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Introduction by Christopher Ball

Historians and political scientists alike should appreciate Dan Reiter's *How Wars End*. It eschews statistical analysis for comparative case-studies because the answers are "complex and nuanced" (6) and defers formal proofs for plain-language explanations. The six empirical chapters are based on case-specific puzzles rather than theory-driven questions. The three reviewers—Dale Copeland, Hein Goemans, and Zachary Shirkey—find few major flaws with *How Wars End*, although each has some reservations over aspects of the argument. Because some readers might not be versed in rationalist theories on war that Reiter engages, this introduction will first provide an overview of them and then discuss the reviews in the next section.

Credible Commitments, Issue Indivisibility, and "Gambling for Resurrection"

How Wars End is part of a sub-set of international relations theories known as rationalist explanations of war.¹ The central puzzle is why states fight wars when any ex post outcome—the terms of a peace agreement—comes at the cost of fighting the war. Short of wars of annihilation, rational leaders should bargain to reach an outcome that avoids the cost of fighting the war. Rationalist explanations seek to explain, in ways consistent with the axioms of rationality, why wars nevertheless occur.²

There are three causal mechanisms in the rationalist explanations. First, the information model argues that states have incentives to misrepresent their true capabilities and positions for bargaining advantages. Miscalculations and uncertainty over relative power matter are the results of incentives to misrepresent real intentions and capabilities rather than primary causes.³ The second mechanism holds that the crucial problem is the difficulty of making credible commitments. A commitment is incredible if a party has an incentive to renege. This is not the same as saying that states face a security dilemma in anarchic world politics. In the security dilemma, uncertainty over others' intentions and capabilities or the belief that intentions could change drive conflict. In the rationalist framework, commitments may be incredible even when intentions and capabilities are known and stable. Instead, the central problem is that there is no guarantee that commitments will be kept so long as incentives to renege exist. Third, wars can be fought because the issue at stake is inherently indivisible—only one side can possess it. In principle, there are few if any truly indivisible issues. Territory can be divided; authority can be shared. Political norms might make an otherwise-feasible division illegitimate, but this is distinct from fundamental indivisibility.⁴ Instead, the rationalists argue, the issue

¹ James Fearon, "Rationalist Explanations of War," *International Organization* 49:3 (Summer 1995): 379-414.

² This is not a trivial issues; the alternative is to argue that all wars are caused by irrationality.

³ Fearon, 391-401.

⁴ Fearon, 389-390.

appears indivisible only because a credible commitment is impossible or states have misrepresented their positions.

These theories can be applied to both why wars begin and why wars end. The information approach has dominated the war-initiation research. The commitment problem has been the focus of war termination (n.39, 233). In *How Wars End*, Reiter integrates both approaches to develop a set of novel hypotheses about how the mechanisms interact in war termination. The indivisibility logic is subsumed under these approaches (48-49).

Goemans, one of the reviewers, created another rationalist theory that examined the interaction between domestic politics and war termination.⁵ A key mechanism in this theory is “gambling for resurrection.” In Goemans’ argument, leaders in semi-democratic regimes faced with a strong likelihood of losing a war and thereby losing office with potentially fatal results will engage in even riskier military strategies rather than make concessions in hopes that victory will be achieved. In doing so, the leaders also increase the chances of an even worse military defeat, hence the gamble. Goemans used this mechanism to explain German escalation in Feb. 1917 and March 1918. Reiter makes the case for the credible commitment being crucial in chapter 9.

Absolute Victory, World War I, and Leyte Gulf

Copeland is in agreement with much of *How Wars End*. His criticism is that Reiter does not take the argument far enough. Consistent with his own research, Copeland argues that commitment problems as a cause of wars should be incorporated into explanations of war termination (5).⁶ In particular, preventive wars occur because a rising power cannot credibly commit to respect the interests of a declining power. The challenges of making credible commitments only intensify because of the animosity generated in the course of the war, Copeland argues, although strong rationalist explanations would not rely on affect (or emotions) to explain war termination dilemmas. Copeland suggests that the severity of the credible commitment problem explains why four wars (U.S. Civil War, 1865; Germany, 1918; Germany, 1945; Japan, 1945) ended in near-absolute victories rather than more equitable, negotiated settlements (6-7). If credible commitments were easier to achieve, then negotiated settlements would be easier to reach. However, the pursuit of absolute victory may have another cause. In the U.S. Civil War, Copeland states that the war aims—an independent Confederacy and a stable Union—were incompatible (9-10). Rather than being an issue of credible commitments, this suggests that the issue at stake was indivisible.

Goemans has more pointed differences with Reiter’s argument. Reiter suggests that imposing a new government, or “foreign-imposed regime change” (FIRC), can resolve the

⁵ Hein Erich Goemans, *War and Punishment: The Causes of War Termination and the First World War* (Princeton University Press, 2000).

⁶ Dale C. Copeland, *The Origins of Major War* (Ithaca, NY: Cornell, 2000).

underlying credible commitment problem by replacing the defeated side's leadership with a new, presumably more trust-worthy one. Goemans is skeptical. Confronted with a belligerent pursuing such a war aim, an opponent would have a further incentive to pursue a gamble for resurrection rather than negotiate. The two causal logics are hard to disentangle. A belligerent might pursue regime change because it fears its adversary will not keep commitments made in a negotiated settlement. Alternatively, a belligerent might pursue regime-change as an end in itself, and thereby press its adversary to gamble for resurrection.

Reiter and Goemans also differ over the end of World War I. Goemans acknowledges that his explanation in *War and Punishment* for German persistence after Brest-Litovsk is strong on logic but weak on evidence; however, Reiter's explanation of German strategy suffers the same flaw, he argues (3). Reiter argues that German insistence on retaining Belgium was due to fear of Anglo-French defection from any post-war settlement (Reiter, 171-172). Goemans claims that German army officials did not consider post-war British military capabilities in their discussions, and that much of the focus on Belgium reflected the parochial views of German naval officers.

Shirkey praises Reiter's theoretical contributions, particularly his integration of the information-based and commitment-based explanations. In particular, Shirkey points out that, in contrast to an information-based model, Reiter finds that states do not change their war aims as more information becomes available: the winning power does not usually increase them and the losing side fails to decrease them. On the historical cases, Shirkey finds Reiter's account of German policy in World War I more persuasive than Goemans does, but Shirkey is less convinced by Reiter's account of Japanese decision-making in World War II. He argues that the Leyte campaign appears to be a gamble for resurrection (5-6). Shirkey also suggests that a different set of cases, ones concerning limited wars, might not yield greater support for the information model than cases focusing on general wars. Given his own combination of statistical and case-study work, Shirkey is skeptical of Reiter's argument that creating a database for a statistical examination of the credible commitment problem is too difficult.⁷

In the author's response, Reiter engages all these points in turn. For historians of the periods, his discussion of German policy in World War I and Japanese strategy toward the end of the World War II should be of particular interest. Methodologically oriented political scientists may be interested in Reiter's strong defense of case-studies over statistical methods for his research of war-termination. Overall, the participants have produced an engaging roundtable.

Dan Reiter is chair of the political science department at Emory University. He received his Ph.D. in political science from the University of Michigan in 1994, and was an Olin post-doctoral fellow in Security Studies at Harvard University in 1994-1995. He is the author of

⁷ Zachary C. Shirkey, *Is This a Private Fight or Can Anybody Join? The Spread of Interstate War* (Burlington, VT: Ashgate, 2009).

several scholarly articles, as well as *Crucible of Beliefs: Learning, Alliances, and World Wars* (Cornell, 1996), *Democracies at War* (Princeton, 2002; coauthored with Allan Stam) and *How Wars End* (Princeton, 2009). He won the 2002 Karl Deutsch Award given by the International Studies Association to the leading international relations scholar under the age of 40 or within ten years of receiving his or her Ph.D. *How Wars End* won the 2010 American Political Science Association Award for Best Book in Conflict Processes, and was recognized in January 2011 by *Choice* as an outstanding academic title of the year.

Christopher Ball is a list and reviews editor for H-Diplo and was a commissioning editor for the ISSF Series. He has taught political science at New York University, the University of Iowa, Johns Hopkins University, Iowa State University, DePaul University, and Loyola College Chicago. He currently resides in Chicago.

Dale Copeland is an Associate Professor at the University of Virginia. His research focuses on International Relations Theory (security studies and international political economy), and he is the author of *The Origins of Major War*, (Cornell U.P., Cornell Studies in Security Affairs, 2000), a study of the link between the rise and decline of great powers and the outbreak of devastating system-wide wars. His second book project, *Economic Interdependence and International Conflict*, examines the conditions under which interstate trade will lead to either war or peace. Other research interests include the origins of economic interdependence between great powers, the realist-constructivist divide, in-group/out-group theory and the logic of reputation-building, and the interconnection between international political economy and security studies.

Hein Goemans is an Associate Professor of Political Science at the University of Rochester. His book *War and Punishment* was published by Princeton University Press (2000). A second book *Fighting for Survival: Leaders and International Conflict* with Giacomo Chiozza (Vanderbilt) was published by Cambridge University Press (2011). Goemans is currently engaged in two research projects. The first extends the research in his first book on the causes of war termination and examines the role and incentives of leaders in international conflict initiation. His second research project explores when and why people become attached to specific pieces of territory that together constitute a "homeland," and the consequences of these attachments.

Zachary C. Shirkey is an Assistant Professor at Hunter College, CUNY where he teaches in the Department of Political Science. He has published in articles in the *Journal of Peace Research* and the *Journal of Theoretical Politics*. His book, *Is This a Private Fight or Can Anybody Join?* (Ashgate, 2009), examines the role revealed information plays in third party decisions to join ongoing wars.

Dan Reiter's new book offers a powerful and convincing explanation for one of the long-standing issues in international relations: the termination of wars. Building on the work of James Fearon,¹ Reiter argues that wars will end when two conditions are met: 1) the belligerents have good information on the relative balance of power and resolve; and 2) both sides have confidence that the other will not violate any peace agreement forged in the midst of conflict. The book does an excellent job demonstrating that these two conditions (particularly the latter) not only were critical to the way the key wars of the twentieth century ended, but that they trumped many of the factors emphasized by traditional war-termination scholars, including regime-type, ideology, and the personalities of leaders.

I may not be the best person to critique this book, since there is so much in it with which I agree. Reiter's theoretical and empirical chapters align nicely with an overall world-view that I am sympathetic to, namely, that uncertainty and fears of the future not only cause wars but ensure that they will continue until these concerns are overcome or mitigated. The historical chapters confirm that the second of the above two conditions is the hardest one to meet. The first condition is usually met early in a war. Wars by their very nature reveal the relative strengths of the two sides' militaries and the willingness of actors to fight even when costs and risks are high. Reiter thus finds that efforts to end wars are usually impeded by the commitment problem that underpins the second condition. If actors have good reason to believe that the other will violate a war-terminating agreement in the future, they have more incentive to continue the war and to seek an absolute victory. Across the variety of cases Reiter studies, including World Wars I and II in Europe, the Pacific War, the Soviet-Finnish conflict of 1939-45, and the Korean War, concerns about the future intentions and power of the other side are generally far more important in explaining the continuation and ultimate termination of wars than any other single factor, including levels of information regarding the current military balance of power and the other's resolve to fight (his "information dynamics").

This is a very important finding. It backs up recent research emphasizing that wars can break out and continue even when leaders have complete information about the other side character and strength.² Reiter's book thus rights the balance within the growing literature on bargaining and war, which up until now has largely focused on problems of private information and incentives to misrepresent one's current power and resolve. This literature has assumed that if actors had solid information about their present situation, a bargaining space would open up and both sides would see that there are a number of peace deals that are preferable to a costly war. Reiter's book is the first full-length empirical study

¹ James D. Fearon, "Rationalist Explanations for War," *International Organization* 49:3 (1995): 379-414.

² Dale C. Copeland, *The Origins of Major War* (Ithaca: Cornell University Press, 2000); Robert Powell, "War as a Commitment Problem," *International Organization* 60:1 (2006): 169-203.

of this claim, and what he finds is quite fascinating. In many situations, a state that discovers after the war starts that it is less powerful than it had believed — what he calls "discouraging information" — will often maintain its war demands or actually increase them, rather than reducing them as a pure informational approach would expect. This was the case for both the Confederacy and the Union (at varying times) during the U.S. Civil War, the Germans during World War I, the British versus Nazi Germany in 1940, Japan after mid-1942, the Americans during 1942 and in 1950 during the first three months of the Korean War. Because of the commitment problem, actors facing discouraging information decided to fight on despite great costs in the hope that they could pull out a victory that would prevent future attacks on their territories.

Even in cases where information dynamics did help to finally resolve the conflict, they usually played largely a supportive role. Discouraging information did sometimes push an actor to seek a peace, as when Finland sought to make a deal with Stalin in 1940 and 1944 after costly and inconclusive wars with the Red Army. Such behavior is consistent with bargaining hypothesis that information that is discouraging should make an actor reduce its war demands. Yet as Reiter's evidence shows, there are still problems with the bargaining argument. Because the state that finds itself doing better than expected on the battlefield is likely to *increase* its own war demands, it will still be hard to find a peace deal that the discouraged party can live with, especially if it fears that the stronger state will pocket the benefits of any peace and attack later with more strength.

Information that creates greater pessimism about one's relative power only seems to push states into a peace deal when no side sees a hope for absolute victory or the costs of achieving such as victory are prohibitively high (213-15). Such was the case for Finland and Russia by 1944 and the North Koreans and the United States by 1951-53. Note what is happening here, however. The parties are not seeking peace because they have full information about power and resolve. They are seeking peace because they have no way to solve the commitment problem through absolute victory and thus realize that an uncertain peace (i.e., one which the other could violate) is better than the certain destruction of their power bases over the long term. In short, the actors moving toward peace are doing so only because they cannot solve the commitment problem via war. Thus, they recognize that they must 'coexist' with the other in peace. The Korean War went on for two more years after Washington first saw the impossibility of a low-cost victory, demonstrating that information dynamics play only a minor role in war termination (by setting a permissive condition for peace that might allow two actors to reach a deal). The fact that most of the wars Reiter studies come to an end only through absolute victory and not through give-and-take bargaining shows just how difficult it is to overcome the commitment problem.

I have few concerns with the evidence Reiter marshals in support of his argument. His analyses of both secondary sources and available documents are even-handed and solid. Throughout the book, but especially in the chapter on World War I, he offers a powerful critique of the domestic-level argument that authoritarian leaders will often increase their war efforts in a hope that they can pull out a victory and avoid overthrow (the 'gamble for

resurrection' thesis).³ Even in the best case for this thesis, the ending of World War I, Reiter shows that strategic concerns, especially commitment problems, were more determinative of German decision-making in 1916-18 than any hope that a victory would save the skins of the leaders.

Because I find Reiter's overall argument and evidence convincing, I will not offer the typical critique of a book of this sort, namely, one that challenges the author's deductive logic or points out flaws in the historical interpretations. Rather, for the rest of this essay, I will focus on one important dimension of the war literature that is severely underplayed in the book, namely, what any particular war is about, why it started, and for what objectives. Clausewitz famously pointed out that war is policy by other means, that is, war is fought for certain ends that cannot be achieved through peace. In a strict Clausewitzian sense, if we want to know why actors continue to fight long and bloody wars, we should understand what got them into the wars in the first place. If one side or both sides have not attained the objectives that pushed them into the war, then they are likely to keep fighting until the objectives are either met or have become unreachable through war.

This view suggests that the causes of war termination cannot be separated from the causes of war itself. In chapter two, Reiter does offer a few suggestions on how wars begin, suggestions that revolve around the informational side of his overall thesis. Consistent with the first part of Fearon's 1995 article,⁴ Reiter argues that wars occur because leaders lack information about the other side's quantitative military power, its technology and strategies, its resolve to fight, and the resolve and power of third parties. In an environment of information deficits, leaders may start wars because of their overconfidence regarding the likelihood of success and the benefits and costs of war itself (10-14). Oddly enough, despite the overriding importance of the commitment problem to Reiter's empirical understanding of why wars end or do not end, he does not talk about the commitment problem as something that *causes* wars to occur. (His chapter on credible commitments focuses solely on war termination.) As Powell's and my own work have shown, large and costly wars can occur even in situations of complete information about the present power and the resolve of others when actors are worried about declining power trends and the future intentions of adversaries. In such situations, there are strong incentives to launch preventive wars to eliminate or reduce the rising actor before it is too late.

Commitment problems are part and parcel of the dilemma declining states face. The rising state may be more than willing to promise to be nice later, once it has grown in power, but the promise lacks credibility. There is nothing to stop this state's leaders from changing their minds in the future. And even if these leaders are known to be reasonable now, they may be out of power in five or ten years, and those replacing them may not be so moderate. Declining states thus have a good reason to worry about the future intentions of rising

³ Hein Goemans, *War and Punishment* (Princeton: Princeton University Press, 2001).

⁴ Fearon, "Rationalist Explanations for War".

states, regardless of any stated commitments to long-term peace. This simple fact means that preventive motivations may be a very important and pervasive cause of war in world history.⁵

Reiter does not discuss this possible explanation for war, but it may be critical to understanding exactly why the costly wars in his empirical work were so hard to end, and why four of his six cases (the U.S. Civil War, 1865; Germany, 1918; Germany, 1945; Japan, 1945) ended not through negotiation but only through the collapse and surrender of one side. If the war begins because one side needs to make major gains to offset what would otherwise be a severe decline in national power, then that actor will likely continue to press on in the war even when it is going badly. After all, the primary objective in the war — the stemming of power decline through territorial victory — has not been achieved, and a return to a form of the old status quo would only put the declining state back into its original dilemma.

Consider an alternative explanation for why some of Reiter's wars went on for so long and then ended as they did. If Germany initiated World Wars I and II at least in part out of fear of the rise of Russia and (more distantly) the United States — two nations with much larger populations and territorial bases and thus much greater potential for long-term growth⁶ — then war termination was extremely difficult from the get-go. German leaders felt they needed to gain access to territory, resources, and markets to offset the potential power advantages of the rising actors. Without these strategic gains, decline would have continued. Commitment problems were critical to the start of these wars, since the rising states could not credibly promise to be nice later once they became the dominant states in the system. Such commitment problems became even more problematic once the wars began, given the hatred and mistrust caused by the wars themselves and the fact that the allies fighting Germany had little reason to believe any promises emanating from Berlin.

Likewise, the Japanese leadership started the Pacific War in 1941 in large part due to fears of declining power, fears exacerbated by the American embargo on oil and raw materials. From the perspective of Japanese officials in late 1941, unless Japan was able to gain control of Southeast Asian resources, the nation would become vulnerable to attacks later, either from the United States or the Soviet Union. Once the war was underway, Tokyo could not afford to make a peace that required the relinquishing of its economic empire. As with the German examples, it was hard for Japan to negotiate a peace when its fundamental strategic objectives for starting the war had not been secured.

There is very little discussion in Reiter's book about the initial war aims of the Germans and Japanese in the two world wars. Thus the reader has little sense of how these aims

⁵ Robert Jervis, *American Foreign Policy in a New Era* (New York: Routledge, 2005); Jack S. Levy, "Declining Power and the Preventive Motivation for War," *World Politics* 40:1 (Oct. 1987): 82-107; Copeland, *Origins of Major War*.

⁶ Copeland, *Origins of Major War*.

might have exacerbated the commitment problems that were so critical to continuation of these horrendous conflicts. More specifically, if these wars were not the result of information deficits but of fears of the future, then the commitment problems that underlay their beginnings (mistrust over the future intentions of adversaries) would only have gotten worse once the wars got underway. This is not to say that Reiter is wrong, only that he has missed something that would have reinforced the power of his primary explanation. The commitment problem did indeed keep these wars going for far longer than a simple informational approach would have expected. But to fully understand why both sides sought absolute victory, one has to go back to why Germany and Japan had such expansive aims from the start and how that exacerbated fears of making deals that might be violated in the future (the allies) or that would not provide enough new territory to overcome decline (Germany, Japan).

A quick glance at the U.S. Civil War suggests that initial war aims were also critical to the prolongation of the conflict and the need for absolute victory. The South wanted one thing: to have an independent country that could protect its peculiar institution, slavery, against perceived northern attempts to undermine it. The North needed absolute victory in order to avoid weakening its long-term position on the continent. Given these aims, it was evident from the start that even once the relative power of both sides was made clear, war would continue until one side conceded. Understanding the reasons for the war is again thus critical to seeing why it was almost impossible to end the war through intra-war bargaining.

To sum up, Reiter has provided the field with a book that will define the debate on the termination of wars for many years to come. Through solid empirical evidence, the book challenges the view that domestic motivations of leaders, particularly authoritarian leaders, interfere with the effort to find a peace that both sides can live with. Despite the fact that the belligerents soon become aware of the true balance of relative power and resolve, wars can continue for years and at the cost of millions of lives, precisely because leaders have a hard time making their commitments to long-term peace credible. If either side believes that peace deals will be violated later when the other has more power or an interest in renewed warfare, then a cessation of war will be difficult to achieve. Only if states believe they have almost no chance of victory or that victory will only come at a huge cost will they seek a deal that avoids further bloodshed.

We have seen that this highly plausible explanation for war continuation and war termination can be supplemented by a return to the causes of particular wars. If wars are driven by fears of relative decline and the inability of rising states to commit to being cooperative later, then the commitment problem is doubly problematic once a war begins. The state most responsible for starting the war is likely to make large territorial demands, since it needs to find a way to stabilize its long-term power position. This will increase the mistrust of others, who will be unwilling to make concessions in the short term for fear of only fueling the aggressor's ability and desire to expand down the road. Yet without concessions, the aggressor will be unlikely to agree to a peace that will only return it to an unacceptable status quo. If Clausewitz is right, therefore, the policy objectives that drove a state into war will continue to animate its war-time decision-making and its willingness to

make a deal that gets it out of war. Wars often end only with absolute victory precisely because peace deals usually do not give the aggressing state enough and because those offering the concessions have reason to believe the other will pocket the gains and then ask for even more. Commitment problems underpin all of this. But the pre-war power trends and the pre-war distribution of territory and raw materials have a lot to do with why such commitment problems are so hard to solve.

The advent of the so-called bargaining model of war seemed to provide the long-sought firm foundations for the study of international conflict.¹ And indeed many scholars of International Relations now take the bargaining model of war as their starting point for their analyses. But the causal mechanisms of this approach have proven remarkably difficult to trace historically. Variables such as private information, incentives to misrepresent, expectations about future growth and exogenous shocks as well as provision of credible commitments and “indivisibility” require careful empirical operationalization and in-depth historical scholarship, often taking many years of study, to trace.² To my taste this is particularly problematic in the case of explanations for war that rely on the commitment problem. If war is caused by a commitment problem, then how *exactly* does war solve the commitment problem?

Dan Reiter’s new book *How Wars End* takes on these fundamental issues with much theoretical creativity and originality and carefully crafted case studies of no fewer than twenty-two wars. The book made me think, and sometimes re-think some of my understanding of recent wars, and the First World War in particular. While there is much I agree with in Reiter’s new book, there are of course also areas of disagreement. Because I assume every reader of these essays will have read *How Wars End*, I will skip a restatement of the book’s central claims, and jump directly to a couple of issues that in my view merit further discussion.

First among these issues is my concern with the argument that “foreign-imposed regime change” (FIRC) can solve a commitment problem by making it credible, or perhaps just more credible, that a new regime will abide by the war-ending agreement (26). This may actually be empirically supported, as Reiter reports some striking statistics, but it nevertheless raises some puzzles. Prominently among these, in my view, is that if a leader or a regime knows that the opponent seeks to overthrow the regime – and perhaps seek subsequent retribution – the leader and the regime have no incentives to accept such a deal and instead will be more or less forced to fight on – wait for it – in a gamble for resurrection. Since both sides must accept a war-ending bargain, it becomes difficult to disentangle these two distinct mechanisms. Moreover, why would one side make a demand so extreme that the other cannot accept it, and thereby ensure the war will last longer and be more costly? Why not seek assiduously, most likely with the help of third parties, to craft a deal that is enforceable, but gives the opposing leader and regime a way out. As Thomas

¹ James Fearon, “Rationalist Explanations of War,” *International Organization* 49:3 (Summer 1995): 379-414; Robert Powell, “Uncertainty, Shifting Power, and Appeasement,” *American Political Science Review* 90:4 (Dec. 1996): 749-764; Hein Goemans, *War and Punishment: the Causes of War Termination and the First World War* (Princeton: Princeton University Press, 2000); R. Harrison Wagner, “Bargaining and War,” *American Journal of Political Science* 44:3 (2000): 469-84; Dan Reiter, “Exploring the Bargaining Model of War,” *Perspectives on Politics* 1:1 (March 2003): 27-43.

² Ron E. Hassner, *War on Sacred Grounds* (Ithaca: Cornell University Press, 2009).

Schelling pointed out long ago, giving your opponent no options but to fight on can be very counterproductive.³ I would have liked some more discussion, both theoretically and empirically, of the conditions under which FIRC is the cheapest and most effective option, especially since it is unclear why the new regime will stick by the war-ending agreement and why it can safely be assumed that the new regime will survive in office and not be replaced by another hostile regime. I am thinking here of the new German regime after World War I and how it was replaced by Hitler. The point being that most of Germany's core strengths were relatively unaffected by Versailles. I recognize that this would require extensive treatment and perhaps would distract from the overall argument of the book, so this is not so much a complaint as an issue of further study.

My second main issue revolves around a case I know well: the termination of the First World War. As Reiter rightly points out, the question why the Germans decided to fight on into 1918 after they made enormous gains at Brest-Litovsk remains a major puzzle. I have argued that Germany fought on because gains exclusively in the East would destabilize the coalition of Iron and Rye, as well as the Federal balance.⁴ Manufacturers and industrialists, as well as western German states such as Bavaria and Wurttemberg, I have argued, insisted on gains in the West to maintain a balance of power with their fellow oligarchs, the Junkers and agrarians, and Prussia, in common opposition to the Social Democrats. As I noted in my book, and admit here again without hesitation, this reasoning fits the main line of my theory, but the evidence is thin. Indeed, I remain puzzled by the German decision to fight on and launch the Spring Offensives of 1918, rather than take their gains in the east to buy off the rising Social Democrats and broader population. That said, I also find Reiter's claims that the Germans decided to fight on because they feared that Britain would not stick to any war-ending deal in 1917 unconvincing in the end. One concern is that Germany's supposed worries about a next war probably should have manifested themselves early, but there is not much evidence to support this. Any successful war – from Germany's perspective – would have fundamentally changed the balance of power on the continent; indeed it has been argued that continental war and dominance was Germany's real objective. Such an outcome, inevitably, would have weakened Britain's position. So Britain from the very beginning should have been willing to prevent this and unwilling to abide by any peace terms that changed the continental balance in Germany's favor. But in my studies of German and British strategic thinking and decision-making, I found little evidence to support it. Then again, it might be argued that the enormous German gains at Brest-Litovsk fundamentally changed British calculations and there is indeed good evidence to suggest that the British did not think this could stand, as the balance had shifted too much in Germany's favor. But I have found no evidence that the British were seriously considering a 'next war.' Such an idea would have faced probably insurmountable odds: no Russian front to soak up large amounts of German manpower all of which could have been stationed in a defensive posture – which three long years had convincingly shown to be extremely difficult, if not well-nigh impossible, to surmount – on the Western Front. While the

³ Thomas C. Schelling, *Arms and Influence* (New Haven: Yale University Press, 1966).

⁴ Goemans, *War and Punishment*.

British might have been able to rely on the French, it is unlikely in the extreme that the Australians, the New Zealanders, the Canadians, the South Africans and the other British colonies would have been willing to supply the manpower for an offensive war on the continent, after their bloody experiences on the Western Front. I would even go so far as to argue that the British population itself would have balked at such a war. Simply put, the British might have been willing to fight on after 1917 because *they* faced a commitment problem: if allowed to absorb the gains of Brest-Litovsk, Germany would have become too strong. While the British probably did fear a second war initiated by *Germany*, they were not planning to initiate a future offensive war against Germany. And this, to me, seems to be the crux of the matter. Ludendorff, Hindenburg, and especially high-ranked members of the Navy may have talked about “the next war” but I saw no evidence that the Germans did any analysis of the British post-war capabilities if a war-ending agreement was signed in 1917. The ‘next war’ some Germans talked about, I believe, would have been another war initiated by Germany to consolidate its gains. To fight on into 1918 because of notions that Britain might start a new war in the future, truly sounds like ‘suicide for fear of death.’ Germany undoubtedly would have been much stronger in a next war after it put its gains to good use, both in domestic political terms, and in terms of strategic and industrial capabilities.

Many, but by no means all of the quotes provided by Reiter in his support for the argument that German leaders feared a next war launched by the British, stem from Naval officers, supporters of the Navy such as Alfred von Tirpitz, the intellectual force behind the German naval build-up, the so-called risk-fleet and a co-founder of the Fatherland Party and politically closely allied industrialists. Their demands for the Belgian coast although sometimes undoubtedly sincere, also fit with the need to claim a post-war powerbase and political relevance for the Navy. After all, the much-vaunted and incredibly costly surface fleet had delivered none of the promises its pre-war advocates had made. If this thesis holds water, it moves our attention back again to worries about post-war domestic political power. I believe such considerations may well deserve more detailed analyses.

All in all, though, like Reiter, I remain puzzled by the German decision to gamble everything on the 1918 Spring Offensive in the West. Whereas Reiter sees the solution in the strategic interaction between Germany and Britain, I would emphasize the domestic political strategic interaction in Germany itself. As is probably most likely, several factors played a role, as Stephen Bailey concludes (260, note 109). Reiter has provided an alternative argument and in the process opened up new questions and lines of thought that definitely deserve more thought and scholarship. That is the hallmark of good work in political science.

Dan Reiter's *How Wars End* makes significant contributions to the study of war termination. It highlights the role commitment problems play in extending the length and potentially the severity of wars. Reiter builds on the rationalist bargaining framework of war most famously presented by James Fearon, who argues that private information, commitment problems, and indivisibility are the three rational causes of war.¹ If the bargaining framework of war is correct—and it has become the dominant approach for rationalist international relations scholars—then it should be able to explain not only war initiation but also how wars are fought and why they end. Reiter shows that the revelation of private information, the portion of the bargaining framework most explored by scholars so far, alone cannot explain why and when many wars end. His case studies show not only that commitment problems are a significant factor in the decisions of leaders regarding war termination, but also that states' behavior during wars is consistent with the bargaining framework—provided both revealed information and commitment problems are considered. *How Wars End* is arguably one of the most lucid and helpful books on war termination using the bargaining framework currently available.

The book's great strength is that it blends the informational and commitment problem aspects of the bargaining framework together to explain war termination rather than simply focusing on the role of commitment problems. The informational portion of the bargaining framework, of course, argues that states will strike bargains to end wars or at least make serious attempts to do so after information that was private or unknowable antebellum, such as the relative balance of forces, is known. This is because wars should reveal information about the relative balance of forces, the resolve of the warring states, and the prospects of third party intervention thereby allowing overlapping bargaining ranges to form and be discovered. Yet, much of the newer rationalist literature has begun to focus on commitment problems as a cause of war as it has become clear that the informational approach is insufficient by itself. However, too little of the literature tries to explain how private information and commitment problems work together and instead most works focus on one to the exclusion of the other. In his focus on commitment problems, Reiter emphasizes their role, alongside but not instead of, the widely accepted importance of information.

Specifically, Reiter argues battles should reveal the relative balance of forces fairly quickly. Many wars, however, continue long after the balance of forces seems quite clear, thus the informational portion of the bargaining framework is insufficient by itself. Reiter deftly shows not just that states often fail to make peace after a great deal of information has been revealed, but that many times they do not even change their peace offers. This is crucial as the informational hypothesis simply suggests states take revealed information into account and adjust their bargaining positions accordingly. It does not claim that a bargaining space will always open up because as Donald Wittman argues, one side may up its demands by

¹ James Fearon, "Rationalist Explanations of War," *International Organization* 49:3 (Summer 1995): 379-414.

more than the other side lowers its own demands.² Reiter's care in showing that states do not alter their demands and sometimes do not even make peace offers of any sort neatly avoids this potential pitfall. For example, during the U.S. Civil War neither the Union nor the Confederacy advanced peace offers after pivotal battles such as Gettysburg. Reiter asserts this is not because leaders are unaware of or irrationally ignoring revealed information, but rather they continue to fight in hopes of overcoming commitment problems. States often pursue absolute victory (annexation, regime change, or theoretically genocide) in order solve commitment problems and thus will not accept lesser offers as long as they believe they can achieve absolute victory at a reasonable cost.

As a whole, the book provides more evidence that states do generally behave during wars as the bargaining framework would expect. This is vital if we are to have any confidence that such a framework can explain more explored topics such as war initiation. Indeed, Reiter is not alone in applying bargaining to areas outside of war initiation. For example, the bargaining framework may help explain third party intervention in ongoing wars. Like Reiter's work, my own research has suggested that surprising battlefield results are good places to look for revealed information and that difficulties created by commitment problems can lead to conflict—in this case between an initial belligerent and a third party. All of this suggests that if the bargaining framework can help us understand a variety of concepts such as war initiation, intervention, and war termination, it should be possible to use it as a more general starting point to explore broader concepts. As the bargaining approach was in part borrowed from economics, this unification of explanations is already underway. At a minimum it should allow conflict scholars to explore inter- and intrastate wars with the same tools. It potentially offers a way forward for more general theorizing in international relations and growing connections between the various subfields of political science.

The book's commitment problem hypothesis is tested against domestic politics explanations as well as against the informational approach. Studies of war termination often incorporate democratic war-weariness and gambling for resurrection in mixed regimes as explanations of war duration. In his cases, especially that of Finland, Reiter finds no support for the notion that democratic publics become weary of war more quickly than publics in other regime types. Certainly, his cases are sufficient to lead one to question the widely-held belief about democratic war-weariness, though whether these cases are simply outliers or part of a broader trend is less clear given the lack of statistical analysis. With regards to the gambling for resurrection hypothesis—the belief that leaders in mixed regimes will take gambles which risk severe defeat in order to avoid modest defeats because any defeat will result in severe punishment for the leadership—Reiter's cases are potentially more telling. He tackles cases in which the gambling for resurrection hypothesis should work very well, including the case which Hein Goemans used to build his

² Donald Wittman, "How War Ends: A Rational Model Approach," *Journal of Conflict Resolution* 23:4 (December 1979): 741-61.

theory—that of Germany in the First World War.³ Thus, in some sense they are so-called critical cases. In the German case, he offers solid evidence that the German leadership failed to negotiate for peace in 1917 not because of domestic concerns, but because of commitment problems, particularly that without annexing Belgium the threat posed by Britain and France was too severe for German leaders to accept. He offers similar evidence that the Japanese leadership during the Second World War continued to favor war out of concerns about commitment problems—specifically that the United States could not credibly commit to not destroy the Japanese polity and remove the emperor—and not due to worries about domestic upheaval. The concerns that the leadership did have about domestic opinion inclined them to end the war rather than prolong it. The book, however, goes further and argues that the Japanese never engaged in strategies which could be seen as gambling for resurrection.

While Reiter makes a compelling argument that from a regime standpoint kamikaze attacks were an effective strategy and also low risk, his failure to discuss the Leyte campaign as a possible gamble for resurrection is troubling. The Japanese intentionally sacrificed several aircraft carriers, essentially offering them as bait to lure the main American naval force away from Leyte Gulf, so that other Japanese forces could attack the American landing vessels and troops ashore without serious opposition. Failure risked essentially the elimination of the Japanese fleet as a serious factor in the war. This is in fact what happened; however, if the plan had worked, it would have been a serious setback for the United States. Whether it would have created an opening for the Japanese to negotiate something less than unconditional surrender is less clear, but it would have at least increased the odds that they could have obtained more favorable terms. Certainly the campaign seems like the exact sort of high risk, high reward action that the gambling for resurrection hypothesis would predict. Even if it could be shown that the Japanese engaged in the Leyte campaign for reasons other than those suggested by the gambling for resurrection hypothesis, any argument claiming they did not gamble for resurrection at least has to consider the Leyte campaign. It seems unlikely that a final verdict on whether mixed regimes gamble for resurrection has been reached.

Outside of this small concern about the book's domestic politics arguments, there are some weaknesses that relate directly to the bargaining framework as well. Specifically, the very nature of selecting cases that show the importance of commitment problems, results in cases that underplay the role of information in war termination. It would be interesting to know if shorter wars or wars where neither side seriously considered pursuing absolute aims, such as the First Gulf War or the Seven Weeks War, are better explained by revealed information than by commitment problems, or if the commitment problems were simply quickly resolved in such wars. While Reiter's cases make it clear that both revealed information and commitment problems are vital factors in war termination, what is lacking is a sense of the proportions of the overall population of wars in which commitment problems are the more important factor and in which informational uncertainties are more

³ Hein Goemans, *War and Punishment: the Causes of War Termination and the First World War* (Princeton: Princeton University Press, 2000).

important. Another implication of Reiter's argument is that commitment problems should make wars last longer unless one side can win an absolute victory very quickly or realize that such a victory is impossible. While the cases support this notion, some statistical work showing whether longer wars are associated with commitment problems would be more compelling.

Reiter, however, reaches his conclusions using only case studies. Objectively recording events within wars which reveal information is challenging and becomes more so if those events must be reduced to numbers. Specifically, Reiter argues that creating a statistical database is very difficult given uncertainties such as how often leaders incorporate new information, how well they understood what a battle meant at the time, whether different actors see the same information the same way, a lack of consistent data across wars and even within wars, and the lack of clear events in guerilla and even some attritional wars. To these I would add the pitfall of hindsight which may make an event look more or less important to scholars than it did to contemporaries. Reiter argues these challenges simply cannot be overcome and thus relies on cases. Decisions about what should be seen as an event, what time period should be used to record events (days, weeks, or months for example) and so on can become arbitrary and if the idea is to show how leaders respond to events, process tracing may well be preferable. While such an approach is useful, and Reiter's cases are compelling, the belief that coding and statistical work are impossible to undertake in relation to commitment problems and revealed information is too strong. Certainly, these tasks would be very difficult and subject to bias and other pitfalls. The potential benefits, however, would make the effort worthwhile. Such a dataset would allow scholars to make at least limited inferences about the broader population of wars— inferences that may be difficult to draw solely from case studies. Reiter is likely correct that such statistical work would never be compelling on its own and would always have to be paired with cases that engaged in process tracing, but this does not mean the effort would be in vain.

More problematic is that Reiter underplays some ways in which information can play a role in war termination. While battles should reveal the current relative balance of forces fairly quickly, this may not answer all the questions about how a war will proceed. For example, if there is significant uncertainty about the belligerents' relative resolve, simply knowing the balance of forces is not enough to create a bargaining space. This is because questions about how much pain each side is willing to endure may remain unanswered well after it is known what the results of most battles will be. For example, in the Vietnam War, while there likely were commitment problems in play, questions about the relative resolve of the United States and North Vietnamese governments were critical as well. These questions were not answered as quickly as questions about the relative balance of forces and each side's ability to inflict pain on the other. Similarly, informational questions about whether an attrition strategy will succeed may take a while to be answered, not only because it may not be obvious even after the first battles which side has the better industrial base, but also because there may be uncertainty about whether the other side's maneuver strategy will prevail first. This sort of uncertainty is directly applicable to Reiter's case on German decision-making in 1917 and 1918. Reiter shows that despite years of deadlocked battles, the Germans and Western powers could not reach a peace agreement due to commitment

problems. However, Reiter downplays a major piece of uncertainty which existed in late 1917 and early 1918: whether or not the German Spring Offensive, using new tactics and troops freed from the Eastern Front by the Russian surrender, would be able to overwhelm the British and French before the American forces would be available in large numbers. Thus, while the battles of 1914 through 1917 made it clear a replay of those battles would result in continued stalemate, the battles in early 1918 were not to be a replay as the relative balance of forces on the Western Front had changed and the tactics used would be different. Thus, though information had been revealed, significant uncertainty remained. Indeed the surrender of Russia, the creation of new tactics, and the entrance of the United States in the war had created new elements of uncertainty. Thus, while battles and other elements of wars reveal much information, significant uncertainty may remain in no small part because new uncertainty may be created. These sorts of informational questions have been seriously understudied in general and their omission is hardly specific to Reiter's work.

On the whole, however, these are minor problems. The first problem is more of a suggestion for future research, and the second suggests that the way we think about the revelation of private information needs to be expanded rather than suggesting that Reiter is wrong about the importance of commitment problems. The work is a valuable contribution to the conflict literature as a whole and war termination specifically. It is very readable—something not always true for important research in the rational choice vein. Most importantly, this clarity does not come at the price of sacrificed detail or nuance. Thus, the book makes a significant contribution to the literature upon which others can and should build and expand. I suspect it will be widely read and have a significant impact on conflict research in the years to come.

Author's Response by Dan Reiter, Emory University

My thanks to Zachary Shirkey, Hein Goemans, and Dale Copeland for giving my 2009 book, *How Wars End*, such careful reads. I could hardly ask for three better scholars to reflect on the book, given Shirkey's 2009 book on war expansion and his efforts to code surprising intrawar events, Goemans' 2000 book on war termination, and Copeland's 2000 book developing commitment-credibility concerns as a cause of war.¹

How Wars End aimed to accomplish three main tasks. First, it generated a new theory of war termination, drawing on the commitment-credibility insights developed in scholarship on the causes of war. Second, it tested an information perspective on war termination, that the ebb and flow of war termination diplomacy should follow battle outcomes and other intrawar developments. Third, it explored specific historical puzzles, such as why Britain decided to fight on in late May 1940, why the U.S. Civil War lasted so long, and why Germany did not settle for peace in early 1918.

I greatly believe that scholarly knowledge is advanced when scholars directly address each other's theoretical and empirical claims. I personally believe that this is one of the most important services to scholarship that H-Diplo offers, providing a respected forum allowing public and focused debate between scholars. Such exchanges force scholars to make their assumptions more transparent and improve their theoretical and empirical arguments. They also help develop new avenues for research. In the spirit of advancing scholarly exchange, I welcome the opportunity to react to the points raised in the Shirkey, Goemans, and Copeland essays.

Commitment Credibility, War Termination, and War Initiation

Copeland makes a very useful point about the importance of trying to understand both war initiation and war termination with the commitment credibility insight. I began the project recognizing the mountain of fine work already executed by Copeland and others on commitment credibility and war initiation. As noted, one goal of the project was to build on this literature by applying the commitment-credibility insight to war termination. I might add that there is certainly room for developing theories that apply the commitment-credibility insight to both war initiation and war termination in an integrated fashion. In that vein, two coauthors and I have recently published an article containing a formal model that applies the commitment-credibility insight to both war initiation and war termination, building on the ideas developed informally in *How Wars End*.²

¹ Zachary C. Shirkey, *Is This a Private Fight of Can Anybody Join? The Spread of Interstate War* (Burlington, VT: Ashgate, 2009); Hein Goemans, *War and Punishment: The Causes of War Termination and the First World War* (Princeton: Princeton University Press, 2000); Dale C. Copeland, *The Origins of Major War* (Ithaca, NY: Cornell, 2000).

² Scott Wolford, Dan Reiter, and Clifford J. Carrubba, "Information, Commitment, and War," *Journal of Conflict Resolution* 55 (August 2011): 556-579.

Copeland's more specific point is that if a belligerent begins a war motivated by commitment concerns, then war termination may be difficult to manage from the onset of the war. This is one of the central points of *How Wars End*, that if a belligerent has severe commitment-credibility concerns, it will likely have very ambitious war aims, and retain them even in the face of discouraging battlefield outcomes. However, high commitment-credibility concerns at the outset of war do not necessarily guarantee a fight to the finish. That is, starting a war with commitment-credibility concerns may not be a sufficient condition for states to maintain high war aims throughout the war. The book's theory forecasts that even a fearful state commencing a war with high aims may lower its war aims over time, permitting war termination. Sometimes, a belligerent maintains high war aims motivated by credible commitment concerns early in the war, but the emerging prospect of paying unacceptable costs to solve those concerns by fighting to a finish encourages that belligerent to lower its war aims. This was the case for the United States in the Korean War in 1951. In September 1950, the United States elected to overthrow the North Korean government as a means of solving the problem of incredible Communist commitments. By early 1951, though the United States remained concerned about the credibility of any North Korean commitment, it lowered its war aims in recognition that Chinese intervention now made the conquest of North Korea an unacceptably costly war aim. Similarly, the Soviet Union lowered its war aims in the Winter War in the wake of discouraging battle outcomes and the growing likelihood of Anglo-French intervention on the side of the Finns.

Gambling for Resurrection and the Battle of Leyte Gulf

In *How Wars End*, I argue that Goemans' theory of war termination would predict that Japan should have 'gambled for resurrection' in the latter years of World War II. A gamble for resurrection is conventionally understood as a military strategy that increases one's chances of decisive victory in the war while increasing one's chances of decisive defeat and lowering the chances of securing a moderate outcome. Goemans' description in his 2000 book of the German decisions to launch unconditional submarine warfare in January 1917 and Operation Michael in March 1918 are good examples of gambles for resurrection. I argue that Japan did not launch a gamble for resurrection in World War II, and I dismiss three possible Japanese gambles for resurrection, its attempt to secure Soviet mediation, its use of kamikaze missions, and its considered use of biological weapons against Allied military forces and civilians. Shirkey suggests that the Japanese decision to launch a naval attack in October 1944 at Leyte Gulf to thwart the American invasion of the Philippines is a possible gamble for resurrection.

Analyzing the Japanese strategy at Leyte deserves far more complete consideration than I can offer here, but I will hazard to offer a few comments. The implication of the claim that the Leyte strategy was a gamble for resurrection is that absent the attack, Japan might have been able to secure a moderate outcome, perhaps abandoning empire but keeping its polity intact and its territory unoccupied. However, in autumn 1944, in the wake of a long string of American successes, Japan (rightly, in my view) believed that such a possibility was not in the offing, as America was not ready to budge from its public demand for unconditional

surrender (191-2). This distinguishes Japan from a belligerent that could have ended its war on moderate terms. In early 1918, for example, Germany probably could have ended World War I with a moderate outcome, with the Eastern gains provided by Russia's defeat intact and a restoration of some version of the pre-war status quo in the West.³ In the Japanese view, the only hope for securing a moderate outcome was to inflict a decisive defeat on American forces, perhaps encouraging America to abandon its unconditional surrender demand. The Leyte strategy was not a reckless squandering of resources aimed to secure a greater victory at the risk of making decisive defeat more likely. Rather, it was an effort to deploy what dwindling Japanese assets remained to create American vulnerability and inflict damage, in the hope of avoiding absolute defeat.

But was Japan's strategy at Leyte itself excessively risky? Was there an alternative military strategy that posed fewer risks? (Note that we should be careful to distinguish between strategies that are "risky," presenting higher chances of both great success and disaster, from "foolish," incorporating poor tactical decisions or intelligence). One might be tempted to claim that the true low risk strategy would have been for Japan to forego engagement with the numerically superior American fleet, perhaps deploying its vessels in safe harbor to reduce their vulnerability, as Germany did for its surface fleet for most of World War I. However, such an approach would have facilitated the steady American march to the Japanese home islands. Further, the operational goal of the Leyte attack was to curtail the American invasion of the Philippines, and saving the Philippines was crucial to sustaining the Japanese war effort. American aircraft based in the Philippines could threaten Japanese merchant marine vessels carrying raw materials from Southeast Asia to the Japanese industrial base on the home islands, severely degrading Japanese military production and economic activity more broadly.⁴ Hence, it is not clear that forgoing the Leyte attack would have been less risky for Japan.

A different argument might be that the basic goal of engaging American forces was reasonable, but the specific Japanese operational plan of assigning a group of carriers as a sacrificial decoy incurred excessive risk. It is probably not an exaggeration to view the decoy force as a sacrifice, as all four Japanese carriers in the decoy force had constrained fighting power; they were under-manned both in terms of quantity of aircraft and quality of aircrews.⁵ However, dispatching a sacrificial decoy does not render the strategy a gamble for resurrection. The center of a gamble for resurrection strategy is that it contains great risk: the possibility of decisive victory, and the chance of decisive defeat. Hence, an operational plan that presumes the likely sacrifice of a decoy force in either victory or defeat is probably better viewed as costly rather than risky.

³ Goemans, *War and Punishment*, 265-6.

⁴ Kenneth I. Friedman, *Afternoon of the Rising Sun: The Battle of Leyte Gulf* (Novato, CA: Presidio Press, 2001), 30-31.

⁵ H. P. Willmott, *The Battle of Leyte Gulf: The Last Fleet Action* (Bloomington, IN: Indiana University Press, 2005), 72-73.

The other, more general point I make in *How Wars End* is that the particulars of specific military operations aside, Japan's decision-making during World War II does not fit the domestic politics gamble for resurrection narrative laid out in Goemans' book. Goemans' theory forecasts that if a semirepressive, moderately exclusionary regime is losing a war it will gamble for resurrection in the faint of hope of capturing more spoils of war to distribute to leadership supporters, enabling the leadership to remain in power. However, the Japanese leadership had by 1944 reduced its war aims, in ways that reduced material spoils of victory that could be reallocated to leadership supporters. Further, members of the Japanese leadership as early as February 1943 worried that the regime would be threatened if the war *continued*, as the privations of war could encourage popular or Communist uprisings.

Methodology

In *How Wars End* (chapter 4), I argue that for a number of reasons it was essentially impossible to conduct cross-war, quantitative tests of the information perspective of satisfactory internal validity. Shirkey laments my skepticism, proposing that such efforts might be made to work. I certainly hope that I prove to be wrong on this point, and that some day an eager, patient, well-resourced, and energetic scholar tackles the monumental task of systematically coding intrawar events across several wars with sufficient internal validity to permit useful quantitative tests. Perhaps a more promising avenue might be to execute quantitative tests on an individual war. One could build a single war data set of battle outcomes, and better account for the particular context of that war. In insurgency wars like Vietnam, one could focus on month-to-month battle outcomes such as the balance of casualties or population control. In more conventional wars such as the German-Soviet campaign in World War II, one could focus on battle outcomes such as possession of territory. This is an approach I take in *How Wars End* to a limited degree, making use of a previously existing data set of Civil War battles to argue that the ebb and flow of battle fortunes in the Civil War occurs alongside almost no change in the two sides' war termination offers.

Shirkey also broaches the issue of case selection, observing that the analysis would have been enriched by including shorter wars or wars in which belligerents have less than absolute aims. I heartily agree that broadening the empirical base is always helpful, and my hope is that future scholars will apply these or other political science theories to understanding war termination decisions in other wars. I should note that *How Wars End* does include war termination decisions in three wars that ended with limited outcomes: the Korean War, the Winter War, and the Continuation War. There is also passing discussion of some shorter wars, such as the 1991 Gulf War and the 1967 Six Day War. And, though Shirkey wonders if the selection of cases leads to underplaying the role of information factors, the book does include cases in which information dynamics trumped commitment concerns, such as Stalin lowering his war aims against Finland in the Winter War, Stalin seeking a separate peace with Hitler in October 1941, Japan surrendering in 1945, and the United States lowering its war aims against North Korea in 1951. More generally, the point of the book is to present a theory that integrates information and

commitment dynamics, laying out hypotheses which predict when information factors are likely to predominate and when commitment factors are likely to predominate.

Regarding the issue of comparing short wars and long wars, though I have no systematic evidence, my sense is that commitment dynamics can prevail (or be trumped by information dynamics) in either shorter or longer wars. War termination decisions can be guided by commitment credibility decisions in shorter wars if one side is willing to end a war quickly after ameliorating commitment credibility problems through force of arms (such as capturing strategic territory, as in the Six Day War, or destroying the adversary's army, as in the 1991 Gulf War), or if a short war rapidly culminates in an absolute outcome (such as the rapid overthrow of Saddam Hussein in 2003). Conversely, information dynamics can be quite important in longer wars, such as Vietnam.

Germany in World War I

How Wars End posits that Germany continued fighting in 1918, after Russia's defeat, because of a desire to control Belgium, an ambition in turn motivated by German commitment-credibility concerns vis-à-vis Britain and France. Goemans makes a number of critiques of this argument. He questions whether Britain ever considered launching a future war against Germany. Certainly, it may have been quite unlikely for Britain to consider renewing war in the future, but the important point is that there is evidence that Germany feared a long term threat from Britain and France, and that this concern motivated Germany's focus on Belgium as early as August 1914 (168). German fear of Britain should not be underestimated. Deep antipathy toward Britain washed over Germany as early as August 1914, driven both by resentment of perceived British betrayal and fear of an enduring British threat. One German nationalist publication declared in September 1914 that Germany would be forced to continue fighting until "we can re-sheathe the sword, which we have been forced to unsheathe, in full confidence that the world will be safe from English aggression for decades to come."⁶ The defeat of Russia failed to ameliorate German insecurity in the West. The German leadership continued to stress the importance of capturing Belgium after the Bolshevik revolution, both before and after the commencement of Operation Michael, to secure Germany against a future Anglo-French threat (171). Further, the German leadership talked about specific ways in which German control of Belgium would augment German security: by preventing Britain or France from using Belgium as a staging area for an invasion of Germany; by building fortifications in Belgium to create a security buffer outside of German territory; by establishing an artillery base in Belgium close enough to attack British territory directly, and by using Belgian ports to increase German naval power relative to British naval power (168-171).

A separate point Goemans raises is whether domestic politics in turn motivated German obsession with Belgium. He notes that domestic politics may explain why some elements of

⁶ Quoted in Matthew Stibbe, *German Anglophobia and the Great War, 1914-1918* (Cambridge: Cambridge University Press, 2001), 15.

German society and government demanded control of Belgium. Certainly, the claim that German strategic thinking was dominated by domestic political interests is long established in World War I historiography, often in the context of explaining the prewar Anglo-German naval race and German motives for war in 1914. There is a broader implication of Goemans' point, that even if one accepts the general point that commitment-credibility concerns affect war termination behavior, this still begs the question of where those concerns come from. In the book, I give the topic of the sources of commitment-credibility concerns relatively brief treatment, focusing on factors like the balance of power, geography, and the past behavior of the adversary (for example, 222-223). Future work should explore the sources of commitment-credibility concerns more deeply, using domestic politics or other approaches to build a more fully developed theory.

Shirkey makes a separate observation about World War I. He posits that a number of important developments by early 1918, namely Russian surrender, American entry into the war, and the development of the new German "Stormtrooper" infantry tactics, created a new information environment for Germany, meaning that we ought not be surprised that the tremendous amount of information provided by battlefield outcomes from 1914-1917 did not create bargaining space by early 1918. I touched on the general issue of changing expectations for future combat operations in *How Wars End* (17-18), but I agree with Shirkey that changing expectations and uncertainty about future combat operations can delay war termination, and needs to be better understood. I also agree with his broader point that information revelation under some conditions may be less efficient in creating war-ending bargaining space.

Regarding the specific application to Germany in 1918, the real question to ask is, why did Germany covet gains in the West? Even if Germany was encouraged by the Russian exit and the possibility of the new infantry tactics, Germany must have been tempted to close the abattoir in the West and digest the gigantic gains in Eastern territory, economic resources, and population offered by the Treaty of Brest-Litovsk. Germany and the Western powers might have been able to strike a peace deal in early 1918 on the basis of a return to some form of the status quo in the West. So why not accept Western acquiescence to the king's ransom of Brest-Litovsk in return for, say, returning part of Alsace-Lorraine? My theory provides one possible answer to this puzzle, that Germany's insecurity pushed it to continue fighting in the hopes of controlling Belgium. Goemans' domestic politics theory provides another answer, that the German leadership feared falling from power if did not secure the economic resources in the West to redistribute to supporters of the regime.

The Costs of Pursuing Absolute Victory

Goemans rightly observes that pursuing absolute victory can raise a war's length and costs. This begs the question, why does the fearful state not solve the credible-commitment problem in some other way? This is a topic I address in *How Wars End* (35-47). Sometimes a belligerent solves it by capturing strategic territory or by destroying the adversary's military. Sometimes belligerents engage third parties, as when the 1991 coalition's fear of the long-term Iraqi weapons of mass destruction threat was ameliorated by the assurance that Iraqi disarmament would be verified by the United Nations.

However, such tools are not always available or reliable. Sometimes strategic territory is not sufficient to ameliorate credible commitment fears. In 1940 the English Channel was insufficient assurance to Britain against a future German invasion, and in the following year the United States found out the hard way that the Pacific Ocean was not an effective barrier to Japanese aggression. Sometimes destroying an adversary's military is insufficient. In 1950, the United States feared that if it left the Pyongyang government intact after defeating the North Korean army and signing a peace treaty, North Korea would then rearm and re-attack South Korea when it was ready. Third party guarantees are also sometimes not enough. By 2003, the George W. Bush administration had concluded that it could not count on the United Nations to disarm Iraq, and that only overthrowing Saddam Hussein's government would eliminate the long term Iraqi threat. And, third parties are not always available to guarantee war-ending settlements in either civil or interstate wars. Notably, third parties are likely insufficiently powerful to restrain great powers from breaking war-ending peace deals.

That said, I agree with Goemans that a next avenue of research would be to further develop our understanding of the conditions under which belligerents seek foreign-imposed regime change. I talk about this in general terms in *How Wars End*, but clearly more work needs to be done, both theoretically and empirically. I also agree with Goemans that we need more study of the survival of post-defeat replacement regimes.

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