

violence, strategies need to be devised that are appropriate to the context. Finally, strong leadership, especially by democratic states, is needed to enable effective genocide prevention (pp. 251–3).

Responding to Genocide identifies the major stumbling block to preventing and halting genocide – differences among states and other actors. Governments need to know that there is a risk and agree to take concerted action to address it. They need to be willing and able to impose coercive measures as a last resort, if structural prevention and softer direct prevention measures have failed. Debates about semantics – whether a “genocide” is the issue or not – should not foil the primary objective of preventing massive human rights violations. This volume highlights how difficult (but not impossible) it is to overcome that stumbling block. It points to some improvements that could strengthen the capacity of various international actors to prevent genocide, but does not cover others (such as the use of sanctions or measures such as monitoring missions). It would also have been illuminating to broaden the discussion to include obstacles to action, such as the cost (in money and in lives) of preventive measures, the ways bureaucracies can inhibit early warning, and the perennial problem that crisis-response crowds out prevention (in terms of resources, and the attention and time of politicians and officials). Reducing such obstacles could improve the ability of governments and international organisations to prevent genocide. While that does not necessarily improve the will to respond, such measures could improve the capacity of those actors that are willing to do so.

Diversionsary War: Domestic Unrest and International Conflict. By Amy Oakes. Stanford, CA: Stanford University Press, 2012. 280p. \$95.00 cloth, \$25.95 paper.
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— David Brulé, *Purdue University*

For the uninitiated, diversionsary wars are those in which leaders are thought to seek to distract attention from domestic problems by directing military action against a foreign target. In *Diversionsary War*, Amy Oakes, like other scholars, argues that diversion is not an optimal policy response to domestic problems. Indeed, domestic policy choices that directly address problems at home are likely to be better than diversion. Also like other scholars, she draws an important distinction among countries and governments according to their ability to employ various policies. Rather than argue that democracies are the only ones that face obstacles to policy choices in response to domestic unrest (i.e., repression), or that focus on institutional impediments to implementing domestic policy, Oakes suggests that a set of policy alternatives are constrained by the extent to which a country is able to garner the necessary resources to address domestic discontent.

Consequently, leaders may be required to substitute one policy for another. For instance, if a repression is unavailable as a viable policy choice in response to domestic unrest, leaders may substitute diversion for repression. On the other hand, if a leader lacks the resources to launch a military adventure abroad, he or she may have little choice than to employ domestic alternatives.

Unfortunately, the literature on diversion is large and ever more difficult to advance. Perhaps because of this, Oakes—mistakenly, in my view—characterizes her argument as if it is a novel contribution to the literature, suggesting that research on the diversionsary use of force has paid only scant attention to the concepts of policy substitutability, institutional constraints, and resource availability. But her claims are drawn directly from the large literature on the topic. For instance, Bruce Russett, T. Clifton Morgan, and Kenneth Bickers, Christopher Gelpi, and Ross Miller have made very similar arguments since 1990. More recently, Jeffrey Pickering and Emizet Kisangani, among others, have extended these arguments and devised increasingly sophisticated tests of the theory. Although she references much of this work, Oakes appears to oversell her theoretical contribution in light of this large literature.

Be that as it may, Oakes makes an important contribution to the literature by turning her attention to the question of whether diversion actually is a beneficial policy choice relative to other alternatives. Here, she carefully considers previous work on the topic (e.g., Pickering and Kisangani) and logically connects the effects of a number of policy responses—diversion, repression, political and economic reform—on domestic unrest. She approaches the question initially from an agnostic point of view, suggesting that relationships could be either positive or negative. But when situated within her own argument concerning policy resources, she makes precise predictions, which are borne out in the analyses.

Perhaps the chief strengths of the book are found in the author’s methodological solution to the question concerning the fruits of diversion, as well as in her combination of case studies (discussed later) with large-n statistical analyses. She examines the relative effects of policy responses on domestic unrest. Rather than simply assume that diversion is a binary phenomenon, Oakes disaggregates military action into low-level spectacles—uses of force short of war—and war. She also explicitly models the effects of repression and reform on domestic unrest. Her results are intriguing: low-level spectacles increase unrest, while repression is effective in reducing it.

Although the disaggregation of military action into three categories is useful in examining the effects of diversion on unrest, it is problematic when considered as an outcome variable. Oakes suggests that leaders may choose among the military options in a decision-making framework in which implications of a low-level spectacle

are weighed against a full-scale war. The point has some merit, but wars may be unintentional. Indeed, a leader's preferences over a spectacle relative to war are defined in terms of the leader's expectations of resistance from the target.

I find this distinction to be problematic because it relies on the expectation of the target's behavior. And, arguably, leaders who choose diversion may find themselves in a war due to mistaken expectations. The Falklands War, which is examined in the book, is a good example; the Argentine leadership is thought to have miscalculated the response of Great Britain. Oakes considers this possibility of unintentional war and incorporates it nicely into her argument. Specifically, she hypothesizes that states with low policy (i.e., pauper states) resources are more likely to find themselves embroiled in interstate war than states with high policy resources (princely states). However, she does not adequately test this hypothesis. When assessing the probability of diversion, she examines 177 countries over a 22-year period (i.e., country-years, or monads). Rather than an examination of only monads, a dyadic analysis examining conflict initiation and reciprocation would have been more appropriate for assessing whether targets of diversion are likely to retaliate against pauper states. Given her criticism of the previous literature's failure to account for alternatives to diversion, I also expected Oakes to empirically examine the policy choices she identifies using a multinomial model in which the dependent variable includes nominal categories consisting of each of the policy alternatives. Such a strategy would allow for the direct comparison of the alternatives under various conditions identified by her argument.

Oakes's choice of case studies is remarkable. Her first case is the frequently examined Falkland's War, which she deftly analyzes in comparison to the numerous other explanations of the conflict. The next case study in the book examines James Buchanan's choice to launch a military expedition into Mormon-controlled Utah during 1857–58. This case seems an odd choice at first glance. But as the author points out, Utah under territorial governor Brigham Young was essentially a state within a state. Finally, she explores a number of other cases in which no military action was taken in response to domestic unrest. Taken together, these case studies provide an account of the theoretical mechanisms identified by Oakes in her argument.

Overall, *Diversionary War* is a nice contribution to research on the linkages between domestic and international politics. Although I tend to doubt that leaders explicitly consider using force abroad in response to problems at home, Oakes's arguments and evidence are provocative enough to make me reconsider my previous position.

Borders Among Activists: International NGOs in the United States, France, and Britain. By Sarah S. Stroup. Ithaca, NY: Cornell University Press, 2012. 264p. \$39.95.
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— Sarah Wilson Sokhey, *University of Colorado Boulder*

In *Borders Among Activists: International NGOs in the United States, France, and Britain*, Sarah S. Stroup examines how the behavior of international non-governmental organizations (INGOs) differs based on the country in which they were founded. She argues that four domestic factors—the regulatory environment, political opportunities, material resources, and social networks—influence their strategies for fundraising, professionalization, advocacy, and government relations; she demonstrates how these domestic factors influence the behavior of humanitarian and human rights INGOs in the United States, France, and Britain. The book is organized around case studies of humanitarian INGOs in each country, including CARE (US), Oxfam (Britain), and Médecins Sans Frontières (France), and of human rights INGOs in each as well, including Human Rights Watch (US), Amnesty International (Britain), and the Federation Internationale des ligues des Droits de L'Homme (FIDH). By using “mini-cases” of other INGOs, she argues that the dynamics of these main cases are not unique, but reflect national trends.

Stroup's work contributes a better understanding of how domestic influences matter in a globalized era—even for organizations that operate internationally. Her findings are bolstered by extensive interviews with individuals working in these organizations, and by detailed accounts of their operations. The implication of the argument extends beyond INGOs: Any organization that operates internationally including corporations, interest groups, and possibly even subsets of organizations like the European Union, should be influenced by its country of origin.

While interesting, the book's findings raise several issues which are not fully addressed, involving the interplay of domestic and international pressures, the connections between domestic influences, and the role of policy diffusion. These issues reflect broader challenges in the study of international civil society and political economy. Scholars should think more seriously about how to identify when and why global or domestic factors matter, about the consequences of domestic factors, and about the role played by policy diffusion (an influence that links local and international pressures). How does Stroup address these questions? I discuss each in turn.

Domestic or International Pressures. On the one hand, Stroup's finding is not surprising in its noting that international charities behave differently based on the country of origin. Few scholars or policymakers would argue with this contention. Rather, it would be surprising to find that national origin *did not* play an important role in the behavior of charities. In fact, a growing body of