

DETERRENCE AND FOREIGN POLICY

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THE article by Christopher Achen and Duncan Snidal in this issue, however provocative some of its observations, has the merit of raising an important question: How should we think about the relationship between deductive theories of deterrence on the one hand and, on the other, research on deterrence that is at the same time empirical and oriented toward theory? While forcefully defending the deductive form of theory, Achen and Snidal also clearly dissociate themselves from the extreme position that it is a self-contained enterprise that need not take empirical research seriously. Rather, they recognize that empirical case studies are capable of contributing, and, indeed, have to some extent already contributed, to the development of theory, perhaps even to the kind of deductive theory they favor.¹

Their general position on the need for both approaches—although not the superiority and priority they attach to deductive deterrence theory—is entirely compatible with our own long-standing position. The question to be debated is not the superiority of one approach over the other but rather how to improve both approaches and develop fruitful interaction between them.² On this score, the Achen-Snidal article is disappointingly

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¹ Thus: "in the hands of George and Smoke, the case study approach helps generate theory in a very direct way" (p. 156); "we emphatically believe that they [case studies] are essential to the development and testing of social science theory" (p. 167); "indeed, analytic theory cannot do without case studies" (p. 169; see also p. 159). Achen and Snidal, "Rational Deterrence Theory and Comparative Case Studies," *World Politics* 41 (January 1989), 143-69.

² Alexander L. George and Richard Smoke, *Deterrence in American Foreign Policy: Theory and Practice* (New York: Columbia University Press, 1974). Since publishing this book, we have worked steadily to raise standards and explicate procedures for improving the quality of case studies and their contribution to theory development. We have given serious attention to most of the concerns about them raised by Achen and Snidal. See A. 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); A. L. George, "Case Studies and Theory Development," paper presented to the Second Annual Symposium on Information Processing in Organizations, Carnegie-Mellon University, Pittsburgh, PA, October 15-16, 1982; A. L. George and T. J. McKeown, "Case Studies and Theories of Organizational Decision Making," in Robert Coulam and Richard Smith, eds., *Advances in Information Processing in Organizations*, Vol. 2 (Greenwich, CT: JAI

meager in its suggestions, in part because it concentrates on real or alleged limitations of case-study research and has little to say about how the deductive theory of deterrence, which they admit is deficient, might be improved.

In reporting the results of our empirical research on deterrence fifteen years ago, we called attention to the limitations of the deductive, abstract version of the theory then extant; we did not reject the approach itself. In fact, as Achen and Snidal come close to recognizing (fn. 41, and even more so in an earlier version of their paper), the reformulation of deterrence theory we advanced in Chapter 17 of our book presented a number of more refined propositions that suggested how deductive theory might be improved. This was, to be sure, incidental to our major objective, which was to demonstrate by means of a relatively new research strategy ("structured, focused comparison"³) how a relatively small number of historical case studies could contribute to developing a more differentiated theory than deductive theory offered, one composed of conditional generalizations (as exemplified in Chapter 18) that had more specific relevance for policy making.⁴ In this research strategy, individual cases are used as building blocks to create a cumulative development of typological explanatory theory. Since the cases selected for study in this approach do not constitute a representative sample of the entire universe (a goal that is probably infeasible in any case), typological theory does not provide a basis for estimating the frequency distribution of different outcomes and, contrary to Achen and Snidal (p. 160), we have explicitly denied any such claim for it.⁵

Press, 1985), 21-58. Case-study methodology is discussed also in Richard Smoke, *War: Controlling Escalation* (Cambridge: Harvard University Press, 1977), chap. 3 and app. B. George continues efforts to improve case-study methodology and plans additional publications.

A discussion of the uses and limitations of the controlled comparison method of studying a small number of cases for theory development should not overlook the importance of single-case studies. Lack of space prevents us from summarizing the arguments on behalf of the contributions a single case study can make to theory development that have been advanced by such writers as Harry Eckstein, "Case Study and Theory in Political Science," in F. I. Greenstein and N. W. Polsby, eds., *Handbook of Political Science*, Vol. 7 (Reading, MA: Addison-Wesley, 1975), 79-138, and Lawrence B. Mohr, "The Reliability of the Case Study As a Source of Information," in Coulam and Smith, *op. cit.*, 65-97.

³ The description of this method in George and Smoke (fn. 2), 95-103, has since been elaborated, and related methodological and theoretical issues more fully discussed, in the two articles by George (fn. 2) and in the George-McKeown article (fn. 2).

⁴ The formulation of contingent generalizations is necessary in order to capture the fact that deterrence is characterized by the phenomenon of what General Systems Theory refers to as "equifinality" (and what John Stuart Mill referred to as "plurality of causes" when he warned that his "method of agreement" and "method of difference" could not be easily employed for inferring causal relationships). Equifinality refers to the fact that similar outcomes on a dependent variable (e.g., deterrence failures) occur as a result of different causal processes, thus making the search for robust universal causal generalizations infeasible. Abstract deductive deterrence theory has thus far ignored the phenomenon of equifinality.

⁵ See George (fn. 2, 1979), 59-60.

In an additional departure from existing deterrence theory, we disaggregated it into several of its components, which we called “initiation theory,” “commitment theory,” and “warning and response” theory (Chapters 17, 19, and 20).⁶ We did so because we believe that an internally more complex form of deterrence theory will be necessary in order to enhance its policy usefulness. Thus, for example, we need to know more than rational deterrence theory tells us before we can better understand under what conditions, why, and how a state dissatisfied with the status quo will choose to challenge deterrence. Needed for this purpose is an “initiation theory” that is much richer and more complex than that provided by the deductive theory of deterrence.

In another departure from abstract deductive deterrence theory, we found it necessary to reconceptualize the problem of deterrence for different levels of conflict. These are (1) the deterrent relationship of the two superpowers’ strategic forces; (2) the deterrence of local and limited wars; and (3) the deterrence of nonmilitary challenges and “sublimated” conflict at the lower level of the spectrum of violence. We noted that the first of these three levels had received the greatest and most successful attention in the formulation of deterrence theory. Largely because deterrence theory at the strategic level, dealing as it does with a relatively simple structural situation, was so much better developed, theorists were tempted to employ the logic of strategic deterrence as the paradigm case for thinking about deterrence in general. This has proven to be quite unsatisfactory, however, for there are major differences in the problem of applying deterrence strategy effectively at the second and third levels of conflict. Deterrence at those levels is much more dependent upon context than at the strategic level (although contextual variables also affect strategic deterrence—witness the concern over conditions and events that can contribute to crisis instability and inadvertent or accidental war). Thus, situations of substrategic conflict, in which the policy question of whether and how to employ deterrence arises, are often very fluid, ambiguous, and subject to unexpected and substantial changes. The interests and motivations (and hence the objectives) of one or both sides are often much more complex and unstable than in the simpler, paradigmatic strategic case.⁷ And, finally, it is also more difficult to determine the means that

⁶ This kind of disaggregated theory is an example of what Paul Diesing, drawing upon Abraham Kaplan, has referred to as “concatenated theory” in his *Patterns of Discovery in the Social Sciences* (Chicago: Aldine-Atherton, 1971), 22-24.

⁷ Particular attention must be given not only to the strength of the Initiator’s motivation to challenge the status quo but also to the time he has in which to do so. Moreover, asymmetry of motivation favoring the Initiator can sometimes compensate for asymmetry of power favoring the Defender. On this point see, for example, A. L. George et al., *The Limits of Coercive*

are likely to provide effective deterrence, i.e., the type and level of potential damage considered necessary and adequate to convey a credible and sufficiently potent threat.⁸ As a result, not only are the requirements for deterrence often more complicated and elusive for those levels of conflict; they are also more difficult both to identify reliably and to meet.⁹ The problems of deterrence at different levels of conflict offer an additional challenge to deductive deterrence theory that it has not yet addressed and that are not recognized in the Achen-Snidal article.

We need to comment also on the inadequacy of rational deterrence theory's current formulation of the utility framework for dealing with the cost-benefit calculations of a state contemplating whether to challenge deterrence. The theory's assumption that a would-be initiator decides whether to "attack" or "not to attack" is oversimplified and inadequate for prediction. As we noted in our book,¹⁰ the Initiator's choice is often not so limited; rather, the Initiator often has multiple options at his disposal for challenging a deterrence commitment, and his cost-benefit calculations are geared to identifying an option that offers an opportunity for gain while minimizing the risk of an unwanted response by the Defender. A more complex model of strategic interaction than Achen and Snidal offer is needed to grasp the interplay between a Defender who employs deterrence strategy and an Initiator who is considering not merely *whether* to challenge but *how best* do so at an acceptable cost-benefit level. Employing such a model of strategic interaction enabled us to score some cases as having mixed outcomes, i.e., the deterrence strategy employed may have succeeded in dissuading the Initiator from choosing some riskier options for challenging the status quo but it failed to dissuade the Initiator from employing "limited probes" or "controlled pressure" strategies to bring about change. This conceptualization of the interaction process also enabled us to identify several different causal patterns of deterrence failure (the phenomenon of equifinality we referred to in foot-

Diplomacy (Boston: Little, Brown, 1971); Zeev Maoz, "Resolve, Capabilities, and the Outcomes of Interstate Disputes, 1816-1976," *Journal of Conflict Resolution* 27 (June 1983), 195-229; Jack S. Levy, "When Do Deterrent Threats Work?" *British Journal of Political Science* 18 (October 1988), 433-60.

⁸ Robert Wilson notes the difficulty that game-theoretic models encounter with contextual variables, which render most situations indeterminate; "Deterrence in Oligopolistic Competition," in Paul C. Stern et al., eds., *Perspectives on Deterrence* (New York: Oxford University Press, forthcoming).

⁹ Examples of difficulties encountered in attempts to apply deterrence strategy at substrategic levels of conflict are provided in the analysis of the Eisenhower administration's Middle East policies in George and Smoke (fn. 2), chap. 11. For a discussion of thirteen ways in which the characteristics of deterrence at the strategic level differ from those at lower levels of conflict, see chap. 2.

¹⁰ *Ibid.*, 520-22.

note 4) which, as Achen and Snidal acknowledge (p. 156), their rational deterrence theory has not considered and which they regard as of major importance for theory as well as policy.¹¹

The preceding remarks have raised the problem relating to levels of analysis in constructing deterrence theory and, even more, in utilizing it in policy-making contexts. Achen and Snidal evidently prefer to hold fast to a deductive theory, one that “Black Boxes” both the decision-making and strategic-interaction levels of analysis. It is not easy for the reader to grasp how they reconcile this preference with their recognition of the value of findings from empirical research that attempt to study actual events in these two Black Boxes rather than simply make assumptions about decision-making and strategic-interaction processes. Left unanswered in their essay is whether and how they would integrate these empirical findings into a new deductive deterrence theory or how they would make the connection between the two types of theory. In other words, how do they propose to deal with the deterrence phenomenon at different levels of analysis?

This leads us to raise a number of questions regarding the nature of the deductive rational deterrence theory espoused by Achen and Snidal and to pose the critical question of how it might be improved and still remain a deductive theory. Although the major impetus and thrust of their article is to defend deductive deterrence theory, Achen and Snidal clearly admit the absence of a satisfactory rational deterrence theory. They acknowledge that coherent supporting arguments for such a theory are lacking, and state that what they are really praising are its foreseeable conclusions when it is properly developed.¹² Nonetheless, they vigorously praise the contributions it has already made, while at the same time arguing merely that rational deterrence theory should not be abandoned for what they dismiss as “the patchwork of empirical findings currently available” (p. 159). And, toward the end of the essay, they further dilute their defense of this particular theory by concluding: “More importantly, our goal here is to defend theory, not rational deterrence theory” (p. 159).

While the forthright admission that rational deterrence theory remains undeveloped is praiseworthy, the authors should at least tell us what they think an axiomatic-deductive theory must be like in order to be capable

¹¹ *Ibid.*, chap. 18.

¹² See particularly fn. 24 in Achen and Snidal (fn. 1): “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.” However, they go on to offer the confident assurance that “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.’”

of predicting the success or failure of deterrence in specific cases. That it is not currently a full-fledged deductive theory is evident, but what is missing? Have the requirements and standards for such theories been clearly and fully identified and agreed upon by proponents of deductive theory? Do Achen and Snidal adhere to the same conception of these requirements as Harrison Wagner or Bruce Bueno de Mesquita; or are they satisfied with the operationalization of deterrence provided by Huth and Russett.¹³

The task of developing a full-fledged deductive deterrence theory may be much more demanding than Achen and Snidal anticipate. Even if the internal logical structure of such a theory is eventually fully developed, it remains to be seen how well it will perform in making predictions of deterrence outcomes in specific cases. Achen and Snidal offer no clear conception of what kind of axiomatic-deductive theory would be capable of making such predictions. To achieve this capability, rational deterrence theory requires not merely internal logical consistency; it must also be *operationalized* so that the presence or absence of the conditions assumed to be necessary and/or sufficient for deterrence to be effective can be measured and established in each of the specific cases for which predictions are attempted. Since the theory is not yet operationalized, it provides no basis for predicting whether a specific deterrence threat will be sufficiently credible and potent in any particular case. Lack of operationalization leaves rational deterrence theory weakest precisely where it needs to be strongest if it is to be able to make predictions in specific cases, i.e., in assessing the utility calculations of the Initiator in a particular situation. (It was for this reason that fifteen years ago we emphasized the neglected importance of "Initiation Theory" in the abstract deductive theory of that era.¹⁴)

We shall return to problems of operationalizing deductive deterrence theory but, first, we would like to make several observations about its assumption of rationality. An assumption of some kind of "rationality" in deductive deterrence theory is necessary and useful as a starting point, but a general, unrefined assumption of rationality appears to us as grossly insufficient to operationalize the theory adequately. We do not reject the usefulness of the general assumption of rationality—even "pure rationality"—for the development of theory. But, as Thomas Schelling cautioned many years ago, "whether the resulting theory provides good or poor insight into actual behavior is . . . a matter for subsequent judg-

¹³ Paul Huth and Bruce Russett, "What Makes Deterrence Work? Cases From 1900-1980," *World Politics* 36 (July 1984), 496-526.

¹⁴ George and Smoke (fn. 2), chap. 17.

ment.”¹⁵ Even if one operates with the additional assumption of a “unitary actor,” a more specific version of the type of rationality assumed to be operating is required, if only to indicate how one believes actors in general, or the specific actor in question who is contemplating a challenge to deterrence, will deal with the well-known cognitive limits on rationality. (Not all actors deal with these cognitive limits in the same way.)¹⁶ Herbert Simon’s concept of “bounded rationality” is of some use but, as he recognizes, it is often insufficient for fine-tuning rational choice theories.¹⁷ If the theorist wishes to continue to “Black Box” the decision-making process by means of assumptions (instead of getting into the Black Box to observe and/or estimate what is actually going on, *as policy makers with the assistance of their intelligence specialists attempt to do*), then it is often desirable to develop what Simon calls “auxiliary assumptions” as to the type of rationality that actors in general, or the particular actors about whom predictions are to be made, typically exhibit in situations of this kind.

Such auxiliary assumptions regarding rationality are often added to their models, as Simon notes, by investigators who formulate and employ deductive theory. But where do they come from? Not from contemplating one’s navel but rather from reflecting on relevant empirical observations of actual behavior and converting such observations into auxiliary assumptions to be added to the theory and incorporated into its internal logical structure.

Important questions arise in addressing the task of operationalization, the answers to which are not self-evident. First, which of the key concepts of rational deterrence theory should be singled out for this treatment? Achen and Snidal, like other deterrence theorists, emphasize the importance of (1) the credibility of a deterrence commitment/threat, and (2) the need for a threat of punishment that is sufficiently strong to overcome the Initiator’s inclination to challenge deterrence by influencing his cost-benefit calculations. Are these the key concepts of the theory to be operationalized? Or will deductive theorists avoid dealing with these particular concepts and settle for operationalizing the Initiator’s utility calculations? This would require the investigator to determine whether, in contemplating a possible challenge to the status quo, the would-be Initiator ex-

¹⁵ Schelling, *Strategy of Conflict* (Cambridge: Harvard University Press, 1960), 4. For a fuller statement of our concern over the limitations of the rationality assumption in deterrence theory see George and Smoke (fn. 2), 73-77.

¹⁶ See, for example, the discussion of different ways in which individuals attempt to cope with the cognitive limits on rationality (i.e., inadequate information, inadequate knowledge, and value complexity) in A. L. George, *Presidential Decisionmaking in Foreign Policy* (Boulder, CO: Westview Press, 1980), chap. 2.

¹⁷ Simon, “Human Nature in Politics: The Dialogue of Psychology with Political Science,” *American Political Science Review* 79 (June 1985), 293-304. See especially pp. 295 and 300.

periences “positive utility” or “negative utility” in a particular deterrence situation. If so, how useful would such operationalization of the theory be for predicting deterrence success and failure?

Second, what kind of predictions would a full-fledged, adequately operationalized deductive deterrence theory be capable of making, and how useful would such predictions be? Would such a theory modestly claim (as does Bueno de Mesquita, who regards “positive utility” as a necessary condition for war initiation¹⁸) to have identified only a “necessary” condition for deterrence success, leaving unidentified the all-important other conditions? If, for example, either the credibility or the potency of a deterrent threat (or, more generally, “negative utility”) is only a necessary but not sufficient condition for deterrence success, then such a theory does not predict deterrence success but merely observes that the stipulated necessary condition would be present *if* a deterrence success occurred. The predictive power of the theory and its utility would be further diminished because a so-called “necessary” condition, such as credibility of the deterrent threat, could be present (as in the Pearl Harbor case) even in cases of deterrence failure.

On the other hand, would an adequately operationalized deductive theory make the much more ambitious claim that it identified one or more sufficient conditions for deterrence success? If so, what are the candidates for a sufficient condition? Credibility of a deterrent commitment, we already know from historical cases, cannot be considered a sufficient condition for deterrence success. In the Pearl Harbor case, for instance, the Japanese accepted the U.S. deterrence commitment as credible but nonetheless attacked, in part because the United States was also subjecting them to strong coercive diplomacy. Again, is a threat of punishment potent enough to turn the Initiator’s cost-benefit calculations against an “attack” a sufficient condition? Not by itself, quite obviously, since such a threat must also be credible. Perhaps one can say that a sufficiently potent threat (whatever that is) is at least a “necessary,” though certainly not a sufficient, condition for deterrence success. Such a restricted claim is consistent with what we have already learned from history: while “massive retaliation” was an enormously potent threat, it often lacked enough credibility and relevance to deter some types of challenges to deterrence commitments made by the United States on behalf of its foreign policy interests.

Evidently some combination of these two conditions—credibility and potency of deterrence threat—is relevant for deterrence. Leaving aside the important and not easily answered question of their required mag-

¹⁸ See Bruce Bueno de Mesquita, *The War Trap* (New Haven: Yale University Press, 1981).

nitude in any particular situation, difficult questions for deductive theory remain. First, if both these conditions are held to be necessary and/or sufficient, how can they be operationalized? This task is most formidable. The alternative—to avoid dealing directly with them and to focus, instead, on the would-be Initiator's "positive" or "negative" utility—would leave us with a disappointing, almost trivial, theory that could claim to identify only a necessary condition for challenging deterrence and, hence, could not really predict deterrence outcomes.

A second issue would arise if, in applying such a theory, one encountered historical cases in which both conditions required by the theory were measured as being present but, nonetheless, deterrence failed. How would the deductive theory explain such anomalous results? Would the proponents of the deductive theory "save" it by arguing that the incorrect predictions must be due to measurement errors that could in principle be reduced or eliminated by improvements in its operationalization? Or would they concede the possibility that the deductive theory leaves out variables (e.g., domestic political constraints, psychological impediments to optimal information processing, misperception and miscalculation of the opponent's intentions) that affect deterrence outcomes, or that it makes defective assumptions about rationality and strategic interaction? How would deductive theorists decide which of these two possible explanations—measurement error or failure to consider other relevant variables—is correct?

Still another issue—the familiar and vexing one already encountered in empirical research on deterrence—concerns the possibility that the absence of a challenge to deterrence might be *spuriously* scored as a deterrence success. Thus, the conditions postulated by the theory as necessary and sufficient for deterrence success might be established as present in a particular case in which, to be sure, deterrence was not challenged, but nevertheless a claim that the theory has made a successful prediction might not be justified.

Alternative explanations for the absence of a challenge to the status quo, which are not encompassed by deductive deterrence theory, would also have to be considered. Thus, although dissatisfied with the status quo, the Initiator might not have contemplated challenging deterrence and, hence, need not have been deterred. Or, while considering a possible challenge, the Initiator may have decided not to attack for other reasons altogether (e.g., domestic or allied constraints, internal policy disagreements, the expectation of eventually securing a favorable resolution to his grievance by diplomacy), and not because of the presumed credibility and potency of the deterrence threat.

Finally, we feel obliged to say that the failure fully to specify and to

operationalize rational deterrence theory encourages its defenders—unwittingly to be sure—to employ it in ways that risk giving the theory a tautological and untestable character. In its present non-operationalized form the theory is difficult, if not impossible, to refute; as a result, it is being endowed with a validity that is highly questionable, insufficiently qualified, and certainly premature. This is so because the theory, lacking operationalization, is not capable of generating predictions for specific cases. Instead, the outcomes of specific cases—once they are known—can all too easily be claimed to be consistent with the theory. After the fact, one can attribute historical cases of deterrence failure to lack of sufficient credibility and/or to an insufficiently potent threat of punishment. Similarly, ostensible cases of deterrence success can all too readily be chalked up to the use of a deterrence strategy that met the postulated requirements of the theory.

One way of adjusting to some of the difficulties we have noted would be to abandon the effort to state necessary and/or sufficient conditions and settle for the goal of constructing and validating a theory of deterrence that would at least be capable of making predictions on a probabilistic basis. A theory couched in probabilistic terms is not without value—if its expected performance can be specified and validated; but it should also invite efforts to identify additional conditions under which deterrence efforts are likely to succeed or fail. The kinds of “conditional generalizations” that can be abstracted from historical case studies of the kind we presented in our earlier work might be helpful in this respect.¹⁹

Certainly efforts to develop deductive deterrence theory should continue and be pressed forward. We have argued that the standards and requirements for such a theory and the type of predictions it would be capable of making should be explicated rather than obscured by general arguments for the superiority of this type of theory. If this is done, it will become obvious that the achievement of a robust deductive deterrence theory is far removed from, and in any case of quite uncertain value for, providing strong guidance for policy-making purposes. Neorealist structural theory in international relations, with which Achen and Snidal correctly connect rational deterrence theory, addresses only one level of analysis. The foremost expositors and proponents of neorealist structural theory explicitly concede that it is not a theory of foreign policy. Admittedly, a theory of foreign policy, strictly speaking, does not exist and, given the complexity and difficulty of the task, is not likely to emerge in robust form for many years, if ever. Neorealist structural theory, in our view, is necessary, but quite insufficient by itself either for adequately un-

¹⁹ George and Smoke (fn. 2), chap. 18.

derstanding and explaining much of importance in international relations or for managing foreign policy. Empirical studies of decision making and strategic interaction are necessary to fill, to some extent at least, the important vacuum it leaves.

Deductive deterrence theory in its present primitive, undeveloped form (which, unlike Achen and Snidal, we do not equate with the virtue of “parsimony”) contributes very little to an understanding of the uses and limitations of deterrence strategy as an instrument of foreign policy. The possibility of unwarranted and dangerous uses of abstract deterrence theory as a basis for prescriptive advice to policy makers worried many in the early 1970s.²⁰ Ideas associated with the theory stimulated or accompanied the development of strategic theory, which, in turn, has provided a framework within which policy makers (not deductive theorists) have tried to address the operational requirements of strategic deterrence. Contrary to Achen and Snidal, however, we argue that whatever contribution abstract deterrence concepts made to the development of strategic theory, it was not matched by a similar contribution to an understanding of the uses and limitations of deterrence strategy at lower levels of conflict.

Whether deterrence theory is derived from deductive exercises or empirical research, we continue to argue—as we did fifteen years ago—for a sharper distinction between deterrence theory and deterrence strategy, and for a better understanding of the relationship between the two. Deterrence theory (limited and imperfect as it is) should be regarded as, at best, an aid to devising deterrence strategies. Its most relevant and responsible use for policy-making purposes is not its prescriptive power but rather its diagnostic function, i.e., its assistance to policy makers in assessing the configuration of a situation in which some kind of challenge to deterrence may occur. Since, as we argued and attempted to demonstrate in our book, deterrence at the substrategic levels of conflict is highly context-dependent, there is a critical need in policy making for good situational analysis.²¹ This would assess the nature and strength of the Initiator’s motivation, how urgently he feels the need to challenge deterrence, the options available to him for doing so, the kind of utility calculations and assessment of his options he is likely to be making, and which of them he is likely to choose, if any. Situational analysis of this

²⁰ See, e.g., *ibid.*, chap. 3.

²¹ On situational analysis and the diagnostic function of theory, see George and Smoke (fn. 2), chap. 16; also George (fn. 16), chap. 14. For a more detailed discussion of policy-relevant theory see George and Smoke (fn. 2), app. A longer version of this appendix was published by Smoke and George, “Theory for Policy in International Relations,” *Policy Sciences* 4 (December 1973), 387-413.

kind is likely to provide a better understanding of the deterrence problem and of what variant of deterrence strategy (alone or together with other policy options) is likely to be most appropriate and effective in that particular situation. Since the context of even a single deterrence situation can and does change over time, situational analysis is a continuing task. Middle-range theory, derived through empirical analysis and expressed in the form of conditional generalizations, together with general models of the opponent's behavioral characteristics, are of some help in situational analysis, as are the findings of the more recent empirical studies of deterrence subjected to critical analysis by Achen and Snidal.

As these remarks indicate, the policy maker (unlike the deductive theorist) makes the attempt to get into the Black Boxes of decision making and strategic interaction. Would deductive theorists say the policy maker should not do so but, instead, merely rely on abstract rational deterrence theory? In our view deductive deterrence theory can give the policy maker very limited help in assessing the decision-making process of the opponent or in anticipating the outcome of the interaction between his own deterrence strategy and the adversary's choice of action. For this, the policy maker has to rely largely on the information and analysis provided by his intelligence and advisory system.

We have saved for the end of our comment three other major limitations of deterrence theory—whether deductive or empirical. The first is that the theory cannot define its own scope and relevance as a means of achieving foreign policy goals.²² That is to say, as implied by the title given to our comment, deterrence theory per se provides no criteria to indicate when a deterrence policy should be applied in foreign policy. Often, but certainly not always (and particularly not at the substrategic levels of conflict), the criteria are self-evident or easily identified on an ad hoc basis. But, viewed in the broader context of foreign policy, as it should be, deterrence theory is best understood as a contingent policy theory: “*if* a state believes it necessary or desirable to employ deterrence in a particular situation, then the general requirements for devising an appropriate, hopefully viable deterrence strategy are the following . . .” Only a foreign policy theory, or a theory of statecraft, can provide guidance as to when a state should employ deterrence to protect specified types of interests as against (or in addition to) other strategies for protecting its interests and/or resolving points of conflict with other states. Deterrence strategy, in other words, must be viewed as only one of a number of different instruments of foreign policy. Failure to recognize this—either by theorists or

²² See George and Smoke (fn. 2), 77-78.

policy makers—can, and does, easily lead, as we argued in our book and continue to argue, to a narrow over-reliance on deterrence strategy in foreign policy.

Another, related limitation of deterrence theory is its exclusive preoccupation with threats of punishment as a means of persuading an adversary not to challenge a situation in which there is disagreement. Critics of deterrence, extending back to the early years of the Cold War, have emphasized (sometimes overemphasized) that threats can be counterproductive. We have argued that early deterrence theorists erred in their evident assumption that a viable theory of deterrence can be developed independently of a broader theory of processes by which nations influence each other, one that encompasses the utility of positive inducements as well as, or in lieu of, threats of negative sanctions.²³ Thus, for example, the credibility and potency of a deterrence threat might not be so critical if instead (or in addition) sufficient positive incentives for remaining at the status quo are offered.

Finally, deterrence theory ignores the frequent need to regard deterrence, even when it is necessary and effective, as a strategy to buy time. Deterrence can severely frustrate an adversary who is strongly motivated to change a status quo that he regards as invidious, especially when he feels it legitimate to do so. The consequences of continued frustration are not easily predictable and are not always favorable to the deterring power. Deterrence success in the short run is not always beneficial in the longer run; the adversary may become more desperate to mount a challenge and may proceed to acquire greater resources for doing so. Under such circumstances the most reliable benefit of successful deterrence may be more time—time which is best used not in a possibly futile effort to maintain deterrence indefinitely but to work out, if possible, an accommodation of conflicting interests so as to reduce reliance on deterrence and avoid overt conflict.²⁴

²³ *Ibid.*, chap. 21.

²⁴ For additional discussion see George and Smoke (fn. 2), 5-6; also George (fn. 16), 252-54. The importance of “promises” (as against “threats”) was briefly noted by Schelling (fn. 15), 43-46, 131-37, 175-77. Important leads for the development of a broader influence theory were given by David A. Baldwin, “The Power of Positive Sanctions,” *World Politics* 24 (October 1971), 19-38, and “Inter-Nation Influence Revisited,” *Journal of Conflict Resolution* 15 (December 1971), 471-86. The importance of conceptualizing the strategy of coercive diplomacy to include possible use of “carrots” as well as “sticks” was emphasized and illustrated in George et al. (fn. 7). The concept of “crisis bargaining” developed by Glenn Snyder and Paul Diesing includes accommodative as well as coercive actions; *Conflict Among Nations* (Princeton: Princeton University Press, 1977). Other writers, too numerous to mention here, have also emphasized the need for a broader, multifaceted theory of inducement or influence. More recently, Janice Gross Stein and Richard Ned Lebow have emphasized the need for more attention to various forms of “reassurance” to supplement or replace deterrence in conflict situations.