Chapter 1 - Transnational Dynamics of Civil War

By

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Civil war has become the dominant mode of organized violence in the post-Cold War international system. Depending upon the counting rule employed, such wars have afflicted from a third to a half of all nations. This internal warfare is not just extremely common, it is persistent, with 20% of nations experiencing at least ten years of civil war since 1960. During the mid-1990s – to take just one example - nearly a third of the countries in sub-Saharan Africa had active civil wars or conflicts (Blattman and Miguel 2010, 3-4).

Equally important, this form of 'internal warfare' is rarely internal, with 55% of all rebel groups active since 1945 having transnational linkages (Salehyan 2009, 5). Indeed, civil wars nearly always create opportunities and incentives for outside actors to intervene; these actors may be other states, rebel groups, transnational civil society, or the international community, and this intervention may be malign (fanning the war) or benign (transnational NGO’s targeting the use of child soldiers). Moreover, such wars are often fueled by cross-border flows of goods, including material (weapons), money (diaspora financing) and human (new recruits for rebel groups). Finally, civil wars can spur social mobilization across borders – by strengthening senses of community among ethnic co-brethren, say.

So, the transnational clearly matters, with scholars documenting a strong correlation between various transnational factors and actors and changes in civil war dynamics. In this important sense, such wars are no different. Across a variety of subfields and research programs in comparative politics and international relations (IR), it has become a truism to argue that the external and the internal, the global and the local, the state and non-state actors are inextricably linked. The theoretical challenge – for scholars in general and students of civil war in particular – is to explain the interactions across these various levels.

This introduction and the essays that follow take up this challenge, exploring the relation of the transnational to the local in the context of civil war. How do we conceptualize this transnational dimension? In material or social terms? How does it affect civil war dynamics? By bringing new material resources into play? By affecting cost/benefit
calculations? By promoting learning among actors? Under what conditions do transnational factors increase or decrease levels of civil violence? What is the nature of the causal connection between the transnational and the local? Put differently, what is the causal mechanism at work?

We argue that to address these issues requires three moves. Theoretically, the finding of transnationalism’s importance in civil war needs to be linked to existing literatures in other subfields that have extensively conceptualized and empirically documented such non-state dynamics; key here is work on transnational politics in IR theory and sociology. Analytically, one needs a more robust understanding of causality, where the goal is the measurement of causal mechanisms and not simply establishing causal effects. Methodologically, the central challenge is practical – to measure mechanisms in action.¹

The volume thus addresses gaps and promotes learning across three literatures. For students of civil war, we supplement political economy models and correlational analysis with process-based evidence on its social and transnational dimensions. We thus provide new insights to enduring questions related to civil conflict -- agency and motives, group mobilization, and international intervention, to name just a few. For those studying transnationalism, we build upon but go beyond a focus on the benevolent side of world politics by exploring and theorizing transnational violence – that is, cross-border activities with malevolent intent and consequences. For scholars interested in process, we provide detailed evidence for the advantages – and disadvantages – of a move to mechanism-based theorizing.

The remainder of this introductory essay is organized as follows. I begin with a brief review of work on transnationalized civil war, arguing that a diffusion metaphor is often invoked to link the external and internal. To operationalize it, I draw upon – in Section II - theories of transnational politics. Work by international relations specialists and sociologists offers a rich menus of mechanisms – framing, learning, brokerage, persuasion – to explain how transnationalism matters in civil war. Section III connects the theory to data by focusing on

¹ More formally, we measure their observable implications; see below.
method; special attention is paid to process tracing, as this is particularly well suited to measuring causal mechanisms. In Section IV, I conclude and preview the volume’s structure.

I. Civil War and the Transnational

Over the past decade, new research on civil war has put its study squarely in the academic mainstream. At first quantitative in nature, it has been complemented in recent years by a growing qualitative literature on civil conflict. My purpose here is not a wide-ranging review of this rich literature – for this, see Tarrow 2007, and Blattman and Miguel 2010; rather, the concern is how this work conceptualizes and measures transnationalism.

Early efforts emphasized aggregate measures (Fearon and Laitin 2003), thus overlooking the sub-national, international or transnational dimensions of civil war. There was an inclination “to treat civil wars as purely domestic phenomena” and a consequent neglect of “transborder linkages and processes” (Cederman, Girardin and Gleditsch 2009, 404). More generally, the analytic starting point was individual states treated as independent entities – a so-called closed polity approach (Gleditsch 2007). Cognizant of this limitation, several scholars spearheaded a two-pronged move to develop more disaggregated databases. Some have disaggregated geographically and spatially, using so-called geo-referenced conflict data (Buhaug and Gates 2002; Buhaug and Rød 2006).

More important for my purposes, others have disaggregated by moving away from state-level, aggregate proxies – for example, by coding the attributes of non-state conflict actors (Cunningham, Gleditsch and Salehyan 2006). This has allowed researchers to document the impact of new actors and interactions across state boundaries in a wide array of cases. Work of this sort is important, not only advancing the civil-war research program, but also – by adopting an open polity perspective – aligning itself with the bulk of IR scholarship. It has allowed scholars to offer a more nuanced picture of civil conflict, including its transnational dimensions.

As one important example, consider Salehyan’s book-length study on what he calls transnational insurgencies (Salehyan 2009). In it, he utilizes rich and disaggregated data
embedded in a rigorous research design to document that external sanctuaries have played a central role in more than half of armed insurgencies since 1945 (see also Malet 2010). Salehyan also explains exactly how international borders shape the behavior of rebel groups involved in such conflicts. Simply put, insurgencies seeking to challenge a state often have the option of mobilizing abroad – and especially in neighboring states - where they are safely out of reach of domestic foes. The book is anything but a closed polity approach; rather, it demonstrates the analytic power to be gained when civil wars are viewed in their broader international and transnational contexts (see also Kalyvas and Balcells 2010).

**Theory and Analytics.** Scholarship on the transnational dimensions of civil conflict has illuminated the broad trends at work, and has done so in a methodologically self-conscious way that enhances the reliability of the findings. This is no small feat, especially when one remembers that the data come from violent and inaccessible parts of the world, from failed or failing states. Moreover, there has been a concern to integrate the data with theoretical arguments of increasing sophistication – for example, Salehyan’s theory of transnational rebellion that draws upon work on political opportunity structures and conflict bargaining (Salehyan 2009, ch.1). This is a welcome and progressive move (Checkel 2010).

At the same time, such theorizing is typically not linked to the now voluminous literature on transnationalism in world politics (Risse 2002). This seems odd – for a more systematic connection to this research would alert scholars to alternative theoretical starting points for understanding the cross-border dynamics of civil conflict (see also Tarrow 2007, 588-90).

The work of Kristian Gleditsch and his collaborators is typical in this regard. More than any others, they have documented the role of various transnational factors in civil war (Gleditsch and Salehyan 2006; Gleditsch 2007). It is somewhat puzzling, however, that these effects are interpreted primarily through the lens of rational choice, where transnationalism influences the dynamics of civil war by altering material incentives and shifting cost/benefit
calculations (Gleditsch and Salehyan 2006, 341; Gleditsch 2007, 294-99; see also Gates 2002; Fortna 2004, 487-90; Kalyvas 2006, 2008; Humphreys and Weinstein 2007; Blattman 2007, 7-13; Toft 2007, 102-07). Part of the justification for this focus is that non-material factors such as norms, ideas and learning are difficult to measure (Salehyan 2009, 39; Fjelde nd, 14-15) – an assertion that is hard to understand in light of advances in empirical constructivist research over the past 15 years (Adler 2002).

The literature on transnational politics is much more agnostic on social theory, with some preferring rational choice (Cooley and Ron 2002), some social constructivism (Price 1998), while still others combine the two (Risse, Ropp and Sikkink 1999). If the goal is to understand the full range of dynamics affecting civil war, then we can only gain by adopting a broadened social-theoretic starting point. As Cederman, et al, argue “additional research is needed on the details of the border-transgressing bond, especially as regards the nature of the actor-specific mechanism” (Cederman, Girardin, Gleditsch 2009, 433). And from a problem-driven perspective, this ‘bond’ may equally well be captured by rationalist or constructivist perspectives.

**Methodological Issues.** Students of civil war have shown a growing interest in process (Tarrow 2007) – for example, in exploring the linkages from the transnational to a particular civil conflict. Given this shift, it is not surprising that the language of causal mechanisms is now often invoked (Fortna 2004, *passim*; Gleditsch and Salehyan 2006, 335-36, 360; Salehyan 2008). Yet, it is unclear what is meant by such language. In one recent study where the transnational to civil war nexus is explored, causal mechanisms seem central to the analysis, but are never defined. Instead, it is left to the reader to infer that a mechanism equals a hypothesis, diffusion, spillover effects or ethnicity (Cederman, Girardin, Gleditsch 2009, 412, 408, 433; see also Gleditsch 2007, 297; and the discussion in Fjelde nd, 5).

Causal mechanisms can be defined as the process by which an effect is produced or a purpose is accomplished (see next section). However, it is not clear this is what the civil war
scholars are measuring empirically. Moreover, even in those cases where more effort is devoted to theorizing transnational mechanisms, these then vanish in the empirical testing (Gleditsch and Salehyan 2006, 342, 347-50). There would appear to be a mismatch between the language of causal mechanisms and methodological choice, between conceptualizing cause as a process (Æ mechanisms) and measuring it via quantitative techniques (Æcovariation).

Even the most sophisticated, mixed method research on transnationalized civil war has problems at this level. Consider again Salehyan’s Rebels without Borders. Methodologically, the rigor of the book’s quantitative first half is not carried over to its case studies, which is unfortunate given the availability of an increasingly sophisticated case-methods literature (Bennett and George 2005; Gerring 2007a, for example). As a result, the case studies – despite the author’s claims (Salehyan 2009, 108, 110, 122-23, 164) – fail to provide evidence for the causal mechanisms he posits to explain the correlational findings. For example, bargaining dynamics based on cost/benefit calculations figure prominently in Salehyan’s theory; yet, the case study chapters provide no process-level evidence that such dynamics were at work.

Here again, the transnational politics literature offers both conceptual discussions and empirical applications relevant to students of civil war. Conceptually, it has taken seriously the logical implications of open polity models, developing cross-level theoretical frameworks. These move well beyond level-of-analysis approaches (Singer 1961) or arguments about residual variance (Moravcsik 1993), to emphasize cross-level interactions that put the spotlight on process (Keck and Sikkink 1998; Risse, Ropp and Sikkink 1999). Such a conceptual move is necessary if one is to explore the role of transnational causal mechanisms.

Empirically, scholars of transnationalism have put a good bit of thought into the methods one needs to measure such dynamics. Moving beyond the measurement of causal effects, they have demonstrated that techniques are available for capturing mechanisms. In particular, they have convincingly shown the utility of a method known as process tracing (Risse-Kappen 1995; Risse, Ropp and Sikkink 1999), which is seen as key for measuring
mechanisms (Bennett and George 2005, ch.10). These empirical, operational applications of the technique should be of great help to students of civil war, who increasingly invoke it while failing to demonstrate how it works in practice (Weinstein 2007, 53-59; see also Kalyvas 2007).

Summary. Scholarly research has documented that transnationalism plays an important role in civil conflict, but the specific causal mechanisms remain poorly understood, for both theoretical and methodological reasons. From a broader perspective, these limitations are understandable and no surprise. The success to date of the civil war research program (Blattman and Miguel 2010) means these studies have reached a new level of sophistication, where open-polity models and transnational dynamics are stressed, and causal mechanisms are invoked. However, it is one thing to speculate about transnational mechanisms; it is quite another properly to theorize and measure them.

II. Transnational Mechanisms of Civil War

Progress in addressing these gaps requires a two-fold analytic-theoretical move – to the language and practice of causal mechanisms and to theories of transnationalism. I begin by situating this volume’s understanding of causal mechanisms in the (vast) literature on them. Next, I turn to transnationalist scholarship in international relations and sociology. This work suggests specific ways to make operational the diffusion mechanism invoked in much of the civil war literature, a point I demonstrate with examples from the volume’s empirical contributions. A third section then turns the tables, suggesting that transnationalists have something to learn as well, in particular, how to theorize transnational violence.

Causal Mechanisms – From Confusion to Emergent Consensus. Thinking about mechanisms has a long history in the philosophy of science and in the social sciences. Philosophers - for several hundred years – and social scientists - more recently - have debated the nature and meaning of cause (Kurki 2008 for a state-of-the-art review). Is it best captured by a Humean understanding of constant conjunction and covariation or a realist account of cause as process? Among sociologists and inspired by the work of Robert Merton and his colleagues
at Columbia University, mechanisms were the subject of intensive inquiry in the early years after World War II (Hedstroem and Swedberg 1998, 1-2). Thus, in the distant and not-so-distant past, the interest in mechanisms was there.

Such interest has blossomed among political scientists over the past decade, due both to a growing dissatisfaction with structural theories (Hall 2002, 375-88) and the rise of a new generation of scholars. Rationalists now do mechanisms (Elster 1998), constructivists see them as a core component of their social theory (Wendt 1999), quantitative researchers increasingly invoke them (Gleditsch and Salehyan 2006, 341-44), and – among qualitative methodologists – new research on case studies gives mechanisms a pride of place (Bennett and George 2005; Gerring 2007a).

One not surprising result of all this attention is that different authors define a causal mechanism in different ways, a fact now widely noted and bemoaned (Mahoney 2001; Gerring 2007b; Falleti and Lynch 2009). At an intuitive level, it is easy to define a mechanism – it connects things and captures process. However, the devil, as always, is in the details. Are mechanisms easy or hard (i.e., unobservable) to see? Must the use of mechanisms be premised on an ontological stance of methodological individualism?

Building upon recent discussions in the literature, I define a causal mechanism as “the pathway or process by which an effect is produced or a purpose is accomplished” (Gerring 2007b, 178; see also Caporaso 2009). Mechanisms are thus “relational and processual concepts … not reducible to an intervening variable” (Falleti and Lynch 2009, 1149); they are “the operative or motive part, process or factor in a concrete system that produces a result” (Wight 2006, 34). These minimalist definitions capture other extant usages of the term (Gerring 2007b; see also Gerring and Barresi 2003).

Moving from conceptualization to a more operational level, there is also a growing consensus on measurement. Philosophers of social science – and especially scientific realists – view causal mechanisms as ultimately unobservable ontological entities that exist in the world,
not in our heads; they are thus more than mere “analytical constructs that facilitate prediction” (Wight 2006, 31-32, quote at 31). If mechanisms are real but unobservable entities, the implications for measurement are clear: We measure not hypothesized mechanisms, but their observable implications.

Given the invocation of scientific realism in the last paragraph, I should highlight one area where considerable confusion remains: the philosophical foundation of empirical, mechanism-based social science. As a number of sharp-thinking analysts have noted, there is a tension between the growing use of mechanism-based thinking in American political science and this same community’s continuing adherence to a philosophical position – positivism – at odds with a turn to mechanisms (McKeown 1999, 163-64; Johnson 2006; Wight 2006, 31-32; Gerring 2007b, 164). For sure, such philosophical disputes, by their very nature, will not be resolved any time soon (if ever).

In this volume, our response to such confusion and disputes is two-fold. First and pragmatically, we just get on with it, doing methodologically self-conscious mechanism-based social science. Implicitly, all contributors adopt a variant of scientific realism. With its emphasis on measuring causation via mechanisms and a practical stance of “epistemological opportunism” (Wight 2002, 36), it well suits our purposes. The latter point is important, as methods flow from epistemology. This opportunism thus legitimates and indeed mandates a plurality of methods when it comes to the measurement of mechanisms. Contributors to this volume thus measure mechanisms in a variety of ways, from case studies and process tracing to agent-based modeling.

Second and conceptually, we step back and use our results to assess the fit between research practices and their philosophical foundations (Bennett, this volume). Is scientific realism really such a good basis for what we claim to be doing, especially given the strong critiques recently leveled against it (Chernoff 2002)? Would a form of pragmatism or analytic eclecticism be better (Katzenstein and Sil 2010a, b)? Whatever the philosophical foundation,
can we articulate clear and agreed community standards for what counts as good mechanism-based social science?

**Transnational Politics and Civil War.** Students of transnational politics – mainly in political science and sociology – have been thinking about international-domestic connections for quite some time. Over nearly four decades, this work has gone through three distinct phases. The earliest research simply challenged realist assumptions about states as the key actors in international politics, getting transnationalism on the scholarly agenda (Keohane and Nye 1972). A second generation, appearing in the 1990s, marked a significant analytical shift – exploring when and under what conditions transnationalism mattered (Risse-Kappen 1995; Evangelista 1999; Risse, Ropp and Sikkink 1999).

More recently, a third set of scholars has disaggregated key assumptions about transnational actors and domestic politics. They have begun to look inside entities such as NGOs, asking what motivates them to act (Schmitz 2004) and why they mobilize around some issues but not others (Carpenter 2007). If earlier research stressed ideational and normative motivations, this new work sees transnational actors as highly strategic and calculating as well (Cooley and Ron 2002; della Porta and Tarrow 2004; Bob 2005). On domestic politics, newer work theorizes it in greater detail and does so more systematically, thus avoiding the ad-hocism that often prevailed in earlier research (Orenstein and Schmitz 2006; Schmitz 2006).

These multiple disaggregation moves have led to a greater interest in the different kinds of causal mechanisms connecting transnational actors and factors and domestic change (Price 1998; Tarrow 2001; Tarrow and della Porta 2004; James and Sharma 2006; Checkel 2007; see also Symposium 2006). Indeed, we now have a growing roster of transnational causal mechanisms, including emulation, persuasion, flows of resources (ideational or material), framing, and power transitions – to name just a few.

Students of civil war can benefit in four specific ways from this IR/sociology work on transnational causal mechanisms. First, it highlights that mechanisms can have varying social-
theoretic bases. Some are captured by rational choice – power transitions; others by constructivism - persuasion; and yet still others by both social theories - learning in its simple and complex variants. This social-theoretic pluralism would align civil-war studies with the philosophy of science literature, where it is argued that accounts employing causal mechanisms are in principle “quite compatible with different social theories of action” (Mayntz 2004, 248; see also Mahoney 2001, 581).

Second, this roster of causal mechanisms offers opportunities for better specification of the diffusion metaphor invoked in the civil-war literature. To begin, what exactly is transnational diffusion? Research on the topic defines it as the spread of policies, institutions, or beliefs across borders (Simmons, Dobbin and Garrett 2006, 787-90). On its own, however, the term obscures more than it reveals, a point now well appreciated in both the transnationalist and policy diffusion literatures (Symposium 2006). Instead, the cutting edge is to unpack diffusion and explore the underlying causal mechanisms – coercion? learning? emulation? – driving it (see also Tarrow 2010). Indeed, it is precisely the latter that are hinted at but not theorized or tested in recent work exploring the transnational dimensions of civil war (Cederman, Girardin, Gleditsch 2009, passim).

Third, the mechanisms cited above have undergone extensive empirical testing (Risse-Kappen 1995; Keck and Sikkink 1998; Price 1998; Risse, Ropp and Sikkink 1999; Risse 2000; Lynch 2002; Checkel 2003, 2007; della Porta and Tarrow 2004; Schmitz 2006; see also Levy, Young and Zuern 1995; Johnston 2001, 2008; Gheciu 2005; Lewis 2005). For example, scholars have now applied a number of techniques for measuring these mechanisms - panel interviews, surveys, text analysis, process tracing – while maintaining a healthy appreciation of their limits (Gheciu 2005; Johnston 2008). And, again, this research is neutral in social-theoretic terms. Some use these methods to document rationalist cost-benefit causal mechanisms (Schimmelfennig 2005), while others use them to measure non-instrumental constructivist learning (Checkel 2001).
Theorizing non-instrumental mechanisms may be especially helpful as scholars of civil war continue their explorations of transnationalism’s effects. To date, they have neglected the role of “noninstrumental factors, such as norms and emotions” (Kalyvas 2006, 13; see also Blattman and Miguel 2010, 18), or - specifically on rebel groups – have been “silent on the internal psychology of the recruit,” thus making it difficult to uncover “the root causes of … indoctrination” (Blattman 2007, 25-26, 27; see also Annan, et al 2009; and Wood 2008, 2009). Those root causes – to take just one example – may be partly captured by the non-instrumental socialization mechanisms (role playing, persuasion) emphasized by the transnationalists (Checkel 2007).

Fourth, the work by transnationalists provides students of civil war the raw material for developing better mechanism-based theories that are bounded, contingent, but still ‘small g’ generalizable (George 1993; Hall 2002; Gerring 2007b; Wood, this volume). After some false starts – in particular, vague invocations of middle-range frameworks – scholars are thinking hard about how to produce rigorous mechanism-based theory (Elster 1998; Gates 2008; Lichbach 2008) and exploring its requirements in terms of research design (Johnston 2005; Wood 2008, 556). Recently – for example - they have proposed so-called typological theory, where combinations of mechanisms interact in shaping outcomes for specified populations (Bennett, this volume; see also Bennett and George 2005, ch.11).

If the foregoing suggests an ‘in principle’ contribution from the IR/sociology transnationalists, several examples from the chapters that follow indicate its practical utility. In her study of the Chechen civil wars, Kristin Bakke theorizes and traces the diffusion mechanisms through which transnational insurgents affect domestic challengers to the state. Via mediated or relational diffusion, these insurgents engendered learning and emulation among key Chechen rebels groups, in turn (re-) shaping domestic processes of mobilization (Bakke, this volume). Hans Peter Schmitz takes a hard case for external influences – the Lord’s Resistance Army (LRA) in the Ugandan civil conflict – and demonstrates that even here transnational
emulation and social adaptation played roles in shaping its behavior and evolution (Schmitz, this volume).²

Turning to a very different type of transnational influence, Stephan Hamberg asks whether transnational advocacy networks have had any real influence on rebel groups’ use of child soldiers. Examining the case of the Sudan People’s Liberation Army (SPLA), who in the early 2000s demobilized several thousand child soldiers, he argues that social shaming - the most common mechanism of transnational activism – failed to induce a change in rebel group behavior. Rather, child soldiers were demobilized – including by the SPLA – only when the international community granted material concessions or at least promised them to rebel groups (Hamberg, this volume).

A final example returns to the diffusion of civil conflict, but employs computer techniques instead of empirical case studies to capture the causal mechanisms at work. In their chapter, Martin Nome and Nils Weidmann utilize agent-based modeling to analyze the diffusion of social identities as a key process underlying the spread of civil conflicts. They disaggregate - and thus better specify – diffusion as occurring through two possible causal mechanisms: social adaptation in a transnational context, and transnational norm entrepreneurship. Their simulations indicate that norm entrepreneurship is the more robust mechanism of diffusion, which is an important confirmation of a finding in the qualitative, IR/sociology literature.

These examples provoke two final – cautionary – remarks. First, we well appreciate there is an issue of aggregation here. That is, while cataloguing various transnational causal mechanisms is a useful starting point, more is needed. After all, the broader international relations literature is by now replete with such (non-cumulative) lists. In fact, it is possible to construct an orderly taxonomy of theories on the causal mechanisms of civil conflict – one that provides a comprehensive and useful checklist so scholars can ensure they are not leaving out

² It is a hard case because the LRA’s long-time leader, Joseph Kony, has always sought to isolate it from external influences.
important mechanisms in their explanations. This is a task Andrew Bennett takes up in his chapter (Bennett, this volume).

Second, we recognize that the IR/sociology work on causal mechanisms is not the definitive last word. One should maintain a healthy skepticism for how well mechanisms theorized by scholars whose prime interest is peaceful change travel to situations where violence and institutional collapse are the norm. More positively, we can use this translation exercise to push research on transnationalism in new directions, as suggested below.

**Theorizing Transnationalized Violence.** The issue is how to theorize transnationalized violence. And here, the IR/sociology transnationalists – to turn the tables - have much to learn from work on conflict processes. They have focused on peaceful and non-violent change in world politics (the spread of human-rights norms, socialization by international institutions), thus failing to appreciate that their theories may mis-specify social dynamics in settings marked by violent civil conflict.

Consider work on small group dynamics, which clearly play a role in civil war - say, in the transborder recruitment of rebel group members. Students of transnationalism have analyzed them through the prism of socialization, or the process of inducting actors into the norms and rules of a given community (Risse, Ropp, Sikkink 1999). To do this, they have drawn upon a particular strand of socialization literature, one where stable institutions are at work and power and coercion are back-grounded (Checkel 2007).

Yet, this process of induction could surely work in more coercive ways. For example, research on professional militaries and urban gangs explores hazing, physical coercion and even rape as causal mechanisms leading to socialization. Such mechanisms might be highly relevant in civil war settings (Wood 2009, 2010; Cohen 2010). The point is not to reject previous work by transnationalists, but to expand their roster of causal mechanisms, thus capturing the full array of social dynamics at work in the contemporary transnationalized world – both its good and evil sides (Checkel 2011).
Summary. This volume brings together students of conflict processes, IR theorists, comparativists and methodologists to explore the evident fact that civil wars are rarely contained within the borders of one country; they have transnational dimensions that need to be captured. We ask two questions. (1) What are the transnational mechanisms that influence group mobilization during civil war? Under what conditions do they enable organized violence? (2) What are the causal mechanisms at work when transnational actors intervene to end violence in civil conflict?

These questions intentionally capture two key aspects of transnational influence on civil war. The first, a bottom-up perspective, explores how actors in civil conflict may use or be affected by broader transnational processes. The second is a top-down view, asking how transnational actors may intervene to re-shape domestic conflict dynamics. The difference between the two is subtle, but important and essentially concerns the locus of agency. We do not a priori give analytic priority to one or the other.

Providing operational, empirically grounded answers to these questions requires the following steps. First, we think in terms of transnational causal mechanisms, which are defined – adapting Gerring (2007b) - as the transnational pathway or process by which an effect is produced or a purpose is accomplished. We theorize and test a number of causal mechanisms relevant to transnationalized civil conflict.

Second and regarding methods, we argue that there is no one technique that is best for measuring mechanisms. Contributors thus utilize a broad range, from the purely qualitative, to cutting-edge methods such as agent-based modeling. The mix varies, depending upon the specific question asked and a contributor’s methodological proclivities.

Third, we critically evaluate our contributions. In part, this is done in a standard way, as individual authors consider alternatives and challenges to their arguments and evidence. However, we also step back and – in two concluding essays – conduct net assessments. Conceptually, can our findings be used to clarify the outlines of a cumulative civil-war research
program centered on mechanisms (Bennett, this volume)? Theoretically, in what specific ways do we advance the research program on civil war (Wood, this volume)?


This section connects the analytics and theory to data by focusing on method. The discussion proceeds in three stages, beginning with a brief review of the best work exploring the methods-mechanisms nexus. Next, I describe and assess the technique known as process tracing, which seems particularly useful for measuring causal mechanisms. Finally, I address feasibility. Can mechanism-based social science – with its inherent requirement for significant data – be conducted in situations where violence and institutional collapse are the norm?

**State of the Art.** While progress is evident in methods work on mechanisms, there is still a clear need to bring the discussion down to a more operational level - how one actually uses mechanisms in empirical social science. Important efforts by four sets of scholars – Bennett and George, Gerring, McAdam-Tarrow-Tilly, and Wood - are indicative of the challenges that lie ahead. If by methodology, one means how we come to know, then the common denominator for these authors is that we come to know about mechanisms by studying processes, dynamics and narratives.

In their wide-ranging overview of the case-study approach, Bennett and George devote an entire chapter to process tracing (Bennett and George 2005, ch.10). This method “attempts to identify the intervening causal process – the causal chain and causal mechanism – between an independent variable (or variables) and the outcome of the dependent variable” (Bennett and George 2005, 206). The chapter is exemplary in noting process tracing’s compatibility with both rationalist and constructivist thinking, in highlighting its different varieties, and in giving several methodological tips to the novice.

However, this same novice will be frustrated at a practical level, as basic questions sure to be on the lips of any first-time process-tracer remain unanswered. How does one know when to stop with the process tracing? When is there enough evidence to document the
workings of a particular causal mechanism – that is, what are the data requirements (see also Gerring 2007a, 181)? What counts as good process tracing? What are the community standards (see also Bennett and Checkel nd)?

More recently and from a methodologist’s perspective, Gerring (2007a) has produced what critics are rightly calling the state of the art on the case study approach. Like Bennett and George, he devotes an entire chapter to process tracing. Yet, in an otherwise superbly crafted book, this chapter is short and lacking in practical guidance on how to execute the technique (see also Symposium 2007b, 5, 14). Indeed, Gerring (and co-author Craig Thomas) despair of offering systematic advice on process tracing, as it is as much “detective work” and “journalism” as a rigorous social science method (Gerring 2007a, 178).

In light of the foregoing, the very title of McAdam, Tarrow and Tilly’s essay – “Methods for Measuring Mechanisms of Contention” – would seem promising (McAdam, Tarrow, Tilly 2008). Indeed, they start by criticizing Bennett and George (2005) for failing “to tell us how to describe – let alone measure – the causal mechanism or mechanisms at the heart of the processes that interest them” (McAdam, Tarrow, Tilly 2008, 2-3).

In contrast to others, McAdam and co-authors get down to the brass tacks of how to measure mechanisms, offering two direct measures (events data, ethnography) and two indirect ones (comparative cross-national and intra-national data). This is excellent and very much what work on causal mechanisms requires. However, by drawing illustrations only from the contentious politics research program, the utility of their analysis is limited. In addition, while the empirical examples are interesting, it is difficult to extract a set of best methodological practices from them. Again, the novice - or aspiring PhD student - is left wondering how he/she actually does this mechanism-based research (see also Symposium 2008).

Within the literature on civil war, Elisabeth Wood’s social-theoretically plural and carefully documented work on the Salvadoran civil war (Wood 2003) sets the standard for how to capture the presence and role of causal mechanisms. Not only does she theorize a wide range
of mechanisms – ranging from (rationalist) calculations of cost/benefit to (sociological-constructivist) morals, emotions and norms – Wood also provides rich evidence to document and measure them. Her methods are also plural, ranging from ethnography to formal modeling.

Yet this important work could be improved in several ways. At an operational level, it is not entirely clear what procedures Wood followed in reconstructing her group/individual mobilization dynamics. How did she control for the bias generated by her largely micro, individual perspective, where structural factors could be overlooked? How did Wood decide at what level to focus her search for causal mechanisms? How does one decide how close, how micro to go? This problem of so-called infinite regress has been addressed in a general sense (Gerring 2007b, 176), but with little practical advice on dealing with it.

As Wood demonstrates empirically and as others argue from a more methodological perspective (McAdam, Tarrow, Tilley 2008; Staggenborg 2008, 341), ethnography is important for measuring process and mechanisms. Yet, its use raises two potentially vexing issues. First, how did Wood know when to stop? Arbitrarily after 100 interviews? After 150? While the book provides no clear answer, elsewhere she has argued – quite convincingly – that the ethnographic field work continued until it revealed no new patterns. That is, additional interviews were simply providing additional evidence for the operation of her core causal mechanisms (Wood, personal communication, June 2008; see also Gusterson 2008). More formally, one could say that Wood was following a logic of Bayesian inference in deciding when to stop (Bennett 2008).

Second, the epistemological foundation of ethnography – interpretivism – sits uneasily with the strong causal emphasis of mechanism-based social science (see also Schatz 2009). How does one square the circle here? Does it matter that Wood, within the covers of one book, utilizes methods – formalization, process tracing, ethnography - from very different epistemological traditions to measure her mechanisms? She provides no clear answer – and is in good company. In his commentary on McAdam, Tarrow and Tilly’s (2008) analysis of causal
mechanisms, Lichbach (2008) hints at similar epistemological tensions by invoking the oxymoronic phrase positivist constructivism to describe their contribution. Clearly, this is an area in need of further thought and reflection, one to which we return (Bennett, this volume).

In sum, recent years have seen growing attention to the operational issue of how one measures causal mechanisms. This is a welcome and healthy trend, moving scholars beyond conceptual discussions (what is a mechanism, are they real, etc). However, in a way quite similar to the quantitative researchers in the civil war literature, these students of mechanisms are victims of their own success. Their expositions and analysis are generally so clear, it is easy for (even sympathetic) critics to see the remaining gaps and challenges.

**Capturing Mechanisms – Process Tracing.** Recall our baseline definition of a causal mechanism: “the pathway or process by which an effect is produced or a purpose is accomplished” (Gerring 2007b, 178). Given this definition and the foregoing discussion, a number of methods seem relevant for documenting mechanisms. These include ethnography, interview techniques, process tracing and even formal models; in addition, agent-based modeling is a promising technique for exploring the logic and hypothesized scope conditions of mechanisms (Nome and Weidmann, this volume; see also Cederman 2001; Weidmann 2007). Finally, statistical methods play a more indirect, but still important role: establishing that there is a relation in the first place that requires explanation.

Full detail on these techniques can be found in the chapters that follow, as authors operationalize them in specific empirical contexts. Here, I concentrate on practice, that is, how we have sought to craft and implement mechanism-based empirical studies. After a few points on design, I advance several evaluative criteria for accounts that employ process tracing.

On design, a good mechanism-based explanation builds on four components (Earl 2008, 356-57). As a pre-condition – step #1 - one needs to be able to distinguish the elements from the mechanism(s) in the explanation, so we can study the mechanism. What is X and Y and the mechanism that links them? Then - step #2 - we need to show that relations among the
elements were changed. More important – step #3 – we need to show that the hypothesized mechanism was responsible for altering that relationship and that other, alternative mechanisms were not (see also the discussion of equifinality below). So, for example, if diffusion (X) leads to conflict (Y), we may hypothesize that learning was the mechanism explaining that relation. Even more important, we then – step #4 - need to ask in a very empirically grounded way ‘what are the observable implications of learning’?

Bakke’s study of transnational diffusion and the Chechen civil wars is an exemplary illustration of this design in practice. She operationalizes such diffusion as – in part – mechanisms of learning and emulation; develops operational indicators for them; and thinks temporally. For the latter, it is crucial to ask whether domestic learning occurred after intervention by transnational insurgents. Bakke also explores whether alternative – domestic, in her case – mechanisms might have produced the same outcome (Bakke, this volume).

Turning now specifically to process tracing, I argue it is possible to articulate a three-part standard for what counts as a good application of it (see also Symposium 2007b, 5; Bennett and Elman 2007, 183). Meta-theoretically, it will be grounded in a philosophical base that is ontologically consistent with mechanism-based understandings of social reality and methodologically plural, such as that provided by scientific realism (Wight 2006, ch.1), analytic eclecticism (Katzenstein and Sil 2008, 2010b) or pragmatism (Johnson 2006). Contextually, it will utilize this pluralism both to reconstruct carefully causal processes and not to lose sight of broader structural-discursive context. Methodologically, it will take equifinality seriously, which means to consider the alternative causal pathways through which the outcome of interest might have occurred.

With these three signposts in hand, good process tracing needs then to address specific operational issues regarding theory, data, and method (see also Checkel 1997, 2001, 2003, 2007, 2008a). Theoretically, the study of causal mechanisms raises an infinite regress or stopping point issue. When does the process-tracing on mechanisms stop? How micro should we go?
In my own work on socialization, I took one mechanism – socialization – and broke it into three sub-mechanisms: strategic calculation, role playing and persuasion. These were then subjected to process tracing (Checkel 2007). Why stop at this point, though? Persuasion, for example, could be further broken down into its own sub-mechanisms, most likely various types of cognitive processes.

Luckily, an answer to this ‘how micro’ question is straightforward and dictated by the state of disciplinary knowledge. In my case, this indicated it was socialization – and not persuasion – that was ripe for disaggregation into smaller component mechanisms. In addition and at a more practical level, how micro one goes is also a function of available data. It makes little sense to spend time attempting to process trace what cannot be measured empirically.

A second theoretical issue is causal complexity. Thinking in terms of mechanisms abstracts from and simplifies the real world – less than is often the case, but abstract it still does. Moreover, because of the high data requirements, it is all too easy to conduct process tracing on only one mechanism. Yet, in many cases, the outcome observed is the result of multiple mechanisms interacting over time. There is no easy answer to this dilemma; one cannot conduct process tracing on all possible candidate mechanisms. However, thinking explicitly and early about equifinality, where the outcome of interest may be the result of alternative causal pathways (Bennett and George 2005, 161-62), and using agent-based modeling, which allows for systematic exploration of possible interactions among hypothesized mechanisms (Nome and Weidmann, this volume; Cederman 2003, 146; Hoffmann 2008), can bound the problem.

Regarding data, any empirical research project must be able to answer a basic question: When is there enough data? How do we know when to stop with the data collection? If done systematically, process tracing can help provide an answer, with my work on socialization in European institutions providing a case in point (Checkel 2001, 2003). After two rounds of interviewing, I took a break from data collection. Writing up the results – connecting the data to the causal-process story I was attempting to tell – allowed me to see where my data
coverage was still weak. This indicated in a very specific manner the data I would need to collect during future field work. This strategy is consistent with the ethnographic and Bayesian perspectives discussed earlier, which argue that one should stop when new data are simply providing additional evidence for the operation of the hypothesized causal mechanisms.

On method and procedure, good process tracing requires explicit attention to triangulation (more precisely, its limits) and structural context. On the former, there is an assumption that triangulation is a form of methodological nirvana, minimizing threats to causal inference. With triangulation, a researcher cross-checks the causal inferences derived from his/her process tracing by drawing upon distinct data streams (interviews, media reports, documents, say). The belief is that the error term in each stream points in such a way that it cancels those in others. However, what if the errors cumulate, with the result being that the researcher is worse off after triangulating than before (see also Kuehn and Rohlfing 2009)? Indeed, what if there is a “dark side of triangulation” (Symposium 2007a, 10)? Well executed process tracing thus requires that one think beyond triangulation – for example, by making use of counterfactuals – when seeking to strengthen the validity of causal inferences.

Another ‘dark side’ to process tracing is how use of the technique can too easily blind a researcher to broader structural context (see also Wood, this volume). For example, in earlier work, I used a causal-mechanism/process-tracing toolkit to explore the social-psychological and institutional factors that might lead decision-makers to change their minds in light of persuasive appeals (Checkel 2003). Yet, as critics noted, I had overlooked structural context, simply assuming that persuasive arguments were a function of individual-level dynamics alone. It was equally plausible, however, that my persuader’s arguments were legitimated by the broader social discourse in which he/she was embedded. In positivist-empiricist terms, I had a potential problem of omitted variable bias, while, for interpretivists, the issue was one of missing the broader forces that enable and make possible human agency (Neumann 2008; Autesserre 2009).
In either case, the lesson for the process-tracer is clear: Her design must incorporate explicit checks for this potentially serious problem.

In sum, good process tracing builds in part on fairly standard injunctions and checks that are applicable to an array of qualitative methods. These include attention to design, thinking counterfactually, and being cautious in the application of triangulation. At the same time, it demands adherence to additional best practice standards that address problems related to equifinality and issues of infinite regress. Put differently, process tracing, when correctly executed, is far more than a temporal sequencing of events or “detective work” based on hunches and intuition (Gerring 2007a, 178).

**Extensions to Violent Conflict.** Is any of this useful to students of civil war? My examples above often derive from situations where institutions work, social relations are stable and social norms are largely shared; in many cases of civil conflict, none of these conditions hold. Yet, I will argue that a decisive turn to the theory and process-tracing practice of causal mechanisms is an essential next step in the civil-war research program. I substantiate this claim in two steps, addressing both feasibility and necessity.

Regarding feasibility, is a mechanism/process account – with its high demands in terms of data, time, and proximity to the subject matter – realistic in a conflict situation? In fact, a growing array of primary and secondary source material indicates the data problem is not as severe as some might expect. Researchers can now draw upon a growing civil war memoir literature (of former child soldiers – Beah 2007a, b) and carefully designed surveys of ex-combatants (Blattman 2007; Annan et al 2009). In addition, more and more scholars are addressing the proximity challenge by going directly to the source, conducting extensive interviews with ex-combatants as a way of measuring causal mechanisms (Wood 2003; Checkel 2008b; Autesserre 2009; Cohen 2010; see also McAdam, Tarrow, Tilly 2008, 10-11).

A nagging questions remains, though. While there may be more data available on processes and mechanisms, even in conflict situations, is it reliable? In such settings,
individuals have multiple reasons to lie or to otherwise dissimulate. Two techniques, however, allow one to bound this problem. First, just as in non-conflict situations, one triangulates (cautiously – see above) across multiple data streams. On the particular issue of interviewees and their possible dissimulation, one looks for changes and discrepancies in accounts as a function of different audiences addressed. Second, the silences, gestures, jokes and apparent lies of interviewees should be recognized as valuable data in their own right, helping researchers better appreciate how the current social-political landscape shapes what people are willing to say (Fujii 2010, passim; see also Wood 2003, 33-40).

Of course, in conflict/post-conflict situations, it is not just individuals who may have incentives to lie. Organizations as well may intentionally distort historical events in a manner designed to meet current (political) needs, or simply lack the resources to get the story right. However, here too, good research practices suggest ways to deal with such realities. In her work on the Peruvian civil war (1980 – 2000), Leiby sought to document the frequency of wartime sexual violence, and the Peruvian Truth and Reconciliation Commission was a key data source in this regard. Finding serious gaps and omissions in its work, she worked systematically to correct them by consulting other published documents, as well as primary sources. In her case, the end result was a much richer account of the nature and frequency of sexual violence in wartime Peru (Leiby 2009a; see also Leiby 2009b).

This discussion of feasibility begs a different and prior question. Do we even need to bother with adding mechanisms and process to our accounts? Here, the consensus answer among all students of civil war - quantitative (Sambanis 2004; Blattman 2007, 24-27; Gates 2008; Blattman and Miguel 2010) and qualitative (Tarrow 2007; Kalyvas 2008; Wood 2008) - is yes. For very understandable reasons, one particular social process of civil war – violence – has received significant attention (Kalyvas 2006). However, as researchers have moved to disaggregate in recent years – creating subnational data sets, exploring transnationalism’s role, looking inside rebel groups – new processes have been invoked, including community building.
and allegiance (Gates 2002, 2011), transnational ethnic kinship (Cederman, Girardin and Gleditsch 2009), socialization (Cohen 2010), norm-driven behavior (Wood 2008), emotional responses (Wood 2003), framing (Autesserre 2009) and social networks (Tarrow 2007), among others. This fact suggests that not only is research on the processes and mechanisms of civil war feasible, it is also necessary and needed.

IV. Conclusions & Preview

This essay and the chapters that follow argue for improved understanding of civil war and its transnational dimensions by integrating insights, concepts and methods from here-to-for disparate literatures and research communities. Dialogue of this type is no guarantee of theoretical progress or mutual learning; however, its continued absence is a sure-fire way to promote group think, closed citation cartels and academic hyper-specialization.

If one recent state-of-the-art review of the civil war literature can be read as a plea to reconnect it to central concerns of comparative politics (Tarrow 2007, 589, 596; see also King 2004, 432-33), then our argument is that similar gains are to be had via a reintegration with contemporary international relations theory and methods scholarship. As two of the sharpest (quantitative) scholars of civil war have recently argued, the “empirical salience of ... international issues in driving domestic civil conflicts” will require that future work “draw heavily on the existing international relations literature” (Blattman and Miguel 2010, 30).

Preview. The book has three parts, with this introductory, framework essay comprising Part I. The six chapters in Part II form the manuscript’s core; they utilize process tracing and other techniques to document the causal mechanisms of transnationalized civil conflict. Each article follows a similar template, where it: highlights a puzzle linked to the transnational dimensions of civil conflict; advances an argument incorporating one or more of the causal mechanisms articulated in this introduction; addresses questions of methods and data; and – most important – shows these mechanisms in action in one or more cases. This similarity in structure facilitates learning and cumulation, both within the volume and among readers.
In Part III, we step back and use our findings to address two broader issues. In Chapter 8, Andrew Bennett probes the research frontier regarding causal mechanisms. In particular, he systematizes our findings (Part II) by advancing a taxonomy of theories about mechanisms, followed by a discussion of so-called typological theorizing as a means of addressing combinations of mechanisms. Drawing upon the volume’s case studies, Bennett argues that explanation via causal mechanisms entails important costs, most notably a substantial loss of parsimony, but that this is the necessary price for improving our understanding of the complexities of civil conflict.

In the volume’s final chapter, Elisabeth Wood explores how our approach and findings contribute to the further theoretical development of work on civil war. As the contributors demonstrate, clarifying what we mean by mechanisms and systematically measuring them offer methodological rigor and an opportunity to produce cumulable social science research. To buttress this claim, Wood reviews several specific ways in which students of civil war can build upon the findings here to produce new theories of violent inter/intra-state conflict – frameworks that do not a priori favour agent over structure, or political economy over sociology, or the material over the social. Put differently, theoretical bridge-building, despite its inherent limitations, is the way forward – both to better substantive knowledge and a richer civil-war research program.
References


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