Local Dynamics of Civil War

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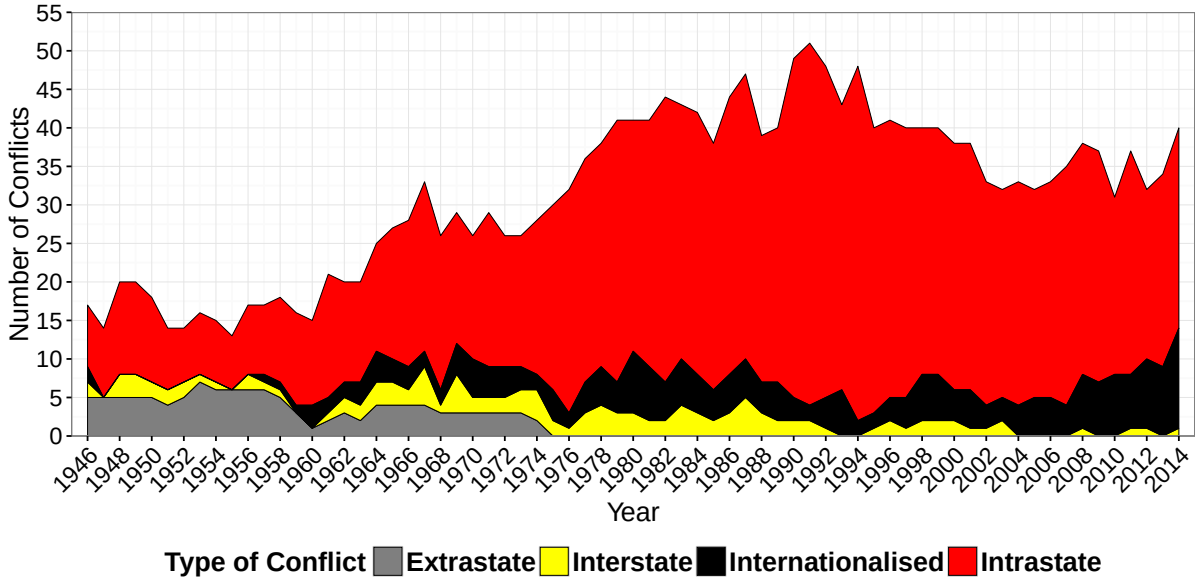
1 Introduction

Andreas Forø Tollefsen

1.1 Introduction

In 2014, 40 armed conflicts took place across the world, of which 39 were internal armed conflicts, fought within the confined borders of a state (Pettersson and Wallensteen, 2015). As Figure 1.1 shows, intrastate conflicts have been the predominant form of armed conflict in the post-cold war era. These intrastate conflicts differ from interstate conflicts, in that they are primarily fought within the confined borders of a state, and seldom engulf countries in their entirety (Hallberg, 2012)[1]. Studies also suggest that areas affected by intrastate armed conflict rarely resemble the country at large (Buhaug and Lujala, 2005). The aim of this thesis is to expand our knowledge of the local causes of these intrastate armed conflicts, as well as the severity and geographical scope of internal armed conflict recurrences.

Figure 1.1: Armed Conflict by Type, 1946-2014. Based on UCDP/PRIO Armed Conflict Dataset



The majority of previous studies of internal armed conflict have explored the causes of conflict at the country level. While factors such as democracy, composition of the ruling elite or memberships of military alliances are best studied at the country level, other factors vary more considerably within the territory of a state - factors such as rugged terrain, socio-economic development and poverty, quality and presence of institutions, environmental conditions or the location of excluded minorities. Few would argue that these factors are homogeneously distributed within a country, but rather that they vary across the territory of a state.

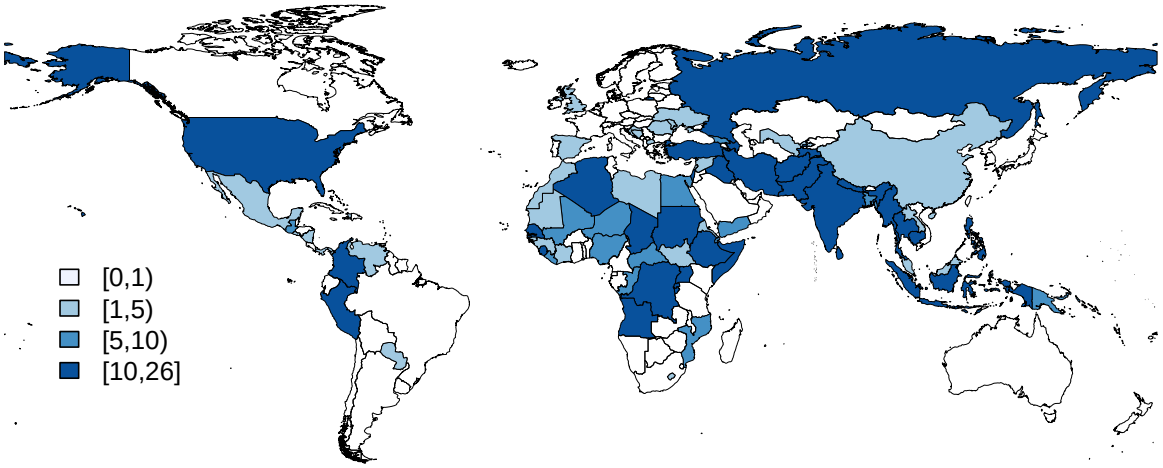
In Figure 1.2, I show all countries that experienced intrastate conflict following the end of the cold-war and the number of years of conflict. Six countries experienced

[1]Hallberg (2012) found internal conflicts fought over government control to cover on average 50 % of state territories, while territorial conflicts covered on average 24 % of the total land area. Similarly, Raleigh, Hegre, Karlsen and Linke (2010) found that among high-risk states in Africa, an average of 48 % of their areas was affected by conflict violence.

internal armed conflict in every year; Colombia, Afghanistan, Philippines, Myanmar (Burma), India and Sudan.

While the map shows that the temporal extent of conflict is considerable, it does not reveal the spatial extent of these conflicts within countries. Russia for instance, had conflict in 22 of the 26 years. However, the scope of these conflicts has primarily been confined to relatively small areas in North Caucasus. Uganda has mostly seen conflict in the Northern region, where the Lord's Resistance Army (LRA) has ravaged. Before ending in 2009, the Sri-Lankan civil war was mostly confined to the Northern and Eastern regions, where the Liberation Tigers of Tamil Eelam (LTTE) fought to create an independent Tamil state. To understand this clustering of conflict events requires a sub-national focus, where two alternatives are presented in this thesis.

Figure 1.2: Number of years with intrastate conflict, by country, for the period 1989-2014. Based on UCDP/PRIO Armed Conflict Dataset



Several scholars in geography and other disciplines have argued that the predominant focus on the state as the common unit of exploration in conflict research is unfortunate (O'Loughlin, 2000). Agnew (1994, 2010) argues that scholars conceiving the state as a fixed unit and a container of society have fallen into a territorial trap. Others, such as Wimmer and Glick Schiller (2002) criticize what they coin "methodological nationalism", where the nation-state is conceived as the natural social and political form of the modern world.

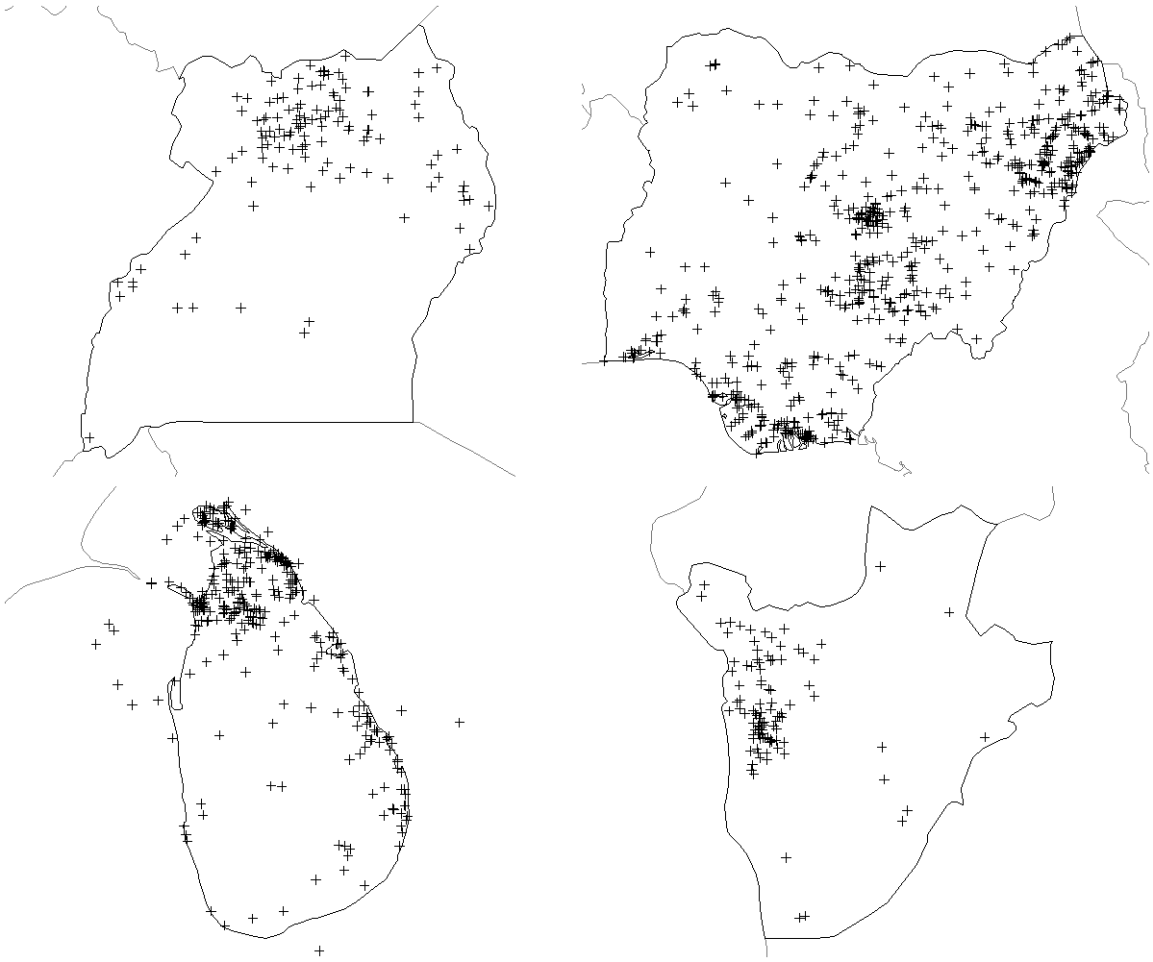
To escape the "territorial trap" and explore the sub-state nature of civil wars, both theoretically and empirically, appropriate research designs must be developed. Otherwise, we are at risk of masking significant variation; that may explain why conflicts occur in some parts of a country, while not in others. This thesis presents alternative research designs for the sub-national analysis of armed conflict.

Many country level studies aim to test propositions that often relate to processes at different geographical scales: the individual, sub-national, or group level. These propositions are often not met with variables measured at the same level. Thus, a

sub-national approach to conflict research may improve the connection between theory and data. As, Cederman and Gleditsch (2009, pp.488) argues, "national aggregates and averages ... are only loosely linked to the rationale for conflict and the postulated micro-level mechanisms".

A look at a subset of contemporary civil war's shows that conflict tends to cluster in some parts of a country. Figure 1.3 shows conflict events that occurred between 2005 and 2014 for four selected countries. The distribution of these events clearly points to the clustering of conflict; in North-Uganda, Northern and Eastern regions of Sri Lanka and western Burundi. The events in Nigeria are more dispersed, although multiple clusters can be identified; along the Niger Delta, around the capital Abuja, and in the north-eastern region, ravaged by the Islamic extremist group Boko Haram. In this thesis, I show that this non-random distribution of conflict events can be attributed to specific local conditions; physical and socio-cultural inaccessibility, local poverty and the quality of local institutions.

Figure 1.3: Map showing local conflict events in Uganda (upper-left), Nigeria (upper-right), Sri Lanka (lower-left) and Burundi (lower-right), between 2005 and 2014. Based on UCDP Georeferenced Events Dataset.



1.1.1 Motivation and Research Questions

The main endeavor of this thesis is to explore the local dynamics of civil war and investigate why some parts of a country experience conflict, while others remain in relative peace. For instance, what can explain the fact that internal armed conflicts rarely affect more than half of a country's territory? Throughout this dissertation, I will emphasize the importance of local explanations, and how this local context shapes both opportunities and motivations for armed conflict.

In this thesis, I aim to answer one broad question: what explains variations in local patterns of internal armed conflict? Below, I develop more nuanced research questions (RQ) related to each chapter; that contributes with added knowledge of the local causes and consequences of internal armed conflict.

In Chapter 3, I explore how variations in physical and socio-cultural inaccessibility create opportunity and motivation for conflict. Inaccessibility is particularly a problem in Africa, where states often have limited reach, and often fail to control their territories (Herbst, 2000). The ability of states to penetrate all parts of their territory hinges on their capability, discounted by levels of inaccessibility, both physically and socio-culturally. Physical obstacles decrease a country's ability to perform the internal ordering of its territory. In states with weak state reach, areas dominated by mountainous terrain or impenetrable forests, create areas beneficial to political opponents and potential insurgents.

Similarly, areas inhabited by ethnic minority groups also increase the friction to the national integration of peripheral regions (Boone, 2003). These areas inhabited by excluded populations provide both opportunity and motivation for rebellion (Cederman, Gleditsch and Buhaug, 2013).

Where all peripheral territories and groups are integrated, and the state exerts control and a monopoly of violence, the risk of conflict dwindles. Fearon and Laitin (2003) argue that the reason why poorer countries experience more conflict are that they are unable to control all parts of their territories and to penetrate inaccessible areas. Thus, areas out of reach of impoverished state agencies present opportunities for collective mobilization and violent action.

While existing studies of terrain and conflict have highlighted the "rough terrain" argument, the majority of these studies have not taken into account the local relationship between inaccessibility and conflict, as well as the notion of a socio-cultural inaccessibility. Socio-cultural inaccessibility refers to areas inhabited by a minority or a excluded ethnic group. These areas increase the threshold of the state to monitor and control the areas of these groups. Thus, in Chapter 3, I ask:

> RQ 1: How does local physical and socio-cultural inaccessibility affect the probability of local conflict-related violence?

While inaccessible areas may provide both opportunity and motivation for conflict, the willingness to take up arms is also dependent on the economic well-being of individuals. Existing studies show that impoverished individuals in peripheral districts are excluded both socially and institutionally, and are also more conflict-prone (Bird

et al., 2002).

The relationship between low per-capita income and the risk of conflict is among the sturdiest determinants for which countries will experience civil war (Hegre and Sambanis, 2006). Collier and Hoeffler (1998) argue that the relationship between low per-capita income and conflict is because poverty decreases the opportunity cost of rebellion. Poorer individuals are more inclined to rebel, as conventional income foregone from taking up arms is low. Fearon and Laitin (2003), argue that poorer countries see more conflict because they are not able to monitor and control their territory. Others, such as Gurr (1970), argue that relative deprivation creates grievances that might trigger violent action.

While existing studies suggest that low-income countries have a higher risk of conflict, we do not know whether a similar relationship exists in a domestic setting. While a few recent studies have explored the sub-national link between poverty and conflict, they have mainly relied on objective proxies such as night-time luminosity or household assets. In Chapter 4, I take a different approach and explore the relationship between a subjective measure of experienced poverty regarding the lack of basic needs, and internal armed conflict. Thus, in Chapter 4, I ask:

> RQ 2: How does local poverty affect the probability of local conflict-related violence?

The prospects to reduce poverty depends on the ability of institutions, and in particular the quality of governance (World Bank, 2001). Meanwhile, the ability for institutions to alleviate poverty requires the capacity to monitor and enforce the rules implemented (Barrett, Lee and McPeak, 2005), but also by strengthening and broadening accountability mechanisms at both local and national levels (Crook, 2003).

Local institutions represent decentralized forms of political authority. If given resources and devolved authority, these institutions can reduce hostile disagreements between local antagonists, provide better public services and exert control by an uncorrupted, effective local police force, as well as a robust local judicial system. Several country level studies have explored the effect of the quality of state institutions on conflict (Hegre and Nygård, 2015; Hartzell and Hoddie, 2007; Hegre et al., 2001). However, these studies have not examined whether the quality of local institutions affects the risk of conflict. Thus, in both Chapter 4 and 5, I ask:

> RQ 3: How does the quality of local institutions affect the probability of local conflict-related violence?

While the quality of local institutions may reduce the probability of local conflict-related violence, studies have shown that the quality of governance reduces the risk of conflict recurrence (Walter, 2015; Hegre and Nygård, 2014). However, the risk of recurrence also depends on how the initial conflict was terminated (Mason et al., 2011). Conflicts that end in a negotiated settlement are more at risk of recurring, than conflicts ending in victories. Although there has been an increased focus on conflict recurrence, no studies have explored the severity of recurrences. In Chapter 6, I examine whether subsequent conflicts are more or less deadly than initial conflict episodes, and if so, what explains these differences. In Chapter 6, I ask:

RQ 4: Are subsequent conflicts more or less severe than initial conflicts?

1.1.2 Definitions

Throughout this thesis, I focus on the local dynamics of civil war. However, what constitutes a civil war, and how do we distinguish between major civil wars and minor internal armed conflicts? Moreover, what is the local?

Civil War, Events, and Severity

Civil war is typically defined as wars that are fought within the border of states, between a government and a non-governmental actor. These conflicts are often referred to as intrastate armed conflicts and are classified by their intensity level. According to the definitions used by the Uppsala Conflict Data Program (UCDP) (Gleditsch et al., 2002), an intrastate conflict causing at least 1,000 battle-related deaths in one calendar year is referred to as a war, while a conflict reaching at least 25 battle-related deaths, is referred to as a minor conflict. When either party to an intrastate conflict receives external support, this is known as an intrastate conflict with foreign involvement or an internationalized intrastate conflict.

In chapter 3, 4 and 5, the outcome is the conflict event, part of a conflict with more than 25 battle-related deaths per year. In chapter 6, the outcome is the number of battle-related deaths and the geographical extent of a conflict episode, where the conflict episode is part of a conflict that, at one point in time, has reached the status of major conflict.

A conflict event is defined by the UCDP Georeferenced Event Dataset (UCDP GED) as "an incident where armed force was by an organized actor against another organized actor, or against civilians, resulting in at least one direct death at a specific location and a specific date" (Sundberg and Melander, 2013). For an event to be registered in the UCDP GED dataset, the event must be part of a conflict as coded in either the UCDP/PRIO Armed Conflict Dataset (Pettersson and Wallensteen, 2015), UCDP Non-State Conflict Dataset (Sundberg, Eck and Kreutz, 2012) or the UCDP One-Sided Violence Dataset (Sundberg, 2009).

In chapter 6, I explore the average annual severity of conflict episodes and the average annual geographical scope. I use a measure of casualties representing the number of battle-related deaths. The measure includes battlefield fighting, guerrilla activities, bombardment and urban warfare. When civilians are killed in crossfire or indiscriminate bombings, these fatalities are also counted as battle-related deaths.

The average annual geographical scope is operationalized as the extent of conflict in space, and is calculated from the Conflict Site dataset (Buhaug and Gates, 2002; Hallberg, 2012). The geographical scope is defined by a center point indicated by a point coordinate pair and the radius, indicating the most distant point in the conflict zone. I use GIS to remove parts of the circular polygon that fall outside of the conflict-affected country's territory.

The Local

A central concept throughout this dissertation is the concept of the local, and how conflict can be seen as a local phenomenon, related to the local characteristics and conditions of where it is fought. The concept is given both theoretical and empirical pertinence across the subsequent chapters. In this thesis, I conceptualize the local as objects with characteristics confined to a smaller geographical area (than the state). These local areas include villages and towns, districts, regions and even artificial grid cells that all represent distinct scales, subordinated below the national.

The local may be conceived as a geographical level between the individual and the national or regional level. Thus, it is a confined space nested in a hierarchy of other bounded spaces of differing sizes, such as (other) local, regional, national and global scales (Delaney and Leitner, 1997).

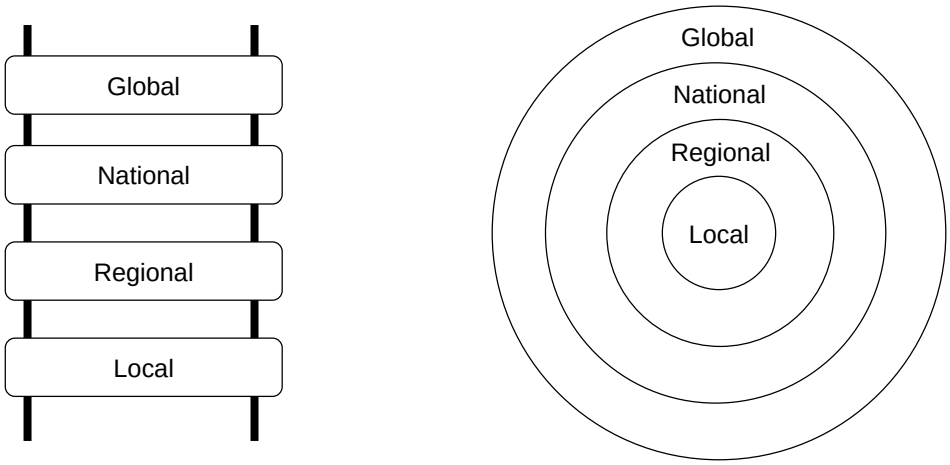
First, the local relates horizontally with the other scales across space. This understanding of scale refers to the cartographic and analytical notion of scale, where we "zoom" out from a large scale (i.e. 1:10 meters) showing only the local, to a small scale (i.e. 1:1000 meters) showing a larger area. Thus, moving out of the local crosses reveals other scales, encompassing the local. Second, scale relates vertically with other scales as a layer in a hierarchy, where the local can be conceived as a layer in the relationship of power and core-periphery relations (and negotiations) between urban and rural political landscapes.

The hierarchical ordering of the local subordinated below the national can be exemplified by the relationship between the core (center) and the rural periphery, and how states choose to devolve authority to it is administrative outposts. The state can either decide to devolve power to local big men or chiefs or by sending out constrained directives from the capital city (Boone, 2003). Regardless of how we conceptualize the local, it remains a scale, "a mental device by which we categorize and make sense of the world" (Herod, 2010, pp.12).

While the use of the "local" relates to the location of objects, it also links to the geographic concept of place. Agnew (1987) argues that place can be understood as; (1) location: a point in space with specific relations to other points in space, (2) locale: the broader context (built and social) for social relations, and (3) sense of place: subjective feelings associated with a place. In this thesis, I view the local as objects, but not disconnected from the experiences of local citizens and relations between individuals and institutions. Exploring local citizens' experiences with poverty, their relationship with local institutions and perceptions of exclusion based on ethnic identity markers is clearly more than an abstract container in space.

Several scholars in geography have argued that international relations' exclusive focus on states as the only territorial unit of interest, limits our understanding (O'Loughlin, 2000). As Agnew (2010, pp.783) argues, "the quintessential states of International Relations are not the only territorial enterprises around, not all states (and other actors) are equivalent in their effective territorial reach or ability to legitimately control their claimed territory, and territory is not the only geographical modus operandi of either states or other actors. Staying in the territorial trap may make the world more intellectually (and politically) tractable, but at what intellectual (and

Figure 1.4: Relationship between different scales. Left Figure shows scale as a hierarchy (ladder) and to on the Right scale as concentric circles. Adapted from Herod (2010).



political) expense!". Agnew's notion of a territorial trap highlight the need for a more disaggregated approach to the study of the relationship between socio-economic and political conditions and events.

While the local is central to this thesis, both theoretically and empirically, it also has a specific methodological connotation. In the spatial analysis, local spatial dependence refers to the interaction between neighboring or nearby spatial units (Anselin and Bera, 1998). When conflict spreads from one unit to its neighboring unit, we typically refer to this process as a dependent spatial process or diffusion. Another issue when estimating a statistical model using spatial data is that results can change depending on the units included (spatial bandwidth). This non-stationary process refers to how the degree of relationships varies spatially, and where a single model is not applicable for all parts of a study area (Brunsdon, Fotheringham and Charlton, 1996). LeSage (1999, pp.7) defines spatial heterogeneity as "variation in relationships over space". I elaborate more on these local processes in the section on methodological caveats.

1.1.3 Quantitative Political Geography and Conflict Research

Political Geography and Conflict

This thesis contributes to the study of peace and conflict, where the majority of publications come from political scientists or international relations. However, this is a thesis within the discipline of geography in general and political geography more specifically.

In this section, I discuss the status of quantitative political geography and the quantitative analysis of conflicts. They show that historically, the use of quantitative methods in political geography has been limited. However, more recently, political geographers have contributed to the local study of armed conflict, showing that context matter, contrary to what has been voiced by King (1996).

Agnew and Muscarà (2002, pp.12) argue that "There is a persisting tendency to insist that politics cannot be adequately understood without understanding the geographical contexts in which it takes place, from global geopolitics at one end of the scale to local politics at the other". Political geography is concerned with the context in which political behavior takes place. However, King (1996), argues that context rarely does matter and that the effect political geographical context of units have rarely made a difference. He argues that "The geographic variation be usually quite large, to begin with, but after we control for what we have learned about voters, there isn't much left for contextual effects. So in a narrow sense, geography matters, but contextual effects do not" (King, 1996, pp.160). In this thesis, I explore the local context of conflict, and show that to understand local conflicts; we need to explore the local contexts where these take place.

Spykman (1938) once stated that geography is the most fundamental factor in the foreign policy of states because it is the most permanent, unchanging and ever-present. The focus on geography in international relations (IR) research was traditionally narrow and primarily equated geography with contiguity and straight line distance (Buhaug and Lujala, 2005). Thus, it typically ignored the centrality of the concept of place given emphasis in the discipline of human geography (O'Loughlin, 2000).

Where political scientists suggest that "space is more than geography" (Beck, Gleditsch and Beardsley, 2006, pp.27), political geographers would argue that "geography is more than space" (Comment by John O'Loughlin during a round-table at the 55th ISA Annual Convention, 2014 in Toronto). Thus, geography is more than the (absolute) distance between entities but also emphasize the relative and relational distance.

In other words, for political scientists, geography has merely been a technique used to analyze spatial data. Along the same lines, O'Loughlin (2000) argues that political scientists tend to view geography as a narrow discipline, and he claims that there exists an import-export imbalance of information, meaning that while political geographers absorb and use the theories, methods and perspectives of political scientists, the reverse is not true.

O'Loughlin criticizes the prevailing belief among some political scientists that spatial analysis constitutes the field of geography, and an accompanying ignorance of the place tradition in human geography. Raleigh, Witmer, O'Loughlin and Denemark (2010, p.3) elaborate on this discrepancy, and argues that many civil war studies "misconstrued the nature of political geography by ignoring the complicated social, cultural, economic and political relations that combine to give locales their special character".

Recent developments in the civil war research, with the introduction of geo-referenced data and more local analysis, show promising signs of smoothing out the imbalance that O'Loughlin (2000) criticizes. Context and spatially disaggregated studies are on the rise, with a focus on the importance of local economic development (Buhaug et al., 2011), local institutions (Wig and Tollefsen, 2016), local politically excluded groups (Cederman, Weidmann and Gleditsch, 2011) and natural resources (Lujala, Gleditsch and Gilmore, 2005) among other factors. As O'Loughlin and Raleigh (2007) argue "The geographic perspective, especially the emphasis on context, scale linkages, diffusion, and spatial analysis, offers a vital and innovative supplement to dominant ap-

proaches″.

"In geography, the most guarded territory is not the earth but disciplinary methodologies and, by implication, the lenses through which the earth is viewed." (Schuurman, 2000, pp.570)

Compared to other social sciences, the volume of published research in political geography using quantitative analysis has, until recently, been relatively limited (O'Loughlin, 2003). The discipline of geography experienced a quantitative revolution in the 1960s, as an answer to internal critics that wanted to create a more scientific discipline. This revolution came about as a result of scholarly concern that the discipline was unsystematic in its explorations, and scientific method could provide systematization and rigor (Hubbard et al., 2002).

The quantitative revolution in geography proved to be short-lived. In the late 1960s and beginning of 1970s, the quantitative shift in geography was brought into question, with scholars opposing quantitative methods all together (Fotheringham, Brunsdon and Charlton, 2000). Critics from the more radical lines in geography claimed that quantitative geography was non-humanistic and failed to acknowledge the human dimension of people, shrinking human activity into vectors or movements. On the other hand, critics often failed to recognize the complex set of practices pursued by quantitative geographers (Sheppard, 2001).

This development differed significantly from other social sciences such as sociology and political science, where the use of quantitative analysis gained ground in the late twentieth century. The result of the debate created unfortunate dichotomies between quantitative/qualitative, empiricist/non-empiricist and simplification/complexity, and GIS became the center stage in a lengthy discussion in the discipline, which eventually formed approaches such as qualitative and critical GIS (Cope and Elwood, 2009).

Political Geography and GIS

The critics within geography wrongly equated quantitative methods with spatial analysis[2], and Geographic Information Systems (GIS) took center stage. As Schuurman (2000, pp.571) states. "It was only a matter of time before GIS, at first glance, a rapidly growing positivist technology attracted the attention of human geographers." Many of the critics operated under an unclear definition of what positivism was. As Schuurman (2000, pp.580) argues, "Positivism, in this context, seemed very vague, given that so much research could fall under its rubric."

This attraction of critics of GIS was going to initiate a partly hostile debate in the discipline of geography. Schuurman (2000) divides the discussion into three waves of GIS-critique. The first wave of debates she coins "Early Rumblings", where GIS was seen as a positivist tool. Taylor (1990) argue that GIS was useful to describe information, but not to create knowledge. He viewed GIS as the positivist revenge, arguing that "the result has been a return of the very worst sort of positivism, a most naive empiricism" and continued by stating that "GIS is geography's own little bit of

[2]Spatial analysis is one type of quantitative methods, but can also be used in qualitative methods. See Cope and Elwood (2009) for examples of qualitative GIS.

the "high-tech" revolution and has suffered accordingly with the seemingly endemic high-tech disease of mega-hype".

A general trend in the first wave of debates was the lack of knowledge by GIS critics of GIS itself, and the lack of knowledge about the philosophy of science among some GIS geographers (Schuurman, 2000). The second wave, being more subtle, related to aspects of power - how GIS was used in warfare, for surveillance and the power that lies in cartographic representation. The third wave brought critics and GIS-practitioners closer together, highlighting how GIS could be used in participatory planning and also in qualitative research. Today, GIS is an essential focus area in the discipline, both in quantitative works (Sheppard, 2001) and in qualitative geography (Cope and Elwood, 2009).

Seminal work by Cliff and Ord (1973) and Anselin (1988) showed that classical statistical models are rarely appropriate for studying geographic data. The reason is that these models do not take into account the particular nature of geographic data. This distinctive character relates to the interdependence between places. Hence, modeling political behavior using geographic data requires more complicated and extended modeling procedures (O'Loughlin and Raleigh, 2007).

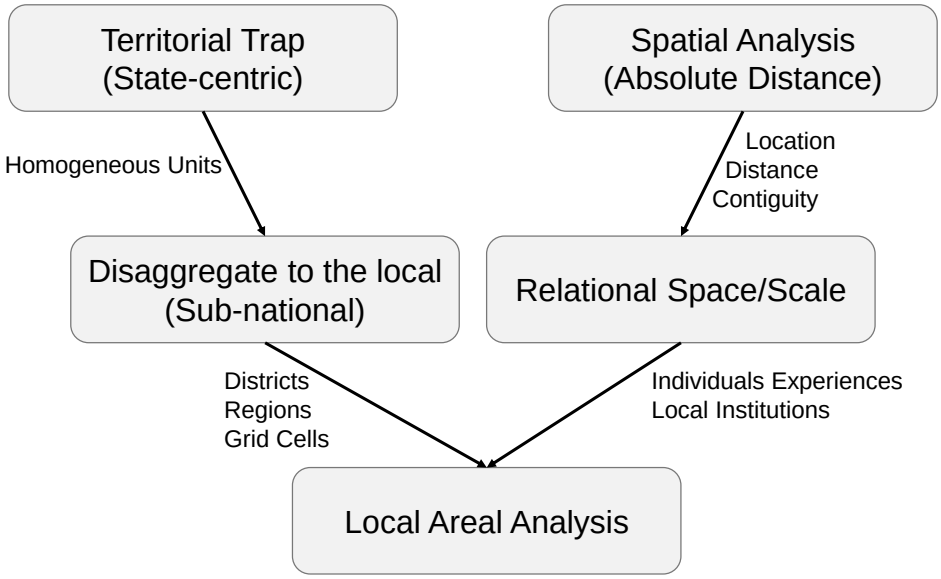
O'Loughlin and Raleigh (2007) argue that if political geography is to remain a vital part of the wider social science enterprise to understand human behavior, theoretical elements, and methodological approaches must be merged with appropriate spatial and statistical modeling techniques.

1.1.4 Geographical Contributions

The two previous sections discussed the concept of the local, and the development of quantitative conflict research in the sub-discipline of political geography. This thesis contributes to a geographical understanding of the local causes of armed conflict in two ways. First, it contributes to shifting away from the state-centric understanding of conflict, or a "territorial trap", through spatial disaggregation from the homogeneous state to the local. This contribution is depicted in the left path of Figure 1.5.

Second, this thesis does not take an absolute understanding of space, where units are only separated by Euclidean distance, but emphasizes the relative and relational understanding of space. Here, relative space refers to the relationship between units, separated by time or effort to travel. Relational distance, on the other hand, suggests that events depend on upon everything else around them (within a limited distance) Harvey (2004). Here, local causes of conflict can only be understood by considering the local conditions where conflict occurs and how events occur as a function of the relationship of individuals and actors physical, social and political contexts. This contribution is illustrated in the right path of Figure 1.5. As such, this thesis differs from the deterministic understanding of space that held sway during the quantitative revolution of 1960s geography.

Figure 1.5: Two geographical contributions. From the state to the local and from absolute to relational space



1.1.5 Content and Structure of Thesis

This thesis consists of this introductory chapter followed by five articles, included as chapters two through six. Table 1.1 presents the chapter number of each article, together with the publication status of each article and if applicable, the names of any co-authors. Chapter 2, 3 and 5 are written together with co-authors, and are all published in international journals. Chapter 4 and 6 are single-authored and have both been submitted to international journals for peer-review.

Table 1.1: Articles titles and publication status

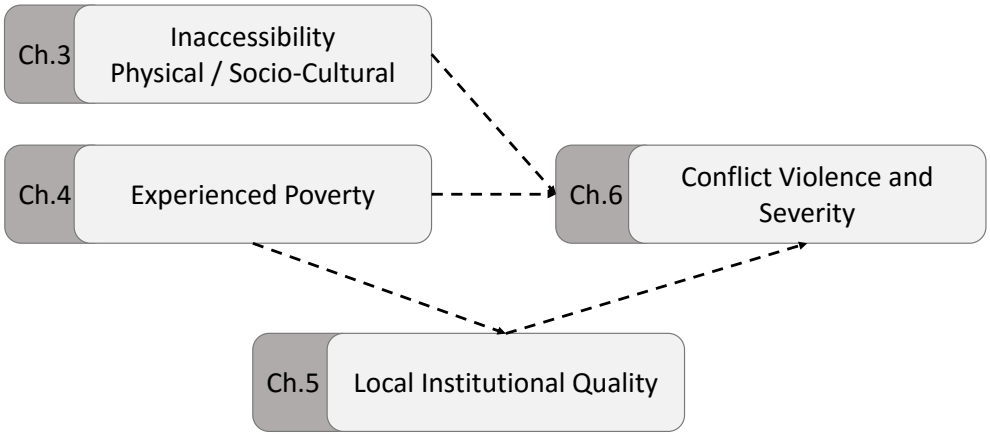
Chapter	**Title**	**Publication Status**	**Co-authors**
2	PRIO-GRID: A unified spatial data structure	Published in Journal of Peace Research	Håvard Strand; Halvard Buhaug
3	Insurgency and Inaccessibility	Published in International Studies Review	Halvard Buhaug
4	Experienced Poverty and Local Conflict Violence	Submitted to Conflict Management and Peace Science	N/A
5	Local institutional quality and conflict violence in Africa	Published in Political Geography	Tore Wig
6	Every Cloud has a Silver Lining: The Severity of Armed Conflict Recurrences	Submitted to International Interactions	N/A

1.2 Conceptual Framework

This section presents the conceptual framework of this thesis, as presented in Figure 1.6. It highlights the three local factors that I explore in the subsequent chapters. In chapter 3, I examine how physical and socio-cultural inaccessibility affects the risk of local conflict violence. In chapter 4, I study how poverty is related to conflict, and how the quality of local institutions affects this relationship. In chapter 5, I explore whether the quality of local institutions affects the risk of local armed conflict. Lastly, in chapter 6, I examine the severity of subsequent armed conflicts regarding their deadliness and spatial extent, as well as how the outcome of the previous conflict episode affects the severity of subsequent conflict episodes.

While Figure 1.6 restrict the presentation to the relationships highlighted in this study, there are several other possible local causes to conflict not addressed in this thesis. There are also several reversed causal arrows not illustrated in the Figure. Meaning, while inaccessibility, poverty, grievances and the quality of local institutions can affect the risk of conflict, conflict can also impact on inaccessibility, poverty, grievances and the quality of local institutions. I highlight and address these threats to valid inference in the chapter on methodological caveats, as well as more specifically in each of the subsequent chapters.

Figure 1.6: Local Dynamics of Civil War - Conceptual Framework



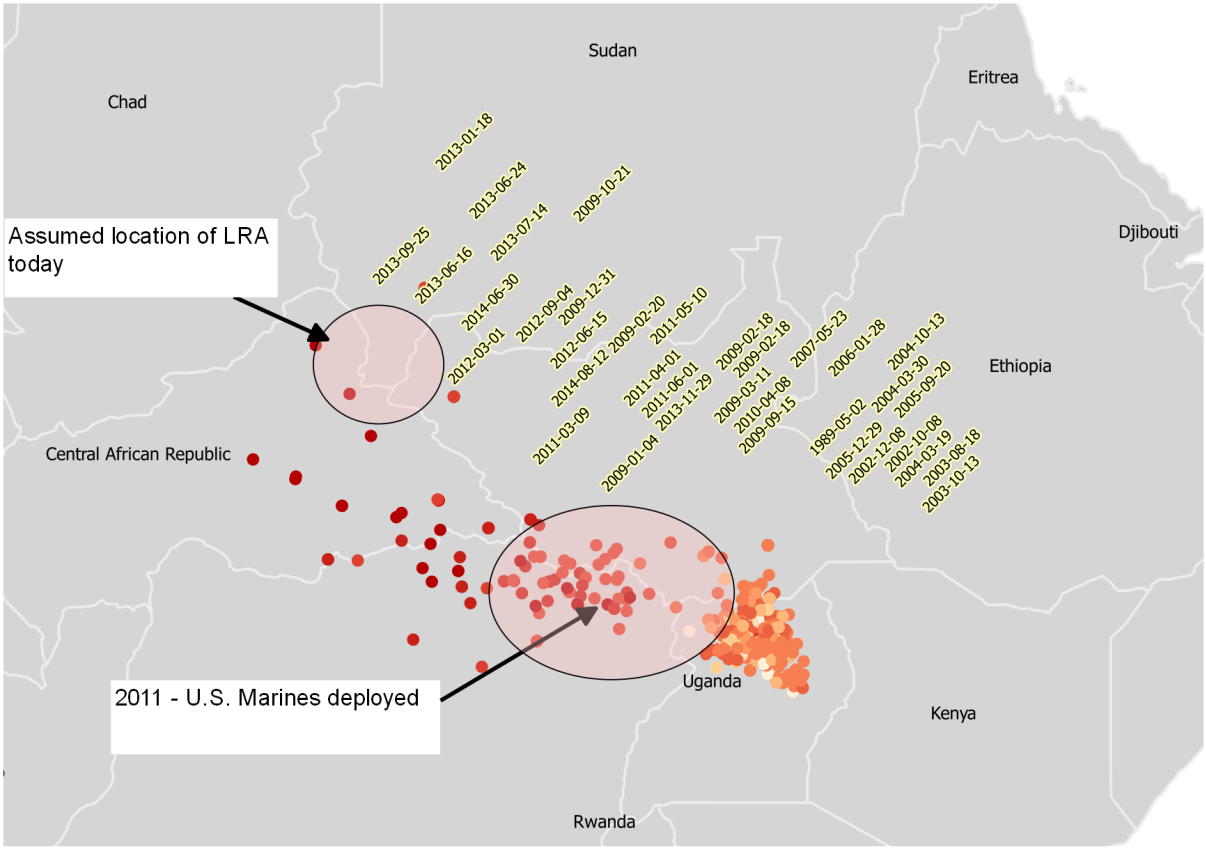
1.2.1 Terrain and Physical Inaccessibility

"At their strongest, the security forces could confine the terrorists to deep jungle, but they could never hope to find them all in one massive offensive - the jungle was too thick" (Crawford, 1958, pp.82).

This quote, taken from Oliver Crawford's book "The Door Marked Malaya" on counterinsurgency, highlights the difficulty facing counter-insurgents going after rebels

in impassable terrain. Forests and mountains provide efficient, safe-havens for small bands of insurgents. One example is the Lord's Resistance Army (LRA) in Uganda. Following their ravaging in northern Uganda in the first decade of 2000, they have more recently been pushed out of Uganda. With the deployment of U.S. Marines to go after LRA in 2011, one would expect that such a small insurgency would crumble. However, as shown in Figure 1.7, LRA retreated to the northwest, into inaccessible areas of the porous border zone between Uganda, Sudan, South-Sudan and the Central African Republic, providing an efficient, safe-haven. Their troop size remains today a couple of hundred soldiers (U.S. Department of State).

Figure 1.7: Conflict Events between 1989 and 2014, where the Lord's Resistance Army is coded as one of the actors. Data from UCDP GED. Darker colors indicate events at a later point in time. Displaced labels show the date of the event.



The capacity of the state is related to the government's ability to access their territories and maintain a monopoly of violence (Fjelde and de Soysa, 2009; Besley and Persson, 2010), also in their outermost peripheral areas. In this thesis, I introduce the concept of inaccessibility, which could reduce state reach, and provide potentially armed challengers with safe-havens. I define inaccessibility as factors that reduce the ability of a central government to project political and military authority throughout its territory. In areas where inaccessibility is low, the ability of the state to project power is high, and vice versa. Thus, rebels and insurgents can benefit from high levels of inaccessibility, providing safe-havens where they can (re-)mobilize and look after their support base.

Physical geography, in the form of terrain, forests and traversable land cover types and the relationship to conflict have been of interest to political and military decision makers, as well as scholars of peace and conflict. Mao (2005[1937]) argued that radical change should start in the periphery, while Tzu (2011) emphasized that attacks should be made with speed, surprise, and deception, or as "Sheng Tung, Chi Hsi" ("Uproar [in the] East, Strike [in the] West")". Similarly, Guevara (2004[1985], pp.8) emphasized that rebels be favored by "zones difficult to reach, either because of dense forests, steep mountains, impassable deserts or marshes."

Some country-level studies have explored whether countries with more mountainous terrain have an increased risk of conflict. Both Fearon and Laitin (2003) and Hegre, Gissinger and Gleditsch (2003) find more mountainous countries to have a higher risk of conflict. However, Collier and Hoeffler (2004) do not come to the same conclusion. Few countries are equally rugged across their entire territory (Pickering (2016) presents a measure of country ruggedness). Also, conflicts rarely engulf the entire territory of countries (Hallberg, 2012). Thus, to identify whether conflicts occur in more rugged areas of a country or not, requires us to operationalize both terrain, and conflict at the local level.

In his seminal book "Conflict and Defense", Boulding (1962) presented his Loss-of-Strength-Gradient (LSG) model, suggesting that a country's power culminates at its home base and decreases with distance from it. Buhaug (2010) adapted the LSG model to a domestic setting. Here the LSG model describes the projection of state capacity about the rebel capacity. While weak rebels who attack near the regime's capital will be defeated quickly, equally, weak rebels in the periphery can withstand a government offensive if inaccessibility is high.

In chapter 3, I introduce two different measures of physical inaccessibility: distance from the capital and the terrain (mountains and forests) that has to be traversed. Both of these factors influence the capability of the state to control and monitor its territory. The size of a country and consequently the distance that needs to be traversed to reach its peripheral territories implies a frictional cost. Thus, larger countries with large peripheral territories require the state to have the capacity to penetrate into such areas. These areas provide favorable terrain for insurgents, creating safe-havens that are hard to both monitor and control.

1.2.2 Political Exclusion and Socio-Cultural Inaccessibility

The previous section highlights the importance of physical inaccessibility to explain the location of armed conflict violence. However, another type of inaccessibility can be envisioned, a socio-cultural inaccessibility, which has largely been disregarded as an important cause in previous studies of conflict (Collier and Hoeffler, 2004; Fearon and Laitin, 2003). In this thesis, I conceptualize social and political exclusion as socio-cultural inaccessibility. Areas where politically excluded groups exist are less accessible of the incumbents, creating increased friction for the successful penetration of the state.

Areas inhabited by preferences different from the core are what Rokkan (1999) defines as cultural peripheries, which imply a higher political, economic, and cultural barrier to internal structuring. If left alone, these areas will not be integrated into the

state. Thus, how the state chooses to control such peripheries, through influence and authority, will affect the trajectory of these areas (Boone, 2003).

Chapter 3 argues that areas inhabited by politically excluded groups be more likely to experience conflict because they remain out of reach for the state, and this affects both opportunity and motivation. These groups are excluded from participation and life opportunities, which may create opportunities for conflict as a result of sociocultural distance. Also, being excluded from political participation might increase the motivation for violent action (Cederman, Gleditsch and Buhaug, 2013).

1.2.3 Poverty and Conflict

The relationship between low per capita income and conflict is among the most robust findings in the literature on intrastate conflict (Hegre and Sambanis, 2006). While there is no consensus on the mechanisms linking poverty to conflict, several propositions have been made. Collier and Hoeffler (2004) argue that poorer countries have an increased risk of conflict violence as impoverished individuals have nothing to lose regarding foregone income. Thus, potential rebels in more affluent countries have more to lose from taking up arms. This proposition is often referred to as the individual opportunity cost argument. Likewise, Jakobsen, De Soysa and Jakobsen (2013) shows that lower per capita income is related to an increased risk of conflict, also attributing this finding to the individual opportunity cost argument.

Another proposed explanation of the poverty-conflict nexus is that low per-capita income constrains the capacity of the state (Fearon and Laitin, 2003). Thus, more affluent countries have a lower risk of conflict, as these countries have the capability necessary to deter conflict, and topple potential challengers or secessionists, even when these arise in the periphery. Both Collier and Hoeffler (2004) and Fearon and Laitin (2003) employs the same measure, GDP per capita, and reveal the same correlation. However, their explanation for this correlation differs.

Subsequent studies of the poverty-conflict link have shown that the relationship between poverty and conflict might be causal (Braithwaite, Dasandi and Hudson, 2014). However, others argue that the poverty-conflict link might be spurious. Djankov and Reynal-Querol (2010) show that once they control for historical variables such as settler mortality rates and population density in the 1500s, poverty does not have an effect on conflict. Thus, they argue that historical variables determine the path to both economic development and peace, explaining the poverty-conflict nexus. Berman et al. (2011) show that local areas with higher unemployment rates do not have a higher risk of conflict, casting doubt on whether impoverished individuals are more likely to take up arms, as the individual opportunity cost argument posits.

Preceding these empirical and methodological complex studies was a discussion of whether poverty in itself leads to frustration and consequently to aggression. Davies (1962) suggested (a J-curve) that revolutions were more likely to occur following periods of economic improvement ending in a prompt reversal. Thus, individuals that fear their earnings and assets will be lost, and this increases their inclination to revolt. The frustration-aggression theory posits that individuals turn aggressive when basic material needs are not met (Van De Goor, Rupesinghe and Sciarone, 1996). Frustration

is thus seen as a necessary premise for violent behavior to take place (Berkowitz (1989) provides an overview).

Later, Gurr (1970) proposed his theory of relative deprivation, where conflict was argued to become more likely if relative deprivation increases. When individuals' discrepancy between what they think they are entitled to, and what they receive, differs, this can lead to frustration and potentially to aggression. Here, poverty can lead to conflict if the gap between actual and anticipated living conditions diverges (Rustad, 2016).

One alternative avenue to expanding our understanding of the poverty-conflict nexus is to explore the relationship at a spatially disaggregated level. Several recent papers have shifted focus from studying the poverty-conflict nexus at the country level to studying the relationship at the local level (Buhaug et al., 2011), or in the form of inequality between groups (Østby, 2008) or regions of countries Østby, Nordås and Rød (2009); Hegre, Østby and Raleigh (2009).

None of these factors alone explains conflict, and conflict itself is not homogeneously distributed across a country's territory (Buhaug and Lujala, 2005). Country-level studies have proposed various explanations for the poverty-conflict nexus. Few studies match their theoretical expectations with data at appropriate levels of analysis. This is unfortunate, as both poverty and inequality show considerable variation within states (Elbers, Lanjouw and Lanjouw, 2003; Kanbur and Venables, 2005).

One limitation of existing local studies of the poverty-conflict Nexus has been the lack of data on poverty at the local level. This is not surprising, given that conflicts mainly take place in developing countries, where data quality is poor, even for the national statistics (Jerven, 2013). Several alternative proxies of prosperity have been used; economic activity by dividing GDP on local population estimates (Nordhaus, 2006), economic activity by measuring night-time luminosity (Shortland, Christopoulou and Makatsoris, 2013) or the use of survey data on individual and household assets (Østby, Nordås and Rød, 2009).

In chapter 4, I address this shortcoming by measuring poverty as people's experiences of a lack of basic needs. This measure is taken from the Afrobarometer and measures whether respondents lack basic needs such as food, water, medicines or medical aid, fuel and cash income in the previous 12 months. The results show that areas with higher poverty have an increased risk of conflict violence, but only when local institutions are poor, and if there is also a high level of local ethnic group grievance present. The findings in chapter 4 also shed doubt on the individual opportunity cost argument, as the level of local unemployment does not seem to influence the poverty-conflict nexus. The individual opportunity cost argument suggests that, if unemployment increases, impoverished individuals' willingness to take up arms should increase in tandem. The results do not suggest such a link.

While country-level studies show that civil war is more likely in impoverished countries, I show that local poverty is related to local conflict, but only when the quality of local institutions is poor, and when local experiences of group injustice are high. Thus, while reducing poverty lowers the risk of conflict, development policy should also strengthen local institutions and focus on reducing group experiences of injustice. There is the reason to believe that strengthening local institutions will also

improve living conditions and be more efficient in resolving group tensions.

1.2.4 Institutional Quality

In chapter 5, we ask whether local institutional quality matter to peace. The majority of cross-national studies on institutions have focused on national institutions typically confined to the capital, such as quality of governance (Hegre and Nygård, 2015) or democracy (Hegre et al., 2001).

Several studies have explored the relationship between democracy and conflict. Hegre et al. (2001) found that the relationship between democracy and conflict resembles an inverted-U shape. Here, consolidated democracies or autocracies have the lowest risk of conflict, where inconsistent or transitionary regime types have a higher risk of conflict. Gleditsch and Ruggeri (2010) also find democracy to decrease the possibility of conflict onset. However, we argue in chapter 5 that institutions are more than what leaders prescribe regarding elections, civil liberties or power sharing. Our argument highlights the quality of local institutions, meaning how well they work an important factor influencing the risk of conflict.

The existing studies of institutions and conflict propose three explanations for the pacifying effect of institutions. First, high-quality institutions can reduce commitment problems, providing constraints on executive power and avoiding the need for political challengers have militias to keep political elites in line (Walter, 2015). Second, institutions can deter grievances from becoming violent by including diverse political entrepreneurs, and provide a channel for marginalized groups to express their discontent (Hegre and Nygård, 2015; Cederman, Gleditsch and Buhaug, 2013). Third, Fearon and Laitin (2003) emphasize that institutions proxy state penetration, where high-quality institutions reduce the opportunity space for potential and active rebels.

While studies such as Hegre and Nygård (2015) and Walter (2015) finds that strong political institutions have a pacifying effect, we argue in chapter 5 that institutions are not confined to the capital or uniformly distributed, but that institutions and their quality vary widely across a country's territory. Measures of the quality of state-level institutions do not necessarily mean that all parts of a country's territory have equally well-functioning institutions. Chapter 5 addresses this shortcoming.

1.2.5 Severity of Recurrences

Studies show that once a country has experienced one conflict, it is at risk of experiencing another (Collier et al., 2003). Walter (2004) argues that economic well-being is an important explanation for why conflict recurs. However, (Call, 2012) argues that economic factors be less important than ethnic and religious determinants. Others highlight how the first conflict ended as an important explanation for recurrence. Both Quinn, Mason and Gurses (2007) and Toft (2010) show that conflicts ending in a victory is less likely to recur than conflicts ending in a settlement.

A less voluminous bulk of studies has explored the severity of conflicts. Studies have shown that conflicts fought in democracies are less deadly, as the governments are more careful in applying massive levels of violence (Lacina, 2006; Balcells and

Kalyvas, 2014). Other factors increasing severity are external support for rebels and small governing bodies (Balcells and Kalyvas, 2014). The presence of natural resources has also been linked to increasing the severity of conflict.

In chapter 6, I relate these two central topics and investigate the severity of conflict recurrences. In particular, I explore whether recurring conflicts are more or less deadly than initial conflicts. I emphasize that how the previous conflict ended is an important explanation for the severity of recurring conflicts. In particular, I find that recurring conflicts where the previous conflict ended in a peace agreement, are less deadly than conflicts that ended in victories.

1.3 Analytical Approach, Data, and Methods

In this section, I present the analytical approach of this thesis and highlight common issues and limitations regarding research design, data, and methods encountered in the five subsequent chapters. While the articles each have their unique research design and approaches to data and the analytical design, there are several shared methodological issues and potential threats to valid inference. First, all five subsequent chapters benefit from spatial data and Geographic Information Systems (GIS)[3]. Thus, I first introduce GIS and why spatial data is special data (Anselin, 1989), along with elaborating briefly on common GIS operations used across the Chapters. Second, I present key data and the potential limitations of these, followed by a methodological discussion of threats to valid inference.

1.3.1 Geographic Information Systems (GIS)

GIS is a computer-based system for collecting, manipulating, analyzing and presenting geographic data. Chrisman (1999) provides a comprehensive definition, where GIS is defined as the organized activity by which people:

- Measure aspects of geographic phenomena and processes.
- Represent these measurements, usually in the form of a computer database, to emphasize spatial themes, entities, and relationships.
- Operate upon these representations to produce more measurements and to discover new relationships by integrating disparate sources.
- Transform these representations to conform to other frameworks of entities and relationships. These activities reflect the larger context (institutions and cultures) in which these people carry out their work. In turn, the GIS may influence these structures.

Geographic objects or phenomena are represented as either vector or raster spatial data types. Vector data is often referred to as discrete objects as these features have clearly defined boundaries. The most simple vector representation is the point, where its position on a plane is given by a pair of x and y coordinates. In this thesis, vector point data is used to represent conflict events and cities. More complex objects can be shown using lines (two or more x and y coordinate pairs), or polygons (four or more x and y coordinate pairs). Country borders or roads may be represented by line features, while objects such as sub-national administrative units or the extent of conflicts are possibly best represented using polygon features.

Raster data consists of a matrix of pixels and is often referred to as continuous fields, in the sense that there are no clearly defined boundaries. Raster data is often used to represent elevation, population density or temperature. These are continuous in the sense that all locations on the surface can be assigned a value.

[3]I use PostGIS and R for all spatial data operations. See Obe and Hsu (2015) for an overview of PostGIS, and Bivand et al. (2008) for an introduction to spatial data analysis in R

Which type of representation we choose to use, put constraints on variation, both within and between units. In raster data, the number of details we can represent depends on the number of pixels we use, or more technically, the resolution (n rows $*$ n columns in a study area). Somewhat similarly, the quality of vector data depends on the number of vertices (break-points) we include. For instance, representing the coastline of Norway with only 1,000 vertices will lead to a much coarser representation than one using 100,000 vertices (when n vertices $\rightarrow \infty$ it approximates the true object). I refer to Longley et al. (2015) for a comprehensive overview of GIS.

In this thesis, GIS is primarily used to generate the variables employed in the statistical analysis, echoing Gleditsch and Weidmann (2012) who see "the ability to generate new measures from spatial data and modeling spatial processes as the key promise of GIS in international studies."

The workflow used in chapters 2 through 5 is to first identify a common unit of analysis. As spatial data comes in several types (vector or raster), file formats or have different resolutions, GIS must be used to combine these data together with a minimum loss of precision. Combining spatial data from various sources is not trivial, but raises the spatial scale problem, as identified by Atkinson and Tate (2000). How do we make spatial data fit together? Moreover, how do we aggregate spatial data of interest to the common unit of analysis such as districts, regions or countries?

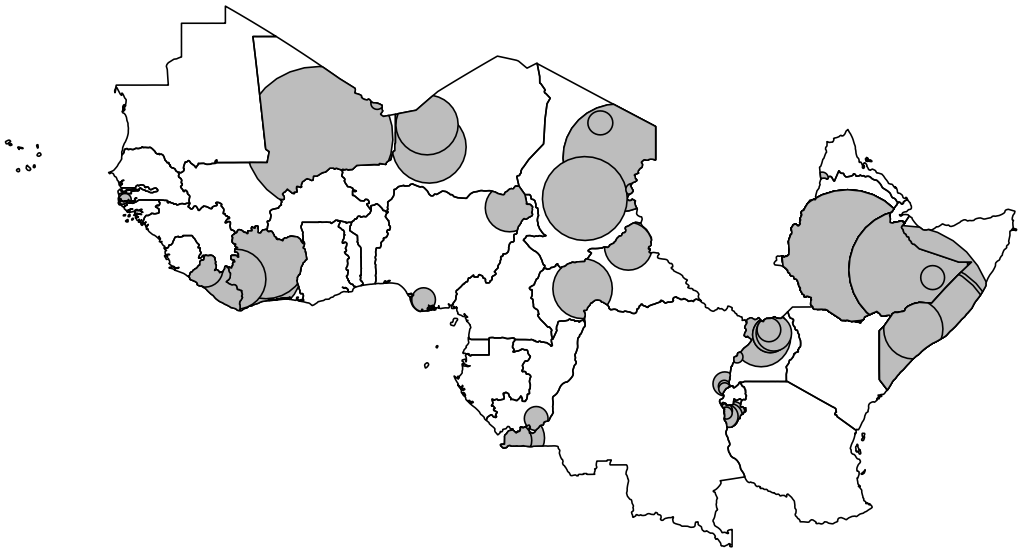
I represent sub-national districts, regions and grid cells using polygon data. These constitute the main unit of analysis and require all data to be aggregated to this level of measurement. To combine conflict data as points with polygon data, I use a point-in-polygon identification technique called intersect (De Smith, Goodchild and Longley, 2013). This identifies the number of conflict events within each unit. I also use intersect operations to combine polygon data with polygons for administrative units. For instance, in chapter 6, I use the intersection between conflict polygons and country polygons, to measure the area of a country affected by conflict, and whether this area differs between initial and subsequent conflicts. Figure 1.8 shows the conflict site polygons superimposed on country polygons.

In Chapter 2 through 5, I aggregate raster data to the sub-national units. For instance, population and elevation data both come in raster format. Thus, I aggregate these raster data to the sub-national unit by identifying the pixels that are inside the unit. This type of GIS operation is called zonal statistics; that makes it possible to estimate the distribution of pixel values within the unit. For instance, taking the mean elevation pixel value, or calculating the distribution of people from a population raster within a polygon unit. Chapter 2 provides a more comprehensive overview of spatial aggregation techniques.

1.3.2 Data

Throughout this introduction, I have highlighted the importance of local conditions in understanding why armed conflict only affects some parts of a country's territory. To answer the research questions presented in the introduction, I develop two spatial datasets. In chapter 2, I introduce the PRIO-GRID dataset, a spatial data framework for the local study of armed conflict. The PRIO-GRID has been developed to serve as a

Figure 1.8: Subset of Sub-Saharan African countries and conflict scope, after 2001



framework for the combination and aggregation of spatial data from various sources. It provides sub-national measures at the grid cell level, where each grid cell covers 0.5 x 0.5 decimal degrees. I apply the PRIO-GRID empirically in chapter 3. In chapter 4 and 5, I show the benefits of georeferencing survey data, which makes it possible to combine survey data with spatial conflict data.

Why local characteristics and their relation to conflict have received less attention in conflict studies is multifaceted. Initially, scholars focused on interstate conflicts, highlighting the characteristics between states, rather than within states. Second, the availability of data on local characteristics within countries has been very limited, at least until quite recently.

Gleditsch and Weidmann (2012) highlights two reasons why scholars interested in intrastate conflict increasingly use spatial data; the need to generate data at smaller units of analysis, and to obtain new types of information, such as the scope of conflict, the terrain within the conflict area, or other local characteristics.

To answer the research questions introduced at the beginning of this thesis, more nuanced data at the local level had to be collected and combined. However, as Atkinson and Tate (2000) argue, a key problem arises when spatial data obtained at different scales is to be combined. A common unit of analysis must be identified.

To address the spatial scale problem, I create two spatial datasets used in this thesis. The first dataset is a global spatial data framework consisting of grid cells, the PRIO-GRID, which enables the combination of spatial data from a wide range of different sources and formats. The second dataset consists of sub-national districts and regions. Here, I matched the district and region name of each respondent in the Afrobarometer survey, with spatial polygons representing each respondent's correct home district or region (GADM, 2012). By identifying the location of respondents, I could determine whether conflict had taken place in the home district or region of

each respondent. To determine this, I aggregated the number of conflict events that took place after the completion of the survey.

The PRIO-GRID dataset is a spatial data framework that makes it possible to combine spatial data originally obtained at different scales, stored in different data formats and different data types, into one unit of analysis, the grid. By using quadratic grid cells as the unit of observation, it enables the researcher to explore variation within countries. It also makes it possible to combine local data on conflict, with local explanatory variables. I describe the PRIO-GRID in greater detail in chapter 2, and in chapter 3, I employ the PRIO-GRID dataset to explore how inaccessibility shapes the motivation and opportunity of conflict.

In chapter 4 and 5, I make use of the georeferenced survey data and aggregate these data to the district and region level. The result is a spatial dataset of polygons, containing information on the average response from respondents in that unit, relating to poverty, political conditions, health, and grievances.

Measuring Local Conflict Violence

The majority of studies of conflict and civil war explore the determinants of onset or duration of conflict. Both Collier and Hoeffler (2004) and Fearon and Laitin (2003) explore the characteristics of countries that make conflict more likely to break out. The onset of conflict is typically coded as a binary outcome, taking the value of 1 in the first year of conflict, followed by a 0 for subsequent years. On the contrary, duration models of conflict typically investigate characteristics that predict when conflicts end, where the outcome is the duration of conflict from initiation to termination (see e.g. Cunningham, Gleditsch and Salehyan (2009) or Fortna (2004)).

With the shift to more spatially disaggregated studies, there has been an increased demand for high-quality data on the location of armed conflict. This has resulted in georeferenced conflict data of conflict onsets (Holtermann, N.d.), the scope of conflict (Buhaug and Gates, 2002; Hallberg, 2012), and, more lately, conflict event location (Raleigh and Hegre, 2005; Sundberg and Melander, 2013).

The conflict site dataset Buhaug and Gates (2002) identifies the centroid of conflicts in the UCDP Armed Conflict Dataset 1946-2000, as well as the scope of the conflict, given by its radius. These data provide approximations of conflict-affected areas, but do not vary across time. The conflict site dataset has later been updated by Hallberg (2012) covering 1989-2008. In addition to covering additional years, it captures annual variations in extent. I use the conflict site dataset in chapter 6, to estimate the extent of conflict in initial and subsequent conflict episodes.

Following the interest in more and improved spatial data on conflicts, Buhaug and Rød (2006) developed more nuanced polygons to represent conflict. One potential issue with using polygons to represent conflict is that they potentially introduce excessive spatial autocorrelation in data, as discussed below.

Raleigh, Hegre, Karlsen and Linke (2010) released the Armed Conflict Location Event Dataset (ACLED) that included the precise location of conflict events from written sources (news articles, reports). The ACLED dataset provides point locations on conflict events, as well as properties of each event. This development was followed

by Sundberg and Melander (2013) with the release of the UCDP Georeferenced Event Dataset (UCDP GED). One potential issue with the relying on news reports is urban bias, where the location reported by news agencies is often generalized to larger cities or regions for easier identification by readers. I use UCDP GED in chapter 3, 4 and 5, but also use ACLED for robustness check in chapter 5.

Measuring Conflict Severity

How to measure fatalities of war has been subject to controversies (Spagat et al., 2009; Obermeyer, Murray and Gakidou, 2008). The main controversy relates to the large variations in estimates of such fatalities between datasets. Much of this debate relates to what types of fatalities we are counting. Also, no method of estimating casualties from war is exact, hence "estimation" (Spagat et al., 2009).

In chapter 6, I focus on battle-related deaths. For the period 1946 to 1988 I use the PRIO Battle Deaths Data (Lacina and Gleditsch, 2005), and complement this with the UCDP Battle-Related Deaths dataset (UCDP, 2015) for the period 1989 to 2014. While the data contains fatalities among warring parties, they also include civilian casualties caused by crossfires and collateral damage. However, the data does not contain one-sided violence (Eck and Hultman, 2007) or non-state violence (Sundberg, Eck and Kreutz, 2012).

The two datasets provide a best-estimate, as well as a high and low estimate of battle-related deaths. While the two datasets differ, Wischnath and Gleditsch (2011) shows that they both illustrate the same trend - that conflict severity is waning (see Pinker (2011) for a more comprehensive overview of this trend).

While severity is traditionally conceptualized as battle-related deaths, other types of severity approximations may be envisioned. Thus, in chapter 6, I also investigate the size of conflict as an alternative measure of severity. Conflicts vary in their spatial extent across time. If severity regarding casualties is lower in some conflicts than in others, this might also be reflected in the spatial extent of conflicts. This measure is taken from the PRIO Conflict Site dataset (Hallberg, 2012). The conflict site dataset represents, for each conflict-year in the UCDP Armed Conflict Dataset (Pettersson and Wallensteen, 2015)), the spatial extent of conflicts as circular polygons. This makes it possible to calculate the area of the spatial intersection between a conflict polygon and country polygon, as a share of the total area of the country.

Georefencing Survey Data

Georeferencing is the process of defining the location of an object or a phenomenon in space. Typically by assigning geographical coordinates. Survey data contains a rich pool of interesting variables. However, when such survey data is not georeferenced, combining this survey data with other data sets is complicated or even impossible. In chapter 4 and 5, I use georeferencing to connect spatial references to Afrobarometer respondents. I use information available in the survey on each respondent's home district or region, to identify the appropriate polygon. The two chapters show the rich potential that lies in survey data if we make such survey data compatible with other datasets.

Chapter 4 aggregates respondents' response values to the district level, while chapter 5 aggregates to both the district and region level. Next, district and region names available in the survey make it possible to identify the corresponding sub-national administrative polygons, taken from the Global Administrative Areas (GADM) data set. While identifying the corresponding GADM district polygon for each Afrobarometer district name should be a trivial task, it poses several challenges. This has mainly to do with the fact that there are inevitable variations in naming conventions. These discrepancies range from differences in the use of upper- and lower-case letters to more significant spelling differences of district names with or without accented letters as well as whitespace issues. I elaborate on how to overcome such discrepancies in chapter 5 and its accompanying appendix.

1.3.3 Data Limitations

When we explore both national and sub-national characteristics and their relationship to conflict, we are limited by data availability and data quality. As noted earlier in this Chapter, the availability of data at the sub-national level has been relatively poor until only very recently. This restricts our empirical possibilities, in the sense that we can only observe the characteristics where data is available, or we can collect it.

Another issue is missing data in existing data. In general, this has to do with the inability of many developing countries to collect sound measures of their economic performance. Unfortunately, these are also the same countries that experience conflict. As Høyland and Nygård (2012) argue, "It is therefore not uncommon at all to read quantitative articles on civil war dynamics with 50, 60 or even 70 percent missing data."

Researchers limited by poor data availability often need to use innovative ways to create approximations or make better use of existing data. As Raleigh, Witmer, O'Loughlin and Denemark (2010) argue "models are hindered by the lack of information for predictors at a level of disaggregation (e.g. for grids of 100 km) that match conflict data." However, researchers are using novel methods to create improved local data.

One example of data shortage is the lack of good measures of local state capacity. To overcome this limitation, Harbers (2015) provides a potential solution, creating proxies of local state capacity by using data on tax collection by local governments and combining these with economic activity measured by night-time luminosity. She validated her local state capacity measure by comparing it with satisfaction rates of the quality of local services and found it satisfactory.

In chapter 4 and 5, we make better use of existing survey data by georeferencing it. This makes it possible to connect the survey data with data on the location of conflict. Without an explicit spatial reference, there would be no way of knowing which respondents experienced conflict or not. The georeferencing of surveys shows that, while we might be limited by data availability, there is also significant untapped potential in existing data.

Survey Data Limitations

Above, I elaborated on the benefits from georeferencing survey data and aggregating it to sub-national administrative units. While this provides an interesting pool of sub-national data, such disaggregation does not come without caveats.

The Afrobarometer, used in chapters 4 and 5, aggregates survey data to the sub-national district and region level. Chapter 4 looks at perceived poverty, grievances and local institutional quality using survey data, while chapter 5 uses perceptions on local institutions (as well as other response variables). As the survey sample is drawn to make a nationally representative sample, disaggregating it to sub-national entities might pose threats to its validity.

Afrobarometer uses a clustered, stratified, multi-stage, area probability sample, where they first select the sub-national units by regional stratification, reducing the likelihood that certain ethnic or language groups are left out. Next, they randomly draw respondents within each region (see Afrobarometer (2016) for an extensive description). As the Afrobarometer survey covers a limited number of African countries, we should also be cautious about generalizing to other contexts. I address these limitations further in chapters 4 and 5, as well as in the respective appendices.

1.3.4 Methodological Issues

When learning about factors shaping the real world, whether quantitative or qualitative, the primary goal in social science is to design research that will produce valid inferences about social and political life (King, Keohane and Verba, 1994). Without sound inference, we cannot establish direction or strength of causal relationships. King, Keohane and Verba (1994) divide inference into *descriptive inference* and *causal inference*. They define *descriptive inference* as "using observations from the world to learn about unobserved facts." while *casual inference* is defined as "learning about causal effects from the data observed"[pp.8].

In quantitative research, we infer by using numbers and statistical methods. Most common are regression modeling to estimate the relationship between one or more explanatory variables (X) and an outcome (Y). Such statistical modeling can be used both to describe the statistical relationship between X and Y, and to make causal claims where we infer that X causes Y. Following Antonakis et al. (2010), we assume a model:

$$y_i = \beta_0 + \beta_1 x_i + e$$

where the dependent variable y is the outcome of interest, i is a sequence of 1 to n observations, β_0 is the intercept where x equals 0 or the average fixed-effects. β_1 represents how much change in one unit of x affects the outcome of interest y. e accounts for the error term which consists of any unobserved causes of y, as well of measurement error. A fundamental assumption when identifying how x relates to y, is that e is not correlated with the explanatory variable x. If x is not correlated with e, x is exogenous, meaning that it does not correlate with omitted causes. If it does, x is endogenous, and the slope of β_1 is adjusted to include the effects of unmeasured causes. Thus, the estimated effect of x on y is biased and which direction this bias drives the estimate can be up or down or even change sign ($^+/_-$) (Antonakis et al., 2010). If estimates are biased, causal inference cannot be made.

However, when do we know when we can make causal claims? As Box-Steffensmeier, Brady and Collier (2008) argue, "most regression analyses in social sciences are probably useful descriptions of the relationship among various variables, but they often cannot properly be used for causal inferences because they omit variables, fail to deal with selection bias and endogeneity..." Making causal claims is of importance for policy-makers and society. Thus, it is of importance that scholars know when they can make causal claims (Antonakis et al., 2010).

In randomized experiments, individuals or units are assigned to treatment or control groups randomly. Thus, every unit has the same chance to be allocated to a treatment or control group. Thus, both groups are approximately similar on backgrounds variables (given sample size). This makes it possible to estimate the causal effect of x on y, called the Average Treatment Effect (ATE) (Gerber and Green, 2012). However, when using observational (non-experimental) data, we do not know whether individuals or units in the treatment or control group are similar, which is likely to produce biased results (Angrist and Pischke, 2008).

As Antonakis et al. (2010) highlights, the list of reasons why x is endogenous (correlate with e) is long. I refer to their article for a comprehensive list, but address

three threats to validity highlighted in the subsequent chapters; omitted variables, selection bias, and simultaneity. Also, I will discuss how spatial data threatens the independence assumption in regression, where units affect each other or are mutually affected by common influences (O'Loughlin, 2003; Bivand et al., 2008).

Sources of Endogeneity

Omitted Variable Bias

If x is correlated with e, x is considered endogenous and thus biased. To cancel out this correlation, all possible sources of variance in y that correlated with the explanatory variable must be accounted for in the model. One way of doing this is by statistical adjustment, where we measure and include all possible sources of variance of y in the regression model (Antonakis et al., 2010). However, knowing or even including all sources of variances in y is often unfeasible. Alternatively, we can account for stable characteristics of units by making use of hierarchical or longitudinal panel data. The former refers to units that are nested, for instance, sub-national units in countries, while the latter refers to repeated observations of the same sub-national unit. Panel data allows us to account for unobserved unit specific constant effects, conventionally termed the "fixed-effects".

A fixed-effects model makes use of the repeated observations within, for example, countries or the same sub-national unit over time, to control for unobserved but fixed omitted variables (Angrist and Pischke, 2008). The fixed effects model benefits from only exploring the within-unit variation. Using a fixed-effects model can account for any possible unobserved heterogeneity in the level of y, which otherwise would be captured by the disturbance term e. This reduces the potential that confounding variables are driving the results. For spatial entities nested within a country, fixed-effects become a useful mitigation to reduce potential omitted variable bias. However, some argue that fixed-effects models be akin to "throwing out the baby with the bathwater", meaning that we cancel out potentially interesting variables that can be correctly modeled by including such variables into the model (Beck and Katz, 2000).

One frequently used way of including fixed-effects, is to add a separate dummy variable for each observation (level 1), indicating which unit it is nested within (level 2). For instance, sub-national units (level 1) nested within countries (level 2). For spatial panel data, Elhorst (2003) provides an extensive overview.

In chapter 3, we explore how inaccessibility within countries affects the risk of conflict violence. Here, the unit is the grid cell, nested within countries. Thus, we include country fixed-effects to account for unobserved heterogeneity between the countries. The country fixed-effects are the unobserved country-invariant constant effects, common to those grid cells nested within a country (Antonakis et al., 2010). Similarly, chapter 5 uses country fixed-effects, but where sub-national units are nested within countries.

In chapter 4, I explore the effect of poverty on armed conflict risk. Using regions as the unit of analysis, I use a fixed-effects model where units are observed over time. Here, constant time-invariant effects of each region are captured by observing a region multiple times. By observing the same area at multiple points in time, it becomes

possible to control for its time-invariant fixed-effects.

Chapter 6 explores whether subsequent conflict episodes have different severity levels than initial conflict episodes, regarding casualties and extent. To limit the comparison to conflict episodes within the same conflict, I use conflict fixed-effects. Here conflict specific fixed effects are captured by the conflict dummy variables.

Omitted Selection

In chapter 6, when comparing the severity of initial and subsequent conflict episodes, I highlight the issue of selection bias, where conflict recurrence is not randomly assigned. Meaning, which conflicts recur, and which conflicts do not recur, is not randomly assigned, but x is explained by other factors. Thus x is correlated with e. Why some conflicts recur, and why others end after an initial fight, is multifaceted.

However, I argue in chapter 6 that democratic countries experience more small-scale and prolonged conflicts because they need to balance the use of military power with humanitarian and political considerations, to a much greater extent than authoritarian regimes. Also, democratic countries are more inclined to end conflicts through negotiations, than crushing their opponents by brute force. Studies also show that negotiation leads to splintering (Lounsbery and Cook, 2011), leading to smaller factions. Thus, democratic countries are more likely to experience conflict recurrences, than autocratic countries. In chapter 6, I account for whether the conflict host country is democratic. While this accounts for some of the omitted selection bias, I cannot fully rule this out.

Another closely related issue is having the non-representative selection of sub-national units included in surveys. This is highlighted in chapter 4, where I use survey data from the Afrobarometer to explore the relationship between poverty and conflict. The question remains whether the sub-national units included are considered a representative sample? While Afrobarometer randomly selects sub-national units to survey, the undersampling of conflict-affected regions might bias the selection process. However, I explicitly show in chapter 4 that conflict-affected areas are indeed sampled.

Another concern is that splitting a nationally representative sample into smaller sub-samples reduces the internal representativeness. However, as chapter 5 shows, excluding units with a low number of respondents does not severely affect the results.

Simultaneity

Simultaneity arises when two variables simultaneously cause each other (Antonakis et al., 2010). In Chapter 5, for example, we propose that local institutions affect the risk of conflict. However, there is the reason to believe that local conflict and conflict risk also affects the quality of local institutions. Then, e correlates with x, making β_1 biased.

One alternative solution to this is to identify an instrument Z that affects the risk of conflict Y, but only through its effect on X. However, as we discuss in Chapter 5, identifying such an instrument is infeasible. Thus, we make use of matching techniques to reduce the potential issue of simultaneity bias. The idea of matching is to match each treated unit with a similar unit in the control group on some background variables describing each unit. This mimics (but is not) the randomized assignment found in true experiments, where we determine each unit's propensity to receive the treatment

(local institutional quality) as a function of selected covariates (Antonakis et al., 2010). The results do not affect the overall conclusion.

Spatial Dependence

An important set of assumption of the least squares regression model are the independent error terms under the null hypothesis Cliff and Ord (1972). However, spatial autocorrelation in the outcomes typically violates this basic assumption yielding too low standard errors (Ward and Gleditsch, 2008). Revealing spatial dependence in outcome and residuals is thus of great importance. Hubert, Golledge and Costanzo (1981, p.224) provide a concise definition of spatial autocorrelation: "Given a set S containing n geographical units, spatial autocorrelation refers to the relationship between some variable observed in each of the n localities and a measure of geographical proximity defined for all $n(n-1)$ pairs chosen from n". Cliff and Ord (1973) coined the concept of spatial autocorrelation, highlighting the importance of taking spatial autocorrelation into account in traditional statistical models. To not do so was to risk misspecifying such models.

Most social phenomena display some degree of spatial autocorrelation. This relates to Tobler's first law of geography (TFL), which states that "All things are related, but nearby things are more related than distant things" (Tobler, 1970). The TFL implies a high level of positive spatial autocorrelation. While exceptions to TFL exist, many attributes belonging to social life display positive spatial autocorrelation - nearby units are more similar than remote units. On the contrary, negative spatial autocorrelation, dissimilar patterns, is what Kao and Bera (2013) refer to as "The Curious Case of Negative Spatial Dependence", typically arising following a competition between nearby units.

Datasets, where units are completely independent of each other, are rare. However, what to do if observations are dependent on each other? If spatial autocorrelation exists, several alternative spatial econometric model specifications are possible, that take this spatial dependence into account. Spatial econometrics is defined by LeSage (1999, pp.1) as "special models and econometric methods for dealing with spatial economic phenomena ... introduced using spatial data sets". An important note relating spatial econometric models is that many models still are being developed, and while progress is being made, not all models are available in standard statistical software.

For linear relationships, spatial dependence can be incorporated into the standard linear regression by introducing a spatially lagged dependent variable (**Wy**), called the spatial lag model, or through autocorrelation in the error term, called the spatial error model. The spatial lag model is of interest when we believe that spatial interaction exists, for instance, when a conflict in unit j affects the risk of conflict in unit i. The spatial error model is useful when we believe that there exist omitted spatially correlated variables (Anselin, 2001). Alternative spatial models exist for binary outcomes, such as the spatial probit model (Franzese Jr and Hays, 2009).

The spatial error model takes the form:

$$y = X\beta + \lambda W\mu + e$$

Here, the usual OLS model is complemented with the spatial structure (λW) in the spatially dependent error term (μ) (Dormann et al., 2007). While I make use of the spatial error model in chapter 2, I apply a spatially lagged dependent variable in chapters 3, 4 and 5 to account for the spatial diffusion of conflict from one unit to the next. The spatial lag model takes the form:

$$y = \rho Wy + X\beta + e$$

Here, y is the dependent variable; ρ is the autoregression parameter, X is the matrix of exogenous variables, and W is the spatial weights matrix. Thus, y is spatially lagged and included on the right side of the equation. Importantly, Bivand et al. (2008) highlight that modeling the spatial structure by including relevant covariates and their functional form can be beneficial if spatial autocorrelation comes from model misspecification. Thus, we should explore significant covariates to explain the spatial dependence before canceling it out using spatially lagged variables.

In chapter 2, I introduce the spatial lag (and error) model in the study of sub-national units. The article shows that when classical OLS models fail to account for spatial dependence or spatially correlated omitted variables, the result is biased upwards, suggesting a much stronger effect than the actual relationship. However, when accounting for spatial dependence, the spatial lag model returns an estimated half of what the OLS returned.

Previous studies have shown that conflicts in one country increase the risk of conflict in neighboring countries. For instance, Most and Starr (1980) show that interstate conflicts diffuse, where the conflict in one country increases the risk of conflict in neighboring countries. Studies also show that intrastate conflict in one country increases the risk of conflict in neighboring countries (Gleditsch and Ward, 2001). Often, such intrastate conflicts spread within countries from their initial location (Schutte and Weidmann, 2011; Raleigh, Witmer, O'Loughlin and Denemark, 2010).

To account for the fact that conflict in a sub-national unit increases the risk of conflict in neighboring units, the statistical models in chapters 3, 4 and 5 includes spatially lagged dependent variables. In chapter 3, we include the logged number of conflict events in contiguous sub-national units. The same variable is included in chapter 5, while chapter 4 includes a simpler dichotomous control variable for conflict in neighboring units.

Modifiable Areal Unit Problem

Generating aggregated variables from points (survey or conflict locations) located within polygons (administrative units) raises a concern about *The Modifiable Areal Unit Problem* (MAUP). The MAUP is that results can change when the unit size or a unit's borders are altered. When results change due to replacing the unit's size, from district to region, for example, this id referred to as a scaling issue. When results change due

to redrawing the borders of a zone, this is referred to as zoning (Fotheringham and Wong, 1991). First, scaling refers to how the results may change when we employ a different unit of analysis, for instance, regions instead of districts. Second, the zoning issue of MAUP relates to the results being sensitive to where the borders are drawn. A minor change in the drawing of a border could alter the distinct points aggregated to each unit, leading to a different aggregated value and consequently affect the statistical estimate.

In chapter 4, I explore whether the MAUP biases the result. First, I aggregate the mean of responses from respondents residing within a district polygon. Second, I aggregate the average of the replies from the same set of respondents to the larger region polygon. Next, I employ both datasets in two different models. The results remain the same and show that the MAUP is most likely not a potential source of bias in the estimates.

1.4 Overview of articles

The remainder of this thesis presents the five independent but related articles. In this section, I provide a brief summary of the subsequent chapters and their main finding.

Chapter 2
Chapter 2, titled "PRIO-GRID: A unified spatial data structure", co-authored with Halvard Buhaug and Håvard Strand and published as an article in Journal of Peace Research (Tollefsen, Strand and Buhaug, 2012), describes the development and application of a spatial framework for the disaggregated study of civil war. It emphasizes the importance of disaggregated research designs and the increasing use of georeferenced data. While this certainly is a positive trend, it necessitates geographic information systems (GIS) skills. Spatial data also comes in different forms, resolutions and file formats. This diversity poses several challenges to the data analyst wanting to combine such data into a uniform data structure.

The majority of existing studies have developed their unique data structure, which makes it difficult to compare findings of sub-national studies of conflict, and replication. Chapter 2 presents the PRIO-GRID, a stand standardized structure for storing, manipulating, and analyzing high-resolution spatial data. The dataset consists of grid cells covering the whole earth. Each grid cell covers 0.5 x 0.5 decimal degrees, which is approximately 55 kilometers x 55 kilometers at the equator. This resolution makes it possible to observe internal variation even in small countries.

Gridded data is exogenous of political and social processes. This exogeneity means that the borders are not shaped by, conflict, ethnic homelands, disasters or other socio-economic conditions. The dataset consists of data at the cell level, on a large selection of political, economic, demographic and environmental variables. While the initial version of the PRIO-GRID covered the years 1946-2008, it has later been updated to cover 1946-2014, and include many additional variables of interest to both conflict researchers and beyond (the new version is accessible at http://grid.prio.org). The article concludes by providing an example of how such disaggregated data may be used in a spatial-econometric setting, and raises the awareness of spatial dependence in sub-national studies of conflict.

Chapter 3
Chapter 3, "Insurgency and inaccessibility", co-authored with Halvard Buhaug and published as an article in International Studies Review (Tollefsen and Buhaug, 2015) studies the location of armed conflicts about both physical and socio-cultural geography. It highlights the importance of physical inaccessibility as a favorable element for the insurgents, showing that the location of conflicts tends to converge on such inaccessible areas.

Several scholars, activists, and practitioners have emphasized the importance of rough terrain and physical obstacles as hindrances to public surveillance, counter-insurgency, and territorial integration, thus providing opportunity and motivation for rebellion. Several examples from today's contemporary conflicts highlight that conflicts tend to locate in peripheral areas. One apparent example is the inability of

Western forces to defeat the Taliban and al-Qaeda in Afghanistan. Even more recently, the al-Qaeda in Islamic Maghreb (AQIM) retreated to the Adrar des Ifoghas Mountains in north-east Mali following the deployment of French Special Forces. Authorities from Mao to Che Guevarra have emphasized the importance of impassable territories as strategic and tactical advantages in conflicts.

Another type of inaccessibility may be envisioned where sociocultural exclusion and alienation from the core increases the threshold for the internal ordering of the state. Areas occupied by excluded and minority groups influence identity formation and perception of collective grievances.

The chapter represents the first comprehensive evaluation of how physical and sociocultural inaccessibility relates to contemporary civil wars. We use GIS and the PRIO-GRID to create sub-national measures of terrain, settlement patterns, and their political status, as well as conflict events. The statistical analysis shows that our various dimensions of inaccessibility increase the risk of intrastate conflict events. However, we only find weaker support for substitutability, where the inaccessibility indicators retain their individual effects when included in the same regression model.

Chapter 4

Chapter 4, "Experienced Poverty and Local Conflict Violence", is single-authored, and examines the effect of poverty and conflict history on the risk of local conflict. While studies show that impoverished countries have an increased risk of conflict, there is no consensus on the mechanisms explaining the relationship.

The majority of existing studies of the poverty-conflict nexus have explored the role of national characteristics in increasing the risk of conflict. However, more recently, a number of studies have examined whether impoverished areas within countries have a greater risk of conflict. However, these studies have mainly relied on objective proxies of poverty, such as night-time luminosity, household assets or gridded data on economic activity and their results have been mixed.

In this chapter, I examine the relationship between experienced poverty, measured as lack of basic needs, and conflict. The measure includes people's lack of basic needs, such as food, water, medicines or medical help, fuel or a cash income.

By analyzing geo-referenced survey data for 4,008 districts, across 35 African states, the results show that impoverished areas are more likely to experience conflict violence. While this result is robust, also in a fixed-effects modeling framework, the results also indicate that the poverty-conflict nexus is moderated by other local conditions such as local institutional quality and ethnic group grievances. Poverty is only related to conflict if local institutions are weak and of low quality. Also, the results show that poverty is more strongly associated with conflict if group grievances exist locally. If local groups perceive themselves as fairly treated by the government, poverty is less linked to conflict.

Chapter 5

Chapter 5, "Local institutional quality and conflict violence in Africa", co-authored with Tore Wig and published as an article in Political Geography (Wig and Tollefsen, 2016) highlights the importance of high-quality local institutions in reducing the risk of armed conflicts.

The chapter argues that existing studies of institutions and conflict have focused on national institutions, and failed to give emphasis to the role of local institutions in shaping conditions for conflict. We georeference survey data by matching name variables on districts with spatial units representing these units. Using survey data, we develop a measure of local institutional quality, including aspects of quality of; police, the judicial system, politicians, as well as levels of corruption.

The paper shows that the quality of local political institutions has a strong pacifying effect on the risk of local conflict, even when we account for national institutions. The relationship holds when controlling for some relevant factors like economic development, demographics, political opinions, urbanization and country-fixed effects. To alleviate potential simultaneity issues, we use matching techniques to make a valid inference. The results remain, and we find support for a pacifying effect on the quality of local institutions.

Chapter 6

Chapter 6, "Every Cloud has a Silver Lining: The Severity of Armed Conflict Recurrences", is single-authored, and investigates whether recurring conflict is more deadly than initial conflict episodes, by looking at the outcome terminating the initial conflict episode, and how local characteristics of the conflict location affect the number of battle-related deaths.

While some articles have considered causes of conflict recurrence, and what makes conflicts more deadly, no study to date has investigated empirically whether subsequent conflicts are more or less deadly than initial conflicts and if so what explains this variation in severity?

The article examines the severity of internal armed conflict recurrence. I generate a dataset of conflict episodes between 1946 and 2014 and explore whether recurrences are more or less deadly than initial conflict episodes. I use a fixed-effects regression model and find that subsequent conflict episodes are significantly less deadly than initial conflict episodes.

The results suggest that recurrences are less deadly than initial conflicts. One explanation is that recurrences is initiated following splintering of rebel groups, and thus waged by smaller factions. The results also point to a war weariness effect where subsequent conflicts are smaller because parties are less able to mobilize similar strength. I also find that recurring conflicts are smaller in geographical extent, but not significantly.

The article proceeds by looking at why subsequent conflicts are less deadly, and whether the outcome of the initial conflict can provide insight into this difference. The existing literature has shown that conflicts that end in peace agreements are more likely to recur than conflicts ending in victories. The results presented in this article show that when peace agreements fail, subsequent conflict becomes less deadly than conflicts that recur following governmental victories. The results also suggest that conflict recurrences where the initial conflict ended in a peace agreement affect a smaller area. In general, the results show that peace agreements indeed have a pacifying effect, even when such settlements fail.

1.5 Conclusion

The aim of this thesis has been to expand our knowledge of the local causes of intrastate armed conflicts. Through five independent but related chapters, I have shown how spatial disaggregation may benefit the study of civil war. The findings indicate that several local conditions affect the risk of local conflict; physical and socio-cultural inaccessibility, experienced poverty, and the quality of local institutions. This thesis also shows that even if conflicts ending in a peace agreement are more likely to recur, these recurrences are less deadly than recurrences where the initial conflict ended in a victory.

At the outset of this thesis, I presented the five research questions. Research question 1 asked "How does local physical and socio-cultural inaccessibility create opportunity and motivation for conflict?" This issue was addressed in chapter 3. The findings show that inaccessibility is a central factor affecting the location of internal armed conflict. Remote and peripheral areas of a country are more likely to see conflict than the more accessible parts of a country. The results also show that areas of a country inhabited by a politically excluded ethnic group have an increased risk of conflict. Thus, the results highlight the importance of accessibility as an important instrument for lowering the risk of local armed conflict.

Research question 2 asked "How does local poverty affect the probability of local conflict-related violence?" This article showed that experienced poverty increases the risk of local armed conflict, but only if the local institutional quality is poor. It also revealed that poverty is more likely to lead to conflict if the majority of individuals in the area perceive their group as being unfairly treated by the government. Also, the results showed that local conflict begets local conflict, suggesting that a local conflict trap might exist.

The third research question asked, "How does the quality of local institutions affect the probability of conflict-related violence?". Chapter 5 addressed this research question, and results showed that high-quality local institutions pacify, and this effect is unlikely to be due to omitted variables. Hence, building and maintaining of local institutions should be a top priority for policy-makers seeking to build more peaceful societies.

Research question 4 asked, "Are subsequent conflicts more or less severe than initial conflicts". Chapter 6 addressed this research question, and the results showed that subsequent conflicts are less deadly than initial conflict episodes. Further, the results indicate that the manner in which conflicts terminate is likely to affect the severity of subsequent conflicts. In particular, the results show that even if conflicts ending in peace agreements are more likely to fail, they are less deadly if they recur. Thus, negotiating for peace has a substantial effect on reducing severity.

The articles have highlighted the importance of territorial control, political inclusion, strong local institutions and negotiations as instruments to create a more peaceful society. The thesis has emphasized the importance of the local when studying internal armed conflict, and those local characteristics that are important in trying to understand contemporary armed conflicts. By developing new data and making better use of existing data, we can address knowledge gaps in the existing research agenda on civil

wars.

Several policy implications can be drawn from the results presented in this thesis. First, chapter 3 shows that improving accessibility to peripheral regions can significantly reduce the local conflict risk. Similarly, building and maintaining high-quality local institutions lowers the risk of local conflict. Thus, integration of peripheral areas through well-connected infrastructure and a dense network of state institutions of high-quality are significant contributions to lowering the risk of local conflict. Building high-quality institutions is even more important in impoverished areas, where these local institutions can provide basic needs and dampen grievances that may lead to conflict.

Second, the inclusion of peripheral ethnic groups and providing them with access to political participation significantly reduces the risk of conflict. Third, the pursuit of peace processes and negotiated settlements as the means to end war can reduce the severity of subsequent conflict by about half. Thus, even if we know that the risk of conflict recurrence is higher following peace agreements than victories, the prior agreements seem to have a substantial pacifying effect when peace fails.

1.5.1 Future Research

Findings presented in this thesis have several implications for future research regarding theory, methods and data. First, the results show that the existing literature conceptualizing state control, poverty and institutions as national phenomena, fails to recognize the heterogeneous nature of these factors. This thesis finds that inaccessible and impoverished areas, under the control of weak local institutions, have an increased risk of conflict. Without a spatially disaggregated approach, the understanding of the local causes of civil war will remain unknown.

Future research could make better use of the timing of events, to explore the spatiotemporal trajectory of conflict, and the spatiotemporal evolution from protests to war. Another interesting research topic would be to connect the location of conflict events with information on the home area of actors involved in the conflict. Combining this information would make it possible to identify whether the perpetrators of violence are locally rooted or traveling armed bands.

This thesis contributes methodologically, by raising awareness of spatial autocorrelation in local studies of conflict. Existing studies have to a large extent failed to address adequately the potential adverse effect spatial autocorrelation can have on valid inference. Thus, future studies should make large efforts to address spatial dependency correctly using models presented in this chapter, the spatial lag, and spatial error model. Controlling for spatial dependency should be customary for studies exploring spatial phenomena, not only in conflict research but research irrespective of thematic focus. Future research should also examine the spatial non-stationarity of results, and whether one model captures the variety of unique processes within the study area.

I have shown that the predominant focus on conflict as national phenomena is unfortunate. A shift towards a local focus allows for a more nuanced understanding of where conflict breaks out, and the relevant local causes. I have contributed to this

change by two avenues. First, by combining existing spatial data into a common spatial data framework. Second, by making better use of existing data through georeferencing of survey data. Future research should focus on tapping the rich potential that lies in georeferencing of non-spatial data, to combine these with other datasets. Future research should explore the possibility in georeferencing surveys, textual material such as reports and directives, and interview transcriptions. By identifying location-specific information, assigning explicit spatial information to these texts makes it possible to utilize the information in new and innovative ways.

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2 PRIO-GRID: A unified spatial data structure

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PRIO-GRID: A unified spatial data structure

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Abstract

Contributions to the quantitative civil war literature increasingly rely on geo-referenced data and disaggregated research designs. While this is a welcome trend, it necessitates geographic information systems (GIS) skills and imposes new challenges for data collection and analysis. So far, solutions to these challenges differ between studies, obstructing direct comparison of findings and hampering replication and extension of earlier work. This article presents a standardized structure for storing, manipulating, and analyzing high-resolution spatial data. PRIO-GRID is a vector grid network with a resolution of 0.5 x 0.5 decimal degrees, covering all terrestrial areas of the world. Gridded data comprise inherently apolitical entities; the grid cells are fixed in time and space, they are insensitive to political boundaries and developments, and they are completely exogenous to likely features of interest, such as civil war outbreak, ethnic settlement patterns, extreme weather events, or the spatial distribution of wealth. Moreover, unlike other disaggregated approaches, gridded data may be scaled up or down in a consistent manner by varying the resolution of the grid. The released dataset comes with cell-specific information on a large selection of political, economic, demographic, environmental, and conflict variables for all years, 1946–2008. A simple descriptive data assessment of population density and economic activity is offered to demonstrate how PRIO-GRID may be applied in quantitative social science research.

Keywords

civil war, data, disaggregation, geography, GIS, grid

Introduction

While the quantitative study of armed conflict traditionally is carried out at the country level, contemporary empirical work increasingly uses geographic information systems (GIS) data to capture conflict dynamics at a subnational level. Recent contributions to the disaggregation trend in cross-country research focus on conflict zones (e.g. Braithwaite, 2006; Buhaug & Gates, 2002), conflict events (e.g. O'Loughlin & Witmer, 2011; Raleigh et al., 2010), and conflict onset locations (e.g. Braithwaite, 2005; Buhaug, 2010). Likewise, the habitual unit of analysis – the country – is increasingly replaced by grid cells (e.g. Buhaug & Rød, 2006; Raleigh & Urdal, 2007), geographically distinct ethnic groups (e.g. Buhaug, Cederman & Rød, 2008; Weidmann, 2009) or subnational administrative entities

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(e.g. Murshed & Gates, 2005; Østby, Nordås & Rød, 2009) to better capture local variation. In addition, a host of local factors plausibly affecting the risk and course of armed conflict have become available in a geo-referenced format, such as population size/density (CIESIN, 2005), settlement areas of politically relevant ethnic groups (Wucherpfennig et al., 2011), economic activity (Nordhaus, 2006), natural resource sites (e.g. Lujala, Rød & Thieme, 2007), and various climate statistics (e.g. GPCC, 2010).

So far, there have been few attempts to coordinate these efforts. As a result, relevant datasets come in different formats, with different spatial resolutions, and rely on different data structures and coordinate systems (e.g. vector versus raster, point versus polygon data, NetCDF versus shapefiles, etc.). Many of these datasets are not easily combined and converted to a common unit of analysis. It currently takes in-depth GIS skills to be able to exploit the rich data and opportunities offered by disaggregated research designs. Most social science scholars do not possess such skills.

This article presents a solution to the technical challenges incurred by using GIS data. PRIO-GRID is a spatio-temporal grid structure constructed to aid the compilation, management, and analysis of spatial data within a time-consistent framework. It consists of quadratic grid cells that jointly cover all terrestrial areas of the world. The basic (static) version of PRIO-GRID contains cell-specific information on a limited selection of core variables (e.g. cell ID, cell area, population, and terrain characteristics), which may be joined with yearly files containing time-varying information measured specifically for each geographic cell (e.g. country code/name, population size, ethnic composition, etc.). Although PRIO-GRID was designed with peace and conflict research in mind, its potential applicability extends well beyond the study of civil war.

In the following, we briefly review arguments for when and how spatial disaggregation is pertinent and describe how this has been carried out in earlier research. The main content of the article is the presentation of PRIO-GRID – its structure, content, and applicability. We offer some examples to highlight how PRIO-GRID may be used to investigate new location-sensitive questions and offer more precise empirical tests of prevalent theories of civil war. We also discuss the prevalence of spatial autocorrelation in geographic data.

Disaggregation: Motives and solutions

What causes civil war? Why do some civil wars last longer than others? Such questions have been subject to systematic scientific scrutiny for decades. Until recently, however, quantitative comparable analyses have been carried out almost exclusively at the level of independent states. Consequently, India – with all its geographic, cultural, political, and socio-economic facets – is treated as a homogenous entity in exactly the same manner as, say, Iceland. This assumption of unit homogeneity may be trivial in some settings but is clearly problematic in others. For example, by modeling civil war outbreak as a function of gross domestic product (GDP) per capita, country population size, ethno-linguistic fractionalization, and other conventional country-level correlates of civil war, we may deduce that India is at higher risk of civil war than Iceland, but we are unable to understand why only some parts of India have a recent history of violence, much less get credible estimates of the local conflict risk. As Buhaug & Lujala (2005) show, there are often significant discrepancies between national aggregates and local-level conditions, and conflict zones are rarely representative of the country at large. Indeed, Cederman & Gleditsch (2009: 488) argue that 'many of the non-findings and conundrums in the existing cross-national research on civil war ... follow at least partly from the near exclusive reliance on country-level attributes.'

As a result of advances in technology and data availability, there has been a wave of disaggregated conflict studies in recent years, perhaps best epitomized by the special issue of the *Journal of Conflict Resolution* on 'Disaggregating Civil War' in 2009. We refer to the introduction (Cederman & Gleditsch, 2009) and subsequent articles in that issue for reviews of the earlier literature, and Sambanis (2004) for a critical discussion of limits to country-level research. At this stage, we limit ourselves to briefly presenting a few alternative means of disaggregation.[1]

We can identify three broad departures from the conventional country-level analysis of armed conflict. First, a number of recent publications focus on subnational groups and actors. For example, the Minorities at Risk project (Gurr, 1993) has collected data on 283 targeted politically active minorities and provides information on organizational structure and violent behavior for some of these groups. Öberg's (2002) study of ethnic rebellion introduced a link between ethnic groups and rebel organizations, analyzed against a control group of 370 ethnic

[1] Let us be clear: spatial disaggregation is not appropriate in all settings, and PRIO-GRID certainly should not be seen as a replacement for conventional country-level research designs. Rather, it constitutes a complement to existing datasets by facilitating capturing and analyzing features of interest at alternative (notably higher) levels of resolution.

Table I. Selected disaggregated studies of civil war

Unit of observation	*Unit size*	*Conflict data*	*Source*
Conflict zone	Various (radius variable)	Circular polygons	Buhaug & Gates (2002)
Grid cell	100 x 100 km	Irregular polygons	Buhaug & Rød (2006)
Conflict zone	Various (radius variable)	Circular polygons	Lujala et al. (2007)
Grid cell	100 x 100 km	Circular polygons	Raleigh & Urdal (2007)
First-order adm. unit	Various	Federal states	Urdal (2008)
Ethnic group polygon	Various	Ethnic polygons	Buhaug et al. (2008)
First-order adm. unit	Various	Irregular polygons	Østby et al. (2009)
Grid cell	0.08 x 0.08 decimal degrees	Event points	Hegre et al. (2009)
Ethnic group polygon	Various	Ethnic polygons	Weidmann et al. (2010)

groups without an armed agent (Öberg, 2002: 97). More recently, Wimmer, Cederman & Min (2009) have constructed a catalogue of politically relevant ethnic groups for most countries in the world and coded these groups against the conflicts in the UCDP/PRIO Armed Conflict dataset.

Cunningham, Gleditsch & Salehyan (2009) introduce a new dataset, largely based on data from UCDP, focusing on variables specific to conflict organizations, such as troop size, organizational aspects, and technology. The Non-State Actor dataset is limited to organizations involved in conflict. It does not feature any control group, hence the dataset cannot be used to assess why some groups are involved in conflict and other are not. A complementary approach is offered by Carey, Mitchell & Lowe (2009), who focus on conflict organizations that are associated with the government, so-called Pro-Government Armed Groups.

A second set of contributions uses subnational administrative entities as the unit of observation (e.g. Østby, Nordås & Rød, 2009; Weidmann & Ward, 2010). Districts and provinces are easily identifiable, with clear geographic boundaries, and their political relevance is undisputed. This approach is quite similar to dividing the world into countries, just at a different scale. However, the composition and outline of political subunits are prone to change over time and their extent and function may vary substantially between countries. While these issues have been known for country-level studies, the magnitude of the problem is much larger for the subnational level. A geo-referenced, global time-series dataset on subnational units that will remedy some of these challenges is currently under development at ETH Zurich (Deiwiks, 2010). While that project is very useful on its own merits, the underlying data structure is not ideal for managing non-political variables and conducting comparative cross-national research.

The final category of disaggregated civil war studies substitutes countries with grid cells that allow for within-country variation (e.g. Buhaug & Rød, 2006; Raleigh & Hegre, 2009). This approach is sometimes referred to as quadrat sampling in spatial analysis, where data are collected using an overlay of a regular form, such as squares or hexagonal polygons (de Smith, Goodchild & Longley, 2007). Gridded data are inherently apolitical entities; they are fixed in time as well as space and are insensitive to political boundaries and developments. For this reason, the grid structure might be deemed worthless and irrelevant for conflict research. Yet, the stationary nature of the grid structure is a significant advantage, allowing for units of observation that are identical in shape and completely exogenous to the feature of interest (e.g. outbreak or incidence of civil war). Moreover, unlike other disaggregated approaches, gridded data may be scaled up or down by varying the resolution of the grid. Hence, we find the grid structure ideal to our objective: to generate a unified spatial data structure that can facilitate GIS-based research on civil war (as well as other phenomena of interest).

Table I illustrates the diversity in disaggregated approaches to the quantitative study of civil war. In the next section, we present the structure and content of PRIO-GRID, before offering a simple illustration of how it can be applied in social science research.

Why standardization?

The departure from the singular country-year data structure has been multifaceted. The plethora of methodological approaches to the local study of civil war opens up possibilities for empirical triangulation and solid generalizations. However, the many alternative solutions found are ineffective for the field as a whole. Data collected by one project are often incompatible with data from other projects. Time and money are wasted trying to reconcile basic but arbitrary differences. The standardization we

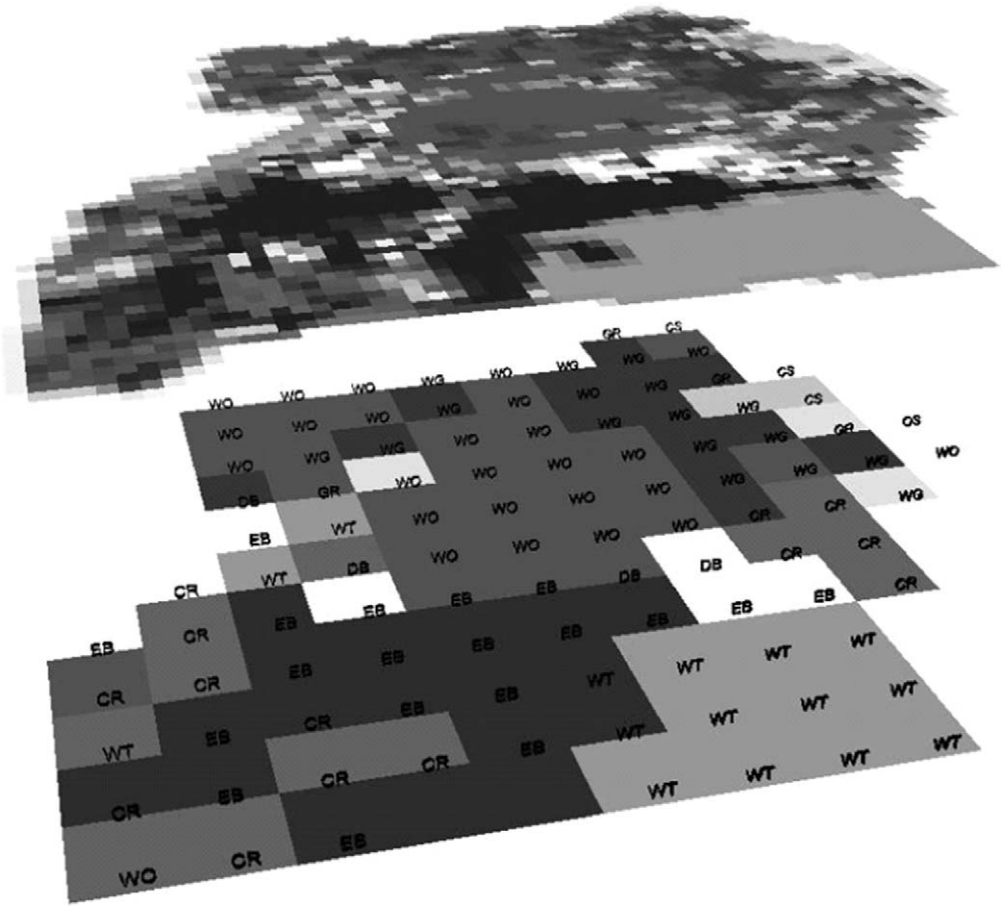

Figure 1. Conversions of a high-resolution land cover raster to grid structure
The figure illustrates how a high-resolution raster (top) is represented in PRIO-GRID (below). The string codes denote different land cover classes (Hansen et al., 2000; 0.08° × 0.08° resolution); CR = cropland, CS = closed shrubland, DB = deciduous broadleaf forest, EB = evergreen broadleaf forest, GR = grassland, WO = woodland, WT = water.

propose is limited to the ways in which gridded data are collected, managed, and stored, but it is important nonetheless.

GIS data come in a variety of different formats. A central distinction runs between raster data (analogous to bitmap or image pixels) and vector data. The latter type is further divided into point, line, and polygon data. Each data format has comparable advantages and disadvantages, and converting data between formats (e.g. from raster to vector) is complicated and may result in loss of information. An additional challenge concerns the unit resolution, which may range from exact geographic coordinates of point features (e.g. oil rigs) via rasterized data at various pixel resolutions (e.g. oil fields) to data available at federal state level (e.g. value of oil production). Various resampling techniques include aggregation of higher-resolution data by calculating mean values for each desired unit of observation, disaggregation of same values into smaller units, and recalculation of cell values by applying geospatial interpolation techniques (see Longley et al., 2005).

Figure 1 illustrates how high-resolution land class raster data are converted into a smaller set of observations in the vector-based grid system. Only trained GIS experts are able to combine such data and convert the variables to a common set of observations. By streamlining spatial data in a unified grid structure, we believe PRIO-GRID constitutes a useful point of departure for scholars working with spatial data.

PRIO-GRID: Structure and content

PRIO-GRID is constructed by imposing a quadratic grid on the two-dimensional terrestrial plane using vector shapefiles, where each cell in the grid is represented by a square polygon vector feature. Each cell's attributes are represented in the attached dBase (.dbf)

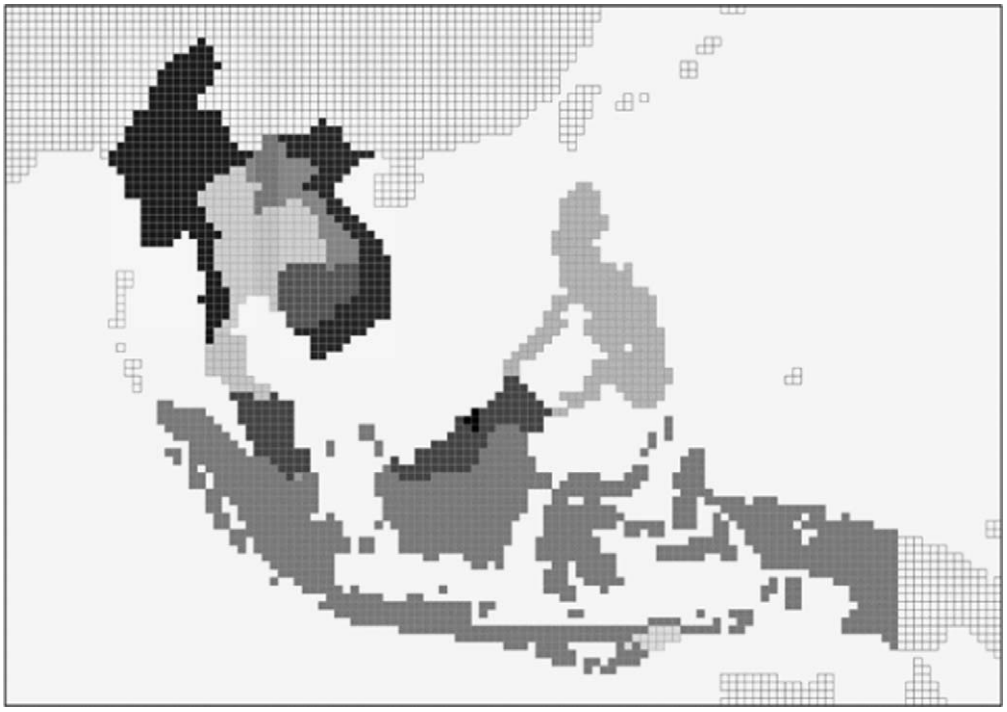

Figure 2. PRIO-GRID representation of Southeast Asia
The figure shows contemporary Southeast Asia in PRIO-GRID. Grid cells with the same color shading belong to the same country.

attribute table, which includes the variables and values for each cell observation in the grid.[2] Spatially, the grid adheres to the dominant geographic coordinate system (the World Geodesic System, WGS84) where the arcs separating the grid cells are defined at exactly 0.5 decimal degree intervals latitude and longitude, originating from the southwestern corner of the coordinate grid (90°S, 180°W). The complete global grid matrix consists of 360 rows x 720 columns, amounting to 259,200 grid cells. A large majority of these cells carry little relevance in most applications as they cover oceans and other unpopulated areas, but 64,818 cells contain at least a tiny sliver of land (Antarctica excluded), that is, cell land area $\geq$ 0.01 km^2 (slightly larger than a football pitch).

The chosen resolution is not coincidental: 0.5 decimal degrees latitude/longitude correspond to roughly 50 x 50 km at the equator. Hence, even very small countries such as Burundi or Bhutan are represented by multiple grid cells that allow within-country variation on spatial data. At the same time, the grid is sufficiently coarse to avoid excessively large data files (though nearly 65,000 observations in a single global cross-section hardly can be considered small).[3] The selected grid size also corresponds well with available GIS data on, for example, population size and other demographic components, infant mortality, and various climate statistics. Figure 2 gives a visualization of the spatial grid structure imposed on Southeast Asia.

PRIO-GRID consists of two sets of files. The first set is the static grid, which contains information on the outline and coordinate system of the grid, including a unique identifier for each cell (*gid*), stored in the attribute table. The static PRIO-GRID additionally contains a limited selection of time-invariant information. Among these are size of the landmass in each cell, expressed in square kilometers; cell-specific population size estimates, based on CIESIN's (2005) Gridded Population of the World v. 3.0 dataset (four variables are provided, giving population estimates for 1990, 1995, 2000, and 2005, respectively); data on mountainous and closed forest terrain (expressed as percentage of cell area covered); estimates of the local level of economic activity, based on

[2] For supplementary information on vector data, shapefiles, and dBase tables, see Longley et al. (2005).

[3] Studies that require a global time-series dataset may have to use a coarser grid dataset to limit data size, or apply a suitable sampling technique. Conversely, single-country studies may require a higher resolution (provided that the data measuring the features of prime interest are of sufficient quality and precision).

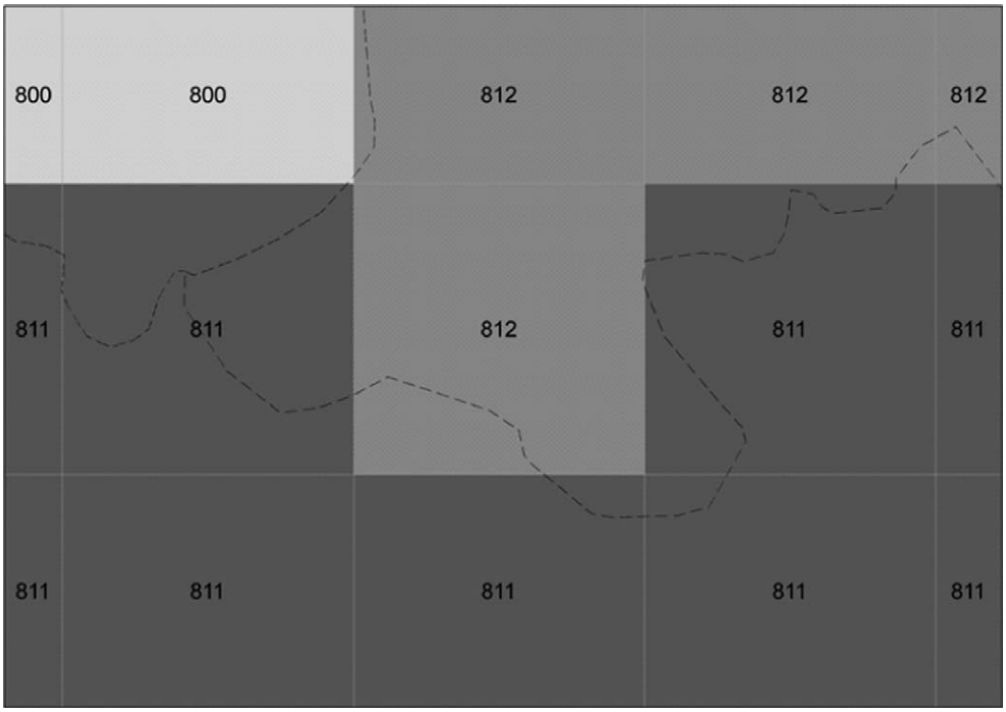

Figure 3. International borders and country assignment
The figure illustrates the border between Thailand, Cambodia, and Laos (dashed line), with grayscale coloring indicating the country to which each cell is assigned. The numbers in the cells refer to the Gleditsch & Ward (1999) numeric code for the corresponding country (800 is Thailand; 811 is Cambodia; 812 is Laos).

the G-Econ dataset (Nordhaus, 2006); and information on ethnic group settlements, represented by group ID variables for spatially distinct ethnic groups that are present in each cell, derived from the GeoEPR Dataset (Wucherpfennig et al., 2011). See the codebook (Tollefsen, 2012) for further details on the data included in PRIO-GRID.

The second set of files in PRIO-GRID contains time-varying indicators.[4] These files come in an annual format and are available for all years, 1946–2008. Each of the yearly grids presents a snapshot of the world for the corresponding year. A key component of the grid year files is the inclusion of country information, which allows combining measures of local conditions and events with country-level information (e.g. democracy score). Since the PRIO-GRID structure in effect is a two-dimensional spatial matrix, it contains no overlapping cells. Each grid cell can only belong to one country during a calendar year. This implies that some level of data manipulation was necessary in cases where a cell overlaps the boundary between countries and where a cell's territory shifts from one country to another during a year. The former challenge was resolved by assigning cells to the country that covers the largest share of the cell area (plurality rule). In case of temporal overlap, a cell was assigned to the country that had legal ownership of the underlying territory on 1 January in the year of observation.

Spatial information on the outline of independent states was derived from the CShapes dataset (Weidmann, Kuse & Gleditsch, 2010). CShapes includes data on start and end date and shape of all country boundary changes since 1 January 1946 and is consistent with Gleditsch & Ward's (1999) system membership list. This information was converted to the vector grid structure by allocating Gleditsch-Ward numeric country codes (*gwcode*) to all cells in correspondence with the plurality rule (Figure 3).

The annual PRIO-GRID files contain a number of time-varying covariates. Among these, we find cell-specific information on the onset and incidence of armed conflicts, represented by a conflict ID variable that corresponds to the case identifier in the UCDP/PRIO Armed Conflict Dataset (Gleditsch et al., 2002; Harbom & Wallensteen, 2010). In case of multiple conflicts within a cell, all conflict IDs are listed in the attribute table. Data on spatial extent of the conflict

[4] The static and yearly files can be combined by joining on the unique cell identifier (*gid*).

Figure 4. Aggregation from grid to national-level data
The figure illustrates how gridded data on annual precipitation for West Africa, 2004, are converted to aggregate country statistics by taking the mean of the cell values for all cells belonging to the same country. Darker colors indicate more rain.

zones were derived from the affiliated PRIO Conflict Site dataset (Hallberg, 2012).[5] Other data of potential interest to users include various climatic indicators that give cell-specific information on annual temperature and precipitation (see Theisen, Holtermann & Buhaug, 2011/12).

In addition to providing a default set of geo-referenced variables, the PRIO-GRID framework opens for easy expansion to include other GIS data. For example, the Demographic and Health Surveys (DHS) program now routinely registers GPS coordinates for the location of surveyed households. This opens up for generating a host of localized socio-economic indicators, including measures of economic vulnerability and inter-group inequalities (see Østby, Nordås & Rød, 2009).

The PRIO-GRID codebook (Tollefsen, 2012) includes user-friendly instructions on how to adapt and import additional data into the PRIO-GRID structure. Future releases of PRIO-GRID are likely to include a larger range of optional data. Moreover, there are concrete plans to release PRIO-GRID with other resolutions and let the user decide which grid size is more appropriate to her particular research objective. Alternative resolutions also facilitate sensitivity analysis and explicit consideration of possible biases relating to the modifiable areal unit problem (MAUP) and increasing spatial dependence with more refined data (see Openshaw, 1984).

A final benefit of PRIO-GRID is the opportunity to generate country aggregates, weighted by cell area or cell population, for example. While some data are unavailable in a geo-referenced format, the reverse is also true; some data come only as raster or gridded data. For example, there are good daily, monthly, and annual climate statistics at the global level, and measurements of temperature and precipitation are also available as geo-referenced data. However, these data are not released in a country-year format. Figure 4 illustrates how PRIO-GRID may be used to calculate country aggregate area-weighted estimates of total annual precipitation by spatially summarizing a precipitation layer (GPCC, 2010) into a CShapes layer containing the contemporaneous outline of states. Note that large countries, such as Nigeria, have substantial within-country variation in climate, implying that the country average rainfall estimate often is a poor proxy for local climatic conditions.

A note on spatial autocorrelation

According to Tobler (1970: 236) 'Everything is related to everything else, but near things are more related than distant things.' The assumption of independent and identically distributed (i.i.d.) observations made in inferential statistics often does not hold when working with geographic data. Positive spatial autocorrelation implies that similar

[5] Additional geo-referenced conflict data, such as ACLED (Raleigh et al., 2010) and the forthcoming UCDP Geo-Referencing and Event Dataset (Melander & Sundberg, 2011) may be imported by the user.

Figure 5. Z-scores from local Moran's I spatial autocorrelation test

values are clustered in space whereas negative autocorrelation denotes a larger heterogeneity in values (checkerboard pattern) than expected by chance. There are various ways to assess the extent of autocorrelation in the data.

A global spatial autocorrelation statistic (e.g. Moran's I) provides information on the degree of similarity among the observations in the whole study area and is analogous to Pearson's r. A complementary measure, the local Moran's I, further reveals where clustering of (dis-) similar attributes are located (Longley et al., 2005).

A spatially gridded dataset such as PRIO-GRID inevitably contains indicators with similar values among proximate grid cells. For example, a global Moran's I test shows that the world's population is concentrated in a small part of all inhabited land area ($I = 0.61$). To reveal where high or low values cluster locally we may apply a test for local spatial autocorrelation using the local Moran's I. This would reveal whether the similarity in neighboring values is greater than what we should expect by chance. Figure 5 illustrates the settlement pattern in India. Unsurprisingly, we find a high degree of spatial clustering in parts of the study area – both highly densely populated areas and areas with scattered populations. The next section illustrates briefly how autocorrelation may bias regression coefficients and how this bias may be corrected.

Demonstration of use

As a demonstration of PRIO-GRID, we investigate the spatial relationship between population density and economic activity in India. It is well established that economic development spurs urbanization; in fact, no country has ever experienced sustained economic growth without simultaneous growth in the urban population (UN, 2010). But a reverse effect is also at play: urban areas provide benefits for industries (economies of scale) through agglomeration and complementary services, a large pool of labor, and proximity to markets (Quigley, 1998). Regardless of which direction of causality is more important, we should expect population density and economic activity to be highly correlated in space (Figure 6).

A visual inspection of economic and demographic data for India verifies that areas with high population density overlap with areas with high economic activity. If we want to estimate the effect of population density

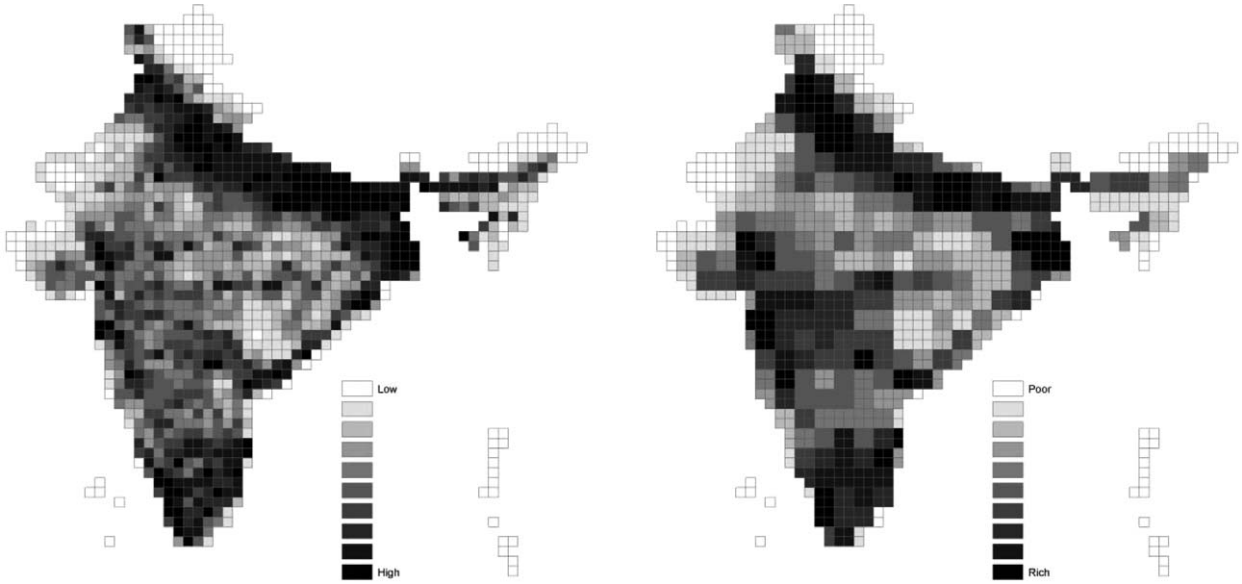

Figure 6. Population density and wealth dispersion in India, 1990
The figure visualizes local population density (left panel) and spatial distribution of wealth (right panel) for India, 1990. Darker cells indicate larger populations and higher income, respectively.

Table II. Determinants of local economic activity in India, 1990

	(1) OLS	*(2) Error*	*(3) Lag*
Cell population	3.00 (0.101)	1.75 (0.126)	1.37 (0.103)
Cell population^2	−0.107 (0.015)	−0.049 (0.015)	0.002 (0.013)
Cell area	0.795 (0.496)	0.841 (1.769)	0.101 (0.389)
Constant	0.630 (1.41)	0.329 (5.04)	0.641 (1.11)
rho			0.66
lambda		0.79	
AIC	5,028.7	4,614.3	4,535.6
N	1,197	1,197	1,197

The table shows regression coefficients with standard errors in parentheses, based on OLS, spatial error, and spatial lag models.

on local income, then, failing to control for spatial autocorrelation will return biased estimates. To illustrate, we run three simple regression models using the data shown in Figure 6: a naïve OLS model (1), a spatial error model (2), and a spatial lag model (3). All three models use the same dependent variable, PPP-adjusted Gross Cell Product (billion 1990 USD) (Nordhaus, 2006), which is regressed against cell population size (million), population squared, and cell area (1,000 km^2). The sample consists of all grid cells in India for a single cross section (1990).

The rationales behind the spatial lag and error models are somewhat different. The spatial lag model assumes that there is a diffusion process where the economic activity of a given cell is dependent on the activity in adjacent cells. The spatial error model is based on an assumption of spatially correlated omitted variables. By decomposing the error term into a spatial part and a unique part, the unique component should be i.i.d. (Ward & Gleditsch, 2008: 39). Although we would have a preference for the spatial lag model in this context, the purpose of this exercise is not to identify key correlates of local economic activity (for which the model clearly is underspecified) but rather to demonstrate how failing to account for correlation structures in the data may bias regression estimates (Table II).

All three models in Table II return a positive parameter estimate for population size. The magnitude of this effect varies between the models, however. The naïve Model 1, which completely ignores the spatial clustering in the data, suggests that the isolated effect of local

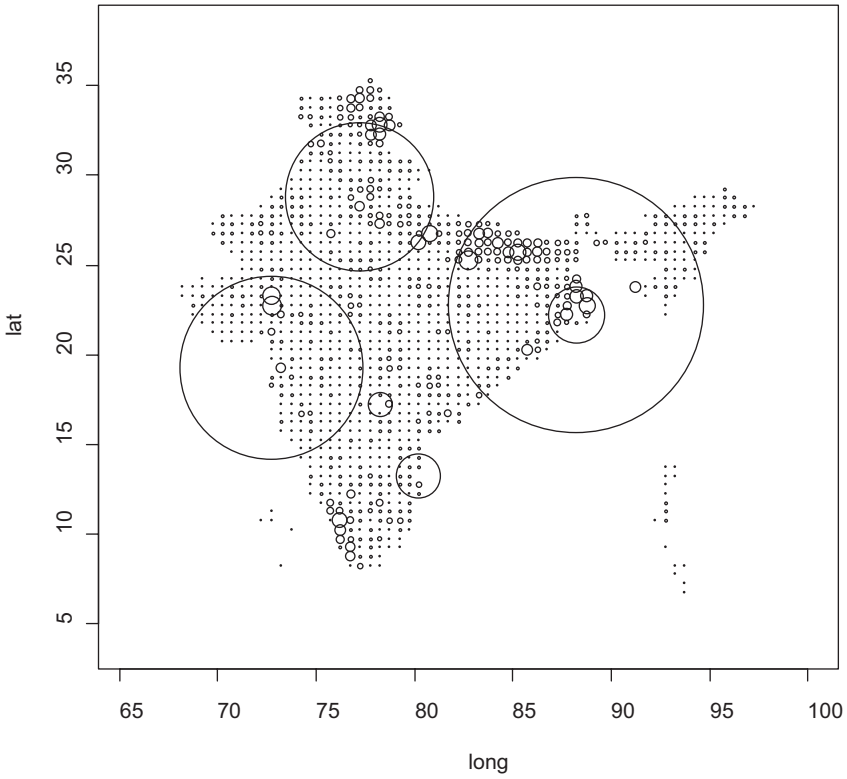

Figure 7. Effect of population on economic activity in India, 1990
The figure visualizes the spatial variation in the estimated population effect on economic activity across India.

population size on economic activity is quite strong, with an estimated addition of almost $3,000 per extra person in each cell (slightly less than the 2009 Indian average GDP/capita estimate and much more than the 1990 GDP/capita estimate). The spatial error model (2) returns a much more modest effect of almost $1,750 per extra person, which is closer to the 1990 PPP-adjusted average of $1,250. The coefficient for the spatial lag model (3), indicating an increase of $1,370 per extra head, is quite close to the 1990 estimate of GDP/capita for India. However, interpreting the estimate in this manner would imply assuming a constant population effect across space, which we know is not true. The spatial lag not only corrects for spatial interdependence, it also carries an effect onto the interpretation of the model (Ward & Gleditsch 2008: 44ff). When we take the spatial lag into account, we find that the relationship between economic activity and population is estimated to be more or less flat for large parts of the sample. Of the 1,197 cells in the sample, 593 have an estimated effect of less than $10 per extra person, while 750 observations have an estimated effect below $100. Only 106 cells have an estimated effect above the 1990 estimate of GDP/capita of $1,250. For five cells, the estimated effect for each additional citizen is an increase in GCP by more than $10,000. These are the cities of Kolkata (two cells), Delhi, Mumbai, and Madras (Figure 7).[6]

For more comprehensive empirical assessments using PRIO-GRID, we refer to Buhaug et al. (2011) and Theisen, Holtermann & Buhaug (2011/12).

Conclusion

The quantitative civil war scholarship is gradually recognizing the widespread disconnect between individual- and group-based theories of mobilization and political violence on the one side and country-level empirical analyses of armed conflict on the other. Most civil wars are highly local events and many have little impact on the society at large. Likewise, national data are often poor proxies for the conditions where conflicts occur, and their use may lead to ecological fallacy: inferring about individual behavior from aggregate data. As ever more geo-referenced data and user-friendly GIS applications develop, spatial disaggregation becomes more viable and attractive.

[6] The replication files include information on implementation of spatial regression models with PRIO-GRID in R and Stata statistical packages.

Spatial disaggregation is not without limitations, however. One challenge is the temporal dimension, which is restricted for many types of spatial data (notably for indicators derived from remote sensing); another is the demand for computational power when working with large datasets. Our recommendation is to let the research question and the theoretical approach determine whether gridded data or other subnational research designs are more appropriate than conventional country-level analysis. Accordingly, we offer PRIO-GRID as a supplement to conventional data structures in an effort to facilitate more detailed and spatially sensitive analyses of social phenomena and conditions that cannot be studied at the country level without loss of information.

Data replication

Data and supplementary documentation are available from http://www.prio.no/jpr/datasets.

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3 Insurgency and Inaccessibility

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Insurgency and Inaccessibility[1]

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A widely held belief within policy and practice contends that rough terrain and other physical obstacles to power projection hinder public surveillance, lower counterinsurgency capability, and generally constitute an important facilitator of rebellion. Likewise, sociocultural exclusion and alienation from the core are widely assumed to increase latent conflict risk through their influence on identity formation and perception of collective grievances. However, there is no scientific consensus on the empirical strength or significance of such a relationship, and many quantitative studies fail to find a robust link between a country's geographical or ethno-demographic characteristics and its estimated conflict risk. This paper represents a first comprehensive evaluation of how physical and sociocultural inaccessibility relate to contemporary civil wars. Drawing on recent advances in geographic information systems and georeferenced indicators of terrain, settlement patterns, ethnopolitical status, and armed conflict, we put the purported causal relationship to empirical test. A statistical analysis of civil-conflict events across post-Cold War Africa gives considerable support to the proposed theoretical framework, revealing that the various dimensions of inaccessibility all exert significant and substantive effects on local conflict risk. We find weaker evidence for the notion of substitutability; the inaccessibility indicators largely retain their individual effects when included in the same regression model.

> Provinces or districts peripheral to the national center [. . .] create (or reinforce) systems of local power which tend to reach extremes of violent, personalistic rule —patrimonial, even sultanistic—open to all sorts of violent and arbitrary practices. (O'Donnell 1993:1358)

Throughout the history of warfare, political and military leaders have observed the importance of physical geography in determining the nature and fate of rebellion. In his manual on guerrilla warfare, the Argentine Marxist revolutionary Ernesto "Che" Guevara (1961:29) developed a doctrine of guerrilla

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war around the notion that rebels are favored by "zones difficult to reach, either because of dense forests, steep mountains, impassable deserts or marshes." The role of human geography as a determinant of conflict dynamics has received less explicit attention. Yet, cultural cleavages shape participation and support in many contemporary conflicts, especially where ethnic lines overlap with deep economic and/or political cleavages (Cederman, Gleditsch, and Buhaug 2013a). Movements close to a regime rarely orchestrate civil wars; rather, the large majority of active insurgencies involve marginalized groups that enjoy strongholds in the remote countryside. Indeed, a notable feature of today's armed conflicts—from Afghanistan to Ukraine—is their tendency to cluster along peripheral, often porous, state borders that cut across traditional ethnic minority homelands (Horowitz 1985; Brancati 2006; Walter 2006).

Rugged landscape, rural hinterlands, and distinct cultural traits are central to the notion of inaccessibility. The concept is best depicted as a center-periphery continuum, where inaccessibility increases with the extent of mountainous or forested terrain, distance from major population centers and government strongholds, and local dominance of distinct minority culture. Inaccessibility of any of these kinds is widely believed to reduce state capacity and counterinsurgency capability by obstructing tax collection and public surveillance, identification of local allies, and projection of police and military power (Fearon and Laitin 2003). Under a *ceteris paribus* assumption, countries shaped by one or more of these inaccessibility dimensions should have a higher latent risk of armed civil conflict.

Despite such intuitive reasoning, the role of inaccessibility (with its various facets affecting the subnational risk of violent conflict) has received relatively little attention, and to our knowledge, no study to date provides an explicit and rigorous comparative assessment of the inaccessibility-conflict nexus. This article seeks to fill this void. We begin by discussing some important dimensions of inaccessibility, generally meant to signal extent of state presence, before we develop a theoretical argument concerning how local opportunities and motives for civil unrest increase with (i) the distance from the capital city; (ii) the availability of latent safe havens; and (iii) the sociocultural distance to the central power holders. Aided by recent advances in geographic information systems (GIS) and georeferenced data, we then develop complementary indicators of inaccessibility as well as aggregated indices, which are then systematically compared with georeferenced data on local conflict outbreak and prevalence. Through a detailed analysis of post-Cold War civil-conflict occurrence across Africa, we find considerable empirical evidence to support expectations: Civil-conflict events tend to concentrate in remote parts of countries, in locations characterized by substantial rugged terrain, and in areas inhabited by politically excluded ethnic groups. The analysis further corroborates earlier findings that local conflict risk is higher in more densely populated areas (especially those with minority populations), close to regional population centers, in relatively poor areas, and in areas surrounded by violent activity.

Inaccessibility

Accessibility, put simply, reflects the potential for interaction (Song 1996). It is a relational concept that concerns the nature of association between two entities, be they geographic locations or social actors. Extent of accessibility can be understood as a function of the interaction opportunity between two locations, discounted by a negative exponential function of distance (and other causes of friction) between them. The concept of accessibility thus is closely linked to the notion of social inclusion (Farrington and Farrington 2005). For a society to

become socially included, a certain level of accessibility must be offered. Accessibility in this sense is fundamentally related to life opportunities; a central government's ability to deliver public goods such as health and education—but also law and order—depends upon its ability, and willingness, to access the population.

The term inaccessibility as used in this article is best understood by considering the concept of *state penetration*, that is, the extent to which a central government manages to project political and military authority throughout its territory. State penetration is not uniform across space: Where inaccessibility is high, state penetration is low and vice versa. Yet, much of the contemporary literature on state capacity and civil war tends to treat capacity in a uniform, state-level manner with little consideration of subnational variation in the state's ability to monitor and control the population (Fjelde and de Soysa 2009; Besley and Persson 2010; Braithwaite 2010; Sobek 2010; Hendrix 2011).

Writing within the context of international relations and interstate conflict, Boulding's (1962) seminal loss-of-strength gradient (LSG) model postulates that a state's strength peaks at its home base and declines as power is projected across distance (see also Hegre 2008; Pickering 2012). The amount of power at disposal depends not only on the total state capacity ("home strength") and the distance across which power is projected but also on the cost of power projection, determined by the LSG. Whether a conflict of interest between a pair of actors is likely to escalate to the use of military force then depends on the distribution of available power. Where the projected power of state A is substantially higher than the local power of state B, state B should give in without resistance. Only where the actors appear to be near parity, or where there is uncertainty about their relative strength, should we expect a military contest to materialize. In reality, most states lack the capability to overcome the LSG and fight distant adversaries, and the large majority of modern militarized interstate disputes and wars involve land-contiguous neighbor states (Starr 1978; Lemke 1995).

Adapted to a domestic setting, the LSG model can be taken to express extent of projected state capacity, or local state control. As indicated in the left panel of Figure 1, a weak rebel group (R) that is located close to the government's (G) core should be defeated quickly, or decide not to rebel, because projected state power (the slope from line GH) is higher at the rebel's headquarters (R) than home strength (RK). On the right, however, the equally weak but peripheral rebels are able to withstand the central government given the latter's inability to project sufficient amount of military force to location R. At a more general level, this is akin to Lichbach's (1995:84) assertion that "if the dissidents' strength approaches that of the regime, the regime usually falls." See Buhaug (2010) for a more extensive elaboration of this model.

In empirical civil war research, the notion of inaccessibility has been restricted to imply physical remoteness, with country-aggregated statistics of mountainous terrain and (in a smaller set of disaggregated studies) distance to the capital provided as the main indicators of state penetration. While important, these factors reveal only half of the story. In this article, we also consider the human and social terrain, which captures central dimensions of sociocultural inaccessibility. This includes aspects of ethnicity and its political configuration. Where physical inaccessibility can be seen as especially relevant in providing opportunities for rebellion, cultural inaccessibility may be equally important in shaping people's motivation for engaging in a conflict against the state. The following sections expand on the two dimensions of inaccessibility—before we discuss, in more detail, how they relate to armed conflict.

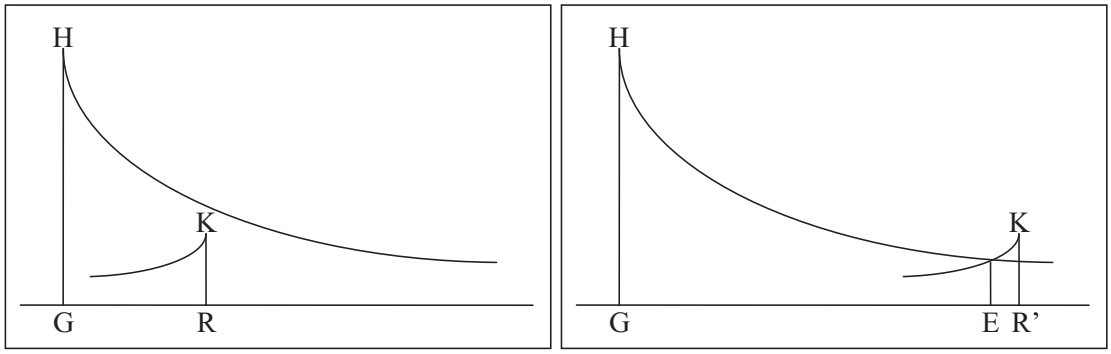

Fig 1. The Domestic Loss-of-Strength Gradient Model
Source: Buhaug (2010).

Physical Inaccessibility

There are two general determinants of physical accessibility: distance from the point of origin and the terrain that has to be traversed. Grundy (1971:45) provides a model of guerrilla confrontation where:

> [in] the context of guerrilla warfare, military space could be expressed in symbolic form as:
>
> $$MS = Mi^2 + Ob + San - CT,$$
>
> where MS = Military space; Mi^2 = Square mileage; Ob = Obstacles; San = Access to a sanctuary in a neighboring state; CT = Effective and defensible communications and transport networks. In this way a few square miles of mountainous jungle may be as strategically invulnerable as, let us say, a hundred square miles of prairie or, perhaps, a thousand square miles of flat plain crisscrossed by roads and telephone wires and dotted with airstrips and radio transmitters.

Absolute distance pertains to the geographical distance between points of interest, one of which is typically the capital city or another government stronghold. Because power diminishes as it is projected across distance, it follows that areas further away from state presence are less accessible, or can only be accessed with higher costs. The linear (or logarithmic) distance to the capital gives some indication of *de facto* periphery, but it is clearly a crude approximation of state penetration. For instance, a straight-line distance measure ignores geographic features such the type of terrain, the political landscape along the way (for example, whether the shortest distance crosses the ocean or a foreign country), and also the extent of available infrastructure.

It is not difficult to imagine more nuanced and theoretically appropriate quantifications of local inaccessibility. One such factor is the prevalence or absence of developed roads and other infrastructure. As detailed by Herbst (2000), roads can serve as proxies for broadcasting of authority, and nowhere more so than in poor, developing societies. The importance of communication networks is especially prominent in countries with challenging political geographies. Herbst's typology of African countries contains two such kinds: the first includes very populous countries with uneven, scattered settlement patterns whereas the second group contains countries with small, densely populated areas and large, scarcely populated hinterlands. It is in the vast areas *between* the main settlement clusters that the regimes struggle to exert authority, making these countries "seem almost impossible to govern" (Herbst 2000:152; see also Zhukov 2012).

A second and complementary physical determinant of inaccessibility is terrain. High mountain ranges and dense forests are fundamental determinants of interaction, migration, and development (and constitute natural barriers of nation–states). It is no coincidence that some of the most backward human communities today are found in inescapable parts of Borneo and the Amazonas rain forest.

The unevenness of statehood often bears a strong legacy to a historical state-building, wherein certain areas are consciously privileged by the core at the expense of what then becomes the periphery. Boone (2012:625) calls such dynamic "unevenness by design," which she argues is the result of three conditions that affect the cost/benefit calculation of power projection: (i) difficult-to-access regions; (ii) zones of low population density; and (iii) poor resource bases (see also Herbst 2000; Thies 2009). Modern technology, notably telecommunication, is less affected by physical obstacles than traditional modes of transport and communication, but it is not immune to geographic friction. However, while the notion of cyberwarfare—in which geographical distance may be truly irrelevant—has inspired science fiction for decades, its manifestation belongs to the future.

A third aspect of physical inaccessibility concerns proximity to an international border. Following the Peace of Westphalia in 1648, an international principle of nonintervention was established, whereby states were prohibited from interference with the internal affairs of any other state. Accordingly, regardless of the local terrain and the proximity to urban centers and other areas of government presence, having access to a neighboring country implies jurisdictional immunity and thereby increased inaccessibility.

Sociocultural Inaccessibility

Complementing its analysis of physical factors, this article considers key aspects of human geography, in particular ethnic diversity and characteristics of the local population, as well as their political status. In heterogeneous societies, the political elite tends to originate from (and represent) the dominant ethnic group(s), populated in core areas of the state. Depending on the nature of the regime, the elite may attempt to impose its preferences and ideologies on the rest of the population, and exploit marginal groups (see, for example, Hechter's 1975 work on internal colonization). These transactions are fraught with costs, the extent of which is dependent on the sociocultural distance between the core and the periphery. Areas inhabited by people with deviating traditions and preferences regarding language, minority rights, wealth redistribution, local autonomy, etc., will be harder to penetrate by the central government. For this reason, rulers with less extensive territorial ambitions (and those anticipating political contenders in the periphery) may decide to leave backward zones alone: not investing in infrastructure or bureaucratic and socioeconomic institutions, and refraining from providing costly public goods that serve no greater political purpose (Boone 2003; Raleigh 2014). Such areas can best be described as cultural peripheries (Rokkan 1999), which are liable to produce distinct identities through a process of "othering" (Cresswell 1996). Higher degrees of "othering" imply higher political, economic, and cultural barriers to internal structuring of the peripheral population.

Ethnicity is certainly not the only sociocultural determinant of inaccessibility; language and religion are other obvious (and often overlapping) features. Class, caste, and political ideology, too, may generate notable friction on projected state authority, although these traits tend to be less geographically clustered among the population. The political configuration of social cleavages is important in this context. While areas inhabited by minority groups are harder to monitor and control as a general rule, this is especially true when identity cleav-

ages overlap with inequalities in basic political and/or economic opportunities and privileges in the society (for example, where certain minority groups are subject to overt discrimination by the core). In accordance with our inclusive understanding of inaccessibility, it follows that the most socioculturally isolated areas are those hosting politically excluded and economically marginalized minority populations.

Linking Inaccessibility to Conflict Risk

Now that we have outlined some basic dimensions of inaccessibility, the next task is to explain how these dimensions relate to intrastate armed conflict. In doing so, it may be useful to consider: first, how inaccessibility affects opportunities for rebellion; and then, secondly, how it influences peoples' motivation to stage or join a rebellion.

Opportunity

The opportunity aspect of the inaccessibility-conflict link is probably the most intuitive one. While we are certainly not subscribing to a deterministic understanding of the role of geography, it is clear that physical obstacles to the exercise of state control in themselves create space for competing authorities, and poor monitoring and counterinsurgency capabilities imply that such contenders may rise to local power with little warning. Beyond facilitating clandestine mobilization and taxation by local elites, physical inaccessibility may be relevant to rebellion in at least two ways. First, rough terrain provides opportunities for establishing safe havens, undetectable and unreachable by governmental forces. Likewise, seeking refuge across the border—and/or enjoying tacit or direct support from a sympathetic neighboring government—facilitates training, regrouping, rearming, and trade (Salehyan 2009). This dynamic, which is analogous to Cunningham, Gleditsch, and Salehyan's (2009) notion of capability to resist, is especially efficient in the early phase of a conflict, when rebels tend to be vastly outnumbered by governmental troops. It does not follow that proximity to a neighboring state always comes with tactical and material opportunities to the nonstate actor, however. While the separatist insurgencies in South Ossetia (Georgia) and Eastern Ukraine undoubtedly have benefitted from strong ties to Russia, transnational Kurdish independence movements have been fighting the governments of Iran, Iraq, Syria, and Turkey for decades (Cederman et al. 2013b).

Second, insurgents may take advantage of terrain to inflict disproportionate damage to the regime, employing what Cunningham et al. (2009) refer to as power to target. The effectiveness of a regular army is restricted in rugged landscapes; swamps, jungles, and mountain ranges present major obstacles to armored vehicles and other heavy equipment as well as putting a strain on supply lines, and dense forest canopies hinder aerial detection. Moreover, rebels often have greater local knowledge, which further amplifies the asymmetrical nature of insurgency (Arreguin-Toft 2005; Fuhrmann and Tir 2009). A relevant example can be seen in the inability of Western forces to defeat the Taliban and al-Qaeda in Afghanistan (and before that, the Soviet's unsuccessful battle against the Mujahideen) in spite of overwhelming firepower and technology. The failure of the US engagement in the Vietnam War is also partly attributable to a military doctrine poorly suited to the physical environment, and the US Army's use of *Agent Orange* for defoliation operations was a deliberate (but failed) attempt to deprive the insurgents of cover and make them more vulnerable to conventional military attacks.

Remote or hostile terrain should not, however, have a substantive influence on local conflict risk in all societies. In developed countries with extensive and well-functioning local administration (for example, taxing authorities, police) and up-to-date infrastructure, physical inaccessibility may have little relevance for national security. However, in weaker states, physical obstacles to interaction are conducive to "unauthorized sequestering of resources by violent specialists as well as to seizure or damage of persons and property along the edges of authorized political claim making" (Tilly 2003:134).

Motivation

Beyond facilitating rebellion, physical and sociocultural periphery can also affect individuals' and groups' willingness to challenge the central government by violent means. Recall that inaccessibility implies costly interaction. This is relevant not only for the exercise of military control but for all kinds of center–periphery interaction, including provision of health care, schooling, and other public services. As outlined above, isolated areas may be associated with higher latent opportunities for anti-regime movements, but they also tend to enjoy fewer privileges than—or be targets of explicit discrimination by—the power holders.

Peripheral location and sociocultural alienation are important also in shaping unique identities and preferences and may contribute to a collective perception of unjust treatment by the core, which may provide motivation to mobilize against the state. Although preferences are affected by individuals' unique experiences, they will also be influenced by a score of common background factors, such as religion, language, economic welfare, and level of education. For this reason, there is a positive relationship between geographic and ideological proximity, whereby the physical distance between two points often serves as a good indicator of distance of preferences (Alesina and Spolaore 2003). Applying the same logic from the rebels' perspective, Gates' (2002:118) formal model shows that rebel leaders have to offer a "higher benefit stream [to distant rebels] to compensate the lower ability to punish defection."

Lastly, at a more general level, inaccessibility—manifested through a scarcity of interaction—reduces information flow, thereby increasing uncertainty about the relative distribution of preferences, capability, and resolve between state and nonstate actor dyads. Hence, civil unrest and military state response can in some cases also be the result of bargaining failure, due to misinterpretation and miscalculation of the opponent (Morrow 1989; Walter 2006).

Empirical Evidence

A glimpse at today's insurgencies provides many examples of conflicts that are fought in rural and peripheral areas where the state has limited reach: the Afar and Ogaden rebellions in remote parts of Ethiopia, the Nagaland and Manipur insurgencies in northeastern India, the separatist conflicts in Patani (Thailand) and Mindanao (the Philippines), and more recently, the retreat of al-Qaeda in the Islamic Maghreb to the Ifoghas Mountains in Mali, the separatist uprising in Eastern Ukraine, and the terrorist activities of ISIS in peripheral parts of Syria and Iraq. The relationship between a country's share of mountainous or forested terrain and the risk of civil war has also been subject to systematic scientific scrutiny—if normally only as a control variable. The findings from these studies are generally weak and inconsistent (Hegre and Sambanis 2006). The conflicting results might be ascribed to data and methodology issues; different studies cover different time periods and apply diverging operational definitions of conflict and terrain. A more serious concern is that studies using country-level aggregates often suffer from a mismatch between data and the hypothesized causal mecha-

nism, which may result in ecological fallacy. Country-averaged indicators do not contain information about the local variance of geographic features. Empirical evidence shows that most civil wars, especially separatist conflicts, are restricted to limited areas of the host countries (Hallberg 2012), and these conflict zones rarely cover a representative subset of the countries' terrain (Buhaug and Lujala 2005). Perhaps somewhat surprisingly, spatially disaggregated studies to date have been unsuccessful in establishing a robust and unambiguous terrain-conflict link (Buhaug and Rød 2006; Rustad et al. 2008).

There is more systematic evidence in favor of the distance indicators. Several recent studies report that subnational conflict risk is higher in locations at some distance from the capital city (Buhaug and Rød 2006; Clayton 2013), but proximate to regional population centers (Raleigh and Hegre 2009). Available empirical evidence further suggests that conflict events are more likely close to borders (O'Loughlin et al. 2012; Wischnath and Buhaug 2014), often involve transnational ethnic groups (Salehyan 2009; Cederman et al. 2013b), and conflicts that abut or cross borders also last longer on average (Buhaug, Gates, and Lujala 2009; Raleigh and Kniveton 2012). Moreover, it has been shown that civil conflicts and instability have a number of negative spillover effects, well beyond increased conflict risk, which may provide additional opportunities for armed challenges to the state (Gleditsch 2008; Iqbal and Starr 2008).

The notion that grievances related to social/cultural marginalization might affect the risk of conflict is not new (Gurr 1970) but, due to data limitations, it is only quite recently that this proposition has been subject to systematic large-N testing at the theoretically appropriate subnational (group) level. For example, Buhaug, Cederman and Rød (2008) postulate, and find, that ethnic groups settled in remote locations more often engage in conflict against the center. Later studies have reported a similar pattern for economic activity and spatial inequality, where the poorest parts of countries are, *ceteris paribus*, more conflict prone (Østby 2008; Buhaug et al. 2011; Cederman, Weidmann, and Gleditsch 2011). Moreover, using statistics on international telecommunication, Weidmann (forthcoming) shows that the conflict-inducing effect of shared ethnic ties extends beyond contiguous countries. Broadening the scope beyond traditional conflict, Raleigh (2014) shows how local political hierarchies across Africa have resulted in distinct conflict landscapes, whereby different forms of political violence cooccur within states, but with little spatial overlap. With the exception of Weidmann (2009), few studies have attempted to evaluate the relative importance of opportunity and motivation in explaining the purported geography-conflict association.

Propositions

Based on the reasoning outlined above, the general expectation to be tested in this article can be expressed as follows: Local civil war risk increases with extent of inaccessibility. From this, we formulate a set of testable hypotheses that refer to specific aspects of physical and sociocultural inaccessibility. The first concerns the relative location of an area:

Hypothesis 1: *Local civil war risk increases with physical distance from the government.*

Next, we consider the notion that availability of safe havens, by means of rough terrain or neighboring territory, increases conflict risk by making prospective rebels harder to detect and defeat through conventional military means:

Hypothesis 2: *Local civil war risk increases with proximity to potential safe havens.*

Finally, sociocultural inaccessibility should increase conflict risk by obstructing state monitoring of the local population, generating grievances related to lack of political and material privileges, favoring creation (and manipulation) of distinct local identities, and making defection less likely:

Hypothesis 3: *Local civil war risk increases with sociocultural distance from the government.*

There is no reason to expect all aspects of inaccessibility to be equally important in all contexts. For example, having access to safe havens across the border may well compensate for lack of rugged terrain, whereas proximate but culturally distinct groups may in effect be less penetrable by the state than ethnic peer settlements located far from the core. For this reason, we anticipate that distance, terrain, and identity profiles are best viewed as substitutable drivers of local conflict risk.

Data and Research Design

In order to test these propositions, we make use of version 1.01 of PRIO-GRID (Tollefsen, Strand, and Buhaug 2012). PRIO-GRID provides a global grid network with a resolution of 0.5 × 0.5 decimal degrees, comprising 64,818 unique terrestrial cells in a single cross section, excluding oceans and unpopulated areas (notably Greenland and the poles). In contrast to administrative entities, grid cells are inherently apolitical units that are fully exogenous to the phenomena of interest to this study. Furthermore, the grid framework is consistent in space and time, making it ideal for statistical analysis of spatiotemporal processes.

PRIO-GRID contains one realization per calendar year. Each cell is assigned to the country to which the majority of its land area belonged at the outset of the year, thereby allowing combining spatial data on, for example, population and terrain with country-level information on political system and economic growth rates. For this analysis, we cover all years between 1989 and 2010 for which high-resolution georeferenced conflict event data are available. Since the inaccessibility indicators are largely time-invariant, we use a simple cross-sectional data structure for the main models; factors that do change over time are set to represent the beginning of the period. Moreover, the analysis is limited to the African continent, which is the spatial coverage of the conflict data.

The outcome of interest to the empirical analysis is civil conflict. We use spatial data capturing the dynamics of civil conflict, based on Uppsala Conflict Data Program Georeferenced Event Data (UCDP GED; Sundberg and Melander 2013). The UCDP GED contains spatial and temporal information on fatal violent events, derived from the UCDP Armed Conflict Dataset (Gleditsch et al. 2002; Themnér and Wallensteen 2013). These data provide details on the precise location of specific civil-conflict events (we excluded intergroup and one-sided violence), aggregated over time to give the total cell-specific count of the number of reported conflict events since 1989. Given the highly skewed cell-specific count distribution, we use a log-transformed count as the dependent variable in the regression models.

A generous selection of measures of inaccessibility is used to capture the concept's various dimensions. Hypothesis 1, on distance to the government's core areas, is represented by the straight-line distance (log km) to the national capital, measured from the centroid of each cell. While intuitive and simple, this variable ignores the type of terrain that has to be traversed, the quality of the infrastructure, and the fact that governmental strongholds extend beyond the capital city. Two complementary measures of rough terrain are used to test Hypothesis 2, on the availability of safe havens. The first gives the share of the

cell covered by dense forests, whereas the second is an index of mountainous terrain. The forest-cover indicator is derived from 2009 GlobCover satellite imagery (Bontemps, Defourny, and Van Bogaert 2010), while the mountain data are computed using the United Nations Environment Programme's (UNEP) World Conservation Monitoring Center (WCMC) definition of mountainous terrain (UNEP-WCMC 2002). The fourth and final measure of physical inaccessibility, which also relates to Hypothesis 2, gives the straight-line distance (log km) from the cell centroid to the nearest neighboring country, inversed to let higher values denote greater extent of remoteness. All four indicators were normalized (that is, bounded within the interval [0, 1] to facilitate direct comparison and the construction of joint inaccessibility indices).[2]

The operationalization of sociocultural inaccessibility (Hypothesis 3) is less straightforward, in part because this concept is decidedly more fluid than its physical counterpart, and also because it gives intangible connotations that may be hard to quantify. Our admittedly crude approximation gives preference to the ethnopolitical status of the local population. In short, we measure extent of sociocultural inaccessibility as the normalized product of local population density and political status; that is, whether the population is denied representation and participation in national politics. In other words, all areas inhabited by an "included" ethnic group are considered fully accessible on this dimension whereas the inaccessibility of "excluded" areas is a function of local population size, with higher concentrations assumed to provide greater resistance to governmental authority (Buhaug et al. 2008). This variable is created by first combining local demographic statistics (CIESIN, Columbia University, and CIAT 2005) with the GeoEPR dataset (Wucherpfennig et al. 2011), which maps all politically relevant ethnic groups around the world since 1946, and then accounting for the groups' political status from the EPR dataset (Cederman, Wimmer, and Min 2010). Because ethno-political status is subject to changes over time (in part as a result of violent conflict), our strictly cross-sectional analysis captures the situation at the outset of the sample period (1989).

Ideally, we would want to account for sociocultural periphery and cleavages beyond the political configuration of ethnicity. Linguistic distance and religious differences would be natural candidates, although data on these factors are, to our knowledge, not available in a suitable, georeferenced format. The same limitation obviously applies to less tangible dimensions, including political ideologies, social class and networks, etc.

Table 1 lists the inaccessibility indicators. Further details on the spatial distribution and descriptive statistics of these variables are found in the Supporting information.

In the interest of parsimony, the models presented here include a limited set of controls. Previous research suggests a positive relationship between population size and local conflict risk (Hegre, Østby, and Raleigh 2009). Hence, we include an indicator of (log) cell population, derived from the Gridded Population of the World v. 3.0 dataset (CIESIN, Columbia University, and CIAT 2005). Population estimates represent the year 1990.

A second, robust predictor of civil conflict is low economic development (Hegre and Sambanis 2006). At a subnational scale, Buhaug et al. (2011) observed that relatively impoverished areas have a higher risk of conflict outbreak, whereas other

[2]In principle, it would be preferable to also account for the nature and salience of borders, as those that follow mountain ranges impose different restrictions and opportunities than non-topographical boundaries that cut across waters or flat lands or contain points of strategic importance (Starr 2002; Starr and Thomas 2002). At the same time, the characteristics of the terrain that must be traversed in order to reach the border will often be of much greater significance than the border in itself, not the least when considering areas (grid cells) in the vast interior of countries, far from the nearest neighboring state. Accounting for this in a satisfactory manner is a highly demanding task that we defer to future research.

TABLE 1. Indicators of Inaccessibility

Dimension	*Operationalization*
Physical distance (H1)	Distance to the capital (log km), normalized [0, 1]
Safe haven (H2)	Mountains (log %), normalized [0, 1]
	Dense forest (log %), normalized [0, 1]
	Proximity to border (log inverse km), normalized [0, 1]
Sociocultural distance (H3)	Population-weighted ethno-political exclusion, normalized [0, 1]

studies have found intergroup inequalities to increase local civil war risk (Østby 2008; Cederman et al. 2011). Data on local income levels were obtained from the G-Econ dataset (Nordhaus 2006), which provides estimates of economic output at a 1 × 1 degree resolution for the year 1990. Disaggregated to PRIO-GRID, we constructed a measure of (logged) gross cell product (GCP) per capita, analogous to the country-level GDP per capita measure.

As a third control, we include a measure of the average travel time (in logged minutes) from the cell centroid to the nearest city of at least 50,000 people (Nelson 2008). These estimates are based on information on land transportation networks, such as roads, railroads, and navigable rivers; the environmental context, including elevation, slope, and forest cover; and political factors (that is, national boundaries). The original travel-time data come in a very high resolution, 0.01 × 0.01 decimal degrees; our indicator gives the mean cell value. The data are from the year 2000, although we assume that they are reasonably representative for the entire post-1989 period. Accounting for proximity to regional urban centers is probably important, as rebel attacks necessarily occur where government forces and representatives are present (radio and police stations, army barracks, etc.). Accordingly, while the inaccessibility argument assumes better opportunities for insurgent activities in remote hinterlands, tactical considerations (and possible bias in media reporting) imply that we should expect most violent activities to occur in the vicinity of population centers.

A final set of controls is included to account for spatial dependencies in the data, as well as countrywide drivers of latent conflict risk. Conflict in one unit often affects the risk of conflict in neighboring units, and failing to account for such spatial dependence violates the assumption of unit independence (Bivand, Pebesma, and Gómez-Rubio 2008; Schutte and Weidmann 2011). Hence, we include a spatial lag of conflict that measures the (logged) mean conflict rate (number of events) among adjacent cells in the same country in the sample period. Moreover, all models are specified with country fixed effects to account for unobserved differences between countries. We exclude observations that are considered highly unlikely to host conflict events; that is, coastal grid cells with only a small sliver of land territory (<100 km^2) and cells with extremely low population density, such as deserts and high mountain ranges (<1 per km^2). This returns a valid sample of 7,465 grid cells across Africa.

Results

As a preliminary test of how our physical inaccessibility indicators relate to each other and to violent conflict events, we generated a set of bivariate scatter plots (Figure 2). With a possible exception for population, we see no clear pattern whereby an increase in an exogenous variable is associated with an increase (or decrease) in the density of conflict events. Equally interesting in this context, however, we also note that there is no strong covariation pattern among the

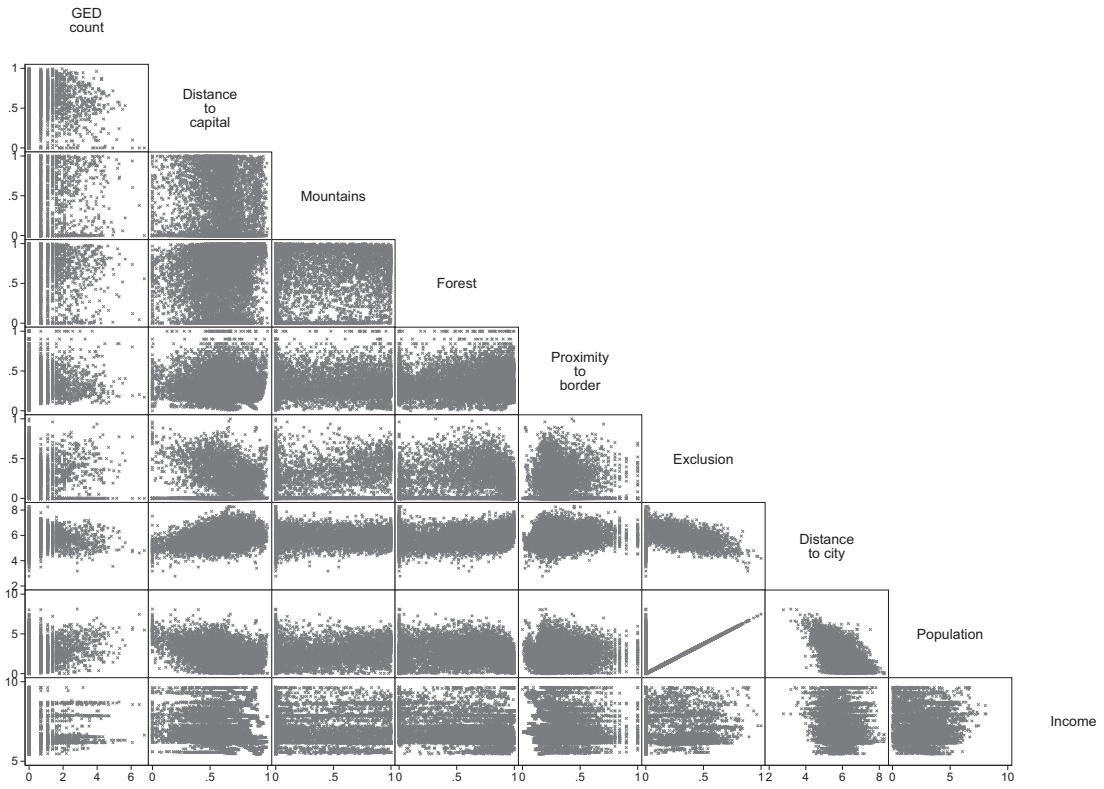

FIG 2. Bivariate Associations

inaccessibility indicators—except for the juxtaposition of population size and exclusion (the latter indicator contains population-weighted estimates).[3] This is an important observation as it implies that the various measures capture complementary dimensions of remoteness.

Next, we put our propositions to test in a more appropriate multivariate regression framework (Table 2). We first introduce the inaccessibility factors in sequential models (Models 1–5) and then estimate a model that includes all parameters simultaneously (Model 6).

As evidenced across all models, the control variables behave much as expected and in accordance with previous literature. On average, conflict events tend to cluster in relatively populated and poor areas of countries, and the models also confirm the distinctly contagious nature of armed conflicts. Units with one or more neighboring units experiencing conflict are themselves significantly more exposed to violence.[4]

Focusing on the five complementary inaccessibility measures, we find much support for our notion that conflicts tend to cluster in areas at the margins of state control: The frequency of battle events increases with the distance from the capital city; it increases with the extent of local mountainous and forested terrain, and it increases with the size of the excluded local population. Only the proximity to border indicator fails to produce a statistically reliable effect,

[3]The two most overlapping physical inaccessibility indicators are forest and distance to the capital ($r = 0.12$). Sociocultural inaccessibility (exclusion) relates most strongly to mountains ($r = 0.24$). The overall most powerful bivariate correlation in Figure 2 is between population and distance to major city ($r = -0.62$). Although this could raise concerns about multicollinearity bias, regression diagnostic tests show it to be unlikely.

[4]An inherent challenge with using time-varying information in a static analysis is accounting for reverse causality. Part of the very powerful effect of neighboring violence on the estimated frequency of conflict events may be caused by nearby events having occurred as a consequence of earlier violence in a given location. See the discussion on sensitivity tests for further details.

Table 2. Insurgency and Inaccessibility

	(1) lnGED	(2) lnGED	(3) lnGED	(4) lnGED	(5) lnGED	(6) lnGED
Capital distance	0.144 (0.046)**					0.123 (0.047)**
Mountains		0.113 (0.025)**				0.099 (0.025)**
Forest			0.090 (0.031)**			0.059 (0.031)*
Proximity to border				0.052 (0.046)		0.022 (0.047)
Exclusion					0.230 (0.047)**	0.195 (0.048)**
Distance to city	−0.109 (0.015)**	−0.107 (0.015)**	−0.110 (0.015)**	−0.106 (0.015)**	−0.107 (0.015)**	−0.118 (0.015)**
Population	0.071 (0.008)**	0.058 (0.007)**	0.061 (0.007)**	0.064 (0.007)**	0.050 (0.008)**	0.051 (0.008)**
Income	−0.056 (0.021)**	−0.067 (0.021)**	−0.063 (0.021)**	−0.067 (0.021)**	−0.069 (0.021)**	−0.053 (0.021)**
Neighbor violence	0.477 (0.011)**	0.473 (0.011)**	0.474 (0.011)**	0.477 (0.011)**	0.474 (0.011)**	0.467 (0.011)**
Constant	0.699 (0.456)	0.831 (0.454)*	0.776 (0.455)*	0.796 (0.455)*	0.890 (0.454)*	0.768 (0.455)*
Observations	7,465	7,465	7,465	7,465	7,465	7,465
R-squared	0.457	0.458	0.457	0.456	0.458	0.460

(*Notes.* OLS with country fixed effects; standard errors in parentheses; $^{**}p < .05$, $^{*}p < .1$.)

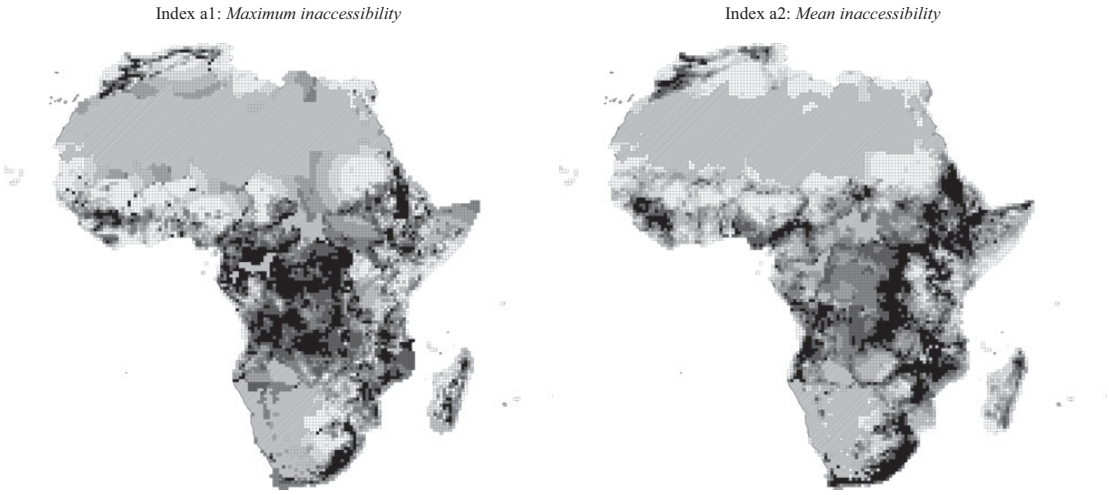

FIG 3. Spatial Distribution of Inaccessibility Across Africa

although the positive sign of the coefficient, indicating more events closer to the border, is consistent with the theoretical model outlined above. Model 6 shows that the effects of our key explanatory variables are robust to controlling for alternative dimensions of inaccessibility, thereby providing further evidence in support of all three hypotheses. The fact that the estimated effects of the individual indicators drop only moderately (15–30%) when all dimensions are considered jointly also suggests that our expectation of substitutability is at most only partially true; inaccessibility along several dimensions makes for especially conflict-prone environments.

Given the encouraging findings from Table 2, we next evaluate simple indices that combine the inaccessibility measures. In doing so, two issues must be considered: first, the assignment of weights (if any) to the individual components in the index; and second, the nature of the index, that is, whether it should represent the most extreme value (maximum or minimum value) or some aggregate measure (mean, product). These are fundamentally theoretical questions with no simple, preferred solution. We believe the relative importance of the inaccessibility dimensions is likely to vary between cases. Yet, the salience of ethnicity (Esteban and Ray 2008) and the notion of indivisibility of territory (Toft 2003) might suggest that ethnic exclusion plays a particularly central role.[5] That being said, any particular weighting scheme would be highly speculative at this stage, and we acknowledge that more rigorous theorizing is necessary in order to make further progress along these lines. As such, for an exploratory empirical evaluation, we assign equal weight to all components in this analysis. Regarding the second issue, we take the pragmatic approach and construct two alternative indicators. The first index, a1, is founded on the logic of substitution, whereby high inaccessibility on one dimension may compensate for lack of inaccessibility on other dimensions. The index thus takes the maximum cell value among the five normalized indicators of physical and sociocultural inaccessibility. The second index reflects a different logic, where the various dimensions are considered complementary and additive rather than substitutable. Accordingly, the a2 index represents the mean cell value across all inaccessibility variables. Figure 3 provides a visual comparison of the two composite indices, with darker shades denoting more remote areas.

[5]For this reason, two of the four indices do not include sociocultural inaccessibility, allowing the importance of ethno-political exclusion to be assessed through a separate indicator.

TABLE 3. Insurgency and Inaccessibility: Indices

	(7) *lnGED*	*(8)* *lnGED*
a1 Max inaccessibility	0.245 (0.054)**	
a2 Mean inaccessibility		0.466 (0.070)**
Exclusion	0.206 (0.048)**	0.201 (0.048)**
Distance to city	−0.119 (0.015)**	−0.119 (0.015)**
Population	0.052 (0.008)**	0.049 (0.008)**
Income	−0.060 (0.021)**	−0.054 (0.021)**
Neighbor violence	0.471 (0.011)**	0.467 (0.011)**
Constant	0.790 (0.455)*	0.777 (0.454)*
Observations	7,465	7,465
R-squared	0.459	0.460

(*Notes.* OLS with country fixed effects; standard errors in parentheses; $^{**}p < .05$, $^{*}p < .1$.)

Table 3 shows the results from the regression analysis, using similar models to those reported above, but replacing the individual inaccessibility components with the two alternative aggregate indices. As expected, both indicators are positive and highly significant. The notable difference in coefficient size is largely due to the different distributions of values within the bounded [0, 1] interval; a1 has a smaller coefficient to compensate for significantly larger values. At the same time, Model 8 appears marginally better fit to the data, as measured by the slightly higher R^2 parameter, although the difference is not statistically significant. Calculations of marginal effects also show that the mean-based a2 index performs better than the winner-takes-all a1, if not by a huge margin. We further see that the improvement in model fit over Models 1–5 is marginal, suggesting that there is also an element of substitutability at play.

Taken together, the empirical models offer strong evidence in support of all three hypotheses. We found that battle events tend to cluster in areas far from the national capital, in areas characterized by mountains and dense forests, and in areas hosting large, politically excluded populations. This pattern is robust in accounting for local population density, proximity to major cities, and local economic activity, as well as the conflict frequency in adjacent locations. One indicator that did not perform as expected is proximity to border. This may be due to the uniqueness of the African continent, encompassing many sizable and heterogeneous countries (for example, Central African Republic, the Democratic Republic of the Congo, Mali, and Sudan) whose weak or fragile central governments may give rise to local challengers even in the vast interior of the state. In some contrast, contemporary civil conflicts in South and South-East Asia and Europe are predominantly ethno-nationalist separatist insurgencies, located along national boundaries. At the same time, it is clear that not all borders offer equal opportunities for safe havens, military support, and illicit trade. A natural next step, in order to shed more light on border-conflict dynamics, would be to incorporate existing efforts to quantify international borders on these dimensions (Starr and Thomas 2002).

A limited number of sensitivity tests were carried out. First, we considered how our inaccessibility proxies behave in a time-varying set-up. Using a temporal analytical design might seem odd, as our focus variables are static, but it has some value in that it addresses a potential bias in the models reported above. Civil conflicts are not randomly distributed across space, and conflict events much less so. A common way to handle spatial autocorrelation is to introduce the so-called spatial lags of the dependent variable, represented by the neighbor conflict indicator in the models above. However, in the strictly cross-sectional design with

spatial lags, one risks adding right-hand-side information that could be the result, rather than a cause, of the observed outcome. Accordingly, the very powerful effect of neighboring events in the models above is partly explained by correlations that reflect a reverse causality. By running time-series models and applying a temporal lag to the neighborhood indicator, we have better control over the spatial correlation pattern, even though it comes at the expense of inflating the number of observations, with very little new information added.[6] Reassuringly, replications of Models 1–8 with annual grid data did not produce findings that deviate markedly from those reported here. Given that our variables of key interest are (largely) time invariant, and thus not susceptible to endogeneity, we find the static design more appropriate. See Schutte and Donnay (2014) for a more comprehensive treatment of causal inference with spatiotemporal event data.

Second, we considered an alternative measure of sociocultural inaccessibility by simply flagging whether the local population belongs to the national majority ethnic group or not, based on the GeoEPR data. This approach remedies the potentially very significant endogeneity problem in the models above, whereby the reported correlation between exclusion and conflict may reflect a causal effect that runs opposite to the theorized direction. History is not short of examples of minority groups that become targets of exclusionary and discriminatory policies as a consequence of past protest or in anticipation of future mobilization—neither of which processes would be picked up in our analytical design. At the same time, ignoring the political configuration of ethnicity comes with its own limitations and tacitly prescribes a deterministic socio-demographic effect that we are not willing to subscribe to. We also note that our exclusion index upholds its powerful effect even if we also include the crude minority dummy.

Moreover, we considered some explicitly data-driven aggregate indices by means of factor analysis, constructing up to three new variables from the five inaccessibility components. This test provided further details on the extent of overlap and uniqueness of individual variables but failed to reveal new constellations of conditions with increased conflict risk.

Lastly, we replaced the GED events data with point data (on the location of the initial battle event) for each civil conflict recorded in the UCDP/PRIO Armed Conflict Dataset, because local explanations for why and where conflicts break out can differ from reasons for conflict diffusion. Probably due to the rareness of onset events in our sample, this test resulted in considerably weaker findings, although the general patterns were consistent with the inaccessibility argument.

Conclusions

This article has shed light on how peripheries can shape space for action and mobilization by shifting focus from the country to the subnational local scale. More specifically, we investigated the extent to which determinants of physical and sociocultural inaccessibility—distance to the capital, proximity to the border, rough terrain, and ethno-political exclusion—increase local civil-conflict risk.

The findings demonstrate that inaccessibility is a central factor affecting local conflict risk, as remote areas are shown to be significantly more conflict prone than more accessible parts of a country. We interpret these results as supportive evidence that physical inaccessibility—notably, remote location and rough

[6]Another significant benefit of adopting a time-series cross-sectional design is that it permits capturing important shifts in the ethno-political status of the local population (sociocultural inaccessibility), which are set to represent the world *anno* 1989 in the analysis above.

terrain—proves conducive to rebellion. In addition, areas hosting sizable politically excluded ethnic populations, on average, show more conflict events than areas inhabited by ethnic groups in power, even after controlling for other dimensions of inaccessibility and local demographic and economic conditions. Lastly, the analysis suggests that physical and sociocultural inaccessibility can to some extent be considered substitutable, as the combined indices that capture all inaccessibility dimensions fail to improve significantly on the simpler models that contain only one dimension at a time.

Where do we go from here? We have already indicated a central theoretical challenge with respect to determining the relative importance of various dimensions of inaccessibility, as well as technical challenges with respect to accounting for type and salience of the terrain separating the state and the local population. In addition, future research should investigate the mediating role of telecommunications. In particular, how does telecommunication infrastructure affect the level of inaccessibility, and how does this relate to conflict? Emerging research investigates how such types of infrastructure relate to conflict (Shapiro and Weidmann forthcoming) but it remains unclear who benefits more from new technology—the state (surveillance, propaganda) or opposition movements (mobilization).

A separate, natural next step is to use models like the ones developed in this study to improve on our ability to forecast new events or important changes in the dynamics of ongoing conflicts (notably escalation and diffusion). Extant attempts to develop general conflict forecasting models (Goldstone et al. 2010; Hegre et al. 2013) are exclusively constructed around country-aggregated input data. This makes sense as a first step; but, in order to offer more precise and policy-relevant early warning, we need to account for local conditions and developments.

One issue that remains to be determined is whether the patterns described here are applicable to other corners of the world, and at other time periods. The fact that the notion of inaccessibility draws heavily on guerrilla doctrine and is inspired by contemporary ethno-national separatist insurgencies—which are much more frequent in South and South-East Asia—suggests that our findings indeed can be generalized beyond Africa. Likewise, it remains to be determined whether physical and sociocultural inaccessibility exert similar effects on the spatial distribution of political violence outside the scope of civil war (for example, communal conflict and one-sided violence). This is the topic of a future paper.

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Supporting Information

Additional Supporting Information may be found in the online version of this article:

Figure S1. Frequency of GED events, 1989–2010.
Figure S2. Distance to the capital city, 1989.
Figure S3. Mountainous terrain.
Figure S4. Forested terrain.
Figure S5. Proximity to borders, 1989.
Table S1. Descriptive statistics.

4 Experienced Poverty and Local Conflict Violence

Andreas Forø Tollefsen

Abstract

While country-level studies have shown that economic development could reduce the risk of conflict, we know much less whether a similar local relationship exist. Existing studies of the local poverty-conflict nexus have used objective proxies of poverty, and their results have been mixed. This article explores the relationship between experienced poverty and post-survey armed conflict. Analyzing geo-referenced survey data for 4,008 districts, across 35 African states, shows that impoverished districts are more likely to experience conflict violence. However, I find a strong pacifying effect of the quality of local institutions. Poverty is only related to conflict if local institutions are poor and ineffective. In addition, I find that poverty is more strongly associated with conflict if group grievances exist locally than if local groups perceive themselves as fairly treated by the government. A region fixed-effect analysis shows that poverty indeed has a positive effect on local armed conflict.

Introduction

Civil war is today more common than any other type of conflict, and the majority of these civil wars occur in low-income countries (Hegre and Sambanis, 2006; Braithwaite et al., 2014). The detrimental consequences of having one conflict increases the risk of subsequent conflict (Collier et al., 2003). Thus, economic development and poverty reduction have been highlighted to significantly reduce the risk of conflict recurrence (Blomberg et al., 2006). While existing literature suggests that low income at the country level increases the risk of conflict and its recurrence, we know much less whether a similar relationship exists at the sub-national level.

Existing country-level studies have used a range of economic indicators to test different mechanisms proposed to explain how poverty might cause conflict. However, no consensus has been made as to how inadequate development and poverty leads to conflict (Justino, 2009). As Hegre (Hegre) argues, these highly correlated indicators make it difficult to distinguish between the various theoretical mechanisms behind the development-conflict nexus. In addition, these proposed mechanisms often relates to processes at different geographical scales; the individual level, group level, and state level. Yet, few studies match their theoretical expectations with data at appropriate levels of analysis, ignoring significant within-country variations of explanatory variables and outcomes. As Cederman and Gleditsch (2009) contend, national aggregates and averages are only loosely linked to the rationale for conflict and the postulated micro-level mechanisms. Furthermore, studies employing country level measures of well-being to infer about individual or group behavior, thus risk falling for the ecological fallacy.

Studies suggest that frustration emanating from impoverishment may provoke aggressive behavior (Berkowitz, 1989). Others, such as Davies (1962) suggest that periods economic growth followed by immediate recession may increase revolutionary motivations. Gurr (1970), however, posits that as the discrepancy between expected and materialized well-being increases, inclination to rebel increases. In this paper, I first test whether a direct relationship between experienced poverty and local conflict exists, and how previous conflict influences this relationship. Second, literature suggests three conditional mechanisms explaining the indirect relationship between poverty and conflict; (1) if the individual opportunity cost of joining a rebellion outweighs foregone conventional income (Collier and Hoeffler, 1998), (2) if the state capacity is weak (Fearon and Laitin, 2003) or (3) if ethnic group grievances exists (Cederman et al., 2013). To test these indirect propositions, I aggregate survey information on local unemployment rates, perceived local institutional quality and the presence of local group grievances.

While a handful of studies have explored the local poverty-conflict nexus, their results have been mixed. Also, studies to date have relied on various approximations of sub-national poverty. In general, these proxies come from three sources, (1) low-resolution spatial data, representing the socio-economic status of subnational entities, (2) high-resolution satellite data on night-time

light emission as a measure of local energy consumption, or (3) georeferenced survey data on the individual or household assets. However, neither proxy measure individuals living conditions per se. In this paper, I explore the poverty-conflict nexus using measures of experienced poverty, in terms of (lack of) basic needs.

To empirically explore the relationship between poverty and conflict at the local level, I use survey data collected by the Afrobarometer on individuals fulfillment of basic needs. I aggregate these survey responses to sub-national districts as well as regions and combine these aggregates with information on the number of post-survey conflict events. I argue that measures arising out of micro-level data are paramount to better approximate the theoretical mechanisms in question. This has previously been highlighted by Blattman and Miguel (2010, p.24) arguing that "microlevel data is likely to yield more convincing answers to the fundamental theoretical question".

The final pooled dataset consists of 4,008 sub-national districts observed across three survey rounds, covering 35 African countries. To increase the number of units with repeated observations, I aggregate survey responses to sub-national regions. This results in a panel dataset of 111 regions observed across multiple points in time, allowing for a panel analysis using a fixed effect model.

Using a pooled negative binomial regression analysis of districts, the results suggest subnational poverty to be significantly related to local armed conflict risk. Districts with a high share of impoverished individuals, both absolutely and about the country average have a higher risk of experiencing conflict than affluent areas, and the previous conflict fortifies this relationship, providing support for a local conflict trap. A fixed-effect analysis of 111 regions observed over time provides significant support for a direct relationship between poverty and conflict. Furthermore, results from the interaction models suggest that poverty increases the risk of conflict, but only whenever local institutions are weak, not when institutions are perceived as strong and robust. The results also suggest that where impoverished areas exist alongside perceptions of group injustice, has a greater risk of conflict than impoverished areas without an aggrieved population. Contrary to expectations, local economic opportunities do not seem to condition the poverty-conflict relationship. In sum, the results suggest that poverty is a primary cause of conflict, but good institutions and inclusion can mediate on this effect.

The remainder of this article first discuss the effects of conflict on local poverty, and how these impacts may increase the risk of subsequent local conflict. Next, I elaborate on the theoretical framework linking poverty to local conflict. Last, I present the research design and modeling strategy, followed by a presentation of the empirical results. Finally, I conclude and propose possible opportunities for future research.

How conflict causes poverty

Armed conflict has detrimental impact on society in general, and on local livelihoods more specifically. Conflict increases poverty levels through "reversed development", as well as local destitution (Collier, 2007). Also, the population, armed as well as civilian, are killed as a result of direct warfare, collateral damage, and indiscriminate violence. In addition to increased lethality, short- and long-term consequences of conflict have an adverse impact on development and health.

Conflict affects the economic growth at the country level, as well as affecting the local economy in conflict-affected areas. Regarding economic growth, civil war is believed to reduce GDP growth with 2.2 % per active conflict year (Gates et al., 2012). While conflict reduces growth, it has also been identified as one of the main causes as to why poverty remains adhesive (Collier et al., 2003). Post-conflict, studies show that the macro-level economic effects of conflict rapidly converges back to steady-state growth (Bellows and Miguel, 2006). Similarly, Justino and Verwimp (2008) finds evidence for this form of convergence, where high investment in affected areas can overcome the temporary destruction by conflict. However, these purely economic perspectives of the consequences of conflict ignore the non-economic consequences and fails to include the unequal impact of war on different areas of the country and among distinct population groups.

Collier (2007) discusses a range of local consequences of armed conflict, among these; damaged infrastructure, impaired institutions, and production, destroyed assets, as well as break up of communities and social networks. In addition to the short-term consequences, the long-term effects of conflict on people and society are considerable (Ghobarah et al., 2003). Civil war increases exposure to diseases and deteriorates medical care and public health services. Studies show that the effect of conflict also has similar negative effects on neighboring countries (Ghobarah et al., 2004).

Gates et al. (2012) explores the effect of armed conflict on the UN's World Development Goals (WDG) and finds that the cost of war is mainly paid by civilians. While the direct cost of the war on humanitarian suffering is awful, the indirect effects are even worse. Conflict have a negative effect on undernourishment, poverty, life expectancy and GDP per capita. Also, conflict increases infant mortality rates and cut-off people's access to fundamental necessities such as the supply of clean water.

Children and women also experience impacts of conflict violence through health and education constraints. Akresh et al. (2011) finds that children in Rwanda exposed to conflict, have a lower height for age of children under five, but impoverished households are impacted more severely than richer households. Education achievement rates are also impacted negatively. Akresh and De Walque (2008) for instance, finds that students exposed to the Rwandan genocide had an educational achievement loss of almost a one-half year of completed schooling. Sexual violence and abuse also affect local populations, both indiscriminately and intentionally (Bastick et al.,

2007). These abuses cause both physical injuries and psychological trauma (Swiss et al., 1998).

Conflict force local inhabitants to migrate or flee (Schmeidl, 1997). This affects the economic status of households by impacting employment and credit opportunities, as well as social relations. Also, lower economic growth at the country level as a consequence of conflict may impact household-welfare through changes in welfare systems (Justino, 2009).

The shattering consequences of conflict on human and physical capital could, in turn, become adhesive, trapping populations in poverty (Collier, 2007). This poverty trap may in turn form motivations and opportunities for future conflict.

A local conflict trap

The results of Braithwaite et al. (2014) and Blomberg et al. (2006) suggest that the poverty-conflict nexus may be reciprocal. This owes to the "reversed development" following a conflict, increasing the risk of subsequent conflict. However, local studies of conflict have not explicitly investigated whether there is a similar local conflict trap, primarily attributed to the lack of appropriate data. This is unfortunate, as both economic conditions (Elbers et al., 2003) and the extent of conflict (Rustad et al., 2011) varies considerably within-countries. Besides, conflict areas are in general atypical from the country at large (Buhaug and Lujala, 2005).

While the consequences of conflict certainly impact the country at large, the direct consequences remain by large local. Impacts of conflict culminate within the scope of conflict, where inhabitants experience the larger share of repercussions to human and physical capital (Barron et al., 2009; Human Rights Watch, 2007). Local institutions will deteriorate, trade and travel abate, and skilled labor will relocate to safer grounds.

Seddon and Hussein (2002) for instance, finds vast impacts on local livelihoods following the Maoist insurgency in Nepal. Notable effects were forced migration from rural areas, reduction in travel and transport of goods, disruption of economic activities, negative impact on food security, destruction of local infrastructure as well as increasing fear and insecurity. If conflict begets conflict, local consequences of one conflict is expected to fortify the same areas risk of repeat conflict. However, the detrimental effect of previous conflict abates as time passes (Collier and Hoeffler, 2004). Thus, the first proposition tests whether local conflict begets local conflict[1].

Hypothesis 1 (H1): *Previous local conflict increases the risk of renewed conflict in the same sub-national area.*

[1]Throughout this article, I define conflict in terms of small-scale violence measured as events, and defined by UCDP as "A incident where armed force was by an organised actor against another organized actor, or against civilians, resulting in at least 1 direct death at a specific location and a specific date". For an event to be included, the event must be part of a larger conflict causing 25 or more annual battle-related deaths, as defined by the PRIO/Uppsala Armed Conflict Dataset (ACD) (Gleditsch et al., 2002).

How local poverty cause conflict

A recent wave of studies has explored the local relationship between poverty and conflict. The majority of these have relied on objective measures of local poverty. Buhaug et al. (2011) for instance, used spatial data from the PRIO-GRID (Tollefsen et al., 2012) on economic activity, and finds conflict onsets to be more likely in poorer areas. Other studies have employed information on assets available through the Demographic Health Surveys (DHS). Østby et al. (2009) for instance, found regions with strong relative deprivation to have a higher risk of civil conflict onset, while Hegre et al. (2009) also used assets, but found conflicts to locate in richer parts of Liberia. Local road density has also been highlighted as a proxy of local development (Dixon, 2009). Both Buhaug and Rød (2006) and Raleigh and Hegre (2009) found conflict to be more likely in areas with roads. More recently, Shortland et al. (2013) employed night-time luminosity (energy consumption) data to proxy income, and found conflict to affect poorer households more than affluent households. Cederman et al. (2011) uses geocoded ethnic group data together with spatial wealth estimates, and finds conflict to be more likely in highly unequal societies. Both rich and poor groups fight more often than groups whose wealth approximates the national average.

The case study literature is ripe with examples of poverty motivated conflicts. de Soysa and Gleditsch (1999) for instance, argues that the conflicts in Liberia and Sierra Leone resulted from the inability of the weak states to provide basic needs and employment, leading to aggrieved unemployed populations, ready to battle over resources and any state power left intact. Deininger (2003) shows that lack of economic opportunities at the community level in Uganda increased the propensity of civil strife. Others, such as Ikejiaku (2012) argue that participants in the Niger Delta attacks were often poor and unemployed youths. Others, such as Irobi (2005), argues that the immediate causes of conflict in South Africa were poverty and unemployment, where the homelands of certain groups were deprived of access to basic needs.

A direct effect of poverty on conflict

Previous studies have proposed alternative theories to explain the relationship between poverty and conflict. Marx and Engels (1848) suggested that the poor working class have nothing to loose but their chains, to improve their position. More recently, the frustration-aggression theory was proposed to explain how poverty causes conflict. Studies such as Van De Goor et al. (1996), argues that individuals turn aggressive when basic material needs are not met. Frustration is thus seen as a necessary premise for violent behavior to take place (see Berkowitz (1989) for an overview of the frustration-aggression rationale).

Others, such as Davies (1962), argued that revolutions were most likely to occur following a period of economic and social development followed by a sudden recession. Individuals wor-

rying their work and improvements might be lost, could generate revolutionary behavior. Following the frustration-aggression theory and Davies theory of revolution, Gurr (1970) proposed his theory of relative deprivation. Here, poverty may fuel conflict if individuals actual living conditions diverge from people's expected and desired level of well-being. As the gap between expected and desired level of welfare widens, individuals may turn violent. However, when individuals reach a certain level of extreme poverty, the likelihood of violence will shrivel. As Hobsbawm (1959, p. 79) states: "When people are really hungry they are too busy seeking food to do much else".

Relative deprivation is widespread in Africa. As Ikejiaku (2012) argues in an African context, the majority of countries have weak governance structures. These weaknesses make institutions incapable of providing groups with equal access to social and economic resources. This leads to the persistence and deepening of poverty among certain groups about others. Relative deprivation then may increase motivation for collective mobilization and ultimately rebellion.

If poverty is directly linked to conflict, we should see conflict locate in poorer areas of a country. According to literature, impoverished populations should exhibit increased frustration, aggression, and general discontent, presumably makes conflict more likely. Thus, I first explore whether perceived poverty among local inhabitants is directly associated with an increasing risk of local conflict violence.

Hypothesis 2 (H2): *Sub-national areas with a high share of impoverished individuals are associated with an increased risk of armed conflict violence.*

An indirect effect of poverty on conflict

While poverty in itself may induce conflict, most scholars suggest that the relationship is indirect, where poverty cause conflict only if certain conditions are present. Thus, I elaborate on three oft-cited conditions and discuss how these three conditions moderate the poverty-conflict nexus at the local level.

The first mechanism, the individual opportunity cost argument, contends that individuals without secure income and limited opportunities for regular income is more likely to enlist and partake in rebel activities, or are exposed as easy targets for effective recruitment campaigns of rebel groups (Collier and Hoeffler, 1998). Collier and Hoeffler (2004) argues that conflict becomes more likely when foregone income is low. Thus, when opportunities for regular economic income is limited, conflict becomes more likely as joining a rebel group may provide viable economic opportunities for individuals.

Similarly, Jakobsen et al. (2013) assesses several proxy measures of poverty in a factor analysis. They find that per-capita income belongs more to a distinct wealth/poverty dimension than to dimensions of grievance and state capacity. Applying a country level analysis, their results show

that lower per capita income is related to an increased risk of conflict, providing support for the individual opportunity cost argument.

Berman et al. (2011) employs survey data from Afghanistan, Iraq and the Philippines to test whether local unemployment increases levels of political violence. However, their results show no support for the opportunity-cost argument. Unemployment does not increase the risk of political violence. However, employed individuals may still partake in rebellion if the anticipated earning outweighs income from regular work (Justino et al., 2013).

Thus, according to the individual opportunity cost argument, joining a militia or partaking in rebellions become economically viable if income from such activity outbids foregone conventional income. Areas with a high number of unemployed individuals might provide an increased number of potential recruits, willing to join rebel factions. Also, grievances may arise if unemployment opportunities are limited.

Hypothesis 3 (H3): *Sub-national areas with a high share of impoverished individuals have an increased the risk of armed conflict, whenever local unemployment opportunities are lacking.*

Second, the poverty-conflict nexus may also be dependent on the capability of local institutions. High capacity local institutions are better equipped to control its territory and deterring possible challengers, maintaining basic needs among the local populations, providing adequate infrastructure for trade and general economic prosperity. Low GDP per capita at the country level has been highlighted as a proxy for low state capacity. If state capacity is weak, opportunities for rebel groups to secure grounds increases. On the contrary, when state capacity is high, institutions are more robust to solve dissension before escalating to violence, or have the capability to strike down revolt and challengers before such groups gain sufficient foothold. These state weaknesses increase the political and military opportunity for conflict (Fearon and Laitin, 2003). As Benson and Kugler (1998, p. 206) argues "Politically efficient governments are much more likely to avert internal challenges".

Holtermann (2012) finds that the capacity and reach of states plays a more important role than poverty in explaining the countries that experience conflict. Poorer countries are often incapable of controlling the rural peripheries of their territory (Buhaug, 2010), making rebellion in these distant areas more likely (Tollefsen and Buhaug, 2015). Rebels can take advantage of these power vacuums, sidelining the government, and establish local political and military control. Once local control within these local power vacuums is established, rebels can choose to substitute public services, such as justice, protection and develop social programs where the government previously was unable to provide for it's own citizens (Kalyvas, 1999). Thus, rebels might build up trust among local populations, necessary for the essential recruitment of rebels and for establishing a taxable income necessary for funding future actions.

Wig and Tollefsen (2016) shows that sub-national areas controlled by high-quality institutions,

being uncorrupt, law-governed, capable, trusted by the public and efficient in performance are less likely to experience conflict violence. First, strong local institutions with a well-functioning rule of law may be more capable of solving local grievances and dissensions before they turn violent. Second, strong local institutions and an uncorrupt police force may by use of force, be able to deter violent uprisings in the early phase, deterring a situation where any violent challengers grow to critical levels. Third, robust local governments develop and maintains infrastructure, important for the control of its territory and to support trade. Last, well-functioning local institutions may be better able to provide basic needs for its citizens, and to distribute these equally across groups

Uncorrupt institutions of high-quality will be better equipped to provide basic needs for its citizens. (Ikejiaku, 2012) discusses some ways corruption may inhibit an institutions ability to provide for the basic needs of citizens; acquisition of public funds, political patronage, money laundering, bribery, invoicing and overestimation of projects and contracts, and misuse of state property.

Hence, high quality and uncorrupt institutions will be more capable to distribute resources equally between groups and to plan and prepare in case of food shortage or other emergencies. They will also be more transparent and accountable in addressing important needs of citizens. Also, areas with high-quality institutions will be better equipped to build and maintain infrastructures such as roads, important for local trade and economic prosperity. Thus, high-quality institutions should matter, not only the proxying capability to deter any rebels, but to better provide for its citizens. Hence, I expect that impoverished areas with high-quality and capable institutions experience less conflict than impoverished areas with low-quality institutions.

Hypothesis 4 (H4): *Sub-national areas with a high share of impoverished individuals have an increased risk of armed conflict, whenever local state capacity is low.*

Third, poverty accompanied with grievances at the group level may increase the motivation for conflict violence. Horowitz (1985), for instance, argued that discriminated groups were more likely to rebel than other groups. Gurr (1993) argued similarly, that countries inhabited by disadvantaged groups were more likely to experience conflict.

According to Gurr (1970), relative deprivation arises when the difference between individuals expected and actual return increases. Dissatisfaction with status quo could inspire citizens to attempt overthrowing the government or see the potential in secession if this might improve upon their individual or group situation.

As Gurr (1970, p. 12-13) states it "The primary causal sequence in political violence is first the development of discontent, second the politicization of that discontent, and finally its actualization in violent action against political objects and actors". According to the relative deprivation argument, poverty is expected to increase grievances and ultimately bring about violent behavior.

Recently, literature has shown that horizontal inequality that between groups is an important determinant for conflict (Stewart, 2002). Horizontal inequality may spur group grievances, and ultimately conflict (Østby, 2008). While poverty may cause discontent in itself, poverty accompanied by perceptions of group injustice and their unfair treatment by the incumbents may be more grievous.

Some recent studies have explored how local group grievances are related to conflict. Using survey data from the DHS, Østby (2008) shows that horizontal social inequality, that between groups are positively related to conflict outbreak. Deininger (2003) found asset inequality between regions to increase the risk of civil strife. Similarly, Østby et al. (2009) found regions with strong relative deprivation to have a higher risk of civil conflict onset. Cederman et al. (2013) finds that the political exclusion of groups increases the risk of conflict. Likewise, Tollefsen and Buhaug (2015) shows that areas of a country inhabited by politically excluded groups have a higher risk of conflict events. Using novel triangulation of data, Cederman et al. (2015) shows that poor groups relative to the country average see more conflict.

An impoverished group experiencing no injustice may have fewer motivations to rebel, relative to impoverished groups that perceive themselves as unfairly treated by the government. Hopelessness may be more profound if governments fail to address such impoverishment equally across groups. I here focus directly on how injustice moderates the poverty-conflict nexus. Thus, I argue that impoverished groups perceiving themselves as unfairly treated by the government have an increased motivation for engaging in violent activity.

Hypothesis 5 (H5): *Sub-national areas with a high share of impoverished individuals have an increased risk of armed conflict, whenever local group grievances are widespread.*

Data and research design

To test the five propositions empirically, I aggregate survey data from round 3, 4 and 5 of the Afrobarometer survey to sub-national units across Africa. To identify the respondents sub-national unit, I spatially intersect the village location of respondents with polygons representing sub-national districts (level 2) and regions (level 1)[2]. I aggregate survey responses to districts and regions, the first- and second-level administrative units below the nation-state. The larger regions provide an alternative unit of analysis, where more units are observed over time than it is possible using districts. Having multiple observations over time of the same unit makes it possible to employ panel analysis, accounting for stable unit characteristics.

The location of respondents was georeferenced by Kotsadam, Olsen, Knutsen, and Wig (Kotsadam et al.). Using a partial string matching method, they connected the town and village vari-

[2]Is use the Global Administrative Areas Database (GADM) (version 2.8)

able to coordinate pairs representing each location. The final result is a coordinate pair for each respondent. Respondents whose town or village could not be identified and georeferenced was excluded from the analysis [3].

Using the spatial intersect between respondents coordinates and sub-national unit polygons, I average the survey responses. This results in survey data for 4,008 districts in 35 African countries, with 1094 districts in round 3 (surveyed in 2005-2006), 1137 in round 4 (surveyed in 2008) and 1777 in round 5 (surveyed in 2012)[4]. For regions, I average similarly, but exclude regions observed only once, as well as regions without variation in the outcome variable, as lack of within variation do not contribute in the fixed-effects framework. This results in 311 observations for 111 unique regions [5].

Measuring sub-national poverty

To improve spatial disaggregation of poverty and improved approximations between theory and data, I employ data from the Afrobarometer survey. The Afrobarometer measures poverty as individuals shortage of basic needs within 12 months before the survey. The sample is a nationally representative cross-section of all citizens of voting age in each country, drawn using a clustered, stratified, multi-stage probability sample. The first step is to select the sub-national units by regional stratification, reducing the likelihood that people living in particular regions, or belong to ethnic or language groups are left out. Next, primary sampling units (PSU) are selected within each region. The informants are then selected randomly starting from an initial sampling start point in the PSU (see Afrobarometer (2016) for an extensive description).

The Afrobarometer data provides an experiential measure of poverty, the lived poverty index (LPI), based on the views and experiences of ordinary citizens. The LPI is the mean response of five questions from the Afrobarometer, asking respondents, *over the past year, how often, if ever, have you or your family gone without enough:* ***Q8A*** *food to eat;* ***Q8B*** *clean water for home use;* ***Q8C*** *medicines or medical treatment;* ***Q8D*** *enough fuel to cook your food; and* ***Q8E*** *a cash income?*. The response options offered are *never for those that experienced no shortages, just once or twice, several times, many times* or *always*. To compute the LPI, I recode these alternatives as 0 through 4 and calculate the mean response. Thus, an individual that *never* experiences lack of basic needs would score 0 while an individual *always* experiencing scarcity would score 4.

To assess how the LPI measure behaves about previously used measures of local economic well-being, I aggregate data on infant mortality rate, gross cell product and night-time luminosity to the *district units*. Table 1 shows the district level rank correlation between the LPI variable and the previously used indicators. While the direction of the relationship is as expected, the results

[3] The georeferencing matched 84 % of the respondent's in round 3, 95 % in round 4 and 91 % in round 5

[4] See the appendix for an overview of the number of districts per country for each round.

[5] 26 regions were observed in two rounds and 285 regions observed in three rounds

indicate a relatively weak correlation between LPI and other proxies of sub-national poverty. Districts with high levels of experienced poverty have greater infant mortality rate, lower economic activity, and less night-time luminosity. The correlations suggest that experienced poverty regarding basic needs capture a different dimension of economic well-being than these purely objective measures. The weak correlation also reveals the inability of these measures to precisely measure experienced poverty.

Table 1: Rank correlations at the district level between the Lived Poverty Index and previously used sub-national economic measures.

	Lived Povery Index	IMR	GCP	Nightlights
Lived Povery Index	1.00	0.34	-0.30	-0.28
IMR	0.34	1.00	-0.41	-0.25
GCP	-0.30	-0.41	1.00	0.50
Nightlights	-0.28	-0.25	0.50	1.00

The left panel in figure 1 exposes the two single areas exhibiting night-time luminosity (shown as gray fields); Arua to the west and Gulu to the east. The figure highlights the complete absence of night-time light outside these two cities and for rural areas in-between. While the night-time luminosity data picks up emissions from major urban clusters, it fails to identify variations in poverty outside and within urban areas. This is unfortunate, as conflict is often clustered in remote and more inaccessible areas (Tollefsen and Buhaug, 2015). The right panel shows the mean levels of experienced poverty at the district level, providing greater variation across the countryside.

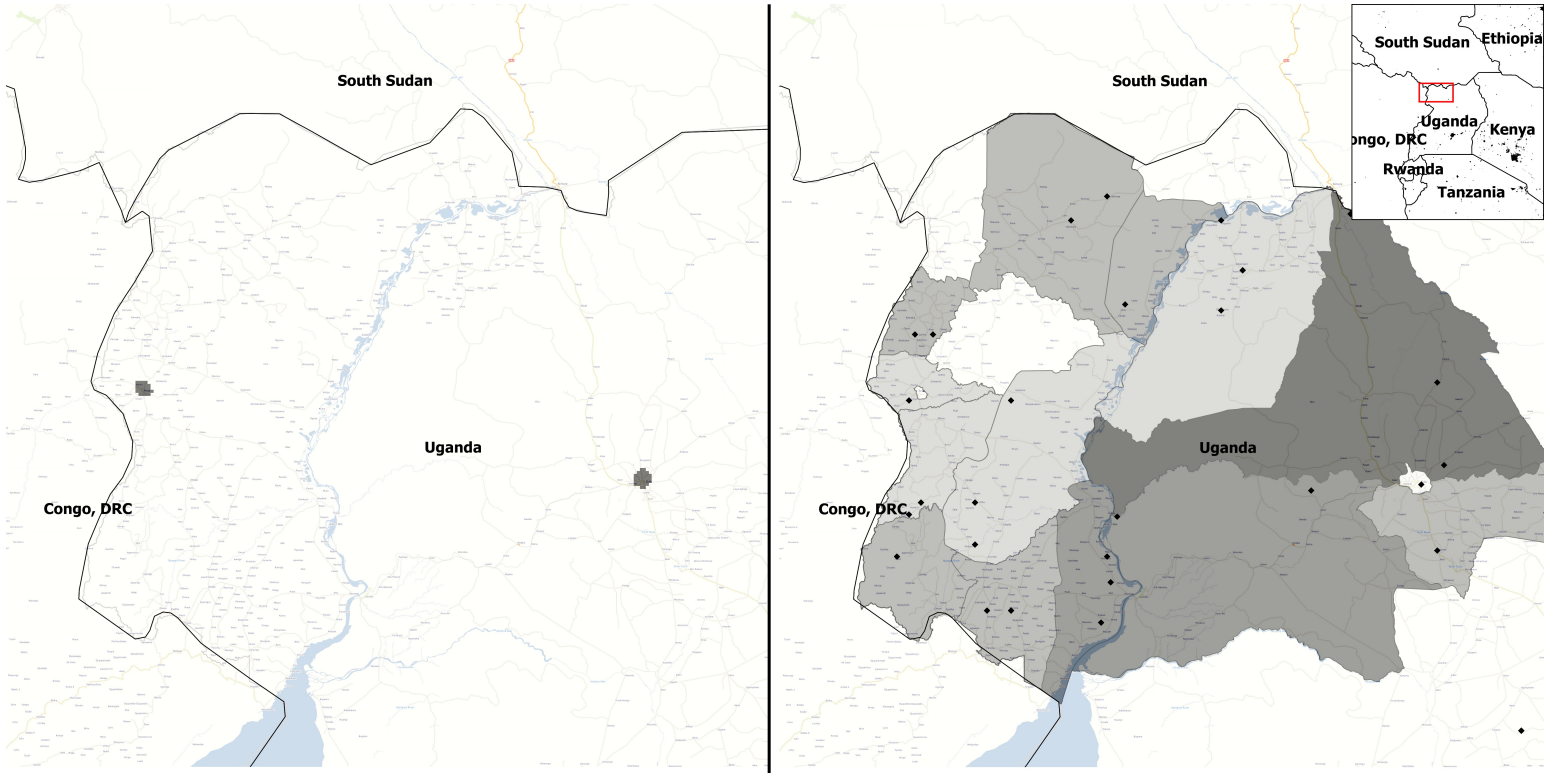


Figure 1: Left panel shows night-time luminosity in north-western Uganda. Except for Gulu to the east and Arua to the west, there are no lit areas. The right panel shows experienced poverty at the district level for the same area. Darker areas have higher levels of experienced poverty. Points represent survey locations.

Moderator variables

While the poverty-conflict association remains relatively uncontested, there is much greater disagreement about the actual mechanisms behind the relationship. To elucidate the understanding of the mechanisms behind the poverty-conflict association, Justino (2009) proposed the use of interaction models. Thus, to assess the conditional hypotheses, I interact sub-national poverty with local unemployment rates, local state capacity and the presence of local grievances. Interaction terms in regression models are used when it is believed that the relationship between x and y is moderated by a third variable z (see e.g. Jaccard and Turrisi (2003)).

The literature suggests that conflict becomes more likely when poor individuals have few conventional economic opportunities, presenting rebel groups as viable economic alternatives. To capture the local levels of economic opportunities, I include the share of respondents in each district stating they are currently unemployed. This variable is provided by Afrobarometer, and asks respondents "Do you have a job that pays a cash income?". Respondents that answers no, are coded as unemployed, while respondents stating to have either full or part time job is coded as employed. The median unemployment rate for districts is 67.5 %, while 4.6 % of the districts have unemployment rates below 20 %. 28 % of the districts above 80 % unemployment rate. One potential limitation with this variable is that it do not capture informal labor such as subsistence farming, not generating a cash income.

To proxy local state capacity, I create an index of variables capturing perceived local institutional quality. Following a factor analysis, I construct an index using variables indicating; how well the local governments is in creating jobs, extent of corruption among local government councilors, extent of corruption among local police, extent of corruption among local tax officials, how well the local government is maintaining roads, trust their local government council, trust courts, and whether respondents approve or disapprove the performance of their local government councilor. All questions offer four response alternatives and were recoded to go from negative to positive, where 1 is low quality, and 4 is high quality. Figure 2 suggests these variables load similarly in a factor analysis, and thus tap the same underlying dimension. The resulting measure of local institutional quality ranges from 1.094 to 3.75, with a median quality of 2.4.

To account for the presence of group grievances, I include the share of respondents in a district that perceive their *group* as always being unfairly treated by the government. This question is asked in the Afrobarometer survey, and the respondents can answer whether their group has been treated unfairly: Never, Sometimes, Often or Always. I assign the values 0 through 3 to each response alternative, respectively. Next, I construct a variable measuring the share of respondents perceiving their group as *Always* being unfairly treated. The resulting variable shows that 12 % of the districts have more than half of the population always feeling unfairly treated.

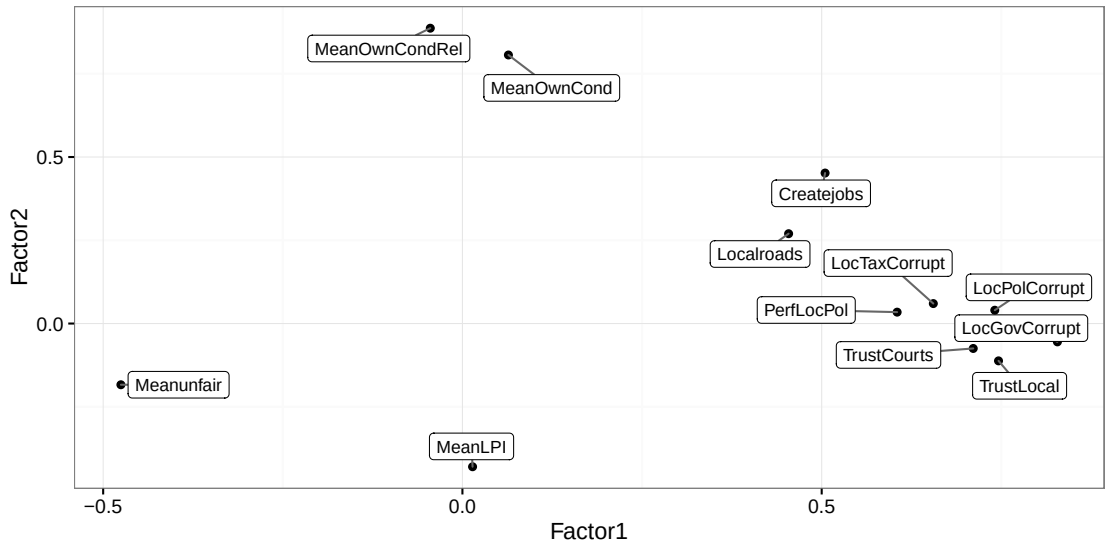


Figure 2: Exploratory factor analysis using variables measured at the district level.

Dependent variable

To measure the spatial location of armed conflict, I use a Geographic Information System (GIS) to count the number of conflict events in each sub-national unit within three years *after* completion of the survey [6]. The conflict events come from the Uppsala Geo-referenced Event Dataset (UCDP GED version 2.0) (Sundberg and Melander, 2013), that provides information on the location of conflict events, defined as "A incident where armed force was by an organised actor against another organized actor, or against civilians, resulting in at least 1 direct death at a specific location and a specific date". Also, for an event to be included in the UCDP GED, the event must belong to a conflict with more than 25 annual battle-related deaths as defined by the PRIO/Uppsala Armed Conflict Dataset (ACD) (Gleditsch et al., 2002). The events are coded according to spatial and temporal information derived from news sources and include state-based, non-state and one-sided violence. For this analysis, I all three types of events into the count variable.

For some countries, round 5 of Afrobarometer was completed in 2012 (as compared to 2011), narrowing the number of countable events, as the UCDP GED coverage ended in 2014. Figure 3 shows the conflict events across districts in Uganda, superimposed on district aggregated data from the Afrobarometer (round 3) on experienced poverty.

To account for the effect of the previous conflict I include a **half-life** parameter where the effect of the previous conflict decreases exponentially with years since the last conflict. This captures the diminishing effect of conflict history, where conflict in the previous year obtains the maximum value of 1, then halved after two years, and infinitesimal towards 1989 (start of UCDP GED

[6] I use PostGIS for all spatial overlays

coding). The half-life parameter is constructed as $2^{-(years\ since\ conflict/2)}$. For districts without any prior conflict, the half-life variable is assigned a zero.

To account for the diffusion of conflict from contiguous districts *j* of unit *i* in year *t-1*, I include a binary **spatial lag**. The spatial lag takes a value of 0 if no neighbors experienced conflict, or 1 if conflict occurred in any contiguous unit in the previous year (see e.g Ward and Gleditsch (2008)).

Other control variables

To account for factors that may affect variations in poverty and conflict, I include a number of covariates. Countries with high **population** size is associated with an increased risk of armed conflict (Brückner, 2010). Large recruitment pools, affluent urban areas and the strategic importance of locations with a high population is believed to explain the association. Moreover, sub-national conflict is more likely in areas with high population outside the capital city (Raleigh and Hegre, 2009). To account for the population size of the district, I include the log of population size in the district, calculated from the Gridded Population of the World dataset (CIESIN, 2005)[7].

Capital cities is often considered the culmination of state capacity and state institutions. The local loss of strength gradient suggests that conflict is likely to be localized near the capital, only if the rebels are strong, about the government. Hence, weaker rebel groups will only gain footing in the periphery (Buhaug, 2010). Hence, the location of conflict violence is a product of state government capacity, measured as the distance from the **capital city**. I include the (logged) distance from the centroid of each district to the capital city.

Literature suggests that **border areas** at the periphery of states provide safe havens for rebel groups (Salehyan, 2007), increasing the likelihood of conflict in these areas (Buhaug and Rød, 2006). Meanwhile, Tollefsen and Buhaug (2015) finds a positive but non-significant support for conflict being located in border areas. To account for the peripheral location of districts, I include the logged distance from each unit centroid to the nearest international border in the Cshapes dataset (Weidmann and Gleditsch, 2010).

Larger districts will have a higher likelihood of experiencing more conflict. Hence, we control for the **area** of the area in square kilometers. The area is calculated from the GADM district polygons.

Across models, I include dummy variables for each survey **round** to account for both round and time specific heterogeneity.

Modeling strategy

The basic count model for non-negative integer responses is the Poisson, regression model. However, the Poisson model assumes equidispersion, where the mean equals the variance. Implicitly,

[7] I use PostGIS to summarize the raster pixel values inside each polygon

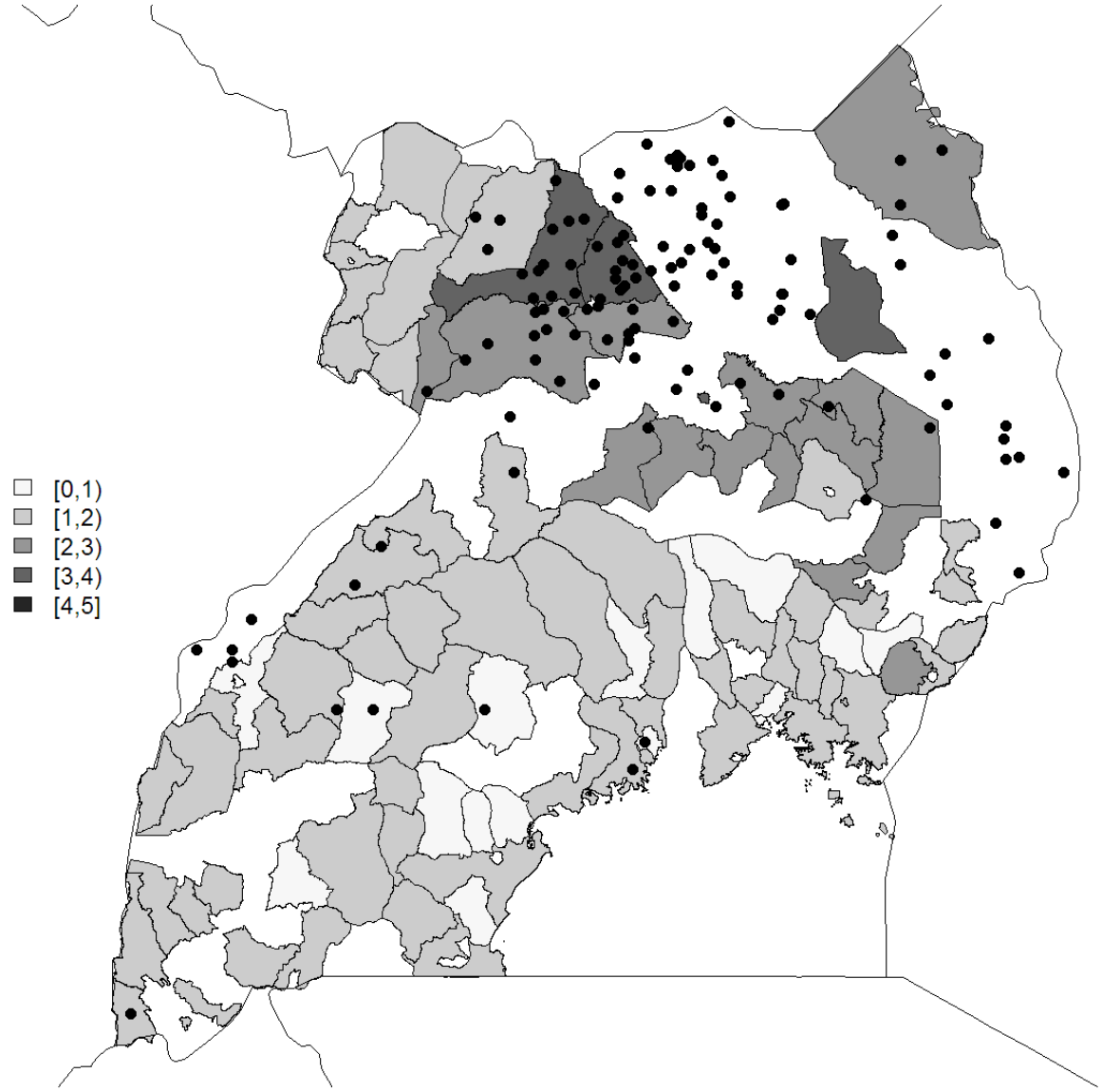


Figure 3: Lived Poverty Index and post-survey conflict in Uganda between 2005 and 2008 (Round 3 data). Higher saturation indicate higher poverty levels. Black points represent conflict events.

this extends to the assumption that the various counts are independent of each other. If overdispersion is not accounted for, the standard errors will be biased, increasing the risk of type 1 errors (Hilbe, 2011).

An alternative modeling strategy is the traditional parameterization of the negative binomial (NB henceforth) model (typically denoted NB2), derived from a Poisson-gamma mixture distribution. This includes an extra parameter α to accommodate for overdispersion.

An overdispersion test[8] of the dependent variable confirms existence of overdispersion. It is also highly likely that one conflict event increases the likelihood of additional events, violating the count independence assumption. Thus, I employ the NB model for the pooled cross-sectional analyses.

While the NB model adequately accounts for overdispersion in cross-sectional data, the independence assumption is typically violated in longitudinal studies, when units are repeatedly observed over time. Conflict events measured in unit *i* at time *t* are likely to be correlated with observations of *i* at *t-1*. The NB model can be extended to accommodate for separate fixed effects (FE henceforth) for each distinct panel in the data. Using FE models on data of units observed over time makes it possible to partial out stable characteristics of these units (Allison, 2009). This provides a better estimate of the effect of *Xit* on *Yit*, reducing potential omitted variable bias. Fixed effects negative binomial (FENB henceforth) models may be estimated, either conditionally as proposed by Hausman et al. (1984) (HHG henceforth) or unconditionally by including a dummy variable for each panel (Hilbe, 2011).

When the number of panels is large, the unconditional FENB model is susceptible to biased estimates due to the "incidental parameters problem" (IP henceforth). Also, it can be computationally inefficient if the number of dummy variables to be estimated is large. Hence, the HHG model was proposed to ameliorate the IP problem, and to increase computational efficiency. However, as both Allison and Waterman (2002) and Hilbe (2011) highlights, the conditional FENB model is not a true fixed-effects model, as it fails to account for time-invariant predictors, by allowing for individual-specific variation in the dispersion parameter, rather than in the conditional mean. Guimaraes (2008) and Greene (2007) reached similar conclusions. By using simulations, Allison and Waterman (2002) do not find clear evidence for the IP problem in the unconditional FENB model, but as Hilbe (2011) argues, the unconditional FENB model should be reported with bootstrapped standard errors. Thus, the panel data are analyzed using unconditional FENB model.

Across all pooled-cross sectional models, I report robust standard errors[9]. For the unconditional FENB model, I report bootstrapped standard errors to explore the variability in estimates[10].

[8] Using *dispersiontest* of package *AER* in R.

[9] Robust standard errors of type *HC3* are computed using the *vcovHC* function of the R package *sandwich*

[10] Bootstrapped standard errors are computed using the *boot* function of the R package *boot*, based on 1000 bootstrap replications.

As models have been run using the *glm.nb* function in R, overdispersion is reported as θ. This is the inverse of α, typically used to denote overdispersion.

Results

Table 2 presents the results of the negative binomial regression analyses of the pooled cross-sectional data. Model 1 tests hypotheses 1, whether the previous conflict has an effect on subsequent conflict. The result shows that past conflict is significantly and positively associated with subsequent conflict. The shorter time since a district experienced conflict, the more likely it is to host subsequent conflict. This may suggest that local repercussions due to the previous conflict, makes the same area susceptible to future instigations. The results of model 1 are thus in line with hypothesis 1, but the results suggest that time reduces the impact of previous conflict, similar to what Collier and Hoeffler (2004) identified as a healing effect of past conflict at the country level.

Next, model 2 introduces the experienced poverty variable. The result suggests a positive association between perceived poverty and post-survey conflict, even when controlling for the spatial and temporal proximity of conflict. Conflicts tend to locate in areas of countries where absolute levels of experienced poverty are high. However, this measure fails to take into account the prevailing conditions at the country level. Thus, I proceed with including a variable of perceived poverty, about the country average. The result in model 3 indicates that as the gap between a district's poverty levels and the national average widens, conflict becomes more likely. Thus, armed conflict is more likely to be located in the poorer areas of countries. In model 4, I include a variable measuring the share of citizens that go without basic needs[11]. As the share of a district's citizens living without basic needs increases, conflict becomes more likely. As a comparison, model 5 indicate that as the number of people that never experiences shortages increases, conflict risk decreases. The results provide ample support for a direct relationship between poverty and conflict.

[11] In the categories of "once or twice" or more

Table 2: Experienced Poverty and Conflict Events, Negative Binomial Count Model.

	Dependent variable:				
	Number of Conflict Events				
	(1)	(2)	(3)	(4)	(5)
Experienced Poverty		1.127***			
		(0.345)			
Relative Experienced Poverty			1.024**		
			(0.487)		
High Experienced Poverty				2.055***	
				(0.678)	
Low Experienced Poverty					−2.047***
					(0.670)
Conflict History	4.767***	5.649***	5.606***	5.785***	5.787***
	(1.022)	(0.657)	(0.606)	(0.706)	(0.705)
Conflict Spatial Lag (t-1)	1.586***	0.807	0.757	0.893	0.884
	(0.316)	(1.124)	(1.042)	(1.264)	(1.262)
Population (logged)	0.361***	0.433	0.521*	0.448	0.451
	(0.115)	(0.318)	(0.293)	(0.333)	(0.335)
Distance to Capital (logged)	0.504	0.090	0.031	0.050	0.049
	(0.444)	(0.216)	(0.188)	(0.232)	(0.233)
Distance to Border (logged)	−0.041	0.386	0.241	0.296	0.293
	(0.101)	(0.251)	(0.262)	(0.291)	(0.290)
Area sq.km (logged)	−0.087	−0.289**	−0.225*	−0.250*	−0.251*
	(0.110)	(0.128)	(0.127)	(0.133)	(0.133)
Round 4 dummy	−0.219	−0.004	−0.205	−0.124	−0.126
	(0.493)	(0.349)	(0.327)	(0.355)	(0.355)
Round 4 dummy	0.092	−0.419	−0.681	−0.468	−0.464
	(0.585)	(0.484)	(0.473)	(0.498)	(0.498)
BIC	2,707.508	2,678.991	2,692.188	2,694.433	2,695.326
Observations	3,952	3,952	3,952	3,952	3,952
Overdispersion (θ)	0.042*** (0.004)	0.050*** (0.005)	0.048*** (0.005)	0.049*** (0.005)	0.049*** (0.005)

Robust Standard Errors in Parentheses (HC3) *p<0.1; **p<0.05; ***p<0.01

Figure 4 visualizes the fitted parameters from model 2 using simulations[12]. The solid line represents the expected number of conflict events across values of experienced poverty, while the dotted lines show the 95 % confidence intervals. In the left panel, both the conflict history and neighboring conflict variable is set to zero and the remaining control variables is set to their medians. The left panel shows that as district level poverty increases, the risk of conflict also increases. The middle and right panel shows simulations where conflict history is set to five and two years respectively. The simulations show that poverty is related to conflict, but this relationship is fortified if the time since the previous conflict is short. Last, the plot suggest an inessential association between affluent districts and armed conflict, across all settings of conflict history.

In substantive terms, using simulations where all controls are held at their median show that the utopia district, with citizens *never* experience lack of basic needs, with no previous conflict, and no conflict in neighboring districts at *t-1* has an expected conflict count of meager .02. Increasing the absence of basic needs to always, increases the expected count to 2.3. The same district, having experienced a conflict event five years before the survey has an expected conflict count of 4.6. On the other hand, an area with low experienced poverty (0) and conflict five years prior, still have an almost negligible expected count of .05. Hence, while impoverished districts seem rather receptive to repeat violence, richer districts seems to be more resistant to repeat conflict, even with a recent history of armed conflict.

Figure 4: Expected count of post-survey conflict events and experienced poverty.

Unconditional FENB model results

To test whether omitted variables may bias the estimates, I employ the unconditional FENB model as described in the modeling strategy section. Model 6 in Table 3 supports the previous findings

[12]The figure is the result of 1000 simulations

presented in table 2. The results suggest that poverty is related to conflict and that the relationship is presumably not biased by omitted controls. The results across all three specifications in table 3 is robust, and shows the same pattern; poverty is related to armed conflict.

Table 3: Experienced Poverty and Conflict Events with Fixed Effects Negative Binomial Models.

	Dependent variable:		
	Number of Conflict Events		
	(6)	(7)	(8)
Experienced Poverty	0.922**	0.922**	0.920**
	(0.389)	(0.400)	(0.424)
Conflict History		−0.389	−0.440
		(0.552)	(0.613)
Conflict Spatial Lag (t-1)			0.064
			(0.305)
Round 4 dummy	−0.038	0.002	−0.002
	(0.238)	(0.257)	(0.250)
Round 5 dummy	−0.259	−0.171	−0.167
	(0.261)	(0.305)	(0.307)
BIC	1,962.822	1,967.757	1,973.44
Observations	311	311	311
Overdispersion (θ)	0.918*** (0.110)	0.919*** (0.110)	0.920*** (0.110)

Bootstrapped Standard Errors in Parentheses (1000 replications) *p<0.1; **p<0.05; ***p<0.01

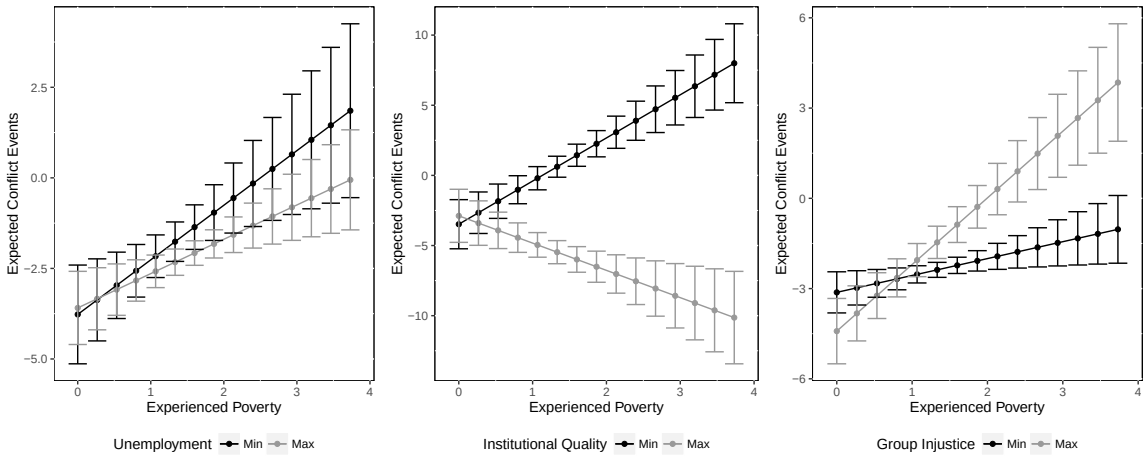
Interaction model results

While the results presented so far, suggest a positive relationship between poverty and conflict, the mechanisms behind the poverty-conflict nexus remains unaccounted for. Thus, I interact poverty with the three moderator variables to test the conditional hypotheses. More specifically, I test whether the poverty-conflict relationship shown in table 2 and subsequently table 3 is conditioned by (H3): conventional income opportunities locally, (H4): local state capacity, and (H5): local group grievances.

Figure 5 presents the results of three interaction models where each of the moderator variables interacts with levels of experienced poverty [13]. Model 1 (and left panel in figure 5) shows that while poverty is positive and significantly associated with more conflict, the level of unemployment does not seem to condition this relationship, contrary to the individual opportunity cost argument. In fact, there does not appear to be a significant difference in the conditional effect of levels of unemployment on the poverty-conflict nexus.

The results are somewhat puzzling when considering how previous country-level studies have found support for an opportunity cost argument. However, some explanations might shed light on this discrepancy. In a micro-level study, Berman et al. (2011) explores the relationship between unemployment and conflict in Iraq, Afghanistan, and the Philippines. Their results reject a positive association between local unemployment rates and the intensity of violent attacks. In general, they attribute this null-finding to the reduced cost of counter-insurgency information in areas with high unemployment rates. This information-cost factor may reduce the risk of conflict in poorer areas of a country. Besides, areas with high levels of potential conflict risk might see severe security measures implemented by the incumbents, reducing conflict propensity, while simultaneously cripple the local economy and reduce employment opportunities for local citizens. Thus, my results align closely with the findings of Berman et al. (2011). Other explanations might be that poorer areas are less attractive for rebel groups motivated by greed, as these regions provide less strategic value.

Figure 5: Expected number of conflict events when experienced poverty is interacted with: Unemployment (left panel), Institutional Quality (center panel), Share of population with perception of group injustice (right panel).



[13] A table showing the regression results is available in the appendix

In line with hypothesis 4, model 2 (center panel in figure 5) suggest that high-quality and robust local institutions have a substantial and significant pacifying effect of the poverty-conflict relationship. While poverty is associated with more conflict when local institution are perceived as impoverished and incapable, areas with high-quality institutions seem to mitigate the effect of poverty on the risk of conflict. Hence, the result suggests that robust institutions are essential to ameliorate the effects of poverty on conflict. The results here supports the findings of Wig and Tollefsen (2016), finding a pacifying role of local institutions.

Last, model 3 (right panel in figure 5) suggests that conflict is more likely in areas with a large impoverished population that also perceive their group to be unfairly treated. The findings are thus in line with hypothesis 5. On the contrary, districts with a large impoverished population which perceive their group as righteously and impartially treated by the incumbents have an insignificant risk of conflict. Poverty is associated with conflict, but only when grievances are widespread among local populations. Being poor will not necessarily increase inclinations to rebel, as long as most individuals perceive their government as fair[14]. However, when impoverished individuals experience collective grievances and unfair treatment from the incumbents, motivations to instigate violent outrage increases.

In addition to the tests presented here, I perform additional robustness checks in the appendix. In particular, I explore whether districts with a large number of conflict events are driving the results. This does not change the results, nor the substantive effect of the coefficients. Another concern is that districts with few respondents reduce the internal representativeness. In the appendix, I run additional models where I exclude districts with few respondents. The results show an increased effect of poverty on conflict intensity. Last, I run a hurdle model, and find that while poverty is related to the onset of local conflict violence, it seems to be more linked to increasing the intensity of conflict.

Conclusion

Existing studies of the relationship between poverty and conflict have operationalized poverty and conflict as nationally homogeneous phenomena. These studies show that impoverished countries have a higher risk of conflict. However, more recently, studies have started to explore the local relationship between poverty and conflict using sub-national data. However, their results have been mixed, mainly attributed to the use of different approximations of local poverty. Also, previous studies have examined the poverty-conflict nexus with purely objective measures of poverty. However, in this paper, I have investigated the relationship between poverty and con-

[14]One concern could be that very few districts are both poor and have high perceptions of local institutions. However, more than half of the poor districts (defined as LPI above the mean), also have perceived institutional quality above the mean. See scatterplot matrix in Appendix.

flict at the sub-national level, by introducing a novel measure of subjective poverty, measured in terms of people's experiences of (lack of) basic needs.

The results show that experienced poverty is positively and significantly linked to conflict. Lacking essential needs such as food, water, medicines or medical aid, fuel and a cash income is related to an increased risk of conflict. While previous local studies have reached similar conclusions, they have primarily used proxies with limited variation within countries, or have used measures of individuals or household assets instead of necessities.

In this paper, I have shown that experienced poverty matters for the risk of conflict, and that the results are robust to controlling for possible omitted variables using unconditional fixed effects negative binomial model. Also, the results showed that previous local conflict has a large impact on the risk of subsequent local conflict, suggesting support for a local conflict trap. The detrimental effects of having one conflict fortify the proneness for additional conflict.

While the fixed-effects model reduces the likelihood of omitted variable bias, it does not solve simultaneity bias, where poverty may be jointly determined with the dependent variable. To solve this, a instrument for poverty should be identified. However, identifying such an instrument is infeasible. While the fixed-effects model alleviates parts of the endogeneity issue of the poverty-conflict nexus, I cannot completely rule this out.

To further explore the mechanisms behind the poverty-conflict nexus further, I employed interaction models, to see whether the relationship between poverty and conflict was conditioned by factors frequently proposed in the literature. The results suggest three findings relevant for theories of local conflict.

First, local unemployment rates do not seem to condition the poverty-conflict nexus, contrary to the individual opportunity argument. Being poor and unemployed does not seem to increase the risk of local conflict, about being poor and employed.

Second, conflict is less likely to occur in impoverished areas where the local institutions are perceived as robust and of high-quality. However, when impoverished areas are governed by poor and low-quality institutions, conflict becomes more likely. Thus, high-quality institutions have a pacifying effect.

Third, group grievances seem to condition the poverty-conflict nexus. Impoverished areas where the majority population is aggrieved and perceive their group to be unfairly treated by the government, has a greater risk of conflict than poor regions with a contented population.

The findings suggest that poverty is linked to conflict. Poverty may create frustration, aggression, and even fuel grievances. However, the results have highlighted the crucial role of institutions in conditioning the effect of poverty on conflict, both through local state capacity to deter potential challengers, but also through its role to resolve the origins of group grievances, hostilities between groups and providing basic needs across population groups.

The results also highlight some possible avenues for how the poverty-conflict relationship can be reduced. First, strong local institutions may improve the organization of society, necessary for the equitable distribution of resources to citizens, independently of group identity. These local institutions need to provide both securities for residents, in addition to infrastructure essential for securing significant trade and flow of both people and goods. Well-functioning infrastructure is also critical for control of territories and in particular the periphery. Moreover, corrupt institutions fail to serve their purpose, and may fuel grievances if officials divert public funds to finance private expenditures.

This paper has explored the use of survey data, and how such data can be used to expand existing knowledge of the sub-national relationship between poverty and conflict. Survey data is an invaluable resource of information. Once georeferenced, this enormous pool of information may be linked with other spatial data sets. Georeferenced data opens up for a plethora of measures of interest to the local study of civil war. Future studies of local causes and consequences of conflict can to a great extent benefit more from survey data, and in particular georeferenced survey data. Also, this study has shown how interaction models may serve as essential tools to disentangle complex and fundamental questions.

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5 Local institutional quality and conflict violence in Africa

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Local institutional quality and conflict violence in Africa

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ABSTRACT

All politics is local. In spite of this familiar dictum, most studies that have investigated how institutions shape the conditions for violence and peace have focused on national institutions, and neglected the local dimension. This paper investigates the effects of high-quality local political institutions on the location of violence in internal conflicts in Africa, demonstrating that the quality of local political institutions matters even when the characteristics of national institutions are accounted for. We combine georeferenced survey data from the Afrobarometer surveys with georeferenced conflict data, allowing us to study the links between institutional quality at the subnational level and the occurrence of conflict-related violence. Crucially, we find that administrative districts with high-quality local government institutions are less likely to experience violence in an internal conflict than poorly governed districts. This relationship holds when controlling for a number of relevant factors like economic development, demographics, political opinions, urbanization and country-fixed effects. We also use matching techniques to improve inference, finding rather robust indications that local institutional quality pacifies.

Introduction

While the claim that "all politics is local" will be familiar to any political scientist, most generalizable research looking at the links between political institutions and civil conflict focuses on institutions at the national level. Studies have predominantly emphasized national institutional features such as regime type (e.g., Hegre, Ellingsen, Gates, & Gleditsch, 2001), the quality of government (e.g., Hegre & Nygård, 2014), or power-sharing institutions (e.g., Hartzell & Hoddie, 2007). Yet, it is a truism that political institutions are more than the parlaments, constitutions and departments that populate national capitals. Crucially, important political institutions can be found at the local level.

A number of contemporary examples indicate the importance of local institutions for violent conflict. Countries such as Kenya, Nigeria and Iraq have all recently experienced localized rebellions that have emerged in conditions of very poor local governance. Al Shahaab (Kenya), Boko Haram (Nigeria), and the Islamic State (Iraq) have all profited from weak local institutions, and capitalized on the frustrations they engender among citizens. Although a handful of studies have investigated the local institutional correlates of violence (e.g., Voors & Bulte, 2014; Tajima, 2013; Bellows & Miguel, 2009), they are restricted to single-case studies of individual countries and predominantly focused on how violence affects institutions, rather than the causal effect(s) of institutions on violence.

We address this gap by investigating how variation in the *quality* of formal political institutions at the local level impacts on the *location* of conflict violence in 20 countries in Africa. We focus on *formal local government institutions*, understood as "the set of formal institutions legally established to deliver a range of specified services to relatively small geographic jurisdictions" (Bratton, 2012, 517). Formal local government institutions are distinct from the ethno-specific customary institutions that also populate the institutional landscape in Africa and have been shown to matter for a range of outcomes (e.g., Michalopoulos & Papaioannou, 2013).

Our explanatory focus is on the quality of local institutions. High-quality institutions are uncorrupt, law governed, capable, trusted by the public, and efficient in their performance, and instantiate the general concept of "quality of government" (see e.g. Rothstein & Teorell, 2008) or "good governance" (e.g. Kaufmann, Kraay, & Mastruzzi, 2009). There is substantial variation in the quality of formal local government institutions in Africa. Some are trusted by the public and function well, with little corruption and efficient administration, while others are corrupt, wasteful and enjoy little trust from the citizens they are set to govern (see e.g. Olowu & Smoke, 1992; Bratton, 2012).

We claim that the quality of local institutions affects conflict risk through two primary channels: By shaping the *motivations* that give rise to violence, and by functioning as *opportunity structures* that can either facilitate or curtail conflict.

To test our main expectation we create a dataset combining spatialized survey data with georeferenced data on conflict events in Africa. Specifically, we rely on georeferenced data from the Afrobarometer rounds 3 (2005) and 4 (2008) to proxy for local institutional quality, as perceived by citizens, and combine this with

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geographically disaggregated conflict data from the UCDP-GED database (Sundberg & Melander, 2013). While acknowledging the limits of survey data for measuring institutional quality (discussed below), we maintain that this dataset presents us with a comprehensive picture of perceived local institutional quality across surveyed countries. Our dataset contains information on over 50,000 respondents in 1638 administrative districts and 20 states in Africa. While the nature of our sample – restricted to countries in waves 3 and 4 of the Afrobarometer – limits the scope for generalization, this allows us to assess more general patterns than the ones probed in extant single-country studies.

Our main finding is that administrative districts with high-quality local government institutions are less likely to experience violence. This relationship holds when controlling for a number of potential confounders, such as previous levels of violence, poverty, demographics, local support for the government, urbanization and geographic location. It also holds when we control for country-level characteristics by including country-fixed effects. A central threat to inference regarding this finding is endogeneity; while institutions have an impact on the risk of conflict, conflict impacts on institutions, creating a circular relationship. While we do not identify a satisfactory instrumental variables strategy for untangling this knot, we rather present a set of robustness tests that go some way toward alleviating at least some of these concerns, such as matching on previous levels of violence and assessing the sensitivity of our results to omitted variables following Altonji, Elder, and Taber (2005). While our results align with and contribute to previous cross-country studies showing that good governance can pacify (e.g., Hegre & Nygård, 2014), we extend this insight to political institutions at the local level, contributing to an emerging discussion on the interlinkages between local institutions and civil war violence (e.g., Voors & Bulte, 2014). Ultimately, the results indicate that the quality of formal local government institutions matters to local civil peace.

Institutional quality and conflict: state of the art

If institutions can pacify societies, they should do so not only through what they prescribe – e.g. elections, civil liberties or power sharing – but through how well they *function*. Institutional quality here refers to quality in the *output* side of the political process, and is thus distinguishable from democracy which is (primarily) conceptualized with reference to how policies and politicians are selected (see e.g., Dahl, 1971). We here draw on extant literature on institutional quality (see for example Kaufmann et al., 2009; Rothstein & Teorell, 2008), and define high-quality institutions as uncorrupt, law governed, capable, trusted by the public, and efficient in their performance.

Does institutional quality matter to peace? The most prominent arguments come in three main varieties. Some claim that well-functioning institutions help solve commitment problems that can lead to armed conflict (e.g., Hartzell & Hoddie, 2003; Walter, 2014), while others have been more concerned with how institutions alleviate conflict-inducing grievances through inclusion in the political system (e.g., Hegre & Nygård, 2014; Cederman, Gleditsch, & Buhaug, 2013). Yet others emphasize that high-quality institutions shrink the opportunity space for rebellion (Fearon & Laitin, 2003). In short, these arguments yield the expectation that high-quality institutions should reduce political violence in a society.

A handful of cross-country studies duly investigate whether institutional quality is indeed associated with peace. Hegre and Nygård (2014) find that informal aspects of institutions, such as low corruption and strong rule of law, have a significant pacifying effect at the national level. This also resonates with Fearon (2011), documenting that "good governance" is associated with less conflict (see also Walter, 2014). Missing from this literature however, is the local dimension of political institutions. This is out of step with recent trends in the study of internal conflict, where studies are increasingly moving beyond focusing on the macro-level of the nation state, to take a geographically disaggregated look at conflict processes at the local level *within* countries (e.g. Buhaug, Gleditsch, Holtermann, Tollefsen, & Østby, 2011; Rustad, Buhaug, Falch, & Gates, 2011). Our paper extends this move toward disaggregation to the link between institutions and conflict.

It is to some extent understandable that the reorientation toward the local has not been followed in studies of the institutions-conflict link. Firstly, most of the political institutions that scholars are interested in only exist at the national level *per definition* (e.g., national elections, supreme courts, power-sharing constitutions etc.). Secondly, there is a disconcerting lack of high-quality data on the design and functioning of local political institutions. In spite of this, studying the impact of local institutions is vital. Crucially, many conflicts have been shown to have local roots and dynamics that do not fit neatly within the national-level perspective (Kalyvas, 2006), and conflict areas are often unrepresentative of the country at large (Buhaug & Rød, 2006). Given this, ignoring local institutions misses a crucial dimension of variation that can give us more leverage in terms of identifying causal effects of political institutions. Moreover, looking at the local level brings us closer to the actual level of interaction; occurring between groups and individuals in their local institutional surroundings. While we readily acknowledge that there are important links between national institutions and local-level institutional patterns (discussed below), this study seeks to isolate the impacts of local institutions as such.

There are indeed a handful of studies investigating how local institutions shape conflict-patterns. These draw on single-country evidence, with examples covering Nepal (Bohara, Mitchell, & Nepal, 2006), and Indonesia (Barron, Kaiser, & Pradhan, 2009; Tajima, 2013). Although few in number, the general pattern appearing in these studies is that high-quality local institutions reduce the incidence of local conflict. Relatedly, a number of recent contributions study the reverse causal direction, namely how conflict violence affects institutions (and related outcomes), in diverse contexts such as Burundi (Voors & Bulte, 2014), Sierra Leone (Bellows & Miguel, 2009), Nepal (Gilligan, Pasquale, & Samii, 2014), and Kenya (Linke, 2013). However, there is a need for studies with a greater potential for generalization than these single-country designs. This article contributes to this.

Why local institutional quality pacifies

This section discusses why local institutional quality should reduce local-level violence. We argue that aspects of local institutions should affect both the motivations and opportunities that give rise to violence in a local context. While explicitly focusing on how institutions affect conflict risk, we acknowledge the potential for reverse causality in the institutions-conflict relationship, and that this affects the scope for drawing causal inferences. Hence, we end this section with a discussion of institutions as endogenous to conflict.

To structure our discussion of how local institutions impact on local conflict risk we sort the causes of local conflict-related violence into two categories: *External* and *internal*. External explanations highlight external actors' strategic motivations for attacking in a given locality. This can be done to target collaborators of the opposing side (Fjelde & Hultman, 2014), terrorize a population into supporting the attackers (Lyall, 2009), gain strategic control of an area (Zhukov, 2013), or to access lootable resources such as diamonds (e.g. Buhaug & Rød, 2006). Another brand of external explanations downplays the strategic aspect, focusing on the spread and diffusion of conflict events across space and time. On this view, conflict can be seen as an "epidemic" that can spill over administrative boundaries and

affect communities with few stakes in the ongoing conflict (e.g. Schutte & Weidmann, 2011).

Internal explanations on the other hand emphasize conflict mechanisms that are endogenous to the local communities where violence occurs, and that interact with the strategic motivations of outside conflict actors. On this view, outside actors and motivations serve as opportunity structures that locals can manipulate (e.g., Kalyvas, 2006). Hence, where the external view focuses on the outside "supply" of violence (spillover, external strategic motivations etc.), the internal view adds local "demand" (local grievances, and opportunistic behavior) to the picture. We are interested in how local institutions condition both internal and external drivers of violence and assume that they do so through affecting opportunities and motivations.

Local institutional quality and opportunities

Local institutions are opportunity structures that affect the costs and benefits of resorting to violence. We argue that high-quality institutions make violence more costly, both for external militias seeking to enter an area, and for local armed groups with endogenous origins. In doing so, we view high-quality institutions as a constituent factor in local state capacity. Districts with high-quality local institutions will have a more efficient police force, and a stronger justice system, that will increase the costs of taking up arms.

First, a strong police can deter external conflict actors. When conflict has originated elsewhere, but is bound to spread, a strong, uncorrupt and efficient local police force can provide collective protection preventing encroaching conflict-actors from entering an area. A corrupt and poorly organized police force can have the opposite effect, and pull rebels in. The M23 rebels in North Kivu province in the DRC is one example of a rebel group taking advantage of poor institutions, and particularly of those tasked with security. Some local police forces have even collaborated with the rebels. Human Rights Watch cites a local police officer charged with investigating M23 killings of civilians:

> …before each investigation, a high-ranking M23 commander, Innocent Kayna, told him: "You will do the investigation. You will say it's bandits in the neighborhood who killed, not M23." (Human Rights Watch, 2013).

A corrupt and inefficient police can thus be manipulated, coerced or bought by rebels, in addition to posing a much smaller security threat to rebels seeking to enter an area. When motivations for conflict are local on the other hand, local police forces can be instrumental in preventing local militias from organizing. They can indirectly disband existing armed groups, seize their assets and weapons, or cut off their resource base through shutting down illicit activities such as drug-trafficing, looting or protection rackets. When local police are corrupt or lack loyalty to the central government, then citizens will turn to rebels or local militias for protection. Areas where the police is weak and corrupt provides fertile grounds for armed groups. The Al Shahaab in Somalia (and Kenya) is one example. The group has stepped in to fill a security vacuum left by dysfunctional public security institutions. In spite of the harsh and dictatorial rule imposed by the group, it has gained some support in the population by introducing a measure of relative security (Human Rights Watch, 2010). In short, high-quality police forces will both make it harder for local rebel groups to form, and it will reduce support for local militias that claim to provide security.

Local institutional quality and motivations

Our second set of explanations highlight how local motivations for conflict can be affected by institutional quality. We will here both focus on the economic or political grievances that generate local conflict-related violence, and on more indirect and parochial motivations that use a wider context of conflict as a window of opportunity to settle scores.

First, local institutional quality might directly or indirectly affect the grievances that generate the conflict in the first place. In the case of conflicts with exogenous origins, poor institutions might cause local grievances that make locals more willing to join already existing rebellions. In other cases, poor institutions can lead to rebellions that are homegrown. High-quality institutions will reduce the number of local grievances that might give rise to such rebellions. Take the example of corruption. Local corruption can fuel grievances in itself, and is often claimed by rebel groups across Africa to be their reason for taking up arms (see e.g. Meredith, 2006). In this way corruption might affect the propensity for revolt *directly*, through amplifying perceptions of injustice.

Corruption and poor governance can also have indirect effects, through suppressing local investments in public-goods projects such as education and infrastructure (Le Billon, 2003), which in turn can lower the opportunity costs of participating in a rebellion. Additionally, corruption can lead to violence through creating rents that are appropriable through violence (Le Billon, 2003). High levels of corruption can thus engender community-wide grievances that fuel rebellions.

Furthermore, institutional quality can also interact with local grievances that are not directly related to the conflict in their content, but that can generate opportunistic behavior that increases violence in a local context of violent conflict. For example, Kalyvas (2006) illustrates how a context of national civil war can trigger local violence with motivations that are not specific to the broader national conflict but to local grievances and unsettled scores. On this view, civil war can amount to what Kalyvas calls the "privatization of politics" (Kalyvas, 2006, 332). Through denunciations and selective information local citizens can use conflict actors to "do their dirty work", for example by using the conflict as a pretext for getting rid of enemies for private gains. Private motivations for local violence can stem from family feuds, land disputes, or other personal conflicts.

Local motivations for violence can also revolve around local political competition. Rebel groups are often used as instruments in local political disputes that have little or no relation to the broader national conflict. For example, Reno (2011, 232–234) illustrates this in his description of what he calls "parochial" rebel groups, with examples from the Niger delta. These are rebel groups that operate in local political patronage networks, and that are often used as tools in local power struggles. When rebels and government forces are thus used as tools for producing violence to solve local conflicts it is because there exists a large number of issues that are not solved peacefully, in the local political system through local institutions. In institutional environments that function well, conflicts should be efficiently and peacefully adjudicated through local courts of justice, while local political competition takes place at the ballot box, reducing the demand for violent actors to step in to tip the scales in local conflicts.

We have now discussed some plausible channels through which high-quality local political institutions might reduce the risk of local conflict violence. This discussion yields the expectation that: *Administrative areas with high-quality formal local government institutions will have a lower probability of conflict-related violence than other administrative areas.* There are, however, potential opposing arguments that could call this expectation into question. Indeed, one could argue that in some cases high-quality institutions might *increase* the risk of violence. For example, when a government turns on parts of its own population, such as during genocides, an efficient police force and high trust in government (among the majority population) would make it easier to carry out massacres. Furthermore, local institutions

that are trusted and respected by a majority of the local population might be detested and feared by a minority, which is often the case in places with ethnic animosities. For example, a highly capable local police force might be used to more effectively repress unwanted minorities, and thus create conflict-inducing grievances. In these cases, there might be no – or even a positive – association between high-quality institutions and conflict violence. While these caveats are important, and could explain a potential null-finding, we believe the sum of arguments discussed above leads to the expectation that local institutional quality should – on average – induce local peace.

Endogenous institutions

A clear threat to inference regarding the pacifying effect(s) of political institutions is reverse causality. Indeed, several theoretical accounts of the relationship between conflict and institutions have treated the two as endogenous (e.g., Acemoglu & Robinson, 2006). First, conflict plausibly has a direct effect on institutional development through creating environments in which specific institutions thrive. For example, Wood (2003) describes how the conflict in El Salvador "militarized" local institutions, gearing them toward serving the demands of conflict actors rather than the needs of civilian citizens. Second, conflict can have indirect effects on institutional development, through destroying physical, human and social capital that is instrumental for institutional development. An example is found in Linke (2013) who documents that local experiences with conflict reduces trust in government. Third the risk of conflict can impact on institutions through altering expectations and preferences. This could, for example, lead to local institutions in highly conflict-prone areas placing greater emphasis on security provision, or collusion with actors with military potential. Finally, institutional quality is probably endogenous to many of the same processes that drive conflict, such as ethnic antagonisms, repression, social distrust, and deeply rooted political cleavages. In line with these arguments, a recent wave of studies indicate institutional effects of local conflict (Bellows & Miguel, 2009; Gilligan et al., 2014; Linke, 2013).

Since we believe it is almost certainly the case that institutional quality affects conflict and vice versa, disentangling the individual causal components in this (arguably circular) relationship is crucial, yet hard. The most obvious way to deal with such a problem is through finding (instrumental) variables that can predict institutional quality but that are otherwise exogenous to violent conflict. Unfortunately, no such instrument is currently available. We therefore opt for a different strategy, where we use matching and sensitivity analysis, both of which will be discussed below.

Local government institutions in Africa

Since we apply our argument to formal local government institutions in Africa, these deserve a brief discussion. Formal local government institutions, defined in the introduction, are manifest in local government councils, municipalities and city governments. While there is great variety in the structures and powers of local governments in Africa, most African states have an administrative level corresponding to the municipality level in some form (GADM, 2012). As highlighted in previous studies (e.g., Olowu & Wunsch, 2004), many African states have undergone a several waves of "decentralization" since independence, entailing a shift in power from central to local governments.

An important aspect of this process of decentralization has been the simultaneous delegation of powers to formal local government councils and so-called "customary institutions", such as traditional Kingdoms with pre-colonial roots (e.g., Herbst, 2000). In one sense, these are distinct from formal local government institutions: They (commonly) did not originate with contemporary states but usually have endogenous pre-colonial roots. Furthermore, they usually constitute "political systems" in the sense that they enjoy limited forms of self-rule and autonomy, with some of the trappings of traditional state-sovereignty. This is for example evident in the Buganda kingdom in Uganda, which has its own king, government and legislature. On the other hand they are often integrated with local government institutions, with vertical links to the state. This is, for example, the case, when local customary chiefs are also the leaders of local municipal councils or hold similar offices. In this sense, formal local government institutions and customary institutions are partly integrated and partly distinct sets of entities. While recognizing these nuances, we still restrict our focus to formal local government institutions. This is partly because we have no empirical tools for disentangling the effects of customary institutions from formal government institutions, but also because they are often overlapping in ways that make such a distinction difficult.

Another crucial aspect of local institutions in Africa is their relationship with central governments. As has been noted by several scholars (Herbst, 2000; Michalopoulos & Papaioannou, 2014), many African regimes have a weak state presence in their peripheries, thus allowing various forms of local government to play a crucial role. Some peripheries are weakly penetrated by national institutions for the simple reason that central governments are not interested in them, while others are weakly penetrated because of a lack of state capacity and inaccessibility (e.g., Tollefsen & Buhaug, 2015). Furthermore, in some cases local institutions may be undermined and corrupted by local governments on purpose, such as when a region is home to a marginalized ethnic group that the government wants to disenfranchise. While these complex relationships warrant a study in their own right, we here zoom in on the quality of local institutions in their own right, aiming to isolate their effects from those of national institutions through either (a) controlling for factors that could capture local–national relationships (such as ethnic exclusion), (b) by trying to parse out omitted variable-bias that is due to macro-level country context, and (c) through trying to handle the noted endogeneity issues, of which a government policy to weaken local institutions would be an instance.

Data

To investigate our expectations we need information on local variations in institutional quality, conflict violence and potential confounders, in a unified data structure. Since we are concerned with local institutions, local administrative units is a natural template for our dataset. We therefore rely on the spatial data of sub-national administrative units from the Global-Administrative Areas Database (GADM, 2012) (version 2.0) to serve as a template. This dataset contains the boundaries for all sub-national political units in Africa within the time period of our study. Since we want our data to be at a high level of spatial resolution, we choose the second administrative level below the state as our unit of analysis (i.e. the level that is below "regions", or "states" etc.), usually corresponding to the "district" level in the Afrobarometer surveys. These administrative units will be referred to as GADM units below.

Measuring local institutional quality

Since data that directly capture aspects of local institutional quality are unavailable, we rely on survey data from the Afrobarometer measuring the quality of local government institutions as perceived by citizens.

Afrobarometer has a number of properties that make it well suited for our purpose. The surveys cover a very high number of African countries and have currently been conducted in five rounds (2001, 2004, 2005, 2008 and 2012). The most attractive feature of

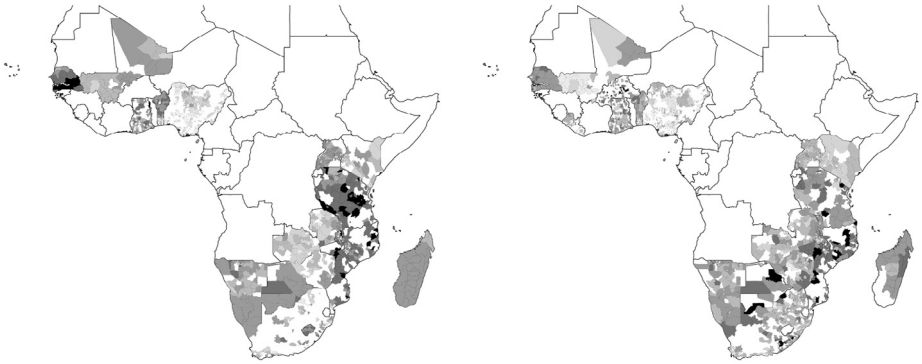

Fig. 1. Map showing the distribution of Local Institutional Quality (district level) in Afrobarometer rounds 3 and 4. Note: Districts with darker shades have higher institutional quality.

the data however is the fact that the Afrobarometer contains information on what region and district a survey respondent inhabits. Rounds 3 and 4 contain both region and district-level information, and are therefore used here. This allows us to link respondents to GADM-units by using the district, region and country identifiers. Using the Jaro Winkler distance matching procedure (Winkler, 1999), we are able to identify GADM units for over 80% of the districts in Afrobarometer rounds 3 and 4, allowing us to integrate the information in afrobarometer in a GIS framework with other spatial data. Further details on this matching procedure is described in the online appendix.

As discussed above, we conceptualize high-quality political institutions as uncorrupt, law governed, capable, trusted by the public, and efficient in their performance. Although we are not able to capture all of these dimensions directly through the survey items, we identify a group of measures that can function as proxies for these aspects. We use the following items: How much trust the respondent has in local politicians, how much trust the respondent has in police, how much trust the respondent has in the courts, how much police corruption he/she perceives, how much local political corruption he/she perceives, how the respondent rates the performance of local politicians over the past year, and whether the respondent has attended a community meeting in the past year (all variable codings are detailed in the appendix).

Since we are interested in institutional quality as a latent variable, and since analyzing one (or many) individual measures in isolation would make our results more sensitive to measurement error, we opt to create an index of local institutional quality. This allows us to make somewhat stronger inferences in the face of reliability and validity issues (discussed below) since the results will hinge less on individual measures. Using the survey items described above, we therefore perform a factor analysis to identify whether they form a latent dimension. In this analysis, where we include a number of other variables, we indeed find that these six items load strongly on the same factor (see appendix). In addition to the fact that these six items form a dimension, they can intuitively be said to capture the same latent concept, namely the quality of local political institutions: High quality institutions have less corrupt officials and police that perform better and whom the public trusts. They should also engage citizens through facilitating their participation. This also aligns closely with the conceptualization of institutional quality discussed above. In summary, we think an aggregate index is both more valid and reliable than individual measures.[1]

[1] We run analyses on each single item in the appendix, with similar results.

To create the index, we rescale the indicators such that they all point in the same direction (i.e. high corruption means *lower* institutional quality), before taking the average score of all the items composing the index (*Local Institutional Quality* henceforth).

Fig. 1 shows a map of *Local Institutional Quality* in all the districts that we have data on in all countries participating in Afrobarometer rounds 3 (left) and 4 (right). This map shows that much of the continental variation is country specific. Countries such as Zimbabwe, Zambia and Nigeria have relatively low *Local Institutional Quality*, while Botswana, Mozambique, and Tanzania have higher levels. Aggregating this to the country-level for round 3, we find that the country with the highest score on *Local Institutional Quality* in 2005 was Mozambique (2.57) and Tanzania (2.51), while the lowest-scoring countries were Nigeria (1.13) and Zambia (1.56). In round 4, the highest scoring country was Mozambique (2.50), while the lowest scoring was Nigeria (1.36).

As a validity test, we correlated the country scores on the *Local Institutional Quality* index with an additive country-level index consisting of the following World Governance Indicators: Control of corruption, government effectiveness, regulatory quality and rule of law. The results of this test (shown and discussed in the appendix) indicate an expectedly positive association between these measures and *Local Institutional Quality*.

Issues with survey data

While the survey data approach presented above currently represents our best shot at measuring local institutional quality, it comes with some limitations. First, while our data cover a wide range of African countries, as shown above, generalizations must be made with caution. This especially applies to generalizations from these data to other African countries. The Afrobarometer sample includes fewer than half of all African states, and the sample does not constitute a random draw. However, the internal representativeness is quite good, and we believe the Afrobarometer's procedures for selecting respondents – they rely on regional stratification and random sampling within the regions covered – ensure that we can draw modest generalizations within countries. While some districts include very few respondents, and thus might not be representative of the district, we run robustness tests where we exclude these cases. These generalization issues notwithstanding, our data provide much more scope for generalization than the single-country studies dominating the literature.

Relying on perceptions measured in survey data also introduces concerns about reliable and valid measurement. Crucially, since survey data provide an inherently subjective measurement (the perceptions of citizens) of an objective latent variable (the quality of

local institutions), biases can arise. Several factors might make perceptions both biased and noisy measures of institutional quality. For example, citizens might have different expectations, that, when varying stochastically, will create classical measurement error. Furthermore, when these expectations vary systematically, for example if people have higher expectations of institutional quality where institutions perform better, this will create systematic measurement biases. Relatedly, assessments of local government performance might also vary with cultural factors, that could be either country- or ethnicity-specific, and that are unrelated to actual institutional quality.

In spite of these concerns, we still believe there are good reasons for using survey data for our purposes. At present it is the best (and only) data source available for tapping local institutional quality. While citizen perceptions might suffer from the above noted problems, they still should be strongly *correlated* with actual institutional quality. Studies using the Afrobarometer data indicate that citizens perceptions of local (and national) institutions display the patterns we would expect if they indeed tapped objective institutional quality. For example, in a study of local administrative districts in Ethiopia, Jilke (2013), using Afrobarometer data, finds that districts that provide more access to information about political decision making and public fora for political deliberations – measured using non-perceptional data – are seen as more accountable by citizens. Perceptional data on government performance in the Afrobarometer data are also found to be internally consistent, and there are indications that they are tapping into the procedural aspects of government performance we are interested in (see e.g., Bratton, 2012).

Fortunately, many of these issues, relating to biases and measurement error can either be handled within the framework of our analysis, or do not constitute great threats to our conclusions. For example, much of the measurement error in perceptional measures will undoubtedly be stochastic and thus only induces attenuation bias. This will pull our estimates toward zero and make for more conservative tests. Moreover, some of the systematic (cultural or ethnic) biases in the perceptional data can be handled within the modeling framework we present below. For example, we can investigate whether our results are driven by country-specific biases by including country-fixed effects, or we can probe whether perceptions of institutional quality simply reflect other latent factors, by including such variables as controls. We return to these issues below.

Dependent variable: conflict-related violence events

Our primary source for conflict data comes from the UCDP-GED dataset, version 1 (Sundberg & Melander, 2013). This dataset contains information on yearly conflict events in Africa in the period 1989–2010, where a conflict event is defined as "the incidence of the use of armed force by an organized actor against another organized actor, or against civilians, resulting in at least 1 direct death in either the best, low or high estimate categories at a specific location and for a specific temporal duration." (Sundberg & Melander, 2013, 4). All events that are part of a conflict with over 25 battle-deaths in a year are part of the dataset, and we include both civil conflict, non-state conflict, and one-sided violence. The GED captures exactly the kind of conflict-violence we are interested in, namely that which involves organized armed groups in internal armed conflicts. While alternative datasets exists, such as the ACLED dataset (Raleigh, Hegre, Karlsen, & Linke, 2010), they are further removed from the types of violence we are interested in. For example, ACLED includes low-scale conflict such as riots and protests, which are conceptually distinct from conflict violence performed by organized armed actors. While protests and riots might indeed also be related to the quality of local political institutions – arguably through many of the same mechanisms mentioned above – the conflict events coded in the UCDP-GED are more representative of armed conflict violence as defined here. We therefore use GED in our main analysis. In the appendix, we include the ACLED dataset in alternative robustness tests, with no qualitative difference in results.

Fig. 2. Map showing the continental distribution of Local Institutional Quality (district level), round 3, and GED conflict events overlaid. Districts with darker shades have higher institutional quality.

The UCDP-GED dataset is constructed based on information from international news sources. Typical events include skirmishes between rebel groups and government forces, assassinations, and violent raids on villages. Since we here rely on information from Afrobarometer rounds 3 and 4, in 2005 and 2008 respectively, we will operationalize our dependent variable as post-survey violence, meaning that we count all instances of conflict related violence in a district after the survey. To best capture the prevalence of post-survey conflict-related violence in a district, we utilize a count version of this variable that registers the number of post-survey violence events in a given district. In the countries surveyed in round 3, there were 557 GED events in the period after the survey (2006–2010), while in round 4, there were 303 (2009–2010). The countries experiencing conflict events in round 3 were Benin, Ghana, Kenya, Madagascar, Mali, Nigeria, Senegal, Tanzania, Uganda, and Zimbabwe, while the countries experiencing events after round 4 are Ghana, Kenya, Madagascar, Mali, Nigeria, Senegal, South Africa, and Uganda. In Fig. 2 we present a map showing our main explanatory variable, with the GED events in the post-round 3 period overlaid. This shows that the variation in *Local Institutional Quality*, and the distribution of conflict events is substantial.

Fig. 3 illustrates the data structure at a more fine-grained level of resolution, focusing on Nigeria and Uganda, where *Local Institutional Quality* is measured in round 3, with a relatively high number of GED events in the post-survey period (i.e. after 2005). They also show the variation in reported *Local Institutional Quality* at the subnational level in both of these countries. Fig. 2 shows the continental distribution of local institutional quality, with conflict events overlaid.

Controls

To avoid the possibility that our estimates are confounded by factors that both affect conflict propensity and the probability of having high-quality formal local government institutions in a district,

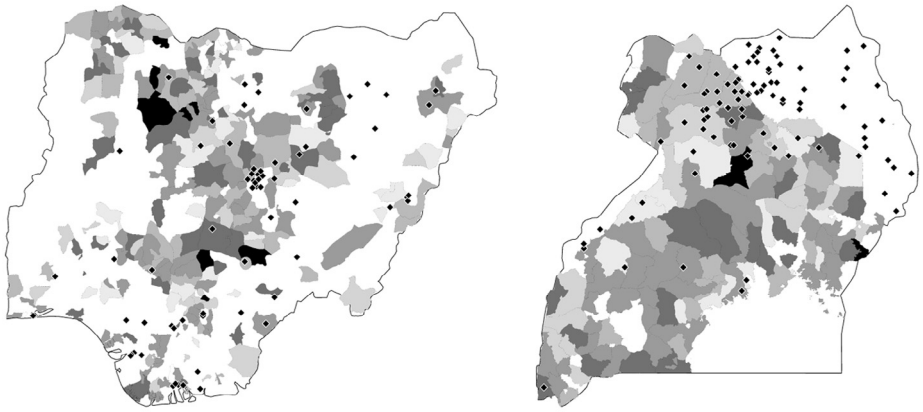

Fig. 3. Map showing the distribution of Local Institutional Quality in GADM districts in Uganda and Nigeria, round 3, and GED conflict events overlaid. Districts with darker shades have higher institutional quality.

we control for a number of variables that can plausibly be suspected to affect both conflict and institutional quality.

Since larger political units might differ in their levels of institutional quality (e.g., Hansen, 2013), and since larger units are likely to have more conflict (e.g. Hegre & Sambanis, 2006), we include covariates for local population size and the area of the district in question. The variable capturing local population comes from the Gridded Population of the World dataset (for International Earth Science Information Network CIESIN, Center, 2005) (version 3), and captures the log of the population in the district (*L(population)* henceforth). Furthermore, we include a control for the area of the district, measured as the log of square kilometers (*L(area)*).

We also include estimates for the number of men (between the ages of 15 and 20) present in the GADM unit, since young men should provide a pool of recruits for rebel groups and other conflict actors (we here take the log, referred to as *L(young men)* henceforth) (Urdal, 2006). These data come from Tatem et al. (2013). Additionally, we include a control capturing the travel time to the nearest city with more than 50,000 inhabitants (*L(travel time)*) (Uchida & Nelson, 2009), tapping the difference between rural and urban areas, which also has been shown to correlate with conflict patterns (Buhaug & Rød, 2006). This variable is taken from PRIO-GRID (Tollefsen, Strand, & Buhaug, 2012).

Local economic conditions are also included, to exclude the possibility that institutions affect conflict through the local economy and not through the mechanisms we put forward. To do this, we include two variables: One capturing subjective perceptions of poverty, operationalized as a Lived Poverty Index (*LPI* henceforth), measuring how often respondents have gone without basic necessities such as food, water, healthcare etc., over the past year. In addition to this we include a variable capturing the log of local infant mortality rate in the district (*L(infant mortality rate)* henceforth), which relies on data from the SEDAC Global Poverty Mapping project (Storeygard, Balk, Levy, & Deane, 2008).

In addition to population, area, travel time to nearest city, demographics, and economic factors, we also include a control for the popularity of the current government, by using the presidential approval rating (*Support for president* henceforth) in a given district. This is because we suspect that people will be less inclined to give positive answers to the questions relating to institutional quality if they disapprove of the current government, and since this approval will both be affected by recent conflict and affect subsequent conflict risk.

Finally, we control for the time since a previous conflict, since that will both affect *Local Institutional Quality*, and the risk of future conflict. We do this by creating a half-life parameter (*Past conflict events (half-life)*), calculated such that *if* there has been a conflict in the past ten years, we perform the following calculation: $2^{-(years\ since\ conflict)/2}$, and where there has been no conflict in the past ten years we assign a zero. In this setup, the effect of a conflict would almost be completely gone after 10 years, and halved after 2 years. We also include a spatial lag of conflict events, the log mean number of conflict events in contiguous neighboring districts (*Spatial lag*, henceforth), to capture spatial clustering in our dependent variable.

Results

As noted, our analysis uses data from rounds 3 and 4 of the Afrobarometer survey. Combined, there are 20 countries in the data and 1638 administrative districts in total. Since some districts and regions do not appear in both rounds, while many do appear in both rounds, it is hard to treat the data as a cross-section time-series panel. We have therefore chosen to analyze data from round 3 and round 4 separately in our main regression analyses.

Since the dependent variable in our analyses is the count of GED events, we use a count model. More specifically, we use a model that can handle the following properties of the data: The counts are not independent of each-other (if a district experiences one GED event, it has a higher probability of experiencing the next), and the variance of the counts is greater than the conditional mean. The negative binomial count model is well equipped for handling these properties, and is chosen as the baseline estimator (c.f Long, 1997, 217–250).

Table 1 shows the results from our first set of negative binomial regressions for rounds 3 (columns 1–4) and 4 (columns 5–8). The coefficients capture the change in the logarithm of a districts expected count of conflict events in the post-survey period. The first column shows a model that only includes population, area, young male population, travel time to nearest city, past conflict and the spatial lag of conflict. This model displays a strong negative association between *Local Institutional Quality* and post-survey conflict events, as expected. The next column adds poverty (*LPI* and *Infant Mortality Rate*) to the basic specification. The inclusion of poverty does not shake the results of *Local Institutional Quality*, which remains negative and strongly significant. Column three adds *Support for the President*, which is added to make sure that we are capturing the effect of institutional quality, and not of the popularity of the current government. The results of this inclusion shows that the negative coefficient for *Local Institutional Quality* is actually strengthened when this variable is included. The final column adds country-fixed effects to the specification, which only slightly weakens the negative coefficient for *Local Institutional Quality*. This suggests that *Local*

Table 1
Negative Binomial Count models regressing GED events on the quality of local institutions.

	Dependent variable							
	Conflict events							
	(Round 3)	(Round 3)	(Round 3)	(Round 3)	(Round 4)	(Round 4)	(Round 4)	(Round 4)
Local institutional quality	−1.682***	−1.723***	−3.107***	−2.080***	−0.852**	−0.898**	−1.034**	−0.800
	(0.268)	(0.267)	(0.444)	(0.618)	(0.402)	(0.386)	(0.405)	(0.521)
Support for president			0.550**	0.560**			−0.545***	0.325
			(0.242)	(0.285)			(0.198)	(0.256)
LPI		0.613**	0.828***	0.122		0.654**	0.391	0.108
		(0.245)	(0.257)	(0.282)		(0.289)	(0.293)	(0.333)
L (infant mortality rate)		0.399	0.329	0.528		1.105**	1.309***	0.859
		(0.430)	(0.422)	(0.685)		(0.501)	(0.504)	(0.764)
L (population)	−0.292*	−0.295	−0.186	−0.500**	0.091	0.033	−0.088	−0.458*
	(0.174)	(0.187)	(0.195)	(0.248)	(0.221)	(0.220)	(0.218)	(0.272)
L (young men)	−0.028	−0.068	0.051	0.225	0.870**	0.795**	1.343***	1.401***
	(0.363)	(0.368)	(0.366)	(0.312)	(0.351)	(0.349)	(0.335)	(0.412)
L (area)	0.342**	0.331**	0.375**	0.711***	0.661***	0.730***	0.697***	0.607***
	(0.148)	(0.151)	(0.155)	(0.207)	(0.186)	(0.186)	(0.191)	(0.221)
L (travel time)	−0.048	−0.091	−0.175	−0.456	−1.016***	−1.164***	−1.089***	−1.566***
	(0.280)	(0.286)	(0.291)	(0.332)	(0.305)	(0.318)	(0.322)	(0.340)
Past conflict	3.403***	3.010***	3.210***	3.845***	4.719***	4.436***	4.596***	4.473***
	(0.707)	(0.707)	(0.708)	(0.679)	(0.783)	(0.738)	(0.745)	(0.744)
Conflict events (spatial lag)	0.525***	0.497***	0.490***	0.220***	0.487***	0.356***	0.310***	0.192***
	(0.054)	(0.055)	(0.055)	(0.051)	(0.062)	(0.058)	(0.058)	(0.049)
Country dummies	No	No	No	Yes	No	No	No	Yes
Observations	1,091	1,089	1,089	1,089	1,339	1,335	1,335	1,335
Log likelihood	−409.823	−400.732	−398.385	−363.845	−322.176	−315.826	−313.114	−276.212
θ	0.140*** (0.023)	0.145*** (0.024)	0.147*** (0.024)	0.227*** (0.038)	0.135*** (0.027)	0.167*** (0.035)	0.178*** (0.038)	0.309*** (0.066)
Akaike Inf. Crit.	835.645	821.463	818.769	781.691	660.352	651.652	648.228	608.424

Note: *p < 0.1; **p < 0.05; ***p < 0.01.
Standard errors in parentheses, intercept and country dummies omitted from table.

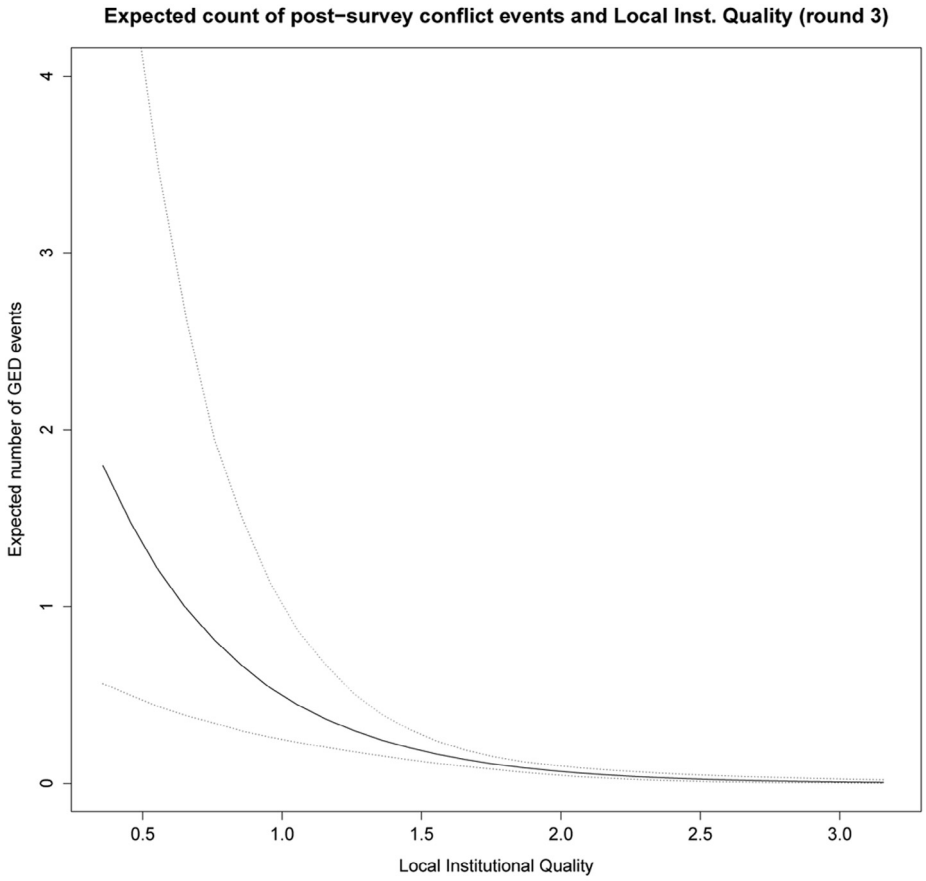

Fig. 4. Expected count of GED-events in a district in the post-survey period.

Institutional Quality is indeed important *at the local level* and not because it only reflects the quality of national-level institutions.

The final four columns repeat this set of estimations for round 4. In these models, the results are similar but slightly weaker: *Local Institutional Quality* is still negatively related to post-survey conflict events in all models. This is precisely estimated, except for in the most demanding model that includes country-fixed effects. Here, the coefficient is –0.800 with a standard error of 0.521 (with a P value of 0.12). It is not unsurprising that the results are weaker for round 4, given that there are much fewer conflict-events in the post-survey period in round 4. However, the similarity of the pattern in both rounds support our main expectation.

The influence of high-quality institutions is also strong in substantive terms. Fig. 4 is based on a simulation of model 3 in Table 1 (which is representative of the general pattern of the estimates for *Local Institutional Quality*). The figure shows the average expected number of GED events when *Local Institutional Quality* increases from its minimum to its maximum value when all other variables are kept at their means. This figure shows that the expected count of GED events drops radically toward zero as institutional quality rises. The average number of GED events in a district with *Local Institutional Quality* at a minimum is 2.7, while the average number where *Local Institutional Quality* is at a maximum is close to zero. This attests to the substantive importance of local institutional quality and supports our claim that local institutions matter to peace.

Additional tests

This section addresses several threats to inference that could throw our results into question.

First, we address the possibility that *Local Institutional Quality* is endogenous to conflict and conflict-risk, and thus not a cause but (partly) a *product* of conflict, as discussed above. Since plausible instruments for *Local Institutional Quality* are very hard to come by, we proceed by using matching techniques to increase our confidence that our result is not driven by this specific source of bias (Ho, Imai, King, & Stuart, 2007). Matching is a technique that preprocesses the data by using matching algorithms to improve *balance* between control units (observations where the treatment is 0) and treatment units (observations where the treatment is 1), thus achieving a greater approximation of the controlled conditions found in an experiment than we do when we use standard parametric models. Inferences drawn from analyses run on properly matched data are less model-dependent and more robust than model based inferences on unprocessed data (Ho et al., 2007). Since standard matching techniques assume a binary treatment variable, we dichotomize local institutional quality to take on the value 1 for units with above-average institutional quality and zero otherwise. Since this truncates the variation on the independent variable of interest, this makes for a more demanding test.

Since we are concerned with the possibility that *Local Institutional Quality* is endogenous to conflict, we would like to compare units that have the same *expected* value on this variable given their conflict history, but different actual levels of institutional quality. Those units would thus be similar in their propensity to receive treatment (i.e. have high-quality institutions) but dissimilar in their actual levels of treatment. To approximate this, we match all observations on four variables relating to conflict-history; A dummy variable capturing whether the district has had conflict in the five-year pre-survey period, our proximity of conflict parameter, a count variable

Table 2
Negative Binomial Count Models when observations are matched on conflict history.

	Dependent variable	
	Conflict events	
	(1)	(2)
Local institutional quality (binary)	–1.136**	–0.576
	(0.464)	(0.427)
Support for president	–0.110	–0.470*
	(0.234)	(0.248)
LPI	0.533	0.827**
	(0.359)	(0.366)
L (infant mortality rate)	0.175	1.005*
	(0.506)	(0.586)
L (population)	–0.049	0.149
	(0.284)	(0.292)
L (young men)	0.0001***	0.00003**
	(0.00002)	(0.00001)
L (area)	–0.035	0.816***
	(0.203)	(0.245)
L (travel time)	–0.135	–1.480***
	(0.363)	(0.420)
Past conflict events (half-life)	0.490	–12.974
	(7.375)	(10.455)
Observations	1,061	1,300
Log likelihood	–248.907	–214.254
θ	0.077*** (0.018)	0.092*** (0.025)
Akaike Inf. Crit.	539.815	458.507

Note: *p < 0.1; **p < 0.05; ***p < 0.01.
Standard errors in parentheses, intercept and country dummies omitted from table.

capturing the absolute number of GED events in the pre-survey period, and a spatial lag of conflict events. We proceed by using Iacus, King, and Porro's (2009) Coarsened Exact Matching (CEM) procedure, which is both computationally efficient, easy to use, and appropriate for our covariates. This groups the data into strata of units that are *as similar as possible in their propensity to receive treatment, given their conflict history*. We do this for both rounds, and run negative binomial models on the matched datasets, with dummy variables for each strata that are created by the matching procedure; In doing so, we attempt to investigate the effect of *Local Institutional Quality* within strata that are similar with respect to how conflict-history has affected the *Local Institutional Quality* in these districts. When estimating models with dummy variables on these strata, we exclude the spatial lag of conflict as a control, since there is not enough variation in this variable that is not exhausted by including the dummies on matching strata.[2]

The results of this procedure is presented in Table 2. The first column in the table shows the analysis on data from round 3. In this column, we see that although the substantial effect is weaker than most of the models presented above on data from round 3, we still find a negative coefficient for *Local Institutional Quality* when we match and run fixed effects on CEM strata. The second column, running the same model on data from round 4, shows a similar pattern, although the coefficient is much weaker (P-value = 0.13). That the coefficients are less precise is not surprising since we (a) loose statistical power due to preprocessing the dataset, and (b) truncate the variation in *Local Institutional Quality*. Nevertheless, the results still indicate a negative impact of institutional quality on the number of post-survey conflict events in a GADM unit. While there arguably are several other sources of omitted variable bias and (more specifically) potential endogeneities that could be driving our result, this exercise increases our confidence that there is indeed an independent effect of local institutional quality on conflict risk.

[2] When we include this variable, it is omitted from the estimation since it is perfectly predicted by the other variables in the model.

Another consideration counting in favor of our results not being caused by omitted variable bias (of which endogeneity is a specific instance) is the fact that our estimates are not severely weakened by the inclusion of additional controls. As Altonji et al. (2005) argue, the sensitivity of an estimate to the inclusion of (observed) covariates often yields a good guide to how sensitive the result is to confounding from unobservables. In our case, the coefficient (from round 3 estimates) changes from –0.983 (SE of 0.313) to –2.080 (SE of .618) when we move from a parsimonious model with no controls (appendix, Table S10, model 1) to a model with a full set of controls (Table 1, model 4). In fact, the coefficient actually becomes *stronger* when including controls, indicating that the confounding we are able to pick up with observables is actually *masking* a fairly strong effect. We conclude from this that our results seem fairly robust to omitted variable bias.

As an additional check, we run a number of models with additional control variable sets. First, we include proxies for *state presence*. This is because part of the *Local Institutional Quality* measure will reflect the mere *presence* of local authorities, and this might contaminate the estimate for *Local Institutional Quality*. For example, it is hard for respondents to describe police corruption if police is absent. To capture this, we include two measures: Whether the investigator (performing the survey) has observed police in the sampling area (*Police presence*), and whether the investigator has observed the army in the sampling area (*Army presence*). The latter variables are only present in round 3 of the survey. Second, we control for a composite measure of local social trust, since social trust potentially affects institutional quality, and conflict (e.g., Voors & Bulte, 2014). To capture this, we create an index of social trust, based on the factor analysis above, containing the following components: How much trust the respondent has in relatives, other countrymen, and neighbors. High scores on this index indicates greater social trust.

Third, we control for two alternative measures of socio-economic development that might pick up other aspects of local development than the two included in the main analysis. We include the mean level of employment in the district, and the mean level of education. Furthermore, we introduce controls for distance to the border and distance to the capital, since conflicts are more often found in the periphery (e.g., Buhaug & Rød, 2006), and state-capacity should be expected to decline in more peripheral areas (e.g Michalopoulos & Papaioannou, 2014). Finally, we control for the presence of excluded ethnic groups, since *Local Institutional Quality* might simply reflect ethnic antagonisms that make respondents hostile to government institutions. We use the spatialized version of the EPR dataset (Wucherpfennig, Weidmann, Girardin, Cederman, & Wimmer, 2011), and register the share of the area of the GADM unit that belongs to an excluded ethnic group.

Table 3 includes these four sets of additional controls (into the baseline model). The first column include Army presence and Police presence, without significantly altering the main result. This is not done for round 4 since these two questions are not included in that round. The next two models include the social trust index, columns 4–5 control for the additional development indicators, while columns 6–7 includes the distance to the capital and border. Column 8–9 controls for the presence of excluded ethnic groups. The estimate for *Local Institutional Quality* remains qualitatively similar across these specifications, although it loses statistical power in the model that controls for ethnic exclusion estimated on data from round 4 (column 9).

In addition to these tests, we perform a number of further investigations, all shown and discussed in the appendix. For example, we run our baseline models on alternative conflict data, and with different functional forms, observing no qualitative change in results. We also investigate hurdle models (see e.g. Zeileis, Kleiber, & Jackman, 2007) that separate between having *any* conflict violence and the

Table 3
Additional control variables.

	Dependent variable								
	GED events								
	(Round 3)	(Round 3)	(Round 4)	(Round 3)	(Round 4)	(Round 3)	(Round 4)	(Round 3)	(Round 4)
Local institutional quality	–3.164***	–2.484***	–1.450***	–3.101***	–1.006*	–1.992***	–0.874*	–2.113***	–0.833
	(0.637)	(0.710)	(0.547)	(0.629)	(0.556)	(0.631)	(0.529)	(0.617)	(0.531)
Police presence	–1.256**								
	(0.542)								
Army presence	1.883**								
	(0.742)								
Social trust		1.511***	1.242***						
		(0.531)	(0.434)						
Education				0.277	–0.067				
				(0.227)	(0.212)				
Employment				–1.183**	0.352				
				(0.483)	(0.547)				
L (capitol distance)						–0.241	0.287		
						(0.248)	(0.243)		
L (border distance)						0.042	0.174		
						(0.179)	(0.198)		
Ethnic exclusion								–0.386	0.631
								(0.404)	(0.468)
Observations	1,088	1,063	1,335	1,089	1,335	1,089	1,335	1,089	1,335
Log Likelihood	–377.442	–331.123	–275.779	–376.796	–278.476	–374.951	–276.425	–373.609	–275.203
Θ	0.196*** (0.033)	0.176*** (0.031)	0.302*** (0.064)	0.200*** (0.034)	0.299*** (0.065)	0.179*** (0.030)	0.309*** (0.067)	0.183*** (0.031)	0.309*** (0.066)
Akaike Inf. Crit.	812.883	716.245	609.558	811.592	616.952	807.902	612.851	803.218	608.407

Note: *p < 0.1; **p < 0.05; ***p < 0.01.
Negative binomial models of GED events for rounds 3 and 4. All controls, intercept and country-fixed effects included but omitted from table. Standard errors in parenthesis.

intensity of counts, estimating these two components simultaneously (see appendix for more discussion). By implementing this model we are able to assess whether *Local Institutional Quality* affects both the probability of having *some* conflict (i.e. more than zero conflict events) and the distribution of counts given that a district experiences conflict. The results from the hurdle estimation (shown in the appendix) indicate that *Local Institutional Quality*, while mattering slightly more for the intensity of conflict than for conflict occurence per se, is relevant for both processes.

These additional tests, and further investigations not discussed here but shown in the appendix, all suggest that our results are quite robust. In summary, they provide evidence for the claim that *Local Institutional Quality* reduces the incidence of local conflict violence.

Conclusion

Local institutions have often been overlooked in the discussion about how institutional quality relates to peace, partly stemming from a lack of data on local institutions. This paper shows how disaggregated local-level research designs also can be used to study the impact of local institutions on conflict patterns in a more generalizable fashion than current studies relying on single-country evidence (e.g. Voors & Bulte, 2014; Bellows & Miguel, 2009; Gilligan et al., 2014). In doing so, we heed the call to go inside the "black box" of the nation state in quantitative civil war studies (e.g. Kalyvas, 2003), as it specifically relates to the study of institutions.

We have argued that local institutions can protect societies from conflict-related violence through three channels: High-quality institutions entails a potent local police force and justice system that will increase the costs of rebels taking up arms in a given area, they solve local political and personal disputes that could otherwise lead to violence in the broader context of civil war, and they reduce local grievances that create motivations for joining or starting a conflict.

We furthermore proceed to show that "good governance" locally, as measured by georeferenced survey responses relating to dimensions like local political corruption, trust in politicians and political performance, is associated with a lower probability of conflict-related violence at the subnational level.

These findings come with several caveats. First, while providing more room for generalization than previous country-level studies, our findings do not support excessive generalization outside of the 20 country samples studied, due to the non-random nature of selection into the Afrobarometer rounds included. Second, the inherent difficulties of capturing "objective" local institutional quality, using inherently subjective survey data, highlight the need for future studies where the patterns discovered here are corroborated using other, more objective, measures than survey data. Third, we have not entirely resolved the potential endogeneity biases that haunt the institutions–conflict relationship; which really warrant a instrumental-variables strategy. While reasonably confident – after matching and sensitivity assessments – that our result is not driven by the noted endogeneity, we can not fully rule this out. Future studies should therefore probe the causal nature of the relationship documented here in greater depth. These nuances notwithstanding, we believe our data and results provide support for the claim that well-functioning, high-quality institutions can pacify, also at the local level. Hence, building well-functioning institutions locally should be a key priority for policymakers wanting to create a sustainable civil peace.

Appendix

Supplementary data to this article can be found online at doi:10.1016/j.polgeo.2016.01.003.

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6 Every Cloud has a Silver Lining: The Severity of Armed Conflict Recurrences

Andreas Forø Tollefsen

Abstract

This article examines the severity of internal armed conflict recurrence. While studies have considered causes of conflict recurrence, and what explains the variation in conflict severity, no study to date has investigated empirically whether subsequent conflicts are more or less deadly than new conflicts, and if so what explains this variation in severity? By using data on internal armed conflict episodes from 1946 to 2014, in a fixed-effects regression design, I show that severity in recurring conflicts is lower than in initial conflict episodes. This decrease may be due to the splintering of rebel groups following the termination of the initial conflict. Other explanations are the weariness of war, depletion of resources and discouraging effect of initial conflict on subsequent clashes. I also find that recurring conflicts are smaller, but not significant. Second, this article looks at whether the outcome of the previous conflict affects the severity in the subsequent episode. While literature has shown that conflicts ending in peace agreements are more likely to recur, the results presented here shows that once a peace agreement fail, subsequent conflict becomes less deadly than conflicts that recur following governmental victories. The results also suggest that conflict recurrences where the initial conflict ended in a peace agreement affect a smaller area. In general, the results show that peace agreements indeed have a pacifying effect, even when such settlements disintegrate.

Introduction

In 2014, more people was killed due to armed conflict [1], than any other year following the end of the cold war. 2013 surpassed by 2014; both became the bloodiest years since 1989 (Pettersson and Wallensteen, 2015). This increase in the number of battle-related deaths, comes after a declining trend, both in the number of conflicts and in their deadliness. Concurrently with this optimistic fall in deadliness, the share of ongoing conflicts that were relapses of old wars increased.

After the cold war, more conflicts ended in peace agreements, than in victories (Kreutz, 2010). However, as Toft (2010) shows, conflicts that end in negotiated settlements is more prone to recur. Thus, the increasing number of recurrences following the cold war can partly be attributed to the increase in the number of negotiated settlements. However, do the coincidental trends of declining death rates and the growing share of recurrences reveal that recurrences were, in fact, less deadly than new wars, and if so, why?

According to the UCDP dataset, the initial conflict episode between UNITA and the Angolan government from 1975 to 1998 caused on average 5000 casualties per year (UCDP, 2015). Following the failure of the Lusaka Protocol, the conflict broke out again in 1998 and lasted for five years, but only caused 1000 casualties per year. Why was this second conflict episode between UNITA and the Angolan Government less severe than the initial conflict? Moreover, why did the first conflict episode between Fretilin and the Government of Indonesia incur more than 5000 battle-related deaths per year, followed by less than 50 annual deaths in the subsequent episodes?

How conflict ends have an influence on the risk of whether the conflict recurs. Toft (2010) found that civil wars that ended in negotiated settlements were much more likely to recur. Human Security Research Group (2012) compared death tolls before termination and after recurrence, and found that peace agreements had the largest decrease in severity by any termination type. Studies also show that mediation efforts increases the risk of a group change, and in particular affects the likelihood of group splintering (Lounsbery and Cook, 2011). If dissatisfied with the outcome regarding ideological or monetary terms, the splinter groups may choose to undermine negotiations, defect, only to restart the conflict under a new umbrella. I refer to these as internal threats to negotiations.

External threats come from excluded groups not included in the negotiations, most often referred to as spoilers. Stedman (1997, pp.5) defines spoilers as "leaders and parties who believe that peace emerging from negotiations threatens their power, worldview, and interests, and use violence to undermine attempts to achieve it." (see (Nilsson and Söderberg Kovacs, 2011) for an overview of the debate about spoilers in peace processes) However, Nilsson (2008) shows that groups not included in the negotiations do not affect the parties vow to a peaceful resolution.

[1] Throughout this paper; I will use the terms internal armed conflict, armed conflict, and conflict interchangeably, all referring to internal armed conflict.

Partial peace is possible.

The concurrent trend in increasing recurrences and failed negotiations, might suggest that recurrences are often the result of splinters and spoilers, refusing or excluded from signing the agreement, they take up arms (again). These conflicts will inevitably be smaller, affecting both a geographically smaller area, and have reduced capability and support base. Buhaug et al. (2009) showed that small and peripheral conflicts also are more likely to persist because the government do not perceive these conflicts as a threat, and are not willing to bear the cost of fighting them. Their results may also suggest that battle-related deaths in smaller conflicts fought by splinter factions may be less deadly as their opponent leaves them to persist, precluded in the shadows on the rim of the state.

While some articles explore the determinants of conflict recurrence, and what makes conflict more or less deadly, no study to date has explored empirically whether subsequent conflicts are more or less deadly than new conflicts, and how the type of outcome affects severity when conflict recurs. While battle-related deaths are the most intuitive measure of conflict severity, the scope of conflicts varies greatly (Hallberg, 2012). Thus, I explore whether the extent of conflict changes between initial and subsequent conflict episodes. Raleigh and Hegre (2005) shows that the average scope of conflicts affects 48 % of conflict-affected countries territory. However, the average share of territory affected by repeat fighting is only 15 %.

I use data from the UCDP/PRIO Armed Conflict Dataset and develop a dataset of conflict episodes, consisting of consecutive years of fighting. I aggregate the average annual battle-related deaths from the UCDP Battle-Related Deaths Dataset (UCDP, 2015). The outcome of each conflict episode is assigned using the outcome definition in the UCDP Termination Dataset (Kreutz, 2010).

Using both OLS and fixed-effects modeling, the results provide considerable support for recurrences being less deadly than initial conflicts. However, subsequent conflict episodes do not seem to be smaller in geographical size, than initial episodes. Next, I analyze whether the outcome of the initial conflict affects the severity in subsequent conflicts. The results show that conflicts that end in a settlement, both ceasefires or peace agreements, are less deadly once they recur. Conflicts that end in governmental victory is more deadly than any other type of outcome. I find weaker support for the expectation that recurrences following peace agreement affect a smaller geographical area than those that ended in a governmental victory.

Previous studies suggest that conflicts that terminate in settlements are more likely to recur. However, the results presented herein shows that once peace agreements fail, the result is a less severe conflict recurrence, than if no agreement was signed.

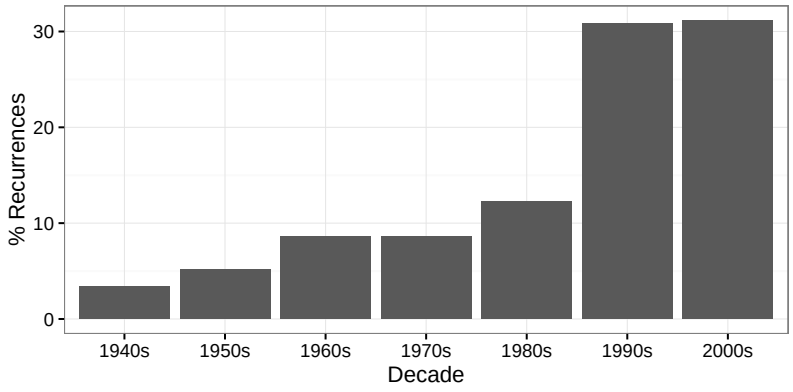

Figure 1: Old wars revisited. Percentage of conflict episodes defined as recurrences.

Internal Armed Conflict Recurrence

Post-conflict peace seems more fragile than ever, and recurring civil wars are today more common than witnessing new civil wars. Previous studies have shown that countries are experiencing one conflict increases the likelihood of experiencing another (Collier et al., 2003; Quinn et al., 2007). Thus, durable peace seems to manifest only in a limited number of cases. Of the 408 internal conflict episodes that occurred between 1946 and 2014, 43.6 % of those were recurring conflict episodes, being fought over the same type of incompatibility. As 1 shows, the share of conflict episodes defined as recurrences has increased in recent decades[2].

Countries experiencing one conflict has an increased risk of experiencing subsequent conflict. Mainly this is due to the negative effects owing from the initial conflict, amplifying favorable conditions for renewed conflict. Collier et al. (2003) refer to the effect of conflict as development in reverse, where conflict and negative development is both the cause and consequence. Civil wars tend to restart over again in the same set of countries, trapping these countries in a cyclic relationship of conflict and negative development.

Collier (2007) refer to countries where conflict persists as "the bottom billion", where favorable conditions for conflicts exists, and revisiting conflicts amplify these favorable conditions, making renewed conflict more likely. War-torn countries often build considerable war-economies, where weapons, skills, training and other equipment are dedicated to violent activity only. Other factors such as resource mining and smuggling can make renewed conflict feasible (Cilliers and Dietrich, 2000).

The consequences of initial conflict may also deteriorate the economy at the country level through loss of investments, loss of human capital, loss of tourism, and high costs of rebuilding(Gates et al., 2012). While economic development and growth seem to be significantly impor-

[2]See research design for clarification on the definition of recurrence and how conflict episodes are defined.

tant for civil war onset, Call (2012) finds that lagged GDP is not significantly related to civil war recurrence. However, Walter (2004) argues that strong economic and political incentives be the most important determinants as to why conflicts recur. In affluent and equal societies, no individual farmers, shopkeepers, and workers will voluntarily choose to enlist in the armies necessary to pursue war. Low quality of life and barriers to political participation are important determinants for which countries will experience recurring conflicts. Also, two factors associated with the previous war increases the risk of recurrence, long wars and partition outcome.

Political exclusion

While economic factors on the risk of conflict and it is recurrence have received an extensive attention, others argue that ethnic composition and exclusion be more fundamental for understanding conflict recurrence. In parts of the civil war literature, ethnic grievances (measured as fractionalization and polarization) has been dismissed (Collier and Hoeffler, 2004; Fearon and Laitin, 2003). However, studies that use more appropriate measures of political and economic exclusion (Cederman et al., 2013) shows that political and economic exclusion at the group level increases the risk of civil war.

Call (2012) finds that while ethnic and religious fractionalization is not significantly related to onset, it is related to the recurrence of conflict. He argues that political exclusion, rather than economic or social factors be the main driver of recurrence. While political exclusion acts as a trigger for civil war recurrence, political inclusion is highly correlated with the consolidation of peace. Post-conflict peace might well have exclusion without recurrence, but when either factions expectations are violated, exclusion might be the trigger mechanism necessary to restart violence, and in particular in the post-cold war era Call (2012). Toft (2010) finds that if the conflict was rooted in ethnic or religious identity, this increases the risk of recurrence. However, she does not look at exclusion per se, but rather on identity markers of ethnic and religious kinds.

Outcome of the Previous Conflict

Quinn et al. (2007) investigate the characteristics of civil war and their post-war environment to explain why some conflicts are more or less likely to recur, in particular looking at the outcome of the previous conflict. Their results show that outcome of the initial conflict matters for explaining which conflicts recur. Wars that ends in rebel victories and wars that end with negotiated settlements supported by peacekeeping forces reduces the risk of subsequent conflict. Also, their results show that peacekeeping forces are important to secure peace following a truce or settlement. Meanwhile, Hultman et al. (2015) show that as the number of UN military troops deployed increases, the chance of civil war recurring decreases.

Toft (2010) finds victory conflicts to be less likely to recur, but only those where the rebels prevail. Also, her results reveal that civil war is more likely after negotiated settlements. Mason et al. (2011) shows that outcomes also matters for the duration of peace. In line with other studies, they find conflicts terminating in victories having longer post-conflict peace periods. Also, rebel victories where rebels survive the initial years of post-conflict to consolidate their victory making peace stick longer. Also, settlements are fragile in the first years after conflict termination, but the risk of peace failure declines more rapidly for settlements compared to victory outcomes.

Government victories also tend to have shorter peace, probably because they fail to annihilate rebels completely. Hence, rebels can regroup, or new groups can form reducing the chances that peace will last. Joshi and Mason (2011) finds that larger governing coalition in post-civil war increases the durability of peace. Peace is more likely to fail when the ruling coalition is smaller. Also, outcome matters for inclusionary peace processes. Settlements lead to larger governing coalitions. Government victories also lead to larger governing coalitions as compared to rebel victories, possibly due to victorious governments having more incentives to accommodate rebels, than for rebels to accommodate losing governments. According to Joshi and Mason (2011) governments ought to fear a recurrence of conflict more than insurgents.

Thus, according to Call (2012), successful peacebuilding is closely linked to inclusionary behavior. His findings show that integration of former enemies into institutions is key to successful peacebuilding following negotiated settlements. For peace agreements, power sharing is the norm among those where peace sticks.

Other characteristics of previous conflict

Mason et al. (2011) finds support for a war weariness effects, where longer wars decrease the risk of conflict recurrence. Conflicts causing high death tolls, however, seems to be more likely to resume, possibly due to increased hostility and distrust. Zartman (1995) finds that hurting stalemates should make combatants more willing to negotiate, owing to exhaustion, high casualty counts, and long wars. Toft (2010) finds that higher average deaths in conflicts increase the risk of recurrence. This echoes the findings by Fortna (2004), showing that more deadly conflicts may also be more likely to recur.

Internal Armed Conflict Severity

While a large number of studies have explored the determinants of conflict recurrence, the empirical scrutiny of determinants of conflict severity has been given less attention. What makes conflicts initiate or recur may not be the same factors explaining the variation in the intensity conflicts. Explaining what makes conflict initiate or recur, often fails to take into account that

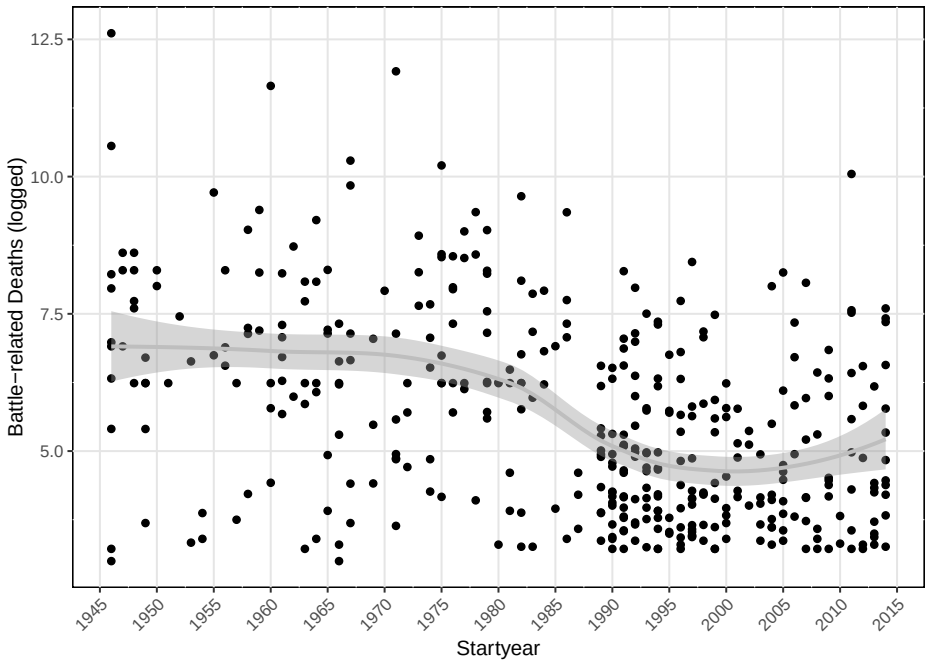

Figure 2: Battle-related deaths in conflict episodes plotted against the startyear of the episode.

conflicts come in many different sizes. What explains why the conflict in Ghana never incurred more than 50 casualties per year, while the conflict in Cambodia between 1967 and 1975 caused almost 30,000 casualties per year.

The main debate within studies of the severity of conflicts has focused on best practices on how to measure casualties, in addition to the declining trend in deaths. Lacina and Gleditsch (2005) shows that global battle deaths are decreasing post world War II, particularly owing to the decline of inter-state and internationalized civil wars. This trend becomes evident when looking at the data for internal armed conflicts, where the mean annual battle deaths before 1989 were 6375, while the same number in the post-cold war period was 743. Figure 2 shows the decline in the average number of annual battle-related deaths across time for conflict episodes.

This decline in violence relates to a larger debate, where the severity of war is not exclusively a post-world war II or post-cold war phenomenon. As Pinker (2011) argues, we are living in the "long peace", owing to the "rise of civilization". The grand decline in violence can be attributed to the increase in the number of centralized governments, effective police, and court systems, increased trade, and improved literacy (Pinker, 2011).

The existing literature on what explains the severity of armed conflict is limited. As Lacina (2006) puts it, "the burgeoning literature on civil war rarely considers why some conflicts are so much deadlier than others".Heger and Salehyan (2007) argues that despite its theoretical and practical importance, there has been limited research on the severity of the conflict. Understand-

ing what makes conflicts particularly bloody, is important, both to be able to know why more severe conflicts might occur, but more so to instruct sound policy to limit the severity of conflicts.

Lacina (2006) investigates the determinants of the number of combat deaths in civil wars and finds that factors explaining the severity of civil wars differ from the factors known to cause civil war onset. In particular, she finds that democracy rather than economic development or state military strength is associated with decreased severity. Post-cold war conflict also tends to be less deadly than the years before 1989.

Similarly, in investigating whether types of warfare matter for the severity of civil wars, Balcells and Kalyvas (2014) finds that democracy reduces the levels of battle deaths. Their results also suggest that external support for rebels' increases severity while external support for government reduces the severity. Post-cold war civil wars are less deadly, and to the authors surprise, ethnic composition measured as fractionalization reduces severity. Conflict in more heterogeneous societies is not more bloody. Also, conventional warfare rather than irregular warfare increases the severity, while irregular warfare increases intentional victimization of civilians.

Heger and Salehyan (2007) looks at whether the size of the governing coalition affects the severity of the conflict. Smaller governing bodies with fewer ruling elites has less constraint and are more willing to use force and violence to repress dissent quickly. Their analysis shows that smaller coalitions repress more heavily, and democracies are indeed more constrained in their use of violence. Also, they find that more groups in a country reduce the number of deaths.

Other studies have shown that natural resources within the scope of the conflict zone affect the severity of internal armed conflicts (Lujala, 2009). Wischnath and Buhaug (2014) looks exclusively at India and finds that harvest loss increases the severity of fighting in following years.

What explains onset or recurrence of conflict, might not account for the severity of these conflicts. Also, studies to date have exclusively looked at conflict in general, and not separated between initial and recurring conflicts. I will now turn to a discussion of how the legacy and outcome of the previous conflict affects the severity of the subsequent conflict.

Severity of Recurrence

Securing peace fails in almost half of post-conflict settings, and once it fails, it causes new casualties and more torment for already long-suffering populations. However, are these subsequent conflicts more or less deadly, and if so what explains this variation in battle deaths when peace fails? When conflict recurs, it is again destructive for the populations, once again uprooted after beginning the rebuilding, again fearing for their loved onset survival, witnessing fragile economic regeneration evaporate. Also, it is deeply demoralizing as well (Call, 2012).

The paper now turns to discuss whether factors affecting severity in initial conflicts should be

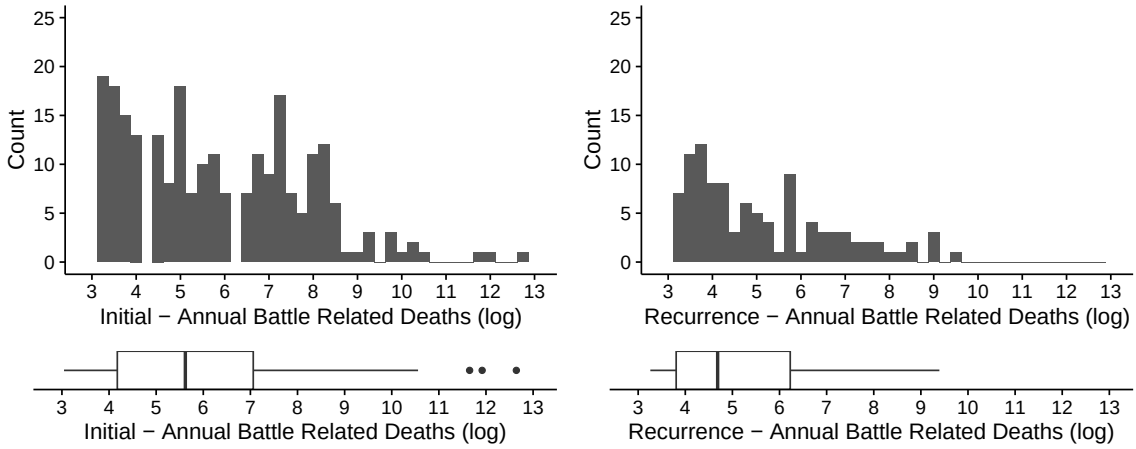

Figure 3: Two histograms, accompanied by respective boxplots below, shows the distribution of average annual battle-related deaths (logged) in conflict episodes. Left column shows initial conflict episodes and right column shows recurring conflict episodes.

expected to influence the severity in recurring conflicts, presenting the overarching framework. Previous studies have studied determinants of conflict onset, conflict recurrence, as well as examined the conditions explaining variations in the severity of internal armed conflicts. However, to date, no article has explained whether severity in subsequent conflict differs from initial conflicts, and if so, what explains this variation in severity.

A glimpse at the data shows that recurrences, on average are less severe than initial conflicts. Figure 3 indicates the distribution of annual battle-related deaths (logged) in initial and recurring conflicts. However, why should we expect subsequent conflict episodes to be less deadly than initial conflict episodes?

Initial vs Recurring Episodes

Between 1970 and 1990, the conflict between the Muslim secessionist group, the Moro National Liberation Front (MNLF) and the Government of Philippines, incurred 2753 annual deaths (a total of 107,389). After some years of relative stability [3], following a quest for a diplomatic solution to the conflict, accompanied by peace talks between the warring sides, the conflict recurred. While MNLF was still an actor in the subsequent conflict episode, the splinter factions, the Abu Sayyaf Group (ASG) and an earlier splinter from MNLF, the Moro Islamic Liberation Front (MILF), dominated the conflict in the years to come.

Still ongoing, this subsequent conflict period has incurred 310 annual deaths between 1993

[3] In 1991 and 1992 the conflict did never reach the 25 battle-related deaths threshold, as the group was reorganizing and search for diplomatic solutions

and today (UCDP, 2015). The top-left panel of figure 5 shows the decline in severity between the first and second conflict episode. While splintering might provide a clue, the puzzle remains, why was this second conflict episode far less deadly than the initial episode.

The National Union for the Total Independence of Angola (UNITA) fought the Government of Angola across three decades, starting in 1975. After the government's successes in 1994, UNITA was in a crisis, and peace talks begun, ending in the Lusaka Protocol, signed on December 20, 1994, backed by UN peacekeeping forces. While violence continued into 1995, the two following years was relatively peaceful. This initial conflict episode incurred on average 5079 annual battle deaths between 1975 and 1995. However, in 1998, UNITA commanders formed a splinter group, the UNITA Renovada. The split was followed by the launch of "Operation Restore" by the Angolan government, a massive operation against UNITA.

Between the re-initiation in 1998 and the end of the Angolan Civil War in 2002, the conflict incurred 1179 annual deaths on average. This five-fold decrease following a relatively peaceful period provides another interesting example of how subsequent conflict episodes exhibit lower casualty numbers than the initial episode. In figure 5, the Angolan Civil War is shown in the upper-right panel, and clearly illustrates the decline between the two periods.

One possible explanation for the decrease in battle-related deaths between the first and subsequent conflict episodes relates to the example above, the MNLF/ASG split, and the UNITA/UNITA Renovada split. Both groups splintered during the process of finding a diplomatic solution to the conflict. Splintering is not uncommon, nor surprising, as groups are heterogeneous, and the intentions and goals among members might show significant divergences (Pearlman, 2009).

Lounsbery and Cook (2011) shows that mediation attempts significantly increases the likelihood of rebel group splintering. While some groups are devoted to mediation, other calculates that the expected gain from rejecting mediation outweighs benefits from sticking to the agreement, either in monetary or ideological terms.

While leaders of a group might be devoted to securing peace, other factions within and outside the group might reject participating in negotiations and continue the fight. While one may expect that such threats to negotiations should hamper a promising path for peace, Nilsson (2008) shows that actors excluded from an agreement do not affect whether signatories stick to the agreement. Her finding also shows that actors not included in the agreement are more likely to continue fighting than actors listed in the agreement.

The splintering of rebel actors could lead to a complex set actors, and possibly smaller actors. If we look at the capability literature, smaller groups would have less capability to fight the government in government strongholds, and would have to modify their type of warfare from more symmetric warfare to more asymmetric warfare, and possibly the use of guerrilla tactics (Buhaug, 2010). This kind of violence would typically lead to a lower number of battle-related deaths than

more direct warfare, possible due to their existence in the periphery of the state. Thus, they do not pose a real threat to the government, and the government is not willing to bear the cost fighting them (Buhaug et al., 2009).

If dissatisfied sub-groups are the ones that reinitiate the violence, the number of groups in recurrences may also be higher. The data shows that the number of groups in recurrence conflicts is 14 % greater than in initial conflicts. This increase suggests that recurring conflicts involve more actors, and possibly the splintering into smaller factions may account for this.

Not many empirical studies exist explaining whether recurring conflicts are more severe than initial conflicts. One exception is Toft (2010) providing a brief glimpse of the severity of recurring conflicts. She investigates whether recurring civil wars were more deadly than initial civil wars comparing the total death in civil wars. Her findings reveal that the mean total deaths for recurring civil wars (258,377) are higher than mean total deaths in initial civil wars (135,897). Conflictingly, when looking only at battle-related deaths, she finds that the mean battle deaths are lower in recurring conflicts (70,517), than in initial conflicts (85,512). However, the t-test of difference in mean is not significant. Also, when comparing battle deaths per capita in initial and recurring conflicts, there was no significant difference.

These results show that there is a difference (however not significant) in deaths between initial and recurring conflicts. Also, the direction of this difference changes when only looking at battle-related deaths versus the total number of fatalities. Recurring civil wars seem to, on average create more deaths, but less battle-related deaths. However, the difference is not significant, and Toft does not provide a reflection on these results.

Consequences of the initial conflict episode are inevitably destructive. Primarily conflicts kill, on both sides, eradicating armed forces, also lowering the pool of potential recruits for subsequent warfare due to causalities. A consequence of initial conflict might be a decrease in military and civilian tolerance and support for yet another round of conflict. Recalling the devastating effect of round one might lower acceptance of more bloodshed. A shift in humanitarian considerations may very well result in lowering death tolls in subsequent conflict episodes, as measures will be implemented to avoid excessive casualties in recurring conflict episode. Also, spirit and morale among armed forces following the initial conflict can also be thought to decrease. If insurgents know the probability of getting killed remains high, lowering this risk could result in less combat casualty in subsequent episodes.

Similarly, long conflicts can be thought to exhaust combatants, eventually possibly leading to a hurting stalemate as casualties mount. Recurring conflicts following long wars could result in combat weariness as previous literature has highlighted. Subsequent conflict can also be thought to lower the morale among the warring actors. In particular, if the previous conflict was costly.

Longer conflicts give competing factions more information about the capabilities, increasing

the knowledge base for calculating the probability of winning another war (Walter, 2004). Smith and Stam (2004) argues that quick wars leave opponents with much uncertainty about the actual balance of power, while longer wars will increase the level of information of opposition side. Hence, longer initial wars decrease the risk of conflict recurrence. Similarly, long initial conflicts may decrease the casualties in recurring conflicts, as the potential rebels would have acquired information enough to moderate their objective, and must take measures to reduce casualties to have a chance to prevail.

Both long and deadly initial conflicts should matter for severity in subsequent conflicts. Primarily, long conflicts will make the potential rebels able to calculate their probability of success better. In the cases where they restart the conflict, they will maximize gains over cost, moderating their initial objective, due to their failure in the initial conflict episode.

On the contrary, conflicts may exacerbate hostilities and fear. If revenge increases due to events that occurred in the first war, the subsequent conflict episodes could be more deadly. As Kalyvas (2000) identified, personal vengeance was an important argument for why individuals partake in conflict. While I accept that revenge is a major factor that could increase the death tolls in subsequent conflict, the costs might be too much to bear, both due to the cost of lives and equipment, but also the cost of popular support and internationalized peer pressure to reduce violence levels in subsequent conflict. Having one conflict should draw attention to events, and pressure from international community might moderate the use of violence, and attract arrangements that reduce violence (i.e. sanctions and diplomacy).

Defeat, hatred, and injustice might be triggers for active revenge. On the contrary is a range of moderating conditions that should make subsequent violence less deadly. Consequently, the level of subsequent violence is a result of unsolved dissension leading to violence, triggered by grievances, hatred, and desire for revenge. Meanwhile, changed tactics to reduce casualties, securing popular support, international peer pressure, and combat weariness, morale, and support among soldiers and potential recruits moderate the level of violence in subsequent conflict.

Figure 5 illustrates the variation in severity across subsequent conflict episodes for six conflicts. The left column shows cases where a less deadly episode followed the initial conflict episode. The right column shows exceptions to this, where subsequent conflicts were much more deadly. The three cases presented in the left column, have different historical roots and conflict dynamics. However, there is a similarity in how the first episode ended. They all experienced mediation or mediation attempts between the deadliest initial episode and the subsequent conflict episode.

In Cuba, the government knocked off the invasion attempt by Fidel Castros men in the 26th of July movement. In Nigeria, the fight between episode between the troops of Patrick Nzeogwu and the Nigerian government ended in rebel victory and the introduction of military rule. Later,

Figure 4: Scatterplot showing the number of annual battle-related deaths (logged) in subsequent versus initial conflict episodes. Text labels indicate UCDP conflict id.

the fight between Boko Haram and the Nigerian government initiated in 2009, but violence dulled of, only to later submerge and incur thousands of casualties. At Sri Lanka, the first episode ended in the negotiations, and ultimately the ceasefire of 2002. Following the ceasefire in 2002, fighting was intermittent in 2003, which led to far fewer casualties on average than any preceding year of the conflict. However, the ceasefire ended, and between 2005 and 2009 and estimated 4600 people were killed on average per year, in the third conflict episode.

The cases illustrate that severity might be dependent on the outcome of the previous conflict. They suggest that mediations and peace agreements may lead to fewer deaths when conflict recurs.

In figure 4, I plot the number of annual battle-related deaths in previous conflict episode on the vertical axis and the casualties in subsequent conflict episodes on the horizontal axis. The plot highlights the cases illustrated in figure 5 and suggests that the predominant share of conflict episodes had a more severe conflict in the previous conflict episode.

Based on the non-significant findings of Toft (2010) and the above discussion of pacifying and amplifying conditions for the mortality of armed conflict recurrence, I expect that recurring conflict episodes should be less deadly than initial conflict episodes.

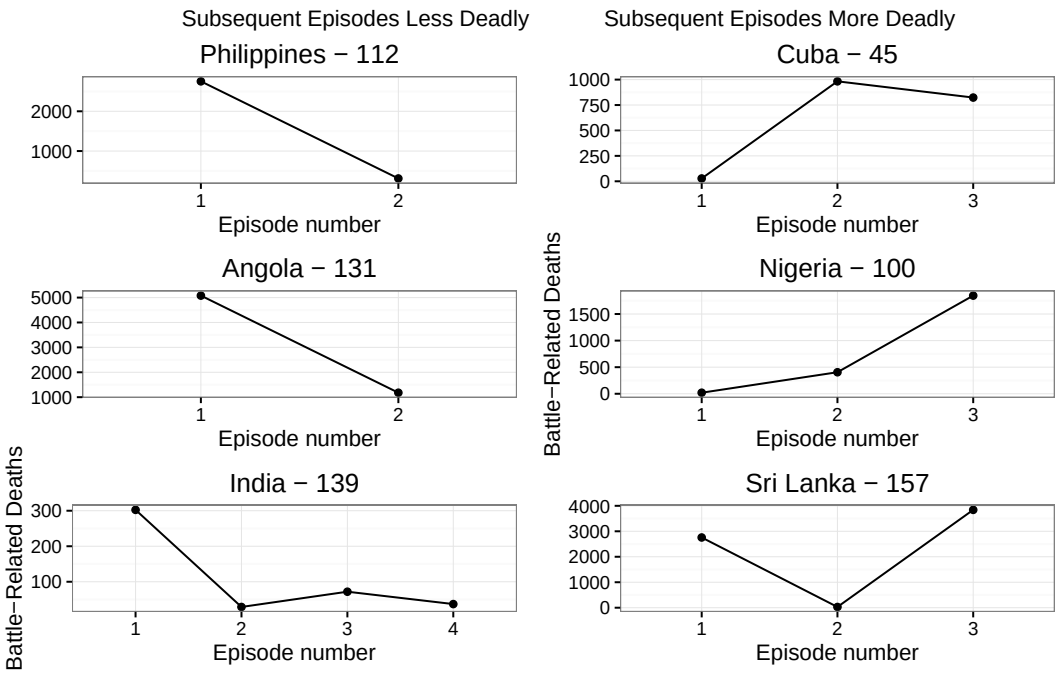

Figure 5: Average Annual Battle-Related Deaths across conflict episodes

Proposition 1: *Recurring conflict episodes are less deadly than initial conflict episodes.*

An alternative measure of the severity of conflict is the area affected by conflict. Thus, as an alternative measure of severity, I test whether recurring conflicts have an impact on a smaller area than initial conflicts. Splintering and the cost of war should all affect the size of conflict in subsequent episodes.

Proposition 2: *Recurring conflict episodes affect a smaller territory than initial conflict episodes.*

Outcome Matter

How conflicts end, varies widely. The UCDP Conflict Termination Dataset operates with four general modes of termination; peace agreements, ceasefires, victories and termination due to low activity. How a conflict is terminated might greatly impact the discrepancy between the actor's interests and expected gains, against the actual gains from the outcome.

The type of outcome in the initial conflict will be used by actors and sub-actors to evaluate their benefits and gains of the outcome, but also their loss and sacrifices. This discrepancy may motivate sub-groups within the actors to create splinter groups, or for the actors in itself to go back to war. While the MNLF searched for a peaceful solution to their claims for autonomy, members defected and created the ASG, which uses very different modes of violence (bombings, kidnappings, and assassinations).

The cases presented in figure 5 suggest that how a conflict ends, seem to impact the severity of subsequent rounds of fighting. If mediations attempt increases the risk of splintering, like Lounsbery and Cook (2011) suggests, recurring conflicts following failed settlements may be expected to be less deadly.

Different outcomes might affect how actors and sub-groups within the actors behave following a conflict termination. While victories should be expected to have a consolidating effect on actors, settlements may lead to members of a group to be dissatisfied with the outcome, increasing the risk of group change or splintering. Lounsbery and Cook (2011) finds significant support for mediation to increase the chance that a rebel group splinters. As she argues "one of the unintentional consequences of mediation may be the shifting of group cohesiveness and splintering of groups. Such splintering can result in the continuation of violence in a conflict that was supposedly settled".

So, if the outcome of an initial conflict affects the chance of splinter groups to form, we should most likely see this in the data. If the example of MNLF-ASG and the failed UNITA mediation attempts echoes a larger picture, we should be able to identify a variation in the number of groups, involved in recurrence conflicts, depending on the outcome of the initial conflict. In fact, the number of rebel groups involved in subsequent conflicts where the initial conflict ended in settlements[4] is 15 % higher than recurrence conflicts that ended in victory[5]. Thus, conflicts that ended in settlements and then recurred involved more actors than conflicts that ended in victories. One implication of this might be a change in the type of warfare, which again affect the number of battle-related deaths incurred.

Vüllers and Destradi (2013) qualitatively explores whether mediation failure increases escalation of violence. Their findings show that 11 out of 23 cases, mediation failure was followed by an escalation of violence. 12 cases remained unchanged or declined.

Looking at the descriptive statistics shows that subsequent conflicts were the previous conflict ended in a negotiated settlement (ceasefire or peace agreement) resulted in 654 average annual battle-related deaths. On the contrary, subsequent conflicts where the previous conflict ended in a victory (rebel or government victory) led to 1038 average annual battle-related deaths. This large difference may suggest that settlements reduce the severity, when peace fails, compared to victories. However, what explains that significant difference in severity levels across different types of conflict termination?

One possible reason might be the splintering argument that suggests that subsequent conflict where the initial conflict ended in a settlement increases the likelihood of splintering, thus fragmenting the existing actors or sub-actors (Lounsbery and Cook, 2011). Splintering would reduce the relative troop size, and might also require the newly formed splinter group to wage a different

[4] ceasefires or peace agreements

[5] both rebel and government victory

type of warfare, producing different severity levels.

Recurring conflicts where the previous conflict ended with a victory, will typically not cause splintering, as the actor will evaluate their chance of winning again, by staying united.

Some studies have investigated the effect various types of conflict termination has on the risk of recurrence. In particular, conflicts ending in victories are less likely to recur. This finding is robust across some studies (Toft, 2010; Dubey, 2002; Fortna, 2004). However, Walter (2004) does not find that any outcome matters for whether a conflict reignites.

Whether conflict actors are invited to discuss and come up with solutions to the initial conflict will affect the willingness to return arms. Joshi and Mason (2011) finds that larger governing coalition reduce the risk of recurring back into conflict. Hence, termination types where actors are invited to the drawing table for peace should matter for severity.

While settlements are more likely to recur, efforts have been made to alleviate the situation that produced the conflict in the first place (Lounsbery and Cook, 2011). Hence, this may alleviate some of the animosity between the sides, causing less severe recurrences. On the contrary, victories have not reduced animosity unless the losing side has been completely obliterated. Hence, once victory terminations fail, this could lead competing factions to apply full force to reach their aim (again), with little effort to alleviate hatred, making the competing factions driven primarily by revenge and desire for retribution. This could initiate more severe conflict recurrences, leading to higher level of casualties. Thus, I propose that:

Proposition 3: *Conflict recurrences where the initial conflict episode ended in a settlement is less deadly than recurrences where the initial conflict episode ended in a victory.*

While the severity of recurring conflicts is typically measured by the number of casualties, I also include a measure of the size of the conflict (Hallberg, 2012). It is likely that any reduction in conflict severity also manifests in a reduction in the geographical scope of the conflict. If outcomes affect the severity, we should see recurring conflicts have a different size than initial conflict.

Proposition 4: *Conflict recurrences where the initial conflict episode ended in a settlement affects a smaller area than recurrences where the initial conflict episode ended in a victory.*

Research Design

To assess the severity of internal armed conflicts, and whether recurring conflicts display different severity levels than initial conflicts, I adopt the conflict episode as the unit of observation. A conflict episode is defined as consecutive years of fighting, terminating when one calendar year has passed without conflict. I use a condition of two peaceful years for conflicts that terminate due to low activity (battle-related deaths below 25), to avoid lulls where conflicts fade in and out

of existence, artificially inflating the number of recurrences. Other termination types require one year without fighting before coded as a recurrence.

To reduce the risk that exclusively low-intensity conflicts fade in and out of the recurrence definition, I only include conflicts that at some point in time have been coded as war (more than 1,000 deaths in a year). This avoids the in and out conflicts (Kreutz, 2010).

Actors within conflicts create complex interrelationships, and can change, splinter or merge. However the UCDP/PRIO Armed Conflict Dataset keeps the same conflict id as long as the objective coded for the conflict remains the same, regardless if the actors change. Thus, conflict episodes followed by long periods of peace may be recurring with new actors, but would have the same conflict id. Thus, to account for shifts in actor composition, I code conflicts that recur after five years as new conflicts, and not as recurrences [6]. This variable is utilized in the following regression analysis.

Information on active conflict years is taken from the UCDP/PRIO Armed Conflict Dataset (UCDP/PRIO Armed Conflict Dataset v.4-2015, 1946 2014) (Gleditsch et al., 2002; Pettersson and Wallensteen, 2015), and is used to generate the conflict episodes. Each conflict episode represents consecutive years of fighting above 25 battle deaths in the same conflict[7] into conflict episodes. Using the above definition of episode and termination, this results in 408 conflict episodes within 192 different conflicts. 104 episodes (25 %) is coded as recurring conflict episodes, while 304 (75 %) is coded as initial conflict episodes.

To operationalize the first outcome, battle deaths, UCDP Battle-Related Deaths Dataset (UCDP Battle-Related Deaths Dataset v.5-2015, 1989-2014) (UCDP, 2015) is used for the years 1989 to 2014. However, as the UCDP dataset do not cover the period between 1946 and 1989, the original PRIO battle deaths data (Lacina and Gleditsch, 2005) is used to extend the data backward to 1946. As each of our conflict episodes cover from one to 61 years, we calculate the average number of annual battle deaths. For the following statistical analysis, I log-transform the average annual battle deaths in each episode to avoid skew and normalize the distribution.

I adopt the definition of severity by Lacina and Gleditsch (2005) where severity is measured in battle-related deaths caused by warring parties, directly related to combat. In cases where crossfire causes collateral damage on civilians, these deaths are also included in the figures. Meanwhile, a number of other causality types due to conflict can be identified, such as deaths incurred by one-sided violence (Eck and Hultman, 2007), and non-state violence (Sundberg et al., 2012). However, as the primary interest for this article lies in explaining the variation in the intensity of conflicts where one side is the government, and the change before and after conflict termination, one-sided and non-state violence is not included in the dataset.

The second outcome, the size of conflict, is taken from the PRIO Conflict Site dataset (Hallberg,

[6] I run robustness tests using alternative specifications in the appendix

[7] decided by the conflict id variable

2012). The conflict site dataset provides circular representations of conflict-affected areas. To avoid measuring the area of the circle that intersects with neighboring countries, I clip the circles against country polygons taken from the cShapes (Weidmann and Gleditsch, 2010).

Termination outcomes are extracted from the UCDP Conflict Termination Dataset (UCDP Conflict Termination Dataset v.2-2015, 1946 2014) (Kreutz, 2010), providing information on how each conflict terminated. As the UCDP Conflict Termination only applies one peace year criteria, this paper uses a slightly different criterion; some episodes are missing outcome information. However, the discrepancy is not severe. The UCDP Termination data for 1946 to 2014 covers 406 internal armed conflict episodes, compared to 408 in the episode data used herein.

The ethnic composition has been shown to affect both the severity and the risk of recurrence. According to previous literature, political exclusion matters for conflict onset. Call (2012) indicates that while ethnic exclusion is an important determinant in some studies of conflict onset, it is even more important to explain recurrence. As Lacina et al. (2006) states, "if ethnic or religious conflicts are grounded in particularly strong antipathy or are inherently zero-sum in nature, it may be impossible for the parties to ratchet down violence in favor of cohabitation." Her finding shows that ethnic polarization causes fewer battle deaths. However, she does not measure the ethnic composition among the conflict actors.

Ethnic composition in the country, and among the actual conflict actors might be very different. Thus, I include a measure on whether rebel groups[8] involved in the conflict episode are politically excluded.

The level of democracy is measured using the Polity IV dataset (Marshall et al., 2013). The Polity IV data contains measures of the degree of democracy and autocracy in each country. By subtracting the autocracy score for each country from the democracy score, resulting in a score of -10 to +10, where -10 is full autocracy and +10 is a full democracy. In this paper, autocracies are states where the polity is -6 or below, anocracies -5 or above, but 5 or below, and democracies are coded when the polity is 6 or above.

As splintering of groups might affect the likelihood of recurrence, and the severity of recurrences, I include a dummy variable coded 1 when the actors identified in a subsequent conflict involves the same group or groups as previous conflict episode in addition to a new group. This is the same definition used by Lounsbery and Cook (2011). Table 1 shows the descriptive statistics.

Empirical Strategy

To estimate whether recurrences display different severity levels than initial conflicts, I first use a pooled OLS model where the dependent variable is the logged average annual battle-related

[8]I use the data on ethnic groups politically excluded taken from the Ethnic Power Relations (Vogt et al., 2015), and merged with UCDP Armed Conflict Dataset using the ACD2EPR link.

Table 1: Descriptive statistics for main dependent and independent variables.

Statistic	N	Mean	St. Dev.	Min	Max
Annual Battle-Related Deaths	400	2,728.996	18,380.650	20.000	310,000.000
Annual Battle-Related Deaths (log)	400	5.595	1.823	3.045	12.644
Recurrence	408	0.255	0.436	0	1
Number of Groups	408	1.139	0.336	1.000	3.000
Splintering	216	0.120	0.326	0	1
Democracy	403	0.231	0.422	0	1
Excluded Rebel Actor	374	0.548	0.498	0	1

deaths[9]. I include dummy variables for the decade of the conflict to account for time specific trends.

Ordinary least squares assume that observations are independent and identically distributed random observations. However, conflict episodes are nested within conflicts, violating the assumption of independent observations. One episode is consequently the result of processes on a grander scale, the conflict level. This nesting would imply a pooled OLS model regarding conflict episode 1 and 2 in the same conflict as independent. However, this is an unreasonable assumption, as these units clearly are related (Bell and Jones, 2014), reducing the effective sample size.

A pooled OLS model is vulnerable to omitted-variable bias (OVB) where certain factors about each conflict is hard to control for. Either because these factors might be latent or that we cannot observe them. We do not have complete control over all factors that might affect both the dependent and the independent variable. As Hsiao (2014) argue, no matter how we expand a pooled OLS model for panel data, there will always be significant variables left out from the estimation.

To overcome potential serial dependency and OVB, I utilize cross-sectional time series data where the conflict is the level 2 unit (higher level unit) and the conflict episode the level 1 unit. Conflict episodes can be said to be nested within conflicts. Such panel data models allow accounting for individual heterogeneity (Allison, 2009). I use conflict-fixed effects (dummy variables) to restrict the comparison of conflict episodes only to within the same conflict (id).

I only include conflicts that at some point in time have reached that status of war, requiring more than 1000s battle-related deaths. This excludes minor conflicts that fade in and out of the dataset due to low activity, and would inflate the number of recurrences artificially.

[9] All analyses have been done in R, using the lm() command.

Results

Are recurring conflicts different from initial conflicts regarding battle-related deaths? Table 2 provides an initial test of hypothesis 1; whether subsequent conflicts are less deadly than initial conflicts. The first test of this proposition is presented in model 1, including only a variable for whether the conflict episode is a recurring internal armed conflict or not. Model 2 controls for whether the host country of the conflict episode is democratic and whether or not. In Model 4, I introduce a control variable identifying whether a rebel actor (side-b actor) represents an ethnic group excluded from political participation, along with time dummies. These represent the decade of the start year of the episode, to account for the grand decline in battle-related deaths as discussed above. To limit comparison of episodes to only within the same conflict, model 4 employs a fixed-effects model, where I introduce conflict (id) dummies to account for conflict specific fixed-effects. Last, Model 5 tests an alternative outcome, that of the area of the conflict.

In line with hypothesis 1, the results in models 1 and 2 in table 2 shows a negative and significant coefficient for recurring conflicts. The results suggest that recurring conflict episodes are less deadly than initial conflict episodes. The result is robust to the inclusion of controls for whether the conflict is fought in a democratic country or not. While the results in model 1 and 2 provides support for a decline in deaths in recurring episodes, this may be because recurrences occur at a more recent point in time, coinciding with the larger declining trend in battle-related deaths (Pinker, 2011). Thus, in model 3 I take time into account by introducing dummy variable for each decade. The results show that recurring conflicts have an independent negative effect on battle-related deaths, not explained by time per se. I also control for whether the non-state conflict actor represents an excluded ethnic group. Still, the results remain.

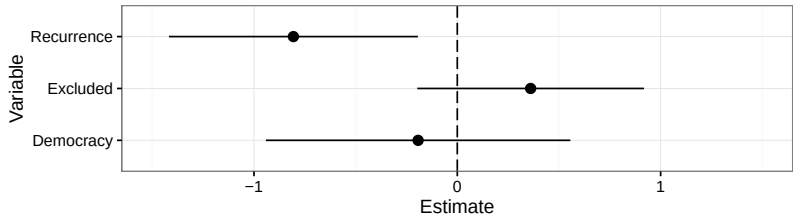

Figure 6: Coefficient point estimates and 95 % confidence intervals (based on model 4)

Figure 6 shows the regression coefficients and their respective 95 % confidence interval. As the coefficient plot indicate, even when accounting for time specific effects, the results suggest that recurrences are less deadly than initial conflicts. I employ estimates obtained from model 4, to calculate the strength of the coefficient in substantive terms. While keeping all regressors stable, an initial conflict episode would incur 701 annual battle-related deaths (95 % CI [515,954]) while

Table 2: The effect of recurrence on the severity of armed conflict episodes, 1946-2014

	Dependent variable:				
	Annual Battle-Related Deaths (log)				Area sq.km (log)
	(1)	(2)	(3)	(4)	(5)
Recurrence	-0.926^{***}	-0.979^{***}	-0.806^{**}	-0.954^{***}	-0.257
	(0.290)	(0.291)	(0.312)	(0.347)	(0.317)
Democracy		-0.329	-0.193	0.647	-0.555
		(0.367)	(0.382)	(0.587)	(0.593)
Excluded Rebel Actor			0.361	0.351	0.932^{*}
			(0.284)	(0.522)	(0.498)
1940s			2.062^{***}	0.924	-0.602
			(0.675)	(1.031)	(0.846)
1950s			1.128^{**}	0.032	-0.658
			(0.570)	(0.854)	(0.771)
1960s			0.640	-0.343	0.611
			(0.539)	(0.646)	(0.633)
1970s			1.451^{***}	0.417	-0.455
			(0.454)	(0.545)	(0.510)
1980s			0.819^{*}	0.688	-0.315
			(0.472)	(0.535)	(0.465)
1990s			-0.046	-0.152	-0.316
			(0.356)	(0.387)	(0.352)
Constant	6.639^{***}	6.675^{***}	5.864^{***}	6.051^{***}	17.793^{***}
	(0.151)	(0.163)	(0.352)	(1.123)	(0.913)
Fixed-Effects	No	No	No	Yes	Yes
Observations	216	211	192	192	161
Adjusted R^2	0.041	0.045	0.129	0.321	0.499
Note:					*p<0.1; **p<0.05; ***p<0.01

a recurring conflict episode would only incur 313 annual battle-related deaths (95 % CI [185,528]). This reduction means that the results presented in model 4 suggest that recurrences reduce the severity of armed conflict episodes by 44 % per year.

While the reduction in severity from initial to subsequent conflict episodes is considerable, the model does not compare conflict episodes that are part of the same conflict. The design is

also vulnerable to omitted variable bias. Thus, to account for conflict specific fixed-effects, I add conflict dummies, one for each conflict. A fixed-effects model uses the variation within the unit (conflict) to account for time-invariant stable effects. While this reduces the effective sample size by only using observations of conflicts with multiple conflict episodes, the fixed-effects model provides an improved comparison between initial and recurrence conflicts regarding deaths.

Thus, in model 4, I present the results of the analysis with conflict dummies[10]. The results show that conflicts that recur are less deadly than initial conflict episodes. The results provide strong support for the proposition that recurring conflicts are less deadly than initial conflicts, and omitted variables are not likely biasing the result.

So far, the results show that recurrences are less deadly than initial conflict episodes regarding battle-related deaths. However, severity can also be measured alternatively, by looking at the area that conflicts cover. If recurrences are less deadly than initial conflicts, one implication might be that they also affect a smaller area. The negative coefficient of recurrences in model 5 suggests that recurring conflicts are smaller, but this result is not significant. Thus, the result shows that recurrences are most likely to reduce the casualties in subsequent conflict episodes, but not the geographical scope of the conflict. While it is possible that recurrences are not smaller than initial episodes, the result can also be attributed to slow or little variation in the size of conflict polygons.

The results presented so far have shown that recurrences are less deadly than initial conflict. However, the results fail to reveal what makes recurrences less deadly, and how does the outcome of the initial conflict affect the severity in recurring conflicts? In table 3, I test whether conditions relating to the outcome of the initial conflict influence the severity of recurrence. To improve the comparison of outcomes, I restrict the analysis to conflict episodes between 1989 and 2014[11]

To account for how the outcome of the previous conflict affects severity, model 6 includes dummy variables representing how the previous conflict terminated. The comparison group is governmental victories. As the results show, settlements decrease the severity in recurring conflicts as compared to governmental victories. While we know from previous studies that settlements are more likely to recur, the results suggest that they are less deadly once peace fails. On the other hand, governmental victories are more deadly than any other type of outcome.

The pacifying effect of peace agreements is confirmed in models 6 and 7 in table 3. Conflict episodes where the previous conflict ended in a peace agreement reduces the severity of recurrence. Similar to the above argument, peace agreements might accommodate the preferences of the antagonist, reducing the desire for retribution and use of excessive violence. It may also reveal that recurrences are often initiated by splinter groups. Being discontented with the outcome, sub-groups may create new factions, smaller in numbers; they have to wage the different type of warfare, affecting the severity in subsequent episodes. In model 7, I also include a variable

[10]Conflict dummies are suppressed in the regression output.

[11]Before 1989, only four conflicts terminated in peace agreements according to UCDP (Kreutz, 2008)

Table 3: The effect of outcome on the severity of recurring armed conflict episodes, 1989-2014

	Dependent variable:			
	Annual Battle-Related Deaths (log)		Area sq.km (log)	
	(1)	(2)	(3)	(4)
Ceasefire Outcome	−1.021**	−1.297***	−0.622	−0.863
	(0.419)	(0.452)	(0.654)	(0.718)
Low Activity Outcome	−1.202***	−1.445***	−0.874	−0.923
	(0.373)	(0.396)	(0.558)	(0.611)
Peace Agreement Outcome	−1.267***	−1.343***	−1.242*	−1.255
	(0.475)	(0.483)	(0.702)	(0.762)
Rebel Victory Outcome	−0.297	−0.286	−0.933	−0.972
	(0.586)	(0.634)	(0.883)	(0.937)
Log Annual Battle-Related Deaths (lag-1)	0.257***	0.235***	0.166	0.125
	(0.080)	(0.085)	(0.113)	(0.131)
Democracy		−0.383		−0.146
		(0.322)		(0.479)
Excluded Rebel Actor		0.093		0.215
		(0.293)		(0.448)
Splitering		0.687		0.021
		(0.485)		(0.696)
1980s	−0.204	−0.391	−2.150	−2.187
	(1.592)	(1.549)	(2.019)	(2.099)
1990s	−0.166	−0.317	−0.369	−0.474
	(0.268)	(0.275)	(0.403)	(0.435)
Constant	4.629***	4.879***	18.689***	18.805***
	(0.554)	(0.595)	(0.828)	(0.972)
Observations	131	115	93	87
Adjusted R^2	0.125	0.181	0.008	−0.036
Note:				*p<0.1; **p<0.05; ***p<0.01

measuring whether splintering occurred. The results remain. Regardless of model specifications, when peace agreements fail, the result is a less severe conflict recurrence, than if no agreement was signed.

In model 8 I explore whether recurring conflict episodes affects a smaller geographical area than initial episodes. The coefficient shows that recurrences where the initial conflict ended in a peace agreement, affects a smaller area, but the result is only significant at the $p<0.1$ level. Still, the results are in line with model 6 and 7; peace agreements seem to have a pacifying effect on the severity of armed conflict recurrence. In model 9, I include controls for democracy, exclusion, and splintering. The result of the peace agreement is no longer significant at $p<0.1$ level (z=-1.646982).

Robustness

The results presented above, suggests that subsequent conflict episodes incur fewer battle-related deaths than initial conflict episodes. While model 4 in table 2 use a fixed-effects model to address the potential that omitted variables bias drives the results, there are other threats to inference. In this section, I will discuss two threats to sturdy inference; outliers, and endogeneity in the form of selection bias.

A concern in regression analysis is that outliers are driving the results. Thus, I turn to cross-validation to explore if outliers might be driving the result. Jack-knifing explores the variation of a statistic, typically following a series of regression models were repeated iterations is executed while dropping one observation in each iteration. Primary use involves the identification of outlying data points (Mooney et al., 1993).

Here, I re-run model 3 in table 2 to assess whether initial episodes with extremely high and recurrences with extremely low casualty numbers are driving the results. I repeat the model but excludes episodes from one conflict at each iteration. The result of the 192 repetitions is plotted as kernel density plot in figure 7, showing the distribution of the estimated Z-scores across model iterations. Lines have been added to compare the distribution of z-scores to the conventional levels of significance; -1.96 and 1.96 as dashed lines, and the less restrictive -1.64 and 1.64 as dotted lines.

Figure 7 shows that the majority of z-scores is located below the critical -1.96 threshold. 96.8 % of the iterations yielded a Z-score below -1.96, and no iterations lead to an estimated Z-score higher than -1.832. Thus, the cross-validation does not suggest that the results are driven by outliers[12].

While there is no evidence to suggest that outliers are driving the result, the cross-validation does not solve the fact that certain unobserved conditions might explain which conflicts is "selected" into the recurrence (treatment) category. In a controlled experiment, only the random assignment of units to either treatment or control group would lead to unbiased results. However, a randomized controlled trial to assign whether conflicts recur or not is not feasible. Whether

[12]Six conflicts out of 192 caused the z-score to dip below -1.96. These were conflict in Guatemala (id=36), Nagorno-Karabakh (id=193), Uganda (id=118), India (id=152), Sri Lanka (id=157), Indonesia (id=134).

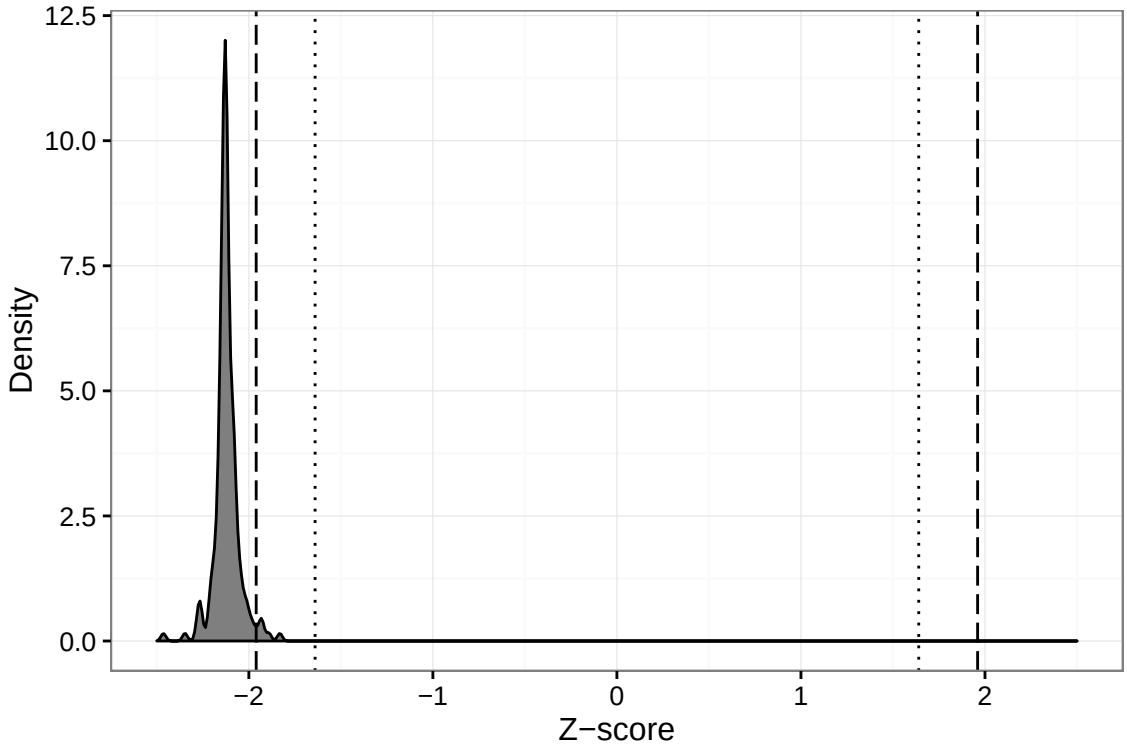

Figure 7: Distribution of estimated Z-scores for recurrence, based on 2000 iterations using Jack-Knife leave-one-out.

or not a conflict is assigned to the recurrence group, is the result of internal and external dynamics of the initial conflict and political and military decisions made by incumbents.

Whether or not a conflict recurs is given by the decision of conflict actors to take up arms (again) or not, and whether the government mobilizes massive efforts to disintegrate their challengers. If the outcome was beneficial to all parties, recurrence is not desirable, and conflict does not recur. On the contrary, certain individuals or factions within the rebel group might perceive their expected gain, monetary or ideologically, to be higher if they restart the conflict.

While rebel groups can influence whether or not a conflict restart, their relationship with the state influences the dynamics of conflict, in particular, the duration and likelihood of recurrence. Conflicts at the periphery of the state pose little threat. Cunningham et al. (2009) shows that the balance between rebels and the state can explain the duration of the conflict. Importantly, they show that relatively weak rebels in the periphery pose little threat to the state, and thus, these conflicts are allowed to persist. Their results also show that conflicts in democracies are much less likely to end.

While democracies increase the duration of the conflict, it also incurs fewer casualties by reduced use of force (Lacina, 2006). Democracies are also constrained in their use of strength. As

Merom (2003, pp.15) argues, "My argument is that democracies fail in small wars because they nd it extremely difficult to escalate the level of violence and brutality to that which can secure victory. They are restricted by their domestic structure, and in particular by the creed of some of their most articulate citizens and the opportunities their institutional makeup presents such citizens. Other states are not prone to lose small wars, and when they do fail in such wars it is mostly for realist reasons". Thus, this may suggest that democracies let conflict persist, or even recur, as long as they do not pose a great threat.

Thus, recurrences might be suffering from selection bias, where recurrences are decided by forces such as democracies, deciding the trajectory of future conflict episodes by balancing force and political cost. As Merom (2003) states it "essentially, what prevents modern democracies from winning small wars is disagreement between state and society over expedient and moral issues that concern human life and dignity...Achieving a certain balance between...the readiness to bear the cost of a war and the readiness to exact a painful toll from others is a precondition for succeeding in war."

While the models take into account whether the conflict episodes is located in a democratic or not or not, and excludes conflicts that are exclusively low intensity [13], this does not fully explain the selection process of recurrences versus initial episodes. Thus, I cannot fully rule out that the result suffers from selection bias.

Conclusion

While some studies have explored the determinants of conflict severity, as well as conflict recurrence, no studies to date explain what makes recurrences more or less deadly than previous conflict episodes. Studies of conflict severity have pooled all conflicts together, disregarding how properties of the previous conflict, it is severity and how it ended, affects subsequent conflicts.

As this article demonstrates, recurring conflict episodes are less deadly than initial conflict episodes. In fact, predictions show that recurrences incur about 44 % fewer deaths, than initial conflict episodes. The results remain when we compare conflict episodes only within the same conflict. This reduction in severity from initial to subsequent conflict episodes can be attributed to the increasing number of conflicts ending in negotiated settlements, causing rebel groups to splinter. These splinter groups initiate renewed conflict, but as splinter groups become smaller, they also inflict less damage and have to wage alternative types of warfare. Small actors also pose less threat to the government, and the government might not be interested in bearing the cost of fighting them, as long as they remain on the rim of the state. In particular, democracies are

[13] Exclusively low-intensity conflicts would fade in and out of the dataset, and many of these conflicts are low-intensity conflicts in the periphery. The discussion above suggests that in particular democracies tend to ignore such conflicts, as they do not pose a threat, and are costly to win.

inclined to leave conflicts in the periphery ongoing for long, as long as these conflicts do not pose a threat.

The results were analyzed using a pooled OLS model. However, as this does not compare initial and subsequent conflict episodes within the same conflict, I employed a fixed effects model to improve comparison and to reduce potential omitted variable bias. Still, the results remain.

While recurrences are indeed less severe than initial conflict episodes, the results did not explain what made these recurrences less deadly. Thus, I explored how the outcome of the preceding conflict episode affected the subsequent episode. The results showed that when conflicts where the previous conflict ended in a peace agreement, became less deadly in the subsequent conflict episode. Peace agreements increase the risk of splintering (Lounsbery and Cook, 2011), causing smaller factions to continue to fighting, leading to decreased severity in the subsequent round.

The results provide an optimistic picture; while conflicts ending in peace agreements fails more often than conflicts terminating in clear victories, peace agreements reduces violence, even when such settlements disintegrates. Continuation of violence following negotiated settlements is less severe, and there is good reason to believe that many of these conflict actors exist on the periphery of the state, posing little threat to the state per se. While the inclusion of all actors might remain a utopia, those that stay out, inflict less damage than what possibly would have been without the agreement.

Future research should focus more on severity in general. There is a need to differentiate on conflicts that cause 25 deaths and 800 deaths per year. The results presented here show that while conflicts ending in peace agreements are more vulnerable to recur, they become more peaceful when they recur than conflicts ending in victories. Future research should explore whether peacekeeping forces have a pacifying effect on recurrence following negotiated settlements, and in particular for recurrences caused by small splinter groups. Peacekeeping is a valuable tool, both to reduce the risk of recurrence (Hultman et al., 2015), and to reduce harm to civilians (Hultman et al., 2013).

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Appendices

7 Appendix to Chapter 3

Andreas Forø Tollefsen and Halvard Buhaug

Insurgency and Inaccessibility[1]

Supporting information

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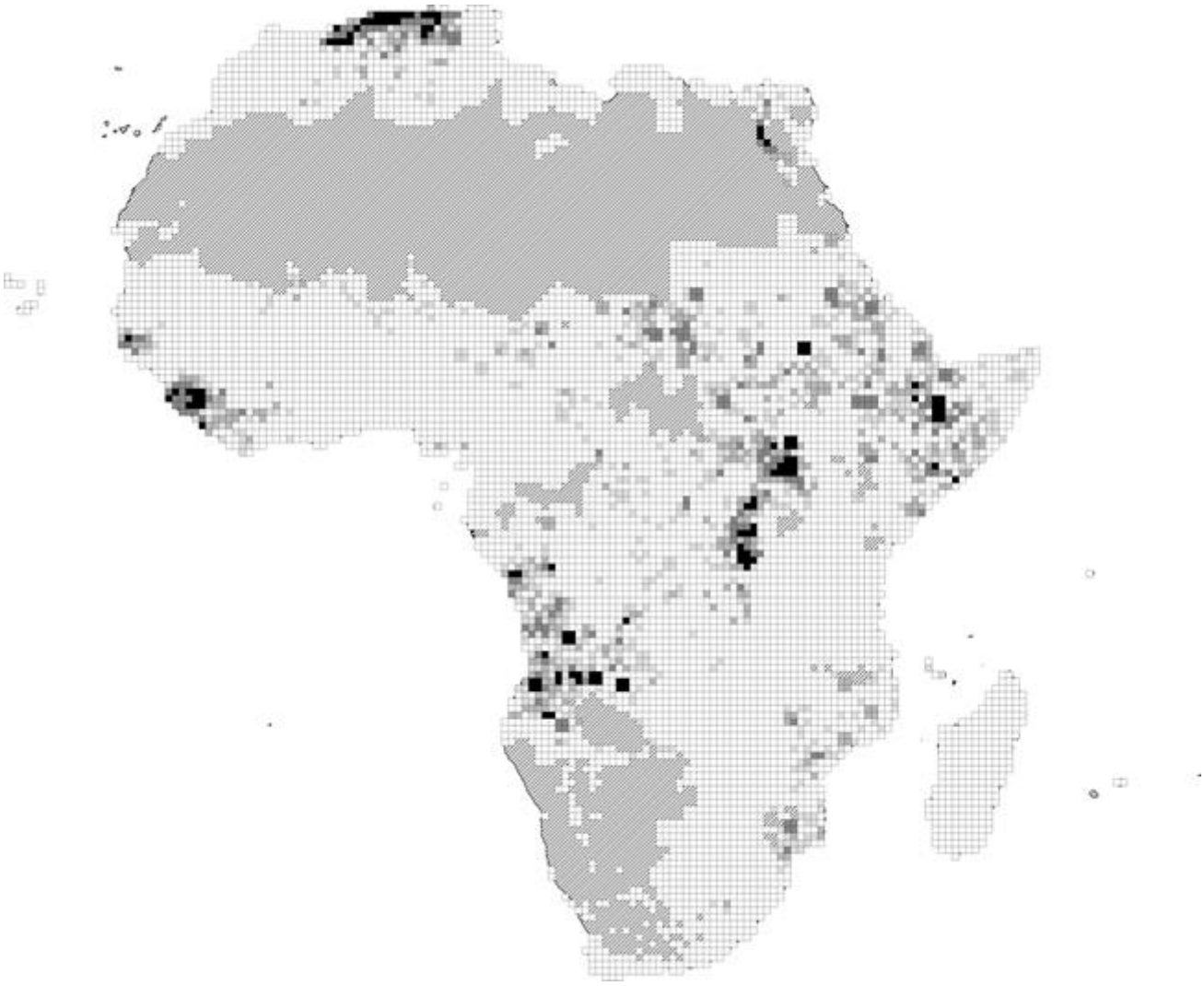


FIG. S1. Frequency of GED events, 1989–2010

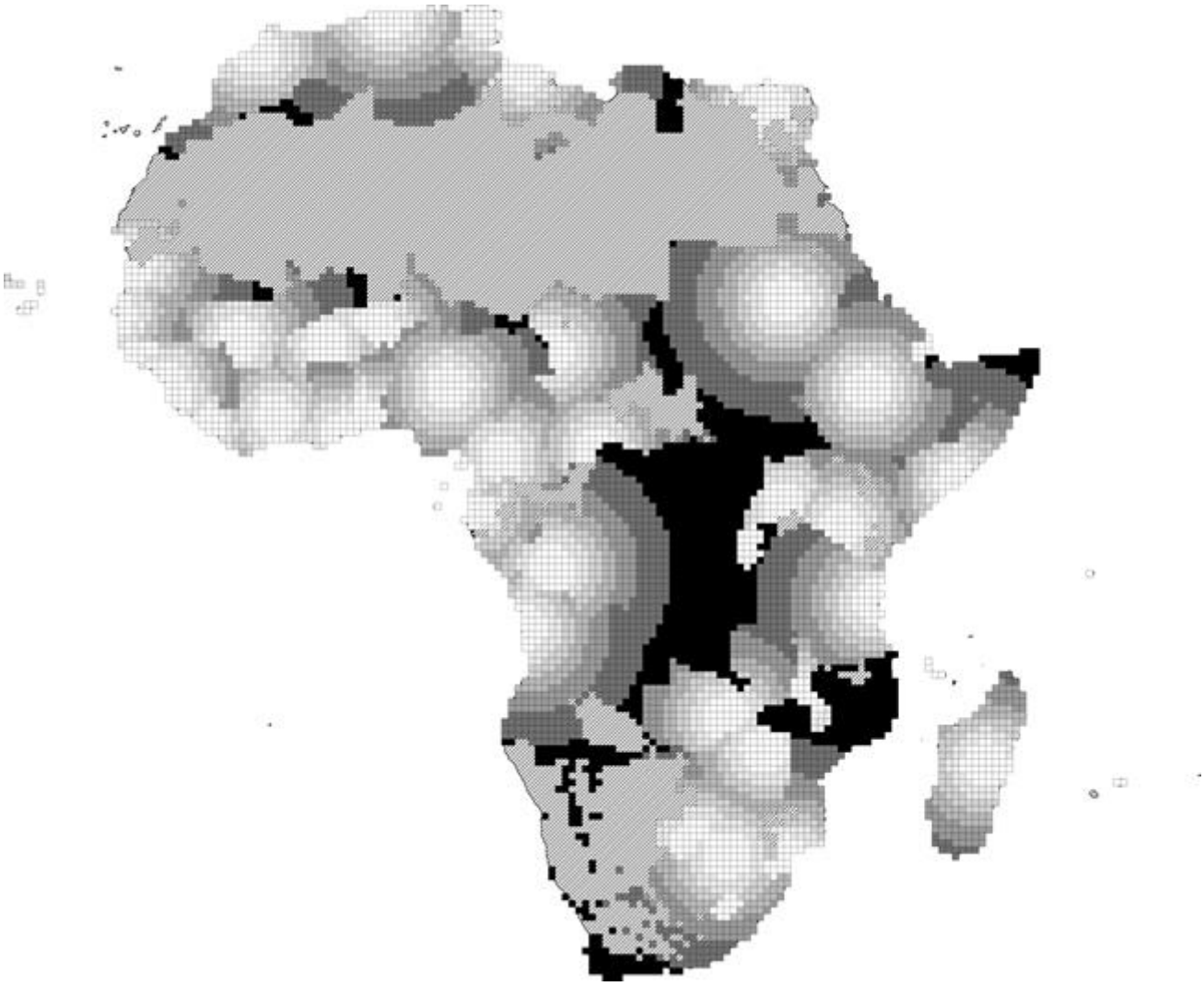


FIG. S2. Distance to the capital city, 1989

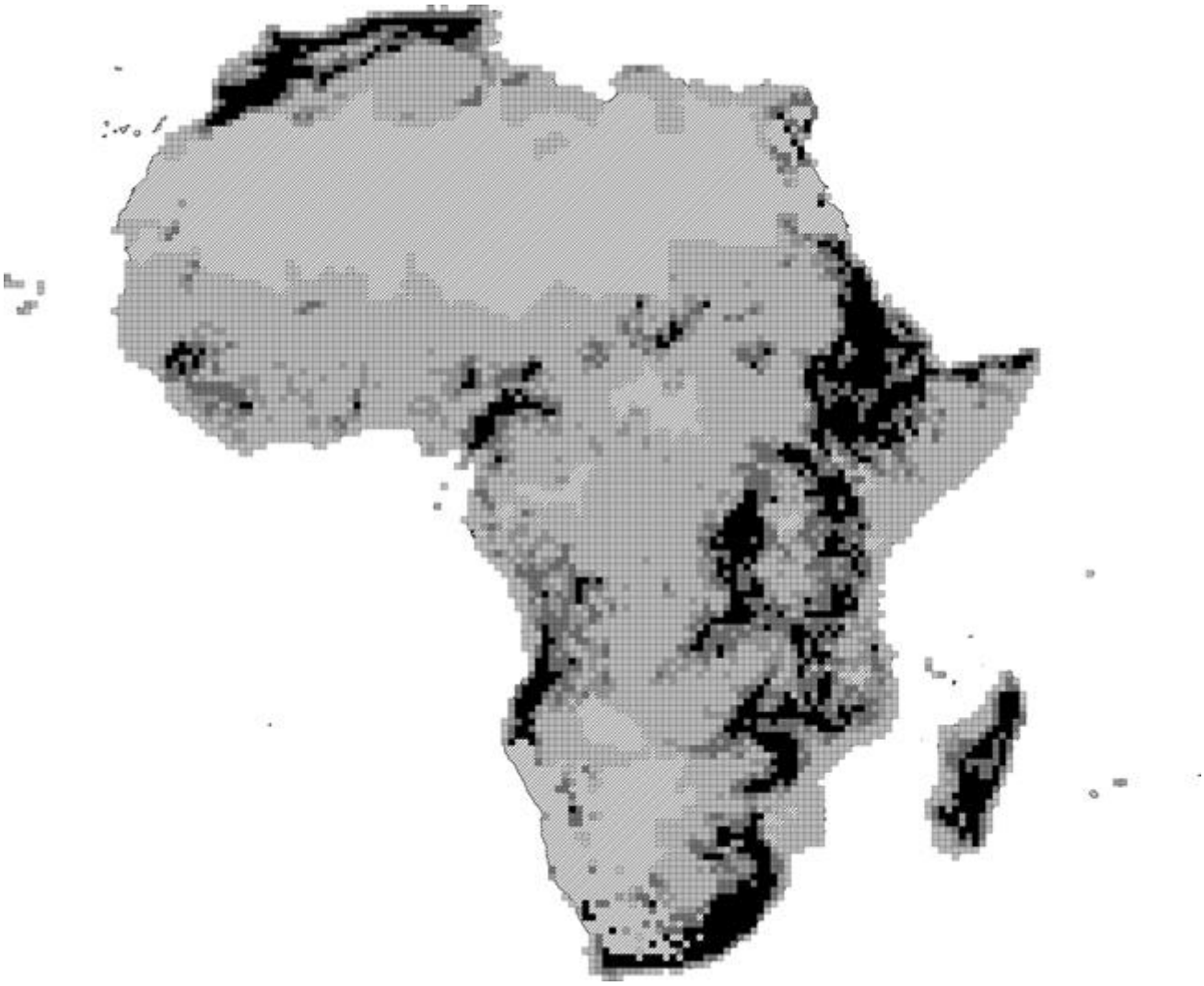


FIG. S3. Mountainous terrain

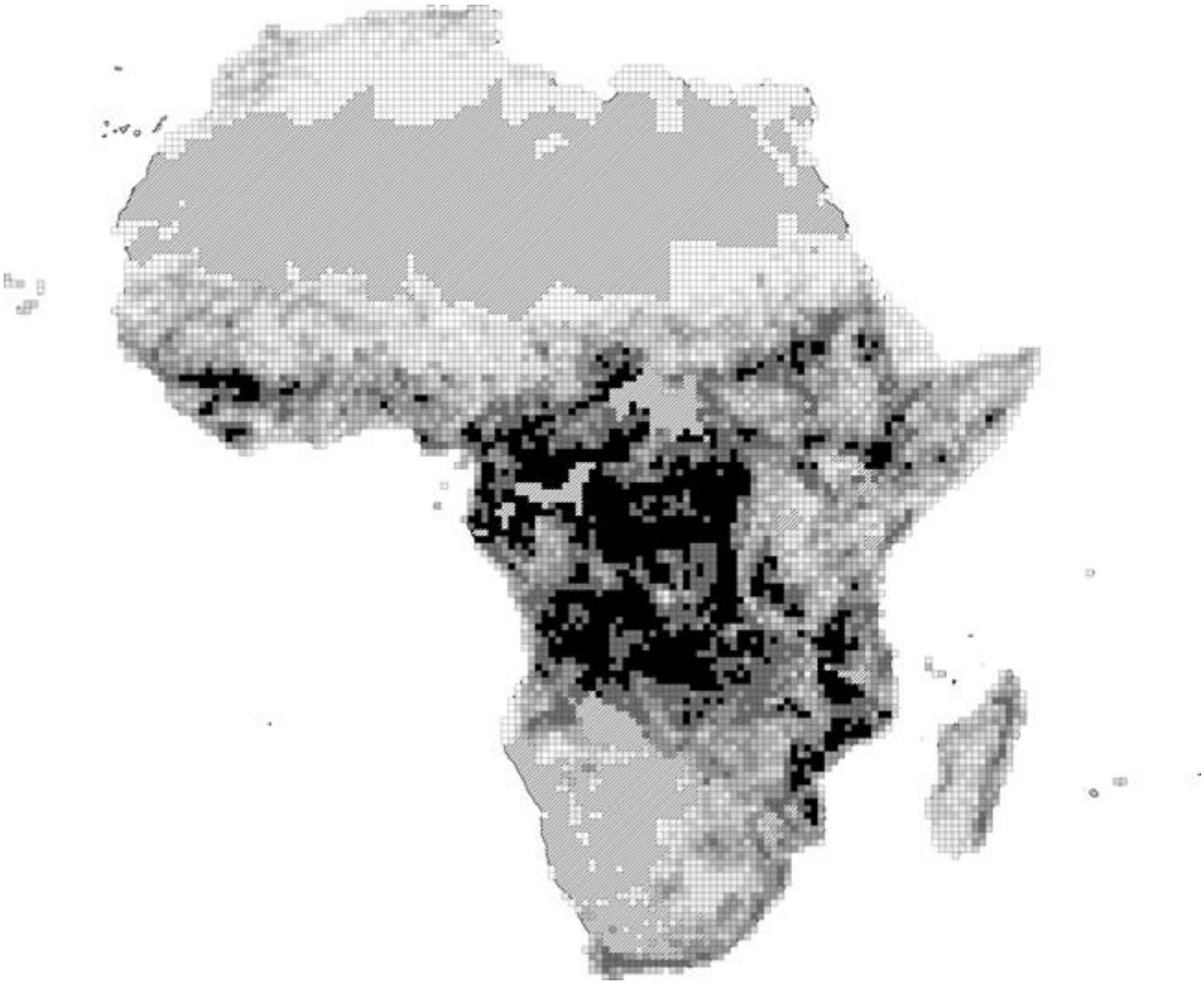


FIG. S4. Forested terrain

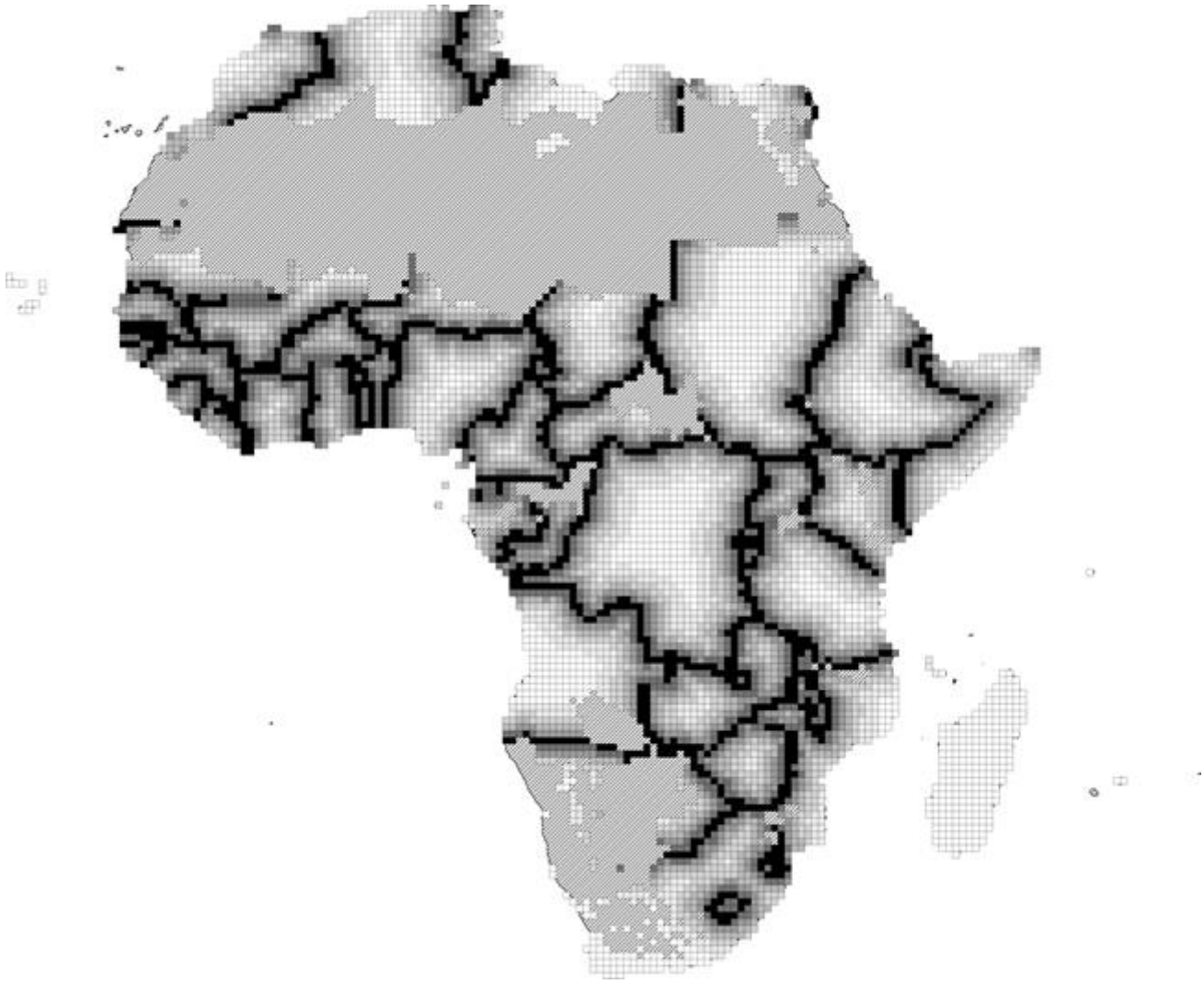


FIG. S5. Proximity to borders, 1989

TABLE S1. Descriptive statistics

	N	Mean	SE	Min	Max
GED events	7,465	0.30	0.76	0	6.78
Distance to capital	7,465	0.60	0.20	0	1
Mountains	7,465	0.24	0.34	0	1
Forest	7,465	0.57	0.35	0	1
Proximity to border	7,465	0.33	0.17	0	1
Excluded	7,465	0.14	0.20	0	1
Index a1	7,465	0.78	0.17	0.10	1
Index a2	7,465	0.38	0.13	0.02	0.83
Distance to city	7,465	5.90	0.67	2.77	8.28
Population	7,465	2.38	1.37	0.001	8.12
Income	7,465	7.06	0.97	5.46	9.64
Neighbor conflict	7,465	0.39	0.75	0	5.42

8 Appendix to Chapter 4

Andreas Forø Tollefsen

Appendix: Experienced Poverty and Local Conflict Violence

June 27, 2016

1 Description of this Appendix

This appendix complements the paper "Experienced Poverty and Local Conflict Violence". I present descriptive statistics for the main dependent and independent variables employed in the article. Also, using figures and plots, I show the distribution and correlation of variables. I proceed with some robustness tests, showing that the results presented in the paper are not caused by districts with a large number of conflict events, nor a low number of survey respondents.

2 Descriptive Statistics

Table 1 presents the descriptive statistics for the main variables emplyed in the paper measured at the district level. Similar descriptive statistics for variables aggregated to the region level is available in table 2.

Table 1: District level descriptive statistics

Statistic	N	Mean	St. Dev.	Min	Max
N Conflict events (3-years post-survey)	4,008	0.244	2.074	0	79
Number of respondents	4,008	23.652	29.013	1	362
Experienced Poverty	4,008	1.285	0.552	0.000	3.731
Conflict History (halflife)	4,008	0.049	0.145	0.000	0.707
Conflict Spatial Lag (t-1)	4,008	0.059	0.236	0	1
Population Sum	3,952	197,669.400	267,897.400	227.323	2,946,036.000
Distance to Capital	4,008	326.757	281.500	0.989	1,859.238
Distance to Border	4,008	131.140	141.274	0.246	1,451.540
Area sq.km	4,008	0.476	1.179	0.0001	28.884
Group Injustice (share)	4,008	0.244	0.283	0.000	1.000
Unemployment (share)	4,008	0.645	0.235	0.000	1.000
Institutional Quality	4,008	2.440	0.333	1.094	3.750

Table 2: Region level descriptive statistics used in fixed-effects model

Statistic	N	Mean	St. Dev.	Min	Max
N Conflict events (3-years post-survey)	1,159	2.096	15.052	0	446
Number of respondents	1,159	81.792	72.692	2	464
Experienced Poverty	1,159	1.302	0.487	0.020	2.975
Conflict History (halflife)	1,159	0.145	0.235	0.000	0.707
Conflict Spatial Lag (t-1)	1,159	0.289	0.454	0	1

3 Variation in Experienced Poverty

Figure 2 presents the frequency of response alternatives across the experienced poverty indicators for three survey rounds. While the largest share of responses come in the *never* category (except for cash income question), about half of the respondents have experienced failure to meet their most basic subsistence needs at one or more occasions. Most essential, more than 56 percentage of respondents in round 3 experienced food shortage on one or more occasions. The same number of round 4 was 57 percent, and 50 percent in round 5. 3-5 percent of the respondents always lacked food, which will have fundamental consequences for well-being. Also, more than two-thirds of the respondents had gone without a cash income at one or several occasions, and almost 20 percent had always gone without an income.

Figure 1: Histogram of Lived Poverty Index at district (left) and region (right) level

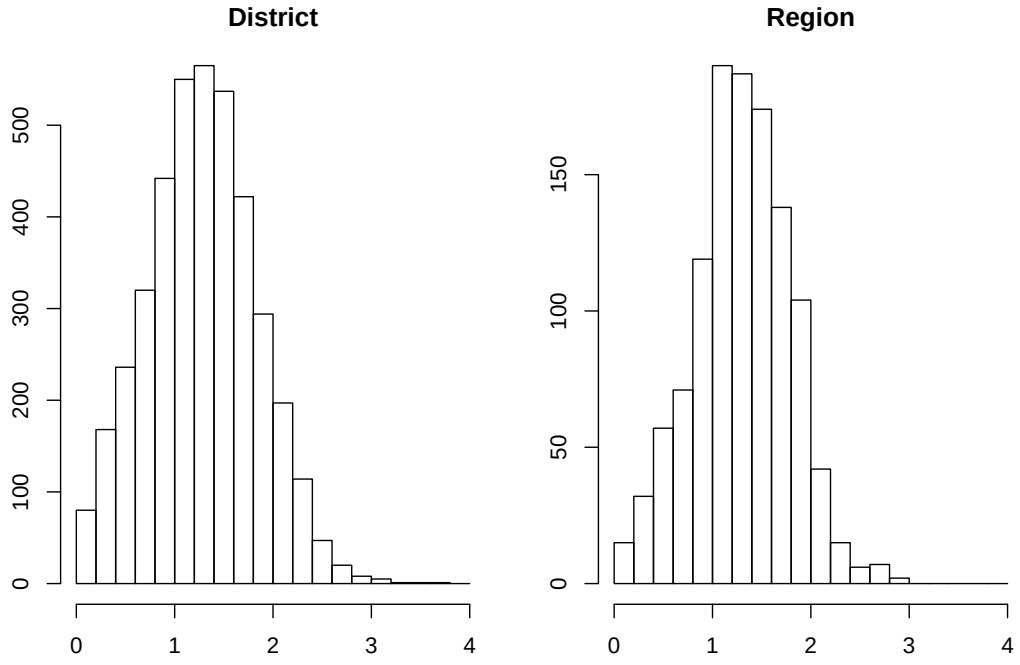
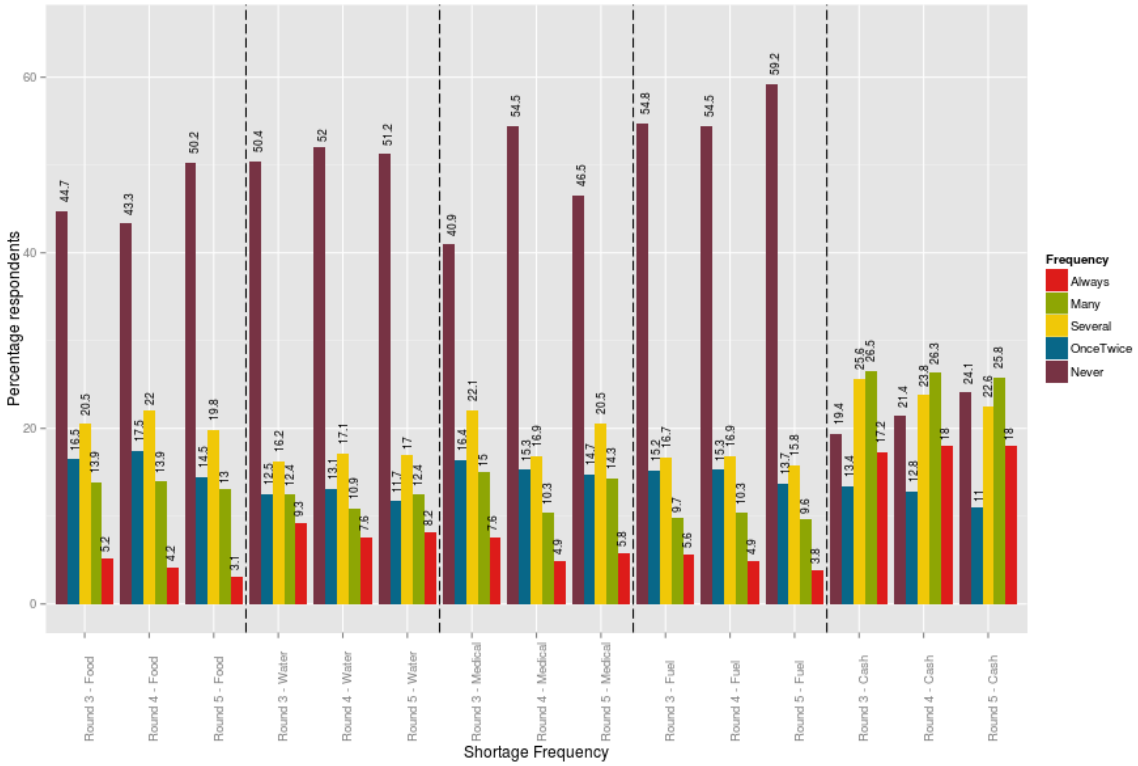


Figure 2: Frequency of shortage of basic needs for round 3, 4 and 5 in Afrobarometer



4 Number of districts in each round

Table 3 shows the number of districts included in each round. This number might differ from the full survey as these numbers only include georeferenced districts. From round 3 to round 5, the number of countries included in each survey round have increased, and in particular in round 5. Consequently, the number of districts included in each round have risen from 1094 in round 3, to 1777 in round 5.

Table 3: Number of districts in each country, by survey round

	Round 3	Round 4	Round 5
Algeria	0.00	0.00	86.00
Angola	2.00	0.00	0.00
Benin	67.00	64.00	36.00
Botswana	27.00	25.00	25.00
Burkina Faso	0.00	40.00	44.00
Burundi	0.00	0.00	71.00
Cte d'Ivoire	0.00	0.00	33.00
Cameroon	0.00	0.00	46.00
Cape Verde	0.00	0.00	13.00
Egypt	0.00	0.00	69.00
Ghana	93.00	99.00	115.00
Guinea	0.00	0.00	34.00
Kenya	74.00	61.00	113.00
Lesotho	9.00	9.00	8.00
Liberia	0.00	45.00	38.00
Madagascar	22.00	22.00	22.00
Malawi	72.00	36.00	54.00
Mali	46.00	44.00	32.00
Mauritania	0.00	1.00	0.00
Mauritius	0.00	0.00	10.00
Morocco	0.00	0.00	39.00
Mozambique	75.00	65.00	80.00
Namibia	76.00	70.00	61.00
Niger	0.00	0.00	34.00
Nigeria	186.00	226.00	150.00
Senegal	37.00	36.00	39.00
Sierra Leone	0.00	0.00	14.00
South Africa	49.00	51.00	52.00
Swaziland	0.00	0.00	40.00
Tanzania	73.00	74.00	99.00
Togo	1.00	1.00	21.00
Tunisia	0.00	0.00	89.00
Uganda	96.00	68.00	103.00
Zambia	54.00	50.00	50.00
Zimbabwe	35.00	50.00	57.00
Sum	1094.00	1137.00	1777.00

5 Factor loadings institutional quality

I create an index that measures the perceived quality of governance, or local institutional quality. This variables is constructed by combining variables revealing how the respondents perceive how well the local governments is in creating jobs (**Createjobs**), extent of corruption among local police (**LocPolCorrupt**), extent of corruption among local tax officials (**LocTaxCorrupt**), how well the local government is maintaining roads (**Localroads**), trust their local government councilor (**TrustLocal**), trust courts (**TrustCourts**), and whether the respondent approve or disapprove the performance of their local government councilor (**PerfLocPol**).

Table 4 shows the factor loadings used to create the institutional quality index. The loadings clearly show that the variables used to create the measure of local institutional quality shares a common dimension. It is also evident that the variables about socio-economic well-being make up a distinct dimension.

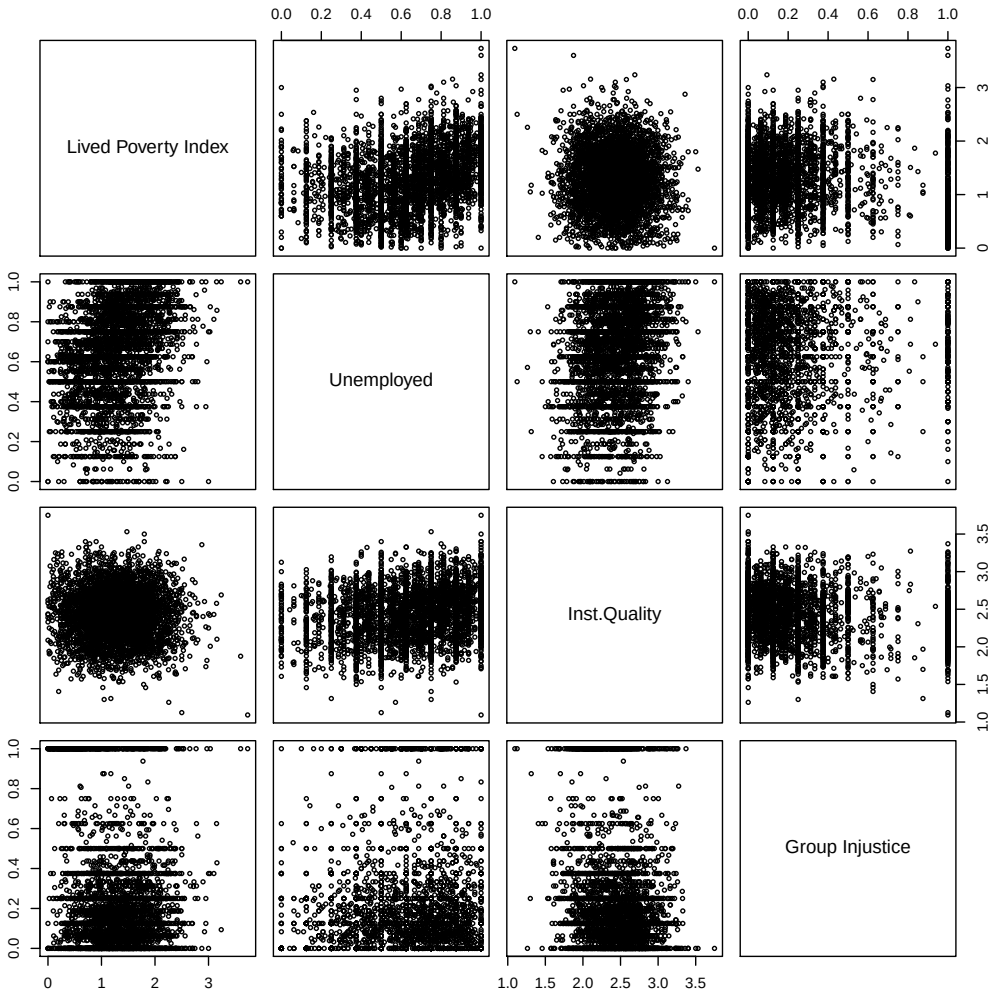
Table 4: Factor loadings

	Factor1	Factor2
Createjobs	**0.50**	0.45
LocGovCorrupt	**0.83**	-0.05
LocPolCorrupt	**0.74**	0.04
LocTaxCorrupt	**0.66**	0.06
Localroads	**0.45**	0.27
TrustLocal	**0.75**	-0.11
TrustCourts	**0.71**	-0.07
PerfLocPol	**0.60**	0.03
Meanunfair	-0.48	-0.18
MeanOwnCond	0.06	0.81
MeanOwnCondRel	-0.04	0.89
MeanLPI	0.01	-0.43

6 Scatterplot of key variables

Figure 3 shows a scatterplot between the Lived Poverty Index, unemployment, institutional quality and levels of perceived group injustice.

Figure 3: District scatterplot between experienced poverty, unemployment, perceived institutional quality and perceived group injustice)



7 Interaction model regression results

Table 5 shows the result of interaction regression models used to create the interaction plots in the main paper.

Table 5: Results from Negative Binomial Interaction Models

	Dependent variable:		
	Number of Conflict Events		
	(1)	(2)	(3)
Experienced Poverty	1.507	5.142***	0.561
	(1.289)	(1.009)	(0.434)
Unemployed	0.180		
	(2.639)		
Local Institutional Quality		0.225	
		(0.579)	
Group Injustice			−1.289
			(0.818)
Conflict History	5.639***	5.272***	5.816***
	(0.604)	(0.455)	(0.712)
Conflict Spatial Lag (t-1)	0.666	0.891	1.037
	(1.089)	(0.667)	(1.190)
Population (logged)	0.405	0.360	0.394
	(0.367)	(0.232)	(0.356)
Distance to Capital (logged)	0.059	0.152	0.094
	(0.232)	(0.164)	(0.230)
Distance to Border (logged)	0.386	0.284	0.371*
	(0.293)	(0.189)	(0.218)
Area sq.km (logged)	−0.269*	−0.177*	−0.316**
	(0.139)	(0.107)	(0.151)
Round 4 dummy	−0.020	−0.009	0.165
	(0.436)	(0.287)	(0.409)
Round 5 dummy	−0.439	−0.256	−0.312
	(0.590)	(0.402)	(0.522)
Experienced Poverty:Unemployed	−0.560		
	(2.135)		
Experienced Poverty:Local Institutional Quality		−1.890***	
		(0.393)	
Experienced Poverty:Group Injustice			1.654***
			(0.636)
Constant	−11.159**	−10.539***	−10.752***
	(5.145)	(3.493)	(4.015)
BIC	2,698.31	2,613.283	2,686.381
Observations	3,952	3,952	3,952
Overdispersion (θ)	0.051*** (0.005)	0.062*** (0.006)	0.052*** (0.005)

Robust Standard Errors in Parentheses. *p<0.1; **p<0.05; ***p<0.01

8 OLS models

Table 6 shows OLS models where the outcome is the logged number of conflict events in a district.

Table 6: OLS Model results, replicating the results in table 2

	Dependent variable:				
	log(Number of Conflict Events)				
	(1)	(2)	(3)	(4)	(5)
Experienced Poverty		0.038***			
		(0.010)			
Relative Experienced Poverty			0.039***		
			(0.012)		
High Experienced Poverty				0.050**	
				(0.021)	
Low Experienced Poverty					−0.050**
					(0.021)
Conflict History	0.794***	0.790***	0.792***	0.793***	0.793***
	(0.037)	(0.037)	(0.037)	(0.037)	(0.037)
Conflict Spatial Lag (t-1)	0.060***	0.050**	0.058**	0.055**	0.055**
	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)
Population (logged)	0.016***	0.016***	0.017***	0.016***	0.016***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Distance to Capital (logged)	0.013**	0.014***	0.012**	0.013**	0.013**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Distance to Border (logged)	0.006	0.011**	0.007	0.009*	0.009*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Area sq.km (logged)	−0.010***	−0.012***	−0.011***	−0.011***	−0.011***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Round 4 dummy	−0.010	−0.009	−0.011	−0.010	−0.010
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
Round 5 dummy	−0.027**	−0.023*	−0.027**	−0.025**	−0.025**
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Constant	−0.252***	−0.332***	−0.269***	−0.302***	−0.252***
	(0.072)	(0.075)	(0.072)	(0.075)	(0.072)
Observations	3,952	3,952	3,952	3,952	3,952
R^2	0.130	0.133	0.132	0.131	0.131
Adjusted R^2	0.128	0.131	0.130	0.129	0.129
Residual Std. Error	0.322 (df = 3943)	0.321 (df = 3942)	0.321 (df = 3942)	0.322 (df = 3942)	0.322 (df = 3942)
F Statistic	73.690*** (df = 8; 3943)	67.369*** (df = 9; 3942)	66.870*** (df = 9; 3942)	66.212*** (df = 9; 3942)	66.198*** (df = 9; 3942)
Note:					*p<0.1; **p<0.05; ***p<0.01

9 Hurdle model

Clearly, there is a difference between having *peace/conflict*, or *conflict/more conflict*. The results presented in the main paper shows that poverty increases the intensity of conflict violence. However, it does no distinguish between having no conflict versus having one or more conflicts. This is the attractive feature of the hurdle model (Zeileis et al., 2007), that distinguishes between the 0 or 1 process using a logit model, and the 1 or more process using a negative binomial model. Table 7 presents the results of a hurdle model, replicating model 2 in Table 2 in the main paper.

The results show that experienced poverty is positively related with having more conflict, given that it already have a conflict. While this is in line with the key findings, the results of the "hurdle" process also shows that poverty seems to increase to risk of experiencing any conflict as well (p=0.058).

Table 7: Hurdle model results

	Negative Binomial Model
Count model: (Intercept)	−15.509
	(213.157)
Count model: Experienced Poverty	1.277***
	(0.300)
Count model: Conflict History	1.616***
	(0.526)
Count model: Conflict Spatial Lag (t-1)	0.276
	(0.402)
Count model: Population (logged)	0.178
	(0.164)
Count model: Distance to Capital (logged)	0.014
	(0.127)
Count model: Distance to Border (logged)	0.004
	(0.172)
Count model: Area sq.km (logged)	−0.022
	(0.092)
Count model: Round 4 dummy	0.235
	(0.376)
Count model: Round 5 dummy	0.798**
	(0.353)
Count model: Log(theta)	−12.603
	(213.140)
	Hurdle model
Zero model: (Intercept)	−9.749***
	(1.155)
Zero model: Experienced Poverty	0.252*
	(0.133)
Zero model: Conflict History	4.196***
	(0.306)
Zero model: Conflict Spatial Lag (t-1)	0.296
	(0.239)
Zero model: Population (logged)	0.322***
	(0.077)
Zero model: Distance to Capital (logged)	0.195***
	(0.072)
Zero model: Distance to Border (logged)	0.223***
	(0.077)
Zero model: Area sq.km (logged)	−0.229***
	(0.046)
Zero model: Round 4 dummy	0.031
	(0.182)
Zero model: Round 5 dummy	−0.479***
	(0.181)
AIC	2460.460
Log Likelihood	-1209.230
Num. obs.	3952

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

10 Removing outliers

The descriptive statistic table presented above shows that the number of conflict events in a district ranges from 0 to 79. To alleviate concerns that the results are driven by districts with a very high number of events, I replicate model 2 from table 2 in the main table and exclude districts with more than 20 and 10 conflict events. Table 8 presents the results of the negative binomial model. The results do not alter the conclusions made in the paper.

Table 8: Models excluding outliers with high number of events

	Dependent variable:	
	Number of Conflict Events	
	(Under 20 events)	(Under 10 events)
Experienced Poverty	1.220***	0.587***
	(0.170)	(0.161)
Conflict History	4.702***	4.269***
	(0.534)	(0.481)
Conflict Spatial Lag (t-1)	0.390	0.680**
	(0.357)	(0.316)
Population (logged)	0.302***	0.408***
	(0.092)	(0.090)
Distance to Capital (logged)	0.468***	0.365***
	(0.089)	(0.092)
Distance to Border (logged)	0.265***	0.414***
	(0.091)	(0.090)
Area sq.km (logged)	−0.164***	−0.176***
	(0.059)	(0.055)
Round 4 dummy	−0.075	−0.104
	(0.228)	(0.216)
Round 5 dummy	−0.051	−0.524**
	(0.214)	(0.208)
Constant	−11.598***	−12.259***
	(1.400)	(1.378)
Observations	3,945	3,935
Log Likelihood	−1,225.429	−1,121.255
θ	0.053*** (0.005)	0.073*** (0.008)
Akaike Inf. Crit.	2,470.859	2,262.510
Note:		*p<0.1; **p<0.05; ***p<0.01

11 Removing districts with few respondents

Another concern is that the results are biased due to few respondents, leading to low internal representativeness. Thus, in table 9 I replicate model 2 in Table 2 in the main paper, but exclude districts with less than 20 and 50 respondents. The results alleviate any concern of internal representativeness and make the regression coefficients stronger.

Table 9: Models excluding districts with a low number of respondents

	Dependent variable:	
	Number of Conflict Events	
	(Over 20 respondents only)	(Over 50 respondents only)
Experienced Poverty	1.589***	2.778***
	(0.316)	(0.738)
Conflict History	5.379***	7.583***
	(0.921)	(1.846)
Conflict Spatial Lag (t-1)	0.704	−3.586**
	(0.550)	(1.655)
Population (logged)	0.394***	−0.125
	(0.148)	(0.286)
Distance to Capital (logged)	0.447***	0.148
	(0.138)	(0.227)
Distance to Border (logged)	0.260*	0.797***
	(0.149)	(0.298)
Area sq.km (logged)	−0.329***	−0.079
	(0.098)	(0.204)
Round 4 dummy	−0.723*	−0.004
	(0.431)	(0.867)
Round 5 dummy	−0.134	−0.008
	(0.364)	(0.779)
Constant	−13.252***	−9.314**
	(2.346)	(4.620)
Observations	1,469	328
Log Likelihood	−471.589	−119.152
θ	0.052*** (0.008)	0.068*** (0.020)
Akaike Inf. Crit.	963.178	258.304
Note:		*p<0.1; **p<0.05; ***p<0.01

References

Zeileis, A., C. Kleiber, and S. Jackman (2007). Regression models for count data in r. In *Research Report Series / Department of Statistics and Mathematics, 53*, Vienna. Department of Statistics and Mathematics, WU Vienna University of Economics and Business.

9 Appendix to Chapter 5

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Appendix to *Local Institutional Quality and Conflict Violence in Africa*

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1 Overview

This appendix complements the paper "Local Institutional Quality and Conflict Violence in Africa". The appendix elaborates on the georeferencing of survey data, variable coding and operationalizations. It also presents a range of robustness tests, along with descriptive statistics for the variables used in the empirical analysis.

It proceeds as follows. Section 2 describes the georeferencing of the Afrobarometer survey data, and how these data are combined with the spatial conflict data. Section 3 describes how the variables used in the paper are operationalized, as well as presenting factor loadings from the factor analysis used to create the *Local Institutional Quality* measure. In this section, we also present a simple validation test, correlating the country-level mean of *Local Institutional Quality* with country-level data on institutional quality from WGI. Section 4 provides descriptive information for the main variables used in the analysis.

Section 5 presents diagnostics for the matching procedure, while section 6 presents a number of additional robustness tests, e.g alternative functional forms, alternative dependent variable (from ACLED), removing districts with few respondents, and removing districts with extreme numbers of violence events. Here, we also present investigations of the individual items composing the *Local Institutional Quality* index, where we see whether each item is (individually) associated with conflict violence. These exercises show that the main finding is robust, with the caveat that the results for round 4 are less robust than for round 3. This difference can most likely be ascribed to fewer conflict events (a consequence of less post-survey conflict years) following round 4, than after round 3. Overall, we believe these additional tests lend more credence to the claim that local institutional quality pacifies.

2 Creating the dataset

Combining non-spatial survey data with spatial conflict data requires a common unit of analysis. Hence, as noted in the paper, we use the district identifier as stated by each respondent in the Afrobarometer data to find a native unit of analysis to attach the conflict data to. Others have obtained the lower-level sampling geographies (i.e. village level, or neighbourhood) to georeference the survey data (e.g. Nunn, 2008). We have not chosen this approach. The reason is that there are often not enough respondents in each sampling cluster to say anything about the institutional quality in the clusters. By using the district variable in the Afrobarometer, we were able to identify the corresponding sub-national administrative polygons, taken from the Global Administrative Areas (GADM) data set. This article utilizes the GADM data set, which contains polygons for all sub-national regions and districts in the world. In this framework, identifying the corresponding GADM district polygon for each Afrobarometer district name should be a trivial task. However, a number of immediate issues with matching using string comparison arises from the fact that there are inevitable variations in naming conventions. These discrepancies range from differences in the use of upper- and lower-case letters to more significant spelling differences of district names with or without accented letters as well as whitespace issues.

To handle such differences in naming conventions when merging the two datasets, we employ the Jaro-Winkler string comparison algorithm (Winkler, 1999) where the similarity of two strings are calculated and can match in the range of 0 (no similarity) to 1 (equality). We show the usefulness of fuzzy string comparison matching by merging district names in round three and four of the Afrobarometer to the best matching district counterparts in the GADM dataset, within each country. However, uncertainty must be minimized, reducing the risk of merging a district name to a non-identical district polygon.

Whenever the Jaro-Winkler identifies a polygon at level 1 or 2 in the GADM matching the Afrobarometer respondent's district above *.9* similarity, that polygon is used to represent that Afrobarometer district. Districts in the Afrobarometer with no match above *.9* is ignored and consequently excluded from further analysis [1] We adopt the above matching procedure for round 3 and 4 of the Afrobarometer surveys, resulting in *83* % matches in round 3 and *80* % matches in round 4. The end result is a set of district polygons representing the district of each respondent in the Afrobarometer.

Next, we aggregate survey answers for respondents belonging to the same districts. Where most questions are answered using the likert scale, we convert this to a simple numeric scale with equal number of alternatives as the likert, assuming equal distance between each point (i.e. 1-5 for 5 point likert). This makes most sense as most questions are answered by ranked ordinal scale options, such as *Never, Just once or twice, Several times, Many Times and Always.* The underlying distance between

[1] A number of alternative matching cut-off thresholds were tested. However, *.9* is a sound trade-off to exclude ambiguous matches and include clear cases of matches.

individual responses therefore remains latent. For count variables such as the number of respondents in a district, we use the absolute count.

Once the Afrobarometer districts were georeferenced using the GADM polygons, we were able to spatially merge the UCDP GED conflict events to each district. Using R we applied count functions to calculate the number of conflict events in the post-survey period, intersecting these events with the district polygons. This count variable is then used as our outcome variable in the further analysis.

3 Descriptions of main variables

3.1 Afrobarometer variables

All variables from the Afrobarometer are aggregated to the district by taking the mean of the respondents answers in a district. The variables described below are found in both rounds 3 and 4, except for army and police presence, which are only found in round 3. The list of variables below describes each variable along with the alternative response levels to each corresponding Afrobarometer question.

- **Local institutional quality:** Based on a factor analysis, this variable is a mean-weighted additive index of the following five items:
  - **Trust in local politicians:** This item asks how much trust the respondent has in his/her elected government council. The responses range from 0 to 3, where 0 is "Not at all" and 3 is "A lot"
  - **Trust in courts:** Same as trust in politicians, applied to courts
  - **Corruption of local politicians:** This is based on a question of the following form: "How many of the following people do you think are involved in corruption, or haven't you heard enough about them to say: Elected Assembly men/women?". The answers are 0=None, 1=Some of them, 2=Most of them, 3=All of them.
  - **Corruption of police:** Same as corruption of local politicians, applied to police.
  - **Performance of local politicians:** This takes the following form: "Do you approve or disapprove of the way the following people have performed their jobs over the past twelve months, or haven't you heard enough about them to say: Your Elected Assembly man/woman?", and where the possible answers are 1=Strongly disapprove, 2=disapprove, 3=Approve, 4=Strongly approve."
  - **Attended community meeting:** This is based on a question taking the following form: "Here is a list of actions that people sometimes take as citizens. For each of these, please tell

me whether you, personally, have done any of these things during the past year. If not, would you do this if you had the chance: Attended a community meeting?." This variable is scored as 1 if the respondent answers "yes" and zero otherwise.

3.2 Baseline controls

This section describes the operationalization of the control variables.

- **Support for president:** Based on the question "Do you approve or disapprove of the way the following people have performed their jobs over the past twelve months, or haven't you heard enough about them to say: The President." It has the following responses: 1 =Strongly disapprove, 2 =disapprove, 3 =Approve, 4 =Strongly approve.
- **Lived poverty index (LPI):** The average value of responses to questions about whether the respondent has gone without access to food, water, cooking fuel, medical care and a cash income, where the possible responses are: 0 =Never, 1 =Just once or twice, 2 =Several times, 3 =Many times, 4 =Always.
- **L(infant mortality rate):** Log of the average infant mortality rate in a district, mapped with data from the SEDAC Global Poverty Mapping project (Storeygard, Balk, Levy and Deane, 2008).
- **L(area):** This is simply the log of area of the district (measured in square kilometers)
- **L(travel time):** This is operationalized as the logged mean travel time in a district (by car) to the nearest city with more than 50 000 inhabitants. The variable is calculated by taking the district-level mean. Taken from PRIO-GRID (Tollefsen, Strand and Buhaug, 2012)
- **L(young men):** This captures the log of a districts male population aged between the 15 and 20. The data is taken from Tatem, Garcia, Snow, Noor, Gaughan, Gilbert and Linard (2013).
- **L(population):** This is taken from the Gridded Population of the World database (Tobler, Deichmann, Gottsegen and Maloy, 1995), and takes the log of the population in a district.

3.3 Factor analysis for local institutional quality

This section presents the factor analysis performed to derive the index of local institutional quality. In addition to the variables described above, the factor analysis includes the following additional variables: Trust in neighbors (**soctrusnei**), trust in other countrymen (**soctrusnat**), most people can be trusted (**soctrusmpeop**), trust relatives (**soctrusrel**), ever paid a bribe for different services (**paybribe** 1-5), ever gotten together with others to raise a political issue (**raiseissue**), attended

protest march (**protestmarch**), contacted government official (**contactlocgov**), post office in respondents area (**postoffice**), police station in respondents area (**policestation**), health clinic in respondents area (**hclinic**), community building in respondents area (**cbuilding**), seen police in respondents area (**seenpol**), seen army in respondents area (**seenarmy**), lived poverty (whether respondent has gone without various basic necessities in the past year, **lpi1-lpi5**), education of respondent (**educ**), whether respondent is employed (**employed**). Together these variables are included in the factor analysis.

Table 1 shows the factor loadings for these indicators when we allow for the extraction of four factors. The table clearly shows that the *Local Institutional Quality* index (described above), constitutes a single dimension. We have experimented with the number of factors to extract, and the *Local Institutional Quality* dimension (Factor 1 in the table below) always comes out as a separate factor. The factor analysis is only done for round 3 below (since this is the round where we have most of the additional variables mentioned above, like **seenarmy**, **seenpolice** and similar variables), but the same analysis on round 4 yields comparable results. The analysis indicates that there are three additional dimensions, with high intuitive validity: The column showing loadings for Factor 2 shows that all of the Lived poverty Index questions load strongly on the same dimension, which is unsurprising. Factor 3 seems to capture the variance in experience with corruption over the past year (whether the respondent has payed a bribe for various services). These also load strongly on the same factor as *Local Institutional Quality* variables when we restrict the number of factors to extract. The final factor, Factor 4 seems to capture elements of local social trust: Whether the respondent trusts neighbours, people of the same nationality, and relatives.

Table 1: Factor loadings from a factor analysis with 4 factors

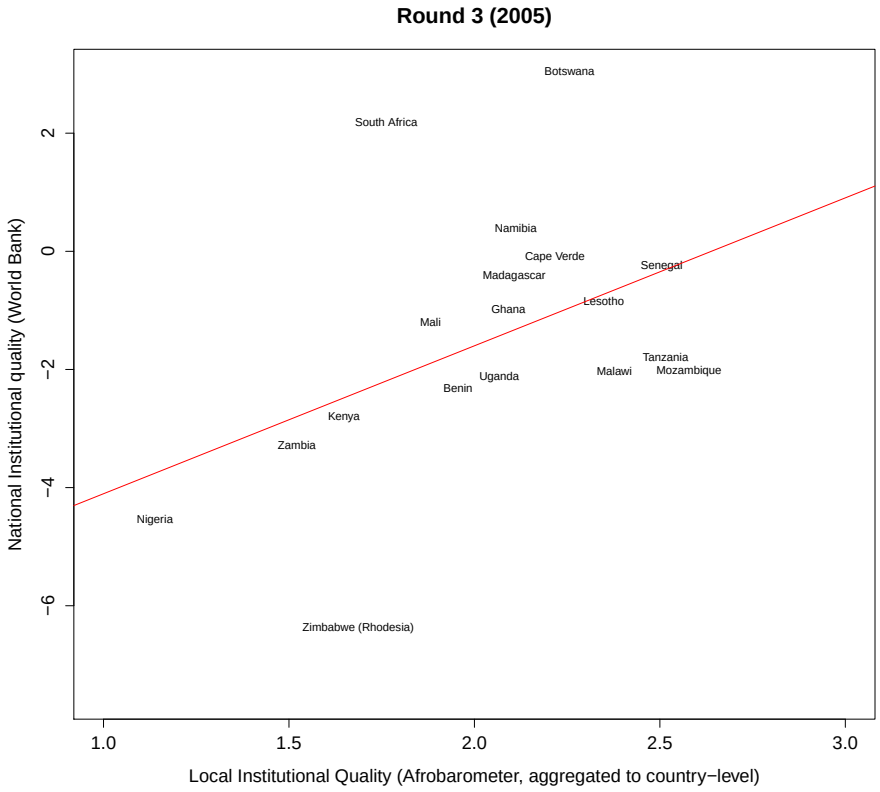
	Factor 1	Factor 2	Factor 3	Factor 4
trustloc	**0.76**	0.07	-0.08	0.32
performanceloc	**0.69**	0.03	0.01	0.27
corruptionloc	**0.79**	0.00	-0.26	0.18
trustcops	**0.79**	0.11	-0.22	0.33
corruptioncops	**0.69**	0.09	-0.31	0.20
corruptioncourts	**0.56**	-0.01	-0.30	0.11
atcomeet	**0.63**	0.16	-0.08	0.04
soctrusnei	0.22	0.23	-0.09	**0.88**
soctrusnat	0.32	0.21	-0.13	**0.70**
soctrusmpeop	0.10	0.01	0.16	0.36
soctrusrel	0.19	0.07	-0.10	**0.82**
paybribe	-0.08	0.04	**0.70**	-0.07
paybribe2	-0.08	0.01	**0.72**	-0.05
paybribe3	-0.22	-0.06	**0.71**	0.00
paybribe4	0.03	0.16	**0.74**	-0.03
paybribe5	-0.23	-0.03	**0.71**	0.03
raiseissue	0.43	0.02	0.15	-0.05
protestmarch	0.03	-0.18	0.24	-0.03
contactlocgov	0.25	0.16	0.14	0.02
postoffice	-0.20	-0.36	0.03	-0.07
policestation	-0.19	-0.29	0.13	-0.12
hclinic	-0.12	-0.20	0.10	0.00
cbuilding	-0.10	-0.26	0.06	-0.01
seenpol	-0.28	-0.29	0.17	-0.14
seenarmy	-0.00	0.03	0.01	-0.03
lpi1	-0.02	**0.77**	-0.03	0.08
lpi2	0.02	**0.64**	0.14	0.07
lpi3	-0.02	**0.89**	0.10	0.14
lpi4	-0.22	**0.67**	0.14	0.00
lpi5	0.28	**0.78**	0.01	0.11
educ	-0.50	-0.42	0.18	-0.21
employed	-0.13	-0.26	0.21	-0.24

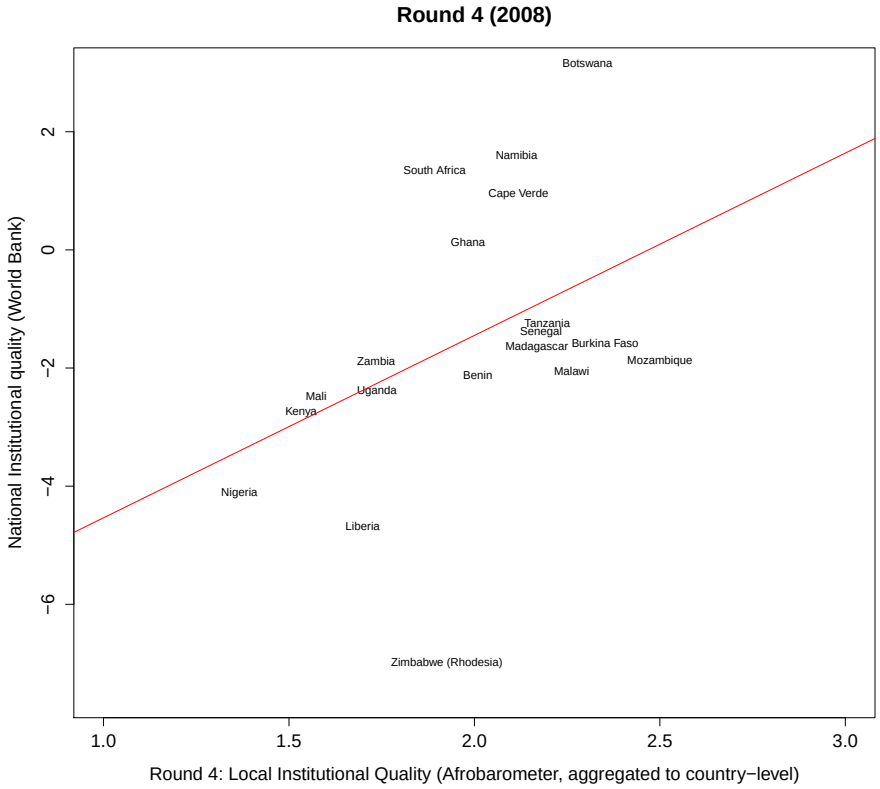
(a) Loadings above .5 are highlighted

3.4 Correlation with national-level measures of governance quality

As a validity check, we correlated the mean country score on the *Local Institutional Quality* index with an additive country-level index consisting of the following World Governance Indicators (WGI): Control of corruption, government effectiveness, regulatory quality and rule of law. Although the *Local Institutional Quality* variable explicitly refers to *local* political institutions in the wording, while the world governance indicators are intended to capture *national* institutions, we expect there to be some positive correlation between *Local Institutional Quality* aggregated, and the index created by using the World Bank indicators of good governance. This is simply because good national-level institutions should conduce high-quality local institutions and vice versa. We find that there is indeed such a correlation, although the association is not as strong as might be expected. In round 3, the correlation between the two is .426 and it is weakly significant (P-value = 0.07). In round 4 the correlation is very similar (.417) and weakly significant (P-value = 0.07). Figure 1 below, shows a scatterplot of the WGI institutional quality index and the mean *Local Institutional Quality* index for both rounds. Although the plots show a clear relationship, a number of countries that are not on the diagonal line are interesting: South Africa and Botswana for example, both have a high score on the WGI index, but a below average score on the *Local Institutional Quality* aggregated index. This might indicate that the quality of national institutions is somewhat higher in those countries than the quality of local political institutions.

Figure 1: National Institutional Quality (World Bank) and mean Local Institutional Quality aggregated to country level





4 Descriptive statistics

Table 2 and 3 presents the descriptive statistics for the primary variables included in the analysis, while figure 2 provides histograms for the main independent variable, Local Institutional Quality. Variables denoted with L are log transformed.

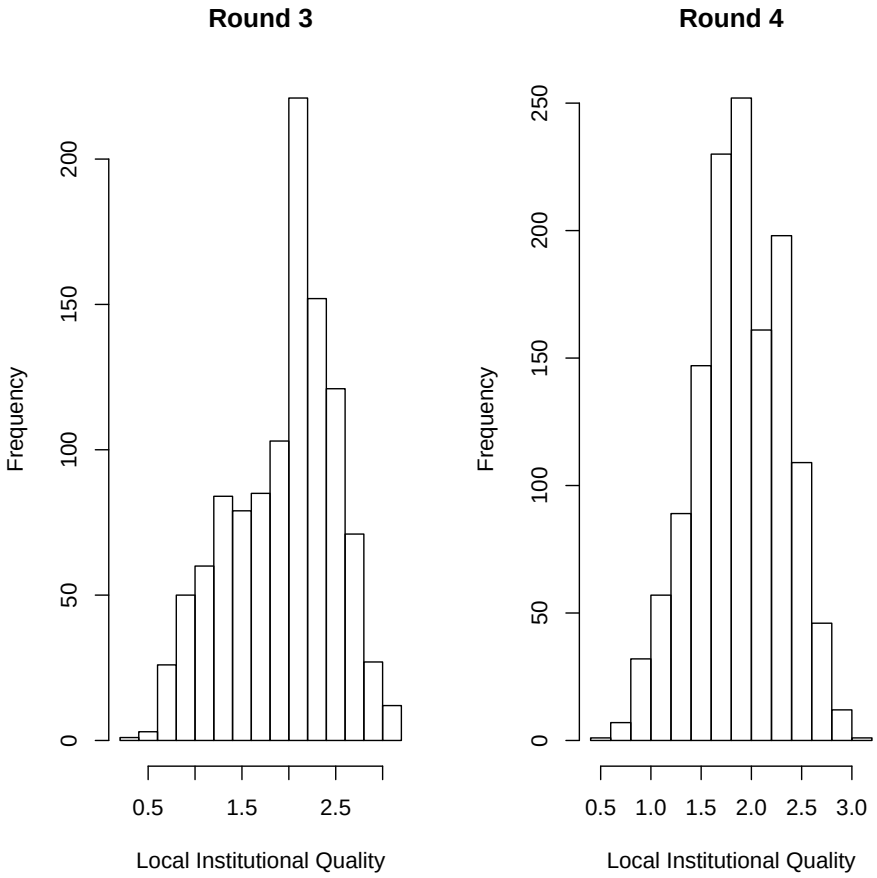
Table 2: Descriptive statistics (Round 3)

	Mean	Std.dev	Min	Maxs
Conflict events (GED)	0.418	2.780	0.000	50.000
Local Institutional Quality	1.936	0.565	0.357	3.179
L(capitol distance)	5.576	0.874	1.618	7.461
L(border distance)	4.081	1.046	-1.890	5.934
Army presence	0.077	0.208	0.000	1.000
Police presence	0.246	0.348	0.000	1.000
Social trust	1.875	0.433	0.458	2.958
Education	2.280	1.046	0.000	6.500
Employment	0.604	0.446	0.000	2.000
Support for president	2.503	0.929	0.000	4.000
LPI	1.385	0.535	0.000	3.150
L(infant mortality rate)	6.765	0.405	4.605	7.399
L(population)	11.737	1.104	5.814	14.863
L(young men)	8.816	1.188	0.001	12.034
L(area)	7.691	1.393	3.573	12.713
L(travel time)	5.453	0.726	2.002	7.934
Conflict events (half-life)	0.036	0.135	0.000	0.707
Conflict events (spatial lag)	0.410	1.689	0.000	18.000

Table 3: Descriptive statistics (Round 4)

	Mean	Std.dev	Min	Max
Conflict events	0.226	2.044	0.000	58.000
Local Institutional Quality	1.882	0.450	0.571	3.066
L(capitol distance)	5.603	0.840	1.128	7.528
L(border distance)	4.080	1.000	-0.596	5.942
Education	2.528	1.098	0.000	6.875
Employed	0.592	0.404	0.000	2.000
Social trust	1.838	0.423	0.467	3.000
Support for president	2.581	0.779	0.000	4.000
LPI	1.254	0.534	0.000	2.700
L(infant mortality rate)	6.742	0.425	4.605	7.399
L(population)	11.679	1.059	7.383	14.863
L(young men)	8.786	1.101	3.045	12.034
L(area)	7.550	1.320	2.452	12.713
L(travel time)	5.379	0.737	1.498	7.934
Conflict events (half-life)	0.031	0.123	0.000	0.707
Conflict events (spatial lag)	0.230	1.385	0.000	29.500

Figure 2: Distribution of Local Institutional Quality (rounds 3 and 4)



5 Matching diagnostics

This section displays balance statistics for the variables used in the matching procedure. They show the balance between treated and untreated cases prior to- and after matching. Table 4 shows pre- and post-matching balance for round 3. It shows that the balance between treated and untreated cases with respect to their conflict history is substantially improved. For example, the difference between treated and untreated cases in the pre-matching sample for past conflict events was -0.1790, while in the post-matched sample the difference is 0.06. For the other variables (Past conflict (dummy) and Past conflict (half-life) the difference is almost completely removed in the post-matching sample. The same pattern also emerges for round 4, where the improvement in balance between pre- and post-matched samples is even greater (see table 5).

Table 4: Covariate balance (pre- and post matching), round 3

Pre-matching	Means Treated	Means Control	SD Control	Mean Diff
Distance	0.5405	0.4930	0.1380	0.0475
Past conflict events	0.0897	1.0280	7.6198	-0.9382
Past conflict (dummy)	0.0275	0.1227	0.3283	-0.0952
Past conflict events (half-life)	0.0092	0.0529	0.1601	-0.0437
Conflict events (spatial lag)	0.0592	0.4150	1.9506	-0.3559
Post-matching	Means Treated	Means Control	SD Control	Mean Diff
Distance	0.5413	0.5357	0.0603	0.0056
Past conflict events	0.0870	0.0804	0.6389	0.0066
Past conflict (dummy)	0.0261	0.0261	0.1595	-0.0000
Past conflict events (half-life)	0.0087	0.0087	0.0596	0.0000
Conflict events (spatial lag)	0.0510	0.1002	0.3647	-0.0492

Table 5: Covariate balance (pre- and post matching), round 4

Pre-matching	Means Treated	Means Control	SD Control	Mean Diff
Distance	0.5955	0.5876	0.0475	0.0079
Past conflict events	0.3953	0.5743	4.9207	-0.1790
Past conflict (dummy)	0.0667	0.0923	0.2898	-0.0257
Past conflict events (half-life)	0.0282	0.0450	0.1545	-0.0168
Conflict events (spatial lag)	0.3050	0.5423	1.8644	-0.2372
Post-matching	Means Treated	Means Control	SD Control	Mean Diff
Distance	0.6011	0.6001	0.0210	0.0011
Past conflict events	0.1170	0.0770	0.5596	0.0400
Past conflict (dummy)	0.0385	0.0385	0.1925	0.0000
Past conflict events (half-life)	0.0121	0.0121	0.0665	-0.0000
Conflict events (spatial lag)	0.1390	0.1889	0.9514	-0.0499

6 Sensitivity tests

6.1 Poisson models

This section presents an alternative modeling strategy that diverges from the assumptions inherent in the negative binomial model. We fit a series of poisson models, embedding the assumption that the counts follow a poisson distribution. The results from this exercise can be seen in table 6 showing that the results are retained and substantively unaltered by the use of a Poisson instead of a negative binomial.

Table 6: Table 1 above replicated with Poisson models

	Dependent variable:							
	Conflict events							
	(Round 3)	(Round 3)	(Round 3)	(Round 3)	(Round 4)	(Round 4)	(Round 4)	(Round 4)
Local institutional quality	−0.728***	−0.755***	−1.524***	−1.780***	−0.871***	−0.830***	−0.482**	0.330
	(0.100)	(0.103)	(0.168)	(0.258)	(0.204)	(0.206)	(0.222)	(0.272)
Support for president			0.610***	0.874***			−0.608***	−0.221
			(0.106)	(0.138)			(0.098)	(0.138)
LPI		0.522***	0.749***	0.163		0.706***	0.487***	0.309**
		(0.083)	(0.094)	(0.128)		(0.121)	(0.128)	(0.144)
L(infant mortality rate)		−0.535***	−0.506***	0.527**		0.704***	0.793***	0.596*
		(0.162)	(0.157)	(0.264)		(0.236)	(0.235)	(0.311)
L(population)	0.708***	0.769***	0.802***	0.661***	−0.822***	−0.576***	−0.614***	−1.278***
	(0.129)	(0.129)	(0.130)	(0.136)	(0.142)	(0.163)	(0.176)	(0.207)
L(young men)	−0.060	−0.054	−0.070	−0.736***	1.572***	1.482***	1.618***	1.828***
	(0.119)	(0.117)	(0.118)	(0.110)	(0.165)	(0.182)	(0.189)	(0.214)
L(area)	0.425***	0.434***	0.447***	0.458***	0.535***	0.512***	0.407***	0.260***
	(0.059)	(0.062)	(0.064)	(0.076)	(0.079)	(0.083)	(0.082)	(0.095)
L(travel time)	−0.243**	−0.212**	−0.277***	−0.598***	−0.348***	−0.400***	−0.188	−0.244
	(0.101)	(0.103)	(0.106)	(0.126)	(0.119)	(0.125)	(0.124)	(0.151)
Past conflict	3.315***	3.137***	3.272***	3.850***	3.626***	3.555***	3.724***	4.082***
	(0.170)	(0.185)	(0.189)	(0.225)	(0.241)	(0.235)	(0.239)	(0.269)
Conflict events (spatial lag)	0.199***	0.172***	0.158***	0.079***	0.038***	0.050***	0.068***	0.030**
	(0.009)	(0.010)	(0.010)	(0.011)	(0.010)	(0.011)	(0.011)	(0.013)
Country dummies	No	No	No	Yes	No	No	No	Yes
Observations	1,091	1,089	1,089	1,089	1,339	1,335	1,335	1,335
Log Likelihood	−946.925	−915.914	−898.700	−679.386	−536.590	−508.815	−489.081	−407.438
Akaike Inf. Crit.	1,909.849	1,851.829	1,819.401	1,412.772	1,089.179	1,037.630	1,000.163	870.876
Note:								*p<0.1; **p<0.05; ***p<0.01

Poisson models. Intercept excluded from table. Standard errors in parentheses.

6.2 Hurdle models

Our theoretical discussion leads to the expectation that local institutional quality should reduce both the probability of having *some* conflict, and the intensity of conflict once it occurs. However, these might be distinct processes. To explore whether this distinction matters, we fit hurdle models to separate between having some conflict and conflict violence intensity. The hurdle model (see e.g. Zeileis, Kleiber and Jackman, 2007) includes two components that are estimated simultaneously: One component captures the distribution of positive counts, and another "hurdle" component modeling the probability of getting more than zero counts (a logit). By implementing this model we are able to assess whether *Local Institutional Quality* affects both the probability of having *some* conflict (i.e. more than zero conflict events) and the distribution of counts given that a district experiences conflict. Below, we present the hurdle models referenced in the main paper. The results from the hurdle models are presented in table 7. The first column models the count-component using a negative binomial model, while the second column uses a poisson model. The results are encouraging for our initial findings: We find that *Local Institutional Quality* has a strong negative impact on the distribution of counts *given* that a district experiences conflict, while it also has a negative impact on the probability of experiencing any conflict (especially so in the negative binomial specification), although this pattern is somewhat weaker. This indicates that although *Local Institutional Quality* might matter more for the intensity of conflict (i.e. the number of GED events), than for the *occurrence* of conflict per se, it seems relevant for both processes.

Table 8 shows the hurdle model for round 4 of the survey. As is the case for the other results from round 4, we find somewhat weaker evidence from this survey round, probably due to the lower number of districts actually experiencing conflict violence in the period after the survey. The coefficients for *Local Institutional Quality* are in the same direction, but fall short of statistical significance. The coeffient for *Local Institutional Quality* for the count component is $-.601$ with a p-value of .27, while the coefficient for the logit component is $-.452$ with a p-value of .30. Note that these models are very demanding to estimate, since they essentially estimate two simultaneous equations.

Table 7: Hurdle model (rounds 3 and 4 combined)

	Negative binomial Round 3	Poisson Round 3
Local institutional quality	-2.485^{**}	-1.845^{***}
	(0.829)	(0.220)
Support for president	1.211^{*}	1.084^{***}
	(0.524)	(0.146)
LPI	0.825	0.712^{***}
	(0.484)	(0.123)
L(infant mortality rate)	0.021	−0.379
	(1.234)	(0.226)
L(population)	0.030	0.566^{***}
	(0.683)	(0.142)
L(young men)	0.121	-0.390^{**}
	(0.609)	(0.121)
L(area)	0.361	0.243^{***}
	(0.259)	(0.067)
L(travel time)	−0.258	-0.311^{**}
	(0.457)	(0.119)
Past conflict events (half-life)	1.026	1.236^{***}
	(1.109)	(0.209)
Conflict events (spatial lag)	0.242	0.029^{**}
	(0.164)	(0.011)
Logit component (1 =one or more conflict events)		
Local institutional quality	-0.879^{*}	−0.669
	(0.426)	(0.363)
Support for president	0.015	−0.029
	(0.246)	(0.217)
LPI	0.594^{*}	0.485^{*}
	(0.252)	(0.208)
L(infant mortality rate)	−0.199	−0.128
	(0.429)	(0.384)
L(population)	0.152	0.199
	(0.397)	(0.332)
L(young men)	0.607	0.423
	(0.381)	(0.315)
L(area)	0.521^{**}	0.506^{***}
	(0.165)	(0.142)
L(travel time)	−0.455	-0.511^{*}
	(0.300)	(0.252)
Past conflict events (half-life)	3.952^{***}	3.461^{***}
	(0.620)	(0.464)
Conflict events (spatial lag)	0.461^{***}	0.372^{***}
	(0.069)	(0.049)
AIC	780.712	1149.390
Log Likelihood	-367.356	-552.695
Num. obs.	1089	1089

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Hurdle models of GED conflict events, for Afrobarometer round 3. Intercepts and θ parameter (negative binomial) excluded from table. Standard errors in parenthesis.

Table 8: Hurdle model, round 4

	Count component (negative binomial)
Local institutional quality	−0.601
	(0.546)
Support for president	0.020
	(0.296)
LPI	0.648
	(0.413)
L(infant mortality rate)	1.497
	(0.882)
L(population)	−0.820
	(0.579)
L(young men)	0.941
	(0.554)
L(area)	0.443*
	(0.220)
L(travel time)	−0.617
	(0.340)
Past conflict events (half-life)	3.564***
	(0.732)
Conflict events (spatial lag)	0.022
	(0.042)
Logit component (1 =one or more conflict events)	
Local institutional quality	−0.452
	(0.439)
Support for president	−0.631**
	(0.217)
LPI	0.104
	(0.302)
L(infant mortality rate)	0.743
	(0.491)
L(population)	−0.577
	(0.383)
L(young men)	1.416***
	(0.413)
L(area)	0.556**
	(0.197)
L(travel time)	−0.443
	(0.330)
Past conflict events (half-life)	2.942***
	(0.771)
Conflict events (spatial lag)	0.482***
	(0.093)
AIC	609.028
Log Likelihood	-281.514
Num. obs.	1335

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Hurdle model of GED conflict events, for Afrobarometer round 4. Intercepts and θ parameter (negative binomial) excluded from table. Standard errors in parenthesis.

6.3 Removing extreme cases

It might be that the results found above are driven by a number of extreme cases with a very high number of conflict events. To investigate this possibility we remove such extreme cases and re-estimate the core model (without country-dummies, since removing a number of cases with conflict events makes these models computationally intractable). Table 9 shows the results of this exercise. The first two columns of the table shows the results when we re-estimate the model on samples where we only include districts with less than 30, and 10 conflict events respectively, for round 3. The last two columns shows the same results for round 4. This shows that general pattern remains when we drop these battle-intense districts, although the coefficient in round 4 looses significance when districts with more than 10 events are dropped from the analysis.

Table 9: Dropping the most intense conflict areas and re-estimating the core models

	Dependent variable:			
	Conflict events			
	(Round 3) (Below 30)	(Round 3) (Below 10)	(Round 4) (Below 30)	(Round 4) (Below 10)
Local institutional quality	-0.832^{***}	-0.559^{**}	-0.377^{*}	-0.057
	(0.194)	(0.259)	(0.221)	(0.269)
Support for president	0.179	-0.071	-0.604^{***}	-0.881^{***}
	(0.120)	(0.155)	(0.101)	(0.132)
LPI	0.478^{***}	0.323^{**}	0.235^{*}	0.149
	(0.109)	(0.149)	(0.140)	(0.177)
L(infant mortality rate)	-0.375^{**}	0.164	1.120^{***}	1.164^{***}
	(0.179)	(0.275)	(0.251)	(0.318)
L(population)	-0.116	-0.119	-0.352^{*}	-0.114
	(0.143)	(0.197)	(0.189)	(0.265)
L(young men)	0.605^{***}	0.821^{***}	1.284^{***}	0.770^{***}
	(0.135)	(0.203)	(0.204)	(0.265)
L(area)	0.565^{***}	0.555^{***}	0.319^{***}	0.670^{***}
	(0.072)	(0.096)	(0.088)	(0.117)
L(travel time)	-0.344^{***}	-0.564^{***}	-0.324^{**}	-0.589^{***}
	(0.125)	(0.165)	(0.129)	(0.199)
Past conflict	2.675^{***}	2.814^{***}	3.429^{***}	3.178^{***}
	(0.228)	(0.307)	(0.253)	(0.337)
Conflict events (spatial lag)	0.179^{***}	0.170^{***}	0.089^{***}	0.087^{***}
	(0.012)	(0.017)	(0.013)	(0.017)
Observations	1,086	1,076	1,334	1,327
Log Likelihood	-656.794	-384.345	-467.509	-305.669
Akaike Inf. Crit.	1,335.588	790.690	957.018	633.338
Note:				*p<0.1; **p<0.05; ***p<0.01

Negative binomial model of GED conflict events, for Afrobarometer rounds 3 and 4. Intercepts and θ parameter (negative binomial) excluded from table. Standard errors in parenthesis.

6.4 Removing low-respondent cases

An additional worry is that our results are driven by districts with a very low number of respondents, that are very internally unrepresentative. To exclude this possibility, we replicate the core model on samples where we remove all districts that have fewer than 20 and 40 respondents respectively, for both rounds. The results are shown in table 10. Here, we see that the result remains in the high-respondent sample for round 3, but not for round 4. That the latter result falls apart is probably due to the much lower number of GED events in the round 4 sample, which makes it very hard to find a result when estimating on only 347 (when cases with fewer than 20 are dropped) and 152 (fewer than 40) observations.

Table 10: Dropping low-respondent districts

	Dependent variable:			
	Conflict events			
	(Round 3) (> 20 respondents)	(Round 3) (> 40 respondents)	(Round 4) (> 20 respondents)	(Round 4) (> 40 respondents)
Local institutional quality	−2.235***	−4.375***	−0.188	−1.490
	(0.698)	(1.510)	(0.894)	(2.671)
Support for president	0.740*	1.926**	−2.227***	1.523
	(0.380)	(0.792)	(0.599)	(1.733)
LPI	1.460***	3.188***	0.061	0.331
	(0.439)	(0.925)	(0.604)	(1.603)
L(infant mortality rate)	−0.784	−3.024*	0.870	0.504
	(0.695)	(1.690)	(0.927)	(3.336)
L(population)	0.735	0.510	0.473	−2.534
	(0.503)	(0.814)	(0.714)	(1.916)
L(young men)	0.638	0.691	2.092***	3.854*
	(0.435)	(0.732)	(0.727)	(2.206)
L(area)	0.262	0.694*	0.279	0.571
	(0.206)	(0.364)	(0.387)	(1.042)
L(travel time)	0.057	−0.011	0.495	−0.634
	(0.362)	(0.695)	(0.551)	(1.584)
Past conflict	3.368***	1.285	2.887***	2.852
	(0.856)	(1.474)	(1.038)	(2.815)
Conflict events (spatial lag)	0.284***	0.197**	0.250**	0.151
	(0.050)	(0.082)	(0.099)	(0.261)
Constant	−13.991**	−0.679	−36.130***	−15.868
	(6.731)	(12.761)	(9.922)	(30.922)
Observations	433	199	347	152
Log Likelihood	−233.447	−89.582	−72.899	−20.102
Akaike Inf. Crit.	488.895	201.164	167.799	62.204
Note:				*p<0.1; **p<0.05; ***p<0.01

Negative binomial model of GED conflict events, for Afrobarometer rounds 3 and 4. Intercepts and θ parameter (negative binomial) excluded from table. Standard errors in parenthesis.

6.5 Parsimonious models

As Achen (2002) argues and shows, models with a high number of control variables can yield misleading estimates and sign flips due to highly correlated control variables, proverbially labeled "garbage can regressions"' (Achen, 2005). To make sure that our results do not hinge on including a large number of controls, we present a number of very parsimonious models below. Table 11 shows the models for round 3. Model 1 only regresses post-survey conflict events on *Local Institutional Quality*. Models 2-3 include Past conflict events and the spatial lag of conflict events respectively, while model 4 adds country-dummies. This shows that our main result is present in these very parsimonious models, avoiding the "garbage can regression" critique. Table 12 shows the same set of models for round 4, indicating a similar pattern.

Table 11: Parsimonious models, round 3

	Dependent variable:			
	Conflict events			
	(1)	(2)	(3)	(4)
Local institutional quality	-0.983^{***}	-1.626^{***}	-1.357^{***}	-0.949^{**}
	(0.313)	(0.288)	(0.257)	(0.467)
Past conflict events (half-life)		6.028^{***}	2.374^{***}	3.339^{***}
		(1.066)	(0.830)	(0.660)
Conflict events (Spatial lag)			0.883^{***}	0.294^{***}
			(0.063)	(0.051)
Country-dummies	No	No	No	Yes
Observations	1,095	1,095	1,095	1,095
Log Likelihood	−498.174	−475.718	−432.266	−382.313
Note:				*p<0.1; **p<0.05; ***p<0.01

Negative binomial models of GED events for round 3. Constant and θ parameter excluded from table.

Table 12: Parsimonious models, round 4

	Dependent variable:			
	Conflict events			
	(1)	(2)	(3)	(4)
Local institutional quality	-2.465^{***}	-1.585^{***}	-1.190^{***}	-1.072^{**}
	(0.447)	(0.366)	(0.374)	(0.474)
Past conflict events (half-life)		6.663^{***}	4.495^{***}	5.502^{***}
		(0.946)	(0.941)	(0.809)
Conflict events (Spatial lag)			1.030^{***}	0.151^{***}
			(0.078)	(0.056)
Country-dummies	No	No	No	Yes
Observations	1,342	1,342	1,342	1,342
Log Likelihood	−395.648	−361.046	−344.955	−289.144
Note:				*p<0.1; **p<0.05; ***p<0.01

Negative binomial models of GED events for round 4. Constant and θ parameter excluded from table.

6.6 Alternative specification with dependent variable from ACLED

In this section we show that replacing the dependent variable with an alternative conflict data source, the ACLED dataset, we find the same pacifying effect of local political institutions on the risk of armed conflict.

Table 13: Alternative conflict data - Acled

	Dependent variable:	
	Acled conflict events	
	(Round 3)	(Round 4)
Local institutional quality	-0.655^{***}	-0.422^{***}
	(0.109)	(0.126)
L(population)	-0.466^{***}	-0.353^{***}
	(0.128)	(0.115)
L(young men)	0.510^{***}	0.614^{***}
	(0.129)	(0.119)
L(area)	0.524^{***}	0.315^{***}
	(0.061)	(0.055)
L(travel time)	-0.570^{***}	-0.490^{***}
	(0.120)	(0.098)
Past acled events (half-life)	0.312^{***}	0.310^{***}
	(0.025)	(0.020)
Observations	1,091	1,339
Log Likelihood	−1,857.837	−2,183.293
Akaike Inf. Crit.	3,729.675	4,380.585
Note:	*p<0.1; **p<0.05; ***p<0.01	

Negative binomial models of ACLED conflict events for rounds 3 and 4. Constant and θ parameter excluded from table.

6.7 Analysis on disaggregated measures of local institutional quality

While the factor analysis show that the components of the *Local Institutional Quality* share much variation, there are reasons to doubt results based on an index if, for example, some items pull in completely opposite directions. To investigate whether this is the case we here show the results of the baseline model using each of the index components separately. This analysis, displayed in table 14, clearly shows that the items all pull in the same direction, and are significantly linked to fewer instances of local civil conflict violence. Note that each item has been coded such that positive scores indicate more institutional quality.

Table 14: Disaggregated components of quality

	Dependent variable:						
	Conflict events						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Trust in local politicians	-0.766^{***}						
	(0.170)						
Trust cops		-0.494^{***}					
		(0.146)					
Corruption local politicians			-1.518^{***}				
			(0.249)				
Corruption cops				-1.622^{***}			
				(0.280)			
Performance local politicians					-1.147^{***}		
					(0.201)		
Corruption courts						-1.540^{***}	
						(0.269)	
Attended community meeting							-0.919^{***}
							(0.275)
L(population)	0.149	0.096	0.065	-0.216	0.256	-0.046	0.273
	(0.361)	(0.346)	(0.359)	(0.342)	(0.366)	(0.347)	(0.362)
L(young men)	-0.003	-0.056	-0.020	0.132	-0.106	0.020	-0.106
	(0.354)	(0.343)	(0.360)	(0.346)	(0.353)	(0.357)	(0.357)
L(area)	0.330^{**}	0.298^{*}	0.415^{***}	0.522^{***}	0.389^{***}	0.429^{***}	0.298^{**}
	(0.148)	(0.152)	(0.158)	(0.166)	(0.150)	(0.160)	(0.151)
L(travel time)	-0.018	-0.069	-0.066	-0.035	-0.144	-0.117	0.061
	(0.291)	(0.292)	(0.298)	(0.307)	(0.293)	(0.308)	(0.296)
Past conflict events (half-life)	0.260^{***}	0.218^{***}	0.180^{**}	0.206^{***}	0.308^{***}	0.195^{***}	0.212^{***}
	(0.072)	(0.074)	(0.073)	(0.072)	(0.072)	(0.076)	(0.074)
Conflict spatial lag	0.705^{***}	0.761^{***}	0.686^{***}	0.736^{***}	0.650^{***}	0.850^{***}	0.796^{***}
	(0.062)	(0.063)	(0.063)	(0.062)	(0.060)	(0.065)	(0.064)
Observations	1,091	1,091	1,091	1,091	1,091	1,091	1,091
Log Likelihood	-430.485	-435.169	-430.759	-427.436	-427.067	-438.071	-434.940
θ	0.105^{***} (0.017)	0.103^{***} (0.017)	0.109^{***} (0.018)	0.109^{***} (0.018)	0.111^{***} (0.018)	0.098^{***} (0.016)	0.101^{***} (0.017)
Akaike Inf. Crit.	876.969	886.339	877.518	870.872	870.133	892.143	885.879
Note:							*p<0.1; **p<0.05; ***p<0.01

Negative binomial models of GED events, rounds 3 and 4. Standard errors in parentheses. Intercept and θ parameter excluded from table.

6.8 Interactions with national-level institution

This section explores whether there are interaction-effects between *Local Institutional Quality* and aspects of national institutions. First, we probe whether national-level democracy matters. It could be that only local-level institutions that are underpinned by a national framework of democracy are conducive to peace, if, for example, high-quality institutions suffer from lower legitimacy in non-democracies. To investigate this conjecture, we rely on regime-type data from Polity (Marshall, n.d.), and create a binary measure counting a country with a Polity score above 0 as a democracy.[2] Columns 1 and 2 in table 15 investigate interaction effects between this binary democracy-measure and *Local Institutional Quality*. These show no significant interaction term, neither for round 3 nor for round 4, and have differently signed coefficients in both rounds. Hence, we find no evidence for an interaction between national democracy and local-level institutional quality.

Second, we investigate whether high-quality national and local institutions are reinforcing. Such a reinforcement effect would be plausible, if, for example, local-level institutions can draw on resources from the central government to a higher extent in countries with good national governance. To explore this, we interact our local institutional quality measure with the WGI-based index of national-level institutional quality presented above, in section 3.4. The results can be seen in columns 3 and 4 in table 15. The interaction effects in both rounds have the expected sign; *Local Institutional Quality* is more strongly associated with peace in countries with good national governance. This coefficient however, is only significant in round 3. Nevertheless, this provides some suggestive evidence for an interaction effect, that should be explored in further studies.

[2]The Polity index ranges from -10 to 10, where positive scores indicate higher levels of democracy. Since so few of the countries in our sample are consolidated democracies, we have opted for this very low threshold for democracy.

Table 15: Interactions with democracy and national level institutional quality

	Dependent variable:			
	gedevents			
	(1)	(2)	(3)	(4)
Local institutional quality	−3.090***	−0.217	−3.445***	−1.378*
	(0.962)	(0.882)	(0.561)	(0.804)
Democracy	−3.224	1.316		
	(2.080)	(1.863)		
National Institutional Quality			1.060***	−0.072
			(0.297)	(0.393)
L(population)	−0.040	−0.672**	−0.177	−0.463
	(0.388)	(0.318)	(0.375)	(0.326)
L(young men)	0.034	1.033***	−0.029	0.505
	(0.376)	(0.328)	(0.365)	(0.340)
L(area)	0.428***	0.928***	0.544***	1.108***
	(0.162)	(0.194)	(0.165)	(0.206)
L(travel time)	0.189	−1.222***	−0.154	−1.287***
	(0.313)	(0.314)	(0.306)	(0.322)
Past conflict	0.183**	0.463***	0.281***	0.516***
	(0.073)	(0.069)	(0.072)	(0.060)
Conflict events (spatial lag)	0.706***	0.431***	0.705***	0.337***
	(0.062)	(0.062)	(0.060)	(0.053)
Local institutional Quality·Democracy	1.076	−1.156		
	(1.017)	(0.963)		
Local institutional quality·National institutional quality			−0.748***	−0.212
			(0.184)	(0.210)
Constant	0.662	−2.528	3.454	−0.700
	(3.194)	(2.823)	(2.942)	(3.008)
Observations	1,091	1,339	1,091	1,339
Log Likelihood	−423.324	−318.308	−421.423	−297.941
θ	0.115*** (0.019)	0.136*** (0.027)	0.119*** (0.019)	0.203*** (0.042)
Akaike Inf. Crit.	866.648	656.617	862.846	615.882
Note:				*p<0.1; **p<0.05; ***p<0.01

Negative binomial models of GED events, for rounds 3 and 4. Standard errors in parentheses. Intercept excluded from table.

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