The Contributions of Rational Choice: A Defense of Pluralism

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In "Rigor or Rigor Mortis? Rational Choice and Security Studies," Stephen Walt warns of the dangers to the field of security studies that are in store "if formal theory were to dominate security studies as it has other areas of political science."[1] He backs up these warnings by evaluating published formal work in the field according to seemingly reasonable criteria, finding that the gain in rigor inherent in formal work is not sufficient to offset its empirical, creative, and policy-relevance weaknesses. Although Walt ends with a plea for diversity (p. 48), the overall structure of his argument puts rational choice on trial, finds it lacking yet threatening to become dominant, and does little to serve the purpose of encouraging pluralism.

As a consumer rather than producer of sophisticated formal theory, I find Walt's critique of formal work off target and his worries about its imminent hegemony unfounded. My own work, as well as that of many other scholars, has benefited enormously from the theorizing of those who have better technical skills and the ability to work through complex mathematical models. The field of security studies would be severely impoverished if formal work were discouraged.

In this response, I make three arguments. The first is to highlight a signal strength of formal work that Walt neglects: its ability to generate linked, coherent sets of propositions and insights. Walt's analysis focuses entirely on individual, isolated hypotheses, finding them lacking in originality, empirical support, or policy relevance. This approach misses the importance of theory in providing insights that are logically connected to one another in an integrated analytical framework, a necessary condition for progress in social science. Second, I address the benefits of formalizing the insights of informal

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1. Stephen M. Walt, "Rigor or Rigor Mortis? Rational Choice and Security Studies," *International Security,* Vol. 23, No. 4 (Spring 1999), pp. 5–48, at p. 46. Further references appear in parentheses in the text.

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rational choice—the generic "Didn't Schelling already say that?" question. These benefits can be summarized as providing specificity to propositions and identifying the contingency of many hypotheses. Third, I address the "dominance" issue by looking at the numbers of articles published in leading journals between 1994 and 1998 that use sophisticated formal models. This review shows that there is no apparent danger of formal work becoming dominant in the field of security studies, calling the need for warnings such as Walt's into question. Better empirical testing of formal models is surely desirable—as is better empirical work in international relations and security studies in general. Singling out formal modeling as a threat to the field, however, is unfounded and does nothing to encourage diversity.

Coherence

The approach that Walt takes in his review of formal models is to single out particular hypotheses and propositions that have been derived in prominent examples of formal work. He argues that, taken individually, none of these insights is valuable enough to justify the technical complexity that went into producing them. I leave it to those authors who were singled out to respond directly to these claims, if they desire.

The point I wish to make here is a larger one, however. The value of formal theory, like any theory, does not lie primarily in its ability to generate isolated propositions, however original or empirically valid such assertions might be. Generating isolated propositions does not require a theoretical framework at all, much less the relatively elaborate framework of mathematical game theory. Any thoughtful observer of international affairs is likely to be able to generate a good insight here or there.

Social science, in contrast, relies on theory. The reason is fundamental to the drive to make social science a progressive, cumulative effort. Proving or refuting an isolated proposition has some value. But without an overarching analytical framework that generates *complexes* of related propositions, determining the empirical validity of a particular proposition is a dead end rather than a step toward cumulative knowledge. Social science does not consist simply of compiling lists of propositions and a tally of which are true, false, or undecided. Instead, it contributes to human knowledge by showing how sets of related propositions are tied to an underlying set of core assumptions and methods of analyzing social interaction. When a framework exists that ties together coherent sets of assumptions and propositions, one strong empirical

finding, positive or negative, has myriad implications. It is this multiplier effect that makes theorizing worthwhile and creates at least the possibility of cumulative knowledge.

This vision of embedded complexes of assumptions and propositions is common to any positivist philosophy of social science, and Walt's style of analysis makes clear that he is a positivist. I share this prejudice. But without an emphasis on theory, empirical testing of propositions can degenerate into a crudely inductive enterprise that is quickly made valueless by the next change of fortune in the practice of international politics. When propositions are tied together, whether in a Lakatosian research program or Kuhnian paradigm, as a set of insights that are commonly derived from coherent assumptions, empirical evidence on one insight leads to updating and deeper understanding of the others. It also contributes to our sense of whether the underlying set of assumptions is a good tool for making sense of the world. We lose this more holistic contribution—the notion that the whole is greater than the sum of its parts—if we neglect the value of an integrated analytical framework. Evaluating a theory only in terms of its ability to generate isolated new, empirically proven, and policy-relevant propositions is akin to stripping a car for parts. One is left without a vehicle that can go anywhere, just a pile of unconnected scrap.

If we can agree on the value of integrated theoretical frameworks for promoting deeper understanding and progress in security studies, the next question is whether formal modeling has any benefits when it comes to providing such an integrated framework. While I do not deny that other methods have the capacity to generate integrated complexes of assumptions, insights, and testable propositions, formal work must be rated very high by this standard. It provides a deductive, logically coherent method for relating assumptions and hypotheses to each other. It forces researchers to make their assumptions explicit, and provides a tool for understanding the extent to which hypotheses are robust or highly sensitive to particular assumptions. It exposes logical flaws in more informal arguments that can degrade their ability to generate coherent complexes of insights. It also creates the potential for different fields within the social sciences to speak to one another. Firmer integration of security studies within the discipline of political science is encouraged by the use of formal methods.[2]

As an example of the contributions of formal work, take the literature on reputation and credibility. Walt reviews a number of articles based on this

2. My thanks to Celeste Wallander for suggesting this point.

literature, extracting a few propositions from each of them. He derides each of these propositions as a reinvention of the wheel, confirmation of what we already know, or simply not a big deal. Many of these assertions could be questioned on their own terms. More important, however, is that by focusing only on these specific propositions we miss the larger picture that is created by formal analyses of reputation and credibility. This larger picture ties together understandings about the role of uncertainty; the importance of initial beliefs; the dynamics and outcomes of updating beliefs; the selection of deterrent or challenging strategies; the availability of actions that impose differential costs on different types of actors; the importance of a common-knowledge framework in which to interpret signals about resolve; the significance of repetition and the shadow of the future; the likelihood of success for different strategies; the ability of different types of actors to mimic one another or bluff; and others.

Some of these insights make up the direct implications of a model—that is, what it was designed to illuminate in the first place. Others are indirect implications of the same models. The important point here is that a *set* of direct and indirect implications flow from any particular model. The process of empirical validation, or application to particular policy challenges, should take into account the integrated nature of these implications. A strong positive or negative finding on any particular proposition has implications for many others, and for the assumptions that drove the model in the first place. The act of formalization therefore dramatically increases the empirical leverage researchers can apply, and provides direction to research efforts that is lacking if research is treated as simply going down a laundry list of propositions.

Specificity and Contingency

Walt raises a concern that I hear repeated frequently by graduate students and colleagues in many different settings. I think of this as the generic "Didn't Schelling already say that?" question. The point of this critique is to show that informal analysts of strategy have been able to produce insight without the benefit of mathematics. Walt hopes to differentiate between informal and sophisticated formal versions of rationalist analysis, putting a high value on the former but criticizing the latter. If Thomas Schelling and Albert Wohlstetter could generate deep insights, the argument goes, what is the value in using more sophisticated mathematical techniques? There are many answers to this question, but here I single out a few of the most general.

The first was summarized in the previous section. Although informal theorizing about strategy has led to important insights, formalizing these insights provides an overarching analytical framework that is valuable in many ways. It suggests new propositions and shows how propositions are related to one another. It forces the analyst to make assumptions and conditions more explicit. In short, it provides a logic of analysis that allows for more reliable progress in our social-scientific endeavors.

Second, any claim that Schelling or Wohlstetter came to their conclusions without being influenced by formal mathematical approaches is totally unfounded. Wohlstetter's background, for example, was in operations research and systems analysis, both highly mathematical and technically complex fields. Although it is true that Wohlstetter had a genius for translating mathematical insights into (relatively) plain English in his RAND studies, this is not to say that he could have produced these studies if deprived of his mathematical background. Many of us who work in the rationalist tradition, but do not spend much time developing our own sophisticated models, are in a similar situation. Our theories and propositions have been deeply influenced by the more technical work of others, and this debt should be made more explicit rather than dismissed. Tracing the chain of causation to show the impact, or nonimpact, of mathematical reasoning on informal studies of strategy would require much deeper analysis than Walt provides. The fact that some good papers on strategy do not themselves include formal models by no means demonstrates that formal modeling made no contribution to these papers.

Third, and the major point I wish to make in this section, formalization adds specificity and contingency to the claims that authors such as Schelling have made. Many of the propositions Schelling advances are brilliant and intuitively plausible. Unfortunately, he also at times advances precisely the opposite proposition, and it appears equally brilliant and plausible. The difficulty here is not that either proposition is wrong. It is that the propositions are often stated without the degree of specificity that is necessary to evaluate their logical or empirical validity, or that they are true only under particular unspecified conditions. Formalization greatly increases specificity, and few methods are better for specifying the conditions under which particular propositions hold. As scientific fields, natural or social, develop, they invariably become more mathematical.[3] This is not the result of a conspiracy of the mathematically

3. Walt approvingly cites Darwin's theory of evolution to demonstrate that seminal theories in the natural sciences are not always mathematical (p. 16). But this claim neglects the fact that evolutionary biology has developed substantially since Darwin and in some areas has become highly mathematical.

gifted or a guildlike attempt to exclude outsiders. It is in part because math provides the tools to sort out apparently conflicting claims by specifying the conditions under which they hold. Using mathematical tools is a method for adding specificity and contingency to the seminal claims of informal theorists.

To take one example, consider Schelling's influential analyses of the problem of credibility in *The Strategy of Conflict* and *Arms and Influence*.[4] In *Strategy and Conflict*, we find one of Schelling's most cited ideas: tight constraints on negotiators that result from domestic politics can enhance credibility and prove an asset in international bargaining. According to Schelling, "If the executive branch negotiates under legislative authority, with its position constrained by law, and it is evident that Congress will not be reconvened to change the law within the necessary time period, then the executive branch has a firm position that is visible to its negotiating partners."[5] In *Arms and Influence*, Schelling argues that "the centralization of decision" and "the divorce of war from political processes"[6] enhance the credibility of the threat to use nuclear weapons once war has begun.

On their face, these two claims appear to contradict each other. It is obvious, however, that Schelling is considering two very different settings in coming to these apparently contradictory statements. It seems entirely plausible that at times decentralized political processes are likely to enhance credibility, while at other times centralization will be an asset. It is difficult, however, to sort out the conditions under which one statement versus the other is true without more carefully specifying assumptions and working through a formal assessment of the credibility of threats under different conditions of strategic interaction, iteration, beliefs and capacities of other actors, and so on. In other words, formalization would allow us to move toward the conditional propositions about credibility that are implied by Schelling's analysis. In addition, it is possible that what exactly is meant by "credibility" differs in these two examples. Precisely defining credibility and adding specificity to the propositions above could constitute another justification for formalizing Schelling's ideas.

One could perform the same kind of analysis for Schelling's discussions of costless communication (what we would today call "cheap talk") and costly moves. Developments in modern game theory have led to a much better

4. Thomas C. Schelling, *The Strategy of Conflict* (Cambridge, Mass.: Harvard University Press, 1980); and Schelling, *Arms and Influence* (New Haven, Conn.: Yale University Press, 1966).
5. Schelling, *Strategy of Conflict*, p. 28.
6. Schelling, *Arms and Influence*, p. 20.

understanding of the conditions under which cheap talk can make a difference for outcomes, and when costly signals can. Similarly, Schelling's discussion of the qualitative nature of useful focal points versus the benefits of randomization (a quantitative concept) could perhaps be sorted out and developed into testable propositions via the application of formal methods.

None of the above should be taken as a criticism of early informal analyses of strategy. These analyses were indeed seminal, and continue to influence policy and research today. The point of a "seminal" idea, however, is precisely that it stimulates future work, leading later analysts to refine and develop the initial idea. Using formal methods to pursue such development is often a highly productive enterprise, as it allows analysts to specify ideas more precisely and to make contingent claims about the conditions under which particular relationships should hold. Pointing out that those who use mathematical approaches are often building on the seminal ideas of informal analysts by no means undermines the value of formal methods. Instead, it suggests that standard procedures of scientific progress are being applied.

The Role of Formal Theory in Security Studies

One of the more disturbing aspects of Walt's article is the sense of imminent danger that it assumes and attempts to spread. The article contains repeated references to what would happen if formal theory were to take over the field. I am a committed proponent of diversity. Different approaches have different strengths and weaknesses. The field of security studies has by no means yet approached the status of "normal science," where one particular approach has proven its superiority and is commonly used by most researchers.

If the point is to encourage pluralism and tolerance, an attack on one particular approach must be based on the presumption that it is overrepresented. This is one issue where we can benefit from examining some concrete facts. How prevalent is the use of sophisticated formal models in security studies? Is it prevalent enough that concerns about its impending dominance are justified?

To answer this question, I undertook a survey of articles on international security issues published in the leading journals in the field between 1994 and 1998. I then calculated the percentage of articles that presented a formal model, using Walt's definition.[7] Table 1 presents these results.

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7. Walt (p. 9) narrows his analysis to scholarship that uses "specific mathematical models" to identify equilibrium outcomes. He excludes analyses that merely draw on game-theoretic concepts.

Table 1. Articles on International Security with a Formal Model, 1994–98.

Journal	Number of Security Articles	Number of Formal Articles	Percentage of Formal Articles
American Political Science Review	26	8	30.8
International Organization	43	5	11.6
International Security	113	0	0
International Studies Quarterly	83	11	13.3
Journal of Conflict Resolution	152	42	27.6
Security Studies	105	0	0
World Politics	21	5	23.8
Total	543	71	13.1

In collecting the data for Table 1, I used the following rules. I assumed that all of the articles in *International Security,* the *Journal of Conflict Resolution,* and *Security Studies* counted as studies of international security. This rule probably led to an overcounting of the number of formal articles, because some of the formal articles published in the *Journal of Conflict Resolution* are pure game theory, without application to a particular issue, or are on areas of conflict other than international security. However, to avoid making judgment calls that could result in undercounting formal articles, I treated the entire journal as being about security issues. I eliminated publications that were book reviews or exchanges with authors; again, including these would have reduced the percentage of publications that were formal. Following Walt's lead, I did not include as formal articles those that drew informally on ideas such as the prisoners' dilemma or mixed strategies.

As Table 1 shows, more than 500 articles were published in these seven journals in this five-year span. Of these articles, 71, or 13.1 percent, presented a formal model.[8] Looking at the breakdown by journal, we see that 42 out of these 71—nearly 60 percent—were published in just one journal, the *Journal of Conflict Resolution*. The journal has clearly selected a niche for itself, becoming by far the most common outlet for formal work in security studies. The other two journals that specialize in security studies, *International Security* and *Secu-*

8. One could argue that acceptance rates would be more revealing than publication rates. Such data were not available on short notice, however. In addition, because authors rationally choose where to submit their manuscripts, any data on acceptance rates would be subject to severe selection bias.

rity Studies, have not published a single article using a formal model in the last five years. The less specialized journals show variation in the percentage of security articles they publish that use a formal model. This figure ranges from a low of 11.6 percent for *International Organization*[9] to a high of 30.8 percent for the *American Political Science Review.*

Looking at these figures, it is hard to work up much of a sense of urgency about the impending dominance of formal work in security studies, or to support a claim that those who do not do formal work are becoming "marginalized" (p. 7). Outside of the *Journal of Conflict Resolution,* fewer than 30 articles using a formal model have been published in the last five years. Overall, formal modeling remains a small piece in the fragmented universe of security studies. Given these numbers, and the contributions of formal modeling discussed above, it seems more appropriate to encourage formal work than to broadcast warnings about how its dominance might doom the field.

Conclusion

Walt's article has raised a series of challenges to those who produce sophisticated formal work in security studies. While presenting these challenges as a plea for diversity and tolerance, Walt adopts an approach that is likely to generate just the opposite: conflict, defensiveness, and countercharges. Many of Walt's claims are stated in comparative terms—for example, that empirical work to test formal models is weaker than that to test other approaches, or that formal models have less policy relevance than do other models. None of the analysis undertaken, however, is at all comparative. Three criteria are specified—logical consistency, creativity, and empirical validity—but in the end, no formal model is held to be valuable unless it makes a direct contribution to policy debates, regardless of its logical consistency relative to other approaches.[10] In other words, these three criteria are by no means weighted

9. It is somewhat surprising that *International Organization* has published 43 articles on security in this period, considering that it is usually understood as primarily an international political economy journal.

10. After admitting that formal methods encourage precision and logical consistency, Walt dismisses these criteria as not demonstrating "the superiority of formal techniques over other approaches" (p. 20). But surely they would imply superiority, all else being equal. One can only conclude that precision and consistency in practice receive little weight in Walt's calculation. In addition, Walt does not address how internally inconsistent and logically incomplete propositions can make a positive contribution to policy debates, except by stimulating others to do better.

equally, and they are not applied in a systematic manner to alternative approaches in security studies.

The most basic weaknesses of Walt's broadside, however, lie not in his specific arguments, but in errors of omission and in unsubstantiated presumptions. Errors of omission lie in the criteria chosen for evaluating theories. Two criteria that would have worked in favor of formal models are neglected. The first is the capacity of an approach to generate integrated, coherent complexes of assumptions and propositions. Extracting a few isolated propositions from models and deriding them for being insufficiently original or proven misses this essential characteristic of a progressive research agenda. The second is the ability to add the necessary elements of specificity and contingency to individual propositions to allow for accurate assessments of their empirical validity. Informal analyses of strategy have provided numerous brilliant insights, but they are often contradicted by their equally brilliant opposites. Formalization provides a technique for sorting out these apparent inconsistencies.

Finally, the presumption that formal work is becoming a prerequisite for publication in security studies, or is in any way threatening to become dominant, has no foundation in fact. A survey of articles on security published in the leading professional journals between 1994 and 1998 shows that only 13 percent of published articles present a formal model, and that nearly 60 percent of those that do have come out in just one journal. Given this reality, the claim that a wide-ranging attack on the contributions of formal models is in fact a plea for tolerance seems disingenuous.