H-Diplo | ISSF

Roundtable, Volume VII, No. 16 (2015)

A production of H-Diplo with the journals *Security Studies*, *International Security*, *Journal of Strategic Studies*, and the International Studies Association's Security Studies Section (ISSS).

http://www.issforum.org

H-Diplo/ISSF Editors: **James McAllister and Diane Labrosse**
H-Diplo/ISSF Roundtable and Web/Production Editor: **George Fujii**
Commissioned for H-Diplo/ISSF by James McAllister

Introduction by James McAllister

Paul Staniland. *Networks of Rebellion: Explaining Insurgent Cohesion and Collapse.* Cornell University Press, 2014. ISBN: 978-0-8014-5266-6 (hardcover, $79.95); 978-0-8014-7929-8 (paperback, $27.95).

Published by H-Diplo/ISSF on **11 May 2015**

Permalink: http://issforum.org/roundtables/7-16-networks-of-rebellion/
PDF URL: http://issforum.org/ISSF/PDF/ISSF-Roundtable-7-16.pdf

Contents

© Copyright 2015 The Authors.
This work is licensed under a Creative Commons Attribution-NonCommercial-NoDerivs 3.0 United States License.

Introduction by James McAllister, Williams College

Understanding the nature of insurgencies has long been an important objective for political scientists, historians, and policymakers. In *Networks of Rebellion*, Paul Staniland argues that scholars have paid insufficient attention to the different organizational structures of insurgent groups. In his view, understanding organizational structure is crucial because "states and their foes spend far more time and resources on organization building and institutional survival than on formulating intricate strategies of violence…Like logistics, organization consumes the attention of professional war-fighters" (220). What explains the different organizational structures of insurgent organizations? Staniland argues that a crucial determinant of the structure of insurgent groups during wartime is the nature of prewar political life. While organizational structures can and do change during wartime, he argues that the prewar ties between elites and local communities "determine the strength of central and local organizational control when rebel leaders mobilize that based for rebellion" (9).

Staniland's theoretical innovations are combined with well researched case studies of insurgent organizations in Kashmir, Afghanistan, and Sri Lanka. All the reviewers agree that *Networks of Rebellion* is an important contribution to the growing literature on civil wars. Fotini Christia arguesthat the book "lucidly shows how rebel groups are born from pre-war networks that act as focal points for mobilization, information, and trust." Kathleen Gallagher Cunningham asserts that Staniland "has made a significant contribution to our understanding of rebellion." *Networks of Rebellion*, according to Idean Salehyan, provides "a rich and theoretically nuanced account of how militant organizations mobilize around pre-existing social bases and come to change over the course of the conflict."

Nevertheless, it is also the case that all the reviewers also have some concerns about Staniland's argument. Christia suggests that Staniland's typology of organizational structures may be of more value to historians than policymakers since she believes it may be too difficult for policymakers to know what type of rebel organization they are confronting in real time. Cunningham is not convinced that "integrated" rebel groups are necessarily the "ideal type" of structure for a successful insurgency. While Staniland's narrative is persuasive when it is concerned with organizational type, Salehyan argues that it is less effective in accounting for the outcomes of wars and the nature of state-building. In addition, he states that Staniland "does not offer clear hypotheses or independent variables that can be evaluated."

H-Diplo/ISSF thanks Professor Staniland and all of the reviewers for contributing to this timely and important debate.

Participants:

Fotini Christia is an Associate Professor of Political Science at MIT. Her research interests deal with issues of conflict and cooperation in the Muslim world. Fotini has done extensive ethnographic, survey and experimental research on the effects of development aid in postconflict, multi-ethnic societies with a focus on Afghanistan and Bosnia. She is the author of *Alliance Formation in Civil Wars*, published by Cambridge University Press and awarded the 2013 Gregory M. Luebbert Award for Best Book in Comparative Politics. Her research has also been published in *Science* and in the *American Political Science Review* among other journals and she has written opinion pieces for *Foreign Affairs*, the *New York Times*, and *The Washington Post*.

Kathleen Gallagher Cunningham is an Assistant Professor in the Department of Government and Politics at the University of Maryland. She received her Ph.D. from the University of California, San Diego and is affiliated with the Peace Research Institute Oslo where she visited as a Fulbright Scholar in 2010. Her research focuses on the politics of self-determination, nationalism, and civil conflict. Her work has been published in the *American Political Science Review, International Studies Quarterly*, and the *Journal of Conflict Resolution*.

Idean Salehyan is Associate Professor of Political Science at the University of North Texas. His research has primarily focused on civil and international conflict, transnational security threats, and the political consequences of environmental scarcity. He is the author of *Rebels without Borders: Transnational Insurgencies in World Politics* (Cornell University Press, 2009). He has also published articles in journals such as the *American Journal of Political Science*, *International Studies Quarterly*, and *World Politics*.

Review by Fotini Christia, MIT

Networks of Rebellion: Explaining Insurgent Cohesion and Collapse, Paul Staniland's recent book, is an intelligently theorized and empirically substantiated study of rebel organization in civil wars. Its argument relies on a powerful premise: that we cannot understand how wars are fought, how long they last, and how they end, unless we have a good grasp on how rebel groups are actually organized. Staniland's theory opens up the black box of insurgencies, whose organization and structure most works have treated as monolithic. In doing so, the book lucidly shows how rebel groups are born from pre-war networks that act as focal points for mobilization, information, and trust. These pre-existing social structures, political, civic, or religious, he argues, affect not only how insurgent groups emerge but also how they evolve.

Organizational cohesion is an important determinant in Staniland's theory and he shows us how groups work hard to try to attain it if they do not have it, and to maintain it if they do. Cohesion is measured through the effectiveness of interactions between leaders within the group and through their relationships to their constituents (linkages which he respectively terms horizontal and vertical ties). The combination of central and local ties determines whether the group falls under one of four categories: integrated, vanguard, parochial, and fragmented. And where a group lies in this typology at the onset of civil war also affects how its organizational structure changes during the conflict's trajectory and how it is affected by the group's ideology; its internal and external resources; as well as the warring state's counterinsurgency strategy.

Right after the end of the Cold War, scholars used theories from international relations, such as the security dilemma or the commitment problem, to explain civil conflict onset and wartime dynamics. In that context, warring groups were seen as unified actors and their structure, identity, and motivations treated as largely exogenous. A corrective to that rather simplified approach came a decade later, with Stathis Kalyvas's classic book, *The Logic of Violence*,[1] which compellingly showed how endogenous dynamics in civil wars affect how people fight, who they see as their enemies, and how they perceive of wartime cleavages. Staniland's book comes to bridge the two paradigms and show what it is that we can consider exogenous in how insurgent groups are formed and in how they adapt during the war. In that regard, the book offers some institutional constraints for a rebel group's evolution by wartime dynamics. Looking at the prewar organizational context, as well as the wartime groups through carefully crafted within and across case comparisons, this is qualitative work immune to accusations of cherry picking. Not only does Staniland clearly tell us what the outcomes would be if his theory was wrong, but he also entertains alternative explanations including instances that do not offer support for his argument.

There is undoubtedly something really powerful about the path dependence around these prewar social-structural blocks of rebel groups that Staniland puts forth. It would be great to be able to hold on to some sort of genetic determinism by pre-existing network

[1] Stathis Kalyvas, *The Logic of Violence in Civil War*, New York: Cambridge University Press, 2006.

structures that can only mildly be adjusted and finessed by wartime environmental factors as a way to know how to bring conflicts to an end. And though the book makes a strong case about its implications for recruitment, violence against civilians, and even post conflict politics, it still leaves the reader wondering how truly predictive such a theory can actually be, if not of civil war outcome, at least of patterns of violence.

Specifically, the theory comes across a bit like a recipe—if you know the type of group you are dealing with, then as a state you know the appropriate counterinsurgency strategy to pursue. But how realistic is it for states facing an insurgency to correctly assess rebel group organizational structures in real time and update those assessments as wartime dynamics unfold? Civil wars are notorious for their areas of contested control and plagued by incomplete and asymmetric information. Staniland's typology seems retrospective-something that really can only be assessed after closely reviewing the historical record. If it is actually feasible and not too costly for states and outside actors to accurately assess group type, why haven't they done so? Why is the counterinsurgency field so replete with failures, as the author himself notes when he discusses the recent cases of Afghanistan and Iraq?

Moreover, we usually perceive of insurgencies as fighting between a state and rebel groups over the allegiance of civilians. In Staniland's book we get a close look at the institutional innards of a rebel group, and through this anatomical experience we get an indirect sense of how the state matters; an even more indirect sense of how civilians matter; and very little sense of how interactions with other rebel groups matter. While we know how a group may move across the four types based on where it started off and on how a state or external sponsor may affect its options, we don't clearly see how other rebel groups and relations with them may affect these options. Does the number and type of other rebel groups matter for inter-rebel group competition beyond discussions of fragmentation or consolidation? Relatedly, should the four categories in the typology be understood in absolute terms or in terms relative to other groups in the fight?

A theory, of course, cannot explain everything-it needs to be falsifiable, while striking a fine balance between parsimony and explanatory power. Staniland does a very good job outlining his scope conditions and making clear what it is that the theory can and cannot explain. And though the empirical material is very well presented -- particularly the cases of India and Sri Lanka, where the author spent extensive periods of time in the field and has a rich set of interviews -- it is still not fully clear how much of it was used to build the theory and how much to illustrate it and test it. It would therefore have been a worthwhile plausibility probe to show how the theory fares against the universe of cases that fall under its scope, which, according to the author, is about half the cases of post-1945 civil wars, and how representative the selected cases are as compared to all the relevant cases in the sample.

An understanding of the evolution of organizations over time in every single case would have undoubtedly be quite time consuming, but at least a coding of the prewar levels of ties and organizational structure, as well as what the groups looked like at the end of the conflict (though they would only offer data points at civil war onset and termination), could have served as a great further external validity test for Staniland's theory. They would have offered good descriptive statistics not only on case selection, but also on the distribution of groups across the different types in different wars, i.e., if there tend to be more integrated versus more parochial groups at the beginning, what that distribution looks at the end, and how it correlates if at all with conflict outcome. If the answer is that it is too painstaking or impossible to realistically do retrospectively, why should we expect states to be able to do that in real time during war when the constraints would arguably be more egregious?

Review by Kathleen Gallagher Cunningham, University of Maryland and Peace Research Institute Oslo

Networks of Rebellion is a tour de force, providing a new theory for understanding why rebel groups have different types of internal organization, and why some hold up to the pressures of war while other collapse. The organization of rebellion is critical for understanding both patterns of violence and the ways that wars end.

Paul Staniland argues that pre-existing social networks create varying resource environments for rebels to draw on. In particular, some social networks allow for greater information flows, the development and maintenance of trust among opposition actors, and promote shared interests and goals. In general, the initial character of rebel organizations will mirror the social network in which they form.

The critical factors that Staniland focuses on to classify rebel groups are the existence of a politicized opposition, and the horizontal and vertical ties among opposition members. Horizontal ties are connections between elites in a rebellion (such as ties among clerics or among labor leaders). Vertical ties are the linkages between individuals in a rebel group and the community they come from (or link into). Rebel groups' structure can be arrayed along these two dimensions: "integrated groups" have strong vertical and horizontal ties, "vanguard" groups only have strong horizontal ties, "parochial" groups have only strong vertical ties, and "fragmented" groups are weak on both dimensions (6).

This theory is tested on a rich set of cases. Three cases provide comparisons across rebel groups (Kashmir, Afghanistan, and Sri Lanka). These studies are rich in detail and analysis, and Staniland clearly explains when his theory performs well, and when it does not. A further set of comparisons of communist insurgencies in Southeast Asia show that Staniland's approach applies more broadly.

One of the strengths of this book is how it deals with complexity and change. The theory is presented clearly, but it is also quite complex, particularly when Staniland deals with the processes of change for rebel groups. Chapter Three lays out a number of ways in which different types of rebel groups change. He emphasizes the role of counterinsurgency (leadership decapitation and disembedding, which are attacks on horizontal and vertical ties respectively) and how rebel groups can ineptly try to expand, inadvertently weakening themselves. The processes for each group are well reasoned, and Staniland walks the reader through them in detail.

Underpinning this treatment of both group type and change is the assumption that integrated groups are the ideal type for rebel movements. That is, all groups would benefit from strengthened vertical and horizontal ties. This assumption seems to stem from observing how groups fail. The fourth type of group – fragmented – is less of a type and more a process of collapse. Thus, groups with either weak vertical or horizontal ties are one step away from collapse. Yet, it is not clear that the integrated group should be seen as an ideal type, or at least not for all wars. They may be the most resistant to the types of counterinsurgency that Staniland focuses on, but in terms of getting concessions from the state, it is not clear that these groups are ideal.

The role of accommodation more generally is unclear. A strength of the study is to show how groups develop out of existing social networks. Yet, what is the consequence of early movement success on this development? The state is largely assumed to be focused on counterinsurgency. If nonviolent organizations in the politicized opposition network get some of what they want (such as concessions related to student or labor protests) does that lead to exclusion from the social network as a potential resource for rebels? Theoretically, there is very little discussion of accommodation by the state and how that shapes the social network resource constraints and opportunities.

The qualitative nature of the study allows for a rich description and analysis of a set of cases. This design is particularly well suited to address the issues of imbeddedness and trust that Staniland deals with. Yet, a challenge for this approach is to envision indicators across a larger set of cases. Staniland's categorization of rebel groups relies on an assessment of vertical and horizontal ties into the community, treating these as equally salient. Are there conditions under which the central or local ties play a larger role in group cohesion or formation? Or are they always equally important in determining group type? Arguably other contextual factors could impact this, such as rebel groups or social networks that cross borders. Moreover, the ability of people in a social network to move geographically, to change status within the state (economically or politically), or to organize in new ways may alter the salience of vertical or horizontal ties.

The role of timing is unclear in some of the theoretical discussions. Staniland makes a number of references to quicker and easier mobilization (which integrated groups should be best at) but it is not clear how the temporal dimension plays a role in sustaining rebellion. For example, vanguard rebels have trouble quickly organizing (28). But the main weakness of vanguard groups is identified as their inability to sustain local control. Sustaining control, however, is not necessarily related to timing per se. Staniland returns to the issue of timing in the conclusion, with respect to when counterinsurgency tactics will be most effective (219). Thus, with are left with a mixed impression of when and in what ways quickness and timing matter.

This book is a pleasure to read. It is elegantly written, well argued, and thoroughly researched. Staniland has clearly made a significant contribution to our understanding of rebellion. Moreover, this book is among the most policy-relevant works in political science at this juncture. It is not only a must read for scholars, but for practitioners trying to grasp the intricacies of insurgency, multiparty civil wars, and conflict resolution more generally.

Review by Idean Salehyan, University of North Texas

Research on civil war and insurgency has tended to ignore variation across rebel organizations and treat opposition groups as unitary actors. More recently, scholars have sought to examine different organizational types,[1] or explore how various groups within a movement interact with one another.[2] It has become clear that merely looking at structural factors such as economic development, demographics, and physical geography—or state-level variables such as regime type and state capacity—is insufficient for understanding how civil wars unfold. While not ignoring such factors, scholars must examine how rebellions organize themselves, interact with the population, and engage the state.

In *Networks of Rebellion,* Paul Staniland offers a rich and theoretically nuanced account of how militant organizations mobilize around pre-existing social bases and come to change over the course of the conflict. By focusing on horizontal as well as vertical ties between units, Staniland offers an extremely useful typology of organizational types. Integrated rebel groups are perhaps the most capable type as they form strong horizontal bonds across geographic space and strong vertical bonds linking organizers to local communities. Vanguard groups are those with a central cadre of leaders, but lack deep ties to towns and villages. Parochial groups do not have robust central command but are embedded in particular localities. Finally, fragmented opposition groups have fragile ties at the center and in localities. These organizational types do not emerge straightforwardly from factors such as ideology or clever planning by rebel entrepreneurs, Staniland argues, but come from pre-war political and social bonds. In other words, the way civil society is organized prior to conflict is a strong predictor of how rebel groups mobilize.

While this suggests a degree of path-dependence, each of these organizational types faces special challenges and opportunities that help to explain change. Although integrated groups tend to be robust, they can fall victim to overwhelming counterinsurgency efforts or fail to expand effectively. Vanguard groups are prone to leadership decapitation and can face defection by local units. Parochial groups may come to merge into a stronger central organization, or can devolve to infighting. And while fragmented groups may come to coordinate efforts, Staniland seems to suggest that such groups are doomed from the start. Therefore, while pre-existing social bases help to explain the initial organizational type, other factors including counterinsurgency efforts, assistance by foreign powers, and rebel infighting can cause organizations to consolidate control or fracture and dissolve.

The most impressive aspect of this book is the carefully argued, well-structured case studies. *Networks of Rebellion* largely focuses on South Asian cases, with chapters on Sri

---

[1] Weinstein, Jeremy. *Inside Rebellion: The Politics of Insurgent Violence.* Cambridge, UK: Cambridge University Press, 2007.

[2] Bakke, Kristin, Kathleen Gallagher Cunningham, and Lee Seymour. 2012. "A Plague of Initials: Fragmentation, Cohesion, and Infighting in Civil Wars." *Perspectives on Politics.* 10(2): 265-283.

Lanka, Kashmir, and Afghanistan. These three empirical chapters examine variation across rebel actors within a single conflict, which allows for controlled comparisons while holding structural factors, government responses, and regional geopolitics constant. For instance, in Indian-controlled Kashmir, the Jammu and Kashmir Liberation Front (JKLF) began as a vanguard organization as it consisted of a relatively close group of middle-class intellectuals but with weak ties to the countryside. By contrast, the Hizbul Mujahideen was an integrated group as it emerged out of the Jamaat-e-Islami political movement, which while not deeply popular, had the advantage of strong roots in local communities. While both groups ultimately failed in the face of overwhelming Indian counterinsurgency efforts, the JKLF was more deeply affected by leadership decapitation while the Hizbul Mujahideen was defeated when its ties to local communities were severed. The final empirical chapter moves beyond South Asia and compares the Malaysian Communist Party, the Huk rebellion in the Philippines, and the Viet Minh. Despite similar Communist/Maoist revolutionary ideology and doctrines, the groups evolved quite differently due to their distinct social bases.

Ultimately, Staniland's theoretical framework is quite convincing and offers a significant advance over the existing literature on rebel groups. One of the major drawbacks of previous literature, and especially quantitative work on civil war, is that it ignores contentious politics prior to the onset of violence. Pre-war dissident action in a highly constrained setting such as Libya, for example, looked quite different from such action prior to the Algerian civil war, although they would both appear as 'zeros' in most datasets. Rebel organizations do not appear out of thin air, but must navigate the social terrain as they win over recruits and collaborators, coordinate collective action, and vie for control of the state. *Networks of Rebellion* is a useful corrective in this regard as it offers rich narratives of the social pillars that South and Southeast Asian rebel groups stand on. In thinking about the genesis of rebel groups elsewhere, one can readily apply the theory to cases outside of the chosen case studies.

Nonetheless, scholars who are more accustomed to formal hypotheses with clearly operationalized independent and dependent variables will find the book somewhat lacking. In this respect, perhaps Staniland promises too much in the introduction. He argues that, "Insurgent cohesion shapes how wars are fought, how wars end, and the politics that emerge after war (2)." Further, he argues that his framework explains violence against civilians, the prospects for peace, and state-building after conflict (2-3). While this is probably the case, these themes are not thoroughly examined in the chapters that follow. The outcome Staniland appears to be most interested in—the dependent variable—seems to be the organizational type itself rather than war outcomes, state-building, and so on. Moreover, in explaining organization change, Staniland offers several pathways such as leadership decapitation, mismanaged expansion, and insurgent fratricide that *could* lead to change, but does not offer clear hypotheses or independent variables that can be evaluated. As such, the reader does not come away with firm expectations or predictions that can either be corroborated or falsified. The book could have done a bit more to provide a deductive theory and precise expectations regarding the outcomes of interest.

While the empirical analyses are quite strong, the discussion of the South Asian cases leaves open an additional empirical puzzle. All of these conflicts were multiparty civil wars with several rebel groups that drew upon distinct social bases. Why was the opposition split between numerous organizations in these cases, while in other conflicts (e.g. Turkish Kurdistan, the Cuban Revolution, the Salvadoran Civil War) there was a single organization or a united revolutionary front? Looking at pre-conflict social bases might offer some clues about why civil wars evolve into two-party versus multiparty conflicts, although Staniland does not address this question directly. The cases that were chosen were all 'fragmented' in the sense that the opposition movement was split between competing rebel organizations and an additional case or two exploring how unified movements emerge would have been interesting to consider.

In all, *Networks of Rebellion* is a must-read for scholars of civil war and those who are interested in the organizational dynamics of violent actors. Staniland's approach offers a solid theoretical framework for understanding rebel organizational types, which can be fruitfully applied to understanding a broad range of rebel and government behavior during conflict. Future research must take seriously the pre-war social and political environment to properly understand how civil conflicts unfold and ultimately end. As such, this book lays the foundation for an exciting research agenda that does away with the notion that "conflict" begins when the first shots are fired, and delves deeper into where militant groups come from.

Author's Response by Paul Staniland, University of Chicago

I am grateful to the editors of H-Diplo/ISSF for organizing this roundtable. I especially thank Fotini Christia, Kathleen Cunningham, and Idean Salehyan for their thoughtful and stimulating reviews. Each has done important research on civil war and it is a rare treat to have them read my work.

The reviewers have positive things to say about *Networks of Rebellion*. Cunningham calls it a "tour de force" and Salehyan argues that *Networks* is a "must-read." Christia concludes that the book is "intelligently theorized and empirically substantiated," using "carefully crafted within and across case comparisons" that are "immune to accusations of cherry picking."

They also offer smart critiques that I will be my focus here. I agree with many of the criticisms and concerns. Some of them reflect failings on my part; others were conscious decisions about what to emphasize. On some topics, I continue to disagree with the reviewers, but their concerns have made me think harder about my arguments.

Networks is built around an analytical typology of insurgent organizational structure. Integrated, vanguard, parochial, and fragmented insurgent groups have distinct social origins, internal lines of dissension and compliance, and pathways of possible change. The reviewers begin with several concerns about this core typology.

First, Christia objects that these group types can only be measured after the fact – "something that really can only be assessed after closely reviewing the historical record." If the typology is purely retrospective, it loses value as an analytical tool. Though I agree that there can be unclear or in-between cases – as in all typologies – it is not clear to me that this issue is as problematic as Christia suggests. The book repeatedly refers to contemporary cases, including Hamas, Al Qaeda in Iraq, the Pakistani Taliban, and the Afghan Taliban, and identifies their type.[1] Other reviewers are less concerned about the real-time value of the book.[2] There are undeniably ambiguous cases that we need to be careful in dealing with, but this is true of all efforts to simplify a complex world.

Second, Cunningham is not persuaded by my premise that integrated groups are the ideal organizational form which all groups seek to achieve. Cunningham is right: some groups intentionally try to de-centralize and de-institutionalize. In general, however, I think it is

[1] I also applied it to ongoing cases in Paul Staniland, "Every Insurgency is Different," *International New York Times*, February 16, 2015.

[2] Cunningham calls it "among the most policy-relevant works in political science at this juncture," while a review in the US Army's *Special Warfare* journal claims that "Army special operations forces leaders at all levels will find the concepts outlined by Staniland extremely beneficial to the building or destruction of insurgent organizations," http://www.soc.mil/swcs/swmag/archive/SW2704/27-4_OCT_DEC_2014_web.pdf.

reasonable to suggest that most insurgent groups, most of the time, are interested in centralized control over violence and governance. The vast majority of the case evidence I am familiar with shows leaders worrying above all about insufficient control, while favoring decentralization primarily for tactical and local initiative rather than fundamental changes in command and control. In cases where this assumption does not hold, the conceptual core of my argument may not work. I suspect that this is a relatively small subset of cases, but Cunningham is right that I could have analyzed these possibilities more carefully. We need more research on such decisions to de-centralize, as opposed to the common focus (including my own) on attempts to maintain or build cohesion.

Third, Salehyan suggests that my dependent variable is narrower than what the book promises. *Networks* suggests that organizational cohesion can affect war termination, negotiation processes, and the use of violence, among others, but it does not offer a general theory of how cohesion influences these various outcomes. He is right that there may be some slippage, since I do make tentative claims, especially in the concluding chapter, about these relationships. Based on a mix of cases and existing theories, it seems plausible to argue that, for instance, integrated groups are better equipped to survive counterinsurgency than parochial or vanguard groups, and that vanguards will have a different set of challenges in negotiating peace than parochial groups. But the book does not systematically examine these connections. I have now begun to explore how armed group organization helps to shape broader state-group political relationships.[3]

There is agreement that my theory has important virtues; Salehyan, for instance, writes that it is “quite convincing and offers a significant advance over the existing literature on rebel groups” and Christia finds that it contributes “something really powerful.” But the reviewers also advance several smart critiques.

The reviewers disagree about my approach to explaining organizational change. Salehyan suggests that my pathways of change could be more tightly specified (this criticism seems inapplicable to my theory of origins – Table 1.2 on p. 9 is straightforward) and is too open to indeterminacy: “the reader does not come away with firm expectations or predictions that can either be corroborated or falsified.” By contrast, Cunningham argues that “One of the strengths of this book is how it deals with complexity and change” and Christia concludes that “Not only does Staniland clearly tell us what the outcomes would be if his theory was wrong, but he also entertains alternative explanations including instances that do not offer support for his argument.”

I think Salehyan underplays the extent to which I specify when each mechanism of change is most likely (such as my discussions of local alliances on pages 45-46 or of local dis-embedding on pages 51-52), which also makes it possible to identify when I am wrong. In the Sri Lanka case, for instance, I explicitly note that my theory of change from vanguard to

[3] Paul Staniland, “Armed Groups and Militarized Elections,” *International Studies Quarterly* (forthcoming).

integrated type does not seem to be supported by the Liberation Tigers of Tamil Eelam (LTTE) case.

That said, I agree that I could have made these mechanisms clearer. In retrospect, Table 3.1 on page 38 was a missed opportunity to better summarize these pathways. It is also true that these are *possible* pathways that could happen, meaning that some of them may be uncommon or relatively unimportant. *Networks* offers one of the first theories of insurgent organizational change, and we do not yet know which pathways will prove to be most useful for explaining reality.[4] Salehyan is picking up on the problems that can accompany a first theoretical foray into new terrain. Ideally, over time scholars will get a better sense of the frequency and importance of each pathway; it may be that some of my hypothesized mechanisms of change rarely occur.

Christia argues that my framework gives insufficient attention to civilians. My conceptualization of vertical networks centers on links to local civilian communities, so civilians are certainly not absent. The difference between integrated and vanguard groups is determined precisely by varying connections to local civilian communities. But civilians' individual or collective decisions, it is true, are subsumed into these network connections. Civilians are not as central to this argument as in some other work on civil wars.[5] This is because insurgents (and state forces) have key organizational and collective action advantages over civilians that allow them to repress, co-opt, or ignore civilian actors. We still do not know very well how civilian sentiment aggregates up into overall organizational outcomes: it is possible that I am under-estimating the bottom-up effects of civilian agency, but it is important to keep in mind that governments and insurgents have a greater, though far from total, ability to set the rules of the political game. Future work will surely complicate the stark picture presented in *Networks*. Nevertheless, groups of men with guns can radically limit civilian options.

Christia and Salehyan both wonder about how organizational cohesion is affected by the existence of other groups. Christia notes that "we don't clearly see how other rebel groups and relations with them may affect these options. Does the number and type of other rebel groups matter for inter-rebel group competition beyond discussions of fragmentation or consolidation?" Salehyan asks "Why was the opposition split between numerous organizations in these cases, while in other conflicts (e.g. Turkish Kurdistan, the Cuban

[4] Valuable previous studies include Jeremy Weinstein, *Inside Rebellion : The Politics of Insurgent Violence* (Cambridge: Cambridge University Press, 2007); Claire Metelits, *Inside Insurgency: Violence, Civilians, and Revolutionary Group Behavior* (New York: New York University Press, 2010); Abdulkader H. Sinno, *Organizations at War in Afghanistan and Beyond* (Ithaca: Cornell University Press, 2008); Patrick Johnston, "The Geography of Insurgent Organization and Its Consequences for Civil Wars: Evidence from Liberia and Sierra Leone." *Security Studies* 17 (1): 107–37; Jacob N. Shapiro, *The Terrorist's Dilemma: Managing Violent Covert Organizations* (Princeton: Princeton University Press, 2013).

[5] See, among others, Zachariah Cherian Mampilly, *Rebel Rulers: Insurgent Governance and Civilian Life during War* (Ithaca: Cornell University Press, 2011); and Ana Arjona, "Wartime Institutions: A Research Agenda," *Journal of Conflict Resolution* 58 (8): 1360–89.

Revolution, the Salvadoran Civil War) there was a single organization or a united revolutionary front?"

These are important questions, and fair concerns. To Salehyan's point, I do examine several cases in which a united opposition emerged – all of the Southeast Asian cases have this structure, and the LTTE in Sri Lanka emerged as dominant – so the case selection include single organizations. It is also the case that many smaller contenders drop out of studies because they fail to organize successfully; unless we look closely at historical contexts before and in the early days of war, rather than reading history backwards, we will miss these cases.[6]

My theory does not have as much to say about Christia's concern. In part this is because the emergence of fragmented movements frequently represents a *consequence* of my theory – factionalization, splits, or the inability of a single group to consolidate control of a broader movement can all occur for reasons that my argument explains. My social-institutional theory is therefore, at least in some cases, causally and temporally prior to wartime decisions. But I agree that once in war, intricate inter-group politics surely matter. In other work I have tackled some of these dynamics, and Christia's book is a definitive account of how alliances work in civil wars.[7] An open future research step involves combining research on the internal organization of groups with relations between groups.

Cunningham identifies what I think is the biggest weakness in my book: dealing with the state. State policy is hugely important in Chapter 3 – and shows up in places in Chapter 2 as well – but the theory's focus is mostly on the military/counterinsurgency side of government behavior, rather than policies of accommodation or incorporation that could also affect cohesion.

In part this reflects a theoretical commitment to giving armed groups potential autonomy from state policy, in a break from an excessively state-centric literature. This is important because some insurgent groups are extremely effective at preventing splits and resisting divide-and-conquer (or divide-and-concede)[8] strategies, while others are far more vulnerable to them. *Networks* tries to re-balance our understanding of state-armed group interactions: insurgents often know that the state is trying to split them and take aggressive steps to prevent that outcome, with varying success. Taking organization seriously is necessary to explain that variation.

---

[6] Implications of these omissions for inference are discussed in Janet Lewis, "Do Ethnic Groups Rebel? Evidence from Nascent Insurgencies in Uganda," Working Paper, US Naval Academy (2015).

[7] Paul Staniland, "Between a Rock and a Hard Place Insurgent Fratricide, Ethnic Defection, and the Rise of Pro-State Paramilitaries," *Journal of Conflict Resolution* 56 (1): 16–40; Fotini Christia, Alliance Formation in Civil War (Cambridge: Cambridge University Press, 2012).

[8] Kathleen Gallagher Cunningham, "Divide and Conquer or Divide and Concede: How Do States Respond to Internally Divided Separatists?" *American Political Science Review* 105 (02): 275–97.

But Cunningham is absolutely correct to urge a more sophisticated account of state strategy and its effects. That was too ambitious a project to take on while also doing new research on insurgent groups. In my current book project, however, I am trying to advance this agenda, by exploring the ideational sources of regime preferences and internal threat perception. I theorize how regimes perceive the politics of armed groups and how these perceptions shape broad patterns of state strategy toward these groups, while also giving groups power and agency to accept or resist these strategies.[9] This agenda aims to move well beyond *Networks* by explaining how regimes and non-state groups create varying armed political orders.

Regarding methodology, the reviewers report favorably on the book's comparative case studies, in terms of both their level of detail and comparative design: "the empirical analyses are quite strong" (Salehyan) based on a research design that is "particularly well suited" (Cunningham) to the question. They identify three areas of concern, however.

First, Cunningham notes that there are not consistent indicators of pre-war politicized networks across cases, which limits broader empirical examinations. I agree with Cunningham that it would have been ideal to have found such indicators. But societies vary dramatically, and so relevant political networks in one place at one time may bear little resemblance to those in another context. Moreover, society-level measurements are unlikely to capture (often dramatic) sub-national variation. There is simply irreducible heterogeneity in political life: Yemen is not Belgium, and northern Yemen is not southern Yemen. Instead of pinning my hopes on universal cross-national indicators, I decided to use detailed sub-national comparisons in hopes of providing more persuasive evidence of variation in pre-war structures, and then used process-tracing to show whether and how they were transformed into insurgent organizations. I appreciate the trade-offs involved, but it is not clear what realistic alternative is available.

Second, both Christia and Salehyan wonder why I did not do a broader, quantitative test of the argument across numerous cases of civil war. In principle, I think this is a reasonable suggestion, and my choice of relying on qualitative research should not be read as hostility to quantitative work. There are two reasons I did not pursue it. The primary reason is what I outlined in the paragraph above: in the absence of theoretically sound cross-national measures, I simply would not be able to assess my core arguments using a quantitative, cross-national research design.

A further concern is that I am not confident that the current cross-national data are representative of the population of armed groups in the world. Impressive studies – including those by both Cunningham and Salehyan - have given us a better sense of where

[9] Paul Staniland, "Militias, Ideology, and the State," *Journal of Conflict Resolution* (forthcoming); Staniland, *Armed Politics and the State in Post-Colonial Asia*, book manuscript, University of Chicago, 2015.

and when insurgent groups have operated.[10] However, a number of the groups I examined in my book do not exist in even the best datasets, and some of those that do exist in the datasets are coded for periods of time that do not match up with the historical record. This eliminated "nested analysis" as an approach for case selection.[11] As much as mixed-methods research has become a rhetorical lodestar in contemporary political science, some research projects are best suited to a single method.[12]

I think these challenges reflect just how little we know about many of the armed groups in the world, whether the inner workings of the New People's Army in the Philippines, the full landscape of armed groups in eastern DRC, the organizational structure of insurgency in Thailand's Deep South, or the factions operating in Burma/Myanmar's Shan State. I am not confident that we know enough about many groups yet to confidently code them. We still have a staggeringly large amount of case-specific research to do before pinning our hopes primarily on cross-national analysis. Indeed, not only do we need scholars to do specialized research on ongoing conflicts, but also to engage in "dynamic historiography" that systematically re-assesses past conflicts through both original historical research and the history and area studies literatures.[13] This is a crucial research frontier, and one where dialogue between qualitative and quantitative scholars can be highly productive.

Finally, Christia is worried that my theory is built on some of the cases that it is "tested" against. I take care not to use a consistent language of testing, since I think true tests of theories or hypotheses are incredibly rare (especially in a single book or article). Nevertheless, this is a fair concern, and one about which I should have been more clear. The core of the theory originated in research on Northern Ireland that is not included in the book (it can be found in my dissertation). It was refined in the Kashmir case, and then applied more broadly in the other conflicts; the theory worked well in some of these other cases, and not as well in others. The relationship between theory and evidence thus reflects an initially iterative process, but was not a process of fitting the argument to match available data after completing the empirical work. A further test will occur over time as other scholars try to apply the concepts and arguments to their research.

Whether the Islamic State in Iraq and Syria (ISIS), Revolutionary Armed Forces of Colombia (FARC), or the Karen National Union (KNU) in Burma/Myanmar, insurgent groups are

[10] Kathleen Gallagher Cunningham, *Inside the Politics of Self-Determination* (New York: Oxford University Press, 2014); David Cunningham Kristian Skrede Gleditsch, and Idean Salehyan, "Non-State Actors in Civil Wars: A New Dataset," *Conflict Management and Peace Science* 30 (5): 516–31.

[11] Evan Lieberman, "Nested Analysis as a Mixed-Method Strategy for Comparative Research," *American Political Science Review* 99 (03): 435–52. DOI:10.1017/S0003055405051762.

[12] Amel Ahmed and Rudra Sil, "When Multi-Method Research Subverts Methodological Pluralism—or, Why We Still Need Single-Method Research." *Perspectives on Politics* 10 (04): 935–53.

[13] Paul Staniland, "Beyond Civil War and Peace: Armed Politics and the Study of Conflict," working paper, University of Chicago, 2015.

pivotal actors in much of the contemporary world. Understanding how they organize, change, and collapse is essential to serious analysis of rebellion. Yet prior to *Networks of Rebellion* we had few integrated theories of the origins, evolution, or termination of insurgent groups. The concepts and arguments of my book aim to bring new clarity to understanding insurgency. One mark of a rewarding research project is that it helps us make sense of important puzzles while identifying new research directions. I hope *Networks* has achieved this goal, and I am deeply grateful to the reviewers for their serious and thoughtful contributions.