Technique Trumps Relevance: The Professionalization of Political Science and the Marginalization of Security Studies

Michael Desch

I explain here the disconnect between our discipline's self-image as balancing rigor with relevance with the reality of how we actually conduct our scholarship most of the time. To do so, I account for variation in social scientists' willingness to engage in policy-relevant scholarship over time. My theory is that social science, at least as it has been practiced in the United States since the early twentieth century, has tried to balance two impulses: To be a rigorous science and a relevant social enterprise. The problem is that there are sometimes tensions between these two objectives. First, historically the most useful policy-relevant social science work in the area of national security affairs has been interdisciplinary in nature, and this cuts against the increasingly rigid disciplinary siloes in the modern academy. Second, as sociologist Thomas Gieryn puts it, there is "in science, an unyielding tension between basic and applied research, and between the empirical and theoretical aspects of inquiry." During wartime, the tensions between these two impulses have been generally muted, especially among those disciplines of direct relevance to the war effort; in peacetime, they reemerge and there are a variety of powerful institutional incentives within academe to resolve them in favor of a narrow definition of rigor that excludes relevance. My objective is to document how these trends in political science are marginalizing the sub-field of security studies, which has historically sought both scholarly rigor and real-world relevance. — Michael Desch.

This essay is followed by responses from Ido Oren, Laura Sjobreg, Helen Louise Turton, Erik Voeten, and Stephen M. Walt. Michael Desch then offers a response to commentators.

Columbia political scientist Robert Jervis reminds us that at one time "there were significant links between theory and U.S. policy," a point echoed by one of the most influential civilian academic strategic theorists, Harvard Nobel laureate Thomas Schelling, who points out that "there 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 to academic ideas but by academic people."[1] But now there is growing concern that the gap between scholars and policymakers has widened in recent years, particularly in the realm of national security affairs.[2] Harvard professor (and former high-level State Department, Defense Department, and Intelligence Community official) Joseph Nye penned

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a widely-discussed article in *The Washington Post*, in which he opined that "'the walls surrounding the ivory tower never seemed so high.'"[3] 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.

This development is puzzling: It flies, for example, in the face of a wide-spread and long-standing optimism about the compatibility of social-scientific rigor and policy relevance that traces its origin back to the Progressive Era and the dawn of modern American social science.[4] As historian Barry Karl remarks of 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 the essential precondition of useful activism."[5] Today, few of Merriam's successors maintain his dual commitment to science and reform.

Moreover, while not all scholars and policymakers agree that the two sides of this yawning Beltway-Ivory Tower chasm have had, or could have, much useful to say to each other in the realm of national security affairs,[6] the majority do. According to the most recent Teaching and Research in International Politics (TRIP) survey—a regular poll of international relations scholars—most believe they should contribute to policy-making in some way. And yet, the majority also recognizes that the state-of-the-art methodologies of academic social science constitute precisely those approaches that policymakers find *least* helpful.[7] A recent related poll of senior national security decision-makers confirms that for the most part academic social science is not giving them what they want.[8] So 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."[9]

Stephen Van Evera first coined the phrase the "Cult of the Irrelevant" to refer to the tendency among many American social scientists to frame and orient their work around internal, disciplinary criteria and agendas, and to ignore the relevance of their scholarship for addressing broader societal concerns, government policy needs, or the concerns of people in the "real world" more generally. Lawrence Mead characterizes the same phenomena as the "the New Scholasticism," Ian Shapiro refers to it as "the flight from reality," and John Mearsheimer laments that the academy has become a "self-enclosed world."[10] Their common concern is that a social science guided primarily by internally-oriented research agendas and assessed by self-generated metrics of excellence is not likely to encourage members of the guild to speak to broader audiences nor gauge scholarly excellence by broader relevance criteria of social or political import.[11] The problem, in a nutshell, is that scholars increasingly equate rigor with particular techniques (mathematics and models) and ignore broader criteria of relevance.[12]

In what follows I expand upon this theme with an account of the changing relationship between political science and one of its key subfields—security studies. I show how disciplinary trends in political science have fostered an increasing insularity and thus to the marginalizing of security studies, which has historically pursued both scholarly rigor and real-world relevance. In my view, these developments have been harmful both to sound scholarship and effective public policy, and so I advocate a more engaged form of political science in which security studies and other policy-relevant sub-fields can prosper.

To be clear, I am not arguing that scholarship that is formal or quantitative is by definition irrelevant. Indeed, one can point to examples of both that are. When applied to economic issues, the discipline of economics has managed to be both highly "scientific" and, at times, quite relevant, though for both good and ill.[13] Likewise, there are examples of highly quantitative political science that policymakers have found useful.[14] Finally, there is much non-quantitative scholarship, particularly but not exclusively in the humanities, that is jargon-laden and otherwise inaccessible to a wider audience. Rather, what I am arguing is that when scholars appear to embrace these approaches for internal, disciplinary reasons rather than because they can help us answer substantively important questions, they are of at best "mixed" benefit to policymakers.[15]

A number of political scientists have argued that security studies should vigorously embrace cutting-edge methodologies in order to be better integrated into the discipline of political science. Nuclear strategist Bernard Brodie once advocated making strategy into a "science," and commended economics as the model for doing so.[16] More recently, Jeffrey Frieden and David Lake argue that "only when International Relations brings science to the discussion does it have anything of enduring value to offer, beyond informed opinion."[17] But in doing so, they ignore that the sub-field pioneered cutting-edge social science methods early on, significantly contributed to their development, but soon discovered their limitations, well before the rest of the discipline.[18]

These dangers are particularly evident in the field of security studies, and indeed they were acknowledged, long ago, by some of the early proponents of the effort to make it a "science." In a letter to James Holland of the Army War College, Brodie later confessed that he was a "trifle uneasy" about his early call to make strategy a science modelled on the methods of economics: "I must tell you," he wrote, "that I am not so disposed today as I was then to toss so many bouquets to the economists. It is not the substance of what I say in that paper that bothers me so much as the tone. I am concerned with the fact that the relevant political issues tend to be automatically

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de-emphasized by giving so much emphasis to the comparability of strategy with economics."[19] More recently, Thomas Schelling admitted that he "was surprised and somewhat perplexed" when he received the Bank of Sweden Prize in Honor of Alfred Nobel in part for game theory, which he applied to national security policy issues in the 1950s and 1960s, given that by 2005 what he did "was not recognizable as game theory," given subsequent technical developments in academic economics.[20]

David Ricci characterizes as a "tragedy" that the discipline of political science's initial effort to become more scientific so it could better guide policy has ironically made it less relevant to policymakers.[21] But why does it matter if academic political science has become increasingly irrelevant to the policy world? *New York Times* columnist Nicholas Kristof ignited a forest fire in the groves of academe when he lamented that the discipline, his "onetime love . . . seems to be trying, in terms of practical impact, to commit suicide."[22] Following on the heels of the US Congress's decision to restrict the National Science Foundation's political science funding on the grounds, in former House Majority Leader Eric Cantor's words, that "funds currently spent by the government on social science—including on politics of all things—would be better spent helping find cures to diseases," it is not surprising that Kristof's remarks singed the professoriate.[23] The subfield of international security studies can plausibly claim to save lives and so its marginalization is a self-inflicted wound at a time in which members of Congress demand greater relevance from the discipline.

I begin by documenting the decreasing policy relevance of political science. I then argue that this retreat from relevance has two primary sources. The first is the increasingly recognized tendency of academic disciplines, for reasons of professionalization and organizational interest, to become narrower and more internally focused. The second, less commonly discussed, is the responsiveness of the academy to the international security environment. War or intense international security rivalry more generally generate opportunities, incentives, and sometimes even imperatives for political scientists to transcend their disciplinary insularity, place a premium on relevance, and foster greater integration between academia and government. At such times there is optimism among social scientists about the compatibility of scientific rigor and concrete relevance. But there are also enduring tensions between those two values, which reemerge during times of peace and relative security. I trace this general pattern of the waxing and waning of policy relevance in the field of US security studies in the twentieth century and show that war and disciplinary dynamics account for it. I conclude by anticipating some likely objections and then offering some recommendations for how to reestablish the balance between rigor and relevance in political science.

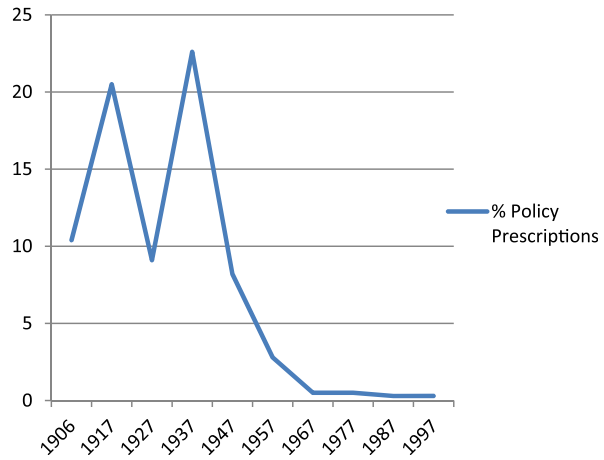
The Declining Relevance of Political Science

One important piece of evidence of the declining relevance of our discipline was furnished in Lee Sigelman's 2006 centennial *American Political Science Review* essay, "The Coevolution of American Political Science and the *American Political Science Review*." His data (refer to figure 1) demonstrates that the policy relevance of articles published in the *APSR,* measured in terms of whether they offered "policy prescription," has declined markedly in recent years.[24] This led him to conclude that "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."[25]

While one might dismiss Sigelman's codings as idiosyncratic, there are reasons to be confident that they measure real trends. As the editor of the journal, he was intimately familiar with the trends within its pages. And his results track well with assessments of trends in other scholarly journals. Figure 2 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 continuing through to the present.

Accompanying these trends has been a movement among many political scientists to regard economics as the epitome of rigorous social science and hold up its methods as a model for their discipline.[26] This increasing

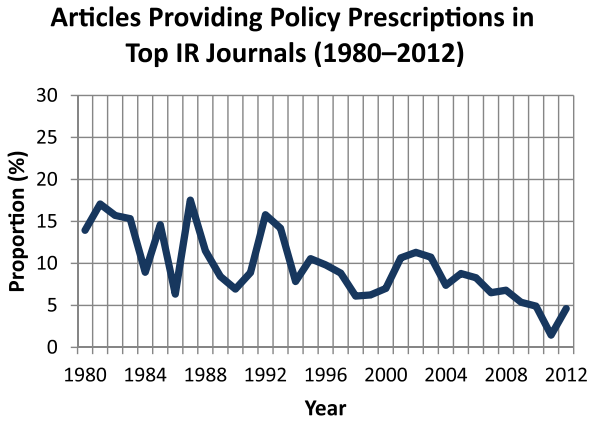
Figure 1
Percentage of policy-relevant articles in *APSR* (1906–2006)



Source: Sigelman 2006, 466, table 2, column 2 and 3. These judgments of whether an article offers a "policy prescription" are Sigelman's.

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Figure 2
Policy prescription used in top IR journals since 1980



Note: The Teaching and Research in International Politics journal coding project is described in Maliniak et al. 2011, 437-464. The data from which I generated these figures is available at http://www3.nd.edu/~carnrank/.

dominance of political science by such "scientific" approaches coincided with its decreasing relevance to policy issues, as figures 1, 2, 3, and especially 4 make clear. Political scientists Andrew Bennett and John Ikenberry explain that "the increasing technical specialization of the profession has made it more difficult for policymakers and citizens to understand and make direct use of political science research."[27] Some go further and argue that as the discipline has become even more wedded to these approaches, it has ceased to even try to be relevant to current policy concerns.[28] The problem is that many political scientists have internalized the notion that the hallmark of science is method (how you know something), rather than the substance of what is learned.[29] Why is this the case?

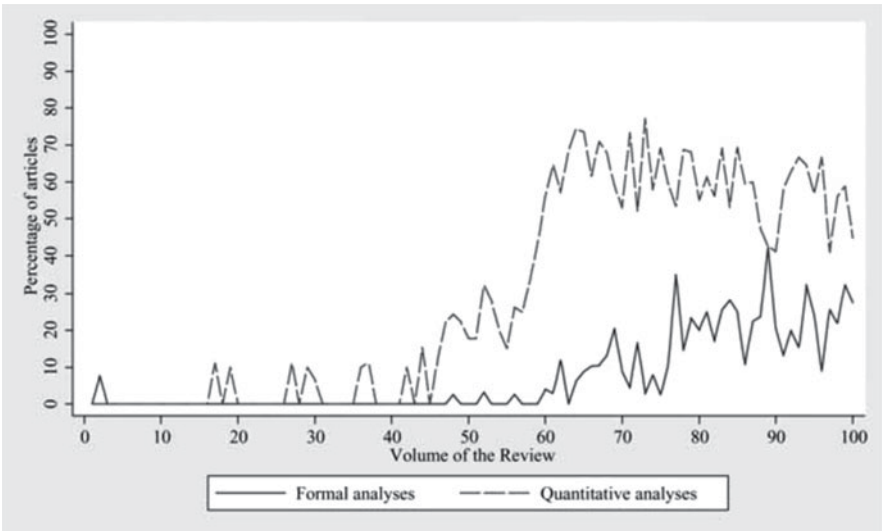
The Waxing and Waning of Relevance in Political Science

Social science, at least as it has developed in the United States since the early twentieth century, has tried to balance two impulses: to be a rigorous science and a relevant social enterprise. But tension between "rigor" and "relevance" is inevitable. As Thomas Gieryn explains, there is "in science, an unyielding tension between basic and applied research, and between the empirical and theoretical aspects of inquiry."[30] During wartime, the tensions between these two impulses have been muted, especially among those disciplines of direct relevance to the war effort; in peacetime, they reemerge and there are a variety of powerful institutional incentives within academe to resolve them in favor of a narrow definition of rigor that slights relevance.

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

The international security environment—among the most intense of external stimuli[31]—can overcome internal disciplinary tendencies during periods of war or heightened threat and foster cooperation between academia and government.[32] This logic follows from the substantial literature on war's effect on domestic politics.[33] War solidifies relations between the government

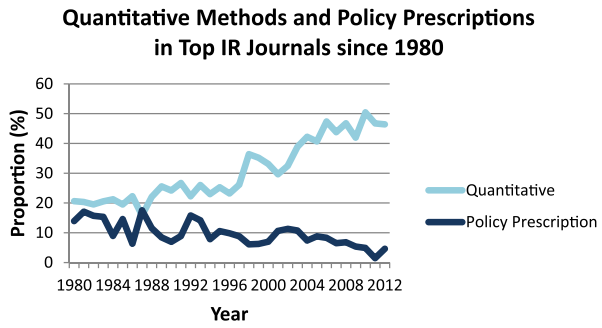
Figure 3
The dominance of "scientific" approaches to the study of politics



Source: Sigelman 2006, 469, figure 4.

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Figure 4
And the same trend recently in IR



Note: The Teaching and Research in International Politics journal coding project is described in Maliniak et al. 2011, 437-464. The data from which I generated these figures is available at http://www3.nd.edu/~carnrank/.

and other elements in society.[34] There are two specific mechanisms that lead to higher levels of wartime cooperation between government and academia. First, the need for wartime expertise leads policymakers to look to universities for natural and social science knowledge that could further the war effort. Second, a common sense of threat fosters a rally-around-the-flag effect and stimulates increased patriotic sentiment that affects even professors.[35] This sentiment tilts the balance of opinion within the disciplines in favor of a broader definition of rigor that does not exclude relevance.[36]

. . . and Peace Weakens Them

Based upon this wartime experience, there is initially optimism among social scientists about the long-term compatibility of rigor and relevance.[37] But there are also tensions between those two objectives that peacetime allows to re-emerge. Left to their own devices, academic disciplines tend to resolve these tensions by becoming narrower and more internally focused.[38] While a variety of different factors play some role in the peacetime decline in policy relevance among social scientists,[39] I focus on the process of "normal science" and the impact of institutional dynamics—both vested interests and institutional self-image. Organizational interest explains 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.[40]

The first logic for the decreasing relevance of social science flows from the dynamics of the scholarly enterprise itself. Given the limits of individual human cognition, it is often only through an intellectual division of labor that science can progress.[41] Philosopher of science Thomas Kuhn explains how growing specialization advances normal science: "By focusing attention upon a small range of relatively esoteric problems, the paradigm forces the scientist to investigate some part of nature in a detail and depth that would otherwise be unimaginable."[42]

However, this progress comes at the cost of the increasing isolation of the various specialties from each other and from society as a whole. In process similar to that through which Max Weber feared that modern rationalism would construct the "iron cage" of bureaucracy, the process of normal science has fostered an intellectual withdrawal from policy relevance among the social sciences.[43] Its hyper-fragmentation of knowledge now makes it difficult for even scholars in different disciplines to understand each other, much less policymakers and the general public.[44] The result is that many academics no longer deal with issues of interest to broader society.[45] In other words, the development of modern science itself makes it less directly applicable to concrete problems as it becomes more specialized.[46]

Second, traditional theories of organizational behavior would attribute the decreasing relevance of academic social science to the fact that universities, like most other complex organizations, seek autonomy, the reduction of uncertainty, and more resources.[47] When these goals conflict, organizations almost always prefer the first. Disciplinary organizational interest can encourage scholars to separate themselves from non-specialists by using language and approaches that are inaccessible to the laity.[48] Many social science disciplines such as political science deal with issues that are not otherwise inherently difficult for educated laypersons to engage. To maintain their autonomy and claim to expertise they need to construct higher barriers to entry. Sophisticated social science methods (modeling, statistics, or abstruse jargon) offer an ideal barrier to entry for the non-professional because they take considerable investment in time and effort to learn. 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 also fostered by disciplinary vested interests.[49] A reinforcing set of organizational incentives are "sunk costs" and the resulting "law of the instrument" mentality, which lead many scholars who invest time and intellectual capital in learning these sophisticated techniques to try to amortize this investment by either choosing only questions amenable to them or forcing questions that are not into that template.[50] Eschewing policy engagement is also a way for social scientists to avoid unwonted political controversy.[51]

Finally, a different, but complementary, organizational-culture explanation for the retreat from relevance in social science involves how academic disciplines define rigor and the incentives individual members of a discipline have to adopt similar approaches to each other. One of the hallmarks of science is its ability to measure causes and effects precisely, especially in mathematical

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terms, so the use of mathematics becomes the technique of choice.[52] It becomes ubiquitous because, as Harold Wilensky explains, "in modern societies, where science enjoys extraordinary prestige, occupations which shine with its light are in a good position to achieve professional authority."[53] The result is that most of the professional incentives academics face today lead them write for each other and pursue disciplinary agendas, rather than write more accessibly and address issues of broader import.[54]

One mechanism for the privileging of technique over relevance is the process of academic peer review, which fosters "homogenization of opinion."[55] Through that process, moreover, even a small group of scholars committed to a narrow definition of rigor can have a disproportionate influence on the development of the discipline.[56] The initiation of peer review in the *APSR* in the early 1960s had this result.[57] Sigelman confirms that this process "makes 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. The end product may be a wide array of narrower articles—greater diversity at the price of even greater fragmentation."[58]

While the professionalization of a discipline and its increasing irrelevance to concrete policy issues is neither historically nor logically inevitable, there nonetheless seems to be an elective affinity between these two trends.[59] First, the increasing emphasis on quantitative research has led to narrower research questions that are often too abstruse or obscure to be relevant to policy.[60] Second, many proponents of the scientific study of politics believe that advocacy of particular policies is incompatible with scientific objectivity and so avoid it.[61] Third, many pressing policy questions are not readily amenable to the preferred methodological tools of social scientists.[62] For example, while statistics and survey research are useful to policy makers for gauging public opinion, the cutting-edge mathematical tools of Systems Analysis proved useless, if not counterproductive, as a basis for formulating higher-level US national security policy.[63] Finally, even when the results of these approaches are relevant to policy questions, they are often not presented in an accessible way for policymakers or the broader public.[64] In other words, there are good reasons for believing that the effort to make political science more "scientific" would tend to make it less policy relevant.

The Waxing and Waning of Security Studies in American Political Science

The history of the last century shows that academic political science has been more receptive to balancing methodological rigor with practical relevance in wartime than was the case in peacetime, particularly in subfields that could directly contribute to the war effort. Process evidence shows that once the war is over or the threat recedes, disciplinary dynamics once again exacerbate tensions between rigor and relevance, with the result that political scientists tend to favor a narrow definition of the former that excludes the latter.

Over the last century war has fostered closer ties between the US government national security community and the academy. Indeed, RAND consultant Warren Weaver explains that this "cooperation [between the two] has resulted only from the pressure and necessity of war."[65] In times of war the willingness of American academics to contribute directly and indirectly to the war effort increases markedly. As the data in figure 1 confirms, the policy relevance of political scientists spiked during the First and Second World Wars and gradually declined during the Cold War.

In the face of what was widely recognized as a total war, the Wilson administration mobilized broad swathes of the university community to support the war effort. Academics snapped to attention and were among the First World War's greatest boosters; many were eager to serve directly in government.[66] One motive was the Progressive Era "service ideal." But self-interest was also a factor. As Richard Hofstadter notes, "the war itself, ironically, raised many [intellectuals] to heights of influence as no domestic issue could. Historians and writers were mobilized for propaganda, and experts of all kinds were recruited as advisers. Military Intelligence, Chemical Warfare, and the War Industries Board swarmed with academics, and Washington's Cosmos Club was reported to be 'little better than a faculty meeting of all the universities.'"[67] The most extensive effort to marshal academic talent for the war was the Inquiry, whose 150 scholars produced almost 2,000 reports, drafted nearly 1,200 maps, and some of whom traveled to Paris after the Armistice to serve as advisors to the American delegation during the post-war negotiations.[68]

As with so many other aspects of American society, the mobilization of the country's intellectual resources for the Second World War was even more extensive. World War II was revolutionary not only in terms of innovative new military technologies (especially the atomic bomb) but also the role of civilian experts played in fostering them and formulating the strategies for how to employ them.[69] Historian Barry Katz notes that the social sciences were called to the colors in a major way:

> World War II has been called the physicists' war in deference to the unprecedented recruitment of the scientific community to government service and the historic pooling of intellectual effort that followed . . . . It was also, however, the economists' war—and the sociologists', the historians', the philosophers' war—even if, viewed from the perspective of the human sciences, it ended less with a bang than a whimper. The social sciences and the humanities were called to arms in an

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intellectual mobilization that pressed into government service a community of academic scholars that was no less extraordinary than that of their scientific counterparts.[70]

The area of the war effort to which scholars—particularly historians and area specialist social scientists—contributed the most was intelligence. As Harvard diplomatic historian William L. Langer recalls, "when General William J. Donovan in August 1941 organized what later became the Office of Strategic Services, his chief aim was to bring into government service scholars who, in addition to their specialized knowledge of foreign countries, were trained in particular disciplines and thoroughly grounded in the methods of assembling, selecting, evaluating, and presenting evidence."[71] The locus for this was the "Research and Analysis Branch," the O.S.S.'s analytical heart. As an intelligence organization it was remarkable for its ideological diversity, including some famous Marxists among its analysts such as Herbert Marcuse, Franz Neumann, and Otto Kirchheimer.[72] By the war's end, the Research and Analysis Branch of the O.S.S. would employ nearly 700 people in Washington and around the world, most of whom were scholars drawn from social science fields like political science, economics, geography, psychology, and even archeology.

Building upon this wartime experience, the Department of Defense and the services promoted policy-relevant social science research early in the Cold War by establishing more permanent connections on university campuses. The post-war return of regional specialists to campus made it possible to do so through "area studies" research centers.[73] Soviet Studies would become the model for many of them.[74] The most prominent of which was Harvard's Russian Research Center (RRC), established thanks to an Air Force grant to study the psychological effects on Russia of nuclear strikes as part of Air Force targeting.[75]

The collapse of *détente* with the Soviet Union and the rekindling of the Cold War in the late 1970s and early 1980s ushered in what one scholar dubbed a "renaissance" in security studies: its "marriage" with social science promised to raise "its standing with in the academic world" after their post-Vietnam estrangement.[76] While the deteriorating international security environment was the key factor driving this renewed academic interest in policy-relevant scholarship on topics such as deterrence theory, nuclear strategy, general purpose forces, US grand strategy, and the interface between security studies and the disciplines of history and international relations, there was still concern among individuals such as Ford Foundation President McGeorge Bundy that "the field of international security was not deeply rooted in the academic world."[77] To deepen those roots, he and other philanthropic leaders provided support for the establishment and growth of a number of centers devoted to brokering the nuptials of national security policy and social science.

Finally, Secretary of Defense Robert Gates' 2008 Minerva Initiative is evidence of the dissatisfaction among policymakers with purely in-house research and analysis and also the desire to stimulate useful scholarly input into some of the most pressing post-Cold War security issues. It clearly indicated a commitment to rebuild the bridges between the Beltway and the Ivory Tower by funding policy-relevant social science research on issues such as the consequences of cultural and religious change; the ideologies of terrorist groups; military transformation in China, the national security consequences of environmental change; new theories of deterrence; and the dynamics of failing states.[78] In the wake of 9/11 and wars in Iraq and Afghanistan another new cohort of security scholars has emerged, committed to employing the cutting-edge tools of political science but also applying them to pressing policy problems such as the war on terrorism and nuclear proliferation.[79] Many of these individuals either came to academia after serving in Government or the military or would take time out of their scholarly careers to work directly in the policy world. Their apparent success has led to renewed optimism that a stable balance between rigor and relevance can finally be established.[80] Once again, war or intense international security competition opened a door to greater academic willingness to cooperate with government.

But the flip side of my argument is that we ought to see a decline in such cooperation in peacetime or when the international security competition becomes more benign. Richard Betts attributes to the intellectual impulse to make war's study a "science" this peacetime waning of support for policy-relevant security studies.[81] This pattern was clearly evident in periods of peace over the last century.

With the end of the First World War and the country's more general retreat into isolationism, the role for academics—or anyone else for that matter—in foreign and defense policymaking, shrank dramatically.[82] The "new science of politics movement" took hold in political science in the early 1920s and "for the first time there emerged a school of thought which argued that the discipline should abandon extra-scientific activities and concentrate on building the systematic body of knowledge implicit in the term 'science.'"[83] Similarly, in the waning days of the war in Vietnam and the beginning of *détente* and the opening to China, there was a marked change in the attitudes on campuses about academics participating in national security policymaking. As historian David Engerman notes, there emerged "a new kind of interface among the policy, public, and academic spheres, one that was more individual and less institutional."[84] Finally, it was growing post-Cold War calls by other political scientists to "abolish the subfield of

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security studies" entirely that prompted Betts to make the case that security studies should survive.[85]

The rise and fall of Yale's Institute for International Studies (IIS) illustrates how the peacetime waning of academic interest in security studies is caused by resurgent tensions between a narrow definition of rigor and broader relevance within political science. In the period between its establishment in 1935 and its collapse in the early 1950s and subsequent relocation to Princeton, IIS for the first time in American history assembled an impressive group of scholars of international affairs who produced some of the most influential national security scholarship of their era. Among those were Nicholas Spykman, Arnold Wolfers, Frederick Dunn, William T.R. Fox, Bernard Brodie, Percy Corbett, Klaus Knorr, Gabriel Almond, Harold Lasswell, and William Kaufmann. The Institute also founded the journal *World Politics*, which for a time was the leading journal in the field.[86] Both the Institute's and *World Politics'* eventual fate proves the rule about the tenuous peacetime standing of national security affairs in academe.

The story of IIS's short career is comprehensively told in the archives of the Rockefeller Foundation, which was the Institute's major benefactor during its roughly 15 year existence. Yale's initial effort to secure Rockefeller Foundation support for a program in international law and international relations in 1931 did not pan out.[87] One factor that seemed to play a role in the Foundation's lack of enthusiasm for the original Yale program was the perception that it, in the words of a later IIS document, "was generally non-scientific and was concerned more with the description of unique events than with revealing the common elements in classes of events."[88] In short, rigor trumped relevance in the interwar period.[89]

A subsequent Yale proposal that sought to balance a more systematic social-scientific approach with direct policy relevance met with success. And in May of 1941, on the eve of the US's formal entry into the Second World War, Rockefeller approved an additional three year grant of $51,500 for the Institute.[90] By now, the Institute was beginning to bear fruit. IIS's highest impact publication was Spykman's book, *The United States and the Balance of Power,* which came out after Pearl Harbor in early 1942 to much critical acclaim.[91] Yale contributed disproportionately to the war effort, primarily through the service of its faculty in the O.S.S. Yale's president Charles Seymour had previously served in the Inquiry, personifying the direct link between these two major efforts to bring scholars into government during war-time.[92]

In 1943, the Institute took stock of its role, and that of other university-based research centers in doing policy-relevant scholarship. Dunn, who had taken over from Spykman as director, penned an optimistic 10-page memorandum on the topic.[93] But by January of 1944, he had become more pessimistic, observing that within universities "the conditions sometimes found inhibit rather than encourage creative activity" in this area.[94] Seymour's successor as president, A. Whitney Griswold, was lukewarm in his support for the Institute, due not only to personality clashes but also academic institutional biases.[95] Recall that the core philosophy of the Institute's work was, in Dunn's words, "the development of a more disciplined type of policy analysis."[96] But its approach cut against the grain of both the traditional scholarly approach to research (the Institute sponsored a lot of team projects as opposed to supporting the work of individual scholars) and it paid scant attention to traditional academic disciplinary boundaries. It was not just Griswold who had reservations about IIS; Yale's political science department also launched a campaign to de-emphasize international relations in favor of "'domestic' political science."[97] By 1954, Yale had lost six of the core IIS members and its main activities—policy research and *World Politics*—to other institutions. Wolfers remained at Yale and tried to make the best of the situation. But even his optimistic gloss on these developments to Rockefeller conceded that policy-relevant research had been severely curtailed at Yale as a result.[98]

With the support of a friendly foundation, many of the IIS principals decamped to Princeton and set up shop again, bringing *World Politics* with them. For a brief time, Princeton became the key link between the new RAND Corporation and those academics still interested in doing policy-relevant research.[99] But the Princeton Center for International Studies was hardly a model of successful integration of national security studies into the disciplinary departments either.[100] In short, disciplinary dynamics and other academic trends made the peacetime place of national security studies in the academy tenuous, even during the supposed Golden Age of such cooperation in the early Cold War. Such a pattern does not auger well for the long-term prospects of current efforts to marry cutting-edge methods with policy relevance.

Response to Objections

Before concluding, I want to anticipate four potential objections to my argument. First, the seeming success of the new cohort of younger security scholars committed to employing methodologically sophisticated social-science techniques for analyzing security-related issues could support the view that rigor and relevance are "not necessarily in opposition to one another."[101] But there are theoretical and historical grounds for pessimism that they have finally achieved a stable balance between them. The inherent tensions between basic and applied research can be managed but never fully eliminated. And previous periods of optimism about maintaining the balance inevitably gave way to a narrow definition of rigor assuming pride of place. When that happens, and scholars appear to embrace these approaches for disciplinary reasons,

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rather than because they can help us answer substantively important questions, they are at best of "mixed" benefit to policymakers.[102] A cautionary tale is the sophisticated social science work of the CIA-sponsored Political Instability Task Force, which some members of the Intelligence Community found useful but which struck others as "very rigid and self-righteous" and regional experts "disputed its findings and did not think its output was credible."[103]

A second objection might be that I have overstated the problem by ignoring the proliferation of new publication venues such as blogs (*The Monkey Cage, Political Violence @ A Glance, Duck of Minerva,* and *Daniel Drezner.com*) and professional journals such as *Perspective on Politics* that provide space for scholars to present more broadly accessible scholarship.[104] While these developments are certainly encouraging and may ultimately foster greater scholarly relevance, it is by no means certain they will. For every *Monkey Cage* that gets picked up by the *Washington Post*, there are large numbers of other blogs that do little more than increase the "noise" to "signal" ratio.[105] And as members of one group blog recently discovered, the receptivity of traditional academic organizations to them remains limited.[106] We also lack systematic evidence of blogs' impact upon the policy community. Finally, as anyone who has ever sat on a political science tenure committee will attest, publication in the "big three" disciplinary journals (*The American Political Science Review*, *The American Journal of Political Science*, or *The Journal of Politics*) still carries more cachet than in more accessible journals. As the TRIP journal survey project demonstrates, the trends in leading international relations journals are also moving away from relevance.

A third objection could be that I have mischaracterized the whole relevance problem to begin with. One critique might be that I have overlooked the division of labor that has emerged between those who want to do policy-relevant work and those who maintain a purely academic focus.[107] But as the late Donald Stokes puts it, "in academic research circles . . . the ideal of pure inquiry still burns brightly."[108] A key component of our modern image of science is the increasingly rigidly distinction between "basic" and "applied" research: the former pursues knowledge for its own sake while the latter seeks solutions to specific problems.[109] Basic research is necessarily driven by its own internal agendas and criteria so as not to contaminate the process of science with normative or practical considerations.[110] As the social sciences increasingly emphasized their "scientific" character, they became more disengaged from practical affairs suggesting that the division of labor often gives way to the marginalization of relevance.[111]

A different version of this argument might maintain that the pursuit of basic research produces applied knowledge via a "trickle down" process.[112] In this view, the progress of "pure" science naturally confers policy benefits in much the same way that some believe that economic growth increases the wealth of the poorest, even if wealth is quite unevenly distributed.[113] Confidence in this reinforces many scholars' inclination not to worry about whether their work is directly relevant.[114] But this image of basic research proceeding on its own without reference to practical problems is a caricature of how, in fact, the two realms relate to each other in practice, in which concrete problems often spur theoretical advance.[115] Moreover, the Department of Defense, the largest supporter of basic research in the early Cold War era, undertook a study of the relationship between basic and applied research—Project Hindsight—and concluded that had it waited for the former to produce useful results, the US military would have had far inferior weaponry.[116] We should therefore not assume that we can focus on basic science and assume useful application would automatically result.

Finally, the notion that science and politics ought to be kept separate on moral grounds is widely shared among scholars across the political spectrum. One critique of policy relevance is that it compromises scholars' ethical principles because policy is often immoral.[117] A related reservation is that policy engagement threatens scholars' scientific integrity.[118] Some even fear that American social science is so deeply infused with residual policy agendas that even today's "scientific" political science is corrupted by it.[119] While there is no doubt that scholarly engagement with policy poses some moral conundrums, critics ignore a different set of moral issues that arise when they eschew engagement with practical affairs. Efforts to resolve this tension by simply throwing up a Chinese Wall between our twin obligations to both science and the wider world inevitably slights one obligation in favor of the other.[120] Stephen Van Evera ties this ethical obligation back to the health of the discipline, arguing that a broader ethical self-reflection is essential for the health of the discipline because "social science operates largely beyond accountability to others. Institutions and professions that face weak accountability need inner ethical rudders that define their obligations in order to stay on course. Otherwise they risk straying into parasitic disutility. Social science is no exception."[121]

Conclusions

In sum, the easiest case to make is that today's Ivory Tower/Beltway gap is detrimental for policymakers and society as a whole. Policymakers need academic expertise because they have to make decisions about issues and areas of the world in which they often do not have deep expertise.[122] They look to the academy for the raw data—whether quantitative or historical—that they use in decision-making. They also depend upon the social sciences for the theories they use to analyze and make sense

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of this data. Since policymakers implicitly use theory in analyzing situations and assessing the various alternatives they have, it makes sense for them to be stated explicitly and analyzed systematically, which is the comparative advantage of social scientists. Academics likewise have the time to develop greater depth of knowledge in issues and regions than most policymakers can. The institution of tenure allows them freedom to explore controversial issues and take unpopular stands, at least in theory. Finally, university-based scholars also have fewer vested interests in certain policies and programs than do policymakers, though that is not to deny that they have their own institutional biases.[123]

To be clear, my argument is not that scholars would make better policy than bureaucrats and elected officials. Academics have their own biases and different agendas.[124] But their voices should be heard in policy debates for precisely that reason. Our system depends upon the successful functioning of the "marketplace of ideas" and the system of checks and balances in which individuals and groups with various strengths and weaknesses and offsetting biases, contribute to the larger policy debate, thereby compensating for each other.[125] One can point to instances—the war in Vietnam and the recent Iraq War—in which had the majority consensus of scholars in academia influenced policy, the country's strategic interest would have been better served.[126]

Less widely recognized, and perhaps more controversial given the prevailing sentiments in the Academy in favor of a sharp distinction between "science" and "policy," is my contention that the gap is ultimately bad for the generation of new knowledge. But as Peter Ordeshook explains, "the practical application of theory and experiences feeds back on our awareness of theoretical inadequacies and on our identification of phenomena that warrant generalization and theoretical understanding."[127] Moreover, as we have learned from the recent NSF controversy, even the most sophisticated social science will be judged in the final analysis by what it tells us about things that affect the lives of large numbers of people and which policymakers therefore seek to influence and control.[128]

Finally, while academics spend a lot of time thinking about their ethical obligations to the norms of their disciplines and their relations with students and colleagues, they talk less about the proper relationship between science and policy. Tellingly, the ethical obligation of political science to society has been a recurrent theme in presidential addresses at the American Political Science Association, implying that from the perspective of eminent senior scholars, the discipline periodically needs to be reminded of it. Most recently, Harvard's Robert Putnam declared in 2002 that "attending to the concerns of our fellow citizens is not just an optional add-on for the profession of political science, but an obligation as fundamental as our pursuit of scientific truth."[129]

There is a middle ground between policy advocacy, journalism, and Fleet Professorship on one side, and scientism, methodism, and the Cult of the Irrelevant on the other. One way to find it is through a commitment to problem- rather than method-driven research agendas.[130] The discipline should likewise embrace a broader definition of rigor that encourages finding the most reliable answers possible to questions the rest of society regards as important using methods appropriate to them.[131] The process of peer-review is invaluable in connecting the wisdom of crowds with an individual scholar's research but we should consider broadening the pool of potential reviewers to include policymakers and other outside consumers as well. Think tanks once played an important role as "transmission belts" from the Ivory Tower to government, rather than simply as holding pens for out of work politicians, and at least some of them today should again take up that role. Finally, we should consider rewarding scholars and ranking departments based upon not only scholarly metrics but also in terms of whether they speak to broader concerns of society.[132] That is how the discipline of political science can contribute to better policymaking, produce better scholarship, and fulfill its ethical obligation to the rest of society. It is only within such an approach to political science that policy-relevant security studies can not only survive, but thrive.

Notes

1 Jervis 2004, 101, and Schelling 2004, 137. See also Kaplan 1983, 336.
2 George 1993; Zelikow 1984; Kruzel 1994; Lepgold 1998; Feaver 1999; Lepgold and Nincic 2001; Jentleson 2002; Walt 2005; Lieberthal 2006; Nye 2008; and Jentleson and Ratner 2011.
3 Nye 2009.
4 See, for example, Ross 1991, 400.
5 Karl 1974, x. Also see Crick 1960, 153 and Somit and Tanenhaus 1967, 2–46, 80–89, and 134–44.
6 Skeptics include Nitze 1958; Vogel 2006; Whiting 1972; Trachtenberg 2010; Krasner 2009; and Wilson 2007. The most comprehensive scholarly dissent is Kuklick 2006.
7 Maliniak et al. 2011, 439 and 450–54.
8 Avey and Desch 2014, 227–46.
9 Advisory Committee on the Management of Behavioral Science Research in the Department of Defense 1971, 2.
10 Van Evera 2010; Mead 2010; Shapiro 2005, 178; and 212; and Mearsheimer 2004.
11 Wildavsky 1972.
12 Walt 1999, 9. See also Lyons and Morton 1965, 7 and 298–99.
13 For a cautionary note on the limits of contemporary academic economics, see Krugman 2009.

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14 A good example is Mansfield and Snyder 2005, a précis of which was published in *Foreign Affairs* as Mansfield and Snyder 1995. Their argument was picked up in the National Intelligence Council 2012.
15 Confidential e-mail from former senior Intelligence Community official, March 21, 2014.
16 Brodie 1949.
17 Frieden and Lake 2002, 137–38. Also see Martin 1999, 76.
18 Walt 1991, 212.
19 Brodie to Holland, July 13, 1966, Brodie Papers, Box 5, folder "Army War College," UCLA Library Manuscripts Room. I thank Marc Trachtenberg for this source.
20 "Excerpt from "Thomas Schelling's Interview: 2. Why Were You Initially Drawn to Game Theory?" at http://www.gametheorists.com/Interviews/schelling.html.
21 Ricci 1984, 296.
22 Kristof 2014.
23 Klein 2013. Also see, Mole 2013. For push back see Drezner 2014.
24 Bennett and Ikenberry 2006, 652, table 2, which shows a drop from a high of about 25 percent of articles aspiring to near-term relevance in the early 1950s to less than 5 percent in the mid-1960s.
25 Sigelman 2006, 467.
26 Hirshman 1970, 19. Also see, Tullock 1972, 325.
27 Bennett and Ikenberry 2006, 653.
28 Balz 2009, 12.
29 Centeno 1993, 312.
30 Gieryn 1983, 787. Also see Smith 1994, 13.
31 The effect of war is greatest on those parts of the social sciences such as security studies with a plausible connection to military affairs, but external developments in other areas have also fostered disciplinary engagement with policy. Two examples are Roosevelt's Brains Trust and Johnson's Great Society. See Tugwell 1968 and Aaron 1978.
32 This logic is similar to Posen 1984, 37 and 80.
33 The classic statement is Hintze 1975, 178–215. Also see Desch 1996, 237–68; Porter 1994; Perret 1989; and Sherry 1995.
34 Gruber 1975, 5.
35 On the mobilizing effect of wartime nationalism, see Posen 1993, 80–124, and Herbst 1990, 122.
36 For a similar dynamic, see Art 1973, 467–90.
37 Farr, Hacker, and Kazee 2006, 582.
38 Smith 1994, 71.
39 Shils 1949, 219–221, attributes the decreasing interest of social science in policy to a number of factors.
40 The humanities have experienced a similar trend as well. See Shapiro 2005, 2 and Lehman 1991.
41 Tullock 1972, 319. See also Eimer 1887, 1.
42 Kuhn 1970, 21 and 24.
43 Weber 1958, 181–83. For discussion of Weber's attribution of this dynamic to universities see Mommsen 1984, 444.
44 See Lafforgue 2011, 15 and Habermas 1971, 76.
45 Bender 1993, 46.
46 Multhauf 1959, 643.
47 For evidence that universities behave in typical bureaucratic fashion, see Clark 1966, 283–91. See also Menand 2010, 15 on their conservative orientation.
48 Clark 1966, 285–86. For a similar sentiment, see Chomsky 1968, 271.
49 Quoted in Bernstein 2001, 7. A compelling explanation is Olson 1982, 41–47.
50 Stokes 1997, 171, n. 31.
51 Ross 2003, 209 and 219. See also Ross 1991, 399–400.
52 Hofstadter 1963, 339. See also White 1943, 375.
53 Wilensky 1964, 138. See also Trilling 1990, 182.
54 Jacoby 1987, 6.
55 Bernstein 2001, 122.
56 Skocpol 2003, 410. See also Gieryn 1983, 781.
57 Patterson, Ripley, and Trish 1988, 920; and Sigelman 2006, 463, n. 3.
58 Sigelman 2006, 475.
59 Ross 1991, 393.
60 For discussion of the growing disconnect between theory and empirical research in international relations, see Braumoller and Sartori 2004, 129–51 and Mearsheimer and Walt 2013, 427–457.
61 Noted in Lynd 1939, 17 and 242. See also Proctor 1991, 92ff.
62 Avey and Desch 2014, 227–46.
63 Schlesinger 1963, 298.
64 For evidence that the discipline is acknowledging the problem, see Cain and Vavreck 2012 and Lupia 2014. Some, for example Hayward 2010, argue that the problem is deeper than just communication.
65 Collbohm and Weaver 1948, 5–6.
66 Gruber 1975, 44. See also 46, 82, and 172.
67 Hofstadter 1963, 211.
68 Gelfand 1963, x.
69 Brodie 1964, 11–12.
70 Katz 1989, xi.
71 Langer 1948, 43–44.
72 Katz 1989, 29. See also 168 and Neumann, Marcuse, and Kirchheimer 2013.
73 Winks 1996, 114–15 and 384. See also Bundy 1964, 2-3; Shils 1949, 230; and Lyons 1969, 112.
74 Engerman 2009, 14.
75 Hershberg 1993, 413.
76 Walt 1991, 211.
77 Miller 2001, 6.

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78 Mahnken 2010, 10–12.
79 Representative, but by no means exhaustive, examples include Biddle, Friedman, and Shapiro 2012; Lyall and Wilson 2009; Kroenig 2009; Furman and Kreps 2010; and Horowitz, 2010.
80 See the generally optimistic remarks in McMurtie 2014.
81 Betts 1997, 10–11.
82 Lyons 1969, 29–30.
83 Somit and Tanenhaus 1967, 139.
84 Engerman 2009, 261.
85 Baldwin 1995, 135.
86 A good history of the Institute is provided by Frederick S. Dunn, "The Growth of the Yale Institute of International Relations," November 7, 1950 at http://rockefeller100.org/archive/files/ccfe7e7718-d3606ab50c8cdd2d88c37b.pdf. For discussion of IIS and *World Politics*, see Fox 1962, 12–19. How they segued into the subsequent wartime mobilization of the social sciences is explained in Ekbladh 2011/12, 107–41.
87 The proposed program is described in "Research in International Law and International Relations at Yale University," May 1931 at http://rockefeller100.org/archive/files/17f0a3c078180850ebeac401b077ac1a.pdf.
88 Frederick S. Dunn, "The Growth of the Yale Institute of International Studies," November 7, 1950 at http://rockefeller100.org/archive/files/ccfe7e7718d3606ab50c8cdd2d88c37b.pdf.
89 For evidence of intellectual trends working against relevance during this time, see Lynd 1939.
90 Rockefeller Foundation, "Minutes of the Rockefeller Foundation regarding the study of international relations at Yale University," 5/16/41 at http://rockefeller100.org/archive/files/65eb13cb2-b7aca8eafcf8e4e8e78ca05.pdf.
91 Rockefeller Foundation, "The United States and the balance of power," http://rockefeller100.org/archive/files/9d86b9c00f134afe11bcb334c3ed6338.pdf.
92 Winks 1996, 29.
93 Frederick S. Dunn, "The Place of University Research Agencies in International Relations," December 23, 1943 at http://rockefeller100.org/archive/files/1a99e311afbff54136f56bcfddc27fe3.pdf.
94 Frederick S. Dunn, "The Position of a Creative Research Organization within a University," January 8, 1944 athttp://rockefeller100.org/archive/files/59f12a209994268f335bb5a2460cb568.pdf.
95 Joseph H. Willits, "Interview with A. Whitney Griswold regarding research at Yale University," March 12, 1951 at http://rockefeller100.org/archive/files/0bb26064370a6261d891c6810dd9e052.pdf.
96 Dunn, "The Growth of the Yale Institute of International Studies," 8.
97 Roger F. Evans, "Interview with David N. Rowe regarding international relations vs. political science," October 2, 1951 at http://rockefeller100.org/archive/files/8679dfe2866bb4a1c589b1-de63291b67.pdf.
98 Arnold Wolfers , "Letter from Arnold Wolfers to Joseph H. Willits," April 2, 1954, 2 at http://rockefeller100.org/archive/files/367c6e651593916e1c1e334914b10f06.pdf.
99 Kaplan 1983, 49–50; and Kuklick 2006, 87.
100 Lyons and Morton 1965, 130–34.
101 King, Keohane, and Verba 1994, 14–19.
102 Confidential e-mail from former senior Intelligence Community official, March 21, 2014.
103 Bates et al. 2010, 190–208.
104 Perspectives on Politics, however, is now ranked second behind The American Political Science Review and ahead of the American Journal of Political Science and The Journal of Politics among Political Science journals in terms of impact factor by Thomsen-Reuters at https://jcr-incites-thomsonreuters-com.proxy.library.nd.edu/JCRJournalHomeAction.action?SID=A2-x2BowAyI0o68i1HheIZJm8PzoClTXG2iAz-18x2djx2FL9UgxxGx2BsIPrQKQ8eH2vwx3Dx3-Dwkbx2FxxCex2FJVI2i3DUQGlhPQx3Dx3D-YwBaX6hN5JZpnPCj2lZNMAx3Dx3D-jywguyb6iMRLFJm7wHskHQx3Dx3D&SrcApp=IC2LS&Init=Yes&wsid=3Djf7skdA3wQr1ABCFW.
105 Silver, 2012, 13.
106 Holpuch, 2014.
107 Gartzke 2011.
108 Stokes 1997, 135
109 Proctor 1991, 4–5.
110 Ibid., 4–5.
111 Schumpeter 1949, 346, and Bender 1993, 5–6 and 131.
112 For example, see Harold D. Lasswell cited in Crick 1960, 181. See also Schilling 1962, 295.
113 Walt 2005, 25. For an example of such optimism, see Bennett and Ikenberry 2006, 651.
114 Schilling 1962, 294.
115 Stokes 1997, 24.
116 Sherwin and Isenson 1967, 1575. See also Sapolsky 1990, 79.
117 See Machiavelli 1952, 84. See also Norton 2004, 73; Chomsky 1968; Cohn 1987, 716.
118 Schaar and Wolin 1963, 148 and 150.
119 Oren 2003.
120 See the discussion in Carpenter 2012.
121 Van Evera 1997, 5. For discussion of this generic problem, see Wildavsky 1972, 509–20.
122 Kennan 1951, 38.
123 Merton 1945, 412.
124 On this last, see H-Diplo/ISSF 2010.

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125 Mill 1956, 19–66 provides the rationale for the marketplace of ideas. Checks and balances are outlined in *Federalist No. 51* in Hamilton, Madison, and Jay 1961, 322.
126 On the former, No Author 1964, 1 and 2. On the latter, Art, et al. 2002 and Security Scholars for a Sensible Foreign Policy 2004. This effort was discussed in Jackson and Kaufman 2007, 95–103.
127 Ordeshook 1995, 181 and Winks 1996, 425.
128 Mankiw 1990, 1659.
129 Putnam 2003, 250. Also see Huntington 1998, 4 and Easton 1969, 1051 and 1053.
130 George and Bennett 2005, 263–86.
131 Smith 2002, 201.
132 For a first cut, see Campbell and Desch 2013.

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How Can We Make Political Science Less Techno-Centric? Widen Rather than Narrow its Distance from the Government

Ido Oren

Michael Desch argues that American political science has become increasingly focused on technique and that the growing prevalence of mathematical and statistical methods has rendered the discipline irrelevant to policy makers. Desch's essay is open to several criticisms. It does not engage with relevant sociological thought such as Max Weber's and Karl Mannheim's theorization of the relationship between social science and politics. It seems to narrowly equate political relevance with advising policy elites as opposed to critiquing these elites and the social order they dominate. The essay also contains some factual inaccuracies; the suggestion that a "majority consensus" (386) of political scientists opposed the Vietnam War is dubious at best.[1]

Here I want to raise another issue. Desch implies that political scientists embrace quantitative techniques largely for "internal, disciplinary reasons" (378). He depicts technical specialization as an attempt by political scientists to increase their autonomy and protect their turf from encroachments by the government or by political interests. For Desch, the government is a force for "relevance" and whenever it "mobilizes" political science—which occurs mainly in times of war—the emphasis on technique recedes. But "once the war is over or the threat recedes, disciplinary dynamics once again" push the discipline toward technical specialization (382).

Without gainsaying the internal dynamics discussed by Desch (who rightly points out that trends of technical specialization are common to other academic fields), I want to suggest that he greatly underestimates the extent to which the valorization of statistical and mathematical methods in political science—rather than being emblematic of an estrangement between political science and the government—has actually been caused or at least reinforced by the government. The government has contributed significantly to the growth of technical political science, especially in the context of war and national security threats. Government funding played a key role in the development and diffusion of data analytical and mathematical techniques.

Consider the National Science Foundation, which was created in 1950 as an outgrowth of the activities of the Office of Naval Research and whose stated purposes included "secur(ing) the national defense."[2] In the late 1950s political science came to be included in the social science program of the NSF, and it is hard to overestimate the impact of that inclusion on the methodological orientation of the discipline. The American National Elections Studies—a massive data gathering and analysis effort that, more than any other project, symbolizes and strengthens the prestige of quantification in political science—depends almost exclusively on NSF funding.[3]

Desch makes two passing references to recent Congressional restrictions on NSF funding of political research. He implies that these cuts are a bad thing but, if he is truly interested in curbing the technical specialization of political science, Desch should consider supporting the Congressional budget cutters, for few events have contributed to the privileging of quantitative and mathematical approaches to political science as much as the earlier Congressional decision to add the discipline to the mandate of the NSF.

The momentum toward quantification in political science received crucial support from other government agencies, especially national security agencies and allied foundations who hoped that rigorous social science

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techniques would aid the government in prosecuting World War II and then the Cold War. Let me offer several examples to establish the plausibility of my claim. The mobilization of social scientists during World War II produced not only qualitative analyses by the likes of Herbert Marcuse (which Desch discusses) but also cutting edge quantitative methods and methodological training. The Morale Division of the U.S. Strategic Bombing Survey (created by President Roosevelt in late 1944) made major technical advances in survey research. Gabriel Almond was a member of the division and the methodological tools he acquired while participating in a survey of German attitudes after the Nazi capitulation helped him in his subsequent quest to place the study of foreign policy and comparative politics on a more scientific, i.e., "behavioral," footing.[4] Almond's mentor, Harold Lasswell, pioneered quantitative content analysis methods during World War II in his capacity as the director of the Experimental Division for the Study of War-Time Communication. Lasswell's unit was part of the government's intelligence apparatus and it served as a training center where young political scientists such as Karl Deutsch, Heinz Eulau, and Ithiel de Sola Pool acquired advanced methodological skills.[5] Pool went on to become a key member of MIT's government-supported Center for International Studies in the 1950s and 1960s, where he developed computer simulation techniques in the service of the government, including COMCOM, a model of communication in communist societies.[6] COMCOM represented but one of many government investments in computer simulation methods. The scandal-ridden Project Camelot, which "drew extensively" on the work of political scientist Harry Eckstein, aimed to develop (in the words of its official title) computer simulation "*Methods* for Predicting and Influencing Social Change and Internal War Potential."[7] Finally, government support facilitated the creation of the infrastructure for the quantitative study of world politics. In the 1960s and early 1970s the Department of Defense and NSF made sizable investments (at least by the standards of social science) in the development of quantitative data sets such as the World Data Analysis Program at Yale University and the Dimensionality of Nations project at Northwestern University.[8]

In the aftermath of the attacks of September 11, 2001, social science funding by national security agencies, which dried up in the 1980s and 1990s, has been resumed, culminating with the launching of the massive Minerva initiative by the Department of Defense in 2008.[9] Desch appears to expect Minerva to mitigate, temporarily at least, the tendency of political science to equate rigor with technique. But if the past is any guide, the tightening relationship between political scientists and the government may accelerate rather than slow down the technical specialization of the discipline.

I do not doubt the sincerity of policymakers who conveyed to Michael Desch their frustration with the limited usefulness of political science research. But the fact of the matter is that the policymaking community is at least in part responsible for this state of affairs. If Desch wants to make political science less focused on technique, more accessible to the public, and more relevant to policy debates, he should probably seek to increase the distance between political science and policymakers rather than narrow the gap between them.

Notes

1 See Oren 2003, 152–4.
2 http://www.nsf.gov/about/ ; see also Oren 2003, 155.
3 http://www.electionstudies.org/overview/origins.htm.
4 Oren 2000, 554.
5 Oren 2003, 138–9; Robin 2001, 64–8.
6 Oren 2003, 149–50.
7 Rohde 2013, 62, 65; emphasis added.
8 Schrodt 1995; Oren 2003, 168–9.
9 Schrodt 2012, 564; Oren 2014.

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Locating Relevance in Security Studies

Laura Sjoberg

Michael Desch argues that the increasing sophistication of quantitative methodology deployed by those who study war and conflict in global politics ("security studies") undermines the relevance of work in the subfield to the "real world" (the Washington Beltway) in ways that are normatively and substantively problematic. I generally agree with Desch that security studies has a relevance problem attributable to its current epistemological and methodological framings. However, I think Desch specifies both the target of relevance and the scope of security studies' methodological issues in ways that obscure the degree to which his own analytical framework is actually part of the problem rather than an effective critique of it.

First, Desch's interpretation of "relevance" is overly narrow, and this causes him to misdiagnose security studies' relevance problem. His argument homologizes "relevance" of security studies and its ability to gain an audience in the Washington Beltway in the United States as it considers U.S. national security. Accordingly, Desch longs for a time when security studies had the ear of the Washington elite. Yet attention from the Beltway is at best orthogonal to and quite possibly negatively correlated with the sort of relevance I find important, intellectually and normatively. Security studies, both before and after the quantitative turn that Desch identifies, has generally been focused on interstate interaction (with a skew towards great power politics). It makes sense from that perspective, then, that its proponents would associate relevance with heed from a great power government.

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Yet even early research from a feminist perspective suggested that secure states produce (gendered) insecure people at their margins, because the provision of national security operates on hierarchies of race, gender, religion, ethnicity, sexual orientation, and national identity.[1] Accordingly, feminist (and other critical) work that intervenes in the field of security studies takes a normative interest in the lives of those people at the margins of the international system, rejecting the assumption that great power dominance is normatively good.[2] This research asks what makes people secure, and what makes people insecure—the referent of "security" is the individual, the family, the community, and the local social network rather than the (great) state.[3] Often, empirical studies of peoples' (in)securities suggest that "the Beltway" is a *security threat* both to many Americans, and to many more people around the world—rather than the entity to which security studies should be making suggestions about increasing its own security.[4] In this view, "security" is as much about the availability of nutrition and clean water than about troop logistics; as much about freedom from domestic violence than about air maneuvers; and as much about the provision of medical care than nuclear deterrence.

This suggests that the Beltway's military-focused strategic and tactical concerns are the wrong subject of security, and elite American policy-makers are the wrong audience for relevance. In an interpretation of security studies that *pays attention to* the margins of global politics, relevance should be pinned to a number of other factors, including but not limited to accessibility and/or benefit to people at the margins of global politics, provision of locally sustainable security solutions, critiques of gendered and raced hierarchies in global social and political life, or transgressions against cycles of interstate violence and intrastate repression. On this interpretation of relevance, there was not a time gone by when security studies *used to be* appropriately relevant; it has *never* been relevant to these concerns, at least not in a way that provides significant practical or normative benefit to people at the margins of global politics.

Still, even in this interpretation of relevance, Desch's suggestion that there is a trade-off between relevance and the use of increasingly sophisticated quantitative methods to study security has some merit. While there are many knowledge-producing benefits to quantification,[5] and

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more benefits to quantification that does a better job of capturing the complexities of the lived realities of (in)security in global politics, there are also many limits—not only in accessibility, but also in the ability to capture a number of different phenomena relevant to that (in)security, such as structures of signification;[6] liminality, marginality, and invisibility,[7] and co-constitutive relationships.[8] These limits may well *net* a decrease in relevance coming from the increased use of quantitative methods in security studies.

However, I believe that the real relevance problem facing security studies is due less to the increased emphasis on quantitative methods than to a more general neo-positivist conception of social science that plagues much qualitative research as well. Desch takes the appropriateness of this conception for granted, and talks about what sort of social science methods to use and how to frame social scientific research usefully to policy-makers. Yet feminist work has suggested that the sort of "objectivity" sought by Desch as much as by those he criticizes in fact represents the universalization of the experiential knowledge of the elite, with the effect of silencing other voices.[9] The notion that the experiential knowledge of the elite is objective licenses the privileged to think (and speak) *for* everyone so long as they do so in a way that is understood to meet the standards of "science."[10] This position is problematic because it ignores relationships between knowers and the knowledge that they have, framing knowledge as universal, when it is actually contingent, interested, and political.[11] Obscuring the politics of elitism in the scientific method makes its work not only inaccurate and exclusive, but substantively problematic for those at the margins in global politics, while at the same time *enforced on* them. In this view, the "scientific method" approach to security studies research, quantitative *or* qualitative, could be seen as another axis of hierarchy and exclusion in the field. While I will not be able to do that argument justice in the short space that I have, I think it is important to point out the ways in which observability and objectivity, as much as quantification and regression, can be seen to narrow what is analyzed and understood as important in security studies.

This suggests that while Desch is correct that security studies has a relevance problem rooted in disciplinary methodology, his problematically narrow interpretation of relevance and his complicity in the discipline's current neo-positivist epistemologies—and the violence of these epistemologies—obscure the real nature of the problem. In order for security studies to contribute meaningfully to substantive threats to *people's* security, it will need to abandon its (gendered) state-centrism and its nostalgia for closer ties to status quo political power, and embrace a more critical perspective on the sources of human vulnerability and suffering and the audiences to whom that analysis would be most relevant.

Notes

1 E.g., Tickner 1992, Peterson and Runyan 1992.
2 E.g., Tickner 1992, Wibben 2010; Sjoberg 2013.
3 Hudson 2005, Hoogenson and Rottem 2004.
4 Young 2003, Sjoberg 2013.
5 See the discussion in Barkin and Sjoberg, forthcoming.
6 E.g., Steans 2014.
7 E.g., Sjoberg 2012.
8 E.g., Bergmand Rosamond 2013.
9 E.g., Harding 1998.
10 Scheman 1993, 211–212.
11 See the discussions in Alcoff and Potter 2013.

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"Please Mind the Gap": Policy Relevance and British IR

Helen Louise Turton

Michael Desch highlights the current pressures American academics are facing to ensure that their research is policy relevant. Despite recent efforts from funding bodies, research councils, university deans, and other actors, Political Science in the United States, and security studies in particular, is not achieving the desired levels of policy relevance. Whilst others have argued that the reason for the widening gap between practitioners and IR scholars is the increased role of think tanks,[1] the different organizational cultures/modus operandi of each sphere,[2] or even the "limited interest on the part of the policy community in reaching beyond the Beltway to connect with scholars and consume academic research,"[3] Desch claims the lack of policy relevance is due to academic research becoming too technical, the methods used too complex, and therefore inaccessible. In this short response I will show Desch's claim to be correct regarding IR scholar's tendency to focus on an "internally-oriented research agenda"(378)—however I disagree with his reasoning. Whilst IR scholars might not be focusing on issues pertaining to U.S. foreign policy or elite demands, this is not the result of professionalization but of the broadening of the ontological scope of the discipline and the fairly static nature of foreign policy concerns. Furthermore, whilst the explanations of methodological sophistication might account for why American Political Science and Security Studies struggle to be policy relevant, this explanation cannot be applied to other academic communities, such as the UK for example. By illustrating the methodological trends in UK IR, I argue that Desch's explanation is only part of the story of the "Cult of Irrelevance."[4]

A recent study into intellectual trends in the global discipline of IR revealed that only 21.9 percent of IR scholarship published from 1999–2009 in 12 prominent IR journals[5] could be counted as addressing areas or issues of international politics that are "policy relevant" to the American foreign policy agenda[6]—meaning that 78.1 percent of IR scholarship during this time period was focusing on other issues outside of the ontological scope defined by the United States and its foreign policy elites.[7] This study empirically supports Desch's claims regarding the declining relevance of Political Science, for it shows that IR scholarship is not predominantly focused on providing policy prescriptions to pressing American foreign policy and security concerns. Instead the body of scholarship that was reviewed in the study revealed that IR scholars are exploring a wide range of other issues and debates, of which some are theoretical, but the majority were critical of or prescriptive to a wide range of international realities and phenomenon.[8]

Despite a seeming "new" range of threats and foreign policy concerns, the agenda of American security practitioners has remained largely static and state-centric.[9] IR scholarship has sought to go beyond this narrow remit and examine a range of other issues arguably to become "more relevant" to other actors.[10] Whilst such research may not be relevant to certain security practitioners (and therefore labelled "insula"'), it arguably is relevant to a wide range of other actors. The question then becomes one of who IR research should be relevant to. According to some scholars, speaking to policy makers is only one very small part of IR's vocation, and we should be focused on making our research more relevant to civil society organisations and other actors.[11]

Scholars such as John Mearsheimer have argued that the move away from policy-relevant research is not an ontological expansion but actually a narrowing of the discipline (a sentiment seemingly shared by Desch), born out of self-interest to ensure professional success in a climate dictated by publication records.[12] Whether one views the decline in policy-relevant IR research positively or negatively, this explanation serves to provide only a small part of the explanation for the widening gap. According to Desch, another prominent reason is the methodological proclivities of the U.S. IR community. Yet this rationale cannot be used outside of the United States to explain similar levels of "irrelevance" in other IR

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communities. For example, the British IR academy experiences the same gap between the Ivory Tower and the Beltway,[13] but Desch's explanation does not suffice in this context.

The way that universities in the UK are financed has changed over the past few decades, which has resulted in British IR scholars being encouraged to become more policy focused and to strive for an "impact" upon policy and politics more broadly speaking.[14] Despite incentives to become more policy relevant, there is still a large gap between the two worlds of academia and policymaking.[15] However, this gap cannot be explained by Desch's arguments regarding the use of overly technical mathematical methods because in the UK British scholars have a clear proclivity for qualitative methods.[16]

A brief look at published research in the British journals the *Review of International Studies* and *International Relations* shows the lack of quantitative research being undertaken and published in the UK. From 1999–2009 only 0.4 percent of the published content in the *Review of International Studies* was quantitative, meaning that out of 493 articles only two used methodological approaches that could be designated as quantitative.[17] In *International Relations*, 4.3 percent of research published in the time frame under review was quantitative; which meant 11 out of 255 articles employed some type of quantitative analysis.[18] Considering that the majority of scholars published in this period were from British institutions this insight highlights that the British IR community favours qualitative methods and that scholars in the UK tend to not embark upon the sort of quantitative endeavours noted by Desch.

Similarly, in a study into the state of British IR, Wayne Cox and Kim Nossal noted that "there are virtually no professors of IR in the British academy who embrace methodologies popular in American IR, such as rational choice and formal modelling,"[19] and furthermore that "there is a marked absence of any indication of followers of rational choice or quantitative approaches at UK universities,"[20] with the University of Essex being the only exception to the norm. If the UK experiences a noticeable absence of quantitative IR research being conducted then the gap between British academics and policy makers cannot be explained using Desch's claims, meaning that methodology is only part of the story both inside and outside of the United States.

The widening gap is more likely to be explained by other factors such as the growth of think tanks and other actors, and ingrained perceptions about academic research held by policy makers.[21] This means that even if the methods used were less complex and more accessible in the United States, and if IR scholars in the United States and UK were focusing on policy relevant issues, there would still likely be a gap. Therefore instead of trying to 'bridge the gap' through changing our methodologies or substantive focus, perhaps we as IR scholars need to focus our efforts on redefining what it means to be relevant and to seek novel ways of engaging with broader audiences.

Notes

1 Wilson 2007, 148.
2 Hill and Beshoff 1994; Erikkson and Sundelius 2005 52–53; Nye 2008, 598–599.
3 Jentleson and Ratner 2011, 8.
4 Shapiro 2005.
5 The twelve journals examined were *Australian Journal of International Affairs*, *Cooperation and Conflict*, *European Journal of International Relations*, *International Organization*, *International Relations*, *International Relations of the Asia Pacific*, *International Studies Quarterly*, *International Studies Perspectives*, *Journal of International Relations and Development*, *Review of International Studies*, *Security Studies and World Politics*.
6 Turton 2015.
7 Ibid.
8 Ibid.
9 Wight 2001.
10 Smith 2007, 12.
11 Erikkson 1999, Andersson 2003, Lawson 2008.
12 Mearsheimer 2004; see also Mearsheimer and Walt 2013.
13 Walt 2005, Lawson 2008.
14 Passi 2005.
15 Walt 2011.
16 Smith 2000, Jørgensen and Knudsen 2006, Brown 2011.
17 Turton 2015.
18 Ibid.
19 Cox and Nossal, 2009, 293.
20 Ibid., 294.
21 Erikkson and Sundelius 2005, 55–56.

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Rigor Is Not the Enemy of Relevance

Erik Voeten

Michael Desch offers an insightful analysis of the claim that political science research, especially in security studies, has become less relevant to policy makers over the past six decades. Desch is particularly concerned with the absence of explicit recommendations to U.S. national security policy makers in most published political science research. Instead, Desch argues, political scientists have concentrated on rigorously examining narrow questions that matter only to other academics. He attributes this trend to two sources. First, professionalization has led to an inward turn whereby political scientists can prosper without ever needing validation from the outside world. Second, the most intense collaborations between the academy and U.S. security policymakers have historically occurred during wartime.

A common response to critiques such as these is that the separation between the policy and the academic worlds is not a problem and perhaps even welcome. Academics are and should be engaged in basic science and the pursuit of pure knowledge rather than solving the particular problems that occupy policy-makers. This works fine as a defense of individual forms of scholarship but it is a poor response for the discipline as a whole. If a discipline becomes too insular, then knowledge accumulated through basic science will not percolate or will not speak to the most pertinent societal questions.

Yet, Desch's understanding of relevance is too limited. First, if the purpose of social science is to ultimately produce knowledge that serves a social good then it is wrong to equate relevance with a narrow concern for U.S. national security policy. The burgeoning scholarship on the workings of international organizations, non-governmental organizations, private actors, and so on also deserves consideration. Political science scholarship may ask more confined questions but it has also broadened its scope considerably over the past half century.

Second, the disappearance of explicitly listed policy recommendations from scholarly articles does not mean that these articles don't have policy relevance. Much scholarly work provides empirical context or conceptual foundations for policy debates. Policy recommendations based on research are often communicated through alternate outlets, like *Foreign Affairs*, op-eds, think tank briefs, crossover books, or blogs. This separation surely reflects professionalization but it should not be equated with the disappearance of relevant research.

Third, we should not care about the academy's relevance per se but about its ability to provide pertinent knowledge that can improve societal outcomes. The goal should not be that political scientists are cited more by the *New York Times*, funded more by the government, or even appointed to more high-level government positions. The goal is to improve social outcomes by offering insights that are better grounded in empirical evidence and based on more solid conceptual foundations than that supplied by others, including the increasingly professionalized and expansive think tank sector.

This point is essential because the main villain in Desch's narrative is the increased turn to sophisticated methodologies, especially formal modeling and quantitative methods. To Desch, the purpose of these methods appears to be little else than to obfuscate academic writing and to keep the laity out. Indeed, Desch categorizes formal modeling, statistics, and abstruse jargon all under the rubric "sophisticated social science methods," thus seemingly discounting the possibility that sophisticated methods may actually yield better answers to questions.

To be sure, there is no deterministic relationship between the sophistication of a methodology and the soundness of empirical conclusions or the coherence of a theoretical argument. Yet many methodological advances are indeed concerned with drawing more valid inferences, including those that potentially benefit policymakers. Consider the recent turn towards more careful causal inference. This movement is shaped fundamentally by a desire to evaluate whether some social outcome of interest has improved as a consequence of a program or policy.

Fourth, it is an error to equate the grandness of a research question with policy relevance. There are grand debates that have little immediate policy relevance and

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many narrow questions that do. If there is any trend among the youngish crop of security scholars it is to ask precise policy-relevant questions and to answer these with sophisticated (quantitative or qualitative) methods that pay careful attention to measurement and causal inference. Indeed the U.S. government sponsored Minerva project, which Desch highlights, is responsible for funding many of these studies, which are often very rigorous and frequently quantitative.

Increased attention to statistics exists not just within the confines of the political science academy. Programs that train professionals in international affairs, including my own, now have required courses in statistics because students increasingly need these skills to compete for policy jobs. By singling out methodological rigor as the culprit, the movement to bridge the gap between scholars and policy makers runs the risk of alienating an important constituency of scholars who share the ostensible goals of the movement and who are meeting demands from parts of the policy community.

Desch acknowledges some of this scholarship in passing but argues that they are "at best 'mixed benefit' to policymakers." That is an impossibly high standard that relies on an implicit comparison with an overly romantic version of what political science security research was like in the 1950s. Surely, there are and always have been difficulties communicating academic research to policymakers. Some of these will be exacerbated by the use of more complex methodologies. But there was never a time when U.S. academics had unfettered access to the government and gave them sound advice that was heeded all the time. The history of U.S. foreign policy during the Cold War does not support such a rosy conclusion.

That said, Desch is correct that academics still have imperfect incentives to conduct research that addresses important societal problems and to ensure that this research reaches broader audiences. Unlike Desch, I believe that the trend over the past decade and a half has been positive. Yet, there are still many areas for improvement, including the academic publishing process. With several co-editors I have recently founded a new journal, *Research and Politics*, which caters to academic incentives by offering quality control through peer review while publishing short open access articles that are easy to find and be read by outsiders. The underlying model is to break the boundaries while maintaining the rigor. I do not expect this to be easy but it is a better bet than hoping that political science returns to (some romanticized version of) what it was like in the 1950s.

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Comment on Michael Desch, "Technique Trumps Relevance"

Stephen M. Walt

Michael Desch believes that external threats largely determine the degree of policy-relevance in political science, and especially the status of security studies. When threats are high—as in World War II or the early Cold War—social scientists focus on providing usable knowledge and policy-makers pay more attention to their work. When dangers wane, however, disciplinary imperatives re-emerge, political science looks inward, and policy-relevant subfields like security studies come under attack.

As someone who has worried about the adverse effects of professionalization in academia, observed the hostility to security studies in several departments, and called for giving more weight to policy-relevant work,[1] I have considerable sympathy for Desch's central argument. Nonetheless, I shall resist the temptation to simply agree with him and focus instead on several issues that merit further investigation.

For starters, does Desch's broad explanation fit the facts? Desch's account highlights the collapse of Yale's Institute of International Studies in the mid-1950s, but the fate of one center at one university is hardly definitive evidence of a recurring tendency. Harvard's Center for International Affairs was established at about the same time, for example, and it served as an intellectual home for scholars such as Samuel Huntington, Henry Kissinger, Kenneth Waltz, Thomas Schelling, and Robert Jervis. Yale's current International Security Studies program was founded in 1988, and it has thrived in a period where the United States was the dominant world power and faced few serious foreign dangers. Thus, looming external threats do not seem necessary to maintain a degree of policy-relevance and an interest in security topics.

Similarly, Desch's theory does not fully explain why security studies came under such strong attack during the Indochina War. Many experts believed defeat in Vietnam would have serious adverse consequences, and this period also witnessed a Soviet arms buildup, an eroding military balance in Europe, and recurring conflicts in the Middle East. The international environment was far from benign from 1965 to 1974, yet security studies languished until the early 1980s.[2] The main reason was anti-military sentiment within the academy, but that is a different problem than Desch's emphasis on the level of external threat.

Second, although I believe Desch's account of trends in political science is generally accurate, academia as a whole presents a more complex picture. In particular, he does not discuss the parallel emergence of schools of public policy, whose ranks include a number of prominent security scholars. Technique may be trumping relevance in traditional "arts and science" departments, but Princeton's Woodrow Wilson School, John Hopkins' School of Advanced International Studies, the Harvard Kennedy School, and other similar institutions have expanded steadily over the past four decades, even as the level of external threat has waxed and waned.

Third, are the disciplinary pathologies Desch identifies offset over the course of individual life academic careers? Younger scholars typically focus on establishing their disciplinary credentials, but many become more engaged by real-world issues after receiving tenure. The careers of Joseph Nye, Zbigniew Brzezinski, Stephen Krasner, or Condoleezza Rice illustrate this familiar tendency, and younger scholars such as Jeremy Weinstein (chief of staff to UN Ambassador Samantha Power) and Colin Kahl (national security advisor to Vice President Joe Biden) seem to be following a similar path. If so, then academia may still be providing useful guidance to the policy world.

Lastly, what is the proper balance between "pure" or "applied" research, either in the field as a whole, within a given department, or in an individual scholar's career? There is no "right" answer to this question, of course, and the optimal balance is bound to vary over time. Here Desch's recommendation that academic departments employ a wider set of criteria when evaluating scholars and not rely solely on outside assessments by their fellow academics or on citation counts strikes me as sound.

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Because we can never know exactly what sort of expertise we are going to need, a diverse intellectual ecosystem is preferable to a monoculture, especially when dealing with issues of war and peace. Emphasizing policy relevance and engagement is an effective way to preserve that essential diversity, instead of allowing purely methodological criteria to define and distort the substance of the field.

Notes

1 Cf. Walt 2005, 2011; Mearsheimer and Walt 2013.
2 Walt 1991.

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Response to Comments on "Technique Trumps Relevance"

Michael Desch

I am grateful to the editors of *Perspectives on Politics* both for publishing my Reflections on how the professionalization of political science has squeezed out policy-relevant parts of the discipline such as security studies and also for inviting five colleagues with different perspectives to comment on it. Despite the editors' generosity, my original piece was limited to 11,000 words and my response to the comments has to be even shorter which means I could not, and cannot, address every important issue they raise here. Fortunately, I am writing a book on this topic in which I go into greater detail about the causal mechanisms I think are at work and provide even more evidence that they are having the effects I suggest, particularly in the changing relationship between security studies and our discipline.

Before responding to some of the specific points my colleagues make, let me reiterate my core argument: The decreasing policy-relevance (narrowly defined as the willingness of scholars to offer concrete policy recommendations) is the direct result of the professionalization of political science. A number of internal disciplinary dynamics tend to privilege method over substance but war or periods of heightened threat can counteract them, at least for a time. Regarding the objective of professional social science, the problem is not so much any specific methodology as the privileging of some techniques preferred for internal disciplinary reasons over broader relevance. I illustrate these dynamics through with the changing disciplinary fortunes of national security affairs.

Steve Walt asks whether the security environment really plays the role I think it does. We could trade anecdotes back and forth about the post-Cold War health of various policy-relevant academic organizations and I would point out that at his home institution only the Kennedy School's Belfer Center remains of the once rich policy-focused community at Harvard, with the Olin Institute for Strategic Studies now defunct and the Center for International Affairs evolving into a more academically-focused research center than it was in the past. But rather than trade war stories, let's look at the data: The policy relevance of scholarship in leading political science and sub-field journals has decreased markedly after spiking during the First and Second World Wars and the early Cold War.

Vietnam hardly undermines my argument because it was precisely the fact that many scholars came to believe that a Communist victory there would not seriously threaten U.S. national interests, as Kenneth Waltz among others famously argued, that makes it difficult to suggest that this was a high-threat period that justified the continued mobilization of the professoriate. Finally, after ten years teaching in professional schools, I am less sanguine that they will continue to serve as bastions of relevance in a sea of disciplinary navel gazing, a point recently made by James Pierson, a former foundation executive who played a key role in supporting policy-relevant security studies during its last "Renaissance."[1]

Erik Voeten is one of a new generation of younger scholars trying to recreate a different Golden Age in which cutting-edge social science techniques were regularly and successfully brought to bear on policy questions. Heirs of Charles Merriam, these "New Progressives" have taken to cyber-space through blogs and new on-line journals to broadcast their research to the wider world in a shorter and more accessible fashion than traditional scholarly journals. Like a recent American Political Science Association task force, their assumption seems to be that all we need are more adroit communication strategies and mechanisms to bridge the Ivory Tower/Real World gap.[2]

I wish these New Progressives well but I think that the tensions between science and policy are much deeper than they acknowledge. This is why the history of previous efforts to find a durable balance between broader relevance and disciplinarily defined rigor is not encouraging. In the late 1930s Robert Lynd chronicled the death of the original Progressive attempt and in the 1960s David Easton acknowledged the failure of the Behavioral Revolution to permanently reconcile cutting-edge methods and sustained practical engagement.[3]

This brings me to Ido Oren's critique. Reading it I found myself agreeing with large parts of it and so wondering where he and I actually disagree. As I pointed out in my original piece, wartime often fosters optimism about the reconcilability of cutting edge-techniques with

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policy-relevant work. As Oren notes, government support often contributes to the former's development. He mentions many examples to which I would add the development of Econometrics and Game Theory at the U.S. Air Force-sponsored RAND Corporation during the 1950s and 1960s.[4]

But U.S. government support does not always have the pernicious effects he posits. For example, it played a significant role in the development of Area Studies as a policy-relevant interdisciplinary enterprise during the early Cold War.[5] But despite that Government "demand" for the work of Soviet and other regional specialists, methodological developments within political science left behind that approach based upon deep historical and cultural knowledge of particular areas of the world, marginalizing one of the parts of our discipline that policymakers find most relevant. In other words, one can accept (and I do) Oren's history without accepting his conclusion that we scholars should widen, rather than narrow, the gap.

Helen Louise Turton questions whether my theory of the waxing and waning of policy relevance has "legs" (or better, fins) and can travel across the Atlantic. Arguing that because European international-relations scholarship eschews quantitative techniques and formal modelling yet is still largely irrelevant to British policymakers, she questions how general my theory is. If my argument was that the problem was just math and models, she would be correct. But what I object to is not particular methods but rather the privileging of methodology and other internal disciplinary-driven research agendas over broader problem-oriented work. I am not by any means an expert on the various Continental academic approaches to international relations and security studies but much of it that I have read substitutes the methods and jargon of the humanities—which are suffering their own well-document crisis of relevance—for those of American social science with the same result: a privileging of preferred disciplinary approaches over broader relevance.[6] Given that, Turton is mistaken about the irrelevance of my theory to developments across the pond.

Finally, Turton, Voeten, and Laura Sjoberg all make an important point about policy-relevance involving much more than just being of use to government policymakers. I agree with this point even though it may not have come through in my original piece, given my focus on the clearest manifestation of policy relevance: offering policy recommendations that ultimately influence government policies. There are certainly other forms of policy relevance beyond that. Indeed, especially in a democratic political system a scholar's vocation for politics can involve educating students and informing the wider public about pressing issues of policy. An engaged scholar could also serve with non-governmental and private organizations rather than just through government service. To do so, however, he or she will have to rise above his or her narrow and parochial disciplinary perspective and embrace an approach that is problem-, rather than method-, driven and that defines rigor as providing the best answer possible to substantively important questions rather than using the academic technique *du jour*.

Notes

1 Pierson and Riley 2013.
2 Lupia and Aldrich 2014.
3 Lynd 1939 and Easton 1969.
4 Hounshell 1998.
5 Engerman 2009.
6 For evidence of the crisis of relevance in the humanities, see Levitz and Belkin 2013. For an example of how the embrace of the methods of the humanities has muddied international relations see Ashley 1984.

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