



VOL. 6 / NO. 4 / SPRING 2012



MIND THE GAP

by Francis J. Gavin and James B. Steinberg

WHY POLICYMAKERS AND SCHOLARS IGNORE EACH OTHER, AND WHAT SHOULD BE DONE ABOUT IT



Top: Francis J. Gavin, Director of the Robert S. Strauss Center for International Security and Law at

Editor's Note: One of the issues that Carnegie Corporation's International Peace and Security Program seeks to address is how the knowledge generated by America's academic community can be linked to the U.S. foreign policymaking process. In this article, two noted experts, Francis J. Gavin, Director of the Robert S. Strauss Center for International Security and Law at the University of Texas and the Tom Slick Professor of International Affairs at the LBJ School, and James B. Steinberg, Dean of The Maxwell School of Citizenship and Public Affairs at Syracuse University and former high-ranking U.S. foreign policy official, take on the question of how academic research can best contribute to the development of sound foreign and international security policy and, in cases when such scholarship might otherwise muddy the waters, what can be done to remedy that effect.

In recent months, the U.S. foreign policy debate has focused with increasing intensity on how to deal with Iran's nuclear program, and in particular, whether, and under what circumstances, the U.S. or Israel should use military force to prevent Iran from acquiring a nuclear weapon. Any decision by a U.S. president to authorize the use of force is a weighty one, but in the case of Iran the decision either to act or acquiesce is

the University of Texas and the Tom Slick Professor of International Affairs at the LBJ School. Bottom: James B. Steinberg, Dean of The Maxwell School of Citizenship and Public Affairs at Syracuse University.

especially difficult and consequential, and will have a profound and lasting effect on world politics and American foreign policy for years to come.

The issue has gained increasing prominence in the national political debate, fueled in part by a blizzard of articles, op-eds and cable network appearances by academic scholars as well as former government officials and professional pundits. Some of these contributions are highly rhetorical, but others purport to draw on academic research and theory. (1) Given the level of scholarly activism, and the willingness of the scholars to go beyond professional journals to enter into the public arena, the issue of how to deal with Iran poses in a very stark way a broader issue that has increasingly preoccupied both scholars and practitioners—just how useful is academic research in areas of national security and international relations to policymakers—and if, as our own experience suggests, that contribution is at best limited, and often even misguided, what can or should be done to remedy the deficit?

Closely examining the Iran problem is useful because too much of the debate over the utility of academic social science in the area of international affairs is highly abstract and prone to assertion instead of analysis. And even more important, it tends to gloss over the real-world complexities and uncertainties that are so sharply illustrated by the dilemmas that policymakers face in dealing with Iran.

In order to answer how academic research and theory might guide policy choices on Iran, one would need to understand both the immediate and long-term consequences of the policy the United States chose. This, in turn, requires an assessment of plausible scenarios that might emerge from competing policy choices. If the United States chose not to bomb Iran, would countries in the region eschew their own nuclear weapons and work with the U.S. to balance against and contain a nuclear Iran? Or would Iran's nuclear capability drive neighboring states to "bandwagon," or ally with Iran, or seek their own nuclear weapons, undermining U.S. influence while destabilizing the region? And if the United States did successfully strike, what are the chances such military action would lead to an overthrow of the current regime and its replacement with a government both friendly to the west and willing to forego nuclear weapons? Or could a military strike provide a lifeline to an unpopular regime, inflame anti-American sentiment throughout the region and unleash a wider military conflagration?

The potential consequences of any of these scenarios are not limited to the region around Iran. How

would key global actors such as Russia, China and various Western European allies respond, and how would our choices affect our long-term relations with them? What would be the effect of particular choices on other countries contemplating a decision to build nuclear weapons? What impact would U.S. actions have on our decades-long global strategy of inhibiting proliferation by extending our own nuclear deterrent to our nonnuclear allies? The sum of the actions of multiple participants, acting and reacting to constantly changing circumstances, in time creates an infinite number of plausible but unknowable futures, some good for the United States, some bad and many indeterminate.

Needless to say, the answers to this highly incomplete list of extraordinarily important questions are critical to any judgment on the costs and benefits of the different policy choices. The academics who have offered their unqualified opinions on what should be done are—explicitly or implicitly—claiming to be able to answer these questions with enough confidence to affix the stamp of academic legitimacy to their prescription.

Yet, the experience of both authors of this article (James Steinberg is a former senior policymaker; Francis Gavin is an historian of U.S. foreign policy) convinces us that the “right” answer—but the one you will never read on the blogs or hear on any cable news network—is that we simply cannot know ahead of time, with any usable degree of certainty, what the answers to these questions will be, and therefore what optimal policy will turn out to be. Why? The answer is that none of the tools that social science academics labor so assiduously to develop and refine are capable of providing predictive outcomes with a usable degree of certainty. In their desire to achieve the rigor of their natural science counterparts, most social science academics have developed a profound aversion to the inherent uncertainty and contextual specificity that plagues strategic policy formulation and hew to the notion that the theories they work with cannot usefully make the transition from the “laboratory” to the real world. What Steve Coll recently called the “crucible between uncertainty and risk” is not unique to U.S. decision-making about Iran. (2) Making global policy—as opposed to punditry—is difficult and unforgiving.

This is, of course, not a novel observation. Nor does it offer much relief to the overworked, overstressed policymakers facing momentous decisions she or he cannot avoid, or encourage the highly trained scholars and analysts who sense that their efforts are utterly ignored by the policy community in Washington. But properly understood, there are important lessons that can help increase the utility of academic social science to international relations practice and lead to better policies. Forecasts and predictions are of little use to a policymaker seeking optimal outcomes in the face of radical uncertainty and immeasurable complexity. Rather than assume away the problem with

artificial simplifications, what policymakers crave is help imagining alternative scenarios and multiple outlooks, while developing strategies to mitigate the downside risks and maximize upside benefits as they jump into an unknowable future.

We believe that if different types of expertise—from across the social sciences, history and “strategic studies/international relations” community—were brought together with practitioners, in an environment that encouraged honest debate and collaboration and not point-scoring, the benefits could be enormous. If participants were encouraged to be candid about the limits as well as the insights of what their disciplines can contribute to understanding the consequences of policy choices, it would be possible to achieve both greater coherence and humility in our foreign policymaking and the process would be greatly enhanced. This would be far more useful to decision-makers than the one-off predictions, historical analogies and binary choices that are currently offered by many experts.

CALL OFF THE MONKEYS

Shouldn't experts—scholars, pundits, analysts and others trained to understand international relations—be able to help us make these difficult predictions? In fact, as Philip Tetlock demonstrated in *Expert Political Judgment*, a 20-year study that looked at over 80,000 forecasts about world affairs, self-proclaimed authorities are little better at making accurate predictions than monkeys throwing darts at a dartboard. According to Tetlocks' research, knowing a lot about an issue can actually make you a worse political forecaster than knowing very little. (3) And recent research casts doubt on some of the core assumptions that underlie important strands of political science and economic theory, which frequently form the basis for policy prescriptions—for example, that political leaders can be assumed to be utility maximizers, or that the internal composition and history of states are largely irrelevant in predicting how they will behave in response to external events.

Ironically, those experts who make the most bold and confident predictions, based on singular views of how the world works—for example, the international system is anarchic and war prone, civilizations clash, dictators should never be negotiated with, democratization and market economies will end war, etc.—are both the most sought after for their judgments and the most likely to be wrong. These “parsimonious theorists” or “hedgehogs,” as the political philosopher Isaiah Berlin once dubbed them, are not scarce when it comes to providing advice to statesmen on any number of critical foreign policy issues. (4) We see this in the current debate over the consequences of a nuclear Iran. One school tells us not to worry; nuclear weapons always provide deterrence and stability and are therefore no threat to U.S. interests. Another tells us that a nuclear Iran will become emboldened, aggressive and perhaps even share its weapons with terrorists. These assessments are made, it should

be pointed out, with almost no access or insight to the calculations and deliberations of the policymakers in Iran responsible for their nuclear program. (5)

Such binary choices—“either-or choices,” which are the standard fare of academic hedgehogs—provide far less to policymakers than the ivory tower realizes. Consider the case of NATO enlargement, one of the most contentious and consequential policy debates of the 1990s. Like the Iran question today, this issue brought out the academic heavyweights. On one side were the “realists” who warned that enlargement was a direct and unwise challenge to Russia’s security interests, risking a new and dangerous Cold War. On the other side stood liberal internationalists, who believed that NATO’s security blanket, in combination with membership in the European Union, would consolidate democracy and economic reform in Central and Eastern Europe, avoid a dangerous security vacuum in Europe’s heart and lead to a more peaceful continent. Each side was dismissive of the other, seeing little room for compromise or nuance. (6)

What did the policymakers do? Statesmen, unlike academics, do not have the luxury of “betting” on one theory or the other, and in this case, borrowed the better elements from both theories, while adding elements no academic had considered. The ensuing strategy enlarged NATO while keeping the door open to Russian membership. New structures, such as the Partnership for Peace and the Organization for Security and Co-operation in Europe, were created to transcend the Cold War divide. Interestingly, the policy innovation came not from the academy, but from practitioners and the think-tank world.

Pundits and policymakers both commonly explore the past to find examples of policies that can guide current decision making. While at first blush this seems wise, it is not fail-safe.

Was the policy a success? As Zhou Enlai purportedly said about the French revolution, “it is too soon to say.” To be sure, the worst predictions of both camps have not been realized, and scholars have not, for the most part, anticipated the challenges that have emerged. The charge made at the time by the distinguished diplomatic historian, John Lewis Gaddis, that the Clinton administration’s policy “violated every one” of the core principles of good grand strategy while positing his belief, shared by

most academics, that NATO enlargement was “ill-conceived, ill-timed, and above all ill-suited to the realities of the post-Cold-War world,” seems, in retrospect, questionable at best. (7) The key, however, is to not argue who was right or wrong, but to highlight how the polarized academic debate did not address many of the key concerns of policymakers and was ultimately of little use. The heavyweight battle between realists and liberal internationalists was not, as advertised in the academy, the “main event.”

LESSONS OF THE PAST?

What about looking to history for lessons? Pundits and policymakers both commonly explore the past to find examples of policies that can guide current decision-making. While at first blush this seems wise, it is not fail-safe. Four decades ago, the historian Ernest May warned against the tendency for policymakers and analysts to employ simple but misleading analogies from the past to justify difficult policies. (8) Would allowing the aggressive and dangerous regime in Iran to acquire nuclear weapons be akin to another “Munich,” the wartime conference where British Prime Minister Neville Chamberlain infamously capitulated to Nazi leader Adolf Hitler’s outrageous demands? Or would a dangerous military action halfway across the world bog us down in another “Vietnam,” a quagmire of a war that saps American blood and treasure not justified by national interest? In both cases, the simplistic use of lessons from the past obscures and distorts more than it reveals, and may be misleading for those trying to make a decision about whether or not to strike Iran. There is no guarantee that using a more recent historical incident—for example, the erroneous intelligence about weapons of mass destruction in Iraq that led to an eight-year, trillion-dollar American military intervention—would be any more helpful in making policy toward Iran.

Even more sophisticated and nuanced uses of history are not without their difficulties. When thinking about the consequences of a nuclear-armed Iran, some historians have pointed to how the Johnson administration responded to the nuclearization of the People’s Republic of China in October 1964. After weighing the potential benefits and costs of a preventive strike, the United States accepted and actually downplayed the significance of China’s nuclear capability. Mao’s China—which had been reckless abroad and ruthless at home—did not become more dangerous as an atomic power. In fact, in less than a decade after its nuclear test, China had become a de facto ally of the United States, and a crucial partner in the Cold War rivalry with the Soviet Union. It is hard to imagine such an alliance if the United States had decided to strike in 1964.

Does this argue against striking Iran? Not necessarily. The Johnson administration’s decision not to strike China can only be understood in a larger and long-since-forgotten context: an important shift in

U.S. strategy aimed at managing the complex and interconnected issues of global nuclear proliferation, relations with the Soviet Union, the war in Southeast Asia, and the volatile issues surrounding the political and military status of Germany.

Policymakers know that to assume the worst is to foreordain it and that even if efforts to manage the U.S.-China relationship may ultimately fail, they will have a hard time explaining to future generations why they didn't even try.

What is often forgotten in the story is that the same policymakers who eschewed preventive strikes against China in the fall of 1964 made several other related decisions they considered even more momentous. First, they made a bold decision to work with their Cold War adversary, the Soviet Union, to aggressively pursue a global nuclear nonproliferation regime. (9) Most controversially, this policy shift included prohibiting some of our closest allies from acquiring atomic weapons. Many experts both within and outside of government worried this could be a potentially catastrophic mistake. It was foolish, many argued, to think cooperation with the Soviets was possible, nor was it prudent to try to prevent sovereign states, particularly our friends, from possessing their own deterrent. Denying modern weapons to the Federal Republic of Germany, some experts predicted, could lead to a resurgence of nationalism and even militarism, as it had during the interwar period. In the end, U.S. policies to slow the spread of nuclear weapons were quite effective, as there are far fewer nuclear states in the world today than anyone in 1964 predicted. Furthermore, the most alarming forecasts about how countries like West Germany and Japan would react to their nonnuclear status were, fortunately, wildly off the mark.

The fall of 1964 also saw these same policymakers decide to escalate U.S. military efforts in Vietnam. (10) One of the reasons for escalating in Vietnam was demonstrating to nonnuclear countries—Japan, South Korea, Taiwan, Indonesia, Australia, India, and yes, West Germany—that the United States would defend vulnerable nations, even if they were threatened by a nuclear-armed state or its proxy, in this case, China and North Vietnam. (11) As Deputy Assistant Secretary of Defense for International Security Affairs, Henry Rowen, wrote at the time, “A U.S. defeat in Southeast Asia may

come to be attributed in part to the unwillingness of the U.S. to take on North Vietnam supported by a China that now has the bomb.” (12) U.S. State Department Policy Planning Director Walt Rostow argued that the Johnson administration could make “U.S. military power sufficiently relevant to the situation in Southeast Asia,” to eliminate the impulse of states in the region to acquire their own atomic weapons. (13) If the United States abandoned South Vietnam, it was feared, America’s allies might lose faith in our promises to protect them and respond by seeking their own nuclear weapons. (14) A nuclear tipping point that might start with Japan could spread throughout East Asia to include Australia, South Korea and Indonesia. (15) Unchecked, proliferation pressures could move to other regions of the world, and even lead to pressure on West Germany to nuclearize, threatening the stability of Central Europe.

Examined on their own merits, two of the policies—the decision not to launch a preventive strike against China and the decision to cooperate with the Soviet Union to limit the spread of nuclear weapons—might be judged great successes, while the third—the U.S. military escalation in Vietnam—is seen as a disaster. But can they really be examined apart from one another? If Vietnam is understood at least in part as a function of the Johnson administration’s successful efforts to encourage nuclear nonproliferation, seek détente and cooperation with the Soviets, and manage the German question, might the policy make more sense (if being still no less disastrous in its consequences)? And since all three policies were crafted by the same policymakers in the same administration at the same time, doesn’t that reveal the difficulties inherent in assessing U.S. foreign policy? The point here is not to judge any of these decisions, or justify the war in Vietnam (quite the contrary), but only to highlight how misleading it can be to cherry-pick particular policies without a greater understanding of the complex, horizontal connections between seemingly unrelated issues, linkages that are rarely recognized by those outside the world of the top decision-makers.

Consider the question of U.S. deliberations over a nuclear Iran. Certainly there are other, interrelated policies, both in the Middle East and worldwide, that would be enormously influenced by a U.S. decision to strike or not strike. Pundits may examine the issue close at hand, in isolation, while policymakers have to think about how their decisions will reverberate over time and on issues seemingly unrelated to the theocracy in Tehran, such as global energy prices, the war in Afghanistan, the Israeli–Palestinian peace process, North Korea’s nuclear capacity, the strength of the global nuclear nonproliferation regime, the credibility of our existing extended deterrence commitments, relations with China and Russia, and the trajectory of the “Arab Spring,” just to name a few. No assessment of what is the “right” policy toward Iran can be made without acknowledging these complex, uncertain connections, and the near impossibility of predicting how these factors will

interact and unfold in the months and years to come.

The Iran nuclear challenge is but one example of how the academic policy debate often shortchanges the real-world policy problem. Consider, for example, the case of U.S.–China relations. Policymakers would be grateful for useful knowledge as they face an issue of extraordinary complexity and consequence. What does the ivory tower offer? There are, to be sure, useful, fine-grained studies that examine the political, cultural, demographic and economic trends in China. The work that generates the most attention and acclaim, however, is again the soundings of the hedgehogs. Once again, the realists do battle with the liberal internationalists.

At the extreme, the realists argue that the security competition between China and the U.S. is inevitable, regardless of what today's policymakers on either side decide to do. Because these decisions can't bind future generations, the only rational policy for the United States to adopt is to prepare for confrontation. (16) The liberal internationalists, on the other hand, ignore the lessons of the first half of the 20th century and argue that interdependence has made military conflict outdated and unthinkable. Neither side spends much time assessing the implications that contingent, unpredictable events, such as an environmental catastrophe in China or a complete meltdown of the global financial markets, might have on U.S.–China relations, because their "parsimonious" theories tend to exclude all other variables. Policymakers understand, in a way that eludes most experts, that there is no such thing as a "unitary" policy toward China, but a complex "mélange" of choices on critical, interrelated issues including human rights, international financial and monetary policy, climate change, global public health, energy, cyber-related issues, nuclear arms control and the future of international institutions, to say nothing of relations with crucial allies, neutrals and potential adversaries in the region and beyond. A choice on each of these issues influences and alters the calculations on other issues, through a complex, never-ending interactive process. And of course, neither camp pays much attention to domestic political factors shaping policymakers' choices.

In approaching U.S. policy toward China, policymakers do not have the luxury to view the world through the simplistic framework of the academic hedgehog. They know that to assume the worst is to foreordain it, and that even if efforts to manage the relationship may ultimately fail, they will have a hard time explaining to future generations why they didn't even try. Yet they also know that relying upon globalization's beneficent invisible hand renders them hostage to ill fortune, which explains the powerful instinct to hedge. None of this means that the practitioners have the better answers—only that they face different imperatives, and the academic debate, as currently constructed, offers little help in how to navigate the complex, difficult and consequential choices they must make.

A PROPOSAL FOR CHANGE

We suspect that one of the reasons the academic debate is so often unhelpful is because, unlike the situation policymakers face, experts rarely face any consequences if they are wrong. As Tetlock's study revealed, these prognosticators and pundits are rarely held accountable for their errors. On the contrary, scholars' reputations and identity are deeply intertwined with their theoretical bent, which is the *ne plus ultra* for academic respectability in most social science disciplines. Experts have no incentive to demonstrate humility or admit what they do not know, nor are they encouraged to show empathy to decision-makers facing momentous decisions under extraordinary pressure. Indeed, their ability to command the precious geography of the op-ed page usually turns on the ability to make categorical, rather than contingent assertions.

Policymakers and elected officials, on the other hand, are not only lambasted in public if a decision turns out poorly and potentially face the loss of their jobs, they also carry the often-heavy personal burden of responsibility for a failed policy. Understanding the different environments that the expert and the decision-maker operate in—the first where error has little or no consequence, the latter where the political and personal costs of mistakes can be astronomical—is critical to understanding why expert ideas have less influence on decision-making than might be ideal and how to improve the utility of the interaction between the two communities.

The truth is, as every experienced policymaker knows, there are rarely “magic bullets,” or simple solutions when facing radical uncertainty and an unknowable future in a complex international environment. Confidence is unwarranted, overconfidence is dangerous and simple, binary choices elusive. This explains why policymakers often prefer to “muddle through,” buy time or seek a compromise between extreme policy options, if only to decrease the downside risk of any decision. These are, unfortunately, the very positions most likely to draw fire from political experts, especially from the ubiquitous hedgehogs that dominate the digital age. Yet these “second best” policies are often less likely to lead to disaster than the bold but untested recommendations of prominent experts. As Adam Gopnik recently pointed out in his assessment of American criminal justice policies, “Epidemics seldom end with miracle cures.” Oftentimes, “merely chipping away at the problem around the edges” is the very best thing to do; keep chipping away patiently and, eventually, you get to its heart.” (17)

Is there a way that experts could contribute more constructively to policymakers eager for any idea or sets of ideas that can help them make better policy choices? During a recent workshop hosted by the

University of Texas, historians, strategists and current and former statesmen gathered to find answers. (18) One big idea emerged: singular theories, models and historical analogies, in isolation and unchallenged, are of little value to policymakers. But various theories, models and histories taken together and in conversation with each other, and which are tailored to recognize the realities faced by policymakers, could potentially provide quite a bit of insight.

How? Imagine a mixed group of experts and statesman, meeting off-the-record, temporarily suspending their desire to predict, blog or be on television, spending a day or two intensely imagining and debating alternative scenarios that might emerge from a U.S. decision to bomb or not bomb Iran. Experts and policymakers would be forced to surface their assumptions, and test their theories, models and historical analogies against each other's, making an effort to match particular knowledge with specific issues. A somewhat similar effort was, of course, tried once before—President Eisenhower's Solarium exercise—with great success. (19) Imagine a comparable if broader and deeper endeavor, incorporating many of the innovations that have emerged since 1953, including game theory, scenario planning and detailed historical case studies.

How would this exercise be different than several other, worthwhile efforts to, in the words of Alexander George, “bridge the gap” between international relations theorists and foreign policy practitioners? (20) Three core principles, often lacking from these otherwise erstwhile efforts, must be present if the exercise is to succeed.

PRINCIPLE ONE: INTERDISCIPLINARITY

This is, of course, everyone's favorite buzzword inside the academy, but the fact is, few in the ivory tower actually embrace the full meaning and consequence of the concept. Why? The division of higher education into singular disciplines has led to an obsession with methods and “tools”—be it game theory, statistical methods or textual analysis—which is inherently at odds with the practitioners' “problem-focused” interest in exploiting whatever tool or method sheds light on the issue at hand. The idea of problem-driven research and teaching was once an impetus behind the creation of policy schools, but these are often looked down on by the disciplinary priesthood, which works hard to persuade the best and brightest future scholars that their professional future depends on their ability to make a mark through theoretical contributions to an individual discipline, rather than through policy-oriented research or eclectic models of explanation. This is a tremendous waste of intellectual firepower. And within the policy schools themselves, there is still a strong bias toward quantitative methods and modeling whose utility in the international affairs context is marginal. If the best minds could go beyond collaboration to truly multidisciplinary theories—perhaps something like a “unified

field theory”—their work would better mirror, and be of greater use, to policymakers. There are examples of this kind of pathbreaking work being done in the applied sciences, but international relations theorists have, up to now, largely scorned such an approach.

PRINCIPLE TWO: EMBRACE “SECOND BEST” THEORY

Policymakers do not operate in an idealized world where initial conditions can be perfectly specified, and troublesome, unquantifiable variables can be ignored or simplified into “dummy” variables. Decision theory is well and good, but as some of the most innovative scholars have repeatedly shown, decision process is at least as decisive. It is no accident that the best of the scholars are those who have also been involved in practice. And as important as good ideas are at the front end of policy, what practitioners really need are ways of assessing the constant stream of “real-time” evidence to determine whether the policy in question is moving in the right direction or not. In other words, scholars could provide help with “signposts” to analyze whether the underlying assumptions are valid and the policy is on track, and tools to avoid type 1, or false positive, and type 2, false negative, errors when interpreting real-world evidence. The hedgehog tendency toward “crying wolf” or excessive skepticism is of little use, and must be left at the door of any exercise.

PRINCIPLE THREE: A SEAT AT THE TABLE

Academics often ask to be invited into the decision-making process, and we believe that under the conditions we lay out, having scholars involved could be very beneficial. But by the same token, decision-makers have to be allowed into the often-inscrutable world of the ivory tower, and help with the designing of curricula, academic programs and the development of research agendas. Cooperation cannot be a one-way street.

The truth is, as every experienced policymaker knows, there are rarely “magic bullets” or simple solutions when facing radical uncertainty and an unknowable future in a complex international environment.

What would be the payoff of “bridging the gap” exercises that embraced these principles? Not only could novel policy ideas emerge; a rigorous vetting of contrasting, alternative futures would act as a

sort of de facto contingency planning should a particular policy choice eventually turn out to be wrong. Policymakers who had gone through this process, removed from the political pressures and groupthink of the Beltway, might learn in advance what she or he should do if something goes awry, and be more willing to recognize when a policy has gone bad and change course quickly.

Statesmen would not be the only ones to benefit. Such an exercise could sensitize experts to the inherent difficulties, the trade-offs and the unintended consequences of making U.S. foreign policy. This might reduce the shrillness and polarization that often mark such debates over important, contested issues, and make expert knowledge more useful and accessible. The very process of working together in this fashion would potentially do far more to increase the levels of understanding between the “expert” and policy worlds than the many well-intentioned programs out there seeking to “translate” academic work for a policy audience.

If both pundits and policymakers alike acknowledged the impossibility of knowing what the future brings, while being willing to both admit and forgive honest mistakes, it could increase both our humility and our flexibility, leading, perhaps, to better, more effective policies. While such a process may not tell us whether bombing Iran or refraining from doing so is “right,” it will better prepare all concerned for unexpected, unintended and challenging consequences that will surely result, regardless of which policy is chosen. Given the enormous long-term stakes of the choices before our president, it is the least that policymakers and experts can do. ■

1. For a sense of how much is out there, look at the activity after “Time to Attack Iran,” an article by Matthew Kroenig, a Georgetown University professor, was published in *Foreign Affairs*. (See “Time to Attack Iran: Why a Strike Is the Least Bad Option,” *Foreign Affairs*, January/February 2012.) Within weeks, three essays in reply from academics/experts were published in *Foreign Affairs* alone: Alexandre Debs and Nuno P. Monteiro, “The Flawed Logic of Striking Iran;” Colin H. Kahl, “Not Time to Attack Iran;” and Jamie M. Fly and Gary Schmitt, “The Case For Regime Change in Iran.” Kroenig’s article was in many ways a response to an earlier set of *Foreign Affairs* articles. This all highlights the at-times insular, “inside baseball,” nature of these debates. (See Eric S. Edelman, Andrew F. Krepinevich, Jr., and Evan Braden Montgomery, “The Dangers of a Nuclear Iran: The Limits of Containment;” James M. Lindsay and Ray Takeyh, “After Iran Gets the Bomb: Containment and Its Complications,” March/April 2010. The Council on Foreign Relations, which publishes *Foreign Affairs*, published a sample of “expert” suggestions in “Ask the Experts: What Would Iran Do with a Bomb,” by Micah Zenko, February 21, 2012 (<http://blogs.cfr.org/zenko/2012/02/21/ask-the-experts-what-would-iran-do-with-a-bomb/#more-1977>). This is only a sample of what has appeared on the Council of Foreign Relations’ Web site. Peruse similar journals/Web sites, such as *The National Interest* and *Foreign Policy*, and you will find many more expert predictions and recommendations. This does not include items that appeared on opinion pages, in the

“blogosphere” or on news networks.

2. Steve Coll, “[Comment, Table Talk](#),” *The New Yorker*, February 6, 2012.
3. See Philip Tetlock, *Expert Political Judgment: How Good Is It? How Can We Know?* (Princeton, NJ: Princeton University Press, 2005). See also Philip Tetlock and Dan Gardner, “[Why Most Predictions Are So Bad](#),” *Forbes*, March 17, 2011.
4. Isaiah Berlin, *The Hedgehog and the Fox: An Essay on Tolstoy’s View of History* (New York: Simon and Schuster, 1970). In this work, Berlin suggested that foxes [are people] who know many things; hedgehogs know one big thing.
5. For a classic example of how academics frame the policy questions surrounding nuclear proliferation in a binary manner, see Scott Sagan and Kenneth Waltz, *The Spread of Nuclear Weapons: A Debate Renewed*, (New York: Norton, 2002).
6. Using terms like “folly” and “flawed logic” in the title of your article to describe your opponent’s position is hardly conducive to respectful, productive debate. For several examples, see Michael E. Brown, “The Flawed Logic of NATO Expansion,” *Survival* (Spring 1995): pp. 34–52; Karl Heinz Kamp, “The Folly of Rapid NATO Expansion,” *Foreign Policy* (Spring 1995): pp. 116–29; Amos Perlmutter and Ted Galen Carpenter, “NATO’s Expensive Trip East: The Folly of Enlargement,” *Foreign Affairs*, January/February 1998. One piece that is quite fair lays out the different perspectives the major international relations theories frame, explains key issues like the NATO debate and recommends mixing and matching theories when appropriate. It is by Stephen M. Walt, “[International Relations: One World, Many Theories](#),” *Foreign Policy*, Spring 1998, pp. 29–35.
7. John Lewis Gaddis, “History, Grand Strategy, and NATO Enlargement,” *Survival: Global Politics and Strategy*, Volume 40, Issue 1, 1998, p. 145. Of course, diplomatic historians had no monopoly on being wrong about NATO. John Mearsheimer—whose prestige among political scientists equals that of Gaddis among historians—suggested, among other things, that giving a unified Germany nuclear weapons was the best way to minimize the instability, conflict and danger of great-power war in Europe that would inevitably ensue at the end of the Cold War. See John Mearsheimer, “Back to the Future: Instability in Europe after the Cold War,” *International Security*, Summer 1990, Vol. 15, No. 1.
8. See Ernest May, “Lessons” of the Past: *The Use and Misuse of History in American Foreign Policy* (New York: Oxford University Press); also Richard Neustadt and Ernest May, *Thinking in Time: The Uses of History for Decision Makers* (New York, Free Press, 1998).
9. Francis J. Gavin, “Blasts from the Past: Nuclear Proliferation and Rogue States Before the Bush Doctrine,” *International Security*, Winter 2005, pp. 100–135
- o. For the best account of how this policy developed, see Frederick Logevall, *Choosing War: The Lost Chance for Peace and the Escalation of War in Vietnam* (Berkeley: University of California Press, 2001).
1. Report by the Committee on Nuclear Proliferation, January 21, 1965, Johnson Library, National Security File, Committee File, Committee on Nuclear Proliferation, Report (Final, 12/21/65), Box 8.

2. Henry Rowen, "Effects of the Chinese Bomb on Nuclear Spread," November 2, 1964, National Security Files, Committee on Nonproliferation, Box 5, LBJL.
3. Memo, Rostow to Rusk, "Nuclear Proliferation and the Crises in South-east Asia and the Atlantic," November 4, 1964, RG 59 Department of State, Records of Policy Planning Council, NARA, p. 1.
4. This argument is made by Matthew Jones in his book, *After Hiroshima: The United States, Race, and Nuclear Weapons in Asia, 1945-1965* (Cambridge, UK: Cambridge University Press, 2012), pp. 401–450.
5. Report by the Committee on Nuclear Proliferation, *supra* at 12.
6. For an example of this view, see Robert D. Kaplan, "[Why John J. Mearsheimer Is Right \(About Some Things\)](#)," *The Atlantic*, January/February 2012.
7. Adam Gopnik, "[The Caging of America](#)," *The New Yorker*, January 30, 2012.
8. See <http://www.strausscenter.org/strauss-news/university-of-texas-inaugurates-new-multidisciplinary-program-in-history-strategy-and-statecraft.html>.
9. For accounts of the Solarium exercise, see Robert R. Bowie and Richard H. Immerman, *Waging Peace: How Eisenhower Shaped an Enduring Cold War Strategy* (New York: Oxford University Press, 2000); and Marc Trachtenbeg, "A 'Wasting Asset': American Strategy and the Shifting Nuclear Balance, 1949–1954," in *History and Strategy* (Princeton, NJ: Princeton University Press, 1991).
- o. Alexander L. George, *Bridging the Gap: Theory & Practice in Foreign Policy*

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