

Rethinking Civil War

by

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Dissertation submitted in partial fulfillment of
the requirements for the degree of Doctor of Philosophy
in the Department of Political Science
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ABSTRACT

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Abstract

Why do some civil conflicts simmer at low-intensity, while others escalate to war? When and why do some civil conflicts experience cycles of escalation and de-escalation? This dissertation challenges traditional approaches to intrastate conflict by arguing the need to distinguish both theoretically and empirically between the onset and escalation of civil conflict. I start with a formal model with incomplete information and a two-stage informational updating process. The model develops a novel, strategic argument about three causal mechanisms that differentially drive low-intensity violence (LIV) versus full-blown war: the information environment, the type of rebel group, and the state's capacity. Violence yields information on group identity and type, but differentially so over time; this inter-temporal variation in formation colors the state's strategic response, conditional on state capacity. For example, stronger groups become relatively more common past LIV, whereas before LIV, states have limited information on challenger type and so less ability to bargain. Empirical implications are tested in the third chapter using data on self-determination disputes from 1960-2005, with strong support for my argument. Results highlight the changing role of state capacity: stronger states are less likely to face LIV, but if they do, they are more likely to escalate to war. The fourth chapter expands on this analysis by using multi-state survival modeling to assess how the conflict evolves from start to finish, yielding nuanced findings about how key covariates affect a conflict's transition from (and cycles through) LIV to war to peace. This approach therefore forces

a reexamination of the seminal findings in civil war.

Dedication

*For Ryan, whose endless curiosity is a constant inspiration. May you always
remember to follow your dreams.*

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Chapter 1

Introduction

Since the end of World War Two, over half of all countries have experienced a civil war. Civil conflicts have occurred: on nearly every continent; in a variety of regime types, from democracies to dictatorships to oligarchies; and in poor and wealthy countries alike. Civil war has become the most common form of violent conflict, and scholars have devoted extensive time and energy to understanding why civil conflicts happen (Collier and Hoeffler, 2004; Blimes, 2006; Cederman et al., 2010, 2011; Cunningham, 2013; Fearon and Laitin, 2003; Gurr and Moore, 1997; Lichbach, 1994; Reynal-Querol, 2002; Ross, 2004).

Yet there is startling variety in how civil conflicts evolve. In some cases, minor violence or clashes are easily resolved, or at the very least, do not escalate further. For example, when the looming NAFTA agreement angered Mexican farmers and indigenous people in Chiapas, the Zapatista Army of National Liberation (EZLN, or *Ejercito Zapatista de Liberacion Nacional*) led an uprising in January 1994, capturing several towns and killing approximately 100 civilians.¹ The federal government

¹For newspaper coverage of the event, see BLOODY INDIAN REVOLT CONTINUES IN MEX-

reacted quickly and reached a ceasefire within two weeks.² When Tunisian protesters demanded the removal of President Zine El Abidene Ben Ali during the Arab Spring revolts in 2011, the President tried to placate the opposition with minor concessions; when that failed, he resigned and fled the country.

All too often, initial violence escalates further into a civil war. A tense spiral of repression and protest emerged in El Salvador in the 1970s; a military coup in 1979 followed by the assassination of the beloved Archbishop Oscar Romero in 1980 caused the conflict to intensify into war. Tens of thousands of people—mostly civilians—would be killed before the war ended in 1992. Other similar examples have played out across the world, from Syria to Myanmar, from Guatemala to Ethiopia. The variation is cross-national and within country. For example, of Nigeria’s hundreds of ethnic groups, most have remained peaceful, while some have violently protested; in one case, the Igbo separatist movement led to the Biafran rebellion in 1967.

We have a limited understanding of how a civil conflict evolves after any initial violence, how strategic expectations might constrain observed behavior, or how the potential for escalation might affect the initial onset of violence. Existing research falls largely into two camps. The first looks at the process behind the start of violence (Aksoy and Carter, 2014; Chenoweth and Cunningham, 2013; Chenoweth and Lewis, 2013; Cunningham, 2013, 2014, 2016; Larson and Lewis, 2017; Lewis, 2017). Key findings demonstrate that governments anticipate and try to counteract the emer-

ICO”. Washington Post. 1994-01-04. ISSN 0190-8286. Retrieved 2019-02-15.

²Tensions continue to periodically flare, but the conflict has never escalated to war.

gence of nascent challengers (Sullivan, 2015, 2016), while potential opposition groups try to predict how the government will respond, and act accordingly (Lacina, 2014; Ritter and Conrad, 2016). The second camp tries to understand when and why civil *war* – versus more minor conflict – happens (Blimes, 2006; Cederman et al., 2011; Collier and Hoeffler, 2004; Fearon and Laitin, 2003; Fearon, 2005; Hegre, 2014; Kalyvas and Balcells, 2010; Muller and Seligson, 1987; Vreeland, 2008). Here, scholars have shown that the countries at greatest risk for civil war are those with: low GDP per capita (Collier and Hoeffler, 2004; Fearon and Laitin, 2003), terrain that enables rebel groups to freely operate (Collier and Hoeffler, 2004), a large population base for potential recruits (Collier and Hoeffler, 2004), deep political cleavages and ethnic heterogeneity (Cederman et al., 2011; Posen, 1993; Reynal-Querol, 2002), and greater income inequality (Muller and Seligson, 1987; Wood, 2000).

This research has been groundbreaking and yielded rich, important insights. However, both lines of research have been distinct from each other; we lack a unified framework for understanding how and why civil conflicts start and then evolve. This is important for several reasons. First, the research that focuses on how state-opposition behavior leads to initial violence cannot account for how the conflict subsequently evolves. We therefore have less of a sense of how strategic expectations might constrain observed behavior or how the potential for escalation might affect the initial onset of violence. If rebel groups (or states) can be deterred by the threat of war – and research shows that they are (Lacina, 2014; Lewis, 2017) – then we need to be considering war when we try to understand initial, less severe violence. Second,

jumping right to war is also problematic, both theoretically and methodologically, because it overlooks all of the violence and strategic interaction that lead up to war. Conceptually, civil wars are a process (Sambanis, 2004b,a): a lot of violence must occur before a conflict-dyad reaches that threshold. Methodologically, overlooking low-intensity violence introduces a selection bias (Clark and Reed, 2003; Clark and Regan, 2003; Fearon, 2002; Reed, 2000, 2002). Anderson and Worsnop (2016) demonstrate that lumping what they call ‘major’ and ‘minor’ conflict together “can lead to indeterminate or divergent findings when causal mechanisms vary across cases” (2).

I therefore argue for a holistic approach to civil conflict, one that distinguishes between minor violence – what is called low-intensity violence (LIV) – and war, but uses a single framework to analyze them. I move beyond the existing empirical definitions of LIV and war (25 battle deaths per year and 1000 battle deaths per year, respectively) to providing a more theoretically grounded distinction between the two. Then, I tackle questions such as: when does intrastate conflict escalate from minor conflict to war? How does the strategic interaction between the state and rebel groups influence this process? What makes each stage of the conflict more likely, and how does that change by stage? In doing so, I challenge existing approaches for understanding the start of civil conflict. I rely on multiple methodological approaches, including a formal model and several different statistical approaches, and I leverage historical examples to provide context where possible.

Several definitions are helpful at this point. First, by civil conflict, I mean a dispute that occurs within a single country, where one of the actors is the central or

federal government of that country. While conflicts can and do occur between two non-state actors within a country, the process and mechanisms are very different. I focus on cases where the government is one of the sides in the conflict. Second, by civil war, I refer to one type of civil conflict – or, more precisely, one stage in the civil conflict – with extreme violence. To date, most research has defined war as conflicts that experience more than 1,000 battle deaths (Collier and Hoeffler, 2004; Fearon and Laitin, 2003), largely as an artifact of initial reliance on the Correlates of War (COW) project’s civil wars list.³ I use this definition as a starting point before building towards a richer understanding. Third, when I refer to the onset of civil conflict, I am talking about the start of LIV. This is somewhat of a departure from the literature: while recent research use this threshold, many of the canonical studies that study onset are really studying the start of war. However, it seems more confusing to conflate onset and war, particularly when I am trying to assess the LIV that precedes war.

The dissertation proceeds as follows. In Chapter 2, “The Spectre of Escalation: A Strategic Approach to Civil Conflicts,” I develop the theoretical heart of the dissertation. I start by establishing key theoretical definitions: how is low-intensity violence different from war? Then, I use a formal model of a dynamic game with incomplete information and two stages of informational updating to assess the start and escalation of civil conflict. The model features two players, the government and

³For an excellent discussion of alternative empirical definitions and the merits and drawbacks of each, see Sambanis (2004b).

a prospective rebel group; the group can be one of three types (inactive, weak, or strong), and while the rebel group knows its own type, the government does not. The model's most informative equilibrium is a separating one where only the strong type of opposition group will violently mobilize after repression, driving the government to repress in order to get a signal of opposition type. The rebel group's decision to use violence (or not) thus provides the government with better, more updated expectations about the cost of fighting a war and the corresponding likelihood of winning: seeing violence, the government believes it faces a strong rebel group. It therefore prefers to negotiate, which the group accepts, and escalation to war is avoided. The model highlights a key unstudied difference between intrastate conflict and interstate conflict: the role of the information environment, which is usually so poor initially (for the state) that the state cannot even identify the specific group that is at risk of violent mobilization. The model also focuses on two key mechanisms, state capacity and rebel group type, and shows how they affect each stage of the conflict.

Chapter 3, "Rethinking Civil War Onset and Escalation," explores the empirical implications associated with the theory in Chapter 2. There are two main goals. First, since so many scholars of civil conflict use statistical models that either combine LIV and war or overlook one in favor of the other, I ask, "what are the implications of current empirical approaches?" I draw on research from interstate conflict to show that lumping LIV and war together can create major methodological issues, including introducing a selection bias that can severely distort results. I use that literature to justify an alternative approach to studying the onset and escalation of civil conflict:

selection models. I conduct two main sets of analysis, the first of which compares my approach to existing models.

The second goal for the chapter is to use hypothesis testing to evaluate the claims of the model in Chapter 2. To that end, I conduct a second set of statistical analyses that uses selection models to further test the empirical implications of the model from Chapter 2. The results support my argument and have strong implications for existing research, showing that relationships are more nuanced than previously shown. One of the main findings contravenes the canonical wisdom that GDP per capita is negatively correlated with civil war. Indeed, I show that, while high GDP per capita decreases the risk of LIV, if a rich/strong country does experience LIV, it is actually more likely to escalate to war.

Then, I extend the theoretical and empirical work to include the entire civil conflict in Chapter 4, “Cycles of Violence: Civil War Onset, Escalation, and De-escalation.” Chapters 2 and 3 focused on a linear progression of civil conflict—a country starts with no conflict, then a rebel group emerges (or not), and the conflict escalates (or not)—but conflicts often wax and wane through one or multiple cycles of escalation and de-escalation. I identify four stages of a civil conflict (no violence, low intensity violence, war, and resolution) and establish a theoretical framework for evaluating conflicts from their start to their finish. I draw on the theoretical model from the first chapter to develop theoretical expectations about how state capacity and rebel group strength will affect key conflict transitions. To test this stage approach to civil conflict, I use a relatively new methodological approach—multi-state survival modeling—that yields

descriptive information about common conflict sequences, frequency of transitions etc., as well as hypothesis testing about what variables matter at which stages. The results provide mixed support for my hypotheses but demonstrate that disaggregating a conflict into its stages yields a much more nuanced understanding of the factors that influence each part of the cycle.

The final chapter concludes by contextualizing the key theoretical developments and empirical findings. I review the main highlights of each chapter, assessing how each piece builds off of the chapter before it. Then, I discuss what the implications are from this dissertation, focusing on new avenues of research. I discuss two lines of inquiry that directly follow from the dissertation and suggest ways of moving forward.

Chapter 2

The Spectre of Escalation: A Strategic Approach to Civil Conflicts

Civil wars have become the most common form of violent conflict worldwide, yet there is incredible variation in how these conflicts play out on the ground. In the Arab Spring revolts that swept the Middle East and North Africa in 2011, Tunisian protesters demanding the removal of President Zine El Abidine Ben Ali were successful: the army officer who had been president since 1987 resigned and fled the country in January 2011 amid massive unrest. Yet when the protests spread to Syria, President Bashar al-Assad did not try and negotiate with the opposition and did not resign or flee the country. Rather, he cracked down, killing hundreds of demonstrators in March 2011, and the conflict quickly escalated into a brutal civil war. This variation is both cross-national and within-country. For example, Nigeria has hundreds of ethnic groups, with some rebelling while others remain peaceful: the Yoruba's demands for greater autonomy have always been peaceful, but the Igbo separatist movement led to the Biafran rebellion in 1967.

When does intrastate conflict escalate from minor conflict to war? How does the strategic interaction between the state and rebel groups influence this process? What makes each stage of the conflict more likely, and how does that change by stage? Existing research on the causes of civil conflict has started to unpack the process behind the onset of violence (Cunningham, 2013, 2014, 2016; Lewis, 2017; Aksoy and Carter, 2014; Chenoweth and Lewis, 2013; Chenoweth and Cunningham, 2013), so we now know that governments anticipate challenger emergence (Sullivan, 2015), and nascent opposition groups anticipate and act based on expected government behavior (Lacina, 2014; Ritter and Conrad, 2016). However, this body of literature focuses on how strategy drives state-opposition behavior *before* any violence occurs. We have less of a sense for how the civil conflict process evolves after initial violence, how strategic expectations might constrain observed behavior, or how the potential for escalation might affect the initial onset of violence.

I argue that, to fully understand the answers to these questions, we must study the civil conflict process holistically. Analyzing only the initial decision to commit violence without accounting for the spectre of escalation yields an incomplete, potentially biased theory because it fails to account for how the expectation of future behavior might drive early events. Therefore, I develop a formal model of a dynamic game with incomplete information and two stages of informational updating. The game features a government and a (prospective) opposition group, which can be one of three types, unknown to the government: inactive, weak, or strong. While there are many possible equilibria, the most informative is a separating equilibrium, in which

only the strong type of opposition groups will mobilize violently after repression, so the government represses in order to get a signal of opposition type. This lets the government better formulate its expectations about the cost of fighting a war (and the probability of victory); since the government expects to face a strong opponent, it negotiates after mobilization. The group accepts, and escalation to war is avoided.

The model makes three main contributions. First, the model highlights a key, unstudied difference between intrastate conflict and interstate conflict: in civil conflict, the initial information environment for the state is so poor that the state usually cannot even identify the specific group that is at risk of violent mobilization (though it might be aware that such a group exists). In interstate conflicts, actors might be uncertain about each other's type, payoffs, and/or goals, but each side is aware of *who* it faces. This is not always the case for intrastate conflict, at least initially. Although the state might be aware that some of its citizens are aggrieved, it does not know *a priori* if they are willing or capable of mobilizing to violence. In interstate conflict, resolve and capacity can both be signaled through violence. This process is even more extreme for intrastate conflict: violence perpetrated by the opposition as it mobilizes provides key information to the state. This improvement in the information environment makes bargaining and negotiations more likely. Note that this information shift can help explain temporal variation in conflict, while variation in a state's ability to gather information (e.g. its intelligence apparatus) can help explain cross-national variation.

Second, the model emphasizes the role of the state's capacity, yielding the coun-

terintuitive finding that stronger states are more likely to experience escalation, if they face an initial challenge. In other words, greater state capacity drives up mobilization costs for the opposition group, deterring many initial challengers. But if a state does face a challenger, the state knows that its chances of winning are greater and prefers to fight. This finding is a key departure from the conventional wisdom in studies of civil war onset, which argues that greater capacity decreases a country's risk for intrastate war (Collier and Hoeffler, 2004; Fearon and Laitin, 2003; Reynal-Querol, 2002)¹. My model shows that the relationship is more nuanced than existing research suggests, and that we need to fundamentally revisit our understanding of the mechanisms driving the start of violence versus its escalation.

Third, the model explores not only the role of the state but also the role of the (prospective) opposition group. Consistent with the literature (Lacina, 2014; Lewis, 2017; Ritter and Conrad, 2016; Sullivan, 2015, 2016), I show that the group's expectation of state violence drives behavior, such that weak groups will often self-censor in the face of state violence; weak groups that do mobilize face a higher chance of getting weeded out by the state. This means that a state facing a group that has successfully mobilized into low-intensity violence (LIV) and is at risk of further escalation is more likely to be facing a strong, resolved rebel group.

The chapter proceeds as follows. I first engage the existing literature about the forces driving intrastate conflict, distinguishing the existing research on the causes of

¹For an exception, see Thies (2015), which argues that greater state fiscal capacity does not decrease the chance of civil war onset.

war from more recent scholarship on the causes of initial, low-intensity violence. I then develop a theoretical distinction between LIV and war that moves beyond current empirical definitions. The next section develops the formal model. I discuss the most informative equilibrium and assess the implications from the model, incorporating examples from civil conflicts to illustrate the logic. The final section concludes with key takeaways and next steps.

2.1 Causes of intrastate conflict

The literature on the causes of civil conflict is extensive, although it has focused primarily on understanding why states experience war.² Scholars have approached this topic by asking two different questions: what conditions make a country vulnerable? And why do people join rebellions? Explanations about vulnerabilities focused on macro-level, permissive structural conditions such as regime type, GDP per capita, population, size, and mountainous terrain (Blimes, 2006; Cederman et al., 2011; Collier and Hoeffler, 2004; Fearon and Laitin, 2003; Fearon, 2005; Hegre, 2014; Kalyvas and Balcells, 2010; Muller and Seligson, 1987; Vreeland, 2008). These studies defined civil war as 1,000 battle deaths/year and tested hypotheses using logit and probit models. For example, Collier and Hoeffler (2004) found that countries at greatest risk for civil war were those with structural conditions that created opportunities

²It is important to note here that scholars have used different thresholds of violence when examining causes. Much of the early research on civil war ‘onset’ used a 1,000 battle deaths threshold (Collier and Hoeffler, 2004; Fearon and Laitin, 2003; Fearon, 2005; Reynal-Querol, 2002); more recent work uses the 25 battle deaths standard established by UCDP (Uppsala Conflict Data Program, 2009; Cunningham, 2013).

for rebellion: lack of economic alternatives to rebellion (poor GDP per capita), the ability for rebel groups to operate freely (mountainous terrain), and many potential recruits (large populations). Fearon and Laitin (2003) agreed that opportunities for rebellion best explained full-blown civil war, keying in on many of the same factors, although they emphasized weak states (proxied by GDP per capita).³ Indeed, one of the most consistent findings in the civil war literature has been that countries with lower GDP per capita are more likely to experience a civil conflict with 1,000 or more battle deaths.⁴

Scholars also demonstrated that political cleavages and ethnic heterogeneity are key conditions for civil war (Cederman et al., 2011; Posen, 1993; Reynal-Querol, 2002). Countries with larger numbers of minority populations also face a greater risk of conflict, and so do countries with greater income inequality (Muller and Seligson, 1987; Wood, 2000), because those societies are more likely to have individuals who are sufficiently motivated to fight. Researchers stressed that democracy could lead to a lower probability of civil war (Collier and Hoeffler, 2004; Fearon and Laitin, 2003), although mid-level democracies were actually more at risk than their autocratic counterparts (Hegre et al., 2001; Reynal-Querol, 2002; Sambanis, 2001).⁵

We know less about low-intensity violence. A newer body of literature has started to address the causes of LIV (Chenoweth and Cunningham, 2013; Cunningham, 2014;

³Note that the two studies disagreed about what GDP per capita might represent (economic grievance or lack thereof, state capacity, etc.).

⁴See also Miguel et al. (2004) and Ross (2004), who argue that poor economic performance matters not for opportunity costs but because it generates discontent.

⁵Note that the relationship is more complex between democracy and the conduct and termination of war (Hultman, 2012).

Larson and Lewis, 2017; Ritter and Conrad, 2016; Sullivan, 2015, 2016), finding that LIV is more likely when groups: can operate in secrecy (Lewis, 2017), are excluded from power but still strong (Cederman et al., 2010), and/or are factionalized, because the government is less able to identify a bargaining partner (Cunningham, 2013). Conversely, LIV is less likely if opposition movements feel their probability of success is negligible, either because of the prospect of external pro-government intervention (Cunningham, 2016) or a strong state's crackdown (Lacina, 2014).

However, these different lines of research are conducted in isolation, and many studies fail to distinguish between LIV and high-intensity wars. We have a poor understanding of how LIV and war fit into the broader intrastate conflict process. What happens after LIV? What, if any, are the differences between the two, and how does one affect the other? These approaches lead to conceptual and methodological shortcomings. Civil wars are a process (Sambanis, 2004b,a), and a lot of violence must occur even before a conflict-dyad reaches that threshold. Indeed, many intrastate conflicts languish at low-intensity without ever escalating: between 1993-2004, only 22 out of 122 minor conflicts escalated (Melander et al., 2009).

Overlooking LIV when analyzing war – or examining either in isolation – is particularly dangerous if there is a broader strategic interaction connecting them. If we believe that either the state or opposition group (or both) are strategic, forward-thinking actors, then that implies that even the threat of war can influence LIV, and LIV itself might reveal valuable information that in turn shapes the prospect of war. While plenty of research has examined the strategic incentives for why states might

decide to go to war with each other (Fearon, 1995; Filson and Werner, 2002, 2004; Meirowitz et al., 2008; Sartori, 2002), this has not been systematically applied to intrastate war. The state as a strategic actor is almost always either missing completely or treated as exogenous. Scholars have noticed this lacuna and started calling for studies that focus on why and how states accommodate or escalate opposition challenges; one even asserts that such efforts will “reinvigorate the political foundations of research on civil war” (Staniland, 2012). Recent work that has started to address this gap has found that: governments anticipate challenger emergence (Sullivan, 2015); the effects of repression on challenger activity depend on the type and timing of behavior targeted (Sullivan, 2016); and nascent opposition groups anticipate and act based on expected government behavior (Lacina, 2014; Ritter and Conrad, 2016). Yet these studies all focus on government-opposition strategic behavior *before* violence emerges.⁶ Much work remains to be done on how this strategic interaction can affect the onset and escalation of civil conflict.

2.2 Low-intensity violence or war?

I argue that LIV and war are qualitatively different stages of the broader intrastate conflict process. These two stages do not occur in isolation but rather affect each other: the former must occur for the latter to happen. For example, the resources required to reach LIV – whether arms, funds, or civilian support – can then be used to

⁶For a notable exception, see Leventoğlu and Metternich (2018), which argues that rural rebel groups that create coalitions with urban, middle class citizens are more successful at extracting government concessions during civil war.

reach or sustain war. To date, most research uses an empirical distinction, defining war as conflicts that cross the 1,000 battle deaths threshold, and LIV as conflicts crossing the 25 battle deaths threshold. This distinction is widely acknowledged as theoretically unsatisfying, not only because of the arbitrariness of the threshold – e.g. why 1,000 and not 700, 500, or 1200? – but also because it fails to address the qualitative distinctions between the different stages.⁷

I posit that we can distinguish between LIV and war by examining the scope of violence, level of mobilization, and amount of information available. LIV will be characterized by violence that is minimal in intensity and scope; by scope, I mean the geographic coverage of the conflict, both in terms of the extent of areas affected as well as the importance of the contested territory (e.g. is the capital experiencing fighting?). In other words, the conflict will not have spread geographically, and pitched battles will not have occurred yet (if it is a conventional war).⁸ The opposition is in the process of mobilizing but is still gaining key organizational/tactical experience, and the state has not yet mobilized its armed forces, typically using police to conduct any repression or violence. Lastly, the information available to the state is limited, with the (prospective) opposition group’s identity often unknown. War, by comparison, has intense violence, in terms of scope and/or deaths; in conventional wars, this is the stage with pitched battles. Both sides have completely mobilized, and the state has

⁷For an excellent discussion of the methodological problems with existing thresholds, see Anderson and Worsnop (2016). The second chapter of the dissertation focuses more extensively on the methodological problems associated with conflating the two stages.

⁸Kalyvas and Balcells (2010) categorize civil wars as conventional, irregular, or symmetric non-conventional.

gained key information about the opposition group, particularly in terms of capacity. Table 2.1 summarizes these key differences. It is important to note that these are ideal types, in the sense that they are the two ends of a continuum: there is not a hard and fast threshold (e.g. 1000 battle deaths) to easily distinguish conceptually between the two.

Table 2.1: Theoretical Differences between LIV and War

	LIV	War
Scope of violence	Minor, localized	Intense, widespread
Level of mobilization	Incomplete	Complete
Amount of information	Limited	Extensive

Considering both LIV and war together is particularly important if we believe that strategy influences the substate conflict process. Doing so facilitates the examination of war as (at least in part) a bargaining failure, where the absence of war might in fact be a strategic decision made by the state or rebel group or both. The potential prospect of war should modify the behaviors of strategic actors, whether by affecting which rebels perpetuate more violence (Lacina, 2014) or by modifying state behavior via repression or accommodation (Sullivan, 2015, 2016).

More importantly, this relationship between the two stages means that the causal mechanisms operate differently at each stage. State strength provides an intuitive example: while strong states might be less likely to face LIV, those that do experience LIV might in fact be more likely to go to war (perhaps because they are more capable

of yielding the military force required).⁹¹⁰ We could imagine similar stories for many different mechanisms, from democracy to rebel group strength.

Taken together, these features – the strategic relationship between stages, the different roles of causal mechanisms – indicate that a formal model is particularly well-suited to developing an understanding of the processes at work. I focus on three mechanisms in this article: the type of rebel group in operation, the information environment, and the state’s capacity. Each of these mechanisms affects the probability of conflict by changing a state’s relative costs of bargaining versus violence.

2.2.1 A formal model of LIV and war

The formal model takes as its starting point two actors, the government and a (prospective) rebel group. The government knows that some component of the population is dissatisfied with the status quo, but it does not know the extent of this dissatisfaction. Specifically, the government does not know if a rebel group has even formed; by extension, this means it is uncertain about a group’s capacity and/or resolve. This is a key distinction between the information environment in interstate versus intrastate conflict: during an international crisis, state A might be uncertain about state B’s abilities or resolve, but A knows that it is facing B (and not for example, a different state). For example, when the US government considered launching an air strike campaign in Syria, it had to wrestle with uncertainty over how Russia

⁹To more specifically preview the model results, this requires a strong rebel group that is not deterred by a strong state. Weak groups will generally be deterred and not act.

¹⁰I provide support for this claim in the subsequent chapter.

would respond to the air strikes and how extensive Syria's air defense capabilities were, but it knew that it would be facing Russia and Syria.

This is often very different in the intrastate context. When a group is first forming, it often prefers to operate in secrecy (Lewis, 2017). For example, The Shining Path in Peru started by organizing secret planning councils in Ayacucho before it was ready to engage in combat. Similarly, in work on rebel formation in Uganda, Lewis (2017) notes:

“several interviews of former rebel leaders indicated that when initially forming their group, their focus was not on mobilizing a broad swathe of the local population, *as this would have led to detection by the government* – rather they were concerned with maintaining the population's silence” (1430, emphasis added).

These examples point to key scope conditions for the model and theoretical argument more broadly. The argument applies most strongly to groups that are completely unknown to the state – like a nascent rebel group that has not yet caused many, if any, deaths – or movements, organizations, and/or groups already operating (often in peripheral territory) but looking to violently mobilize, that the state is only loosely aware of. The informational asymmetries facing the state are less severe for a political party that has become a rebel group; in that case, the state has a general sense of the group's identity, demands, and citizen support, although it might be uncertain about the group's fighting capabilities or resolve for violence. A rebel group that lingers from a previous war typically is associated with the lowest informational asymmetry, as the state already has substantial information from the prior conflict.

In general however, while the state might suspect a group is forming – or more generally anticipate some aggrieved population – at the very early stages, it cannot typically identify a specific group that it faces. More precisely, it does not know if it faces a group that is willing to violently mobilize to contest the state. This is very difficult to do before a group has committed any violence, because it requires that the state is able to precisely identify a nascent group and identify that group’s goals. Doing so levies high informational costs that many states are unwilling (and in some cases unable) to pay: that information usually cannot be obtained without a strong surveillance infrastructure. Even with a strong surveillance infrastructure, a state’s police and intelligence apparatus must sift through an extraordinary amount of information with a high degree of uncertainty in order to identify potential threats. States do not have unlimited resources – in terms of time, money, or personnel – to continuously sift through information about all aggrieved citizens. They therefore are frequently unable to precisely identify who is poised to commit violence.

An extreme case: The United States

The United States has arguably the strongest surveillance infrastructure worldwide. Yet even its defense and intelligence agencies grapple with this informational asymmetry problem. The NSA’s website states “We cope with the overload of information in our environment.”¹¹ David Shedd, the former Acting Director of the Defense Intelligence Agency, has said “The Department of Defense spends an inordinate amount

¹¹This quote was taken from www.nsa.gov in April 2018

of time and effort on the pursuit of indications and warning,” and similar statements are frequently made to congressional committees, in national intelligence documents, and during public appearances more broadly.¹²

Domestically, the challenges are even greater, because legal protections for U.S. citizens (and even non-U.S. citizens on U.S. soil) severely restrict the government’s ability to gather information indiscriminately against large portions of the population. Traditional information gathering techniques like wiretaps or surveillance require warrants and/or precisions, which are often unavailable before any violent behavior.

It might be tempting to dismiss these statements as political pandering, but the difference in capabilities for known versus new threats is tangible in the U.S. case. The national security apparatus handles so-called emerging threats (e.g. lone wolf attacks like the Boston Marathon bombing) differently than known, established threats (e.g. Al Qaeda). This is true not only for threats with international origins but also for purely domestic threats. From the FBI to NSA, the ‘unknown’ unknowns are pursued in separate offices. And the challenges are multi-faceted. First, the resource constraints are often more acute. Analysts and staff officers have a much broader scope, because of the ambiguity inherent in unknown threats, but the government typically dedicates more money and resources to the certain threats – the top priorities. Second, finding information about a known group or adversary is not necessarily

¹²Shedd’s comments were made at an intelligence community panel moderated by CNN’s Kimberly Dozier on September 18, 2014. For the full transcript, see <https://www.nsa.gov/news-features/speechestestimonies/speeches/insa.shtml>. For additional examples, see the Department of Homeland Security’s 2014 Quadrennial Review; NSA General Counsel’s speech to the University of Texas School of Law; and the collection of speeches and congressional testimony available at www.cia.gov.

easy – the U.S. struggled for decades to find Osama bin Laden and has notoriously been challenged in its efforts to deal with mob and gang violence – but it is easier than identifying an emerging group that wishes to stay hidden. When trying to learn more about the capabilities, goals, tactics, etc. of a known group, there is a clear starting point and a trail of bread crumbs to follow: spies know what organization to try and infiltrate (although they may not be successful), and analysts know whom to target for wiretaps or other electronic surveillance, even if a judge might deny the request. Those options are not available for emerging threats; instead, the government has to look for indirect signs of a group. Additionally, the sheer lack of a specific target to pursue means attention has to be spread much more thinly, across all potential targets rather than a smaller group of definite ones. At a 2018 Senate hearing, FBI Director Christopher Wray stressed the difficulty of detecting and preventing lone wolf attacks on U.S. soil, stating “what makes it so hard is that there are not many dots to connect with some of these people.”¹³ In the early days of the U.S. occupation of Iraq, the U.S. so miscalculated the extent of any Iraqi resistance that, even after the U.S. and allied forces had gained control of the country, President Bush was still planning a quick exit; one military official bluntly admitted ‘the Baathist insurgency surprised us’ (Gordon and Trainor, 2006).¹⁴ This is not to say that the U.S. could not have anticipated an insurgency, and in fact many have suggested that the U.S. should

¹³*FBI Investigating 1,000 suspected ‘lone wolf’ militants, director says*. <https://www.theguardian.com/us-news/2018/may/17/fbi-investigating-1000-suspected-lone-wolf-militants-director-says>. Accessed February 15, 2019.

¹⁴See p. 569.

have expected resistance. Rather, I use this example to point out that, even when the U.S. defense apparatus was wholly focused on Iraq, it was difficult to identify an emerging threat.

More generally, I use the U.S. case here precisely because it is an extreme case. As the strongest country – not only militarily but also, and more to the point, in terms of surveillance capabilities – it still faces a severe informational deficit, even against threats emerging from and targeted towards its own soil. We can imagine how much stronger the problem is for countries with less extensive surveillance infrastructure. Particularly in countries where the telecommunications infrastructure is limited, and the government’s ability to capitalize on mobile or Internet communications is correspondingly restricted, governments are at a disadvantage. This is also true in countries where the geographical terrain makes physical access difficult, or in places where noticeable physical markers challenge government attempts to infiltrate local challenger groups.

The model: the information environment, group type, and order of play

I capture this informational deficit and dynamic in the model in two ways. First, I use state uncertainty over rebel group type, where type includes an option for no violence at all. In the model, the rebel group can therefore be one of three types: inactive, weak, or strong. This is a trichotomous set. The group is first distinguished by whether or not it can fundamentally solve the collective action problem to violently mobilize; those groups that could/would mobilize are further distinguished by whether

they are weak or strong. Note that there are many different reasons why we might observe an ‘inactive’ group empirically: no rebel group might exist, a rebel group might form initially but then be eliminated by the state before it can commit violence (Lewis, 2017), or it might be ethically committed to nonviolence. Each of these possibilities is caused by different mechanisms and processes. However, the focus of my model is not about disentangling the different causes of a lack of violence, but rather about understanding why violence does emerge, and why it sometimes escalates to war. I therefore abstract away from the different reasons we might fail to see violence: the group either mobilizes (commits violence) or it does not. Once the group starts violence, additional action possibilities emerge.

Indeed, the second way I capture the information dynamic is via the order of play. The game begins in the first stage with the government’s decision to repress (R) or accommodate (A). Here, accommodation means some kind of boon ($\kappa\gamma$) to the aggrieved population, not targeted concessions to a rebel group. For example, this might be something like easing cultural restrictions such as language prohibitions (versus offering a prisoner swap to a rebel group). Note that the government moves first, before the rebels, to further emphasize that the government must choose its actions first, with very little information about its prospective opponent. Specifically, when the government moves, it has only its priors about the opposition type (inactive, weak or strong).

Then, the rebel group decides to mobilize (M) or not (NM). Mobilization in this instance is taken to be violent mobilization akin to LIV. At this point, the government

updates its priors about the rebel group's type, based off of two factors: 1) did the group mobilize at all? 2) did the group mobilize after repression or after accommodation? The intuition here is that the government updates its priors not only based off of (in)action, but also based off of the context; a group that mobilizes in spite of a government crackdown is likely to be particularly resolved. Lacina (2014) shows that during India's federal reorganization in the 1950's, weak linguistic groups did not try to mobilize for statehood because they feared repression.

If the group does mobilize, it dies with probability p_d . This move by Nature captures the empirical reality that many groups start to mobilize but never make it to LIV; in a study of all rebel groups that formed in Uganda after 1986, Lewis (2017) finds that over half of all groups failed before committing enough violence to reach the 25 battle death threshold. If the group survives, the state government updates its priors a second time. Then, in the second stage of the model, the government decides whether to fight a war (W) or negotiate (N) with the rebel group. If faced with war, the rebel group can choose to surrender or fight; if faced with negotiations, the group can accept or reject and fight. A decision to fight in either case means that the state wins with probability p_{state} . Figure 1 provides a game tree for the model.¹⁵

¹⁵Note that this figure captures the game with one type; for size constraints, I do not depict the full game, which includes the diagram in Figure 1, replicated two additional times, and an initial move by Nature to determine the opposition's type.

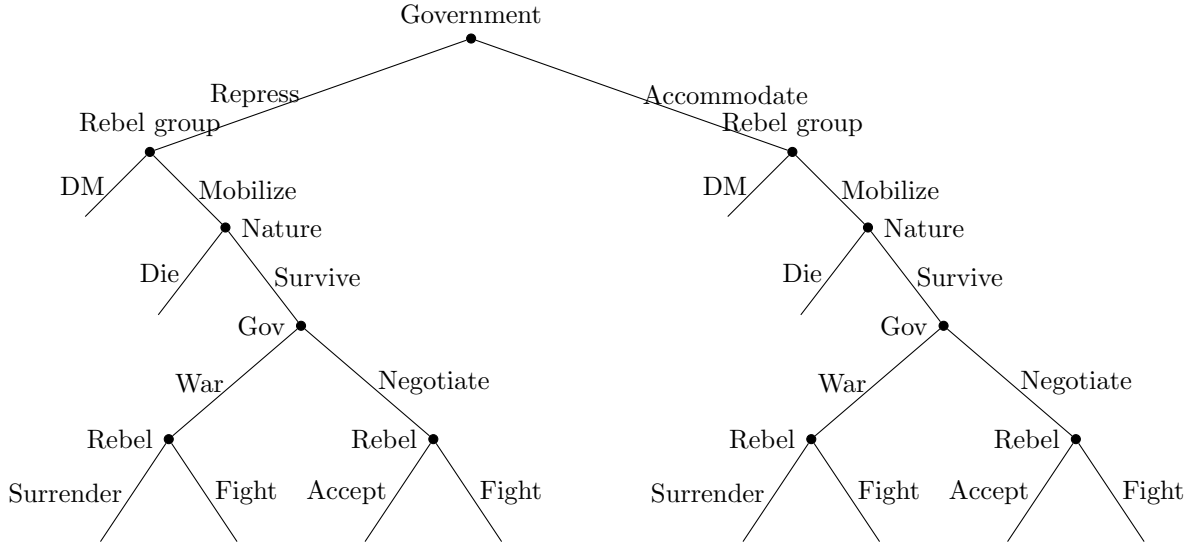


Figure 2.1: The Game

The value to each side for deciding policy (or who controls the government) is set at 1. This means that the government values the status quo at 1; if a rebel group decides to fight and wins, its maximum payoff is 1. The value of the good that the government offers is γ ; I assume that an initial accommodation (if made) is portion, κ , of γ . This means that if the government chooses to accommodate, and the group does not mobilize, the government receives $1 - \kappa\gamma$, while the group receives $\kappa\gamma$.

There are several other parameters for the model. Group type is represented by $\theta \in \{inactive, weak, strong\}$. State capacity, α , is positive and increasing. The government faces two costs: $c_R(\theta, \alpha)$, its cost of repression, and $c_w(\theta, \alpha)$, its cost of going to war with the rebel group. Both are functions of θ and α , such that costs decrease with increasing α but rise as θ shifts from inactive to weak to strong.

Table 2.2: Summary of Parameters

Symbol	Meaning	Components	Values
SQ	value of govt control	–	1
γ	good offered by govt	–	$0 \leq \gamma \leq 1$
κ	portion of γ	–	$0 \leq \kappa \leq 1$
θ	group type	–	IA, weak, strong
α	state capacity	–	positive, increasing
c_R	govt repression cost	θ, α	$c_R \geq 0$
c_w	govt war cost	θ, α	$c_w \geq 0$
c_{GR}	group repression cost	α	$c_{GR} \geq 0$
c_m	group mobilization cost	$\theta, \alpha, \mathbb{1}\{Repression\}$	$c_{m,NV} \geq 1 \geq c_{m,L} \geq c_{m,H} \geq 0$
c_f	group fighting cost	θ	$c_f \geq 0$

Intuitively, this means that it is easier for stronger states to fight a rebel group, *ceteris paribus*. Rebels face up to three costs: c_{GR} , the difficulties the group faces when the government represses, c_m , the cost of mobilization, and c_f , the cost of fighting. The mobilization cost is a function of group type (IA > weak > strong), state capacity (increases as α increases), and whether or not the government represses or accommodates (higher for the former than the latter). The group must pay a cost for fighting that is separate from mobilization because additional resources are required for sustained conflict.

Table 1 provides a summary of the parameters and their meaning.

2.2.2 Equilibrium and key takeaways

I reserve any equilibrium proofs for the appendix. There are many potential equilibria, so this section focuses on the most important. In this equilibrium, the groups separate by type, so that the group only mobilizes after repression when $\theta = \text{high}$. The

government engages in costly repression in order to get a clearer signal of group type. In the second stage of the game, after mobilization, there is no war, because the government is confident it faces a strong group and does not want to pay the higher costs of fighting against a stronger opponent. Rather, it negotiates with the group, and the group accepts. There are three key takeaways.

Group Type

First, this equilibrium means that, of the universe of potential opposition groups, only a very specific subset will progress to LIV: those that are most capable and/or resolved. These are the groups that are not ideologically opposed to violence, are not deterred by the threat of state violence, and are not eliminated by state repression if it occurs. Indeed, the expectation or threat of violence from the state can lead many groups to choose nonviolence or no action at all. This finding from the model resonates with existing research. Ritter and Conrad (2016) show that in both the US and across the world, dissidents that anticipate government repression may opt for “strategic self-censoring” and refrain from any action. We can also see this dynamic during India’s federal reorganization in the early 1950’s, when any ethnic group had the ability to petition for statehood. The strongest groups were completely accommodated by the central government, and the weakest groups – fearing state violence – never bothered to even petition; it was the groups in between that were more likely to petition, be denied, and then commit violence (Lacina, 2014).

Because only the stronger, more resolved groups are likely to make it to LIV, this

starts to shift the relative cost of bargaining versus violence for the state. While a state is still uncertain about whether it faces a strong rebel group, it might prefer to use violent tactics (repression, killing, etc.) rather than bargaining to eliminate or deter a rebel group. However, if the state believes it faces a stronger group, it can anticipate that using violence against the rebel group will become more costly, because it would be against a more capable challenger that can inflict more damage. This makes bargaining more attractive to the state, *ceteris paribus*. In other words, if a group can successfully perpetrate LIV, the state is able to update its information about the group's capacity and resolve. Whereas prior to LIV, a state might be uncertain about the type of group it faces, the violence provides critical information to the state. Then, when a state believes that it faces a strong, resolved threat, it is more likely to want to bargain with that group in order to avoid escalation to a costlier conflict. Thus, we should expect that the strongest groups are more likely to reach LIV but, counterintuitively, less likely to escalate to war.

For example, in Sudan, when Jaafar Nimeiri seized power in 1969 after Sudan spent its first decade of post-colonial independence in turmoil, he made no initial moves to resolve the Southern resistance movement. However, after a failed coup attempt in 1971, Nimeiri realized that his regime was in fact at risk, and he not only promised a national constitution and assembly but also tried to resolve the Southern rebellion. Ultimately, the Addis Ababa peace pact was signed in 1972.

State capacity and the information environment

The second takeaway relates to the state. Because c_m is in part a function of α , group costs rise as government's capacity increases: mobilization is less likely against stronger states. Stronger states are therefore less likely to experience LIV, largely because they deter challengers. However, if a challenger does emerge to commit LIV, as capacity increases, the government will be less willing to negotiate with rebels and more willing to go to war, precisely because it is stronger. The greater strength means that the government should expect a higher probability of winning vis-a-vis the rebel group and therefore be more willing to fight. This leads to a counterintuitive finding that contrasts strongly with the conventional wisdom: stronger states may be less likely to face LIV, but if they do, they are in fact more likely to escalate to war.

The case of Syria illustrates this intuition nicely. The Assad government (first Hafez al-Assad and then Bashar al-Assad) maintained extreme control over Syria for decades via a particularly repressive regime that discouraged most protesters. Prior to the Arab Spring, the last major civil conflict in Syria was an Islamist uprising from 1976-1982 led primarily by the Muslim Brotherhood. It culminated in 1982: when the Muslim Brotherhood captured control of Hama, Hafez al-Assad's military bombed the city for month, defeating the insurgency and resulting in thousands of civilian deaths. For the next thirty years, both Assad regimes successfully deterred any insurgents. Thus, when the Arab Spring movement swept the region, toppling the Libyan dictatorship and encouraging the Egyptian military to oust Mubarak and

attempt democracy, most analysts believed Syria would be ‘safe’ from the unrest, because opposition movements had been discouraged for so long. Bashar al-Assad responded to Syrian protests with extreme force. He refused to negotiate and in fact responded to initial protests with large massacres. As the nascent rebellion intensified, more rebel groups emerged (including the notable Free Syrian Army, formed of former Syrian military officials), and war swept the country.¹⁶

For the third takeaway, the information environment can explain temporal variation in substate conflict. As a group starts committing violence, the government gains significant information. It learns that there is definitely an active, viable rebel group. (Many groups start to mobilize but never make it to LIV; in a study of all rebel groups that formed in Uganda after 1986, Lewis (2017) finds that over half of all groups failed before committing enough violence to reach the 25 battle death threshold.) The state no longer has to spend resources trying to figure out who it might need to bargain with. In the real world, this means that the government can now be more precise in its surveillance and information gathering, which is much less costly. Likewise, it can negotiate with a particular group, rather than making broad accommodations to an aggrieved population. Taken together, this shifts the cost of violence versus negotiation/accommodation from one stage to the next: negotiation usually becomes cheaper and therefore more likely as the conflict advances.

¹⁶Syria is an extremely complex example, and I do not mean that state capacity was the only cause.

State capacity and group strength: expected types of conflict

If we look at state capacity and group strength together, what types of outcomes are we likely to see? As the takeaways above imply, war is most likely when a strong rebel group is willing to challenge a high capacity state. War is much less likely for stronger states, because they deter so many challengers from mobilizing at all, but it is precisely in the situations where a strong challenger emerges against a high capacity state that the conflict is likely to escalate, with neither side backing down. The Syrian civil war provides one example, as does the civil war in El Salvador (1979-1992). In El Salvador, state power was solidly consolidated by a military-political elite alliance, where 300 ruling families controlled an overwhelming majority of the state's wealth. As food prices and discontent increased through the 1970s, the state relied heavily on repression, particularly after General Carlos Humberto Romero won the presidency in 1977.¹⁷ The state's capacity was also strongly bolstered by U.S. military and intelligence support. The FMLN (Farabundo Marti National Liberation Front) emerged in 1980 by unifying small existing groups, workers, and socialists. Scholars have characterized the FMLN as particularly unified and cohesive (Wood, 2000, 2009), and it was not deterred by the government repression. Rather, the brutal murder in 1980 of Archbishop Oscar Romero proved to be a catalyst, with the FMLN launching its first major attack shortly thereafter (Wood, 2000). The Salvadoran government did not try to negotiate but rather increased its campaign of violence and repression, partic-

¹⁷Earlier in the decade, then-President Molina pushed for land reform but failed because the ruling elite were so opposed (Wood, 2000).

ularly through the paramilitary group ORDEN (National Democratic Organization). The conflict escalated quickly, with over 11,000 dead in the first year; the civil war was not resolved until 1992.

What about other combinations of state capacity and group strength? By the logic outlined above, I would expect weak groups that emerge in low capacity states to experience protracted LIV. In this type of case, the low capacity state deters far fewer groups, making the barrier to entry much lower, but the weak group will still have difficulty getting the state to negotiate, as it cannot inflict too much damage. If a strong group emerges in the same type of context, I would expect it to attain either de facto autonomy (e.g. the Kurds), as the state would be unable to counter the group’s military and territorial claims, or bargaining and concessions. Conversely, in the case of a high capacity state and (prospective) weak group, I would expect the strategic absence of mobilization—as was the case in India’s federal reorganization—or that the group would be eliminated early.

Figure 2.3 provides a summary of my expectations of how group strength and state capacity interact.

Table 2.3: State Capacity and Group Strength

	Low Capacity	High Capacity
Weak group	Protracted LIV (Sahrawi)	Strategic absence of mobilization (India) or group eliminated early
Strong group	De facto autonomy (Kurds), bargaining (Sudan)	War (El Salvador)

2.3 Conclusion

This chapter has presented a formal model that develops our understanding of how the strategic interaction between the state and a (prospective) opposition group influences not only the start of intrastate violence but also its escalation, or lack thereof. This builds on existing literature that examines how the state-opposition relationship might condition the emergence of violence; I show that the process does not end with the start of violence but rather continues throughout the broader intrastate conflict process. Moreover, by modeling the state and opposition together, I shift the focus from the rebel group back to the state. The model highlights how state capacity can affect escalation differently than initial violent mobilization, and this relationship is more nuanced – and at times in direct contrast to – conventional expectations about the causes of civil wars. Additionally, the state’s information environment is critical: states often cannot even identify if or who their opponent is initially. This informational asymmetry is different and more extreme than the interstate context, where one actor might not know the other’s goals or resolve or payoffs, but does at least know its identity. In the intrastate context, the low initial information environment means that initial negotiations are unlikely; in fact, the state has an incentive to repress, so that it can get a credible signal of the opposition group’s type. If a group mobilizes to violence, that provides key information to the state, which is thus able to update its beliefs about the opponent’s type. Negotiation therefore becomes more likely after the initial violence. Thus, the information environment explains not

only temporal variation but also cross-national variation, because variation in state surveillance capabilities can mitigate or exacerbate the informational asymmetry.

The model also suggests several avenues for future research. First, there is no ideal way to test the implications empirically. Standard logit or probit models cannot handle the selection issues or the strategic interaction (Clark and Reed, 2003; Clark and Regan, 2003; Fearon, 2002; Reed, 2000, 2002). This suggests room for a new empirical approach that more efficiently and effectively captures the multi-stage nature of the civil conflict process. Likewise, this highlights the need for better data on state surveillance infrastructure, in order to more cleanly test how the information environment influences state-opposition interaction. Lastly, I have abstracted away from the multiple pathways that can lead to an observation of no violence. One potentially productive line of research would be to integrate my approach with the work on strategic nonviolence (Chenoweth and Cunningham, 2013; Chenoweth and Lewis, 2013) to better understand the full range of strategic behavior throughout the civil conflict process.

Chapter 3

Rethinking Civil War Onset and Escalation

In the 20th and 21st centuries, civil war surpassed interstate war as the most common form of violent conflict: over half of all countries between 1946 and 2012 experienced a civil conflict (Gleditsch et al., 2002; Lacina, 2006). If we consider less organized forms of intrastate violence, that number increases (Lacina, 2014). Yet there is substantial variety in whether conflicts remain at a low-intensity or escalate. This is particularly true for the most pervasive type of conflict, self-determination (SD) disputes. Many SD disputes remain nonviolent: only 45 percent since 1955 have become violent (Cunningham, 2013). Of those that experience violence, only 25 percent escalate from low-intensity to war.¹ Even within countries, some self-determination disputes experience no violence, while others suffer long civil wars. For example, in Nigeria, the Yoruba movement has remained nonviolent, while the Biafran separatist movement developed into a brutal rebellion. What can explain this variation?

¹That statistic is based on analysis of data from Cunningham (2013) and the Uppsala Conflict Data Program (2009).

This chapter challenges existing approaches for understanding the start of civil conflict. It situates the theoretical foundation developed in Chapter 2 in the context of existing empirical research on the start and causes of civil conflict. I unpack the issues with failing to distinguish between low intensity violence (LIV) and war, demonstrate the implications for existing research, and draw on the quantitative interstate conflict literature to provide an alternative statistical approach. I argue that low-intensity conflict and civil war are two qualitatively different stages of the same broader conflict process, so we must analyze them as such, both theoretically and empirically.

Failing to distinguish between the start of low-intensity civil violence and full-blown war leads to both theoretical and empirical problems. Theoretically, they are two different phenomena: each has a different information environment, has different types of opposition groups, requires different levels of mobilization and organization, displays different levels of violence, and presents different opportunities for bargaining failure. Conflating LIV and war leads to underspecified theories. Methodologically, doing so produces biased, misleading results. Analyzing war without accounting for LIV introduces selection bias—states do not randomly enter into war—and can change coefficient signs and significance and inflate error terms (Reed, 2000, 2002). In other words, what we expect to increase the probability of war might actually decrease it. Yet this is precisely what many foundational civil war studies overlook: they analyze when full-blown civil wars emerge, without first controlling for the start of violence, and thus they require reassessing. Studying the start of LIV without addressing the

looming prospect of war is equally problematic. The potential for escalation should affect a strategic actor's earlier behavior; that is missed if LIV is examined in isolation.

To mitigate these concerns, I test the dissertation's argument about the relationship between the two stages, highlighting three key mechanisms that distinguish and drive LIV and war: the information environment, the type of opposition group that is operating, and the state's capacity.² Recall that I argue that the state faces an extraordinarily poor information environment before LIV, such that the state usually cannot even identify the specific opposition group at risk of mobilization (though it might be aware that such a group exists). For intrastate violence: the state gains information about group identity (existence) and type through conflict, so that after LIV is reached, the information environment shifts and improves, making bargaining and negotiations more likely. For the second mechanism, group type, I have argued that mobilization to LIV (and then war) is part of a strategic interaction between the state and opposition group, where the expectation of violence can affect behavior. While both "strong" and "weak" groups might exist in early conflict stages, weak groups will get weeded out, either by self-censoring or by state violence. This means that a state facing a group that has successfully committed LIV and is at risk of further escalation is more likely to be facing a strong, resolved opposition group. Lastly, I have shown that the third mechanism, state capacity,³ has a nuanced relationship not captured in conventional approaches. Strong states are indeed less likely

²These are not the only possible mechanisms.

³I mean capacity broadly, both in terms of military abilities but also in terms of scope, i.e. the state's ability to project its presence throughout the country.

to face initial violence: more (prospective) challengers recognize the longer odds of winning and strategically opt out. However, the relationship between state capacity and violence is not monotonic; there is a selection effect at play. If a stronger state is challenged, it is more confident in its chance of victory, making it less willing to negotiate and therefore more willing to escalate to war.

To test my arguments, I focus on self-determination movements, using the data from Cunningham (2013) to examine LIV (defined as 25 or more battle deaths in a year) and war (1,000 or more battle deaths in a year) between state-movement dyads from 1960-2005.⁴ I choose self-determination disputes because they are the most common type of war (Cunningham, 2013), and they provide a clean research design. Because there is a clear set of cases where no violence occurs, another where only low-level violence occurs, and a third where dyads experience escalation to war, I can first compare between dyads with no conflict and those with LIV, and then subsequently between dyads with LIV and those with war. I take this approach because it is particularly imperative to control for LIV when examining war, as discussed above.

The results support my argument and have strong implications for existing research, which I explore further by reexamining seminal findings about the causes of civil war. This additional analysis shows that relationships are more nuanced than previously shown. For example, the canonical wisdom is that GDP per capita is

⁴This is consistent with existing literature and UCDP coding conventions, where onset is defined as when a conflict dyad first experiences 25 or more battle deaths in a year, and escalation to war occurs as when the dyad reaches the traditional level of war, or at least 1,000 battle deaths in a given year (Uppsala Conflict Data Program, 2009; Bueno de Mesquita et al., 1997; Clark and Reed, 2003; Hart Jr and Reed, 1999).

negatively correlated with civil war: strong states are at lower risk. However, I show that while high GDP decreases risk for LIV, **if** a rich/strong country does experience LIV, then it is actually more likely to escalate to war.

This chapter therefore makes several key contributions. First, I draw on quantitative international relations work to demonstrate that existing empirical approaches that test for civil war onset but fail to control for LIV are inherently biased, and many classic findings are therefore mis- or under-specified. Second, I develop the empirical implications and testable hypotheses that follow from Chapter 2's theoretical framework for understanding the interplay between LIV and war in intrastate conflict. This framework lays out fundamental substantive differences between these two stages, filling in critical theoretical gaps in the literature. Third, I test the model's implications and account for both LIV and war using an approach suggested by analysts of interstate crises: the Heckman selection model. I compare the findings from the selection model to those from a more standard probit or logit model to demonstrate the benefits of a more nuanced statistical approach. This also forces a fundamental re-examination of many of the seminal findings in the research on the causes of civil war.

3.1 Methodological Concerns in the Study of the Causes of Civil War

The research on civil war causes has deepened our understanding of intrastate conflict. However, it treats the onset of civil war in a vacuum, failing to account for the initial

violence and strategic state-rebel interaction that leads to war. Additionally, while it can explain cross-national variation, it cannot account for within country differences, or explain why specific conflicts emerge at specific points in time. Micro-level studies of civil war attempted to understand why specific individuals joined insurgencies or rebellions, and the repression-dissent literature underscores some of the complexities in trying to understand what drives civil conflict. For example, some scholars argued that people join because of grievances created by state repression (Gurr and Moore, 1997; Lichbach, 1994; Lindstrom and Moore, 1995), although recent research has shown that the relationship between repression and mobilization is strategic and not as straightforward (Ritter and Conrad, 2016). Others assert that a person's decision to rebel comes from a rational, strategic cost-benefit calculus (Lichbach, 1994; Popkin, 1979; Verwimp, 2005). One study suggested that participating in a rebel group might be safer than being a civilian (Kalyvas and Kocher, 2007). Lastly, some posit that individuals join because of "a more general susceptibility to engage in violent action or a greater vulnerability to political manipulation by elites" (Humphreys and Weinstein, 2008).

Overlooking LIV when analyzing war – or examining either in isolation – leads to conceptual and methodological shortcomings. Civil wars are a process (Sambanis, 2004b,a), and a lot of violence must occur even before a conflict-dyad reaches that threshold. Indeed, many intrastate conflicts languish at low-intensity without ever escalating: between 1993-2004, only 22 out of 122 minor conflicts escalated (Melander et al., 2009). Using the 1,000 battle death cut-off overlooks this violence. It

also creates methodological problems. For example, Anderson and Worsnop (2016) argue that lumping what they call ‘major’ and ‘minor’ conflict together “can lead to indeterminate or divergent findings when causal mechanisms vary across cases” (2). Anderson and Worsnop (2016) survey the civil war literature, finding that: under 25 percent of published articles check whether their results are consistent at both thresholds, and of those that do, almost half acknowledge that the results varied across thresholds. They label this problem as partly one of ‘causal heterogeneity,’ where mechanisms have different effects at different stages.

Beyond causal heterogeneity, overlooking LIV when analyzing war introduces a selection bias, because LIV must be accounted for when checking for war (Clark and Regan, 2003; Clark and Reed, 2003; Fearon, 2002; Reed, 2000). This is because some variables might have one effect on LIV but another on escalation once onset has happened. As a result, the substantive and statistical significance of variables might change – up to and including a change in the sign of the coefficient – and standard errors might be incorrect. Put more simply, what we expect to increase the probability of war might actually decrease it, and we might be underestimating the uncertainty. Where does this selection effect come from? It could emerge if some of the factors that affect onset also affect escalation (in the same or different ways). For example, we know that in interstate conflicts, resolve influences both onset and escalation; looking only at escalation thus underestimates how important resolve is to reaching escalation (Reed, 2000). In a more extreme case, if we only look at escalation, we might overlook variables that truly do matter: the selection effect could drive the

coefficients of important variables to zero. This could occur if a variable matters for both stages, but other unobservable factors determine selection into onset when that variable would have predicted no conflict. Another possibility is that a variable matters for onset but not escalation, and so looking only at escalation mis-attributes that variable's importance.

This strategic approach is often missing in the literature on intrastate conflict⁵, although research on interstate wars underscores its importance for both onset and escalation (Achen and Snidal, 1989; Banks, 1990; Clark and Reed, 2003; Bueno de Mesquita et al., 1997; Fearon, 1994, 1995; Huth and Russett, 1984; Huth, 1988; Huth et al., 1993; Huth, 1997; Lebow and Stein, 1990; Morrow, 1989; Senese, 1997).

Additionally, as Reed (2000) pointed out, selection effects were a serious problem for statistical analyses of crises. It is not enough to look only at cases of onset and then assess whether those cases escalate. One must have a research design that compares cases of no violence to cases of onset, and then examines escalation conditional on onset. In the intrastate conflict case, the parallel is to compare cases of no violence to cases of LIV, and then examine war conditional on LIV. Otherwise, results will suffer from selection bias.

Using these new approaches, scholars showed that democracy and satisfaction with the status quo decrease escalation, but joint democracy and joint satisfaction affect only onset, not escalation (Hart Jr and Reed, 1999; Reed and Clark, 2000). Huth and Allee (2002b) delved more deeply into the relationship between regime type and

⁵For notable exceptions, see Cunningham (2013); Walter (2009).

onset and escalation, finding that democracies are less likely to initiate but more likely to concede, and challenges and concessions are both more likely after recent elections. Lastly, power parity and economic development nonmonotonically increase conflict (Reed, 2000), while observable relative strength nonmonotonically raises the probability of escalation (Bueno de Mesquita et al., 1997).

3.2 Key Mechanisms Driving LIV and War

In this section, I provide a brief review of the argument developed in Chapter 2. I have argued that LIV and war are qualitatively distinct stages of a civil conflict—differentiable based on their scope of violence, level of mobilization, and amount of information available—but belong in the same analytic framework, as they both influence each other. This means that the same causal mechanisms might matter at each stage, or what is important for LIV might have no effect on war (or vice versa). The effects could also be opposing: what makes LIV more likely (unlikely) also decreases (increases) the probability of war. I focus on three mechanisms: the information environment, the state’s capacity, and the type of rebel group in operation.

First, I have argued that the information environment at the beginning of an intrastate conflict is asymmetrical. The state faces an extreme informational disadvantage. Rebel groups often prefer to operate initially in secrecy (Lewis, 2017), and the government – at least initially – does not know what the group wants, how strong and/or resolved it is, or even if a group has formed. While the state might suspect a group is forming, at the very early stages, it typically cannot identify a specific

group that it faces. To be able to do so would require extreme informational costs, including but not limited to a strong surveillance infrastructure; even for states that are willing to invest in that type of apparatus, potential threats require that police and intelligence forces sift through an extraordinary amount of information with little guide or direction, which quickly expends money, time, and personnel. This means that violence, especially early in the conflict, provides crucial information. It enables the government to update its beliefs about the type of rebel group it faces. It learns that there is definitely an active, viable rebel group. Negotiation usually become cheaper and therefore more likely.

Second, the model indicates that the relationship between state capacity and civil conflict depends on the stage. This is in contrast to existing research, which argues that state capacity makes conflict less likely, whether LIV or war. Rather, I show that greater state capacity does deter more challengers, making LIV less likely. However, if a group does emerge to challenge a strong state, the government will be less willing to negotiate and therefore more likely to go to war. Conditional on LIV, stronger states are more likely to escalate to war.

Third, rebel group type has the opposite effect. If we consider the universe of all prospective rebel groups, only some subset will inflict violence; many groups, particularly weaker groups, will self-censor and refrain from any action. This means that the groups most likely to progress to LIV are the most capable/resolved and undeterred (or eliminated) by the threat posed by the state. The state, upon seeing a group commit LIV, can update its priors; realizing that it is facing a stronger group,

the state prefers to negotiate with the group and avoid war. This suggests that the strongest groups are more likely to reach LIV but, conditional on LIV, are less likely to escalate to war.

There is an important point to highlight here for all of these mechanisms: I am talking about both conditional and unconditional probabilities. That is, when considering how these mechanisms influence the chance of LIV, I am looking at unconditional probabilities, which are straightforward. However, my discussion of the mechanisms' relationship with war is about *conditional* probabilities. I am talking about how these mechanisms – state capacity, the information environment, and rebel group type – affect the probability of war, *assuming a state and rebel group are already experiencing LIV*.

3.3 Empirical Implications: Hypotheses and Research Design

What are the empirical implications and testable hypotheses that this argument yields? Before identifying specific hypotheses, it is worth noting that, while my argument speaks to intrastate conflict in general without discriminating on type of conflict, it requires a unique research design with regards to the universe of cases. That is, I need a set of cases where there are clean examples of violence but also clean cases of no violence, which precludes a typical country-year or country-rebel-year research design. I therefore focus specifically on self-determination groups, drawing from the data in Cunningham (2013), which examines any opposition movement at-

tempting to achieve national self-determination between 1960 and 2005. There is no restriction on whether these groups use violent or non-violent means to achieve their goals, so examining all self-determination groups worldwide allows me to first identify cases where no violence occurs at all and compare those to cases of LIV. Then, I can compare cases where violence remains low-level to cases where it escalates to war.⁶

Proceeding to testable hypotheses that can be derived from the argument, the first and most straightforward is state capacity, which is typically proxied in the literature by GDP per capita. I am consistent with this approach, yielding the following hypothesis:

State capacity hypothesis: As GDP per capita (state capacity) increases, states are less likely to experience LIV. However, if a state with high GDP per capita (strong state) does face LIV, it is more likely to escalate to war.

Identifying a testable hypothesis is less straightforward for the mechanisms about rebel group type and the information environment. We cannot directly identify the information environment or the group's type, so we must search for observable implications.⁷ I argue that one key factor that is related to both the information and group type mechanisms is whether or not the rebel group has a territorial base. From

⁶Additionally, self-determination disputes have become the most common type of civil war: self-determination movements exist across the globe and have led to intrastate conflict in Europe, Asia, Africa, and the Middle East (Cunningham, 2013). Better understanding the evolution of these disputes therefore represents an important step in our understanding of civil conflicts.

⁷The options for capturing these concepts are limited by the case set: I analyze self-determination movements, not rebel groups, because I need to be able to include observations where no violence occurs. However, the data on self-determination movements is more limited.

the opposition's perspective, a territorial base can improve capability in two ways. First, it can help solve coordination problems associated with collective action (Collier and Hoeffler, 2000; Herbst, 2000; Lichbach, 1995; McColl, 1969; Raleigh and Hegre, 2009). Lichbach (1995) posits that a territorial or regional base is often linked to an increase in collective dissent, because it helps opposition groups communicate better, coordinate mutual expectations, and reduce organizational costs.⁸ A territorial base is particularly helpful as it is often correlated with a concentrated, supporting population (Raleigh and Hegre, 2009; Collier and Hoeffler, 2000). In addition to helping mobilize action, it can provide resources to the opposition – e.g. shelter from government forces, money from natural resources (drugs, gems, etc.) – depending on the type of terrain involved. On several levels, then, territorial strongholds might help opposition groups prepare for violence, making them a useful proxy for rebel group type.⁹

Territorial strongholds also relate to the information mechanism: the base provides a wealth of information to the state. While a base in and of itself does not indicate that a group is about to rebel, if a group with a base does reach LIV, that can affect the state's posterior about group type. The base also can help the state identify what the group's grievances might be, so that the state can decide if it prefers to bargain

⁸Lichbach (1995) also argues that a territorial base can make it harder for rebels to spread through the rest of the country, but that is less of a concern for self-determination groups, who typically do not want to take over the entire country.

⁹Some readers might wonder if the stronghold captures group motive (grievance) rather than ability. Since all groups in my universe of cases have the same objective – self-determination – this is unlikely. To be sure, I include a control for grievance in my empirical models. See the subsequent sections for more details.

(or not).

Lastly, it is important to note what cannot be used as an observable implication: variables like troop size that are only characteristics for militarized groups. This is because any factor for group type must be available not only for strong and weak groups but also for those that do not (violently) mobilize. Territorial bases are both theoretically connected to the mechanisms and also plausibly observable for all group types. Taken together, this line of reasoning yields the following hypothesis about LIV and war:

Group type and information environment hypothesis: An opposition group that has a territorial base will have an increased probability of experiencing intrastate LIV, but if LIV occurs, the group will have a decreased probability of war.

3.3.1 The empirical model

Given the selection bias concerns raised earlier, what is the best approach for modeling causes of sub-state conflict? Studies demonstrated that both censored probit models and 2-stage Heckman selection models could be used for interstate conflict onset and escalation (Clark and Reed, 2003; Hart Jr and Reed, 1999; Reed, 2000) and stressed that onset and escalation need to be modeled together, not separately. It is worth emphasizing here how these findings changed from earlier results about interstate conflict: not only did standard errors for variables change, but often so did the fundamental direction of the relationship between a variable and the start of conflict. It is essential that we apply similar theoretical logic and statistical methodology on

the intrastate side to ensure that existing inferences are correct. I therefore follow the international relations literature and use a 2-stage Heckman selection model in order to properly control for LIV when testing for war. As a robustness check, I also use a multinomial logit model to test my hypotheses. The results are consistent.

3.3.2 Data and Main Variables

The set of cases is all SD movements worldwide, 1960-2005, and since I focus on rebel-state strategic interaction, the unit of analysis is the government-SD movement dyad-year, with 3898 observations. Cunningham (2013) developed the universe of cases in two steps. First, she identified SD movements worldwide between 1960 and 2005, using the CIDCM Peace and Conflict Report (Marshall and Gurr, 2003); that yielded 146 movements. Then, she focused only on SD organizations that were actively making self-governance related demands against a government in a given year. To be included, groups do not have to use violence; most did not. Only 20 percent of the observations reached the level of 25 or more battle deaths in a year. Of those, only 25 percent escalated to full-blown civil war. Groups range from the peaceful Flemish in Belgium to the extremely violent Tamils in Sri Lanka. Indeed, there are cases from every continent, from strong and weak states, and even multiple cases within a single country.

The two dependent variables are LIV for the Heckman selection model's first stage and civil war for the second stage. These correspond to the two outcomes of the two stages of the formal model. LIV represents the group's initial decision to

(violently) mobilize; war corresponds to the escalation outcomes in the second stage of the formal model. Both dependent variables come from the Uppsala Conflict Data Project (UCDP)/Peace Research Institute Oslo (PRIO) (Gleditsch et al., 2002). LIV occurs if there are at least 25 battle deaths in a dyad-year, and war occurs if the dyad experiences at least 1,000 battle deaths in a given year. Table 1 provides a brief overview of which SD movements experienced no violence, experienced LIV, and/or escalated to war.

Table 3.1: Observations by Outcome

	No violence	LIV	War
Total Obs.	3165	743	189
Asia	875	379	73
Eastern Europe	469	35	14
Western Europe	885	32	1
Sub-Saharan Africa	558	168	55
Middle East and North Africa	150	125	43
Latin America	216	0	0

To test the hypothesis about territorial bases, I use an independent variable drawn from Cunningham (2013), which has a dummy variable for whether a movement has a territorial base in a given year or not, drawn from the Minorities at Risk (MAR) dataset (MAR, 2009).¹⁰ For the state capacity hypothesis, the independent variable is GDP per capita from the Gleditsch Expanded Trade and GDP dataset (Gleditsch, 2002).

¹⁰As an alternate measure of group capacity, I use relative size; the results are generally consistent.

3.3.3 Controls

I use two broad categories of controls: state and group controls. For the state-level controls, I include most of the traditionally used controls in the civil war literature.¹¹ Specifically, I include a log of the population (from the Gleditsch Expanded Trade and GDP dataset); a state's level of democracy (from the Polity IV project (Marshall et al., 2016)); a dummy for the Cold War period; a log of mountainous terrain; and a dummy for whether the state is an oil exporter or not.¹² All of these appear in both the LIV and war equations.

For group-level controls, I am somewhat more restricted in my selection, because I am examining SD movements, not SD rebel groups.¹³ (Of course, some of these become rebel groups, but not all do.) I nevertheless include four group-level controls in either the LIV or war equations (or both) that could affect whether a group has a territorial base and whether it experiences LIV and/or war. To account for the possibility that LIV and/or war is related to the group's degree of grievance, I control for the level of economic discrimination against the group. I include this variable also to address the possibility that the territorial base variable is capturing grievance, not group strength or capability. I also check for whether or not a movement has kin in a neighboring country, because neighboring kin might provide sanctuaries that

¹¹This is key when I reproduce and compare existing civil war research to my approach, because including the same controls as previous studies lets me confirm that new findings are a result of my approach and not an artifact of my different case set.

¹²These other controls are all drawn from Cunningham (2013).

¹³Most notably, it would be extremely useful to have data on what the outcome of the movement is, i.e. whether or not the government offers concessions. However, this information is not available for SD movements.

substitute for territorial bases. Kin might also affect whether or not a state responds with violence (Jenne, 2006; Koinova, 2008). I control for the group's relative size with respect to the state (in terms of population), as this could affect both whether the group has a base and also whether it resorts to violence.¹⁴

The fourth and final control is also the exclusion restriction, so it appears only in the LIV equation: the movement's factionalization. I use this for theoretical and empirical reasons. Theoretically, studies have shown that more divided movements are more likely to experience civil conflict onset (defined as the 25 battle deaths per year threshold) (Cunningham, 2013, 2014). Particularly divided movements might also be less likely to have their own base. The logic behind the relationship does not extend to war. According to Cunningham (2013), divided movements are more likely to experience LIV because the government will not be sure about who to bargain with, and this is certainly true pre-LIV, when multiple factions within the movement have the potential to use violence. However, once LIV has occurred, the question of whom to bargain with becomes much clearer, thereby dampening the effect that factionalization might have on war. To be sure that this theoretical relationship is also true empirically, I ran a robustness check including the factionalization variable in both stages of the Heckman model. Results did not change, and the factions variable was close to zero and statistically insignificant.

¹⁴These are also in the Cunningham (2013) dataset. The factions variable is unique to the data, whereas the other three controls come from the MAR dataset.

3.4 Statistical Results

In this section, I present the results from the empirical analysis. I first address the findings directly related to my own theory before reviewing the implications for existing findings.

3.4.1 Territorial base findings

I start with two Heckman selection models: a more parsimonious model (Model 1) that includes only the group-level controls but not the state-level variables, and a full model (Model 2) with all of the controls discussed above. I include these two different models to have a more parsimonious approach as well as a more inclusive, traditional model that is consistent with the broader civil conflict literature. Each Heckman selection model has two stages and equations, so I present results from each stage separately. The first stage results from Model 1 (group controls only) and Model 2 (full model) appear in Table 3.2; the DV here is LIV. Table 3.3 provides results from the second stage of the same Heckman selection models, which focus on escalation to war (conditional on LIV occurring).

Several points merit attention here. First and most importantly, the territorial base variable is consistently positive and quite large for the LIV equation.¹⁵ This indicates that, consistent with my hypothesis, state-SD dyads where the SD movement has a territorial base are more likely to experience LIV. However, when we move to the

¹⁵The variable is statistically significant in the first model ($p < .01$) and just barely misses the threshold in the second model.

war equation of the model, the territorial base variable changes sign: it is consistently negative and again quite large.¹⁶ Once again, this is consistent with my hypothesis: among state-SD dyads that do experience low-level conflict, when an SD movement has a territorial base, it is actually less likely to escalate to war.¹⁷

Second, turning to the state capacity hypothesis about GDP per capita, the findings in Model 2 in Table 3.2 and Table 3.3 show that stronger states are less likely to experience onset. This is consistent with my argument and the literature more broadly.¹⁸ However, when we turn to the second stage of Model 2, in Table 3.3, which examines factors driving escalation to war (controlling for LIV), GDP's effect reverses completely. There is a positive relationship between GDP and escalation, which is consistent with the idea that stronger states might also be less willing to negotiate, if they do face onset.¹⁹ These findings are broadly consistent with the theoretical model. However, they represent a significant departure from the conventional wisdom about civil war onset. The next section discusses these implications in more detail.

Third, the performance of the group-level controls is also quite interesting. With

¹⁶The territorial base variable is highly statistically significant in the second model and just barely misses the threshold in the first.

¹⁷In alternate models, I also control for temporal dependence between conflicts by using a cubic spline of the number of years since conflict. This appears in the LIV equation only (not war). The war results are consistent with those presented in Table 3, but the territorial base variable in the LIV equation approaches zero (and is slightly negative). It is possible that, if we control for past conflict, territorial bases have no (or a slightly negative) effect on LIV, particularly if they did help obtain concessions in the past conflict.

¹⁸However, I cannot confirm if this is because they deter challengers.

¹⁹Note that in the second stage of Model 2, the GDP per capita variable is positive but not statistically significant. In other models in subsequent sections, it is statistically significant at both the $p < .05$ and $p < .01$ thresholds, but what matters most here is the sign, which is consistently in the opposite direction of conventional expectations.

Table 3.2: Heckman Selection Models, Stage 1: LIV

	<i>Dependent variable:</i>	
	LIV	
	(1)	(2)
Terr. base	0.302*** (0.094)	0.214* (0.114)
GDP		-0.225*** (0.028)
Rel. size	0.158*** (0.020)	0.111*** (0.025)
Econ. discrim.	0.221*** (0.020)	0.150*** (0.023)
Kin	0.166*** (0.055)	-0.002 (0.065)
Polity2		-0.016*** (0.005)
Oil exporter		0.190** (0.079)
Mountainous		0.027 (0.029)
Cold War		0.313*** (0.063)
Population		-0.087*** (0.022)
Factions	0.081*** (0.009)	0.179*** (0.012)
Constant	-3.020*** (0.180)	-0.326 (0.372)
Observations	3,311	3,067
ρ	0.210 (0.207)	0.039 (0.103)
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01	

Table 3.3: Heckman Selection Models, Stage 2: War

	<i>Dependent variable:</i>	
	War	
	(1)	(2)
Terr. base	-0.131* (0.070)	-0.204*** (0.073)
GDP		0.011 (0.018)
Rel. size	-0.032 (0.022)	-0.008 (0.017)
Econ. discrim.	-0.0005 (0.022)	-0.018 (0.016)
Kin	0.048 (0.038)	-0.005 (0.043)
Polity2		-0.005* (0.003)
Oil exporter		-0.081* (0.049)
Mountainous		-0.050*** (0.018)
Cold War		0.063* (0.036)
Population		-0.029** (0.014)
Factions	0.478 (0.342)	0.877*** (0.257)
Observations	3,311	3,067
ρ	0.210 (0.207)	0.039 (0.103)
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01	

the exception of the kin variable, the controls are all positively correlated with LIV but negatively related to war. The LIV results are consistent with existing research, but the escalation results are more puzzling, in part because most of the group-level controls appear unimportant. One possible explanation is that we are missing some variables that might matter more for escalation to war, such as whether or not the group receives aid (e.g. weapons) from a foreign source. Unfortunately, many of these types of variables are only available for rebel groups, not for SD movements more broadly. The kin variable performs inconsistently. It is possible that the kin variable is too coarse a measure: some studies suggest that what matters is not simply the presence of kin in a neighbor state, but kin in a rival state that matters (Jenne, 2006; Koinova, 2008).

3.4.2 Implications for existing findings

The results I have shown so far have strong implications for seminal findings about the causes of civil wars, particularly regarding the role of GDP per capita. To better understand these results, I run three additional models. First, I reproduce conventional findings by running a probit model for LIV (Model 3) with state-level controls only; second, I run a separate probit model with the same variables, with war as the dependent variable (Model 4), which does not control for LIV. I do this to provide a baseline for the existing findings. That is, most existing studies about the causes of civil war look at all types of civil war, not only SD conflicts. I need to ensure that any differences I might find in my results, particularly regarding GDP per capita, are

due to the 2-step approach – and not an artifact of focusing on SD movements. I therefore use Models 3 and 4 to ensure that I can reproduce conventional findings on my dataset. I then compare Models 3 and 4 to Model 5, a Heckman selection model with the exact same variables as Models 3 and 4 (i.e. state-level controls only). We should not expect any differences between Model 3 and stage 1 of Model 5 because both are probit analyses of LIV, but if my approach is correct, bias from overlooking LIV in Model 4 (probit of war) should make its results different from Model 5, stage 2. Table 3.4 presents the results from Model 3, side-by-side with the results from the first stage of Model 5. Table 3.5 presents the results from Model 4, along with the results from the second stage of Model 5. Note that I use the same equation for LIV and war for Model 5 in order to provide as direct a comparison to the probit models as possible.²⁰ Below I discuss how the probit models compare to the results in all tables.

The results are striking. In both probit models, higher values of GDP per capita are correlated with a lower probability of LIV and war. This is consistent with the existing research, indicating that applying the standard probit model to SD disputes yields the traditional results. This means that any differences that emerge in the Heckman model are in fact due to analyzing LIV and war together – they are not a figment of different variables or different case sets. And there are noticeable differences. For the Heckman model in Table 3.5, GDP per capita is negatively correlated

²⁰This is not technically correct for a Heckman model, but I do so in order to convince the reader that any differences in results are in fact due to the modeling technique used, rather than changes in the model itself.

Table 3.4: Probit v. Heckman Selection: LIV

	<i>Dependent variable:</i>	
	LIV	
	<i>Model 3: Probit</i>	<i>Model 5: Selection</i>
	(1)	(2)
Polity2	-0.020*** (0.004)	-0.020*** (0.004)
Oil exporter	0.096 (0.067)	0.097 (0.068)
Mountainous	-0.014 (0.024)	-0.014 (0.024)
Cold War	-0.025 (0.053)	-0.026 (0.053)
GDP	-0.179*** (0.024)	-0.178*** (0.024)
Population	0.040** (0.016)	0.041** (0.016)
Constant	0.138 (0.278)	0.134 (0.282)
Observations	3,633	3,632
Log Likelihood	-1,711	
Akaike Inf. Crit.	3,437	
ρ		-1.143
Inverse Mills Ratio		-6.464** (2.588)

Note: *p<0.1; **p<0.05; ***p<0.01

Table 3.5: Probit v. Heckman Selection: War

	<i>Dependent variable:</i>	
	War	
	<i>Model 4: Probit</i>	<i>Model 5: Selection</i>
	(1)	(2)
Polity2	-0.030*** (0.006)	0.094** (0.046)
Oil exporter	-0.012 (0.102)	-0.491 (0.401)
Mountainous	-0.098*** (0.035)	0.031 (0.122)
Cold War	0.162** (0.079)	0.209 (0.267)
GDP	-0.093*** (0.035)	0.890** (0.435)
Population	-0.042* (0.025)	-0.243** (0.119)
Constant	-0.327 (0.412)	4.976*** (1.846)
Observations	3,632	3,632
Log Likelihood	-693	
Akaike Inf. Crit.	1,400	
ρ		-1.143
Inverse Mills Ratio		-6.464** (2.588)

Note: *p<0.1; **p<0.05; ***p<0.01

with LIV, consistent with existing research. However, for war, the signs flip completely: it is **positively** correlated with war. Interestingly enough, this is also true for the democracy variable. In other words, when a country-SD movement dyad experiences LIV, if the country is more democratic or has a higher GDP per capita, the dyad is more likely to escalate. If we use the results from Table 3.3, we see similar results for GDP per capita. Democracy, however, is negatively correlated with war, but the coefficient is extremely small and approaches zero. While these democracy findings are more mixed, both sets of results indicate that the relationship between democracy and war is also not nearly as overwhelmingly negative as existing studies might indicate. These results stand in direct contrast to many of the traditional findings in the intrastate conflict literature. How can we best make sense of these new findings? What best justifies my approach?

Interstate conflict scholars have shown that considering escalation (war) without also controlling for onset (LIV) can bias results (Clark and Regan, 2003; Clark and Reed, 2003; Hart Jr and Reed, 1999; Reed and Clark, 2000; Reed, 2000, 2002; Fearon, 2002). Failing to account for onset leads to several problems. First, it introduces selection bias, because escalation cannot occur without onset happening first.²¹ For interstate conflicts, this selection bias occurs according to Reed (2000) because “pairs of states do not become entangled in hostilities randomly...they instead select or are selected into disputes by a strategic process” (84). The same idea applies to the

²¹Note that I make no assumptions here about the rapidness of the transition from onset to escalation.

causes of civil wars, since state-opposition group dyads also select into conflicts. Since selection bias can drive coefficients towards zero,²² it is possible that by conflating onset and escalation, scholars have overlooked important variables. Additionally, if different mechanisms drive LIV versus war, then conflating the two could lead to model misspecification.

Second, the strategic nature of these interactions introduces another source of bias. International relations scholars first distinguished between the start of an interstate crisis and its escalation to war in order to properly account for the strategic and sequential nature of international crises, which made causal relationships more nuanced than they might initially seem (Fearon, 1994).²³ Researchers also showed that simple logit or probit analyses were not appropriate statistical models for strategic, sequential phenomena. A simple logit model could not handle the censored dependent variables, did not capture strategic interdependence, and led to biased standard errors (Signorino, 1999; Smith, 1998, 1999). Taken together, my results and this research indicate that the need to account for LIV when analyzing war applies to not only interstate but also intrastate conflicts.

²²Selection bias can also change the signs of coefficient estimates, so that an independent variable might seem to be positively (negatively) correlated with a dependent variable when it is in fact negatively (positively) related.

²³For example, according to Fearon (1994), the hypothesis that increasing a defender's relative strength would increase deterrence success was overly naive: he posited that crises were actually more likely to escalate when a relatively stronger defender faced a challenge, because that indicated that the challenger was particularly resolved.

3.5 Conclusion

This article has challenged traditional approaches to, and understandings of, the start of intrastate conflict. I have shown that we need to model LIV and war together – both theoretically and empirically – to understand how civil conflicts emerge and evolve. Theoretically, LIV and war are distinct but linked: both represent different stages in the same broader conflict process, and war cannot happen without LIV. Indeed, both reflect the strategic interaction between the state and the opposition; each presents different opportunities for bargaining failures. Both are affected, albeit in different ways, by the information environment, state capacity, and group type. One of the major contributions of this study is to demonstrate that one testable hypothesis derived from those mechanisms, whether an opposition movement has a territorial base and how that might influence a state’s behavior, matters for both LIV and war at the intrastate level. These effects differ from LIV to war, and considering the former is essential when analyzing the latter. The role of a territorial base highlights this, since a group with a base is more likely to experience LIV, but less likely (conditional on LIV) to experience war.

Empirically, I have shown that we cannot overlook LIV when trying to explain war. The same variables do not drive both processes equally or even in the same way, and modeling war without accounting for LIV (or modeling them separately) yields biased, misleading results. Indeed, the second major contribution of this study is to show how our traditional understanding of the causes of civil war will change when we

properly account for the selection effects into conflict. The finding that greater levels of GDP per capita decrease the probability of LIV but actually increase the chance of war for those states already experiencing violence underscores that the causes of civil war might be more nuanced than originally thought.

Yet much remains to be done before we can fully understand exactly why some civil conflicts escalate, while others do not. Several avenues for additional research merit attention. First, I have restricted my analysis here to self-determination movements. It would be worth exploring whether or not the relationships established in this paper hold for other types of civil conflicts. Second, I have focused on three causal mechanisms related to the strategic interaction between the state and the opposition – the state’s capacity, the opposition group type, and the information environment available to the state – but there are many others that can (and should) be addressed. Taken together, these lines of research indicate that analyzing the state and opposition together is a newer approach for intrastate conflict but one that promises to be quite fruitful.

Chapter 4

Cycles of Violence: Civil War Onset, Escalation, and De-escalation

So far, this dissertation has developed and tested a theory about when and why civil conflicts emerge, and when they escalate. I have built on existing literature about the onset and escalation of civil wars to show that it is essential to model (theoretically and empirically) both low-intensity violence (LIV) and war together. As different stages in a single, broader conflict process, they are deeply connected but still shaped in different ways. I have focused in particular on how they are affected by the information environment, state capacity, and group type. This builds on existing research about the start of civil violence and the causes of intrastate war, offering both theoretical and methodological advances.

In the first chapters of this dissertation, I have focused on a linear progression of a civil conflict: a country starts out with no conflict, a rebel group emerges (or is deterred) to challenge the state, and then the conflict escalates (or not). Realistically, however, conflicts are often much more complex, waxing and waning through one

or multiple cycles of escalation and de-escalation. For example, between 1984 and 2005, the Tamil Tigers (Liberation Tigers of Tamil Eelam, LTTE) fluctuated between periods of LIV and war five times, as support for the group waxed and waned and several rounds of negotiations with the Sri Lankan government started and failed.

Yet most existing research examines escalation and de-escalation separately. Major works on the causes of civil violence and war focus on structural conditions that make a country vulnerable to conflict (Cederman et al., 2010, 2011; Collier and Hoeffler, 2004; Fearon and Laitin, 2003; Gurr and Moore, 1997; Ross, 2004; Reynal-Querol, 2002; Vreeland, 2008) and/or that make individuals or groups more or less likely to rebel (Cunningham, 2013, 2014; Krueger and Laitin, 2008; Lacina, 2014, 2015; Lewis, 2017; Lichbach, 1994, 1995; Lindstrom and Moore, 1995; Muller and Seligson, 1987; Tilly, 1978; Popkin, 1979; Verwimp, 2005).¹ Research on de-escalation tends to emphasize a different set of variables, namely factors that make conflict resolution more likely (Chapman and Roeder, 2007; Balcells and Kalyvas, 2014; Downes, 2001; Toft, 2009; Walter, 2002), such as changes in the number or identities of actors involved (Jacob Aronson and Chang, Jacob Aronson and Chang; Cunningham et al., 2009; Cunningham, 2006; Cunningham and Sawyer, 2012; Findley and Rudloff, 2012; Staniland, 2014), the presence of peacekeepers (Fortna, 2008), and third party intervention (Kaufmann, 1996; Sambanis and Schulhofer-Wohl, 2009).

To more fully understand how conflicts evolve, we need to be able to explain both

¹See also the extensive literature on repression (Chenoweth and Cunningham, 2013; Davenport, 2007; Lichbach, 1987; Ritter and Conrad, 2016; Siegel, 2011), including when and if states repress, how effective repression can be, and whether it deters or escalates conflicts.

escalation and de-escalation within a single framework. As earlier parts of the dissertation have shown, the prospect of escalation influences the very start of violence. Logically, we can expect this concept to extend further, such that the actors in the midst of a civil war will be looking forward and anticipating not only how the conflict might resolve but also whether and how periods of relative calm (de-escalation) might shape the outcome. This speaks to a broader theoretical point: the probability of escalation or de-escalation is conditional on the current stage of the conflict. In other words, the chance of experiencing LIV (and what drives that possibility) depends on whether there is currently war or a period of calm. This also means that it is important to distinguish between initial and subsequent (de)escalation, as they might have different causes.

Indeed, there are several reasons why it is important to understand when and why violence might wax and wane over the course of a conflict. First, for practitioners and policymakers looking to manage conflicts, it is equally critical to be able to identify opportunities for de-escalation and also understand when periods of relative calm are at greater risk for a flare-up. Second, taking this approach allows me to disentangle the drivers of initial escalation from variables associated with subsequent spikes in violence. Doing so yields a more nuanced understanding of which variables matter at which points in a conflict. This is important if different stages of a conflict exhibit causal heterogeneity, and there is strong evidence to believe that they do (Anderson and Worsnop, 2016). Third, it also enables me to understand civil conflict cycles more broadly. For example, what are the most common patterns through which a

conflict evolves – is it linear (no violence to minor conflict to war to minor conflict to peace) or more cyclical? Is it more common for a country experiencing civil war to transition directly to peace, or to de-escalate to more minor violence first?

In this chapter, I adopt a holistic approach to studying civil conflicts. Building off of the previous two chapters, I identify four stages of a civil conflict:² no violence, low-intensity violence (LIV), war, and resolution, and I make three main contributions. First and theoretically, I establish a theoretical framework for evaluating conflicts from their start to their finish, identifying the different parts, or stages, of each conflict and justifying a stage approach.³ Second, I extend the theoretical model from the first chapter to the entire life-cycle of a conflict. I derive expectations from the model for certain transitions, focusing in particular on how state capacity and rebel group type affect the probability of transitioning to no violence from LIV and from war.⁴ Third, methodologically, this is the first paper to my knowledge to adopt a stage approach to civil conflicts, and doing so yields information not only on what variables matter at which stages but also provides a wealth of descriptive information

²These are not the only possible four stages. However, as this is to my knowledge one of the first attempts to quantitatively and theoretically study civil conflicts by stage, there is very little previous work to suggest alternatives. I discuss the choice of each stage in more detail in subsequent sections, but the choice of four stages is not only consistent with the different empirical definitions we see for conflict, but also aligns with recent research on conflicts by stage (Jones and Metzger, 2018), which analyzes territorial disputes between countries. For an alternative conceptualization, see Bloomfield and Leiss (1969); Haas (1968); Sherman (1994). See section 4.2 for a more detailed discussion.

³In sum, I argue that a stage approach – versus a continuous approach of points on a continuum – provides theoretical clarity and allows us to empirically test what factors are most important for major shifts (either increases or decreases) in a conflict. See the beginning of Section 4.2 for a fuller version of this justification.

⁴These are conditional probabilities: for example, if a dyad is currently experiencing war, how does a greater state capacity (versus a lower state capacity) affect the probability of a transition to LIV?

about civil conflicts, including the most common sequences, how often civil conflicts experience transitions, and so on. The results show that over ten percent of self-determination conflicts experienced more than ten transitions (ex. escalating from LIV to war, de-escalating from LIV to no violence), and violent conflicts experienced on average over five stages. Civil conflicts are highly recursive and cyclical, which underscores how problematic it is to only examine escalation or de-escalation.

The paper proceeds as follows. First, I synthesize several lines of research about conflict escalation, dynamics, and de-escalation, paying careful attention to draw out what the existing literature identifies as likely drivers of transitions between each stage of the conflict. Then, I develop a stage approach to civil conflicts by describing and justifying each stage and the set of possible transitions. I apply the theoretical foundations of the previous two chapters to understand how my formal model is likely to apply to the conflict at each stage, with more emphasis on how state capacity, the information environment, and rebel group type are likely to influence cycles of violence and de-escalation. The next two sections explain the empirical approach that I use – multistate survival modeling – before describing the data and key transformations made. The penultimate section provides the results, including descriptive information (e.g. the most likely conflict sequences) as well as regression results. The final section concludes.

4.1 Existing research: what we know about escalation and de-escalation

Taking this holistic approach to civil conflicts requires synthesizing three different strands of literature: work on onset and initial escalation to war, work on de-escalation, and work on changes during the conflict (dynamics). Research on the causes of onset (and/or initial escalation) of civil conflict is extensive; I present a brief overview here given the lengthy discussion in the previous two chapters. The classic research on the causes of civil wars focuses on macro-level factors that make a country more or less vulnerable, such as high degrees of ethnic or religious fractionalization, large populations, mountainous terrains, regime type, or low GDP per capita (Collier and Hoeffler, 2004; Fearon and Laitin, 2003; Fearon, 2005; Reynal-Querol, 2002; Ross, 2004; Vreeland, 2008). This research focused on the start of war rather than civil conflict more broadly, leading to some methodological and theoretical concerns. (See chapters 2 and 3 for a fuller discussion.)

More recently, scholars turned to understanding what causes the start of civil violence, pointing out not only that many potential challengers are deterred (or eliminated) by the prospect of government violence (Lacina, 2014; Lewis, 2017; Ritter and Conrad, 2016; Sullivan, 2015, 2016) but also that LIV becomes more probable when prospective rebel groups are factionalized (Cunningham, 2013), able to operate covertly (Lewis, 2017), and/or excluded from power but still strong (Cederman et al., 2010).

My work in the previous two chapters has focused on two factors in particular—state capacity and rebel group strength—and shown that while stronger states successfully deter more challengers (leading to lower probabilities of LIV), they are in fact less willing to negotiate if challenged and therefore more likely to escalate to war. Conversely, I have shown that stronger rebel groups are usually more capable of reaching LIV, but once they do so and demonstrate that they pose a greater threat, states are generally more willing to negotiate, leading to a lower probability of war.

What happens after the initial onset and escalation to war? The de-escalation literature is also extensive, although it does not typically examine de-escalation through the lens of a broader conflict trajectory. That is, most studies do not examine if or how a conflict's start affects its de-escalation. Yet several points speak to the broader question of changes in the trajectory of a civil conflict. First, there is an extensive literature about how to get actors in a civil conflict to the negotiating table and craft durable commitments (Toft, 2009; Walter, 2002). Driscoll (2012) shows that the ability to co-opt rebel leaders can be critical for de-escalation, and Fortna (2008) provides strong evidence of the effectiveness of UN peacekeeping.⁵ Indeed, when examining the role of third parties more broadly, several studies emphasize that peacekeeping interventions—particularly when led by the UN—can help lead to de-escalation, but other third party interventions have mixed effects and can often prolong conflicts (Beardsley et al., 2017; Cunningham, 2010, 2016; Doyle and Sambanis, 2000; Kydd,

⁵For additional research on peacekeeping effectiveness, see Bove and Ruggeri (2018); Fjelde et al. (2019); Howard (2008); Ruggeri et al. (2017).

2003; White et al., 2018) This extrapolates to a broader point: increasing the number of players, whether through third party entry or through actor fragmentation, can make a conflict drag on (Cunningham, 2006; Cunningham et al., 2009).

Second, the literature on preventing conflict recurrence focuses on risk factors for reverting from peace back to violence, but there are potential lessons for identifying when a dyad is at risk of re-escalating from LIV back to war. The most prominent example is the extensive debate over the use of partition as a solution, particularly for ethnic conflicts. As a proponent of partition, Downes (2001) argued that fighting hardens ethnic identities, making separation essential; Chapman and Roeder (2007) demonstrated that conflict re-escalation (in the sense of recurrence) is more likely when national identities are ‘incompatible,’ conflict is zero-sum, and one or both sides receives an increase in force-fungible resources. Yet the evidence on partition effectiveness specifically – and living together more broadly – remains largely mixed (Sambanis and Schulhofer-Wohl, 2009), making it difficult to understand how partition and/or geographical separation might influence the waxing or waning of violence.

All of these research agendas focus on moving from violence to peace. Transitioning between more and less violence (i.e. war to LIV) is much less studied, but still important for several reasons. For example, when peace emerges from LIV versus from war, are there different implications for peace durability? Are we likely to see different conflict outcomes if a conflict transitions from war to peace versus from war to LIV to peace?

The conflict dynamics literature is newer and less developed than either the conflict

onset or conflict resolution literature but has multiple points relevant to this chapter. For example, in research on effectiveness of rebel group tactics, Thomas (2014) argues that terrorism is effective during civil wars, in that it leads to more negotiations and concessions. This is consistent with the argument I have developed on group strength and negotiations post-LIV, which suggests that groups that demonstrate strength via violence are more likely to avoid escalation because they are better able to convince the state that they pose too costly a threat to fight. A related but opposite line of research addresses whether state counterinsurgency (COIN) strategies actually mitigate violence (Arreguin-Toft, 2001; Dugan and Chenoweth, 2012; Sullivan, 2014). The most relevant here is Staniland (2014), who elaborates pathways of collapse for different types of rebel groups based on their social network structure. Lastly, several scholars have studied how changes in the conflict actors—emergence, elimination, fracturing—alter a conflict’s dynamics.⁶ For example, Findley and Rudloff (2012) argue that rebel group fragmentation leads to conditions for immediate cooperation, because fragmentation weakens the combatants who split. While they focused on civil war duration and outcome, it is possible to draw out implications for when de-escalation is more likely: after rebel group splits.⁷ This contrasts with some of the work on conflict duration and escalation, which argued that increasing the number of actors – either via splits or third party entrance – makes escalation more likely

⁶There is also an extensive literature on how alliance changes, particularly among rebel groups, affect conflict dynamics. See Christia (2012) and Otto (2017) for examples.

⁷Findley and Rudloff (2012) also argue for an approach that conceptualizes war as ‘an evolutionary system;’ they use a computational modeling approach given conceptual and empirical challenges in modeling fragmentation.

and increases the probability of violence (Cunningham, 2006, 2013). More recently, Cunningham and Sawyer (ming) show that when a new leader emerges via election⁸ from a rebel group, that rebel group is much more likely to make it to the negotiating table with the state. Here, the logic is that the election signals credibility to the state about the rebel group's viability as a bargaining partner. This is also consistent with my logic that signals of rebel strength convince the state to negotiate rather than fight, although I focus on violent signals.

4.2 State capacity, group strength, and the information environment

If we adopt a holistic approach to studying civil conflicts, can we understand not only when states and challengers are likely to reach onset and escalate to war, but also subsequent cycles of violence? It is important to use the same framework but still distinguish between each stage for three reasons. First, ignoring earlier parts of the conflict can lead to selection bias, as discussed in Chapter 3. Second, if causal effects are conditional—the probability of LIV differs when a dyad is at war versus when it is not experiencing any violence—we need to specify how the current stages shape possible future outcomes. Third and relatedly, there is strong evidence of causal heterogeneity: what matters for LIV is not the same as what drives war.⁹

⁸Cunningham and Sawyer refer to leaders who come to power through what they term “a local selection focused process.” The most prominent example—and the one with strongest effects—is via election; other types in this category are selection by rebel officers or inheriting power. They distinguish local selection process from “orientation toward other actors,” e.g. mergers, splits, group founding, etc.

⁹See Anderson and Worsnop (2016) for additional discussion.

This section lays out a theoretical framework – based off of the theory developed in Chapter 2 – for answering these questions. I do so in several steps. First, I define and justify each stage. Then, I use that framework to develop conditional, dynamic expectations: if a dyad is at war, what makes a transition to LIV more or less likely? What makes a conflict more or less prone to many transitions? Note that there are many possible transitions (nine in total), so I specify which transitions would have clear expectations based on my theory. I am agnostic about those with no deductive hypotheses.

I start by defining what I mean by each stage. I follow the first two chapters of the dissertation and distinguish between LIV and war, where LIV is characterized by minimal violence with incomplete mobilization (of either/both sides) and limited information, and war refers to periods of intense violence, pitched battles (if applicable), and fuller information about rebel group type. I identify two additional stages per conflict: ‘no violence’ and ‘resolved.’ I include no violence not only to capture the period before any battles have started but also because conflicts often experience lulls in fighting and hostilities, where a dispute is ongoing but some period of time passes without active conflict. Note that I use ‘no violence’ to refer to the absence of violent events or activities, not to refer to nonviolent anti-state behavior, which often co-occurs with violence (Chenoweth et al., 2019). The ‘resolved’ stage captures a conflict that is over, although it is agnostic about how the conflict ends; it includes conflicts that end via negotiated settlement or military victory/defeat. Conceptually, by resolved, I mean conflicts where the parties are no longer making demands to

change the status quo.¹⁰ Sometimes, this is easier for combatants to distinguish than others: a signed peace agreement is visible and known to both sides. Lulls in violence can be distinguished from resolved conflicts when the challenger is no longer making demands of the state (for at least three years). For example, the Northern Ireland conflict is unresolved throughout the dataset – despite several lulls of no violence – because the Catholics continued to make demands of the government. Figure 3.1 illustrates the four different stages and possible transitions between each. Note that there are nine possible transitions: NV to LIV, NV to War, NV to Resolved, LIV to NV, LIV to War, LIV to Resolved, War to NV, War to LIV, and War to Resolved.¹¹

Why take this approach to conflict? In other words, why (those) four stages and not five or three or six, and why even use a stage approach versus points on a continuum? I use a stage—not continuum—approach for two reasons. First, there are prominent differences between LIV and war; for example, the protests and minor skirmishes of the Sahrawi conflict in Morocco looks a lot different than the pitched battles, sieges, and high casualties of the Baifran war. Using a stage conception allows for a clear theoretical distinction. Second, using a stage conception provides better traction when trying to statistically model what drives transitions from one type of conflict to another; it is easier to identify when a major change has occurred and

¹⁰I measure a conflict stage as resolved if there has been a negotiated settlement, military victory of one side over the other, or cessation of violence *and demands* for three or more years, which is consistent with the self-determination data coding and the standard approach in the conflict literature. See Section 3.3 for more details.

¹¹The stage model is agnostic about order or linearity, in that while it allows for a linear transition sequence, it is not required. Conflicts can start in NV, LIV or War (although the latter two are empirically rare) and progress through any sequence until Resolved.

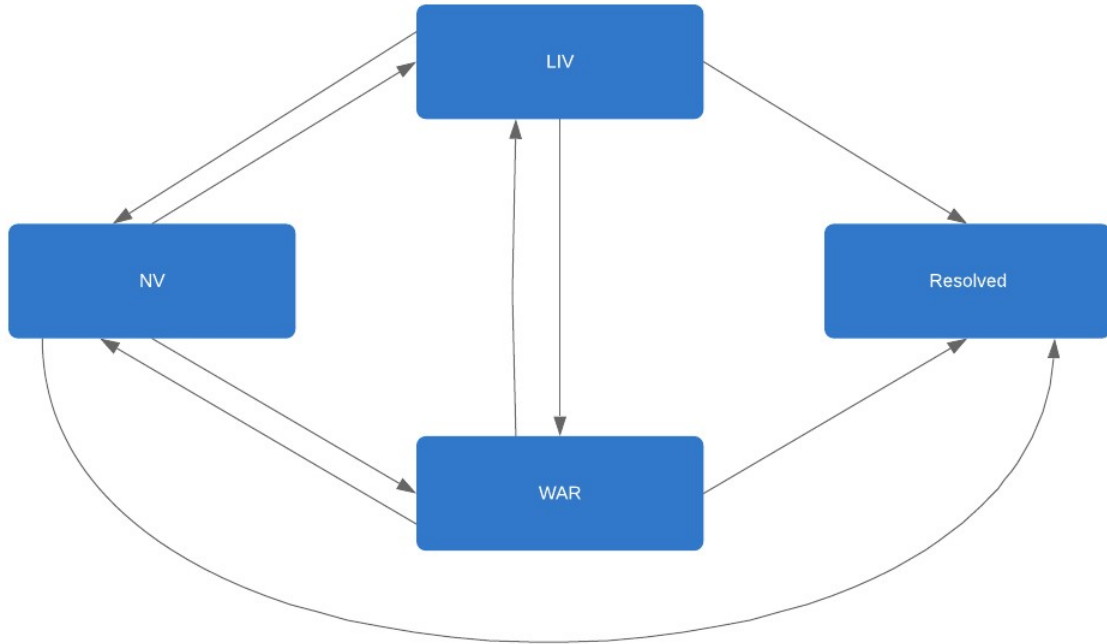


Figure 4.1: Possible Civil Conflict Stages and Transitions

what drives that change, rather than potentially conflating major and minor shifts in the conflict.

Identifying which stages—and the appropriate number—to use is trickier. There is very little guidance in the statistical literature about how many stages to use (De Wreede et al., 2010; Jones and Metzger, 2018; Metzger and Jones, 2016). I choose four stages because it is consistent with most of the few existing studies that separate conflicts into stages (Jones and Metzger, 2018; Huth and Allee, 2002a), although some prior work has divided conflicts into six stages (Bloomfield and Leiss, 1969; Eyerman and Hart Jr, 1996; Haas, 1968; Sherman, 1994).¹² I do not use the six

¹²The SHERFACS project collected data on international disputes between 1945 and 1984 (Sherman, 1994), and it followed the Bloomfield and Leiss (1969) CASCON set of six phase – dispute, conflict, hostilities, post-hostilities conflict, post-hostilities dispute, and settlement (Eyerman and

stage structure because most of the focus (five out of six stages) is on variations in non-violent portions of conflict, which are not the focus of this dissertation. Lastly, distinguishing between LIV and war is not only consistent with the previous two chapters of this dissertation but also reflects the fact that most empirical studies of civil conflict look at either LIV or war.

4.2.1 Initial Escalation, State Capacity, and Group Strength

I build off of the first two chapters by continuing to focus on the information environment, state capacity, and group strength. Of the nine possible transitions, two are explicitly addressed already in Chapters 2 and 3: NV to LIV and LIV to war. Recall that I argue that the information environment is particularly challenging for the state—much more so than in interstate conflict—because the state does not initially know if it even faces a rebel group, let alone how strong the group is. Violence becomes particularly informative, so that the state is much less likely to try and negotiate until after LIV, when it gains a better sense of its challengers. The first two chapters also demonstrated that state capacity and group strength affect LIV differently than war. While strong states successfully deter most challengers, making LIV less likely, those strong states that do face a rebel group are much less likely to back down, making war more likely, because they are much more confident in their probability of winning. Conversely, strong rebel groups are much more likely to reach LIV, but as the government realizes the severity of the threat the group poses, it be-
(Hart Jr, 1996).

comes much more likely to bargain, making war less likely. The hypotheses from the first two chapters can therefore be restated here, using the stage framework language.

NV to LIV: Conditional on no violence, the transition to LIV will be more likely for stronger groups. Likewise, conditional on no violence, the transition to LIV will be less likely for greater state capacity.

LIV to War: Conditional on LIV, the transition to war will be less likely for stronger groups. Likewise, conditional on LIV, the transition to war will be more likely for greater state capacity.

4.2.2 Initial De-escalation

If stronger states are, *ceteris paribus*, less likely to see conflict, but more likely to escalate to war if they do, what happens after that? And how does a conflict de-escalate from war using the framework outlined above? As I have argued, the information environment is particularly challenging for the state in a civil conflict, so bargaining becomes much more likely after LIV, once the state has a better sense of whether a group is a credible threat, what it wants, etc.

Extending this logic means that information shifts during war can also facilitate de-escalation by convincing the state to try and negotiate with rebels. It is important to be specific here, because not all information gains are going to lead the state to try and negotiate; if, for example, the state suddenly learns where the leaders are

hiding, it might make a concerted effort to use force to win the conflict. So, here I mean information that, broadly speaking, convinces the state that it was wrong about the group (or something has changed), such that negotiations are now preferred to violence. As Cunningham and Sawyer (forthcoming) show, the presence of a new leader via rebel group election (or a similar local selection process) can demonstrate that not only has a new opening for negotiations emerged, but also that the group has more local support and internal cohesion than the state might have previously thought. Similarly, when rebel groups demonstrate new repertoires of violence, that can provide the state with new information about the group's capabilities and resolve, which in turn changes the government's calculus. Thomas (2014) shows that governments are more likely to negotiate with—and offer concessions to—rebel groups that execute more terror attacks during civil wars.

Indeed, the formal literature on learning during war speaks extensively to how fighting can reveal important information to one or both parties (Blainey, 1988; Filson and Werner, 2002; Leventoğlu and Metternich, 2018; Powell, 2004; Reiter, 1996; Slantchev, 2003; Smith and Stam, 2004; Wagner, 2000). According to Fearon (2007), screening by fighting becomes particularly likely if the government is sufficiently confident in its military prospects, and the government will decide to try and bargain once it can understand if it faces a strong (not weak) rebel group. This suggests that, if the government is unable to determine the group's type after LIV, bargaining and de-escalation becomes more likely once the type is revealed during war.

How does state capacity affect the likelihood of de-escalation transitions? Similar

to the arguments discussed above, I would expect that, conditional on being at war, higher capacity states are less likely (than lower capacity states) to de-escalate. In other words, if we are comparing dyads at war, those with higher capacity states will be less likely to de-escalate. As the first two chapters showed, these are the states that are more confident in their chance of winning and more likely to escalate given the onset of conflict. By extension, they are more likely to be able to carry the costs of conflict for longer and take longer to change their minds about the costs of fighting outweighing negotiations.¹³ Here again the learning during war literature provides additional support. Smith and Stam (2004) develop a model for interstate conflict that shows when states believe their analysis of the military balance of power is correct (and the other side is overly optimistic), war occurs and fighting takes longer, until the ‘truth’ can be revealed. Smith and Stam’s model is particularly relevant in the civil conflict context, as they allow a state to know that the other side has the same information about the state’s capabilities. In the intrastate context, a state knows that the rebel group has a lot of information about the state’s abilities and should take that into account when seeing the group’s willingness to fight. But especially if the state is strong, it will still think that the rebel group is too optimistic, leading to war and more extended fighting; the stronger state takes longer to ‘learn’ through war about the true balance of power and be willing to de-escalate. In terms of stages and transitions, this leads to specific hypotheses:

War to LIV: Conditional on being at war, the probability for a dyad to transition

¹³This requires the optimism assumption, which is plausible for higher capacity states.

to LIV decreases with greater state capacity.

War to NV: Conditional on being at war, the probability for a dyad to transition to NV decreases with greater state capacity.

How might rebel group strength affect these two transitions? I have argued that stronger rebel groups are more likely – once they demonstrate their strength via violence – to convince the government that negotiations will be better (less costly) than fighting. This is also consistent with research that governments become willing to negotiate once rebel groups demonstrate political signals of strength (Cunningham and Sawyer, 2013) or greater coercive capacity via terrorist attacks (Thomas, 2014). I would therefore expect this logic to hold once a government-rebel group dyad is experiencing war, too. Indeed, during war, when a government realizes that it is facing a stronger group¹⁴, the stakes are even higher, as the government (and rebels) are already paying higher costs associated with more intense violence. I would therefore expect that, conditional on war, stronger rebel groups will be more likely to get a seat at the table. Since negotiations are often associated with lulls in fighting (even if they are unsuccessful and do not lead to a complete peace agreement), I expect that stronger groups will be more likely to experience de-escalation, conditional on war. Collectively, this yields the following additional two hypotheses:

War to LIV: Conditional on being at war, the probability for a dyad to transition

¹⁴One possibility is that a government might need more extreme violence, like war instead of merely LIV, to be convinced that a group is sufficient threat to bargain with rather than eliminate via violence.

to LIV increases with greater rebel strength.

War to NV: Conditional on being at war, the probability for a dyad to transition to NV increases with greater rebel strength.

So far, I have developed expectations for four transitions: NV to LIV, LIV to War, War to NV, and War to LIV. The first two are restatements of the hypotheses in Chapters 2 and 3, while the second two have built off of that framework. There is another transition whose expectations can be clearly deduced: de-escalating from LIV to NV. Expectations for this transition are relatively straightforward because they are the opposite of expectations for the transition from LIV to war. If stronger states, conditional on onset, are more likely to escalate to war, then they are also less likely to de-escalate to no violence. This is also similar to the logic discussed above for the other de-escalatory transitions. Higher capacity states will, conditional on violence occurring, be less willing to back down. Conversely, we can expect that strong groups will be more likely to de-escalate by transitioning from LIV to NV, since their ability to successfully perpetrate LIV makes the government more convinced about the costliness of fighting and therefore more willing to negotiate. This leads to the following hypotheses:

LIV to NV: Conditional on LIV, higher capacity states are less likely to de-escalate to no violence. Conditional on LIV, stronger groups are more likely to de-escalate to no violence.

What transitions have no clear expectations based on my theory? There are four

with no clear predictions: NV to war, NV to Resolved, LIV to Resolved, and War to Resolved. The case of NV to war is not well-covered by my model, which focuses less on explosive conflicts that jump directly from no conflict to war, which also makes it difficult to develop expectations of future transitions from lulls directly into war. Similarly, my theory can be extended to address de-escalation, as it suggests when negotiations and lulls in violence will be more or less likely. Yet the implications are not always clear for when those negotiations will succeed, such that a conflict will actually be resolved. Negotiations are not the only way for a conflict to be resolved, but the model does not offer predictions on complete military victory by one side or another, either. I therefore abstain from developing predictions about these transitions.

4.2.3 Subsequent Periods

The most straightforward trajectory for a conflict is linear: no violence (pre-conflict) to LIV to war before de-escalation (potentially through LIV) to peace. Yet, as the above discussions suggest, fluctuations often happen over the course of a conflict, whether because a military stalemate leads to a period of lower violence or because negotiations fail and tensions flare up again.¹⁵ When are cycles likely?

I argue that fluctuations are more likely when dealing with strong rebel groups and/or low capacity states. To understand why, it is helpful to examine the opposite

¹⁵Note that I am not talking about conflict recurrence here, in the sense of a new war between the same two actors after peace for several years. Rather, I am referring to fluctuations throughout a single conflict between no violence, LIV, and war.

case of what should happen with weak groups. If a weak rebel group faces a strong state, conflicts should be short and decisive, with few fluctuations. The group will most likely be deterred from mobilizing, as I have shown both with the theoretical model and empirical tests in Chapters 2 and 3, respectively. However, in the rare event that a weak rebel group does emerge in that case, I expect a quick state victory. The strong state will not need to negotiate with a weak rebel group and can instead eliminate the threat (and deter future prospective groups) easily. This has long been the case with strong authoritarian states, from China's response to Tiananmen Square to the 1982 Hama massacre in Syria.

If a weak rebel group faces a weak state, I expect the conflict to linger for a long time at LIV, as neither side will have the ability to decisively end the conflict, or even necessarily escalate it to war. The low capacity state will have difficulty eliminating the rebel group via violence, and the weak rebel group will have difficulty inflicting enough damage to get the state to the negotiating table. Indeed, recall from Chapter 2 (Table 2.3) that the expected outcome in this case is protracted LIV, which suggests conflicts that are more drawn out. This is also consistent with the findings from Cunningham et al. (2009), which shows that civil conflicts with weaker groups (particularly those with access to international safe havens or similar ways of escaping government violence) tend to be more drawn out.

When a rebel group is strong, it is harder to linearly resolve the conflict. In the event of strong rebels and a strong state, I predict war. Civil wars last a long time – about 3.5 times as long as interstate wars (Brandt et al., 2008). This provides more

opportunities for fluctuations, which is compounded by the fact there are likely to be transitions between LIV and war as power shifts on the ground. Fluctuations are also likely when there is a strong rebel group in a weak state. Even if the state is weak, there is still a power asymmetry, such that it is harder for the rebel group to completely defeat the state. A pure state victory is also difficult: especially if the rebel group is operating in a remote region, the ability for the state to project its force far enough makes a decisive military outcome difficult.

Table 4.1 provides a summary of my expectations. The bolded rows represent hypotheses that are new to this chapter, versus restatements of those derived in Chapters 2 and 3.

Table 4.1: Summary of Expectations

	State Capacity	Group Strength
P(LIV NV)	-	+
P(War LIV)	+	-
P(NV LIV)	-	+
P(LIV War)	-	+
P(NV War)	-	+
Number of fluctuations	-	+
P(War NV)	NA	NA
P(Resolved NV)	NA	NA
P(Resolved LIV)	NA	NA
P(Resolved War)	NA	NA

4.3 Multi-state survival models

To best capture this holistic approach to civil conflicts in my empirical analysis, I use multi-state survival modeling. Multi-state survival models are a broad class of statis-

tical models that are largely new to political scientists,¹⁶ although they are often seen in biostatistics, where they are used to follow a subject of interest—e.g. a conflict, an individual, or a disease—across transitions between multiple stages (ex. diagnosis, surgery, relapse, etc). More specifically, “a multi-state survival model is a model for time-to-event data in which all individuals start in one or possibly more starting states...and eventually may end up in one (or more) absorbing or final state(s)” (De Wreede et al., 2010). The multi-state survival model is in fact a generalization of both the Cox proportional hazard model and the competing risks survival model: instead of the single risk set in a competing risks model, the multi-state model incorporates multiple risk sets (Putter et al., 2007). The competing risks model—which is more familiar to political scientists—allows for only one starting state and no intermediate states, although it does account for at least two different absorbing states. The multi-state model generalizes on this by allowing for multiple starting states and, more importantly, intermediate steps or ‘states.’ This also means that the process being modeled does not have to be sequential but can rather be recursive, as is often the case with conflicts. Additionally, multi-state survival models do not drop an observation after a transition (as in competing risks models) but rather consider what new transitions the unit is at risk for.

Broadly speaking, multi-state survival models offer three advantages. First, dif-

¹⁶To my knowledge, the two exceptions are Jones and Metzger (2018) and Metzger and Jones (2016). Only Jones and Metzger (2018) looks at multi-state models in a conflict setting; their focus is on interstate conflicts and territorial disputes, and they focus on extending Huth and Allee (2002b,a), which uses a multinomial logit approach.

ferent transitions have their own baseline hazard rate.¹⁷ In the case of this analysis, with the four stages and nine transitions illustrated in Figure 3.1, the model would estimate nine different baseline hazards. Substantively, this means that the model distinguishes between the risk of transitioning from no violence to war and from war to LIV—or any of the other transitions. This is particularly important not only so that we can distinguish between the risk of transitioning to LIV vs war, but also so that we can separate out the risks of escalation and de-escalation. Second, because the model stratifies the baseline hazard rate, we can also specify transition-specific covariates, such that the effect of any given variable can vary from one transition to another. In other words, whether a group has a territorial base might have a different effect on the transition from no violence to LIV than it does on the transition from LIV to war or from war to no violence. Third, the transition probability from one stage to another incorporates both indirect and direct probabilities of transition, given a certain time frame. For example, the transition probability from LIV to war from time t to time $t+n$ includes the direct probability (LIV to war) as well as indirect probabilities (LIV→NV→War, etc.).¹⁸ This is important because it means the model can handle both sequential and recursive events.¹⁹ In other words, this method is flexible enough

¹⁷Another way of phrasing this is to say that the underlying baseline hazard is stratified.

¹⁸More specifically, a transition probability matrix $P(s,t]$ is estimated such that each element is the probability of transitioning from that row's stage to that column's stage within the time frame denoted by $(s,t]$. The diagonals are therefore blank – it is impossible to transition from State A into State A. Since the dyad must be in one of the stages (but not more than one) at the end of t , the rows also sum to one (Jones and Metzger 2018, p. 831).

¹⁹By recursive, I mean a conflict that cycles through stages repeatedly. For example, consider a conflict that starts with no violence, experiences the start of LIV before escalating to war, but then de-escalates back to LIV, fluctuating multiple times between LIV and war before finally being resolved. In short form, that path might be (depending on the number of fluctuations)

to handle multiple paths – multiple sequences, to use consistent phrasing – between a start and endpoint. This allows for focusing on the primary goal of understanding transitions between stages (between any two times).

According to Jones and Metzger (2018), “multistate models greatly expand researchers’ ability to model *complete* processes of theoretical interest” (458). Indeed, the mathematical properties described above mean that multi-state models are particularly appropriate and advantageous for studying civil conflicts. Multi-state models provide a single analytic framework for studying a civil conflict from the start – when a challenger emerges – to the end – when a conflict is resolved (peacefully or militarily). As earlier parts of the dissertation have shown, analyzing conflict stages in isolation is problematic: analyzing war without accounting for LIV overlooks the strategy and process leading up to war, while analyzing only the start of violence misses how the shadow of future conflict can influence early events. Chapter 2 used selection models to mitigate those concerns, but left the question of recursive transitions—and what happens after the initial escalation to war—unanswered. Multi-state models offer not only a more unified understanding of civil conflicts (e.g. what transitions are most common? how often do conflicts cycle through escalation and de-escalation? etc.) but also the ability to drill-down into specific covariate effects for each transition.

Multi-state models require the specification of beginning and end (absorbing) states. Here, the end state is ‘Resolved’, which occurs when the conflict has terminated, either by formal agreement, military victory, or cessation of violence for three $NV \rightarrow LIV \rightarrow \text{war} \rightarrow LIV \rightarrow \text{war} \rightarrow LIV \rightarrow \text{Resolved}$.

or more years.²⁰ Because Resolved is an absorbing state, no transitions can occur out of that state. If hostilities subsequently emerge again between the same two actors, it is treated as a new conflict. Conflicts that are not resolved (are still ongoing) at the end of the dataset are treated as right-censored. While the beginning state is most often pre-conflict (what I label in the data as ‘no violence’ or NV), I allow the model to also account for LIV and war as beginning states. I do this so that the model can also capture instances of explosive conflict, where a challenger emerges and the conflict becomes violent so quickly that the first year of a conflict-dyad already breaks the LIV and/or war thresholds.²¹

Formally, the hazard rate for the model accounts for the hazard of a transition, q , occurring in time t as $\alpha_q(t)$, given by $\alpha_q(t) = \alpha_{q0}(t)e^{\beta'X_q}$. Note that X_q refers to the transition-specific vector of covariates, while β' is the transposed vector of coefficients. This yields a cumulative transition hazard of $A_q(t) = \int_0^t \alpha_q(u)du$.²²

4.4 Data and Analysis

I continue to use the self-determination data from Cunningham (2013) so that it is easy to see how the results change by adopting a multi-state survival model approach. Likewise, most of the variables remain the same from Chapter 2, with GDP per capita and territorial bases being the key proxies for state capacity and rebel group strength,

²⁰This is consistent with how the self-determination data is coded and also reflects the general approach in conflict literature about when to code a conflict as over/resolved.

²¹See below for details on frequency by start stage.

²²The cumulative transition hazard is then used to estimate the transition probability matrix, $P(s,t)$. For full mathematical details, see De Wreede et al. (2010) and Putter et al. (2007).

respectively. I continue to operationalize LIV as violence between 25 and 1000 battle deaths, war as violence of over 1,000 battle deaths,²³ and no violence (NV) as when the dispute/conflict is ongoing but the violence in a given year does not reach the 25 battle deaths threshold.²⁴ I follow Cunningham (2013) and code a conflict as resolved if a settlement is reached or if more than three years pass of inactivity.^{25,26} These four possible stages are captured initially in a single variable, ‘stage’, which can assume any of the four values (NV, LIV, WAR, Resolved).

The data starts in a standard dyad-year format, where the dyad in this case is the self-determination movement-country. Table 4.2 provides an example from the dataset for reference (note that most of the columns for covariates are omitted, in order to most clearly focus on the overall structure).²⁷

There are several transformations that I make to the data in order to prepare it for multi-state survival modeling. First, the data must be changed from the discrete, dyad-year format to a time format that identifies the dyad, the current stage, when

²³One concern is that this operationalization of LIV would pick up other forms of violence beyond what is perpetrated by the rebel group, such as communal violence. Using self-determination movement data helps mitigate this concern. This type of data excludes other minor violence occurring generally in the country—the violence must still be associated with the self-determination group—and is restricted to battle-related deaths only, distinguishing it from protest violence and other similar events.

²⁴See Chapter 2 for a fuller discussion of the debates about these empirical definitions. I continue to use these thresholds for two reasons: consistency with the rest of the dissertation, and lack of better alternatives for the self-determination movement data.

²⁵There is an important but subtle distinction here. Inactivity does not necessarily mean non-violence; the data contain plenty of instances of groups that are active and nonviolent for decades (e.g. Native Americans in the U.S.). Rather, inactivity here means groups that are presumed to have dissolved and/or are no longer making demands – violently or nonviolent – of the government.

²⁶Note that this also means that many cases are right censored, i.e. not resolved before the end of the dataset (2005). Because self-determination disputes are particularly protracted, one interesting and important extension of this study would be to apply the analysis to other types of civil conflicts.

²⁷Not pictured are the first several observations for the Tajik-Afghanistan dyad, which starts in 1978 with non-violence before transitioning to LIV in 1979; it remains in LIV through the 1980’s.

Table 4.2: Snapshot of Initial Data Structure

Actor 1	Actor 2	Year	Current Stage
Tajik	Afghanistan	1991	LIV
Tajik	Afghanistan	1992	LIV
Tajik	Afghanistan	1993	NV
Tajik	Afghanistan	1994	NV
Tajik	Afghanistan	1995	War
Tajik	Afghanistan	1996	LIV
Tajik	Afghanistan	1997	LIV
Tajik	Afghanistan	1998	LIV
Tajik	Afghanistan	1999	LIV
Tajik	Afghanistan	2000	LIV
Tajik	Afghanistan	2001	LIV
Tajik	Afghanistan	2002	NV
Tajik	Afghanistan	2003	NV
Tajik	Afghanistan	2004	Resolved

that stage started, what the next stage will be, and how long the dyad will spend in that current stage (Jones and Metzger, 2018).²⁸ The time in stage is a count in years that includes the first year in that stage. This shift from dyad-year to dyad-‘stage’ condenses the rows, so that there are fewer observations: the 3908 dyad-year observations transform into 508 observations. Table 4.3 shows the transformation.

Table 4.3: First Transformation of Data Structure

Actor 1	Actor 2	Current Stage	Next Stage	Current stage start	Time in stage
Tajik	Afg.	LIV	NV	1979	14
Tajik	Afg.	NV	War	1993	2
Tajik	Afg.	War	LIV	1995	1
Tajik	Afg.	LIV	NV	1996	6
Tajik	Afg.	NV	Resolved	2002	2
Tajik	Afg.	Resolved	–	2004	–

²⁸Jones and Metzger (2018) refer to this as a ‘continuous’ time format, which is slightly misleading. They mean continuous in that, for an entire conflict, there cannot be any gaps in time, and the end of one stage must lead directly into the start of another. This labeling might seem inconsistent with my approach of four discrete stages, but it is purely a case of confusing wording; they mean that there the entire temporal period of a conflict must be accounted for.

At this point, the data reflects the sequence of stages each conflict dyad experiences. The next part of the data transformation is aimed at identifying all of the possible sequences that the dyad could have taken (but did not). More specifically, for nearly every row, I create two additional rows that indicate the other possible stages that could have been taken but were not (Jones and Metzger, 2018). These additional rows are identical in almost every way to the parent row, except that they have a different ‘next stage’. An example is useful here. If a conflict-dyad is in war and subsequently switches to LIV, the two stages it could have switched to (instead of LIV) are NV and resolved. Thus, the parent row of war (current stage) to LIV (next stage) gets two additional rows: 1) war (current stage) to NV (next stage) and 2) war (current stage) to resolved (next stage). I do this because it is essential to be able to compare the transitions that did occur to those that could have but did not (Metzger and Jones, 2016; Jones and Metzger, 2018). The null case – no transition happens – is treated as the base case.²⁹ The exception here is when a conflict dyad is in the resolved stage; that is an absorbing state and so I do not create any duplicate rows. Lastly, each possible transition gets a number, which is a requirement for the `mstate` R package (De Wreede et al., 2010).³⁰ Table 4.4 shows the final version of the data. Note that the addition of alternate rows roughly triples the number of observations,

²⁹The easiest analog is to a set of dummies that covers the entire space, where one category is withheld as the reference category. For data where no transition occurs, the value for “transition occurs?” is 0 for all three rows. For example, if at the end of the data, a dyad remains in LIV, each of the three possible transitions – LIV to War, LIV to NV, LIV to Resolved – is marked as 0.

³⁰The possible transitions and corresponding numbers are NV to LIV (1), NV to War (2), NV to Resolved (3), LIV to NV (4), LIV to War (5), LIV to Resolved (6), War to NV (7), War to LIV (8), and War to Resolved (9). Figure 3.1 shows all of the possible transitions.

so that the final number of observations in the dataset is 1584.

Table 4.4: Final Data Structure

Actor 1	Actor 2	Current Stage	Next Poss Stage	Current stage start	Time in stage	Transition Occurs?	Transition ID
Tajik	Afg.	LIV	NV	1979	14	Yes	4
Tajik	Afg.	LIV	War	1979	14	No	5
Tajik	Afg.	LIV	Resolved	1979	14	No	6
Tajik	Afg.	NV	War	1993	2	Yes	2
Tajik	Afg.	NV	LIV	1993	2	No	1
Tajik	Afg.	NV	Resolved	1993	2	No	3
Tajik	Afg.	War	LIV	1995	1	Yes	8
Tajik	Afg.	War	NV	1995	1	No	7
Tajik	Afg.	War	Resolved	1995	1	No	9
Tajik	Afg.	LIV	NV	1996	6	Yes	4
Tajik	Afg.	LIV	War	1996	6	No	5
Tajik	Afg.	LIV	Resolved	1996	6	No	6
Tajik	Afg.	NV	Resolved	2002	2	Yes	3
Tajik	Afg.	NV	LIV	2002	2	No	1
Tajik	Afg.	NV	War	2002	2	No	2
Tajik	Afg.	Resolved	—	2004	—	—	—

4.5 Results

Because this is a new estimation strategy for studying civil conflicts, there are two broad categories of results to explore. The first is descriptive—information about transition frequency, conflict sequences, etc.—and largely inductive. It helps justify the stage approach as it provides a sense of whether conflicts are largely linear or if the fluctuations and cyclical behavior I have addressed are in fact common. To preview this part of the results, violent conflicts on average experience at least one stage twice, and over ten percent of conflicts experienced over ten transitions. This section addresses broad themes from the findings before turning to how the descriptive results

relate to my expectations about when conflicts are more likely to fluctuate. Overall, this portion of the results underscores how important it is to be able to differentiate covariate effects for each transition.

I then turn to hypothesis testing of the expectations outlined in Table 4.1. This second set section features the regression results from the main multi-state survival model and addresses which expectations were supported and which were not. It also offers interested readers the chance to examine the role of other covariates of interest.

4.5.1 Descriptive statistics: how civil conflict evolve

Do conflicts experiencing LIV more frequently escalate to war or de-escalate to NV? Are resolutions more frequently reached during war or during a period of peace? To provide an initial sense of conflict transitions, I start with a general overview on which transitions are more likely by stage. Table 4.5 provides a matrix with counts for each transition, where the row represents the stage a conflict is transitioning *from* and the column represents the stage a conflict is transitioning *to*. For example, in row 1, column 2, there are 79 observations, meaning 79 instances of a conflict transitioning from no violence to LIV. The censored column indicates observations that were right-censored (i.e. not resolved) by the end of the time frame of the data set (2005). Note that values for the diagonal must be zero, because a conflict cannot transition from one stage into the same stage (i.e. no NV→NV, etc.)

Several points bear further attention. First, as discussed in Chapter 2, many of the conflicts never resort to violence: over half are nonviolent (or, more precisely,

Table 4.5: Transition Matrix

	NV	LIV	War	Resolved	Censored
NV	0	79	33	26	107
LIV	75	0	50	6	13
War	27	52	0	1	6
Resolved	0	0	0	0	0

experience less than 25 battle deaths) for the entire conflict duration. Many of these are long-standing disputes; in fact, the average length of time a conflict-dyad remained in a non-violent stage without transitioning was 13.85 years. This is quite different than the average time in a war stage without transitioning—2.2 years—or even in an LIV stage—3.92 years. Second, these results also speak to nuances in the effectiveness of non-violent action (Chenoweth and Cunningham, 2013; Chenoweth and Lewis, 2013; Stephan and Chenoweth, 2008). A nonviolent stage is the most likely to transition into “Resolved,” with approximately 11 percent of conflicts in a nonviolent stage moving to “Resolved,” compared to only five percent for those in LIV and a mere one percent for those at war. Yet simply saying that nonviolence is more effective than violence is, unfortunately, not necessarily accurate, either. Almost half of the conflicts that are resolved experience LIV or war before a resolution can be reached, which speaks to many of the points this dissertation has made about how violence can be informative for the state when deciding whether or not to try and bargain with an opposition group. The fact that purely nonviolent conflicts also last much longer on average is also consistent with my argument that states are much less likely to bargain with challengers until after initial violence.

The descriptive analysis also highlights another important point: many of these conflicts are highly recursive and cyclical. For example, the average number of stages per conflict is 3.24, but that is a somewhat skewed statistic, as it includes the many conflicts that never experienced violence. When we exclude the conflicts that were purely nonviolent, the average number of stages per conflict increases to 5.27. Over ten percent of conflicts experienced more than ten transitions. Indeed, this is largely why there are few conflicts with the exact same sequences - there is an infinite number of possible sequences when transitions are allowed to repeat.³¹ Table 4.6 provides a list of the conflicts with the most number of transitions, along with the sequence of stages and transitions.

The conflicts with the greatest number of transitions also speaks to my expectation about how state capacity and group strength affect conflict fluctuations, although these results are descriptive and not causal. Nevertheless, these descriptive results lend support to my argument. Most of the conflicts with a lot of fluctuation are weak states like Myanmar (on the list twice), Senegal, and Ethiopia. The exception is the United Kingdom, which is often an outlier in civil conflict studies.³² Conversely, many of the rebel groups on the list are strong and resolved, in particular the IRA, the Kurds, and the Somalis (who have since gained full independence). Even the

³¹There are nine conflict sequences that are exactly repeated: NV (censored, 76), NV to Resolved (18), NV to LIV to NV (censored, 5), NV to LIV to Resolved (5), NV to LIV (censored, 3), NV to LIV to NV to LIV to NV (2, censored), NV to War to NV to Resolved (2), LIV to NV (censored, 2), and NV to LIV to War to NV (2, censored). That being said, many sub-sequences (ex. LIV to War to LIV) can be found across multiple conflicts.

³²Note that while Iran might seem like another counter example, this is Iran after the revolution and during the Iran-Iraq war, when the regime was still consolidating and the country was militarily and economically overstretched.

Table 4.6: Conflicts with the Greatest Number of Transitions

Conflict	No. Transitions	Sequence
Karens (Myanmar)	21	LIV, War, LIV, war, LIV, war, LIV, war, LIV, war, LIV, war, LIV, NV, LIV, NV, LIV, War, LIV, NV, LIV
Somalis (Ethiopia)	14	NV, War, LIV, War, NV, War, LIV, NV, War, NV, War, LIV, War, LIV
Kurds (Iran)	14	NV, LIV, NV, War, LIV, War, LIV, NV, LIV, NV, War, NV, LIV, NV
Shan (Myanmar)	14	LIV, War, NV, LIV, NV, LIV, War, LIV, War, LIV, NV, LIV, NV, LIV
Moros (Philippines)	13	NV, LIV, War, LIV, War, LIV, War, LIV, NV, War, LIV, War, LIV
Catholics (UK)	13	NV, LIV, War, LIV, War, LIV, War, LIV, War, LIV, NV, LIV, NV
Casamacias (Senegal)	12	NV, LIV, NV, LIV, NV, LIV, War, NV, War, NV, LIV, NV
Kurds (Iraq)	12	LIV, War, LIV, NV, LIV, War, LIV, War, LIV, NV, LIV, NV

Casamacias of Senegal support the broader story: almost completely an exclave³³, the Casamacias had mostly de facto autonomy throughout the 1980s, fluctuating between peace and LIV with the weak Senegalese state. The weak central government did not bother trying to bargain or achieve military victory until oil was discovered in the 1990s, at which point the conflict vacillated between periods of war, with both sides having trouble achieving a decisive military victory (or agreeing on a negotiated settlement). This case also highlights that rebel group strength and state capacity are not the only important factors that affect transition probabilities, as will be evident in the next subsection. In this case, for example, oil was crucial, but the broader

³³Casamance is almost completely separated from southern Senegal by Gambia, although there is a small connection on its eastern border to the rest of the country.

point is that many other key variables merit attention using a multi-state modeling approach.

The fact that so many conflicts have multiple transitions is also important because it underscores how problematic only looking at escalation or de-escalation can be. When conflicts are fluctuating repeatedly between different stages, it is possible that factors that matter for any initial escalation or de-escalation might be different than the factors driving subsequent cycling. If this were a rare problem, it would be easier to overlook, but given that, on average, the violent conflicts experience at least one stage twice, we need to be able to disentangle covariate effects for the different transitions. The next section therefore turns to regression results.

4.5.2 Regression results

Table 4.8 provides the results from the main multi-state model that I run.³⁴ Results in bold are those that are directly related to my expectations. Positive coefficients indicate that increases in a variable are associated with increases in the probability that the transition occurs. This also means that the results do not provide unconditional probabilities (i.e. they do not directly indicate how a variable affects the probability

³⁴The model results are robust to more restricted models with only country-level or group-level covariates and models with alternate methods for tie handling, as discussed in (De Wreede et al., 2010). The results in the more restricted models are also stronger in favor of my expectations; what is reported in this table is a more conservative finding.

of war, but rather of war from NV or LIV).³⁵ Table 4.7³⁶ provides a summary of the expectations and the results. In general, the results are consistent with the broad argument I have made about causal heterogeneity and the need to disaggregate conflicts by stages, but the support is more limited in regards to my arguments about state capacity and rebel group type. Many of the results are suggestive that my theory might be correct, but they lack statistical significance. It is highly likely that this is at least in part because this approach increases the number of coefficients that have to be estimated (by a factor of 9), which quickly eats through the model’s power.³⁷ One key implication is that further testing on a larger dataset would be very useful.

Table 4.7: Summary of Expectations & Results

	State Capacity	Group Strength	Supported?
P(LIV NV)	-	+	Yes
P(War LIV)	+	-	Yes (base) & No (GDP)
P(NV LIV)	-	+	Mixed
P(LIV War)	-	+	Expected direction
P(NV War)	-	+	Expected direction
Number of fluctuations	-	+	Yes, descriptive
P(War NV)	NA	NA	NA
P(Resolved NV)	NA	NA	NA
P(Resolved LIV)	NA	NA	NA
P(Resolved War)	NA	NA	NA

³⁵Note also that there are no results for the LIV→Resolved and War→resolved transitions. This is because the number of covariates balloons very quickly—one variable gets stratified into 9 different variables to account for each transition—and so there are not enough degrees of freedom to estimate these two very rare outcomes.

³⁶The results for the number of fluctuations are based off of the descriptive statistics and not supported by hypothesis testing but are suggestive that the expectation is correct.

³⁷This is further supported by the fact that the results increase in statistical significance in more restricted models.

Turning specifically to how the results play out for my different expectations, there are several points that merit attention. First, groups with a territorial base are more likely to reach the onset of LIV, less likely to escalate from LIV to war, and more likely to de-escalate from LIV to no violence. They are also more likely to de-escalate from war to either no violence or LIV. These results are not always statistically significant but are substantively quite large: for example, a group with a base is more than three times as likely to de-escalate from war to a period of no violence. The signs are also consistent with the hypotheses. The first two findings are also consistent with the Heckman selection model results: stronger groups are more likely to move from no violence to the onset of LIV, but if they do, they are less likely to escalate to war. This, combined with the fact that groups with a base are more likely to de-escalate to NV or LIV, lends credence to the idea that governments prefer to try and work things out with stronger rebel groups. However, this exact mechanism – that the conditional decrease in violence among stronger groups is due to negotiations – cannot be confirmed by this testing, but it is an additional piece of evidence that helps support the earlier empirical tests, the formal model, and the various examples provided.

Second, the results for GDP per capita are mixed. While it is in the expected direction in many of the transitions – higher GDP is associated with a lower probability of moving from no violence to LIV and a lower probability of de-escalating from war (whether to no violence or back to LIV) – the results on the transitions from LIV (to war and to NV) are in the opposite directions as expected. States with higher GDPs

are less likely to experience onset; this is consistent not only with my theoretical argument about greater state capacity deterring most challengers but also with the vast majority of research on the causes of civil conflict. What is more puzzling is the relationship between GDP per capita and de-escalation, which appears to be quite nuanced: if states with higher GDP per capita do experience war, they are less likely to de-escalate, but it seems that they are also less likely to experience a shift from LIV to war. This suggests that higher capacity states are indeed less willing to back down once they are involved in a conflict, as I would expect. Yet that tendency does not necessarily kick in with LIV. This is puzzling, in part because the lower probability of de-escalating from war seems at odds with the greater probability of de-escalating given LIV. It is possible that these results offer a refinement of the earlier theory: states with higher capacity are less likely to back down once in a war, but continue to more effectively deter or eliminate challengers that make it to LIV.

Third, these results speak more broadly to a key point: a single variable often has a different effect on the probability of an outcome (e.g. war) depending on what part of the conflict it is in. For example, the relationship between democracy and whether or not a dyad experiences LIV depends on whether or not the dyad is currently experiencing no violence (in which case LIV becomes less likely) or war (in which case it becomes more likely).³⁸ This is particularly important for studies that use incidence of violence, as it suggests that doing so can conflate very different

³⁸One notable exception is the group's relative size, which is consistently negative across all of the transitions, escalation and de-escalation alike. This is the only variable whose sign does not change at all across the stages and merits further study to better understand the results.

Table 4.8: Multi-state Models: Results

	NV → LIV	NV → War	NV → Res	LIV → NV	LIV → War	LIV → Res	War → NV	War → LIV	War → Re
GDP	-0.447*** (.132)	-0.56*** (.210)	-1.18*** (.334)	0.062 (.155)	-0.17 (.176)	-	-0.358 (.316)	-0.201 (.243)	-
T. Base	0.389 (.557)	3.058 (2.684)	-0.718 (1.194)	0.162 (.715)	-1.149* (.613)	-	3.041 (4.773)	0.423 (.851)	-
Rel. Size	-0.01 (.112)	-0.142 (.171)	-0.072 (.242)	-0.322** (.125)	-0.354** (.157)	-	-0.478* (.252)	-0.076 (.204)	-
Econ Dis.	0.199* (.102)	-0.129 (.163)	-0.077 (.223)	-0.282** (.114)	-0.092 (.146)	-	-0.100 (.205)	0.049 (.163)	-
Kin	0.225 (.278)	0.764 (.487)	-0.841 (.536)	0.145 (.340)	0.464 (.433)	-	-0.873 (.871)	0.940 (.648)	-
Polity2	-0.006 (.025)	-0.02 (.040)	0.104* (.059)	-0.047* (.028)	-0.006 (.032)	-	0.027 (.040)	0.002 (.031)	-
Oil	0.453 (.346)	1.103** (.523)	-1.877 (1.461)	-0.25 (.486)	0.175 (.545)	-	1.324* (.77)	0.153 (.755)	-
Mountains	-0.123 (.134)	-0.029 (.200)	-0.176 (.241)	-0.135 (.130)	-0.252 (.157)	-	-0.076 (.215)	0.093 (.178)	-
Population	-0.028 (.095)	-0.081 (.158)	-0.224 (.211)	-0.005 (.112)	-0.062 (.140)	-	-0.270 (.244)	0.413** (.198)	-

Note: *p<0.1; **p<0.05; ***p<0.01. Standard errors in parentheses. N = 1584.

effects. Indeed, not only do the signs often change, but so too do the substantive and statistical significance of the coefficients. Disaggregating a conflict into its stages yields a much more nuanced understanding of the factors that influence each part of the cycle. Unfortunately, it is also important to note that doing so comes at a cost: using a stage approach drastically increases the number of variables to be estimated in the model, as GDP (and territorial base, relative size, etc.) becomes stratified into each transition-specific variable. This privileges having larger datasets, making the approach difficult to apply in the context of a single country (even if the conflict lasts decades).

4.6 Conclusion

This chapter has extended the theoretical and empirical analysis from the first two chapters by developing a holistic framework for understanding civil conflicts. I have argued that a stage approach—disaggregating conflicts into separate phases like LIV and war that can be recursive and/or sequential—provides greater nuance when trying to understand how a conflict unfolds. Importantly, a stage approach also helps mitigate the issues that can emerge by studying only one part of a conflict in isolation.

Theoretically, I have focused on the role of the information environment, state capacity, and rebel group type to understand when conflicts are likely to experience shifts in their stage and when they are likely to experience many such transitions. I have drawn on the formal models of learning through war literature to argue that, broadly speaking, shifts in the information environment can lead to shifts in the

conflict stage: de-escalation is particularly likely when the state gains new information about the rebel group that makes negotiations more useful vis-a-vis fighting. Indeed, I extend the logic to argue that stronger rebel groups will be more likely to de-escalate, as the state will be more willing to try and negotiate. Conversely, stronger states will be less likely to de-escalate, as it will take more—battlefield losses, new rebel group tactics, etc.—to get them to update their prior confidence in winning. Stronger states will also be less likely to experience a greater number of transitions throughout the conflict; it is conflicts with strong rebel groups, who can be militarily active for long but will still have trouble overcoming the inherent power asymmetry, that will be more prone to many cycles.

I have used a relatively new empirical approach to address these theoretical arguments. Multi-state survival models are a broad class of survival models that specify baseline hazard rates for each transition, leading to transition-specific covariates. This allowed me to provide a unique assessment of civil conflicts: to my knowledge, this is the first study that has disaggregated civil conflicts into different stages and provided a broader understanding of how conflicts wax and wane. Indeed, this is not a rare phenomena – over ten percent of the conflicts experienced over ten transitions – which underscores how important it is to be able to separate out what drives a transition, given where a conflict currently is in the cycle. Covariate effects on LIV, war, and NV all depend on the current stage: as expected, groups with a territorial base are more likely to shift from no violence to war, but less likely to transition from LIV to war.

Because this is the first study to take this approach to studying civil conflicts, there are several steps that can and should be taken going forward. First, I have chosen to be consistent with the rest of the dissertation and focused on self-determination movement conflicts, but it is important to apply this approach to other types of civil conflicts. The caveat here is that the transition-specific covariates quickly use up degrees of freedom, meaning that any study will need to be cross-national rather than restricted to a single country or small region. Second, I have focused on two variables, state capacity and group strength, but it is also important to understand how other factors influence conflict cycles. Third and relatedly, this approach should be particularly useful to peacekeeping and conflict management scholars, because it provides the ability to distinguish between de-escalation from (or recurrence of) LIV and war.

Chapter 5

Conclusion

This dissertation has advocated for a reexamination of how scholars approach the study of civil war, both theoretically and empirically. To date, most studies of the causes of civil conflict either look at the causes of war, overlooking all of the strategic interaction, learning, and resource changes leading up to war, or focus on the start of LIV, without then addressing how the conflict subsequently evolves (or how the prospect of future behavior might shape whether or not the LIV occurs in the first place). I have argued for the need to analyze LIV and war within a single conceptual (and empirical) framework, one that distinguishes between the two different stages but also accounts for how they shape each other. To do so, I have relied on multiple methodological approaches, using a formal model to develop the theory, drawing on historical examples to provide context, and using multiple types of statistical analysis to test the various hypotheses developed throughout the dissertation.

The second chapter focused on the onset and escalation of intrastate conflict, using formal modeling to develop a theory about the drivers of low intensity violence and war. The chapter is the first, to my knowledge, to establish a theoretically-driven

definition distinguishing between LIV and war; I argue that the two are differentiated by the scope of violence, level of mobilization, and amount of information. The chapter then turns to a two-player formal model of a dynamic game with incomplete information and two stages of informational updating. The game is played between a government and a prospective opposition group that is one of three types, inactive, weak, or strong, and I focus the analysis on the most informative equilibrium, where only the strong type of opposition group violently mobilizes after governmental repression. This encourages the government to repress in order to get a signal of opposition type and enables the government to update its beliefs after the group's mobilization; confident that it faces a strong group, the government prefers to negotiate rather than escalate, avoiding war.

The analysis focuses on three factors—the information environment, state capacity, and rebel group type—that are pivotal to LIV and war but also have different effects at each stage. Here, one of the main contributions is to argue for the importance of the information environment facing the state. I draw on examples ranging from Uganda to the United States to demonstrate that the state faces an extreme informational asymmetry much more severe than in interstate contexts, such that the state initially is uncertain about not only the rebel group's type (resolve and capability) but also fundamentally about if there is even a group that is sufficiently willing and capable of violently mobilizing to contest the state. Second, the model shows that while stronger states are less likely to experience LIV because they deter more challengers, if a challenger does emerge in a stronger state, then the government will

be less willing to negotiate with rebels. Higher capacity states are more willing to go to war precisely because they are stronger and expect a higher chance of winning. Third, the model shows that many weaker prospective rebel groups will strategically self-censor, opting out of mobilization if they expect a strong government response (or getting eliminated by government repression). If a rebel group can successfully commit LIV, that violence provides critical information to the state, enabling the state to update its information about the group's type. When the state is more confident that it faces a strong, resolved threat, it is more likely to want to bargain with that group in order to avoid escalation to a costlier conflict. Thus, the model expects the strongest groups to be more likely to reach LIV but, counter-intuitively, less likely to escalate to war.

The third chapter further motivates this approach – of differentiating between co-analyzing LIV and war – by digging into the methodological concerns associated with empirical tests that lump LIV and war together. It draws on international relations literature that has assessed similar issues in statistical analyses of conflict between countries to argue for a statistical model that accounts for the selection bias that emerges in studies that focus solely on war. The chapter tests the theoretical arguments developed in Chapter 2 with data on self-determination movements worldwide from 1960-2005; I operationalize state capacity via GDP per capita, and I use a dichotomous variable of whether or not a group has a territorial base in order to capture rebel group strength. There are two main sets of results. The first replicates canonical civil war studies with the self-determination data and compares those replication

findings to results using a selection model. The findings demonstrate how important it is to use a more nuanced empirical approach, as several key variables have different effects for LIV than war. Most notably, the findings undermine the conventional wisdom that higher GDP per capita is correlated with a lower risk of war: while states with higher GDP are less likely to experience LIV, if they do experience the onset of a conflict, they are in fact *more likely* to escalate to war. This is consistent with the theoretical argument developed in Chapter 2, and echoed in the second set of main results in Chapter 3. There is also strong support for my argument about group strength – proxied by territorial bases – i.e., that stronger groups are more likely to conduct LIV, but then less likely to experience war, because states will be more inclined to try and negotiate with them.

Chapter 4 builds and extends on the first chapters by moving beyond the first two stages of a civil conflict. Here, I adopt a stage approach to the entire civil conflict, from the emergence of a challenge before any violence has occurred, through to the resolution. I extend the theory developed in Chapter 2 to identify and define the four stages of a civil conflict: no violence, LIV, war, and resolution. I establish expectations for how state capacity and group strength will likely drive key transitions in a conflict, arguing that stronger rebel groups will be more likely to de-escalate, as the state will be more willing to try and negotiate, while stronger states will be less likely to de-escalate, as it will take more—battlefield losses, new rebel group tactics, etc.—to get them to update their prior confidence in winning. Conflicts with strong rebel groups, who can be militarily active for a long time but still have trouble

overcoming the inherent power asymmetry against the state, that will be prone to more transitions and fluctuations.

To address these arguments, I use a relatively new statistical approach known as multi-state survival modeling to test the implications on the self-determination movement data used in Chapter 3. The results provide mixed support for my hypotheses but underscore the importance of a stage approach, as a variable often has a different effect on the probability of an outcome, such as war, depending on what part of the conflict it is in. Indeed, one of the key points from the third chapter is that civil conflicts often fluctuate—one conflict had twenty one transitions from its start to resolution. While twenty one transitions is extreme, many conflicts do wax and wane repeatedly; over ten percent of the conflicts experienced more than ten transitions. This matters because it demonstrates how essential it is to be able to separate out what drives a transition, conditional on where the conflict currently is in its cycle.

Overall, this dissertation demonstrates the importance of conceptualizing the civil conflict process within a single framework that not only acknowledges the distinctions between each stage but also accounts for how they influence each other. This suggests that some of the key findings in the civil conflict literature warrant reconsideration and further attention. First, as I have emphasized throughout the dissertation, is the role of GDP per capita and state capacity. The idea that GDP per capita is negatively correlated with civil war onset is perhaps the single most consistent finding throughout the civil war literature, yet this dissertation shows that the relationship is not so straightforward. Stronger states do deter more challengers, facing a lower risk

of the start of minor conflict, but are more likely to escalate if they are confronted with a challenger. Second, many of the results suggest that the relationship between democracy and conflict likewise merits additional research. In the comparison between the standard probit and censored probit models, democracy's relationship with conflict flips once LIV is accounted for. The standard findings suggest democratic countries are less likely to go to war, but results in Chapter 3 suggest that democratic countries might experience less minor conflict, but when they do, they might be more likely to escalate. This possibly parallels some of the interstate conflict literature on the democratic peace that suggests that while democratic countries might be less likely to enter into a conflictual dyad, they are no more or less likely to escalate than their autocratic counterpoints (Senese, 1997). Third, the results from Chapter 4 also suggest that our understanding of peace processes and conflict resolution should incorporate not only the conflict's trajectory but also its current stage. The chance of a resolution depends on whether or not the conflict is at the height of hostilities or currently experiencing a (potentially temporary) lull, which has important implications for academics and policymakers alike.

Taken collectively, this dissertation suggests two main lines of research moving forward. First, the third chapter demonstrates the potential with a stage approach to conflict. Yet much remains unanswered: what are the implications for conflict resolution? For violence against civilians? What affects the length of time in a stage, and how does that stage duration in turn shape the conflict and its outcomes? Because this new approach yields much more nuanced results and insights, there is much to

analyze and theorize moving forward. Indeed, it would also be interesting and useful to use some of the outputs of the regression – for example, the number of transitions in a conflict – to investigate post-conflict phenomena such as recurrence, peace duration, etc. More broadly, I have analyzed conflicts involving self-determination movements, but an essential next step will be to broaden the scope to other types of civil conflicts (or even interstate conflicts). This is a potentially large and fruitful research agenda containing several individual projects.

Second, one of the key points of the theoretical model in Chapter 1 is about the role of the information environment. The model underscores the informational asymmetry facing the state and demonstrates how that influences conflict escalation and negotiations. It points to the central role of the state’s surveillance infrastructure. This is a highly understudied topic in conflict literature, in large part because no data exists (to my knowledge) that would allow scholars to systematically analyze variations in state surveillance capabilities and what those differences mean for conflict.

Indeed, the finding I am most interested in pursuing moving forward is about how variations in the state’s information environment affect its decisions to bargain or resort to violence. I intend for my next major project to dig into this question. Doing so requires not only theoretical development but also a large data effort: the project will start with building an original, cross-national dataset on state surveillance capabilities. This is still tricky, as states often do not fully or clearly disclose their intelligence apparatus, but certain types of information are typically available. For

example, does the country merge its police and foreign surveillance forces? Does it have signals intelligence agencies? Does it have intelligence sharing agreements or treaties with other countries? Can it access/surveil all areas within its territory?

Gathering this data – which has never been done before in political science, to my knowledge – will allow me to explore how both cross-national and temporal variations in surveillance infrastructure affect a states propensity for civil violence, the strategies it prefers to use, and its repressive behavior more broadly. There are fascinating and important interstate implications, too: does stronger surveillance capacity make a state more or less likely to engage in interstate war? How might surveillance capacity influence crisis behavior? Pursuing this research agenda allows me to build off of my dissertation work while still investigating new, intriguing puzzles that promise both academic and policy contributions.

Appendix

There are many possible equilibria. This proof focuses on a separating equilibrium where the government initially represses, only strong groups mobilize, and then the government negotiates and the group accepts, avoiding escalation to war. Formally, this is an equilibrium with the following strategies:

- Government: Repress, Negotiate
- Opposition, type IA: [Don't Mobilize|Repress; Don't Mobilize|Accommodate]
- Opposition, type Strong: [Mobilize, Accept|Repress, Negotiate; Fight|War; Mobilize, Fight|Accommodate, War, Negotiate]
- Opposition, type Weak: [Don't Mobilize|Repress; Mobilize, Accept|Accommodate, Negotiate; Fight|War]

1. By backwards induction, we first need to solve for when the opposition prefers to accept the state's negotiation or fight. This occurs when $EU_o(\text{Accept}|\text{R}, \text{M}, \text{N}) > EU_o(\text{Fight}|\text{R}, \text{M}, \text{N})$, or $\gamma - c_m - c_{GR} > p_{win}(0) + (1 - p_{win})(1 - c_m - c_{GR} - c_f)$, which reduces to $\gamma > (1 - p_{win}) - c_f + p_{win}(c_m + c_{GR} + c_f)$.

2. Likewise, we need to identify when the opposition prefers to fight or surrender, if the state goes to war. $EU_o(F|R, M, W) > EU_o(S|R, M, W)$ if $p_{win}(0) + (1 - p_{win})(1 - c_m - c_{GR} - c_f) > 0 - c_m - c_{GR}$, or $1 > p_{win}(1 - c_m - c_{GR} - c_f)$, which is always true. The opposition always prefers to fight (regardless of type).

3. Now we can solve for when the government prefers to negotiate over going to war. Note that, given the separating strategy of the opposition, the government updates its beliefs after mobilization as follows: $P(\text{Strong}|M) = 1$, $P(\text{Weak}|M) = 0$, $P(\text{Inactive}|M) = 0$. We can now solve for when $EU_g(N|R, M, \text{Accept}) > EU_g(W|R, M, F)$, or $1 - \gamma - c_r > p_{win}(1 - c_r - c_w) + (0)(1 - p_{win})$, which reduces to when $p_{win} < p^* = \frac{1 - c_r - \gamma}{1 - c_r - c_w}$. The state prefers to negotiate when it is more uncertain of its chances of winning, and it knows that the opposition will accept an offer but fight if faced with war.

4. Now, we need strong Opposition types to prefer to mobilize given these outcomes, while weak types prefer nonviolence. $EU_{strong}(M|R, N, A) > EU_{strong}(NM|R, N, A)$, or $p_{dies}(0 - c_m - c_{GR}) + (1 - p_{dies})(\gamma - c_m - c_{GR}) > 0 - c_{GR}$, or $c_m < c_m^* = \gamma(1 - p_{dies})$. The opposite is true for weak Opposition types: using the same equation, $EU_{weak}(NM|R, N, A) > EU_{weak}(M|R, N, A)$ if $c_m > c_m^* = \gamma(1 - p_{dies})$. Note that weak type has higher c_m than strong type. For IA type, $c_m > 1$, so NM is always strictly preferred.

5. **Off the path behavior.** Before we can show that G prefers R over A, we must identify the off-the-path behavior. Consider the following off the path behavior: G accommodates, both weak and strong types of O mobilize (IA does not), both

fight if faced with war, but weak accepts negotiations whereas strong will fight. If G accommodates then negotiates, $EU_o(F) > EU_o(Accept)$ if $p_{win}(0) + (1 - p_{win})(1 - c_m - c_f) > \gamma - c_m$, or $p_{oppVictory} > \frac{\gamma - c_m}{1 - c_m - c_f}$. Likewise, if G accommodates and then goes to war, $EU_o(F) > EU_o(S)$ if $p_{win}(0) + (1 - p_{win})(1 - c_m - c_f) > 0 - c_m$, or $p_{oppVictory} > \frac{-c_m}{1 - c_m - c_f}$. However, we also know that the weak type would prefer not to mobilize over mobilizing and then surrendering (given accommodation and war), because it is never true that $EU_{weak}(S|A, M, W) > EU_{weak}(NM|A)$, because it is never true that $p_{dies}(0 - c_m) + (1 - p_{dies})(0 - c_m) > \kappa\gamma$, which reduces to $-c_m > \kappa\gamma$. This means that $EU_g(W|Accom, M, F) > EU_g(N|Accom, M, Accept \text{ if weak, Fight if strong})$, or $p_{win}(1 - c_w) + (1 - p_{win})0 > p(\theta = L)(1 - \gamma) + (1 - p(\theta = L))p_{win}(1 - c_w)$, or $p_{win} > \frac{1 - \gamma}{1 - c_w}$. Now, we just need to show that the strong type prefers to fight given accommodation and negotiation over not mobilizing, whereas the weak type prefers to accept given accommodation and negotiation over not mobilizing. For the first condition, we need $EU_{strong}(F|Accom, M, N) > EU_{weak}(NM|Accom)$, or $p_{die}(0 - c_m) + (1 - p_{die})[p_{win}0 + (1 - p_{win})(1 - c_m - c_f)] > \kappa\gamma$, or $p_{die}(-c_m) + (1 - p_{die})(1 - p_{win})(1 - c_m - c_f) > \kappa\gamma$. For the second, we need $EU_{weak}(Accept|Accom, M, N) > EU_{weak}(NM|Accom)$, or $p_{die}(-c_m) + (1 - p_{die})(\gamma - c_m) > \kappa\gamma$, or $\gamma(1 - \kappa - p_{die}) > c_m$.

6. To show that the Government prefers R over A given the off-the-path behavior in step 5, we need $EU_g(R|M, N, Accept) > EU_g(Accom|M)$, or $p(\theta = strong)[p_{dies}(1 - c_r) + (1 - p_{dies})(1 - \gamma - c_r)] + p(\theta = NOTstrong)(1 - c_r) > p(\theta = strong)[p_{dies}(1 - \kappa\gamma) + (1 - p_{dies})p_{win}(1 - c_w)] + p(\theta = IA)(1 - \kappa\gamma) + p(\theta = W)[p_{dies}(1 - \kappa\gamma) + (1 - p_{dies})(1 - \gamma)]$,

which reduces to a prior where $p(\theta = strong) > \frac{p(\theta=IA)(c_r-\kappa\gamma)+p(\theta=W)[c_r-\gamma(1-p_{dies}(1-\kappa))]}{1-c_r-\gamma-p_{dies}(1+\gamma(1-\kappa))-p_w(1-p_{dies})(1-c_w)}$.

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Biography

Kaitlyn Webster graduated from the University of Rochester in 2011, where she received a B.A. in International Relations, earning the James D. McGill Memorial Prize for the undergraduate with highest achievement in political science. She graduated *summa cum laude* and *Phi Beta Kappa*. She worked for several years as an intelligence analyst for the Department of Defense and the National Intelligence Council, where she authored long-term strategic analyses in support of the NIC's Near East portfolio and conducted research in support of a wide variety of analytic products, including National Intelligence Estimates. She received several awards for her work.

At Duke, she has been a James B. Duke Fellow and an NSF Graduate Research Fellow Honorable Mention. Her work has been published in *International Organization*. Part of her dissertation won the 2017 Stuart A. Bremer Award for Best Graduate Student Paper and has been featured in the Online Peace Science Colloquium. Her work has been supported by the Folke Bernadotte Academy, the National Science Foundation, an APSA Travel Grant, an Alona Evans Research Grant, and the Duke Graduate School's Summer Research Fellowship.