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The Maginot Mentality in International Relations Models

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Throughout history formulaic thinking has been the high road to military disaster. The French outnumbered the British by odds of more than three to one at Agincourt, but numerical advantage turned out not to be the deciding factor, and the prevailing idea that armored cavalry would always defeat infantry was proven wrong.[1] In 1806 the army of Prussia had one of the most impressive reputations in Europe, but Prussia's mastery of 18th-century "chessboard warfare" could not save it from Napoleon's onslaught.[2] The Wehrmacht managed to smash through France's defenses in 1940, not because Germany possessed any superiority in either numbers or quality of men and machines, but because the French had anchored their strategy upon a rigid and misguided concept of warfare.[3] A probably apocryphal story tells that when President Nixon took office in 1969, members of the new Administration applied a computer model to determine how long it would take to win the Vietnam War. Once data concerning body counts, kill ratios, gross national products, and the like had been entered, the answer came back: "You won the war in 1964." [4]

Academic political scientists have become fond of formulaic thought in the most literal sense. A sizable body of scholars within the social science discipline practice methods of analysis in which they reduce issues of international relations and strategic affairs to mathematical equations in the attempt to explain or forecast political events. When one considers the advantages of a system that allows one to predict the future, it is easy to see why those who favor this method have begun to suggest it as a tool for both makers of foreign policy and commanders in the armed forces. Military professionals should approach these methods with caution.[5]

This article discusses two complementary techniques used in the effort to transform international relations into a mathematical science. These techniques are formal modeling and statistical methods, also referred to as quantitative analysis. Formal modeling refers to the process by which scholars develop abstract intellectual theories to explain how political events take place. These abstract theories are deductive, use formal (or mathematical) logic, and are depicted as mathematical models; thus the term "formal modeling." In practice, many formal modelers are content to stop their research at the point of model design, and the hypotheses of these formal models may be tested using various statistical methods.

In statistical analysis, researchers attempt to fit statistical information into the theories represented by the models.[6] Many scholars engaged in this work are content merely to hunt for correlations that seem to confirm the validity of theoretical propositions. However, those who practice a discipline known as "expected utility" analysis believe that they have advanced to the point at which their models, when combined with the correct data, will allow them to predict the outcome of current and prospective political events.

Our use of the terms "formal modeling" and "quantitative analysis" throughout this article is intended to be broadly inclusive. Political scientists have invented many variants of these techniques, and the debates among practitioners are fierce, but in our opinion all of these scholars make the same fundamental mistakes. Their understanding of politics and warfare is dangerously rigid and hopelessly abstract. Where strategic issues demand creative approaches to often unique conditions, they would substitute one-size-fits-all analytical methods whose input can be highly selective, processes unduly rigid, and outcomes dangerously unverifiable.

The Nature of Theory

Those who practice quantitative methodology in the academic sense would reduce all of war--indeed all of human affairs--to mathematical equations. Formal modelers aspire to give shape to what they define as "scientific" norms of human behavior, with the goal of answering political questions in the same definitive way that natural scientists answer questions about the behavior of subatomic particles. These theorists are not particularly interested in the details of specific wars, political issues, or historical epochs. Rather, they seek general theories of politics that will apply universally.

A general theory is, by its nature, purely abstract. Information of the sort found in newspapers, historical accounts, and intelligence reports has little place in developing such a theory, and the knowledge of veteran statesmen counts for little. Kenneth Waltz, whose book *Theory of International Politics* ranks him among the founding fathers of academic international relations, admonishes us to remember that "observation and experience never lead directly to knowledge of causes." [7] In a later section, Waltz criticizes those who believe in a "reality that is out there," and tells us that "what we think of as reality is itself an elaborate conception constructed and reconstructed through the ages." [8]

Raw information, in Waltz's view, is meaningless until we develop a theory to explain it. When we look at the world, we see an indescribably complex--and seemingly infinite--array of objects and events. According to Waltz and those who subscribe to his philosophy, we need theory to simplify this situation by telling us which of these things are relevant, and what they mean. [9] Therefore, when formal modelers set out to study politics and strategy, they develop their theories first, and apply their concepts to the material world afterward. We have no problem in the abstract with deductive reasoning of the sort that undergirds theories of international relations; however, we observe that inductive reasoning has a place in the study of politics. The theories resulting from strictly deductive reasoning may not correspond to our conventional understandings of reality, but to those who follow this school of thought, this is not necessarily a cause for concern. Such theorists, after all, are trying to explain and predict political events, not to reproduce them.

Anthony Downs, author of *An Economic Theory of Democracy*, urges us to judge models not by the realism of their underlying assumptions, but by the accuracy of their predictions. [10] Indeed, a reliable system that allowed us to forecast the future would seem valuable. One is entitled to ask, however, how scholars can take an abstract theory of the sort described above and use it to make specific, quantitative predictions about events in the real world. Academics engaged in designing predictive models come in many varieties, but all of them employ the same basic technique. First, researchers develop their fundamental assumptions about human behavior, associations, and relationships. We are asked never to judge these assumptions, but to hold our criticism for the model's results. Next they express their theory in terms of a mathematical relationship among discrete concepts known as variables. Finally, quantitative analysts "operationalize" those variables by equating them with factors that can be measured in numerical terms.

The operationalization of variables is problematic. For example, as one aspect of national power, quantitative analysts who wished to predict the outcome of a war might decide to condense their beliefs about the fighting spirit of armies into a single factor labeled "morale," and decide arbitrarily to measure the "morale" of an army in terms of the average amount which it pays its soldiers. This is the manner in which the assumptions to which Downs refers are put to use.

Assumptions, however, may matter more than Downs and others care to admit. The fact that theorists believe they can predict events within the artificial realm of their own models does not mean that they can make meaningful observations about the world outside the models. If, for instance, the French general staff had used this method to forecast the outcome of a German invasion in 1940, we can safely assume that the forecast would have reflected their belief that a defensive strategy would always be stronger than the offensive.

According to the Phoenix Factor theory, which has achieved some prominence in accounts of quantitative research into international relations, [11] warfare does not change the balance of power between warring nations, except perhaps in the short term. [12] Victorious nations seldom seem to realize long-term gains in power, and defeated nations seldom suffer long-term losses. A. F. K. Organski and Jacek Kugler, the political scientists who formulated this hypothesis, claim to have supported these propositions through empirical research. In their study, they chose to operationalize the concept of national power in terms of Gross National Product. [13] When Organski and Kugler examined economic statistics for selected participants in World Wars I and II, they found that, just as they expected, both losers and winners promptly returned to levels of GNP similar to what their model predicted they would have enjoyed if there

had been no war. Thus, they conclude: "We are tempted to suggest that the outcomes of war, insofar as international power is concerned, make no difference." [14]

Upon examination, however, one finds that Organski and Kugler have defined the term "nation" in a peculiarly narrow fashion. Their study recognizes no connections between nations and types of national governments. When these researchers conducted their study in the 1970s, they were able to look at a data table and see, for instance, that the larger of the two regions labeled "Germany" had an acceptably high level of GNP. [15] Therefore, their model led them to conclude that the German nation suffered no long-term loss of power from its defeat in World War II. [16] Some observers, however, may feel that it is inappropriate to dismiss the racial, ideological, legal, and territorial motivations for the World Wars. But according to the conventions of academic theory, Organski and Kugler may make whatever assumptions they wish, and therefore the Phoenix Factor study is, by its own standards, a success.

The assumptions in this model are not unusual. Like most scholars of the popular "neo-realist" school, Organski and Kugler prefer to assume that nation-states are "unitary actors," operating independently from leaders, people, and government institutions. When academics apply these assumptions rigidly, however, their models can provide absurd outcomes. Those who observe the world unaided by the model have little difficulty understanding the difference between winning and losing a world war. So, even if one accepts the dubious proposition that the real significance of conflicts between nation-states can be measured in purely economic terms, those of us who use the word "nation" in a more ordinary sense must observe that as a consequence of World War II, the GNP of the Third Reich is zero.

Decisions, Decisions

Many formal modelers make assumptions about human behavior that deserve special attention. Most current theories of formal modeling rely on the idea that people behave as what economists call "rational actors." According to the rational actor theory, people make all the decisions in their lives on the basis of self-interest; even those who appear to be acting altruistically must always have selfish purposes in mind. [17] Those who give money to charitable causes, for instance, may do so with the hope, if not the intent, of deducting the donation from their taxes. To a rational actor theorist, even people who honestly believe that they act out of motivations other than greed in fact actually base their lives on unconscious cost-benefit calculations. The rational actor theory was popularized by economists who used it to explain the world of business and investment. Political scientists have expanded this principle into a general rule for all human activity, to the point where many academics deny that differences in culture, governmental system, ideology, religion, or individual character have any significant effect upon international relations. Therefore, many formal modelers see no reason to study history, geography, or foreign languages. According to their theories, all people behave predictably and in comparable ways. [18]

Others find these generalizations of human psychology disturbing. Chalmers Johnson, a noted American expert on the subject of East Asia, finds an upsetting lack of cultural understanding in quantitative research. Johnson and E. B. Keehn observe:

In a recent book published by Harvard University Press, the professor of Japanese politics at Yale and the professor of Japanese law at Chicago intend to enlighten us about Japanese politics by applying what is called "rational-choice theory" to Japan. The two authors, whose chief qualification for this task seems to be that they were both raised as children in Japan, get off to a rocky start. In order to illustrate the distinction between principals and agents in their theory--bosses and underlings in plain language--they invoke the "rational choice" made by Chinese coolies pulling barges through the Yangtze River gorges. They offer the explanation that, "Acting collectively as principles, the coolies hired supervisors with whips to prevent each other from free riding." It evidently never crossed the minds of these savants of coolie motivation that their conclusion is so preposterous that it could be established (if at all) only empirically--by some on-the-spot discovery of a hitherto unknown guild of Chinese masochists. The idea that the coolies paid supervisors to whip them is not one that can be established deductively from theory. What is needed is evidence. Did the coolies actually have a choice, or were they perhaps members of a prison gang? Where and how did they hire their tormentors? Worthless research in academic political science is not new, but in the face of stern competition this seems to establish a new low. [19]

The fact that rational actor theorists intentionally ignore realities such as the nature of coolie labor would seem to discredit their ideas as a reliable means for examining international relations. The refusal of some theorists to acknowledge the possibility that people might act on the basis of motivations such as duty, honor, or community spirit flies in the face of history and, perhaps, personal experience. Those who have committed themselves to serve their communities or to defend their country in war may be entitled to find this proposition offensive.

Furthermore, when the rational actor principle is used to develop theories of decisionmaking apart from economics, the level of abstraction in formal theory climbs to new highs. The idea that one can use a concept such as the rational actor to forecast outcomes of political events rests on four inherent beliefs:

- It is possible to identify the key decisions that must be taken to shape a particular policy, and to isolate those choices from the other decisions that surround them.
- It is possible to identify and differentiate all the options available to decisionmakers.
- It is possible to calculate the costs and benefits that each decisionmaker will experience from each option.
- People in the real world, subconsciously if not consciously, will calculate their costs and benefits according to the principles and methods employed by the modelers.

With these assumptions one can construct elaborate tables and decision trees to explain how a political decision will unfold. These models, however, are almost certainly too stylized to survive contact with the actual environment of international relations. In politics and war, seemingly minor decisions can have long-term consequences. The costs and benefits of each "option" are changing and uncertain. A nation that fields a new class of weapon may be able to change dramatically strategic cost-benefit analysis, while creating risks and vulnerabilities elsewhere in military or political activities. And leaders who cannot change the actual costs and benefits of a given action may attempt to fool their adversaries by adopting an unanticipated strategy.

Even if we presume that political scientists can factor into their models all of these sources of change, they will still have failed to address the real issues of politics and war. The difficult part of strategic planning and military operations is not deciding which option to choose, it is putting one's decision into effect. One cannot simply opt to carry out a surprise attack; one must make sure that the enemy is surprised. Similar principles apply in the world of politics. This, perhaps, is the meaning behind Clausewitz's remark that although everything in war is simple, the simplest things are difficult.

Causality or Coincidence?

Not only are the assumptions of formal modeling questionable, but the premise of quantitative research--the idea that statistical analysis can reveal previously unknown truths about politics--is doubtful as well. Statistical correlations cannot explain the causes of political events. In order to understand the fallacy in the statistical approach, it is helpful to consider its origins. Quantitative methods of analyzing human affairs have been flawed since their inception.

One of the first 20th-century books on the subject was Lewis Fry Richardson's *Mathematical Psychology of War*, published in 1919.[20] Richardson's service in the Friends' Ambulance Unit in France during World War I had exposed him firsthand to the horrors of war. This experience, coupled with his existing deep commitment to pacifism as a Quaker, led him to abandon other academic endeavors in order to pursue what is now known as peace research. In 1960 Richardson published two books that remain influential among quantitative researchers, *Arms and Insecurity* and *Statistics of Deadly Quarrels*.[21]

Richardson theorized that arms races cause wars. To confirm his hypothesis, he used the two World Wars as case studies, comparing his theories to historical data until he found a set of statistics that supported his hypothesis. According to Richardson, his method of analysis had created a breakthrough, and others have adopted his technique. When academic political scientists test their models against historical examples, they adapt their statistical formulas to explain as many examples of past war as possible. Political scientists who employ this method as a matter of course are free to study any set of data that suits their theory, free to ignore any data that seem troublesome, and free to tinker with their data to make the theory "predict" or "describe" the consequences of actions for which history has already revealed the results.

Most reputable scholars, one presumes, resist the temptation to confirm their theories (and thus advance their careers) through the selective use of statistics. Nevertheless, even the most forthright are on dubious ground when they claim that their quantitative models have identified "causes" of political events. Statistical analysis allows us to measure correlations or trends between events, but it does not tell us what these correlations or trends mean. The fact that two things happen in sequence may mean that one caused the other, but it may also be true that some outside factor caused either or both. Worse yet, the sequence may merely be a coincidence.

The following simplistic example illustrates the problem with correlations. Suppose a political scientist theorizes that dairy production greatly affects the incidence of warfare. He expresses this idea as a mathematical equation and uses statistical analysis to test his hypothesis. In 12 of 15 cases he studies, he finds that dairy production among the combatants dropped by more than 10 percent in the month preceding the conflict. Does this discovery, referred to as "variable correlation," imply that national security professionals need to keep an eye on dairy production? Probably not. The fact that dairy production declined before a war does not mean that the drop in dairy production caused the war. Such data, or "variables" if you prefer, must not merely have a "relationship" or "correlation" with the incidence of war. For these mathematical models to be successful, data must be proven to be "causally related" to war.

In a more realistic scenario, suppose the same formal modeler assumes that increases in arms expenditures lead to a higher incidence of warfare, expresses the idea as a mathematical equation, and uses statistical analysis to test his hypothesis. In 14 of 17 cases of war analyzed, arms expenditures among the combatants increased by more than 12 percent in the month preceding the conflicts. Does this mean that arms races cause wars? Again, one has every reason to be skeptical. Is it not equally plausible that the deterioration of political relations that leads to war also causes national leaders to embark on arms races? What of the examples where war did not occur? The idea that a correlation between two events proves that one has caused the other is a classic error in logic, which goes by the Latin phrase *post hoc ergo propter hoc* (after this, therefore because of this). Contemporary statisticians caution that "correlation does not equal causation," and devotees of academic political science are aware of the questions this principle raises about their technique. In a book written to explain proper quantitative modeling technique, David Singer observes:

In the methodological literature, one increasingly encounters the concepts of internal and external *validity* when examining a research design. Essentially, the validity issue can be translated into the question: to what extent does the research design permit us to infer "causality" or some approximation thereof? To be more precise, the issue is not one of the validity of the *design* (or of the experiment), but of the *inferences* we seek to draw from the results of the investigation.[22]

In his treatment of "causality" Singer is forced to acknowledge that political events may occur as the result of two (or many more) causal sequences. For example, wars may be caused by C, which was caused by B, which was caused by A. Accepting this admission, we can never know if the model has captured the correct series of sequences or all the contributing events within each intermediate condition, since we are not sure of the number of sequences involved in the political event. Further, Singer acknowledges that a single variable may sometimes be the key to a political event, but that other events appear to be caused by multiple variables. He is absolutely right. Modelers and statistical analysts can never be sure that they are capturing the correct variables, much less that their method of operationalizing those variables is reasonable or even relevant to the matter at hand. Singer, moreover, admits that some political events are caused not merely by the *sum* of multiple variables, but by the *interactions* of those variables.

Singer's recommendation for the academic political science discipline is to lower the scientific standard of demonstrating causality. He states that "in place of the notion of 'causality,' my preference is for the related notion of 'explanation.'"[23] To an observer, however, these are differences without distinctions. It does not matter whether political scientists use the word "explanation," "correlation," or "trend" as long as they continue to perform their studies in the same way and expect others to accept their findings without challenge. However carefully they qualify their language, quantitative political scientists imply that their work tells us something useful about cause-and-effect relationships. Exercises in semantics do not sustain such claims.

The Quest for a Crystal Ball

It is useful to review the key claim of expected utility modelers: their methods allow them to forecast political events.

Anyone can appreciate the value of a crystal ball, so their claim should not be dismissed out of hand. When one examines carefully the concept of prediction in political science, however, the claim leaves much to be desired.

Quantitative forecasts often appear successful because, after a certain point, the outcome of any given event does indeed become predictable: "To hear the thunderclap is no indication of acute hearing." [24] Formal modelers, whose training led them to think only in terms of existing models, completely failed to anticipate the collapse of the Soviet Union. Traditional analysts, on the other hand, were free from the beginning of the East-West confrontation to speculate on the true nature of new international system in all its richness. George Kennan's analysis of the future of a bipolar world, published in 1947, clearly foresaw the possibility that a state with the economic weakness, ideological expectations, and overextended military commitments of the Soviet Union might collapse internally. [25] Furthermore, Kennan did not merely predict how the Cold War would end, he proposed a philosophy of foreign policy designed to ensure that it would end benignly. Kennan's insight came not from the use of a rigid methodology, but from the wisdom gained through an understanding of history and a relatively bias-free appraisal of conditions in the postwar Soviet Union.

In order to defend their relevance, most quantitative modelers refuse to admit that it is possible for us to take this kind of active role in shaping history. This attitude is well demonstrated in a recent issue of *Military Intelligence* in which a supporter of quantitative methods states that "respected historians" agree that "major historical events" would have varied little with different actors. [26] Although it is difficult to respond to such vague claims by unspecified historians, the first chapters of Liddell Hart's *Defense of the West* seem relevant here. Hart discusses the French collapse in 1940, the German defeat after Stalingrad, and the success of the Normandy invasion, noting that in each case there was a point when the leaders of the losing side could have turned defeat into victory simply by following a different strategy. If Hart is correct, these leaders could have changed the outcome of World War II.

The article in *Military Intelligence* also asserts that "switching Generals Robert E. Lee and 'Stonewall' Jackson with Ulysses S. Grant and William Tecumseh Sherman would not have caused a different end state for the Civil War." [27] Perhaps. It might be more pertinent to ask what would have happened if fate had switched Jefferson Davis with Abraham Lincoln. With such simplistic historical analyses, supporters of quantitative methodology inadvertently point to the weaknesses of their predictive technique. Modern political science may affirm the fact that broad trends can lead to "inevitable" results. But as theories of chaos and complex adaptive systems mature, their tendency is to challenge linearity in all its manifestation in human affairs. Few concepts for explaining human behavior could be more rigidly linear than quantitative analysis in its present form.

Academic Debates and Beyond

Formal modeling and quantitative research have found wide acceptance in American universities, but they have also aroused powerful counter-arguments. Even political scientists who favor the use of abstract theory in the study of politics have decried the Procrustean rigidity of statistical modelers. For those who still find quantitative methods attractive, theorist Hedley Bull's 1969 criticism of the emerging method remains devastating:

I know of no model that has assisted our understanding of international relations that could not have been expressed as an empirical generalization. This, however, is not the reason why we should abstain from them. The freedom of the model-builder from the discipline of looking at the world is what makes him dangerous; he slips easily into a dogmatism that empirical generalization does not allow, attributing to the model a connection with reality it does not have. . . . He has provided an intellectual exercise and no more. [28]

Writer and nuclear strategist Bernard Brodie noted that the spread of the quantitative mind-set has had a harmful effect upon policy analysis. Brodie remarks that while intangible issues of morale, political ideology, and national will may decide wars, the concepts do not fit into models. Therefore, those who find models appealing too often end up shunting these factors aside. He wrote about his experience at RAND:

Within RAND itself there was a quiet but strongly-felt differential between those who knew how to handle graphs and mathematical symbols . . . and those who merely knew how to probe political issues. Elegance of method is indeed marvelously seductive, even when it is irrelevant or inappropriate to the major

problems.[29]

In response to such arguments, modelers and quantitative researchers have tried to brand their opponents as Luddites. They would have us believe that those who object to their technique are merely trying to resist the inevitable development of political studies into a true science. One author, for instance, labels critics of the formal quantitative approach as that "large segment of the community" which "detests science, statistics, and theory." [30]

To the contrary, Leo Strauss summarizes the issue of quantitative research as follows:

Only a great fool would call the new political science diabolic: it has no attributes peculiar to fallen angels. It is not even Machiavellian, for Machiavelli's teaching was graceful, subtle, and colorful. Nor is it Neronian. Nevertheless one may say of it that it fiddles while Rome burns. It is excused by two facts: it does not know that it fiddles, and it does not know that Rome burns.[31]

Opponents of the quantitative method value objective techniques as much as those supposedly "scientific" analysts who practice statistical modeling. But academic theories should conform to the material world in a way that quantitative models cannot. Strategists have to cope with the art of war as well as the science of war. The two are inseparable, and the strategic leader who favors one over the other places his force and the nation it defends at great risk. Academia offers a wealth of invaluable resources to military professionals, but among these resources are concepts and conclusions that are worse than useless. Let the user beware.

NOTES

1. *The New International Encyclopedia*, 1911, p. 201.
2. For a fine treatment of this episode, see Roger Parkinson, *Clausewitz* (New York: Stein and Day, 1971).
3. See B. H. Liddell Hart, *Defense of the West* (London: Cassell, 1950), pp. 3-13.
4. Cited in Harry G. Summers, *On Strategy: The Vietnam War in Context* (Carlisle, Pa.: US Army War College, Strategic Studies Institute, 1981), p. 11.
5. For an example of a military professional recommending the use of these methods, please see Captain David Resch, "Predictive Analysis: The Gap Between Academia and Practitioners," *Military Intelligence*, April-June 1995. Examples of academics recommending the use of these methods is common. See Bruce Bueno de Mesquita, "The Contribution of Expected-Utility Theory to the Study of International Conflict," for a representative opinion regarding the superior value of "academic" political science. Bueno de Mesquita's paper is found in Manus I. Midlarsky, ed., *Handbook of War Studies* (Ann Arbor: Univ. of Michigan Press, 1993).
6. In addition to use in testing formal models, statistical (or quantitative) methods are also routinely employed in the empirical testing of inductive political theories. Political scientists using statistical methods might then, for example, seek to statistically test "balance of power" theory. Due to their openness to inductive theories, some quantitative theorists make the claim that it would be wrong to place all formal modelers and quantitative theorists into the same academic category. We recognize this important distinction in relative degree of formulaic thinking, but would nonetheless contend that both research methods are misguided.
7. Kenneth N. Waltz, reprinted in Robert O. Keohane, ed., *Neorealism and its Critics* (New York: Columbia Univ. Press, 1986), p. 30. Waltz's entire article (pp. 27-46) is worth reading.
8. *Ibid.*, p. 31. Those with an eye for academic irony might note that Waltz is currently under severe criticism from followers of the critical and postmodern schools of thought, who feel that he is too wedded to ideas of progress and objective truth.
9. *Ibid.*, p. 36.

10. Anthony Downs, *An Economic Theory of Democracy* (New York: Harper, 1957), p. 21. Downs and Waltz come from two extreme (though both popular) views within academic political science. Though they would, no doubt, claim to be conducting vastly different types of research, in our opinion similar fundamental mistakes occur in both approaches.

11. The fact that the Phoenix Factor has appeared in print repeatedly over the course of two decades indicates that it has sustained the interest of others in the field. See A. F. K. Organski and Jacek Kugler, "The Cost of Major Wars: The Phoenix Factor," *American Political Science Review*, 71 (December 1977), 1347-66; Marina Arbetman and Jacek Kugler, "Exploring the Phoenix Factor With the Collective Goods Perspective," *Journal of Conflict Resolution*, 33 (March 1989), 84-112; A. F. K. Organski and Jacek Kugler, *The War Ledger* (Chicago: Univ. of Chicago Press, 1980), pp. 104-46.

12. Organski and Kugler, "The Cost of Major Wars," p. 1365.

13. *Ibid.*, pp. 1347, 1349. Organski and Kugler contend that GNP does not equal national power, but that GNP is nonetheless an excellent measurement of national power. In a limited fashion GNP certainly speaks to actual, or at least potential, national power. However, an almost infinite number of other factors affect the concept of "national power," and to exclude them is to invite serious misreadings of national power.

14. *Ibid.*, p. 1365.

15. Data tables contain no entries for the German Democratic Republic. *Ibid.*, pp. 1353, 1358.

16. Some quantitative researchers might take refuge in the response that an individual case study such as Germany may be an exception to the aggregate trends that they have uncovered. Organski and Kugler, however, make an explicit attempt to "disaggregate" their sample, and they do not claim that Germany is an exception to the rule. *Ibid.*, pp. 1357-58. Furthermore, the authors feel that their argument applies equally well to Imperial Japan, and nearly all the other defeated nations of World Wars I and II.

17. Bruce Bueno de Mesquita explains the essence of rational actor theory in the following five conditions.

1. individual decisionmakers are rational in the sense that they can order alternatives in terms of their preferences; 2. the order of preferences is transitive, so that if A is preferred to B and B p C (where "p" is to be read as "is preferred to"), then A p C; 3. individuals know the intensity of their preferences, with that intensity being known as utility; 4. individuals consider alternative means of achieving desirable ends in terms of the product of the probability of achieving alternative outcomes and the utility associated with those outcomes; and 5. decisionmakers always select the strategy that yields the highest expected utility.

See "The Contribution of Expected-Utility Theory to the Study of International Conflict," in Midlarsky, p. 144.

18. Of course, the rational actor theory implicitly acknowledges that it cannot predict the decisionmaking calculations of the irrational. Thus, the irrational or insane escape prediction.

19. Chalmers Johnson and E. B. Keehn, "A Disaster in the Making: Rational Choice and Asian Studies," *The National Interest*, 36 (Summer 1994), 14. The book being critiqued by Johnson and Keehn is J. Mark Ramseyer and Frances McCall's *Japan's Political Marketplace* (Cambridge, Mass.: Harvard Univ. Press, 1993).

20. See G. D. Hess, "An Introduction to Lewis Fry Richardson and His Mathematical Theory of War and Peace," in *Conflict Management and Peace Science*, 14 (No. 1, 1995), for an examination of Richardson's private and professional life.

21. Michael D. Intriligator and Dagobert L. Brito examine the influence of Richardson's early models, and newer models that are direct descendants of his early models, in "Richardsonian Arms Race Models," in Midlarsky.

22. J. David Singer, *Models, Methods, and Progress in World Politics* (Boulder, Colo.: Westview Press, 1990), p. 139.

Italics in original.

23. Ibid., p. 141.

24. Sun Tzu, *Sun Tzu's Art of War*, trans. Yuan Shihing (New York: Sterling Publishing Co., 1987), p. 101.

25. George Kennan, "The Sources of Soviet Conduct," *Foreign Affairs*, 25 (July 1947). Kennan originally had sought anonymity as author when the article was published; thus, the article appeared in print as being written "By X."

26. Resch, p. 27.

27. Ibid.

28. Hedley Bull, "International Theory: The Case for a Classical Approach," in *Contending Approaches to International Politics*, ed. Klaus Knorr and James N. Rosenau (Princeton, N.J.: Princeton Univ. Press, 1969), p. 31. Importantly, in addition to his list of reasons for rejection of the quantitative method, Bull also provides a list of illogical complaints too often lodged against quantitative researchers. First, the fact that much of this research is "tortuous and inelegant" to read does not of itself render the research flawed. Einstein is difficult to read, but his ideas are still useful. Second, Bull suggests that we must not question the motives of quantitative theorists. This criticism seems a bit dated today, but initially these theorists were seen by traditional political academics as mathematicians or natural scientists who could not make a success of themselves in their own fields, and who came to rest in the study of politics. Such criticism is unfair, and quite beside the point when considering whether these political theories have merit. Lastly, Bull claims that it is wrong to see the quantitative method as an instrument of a particular policy preference in national security issues. In their personal preferences, quantitative modelers encompass the broad spectrum of political beliefs.

29. Bernard Brodie, as cited in Colin Gray, *Strategic Studies* (Westport, Conn.: Greenwood Press, 1982), pp. 129-30.

30. Resch, p. 26. Resch provides a refreshingly frank analysis of academic political scientists' views of traditional political and strategic theorists. For those interested in the views that academic political scientists generally hold of traditional theorists, we recommend attending a regional political science conference or paper reading.

31. Leo Strauss, "An Epilogue," in *Essays on the Scientific Study of Politics*, ed. Herbert Storing (New York: Holt, Rinehart, and Winston, 1962), p. 327.

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