michael D. Wallace, *War and Rank Among Nations* (Lexington, Mass.: Lexington Books, 1973).

“war” island would be linked with other middle-range theories of alliance behavior, arms transfers, defense expenditures, and so on. This theoretical synthesis, or bridging of the islands of middle-range theory, is expected to provide a more abstract, broad-gauge, or overarching understanding of foreign policy and international behavior.

*Empirical generalizations, laws and “always-true” theories.* The tendency to define puzzles or research questions in terms of particular, concrete empirical phenomena and to focus on one—but only one—type of event at a time combines with the fact that most scholars who study foreign policy and international relations systematically or scientifically seem to think of social laws and theories as being either true or false.<sup>6</sup> As we have noted previously, the search for *empirical* generalizations seems to have become the *sine qua non* of at least those scholars who study international relations quantitatively. To understand or explain a phenomenon such as an overt military attack, they consider it necessary to identify the factor(s) that, alone or in various combinations, correlate with the occurrences of attacks or that seem generally useful—according to some statistical criterion—for postdicting the occurrences of attacks in the data set in question. Although a few exceptions may be noted, the most common view is that a “good” explanandum is one that is generally associated through time or across space with attacks; a “less than good” explanandum is one that is not. Research proceeds by identifying and retaining the former while rejecting and abandoning the latter.<sup>7</sup>

On first consideration, these practices seem largely commonsensical. They do not seem to be at all problematic. Indeed, the practices are so uncontroversial that they are seldom discussed in the current literature. Researchers may be led astray, however, if they narrowly focus their research on only one type of empirical foreign policy behavior, define

<sup>6</sup> See James S. Coleman, *Introduction to Mathematical Sociology* (Glencoe, Ill.: The Free Press of Glencoe, 1964), 516.

<sup>7</sup> We are referring here to standard uses of the so-called “general covering law” or “refutationist” approach developed by scholars such as Hempel and Popper. For a brief rehearsal, see: Gregory A. Raymond, “Introduction: Comparative Analysis and Nomological Explanation,” in Charles W. Kegley, Jr., Gregory A. Raymond, Robert M. Rood, and Richard A. Skinner, eds., *International Events and the Comparative Analysis of Foreign Policy* (Columbia: University of South Carolina Press, 1975), 41-51. The reasoning underlying this approach can be crudely reconstructed in syllogistic form: If a “true” (i.e., universal) law exists between some concepts *X* and *Y*, then that law should *always* hold in the empirical world and should be evidenced by whatever are the appropriate associations between the respective operational indicators of the concepts, *xi* and *yi*. Empirical analyses are conducted to test for such associations between the occurrences of the *xi* and *yi* in the expectation that those results will inform us about whether or not a law actually exists between *X* and *Y*. The absence of the association between the *xi* and *yi* allows us, if the reasoning is sound, to reject the *X/Y* law, while the appearance of the anticipated *xi/yi* relationship allows us to say that the data are consistent with, or support the existence of, the *X/Y* law.

problems or potential islands in terms of such concrete phenomena, search exclusively for empirical-level generalizations, and equate laws with universal truths.

#### PROBLEM NO. I:

##### THE OVERLOOKED POTENTIAL FOR POLICY SUBSTITUTION

The practices described above are likely to lead to a more abstract understanding of why states do what they do *only if* they do not substitute one empirically distinct foreign policy or behavior for another in pursuit of their (perhaps heterogeneous) national goals. If foreign policies can indeed be alternative routes by which decision makers attain their goals, then it would seem plausible that decision makers who are confronted with some problem or subjected to some stimulus could, *under at least certain conditions, substitute one such means for another*. If that is the case, any factor, stimulus, or problem that triggers *some particular* type of empirical foreign policy response could, under different conditions, trigger other, *apparently* distinct, *apparently* incommensurable behaviors. If the argument is valid, *similar* factors could lead to *distinct* concrete or empirical foreign policy responses.

The confounding effects of this potential for foreign policy substitutability can hardly be overemphasized. For example, Realists have argued that states seek the goal of security in the Westphalian self-help system. To the extent that states do indeed pursue such a grand or meta-conceptual goal ( $Y$ ), and *in principle* have a variety of partially substitutable, alternative means—war ( $y_1$ ), alliance formation ( $y_2$ ), arms importation ( $y_3$ ), arms increases ( $y_4$ )—for attaining the goal, a given presumed causal factor ( $x_1$ ; e.g., an increase in an opponent's arms) could logically be expected to explain why the decision makers of a given state would be willing to adopt *some* attempt to resolve the problem,  $y_i$ . Because the stimulus ( $x_1$ ) could be expected to trigger the adoption of  $y_1$  by some decision makers while others might choose different options such as  $y_2, y_3, \dots, y_n$ , however, an understanding of *which particular*  $y_i$  would be adopted by different decision makers under different conditions would involve a consideration of how they make comparisons across, and eventually choose from, the range of those available options. It would require that analysts focus on more than a single type or form of foreign policy behavior, rethink the strategy of focusing on particular empirical phenomena, and reconsider the construction of middle-range islands of theory on the basis of concrete, unabstracted phenomena.<sup>8</sup>

<sup>8</sup> If  $y_1, y_2, \dots, y_n$  are all potential means for attaining an assumed foreign policy goal, then each  $y_i$  is a potentially valid indicator of  $Y$  (e.g., the initiation of an overt military

Two examples should help to clarify these points. First, let us imagine a simple, totally deterministic, world in which the development of a "problem" ( $X$ ) is invariably sufficient—but perhaps not empirically necessary—for an effort to resolve the problem ( $Y$ ). Wherever and whenever  $X$  occurs,  $Y$  follows. The only complication is that efforts to resolve the problem ( $Y$ ) can take any of three different empirical forms ( $y_1, y_2, y_3$ ) which are not necessarily mutually exclusive (i.e., an actor could respond to  $X$  by adopting any one or any combination of them). If (1)  $x_1$  is a valid and reliable indicator of  $X$ , (2)  $y_1, y_2$ , and  $y_3$  are valid and reliable complementary indicators of  $Y$ , and (3)  $x_1$  and  $-x_1$  denote the presence or absence of the problem  $X$  while  $y_i$  and  $-y_i$  denote the presence or absence of a given attempt to resolve the problem, the following case structures are possible:

Case 1	$x_1$	$y_1$	$-y_2$	$-y_3$	$Y$
Case 2	$x_1$	$-y_1$	$y_2$	$-y_3$	$Y$
Case 3	$x_1$	$-y_1$	$-y_2$	$y_3$	$Y$
Case 4	$x_1$	$y_1$	$y_2$	$y_3$	$Y$
Case 5	$-x_1$	$-y_1$	$-y_2$	$-y_3$	$-Y$
Case 6	$-x_1$	$y_1$	$-y_2$	$-y_3$	$Y$
Case 7	$-x_1$	$-y_1$	$y_2$	$-y_3$	$Y$
Case 8	$-x_1$	$-y_1$	$-y_2$	$y_3$	$Y$

Analysts who recognize the complementarity of the  $y_i$  and focus on abstract attempts to resolve the problem ( $Y$ )—rather than on the specific empirical forms that such solutions take—would not be at all confused by this pattern; each time  $x_1$  appears,  $Y$  follows. Because  $Y$  also appears when  $x_1$  is not present, however, these analysts would conclude that  $X$ , as indicated by  $x_1$ , is sufficient (but not necessary) for  $Y$ . Other analysts who focus narrowly and specialize in attempts to understand the occurrence of any one—and only one—of the concrete  $y_i$  behaviors would run the risk of overlooking the relationship, however; the occurrence of  $x_1$  sometimes—but not always—precedes any particular  $y_i$ . In short,  $X$ , as indicated by  $x_1$ , would *not* appear to be sufficient (or necessary) for any one of the  $y_i$ .<sup>9</sup>

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strike or an increase in defense spending are complementary indicators of the concept "efforts to maintain or enhance security" or "foreign policy means"). Once this is recognized, the problem is easily understood: A focus on any one  $y_i$  fails to provide full coverage of the range of possible behaviors in the "means" concept. Analysts who focus on only one  $y_i$  are thus focusing only on "successful" occurrences. To that extent, an examination of the possible sufficiency relationship between any presumed causal factor ( $x_i$ ) and the  $y_i$  is precluded. The point here is analogous to our criticism of analysts who focus only on actual occurrences of war. See Benjamin A. Most and Harvey Starr, "Case Selection, Conceptualization and Basic Logic in the Study of War," *American Journal of Political Science* 26 (November 1982), 834-56, and "Conceptualizing War: Consequences for Theory and Research," *Journal of Conflict Resolution* 27 (March 1983), 137-59.

<sup>9</sup> The reader may think that researchers could readily resolve this problem by constructing

A consideration of the hypothetical interactions among states  $m$ ,  $i$ , and  $j$  in columns 1 to 3 in part A of Table 1 may clarify the problem further. The  $m$ th state increases its defense spending in each alternative round. States  $i$  and  $j$  respond by increasing their own expenditures (at  $t_2$ ), by offering to form an alliance (at  $t_4$ ), by dissolving an alliance or seeking foreign arms acquisitions respectively (at  $t_6$ ) and, respectively, by seeking to negotiate or attack (at  $t_8$ ).

Recognizing that the illustration is purely hypothetical and does not exhaust the full range of states' possible foreign policy behaviors, the basic point should be obvious: the situation would appear chaotic to analysts who ignored or overlooked the substitutability potential and focused on only a single type of behavior. The typical arms race theorist who focuses on reciprocated increases in defense spending would discover that increases by one state sometimes do, but sometimes do not, lead to similar responses by other states. The alliance theorist would discover that an increase in defense spending antedates an effort to form an alliance in one instance, but that other such increases precede the breakup of an alliance or states' complete inactivity in the alliance sphere. The war theorist and the expert on arms transfers would encounter similar complexities. Stepped-up defense spending precedes an attack in one case but also antedates conciliatory efforts in another; spending increases generally do not, but in one case does, appear to lead to an effort to import arms.

Thus, analysts who elect to focus on only one of the behaviors shown in the table are likely to be led astray. The strategy would make sense only if decision makers of all states responded identically to identical stimuli and were unable to substitute. If, however, the behaviors of decision makers of different states could in fact take a variety of forms, there would be no reason to expect a systematic, sufficient relationship between any initial condition and *any other* specific form of foreign policy behavior. To the extent that decision makers have some latitude in their choice of options and are sometimes able to substitute one alternative for another, a given factor could be expected to lead to, stimulate, or "cause" a variety of empirically distinct foreign policy acts, events, or behaviors.

The substitutability phenomenon, then, provides a possible answer to

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some composite scale of the various  $y_i$ . In the past, the fashion would have been to factor analyze the  $y_i$ . Such a strategy would work only if it is reasonable to assume that a given phenomenon  $y_i$  invariably indicates the concept  $Y$ . As the discussion in the next section suggests, such a postulate might not be plausible. While the initiation of conflict may sometimes reflect the desire of decision makers to establish (or reestablish) their state's external security or viability, for example, the same behavior could reflect their desire to increase their government's viability in the face of domestic pressures.



TABLE I  
 HYPOTHETICAL ACTION/REACTION PROCESS  
 A

<i>Time</i>	<i>States</i>				
	<i>m</i>	<i>i</i>	<i>j</i>	<i>p</i>	<i>q</i>
<i>t1</i>	x4				
<i>t2</i>		x4	x4	00	00
<i>t3</i>	x4				
<i>t4</i>		x5	x5	00	00
<i>t5</i>	x4				
<i>t6</i>		x6	x7	00	00
<i>t7</i>	x4				
<i>t8</i>		x8	x9	00	00

x4: Increase defense spending

x5: Offer to form an alliance

x6: Dissolve an alliance with state *k*

x7: Seek to import arms

x8: Seek to negotiate outstanding grievances

x9: Initiate an attack on the threatening state

00: No behavior

B

<i>Time</i>	<i>States</i>				
	<i>m</i>	<i>i</i>	<i>j</i>	<i>p</i>	<i>q</i>
<i>t1</i>	X2				
<i>t2</i>		X2	X2	00	00
<i>t3</i>	X2				
<i>t4</i>		X2	X2	00	00
<i>t5</i>	X2				
<i>t6</i>		X2	X2	00	00
<i>t7</i>	X2				
<i>t8</i>		X3	X3	00	00

X2: Increase national defense capacity (*C*)

X3: Decrease national defense risk (*R*)

00: No behavior

## C

Time	States				
	<i>m</i>	<i>i</i>	<i>j</i>	<i>p</i>	<i>q</i>
<i>t</i> 1	X1				
<i>t</i> 2		X1	X1	X1	X1
<i>t</i> 3	X1				
<i>t</i> 4		X1	X1	X1	X1
<i>t</i> 5	X1				
<i>t</i> 6		X1	X1	X1	X1
<i>t</i> 7	X1				
<i>t</i> 8		X1	X1	X1	X1

X1: Decision makers attempt to have  $C > R$

## D

## PERIOD-END STATUS

Time	States				
	<i>m</i>	<i>i</i>	<i>j</i>	<i>p</i>	<i>q</i>
<i>t</i> 1	$C > R$	$C < R$	$C < R$	$C < R$	$C > R$
<i>t</i> 2	$C < R$	$C > R$	$C > R$	$C < R$	$C > R$
<i>t</i> 3	$C > R$	$C < R$	$C < R$	$C < R$	$C > R$
<i>t</i> 4	$C < R$	$C > R$	$C > R$	$C < R$	$C > R$
<i>t</i> 5	$C > R$	$C < R$	$C < R$	$C < R$	$C > R$
<i>t</i> 6	$C < R$	$C > R$	$C > R$	$C < R$	$C > R$
<i>t</i> 7	$C > R$	$C < R$	$C < R$	$C < R$	$C > R$
<i>t</i> 8	$C < R$	$C > R$	$C > R$	$C < R$	$C > R$

the most intriguing of Zinnes's "puzzles." Posing the question, "Do nations interact?" she examines the work on arms races—an area in which substitutability could have important effects—and concludes that "the overall and overwhelming result is that nations *do not interact* in this domain."<sup>10</sup> The few studies that do discover interaction do not focus exclusively on armaments but on *overall* interaction patterns which are based on a wide range of hostile/cooperative events.

Although a thorough understanding of the best apparent solution to this difficulty must await the consideration of a few additional points,

<sup>10</sup> Dina A. Zinnes, "Three Puzzles in Search of a Researcher," *International Studies Quarterly* 24 (September 1980), 315-42, at 323 (emphasis in original).

its basic outlines can be traced. *If* scholars are genuinely interested in understanding why states do what they do, they need to move beyond efforts to focus separately on particular concrete behaviors. Rather than asking middle-range questions about specific empirical phenomena, they should begin with the initial “grand” question with which they were allegedly concerned in the first place; rather than asking why states arm, form alliances, import arms, negotiate, attack, and so on, they should begin by asking what each behavior does, or at least could, represent.

Thus, we need to return to the more generic conceptualizations, such as “response to threat” or “response to uncertainty under the Westphalian security dilemma,” that appeared in the traditional literature under the rubric of “security,” “national interest,” or “balance of power.” A whole range of activities that are currently discussed under headings such as trade, aid, international law, regimes, international organization, and so on, could be conceptualized more broadly as “adaptation for coordination or collaboration” or some similarly overarching concept. The point to be emphasized, however, is the need to reconceptualize *exactly what* it is that we want to study, and *why*. Students of international relations have, in many cases, actually reified the operational indicators of international interaction. We have studied war *qua* war, alliances *qua* alliances, and have tended to overlook the broader international processes and phenomena that such specific forms of behavior represent.

The results of this narrowness have been recognized in recent reviews of the alliance literature by Ward and Job.<sup>11</sup> Both authors note that there has been little cumulation or theoretical development. Both conclude that alliance research is focused too narrowly—and rather unfruitfully—on questions such as the alliance/war relationship. While neither calls explicitly for reconceptual efforts, both suggest that the time may have come to move beyond the study of alliances *per se* and efforts to develop alliance theory. Job in particular seems to recommend asking what alliances do (or under certain conceptualizations, could) “really” represent.<sup>12</sup>

<sup>11</sup> Michael Don Ward, “Research Gaps in Alliance Dynamics: Literature Review,” mimeo. (Evanston, Ill.: Northwestern University, 1980), and Brian L. Job, “Grins Without Cats: In Pursuit of Knowledge of International Alliances,” in P. Terrence Hopmann, Dina A. Zinnes, and J. David Singer, eds., *Cumulation in International Relations Research*, Monograph Series in World Affairs, Vol. 18, Book 3 (Graduate School of International Studies, University of Denver, 1981).

<sup>12</sup> The need to reverse the tendency to attack international and foreign policy phenomena in fragmented, piecemeal fashion can be defended on other grounds. The point has been tellingly made by both Ostrom and Boynton. See Elinor Ostrom, “Beyond Positivism: An Introduction to This Volume,” in Ostrom, ed., *Strategies of Political Inquiry* (Beverly Hills, Calif.: Sage Publications, 1982), 11–28, and G. Robert Boynton, “Linking Problem Definition and Research Activities: Using Formal Languages,” in Judith A. Gillespie and Dina A.

The beginnings of one such unifying formulation are shown in Table 2. The "image" is one of value-maximizing decision makers who perceive themselves to be confronted with a security dilemma. A perceived imbalance in the Capacity-to-Risk inequality (i.e.,  $C \leq R$ ) acts as the motive force; given an imbalance, decision makers are inclined or "willing" to attempt to reverse it by adopting some policy initiative that is likely to increase their defense capacity or decrease their risks. This "probable" behavior is expected to interact with what is "possible."<sup>13</sup> Put simply, decision makers will be constrained by the range of available policy options; they will adopt a given policy alternative if, and only if, they have both the "willingness" and the "opportunity" to do so.<sup>14</sup>

Assuming for the moment that the axioms in Table 2 apply to states  $m$ ,  $i$ , and  $j$ , that each state has unlimited implementation capability or opportunities to act, and that  $m$  views  $i$  and  $j$  as risks while  $i$  and  $j$  view  $m$ —but not each other—as possible sources of danger, the interactions among states  $m$ ,  $i$ , and  $j$  in part A of Table 1 begin to become intelligible. The apparently diverse behaviors and acts become commensurable. If the formulation and the just-mentioned auxiliary conditions had been

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Zinnes, eds., *Missing Elements in Political Inquiry* (Beverly Hills, Calif.: Sage Publications, 1982), 43-60. Perhaps J. Bronowski, however, puts the point most clearly:

All science is the search for unity in hidden likenesses. . . . The scientist looks for order in appearances of nature by exploring such likenesses. For order does not display itself; if it can be said to be there at all, it is not there for the mere looking. There is no way of pointing a finger or a camera at it; order must be discovered and, in a deep sense, it must be created. What we see, as we see it, is mere disorder. . . . [Full grown] science grows from a comparison. It has seized a likeness between two unlike appearances. . . . The progress of science is the discovery at each step of a new order which gives unity to what seemed unlike (Bronowski, *Science and Human Values* [New York: Harper Torchbooks, 1965], 13-15).

<sup>13</sup> Harold and Margaret Sprout, *An Ecological Paradigm for the Study of International Politics*, Center of International Studies, Princeton University, Research Monograph No. 30 (Princeton, 1968), and Harold and Margaret Sprout, "Environmental Factors in the Study of International Politics," in James N. Roseneau, ed., *International Politics and Foreign Policy*, rev. ed. (New York: The Free Press, 1969), 41-56.

<sup>14</sup> The careful reader will note that major elements of "Realism" and the "security dilemma" are being formalized in Table 2. See, for example, Hans J. Morgenthau, *Politics Among Nations*, 5th ed. (New York: Alfred A. Knopf, 1973); Robert Jervis, "Cooperation Under the Security Dilemma," *World Politics* 30 (January 1978), 167-214, and Robert O. Keohane, "Theory of World Politics: Structural Realism and Beyond" (Paper presented at the Annual Meetings of the American Political Science Association, Denver, 1982). These elements are being formalized around an expected utility framework that draws on the "cognitive behaviorism" of the Sprouts (fn. 13) in a manner eschewed by Realist theorists. While the model is somewhat similar to the Realist approach, therefore, it is also consistent with the Sprouts' dictum regarding environmental determinism: i.e., motivating factors need to be mapped through decision makers' perceptions *even if* all decision makers perceive alike and they do not make a (statistical) difference. Mapping through the decision makers is a *logical* requirement, not a statistical concern; nothing would happen (no relationships would hold) if decision makers did not exist. (We depart from Sprout and Sprout, however, insofar as we see policy initiations as "outcomes" of the interplay between what decision makers want to do and are capable of doing.)

TABLE 2  
A UNIFIED ACTOR/NATIONAL SECURITY DILEMMA FORMULATION

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Axiom 1: The decision makers of an  $n$ th state are, or can be treated as, unified and value-maximizing actors who possess perfect information regarding all options and their consequences.

Axiom 2: At any given point in time, the decision makers in an  $n$ th state perceive that they have a certain national defense capacity ( $Cnt$ ) and are confronted by some degree of national risk or vulnerability ( $Rnt$ ).<sup>a</sup>

Axiom 3: The decision makers of an  $n$ th state are motivated or willing to establish the following inequality:

$$Cnt > Rnt$$

i.e., the decision makers want to be unconditionally viable and maintain at least local supremacy.

Postulate 1: Only if the decision makers of an  $n$ th state command the necessary objective capability or opportunity (0) to adopt some unilateral policy initiative, then the adoption of that initiative.

Postulate 2: If the decision makers of an  $n$ th state perceive that their state's defense capacity has been neglected or allowed to deteriorate between  $t_0$  and  $t_1$ , they will perceive that  $Cnt_0 > Cnt_1$ .

Postulate 3: If a given state increases its national defense capacity between  $t_0$  and  $t_1$  and the decision makers in an  $n$ th state perceive that action as a threat, they will perceive that  $Rnt_0 < Rnt_1$ .

Postulate 4: If  $Cnt_1 < Rnt_1$ , then the decision makers of the  $n$ th state will be motivated to adopt some policy initiative designed to increase  $Cn$  and/or decrease  $Rn$  at  $t_2$ ; i.e., if the decision makers perceive that they are only conditionally viable at  $t_1$ , then they will be willing to increase their capacities or decrease their risks.

Postulate 5: If, and only if, the decision makers of an  $n$ th state command the necessary capacity (or opportunity) to adopt some unilateral policy initiative and they are motivated (or willing) to do so, then the adoption of that initiative.

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<sup>a</sup> Given the purpose of the illustration, it is not important to consider how one might operationalize either  $Cnt$  or  $Rnt$ . Only three points need to be noted. First, it should be understood that both capacities ( $C$ ) and risks ( $R$ ) are conceptualized here in terms of decision makers' perceptions. Put simply, a state's defense capacities and the risks it confronts are as they are recognized and understood by the decision makers. Consistent with this conceptualization, the capacities of "friendly" or distant states may be discounted by an  $n$ th state's decision makers in their calculation of  $Rnt$ ; similarly, the capacities of friendly, and especially allied, states could be counted by the  $n$ th state's decision makers as supplemental sources of  $Cnt$ . Finally, it should be clear that capacities ( $C$ ) and risks ( $R$ ) may differ from the decision makers' objective capabilities or opportunities (see Postulate 1), which affect policy initiatives regardless of whether or not they are (accurately) perceived by the decision makers.

in place at the outset, it would have been possible to deduce that each state would adopt *some* measure to increase capacity (*C*) or decrease risk (*R*) in each successive round (see part B of Table 1). Each of these concepts would have brought together a number of hitherto apparently diverse behaviors. Within the context of the model, increases in defense expenditures, offers of alliance, and efforts to import arms could have been seen as commensurable; they could have been recognized as alternative indicators of decision makers' efforts to increase their defense capacity. Alliance dissolutions, offers to negotiate, and military strikes could also have been rendered commensurable as efforts to decrease risk. It would have been possible to go further; efforts to increase capacity or decrease risk—along with the distinct empirical behaviors that each embraces within the formulation—could have been linked in turn to decision makers' efforts to manage their respective *C*-to-*R* inequalities (see part C of Table 1).

With all of this in mind, consider the behaviors of nations *p* and *q* in columns 4 and 5 of Table 1 above. While states *m*, *i*, and *j* display a wide range of behaviors—presumably in an effort to manipulate their Capacity-to-Risk ratios—*p* and *q* remain completely inactive. Viewed at the raw empirical level, there appears to be no consistency whatsoever in the actions of the five states. Each responds differently. Even for analysts who recognize the underlying unity of the actions of the first three states, the behaviors of *p* and *q* would probably appear to be distinct departures from the expected pattern. While the acts of nations *m*, *i*, and *j* are consistent with the postulate that the decision makers in those nations perceive a security problem and are therefore undertaking policy initiatives to increase their defense capabilities or decrease their risks, *p* and *q* appear to behave quite differently. They do nothing at all; the conceptualization does not seem to apply.

The point to be noted, however, is that states *p* and *q* could be imbedded in the pattern if one began with the model in Table 2 and also knew the following: (1) The decision makers of *p* view *m* as a potential threat that upsets their *C*-to-*R* ratio throughout the sequence, but they lack the capability to undertake any action to reset the inequality (see Postulates 1 and 5 in Table 2); (2) the decision makers of *q* do not perceive the acts of any of the other states as threatening, perhaps because they are distant from or ideologically allied to them (see Postulate 3). Given these auxiliary conditions, one would expect the empirical behavioral patterns of *p* and *q* to appear distinct from those of states *m*, *i*, and *j*. While the latter three would be expected to increase their defense capacities or decrease their risks if the model were "true," *p*

would remain inactive because its decision makers' willingness to act would be frustrated by lack of capability, while  $q$  would remain inactive because it simply is not motivated to act. The acts of all five states  $m$ ,  $i$ ,  $j$ ,  $p$ , and  $q$  in Table 1 are in fact consistent with the postulate that decision makers are operating to establish favorable Capacity-to-Risk inequalities.<sup>15</sup>

These observations raise an important point that is generally overlooked: correlations and other such associations may not exist among empirical indicators simply because a given process is operating. If the security dilemma formulation shown in Table 2 were "true," states could reasonably be expected to behave differently. The relationships among the theoretical concepts could be general, but even if that were the case, the outcomes of the decision processes of different states *could be* rather heterogeneous. Again, if the model were "true," different empirical behaviors could follow.

#### PROBLEM NO. 2:

#### THE OVERLOOKED POSSIBILITIES OF ALTERNATIVE TRIGGERS AND "SOMETIMES TRUE" LAWS

The problem created by the potential for foreign policy substitutability is exacerbated by the practice among analysts of equating laws, theories, and models with universal—or at least highly generalizable—empirical "truths." At one level, this problem arises because the operation of even universal laws may not invariably be evident empirically. Scholars already recognize some of the reasons why this could be the case.<sup>16</sup> Our concern here is with a more fundamental—and generally overlooked—possibility. Put simply, analysts may be led astray when they associate

<sup>15</sup> Another reason why states  $p$  and  $q$  are of interest derives from the so-called "empty cell" problem. Instead of ignoring the "nonbehaviors" of actors (the zeros or empty cells in data matrices), analysts might attempt to utilize the important information conveyed by such nonbehaviors when they construct and evaluate their models. For an empirical example of this phenomenon in which "neutral" war behavior plays the key role in model structure and evaluation, see Dina A. Zinnes and others, "From War to War: A Stochastic Model of Alignment Behavior" (Paper presented at the Annual Meetings of the International Studies Association, Philadelphia, 1981).

<sup>16</sup> Even though a universal or "true" relationship may exist, for example, difficulties with indicator validity and reliability could mask that association at the empirical level. In our own earlier work, we have suggested that additional complications may develop as a result of faulty case selection procedures or the failure to specify the correct logical linkages among the concepts and indicators (see fn. 8). Those problems are critical. They raise doubts about the standard approach (see fn. 7). To the extent that such difficulties exist, or at least cannot be ruled out, the results of the tests become ambiguous. The lack of empirical support in a given analysis could indeed imply that a supposed law is not true, but it could also mean that operation of a true law is obscured by the procedures adopted by the researcher conducting the test.

laws and theories with questions of universal truth or empirical generalizations because a given phenomenon may occur for a variety of distinct, totally incommensurable, reasons.

Just as the possibility of foreign policy substitution suggests that a given factor may lead to different results (a one-to-many mapping), the logical reverse is also imaginable. Different processes may lead to similar results; different factors may trigger similar responses (a many-to-one mapping). If policy makers can use different options in their pursuit of similar goals (i.e., substitute), they can also adopt identical options for different reasons, or employ similar means to pursue different goals.<sup>17</sup>

One aspect of the alternative trigger problem is implicit in part A of Table 1 above; in this guise, at least, the difficulty is easily circumvented by employing existing procedures. As we showed in that illustration, the  $m$ th state's increases in each round could plausibly be expected to lead to different responses by the  $i$ th and  $j$ th states; in a process of foreign policy substitution, a given trigger could lead to different empirical reactions. The point to be noted here is that even though the  $m$ th state elects to increase its defense spending in each round, we should not necessarily expect to find any single empirical trigger for the selection of that option. If we assume that the leaders of the  $m$ th state are concerned with their country's security situation, then increases in defense spending by  $i$  and  $j$ , the formation of an alliance between them, the dissolution of an alliance, efforts by  $j$  to import arms, and certainly attacks by the  $j$ th state should *all* be expected to contribute to decisions by leaders of the  $m$ th state to increase defense capabilities.

A second aspect of the alternative trigger problem is perhaps more important; in this form, the difficulty is less easily resolved. Let us imagine a world that comprises two sets of cases: those characterized by condition  $A$  and those that are not, i.e., a world in which there are

<sup>17</sup> Bruce Bueno de Mesquita appears to have exactly this problem in mind when he considers research on the outbreak of war and observes,

... the set of conditions sufficient for war may be so large that its specification is virtually impossible. ... Most efforts to find the cause or causes of war focus on environmental circumstances that compel policymakers to wage war, but if we attempt to show causal relationships between environments and war, we are forced to ignore the role of national leaders and to act as though nations were no more than automatically reacting mechanisms (Bueno de Mesquita, *The War Trap* [New Haven: Yale University Press, 1981], 4-5).

It is this line of argument—combined with his contention that decision makers are not mechanistically compelled—that leads Bueno de Mesquita to conclude that the search for the sufficient conditions for war should be abandoned in favor of a search for necessary conditions. A similar position is suggested by Singer (fn. 1). Other related points are contributed by James R. Kurth, "A Widening Gyre: The Logic of American Weapons Procurement," *Public Policy* 19 (Summer 1971), 373-404; and, Ludwig Von Bertalanffy, *General Systems Theory* (New York: Braziller, 1968).



*A* and  $-A$  cases. Presume that type *A* case are those in which decision makers are concerned with maintaining their states' national security, while decision makers in  $-A$  polities pursue other goals. Imagine further that within the set of *A* cases, the development of an external risk (*X* or any *xi*) is invariably sufficient for an effort to resolve the security problem (*Y*), but external risk is not necessary for efforts to resolve the security problem because leaders are also concerned with through-time deteriorations in their defense capacities. Within the set of *A* states, *Y* follows wherever and whenever *X* (or any *xi*) occurs, but *Y* sometimes occurs in the absence of *X* (or all *xi*). By contrast, when *A* is not present, *X* (and the *xi*) do not lead to *Y*, and *Y* sometimes occurs in the absence of *X* (or all *xi*).<sup>18</sup> Finally, let us make the usual presumptions regarding the validity and reliability of the indicators and consider the following possible case structure:<sup>19</sup>

Case 1	<i>A</i>	<i>x</i> <sub>1</sub>	$-x_2$	$-x_3$	<i>X</i>	<i>Y</i>
Case 2	<i>A</i>	$-x_1$	<i>x</i> <sub>2</sub>	$-x_3$	<i>X</i>	<i>Y</i>
Case 3	<i>A</i>	$-x_1$	$-x_2$	<i>x</i> <sub>3</sub>	<i>X</i>	<i>Y</i>
Case 4	<i>A</i>	<i>x</i> <sub>1</sub>	<i>x</i> <sub>2</sub>	<i>x</i> <sub>3</sub>	<i>X</i>	<i>Y</i>
Case 5	<i>A</i>	$-x_1$	$-x_2$	$-x_3$	$-X$	<i>Y</i>
Case 6	<i>A</i>	$-x_1$	$-x_2$	$-x_3$	$-X$	<i>Y</i>
Case 7	<i>A</i>	$-x_1$	$-x_2$	$-x_3$	$-X$	<i>Y</i>
Case 8	<i>A</i>	$-x_1$	$-x_2$	$-x_3$	$-X$	<i>Y</i>
Case 9	$-A$	<i>x</i> <sub>1</sub>	$-x_2$	$-x_3$	<i>X</i>	$-Y$
Case 10	$-A$	$-x_1$	<i>x</i> <sub>2</sub>	$-x_3$	<i>X</i>	$-Y$
Case 11	$-A$	$-x_1$	$-x_2$	<i>x</i> <sub>3</sub>	<i>X</i>	$-Y$
Case 12	$-A$	<i>x</i> <sub>1</sub>	<i>x</i> <sub>2</sub>	<i>x</i> <sub>3</sub>	<i>X</i>	$-Y$
Case 13	$-A$	$-x_1$	$-x_2$	$-x_2$	$-X$	<i>Y</i>
Case 14	$-A$	$-x_1$	$-x_2$	$-x_3$	$-X$	<i>Y</i>
Case 15	$-A$	$-x_1$	$-x_2$	$-x_3$	$-X$	<i>Y</i>
Case 16	$-A$	$-x_1$	$-x_2$	$-x_3$	$-X$	<i>Y</i>

<sup>18</sup> Expressed simply, the hypothesis is of the form, "Given *A*, if *X* (or any *xi*), then *Y* (or any *yi*)"; i.e., given *A*, *X* is sufficient, but not necessary, for *Y*. If *A* were a measured variable rather than a theoretical postulate or axiom which is presumed to be either true or untrue, the proposition could be rewritten: "If *A* and *X* (or any *xi*), then *Y* (or any *yi*); i.e., *A* and *X* are jointly sufficient for *Y*." Seen in this fashion, axioms are simply dichotomous variables that analysts either choose or are forced to leave unmeasured. This suggests that axioms should in general not be viewed as assertions of universal truth. They should instead be seen as antecedents in complex "if . . . , then . . ." statements which, in collapsed form, would read: "If the specified axioms are true for a given case, then the model should apply." Once this is seen, we can move quickly beyond debates over the universal truth or falsity of the assumptions themselves: the question of the truth or falsity of the model is put aside. We can even move beyond concerns for generality and become more directly concerned with (a) identifying cases in which the axioms are true and in which the model should therefore be useful, and (b) developing complementary models that hold under alternative sets of initial axioms.

<sup>19</sup> It is perhaps worth noting that this illustration entails an important simplifying as-

Even for those analysts who recognize both the substitutability problem and the first type of alternative trigger problem and therefore elect to focus on  $Y$  and  $X$  rather than exclusively on the occurrence of any one of the  $y_i$  or  $x_i$ , interpretation of the pattern would not be a simple matter if they ignored the  $A/-A$  bifurcation of the set of cases and were concerned only with the  $X/Y$  relationship. Put simply, application of the standard procedures would lead them either to reject the hypothesis that  $X$  generally leads to  $Y$  or to underestimate the importance of that linkage.

Within the domain circumscribed by the presence of  $A$  (cases 1 to 8 above), however,  $X$  is sufficient for  $Y$ . Outside that domain,  $X$  does not lead to  $Y$  (cases 9 to 16). The occurrences of  $Y$  in the  $-A$  domain must be triggered by something else. Knowing about  $X$  or any  $x_i$  does not help.

That said, however, the point should be clear. While the  $X/Y$  hypothesis is not general, it is nevertheless a perfectly sound—indeed, even “lawlike”—proposition within its domain of applicability (the cases in set  $A$ ). Although ultimately we should like to develop an explanation that applies to all sixteen of the cases above, no additional work—either theoretical or empirical—is necessary to account for the first eight.

Thus, there are two different ways to approach the cases above. One is to develop and test hypotheses that are expected at the outset to be general or applicable to the universe of cases. Such a method would be consistent with current practices and the standard approach in which generalizations are found by aiming directly at them. Unfortunately, that strategy seems to reduce to an all-or-nothing situation in which we are forced to play to weakness; to explain completely any occurrence of a phenomenon, we must account for all occurrences.

The other strategy is to develop and test hypotheses that are expected to hold only under certain conditions (e.g.,  $A$  or  $-A$ ; when decision makers seek to maintain their national security, and when they do not). Although we are ultimately interested in accounting for all occurrences of a phenomenon, the immediate (somewhat counterintuitive) strategy would entail an initial consideration of *what* relationships should hold *when*, and a focus on only those cases in which the conditions pertain. Once those occurrences were explained, we could move on, perhaps developing very different models that would be applicable to the other

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sumption: states have the opportunity to act whenever they are willing to do so. Under the unified actor/national security dilemma formulation outlined in Table 2, we would expect either increasing external risks or decreasing defense capacity to contribute to the leaders' willingness to resolve the security problem if the change in either factor is sufficient to reverse previously positive Capacity-to-Risk inequalities. Whether or not the decision makers would act on that disposition, however, would be affected by their objective opportunities to act.

cases. This domain-specific theory approach would thus let us play to strength; rather than having to account for the phenomenon *Y* both when decision makers are and are not concerned with their states' security dilemmas, we could conclude that there are occurrences that are explicable (i.e., those in the *A* set) and others whose explanation requires more theoretical and empirical research (i.e., those in the  $-A$  set). Rather than resting progress on the isolation of *one* explanation that holds for all, this approach allows us to move ahead by identifying *an* explanation that holds for some.

Let us consider now the unified actor/security dilemma formulation discussed above (see Table 2). Among other things, that model specifies that decision makers are motivated to establish the inequality expressed in Axiom 3:  $Cnt > Rnt$ . It suggests that decision makers want their states to be unconditionally viable; it would be consistent with their attempts to maintain at least local supremacy.<sup>20</sup>

If we presume for the purpose of argument that at least some decision makers in at least some settings do in fact key their decisions to that inequality, we come up against an obvious problem. With very little in the way of data analysis, we know that there are (or in principle, could be) states that act to increase their defense capabilities and decrease their risks at given times even though they have already so clearly satisfied the inequality and established their unconditional viability that the acts of other states do not reverse it. Because such states have already attained their goal, the motivation that presumably drives decision makers is lost; the antecedent condition in Postulate 4 (i.e., the condition  $Cnt_1 < Rnt_1$ ) would simply never be established. Even though the model would therefore be adequate for accounting for the behaviors of states such as *m*, *i*, and *j* in Table 1, it would leave totally unexplained the behaviors of states that are already convincingly viable.

Still assuming, then, that the formulation in Table 1 is useful for accounting for the decisions of some states, but also concluding that it could not logically be expected to account for the decisions of all polities, we are in a difficult situation if the goal is to discover a single, general formulation that applies to all cases both through time and across space. Even though the axioms of the model might be valid for some cases and the model's postulates might always hold within that domain, current practices would lead us to reject the model because neither it nor the relationships it implies are universally "true." Put more squarely, a model that might be perfectly adequate for explaining what happens in

<sup>20</sup> See Kenneth Boulding, *Conflict and Defense* (New York: Harper Torchbooks, 1962).

*some well-defined* cases might be abandoned because it does not always apply or help to explain *all* cases.

The importance of this point becomes more apparent once we recognize that a variety of other motive assumptions could be postulated to characterize decision-making processes. One such possibility would be a variation on the national-security model in Table 2; rather than attempting to establish the  $Cnt > Rnt$  inequality, decision makers could be posited as seeking the maintenance of the status quo so that the Capacity-to-Risk ratio at any time is at least equal to that of an earlier period. (Such a modified national-security formulation might be applicable to states that continue to increase their defense capacity and reduce the threat engendered by their opponents *even though* they are already unconditionally viable; it might be consistent with the illusory quest for perfect security or with the security dilemma of never knowing how much capability is enough.) In contrast to such national-security motivations, one could posit "domestic" viability models in which decision makers seek either to maintain enough political strength to keep from being deposed by their domestic opposition or to maintain at least their prior position vis-à-vis domestic threats. Other goals, and indeed combinations of several goals, could also be presented; any one of them could provide a rationale for expecting that decision makers might increase their military expenditures, create alliances, dissolve them, import arms, open negotiations, launch preemptive military strikes, and so on. Different processes, including a number that depart from an assumption of unified, value-maximizing decision makers, could lead to similar results.

A consideration of such alternative formulations suggests several important points. First, there is the partitioning role of axioms. Although there are probably some actual cases in which decision makers pursue almost any goal that one might reasonably wish to posit, there may also be instances in which decision makers are motivated by other concerns. To the extent that this seems plausible, such alternative motive forces or goals would not in any sense constitute the bases for rival or competing models. To be sure, one of the motive assumptions might be true for more cases than the others. If so, the formulation appropriate to that axiom would be more useful than the other models. The point, however, is that it would probably not be plausible to argue that any one of the motive axioms is always true. Thus, it would not be reasonable to expect any of the models to be universally applicable. We would therefore not be inclined to conduct a critical test in order to discover which is the more general. Instead, we would be interested in exploring whether or

not a given model applies to a given, well-defined case or set of cases.

Second, if different processes do indeed lead to similar concrete empirical behaviors, the concept reflected by one such act could be different within different formulations. If the decision makers are dealing with a national viability or security dilemma, for example, an increase in their defense expenditures or the formation of an alliance would be interpretable as—and more importantly, serve as an indicator of—efforts to increase their state's defense capacity or to decrease the foreign risks with which they perceive it to be confronted; if they are seeking to resolve a simple governmental viability problem, on the other hand, those same acts would reflect efforts to increase their government's strength or to decrease the degree to which it is threatened by domestic forces. Even though analysts may tend to view acts as inherently or intrinsically falling on some fixed scale between conflict and cooperation, or between friendly and hostile behaviors, therefore, it might make more sense to interpret the actions of decision makers in the context of what they are attempting to accomplish.

Third, and perhaps most important, the argument suggests that it may be useful to expect that true social laws may *not always* hold, operate, or apply empirically. There may well be a variety of social laws, each of which is true, but which should be expected to hold only under certain—perhaps very special—conditions. Although it is possible that universal, always true, laws exist and we believe that scholars should continue their efforts to identify them, it is difficult to think of very many empirical universals that have been identified even by physical scientists. Thus, it may be useful to recognize that there could very well be laws that are in some sense “good,” “domain-specific,” or “nice,” even though the relationships they imply are not necessarily empirically general.<sup>21</sup> Rather than assuming that there need be a single “always true” law that accounts for a given phenomenon whenever and wherever it has occurred or will occur, it may be more productive to think of several laws, each of which is always true under certain conditions (or within certain domains), but which is only sometimes true empirically because those conditions do not always hold in the empirical world. It might be more profitable to distinguish between the truthfulness of a relationship and the question of whether or not it holds in a given instance. Although the identification of universal relationships is still a

<sup>21</sup> Technically, it is not necessary to use words and phrases such as “domain-specific” or “nice” in connection with laws. They are used here only to emphasize our point that analysts may err if they necessarily equate laws with universals (see fn. 22 below). The adjective “nice” is used in similar fashion by G. Robert Boynton, “On Getting from Here to There: Reflections on Two Paradigms and Other Things,” in Ostrom (fn. 12), 29-68.

worthwhile goal, it may be useful to consider other research objectives that are equally important and perhaps more attainable.<sup>22</sup>

Ultimately, of course, analysts may develop several models, each of which may serve to explain a given phenomenon under different conditions. When that occurs—when we understand why *Y* occurs in *both* the *A* and  $-A$  domains—it may be possible to integrate or synthesize those islands of understanding. In the interim, however, we should play to strength; we should learn not to reject or disregard an explanation that is only sometimes true.

#### SOME CONSEQUENCES FOR THE STUDY OF INTERNATIONAL RELATIONS

*Cumulation and theory.* In recent years, James N. Rosenau has become concerned with integrative cumulation and theory building in inter-

<sup>22</sup> Because the points depart so sharply from accepted arguments and assumptions, it may be worthwhile to consider the following observations from scholars who use somewhat different terms, but come to identical conclusions:

There is a widespread misconception that theories are either “true” or “false.” A number of examples in physical science stand in direct contradiction to this. . . . Each of these theories or models is tautologically true, *when* their postulates are fulfilled. . . . They are not theories to be confirmed or disconfirmed in general, but only confirmed or disconfirmed in specific applications. As a result, they are not theories which explain “how people behave”; they are theories or models which describe how people behaved in this or that circumstance . . . one fruitful line of development . . . will be not to ask what is *the* theory of a certain kind of behavior, or what are *the* postulates which correctly describe a general area of behavior. The tactic proposed here is to set about developing and applying a number of sometimes-true theories which relate consequences to postulates, and which may adequately describe behavior in a given situation (Coleman [fn. 6], 516-18; emphasis in original).

. . . the laws of mathematics were invented too. The only demand made on them is that of consistency in a given context. *But in different contexts, different laws may operate.* Therefore, any statement of mathematics is valid or invalid not because certain relations are true or not true in the real world, but because these statements are or are not consequences of certain definitions and assumptions, which we are free to choose. The ambiguity of a mathematical statement results from a failure to specify with sufficient precision the exact context in which the statement is made (Anatol Rapoport, *Fights, Games and Debates* [Ann Arbor: University of Michigan Press, 1960], 297-98; emphasis added).

Laws of nature . . . are different: to them the words “true,” “probable” and the like seem to have no application. . . . [We could] adopt Snell’s formula tentatively, hypothetically, as a guide to further experiments to see whether the phenomena always happen so. On this level, we might ask “Is Snell’s *hypothesis* true or false?” meaning, “Have any limitations been found to the application of his formula?” But very soon—indeed as soon as its fruitfulness has been established—the formula in our hypothesis comes to be treated as a *law*, i.e., as something of which we ask not “Is it true?” but “When does it hold?” When this happens, it becomes part of the framework of optical theory, and is treated as a standard. Departures from the law and limitations on its scope . . . come to be spoken of as anomalies and thought of as things in need of explanation (Stephen Toulmin, *The Philosophy of Science: An Introduction* [New York: Harper Torchbooks, 1953], 78-79; emphasis in original).

Similar points have been made by Boynton (fns. 12 and 21), and Ostrom (fn. 12) and “Introduction: Making Sense Out of a Muddle,” in Gillespie and Zinnes (fn. 12), 37-41.

national relations. Perceived lack of progress in this area has prompted Dina Zinnes to inquire about *why* there has apparently been so little cumulation. Hopmann, Zinnes, and Singer have noted the need to assemble diverse pieces of evidence. Other analysts have considered the unconnected nature of the existing islands of theory and called for their integration.<sup>23</sup>

Our discussion has attempted to delineate a particular impediment to cumulation: *The general failure to conceptualize questions and indicators broadly enough to capture the relationships and processes that scholars are actually interested in studying.* We have argued that the islands of theory extant in the international relations literature have been too narrowly conceived for answering the questions that have been asked; we have suggested that analysts' ways of theorizing—and perhaps even their expectations about the results of such efforts—may have been inappropriate for dealing with the problems at hand. Analysts have lost their perspective, perhaps as a consequence of the abandonment of grand theory, their efforts to develop middle-range formulations, or their adoption of the even narrower hypothesis-testing procedures that were fostered by the empirical “third wave” of the 1960s and 1970s.

We have offered a number of specific recommendations about what should be done. Two remedies should be reemphasized. First, scholars should endeavor to merge the rigor and systematization of scientific inquiry with the broader, grand theoretical conceptualizations of the traditional literature in which substitutability and domain-specific laws were at least implicitly recognized. Second, analysts should begin to pay more careful attention to basic epistemological issues.<sup>24</sup> Nothing we have said casts doubt on existing research practices or suggests they should be abandoned. Our point is only that those practices, drawn as they are from other disciplines that are concerned with other types of problems, should be utilized with circumspection. Even though scholars are often impatient with epistemological issues and find themselves anxious to get on with their analyses, they should bear in mind that how they approach their problems—the manner in which they conceptualize them and the

<sup>23</sup> Rosenau, ed., *In Search of Global Patterns* (New York: The Free Press, 1973), Hopmann and others (fn. 11), and Zinnes (fn. 10). Scholars such as Singer and Russett have argued that analysts' collective research efforts are beginning to add up: J. David Singer (fn. 1), and Singer, “Tribal Sins on the QIP Reservation,” in Rosenau (above), 167-73, and Bruce M. Russett, “International Interactions and International Processes: The State of the Discipline” (Paper presented at the Annual Meetings of the American Political Science Association, Denver, 1982).

<sup>24</sup> A similar point has been made recently in Bueno de Mesquita (fn. 17) and “Theories of International Conflict: An Analysis and an Appraisal,” in Ted Robert Gurr, ed., *Handbook of Political Conflict* (New York: The Free Press, 1980), 361-98.

methods they utilize in their attempts to solve them—will ultimately impinge on their results. If apparently distinct foreign policies could be responses to similar problems as a result of substitutability, and similar foreign policies could be responses to different problems as a result of the operation of domain-specific laws, then we may need to rethink the accepted procedures.

*Microlevel decisional analysis and process.* We have tried to do more than issue yet another call for theory, for more fruitful merging of tradition and science, or for greater attention to the implications of our methods. As in our other work, we have suggested here that a particular type of theoretical development might be especially useful. Following Theodore Abel, and using the work of the Sprouts as a starting point, both “opportunity” and “willingness” are posited as fundamental considerations.<sup>25</sup> Each state’s objective opportunities circumscribe the range of possible acts and behaviors available to its decision makers. In the terms of Russett and Starr, the opportunities of states define their “menus for choice.”<sup>26</sup> Because the factors that create or preclude different options are differentially distributed across states and even within states through time, such concerns are in some sense likely to be important for understanding their policy undertakings. A focus on such attributes could provide insights into the basic parameters or limits of the foreign policies of different countries.

To the extent that analysts are less interested in the parameters within which states operate and are ultimately more concerned with understanding why certain options are adopted at particular times, however, our contention is that a focus on the attributes of states will not suffice. In this, we agree with Waltz that

... different states have produced similar as well as different outcomes, and similar states have produced different as well as similar outcomes. The same causes sometimes lead to different effects, and the same effects sometimes follow from different causes.<sup>27</sup>

Expressed in our terms, it appears that a focus on attributes could tell us what states can do, and therefore possibly what the aggregate probabilities are that certain types of events will occur in a given group or sample of states. A focus on attributes cannot tell us, however, which particular states will do what, or when they will do it. Certain objective

<sup>25</sup> Abel, “The Element of Decision in the Pattern of War,” *American Sociological Review* 6 (December 1941), 853-59; Sprout and Sprout (fn. 13).

<sup>26</sup> Bruce Russett and Harvey Starr, *World Politics: The Menu For Choice* (San Francisco: W. H. Freeman, 1981).

<sup>27</sup> Waltz (fn. 1), 37.



capabilities may be *necessary* for a foreign policy initiation, but they should not be expected to be *sufficient* for that undertaking—unless, that is, decision makers are homogeneously willing to avail themselves of all their opportunities. The problem is that it does not seem reasonable to expect all foreign policy makers in all states to pursue identical goals.<sup>28</sup>

It is the recognition of this problem that leads Waltz to reject “reductionist” or “analytic” theories focused on the individual, decisional, or national levels of analysis, and to argue instead for a systemic approach.<sup>29</sup> While we agree with Waltz and other systems-level theorists on the problem, and also conclude that static analyses of the attributes of states will not bring progress, our own solution is quite different. We argue that, while the objective capabilities of states may define the range of possibilities, progress can be made by understanding the *processes* by which decision makers choose certain options over others. This suggests that we might usefully follow Bueno de Mesquita’s expected utility approach<sup>30</sup> and study how decision makers select from among varying sets of available, potentially substitutable alternatives, paying particular attention to the cognitive or perceptual processes that we couch in terms of “willingness,” and that the Sprouts cast in terms of “cognitive behaviorism.”<sup>31</sup> Put differently, we need to consider how the willingness and opportunities of states interact to produce foreign policy behaviors.

<sup>28</sup> Singer, himself a long-time proponent of the systemic level approach and the view that foreign policy decision makers may at least initially be presumed to be rather homogeneous, comes close to this point in reviewing the realpolitik, arms race, power transition, economic development, and imperialism models:

... at rock bottom the most important difference amongst the contending causes of war models is that of the foreign policy decision process. That is, each model assumes—often implicitly—a different class of decision makers in power and each postulates a different set of decision rules. ... Note that we have assumed, to this juncture, a high degree of homogeneity in decision makers and the rules they employ, but *to move closer to a full explanation, that assumption would have to be relaxed* (Singer [fn. 1], 14-15, emphasis added).

See also J. David Singer, “The Level-of-Analysis Problem in International Relations,” in Rosenau (fn. 13), 20-29.

<sup>29</sup> Waltz (fn. 1), esp. pp. 37 and 65.

<sup>30</sup> Bueno de Mesquita (fn. 17).

<sup>31</sup> One critique of Waltz (fn. 1) is that he has underestimated the cognitive links between social entities and their environments. The environment—the international system—is important. Types of international systems could (but need not) be conceptualized as operating to define domains such as the *A* and *-A* contexts discussed in our example in the previous section. Nevertheless, Waltz artificially separates the “entity” and the “environment,” using the Sprouts’ terms for the “ecological triad” (Sprout and Sprout, fn. 13). He seems to ignore the third leg of that triad, the entity-environment relationship, which *must* have a cognitive component. The “enduring anarchic character of international politics” (fn. 1, p. 66), which he regards as the central factor in the analysis, *cannot* explain the variance in behavior.