

# Critical Dialogue

**Peacekeeping, Policing, and the Rule of Law after Civil War.** By Robert A. Blair. Cambridge: Cambridge University Press, 2020. 267p. \$105.00 cloth, \$34.99 paper.  
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— Audrey L. Comstock , Arizona State University  
audrey.comstock@asu.edu

Since its creation in 1948, the UN has sent more than 70 peacekeeping operations around the world. About 50 of these were sent after the end of the Cold War, of which about 30 were in Africa. The scope of peacekeeping mandates shifted during this time. Although still interested in ending conflict and creating peace agreements, the program increasingly focuses on nation-building or peacebuilding. It is within this space that *Peacekeeping, Policing, and the Rule of Law after Civil War* provides an in-depth study of how one such focus—strengthening the rule of law—has fared when taken on by UN peacekeeping forces in the post-Cold War African context.

*Peacekeeping, Policing, and the Rule of Law after Civil War* addresses the question of how and when states embrace the rule of law (ROL) after civil war. This multifaceted and ambitious book emphasizes the importance of interlevel interactions in the process of embracing ROL. Throughout the book, Blair argues that UN peacekeeping missions act “as catalysts for state reform” (p. 5) across the macro (national), micro (local), and meso (between citizens and states) levels. For the ROL norm to be adopted, it is crucial that all levels be engaged and convinced of its importance.

The book begins with an historical overview of UN intervention and the rule of law after civil war (chapter 2), followed by the core theoretical contribution (chapters 3 and 4). Then, the ROL concept is applied to UN peacekeeping, making clear the UN’s role in revising state laws, disseminating information, and “sensitizing” citizens to the changes (chapter 4). The UN’s global legitimacy is a crucial tool enabling norm diffusion throughout the macro, micro, and meso levels after the conflict.

The second half of the book (chapters 5–8) is its analytical heart. Chapter 5 conducts a statistical analysis of 33 African postconflict settings where the UN deployed peacekeeping missions (macro). The nuanced contribution of this chapter is its presentation of the multidimensional

measurement of peacekeepers and their functions. Blair makes a great case for working past a blunt measure of peacekeeping troops. His novel data collection, drawn from the UN secretary-general’s annual budget requests and program reports (p. 95), is especially convincing because it directly measures the extent to which UN resources are devoted to strengthening the ROL. Findings indicate that once peace is established, peacekeeping presence (when devoted to ROL tasks) is a significant indicator of improvements in numerous measurements of ROL.

The rest of the book examines the case of Liberia (chapters 6–8). Chapter 6 provides a rich historical case overview of Liberia from the 1800s through modern peacekeeping in the 2010s. After chapter 7 describes the research design, chapter 8 analyzes surveys conducted in 243 Liberian towns and 43 interviews (micro and meso levels). These analyses provide further support for his argument: together, they point to the importance of exposure to peacekeepers (particularly through patrols) in shaping individuals’ confidence in the Liberia mission and ROL, as measured by the self-reported likelihood to use formal rather than informal institutions in adjudicating criminal disputes.

*Peacekeeping Policing, and the Rule of Law after Civil War* contributes to the literatures on international peacekeeping, postconflict nation-building, and norm diffusion through its nuanced and layered unpacking of the ROL and its description of how full societal engagement through multiple layers is needed for change to occur. Blair’s contribution is conceptually rich and empirically rigorous. The framing of ROL as a more complex concept operating across levels is fruitful for application beyond the African context and even beyond peacekeeping contexts to broader literatures on norm diffusion when introduced by a foreign actor. The book’s generally optimistic findings speak to how effective the UN has been at peacekeeping overall and how effective it is in translating policy goals into meaningful changes in postconflict settings.

Even with these contributions, however, several questions arise and offer potential avenues for future research. First, other international actors are almost completely missing from the book. The book seeks to use UN peacekeeping as one example of international intervention and the advancement of the ROL, but it is left unclear how the UN compares with other international interventionists.

There are brief mentions of NGOs' presence (e.g., p. 169) and that some of the individual-level interviews were connected to NGOs. However, when it came time to explain ROL outcomes either through large-N (chapter 5) analysis or the survey of individual-level beliefs (chapters 6–8), neither NGOs nor other types of international influence are controlled for. The mean number of visits by NGOs was reported as higher than that of police (p. 170), yet we cannot tell whether NGO engagement contributed to, mitigated, interacted with, or related to individual-level views on the ROL. Can these actors also serve as catalysts for ROL change? In the conclusion, Blair generalizes the argument to the United States, although application to NATO may have been a more comparable extension. Blair might have considered that the US mechanisms for norm diffusion were drawing on power and strategic imbalances in different ways than those of the UN, given his argument that the UN's legitimacy comes from its global scope and broad membership.

Second, a central part of the argument relies on individual-level interactions with UN personnel being net-positive in supporting the norm diffusion of the ROL. Unfortunately, repeated individual-level interaction with the UN may be a net-negative in some contexts. More research and a growing number of negative newspaper headlines make it clear that sexual exploitation and abuse (SEA) have been part of UN peacekeeping. Blair does briefly acknowledge UN scandals but does not engage with their implications for his argument. It does not appear that any survey scenarios examined how UN scandals or distrust might shape the willingness of individuals to use formal institutions (p. 164). The increased role that the UN has assumed in interacting with individuals can contribute to more SEA and distrust of the UN (see Frédéric Mégret, and Florian Hoffman, 2003, "The UN as a Human Rights Violator? Some Reflections on the United Nations' Changing Human Rights Responsibilities," *Human Rights Quarterly* 25 [2], 2003; and Carla Ferstman, *International Organizations and the Fight for Accountability: The Remedies and Reparations Gap*, 2017). SEA in Haiti combined with the cholera scandal contributed to distrust in the UN and a reluctance to work with the organization among Haitians (see, for example, Georgia Fraulin, Sabine Lee, and Susan A. Bartels, "They Came with Cholera When They Were Tired of Killing Us with Bullets: Community Perceptions of the 2010 Origin of Haiti's Cholera Epidemic," *Global Public Health*, 2021).

This area of research indicates that there are serious issues with UN personnel activities that undercut trust, confidence, and the willingness to interact with UN peacekeeping troops. Blair has not made clear how and when ROL advancement can overcome distrust from the scandals that UN personnel perpetrate. This may help explain why respondents viewed formal institutions and the United Nations Mission in Liberia (UNMIL) itself as

corrupt after interaction with peacekeepers (p. 188). Blair offers an explanation for this finding—that UNMIL sent mixed signals about the corruption of the state, and hence the need for peacekeepers, with the signal to rely on formal state institutions. However, the real explanation may lie in peacekeepers' behavior.

On a related issue, the book has a convincing argument that all three levels matter for norm diffusion, but it is unclear how knowledgeable and receptive individuals actually are to UN intervention. There is evidence throughout the book of the lack of understanding or knowledge (or even confusion) of the general population. How much does the average civilian know about what the UN is doing? The interviews admittedly did not aim to sample the general population: instead, "the goal throughout was to interview individuals who could speak knowledgeably about UNMIL and its role in Liberia" (p. 161). However, that still leaves this question: What amount of public knowledge, support, and norm adoption enables substantive change?

In practical application, at what point can the UN's entrance act as a catalyst? Or what is the threshold or tipping point? This aspect of the norm diffusion connection is undertheorized. UN peacekeeping may have repeat missions in locations after peace fails and new conflicts arise. Does the catalyst only work once, once per conflict, once per generation, or for a period after a certain influx of troops? Prior exposure to the UN after past conflicts may prime individuals, societies, and states to ROL norms or potentially predispose them against the norms if prior UN missions are perceived as failures. Liberia, for example, saw a prior mission in the 1990s. Haiti has had at least six missions since 1993. How might the UN ROL endeavors be received if done repeatedly?

Despite these queries, overall *Peacekeeping, Policing, and the Rule of Law after Civil War* is a strong contribution to the study of peacekeeping, postconflict studies, and institutional legitimacy that strengthens our understanding of how UN peacekeeping can make effective changes after conflicts have ended.

### Response to Audrey L. Comstock's Review of *Peacekeeping, Policing, and the Rule of Law after Civil War*

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— Robert A. Blair 

Audrey L. Comstock's thoughtful review raises three important questions about my argument and findings. First, she asks about the role that NGOs and other international organizations (IOs) play in promoting the rule of law after civil war. As she rightly notes, my book focuses almost exclusively on the UN. This is because the UN is more active and ambitious in its rule of law (ROL)

agenda than any other domestic or international institution in the world. Although entities like NATO, the European Union, and the World Bank have all contributed to ROL reforms in postconflict settings, their efforts tend to be much smaller in scope. As Calin Trenkov-Wermuth observes, “No other organization has been involved in such reforms to the same extent as the United Nations” (*United Nations Justice: Legal and Judicial Reform in Governance Operations*, 2010).

I control for the presence of other NGOs at the community level in my within-country analysis of Liberia (p. 169), and I measure ROL promotion by IOs other than the UN in my cross-country analysis (p. 98). However, as I explain in a companion article (Robert A. Blair, “UN Peacekeeping and the Rule of Law,” *American Political Science Review*, 115 [1], 2021), because the efforts of other IOs are so often implemented alongside (and in support of) the UN’s, it is difficult to disentangle the two empirically. I suspect the UN’s impact on the ROL is much greater than that of other IOs. But this is an empirical question, and I agree with Comstock that answering it is a worthwhile endeavor for future research.

Second, Comstock asks whether acts of sexual exploitation and abuse (SEA) by peacekeepers undermine their efforts to promote the rule of law. I agree that this is an important concern; indeed, it is the motivation for one of the three rival theories that I pit against my own (pp. 84–89). If peacekeepers routinely engage in misconduct, and if misconduct undercuts citizens’ trust and cooperation, then I should observe a negative correlation between exposure to peacekeepers and citizens’ perceptions of the UN at the micro level. But I do not. In fact, I observe the opposite: after interacting with UNMIL personnel, Liberian citizens generally express *more* rather than less favorable perceptions of the mission (p. 204). This is not because UNMIL was uniquely virtuous: to the contrary, UNMIL personnel faced frequent accusations of misconduct toward Liberian women and girls. Although Comstock is right that SEA may undermine the UN’s ROL agenda, and some UN missions may be so predatory that they simply cannot promote ROL at all, consistently righteous behavior on the part of UN personnel does not appear to be a scope condition for my results.

Finally, Comstock asks whether there is a dose-response relationship between UN intervention and the ROL. What fraction of the public must be “exposed” to peacekeeping before attitudes and behaviors begin to shift? What happens if the UN deploys and withdraws repeatedly? This is an important question that my data are unfortunately not well suited to answer. I do find some evidence that UNMIL’s impact on the ROL in Liberia increases with the intensity of citizens’ exposure to UNMIL personnel (p. 191). I also find that the quality of the ROL cross-nationally is an increasing function of

the number of uniformed and civilian personnel deployed to each UN mission (p. 108). These results are consistent with a dose-response relationship but are not granular enough to identify a specific tipping point beyond which the ROL begins to take root. Comstock’s question points to a fruitful avenue for future scholars to explore.

### **Committed to Rights: UN Human Rights Treaties and Legal Paths for Commitment and Compliance.** By

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— Robert A. Blair, *Brown University*  
robert\_blair@brown.edu

Why do states abide by the terms of international human rights treaties? In this interesting and informative book, Audrey L. Comstock argues that part of the answer lies in the way states commit to those treaties in the first place. She distinguishes between four commitment mechanisms: signature, ratification, accession, and succession. Signature is nonbinding; although it is only an “initial commitment step” (p. 17), it can nonetheless serve as a “focal point” around which human rights activists and NGOs mobilize and can induce “advocate executives” to promote human rights prior to ratification (p. 80). Ratification, accession, and succession are all legally binding but, in Comstock’s account, involve different groups of states: those that participated in negotiations over the treaty (ratification), those that did not participate in negotiations but wish to bind themselves anyway (accession), and those that previously ratified a treaty before regime change brought a new state into being (succession).

Comstock argues that these divergent commitment pathways produce equally disparate human rights records. Her theory is complex and counterintuitive. She hypothesizes that human rights will improve after a state signs a human rights treaty, but only if the state requires legislative approval for ratification; in states where the executive can ratify unilaterally, Comstock expects signature to yield little or no change. In these latter “executive approval” states, Comstock hypothesizes that human rights will improve after ratification; in contrast, in states that require legislative approval, she expects no change after ratification or even a deterioration in human rights. She expects human rights to improve after accession, but only among states that were not members of the UN at the time the treaty was negotiated; among states that *were* members but opted out of negotiations, she again expects human rights to plateau or decline. The only commitment mechanism that she expects to have unconditionally positive effects is succession—although, as she notes, this is an “infrequent and unique” pathway that is relevant to only a “small handful of states” (p. 55).

Comstock's account offers both theoretical and methodological advances beyond existing human rights scholarship, most of which focuses on ratification. Previous studies have typically assumed that ratification constitutes no more or less of a commitment than accession and that signature is no commitment at all. Comstock argues that this assumption is misguided. She provides theoretical reasons to believe that even an act as (apparently) ceremonial as signing a treaty with no legally binding consequences can nonetheless precipitate significant changes in states' human rights behavior; she also provides empirical evidence to suggest that different commitment pathways do indeed correlate with different human rights outcomes. Treaty law has become the dominant form of international law since the end of World War II, and international human rights treaties are among the most prominent mechanisms for disseminating human rights protections around the globe. Understanding why and under what conditions states adopt these protections is a first-order concern for anyone interested in international law, international organizations, or the rights of historically marginalized groups.

These strengths notwithstanding, the book does have some limitations. Perhaps the most important one is the difficulty of isolating the impact of human rights treaties from the conditions that lead states to commit to those treaties in the first place. Does committing to a treaty cause states to improve their human rights practices? Or do states commit because they are already invested in improving human rights and wish to signal their investment to domestic and international stakeholders? Disentangling these possibilities requires overcoming enormous inferential obstacles. Consider, for example, the United States' commitment to the Committee on the Elimination of Racial Discrimination (CERD) treaty, which Comstock discusses in chapter 4. The United States is a legislative approval state; it signed CERD in 1966 but did not ratify it until 1994. According to Comstock, this is an example of a state "signing a human rights treaty early, moving toward improved rights, and then ratifying much later" (p. 91). The implication seems to be that there is a causal connection between the decision to sign CERD and the United States' subsequent successes (limited and halting though they may be) in mitigating racial discrimination.

But is this the most plausible interpretation of the historical record? By the time President Lyndon Johnson signed CERD in 1966, the civil rights movement was already in full swing. The Civil Rights Act passed in 1957 and the Voting Rights Act in 1964. Johnson mandated equal opportunity for minorities in federal contracting in 1965. Indeed, in Comstock's own telling, Johnson signed CERD to "signal US commitment" to the treaty's provisions and strengthen the United States' position in negotiations over other international human rights laws (p. 91). Did Johnson sign CERD because he was already pursuing

a civil rights agenda and wanted to signal his commitment to fulfilling it (a selection effect)? Or did signing CERD catalyze improvements in civil rights that would not have materialized otherwise (a treatment effect)? It's hard to say for certain, and both dynamics could be at play. But the former seems more likely than the latter.

Or take the case of Nigeria and the Convention on the Rights of Persons with Disabilities (CRPD), also discussed in chapter 4. Nigeria—a legislative approval state—signed the CRPD in 2007 and ratified it in 2010. Per Comstock's theory, we should expect to observe an improvement in Nigeria's human rights record after 2007, followed by stagnation or deterioration after 2010. In reality, however, Nigeria's score on the Freedom House civil liberties index (one of Comstock's key dependent variables) remained constant between 2007 and 2010 (and beyond). Nigeria's CIRI scores (the other key dependent variable) actually *fell* between 2007 and 2010: of the 16 CIRI indicators that are available for this period, Nigeria's score improved on just one (women's political rights) and by just one point. Comstock notes that Nigeria created a ministerial committee on albinism but only after it signed *and* ratified the CRPD. Similarly, the state of Lagos passed a law aimed at securing the rights of people with disabilities, but only *after* CRPD ratification. It is possible, of course, that activists' efforts take time to bear fruit, but these temporal dynamics only compound the inferential problems. If a state signs a treaty in one year, ratifies it a few years later, and improves its human rights record a few years after that, it is very hard to tell what explains the improvement: signature, ratification, or something else entirely.

Ambiguity between selection and treatment effects is implicit in the hypotheses as well. Comstock argues that signature can "activate human rights mechanisms" as activists, NGOs, and other states pressure, shame, and socialize governments toward ratification (p. 17). This is a treatment effect. But signature also "signifies a state's willingness to recognize and support international standards of human rights behavior" (p. 17). This is a selection effect. Similarly, Comstock explains that negotiation is a lengthy and sometimes contentious process that leaves states "more invested and likely to adhere to the treaty's terms" (p. 18). This is a treatment effect. But she also notes that "states that are at the negotiating table for human rights treaties have an interest in promoting international human rights" (pp. 122–23). This is a selection effect. Are treatment or selection effects more important for explaining states' subsequent human rights records? Again, it's hard to say. But the latter seem at least as important as the former.

To her credit, Comstock does address the possibility of selection effects in her chapter on accession. But the discussion is brief and a bit too sanguine. Indeed, she devotes parts of chapters 4 and 5 to documenting systematic differences between states that sign human rights

treaties and those that do not; between states that sign *and ratify* and those that do not; and between states that ratify and those that accede. These differences only magnify potential selection concerns. Comstock also uses instrumental variables (IV) to attempt to mitigate these concerns, but the book is surprisingly silent on the logic underlying the instruments and the (quite demanding) assumptions that using IV requires. For example, Comstock uses an indicator for common law states as an instrument for committing to the International Covenant on Civil and Political Rights (ICCPR). But other studies that Comstock cites (e.g., Sara McLaughlin Mitchell, Jonathan J. Ring, and Mary K. Spellman, "Domestic Legal Traditions and States' Human Rights Practices," *Journal of Peace Research*, 50 [2], 2013) have shown that common law states have better human rights records for a variety of reasons that are entirely independent of their decision to sign a particular treaty. This seems like a clear violation of the excludability assumption needed for consistent IV estimation.

These limitations notwithstanding, Comstock's book offers an important corrective to a literature focused too narrowly on ratification, and it provides valuable insights to understand how even a legally nonbinding action like signature can induce meaningful behavioral change. Although the book will be of particular interest to human rights scholars, these latter insights are potentially applicable to a wide range of questions in international relations and political science more broadly. It is not uncommon for states to comply with international laws, norms, and customs to which they are not legally bound. Comstock's argument helps explain why.

### **Response to Robert A. Blair's Review of *Committed to Rights: UN Human Rights Treaties and Legal Paths for Commitment and Compliance***

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— Audrey L. Comstock

I thank Robert A. Blair for the thoughtful review and critical engagement with my book *Committed to Rights: UN Human Rights Treaties and Legal Paths for Commitment and Compliance*. I was particularly pleased that the book's call to expand the study of treaty commitment beyond ratification was seen as a convincing argument and a beneficial path for both human rights and international relations scholars.

The central criticism that Blair raises is an important and challenging one: Does the book really disentangle and isolate the unique impact that human rights treaty commitment has from conditions that lead states to commit to treaties in the first place and from the other factors shaping compliance? This is a challenge with which scholarship examining treaty commitment, and human rights treaty

commitment in particular, has wrestled since the burgeoning of the quantitative study of international human rights law in the late 1990s. As Blair notes, researchers deploy several strategies to tackle this question and distinguish between selection and treatment effects, including matching techniques, instrumental variable estimation, and complementary case study pairing; my book uses the last two.

I agree with Blair that these strategies are imperfect and frequently lead to more questions about the sequencing and motivations of various actors as they engage with international human rights law. He raises particularly good critiques related to the UN CRPD and CERD cases. Given President Johnson's other efforts addressing racial discrimination in the United States, how can the signing of CERD be isolated and assigned the full weight of changes? In many ways, the cases offer illustrations of how the mechanisms of change can work in domestic contexts. Johnson was a supportive executive, I argue, who used treaty signature to signal support for the treaty rights before a divided US Senate could. In this case, signature was a more informative commitment act for examining domestic change than ratification, which occurred decades later after a supportive executive moved to embed rights changes. Limiting understanding of CERD commitment in the United States at the time to only an absence of ratification, which was the approach earlier studies took, served to overlook the nonbinding commitment, executive support, and ways NGOs and other groups could mobilize around signature leading up to ratification. I do agree, however, that Blair is correct to push for more disentanglement of how signature contributed to this process, even in this enhanced context.

Blair's thoughtful critique of selection and treatment effects suggests an important additional question: At what point do we distinguish selection from treatment effects when it comes to treaty creation and compliance? Exploring rights commitments argues for broader analysis of when states became involved in the treaty-making process. However, given the nature of commitment, signature selects into ratification, and treaty negotiation can select into signature and ratification. The multiple layers and steps involved in treaty creation and commitment have, for the most part, been viewed separately or overlooked as components of treaty commitment.

In the book, I use strategies like instrumental variables, case studies, and specific analyses of state votes on treaty draft provisions to disentangle interests in sets of human rights issues from negotiations themselves and commitment. However imperfect, these strategies do address selection into treaty creation and commitment to a greater degree and lend support to the overall argument of the book that commitment actions should be disaggregated and contextualized in terms of domestic politics and that they matter for compliance outcomes.