

Counselors, Kings, and International Relations:

*From Revelation to Reason,
and Still No Policy–Relevant
Theory*

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“The inseparable conjunction of counsel with kings” predates the rise of the analytic mind (Francis Bacon, quoted in Goldhamer 1978, 7). Even when political leaders believed in “divination, oracles, avenging deities, and other manifestations of the supernatural,” they looked to masters in rites and rituals to advise them on affairs of state (Goldhamer 1978, 230). As reason replaced revelation as the basis for decision, kings turned to counselors with new bases of expertise, drawn from different ranks within society.¹

In the modern world, leaders constantly seek knowledge and expertise even as they and others belittle the quality and relevance of the scholarly product. They lament that “those who can’t, teach,” to deride and discredit both the concrete policy recommendations of academics and the abstract scholarship produced in much of the academy.² Yet in many areas, especially technical ones, the importance of policy expertise, and with it professional policy training, has grown. During the twentieth century, the social sciences solidified their intellectual autonomy and developed professional degrees and programs for the education of practitioners. In international relations, this meant the development of professional training programs for the foreign service and related careers.³

As research has become more important to policy formulation, academics have become important policy players. Scholars in some disciplines advise government officials; in some fields they can move smoothly between government and the academy. The Council of Economic Advisors, for example, institutionalizes the policy relevance of economics and is statutorily presumed to provide a governmental platform for academic economists.⁴ There is nothing comparable for scholars of international relations, although academics have become more prominent in the ranks

of foreign policy advisers and have largely replaced lawyers as the profession from which such advisers are drawn.

In this essay, I argue that international relations theorists, despite their obvious interest in current events and relevance, and despite the importance of the knowledge they possess, do not directly address the needs of policymakers. I locate the problem in the nature of the field its current level of development, and the nature of policy relevance.

There Should be Policy-Relevant Theory

There is every reason to expect the field of international relations to be policy-relevant. It is not plagued by the kinds of problems that make scholarship irrelevant. It does not separate policy and theory institutionally and so force ideas to move so slowly from one isolated scholarly community to another that theoretical scholarship remains unknown to policymakers. It does not focus solely on theoretically generated questions. Its intellectual agenda, unlike those in many disciplines, is driven as much by questions of immediate policy concern as by issues that emerge purely from the intellectual evolution of a scholarly paradigm, the need to develop a general perspective and address anomalies unexplained by current theory.

Field Organization and Policy Relevance

Professionalization has typically meant separate training programs for practitioners and theoreticians, but in international relations the two remain in the same tent. Unlike basic scientists and engineers, or psychoanalysts and clinical psychologists, they generally share a single institutionalized path for training, research, and teaching. The economists who teach in economics departments often bear little resemblance to those who teach in business schools, and the training of graduate students in economics is quite different than that of MBAs.⁵ But in political science and international relations, the division between theory and application is minimal. Although graduate professional programs in public administration and international relations exist, they are few in number and count predominantly political scientists among their faculties. Many scholars who study mainly policy and address primarily policy audiences teach in mainstream political science departments. Moreover, even though there are no policy positions in foreign affairs that require political scientists, those who have held such posts have sometimes been eminent ones (Kissinger, Brzezinski).

Focus on Real-World Events

International relations should also be policy-relevant because its students are interested in, if not preoccupied by, the real world – perhaps too preoccupied for the field's own good. To develop as a science, natural or social, inquiry must focus on analytic rather than practical questions. It must focus on theory development and assessment and their attendant requirements of abstraction and methodology. The scientist's audience is other scientists working on similar questions. That is how science evolves. Whatever its speed, intellectual progress proceeds linearly. Eventually, cumulative knowledge about how some facet of the world works may make policy intervention possible, but that is a by-product, one often deferred for a long time and not readily predictable from initial lines of inquiry. The distinguished social scientist Donald Campbell made the point directly by holding that social scientists should avoid reading the daily newspaper. It was his way of stressing that social scientists should look primarily toward the theoretical development of their respective fields. Their task, like every scholar's, is to push back the frontiers of knowledge. Although that knowledge is about the world, a focus on daily events would only deflect them from their core activity.

But international relations theorists in the twentieth century have been overwhelmingly preoccupied with the real world, often shifting the focus of their scholarly inquiry as a function of current events. The world of the 1920s and early 1930s, with its emphasis on such international institutions as the League of Nations, led scholarly attention to international law and political economy, and to the league's specific attempts to use international agreements to constrain and even outlaw war. Within a few years, however, the overwhelming post-World War II desire to avoid nuclear war swept away these subfields.⁶

In the last half of the twentieth century, every important historical development has left a new literature in its wake, often one financed by government and private foundations. Not a few subfields have drowned in the process. The postwar fear of nuclear weapons generated deterrence theory.⁷ The creation of the United Nations led scholars to focus on international organizations. The Korean War spawned a literature on limited war. The creation of the European Economic Community generated a subfield on regional integration, and the Cuban missile crisis spawned an industry devoted to the study of crises. The war in Vietnam brought new interest both in how wars end and in their domestic consequences. In the 1970s, the combination of detente, oil crisis, and the collapse of Bretton Woods led to the brief demise of security studies and the birth of international political economy. At the same time, the fail-

ure of the United Nations and the seeming end of integration in Europe led to the disappearance of regional integration studies and the virtual disappearance of work on international organizations. The collapse of the oil market, U.S. rearmament, and heightened fears of war led to a brief eclipse of international political economy and an equally brief renaissance of security studies in the middle 1980s. But the end of the Cold War created tough times for security analysts, as ethnic conflict and terrorism have become the prime foci of the early 1990s. Renewed momentum toward European integration and the seemingly renewed relevance of the UN simultaneously led to the reemergence of the moribund subfields of international organization and regional integration.

Indeed, international relations as a field is so responsive to new real-world developments that its very core remains in flux. Extended great-power trade and cooperation brought arguments that the increased costs of great-power wars may have caused their extinction. That there has actually been no such war in the half-century after World War II generated debate about whether there has been a fundamental change in international relations (Mueller 1989; Kaysen 1990). The emergence of the multinational corporation led to discussions in the early 1970s of diminished sovereignty and to predictions that transnational relations would replace international ones.⁸

Ironically, scholars may have contributed to the field's lack of cumulation and intellectual progress by responding to every twist and turn on the international scene.⁹ The regular shifts in focus have contributed to the field's retardation rather than making it more useful to policymakers. Responding to real events is no guarantee of developing policy-relevant theory, and it risks creating the academic equivalent of generals who prepare to wage the last war. Indeed, the ability of scholars to be caught napping is best illustrated by the end of the Cold War and the collapse of bipolarity, which was widely believed to be stable and enduring.¹⁰

Yet faddism is an inadequate explanation for shifts in scholarly inquiry and the lack of progress. One root of the problem lies in the very nature of the phenomena that the field studies. Other areas of political science analyze department variables that recur and sustain continuous interest. Students of U.S. elections are certain to see another election. International relations scholars are not assured of much. A war between the great powers may be followed by a period of peace that is longer than a scholar's career. A superpower confrontation like the Cuban missile crisis may never recur. Critical phenomena do not occur with regularity and may not return with sufficient frequency to sustain interest and funding or provide sufficient data.¹¹ As a result, scholars respond to new historical developments.

Moreover, fads can arise even in fields with a consistent focus. That economists consistently focus on firm profits and market competition has not prevented business management's many gurus from preaching pseudoscience and creating or following crazes with pernicious consequences.¹²

Scholarly focus shifts in response to changing political developments for three reasons. First, many scholars who are genuinely interested in the real world want their work to address contemporaneous concerns. Second, government – a major source of demand for scholarly product – has interests of immediate policy relevance.¹³ Third, the private foundations that support scholarship often have agendas that shift with the times. In the wake of detente, for example, the key foundations supporting international relations scholarship abandoned certain lines of inquiry. The Ford Foundation, for example, decided it would no longer fund security studies. Ironically, the fear of Reagan's rearmament program in the early 1980s brought many foundations back to supporting work on national security and on the Soviet Union. Yet that interest cooled once more with the end of the Cold War. Whether because of supply (the natural interest of scholars) or demand (governments and foundations with money), international relations scholars focus on real-world concerns and should presumably be policy-relevant.

Despite the field's overwhelming concern with the real world and despite the absence of a rupture between theoreticians and engineers, international relations remains a field that most find has little to say to policymakers. It seemingly has all the ingredients for policy relevance. Its practitioners are interested in the real world and are encouraged and supported to study current events. Social engineers and theoreticians cohabit departments and schools. Yet international relations theory remains a disappointment to policymakers, and this is a source of frustration for scholars.

The Problem

That leaders have turned to advisers and intellectuals for millennia does not belie the problem. Nor does the advent of political science, which long postdates the existence of policy counselors. In whatever era, however, the question is whether advice is grounded in some body of expertise – whether it is rooted in tested and accepted knowledge-claims. Foreign policy, no less than medicine, must distinguish between the quack and the physician.

Whatever its weaknesses, the field of international relations can

have some policy relevance. All knowledge about how the world works is relevant.¹⁴ Any knowledge of means/ends relationships matters. Atheoretical work can be policy-relevant. Purely descriptive studies can be useful. Policymakers would have found it helpful, for example, to have a study showing that the Soviet Union was typically cautious in crises and withdrew at the first sign of serious opposition. Even if no theory explained the pattern, simply describing it might have been useful.¹⁵

Medicine illuminates the range of useful knowledge available to those who would act. Doctors can sometimes treat symptoms, such as pain, without treating or even understanding the etiology of a disease. They excise tumors, for example, without knowing what causes them or if the surgery will work. Doctors can also diagnose a disease and detail its likely prognosis without being able to intervene. Such prognosis even without control, help, or relief of underlying causes can still be useful. Yet, even when doctors know both the cause and the evolution of a disease, they cannot always control or reverse it – a case exemplified by the plaintive desire for a cure for the common cold.

International relations, like medicine, can be useful even when it cannot explain and cannot offer the hope of cure or control. Yet there remains a deeper problem.¹⁶ For a set of reasons developed below, the propositions developed in the field are not particularly relevant to the making of foreign policy. .

Researching the Exogenously Nonmanipulable

Although the most useful knowledge pertains to levers that are directly manipulated by policymakers, many international relations theories do not involve such manipulable exogenous factors. Recent scholarship on the implications of the end of the Cold War provides an example. One implication of realist arguments about the stability of bipolar worlds, for which the Cold War is an exemplar of stability assuring an extended peace between the superpowers, is that the end of the Cold War should bring conflict and instability. It should bring back the bad old days of European chaos, leaving us hankering for the good old Cold War.¹⁷ Despite this argument's importance, one can only imagine the Oval Office reaction to a briefing on how the multipolarity of the future will be more unstable than the bipolarity of the past. There is nothing that any chief executive of any nation can do to alter such historical developments. Nothing in the theory provides a way to make multipolarity more stable or to bring back bipolarity.

Knowing the relative stability of different systems is interesting and

may be useful to those seeking to alleviate systemic problems, but provides little guidance by itself. It is equivalent to the information offered by a weather forecaster, who need not know the etiology of weather patterns, who cannot control them, and whose predictions have substantial variance and little predictability. Although we cannot control the weather, we still find it useful to know that it might rain. But the utility of the weather forecast is conjoined with our having umbrellas and raincoats – protections not discovered, created, or provided by weather forecasters. International relations forecasts of increased instability under multipolarity are equivalent to imperfect weather forecasts without umbrellas or raincoats.

Work at the systemic level of analysis is of limited utility to policymakers, who necessarily work at the individual and national levels. Political

leaders interact with other leaders and develop policies to deal with other nations, whose actions and reactions they must assess. Systemic scholarship, however interesting, remains of limited utility. It deals at a level of aggregation not commensurate with that at which policy takes place, and it involves independent variables that are not manipulable policy levers. Systemic characteristics are a product of the actions and interactions of states – no one state's choices mold the system.¹⁸ Policymakers can create neither bipolarity nor multipolarity, because foreign policy operates at the individual and national levels. International relations theory deals with broad sweeping patterns; while such knowledge may be useful, it does not address the day-to-day largely tactical needs of policymakers.¹⁹

Not Researching the Levers of Policy

Not only have scholars analyzed much that is in no way manipulable by politicians, but they have often failed to study the implications of those levers actually available to elites. A simple catalog of policy measures makes the point.

The positive actions states take toward each other include recognition, economic and military aid, the extension of commitments, and a variety of other measures culminating in full alliances. The consequences of many of these are often unstudied. Then, too, those scrutinized are not always relevant or important to policy. We know little about the political consequences of one nation's recognizing or not recognizing another. Most studies of foreign aid deal with its determinants; few examine its consequences. Nations commonly sell weapons as a way of improving and strengthening bilateral relations, but little research has addressed the actual impact of these transactions. Even analyses of the

consequences of alliances and deterrence commitments have been limited by a primary focus on their relationship to war and successful deterrence. Although important, this does not tell policymakers all they need to know.

Governments signal displeasure with each other by withdrawing ambassadors and recognition, isolating other states diplomatically, displaying military force, and imposing economic sanctions, and by threatening, warning, and actually using force. Analysts have totally ignored some of these policy levers and have adequately studied only the impact of economic sanctions.

Most scholars focus on the extremes of the cooperation/conflict continuum. Since their interest lies in war and peace, they study war and alliances. But much of international relations occurs between the extremes; most policy choices do not involve declaring war or joining an alliance. The full range of policy instruments remains to be adequately conceptualized and studied.²⁰ There are different kinds of alliances, but we have no labels to distinguish them.²¹ Similarly, there is a need to distinguish along the conflict continuum, between enemies and rivals, for example. Neither scholars nor policymakers have an adequate vocabulary for discussing the range of relationships. Moreover, scholars have inadequately studied the ramifications of using the levers that do exist.

Academic Discourse and the Policy Problem

The problem of policy relevance is also a function of how the academy differs from the policy world; the very nature of the scholarly enterprise and its institutional organization impede policy relevance. Scholars focus narrowly, with the consequence that what a policymaker needs to know is to be found across disciplines and fields. Moreover, scholars focus on the general and generic, and policymakers are interested in the particular.

Because scholars focus on dependent variables, on explaining some phenomenon, they debate the relative importance of different explanations. As a result, literatures cluster around common dependent variables. The core debate in international relations about the causes of war involves disputes about the relative importance of different explanatory variables. This can be policy-relevant if policymakers want to achieve or avoid or change the dependent variable.

But because policy entails the choice of particular instruments, it requires knowing the consequences of specific policy choices, information that, even if available, will likely be found across many literatures.

The consequences of any policy X are not to be found in any literature focusing on X. Rather, they are found in the disparate literatures that each focus on one of X's particular consequences, whether Y or Z or something else. This is a general problem for any field's policy relevance: it cannot provide useful information about the consequences whose determinants are the focus of inquiry. Gathering information on wars' consequences is much harder than finding out about their origins, for studies of wars' effects are spread across entire disciplines, including a small body of work on the international systemic consequences of war. But ascertaining the impact of war requires assembling information from all the areas of research in which it appears as an independent variable (Stein 1980).

This problem is exacerbated when policymakers want to know about not only their envisioned objectives but the unintended consequences of their choices. The effect of war on domestic politics, which is of keen interest to politicians, is detailed in literatures focusing on specific aspects of domestic politics. War appears as an independent variable in a host of disciplines and literatures, including those on the growth of government, inflation, divorce, and social cohesion.²²

The organization of scholarly activity is problematic for policy relevance for still another reason, its focus on generic rather than particular knowledge. Science values general theory, which strives to replace proper names with analytic constructs (Przeworski and Teune 1970). Among the social sciences, which have not moved as far down this intellectual path as other sciences, economics has made the greatest advances in this direction. Economists do not teach courses on General Motors or Intel. They do not even teach courses on the automobile or computer industries or, for that matter, on the U.S. economy. Rather, they teach theories of the firm and of markets. Students analyze supply and demand rather than particular products or commodities. They are educated to apply what they learn in general courses on micro- and macroeconomics to specific information on their own, to discover the vagaries and particularities of soybeans or computer chips on the job. It is not surprising, therefore, that the business school curriculum only slightly overlaps that of graduate economics training. And it is not surprising that professional schools fill in the particulars through the use of case studies.

In political science, the movement toward general theory has not yet had the same curricular and intellectual impact. Along with courses on theory, most departments continue to teach U.S. foreign policy and the institutional politics of particular nations. Political scientists still use proper names. Nevertheless, the eclipse of area studies and the

emphasis on general theory has begun. Yet governments continue to be organized along area-study lines, and policymakers need particularistic information.

A poignant illustration of the problem occurred when the chief architect of a U.S. bombing campaign of North Vietnam asked his professional school mentor, the scholar who had taught him deterrence theory, for counsel on signaling American intentions, but the teacher could not help as his former student had hoped.²³ He needed too much specific knowledge about Vietnam to apply the general model. Ironically, critics of U.S. policy in Southeast Asia had decried the hawks' intellectual commitment to nonspecific propositions without any knowledge of Vietnamese history and culture. Applying general arguments about signaling and even about war's costs and benefits often require knowing how particular actors look at the world and how they assess their costs and benefits. Distressingly, the inability to ascertain the values that underlie other leaders' choices sometimes results in frustrated analysts and policymakers characterizing what they do not understand as crazy and irrational. Many U.S. scholars and political leaders labeled Saddam Hussein a nut when they could not reconstruct (or did not share) the value system in which his choices were perfectly rational and understandable. Marrying the general to the particular is necessary for true policy relevance, but the combination is rarely found in the academy.

The policy inadequacies of generic knowledge-claims is greater when international relations are actor-specific. In other fields, correlational analysis can be the basis for important public policy choices even absent precise causal links. The strong correlational link between smoking and lung cancer involved enough observations that the inference was strong enough to adopt a national health policy aimed at changing the public's attitude toward smoking even though tobacco companies could counter, as they did for many years, that the etiology of lung cancer remained a mystery and that correlation was not causation. That a substantial number of individuals who smoked heavily lived long lives without developing lung cancer provided adequate grounds not to act.

In contrast, the number of available observations of international relations is quite small, and correlations in the social sciences are much smaller.²⁴ Correlational knowledge that 9 percent of a large population will get some disease may be enough to warrant a policy intervention. But when one scholar found a .3 correlation between alliances and war (meaning that in his data 9 percent of wars could be explained by alliances), his initial conclusion, that NATO should be disbanded, was based on too little data and too weak a link to carry much policy weight.²⁵ There was no large population of alliances to strengthen the

basis for the inference, and the data could not be used to infer that 9 percent of all alliances end in war. Most problematic, however, was the use of aggregate correlational data to draw an inference about one particular entity. In a small N world, the ability to make policy prescriptions for particular actors from poor data is virtually nil.

In their world of general theory, social scientists do not often supply the particularistic information demanded by policymakers. Someone designing a public health policy does not care if a given individual will get lung cancer but about lung cancer rates for the entire population. In contrast, a policymaker devising a foreign policy cares very much about the likely responses of particular countries to particular circumstances. Generic patterns matter only if they give clues to specific situations. But an international relations theorist's generic policy observations are not specific to any actor, which is the interest of the policymaker. The proposition that states balance against the most immediate and proximate threats without regard to ideology would be the basis to conclude that the new Iranian government of 1979 would eventually turn to the United States as the only great power capable of balancing the clearly more direct threat of the Soviet Union. Such analysts waited in vain for Iran to turn to the United States in the 1980s. Similarly, the commonly and strongly supported proposition that leaders who lose wars get replaced provides little comfort to U.S. policymakers still dealing with Saddam Hussein as president of Iraq at the end of a decade that began with the defeat inflicted upon Iraq by the United States. Generic probabilistic knowledge-claims are less useful than particularistic knowledge. A regional specialist's knowledge of Iranian mullahs or Iraqi politics would likely have been more accurate and more useful to U.S. policymakers, even if that knowledge was neither theoretically well grounded nor generalizable. In many areas, in-depth experiential knowledge dominates general theorizing and statistical generalizations for the formation of policy.

High-Stakes Public Policy and Social Science

International policy choices can have great consequences, ones that may be neither limited nor reversible. Bad foreign policy advice can result in otherwise avoidable wars. This makes the burden for both counselors and advisees a heavy one. They must weigh recommendations carefully. Even broad consensus does not exempt prescriptions from careful scrutiny and possible rejection.

Scholarship develops and cumulates over time. One era's truth is revised and modified, if not turned on its head.²⁶ The character of the

scientific revolutions in the development of the hard sciences should give scholars pause before they rush to make policy pronouncements.

Over a substantial period, a general scholarly consensus on what I call *nuclear stability theory* drew from both historical experience and theoretical models of deterrence to conclude that nuclear weapons stabilized world politics. Mutual assured destruction, in which both the United States and the USSR could withstand an initial strike and still hold sufficient weapons for retaliation, provided a robust and stable deterrence. Yet only a brave few have been willing to venture forth on this foundation to argue that nuclear proliferation might stabilize world politics, should not be opposed, and should perhaps be encouraged. A bolder position, though unvoiced, might be that the superpowers should manage proliferation and provide sufficient survivable forces to new nuclear powers to assure stability and avoid the unstable transition period to mutual assured destruction, the time during which not all nations in a region have yet developed sufficient survivable nuclear capability and those with small vulnerable forces have strong incentives for first-strike preemption.

Few are prepared to extol the virtues of nuclear proliferation, and no one is ready to recommend that great powers encourage and underwrite the process. Policymakers would be appalled, but it is not the expected political response alone that leads scholars to shrink from the consequences of their own work. In 1964, Karl Deutsch and J. David Singer developed their famous argument that multipolarity is more stable than bipolarity. Their case was coherent, logical, and sustained by multiple arguments. But they shied away from the obvious policy implication of their own work and ended with an almost comical retreat from the intellectual ground they had so elegantly staked. Almost three decades later, John Mearsheimer drew on arguments about the stability of the bipolar nuclear world to conclude that analysts would soon look back nostalgically to the stability of the Cold War. Clearly, he argued, the multipolar future would be less stable. But since nuclear weapons had been a stabilizing force in themselves, he pointed out, the world would get a chance to find out whether the stabilizing consequences of nuclear weapons or the destabilizing consequences of multipolarity were more important. Mearsheimer predicted nuclear proliferation and recognized its generally stabilizing impact, yet pulled back from recommending proliferation. Indeed, he shifted the analytic focus of his argument to conclude with a set of ad hoc observations about the undesirability of nuclear weapons in the hands of unstable regimes.²⁷

Nuclear stability theory is an example of a long-term scholarly consensus that had no impact on policy.²⁸ Scholars themselves drew back

from the obvious policy implications of their work. They went through extensive mental contortions to sustain a belief in the undesirability of nuclear proliferation. This may have reflected their underlying values, an unwillingness to oppose government policy, or a fear of the consequences of being wrong. Ironically, the first scholarly and analytic critique of *proliferation stability theory* came after more than two decades of a policy-irrelevant consensus. Only in the post-Cold War world did there develop a legitimate intellectual debate in which both sides drew on powerful analytic arguments to sustain their respective views on nuclear proliferation. Such a shift in scholarly outlook is one reason not to accept even those policy prescriptions with a broad scholarly consensus behind them.²⁹

Politics and Policy

Foreign policy, like other public policies, entails choosing ends as well as means, yet science, which analyzes means/ends relationships, has little to say about the latter.³⁰ Social scientists making policy pronouncements have at best the expertise to address means/ends relationships. When they discuss desirable objectives, their opinions should carry no more weight than that of any other voter. How, then, are scientists to separate their own policy preferences from their policy prescriptions?³¹ It is no wonder that social scientists (who mask their policy preferences in expertise) and policymakers (who worry as much about the political as the policy risks of their programs) so suspect one another.³²

U.S. security policy during the Cold War rested on the country's nuclear weapons, and the continued development of that arsenal and the desirability of negotiated nuclear agreements both depended critically on scientific advice. Yet despite the relatively well developed body of accepted knowledge claims in physics, the relationship between scientists and government was amazingly conflictual:

At the root of the progressive decline of the president's science advisory apparatus has been a progressive loss of faith in the process by both sides: by scientists in the ability of the government to make proper choices based upon the evidence, rather than partisan pressures; and by presidents in the capacity of scientists to give objective advice, uncolored by personal views. (Herken 1992, 217)

Physicists were disappointed when their views were ignored, but policymakers often found the scientists' views a reflection less of science than of political belief. Yet throughout the Cold War, presidents have had to rely on the technical knowledge of the scientific community even as the

problematic relationship between scientists and foreign policy affected scientific advice on national security issues.³³ During the late 1930s, physicists saw their sole advisory problem as an inability to get their views about the implications of scientific developments to the president of the United States. Beginning with the National Defense Research Council's creation as the United States neared entry into World War II, a variety of institutions were established to bring scientific advice to the president.³⁴ The President's Science Advisory Council, born in 1957, set up the first direct channel between scientists and the White House, although it and the post of science adviser were abolished in 1973.

Like scientific counsel, foreign policy advice is encumbered by scholars' implicit preferences and their disdain, nonetheless, for the political. The theorists' policy irrelevance is magnified by their paucity of empirically verified knowledge-claims and the uncertainty of and variance in their findings. Like physicists, political scientists have strong political beliefs and constitute a comparable aviary of hawks and doves. As noted above, their views can even keep them from recommending policies that flow logically from their scholarship. And they, too, proffer their recommendations with a concern for policy rather than political optimality. In fact, they often decry the impact of politics upon policy.³⁵

Process-Oriented Prescription

Much of the important policy-relevant work in the field has focused not on substance but on process – on the *how* rather than the *what* of foreign policy.³⁶ Its aim, to improve the quality of decision making by improving the way decisions are made, reflects the more general intellectual developments of the last centuries. Across an array of disciplines, scholars have focused on improving the quality (read rationality) of human decision making. Early developers of probability theory hoped to improve their gambling fortunes. Logicians wanted to help people reason better. Decision theory, in all its manifestations across disciplines, is one of the great achievements of the last century and a half (Nozick 1993). As developed in philosophy and economics, this typically formal work tells reasoners what to do rather than what to avoid; it stresses how to be rational. Individuals, presumed to be self-interested, can learn to apply the tools of decision making to improve their outcomes.

Such studies of decision making, built on the presumption that individuals are crippled decision makers, have been largely normative, with an emphasis on increasing their rationality. Decision theory can, therefore, be seen as a tool. Just as the telescope and microscope allow people to see things beyond their normal eyesight (the simple observational

mechanism they are stuck with), such tools as probability theory, logic, decision theory, and game theory can improve the choices that individuals would otherwise make.³⁷

The self-conscious use of decision theory by self-interested individuals becomes *self-validating*.³⁸ Once normative tools are used self-consciously to make choices and guide behavior, they can be employed as positive explanations by scholars interested in explaining behavior. Models of choice that began as normative guides to improve behavior become positive explanations for it.

In contrast, psychological studies of decision making have analyzed how individuals failed to match the requirements of the rational actor model. Cognitive psychologists have shown how people misassess probability and how decision is affected by the ways in which choices are framed. They have also demonstrated that biological (hardwired) inference shortcuts often lead to decisions that violate statistical inference and how crisis and stress reduce the effectiveness of individual and group decision making.

The normative implication of psychological explanations of actual decision-making processes is that an awareness of these psychological pressures and forces can make individuals better decision makers. In fact, such a normative argument permeates this work. Through role-playing and crisis simulation, people can learn how to fight groupthink and premature closure.

Psychological studies of actual decision making are, therefore, *self-falsifying* once known. They presume that self-awareness vitiates the implications of psychology, that knowledge will lead people to avoid pitfalls they might otherwise fall into.³⁹ The normative implication of positive knowledge of how people make decisions is that people will no longer make decisions in those ways once they become self-aware.⁴⁰

Scholarship on foreign policy process has taken two tracks. One is directed at optimality; it is *formal, normative, and self-validating*. Awareness of these arguments leads people to use them, and so the normative becomes the positive. A second track focuses on the constraints and limitations to rationality in foreign policy decision making. This path is empirical and positive, but its knowledge claims are *self-falsifying*. Awareness of the ways in which individuals actually make decisions leads them to avoid pitfalls and do better.

The Limits of Policy Relevance

In addition to the policy inapplicability of extant theory, formal work on the logic of decision making tells us that there are limits to the policy

relevance of theory. The tools of strategic choice (commonly known as game theory) seem to provide the optimal policy tool for decision makers. The models incorporate the preferences of those who need to make a choice with whatever knowledge is available about the preferences of others. Foreign policy is, after all, inherently strategic. Leaders are making choices on behalf of states in a context of interaction with other states. Such models, then, are perfectly suited for use by both decision makers and analysts interested in explanation. That such tools were created with a normative intent, as tools to improve decision making, only reinforces their prospective decision-making utility. Ironically, developments in the field of strategic choice suggest the limitations of the models themselves. These results suggest that the informational requirements for applying theories of choice are immense and unlikely to be met in any real-world situations. Moreover, the models are plagued with incompleteness and indeterminacy.

In cases where a government needs to know the choices that another government will make, for example, the literature on social choice provides an indication of both what information is required and the limits of what we can infer from that knowledge when that other state is not a unitary actor. The other state may consist of different branches of government, different political factions, different governmental agencies, or even a number of high-level officials who will jointly make decisions. Such cases can be modeled, but they very quickly reach the limits of the tractable and knowable. Formal results, such as Arrow's impossibility theorem, establish that even a knowledge of the preferences and the rules of decision making may not be enough to determine the social choice without a more exact specification of the process. When dealing with a nonunitary actor, the aggregation problems associated with arriving at a collective choice bedevil attempts to assess the basis of others' choices and the nature of their likely reactions. The implications of Arrow's impossibility theorem led one scholar to conclude that international relations theorists had no choice but to treat states as unitary integrated actors, as if they were a single person with a utility function (Bueno de Mesquita 1981, 11–18).

Finessing the aggregation problem by treating states as unitary actors only simplifies the analytic problem somewhat. Applying strategic interaction models in such cases still requires a knowledge of the other actors' complete options and their preferences. Knowing their preferences is an exceptionally strong requirement that extends beyond the typically discussed notion of knowing the intentions of other states.⁴¹ One needs to know how others evaluate the full range of outcomes, including ones that will never be reached. The existence of incomplete

information typically generates situations with multiple equilibria and takes the analysis into a realm in which there are competing equilibrium concepts. In such cases, strategic interaction models provide no further clue for making decisions – they are incomplete and indeterminate.⁴²

Formal work in decision theory, and in social and strategic choice, delineates both the informational requirements for decision making and the limitations of advice given the available information. It tells us that even as we accumulate knowledge about how the world works, there are inherent limits to our ability to generate unique optimal policy prescriptions.⁴³

Conclusion

Policymakers will always turn to advisers and new ideas. Public policy is purposeful behavior sustained by some objective and some sense of appropriate means. In that sense, it is always rooted in ideas. As John Maynard Keynes put it:

The ideas of economists and political philosophers, both when they are right and when they are wrong, are more powerful than is commonly understood. Indeed the world is ruled by little else. Practical men, who believe themselves to be quite exempt from any intellectual influence, are usually the slaves of some defunct economist. Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribblers of a few years back. I am sure that the power of vested interests is vastly exaggerated compared with the gradual encroachment of ideas. (1936, 383)

If ideas are indeed the cornerstone of policy, then knowledge rooted in theory and empirical observation is better than a hunch without any foundation in theory or empirical regularity, even though well-grounded choices may still involve mistakes. It is better than studying entrails or turning to the stars. Policy errors arise even in fields far more scientific than international relations, those with better and more plentiful data and with possibilities for experimentation. Policy decisions about medical treatments cannot always be made from animal models and experiments. Additional data sometimes leads to complete reversals in medical recommendations. The medicine practiced in earlier centuries was often wrongheaded and caused mischief and tragedy. But even though it consisted of knowledge-claims rooted in an analytic logic and subject to empirical falsification, it took generations before

individuals going to a physician for help could be certain of receiving treatment that was more beneficial than harmful.

Closer to home, the field of economics has clearly grown in its understanding of the way the economic world works. Harry Truman, who regularly heard economists demur from offering clear-cut advice by noting “on the one hand,” and “on the other hand,” said he wanted “a one-handed economist.” Some are now prepared to say that we have finally entered the world of that one-handed economist (Mueller 2000; but see Lal 2000). Politicians following the advice of economists today are more likely to benefit than in times past, although harmful economic advice was peddled in some subfields (e.g., development economics) as recently as the 1970s.

The changing nature of the policy role of international relations is likely to follow that of other fields, especially other social sciences. The advisers to the throne who proffered policy advice in times past were as likely to offer quackery as sound counsel. The ability to sell unproven home remedies without much theoretical and empirical validation remains even in developed fields.⁴⁴

International relations as a field remains at an early stage of development. Its data are sparse, its theoretical disputes still largely unresolved, its advice still of the two-handed variety. It is not yet a rigorous discipline from which the uncredentialed can be readily ascertained. There are too many practitioners practicing without a license, and even those with credentials carry a small tool kit with little to offer and little of that assured. At the moment, it is not clear whether the policymaker who seeks scholarly advice benefits upon receiving it. But, within the limits of the knowable, that will change.

NOTES

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1. Residual uses of the supernatural continued well past the Enlightenment. The Nazis consulted astrologers, as did Nancy Reagan. Goldhamer (1978, 141) points out that the conflict between the analytic and intuitive mind in politics has always existed, labeled by such dichotomies as “Mind versus Heart, Rational versus Traditional, Modern versus Ancients, Reason versus Intuition, Calculation versus Judgment, Scholar versus Actor, Theory versus Practice and Experience, Book Learning versus Common Sense.” He goes on to note (142) that the scholar “has a somewhat ambiguous status” and is “just as likely to be rejected by the intuitive and practical man as by the theoretician and calculator.”

2. Edwin Meese III, White House counsel to Ronald Reagan, described

an expert as “somebody who is more than 50 miles from home, has no responsibility for implementing the advice he gives, and shows slides” (Simpson 1988, 20).

3. Princeton University’s Woodrow Wilson School was originally founded in 1930, the Fletcher School of Law and Diplomacy in 1933, Johns Hopkins School of Advanced International Studies (SAIS) in 1943, Harvard University’s John F. Kennedy School of Government (founded as the Graduate School of Public Administration) in 1936, and Columbia University’s School of International and Public Affairs (founded as just the School of International Affairs) in 1946. In contrast, Columbia University’s other socially relevant professional schools were founded much earlier: the School of Journalism in 1912, the School of Social Work in 1898, and the School of Public Health in 1919.

4. One exception came when President Reagan long delayed appointing a council chair, was quite willing to go outside the academy to find one, and considered the possibility of abolishing the office.

5. There has also been a temporal component to this divide in economics. In some periods, abstract theory and formalization were unwelcome. This was the charitable reason for Harvard’s failure to offer Paul Samuelson a job (Silk 1976; Samuelson 1992). In the 1990s, pure theory was again increasingly downplayed by many economics departments that instead stressed the importance of empirical application.

6. See the discussion of the impact of World War II on scholarly research interests in Stein 1980.

7. For popular treatments, see Kaplan 1983 and Herken 1985. Governmental assistance for the development of deterrence theory extended to very abstract theoretical enterprises, including mathematics and game theory, that might be only remotely applicable to the real world. For a discussion of game theory’s normative roots and the governmental financing of its development, and for additional cities, see Stein 1999.

8. Vernon 1971; Keohane and Nye 1972; Chayes and Chayes 1993. The counterattack emphasized the continued centrality of nation-states and saw the prospect for war as defining international relations. Waltz’s (1979). *Theory of International Politics* should be read as an example of the empire striking back to reaffirm the central core of international relations theory. Even its title suggests a rebuke to alternative formulations. Attacked, proponents of the position quickly retreated from their excessive optimism, although not so fully as to extinguish all debate. Keohane and Nye’s delineation of “complex interdependence” was a dramatic retreat from transnational relations. The 1990s version of the debate included Mearsheimer’s (1994) attack on the utility of international organizations and the responses to him from the advocates of “liberal institutionalism.”

9. The field of economics came under increasing criticism from the 1970s to the 1990s for a focus defined by theoretical concerns internal to the field rather than by external economic developments. Important economic trends in productivity and wages, for example, were little understood by modern economists and, more important, seemed of little concern and interest to many of them. In contrast, John Maynard Keynes, earlier in the century, was dramati-

cally affected by and interested in such real-world developments as the Great Depression of the 1930s.

10. The failure to predict the end of the Cold War suggests a problem for the field's policy relevance, but not for international relations theory as such, despite the claims of many that international relations theory must be rethought and reconstructed. The end of the Cold War, as well as its unpredictability, are both fully explainable within the core realist tradition. Realism is a typical equilibrium argument in which perturbations are exogenous shocks (Stein 1993). The theory has greater difficulty with explaining the Soviet Union's retreat from its empire without a fight, though a consistent story for this could be developed within the framework of the theory. More problematic for realism is the failure of the exogenous shock of the Soviet Union's implosion to have resulted in the reequilibrium dynamics central to the model's operation.

11. This poses a special problem for scholars who must invest their human capital in areas that may literally disappear, at least for substantial periods of their intellectual lifetimes. In some cases, a focus of inquiry can disappear almost entirely, as happened with the end of the Cold War and the collapse of the USSR when those whose expertise involved the Cold War, deterrence, weapons systems, and the Soviet Union itself were forced to find alternative areas of study. When currents shift, scholars must decide what to do. Often, bitter generational battles can ensue. Peace scientists were aghast to see the eclipse of their field and the emergence of international political economy. One eminent peace theorist reacted with horror to young scholars studying the trade in commodities by noting at one international meeting that coffee was less relevant than nuclear weapons, which remained the central problem for international politics. He decried the diminution of intellectual talent that still focused on the planet's core concern, the avoidance of nuclear war.

12. See Micklethwait and Wooldridge 1996. One painful example was the downsizing fad begun by the best-seller *Reengineering the Corporation*, which, most soon came to admit, did not work as advertised. Faddism also exists even in the hardest sciences. One physicist describes it this way: "It is easy to jump on the latest bandwagon when your mind is in one of its usual states of hibernation and you don't have any decent ideas of your own. This is particularly true in my own field, cosmology, where definitive experimental tests of theories are slow in coming. So you can calculate to your heart's content without fear of being proved wrong in the near future" (Rothman 1989, viii).

13. A few isolated bureaucratic agencies have, during certain periods, been willing to support basic research without any sense of immediate or even potential utility.

14. Lindblom and Cohen (1979, 5–6, 8, 29) argue that professional social inquiry includes the entire panoply of academic social science and can affect social problem solving quite indirectly.

15. Analogously, technical analyses of the stock market, which are atheoretical trend-trackers, remain useful. Medicine also finds a correlational knowledge of factors dubbed *markers* to be useful for diagnosis even absent theories causally linking them to underlying diseases. Purely descriptive work can also

have profound theoretical consequences. Margaret Mead's description of the gender relationships on a single island was of great theoretical importance because she depicted something that should not have existed if Freudian theory were correct.

16. Below, I offer a set of reasons why much international relations research is not particularly relevant to the making of foreign policy. But there are also more general problems that make the social sciences of limited use in solving social problems (Lindblom and Cohen 1979).

17. This argument was most forcefully made in a popular magazine and in an applied journal by John Mearsheimer (1990a, 1990b).

18. This can be illustrated using an analogy common to the field, that of states as firms. Just as firms cannot affect the market's structure, for it remains outside them and results from the cumulative combination of firms' behaviors, so states cannot affect the structure of the international system.

19. This is Kissinger's position about the utility of international relations theory.

20. The emergence of events-data studies in the late 1960s and 1970s was one response to the failure of international relations scholars to study the bulk of international relations that occur in between the extremes of alliances and war. Compiling databases of international events and coding them on a scale of cooperation and conflict was a way of broadening the empirical reach of the field. Unfortunately, too many empirical studies focused on descriptive statistics, and the entire exercise ultimately got bogged down in methodological disputes. Later, the recognition of the need for broader data could be seen in the development of a behavioral component for the Correlates of War data project.

21. The term is not always applied consonantly. The characterization of a "special relationship" between the United States and Britain signaled that Britain was closer than America's other allies. But the term has also been used to signal the U.S. commitment to Israeli security absent an alliance (though many consistently and incorrectly call Israel a U.S. ally). The problem is similar to that which exists in interpersonal relationships as new terms are periodically required to distinguish different degrees of commitment (*significant other* joins *fiancé* and *spouse*). Our vocabulary, in international as well as other relations, is inadequate for the range of observable relationships.

22. For a longer intellectual history of the study of the impact of war and its evolution in this century, see Stein 1980.

23. The relationship, between Thomas Schelling and John McNaughton, is discussed in Kaplan 1983, 332–36).

24. There are generic empirical problems inherent in international relations. There are too few countries and, happily, too few wars and alliances on the dependent side, for adequate empirical analysis. But the number of independent variables is large, and so significant findings are hard to come by. Not surprisingly, the scientization and mathematization of the field have emphasized formal studies and simulation rather than empirical work.

25. When the paper was subsequently published, the policy prescription was substantially toned down.

26. The change to viewing the earth as round rather than flat marked a significant shift with important consequences, as did the change from believing that the sun moved around the earth to the other way around. Such dramatic changes are also found in the social sciences. Economic theory once held that printing money would lead to lower interest rates; later theory held that printing money led to higher ones. Plausible theory and historical experience both underlay the comparable certainty with which the two arguments were propounded. But one economist notes that a Rip van Winkle who went to sleep in 1870 and awoke in the 1990s would find that the economic theory he knew before nodding off remained perfectly adequate – that he could do without the intellectual twists, turns, and diversions of the intervening century (Lal 2000).

27. Mearsheimer's analysis, until the very end, is purely systemic. The ad hoc arguments at the end bring in domestic politics; they are unsubstantiated, undeveloped, and outside his analytic framework. The theory about nuclear stability does not hold that possession of nuclear weapons by sober states is stabilizing. Rather, the point of that argument is that possession is stabilizing since it makes states sober in the realization that no objective is worth the cost. To argue that unstable regimes should not have nuclear weapons is wholly outside the argument and inconsistent with the whole thrust of the nuclear stability hypothesis.

28. The scholarly work on deterrence stability was consistent with a finite-deterrence strategy and not with large-scale deployments driven by war-fighting doctrines. Yet both U.S. procurements and U.S. targeting plans reflected warfighting doctrines held by the military and did not reflect the scholarly consensus.

29. A scholarly consensus, especially as regards an uncertain future, is also harder to find than most imagine. Paul Ehrlich of Stanford, in discussing the National Academy of Sciences, said it "would be unable to give a unanimous decision if asked whether the sun would rise tomorrow" (quoted in Simpson 1988, 139).

30. See the classic statements by Max Weber.

31. Lindblom and Cohen (1979, 11) point out that political scientists will push for analytical problem solving even when there are conflicting values.

32. The distinction between political and policy risk comes from Lamborn 1985.

33. This paragraph draws upon Herken 1992.

34. This was not the first time that war spurred institutional development in the relationship between the state and science. The National Academy of Sciences was created during the Civil War, and the National Research Council was established in 1916.

35. The disdain of the foreign policy advising elite for domestic politics is sometimes articulated in the strongest terms. George Kennan, an intellectual architect of containment, complained about the ability of democracies to respond early and smoothly to foreign policy challenges. A comparable complaint

was later voiced by Oliver North, who felt frustrated by the Reagan administration's inability to awaken the public to the challenge he believed Nicaragua posed.

36. Henry Kissinger (1979) argues in his memoirs that the same can be said of what one learns in government service: "High office teaches decision making, not substance."

37. For an extended discussion of this and the use of strategic interaction models in international relations, see Stein 1999.

38. An interesting example comes from the government's sale of cellular phone rights. All the bidders hired their own game theorists, and most accepted the recommendations they received from them. Not surprisingly, the bids did not meet the projections made by non-game theorists prior to the bidding. One bidder who did not follow the advice did not do as well (Koselka 1995).

39. This is the claim of psychoanalysis, with its core presumption that patient awareness is the key to treatment.

40. Much the same can be said about organizational process models of decision making (more typically called bureaucratic politics). The presumption here, too, is that awareness of how organizational dynamics impair decision making can itself improve the quality of decision. The process literature often

portrays itself as offering an alternative explanation to purpose. Yet much of it focuses on policy implementation rather than adoption, and that which focuses on the latter is descriptive rather than theoretical and constitutes a supplement rather than an alternative to purposive explanation. For one of the few attempts to integrate organizational with cognitive process in an explanation of foreign policy choice, see Gronich 1991.

41. A sense of the magnitude of the problem for those wishing to apply these tools is provided by scholarly disagreement about the exact nature of past historical episodes. If scholars with access to the documents of all parties cannot agree long afterward about the nature of the strategic game that states thought they were engaged in, then policymakers in the midst of a crisis have a much more difficult time.

42. The strategic interaction literature is full of competing equilibrium concepts and paradoxes in which plausible decision criteria generate different choices. Indeed, one general implication of work in decision theory and social choice theory is that the imposition of too many criteria for any choice generates at some point a null set – that is, there is no choice that meets all conditions. Selection requires relaxing one or more of the desired criteria. But relaxing different criteria will lead to a different choices. Often, relaxing anyone criterion generates a feasible set rather than a unique outcome.

43. For an excellent introduction to the utility and potential of strategic choice for international relations theory, see Lake and Powell 1999. For my views of the strengths and limitations of the approach, see Stein 1990, 1999.

44. An interesting example comes from Arthur Laffer's marketing of supply-side economics and the Laffer curve. His specific argument was scoffed at by mainstream economists, who complained not only about his argument but about his credentials and lack of a Ph.D. in economics.

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