1 TOWARD A BETTER UNDERSTANDING OF THEORIES OF INTERNATIONAL CONFLICT

i pass with relief from the tossing sea of cause and
theory to the firm ground of result and fact.
—winston churchill, 1898

Social science, like any science, achieves progress through the accumulation of systematic knowledge. To improve our collective understanding of international politics we need to regularly assess both the empirical regularities observed in the world around us and the theories that profess to explain these facts as we have come to know them. Political science in general and the study of international politics in particular suffer from having numerous competing theories, assertions, and conjectures describing the same phenomena. Although new explanations and descriptions of interstate war appear almost as frequently as the events themselves, new ideas rarely supersede those previously developed. Instead, they simply accumulate with little regard paid to the explanatory power of new accounts versus those previously advanced. This has resulted in the multitude of models, untested hypotheses, conjectures, and normatively grounded assertions that constitute the discipline.

The study of international conflict exemplifies these problems. Over the last forty years, the development of models and conjectures purporting to explain the incidence and escalation of international conflict has proceeded at a rapid pace. In their 1990 survey, for instance, Dougherty and Pfaltzgraff identified over thirty examples of what they characterize as theories of international relations. If we counted variations on familiar themes and arguments developed since, the number would be much higher. Having an array of theoretical approaches and empirical conjectures is not a problem if scholars have a well-founded sense of which explanations or descriptions account for the greatest

proportion of events or facts or, more colloquially, which theories or hypotheses work best. While political scientists have been very successful in developing numerous interesting stories about the nature of international conflict, we have been much less successful at conducting the type of rigorous analysis that would allow us to (1) evaluate the relative explanatory power of these various descriptions and (2) reach some sense of consensus about which stories are the most useful or valuable in understanding international political processes. The abandonment of "failed" theories or presumed empirical regularities is rare in political science—and particularly so in the study of international politics.

Apologists for the current state of affairs might claim that, compared to other disciplines, cumulative progress in political science may be particularly difficult to achieve for at least two principle reasons. One has to do with the limited availability of some critical data. For some problems in political science, there simply is insufficient data for researchers to test their hypotheses or arguments. For example, the complete absence of wars between modern liberal democracies after World War II makes it difficult to sort out the competing and often collinear explanations for this important fact. Similarly, tests of explanations focusing on human misperception or the role of private information require data that are notoriously difficult to assemble. Another problem we face, and one that is potentially damning, lies in the possibility that in the realm of politics there may be no fundamental regularities, or equilibria, to predict. William Riker (1980, 443) suggests that the prospects for theoretical advancement in political science are quite bleak by concluding that "politics is *the* dismal science because we have learned from it that there are no fundamental equilibria to predict." From this hypothesized lack of formal equilibria, Riker then concludes that "In the absence of such equilibria we cannot know much about the future at all, whether it is likely to be palatable or unpalatable, and in that sense our future is subject to the tricks and accidents of the way in which questions are posed and alternatives are offered and eliminated."[1] Of course, Riker's gloomy conclusion has not prevented others from (1) vigorously disagreeing with him, (2) continuing to develop new explanations of the political world, and (3) claiming the discovery of new empirical regularities of normative or substantive significance.

These protestations notwithstanding, it is probably premature to conclude that there are no predictable international political events or that there are insufficient data to assess the validity of carefully specified arguments. Nevertheless, we believe that there is a third critical problem hindering scientific advancement in the study of international conflict behavior, namely, the paucity of systematic comparative testing

that would allow us to examine, judge, and occasionally abandon explanations and descriptions of the origins of international disputes and wars. Without testing multiple explanations of organized international violence in a simultaneous framework, we cannot judge the relative power of competing explanations or predictions. We believe this lack of comparative testing to be a main culprit for our current state of affairs. In this volume, we turn to developing such comparative tests.

STANDARDS FOR COMPARATIVE HYPOTHESIS TESTING

When testing an empirical conjecture or the observable implications of one theory versus another (that is, conducting comparative hypothesis testing), one of the first tasks is to choose from the variety of standards one could use when deciding that one model of the world supersedes or replaces another. For example, one method we might apply is Popper's (1968) "method of elimination." Using his approach, we end up with relatively more powerful theories, as stronger theories replace weaker ones through a process of "dualistic elimination." An example of dualistic elimination—the process of pitting one "theory" against another—appears in the simulation used by Robert Axelrod in *The Evolution of Cooperation* (1984). There, in round-robin fashion, Axelrod pitted one computer simulation strategy after another against the competition until an overall winner emerged after thousands of iterations. In this case, Popper's method provided a suitable approach because each strategy was an exclusive set of rules to play the game of interest (Prisoners' Dilemma), with each strategy having a clear and unique payoff.

Although simple, Popper's method is not appropriate for testing most theoretical explanations or empirical conjectures in international politics. He notes an important caveat to applying his method of elimination, arguing that it is appropriate only if a theory may be demonstrably falsifiable, that is, if the theory is "sufficiently *precise* to be capable of clashing with observational experience" (Popper 1968, 131; italics in original). The use of such rigid tests is only appropriate when the theories themselves and the data used to test them are well specified and precise. In his work, Popper presents an example of disproving Kepler's description of circular, rather than elliptical, planetary motion. Unfortunately, the level of precision found in most theories and empirical conjectures of international politics is not nearly as high as in the planetary sciences, and the nature of the objects under study is rather different. In fact, most so-called theories of international politics are not really theories at all.

Here, by "theory" we mean a logically consistent and empirically falsifiable causal explanation of why some event or set of events occurred in the past and, given similar conditions, will occur in the future. Most international relations "theories" simply describe conditions where some factor supposedly influenced the likelihood or nature of some past international conflict. In other cases, we see arguments about some factor (such as economic interdependence) that supposedly affects the likelihood of conflict without defining precisely what sort of conflict is in question (e.g., small disputes, crises, or large-scale wars). Other "theories" such as balance of power theory suffer from logical inconsistencies when moving from cause to effect. Moreover, since many of these causal factors that are linked to increased risk of disputes or war are not mutually exclusive—that is, multiple factors associated with various theoretical perspectives may simultaneously influence the conflicts we are trying to understand—dualistic elimination is inappropriate.

In addition, because we view human behavior as probabilistic rather than deterministic, we often will not be able to conclude that one explanation is clearly superior in all circumstances, as no single observation is sufficient to falsify a theory in a probabilistic world. By "probabilistic" we mean that there is a stochastic component to human behavior, that under apparently identical conditions, state leaders might choose to do one thing at one time and something quite different at another, but with some predictable probability of doing each. By contrast, a deterministic relationship would be one such as Newton's theory of force, mass, and acceleration, $F = MA$. In the context of classical mechanics it would be nonsensical to assert that a given force would accelerate a known mass with some acceleration 70 percent of the time. Theories of political behavior are more akin to meteorology, where forecasters hope to predict the probabilities of various weather outcomes occurring, given a set of observed meteorological conditions.

In the political world there may be many factors leading to the probabilistic nature of political behavior, ranging from the tiny influences of unmeasured or immeasurable factors to conscious or unconscious strategies of randomized behavior, irrational or impulsive behavior, strategic decision making, and, finally, to the effects of actors making their decisions based on rational expectations in the setting of strategic choice. From this perspective, where chance plays a powerful role in determining the events we ultimately study, we become engaged in the task of forecasting tendencies and influences of a set of observable conditions on the relative likelihood that a decision maker will behave in a particular way. In this book, we focus solely on assessing the empirical

content of various arguments purporting to predict or to explain the relative likelihood of war.

Taking a slightly different approach to the problem of falsification and theory rejection than Popper, we generally follow Lakatos's (1978, 32) standard that we should reject an explanation of past events or a prediction of future events if and only if another explanation predicts everything and more that the first one does and that this new empirical content is verifiable. We implement what we see as a statistical version of Lakatos's perspective. By adopting the maximum likelihood logic of inference (King 1989) we are able to make two sets of judgments. First, we can assess whether a set of indicator variables derived from an argument about the relative likelihood of war makes novel contributions to the fit of our statistical models. Second, we can evaluate the relative predictive power associated with each of these variables. This approach allows us to demonstrate that multiple factors simultaneously influence conflict. It also allows us to judge whether or not some conjecture is consistent with more than one unique event when controlling for other explanations, thereby suggesting elimination of this factor as a systematic predictor of war.

Following this perspective, with our analyses we do not attempt to eliminate conjectures based on ontological rigor or the internal logical consistency of the arguments. Rather, we focus solely on their empirical content. A quite different form of comparative analysis might carefully examine the internal logic and assumptions of multiple theories of international conflict and reject those that are logically contradictory or inconsistent (e.g., Zinnes 1967; Niou, Ordeshook, and Rose 1989). While there is much to be said for that approach, all too frequently inductively driven or normatively motivated stories remain at the center of academic and policy debate even after compelling demonstrations of the deductive flaws in the "theory's" logic. For example, consider balance of power theory. Niou, Ordeshook, and Rose (1989) conducted an excruciatingly careful and nuanced evaluation of the logical underpinnings of classical balance of power arguments and found them sorely lacking. However, their formal mathematical analysis did little to change the beliefs of those supporting the view that an equitable balance of power or capabilities will keep the peace between potentially warring nations. Balance of power and other realpolitik arguments appear before students in much the same way as they have for decades, unaffected in many classrooms by careful logical analysis demonstrating the logical flaws of the "theory." With the hope that unimpeachable empirical regularities may be persuasive where elegant

logic has not always been, we focus on the empirical content of our models, hypotheses, and conjectures, represented by their associated variables and operational indicators.

SCIENCE AND CUMULATIVE PROGRESS IN INTERNATIONAL POLITICS

Clearly, evaluating the relative explanatory power of different empirical assertions and dropping or modifying those that receive little or no empirical support is an important part of the scientific enterprise. Our beliefs about the power of various empirical conjectures drive our collective research agendas and often mark the starting point for policy prescriptions. To date, however, only limited efforts exist to compare systematically the predictive power of the myriad different explanations of international conflict—or, more precisely, the explanatory power of the independent variables expected to correlate with conflict behavior. While many researchers pay lip service to Lakatos and his principles of progressive scientific research based on careful theory development and testing, in practice most studies of international politics have failed to follow this model. Most so-called theories of international politics are simply broad-brush descriptions based on the observation of small numbers of events rather than carefully deduced explanations of political behavior.

Many of the existing empirical studies fall short in another dimension as well. Studies seeking to compare or cross-validate existing empirical claims all too often use different subsets of data, data cast at different units of analysis, and data sets with different dependent variables. Rather than conducting broad tests of multiple theories, most existing tests of various explanations of international politics assess new explanations against a null model or a small and carefully selected set of competing claims. Typically, an author presents a contrasting set of explanations where one argument is of primary interest with competing explanations presented as control variables. We find, for example, rational deterrence hypotheses compared to variables drawn from psychological approaches (Huth and Russett 1993) or a selected set of international system structure variables compared to a set of variables drawn from a dyadic perspective (Bueno de Mesquita and Lalman 1988). Maoz and Russett launched a veritable cottage industry based on pitting the democratic peace proposition against a variety of control variables (Maoz and Russett 1993; Russett and Oneal 2001). Bueno de Mesquita and Lalman (1992) tested their expected utility predictions

along with other predictions drawn from power-based stories but failed to include other non-power-based stories. A few broader studies move beyond a set of closely related alternative explanations to examine several "likely suspects" in the hunt for the correlates of international conflict behavior (Bremer 1992; Huth, Bennett, and Gelpi 1992; Oneal and Russett 1999a). In none of these cases, however, do the authors attempt a comprehensive examination of competing explanations, nor do they systematically assess the strength of the various arguments' predictive power.

In the domain of formal rational choice theory, the lack of comparative testing is particularly noticeable and unfortunate. While the expected utility approach has been the target of both theoretical and empirical criticism, using both normative arguments and empirical case studies (e.g., Jervis, Lebow, and Stein 1985), formal models of international conflict have received remarkably little empirical evaluation with large-*n* statistical tests, particularly against a wide range of alternative explanations or predictors. This dearth of empirical testing led, in part, to Green and Shapiro's blistering critique of rational choice models and their advocates in *Pathologies of Rational Choice Theory* (1994, 7). There they find weak empirical support for rational choice theory generally and suggest that, "Despite its enormous and growing prestige in the discipline, rational choice theory has yet to deliver on its promise to advance the empirical study of politics. . . . we believe that this claim can be defended across the board."

While formal rational choice models are not the only arguments that suffer from a lack of systematic empirical testing, advocates of the rational choice approach make particularly strong claims about the power of expected utility theory. Green and Shapiro also make sweeping and speculative claims about the (lack of) explanatory or predictive power of rational choice theories in all fields of political science. Ironically, they offer no compelling evidence that alternative approaches might perform any better. Their study also suffers from a notable omission—they do not address the rational choice literature on international politics at all, where there have been a few serious efforts to conduct some rigorous tests (Bueno de Mesquita 1980; Bueno de Mesquita and Lalman 1992; Smith 1996a, 1999; Signorino 2000; Filson and Werner 2001).

There are a few noteworthy attempts to execute a comparative analysis of the empirical literature on international conflict. For example, in Gurr's *Handbook of Political Conflict* (1980) Bueno de Mesquita presents a review of the theoretical and empirical claims of balance of power arguments, power transition models, system structure conjectures, status inconsistency, arms race models, deterrence, and the

externalization of domestic conflict. He rejects some approaches (such as balance of power) because of contradictions in their internal logic. His review, while helpful for understanding the reasoning underlying a variety of common explanations for conflict, does not provide any systematic empirical evidence directly comparing the various arguments' relative explanatory or predictive power. More recently, John Vasquez's *The War Puzzle* (1994) provides a self-described meta-analysis of the literature on interstate war. Vasquez similarly makes no overall empirical comparison of the various theories or conjectures that he identifies. Numerous other edited volumes on international conflict exist as well that take a generally similar approach, where each chapter in a volume takes a different tack on the problem of international conflict by focusing on a single argument or approach (e.g., Midlarsky 1992, 2000). While these works help us understand the logic and possible strengths of the various arguments in isolation, they do not provide empirically based comparative hypothesis testing.

Since no project can hope to specify all of the possible variables expected to correlate with the onset of violent conflict, and since few research designs can handle propositions drawn from different levels of analysis, these commonly found testing practices and collective assessments might appear quite sensible. However, the lack of broader comparative testing has had several unintended consequences. Because of ad hoc variations in sets of control variables and the populations of cases across studies, we continue to be uncertain about what explanations or descriptions of the precursors of international conflict are most likely valid. For example, the basic question of whether a relatively equal balance of military and industrial capabilities between two nation-states increases the likelihood of peace or war remains in dispute, even though many policymakers assume that an equal balance of capabilities makes international stability more likely. In the absence of such testing and rejection of unsupported arguments, advocates of various models or empirical conjectures make claims about the power of their explanations and descriptions of historical events that are, in reality, unsustainable.

These authors argue that the regularity they identify (such as the so-called democratic peace) is useful and can stand on its own. This is because they claim that the supposition can be used to make unique predictions about future events (based implicitly on the assumption that the past is a good predictor of the future, something our stockbrokers assure us is a misguided way to pick stocks) or can provide improved understandings of past happenings.[2] Some claim that their "theory" is the best at explaining the events in question and that competing expla-

nations make at best marginal contributions to our understanding of politics. Readers should examine closely claims by those who have apparently identified novel explanations of international politics, as these claims frequently have minimal empirical (and sometimes logical) referent (e.g., Van Evera 1999). When a study presents the apparent effects of different explanations in carefully constructed settings, we must recognize that other excluded factors might be more important than the factors included in the analysis and that the subsequent inclusion of the potentially confounding explanations could even reverse the direction of the previous findings (Gowa 1999; Mansfield and Snyder 1995).

Finally, the lack of large-scale comparative testing has led to recurring and fruitless arguments over what approach to understanding international politics is "best." Scholars often assert the superiority of one perspective or paradigm over another. We see this with "realists," or those advocating the realpolitik approach often associated with Morgenthau, Lippman, Kissinger, and others, arguing that domestic political processes are important only at the margins. In like fashion, rational choice proponents argue that expected utility maximizing behavior explains the critical part of international conflict behavior, with formal theorists claiming that mathematically deduced theory is superior to more informal natural language approaches (Walt 2000). Absent broad comparative tests and rigorous evidence, claims that the realist approach is better than approaches based in domestic politics are certainly premature, despite prominent scholars' assertions to the contrary.[3]

TOWARD A METHOD OF COMPARATIVE HYPOTHESIS TESTING

In dealing with the problems discussed previously, we start with three priors about comparative hypothesis testing and the evaluation of the relative predictive power of international politics explanations. First, we believe (and demonstrate later) that no single current theory, conjecture, assertion, or description stands alone as a dominant predictor of international conflict. In subsequent chapters we will show that there is no single indicator for the onset of international conflict with predictive power approaching a level that we would consider high. None of the variables associated with the arguments we investigate accounts for 75 percent, 25 percent, or even 5 percent of previous conflicts. Instead, we will show that a combination of several factors is necessary to understand the initiation and escalation of international conflict. As we subsequently demonstrate, many different conjectures about international

conflict are simultaneously valid, as each operational indicator for a given explanation or description accounts for unique aspects of the conflict initiation and escalation process. From this perspective, international conflicts arise through the confluence of multiple weak factors. This suggests that debates such as whether realist or domestic politics approaches are "best" simply miss the point. A more fruitful question to ask is how much, or when, or under what conditions does each conjecture appear most consistent with some aspect of international conflict behavior.

Our second prior is that, while no single factor is adequate to explain international conflict, we believe that with careful empirical tests we can show that some variables have substantially larger predictive power associated with them than others do. While we conclude that there is no single dominant explanation of international politics, we do not suggest that every model or argument is valid or equally useful. Even if there is, as yet, no governing theory of international politics, we need not descend into some postmodern, antipositivist intellectual anarchy, where all arguments are of equal relative value. Rather, our findings point to the need for more, and more careful, theory development in order to accommodate the insights gained from various lines of research drawing from multiple levels of analysis.

This leads to our final prior. We believe that it is appropriate, necessary, and possible to include factors from multiple levels of analysis in comparative hypothesis testing, as we described previously and execute subsequently. If variables from any level of analysis are to influence international affairs, they must ultimately do so by affecting the decisions of individual actors in the system. Even system-level factors must ultimately influence the outcomes we observe by affecting the decisions of states' leaders since the system is not an autonomous actor, somehow acting on its own to directly influence states' behavior. Rather, identifiable characteristics of the system provide incentives or conditions to which actors may or may not respond.

Given these priors and our Lakatosian approach, we argue that the best way to draw conclusions about the relative power associated with various factors assumed to predict conflict is to estimate a single statistical model incorporating as many of these factors as possible. We do this through a process that we refer to as comparative hypothesis testing. Every testable model or empirical conjecture explicitly or implicitly argues that some measurable variable should correlate with some observable behavior, in our case international conflict. In this book, we focus on a simultaneous analysis of a large set of these variables, which we draw from a variety of arguments, conjectures, and empirical sup-

positions, cast at multiple levels of analysis. It is possible to include a wide range of key variables drawn from multiple levels of analysis because we use the directed dyad-year as the unit of analysis in our model (more on this later). Since it is possible for multiple explanations to predict the same events, to sort out the competing arguments, it is critical to evaluate them simultaneously, as we cannot have confidence that any particular story actually explains novel events until we control for a range of other competing explanations. Without controlling for a range of other hypotheses, we also cannot assess a conjecture's relative predictive power. Of course, there may be limits to what we can learn with this approach. For example, the variables suggested by the competing explanations may be so collinear that we cannot tell which factors are systematically related to conflict and which are not.

For brevity's sake we might be tempted to lump together under the single and parsimonious moniker "theory" various rigorously specified deductive theories, empirical conjectures, carefully stated hypotheses, and the occasional hunch. While this would spare the eye, it does considerable injury to the word "theory," and so we refrain from the standard practice of referring to all predictive or explanatory arguments about the nature of international politics as "international relations theory." Few so-called theories of international politics contain deductively formal logic or even careful attention to internal consistency and instead pose loosely specified relationships among typically vague concepts along the lines of "more of X will probably lead to more of Y." The lack of internal logic and conceptual clarity is particularly troubling to scholars who argue that most models of international politics are actually, or should be, theories of strategic interaction (e.g., Signorino 1999; Lake 1992). If Lake is correct, then testing causal arguments about relationships between variables and international conflict across levels of conflict will prove particularly challenging and may require statistical estimators whose design matches the strategic causal logic of a particular argument. Under some circumstances of strategic choice, particularly in situations where the logic behind signaling games is particularly important, the unobserved effects of variables such as balance of forces may be more powerful than, and in the opposite direction of, the observed effects, thereby leading to results opposite of what we might expect otherwise (Fearon 1994a; Smith 1996a; Ritter 2001).

In our analysis, we do not redevelop the strategic logic of the typically casually stated arguments we test. Rather, we take the arguments as given by the original authors and test the hypothesized relationships and measures and include them "as is" in our statistical models, even if it means that many of the hypotheses we test are not drawn from carefully

specified theories but instead represent an empirical hunch or conjecture. As such, many of the tests here are not really tests of theories or even of careful causal logic. Rather, in the chapters that follow we present careful tests of numerous empirical propositions about the onset of war; a few of these arguments have been deduced from carefully laid out theory, but most of them have not. In the latter case, we are establishing sets of "facts" that need explaining rather than providing tests of a causal explanation of conflict.

If the conjectures we include in our tests suffer from flaws in their internal logic, or if the operational variables do not accurately measure the purported causal influences, our empirical findings will likely be inconsistent with the original authors' predictions. As we reach the end of this book, we will revisit this theme and address in detail the somewhat startling paucity of rigorous theory in international politics. We end with the conclusion that the field of international politics is undertheorized, particularly in terms of the dynamic linkages between existing models and arguments.

In the remainder of this book, we will test the relationships of key variables that emerge from sixteen important explanations and descriptions of international conflict at multiple levels of analysis. We seek to discover which of these variables—and in turn what underlying explanations—are consistent with the largest number of empirical facts about the onset and escalation of interstate conflict while we simultaneously control for several alternative and frequently competing predictors of violent interstate conflict. They include the following:

State Level of Analysis

1. Democratization
2. Polity Change and Externalization of Violence

Dyadic Level of Analysis

3. Alliance and Defense Pact Membership
4. Arms Races
5. Balance of Power
6. Democratic Peace
7. Expected Utility
8. Geographic Contiguity
9. Nuclear Deterrence
10. Power Transition
11. Rational Deterrence
12. Trade Interdependence

International System Level of Analysis

13. Economic Cycles/Kondratieff Waves
14. Hegemonic Stability
15. International System Polarity
16. Systemic Power Concentration and Movement

For some of these explanations, and in particular the expected utility variant we will test, the theoretical logic underlying the relationship between the explanatory variables and the onset of war is quite explicit. In other cases, we have largely ad hoc explanations for why some particular factors appear related to conflict. For instance, "democratic peace theory" is not one clear theory. Rather, the so-called democratic peace is a relatively strong empirical regularity in search of a theory, or explanation, with scholars pursuing multiple arguments about the causes of that regularity (e.g., Maoz and Russett 1993; Gartzke 1998; Gowa 1999; Schultz 1999; Bueno de Mesquita et al. 1999, 2001; Reiter and Stam 2002). The purpose of this book is not so much to test the theoretical explanations for the facts as we know them but rather to more carefully establish the facts for which we need to develop theoretical explanations. Typically, the explanations of conflict that fall short of the bar that constitutes true theory emerge inductively from one or two observations and as such are actually better understood as descriptions of events rather than theories of political behavior. We include several of these important conjectures in our analyses even if there is no deductive or rigorously specified theory behind them. For instance, most scholars reasonably include geographic contiguity in empirical models of conflict because they understand that many states cannot fight across long distances, although they do not have an explicit theory of force projection on which to build this expectation.

In the chapters that follow, our analysis proceeds in a series of careful steps. First, we evaluate whether the empirical predictions associated with each description or explanation stand up in the presence of other explanations. Given that most tests include only a few control variables, this first step is important for those seeking to establish the power and reliability of current predictors of international conflict. The crux of our analysis is to find out whether the empirical predictions drawn from the basic international politics literature continue to find empirical support after we include many other competing explanations in the analysis.

Following our basic hypothesis testing (whether an operational indicator makes a statistically significant contribution to explaining the

outbreak of conflict), we evaluate the conjecture's associated relative predictive power. Here our aim is to ascertain whether there are certain dominant variables that are associated with more of the observed conflict than others are. In our analysis, we actually find the opposite. Rather than there being a small number of factors consistent with a majority of the conflict behavior variance, we find that literally dozens of variables have statistically significant but substantively weak associations with international conflict. There is no clearly dominant factor (or even a small set of factors) systematically associated with international conflict behavior. This finding has important implications both for how we study theories of international politics and for the formulation of public policy. It suggests that the search for, or emphasis on, single factors or paradigms is misguided. It also leads us to conclude that we should turn our attention to searching for more interactive and dynamic explanations of interstate conflict that can take into account a multitude of factors from multiple levels of analysis. We conclude with observations about the nature of research across multiple levels of analysis and a demonstration that we can integrate information across multiple levels to make successful predictions about the relative likelihood of interstate conflict at both the dyadic and system levels.