Rethinking Civil War Onset and Escalation

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Abstract

Why do some civil conflicts simmer at low-intensity, while others escalate to war? This paper challenges traditional approaches to the start of intrastate conflict by arguing the need to distinguish both theoretically and methodologically between the onset and escalation of civil conflict. I develop a novel, strategic argument about two causal mechanisms that differentially drive low-intensity violence (LIV) versus full-blown war: the information environment and the types of rebel group in operation. I posit that before LIV, the state often does not even know the identity of its challenger, making bargaining highly unlikely; negotiations become more probable as the state gains key information through violence. This is reinforced by strategic behavior by the prospective rebel groups, whereby only the strongest groups make it past LIV. If this approach is correct, it forces a reexamination of the seminal findings in civil war literature by showing how failing to properly account for LIV when examining war has led to inaccurate results.
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 even more (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 outright civil war.\(^1\) Indeed, even within countries, some self-determination disputes experience no violence, while others suffer long civil wars. For example, in Nigeria, the Yorubi movement has remained nonviolent, while the Biafran separatist movement developed into a brutal rebellion. What can explain this variation?

This article challenges existing approaches for understanding the start of civil conflict, and it develops an explanation for why some conflicts are likely to reach only low-intensity violence (LIV), while others escalate to war. 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. I focus on two key mechanisms that distinguish and drive LIV and war: the information environment and the type of opposition group that is operating.\(^2\) 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

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

\(^2\)These are not the only possible mechanisms; others are reserved for future research.
group exists). This is a key, unstudied difference between intrastate conflict and interstate conflict, where the identity of both sides is always known. For substate violence, the state gains information through conflict, so that after LIV is reached, the information environment shifts and improves, making bargaining and negotiations more likely. This information shift helps explain temporal variation in conflict, while variation in a state’s ability to gather information (e.g., its intelligence apparatus) helps explain cross-national variation.

For the second mechanism, group type, I argue 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. In other words, 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.

If it is true that LIV affects a conflict’s escalation to war (and vice versa), then there are strong implications for existing research on the causes of civil conflict. 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.
It is particularly imperative to control for LIV when examining war; failing to do so 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 so many of the foundational studies in civil war research overlook: they analyze when full-blown civil wars emerge, without first controlling for the start of violence and thus require reassessing. I therefore also reexamine seminal findings about the causes of civil war to show that the relationships are much more nuanced than previously shown.

To test my arguments, I focus on self-determination groups, 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-group dyads. I choose self-determination disputes because they are a particularly important category of intrastate violence. They are the most common type of war in general (Cunningham, 2013), and they are also particularly vulnerable to internationalization (Gleditsch et al., 2008; Saide-man, 2012). Focusing on self-determination movements also provides a clean research design, because there is a clear set of cases where no violence occurs at all, another where only low-level violence occurs, and a third where dyads experience escalation to war. This allows me to make initial comparisons between dyads with no conflict and those with LIV, and then subsequently between dyads with LIV and those with war.

This paper therefore makes several key contributions. First, I develop a novel theoretical

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3This 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).
framework for understanding the interplay between LIV and war in sub-state conflict. This framework lays out fundamental substantive differences between these two stages, filling in critical theoretical gaps in the literature. If we cannot fully and precisely understand the differences between the two, then we are likely to fundamentally misunderstand what puts a country at risk for different types of intrastate violence. Second, this framework allows me to develop a key distinction in the bargaining environment between inter- and intra-state conflict: incomplete information in the international context means that actor A and B know that they face each other but do not know certain characteristics about the other side (type, payoffs, etc.). In sub-state conflict, information issues are even more basic and mean that sometimes the state does not know who the other actor is. Third, 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. For example, the canonical wisdom is that GDP per capita is 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.

The paper proceeds as follows. First, I discuss what we know about the causes of civil conflict. Next, I lay out a new theoretical framework that explains how LIV and war are distinct but interconnected stages in the broader civil conflict process. I then use that framework to develop observable implications and discuss the ramifications for existing research. The following section describes the research design, after which I proceed to results and discussion. The final section concludes.
Causes of sub-state conflict

The literature on the causes of civil conflict is extensive, although it has focused primarily on understanding why states experience war. 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 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). 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.

4It 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).

5Note that the two studies disagreed about what GDP per capita might represent (economic grievance or lack thereof, state capacity, etc.).

6See also Miguel et al. (2004) and Ross (2004), who argue that poor economic performance matters not for opportunity costs but because it generates discontent.
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 midlevel democracies were actually more at risk than their autocratic counterparts (Hegre et al., 2001; Reynal-Querol, 2002; Sambanis, 2001).

The research on civil war causes has deepened our understanding of intrastate conflict. However, they treat 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 they can explain cross-national variation, they cannot account for within country differences, or explain why specific conflicts emerge at specific points in time.

We still know little about low-intensity violence. While a newer body of literature has started to address the causes of LIV (Melander et al., 2009; Ritter and Conrad, 2016; Sullivan, 2015, 2016), 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, if any, are the differences between the two, and how does one affect the other? Historically, most studies looked at civil war ‘onset’ as when a country first experienced 1,000 or more

7Note that the relationship is more complex between democracy and the conduct and termination of war (Hultman, 2012).
battle deaths in a year; more recently, researchers have adopted a 25 battle death threshold without distinguishing low-level violence from war. 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). Using the 1,000 battle death cut-off overlooks this violence. It also creates methodological problems. Specifically, it 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. Since selection bias can drive coefficients towards zero, 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.

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

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8Selection 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.

9I expand on these issues in subsequent sections.
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); 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.

Much work remains to be done on how this strategic interaction can affect the onset and escalation of civil conflict.

**Low-intensity violence or war?**

I argue that LIV and war are qualitatively different stages of the broader substate 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 acquired to reach LIV – whether arms, funds, or civilian support – can then be used to reach or sustain war. Considering both 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).\footnote{I provide support for this claim in the empirical analysis.} We could imagine similar stories for many different mechanisms, from democracy to rebel group strength. I focus on two mechanisms in this article: the type of rebel group in operation, and the information environment. Both of these mechanisms affect the probability of conflict by changing a state’s relative costs of bargaining versus violence.

**Rebel groups and strategic mobilization**

I view the state and (prospective) rebel group(s) as strategic actors. This means that it is important to account for both parties – instead of examining one side in isolation – and their strategic incentives. By strategic, I mean that each side is self-interested and payoff maximizing. The actors are also forward-looking in that they take into account the other side’s behavior (both expected and actual). These assumptions are well-grounded in both the interstate (Achen and Snidal, 1989; Banks, 1990; Bueno de Mesquita et al., 1997; Fearon, 1994, 1995; Filson and Werner, 2002, 2004; Huth and Russett, 1988; Meirowitz, 2015, 2016).
et al., 2008; Sartori, 2002) and, to a lesser extent, the intrastate violence literature, where they have been most frequently applied to understand actor behavior during conflict or conflict termination (Cunningham et al., 2009; Cunningham, 2006, 2013; Hultman, 2012; Kalyvas and Kocher, 2007; Walter, 2002, 2006, 2009).

More specifically, if we consider the universe of all prospective rebel groups – all groups of citizens with some shared grievance– only some subset will inflict violence. The lack of violence might occur for ideological reasons, but it may also be strategic. Strategic considerations are pivotal: the expectation or threat of violence from the state can lead many groups to choose nonviolence or no action at all. 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. During India’s federal reorganization in the 1950’s, weak linguistic groups did not try to mobilize for statehood because they feared repression (Lacina, 2014). Similarly, a group might start to mobilize but never actually make it to LIV, especially if the state does choose repression and is able to eliminate the group (kill, capture, or prohibitively raise the costs). In a study of all rebel groups that formed in Uganda after 1986, Lewis (2017) finds that over half of all groups failed early, before committing enough violence to reach the 25 battle death threshold of LIV.

This 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. 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. Note that this does not mean that no strong groups ever escalate to war. Rather, I mean that when we hold all other factors constant – such as a group’s demands or the prospect of future conflict (Walter 2006) – we should expect that strong groups, conditional on having reached LIV, are less likely to escalate to war. Adding in other those other factors should help explain the rare events of war.

The information environment

Changes in the relative cost to the state of bargaining versus violence against an opposition group help distinguish differences between LIV and war. As we have already seen, one
mechanism that changes those costs is the type of group operating at each stage. Another pivotal mechanism is the information environment available to the state. Here, I mean not only the state’s knowledge of the group’s capacity or resolve, but also *who that group is*. 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). This is often very different in the intrastate context. When a group is first forming, it often prefers to operate in secrecy (CITE LEWIS 2017). Consequently, 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.

For a state to negotiate with a specific group, it has to be able to identify that group. 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. If the state cannot identify a specific opponent, they cannot negotiate with them (or selectively target them with violence), which makes LIV more
likely. This helps explain cross-national variation in LIV: states with stronger surveillance infrastructure should enjoy better information environments and therefore be more capable of eliminating or mitigating threats before any violence.

The information environment also explains temporal variation in substate conflict. As a group starts committing violence, the state gains significant information. First, it learns that there is definitely an active, viable rebel group. Identifying the particular opposition group becomes much easier for the state: the state might not know all of the individual members, but it likely can now identify the group and the group’s goals. This substantially decreases the logistical costs to the state that are associated with bargaining. The state no longer has to spend resources trying to figure out who it might need to bargain with. It can now be more precise in its surveillance and information gathering, which is much less costly. Note, however, that even though the state can identify the group, violence aimed at eliminating the group might not be cheap. This is not only because of the group’s capabilities, but also because the precision of information required to sufficiently identify specific locations and/or member identities in order to eliminate the group is still extremely expensive. Consequently, bargaining usually becomes cheaper than violence. We can expect to see more bargaining after LIV than before, ceteris paribus.

The role of the state

How does the state factor into this analysis? Civil war scholars have repeatedly shown that GDP per capita is very important for whether a country will experience civil war, with low GDP per capita signaling greater risk for civil conflict (Collier and Hoeffler 2000; Fearon and Laitin 2003). However, my approach implies that this relationship might not
be so straightforward. What matters for determining LIV might have a different effect – or none at all – on war.

I argue that there is a particularly strong selection effect at play with respect to a state’s capacity. Stronger states are indeed less likely to experience onset, largely because they deter challengers. *Ceteris paribus*, groups will be less willing to mobilize violently against a more capable opponent, because the costs will be much higher. This is consistent with existing research that uses GDP per capita as a proxy for state capacity.

However, the effect of state capacity changes once LIV has occurred. Consider the case of a strong state. Most of the time, the strong state will not face any challengers, because prospective rebels will be sufficiently deterred. But, if a challenger does emerge to commit LIV, a stronger state will behave differently than a weaker one: it will be less willing to negotiate with rebels and more willing to escalate the fight, precisely because it is stronger. The greater strength means that the state should expect a higher probability of winning vis-a-vis the rebel group and therefore be more willing to fight. Taken together, this line of reasoning yields the following hypothesis:

**Stronger states are less likely to experience LIV than weaker states. However, if a strong state does face LIV, it is more likely to escalate to war than a weaker state.**

**An observable implication: territorial strongholds**

Developing a testable hypothesis about state capacity was relatively straightforward, but doing the same for the information and group type mechanisms is less straightforward, because they are not as clearly observable. We cannot directly identify the information
environment or the group’s type, so we must search for observable implications. I argue
that one key factor that is related to both the information and group type mechanisms
– and correspondingly influences both LIV and war – is whether or not the rebel group
has a territorial base.\footnote{This is by no means the only type of strategic factor that affects LIV and war.}

\footnote{Lichbach (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.}

\footnote{Some 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.}

\footnote{Why then don’t all groups have a territorial base? To start, not every group can: not all opposition movements (including self-determination ones) are regionally concentrated \cite{Cunningham2013, MAR2009}. Bases come with costs, too, ranging from governance costs \cite{Arjona2015, Arjona2016} to becoming a greater target of government repression, since weaker groups are often left alone \cite{Lacina2014}.}

From the opposition’s perspective, a territorial base can improve capability in two ways. First, it can help solve coordination problems associated with collective action \cite{Collier2000, Herbst2000, Lichbach1995, McColl1969, Raleigh2009}. Lichbach \citeyear{Lichbach1995} 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.\footnote{A territorial base is particularly helpful as it is often correlated with a concentrated, supporting population \cite{Raleigh2009, Collier2000}. 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 capacity.}

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, the state can more quickly
focus its resources on citizens within or around the base. 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 (or not).

From the state’s perspective, an opposition movement that has a territorial base is
a more dangerous challenger, largely for the reasons discussed above. A more organized
group is stronger, and it can be harder tactically to move against or eliminate a stronghold.
How might a state respond to this type of challenger? I hypothesize that the state will
try to anticipate and prevent such a stronger challenger from emerging, but if one does
develop, the state will try to bargain or accommodate the challenger. This is because the
state will recognize the higher conflict costs associated with fighting a stronger opponent,
all else equal. This is also broadly consistent with existing research on state repression:
studies have shown that states first try to repress and prevent challengers from emerging
but will also accommodate those that have the potential to be very strong (Lacina, 2014;

Why do rebel groups with a territorial base accept these concessions? First, it is
possible that the government offers exactly what the movement is asking for. Not all rebel
groups are seeking succession, so those with milder grievances could have demands that
the government is fully willing to accommodate to avoid the costs of fighting a stronger
challenger. This is precisely what happened with India’s federal reorganization in the
1950’s: various ethnic groups wanted their own state (akin to U.S. states, not independent
countries) within India, and the government was willing to accommodate the stronger movements in order to avoid violence (Lacina, 2014). Second, I do not claim that all state concessions will be accepted. However, if a territorial base opens up the possibility of bargaining where it previously did not exist, then ceterus paribus, there is a lower chance of conflict. Territorial bases might help rebel groups move away from the corner solution: they could strengthen (via the mechanisms discussed above) what would otherwise be a weak group, enough to gain it a seat at the bargaining table with the government.

Taken together, this line of reasoning yields the following hypothesis about LIV and war:

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.

Implications for existing findings

If my approach is correct, the implications for existing research on causes of substate conflict are significant. 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. It introduces selection bias (Cunningham, 2016) offers useful insight into how this might work. He argues that the threat of pro-government external intervention can create a corner solution where the probability of rebel victory is so low that no bargaining even occurs, as rebels see no utility in violence. Cunningham states that “a corner solution means that one actor is at the boundary of the game and thus the game–in this case, where each side tries to offer a deal that makes the other side slightly better off than they would be through fighting–never occurs” (7).
bias, because escalation cannot occur without onset happening first. 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.

Strategy further complicates these methodological concerns. 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). Researchers also showed that simple logit or probit analyses were not appropriate

\[^{16}\text{Note that I make no assumptions here about the rapidness of the transition from onset to escalation.}\]

\[^{17}\text{For example, according to Fearon (1994), the hypothesis that increasing a defender’s relative strength would increase deterrence success was overly naive: he posited that crises were actually more likely to escalate when a relatively stronger defender faced a challenge, because that indicated that the challenger was particularly resolved.}\]
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). 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. This selection bias occurs according
to Reed because “pairs of states do not become entangled in hostilities randomly...they
instead select or are selected into disputes by a strategic process” (84). Jumping straight
to war overlooks that. Reed was focused on interstate conflicts, but these same issues can
be applied to the causes of civil wars, since state-opposition group dyads also select into
conflicts.

How then should we test our hypotheses about 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 therefore essential that we apply similar theoretical logic and
statistical methodology on the intrastate side to ensure that existing inferences are correct. For these reasons, I also reexamine some of the most robust findings in the studies of civil war onset.

Research design

To test these arguments, I focus specifically on self-determination groups, drawing from the data in Cunningham (2013), which examines any opposition movement attempting to achieve national self-determination between 1960 and 2005. There are two reasons motivating this choice. First, it provides the cleanest set of possible cases. 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, and then compare cases where violence remains low-level to cases where it escalates to war. Second, 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.

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. The unit of analysis is therefore the government-SD movement.

There is no restriction on whether these groups use violent or non-violent means to achieve their goals.
dyad-year, with 3898 observations. To be included, groups do not have to use violence – and in fact 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 and civil war. Both 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 remained nonviolent, experienced onset, or escalated.

<table>
<thead>
<tr>
<th></th>
<th>Nonviolence</th>
<th>LIV</th>
<th>War</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total Obs.</td>
<td>3165</td>
<td>743</td>
<td>189</td>
</tr>
<tr>
<td>Asia</td>
<td>875</td>
<td>379</td>
<td>73</td>
</tr>
<tr>
<td>Eastern Europe</td>
<td>469</td>
<td>35</td>
<td>14</td>
</tr>
<tr>
<td>Western Europe</td>
<td>885</td>
<td>32</td>
<td>1</td>
</tr>
<tr>
<td>Sub-Saharan Africa</td>
<td>558</td>
<td>168</td>
<td>55</td>
</tr>
<tr>
<td>Middle East and North Africa</td>
<td>150</td>
<td>125</td>
<td>43</td>
</tr>
<tr>
<td>Latin America</td>
<td>216</td>
<td>0</td>
<td>0</td>
</tr>
</tbody>
</table>

To test my argument about territorial bases, I again draw 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). The independent variable for GDP per capita comes from the Gleditsch Expanded Trade and GDP dataset (Gleditsch, 2002).
The model

I use a two-step Heckman selection model in order to properly control for LIV when testing for war. Under this type of model, one first estimates a selection equation for the first step of the model (Bushway et al., 2007; Dubin and Rivers, 1989; Reed, 2000) – in my case, LIV. This is a probit model, so the dependent variable in the first stage must be binary. Then, the results from that probit model are used to calculate a correction factor, which is used in the second stage of the model (Bushway et al., 2007). Since my second stage equation is also binary, I use a Heckman probit model instead of the OLS version. Note that the other requirement for a Heckman selection model is to have at least one variable in the selection equation that is not present in the outcome stage. I accomplish this via the controls.

As a robustness check to alleviate any concerns about the Heckman model, I also use a multinomial logit model to test my hypotheses. The results are consistent.

Controls

I use two broad categories of controls: state and group controls. For the state-level controls, I try to include most of the traditionally used controls in the civil war literature, particularly when reexamining the findings about GDP per capita. I do this to provide the closest comparison possible to original models that tested the effects of those variables. Specifically, I include a log of the population (also from the Gleditsch Expanded Trade and GDP dataset), a state’s level of democracy comes 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.\footnote{These other controls are all drawn from Cunningham (2013).} 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\(^{20}\) (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 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\(^{21}\).

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 war onset (defined as the 25 battle deaths per year threshold) (Cunningham, 2013, 2014). Particularly divided movements might also be less likely to have

\(^{20}\)Most 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. This would allow me to directly test the mechanism behind my territorial base hypothesis. However, this information is not available for SD movements. One possible future extension of this research would therefore be to gather that data and test for the mechanism explicitly.

\(^{21}\)These 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.
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.

Statistical Results

In this section, I present the results for each of the hypotheses. I first address the findings directly related to my own theory before reviewing the implications for existing findings.

The role of territorial bases

Table 2 provides results from the first stage of Heckman selection models that test for Hypothesis 1, about the role of a territorial base in LIV. Table 3 provides results from the second stage of the same Heckman selection models, which focus on escalation to war (having controlled for LIV). For both tables, the first model tests for LIV and war using only group-level variables, while the second includes the state-level controls as well. (Note that I ran robustness check using multinomial logit models instead of the Heckman selection models; the results were consistent.) 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. I reserve the discussion of the state-level results for
the next section, but several points merit attention here. First and most importantly, the
territorial base variable is consistently positive and quite large for the LIV equation. 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 war
equation of the model, the territorial base variable changes sign: it is consistently negative
and again quite large. 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. While these results cannot confirm the
mechanism behind my hypothesis, they indicate that it would be worthwhile to develop
the data on concessions and state surveillance infrastructure in order to test it explicitly.

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

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22The variable is statistically significant in the first model and just barely misses the threshold in the second model.
23The territorial base variable is highly statistically significant in the second model and just barely misses the threshold in the first.
24In 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.
25Gathering data on whether or not SD groups obtain concessions after onset would allow me to test whether the state is more likely to bargain and less likely to escalate with groups that have a base. This would provide direct support for the strategic component of Hypothesis 1.
Table 2: Heckman Selection Models, Stage 1: LIV

<table>
<thead>
<tr>
<th>Dependent variable:</th>
<th>LIV</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(Model 1)</td>
</tr>
<tr>
<td>factions</td>
<td>0.082***</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
</tr>
<tr>
<td>kin</td>
<td>0.164***</td>
</tr>
<tr>
<td></td>
<td>(0.055)</td>
</tr>
<tr>
<td>Terr. base</td>
<td>0.301***</td>
</tr>
<tr>
<td></td>
<td>(0.094)</td>
</tr>
<tr>
<td>Rel. size</td>
<td>0.157***</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
</tr>
<tr>
<td>Econ. discrim.</td>
<td>0.220***</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
</tr>
<tr>
<td>Polity2</td>
<td>−0.016***</td>
</tr>
<tr>
<td>Oil exporter</td>
<td>0.191**</td>
</tr>
<tr>
<td>Mountainous</td>
<td>0.027</td>
</tr>
<tr>
<td>Cold War</td>
<td>0.313***</td>
</tr>
<tr>
<td>GDP</td>
<td>−0.225***</td>
</tr>
<tr>
<td>Population</td>
<td>−0.087***</td>
</tr>
<tr>
<td>Constant</td>
<td>−3.018***</td>
</tr>
<tr>
<td></td>
<td>(0.180)</td>
</tr>
<tr>
<td>Observations</td>
<td>3,311</td>
</tr>
<tr>
<td>ρ</td>
<td>0.195</td>
</tr>
<tr>
<td>Inverse Mills Ratio</td>
<td>0.085 (0.072)</td>
</tr>
</tbody>
</table>

*Note:* *p<0.1; **p<0.05; ***p<0.01
Table 3: Heckman Selection Models, Stage 2: War

<table>
<thead>
<tr>
<th>Dependent variable:</th>
<th>war</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(Model 1)</td>
</tr>
<tr>
<td>kin</td>
<td>0.047</td>
</tr>
<tr>
<td></td>
<td>(0.037)</td>
</tr>
<tr>
<td>Terr. base</td>
<td>-0.133*</td>
</tr>
<tr>
<td></td>
<td>(0.068)</td>
</tr>
<tr>
<td>Rel. size</td>
<td>-0.033*</td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
</tr>
<tr>
<td>Econ. discrim.</td>
<td>-0.002</td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
</tr>
<tr>
<td>Polity2</td>
<td>-0.005*</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
</tr>
<tr>
<td>Oil exporter</td>
<td>-0.081*</td>
</tr>
<tr>
<td></td>
<td>(0.049)</td>
</tr>
<tr>
<td>Mountainous</td>
<td>-0.050***</td>
</tr>
<tr>
<td></td>
<td>(0.018)</td>
</tr>
<tr>
<td>Cold War</td>
<td>0.063*</td>
</tr>
<tr>
<td></td>
<td>(0.036)</td>
</tr>
<tr>
<td>GDP</td>
<td>0.010</td>
</tr>
<tr>
<td></td>
<td>(0.018)</td>
</tr>
<tr>
<td>Population</td>
<td>-0.029**</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.500*</td>
</tr>
<tr>
<td></td>
<td>(0.279)</td>
</tr>
<tr>
<td>Observations</td>
<td>3,311</td>
</tr>
<tr>
<td>ρ</td>
<td>0.195</td>
</tr>
<tr>
<td>Inverse Mills Ratio</td>
<td>0.085 (0.072)</td>
</tr>
</tbody>
</table>

Note: *p<0.1; **p<0.05; ***p<0.01
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 crude a measure: one study suggests 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).

**GDP**

To better understand the results about GDP per capita, I also run probit models for LIV and war, separately. 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. To ensure that any differences I might find in my results are due to the 2-step approach – and not an artifact of focusing on SD movements – I run the traditional model with the state-level controls. Table 4 presents the results from the probit model for LIV, side-by-side with the results from the first stage of a 2-step Heckman model using (only) the state-level controls for the LIV and war equations. Table 5 presents the results from the probit model for war, along with the results from the second stage of the corresponding Heckman model. Note that I use the same equation for LIV and war here in order to provide as direct a comparison to the probit models as possible.\(^{26}\) Below I discuss how the probit models compare to the results in all tables.

The results are striking. In the probit models, higher values of GDP per capita are correlated with a lower probability of LIV and war. This is consistent with the existing

\(^{26}\)This 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 4: Probit v. Heckman Selection: LIV

<table>
<thead>
<tr>
<th></th>
<th>Model 3: Probit</th>
<th>Model 4: Selection</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dependent variable:</td>
<td>LIV</td>
<td></td>
</tr>
<tr>
<td>Polity2</td>
<td>−0.020***</td>
<td>−0.020***</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Oil exporter</td>
<td>0.096</td>
<td>0.097</td>
</tr>
<tr>
<td></td>
<td>(0.067)</td>
<td>(0.068)</td>
</tr>
<tr>
<td>Mountainous</td>
<td>−0.014</td>
<td>−0.014</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.024)</td>
</tr>
<tr>
<td>Cold War</td>
<td>−0.025</td>
<td>−0.026</td>
</tr>
<tr>
<td></td>
<td>(0.053)</td>
<td>(0.053)</td>
</tr>
<tr>
<td>GDP</td>
<td>−0.179***</td>
<td>−0.178***</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.024)</td>
</tr>
<tr>
<td>Population</td>
<td>0.040**</td>
<td>0.041**</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.138</td>
<td>0.134</td>
</tr>
<tr>
<td></td>
<td>(0.278)</td>
<td>(0.282)</td>
</tr>
<tr>
<td>Observations</td>
<td>3,633</td>
<td>3,632</td>
</tr>
<tr>
<td>Log Likelihood</td>
<td>−1,711</td>
<td></td>
</tr>
<tr>
<td>Akaike Inf. Crit.</td>
<td>3,437</td>
<td></td>
</tr>
<tr>
<td>ρ</td>
<td></td>
<td>−1.143</td>
</tr>
<tr>
<td>Inverse Mills Ratio</td>
<td></td>
<td>−6.464** (2.588)</td>
</tr>
</tbody>
</table>

Note:  *p<0.1; **p<0.05; ***p<0.01
### Table 5: Probit v. Heckman Selection: War

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

**Note:** *p<0.1; **p<0.05; ***p<0.01
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 5, GDP per capita is negatively correlated with LIV. 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, 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 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.

These findings, particularly for GDP per capita, are broadly consistent with the theoretical story I have proposed. Using GDP per capita as a proxy for state capacity, the findings show that stronger states are less likely to experience onset (although we cannot confirm if this is because they deter challengers). Yet 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. This would represent the flip side of the territorial base argument. More research needs to be done to better understand exactly what drives these findings.
Conclusion

This article has challenged traditional approaches to, and understandings of, the start of intrastate conflict. I have shown that we need a comprehensive model of LIV and war together – both theoretically and empirically – for understanding 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, mechanisms for group type and the information environment. One of the major contributions of this study is to demonstrate that one observable implication of 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. It also points to the need for future research that clarifies the exact mechanisms behind these findings.

Yet much remains to be done before we can fully understand exactly why some civil conflicts escalate, while others do not. This article represents a first cut at doing so, and moving forward, it identifies several avenues for additional research. First, why exactly is the relationship between GDP per capita and LIV so different than that between GDP per capita and war? I have identified one possible explanation – that GDP per capita proxies for state capacity, and those states that experience LIV have a greater capacity for escalation to war – but this merits additional attention and testing. Second, 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. Third, I have focused on two causal mechanisms related to the strategic interaction between the state and the opposition – the opposition group type that is operating, 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.
References


