

International Relations as a Social Science: Rigor and Relevance

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Progress in the study of international politics depends on systematic, rigorous theory and empirical testing. International Relations is most useful when scholars can identify with some confidence the causal forces that drive foreign policy and international interactions, not when they use their detailed empirical knowledge to offer opinions, however intelligent and well informed. Deterrence theory, the democratic peace research program, and the political economy of trade policy demonstrate the importance of both theory and empirical research in enhancing the understanding of international relations. The bargaining theory of war and open economy politics are the current frontiers of research on international relations and promise even greater understanding in the future.

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When the American Academy of Political and Social Science published its first volume in 1890, International Relations did not exist as an area of systematic inquiry.¹ The United States itself was only slowly awakening to the world beyond its borders. In contemplating the role this infant giant might play on the world stage, early thinkers on international relations simply extended the formal-legal approach that

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then dominated political science to the international realm.² The first scholars of international relations were mostly international lawyers who, as late as the mid-1930s, excluded the study of military strategy from the discipline because to allow serious discussion would violate the spirit of the Kellogg-Briand Pact, a multilateral agreement outlawing war (Kaplan 1983, 13). Yet as the United States flexed its muscles and claimed its position as a great power in World War I and a superpower after World War II, International Relations grew into a legitimate area of scholarly inquiry and, shedding its legal roots, found an institutional home in the larger discipline of political science.³

International Relations emerged as a social science relatively late. The first self-consciously positive rather than normative theories date from the work of Charles Merriam, Harold Lasswell, and Quincy Wright at the University of Chicago in the 1930s. Even as the behavioral revolution swept through political science, International Relations lagged. The value of a scientific relative to a historical or philosophical approach to world politics was still being actively debated in the pages of *World Politics*, then the discipline's leading journal, as late as 1967 (Knorr and Rosenau 1969). By the 1970s, social science triumphed, with the discipline becoming explicitly theoretical and empirical and its rewards going to those scholars who embraced a scientific approach.

Echoes of the tradition versus science debate are still heard in the halls of academe today in the calls for a more "open" approach to political science, such as that championed by the "Perestroika" movement in the American Political Science Association.⁴ In light of the continuing debate, the celebration of the six hundredth volume of the *Annals* seems a fitting place to reflect on the relevance and usefulness of International Relations as a social science.

Our primary argument is that progress in the study of international politics—including in making its lessons more relevant to policy—depends on more, not less, rigorous theory and more, not less, systematic empirical testing. This argument is sure to be controversial, as many assessments of the policy relevance of International Relations plead for midlevel theories, contingent propositions, and empirically grounded generalizations drawn from the intense study of particular cases.⁵ Although we do not object to these means, we believe that the accuracy—thus relevance—of international relations as a discipline requires that it become more scientific in approach. Although policy makers need situationally specific guidance, it would be a mistake to make this the ambition of the discipline. Relevance requires better theory and better-designed tests to fulfill the expectations and needs of those who make policy, or simply those who want to understand better our complex world. In short, International Relations is most useful not when its practitioners use their detailed empirical knowledge to offer opinions, however intelligent and well informed, but when they can identify with some confidence the causal forces that drive foreign policy and international interactions. The more we can claim to "know" with confidence, the more useful and relevant the discipline will be.

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The remainder of this article is divided in three primary sections. We first outline our argument in greater detail. In the second part, we highlight three examples that demonstrate the power and relevance of solid theoretical and empirical work in International Relations: deterrence theory, the democratic peace, and the political economy of trade. We then survey the theoretical “state-of-the-art” in international conflict studies and international political economy.

The Relevance of Rigor

International relationists have long been involved in foreign policy debates. The pages of thoughtful journals of opinion like *Foreign Affairs* or *Foreign Policy* are often filled by academics writing for broad audiences. And professors, of course, have frequently engaged in government service.

There will always be a need for policy-relevant expertise. Through a lifetime of study, even the most theoretically inclined academics accumulate substantial country- or policy-specific knowledge that can supplement that possessed by those in government. Universities are repositories of country and policy experts “on call” to buttress hard-pressed policy makers confronted with crises in countries or over issues for which they lack immediate knowledge.

Yet in nearly all cases, the academics involved in past policy debates have acted as individuals, not as the embodiments of the accumulated knowledge of scholars of international relations. Rather, the real test of relevance is what the *discipline* provides in the way of approaches, theories, and analytical tools that can be marshaled to explain why events happen and what can be done to alter the course of future events. International Relations makes its biggest contribution—is most relevant—when it is most “scientific.” These contributions are of two types: well-developed, coherent, and convincing theoretical arguments for which there is systematic empirical support; and empirical evidence for a regularity, “law,” or other relationship that appears to hold beyond a reasonable doubt. Only when International Relations brings science to the discussion does it have anything of enduring value to offer, beyond well-informed opinion. And observers, policy makers, and journalists should be most influenced when scholars have something that is of enduring value to say.

Theory is not a substitute for policy expertise, problem-solving abilities, or political experience; nor does it trump political struggle. Theories of international relations aim to capture general features of events and processes in ways that highlight their principal causes. Successful decision makers typically understand these forces intuitively, at least in the instances with which they are familiar. But explicit attention to scientific rigor can provide a degree of generality and clarity that might not be obvious even to experienced policy makers. Theory can also discipline the thinking of policy makers so that sloppy or wishful thinking does not lead them astray. The scientific study of international politics provides a logical and empirical check on attempts to draw inferences from the ideas or experiences of those who may be too close to the events they wish to influence. It would be a foolish policy maker who

relied only on the scientific approach for guidance, but well-specified theories and tests of international relations are an essential part of a sound policy-making process.

The Practice of International Relations

In this section, we provide three examples of research in International Relations that use rigorous theoretical or empirical methods in ways that are generally recognized to have enriched the policy-making process. The first example is that of a contribution that came out of purely theoretical work, deterrence. The second is an almost exclusively empirical finding, the pacific tendencies of democratic governments in their relations with one another. The third is the result of both theoretical and empirical rigor, the study of trade policy.

A. Nuclear deterrence theory

Nuclear weapons revolutionized the study of military strategy. Between 1952 and 1966, the theory of nuclear deterrence was developed and its implications largely worked out in a remarkable burst of intellectual activity hastened by the cold war and fears of nuclear Armageddon.⁶ Much of the theoretical work during this period was done at the RAND Corporation under contract to the U.S. Air Force by an interdisciplinary group of military strategists, scientists, economists, and political scientists.⁷ By the late 1960s, nuclear deterrence theory had become a central part of the rapidly growing field of International Relations. Many of the later contributions were made by scholars in the academy and by international relationists in particular.⁸

The core problems addressed by nuclear deterrence theory were how to prevent attacks on the United States (immediate deterrence) and its interests abroad (extended deterrence) and, simultaneously, how to wield nuclear threats to gain bargaining advantages over the Soviet Union (Trachtenberg 1991). The puzzles deepened as the two superpowers moved toward mutual assured destruction (MAD) by acquiring second-strike forces. Each superpower was now able to destroy the other even after absorbing a first strike. MAD challenged the credibility of deterrence itself since retaliation after a nuclear attack could serve no rational purpose. If the Soviet Union did launch an attack, it would be better for the United States not to retaliate with all its remaining weapons; the reserve could then be used to coerce concessions from the Soviets under the threat of attack or, at the very least, to deter future Soviet attacks. But if it was irrational to retaliate after a nuclear first strike, there was nothing to deter the other side from actually striking first, and deterrence would collapse. MAD also appeared to vitiate the coercive potential of nuclear weapons. If Moscow could threaten retaliation as well, then American threats intended to coerce the Soviet Union would be negated.

Central to understanding the stability of deterrence as well as the continuing leverage of nuclear weapons in a MAD world was found in Thomas Schelling's (1960) notion of threats that "leave something to chance." In other words, even if

retaliation was irrational, the *possibility* of retaliation—the chance that the president would react emotionally to a Soviet first strike or believe that he had no alternative but to respond in kind—would prevent an attack in the first place. Similarly, coercive bargaining under the shadow of MAD became a competition in risk-taking. The side with the greater willingness to risk the possibility that events would escalate out of the control—in other words, the side with the greater resolve—would still have leverage over other. If both knew the other's resolve with certainty, the more risk-averse side would simply concede to the demands of the other, and the demands of the second would be calibrated to the maximum the first would be willing to concede given its resolve. Crises would arise only if there was uncertainty over the resolve of one or the other party and would continue until one side reached its tolerance for risk or events really did spiral out of control.

[T]he real test of relevance is what the discipline provides in the way of approaches, theories, and analytical tools that can be marshaled to explain why events happen and what can be done to alter the course of future events.

It is hard to overestimate the importance of nuclear deterrence theory for the cold war. Although it was unevenly reflected in weapons procurement and force postures (see Jervis 1984), the theory's insights into brinkmanship guided nuclear doctrine and the conduct of nuclear crises for the United States—and possibly for the Soviet Union as well—through this long, tense period of competition. With the end of superpower rivalry, interest in deterrence has also waned. Nonetheless, the theory remains relevant and insightful even today. In a recent essay, Robert Powell (2003) extended the theory to a world of rogue states, rather than superpowers, and showed how it can be applied—with startlingly counterintuitive results—to national missile defense (NMD). Rogue states have been defined as countries willing to take greater than usual risks in pursuit of their objectives. Powell showed that such states will possess higher than normal resolve and are more likely to be able to deter the United States in high-stakes conflicts, especially those involving the possible overthrow of the regime. This confirms the intuition held by many that states with even minimal nuclear capabilities will be able to restrain the United States in conflicts of sufficient importance to their survival.

By working through the logic of deterrence theory, Powell (2003) also showed that, contrary to intuition, NMD is actually likely to increase (rather than decrease) the probability of a nuclear attack on the United States. Because NMD reduces the expected costs to the United States of a nuclear attack on its homeland, in any conflict it increases American resolve. That is, since the costs of a possible attack are lower, the United States will choose to run a greater risk of attack, all else equal. With a greater actual risk that events will escalate beyond our control, the probability that a nuclear exchange will occur must go up as well. Only if the missile shield is expected by the rogue state to work perfectly (or very nearly perfectly) will its resolve necessarily be less than that of the United States and will it capitulate before a crisis begins. At the same time, NMD gives the United States an expanded ability to coerce other countries—an ability that increases with the efficacy of the shield. The implication of the model is that rather than reducing the risk of a nuclear attack on the United States, with all its attendant dangers and risk of devastation, NMD will actually serve to increase America's coercive power over other states. By shielding the United States, it makes American power more usable. Although Powell himself did not draw this conclusion, this seems to us to help explain why foreign policy "hawks" have pushed NMD so hard over the past two decades. Whether one supports NMD or not, there is no doubt that deterrence theory and its implications are still relevant to today's world.

It is worth emphasizing that the development of deterrence as a body of thought was entirely theoretical. There has never been a thermonuclear war; indeed, the theory of nuclear war was developed in the interests of avoiding the very thing it studied. Although deterrence is a feature of conflicts other than thermonuclear war, the theoretical and empirical implications of a war with the potential to destroy entire societies could only be imagined. There are few ways to "test" deterrence theory against real-world data. Indeed, the broader implications of the theory for crisis behavior are complicated, at best, and poorly understood, at worst.⁹ And yet, this purely theoretical exercise governed American (and, arguably, Soviet) foreign policy for decades.

The power of the theory rests on the clarity of its logic and the force of its (untested) implications. Only by developing "pure theory," deducing the logical implications from plausible premises, and subjecting intuition to the demands of mathematical proof could nuclear strategists, policy makers, and the citizens whose fates depended on the conclusions rest secure—or reasonably secure—in the face of tremendous peril. Deterrence theory is relevant not in spite of this rigor but because of it. Perhaps more than any other area of inquiry, deterrence theory illustrates the power of a rigorous theoretical approach to international relations.

B. The democratic peace

Immanuel Kant first posited in 1795 that a perpetual peace could be formed from the principles of representative democracy, international law, and free and unrestricted international commerce (Russett and Oneal 2001, 29). At the time, the argument was purely philosophical, as there were precious few democracies,

only “thin” international law, and commerce still suffered under the bounds of mercantilism. Nearly two centuries later, however, scholars found systematic evidence of a separate peace between democracies (Small and Singer 1976; Doyle 1986). The empirical finding is specific: democracies are no more or less prone to war than other types of regimes, on average, and have disputes with one another, but they (almost) never escalate their conflicts with each other to the level of war. In the words of Jack Levy (1989, 270), the “absence of war between democratic states comes as close as anything we have to an empirical law in international relations” (see also Russett 1990, 123).

The democratic peace conflicted with the views of some scholars about the essential irrelevance of domestic politics (Waltz 1979) and with the beliefs of others that democracies were particularly weak and inept players on the world stage (Wright 1965, 842). As a result, the finding ignited a storm of critical commentary (see Ray 1995), but none of the criticisms displaced the central empirical claim that democracies are distinctly less likely to fight one another. It is nonetheless somewhat troubling that scholars have not been able to agree on a theory that explains the democratic peace (but see below). Many alternatives have been offered, but all have been found wanting or incomplete.¹⁰ Although the clear establishment of this empirical regularity provided it with a great deal of power, the absence of an accepted explanation weakened the breadth and depth of its influence.

Nonetheless, the democratic peace finding was welcomed by many policy makers. The ideal of democracy and the strategy of democracy promotion had long been centerpieces of U.S. foreign policy (Owen 2002), informing the idealism of both Presidents Woodrow Wilson and Franklin Roosevelt as they attempted to construct new global orders after the two world wars. By the early 1990s, the “academic” finding of a specific democratic peace had become a key component and justification for the Clinton administration’s vision for a new, post-cold war international order. In his 1994 State of the Union address, President Clinton declared that “ultimately, the best strategy to ensure our security and to build a durable peace is to support the advance of democracy elsewhere.” The justification for this strategy, he continued, is that “democracies don’t attack each other.”¹¹ Once democracy promotion emerged as an important theme in the foreign policy of the Bush administration, especially after the invasion of Iraq, President George W. Bush concurred, arguing that “the reason why I’m so strong on democracy is democracies don’t go to war with each other.”¹² The independent effect of the scholarly finding on policy is hard to discern. References to the scholarship most likely serve primarily to justify what these presidents and the United States wanted to do for other reasons. Nonetheless, the strength of the empirical law cleared an easy path by which it could enter the policy debate.

The democratic peace research program has two implications. First, this research became relevant outside the academy because of the lawlike status of a particular empirical finding. Scholarly consensus on the empirical regularity was a precondition for its entry into the policy arena. Second, the absence of a consensually accepted explanation, unlike in the case of deterrence, weakened the overall impact of the finding. The lack of a generally accepted theory meant that it

was not clear which dimensions of democracy might matter, and under what conditions. Deterrence theory was powerful because it was logically consistent, even in the absence of much empirical evidence. The democratic peace was influential because of strong evidence for it, but its ability to provide policy makers with a useful tool was limited by the lack of theoretical underpinnings.

C. The political economy of trade

A third scholarly research program that has found a substantial audience among policy makers and observers is that on the politics of trade policy. This literature had the advantage of combining theoretical rigor with strong empirical findings, and this helped turn it into the received wisdom of virtually all—scholars, journalists, policy makers—interested in the subject.

Countries have protected their producers from foreign competition for as long as there has been international trade. But the study of trade protection was, until quite recently, haphazard. Observers put forward a number of possible explanations for the phenomenon: concentrated interests exert pressure for protection, there is an asymmetry between concentrated producer interests in protection and diffuse consumer interests in free trade, protectionist interests can “logroll” their way to a level of trade protection that nobody really wants, and political systems might be structured in ways that favor protection over free trade.¹³ In 1965, Harry Johnson (1965, 183) famously ascribed many countries’ protectionism to a “taste for nationalism,” a willingness to “direct economic policy toward the production of psychic income in the form of nationalistic satisfaction, at the expense of material income.”

The literature was manifestly incapable of explaining the most striking facts of trade policy: there is great variation among countries in their overall levels of protection; protectionism has varied greatly over time, both globally and within countries; and even within a single country, some industries are typically strongly protected while others are not. Despite the centrality of trade policy to economic policy, and to the international economic order, despite the overwhelming support among economists for freer trade, and despite significant trends after 1945 in the direction of trade liberalization, there was little analytical apparatus available to explain which countries or industries were more or less likely to be protected.

Starting in the 1970s, systematic analysis of the problem advanced on both theoretical and empirical fronts. Trade theorists developed models about the winners and losers from trade protection; although the long-standing Stolper-Samuelson and Ricardo-Viner models generated different empirical expectations, they were rigorous and their implications were clear and easily tested (reviewed in Alt and Gilligan 1994). On the empirical front, economic historians, political scientists, and economists began looking at trade policy more systematically (Kindleberger 1951; Pincus 1975; Caves 1976; Gourevitch 1977). By the early 1980s, there was a substantial body of literature on trade policy. Both theory and empirical work pointed to the impact of organized special interests in the formulation of trade policy. By the early 1990s, the standard scholarly interpretation was that trade policy

was largely the result of the political influence of powerful interest groups, and this interpretation began to affect the policy and journalistic communities.¹⁴

Since the early 1980s, the study of the political economy of trade has advanced further: theories have become more rigorous, empirical work has improved, and scholars have incorporated more factors into their analysis. An important theoretical advance was the formulation of more rigorous models of the trade policy determination process, synthesized in Grossman and Helpman (1994), which built on more general models of institutions and decision making being developed in the study of American and comparative politics. On this basis, a new generation of empirical studies has emerged based more firmly on theory. These studies have confirmed the previous special-interest focus but produced clearer results and conclusions.¹⁵

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The theoretical and empirical accomplishments of the study of trade policy have had several effects. Among scholars, they have encouraged researchers to explore frontiers that address issues neglected in the existing literature. Two such frontiers are the impact of different kinds of domestic political institutions on trade policy outcomes and the impact of interstate bargaining and international institutions in the domestic political economy of trade. We return to these two areas below.

Another effect of the establishment of a more or less consensual academic view of trade policy has been its transmission to policy makers, observers, and other participants. Today, popular, policy, and journalistic analyses of trade policy issues simply take as given the hard-fought arguments and findings of the scholarly community: protection responds to interest-group pressures, economic characteristics of industries and products affect their ability and willingness to organize and receive favorable trade policies, organized consumers can mitigate pressures for protection, and so on. More than the specifics of the theories and findings, however, the general approach to trade policy is taken as a given by most.

Governments routinely use the insights of this approach to target specific industries through sanctions to influence the policies of other states (Rowe 2001) or to

extract concessions in trade negotiations and disputes. By understanding the political economy of trade policy, governments can better identify how to bring maximal pressure to bear on others while imposing the fewest costs on their own economies. The United States follows such an approach in trade bargaining, aiming its threats at the products of pivotal groups, such as when, in a mid-1990s dispute with the European Union, it prepared sanctions against perfumes and against very specific kinds of cheeses to attempt to affect the position of the pivotal French government (Goldstein and Martin 2000). When, several years later, the Europeans geared up to retaliate for American steel tariffs, they targeted products from similarly pivotal states, such as orange juice from Florida.¹⁶

International financial institutions such as the World Bank and International Monetary Fund routinely refer to political economy considerations in discussing member states' trade policies; national governments allude to political economy forces to explain (or excuse) their policies. Even activist groups incorporate political-economy factors into their analyses of national and global trends—such as when Oxfam International focused attention on the impact of protectionist agricultural policies in the developed world on poverty in the developing nations (Oxfam International 2004) or when the Environmental Working Group publicized the names of the beneficiaries of American farm subsidies.

Thirty years ago, most analyses of trade policies ascribed trade protection (whether favorably or unfavorably) to nationalism or other ideologies or to vaguely characterized special interests. That Johnson's suggested "taste for nationalism" now sounds quaint is evidence of how far we have come. Today's policy makers and analysts have decades of sound theoretical and empirical scholarship to build on and a much better understanding of the forces at play in the evolution of national trade policies.

The New Frontiers in International Relations Theory

Sound scholarship in International Relations has contributed to general understanding on the part of the public and policy makers. Nonetheless, International Relations as a discipline is still young, and the issues it has analyzed in any detail are limited. In the past decade, two new approaches have emerged that unite the subfields of international conflict studies and international political economy, respectively. In this section, we survey this state-of-the-art scholarship. These approaches create the foundations for a new, more rigorous and scientific approach to international relations. We anticipate this exciting research will eventually progress to the point of providing useful analyses for policy makers and other participants.

A. The bargaining theory of war

The field of International Relations has undergone a revolution in conflict studies. Where earlier approaches attempted to identify the attributes of individuals,

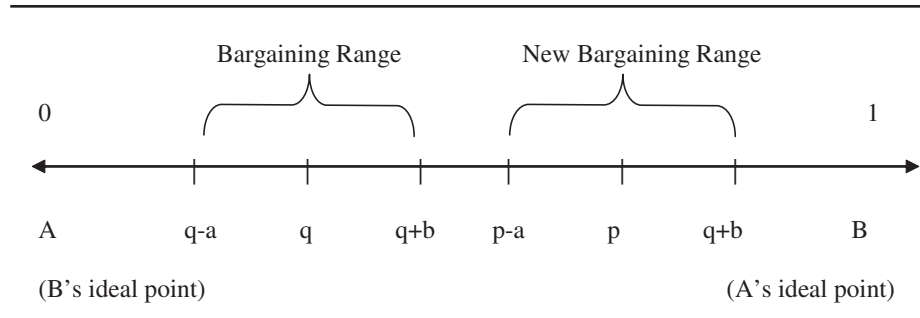
states, and systems that produced conflict, the bargaining theory of war now explains violence as the product of private information with incentives to misrepresent and problems of credible commitment.¹⁷ In this new approach, war is understood as a bargaining failure that leaves both sides worse off than if they had been able to negotiate an efficient solution. This general theory of violence, in turn, is similar to models of strikes and labor unrest, law (especially whether to contest disputes through trial or settle beforehand), and many forms of “market failure.” Similarly, the theory applies equally to both internal conflicts, whether civil wars or ethnic violence, and interstate wars. Although the particular circumstances differ, of course, the general causes of violence are understood in very similar terms.

The basic idea is quite simple and is illustrated in Figure 1. Two actors, A and B, have well-defined preferences over the division of an issue, say a piece of territory that lies between them or a set of rules (i.e., property rights) that will generate income (for simplicity, a one-time event). A prefers to control all the territory or enact that set of rules that gives it all the income; the same for B. Arrayed on a single dimension and valued (without loss of generality) between zero and one, A’s ideal point is to the far right at one; B’s ideal point is to the far left at zero.¹⁸ The division of the issue is determined by the (actual or expected) outcome of a violent contest (q). If the actors were to fight to alter the division, they would incur costs a and b , respectively. Their net benefits to fighting are, for A, $q - a$; and for B, $q + b$.¹⁹ Since fighting is costly, this opens up a bargaining space (between $q - a$ and $q + b$) in which both parties would prefer any division of the issue to actually fighting. Even if one side, say A, becomes more powerful and could shift the division to p (representing the expected outcome of a war under a new distribution of capabilities), a bargaining space would still exist between, now, $p - a$ and $p + b$. Thus, even though one side becomes more powerful and the old status quo (q) is no longer satisfactory, both parties still have an incentive to negotiate rather than fight.

As James Fearon (1995) succinctly showed, bargaining may fail and war may occur in this framework only if either or both of two conditions holds. First, bargaining failures can arise when the parties have private information with incentives to misrepresent. Private information is knowledge an actor possesses about its preferences (incomplete information) or prior moves (imperfect information) that is not available to the other. For bargaining failures and war to occur, however, an actor must also have some incentive not to reveal its private information, as doing so would otherwise allow a mutually preferred bargain to be reached and the costs of war to be avoided. War plans are especially prone to misrepresentation and, thus, bargaining failures. Since the utility of war plans is greatly reduced once known, as the opponent can then devise a more effective counterresponse, actors have little incentive to truthfully reveal their information, thereby making successful negotiations less likely.

Private (imperfect) information with incentives to misrepresent may have contributed to the 1991 Persian Gulf War between Iraq and the U.S.-led coalition. Iraq anticipated a coalition invasion through Kuwait and counted on a longer, bloodier battle that would have raised the costs of war to the United States. Expecting the coalition to bear a higher cost, Iraq held out for a bargain more favorable to

FIGURE 1
THE COSTS OF WAR AND EFFICIENT BARGAINING



SOURCE: Adapted from Fearon (1995).

itself.²⁰ Coalition forces, in turn, planned the now-famous “left hook” in which they deployed farther west along the border of Saudi Arabia and Iraq and drove rapidly north and then east to attack the entrenched and unsuspecting Iraqi forces. Expecting a low-cost war, the United States refused any bargain with Iraq short of complete capitulation and retreat from Kuwait. Had the United States revealed how it intended to minimize its costs of war before the outbreak of hostilities, in an effort to convince Iraq to withdraw, the value of this plan would have been negated. In this case, the two sides disagreed fundamentally about the expected costs of the war *ex post*, preventing them from reaching a satisfactory bargain *ex ante*.

A similar situation arose in the most recent Iraq War in 2003. As is now apparent, the Iraqis formulated a plan in which the army would not fight against American forces directly but would reorganize into smaller cells to carry on a guerrilla-based war of attrition. Knowing that they could not defeat the Americans directly, they planned to fight them indirectly under the guise of an insurgency. But just as the United States could not reveal its left-hook strategy in 1991, and thereby align expectations of both sides on the likely outcome of the war and an appropriate bargain, the Baathist regime in Iraq could not announce its plans for an insurgency in advance either, leading the United States to believe that the conquest of Iraq would be relatively easy and cheap and that it could be intransigent in prewar negotiations.

Second, wars also arise when the parties are unable to commit credibly to respect the bargain they may reach. A bargain is credible only when it is in the interests of the parties to fulfill its terms when called upon to do so. Problems of credible commitment often follow from the informational imperfections just discussed. When one side is unsure of the other’s preferences (its “type”), it may not put great faith in its opponent’s promises of future behavior. During the 1990s, for instance, the United States became sufficiently frustrated with Iraq’s apparent failure to disarm as required under various United Nations resolutions passed after the 1991 war that it was unwilling to believe any statements from Baghdad that it had dismantled its weapons of mass destruction or any promises that it would not rebuild these weapons in the future. As a result, the administration of President

George W. Bush became convinced that it had no choice but to remove the regime of Saddam Hussein.

Even when both sides possess complete information about each other, problems of credible commitment may also arise when relative capabilities shift exogenously over time or there are random shocks that affect capabilities. If one party is expected to grow stronger in the future, any self-enforcing bargain the opponents might reach today will become incredible tomorrow; the actor that is growing stronger will not be able to convince the other that it will abide by the agreement possible today and not demand more later when it can. Uneven rates of growth, as a result, are especially destabilizing and may have contributed to the outbreak of World War I.²¹

This bargaining theory of war has generated an active and fruitful research program. Much recent work has focused on the problem of private information, with the implication, described by Erik Gartzke (1999), that it is precisely the unobservable traits of the actors that lead to violence and, in turn, make war so difficult to predict. The major study using this approach, Robert Powell's *In the Shadow of Power* (1999), examined exogenous changes in the distribution of capabilities and, in turn, the probability of war under different configurations of power. Problems of credible commitment have been addressed more fully in the literature on war termination (Fortna 2004; Goemans 2000; Walter 2002). Even more recent work is addressing the anomaly of why, once they start, wars are not ended quickly, with the idea that conflict is a process in which information is revealed, prior beliefs are updated, war aims are altered, and so on until a viable bargain is eventually reached.²²

The theory has also proven remarkably useful in understanding war, as the brief discussions of the two Persian Gulf Wars above suggest. Most visibly, it now provides the foundation for several important but still-competing explanations of the democratic peace (see, among others, Bueno de Mesquita et al. 1999; Schultz 2001). It has also been usefully applied to the study of ethnic conflict (De Figueiredo and Weingast 1999; Fearon 1998; Fearon and Laitin 1996; Lake and Rothchild 1998). It directs our attention away from ancient hatreds, animosity, and competing claims to territory to the proximate causes that turn domestic disagreements into violence (Fearon and Laitin 2003). It also suggests clear mechanisms for enhancing peaceful bargaining through greater transparency, confidence-building measures, mediation, and third-party guarantees (Lake and Rothchild 1998; Walter 2002).

The bargaining theory of war has revolutionized International Relations, providing a new and theoretically sound foundation for understanding the causes of large-scale conflict. As the theory is extended, revised, and refined in the years ahead, our understanding of war—both within and between states—will be deepened and enriched, and new directions in policy will be highlighted. Even though countries will always have incentives not to reveal their private information and, therefore, bargains will always be hard to reach *ex ante*, the theory cautions all parties to disputes to be wary of exaggeration and hubris. Every shrewd plan by one party most likely faces an equally clever and possibly devious plan by the other

party. By failing to anticipate that opponents will counter one's own moves as fully as possible, parties to disputes may well reject bargains *ex ante* that they wish, *ex post*, they had accepted. More positively, the problem of credible commitment challenges theorists and policy makers to devise new institutions to enhance transparency, "lock in" agreements so that it is in the interests of the parties to live up to their promises, and guarantee the peace. Tremendous strides have been taken since the end of the cold war in building new institutions through the United Nations, including new roles for militaries not only as peacekeepers but as peace enforcers as well. Theory can help us understand better such innovations and provide further insights into how such institutions can be improved.

B. Open economy politics

The study of international economic policy has also changed importantly in recent years. As with the rationalist approach to war, by the 1990s there emerged within the parallel subfield of international political economy an influential approach that combines strong theory and empirical research. This approach, known as open economy politics (OEP), attempts to analyze economic policy making with explicit attention to the international context within which it takes place.²³ This had always been a weakness of scholarship on trade policy, much of which behaved as if trade were just another domestic economic policy. OEP, instead, attempts to incorporate both the domestic and international aspects of international economic policy making and to do so in a rigorous manner.²⁴

Scholars in the OEP tradition begin with firms, sectors, or factors of production as the units of analysis, then derive their interests over economic policy from each unit's position within the international economy. They also attempt to incorporate the impact of domestic political institutions, conceiving of institutions as mechanisms that condition the interaction of competing societal interests; and they introduce interstate bargaining at the international level. The process involves many complex and interrelated steps, and scholars typically focus on one step—for instance, how institutions aggregate societal interests. Nonetheless, the broadly shared assumptions allow the components to be connected together into a more complete whole.

The fundamental building blocks of OEP are interests, how individuals or groups are affected by particular policies. Typically, while theories start with individuals, the politically relevant actors are firms, sectors, or factors of production (classes). Most scholars in OEP deduce the interests of relevant groups from existing economic theory.²⁵ This provides the approach with a very solid theoretical foundation and is an important source of its scholarly appeal.

Preferences with respect to different economic policies are defined by how groups are located relative to others in the international economy.²⁶ This is the first component part of the approach that relaxes traditional distinctions between international and domestic economics and politics. Firms vary by whether they are in the tradable or nontradable sectors, produce import-competing or export-competing goods, use imported components, and so on. By knowing a firm's production pro-

file (Gourevitch 1986), OEP predicts how it will be affected, for instance, by policies to increase international openness. Sectors vary by similar characteristics. Factors of production, in turn, vary by their scarcity relative to the world economy (Rogowski 1989; Stolper and Samuelson 1941). These methods have been used, as discussed above, to deduce interests in trade policy and, more recently, to analyze international financial and exchange rate policies (Frieden 1988, 1991, 1997; O'Mahony 2003) as well as foreign direct investment (Pinto 2004).

Interests are mediated and transformed into policy through political institutions. These institutions aggregate and transmit societal interests, with varying degrees of bias, and condition bargaining among groups. In most domestic political systems, established rules and procedures generally reflect group strength over the long term. But institutions can develop independent effects that offset numbers or force. Institutions define what political power means in a particular society, whether the competition over policy will be conducted via votes, via contributions and bribes, or via ideas and argument. In short, institutions determine the rules of the political game.

Political scientists and, increasingly, economists are studying in detail how institutions aggregate interests. The findings are preliminary and complex, but the more influential in defining research frontiers for OEP include the following:

Large constituencies—at the extreme, a single electoral district for the entire country—incline policy toward the general, while small constituencies bias policy toward the interests of organized and concentrated groups (Rogowski 1987).

The more actors there are within a political system with the authority to block the enactment of policy—known as veto points—the more likely existing policies are to be maintained (Tsebelis 2002). This could, for example, reduce the credibility of any promise to adopt political or economic reforms (Cowhey 1993), reduce the ability of a government to respond effectively to external shocks (MacIntyre 2001), or enhance the government's ability to make credible promises to sustain existing policies.

Proportional representation systems tend to produce policy stability and inflexibility, implying less credible commitments to reform and less ability to respond effectively to external shocks, whereas majoritarian electoral systems tend toward policy flexibility and instability, with the opposite effects on credibility and effective response (Rogowski 1999).

There are other theoretical and empirical interpretations of the impact of electoral and other political institutions, of which these examples give only a flavor.²⁷ The more general point is that OEP recognizes the need to study how interests are refracted through political institutions that affect policy choices.

The third and final step in the OEP approach is to study how national states, given their goals as determined by interests and institutions, interact to determine the joint outcome of their actions. Sometimes the focus is on international bargaining, especially when the policies of one state have important effects on others and there are opportunities for national states to jointly improve their welfare by cooperating. In other instances, the focus can be on how international institutions, like their domestic counterparts, structure bargaining and affect outcomes. The scholarly literature on international bargaining and international institutions is enormous; the principal point to be made here is that in this final step, too, OEP crosses

lines between domestic and international politics and economics.²⁸ Domestic interests defined in terms of their position in the global economy affect national policy preferences, which then determine the ways in which national states interact in the international arena.

Recent analyses of the politics of international economics in the OEP tradition have signaled a strong scholarly interest in exploring the mutual interaction of international and domestic political economies. While there is no canonical statement of how to think about this extremely complex relationship, scholars are working toward analyses that incorporate both “levels of analysis” without imposing artificial barriers between them.

The OEP research program has, like that on the bargaining theory of war, been extremely fruitful over the past fifteen years. Its theoretical foundations are firm and have gotten firmer. Scholars have used the framework to undertake empirical investigations of a wide variety of international economic policies in a wide variety of settings. The approach and its findings are now standard fare among scholars, and we believe that it will continue to produce important results over the coming decade. As it does so, its relevance to observers and policy makers will increase.

Conclusion

When scholars combine carefully specified theory with systematic empirical testing, they can provide important explanations of world politics. As the reviews of international conflict studies and international political economy indicate, a science of international relations is emerging that shares a set of core assumptions, agrees on the puzzles that good theories should be able to explain, and—as empirical work presses forward—increasingly concurs on the anomalies that cannot yet be explained. We believe that the study of international relations is in the early stages of this scientific revolution but that the revolution is nonetheless under way.

The emerging science of international relations has a long way to go before it can be of direct use to policy makers. International Relations is not yet ready to undertake the political equivalent of physics and engineering’s feat of successfully sending humans to the moon, or even of economics’ improved management of the economy. But like these other disciplines, International Relations will eventually be most useful to policy makers by producing scientific discoveries that withstand rigorous theoretical and empirical scrutiny. The more we know about international relations—and the more confident we are in that knowledge—the more we will have to tell those whose job it is to make foreign policy. Relevance without knowledge is, at best, informed opinion and, at worst, the scholarly equivalent of malpractice. The route to relevance may be arduous and long, but the discipline is on the right road.

Yet as any political scientist knows, theory and evidence will never trump politics. Partisans will always exert whatever influence they can bring to bear to shift policy in their preferred directions. A more scientific understanding of international relations will not negate the push and pull of political struggles within and

between countries and may, like the “democratic peace” finding we discussed above, be invoked by partisans only when it serves their purposes. Nonetheless, a scientific approach will yield a better understanding of politics and, eventually, may give actors on all sides of issues more effective tools by which to pursue their ends. The side that understands politics better will be at a distinct advantage.

Notes

1. We adopt the convention here of capitalizing the study of International Relations but, for clarity, lowercasing the phenomenon of international relations.
2. On the early history and development of the discipline, see Schmidt (1998) and Kahler (1997).
3. The fit of International Relations into political science has always been awkward. Military strategy blends into many fields. The move into political science severed international relations from international economics, a dismemberment that was only partly corrected with the emergence of international political economy in the 1970s. Many universities have both political science and international relations or international studies majors, and most master’s programs in international relations are distinct units. Although dominated by political scientists, the International Studies Association defines itself as an interdisciplinary organization.
4. For an overview of this movement, with a particularly “Chicago” cast (one of the hotbeds), see <http://magazine.uchicago.edu/0306/features/index.shtml>.
5. One of the most cited calls for policy-relevant International Relations is George (1993).
6. For histories of deterrence theory, see Trachtenberg (1991) and Kaplan (1983).
7. Among the principal architects, Bernard Brody and William Kaufman were trained in political science. Thomas Schelling and Daniel Ellsberg were economists. The rest had backgrounds in physics (Herman Kahn), mathematics (Albert Wohlstetter), or other technical fields.
8. See Steinbruner (1974), Jervis (1984, 1989), and Powell (1990).
9. This is sometimes referred to as the “rational deterrence debate”; see the symposium in the January 1989 issue of *World Politics* and especially the article by Achen and Snidal (1989).
10. See among others, Lake (1992), Fearon (1994), Gartzke (1998, 2000), Bueno de Mesquita et al. (1999), Leeds (1999), and Schultz (2001).
11. Text available at <http://www.clintonfoundation.org/legacy/012594-speech-by-president-sotu-address.htm> (accessed April 27, 2005).
12. President George W. Bush, in transcript of press conference, “President and Prime Minister Blair Discussed Iraq, Middle East,” November 12, 2004, available at <http://www.whitehouse.gov/news/releases/2004/11/20041112-5.html> (accessed April 27, 2005).
13. Schattschneider (1935) is the classic statement of most of these views.
14. Summaries and extensions of this early literature can be found in Baldwin (1985), Lavergne (1983), and Lake (1988). Important historical extensions to the politics of trade liberalization are in Schonhardt-Bailey (1991a, 1991b). Applications are found in Keohane and Milner (1996).
15. Gawande and Krishna (2003) is an excellent survey.
16. “Cold Steel,” *The Economist*, November 13, 2003.
17. Early works here include Wittman (1979), Bueno de Mesquita (1981), and Bueno de Mesquita and Lalman (1992). The approach is synthesized and elaborated in Fearon (1995). Fearon also identified issue indivisibility as a third source of bargaining failure. Powell (forthcoming) demonstrated clearly that such indivisibilities are really just another form of the commitment problem. Following Powell, we limit our discussion to problems of private information and commitment.
18. A single dimension is merely an expository simplification. The same framework carries over to an n -dimensional issue space. In this case, the single line in Figure 1 is equivalent to the contract curve created by the tangencies of the indifference curves of the two parties. This would have the effect of enlarging the number of Pareto-preferred points (to include the entire lens created by the relevant indifference curves) but does not contravene the basic point that, as long as war is costly, some mutually preferred bargain always exists to war.

19. Both sides incur costs in fighting. Adding b to q is required by the assumption that the issue ranges from zero to one. It does not imply that B somehow benefits from fighting.

20. Iraq's motivations and calculations in 1990 to 1991 remain somewhat opaque. With the defeat of the Bathist regime in 2003, new information may become available. For a detailed study of the war based on then publicly available information, see Freedman and Karsh (1993).

21. Traditional explanations for World War I emphasize Germany's growing economic power at the center of Europe (see Calleo 1978; Choucri and North 1975). Copeland (2000) argued that Russia's growing might was the destabilizing force.

22. See Wagner (2000), Filson and Werner (2002), Reiter (2003), Slantchev (2003a, 2003b). For a review of the "state-of-the-art," see Powell (2002).

23. The term comes from Bates (1997). This approach has also been referred to as "the second-image reversed" (Gourevitch 1978).

24. Open economy politics (OEP) is a subset of strategic choice theory, discussed in Lake and Powell (1999).

25. Alternatives attempt to derive groups and interests from institutional structure (Verdier 1994) or see interests as socially constructed (Abdelal 2001; Simmons and Elkins 2003).

26. On studying preferences more generally, see Frieden (1999).

27. Persson and Tabellini (2000) is an influential general statement of the relationship between institutions and policy outcomes. For representative examples as applied to trade policy specifically, see Bailey, Goldstein, and Weingast (1997); Gilligan (1997); Hiscox (1999); and McGillivray (1997). The papers in McGillivray et al. (2001) are a particularly interesting application of the approach to important historical experiences; the concluding chapter summarizes some theoretical and methodological implications. Milner and Kubota (2005) addressed the important question of the relationship between democratization and trade liberalization.

28. For examples of the burgeoning literature on international institutions in the trade arena, see Bagwell and Staiger (2001), Maggi (1999), and Rosendorff and Milner (2001).

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