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The Relationship Between Theory and Policy in International Relations

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■ **Abstract** Policy makers pay relatively little attention to the vast theoretical literature in IR, and many scholars seem uninterested in doing policy-relevant work. These tendencies are unfortunate because theory is an essential tool of statecraft. Many policy debates ultimately rest on competing theoretical visions, and relying on a false or flawed theory can lead to major foreign policy disasters. Theory remains essential for diagnosing events, explaining their causes, prescribing responses, and evaluating the impact of different policies. Unfortunately, the norms and incentives that currently dominate academia discourage many scholars from doing useful theoretical work in IR. The gap between theory and policy can be narrowed only if the academic community begins to place greater value on policy-relevant theoretical work.

INTRODUCTION

If the scholarly study of international relations—and especially work on IR theory—were of great value to policy makers, then those charged with the conduct of foreign policy would be in a better position today than ever before. More scholars are studying the subject, more theories are being proposed and tested, and outlets for scholarly work continue to multiply.[1]

The need for powerful theories that could help policy makers design effective solutions would seem to be apparent as well. The unexpected emergence of a unipolar world, the rapid expansion of global trade and finance, the challenges posed by failed states and global terrorism, the evolving human rights agenda, the spread of democracy, concerns about the global environment, the growing prominence of nongovernmental organizations, etc., present policy makers with

[1]One recent study reports that "there are at least twenty-two English-language journals devoted exclusively or largely to international relations, aside from the general politics and policy journals that also publish IR articles" (Lepgold & Nincic 2001, p. 15). IR scholars can also disseminate their work through weblogs, working papers, and outlets such as the Columbia International Affairs Online (CIAO) service.

problems that cry out for new ideas. These phenomena—and many others—have all been objects of sustained scholarly inquiry, and one might expect policy makers to consume the results with eagerness and appreciation.

Yet despite the need for well-informed advice about contemporary international problems, and the energy and activity being devoted to studying these questions, there has long been dissatisfaction with the contributions of IR theorists (Morgenthau 1958, Tanter & Ullman 1972). According to former diplomat David Newsom, "much of today's scholarship [on international issues] is either irrelevant or inaccessible to policymakers. . .much remains locked within the circle of esoteric scholarly discussion" (Newsom 1995–1996, p. 66). Another observer declares that "the higher learning about international relations does not loom large on the intellectual landscape. Its practitioners are not only rightly ignored by practicing foreign policy officials; they are usually held in disdain by their fellow academics as well" (Kurth 1998, p. 29). The veteran U.S. statesman Paul Nitze described theory and practice as "harmonic aspects of one whole," but he believed that "most of what has been written and taught under the heading of 'political science' by Americans since World War II. . .has also been of limited value, if not counterproductive as a guide to the conduct of actual policy" (Nitze 1993, p. 15). Similarly, George (2000) reports that policy makers' eyes "would glaze as soon as I used the word *theory.*" Nor is the problem unique to the United States, as indicated by the Chief Inspector of the British diplomatic service's comment that he was "not sure what the academic discipline of IR—if indeed there be such a thing as an academic discipline of IR—has to contribute to the practical day-to-day work of making and managing foreign policy" (Wallace 1994).

A low regard for theory is also reflected in the organizations responsible for conducting foreign policy. Although academics do work in policy-making circles in many countries, a sophisticated knowledge of IR theory is hardly a prerequisite for employment. In the United States, for example, there is no foreign policy counterpart to the President's Council of Economic Advisors (which is staffed by Ph.D. economists), and being an accomplished IR scholar is neither necessary nor sufficient for appointment to the National Security Council or other similar bodies.[2] Instead, senior policy makers are more likely to be selected for their intelligence, loyalty, and/or intimate knowledge of a particular region or policy area. Nor is there much evidence that policy makers pay systematic attention to academic writings on international affairs.

Dissatisfaction with the limited influence of IR has inspired a small but growing literature that seeks to reconnect the worlds of theory and policy (George et al. 1971; George & Smoke 1974; Feaver 1999; Hill & Beshoff 1994; Kruzel 1994; Zelikow 1994; Lepgold 1998; Jentleson 2000, 2002; Lupia 2000; Nincic & Lepgold 2000;

[2]Several academics have served as U.S. National Security Advisor (e.g., Henry Kissinger, Zbigniew Brzezinski, Anthony Lake, Condoleezza Rice), but so have individuals with little or no formal training in IR (e.g., William Clark, Colin Powell, Sandy Berger, Robert McFarlane, and John Poindexter).

Lepgold & Nincic 2001; Siverson 2001). Taken as a whole, these works emphasize several key themes.

First, the literature sees a wide gap between academic theories of international relations and the practical conduct of foreign policy. Most works in this genre deplore this situation and offer various remedies for correcting it, although a few authors warn that greater emphasis on "policy relevance" might be detrimental (Hill & Beshoff 1994, Stein 2000).

Second, these works attribute the gap in part to the complexity of the policy maker's task and the limitations of existing social science theories, but also to the incentive structures and professional ethos of the academic world. In other words, IR theory is less relevant for policy makers because scholars have little incentive to develop ideas that might be useful.

Third, the literature tends to adopt a trickle-down model linking theory and policy. General or basic IR theory is seen as too abstract to influence policy directly, although it can provide overarching conceptual frameworks and thus influence scholars analyzing specific regional developments or applied "issue-oriented puzzles" (Lepgold 2000, Wilson 2000). These latter works will inform policy analyses of specific problems, thereby helping to shape the debate on specific actions and decisions. It follows that the current gap might be narrowed by strengthening the transmission belt linking these different activities, so that academic ideas reach the policy maker's desk more readily.

The present essay explores these themes in greater detail. Can theoretical IR work help policy makers identify and achieve specific foreign policy goals? What are the obstacles that limit its contribution? Given these obstacles, what should be done?

WHAT CAN THEORY CONTRIBUTE TO THE CONDUCT OF FOREIGN POLICY?

What Types of Knowledge Do Policy Makers Need?

Policy decisions can be influenced by several types of knowledge. First, policy makers invariably rely on purely factual knowledge (e.g., how large are the opponent's forces? What is the current balance of payments?). Second, decision makers sometimes employ "rules of thumb": simple decision rules acquired through experience rather than via systematic study (Mearsheimer 1989).[3] A third type of knowledge consists of typologies, which classify phenomena based on sets of specific traits. Policy makers can also rely on empirical laws. An empirical law is an observed correspondence between two or more phenomena that systematic inquiry has shown to be reliable. Such laws (e.g., "democracies do not fight each other"

[3]For example, someone commuting to work by car might develop a "rule of thumb" identifying which route(s) took the least time at different times of day, based on their own experience but not on a systematic study of traffic patterns.

or "human beings are more risk averse with respect to losses than to gains") can be useful guides even if we do not know why they occur, or if our explanations for them are incorrect.

Finally, policy makers can also use theories. A theory is a causal explanation—it identifies recurring relations between two or more phenomena and explains why that relationship obtains. By providing us with a picture of the central forces that determine real-world behavior, theories invariably simplify reality in order to render it comprehensible.

At the most general level, theoretical IR work consists of "efforts by social scientists. . .to account for interstate and trans-state processes, issues, and outcomes in general causal terms" (Lepgold & Nincic 2001, p. 5; Viotti & Kauppi 1993). IR theories offer explanations for the level of security competition between states (including both the likelihood of war among particular states and the war-proneness of specific countries); the level and forms of international cooperation (e.g., alliances, regimes, openness to trade and investment); the spread of ideas, norms, and institutions; and the transformation of particular international systems, among other topics.

In constructing these theories, IR scholars employ an equally diverse set of explanatory variables. Some of these theories operate at the level of the international system, using variables such as the distribution of power among states (Waltz 1979, Copeland 2000, Mearsheimer 2001), the volume of trade, financial flows, and interstate communications (Deutsch 1969, Ruggie 1983, Rosecrance 1986); or the degree of institutionalization among states (Keohane 1984, Keohane & Martin 2003). Other theories emphasize different national characteristics, such as regime type (Andreski 1980, Doyle 1986, Fearon 1994, Russett 1995), bureaucratic and organizational politics (Allison & Halperin 1972, Halperin 1972), or domestic cohesion (Levy 1989); or the content of particular ideas or doctrines (Van Evera 1984, Hall 1989, Goldstein & Keohane 1993, Snyder 1993). Yet another family of theories operates at the individual level, focusing on individual or group psychology, gender differences, and other human traits (De Rivera 1968, Jervis 1976, Mercer 1996, Byman & Pollock 2001, Goldgeier & Tetlock 2001, Tickner 2001, Goldstein 2003), while a fourth body of theory focuses on collective ideas, identities, and social discourse (e.g., Finnemore 1996, Ruggie 1998, Wendt 1999). To develop these ideas, IR theorists employ the full range of social science methods: comparative case studies, formal theory, large-*N* statistical analysis, and hermeneutical or interpretivist approaches.

The result is a bewildering array of competing arguments (Viotti & Kauppi 1993, Dougherty & Pfaltzgraff 1997, Walt 1997a, Waever 1998, Baylis & Smith 2001, Carlsnaes et al. 2002). With so many theories from which to choose, how do we know a good one when we see one?

What is a Good Theory?

First and most obviously, a good theory should be logically consistent and empirically valid, because a logical explanation that is consistent with the available evidence is more likely to provide an accurate guide to the causal connections that shape events.

Second, a good theory is complete; it does not leave us wondering about the causal relationships at work (Van Evera 1997). For example, a theory stating that "national leaders go to war when the expected utility of doing so outweighs the expected utility of all alternative choices" (Bueno de Mesquita & Lalman 1992) may be logically impeccable, but it does not tell us when leaders will reach this judgment. Similarly, a theory is unsatisfying when it identifies an important causal factor but not the factor(s) most responsible for determining outcomes. To say that "human nature causes war," or even that "oxygen causes war," is true in the sense that war as we know it cannot occur in the absence of these elements. But such information does not help us understand what we want to know, namely, *when* is war more or less likely? Completeness also implies that the theory has no "debilitating gaps," such as an omitted variable that either makes its predictions unacceptably imprecise or leads to biased inferences about other factors (Nincic & Lepgold 2000, p. 28).

A third desideratum is explanatory power. A theory's explanatory power is its ability to account for phenomena that would otherwise seem mystifying. Theories are especially valuable when they illuminate a diverse array of behavior that previously seemed unrelated and perplexing, and they are most useful when they make apparently odd or surprising events seem comprehensible (Rapaport 1972). In physics, it seems contrary to common sense to think that light would be bent by gravity. Yet Einstein's theory of relativity explains why this is so. In economics, it might seem counterintuitive to think that nations would be richer if they abolished barriers to trade and did not try to hoard specie (as mercantilist doctrines prescribed). The Smith/Ricardo theory of free trade tells us why, but it took several centuries before the argument was widely accepted (Irwin 1996). In international politics, it seems odd to believe that a country would be safer if it were unable to threaten its opponent's nuclear forces, but deterrence theory explains why mutual vulnerability may be preferable to either side having a large capacity to threaten the other side's forces (Wohlstetter 1957, Schelling 1960, Glaser 1990, Jervis 1990). This is what we mean by a powerful theory: Once we understand it, previously unconnected or baffling phenemona make sense.

Fourth, at the risk of stating the obvious, we prefer theories that explain an important phenomenon (i.e., something that is likely to affect the fates of many people). Individual scholars may disagree about the relative importance of different issues, but a theory that deals with a problem of some magnitude is likely to garner greater attention and/or respect than a theory that successfully addresses a puzzle of little intrinsic interest. Thus, a compelling yet flawed explanation for great power war or genocide is likely to command a larger place in the field than an impeccable theory that explains the musical characteristics of national anthems.

Fifth, a theory is more useful when it is prescriptively rich, i.e., when it yields useful recommendations (Van Evera 1997). For this reason, George advises scholars to "include in their research designs variables over which policymakers have some leverage" (George 2000, p. xiv; also Glaser & Strauss 1967, Stein 2000). Yet a theory that does not include manipulable variables may still be useful to policy makers. For example, a theory that explained why a given policy objective was impossible might be very useful if it convinced a policy maker not to pursue such an elusive goal. Similarly, a theory that accurately forecast the risk of war might provide a useful warning to policy makers even if the variables in the theory were not subject to manipulation.

Finally, theories are more valuable when they are stated clearly. *Ceteris paribus*, a theory that is hard to understand is less useful simply because it takes more time for potential users to master it. Although academics often like to be obscure (because incomprehensibility can both make scholarship seem more profound and make it harder to tell when a particular argument is wrong), opacity impedes scientific progress and is not a virtue in theoretical work. An obscure and impenetrable theory is also less likely to influence busy policy makers.

How Theory Can Aid Policy (in Theory)

Although many policy makers dismiss academic theorizing and many academics criticize the actions of government officials, theory and policy are inextricably linked. Each day, policy makers must try to figure out which events merit attention and which items or issues can be ignored, and they must select objectives and choose policy instruments that will achieve them. Whether correct or not, they do this on the basis of some sort of theory.

Furthermore, policy debates in both domestic and foreign affairs often hinge on competing theoretical claims, and each participant believes his or her preferred policy option will produce the desired result. For example, competing prescriptions for halting the ethnic conflicts in Bosnia and Kosovo rested in part on different theories about the underlying causes of these wars. Those who favored intervening to establish a multiethnic democracy in Bosnia (and Kosovo) tended to blame the fighting on the machinations of autocratic leaders such as Slobodan Milosevic, whereas those who favored ethnic partition blamed the conflict on a security dilemma created by intermingled populations (cf. Kaufmann 1996, Stedman 1997, Sambanis 2000). More recently, the debate over war against Iraq hinged in part on competing factual claims (did Iraq have weapons of mass destruction or not?) but also on competing forecasts about the long-term effects of the war. Advocates believed war would lead to a rapid victory, encourage neighboring regimes to "bandwagon" with the United States, hasten the spread of democracy in the region, and ultimately undermine support for Islamic terrorism. Their opponents argued that the war would have exactly the opposite effects (Sifry & Cerf 2003), and these disagreements arose in part because of fundamentally different views about the basic dynamics of interstate relations.

History also shows that bad theories can lead directly to foreign policy disasters. Prior to World War I, for example, Admiral Von Tirpitz's infamous "risk theory" argued that German acquisition of a large battle fleet would threaten British naval supremacy and deter Great Britain from opposing German dominance of the continent; in fact, the building of the fleet merely accelerated Britain's alignment with

Germany's continental opponents (Kennedy 1983). During the Cold War, Soviet policy in the Third World was justified by Marxist claims that the developing world was evolving in a socialist direction, and that this evolution would naturally incline these states to ally with the USSR. This theory of cooperation was flawed on both counts, which helps explain why Soviet efforts to build influence in the developing world were costly and disappointing (Rubinstein 1990). Similarly, U.S. intervention in Indochina and Central America was justified in part by the so-called domino theory, even though the logic and evidence supporting the theory were dubious at best (Slater 1987, 1993–1994). All of these examples show how bad IR theories can lead policy makers astray.

The converse is also true, however: Sometimes good theory leads to good policy. As discussed above, the Smith/Ricardo theory of free trade has for the most part triumphed over mercantilist thinking and paved the way for the rapid expansion of the world economy after World War II, thereby facilitating an enormous increase in global wealth and welfare. In the same way, the theory of deterrence articulated in the 1940s and 1950s informed many aspects of U.S. military and foreign policy during the Cold War, and it continues to exert a powerful impact today.[4]

The relationship between theory and policy is not a one-way street. Theory informs policy and policy problems inspire theoretical innovation (Jervis, 2004). For example, the development of the bureaucratic politics paradigm and the theory of nuclear deterrence illustrate how new political issues can spark theoretical developments, with implications that extend beyond the specific problems that inspired the theoretical innovation (Trachtenberg 1992). More recently, efforts to analyze the collapse of the Soviet empire (Kuran 1991, Lohmann 1994, Lebow & Risse-Kappen 1995, Evangelista 2002), the dynamics of unipolarity (Wohlforth 1999, Brooks & Wohlforth 2000–2001), or the origins of ethnic conflict (Posen 1993, Fearon & Laitin 1996, Lake & Rothchild 1998, Toft 2004) show IR theorists fashioning new theories in response to new concerns. Theory and policy form a web, even if the web has many gaps and missing strands. Despite these gaps, there are at least four ways that theoretical scholarship can help policy makers: diagnosis, prediction, prescription, and evaluation.

DIAGNOSIS The first contribution that theory can make is diagnosis (Jentleson 2000). Like all of us, policy makers face a bewildering amount of information, much of it ambiguous. When seeking to address either a recurring problem or a specific event, policy makers must figure out what sort of phenomenon they are facing. Is expansionist behavior driven by a revolutionary ideology or individual megalomania, or is it rooted in legitimate security fears? Are trade negotiations in jeopardy because the participants' preferences are incompatible or because they do not trust each other? By expanding the set of possible interpretations, theories provide policy makers with a broader set of diagnostic possibilities.

[4]Of course, not all aspects of U.S. nuclear weapons policy conformed to the prescriptions of classic deterrence theory (Jervis 1984).

Diagnosis does not require a sophisticated theory, however; even simple typologies can help policy makers devise an appropriate response to a problem. In medicine, even if we do not know the exact mechanism that produces a disease, we may be able to treat it once the correct diagnosis is made (George 2000). Similarly, even if we cannot fully explain why certain international events occur, we may still be able to fashion a remedy once we have identified the problem.

Theory also guides our understanding of the past, and historical interpretations often influence what policy makers do later (May 1975, May & Neustadt 1984). Did the Cold War end because the Soviet economy was dying from "natural causes" (i.e., the inherent inefficiency of centrally planned economies), because Soviet elites were persuaded by norms and ideas imported from the West, or because the United States was putting greater pressure on its overmatched adversary? The question is not merely academic; it tends to shape attitudes on how the United States should use its power today. Hardliners tend to attribute the Soviet collapse to U.S. pressure, and they believe similar policies will work against contemporary enemies (e.g., Iraq, Iran, North Korea) and future "peer competitors" (Mann 2004). By contrast, if the Soviet Union collapsed because of its own internal contradictions, or because Western ideas proved contagious, then U.S. policy makers should consider whether future peer competitors might be more readily coopted than contained (cf. Wohlforth 1994–1995, Evangelista 2002).

The recent debate on war with Iraq offers an equally apt example. Analysts who focused primarily on Saddam Hussein's personality and the nature of the Ba'ath regime saw Hussein's past conduct as evidence that he was an irrational serial aggressor who could not be deterred and thus could not be permitted to acquire weapons of mass destruction (Pollock 2002). By contrast, scholars who focused on Iraq's external situation tended to see Hussein as a risk-acceptant but ultimately rational leader who had never used force in the face of a clear deterrent threat and thus could be deterred by superior force in the future (Mearsheimer & Walt 2003). Thus, interpretations about Iraq's past conduct were partly shaped by contrasting theoretical views and had a clear impact on contemporary policy recommendations.

Once a diagnosis is made, theory also guides the search for additional information. As discussed above, policy makers inevitably rely on different forms of knowledge—including purely factual information—but theory helps them decide what sort of information is relevant. To take a simple example, both policy makers and IR theorists know that power is an important concept, although there is no precise formula for measuring the relative power of different actors. We do not judge the power of nations by examining the quality of their opera productions, the average hair length of the citizenry, or the number of colors in the national flag. Why? Because there is no theory linking these measures to global influence. Rather, both policy makers and scholars generally use some combination of population, gross national product, military strength, scientific prowess, etc., because they understand that these features enable states to affect others (Morgenthau 1985, Moul 1989, Wohlforth 1993, Mearsheimer 2001). That is why U.S. and Asian policy makers worry about the implications of China's economic growth but do not express similar concerns about Thailand or Brunei.

PREDICTION IR theories can also help policy makers anticipate events. By identifying the central causal forces at work in a particular era, theories offer a picture of the world and thus can provide policy makers with a better understanding of the broad context in which they are operating. Such knowledge may enable policy makers to prepare more intelligently and in some cases allow them to prevent unwanted developments.

To note an obvious example, different theories of international politics offered contrasting predictions about the end of the Cold War. Liberal theories generally offered optimistic forecasts, suggesting that the collapse of communism and the spread of Western-style institutions and political forms heralded an unusually peaceful era (Fukuyama 1992, Hoffman et al. 1993, Russett 1995, Weart 2000). By contrast, realist theories of IR predicted that the collapse of the Soviet threat would weaken existing alliances (Mearsheimer 1989, Waltz 1994–1995, Walt 1997c), stimulate the formation of anti-U.S. coalitions (Layne 1993, Kupchan 2000), and generally lead to heightened international competition. Other realists foresaw a *Pax Americana* based on U.S. primacy (Wohlforth 1999, Brooks & Wohlforth 2000–2001), whereas scholars from different traditions anticipated either a looming "clash of civilizations" (Huntington 1997) or a "coming anarchy" arising from failed states in the developing world (Kaplan 2001). Some of these works were more explicitly theoretical than others, but each highlighted particular trends and causal relationships in order to sketch a picture of an emerging world.

Theories can also help us anticipate how different regions or states are likely to evolve over time. Knowing a great deal about a particular state's current foreign policy preferences can be useful, for example, but this knowledge may tell us relatively little about how this state will behave if its position in the world were different. For that task, we need a theory that explains how preferences (and behavior) will evolve as conditions change. For example, China's foreign policy behavior is virtually certain to change as its power grows and its role in the world economy increases, but existing realist and liberal theories offer sharply different forecasts about China's future course. Realist theories predict that increased power would make China more assertive, whereas liberal approaches suggest that increased interdependence and/or a transition to democracy is likely to dampen these tendencies significantly (cf. Goldstein 1997–1998, Mearsheimer 2001).

Similarly, there is a growing consensus that economic development is encouraged by competitive markets, the rule of law, education for both sexes, and government transparency. If true, then this body of theory identifies which regions or countries are likely to develop rapidly. In the same way, Hudson & Den Boer's (2002, 2004) work on the impact of "surplus males" may provide an early warning about particular regions or countries.[5] Dynamic theories of the balance of power offer similar warnings about effects of rapid shifts in relative power and thus can be used to identify potentially unstable regions (Gilpin 1981, Friedberg 1993–1994, Copeland 2000). In each of these cases, a theoretical argument carries important implications for future events.

The relationship between theoretical forecasting and real-world policy making is not straightforward, however (Doran 1999). Social science theories are probabilistic, and even a very powerful theory will make some false forecasts. Moreover, the objects of social science theory are sentient beings who may consciously adjust their behavior in ways that confirm or confound the theories on which they have based their decisions. In response to Huntington's forecast of a "clash of civilizations," a policy maker who concluded that the clash was inevitable would be inclined to adopt defensive policies that could easily make such a clash more likely, but someone who felt it was avoidable could take steps to minimize civilizational frictions and thus make Huntington's predictions appear false (Walt 1997b). In other cases, such as the Hudson & Den Boer study of "surplus males," the knowledge that certain countries were prone to conflict could enable policy makers to take preventive steps against anticipated problems.[6] In social science, in short, the observed validity of a theory may be affected by the degree to which it is accepted and acted on by policy makers.

PRESCRIPTION All policy actions rest on at least a crude notion of causality. Policy makers select policies A, B, and C because they believe these measures will produce some desired outcome. Theory thus guides prescription in several ways.

First, theory affects the choice of objectives by helping the policy maker evaluate both desirability and feasibility. For example, the decision to expand NATO was based in part on the belief that it would stabilize the emerging democracies in Eastern Europe and enhance U.S. influence in an important region (cf. Goldgeier 1999, Reiter 2001–2002). Expansion was not an end in itself; it was a means to other goals. Similarly, the decision to establish the World Trade Organization arose from a broad multilateral consensus that a more powerful international trade regime was necessary to lower the remaining barriers to trade and thus foster greater global productivity (Preeg 1998, Lawrence 2002).

Second, careful theoretical work can help policy makers understand what they must do to achieve a particular result. To deter an adversary, for example, deterrence theory tells us we have to credibly threaten something that the potential

[5]Hudson & Den Boer argue that cultural preferences for male offspring produce a demographic bulge of "surplus males" with limited marriage prospects. This group generates high crime rates and internal instability, and their presence may also encourage states to adopt more aggressive foreign policies.

[6]As I have written elsewhere, this feature "is both the purpose and the paradox of social science: by gaining a better grasp of the causal forces that shape social phenomena, we may be able to manipulate them so as to render our own theories invalid" (Walt 1996, p. 351).

adversary values. Similarly, the arcane IR debate over the significance of absolute versus relative gains helped clarify the functions that international institutions must perform in order to work effectively. Instead of focusing on providing transparency and reducing transaction costs (as the original literature on international regimes emphasized), the debate on absolute versus relative gains highlighted the importance of side payments to eliminate gaps in gains and thus remove a potential obstacle to cooperation (Baldwin 1993).

Third, theoretical work (combined with careful empirical testing) can identify the conditions that determine when particular policy instruments are likely to work. As discussed above, these works focus on "issue-oriented puzzles" (Lepgold 1998), or what is sometimes termed "middle-range" theory, and such works tend to produce "contingent" or "conditional" generalizations about the effects of different instruments (George & Smoke 1974, George 1993). It is useful to know that a particular policy instrument tends to produce a particular outcome, but it is equally useful to know what other conditions must be present in order for the instrument to work as intended. For example, the theoretical literature on economic sanctions explains their limitations as a tool of coercion and identifies the conditions under which they are most likely to be employed and most likely to succeed (Martin 1992, Pape 1997, Haass 1998, Drezner 1999). Pape's related work on coercive airpower shows that airpower achieves coercive leverage not by inflicting civilian casualties or by damaging industrial production but by directly targeting the enemy's military strategy. The theory is directly relevant to the design of coercive air campaigns because it identifies why such campaigns should focus on certain targets and not others (Pape 1996, Byman et al. 2002).

Fourth, careful scrutiny of an alleged causal chain between actions and results can help policy makers anticipate how and why their policies might fail. If there is no well-verified theory explaining why a particular policy should work, then policy makers have reason to doubt that their goals will be achieved. Even worse, a well-established body of theory may warn that a recommended policy is very likely to fail. Theory can also alert policy makers to possible unintended or unanticipated consequences, and to the possibility that a promising policy initiative will fail because the necessary background conditions are not present.

For example, current efforts to promote democracy in the Middle East may be appealing from a normative perspective—i.e., because we believe that democracy leads to better human rights conditions—but we do not have well-verified theories explaining how to achieve the desired result. Indeed, what we do know about democracy suggests that promoting it in the Middle East will be difficult, expensive, and uncertain to succeed (Carothers 1999, Ottaway & Carothers 2004). This policy may still be the correct one, but scholars can warn that the United States and its allies are to a large extent "flying blind."

EVALUATION Theory is crucial to the evaluation of policy decisions. Policy makers need to identify benchmarks that will tell them whether a policy is achieving the desired results. Without a least a sketchy theory that identifies the objectives to be sought, the definitions of success and failure, and the purported links between policy actions and desired outcomes, it will be difficult to know if a policy is succeeding or not (Baldwin 2000).

At the most general level, for example, a "grand strategy" based on liberal theory would emphasize the spread of democratic institutions within states or the expansion and strengthening of international institutions between states. Success would be measured by the number of states that adopted durable democratic forms, based on the belief that the spread of democracy would lead to other desirable ends (improved human rights performance, freer trade, decreased risk of international conflict, etc.) By contrast, a grand strategy based on realist theory would devote more attention to measuring the balance of power, and success might be measured by increases in one's own relative power, the successful courtship of powerful new allies, or the disruption of an opponent's internal legitimacy.

Similar principles apply in thinking about the role of external intervention in civil conflicts. If efforts to build democracy in a multiethnic society (such as Bosnia or Iraq) were based on the theory of consociational democracy advanced by Lijphardt (1969), then appropriate performance measures might include a successful census, high levels of voter turnout, the creation of institutions that formally allocated power across ethnic or other lines, etc. However, if the underlying theory of ethnic peace prescribed ethnic partition, then appropriate performance measures would focus on settlement patterns (Kaufmann 1996, Toft 2004). And theories of postwar peace that emphasize the enforcement role of third parties imply that we should try to measure the staying power of outside guarantors (Walter 2002).

EXPLAINING THE GAP: WHY THEORY AND POLICY RARELY MEET

If theory can do all these things for policy makers, and if it is impossible to formulate policy without a least a vague theory that links means and ends, then why doesn't theoretical IR scholarship play a larger role in shaping what policy makers do? Part of the explanation lies in the particular qualities of contemporary IR theory, and the remainder derives from the norms and incentives that govern the academic and policy worlds.

Is IR Theory Too General and Abstract?

A common explanation for the gap between theory and policy is that important works in the field operate at a very high level of generality and abstraction (George 1993, 2000; Jentleson 2000, p. 13). General theories such as structural realism, Marxism, and liberal institutionalism attempt to explain patterns of behavior that persist across space and time, and they use relatively few explanatory variables (e.g., power, polarity, regime type) to account for recurring tendencies. According to Stein (2000), "international relations theory deals with broad sweeping patterns;

while such knowledge may be useful, it does not address the day-to-day largely tactical needs of policymakers" (p. 56).

This criticism does not mean that general theories are of no value, however. General theories provide a common vocabulary with which to describe global issues (such terms as globalization, unipolarity, credibility, preemption, and free-riding) and create a broad picture of the context in which statecraft occurs. Moreover, some general theories do offer strategic prescriptions that policy makers can use to make choices. Abstract models can also help us understand many familiar features of international life, including the role of information asymmetries, commitment problems, and dilemmas of collective action. Thus, even abstract basic theory can help policy makers understand the context in which they are operating and suggest solutions to some of the challenges they face.

Nonetheless, many prominent works of general theory are simply not very relevant for informing policy decisions (and to be fair, they are not intended to be). For example, critics of Waltz's neo-realist theory of international politics have commonly complained that it is essentially a static theory that offers little policy guidance, a position reinforced by Waltz's own insistence that he did not assay a "theory of foreign policy" (Waltz 1979, 1997; George 1993; Kurth 1998). Although other scholars have found considerable policy relevance in Waltz's basic approach (e.g., Elman 1997), it is nonetheless true that *Theory of International Politics* offers only very broad guidelines for the conduct of statecraft. It provides a basic perspective on relations between states and sketches certain broad tendencies (such as the tendency for balances of power to form), but it does not offer specific or detailed policy advice.[7]

Similarly, Wendt's (1999) *Social Theory of International Politics* is an impressive intellectual achievement, and it has important implications for how scholars might study relations between states. But it does not offer even general prescriptions or insights concerning policy. Such works suggest that societies have greater latitude in constructing international reality than materialist conceptions imply, but they rarely offer concrete guidance for how policy makers might create a better world.

So Many Theories, So Little Time

A related problem arises from the limited explanatory power of the available theories. States' actions, and the effects of those actions, are the product of many different factors (relative power, domestic politics, norms and beliefs, individual psychology, etc.), and scholars have therefore produced a host of different theories employing many variables. Unfortunately, we do not have a clear method for combining these partial theories or deciding when to emphasize one over another. Policy makers can perhaps be forgiven for failing to embrace the theoretical scholarship in the field, when scholars of equal distinction offer radically different views.

[7] Waltz did offer specific policy recommendations in other works (e.g., Waltz 1967, 1981).

This problem is exacerbated by the nature of many policy problems. In general, social science theory offers the clearest advice when applied to situations with a well-defined structure: when the actors' preferences can be specified precisely, when the results of different choices are known, and when there are ample data with which to confirm and refine conjectures. For example, game theory and microeconomics are clearly useful—though not infallible—tools for analyzing public policy problems whenever these conditions are present (Stokey & Zeckhauser 1978, O'Neill 1994).[8]

Unfortunately, these conditions are often absent in the realm of foreign policy, where actors' preferences are frequently unknown, where each participant has many strategies available, and where the costs and benefits of different outcomes are uncertain. Nonlinear relationships and other systemic effects abound, and preferences and perceptions are constantly being revised (but not always according to Bayes' rule) (Jervis 1997). Scholars, as well as policy makers, often lack sufficient data for statistical analysis, and the data that are available are rife with endogeneity problems and other sources of bias (Przeworski 1995).

These problems may be less acute when scholars descend from the rarified heights of general theory to the level of specific policy instruments, where it is sometimes possible to devise convincing quasi-experiments that explain policy effects. Not surprisingly, therefore, the literature on the theory-practice gap tends to extol the virtues of middle-range theory. Such theory focuses on situations, strategies, or tools that are of direct concern to policy makers and can employ a more controlled, quasi-experimental assessment of the tools in question. Although these efforts often face a paucity of data (e.g., Pape's study of coercive airpower is based on a universe of only 33 cases, which is relatively large for an IR study), they can provide policy makers with at least a rough estimate of the effects of a given course of action.

These gains are bought with a price, however. Middle-range theory often sacrifices parsimony and generality, and it tends to produce contingent generalizations of the form, "if you do X, then Y will occur, assuming conditions a, b, c, and q all hold, and assuming you do X in just the right way." Indeed, prominent advocates of greater policy relevance see this feature as one of the main virtues of middle-range theory. It can sensitize decision makers to the contextual features that will affect the odds of success, and it emphasizes tailoring policy instruments to particular conditions (George 1993, Lepgold & Nincic 2001). The danger, however, is that the resulting generalizations become so heavily qualified that they offer little guidance beyond the original cases from which they were derived (Achen & Snidal 1989). This problem does not arise in all middle-range theory, but it is a legitimate criticism of much of it.

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[8]Game theory and decision theory have been used to design public auctions of broadcasting licenses, for example, and decision theory has been used to develop more effective antisubmarine warfare strategies. Microeconomic models are commonly used to conduct cost-benefit analysis and to estimate the impact of rent control, health insurance, environmental regulations, insurance plans, and a host of other public policy initiatives.

Efforts to analyze the effects of different policy instruments are also bedeviled by complicated selection effects. No matter how large the universe of cases is, the observable connection between causes (i.e., policy actions) and effects is hard to measure with precision, because policy makers tend to choose the instruments that they deem most likely to work in a given situation. As a result, observed success or failure rates do not necessarily tell us which policies are "best" in any absolute sense. We can try to minimize these problems through appropriate research designs and control variables, but these biases can never be entirely eliminated. A particular policy instrument can fail quite often, for example, and still be the best available choice in certain circumstances.[9] These problems help explain why policy makers tend to view the results of even high-quality academic research with some skepticism.

Different Agendas

A recurring theme in the literature on theory and policy is the fact that scholars and policy makers have different agendas (Eckstein 1967, Rothstein 1972, Moore 1983). Social scientists (including IR theorists) seek to identify and explain recurring social behavior, but policy makers tend to be concerned with the particular problems they are facing today. Policy makers should be curious about general tendencies—if only to know whether their current goal is generally feasible—but knowing what happens most of the time may be less pertinent than knowing what will happen in the particular case at hand. Thus, a scholar might be delighted by a theory predicting that, on average, a 20% increase in X would produce a 25% decrease in Y, but a policy maker will ask whether the problem now occupying his inbox is an outlier or an exception to this general tendency. As a result, notes Stein (2000), "in-depth experiential knowledge dominates general theorizing and statistical generalizations for the formation of policy" (p. 60).

Furthermore, policy makers are often less interested in explaining a general tendency than in figuring out how to overcome it. A theorist might be content to explain why states are strongly inclined to form alliances against potential aggressors, or to demonstrate why economic sanctions rarely work, but a policy maker (e.g., Bismarck on the eve of the Franco-Prussian War) might be interested in how balancing tendencies might be impeded or how a particular sanctions campaign might be given sharper teeth.

A third contrast is the different attitudes that theorists and policy makers have toward time. Scholars want to make their work as accurate as possible, even if this takes longer, but policy makers rarely have the luxury of waiting. As Harvard professor and former State Department official Robert Bowie once put it, "the policymaker, unlike the academic analyst, can rarely wait until all the facts are in. He is very often under strong pressure to do something, to take some action" (quoted in May 1984). A policy maker who sought scholarly input, and was told that the

[9]For example, surgery to repair congenital heart defects in infants often fails, but alternative approaches (e.g., do nothing) are even less promising.

answer would require months of research and analysis, would be understandably reluctant to seek such advice again.

Finally, even a well-constructed and highly relevant theory may not help policy makers with a key aspect of their jobs: implementation. Theory can help diagnose the situation and identify the appropriate policy response, but the actual form of the response requires much more specific knowledge. First, the general decision (to seize foreign assets, declare war, reduce a tariff, issue a threat, etc.) must be fashioned into an action plan that identifies what government personnel will actually do (Zelikow 1994). And once the policy design is complete, the time-consuming work of overcoming bureaucratic resistance, legal constraints, fatigue, and partisan opposition still remains. Contemporary IR theory is largely silent on these problems, but they loom large in the life of any policy maker.

The Professionalization of the Discipline

The modest impact of contemporary IR theory on policy makers is no accident, because the creation of IR theory conforms to the norms and incentives of the academic profession rather than the needs of policy. IR scholarship is often impenetrable to outsiders, largely because it is not intended for their consumption; it is written primarily to appeal to other members of the profession. It is hard to imagine busy policy makers (or even their assistants) sitting down with a copy of *International Organization* or *World Politics* or devoting a weekend to perusing Waltz's *Theory of International Politics*, Wendt's *Social Theory of International Politics*, or Powell's (1999) *In the Shadow of Power*. Even when theorists do have useful ideas to offer, the people who need them will not know they exist and would be unlikely to understand them if they did.

This is not a new phenomenon; both scholars and policy makers have been complaining about it for decades (Rothstein 1972, Tanter & Ullman 1972, Wallace 1994). It is a direct consequence of the professionalization of the academic world and the specific incentives that scholars within the academy have established for themselves. The academic field of IR is a self-regulating enterprise, and success in the profession depends almost entirely on one's reputation among one's peers. There is therefore a large incentive to conform to the norms of the discipline and write primarily for other academics.

Over the past century, the prevailing norms of academic life have increasingly discouraged scholars from doing work that would be directly relevant to policy makers. Membership in the profession is increasingly dominated by university-based academics, and the discipline has tended to valorize highly specialized research (as opposed to teaching or public service) because that is what most members of the field want to do.[10] Younger scholars learn that the greatest

[10]In 1900, there were fewer than 100 full-time teachers of "political science" in the United States; by the late 1970s, the American Political Science Association (APSA) had over 17,000 members. In 1912, only 20% of APSA members were academics; by 1970, that

rewards go to those who can offer a new theory and that a careful policy analysis that offered valuable advice but broke no new theoretical ground would count for relatively little (Jentleson 2000). When success depends on the opinion of one's peers (rather than on the practical value of what one knows), and when those peers are all university-based scholars, there is a clear incentive to strive for novel arguments that will impress other scholars.

In the distant past, writers such as Machiavelli, Locke, Hobbes, Madison, Rousseau, and Marx were engaged in and inspired by the political events of their time. Similarly, the founders of modern political science in the United States consciously sought to use their knowledge to improve the world,[11] and the American Political Science Association was founded in part "to bring political science to a position of authority as regards practical politics." Not so very long ago, it was even common for prominent IR scholars to work in policy-making circles before returning to active (and prominent) academic careers.

This situation is quite different today. Although "in-and-outers" still exist, academics seem less interested in spending time in government even temporarily.[12] Prominent IR theorists rarely try to write books or articles that would be directly relevant to policy problems; policy relevance is simply not a criterion that the academy values. Indeed, there is a clear bias against it. Younger scholars are cautioned not to "waste" their time publishing op-eds, weblogs, or articles in general readership journals, and scholars who write for *Foreign Affairs*, *Foreign Policy*, or even peer-reviewed journals such as *Political Science Quarterly* or *International Security* run the risk of seeming insufficiently serious, even if they also publish in more rarified academic journals. According to Adam Przeworski, "the entire structure of incentives of academia in the United States works against taking big intellectual and political risks. Graduate students and assistant professors learn to package their intellectual ambitions into articles publishable by a few journals and to shy away from anything that might look like a political stance.... We have the tools and we know some things, but we do not speak about politics to people outside academia" (quoted in Munck & Snyder 2004, p. 31). Given these biases—which are even more prevalent in other parts of political science—it is not surprising that academic research rarely has immediate policy impact.

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percentage was over 75%. Since World War II, only one APSA President (Ralph Bunche) has been a nonacademic at the time of election (Ricci 1984, pp. 65–66).

[11]The extreme case, of course, is Woodrow Wilson, the president of APSA who became President of the United States—but the same is true of such men as Charles Merriam, Quincy Wright, and Hans Morgenthau.

[12]During the past five years, about one third (just under 37%) of International Affairs Fellowships at the Council on Foreign Relations were granted to academics on tenure track at universities, even though the original purpose of the fellowship was to provide academic scholars with an opportunity to get direct experience in government. Data from 2000–2001 to 2004–2005 are available at http://www.cfr.org/about/fellowship_iaf.php (accessed 9/2/2004).

Is a Division of Labor the Answer?

Most students of the theory-practice gap believe the chasm can be bridged by a division of labor between scholars and policy makers, because scholarly theorizing will eventually "trickle down" from the ivory tower into the mind-sets, in-boxes, and policy responses of policy makers.[13] In this knowledge-driven model of impact, general theory establishes the key concepts, methods, and principles that guide the analysis of specific empirical puzzles (such as alliance behavior, institutional effects, crisis behavior, and ethnic conflict), and these results are then used by policy analysts examining specific cases or problems (Weiss 1978). The latter works, in turn, provide the basis for advocacy and action in the public arena and in government (Lepgold 1998, Lepgold & Nincic 2001).

This is a comforting view insofar as it places academic theorists at the pinnacle of the status hierarchy, leaves scholars free to do whatever they want, and assumes that their efforts will eventually be of value. There is also much to be said for allowing scholars to pursue ideas that are not tied to specific policy problems, because wide-ranging inquiry sometimes yields unexpected payoffs. But there are also grounds for questioning whether the current division of labor is optimal.

First, the trickle-down model assumes that new ideas emerge from academic "ivory towers" (i.e., as abstract theory), gradually filter down into the work of applied analysts (and especially people working in public policy "think tanks"), and finally reach the perceptions and actions of policy makers (Haass 2002, Sundquist 1978). In practice, however, the process by which ideas come to shape policy is far more idiosyncratic and haphazard (Albaek 1995). An idea may become influential because of a single well-timed article, because its author happened to gain personal access to a key policy maker, or because its inventor(s) entered government service themselves. Alternatively, social science theory may exert its main impact not by addressing policy problems directly, but via the "long-term percolation of. . .concepts, theories and findings into the climate of informed opinion" (Weiss 1977).

Second, the gulf between scholars and policy makers may be getting wider as the links between theoretical research and policy problems grow weaker. As academic theory becomes increasingly specialized and impenetrable, even those scholars who are working on applied problems and other so-called research brokers (Sundquist 1978) may pay less and less attention to it. This is even more likely to be true in the world of policy-oriented think tanks, which are increasingly disconnected from the academic world. As is well known, in the 1950s and 1960s the RAND Corporation made seminal contributions to strategic studies and international security policy, as well as to social science more generally, and many RAND

[13]The classic expression of this view is Keynes' (1936) famous statement that "practical men, who believe themselves to be quite exempt from any intellectual influence, are usually the slaves of some defunct economist. Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribbler of a few years back."

staff members had prominent careers in academe as well. Today, by contrast, a RAND analyst would be unlikely to be a viable candidate for an IR position at a major university, and RAND's research products do not exert much influence on the scholarly world. Similarly, in the 1970s and 1980s, the Foreign Policy Studies group at the Brookings Institution was not very different from an academic IR faculty, and publications by its staff were similar to works produced within prestigious departments.[14] Moreover, the director of the group, John Steinbruner, held a Ph.D. from MIT, had previously taught at Harvard, and was the author of several important theoretical works (Steinbruner 1974, 1976). Today, by contrast, the Foreign Policy Studies staff a Brookings writes relatively few refereed journal articles or scholarly books and concentrates primarily on producing op-eds and contemporary policy analyses (e.g., Daalder & Lindsay 2003, Gordon & Shapiro 2004). Consistent with this focus, the current director of Foreign Policy Studies is a lawyer and former government official with little or no scholarly training. My point is not to disparage the work done at Brookings (or at similar institutions); it is simply to note that the gulf between academic scholars and policy-oriented analysts is widening. One suspects that scholars in think tanks pay less attention to their academic counterparts than they did previously, and vice versa. This tendency is exacerbated by the emergence of "advocacy think tanks" (e.g., the Heritage Foundation, the American Enterprise Institute, the Cato Institute) in which analysis is driven by explicit ideological preferences (Wallace 1994, Weaver & Stares 2001, Abelson 2002). As connections between the ivory tower and more policy-oriented scholars become tenuous, the trickle-down model of scholarly influence seems increasingly questionable.

WHAT IS TO BE DONE?

The literature on the gap between theory and practice addresses most of its recommendations toward reforming the academic world, for two obvious reasons. First, scholars are more likely to read these works. Second, policy makers are unlikely to be swayed by advice to pay greater attention to academic theory. If scholars produce useful knowledge, policy makers will want to know about it. If academic writings are not useful, however, no amount of exhortation will persuade policy makers to read them.

What is needed, therefore, is a conscious effort to alter the prevailing norms of the academic IR discipline. Today's professional incentive structure discourages many scholars—especially younger scholars—from striving for policy relevance, but the norms that establish that structure are not divinely ordained; they are collectively determined by the members of the discipline itself. The scholarly community gets to decide what it values, and there is no reason why policy relevance cannot be

[14] I am thinking of the work of Richard Betts, John Steinbruner, Joshua Epstein, Bruce Blair, Paul B. Stares, Raymond Garthoff, Yahya Sadowski, William Quandt, and others.

elevated in our collective estimation, along with the traditional criteria of creativity, rigor, and empirical validity.

What would this mean in practice? First, academic departments could give greater weight to real-world relevance and impact in hiring and promotion decisions. When evaluating job candidates, or when considering someone for tenure, reviewers and evaluation committees could consider what contribution a scholar's work has made to the solving of real-world problems. Policy relevance would not become the only—or even the most important—criterion, of course, and scholars would still be expected to meet high scholarly standards. But giving real-world relevance greater weight would make it more likely that theory would be directed at real-world problems and presented in a more accessible fashion. "Should it really be the case," Jentleson (2000) correctly asks, "that a book with a major university press and an article or two in [a refereed journal]. . .can almost seal the deal on tenure, but books with even major commercial houses count so much less and articles in journals such as *Foreign Affairs* often count little if at all?. . . The argument is not about padding publication counts with op-eds and other such commentaries, but it is to broaden evaluative criteria to better reflect the type and range of writing of intellectual import" (p. 179). To put it bluntly: Should our discipline really be proud that relatively few people care about what we have to say?

Second, academic departments could facilitate interest in the real world of politics by giving junior faculty greater incentives for participating in it. At present, academic departments rarely encourage younger faculty to take time off to serve in government, and very few departments will stop the tenure clock for someone doing public service. A scholar who is interested in acquiring real-world experience (or in helping to shape policy directly) generally has to wait until after tenure has been granted. By allowing younger faculty to "stop the clock," however, academic departments would have more members who understood real-world issues and knew how to translate theoretical ideas into policy-relevant analyses. One suspects that such individuals would become better teachers as well, because students, unlike many scholars, do care about the real world and have little tolerance for irrelevant abstraction.

Third, academic journals could place greater weight on relevance in evaluating submissions,[15] and departments could accord greater status to journals that did this. Similarly, prize committees could consider policy relevance a criterion for awarding noteworthy books and articles, instead of focusing solely on contributions to narrow disciplinary issues. Finally, creating more outlets for work that translated "higher-end" theory into accessible form (as the *Journal of Economic Perspectives* does in economics) would strengthen the "transmission belt" from theory to policy.

The goal of such reforms is not to make the academic world a homogeneous mass of policy analysts or even worse, a community of co-opted scholars competing to win the attention of government officials. Rather, the purpose is to encourage a

[15]Some peer-reviewed journals do this (e.g., *International Security* and *Security Studies*), but it is hardly a widespread practice.

more heterogeneous community at all levels of academe: some scholars who do highly abstract work and others who do explicit policy analysis, but where both groups are judged according to their broad contribution to our understanding of critical real-world problems.

Is this vision a pipe dream? Perhaps not. Indeed, there are encouraging signs throughout the social sciences. In economics, for example, the awarding of the Nobel Prize to Douglass North, Amartya Sen, James Heckman, Amos Tversky, and Daniel Kahneman suggests a greater desire to valorize ideas and techniques that proved to be of value to public policy issues. In addition, a group of prominent economists has recently announced the creation of a new online journal (*The Economists' Voice)* to provide a more visible outlet for policy analysis informed by rigorous economic reasoning. Within political science, the perestroika movement arose partly in reaction to the formalism and irrelevance of recent scholarship, and the movement deserves partial credit for the creation of the journal *Perspectives on Politics* and the increased intellectual diversity and policy relevance of the *American Political Science Review.* These developments remind us that the criteria of merit used in academic fields are always subject to revision. In other words, IR scholars need not accept things as they are; we collectively determine how our field develops. IR theorists can provide valuable ideas for policy makers without sacrificing our integrity and objectivity, but only if we decide we want to.

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Annual Review of Political Science
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CONTENTS

INDEXES

ERRATA

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