1 The Theory-Practice Gap in International Relations

> It is by action—in my terms, by the practice of politics—that theory . . . can be kept in touch with reality. . . . The two are inseparable; theory and practice being complementary, they constitute harmonic aspects of one whole.[1]
>
> —Paul H. Nitze

> It is natural to assume that, of all the institutions focusing on public policy, the free realm of the universities would have the most to offer in knowledge and insight. Challenges to conventional wisdom and provocative explorations of international issues not possible in the political world should be and are part of the domain of the scholar and teacher. . . . [Yet] much of today's scholarship is either irrelevant or inaccessible to policy makers. . . . much remains locked within the circle of esoteric scholarly discussion.[2]
>
> —David D. Newsom

> . . . the more [scholars] strain for policy relevance, even if only to justify our existence in the eyes of society at large, the more difficult it becomes to maintain intellectual integrity.[3]
>
> —Christopher Hill

The first two observations, both from distinguished former U.S. officials, typify many policymakers' views about contemporary scholarship in international relations: while it *ought* to be useful to practitioners, little of it is. Much, they believe, is useless and arcane. These particular statements are striking because they do not reflect ignorance about the mission and culture of university scholars. The individual quoted in the first passage has written widely on foreign policy and helped to found the Johns

Hopkins School of Advanced International Studies, one of America's premier professional schools of international affairs. The author of the second passage held a faculty position at the University of Virginia and was Acting Dean of Georgetown University's School of Foreign Service. The book in which the second passage appeared was published by a university press and was addressed to a largely academic audience. Indeed, much of the chapter from which the second passage was taken betrays keen disappointment that scholarly writing on international affairs *does not* speak more clearly to the many uncertainties and daunting analytic tasks practitioners face. The author of the third passage, a professor at the London School of Economics, offers a view common among international relations scholars—that they will lose professional independence and credibility by trying to speak about practical issues.

Such sentiments, however, have become common only in the last few decades. As readers of Machiavelli, Hobbes, and Hobson appreciate, theory in the study of politics, including world politics, has traditionally been intended to guide practice. Diplomats of earlier generations would have found quite odd the notion that university scholars who studied international relations had little of interest to say to them. Important examples of such influence are not hard to find. Several generations of post-World War II U.S. officials had much of their general worldview formed or reinforced by exposure to Hans Morgenthau's stark Realpolitik in *Politics Among Nations*. During the 1970s, models that focused on the catalysts and implications of transnational economic forces had a comparable, if more limited, impact on official thinking. From the late 1950s onward, the important conceptual literature on arms control—work derived from theories focused on unintended conflict spirals—had an impact on key aspects of U.S. nuclear weapons deployments, investments in the command-and-control apparatus, and operational nuclear doctrines. Since this work focused on the interplay between military postures and the likelihood of inadvertent war, it gave policymakers a coherent way to diagnose an important problem as well as manipulable levers—tacit and formal measures to promote invulnerable nuclear forces—through which they could try to deal with it.[4]

For many reasons, connections between scholarly ideas and policymakers' thinking in international relations are less common today, and the gap may grow unless we rethink carefully our approach to policy relevance. Deep, often ritualized rivalry among theoretical schools makes it unlikely that future officials will leave their university training in this subject with a clear,

well-formed worldview. Such intellectual competition, of course, *could* be stimulating and useful, especially if it led officials to question their basic causal assumptions or consider rival explanations of the cases they face. More commonly, officials seem to remember the repetitive, often strident theoretical debates as unproductive and tiresome. Not only is much international relations scholarship tedious, in their view; it is often technically quite difficult. Partly for this reason, much of it is so substantively arid that even many scholarly specialists avoid trying to penetrate it. From a practitioner's perspective, it often seems as if university scholars are increasingly "withdrawing . . . behind a curtain of theory and models" that only insiders can penetrate.[5]

In addition, for many observers, the end of the cold war has made it harder to find models providing a compelling link between the international environment and manipulable policy instruments. One exception to this growing split between scholars of international relations and policymakers is the work on the inter-democratic peace, which we discuss in chapter 5. This work, as we will show, has deeply influenced many contemporary policymakers. But, for the most part, it remains the exception; the professional gap between academics and practitioners has widened in recent years. Many scholars no longer try to reach beyond the Ivory Tower, and officials seem increasingly content to ignore it.

According to much conventional wisdom, this situation is unsurprising. International relations scholars and practitioners have different professional priorities and reflect different cultures. Not only is it often assumed that good theory must sacrifice policy relevance; but also those seeking guidance in diagnosing policy situations and making policy choices, it is often thought, must look for help in places other than contemporary social science research.

This book challenges much of the conventional wisdom on these issues. It argues that IR theorists and foreign policy practitioners have important needs in common as well as needs that are different. Social science theory seeks to identify and explain the significant regularities in human affairs. Because people's ability to process information is limited, they must perceive the world selectively in order to operate effectively in it; constructing and using theories in a self-conscious way helps to inject some rigor into these processes.[6] For these reasons, both theorists and practitioners seek a clear and powerful understanding of cause and effect about policy issues, in order to help them diagnose situations, define the range of possibilities they confront, and evaluate the likely consequences of given courses of action. At

the same time, a deep and continuing concern for the substance and stakes involved in real-world issues can help prevent theorists' research agendas from becoming arid or trivial. This book therefore has two objectives: to elaborate and justify the reasoning that leads to these conclusions, and to illustrate how scholarship on international relations and foreign policy can be useful beyond the Ivory Tower.

Three issues should be clarified at the outset. One concerns the primary audience for this book. It is not a handbook for the conduct of foreign policy. We lack the detailed substantive and process knowledge needed to write such a book, not to mention the practical, accumulated experience that would make it credible. Our comparative advantage is in framing issues for our fellow academics to think about, and it is primarily to them that this work is directed. In arguing that IR scholars should embrace policy-relevant work, we clearly cannot guarantee that it *would* resonate widely outside the Ivory Tower. For that to happen the potential audience outside the scholarly community must be willing to listen, a matter over which academics have relatively little control. What they do control is their own agenda—one that we argue has become progressively and needlessly narrowed to issues that resonate *only within* the academy. This book argues that this agenda can be broadened in ways that would benefit both scholars and foreign-policymakers. In support of this position, the chapters that follow describe the various types of policy-relevant knowledge, how such knowledge is acquired and could be used, and illustrate these arguments with a variety of real-world examples. In doing this, we emphasize that relevant scholarship implies no necessary compromise of professional scholarly standards.

A second issue concerns the way in which the terms "international relations" and "international relations theory" are used in this book. International relations consist of the political, economic, military, social, and cultural exchanges that occur across the boundaries of sovereign states, in institutionalized as well as ad hoc contexts.[7] Likewise, the study of international relations has always enlisted participation from historians, lawyers, theologians, philosophers, psychologists, and economists in addition to political scientists. We thus need to distinguish between international relations as a set of real-world processes and the scholarship that analyzes these processes. We will designate the former as IR and the latter—academic scholarship in international relations—as SIR. Finally, despite the many dimensions of IR activity in the real world, the *theory* of IR in its modern

guise is largely, though certainly not entirely, the work of academic political scientists. For that reason, we take *IR theory* in its modern sense to mean efforts by social scientists, especially political scientists, to account for interstate and trans-state processes, issues, and outcomes in general causal terms.

A third issue concerns an important type of policy relevance we do not discuss. In addition to the substantive knowledge that might help officials identify better options or better understand their environments, "process knowledge" might help them better organize their decisionmaking procedures. The assumption behind this claim is that improving the policy machinery, all else equal, will lead to better policy choices.[8] Sound decision processes are certainly preferable to poor ones, but those processes, no matter how well designed, can work only as well as their inputs—that is, the substantive questions, assumptions, and empirical generalizations that are brought to bear on the conduct of foreign policy. Much SIR addresses issues of *substance* rather than process, and we discuss why and how it could improve the substance of thinking on foreign policy.

The balance of this chapter serves four purposes. The first two sections explain why international relations has important, practical implications. Whatever their precise professional duties and roles, most observers of the subject care about these practical issues; for many, these interests bring them into the field in the first place. While traditional SIR was often narrowly focused on the concerns of a small handful of states and policy constituencies, much of it was solidly rooted in the real-world problems that preoccupied those actors. It spoke to thoughtful practitioners, much as the influential periodicals *Foreign Affairs* and *Foreign Policy* do today. In their efforts to create a rigorous science of politics, many of the scholars who championed the behavioral revolution in political science moved away from anything smacking of policy commentary. In so doing, they fostered a style of academic work that inevitably—in some cases deliberately—created the current theory-practice gap. Section three discusses these developments, highlighting the way in which notions of appropriate scholarly inquiry in international relations changed some four decades ago. The shift toward a more technically intricate style of research meant that whatever analytic guidance SIR could provide policymakers was increasingly placed out of the latter's reach. Section four discusses those needs of policymakers that should be satisfied by scholarly guidance, laying the basis for a closer examination in chapters 3 and 4 of how explicitly relevant research and theorizing could improve both policymaking and scholarship. Section five discusses the

organization of the book and spells out a bit more about the content of the subsequent chapters.

Scholarship's Practical Implications

Unlike literature, pure mathematics, or formal logic, the study of international relations may be valued largely for its practical implications and insights. SIR, like the major social-science disciplines, initially gained a firm foundation in academia on the assumption that it contributes to improved policy.[9] It is part of what August Comte believed would constitute a new, "positive" science of society, one that would supersede the older tradition of metaphysical speculation about humanity and the social world. Progress toward this end has been incomplete as well as uneven across the social sciences. But, in virtually all of these fields, it has been driven by more than just curiosity as an end in itself. Tightening our grip on key social processes via improved understanding has always been a major incentive for new knowledge in the social sciences, especially in the study of international relations.[10]

This broad purpose covers a lot of specific ground. Policymakers want to know what range of effective choice they have, the likely international and domestic consequences of various policy decisions, and perhaps whether, in terms of more general interests and values, contemplated policy objectives are really desirable, should they be achievable. But the practical implications of international issues hardly end there. How wars start and end, the causes and implications of economic interdependence, and what leverage individual states might have on trans-state problems greatly affects ordinary citizens' physical safety, prosperity, and collective identity. Today, it is hard to think of any major public-policy issue that is *not* affected by a state's or society's relationships with other international actors.

Because the United States looms so large within the international system, its citizens are sometimes unaware of the range and impact of international events and processes on their condition. It may take an experience such as the long gas lines in the 1970s or the foreign-inspired terrorist bombings in the 1990s to remind them how powerfully the outside world now impinges upon them. As Karl Deutsch observed, even the smallest states can no longer effectively isolate themselves, and even the largest ones face limits on their ability to change others' behavior or values.[11] In a broad sense, globalization

means that events in many places will affect people's investment opportunities, the value of their money, whether they feel that their values are safe or under attack, and perhaps whether they will be safe from attack by weapons of mass destruction or terrorism.

These points can be illustrated by observing university undergraduates, who constitute one of the broadest categories of people who are potentially curious about IR. Unlike doctoral students, they care much less about political science than about the substance of politics. What they seem to understand is that the subject matter of SIR, regardless of the level of theoretical abstraction at which it is discussed, inherently has practical implications.

One might argue that whatever our purpose in analyzing IR might be, we can have little confidence in our knowledge absent tightly developed theory and rigorous research. One might then infer that a concern with the practical implications of our knowledge is premature until the field of SIR is better developed on its own terms. But if one assumes that SIR inherently has significant real-world implications, one could also conclude that the balance in contemporary scholarship has veered too far from substance and too close to scholasticism.

As in other fields driven by a concern with real-world developments, SIR research has been motivated by both internally- and externally-driven concerns. The former are conceptual, epistemological, and methodological matters that scholars believe they need to confront to do their intellectual work: Which research programs are most apt to resolve the field's core puzzles? What is the meaning of contested concepts? Which empirical evidence or methods are especially useful, convincing, or weak in this field? The latter consist of issues relevant to policy practitioners and citizens: How can people prepare to deal with an uncertain future? More specifically, how can they anticipate future international developments to which they might need to adapt, assess the likely consequences of measures to deal with that future, or at least think about such matters intelligently?[12] While the best scholarly work tends to have important ramifications for both types of concerns, the academic emphasis has shifted too far toward work with little relevance outside academia. This balance must be redressed if SIR is to resonate outside the Ivory Tower.

Beyond this, shifting scholars' attention toward the claims about the world they seek to account for would help improve their work by the standards of academic scholarship itself. If SIR were, at least partly, justified by the light that it sheds on practical foreign policy issues, this would help academics

identify significant substantive questions, and, we feel, provide answers that clearly pass the "so what" question. Curiosity about practical problems and how they can be manipulated is what gives scientists many ideas about what areas of basic research need to be explored, what is generalizable within those areas, which empirical patterns can be explained by existing theory, and which puzzles require further attention.[13] Just as important, a grasp of practical issues helps ground theory in the facts for which it seeks to account.

In making the case that the balance between internally- and externally-driven concerns could be readjusted *without* diluting the intellectual value of SIR, it is worth noting that the large emphasis on the former is quite recent. Accordingly, it is worth examining the field's traditional preoccupation with externally-driven concerns, as a way to see where we have been and why that intellectual stance toward policy-relevance was taken for so long.

The Focus and Purpose of Traditional Scholarship

If "traditional" SIR implies work that preceded efforts to build a cumulative social science of international relations, such work goes back to Thucydides, if not Homer and Herodotus.[14] It was dominated by external concerns.[15] Most of the major ideas were developed in Europe during the early modern period, prompted by a desire to understand and address the problems of state building, the gradual acceptance of a norm of sovereign autonomy, and efforts to rationalize the use of force among states. Over time, a fairly coherent picture of world politics emerged. Relations among states were conducted through diplomacy, though the threat and use of force provided a continuing backdrop. Diplomacy was further shaped by a minimal international legal code that laid down the essential rights and duties of states. While the intellectual heirs of Machiavelli shaded this framework in one direction, emphasizing that sovereignty had to be continually defended, and those who wrote in the Grotian tradition shaded the picture differently, emphasizing the pull of common norms, there was broad agreement that the separate states had to find mutually advantageous ways to coexist.[16] In terms of method, historical, practical, legal and philosophical reflection helped to stimulate these insights.

This intellectual framework has been remarkably durable. According to Michael Banks, it produced "a conceptual toolbox which continues to this

day to dominate both the practice of world politics and much of its interpretation."[17] The key concepts and terminology that went with it—national interest, sovereign rights, just war, and so on—continue to provide a *lingua franca* for much of the field, among practitioners and scholars alike.

What was missing until well into the twentieth century was a discrete, coherent area of inquiry. Until then, SIR consisted of rather disconnected observations scattered across political philosophy, political economy, international law, and diplomatic history. As a distinct field in its own right, SIR was catalyzed by the shock of the First World War. Before the War, a certain complacency afflicted European thinking on international affairs—a sense that the key problems could be managed effectively, given existing practices and knowledge. That smugness was destroyed by a sense that the unprecedented destruction might have been prevented by more effective crisis management, a different approach to Germany before the crisis, or a less power-centered approach to diplomacy more generally. Galvanized by these might-have-beens, a broad elite consensus concluded that existing knowledge was inadequate; inter-state relations were sufficiently important and complex that a greater understanding was required. John Hobson summarized this view soon after the War began: " . . . at the present stage it is of paramount importance to try to get the largest number of thoughtful people to form clear, general ideas of better international relations, and to desire their attainment."[18]

The result was "a burst of activity in the universities," producing a rudimentary scholarly field of international affairs. Professorships were created, new curricula developed, and academic conferences abounded.[19] Alongside the new academic institutions, other organizations were created to educate professional elites about the importance of international affairs: the British Royal Institute of International Affairs and the U.S. Council on Foreign Relations were inaugurated in the early 1920s. The impetus for this activity, both inside and outside the universities, was externally-driven. The world statesmen had known for centuries had broken down along with deeply rooted assumptions, and some way had to be found to repair it. The title of the book in which Hobson's plea appeared—*Towards International Government*—captured the orthodoxy as well as the sense or urgency within the new field during the 1920s and 1930s in much of Anglo-America.

Whether inside or outside universities, most of the people who created this new field were "public intellectuals" whose purpose was to communicate ideas to a broad audience. Until quite recently, political and social

intellectuals have been those who by virtue of their interests have been deeply engaged in public discussion and debate. The term "intellectual" was coined to describe the writers who came to the defense of Captain Alfred Dreyfus when he was charged with treason in France in 1898. During the twentieth century a "public intellectual" was typically a writer, often driven by moral or political convictions, who addressed a general, albeit literate audience about public issues.[20] This description fit many key figures in the new field of SIR in the early post–World War I years: E. H. Carr, David Mitrany, Pitman Potter, and Alfred Zimmern. Somewhat later, Hans Morgenthau also fit the pattern. Trained as a lawyer in Europe, he was animated by the way Max Weber simultaneously pursued scholarship and social activism.[21] Morgenthau's political "realism" was shaped by his deep disappointment with the appeasement of the 1930s, and even though he was best known among academics for his theoretical work, he became a very public critic of U.S. policy in Vietnam during the 1960s.

As public intellectuals, these thinkers saw no sharp division between theory and practice in international relations. At various points in their careers, many combined writing and reflection with policy practice or advice to other practitioners. Before wartime service in the British Foreign Office gave Mitrany an opportunity to help design the functional agencies of the UN, he had honed his outlook on economic and social progress in a practical way as a director of the Unilever Corporation. Walter Lippmann was much better known for his newspaper columns, lectures on contemporary issues, and advice to senior political figures than for forays into academic scholarship. Because their observations about more general issues often grew out of contemporary policy concerns, the professors within this group drew little distinction between the language and content suited to the four major audiences for international relations thinking: university students, fellow academic professionals, foreign policy officials, and the wider public. Consequently, they published in the leading journals of opinion as well as in more specialized academic outlets.

Thoughtful traditionalists articulated a distinct logic of inquiry, one characterized as a "wisdom-centered" or holistic view of knowledge. From this perspective, social and political knowledge is gained by long experience with and deep immersion in substantive policy issues, historical periods, or specific actors. Rather than invoking general causal laws, holists believe that action can be explained by understanding it from the actor's own frame of reference, located within a rich historical or ideational context.[22] In an in-

fluential essay, Hedley Bull made a strong case for the broad relevance of this approach. He argued that efforts to formulate and test general hypotheses about IR—that is, efforts aimed at establishing a scientifically cumulative base of knowledge—could not succeed. Social science, he claimed, could not be used to come to grips with the inherent "substance" of international relations; it inevitably would miss or trivialize questions about social meaning, purpose, and causation in the international realm, or would seek uniformities and generalizations where they do not exist.[23]

By 1966, when Bull's essay appeared, methodological traditionalists were already losing ground to scientific behaviorists in many U.S. universities. Since foreign-policymakers found that they could go on with their work without paying attention to most of the new SIR literature, a significant gap between theory and practice began to develop. If SIR had remained methodologically where Bull wanted to keep it in the early 1960s, practitioners and theorists would have retained more of a shared language and there would be less of a theory-practice gap today. Still, these methodological developments did not make a widening gap inevitable. The gap grew out of changing scholarly fashions *combined with* the incentive structure within the academic profession, one that increasingly rewards internal and self-referential scholarly communication at the expense of concerns originating outside the Ivory Tower.

The Development of a Theory-Practice Gap in International Relations

In some areas, foreign-policymakers *have* been deeply influenced by the theoretical literature in International Relations. Aside from the work the work on the interdemocratic peace discussed in chapter 5, and, to a lesser extent, some of the literature on international institutions examined in chapter 6, strategic studies has been most important in this respect. Such concepts as "escalation dominance" as well as the more general notion of the prisoners' dilemma were conceived by academics but have become part of the daily vocabulary of many practitioners. Work on deterrence, nuclear proliferation, arms control, and the use of coercive force has influenced a host of U.S. weapons-acquisition and force-management issues.[24] At one time, such an impact on official thinking was not unusual. Concerns about effective public policy have traditionally been part of the academic study of politics;

the American Political Science Association (APSA), for example, was founded in part to "bring political science to a position of authority as regards practical politics."[25] By moving professional scholars away from externally-driven issues, the professionalization of political science has molded the kind of work by which they earn professional prestige, making them less able or willing to communicate with policymakers. From the perspective of many officials, SIR scholars are comfortable on their side of the gap, free of any obligation to address practical issues.[26] As a result, the public intellectuals who address current foreign policy issues now tend to have few or weak connections to universities, while the prominent scholars in this field tend to write almost exclusively for their own colleagues.

The Scientific Revolution in Political Science and International Relations

Scholarly focus on policy issues in international relations declined in the 1960s, as the social-scientific movement gained momentum. We use the term "scientific" rather than "behavioral" to characterize this shift, since traditionalist scholars were also interested in the sources and consequences of policymakers' behavior. What differentiated the scientists from those in the older tradition was their view that politics should be studied through the presentation and testing of explicit, falsifiable hypotheses, and that methods of testing should emulate those employed by the natural sciences. Consequently SIR's language, method, and focus drifted away from the "practical" matters that had animated APSA's founders.

As the "scientists" saw it, traditional scholarly literature about politics was a hopeless conflation of factual and evaluative propositions. To separate these elements, systematize the empirical side of the discipline, and deemphasize anything approaching policy prescription, the scientists articulated a strongly positivist conception of science. Their objective was a system of theoretical propositions from which testable implications about concrete observables could be derived, and where, in the absence of possibilities for strict experimentation, tests would employ as rigorously systematic methods as possible. Science was viewed as a methodological unity across the empirical disciplines; in principle, students of politics could aspire to the same logic of discovery and verification as those who studied physics.[27] As one prominent member of this movement put it, this view entailed "the idea that methods

of investigation, in all their aspects, are problematic and, accordingly, merit special concentrated attention."[28]

Two implications for research and teaching were quickly evident. Once "methods of investigation" are seen to merit privileged attention, internally-driven concerns tend to become much more important relative to externally-driven ones. And "if it is no longer necessary to test the relevance of research findings by their significance as possible solutions to practical problems,"[29] as this same scholar argued, the professional culture no longer even values the externally-driven concerns much at all. By the mid-1960s, the scientific revolution had encompassed SIR, especially at the major public universities in the U.S. Midwest. Scientifically oriented scholars disparaged the traditional IR literature, arguing that the field essentially had to be reinvented from the ground up. Ultimately, it was argued, to every empirical proposition a precise measure of confidence should be assigned: " 'knowledge' which is unconfirmed, incomplete, or based on the prestige of the source rather than the credibility of the evidence" should be rejected.[30] By these criteria, little existing work comprised acceptable knowledge.

This attitude impugned the traditional wisdom that had accumulated over the centuries *before* anything comparable had been developed to replace it. In place of propositions that had, however imperfectly, provided some guidance to thoughtful statesmen, much more attention was now paid in university courses to aggregate data analysis, research design, mathematical modeling, and philosophy-of-science issues. However much this self-conscious attention to rigorous strategies of inquiry paid off in actual knowledge acquired—and that remains a controversial issue among many scholars even today—it profoundly changed the ethos of the scholarly field. Rather than trying to help thoughtful practitioners interpret the world in which they operate, SIR scholars increasingly talked among themselves about the means rather than the ends of their enterprise.

More recently, many SIR scholars have gravitated toward self-contained groups of like-minded scholars who share epistemologies and research agendas. In many (though certainly not all) of these groups, the driving intellectual issues are of a technical, not substantive, nature. Thus, for reasons to be laid out shortly, most of SIR has not moved back closer to an immersion in real-world problems, nor in many cases even to work that could be plausibly connected to such problems.

The extent of the gap between scholarship and policy can be appreciated by noting that an academic background in SIR is not a requirement for

policy positions. Senior foreign-policymakers are just as likely to come from law, business, or other fields as they are from a university background in SIR. This pattern can be compared to the usual situation in economics, where formal training is generally considered a prerequisite for policymaking responsibilities in international as well as domestic economic policy. Economic theorists and policymakers thus have little trouble understanding each other's intellectual frames of reference, making it likely that they will at least appreciate each other's concerns. Lacking such a basis for communication, scholars and practitioners of international relations learn from each other much less often, in part because professional mobility between the two groups is very limited.[31] While candidates for the U.S. presidency now routinely rely on scholars to provide them with position papers and material for speeches, the people who play this role today for foreign policy issues tend not to contribute to cutting-edge IR theory. In effect, those IR experts who "speak to the Prince" in the tradition of Machiavelli are now almost wholly distinct as a group from those who speak mainly to the academic field. This distance between the two groups reduces officials' incentives to seek academic guidance and theorists' incentives to produce policy-relevant knowledge.

The Effects of the Academic Incentive System

The chasm separating scholarship and policy in IR is not inevitable, especially when compared to the situation in other fields with applied and theoretical facets. Although scientists typically do not earn scholarly recognition in their own fields by sharing knowledge with those in applied areas or the general public, they often derive other professional rewards from doing so.[32] Just as medical researchers see physicians as their primary audience, IR scholars *could* measure their professional prestige at least partly in terms of how seriously their ideas are applied outside the academy. The modern academic incentive system, however, operates to frustrate any such goal. At least since Max Weber discussed the differences between the vocations of politics and science, a large literature has developed that probes the sociology of modern academic life, especially within disciplines that are scientific or aspire to that status.[33] From that work, and an insightful critique of the political science profession written from a sociology-of-knowledge perspective, three features of academic life stand out as particular culprits in the

growing practical irrelevance of much SIR. First, scholars are increasingly inclined to tackle smaller, often trivial, research problems, rather than questions of a more fundamental nature and broader reach. Second, technique has in many cases triumphed over substance in IR research programs. Third, the professional status of academics depends mainly on how their work is received by fellow scholars, rather than by those outside the Ivory Tower.

Narrowed Concerns: Within scholarly communities, a recognition for originality signifies professional accomplishment. Since originality comes at more of a premium the older a field becomes, scholars tend to define original to mean "novel." In practice, they often look for research projects and intellectual niches that are novel precisely because others have ignored them. Academic fields thus tend to shrink into ever-smaller areas of specialization and expertise, "so that some scholars can quickly stand forth as patently competent with regard to subjects that other scholars have somehow overlooked."[34]

These patterns are clearly evident in contemporary scholarship. SIR academics do relatively little creative work, if that is taken to mean the charting of new intellectual paths. Instead, they tend to be professionally risk-averse, and thus tend to remain well inside the boundaries of inquiries in which most of their colleagues operate. These behaviors seem to be driven by several related assumptions. Like other scholars, IR scholars tend to assume that issues occupying a sufficient number of others must indeed merit a substantial investment of scholarly resources. They also appear to believe that possibilities for intellectual support and useful feedback are better in well-trampled areas. Finally, professional visibility and advancement require that one's work be frequently cited by other academics, and this generally occurs when one works within an area that claims the attention of many scholars. As a result, novelty is achieved by looking for new, usually smaller questions within broadly traveled approaches and areas. The result is an expanding but increasingly hyperspecialized and often arid body of knowledge.

A good indicator of these patterns is the growing number of academic journals in the field. The most recent edition of a guide to publishing in political science journals lists 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. (There are more than one hundred such English-language politics and policy journals.[35]) These journals comprise qualitative and more quantitative outlets, as well as those

that specialize by subject matter within the IR field. While much of the work published in these journals is valuable, one trend is clear: as the overall readership within the field has segmented along substantive and methodological lines, scholarly authors have less reason to communicate to a broad audience about fundamentally important arguments or research results. For example, when an article titled "Alliance Formation and National Security" uses an expected-utility model to discover that "the pattern of alliance formation through time is related to the opportunity to enhance security" and that "*realpolitik* considerations of security are crucial to alliance formation decisions,[36]" practitioners might reasonably wonder what IR theory can tell them that do not already know.

The triumph of technique: The related tendency for research techniques to triumph over substance constrains our ability to derive real meaning from our subject matter. As Max Weber noted:

> Science . . . presupposes that what is yielded by scientific work is important in the sense that it is "worth being known." In this, obviously, are contained all our problems. For this presupposition cannot be proved by scientific means. It can only be *interpreted* with reference to its ultimate meaning, which we must reject or accept according to our ultimate position towards life.[37]

Weber was reacting to the professionalization of scientific research in German universities during the late nineteenth century, a development that spawned imitation elsewhere but was viewed with suspicion by those with a more humanistic outlook. As science came to require highly technical procedures, it ceased to be an amateur activity; to be able to do scientific work, one had to become an accomplished craftsman in those techniques.[38] This ethos has served important functional purposes for the growth of scientific disciplines. But it has also allowed techniques to define the essence of some disciplines and research traditions, aside from any independent assessments of their substantive results. For example, according to a respected game theorist, so many formal models have been developed that political scientists cannot meaningfully compare their empirical performance. Failing such a test, "the discipline of political science bases its evaluation of them on their mathematical elegance, the complexity of their notation, the journals in which they appear, or simply the reputations of those who design them."[39]

A more extreme example of this syndrome is found in economics, where tool-driven training has come to dominate graduate education. In 1999, the

MacArthur Foundation sponsored a conference at which PhD students in economics were shown how they might do applied research. The sponsors believed that first-year graduate training in economics has become so relentlessly mathematical that students in those programs do not know how to formulate an applied research project. Aside from the sponsors, a number of prominent economists fear that this kind of disconnect with the real world might drive bright undergraduates from the field. One of them, while reluctant to criticize the field's graduate training as "too theoretical," was quick to label it "increasingly aloof and self-referential."[40] A significant part of political science seems to be moving in the same direction. Many social scientists in other fields have long envied economists for their seeming ability to capture a complex reality through elegant models. Because political science deals with a more confined area of human activity than anthropology and sociology, the questions it asks have seemed more susceptible to formal approaches. Ordeshook is again cautiously skeptical about this trend, arguing that debates over such real-world topics as lags in investment and unanticipated inflation have been a more important catalyst of theoretical insights than statistical tests of formal models.[41]

These arguments should not be interpreted as a blanket critique of statistics or formal models, both of which have been quite valuable in IR work. Statistical methods are necessary to find or verify many empirical generalizations. Formal models can be used to clarify key concepts; they also serve to establish the logical preconditions of more as well as less obvious research results, thereby increasing our confidence in both. SIR work that uses these methods can be just as policy-relevant as work that uses qualitative approaches. Nevertheless, the scholarly work that uses formal and statistical techniques often hides behind them and fails to yield results that appear interesting or important outside a very small, self-referential audience. Ultimately, the quality of such scholarship is too often assessed by how esoteric its techniques are. Preferred techniques tend to be those employed by disciplines at least one rung higher on the ladder of academic prestige: in the case of political science and SIR, the techniques emulated tend to be those of tool-rich contemporary economics.

A Restricted Audience: All of these problems are reinforced by academic faddishness, a pattern that reflects scholars' tendency to take their cues from one another rather than any external standard. Especially in the United States, a scholar's standing within her discipline, or within a still narrower subset of that discipline, is the key to professional prestige.[42] Scholarly standards must, of course, be applied when that kind of expertise is necessary to

judge the value and quality of scholarly work. But those standards also tend to become a professional benchmark for narrower, more instrumental reasons—reasons that often have negative effects on the direction of scholarly agendas. By deciding what is published in which outlets, who gets which grants, and how other scholarly rewards are distributed, one's scholarly colleagues and especially the leaders of one's field have a large impact on a scholar's professional reputation and visibility. Accordingly, "most academics are only concerned about the good opinion of about a dozen other academic specialists in their particular sub-sub-field."[43] The result is to make scholarly fashions, including those that discourage policy-relevant work, strongly self-reinforcing.

The cost has been a growing gap between the field's applied and theoretical sides. Insofar as the field's language and methods have moved toward those of the hard sciences, few foreign policy practitioners understand its literature. Insofar as its content has become narrow and self-referential, they have little incentive to try. Unlike the situation in economics, where practitioners must retain their scholarly fluency to communicate with other practitioners, foreign-policymakers can ignore the theoretical literature in that field if they wish. Foreign policy practitioners tend to think eclectically and holistically, drawing on their knowledge of particular states, regions, or people when they confront a problem. They do not draw the disciplinary lines that scholars, especially contemporary ones, typically draw. It is no accident that the most broadly influential recent scholars of international relations—Francis Fukuyama, Samuel Huntington, and Paul Kennedy—are big-picture thinkers who address a wide audience. Though each is a respected scholar, all in recent years have functioned more as public intellectuals of the older type than as technique-intensive academics.

This is not to suggest that one cannot be a significant theorist *and* an effective public intellectual, as a number of scholars of international security (such as, for example, John Mearsheimer) have demonstrated. As we will argue in chapter 4, there is no *necessary* incompatibility between scientific excellence and policy relevance in international relations. But any effort to pursue these agendas simultaneously raises basic questions about what knowledge is for and how it is packaged. As one British observer asked,

> What is the relative importance of the three different audiences for which we write and speak: our colleagues, our students, and the wider public? Does the intellectual have a duty to all three audiences—to

> educate a wider group than her own students, even to contribute to raising the quality of debate in society as a whole?[44]

If the answer to these questions is affirmative, it has implications for the kinds of problems SIR scholars examine, the publication outlets they choose, and the style in which they package the arguments and evidence. If we take seriously what policymakers themselves say about these issues, they will continue to ignore the Ivory Tower until it focuses more seriously on policy-relevant matters.

Policymakers and the Theory-Practice Gap

An obvious question at this point is whether decisionmakers would *ever* be likely to find SIR useful; everyday observation suggests that practitioners tend to ignore it. To push the point a bit further, wouldn't this book, written by two professors, be more compelling if it were written by policymakers who decided after a lot of trial and error that they could use more scholarly guidance after all? These are important questions. It may be that the theory-practice gulf in IR is too wide to be crossed with any regularity. We believe, however, that such a judgment is premature. If one examines what thoughtful IR practitioners say about this problem, it is evident that they want useful guidance from SIR, including theorists, and that they might actually use it if theorists were to meet them half-way.

To do that, academics must appreciate the constraints and incentives under which decisionmakers operate. Officials have very little time to read and reflect. Joseph Nye, one of the few people who has flourished as both a scholar and a policymaker, was surprised at how "oral" the culture of top-level government service has become. As he put it,

> The pace did not permit wide reading or detailed contemplation. I was often bemused by colleagues who sent me thirty- or forty-page articles they thought would be helpful. It was all I could do to get through the parts of the intelligence briefings and government papers that my various special assistants underlined for the hour or two of reading possible on a good day.[45]

Nye also emphasized, as has Henry Kissinger, that one typically operates in office on the basis of whatever intellectual capital had been accumulated beforehand. So unless an official tries to stay in touch with academic developments while in office—and Nye's comments suggest that this is unlikely—getting her (or even those busy special assistants) to pay attention to what scholars say will be difficult.

One might deal with this problem by assuming that even though officials will *not* read the scholarly article, let alone the book, they might read an op-ed piece or a *Foreign Affairs* article that digests it and highlights the policy-relevant implications. Along with his work in scholarly journals, Mearsheimer produced a steady stream of opinion pieces during the 1990s in *The New York Times*, mainly on such front-page topics as the Balkans conflict. Along with an intriguing but distinctively "academic" version of an argument linking the probability of war to the process of democratization, Edward Mansfield and Jack Snyder produced a shorter, more accessible version of the same material for *Foreign Affairs*.[46] Even if busy officials cannot read the more user-friendly versions, their staffs might do so, and *future* officials will be more likely to absorb the ideas if they are presented in accessible forms and outlets.

When asked, policymakers tend to be forthright about what they find useful from SIR. "The simple, well-founded empirical proposition"[47] is one such contribution. For example, the link between democratization and the incidence of conflict has been influential because it is intuitive: it accords with common sense and can be explained easily to almost any audience. Of course, few SIR generalizations are as straightforward and well-supported as this one. Still, decades of empirical work have yielded more of them than is often realized. We now understand reasonably well how cooperative and more coercive strategies can be used to maximize the likelihood of cooperation, when deterrence is likeliest to fail, the conditions under which economic sanctions seem to work, and the causes and effects of nuclear proliferation. If it were presented in digestible forms, such research might be more useful to policymakers than it now seems to be.

Another such contribution consists of "models of strategy"[48]—propositions that link various tools of statecraft to foreign policy objectives. Alexander George's influential book *Bridging the Gap* argues that such models, along with the case studies that show how the various strategic options have performed, constitute the IR theorist's most effective contribution to better policymaking.[49] George's suggestion is buttressed by the organization of the

IR field, especially in the United States. Most scholarly work in IR either consists either of "issue-specific" puzzles that examine empirical or theoretical problems in generic causal terms or more detailed, less generalizable case studies, often dealing with these same issues. Some of the most enduring, important IR puzzles include those mentioned or implied in the previous paragraph: Are economic sanctions useful? If so, when and for what? When is accommodating an adversary likely to avert war, and when is such a strategy likely to induce it? These are precisely the kinds of issues policymakers must deal with and the questions they want answered. IR scholars have produced a wide body of empirical literature that might, if appropriately packaged, provide them with guidance.

Foreign-policymakers are equally clear about the elements of academic work and culture they dislike. Not surprisingly, these sound a lot like the worst products of the contemporary academic incentive system. They have little use for research that does not address important, real-world problems. As the belief takes hold that SIR scholars no longer care about these issues, even officials with academic backgrounds pay less attention to scholarly conferences and publications.[50] They dislike excessive jargon, especially when it seems employed in the service of trite findings. And they have no use for work that seems overly self-referential; it seems designed *not* to appeal to a wider audience.[51]

These sentiments reflect the fact that foreign-policymakers come to such work from a variety of backgrounds and lack a common professional language. Unlike, for example, lawyers, economists, or political scientists, they share neither a specific professional vocabulary nor any specific type of methodological training. The knowledge they need to do their jobs is mainly acquired in other ways, typically on or just prior to taking the job. In the United States, junior foreign service officers are recruited from a wide range of educational backgrounds and pick up the languages and substantive knowledge they need in intensive, government-run programs. Senior officials learn the detailed substance of their positions on the job as well. Policy specialists earn that status by immersing themselves in the substance and process of their work and by being recognized as such by fellow practitioners; there is no standardized intellectual socialization or certification.

From the point of view of scholars who want to produce relevant research and to communicate it outside the Ivory Tower, these patterns present a double-edged sword. The absence of a common language connecting foreign policy specialists makes it difficult for scholars to speak to them. To be

credible within *their* professional circle—that is, among fellow scholars—academics typically must use and assume their primary audience's familiarity with certain concepts, lines of argument, and research tools. Any or all of these may be "foreign" to segments of the policy community. For this reason, even substantively important, relevant SIR may not travel equally well to all constituencies of policy specialists.

At the same time, of course, a greater reliance on the common vocabulary employed by social scientists could improve the clarity and reliability with which concepts are communicated *within* the policymaking community. Moreover, policymakers can often benefit from the more detached perspective and greater rigor that scholars can provide. Because working officials learn by doing, they often become very skilled in analyzing today's problems. What they often miss, because they lack the time or detachment to consider it, is how the present might reflect important features evident from the past, or how comparable cases in different issue-areas might shed light on their own immediate problems. In this sense, academics may be able to help decision makers see patterns evident at the level of the forest that are obscured when one stands in the shadow of a single tree.

So far, we have treated "policymakers" and "theorists" as if each were a highly homogenous group. While useful heuristically, this simplification also obscures some key distinctions within the groups. Some policymakers resist the notion that there are significant regularities in IR about which one can generalize, while others accept that premise. Correspondingly, some IR theorists are interested in patterns that are *not* issue-specific, though most tend to generalize at a somewhat lower, issue-specific level of abstraction. For the foreseeable future, the most obvious bridge across the theory-practice divide will probably connect "mid-range" theorists to those policymakers who have some familiarity with the literature, often produced in think tanks, that *uses* generic knowledge to explain certain types of real-world problems. Given the way the overlapping but distinct IR groups are organized, at least in the United States, these professional connections are already the best developed and seem likeliest to flourish in the future.[52]

Why Revisit the Scholar-Practitioner Problem?

Even if there is a reason for scholars and practitioners of international relations to communicate better than they now do, the thrust of the previous pages must surely indicate that the gulf is wide. Why then bother to revisit

this issue again? The answer is twofold. The world has changed in ways that make officials less confident about what they know or believe they can project about the future, and many of them frankly admit it. If ever there was a time when pertinent scholarly expertise might really help them, this is it. In addition, the existing literature on policy relevance in international relations has, in our view, interpreted the notion and benefits of "relevance" too narrowly. As policymakers increasingly need to understand a complex, unfamiliar word, they may come to see academic knowledge as useful in new ways.

The last several decades of SIR have been dominated by concerns with the superpower conflict, but the end of the cold war and newly emerging international concerns have decreased the relevance of traditional issues. During the cold war, bipolarity seemed so stable that little effort was made to explore other aspects of conflict and community in world politics, such as the genesis and evolution of values across states, the consequences of intense ethnic loyalties, or the impact of an increasingly globalized marketplace. But these issues now occupy center stage in the real world, and both practitioners and scholars have incentives to understand them better.

Since the fall of Communism in Eastern Europe, the sources of security threats, the composition and cohesion of alliances, and the shape of regional orders have been in flux. The further enlargement and responsibilities of NATO, not to mention the broader possibilities and limits of multilateral security cooperation, hang in the balance. In this new century, ethnoreligious conflicts are likely to dominate the security landscape in much of the Third World, even as a zone of peace seems to be taking hold over much of the Northern Hemisphere. Policymakers have a clear interest in anticipating and understanding possible conflicts across these two broad regions. The effects on Japan and on Eurasian stability more generally of a rising China and an imploding Russia are likely to be profound, and officials will want to understand them. Controlling the proliferation of weapons of mass destruction is likely to become harder as it becomes more urgent. Since these problems are developing against a strategic backdrop quite different from that of the cold war, decisionmakers may require and desire help in sorting them out. Security-focused SIR that brings in considerations of ethnicity and community and work on identity that has implications for security might shed light on key policy issues.

Outside the realm of security as traditionally defined, the picture has become even more complex. Deepening but often destabilizing economic linkages, massive refugee flows and other humanitarian emergencies, and

unprecedented global ecological problems have created a new set of issues that also vie for official attention—issues that challenge old ways of thinking about national interests and appropriate policy tools. None of this renders the traditional Westphalian analytic paradigm obsolete or unhelpful. It still provides analytic leverage on concerns related to inter-state conflict and conflict prevention. But many issues on the contemporary foreign policy agenda arise from internal societal pressures and thus fall outside its purview. Policymakers may thus benefit from consulting those areas of SIR that strive to connect general insights about international relations to country and region-specific knowledge about community and identity. As scholars are coming to appreciate, that kind of academic work has much to recommend it on intellectual grounds.[53] As a significant byproduct, it could also become highly policy-relevant.

Interestingly, decisionmakers recognize these intellectual challenges and seem to desire help in dealing with them. As the late Joseph Kruzel—a senior Defense Department official at the time of his death and a one-time professor at Ohio State—put it,

> with the collapse of the Soviet Union, there has been a profound breakpoint in the policy process. The bureaucratic predilection to do tomorrow what you did yesterday does not work when the whole world has changed. When the Berlin Wall came down, bureaucrats looked at each other and asked "what do we do now?" They did not know, and they looked to the academy for ideas about how to deal with this new world.[54]

Kruzel inferred that "the academy," meaning the theoretical side of the IR profession, had not responded to this opportunity. We should not be surprised: as suggested earlier, powerful incentives within university life have pushed much of political science toward practical irrelevance.

Not all scholars are content with this state of affairs. Often enough, SIR works end with thoughts on policy implications, even if they look like afterthoughts and receive little notice from reviewers and other readers. The unwillingness to neglect policy implications entirely suggests a residual desire on the part of some SIR scholars to be useful. Every issue of applied IR periodicals such as *Foreign Affairs*, *Foreign Policy*, *The Washington Quarterly*, and *Survival* is full of articles dealing with the policy-relevant implications of the new security environment, the consequences and effects of globali-

zation, and so on. Yet precisely because their methodology and analytic approaches are similar to those employed by many policymakers, this literature may not tell officials much that they do not already know. What many of these works supply in relevance they often lack in a studied distance from issues in the headlines, not to mention scholarly rigor. Articles in *World Politics*, *International Organization*, and *International Security* at times deal with these same topics, albeit from a professional social-science standpoint. Even if the scholarly agendas that produce these pieces reflect mainly internally-driven concerns, their authors *could* highlight and elaborate upon their practical implications.

Doing so would add relevance to the rigor contributed by social science. The combination could conceivably lead to the emergence of a new breed of public intellectuals who speak about foreign policy. These intellectuals would combine a concern for real-world issues, a desire to communicate to a broad audience, and a systematic set of analytic procedures.

Well-designed scholarly research could provide a key analytic check on officials' reasoning. Whether implicit or explicit, theoretical frameworks affect what one sees and how it is interpreted. No phenomenon can be perceived meaningfully without prior conceptions of it; knowledge is therefore embedded in theoretical understanding. Like other ideas, international relations concepts such as "engagement," "containment," "power," and so on are intelligible and acquire meanings only in the context of some explanation. Since people have a strong tendency to fit incoming information into their existing assumptions, images, and beliefs, it is important that they understand how such ideas affect search, evaluation, and decision procedures.[55] Explicit encounters with appropriate scholarly work can serve as a check on the content and suitability of policymakers' assumptions, images, and beliefs—embedded as they will be in ideas that may need to be unpacked, analyzed, and modified in light of new evidence or better scholarly understandings of the subject. One could even argue that *unless* policymakers are self-conscious about their assumptions, they will be likely to act on the basis of oversimplified, outdated, or otherwise inappropriate premises. Theoretical self-consciousness in this sense cannot eliminate perceptual and analytical errors, but it should help in reducing their scope and impact.

Just as IR academics can do more than they are now doing to be relevant outside the Ivory Tower, their work would often be enriched by more frequent and meaningful encounters with practice. As we discuss in more detail in chapter 3, the relationship between theory and practice is a two-way street.

Not only can good theory influence practice by shaping the questions people ask and the hypotheses they consider; a careful study of past experience is often helpful, even necessary, in developing good theories. In our everyday lives as well as our professional lives, it is the unexplained but persistent behavioral pattern we often want to make sense of, or the deviant case that we strive to "make fit," given what we think we know. In these senses, past practice provides many of the questions academics want to explain. Even scholars who do deductive work benefit when their substantive assumptions are shaped by a perceptive reading of past experience. To take just one example, it was long assumed that when deterrence failed, it was by a process that occurred all at once or not at all. By carefully examining a number of important cases of deterrence failure, Alexander George and Richard Smoke showed that deterrence can also fail in stages, as an initiator gradually nibbles away at the status quo.[56] The kinds of questions this opened up for scholars were matched by their policy relevance: if deterrence fails in stages, the initiator's *and* the defender's resolve is continually challenged, heightening the need to better understand how costly miscommunication can be avoided.

The Outline of the Book

This book discusses the possibilities and limitations of policy-relevant knowledge, its forms, its range, and the paths by which it may be brought to the attention of policymakers. Chapter 2 argues that knowledge is relevant under two conditions: if it establishes the range of possibilities for policy, and if it identifies the consequences of various courses of action. Within this framework, we discuss the forms of knowledge and reasoning upon which policymakers base their decisions, and we explain how this knowledge and reasoning can (and cannot) be complemented or supplemented by the contributions of social science. In the process, we focus on the properties of explanation, and show how good explanations of important phenomena lie at the foundation of the academic contribution to better policy. Chapter 3 discusses the various paths by which academic knowledge can enter the policymaking process: either in response to a specific demand for scholarly assistance expressed by government, or as a result of improved understanding generated autonomously within the academic community. Here, we also examine the various professional contexts within which such knowledge is produced, and the manner in which they may operate as links in the trans-

mission mechanism by which understanding travels from the Ivory Tower to policymaking institutions.

We believe that the value of policy-relevant IR theory should be assessed not simply from the decisionmaker's vantage point, but also from the perspective of the scholarly enterprise itself. Many scholars fear that knowledge tethered to practical purposes may cause them to lose their independence or impair their uncommitted speculative curiosity. Chapter 4 argues that both beliefs are mistaken. The first part of the chapter identifies two attributes of good theories—soundness and attention to meaningful questions. The second part examines whether there is a tradeoff between good theory and practical relevance. We argue that policy-relevant knowledge stands to be as good, or better, from a purely scholarly perspective than knowledge produced with no regard to its utility.

Chapters 5 and 6 discuss specific instances of scholarship that seem relevant to the conduct of contemporary U.S. foreign policy. Chapter 5 examines contemporary literature on the interdemocratic peace, discussing its assertion that democracies, unlike other types of political systems, do not fight one another. If this claim is to serve as a foundation for the architecture and conduct of U.S. foreign policy, its empirical truth must be closely evaluated. Beyond this, questions must be raised about the ceteris paribus conditions that qualify the relationship between democracy and peace, and about the indirect consequences of acting on this assumed relationship. This chapter does that. Chapter 6 discusses scholarship on institutionalized cooperation in world politics. Policymakers who want to pursue coordinated policies within the framework of international institutions must ask several questions: Is the distribution of state preferences conducive to cooperation at all? Even if cooperation is possible, what kinds of costs and benefits would a regime carry? The school referred to as *political realism*, and that designated by the term *liberal institutionalism*, offer very different advice: their claims and counterclaims are evaluated here.

Chapter 7 concludes by emphasizing the common interests of international relations scholars and practitioners in knowledge that clarifies the range of the possible and the consequences of various courses of action. It also suggests ways in which firmer bridges between the two communities may be built.