

regulation. The first omission is surprising given the widely-recognized importance of knowledge co-production in global environmental policy debates and demands for better two-way communication between scientists and local policy makers—for instance, on how to use information distilled from climate change models for meeting local community needs for long-term resource planning (e.g., Sheila Jasanoff, *States of Knowledge: The Co-Production of Science and the Social Order*, 2004). Regarding the precautionary principle, the omission of recent comparative politics investigations of its use in diverse environmental and health policy contexts (e.g., David Vogel, *The Politics of Precaution*, 2012) is also surprising.

Finally, while the book's contributors acknowledge the importance of ensuring policy acceptability by adapting adopted policy measures to the moral norms and collective preferences of society, the editors' abbreviated conclusions fail to discuss lessons from these intriguing cases regarding how to do this more effectively. In particular, how do co-production of knowledge and collaborative processes that seek to articulate a consensus around policy reform change the preferences of protagonists over time? Political scientists who have examined the evolving agendas of local and regional governments relative to global environmental issues (e.g., Henrik Selin and Stacy VenDeveer, *Changing Climates in North American Politics*, 2009) have, for instance, noted the importance of value change, community capacity, and shifting agendas in explaining the emergence of multi-level governance of these issues.

These are probably unavoidable shortcomings in an ambitious, multidisciplinary collection such as this. Nevertheless, given the enormous effort the contributors invested in the volume's production, its usefulness to political scientists who study the governance of global public goods could have been strengthened by better linking contributors' findings to other recent work, as well as by connecting these findings to cases studied by scholars outside of this volume's network of contributors.

Barriers to Peace in Civil War. By David E. Cunningham. New York: Cambridge University Press, 2011. 296p. \$90.00.
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Answering the question as to why some civil wars end in a matter of months, while others persist for decades (Afghanistan, Angola, Burundi, and Colombia), is both theoretically interesting and critical to the design of more effective methods for ending intractable civil wars. David E. Cunningham's *Barriers to Peace in Civil War* stands as a lively addition to a growing scholarship on this important topic. The author begins by pointing out the huge variation in the duration of wars, with the vast majority ending in a matter of months or years but a significant minority carried on for many years, if not decades. He explains why

we should care about these seemingly endless civil wars, as they are responsible for far more casualties, are more likely to involve genocide, and are also more likely to recur.

To address this question, Cunningham first reviews explanations in the literature for the duration of civil wars. For instance, some conflicts may last longer because they are fought over things that cannot be divided, such as when the conflict is waged over control of national territory (Monica D. Toft, *The Geography of Ethnic Violence: Identity, Interests, and the Indivisibility of Territory*, 2003) or sacred spaces (Stacie E. Goddard, "Uncommon Ground: Indivisible Territory and the Politics of Legitimacy," *International Organization* 60 [2006]: 35–68; Ron E. Hassner, "'To Halve and to Hold': Conflicts over Sacred Space and the Problem of Indivisibility," *Security Studies* 12 [2003]: 1–33). Alternatively, informational asymmetries may cause one or both sides to overestimate their chances of prevailing in battle, inducing them to fight on (Branislav L. Slantchev, "The Power to Hurt: Costly Conflict with Completely Informed States," *American Political Science Review* 97 [2003]: 123–33). It may also be that the winning side (usually the government) cannot credibly commit to protecting the losers once they disarm, making the losing side unwilling to lay down their weapons (Barbara F. Walter, "The Critical Barrier to Civil War Settlement," *International Organization* 51 [1997]: 335–64; Barbara F. Walter, *Committing to Peace: The Successful Settlement of Civil Wars*, 2002; James D. Fearon, "Why Do Some Civil Wars Last So Much Longer Than Others?" *Journal of Peace Research* 41 [2004]: 275–301). Finally, civil wars may involve wartime looting, creating pecuniary disincentives for the combatants to reach a settlement (see Paul Collier, Anke Hoeffler, and Måns Söderbom, "On the Duration of Civil War," *Journal of Peace Research* 4 [2004]: 253–73).

Cunningham discusses each of these factors in turn, concluding that "they cannot, however, come close to explaining the extreme variation in civil war" (p. 12). This is because many wars end quickly despite incentives for war profiteering, the apparent indivisibility of the stakes of conflict, and informational asymmetries, while other wars last for decades despite the relative *absence* of such barriers to peace.

The author then advances an alternative explanation that the number of "veto players" ("a set of actors that have separate preferences over the outcome of the conflict and separate abilities to block an end to the war"; p. 15) largely determines how long a conflict will last. Prolonging the war is, according to the author, something veto players do deliberately "because, in the end, it gets them a better deal" (p. 4). Cunningham argues that the more veto players there are in a given conflict, the more difficult it is to collectively agree on a division of state resources that all participants prefer to the continuation of war. This is the major reason why some conflicts are easier to end than others—because "spoilers," or veto players, emerge

to scuttle the peace agreement (Stephen John Stedman, "Spoiler Problems in Peace Processes," *International Security* 22 [1997]: 5–53).

To test these hypotheses, Cunningham conducts statistical analyses on 200 civil wars since World War II, demonstrating that wars that involve more participants last longer than those with fewer. He then conducts a controlled comparative analysis on two civil wars in the Great Lakes region (Rwanda and Burundi), showing that despite their many similarities, the Burundi conflict was much more difficult to resolve because there were more veto players in the conflict. This created incentives to hold out for a better deal, shifting alliances and, therefore, information asymmetries about each player's expected likelihood of prevailing in war. As a result, the negotiations to end the Rwandan conflict were relatively expeditious, whereas negotiations to end the Burundi conflict were very complicated, resulting in another 14 years of war.

Despite its powerful argumentation and trenchant analysis, the book has its weaknesses, a fact that the author himself recognizes. First, there is the problem that the number of veto players is used as a proxy for player preferences—with more veto players indicating more divergent preferences. However, more veto players does not necessarily mean more divergent preferences. A large number of veto players will not upset or hinder a settlement so long as the players' preferences are closer to one another than they are to the status quo; by contrast, a smaller number of veto players *can* hinder a negotiated settlement if their preferences are such that there is no alternative division that both prefer to the status quo.

A second problem relates to causality. The empirical analysis suggests in certain instances that there are structural conditions that may drive *both* the number of veto players and the difficulty of peace negotiations. For example, the hard-line Hutu faction (Coalition for the Defense of the Republic, the CDR) that scuttled the peace agreement in Rwanda in 1994 had been a "latent" veto player until events in neighboring Burundi and the actions of the international community convinced the hard-liners that the peace deal would yield an unsatisfactory outcome, leading them to "spoil" it (p. 175). This indicates that, at least in some cases, the number of veto players may at best be an intervening, rather than independent, variable in explaining the success of peace negotiations. If this is true, then the fundamental drivers of stalled peace agreements might lie farther up the causal chain.

Despite these problems, Cunningham has written a highly important and persuasive book on the impact of the number of conflict parties on the likelihood of successful conflict resolution. The policy conclusions are fascinating, if a bit vague: Include no more and no fewer than the actual number of veto players in the negotiation process—too few leads to spoiling by the omitted player and too many increases the difficulty of finding a viable

settlement. Identifying all of the relevant veto players in turn "necessitates a deep understanding of the individual players involved in conflict and in society" (p. 254). This is a strong call as any for involving country and conflict experts in the negotiation setting—for getting the process right may make the difference between an expeditious resolution and one that is many years off.

Why Adjudicate? Enforcing Trade Rules in the WTO.

By Christina L. Davis. Princeton: Princeton University Press, 2012.

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In her book, Christina L. Davis provides an insightful and careful analysis of the domestic underpinnings of international trade law litigation. *Why Adjudicate?* fills a real gap in the literature by examining the factors that both shape and determine when states pursue legal challenges before the World Trade Organization's dispute-settlement mechanism. The author moves beyond the traditional scholarly focus of emphasizing the role of power politics in international law and provides a critical assessment of the reasons why state actors use international courts. Focusing on the United States and Japan as her primary case studies, she has completed an exceptional and valuable study that will be read by both scholars and professionals for years to come.

Davis makes a number of richly layered arguments with strong evidence to support each of them. Most significantly, she demonstrates that in countries such as the United States and Japan, policymakers use the international legal forum as a tool to defuse domestic political problems. She convincingly argues that "[g]overnments may set aside legal principles and economic criteria in order to gain support from influential interest groups and achieve diplomatic goals" (p. 286). While she recognizes that informal negotiations between state actors could potentially resolve legal disputes, Davis underscores that addressing questions of law is not the only reason for pursuing WTO claims. Rather, the very act of initiating challenges before the WTO serves as a mechanism by which national leaders can demonstrate that they are representing domestic interests and serving the public interest. She underscores that the "decision to invoke international law in a dispute does not proceed automatically on merits of the case alone" (p. 2). Rather, a number of domestic factors are considered, too.

The author also demonstrates a clear linkage between democracy and the use of litigation in international trade. Davis explains that elected officials rely on international legal action not only to emphasize their commitment to the protection of domestic interests but also to enhance electoral support among constituents. She emphasizes that citizens of democratic nations conceptualize courts as legitimate dispute-settlement bodies in a democratic society.