

All Mortis, No Rigor

Frank C. Zagare

Like Caesar's view of Gaul, Stephen Walt's evaluation of the recent rational choice literature in strategic studies is divided into three parts.¹ But all the king's horses and all the king's men could not put his article back together again: the analysis of the second section does not follow from the first, and the conclusions of the third cannot be drawn from the second. In the end, Walt's discussion provides a clear illustration of why formal models are so valuable: they provide the strongest possible protection against improper argumentation.

Walt's first section is a reasoned and balanced discussion of the underlying premises of rational choice theory and the rationale for formal modeling. In fact, Walt's summary of the foundations of this methodological technique is refreshing. Unlike many other efforts to evaluate the contributions of game theory to international affairs, it is no caricature.² Also uplifting is the absence of vitriol that turned one recent exchange between scholars into an intellectual food fight.³

Walt begins by noting the usefulness of mathematical models in ensuring logical consistency, one of three criteria he lists as important for evaluating theories and bodies of literature. Insightfully, he recognizes that the formal literature is not monolithic, that there are important differences among those who use game theory to analyze international politics. As well, Walt's discussion demonstrates a sophisticated understanding of the rationality postulate. Although he does not discuss the issue explicitly, he does not fall into the

Frank C. Zagare is Professor and Chair of the Department of Political Science at the University at Buffalo, State University of New York. He is author of The Dynamics of Deterrence (Chicago: University of Chicago Press, 1987) and numerous articles applying game theory to international security affairs. He is currently writing a book with D. Marc Kilgour titled Perfect Deterrence (New York: Cambridge University Press).

I wish to thank Bruce Bueno de Mesquita, Erick Duchesne, D. Marc Kilgour, James Morrow, Stephen Quackenbush, William Reed, Paul Senese, and Catherine Zagare for their helpful comments on an earlier draft.

1. Stephen M. Walt, "Rigor or Rigor Mortis? Rational Choice and Security Studies," *International Security*, Vol. 23, No. 4 (Spring 1999), pp. 5–48. All subsequent citations are given by page numbers in the text.

2. See, for example, Martin Hollis and Steve Smith, *Explaining and Understanding International Relations* (Oxford: Clarendon Press, 1990).

3. Specifically, Chalmers Johnson, "Preconception vs. Observation, or the Contributions of Rational Choice Theory and Area Studies to Contemporary Political Science," *PS: Political Science and Politics*, Vol. 30, No. 2 (June 1997), pp. 170–174.

common trap of confounding the concept of *instrumental rationality*, which lies at the heart of most applications of game theory, with the theoretically distinct concept of *procedural rationality*, used most frequently by scholars who write in the psychological tradition.⁴

Citing Jon Elster out of context, however, Walt (p. 11) notes that there is *some* disagreement among *some* scholars about the extent to which the rationality assumption is descriptive of actual real-world decisionmaking processes.⁵ But Walt seems to gloss over the fact that the vast majority of rational choice theorists, including perhaps all of those whose work he surveys, would agree with Christopher Achen and Duncan Snidal that “the axioms and conclusions of utility theory refer only to choices. Mental calculations are never mentioned: the theory makes no reference to them.”⁶ In other words, there is almost unanimous agreement among its practitioners that rational choice theory seeks to explain and predict a specific form of human behavior: the choices of real-world decisionmakers. This is one important reason why it is called “choice” theory. Game theory and other theories based on the rationality assumption are not generally viewed as theories of the cognitive process. Walt’s (pp. 11–12) suggestion to the contrary is not only beside the point (see below), it is also misleading.

Walt seems to well understand the many virtues of formal methods. Two in particular stand out. Formal models help ensure logical consistency, the sine qua non of good theory, and they enhance clarity by helping to “make the assumptions that drive a conclusion more apparent” (p. 15). Still, he qualifies the significance of these virtues, arguing that originality and empirical accuracy are more important than logical consistency. This is a difficult qualification to accept. Without logical consistency, empirical accuracy cannot be determined, and without empirical accuracy, originality is of little moment, as Walt rightfully notes (p. 13).

At this point, we detect the first hint why Walt is so tolerant of logical inconsistency. Being able to argue both ways is a valuable rhetorical skill. For example, he praises Kenneth Waltz’s *Theory of International Politics* for its

4. Frank C. Zagare, “Rationality and Deterrence,” *World Politics*, Vol. 42, No. 2 (January 1990), pp. 238–260.

5. After noting what would constitute an “ideal” rational choice explanation, Elster also goes on to say that “scrutinizing mental states to ensure satisfaction of the causal conditions is then both impossible and pointless.” See his “Introduction,” in Jon Elster, ed., *Rational Choice* (New York: New York University Press, 1986), p. 16.

6. Christopher H. Achen and Duncan Snidal, “Rational Deterrence Theory and Comparative Case Studies,” *World Politics*, Vol. 41, No. 2 (January 1989), p. 164.

originality, excusing its contradictions.⁷ But if Walt were to use the same definition of originality that he applies to the formal models he later surveys (see pp. 26–31), Waltz's nuanced reformulation of balance-of-power theory would not pass the test. Indeed, the attractiveness of Waltz's (informal) deductive model lies in the transparency and strength of its logic, and in Waltz's stubborn refusal to abandon that logic to reach logically inconsistent policy conclusions. More specifically, one important reason that Waltz's theoretical work is much to be admired is that it demonstrates a logical connection between his assumption that states are security maximizers, his predictions about balancing and the stability of bipolar systems, and his prescription that favors the selective proliferation of nuclear weapons. But neither the argument that nuclear parity relationships are exceedingly stable, the claim that states tend to balance, nor the recommendation that nuclear technology ought to be shared is original to Waltz.

Walt next seems to forget his earlier observation that there is a controversy among some theorists "over whether rational choice theories must merely be consistent with the observed outcome, or whether they must also be consistent with the actual process by which decisions are made" (p. 11). Speaking about the descriptive accuracy of Bayes's rule, he takes the extreme (minority) view, asserting that "if human decisions in the real world are not made in the way that rational choice theorists assume . . . then the models may be both deductively consistent and empirically wrong" (p. 17). Some rational choice theories may indeed be empirically inaccurate and, therefore, rightfully ignored or discarded, but the fact that decisionmakers do not consciously use Bayes's rule to update their prior beliefs after new information is acquired is beside the point, as the quote from Achen and Snidal should make clear.

For one thing, it would be a simple matter to substitute other updating rules. In this case, Walt's point evaporates. The strengths of formal models are not tied to any particular assumption about the way beliefs are updated in the light of new evidence. For another, assumptions are extremely useful simplifying devices. They would, in fact, lose their utility if they were completely accurate descriptions of real-world processes. In other words, theories should be judged by their logical consistency and empirical accuracy, not by the descriptive validity of their assumptions. Assumptions should be judged by their ability to generate empirically correct propositions. *If* Bayes's rule is problematic

7. Kenneth N. Waltz, *Theory of International Politics* (Reading, Mass.: Addison-Wesley, 1979).

because it leads to empirically inaccurate statements, it could and should be eliminated.⁸

Finally, Walt changes course and contradicts himself, once again, by asserting that “formal methods . . . make it easier to bury key assumptions within the model” (p. 20). Walt’s complaint seems to be motivated by the “time and effort [needed] to unearth the basic logic of the argument.” For Walt, this is regrettable because “the time required to understand an elaborate formal demonstration . . . is time that cannot be spent questioning underlying assumptions or testing the empirical validity of the argument” (pp. 21–22). But what better way is there to evaluate assumptions than by exploring their logical consequences, and what better way is there to judge an empirical generalization’s standing than by evaluating the logic that can explain it?

Consider now the third conceptually distinct section of Walt’s article, which contains his main conclusion: the field of “security studies should welcome contributions from formal theory, large-*N* statistical analysis, historical case studies, and even the more rigorous forms of interpretive or constructivist analysis” (p. 48). It is hard to find fault here. Tolerance, intellectual or otherwise, is an important virtue. Without it, intellectual fields would calcify: new ideas would not surface, and scientific progress would come to a dead halt.

Walt’s conclusions are admirable, but they are not supported by his literature review. To show this, I next assess the merits of the arguments and criticisms Walt makes in the second section of his article. My purpose in doing so, however, is not to undermine his plea for intellectual toleration. Rather, it is to highlight inconsistencies and inaccuracies in his argument. After all, the accurate representation of ideas is another important scholarly virtue. It is simply not necessary to accept Walt’s characterization of the rational choice literature in order to be sympathetic with his main conclusion.

Given that others will no doubt focus on Walt’s understanding of their research, I begin with his characterization of my work with D. Marc Kilgour. It is interesting to observe at the outset that Walt discusses only one of a number of articles that Kilgour and I have coauthored, and an article that was published in 1991 at that. Since then, we have extended the basic mutual deterrence model that Walt offers as a leading example of “methodological overkill” to analyze unilateral (or asymmetric) deterrence relationships in which there is both a challenger and a defender of the status quo; we have refined the core unilateral deterrence model to explore the dynamics of the

8. Parenthetically, Walt misstates the so-called folk theorem, which holds that in an infinitely repeated *n*-person game any combination of action choices that is individually rational can be part of some equilibrium.

escalation process and to evaluate competing extended deterrence deployment policies such as massive retaliation and flexible response; we have used the extended deterrence model to delve into the conditions associated with limited conflicts and escalation spirals; and we have applied the underlying model to determine the role played by a client state in determining the success or failure of extended deterrence.⁹

Because we stand by most of what we said in the article in question, it would be very easy to overlook the selective treatment of our work, except that, in another forum, Walt complained about the "small sample size" of John Vasquez's evaluation of classical realism in general and of Walt's work in particular.¹⁰ No wonder, then, that Walt does not place the highest value on logical consistency. Logical consistency would not allow Walt to use one standard by which to denigrate Vasquez's assessment of his own work, and then to ignore that standard in evaluating the work of others.

Walt claims that the conclusions of our model are "for the most part affirmations of the conventional wisdom" (p. 23), asserting that "Kilgour and Zagare have reinvented the central elements of deterrence theory without improving on it" (p. 24). I will not comment on whether or not our argumentation is an improvement on classical deterrence theory. That is for others to judge. But Walt's claim that our argument is merely a restatement of classical deterrence theory is clearly inaccurate.

Our theory is drawn from an entirely different axiomatic base than is classical deterrence theory. Classical deterrence theory starts with the assumption that war in the nuclear age is irrational, which is simply another way of saying that all endgame threats are inherently incredible. As well, game-theoretic models in the classical tradition generally assume that all attack choices result in war.¹¹ By contrast, in the theory that Kilgour and I have developed—we

9. See, in particular, Frank C. Zagare and D. Marc Kilgour, "Asymmetric Deterrence," *International Studies Quarterly*, Vol. 37, No. 1 (March 1993), pp. 1–27; Zagare and Kilgour, "Modeling 'Massive Retaliation,'" *Conflict Management and Peace Science*, Vol. 13, No. 1 (Fall 1993), pp. 61–86; Zagare and Kilgour, "Assessing Competing Defense Postures: The Strategic Implications of 'Flexible Response,'" *World Politics*, Vol. 47, No. 3 (April 1995), pp. 373–417; Zagare and Kilgour, "Deterrence Theory and the Spiral Model Revisited," *Journal of Theoretical Politics*, Vol. 10, No. 1 (January 1998), pp. 59–87; Kilgour and Zagare, "Uncertainty and the Role of the Pawn in Extended Deterrence," *Synthese*, Vol. 100, No. 3 (September 1994), pp. 379–412; and Zagare and Kilgour, *Perfect Deterrence* (New York: Cambridge University Press, forthcoming).

10. Stephen M. Walt, "The Progressive Power of Realism," *American Political Science Review*, Vol. 91, No. 4 (December 1997), p. 934; and John A. Vasquez, "The Realist Paradigm and Degenerative versus Progressive Research Programs: An Appraisal of Neotraditional Research on Waltz's Balancing Proposition," *ibid.*, pp. 899–912.

11. For a discussion, see Frank C. Zagare, "Classical Deterrence Theory: A Critical Assessment," *International Interactions*, Vol. 21, No. 4 (1996), pp. 365–387; Zagare, "Rationality and Deterrence," pp. 251–257; and Zagare and Kilgour, *Perfect Deterrence*.

now call it “perfect deterrence theory”—threat credibility is a variable. Moreover, in the family of interrelated deterrence models we have developed, states are afforded an opportunity not to respond to an outright attack. These are small but critical differences.

Given the above, it should not be surprising to learn that many (but not all) of our conclusions are at odds with more standard formulations. For example, contrary to those classical deterrence theorists who support an “overkill” capability, perfect deterrence theory suggests that a “minimum deterrent” capability is better. Perfect deterrence theory also suggests that, during a crisis, reciprocating rather than preemptive strategies should be adopted. This prescription clearly runs counter to the implications of the work of Thomas Schelling, Daniel Ellsberg, and Robert Powell.¹² And finally, contrary to arguments made by Waltz, Bruce Bueno de Mesquita and William Riker, and several others, perfect deterrence theory concludes that proliferation policies are dangerous and should be eschewed.¹³ In short, perfect deterrence theory seeks to refine and improve classical deterrence theory, much the way Waltz’s theory seeks to refine and improve classical balance-of-power theory.

Perhaps it is an inconsequential fact that Walt has misrepresented our work, committing in the process the very same sin he himself railed against in “The Progressive Power of Realism.” Still, it is interesting to observe that Walt at once suggests that the implications of our model are “not very illuminating” (p. 24) and that James Morrow’s crisis bargaining model yields “rather trivial results” (p. 23).¹⁴ Yet he also claims that “Kilgour and Zagare’s model produces results different from Morrow’s model” (p. 25 n. 51). For the sake of argument, I am prepared to accept Walt’s evaluation of our model’s implications, but then Morrow’s model cannot also be said to be theoretically trivial. Again, it is not surprising that Walt is willing to tolerate logical inconsistency. It allows him to assert that two models with divergent implications both produce obvious

12. Thomas C. Schelling, *The Strategy of Conflict* (Cambridge, Mass.: Harvard University Press, 1960); Schelling, *Arms and Influence* (New Haven, Conn.: Yale University Press, 1966); and Daniel Ellsberg, “The Theory and Practice of Blackmail,” lecture at the Lowell Institute, Boston, Massachusetts, March 10, 1959, reprinted in Oran R. Young, ed., *Bargaining: Formal Theories of Negotiation* (Urbana: University of Illinois Press, 1975). See also Robert Powell, *Nuclear Deterrence Theory: The Search for Credibility* (New York: Cambridge University Press, 1990).

13. Kenneth N. Waltz, “The Spread of Nuclear Weapons: More May Be Better,” Adelphi Paper No. 171 (London: International Institute for Strategic Studies, 1981); and Bruce Bueno de Mesquita and William H. Riker, “An Assessment of the Merits of Selective Nuclear Proliferation,” *Journal of Conflict Resolution*, Vol. 26, No. 2 (June 1982), pp. 283–306.

14. James D. Morrow, “Capabilities, Uncertainty, and Resolve: A Limited Information Model of Crisis Bargaining,” *American Journal of Political Science*, Vol. 33, No. 4 (November 1980), pp. 941–972.

conclusions. Like former New York Yankees manager Billy Martin, Walt apparently feels strongly both ways.

The larger issue, however, is whether a formal model that produces conclusions that may have been stated elsewhere is still to be valued as having made a contribution to international relations theory. Unless Walt is ready to deny the importance of rigorous argumentation, the answer must be in the affirmative. Conclusions, empirical or otherwise, devoid of logical argumentation are of little value. Opinions about national security policies are a dime a dozen. They acquire currency only when they are supported by a logical structure. In this sense, a formal argument adds value, even to widely accepted conclusions.

But the contributions of rigorous analysis go much further than this. Suppose that two logical structures support diametrically opposite positions, as do Bueno de Mesquita and Riker's deterrence model, which supports selected proliferation policies, and the corpus of Zagare and Kilgour, which supports the opposite conclusion. Unlike loosely stated arguments that favor or oppose a particular policy, the logical structure of the underlying formal models can easily be counterposed, revealing the assumptions that give rise to the differences.

I shall not continue to rehearse the many additional benefits of formal models. As mentioned, Walt does a good job of this, even though, Janus-like, he felt compelled to abandon his even-handed analysis with a desultory review of prominent examples of applications of formal work in international security. Nonetheless, it is difficult to pass over the opportunity to highlight one final inconsistency. Walt (correctly) points out that most formal theorists have not devoted themselves to rigorous empirical validation of their models, although there have been attempts to use game-theoretic models to analyze particular crises and critical strategic relationships that he overlooks (for reasons I shall not speculate on).¹⁵ But at least two points should be emphasized here. First, those who do large-*N* quantitative research do not regard Walt's own work as systematically empirical, so it is odd that he would find fault with formal theorists on this count.¹⁶ Second, and more important, the lack of systematic

15. See, for instance, Frank C. Zagare, *The Dynamics of Deterrence* (Chicago: University of Chicago Press, 1987).

16. See, for example, Randolph M. Siverson's review of Walt's *The Origins of Alliances* in the *American Political Science Review*, Vol. 82, No. 3 (September 1988), pp. 1044–1045. This may be an unfair criticism if Walt does not mean to imply that formal modelers should subject their work to large-*N* statistical tests. But then why does he ignore those studies that apply game theory to real-world situations? See, for example, Frank C. Zagare, "A Game-Theoretic Analysis of the Vietnam Negotiations: Preferences and Strategies 1968–1973," *Journal of Conflict Resolution*, Vol. 21,

empirical research by formal theorists has more to do with the division of labor in the discipline than it does with any innate limitations of formal (or even informal) theory. Thus Walt's observation, even if accepted, is largely irrelevant. In other words, careful empirical research is not precluded by the tenets of the paradigm.

To conclude, I readily accept Walt's call for intellectual tolerance and, indeed, applaud it. Nonetheless, I reject his point of view that logical inconsistency is a price that must be paid for scientific advancement. There can be no compromise here. Without a logically consistent theoretical structure to explain them, empirical observations are impossible to evaluate; without a logically consistent theoretical structure to constrain them, original and creative theories are of limited utility; and without a logically consistent argument to support them, even entirely laudable conclusions, such as Walt's, lose much of their intellectual force.

No. 4 (December 1977), pp. 663–684; Zagare, "The Geneva Conference of 1954: A Case of Tacit Deception," *International Studies Quarterly*, Vol. 23, No. 3 (September 1979), pp. 390–411; Zagare, "Nonmyopic Equilibria and the Middle East Crisis of 1967," *Conflict Management and Peace Science*, Vol. 5, No. 2 (Spring 1981), pp. 139–162; and Zagare, "A Game-Theoretical Evaluation of the Cease-Fire Alert Decision of 1973," *Journal of Peace Research*, Vol. 20, No. 1 (April 1983), pp. 73–86.