7 Useful Knowledge: Value, Promise, and Limitations

The gap between international relations scholars and decisionmakers has assumed a character of ineluctability—a condition that is surprising in a field created less than a century ago with the express purpose of shedding light on pressing policy problems. Although scholars and policymakers have different professional goals, both have a strong interest in understanding the processes and parameters of international relations. One would therefore expect sound analysis from inside the Ivory Tower to find resonance within the corridors of power. Good "ordinary" knowledge provides, at best, a partial basis for policy, and there are many ways in which it can be misleading. And policy relevance, as we have seen, goes far beyond relevance of a directly instrumental sort. Moreover, since there are no precise historical analogies to current international developments, practitioners need analytic help more than ever before.

Five key arguments have found form in this book. They suggest that international relations theory can be useful in more ways than is commonly thought, and with little or no cost to the quality of scholarship. They further specify what shape relevant knowledge can assume, the settings in which it can originate, and the paths by which it can be brought to inform policy. Taken together, these arguments provide a foundation for reorienting our thinking on the practical value of scholarship in international relations and foreign policy. We begin by summarizing the book's five major arguments, following which we will suggest some tentative steps via which a process of bridge-building between academia and government might be initiated.

What Have We Learned About Useful Knowledge? The Five Arguments

Our first argument is that SIR naturally has policy implications, and that the profession has lost sight of that fact for reasons lacking intellectual justification. Except in policy schools, university-based IR scholars have come to focus more on technical refinements and winning intellectual turf battles than in making sense of significant real-world developments. SIR was created for quite different reasons. Reflecting on the tragedy of World War I, a group of public intellectuals in the 1920s set out to give future foreign-policymakers analytic assistance in confronting issues of war and peace. Like the other major social-science disciplines, SIR soon found a place in university curricula on the assumption that it could contribute to improved policy. Hans Morgenthau, E. H. Carr, Inis Claude, and Arnold Wolfers saw no reason why good scholarship should not also address the major foreign-policy issues of their times. But beginning in the 1960s, the scholarly study of IR increasingly veered away from the interests and concerns of most foreign-policymakers, especially in the United States. The new emphasis on methodologically rigorous inquiry had value, but it came to be pursued at the expense of substantive significance.

Rather than help thoughtful practitioners interpret the world, SIR has become almost entirely self-referential. PhD's are trained to speak only to each other, and to train future PhD's. Unless they deliberately seek projects or professional experiences forcing them to confront real-world dilemmas (for example, via the Council on Foreign Relations Fellowship Program), they spend their careers wholly within the confines of internally defined problems. Because the status of professional scholars rests on how their work is received by their peers, scholarly fashions—including those that discourage policy-relevant work—become powerfully self-reinforcing.

The cornerstone of relevance is a quest for valuable knowledge, whereas the sociology of academic life, especially the reward structure of the social sciences, provides few incentives in this regard. Where the primary quest is to emulate the epistemologies of disciplines a perceived notch higher in academia's status hierarchy, rather than to address empirically and theoretically meaningful questions, and where method rides roughshod over significance, the aridity of technique naturally eclipses the value of substance. Rather than work that is intellectually powerful, much contemporary SIR scholarship offers findings that at best are commonplace, a good bit of which seems to be focused on demonstrating methodological mastery rather than illuminating major real-world problems. As scholars have come to tackle smaller, narrower, and sterile issues, practitioners have increasingly ignored them, and public foreign-policy intellectuals have lost standing in the university culture.

Accordingly, a major reason for the gap between scholarship and the policy process must be sought in the evolving cultures of academia and the policymaking community, rather than in the intellectual incompatibility of their respective enterprises.

Our second argument is that the notion of policy relevance should not be limited to knowledge of a directly instrumental sort, i.e. to that specifying a link between policy tools and desired outcomes, subject to certain (more or less fully elaborated) qualifying conditions. Useful knowledge has a greater span and can assume other forms. Specifically, it can help identify the context within which the instrumental relationships can be expected to operate, and it can help project the costs and consequences associated with the use of particular policy tools.

Contextual knowledge identifies the ceteris paribus conditions under which means lead to certain ends, and it specifies the circumstances that shape the availability or malleability of policy instruments, helping officials fully diagnose the challenges they confront. It is of little value, for example, to know that conventional deterrence can at times substitute for nuclear deterrence in controlling the outbreak of aggression, or that economic sanctions might change a target state's behavior, if those policy instruments are unavailable in practice (perhaps for political reasons), or could not work in a given context. At a time of pervasive international change, the right kind of contextual knowledge can help decisionmakers reevaluate whether old policy tools *are* still appropriate to the tasks at hand or whether new strategies must be devised.

Policymakers must also know what costs and consequences their actions might have beyond those directly intended. If the U.S. builds a limited missile shield to protect against threats or attacks from rogue states with modest ballistic-missile capabilities, how will that affect relations with Europe and China over the long term? If globalization continues at present rates for another generation or two, how much more day-to-day economic policy flexibility will U.S. leaders lose? These are issues that thoughtful policymakers and political leaders must understand, but will have a harder time grasping without policy-relevant SIR.

Our third major argument is that, whether instrumental, contextual, or consequential, the value of the professedly relevant knowledge depends on the quality of explanation it furnishes. Explanation of some positive statement (a conclusion) requires propositions about initial conditions (i.e., particular events, issues, or actors), *and* about generally applicable relationships (those that apply across various sets of initial conditions). While policymakers may provide much of the specific information required by explanation, scholars generally are better placed to furnish general propositions derived from, or embedded in, some theoretical structure. Thus, virtually any policy-relevant reasoning requires the kind of knowledge that SIR provides.

This is not to say that the fruits of SIR must always trump policymakers' "ordinary" knowledge, even when it comes to producing generalizations. For reasons discussed in chapter 2, some problems that interest government officials may have evoked little or no research from scholars. Aside from this, policymakers may be able to recognize patterns or diagnose situations that would be less intelligible to those lacking an applied background in foreign affairs. But the way in which officials obtain and use ordinary knowledge often leaves them prone to perceptual biases and inferential errors that distort what they see, how they react to it, and how they make decisions. Academics are by no means immune to such errors, but they are less apt to make them; and their professional peers can usually be counted on to notice them in the process of scholarly evaluation. It follows that policymaking should improve to the extent that officials become self-conscious about the content and process of their thinking—that is, insofar as their assumptions, their evidence, and their conclusions are subjected to rigorous examination and critique. Judicious use of SIR should help promote these goals.

Fourth, we explain that policy-relevant IR knowledge can reach officials by various paths, which are more numerous than is often assumed. For heuristic purposes, we assumed two ideal-typical models. In the demand-driven scenario, decisionmakers realize that they do not understand an issue on the policy agenda well enough to act effectively. They then request scholarly help: for example, from a university academic, a think tank, or from scholars serving in government positions. Consequently, useful knowledge that is not yet available becomes so following governmental demand. Alternatively, an analysis focused on the problem might originate from the scholarly community itself, independent of any explicit governmental commission, in response to a need to better understand an issue, and it might reach policymakers by various, often circuitous, paths.

While the demand-driven scenario provides direct access to policymaking, and generally involves responses to significant problems, the supply-driven model's virtue is that it expands scholarship's role beyond one that is merely reactive, to include shaping the policy agenda and anticipating problems. It can help to frame a problem as well as its solution, by encouraging new ways of thinking about existing issues. Moreover, scholars need not accept officials' values or their conception of ends-means relationships in order to make such a contribution. A key disadvantage with the supply-driven model is that the knowledge needed to inform policy may not exist; in the demand-driven scenario, by contrast, useful knowledge is explicitly brought forth.

In practice, elements of both models are often present. For example, an early wave of research—on, say, the interdemocratic peace—may stimulate official interest in associated scholarship. The academic reward structure notwithstanding, that interest may make the problem attractive to other scholars, who see its applicability to current policy issues as one reason to refine, critique, or replicate the early findings. Alternatively, at times when scholars are working on a problem for their own reasons, policymakers may seek additional or differently focused academic work within the same broad area. Senior decisionmakers may be more open to outside academic input at some times rather than others, and once a policy has been established, officials may be loath to reconsider it. But if an existing strategy is rendered obsolete by events, if senior officials disagree about some issue, or if political circumstances no longer favor a prior policy objective, scholars with something significant to say may be able to shape the terms of the policy debate.[1]

Significantly, relevant knowledge does not originate in a single institutional setting; rather, it is produced within four contexts of scholarly activity, distinguished largely by the extent to which they focus on generalizations or on concrete information. The four settings are those associated with General Theory (Group I), Empirically Focused Theoretical Analysis (Group II), Case-Specific Analysis (Group III), and Direct Policy Analysis and Advice (Group IV). Group I is furthest from concrete policy issues, Group IV is closest. There is thus a wide range of settings within which thinking on international relations is conducted, and members of the four groups interact more than is typically assumed. Their activities are supported by institutions and professional networks, including think tanks, foundations, and academic associations, that effectively create a transmission belt running from "pure" theory to "pure" policymaking and advice. In any case, relevant knowledge typically traces multiple, indirect, and sometimes discontinuous paths, and its impact on policy may have little to do with the purpose for which it was first produced. If policy-relevant IR knowledge exists, people who want to use it can do so.

The book's fifth major argument refutes a common misperception within academia—that relevant knowledge implies weak scholarship. This assertion cannot survive close scrutiny. As Abraham Kaplan pointed out, any theoretical argument concerning variables that *could* be policy-relevant *must* have real-world applications; otherwise, it becomes meaningless to claim that the argument explains much of anything.[2] If this is correct, IR theoretical knowledge must be at least potentially useful to foreign-policymakers, and the better the theory *qua theory*, the more useful it should be. After all, policymakers just as much as theorists need a sound and significant causal understanding of the world in order to do their jobs effectively. Sound theories, as we discussed in chapter 4, build on premises that are true, and omit no general proposition needed to address the phenomenon at hand. Valuable theories correctly explain a wide range of phenomena, and are significant in the sense of explaining important phenomena in ways that are not obvious. On its own terms, then, good theory provides a logically compelling account of a wide range of important phenomena in ways that add to our understanding of the real world. There is no reason why arguments that meet these criteria would *not* be more useful to foreign-policymakers than arguments that do not.

Consequently, it is wrong to assume that scholars would compromise their intellectual integrity by being useful. One can take cues from the world of practice without taking them from particular practitioners.[3] In any case, when scholars analyze whether, or when, a given policy would work, and with what direct and indirect effects, they can affect policy without necessarily accepting the particular ends-means connections that enjoy official favor at a given time.

From the perspective of SIR, a concern with policy relevance should help steer scholarship away from triviality, and keep the field's principal concepts tied to clear empirical referents. Until a few decades ago, when the incentive structure of modern university life made policy-relevant scholarship unfashionable, few would have argued that academics and practitioners do not have important common objectives. That commonality should be reexamined at a time when foreign-policy officials, navigating a sea of global uncertainty, need reliable analytic charts. We have found examples of valuable, *relevant* theoretical SIR to suggest that efforts to use it are worthwhile. Apart from what theories of the interdemocratic peace and of international institutions may contribute to policy within the subject matter they cover, they have broader uses as well. They demonstrate that scholarship can shed light on the range of the possible and on the consequences of various courses of action, and that it can do so with no costs to the quality of its work. Policy-relevant scholarship need be neither better nor worse than nonrelevant work (although it *may* be better); but it certainly stands to be more useful. Accordingly, we suggest some ways in which policymakers and IR theorists can benefit more from each others' insights and expertise.

Suggestions for Bridging the Gap

Realistically speaking there are substantial impediments to a broader use of relevant knowledge on the part of foreign policy makers. Government officials are far from convinced that scholarship might help them. Often they are too busy to do the priority official reading on their desks; the suggestion that they invest in what seems to be peripheral material, often written in arcane language, may be dismissed. Frequently, part of the problem is that the scholarship is not conclusive enough to be taken very seriously. Little social science is as authoritative as the best work in natural science: measurement is often too indirect, axiomatic postulates are rarely uncontested, and many substantive conclusions are submerged in caveats. On top of this, policymakers tend to frame questions differently than academics, they face unforgiving deadlines for answers, and typically they need crisper guidelines than scholars can provide.

For their part, many IR academics are quite comfortable with the gap. Some of their reasons are understandable, others are less defensible. On the one hand, scholars often are as happy to establish what we do not know as to push knowledge forward. Researchers cannot (at least should not) claim more authoritativeness than their research design or subject matter permits, and negative conclusions can more affirmatively be stated than affirmative conclusions. On the other hand, a purpose of scholarly research is to stimulate one's curiosity about a phenomenon. The result may be work that raises more questions than it answers. The fact that it is of little help to officials when quick action is needed need not be academia's main concern. All this is perfectly understandable and acceptable. Quite unjustified, however, is the fashionable conviction that relevant work reflects compromised academic aspirations. There also is little to support that all-too-common attitude among academics that they are more intelligent or thoughtful than government officials. It is easy to criticize official mistakes and misconceptions; it is more challenging to indicate how one would have acted differently under existing time pressures, political as well as international constraints, and informational constraints.

None of these obstacles to policy-relevant knowledge can be easily surmounted. Still, the gap is not yet a chasm. Some international relations scholars might reevaluate the benefits of producing useful knowledge—and some policymakers might then use more of it—if the communications barriers between the two groups were lowered and if the fruits of their interaction loomed larger. Practical suggestions for narrowing the gap should focus on these objectives.

One idea implied by our analysis is to build on the bridging role of Group III analysts: those who focus on specific IR cases, such as decisionmaking in particular crises, examples of multilateral bargaining, or instances of humanitarian intervention in civil conflicts. Scholars of this sort can play a pivotal part in connecting insights of Group I and Group II work to the concerns of policymakers. The link between Group II and Group III analysts is especially important in this regard. Group III work can benefit from Group II's efforts to provide empirical referents and findings that flesh out more abstract ideas. Correspondingly, Group II work may benefit from Group III's case-specific analyses, since these are often deeply grounded in practically useful examples. In sum, Group III scholars help transmit abstract yet potentially useful ideas and arguments to policymakers through in-depth empirical analyses that are often framed around theory-driven ideas. Because policymakers have neither the time nor expertise to probe the logical or evidentiary basis of theory-driven work, Group III analysts are especially valuable go-betweens for this purpose. And because many Group III analysts have strong ties to policy institutes, this bridging role makes them key suppliers of policy-relevant knowledge to interest groups, bureaucratic factions within government and IGOs, and other policy entrepreneurs—all of whom might wish to use or disseminate practical knowledge.

We suggested earlier that Group III analysts will probably play a larger role in providing and publicizing foreign-policy analysis as time goes on. In the United States, congressional interest in foreign policy is likely to remain low for the foreseeable future. Because their analysts typically have a good sense of the broader political context as well as their specific areas of expertise, international-affairs think tanks are well-positioned to shape the foreign-affairs agenda. At least some government officials recognize the value of generalizing about the effectiveness and appropriateness of various foreign-policy tools, although it is hard to judge how widespread this attitude is.[4] By synthesizing appropriate cases and general IR propositions in an accessible way, Group III analysts are well-suited to exploit this opportunity.

General IR propositions would "travel" better to policymakers if the contingent nature of causal claims were more explicitly specified. As discussed in chapter 3, many Group II arguments that are in fact highly conditional are presented by SIR literature in absolute terms; the contextual conditions that affect relationships among the variables are left unidentified. If these arguments were specified more carefully, it would be easier to connect empirically focused theoretical research (the product of Group II) to the in-depth analysis of real-world cases (the product of Group III). One way to incorporate ceteris paribus and contextual conditions more explicitly into the contingent generalizations that dominate Group II work is by analyzing typologies. A "type" is a group of cases—for example, wars produced by actors' misperceptions, or weapons-procurement decisions driven by similar sorts of domestic political pressures—in which the values of the variables are strongly associated. A typology, or set of such similarly grouped cases, rests on the assumption that the relevant variables occur together in fairly few combinations.[5] Typological analysis can help clarify the defining features of the research puzzles that dominate Group II work, both for those who focus on policy applications and those whose main interest is in solving the intellectual problems for their own sake. Typologies also provide a clear and accessible manner of communicating relational knowledge to those who are not scholars. This would not only help Group III analysts—and by extension, those policymakers who follow Group III work in their areas of interest—identify the Group II work they can use; it might also stimulate conversations about the dimensions and causal processes of international politics that transcend particular research puzzles.

Ultimately, however, the Group II–III connection can only be strengthened if policy-relevant SIR work becomes better appreciated within the academic community. A major and difficult task is to challenge an academic reward structure that penalizes relevance and celebrates technique. Left unchecked, such values make policy-*ir*relevance a self-fulfilling prophecy. Vicious circles get broken only when incentives outside the closed loop penetrate the processes inside it. In this regard, two developments may portend some change. Some of the constituencies behind public universities have come to demand that curricula reflect relevance. At their most thoughtful and compelling, such arguments insist that university courses acquaint students with the implications of what they are learning for their own lives, or for the society of which they are a part. While these demands are intermittent and often not well articulated, motivated university provosts, deans, and department chairs could incorporate relevance into the criteria by which hiring and promotion decisions are made. Insofar as doctoral programs in political science are attentive to the market for their product, such behavior on the part of university leaders could foster more respect (or at least tolerance) for IR research that suggests or demonstrates applications to real-world issues. Equally promising in this regard is a standard employed by the National Science Foundation in assessing project proposals for funding. The primary criterion is scientific merit, as judged by the panels of outside referees. But a project's potential applied consequences is also supposed to be considered (along with the qualifications of the researcher[s] and adequate support from available university facilities).

Professors might become more sympathetic to relevant knowledge in other ways as well. Editors at scholarly journals and university presses could ask authors to discuss what difference their findings and conclusions *might* make for policy issues. Any type of relevance would be appropriate in this regard—either heightened instrumental knowledge, contextual knowledge, or a better understanding of a policy's costs or other consequences. Obviously, not all scholarly output would have to be practically relevant, for reasons we have discussed. But if attentiveness to policy implications were an integral aspect of much published SIR, and if the cogency of such work were a measure by which scholarship was evaluated within academia, policy-relevance might become a criterion used in choosing their research problems. Further down the road, one can imagine scholarly debates and research agendas in international relations turning in part on the logical strength and evidentiary fit between the various arguments and their practical policy implications.

Even if relevant knowledge was provided, would it shape the calculations of policymakers? For this to happen, scholars should also reduce their jargon to the minimum needed to convey scientific information, elucidating substantive results as clearly as possible. Since policymakers can do their jobs, adequately in their view, without academic input, they tend to be impatient with the scholarly apparatus that accompanies scholarly conclusions. Even if they value the results, few are curious in any detail about how the conclusions were derived, and even fewer care what the knowledge implies for the scholarly field. Accordingly, scholars must learn to frame their work in ways that are meaningful both to their colleagues and to practitioners.

Taking such suggestions to heart would help revive the tradition of "public intellectuals" in this field. At their best, public intellectuals are people who speak astutely about public affairs *from a perspective honed by cogent theoretical analysis and a thoughtful immersion in substantive problems*. Neither by itself suffices to make an impact. Without a coherent theoretical foundation for their recommendations, outside analysts have little insight to offer practitioners beyond what they already know. Without carefully *connecting* their theoretical insights to important substantive problems over which decisionmakers have leverage, such intellectuals have nothing important to say about the real world. The title of "professor" by itself does not really validate such analyses, either outside the academy or within it; it is the ability to *connect* appropriate generalizations and initial conditions in a cogent way that matters.

Assume for the moment that scholars were to act on these suggestions and policy-relevant work became more prestigious and common within universities. Members of the policy community who want to use such research might still want some guidance in finding it. Two suggestions come to mind here. First, editors of policy journals might ask their authors to make explicit the intellectual basis of their recommendations, along with the most formidable opposing arguments. Bearing in mind that policy debates often do not turn on the logic of the arguments, this kind of presentation would at least summarize the analytic side of such debates from a practitioner's point of view. That summary could then be compared with the academic discussion on the comparable issues. If it turned out that academics were framing the issue in similar terms—differences in professional jargon aside—policymakers could see if the SIR discussion provided any new empirical generalizations or reasoning that might be of practical use. If the scholarly discussion was quite different, thoughtful practitioners might find new ideas, scenarios, or evidence to consider, even if this input was ultimately rejected.

For reasons discussed in chapter 1, no one has had very powerful incentives to build bridges across these literatures. Consequently, the discussion of globalization in *Foreign Policy* proceeds very separately from the one in *International Organization*, and discussions of weapons proliferation in *Sur-*

vival seldom refer to work on the same topic in *Security Studies*. Differences in jargon aside again, it is hard to imagine that the underlying analytic issues could be that different on the same subjects. The main difference should reside in the ratio of general statements to statements about initial conditions, and the degree to which each is discussed explicitly or in depth. If so, at least some readers of each type of journal would probably benefit from gaining the other kind of knowledge, if only to make their own arguments more effectively, and might do so more readily if that became more convenient. At the least, policy specialists would have an easier way to find relevant Group II (and, less often Group I) work on the issues they cared about, and journal editors might find the implicit exchange of views would broaden their readerships.

Our second suggestion is that more informal dialogue between theorists and practitioners be encouraged, especially in instances where the people on each side have interests in common. The most productive exchanges might take place between professors who had spent some time in government and government officials who had some academic training in social-scientific IR. The premise here is similar to that in the first suggestion. Stripped of the pressures of speaking formally to different audiences, the two groups might find that they thought more alike than differently, at least about diagnosing situations, speculating about causal relationships, and assessing prominent cases. Our hunch is that the major grant-making foundations interested in international affairs would welcome a proposal for this kind of interchange, perhaps structured around clusters of issues that might be expected to provoke reactions from both groups.

Two types of research projects might also help bridge the theory-practice gap. One would involve detailed case studies of past policy deliberations to determine what type of analysis was used (or might have been useful, if it had been available) at various points and whether good theory might have met the need for instrumental, contextual, or cost-related knowledge. For example, in diagnosing Mikhail Gorbachev's objectives and strategies in the mid-1980s, one could reconstruct how the United States tried to determine the range of possibilities for dealing with an unorthodox type of Soviet leader. What kinds of analogies and inferences did they use to make judgments? Did appropriate SIR knowledge exist at the time that might have sharpened their inferences or caused them to ask different questions? If so, would it have suggested other, less obvious policy options than the ones employed? More ambitiously, practitioners might be brought into a collaborative project with scholars to simulate various kinds of decision situations, and perhaps reconsider some actual decisions. Here, the proximate consumers of theory-driven policy recommendations would be asked explicitly what they need or would have needed analytically in order to make good decisions and how they would use that knowledge if it were available. The scholars might be asked to respond by critiquing their own product from this perspective, and then suggesting how good theoretical arguments might do better at satisfying these needs *without compromising intellectual quality*.

Final Thoughts

Representative democracies delegate the responsibility for formulating and conducting foreign policy to elected officials and their subordinates. Those officials typically know the issue-specific facts better than almost any outside observer—something Lyndon Johnson and Robert McNamara seldom hesitated to remind U.S. critics of the Vietnam War. We know better than our critics, they said, because we have the relevant data and they do not. Even so, the architects of that war did not understand the links between the sorts of aims and means involved, nor how to decide whether the war should have been fought and, if so, how the intervention might have produced the desired conclusion. That kind of knowledge is certainly not the exclusive property of scholars, but they are often better-suited to use and certainly to produce those generalizations than are policymakers.

It is not hard to think of prominent cases that *might* have worked out differently if political leaders and policymakers had possessed appropriate knowledge. The U.S. involvement in Vietnam, as is often noted, reflected flawed instrumental knowledge—bearing on the utility of force in the cultural and geopolitical context of Vietnam. It also reflected flawed contextual knowledge, involving, for example, the U.S. public's tolerance for a painful war of attrition. Similarly, as Bruce Jentleson argues, the U.S. policy to appease Saddam Hussein in the decade before he attempted to conquer Kuwait reflected a poor understanding of the conditions under which concessions can produce mutually satisfactory policy cooperation.[6]

Perhaps no conceivable scholarship would have affected official thinking in a way that would have prevented these failures. But this does not relieve either decisionmakers or scholars of their obligation to use and produce knowledge that can make a difference. The suggestions in this chapter might be seen as a way to produce the kind of theory-policy dialogue that makes such fiascoes less likely.

The Ivory Tower exists for a good reason: we expect university-based intellectuals to reflect on the world at some distance, and not simply to do the work of policy commentators or journalists at a slower pace. But in our view, the separation from the world of decisions and consequences has gone too far in international relations. It is odd to think that no practical implications should follow from a better understanding of the world. If scholars address important, real-world issues, they will more often than not improve their own work and have more to share with those who must act.