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The Relevance Question

PROFESSIONAL SOCIAL SCIENCE AND THE FATE OF SECURITY STUDIES

In his April 14, 2008, speech to the Association of American Universities, former Texas A&M University president and then secretary of defense Robert M. Gates declared that "we must again embrace eggheads and ideas." What he meant was that "throughout the Cold War, universities were vital centers of new research" and that at one time U.S. national security policymakers successfully tapped intellectual "resources outside of government" to help them formulate policy.[1] One of the most influential civilian academic strategic theorists, the late Harvard Nobel laureate Thomas Schelling, confirmed that there once was "a wholly unprecedented 'demand' for the results of theoretical work: scholars had an audience and scholars had access to classified information. Unlike any other country . . . the United States had a government permeable not only by academic ideas but by academic people."[2]

While not all scholars and policymakers agree that the two sides of what many now see as a yawning chasm have had, or could have, much useful to say to each other in the realm of national security affairs, the vast majority do. Former ambassador David Newsom, for example, thought that of all the various groups in American society that could shape U.S. foreign policy, "the free realm of academia—the 3,638 institutions of higher education and the persons associated with them—should have the most knowledge and insight to offer to policymakers."[3] MIT professor and long-term U.S. government

consultant Ithiel de Sola Pool agreed that training in the social sciences constituted a useful tool for policymakers.[4]

Despite this general optimism and the best of intentions among both scholars and policymakers "the relationship between the federal government and the social sciences generally and historically, while substantial in scope, has not been altogether harmonious," to put it mildly.[5] According to a Teaching and Research in International Politics (TRIP) survey, a regular poll of international relations scholars, very few believe they should not contribute to policy making in some way. Yet the majority also recognize that the state-of-the-art approaches of academic social science constitute precisely those approaches that policymakers find least helpful.[6] A related poll of senior national security decision makers confirmed that for the most part academic social science is not giving them what they want.[7] The problem, in a nutshell, is that scholars increasingly equate rigor with the use of particular techniques (mathematics and universal models) and ignore broader criteria of relevance.

Gates's efforts to bridge the Beltway and Ivory Tower gap thus came at a time when it seemed to be growing wider. In April 2009, Harvard professor (and former high-level State Department, Defense Department, and intelligence community official) Joseph Nye opined in a widely discussed article in the *Washington Post* that "the walls surrounding the ivory tower never have seemed so high."[8] The gap between scholars and policymakers has widened in recent years, particularly in the realm of national security affairs, once a model of collaboration.[9] And there is hard data undergirding this concern. As figure 1.1 shows, the willingness of leading international relations scholars to offer such policy recommendations has declined in absolute terms, at least since 1980 (and I will show well before then).[10] In the view of many on either side of the chasm, the bridge between the Ivory Tower and the Beltway has become an increasingly rickety one, particularly as the discipline of political science has striven to become more scientific.

This development is puzzling: it flies in the face of a widespread and long-standing optimism about the compatibility of rigorous social science and policy relevance that goes back to the Progressive Era and the very dawn of modern American social science.[11] As historian Barry Karl remarked apropos Charles Merriam, one of the founders of the modern discipline of political science, he "was an American activist of his generation before he was a political scientist; it was his reason for becoming a political scientist. He saw no conflict between activism and science. Indeed, he saw science as

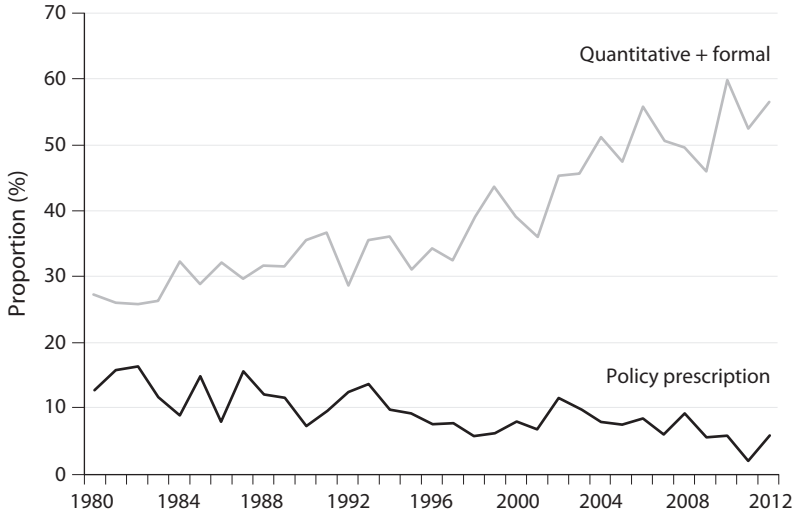

FIGURE 1.1. Quantitative methods, formal modeling, and policy prescriptions in top IR journals since 1980. (The TRIP journal data from which I generated this and subsequent figures are available at http://www3.nd.edu/~carnrank/.)

the essential precondition of a useful activism."[12] And early in the Cold War at the height of the Behavioral Revolution in the social sciences, his student Harold Lasswell sought to craft a "policy science" that would apply cutting edge social science to the pressing policy problems of the day. Indeed, there is confidence that the effort to make the social sciences more "scientific" is not incompatible with relevance.[13] Some scholars go so far as to argue that it is the sine qua non of real relevance.[14] This confidence persists today.[15]

I suggest that this growing scholarly/policy gap is the result of the professionalization of the discipline of political science. While the professionalization of a discipline and its increasing irrelevance to concrete policy issues is not inevitable, there nonetheless seems to be an elective affinity between these two trends.[16] Rigor and relevance are not necessarily incompatible but they are often in tension, which is why social science's relevance question endures. Figure 1.1 demonstrates this point clearly: as the number of scholarly articles using sophisticated quantitative or formal methods increased since 1980, the percentage of them offering concrete policy recommendations—the core of policy relevance—has declined.

Second, many proponents of the scientific study of politics now eschew advocacy of particular policies on the grounds that doing so is incompatible with scientific objectivity.[17] This is the widely embraced, but frequently mischaracterized, value-neutrality concern that the early twentieth-century

German social scientist Max Weber first raised.[18] Third, many pressing policy questions are not readily amenable to the preferred methodological tools of social scientists. Fourth, even when the results of these approaches are relevant to policy questions, they are often not accessible to policymakers or the broader public.

Finally, many scholars are overly optimistic that despite these other problems the pursuit of basic research will nevertheless produce applied knowledge via a "trickle-down" (or bubble-up) process.[19] Adherents of this view believe that normal progress of science naturally confers policy benefits in much the same way that some economists are sanguine that economic growth will increase the wealth of the poorest, even if wealth is quite unevenly distributed.[20] As F. A. Lindemann, Winston Churchill's wartime science adviser put it, "Every addition to our knowledge re-acts upon industrial problems and either suggests improvements in technical processes or at any rate prevents the waste of time entailed in attempting impossibilities."[21] This reinforces the inclination of social scientists not to worry about whether their own work is directly relevant.[22]

These factors explain why the more "scientific" approaches to international relations scholarship seem to be the least relevant, at least as measured by their practitioner's willingness to offer policy recommendations. The problem, in my view, is not so much that "scientific" approaches to national security policy are irrelevant by definition; rather, their current dominance is a symptom of a larger trend among the social sciences to privilege sophisticated method and universal models over substance with a resulting decline in policy relevance. As Kenneth Waltz warned, methods-driven work is likely to be at best only "accidently relevant."[23] Method-driven and model-driven research do not cause identical pathologies but both can inhibit "problem-driven" research, the sine qua non of policy relevance.[24]

This is by no means an argument against the importance of theory in security studies. Social science theories matter because they can serve as analytical models, rhetorical instruments, and cognitive frameworks for policymakers as they make and implement policy.[25] The key is that scholars try to address problems of concern to the policy community and in a way that informs action. Rather, it echoes the caution expressed by participants in the Rockefeller Foundation Conference on International Politics, held on May 7–8, 1954, such as Reinhold Niebuhr who maintained that "the theorist's contribution would be very irrelevant if he thought that the only rational theory was one based on constants and general laws. Theory must be built into the knowledge of what the statesman faces."[26]

This book seeks to answer social science's larger relevance question: How can it be both a rigorous scholarly enterprise while also engaging with society's practical problems? To do so, it engages four specific questions: First, what do I mean by policy relevance? Second, what has been the influence of academic social science on policy historically? Third, what explains variation in its influence over time? Finally, what, if anything, should be done to close any gaps between scholars and policymakers?

In general, policy-relevant scholarship limns the range of possibilities open to policymakers and assesses the consequences of the particular policy choices they make.[27] While such work does not have to be produced directly for policymakers, it should offer concrete policy recommendations derived from systematic investigation aimed at shaping government action, directly or indirectly. The best metaphor for describing policy-relevant scholarship is that it provides policymakers (or journalists or citizens) with a mental map to help them navigate the real world.[28]

Expectations for what sort of influence scholars can have need to be reasonable. The notion that to matter academic social science must regularly shape high-level national security decisions on a consistent basis is too demanding a standard. As RAND Corporation historian Bruce Smith noted, "The end product of most planning and research activities is not an agenda of mechanical policy moves for every contingency—plainly an impossible task—but rather a more sophisticated map of reality carried in the minds of the policy makers."[29] Relevance, of course, is not identical with influence. A scholar can offer concrete policy recommendations but policymakers may not adopt them. Moreover, even if policymakers adopt these recommendations, that is no guarantee that good or effective policy will result. So relevance, in my view, is a necessary, if not sufficient, condition for influence. And I will offer logical arguments and historical evidence to suggest that scholarly input into policy is more often than not beneficial and its absence detrimental to good policy.

Ascertaining the extent to which academic social scientists had influence on policymakers is challenging:[30] As political scientist John Kingdon warned, the influence of academics on policy debates is often "hidden," and the secrecy surrounding national security decision making makes their role in national security strategy even more opaque.[31] An internal State Department report highlighted the problem of measuring the impact of external research: "Actual utilization of this information is difficult to measure. Reports and written memoranda are distributed to approximately 500 officers in the Department and other agencies concerned with foreign policy and national security matters. The external research division answers some 35

telephonic queries per day. The continuing demand for this kind of information indicates a felt need on the part of policy and intelligence officers, but it is not known exactly how or to what extent this information is put to use in the actual formulation of policy or analysis of issues."[32] Indeed, such an exercise shares the more general challenge of tracing the influence of ideas—the currency of academics—on policy outcomes.[33]

To answer this second question about the influence of social science on policy, I explore the changing relationship between the discipline of political science and its subfield of international security from the early years of the twentieth century through the post–Cold War era. Most security scholars share Columbia political scientist Robert Jervis's view that there was a "golden age,"[34] during which "there were significant links between theory and U.S. policy."[35] International security has long been among the most policy relevant of subfields within the discipline of political science. This is still the case today, at least as measured by the willingness of authors in top international relations journals to offer explicit policy recommendations. There is a significant difference in this regard, as figure 1.2 shows, between articles since 1980 dealing with security issues (i.e., weapons of mass destruction, weapons acquisition, terrorism, and military intervention) and other issue areas in the field of international relations.

Admittedly, this view of an academic-policy golden age is not universally shared.[36] After serving in the Second World War, U.S. Navy anthropologist Alexander Leighton reported that the conventional wisdom among social scientists in government during the war was that "the administrator uses social science the way the drunk uses a lamppost, for support rather than illumination."[37] More recently, highlighting the difference between U.S. nuclear declaratory policy (in which civilian defense intellectuals apparently had influence) and actual operational doctrine and war plans (where they did not), historian Bruce Kuklick presented the most sustained critique of the Golden Age nostalgia.[38] One basis for pessimism that policymakers and scholars could have much to say to each other is that the former operate in a very different environment from the latter. Policymakers need good enough answers in a short period of time while scholars are hesitant to say anything about an issue until they are highly certain of their answer.[39] Former director of the State Department Policy Planning Staff during the George W. Bush administration and Stanford professor Steve Krasner also blamed the "complexity" of the policymaking process for the inability of scholars to intervene effectively in it.[40] Given this, so the pessimists maintain, it is futile to think that it can mesh with the academic enterprise.

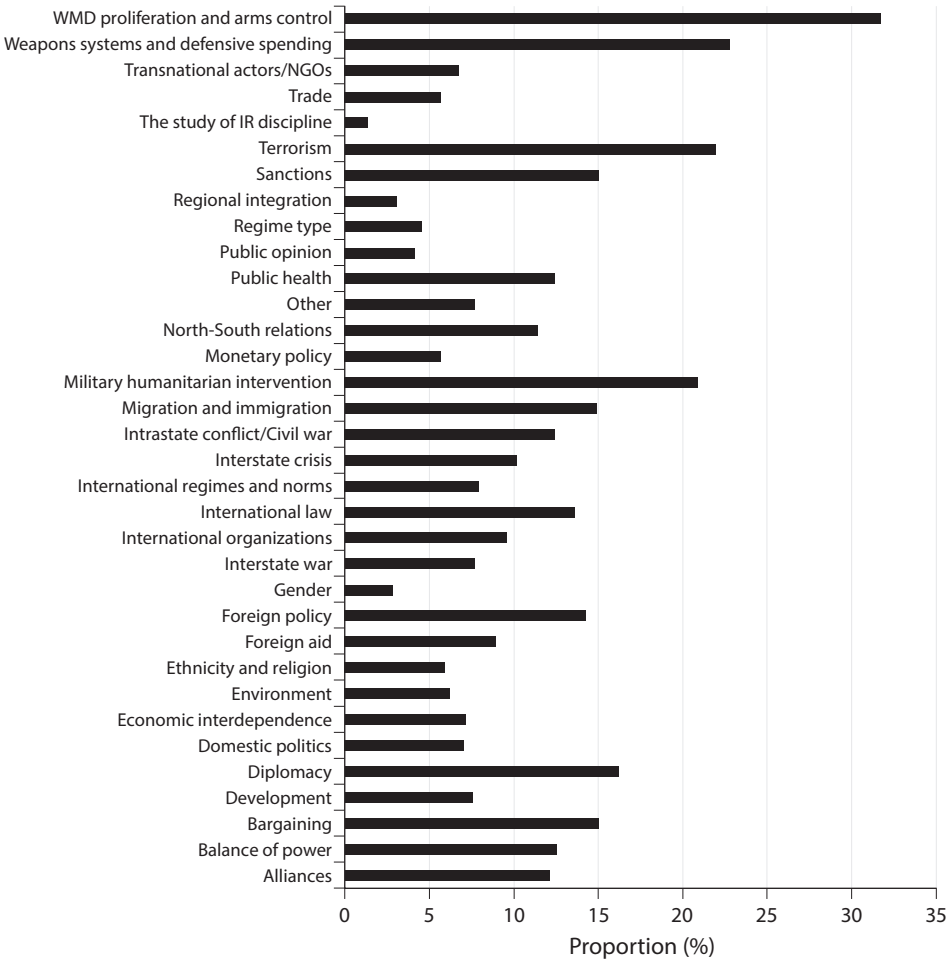

FIGURE 1.2. Articles providing policy prescriptions by topic. (Source: TRIP.)

Another objective of this book, therefore, is to look at a broader swath of history and a wider array of national security issues. Doing so reveals that, despite waxing and waning, there were periods in which social scientists had significant policy influence. Indeed, few people outside the subfield of international security are aware of the extent to which the U.S. government routinely reached out to academic social scientists to meet these challenges in the past. The history of the last hundred years shows that the scholarship of some social scientists did have real impact on presidential and senior policymakers' decision making at certain junctures, particularly during wartime and periods of crisis. Table 1.1 highlights some of these particular national security issues.

TABLE 1.1. Social Science and National Security Issues

World War I
The Inquiry and postwar settlement
Public information and morale
Psychological testing of soldiers
Economic mobilization of the economy
World War II
Military job assignment and training
Morale studies
Psychological health of troops
Race relations
Enemy military assessments
Managing U.S. war production
Price controls and rationing
Foreign area and language training
Bombing strategy
Propaganda
Cold War
Nuclear strategy
Political development and modernization theory
Psychological warfare
Human performance
Manpower retention and training
Human factor engineering
Foreign security environments
Policy planning strategies
Effect of civilian morale on military capabilities
Social effects of fallout
Strategies for undermining Communist rule
The psychology of bombing strategies
Propaganda
Counterinsurgency
The operational code of the Bolsheviks
Arms control and stability
Limited war
Coercive bargaining
Post–Cold War
Ethnic conflict and civil war
Nuclear proliferation
Post-9/11
Terrorism
Counterinsurgency
Foreign imposed regime change

This book seeks to trace and explain this influence in national security policymaking, much as political scientist Robert Gilpin did for the role of natural scientists in U.S. nuclear weapons policy.[41] The late Stanford political scientist Gabriel Almond pointed out that many of his colleagues ignored the history of the discipline, save for occasionally dismissing it as a prescientific dark age. In his view, this was part of an intentional strategy to shape the future of the discipline by ignoring its past.[42] If the policy-relevant past was a period of methodological and intellectual stagnation, then moving forward, political science ought to eschew policy relevance in the interest of scientific progress. I want to reintroduce this history to challenge the facile view that policy-relevant security studies is an artifact of the discipline's prescientific era and also highlight the downsides for relevance of previous efforts to modernize and professionalize the discipline.

Another model for this book is historian Peter Novick's history of history. In it, he tells the story of his discipline through the waxing and waning of the notion of "objectivity"; I aim to shed light on the development of political science through changing notions of "relevance."[43] Both "objectivity" and "relevance" define the core of these disciplines but also connect them with issues outside of the guild. Changes in them shape both the discipline and its relationship with the rest of society. And they remain deeply contested, which is why both the "objectivity" and "relevance" questions remain open today.

My third question, then, is when and under what conditions do social scientists do work that matters for policymakers? An answer to it matters for two reasons: as we saw already, there is some debate about whether and if it has really affected U.S. national security policy. This debate about the extent of the influence of academic social scientists calls for further historical investigation to ascertain, in the words of a National Research Council study, whether there is any "relationship . . . between basic [social science] research and programmatically useful results."[44] I share Elizabeth Crawford's hunch that "a more widespread knowledge of the 'history' of the relationship of government and social science may also make the discussants realize that whatever happens to be the particular controversy of the moment, it is not likely to be unique."[45]

In the literature on the role of the ideas of "policy experts," there is now a trend toward moving beyond general characterizations (they matter or they don't) to establishing more nuanced propositions about when and under what conditions experts influence policies.[46] An answer to this

question requires me to provide a more general explanation—a theory—for the variation in their willingness to do so. I will show that academic social scientists did have some influence on national security policy but that the so-called Golden Age of strategy (1945 to 1961, according to economist Thomas Schelling) exerted an influence that was both earlier and shorter than we have generally recognized. The waxing and waning of policy relevance among the social sciences has two components to it: the relationship between academic disciplines and the policy realm and the dynamics within those disciplines themselves. A complete account of this relationship would also have to consider developments in government (the increasing internalization of social science research) and public opinion (the decline in confidence in academic expertise).[47] I focus here primarily on developments within social science as the heart of the relevance question.[48]

Concern about how the professionalization of social science disciplines has led to a disengagement from practical affairs is long standing, manifesting itself in books from Robert Lynd's 1939 *Knowledge for What?* to Ian Shapiro's 2005 *Flight From Reality in the Human Sciences*.[49] My explanation for social science's enduring relevance question is that social science, at least as it has developed in the United States since the early twentieth century, contains two contradictory impulses: to be a rigorous science and a more broadly relevant social enterprise.[50] Initially, there was optimism among social scientists about the compatibility of these two goals.[51] But there are also tensions between them.[52] Historically the most useful policy-relevant social science work in the area of national security affairs has often been interdisciplinary in nature, and this cuts against increasingly rigid disciplinary boundaries in the academy. There are also widely recognized tensions between "basic" and "applied" research more generally, which not surprisingly manifest themselves in the social sciences as well.[53] In essence, the professionalization of the social sciences sparked a process of goal displacement in which steps taken to make them more rigorous so as to enable them better contribute to policy had the unintended result of eventually making them less relevant.[54]

This pattern has been evident in the changing relationship between political science, its subfield international security, and the policy world over the past century.[55] I begin with this observation but add to it exploration of the specific mechanisms that lead to this retreat from relevance and a detailed historical account of the field of security studies that illustrates it in action.

War Builds the Bridges between the Ivory Tower and the Beltway . . .

During wartime, the tensions between these two impulses have been generally muted; in peacetime, they intensify and there are powerful institutional incentives within academe to resolve them in favor of rigor rather than relevance.[56] The international security environment—among the most intense of external stimuli—can restrain internal disciplinary tendencies to retreat from relevance during periods of war or heightened threat and foster cooperation between academia and government.[57] My explanation for this recurring pattern follows from the substantial literature on the effect of war on the state and domestic politics.[58] War solidifies relations between the government and other elements in society.[59] As Gene Lyons explained, "War is a moment of crisis which compresses time and illuminates needs that less dramatic environments obscure or never force to the surface. War also involves the entire society and gives the central government new powers, for the exercise of which it needs new sources of information and expertise."[60]

Two specific mechanisms lead to higher levels of wartime cooperation between government and academia. First, the need for expertise leads policymakers and the public to look to universities for natural and social science knowledge that could contribute to the war effort.[61] Second, a common sense of threat fosters a general rally-around-the-flag effect and stimulates increased patriotic sentiment, which affects even professors.[62] This sentiment helps tilt the balance of opinion within the disciplines in favor of a broader definition of rigor that does not exclude relevance and increases scholars' willingness to balance the tensions between rigor and relevance.[63] In other words, war fosters, paraphrasing philosopher of science Thomas Kuhn, a "revolutionary" approach to science.

And Peace Weakens Them as Disciplinary Dynamics Come to the Fore

Left to their own devices, however, academic disciplines tend to resolve these tensions between rigor and relevance by favoring the former.[64] While a variety of different factors play some role in the peacetime decline in policy relevance among social scientists,[65] I focus on the process of what Kuhn famously termed "normal science" and the impact of institutional dynamics—both vested interests and institutional self-image. Normal science and

organizational interest explain why the social sciences tend to isolate themselves from the rest of society and the culture of "science" accounts for the particular way in which they do so.[66] The tragedy of the professionalization of social science is that it is both the engine of scientific progress but also contains the seeds of its own irrelevance.

The first logic for the decreasing relevance of social science flows from the dynamics of the scholarly enterprise itself. The French sociologist Emile Durkheim famously argued that the division of labor is a fundamental fact of modern life because it is an efficient way to accomplish a variety of complex tasks.[67] Given the limits of individual human cognition, it is only through an intellectual division of labor that science can progress.[68] This growing specialization advances normal science through deeper investigations focused on increasingly narrow questions.[69] Kuhn explained that "normal research, which is cumulative, owes its success to the ability of scientists regularly to select problems that can be solved with conceptual and instrumental techniques close to those already in existence."[70]

Such progress, however, comes at the cost of the increasing isolation of the various specialties from each other and from society as a whole. As Friedrich Nietzsche colorfully put it, "A specialist in science gets to resemble nothing so much as a factory workman who spends his whole life in turning one particular screw or handle on a certain instrument or machine, at which occupation he acquires the most consummate skill."[71] The result is a hyper-fragmentation of knowledge that now makes it difficult for even scholars in different disciplines to understand each other, much less policymakers and the general public.[72] The result of this narrowing of focus is that many academics no longer deal with issues of interest to broader society.[73]

Max Weber famously lamented that the ethos of modern rationalism was ushering in the "iron cage" of modern bureaucracy.[74] I fear that in a similar fashion the process of normal science has fostered an intellectual withdrawal from policy relevance among the social sciences. Sociologist Andrew Abbott explained that "specialists in knowledge tend to withdraw into pure work because the complexity of the thing known eventually tends to get in the way of the knowledge system itself. So the object of knowledge is gradually disregarded."[75] In other words, the advancement of modern science ironically makes it less directly applicable to concrete problems as it becomes more specialized.[76]

Second, one of the hallmarks of professionalism is "corporateness," which Samuel Huntington defined as "a sense of organic unity and consciousness of themselves as a group apart from laymen."[77] Explaining the

paradox that the public holds greater esteem for their personal physicians while the medical establishment favors leading medical researchers, Abbott attributed this process to a desire to maintain professional "purity," which can only be done through a withdrawal "from precisely those problems for which the public gives them status."[78] Traditional theories of organizational behavior would also attribute the decreasing relevance of academic social science to the fact that universities, like most other complex organizations, seek autonomy, reduction of uncertainty, and more resources.[79] When these goals conflict, organizations almost always prefer the first.

One means by which disciplinary organizational interest encourages scholars to separate themselves from nonspecialists is by using jargon and other modes of discourse that are incomprehensible to the laity.[80] Economist John Kenneth Galbraith recounted that economists regard this as "the filter by which scholars are separated from charlatans and wind-bags."[81] Such a screen is even more attractive to social science disciplines such as political science, which deal with issues that are not otherwise inherently difficult for educated laypersons to engage. To maintain their autonomy and protect their monopoly on expertise they need to construct higher barriers to entry. Sophisticated social science methods (models, statistics, or abstruse jargon) offer an ideal barrier to entry for the nonprofessional because they take considerable investment in time and effort to learn. Speaking within the guild makes it possible to maximize autonomy by making the university more distinct from, and hence independent of, the rest of society.[82] In short, one does not have to be as cynical as George Bernard Shaw, who famously quipped that "all professions are conspiracies against the laity," to believe that the increasing withdrawal from relevance within the social sciences is in part fostered by disciplinary vested interest.[83]

A reinforcing set of organizational incentives are "sunk costs" and the resulting "law of the instrument" mind-set, which lead many scholars who invest the time and intellectual capital in learning particularly sophisticated research techniques to amortize their investment by either choosing only questions amenable to them or forcing questions which are not into their template.[84] Such an approach may occasionally address policy issues, but only in the way a broken clock tells the correct time twice a day. This trend may also be reinforced by another bureaucratic rationale: failure while pursuing normal science is punished far less often in the academy than failure operating outside the reigning paradigm. Finally, eschewing policy engagement is also a way for social scientists to avoid political controversy that might bring unwanted government and public scrutiny.[85]

A somewhat different, but complementary, organizational interest explanation for the retreat from relevance in academic social science involves how disciplines define rigor and the incentives the individual members of a discipline have to adopt similar approaches to each other. Harold Wilensky explained that "in modern societies, where science enjoys extraordinary prestige, occupations which shine with its light are in a good position to achieve professional authority."[86] One of the hallmarks of science is its ability to measure causes and effects precisely, ideally in mathematical terms.[87] In order to comport with the canons of modern science, scholars increasingly believe they should pursue only those research questions with variables that they can quantify.[88]

To square this circle, many have embraced the distinction between "basic" and "applied" research. Basic research, according to the National Research Council's Study Group on Opportunities in Basic Research in the Behavioral and Social Sciences for the U.S. Military, "is defined as systematic study directed toward fuller knowledge or understanding of the fundamental aspects of phenomena" while "applied research is defined as systematic study to gain knowledge or understanding necessary to determine the means by which a recognized and specific need may be met."[89] The former pursues knowledge for its own sake while the latter seeks solutions to specific problems.[90]

Today, among American universities, an "ideology of basic research" now defines their mission.[91] As the late Donald Stokes put it, "In academic research circles . . . the ideal of pure inquiry still burns brightly."[92] Such an approach is necessarily driven by its own internal agendas and criteria so as not to contaminate the process of science with normative or practical considerations.[93] Lord Acton had outlined that mind-set many years earlier: "I think our studies ought to be all but purposeless. They want to be pursued with chastity, like mathematics."[94] More recently, Abraham Flexner, the founding director of Princeton's Institute for Advanced Study, reportedly preemptively declined an invitation to his colleague Albert Einstein from President Franklin Roosevelt on the grounds that "Professor Einstein has come to Princeton for the purpose of carrying on his scientific work in seclusion, and it is absolutely impossible to make any exception which would inevitably bring him into public notice."[95] It should therefore not be surprising that as the social sciences increasingly emphasized their "scientific" character, they became more disengaged from practical affairs.[96] Many scholars seek to salve their relevance consciences by assuming that basic research's results will nonetheless trickle down (or bubble-up) to policymakers.

John Gunnell confirmed in his history of the American Political Science Association, that despite its desire to have its cake (be relevant) and eat it too (be highly rigorous), as the discipline professionalized it became less committed to practical reform.[97] During the 1950s and 1960s the "technification" or "scientification" of political science increased under the banner of the Behavioral Revolution. Since then, "method" has become the defining feature of its claim to being a "science."[98] The result is that political science is increasingly dominated by the belief that the systematic study of politics can only be conducted with a set of "prescribed techniques."[99]

There are good reasons for believing that the effort to make political science more "scientific" would tend to make it less policy-relevant. "Technification" often leads political scientists to use research methods for their own sake, rather than based on their appropriateness for the research questions at hand; privilege technique over in-depth knowledge of the issue; and in general, steer the research enterprise away from doing work of broader interest.[100] Pursuit of rigor defined narrowly as technique means that less and less of political science is relevant to practical problems.[101]

Increasingly, if one wants to be a "good" political scientist, one emulates the approaches and practices of the leading scholars, institutions, and disciplines. Homogenization is thus a rational response to competition with other organizations for status and legitimacy.[102] One key mechanism through which disciplines become homogenous is through faculty hiring in which universities compete for the same group of leading scholars.[103] Another mechanism is the process of academic peer review, which can foster "the homogenization of opinion."[104] The initiation of peer review in the *American Political Science Review* (*APSR*) in the early 1960s clearly had this result.[105] Former editor Lee Sigelman explained that this process made "it more likely that a given paper will be selected for publication because it passes muster among a narrow range of specialists rather than because it is considered to be of potentially great interest and importance to a broad range of readers. Thus, the end product may be a wider array of narrower articles—greater diversity at the price of even greater fragmentation."[106] Also, most of the professional incentives academics face today lead them to write for each other and pursue disciplinary agendas, rather than write more accessibly and address issues of broader import.[107] In this way, even a small group of scholars committed to a narrow definition of rigor can have a disproportionate influence on the development of the discipline.[108]

Figure 1.3 shows that the policy relevance of articles published in the *APSR* (measured in terms of whether they offered "policy prescription")

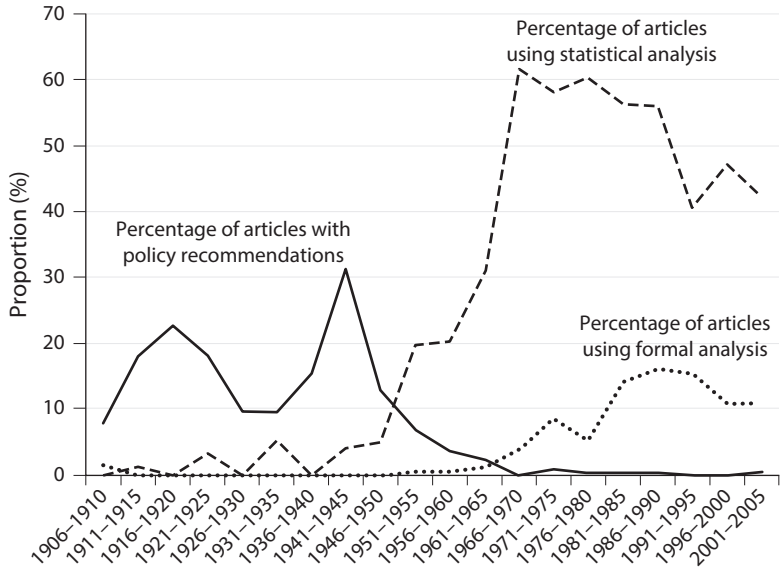

FIGURE 1.3. Percentage of policy-relevant articles in *APSR* from 1906 to 2006. (The data for this graph were compiled by the late Lee Sigelman. Chris Deering of George Washington University kindly shared it with me, and Luis Schenon used it to produce this graph. The data are also available at http://www3.nd.edu/~carnrank/.)

has declined precipitously.[109] As Sigelman put it, "By the early 1960s, prescription had almost entirely vanished from the *Review*. If 'speaking truth to power' and contributing directly to public dialogue about the merits and demerits of various courses of action were still numbered among the functions of the profession, one would not have known it from leafing through its leading journal."[110]

While one might question Sigelman's codings of the policy relevance of articles in the journal, there are three reasons to still regard them as reflective of real trends: as the editor of the discipline's flagship journal he was intimately familiar with the trends within its pages. Since he did not code them to support my argument, we can be confident that his judgment was not biased in favor of finding the pattern I predict. Finally, they track well with assessments of trends in other scholarly journals. Figure 1.1, as we saw, shows the results of a similar analysis of leading international relations journals (including the *APSR*) from 1980 through 2011, which demonstrates the same decline as in the Sigelman data.

The final question, then, is how to strike a better balance between rigor and relevance. Despite doubts about when and how academic social science

influences national security policy, much time and money has been devoted to trying to connect these two realms, both inside and outside the Beltway. It would thus seem worthwhile to figure out how to ensure that these resources are well spent.[111] There are also good reasons for thinking that more policy-relevant social science would contribute to better policy. Finally, there is abundant evidence that the relationship between practical problems and basic science is reciprocal. Rather than the findings of the former trickling down to the latter, a growing number of historians of science have shown that the solution of practical problems at least as often contributes to theoretical advances.[112]

Finding such a balance between rigor and relevance, I argue, also flows in part from the moral obligations of scholars not only to "science," but also to "politics," in an effort to combine and reconcile Max Weber's discussion of these two distinct vocations. I am also inspired in this effort by C. P. Snow's incisive limning of the "two cultures" of science and literature and his related concern that a new cultural divide between academic social science and policy was emerging, with similarly deleterious consequences for both.[113] Policy engagement ought to be viewed not so much as an avocation for social scientists but as a core component of the scholarly enterprise.

Tracing and explaining the changing influence of social science on policymakers is too large an undertaking for one book.[114] Therefore, I propose to focus primarily on the changing nature of one social science discipline—political science—and its policy-relevant subfield, security studies, as a window into this larger question. The stories of the development of political science as a separate social science discipline and the subfield of security studies have been told elsewhere; in this book I focus on telling the story of their changing relationship, emphasizing its closeness in wartime and its estrangement in peace.

Many political scientists today believe the discipline of economics has answered the relevance question once and for all, managing simultaneously to be both the most rigorous of the social sciences and also the most influential in terms of broader policy.[115] According to this view, if security studies would only more vigorously embrace cutting-edge methodologies, it would be better integrated into the discipline of political science.[116] It is precisely the effort of some political scientists to push an exclusively method-driven, rather than a problem-oriented, approach to security studies that lies at the root of the discipline's relevance question today. Security studies embraced cutting-edge social science methods such as operations research, systems analysis, econometrics, and game theory early on, significantly contributed

to their development, but soon discovered their limitations, well before the rest of the discipline.[117] Moreover, two of the most prominent economists in security studies—Thomas Schelling and Walt Rostow—highlight both the promise and the peril of pursuing a science of strategy, suggesting that remaking political science and security studies in economics' image is no shortcut to settling the relevance question.

Let me be clear, this book is by no means an argument against the use of models and sophisticated research methods in security studies. Indeed, one of its arguments is that theory is one of the most important contributions that scholars can make to policy analysis.[118] Likewise, and all other things being equal, if substantively important national security policy questions can be answered using statistical or formal methods, these approaches certainly ought to be employed. What it does argue against, however, is the conflation of "rigor" with the exclusive use of such techniques and its privileging over relevance. The cost of such a mind-set is to ignore many important policy issues because we cannot engage them "scientifically." It also challenges the notion that disciplinary professionalization will put to rest social science's relevance question.

Finally, political science and security studies are excellent cases for a variety of reasons. First, while national security studies has been an inter- and multidisciplinary enterprise for much of this period, political science has been its most consistent home.[119] Although the place of security studies in political science over the years has been uncertain, it has been even more "tenuous" and "precarious" in other disciplines such as history, sociology, or economics. These cases offer variation over this period of time in terms of both the causes (threat environment and professionalization) and also the consequences (the engagement or disengagement of scholars with national security affairs).[120] They also cover much of the period in which political science was developing into a separate and distinct social science discipline and security studies was becoming an important part of its subfield of international relations.

The following seven chapters trace this pattern of wartime social science relevance and peace-time irrelevance across American history in the twentieth century from the First World War through the Global War on Terror. In each, I explore how cutting edge social science was applied initially with great optimism as a tool to answer social science's relevance question from the First World War to the present for issues such as nuclear deterrence, political development and nation building, and coercive bargaining, only to find in each case, that the effort to craft a science of strategy

led to intellectual dead ends and policy debacles as disciplinary dynamics increasingly privileged narrowly defined scientific rigor over broader policy relevance, particularly in peacetime. Chapter 9 summarizes the argument, anticipates the most likely objections to it, and offers some concrete suggestions for how to reestablish the balance between rigor and relevance in the years to come.