RATIONAL DETERRENCE THEORY AND COMPARATIVE CASE STUDIES

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INTRODUCTION

RATIONAL deterrence is a highly influential social science theory. Not only has it dominated postwar academic thinking on strategic affairs, but it has provided the intellectual framework of Western military policy in the same period as well. The theory's success derives largely from its clearheaded logic, which is as persuasive as it is elegant.

Yet rational deterrence theory has been sharply criticized by an impressive array of case-study analysts.[1] Examining historical instances of

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EDITORIAL NOTE: The essay by Christopher H. Achen and Duncan Snidal on "Rational Deterrence and Comparative Case Studies" raises fundamental questions of theory and methodology with implications that extend well beyond security affairs. The Editorial Committee invited Alexander George and Richard Smoke, Robert Jervis, and Richard Ned Lebow and Janice Gross Stein to respond to the essay, and then asked George Downs to write a commentary on the rational deterrence debate as a whole. The Editorial Committee would like to express appreciation to all of these authors for contributing to this exchange.

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[1] Alexander L. George and Richard Smoke, *Deterrence in American Foreign Policy* (New York: Columbia University Press, 1974); Robert Jervis, *The Logic of Images in International Relations* (Princeton: Princeton University Press, 1970); Jervis, *Perception and Misperception in International Politics* (Princeton: Princeton University Press, 1976); Richard Ned Lebow, *Between Peace and War: The Nature of International Crisis* (Baltimore: The Johns Hopkins University Press, 1981), and "Windows of Opportunity: Do States Jump through Them?" *International Security* 9 (Summer 1984), 147-86; Glenn H. Snyder and Paul Diesing, *Conflict among Nations: Bargaining, Decision Making, and System Structure in International Crises* (Princeton: Princeton University Press, 1977); John D. Steinbruner, *The Cybernetic Theory of Decision*

deterrence and deterrence failure, they conclude that the theory fails both descriptively and prescriptively. As George and Smoke put it, "the contemporary abstract, deductivistic theory of deterrence is inadequate for policy application," since actual cases exhibit "complexities which in many respects are not addressed by the abstract theory of deterrence."[2] A more extreme critic asserts that the theory is completely nonpredictive: "We found that challenges of commitments were largely independent of whether or not those commitments appeared to satisfy our four conditions for successful deterrence";[3] moreover, "deterrence is inadequate as an explanatory theory of international politics because [of] the growing body of empirical evidence. . . ."[4]

The current state of deterrence studies is therefore rather puzzling. On the one hand, the theory is widely regarded as logically compelling. On the other, the most substantial body of empirical evidence leads to the conclusion that it is seriously deficient. The apparent contradiction between logic and evidence provides the starting point for this paper.

The issues raised have broad relevance for social science generally, since the deterrence literature is in many respects a model of cooperation among analysts of different theoretical perspectives. Devotees of comparative case studies have read the rational deterrence theorists with some care. Deterrence researchers who use statistical methods often cite both the historical-case researchers and the abstract theorists; in turn, both groups sometimes cite the quantitative studies. The intellectual interconnections are imperfect, but in most respects the deterrence field is a single literature.

Thus disagreements about deterrence theory raise not just the usual

(Princeton University Press, 1974). Also see the following chapters in Robert Jervis, Richard Ned Lebow, and Janice Gross Stein, eds., *Psychology and Deterrence* (Baltimore: The Johns Hopkins University Press, 1985): Jervis, "Introduction and Assumptions," 1-12, and "Perceiving and Coping with Threat," 13-33; Stein, "Calculation, Miscalculation, and Conventional Deterrence I: The View from Cairo," 34-59, and "Calculation, Miscalculation, and Conventional Deterrence II: The View from Jerusalem," 60-88.

Many other excellent recent case studies in strategic affairs are excluded from this list, either because they do not deal directly with the theory of rational deterrence (e.g., Barry R. Posen, *The Source of Military Doctrine* [Ithaca, NY: Cornell University Press, 1984]; Richard K. Betts, *Nuclear Blackmail and Nuclear Balance* [Washington, DC: Brookings, 1987]; Stephen M. Walt, *The Origins of Alliances* [Ithaca, NY: Cornell University Press, 1987]), or because they do not criticize it (John Mearsheimer, *Conventional Deterrence* [Ithaca, NY: Cornell University Press, 1983]). In addition, we pay relatively little attention to two other important discussions: Snyder and Diesing (fn. 1), which has been discussed elsewhere (see Duncan Snidal, "The Game *Theory* of International Politics," *World Politics* 38 [October 1985], 25-57); and Richard Ned Lebow and Janice Gross Stein, "Beyond Deterrence—Building Better Theory," *Journal of Social Issues* 43 (1987), 5-71, which appeared after this article was written.

[2] George and Smoke (fn. 1), 503.

[3] Lebow, *Between Peace and War* (fn. 1), 274.

[4] Richard Ned Lebow, "Conclusions," in Jervis, Lebow, and Stein (fn. 1), 203.

suspicions of too little reading by too many scholars, but also important questions about the possibilities and limits of alternative modes of research. It is this latter set of topics that we wish to address. Why have different methodological traditions in deterrence research come to opposing conclusions? On which topics can each school be trusted? More generally, what is the appropriate allocation of tasks in building deterrence theory and understanding?

Obviously, the implications of what we say are not confined to international studies. Although we know of no other area of political science in which the conundrums are posed so sharply, the differences among subfields derive from stages of intellectual development and accidents of intellectual history rather than from inherent features of deterrence. Thus, although our subject matter is deterrence, we believe that our remarks have wide relevance to the study of politics generally.

Within the deterrence field, our primary attention will be focused on comparative case studies and their limitations rather than on the corresponding and at least equally impressive limits of analytic theorizing or statistical analysis, including our own. Our choice is motivated not by the weakness of the historically oriented part of the field, but by its very strength. The comparative study of history has been among the most exciting developments in deterrence thinking during the past 15 years. The sheer number of these studies, along with the many discussions of them in the professional journals, testifies to their intellectual stature. While the analytic literature is only just arising from its quarter-century of slumber, and many statistical investigations continue to struggle for genuine theoretical impact, studies of the two World Wars, Korea, Vietnam, and the Falklands, along with crises such as Berlin, Quemoy and Matsu, Lebanon, and Cuba have had a powerful effect on how political scientists think about deterrence. Much of what the profession knows about deterrence derives from these cases. For precisely that reason, however, it is important to be clear about what exactly has been learned.

In this paper, we examine the claims of the case-study critics of rational deterrence. To preview our argument, we believe that these studies have been enormously valuable for what they contribute—historical wisdom about the limits of current theory, and empirical generalization to be explained by future theory. But case studies have failed when used for two tasks for which they are not suited—theory construction and theory verification. The failures derive not from peculiarities of the deterrence problem, but from the nature of the methods. The logic of comparative case studies inherently provides too little logical constraint to generate dependable theory and too little inferential constraint to permit trustworthy theory testing. Only when yoked closely to deductive theory and to statistical inference, and made to serve their ends, can case studies provide genuine theoretical contributions.

We begin our argument by summarizing the logic that supports comparative case studies and by reviewing their positive contribution to deterrence thinking.

The Theoretical Logic of Comparative Case Studies

Adherents of the case-study school argue that theoretical development through historical generalization provides an antidote to the ahistorical and overly abstract theory of rational deterrence.[5] History yields rich insights into nuance and context that escape the simpler rational models. Case studies are therefore essential if the understanding of deterrence is to be grounded in experience and not just in abstract analysis. The ultimate goal is "an explanatory theory of deterrence that is empirically rather than deductively derived."[6]

The logic of using multiple case studies emerged from the limitations of single cases. Analysis of individual deterrence situations, no matter how skillfully executed, does not necessarily "distinguish between what is unique to the case and what is common to the class of events as a whole."[7] Even though single case studies provide interesting insights, they do not *by themselves* provide clear guidance for generalization to other cases. Cumulation is further frustrated by the typical lack of a common theoretical focus, without which meaningful comparisons are difficult. This dissatisfaction with individual case studies, coupled with a lack of faith in the abstractions of rational theory, creates the need for an alternative approach.[8]

[5] The charge is not that deterrence theory is devoid of any appreciation of empirical materials; the major works (e.g., Thomas C. Schelling, *Arms and Influence* [Cambridge: Harvard University Press, 1966]) are replete with examples from across the millennia. Instead, the concern is with the well-known dangers of facile rational reconstruction of events. Intensive case studies—not attempted by scholars such as Schelling—provide a check.

[6] Stein, "The View from Cairo" (fn. 1), 34; see also Alexander L. George, "Case Studies and Theory Development: The Method of Structured, Focused Comparison," in Paul Gordon Lauren, ed., *Diplomacy: New Approaches in History, Theory and Policy* (New York: Free Press, 1979), 48.

[7] Lebow, *Between Peace and War* (fn. 1), 6.

[8] This sort of transition in thinking is hardly unique to deterrence analyses. It parallels the general shift in the understanding of the methodology of comparative analysis throughout political science. For example, George's discussion of the problem (fn. 6) acknowledges significant debts to Harry Eckstein, "Case Study and Theory in Political Science," in Fred I. Greenstein and Nelson W. Polsby, eds., *Handbook of Political Science* (Reading, MA: Addison-Wesley, 1975), Vol. 7, pp. 79-138; also see Arend Lijphart, "Comparative Politics and the Comparative Method," *American Political Science Review* 65 (September 1971), 682-93, and

By far the most sophisticated methodological treatment of comparative case studies has been done by Alexander George and his coworkers.[9] They recommend the development of *contingent empirical generalizations*—contingent because they apply only under certain (specified) conditions, and empirical because they are derived from analyses of multiple historical cases. These generalizations are produced by the method of "structured-focused comparisons,"[10] which consists of analyzing different cases in terms of common theoretical concepts while simultaneously placing their diversity in a theoretical perspective. Because the cases are tied together conceptually, the generalizations produced are expected to cumulate and become the basic elements of further theory building. Thus, the goal of general explanatory theory is replaced by the goal of segmental theory, which seeks to identify distinct causal patterns and the conditions under which they occur. In this manner, the multiple case-study approach aims to retain the richness of individual case studies without retreating into ideographic explanation, while simultaneously achieving theoretical generality without becoming lost in abstractions.

What George and Smoke are calling for, then, is a sophisticated version of "middle-range theory," inductively derived if-then propositions that are neither purely descriptive nor derived from more general propositions about human behavior. To be consistent with usage in psychology, economics, and the natural sciences, a more accurate and equally honorable title for the kind of generalization George and Smoke seek would be "law," as in Fechner's Law, Say's Law, or Boyle's Law. But, since social science generalizations of any sort so rarely approach the status of laws, we shall refer to them as "empirical generalizations"; we reserve the word "theory" for its more conventional usage in adjacent social sciences—a very general set of propositions from which others, including "laws," are derived. (For example, the empirical generalization that markets clear in capitalist societies can be derived logically from the propositions of neoclassical microeconomic theory.) The reader who wishes to call empirical generalizations "theory," while giving our version of theory a less honorific title, is free to do so. The name will not affect our conclusions.

Lijphart, "The Comparable-Case Strategy in Comparative Research," *Comparative Political Science* 8 (July 1975), 158-77.

[9] George and Smoke (fn. 1), chaps. 4 and 16; George (fn. 6); and George, "Case Studies and Theory Development," presented to the Second Annual Symposium on Information Processing in Organizations, Carnegie-Mellon University, Pittsburgh, October 15-16, 1982; Alexander L. George and Timothy J. McKeown, "Case Studies and Theories of Organizational Decision Making," *Advances in Informational Processing in Organizations* 2 (1985), 21-58.

[10] George (fn. 6).

Comparative Case Studies and Deterrence

The case-study community has produced detailed case studies of virtually every major international crisis since the late 1890s. The findings have enriched not just deterrence theory but also a wide range of related pursuits, including the study of military coercion, escalation, and war initiation.

These studies are united by misgivings over the rational theory of deterrence, but divided over what should replace it. In one popular approach, deterrence is seen as a fundamentally psychological process, in which cognition failures, fear, or simple time pressures disrupt the rational calculations assumed by deterrence theory. Different authors emphasize different routes to the failure of rational calculations. The first possibility is that decision makers are simply not able to carry out the calculations required by the theory. That is, elites can grasp the costs and benefits involved, they have a sense of the relevant probabilities, and they may be able to perceive the corresponding values for their opponents with reasonable clarity. But because of tension, fear, fatigue, or other thought-inhibiting forces, they cannot combine these elements in the manner mandated by expected-utility theory.

This view of deterrence failures has been taken by several authors, including Janis[11] Lebow[12] and others, but perhaps most systematically and theoretically by Steinbruner.[13] Working from the vast literature on human cognitive limitations, Steinbruner argues that the rational model of human decision making fails both historical and laboratory tests, and must be replaced by something else. He maintains that "real decision makers can achieve only the faintest approximation of the [rational] theory's requirements, and it is not likely that they do even that without unusual (and hence infrequent) effort."[14] His proposed substitute is "cybernetic decisionmaking," in which people avoid all consideration of long lists of alternatives and potential payoffs. Instead, they use feedback from the environment to adjust their behavior marginally, in the same way that a thermostat can keep a house warm without understanding the chemistry or engineering principles of the furnace.

Another psychological factor inducing the cognitive failure of rational deterrence is misperception. Here the emphasis falls, not on the difficulties of computation, but on the assessment of costs, benefits, and their as-

[11] Irving L. Janis, *Victims of Groupthink* (Boston: Houghton Mifflin, 1972).

[12] Lebow, *Between Peace and War* (fn. 1).

[13] Steinbruner (fn. 1), and Steinbruner, "Beyond Rational Deterrence: The Struggle for New Conceptions," in Klaus Knorr, ed., *Power, Strategy, and Security* (Princeton: Princeton University Press), 103-25.

[14] *Ibid.*, 115.

sociated probabilities. After all, the complexities of deterrence calculations are often unforbidding: if a country knows that it is likely to lose a long, nasty war in the process, it will probably not seek to press its claims against a rival. The trick is to learn the likelihood that the rival country will fight—and if it fights, how likely it is to win.

Robert Jervis has often expressed the view that misperceptions occur routinely in international affairs:[15] "It is hard to find cases of even mild international conflict in which both sides fully grasp the other's views."[16] Although Jervis never quite says that rational deterrence theory fails due to omnipresent misperceptions, the thrust of his work is such that the theory becomes suspect. In practice, Jervis argues, learning enough about the opponent's intentions to make the requisite calculations will be no easy matter. He proposes a variety of cognitive consistency and other psychological models as alternatives.

Psychological approaches constitute the first school of thought about deterrence failures. Although they use case studies, none of the authors mentioned above employ intensive comparative case studies to make their case. To find the method fully applied, we turn to a second school of critics of rational deterrence, who make quite a different argument. They maintain that, although decision makers may respond to those forces modeled by rational-deterrence theory, they are subject to so many other disturbing factors that the theory will not account for much of what happens. Analysts of this school are less interested in pointing to any one type of mental mistake (misperceptions, missed opportunities, bureaucratic bungling) than to the overall predictive power of the theory. Their implicit criterion is a measure of explanatory fit: if one takes rational deterrence as the forecast—namely, that sufficiently strong, clear, credible threats will deter—how often does the theory predict accurately?

This school of thought is capably represented by George and Smoke,[17] and Lebow[18] makes a strong case as well. They point to historical examples in which the threat of retaliation was clear and credible, yet deterrence failed. Thus, George and Smoke report that in

> all three Berlin cases, the Korean War and the Cuban missile crisis, American policy-makers were surprised by the action the opponent took. In each case American officials had thought the opponent would not act as he did because such action would entail high risks. . . . It is evident that to make the diagnoses needed in assessing situations, the policy-maker cannot work

[15] Jervis, *Perception and Misperception* (fn. 1); Jervis, "Deterrence and Perception," *International Security* 7 (Winter 1982-83), reprinted in Steven Miller, ed., *Strategy and Nuclear Deterrence* (Princeton: Princeton University Press, 1984), 57-84; Jervis, "Introduction and Assumptions," and "Perceiving and Coping with Threat" (both fn. 1).

[16] Jervis, "Deterrence and Perception" (fn. 15), 58.

[17] George and Smoke (fn. 1).

[18] Lebow, *Between Peace and War* (fn. 1).

on the assumption that all actors operate with the same kind of "rationality."[19]

Rather than take issue with the psychological predictions of deterrence theory, as do Steinbruner and Jervis, these authors put more emphasis on its failure to track crisis outcomes.

All these critics of deterrence theory have their disagreements, of course, and one might be forgiven for thinking that they share only an aversion to rational deterrence theorizing. In our view, however, a more important commonality is their belief that explanation consists of giving an account that matches the historical record of particular cases. In contrast to most rational-deterrence discussions (where the cases used are merely illustrative, if they are mentioned at all), each of the authors mentioned above discusses historical cases at some length, and most of them exhibit detailed case studies, usually more than one, from which their lessons are derived. "Theory" here is empirical generalization, not derivation from general assumptions about how people act; the criterion of excellence is a close fit to historical cases, not analytic power or theoretical surprise.

The Theory of Rational Deterrence

Before going on, we want to reacquaint the reader with the bête noire under discussion—rational deterrence theory. A great many deductive arguments are grouped here. However, the rational deterrence literature is unified by a number of working assumptions about human behavior. Together they constitute, not so much a set of beliefs as an explanatory framework—a set of choices about what will be explained and how.

The first two postulates are common to all rational choice explanations; the last is specific to deterrence theory.

1. *Rational actor assumption*. Actors have exogenously given preferences and choice options, and they seek to optimize preferences in light of other actors' preferences and options. (For example, cases of psychopathological decision making are set aside as unsuitable for the rational actor approach.)
2. *Principal explanatory assumption*. Variation in outcomes is to be explained by differences in actors' opportunities. (Appeals to exogenous changes in preferences, or to norms, roles, or culture, are temporarily or analytically suspended.)
3. *Principal substantive assumption*. The state acts as if it were a unitary rational actor. (Changes in personnel, in decision-making patterns, or in bureaucratic politics are not the explanatory focus.)

[19] George and Smoke (fn. 1), 505.

Many different models of deterrence have been constructed using these assumptions. What all these models have in common is a concern with what we shall call *the fundamental deterrence problem*—the use of threats to induce the opponent to behave in desirable ways.[20] The concept is as old as realist theory itself. Indeed, at this level of abstraction, neither the nature of the weaponry nor the stakes make much difference in the logic. However, the challenges of postwar American foreign policy forced analysts to clarify the model and strengthen its analytical rigor. Initial interests focused on thermonuclear threats to the superpowers' homelands: Type I or basic deterrence.[21] Later work addressed Type II or extended deterrence[22] and conventional threats.[23] Although the distinctions among these situations are life-and-death matters for policy and refer to completely distinct historical cases, they generate only minor differences in the theory: the fundamental problem of deterrence is central to each of them.

In the simplest version of rational deterrence theory, there are two rational actors, the initiator and the defender. The defender seeks to prevent some action by the initiator. (For concreteness, we will assume that it is an attack on the defender or on a third party.) The initiator moves first, either attacking or not. Then the defender chooses whether to engage in war or to capitulate. All this is common knowledge between the two players. In the politically most relevant version, however, what is not known to the initiator with certainty is the defender's ability and commitment to fight back after the attack.

In accord with the conventional theory of extensive form games, analysis proceeds by working backward from the end of the sequence. Suppose, first, that the defender's threat to retaliate is *credible*. That is, the initiator believes it likely that the option to retaliate actually exists (the defender has the military means to retaliate after an attack and is politically free to do so), and that the defender would find retaliation in his interest if the prize is threatened. Then, if the initiator is deterrable (the threatened punishment exceeds his gains from attacking), he will see that an attack will make him worse off than restraint. Hence the initiator will not attack.

[20] An excellent overview is Patrick M. Morgan, *Deterrence: A Conceptual Analysis*, 2d ed. (Beverly Hills, CA: Sage, 1983).

[21] Albert Wohlstetter, "The Delicate Balance of Terror," *Foreign Affairs* 37 (January 1959), 209-34; Glenn H. Snyder, *Deterrence and Defense* (Princeton: Princeton University Press, 1961); Daniel Ellsberg, "The Crude Analysis of Strategic Choice," *The American Economic Review* 51 (May 1961), 472-78.

[22] For example, Bruce Russett, "The Calculus of Deterrence," *Journal of Conflict Resolution* 7 (June 1963), 97-109; Schelling (fn. 5), chap. 2.

[23] For example, Mearsheimer (fn. 1).

Conversely, if a deterrable initiator believes that it would very likely not be in the defender's interest to retaliate, or that the defender lacks the means or the will to do so, the initiator will attack. Thus, under conventional rational-choice assumptions, when the attacker is deterrable, successful deterrence turns on the defender's credibility. If the latter can convince the attacker that he has the political and military ability to fight, and that the prize (or his reputation for fighting when challenged) is worth more to him than the cost of the war, then, and only then, will a deterrable initiator be deterred.[24]

This simple model is less brittle than sometimes thought. Its propositions are contingent: if the expected punishment exceeds the gain, then opponents will be deterred. Thus, for example, the model implies that some conceivable punishment would deter, but not that any particular one will, nor even any feasible one. Put more strongly, the model implies that deterrence will fail for sufficiently determined attackers. Not all conceivable opponents are deterrable.[25]

Critics of rational deterrence sometimes write as if failures of *deterrence* were equivalent to failures of *deterrence theory*. Levy, for example, notes, "this is an ad hoc hypothesis which cannot technically be derived from any formal theory of deterrence."[26] The truth is that the theory actually predicts some breakdowns. When deterrence fails because the retaliatory threat is absent, incredible, or less valuable than the prize, the theory has forecast perfectly. Rational deterrence theory implies that deterrence will not always be successful.

Rational deterrence is very much an ideal-type explanation. No sensible person pretends that it summarizes typical deterrence decision making well, or that it exhausts what is to be said about any one historical case. Yet it has dominated discussion in all traditions of deterrence research,

[24] More precisely, the initiator's subjective expected utility of attacking must be less than that of continuing the status quo. Among other factors, his expectations depend upon his subjective estimates of the expected costs of war, the probability of winning, and the estimated probability that the defender will retaliate. The latter, in turn, is a function of the initiator's subjective prior distribution over the defender's utilities in a game of incomplete information.

Analysts continue to struggle painfully for a fully satisfactory version of this game; as Harrison Wagner has remarked to us, "the rational theory of deterrence" doesn't exist. (Morgan [fn. 20, chap. 4], gives a clear exposition of the principal difficulty in the theory.) Hence the grim looks from game theorists when international relations scholars remark that there is no work left to do on rational deterrence. But just as one can believe Newton's laws without waiting for physicists to fully comprehend the nature of gravitation, so also the principal conclusions of a legitimate theory of deterrence are foreseeable even if the supporting arguments are at present incomplete; it is the former that we call "rational deterrence theory."

[25] Schelling (fn. 5), 85-86.

[26] Jack Levy, "Quantitative Studies of Deterrence Success and Failure," paper prepared for the meeting of the American Political Science Association, Chicago, IL, September, 1987, p. 2.

including historical investigations. For example, George and Smoke criticize rational deterrence for its assumption that a government is a rational actor and can be treated as if it were a single person.[27] Yet the same authors summarize their case-study evidence as follows:

> In almost every historical case examined, we found evidence that the initiator tried to satisfy himself before acting that the risks of the particular option he chose could be calculated and, perhaps more importantly, controlled by him so as to give his choice of action the character of a rationally calculated, acceptable risk.[28]

The rational unitary-actor model is not easily evaded.

The power of rational deterrence theory is conceptual, not mathematical. It derives from the underlying logical cohesion and consistency with a set of simple first principles, not from the particular language in which it is expressed. In consequence, the model has been astonishingly fecund, both for theory and for policy. Its surprising implications, now familiar from the literature, include "the rationality of irrationality," the dangers of total disarmament, and the value of aiming for strategic equivalence between the superpowers.[29] Perhaps most importantly, it was rational deterrence theory that sensitized policy makers to the negative aspect of defensive systems such as civil defense, the ABM, and SDI, which make first strikes less dangerous. The point is strongly counterintuitive; indeed, Aleksey Kosygin told Lyndon Johnson at Glassboro that he didn't understand it.[30] But this surprising conclusion is a clear implication of rational deterrence theory.

Contrary to George and Smoke's view, rational deterrence theory has proved itself in practical policy applications. No other theoretical perspective has had nearly the impact on American foreign policy, certainly not the conclusions of the case-study literature. Far from being an abstract, deductivistic theory developed in a policy vacuum, rational deterrence theory has repeatedly taken inspiration from the most pressing policy questions of the day, from decisions of bomber-basing in the 1950s to SDI in the 1980s. It has set the terms of the debate, and has often influenced the outcome. One may choose to applaud or decry its impact, but one cannot deny that the theory of rational deterrence, like any good theory, has been of immense practical importance.

[27] George and Smoke (fn. 1), 72-77, 505.
[28] *Ibid.*, 527.
[29] Many of these appeared first in the work of Thomas Schelling, notably *The Strategy of Conflict* (Cambridge: Harvard University Press, 1960), and *Arms and Influence* (fn. 5).
[30] Henry Kissinger, *White House Years* (Boston: Little, Brown, 1979), 208.

Comparative Case Studies and the Quest for Theory

We now must contrast the empirical generalizations of the multiple case-study approach with those of rational deterrence theory. As noted above, the case-study method at its best uses side-by-side comparisons of different deterrence crises to produce contingent empirical generalizations. These generalizations are viewed by some as constituting theory,[31] and by others as potential substitutes for the rationality postulates of deterrence theory.[32] Still others, arguing that abstract theory is infeasible, substitute a long set of possible variables and call the result a "paradigm."[33] In any case, the generalizations are viewed as filling the empirical void opened by rational deterrence theory.

What are the new contingent generalizations or empirically grounded theories that case studies have produced? Examples are not as easily found as one might imagine. Consider, for instance, Lebow's analysis of certain "acute" international crises from 1897 to 1967.[34] He groups them into three categories. The first crisis category is that of "justification of hostility": the initiating state, having already decided that war is in its interest, creates a crisis to provide a casus belli and to mobilize support for the war. The second category includes nine "spinoff" crises that lead to a war secondary to some other conflict. Lebow does not analyze either of these first two categories of crisis in great depth. We would observe only that *all* the outcomes in these categories correspond exactly to rational deterrence theory. War occurred because it was "rational" for one or both states.

The third and final category is that of "brinkmanship" crises; war occurs sooner or later after most of them. Lebow mentions 24 factors that influence the outcome of such crises.[35] He does no full-fledged "process tracing" of comparative historical sequences in the sense that George and Smoke do, but he reviews a great many crises in comparative perspective, and he draws attention to certain key dimensions—for example, domestic politics—which are not currently addressed by deterrence theory. The fact that domestic politics matters has important consequences not only for deterrence, but for realist international-relations theory more generally. Its ultimate implication would be the desirability of including domestic processes in all models of international politics. More immediately, Lebow's hypothesis reminds us that the characterization of states as unitary actors is an analytic assumption, not a truth.[36]

[31] George and Smoke (fn. 1).

[32] Jervis (fn. 1).

[33] Lebow, *Between Peace and War* (fn. 1), 304; see also "Conclusions" (fn. 4).

[34] Lebow, *Between Peace and War* (fn. 1).

[35] *Ibid.*, 304.

[36] Lebow's later work is suggestive of auxiliary questions that arise from these considera-

Lebow's work, like that of Steinbruner and Jervis, is typical of the case-study literature in that many of his findings are statements of the form "Factor X is a key feature of the international system," or "in the time period under study, Factor X was more important than Factor Y," where Factors X and Y are domestic forces, misperception, bureaucratic politics, and so on. Results of this kind are crucial to theory building: learning the important variables is often the most demanding task. Lebow therefore makes a genuine contribution when he reminds us of the domestic factor. Like the other variables he mentions, "it matters," and we need to remind ourselves that it does. Case studies can perform that function admirably.

Lists of variables, however, are not "theory," no matter how one defines that elusive concept. Not much successful theorizing from any methodological standpoint about the effects of domestic politics has been accomplished in international relations—simply because domestic factors add complications that are currently impossible to deal with. (The economists have done little better with the closely related topics of social choice and principal agency.) As a discipline, international relations is nowhere near understanding these aspects of deterrence in a coherent and theoretically rigorous manner. Reminding ourselves that we do not understand them is important, but does not itself solve the problem.

Instead of lists of variables, we must look for plausible empirical generalizations—well-specified causal sequences which can be found across a significant range of historical case studies, which are not already familiar from the long history of international relations thought, and which are not straightforward applications of ideas borrowed from other literatures. Again, we turn to George and Smoke.

In *Deterrence in American Foreign Policy*, they present a comprehensive historical analysis of recent American deterrence policy.[37] They are self-consciously concerned about methodology, and their approach is very close to that in George's "Case Studies and Theory Development" described above.[38] While they offer a wide range of useful insights on individual cases, their most systematic set of contingent generalizations concerns "patterns of deterrence failure." These are presented as a major reformulation of deterrence theory, derived from empirical case studies.

George and Smoke offer a typology of three main sequences for deterrence failure.[39] The first ("the fait accompli") is simply the absence of

tions. He proposes the "general principle . . . that policy makers . . . are probably inordinately influenced by immediate and predictable costs" based on analysis of how the short-term interests of politicians diverge from the long-term interests of states. (Richard Ned Lebow, "Miscalculation in the South Atlantic: The Origins of the Falklands War," in Jervis, Lebow, and Stein [fn. 1], 123-24.)

[37] George and Smoke (fn. 1).

[38] George (fn. 6).

[39] George and Smoke (fn. 1), chap. 18.

credibility in the deterrent threat. As noted earlier, this breakdown is a clear consequence of rational deterrence, and need not concern us here. The other two are considerably more interesting.

In the second pattern ("the limited probe"), the initiator employs a limited and relatively riskless challenge—for example, China's shelling of Quemoy in 1958. George and Smoke argue that the Chinese were simply interested in learning the definition and strength of the American commitment to Taiwan. By engaging in a low-level and essentially nondeterrable act, they not only learned a good deal about U.S. values and intentions, but also brought the matter forcibly to Washington's attention. In one sense, deterrence failed; in another, it succeeded, because Taiwan was not invaded.

The third pattern ("controlled pressure") may follow the second, or it may be generated independently. In either case, the initiator uses nonmilitary tactics that are not easily countered without military retaliation. Soviet pressure on Berlin in 1958 and in 1961 are examples. Its purpose, Smoke and George maintain, was to create pressure for a solution, divide the Allies and, if possible, erode the American commitment.

George and Smoke argue correctly that the deterrence failures of the second and third patterns are not those envisaged by the conventional rational deterrence theory. We would add that rational deterrence does not contradict these scenarios; it simply does not consider them. In conventional deterrence theory, crises have to do with competitive risk taking and the slippery slope of inadvertent war[40]—a very different process from the one discovered by George and Smoke. To our knowledge, crisis sequences like Quemoy and Berlin have received no attention at all from rational deterrence theorists. Yet they are clearly of major importance for policy, and for theory as well. The discovery of empirical generalizations like these is a considerable achievement, and a success that only comparative case studies are likely to achieve.

In the hands of George and Smoke, the case-study approach helps to generate theory in a very direct way. Indeed, generalizations like theirs are a necessary condition for building relevant theory. Yet we would insist that even in the hands of masters, empirical generalizations do not constitute theory—or at any rate, not the most useful kind of theory for social science. Even for policy purposes, their usefulness is limited. Case studies of crises can provide insights to policy makers, but those insights rarely transcend the particular issues for which they were developed. Unlike rational deterrence theory, scenarios for crises provide no general framework for thinking about the broad range of security issues such as

[40] See, for example, Schelling (fn. 5), chap. 3.

ABM treaties, SDI, or antisubmarine warfare. Empirical generalizations lack the universality that is the hallmark of good theory. A typology, no matter how novel and insightful, is not an interrelated set of propositions from which powerful and surprising consequences can be deduced.[41]

For theoretical purposes, the difficulty with explaining individual cases is that there are so many details in every case that no single theory can reproduce them all, and *some* evidence can be found for too wide an array of variables and propositions. Even if the goal is the explanation not of individual cases but of causal sequences, the dilemma remains. Since there is no requirement that behavior be logically consistent with any one explanatory framework, there are too many degrees of freedom for explanation. As George and Smoke note, typologies and causal sequences can be multiplied indefinitely, with the number found being partly a matter of how detailed the investigator wishes to be.[42] If explanations are not required to apply to all cases, we can specify contingent conditions to protect any favored generalization, so long as it can be plausibly supported in *some* cases. And the more we learn about each case, the more distinct it becomes. Even in the ideal case, nothing in this approach precludes having one causal pattern per case.

A deductive theory like rational deterrence reduces this problem by providing stricter criteria for admissible hypotheses. Only those that follow directly from the theory are to be considered. This imposes a requirement of logical consistency and interrelatedness among explanations which maintains parsimony and prevents the proliferation of ad hoc hypotheses. Moreover, the theory is universal rather than typological: if more than one causal process can occur (e.g., deterrence success *or* failure), the theory is expected to give conditions under which each applies.

Parsimony has a price, however: deductive power is usually purchased at the cost of historical accuracy. What most deeply separates scholars of the historical-comparative school from the deductive tradition (which in-

[41] The typological historical sequences in George and Smoke seem to us their most important contribution. However, the authors also give prominence to a list of more abstract propositions that derive from their work. Two typical examples:

> Proposition 3: The initiator's belief that the risks of his actions are calculable and that the unacceptable risks of it can be controlled and avoided is, with very few exceptions, a necessary (though not sufficient) condition for a decision to challenge deterrence, i.e., a deterrence failure (George and Smoke, fn. 1, 529).
>
> Proposition 6: Deterrence success will be favored but not ensured by the belief of the initiator that the defender possesses (a) an adequate and appropriate spectrum of capabilities; (b) sufficient motivation to employ them; and (c) probable freedom from impeding political constraints in the relevant time period (*ibid.*, 530-31).

It is not clear to us that propositions of this kind are a challenge, or even an addition, to unitary rational-actor models of deterrence.

[42] *Ibid.*, 535.

cludes approaches other than rational choice) is the question of trade-offs between analytic power and historical concreteness.[43] Should theory seek middle-range propositions closely related to individual cases? Or should it abstract further away from concrete instances to achieve deductive power? Put more precisely, should theory be applicable to historical instances, so that one or another causal pattern may be attached to each case? Or should it be a ceteris paribus explanation, accounting for an aspect of many cases, but not fully for any, and perhaps not at all for some?

This debate has a long history. In 1883, Carl Menger published the first edition of his *Investigations into the Methods of the Social Sciences with Special Reference to Economics*, an attack on the historical school of political economy.[44] Thus began the famous *Methodenstreit* in late nineteenth-century Austrian academia, pitting Menger against Gustav Schmoller and others. Menger argued for ideal-type rational-choice explanation, in which social facts were explained as consequences of individuals' choices. Schmoller preferred historical explanation at the level of the society, with an emphasis on the study of particular cases.

The dispute between rational-choice theorists of deterrence and their historically oriented counterparts essentially recapitulates the *Methodenstreit*. A debate of this sort is never settled (and perhaps never should be).[45] With respect to deterrence theory, however, the choice seems clear. Rational deterrence has not been seriously challenged theoretically by the comparative case-study school. As the case-study analysts themselves admit, they have not yet produced anything that approaches "integrated theory,"[46] and their analyses are best characterized as a "methodological potpourri,"[47] offering "an elaborate array of hypotheses."[48] These hy-

[43] *Ibid*., chaps. 3, 4.

[44] Menger, *Investigations into the Methods of the Social Sciences with Special Reference to Economics* (New York: New York University Press, 1985).

[45] Terry M. Moe, "On the Scientific Status of Rational Models," *American Journal of Political Science* 23 (February 1979), 215-43, for example, offers an interesting recent twist that might be profitably read for contrast with our position. He uses the traditional positivist covering-law model of explanation to attack a variety of bad arguments for rational choice models. The main issue, however, is whether rationality models use abstraction in the same way as any other scientific theory does. Moe is brief on this point, but he argues that natural science uses empirically based abstractions, while rational choice builds causal processes in by assumption. Unfortunately, he arrives at this conclusion by comparing rational choice theory, not to a natural science theory, but to an empirical generalization (Galileo's Law). Had he—more logically—compared the causal structure built into rational choice with the causal structure built into Newton's theory (which explains Galileo's Law), his conclusions would have been reversed.

Another approach to the role of formal theorizing, particularly in international relations, appears in Bruce Bueno de Mesquita, "Toward a Scientific Understanding of International Conflict: A Personal View," *International Studies Quarterly* 29 (June 1985), 121-36.

[46] Jervis, "Introduction and Assumptions" (fn. 1), 5; Lebow, *Between Peace and War* (fn. 1), 102.

[47] *Ibid*., 19.

[48] Lebow (fn. 4), 232.

potheses provide a better understanding of individual cases than does rational deterrence, along with useful advice for policy makers. At their best, they have also provided certain contextualized generalizations that may become a major step toward deductive theory building. But what George and Smoke call

> the significant aspects of theory—the precision of concepts, the formality and exactitude of their logical relationships, the degree to which the theory can be expressed in mathematical or quantitative terms, the degree of general consensus enjoyed by the major ideas and the relative comprehensiveness of these agreed-upon ideas, the stability of theory over time, and the level of confidence in the theory held generally and especially by policymakers[49]

—these are aspects of theory that have been absent from the case-study literature.

We are not complacent about the current state of rational deterrence theory. For instance, while George and Smoke regard signaling and commitment as the "best developed" part of rational deterrence theory,[50] we consider these elements to be woefully underconceptualized. Most rational deterrence theorists believe that Schelling's "threat that leaves something to chance" has yet to be given a coherent statement within rational-choice theory, and the same is true of NATO's doctrine of "flexible response."

More importantly, our goal here is to defend theory in general, not rational deterrence theory. Rationality postulates and unitary-actor assumptions are always debatable. Rational-choice models explain a good deal in some situations, and not much in others. We agree with the critics that, in spite of exciting recent developments in game theory, there is much to do before rational deterrence models begin to track certain key features of important historical crises, and a good deal of empirical work remains before the overall usefulness of rational deterrence in explaining actual policy decisions can be assessed. Ultimately, better theories based on more realistic assumptions about human decision making may be developed.[51] As we have argued, however, rational deterrence theory is considerably stronger than it has been portrayed in the case-study literature. In the absence of a well-developed theoretical alternative, it would be folly to abandon it for the patchwork of empirical findings currently available.

[49] George and Smoke (fn. 1), 45.

[50] *Ibid.*, 65.

[51] When such theories appear, however, they are not likely to emerge directly from the results of case studies. In our view, the notion—common to both case-study and behavioral traditions—that theory emerges from masses of facts and lower-level generalizations, is false both to the history of the natural and social sciences and to the concept of theory.

The Empirical Logic of Comparative Case Studies

We now turn to the attempt to disconfirm rational deterrence empirically by means of case studies. The logic of "testing" used in the multiple-case approach is again best described by George.[52] Its central element is the "plausibility probe," which assesses various hypotheses by detailed analyses of multiple cases. This approach is different from traditional statistical testing, but complementary to it.[53] The rich details of case studies allow the analyst to look at more than linear correlations between variables across an entire population, and to look for more complex processes in particular contexts. Finally, the use of multiple cases allows for comparisons of causes across contexts.

In practice, however, the empirical testing of rational deterrence by comparative case studies has been much less impressive than George's description would imply. These studies routinely violate standard principles of inference, and the resulting logical looseness makes it all too easy to draw conclusions in accord with the investigator's predilections.

Two of the inferential felonies in the case-study literature are of special importance. First, the selection of cases is systematically biased. Cases are chosen nonrandomly by criteria that make an evaluation of rational deterrence theory impossible. Second, case-study analysts have misinterpreted the propositions of rational deterrence as descriptions of decision makers' thought processes. The resulting "tests" are essentially useless as a judgment on the validity of rational deterrence theory.

To begin with, the nonrandom character of case selection is evident in Lebow as well as in George and Smoke. Both focus on crises which, in one sense or another, are already deterrence breakdowns. For example, Lebow treats 26 "acute" cases, of which all but 3 are either fait accompli crises, "spinoffs" from an ongoing war, immediate precursors to war, or rehearsals for a later crisis that did end in hostilities, such as Bosnia or Munich. Readers looking for happy endings must content themselves with Fashoda, Berlin (1948), and the Cuban Missile Crisis. George and

[52] George (fn. 6).

[53] George (*ibid.*), notes that Bruce Russett, "International Behavior Research: Case Studies and Cumulation," in Michael Haas and Howard Kariel, eds., *Approaches to the Study of Political Science* (San Francisco: Chandler Publishing, 1970), makes a parallel case from a quantitative perspective in favor of case studies for tracing causal patterns and for policy purposes. More recently, Charles Ragin, *The Comparative Method* (Berkeley: University of California Press, 1987) has made similar claims. He argues that, at least as used by most social scientists, purely statistical methods rarely turn up intriguing findings or hypotheses. Social forces are nonlinear ("contextual") and interactive ("holistic"). Hence they are blurred and averaged meaninglessly in "variable-oriented" approaches (or at least in the linear statistical models and casual data analysis in common use). He recommends case studies to find the detailed causal patterns in different contexts.

Smoke's sample of crises is not very different from Lebow's. Indeed, in hundreds of pages, the reader rarely encounters anything but deterrence failures. The cumulative impression is overwhelming, and the mind tends to succumb.

Let us consider, however, some of the "crises" and "wars" that go undiscussed in this literature. The first Soviet-American War which erupted over Hungary in 1956 is missing, as is the second one (over Chile) in the early 1970s. The U.S.-China War, which began when the United States bombed the North Vietnamese dikes is missing, as is the second Korean War. The point, of course, is that none of these "wars" occurred, or came anywhere near occurring. And deterrence is a likely cause of their prevention.

Dozens of such examples will occur to the reader. But, because the details of successful deterrence cases are rarely discussed in the historical literature (for good and obvious reasons), analysts who want to know how often deterrence fails *and how often it succeeds* can be badly misled by consulting only crises and wars. An example may perhaps make this point clear. Suppose that there are 100 countries in the world, each of them in a deterrence relationship with an average of 5 of the others. Thus, each year there are 250 war-prone dyads. In 40 years of observation, this system provides a total of 10,000 opportunities for war. Suppose that all the countries are deterrable, and that sufficiently strong, credible threats have been communicated in each case. In this rather special world, rational deterrence theory predicts no war at all.

Now suppose that the theory predicts correctly 99.5 percent of the time—meaning, of course, that it is a spectacularly good social science theory. In this period there will then be 9,950 instances where rational deterrence saved two nations from war. But there will also be 50 wars or crises. If the analyst chooses to write solely about the latter, the book that results will be full of nothing but deterrence failures. In short, *studies of crises and wars give no information about the success rate of rational deterrence.*

This problem has been recognized in the case-study literature, but its devastating implications have not been appreciated. Jervis recognizes the "error,"[54] as do George and Smoke;[55] but, in the absence of a solution to the problem, both continue with their analysis and the attendant pitfalls. The problem is most severe with Lebow who studies only "acute" crises, defined (retrospectively by him) as posing "a significant prospect of

[54] Jervis, "Perceiving and Coping with Threat" (fn. 1), 13.
[55] George and Smoke (fn. 1), 516-17.

war."[56] Needless to say, this further selection criterion compounds the bias and makes it even more difficult to interpret any "generalizations" that might result.[57] Only Russett and Huth, focused on sampling issues by the quantitative tradition in which they work, are alert to its specific consequences for their conclusions.[58]

There is nothing wrong with nonrandom samples so long as they are not treated as random. Indeed, there may be good reasons not to choose cases randomly. Thus Mearsheimer, a defender of rational deterrence, studies a dozen major cases, of which ten (83 percent) are deterrence failures.[59] One cannot thereby conclude, however, as does Huntington, that there exists an "83.3 percent failure rate for rational deterrence."[60] The failure rate represents the author's choice of cases, not a feature of the international system; and the 83 percent figure would hold no matter how often rational deterrence worked in the larger universe of deterrence situations.

Case studies of deterrence typically use an informal research design in which cases are chosen at least partially by their score on the dependent variable (deterrence success or failure). When the cases are then misused to estimate the success rate of deterrence, the design induces a "selection bias" of the sort familiar from policy-evaluation research.[61] The seriousness of the difficulty is readily apparent once the research design is specified in the conventional sampling framework. Without that discipline, the bias is not easily detected.

How might a proper sample be constructed to assess the effectiveness of deterrence? The answer, of course, is by random sampling from the appropriate population. But how should the population be defined? This fundamental question, which has received no discussion in the case-study literature, is critical to any discussion of rational deterrence theory. No one has given a fully satisfactory answer.

[56] Lebow, *Between Peace and War* (fn. 1), 11; see also Snyder and Diesing (fn. 1), 6.

[57] An excellent example of the difficulties of understanding the implications of different selection criteria is provided by the case of the 1958 Quemoy crisis, which Russett (fn. 23) treats as a deterrence success, George and Smoke (fn. 1) treat as a deterrence failure, and Lebow, *Between Peace and War* (fn. 1), 13, excludes entirely because he judges it insufficiently "acute."

[58] Paul Huth and Bruce Russett, "What Makes Deterrence Work? Cases from 1900 to 1980," *World Politics* 36 (July 1984), 496-526. Levy (fn. 26) has provided a broad discussion of this issue for the quantitative deterrence literature.

[59] Mearsheimer (fn. 1).

[60] Samuel P. Huntington, "Conventional Deterrence and Conventional Balance in Europe," *International Security* 8 (Winter 1984), 38.

[61] James Heckman, "Sample Selection Bias as a Specification Error," *Econometrica* 47 (January 1979), 153-61; G. S. Maddala, *Limited-Dependent and Qualitative Variables in Econometrics* (Cambridge: Cambridge University Press, 1983); Christopher H. Achen, *The Statistical Analysis of Quasi-Experiments* (Berkeley: University of California Press, 1986).

Presumably the relevant population for any time period consists of those states that have a "serious" dispute with another state, where "serious" is defined by the potential for war. There is no difficulty with this definition when deterrence fails and violence ensues: the dispute is *seen* to be serious. But what if no war breaks out? Have the Soviets stayed their hand from Western Europe because of NATO or because of satisfaction with the status quo? Pacific intentions are easily confused with successful deterrence, and vice versa.

The relevant population for deterrence, then, is not easily specified. One imperfect but useful approach is to employ a proxy, such as common borders, for the unobservable hostile intentions. Historical intuition would suggest that states with common borders are more likely to fight than states that do not have common borders.[62] The sampling population is then defined to be the set of all contiguous pairs of states. Creating a sample of all such pairs will include some irrelevant peaceful dyads with no disputes, but it will also include many relevant hostile pairs. When a number of such proxies are employed, the result is a sample that includes as a subset most of the relevant cases.

Procedures like these are frequently used in the quantitative literature to generate a suitable sample. The frequency of war is then computed for various types of deterrence relationships, as in Ferris and in Weede.[63] Both scholars find that rational deterrence has played a substantial part in preventing war. Thus, different conclusions about the effectiveness of deterrence emerge when samples are drawn scientifically rather than informally. Moreover, the formal procedures make clear where the remaining imperfections of these studies lie and how they might be reduced.[64]

Rational Deterrence and Decision Makers' Calculations

There is a second objection to the empirical testing of rational deterrence in comparative case studies—namely, that the verification proce-

[62] Harvey Starr and Benjamin A. Most, "A Return Journey: Richardson, 'Frontiers' and Wars in the 1946-1965 Era," *Journal of Conflict Resolution* 22 (September 1978), 441-67.

[63] Wayne Ferris, *The Power Capabilities of Nation-States* (Lexington, MA: Lexington Books, 1973); Erich Weede, "Extended Deterrence by Superpower Alliance," *Journal of Conflict Resolution* 27 (June 1983), 231-54.

[64] Study designs like those of Ferris and Weede lack textbook purity, but the clarity of their procedures makes it possible to assess the direction of remaining biases unambiguously. It is clear that both estimate the effect of rational deterrence conservatively. That is, the clutter of irrelevant, nonconflictual dyads in the samples underestimates the effectiveness of deterrence. For example, suppose, with Weede, that alliance with the same superpower is estimated to reduce the dyadic probability of war by 10%. Then suppose that half the nation-pairs in the sample are not really relevant—that is, they never had hostile intentions toward each other. Then the 10% difference derives only from the relevant half of the sample, making the true difference twice as large, namely 20%.

dure fails to engage the rational deterrence theory of how states behave. In particular, rational deterrence is implicitly misconstrued as a theory of how decision makers think.

Case study analysts have often expressed the opinion that, if decision makers do not really carry out the appropriate mental calculations, rational deterrence theory does not apply. This argument is the "descriptivist fallacy." As a glance at any appropriate text will show, the axioms and conclusions of utility theory refer only to choices. Mental calculations are never mentioned: the theory makes no reference to them. Indeed, a major reason for the various axiomatizations of expected-utility theory is to show that decision makers need not calculate. If they simply respond to incentives in certain natural ways, their behavior will be describable by utility functions.[65] Rapoport puts it this way:

> Now, in being asked to choose between the two [lottery] tickets, the man is not asked to calculate anything. He is asked simply to choose between the two. It is from his *choices* that the theoretician will construct the man's utility scale on which all the outcomes will be assigned numbers (utilities).[66]

To our knowledge, no one who does rational-choice theorizing disputes this point, but it seems not to be widely understood.

Rational deterrence theory does contain some minimal psychological content: for example, the initiator must realize that the defender exists and threatens to defend. But rational deterrence is agnostic about the actual calculations decision makers undertake. It holds that they will act as if they solved certain mathematical problems, whether or not they actually solve them. Just as Steffi Graf plays tennis as if she did rapid computations in Newtonian physics (and in game theory, too—at least against Navratilova), so rational deterrence theory predicts that decision makers will decide whether to go to war as if they did expected-utility calculations. But they need not actually perform them.[67]

To avoid misunderstandings, we want to point out that our point here has nothing to do with Friedman's famous argument that the truth of assumptions need not matter.[68] Friedman maintained that a theory might

[65] R. Duncan Luce and Howard Raiffa, *Games and Decisions* (New York: John Wiley, 1957), 31-32.

[66] Anatol Rapoport, *Two-Person Game Theory: The Essential Ideas* (Ann Arbor: University of Michigan Press, 1966), 34.

[67] We are ourselves divided over whether international decision makers often do carry out expected utility computations to a reasonable approximation, or whether rational choice theory will usually be hopelessly inept at describing their conscious mental processes. Our point is that either view is consistent with the theory so long as their *behavior* follows rationality axioms.

[68] Milton Friedman, "The Methodology of Positive Economics," in *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953).

sometimes work well for certain purposes, even though its assumptions were known to be *false*. We have our doubts; but whatever its merits, this line of reasoning has no bearing on our argument. Our point is that even if decision makers do not actually calculate, or if they rationalize their actions after the fact with foolish calculations, the assumptions of rational choice theory may yet remain *true*.

Understanding what rational deterrence demands of decision makers is important because it sharply limits what counts against the theory. A proper understanding of the theory eliminates most of the arguments in the historical deterrence literature that are supposed to overturn it. Consider, for example, the fact that many decision makers do not seem to think in terms of probabilities at all. This point is made by Steinbruner with respect to Kennedy's remark that he faced certain impeachment if he did nothing about the Cuban missiles;[69] Stein has made it in connection with Anwar Sadat's forecasts of various foreign policy consequences for Egypt in the early 1970s.[70] Both authors maintain that, because each decision maker failed to discuss probabilities in situations where rational deterrence theory implies he should, rational deterrence is inaccurate in describing what happened. Related arguments have been advanced by Jervis and by Betts.[71]

These conclusions do not follow. Let us repeat that rational deterrence theory deals with choices, not mental calculations. It makes no predictions about what decision makers *say* influenced them, only about what actually did so. The distinction is particularly important in the case of postdecision reconstructions. As diplomatic historians have long been aware, the historical record often differs sharply from decision makers' memories, even memories about their own thoughts at the time.[72] Since the critics of deterrence wish to adduce evidence that is not only dubious but apparently irrelevant to the theory under discussion, they bear the burden of proof. They must show that no plausible psychological mechanisms could have introduced a gap between the reality and the reconstruction, and they must demonstrate that what decision makers say influenced them is identical with what actually happened.

Neither Stein nor Steinbruner have made such a case. Indeed, several plausible psychological mechanisms would render their evidence irrelevant—mechanisms of the sort that they and others have often put forward as key factors in international decision making. Suppose, then, that

[69] Steinbruner (fn. 2), 110.
[70] Stein, "A View From Cairo" (fn. 1), 55.
[71] Jervis, *Perception and Misperception* (fn. 1); Betts (fn. 1).
[72] For some striking examples, see *ibid.*, 115-16, 121-25.

decision makers actually behave according to rational-choice theory. Indeed, no matter what he said, it is hard to believe that Kennedy's Cuban-missile decisions were unaffected by the difference between a *possibility* of impeachment, a *likelihood* of impeachment, and a *certainty* of impeachment. But suppose that, like the rest of us, Kennedy subsequently bolstered his decision by thinking and saying that any other choice would have brought on unpleasant political consequences with certainty. If so, he would speak in the apparently nonrational language that he actually used, but his behavior would be predicted perfectly by rationality assumptions.

Obvious competing interpretations like this one have not been excluded by the critics of rational deterrence. It is not easy to do so. Indeed, no matter how detailed the historical records, disagreements break out routinely over interpretations of decision makers' thoughts and intentions.[73] The degree of uncertainty in historical interpretations of motive and intention is often substantial, and conclusions deriving from such interpretations must be discounted appropriately. Yet case-study analysts usually provide no assessment of the reliability of their historical judgments. In particular, without a detailed showing that the questionable memories of decision makers represent the historical process accurately, no amount of evidence about their apparent calculation errors is relevant to the issue of whether rational deterrence theory predicts decision making.[74] In the absence of such a showing, rational deterrence theory stands unrebutted.[75]

It is not our central point here to set out one particular list of inferential

[73] See, for example, John Orme's critique of Lebow's historical research in "Deterrence Failures: A Second Look," *International Security* 11 (Spring 1987), 96-124.

[74] The relative unreliability of historical interpretation helps to explain why the quantitative literature on deterrence so often concentrates on power ratios and alliance bonds as causes of deterrence failure while ignoring mental events. The econometric theory of errors in variables demonstrated long ago that, to avoid serious inferential blunders, noisy data like self-reports of international decision makers must be discounted well beyond what common sense would suggest. This point is much less obvious within the informal inferential norms of the case-study tradition.

This does not imply that decision makers' self-reports need be problematic in all contexts. For example, survey research reports of preferences for political candidates will not ordinarily be subject to strong distortions, and they may therefore be used in tests of rational-choice assumptions. In general, tests of rational-choice theory based on reported preferences rather than on observed actions are tests of the joint proposition that the theory is correct and the reports are accurate. Only when we can have confidence in the latter proposition does the evidence bear on the rational-choice theory under consideration. (We are indebted to Henry Brady for this latter point.)

[75] The attentive reader will note that "unrebutted" does not mean "supported." In particular, to read our hypothetical reconstruction of Kennedy's thinking as an attempt to provide evidence *for* rational deterrence theory is to miss our point. What we are trying to demonstrate is that all such arguments are inherently flimsy in the absence of other evidence, and that such evidence is usually missing in the case-study literature, just as it is in our reconstruction.

slips by case-study analysts. After all, one can imagine case studies that select cases in a nonprejudicial fashion, and that take proper account of the fallibility in assessments of decision makers' intentions. Well-designed case-study tests may not be decisive, but they can be highly enlightening and strongly persuasive.[76] They are certainly an indispensable first step before proceeding to statistical methods—a point quantitative researchers have too often ignored at their peril.

In principle, case studies are capable of doing everything statistical analysis can do; they may simply replace symbols with words and replicate the random samples, precise definitions, and rigorous inferences of statistical methods. But in practice, inferential rigor is not the best tool for what case studies should accomplish, and the comparative method works best when it enforces no such discipline. As Jack Snyder notes, "The best analysts in the [case study] field have always used a rough-and-ready version of the scientific method," which they have identified, not with the powerful counterintuitive logic of the real McCoy, but rather with the substantially tamer "self-conscious, systematic application of commonsense rules of inference."[77]

When the purposes of comparative case studies are properly understood, informality is the right choice. Creativity is enhanced when historical cases can be chosen at liberty and analyzed in accord with the investigator's intuitions. The great many degrees of freedom in these studies are inordinately helpful in finding useful variables and producing empirical generalizations. Like all good things, however, this free play for unaided common sense has a price. Used in isolation from scientific methods, it creates inevitable inferential errors and makes decisive theory verification well-nigh impossible. In case studies of rational deterrence, the consequences have been both predictable and devastating for the credibility of their conclusions.

Conclusion

Although many of our comments have criticized how case studies are used in practice, we emphatically believe that they are essential to the development and testing of social-science theory. They have the same claim to scientific status as formal theory, statistical methods, or any other research tool.[78] In international relations, only case studies provide the in-

[76] For example, Mearsheimer (fn. 1); Posen (fn. 1); Walt (fn. 1).
[77] Jack L. Snyder, "Richness, Rigor, and Relevance in the Study of Soviet Foreign Policy," *International Security* 9 (Winter 1984-85), 89-108.
[78] George and McKeown (fn. 9), 54.

tensive empirical analysis that can find previously unnoticed causal factors and historical patterns such as those discussed by George and Smoke. But empirical generalizations and lists of variables are just a first step. They can substitute neither for theorizing nor for empirical verification. To misuse case studies in an effort to get what they cannot provide slights both theory development and empirical investigation.

Analysts who employ case studies of deterrence have done well at producing lists of variables that influence deterrence success, along with certain empirical generalizations about how different types of crises unfold. But they have produced no impressive general propositions to compare with those of rational deterrence.

Case-study generalizations are not a substitute for theorizing; empirical laws should not be mistaken for theoretical propositions. More than anything else, the hallmark of good theory is a fecundity that logically entails novel, sometimes surprising, insights and predictions. Inductive "theory" lacks this fecundity because it contains too few logical constraints. Categories can be multiplied to fit all cases. "Surprises" emerge not from the generalization, but from the case. Hence, we often cannot tell a consequential finding from an artifact; and when we succeed, the next case makes us begin all over again. By contrast, deductive theoretical propositions are of interest precisely because they interconnect with one another and prevent arbitrary multiplication of explanatory categories. The investigator is forced to be coherent.

Multiple case studies are also no substitute for statistical testing of theoretical propositions; contingent empirical generalizations should not be confused with confirmed statistical regularities. Here again, the informal procedures of case-study analysis provide too few constraints on the imagination of the analyst. In consequence, empirical evidence from case studies is rarely strong enough to falsify a theory. In the case-study literature on deterrence, these weaknesses have appeared, first, in selection bias—the tendency to oversample deterrence failures—and, second, in the uncritical use of decision makers' own reconstructions of their thinking. Each of these virtually guarantees a negative evaluation of rational deterrence theory, no matter how well it actually performs. Only statistical analysis, with its formal criteria for inference, is likely in practice to provide honest tests of theories.

Case studies are an important complement to both theory-building and statistical investigations, for precisely the reason Russett and George indicate: they allow a close examination of historical sequences in the search for causal processes.[79] The analyst is able to identify plausible causal var-

[79] Russett (fn. 53); George (fn. 6).

iables, a task essential to theory construction and testing. Comparison of historical cases to theoretical predictions provides a sense of whether the theoretical story is compelling, and yields indispensable prior knowledge for more formal tests of explanatory adequacy. The method also generates novel empirical generalizations, which pose puzzles and challenges for theory to explain. In all these ways, case studies provide guidance in the revision and reformulation of analytic theory to account for a broader range of phenomena. Indeed, analytic theory cannot do without case studies. Because they are simultaneously sensitive to data and theory, case studies are more useful for these purposes than any other methodological tool. Too often, however, their findings have been interpreted as bodies of theory and tests of explanatory power. It is to these misuses that our criticisms have been directed.