Why the Wizards of Armageddon Ran Into An Intellectual Dead-end: And What That Tells Us About the Relevance of Academic Nuclear Strategy Today

Paul C. Avey
Southern Methodist University
and
Michael C. Desch
University of Notre Dame
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I. Introduction.

International security has long been among the most policy relevant of sub-fields in field of academic international relations.¹ This is still the case today, at least as measured by the relative willingness of authors in top international relations journals to offer explicit policy recommendations. There is a significant difference in this regard, as Figure 1 shows, between articles since 1980 dealing with security issues (i.e., weapons of mass destruction, weapons acquisition, terrorism, and military intervention) and other issue areas in the field.²

But as Figure 2 shows, the willingness of leading international relations scholars to offer such policy recommendations has declined in absolute terms since 1980 and this drop may have something to do with the professionalization of the discipline of political science. 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. First, the increasing emphasis on quantitative research has led to narrower research questions which are often too abstruse or obscure to be relevant to policy. Second, many proponents of the scientific study of politics believe that advocacy of particular policies is incompatible with scientific objectivity and so avoid

² The Teaching and Research in International Politics journal coding project is described in Daniel Maliniak, Amy Oakes, Susan Peterson, and Michael J. Tierney, “International Relations in the Academy,” International Studies Quarterly, Vol. 55, No. 2 (2011)): 437-64. The data from which I generated these figures is available at http://www3.nd.edu/~carnrank/.
Third, many pressing policy questions are not readily amenable to the preferred methodological tools of social scientists. 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. The more “scientific” approaches to international relations scholarship seem to be the least relevant, at least as measured by their practitioner’s willingness to offer policy recommendations, as Figure 3 indicates. The problem, in our view, is not so much that “scientific” approaches to national security policy are irrelevant by definition; rather, their current dominance is a symptom of a larger trend among the social sciences to privilege method over substance, which Figure 3 suggests, with a resulting decline in policy relevance.

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Figure 1: Articles With Policy Prescriptions by Subfield
Since many believe it was nuclear strategy that first opened the door for the Cold War Wizards of Armageddon to shape nuclear strategy, and remains among the most relevant today, we believe that this issue area is a good one to explore when and under what conditions academic international
relations influences policy. “Academic strategists” such as Bernard Brodie, Albert Wohlstetter, Herman Kahn, William Kaufman, and especially Thomas Schelling reputedly exercised such influence that the period between 1945 and 1961 is regarded as the “Golden Age” of academic national security studies. For example, Brodie himself avers that “it is no exaggeration to say that all the distinctively modern concepts of military strategy, most of which have been embraced by the military services themselves, have evolved out of [the] ranks [of the ‘scientific strategists’].” Former RAND staffer Roman Kolkowicz claims that “the deterrence paradigm, developed by the defense intellectuals in the 1950s and 1960s, provided the central concept for the management of nuclear strategy.” Even then-Air Force Chief of Staff General Thomas White, a skeptic of civilian defense intellectuals in general, conceded that some of them knew more about some nuclear policy issues than he did.

Many scholars agree. “All the fundamental ideas about nuclear strategy came from civilians,” in Robert Jervis’ view. Retired Army General and former University of Pittsburgh Provost Wesley Posvar cautions that “the impact of this work [by academic strategists] is so great that any serious new study which treats of the present military situation in international politics, including the whole fields of deterrence and arms control, must take account of it.” Journalist Fred Kaplan was so impressed with their influence that he was inspired to use a theological metaphor to describe it: “the ideas of these thermonuclear Jesuits would have so thoroughly percolated through the corridors of power – and through their annexes in academia – that, at least among fellow members of the congregation, their wisdom would be taken almost for granted, their assumptions worshipped as gospel, their insight elevated to an almost mystical level and accepted as dogma.... [T]his small group of theorists would

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4 This is Joseph Kraft’s phrase in “The War Thinkers,” Esquire, Vol. 58, No. 7 (1962) 103.
8 Kraft, “The War Thinkers,” 103.
devise and help implement a set of ideas that would change the shape of American defense policy...”

Even root-and-branch critics of American academic strategists such as P.M.S. Blackett ruefully admitted that their “writings had a rather big influence.”

To be sure, not everyone is convinced that there really was such a Golden Age in which social science actually influenced U.S. national security policy. Pointing to the divergence between U.S. nuclear declaratory policy (in which defense intellectuals apparently had influence through the theory of Mutual Assured Destruction [MAD]) and actual operational doctrine and war plans (where in his view they did not), historian Bruce Kuklick presents the most sustained critique of the conventional wisdom. According to his account, “the men who actually made decisions were least concerned with scientific ideas of any sort” and he concludes that the ideas of strategists had “little causal impact,” save perhaps as *ex post facto* rationalizations for policymakers’ decisions. In his view, “none of the critical aspects of decision making had much to do with the prevalent model of American social science – ‘whispering in the ear of princes’ – the middle-range generalizations supposedly necessary to benign policy and learned in a Ph.D. course.” Deborah Larson agrees that even the dean of civilian nuclear strategists – Bernard Brodie – actually had little impact on the government or the wider public.

This debate about the extent of the influence of academic social scientists on nuclear strategy matters for several reasons. First, if civilian academic strategists had no influence on nuclear thinking during the early Cold War, there is little reason to expect them to have any today. Second, if there is no variation in their influence then this case would not be helpful in drawing conclusions about when and

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11 Kaplan, *The Wizards of Armageddon*, 11. Also see 32, where Kaplan points out that this trend began already during the Second World War.


under what conditions they might have it.\textsuperscript{15} Finally, it speaks to the larger question, in the words of a National Research Council study, whether there is any “relationship between basic [social science] research ... and programmatically useful results.\textsuperscript{16} Or to quote Aaron Wildavsky, “the real question ... is whether disciplined intuition (meaning theorizing) has something to offer the untutored kind.”\textsuperscript{17}

To answer this question, we will explore the changing relationship between academia and the national security policy during the so-called “Golden Age” of cooperation between the two realms from roughly 1945 through 1965, focusing particularly on the issue of nuclear strategy. We aim to establish the academic strategists’ actual influence (the value of dependent variable) as well as to explain their changing role in nuclear strategy (identifying the independent variable). Using this history we develop criteria for when scholarship can influence policy. We further demonstrate the plausibility of this framework by utilizing it to explain why the first post-Cold War wave of nuclear studies failed to influence policy.\textsuperscript{18} We then employ these criteria to determine when and how the current generation of academics working on nuclear issues might influence policy much as political scientist Robert Gilpin did for the role of natural scientists in U.S. nuclear weapons policy.\textsuperscript{19}

Briefly, we agree with Jervis that the Golden Age theorists’ “influence may be greater than Kuklick portrays,”\textsuperscript{20} but show that it waxed and then waned during the Cold War. It did so even before the Vietnam War, eventually reaching an intellectual dead-end. By this, we mean that intellectual


\textsuperscript{18} In a sense we are ‘testing’ the theory against new data, see King et al., \textit{Designing Social Inquiry}, 21-23, 30, 46.


developments within the academy – the privileging of theory and method over substantive relevance – reduced the relevance of the work of many academic defense intellectuals. What led to that, we suggest, was their growing embrace of Economic approaches to nuclear strategy. We are not arguing that sophisticated social scientific methods are necessarily incompatible with policy relevant scholarship; rather, just that when they are employed without attention to their appropriateness for the concrete question at hand, and pursued for internal disciplinary reasons such a theoretical elegance or congruence with our preferred image of what a “science” should be, they are not very helpful to policymakers. The increasingly abstract character of late Golden Age theories would eventually disconnect them altogether from real-world policy concerns by pushing nuclear strategy beyond MAD. The end of the Golden Age is thus a cautionary tale for today’s generation of nuclear scholars, many of whom in their effort to recreate a “science” of strategy seem once again to be emulating the methods and approaches of Economics that brought academic nuclear strategy to a dead-end.21

This paper proceeds in three sections: We begin by tracing and then explaining the waxing and waning of the Golden Age of academic nuclear strategy to develop criteria for influence. Next we apply these insights to post-Cold War nuclear theorizing including the optimist-pessimist debate and the purported “renaissance” in nuclear studies. We conclude with some recommendations for how to balance rigor with relevance so that the current wave of academic nuclear theorizing can avoid ending up once again in an intellectual cul-de-sac.

II. The Waxing and Waning of the Golden Age” of Academic Nuclear Strategists

Given that there is some debate about the actual influence of the “Golden Age” civilian nuclear strategists, our first task is to establish whether they in fact had any. Having established that they did, at least early on, we need to consider two explanations for the waning of their influence. The conventional

wisdom is that it ended as a result of the political turmoil of the Vietnam War which delegitimized collaboration with the U.S. Government among academics. While there is some truth in that view, we suggest that it was the increasing influence of Economic approaches to strategy that further undermined the relevance of academic strategy by privileging internal disciplinary concerns like theoretical eloquence and the use of sophisticated methods over addressing concrete policy problems.

*The Influence of Academic Nuclear Strategists During The Early Cold War*

Ascertaining whether academic nuclear strategists had influence will be challenging for two reasons: As political scientist John Kingdon warns, the influence of academics upon policy debates is often “hidden” and the secrecy surrounding national security decision-making makes their role in nuclear strategy even more opaque. Moreover, such an exercise partakes of the more general challenge of tracing the influence of ideas – the currency of academics – upon policy outcomes. Given that, our expectations for what sort of influence they could have need to be reasonable. As Bruce Smith notes, “the end product of most planning and research activities is not an agenda of mechanical policy moves for every contingency – plainly an impossible task – but rather a more sophisticated map of reality carried in the minds of the policy makers.” Therefore, we need to look more closely at the history of the Golden Age, which we believe shows that some of these ideas about the effects of nuclear weapons upon statecraft – particularly the Nuclear Revolution and MAD – did have real impact on Presidential crisis decision-making at certain junctures even if they did not shape operational plans and doctrine.

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Two related events at the end of the Second World War set the stage for the longest peacetime period of national security policy cooperation between the academy and the U.S. Government. These were the invention and use of the Atomic Bomb against Japan in the last weeks of the war and then the outbreak of the Cold War two years later. Not surprisingly given Yale University’s role during the Second World War, the locus for thinking about the implications of the “nuclear revolution” was at its Institute for International Studies. It was there that political scientist Bernard Brodie first argued that the development of these weapons would fundamentally change the nature of international relations.

For much of history, in Brodie’s view, military force had been a viable instrument of statecraft. But with the advent of nuclear weapons, only the threat of the use of force remained available to statesmen because the actual use of nuclear weapons would be mutually catastrophic. Brodie foresaw that the development of the H-bomb would tilt the cost/benefit ratio against war permanently: “Thus far the chief purpose of our military establishment has been to win wars. From now on its chief purpose must be to avert them. It can have no other useful purpose.” It was actually Brodie’s mentor University of Chicago economist Jacob Viner who drew the logical implication of his student’s argument that nuclear weapons were only useful for deterrence. All these ideas crystalized in a volume of essays Brodie edited for IIS entitled *The Absolute Weapon* which established the contributors as the country’s

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27 See the discussion of Brodie’s influence in Herken, *Counsels of War*, xi-xii.
leading experts on nuclear strategy, their services much in demand in government and the military. With that, the institution of the civilian “defense intellectual” was established.

Because they were so revolutionary, nuclear weapons would not remain the exclusive purview of the uniformed military. After all, it was natural scientists, many with academic affiliations, who had invented the bomb in the first place, and so it was not surprising that it would be academic social scientists, some of whom remained in the academy and some of whom migrated to intellectual half-way houses like the new RAND Corporation, who played the key role in clarifying their effects upon statecraft. Civilians could claim expertise in the nuclear realm because, as RAND economist and later Department of Defense Comptroller Alain Enthoven told one Air Force General, “‘I have fought just as many nuclear wars as you have.’” Brodie added a second rationale: In a world of MAD, only pre-war political decisions really mattered, which were squarely in the realm of civilian expertise.

In addition to Yale, there were social scientists at other universities whom policymakers consulted on nuclear strategy. The staff of the Weapon System Evaluation Group, for example, an organization established in 1948 to advise the Secretary of Defense and the Joint Chiefs of Staff on the various technical aspects of weapons systems, reached out to M.I.T. professor Max Millikan and a group of eight other social scientists to solicit their views on how many casualties the United States would have to inflict in order to break the Soviets’ will to continue to wage war. Not surprisingly, they concluded that “there presently exists no basis on which to assess quantitatively the effects on the Soviet will to continue to fight.”

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32 As Gilpin, *American Scientists & Nuclear Weapons Policy*, 112-21 shows, in the late 1940s and early 1950s, academic physical scientists at MIT and Cal-Tech continued to address strategic issues through various projects such as “Hartwell” (undersea warfare), “Charles” (continental air defense), “Lincoln” (radar), “Summer Lincoln” (active air defense), “East River” (passive air defense), and most importantly “Vista” (diversification of delivery systems for America’s nuclear weapons systems).
General Hoyt Vandenberg from December 1950 throughout 1951 helping the Air Force select Soviet targets. He was, in fact, the first civilian to review the U.S. emergency war plan.

All was not sweetness and light in the groves of post-World War II academe, however. As Herken argues, “there was an early tension ... between the scientists at RAND and their counterparts at colleges and universities,” which he attributes to the political differences between the two groups. Dissension among the defense intellectuals themselves also undercut their influence on nuclear strategy. Brodie was slated to conduct a major study of nuclear targeting for Air Force Chief of Staff Hoyt Vandenberg but was “abruptly terminated” before it was completed after Edward Earl Meade black-balled him with Secretary of the Air Force Thomas Finletter.

But it was the intellectual tensions between scholarly rigor and policy relevance which were the major obstacle to sustained cooperation. For example, the winds at Yale changed dramatically when President Seymour was replaced by A. Whitney Griswold, who disapproved of the defense research IIS was doing and closed it down. The core philosophy of the Institute’s work was, in Dunn’s words, “the development of a more disciplined type of policy analysis.” However, this approach still 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

38 Herken, Counsels of War, 75.
39 Kaplan, The Wizards of Armageddon, 49, note, lays out the compelling circumstantial case. Also see, Herken, Counsels of War, 32.
favor of “domestic’ political science.”

By 1954, Yale had lost six of the core IIS members and its main activities – policy research and *World Politics* – to other institutions.

As a result of more wide-spread lack of enthusiasm among universities for doing policy-relevant national security research, interest grew in finding an alternative way to marshal academic resources for the Cold War. The key proponent was Air Force General Henry “Hap” Arnold, whose experience working with physical scientists at MIT during the war on projects such as radar convinced him that the government ought to find a way to institutionalize this cooperation after the war. In September of 1945 Arnold convened a meeting at Henderson Field in California with, among others, the head of Douglass Aircraft Corporation, and announced a $30 million contract for Project RAND.

As journalist Joseph Kraft put it, “a main part of [RAND’s] job was to bridge the ancient gulf between scholars and soldiers.” It was in many respects the ideal intellectual environment: It was interdisciplinary, it attracted some of the brightest minds in the national security field, it gave staff access to classified information without imposing military discipline, and, best of all, there were no teaching obligations. While RAND was not the first “think tank” by any means, its success in the national security realm in the early Cold War led to their proliferation in number and growth in influence. This new Federally Funded Research and Development Center’s (FFRDC) initial focus was on the natural science, technology side of civilian defense work, but quickly came to include a large number of social scientists as well.

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41 Roger F. Evans, “Interview with David N. Rowe regarding international relations vs. political science,” October 2, 1951 at http://rockefeller100.org/archive/files/8679dfe2866bb4a1c589b1de63291b67.pdf.
The founder of RAND’s social science division was a former student of Viner’s at the University of Chicago, a polymath by the name of Leo Rosten. In addition to writing Hollywood screen-plays and books on Yiddish, Rosten had an advanced degree from the London School of Economics and earned a fellowship from the Social Science Research Council before serving in the Office of War Information during the Second World War. He was charged with recruiting top-notch academic talent to work at RAND. RAND social scientists made a number of contributions to policymaking spanning the gamut from softer social science analyses – a good example here is Nathan Leites seminal work on Soviet strategic culture and the “operational code” of the Bolshevik leadership, which reportedly influenced Paul Nitze’s thinking in drafting National Security Council Memorandum 68 – to much harder social science approaches that embraced the cutting edge methodologies of Mathematics and Economics and applied them to defense problems.

Perhaps RAND’s most important national security research involved how the United States ought to think about nuclear strategy once the Soviet Union developed its own nuclear capability. While a number of RAND analysts studied this issue, the most influential was Albert Wohlstetter, later a professor of political science at the University of Chicago. A trained mathematician, Wohlstetter was, like Rosten, a man of broad interests, ranging from business to the philosophy of science. During the Second World War, Wohlstetter served in diverse branches of the U.S. Government from the War Production Board to the National Housing Agency. Wohlstetter’s first project for RAND was his famous “basing study” which explored how the Strategic Air Command’s dependence upon a small number of air bases might make it vulnerable to a Soviet first strike, thus eliminating a large part of the United States’ nuclear arsenal. While he struggled to get SAC to concede this vulnerability, Kaplan concludes that in the end Wohlstetter’s study “finally led SAC to reduce its dependency on elaborate overseas

bases .... There is no question that the Wohlstetter study helped reduce this reliance.” Smith corroborates that the basing study eventually lead to Air Force policy shifts after an Air Staff committee signed off on its recommendations.

Wohlstetter’s second contribution to U.S. nuclear strategy was the research that led up to his famous January 1959 Foreign Affairs article “The Delicate Balance of Terror,” in which he argued that the United States could not depend upon the continuing invulnerability of its nuclear deterrent. Wohlstetter’s analysis of the vulnerability of U.S. Intercontinental Ballistic Missiles overcame Air Force skepticism that “hardening” them in concrete silos would serve to mitigate their vulnerability. It not only bolstered Democratic critics of the Eisenhower strategy of Massive Retaliation, but the general RAND approach to defense analysis (quantitative data analyzed using sophisticated methods such as operations research or systems analysis), which Wohlstetter epitomized, also raised the bar for strategic analysis both inside and outside of the U.S. Government.

While a number of academics including Henry Kissinger and Robert Osgood were skeptical of the Eisenhower Administration’s policy of “Massive Retaliation,” the most trenchant critic of the strategy of relying solely on nuclear weapons to defer a conventional Soviet attack was Princeton University political scientist William Kaufmann. Like Rosten and Wohlstetter, Kaufmann had an eclectic background that included Yale, Wall Street, and war-time service in the U.S. Army. His Princeton Center for International Studies report “The Requirements of Deterrence” landed the “critical blow” in the public debate about whether to rely exclusively upon the threat of nuclear retaliation to deter a

51 Kaplan, The Wizards of Armageddon, 110. Also see Trachtenberg, History and Strategy, 19 who concludes that Wohlstetter’s conclusions “were enormously influential;” and Morton H. Halperin, “The Gaither Committee and the Policy Process,” World Politics, Vol. 13, No. 3 (April 1961): 366, fn. 21, which argues that the “committee’s proposals on strategic vulnerability were heavily influenced by a classified RAND report prepared under the direction of Albert Wohlstetter.”
52 Smith, The RAND Corporation, 233.
54 Herken, Counsels of War, 156.
Soviet conventional attack upon Western Europe. It attracted the attention of Hans Speir, the new head of the RAND social science division, for whom Kaufmann began doing summer consulting. His recommendation that the United States engage in a build-up of its conventional forces not only appealed to the bureaucratic interest of the U.S. Army but also became a major plank in Kennedy’s campaign platform.

In addition to renewed attention to conventional forces, the other result of Kaufmann’s work was to spur interest in the use of strategic nuclear forces to limit damage from the Soviets’ nuclear arsenal. “Counter force” was a response not only to the growing problem of the credibility of the United States’ extended deterrent in Europe, but it also represented a possible means for limiting damage and avoiding bloody “counter value” exchanges against cities and population centers. Kaufmann’s work was influential not only among defense intellectuals, but also in the U.S. Air Force, where it was viewed as a potent bureaucratic weapon in its war against the U.S. Navy, whose Submarine Launched Ballistic Missiles were not yet accurate enough to conduct such strikes. The peak of his influence would come when Secretary of Defense Robert McNamara would announce his “no cities” doctrine in two speeches in Athens, Greece and Ann Arbor, Michigan in 1962.

While the Eisenhower Administration had, ironically given his dislike of civilian strategists, started the trend of relying upon outside experts in strategic affairs, the high water-mark for the influence of academic defense intellectuals was the Kennedy Administration. Throughout the 1950s a group of Harvard and MIT faculty played an increasingly public role in the debate about U.S. defense

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60 Kaplan, *The Wizards of Armageddon*, 245.
policy, including as members of the academic advisory group during the 1960 presidential campaign. It members were a veritable who’s who of academic luminaries, including Kissinger, John Kenneth Galbraith, Carl Kaysen, Paul Samuelson, Richard Nuestadt, and Arthur Schlesinger, Jr. A young Harvard graduate student by the name of Daniel Ellsberg, who was working at RAND while finishing his Ph.D., served as a back channel to the campaign for staffers there like Wohlstetter, Enthoven, Harry Rowen, and Fred Hoffman.

Kennedy drew liberally upon universities and other repositories of “the best and the brightest” throughout his administration, but the appointment of Ford Motor Company President (and former Harvard Business School professor) Robert McNamara as Secretary of Defense opened wide the Pentagon doors to civilian defense intellectuals from RAND and academia. The social scientists whom McNamara recruited to the Pentagon were collectively referred to as the “Whiz Kids” in recognition not only of their brilliance, but also their youth. These young analysts included people like Kaufmann, who had left RAND and was then teaching at MIT; Enthoven; the Harvard economist Thomas Schelling; and former Oxford don Charles Hitch, then the director of the Economics division at RAND.

The infusion of these brash, young intellectuals was not universally welcomed among the military. The Air Force had chaffed at Brodie’s meddling in war planning in the early 1950s but the Whiz Kids presented a much greater threat given their numbers and support from the Secretary of Defense. Former Air Force Chief of Staff General Thomas White confessed that “in common with many other military men, active and retired, I am profoundly apprehensive of the pipe-smoking, tree-full-of-owls type of so-called professional ‘defense intellectuals’ who have been brought into this nation’s capital. I don’t believe a lot of these often overconfident, sometimes arrogant young professors, mathematicians

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63 Kaplan, The Wizards of Armageddon, 195.
and other theorists have sufficient worldliness or motivation to stand up to the kind of enemy we face.’”

Even some civilians in government took the view that the value of asking defense intellectuals about nuclear strategy “‘was like asking people in the street.’” Still, it would be a mistake to think that these attitudes led the uniformed military to dismiss the “defense intellectuals” out of hand. In light of McNamara’s patronage, the services had to learn to beat the Whiz Kids at their own game of systems analysis. In that sense, the McNamara revolution endured well beyond his tenure and eventually would spread throughout the bureaucracy of the U.S. government.

So what can we conclude about the influence of civilian strategists during the “Golden Age”? The primary evidence that Kuklick adduces concerning the irrelevance of civilian theories of the nuclear revolution and MAD is that they were not reflected in the military’s actual war plans and operational doctrine. But evidence that academic notions of MAD were not reflected in actual war plans and military doctrine hardly constitutes proof that these ideas were irrelevant. Even though they did not influence the military, civilian nuclear theories could influence other levels of U.S. Government, particularly Presidential decision-making during nuclear crises. Jervis, for example, concedes that for “most of the history of American doctrine and war planning has been the attempt to design substitutes

68 Spurgeon Keeny quoted in Herken, Counsels of War, 81.
for damage limitation...” But he goes on to suggest that “the influence of the fear of war on political leaders is not sensitive to the details of doctrine and war planning that preoccupy the specialists... [because] mutual vulnerability exists and casts an enormous shadow. This condition is not subtle nor does it depend on the details of the strategic balance or targeting that may loom large to academics or war planners; such details are dwarfed in the eyes of decision makers by the danger of overwhelming destruction.”  

Confirming Jervis’s intuition, former Kennedy White House staffer Marcus Raskin attests that “most of the people who actually made high policy thought that planning for nuclear war was merely an exercise in the theory of annihilation, that it could have no practical consequences.”  

Secretary of Defense Robert McNamara informed Paul Nitze in 1962 that “The concept of a ‘worsened relative military position after a general nuclear war’ is not a meaningful one to me when each side has the capacity to destroy each other’s civilization.”  

Even President Eisenhower, the architect of Massive Retaliation, did not really believe that the U.S. could fight a nuclear war, remarking that “you can’t have this kind of war. There just aren’t enough bulldozers to scrape the bodies off the streets.”  

Endorsing a version of MAD, Eisenhower was confident that, “until an enemy [has] enough operational capability to destroy most of our bases simultaneously and thus prevent retaliation by us, our deterrence remains effective.”  

In other words, there is good evidence that MAD provided the mental map that guided policymakers through the early days of the nuclear age.  

One might argue, though, that even if policymakers did not accept nuclear war-fighting plans and doctrines, their enshrinement in military standard operating procedures meant that in the event of

73 Jervis, The Meaning of the Nuclear Revolution, 8-9 and 98.
75 Quoted in Gavin, Nuclear Statecraft, 36.
war they would nonetheless be implemented, much as the German Schlieffen Plan supposedly dragged
the German government into a war civilian leaders did not desire. But the Schlieffen-Plan-caused-the-
First-World-War argument has been largely discredited and there are two additional reasons to be
skeptical that operational doctrine and plans would force the hands of civilian policymakers.

First, the argument that they would assumes that civilians were unaware of the plans’ content.
But there is ample evidence that policymakers in the Kennedy Administration, which took the first step
toward imposing civilian preferences on war plans, were well aware of the details of extant plans and
doctrine. Civilian strategists including Kissinger and Ellsberg learned the details of Joint Strategic
Capabilities Plan (JSCP) and independently brought them to the attention of National Security Adviser
McGeorge Bundy. Bureaucratic and alliance politics, rather than rational strategic calculation,
accounted for the content of these war plans.

Second, it assumes that senior civilian policymakers would actually be hand-cuffed by them in a
crisis. But nuclear analyst Desmond Ball identifies 20 instances since 1945 when U.S. policymakers
considered using nuclear weapons but did not, despite the existence of these rigid war plans and
offensive nuclear doctrines. The reason, in his view, was that “that the pre-planned options in the
SIOP are too idealized, that they are a-strategic in the sense that they would be of little relevance in

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83 Ball, “U.S. Strategic Forces,” 41-42.
real-life situations." While Bundy worked to bring the Single Integrated Operational Plan (SIOP) more in line with MAD, President Kennedy’s approach was to avoid taking on the Chiefs directly and ignore their war plans in the short term while trying to change them in the longer term. His crisis behavior, particularly but not exclusively during the Cuban Missile Crisis, amply demonstrates his willingness to throw out the military’s war plans well before first contact with the enemy.

A final question skeptics might pose is how do we know whether it was the ideas of the civilian strategists that shaped Presidential thinking about nuclear strategy? Proving that they did definitively will be a challenge for the reasons we suggested earlier. Despite that, there are nonetheless grounds for believing that they did. First, given that military plans and doctrine rejected MAD, there is no reason to think that their views shaped Presidential thinking. Second, it is widely recognized that the ideas of the civilian strategists were “in the air” during policy debates – both in internal U.S. Government discussions but also in many public fora such as popular magazines like Look, Esquire, The Atlantic Monthly, and The Saturday Evening Post -- and so there are reasonable grounds for thinking they were influential in leading Presidents to think differently from military leaders about nuclear strategy. Finally, while we cannot rule out that high-level civilians came to their understanding of the nuclear revolution independent of both the military and the civilian defense intellectuals, that does not seem like the most likely explanation for how high-level civilian decision-makers with little expertise in the issue and lots of other items on their plates came to think about the nuclear revolution.

Vietnam And The End of the Golden Age

84 Ball, “U.S. Strategic Forces,” 44.
There is no doubt that the Golden Age came to an end roughly coincident with the Vietnam War. Many think that this was no coincidence. Kaplan, for instance, links the two, arguing that America’s “Vietnam strategy was essentially a conventional-war version of the counterforce/no cities theory – using force as an instrument of coercion, withholding a larger force that could kill the hostage of the enemy’s cities if he didn’t back down.” The logic here is that Kaufmann’s critique of Massive Retaliation connected a limited war in Vietnam to the larger struggle with the Soviet Union through the need to maintain U.S. credibility. Schelling’s work on bargaining in trade negotiations seemed readily transferable to strategic interaction, especially reinforcing the importance of holding off Communism in South Vietnam to maintain America’s reputation as a stalwart ally and determined foe. Also, it suggested that the limited use of force could “signal” U.S. resolve to stand by its ally in South Vietnam through a graduated bombing campaign against the North.

Proponents of the view that Schelling’s theories influenced Kennedy Administration policies offer three pieces of evidence: First, Schelling’s work on nuclear strategy during the Berlin Crisis made its way with President Kennedy to his weekend retreat in Hyannis Port, MA in the summer of 1961. National Security Advisor McGeorge Bundy subsequently reported that it made a “deep impression” on the President. Second, Schelling’s biographer Robert Ayson concludes that “his thinking behind this body of work [in the early 1960s] was a major inspiration for the development of US strategic policy during this period.” One concrete implementation of his theories was the establishment of US-Soviet

87 Trachtenberg, *History and Strategy*, 3.
89 Larson, “Deterrence Theory and the Cold War” 100.
“hot-line.”\textsuperscript{93} Finally, John McNaughton, Assistant Secretary of Defense for International Security Affairs, and one of Secretary of Defense McNamara’s key advisors on Vietnam strategy, had been a friend and colleague of Schelling’s at Harvard and constituted a direct personal link to the Administration’s Vietnam policy.\textsuperscript{94} The failure of these policies, so this conventional wisdom holds, discredited the whole defense intellectual enterprise.\textsuperscript{95}

Attesting to the deteriorating intellectual climate for policy-relevant national security research, Kaufmann and Schelling argued that the unrest caused by the Vietnam War in Cambridge nearly put an end to the academic study and teaching of national security.\textsuperscript{96} As Schelling remarked apropos of his post-Vietnam intellectual reorientation away from security studies, “‘I lost the access, I lost the audience, and I lost the motivation.’”\textsuperscript{97} The poster-child for how Vietnam supposedly brought to a close the chapter on the civilian academic defense intellectual, of course, remains Daniel Ellsberg, who went from über-hawk to ultra-dove on Vietnam without even pausing in the middle.\textsuperscript{98} Indeed, some major universities such as Columbia, MIT, Chicago, Princeton, Stanford, Berkeley and Case Western Reserve had institutional connections with other FFRDCs aside from RAND, such as the Institute for Defense Analysis (IDA) which played a significant role in connecting academic research with U.S. national security policy. These connections became particularly controversial during the height of the anti-war movement on campus and most were severed.\textsuperscript{99}

By the early 1970s, there was a marked change in the attitudes on campus about academics participating in national security policy-making. Many still did as individuals, think of Kissinger and

\textsuperscript{93} Ayson, \textit{Thomas Schelling and the Nuclear Age}, 32.


\textsuperscript{96} Herken, \textit{Counsels of War}, 220-21.

\textsuperscript{97} Quoted in Herken, \textit{Counsels of War}, 313.

\textsuperscript{98} Abella, \textit{Soldiers of Reason}, 213-29.

Zbigniew Brzezinski, but the bridge from the Ivory Tower to the Beltway increasingly became a one-way street, with most of those taking it to Washington never returning to academe. 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.” To be sure, there were a handful of senior scholars who managed to go back and forth – people like Samuel Huntington, Joseph Nye, and Jervis – but as time went on, they became more and more the exception, rather than rule among leading scholars after the Vietnam War.

**Following Economics to an Intellectual Dead-end**

But was Vietnam the only, or even the most important, reason for the estrangement of academics from policy relevant issues like nuclear strategy? As we saw above, there was evidence of increasing intellectual tensions between academia and government well before Vietnam. The establishment of RAND and other FFRDCs was a response to the growing difficulty in working with universities directly. Their root was the increasing influence of the Economic approach to strategy. As historian Marc Trachtenberg explains, “there was an intellectual vacuum in the whole national security area. The economists, and people heavily influenced by their style of thinking, were for a variety of reasons drawn to this vacuum. What they had was something very general, a way of approaching issues, rather than anything that in itself suggested substantive answers that went right to the heart of the strategic problem.”

“What was new,” Eliot Cohen agrees, “was not the use of numbers or equations to help solve military problems but rather the coronation of one social science – Economics – as the rightful queen of war-planning and strategy.”

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100 Kuklick, *Blind Oracles*, 182-203.
102 For evidence of that, see Avey and Desch, “What Do Policymakers Want From Us?” 227-46.
103 Trachtenberg, *History and Strategy*, 15. Also see, 13 and Herken, *Counsels of War*, 49 and 75-76;
104 Cohen, “Guessing Game,” 166.
Brodie himself had initially believed that strategy could become a “science” and Economics provided the model for it: “the strategy of strategic bombing is very largely a matter of target selection, where the economist (possibly also the psychologist) has at least as much to offer as the military specialist.”

Kuklick and Alex Abella chronicle how the methodology of Economics came to dominate RAND’s approach to social science, pushing out the less formal and more historical approaches to defense analysis. But as Economics came to dominate strategic analysis, method and theory became increasingly more important than substance. Reviewing political scientist William Kaufmann’s book *The McNamara Strategy*, Brodie enthused that “what is novel about the modern type of [defense] analysis is the marvelous development and refinement of the method and also the conscious and open dedication to the effort.” The increasing dominance of Economic approaches to nuclear strategy led the field to privilege academic concerns such as theory and methodology. As Brodie later reflected, “elegance of method is indeed marvelously seductive, even when it is irrelevant or inappropriate to the major problems.”

This early confidence among civilian academic strategists about their ability to forge a “science” of strategy quickly gave way to pessimism about the enterprise. In a letter to James Holland of the Army War College, Brodie 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 de-emphasized by giving so much emphasis to the comparability of the

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strategy with economics.”¹¹⁰ The problem was that by the mid-1960s the Economic approach to strategic thought in the United States had reached something of an intellectual “dead-end.” This was the result, in Trachtenberg’s view, of the fact that “the most basic issues were [at that point] analyzed on a very abstract level. One could work out in general terms the argument for ‘strategic stability,’ or for various nuclear war-fighting strategies. But at this level of abstraction there were no final answers – at this highly abstract level of conceptualization the most basic intellectual tensions could not be resolved.”¹¹¹ In retrospect, what contributed to ending the Golden Age of security studies was the increasing influence of Economics in the sub-field which led to a growing preoccupation with theory and method rather than substance.¹¹²

As they fell for the seduction of theoretical elegance and method, the Golden Age nuclear strategists lost touch with the practical concerns of policymakers.¹¹³ Typical of the responses of policymakers to this increasingly academic approach to strategy, Harvard Dean and Kennedy National Security Advisor McGeorge Bundy lamented that “there is enough, and perhaps too much, analysis aimed at scholarly rigor and scientific validity. There is enough and perhaps too much system-building in which models of this or that political process are constructed. There is enough, and perhaps too much, detailed historical recording of political phenomena. What there is not enough of yet, and what I come to praise, is the kind of academic work which proceeds from the same center of concern as that of the man who is himself committed to an active part in government.”¹¹⁴ Their “center of concern” was the concrete policy problems they faced.

¹¹⁰ Brodie to Holland, July 13, 1966, Brodie Papers, Box 5, folder "Army War College," UCLA Library Manuscripts Room. We thank Marc Trachtenberg for this source.
¹¹¹ Trachtenberg, History and Strategy, 44.
There were clear limits to the influence of civilian strategists, particularly when their increasingly arcane theories led them beyond MAD. Bundy notes that “there is an enormous gulf between what political leaders really think about nuclear weapons and what is assumed in complex calculations of relative ‘advantage’ in simulated strategic warfare. Think Tank analysts can set levels of ‘acceptable’ damage well up in the tens of millions of live. They can assume that the loss of dozens of great cities is somehow a real choice for sane men. They are in an unreal world. In the real world of political leaders ... a decision that would bring even one hydrogen bomb on one city of one’s own country would be recognized in advance as a catastrophic blunder.”

German Defense Minister Franz Josef Strauss similarly dismisses post-MAD American strategic thought as “‘conceptual aids for the precalculation of the inconceivable and incalculable nature of the specific.’” In other words, MAD was not only a fact, it was also de facto policy despite war plans and doctrines that seemed to reject it.

**Developing Criteria for Influence**

We propose four criteria for influence based upon our examination of the decline of the early Cold War ‘Golden Age.’ First, the work should deal with “important” topics that policy-makers and members of the general public care about. Moreover, it should focus on pressing problems within that issue area; that is, it should be problem-driven. The initial golden age of academic theorizing provided policymakers grappling with the problems of new technologies (the atomic bomb) in a new security environment (the Cold War) with a new way of thinking. By contrast, as Economics approaches came to dominate nuclear theorizing the scholarship seemed increasingly divorced from the problems that policymakers actually encountered. Second, scholarship should focus on variables that government policy can manipulate. The basic MAD logic provided a framework for policymakers to guide behavior in nuclear crises.

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Third, it should make explicit policy-recommendations about how to do so in both general classes of cases as well as their application in specific instances on the policy agenda. The Economics approaches gave stylized interpretations of how actors might behave but little discussion of how historical and political circumstances would lead specific actors to behave. As James Goldgeier notes: “Policymakers typically want to know how to respond to a particular challenge. Informing them that X% of cases turn out a certain way is unhelpful; advising them that the case they are facing is likely to proceed in a certain direction because of particular underlying factors and offering prescriptions they can follow is a more useful approach.”¹¹⁸ Finally, scholarship needs to offer clear and consistent findings about the effect of specific variables, something the basic MAD logic provided.¹¹⁹

Wohlstetter’s influential basing study nicely illustrates these criteria in practice. In it, he dealt with an important, problem driven issue: how to make the deterrent effective as Soviet capabilities increased. He identified a policy that the U.S. government could enact and made a clear recommendation: diversify basing and platforms to guarantee survival. Finally, most accept the basic argument that nuclear weapons must be survivable to effectively deter even if they differ on the precise number of weapons necessary to meet that requirement.

This is also why, in the final analysis, it was the more eclectic Brodie, rather than Economist Thomas Schelling, whose ideas had the most influence upon actual nuclear statecraft.¹²⁰ Already in 1946, the former had set the “basic axioms of the nuclear age” – the absence of any meaningful defense against nuclear attack, the inevitable mutual vulnerability of each sides’ population, and the need for secure second-strike capability – which were the enduring facts of the nuclear age that policymakers

¹²⁰ Jervis, The Meaning of the Nuclear Revolution, 49. Of course, other theorists made important contributions as well such as Wohlssetter’s distinction between first vs. second strike capability; Glenn Snyder’s examination of the challenges of extended deterrence; and Schelling’s discussion of the “threat that leaves something to chance.”
recognized they had to grapple with.\textsuperscript{121} Brodie’s views directly influenced Secretary of Defense Robert McNamara’s thinking on the subject.\textsuperscript{122} While Brodie never achieved the insider influence on U.S. nuclear strategy – particularly doctrine and war plans -- that he sought, he became a very influential public commentator on these issues.\textsuperscript{123} The reason he did, in Lawrence Freedman’s judgment, was that he had a better feel for real world politics because he “was not a formal strategist in that he lacked confidence in conclusions derived from ‘war gaming’ or rigorous but abstracted analysis, isolated from the real world.”\textsuperscript{124}

Schelling, on the other hand, is the more highly esteemed in scholarly circles these days, recently winning a Nobel Prize in Economics.\textsuperscript{125} He undoubtedly made “an original (and very important) contribution to the study of strategy,” though it was largely derivative, applying concepts from Economics to the world of strategy.\textsuperscript{126} But after reviewing Schelling’s contributions to strategy, his intellectual biographer Robert Ayson concludes that he ultimately fell “victim to the sense of unreality which afflicts aspects of nuclear strategy.”\textsuperscript{127} Or as P.M.S. Blackett put it, Schelling’s approach, and that of other game theorists like John von Neumann and Oskar Morgenstern, ultimately failed because it did not “clothe the skeleton conflicts of the theory of games with the complex flesh and blood attributes of real nations; hence the bizarre nature of some of their practical conclusions.”\textsuperscript{128} Schelling himself eventually acknowledged the limits of using domestic bargaining analogies to analyze international conflict between states in the nuclear age.\textsuperscript{129} And by 1964, well before the Vietnam War had begun to

\textsuperscript{121} Freedman, \textit{The Evolution of Nuclear Strategy}, 44.
\textsuperscript{124} Freedman, \textit{The Evolution of Nuclear Strategy}, 300.
\textsuperscript{125} Though eventually even Schelling, who pioneered the use of game theory in the analysis of deterrence, was not regarded among academic Economists as being particularly rigorous methodologically. John J. Mearsheimer, “A Self-Enclosed World?” in Ian Shapiro, Rogers M. Smith, and Tarek E. Masoud, eds., \textit{Problems and Methods in the Study of Politics} (New York: Cambridge University Press), 393-94.
\textsuperscript{126} Ayson, \textit{Thomas Schelling and the Nuclear Age}, 113.
\textsuperscript{127} Ayson, \textit{Thomas Schelling and the Nuclear Age}, 200.
\textsuperscript{128} Blackett, 16.
\textsuperscript{129} Ayson, \textit{Thomas Schelling and the Nuclear Age}, 152-53.
poison Ivory Tower/Beltway relations, he began to move away from strategic studies, perhaps a tacit admission that his Economic approach to strategy had in fact reached a dead-end.\footnote{Ayson, \textit{Thomas Schelling and the Nuclear Age}, 35.} Appropriately, his eulogy of Brodie seems to cede the field of nuclear strategy to “the dean of us all.”\footnote{See Ayson, \textit{Thomas Schelling and the Nuclear Age}, 114.} In sum, the end of the Golden Age was not only, or even primarily, driven by real-world events like Vietnam; it was largely the result of the effort to make strategy a “science” modelled on Economics which led it to an intellectual dead-end.

\section*{III. A “Renaissance” in Policy-Relevant Academic Nuclear Theorizing Today?}

Given changes in the world since 1989, the field of national security affairs ought to be ripe for a new golden age of theorizing about the influence of nuclear weapons upon statecraft. The changing geopolitical circumstances lead some observers to argue that we are currently living in a “second nuclear age” while others point to technological changes that may upend the basic MAD logic.\footnote{Paul Bracken, \textit{The Second Nuclear Age: Strategy, Danger, and the New Power Politics} (New York: Times Books, 2012); Keir A. Lieber and Daryl G. Press, “The End of MAD? The Nuclear Dimension of U.S. Nuclear Primacy,” \textit{International Security}, Vol. 30, No. 4 (Spring 2006), 7-44; and Keir A. Lieber and Daryl G. Press, \textit{Nuclear Weapons and International Politics}, unpublished book manuscript.} Other scholars are taking advantage of newly available data to look back over the “long peace” of the Cold War, including the non-use of nuclear weapons, to challenge our understanding of the effects of nuclear weapons on statecraft.\footnote{Gavin, \textit{Nuclear Statecraft}; Nina Tannenwald, \textit{The Nuclear Taboo: The United States and the Non-Use of Nuclear Weapons since 1945} (Cambridge: Cambridge University Press, 2009); T.V. Paul, \textit{The Tradition of Non-Use of Nuclear Weapons} (Stanford: Stanford University Press, 2009); John Mueller, \textit{Atomic Obsession: Nuclear Alarmism from Hiroshima to Al Qaeda} (Oxford: Oxford University Press, 2010).} Given these and other important changes in the international security environment, we ought to see an efflorescence of scholarship on nuclear weapons today.

But this does not appear to be the case. Figure 4 shows that the trend of articles dealing with WMD and arms control (which no doubt includes articles not dealing with nuclear strategy specifically) in top IR journals. While there was a brief surge in 2009 (largely accounted for by a special issue on nuclear issues in the \textit{Journal of Conflict Resolution} that year) the overall trend shows a steady decline in
articles on these issues since the 1980s and especially since the mid-1990s. Anecdotal evidence lends support to this basic claim. For example, Paul Bracken laments that today “the quality of thinking about nuclear weapons has reached a dangerously low level, in Washington and in the country. ... In universities there is little knowledge of nuclear matters ... Think tanks have not filled the gap, either.”\textsuperscript{134} Moreover, as Figure 5 indicates, the policy relevance of the work that has been done during this period has also generally declined since the 1980s despite the brief surge in 2003. It is worth pointing out that this surge was during a period in which the overall number of articles dealing with these issues was very small. Finally, while the Stanton Foundation has generously funded work by junior scholars on nuclear issues this has not yet been accompanied by an increase in demand in political science departments for faculty with in depth knowledge of these issues.

\textbf{Figure 4: Decreasing Proportion of Nuclear Weapons Articles}

Figure 5: Its Policy Relevance Varies Widely but Appears to be Declining

While these data reflect a broader set of issues than our focus on nuclear strategy, a closer examination of the two broad waves of political science scholarship on nuclear politics reinforces this interpretation of the data for the 1990 and 2000s and thus our qualified pessimism today. The first wave occurred in the 1990s and utilized the U.S.-Soviet Cold War experience to debate what further nuclear proliferation implied for international politics. In many ways the first wave followed the pattern apparent in the early Cold War with the result that it eventually hit something of intellectual dead-end. A second wave began in the late 2000s. This scholarship sought to utilize a wider array of evidence and methodologies on nuclear strategies and behavior, including data from beyond the superpower experience, to shed light on these questions. Scott Sagan characterizes this as a “renaissance” in nuclear security studies.  

135 The question is whether the latest incarnation of academic nuclear strategizing will avoid the Golden Age’s and the first post-Cold War wave’s dead-end?

In the rest of this section we first outline the major premises of the optimism-pessimism debate that flourished and then declined during the 1990s. We assess it based on the criteria for influence we

\[\text{Articles on WMD That Provide Policy Prescriptions in Top IR Journals since 1980}\]

\[
\begin{array}{ccccccccccc}
\text{Proportion (%)} & 90 & 70 & 50 & 30 & 10 & 90 & 80 & 70 & 50 \\
\end{array}
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outlined above. Next, we discuss some of the most important works in the recent wave of scholarship. A full review of all the political science work on this topic is not practical given space constraints, but by focusing on the most prominent pieces within the discipline we can get a clearer picture of the degree to which that wave will constitute a renaissance in policy-relevant scholarship.

The 1990s Optimism-Pessimism Debate

The first wave crested in the wake of the Cold War and drew its lessons from the superpower experience during that time.\textsuperscript{136} To be sure, scholars marshalled evidence from emerging nuclear states as well, but the core of the historical evidence was U.S. and Soviet behavior and what it meant moving forward. Yet by the end of the 1990s the debate tapered off with no real resolution. “As lively and consequential as the exchange was,” writes David Karl, “it quickly approached the point of diminishing returns.”\textsuperscript{137}

No single work better epitomizes this scholarship than the Scott Sagan and Kenneth Waltz volume \textit{The Spread of Nuclear Weapons} in which the two authors debated one another on the consequences of nuclear proliferation.\textsuperscript{138} The volume was easily the most influential contribution of the optimist-pessimist debate within the academy. It is currently in its third edition and taken together the three editions have generated a whopping 644 citations.\textsuperscript{139} Despite the dramatic global changes, the positions of the two authors have not changed during this time. An examination of this work is therefore useful in assessing why the debate faded at the end of the 1990s with little noticeable influence.


\textsuperscript{139} Citation count from Google Scholar for the three editions, December 4, 2014.
Both Sagan and Waltz accepted that nuclear weapons generally made conflict between states less likely. They also agreed that relatively small nuclear arsenals could create this effect. Where they parted company over how robust this stability was, particularly among the emerging nuclear powers. For Waltz nuclear deterrence was robust and the United States should not worry about nuclear proliferation to new states. Sagan countered deterrence could fail in any number of ways and therefore proliferation was dangerous. For him, new nuclear states would repeat the mistakes of the Cold War superpowers, such as formulating destabilizing operational plans, contemplating and perhaps executing preventive wars (especially in the absence of civilian control of the military), and failing to adequately safeguard their arsenals.

Despite its widespread attention in the academy, the debate reportedly had little influence on actual policy. To begin with, policymakers in Washington flatly rejected the optimist prescription. They accepted the pessimist prescription to oppose proliferation but did not do so as a result of the pessimists’ logic. This lack of influence for the pessimist position is evident in two ways. First, U.S. policy had vigorously opposed proliferation since at least the 1960s. Second, the reason had less to do with

142 Thus Peter Feaver writes that “Regardless of how persuasive Waltz and the nuclear optimists may be in an academic setting, it is doubtful they would ever be persuasive in a U.S. policy setting. ... It is ... official U.S. policy that the spread of nuclear weapons is bad.” Frank Gavin is blunter, stating that “Waltz recommends policies that have no chance of being treated seriously, to say nothing of being adopted...” Peter D. Feaver, “Optimists, Pessimists, and Theories of Nuclear Proliferation Management: Debate,” Security Studies, Vol. 4, No. 4 (Summer 1995), 770; and Francis J. Gavin, “Politics, History and the Ivory Tower-Policy Gap in the Nuclear Proliferation Debate,” Journal of Strategic Studies, Vol. 35, No. 4 (August 2012), 597.
concerns about safety and civilian control – the focus of the pessimist position – than an underlying strategic logic: If other countries acquired nuclear weapons it would constrain U.S. freedom of action. As Les Aspin, then Chairman of the House Armed Services Committee, put it in June 1992: “A world without nuclear weapons would not be disadvantageous to the United States. In fact, a world without nuclear weapons would actually be better. Nuclear weapons are still the big equalizer, but now the United States is not the equalizer but the equalizee.”\(^{144}\) In making these arguments policymakers implicitly accepted the underlying MAD logic: nuclear weapons restrain opponents, including the United States.\(^{145}\)

To be sure, part of the lack of influence was the result of different concerns from policymakers. In other words, the demand-side part of the equation, the specific challenges policymakers face and thus how they utilize social science, differ from academic concerns. For instance, as Peter Feaver notes, a finding that nuclear deterrence works 99.5% of the time would be in the “social science theory hall of fame, but it would not make nuclear proliferation trivial” and would still generate concern in government circles.\(^{146}\) Moreover, policymakers in Washington no doubt focus on how nuclear proliferation effects U.S. interests. They may be less interested or concerned with the other issues that Sagan identifies or implications for overall global stability if that stability is bought at the price of U.S. leverage. How much risk to accept and the nature of U.S. interests are ultimately political judgments that social science is hard pressed to adjudicate.

The lack of influence on either side of the optimist-pessimist debate also reflects the absence of several critical components for relevance we identified above. This supply-side problem, the content of what scholars were providing, magnified the difficulties of the tough demand-side environment. Most


\(^{145}\) Stephen Walt makes a similar point, “All the Nukes that You Can Use,” May 24, 2010, ForeignPolicy.com, http://www.foreignpolicy.com/posts/2010/05/24/all_the_nukes_that_you_can_use.

importantly, the Waltz-Sagan and optimist-pessimist debate more broadly moved away from problem-driven scholarship. The questions the debate posed and the problems it identified simply could not be answered with the evidence and methods available at the time leading to an inconclusive outcome. As a result, scholarship came to rely largely upon deduction, becoming less sensitive to policy concerns as it sought to establish the superiority of one academic theory over another. In this environment, just as earlier, basic intellectual tensions could not be resolved. As Gavin points out, the Sagan-Waltz debate focused “on the weapons and postures, and not the underlying politics.” As a result, this limited its potential for influence. Nor has there been much effort to incorporate the newly available history into the more recent volumes. In addition, there was no sustained discussion on how to link nuclear strategy to foreign policy objectives in the new environment. This is particularly problematic given the somewhat incoherent nuclear strategy outlined in the 1994 Nuclear Posture Review (and repeated in 2002 and 2010). Finally, there was little in the way of specific policy recommendations. This was particularly noticeable on the optimist side, as Waltz made little effort to identify how certain cases might deviate from the dominant trends. Such an omission is particularly important given the concerns surrounding even a single instance of nuclear use. This is not to say that the debate was completely irrelevant or unimportant; the two sides usefully challenged conventional wisdom among national security officials. Yet ultimately the debate had less impact than it might have because the respective positions were driven more by debates among international relations theorists than by the concrete concerns of policymakers.

A Contemporary Nuclear Studies “Renaissance”?

A second post-Cold War wave of nuclear studies builds upon the optimist-pessimist debate as well as the earlier nuclear literature to assess the broader political effects of nuclear weapons. While there are important disagreements, two important findings are emerging. First, larger force structures

148 Nichols, No Use, Chapter 2.
and more active force postures may be better for individual states.\textsuperscript{149} Second, this does not mean that nuclear weapons will lead to a more pacific international system.\textsuperscript{150} To reach these conclusions scholars have expanded the evidentiary base and methods deployed to tackle these questions. The common point of departure for these studies is the basic MAD insight that nuclear weapons can inflict terrible destruction and there is no robust defense possible. As a result, they are bound to affect interstate politics. The key is to use the new evidence to determine the types and limits of these effects.

It is too early to fully assess the influence of these works on policy and space constraints prevent discussion of all the recent works on nuclear politics. Rather, we present some basic data on trends and then highlight four of the most influential recent works on nuclear strategy to assess the likelihood that they will contribute to a renaissance of policy-relevant nuclear scholarship. We find a mixed picture: While there is reason for some optimism there is also cause for concern that this renaissance, like the Cold War Golden Age, may end up in an intellectual dead-end as method-driven research crowds out problem-driven work.

To begin, work on nuclear politics continues to deal with a very important topic. Public opinion polls consistently show that nuclear proliferation and its consequences remain at the top of the publics’ perception of “critical threats to U.S. vital interests,” with 63% saying this about general proliferation


and 64% about Iran specifically (see figure 6).\textsuperscript{151} Concern with nuclear weapons extends to top policymakers as well. For instance, in 2009 President Obama noted that “if one nuclear weapon exploded in one city ... there is no end to what the consequences might be.”\textsuperscript{152}

### Figure 6: Proliferation as a Threat

There is cause for concern, though, that the new scholarship is not engaging the most salient problems of nuclear proliferation and strategy. Following broader trends in political science, the work has become increasingly quantitative over time. As Figure 7 suggests, the percentage of articles employing “scientific” approaches to studying these questions has increased, with quantitative approaches becoming increasingly common in recent years. There is a vigorous debate on coding

\textsuperscript{151}Foreign Policy in the New Millennium: Results of the 2012 Chicago Council Survey of American Public Opinion and U.S. Foreign Policy, Dina Smeltz, Project Director, [Chicago, IL: CCGA, 2012], Figure 2.1 at http://www.thechicagocouncil.org/UserFiles/File/Task%20Force%20Reports/2012_CCS_Report.pdf

\textsuperscript{152}The White House, Office of the Press Secretary, “Remarks by President Barack Obama,” Hradcany Square, Prague, Czech Republic, April 5, 2009. See also Mueller, Atomic Obsession, x-xi.
decisions and how appropriate such methods are to studying nuclear issues in the first place. Our purpose is not to engage that debate but rather to assess the implications of this shift for the likelihood the work will influence policy. On this score the trends are not cause for optimism. As Figure 8 demonstrates, the more scientific approaches tend to be less associated with a willingness to make explicit policy recommendations than their qualitative brethren. Moreover, Yale political scientist Alexandre Debs writes that “the new wave of quantitative studies has not lived up to its promise because it has focused too much on questions of methodology. In order to move forward … nuclear studies should pay greater attention to theory, history … and politics.” In other words, the discipline seems to be following the Cold War wave into an intellectual dead-end by privileging method over problem.

Figure 7: It is Increasing Formal and Quantitative

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Matthew Fuhrmann, Todd Sechser, and Matthew Kroenig have produced some of the most widely discussed recent pieces on nuclear strategy using sophisticated methodological approaches. The authors various works spurred lively discussions at the Duck of Minerva political science blog, a forum hosted by H-Diplo that engaged some of the most prominent nuclear scholars, and comments from political scientists and former policymakers at the Monkey Cage blog at the Washington Post. In their first article, Sechser and Fuhrmann argue that while nuclear weapons may be useful for deterrence they find that the weapons are not useful when compelling adversaries (in fact nuclear superiority can make compellence less likely to succeed). In a follow-up article the authors find that formal defense guarantees by nuclear powers, but not the actual positioning of nuclear weapons on allied territories, reduces challenges to nuclear clients. This, they argue, shows the value of extended deterrence.

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commitments. Matthew Kroenig argues that in crises between two nuclear states the side with the larger nuclear arsenal is more likely to win the dispute. Though many have critiqued the basic claim that superiority can translate into political leverage it is worth pointing out that several quantitative and qualitative studies have reached similar conclusions.

These studies are important but are unlikely to have much policy influence as they currently stand. First, in good social science fashion the authors seek to isolate the impact of nuclear weapons and then report general changes in the risks, odds, or likelihood of conflict. This is not limited to these studies, indeed, most qualitative studies seek to generalize from a set of representative or most-likely cases to some larger universe. To some extent this can be addressed by simply examining if specific cases of interest share characteristics with those in the model.

There is a more fundamental problem, however. Most of the current and likely recommendations are vague, face major political problems of implementation, or are difficult to implement even if there was a willingness to do so. For example, Colin Kahl, a political scientist and national security advisor to Vice President Joe Biden, notes that Washington must often worry about factors difficult to include in typical models such as the goals of states, the possibility of a stability-instability-paradox (the fear that stability at the strategic nuclear level essentially cancels out the arsenals and makes conventional conflict more likely), and experience with nuclear weapons. This has obvious implications for how worried policymakers will be about the prospects of new nuclear states.

attempting to compel adversaries.\textsuperscript{158} There are different problems with extended deterrence schemes: If they are effective generally why should we not simply extend such guarantees to all states? Would that limit conflict or simply be seen as incredible in most cases? At the least, it would risk embroiling the nuclear patron in numerous disputes of peripheral importance.

Or take Kroenig’s major finding: nuclear advantage conveys bargaining leverage. How large does a nuclear arsenal and the advantage need to be in order to gain leverage? How much leverage does one get? And how can one best exercise this leverage? Presumably, there are diminishing returns at some point, some diplomatic and conventional military actions alongside a nuclear advantage are more effective than others, and some types of challenges are more likely to be deterred than others. Expanding nuclear arsenals can also spark arms races that leave states less secure, hinder other policies, and face cost and domestic political constraints. To be fair, Kroenig recognizes these limitations.\textsuperscript{159} Nonetheless, there are clear limits to how useful nuclear weapons can be in different circumstances. Taking the next step to better understand magnitude, situational, and threshold effects – something that both quantitative and qualitative scholarship often struggles with – will help scholars and policymakers better adjudicate between the various tradeoffs they face in adopting any given policy and increase the likelihood this type of scholarship will have influence.

Keir Lieber and Daryl Press have written several articles on U.S. nuclear history, force posture and capabilities. Some of the work is consistent with the Kroenig claim that more is better, as Lieber and Press argue that a larger number of nuclear weapons constitute a more effective deterrent against


\textsuperscript{159} Kroenig, “Nuclear Superiority and the Balance of Resolve,” 168.
nuclear strikes than only a few such weapons.\textsuperscript{160} While qualitative and more attentive to context there are still issues with threshold effects and the manipulability of some of these factors that may vex policymakers.

The other strand of their work meets many of the criteria we outlined above for relevance. Specifically, they conduct careful examinations of how changing technology and adversary capabilities influence the lethality and reliability of U.S. nuclear arsenals. They presented their initial findings in two articles in 2006: one in the academic journal \textit{International Security} and the other in the influential \textit{Foreign Affairs}. Both articles sparked responses from scholars and policymakers and both enjoy garnered high citation counts.\textsuperscript{161} Building upon this work, Lieber and Press highlight the implications of technological changes for U.S. policy toward specific adversaries and likely adversary responses. They therefore focus on manipulable variables, make specific recommendations, and provide clear findings.

In new work Vipin Narang makes an important contribution to understanding regional nuclear powers’ force postures and the consequences of those postures. Its recent nature makes it difficult to determine the academic impact of the work at this time. Narang uses qualitative and advanced quantitative techniques to argue that nuclear force postures that envision using nuclear weapons early against conventional forces are best at deterring low- and high-level challenges. This asymmetric escalation posture means delegating launch authority and directly integrating nuclear weapons into


battlefield planning. In other words, to deter conventional assaults states must develop a force posture explicitly designed to do so.162

This meets many of the criteria outlined above for a work to be in a good position to influence policymaking. It pays close attention to the politics and history of specific countries to show how general conditions cause specific countries to act and the consequences for those decisions for international stability. The focus on force posture is a manipulable variable in those specific countries. It also highlights the tradeoffs U.S. policymakers face if and when they encourage regional nuclear powers to adopt specific force postures. Narang’s work provides new insight into when problems such as the stability-instability paradox are likely to be particularly acute.163 In addition, though he does not discuss it specifically, his findings reinforce the basic intuition of MAD by showing that the risks of war go to almost zero even if both sides do not have a robust second strike capability.

IV. Conclusions and Recommendations.

What are the lessons of the Golden Age for today? We argue that academic strategy did have important effects on policy. At times this was direct, as was the case with Wohlstetter’s basing study. More often it was indirect, working not through the formulation of doctrine or the drafting of operational plans, but rather indirectly by providing the intellectual frameworks and mental road-maps that shaped Presidents’ thinking about the utility of nuclear weapons during confrontations with other states. Despite bureaucratic pathologies within the U.S. military, which led the services to adopt nuclear warfighting doctrines and force postures, in the heat of crises, Presidents spoke and behaved as if Brodie’s assessment of the “nuclear revolution” was correct that the absolute weapon was of use only


for deterring an adversary from using one. In other words, MAD was not only a fact, it was also a policy at the highest level.

But the history of the Golden Age also tells a cautionary tale: Intellectual dynamics within universities and even among social scientists working at places like RAND can under certain circumstances lead nuclear strategists into intellectual dead-ends when internal disciplinary dynamics lead to the privileging of sophisticated methods and theoretical elegance over real world problems. This is by no means an across-the-board indictment of sophisticated social science methods, but it is an illustration of their limits. For example, the highly quantitative approach of operations research proved extremely useful to military planners during the Second World War but the mathematical tools of Systems Analysis proved useless, if not counterproductive, as a basis for formulating higher-level U.S. national security policy during the Cold war. The Golden Age has many examples that demonstrate the compatibility of cutting-edge social science and policy relevance but it also shows that the quest to completely make strategy a “science” is quixotic.

Reports of a “renaissance” in academic nuclear strategy today therefore seem premature. As the TRIP journal survey shows, academic international relations in general is becoming less policy relevant, at least in terms of willingness to offer explicit policy recommendations. The trends among articles looking specifically at nuclear issues are similar: Fewer scholars are exploring these issues and their willingness to offer policy advice varies widely. The explanation for this seems to be that as more “scientific” approaches become dominant in the field, willingness to do policy-relevant work declines. In order to avoid following the Golden Age into an intellectual cul de sac, and to truly realize a “renaissance” in policy relevant academic strategy, the current generation of scholars ought to embrace the sort of problem-driven approach to their work that characterized the early years of the nuclear age. Method-driven or purely theoretical scholarship is likely to lead us back into a dead-end.

While the most common examples of methods-driven research tend to use quantitative approaches, there is no logical reason that such approaches cannot be policy relevant. As our Golden Age history shows, many of the leading figures in nuclear strategy employed cutting-edge social science techniques and developed elegant theories along the way. The problem is not so much the technique itself or the effort to employ theory but rather the privileging of them over practical problems. Indeed we see some danger today with the growing preoccupation with qualitative and experimental methods that they too, if taken too far, could lead to an intellectual dead-end. Figure 3 tells a cautionary tale in this regard inasmuch as qualitative methods seem to be only slightly more relevant than other approaches save explicit policy analysis.

While much of this recent work could be relevant, scholars must work harder to translate their general findings into specific recommendations for nuclear strategy. Many of these questions are amenable to the type of careful historical analysis policymakers tend to find appealing. For example, scholars can trace the underlying processes in otherwise similar cases that resulted in success and failure. They can also examine in more depth the decisions of U.S. allies to abandon nuclear programs, as this is often put forward as a justification for a large U.S. nuclear force. Conducting these types of analyses can help determine the extent to which nuclear weapons were important in the outcome and how policymakers managed or failed to manipulate their nuclear arsenal for political leverage. Some scholars have done this in a single work. Others can build on the existing work in the most recent nuclear wave. These are by no means the only ways that scholars can address these questions. However scholars choose to do so it is clear that if the findings of the most recent wave are going to be relevant

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167 For a good example see Narang, Nuclear Strategy in the Modern Era, chaps. 9-10.
and ultimately have an influence on the policy debate scholars must address broadly important problems, present clear findings, focus on factors that policy can influence, and make explicit recommendations when and how to do so.

It is important to add a final caveat: Our criteria for relevance increase the likelihood for influence but do not guarantee it. In this sense they are akin to a “necessary” rather than a “sufficient” condition for influence. Policymakers may choose to ignore, dismiss, or simply be unaware of the findings of a very relevant study for any number of reasons. On the other hand, as the history of the Golden Age of nuclear strategy shows, studies that fail to meet these criteria are unlikely to have any influence by definition.