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Introduction by Peter D. Feaver, Duke University

The scholarly study of American foreign policy and international relations has one (at least one?) important peculiarity that distinguishes it from many other forms of scholarly inquiry: a fairly high degree of intellectual exchange between social scientist and subject. Of course, zoologists interact with the animals they study, but the animals are not reading what the zoologists say about them and reacting accordingly. Nor do children tend to read child psychology texts, or the indigent subjects of socio-economic inequality keep up with political economy treatises.

Yet practitioners of American foreign policy do have a certain degree of familiarity with what scholars say about their craft. At a minimum, they learn basic political science concepts in college and graduate school. Every foreign policy practitioner I have ever met can deliver a short riff on 'Realism' and perhaps an associated diatribe about the limits of 'theory.' Moreover, a goodly number of scholars of foreign policy and international relations have spent time actually working within the foreign policy and national security establishment and some have gone back and forth more than once.

All of this has fed a lively conversation about the so-called 'gap' between practitioners and scholars – whether policymakers are adequately taking advantage of insights generated by scholars, whether scholars are adequately providing insights of value to practitioners, and whether each side properly understands the other.

Which brings me to the subject of this roundtable: *Fighting for Credibility: US Reputation and International Politics*, co-authored by Frank P. Harvey and John Mitton. They take as their subject one of the hoary scholar-practitioner gaps of understanding between the two sides: Does credibility matter in American foreign policy? And they provide a provocative answer: Yes, much more so than one prominent school of academic scholars led by Daryl Press, Jonathan Mercer, and Ted Hopf has claimed. (Harvey and Mitton call this school the "PMH" school.)

The Harvey-Mitton argument is provocative, but only to scholars. Virtually every policy practitioner would have accepted as obvious a claim that a reputation for credibility–a reputation that when you make a threat you are likely to carry it through, or when you make a claim it is likely to be true, or when you make a deal you are likely to honor it–is an important ingredient in statecraft. No policymaker would say that this ingredient eclipses all others. No policymaker would have claimed that reputation (or credibility or prestige) is so valuable and powerful that it is worth preserving at any price.

But nor would policymakers have found very plausible the line of reasoning that led the PMH school to discount the utility and importance of reputation altogether on the grounds that policymakers decide each new crisis solely on the basis of calculations of material interest and other issue-specific considerations unfettered by a reputation based on past behavior. On the contrary, policy practitioners would have said that such reputations were important factors (though hardly the only important factors) that shaped the decision calculus–perhaps powerfully shaped the calculus when the conditions were right–and this justified incurring some costs to bolster reputations for effective action in coercive diplomacy. It was precisely this common understanding of policy practitioners that the PMH attacked–and it was precisely their contrary-to-the-conventional-wisdom claims that made their arguments so prominent and quotable. The policy practitioners do not know what they are doing, the PMH school claimed; whereas the policy practitioners, to the extent they were aware of the PMH school argument, would have answered back, 'no, it is you who are mistaken.'

Into this gap step Harvey-Mitton, who argue that reputations are indeed worth fighting for and that the policymakers were right to reject or ignore the arguments of the PMH school. They make this argument with a detailed study of U.S. coercive diplomacy in the post-Cold War era, focusing especially on President Barack Obama's policy vis-à-vis Syria.

The four reviewers in this roundtable have all made important scholarly contributions to the reputation debate themselves, and so they are well-positioned to evaluate the Harvey-Mitton argument. By and large, each of them reaches the same tri-partite assessment: (1) Harvey and Mitton have written an important book that will be a go-to resource for any future contributor to the reputation debate; (2) the book makes a convincing case that the PMH school has overstepped the evidence and that a more nuanced argument that assigns greater importance to reputation better fits the empirical evidence; yet (3) Harvey and Mitton themselves leave some important questions unaddressed or at least unresolved, and so the book will not be the last word on the subject.

Rebecca Lissner praises Harvey and Mitton for addressing an important question and offering a nuanced answer that might disappoint "zealots" who want a more simplistic narrative. She critiques Harvey and Mitton, however, for failing to exploit sufficiently the new archival evidence that shows in greater detail how Iraqi President Saddam Hussein actually made inferences (or, at the very least, how the archives indicate that he made inferences) about reputation in the very crises they examine. She likewise calls on Harvey and Mitton to provide more evidence to support their claims of how Syrian President Bashar al-Assad and Russian President Vladimir Putin interpreted the U.S. post-Cold War record in coercive diplomacy.

Danielle Lupton assesses that Harvey and Mitton have made a "significant contribution" to the lively IR debate over reputation with a "straightforward and accessible" argument. Lupton raises concerns, however, about how Harvey and Mitton rely on outcomes to measure the operations of reputation when a more detailed path-tracing is needed to support a causal inference in qualitative case studies.

Rupal Mehta praises Harvey and Mitton for their critique of the PMH school, but flags some weaknesses in their own argument that will require further research to resolve. Her primary critique refers to their simplifying assumption that brackets off third-party actors from the dynamic. Mehta argues that much of the work of reputation is done in the transference of third-party inferences from case to case and so flags this for further work. She also calls for greater attention to the leader-, versus the state-, level of analysis.

Keren Yarhi-Milo lauds the book for its comprehensive literature review and its extension of the empirical domain of reputation studies into the age of unipolarity during the post-Cold War era. However, she claims that Harvey and Mitton rely on a unitary rational-actor assumption that does not work well when the causal mechanism involves the perceptions of leaders. She also argues that their theory is underspecified, and shows how different interpretations of their own evidence would be consistent with the overall thrust of their argument, but require some revision to their theory.

Adding my two cents, I challenged Harvey and Mitton in correspondence related to this roundtable to do a better job justifying their coding of the 2013 Syrian WMD red-line episode as a "major coercive diplomatic success" (5) by the Obama Administration. The Harvey-Mitton critique of PMH and the utility of their alternative theory does not hinge on them coding the success or failure of the Syrian case correctly, which is good for their argument because I pointed out that the Syrian episode was better coded as a case of successful coercive diplomacy *by* the Assad regime *of* the Obama Administration, not vice-versa, as they claim. In

exchange for surrendering part of his declared chemical arsenal, Assad got the Obama Administration to forgo enforcement of the chemical weapons-use redline and also implicitly to abandon the other 'regime change' redline of 'Assad must go.' Obama gained the reputation of bluffing, and, since the Russians were brought in to broker the deal, gained the further reputation of weakly defending the long-time American foreign policy goal of preventing Russian/Soviet meddling in Middle Eastern geostrategy. This reputation came back to haunt the Administration when Syria once again defied outside powers by again using chemical weapons against the rebels – and set up the Administration of President Donald Trump to claim a coercive diplomacy success when he finally enforced the redline in 2017.

In their response, Harvey and Mitton welcome the mostly positive reviews, and offer some explanations for how they would handle the substantive critiques offered by the reviewers. Their response begins with something like a declaration of victory over the PMH school and ends with a call for further work along the robust research agenda that no longer tries to defend the extreme claim that reputation does not matter, but instead focuses on how and why it matters.

Participants:

Frank P. Harvey is Dean of the Faculty of Arts and Social Sciences at Dalhousie University and former Chair of the Department of Political Science. He currently holds the Eric Dennis Chair of Government and Politics at Dalhousie and will be taking up the Fulbright Visiting Research Chair at Yale University in January 2018. His book *Explaining the Iraq War: Counterfactual Theory, Logic and Evidence* (Cambridge University Press 2012), received the 2013 Canadian Political Science Association Book Prize in International Relations. He has published on post-9/11 security, the Iraq war, American foreign and security policy, nuclear and conventional deterrence, coercive diplomacy, proliferation, crisis decision-making, protracted ethnic conflict and ballistic missile defence in *International Studies Quarterly*, *Journal of Conflict Resolution*, *Journal of Politics*, *Security Studies*, *International Political Science Review*, *Canadian Journal of Political Science*, and *Conflict Management and Peace Science*. Professor Harvey received Dalhousie's Alumni Association Award for Excellence in Teaching (2012), Dalhousie's Outstanding Graduate Advisor Award (2009), the Faculty of Arts and Social Sciences Award for Excellence in Teaching (1998), and the Burgess Research Award (2000).

John Mitton is a Ph.D. Candidate in the Department of Political Science at Dalhousie University and a Lecturer in the Department of Political Science at the Memorial University of Newfoundland. In 2016/17, he was a Visiting Fulbright Scholar in the School of International Relations at the University of Southern California. He has published on international security, international rivalry, American foreign policy, and coercive diplomacy in *Canadian Journal of Political Science*, *Contemporary Security Policy*, *Canadian Foreign Policy Journal*, *Journal of Military and Strategic Studies*, and *International Journal*. His article, "Selling Schelling Short: Reputations and American Coercive Diplomacy after Syria," *Journal of Contemporary Security Policy* 36:3 (2015): 408-431, won the 2016 Bernard Brodie Prize.

Peter D. Feaver is Professor of Political Science and Public Policy at Duke University, where he directs the Triangle Institute for Security Studies and the Duke Program in American Grand Strategy.

Rebecca Friedman Lissner is a Stanton Nuclear Security Postdoctoral Fellow at the Council on Foreign Relations. Previously, she was a Brady-Johnson Predoctoral Fellow at Yale University's International Security Studies program and a Smith Richardson Foundation World Politics and Statecraft Fellow. Rebecca's research interests focus on international security and American foreign policy, and she is working on a book (based on

her dissertation) that examines how lessons learned from military interventions have shaped U.S. grand strategy since World War II. Rebecca's scholarship on national security decision-making during presidential transitions and conflict early warning systems has been published in Presidential Studies Quarterly and International Peacekeeping. Her policy writing has appeared in Foreign Affairs, Foreign Policy, The Atlantic, and War on the Rocks, among other publications. Rebecca received an AB in Social Studies from Harvard University and a MA and PhD in Government from Georgetown University. In the 2017-2018 academic year she will be a Postdoctoral Fellow at the University of Pennsylvania's Perry World House.

Danielle L. Lupton is an Assistant Professor of Political Science at Colgate University. She received her Ph.D. in political science from Duke University in 2014. Her research and teaching interests focus on international security, American foreign policy, and elite decision-making. Her book project, entitled *Leaders, Perceptions, and Reputations for Resolve*, examines how new leaders establish reputations for resolute and irresolute action through their statements and behavior. Her research is published or forthcoming in *International Interactions* and *Political Research Quarterly*.

Rupal N. Mehta is an Assistant Professor in the Department of Political Science at the University of Nebraska-Lincoln. Her research interests lie in international security and conflict, with a specialization in nuclear nonproliferation/counter-proliferation, extended deterrence, nuclear latency, force structure, and deterrence and coercion strategy. Her book, *The Politics of Nuclear Reversa*l (under consideration), explores the conditions under which states that have started nuclear weapons programs stop their pursuit. Her work has appeared or is forthcoming in the *Journal of Conflict Resolution*, *International Studies Quarterly*, *The Washington Quarterly* and her commentary has been published in War on the Rocks, International Studies Quarterly, and the Washington Post's Monkey Cage. Her research has received support from the Institute for Global Conflict and Cooperation (the UC system), the Stanton Foundation, USSTRATCOM, the Defense Threat Reduction Agency, the Senning Foundation, and the University of California, San Diego. Previously, she was Stanton Nuclear Security Postdoctoral Fellow in the Belfer Center's International Security Program and Project on Managing the Atom.

Keren Yarhi-Milo is an Assistant Professor of Politics and International Affairs at Princeton University. She is the author of *Knowing the Adversary: Leaders, Intelligence and Assessment of Intentions in International Relations* (Princeton University Press, 2014). Her new book, titled "Who Fights for Reputation? The Psychology of Leaders in International Conflict" is forthcoming (Princeton University Press 2018).

Review by Rebecca Friedman Lissner, Council on Foreign Relations

Do reputations matter in international politics? In Fighting for Credibility, Frank P. Harvey and John Mitton address this important question–a question with significant implications for the theory and practice of U.S. foreign policy. Their answer will disappoint zealots on both sides of the credibility debate. The authors conclude that reputation is neither universally relevant nor irrelevant for crisis bargaining. Instead, states are most likely to use past actions to calculate the credibility of a coercive threat when their adversaries have previously attempted coercion under similar circumstances. Harvey and Mitton develop this argument in a study of American post-Cold War "deterrence encounters" in Bosnia-Herzegovina (1992-1995), Kosovo (1998-1999), and Iraq (1991-2003), then explore the strategic logic of coercion in greatest depth with a case study of President Barack Obama's infamous "red line" with regard to Syrian chemical weapons (2011-2013). Bucking conventional wisdom, they contend that Syrian President Bashar al-Assad's agreement–brokered by Russia–to surrender its chemical weapons stockpiles represented a "major coercive diplomatic success" for the Obama Administration (5). Taken as a whole, Fighting for Credibility is a worthwhile and provocative intervention into the debate about reputation in international politics: by renewing a debate that some view as settled, this book will inspire scholars and policymakers to rethink their assumptions about credibility.

Harvey and Mitton's theoretical contributions build on the insight that calculating an adversary's credibility is a decidedly difficult undertaking. This observation is central to the authors' critique of what they dub the "P-M-H" consensus – that is, the work of Daryl Press, Jonathan Mercer, and Ted Hopf, all of whom are dismissive of the role played by generic reputation in establishing the credibility of discrete threats.[1] In their first chapter Harvey and Mitton bemoan the "premature closure of inquiry" into reputational research, as the P-M-H wave of scholarship seemed to sound the death knell of rational deterrence theory (32). Although the Mercer and Hopf contributions came first, Press's parsimony and definitive conclusions have positioned his work at the forefront of the debate, particularly in policy circles. In Calculating Credibility, Press finds no evidence that German, American, and British policymakers considered their adversaries' reputations when assessing the credibility of military threats during major mid-twentieth century crises. Consequently, he contends a "current calculus" of power and interests–rather than "past actions"–determines credibility.[2]

A major contribution of Harvey and Mitton's book is its exposition of how Press's past actions versus current calculus framework has distorted the reputation debate. By using the term "past actions" as a proxy for reputation, Press obscures the ways in which a state's prior behavior may "cause credibility" for reasons unrelated to reputation.[3] The appropriate question is not only if past actions matter for deterrence and compellence but also why they matter. Are past actions consequential because of the enduring reputational effects of bluffing versus following through on threats–or because adversaries are learning about underlying

[1] Daryl Grayson Press, *Calculating Credibility: How Leaders Assess Military Threats* (Ithaca: Cornell University Press, 2005); Jonathan Mercer, *Reputation and International Politics*, Cornell Studies in Security Affairs (Ithaca: Cornell University Press, 2010); Ted Hopf, *Peripheral Visions: Deterrence Theory and American Foreign Policy in the Third World, 1965-1990* (Ann Arbor: University of Michigan Press, 1994).

[2] Press, 20, 37.

[3] Press, 16.

interests and capabilities? Harvey and Mitton make a strong case in favor of the importance of past actions in establishing the credibility of coercive threats–but the authors come up short when they attempt to show that past actions matter *because* of reputational effects.

Indeed, Fighting for Credibility is at its best when it deconstructs the apparently clean and self-evident nature of the credibility formula Press suggests in Calculating Credibility. While accurately assessing capabilities and interests may be straightforward ex post facto as a matter of scholarship, states subject to coercive threats often lack adequate information to judge these factors. Interests are particularly tricky, as the book documents in several cases of deterrence encounters between the United States and substantially weaker states. In each case, there was little doubt that the U.S. military possessed the capabilities required to execute on its threats – but the extent of Washington's interests was much more difficult for adversaries to discern.

Any crisis involving the United States is apt to engage many overlapping interests, both international and domestic. As Harvey and Mitton point out, in the case of U.S. involvement in the Syrian civil war, plausible American interests ranged from moral or humanitarian concerns, to the desire to forestall spillover effects on allies and partners in the Middle East and Europe, to the need to counterbalance Iranian and Russian influence. Once the Assad regime conducted its chemical weapons attack at Ghouta, the United States had a further interest in enforcing the norm against the use of chemical weapons. Despite these myriad interests, however, the domestic-political climate in the United States was averse to protracted engagement in an internecine struggle in Syria–particularly after recent experiences in Iraq and Afghanistan–and many in Washington questioned whether any of the aforementioned interests warranted the risk of a costly, long-term commitment. Given such a complex landscape, how should the Assad regime and its Russian patrons have assessed the credibility of American threats to enforce the red line against chemical weapons use?

This question leads to a more fundamental but oft-overlooked point: states struggle to accurately assess their own power and interests. IR theorists in the rationalist tradition generally assume that states hold this information privately, and have incentives to misrepresent it to the wider world.[4] My own research demonstrates that such certainly rarely obtains when it comes to matters of national security.[5] Consider three of the United States' most consequential post-World War II military interventions: the Korean War, Vietnam War, and First Gulf War. The ultimate degree of American involvement in each instance could not have been predicted based on Washington's prevailing pre-war assumptions about the United States' own capabilities and interests.

In the case of Korea, prior to the North Korean invasion of South Korea, the Truman Administration had defined its interests in the Peninsula fairly minimally. While Washington saw value in strengthening the South Korean government's ability to resist communist encroachment through economic and military aid, American war plans deemphasized Korea's importance in its global strategic concept and, in 1949, the United

[4] James D. Fearon, "Rationalist Explanations for War," *International Organization* 49:3 (1995): 379-414.

[5] Rebecca Lissner, "Grand Strategic Crucibles: The Lasting Effects of Military Intervention on State Strategy," (Ph.D. Dissertation, Georgetown University, September 2016).

States withdrew its remaining forces, leaving only a military advisory group. This context animated Secretary of State Dean Acheson's infamous placement of the Korean Peninsula outside the U.S. defensive perimeter.[6]

In Vietnam, U.S. military power was constrained in unexpected ways due to limited domestic-political will behind the war effort. When President Lyndon Johnson authorized a coercive air campaign, Operation Rolling Thunder, in February 1965, early polling demonstrated popular support, with the Washington Post reporting a 67 percent approval rate for U.S. airstrikes and 64 percent of respondents supporting continued U.S. efforts in Vietnam.[7] Congress also expressed its support for Johnson's escalating involvement–the Gulf of Tonkin Resolution passed 512-2, and supplemental appropriations for Vietnam passed both houses of Congress with only 10 combined votes against. Indeed, dissent did not enter the mainstream of American political opinion until early 1968, after the Tet Offensive. The political pressure that prompted Johnson's March 1968 decision to begin retrenching from the Vietnam conflict, and to refrain from seeking reelection, ultimately limited the expression of U.S. military capabilities–but this constraint on the exercise of American power was not obvious when Johnson increased involvement in 1965.

Beyond political will, of course, national power is also a function of military capabilities. The First Gulf War demonstrates how states can miscalculate their own military power. When the U.S. military planned Operation Desert Storm, it expected a tough fight; once the war began, however, the performance of U.S. forces far exceeded the George H.W. Bush Administration's expectations. During the air campaign, planes from the U.S.-led coalition destroyed 41 Iraqi aircraft in air-to-air combat without suffering a single confirmed loss. Flying more than 100,000 sorties that successively destroyed the Iraqi air defenses, command and control targets, and prepared the battlefield for ground combat, the Coalition lost 38 fixed-wing aircraft.[8] Once the ground war began, "Coalition armored forces traveled over 250 miles in 100 hours, one of the fastest movements of armored forces in the history of combat, to execute the now famous 'left hook' that enveloped Iraq's elite, specially trained and equipped Republican Guards."[9] Although the hollowness of the Iraqi military was certainly an important factor, the Gulf War demonstrated the battlefield potential of a new generation of military technology. The result was an exercise of American military power that far exceeded the Bush Administration's own expectations.[10]

Since it is very difficult for countries, including the United States, to assess their own power and interests, it certainly follows that the targets of American coercion would not see Press's formula for calculating credibility as straightforward. Any theory of coercion must therefore take incomplete information into account. In *Fighting for Credibility*, Harvey and Mitton contend that the poor quality of information available during 'crisis bargaining encounters' leads states to look to their adversary's past actions for additional credibility

[6] Lissner, chapter. 2.

[7] Lissner, chapter. 3.

[8] U.S. Department of Defense, *Conduct of the Persian Gulf War: Final Report to Congress* (U.S. Department of Defense, 1992), xiv.

[9] *Ibid.*

[10] Lissner, chap. 4.

signals. While prior behavior does not always contain relevant clues, past actions will matter more when they resemble present circumstances. The contention that poor information will motivate states to seek additional context for credibility calculations makes good theoretical sense and is supported by the book's empirics. Moreover, the finding that past actions can inform credibility calculations represents a serous contribution to scholars' understanding of the dynamics of coercive bargaining.

Harvey and Mitton push the argument too far, however, when they argue that states look to their adversaries' past actions *because* they are searching for reputational signals (as opposed to higher-fidelity information about capabilities and interests). The authors claim that, across similar strategic situations, states accrue transferrable reputational characteristics; these reputational factors then inform an adversary's assessment of the state's resolve, which figures alongside power and interest in credibility calculations. Although plausible, the evidence supporting this element of Harvey and Mitton's theory comes up a bit short. Each of the case studies makes strong assertions regarding the mechanisms by which U.S. adversaries calculated American credibility in high-stakes deterrence encounters. Yet the book would be stronger if it provided better documentation of the decision-making of Bosnian Serb leader Ratko Mladić, Serbian President Slobodan Milošević, Iraqi dictator Saddam Hussein, and their associates. In the case of Hussein, some such information is readily available through captured Iraqi records.[11] This evidentiary challenge is most acute in the Syria case, which is the empirical core of the book. Harvey and Mitton repeatedly make assertions about judgments made by the Syrian and Russian leaders based on projection rather than documentation, writing, for example, "both Bashar al-Assad and [Russian President Vladimir Putin concluded…;" and "Assad and Putin expected as much…" (176; 186).

One passage, which hints at the shortcomings of this approach, is illustrative. "It is difficult to be definitive about how Assad and Putin processed information about US domestic politics," the authors admit, "But it is not unreasonable to argue that the dominant account for the Iraq War (notwithstanding its fundamental weaknesses as a plausible theory) would inform an adversary's calculations under similar circumstances"(188-189). To reach this conclusion, they caricature the conventional wisdom about the 2003 Iraq War as: "All the blame for the onset of hostilities should be placed on a small group of neoconservatives who managed to push the country into a war of choice–democratic institutions essentially failed to check the abuse of power in this case" (188). Although Harvey and Mitton think such an interpretation is wrong–Harvey even wrote a book critiquing this take on the war–they nevertheless assert, with no evidence, that Assad and Putin made decisions on the basis of this narrative.[12] Of course, Harvey and Mitton may well be correct in their assessment of how and why U.S. adversaries arrived at their responses to American coercion attempts–but more empirical work is needed to thoroughly test the central reputational mechanism they propose. This task calls for a research design that disaggregates the content of new information gleaned from past actions, to isolate the independent causal effect of learning about reputation–that is, the propensity for a state/leader to adhere to its word–as opposed to learning about capabilities and interests.

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[11] Hal Brands and David Palkki, "'Conspiring Bastards': Saddam Hussein's Strategic View of the United States*," *Diplomatic History* 36:3 (2012): 625-659.

[12] Frank Harvey, *Explaining the Iraq War: Counterfactual Theory, Logic and Evidence* (Cambridge and New York: Cambridge University Press, 2011).

Insofar as *Fighting for Credibility* is a plea for nuance in the debate about credibility in international politics, it succeeds in its mission. Contrary to the claims of those who entirely dismiss the importance of past actions, Harvey and Mitton show how prior behavior can reinforce or undermine the credibility of threats. Moreover, the authors deserve great credit for their meticulous documentation of the Syria 'red-line' debate–which highlights several enduring oddities in the way scholars and policymakers discuss credibility. Most notable is the chasm separating the conventional wisdom in Washington (in the words of Vice President Joe Biden, "big nations don't bluff") from that of the Ivory Tower (bluffing has no lasting costs).[13] Harvey and Mitton document this polarity well, particularly in the context of intense debate over the use of military force in response to Syria's violation of Obama's chemical-weapons red line. That said, a surprisingly quiet voice in this book is that of Obama himself, who came out swinging against the Beltway credibility fetish in his April 2016 interview with *The Atlantic*.[14] (As an aside, I cannot help but wonder how the authors would judge President Donald Trump's April 2017 decision to lob fifty-nine cruise missile into Syria in response to the Assad regime's chemical attack on Khan Sheikhoun–as well as the fawning praise that decision received from bipartisan elites.)

Finally, *Fighting for Credibility* points to, without explicitly discussing, another chasm: that which separates qualitative and quantitative scholars of Security Studies. Harvey and Mitton are not alone in calling into question the resolute skepticism of the P-M-H school when it comes to reputational effects–a number of recent quantitative studies have reached the same conclusion.[15] Yet, the Syria debate shows little evidence that these new findings have influenced the thinking of prominent international affairs scholars. Although consensus may well prove elusive when it comes to the role of reputation in international politics, *Fighting for Credibility* is an important reminder that intellectual silos impoverish both the theory and practice of foreign policy.

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[13] Jeffrey Goldberg, "The Obama Doctrine," *The Atlantic*, 2016.

[14] Ibid.

[15] See, for example, Alex Weisiger and Keren Yarhi-Milo, "Revisiting Reputation: How Past Actions Matter in International Politics," *International Organization* 69:2 (2015): 473-495; Allan Dafoe, Jonathan Renshon, and Paul Huth, "Reputation and Status as Motives for War," *Annual Review of Political Science* 17 (2014): 371-393.

Review by Danielle L. Lupton, Colgate University

Frank Harvey and John Mitton's *Fighting for Credibility: US Reputation and International Politics* is a much-needed book about the role reputations play in international crises. Harvey and Mitton challenge the established and oft-cited argument that reputations do not matter in international politics. Instead, they show that a state's past actions can impact how adversaries assess the resolve and credibility of that state in the future. In doing so, their study makes a significant contribution to the thriving international relations literature on reputations for resolve and credibility.

Harvey and Mitton's argument is straightforward and accessible. They argue, contrary to scholars such as Daryl Press, Jonathan Mercer, and Ted Hopf,[1] that reputations can indeed be worth fighting for. To explain this, they present a version of Rational Deterrence Theory that stresses the importance of past actions to foreign policy decision-making. They argue that credibility is based on four components: communication of intentions, strategic interest, military capability, and resolve. They then explain that reputations are perceptional in that they are in the "eye of the beholder" (104). This component of their theory, therefore, suggests that adversaries have incomplete information about the requirements of credibility and can misperceive a state's actual resolve and intentions. In other words, reputations can form, but this does not mean these reputations will always be accurate. Finally, because reputations do exist and because there are similarities between cases, the authors posit that reputations can be transferrable across crises. Harvey and Mitton then test their arguments across four recent cases of deterrence and compellence involving the United States, including Bosnia-Herzegovina (1992-1995), Kosovo (1998-1999), Iraq (1991-2003), and Syria (2011-2013).

One of the most important—if not the central—contributions of Harvey and Mitton's study is its debunking of the supposed "consensus" that reputations are not important or influential. In the first two chapters of the book, the authors demonstrate that skeptics of reputation have succumbed to a "premature closure of inquiry" (30). They show that these skeptics have largely ignored other research that contradicts the argument that reputations are irrelevant or do not form. The authors' systematic presentation of evidence contradicting this "consensus" is well laid out, and their arguments are thoroughly supported and compelling. Furthermore, at the end of the book, Harvey and Mitton show how dangerous such skepticism can be for policymakers if, as the authors demonstrate, reputations can form and impact crisis decision-making.

This particular contribution is also incredibly timely. For researchers of international relations, it serves to bolster recent scholarly work about the formation and impact of reputations for resolve and credibility that has emerged since the publication of Harvey and Mitton's book last year.[2] For policymakers, it explains how actions in one crisis can constrain or, alternatively, further a state's opportunities to establish resolve and

[1] Daryl Press, *Calculating Credibility: How Leaders Assess Military Threats* (Ithaca: Cornell University Press, 2005); Jonathan Mercer, *Reputation in International Politics* (Ithaca: Cornell University Press, 1996); Ted Hopf. *Peripheral Visions: Deterrence Theory and American Foreign Policy* (Ann Arbor: University of Michigan Press, 1994).

[2] For examples, see Danielle Lupton, "Signaling Resolve: Leaders, Reputations, and the importance of Early Interactions," *International Interactions* (forthcoming); Roseanne McManus, "The Impact of Context on the Ability of Leaders to Signal Resolve," 43:3 (2017): 453-479; and Joshua Kertzer. *Resolve in International Politics* (Princeton: Princeton University Press, 2016).

credibility in another crisis. Reputation for resolve also remains a hot-topic in the current policy climate. In particular, there has been much debate about the tools the Trump administration has used to try to demonstrate its resolve, especially regarding Syria and North Korea.[3]

The final chapter of the book has the potential to be particularly instrumental to the U.S. policy community. One of the key implications of Harvey and Mitton's argument is that policymakers around the globe must recognize that their interactions in one crisis can impact their credibility in other crises, even if that is not their intention. This study, therefore, serves as both a cautionary tale—if leaders signal they lack resolve in one context—and an opportunity to establish credibility—if leaders act resolutely. Furthermore, the authors warn against applying scholarly findings to help bridge the gap between academia and policymakers without first being "cognizant of the potential for confirmation bias to influence the particular interpretations they [scholars] privilege when providing advice to policymakers during a military-security crisis" (264).

One of the other primary strengths of the book is its recognition of a middle ground in terms of the impact of reputation. The authors note, early on, that their goal is not to show that "past actions alone determine an adversary's behavior and/or crisis outcomes" (107). Rather, they instead demonstrate that past actions, and thereby reputations, *can* impact foreign policy and crisis decision-making. This acknowledgement that reputations may not always impact international events is an important proposition of the book. This approach is also highly beneficial to the examination of the impact of reputation, as it allows for a nuanced analysis of the historical evidence presented in the case studies. Indeed, where much of the past literature on reputation has focused on whether or not reputations matter, Harvey and Mitton's study pushes this literature forward by furthering our understanding of when and under what conditions reputations will be influential. Thus, their book has the opportunity to mark a key turning point in how we discuss and think about the conditionality of reputations in international politics.

For example, Harvey and Mitton do not argue that reputations for resolve and credibility are always or never transferrable to different cases. Rather, they focus on the conditions under which perceptions of credibility will be applicable across crises in different contexts. In this regard, the authors argue that reputations are transferrable when cases are similar, and their primary argument regarding similarity appears to focus on cases of asymmetric conflict between the U.S. and other states. This is one area of the book that would benefit from both more specificity and more explanation. Most of the conflicts and crises the U.S. has been involved in since the end of the Cold War have included states that are significantly materially weaker. Thus, power asymmetries alone would not seem to be an adequate condition for the transferability of reputation, at least in the post-Cold War or post-9/11 era. In particular, a more through discussion of how the issue under dispute, American interests, or other factors contribute to the transferability of reputation across crises would help to clarify and bolster the authors' arguments. Indeed, these factors are hinted at in the case studies, particularly regarding the transferability of perceptions of America's resolve and credibility in Syria to Iran and North Korea; however, a more detailed analytical framework would further heighten the already significant contributions of the work on this point. In particular, this may be an area of future research that the authors

[3] Max Fischer, "Do U.S. Strikes Send a 'Message' to Rivals? There's No Evidence," *New York Times*, 21 April 2017; and Danielle Lupton, Roseanne McManus, Keren Yarhi-Milo, "Trump is trying to send a message to North Korea. He's got a long way to go," *Washington Post* Monkey Cage (blog), 26 April 2017.

or other scholars should investigate further, especially as Harvey and Mitton make a compelling argument regarding the relevance of reputation in one context to other crises.

In their extended discussion of the conditions under which reputations impact crises, Harvey and Mitton focus on the distinction between general reputations–which are more long-term reputations regarding a state's foreign policy–and specific reputations–which emerge more narrowly from individual crises. In the case studies, the authors show that both general and specific reputations can impact perceptions of America's resolve and credibility during international crises. This distinction and these findings also leave the reader with two important questions: How do these general and specific reputations interact to inform adversaries' perceptions during crises? And under what conditions will adversaries rely on general versus specific reputations? These questions are not meant as an implicit criticism of the work. Rather, I recognize the fact that a full examination of these questions is beyond the scope of the book. Thus, Harvey and Mitton's theory and findings provide important avenues of future research to further understand how these types or categorizations of reputation impact crisis decision-making and foreign policy, especially beyond the context of the United States.

Regarding the book's methodology and case selection, I applaud the authors for focusing on more recent case studies. Yet, I would have welcomed a more detailed analysis of how adversaries viewed the resolve and credibility of the U.S. in each case study. Harvey and Mitton argue that the outcome of a crisis is "the best point of departure for understanding the adversaries' perception of US resolve and credibility" (187). However, many factors beyond resolve, credibility, or reputation can impact crisis outcome. In contrast, others have argued it is critical to first demonstrate that a specific reputation formed before showing how it contributed to the outcome of a particular crisis.[4] Thus, I have reservations regarding the argument that examining the outcome of events serves as the best indication of the formation or impact of reputational assessments. The assumption that outcome is always a good measure of reputation may oversimplify the causal relationship between reputation and crisis outcome and may muddy our conclusions about when and how reputations influence international events.

At the same time that I offer this critique, however, I also recognize that there is often a trade-off between data richness and the applicability of case studies to contemporary political events. It takes decades for policy documents to be declassified, and many states keep poor records of their decision-making. Thus, direct evidence regarding Syrian President Bashar al-Assad or Russian President Vladimir Putin's direct views of American resolve and credibility, for example, may simply not yet be available or may be difficult to come by even in future years. It is also important to note that the logic of each case study is well supported, and the authors provide a thoughtful discussion and rebuttal of critiques and alternative arguments throughout the book. Yet, I found myself wanting more evidence to support the conclusion made in each case study regarding the specific reputation that formed (i.e., a reputation for having or lacking resolve) and how that reputation impacted perceptions of credibility as well as crisis outcome.

This being said, Harvey and Mitton's work offers an insightful and engrossing look at the power of reputations in international conflict. Their study significantly improves our understanding of how adversaries

[4] See, for example, Todd Sechser, "Reputations and Signaling in Coercive Bargaining," *Journal of Conflict Resolution* (forthcoming) and Joshua Kertzer, *Resolve in International Politics* (Princeton: Princeton University Press, 2016), 3.

assess the credibility of the United States and the impact past actions can have on these perceptions. Harvey and Mitton's findings implicitly urge scholars to continue to investigate the conditions under which reputations form and impact crisis decision-making. In short, this book is a necessary addition to the bookshelf of any scholar or practitioner interested in reputation, deterrence and compellence, or American foreign policy.

Review by Rupal N. Mehta, University of Nebraska-Lincoln

Frank P. Harvey and John Mitton's new book, *Fighting for Credibility: U.S. Reputation and International Politics*, provides a thoroughly researched and compelling response to the existing literature on credibility and reputation in U.S. foreign policy. Couched in the contemporary context of the Obama administration's reaction to the use of chemical weapons by the Syrian government, the authors develop a methodical approach to appraising prior scholarship on credibility and addressing the theoretical, empirical, and policy limitations of that scholarship. They also develop a compelling new theoretical approach, grounded in rational deterrence theory, to clearly and persuasively outline the very important role that credibility can play in U.S. foreign policy decision-making and adversarial response. In this commendable undertaking, Harvey and Mitton conclude with a careful discussion of the theoretical and empirical implications for policymakers and, more broadly, how scholarship must thoughtfully approach bridging the scholar/practitioner divide.

This book provides a welcome reply to the prior generation's critique of credibility and reputation-building in international relations and contributes to contemporaneous, mixed-method research on these issues. First, the book establishes a methodical and carefully-outlined response to core work on credibility and foreign policy by Daryl Press, Jonathan Mercer and Ted Hopf–herein known as PMH[1]. The first task for Harvey and Mitton is to present an overview of the limitations of PMH's work–ranging from PMH's misspecification of rational deterrence theory as a foil for their own theoretical framework to concerns regarding the case selection method (i.e. PMH's focus on pre-Cold War era cases used as evidence for comparison and recommendations on the Syrian crisis). In clearly articulating, often in great detail from the original sources, how PMH's work aimed to summarily dismiss the importance of credibility based on their theoretical framing and assumptions, Harvey and Mitton are able to identify clear avenues for departure from this research and, more importantly, where their original theory on the importance of credibility may yield better answers to these questions. Specifically, Harvey and Mitton base their theoretical framework (rational deterrence theory or RDT) and analysis on four core prerequisites: communication, interests, capabilities, resolve - rather than simply the two realist-based assumptions, power and interests, that underlie PMH's framework. It is through these four factors and other components of their approach, grounded in rational deterrence theory, that Harvey and Mitton persuasively identify the conditions under which states can issue credible coercive threats and how they can impact strategic interactions between the United States and its adversaries.

Similarly, Harvey and Mitton argue that part of the reason that PMH articulate no clear impact of credibility for U.S. foreign policy is their choice of empirical evidence on which to base their claims. Harvey and Mitton contend, for example, that PMH rely on cases of major power conflict, many of which stem from the early 1900s, that lack sufficient similarity with the case in question–the Syrian chemical weapons crisis. To address this concern of case selection, Harvey and Mitton include three case studies on U.S. reputation-building in asymmetric military situations from the past two decades–meant to provide a closer approximation of the Syrian crisis and to investigate the relevance of recent past behavior and reputation on specific questions of deterrence. Given the utility of this argument in my work (i.e. addressing how U.S. actions in managing the

[1] Daryl Press, *Calculating Credibility: How Leaders Assess Military Threats* (Ithaca: Cornell University Press, 2005); Jonathan Mercer, *Reputation and International Politics* (Ithaca: Cornell University Press, 1996); Ted Hopf, *Peripheral Visions: Deterrence Theory and American Foreign Policy in the Third World 1965-1990* (Ann Arbor: University of Michigan Press, 2004).

spread of nuclear weapons in Iran through the Joint Comprehensive Plan of Action has important implications for future negotiation attempts with North Korea), I found the choice of these crises, varying on a host of important factors, to be a compelling context with which to examine the robustness of the theory.[2]

Lastly, this book achieves its objective in questioning the logic and evidence from PMH's work that influenced wonks and policymakers–often through PMH's own articulation for how U.S. foreign policy should be influenced by their research. Through the course of their analysis, Harvey and Mitton provide a robust and rigorous answer to policy-relevant questions regarding the impact of coercive threats in present and future crises and, in doing so, incorporate a thoughtful discussion of how and when scholars can contribute to policy debates to address the crisis du jour. As I have discussed elsewhere and experienced first-hand,[3] scholars undertaking policy-relevant research must be cautious in adapting their necessarily probabilistic, multi-causal scientific-based approach into clear, concise, and definitive policy recommendations. Harvey and Mitton provide important suggestions for how scholars aiming to bridge the gap and contribute to policy-making can more effectively and appropriately do so.

This analysis, in addition to yielding a persuasive counter to PMH on the importance of reputation, also provides two additional contributions to the discipline. First, this book advances the literature on credibility by taking a necessary pause in order to rigorously examine existing logics and provide a systematic, empirical confirmation/disconfirmation of 'grand theory' given the historical and contemporaneous complexities. To progress in our understanding of how reputation unfolds in international crises, it is similarly important to rigorously assess evidence that is used for falsification and what Harvey and Mitton call the 'premature closure of inquiry.' (30).

Second, and relatedly, this book presents one of the clearest examples of 'ideal' research design for Ph.D. students who are designing their own projects. While their review of the literature is necessarily extensive for the purposes of the study, Harvey and Mitton provide a useful template for students (and others) who are learning to identify flaws in existing literature and attempting to draw upon these weaknesses in their own theory development. Further, Harvey and Mitton's focus on and discussion of case selection is similarly important for those learning about case selection design and identifying which approach best suits their inquiry and objectives.[4]

Despite this progress in rigorously analyzing and testing existing scholarship and the providing an original, useful framework with which to better understand contemporary foreign policy crises, the book left me searching for three additional parts to this story. While already complex in examining core strategic interactions between two actors to identify both short and long-term costs and benefits, these absences yield

[2] Rupal N. Mehta, *The Politics of Nuclear Reversal*, manuscript in progress.

[3] Mehta, "The Search for Significance Continues: The Progress of Quantitative Nuclear Studies and the Bridges Yet to Cross," *International Studies Quarterly Symposium*, "Quantitative Nuclear Proliferation: What Have We Learned," January 2016.

[4] Jason Seawright and John Gerring, "Case Selection Techniques in Case Study Research: A Menu of Qualitative and Quantitative Option," *Political Research Quarterly* 61:2 (2008): 294-308.

some weaknesses with the book and, more importantly, highlight important avenues for further research by the authors or other scholars who are similarly focused on credibility in international relations.

The first two challenges arise in part from Harvey and Mitton's objective to address and improve upon the existing scholarship, specifically PMH's theoretical framework. PMH employ a realist, unitary-actor perspective that emphasizes power and interests in understanding dyadic relationships of credibility and reputation. In attempting to mirror this set-up, Harvey and Mitton too are relegated to surveying the puzzle in the same manner, to the unfortunate exclusion of two important pieces of the puzzle: third-party actors such as allies for whom credibility is critically important (i.e. extended deterrent agreements); and leader-specific behavior.

This theory does not emphasize the importance of allies or other party-actors involved in strategic interactions that can assess credibility. By focusing on interactions where adversaries are solely responsible for examining information on U.S. resolve as an insight into its long-term credibility, this theory ignores significant other interactions between the United States and its allies in alliances, extended deterrent agreements, or other larger-scale networks where reputation and the credibility of U.S. behavior and action are critical. Indeed, each of the six components underlying Harvey and Mitton's RDT readily apply to broader dynamics beyond that between actor and adversary. While mentioned in the discussion of the last component, the logical implications of 'reputation as in the eye of the beholder,' its broader impact is less-developed in the theory writ large. If allies are included systematically and formally as a part of an audience observing U.S. behavior, their ability to assess reputation through their own prism of interests, capability, power etc., yields important information. Further, this perspective on U.S. credibility can similarly influence how adversaries view U.S. resolve. Take, for example, NATO's concerns over the likelihood of U.S. intervention and the invocation of Article 5 in defense of an ally. This no doubt has consequences for how NATO members view the strength of U.S. resolve but it also may yield important implications for how *adversaries* view U.S. credibility. If an ally doubts this commitment, adversaries may view this as an opportune time to initiate disputes or challenge the status quo.

This becomes especially salient while considering one of the components of RDT-transferability across cases. Allies often look to other instances of U.S. behavior in crises to assess how the U.S. will uphold its alliance commitments and whether their rhetoric of defense is deemed credible. This may be especially useful as allies attempt to determine if they should pursue their own defensive capabilities because the U.S. alliance commitment is no longer seen as credible. In my research with Neil Narang on extended deterrence and moral hazard, questions about the degree to which the U.S. is resolved to assist allies in other crises can directly influence how allies view their own commitments and whether they may engage in riskier behavior or moral hazard if they don't see U.S. policy as credible.[5] Transferability of reputation from prior conflicts with other actors thus becomes an important guidepost for allies designing their own behavior in extended deterrence agreements.

Second, this study mirrors PMH's focus on unitary state actors. While Harvey and Mitton acknowledge this limitation and suggest that a focus on specific leaders is beyond the scope of their project (99, Footnote 60), I did not find this to be a compelling explanation for including a more disaggregated approach. Historically,

[5] Neil Narang and Mehta, "The Unforeseen Consequences of Extended Deterrence: Moral Hazard in a Nuclear Protégé," Accepted by the *Journal of Conflict Resolution*.

many of the most formal, and often the most employed, models of conflict–including the generic realist models employed by PMH—suggest that "uncertainty exists over the distribution of military power or the costs of war and not over the resolve of individual leaders."[6] It follows that if uncertainty is derived from capabilities, there are unlikely to be significant differences among leaders—successive leaders do not vary in their resolve over conflict behavior. Yet, this is clearly not the case.

Concerns over this misspecification or biased estimates that relied solely on state-level data prompted the incorporation of leader-specific dynamics, resulting in a host of different implications for the role of states in conflict behavior, reputation, and interstate relations more broadly. Perhaps it could be the case that leaders vary in their resolve, in the value they place in certain domestic and international policies, and ultimately in their decisions to maintain or deviate from their predecessors' choices. With this granularity and the incorporation of relevant factors like time in office, reputation, relationship to the prior leader, and political position (incumbent, first- or second-term leader), recent scholarship has been able to differentiate between leaders in the level of their resolve and to identify the origin (and outcome) of their policy differences.[7] This important development in theoretical approach suggests that studies, like this, may similarly benefit from the incorporation of leader-specific dynamics, especially in the current political climate, when assessing the impact of credibility on foreign policy outcomes. This may be especially critical in assessing credibility in states that experience regular leader change less frequently, i.e. dictatorships with rare turnover. For example, this becomes critical when assessing the likelihood of reaching negotiated settlements with Supreme Leader Ayatollah Khamenei in Iran or Chairman of the Workers' Party of Korea Kim Jong-Un in North Korea over their nuclear programs[8] or, even with the case in question, the Syrian crisis and President Bashar Al Assad's use of chemical weapons. Examining leader-specific behavior in domestic and other foreign-policy crises to establish patterns of behavior and perspectives on credibility writ large can influence how the United States interacts with these states.

Last, the third challenge stems from the methodological choice to parallel PMH's use of only case studies to empirically test their logic. PMH, given their theoretical bent, rely on historical case studies to provide empirical evidence to disconfirm the presence of credibility in international relations. As mentioned, the choice of these cases is problematic for reasons listed above. Harvey and Mitton significantly improve upon

[6] Scott Wolford, "The Turnover Trap: New Leaders, Reputation, and International Conflict." *American Journal of Political Science* 51:4 (2007): 772-788.

[7] Wolford, "The Turnover Trap: New Leaders, Reputation, and International Conflict;" Elizabeth N. Saunders, *Leaders at War: How Presidents Shape Military Interventions* (Ithaca: Cornell University Press, 2011); Michael Horowitz, Rose McDermott, and Allan C. Stam, "Leader Age, Regime Type, and Violent International Relations," *Journal of Conflict Resolution* 49:5 (2005): 661-685; H.E, Goemans, Kristian Skrede Gleditsch, and Giacomo Chiozza, "Introducing Archigos: A Data Set of Political Leaders," *Journal of Peace Research* 46:2 (2009): 269-283; Alexander George, "The Operational Code: A Neglected Approach to the Study of Political Leaders and Decision-Making," *International Studies Quarterly* 13:2 (1969): 190-222; Rachel E. Whitlark, "Nuclear Beliefs: A Leader-focused Theory of Counter-Proliferation," *Security Studies* 24:4 (2017): 545-574.

[8] Mehta, "Windows of Opportunity: Positive Inducements, Leadership Change and Nuclear Reversal." Working Paper, Presented at the International Studies Association 2016, Atlanta, GA.

this design by finding cases that match J.S. Mill's most similar design approach.[9] While emphasizing historical, qualitative evidence makes sense from the perspective of countering PMH's design and approach, I found that the relative dearth of discussion on newer quantitative and experimental studies (beyond references to some seminal work by Alex Weisiger and Keren Yarhi-Milo Weisiger and Yarhi-Milo (2015) and Michael Tomz) surprising[10]. One way to strengthen a reply to PMH is to discuss a host of statistical and experimental studies that may explain previous null or mixed findings and provide robust empirical evidence of reputation in U.S. foreign policy. First, one may consider work by Allan Dafoe Jonathan Renshon, and Paul Huth, the aforementioned work by Weisiger and Yarhi-Milo, and the slightly older article by Kenneth A. Schutlz, all of which seek to grapple with these mixed findings and not 'prematurely close the door of inquiry' by understanding the conditions under which past actions can influence policy outcomes.[11] Further, this study could benefit from a detailed discussion of recent experimental (laboratory and survey), and even formal, research in search of reputation – as this is likely to be an important avenue for future research on reputation. Recent analyses by Renshon, Dafoe, and Huth; Dustin Tingley and Barbara Walter; and Joshua Kerzter, among many others, provide innovative answers and rigorous evidence of the presence of resolve and credibility.[12]

In sum, Harvey and Mitton's book offers a critical and informative reply to the existing scholarship on reputation. While previously the academic and policy communities have relied on flawed studies with biased theoretical frameworks and empirical evidence, this book takes a necessary step forward in identifying the theoretical, historical, and contemporaneous importance of credibility. Its strengths include its clear, methodical reply to prior scholarship, a detailed, original theoretical approach, excellent case studies steered by this framework, and a careful discussion of how political science scholarship must still be cautious in its delivery of policy recommendations. While some flaws and limitations remain, these should not detract from the clear contributions of this research. Instead, they should be taken as a call to arms for further research to identify how credibility, both at the state and individual level, can influence policy outcomes. Indeed, given the current political landscape and the rise in bombastic leaders and potential crises across the international system, there can be no better time to understand how the behavior and actions of leaders and their governments can influence how other actors assess their reputations for trustworthiness and integrity.

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[9] John Stuart Mill, *A System of Logic Volumes 1 and 2* (Cambridge: Cambridge University Press, 1843).

[10] Alex Weisiger and Keren Yarhi-Milo. "Revisiting Reputation: How Do Past Actions Matter in International Politics," *International Organization* 69.2 (2015): 473-449. Michael Tomz, *Reputation and International Cooperation: Sovereign Debt across Three Centuries* (Princeton: Princeton University Press, 2007).

[11] Allan Dafoe, Jonathan Renshon, and Paul Huth, "Reputation and Status as Motives for War," *Annual Review of Political Science* 17:1 (2015) 371-393. Weisiger and Yarhi-Milo. "Revisiting Reputation;" Kenneth A. Schutlz, "Looking for Audience Costs," *Journal of Conflict Resolution* 45 (February 2001): 32-60.

[12] Renshon, Dafoe, and Huth, "Leader Influence and Reputation Formation in International Politics," *American Journal of Political Science*, 2017 forthcoming; Dustin H. Tingley and Barbara F. Walter, "The Effect of Repeated Play on Reputation Building: An Experimental Approach." *International Organization* 65 (Spring 2011): 343-365; Joshua D. Kerzer, *Resolve in International Politics* (Princeton: Princeton University Press, 2016); Kertzer, "Resolve, Time, and Risk," *International Organization* 71(S1) (2017): 109-136.

Review by Keren Yarhi-Milo, Princeton University

Fighting for Credibility offers a comprehensive treatment of how reputation for resolve forms and changes in international relations, and how it affects the assessments of allies and adversaries. Unlike most qualitative empirical work on this topic, which mainly relies on archive-based historical case studies, this book uses a set of mini case studies of recent asymmetric coercive encounters between the United States and smaller powers in which, the authors argue, reputation for resolve played an important role. These mini cases include Bosnia-Herzegovina (1992-1995), Kosovo (1998-1999), and Iraq (1991-2003). The main empirical contribution of the book, however, is its analysis of the aftermath of President Barack Obama's decision not to bomb Syria after President Bashar al-Assad violated Obama's 'red line' and used chemical weapons. The book's main findings will not be surprising to those who already believe in the importance of reputation (although, as I note below, the book's treatment of the Syria case is somewhat surprising).

The authors find that reputation for resolve plays an important role in how adversaries assess credibility of threats. This book, however, acknowledges both the importance and limitations of such reputation. Thus, the book explains that credibility of threats is neither perfectly transferable nor entirely nontransferable to other crises. Rather, using a more nuanced rational deterrence theory, the authors argue that decisions to back down or stand firm in previous crises are not definitive for reputation. Furthermore, since credibility is in the eye of the beholder, reputation for resolve is likely to be more consequential under some conditions than others (29). Overall, the book offers a balanced assessment of the role that reputation for resolve plays in making subsequent coercive threats credible.

There is a lot to like about this book. It will no doubt become a must-read for all students interested in theoretical and policy debates about when and why it is worthwhile for the United States to fight for credibility. The book offers the most comprehensive literature review to date on reputation for resolve in international relations, and it does an excellent job in unpacking the policy implications that follow from the different schools of thought concerning this topic (i.e., the reputation critics school on the one hand, and the reputation proponents school on the other). Finally, the emphasis on crises after the end of the Cold War demonstrates the importance of reputation for resolve even under unipolar conditions. This finding strengthens the argument of reputation proponents, who up until now have supported their arguments mainly with from statistical analysis of Militarized Interstate Disputes (MIDs) and historical case studies, largely from the pre-Cold War and Cold War periods.

In the rest of my discussion, I will focus on three points that deserve more clarification and discussion. First, the theoretical framework of the book assumes that states are rational actors, and, following from this assumption, much of the discussion and analysis in this book concerns the degree to which other states draw inferences about credibility of threats from states' past actions. This is the standard way scholars have approached the issue. At the same time, there is some tension between this rational framework and the book's emphasis that reputation and credibility are in the eye of the beholder, and so inferences drawn from past action are not homogenous. As the authors point out, the empirical analysis demonstrates that leaders miscalculate when they interpret their adversary's reputation for resolve, as well as their own. This theme runs across all the case studies. And yet, given that the theory highlights the importance of perceptions and misperceptions of credibility, there is little theoretical discussion about what types of leaders are prone to draw inferences from past actions to begin with; what types of biases should be especially dominant in asymmetric coercive encounters; and under what conditions are misperceptions likely to have consequential effects.

These are crucial and unresolved issues that are directly relevant to one of the book's main claims that "reputations, credibility and transferability are in the eye of the beholder" (sub-heading 2.6, chapter 2). I sympathize with the difficulty in this type of theorizing. Indeed, the authors reference my book, *Knowing the Adversary*, which deals with the complexity of drawing inferences about the long-term political intentions of adversaries, and it shows that leaders and intelligence organizations use different indicators to assess intentions.[1] Given the difficulty in theorizing about perceptions, I understand the authors' wish to treat this as outside the scope of this book. And yet, I would have liked to see the authors wrestle a bit more with this important question in the context of their dependent variable (the importance of past actions in assessing credibility of coercive threats). They could have done so either deductively, by deriving clear theoretical predictions, or inductively, by highlighting any systematic patterns in the empirical record.

A second and related point concerns the concept of reputation for resolve. The authors distinguish between two types of reputation: general reputation (which is "patterned, consistent, dispositional, and relevant between cases") and specific reputation (which is "situational, short-term, and relevant primarily within protracted crises" 104). This distinction makes a lot of sense. But the analysis in the book could be strengthened if the authors spent more time unpacking the relationship between these two types of reputation. For example, do states tend to invest more in specific reputation when they perceive their general reputation to be weak? Do states tend to more accurately assess their general reputation than their specific reputation? Can specific reputations change the state's general reputation? And if so, under what conditions? Addressing these and related questions could have offered the authors a unique opportunity to make a significant contribution to our conception of reputation for resolve. Instead, the book leaves the discussion abstract, and there is little in the empirical analysis that circles back into this part of the conceptual framing.

Relatedly, it seems to me that given that this book seeks to understand the role of reputation for resolve in making threats credible, another important distinction should have been made between general reputation for resolve and what Robert Jervis calls "signaling reputation."[2] The latter refers, more specifically, to the state's history of backing down from *threats*. This seems to me a more relevant distinction considering the book's concern about credibility during crises. It is thus unclear whether "signaling reputation" is part of a general reputation, specific reputation, or both. Moreover, the book could be clearer about whether reputation appropriately resides in leader, state, or more broadly, with certain regime types. The authors indicate in footnote 60 on page 99 that they do not address whether reputation adheres to leaders, states, or regimes. But this also introduces some tension into the analysis. On the one hand, the unitary rational framework of the book implicitly assumes that reputations adhere to states. On the other hand, we are told that reputation is in the eye of the beholder, without specifying whether the beholder is the adversary's leader, state, regime, etc. Here, too, I believe the book could have made a larger impact if, at the very least, it used the empirical evidence to shed some light on the unit of analysis question, even if such evidence is only suggestive.

The first two points above highlight some limitations of the authors' theory. But on closer inspection, it appears that rather than advancing a specified theory about when reputation for resolve should matter, the

[1] Keren Yarhi-Milo, *Knowing The Adversary: Leaders, Intelligence, and Assessment of Intentions in International Relations* (Princeton: Princeton University Press, 2014).

[2] Robert Jervis, "Signaling and Perception: Drawing Inferences and Projecting Images," *Political Psychology* (2002): 293-312.

authors are more concerned about arguing *against* the two extreme arguments in the literature—that reputation is never transferable and that reputation is perfectly transferable—and they successfully demonstrate that neither holds empirical scrutiny. In particular, the authors are persuasive in showing that the claim of reputation critics that adversaries *never* assign reputation for resolve based on past action is inaccurate and misleading.[3] The points I have raised above intend to push the authors to specify more sharply the observable implications that should follow from *their own theory*, and to offer a clear and falsifiable set of conditions under which reputation should be more or less relevant for the credibility of coercive threats.

My third and final point seeks to highlight why the absence of such clear predictions (rather than evidence against extreme reputational claims) complicates the interpretation of empirical evidence and the lessons readers should draw from the case studies. For example, the authors argue that reputation will be more transferable across the most similar cases. But the case studies do not provide a clear operationalization of similarity, or indicate how cases appear similar to both U.S. leaders and adversaries. The analysis is even less clear cut in its prediction because such similarities are in the eye of the beholder (and, as I note above, we are given no guidance as to the direction in which the biases could operate). As such, readers are left wondering about the *a priori* theoretical expectations regarding the extent to which reputation for resolve should have mattered in any given case. Again, the authors do a great job arguing against the reputation critics, but this bar is a bit too low for my taste. For example, can the theory allow us to assess what types of actions are more likely to produce 'tipping points' about resolve in the eyes of adversaries? What other factors or alternative explanations could have also contributed to those 'tipping points' and why reputational considerations offer a superior explanation to those alternatives?

Similarly, the analysis of the role of general and specific reputation in the context of the Kosovo war and the Syrian crisis is very interesting. These cases point to a more complex interplay between the authors' two types of reputations, although, again, those types lack theoretical explication. For example, these cases suggest that some military actions might be particularly effective in bolstering one's specific reputation for resolve precisely because they circumvent constraints that weaken one's general reputation. In the context of Kosovo, the argument goes, the United States' and NATO's sustained air campaign was successful because it allowed the allies' leaders to avoid military casualties, a concern that long weakened U.S. general reputation for resolve in the eyes of the Serbian leader, Slobodan Milosevic. The theoretical implications are interesting (although never drawn out): actions designed to bolster one' specific reputation might be seen as credible not because they are inconsistent with well-known weaknesses in general reputation, but rather because they circumvent such weaknesses. More clearly, actions that increase one's specific reputation might at the same time reinforce weaknesses in one's general reputation.

Beyond this theoretical point, the main argument of the Kosovo case is that Milosevic did not back down earlier in this crisis because he was aware of the United States' general reputation for casualty aversion, as well as the behavior of U.S. and NATO officials in other crises including Bosnia and Iraq. Yet, the analysis in this case does not provide sufficiently convincing evidence as to why alternative explanations for Milosevic's behavior are not equally convincing. Admittedly, accessing the adversary's calculations, especially in more

[3] For critical work regarding the role of reputation see, for example, Jonathan Mercer, *Reputation and International Politics* (Ithaca: Cornell University Press, 2010); Daryl Grayson Press, *Calculating Credibility: How Leaders Assess Military Threats* (Ithaca: Cornell University Press, 2005); Shiping Tang, "Reputation, Cult of Reputation, and International Conflict," *Security Studies* 14.1 (2005): 34-62.

recent crises where primary evidence is virtually nonexistent, is very difficult. Thus, the conclusions regarding the adversary's inferences and calculus should be read with caution.

This point is even more significant when considering the case of the Syrian use of chemical weapons in the summer of 2013. The argument the authors offer in this book departs, by their own account, from the conventional wisdom that views Obama's backing down from his 'red-line' threat and limited involvement in Syria as significantly damaging U.S. credibility and reputation for resolve. *Fighting for Credibility*, in contrast, argues that notwithstanding Obama's withdrawal from the 'red-line' threat, "US reputation extracted from previous crises (such as Kosovo, Iraq and Libya) had a significant effect on how adversaries in this case assess the credibility and probability of military force. (167). In addition to the United States' general reputation, they argue that Obama's actions in the aftermath of the chemical attack—including threats of escalation—reestablished the credibility of the red line. This enhanced the United States' specific reputation, the argument goes, thereby leading Russia and Syria to agree to dismantle Assad's chemical stockpile. To the extent that Obama achieved his stated objective as outlined by his team at the time—that is, "deterring [Weapons of Mass Destruction] WMD use and compelling WMD disarmament under [Chemical Weapons Destruction] CWC"—the authors consider the outcome of this crisis to have been a success, and a good example for how general and specific reputation for resolve made U.S. coercive threats credible and effective.

This interpretation of the crisis is certainly possible, but it raises more questions than answers. For one, the authors offer nearly no evidence to support their conclusion that the United States' general and specific reputation enhanced U.S. credibility in the eyes of Russian President Vladimir Putin and Assad, and that these considerations played any role in their calculus. It is also problematic that the authors do not offer a systematic treatment of alternative explanations for Russia's behavior. Some would argue that Russia had incentives to force Syria to accept the disarmament deal regardless of U.S. reputation for resolve.[4] Moreover, in 2014, 2015, and 2017 the Syrian regime once again used chemical weapons against civilians. In light of these events, should we still consider Obama's actions a success? Given the authors' definition of success, what should we make of these recent events? Finally, the analysis is unclear about whether Obama's backing down from his 'red line' had no negative effect on the United States' general reputation for resolve in the eyes of U.S. allies and adversaries. Surprisingly, the authors do not reflect on those implications. Rather, they conclude by arguing that the extent to which the success in Syria is transferable to cases of Iran and North Korea depends on the similarities between the cases, and those rest in the eyes of the beholder.

My comments should not take away from the important contribution this book makes to our understanding of how reputation emerges and changes, and the importance of reputation for the success and failure of coercive threats. If there is one takeaway from this book, it is that while reputation for resolve certainly matters, and we should be careful not to exaggerate its transferability. The complexity of how and when reputation for resolve will transfer across observers and domains necessitates more empirical and theoretical work. *Fighting for Credibility* gets us one step closer to understanding these important dynamics.

[4] One such interpretation would point to Russia's desire to be seen as a major player in this crisis. The disarmament deal provided a good opportunity for Putin to show his ability to control his ally. Another explanation is even simpler: Russia had its own interest in dismantling Assad's chemical weapons, knowing that his use of such weapons could significantly diminish the probability that he could stay in power, not because the U.S. would escalate, but because such actions will further reduce his legitimacy in the eyes of the international community.

Author's Response by John Mitton and Frank P. Harvey

Questions of reputation and credibility remain at the center of debates over the proper conduct of American foreign policy. Recent international crises–most notably on the Korean Peninsula and in the form of renewed chemical weapons attacks in Syria–are unfolding in the midst of a paradigm-changing leadership transition in Washington. An undeniable consequence of this change has been unprecedented levels of unpredictability and *uncertainty*; and where uncertainty reigns, questions of reputation, resolve, and credibility naturally resurface. We are therefore very grateful to the ISSF editors for the opportunity to reengage this important scholarly discussion, particularly given the thoughtful and productive reviews of our work offered by Rebecca Friedman Lissner, Danielle L. Lupton, Rupal N. Mehta, and Keren Yarhi-Milo. We also thank Peter D. Feaver for organizing and editing the roundtable, and for providing excellent comments of his own.

In this reply, we seek to accomplish several things. First, we establish points of agreement and consensus that clearly and productively move theoretical and empirical debates on international reputations and credibility forward. We then address several thoughtful criticisms and suggestions raised in the reviews, particularly with respect to a) theoretical refinements and b) our interpretation of the Syria case. Next, we briefly comment on developments which have occurred since publication (and which were invoked by several of the reviewers) that pertain to the arguments and interpretations we advanced. Finally, taking our cue from suggestions by the reviewers and adding our own thoughts, we outline avenues of future research.

The verdict on the role of reputations in international politics has been overturned.[1] This conclusion emerges not only from the research and findings explored in our book but in several other recent contributions to the literature, including by scholars involved in this exchange. Our work (and that of others) successfully challenges the 'new' consensus view outlined by reputation critics (the most prominent of whom are Daryl Press, Jonathan Mercer, and Ted Hopf[2]–PMH, in our adopted shorthand) which suggests that backing down, bluffing, or otherwise undermining one's reputation for resolve carries no significant consequences. The PMH consensus, we argue, is not only wrong but potentially dangerous if used as the basis for foreign policy advice and action, primarily because it is derived from a superficial understanding of how adversaries calculate and predict U.S. credibility and associated coercive diplomatic strategies. Assessments of reputation based on past actions, we argue, help to mitigate some of the uncertainty facing adversaries as they attempt to determine what the United States will do in a particular crisis–a necessary corrective to the simplistic assertion by Press and others that American power and, even more problematically, 'interests' are readily apparent to adversaries. We similarly disaggregate reputation into its 'specific' and 'general' forms, the former operating within protracted crises and the latter adhering over time and across cases.

We are encouraged that the reviewers highlight the importance of these contributions (along with others) and generally agreed with our argument that understanding 'how' and 'why' reputations matter is more

[1] Or–to torture the metaphor–given the original centrality of reputation in classic works of deterrence, the most recent round might be described as the rejection of the 'appeal' petitioned by reputation critics.

[2] Daryl Press, *Calculating Credibility: How Leaders Assess Military Threats* (Ithaca: Cornell University Press, 2005); Jonathan Mercer, *Reputation in International Politics* (Ithaca: Cornell University Press, 1996); Ted Hopf. *Peripheral Visions: Deterrence Theory and American Foreign Policy* (Ann Arbor: University of Michigan Press, 1994).

productive than the all-or-nothing question of 'whether or not' they matter at all. We hope that our book, and the strengths identified therein, can–along with the other excellent recent work on the subject identified by several of the reviewers–help shift the policy discussion in a more productive, and accurate, direction.

In addition to highlighting the strengths of the book, the reviewers similarly outline relatively minor issues with the logic, theory, and evidence we present. These critiques were generally offered as suggestions for theoretical refinements and future case-study/empirical work–an encouraging sign – and we take up several of the most pressing insights here.

One recurring critique–offered in particular by Mehta and Yarhi-Milo–questioned what they view as our heavy reliance on a unitary actor assumption. Both reviewers point out–correctly–that we specify in footnote 60 of page 99 that we do not explicitly address whether reputation adheres to the state, the leader, the regime type etc. By failing to do so, they argue, we limit our theoretical contributions.

Yet our theory, as presented, is not bound by, or limited to, the unitary rational actor assumption. In specifying that reputation is in the eye of the beholder, for example, we allow for miscalculation and misperception, noting in our cases those instances in which an adversary (e.g. Serbian leader Slobodan Milosevic or Iraqi dictator Saddam Hussein) erred in their assessment of American resolve based on flawed reputational inferences. We likewise discuss the role of domestic politics and other societal considerations (including the propensity for casualty aversion in democratic publics) and how these factors shape the assessments adversaries make. Indeed, by not specifying the precise level or entity to which reputation adheres, the theory suggests that reputational inferences can be made with respect to *multiple* levels, a possibility reflected in our case studies. Adversaries can assess U.S. reputation as a superpower; the reputation of the American public to be casualty averse; the different foreign policy reputations of Democrat and Republican leaders; and the reputations of specific U.S. leaders/advisors in similar crises. Unpacking the precise influence of each of these assessments under particular conditions (and the direction of potential misperceptions, as well as the influence of different individual psychological dispositions or personality types) is an important research question that scholars should certainly explore, but the theory we present does not in any way preclude or bias these research questions and, in fact, provides a useful framework within which to accomplish them. We acknowledge that our book does not investigate, for example, individual psychology in great detail, but our theory does not *exclude* this possibility. The point is not to dismiss the critique wholesale (the reviewers make legitimate points regarding the importance of expanding the scope of the argument) but rather to maintain that the theory presented was not intended to be a unitary-actor alternative to more disaggregated explanations. Finally, our commitment to rationality is bounded by instrumentality; we simply assume that leaders do not act against their own *perceived* self-interest, thereby significantly relaxing strict rationalist assumptions.

Other concerns raised by reviewers focused on the empirical evidence we compile through our case studies. Lissner and Lupton (most explicitly) express a desire for additional evidence with respect to an adversary's statements regarding the substance and/or importance of past U.S. behavior as it relates to their assessments of U.S. credibility. We agree that such evidence is helpful (and indeed provide several such statements throughout each of the case studies). But focusing exclusively on collecting and interpreting these statements (or waiting for such statements to be uncovered, if ever) is not the only or even most effective way to assess the validity of our theory. First, as is well known, the meaning and/or relevance of statements are not self-evident or consistently reliable, given the many obvious incentives to misrepresent and deceive under particular circumstances. It is therefore just as important to provide a logical explanation for the behavior

observed in a particular case and to assess this interpretation against plausible alternative explanations, which we do in our book. Ideally, each case would offer a persuasive combination of archival evidence and compelling logical explication derived from theory. In those contemporary cases in which archival evidence is limited or non-existent (such as the Syria case) the burden will naturally shift to the latter approach. That being said, we absolutely support the call for further and more in-depth research on all of the cases covered in the book, and remain confident that the interpretations we offer will hold-up in the light of additional evidence.

Of these interpretations, the most controversial–or, as Yarhi-Milo puts it, "surprising"–one relates to our explanation of Syrian and Russian behavior in the 2013 chemical weapons crisis. We certainly admit to bucking conventional wisdom here, particularly with respect to our attribution of coercive 'success' to the strategy employed by the Obama administration. This interpretation has been challenged, many argue,[3] by subsequent events, specifically the use of sarin gas by the Assad regime in April 2017. We address these issues below.

Feaver points out, for example, that the 2013 case may be interpreted as a coercive diplomatic success for Syria vis-à-vis the United States, and not the other way around. In this formulation, Feaver argues, Syrian President Bashar al-Assad secured an implicit security guarantee from the U.S. not to pursue regime change in exchange for the chemical weapons deal. In fact, this interpretation does not diverge fundamentally from our own. An 'implicit guarantee' was offered *with respect to the use of chemical weapons* (CW)–this was the basis of the threatened air-strikes. As we note in the book, not following through once the adversary acquiesces is an essential component of coercive diplomacy as it indicates to other adversaries (i.e. Iran, North Korea) that cutting deals is worthwhile. We did not comment on the broader implications for U.S. strategy in Syria, because the intent of the red-line was never about ending the conflict itself; it was about preventing the use of CW. It's likely that Assad had the broader conflict in mind when he agreed to the disarmament deal, but only because the threat of U.S. air-strikes (and the subsequent possibility of escalation) was credible; otherwise, he would have simply retained the weapons and continued to use them sporadically at low-levels, as he had done in the months and years leading up to the August 2013 attacks in Ghouta, and over which time the U.S. had clearly signaled its lack of desire to become directly involved.

Yarhi-Milo, for her part, posits two alternative interpretations for Russian behavior. First, that Moscow had an incentive to broker the disarmament deal so as to demonstrate Putin's ability to control Assad and assert Russia's status as a major player in the crisis. Or, second, that Moscow had its own interest in seeing Assad's CW stockpiles dismantled, as the continued use of such weapons would "diminish the probability that he

[3] This challenge was raised by both Feaver and several of the reviewers, who noted, quite reasonably, that if our benchmark for success was the deterrence of CW use, the 2017 attacks would appear to undermine the basis for our interpretation. Many articles written at the time similarly positioned the new round of attacks as additional evidence that the initial red-line strategy from 2013 had been a failure. See for example Hisham Melhem, "How Obama's Syrian Chemical Weapons Deal Fell Apart," *The Atlantic*, 10 April 2017, https://www.theatlantic.com/international/archive/2017/04/how-obamas-chemical-weapons-deal-fell-apart/522549/; David Iganatius, "Trump enforces the 'red line' on chemical weapons," *The Washington Post*, 6 April 2017, https://www.washingtonpost.com/opinions/trump-faces-a-moral-test-in-syria/2017/04/06/bea8bdde-1aee-11e7-bcc2-7d1a0973e7b2_story.html?utm_term=.008c66177342 ; Sean Keeley, "Legacy of the Red Line: More Questions for Obama on Syria," *The National Interest*, 7 April 2017, https://www.the-american-interest.com/2017/04/07/more-questions-for-obama-on-syria/.

could stay in power…because such actions [would] further reduce his legitimacy in the eyes of the international community."

With respect to the first possibility, we have no doubt that Moscow wished to demonstrate its ability to exert influence in the region; but the decision to exercise this influence in the form of the CW deal was the result of a credible U.S. threat (there was no mention of Russia disarming Assad's CW *before* the threat of air-strikes). That Putin might 'seize' the opportunity to remove a significant strategic capability from his regional ally in the face of a *non*-credible American threat stretches the imagination–in effect, this would imply an effort by Moscow to needlessly acquiesce to American interests in order to demonstrate an ability to challenge American interests if necessary.

As to the second possibility, we largely agree–Moscow did have its own interest in seeing Assad's CW dismantled, but those interests were the product of (caused by) the threat of military action and the increased probability of losing yet another ally in the Middle East. The concern that Assad could fall from power was related to potential American escalation, and not diminished "legitimacy" in the broader international community which, following the Ghouta attacks, had essentially bottomed-out already. Moscow, rarely concerned with international norms or related questions of legitimacy, could prop-up the Assad regime through most if not all challenges, except one that included an expansion of American military intervention.

It is worth noting that some of the first pieces of direct evidence to emerge relating to the regime's perceptions of the U.S. threat corroborates our explanation. On the basis of original reporting within Syria, Christopher Phillips notes that pro-regime elites as well as the families of regime officials began to flee into Lebanon in late August and early September 2013–i.e. shortly following the Obama administration's threat of air-strikes. "The prompt for this abrupt exodus," Phillips writes, "was the belief that any day a western military coalition, led by the US, would launch missile strikes on the regime."[4] He goes on to quote a businessman making the border crossing who, in speaking with reporters, said: "My uncle is a senior officer. He is one of the decision-makers, and this week the only decision he's making is where to take shelter from the American planes."[5] Of course, this evidence is hardly conclusive (its probative value is significantly less than, say, transcripts or memos of regime deliberations), but it is suggestive. It is also consistent with our view that the U.S. threat of air strikes was credible, impactful, and successful.

Perhaps the most significant development since the publication of the book is the use of sarin gas by the Assad regime in April 2017–an episode invoked by more than one reviewer, with Yarhi-Milo questioning what the incident might mean for our definition of 'success' in the Syria case and Lissner explicitly wondering how we might interpret President Donald Trump's decision to launch cruise-missile strikes in retaliation. In fact, Mitton addressed this episode shortly after the fact,[6] but a few comments are germane here.

[4] Christopher Phillips, *The Battle for Syria: International Rivalry in the New Middle East* (New Haven: Yale University Press, 2016), 168.

[5] Phillips, 169.

[6] John Mitton, "Trump Channels Obama on Deterrence in Syria," MacDonald Laurier Institute, 10 April 2017, http://www.macdonaldlaurier.ca/trump-channels-obama-on-deterrence-in-syria-john-mitton-for-inside-policy/.

First, the latest round of sarin use by Assad is *consistent* with the theory and related expectations we outline in the book; as we repeatedly make clear, successful deterrence is rarely permanent – it unfolds over time and in stages, as adversaries probe for updated information regarding U.S. resolve. Consider, then, the signals being sent by first candidate and then President Trump over the last several years–numerous tweets about prioritizing the Islamic State of Iraq and Syria (ISIS) over Assad, repeated statements regarding limiting American involvement in the civil war, a clear affinity for all things related to Russia (Assad's principal backer), statements from Secretary of State Rex Tillerson that Assad's ouster was no longer a priority (or even a position) of the new administration, etc.–all of these naturally led Assad to believe that he could get away with slowly escalating CW use (sporadic chlorine use had continued since 2013–but, for reasons we outline in the book, chlorine gas is of a different character insofar as it is not technically a banned chemical–and alleged sarin use against ISIS militants in May 2016 had drawn little to no response or opprobrium[7]). The probe in April 2017 was therefore largely predictable. Trump's response, though limited, was intended to reestablish the credibility of the on-going American red-line against CW.[8] Irrespective of "fawning praise" or otherwise, the decision is broadly consistent with the coercive strategy implemented during the Obama administration. In fact, Trump appears to be following Obama's lead in other foreign policy crises–consider, for example, the recently announced 'new' strategy in Afghanistan which similarly bears a striking resemblance, in form if not rhetoric, to that of his predecessor. Whether President Trump's moves in Syria are sufficient to firmly reestablish effective deterrence for any appreciable duration of time remains to be seen; in the short-term, it has likely curbed the deployment of sarin gas by the regime.

Certainly, the 2017 attack undermines the notion that the 2013 disarmament deal was an unqualified success, particularly with respect to either ridding the country entirely of prohibited CW or preventing Assad from re-acquiring limited CW capabilities.[9] But again, the terms of 'success' we outline in the book are related to the *use* of CW (the purpose of the initial red-line and subsequent coercive diplomatic strategy was to prevent this use); here again the strategy was largely successful for three and a half years before an entirely predictable probe in the context of a new administration and associated signals indicating less commitment to the established deterrent posture. As in the debate regarding reputation more generally, insisting on a strict either/or distinction between (total) success or (complete) failure may be rhetorically satisfying but is not

[7] David Blair, "Assad's Forces have 'used' Sarin Nerve Gas' for the First Time Since Syria's 2012 Massacre," *The Telegraph News*, 17 May 2016, http://www.telegraph.co.uk/news/2016/05/17/assads-forces-have-used-sarin-nerve-gas-for-the-first-time-since/.

[8] It is easy but disingenuous to suggest that any and all CW use violates this red-line; this clearly was not the case during the Obama administration, in which periodic but limited use in the lead up to the August 2013 attack was tolerated. The point is that the red-line stands at an implicit *threshold* of CW use; indeed, it is precisely the location of this threshold that Assad's probes were and are intended to locate, particularly as he receives signals that American resolve to maintain it may be changing or eroding.

[9] Initially it appeared the attacks confirmed that Assad regime had retained some of its CW capabilities; a recent report from the UN suggests that perhaps the regime re-constituted a capability with the help of North Korea. Such a re-acquisition itself would constitute a limited 'probe', followed by escalation into actual use. Michelle Nichols, "North Korea Shipments to Syria Chemical Arms Agency Intercepted: UN Report," *Reuters*, 21 August 2017, https://www.reuters.com/article/us-northkorea-syria-un-idUSKCN1B12G2.

particularly helpful for assessing the real-world implication of important coercive diplomatic strategies related to deterring the use of devastating WMD.

We conclude this reply by noting some additional suggestions from the reviewers which we believe constitute potentially fruitful extensions of our theory and productive avenues of future research.

First, Mehta makes an excellent point regarding reputation and third-party actors–both with respect to extended deterrence relationships and within formal military alliances such as NATO. While the role of third-party actors and alliances is not ignored in the book (we examine for example the role of the UN in Iraq and NATO in Bosnia and Kosovo, as well as the extended deterrence relationship between Russia and Syria) we do not explicitly address the interactions between the U.S. and its allies and how the United States' reputation is relevant within those relationships. Future work could extend our theory to ally in addition to adversary perceptions.

Second, several of the reviewers suggest that the relationship *between* general and specific reputation could be more systematically theorized. As Lupton asks, "how do these…reputations interact to inform adversaries' perceptions during crises?" Yarhi-Milo similarly wonders whether "states tend to invest more in specific reputation when they perceive their general reputation to be weak" while also suggesting that an additional 'signaling reputation' might be considered as a third category between, or in relation to, general and specific reputation. The Kosovo case, in particular, highlights certain possibilities in this regard. Exclusive reliance on air strikes to avoid U.S./NATO casualties, for example, actually enhanced specific resolve/reputations and credibility; U.S./NATO officials credibly threatened ongoing military operations directed at specific high-value targets in Belgrade, thus increasing the costs to Milosevic. But this strategy also reinforced a general U.S. reputation for casualty aversion (no ground troops), which could conceivably affect adversary perceptions in other cases. This case also raises interesting theoretical questions about the role of 'different' capabilities in a crisis and their combined effects on resolve and credibility—i.e., capabilities are not additive. Focusing exclusively on airstrikes and rejecting the addition of ground troops raised the credibility of the threat to sustain the bombing and the direct costs on Milosevic, while minimizing corresponding costs on the U.S./NATO. Utilizing fewer capabilities, in other words, produced a more 'powerful' response that led to capitulation by Milosevic. Space constraints preclude a more exhaustive discussion of such possibilities. Suffice to say we agree that the interplay between different types of reputation–including the ways in which they might reinforce or contradict one another–are interesting and important research questions to explore.

These and other possibilities outlined by the reviewers suggest a robust research agenda. Clearly, scholars have taken up the challenge (outlined initially by Dafoe et al. in their survey of the field and taken up by us in our book and by others elsewhere)[10] to examine the *how* and *why* of reputation in international politics. Given present international realities–in which the management of coercive diplomatic crises are crucial for both the national security of the United States and the stability of the global system–this effort is timely and necessary. We again thank each of the reviewers as well as the editor of this roundtable for continuing this important conversation.

[10] Allan Dafoe, Jonathan Renshon, and Paul Huth, "Reputation and Status as Motives for War," *Annual Review of Political Science* 17:4 (2014): 371-93.