7 Process tracing, causal inference, and civil war

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Introduction

Process tracing is an invaluable tool in the civil war scholar's toolkit. Or, rather, it *should* be, for it provides the ability to move beyond statistical association toward causal inference about why (and how) outcomes are produced in civil war settings. Yet, scholars have too often neglected its use. Instead, great pains have been taken to construct research designs that (at best) are able to identify suggestive correlations between variables, but lack the ability to test the mechanism(s) at work. Qualitative research is not immune to this criticism, either, for process tracing, when properly conducted, establishes a standard for rigor that often goes unmet even in detailed historical cases (see also Evangelista, this volume, Chapter 6). This is an unfortunate state of affairs; without understanding the causal processes that underpin associations, we foreclose opportunities to advance our theories of civil war and contribute to debates about the efficacy of different policies in violent settings.[1]

This chapter emphasizes the practicalities of marrying design-based inference with the strengths of process tracing to improve our ability to build and (especially) test theories about civil war onset and dynamics. Bennett and Checkel's ten best practices for process tracing (this volume, pp. 20–31) provide a springboard for a discussion of how to identify and conduct rigorous process tracing in settings marked by poor (or no) data, security concerns, and fluid events. The chapter also introduces ideas from the now-burgeoning literature on causal inference to help guide decisions about case

[1] Civil war is defined here as an armed confrontation resulting in at least 1,000 battle deaths between two or more combatants that were subject to the same political and legal system prior to the war.

selection and evidentiary standards. In particular, the approach advocated here draws on a potential outcomes framework that hinges on the use of counterfactual observations, "elaborate" theory, and qualitative evidence on treatment assignment to facilitate drawing causal inferences about why wars break out and how they are fought (see also Dunning, this volume, Chapter 8, on using process tracing to assess assignment to treatment).

I proceed as follows. The first section details the near absence of process tracing as a methodological approach in journal articles published since 1994 on civil war onset and dynamics. The second section draws on Elisabeth Wood's *Insurgent Collective Action and Civil War in El Salvador* (2003) as an illustration of Bennett and Checkel's ten "best practices" of process tracing. The third section discusses four additional "best practices" that arise from the causal inference literature and that are especially likely to be useful in civil war settings. Next, I detail potential research designs and the utility of process tracing for two literatures: the cross-national study of why civil wars break out, and the micro-level (for example, subnational) study of civilian victimization and its effects on subsequent participation in an insurgency. A fifth section briefly details the ethical and practical challenges faced by researchers in these environments. I conclude with thoughts about the use of process tracing to further our theoretical and practical understandings of civil war.

Process tracing and civil war

The meteoric rise of research on civil war has largely centered around two questions. One research agenda, heavily dominated by cross-national statistical analyses of the post-1945 era, has sought to explain civil war onset. These studies seek to draw an association between structural factors – state capacity, lootable resources, and ethnic exclusion from political power, to name three – and the outbreak of civil war. A second research program has drawn on a "micro-level" framework that explores the dynamics of violence – including its location, nature, and timing, especially toward civilians – at the subnational level. Unlike cross-national studies, these micro-level studies typically pay close attention to identifying the causal relationship between independent variables and outcomes using disaggregated time-series data and a host of sophisticated approaches, including quasi- and natural experiments, matching, and instrumental variable regression.

What role has process tracing played in these two research programs? Very little, it turns out. Figure 7.1 plots the sharp increase in the number of articles
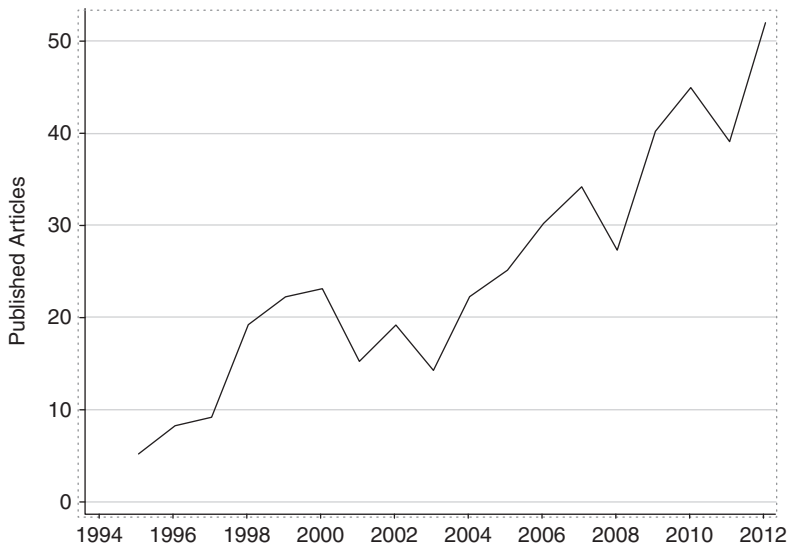
Figure 7.1   Number of articles published on civil war onset or dynamics in fifteen political science journals, 1995–2012

Note: The sample consists of 448 articles. Review articles and those in related fields (for example, genocide studies) were not included.

published annually in fifteen political science journals on the topic of civil war from 1995 to 2012.[2] Of these 448 articles, only 12 explicitly claim to be employing process tracing (all since 2004). While other work may be drawing implicitly on process-tracing insights (Checkel 2013b: 6), these articles reflect a more general trend away from the use of case studies, process tracing's natural habitat. Indeed, the share of articles with evidence from at least one case study has fallen from 80 percent in 1995 to about 50 percent in 2012. Over the past five years, an average of 44 percent of articles have had some form of case study, generously defined as a systematic discussion of a particular historical case at least four paragraphs in length.

The curious under-utilization of process tracing in civil war studies to date likely has several causes. Detailed process tracing can be difficult to execute within journal word limits. Perhaps relatedly, both cross-national and micro-level studies have increasingly adopted research designs built to measure the

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[2] The journals surveyed include: *American Political Science Review*, *American Journal of Political Science*, *Perspectives on Politics*, *Journal of Conflict Resolution*, *International Organization*, *International Security*, *Journal of Peace Research*, *Security Studies*, *Journal of Politics*, *World Politics*, *Comparative Politics*, *Comparative Political Studies*, *Civil Wars*, *Terrorism and Political Violence*, and *International Studies Quarterly*.

direction and magnitude of the relationship between independent variables and outcomes rather than the mechanisms that underpin this relationship. This is a pragmatic move for research programs in their early stages. It can be difficult enough simply to identify the existence of a relationship given the multiple threats to inference, poor or absent data, and noisy proxy measures that often characterize research in conflict settings. Moreover, research designs that are tasked with establishing associations between variables may not be suitable for testing mechanisms. Yet, without moving beyond correlation, we are left blind about the processes and dynamics that drive these relationships, impoverishing both our theories and our ability to contribute to policy debates.

Process tracing in action: an example

The apparent neglect of process tracing in journal articles notwithstanding, there are still exemplars of the craft within political science and civil war studies. I use Elisabeth Wood's (2003) book, *Insurgent Collective Action and Civil War in El Salvador*, as an illustration of the ten "best practices" of process tracing outlined by Bennett and Checkel (this volume, pp. 20–31).[3] *Insurgent Collective Action* tackles the twin questions of why peasants supported (and joined) an armed insurrection against El Salvador's government during the 1970s and 1980s and how that participation evolved over time. Wood's argument, developed inductively and deductively in equal measure, is a nuanced one. Individuals supported the armed opposition, she argues, through a series of emotional mechanisms, including a belief in the moral purpose of acting, defiance in the face of state repression, and "pleasure in changing unjust social structures through intentional action" (Wood 2003: 235). More simply, pride in the "authorship" of their wartime actions (ibid.: 231) led some individuals to eschew the relative safety of fence-sitting in favor of risky acts that carried no credible promise of immediate (or future) material pay-off.

This interpretation of high-risk collective action is pitted against alternative explanations that emphasize the need for material incentives (Olson 1965; Popkin 1979), protection from state violence (Mason and Krane 1989; Goodwin 2001), or strong horizontal networks among peasants (Moore 1978; Scott 1976) to induce participation. In the language of this volume's best practices, Wood clearly "casts her net widely" for alternative explanations

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[3] Waldner, this volume, Chapter 5, assesses Wood's use of process tracing in a different book – and comes to similar conclusions on its quality.

(criterion 1). She is also equally tough on these alternative explanations (criterion 2), marshaling an impressive array of ethnographic evidence from prolonged fieldwork to build her case.

To test these claims about the connection between emotions and participation, Wood initially engaged in eighteen months of fieldwork in four different sites in Usulután, a wealthy but conflicted department of El Salvador, and one site in Tenancingo in the northern department of Cuscatán.[4] Interviews with 200 *campesinos*, all but 24 of whom participated in the insurgency in some fashion, and mid-level Farabundo Martí National Liberation Front (Frente Farabundo Martí para la Liberación Nacional, or FMLN) commanders comprise the bulk of her evidence. In a particularly innovative (and non-intrusive) practice, twelve *campesino* teams engaged in collective map-making during three workshops in 1992 to provide a window into how peasant culture, especially pride in collective achievements, manifested itself. Wood is alert to the potential biases of her sources (criterion 3), particularly the problems associated with memory and (selective) recall of wartime activities. She also notes that her interviewees were not randomly selected, but instead chosen through *campesino* organizations, skewing her sample toward individuals who participated in the insurgency.

These materials, and the process of gathering them, enable Wood to generate inductively a wealth of insights (criterion 8). Yet, Wood's empirical claims do not rest solely on induction, for she also outlined the argument a priori using a formal model of individual decision-making (Wood 2003: 267–274). The micro-level motives for individual actions are also supported by insights from laboratory experiments developed by social psychologists. As a result, the book's argument draws on both inductive and deductive approaches to discipline its data gathering and to identify the specific processes that lead to *campesino* participation (criterion 9).

Wood selected her five field sites according to a fourfold criterion: their accessibility to an outside researcher; the presence of both supporters and non-supporters (for example, the regions had to be "contested"); variation in agrarian economies (to examine multiple pathways that peasants could take into the insurgency); and the presence of only one or two guerrilla factions (Wood 2003: 52–54). Taken together, it appears that these regions do offer representative examples of broader patterns of participation and violence in El Salvador's contested areas. What remains unclear, however, is whether these cases represent a "most likely" or "least likely" test for alternative explanations (criterion 4).

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[4] The book draws on additional research and visits over the following twelve years (Wood 2003: xiii).

By truncating variation on the degree of state control or rebel presence, we may be working outside scope conditions where material incentives or desire for protection from state violence are most operative, for example.

Moreover, while Wood's "starting rule" (criterion 5) is clearly justified – sometimes researchers must simply take advantage of opportunities to start work that are created exogenously by lulls in fighting – her "stopping rule" is less clear (criterion 6). It appears that repetition in the *campesino*'s own stories for why they participated was the decision rule for ceasing data collection; once the researcher has heard the same stories repeated across different respondents, data collection stops.

In this instance, however, the process tracing is not necessarily conclusive (criterion 10). The decision to over-sample participants, for example, even though two-thirds of the population did not participate meaningfully in the insurgency (Wood 2003: 242), could overestimate the importance of emotive mechanisms. Wood herself notes how past patterns of state violence and proximity to insurgent forces (ibid.: 237–238) conditioned whether these emotions could be acted upon. Sorting out the relative causal weight between emotions and mechanisms of control or prior exposure to violence would require additional interviews among non-participants both within and out-side of these five areas. Not all process tracing is definitive – indeed, the best examples typically raise more questions that could be tackled by adjusting the research design or sample frame to provide additional empirical leverage on the original process under study.

Avoiding "just-so" stories: additional best practices

In the spirit of this volume's emphasis on practicality, I offer four additional process-tracing best practices that can help researchers avoid "just-so" stories when exploring civil war dynamics. These include: (1) identifying counter-factual ("control") observations to help isolate causal processes and effects; (2) creating "elaborate" theories where congruence across multiple primary indicators and auxiliary measures ("clues") is used to assess the relative performance of competing explanations; (3) using process tracing to under-stand the nature of treatment assignment and possible threats to causal inference; and (4) out-of-sample testing. The emphasis here is on situations where researchers wish to test empirical claims, but cannot randomize the "treatment" (for example, state violence, rough terrain, etc.) due to practical limitations or ethical concerns.

First – and taking the Rubin Causal Model (RCM) as a point of departure – I emphasize the need for counterfactual reasoning to measure causal effects (Rubin 2006; Rosenbaum 2010; see also Evangelista, this volume, Chapter 6). The intuition here is a simple one: every unit – be it a village, province, or state – has a different potential outcome depending on its assignment to a particular treatment. Since we cannot by definition observe all outcomes in the same unit, we must engage in counterfactual reasoning to supply the "match" (or "control") for the unit where an outcome was unobserved. The more similar the control and treated observations along the values of their independent variables, the greater the confidence we have in our estimates of the treatment's causal effects.

The comparative nature of the RCM framework strengthens inferences from process tracing in several ways. By matching treated and control observations, the number of possible alternative explanations is reduced, simplifying the task of process tracing since some (ideally all but one, but hopefully many or even most) mechanisms are being held constant by a research design pairing cases that have similar values on independent variables. Process tracing can then be used to assess whether the treatment variable and the variables that could not be properly controlled for might account for observed outcomes. More generally, without the counterfactual, we cannot rule out the possibility that the same causal process is present in both the treated and control cases. To be confident about one's inferences, within-case process tracing should thus be paired with cross-case process tracing in a control observation where the presumed relationship between treatment and outcomes is not present.

The RCM framework also provides a natural bridge to emerging Bayesian approaches to process tracing (Bennett, this volume, Appendix; see also Beach and Pedersen 2013a: 83–88).[5] At its core, the Bayesian principle of "updating" one's prior beliefs in light of new evidence hinges on counterfactual reasoning. Bayesian updating is guided by the prior probability of a theory's validity and the likelihood ratio between "true positives" (instances where the evidence suggests a theory is true and the theory is in fact true) and "false positives" (instances where the evidence is consistent with a theory, but the theory itself is in fact false). The likelihood ratio itself relies, often implicitly, on control observations to provide both affirmative evidence for the preferred theory and eliminative induction that rules out alternative explanations and the possibility that a theory's claims are false. As Bayesian reasoning underscores, ruling

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[5] See also Humphreys and Jacobs 2013: 20–22.

out alternative explanations can sometimes generate greater discriminatory power for a test between hypotheses than discovering evidence that (further) confirms a preferred theory's validity.

Second, scholars should craft elaborate theories (Rosenbaum 2010: 329) that articulate multiple measures for the mechanism(s) at work (see also Jacobs, this volume, Chapter 2; Schimmelfennig, this volume, Chapter 4). If multiple mechanisms are thought to be present, then the sequence by which a process or effect is created should also be mapped out.[6] These hypotheses and measures should be specified before moving to empirical testing. Backward induction from a known outcome to the mechanisms that produced the outcome should be avoided, especially if counterfactuals are not used to eliminate the possibility that these mechanisms are also present in control cases.

Specifying multiple measures a priori enables the researcher to test for the congruence between these observations, helping to differentiate competing explanations that might rely on the same mechanism to explain an outcome. Put differently, the comparative strength of a particular argument may be decided not on the strength of evidence linking a variable to a mechanism, but instead on its ability to account for auxiliary observations as well as the sequence producing the outcome itself. From a Bayesian perspective, these auxiliary observations are "clues" that can shift beliefs about a theory's validity since their presence denotes that a specified process – and only that process – is responsible for the observed outcome.[7]

Third, treating potential outcomes explicitly also focuses one's attention on the key question of treatment assignment. The non-random nature of most "treatments" that interest civil war scholars means dealing with a host of methodological issues that can frustrate causal inference. Process tracing can help here, too. Qualitative data can be used to trace how the treatment was assigned to treated and control units, for example, a procedure Thad Dunning in Chapter 8 refers to as a treatment-assignment causal-process observation (see also Dunning 2012: 209). Understanding how the treatment was assigned, and whether it was truly assigned "as-if" random across units, is pivotal for micro-level studies that rely on natural or quasi-experiments to find starting points in the dynamics of civil war violence. Tracing the logic of assignment is especially important when evidence for these conditioning variables is private information among combatants, making it difficult to match across cases.

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[6] In Chapter 5, Waldner formalizes this insight through the use of "causal graphs."

[7] It is worth emphasizing that the probative value of these clues hinges on whether they are uncovered in a treated, but not a control, case.

More broadly, process tracing can be used to explore whether the proposed causal pathway between an independent variable (or treatment) and the suggested mechanism is even plausible. This task is especially relevant for cross-national studies, where the language of mechanisms is often invoked in fairly coarse terms – "state capture," for example, or "opportunity costs" – which obscures rather than reveals the causal processes unfolding at different subnational levels (Sambanis 2004; see also Checkel 2013b: chapter 1).[8] Similarly, cross-national studies that rely on exogenous events such as price commodity shocks to explore changes in conflict incidence across different states could be strengthened by using process tracing to clarify the channel(s) through which a shock affects state capacity or rebel recruitment at the subnational level. In this setting, since numerous mechanisms are plausible, process tracing the link between the shock and the mechanism would also be an important step in reducing the problem of equifinality that plagues cross-national studies.

Fourth, the distinction between process tracing for theory building versus theory testing is an important one (Bennett and Checkel, this volume, Chapter 1, pp. 7–8; see also Beach and Pedersen 2013a). While comparative observations (say, villages) within a particular case (say, a region within a country) are useful for theory building, out-of-sample tests are generally preferred for empirical testing to avoid "fitting" one's argument to the cases used to develop it. Lubkemann (2008) provides a neat illustration of this principle at work. Seeking to explain forced migration as a function of war, he began his empirical investigation in the Machaze district of Mozambique, which witnessed a high degree of violence and refugee outflow. He then followed the trail of internally displaced persons to new field sites, treating "dispersion as a field site" (ibid.: 25), including dispersion to the capital of the neighboring district and to the area across the border in South Africa. While his fieldwork in Machaze was formative in establishing propositions about refugee flows, it is the testing of these insights in locations not originally envisaged by the research design – process tracing out-of-sample, as it were – that provides greater confidence in his claims about the nature of wartime forced migration.

Working examples

I draw on two empirical examples to demonstrate the importance of process tracing to civil war studies. I first concentrate on the (mostly) cross-national

[8] See also Bazzi and Blattman 2011; and Berman and Couttenier 2013.

debate about the determinants of civil war onset. I then turn to emerging micro-level debates about the effects of civilian victimization on subsequent insurgent violence. In each case, I suggest possible research designs that use process tracing within a potential outcomes framework to adjudicate between proposed mechanisms linking independent variables to outcomes.

Working example 1: civil war onset

Why do civil wars break out? To date, scholars have sought answers to this question by predominantly utilizing cross-national regressions that link national level characteristics to the probability of civil war onset. In one notable example, James Fearon and David Laitin draw on data from 127 conflicts in the 1945 to 1999 era to argue that war is driven by opportunities for rebellion, not percolating grievances within the population. Instead, weak state capacity, as proxied by per capita income, and mountainous terrain are key drivers of insurgency; the weaker and more mountainous the state, the more likely we are to witness war (Fearon and Laitin 2003).

A recent spate of work has taken exception to this state capacity claim, however, and has instead argued that the exclusion of ethnic groups from executive political office better captures the origins of civil war onset. The larger the size of the excluded ethnic group, the greater the likelihood of civil war, especially if the now-excluded group once held the reins of political power (Cederman and Girardin 2007; Buhaug *et al.* 2008; Cederman *et al.* 2010).

This is an important and productive debate, but one subject to diminishing returns if the underlying processes that produce these outcomes continue to be left unexamined or measured with crude national-level proxy indicators. Absent new cross-national data, the greatest returns to investment appear to lie in the testing of proposed mechanisms at the subnational level.[9]

Take the argument by Cederman *et al.* (2010). These authors identify 124 ethnic civil wars (1946 to 2005) and employ a new data set (Ethnic Political Relations, or EPR) that measures the annual level of political exclusion from executive power for relevant ethnic groups within a given state. Using multivariate regression and several measures of political exclusion, they conclude that "we are able to establish an unequivocal relationship between the degree of access to state power and the likelihood of armed rebellion" (Cederman *et al.* 2010: 114).

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[9] For examples of the use of qualitative case studies to refine cross-national models, see Sambanis 2004; Collier and Sambanis 2005.

Table 7.1 Mechanisms and measures as proposed by Cederman *et al.* (2010)

Proposed mechanisms	Possible measures
Status reversal	Fear of domination; desire for revenge
Mobilization capacity	% of population (collective action)
Prior exposure to violence	Nationalist histories; violence as "thinkable"
State capacity	Force structure; deployment; bureaucracy; police
Spoils	Center-seeking behavior; spoil-seeking

Note: Below the dotted line are alternative mechanisms and proposed measures.

The authors cite five possible mechanisms that could undergird the relationship between rising ethnic exclusion and a greater likelihood of ethnic civil war. First, political exclusion can generate a fear of domination and resentment among excluded individuals, leading to a desire for (armed) revenge. Such motives are especially likely if the ethnic group was only recently excluded from political office. Second, the larger the excluded group, the greater its mobilizational capacity, and the greater its likelihood of leading an armed challenge against the state. Third, a history of prior conflict between ethnic groups can heighten the risk of war via three channels: (1) ethnonationalist activists glorify their group's history through one-sided narratives that stress their own victories and attribute blame for military losses to traitors, weak-spirited leaders, or a ruthless enemy; (2) past experiences of violence may become part of oral tradition or official narratives, nourishing calls for revenge; and (3) prior exposure to combat means that violence is no longer unthinkable, but constitutes part of the accepted repertoire of action.

These hypothesized mechanisms are summarized in Table 7.1. Mechanisms suggested by other theories are also listed, although these are illustrative rather than comprehensive. While the mechanisms offered by Cederman *et al.* (2010) are plausible, the evidence marshaled to support their presence is thin, consisting typically of a few short sentences (see, for example, ibid.: 110–111).

How could we go beyond statistical associations to examine the causal processes at work? One possible approach uses a potential outcomes framework to identify a series of comparative cases that isolate the mechanisms and their role in producing war onset. Political exclusion would be recast as a "treatment," while countries without ethnic group-based discrimination would represent the pool of available control observations. Matching could then be used to identify pairs of cases that have similar values across a range of theoretically important independent variables (or "covariates"), including

level of state capacity, ruggedness of terrain, and size of standing army. Assuming the statistical relationship identified in the full data set survives the matching procedure, we could then identify matched pairs of cases that are dissimilar only in their treatment status and the outcome (war onset/no war onset). Since the proposed argument rests on at least five mechanisms, no one matched pair will be able to test all possible mechanisms and their relationship to war onset. Instead, the matching procedure creates a pool of available paired comparisons that could be used to isolate individual mechanisms through a series of cascading comparisons.

For example, Comparison A could involve process tracing within and across a pair of similar cases where civil war onset was observed in the treated case (for example, the politically exclusionary state), but not in the control case. Each state could also have been subjected to an external shock – ideally, the same shock, such as a sharp decrease in commodity prices – that impacts each in a similarly negative fashion. This type of design would allow for separation of the effects of political exclusion from those of state capacity, as the price shock should affect each state in equal measure, yet civil war is only observed in the politically exclusionary state. Similarly, matching on additional (new) measures of state capacity such as bureaucratic penetration or the nature of infrastructure would enable the sifting out of the effects of status reversal or mobilizational capacity from the potentially confounding effects of (weak) state capacity (Comparison B).

Disaggregating an ethnic group's experience with political exclusion can provide additional causal leverage. Comparison C could involve two states that have similar characteristics, including presence of political exclusion, but where one group has experienced a sudden and recent reversal, while the other excluded group has not. A related set-up could examine a matched pair where the size of the excluded group varies (one large group, one small group) to test the link between mobilizational capacity and war onset (Comparison D). Another matched pair could examine two similar states with equivalent levels of political exclusion, but where one marginalized ethnic group has experienced prior violence at the hands of the state, while the "control" group has not suffered prior victimization (Comparison E). More ambitious designs could use matched pairs that control for several mechanisms across cases – say, status reversal and mobilizational capacity – and vary a third mechanism such as prior exposure to state violence (Comparison F).

Once the relevant comparisons have been established via matching, the actual process tracing can begin. To establish the credibility of ties between ethnic exclusion and war onset, we might consider qualitative evidence from

the recruitment drives of insurgent organizations. What types of appeals do they use to mobilize individuals? Are insurgents organized along ethnic lines? We should also observe that proportionately larger ethnic groups more readily overcome collective action problems when attempting to mobilize recruits. Ideally, evidence from both public and private claims about the nature of (ethnic) grievances would be uncovered and would dominate more tactical considerations such as perceptions of state weakness ("now is the time to strike because the state is weak") or a desire for spoils.

Process tracing is also essential for articulating the sequence of events leading up to the war. Cederman *et al.* (2010) suggest that rebels, not the state, should initiate the conflict. Did fear of ethnic domination precede the conflict, or were such concerns actually a product of the fighting? Were nationalist histories and memories of prior violence widespread, or did such myths emerge as a post-hoc rationalization for the war? And, perhaps most importantly, are these myths only actionable in political systems that exclude along ethnic lines, or can would-be rebels craft such narratives even in the absence of prior ethnic exclusion?

Finally, process tracing can play a crucial role in sifting out the indirect effects that state capacity might have on the mechanisms proposed by the ethnic exclusion argument. Although these arguments are typically pitted against one another, it is possible, indeed likely, that state actions can condition the effects of ethnic exclusion (and vice versa). Political exclusion may be a response to state weakness, for example, as an embattled elite seeks to "harden" its regime against potentially disloyal populations (Roessler 2011). More subtly, fear of ethnic domination may be a reflection of the military's ethnic composition, while opportunities for group mobilization may be conditioned by the size and deployment patterns of a state's armed forces. Cederman *et al.* (2010: 95, 106) also note that rapid and sudden ethnic reversal is especially likely in weak states, suggesting a more complicated relationship between state power (and violence) and grievance-based mechanisms (see also Wood 2003).

In short, adopting a potential outcomes framework involves the use of multiple comparisons ("cascades") to screen out competing theories and their mechanisms. It also enables a closer examination of the sequence by which ethnic exclusion translates into a heightened risk of conflict onset, helping to guard against reverse causation. Articulating an elaborate theory with numerous measures for each mechanism also strengthens our inferences about these processes by permitting congruence tests across multiple indicators, increasing our confidence that we have correctly identified the process(es) at work.

Working example 2: civilian casualties and insurgent violence

Civilian victimization and its effects on subsequent insurgent violence represents one of the fastest growing research areas in the study of civil war dynamics. Despite divergent methods, it has become a near article of faith that indiscriminate victimization of civilians facilitates the recruitment of newly abused individuals by insurgents, contributing to bloody spirals of escalatory violence between counterinsurgent and rebel forces (for example, Kalyvas 2006; US Army 2007; Jaeger and Paserman 2008; Kocher *et al.* 2011; Condra and Shapiro 2012; Schneider and Bussmann 2013). While this view is not uncontested (Lyall 2009), much of the debate now centers around the causal processes linking victimization to subsequent patterns of insurgent violence. To date, however, our research designs have not kept pace with the profusion of mechanisms cited by scholars as facilitating insurgent recruitment or producing escalatory spirals.

Setting aside for the moment the inherent difficulties in process tracing such a sensitive issue, the abundance of possible mechanisms, operating singularly or jointly, can frustrate efforts to establish defensible causal claims. Consider the following example from a January 2013 drone strike in Yemen, which killed at least one, and possibly five, innocent civilians:

As the five men stood arguing by a cluster of palm trees, a volley of remotely operated American missiles shot down from the night sky and incinerated them all, along with a camel that was tied up nearby.

In the days afterward, the people of the village vented their fury at the Americans with protests and briefly blocked a road. It is difficult to know what the long-term effects of the deaths will be, though some in the town – as in other areas where drones have killed civilians – say there was an upwelling of support for Al Qaeda, because such a move is seen as the only way to retaliate against the United States.

Innocents aside, even members of Al Qaeda invariably belong to a tribe, and when they are killed in drone strikes, their relatives – whatever their feelings about Al Qaeda – often swear to exact revenge on America.

"Al Qaeda always gives money to the family," said Hussein Ahmed Othman al Arwali, a tribal sheik from an area south of the capital called Mudhia, where Qaeda militants fought pitched battles with Yemeni soldiers last year. "Al Qaeda's leaders may be killed by drones, but the group still has its money, and people are still joining. For young men who are poor, the incentives are very strong: they offer you marriage, or money, and the ideological part works for some people."[10]

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[10] "Drone Strikes Risks to Get Rare Moment in the Public Eye," *New York Times*, February 6, 2013, A1.

This brief example usefully highlights at least five of the mechanisms that scholars typically invoke to explain the process from victimization to participation in an insurgency. A desire for revenge, tribal (group) ties, selective incentives in the form of money and marriage, and ideology all intermingle as plausible mechanisms in just this one instance. We might also add property damage, which leads to economic hardship and shifting reservation values for joining an insurgency (Abadie 2006),[11] and the belief that greater risk is associated with non-participation in an insurgency (Kalyvas and Kocher 2007), as two additional mechanisms not captured by this example.

The example also illustrates a second, less appreciated, issue: without prior baseline levels for these mechanisms, and without a similar control village that was not struck, we cannot assess the relative importance of these mechanisms or the causal effects of the air strike on subsequent behavior. Once again, a potential outcomes framework that emphasizes counterfactual observations provides insights not possible with a singular focus on within-case observations. Without a control observation, for example, we cannot establish either the direction or the magnitude of the air strike's effect on support for Al Qaeda. Similarly, without a before-and-after comparison of civilian attitudes and behavior across cases, we cannot determine whether the air strike increased, decreased, or had no effect on subsequent insurgent recruitment and violence.

Given the number of plausible mechanisms and the possibility that they might interact, how could process tracing be used to explore the links between victimization, recruitment, and subsequent participation in an insurgency? Table 7.2 outlines one possible research design.[12]

The basic idea is again one of maximizing comparisons by exploiting variation in the nature of the victimization and how it was administered. More specifically, we can create additional comparisons by decomposing the "treatment" – here, experiencing a drone strike – into different types of victimization, while including individuals in the sample who were present (i.e. in the same village) at the time of the strike, but who were not hurt, as counterfactual observations.

Variation in civilian victimization, for example, can be used to create comparisons that enable process tracing to link state violence to insurgent behavior. To separate the "revenge" mechanism from an economic hardship

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[11] See also Lyall 2013.

[12] This design draws on the author's experiences with USAID's Afghan Civilian Assistance Program II, administered by International Relief and Development (IRD) in Afghanistan during 2012 to 2013.

Table 7.2 Sample research design for assessing effects of civilian victimization using process tracing

Context (violence)	Assignment	
	Random ("as-if")	Targeted (selective)
Low	A, B, C	A, B, C
Medium	A, B, C	A, B, C
High	A, B, C	A, B, C

Note: A represents personal victimization; B represents property damage; C is a control individual in the selected location, but who was not victimized. A, B, and C are in the same village in this design. Violence is used as an important example of context. Assignment refers to the manner in which individual(s) were targeted, i.e. plausible claim to "as-if" random or selected according to some criteria. Context is by village.

one, we could compare individuals who are victimized but do not experience property damage (Type A) with those who only have property damage (Type B). We could then compare individuals A and B to individual C, who was present but unharmed by the drone strike. These individuals could be chosen via random selection (for example, from a list of victimized individuals and locations). A screening question could be used to insure that these individuals share similar socioeconomic characteristics. This procedure creates a two-control group comparison (Rosenbaum 2010) between individuals A and B, and between A and C, permitting in-depth process tracing to sort out the role played by different mechanisms in shaping an individual's attitudes.

We can also draw on process tracing inductively to explore the nature of the sample and the context in which the civilian victimization occurred. In particular, we should stratify our sample by levels of key covariates to account for victimization's conditional effects. In Table 7.2, I use the example of prior violence in a village by the counterinsurgent as one key conditioning factor with varying levels (here, high/medium/low). We might imagine that different mechanisms operate under different circumstances; a one-time event may have a different meaning from repeated violence, and so revenge motives or nationalism may have more purchase when heavy oppression is used rather than a one-time, possibly accidental, event. Stratifying our sample along these important covariates before process tracing also aids in illustrating gaps in our coverage. It may be impossible, for example, to access high violence areas, placing an important limit on the generalizability of our findings.

The credibility of our estimates about the effects of violence is also enhanced if we can demonstrate that this victimization occurred "as if" randomly. For most micro-level studies, the problem of selection bias looms large. That is, the individuals victimized differ in some important fashion from non-victims, since they were selected by the state for victimization. Some studies (for example, Condra and Shapiro 2012), however, contend that we can assume casualties are inflicted more or less randomly – unlucky individuals are in the "wrong place and time" – and so we can treat these casualties as unconnected ("plausibly exogenous") to broader patterns of war. The benefit, of course, is clear. If civilian casualties are not intimately tied to broader patterns of violence, then we are able to estimate cleanly the effects of these casualties on subsequent violence, without worrying about selection effects that might confound our study.

Whether this claim is plausible given the possibility of substantial heterogeneity in how civilians are victimized, variation in the meaning of victimization depending on the perpetrator's identity, and the prospect that civilians are often targeted strategically, is a central question for inductive process tracing. Determining whether (and when) the "as-if" random assumption holds also helps determine to which populations we can generalize when making claims about the effects of violence.

What form does the process tracing actually take? Given the observational equivalence of these mechanisms, it makes sense to shift the debate to examine how victimization affects attitudes, not behavior. Once again, we witness the virtues of elaborate theories, which force us (in this case) to create attitudinal measures for each mechanism that enable us to distinguish among causal pathways to insurgency. Table 7.3 offers an initial cut at measures for five

Table 7.3 Possible mechanisms linking civilian victimization to insurgent recruitment and violence

Proposed mechanisms	Possible measures
Revenge	View of government/counterinsurgent, sense of loss
Economic hardship	Changes in livelihood, beliefs about (future) well-being
Group identity	Perception of status; magnitude of co-ethnic bias
Risk	Willingness to consider risky actions
Selective incentives	Receipt and views of rebel provision of goods/services

Note: Proposed measures (not exhaustive) are designed to be consistent with multiple methodologies, including survey and behavioral experiments, focus groups, interviews, and ethnographic approaches that remain open to post-positivist notions of causation. Measured relative to control observations (individuals with no or different exposure to civilian victimization).

mechanisms that link victimization to increased participaion in an insurgency via changes in attitudes.

Creating multiple measures for each mechanism also creates more space to adopt different methodologies when process tracing (see also Checkel and Bennett, this volume, Chapter 10). Interviews with rebels, for example, have become a standard tool in the civil war scholar's methodological toolkit (Wood 2003; Weinstein 2007; Ladbury 2009), although care should be taken to insure that non-rebels are also interviewed. Survey experiments could also tap into these concepts using indirect measurement techniques that mitigate incentives for interview subjects to dissemble due to social desirability bias or concerns about reprisals (Humphreys and Weinstein 2008).[13] Focus groups provide an opportunity to explore not just individual level dynamics, but also the construction of narratives about civilian victimization and, in particular, how blame for these events is assigned. Behavioral "lab-in-the-field" experiments provide an additional means of measuring how violence affects attitudes, including preferences over risk, time horizons, and decision-making (Voors *et al.* 2012). Finally, ethnography may offer a window into how these dynamics shift over time. These processes are difficult to capture with surveys or one-off interviews, especially if the process between victimization and subsequent behavior has more of a "slow burn" than a "quick fuse" logic.

Each of these methods has its own particular strengths and weaknesses. Moreover, the environment after a civilian casualty event is among the most sensitive a researcher can experience. These factors combine to make "smoking-gun" evidence elusive in such settings; it is unlikely that evidence will be found to support one mechanism while trumping all others. Good process tracing may still not yield wholly conclusive evidence, as emphasized by Bennett and Checkel (this volume, Chapter 1). Instead, it may be more productive to explore the scope conditions that make certain pathways more or less likely to lead to insurgency. A potential outcomes framework that stresses the role of counterfactuals (i.e. non-victims), the need for multiple measures for each mechanism (i.e. "elaborate theory"), and a clear understanding of the selection mechanisms (was victimization deliberate or by chance?) offers one means for harnessing process tracing to the task of producing generalizable claims.[14]

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[13] See also Lyall *et al.* 2013.

[14] The relation of process tracing to theory type (mid-range, typological, general) remains a key challenge for future work. See also Checkel (this volume, Chapter 3); and Checkel and Bennett (this volume, Chapter 10).

Practicalities

My arguments thus far have tacitly assumed that fieldwork is necessary to gather most, if not all, of the data required for process tracing. Indeed, many of the methodologies best suited for process tracing – including lab-in-the-field and survey experiments, in-depth interviews, and ethnography – mandate an often-substantial investment in field research.

Yet, fieldwork in (post-)conflict settings presents a host of methodological, logistical, and ethical challenges (Wood 2006). A short list of such issues includes: the threat of physical harm to the researcher, his or her team, and local respondents; variable (and unpredictable) access to field sites due to changing battlefield conditions; the twin dangers of social desirability bias and faulty memories that may creep into interview and survey responses, especially in areas contested between combatants; the often-poor quality of data for key measures; the changing nature of causal relationships, where effects of a particular intervention may be large in the initial conflict period, but diminish over time as the conflict churns on; and reliance on outside actors and organizations for access and logistics that might shape perceptions of the researcher's work among potential respondents.

Context typically trumps generalization in these environments, so solutions to these problems are necessarily local in nature. That said, there are three issues that all researchers are likely to face when gathering data for process tracing in conflict zones.

First, researchers must obtain the voluntary consent of would-be interviewees and respondents. Though this is a common injunction for Institutional Review Board (IRB) approval at American universities, the requirement takes on a special cast in conflict settings, where individuals may run risks for simply meeting with (foreign) researchers or survey teams. Informed consent in these settings requires that participants understand the nature of the study (at least broadly), its funding source, and plans for dissemination, so that they can properly judge the risk associated with participating. It also requires that individuals recognize that they will receive no material benefits – for example, new disbursements of economic assistance – from participation.

Moreover, in many settings, such as Afghanistan, obtaining consent is a two-step process: first, with the stakeholders who control access to a given village and, second, with the prospective participant(s). Obtaining consent from these gatekeepers, whether government officials, local authorities, or rebel commanders, can mean the difference between accessing or being

excluded from certain locations. In addition, obtaining permission from local authorities can lower individuals' concerns about participating, potentially also reducing the bias in their responses to interview or survey questions. Consent from local authorities and individuals becomes especially important if one's process tracing hinges on gathering longitudinal data.

Second, maintaining the anonymity of interviewees and survey respondents is essential in wartime settings. Researchers must work to secure data and to insure that if compromised, it does not allow third parties to identify their sources. The simplest expedient is not to record an individual's name and instead use a randomized identification number. The advent of computers, cell phones, and portable data storage devices in the field has changed the calculus, however, making it possible to reconstruct an individual's identify even if his or her name was not recorded. Survey firms routinely use respondents' telephone numbers to call back for quality control purposes, while enumerators use GPS devices and maps to track their "random walks" in selected villages when creating samples.

Confidentiality and guarantees of anonymity must extend to these personal data, not simply an individual's identity, especially given the prospects for rapid dissemination if these electronic storage devices are compromised. In areas with good cell phone coverage – an increasing share of once remote locations – data from interviews, surveys, or maps should be stored remotely (for example, on a "cloud" storage site) and local copies deleted to mitigate the risks of unwanted data capture. Researchers should also maintain robust networks for returning surveys, interview notes, or other sensitive materials to a central safe location if electronic means are not available. In Afghanistan, for example, trucks carrying market wares to Kabul can be enlisted to deliver sealed packages of completed surveys back to Kabul, where they are then scanned and destroyed. This system avoids having dozens of enumerators risk exposure while carrying materials through potentially hundreds of checkpoints between their field sites and Kabul. Similarly, quality control call-backs can be completed by a manager at the field site; the phone's log is then deleted, thereby avoiding transporting these data across checkpoints.

Third, researchers must work to safeguard both themselves and members of their team. Surprisingly, existing IRB guidelines do not address the issue of researcher safety nor that of the enumerators, translators, fixers, and others who might work under the researcher's direction and who also assume risks by participating in the research. Establishing a baseline of risk before conducting research – How violent? Which actors are present? What types of movement restrictions exist? – can be useful in detecting sudden changes that

suggest increased risk for one's team. This baseline is also useful in selecting potential field sites as well as replacements, often via matching, which enables researchers to switch sites quickly without compromising their research design. Locals, who often have a far better sense of security risks than outsiders, should also be consulted when establishing notions of baseline risk. Finally, it is useful to construct a "kill-switch" protocol that can be activated if team members have been threatened (or worse). Activating the "kill-switch" (often via SMS) would signal to team members to wipe their data and withdraw to central points to avoid a credible threat, such as specific targeting of the team by rebel or government forces.

Conclusion

The explosion of research on the origins and dynamics of civil wars has not (yet) been accompanied by a turn to process tracing to identify and test the causal mechanisms that underpin our theories. This state of affairs is unfortunate, not least because political scientists have developed an increasingly sophisticated and eclectic methodological toolkit that could be applied toward process tracing in violent settings. Certainly, feasibility and safety concerns are paramount in these environments. Yet, as this chapter has sought to demonstrate, there are research designs and strategies that can be adopted to heighten our ability to make casual inferences despite these challenges.

The advantages of incorporating process tracing into conflict research also spill over to the policy realm. Process tracing offers an excellent means of uncovering the contextual "support factors" (Cartwright and Hardie 2012: 50–53) that help produce a causal effect. Without exploring these contextual factors, as well as the nature of the link between treatment and its mechanisms, we are left on shaky ground when trying to determine whether a particular effect or process generalizes to other settings. Moreover, process tracing is ideally suited to investigating possible interactions between multiple mechanisms. Policymakers, not to mention scholars conducting impact evaluations, are likely operating in settings marked by multiple mechanisms that interact in complex ways to produce a given effect. Pre-specifying the possible causal pathways and identifying several measures for these mechanisms, as called for by elaborate theorizing, will also help to avoid fishing for the "correct" mechanism via backward induction. The result of these efforts is likely to be a better understanding of how these processes unfold, thus contributing to our theories of civil wars as well.

Process tracing does have its limits, however. Without explicitly incorporating counterfactuals to facilitate cross- and within-case comparisons, theory-testing process tracing can lead to mistaken causal inferences about the robustness of a presumed relationship between an independent variable and outcomes. Moreover, crafting research designs that are capable of both identifying a statistical association and then competitively testing the mechanisms responsible for it may be a bridge too far. What may be required is a shift toward designs that take a particular relationship as a given and instead explicitly engage in process tracing to detail why this pattern is present. Danger lies in this type of strategy, though: the more micro-level the process tracing, the more contextual factors trump abstraction. The result may be a wonderfully nuanced account of a specific process that doesn't generalize to other settings even within the same case. Finally, a too-specific focus on mechanisms and process tracing might lead to neglecting the importance of structural factors that might condition which mechanisms are present and the magnitude of their effects (Checkel 2013b: 19).

Of course, process tracing is not unique in having drawbacks; no methodological approach is without its shortcomings. And the pay-offs, measured in terms of theoretical progress and policy insights, are considerable. By seeking to move beyond statistical associations to understanding why these relationships are present, scholars can open new avenues for exciting research into substantively important questions about the onset and battlefield dynamics of civil wars.