



BOOK REVIEW ROUNDTABLE: CULT OF THE IRRELEVANT

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1. The “Relevance Question” in Security Studies

By Galen Jackson

In *Cult of the Irrelevant*, Michael Desch sets out to address what he sees as a growing existential crisis among scholars of security studies. As the book’s title makes clear, the field, in Desch’s view, has become increasingly unhelpful to policymakers working in national security, and the gap, he believes, is only getting wider.¹ The implication of this argument, of course, is that major changes to the discipline are necessary if security studies analysts actually want to have any sort of policy impact.

To Desch, the principal cause of this turn toward irrelevance is obvious. The “professionalization” of political science, he writes, has generated a number of dynamics that are detrimental to the objective of producing policy-relevant scholarship. As political scientists — who, according to Desch, suffer rather severely from “economics envy” and an “inferiority complex vis-à-vis the natural sciences” — have “striven to become more scientific,” they have increasingly approached security studies issues in a manner that is essentially counterproductive.² For many years now, he argues, the trend in the field has pointed strongly in this direction, manifesting itself in the enormous value political scientists place on things like quantification, “rigorous” methodologies, the development of elegant formal and mathematical models, and theories with supposedly universal applicability, even in the messy realm of international security affairs. To be sure, in times of acute international crisis, Desch claims that security studies analysts tend to engage themselves more directly in policy matters — this, in fact, is one of the book’s core themes — but such circumstances represent the exception rather than the norm.

The essential problem here, Desch argues, is that “‘scientific’ approaches to international relations scholarship seem to be the least relevant.”³ Analysts, in his view, are increasingly seeking ways to use sophisticated methods as an end in itself, instead of taking a “problem-

¹ Michael C. Desch, *Cult of the Irrelevant: The Waning Influence of Social Science on National Security* (Princeton, NJ: Princeton University Press, 2019), 2-3.

² Desch, *Cult of the Irrelevant*, 2, 248.

³ Desch, *Cult of the Irrelevant*, 4.

driven” approach. The result is that scholars are more and more concerned with relatively parochial research questions that are of little significance to most of society. Nor, Desch claims, are there reasons to think that “basic,” as opposed to “applied,” research is likely to influence policy meaningfully, especially since scholarly knowledge is unlikely to “trickle down” to decision-makers. Relatedly, other approaches that have previously been shown to produce helpful, policy-relevant research — Desch here points specifically to area studies, qualitative methodologies, and interdisciplinary work — have basically been sidelined. Likewise, security studies specialists, according to Desch, now tend to rely heavily on technical jargon, a trend that has made it difficult for policymakers to comprehend the significance of their research.⁴

The problems do not end there. Perhaps even more worrisomely, the demand for “generalizable” models and “universal” theories has contributed to the formulation and pursuit of disastrous policies, which, in turn, has only served to further discredit security studies as a discipline capable of informing sound national security strategies. Even Thomas Schelling, one of the most celebrated analysts in this area, in Desch’s view proves the point. “Schelling the economist prevailed over Schelling the policy analyst,” he writes, with the result that his policy recommendations ultimately proved either unpersuasive or produced “catastrophic consequences,” most notably in the context of the Vietnam War.⁵ And unfortunately, Desch implies, the sorts of institutional dynamics that have led academics to favor “rigorous” methods and generalizable models over other approaches are now deeply entrenched in the field.⁶

⁴ Political scientists, Desch writes, have an even greater incentive to employ highly technical language, given that they tend to “deal with issues that are not otherwise inherently difficult for educated laypersons to engage.” See Desch, *Cult of the Irrelevant*, 13.

⁵ Desch, *Cult of the Irrelevant*, 164, 168. For Schelling’s most notable works, see Thomas C. Schelling, *The Strategy of Conflict* (Cambridge, MA: Harvard University Press, 1960); Thomas C. Schelling, *Arms and Influence* (New Haven, CT: Yale University Press, 1966).

⁶ The dynamics surrounding faculty hiring and peer review, which Desch briefly discusses, are perhaps the most significant in this respect. See Desch, *The Cult of the Irrelevant*, 15. An even more pronounced trend of this sort, one could argue, exists in the fields of diplomatic and military history, which, in one scholar’s words, “are in the midst of a decades-long crisis,” in great part because topics such as these are “simply not a priority within many history departments.” See Francis J. Gavin, *Nuclear Statecraft: History and Strategy in America’s Atomic Age* (Ithaca, NY: Cornell University Press, 2012), 15. On this issue, see also Hal Brands and Francis J. Gavin, “The Historical Profession Is Committing Slow-Motion Suicide,” *War on the Rocks*, Dec. 10, 2018,

What is to be done about this sorry state of affairs? To Desch, the necessary solutions are quite straightforward. The answer is neither to simply jettison formal and quantitative scholarship, nor to abandon theory development, but instead to counter the “tendency of many social scientists to embrace methods and models for their own sake rather than because they can help us answer substantively important questions.”⁷ In practice, this means that security studies needs to not only embrace “methodological pluralism,” but also to commit to “problem-, rather than method-, driven research agendas.” Likewise, scholars should think more carefully about how they use theory in their work, given that “too much of it can be a bad thing.”⁸ Finally, analysts ought to make a greater effort to address issues that matter most to policymakers and to engage them directly, thereby serving as “their own policy ‘transmission belts.’”⁹ Unless such steps are taken, Desch concludes, security studies scholars in political science are unlikely to be capable of producing policy-relevant knowledge, a task he views as a “moral obligation.”¹⁰

A Rigorous Debate

Desch’s argument will likely produce major disagreements among security studies scholars. Indeed, based on the reactions of the three reviewers in this roundtable — Bridget Coggins, Peter Dombrowski, and John H. Maurer — such an outcome seems probable. As Dombrowski observes, the book “will surely generate controversy.” Fortunately, all three

<https://warontherocks.com/2018/12/the-historical-profession-is-committing-slow-motion-suicide/>; Fredrik Logevall and Kenneth Osgood, “Why Did We Stop Teaching Political History?” *New York Times*, Aug. 29, 2016, <https://www.nytimes.com/2016/08/29/opinion/why-did-we-stop-teaching-political-history.html>.

⁷ Desch, *Cult of the Irrelevant*, 241. In this respect, it is worth noting that Desch is capable both of employing quantitative methods and engaging theoretical debates in international relations. For example, see Michael C. Desch, “Democracy and Victory: Why Regime Type Hardly Matters,” *International Security* 27, no. 2 (Fall 2002), 5–47, <https://www.jstor.org/stable/3092142>.

⁸ Desch, *Cult of the Irrelevant*, 250.

⁹ Desch, *Cult of the Irrelevant*, 251. Achieving these goals, Desch writes, would involve altering the incentive structure for academics. Scholars, he notes, would have to be rewarded for doing policy-relevant writing and for engaging with policymakers. In addition, key institutional processes would probably require reform, with non-academics playing a greater role in peer review and tenure decisions. See Desch, *Cult of the Irrelevant*, 253.

¹⁰ Desch, *Cult of the Irrelevant*, 255.

reviewers are well situated to provide valuable insights when it comes to the issues with which Desch is concerned. The book, Coggins writes, was “tailor made” for someone with her background as an academic who has seriously engaged policy issues and has experience working in the policy world. As academics who teach in the professional military education system at the Naval War College, Dombrowski and Maurer are similarly well positioned to debate what Desch refers to as “the relevance question.”

Coggins, in the main, found Desch’s argument unconvincing. As the title of her review suggests, she is skeptical about the existence of a security studies “cult” dedicated to producing irrelevant scholarship. “[T]he evidence,” she writes, “just does not fit.” To her mind, the list of academic analysts who have recently made meaningful policy contributions is actually rather impressive. Moreover, Coggins believes, it is not that academics have grown irrelevant, but that the ways in which they influence policy have changed. PhD and international affairs programs are expanding, their graduates now come from more diverse backgrounds, and the focus of security studies analysts’ research has expanded — all welcome developments. Scholars, she adds, are also more aware of the limits of their work, something that “should ultimately help produce more robust policy than what has come before.” In short, Coggins neither believes the field is experiencing a crisis, nor that the solution is to give up on quantitative methods.¹¹ On the contrary, she writes: “Scholarship is at its most potent when it gives leaders a different, better way to think about the problems and the strategic environment they face, something that has certainly taken place.”

Dombrowski also seems less concerned than Desch about the state of the field. Academics, he claims, *do* have an impact on policy, just not in the way Desch supposes they should. Perhaps “grand theories” like realism are not directly of much use to decision-makers, but “mid-level theories” certainly are. Moreover, Dombrowski is more sanguine about academic knowledge “trickling down.” “[O]ne way or another,” he writes, policymakers will ultimately come to rely “on John Maynard Keynes’s famous ‘scribblers.’” Likewise, he is very “optimistic that the emerging generation of scholars will make choices conducive to policy engagement.” His main criticism of the book — and here he seems to echo Coggins — is that it lacks evidence showing that academics have grown increasingly irrelevant. The

¹¹ To reiterate, Desch does not call for security studies to abandon the use of quantitative methods.

book's two main case studies, after all, deal with two men who had an enormous influence on policy: Schelling and Walt Rostow.

Maurer, however, is more receptive to Desch's arguments. The problems the book identifies, he notes, are by no means new. For example, former German Chancellor Otto von Bismarck, Maurer writes, "held professors in contempt." But according to Maurer, in the past scholars have nevertheless found effective ways of shaping policy. President Woodrow Wilson, after all, started his career as a political scientist. Likewise, Maurer provides fascinating discussions of the efforts of Andrew Marshall, the former head of the Office of Net Assessment, to promote scholarly research on policy-relevant issues, and of the Secretary of the Navy Fellows program, which had a very similar objective. Ultimately, however, Maurer agrees with Desch's view that security studies has experienced a "depressing" decline, particularly since the end of the Cold War. The fellows program, for example, was discontinued after only a short time. Consequently, Maurer concludes by saying he hopes, like Desch, that "policy-oriented scholars might yet gain recognition for their efforts among the great and the good at university, within departments of political science."

Although the reviewers had a wide range of reactions to the book, they were all more or less in agreement that Desch's specific recommendations in the conclusion were useful and persuasive. Coggins, for her part, says Desch's suggestions "are good ones." Likewise, although Dombrowski and Maurer do not comment specifically on the book's takeaways, the various arguments they make in their respective reviews imply that they, too, would find them helpful. Thus, although it appears unlikely that Desch's book will generate any sort of consensus on the "relevance question," there does seem to be hope that scholars can agree on the sorts of steps that need to be taken to move the field in the right direction.

Swimming Against the Tide

My own reaction to Desch's argument was somewhat different. To be sure, the reviewers made a number of very significant points. Security studies' increased diversity, as Coggins notes, is surely a source of strength for the field.¹² Dombrowski's point about "mid-level"

¹² As Coggins observes, this is still very much a work in progress.

theories, likewise, is an important one, given that policymakers, whether consciously or not, must rely on certain frameworks about how the world works when making key decisions.¹³ Moreover, Desch's reluctance to embrace the idea of academics going directly into policy strikes me as odd. Although Rostow's experience ultimately resulted in failure, one should not draw the conclusion that academics cannot directly shape policy in productive ways. And Maurer's suggestion that the types of programs that the Office of Net Assessment and the Secretary of the Navy used to support be revived seems like a very effective step that could be taken to improve the discipline.

In more general terms, though, I think that the sorts of problems Desch is describing are real and need to be taken seriously. The fact that, by most accounts, the *American Political Science Review* (*APSR*), the discipline's flagship journal, tends to publish only pieces that have little relevance to policy should be taken as a strong indication that the field could do more to address the "relevance question." As Desch quotes one former editor of the journal as saying, the nature of the *APSR*'s peer review process has made "it more likely that a given paper will be selected for publication because it passes muster among a narrow range of specialists rather than because it is considered to be of potentially great interest and importance to a broad range of readers." "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," he added, "one would not have known it from leafing through its leading journal."¹⁴

In addition, I found Desch's intellectual history of this whole issue quite useful. Indeed, Desch *does* at several times in the book provide some important evidence to support a number of his assertions. Specifically, he explores how decision-makers viewed various policy proposals made by academics during World War II, the early Cold War, the Vietnam War, and the Iraq War.¹⁵ With that in mind, Desch's recommendations were persuasive and useful.

¹³ Desch recognizes this point but perhaps downplays it too much. One example Desch does discuss at length, however, is the extent to which academic strategists helped policymakers form "mental maps" for how to think about international politics in the nuclear age. See Desch, *Cult of the Irrelevant*, 147–54.

¹⁴ Quoted in Desch, *Cult of the Irrelevant*, 15–16.

¹⁵ Desch, *Cult of the Irrelevant*, 52, 58–59, 110, 198, 230–33.

But it is precisely because I find Desch's historical account basically convincing that I am rather skeptical that security studies will move in the direction he prefers. In fact, what is most striking about the historical background the book provides — and here Maurer's allusion to Bismarck is also helpful — is that it makes clear that the issues Desch identifies are by no means new.¹⁶ A 1962 report by the President's Science Advisory Committee, Desch notes, excluded “those social science investigations in which more qualitative and historical approaches were employed.”¹⁷

And that sort of attitude was hardly novel even at the time. Nearly 20 years earlier, no less a figure than Bernard Brodie had been informed by his department at Dartmouth that his work simply did not constitute political science. He belonged, instead, in a “department of foreign relations or some such thing.”¹⁸ Having been told repeatedly over the course of the past decade, “You're not a political scientist, you're an historian,” from my standpoint it does not seem like attitudes have changed much.¹⁹ Consequently, I am rather skeptical that the discipline will readily embrace the sort of “methodological pluralism” for which Desch is calling.²⁰

Moreover, Dombrowski's assertion that decision-makers are primarily looking for research to support their preconceptions, rather than to help shed light on key issues, seems convincing.²¹ For many years, in fact, I used to fret over precisely this possibility. If policymakers are uninterested, one might ask, then what purpose are security studies scholars actually serving?

But as I have thought more about these issues, I have reached the conclusion that there is nevertheless tremendous value in engaging policy-relevant issues. Even if one's research fails to shape the views of high-ranking decision-makers, studying and writing about concrete policy problems is likely to strengthen an individual's scholarship, a point Desch

¹⁶ Indeed, one of Desch's key points is that security studies' struggles with the “relevance question” predate the Vietnam War and its attendant negative impact on the field.

¹⁷ Quoted in Desch, *Cult of the Irrelevant*, 74.

¹⁸ Quoted in Desch, *Cult of the Irrelevant*, 38.

¹⁹ To be clear, I in no way mean to denigrate the work of historians working on international security issues. To the contrary, historians do some of the very best work in this area.

²⁰ I would, of course, love to be proved wrong.

²¹ Indeed, Desch occasionally highlights this sort of dynamic. See Desch, *Cult of the Irrelevant*, 173, 191, 198.

emphasizes.²² Perhaps even more importantly, given that few people are well positioned to help shape the public debate about international security problems, or to help educate students about them, teaching policy issues seems to provide an extremely effective way for security studies scholars to confront the “relevance question.” And, to provide a point of optimism, I have found that undergraduates are very hungry to learn more about precisely the sorts of issues that such analysts study.²³

Michael Desch has written an important book, one that will surely produce considerable debate. I suspect that many political scientists will object to some of its core arguments. But if the book starts a robust conversation, especially among security studies scholars, it will have served a very useful purpose. Such a conversation is, to my mind, long overdue.

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²² For example, see Desch, *Cult of the Irrelevant*, 17.

²³ Desch’s impression, incidentally, is that the reverse is not true: Students tend *not* to be interested or to see the usefulness in learning abstruse methods. See Desch, *Cult of the Irrelevant*, 247.

2. A Cult Without Members and Influence Without Access

By Bridget Coggins

According to the late Thomas Schelling, “...the United States [once] had a government permeable not only by academic ideas, but by academic people.”²⁴ But that’s no longer the case. So, what happened? Given the current state of affairs, it might feel like a one-sided governmental disdain for academic expertise has arisen. However, in *Cult of the Irrelevant*, Michael Desch argues that that’s not the case, outlining how and why academics in the national security field have alienated themselves from the policymakers they should be engaging.²⁵

Desch’s argument is straightforward. Covering the period from the turn of the 20th century to the present, he sees a historical ebb and flow of academics making an effort to contribute to national security. He asserts that social scientists’ attempts to influence policymakers have increased in wartime and declined in peace.²⁶ As the nation rallies to serve, policymakers and bureaucrats similarly recognize that they require experts for understanding and responding to pressing security matters. The collaboration does not always go smoothly, however. When academics are influential but excessively model driven, failed policy often follows — most disastrously in conflicts such as Vietnam.

This yields another obstacle to influence, according to Desch, which is that international security scholars unwittingly choose policy “irrelevance” in their (a) use of quantitative methods with impenetrable mathematical jargon; (b) production of universal models; and (c) aspiration to “basic” science, i.e., inquiry for its own sake, that is simply inapplicable to the messy politics of the real world.²⁷ Prioritizing “rigor” over research that focuses on important national security issues, international relations scholarship is scarcely read in

²⁴ Cited in Michael C. Desch, *Cult of the Irrelevant: The Waning Influence of Social Science on National Security* (Princeton, NJ: Princeton University Press, 2019), 1.

²⁵ Desch recognizes that there are reasons for such alienation originating from the policymaker’s side of the equation but leaves those reasons to the side for the purposes of this study.

²⁶ Desch, *Cult of the Irrelevant*, 11–12.

²⁷ Desch, *Cult of the Irrelevant*, 12–17.

the halls of power today. And while the turn away from relevance is particularly acute in peacetime, one gets the sense that the distance between rigor and relevance is greater now than it was in the past.

Academics' ardent belief that social science could approximate the hard sciences, where their conclusions would "trickle down" to the policymaker just as a theoretical physicist's work would later be used by engineers, was fundamentally flawed. Now, social scientists must change if they hope not to be ignored.

What should be done to increase scholarly impact on topics where their expertise could prove critical? Desch recommends implementing two changes within academia: incentivizing methodological pluralism and emphasizing problem-driven research. He suggests this might be achieved using strategies such as including policymakers in academic promotion decisions and by reorienting the work of think tanks (now too beholden to special interests) to serve as transmission belts translating academic ideas for policy audiences.

This book was tailor made for me. I spent a year away from academia in the policy world with the Council on Foreign Relations, I am affiliated with a Washington D.C.-based think tank, and I have participated in public engagement training programs. I am, according to Desch's metrics of relevance, part of an unfortunately underrated and policy-relevant international relations group.²⁸ I also write for lay audiences, tweet, and provide media commentary. I lament that most academic institutions don't count public engagement and policy-facing work in their promotion decisions. I believe that political science's methods du jour — be they formal modeling or causal identification — often limit the field's accessibility, and anyway are not as uniquely compelling as their most dogmatic adherents believe them to be. Finally, as I sit here writing, I am dumbfounded by an academic argument raging online about whether PhD candidates ought to be writing their CVs in LaTeX in order to signal that they are "serious scientists" to hiring committees.

²⁸ Peter Campbell and Michael Desch, "Ranking Relevance: Which Universities Rise and Which Fall in International Relations?" *New America*, Nov. 27, 2018, <https://www.newamerica.org/international-security/reports/ranking-relevance/>.

But the evidence for academic irrelevance in national security policy on account of our methodology just does not fit. The patterns that Desch identifies do not conform to my understanding of the history of foreign policy or to the international security field's contemporary dynamics. Perhaps I could still be convinced, if only the analysis were more rigorous.

Who Is the Subset of Academics Working on International and National Security?

To make his case, Desch uses broad patterns from a current opinion poll of international relations scholars, The College of William & Mary's TRIP survey.²⁹ This survey illuminates much about the average scholar in the field of international relations, but substantially less about the tail end of the access and influence distribution — those individuals whose careers were actually chronicled in the wartime sections of the book. If security scholars in the Ivy League, at MIT, Stanford, or Georgetown are more insular and less influential than their predecessors, then we have a trend. But my sense, when I observe the careers of well-published academics including Anne-Marie Slaughter, Thomas Christensen, Amy Searight, Victor Cha, Stephen Krasner, Jeremy Weinstein, Condoleezza Rice, and many others, is that the interest in, and potential for, policy influence remains for some. Instead, the book's evidence is unbalanced, shifting back and forth between the field writ large today and specific individuals' relationships to academia, or even their particular departments, in the past.³⁰

Also unclear is who counts as a social scientist. Some of the individuals Desch chronicles are unambiguously social scientists, but others are not. Emblematically, he often refers to Albert Wohlstetter, who never completed his PhD and was, in any case, not pursuing a social science degree, as an “academic strategist” and part of the “golden age” of academic

²⁹ “All TRIP Survey Data from February 2014 – December 2018,” Teaching, Research & International Policy, College of William and Mary, Dec. 6, 2018, <https://trip.wm.edu/charts/>.

³⁰ Few will blame social science when Desch describes how Henry Kissinger could not suffer flying commercial after Air Force 707, and so abandoned academic life (see pages 87–88). There is a quote that follows about methods, but this hardly seems to have been the driving force or critical tipping point.

national security studies.”³¹ Yet, there is no mention of his wife Roberta Wohlstetter a history PhD who wrote an influential book on intelligence failures and Pearl Harbor.³² Already an employee at the RAND Corporation, she helped her husband secure a job there. In fact, President Ronald Regan awarded them *both* the Presidential Medal of Freedom in 1985, noting her “profound influence” on policy.³³ While it would be fair to say that history is not properly a social science, it is not fair to say that Albert Wohlstetter was an academic at all — until the University of Chicago hired him as a political science professor in 1964 (despite his lack of training and without a credential in the field).

In order to identify a declining trend in academic influence, we first need a clearly identified set of individuals who have the potential to influence policymakers. More recent time periods may be more inclusive of women, minorities, and those outside of a small handful of private research universities, if the Department of Defense’s Minerva project grants are taken as a good indicator.³⁴ There may, therefore, actually be a larger set of potential influencers than in prior eras. For me, this raises interesting questions about other historical trends, since research also looks different today than 50 or 100 years ago.

Have Academics Turned Away? Or Has the Nature of Influence Changed?

The trend toward positivist and soft-positivist social science has certainly changed the field. Where there was once a handful of publication outlets, there are now tens. And not only are there many more PhD programs in security studies than there once were, there are many more public policy and international affairs schools as well. Those who take jobs in these schools are able to become “policy engineers,” even while remaining in the ivory tower. Predictably, this growth in outlets and programs means much greater scholarly

³¹ Desch, *Cult of the Irrelevant*, 145.

³² There may be references to her work that are not listed in the index, but the use of endnotes makes it almost impossible to find them.

³³ Ronald Reagan, “Presentation Ceremony for the Presidential Medal of Freedom,” Nov. 7, 1965, Ronald Reagan Presidential Library and Museum, <https://www.reaganlibrary.gov/research/speeches/110785a>.

³⁴ “Funded Projects,” Minerva Research Initiative, <https://minerva.defense.gov/Research/Funded-Projects/>.

output and a wider and more diverse field (though less diverse in some important ways than many would prefer).³⁵

The biggest resulting change in scholarship is in the scope of the articles being produced. Most articles now tackle a small piece of a larger puzzle. Therefore, fewer claim to have policy answers or offer prescriptions. Authors more humbly hesitate to draw broad conclusions beyond what their work demonstrates. It isn't, then, that scholars have turned away from relevance, but that they are acutely aware of the limitations of their findings, the complexity of real-world politics, and the slippery slope of cooptation. The pattern Desch observes is accurate — the percentage of articles offering policy prescriptions has declined — but the relationship is spurious. Taken together, this new work should ultimately help produce more robust policy than what has come before. But it will require synthesis first.

I fear that those counseling policy engagement often lose sight of the context, contingency, and overall limits of what can be inferred from any individual research finding — regardless of the type or combination of methods employed. As Janice Gross Stein and her co-authors rightly asserted, “God Gave Physics the Easy Problems.”³⁶ The right way forward is not to attempt to go back to the days of Professor Henry Kissinger by eschewing quantitative methods because Washington is full of luddites who love a good story and do not like math.³⁷ It is to carefully, humbly, and clearly use social science to help inform policy and politics, inside the government or out, for the good of the United States and, perhaps, for the good of the world.

I humbly suggest that influence is not on the wane. Were Desch to survey those who work at the Congressional Research Service and the CIA; the professional staffs at the foreign relations, finance, and judiciary committees; those designing wargames; or influential

³⁵ A greater percentage of these PhDs are also foreign nationals, who may or may not be dedicated to the increased security of the United States. Nor should we expect them to be. Further, we should recognize these students' precarity and well-founded reticence to engage in policy debates.

³⁶ Steven Bernstein et al., “God Gave Physics the Easy Problems: Adapting Social Sciences to an Unpredictable World,” *European Journal of International Relations* 6, no. 1 (2000): 43–76, <https://doi.org/10.1177%2F1354066100006001003>.

³⁷ Indeed, we moved beyond this argument two decades ago. Stephen M. Walt, “Rigor or Rigor Mortis? Rational Choice and Security Studies 23, no. 4 (Spring 1999): 5–48, <https://doi.org/10.1162/isec.23.4.5>.

international organizations outside the United States, I wonder if he would find so little influence. Perhaps it is true that Bruce Russett's work on the democratic peace theory cannot be credited with a particular policy outcome.³⁸ However, to argue that the democratic peace *literature* has not informed American strategic or liberal thought, or that Michael Doyle's qualitative work "matters more" is unfounded.³⁹ Scholarship is at its most potent when it gives leaders a different, better way to think about the problems and strategic environment they face, something that has certainly taken place.

In addition, perhaps academic area experts, especially those with hyphenated nationalities or strong personal ties to "threatening" countries or groups, are dissuaded for normative, security-clearance, or research-based reasons from government service and consultation, and therefore work primarily with non-governmental organizations or activist networks. Desch discounts the chilling effect of McCarthyism and with it the government's routine tendency to see enemies within, even in peacetime. But consider those challenging the "securitization" of the southern border or migration freezes and the challenges that they face in opposing the White House and wider government.⁴⁰ When academic work may be considered "material support to terrorism" it is no wonder scholars are finding alternate routes to affect policy change. These scholars are not influencing principals via the same channels, and so Desch does not see them. In general, academic influence today occurs in a different way than Desch expects.

Even if the problem is less apparent and the causal relationship is different than Desch argues, his recommendations are nevertheless good ones. It would be terrific to have think tanks play a meaningful role in synthesizing academic research for policy audiences. Academics aren't generally great at doing it for themselves and many think tanks have erred

³⁸ Bruce Russett, *Grasping the Democratic Peace: Principles for a Post-Cold War World* (Princeton, NJ: Princeton University Press, 1993); John R. Oneal and Bruce Russett, *Triangulating Peace: Democracy, Interdependence and International Organizations* (New York: W. W. Norton & Company, Inc., 2001).

³⁹ Desch, *Cult of the Irrelevant*, 243-44.

⁴⁰ For example, the TRAC Program at Syracuse University, which recently found that ICE is less likely to target those with serious criminal records than those without: "ICE Focus Shifts Away from Detaining Serious Criminals," TRAC Immigration, Syracuse University, <https://trac.syr.edu/immigration/reports/564/>. The relative absence of contact points due to unfilled positions within the Trump administration is also a recent hinderance to academic influence.

toward their funders and inside-the-beltway incentives (though I suspect that think tanks' problems lie more with them than with the academy). Conducting more problem-driven research using the methods that offer the greatest leverage also seems like the right approach. Ultimately, what first appeared to be a book-length condemnation of contemporary social science, arrived at a rather moderate, catholic ideal-point and offers reasonable advice. Although I was not convinced by Desch's diagnosis, his book was certainly a thought-provoking read.

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3. From Rigor to Rigor Mortis

By Peter Dombrowski

Michael Desch's latest book, *Cult of the Irrelevant: The Waning Influence of Social Science on National Security*, is sure to generate controversy within certain segments of academia: Scholars who produce work that they believe is policy relevant will protest that Desch does not account for their valuable contributions, while scholars who have labored hard to "bridge the gap" by promoting "an informed and engaged public discourse on U.S. and global foreign policy challenges and solutions" for the latest generation of national security

scholars will find their work undermined by many of Desch’s generalizations.⁴¹ After all, if even famous (Thomas Schelling) and infamous (Walt Rostow) academics have tried and failed to use their scholarship in the service of successful public policies in the past, what hope does a graduate student or assistant professor have of getting his or her policy-relevant research in front of actual decision-makers? As for civilian academics who focus more on the “science” of their discipline and happily eschew the rewards of relevance, they may find Desch’s categorization of their contributions (or lack thereof) insulting.⁴²

The small handful of policymakers who read *Cult of the Irrelevant* or, more likely, read Desch’s own summary in *The Chronicle of Higher Education*,⁴³ will yawn. Desch says little that does not confirm what they already knew or suspected: The most academically focused social scientists do not often produce policy-relevant work on demand or in a palatable format.

For those government officials who do draw upon the work of academics and who might be discouraged by Desch’s book, as well as for academics who aspire to impact policy but despair over the stark choices implied by this description, take heart. The situation is both better and worse than Desch describes. It is better because academics and scholars do have an impact — just not in the ways that Desch seems to believe they ought to. It is worse because the reason policymakers make little use of academic research says more about their own environment and incentives than it does about scholarly research, analysis, and dissemination. If it is hard to change the nature of scholarship, it is likely even harder to change the behavior of policymakers, especially with regard to how useful they find scholarship to be.

What is the core of Desch’s book? He makes a simple observation: In advising policymakers, social scientists encounter a tension between “rigor and relevance.” For the most part, and

⁴¹ See, “New Voices in National Security,” Bridging the Gap Project,

<http://bridgingthegapproject.org/programs/new-voices-in-national-security/>.

⁴² In the interest of full disclosure, I have a small stake in the issue of policy relevance. For the last 21 years, I have been a professor in the Center for Naval Warfare Studies, the research arm of the U.S. Naval War College in Newport, RI. Much of our work sits at the intersection of theory and practice.

⁴³ Michael C. Desch, “How Political Science Became Irrelevant: The Field Turned Its Back on the Beltway,” *Chronicle of Higher Education*, Feb. 27, 2019, <https://www.chronicle.com/article/How-Political-Science-Became/245777>.

with relatively few exceptions, social scientists tend to prioritize rigor over scholarly relevance. In practice, then, social scientists have not been nearly as useful for policymakers as they might have been had they been concerned with the utility of their work for officials. Even when employed in (or by) governmental research and analysis organizations or incentivized through public and private partnerships (including government funding for social science research), social scientists tend to produce basic, as opposed to applied, research. They seek answers to foundational questions and develop generalizable propositions rather than contribute to the immediate policy questions of the day, whether they be the problems of economic development, how to achieve success in counterinsurgency operations, or developing new approaches to counterterrorism. If, as is often the case, policymakers require “close enough” answers quickly, social scientists often aspire to unnecessary precision.

What Do Policymakers Really Want?

The arguments offered in *Cult of the Irrelevant* are in many ways appealing. The underlying assumption that social scientists can, if they choose to, influence public policy, is comforting. The argument also appeals to policymakers who have been disappointed by their interactions with academics. It is not policymakers’ fault that academics have failed to help. Rather, it is the fault of obtuse eggheads who prefer exactness and the scientific method to utility and practicality. *Cult of the Irrelevant* may even appeal to those conducting policy-irrelevant research. After all, what is nobler than laboring for knowledge itself rather than satisfying the needs of the state? There is a certain purity to staying above the fray, accumulating wisdom and allowing knowledge to seep into the public consciousness by osmosis.

At the same time, *Cult of the Irrelevant* is oftentimes more notable for what it does not say than for what it does. Although Desch concludes that policymakers find academic national security scholarship less-than-relevant, overly reliant on esoteric methods, or too narrowly focused on precision, Desch rarely quotes, or even cites, specific decision-makers (much less describes their underlying decision-making processes) on this issue. His two detailed cases actually involve academics who, for better or worse, played enormous roles in shaping policy in two distinct areas: bargaining theory, as applied to Vietnam and nuclear deterrence (Schelling), and economic development, again as applied to Southeast Asia

(Rostow). Both are *sui generis* in their respective intellects, connections, and pedigrees, as well as the opportunities provided by the times in which they lived. Having Schelling and Rostow serve as stand-ins for all scholars is, consequently, a questionable choice. Although both cases are fascinating in and of themselves, they do not necessarily support Desch's wider thesis about "rigor versus relevance." Schelling was most certainly as rigorous as he was relevant, and Rostow was far less rigorous but equally relevant. Most importantly, Desch does not convincingly trace their failures (or successes for that matter) to their methods or precision.

Furthermore, although Desch acknowledges that the Department of Defense provides the lion's share of funding for social science research, he does not present direct evidence from the officers and civilian defense officials who commission this work. Why not? Likewise, a number of important military strategists have studied the social sciences and earned doctorates: Gen. David Petraeus (Ph.D., Princeton) and Gen. H.R. McMaster (Ph.D., University of North Carolina), as well as Adm. James Stavridis (Ph.D., Fletcher School of Law and Diplomacy), all of whom are relatively prolific authors who have nevertheless not sought to contribute to general academia, come immediately to mind. What would they say about the tension between rigor and relevance, if asked? They were, after all, trained in much the same way as the rest of us civilian academics were, graduated from similar programs, and, most importantly, had ample opportunity to work with scholars as senior military leaders. Personally, I expected more such evidence from *Cult of the Irrelevant* and fewer assumptions and second-hand accounts.

Another omission in the book is its lack of attention to alternative explanations. In the concluding chapter, Desch does raise — and quickly dismiss — seven potential objections to his analysis.⁴⁴ This, however, is inadequate, for none of the various objections he discusses amounts to a fully-detailed alternative explanation. Here is one possible explanation worth exploring further: For professional reasons, policymakers generally have limited interest in academic output. Politicians, policymakers, and military officers are often only interested in the theories and evidence that support their own professional needs.

⁴⁴ Michael C. Desch, *Cult of the Irrelevant: The Waning Influence of Social Science on National Security* (Princeton, NJ: Princeton University Press, 2019), 243–48.

This, of course, raises an important question: Do policy and decision-makers actually want knowledge from social scientists? The answer is yes — but only within very strict limits. They want research that supports their personal, policy, and institutional agendas. They are far less concerned with crafting optimal policies or resolving the world’s enduring mysteries. Knowledge for knowledge’s sake is far less attractive — especially if precious time, money, and energy are necessary to acquire it — than information and analysis that supports preexisting positions, preconceived answers, or professionally rewarding options.

Nor does *Cult of the Irrelevant* sufficiently examine the role of theory in policymaking. In his discussion of the potential objections to his argument, Desch identifies the democratic peace theory and realism, especially neorealism, as not especially useful for policymakers. Yet, these are grand theories. In the case of realism, it might better be understood as a paradigm, given the rather large array of variants that coexist uneasily under its rubric (defensive, offensive, structural, and so forth). Realist thinking tends to support generalities such as the well-known preference of most states for acquiring military power, but it has relatively little to say about more specific questions, such as what types of forces are required, how much power is sufficient, and how the state should employ that power. Many scholars have hypothesized about such matters,⁴⁵ but relatively few are especially reliable as guides to action and implementation.⁴⁶

With this in mind, Desch does not pay sufficient attention to the importance that mid-level theories have for policymakers, especially those charged with implementing policy decisions. Classically middle-range theories, in comparison to grand theories like realism, are relatively limited in scope and seek to explain only a specific set of phenomena. In David

⁴⁵ For a classic overview of the arguments about the relationship between one major variant of realism and foreign policy, see Gideon Rose, “Neoclassical Realism and Theories of Foreign Policy,” *World Politics* 51, no. 1 (October 1998), 144–72, <https://www.jstor.org/stable/25054068>.

⁴⁶ For a famous example of the difficulties faced by realists (and neo-conservatives for that matter) in advising the U.S. government during the run up to the Iraq war, see, Brian C. Schmidt and Michael C. Williams, “The Bush Doctrine and the Iraq War: Neoconservatives Versus Realists,” *Security Studies* 17, no. 2, (2008), 191–220, <https://doi.org/10.1080/09636410802098990>.

Lake's words, "mid-level theories ... focus on parts of the political process, rather than the whole, and study the effects of one or more variables on policy choice and outcomes."⁴⁷

Understanding the political process and specific key variables, especially ones a policymaker might possibly control or influence, is of great interest to those in the policy world. Policymakers already come equipped with baseline assumptions and overall worldviews. They do not have the inclination or time to seek overarching explanations about, for example, the nature of the international system or the fundamental characteristics of democracy. They might, however, be attracted to mid-level theories because they offer insights about manageable parts of the political process.

But Desch does not really discuss the utility of mid-level theories for policymakers. To be sure, he acknowledges the policy importance of democratic peace theory for the Clinton, George W. Bush, and Obama administrations. But in contrast to Lake, Desch does not really explore its relevance as a mid-level theory, much less how it influences detailed programs within, for example, USAID or the Department of Defense's military assistance programs, or America's position within international organizations.⁴⁸ Rather, he focuses on the theory's normative and quantitative origins, which obscure, rather than illuminate, its important impact at the level below senior leadership.

In my experience, senior defense officials and military leaders are, contrary to Desch's arguments, often frustrated by the *lack* of precision in social scientists' data and methods. Take for example the very hot topic of cyber operations. Strategists, planners, and programmers have been searching for at least a decade for better and more precise models, especially regarding causal relationships. Indeed, they long for predictive power. They would love to be able to launch an information operation and have confidence that its impact would be of the desired scale and scope. Yet, with regard to cyber operations, as with so many social phenomena, social science is, at best, suggestive or probabilistic, at least from the point of view of the decision-maker tasked with taking actions that might mean life or death. If models or inferences based on probabilities imply that an adversary

⁴⁷ David A. Lake, "Theory Is Dead, Long Live Theory: The End of the Great Debates and the Rise of Eclecticism in International Relations," *European Journal of International Relations* 19, no. 3 (September 2013), 572, <https://doi.org/10.1177%2F1354066113494330>.

⁴⁸ For Lake's discussion of democratic peace theory as a mid-level theory, see Lake, "Theory is Dead, Long Live Theory."

will respond in a certain way to cyber-attacks but the adversary responds differently, and if decision-makers base their actions even in part on those models, then the amount of force that may be necessary to achieve certain objectives may increase. Depending on what types of military instruments are then employed — manned aircraft or ground troops, for example — the likelihood of casualties will be greater (than if cyber instruments were both used and effective).

Where Scholarship *Is* Having an Impact

One way that scholarship is having an impact on policymaking, which Desch does not discuss, is through academics who are close to policymakers in the military. No, not think tank denizens, but the scholars who work in professional military education, including the war colleges. There are at least two types of scholars in this system: those charged with teaching mid-career and senior officers and those who conduct research and analysis at the behest of the U.S. government and, more specifically, the military service that employs them. The teaching and research faculties in these institutions include traditional scholars educated by some of the most prestigious universities in the world — including schools like Harvard, Princeton, the University of Chicago, Berkeley, and Oxford — and who publish in the most prestigious outlets for national security scholarship, such as *International Security*, *Security Studies*, and the *Journal of Peace Research*. They also write reports, provide briefings, advise civilian and military leaders, contribute to white papers, collect data, and design war games of all sorts to influence national security policy.

But professional military institutions also often host national security journals and other publications with at least a veneer of scholarly credibility. Here I would include the *Naval War College Review* (I was the editor in the early 2000s), *Parameters*, and *Strategic Studies Quarterly*. Rarely do these journals publish new theoretical work — much less sophisticated quantitative analyses — but they do provide a venue for engaging the officer corps and senior officials. Serious social scientists do contribute to these journals, either with accessible variants of their more “academic” work or policy arguments on pressing national security issues. Articles in these venues often result from, or encourage, long-term engagement between policymakers, military leaders, and civilian academics.

The point here is that policymakers do not have to delve into the *American Journal of Political Science*, the *European Journal of International Relations*, or *International Interactions* to find policy-relevant security studies scholarship. Alternative scholarly venues do exist, and a number of scholars with a traditional background often take advantage of them. While this does not undermine Desch's more general point about the relevance of social scientists, it does suggest that he has overstated the case.

Of equal importance, there is little likelihood that members of the intelligence community or various policy analysis organizations within the national security apparatus like the Department of State's Policy Planning Staff, the Office of the Secretary of Defense, or the National Intelligence Council will cease looking to scholars for ideas and research. They are even less likely to stop awarding grants or seeking counsel from those with substantive knowledge about important areas of the globe — China or Russia, for example — or relevant policy issue areas like nonproliferation and arms control. Even if policymakers believe, like Desch, that scholars focus too much on method, quantification, and questions that are not of immediate interest, they will rely, one way or another, on John Maynard Keynes's famous scribblers.

In other words, Desch frames the whole issue of policy relevance too narrowly by focusing almost exclusively on the direct transmission of scholarly findings into official documents or policies. There are, in fact, a number of less direct ways to educate policymakers and to shape the general national security discourse. Many, if not most, officials within the national security apparatus have advanced degrees not just in technical disciplines but in the social sciences or the law as well. Even if they are not up to date on cutting edge or explicitly tailored research, they are largely conversant with the paradigms, theories, and concepts underpinning international relations, international security, and national security decision-making.

The advantage of my particular perch for reviewing Desch's book is that I have witnessed firsthand the interactions between policymakers and senior military officers on the one hand and academics of all sorts on the other in a variety of settings for nearly 30 years. This vantage point leads me to be sympathetic to Desch's concerns but far more optimistic about the state of the social sciences and their future contributions to developing better laws, policies, and programs. I am especially optimistic that the emerging generation of scholars will make choices conducive to policy engagement. Depending on their own

preferences and incentives, they will either develop an understanding of the needs of policymakers or stay within the narrower confines of disciplinary obsessions. For their part, politicians, policymakers, and military leaders will continue to seek out, and even fund, academic analysis, but no one should be surprised or disappointed that there is nothing like a one-to-one correspondence between scholarly research and policy outputs. After all, decision-makers have diverse motivations and operate in complex political and bureaucratic environments.

I am quite confident that those social scientists more interested in having a policy impact will find ways to do so while those less interested in applied research will make different choices. Why not simply leave the policy business to those either willing to be employed in positions inside the system or those entrepreneurial enough to negotiate the well-known hurdles to making an impact on public policy? Academics perform many other important societal functions — from teaching to public outreach — that have little to do with being directly relevant to policymakers.

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4. Statecraft and Scholarship

By John H. Maurer

Bismarck held professors in contempt. When it came to matters of blood and iron, the arch realist of politics — both high and low — derided professors as hopeless and useless. “There is no exact science of politics,” he insisted. “Only professors are able to package the sum of the changing needs of cultural man into scientific laws.”⁴⁹

Alas, Bismarck has not been alone among those holding high office in ridiculing professors as unable to offer practical advice. That those serving in government ignore scholars should not come as much of a surprise. The Greek root for scholar after all is to possess free (or leisure) time. Those in government, however, pride themselves on having no leisure time. They must run things, caught up in the day-to-day whirlwind of events, taking care of urgent business.

Of course, too much is at stake in this clash of cultures for each side to ignore the other. Sober analysis is, after all, the bedrock of good policy. The talent residing in the academic community is a national treasure that the government and armed services should draw upon in crafting policy and strategy. Former Secretary of Defense Robert Gates judged it as a national security imperative that government leaders “embrace eggheads and ideas.”⁵⁰

Still, even when government leaders respect the work of scholars, difficult questions bedevil partnerships between the groves of academe and the corridors of power. What is the appropriate role for scholars to play in policymaking? What barriers stand in the way of meaningful collaboration between gown and government? Can those barriers be overcome so that the talent of the academic community can be harnessed to make better policy and enhance national security? What responsibility does the academic community have in directing public discussion about policy? What temptations corrupt scholars so that they

⁴⁹ Otto Pflanze, *Bismarck and the Development of Germany, Vol. 1: The Period of Unification, 1815-1871* (Princeton, NJ: Princeton University Press, 1963), 88.

⁵⁰ Quoted in Michael Desch, *Cult of the Irrelevant: The Waning Influence of Social Science on National Security* (Princeton, NJ: Princeton University Press, 2019), 1.

turn away from following the evidence and instead politicize their research findings? How can social science disciplines encourage the study of policy-oriented questions by scholars?

Political Science and the Rise of America as a World Power

Michael Desch's *Cult of the Irrelevant* explores the difficulties that security studies scholars have faced in sorting out these thorny questions in practice from the 1890s down to the present day. Desch places this intellectual history alongside a narrative of the rise of the United States as a world power and then as leader of the international system. Desch also is interested in examining how the discipline of political science, in a quest to develop formal theory in imitation of the hard sciences, thinks too little of the work produced by policy-minded scholars. He laments the baneful consequences of what he calls a contest between rigor and relevance.

The role played by political science professors in guiding American policy is a remarkable story. At the beginning of the 20th century, one of the country's foremost scholars in the discipline, Woodrow Wilson, observed that the rising role of the United States in world affairs would alter the traditional checks and balances among the branches of the government within the American political system, strengthening the power of the presidency. This transformation of the United States into a world power entailed that the office of the

President can never again be the mere domestic figure he has been throughout so large a part of our history. The nation has risen to the first rank in power and resources. Our President must always, henceforth, be one of the great powers in the world, whether he act greatly and wisely or not.⁵¹

Wilson would put into practice what he wrote about the power of the presidency, certainly acting greatly, if not always wisely. When Germany crossed the red line of resuming unrestricted submarine warfare in early 1917, the bespectacled ivy-league professor-turned-president brought the United States into the Great War. As commander-in-chief, Wilson authorized the deployment of 2 million American soldiers to go abroad in search of the

⁵¹ Woodrow Wilson, *Constitutional Government in the United States* (New York: Columbia University Press, 1908), 59, 78–79.

monster of Prussian militarism to destroy. As American soldiers fought on the killing fields of the Western Front, Wilson banged out diplomatic notes and war termination terms on his Hammond typewriter to bring about regime change with the overthrow of the Kaiser. The typewriter proved mightier than the sword. What would the shade of Bismarck think about the downfall of the Prussian-German Empire at the hands of an American political science professor?

At the war's end, the American people would reject Wilson's vision of the United States playing a leading role in providing for European security. Wilson's league to enforce the peace, his critics maintained, violated the traditional American foreign policy axiom to avoid entangling alliances and reduced the role of the Congress in determining the national interest. Wilson's views ran too far ahead of domestic opinion. In undertaking a nationwide speaking tour to persuade the American people to take on a larger international role — and to prepare to run for a third term — Wilson destroyed his health and whatever prospects of the United States underwriting security guarantees to other countries overseas. It would take another world war before the American people proved willing to make security commitments to countries in Europe and Asia.

Desch examines the role social scientists played in the tortured relationship between domestic political opinion and America's international behavior during World War I and the interwar period.⁵² Between the world wars, the best-selling political scientist and historian Charles Beard repudiated Wilson's deployment of massive American military power abroad in the Great War and his attempt to establish a new system of international security with the United States playing a leadership role. Beard feared that, in the search to destroy the monster of militarism abroad, the United States would erect its own militaristic regime and thereby destroy American freedoms. Beard therefore wanted the United States to show restraint in its ambitions abroad.

Other political science scholars disagreed, arguing against isolationism and hemispheric defense as a grand strategy for the United States. Desch, for example, recounts the rise and fall of Yale's fabled Institute for International Studies.⁵³ At Yale, William T. R. Fox produced the wonderful book *The Super-Powers*. Fox took to task Beard and those who "advocated

⁵² Desch, *Cult of the Irrelevant*, chap. 2.

⁵³ Desch, *Cult of the Irrelevant*, 39–41.

the ‘isolationist’ way out of the game [of power politics], which meant simply refusing to play.” Fox stated an obvious tenet of realism: “power politics is not a game but a central feature of the organized political life of the twentieth century, it makes the most enormous difference in whose hands predominant power rests.”⁵⁴ Realism, he wrote, must take into account the ideological aims and potential military capabilities of adversaries to project power.

Also at Yale, Nicholas Spykman examined the alternative courses of action confronting the United States in a world at war. Innovative maps by the brilliant cartographer Richard Edes Harrison accompanied the book. Spykman argued against a strategy of hemispheric defense. He advocated for the United States to promote balances of power along the rimlands of Eurasia. We live in the intellectual shadow of *America’s Strategy in World Politics*, Spykman’s treatise on geopolitics and grand strategy.

Writing just as the United States entered the fighting, Spykman looked not only at the war to be won but also ahead to the international distribution of power after the final defeat of Nazi Germany and Imperial Japan. In a passage that has an eerie ring today, Spykman foresaw a day when a “modern, vitalized, and militarized China of 400 million people is going to be a threat not only to Japan, but also to the position of the Western Powers” in the Western Pacific. “It is quite possible to envisage the day when this body of water will be controlled not by British, American, or Japanese sea power but by Chinese air power.”⁵⁵ The anti-access and area denial problem described by Spykman confronts current-day American planners preparing for war with China.

Security Studies, the Nuclear Revolution, and the Cold War

Cult of the Irrelevant likewise makes a valuable contribution in describing the mobilization of social science during the Cold War.⁵⁶ The nuclear revolution confronted political leaders

⁵⁴ William T. R. Fox, *The Super-Powers: The United States, Britain, the Soviet Union—Their Responsibility for Peace* (New York: Harcourt, Brace, 1944), 6–7. In the acknowledgments, Fox thanked his wife, Annette Baker Fox, “in her capacity as a political scientist.”

⁵⁵ Nicholas John Spykman, *America’s Strategy in World Politics: The United States and the Balance of Power* (New York: Harcourt, Brace, 1942), 469.

⁵⁶ Desch, *Cult of the Irrelevant*, chaps. 4–7.

and military planners with a transformation of incredible proportions. Bernard Brodie's contribution to the book *The Absolute Weapon* is justly celebrated for comprehending this revolutionary development at an early date.⁵⁷ Brodie had already made his mark as a strategic thinker before the nuclear age. His book *Sea Power in the Machine Age* still deserves examination by anyone concerned with naval warfare, technological change, and maritime strategy.⁵⁸ Albert Wohlstetter's work also stood out for the clarity of its analysis in addressing the problem of enhancing the survivability of nuclear forces and denying first-strike incentives to an aggressor.⁵⁹ Meanwhile, Henry Kissinger would wrestle with the problems of relating overall national policy with strategies for the employment of nuclear weapons.⁶⁰ Paul Bracken would later raise awareness of the danger in thinking that command-and-control systems would withstand real world conditions and actually permit the use of nuclear weapons for strategic purposes.⁶¹ All of this work on nuclear weapons remains of the greatest importance in our own era of great power competition.

One important collaborative effort that is missing in Desch's account is the role of the Office of Net Assessment in sponsoring research by scholars. The late Andrew Marshall, as head of this office, reached out to scholars to promote research linked to current-day strategic concerns. Andrew Krepinevich and Barry Watts have written about Marshall in their book *The Last Warrior: Andrew Marshall and the Shaping of Modern American Defense Strategy*. Krepinevich and Watts approvingly quote Marshall as saying: "I'd rather have decent answers to the right question than great answers to irrelevant questions."⁶²

⁵⁷ Bernard Brodie, ed., *The Absolute Weapon: Atomic Power and World Order* (New York: Harcourt, Brace, 1946); and Bernard Brodie, *Strategy in the Missile Age* (Princeton, NJ: Princeton University Press, 1959).

⁵⁸ Bernard Brodie, *Sea Power in the Machine Age* (Princeton, NJ: Princeton University Press, 1941).

⁵⁹ Albert Wohlstetter, "The Delicate Balance of Terror," *Foreign Affairs* 37, no. 2 (January 1959).

⁶⁰ Henry Kissinger, *Nuclear Weapons and Foreign Policy* (New York: Harper, 1957).

⁶¹ Paul Bracken, *The Command and Control of Nuclear Forces* (New Haven: Yale University Press, 1983).

⁶² Andrew F. Krepinevich and Barry D. Watts, *The Last Warrior: Andrew Marshall and the Shaping of Modern American Defense Strategy* (New York: Basic Books, 2015), 1. A critical assessment of Marshall is offered in an article by Greg Jaffe, "U.S. Model for a Future War Fans Tensions with China and Inside the Pentagon," *Washington Post*, Aug. 1, 2012, https://www.washingtonpost.com/world/national-security/us-model-for-a-future-war-fans-tensions-with-china-and-inside-pentagon/2012/08/01/gJQAC6F8PX_story.html.

Marshall's aphorism addresses Desch's concern about the relevance of political science studies to decision-makers. Marshall sponsored projects that both focused on specific strategic problems as well as basic research. In the latter category, he supported a conference at Harvard's Kennedy School during the summer of 1980 that brought together an incredible array of scholars to write on intelligence assessment. The late Ernest May, who organized the conference, then edited the book *Knowing One's Enemies*.⁶³ Derek Leebaert, the founding managing editor of *International Security*, attended the conference in an effort to promote the relatively new journal that sought to publish policy-relevant scholarship such as that sponsored by Marshall. Another example is Aaron Friedberg's first book *The Weary Titan*. Friedberg acknowledged his debt to Marshall, who "planted the seeds from which this book grew. I am very pleased to add one more item to the long list of studies for which he has been directly or indirectly responsible."⁶⁴ The list is indeed long and impressive.

Marshall also held summer study conferences, typically in Newport at the Naval War College, where specific strategic problems received in-depth examination from invited scholars. With the end of the Cold War, Marshall made it a special task to think about the strategic contours of competition with China, even as many American elites wanted to downplay the notion of renewed great power geopolitical and military rivalries. Bracken writes that for "Marshall the return of great power rivalry was occurring at the same time as revolutionary changes in technology, and the reappearance of nationalism. The new advanced technologies, cyberwar, AI, and war in space were poorly understood."⁶⁵ The Office of Net Assessment provides an important example for how the academic community can deliver research that is both relevant and rigorous.

Another example of a government effort to engage the academic community was the Secretary of the Navy Fellows program that brought scholars to the Naval War College during the 1980s. Secretary of the Navy John Lehman was behind this initiative. The

⁶³ Ernest R. May, *Knowing One's Enemies: Intelligence Assessment Before the Two World Wars* (Princeton, NJ: Princeton University Press, 1984).

⁶⁴ Aaron L. Friedberg, *The Weary Titan: Britain and the Experience of Relative Decline, 1895-1905* (Princeton, NJ: Princeton University Press, 1988), xv.

⁶⁵ Paul Bracken, "Andy Marshall's Century," Foreign Policy Research Institute, E-Notes, April 8, 2019, <https://www.fpri.org/article/2019/04/andy-marshalls-century/>.

program was a precursor to and inspired the more recent Minerva project developed by Thomas Mahnken to support research professors in professional military education, examined by Desch.⁶⁶ Lehman himself had earned a PhD in political science at the University of Pennsylvania and worked on national security affairs at the Foreign Policy Research Institute, before heading into government service. He believed that the field of security studies and the discipline of history would benefit from establishing fellowships for scholars at Navy institutions. Alvin H. Bernstein, then chair of the Strategy and Policy Department at the Naval War College, led the effort to recruit scholars for Lehman's fellowships.⁶⁷

Bernstein brought together a mix of political scientists and historians to serve on the faculty of the Naval War College: Eliot Cohen, John Gooch, Holger Herwig, Williamson Murray, and Stephen Rosen were just some of the scholars who came to teach and write while in the Strategy and Policy Department. Cohen and Gooch, for example, collaborated to write the book *Military Misfortunes*, which grew out of a course they taught at the Naval War College.⁶⁸ Another major work in strategic studies produced during this period was Stephen Rosen's *Winning the Next War*. Rosen would write that the Strategy and Policy Department "gave him an intellectual home" to carry out his research and writing.⁶⁹ Lehman's initiative, however, proved short-lived, coming to an unfortunate end not long after his departure as secretary of the Navy, along with changes in the leadership of the Naval War College. Less enlightened naval leaders did not see much value in continuing the Secretary of the Navy fellowships to promote partnerships between scholars and sailors.

Security Studies and the Return of History

The end of the Secretary of the Navy Fellows program in Newport is of a piece with Desch's depressing account of the downturn in the field of security studies during the immediate

⁶⁶ Desch, *Cult of the Irrelevant*, 233–38.

⁶⁷ A tribute to Bernstein by Eliot Cohen can be found here: Eliot A. Cohen, "A Teacher Among Bureaucrats — the Legacy of Alvin H. Bernstein," <http://www.cornell61.org/bernstein.htm>.

⁶⁸ Eliot A. Cohen and John Gooch, *Military Misfortunes: The Anatomy of Failure in War* (New York: Free Press, 1990).

⁶⁹ Stephen Peter Rosen, *Winning the Next War* (Ithaca, NY: Cornell University Press, 1991).

aftermath of the Cold War. His section “No Fighting in the Classroom: The Waxing and Waning of Academic Security Studies since Vietnam” makes for essential reading for anyone in the field.⁷⁰ At the time, serious political scientists warned that new security challenges would emerge before long and that the end of history was nowhere in sight. Samuel Huntington, in *The Clash of Civilizations*, ridiculed the utopian attitude of “the president of, arguably, the world’s leading university [who] vetoed appointment of a professor of security studies because the need had disappeared. ‘Hallelujah! We study war no more because war is no more.’”

Of course, Huntington provocatively argued that the future would be marked by clashes of civilizations. Indeed, he maintained that the United States was already waging a quasi war in the Middle East, an asymmetric conflict marked by “terrorism on one side and air power, covert action, and economic sanctions on the other.”⁷¹ Other scholars, like Christopher Layne, also argued that great power challengers would eventually reemerge to contest the leadership of the United States in the international arena.⁷² Meanwhile, Bracken drew attention to how ballistic missiles and nuclear weapons threatened the ability of the United States to operate in the rimlands of Eurasia.⁷³ Scholars in the fields of international relations and security studies were alive to looming security challenges that would confront the United States in the aftermath of the Cold War.

The wars fought since the attacks on September 11 and the escalating tensions among the great powers make clear that history never ended. Current-day wars and rumors of war should give impetus to worthwhile policy-relevant research by scholars.

Desch concludes by emphasizing “the ethical obligation of the scholar to the rest of society.” Scholars are accustomed to think in a disciplined way about difficult problems. In our turbulent era, national security professionals in government, as well as an informed reading public, benefit from scholars who produce serious policy-relevant analysis. The

⁷⁰ Desch, *Cult of the Irrelevant*, 211–21.

⁷¹ Samuel P. Huntington, *The Clash of Civilizations and the Remaking of World Order* (New York: Simon and Schuster, 1996), 31, 216.

⁷² Christopher Layne, “The Unipolar Illusion: Why New Great Powers Will Rise,” *International Security* 17, no. 4 (Spring 1993): 5–51, <https://www.jstor.org/stable/2539020>.

⁷³ Paul Bracken, *Fire in the East: The Rise of Asian Military Power and the Second Nuclear Age* (New York: Harper Collins, 1999).

outlets for scholars to publish their work and reach diverse audiences also have never been greater. Desch is hopeful that policy-oriented scholars might yet gain recognition for their efforts among the great and the good at university, within departments of political science. We can only hope that his optimism is not misplaced and that the discipline of political science will heed his call to recognize “its moral obligation to answer the enduring relevance question.”⁷⁴

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⁷⁴ Desch, *Cult of the Irrelevant*, 254-55.