Critical Dialogue

Making and Unmaking Nations: War, Leadership, and Genocide in Modern Africa. By Scott Straus. Ithaca, NY: Cornell University Press, 2015. 400p. $79.95 cloth, $26.95 paper.
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— Paul Staniland, *University of Chicago*

Scott Straus has written an extremely important book, arguing that genocide has crucial ideological foundations, but that these conditions only lead to genocide when situational incentives drive a process of escalation. This contribution highlights the central role of ideas as a cause of genocide, while also outlining forces of restraint that can hold mass categorical violence at bay. Anyone interested in political violence must engage with this book.

Making and Unmaking Nations is driven by two observations. First, many existing explanations of genocide heavily overpredict its onset. Second, much of this literature does not systematically study "noncases" in which genocide could plausibly have occurred but did not. According to Straus, thin theories and sparse empirics limit our understanding of genocide and mass killing.

Straus offers several correctives to the unsatisfying state of the field. The most important is to insist on the causal importance of ideas. This is a leading contribution to a nascent ideational turn seeking to systematically incorporate ideas into our understanding of political violence. It marks a break from recent political violence research, which too often avoids or takes for granted political preferences, fears, and commitments in favor of purely organizational, strategic, and military explanations.

Straus argues that political systems are most likely to be sites of genocide when state-building elites create a "foundational narrative" that identifies a "primary identity-based population whom the state serves" (p. 64). These exclusionary narratives lead elites to perceive threats from excluded groups that are seen as both subordinate and dangerous, and to make their annihilation as a social category seem both thinkable and necessary. He contrasts these exclusionary narratives with inclusionary narratives that "have little to do with primary political communities" (p. 64). In these contexts, social groupings are not arranged into hierarchical categories and mass categorical violence is consequently not seen as a solution to political challenges.

Straus argues that these foundational narratives can be measured and compared (p. 67), focusing on the discourses advanced by leaders at moments of decolonization and regime change. Leaders, he argues, have substantial, though not unconstrained, agency in how they forge these narratives. Threats are interpreted and state strategic interests are constructed, rather than given or obvious.

Yet this is not a simple unicausal argument, since foundational narratives are potentially compatible with a wide range of state policies. The book offers two lines of argument to explain when exclusionary ideologies actually contribute to genocide. First, Straus notes that genocide generally occurs in contexts of warfare and political instability, in which material security fears combine with ideological frames to heighten threat perception and incentives for exterminatory violence (pp. 34–41). Genocide is the result of a process of escalation, often over years, rather than a single decision. He provides cross-national quantitative evidence from sub-Saharan Africa that shows the importance of threat and security for explaining variation in mass killing (Chapter 4).

Second, there are important overlooked sources of restraint that can put brakes on an escalatory process. Straus emphasizes, correctly, national-level forces in contrast to micro- and meso-level forms of restraint that, he suggests, can be overridden by determined elite efforts (pp. 42–48, p. 53). Restraint at the level of state leadership may occur through any or all of four mechanisms: ideological counternarratives, state capacity, international pressure, and economic costs (pp. 75–79). These can push back against escalatory pressures, helping to explain why so many cases that seem primed for mass killing come back from the brink.

Straus combines the ideological, security, and restraint strands of the theory most fully in five detailed case studies of both genocide and nongenocide. He compares state responses to armed challenges in Cote D'Ivoire, Mali, Senegal, Rwanda, and Sudan. We see mass categorical violence in Sudan and Rwanda, but much more restrained, accommodative state strategies in the other three less-well-known cases. Straus's extensive interview and historical research lend serious credibility to his argument: His systematic comparative reconstructions of founding narratives in each case are carefully researched and presented. He has done the hard empirical work of tracking down surviving policymakers, compiling extensive public statements by key leaders, and delving into the operational activities of state counterinsurgent forces. I learned an enormous amount from the cases even beyond their value for probing the theory's plausibility.

The theory and evidence Straus provides are compelling. They force scholars to take seriously the ideological foundations of politics and the agency of elites in imagining the political community. After *Making and Unmaking Nations*, it is no longer sufficient to distinguish regimes by their Polity score, per capita GDP, and/or military power and then call it a day. Understanding the deeper politics of elites' definitions of state and nation is essential for understanding genocide.

Nevertheless, there are limitations to Straus's approach. His dichotomy between inclusionary (good) and exclusionary (bad) narratives creates two kinds of problems. First, even inclusionary discourses define inclusion *on the terms of* state elites. Discourse here remains an exercise of power: What looks like a pluralistic, inclusionary conception in the eyes of national leaders may be seen as a hegemonic, assimilatory, and ultimately malevolent political project from the perspective of those who have little interest in the ostensibly pluralistic inclusion on offer.

Regardless of whether one likes the substantive results or not, state and elite political discourses discipline and define the political arena. Invocations of the nation, unity, or shared culture often silence or ignore dissident mobilization. Indeed, elites who herald "unity in diversity" (as in Senegal, p. 226) and pluralistic democracy feel compelled to garrison the political arena against threats to these commitments. Senegal and Mali have experienced recurrent, long-running insurgencies despite narratives that Straus praises for their inclusion and accommodation.

The discourse of inclusion and opportunity that is so powerful in the United States does important political work by eliding key hierarchies and exclusions that are also foundational to American politics. Whether in postwar Europe's militant democracies or Nehruvian India, elite visions of pluralism and nationalism have been haunted by the specter of enemies within. This book would have benefited from a more cold-eyed understanding of discourse and ideas as intertwined with political power and nationalizing projects. While the expansiveness of boundaries certainly matters, so too does the location of those boundaries. In both inclusionary and exclusionary regimes, ideology helps to determine where to lay down coercive *cordons sanitaires* against dangerous ideas.

The second challenge created by the inclusion/exclusion dichotomy is what to do with ambiguous or "in-between" social groupings. The author's evidence repeatedly shows political leaders neither fully including nor completely excluding particular social categories. Many social groups seem instead to straddle the boundaries of the body politic. Straus struggles in the empirics with exactly how to deal with ambiguous, contested, or changing elite discourses about the political community. Even in his seemingly straightforward cases of exclusion, we are told that in Rwanda, President Habyarimana's "record is not clear-cut" (p. 287) over 25 years, while in Sudan, "it was also not that non-core groups lacked citizenship rights entirely" (p. 272). Agency and structure operate in an ultimately (and understandably) unresolved tension with one another as these discourses emerge and evolve.

The important variation is therefore not whether states seek to make or unmake a nation; it is instead about the content of the nation. Straus's brilliant invocation of "the political grammar of the state" (p. 172) moves us into a terrain of power, contestation, and imagination that remains to be fully exploited.

The author makes a major contribution by emphasizing restraint. Yet future work would do well to tighten and better specify his arguments. At present, they are numerous and multifaceted, even at the macro level (leaving aside the complex interactions among elites, local powerholders, and individuals). This complexity is both a feature and bug: It valuably reveals diverse mechanisms of both escalation and de-escalation, but opens the door to post hoc explanations that could allow less scrupulous scholars to explain away cases that would otherwise challenge the argument.

For instance, "capacity" alone has four distinct dimensions (p. 77). It is not clear how these subcomponents aggregate, much less how the four overall forms of restraint relate to one another and when they should be most/least powerful. Straus has done valuable work in putting restraint more fully on the scholarly agenda, but the next wave of research on genocide needs to prioritize and structure theories of restraint in order to generate more specific predictions.

Making and Unmaking Nations is a major achievement. Not only does it help us better understand the ever-vexing question of genocide, but it also identifies key open questions for future research and offers a set of useful policy diagnostics and prescriptions. As the prospect of mass killing looms over ongoing conflicts in the Middle East, Africa, and Asia, this is a particularly timely and important work.

Response to Paul Staniland's review of *Making and Unmaking Nations: War, Leadership and Genocide in Modern Africa*.

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— Scott Straus

Many thanks to Paul Staniland for his thoughtful assessment of my book. He identifies and parsimoniously conveys the book's main contributions. It is such a pleasure to have a smart, incisive, concise review of one's work!

Staniland develops three important critiques. First, he argues that even benign-sounding political discourse constitutes a practice of power. Political narratives of pluralism, integration, and inclusion—which, I argue, make the strategic choice of genocide less likely—may alienate some in society and elide ethnic hierarchies. Agreed. I see now how more attention to this matter would have been useful.

That said, some context is in order. The book compares genocide cases to non-genocide cases. The latter are not Denmark. They are situations that could result, plausibly, in genocide. They are countries with deep political instability in a civil war with states that have committed significant human rights violations, and with militias that have sown havoc and violence. I find that certain political narratives restrain elites from choosing to respond to military threats through mass categorical violence. The implication is not that the situation in these countries is hunky-dory or that these narratives are unequivocally "good." The claim is that these narratives tip dangerous situations away from mass violence and toward political accommodation. While I see how Staniland develops the implication he does, that is, that I am making normative claims about "good" and "bad" political discourse, the intention was to limit the argument to the ways in which elites develop strategies of violence.

Second, Staniland argues perceptively that the case studies show "political leaders neither fully including nor completely excluding" social categories. Again, agreed. Indeed, after spending time with the empirical record, I concluded that the reality is not a simple dichotomy of "exclusionary" versus "inclusionary" narratives, as Staniland suggests. Rather, genocide is more likely when a founding narrative establishes a primary identity-based population whom the state serves (p. 66). Through time, in the genocide cases, elites sometimes provided citizenship rights and afford limited political power to non-core populations. However, those leaders always maintained a hierarchical vision of political community and political power; they associated an identity population with the core political community and with the state's rightful ruler—in contrast to the non-genocide cases, where elites did not associate the state with a core identity population. In short, I agree with Staniland's reading of the empirics, and I sought to represent that nuance in the book.

Third, Staniland raises a concern that my theoretical emphasis on restraint requires better specification. The book develops two claims. First, the book makes the theoretical case for the reasons that restraint should factor into the analysis of violence, and the book offers a number of plausible sources of restraint. Second, the book finds empirically that three sources of restraint mattered, in addition to some narratives: capacity, economic structure, and external intervention. Capacity is a notoriously slippery concept, and thus I disaggregate it into coordination, identification, control, and infliction. I argue that all are necessary for genocide (with some rare empirical exceptions that I discuss). That said, Staniland's point is well taken. Restraint is something of a residual category; no single source of restraint reappeared across the cases. I find that restraint matters, but I would be delighted if future research refines the claims.

I am grateful for Staniland's careful reading of *Making and Unmaking Nations*. I hope the contributions he identifies will resonate for readers. Other readers may benefit from different parts of the book, in particular where I discuss the concept of genocide, the legacies of leadership in Africa, a framework for atrocity prediction and identification, and a template for atrocity prevention and response.

Networks of Rebellion: Explaining Insurgent Cohesion and Collapse. By Paul Staniland. Ithaca, NY: Cornell University Press, 2014. 312p. $82.50 cloth, $27.95 paper.
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— Scott Straus, *University of Wisconsin, Madison*

Paul Staniland is emerging as one of the most creative and influential scholars of political violence. His *Networks of Rebellion* will cement that reputation. Already the winner of two awards (the Joseph Lepgold Book Prize and the Peter Katzenstein Book Prize), the book is a model of cogent theorization, inventive but systematic research design, and effective writing. It constitutes a substantial contribution to the scholarship on political violence and a milestone for field-based, comparative research.

The book's central research question is straightforward: What explains insurgent cohesion and collapse? The inquiry matters for at least three reasons. First, the puzzle is real. Some rebel organizations are durable and effective; others are not. That variation exists across and within countries. Why? As far as I know, political scientists have not asked or answered that question in any detail.

Second, civil wars take (at least) two to tango. States fight rebels, and vice versa. Understanding civil wars means understanding insurgent organizations. However, to date, the literature has privileged states and country-level characteristics that make civil wars more or less likely. Staniland's work forces us to focus on the rebel organizations themselves. He is not alone. Jeremy Weinstein wrote a key book on the social and economic endowments of rebel organizations. Ana Arjona, Nelson Kasfir, and Zachariah Mampilly, among others, have examined how rebels govern. Fotini Christia explores alliances among rebels. Janet Lewis and Jason Stearns also look at the initial foundations of rebellion. Staniland's book is thus part of a growing research agenda on insurgents and a core contribution to that area.

Third, the political violence literature often divides into macro-level and micro-level research. The former privileges national-level variables and processes, the latter individual-level ones, such as recruitment or participation in violence. With a focus on the social bases of insurgent organizations, Staniland's research draws attention to the "meso" level.

Staniland's core argument is that the characteristics of "prewar politicized social networks" (p. 9) shape the cohesion and effectiveness of insurgent organizations. The author cites political parties, peasant associations, kinship groups, religious associations, and student organizations as the kinds of organizations that influence the strength of rebel organizations once they form. He calls his theory a "socio-institutional" one.

Staniland identifies four types of insurgent organizations: integrated, vanguard, parochial, and fragmented. Integrated are the most durable, fragmented the least. He in turn analyzes organizational strength along two dimensions: vertical and horizontal. Vertical ties shape the relations between the top and the bottom, between the leaders and local communities. Horizontal ties connect people across space. They "make possible the consolidation of shared political visions at the regional or national level" (p. 21).

The initial ties shape the strength of insurgent organizations. Integrated rebel organizations exhibit strong cohesion among the leadership (horizontal ties) and strong cohesion from the leadership to the ground level (vertical ties). By contrast, vanguard organizations are cohesive at the top, horizontally, but do not extend their control to the local level. Parochial groups demonstrate strong vertical ties but weak horizontal ones; they are built on patrimonial connections to individual leaders. Fragmented groups cannot build on any kind of social ties to build their organization.

The argument is not deterministic. Staniland accounts for insurgent change. He identifies insurgent strategy, counterinsurgent policy, and external sponsorship as key sources of change. Through these and other influences, insurgent organizations may shift from being integrated to parochial, or vice versa, and from vanguard to integrated, or vice versa. Staniland's effort to incorporate endogenous processes of change into this theory is one of the strengths of the book. Another is Staniland's transparency: Throughout, he identifies how his theory is falsifiable. In the empirical sections, he is forthright when the evidence does not support his argument.

Staniland's framework is generalizable. Scholars studying insurgents in any world region should be able to apply his theory. That said, the book's empirical strategy and empirical sections are extremely compelling. The research design includes both cross-country and within-country comparisons. The most in-depth research covers South Asia. Staniland examines the fate of five insurgent organizations in Indian-administered Jammu and Kashmir; seven organizations in Afghanistan; and five organizations in Sri Lanka. In so doing, he is able to examine variation in insurgent fate while holding the conflict and country constant. In addition, he studies three communist insurgent organizations in Southeast Asia—in Malaya, French Indochina (Vietnam), and the Philippines. This provides "out of sample" validity to his arguments.

The case studies are carefully rendered and impressive. Insurgent organizations are not an easy subject to research. Staniland did 10 months of fieldwork in India and Sri Lanka. He also conducted in-depth secondary research in those countries and the others. This is an ambitious empirical agenda for a first book; mastery of multiple countries usually takes scholars many years.

For the reasons noted, *Networks of Rebellion* is a major accomplishment. All the same, I wish to raise a few concerns. First, there is a tension in the theory between initial structure and change over time. The book's theoretical emphasis, it seems to me, is the initial social base. Yet the case studies are primarily about how insurgent organizations shift from one type to another. With few exceptions, organizations change; they do not start and stay the same type. Those trajectories raise a conceptual question about whether there are, in fact, four insurgent "types." More importantly, the trajectories suggest that the key question is how insurgent organizations adapt to war and manage adversity. Staniland claims that the initial social base makes some organizations better at adaptation than others. But how is the initial foundation versus major factors thereafter, such as leadership, strategy, state behavior, the action of rival rebels, and international sponsorship, to be balanced? Staniland is aware of these dynamics. Still, the question remains: What is doing the work of social cohesion? And how do we know?

Second, the theory's main causal variable is close conceptually to the main outcome. Crudely, more integrated prewar networks produce more integrated insurgent organizations; weaker prewar networks produce weaker rebels. I like Staniland's emphasis on history and institutional footprint. Still, is there something tautological about claiming that strongly networked foundations lead to stronger organizations or that weakly networked foundations lead to weaker organizations?

Third, I would have welcomed additional clarity on, and measurement of, key concepts. For example, "networks" is the in the title, yet the text primarily refers to "social bases." Are they the same? What is the working definition of a network? How is a strong vertical network or social base measured? How is insurgent cohesion or fragmentation measured? Staniland sometimes introduces tantalizing ideas, such as how "shared political meaning" (p. 217) is key to building an organization. Yet this idea remains underdeveloped in the text.

Those concerns aside, Staniland's work is seminal. To understand civil war we need to understand insurgents, and to understand insurgents we need to look at the social foundations of insurgent organizations. Rebellions are, after all, hard to sustain. Rebel organizations face unusual types of strains and stresses; states and rival rebel organizations seek to destroy them. Staniland's parsimonious theory provides a compelling and concrete answer about which rebel organizations are most likely to survive. *Networks of Rebellion* is impressive empirically. The writing is clear and compelling. The book deserves the recognition it has received and is likely to continue to receive.

Response to Scott Straus's review of *Networks of Rebellion: Explaining Insurgent Cohesions and Collapse*

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— Paul Staniland

Insurgent groups are key military and political players throughout the contemporary world. *Networks of Rebellion* seeks to improve our understanding of the ways in which they mobilize, organize, and fall apart. I am deeply grateful to Scott Straus for his careful assessment of the book's strengths and weaknesses. While Straus generously assesses it as "seminal" and "a milestone," he also identifies three areas of concern that will be my focus here.

The first is my argument about the sources of change over time. As Straus rightly notes, insurgent change is complicated. There are numerous variables that can come into play, from individual leaders' decisions to macro-historical shifts in the international system. Chapter 3 of *Networks* systematically links the origins of groups to their most likely pathways of change. Consequently, vanguard groups are likely to change in different ways than parochial or integrated organizations. These differences are caused by the tensions and opportunities created by the underlying social bases.

This is an important advance over existing theories, which have either made the case for path dependence or taken organizational structure as a given and used it to explain other outcomes. *Networks* provides a new framework for conceptualizing and measuring variation over time. It puts theoretical structure on the processes of change that we are most likely to see, and, importantly, identifies pathways that are unlikely for eachtype of group.

Straus is right that this approach is not comprehensive or able to explain everything; far from it. There are real limits to what the book achieves in this area. It aims to launch a scholarly conversation rather than to decisively end it. I hope other scholars will expand on this agenda to deepen our understanding of insurgent evolution.

Straus is also concerned about the linkage between social bases and organizational outcomes. They are certainly, and importantly, connected, but the theory is not tautological. I repeatedly identify cases in which social bases and organizational structure do not align. Differences between the two can be easily measured empirically, while alternative theories provide explanations concerning how these disjunctures may occur. I further show in the empirics that founders of prewar social bases often did not create these structures with an eye to protracted future insurgency. The claims of *Networks* can be evaluated and disconfirmed.

Finally, Straus has several fair concerns about operationalization. The problem I faced in *Networks* is that the manifestations of social bases are specific to individual societies: Politicized prewar networks in 1980s Egypt likely take on different forms than in 1910s China or 2014 Iraq. This leads to theoretical sparseness that can risk under specification. I try to make up for some of these problems by using detailed sub- and cross-national comparisons, but the most important test of the book will be whether future researchers can concretely apply the concepts to a wide range of cases.

I appreciate Straus's excellent critiques. They identify crucial areas for further research that can both build on and move beyond *Networks of Rebellion*.