

Domestic Politics and International Relations

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In reviewing the history of portions of international studies I reflect on how we might best advance knowledge. I dwell on two issues: questions of method and the urgency of refocusing our efforts on leaders and domestic affairs as the centerpiece for understanding the world of international relations. I argue that scientific progress is best made by combining three methodological approaches in our research: formal, mathematical logic to ensure internal consistency in arguments about complex and contingent relations among variables; case studies and archival research to evaluate verisimilitude between theory and action; and statistical analysis to establish the generality of the hypothesized relations among variables. Often such methodologically diverse and progressive research will best be accomplished by encouraging collaboration rather than by perpetuating the current norm of penalizing co-authorship especially among junior scholars. I offer concrete examples of advances in knowledge achieved through the employment of mathematical reasoning and statistical analysis as many have cast doubts about the substantive contributions of these particular approaches. My perspective is, of course, personal and may not be shared by many others. I set out my thoughts, therefore, with the hope that they will stimulate constructive debate and dialogue and that they will serve to integrate diverse approaches to international affairs.

In this essay I review the history of portions of international studies and ponder its future. In doing so, I reflect on how we might best advance knowledge. I dwell on two issues: questions of method and the urgency of refocusing our efforts on leaders and domestic affairs as the centerpiece for understanding the world of international relations. My perspective is, of course, personal and may not be shared by many others. I set out my thoughts, therefore, with the hope that they will stimulate constructive debate and dialogue and that they will serve to integrate diverse approaches to international affairs.

International relations is a venerable subject. Before there were nations, people were already studying international relations. Students of international affairs are the heirs and descendants of Sun Tzu, Kautilya, Thucydides, and Herodotus, all writing about 2,500 years ago. Each sought to glean laws of human nature from dual sources: the record of history and the power of reasoning. Thucydides stated this purpose clearly when he wrote, "The absence of romance in my history will, I fear, detract somewhat from its interest; but if it be judged useful by those inquirers who desire an exact knowledge of the past as an aid to the interpretation of the future . . . I shall be content" (1961, Book I). Content, indeed; he should be.

Our intellectual forebears shared the conviction that one fundamental law of international relations is that such politics is shaped by and rooted in domestic affairs. As Sun Tzu observed, the first of the five constant factors governing war is the principle of the moral law which “causes the people to be in complete accord with their ruler, so that they will follow him regardless of their lives, undismayed by any danger” (1983:11). Sun Tzu’s moral law is remarkably reminiscent of Caspar Weinberger’s later doctrine restraining the use of force to those occasions when the public will is firmly behind such action.

We can today still take many lessons from our field’s founders, though we should be careful neither to canonize their words nor to beatify them. Many of their insights withstand the test of time, but equally, many do not. We have, in fact, learned a great deal in the intervening two and a half millennia. Progress in knowledge is our abiding companion. Still, we wrestle with how best to integrate the high politics of international affairs and the daily struggles of domestic political, economic, and social concerns that motivate the actions of citizens and leaders. And still we debate what seems to have been obvious to our predecessors: to gain understanding, we need to integrate careful empirical analysis with the equally careful application of the power of reason.

I believe that when evidence and logic are joined together, they bring great gains in the confidence we can have in our knowledge. One path to insight is the detailed analysis of individual events; the method that today we call the case study. This technique, often relying on archival research, proves to be a fertile foundation from which new and interesting ideas germinate, ideas that suggest hypotheses about regularities in the world that are worthy of being probed through close analysis of individual events and through careful repetition across many events. The close probing of case study analysis enhances the prospects of achieving verisimilitude as it brings the proposed explanation into close proximity with the known details of the situation. It does not, however, provide evidence that the specific details are germane to other, similar occurrences.

A second method investigates general patterns among cases within a class of situations, patterns that can only be discerned by examining large numbers of events as in statistical analysis. This is especially true when the predicted pattern involves a probability distribution across possible outcomes rather than a deterministic result. Few expectations in social science, or even in important branches of a seemingly more exact science like small particle physics, are deterministic. Statistical studies uncover ideas about the general orderliness of the world of international affairs through the assessment of regular patterns tying dependent variables to independent variables. While the case study approach provides confidence in the internal workings of specific events, statistical analysis probes the generality or external validity of the hypotheses under investigation, the very objective to which Thucydides pointed when speaking of the pursuit of exact knowledge of the past as an aid to the interpretation of the future. Statistical patterns offer evidence about how variables relate to one another across similar circumstances, but fail to illuminate the specifics of any single case.

A necessary condition for an explanation to be true is that it passes tests of both its internal and external validity. However, the two empirical methods of case analysis and statistical assessment alone are insufficient to establish that a given conjecture, hypothesis, hunch, or observation provides a reliable explanation. After all, correlation does not prove causation, though a strong correlation certainly encourages a quest for causality and the absence of correlation often provides evidence against causality. In both instances, in the pursuit of internal and external validity, we grow confident in our observations and the prospective causal ties between independent and dependent variables when what we test is designed as a direct exploration of the *logic* of action. This third methodological approach—the logic of action—furnishes an explanation for the regularities we

discern. The logic of action establishes the internal consistency of the claims we make or the observations we report. The test of logical consistency establishes a coherent link between those observations and evidence that *describe* the world and the theory purported to explain why our observations look as they do. Without logical consistency any argument can be maintained and nothing can be proven to be false. Reliable progress in our understanding of international affairs is bolstered by and may in fact require the application of all three methods.

Let me illustrate what I mean with a further brief foray into our intellectual roots. The High Middle Ages provides a window through which we can see the power of these three methods of analysis. The use of logic and evidence to explain behavior surely was the intention of one of the early luminaries of international political economy, Thomas Aquinas (1991), who was deeply concerned with logic and evidence in his assessment of the just price, a factor that remains of great importance today as we study globalization, and international financial and commercial exchange. It was, likewise, a fundamental interest of the twelfth-century monk, Abelard, who was determined to uncover the logical principles that might explain apparent contradictions in the teachings of the Christian Scriptures. And it was at the heart of the fourteenth-century essay *De Potestate Regia et Papali*, that is, *On Royal and Papal Power*, written by John Quidort of Paris around 1302.

John challenged Pope Boniface VIII on behalf of Philip the Fair, King of France, in what was the main international conflict of the day, a conflict that resulted in war, in the seizure, imprisonment, and death of the pope, and in the gradual decline of the Catholic Church as the hegemonic power in Europe. In defending the claims for French sovereignty, John of Paris resorted to all three methods of analysis: logical exegesis, detailed reference to evidence in history (and Scriptures), and an appeal to the general pattern of relations between the Church and monarchs over many centuries in his effort to show that the pope had no authority to depose kings, contrary to Boniface VIII's claim in his epistle *Unam Sanctum*. Indeed, John, foreshadowing modern debate over methods of analysis, warned of the dangers of generalizing *solely* from a single case. He observed of the deposition of an earlier French king, Childeric III, allegedly by Pope Zachary, that "[i]t is inappropriate to draw conclusions from such events which . . . are of their nature unique, and not indicative of any principle of law" (167). He went on to state principles of law and to document them empirically. John clearly understood that a case history or example *by itself* is insufficient for stating general principles. In fact, the case of Childeric and his actual dethroning by Pepin III for domestic reasons and not by the pope, illustrates the dangers of too readily generalizing from a singular event.

Abelard and John Quidort both suffered for their views and their intellectual rigor in challenging the accepted ideas of the day. Each endured the malice of church authorities who sought to silence them—alleging that they did so to protect against Abelard's and John's intolerant views—rather than allow the competition of ideas to sort out the wheat from the chaff. We might all benefit by reflecting on how often, even today, ideas and colleagues are stifled not because of inadequacies in their studies, but because of personal stakes by those whose views or positions are challenged by the results of research. Differences in perspective are best resolved through the competition of ideas openly and freely expressed rather than by intimidation and suppression of logic and evidence.

So what can we learn from the experiences and studies of our intellectual ancestors? We certainly can extract lessons from their focus on the interdependence between international and domestic politics. For most of the more than half century since the end of World War II, a great part of international relations research has focused on the state and the international system. These studies were useful and productive in uncovering knowledge and insight and also in

revealing significant areas in which such an outlook proves inadequate. It is through just such revelations that theory and knowledge progress.

For instance, the focus on system structure which has beneficially occupied so much research clearly also leads to the mistaken implication that the Soviet Union, presumptively like all states, was immortal. Today we can see that this view is flawed, and yet the immortality of states, especially powerful states, is taken as axiomatic by many theories of system structure. To salvage these system perspectives necessitates their wholesale reconstruction and metamorphosis into radically different and probably incompatible theories. Alternatively, we can accept the idea that they are falsified. Resolution of these choices is one of the exciting challenges facing us today. By debating these matters and by being explicit about what constitutes a sufficient failure for a theory to be rejected we can reduce the set of plausible explanations of patterns of action in international affairs. In that way we come closer to uncovering whatever laws of international relations may exist.

To me personally it is a little odd that the view of states as immortal seekers of national security has dominated discourse for so long. I say it is odd because long before realist accounts came to dominate debate there were ample instances of states that died, their leaders having committed national suicide. Certainly the Third Reich's leaders pursued policies that resulted in 1945 in the destruction of their state, a destruction that was not a certainty in 1939, but assuredly represented a significant probability given the policy risks taken in the name of the German people. The same, of course, can be said with equal vigor about the policies pursued by the Ming emperor at the battle of T'u Mu, or policies followed in 1492 by the leaders in Granada, or by Austria-Hungary in 1914 or by many other states throughout history. Now, with these examples firmly in mind and with the knowledge gained from the effort to treat the international system as a dominant constraint and states as the wellsprings of action in international affairs, I believe it is time to bring the study of citizens, leaders, and leadership back to the forefront.

Leaders, not states, choose actions. Leaders and their subjects enjoy the fruits and suffer the ills that follow from their decisions. Alas, leaders seem to be motivated by their *own* well-being and not by the welfare of the state. The state's immortality beyond their own time is secondary to the quest of leaders for personal political survival. How else can we explain the long survival in office of such figures as Saddam Hussein, Fidel Castro, Mobutu Sese Seko, or Ferdinand Marcos even while they impoverished their nations. It is a fact of politics that leaders who bring war and poverty to their nation last in office substantially longer than leaders who bring peace and prosperity. For instance, autocrats who produce remarkable levels of prosperity have a 10 percent chance of lasting in office for an impressive 13.5 years, but if instead they produce exceptional opportunities for their cronies to get rich by exploiting a vibrant black market, their same survival prospect rises to a prodigious 17.7 years (Buena de Mesquita, Smith, Siverson, and Morrow, forthcoming). Plainly, effective public policy does not serve the personal political interests of such leaders nearly as well as venality and corruption. Surely there must be domestic incentives that explain this odd relation between the promotion of national welfare and personal political success. Without bringing leaders and their domestic incentives back to the forefront of our research, I believe that we cannot really hope to understand the motivations and constraints that shape international politics and economics, the very factors we hope to explain.

We have the good fortune to live in a time when our subject is ripe with competing ideas and the tools with which to sort them out. Those tools include, as I suggested earlier, careful archival research to evaluate whether specific decision makers *really* analyzed problems in the ways our theories suggest; statis-

tical analysis to see if our explanations fit well the broad history of many decision makers and their states; and explicit formal logic to ensure that the ideas tested in archives and through statistics are logically coherent.

Mathematics is the best tool we have for pursuing careful logical analysis of the complex problems we study. It is the best tool because the alternative—ordinary language—contains much greater ambiguity and imprecision in meaning. With ordinary language we run too great a risk that arguments that *seem* to make sense, when subjected to the closer scrutiny of formal logic fail the test of reason. Yet today there are still many who denounce the application of mathematical reasoning and quantitative assessments as conceits that cannot or do not illuminate understanding. Often they argue that the interesting problems of politics are too complex to be reduced to mathematical equations even though it is *exactly* when dealing with complex problems that mathematics becomes an attractive substitute for ordinary language because it is in *complex problems* that errors in informal logic are most easily made and hardest to discover. Therefore, I wish to suggest that the best hope we have for uncovering the laws of international affairs, laws long hidden from our understanding, lies in the application of these methods and their empirical linkage to archival and case study techniques.

In suggesting the importance of mathematical and statistical rigor, I do not deny or diminish the important benefits of close historical studies. I have already emphasized the *necessity* of such studies as one prong in any scientific endeavor. Nor do I suggest that every researcher must engage in all three techniques. That is as inefficient as it is impractical. Rather my hope is to facilitate our appreciation that individual case analyses, *by themselves*, as John of Paris aptly noted seven centuries ago, are of their nature unique and not indicative of general laws. We uncover such laws by combining techniques of investigation (Haber, Kennedy, and Krasner, 1997; Bueno de Mesquita and Skinner, 1998; Goemans, 2000; Levy, 2001). We should, therefore, encourage the productive collaboration of colleagues with case expertise, statistical acumen, and mathematical knowledge. Such teams of researchers are likely to greatly uplift the confidence we can have in the insights we uncover. One step toward that goal is to ensure that our students, both graduate and undergraduate, are reasonably educated in all these methods so that they can evaluate the politics that surround them. A second step is to reward rather than penalize young colleagues for co-authorship when their research requires multiple skills.

The gains from the case study method are well known to most of us. There seems, however, to be sincere doubts about the gains from mathematical and statistical rigor. To address these doubts, I ask now and answer briefly, What do we know today as a result of the combined benefits of quantitative and mathematical explorations into the logic of international affairs and the long and broad record of history?

We should acknowledge the lively, contentious and heated debate over exactly what, if anything, constitutes the democratic peace. The statistical evidence in support of the idea that democracies rarely fight wars with one another is so strong as to have prompted a rich and forceful literature that commands a notable impact on foreign policy decision making (Maoz and Abdolali, 1989; Bremer, 1992; Maoz and Russett, 1993; Russett, 1995; Ray, 1995; Oneal and Russett, 1997; Maoz, 1998). Here is an area where statistical patterns have uncovered what may well be a law of nature, a law that could not be discerned by observing only a few individual examples. The exact statement of that law remains open to debate and is the subject of intense scrutiny among those who attempt to deduce propositions from formal models and logic. Is the apparent relative peace among democracies a consequence of shared values and norms, as some argue (Maoz and Russett, 1993; Dixon, 1994), or the product of specific constraints on action that make it particularly difficult for leaders who depend on

broad support to wage war, as I and others have maintained (Morgan and Campbell, 1991; Bueno de Mesquita and Lalman, 1992; Bueno de Mesquita, Morrow, Siverson, and Smith, 1999), or is there some other, as yet uncovered explanation that best accounts for the numerous regularities that collectively are drawn together under the label, the democratic peace? So strong are the statistical patterns that policymakers as well as scholars have embraced the idea of a democratic peace.

Indeed, so strong is the impetus among policymakers that many believe an all-democratic world, if it is ever attained, will be a peaceful world. These sanguine beliefs require a note of caution. Another strong statistical and logically deduced pattern shows that democracies are frequently created through war. Leaders pursue war for different reasons, but one apparently is domestic inducements to change a foe's policies by altering its regime. Such a motivation is common among democratic leaders as illustrated by regime changes imposed by the democratic victors in World War II. A glance at the Thirty Years' War makes apparent that such a motivation was remote indeed in a world led by monarchs. None were deposed at war's end. The statistical evidence for the generalization that democracies fight more often than autocracies to overthrow regimes and impose new leaders is strong. Autocrats fight for spoils, to gain territory and riches, and only rarely to overthrow their foreign foes (Morrow, Bueno de Mesquita, Siverson, and Smith, forthcoming).

Another strand of fundamental insight comes from the use of repeated games to understand the roots of cooperation. The prisoner's dilemma is a model situation in which the absence of trust or credible commitment can prevent cooperation. Through repeated play, however, we know that cooperation can be achieved between patient decision makers (Taylor, 1976; Axelrod, 1984; Bendor and Swistak, 1990). This insight is important in uncovering prospective solutions to enduring conflicts.

Equally significant, however, we also know through repeated games that patience does not always ensure cooperation. Indeed, the chance for cooperation can be lost through patience; that is, through a long shadow of the future. When costs endured now in preparation for war provide improved prospects for gains later through military conquest, then patient decision makers more keenly appreciate the future benefits than do their impatient counterparts. The result is that it is more difficult to deter patient leaders when current costs presage greater subsequent gains. Aggression rather than cooperation is the prospective consequence, as has been demonstrated in studies of spending on guns or butter (Powell, 1999). So, when costs precede benefits, patience is not a virtue that stimulates cooperation; it can be a liability that promotes conflict. The sequence in which costs and benefits are realized alters the prospects of peace. This is an important insight for those who shape agendas in international negotiations, an insight that was not revealed either through case analysis or statistical assessment. It was uncovered through the rigorous, formal logic of game theory.

In still another area, quantitative studies leave little doubt that rivals with about equal power are more likely to end up at war than are foes with very different wherewithal (Organski and Kugler, 1980; Kugler and Lemke, 1996; Tammen et al., 2000). Why this is so remains a contested question that has been subjected to formal as well as informal logical analysis (Organski and Kugler, 1980; Bueno de Mesquita and Lalman, 1992; Kim and Morrow, 1992; Powell, 1999). That it is so seems little in doubt. It is a lesson that shakes the received wisdom that a balance of power is an asset in the pursuit of peace and it is an observation that warrants close and careful logical examination to uncover the explanation for the observed pattern of action. It is an oft-replicated statistical result that contradicts arguments by some of our most venerable and respected intellectual ancestors.

Only recently have all three methodologies—formal logic, detailed case analysis, and large-sample statistical assessment—been marshaled to evaluate how international affairs are shaped by and give shape to domestic politics. We know, for instance, that leaders in democracies are more likely than autocrats to be punished by losing office if they lose long and costly wars (Bueno de Mesquita and Siverson, 1995). This fear of being turned out of office by domestic constituents appears sufficient to diminish the willingness of democratic leaders to risk war, making them more inclined to negotiate their way out of disputes except when the odds are overwhelmingly in their favor (Fearon, 1994; Schultz, 1998, 2001). Perhaps that is why democracies rarely fight each other and yet show no special reluctance to engage in wars of colonial and imperial expansion against extremely weak foes, as has been shown theoretically and empirically (Bueno de Mesquita, Morrow, Siverson, and Smith, 1999; Bueno de Mesquita, Smith, Siverson, and Morrow, forthcoming). After all, it is most unlikely that two leaders simultaneously believe that they are nearly certain of winning a war. If both are democrats, the failure to hold this belief is sufficient to lead one or the other to seek a negotiated peace or to acquiesce in the face of the threatened or real use of force. Autocrats, being less likely to be punished at home for policy failures, are more willing to gamble on war even when their chances of victory are not so great. Indeed, under dire circumstances, such leaders take extreme risks, gambling on resurrecting themselves politically even when failure can mean the utter destruction of their state (Downs and Rocke, 1995). Finally, I should note that formal models have influenced not only academic understanding, but also practical policymaking. One such model is described by the United States government as producing “forecasts . . . given to the President, Congress, and the U.S. government.” These forecasts are described as “a substantial factor influencing the elaboration of the country’s foreign policy course” (*Izvestia*, April 3, 1995). So, contrary to what we sometimes hear, policymakers in the United States and elsewhere rely in part on inferences drawn from mathematical models of politics to choose their course of action (Feder, 1995). I could go on with innumerable examples of studies that show the benefits for foreign policy of political partisanship and “yes men” (Calvert, 1985), or explanations of alliance behavior (Smith, 1995), or of trade policy (McGillivray, 1997; Milner and Rosendorff, 1997), and so forth, but the point is clear. We have learned much about international affairs through mathematical reasoning and statistical assessments.

When we examine international affairs through the lens of domestic decision making we provide a way to think about how properties of the international system are shaped by local considerations as part of the larger strategic fabric of politics. Systems become bipolar or multipolar, balanced or unbalanced, nuclear or nuclear-free, polluted or clean, growing or contracting because of the interdependence among individual decisions. International politics are not given to us as some predetermined exogenous fact of life, wholly formed and shaped independent of our choices. International politics are formed by the aggregated consequences of our individual and collective decisions. I cannot help but reflect on the extent to which American policy toward the Kyoto Protocols, the Comprehensive Test Ban Treaty, engagement or disengagement in Europe, and commitment to nation-building or defensive security have been framed for at least the period 2001–2005 by the hole-punching skills of a few hundred Floridian voters with diverse interests regarding prescription drugs and, perhaps, little interest at all in foreign policy or international affairs.

Make no mistake about it, examining international relations as a form of domestic politics leads to radically different ideas and propositions than those that arise when we think of leaders as surrogates or fiduciaries for the well-being of the state and all of its citizens. That which makes a state strong and its citizens secure need not, and often does not, make a leader more secure in office. The

pursuit of good public policy, including security within national borders, can be disastrous for the welfare of leaders whose hold on office depends on the support of a small coterie of cronies. Those cronies are loyal as long as their leader enriches them rather than spending the nation's resources on benefits for citizens at large, benefits that come by diverting resources from the cronies (Bueno de Mesquita, Smith, Siverson, and Morrow, forthcoming).

When we construct theories in which the state is the focal actor we miss all of the institutional and political incentives that shape the policies leaders choose. And yet it is those policies—decisions to align or not, decisions to build up armaments or promote economic growth at home, and so forth—that determine whether the international system is balanced or not, bipolar or not, and on and on. That is why the study of international relations from a comparative politics perspective seems to me so fundamental to further progress in knowledge. International relations is, simply put, a venue for politicians to gain or lose domestic political advantage. From this viewpoint, concepts such as the national interest, grand strategy, and international politics as a domain distinct from foreign and domestic calculations are troubling. Our perspective is wholly altered by shifting to an outlook that understands international affairs as a normal and routine aspect of ordinary domestic politics.

None of us can be better scholars or teachers than those who seek to understand and convey to others the motives and principles governing individual choices and their consequences for world affairs.

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