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H-Diplo/ISSF Forum on "Why Isn't There More Scholarly Evaluation of U.S. Wars?"

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Introduction by Christopher A. Preble, Cato Institute

Our panel at the annual meeting of the American Political Science Association (APSA) in San Francisco in 2015 was organized around the question "Why isn't there more scholarly evaluation of war?" I'm grateful to the editors at H-Diplo for their interest in this topic, and for the invitation to continue our discussion online.

Not all of the APSA panelists agreed with the premise – at least one argued that there has, in fact, been quite a bit of study of U.S. wars. But while the respondents here concede that there have been *some* scholarly evaluations, there is still reason to be concerned about the volume, quality, and character of that scholarship. These three essays focus on each of these problems. Jon Lindsay explains the relative low volume of scholarly war studies. Alan Kuperman argues that there have been many studies of war, but too few *scholarly* ones. Lastly, Benjamin Friedman critiques the character of these studies, specifically their tendency to focus on the conduct of warfare, not the rationales for going to war.

I tend to agree with my Cato colleague Friedman, but the points he raises also connect with some of Lindsay and Kuperman's observations.

Among the civilians who are willing to study all aspects of the nation's foreign policy, many are drawn from think tanks, not from the academy. These think tanks reflect the interventionist consensus, which tends to skip over the 'whethers' and 'whys' of the nation's wars, and goes instead directly to questions of 'how' and 'when.'

This 'operational mindset' is reflected across the board, regardless of the presumed ideological or partisan inclinations of various think tanks. It is generally true, for example, that think tanks have become more ideologically affiliated over the years, with the Heritage Foundation and the Center for American Progress being perhaps the most dramatic examples of this phenomenon on either side of the ideological/partisan spectrum. But one would be hard pressed to identify the ideological bias of the Council on Foreign Relations, or the Atlantic Council, or the Center for Strategic and International Studies. And, in the end, these non-ideological/non-partisan organizations also tend to focus on the operational aspects of wars. You are less likely to see think-tank scholars questioning the rationales for going to war than you would find in the typical political science department at a university. Recall that in the run-up to the Iraq War, most IR scholars in the academy opposed it,[1] while the vast majority of think-tank scholars either supported it, or were silent.[2] Subsequent analysis of the nation's wars, Kuperman explains, is heavily biased by one's hawkish or dovish tendencies.

Another reason why scholars might avoid studying war is a general sense of futility. Up and coming academics are advised to avoid writing about policy-relevant matters (notwithstanding several noble efforts to counter

[1] See, for example, James D Long, Daniel Maliniak, Susan M Peterson, and Michael J Tierney, "Knowledge without Power: International Relations Scholars and the U.S. War in Iraq" *International Politics* (2015) 52: 20–44.

[2] Stefan Halper and Jonathan Clarke, *The Silence of the Rational Center: Why American Foreign Policy Is Failing* (New York: Basic Books, 2007).

that state of affairs)[3], especially if such work comes at the expense of 'serious' study, usually involving lots of numbers and Greek symbols. And, as Lindsay notes, "Large-n datasets are hard if not impossible to find or compile for relevant activity, so quantitative empiricists tend to ignore intelligence operations and covert action." To properly evaluate the nation's wars, and the lesser conflicts that do not rise to that exalted classification, "scholars must get outside of their field, where the data is a mess, and study something their peers do not reward. These are bad incentives for good scholarship."

Scholars are also likely to run up against a generally hawkish bias[4] among the public at large, or, as Kuperman observes, the many doves within academia. And, lastly, even if scholars could convince their professional colleagues that an article studying war was important (and tenure-worthy), all that hard work might still elicit barely a ripple within the policy community, or the public at large.

And this is the third major factor why there might not be enough scholarly studies of war: there is, I am afraid, a general lack of interest in America's wars. But that doesn't mean we will not fight them. "Because the United States is relatively rich, safe and powerful," Friedman explains, "many wars are possible and few will seem costly. That is a recipe for having continual, ill-considered wars."

It is also a reason why the scholarly study of war is so important. It is a credit to the contributors to this forum that they have bucked these trends and have chosen to comment on this vital topic.

Participants:

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[3] See, especially, Bridging the Gap, http://bridgingthegapproject.org/about/; and also an online symposium hosted by *International Studies Quarterly*, http://www.isanet.org/Publications/ISQ/Posts/ID/1484/But-If-You-Try-Sometime-You-Might-Get-Some-of-What-You-Need-A-Response-to-Goldgeier-Weaver-and-Peterson; and the proceedings from a conference held at Tufts University, April 29, 2014, http://fletcher.tufts.edu/Ideas_Industry/Marketplace.

[4] Daniel Kahneman and Jonathan Renshon, "Why Hawks Win," *Foreign Policy*, October 13, 2009, http://foreignpolicy.com/2009/10/13/why-hawks-win/.

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Jon R. Lindsay is Assistant Professor of Digital Media and Global Affairs at the University of Toronto Munk School of Global Affairs. He is the author of *China and Cybersecurity: Espionage, Strategy, and Politics in the Digital Domain* (Oxford University Press, 2015), with Tai Ming Cheung and Derek Reveron, and his articles on cybersecurity and military innovation have appeared in *International Security*, *Security Studies*, *Journal of Strategic Studies*, and *Technology and Culture*. His current research includes a book on the impact of information technology on military power and a multi-institutional project on cross-domain deterrence. He holds a Ph.D. in political science from the Massachusetts Institute of Technology and an M.S. in computer science from Stanford University, and he has served in the U.S. Navy with operational assignments in Europe, Latin America, and the Middle East.

Review by Jon R. Lindsay, University of Toronto

Transaction costs in the evaluation of war

Strategy, in the classic war college formulation, is a relationship between ends, ways, and means. The evaluation of war can address any part of this relationship. Whose interests are best or worst served by the use of force? What military postures or employment doctrines are more or less effective? What are the implications of developing, using, or withholding certain types of weapons? These questions can be asked at any level of analysis, from grand strategic choices about prioritizing threats to tactical choices about prosecuting targets, and they can be asked about general categories of war or about specific wars.

These questions are always complex, but they are becoming even more complex in an age of economic globalization and technological sophistication. Innovation increases the portfolio of means that political actors can leverage to pursue their interests. Interdependence of communication, transportation, and finance provides many new ways to combine these means. Linkage across policy goals for industrial regulation, intelligence gathering, international governance, domestic security, and national defense also creates linkage, or interference, across the agencies that implement them. Counterinsurgency, for example, blends military operations with state building to suppress violence. Practitioners bemoan the lack of integrated 'whole of government' approaches to conflicts in Afghanistan and Iraq, often noting that strategy seems to be drifting or absent as different agencies pursue their own objectives. The coordination problem is perhaps worse for cybersecurity, because the private sector invents and operates a vital global infrastructure. Haphazard interconnection of critical systems for the sake of profit and convenience creates dangerous security externalities, while overzealous internet surveillance, once revealed, incentivize states to exclude internet firms perceived to be a threat vector.

The same strategic complexity that bedevils policy is also an obstacle to the evaluation of war. Most work on the relevance gap focuses on the motivations scholars have (or lack) to cross it. Joseph Lepgold and Miroslav Nincic argue that academia incentivizes scholars to pursue narrow questions with technical methods for a limited audience of academic peers, while policymakers ignore the theoretical literature and seek instrumental knowledge from their own bureaucracies or sympathetic think tanks.[1] Paul Avey and Michael Desch find an increasing gap between what policymakers want from the international relations field and what scholars provide.[2] Yet scholars also have a hard time providing what policymakers want even if they are motivated to do so. The sociotechnical complexity of warfare widens the gap even further. When there are large transaction costs in the connection of supply and demand, markets tend to fail.

The evaluation of war is hard and thankless work. Scholars must acquire specialized technical knowledge outside of their discipline and deal with missing or deceptive information to address questions that many of their peers view as marginal or uninteresting. The technological means of war have never been more

[1] Joseph Lepgold and Miroslav Nincic, *Beyond the Ivory Tower: International Relations Theory and the Issue of Policy Relevance* (Columbia University Press, 2001).

[2] Paul C. Avey and Michael C. Desch, "What Do Policymakers Want From Us? Results of a Survey of Current and Former Senior National Security Decision Makers," *International Studies Quarterly* 58, no. 2 (June 1, 2014): 227–46, doi:10.1111/isqu.12111.

sophisticated, so it takes great effort to understand the science behind how they work. These means tend to be employed with great secrecy, so it is hard to get reliable information about them. And they tend to be used in support of ambiguous, limited aims short of total war, so the subject matter is something other than the traditional IR emphasis on major, existential threats.

An understanding of technology has always been important for security studies. Indeed, the advent of nuclear weapons was a major catalyst in the development of the field. The details of delivery systems, nuclear force postures, and civilian nuclear power matter for assessing strategic alternatives and arms control initiatives. As the range of threat technologies and scenarios expanded during and after the Cold War, comparable technical depth was slow to develop, if ever, for other non-nuclear technologies. It is helpful to know something about computer programming or orbital dynamics if one wants to assess the prospects of war in cyberspace or outer space, for instance, or to evaluate the effectiveness of operations we have observed. At the same time, the once-central nuclear expertise in the field has attenuated as hiring committees and graduate students in security studies look to address a broader menu of other important problems, such as terrorism and ethnic conflict.

Understanding technology in this context, moreover, includes understanding the institutions and doctrine that make it possible to use material equipment for political purposes. Cyber weapons depend not only on software vulnerabilities and exploit methods but also the intelligence and planning capacity needed to tailor them to particular targets. Likewise, to evaluate the performance of counterinsurgency and counterterrorism campaigns, it is necessary to understand the contributions and limitations of modern intelligence, surveillance, and reconnaissance systems, precision force application platforms, and the interagency information sharing protocols that make them work together (or not). Greater sophistication and flexibility in technology tends to require a more educated workforce and elaborate bureaucratic procedures to manage it, so scientific-engineering expertise alone is insufficient for scholars to understand how modern weapons systems work.

Even if analysts master the technological foundations of military power, real world operations are usually, and increasingly, classified. Secrecy is indispensable for intelligence collection or cyber warfare because public revelation of sources and methods would enable targets to employ countermeasures. We are only now beginning to appreciate how the U.S. capacity for preemptive nuclear counterforce fielded by the end of the Cold War undercut widespread contemporary faith in the security of second strike forces.[3] The debate over the effectiveness of 'population centric' counterinsurgency tactics in Iraq has largely ignored or been unable to evaluate the contribution of the industrial-scale counterterrorism campaign conducted by Joint Special Operations Command, which killed or captured as many as 12,000 insurgents between 2003 and 2008.[4] To the degree that warfighting capabilities depend increasingly on intelligence, we can expect more warfighting itself to also be conducted in great secrecy.

[3] Austin Long and Brendan Rittenhouse Green, "Stalking the Secure Second Strike: Intelligence, Counterforce, and Nuclear Strategy," *Journal of Strategic Studies* 38, no. 1–2 (2014): 38–73.

[4] Mark Urban, *Task Force Black: The Explosive True Story of the Secret Special Forces War in Iraq* (Macmillan, 2011), 270–271.

Secrecy may also result from the political sensitivity of covert operations or bureaucratic maneuvers to impede critical assessment. There is much opportunity to leverage the secrecy needed to enhance military power to protect turf or avoid review. Policymakers may be tempted to use covert actions to pursue goals that they prefer not to publicly reveal to their electorate. Agencies may be tempted to inflate threats without providing details to secure greater budget share. The more that deception and clandestine operation become fundamental to national security, the greater the risk that less savory forms of political deception will be used to obfuscate their true purpose. Secrecy both complicates outside assessment and creates suspicion about motives.

Obviously it is difficult to evaluate war without data. Academic assessments of classified operations must look to the partial and contradictory accounts available in press reporting, expert interviews, and declassified documents to start connecting the dots. The irony is that modern intelligence systems behind the veil of classification generate some of the biggest 'big data' anywhere, yet scholars who would study contemporary operations in the open usually have to rely on more qualitative methods. Large-n datasets are hard if not impossible to find or compile for relevant activity, so quantitative empiricists tend to ignore intelligence operations and covert action. State secrecy and the methodological biases of political science combine to disincentivize the evaluation of modern war. Self-hiding phenomena tend to be left unexplained.

Further complicating the evaluation of modern war is the fact that a lot of conflict takes place in an expanding gray zone between peace and war. Not coincidentally, secrecy has an important role to play wherever ambiguity is an important strategic requirement, and modern technology provides more ways to act in secret without triggering an undesirable military response. Novel ways and means for coercive influence and *sub rosa* predation become attractive precisely because they are unhandled exceptions for existing policy. By the same token, it is easy, perhaps even wise, for scholars to marginalize cyberspace aggression or even terrorism and civil conflict as not really war. Many such activities do not to fit neatly into existing conflict databases. The ontological incompatibility of irregular conflict complicates quantitative analysis, above and beyond the secrecy problem, which again reduces the incentives for many political scientists to study them.

The gray zone is not a new phenomenon—intelligence collection and proxy war both have a long and intertwined history. Yet in previous eras their importance was swamped by the risks and dynamics of major power wars, so that intelligence, irregular warfare, and policing were rather minor concerns in the field of international relations. As states now face reduced incentives to fight openly, whether because of the robustness of military deterrence, the benefits of economic interdependence, or some combination thereof, they have incentives to find other ways to pursue their interests short of devastating conflict. Once-marginal activities become more and more typical. Unfortunately, the field of international relations lacks good theory about intelligence effectiveness or the interaction between war and policing comparable to the theoretical foundations of major war and high-end deterrence. How are we to think about, much less evaluate, the intelligence and counterintelligence activity that is occurring today on an unprecedented scale without a sound theoretical foundation?

In sum, political actors today use complex technology in secret ways for limited ends. To evaluate war—or rather the confusing conflicts short of what we usually recognize as war—scholars must get outside of their field, where the data is a mess, and study something their peers do not reward. These are bad incentives for good scholarship.

Response by Benjamin H. Friedman, Cato Institute

Security analysis in Washington, D.C. responds to political demand. The hawkish consensus reigning there limits demand for critical evaluation of U.S. defense goals, including wars. The U.S. government generally wants two other things from security analysts: legitimization of policy and help with its conduct. Those requests encourage analysts to study how best to achieve goals, not whether they are worth pursuing.[1] That means asking how to prosecute wars more than whether to have them. Academic scholars encounter similar pressures to the extent that they seek influence in the capitol.

There is plenty of debate about foreign policy in Washington. But little of it questions the major arguments justifying the nation's wars, especially those that seem likely to be cheap, like air and drone strikes. Even the 2003 Iraq War was, at the outset, relatively uncontroversial among foreign policy elites, as opposed to the public and academics.[2] Pre-war debate and think tank analysis centered on how to go to war: the adequacy of the intelligence and international support. As the war grew unpopular, the analytic focus shifted to deficiencies in intelligence analysis, war-planning, counterinsurgency doctrine, rather than the theories of democratization, energy security, non-proliferation, and failed states justifying the war and occupation.[3] Academics write about those topics, but Washington's debates and writings mostly ignore academic work.

The Washington take-away from the troubled wars in Iraq and Afghanistan seems to be to tolerate less cost in support of similar goals—avoiding prolonged military occupations while still using military force to achieve revolutionary ambitions in troubled countries. For example, one seeming lesson of recent U.S. wars is that overthrowing autocrats ruling over fractious polities might unleash lasting conflict rather than stability, let alone liberal democracy. That conclusion vitiates the main rationales offered for disposing the Qaddafi and Assad regimes in Libya and Syria. Nonetheless, foreign policy elites, including scholars at major think tanks, broadly supported recent U.S. efforts to displace the Libyan and Syrian regimes, generally without engaging that counterargument.[4]

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[1] The argument here comes largely from a co-authored paper discussing why there is so little evaluation of grand strategy in Washington. Benjamin H. Friedman and Justin Logan, "The Operational Mindset: Why Washington Doesn't Debate Grand Strategy," paper under submission.

[2] On elites, see Adam J. Berinsky, "Assuming the Costs of War: Events, Elites, and American Public Support for Military Conflict," *Journal of Politics* 69:4, (2007): 975–97. On academics, see Daniel Maliniak *et al*, "The View from the Ivory Tower: TRIP Survey of International Relations Faculty in the United States and Canada," February 2007, https://www.wm.edu/offices/itpir/_documents/trip/ivory_tower_view_2007.pdf.

[3] Benjamin H. Friedman, Harvey M. Sapolsky and Christopher Preble, "Learning the Right Lessons From Iraq," Cato Institute Policy Analysis 610, (February 2008).

[4] See, for example, Ivo H. Daalder and James G. Stavridis, "NATO's Victory in Libya: The Right Way to Run an Intervention," *Foreign Affairs* 91/2 (2012): 2–7; Anne-Marie Slaughter, "Why Libya Skeptics Were Proved Badly Wrong," *Financial Times*, August 24, 2011; and Anne-Marie Slaughter, "Syrian Intervention is Justifiable, and Just," *Washington Post*, June 8, 2012.

So what needs explanation is not non-evaluation of war but under-evaluation of war.[5]

My explanation has two parts, dealing with the demand and supply of analysis. The former considers how the foreign policy elites' hawkishness suppresses debate. The supply side explains why analysts, even those at seemingly independent think tanks, rarely supply evaluation anyway. They serve the political system rather than guiding it.

The primacy cause of the limited evaluation of war is relative power, meaning the advantages in wealth, geography, and military capability that allow the United States to adopt ambitious military objectives abroad. Over time, relative power has produced bipartisan support for a grand strategy of primacy among foreign policy elites, which generates hawkish beliefs.[6] Primacy, to simplify, consists of two core beliefs.[7] One is that U.S. leadership is crucial to the maintenance of global order, which refers generally to peace among great powers, international commerce, and state cooperation through international organizations. A second belief is that U.S. leadership is comprised largely of military commitments—allies, overseas bases, naval patrols, and threats or acts of war. The reasoning is generally that U.S. military power deters aggression, limiting the need for states to defend themselves, preventing security dilemmas.

Primacy's advocates see many threats. They worry about the credibility of the many promises the United States makes to defend allies. They fear proliferation of weapons technology, especially nuclear weapons. Especially in the Beltway, they tend to argue that internal conditions abroad—foreign civil wars, failed states, or illiberal government—can undermine U.S. global leadership, creating danger. This expansive view of interests and threats makes primacy conducive to war.[8] It offers a grab bag of reasons to support proposed wars or military strikes and few arguments for peace.

Relative power produces support for primacy in two ways. First, over time, it distributes the human and material costs of hawkish policies, diminishing their electoral relevance. Military prowess and geography insulate most Americans from threats, allowing them to be relatively indifferent—rationally ignorant—about

[5] On non-evaluation, see Stephen Van Evera, "Why States Believe Foolish Ideas," in Andrew K. Hanami, ed., *Perspectives on Structural Realism* (New York: Palgrave, 2003), 163-198.

[6] I develop this argument in Benjamin H. Friedman and Justin Logan, "Why the U.S. Military Budget is Foolish and Sustainable," *Orbis* 56/ 2 (2012); and Benjamin H. Friedman "Alarums and Excursions: Explaining Threat Inflation in U.S. Foreign Policy," in Christopher A. Preble and John Mueller, eds., *A Dangerous World? Threat Perception in U.S. National Security* (Washington DC: Cato Institute, 2014), 281-303.

[7] Another term that applies here is "liberal hegemony," a label for the updated version of primacy critiqued in Barry R. Posen, *Restraint: A New Foundation for U.S. Grand Strategy* (Cornell University Press, 2014). A recent article making the case for primacy calls it "deep engagement." Stephen G. Brooks, G. John Ikenberry, and William C. Wohlforth, "Don't Come Home, America: The Case against Retrenchment," *International Security* 37/ 3 (2012/13): 7–51.

[8] On how unipolarity encourages war, see Nuno Monterio, "Unrest Assured: Why Unipolarity Is Not Peaceful," *International Security* 36/3 (Winter 2011/12): 9–40.

war's wisdom.[9] As a result, security policy tends to rank low among voters' concerns and politicians have little incentive to cater to voters' foreign-policy views. They are relatively free to adopt undemocratic stances.[10]

U.S. wealth creation, meanwhile, spreads the economic burden of U.S. security policies. For example, U.S. defense spending authority, adjusting for inflation, was about $614 billion in fiscal year 2014 versus $670 billion in 1952, the highest annual total of the Cold War.[11] In 1952, that spending amounted to 13 percent of gross domestic product and 68 percent of federal spending. Today those percentages are 3.5 and 16.5, respectively.[12] U.S. spending on the Iraq War never took more than one percent of GDP.[13] Drone strikes and air campaigns, like the 2011 bombing of Libya, cost tiny fractions of that.[14]

Thus in the material and human sense, U.S. security policy has become less burdensome, though barely cheaper. That shift means that primacy's policies, including wars, require a smaller portion of taxes and less painful tradeoffs from other government programs. Interest groups associated with low taxes and groups defending domestic spending programs have less reason to organize opposition. Because few Americans worry about going to war, peace groups suffer. Lack of organized opposition mutes pluralistic debate among competing societal ends.

The other way relative power encourages primacy is by concentrating its benefits and generating organizations and interest groups that promote it. The exercise of global military power generated an institutional support base for continuation of those policies. This is the military-industrial-congressional-complex, plus various friends and clients that rely on its largesse, including some think tanks.[15] As with other public policies with

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[9] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957).

[10] Joshua Busby and Jonathan Monten, "Republican Elites and Foreign Policy Attitudes," *Political Science Quarterly* 12:1 (2012):105-142.

[11] National Defense Budget Estimates for FY 2016, Office of the Under Secretary of Defense (Comptroller), March 2015, http://comptroller.defense.gov/Portals/45/Documents/defbudget/fy2016/FY16_Green_Book.pdf, pp. 140-146. Measured in outlays, U.S. defense spending is higher now than at any time during the Cold War.

[12] *Ibid.*, 264-266.

[13] On the Iraq War direct costs, see Amy Belasco, "The Cost of Iraq, Afghanistan, and Other Global War on Terror Operations Since 9/11," Congressional Research Service, December 8, 2014.

[14] The United States spent $1-2 billion on the Libya campaign in 2011. Kevin Baron, "For the U.S., War against Qaddafi Cost Relatively Little: $1.1 Billion," *The Atlantic*, October 21, 2001, http://www.theatlantic.com/international/archive/2011/10/for-the-us-war-against-qaddafi-cost-relatively-little-11-billion/247133/.

[15] Richard K. Betts, "The Political Support System for American Primacy," *International Affairs* 81, no. 1 (January 2005): 1–14.

concentrated benefits and diffuse costs, a minority with strong and generally mutual interests rule over majority of the rationally apathetic.[16]

These interests typically oppose policies which are adverse to primacy—like exiting alliances. That encourages leaders, including presidents, to stick with the strategic status quo and defend it with primacy's arguments. By creating an echo of arguments favoring primacy, these interests heighten support for it. Repetition creates both true believers and social adherents who outwardly concur for professional or social reasons.[17]

The consensus around primacy makes policy-makers in both parties generally hawkish relative to the public.[18] These views convey themselves through various political incentives to think tanks. Before elaborating on those incentives, two caveats are useful.

First, incentives operate on people to differing degrees depending on their employer, ambitions and personality. Analysts may buck incentives, but their collective effect is strong. Second, these incentives do not encourage analysts to take aggressively pro-war stances. As the Iraq War demonstrated, being overly bellicose can have unfortunate professional consequences for analysts, at least on the left. The better course is to avoid strong anti-war positions and to focus on operational questions.

Understanding what think tanks produce requires understanding their main tasks: raising money and touting access to a powerful audience. Non-profits must raise money in order to operate. The ability to speak to policy-makers or a large audience allows think tanks to promise results, which keeps up morale and aids fundraising.

Raising money requires pleasing funders and thus doing what many of them want. People, including those running foundations or grant-making arms of government agencies, fund think-tanks for two main reasons. One is to get a particular question answered. The other reason, probably more common, is to influence politics—to promote some change.

Access to policy-makers means responding to their wants, as well. In theory, that can be three things. The first is help with preference formation or goal setting, where think tank analysts are like salespeople in a free marketplace of ideas. Policy-makers also want help with preference implementation. That can be operational

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[16] Mancur Olson, *The Logic of Collective Action: Public Goods and the Theory of Groups* (Cambridge, MA: Harvard University Press, 1971); and Robert Dahl, *A Preface to Democratic Theory* (Chicago: University of Chicago Press, 1956), 124–51.

[17] This is a softer version of the "preference falsification" that occurs in autocracies according to Timur Kuran, *Private Truths, Public Lies: The Social Consequences of Preference Falsification* (Cambridge, Harvard University Press, 1997).

[18] On this gap, see, e.g., Marshall M. Bouton and Benjamin J. Page, *The Foreign Policy Disconnect: What Americans Want from Our Leaders but Don't Get* (Chicago: University of Chicago Press, 2006). A recent demonstration of the gap is Dina Smeltz et al, *United in Goals, Divided on Means: Opinion Leaders Survey Results and Partisan Breakdowns from the 2014 Chicago Survey of American Opinion on U.S. Foreign Policy* (Chicago: Chicago Council on Global Affairs, 2015).

support, where outside experts help turn general goals into policy programs or evaluate alternative means to an end. It can also mean marketing, where experts' endorsement heightens support.

There are several reasons why policy-makers want help less with policy formation than policy-implementation, especially the sales element. First, other politicians, pollsters, political consultants, staff, interest groups and parties already compete for the policy guidance role. Second, office-holders often lack the time required to investigate intellectual alternatives. Third, the diffusion of power in the U.S. political system creates status quo bias, which makes opportunities for big changes rare. And the difficulty of change forces leaders to constantly sell their policy preferences to others.[19] Expert support gives policies a sheen of scientific legitimacy.

One U.S. Senator described this legitimization function this way: "you can find a think tank to buttress any view or position, and then you can give it the aura of legitimacy and credibility by referring to their report."[20] Rory Stewart, an expert on Afghanistan who opposed the 2009 surge, describes how this dynamic played out in his consultation with Obama administration officials planning the surge:

> "It's like they're coming in and saying to you, "I'm going to drive my car off a cliff. Should I or should I not wear a seatbelt?" And you say, "I don't think you should drive your car off the cliff." And they say, "No, no, that bit's already been decided—the question is whether to wear a seatbelt." And you say, "Well, you might as well wear a seatbelt." And then they say, "We've consulted with policy expert Rory Stewart and he says…"[21]

The need to please funders and possibly political leaders makes think tanks a part of the politics they analyze. There is nothing wrong with that besides violation of the polite fiction that think tanks are totally non-political. But think tanks' competitive advantage is their balance of subordination to a political agenda and retention of independent expertise. Lobbyists have expertise, but it is worth little as an endorsement because everyone knows it is bought. Academics may be more impressive scholars, but they're less attuned to and interested in what political leaders want. If think tanks ignored what their sponsors wanted and had no predictable politics, they would cease to raise money.

These general observations about think tanks point to three pressures, active to varying extent in different think tanks, which encourage analysts to avoid controversial questions about war. The first is funding. Some think tanks rely almost entirely on U.S. government funds, some on major defense contractors. Some analysts are even consultants to defense contractors.

Funders are unlikely to demand particular answers. But they bind analysts' output by controlling what questions they answer. And for the reason discussed above, their questions are unlikely to challenge Washington's hawkish consensus. Aggressive questioning of war goals, even outside of sponsored research, is

[19] Theodore Lowi, *The End of Liberalism*, 40th Anniversary Ed. (New York: W. W. Norton, 2009).

[20] Senator Olympia Snowe quoted in Ezra Klein, "Unpopular Mandate: Why Do Politicians Reverse Their Positions?" *New Yorker*, June 25, 2012.

[21] Emily Stokes, "Lunch with the FT: Rory Stewart," *Financial Times*, August 1, 2009.

unlikely to draw immediate consequences. But it might affect future funding. Foundation grants also create pressures to avoid certain arguments.

Professional ambition is a second cause of analytic restraint. Because most think tank analysts want top government jobs at some point, they have reason to avoid offending the dominant foreign policy views of their party. That creates pressure to avoid excessive criticism of recent war aims, which, as noted, tend to have bipartisan support. For example, ambitious Democratic analysts, now preparing themselves for the Hillary Clinton administration, will avoid dovish criticism of the wars in Libya and Syria.

A third cause is socialization. The dominance of primacy and hawkishness among foreign policy elites creates social pressure to conform. Analysts may avoid criticizing U.S. war goals to avoid the social discomfort of being at odds with their peers and seen to hold irrelevant views. Even Leslie Gelb, as president of the Council on Foreign Relations, was not immune to such pressures. He attributed his support for the Iraq War, which he'd come to regret, to "unfortunate tendencies within the [Washington] foreign policy community, namely the disposition and incentives to support wars to retain political and professional credibility."[22]

If academics seek grants, appointments, and access at Washington's foreign policy institutions, they confront some of the incentives think tank analysts do.[23] Still, the academy's professional incentives leave its scholars less susceptible than think tank analysts to these pressures. Tenure protects those who have it and see little to gain from official favor. And by rewarding novel theory and bold conclusions, political science creates incentive to find flaws in key theories underlying popular foreign policies and grand strategies.

One conclusion is that scholars should provide the war evaluation that think tanks avoid. But policy-makers will pay little attention to this analysis—not because it is esoteric but because it will not help them. Another road to relevance is to influence public beliefs in ways that eventually constrain Washington. Academia should reward that brand of relevance but understand that it often means being a naysayer that officialdom ignores or attacks.

Another conclusion is that debate about war would improve with greater dissent among political elites—broader competition among parties, organizations, interest groups and beyond. If elites demanded tougher evaluation of wars, aligned think tanks would provide them. That seems unlikely now, because the conditions – U.S. wealth and power— that encouraged primacy and its hawkish outputs appear durable.[24]

Under-evaluation of war is likely to remain a problem for U.S. democracy. It seems that a cost-bearing public and the separation of power do little to encourage intelligent war policy, at least where costs are low for most

[22] Leslie H. Gelb with Jeanne-Paloma Zelmati, "Mission Unaccomplished," *Democracy*, no. 13 (Summer 2009), 24. Another example showing the confluence of several these pressures is RAND's research on the Vietnam War. Mai Elliott, *RAND in Southeast Asia: A History of the Vietnam War Era* (Santa Monica, CA: RAND Corporation, 2010).

[23] Hans J. Morgenthau, "The Purpose of Political Science," in James Clyde Charlesworth, *A Design for Political Science: Scope, Objectives, and Methods* (Philadelphia: American Academy of Political and Social Sciences, 1966): 63-79.

[24] Friedman and Logan. For a more recent and optimistic take relying on recent public opinion trends, see Michael C. Desch, "How Popular is Peace," *The American Conservative*, October 21, 2015, http://www.theamericanconservative.com/articles/how-popular-is-peace/,

voters.[25] Because the United States is relatively rich, safe and powerful, many wars are possible and few will seem costly. That is a recipe for having continual, ill-considered wars.

[25] For more carefully-developed skepticism about U.S. intelligence in making war, see John M. Schuessler, *Deceit on the Road to War: Presidents, Politics, and American Democracy* (Ithaca, Cornell University Press, 2015). See also, A. Trevor Thrall, "A Bear in the Woods? Threat Framing and the Marketplace of Values, *Security Studies* 16:3 (2007): 452-488.

Response by Alan J. Kuperman, University of Texas at Austin

I agree with this forum's premise that there is insufficient scholarly evaluation of U.S. wars. However, this is not due to any dearth of evaluation of such wars by scholars. Rather, the problem is that much of their evaluation is not 'scholarly.'

Too often, academic analysis of military action is distorted by the scholars' underlying bias – typically either 'hawk' (favoring use of force) or 'dove' (opposing use of force) – rather than reflecting objective evaluation. Such bias may operate either consciously or unconsciously.[1] The biased analysis is typically dressed up in scholarly language so as to appear objective.

(The bias of some scholars is more complicated. A third type may be labeled 'national-interest' bias, which favors use of force in pursuit of a state's selfish goals but opposes it for altruistic ones. A fourth is the opposite, 'humanitarian' bias, which favors use of force for altruistic goals but opposes it for selfish ones. The analysis below applies logically to these biases too.)

My critique is not entirely novel. Richard Betts, in his 2000 article, "Is Strategy an Illusion?" argued that politicians fall victim to such bias when attempting strategic evaluation:

> "Strategy is an illusion because leaders do not understand what motives drive them. . . . They use war . . . for subliminal personal [reasons], so the link between political ends and military means is missing at the outset."[2]

Although scholars should aspire to overcome such biases, they are susceptible to the same cognitive foibles, so their analyses often are likewise distorted. Thus, one can modify Betts's critique to apply to academics, as follows:

> 'Scholarly evaluation of war is an illusion because scholars do not understand what motives drive them. . . . They use the study of war . . . for subliminal personal [reasons], so their evaluation of the link between political ends and military means is doomed from the outset.'

This bias problem might be manageable if the resort to force were always a good or bad idea. For example, if it were always a good idea, then hawks would always be right, so we could embrace their analysis and ignore doves. Or vice-versa, if force were always a bad idea.

In reality, however, force is sometimes a good idea and at other times a bad idea. This assertion can be proved logically by citing just one instance of each. Most obviously, the use of force by the Allies against Hitler's Nazi Germany in World War II was beneficial in protecting Anglo-American interests and ideals by liberating Western Europe. By contrast, the use of force by a U.S.-led coalition against Saddam Hussein's Iraq in 2003

[1] The causes and consequences of such biases are discussed in Robert Jervis, "Hypotheses on Misperception," *World Politics*, Vol. 20, No. 3 (April 1968): 454-79.

[2] Richard K. Betts, "Is Strategy an Illusion?" *International Security*, Vol. 25, No. 2 (Fall 2000): 5-50.

was disastrous for American interests and ideals by wasting a trillion dollars to create chaos and a safe haven for international terrorists in a country that previously had opposed such terrorists and that did not have an active weapons of mass destruction program.

Because force is only sometimes a good idea, scholars with persistent bias – whether hawk or dove – are guaranteed to produce erroneous evaluation some of the time. Only scholars who avoid bias – by sometimes favoring force and at other times opposing it – have any chance of being right all the time.[3] (Of course, merely avoiding bias does not guarantee perfect evaluation but only makes it possible. Particularly poor analysts might improve their performance by being biased, which would at least ensure that they were right some of the time, just as a broken clock is guaranteed to display the correct time twice a day.)

Why So Much Bias in Scholars' Evaluation of U.S. Wars?

The causes of bias in the evaluation of military force are not limited to the cognitive shortcomings of individuals, as highlighted by Betts and Jervis, but also include institutional dynamics. Academic programs in security studies have strong biases, and their leading professors instill those biases in their students.

This problem was illustrated to me several years ago when a junior professor, who had graduated from a leading dove-biased security-studies program, asked a poignant question. He did so after he and his mentors had opposed a particular military intervention, warning of all the possible costs and dangers, only to see the intervention produce a quick and easy victory.

The young professor asked: Is it really scholarly, or productive, to always focus one's evaluation on the dangers of using force, as dove-biased scholars do? Of course, such distorted analysis does serve a social function by reminding policymakers and the public of the potential downside of military action. However, it is not 'scholarly evaluation,' any more than is hawk-biased analysis that selectively highlights the potential benefits of force.

(Full disclosure: My own publications have been the target of criticism from both hawks – when I opposed military intervention in Kosovo, Iraq, and Libya – and doves, when I favored threats of force to compel Iran to curtail its nuclear weapons program. This does not imply that all who opposed my views were biased, but many of them had consistent track records of supporting or opposing the use of force.)

How to Make Evaluation of U.S. Wars More Scholarly

If my premises are correct – that biased academic programs instill such bias in their students and thereby perpetuate the biased evaluation of U.S. wars – then remediating the problem would require one of two solutions. In either case the necessary first step – itself a major hurdle – would be for the senior scholars who lead these programs to recognize, at least to themselves, that their scholarship and institutions are biased. If

[3] For another discussion of overcoming the hawk-dove divide, see Graham Allison, Albert Carnesale, and Joseph S. Nye, Jr., eds., *Hawks, Doves, and Owls: An Agenda for Avoiding Nuclear War* (W. W. Norton & Company, 1986).

they could do so, then one hypothetical remedy would be for these scholars to purge bias from their own evaluation of U.S. wars, but that is unlikely as it would require them to acknowledge publicly their past bias.

The second, somewhat more plausible remedy would be for biased academic programs to hire faculty who have the opposite bias. In that way, students could be exposed to opposing biases, which might motivate and enable them to engage in unbiased, scholarly evaluation. (I was lucky to gain such exposure by pursuing two graduate degrees in two different programs that had opposing biases, but most students are not so fortunate.)

The good news is that this remedy is straightforward and would be relatively easy to implement. The bad news is that the scholars who lead academic security-studies programs are unlikely to embrace it. That is because they view the evaluation of military force as a Manichean battlefield. Any concession to the opposing perspective is resisted as surrendering ground to the enemy.

If my critique appears overstated, I would ask the reader to consider a thought experiment. Pick your favorite dove scholar who leads an academic security-studies program. Now imagine that scholar using one of his or her precious tenure-track lines to hire a hawk scholar who would promote neoconservative ideas among the program's students. A fair-minded reader will agree that it is hard to envision. (Of course, the experiment works equally well in reverse.)

Yet, until our leading scholars of security studies gain the courage and confidence to challenge their own biases – by hiring colleagues who espouse contrary views – our students will suffer, as will the scholarly evaluation of U.S. wars.