

Formal Models and Conflict Intervention: Success as a Research Program and Policy Relevance

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International relations theory can identify and frame important questions, but Pentagon and State Department officials will probably always be more interested in detailed case studies, prepared by area-studies experts. Theorizing about the causes of war might occasionally generate clean, law-like propositions that appeal to policymakers. But more typically, the discipline generates broad patterns that can be applied to particular cases only with a great deal of caution. "We have to recognize that there are limits to the predictive powers of political science," says Mr. [Robert] Art. "That's not an excuse to be sloppy. It's just to say that we don't have unified grand theories of many phenomena, especially not something as complex as war. None of us can predict the consequences of what will happen in the Middle East. Maybe this is why policy makers don't pay much attention to academics."¹

This article is an assessment of conflict intervention models and what can be done to improve the possibilities that formal techniques of conflict analysis can have a broader policy-relevant audience and impact. First, we examine the effectiveness of formal intervention modeling as a research program. More specifically, we evaluate the success of formal modeling in meeting the objectives of accumulation, integration, and synthesis. Second, we examine how its strengths and failures as a research program affect the policy relevance of conflict intervention modeling. We conclude with observations about how to strengthen future research in order to enhance contributions to policy applications.

EVALUATING FORMAL MODELING OF INTERVENTION AS A RESEARCH PROGRAM

To help us understand and evaluate the progress of formal modeling, we consider its capacity to meet three key objectives within the broader research program of conflict analysis. The first objective is “accumulation,” or the ability to build on previous findings and modify or discard arguments for which empirical support is lacking. The second objective is “integration,” the drawing on alternative

methodologies that provide similar findings in a different research context. The third objective is “synthesis,” the use of multiple levels of analysis ranging from individuals to large groups or nations.² We examine each of these objectives in turn.

ACCUMULATION

Beginning with the objective of accumulation, there is a higher degree of success in this area than in integration and synthesis. We identify convergence with respect to three key areas: rationality, intervention as a dynamic bargaining strategy, and bias. Although it is probably too early to identify any nascent consensus regarding precise modeling approaches to conflict intervention, it does appear that there is consensus on these basic minimal assumptions about what third parties do and the impact they have.

Rationality

For the most part, formal models of conflict rely on assumptions of rationality, even though rational behavior is often constrained by limited information. This focus may be in part derived from the existence of relevant modeling antecedents in other social science disciplines, primarily economics. In addition, formal models must identify general but persuasive behavioral rules to determine the choices of different combatants. Rationality provides such a framework, and hence forms the basis of most formal models.

Fearon provides a restricted, but rigorous, typology of conflict focused exclusively on rational conflict in its strictest, almost hyper-rationalist, sense.³ On this basis, Fearon asserts that there are only two purely rational explanations of conflict. The first explanation is one of private information about willingness or ability to fight. Since each antagonist has a clear incentive to exaggerate its ability, or willingness, to fight, any information shared in an attempt to avoid conflict will not be considered credible. Consequently, “collectively irrational” assessments of combat willingness and ability may arise in which the sum of the antagonists’ own calculated expected gains from fighting exceed that which would be available by sharing between them. Only fighting itself becomes a credible signal, frequently leading to games of escalation.

While Fearon provides only a heuristic discussion of this explanation, bolstered by some empirical examples, others present formal models of this process. Brito and Intriligator present a more complex representation of a similar problem of uncertainty (asymmetric information in this case), where the first stage involves selecting a strategy for arming (aggression or deterrence) and the second stage involves the possible use of a challenge that could lead to war.⁴ The result is that when a country does not know its opponent’s true propensity for fighting, its optimum strategy may be to react to challenges in a probabilistic manner—sometimes acquiescing and sometimes resisting—in order to deter bluffing by a weaker opponent. As a consequence of this strategy, war may occur in an otherwise rational framework.

The second rationalist explanation of war in Fearon deals with problems of commitment. Two more formal approaches are provided in this case: pre-emptive war with offensive advantages, and preventive war as a commitment problem. The former is the traditional “gunslinger” problem that has simple interpretations in a Prisoner’s Dilemma framework. As in Brito and Intriligator, you are either at war or you are not; in gunfighter’s parlance, you are either quick or you are dead. There is no temporal point at which one side can back down once all of the information for calculating the final outcome is known. As Schelling and others have noted, this type of model seems very applicable to nuclear confrontation. Stability and prevention in this sort of situation comes from either confidence building and commitment mechanisms, or by eliminating the first mover advantage by the presence of a credible retaliatory strike.

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The second of Fearon’s formal presentations of the commitment problem is a more interesting approach utilizing a dynamic multi-period framework. In this model, the most powerful country initiates war in the first period in order to prevent an up-and-coming rival from dictating less preferred outcomes in future periods. This story is a modification of the declining hegemon argument for war; a hegemon in its sunset “lashes out” at rivals in a doomed attempt to maintain its status. Fearon provides the important insight from rationality that conflict emerges because the challenger cannot credibly pre-commit to not challenge the current hegemon in the future. While the outcome of current fighting is unknown to both players, and may lead to the initiator’s defeat, Fearon shows that the initiator’s expected outcome for fighting now can exceed the certain bad outcome to which it would have to acquiesce in the future. The rising challenger faces a classic problem of time-inconsistency; it would like to be able to assuage its rival’s fears by pre-committing to a non-aggressive policy in the future, but it has no credible mechanism for such a commitment.

However, we feel that Fearon’s typology is incomplete. Garfinkel and Skaperdas, for example, provide a formal model that is arguably distinct from Fearon’s models but closely related to the preventive war with commitment problems.⁵ Using a two period model, Garfinkel and Skaperdas portray two countries that must divert resources towards their militaries in order to secure their respective share of resources. War becomes Pareto superior for both sides, as victory by either side settles the security dilemma and eliminates, or significantly diminishes, the need for subsequent investment in military preparation. The gain from war now is the reduction in military spending in the future. This approach can be thought of as cashing in on an extreme “peace dividend,” even though recent history has taught us that such dividends are often illusory or short-lived.

Other formal models effectively accept conflict as an inherent and unavoidable

element of human affairs. These same models implicitly accept that commitment problems prevent more peaceful means of settling distributional disputes and seek instead to explain only the intensity of conflict. Models by Hirshleifer are prime examples of this approach to formal conflict analysis.⁶ While Hirshleifer is careful to build on rationalist foundations of utility maximization, there is no explicit consideration of why they are unable to overcome coordination problems to reach Pareto superior outcomes. Associated models of intervention that build on Hirshleifer's approach are able to examine the details of marginal reactions to intervener behavior but cannot easily deal with the corner solutions of complete peace or complete victory.⁷

Dynamic bargaining

There is also consensus that intervention is a kind of dynamic bargaining strategy, where the characteristics of the intervener and its choice of strategy are treated endogenously.⁸ Alternative static approaches are unable to explain, for example, the development of crises over time or the temporal aspects of conflict management. Consequently, conflict intervention modeling has tended to go beyond the static approach in order to incorporate essential dynamic processes. One key aspect of this approach is the recognition of the importance of escalation, which Schelling identifies as the coercive side of negotiating a peace plan. Since intervention imposes costs on all parties to a conflict (including the intervener), the threat of escalation and higher cost imposition motivates actors to make concessions at the bargaining table.⁹

Many of the earliest attempts to address the question of escalation between states placed the bargaining process at center stage. Building on Schelling's insights, Harvey and Powell argue that since both actors are engaged in demonstrating their superior ability to tolerate the risks of higher cost imposition, escalation is conceptualized as a game of competitive risk-taking.¹⁰ It also seems to be accepted that the rate at which states escalate (impose costs) can have an important effect on the bargaining process.

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Schelling was also the first to note that deterrence situations are akin to non-zero-sum games such as Prisoner's Dilemma or Chicken. Unfortunately, in these games the least best outcomes may arise as a consequence of either the pure Nash equilibrium strategies (Prisoner's Dilemma), a lack of coordination in games where there are multiple Nash equilibria (such as Chicken), or a mixed strategy equilibrium. The more these games are repeated in an uncoordinated setting, the higher the probability of a disastrous outcome eventually occurring. Schelling's contribution

was a reorientation of game theory, introducing elements of commitment and resolve in strategic interaction. This effort made the games more realistic and potentially more policy relevant.

In practice, many models of deterrence and escalation are based on imperfect information.¹¹ These modeling approaches use extensive form games to illustrate how antagonists probe each other until sufficient information is revealed for a resolution to occur on mutually acceptable terms.¹² In the specific context of intervention models, Carment and Rowlands construct an intervention game with full information in which a dominant combatant and the intervener play sequentially.¹³ As in Brams and Kilgour, the payoff matrix ultimately determines the extent to which either side is prepared to escalate in order to acquire benefits at the margin; full information implies that both sides know how far the opponent is prepared to go to achieve or avoid certain outcomes.¹⁴ Powell's recent study on war is certainly helpful in understanding why.¹⁵ He shows that recent formal work on conflict management issues draws very heavily on Rubinstein's seminal analysis of the bargaining problem and the research that flowed from it.¹⁶ More importantly, he suggests that there is now what might be called a standard, or canonical, model of the origins of war that sees its occurrence as a bargaining breakdown.

The Effects of Bias

A third area where there is emerging consensus focuses on the importance of bias and moral hazard, as is Andrew Kydd's assessment of biased mediators.¹⁷ Kydd argues that mediators are often thought to be more effective if they are unbiased or have no preferences over the issue in dispute. His article presents a game theoretic model of mediation drawing on the theory of "cheap talk" that highlights a contrary logic. Conflict arises in bargaining games because of uncertainty about the resolve of the parties. A mediator can reduce the likelihood of conflict by providing information on this score. For a mediator to be effective, the parties must believe that the mediator is telling the truth. This is especially true if the mediator counsels one side to make a concession because the opponent has high resolve and the will to fight.

An unbiased mediator who is simply interested in minimizing the probability of conflict, however, will have a strong incentive to make such statements even if they are not true. Hence, the parties will not find the mediator credible. Only mediators who are effectively "on your side" will be believed if they counsel restraint. The intuition behind Kydd's result is simple and persuasive; the mediator is effectively acting to replace steps in a game of escalation under imperfect information. As in traditional public goods problems in economics, Pareto inefficient solutions emerge because of the difficulties of preference revelation. A biased mediator can credibly solve the problem of incentives to misrepresent in terms of one player.

Using a theory of mediation and peacekeeping, Smith and Stam point to the sources of recent events in the Middle East and reasons for the more general pattern of failed mediation.¹⁸ In this light, third-party intervention and mediation are

explored in the context of a random walk model of warfare and war termination. In considering how third parties can hasten the end of conflict, it is shown that while mediators can use side payments or threats to intervene directly, unlike in Kydd, they cannot help nations resolve informational differences. The model's equilibria demonstrate that conflict continues until beliefs converge sufficiently for both sides to agree that the costs of fighting exceed likely gains in the bargaining process. Thus, at issue is whether the mediator can end such wars by speeding up the convergence via non-violent presentation of information. It is concluded that deductive reasoning allows for the parsing out of those mechanisms through which third parties influence conflict.

INTEGRATION

Our second objective of integration is defined as efforts to draw on findings from different methodologies and present them in one package. Within the literature, findings appear to be quite diverse and inconclusive, and there appear to be only a few efforts at integration. Part of the problem in achieving integration is that, in contrast to our example of economics, formal conflict theorists do not yet have a sufficiently developed consensus about how to model conflict (not just intervention) or identify when one approach is more applicable than another. This mirrors the problems that are manifested in the intervention literature in general.

In the absence of a comprehensive set of conflict (and conflict intervention) models, the only alternative is to use formal approaches to intervention that are sufficiently robust to transcend any underlying conflict model. One possible candidate is deterrence theory, the formal analysis of which has wide, though not universal, acceptance. Deterrence theory has sufficient rigor in structure to be generalizable and sufficient flexibility in interpretation to be tailored for specific application. More importantly, deterrence theory has been broadly applied using a variety of different methodologies: induction, deduction, and assumptions of rationality and non-rationality. Rational deterrence has a proven ability to permeate government institutions, having been the foundation of Cold War security policy, and broad empirical content.

While promising, certain caveats need to be acknowledged before embracing rational deterrence as the only or best approach to intervention analysis. First, failures of deterrence have been frequent despite its apparent acceptance within the policy community. Whether these represent teething problems in recalibrating the theory to fit intrastate conflict conditions or more fundamental defects that preclude its universal application is difficult to say. Certainly it is plausible that deterrence would be more difficult when dealing with irregular forces lacking a clear political or military hierarchy, and operating outside the control of a clearly recognizable political structure. Second, it is not apparent that rational deterrence is the most efficient basis for organizing intervention, especially if the "cause" of the war is informational asymmetry. Third, even if it is the most efficient, past practice suggests that interveners may not have the inclination to apply sufficient effort to make deterrence

effective. Finally, while deterrence theory may provide a shortcut to modeling conflict intervention, it still requires an understanding of what motivates the different combatants in a conflict. This brings us back to the initial and fundamental problem regarding the nature of the conflict.

SYNTHESIS

We turn now to our third objective of synthesis: the integration of findings at different levels of analysis. It could be argued that conflict analysis and, more specifically, intervention theory should lend themselves to synthesis, because so much has been written on the topic from a variety of methodological perspectives. However, there appear to be few efforts to take findings from one level of analysis (e.g. the individual or group) and apply them at higher levels of aggregation. An exception to this would be mediation and negotiation research, wherein insights and research on small group interactions—questions of bias and impartiality—lend themselves to questions of third-party effectiveness at the state level.¹⁹

Of course, synthesis has its limitations. Rationalist explanations assume interveners are capable of making decisions on a conflict according to coherent, well-ordered preferences. Despite the presence of complex coalitions and domestic political economy processes, interveners and belligerents are generally treated as rational actors for analytic convenience. Even if we accept the abstraction of third parties and belligerents as rational unitary actors, we still must satisfactorily specify the objectives of their decision makers.

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Arrow's theorem suggests that although states unified under a multilateral coalition may act as if they are unitary decision makers, they may also act incoherently by not revealing a complete set of transitive preferences.²⁰ It may be impossible to argue that any collection of persons or states is acting as if they were pursuing an identifiable goal. Bueno de Mesquita has suggested that we cannot truly understand any international behavior or process unless we specify the role of decision makers in the process. The difficulty lies in estimating the values that policymakers assign to particular goals or objectives and their willingness to bear the potential risks and costs of a particular action.

Maoz offers some valuable lessons on synthesis, accumulation, and integration.²¹ Maoz first develops a game theoretic model with modified versions of: a) conflict-initiation, b) conflict management, and c) negotiation. These factors are viewed first from the perspective of a single actor, and then from the perspective of both actors. This approach cuts across levels of analysis and draws on findings from disparate research on management and conflict analysis. Maoz uses this model to address three questions: a) what is the relationship between the preferences of individual decision

makers and aggregate outcomes which individual decision makers and groups observed at the international level? b) What is the relationship between choice and consequence in determining and assessing foreign policy outcomes? and c) Is there a link between micro and macro decision making and choices as evolutionary patterns develop over time? Maoz argues that micro and macro decision making behavior cannot be treated as discrete and independent variables if one wants to explain change in outcomes over time. Thus, Maoz sets about attempting to synthesize micro and macro models in order to explain changes over time. According to Maoz, synthesis leads to propositions that are both surprising and theoretically testable. Furthermore, synthesis is capable of explaining situations where “good results” would not be expected.

To recapitulate, one of the primary impediments to bringing formal modeling into policymaking is simply the absence of consensus regarding what should be the primary object of analysis. We interpret this problem as a function of the relative immaturity of the discipline, and not an inherent feature of the methodology. Conflict and conflict management are inherently complex phenomena, and modeling them for both precision and nuance is difficult. For this reason, deriving policy implications from formal models is difficult. This realization carries with it some implications.

First, policy recommendations that flow from formal studies can be inconsistent or contradictory. Inconsistency leaves the policy maker to choose alternative forms of analysis that provide a more consistent perspective. To be fair, some inconsistencies can be traced to differences in the evaluation technique, not flaws in the methodology itself. For example, some studies are concerned with explaining intervention outcomes. Others focus on the relative effectiveness of different types of actors, while still others focus specifically on procedure.²²

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Other problems reside within the logic of models themselves. In this vein, Bueno de Mesquita argues that a theory of conflict must be first deduced and must be logically consistent internally. Deduction begins with value-based assumptions about what are the important areas to study. Generally, but not always, this occurs through consensus among researchers working within a common paradigm.²³ This approach is consistent with “sophisticated methodological falsification” used to test propositions of deductively derived theories. The “truth” of a theory resides in whether or not its conclusions can be arrived at without faulty logic, and whether the properties of the model are clear. If a deduction follows logically from a set of assumptions, then that deduction is necessarily true under the precise conditions assumed in the theory. The truthfulness of a deduction is not an empirical

investigation. Falsification requires more than observation; it requires a clear analytical critique of the logic and concepts used in the model.

Second, there is a need to separate out *explaining* the process of strategic interactions from *understanding* the decisions that policy makers face at any specific point. Modeling can contribute to understanding the processes and choices. However, the modeling and explanatory dimensions must be refined before developing an approach that would help guide policymakers on specific choices in a given context.

Third, there is a need for accessibility. In our review of the formal modeling literature, there are few efforts to render formal modeling accessible to policy makers. It should be noted that our assessment of formal approaches is not exhaustive, but indicative.

IMPLICATIONS FOR POLICY

There are two major reasons why policymakers pay greater attention to case studies than empirical models. First, they are generally older, having completed their primary education well before the behavioral revolution, and the government doesn't provide much incentive to stay current in your field. Case studies are generally easy to understand and appreciate regardless of your educational level or methodological training. Second, the tension between qualitative and quantitative analysis in the government is, in times and places, much more acrimonious than it is in many political science departments, but this applies more at the level of the government analyst than the policymaker...."I don't think there's anything unique about international behavior that makes it less predictable than, say, economic behavior," says Philip A. Schrodt, a professor of political science at the University of Kansas. "If anything, an economic system is far more complicated than an international system. And yet we just constantly engage in economic forecasting."²⁴

Conflict analysis and formal theories of intervention that expect to be policy relevant must do three things. First, they must specify which elements of intervention are the most effective in assisting policy makers in designing more effective policies. In order for a theory to be politically useful it must have a solid body of empirical evidence to back its propositions. We have argued that efforts to provide empirical support for formal models is still in the nascent stages, but improving.

Second, intervention theory can aid policy by helping decision makers analyze problems in a manner that is superior than without it. In this case, intervention theory serves as a set of analytical tools; policy relevance stems directly from observing the behavior of interveners and belligerents each with its own logic and behavioral properties. Additionally, identifying systematic deviations from optimal decision making and the identification of certain correcting principles adds to our toolbox.

In each of the aforementioned areas there has been some progress. The connection between formal modeling and policy is not a simple one. It is useful to consider analogous situations where models have come to underpin policy analysis

and formulation. Good examples come from economics, where policy discussions are routinely informed by the analysis derived from formal models. Four observations from economics may prove useful for conflict resolution modeling and policymaking. First, the degree of consensus within the discipline regarding the basic models and their assumptions is overwhelming. Second, the models that tend to underpin most policy discussions have generally been tested and refined through empirical investigation. Hence, the influence of formal modeling is often indirect and filtered through a more complete “scientific” research programme that includes some degree of empirical verification. While differences of opinion are common in economics, it is often relatively easy to isolate the points of contention and identify empirical approaches to settling the dispute.

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Third, the substantial consensus in the discipline is perpetuated by the essentially uniform education of economists. Fourth and finally, most government economic policymakers who consume formal analyses are themselves trained in economics. Even if they cannot produce the formal analyses, economic policymakers will possess sufficient familiarity with the assumptions, techniques, concepts, terminology, and disciplinary biases to make the models accessible and more compelling. While this portrait is no doubt idealized, it is arguable that all of these elements are rarely present when it comes to formal models of conflict analysis. Each of these points are examined in turn.

First, formal modeling of conflict intervention cannot easily be translated into policy terms because of the lack of a developed consensus on conflict causes. If there was stronger agreement on how to model conflict, or how to model intervention, policymakers would have greater confidence in the consequent policy recommendations. Natural suspicion of novel and unconfirmed theories is reinforced by the fact that modeling economics and modeling political behavior is inherently different. In contrast to Glenn’s quote we might suspect that fewer actors and a simpler, less regulated, system will lead to more challenging and less predictable behavior. While the law of large numbers may allow us to ignore aberrant individual behavior in a disaggregated market context, no such convenience exists in international relations. Fewer actors encourages strategic behavior where actions can be changed abruptly and dramatically in response to the choices of others. Markets are typically constrained by well-specified and enforced rules—something the international system decidedly is not.

The absence of a clear consensus about the theories and models of conflict and intervention translates into the absence of an empirical consensus. Indeed, the two are clearly related in a scientific sense; empirical investigation should be weeding out those models that fail the test of evidence. While considerable progress has been

made on the empirical examination of both conflict and intervention, any consensus remains elusive even at the most fundamental levels of analysis.

Second, the research endeavour has not proceeded to the stage of refinement and qualification. Consequently, the margin of error for any associated policy suggestion is daunting. Any error has the potential implications for thousands of lives and millions of dollars, generating understandable reluctance to rely on any single formal model that may produce counter-intuitive results. In simplest terms, formal models of intervention largely remain untried, untested, and potentially not true.

The third barrier is simply one of the larger challenges for formal modeling arising from the lack of consensus. In economics, university courses are largely standardized and formal modeling is pervasive. Ironically, economics seems to suffer from the opposite problem as the study of conflict; common sense discursive analysis is viewed with extreme scepticism or dismissed entirely in the absence of corroborating formal models. Furthermore, the battle between competing ideas, methodologies, and normative standards is far more intense in international relations than in economics. As a result, there are conflicting schools of thought that are unwilling to acknowledge the legitimacy of their competitors. This absence of convergence is apparent in course structures, doctoral thesis expectations, and even journal refereeing. As long as there are large sections of the academic establishment that are incapable of understanding or producing formal models with mathematical representations, then there will be tremendous difficulty in forging a consensus on how they might be incorporated into policymaking.

Finally, the policymaking community itself will generate the same sort of resistance to applying formal models of intervention. Even when policymakers and modelers are drawn from the same discipline, usually the models have been tested empirically and translated into more accessible language prior to their emergence in any policy discussions. Disciplinary uniformity and complementarity would undoubtedly expedite this process in a number of ways. Firstly, policy makers can engage the theory and theory-builders directly. This aids in directing the terms of how the model may need to be modified, refined, or repackaged in order to be useful in policymaking. Secondly, the extent of these modifications may be minimized by the presence of a common analytical and terminological framework for discussion. Thirdly, the affinity of policymakers to modeling will be stronger if they have formal training that is in common with the modeler. Finally, there will be a natural bureaucratic reluctance to adopt novel techniques. Adoption may lead to failure, and there is a good chance that an external evaluator will disagree with the theory underlying the technique. It is harder to blame a single bureaucrat for a policy failure if he or she is following the prescriptions of a model with widespread currency.

FURTHER RESEARCH

A predictive capacity, based on dynamic theories of intervention and careful empirical work, can provide policy-relevant forewarning to interveners. This paper

highlights some of the theoretical and empirical challenges that emerge in identifying the consequences of intervention strategies. Addressing such challenges is crucial as current policy initiatives continue to race ahead of clear and precise strategic analysis.

As a next step, we suggest greater incorporation of findings from different methodologies and greater efforts at synthesis. Current models need to be placed within a typology in order to identify areas of distinction and similarity. Fearon and Powell begin this process. Once a typology has been created, classes of models can be developed, refined in a cumulative fashion, and gaps can be identified by theoretical innovation and the presence of empirical anomalies.

Current and future models then need to be subjected to more systematic empirical testing, starting with case studies. For readers, especially policymakers, who may be unfamiliar with formal modeling, a case study can provide an accessible and practical way of acquiring the insights of a model and its insights into causality. Simultaneously, irregularities between a model and a case study, while insufficient to refute propositions, can identify potential directions for model modification or refinement. By the same token, consistency between the model and the cases does not provide irrefutable support for the model; for this, we would need the confidence of larger sample studies. Druckman demonstrates how case study and large N study approaches can be synthesized.²⁵

The work of Maoz illustrates another useful direction for research in terms of addressing accumulation, integration and synthesis. By explicitly marrying micro and macro levels of analysis, a model can provide richness without sacrificing generality and vice versa.

The question of uncertainty points to the last research direction that needs to be addressed. Information plays an important role in game theory and in real life. Analyzing models for robustness, particularly with respect to variant assumptions on information, is critical for good policymaking. The recent furor over intelligence failures in both the 9/11 attacks and the 2003 invasion of Iraq demonstrates the centrality of information sets in determining behavior. Testing inferences for sensitivity to information both at the formal and empirical levels is critical. Testing also permits decision makers to examine risks with a better sense of probabilities and boosts confidence in the models that are underpinning policy choices. Ultimately, policymakers will adopt formal models when there is confidence in them. This will not occur until the academic community has that same requisite confidence that is born from sufficient accumulation, integration, and synthesis. These are the hallmarks of good scientific research.

Notes

¹ Portions of this paper were presented by the authors at a conference on Formal Models and Negotiation at the International Institute for Applied Systems Analysis, June 2004, Laxenberg, Austria, and will appear in the forthcoming: David Carment and Dane Rowlands, "Formal Models of Intervention," in *Diplomacy Games: Formal Models of, on, and for International Negotiation*, ed. Rudolf Avenhaus and I. William Zartman (London: IIASA); David Glenn, "Calculus of the Battlefield: Do game theory and number crunching—the New Math of international relations—shed light on the conflict with Iraq?" *The Chronicle*, November 8, 2002. Available at: <http://chronicle.com/free/v49/i11/11a01401.htm> (accessed January 11, 2007).

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- ²² For a discussion of the range of research see. Alvin Saperstein, "The Prediction of Unpredictability", in *Chaos Theory in the Social Sciences*, eds. Douglas Kiel and Euel Elliot (Ann Arbor, MI: University of Michigan Press, 1996).
- ²³ Bruce Bueno de Mesquita, "An Expected Utility Theory of International Conflict," *American Political Science Review* 74 (1980): 917–931; Bruce Bueno de Mesquita "The War Trap Revisited: A Revised Expected Utilities Model," *American Political Science Review* 79, no. 1 (1985): 156–173.
- ²⁴ David Glenn, "Calculus of the Battlefield: Do game theory and number crunching—the New Math of international relations—shed light on the conflict with Iraq?," *The Chronicle*, November 8, 2002. Available at: <http://chronicle.com/free/v49/i11/11a01401.htm> (accessed January 11, 2007).
- ²⁵ Daniel Druckman, "Four Cases of Conflict Management: Lessons Learned," in *Perspectives on Negotiation: Four Case Studies and Interpretations*, ed. Diane Bendahmane and John McDonald (Washington, DC: Foreign Service Institute, 1986). Four case-based research approaches to analysis of data on international negotiation are discussed: the single, analytical case study, the temporal or time-series case study, the focused comparison of a small number of similar cases, and aggregate comparisons of a large number of different cases.