

The Modeling Enterprise and Security Studies

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The modeling enterprise is a way of trying to improve our understanding of empirical phenomena. Models serve in this enterprise as a tool for disciplining our thinking about the world, and formal models instill a particular type of discipline. Formalization provides a kind of “accounting standard” that can often help us think through some issues more carefully than ordinary-language arguments can. Just as good accounting standards make a firm’s financial situation more transparent to those inside the firm and those outside it, formalization makes arguments more transparent to those making them and to those to whom they are made. When mathematical models are well constructed, they offer us a relatively “clear and precise language for communicating ideas and intuitions.”¹

The contribution that such a standard has to offer to security studies is likely to appear small to those who believe that nonformal or traditional work has already proved its power by amassing a large number of well-established empirical regularities and theoretical explanations of them. By contrast, the benefit of a more transparent standard will seem much higher to those who believe that security studies, like much of international relations theory, has established few robust empirical regularities; to those who have been frustrated to see that almost any outcome can be “explained” after the fact in a way that makes it consistent with existing theories; and to those who have repeatedly tried to formalize many widely held ordinary-language arguments in international relations theory (e.g., anarchy induces a concern for relative gains, anarchy leads to a tendency to balance, and a balance of power is more stable than a preponderance of power), only to find that these arguments are, at best, seriously incomplete and in need of significant qualification.

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1. David Kreps, *Game Theory and Economic Modeling* (New York: Oxford University Press, 1990), p. 6. This book also provides an excellent and accessible overview of some of game theory’s contributions and weaknesses.

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Like most tools, formal models do some things well and others less so.² Nevertheless, Stephen Walt denies in “Rigor or Rigor Mortis?” that he is comparing “the relative merits of formal theory with other methodological approaches.”³ I, however, have trouble reading his article any other way. Indeed, a few lines before this denial he seems to say the opposite: “recent formal work has *relatively* little to say about contemporary security issues” (p. 8, emphasis added). And when discussing the originality of the contribution of formal work a few pages later, he also claims, “When *compared* to other research traditions, however, their [formal rational choice theorists’] production of powerful new theories is not very impressive” (p. 22, emphasis added).

My views differ. In the next three sections, I draw on major works taken from nonformal security studies to discuss the issues of reproducibility and transparency (which touch on many of the issues Walt considers under the label “logical consistency”), originality, and empirical evaluation. My purpose is threefold. First, I want to suggest that there are significant foundational problems with many of the most important, widely held arguments in security studies and international relations theory. Even if tightening the connections between assumptions and conclusions were all that formal theory had to offer, this would be a very important contribution at this stage in the development of these fields. After all, these arguments are presumably the intellectual bedrock for more policy-relevant analyses. Second, I believe that when one compares the contribution to security studies of the latest wave of formal theory, which began in the mid-1980s, to that of nonformal theory, the former holds up quite well. Formal theory has made important original contributions, and many formal theories are being tested empirically. Third, I want to show that one can still obtain a badly distorted picture of an entire literature even if one examines only major contributions. No one should judge the rational choice literature on the basis of Walt’s summary of eleven examples; nor should anyone judge the overall contribution of mainstream security studies on the basis of the few examples discussed below. One should read the original work with an open mind after attaining some basic background in game theory.⁴

2. Paul Krugman, *Development, Geography, and Economic Theory* (Cambridge, Mass.: MIT Press, 1995), offers a thoughtful discussion of the costs and benefits of modeling.

3. 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. 8. Additional references appear parenthetically in the text.

4. Walt worries about the accessibility of game theory, and so do I. But unlike a decade ago, introductory texts are now available to those willing to make a modest investment of time. See,

Finally, a qualification is in order. Walt's article stretches across forty-four pages, whereas I was invited to write a ten-page response. The following discussion will therefore seem abbreviated and perhaps gratuitous at times. To mitigate this, I refer readers at several points to my new book, *In the Shadow of Power*, where I coincidentally address at greater length many of the issues Walt raises.⁵

Reproducibility and Transparency

Reproducibility is an essential element of science. But the importance of reproducibility is not limited to empirical or experimental findings. It also applies to theoretical arguments: if a theoretical argument is given to a group of experts, they should in some sense be able to reproduce it. They should be able to identify the key assumptions and the sequence of steps that lead from those assumptions to the purported conclusions. These experts should also be able to agree if one step follows deductively from previous steps or if it is really an additional assumption.

To be reproducible, arguments need to be transparent. Many ordinary-language arguments in international relations theory lack transparency, however, and this has impeded the development of the field. As an illustration of the lack of transparency and the inability to determine what follows from what and why, consider John Mearsheimer's discussion of realism in "The False Promise of International Institutions."⁶ Mearsheimer argues that realism's "pessimistic view of how the world works can be *derived* from realism's five assumptions about the international system" and that "three main patterns of behavior result."⁷ The third is that "states aim to maximize their relative power positions over other states."⁸ But he then qualifies this derivation in a footnote: "There is disagreement among realists on this point. Some realists argue that states are principally interested in maintaining the existing balance of power, not maximizing relative power."⁹

How is it possible for realists to disagree about a point if it is truly *derived* from realism's basic assumptions? What accounts for these different "deriva-

for example, James D. Morrow, *Game Theory for Political Scientists* (Princeton, N.J.: Princeton University Press, 1994).

5. Robert Powell, *In the Shadow of Power* (Princeton, N.J.: Princeton University Press, 1999).

6. John J. Mearsheimer, "The False Promise of International Institutions," *International Security*, Vol. 19, No. 3 (Winter 1994/95), pp. 5–49.

7. *Ibid.*, pp. 10, 11 (emphasis added).

8. *Ibid.*, p. 11.

9. *Ibid.*, n. 27.

tions"? Is it some other (unstated) assumption that, given the importance of the claim, must surely also count as one of realism's basic assumptions? Without a transparent argument, we have no way of knowing.

The ability to determine what follows from realism's basic assumptions is, moreover, terribly important for both theoretical and policy reasons.¹⁰ To test a theory, we need to be able to compare empirical findings with theoretical predictions, and this is impossible if we cannot tell what the theory predicts. Furthermore, policy analyses based on the belief that states "aim to maximize their relative power positions" are likely to be seriously misguided if states actually "are principally interested in maintaining the existing distribution of power" and vice versa.

As a second example of the need for greater transparency, consider Walt's discussion of my "Absolute and Relative Gains in International Relations" and the role that the cost of fighting plays in that analysis.¹¹ In the simple model I develop in that article, the size of this cost determines whether or not states cooperate. Walt, in turn, claims that this cost is "essentially identical to the concept of the offense-defense balance" (p. 27).¹²

Perhaps so. But one natural formulation of the offense-defense balance is to ask, as Robert Jervis does in his seminal article "Cooperation under the Security Dilemma": "With a given inventory of forces, is it better to attack or to defend?"¹³ The larger the difference between the payoffs to attacking and to being attacked, the larger the offensive advantage. Expressing this formally suggests that there may be an important analytic distinction between the overall cost of fighting and the offense-defense *balance*. Suppose there are two states S_1 and S_2 with military inventories m_1 and m_2 , respectively. Now take $p_A(m_1, m_2)$ and $p_D(m_1, m_2)$ to be the probabilities that S_1 prevails if it attacks and if it is attacked. Finally, let the payoffs to prevailing and losing be, $1 - c$, and, $-c$, where c is the cost of fighting. Then the expected payoff to attacking is the payoff to winning weighted by the probability of winning plus the payoff to losing times the probability of losing. In symbols, the payoff to attacking is $A = (1 - c) \times p_A(m_1, m_2) + (-c) \times (1 - p_A(m_1, m_2)) = p_A(m_1, m_2) - c$. Similarly,

10. See Charles L. Glaser, "Realists as Optimists: Cooperation as Self-Help," *International Security*, Vol. 19, No. 3 (Winter 1994/95), pp. 50–90, for a nonformal effort to trace these consequences. See also Powell, *In the Shadow of Power*, for a more formal effort.

11. Robert Powell, "Absolute and Relative Gains in International Relations," *American Political Science Review*, Vol. 85, No. 4 (December 1991), pp. 1303–1320.

12. Mearsheimer, "The False Promise of International Institutions," p. 22, makes a similar comment.

13. Robert Jervis, "Cooperation under the Security Dilemma," *World Politics*, Vol. 30, No. 2 (January 1978), p. 188.

the payoff to being attacked is $D = p_D(m_1, m_2) - c$. Thus the offense-defense balance (i.e., the difference between the expected payoffs to attacking and to being attacked) is $A - D = p_A(m_1, m_2) - p_D(m_1, m_2)$. This, however, implies that the offense-defense balance does not change if the overall cost of fighting does. Changes in this cost are therefore analytically distinct from the offense-defense balance in this formulation.¹⁴ Whether an increase in this cost and a shift in the offense-defense balance in favor of the latter have similar effects is a conjecture that needs to be investigated theoretically and empirically.¹⁵

Of course, the empirical effects of a shift in the overall cost of war do not depend on how we define the offense-defense balance and whether or not we incorporate these costs in that definition. Definitions are not given a priori and should be judged by their theoretical usefulness.¹⁶ Walt's inclusion of the cost of fighting in the offense-defense balance suggests that he has a different formulation in mind than the one I have just sketched. But absent a clear specification, it is impossible to tell what that formulation is and what its empirical implications are. Indeed, this is all the more confusing because Walt, like me, cites Jervis's seminal article as the basis for his analysis.

Whether formalization contributes to transparency and reproducibility, and thereby helps further the development of international relations theory, is a pragmatic judgment. Walt and I agree that "formalization is neither necessary nor sufficient for scientific progress" (p. 15). Any formal argument can be translated into ordinary language. One can write out a mathematical equation as an English sentence. Thus any conclusion derived from a formal analysis can in principle be derived from an ordinary-language argument. But, what is possible in principle may not be so in practice. The ordinary-language translations are likely to be long and complicated and difficult to work with.

14. If the cost of fighting on the offensive differs from the cost of fighting on the defensive, then the offense-defense balance is $A - D = p_A(m_1, m_2) - p_D(m_1, m_2) - (c_A - c_D)$, where c_A and c_D are the costs of attacking and defending. Again, the offense-defense balance does not change if the overall cost of fighting rises, that is, if c_A and c_D increase by the same amount. Similarly, Charles L. Glaser and Chaim Kaufmann define the offense-defense balance as "the ratio of the cost of the forces that the attacker requires to take territory to the cost of the defender's forces," in "What Is the Offense-Defense Balance and Can We Measure It?" *International Security*, Vol. 22, No. 4 (Spring 1998), p. 46. If, therefore, the costs of fighting on the offensive and defensive rise proportionately, the overall cost increases, but the ratio of these costs and consequently the offense-defense balance, remain constant.

15. Changes in the cost of fighting and the offense-defense balance do have different effects in James D. Fearon, "Bargaining over Objects That Influence Future Bargaining Power," unpublished manuscript, University of Chicago, 1996. See also Powell, *In the Shadow of Power*, for an effort to trace the implications of changes in the cost of fighting and in the offense-defense balance.

16. For a recent discussion of how to define the offense-defense balance, see Glaser and Kaufmann, "What Is the Offense-Defense Balance and Can We Measure It?"

(Indeed, this is the reason for adopting a more formal language.) In practice, recent formal work has produced more transparent and reproducible arguments that show, among other things, that a costly signaling formulation fits the data on deterrence success and failure better than traditional balance-of-interests or balance-of-capabilities arguments do; that the claim that a balance of power is more stable than a preponderance of power needs significant qualification; that the standard argument that anarchy induces a concern for relative gains does not work very well theoretically or empirically; and that the received argument that states generally balance (whether against power or threat) whenever the system is anarchic and populated by units that seek to survive is at best very fragile and often fails to hold.¹⁷

Originality

Walt conflates two issues in his discussion of originality and formal theory. The first deals with the source of new ideas, and the second is whether work that uses formal models has made a substantial original contribution. As for the first issue, I do not know where deep insights and new ideas come from, and I see no reason to believe that formal theory generally enjoys any “particular advantage as a source of theoretical creativity” (p. 30). The transparency of a model may sometimes spark a new idea for some scholars. But I, like Walt, also believe that “case studies can be an extremely fertile source of new theories” or ideas (p. 31).

The multiplicity of sources of new ideas, however, is not the point. The modeling enterprise is about disciplining our thinking about our ideas regardless of where they come from. Models help forge tighter links between those ideas and their empirical implications, which is an essential step in testing and developing those ideas.

As for the second issue, research, whether formal or not, generally builds on what has come before it. Consequently, judgments about what does and does not constitute an original contribution tend to be subjective. Walt believes that

17. For a sampling of formal work on these problems, see James D. Fearon, “Rationalist Explanations of War,” *International Organization*, Vol. 49, No. 3 (Summer 1995), pp. 379–414; Fearon, “Signaling versus the Balance of Power and Interests,” *Journal of Conflict Resolution*, Vol. 38, No. 2 (June 1994), pp. 236–269; James D. Morrow, “When Do ‘Relative Gains’ Impede Trade?” *Journal of Conflict Resolution*, Vol. 41, No. 1 (February 1997), pp. 12–37; Robert Powell, “Stability and the Distribution of Power,” *World Politics*, Vol. 48, No. 2 (January 1996), pp. 239–267; and Powell, *In the Shadow of Power*.

the formal literature in security studies suffers from a “lack of originality” (p. 23) and briefly summarizes eight examples to try to make his point.

Because of space limitations, I can mention only one. Walt discounts the originality of the work on costly signaling because “the basic idea is virtually identical to Robert Jervis’s distinction between ‘signals’ and ‘indices,’ which he laid out more than twenty-five years ago” (p. 29) in *The Logic of Images in International Relations*.¹⁸ Jervis’s book does make many original contributions. But it seems slightly extreme to suggest that no original work on a subject can be done once a key distinction has been made. After all, John Herz and Herbert Butterfield discussed the basic idea of the security dilemma more than twenty-five years before Jervis’s seminal analysis of it.¹⁹

In “Signaling versus the Balance of Power and Interests,” James Fearon differentiates between ex ante and ex post indicators of resolve. An ex ante indicator is a costly signal that is observable before a crisis (e.g., an alliance or foreign assistance), whereas an ex post indicator is a costly signal that is observable only after a crisis begins (e.g., escalation). Both then would seem to be indices in Jervis’s terms.²⁰ The distinction Fearon is making is *not* the same one that Walt attributes to Jervis. Fearon, moreover, goes on to derive specific hypotheses—for example, that ex ante indicators should be positively correlated with general deterrence success but negatively correlated with immediate deterrence success—that actually fit the data better than the received arguments based on the balance of interests or power do. At least by my reading, this contribution is not in *The Logic of Images in International Relations*.²¹

18. Robert Jervis, *The Logic of Images in International Relations* (New York: Columbia University Press, 1970).

19. John Herz, “Idealist Internationalism and the Security Dilemma,” *World Politics*, Vol. 2, No. 3 (January 1950), pp. 157–180; and Herbert Butterfield, “The Tragic Element in Modern International Conflict,” *Review of Politics*, Vol. 12, No. 2 (Winter 1950), pp. 147–164. Arguing, correctly in my view, that Jervis makes a major, original contribution to the security dilemma by linking it to the offense-defense balance is Charles L. Glaser, “The Security Dilemma Revisited,” *World Politics*, Vol. 50, No. 1 (October 1997), pp. 171–201.

20. Jervis’s discussion of the differences between “signals” and “indices” combines several distinctions that make it difficult to determine if a costly action that a resolute actor would be willing to take but an irresolute actor would be unwilling to take is a “signal” or an “index.” For his discussion of these terms see, *The Logic of Images in International Relations*, especially, pp. 18–40.

21. Walt is aware of Fearon’s article but treats it oddly. He discusses Fearon’s article in the context of logical consistency, where he says that it suggests “new ways to interpret a body of empirical data,” but does not mention this article later when discussing formal theory’s originality or its emphasis on empirical validity. See Walt, “Rigor or Rigor Mortis?” p. 15 (emphasis added), for quotation.

Empirical Validity

Walt and I and, as far as I know, all formal modelers agree that the ultimate goal of theory is “to explain real events in the real world” (p. 31). “Mere logical consistency is not sufficient” (p. 32). But Walt and I see different things when we look broadly at the literature. By Walt’s count, about 40 percent of the formal articles published in the four major international relations journals between 1989 and 1998 contain systematic empirical tests. He interprets this as evidence that “empirical testing is not a central part of the formal theory enterprise” (p. 33), whereas I see it as evidence of exactly the opposite. Limitations of space prevent me from an extensive discussion of why some formal and nonformal articles may not and, ideally, should not contain systematic empirical tests. Suffice it to say that one reason is that the modeling enterprise often develops through a series of models in which the early models may be very suggestive and insightful but are not tested systematically, just as important ideas in the nonformal literature are often first presented with only brief historical illustrations or a single “plausibility probe.”²²

Walt and I also see different things when we look at specific examples. In my view, Kenneth Waltz’s *Theory of International Politics* and Jervis’s “Cooperation under the Security Dilemma” are two of the most important and influential pieces published within the last twenty-five years. But neither of these works presents systematic empirical tests of the propositions it develops; nor do they provide a large-*N* statistical test or offer carefully constructed and executed comparative case studies.

But so what? Systematic empirical testing was not the primary goal of those individual contributions, and I do not infer from its absence that those authors or the literature as a whole is uninterested in systematic empirical evaluation. Similarly I, like Walt, believe that Fearon’s “Domestic Political Audiences and the Escalation of International Disputes”²³ “offers an interesting and intuitively plausible conjecture about crisis bargaining, one well worth further exploration” (p. 24). But just as I do not infer from the absence of systematic tests in *Theory of International Politics* or “Cooperation under the Security Dilemma” that Waltz, Jervis, or the nonformal literature as a whole is uninterested in empirical evaluation, I do not infer from the absence of a systematic test in

22. See Powell, *In the Shadow of Power*, pp. 23–38, for a discussion of the modeling enterprise.

23. James D. Fearon, “Domestic Political Audiences and the Escalation of International Disputes,” *American Political Science Review*, Vol. 88, No. 3 (September 1994), pp. 577–592.

Fearon's article that he or the formal literature as a whole is uninterested in systematic empirical testing.²⁴ In fact, these ideas are being tested.²⁵

Conclusion

I conclude with two points. First, Walt believes that "rational choice theorists have been largely absent from the major international security debates of the past decade (such as the nature of the post-Cold War world; the character, causes, and strength of the democratic peace; the potential contribution of security institutions; the causes of ethnic conflict; the future role of nuclear weapons; and the impact of ideas on strategy and conflict)" (p. 47). I suppose this depends on how one defines "largely absent," but in my view, formal rational choice work is actively contributing to the research on the democratic peace, ethnic conflict, domestic institutional reform, collective security and international institutions, and so on.²⁶

Finally, Walt's article and mine are exercises in rhetoric; both exemplify the worst in research design. Walt makes comparative claims about the relative merits of different approaches without presenting systematic comparative evidence, and I carefully select my cases on the dependent variable to counter his

24. Indeed, I would find the idea that Fearon's work could in any way be used to exemplify a lack of concern with empirical testing preposterous were that assertion not appearing in the lead article in a major journal.

25. See, for example, Joe Eyerman and Robert Hart, "An Empirical Test of the Audience Cost Proposition," *Journal of Conflict Resolution*, Vol. 40, No. 4 (December 1996), pp. 597–616; Christopher Gelpi and Michael Griesdorf, "Winners or Losers? Democracies in International Crisis, 1918–1988," paper presented at the annual meeting of the American Political Science Association, Washington, D.C., August 28–31, 1997; Gelpi and Joseph Grieco, "Democracy, Crisis Escalation, and the Survival of Political Leaders, 1918–1992," paper presented at the annual meeting of the American Political Science Association, Boston, Massachusetts, September 3–6, 1998; and Kenneth Schultz, "Do Democratic Institutions Constrain or Inform?" *International Organization*, Vol. 53, No. 2 (Spring 1999), pp. 233–266.

26. See, for example, Bruce Bueno de Mesquita and David Lalman, *War and Reason* (New Haven, Conn.: Yale University Press, 1992); Bueno de Mesquita, James D. Morrow, Randolph M. Siverson, and Alastair Smith, "An Institutional Explanation of the Democratic Peace," *American Political Science Review*, Vol. 93, No. 4 (forthcoming, December 1999); George Downs and David Rocke, *Optimal Imperfection?* (Princeton, N.J.: Princeton University Press, 1995); Fearon, "Domestic Audience Costs and the Escalation of International Disputes"; Fearon "Commitment Problems and the Spread of Ethnic Conflict," in David A. Lake and Donald Rothchild, eds., *The International Spread of Ethnic Conflict* (Princeton, N.J.: Princeton University Press, 1998), pp. 107–126; Fearon and David Laitin, "Explaining Interethnic Cooperation," *American Political Science Review*, Vol. 90, No. 4 (December 1994), pp. 715–735; Morrow, "Modeling International Institutions," *International Organization*, Vol. 48, No. 3 (Summer 1994), pp. 387–423; and Schultz, "Do Democratic Institutions Constrain or Inform?"

points.²⁷ Moreover, neither of us is a disinterested observer; consequently, we tend to see what we want or expect to see, especially because we are not employing any method to help discipline our thinking. No social scientist would take Walt's assessment or mine seriously based on the "evidence" presented. If one wants to assess the relative strengths and weaknesses of the traditional and formal approaches to security studies and international relations theory, one needs to read the work.

27. Walt (p. 8) claims to mitigate this bias by "focusing on some of the best and most widely cited work." But note that I have referred only to some of the best and most widely cited work in nonformal security studies. Observe further that Walt sometimes treats his examples strangely (see, e.g., footnote 21 above).