

the so-called good war in Afghanistan. The overarching message of the book is a policy prescription: Come Home, America. As the subtitle of the book suggests, America's strength is its Achilles heel. To support this claim, Christopher A. Preble peppers the book with eye-opening numbers and threads it with a convincing logic: The United States spends two times as much on its military than its NATO allies combined, 74 times what Iran spends, and 24 times India's military spending. These exorbitant Pentagon expenditures—which represent 93% of all American spending on foreign affairs—go toward high personnel costs, obsolete technologies in the F-22 fighter plane, and the maintenance of an oversized nuclear arsenal. The problem is not simply the costs, although it is that, too, especially in tight economic times. The problem is one of technological determinism. Once the United States has a large, well-equipped military, it feels compelled to use it. In other words, having equipped itself with a fancy hammer, everything is bound to look like a nail. The result is that the United States becomes overextended and its needless interventions provoke counterbalancing efforts that threaten American interests.

Who is the villain in this story? Preble cites Congress as a primary one. The defense industry has strategically placed its facilities in every congressional district, making the preservation of major weapon systems a bipartisan affair. Killing the V-22 Osprey aircraft, for example, was structurally impossible, since doing so would effectively kill jobs in representatives' home districts, which would amount to political suicide.

What are the answers to this conundrum of how power becomes a predicament? First and paradoxically, Congress is not just the problem but one of the solutions. While it has helped finance the large, unwieldy military, it is also, in principle, the institution that can constrain its use. The problem is that in practice, the motivation for Congress to appear hawkish typically mirrors that of the executive. According to a number of accounts, including Cramer's in the previous volume, norms of militarized patriotism in the United States caused Democratic members of Congress to vote in favor of the Iraq war in order to appear strong on national security after 9/11. If indeed these militarized norms are present, then Congress is unlikely to play the moderating role that Preble hopes.

A second answer to the problem of a large and overreaching military is public scrutiny. As Preble notes, the current strategy—and by “current” he means the Bush Doctrine—of preventive and expansive war “doesn't align with the wishes of the American people” (p. 167). That the public's support should be a *sine qua non* for the use of force seems consistent with democratic principles but is potentially unwise in practice. It assumes that the public is enlightened and informed. It assumes that the public is more restrained in its preferences on force than the executive, when public opinion data often tell a different story.

After all, the public actually supported the Iraq war (albeit generally favoring a multilateral approach) before the administration began making the case for war and has continued to support operations in Afghanistan despite a number of qualified observers who now refer to those operations as a war of choice. According to public opinion polls taken in 2009–10, a strong majority of the American public has indicated a willingness to use force to prevent Iran from acquiring nuclear weapons. Such enthusiasm suggests that attending to public opinion could actually encourage intervention when the executive might otherwise be more restrained.

A third answer is to exhaust diplomatic and economic instruments before using force, and to use only force to defend the American way of life, as Preble puts it. As the edited volume cautions us, however, elite framing can have a strong influence on whether the public, Congress, and the media ultimately come to support a particular use of force. As Trevor A. Thrall points out with the case of the Iraq war, support tended not to be contingent solely on the facts but on framing. Elites can probably frame almost any use of force as a mission to defend American values. Thus, the criterion that an intervention only be undertaken on this principle is indeterminate at best, easily manipulated at worst.

Although some of these criteria for the use of force might be difficult to adhere to in practice, they certainly appear persuasive in a world of vast American budget deficits and open-ended wars. Indeed, the message of retrenchment finds support from an unlikely advocate: Secretary of Defense Robert Gates. That Gates would go against all theories of bureaucratic politics to support reduced defense spending suggests that Preble is very much onto something.

How Wars End. By Dan Reiter. Princeton: Princeton University Press, 2009. 320p. \$65.00 cloth, \$26.95 paper.

Paths to Peace: Domestic Coalition Shifts, War Termination and the Korean War. By Elizabeth A. Stanley. Stanford: Stanford University Press, 2009. 408p. \$60.00 cloth. doi:10.1017/S1537592711001368

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What causes wars to end? Many years ago, Geoffrey Blainey (see *The Causes of War*, 1973) argued that if wars begin because states disagree about relative power and their inflated war expectations prevent them from finding a mutually acceptable deal that would preserve the peace, then wars end because combat provides the “stinging ice of reality” that corrects their estimates and opens up the road to agreement. Since this pioneering work, studies of crisis bargaining have proliferated, while the question of war termination has been relatively neglected. The two books under review are among the very few attempts to fill that glaring hole. What is especially intriguing is that whereas both studies

start with essentially the same fundamental premise, they develop in very different directions, reach incongruent conclusions, and give contradictory historical accounts. At the very least, I hope that this will set the stage for a flurry of research that would address their seemingly anomalous findings.

Remarkably, both books begin with a concession—to the rationalist explanations of war (and peace) that commonly go under the moniker “bargaining theory of war” and their formalization (because most of this theory is developed in a series of game-theoretic models) of Blainey’s original approach, which itself was far from rationalist. Both Elizabeth A. Stanley (who calls this “the standard Bayesian model”) and Dan Reiter (who refers to it as the “information-based solution”) accept the “informational story” supplied by this theory as their starting point: Wars end because fighting allows for war expectations to converge. Battlefield performance provides an objective clue to the fundamental balance of power, and diplomatic behavior of actors reveals privately held knowledge about their strength. Both of these sources, the first far less manipulable than the second, transmit information to the belligerents, which makes them revise their beliefs and eventually enables them to find peace terms that would satisfy them both. Of course, this idealized account is highly simplified and appears incapable of explaining protracted wars and the many instances in which the revision of war aims during the war went opposite to what the neat informational convergence story would predict. The two authors part ways in explaining these anomalies.

In *How Wars End*, Reiter attempts to integrate the informational approach with the other strand of the bargaining theory of war—the credible commitment explanation. In the “commitment story,” war occurs because actors cannot credibly promise to abide by the peace deal. An actor who expects to acquire a large advantage in the distribution of power in the near future cannot commit not to use that advantage to extract concessions from the weakened opponent. If the gap between what it will extort tomorrow and what it can offer for peace today is sufficiently large, and if there is no time to make piecemeal adjustments to the distribution of the benefits, then the declining state is better off starting a preventive war that, if victorious, would forestall this unpleasant future. Importantly, this theory does not rely on asymmetric information and as such is wholly distinct from Blainey’s. Implicit in this account is another explanation for war termination: Wars end because fighting eliminates one of the belligerents and thus renders the commitment problem moot.

Indeed, this is precisely the argument Reiter makes: He calls it “absolute war” and argues that contrary to claims sometimes made in the literature, such wars are not empirically rare. By his count, which includes both “violent state deaths” (when one state conquers and annexes another) and “foreign-imposed regime change” (when the victor

“impose[s] a puppet regime, install[s] democratic institutions, and/or hardwire[s] pacifism into a nation’s laws” [p. 26]), about a quarter of all wars between 1815 and 1992 were absolute. Now, one may quibble, along with Clausewitz, that a war that preserves enough independence for the vanquished to enable it to regain military strength and seek a *revanche* is not absolute. More important, however, is the puzzle even this generous statistic raises: If absolute war is the only sure way to solve the commitment problem, then why do the vast majority of wars remain limited? More precisely, if wars are caused by commitment problems and end without the elimination of an adversary, then how does fighting *resolve* the commitment problem?

Although this question is not new (and we have a few tentative answers), Reiter’s main contribution is to attempt a synthesis of the informational and commitment stories. He has an intriguing take on the puzzle: The “best,” and possibly most enduring, ending of a war would be to disarm the opponent (absolute war), and one must explain the failure to pursue it. His answer is threefold. The war can remain limited because fighting might convince one actor that it has no hope of eventual victory, or that the costs of staying the course will dramatically escalate. It can also end “prematurely” if one actor captures some portion of a good (e.g., strategically important territory) that can alleviate the commitment problem by reducing the opponent’s ability to wage war in the future. Finally, actors might be less likely to worry about their commitments if a third party guarantees the settlement by providing some enforcement of its terms.

Although these arguments all appear reasonable, one blind spot they all share (with the possible exception of the first) is that it is unclear how *fighting* is supposed to induce these solutions. Why do the actors fail to foresee the cost escalation, or find ways to divide the strategically important good, or get third parties involved without a war? In other words, while I can see how these solutions might make war termination easier, I am not sure why it is necessary to fight in order to obtain them. Thus, while intriguing, Reiter’s theory appears incomplete in its present state.

Whereas Reiter maintains parsimony with a model that assumes (almost) unitary actors, Stanley, in *Paths to Peace*, breaks up the state black box and looks to domestic political coalitions to explain why wars end when they do. In her reading, the crucial puzzle is why states continue to fight after rational learning should have told them to stop. Wars caused by asymmetric information “should” end when battlefield outcomes and intrawar diplomacy reveal enough information to allow expectations to converge. Although the majority of wars last less than six months (which makes them potentially explicable by the informational story), there is a sufficient number that last substantially longer. She notes that protracted wars defy this explanation because it is hard to see why belligerents need many months, sometimes years, of costly stalemate to update their beliefs.

Stanley identifies three “obstacles to peace” that arise from domestic political dynamics and that might prolong fighting: preference obstacles (when leaders benefit from the war and do not want to end it); information obstacles (when leaders fail to learn properly because they are given poor information, have access to different information, look at incompatible indicators, or suffer from individual or organizational biases); and entrapment obstacles (when leaders want to end the war but are prevented from doing so by domestic or foreign actors). Different governing coalitions (meaning political units relevant to the decision to end the war) are subject to these problems in varying ways, and their wartime behavior will differ accordingly. Stanley elaborates on how changes in domestic governing coalitions should be expected to affect how the state responds to battlefield developments or changes in the opponent’s coalitional dynamics. Thus, war termination is to be explained by changes in the composition of the governing coalition. For instance, when one side undergoes a moderately hawkish shift and the opponent experiences a dovish shift, the prospects for peace should increase because the desire to come to terms on one side will not be overwhelmed by the expansion of war aims on the other.

Thus, while Reiter sees war fighting as rational (in that it constitutes optimal behavior given informational and commitment constraints), Stanley views protracted fighting as inherently irrational, at least from the perspective of the active belligerents. Long wars do not resolve an underlying cause: They are manifestations of a dismal failure to learn or escape the constraints imposed by allies or one’s own wartime policies. This conclusion, of course, is solely the consequence of her adopting only the first variant of the bargaining model of war. By ignoring the problem of credible commitments that Reiter focuses on, she misses a viable rationalist explanation of long wars that does not rely on informational asymmetries and, thus, does not require one to explain apparent failures to learn.

This is not to say that the two-level approach is not the way to go. I happen to think that Stanley’s is the research agenda of the future, and even Reiter will probably agree with that—after all, his “foreign-imposed regime change” solution to the credible commitment problem is essentially an argument about domestic politics: how governments with these special institutions will behave differently than did the original governing coalition that prosecuted the war. The problem is that this research is very hard, a lot harder than Stanley’s book makes it look. The fundamental question that is not sufficiently explored has to do with the causes of coalitional changes. While some, like Josef Stalin’s death in March 1953, are conveniently exogenous to the war (in this case, the Korean), most might not be. In fact, some changes are made specifically to implement new war policies or replace a government whose policies have been discredited (e.g., Winston Churchill in May 1940). In other words, when the war ends because

the new coalition brings “new preferences” to the table, can we say that the coalitional shift caused the war to end? The answer to this clearly depends on whether the coalition came precisely in order to end the war, a change in the strategic calculus that came about because other factors convinced enough people that the present policy was not working. If that is the case, then it is these “other factors,” together with the political institutions that determine whose “voice” is heard, that can be said to be the cause of the war’s end.

Some of these issues can be addressed by careful research design. The cores of both books comprise historical case studies. Stanley traces in depth the internal policies of the three main actors in the Korean War (the USSR, the United States, and China). In addition to this war, Reiter examines the American Civil War and three wars of the mid-twentieth century (the European war in 1940–42, the Pacific war in 1944–45, and the Russo-Finnish wars of 1939–44). Stanley also offers a quantitative test of her hypotheses using two data sets, one of which she collected (20 post–World War II wars). Since process tracing is the privileged procedure and both studies offer an explanation of the Korean War, it might be worth comparing what they have to say about it.

Both authors puzzle over the last 15 months of the war when the fighting had stalemated, the costs were very high, and the only issue blocking a cease-fire had to do with the treatment of prisoners of war. Why did it take the two sides so much more pain and suffering to reach an agreement that was essentially equivalent to what had been proposed over a year earlier? Reiter’s answer is that there were many more communist POWs, and the full swap the communist side was demanding would have returned disproportionately more soldiers to their side. This would have created a power shift in their favor and caused them to renege on the peace deal. Thus, the story goes, the United States fought in order to prevent this from happening. I am not aware of a single shred of evidence to support this interpretation. It is completely speculative and based on arguments by some minor American officials, arguments that could be interpreted to imply that but need not do so. Stanley’s case study shows quite convincingly that Harry Truman rashly made voluntary repatriation a public issue without really thinking through what the communist reaction might be. She also shows that many on the U.S. side thought it was both a matter of principle and reputation to uphold the commitment to human liberty that the United Nations was ostensibly fighting to preserve in Korea. Finally, given the well-known treatment of Soviet POWs who were punished upon their return to the USSR after World War II, it is quite uncertain that returning communist soldiers would have resulted in augmentation of the Communist forces.

If Reiter’s explanation is not convincing, then what about Stanley’s? There are some difficulties there as well. The essence

of her account is that Stalin was benefiting from the war, and so the USSR was not interested in ending it, and since the Chinese were so heavily dependent on Soviet aid, he managed to entrap them and force them to continue fighting even when they had come to believe that they should compromise. Only when he died and the moderate Triumvirate came to power could Beijing finally move with a peace offering. On the American side, the Truman administration was “trapped in the NSC-68 mindset” that saw the conflict as part of a global Moscow-controlled communist expansionist attempt, which committed it to the fighting but without sufficient resources so that enough might be ready for a global war with the USSR. It could not back down on the POW issue for domestic political reasons and did not want to escalate for strategic ones. Only when Dwight Eisenhower came to power with his openly stated readiness to escalate in order to end the war and impeccable Republican credentials did the communists, freed by Stalin’s death, agree to end the war.

The separate components of this explanation are all plausible, but the whole story leaves some important gaps in the logic. For instance, if the war was so costly to the Chinese and was pushing them, against their wishes, more deeply into the Soviet embrace, why not end it? How, exactly, was Stalin able to entrap them? He might have threatened to withdraw his aid, but then it would not have mattered since the fighting would have been over. If the Chinese needed the aid in order to reconstruct the country after the war, then they could have saved themselves some of the destruction and expenses by ending it early. By Stanley’s own account, they were getting a raw deal from the Soviets: All the “fraternal” aid was coming at a hefty price in terms of loans and food exports. Furthermore, if Stalin truly was the major obstacle, then why blame the Truman administration for its failure to perceive the changes in the Chinese position? According to Stanley’s own explanation, it would have been quite correct to focus on the Soviets! It is also important to emphasize that when Eisenhower came to office, he could afford military policies that Truman could not, precisely because Truman’s unpopular mobilization measures had borne fruit. In other words, it could be that he could escalate where Truman simply could not. A less tortuous account would have the Chinese fighting on for domestic reasons (e.g., Mao could use the war to consolidate power, push through painful policies, avoid the propaganda disaster that nonreturning POWs would create), and Stalin egging him on while it was safe that the United States would not attack China itself (and thus activate the mutual defense treaty, dragging the USSR into the war). When Eisenhower came to power, the threat that the US would escalate the fighting to China itself caused both sides to reassess their policies and move closer to an agreement. Ironically, Stalin’s death might have been exactly what the Americans (with their perception of Moscow

pulling the strings) needed in order to accept these changes as a genuine rather than tactical ploy. I am not arguing that this is what happened, but it does seem to me that this story gives a more straightforward account of the facts.

What this reveals, I believe, is the difficulty with using process tracing for evaluating the causal mechanism specified by an underdeveloped theory pitched at very high levels of abstraction. The temptation to read history in a way that would fit the causal pathway is simply irresistible. The sparse theories do not provide enough guidance as to the conditions (counterfactuals) that are necessary for sustaining their inferences, and as a result there is just too much interpretive freedom. This is not meant to lambast the method or its present application; it is to suggest that we really need more work on war termination, most of which will doubtless be along the lines shown by Reiter and Stanley.

Usable Theory: Analytic Tools for Social and Political Research. By Dietrich Rueschemeyer. Princeton:

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From one of our most distinguished political sociologists comes an encyclopedic survey of “usable theory” across the social sciences. Few scholars can rival Dietrich Rueschemeyer’s record of broad theoretical contributions—as coeditor of the landmark *Bringing the State Back In* (1984), as coauthor of *Capitalist Development and Democracy* (1992), as coeditor of *Comparative Historical Analysis in the Social Sciences* (2003), and many other works—and this book offers readers access to some of the seminars he has long taught on social theory.

The book’s goal is to provide a tool kit of usable theory, but not *theories*. Rueschemeyer contrasts his aspirations to the classic kind of survey of grand theories of Karl Marx, Emile Durkheim, Max Weber, George Herbert Mead, Talcott Parsons, Jürgen Habermas, Michel Foucault, and so on. Instead, he chooses to “deliberately leave aside the dialogue and contention among grand theoretical conceptions” (p. 3), surveying a lower level of empirically relevant theory “that is to a large extent independent of—and shared across—the different major positions of ‘grand theory’” (p. ix). He inscribes his effort in the tradition of Robert Merton, who called for a focus on practical “middle-range theory” as an alternative to the grand theory-of-everything approach advocated at the time by Parsons.

The main advances in the social sciences, Rueschemeyer suggests, have come not in grand theories but at this lower level of “theory frames.” These are not full-fledged theories in the sense that they “do not themselves contain or logically entail a body of testable hypotheses” (p. 1).