Horizontal Inequalities and Political Violence

Gudrun Østby

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Department of Political Science
University of Oslo

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Contents

Abbreviations

ACD	Armed Conflict Dataset
ACLED	Armed Conflict Location Events Dataset
AFL	Armed Forces of Liberia
CIESIN	Center for International Earth Science Information Network
CSCW	Centre for the Study of Civil War
DHS	Demographic and Health Surveys
EA	Enumerated Area
ECOMOG	Economic Community of West African States Monitoring Group
ELF	Ethno-Linguistic Fractionalization
ESRI	Environmental Systems Research Institute
GCOV	Group-based Coefficient of Variance
GDP	Gross Domestic Product
G-Econ	Geographically Based Economic Data
GIS	Geographic Information Systems
GWR	Geographically Weighted Regression
HI	Horizontal Inequality
IDW	Inverse Distance Weighted
IMR	Infant Mortality Rate
LDC	Least Developed Country
LRD	Local Relative Deprivation
LURD	Liberians United for Reconciliation and Democracy
MAR	Minorities at Risk
MFDC	Mouvement des Forces Démocratiques de Casamance
MODEL	Movement for Democracy in Liberia
MRD	Migrant Relative Deprivation
NPFL	National Patriotic Front of Liberia
OLS	Ordinary Least Squares
PAC	Polarization and Conflict
PRIO	Peace Research Institute Oslo
PV	Political Violence
RCN	Research Council of Norway
RD	Relative Deprivation
RRD	Regional Relative Deprivation
SIP	Scalar Index of Polities
SSA	Sub-Saharan Africa
SSIP	Scientific Study of International Processes
UCDP	Uppsala Conflict Data Program

ULIMO	United Liberation Movement of Liberia for Democracy
UN	United Nations
UNDP	United Nations Development Programme
UNSFIR	United Nations Support Facility for Indonesian Recovery
UNU	United Nations University
USAID	US Agency for International Development
USD	Urban Social Disorder
VI	Vertical Inequality
WDI	World Development Indicators
WDR	World Development Report
WIDER	World Institute for Development Economics Research

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Introduction

Introduction[*]

1. Motivation

The overall aim of this dissertation is to systematically investigate the relationship between horizontal inequalities, or socioeconomic inequalities between culturally defined groups, and political violence in developing countries. This ambition is my response to three major and so far unsolved puzzles that feature in the academic literature on political violence.

First, while qualitative case studies conclude that socioeconomic inequality is an important cause of conflict, nearly all statistical studies reject this idea. Second, despite the fact that the relative share of ethnic conflict has been steadily rising since World War II, ethnic heterogeneity as such does not seem contribute to an increased risk of conflict. Third, it is a paradox that the regions of the world which are clearly the most violence-prone (Sub-Saharan Africa and South Asia) remain largely under-researched in global investigations of armed conflict, due to low-quality or missing data. In concert, these three puzzles underscore a serious knowledge gap in the literature on political violence. The purpose of this thesis is to contribute to fill this gap. Below, I present the puzzles in more detail. I start out with the first and most concrete puzzle, which almost begged to be addressed by a PhD project. I then introduce the two more general, albeit often ignored, puzzles which are inherently related to the first one, and which in my view should inspire a shift of focus in general conflict research.

1.1. Puzzle I: The quantitative–qualitative mismatch

For almost half a century, scholars have tried to test the assumption that inequality breeds political conflict, relying on statistical as well as qualitative methods. These efforts have not produced a conclusive answer to the question: 'What is the relationship between economic inequality and political violence?' (see e.g. Blattman & Miguel, 2010 Lichbach, 1989; Murshed, 2010). In line with earlier critics of relative deprivation theory (Skocpol, 1979; Snyder & Tilly, 1972; Tilly, 1978), contemporary statistical studies (Collier & Hoeffler, 2004; Fearon & Laitin, 2003) have largely dismissed the role of inequality and other grievances alike, focusing instead on opportunities for violent mobilization and state capacity.

In contrast to this statistical rejection of the inequality–conflict link, a case-based literature has emerged, spearheaded by the Oxford-based development economist Frances Stewart. She focuses on the role of 'horizontal inequalities' (HIs), or systematic economic and political inequalities between ethnic, religious or regional groups, in affecting conflict likelihood and conflict dynamics (see e.g. Stewart, 2002; Stewart, 2008). The concept of horizontal inequality differs from the 'normal' definition of inequality, often referred to as 'vertical inequality' (VI), because the latter type lines individuals up vertically and measures inequality over the range of individuals rather than groups. Furthermore, HIs are conceived of as inherently multidimensional, encompassing economic, social and political dimensions, unlike previous accounts and measures of inequality that seem to concentrate exclusively on economic inequality (usually operationalized as income inequality or inequality in land distribution). In brief, the horizontal inequality argument states that inequalities coinciding with cultural cleavages may enhance group grievances which in turn may facilitate mobilization for conflict.

Based on material from several case studies, Stewart (2002; 2008) and her collaborators have concluded that horizontal inequalities have indeed provoked violence, ranging from a high level of criminality in Brazil to civil war in Uganda, Côte d'Ivoire and Sri Lanka. The lessons derived from such cases provide deep insight into specific cases. However, a restricted number of cases does not yield an ideal basis for generalizations about the relationship between horizontal inequalities and violent conflict; especially when dimensions of horizontal inequalities (and political violence) are

not systematically measured across countries. In order to evaluate the generalizability of the horizontal inequality–political conflict nexus, a systematic quantitative research design is needed. This is where this project enters.

If the statistical studies are right, the contradictory evidence from some case studies should be viewed as anecdotal and cannot be generalized further. On the other hand, if the findings from case studies do actually reflect a more universal relationship, then the majority of the statistical inequality–conflict studies must have missed the target with their exclusive focus on inequality between individuals rather than groups. Drawing on the insights from the case-based literature, I aim to test whether horizontal inequalities affect political violence when tested across many cases.

1.2. Puzzle II: The 'ethnic paradox'

The second conundrum in the conflict literature that spurred my curiosity and interest in the topic of horizontal inequalities is the unclear relationship between ethnicity and political violence. Globally, the total number of armed conflicts has decreased since the Cold War, but the share of ethnic conflict (i.e. conflicts fought between ethnically distinguished belligerents over some ethno-nationalist aim)[1] in the international system has been continuously rising since World War II. During the Cold War, many conflicts were portrayed as disputes about class or ideology, following the East–West division, with each side supported by the major powers along ideological lines. Since the end of the Cold War in the early 1990s, however, ideological differences have diminished, and the identity basis of conflicts has become much more explicit. Data on conflict confirm this trend revealing a significant increase in the proportion of all conflicts characterized as ethnic, as shown in Figure 1.

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[1] More specifically, I adapt Cederman, Min & Wimmer's (2008: 1–2) definition of ethnic conflict as conflicts in which armed organizations both explicitly pursue ethnonationalist aims and interests (such as selfdetermination, the ethnic balance of power in government, and ethnic and racial discrimination), and recruit fighters and forge alliances on the basis of ethnic affiliations.

Figure 1. Ethnic Conflict as a Proportion of all Armed Conflicts, 1946–2005
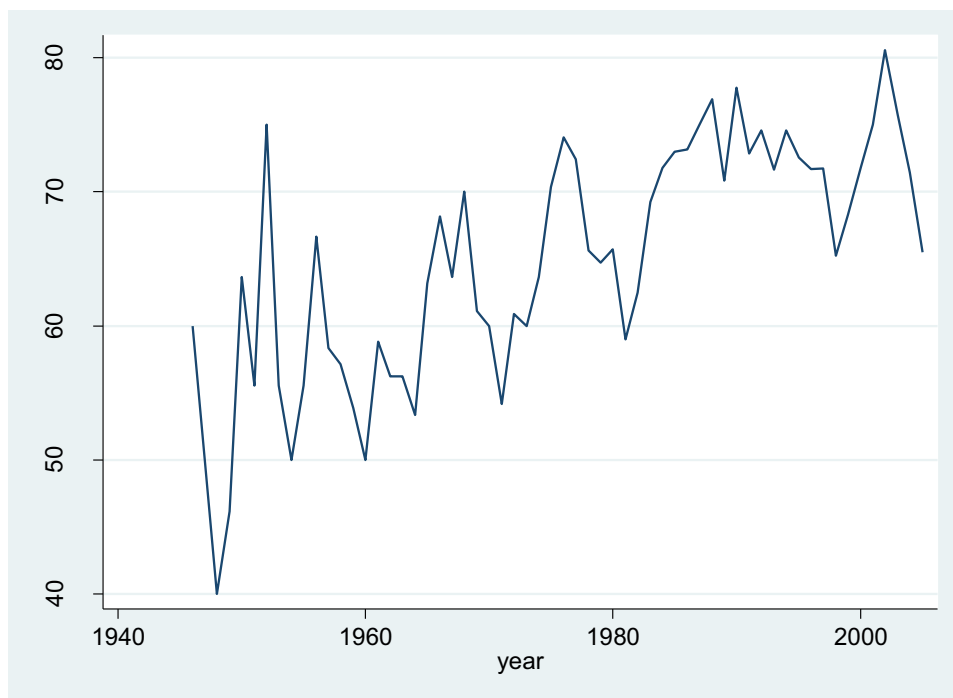
The graph is generated based on data from Cederman, Min & Wimmer's (2008), 'Ethnic Armed Conflict Dataset' available at http://hdl.handle.net/1902.1/11797, and the UCDP/PRIO Armed Conflict Dataset available at http://www.prio.no/CSCW/Datasets/Armed-Conflict/UCDP-PRIO/.

Ethnic identity is indeed a factor which has received wide attention in the civil conflict literature, and conflicts are often expected to occur between groups with different ethnic identities. Yet, the results regarding different forms of ethnicity and conflict are at best mixed (see e.g. Ellingsen 2000; Fearon & Laitin 2003; Østby, 2008a [Chapter 2]; Reynal-Querol 2002). It seems evident that cultural/ethnic differences are not a sufficient cause of conflict. On the contrary, most multiethnic societies are relatively peaceful. In fact, Fearon & Laitin (1996) estimated that in the period from 1960 to 1979, of all the possible conflict scenarios between neighboring ethnic groups in Africa, only 0.01% actually turned into violent conflict. This leaves us with what I refer to here as the 'ethnic paradox': the relative share of ethnic conflicts is rising, but according to statistical tests, ethnic diversity as such does not cause conflict, nor does it

influence war duration (Fearon, 2004).[2] In other words, the finding that many conflicts are fought partially along ethnic lines is not sufficient to make the case that ethnic-based grievances are driving the fighting. The critical question, then, becomes: Why do certain multiethnic countries experience civil conflict, while others do not? To answer this question one needs to go beyond the sheer cultural differences between ethnic groups and explore what contextual factors may contribute to an increased risk of conflict across ethnic lines. Indeed, Woodward (1995) holds that so-called ethnic conflicts are in fact driven by underlying economic inequalities, which in turn politicize ethnic identities. This line of reasoning is captured by the horizontal inequality argument. Hence, the second motivation for my PhD project is to investigate the role of *one* potential key to ethnic conflicts: horizontal inequalities.

1.3. Puzzle III: The key and the lamppost

The third paradox which has motivated this project pertains to the geographical scope and quality of the data we use in order to investigate theoretical claims about the causes of conflict. This problem is of a more general nature and is more or less present in almost all general investigations of political violence.

When it comes to the variable of interest – political violence, or conflict ('the right hand side of the equation'), the status of the data is actually not so bad. The main source of conflict data used in this thesis, the UCDP/PRIO Armed Conflict Database (Gleditsch et al., 2002)[3] provides systematic yearly data on the outbreak and incidence of violent conflict for *all* countries in the global system since World War II. According to *Human Security Report,* this dataset is indeed 'the most comprehensive yet created on political violence around the world' (Human Security Centre, 2005).

The graph in Figure 2 shows the yearly incidence of armed conflict (including both internal and inter-state conflict), broken down by world region. The figure yields

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[2] However, Freaon (2004) finds that civil wars which involve land or natural resource conflicts between state supported migrants from a dominant ethnic group and the ethnically distinct 'sons of the soil' who inhabit the region in question tend to last significantly longer. Such conflicts would be classified as ethnic conflict according to Cederman, Min & Wimmer's (2008) definition.

[3] See http://www.prio.no/CSCW/Datasets/Armed-Conflict/UCDP-PRIO/.

two immediate observations: First, throughout the entire period since 1946, Africa and Asia have seen much more conflict than the other world regions. Second, whereas the total amount of conflict has gone down since the Cold War, this has not been the case for Africa and (South) Asia, which are still plagued by several armed conflicts,[4] including the bulk of the so-called ethnic conflicts. Civil conflicts occur disproportionately in poor countries, and retard economic development in entire regions. For this reason, civil conflict is increasingly seen as a development problem (Collier et al., 2003; Collier & Sambanis, 2005: xiii).

Figure 2. Conflicts by Region
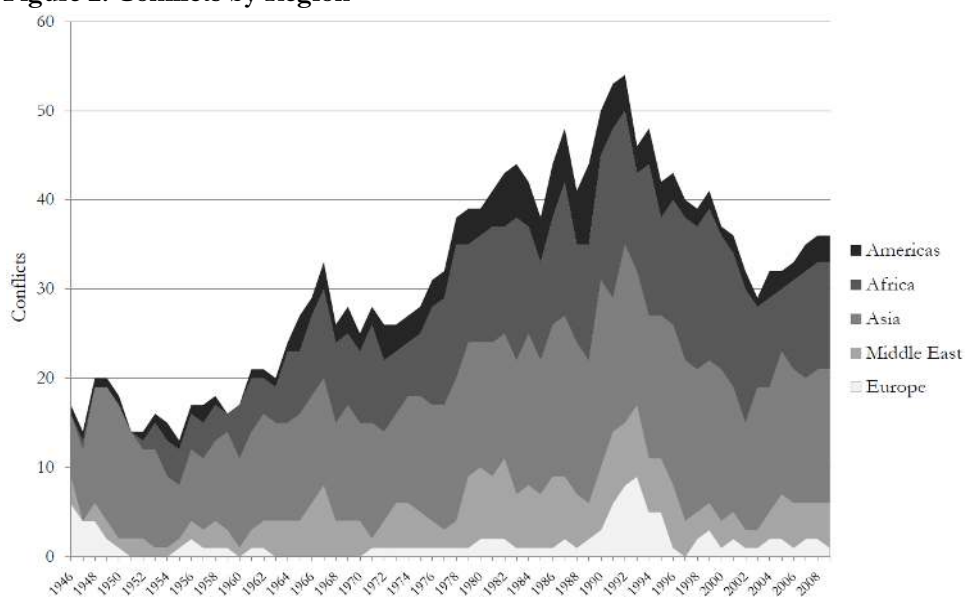
The graph is generated based on data from the UCDP/PRIO Armed Conflict Dataset: http://www.prio.no/CSCW/Datasets/Armed-Conflict/UCDP-PRIO/. Figures for year 2009 are added from Harbom & Wallensteen (2010).

A clear implication of the graph in Figure 2 is that we should put more energy and resources in trying to understand why conflicts erupt, especially with an eye to the most conflict-ridden parts of the world. Ironically, though, most of the alleged 'global' statistical studies on civil conflict tend to suffer from poor data on the left hand side of

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[4] Of the 36 conflicts which were ongoing in 2009, 1 was taking place in Europe; 5 in the Middle East; 15 in Asia; 12 in Africa; and 3 in the Americas (Harbom & Wallensteen, 2010: 506–507).

the equation, often as a result of missing or extremely low-quality data for several developing countries. This is particularly the case with regard to economic indicators, as I will explore further in Section 4. Indices of income inequality data are measured with particularly high levels of error (Cramer, 2001). Add to this that for many countries we hardly have any income or inequality data at all, which implies that these countries are literally thrown out of the analyses.

When studying conflict, a biased sample of cases may disturb the effect of the independent variable. Humphreys (2003: 3) notes that if we for instance are less likely to have income inequality data for countries where there are civil wars, this could bias the estimated effect of inequality downwards, which will make us believe that the relationship is weaker than it actually is. This problem is aptly captured by the parable of the key and the lamppost, which describes a man searching for a lost key underneath a lamppost. When questioned as to exactly where the key was lost, he indicated that it was lost elsewhere but that he was searching under the lamppost because the light was better there. The key to the inequality–conflict nexus is not likely to be found with data for the overall relatively peaceful, developed countries of the Western world. And even though there have been some conflicts in this part of the world, it is not given that we should be significantly better suited to understand the conflict dynamics in Sub-Saharan African countries like Chad and Sudan by studying the conflicts in Northern Ireland, Spain or the former Yugoslav republics. Despite the 'better light' afforded by focusing on this area, the proper method is to look carefully for the inequality–conflict nexus where it is more likely to be found: in regions of the world which still suffer from a great amount of conflict and deteriorating economic conditions.

1.4. Research questions

Motivated by the three puzzles described above, this doctoral project is an attempt to rectify the notable lack of systematic studies of the horizontal inequality–political violence nexus, with a strong priority for collecting comparable and high-quality HI data for developing countries. Through six independent but related articles I – and my various co-authors – attempt to answer whether various forms of horizontal inequalities increase the risk of civil conflict as well as other forms of political violence. The dissertation has a clear quantitative orientation, as all the articles include empirical

investigations that employ statistical models to evaluate various aspects of the impact of horizontal inequalities on political violence. The project has been guided by four broad research questions, which together form the basis for the formulation of specific hypotheses that are tested in the subsequent chapters:

1. Are societies that experience severe horizontal inequality more prone to internal armed conflict?

2. Are there any contextual or intervening factors that influence the relationship between horizontal inequality and internal armed conflict?

3. Are horizontal inequalities relevant across different forms of political violence?

4. Does the effect of horizontal inequalities vary at the sub-national level?

The rationale behind formulating these research questions is developed in the subsequent sections, and in Section 5 I elaborate on how the four research questions are addressed in the various chapters.

1.5. Defining concepts

As all the chapters of the dissertation include empirical investigations that employ statistical models and numerical data to evaluate various aspects of the relationship between horizontal inequalities and political violence, it is timely to clarify what I mean by these two concepts before proceeding further.

1.5.1. Horizontal inequalities

The concept of 'horizontal inequalities' (HIs) and its theorized relationship with conflict was first developed by Frances Stewart and her collaborators (e.g. Stewart, 2000). Stewart (2008: 3) defines HIs as 'inequalities in economic, social or political dimensions or cultural status between culturally defined groups'. In other words, HIs are present when e.g. ethnic or religious cleavages coincide with systematic socioeconomic divisions in society. There are two particularly important aspects pertaining to the concept of HIs. First, current thinking about inequality tends to place the individual firmly at the center of concern, and measures of inequality typically relate to the ranking of individuals (or households) vertically within a country, or sometimes the globe. As mentioned above, in the development literature such inter-individual inequality is referred to as vertical

inequality. Stewart (2002a: 2) argues that this definition of inequality neglects a vital dimension of human well-being and of social stability, namely the group dimension. Sen (1992: 117) agrees that general analyses of inequality must, in many cases, proceed in terms of groups – rather than specific individuals – and that one should focus on *inter-group* variations. Possible group-identifiers include e.g. ethnicity, religion, regions, gender, age cohorts, and migrant status. The relevant group definition varies across different arenas and societies.

Measures of vertical inequality look at differences between all individuals in a society, whereas measures of horizontal inequality look instead at differences in income between groups or e.g. subnational regions. Although there is often some correlation between the two measures, they need not be related to each other. In practice, a country can have large income inequalities between groups (HIs), despite the fact that the overall (vertical) income inequality is rather low (as is the case in Rwanda), and vice versa; a country can have a high vertical income inequality score, even though the structural differences between groups might be low (e.g. Brazil). Besides, a country can have both strong vertical and horizontal inequalities at the same time (e.g. South Africa), or it can score low on both (e.g. Switzerland). Figure 3 illustrates these four possible scenarios (i.e. combinations of VI and HI) in a society consisting of two equally sized groups: people from the South (dark grey) and people from the North (light grey).

Figure 3. Four Stylized Combinations of Horizontal and Vertical Inequalities
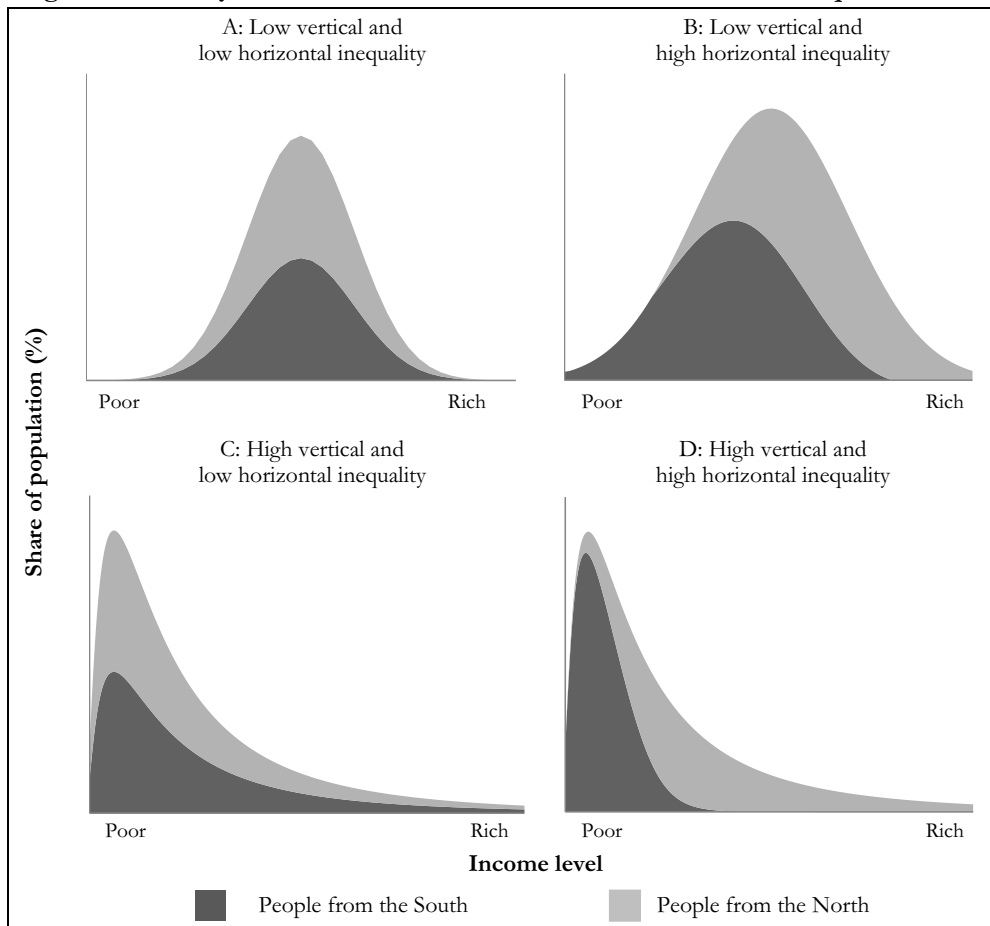
The graphs in scenarios A–D are stacked, so that the uppermost line indicates the overall income distribution. Thanks to Håvard Strand for generating these graphs.

The second essential thing to note about HIs is that they are multidimensional – with political, economic and social elements (as indeed are VIs, but they are rarely measured in a multidimensional way[5]). Despite that fact that VIs are usually measured only in terms of income (or land) distribution, the esteem of a group, which impacts on individual well-being, arises from the relative position of the group in a large number of areas, not just in incomes. Stewart (2008: 13) roughly categorizes HIs into four areas: political participation; economic aspects; social aspects; and cultural status. HIs in

[5] For an exception to this rule, see Hicks (1997).

political participation can occur e.g. at the level of the cabinet, the parliament, the bureaucracy, or the army. Economic HIs encompass access to ownership of assets (e.g. financial, land, livestock), employment and income. Social HIs may involve unequal access to health services and education. Finally HIs in cultural status may pertain to the extent to which a society recognizes (or fails to recognize) a group's cultural practices in matters of e.g. language, dress, and holidays. In this dissertation I explore HIs between various group indicators, including ethnicity, religion, region, locality, and migrant status. With regard to dimensions, I mainly focus on economic and social aspects of HIs, but I also include some level of political HIs in Chapter 3.

1.5.2. Political violence

The second central concept in this dissertation is (domestic) political violence (PV). The key question asked is under what circumstances political organizations use violence to achieve their political goals. This broad concept encompasses different forms of collective, politically motivated violence, including demonstrations, riots, terrorism, communal conflict, and civil war.

The collective aspect of political violence is central. While history often recognizes the importance of individuals, politics is a collective phenomenon. This becomes particularly important when studying group-level inequality. The level of organization can differ considerably, from small and tight terrorist organizations to large but somewhat ad hoc public demonstration organizers. While they differ, some organizations use violent means to reach political ends, and this is what distinguishes political violence from other forms of political protest.

Furthermore, distinguishing political violence from crime, which is often organized but lacks the political aspect, can be difficult. What is a political motivation? The US 'War against drugs' is fought against a number of criminal gangs, of which some are recognized as rebel organizations and others are not. This difference can be difficult to define, but in the end, the most credible piece of information is probably the statements of the groups. Do they make political statements? Do they justify their violent means with political ends?

In my various articles I look at political violence between various actors, both state- and non-state ones. A state is by definition regarded as a formally organized group.

For non-state actors the level of organization can vary from formally organized groups to various types of informally organized groups (see Harbom & Pettersson, 2010). The former typically refers to e.g. highly organized rebel groups with an announced name. Informally organized non-state actors can range from groups composed of supporters and affiliates to political parties (often not permanently organized for combat, but who at times use their organizational structure for such purpose) to groups that simply share a common identification such a common ethnic, clan, tribal, or religious affiliation. The latter level of organization is often associated with what is referred to as 'communal violence'. Finally, there can be episodes of political violence which may lack an organizational structure altogether, such as riots or strikes. In this dissertation I assess whether horizontal inequalities are relevant across various forms of political violence, as spelled out in Research Question 3 above.

Most of the attention in this dissertation is devoted to one particular form of PV, namely civil war. The most apparent variations within the quantitative tradition relate to fatality thresholds and whether or not civilian casualties should be counted (Buhaug, 2006). The analyses presented in Chapters 2–5 are based on the UCDP/PRIO Armed Conflict Database, which includes all armed conflicts between a government and an organized opposition group with a clearly stated incompatibility, or aim (governmental change or territorial secession) that generated at least 25 battle-related deaths (civilians not included) per calendar year (see Gleditsch et al., 2002). Less than half of these conflicts reach the stricter 1,000 fatalities threshold, which is required for them to be classified as 'war'. Yet, I use the terms 'conflict' and 'war' interchangeably in this dissertation unless specified otherwise. For more comprehensive discussions of the conceptual issues of civil war, see Sambanis (2004a) and Strand (2006).

However, the HI–conflict argument is not restricted to cases where the state is a participant in the violence. On the contrary, one should expect lower organizational barrier for non-state violence than for mobilizing and sustaining and armed challenge against the state.[6] In Chapter 6 (Østby, Urdal, Tadjoeddin, Murshed & Strand, 2011), we distinguish between two different forms of political violence in Indonesia: 'routine' and 'episodic'. Routine violence happens on an 'everyday' basis, and is centered around

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[6] See Fjelde & Østby (2010) for more discussion on how HIs relate to non-state violence.

group-based vigilante violence/popular justice and intergroup/neighborhood brawls. Episodic violence refers to ethno-communal and separatist violence, the latter which partly overlaps with UCDP/PRIO's broader definition of civil war. While routine violence is more frequent and spatially widespread compared to episodic violence, the latter can lead to greater fatalities when and where it occurs (see Tadjoeddin, 2002; Varshney, Tadjoeddin & Panggabean, 2008). Less attention is devoted to routine violence in the conflict literature, which tends to be dominated by accounts of civil war.

Since civil wars often take place in rural areas (Collier & Hoeffler, 2004; Fearon & Laitin, 2003), this also raises the question on whether inequality has similar effects on urban violence. Finally, in Chapter 7 I shift the focus to cities and address the determinants of various events of 'urban social disturbance'.[7] I adopt Urdal's (2008) definition of 'social disturbance', which refers to a broad range of non-violent and violent political activity ranging from peaceful demonstrations and strikes to organized warfare or acts of terrorism.

1.6. Structure

This introduction proceeds as follows: In Section 2 I set the stage by reviewing the extensive literature on (vertical) inequality and conflict. In Section 3 I narrow the scope and present a theoretical framework that links horizontal inequalities with political violence through various mechanisms relating to both motivation and opportunities. Section 4 outlines the analytical approach underlying this doctoral project, with an emphasis on how to generate measures of horizontal inequalities based on survey data. In Section 5 I provide an overview of the various chapters and outline their individual and joint contributions and findings, situating my own research within the larger debate on inequality and political violence. I conclude by outlining some lessons from the current research and providing some directions for future research.

2. Inequality and Civil Conflict: A Review

Ideas about human frustration and responses to grievances are inescapably part of the rationale for believing that there is a relationship between inequalities and political

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[7] For a brief introduction to urban violence, see Buhaug, Urdal & Østby (forthcoming).

violence. Such ideas are not of recent origin. Explanations of aggression and relative deprivation have deep roots in the history of thought. At least since Aristotle (e.g. 350 B.C./1984), political theorists have believed that political discontent and its consequences – protest, instability, and violence – depend not only on the absolute level of economic wealth, but also its distribution, i.e. inequality between the rich and poor. A remarkably diverse literature, both ancient and modern, theoretical as well as empirical, has coalesced on the proposition that political violence is a function of economic inequality.

In this section I review the most central theoretical arguments and empirical studies of vertical inequality and conflict. As we will see, the proposed relationship between inequality and conflict has a very mixed record in the empirical literature (see e.g. Blattman & Miguel, 2010; Lichbach, 1989; Murshed, 2010). I discuss some of the problems associated with this extensive literature, and suggest how a reconceptualization of inequality may be a solution to the empirical confusion in the field.

2.1. Theoretical arguments and empirical findings

Different theoretical approaches to inequality and conflict include Marxist theory of class struggle and revolution (Marx 1887/1967)[8], relative deprivation theory (e.g. Davies, 1962; Feierabend & Feierabend 1966; Gurr, 1970) and theories of ethnic conflict and structural inequality (e.g. Galtung, 1964; Gurr, 1993, 2000; Hechter, 1975; Horowitz, 1985). What these theories have in common is the interpretation of conflict as a result of widely felt grievances among the relatively disadvantaged in society.

Marxist theory emphasizes the violence potential of economic inequality, as the industrial working class is expected to rebel because they have 'nothing to loose but their chains'. Exploitation is the fundamental source of class struggle according to Marx's theory (see e.g. Boswell & Dixon, 1993).

As Marx had articulated in the 19th century the discontent arising from political oppression and economic exploitation, psychologist Sigmund Freud provided a theory

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[8] Despite the importance of Marx's theory of class struggle and revolution, surprisingly little attention has been devoted to class exploitation in cross-national studies of violent political conflict. Exceptions include Boswell & Dixon (1993) and Shock (1996).

expanding such ideas in the direction of frustration and alienation. Freud (1920/1950) regarded the tendency to seek pleasure and avoid pain as the basic goal for individuals. Frustration was expected to occur whenever pleasure-seeking or pain-avoiding behavior was blocked. He believed that the natural reaction to this state of affairs would be aggression, normally directed toward those persons or objects that were perceived as the source of the frustration.

Inspired by Freud, the most influential formulation of frustration–aggression theory was proposed by Dollard and his colleagues at Yale in 1939 with the book *Frustration and Aggression*. Their theory is quite simple. The authors' basic assumption is that aggression is always a consequence of frustration (Dollard et al., 1939/1964: 1). More specifically, the proposition is that the occurrence of aggressive behavior always presupposes frustration and, on the contrary, that frustration always leads to some form of aggression. Aggression is defined as 'an act whose goal-response is injury to an organism'

Later, Davies (1962) applied the frustration–aggression hypothesis to revolutions and developed the first concrete drafts for the theory of relative deprivation. Combining the two perspectives of de Tocqueville and Marx, Davies predicted revolutions to occur when a population is exposed to a 'de Tocqueville-effect' (a socio-economic improvement) followed by a 'Marx-effect' (a deterioration of the situation). Hence, according to Davies, relative deprivation results when expected need satisfaction increases linearly over time, whereas the actual need satisfaction levels off after some time. This leads to a growing gap between the expected and the actual, which causes frustration and mobilizes people to engage in conflict, commonly referred to as the inverse J-curve of need satisfaction and revolution (Davies, 1962: 6).

Following in the wake of Davies, Gurr (1969, 1970) developed relative deprivation theory further. For Gurr (1970) the magnitude of relative deprivation is the extent of the difference between a person's desired and actual situation. More specifically, Gurr (1970: 13) defined relative deprivation as the perceived discrepancy between people's 'value expectations' (the goods to which people believe they are entitled) and their 'value capabilities' (the goods and conditions they think they are capable of obtaining), which he saw as a fundamental and necessary precondition for civil conflict (Gurr, 1969: 596)

Most studies of inequality and conflict relate somehow to the relative deprivation theory. However, classical variants of relative deprivation theory do not explicitly focus on interpersonal or inter-group wealth comparisons (Gurr & Duvall, 1973; Hogg & Abrams, 1988; Stewart, 2009), but rather concentrate on what Boswell & Dixon (1990) refer to as 'diachronic' relative deprivation, which occurs when the standard of living decays over time. More relevant for empirical studies of civil conflict is 'synchronic relative deprivation', or simply, inequality. This variant of relative deprivation theory argues that while absolute poverty may lead to apathy and inactivity, comparisons with those in the same society who do better may inspire radical action and even violence.

Early on, the theory of relative deprivation attracted criticism from advocates of what has come to be called the 'resource mobilization'– or 'mobilization opportunity' approach to the explanation of collective violence and protest (e.g. Snyder & Tilly, 1972; Tilly, 1978). They reject grievance explanations hypotheses for the reason that inequality and discontent are more or less always present in practically all societies (see also Skocpol, 1979). Hence, they believe that the most direct and influential explanatory factors are not perceived grievances, but rather financial and political *opportunities* for mobilizing a rebel organization. Furthermore, a series of statistical studies challenged the results pertaining to income inequality, which was usually seen as the main indicator of relative deprivation (Weede, 1981). In theory there are five possible relationships between economic inequality and political conflict: positive, negative, convex (inverted U-shaped), concave (U-shaped), or null. The literature includes examples of all.[9]

The pioneering cross-national research on the inequality–violence relationship was Russett's (1964), who documented moderate correlations between inequality in land tenure systems and political instability in 47 countries. A subsequent study by Parvin (1973) came to the opposite conclusion. Working with a sample of 26 predominantly Western nations, he found that inequality proved to be only marginally significant and even inversely related to political unrest. Nagel (1974) tried to combine the two

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[9] Lichbach (1989: 436−439) identified seventeen studies which posit that the inequality–conflict nexus is positive. He found only a handful of scholars who anticipate that the relationship will be negative. As regards curvilinear relationships, Lichbach identified six studies, four of which suggest that it is convex and two which suggest that it is concave. Finally, several of the studies failed to find a significant inequality–conflict nexus at all.

assumptions and resolve the contradiction. The discontent triggered by inequality, Nagel believed, consisted of the tendency of individuals to compare wealth (a tendency he assumed was *inversely* related to the amount of objective inequality), and the extent of the grievance resulting from such comparisons (a direct function of inequality). Combining these two factors multiplicatively, Nagel suggested that the inequality-violence relationship resembled an inverse U-curve, with political violence most likely at intermediate levels of inequality. He found some support for this assumption in a study of Vietnamese provinces, but not with a cross-national sample.[10]

Sigelman & Simpson (1977) were the first to have access to personal income data. They assumed that data on income inequality would have greater violence potential than land inequality because in many societies – particularly those at higher development levels – life chances are not so closely connected to land ownership. They found some support for a linear relationship between the Gini index[11] for personal income inequality and internal war, but concluded that 'the overall level of societal well-being is a more critical determinant of political violence than is income inequality.' (Sigelman & Simpson, 1977: 124) The latter was supported in a subsequent study by Weede (1981), who found a strong impact of average income, but no effect of inequality on collective violence.

Integrating relative deprivation theory with the resource mobilization approach, Muller & Seligson's (1987) postulated that whereas a high level of income inequality nationwide would significantly raise the probability that at least some dissident groups would be able to organize for violent collective action, a high level of agrarian inequality

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[10] Davis (1948) offered another curvilinear interpretation of the inequality-violence relationship, diametrically opposed to that of Nagel. A narrow concentration of incomes, Davis believed, would spur mass resentment and lead to revolution, while a wide dispersion would endanger elite dissatisfaction and ultimately cause civil war. To the best of my knowledge, this has not been demonstrated empirically.

[11] The most common measure of income inequality is the Gini coefficient – an index between 0 and 1 (or 0 and 100) where 0 implies an egalitarian distribution (perfect equality) and 1 (or 100) indicates total concentration (perfect inequality). The Gini coefficient is defined graphically as the area of concentration between the Lorenz curve and the line of perfect equality. The Lorenz curve is a graphical representation of the proportionality of a distribution (the cumulative percentage of the values) (Lorenz, 1905). See Sen (1997) for an overview of inequality measures.

would not have the same effect on collective violence because it would be more difficult to mobilize people in the countryside. They found support for a positive relationship between income inequality and domestic conflict, whereas land inequality was found to be relevant only to the extent that it was associated with the nationwide distribution of income inequality.

With a plethora of inconsistent findings in the literature, the inequality–conflict riddle remained unsolved by the late 1980s (see Lichbach, 1989). The end of the Cold War, which entailed a new wave of ethno-national conflict, inspired Gurr (1993; 2000) to extend his previous theory on relative deprivation. He now began to focus on ethnic minorities' reactions to socioeconomic and political disadvantage as well as state-imposed discrimination, and found that ethnically-based grievances resulting from such factors contributed to ethnic mobilization and hence increased risk of collective violence. Gurr's results were in line with Horowitz' (1985) seminal study of ethnic groups in conflict.

Overall, relative deprivation theory remains the most prominent explanation that connects inequality, (as well as other grievance-related factors), with conflict. However, despite the persistence of the theme, grievance models have not fared well in the contemporary empirical literature on inequality and conflict (see Blattman & Miguel, 2010). In the mid-1990s, World Bank researchers Deininger & Squire (1996) presented a new dataset on income inequality, which was later expanded into *The World Income Inequality Database* (UNU/WIDER & UNDP, 2000). These data represent a great improvement in terms of quality and spatio-temporal coverage compared to previous datasets, and soon became the standard source of inequality data. Subsequently, in virtually all cross-country regressions of civil conflict, economic inequality is not significant.[12]

The contemporary conflict literature has been strongly marked by the pioneering works of Collier & Hoeffler (2004) and Fearon & Latin (2003). Echoing earlier critics of relative deprivation they largely dismiss grievances as causes of conflict for the reason that inequality and discontent are more or less always present in practically all societies.

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[12] The one exception I am aware of is Auvinen & Nafziger (1999). However, see Humphreys (2002: 3).

In their seminal article, Collier & Hoeffler (2004) discuss whether civil conflicts are caused by 'greed' or 'grievance'. They present two alternative explanations for civil war: atypical grievances or atypical opportunities for forming a rebel organization. Collier & Hoeffler's grievance model consists of factors such as high income inequality, a lack of political rights and ethnic and religious divisions in society. Among the 'greed' factors in their opportunity model, are access to finance, such as the scope for extortion of natural resources, and geographical factors such as the extent of mountains and forests. Relying on the Gini coefficient from the Deininger & Squire (1996) data, Collier & Hoeffler find no statistically significant effect for inequality and other proxies for grievances, such as ethnic heterogeneity, which makes them conclude that greed outperforms grievance' (Collier & Hoeffler, 2004). In more recent research, Collier has toned down the greed focus (e.g. Collier, 2007), but Collier, Hoeffler & Rohner (2009) maintain that conflict is caused by factors associated with what they refer to as 'feasibility', rather than grievances. In another influential study that focuses on political and institutional causes of civil war, Fearon & Laitin (2003) reach the same conclusion as Collier & Hoeffler (e.g. 2004), i.e. that there appears to be no cross-national relationship between inequality and conflict onset.

2.2. Problems with the inequality–conflict literature

There are a number of potential reasons why the studies reviewed here come to so different conclusions with regard to the relationship between inequality and conflict. I have divided the critique of the literature into two parts. The first considers various methodological problems, and the second provides a more fundamental critique, relating to the conceptualization of inequality, and underscores the purpose of my dissertation project.

2.2.1. Methodological objections

It has been argued that the contradictory inequality–conflict results are due to variations among the studies in all aspects of research design (see e.g. Cramer, 2001, 2003; Lichbach, 1989; Zimmerman, 1983). Various critics have suggested that the inconsistent conclusions arise from a lack of essential control variables, from the different cases and

time frames in which the effects of conflict are examined, and not least from poor data and inadequate level of analysis.

First, Zimmerman (1983) and Lichbach (1989) warn that those studies that find a positive relationship between inequality and conflict may be spurious because they failed to include control variables like the level of economic development and regime type. A related critique comes from Hegre, Gissinger & Gleditsch (2003: 257), who claim that 'scholars have focused on relative deprivation at the cost of ignoring more important explanatory factors.'

Second, the spatio-temporal domain covered by empirical inequality–conflict studies has varied greatly. Some of the recent cross-national studies have employed a global sample of states (Collier & Hoeffler, 2004; Fearon & Laitin, 2003; Hegre, Gissinger & Gleditsch, 2003). Others have focused on a restricted spatial domain (e.g. Nagel 1974; Parvin, 1973). While there may be a number of good reasons for doing so, focusing on a limited number of states makes it harder to make generalizations due to potential lack of representativeness. Also, most of the studies reviewed are cross-sectional studies, with only one year of observations for each variable. A cross-sectional study is not the best approach to analyzing domestic conflict, which may erupt at any given time during the observation period. This makes it problematic to study the relationship between inequality and conflict over time.

One of the most serious objections to previous empirical studies concerns the poor data on income inequality and the high level of missing observations. Before the Deininger & Squire (1996) dataset, and the recent appearance of the World Income Inequality Database (WIID) (UNU/WIDER & UNDP, 2000), cross-national data on inequality were distressingly scarce and imprecise. Yet, with this progress, the problem of a very large amount of missing data is still present. Deininger & Squire include inequality data from quite a limited number of countries and years.[13]

Worse than the problem of poor and missing data itself, is the problem that arises when the pattern of missing data is non-random, or biased. Many countries do not have

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[13] Deininger & Squire (1996) use an objective and valid operationalization of income inequality, but, as noted by Székely & Hilgert (1999), the observations are not always comparable because they do not necessarily refer to the same notion of income.

any inequality data at all. Trying to locate income inequality for these countries, Strand & Gates (2002) put a request to the Scientific Study of International Processes (SSIP)[14] listserv for information, and got the following answer from Phil Schrodt (quoted in Strand & Gates, 2002: 5–6):

Missing data is usually missing for a reason and this is a splendid example. Seems to me almost all of these cases fall into one of three cases:

> 1. None of your business, infidel;
> 2. None of your business, capitalist running dog CIA lackey;
> 3. We'd be delighted to give you the information, but we haven't had a decent meal in thirty years;
> 4. All of the above (Somalia).

The missing data problem relates to the 'key and the lamppost' puzzle introduced in Section 1. The point is that a situation in which we are less likely to have inequality data for conflict-ridden conflicts, this bias could imply that we infer that the effect of inequality on conflict is weaker than it actually is (see e.g. Gates, 2004).

A final methodological caveat relates to the level of analysis. Civil wars often take place within limited areas within countries. Since features of wealth and income distributions tend to vary considerably within countries, the use of national level indicators of inequality to explain variations in civil conflict is likely to be inappropriate (see e.g. Buhaug & Lujala, 2005; Buhaug et al., 2011). Based on a similar reasoning, Cramer (2001) describes the national-level Gini coefficient as a 'superficial outward sign of inequality'. His point is illustrated by the examples of Indonesia and Rwanda, which are commonly regarded as two countries with low Gini coefficients. Cramer claims that to draw from the published data on inequality that either of these two countries is a low-inequality country would be misleading, or even absurd:

> Indonesia has probably experienced rapid increase in income and wealth inequality in recent years, a fact that is directly observable to the eye in and around Jakarta, for example, with its extravagant shopping emporia coexisting with extreme poverty and, further afield, dire indigence in rural areas. Rwanda also is not quite the Cuba or Kerala of its Gini image' (Cramer, 2001: 5–6).

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[14] See SSIP homepage: http://www.isanet.org/ssip/

In other words, there may be severe inequalities locally (micro-level) even though a country, on the whole, scores relatively low on the Gini index. Cramer holds that in the majority of civil conflicts the intensity of violence is conflict at 'close quarters', i.e. about visible and felt inequalities at the local level rather than the extremes of the Gini coefficient and the ratio between earnings of the richest and the poorest quintile of the population. This line of criticism relates to the more conceptual objections discussed below.

2.2.2. Conceptual and theoretical objections

> Scholars suspect that inequality (whatever it is) is related to political instability (whatever that is), but they are not sure; nor are they sure what the relationship should look like if it is there. We have some conceptual work to do.
>
> (Linehan, 1980: 195)

The standard (technical) critiques about lacking control variables; restricted samples; poor data; and inappropriate level of analysis may apply to most of the studies reviewed. However, I contend that there is a more fundamental problem that produces the conflicting results concerning the inequality–conflict relationship: One of the most important flaws of the quantitative studies of inequality and conflict may be conceptual. My first conceptual objection is that in the inequality–conflict literature, most attention has been focused on inequality between individuals. However, the topic of interest, violent conflict, is a group phenomenon, not situations of individuals randomly committing violence against each other. Group identity is critical to recruitment and maintaining allegiance to a military organization. Hence, we should focus the attention on the relevant form of inequality – that between groups.

Such reasoning is supported by psychological experiments. For example, Brewer (1991: 478–479) concludes that individuals derive value from the group to which they belong. The willingness of individuals to make any sacrifice for group action is predicted more by a sense of collective rather than individual relative deprivation. Improvement of the group's condition, in other words, may be a more powerful motivation to participate in collective actions than improvement of the individual's condition. I recognize that ethnic or religious groups are to some extent socially constructed, sometimes with fluid

membership. Nevertheless, the relative performance of identity groups is an important source of individual welfare, and can hence cause serious conflicts where structural economic or political differences coincide with cultural cleavages (see e.g. Stewart, 2002). Also, as demonstrated in Section 1, vertical and horizontal inequalities do not necessarily overlap.

My second conceptual objection concurs with Sen (1992) and Stewart's (2002) complaint that most studies of the relationship between inequality have exclusively focused at *economic* inequality (usually measured by income). I have consistently talked about horizontal *inequalities* in plural. This choice of words is not incidental. In order to fully explore the inequality–conflict nexus, one should study various dimensions of inequality in addition to the strictly economic dimension. Sen (1992) asks an essential question: 'Equality of what?' Given the fact that the human population is different in many respects, it is important to remember that inequality can be much more than just income inequality measured by e.g. the Gini index. Sen (1992; 2006) focuses on three different categories (or 'spaces') of equality: equality of income or other financial assets; equality of welfare and equal rights and liberties, and argues that the various categories of equality cannot be combined perfectly, since the differences in environmental factors and human capacities influence the final outcome. Stewart (2008) also stresses that horizontal inequalities are multidimensional – with political, economic, and social elements (as indeed are vertical inequalities, but they are rarely measured in a multidimensional way).[15]

Finally, it has been argued that the general lack of theory and explanation is a fatal flaw of many statistical models of the inequality–conflict nexus (Lichbach, 1989). Many studies begin by assuming that there is such a relationship – often citing one of the 'classics' à la Russett (1964) – and then jump straight to the empirical analysis, leaving unexplored what Elster (1983) refers to as the 'black box' in the causal chain. In other

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[15] One study of the relationship between vertical inequality and conflict, however, stands out in this regard: de Soysa & Wagner (2003) test the effect of vertical schooling inequality rather than income inequality on conflict, using data from Castelló & Doménech (2002) on differences in educational attainment. The authors even argue that this vertical measure captures some of the logic of horizontal inequalities, due to the assumption that dominant ethnic groups control state resources and often use education policies to discriminate against minorities.

words, the reasoning behind the various propositions – *how* and *why* inequality breeds conflict, has typically been lacking.

For the reasons presented above, we cannot conclude from the extant literature that inequality is unrelated to political violence. On the contrary, I argue that the rejection of the inequality–conflict nexus is at best premature. Indeed, it could be the case that vertical inequality in a homogenous population, despite the class differences it engenders, does not seriously increase the risk of conflict, but that could still leave a role for group inequality. Yet, most of the contributions reviewed here ignore how different dimensions of inequality are institutionalized and shaped by history and various social and cultural cleavages, and how such inequalities can be translated into collective violence. A more promising avenue to capture the inequality–conflict link has been taken by Stewart (e.g. 2000, 2002, 2008) and her collaborators, who focus on the role of horizontal inequalities, or 'inequalities in economic, social or political dimensions or cultural status between culturally defined groups' (Stewart, 2009: 3). In the next section I outline the theoretical framework which has guided this project. My argument demonstrates how horizontal inequalities can spur violent group mobilization through both grievance-based and opportunity-based mechanisms.

3. Theorizing Horizontal Inequalities and Political Violence

Although the concept of horizontal inequalities is quite new, there are clear synergies between this and other approaches to understanding multidimensional inequalities and the dynamics of violent group mobilization in ethnically heterogeneous countries. For example, Barrow's (1976) concept of 'ethnic group inequality', Horowitz's (1985) 'ranked ethnic groups'; and Tilly's (1999) 'categorical inequalities' describe similar inter-group inequalities. Gurr's (1993) concept of 'relative deprivation' as a cause of minority rebellion represents another related perspective. As noted in the last section, the general concept of relative deprivation is often conceived of as diachronic, or inter-temporal, often measured in terms of economic growth (or the lack thereof).

Less commonly discussed, but more important for empirical studies of inter-ethnic conflict is what Boswell & Dixon (1990: 542) refer to as synchronic relative deprivation, which is usually measured in terms of income distribution. Add to this that the literature has distinguished between individual vs. collective relative deprivation.

According to social identity theory individuals' investment in their membership group and the salience of group boundaries increase the likelihood that relative deprivation will be experienced in its collective form (Walker & Smith, 2002). Yet, most studies of inequality and conflict operationalize relative deprivation at the individual level by various measures of vertical inequalities, such as the Gini coefficient.

If we combine the distinctions diachronic/synchronic and individual/collective relative deprivation in a 2x2 matrix (see Table 1), alternative d) – collective synchronic relative deprivation – comes closest to the concept of horizontal inequalities. However, there is one important feature that distinguishes the HI approach from that of relative deprivation (see Stewart, 2008). Whereas relative deprivation theory by definition focuses on the motives of the disadvantaged in society, the HI thesis stresses that it is not only resentment among the deprived that may cause political instability – although this clearly seems to be the case in many disputes (e.g. the Hutus vs. Tutsis in Rwanda or race riots in industrialized countries). The relatively privileged can also attack the unprivileged (or the state) as a reaction to what they may perceive of as unfair redistribution, or out of fear that the relatively deprived may demand more resources and political power (e.g. the Biafra war in Nigeria, or the Basque conflict in Spain).[16]

Table 1. Typology of Different Forms of Relative Deprivation[17]

Aggregation level / Time perspective	Individual	Collective
Diachronic	a) Intra-Individual	b) Intra-Group
Synchronic	c)Inter-Individual	d) Inter-Group

3.1. Origins of horizontal inequalities

There can be many causes and origins of systematic differences between different ethnic, religious groups, or regions. They relate to different factors such as ecological and climatological differences, the distribution of natural resource endowments, the

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[16] In fact, Gurr's (2000) minorities at risk also include advantaged minorities like the Sunni Arabs of Iraq and the overseas Chinese of Southeast Asia, but his focus is on relative deprivation, i.e. that these groups are vulnerable to challenges from disadvantaged groups.

[17] The labels 'diachronic' and 'synchronic' RD stem from Boswell & Dixon (1990: 542).

differential impacts of colonialism, as well as various economic policies (Brown & Langer, 2010: 30). Horizontal inequalities often have their origin in historical circumstances – often colonial policy which privileged some groups over others. Sometimes, however, horizontal inequalities are not caused by deliberate agency at all but simply become evident for example when traditional peoples on the periphery of modernizing societies are drawn into closer contact with the more powerful and technologically proficient groups (see Gurr, 2000). Furthermore, as shown in Chapter 3, HIs tend to reproduce over time, sometimes lasting for decades. An initial advantage often leads to long-term cumulative advantages, as resources and education allow the more privileged groups to secure further advantages (Stewart, 2009). For example, children growing up in poor communities usually have less access to good schooling and must travel further, in social and geographical terms, to raise their own children out of poverty. According to HI theory, the risk of violent group mobilization should be higher when people are convinced that their socioeconomic deprivation is caused by deliberate discrimination by the state. Conversely, if a country's government introduces policies designed to reduce HIs, this may reduce the political salience of the prevailing HIs, even when the actual redistributional effect is rather limited (Brown & Langer, 2010: 31).

3.2. The formation of identity groups

For a group to mobilize, it first needs a common identity and unifying structure among its members (Tilly, 1978: 84). Gellner (1964: 149) states that there is a human need to 'belong, to identify and hence to exclude'. An intrinsic part of life is group membership – in fact it is what makes up the identity of individuals. Since groups are the central building blocks in thinking of horizontal inequalities it is important to understand how groups are formed. Although personal motivation also obviously plays a part in causing people to fight (Collier & Hoeffler, 2004), I assume that in many conflicts people are primarily motivated and organized based on their group identity. In order to mobilize a group there must be some way that it is differentiated from the other groups. Depending on the context, most people have multiple affiliations and identities – some locally based, some family based, some age or class based, and some culturally and ethnically differentiated (see, e.g. Smith, 2001). Some identities are fluid, short-lived and insignificant (for example, being a member of a sports club or a students' choir), whereas

others are more permanent and personally and socially more significant, such as gender, ethnicity, religion or regional belonging (Stewart, 2008).

Psychologists and other social scientists of diverse orientations have developed a variety of theories regarding the development and functions of identity. In particular, much social psychological research has shed light on the ways in which individuals and groups' efforts to establish and maintain secure identities can produce conflict between identity groups (Seul, 1999). It is common to distinguish between individual and group identity, though the two levels of analysis are integrally and reciprocally related to each other. Individual identity refers to the relatively stable elements of an individual's sense of self (Seul, 1999). The contents consist of one's values, motives, emotions, feelings, attitudes, thoughts, goals and aspirations on the one hand, and one's group memberships, social influence and roles, on the other. A group is a self-defining collection of individuals. Like an individual, a group can be said to have an identity of its own (Seul, 1999: 556). The identity of a group is born and communicated by the group's members, but it cannot be thought of as the sum of the members' respective individual identities, nor is an individual's identity merely a composite of the identities of the various groups to which one belongs. According to Kelman (1998: 16) a group's identity consists of the members' shared 'conception of its enduring characteristics and basic values, its strengths and weaknesses, its hopes and fears, its reputation and conditions of existence, its institutions and traditions, its past history, current purposes and future prospects'. Like individual identity, group identity is fluid and dynamic, and levels of involvement and emotional commitment may differ widely among the group's members.

Incompatible interests may be the apparent cause of conflict among groups in many cases, but conflict, arguably, will not occur in the absence of some inter-group identity competition. Tajfel & Turner (1986: 23) hypothesize that when a group's action for positive distinctiveness is frustrated, impeded or in any way actively prevented by an out-group, this will promote overt conflict and hostility between the groups. Accepting that groups are central, the critical question is why and how some groups are perceived as socially significant, and others not.

3.3. Theoretical perspectives on ethnic identity and conflict

Ethnicity is a group identifier that is often assumed to influence behavior and well-being in a significant way, and hence form the basis of identity conflicts. Hence, before discussing the mechanisms that link horizontal inequalities to violent group mobilization, it is useful to address the epistemological question about the nature of ethnicity and how it relates to political violence. Theoretically, there are three broad approaches to the study of ethnicity and conflict (see e.g. Ferguson, 2003; Gurr, 1993; Lake & Rotchild, 1998). In an influential argument, Young (1993) sorted accounts of ethnic identity under the labels 'primordialism', 'instrumentalism', and 'constructivism' (see also Ellingsen, 2000).

At one extreme, the so-called *primordialist* approach takes ethnicity as a fixed characteristic of individuals and communities, defined by one's descent (e.g. Geertz, 1963; Smith, 2001; Vanhanen, 1999).[18] Primordialist analyses of conflict stress the uniqueness and overriding importance of ethnic identity. In this view ethnic affiliations are highly emotionally charged and, on some accounts, irrational. Although the primordialists recognize that ethnic warfare is not a constant state of affairs, primordialists see conflict as resulting from ethnic differences and, consequently, not in need of further explanation. In other words, conflict is understood as being ultimately rooted in ethnicity itself. For the primordialists ethnic conflict is something apart; what one learns about ethnic conflict is typically not relevant to other social, political or economic conflicts (Lake & Rotchild, 1998).

The most common criticism of the primordialist approach is its assumption of fixed identities and its failure to account for variations in the level of conflict over time and place. The primordialist view does not explain why ethnic groups change over time.[19] Nor does it explain why some multiethnic countries live in peace, while elsewhere violence erupts. As shown above, the great majority of ethnically oriented conflicts within nations have not led to violence (see also, Licklider, 1998). Yet another criticism

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[18] In its extreme form, primordialism wanders into the zoological gardens of socio-biology. In 1978 Pierre Van den Berghe suggested that ethnicity should be understood as an extended form of kin selection, best grasped as an elemental instinctual impulse (Van den Berghe, 1978).

[19] Cohen (1969), for example, describes how some rural people moving to towns in Nigeria became 'detribalized' whereas tribal identity became more important for other urbanized Nigerians.

has been articulated by Horowitz (1998), who notes that primordialists have not provided a satisfying explanation as to *why* ethnic affiliations are so emotive.

The second approach to ethnic identity, *instrumentalism*, is founded in a political economy paradigm and rooted in the premise that social action rests on rational calculation. This approach originally arose as a challenge to modernization theory, which held that social and economic development would level out ethnic differences and replace them with class stratification. Rotchild (1981) claims that the ambivalent effects of modernization create opportunities for political and cultural entrepreneurs to mobilize ethnicity into an effective instrument of political leverage and economic interests. *Instrumentalists* thus (e.g. Bates, 1986; Rotchild, 1981) see ethnicity as being used by groups and their leaders in order to achieve political or economic goals. In this view, ethnicity has little independent standing outside the political process in which collective ends are sought. Ethnicity is primarily a set of symbolic ties that may be used for political and economic advantage – like political party affiliation. For instrumentalists, conflicts are largely stimulated by elites who mobilize ethnicity in pursuit of their own interests. Hence it seems to be the case that, for the instrumentalists, ethnicity is 'there' to be exploited at will. In contrast to the primordialist view, instrumentalists believe that politicized ethnicity is not inherently different from other forms of political association, and that the knowledge deduced from ethnic conflicts may also be applied to other kinds of conflicts.

Critics of instrumentalism argue that ethnicity is not something that can be decided upon by individuals at will, like other political affiliations, but that it is rooted in the larger society. While people have some choice over their own identities, this is not unconstrained (Stewart, 2002). For example, in Kenya, a Kikuyu cannot decide that from tomorrow he will be a member of the Kalenjin ethnic group. Choice of identity is constrained both by characteristics of the group (its customs, symbols, norms etc.) and by other groups' willingness to admit new members. Critics of instrumentalism focus on the inherently social nature of ethnic identity, and argue that ethnicity can only be understood within a relational framework (Lake & Rotchild, 1998).

Finally, bridging the other two perspectives, *constructivists* emphasize the social origins and nature of ethnicity (e.g. Anderson, 1991; Gurr, 2000; Hobsbawn & Ranger, 1983). Arguing that ethnicity is neither immutable nor completely open, this approach

represents an emerging scholarly consensus. Distinguishing their position, the constructivists believes that while identities can be reshaped, they can only be altered at significant costs (Bates, 2004). The constructivists believe that ethnicity is not an individual attribute but a social phenomenon – a person's culture is partly inherited, but also constructed and chosen, with many people having multiple identities. It is widely agreed that many tribal distinctions in Africa, for example, were invented by the colonial powers. An example is the distinction between Hutus and Tutsis in Rwanda, largely invented by the colonial powers for administrative conveniences (Stewart, 2008). For the constructivists, thus, a person's identity remains outside the control of that individual. In accordance with the instrumentalist view, constructivists do not see ethnicity as inherently conflictual, and believe that it is the *social system* that breeds conflict rather than individuals (Lake & Rotchild, 1998). In the constructivist view, accounts of ethnic conflict are generalizable, but only to other conflicts that are largely based on socially constructed groups and cleavages. From the perspective of horizontal inequalities, this provides a causal explanation of why HIs may lead to conflict, with the possibility of ethnic conflict being driven by structural inequalities in society.

Although clearly important, ethnicity is not the only relevant group distinction with regard to horizontal inequalities and conflict (see Stewart, 2008). However, ethnicity often overlaps with other fundamental factors like language, religion, and territorial affiliation. Furthermore, ethnic cleavages in developing countries are often ambiguous, and whether perceived distinctions become considered as 'ethnic' may in part depend on past conflict and lack of integration (Barth 1969; Tronvoll, 2009). Ethnicity is a slippery concept and can be difficult to define in certain contexts. This is not the case with e.g. administrative boundaries, which are usually (but not always) unambiguously defined.

The case-based literature shows how a number of various group affiliations have been the source of group differentiation and mobilization, and how group-identifiers may overlap with each other (see e.g. Stewart, 2008). In some cases, the main source of affiliation has been ethnicity (as in Rwanda & Sri Lanka). In other cases religion is the binding factor (e.g. Protestant/Catholics conflicts in Northern Ireland and Muslim/Hindu conflicts in India). Sometimes, the binding agent seems to be race (for example in Fiji), and sometimes, it is clan (for example, in Somalia), while in Central America, group identification and organization has developed along social class lines

(which tends to overlap with ethnicity).[20] Finally, regional location is also a source of group differentiation, which often overlaps with ethnic or linguistic cleavages, as for example in Uganda (Minority Rights Group International, 1997) and Zambia (Posner, 2004). In Østby, Nordås & Rød (2009 [Chapter 4]) we make the case why regions matter in a horizontal inequality framework.

3.4. *How do HIs spur collective action?*

When and how does ethnic identity (or other group identities) lead to political violence? A constructivist answer to this question would be: when ethnicity has collective consequences for a group in its relations with other groups. More specifically, to the extent that ethnicity is a major determinant of a group's security, status, material well-being, or access to political power, it is likely to be a salient part of their identity. In line with such reasoning we need to go beyond the sheer cultural differences to understand the causes of conflict. The literature on political violence emphasizes two factors in addition to a shared *identity* that may lead to group mobilization: *grievance* (or frustration; resentment) and *opportunity* (see e.g. Ellingsen, 2000; Gurr, 1993; 2000). These concepts serve as the main guideline for my understanding on how horizontal inequalities influence collective action. I posit that all three factors operate interdependently and that they can be incorporated in a synthetic model of horizontal inequalities and collective mobilization.

Stewart (2000) argues that in societies where economic, social and/or political inequalities coincide with ethnic cleavages, identity can be a mobilizing agent that can lead to political violence. Her theory of horizontal inequality and conflict hence combines elements from both social identity theory and theories of relative deprivation. Gurr (1993: 127) explicitly argues that ethnic identities and grievances mutually reinforce each other: 'a group's grievances and potential for political mobilization are both influenced by the strength of group identity. Accepting Gurr's reasoning, horizontal

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[20] A former colleague who recently came back from fieldwork in Bolivia where he had interviewed people about perceptions of ethnicity told me that one informant had categorized the main ethnic groups in Bolivia as follows: 'Well, we have the Aymara, the Quechua, and the private sector...'.

inequalities may increase both the perception of a common identity *and* the level of group grievances.

As mentioned above, conflict can be initiated by both relatively deprived and relatively privileged groups, although the literature tends to focus mostly on the former. According to Horowitz (1985) in stratified social systems, social comparison reflecting superiority or inferiority should be particularly likely to trigger conflict. Gurr (2000) found that ethnic groups are in fact often subject to economic discrimination to the extent that their members have been systematically limited in access to desirable economic goods and conditions. Whether such inequalities are due to overt discrimination or not, unequal access to economic resources by different groups can provoke collective grievances. In the words of Petersen (2002: 40) 'resentment is the feeling of being *politically* dominated by a group that has no right to be in a superior position'. Conversely, groups that are relatively advantaged may also experience collective grievances due to their fear that the deprived groups may gain political power and demand more resource redistribution, or turn to armed aggression to redress their grievances. For example, inter-regional inequality is often associated with inter-regional transfers from richer to poorer regions. If richer regions view these transfers as too large, this increases grievances, and they may seek to secede or they can push poorer regions to exit by insisting on a lower transfer rate (Sambanis & Milanovic, 2004: 24). In a study of conflict in Indonesia, Tadjoeddin (2007: 23) refers to the demands of the richer regions for a degree of community welfare that corresponds to their relative high regional prosperity as 'aspiration to inequality'.

It is clear that collective grievances do not automatically lead to violent action. Without resources and organization, grievances as such can do little to challenge powerful defenders of the status quo (Tilly, 1978). Even Gurr (1970) admitted that affect and frustration are insufficient to create rebellion. In his rational-actor model of political violence, Tilly (1978) argued that only when resources, organization and opportunity become available, people will mobilize for collective action, including rebellion, if they calculate that it is in their interest to do so. For Tilly (1978: 7) opportunity concerns the relationship between a group and the world around it. It may come from a government weakness or an opposition organization's calculation of its own strength. Gurr (1993: 130), on the other hand, distinguishes between *internal* and *external* opportunities for a

group to mobilize. Opportunity factors internal to the group are the salience of group identity, networks among its members, and the extent of common grievances. Opportunity factors external to the group include the character of the state and its resources, and whether the group has transnational kindred.[21] According to Gurr's theory (2000: 95), the factors of identity and frustration are innate in a group's internal opportunities to mobilize.[22] Internal opportunities are seen as the elements from which skilful leaders and ethnic entrepreneurs build collective movements for political action. However, the timing of action and the choices of strategies of participation, protest, or rebellion, depends largely on political and economic opportunities *external* to the group, such as the repressiveness of the state (see also Østby & de Soysa, 2008).

Whereas political scientists such as e.g. Gurr (2000) tend to focus mainly on the *political* opportunities external to the group (e.g. regime type), economists such as e.g. Collier & Hoeffler (2004), focus more on *economic* opportunities (e.g. in terms of the extortion of lootable natural resources), treating the objective of rebellion as financial gain. I argue that the level of socioeconomic horizontal inequalities may influence a rebel group's calculation of its own strength and thus serve as an indicator of the internal opportunities for a group to mobilize. Soldiers must be paid, and the cost of recruiting is related to their income forgone by enlisting as rebels. By definition, richer groups are more capable of supporting a rebel group with economic contributions and other kinds of material resources. On the other hand, members of the relatively disadvantaged groups are more likely to enlist as rebel recruits due to lower opportunity costs, and because perceptions of injustice generate grievances that serve as a strong tool for recruitment. A final source of opportunity, or the ability to take coordinated action, is strong group cohesion, which in turn is often strengthened by commonly felt grievances.

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[21] In a later work, Gurr (2000) uses a different terminology, distinguishing between capacities (similar to internal opportunities) and opportunities (similar to external opportunities) for group action.

[22] According to Gurr (2000: 65−95) the salience of group identity is partly attributed to previous collective disadvantages. Collective grievances and identities in their turn form the basis of a group's capacity, or internal opportunities for mobilization. Gurr's (2000: 74) admits that there is no single answer to this 'chicken-and-egg problem' concerning the root causes of ethnic conflict.

Figure 4 summarizes my argument, visualizing how various aspects associated with horizontal inequalities influence the risk of violent group mobilization.[23]

Figure 4. Mechanisms Linking Horizontal Inequalities to Group Mobilization
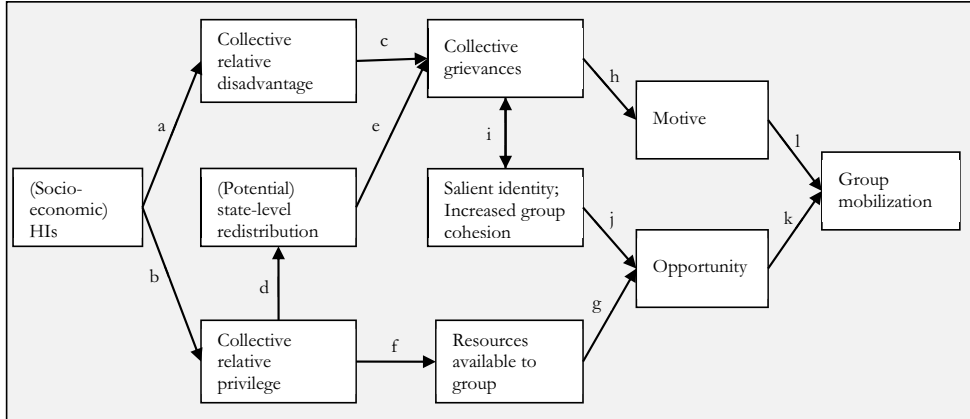
In short, Figure 4 shows that HIs imply both relative deprivation (a) and relative privilege (b). RD may lead directly to collective grievances (c), whereas relative privilege is assumed to lead to grievances via fears of potential or real redistribution (d, e). Grievances of both types are likely to constitute a strong motive for collective action (h). Furthermore, richer groups have more material resources (f) which implies better opportunities to establish and sustain a rebellion (g). But opportunity can also increase as a result of strong group cohesion (j) which in turn is reinforced by and reinforces collective grievances (i). Finally, violent group mobilization is assumed to result from a combination of opportunity (k) and motive (l). I have not been able to test each individual mechanism (arrow) in this dissertation, but together they form the theoretical basis from which I develop my hypotheses.

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[23] For the sake of simplicity, I have kept the boxes in Figure 4 to a minimum, although one could have envisioned additional boxes and arrows. For example the figure only includes socioeconomic HIs, despite the fact that I have stressed the multidimensionality of HIs. However, the two types of HI are often likely to be interrelated and the main focus in this thesis is on economic HIs. See also Langer (2005), who argues that political HIs are of great importance to leaders, whereas socioeconomic HIs matter more for the masses.

3.5. Contextual factors

Clearly, horizontal inequalities do not exist in a vacuum. If HIs are really as static and persistent as they are often considered to be (see e.g. Ahluwalia 1976; Østby, 2008b [Chapter 3]; Williamson, 1965), there is good reason to assume that contextual factors play a significant role in translating HIs into conflict, particularly with regard to state regulation and the absolute economic level. In this thesis I particularly focus on three mediating aspects of relevance: political conditions (regime type, electoral system, political exclusion) (see Chapter 3); economic conditions (natural resources) (see Chapter 4), and demographic factors (population growth) (see Chapter 6). I elaborate more on the postulated interaction effects between socioeconomic HIs and these factors when I describe the contributions of the individual chapters in Section 5.

3.6. HIs and different types, features and locations of political violence

Since the group perspective is so integral to the thinking of HIs, it seems almost intuitive that HIs should be particularly relevant for politically motivated violence directed against the state or other group(s). Vertical inequality, on the other hand, should be more likely to spur violent actions that lack a clear political basis, such as crime (e.g. Fajnzylber, Lederman & Loayza, 2002; Hagan & Peterson, 1995; Neuman & Berger, 1988).

Although I argue that HIs should be more relevant for political violence than pure crime, the assumption that HIs may provoke politically motivated violence is rather general, as the term 'political conflict' encompasses a wide range of empirical phenomena. The HI literature (e.g. Stewart, 2008) is not very specific as to what kind of political violence may be caused by horizontal inequalities. Stewart's (2002) broad conclusion based on a number of case studies is that HIs may provoke a range of different forms of violence ranging from riots to civil war.

It is beyond the ambition of this dissertation to construct a thorough typology of political violence and discuss and test whether and how HIs are associated with each possible variant of this broad phenomenon. Nevertheless, the various chapters jointly offer some discussion and insights as to whether HIs are more likely to produce some forms of violence rather than others. For example, in Chapter 6 (Østby et al., 2011) we postulate that HIs should have stronger effect on episodic violence than on routine violence. The rationale behind this expectation is that low-scale 'routine' violence usually

has little or nothing to do with ethnic groupings. In line with this, Tadjoeddin, Chowdhury & Murshed (2010: 6) hold that 'horizontal inequality is more appropriately located in the context of secessionist and ethnic conflicts', an argument that resonates with recent findings by case studies (e.g. Sambanis, 2004b).

With regard to conflict dynamics, HIs could also have different effects on the various stages of conflict. The features shaping how and why conflicts escalate or spread after the initial outbreak can differ significantly and take place in very different areas. Subsequent conflict events are likely to be influenced by strategic considerations and subsequent battle outcomes, and may often take place in remote and scarcely populated areas that bear little if any resemblance to the initial area where violence emerged. It is my assumption that HIs should be particularly relevant for conflict onset since HIs are assumed to form a strong motive for group mobilization in the first place. A related argument is more fully developed in Hegre, Østby & Raleigh (2009) [Chapter 5].

Related to the discussion of HIs and various forms of political violence, a final point hinges on geographical features, such as the urban–rural distinction. As noted by Moser (2004), inequalities are generally more marked in urban than in rural areas. Add to this Brown's (2008) observation that the experience of horizontal inequality is rooted in locality and day-to-day interactions. The implication of this is that HIs should be particularly pronounced and visible in cities (which are often highly demographically heterogeneous) and produce higher risks of urban violence, due to e.g. systematic differences between migrants and born city dwellers. This line of reasoning is not new. Sociological research in the United States has long identified 'racial inequalities' as an important explanatory factor behind interracial violence in urban areas (e.g. Blau & Blau, 1982; McCall & Parker, 2005). These accounts broadly point to issues of 'racial competition' between ethnic groups as key to understanding the dynamics of urban violence. In Chapter 7 (Østby, 2010) I explore the effects of HIs on urban violence.

Despite these modest efforts at theoretically linking HIs to certain forms, features and locations of political violence, the dissertation primarily uses these typologies for heuristic purposes in constructing the research designs underlying the various articles.

3.7. Evidence of HIs and conflict

Upon closing this theory section it is timely to briefly review the existing empirical evidence on horizontal inequalities and political violence.[24] As we have seen, so far, most researchers of the horizontal inequality–conflict relationship have relied on qualitative case studies rather than large-N comparisons (e.g. Humphreys & Mohamed, 2005; Langer, 2005; Sambanis, 2004b; Stewart, 2002, 2009). The main picture that emerges from these works is that horizontal inequalities are indeed associated with increased risks of political violence, ranging from occasional racial riots (e.g. Malaysia) to full-blown civil war (e.g. Uganda, Sri Lanka, South Africa). Another conclusion from this research is that both disadvantaged and advantaged groups have a higher likelihood of getting involved in internal conflict than groups closer to the national average (Stewart, 2009). There is also a handful of quantitative case studies that have addressed the HI–conflict link (e.g. Barron et al 2009; Mancini, 2008; and Østby et al., 2011 [Chapter 6] on Indonesia; Hegre, Østby & Raleigh, 2009 [Chapter 5] on Liberia; and Murshed & Gates, 2005 on Nepal), and these studies largely support the conclusions from the qualitative literature.[25]

Although Stewart (2000) was the first analyst to explicitly use the term 'horizontal inequalities', a handful of scholars have attempted to study structural differences between ethnic groups on a cross-national (or cross-group) basis. In a pioneering test Barrows (1976) analyzed the determinants of political instability in 32 African states south of Sahara during the 1960s. In a multiple correlation analysis he found that inequality was a consistent predictor of political instability when measured along a scale of 'ethnic group inequality' based on 'the size of ethnic groups and their share of political power and/or other values [wealth, education and the like]' (Barrows, 1976: 154–155). Barrow's study is particularly noteworthy since it appears to be the first attempt at measuring horizontal inequalities quantitatively. A major problem with his index, however, is that his personal judgment was the only source for determining the group inequality scores for each country. Another early strand of research on HIs and violence is Blau & Blau (1982) and others' work on Black/White relationships in US

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[24] See Brown & Langer (2010) for a more extensive review of this literature.

[25] The one exception I am aware of is Barron, Kaiser & Pradhan's (2009) study of local conflict in Indonesia, which found that educational HIs were associated with *lower* levels of conflict in rural areas.

cities, which explores whether riot incidence is related to economic and social characteristics of the cities, including horizontal inequalities. Though some of these studies have led to mixed results (see Balkwell, 1990: 54–55) the general finding is a positive relationship between racial inequality and violence.

More recently, Gurr (2000) has developed an index of political, economic and cultural disparities for some 275 minority groups in 116 countries. Based on the 'Minorities at Risk' (hereafter MAR) database, he found that where there are strong identities *together with* large group grievances (i.e. major political, economic, or cultural differences/discrimination), protest is more likely. Gurr's data, thus, provide strong support to the hypothesis that horizontal inequalities are liable to lead to political violence.[26]

My own cross-national investigations (Chapters 2–3) represent the first attempts at measuring objective HIs based on data from household surveys. The main conclusion from these and subsequent disaggregated studies (Chapters 4–7), as well as a recent paper by Condra (2009)[27] largely support the validity of the positive relationship between various forms of HIs and conflict. A different approach is taken in a very recent paper by Cederman, Gleditsch & Weidmann (2010), who provide global evidence for the HI–conflict nexus, based on geographically disaggregated data on economic output. Although the work presented in this dissertation and that of Cederman, Gleditsch & Weidmann (2010) differ considerably with regard to the operationalization of HIs, which will be discussed further in the next section, we do reach the same overall conclusion: HIs indeed seem to matter for political violence.

4. Analytical Approach, Data and Measurement

The issue of methodological design is central in the debate on the relationship between horizontal inequalities and political violence. So far, the field has been methodologically dominated by carefully selected qualitative case studies of particular countries, groups

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[26] Despite its wide use and great potential, however, MAR suffers from some fundamental flaws (see Section 4).

[27] However, Condra's (2009) results suggest that it is not the poorest groups in the country that are at highest risk of rebellion, but rather those that are relatively better off.

and conflict settings, which by definition imply restricted scope conditions and limited claims to generalizability. Given that there are a number of plausible mechanisms (and a wealth of empirical material from case studies) that link horizontal inequalities to conflict, testing the statistical relationship between HIs and political violence is much warranted.

This PhD project includes the first systematic cross-national and subnational tests of the horizontal inequality–conflict link, relying on HI data based on national household-level data from the Demographic and Health Surveys (DHS). Recently, a paper by Cederman, Gleditsch & Weidmann (2010) has taken an alternative approach to the measurement of socioeconomic HIs, combining new geographical data on wealth and ethnic groups' boundaries. In this section I will discuss some strengths and weaknesses of my own approach compared to that of Cederman, Gleditsch & Weidmann (2010). But first, I will comment briefly upon some other important aspects pertaining to the collection of HI data: potential data sources, measurement issues, and levels of analysis.

4.1. Where to find data on HIs

Gurr's Minorities at Risk Database (1993, 2000) is the first worldwide dataset providing group-level inequality data. Despite its wide use, however, MAR suffers from fundamental flaws, notably selection on the dependent variable, i.e. focusing exclusively on groups that are *at risk* of engaging in conflict.[28] This exclusion of apparently 'non-relevant' ethnic groups may be quite problematic mainly because what we want to capture is not only actual conflict but also potential conflict. Furthermore, the various indicators of relative group grievances provided by MAR are quite crude and are largely based on statements and actions by group leaders and members (Minorities at Risk, 2009: 12), which produces rather subjective evaluations of group deprivation.[29]

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[28] The Minorities at Risk Database is currently being expanded to collect data on *all* ethnic groups worldwide – both minorities and majorities (see Birnir & Inman, 2010).

[29] Since the MAR dataset mostly focuses on relatively deprived groups it is not possible to single out the level of privilege among the groups that are better off than the national average, as these are simply coded as having 'no grievances'.

It is a formidable challenge to get at objective and comparable data on horizontal inequalities. Horizontal inequalities (and vertical ones alike) can be politically sensitive, and national and subnational governments are likely to report biased data if any. My solution to the problem has been to construct HI data based on national surveys which include information on both socioeconomic well-being and ethnic/religious/regional group affiliations. More specifically, the data presented herein is based on a large number of national household surveys provided by the Demographic and Health Surveys (DHS) project.[30] Biased information is very unlikely when data are generated from national surveys like the DHS, as the original intention behind these was far from assessing socio-economic inequalities between ethnic groups. Finally, the aggregation of survey data ensures *descriptive* rather than *evaluative* data. That is, researchers do not need to rely on their personal judgment as the sole source for determining group inequality scores (as opposed to e.g. the Minorities at Risk project).

Since 1984, the MEASURE DHS project, funded by USAID and implemented by ICF Marco, has administered more than 240 surveys in over 85 countries (see Figure 5), collecting accurate, nationally representative data on fertility, family planning, maternal and child health, gender, HIV/AIDS, malaria, and nutrition.

Figure 5. Countries Covered by the DHS Project as of 2009

Source: DHS webpage: http://www.measuredhs.com/aboutdhs/.

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[30] DHS webpage: http://www.measuredhs.com.
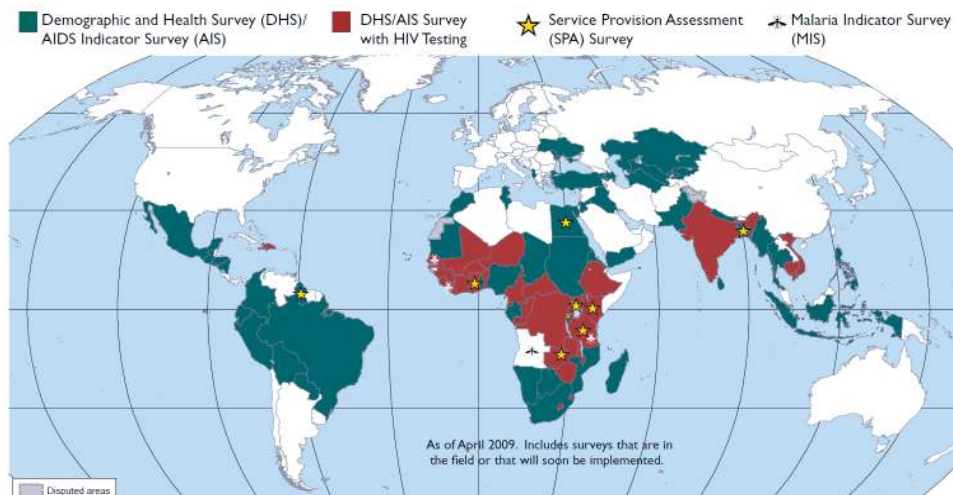
The DHS surveys were conducted primarily to provide researchers and policy-makers with comprehensive and comparable data on fertility and child health and their determinants, and the DHS project has become the gold standard of survey data in the population and health sector in developing countries. However, the content of the surveys has changed over the years to adapt to changing circumstances and priorities. Apart from health and nutrition indicators, most of the surveys also include a host of question relating to socioeconomic background factors such as the possession of various household assets (such as electricity, radio, and refrigerator) and education levels. Furthermore, the surveys include information about the region of residence of the respondents, and sometimes, but not always, information of ethnic and religious affiliation. In a typical DHS survey, a sample of households is selected throughout the entire country and then interviewed using a household questionnaire to collect housing characteristics. Women between the ages 15 and 49 are interviewed using a women's questionnaire to collect information mainly on background characteristics, children and women's health and other issues, such as education level. Samples vary considerably in size, ranging from less than 5,000 women (e.g. Ghana DHS 1998) to more than 120,000 (e.g. India DHS 2005–06).[31] During the last decade, the DHS has begun to include detailed information about the geographical location of each EA. Together, these surveys provides a very rich dataset, from which one can construct reliable and valid[32] group inequality indicators.

Stewart (2002, 2008) proposes a very rich scheme for operationalizing horizontal inequalities. Within the four main categories economic, social, political, and cultural status HIs she fits a range of variables, some which are easily measured (such as composition of government), and some which are harder to measure (such as human capital). However, Stewart does not study the same types of inequalities in each country. For example, in her study of horizontal inequalities between indigenous and Latinos in Chiapas, Mexico, she measures inequality in terms of income, illiteracy and household access to safe water. In South Africa, on the other hand, she focuses on the differences between the Black and White population in terms of managerial and civil service

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[31] See Demographic and Health Surveys (1996) for details on the sampling methodology.

[32] See Chapter 2 for more details regarding the reliability and validity of the DHS data.

employment, life expectancy and literacy. Clearly, the richness of Stewart's scheme for operationalizing horizontal inequalities is well suited for individual case studies. However, such broad conceptualizations of HIs are not particularly useful when the goal is to evaluate the generalizability of the horizontal inequality–political conflict nexus across many cases.

For the various chapters in this dissertation, I have applied the DHS surveys to generate HI measures based on different group indicators: ethnicity, religion, region or locality of residence, and migrant status. I have also measured inequalities along various dimensions, using variables pertaining to household assets, educational levels, and infant mortality rates (see Table 2 in Section 5).

4.2. *Measurement of HIs*

Even with adequate group-level data for various dimensions of socioeconomic well-being, there remains the challenge of measuring HIs at the aggregate level. In order to compare HIs across different countries a standardized measure is needed. For a comparison of two groups, the simplest measure conceivable would be the following:

$$HI = 1 - \frac{worst}{best}$$

where *worst* refers to the average share of some asset owned by members of the poorer group and *best* refers to the average share of the asset owned by members of the richer group. The measure potentially ranges from 0 (perfect equality) to 1 (horizontal inequality with the richer group owning all the assets). In Chapter 2 I propose an application of this formula calculating HIs between the two largest ethnic groups per country, disregarding the status of smaller ethnic groups, and also compare HI measures with measures of polarization. Although Stewart (2009: 41) points to the problem of how to deal with intra-group inequalities, Mancini, Stewart & Brown (2008: 90) state that 'we wish to separate our measure of HIs from what is happening within the group'. Also, in an earlier paper, Stewart (2002a: 12) stresses that 'we need to measure intra-group as well as inter-groups differentials in order to explore how intra-group differentials affect the consequences of HIs.' To the best of my knowledge, there still is no ideal HI measure which fully combines information on both inter- and intra-group inequalities as

well as relative group sizes (see Østby, 2008a [Chapter 2] for more details and discussion of various measures).

In general, Stewart's and her team focus mostly on aggregate situations, taking an overall (as opposed to a group-specific) perspective on measuring HIs (see Table 2). Along similar lines, Murshed & Tadjoeddin (2009), insist that inequality has to be measured at the level of the nation state. However, as we point out in Østby, Nordås & Rød (2009) and Hegre, Raleigh & Østby (2009) [Chapters 4 & 5], any country-level measure of HI risks failing to capture the relevant groups in society. In fact, the horizontal inequality argument only requires one under-privileged group to predict conflict. If the rest of the population in the country is homogenous or have small income differences, a country-level measure would be attenuated and unable to capture this. This problem is present in any country-level study of HIs and conflict, including Chapters 2 and 3 of this dissertation. In Chapters 4 and 5 we (Østby, Nordås & Rød, 2009; Hegre, Østby & Raleigh, 2009) minimize this problem by disaggregating the study of conflict below the national level. This way one can simply compare the group (or region) of interest with some other unit (e.g. the capital or the national average), and hence stick to simple ratio measures, or group-specific measures, as I have chosen to label them in Table 2. With a disaggregated design it is also easy to add an individual variable for intra-group inequality into the model.[33] In addition, there are other good reasons to disaggregate the study of conflict, not least the fact that it is rarely the case that a conflict engulfs an entire country (see e.g. Buhaug & Gates, 2002; Buhaug & Rød, 2006).[34]

Most of the African DHS surveys now include the exact geographical coordinates for each sampled village or town. This has opened up new possibilities for measuring spatial inequalities within countries. The maps in Figures 6a and 6b shows the variation in economic (household assets) and social (educational) horizontal inequality between

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[33] Cederman, Gleditsch & Rød (2010) also take a disaggregated approach.

[34] Disaggregating the unit of analysis from e.g. the state to the regional level does not necessarily imply a disaggregation of the HI measure. In Chapter 6, for example, which is a disaggregated study of Indonesian provinces, we still apply an aggregate level of HIs, which summarizes the level of HIs between religious groups within the region.

regions, based on the most recent data from DHS (see Chapter 4 for details about calculations).[35]

Figure 6a. Horizontal Inequality in Terms of Assets, African Regions, 2000

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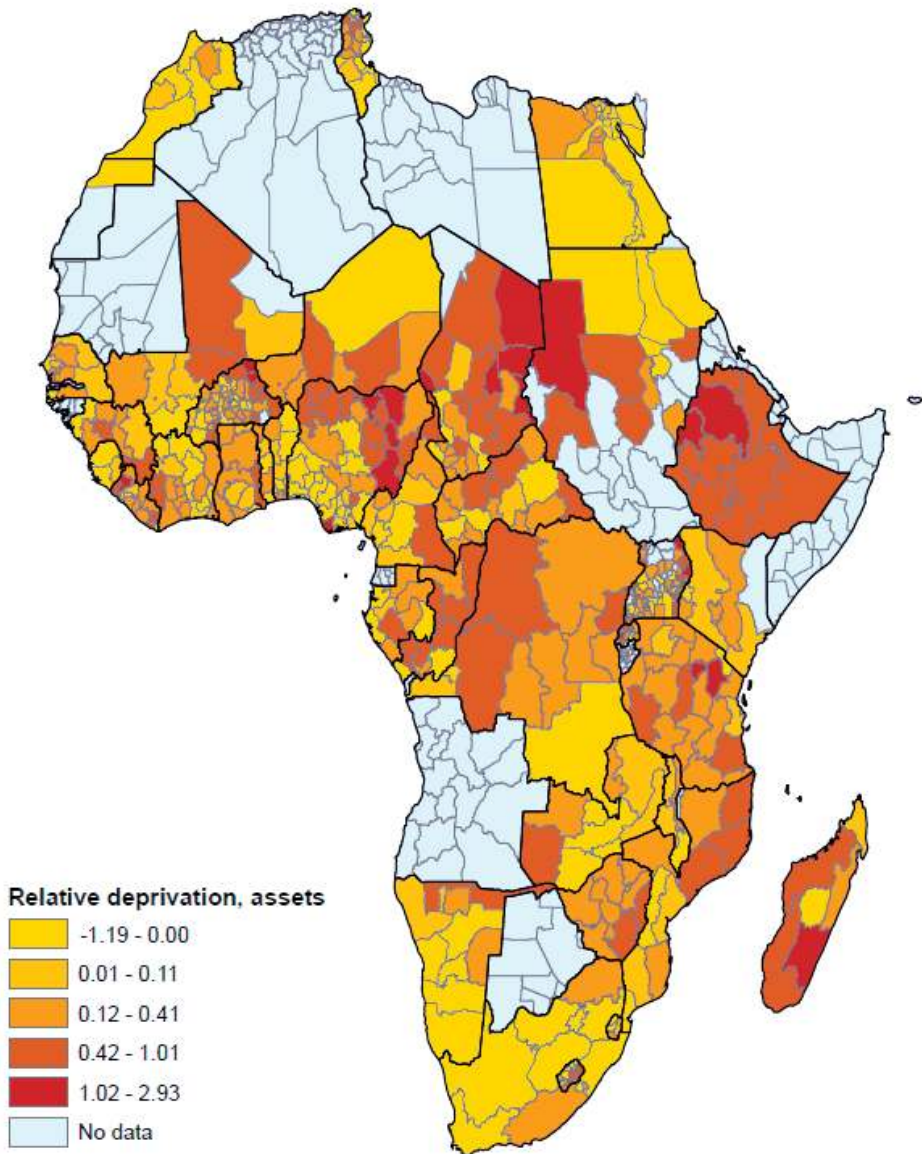
[35] Since the DHS project is currently ongoing I can continuously update my HI data. Many new countries and surveys have been included since the two first chapters in this dissertation (Østby, 2008a,b) were published. See Appendix A for some additional tests with updated data for 73 countries.

Figure 6b. Horizontal Inequality in Terms of Education, African Regions, 2000

Relative deprivation, education

- -1.36 - 0.00
- 0.01 - 0.26
- 0.27 - 0.89
- 0.89 - 1.87
- 1.88 - 3.42
- No data
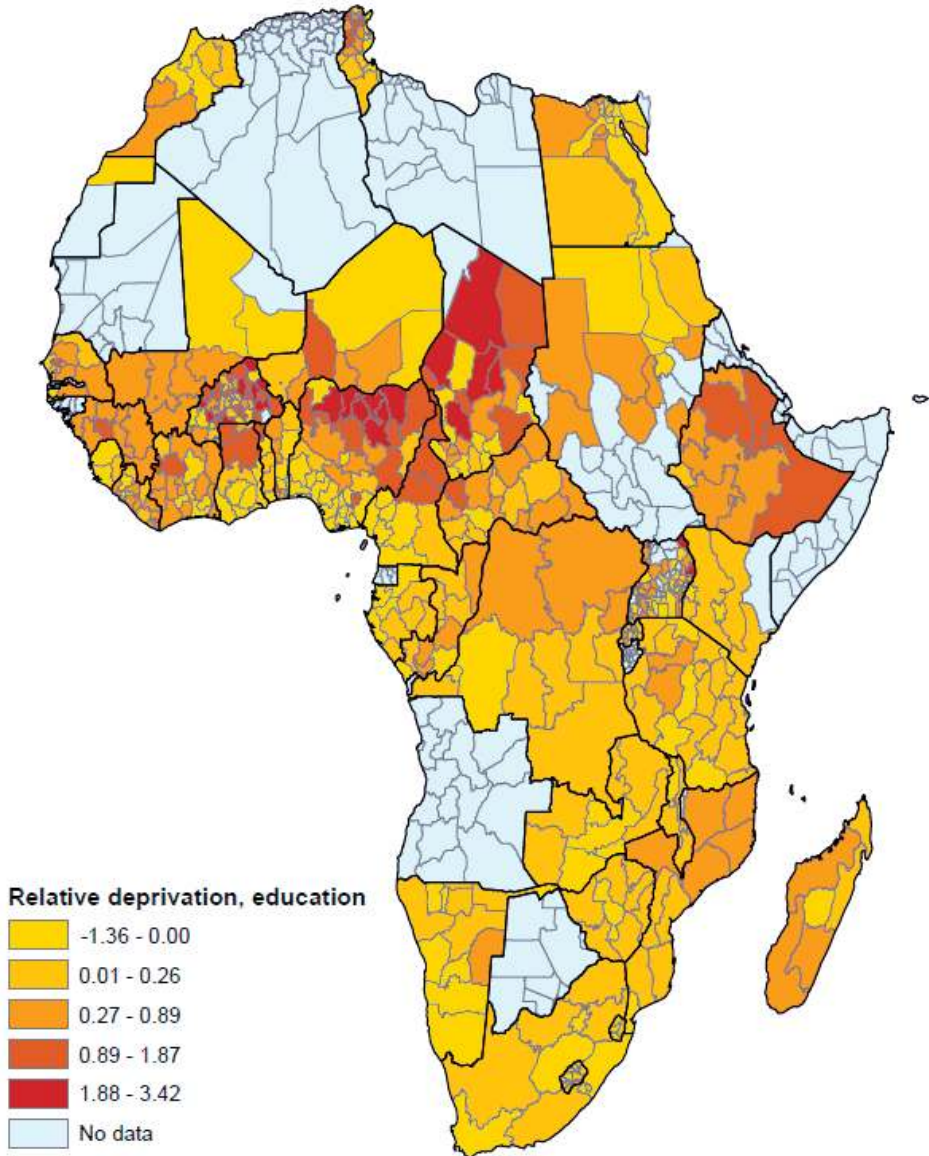
Yellow color represents regions that are better off or have the same level of welfare as the national average, whereas the more reddish the color, the stronger relative deprivation of the region. The maps show that HIs seem to be more marked in Central Africa. Furthermore, there is no perfect correlation between economic and social HIs

4.3. *Geographical Scope vs. Data Quality*

Cederman, Gleditsch & Weidmann (2010) provide a new global dataset on economic HIs, combining their newly coded data on ethnic groups' settlement areas (Min, Cederman & Wimmer, 2008) with Nordhaus et al.'s (2006) G-Econ dataset on local economic activity. In short, the G-Econ dataset tries to assemble the best available data on local economic activity within countries for geographical grid cells, and convert these to comparable figures in purchasing power parity to allow for meaningful comparisons. The resolution of the spatially explicit data set is 1 degree grid cells. The data are constructed from a variety of sources, including regional gross product data for the lowest available political subdivision, estimates of regional income by industry, and estimates of rural population and agricultural income. The specific methodologies differ by countries and data availability (see Nordhaus et al., 2006 for a detailed discussion). The database has global coverage, but the temporal scope is limited to a single year, 1990.

Cederman, Gleditsch & Weidmann (hereafter CGW) (2010: 13) admit that the DHS 'offer a relatively direct measure of well-being', but point to a number of limitations afflicted with the use of DHS to create HI measures. Most importantly, they point to the restricted geographical coverage, and the focus on developing countries. They also mention potential problems associated with representativeness at the subnational level and potential response biases, such as the possibility that poorer individuals might overstate (or understate) their assets. As discussed in Chapters 2 and 4 these problems should not be too severe, though.

CGW are of course right that the DHS cannot be used to evaluate the role of HIs on a global basis. However, I do not agree with their position that the Nordhaus-based HIs data are necessarily superior to the DHS-based HI data for the countries that are included in both databases. First, the Nordhaus data cannot account for the informal economy, which very often benefits groups engaged in agriculture. This is particularly relevant for African and Asian countries where large segments of the population still depend on agricultural livelihoods. Any measure of economic productivity is a 'flow measure', and hence an imperfect proxy for the actual level of income or wealth.

Second, and far more serious, a closer inspection of the documentation of the Nordhaus data reveals that the overall data quality is indeed very poor for large parts of

the developing world (where most conflicts occur) – exactly where the DHS surveys are conducted. The world map in Figure 7, which depicts the quality of the G-Econ data, with red color indicating top quality data, speaks for itself. With the exception of South Africa, the entire continent of Africa has 'low quality or *some* regional data' [36] (my emphasis). Also parts of Asia, such as Indonesia, have equally poor data. According to Cederman, Gleditsch & Weidmann (2010: 14) 'on some countries the official data may be of so poor quality that the variable is suppressed and accuracies over survey reports may be questionable'. This is at best an understatement.

Figure 7. Quality of the G-Econ Data
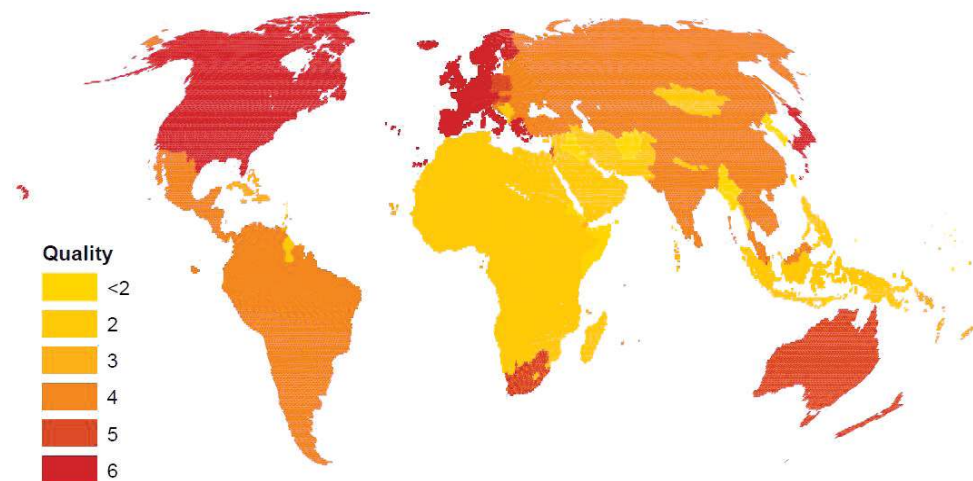
Source: Map generated by author on the basis of Nordhaus et al.'s (2006) G-Econ data and PRIO-GRID (Tollefsen, Buhaug & Strand, 2010). Quality designation: -999= some defect not yet determined; -99= very small or zero area, to be set at zero area (non-existent); 1= lowest quality (disputed, essentially non-existent data); 2= low quality and some regional data; 3= small islands; 4= medium quality, developing country; 5= high quality but not complete or poor resolution regional data; 6= highest quality data and regional resolution.

How well do the two data sources correspond to each other? In Africa, the continent with the poorest data from Nordhaus, the correlation between the survey-generated asset indicator and the G-Econ-based GDPpc (Gross Domestic Product per capita) is r=.61 at the national level (N=38), compared to .41 (N=517) at the

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[36] See updated data from G-Econ 2.11 at http://gecon.yale.edu/data-and-documentation-g-econ-project.

subnational, regional level.[37] Admittedly, a correlation above .6 is not so bad (especially since the variables measure slightly different phenomena). However, the fact that the correlation drops when we go below the national level could very well be a result of the lack of regional variation in the G-Econ data (i.e. that the data are to a certain extent geographically extrapolated). Adding this to the arguments presented above, I feel quite confident that the G-Econ data are not at all superior to the DHS data for the purpose of measuring horizontal inequalities in developing countries. Although CGW manage to construct a global dataset on HIs, their data are nevertheless of poor quality for most of the developing world. This highlights the puzzle of the key and the lamppost described above and only underscores the acute need for high quality economic data for such countries.

It is not my intention to discredit the CGW study. On the contrary, I find their combination of new geographic data truly innovative and fascinating and hope that it will inspire and generate future attempts along similar lines. However, if King (2001: 505) is right that 'good data beats better methods every time', we still have a lot of work to do. It seems to me that basic socioeconomic indicators from household surveys is really a fruitful starting point for measuring HIs in developing countries. Nevertheless, this dissertation and Cederman, Gleditsch & Weidmann (2010) do reach the same overall conclusion; that HIs indeed matter for political violence.

5. Content of Dissertation

The dissertation consists of six independent but related research papers, of which four have been published in international peer-reviewed journals, one is a book chapter, and one is currently under review in an academic journal. Chapters 2, 3 and 7 are single-authored and Chapters 4, 5, and 6 are co-authored. The project has a clear quantitative orientation as all the chapters apply statistical models and numerical data to evaluate various aspects of the relationship between horizontal inequalities and political violence. In all the articles the main independent variable is some kind of socioeconomic horizontal inequality, and the dependent variable is some kind of political violence

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[37] The corresponding correlation coefficients if we replace the DHS asset index with education years are even lower: .429 at the national level and .250 at the subnational, regional level.

involving civil conflict, low scale routine violence, (non-state) ethno-communal (episodic) violence, and various kinds of urban social disorder. The articles evaluate the link between various forms of HIs and political violence at various levels of analysis, and address various intervening contextual factors, such as political conditions, natural resources, and population pressure.

Because the dissertation is a compilation of stand-alone articles rather than a coherent monograph, there is inevitably some repetition of content across the various chapters, so please bear with me. This particularly pertains to descriptions of the survey data and how the various HI measures are generated.

Table 2 summarizes how the various research questions (which were introduced in Section 1), types and aspects of political violence, unit of analysis, geographical coverage, as well as various aspects relating to the independent variable (group identifier, dimension, and perspective of HIs) feature in the six dissertation chapters. The order of the articles is not random. I start out in Chapter 2 by addressing the main research question of whether HIs between ethnic groups influence civil conflict in a cross-country setup. In Chapter 3 I provide a more nuanced test of this relationship looking at various group identifiers and taking into account the importance of the political context. In Chapter 4 we make the case for unpacking the HI measure and go below the national level to capture the link between regional inequalities and conflict in Africa, and we also look at the contextual influence of natural resource endowments. In Chapter 5 we stick to the subnational setup, but switch to a purely geographically defined unit of analysis and look at how absolute and relative local deprivation influences conflict dynamics (events), and not only conflict outbreak. In Chapter 6, we investigate how HIs at the regional level in Indonesia relate to different forms of political violence, and also pay attention to demographic mitigating factors. Finally, in the last chapter I focus particularly on urban violence, since civil and communal conflicts often happen in rural areas, whereas inequality is held to be a typically urban phenomenon. The individual chapters are described in more detail below.

Table 2. Schematic Overview of Dissertation Chapters

Ch.	Research Question	Type of PV	Aspect of PV	Unit of analysis	Geographical coverage	HI group identifier	HI element/ dimension	HI perspective
2	1	Civil conflict	Onset	State	36 developing countries	Ethnic	Assets; education	Overall
3	1,2	Civil conflict	Onset	State	55 developing countries	Ethnic; religious; regional	Assets; education; political	Overall
4	1,2,3	Civil conflict	Onset	Subnational region	Regions in 22 SSA countries	Regional	Assets; education	Group-specific
5	1,3	Civil conflict	Event	Grid cell (76 km²)	Liberia	Geographic (local)	Assets	Group-specific
6	2,3,4	Routine violence/ Episodic violence	Event/ Incidence	Province	Indonesia	Regional	Infant mortality rate	Overall/ Group-specific
7	3,4	Urban social disorder	Event	City	34 cities in Africa and Asia	Migrant status	Assets; education; public services	Group-specific

5.1. Chapter 2: Polarization, Horizontal Inequalities and Violent Civil Conflict

Chapter 2 was published in *Journal of Peace Research* (Østby, 2008a). Inspired by the finding of a positive link between group inequalities and conflict in a series of qualitative case studies, the article addresses whether economic and social inequalities between ethnic groups at the country-level are associated with onset of civil conflicts. To the best of my knowledge, this is the first published cross-national statistical study of objective horizontal inequalities and conflict. The article compares established as well as new measures of inequality and polarization and takes issue with the finding of previous quantitative studies concluding that (vertical) inequality does not increase the risk of internal armed conflict. I argue that these studies fail to account for horizontal inequalities and thus the concurrence of systematic inequalities with ethnic cleavages. Relying on data from the Demographic and Health Surveys (DHS), I construct polarization indices and new horizontal inequality measures across 36 developing

countries during the period 1986–2004.[38] These measures are constructed for both the economic and social dimension, i.e. based on ownership of household assets and educational attainment. The results from both panel and cross-sectional analysis show that social and economic polarization, and social horizontal inequality based on education significantly contributes to conflict. In contrast, measures for vertical inequality (Gini coefficient) and purely ethnic polarization do not produce significant effects on conflict. As an alternative to horizontal inequality, I also propose a hybrid measure of polarization that combines identity and economic polarities, but this term does not perform as well as the simpler ratio-measure of horizontal inequality. Finding that horizontal inequalities indeed matter for conflict, the main insight from this chapter is that previous studies' rejection of the inequality–conflict nexus is clearly premature.

5.2. Chapter 3: Inequalities, the Political Environment and Civil Conflict: Evidence from 55 Developing Countries

Chapter 3, which first appeared as a World Bank working paper (Østby, 2007), and was later published as (Østby, 2008b), is another cross-country time-series analysis. Drawing on Stewart's multi-dimensional conceptualization of HIs, this chapter investigates whether the relationship between socioeconomic horizontal inequalities and civil conflict onset is influenced by various aspects of the political context: regime type, electoral system, and explicit political exclusion of minorities. Furthermore, this chapter expands the country-level dataset on horizontal inequalities to 55 developing countries and measures horizontal inequalities between ethnic as well as religious groups and subnational regions. All the HI measures are found to be positively associated with conflict, but the effects seem to be most robust when using the regional group identifier. The regional HI measures are then interacted with terms for regime type; electoral system and political exclusion. The results show that the conflict potential of regional HIs is stronger for democracies and semi-democracies than for autocracies. Institutional arrangements also seem to matter since the positive effect of socioeconomic HIs on civil

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[38] For a dynamic visualization of my HI data from this article, see the 'Data Visualizer' of the *World Development Report 2011*'s Conflict Database:

http://wdr2011.worldbank.org/datafinder/BubbleChart00_101.html.

war increases with the level of inclusiveness of the electoral system. As argued in the chapter, democracy does not ensure political inclusion of all groups. Furthermore, the interaction between political exclusion and socioeconomic regional HIs seems to make countries particularly at risk for conflict, which provides evidence for the hypothesis that consistent horizontal inequalities across various dimensions increase the conflict risk. This suggests that what is required to secure peace in developing countries is the combination of politically and economically inclusive government.

5.3. Chapter 4: Regional Inequalities and Civil Conflict in Sub-Saharan Africa

Chapter 4 (Østby, Nordås & Rød, 2009) was published in *International Studies Quarterly*. In this article we acknowledge that civil conflicts are usually confined to limited areas within a country, and that features of socioeconomic welfare also tend to vary considerably within countries. For this reason, we argue that country-level aggregate measures of horizontal inequalities and conflict do not allow for adequately testing the relationship between geographically clustered poverty (or wealth) and regional engagement in conflict. For example, the horizontal inequality argument only requires one under-privileged group or region to predict conflict. If the rest of the regions in the country were homogenous or had small income differences, a country-level measure would mask that one particular region was under-privileged. We overcome this potential shortcoming by applying GIS (Geographic Information Systems) operations to *Demographic and Health Surveys* to construct disaggregated data on welfare and socioeconomic inequalities *between* and *within* sub-national regions in 22 countries in Sub-Saharan Africa. Moving to the subnational level also allows us to unpack the inequality measure and single out the separate effects of being relatively deprived and relatively privileged, as well as addressing the violence potential of *intra*-regional inequalities. By coupling these data with existing data on the spatial location of conflict zones we are able to investigate whether absolute poverty, relative regional welfare (inter-regional inequality), as well as inequality within regions affect the likelihood of conflict onset in a region. We also test the argument that relative deprivation in terms of actual living standards may be particularly conflict-provoking in regions rich in natural resources due to people's perception that they do not benefit sufficiently from the regional resource revenues. We

find that conflict onsets are more likely in regions with overall low levels of education; strong relative deprivation regarding household assets; strong intra-regional inequalities; and, finally, that the positive effect of regional relative deprivation on conflict onset is particularly strong in regions with valuable natural resources endowments, such as oil and diamonds.

5.4. Chapter 5: Poverty and Civil War Events: A Disaggregated Study of Liberia

Chapter 5 (Hegre, Østby & Raleigh, 2009), published in *Journal of Conflict Resolution* is a quantitative case study of the Liberian Civil War (1989–2002), which represents a different approach to disaggregating the study of political violence in two ways. First, unlike Chapter 4, which focuses on subnational administrative units, this article disaggregates the study of conflict to purely geographically defined units (grid cells of approximately 8.5 km x 8.5 km).[39] Second, we shift the focus from conflict onset in Chapters 2–4 to conflict dynamics, or individual conflict events drawing on the ACLED dataset (see Raleigh et al., 2010). We develop a theoretical framework for studying the relationship between spatial distributions of welfare and the location of conflict, introducing the concepts 'support level' and 'target value'. This theorizing implies that within-country variation in absolute and relative poverty levels should affect where and if conflicts emerge, where rebel groups are able to recruit soldiers, and where rebel groups operate. We use georeferenced data from the Liberian Demographic and Health Survey of 1986 to construct a welfare index based on a host of variables weighted according to their individual loadings resulting from a principal component analysis. Since we are analyzing locations within a single country, all estimated wealth levels may be interpreted as wealth relative to the average of Liberia. However, we also constructed a more geographically local measure of relative deprivation (LRD), defined as the absolute gap between each cell's value on the wealth index compared to the overall performance of the neighboring cells. The data were analyzed by means of a zero-inflated negative

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[39] Since the grid cells are purely geographically determined (and hence their function as proxies for group identifiers could be problematic), they are not ideal in order to generate HI measures. I still chose to include this article in the dissertation, as I think it makes a clear contribution to the overall project, particularly with regard to the focus on events and conflict dynamics.

binomial model. Controlling for such factors as neighboring conflict events, distance to Monrovia and national borders, population density, diamond deposits, and ethnic affiliations, we find that war events were more frequent in the richer locations of Liberia, both in the absolute and relative sense, although local wealth relative to the national average is more salient than the wealth relative to the immediate neighboring cells. This could be interpreted as better support for 'opportunity' explanations than for 'relative deprivation' theories of conflict, but we argue that the relative weakness of the Liberian government makes it difficult to distinguish between the two.

5.5. Chapter 6: Population Pressure, Horizontal Inequality and Political Violence: A Disaggregated Study of Indonesian Provinces, 1990–2003

Chapter 6 (Østby, Urdal, Tadjoeddin, Murshed & Strand, 2011) was published in *Journal of Development Studies*. This article addresses two broader and distinct claims of factors motivating violent conflict; the *scarcity* and competition over natural renewable resources, and *intergroup dynamics* in terms of group identity and horizontal inequalities. While these perspectives arise from disparate literatures, we empirically combine them and test whether their individual and simultaneous presence may help account for the variation in the level of political violence between provinces in Indonesia for the period 1990–2003. We investigate two types of violence. First, we look at 'routine' social violence, centered around group-based vigilante violence/popular justice and intergroup/neighborhood brawls. It should be distinguished from simple criminal acts with no socioeconomic or political basis. Second, we look at 'episodic' violence. This refers to ethno-communal and separatist violence. With regard to both types of violence, the study investigates the role of inter-group dynamics, in particular religious polarization and issues of socioeconomic inequalities (HIs) between religious groups at the provincial level. The study makes use of regional demographic and socioeconomic data from population censuses and province-level data from various Indonesian Demographic and Health Surveys (DHS). Like Chapter 4 this is a disaggregated study, although the HI measures applied are overall, and not group-specific, since we focus on the overall inequality between religious groups within the subnational region (cf. Table 2). We find that demographic pressure and horizontal inequality seem to have little effect in isolation for the risk of political violence of either type across Indonesian provinces. However, in

provinces where population growth is high, greater levels of inequality between religious groups (proxied by different infant mortality rates) appear to increase the violence risk.

5.6. Chapter 7: Internal Migration, Inequality and Urban Social Disorder: Evidence from African and Asian Cities

Finally, the article presented in Chapter 7 (Østby, 2010) is currently under review in an academic journal in response to an invitation to contribute to a special issue on climate change and conflict. However, the paper does not directly assess the link between climatological variables and conflict. Rather, I study the level of urban political violence focusing on a host of demographic and economic factors that are often seen as a consequence of climate change and increasing population pressure in the countryside: rural-urban migration, urban poverty, and urban inequality between individuals as well as between migrants and non-migrants. Previous quantitative research on the security consequences of high urban population growth has produced little support for the widespread concern over increasing urbanization. With a more nuanced measure of rural-urban migration, this study adds momentum to the general conclusion that it is not the movement of people as such that seems to create problems. The article finds, however, that both inter-individual and inter-group inequalities between migrants and non-migrants seem to matter for lethal forms of urban political violence. More specifically, overall poor and unequal educational opportunities as well as marginalization of rural-urban migrants in terms of household assets as well as access to public services are found to spur increased levels of lethal urban political violence.

6. Conclusions

The main motivation behind this dissertation project was to provide a systematic evaluation of the relationship between structural inequalities between groups – horizontal inequalities – and political violence in order to transcend and supplement the broad qualitative case-study literature in the field. The principal research question was whether societies with marked horizontal inequalities are generally at a higher risk of experiencing armed conflict and other forms of political violence. In order to answer this question in an adequate manner, I have clarified theoretically how HIs can be translated to collective violence, provided new data on HIs, and conducted a set of statistical

studies, including both cross-national studies as well as various kinds of disaggregated studies.

This thesis has contributed to the inequality–conflict research in several ways. First, I have demonstrated that in order to grasp the complex relationship between inequality and conflict, we need to move away from a single-tracked concept of inequality, distinguishing vertical from horizontal inequalities, and considering other dimensions than purely income inequality. Traditional inequality studies tend to ignore this diversity. Theoretically, the thesis provides a synthetic model of grievance and opportunity explanations of civil war, demonstrating that horizontal inequalities may affect both these preconditions of conflict simultaneously. The main contribution of this thesis, however, relates to the development of new data on HIs based on national surveys. This approach represents a way of generating data that are difficult to find elsewhere. Furthermore, the aggregation of survey data ensures *descriptive* rather than *evaluative* data.

To the best of my knowledge this dissertation project provides the first systematic cross-national test of the horizontal inequality theory (Stewart, 2002, 2008). In my own view, the single most important finding in this project is that *type* of inequality matters. I am quite confident about the main conclusion: Whereas I find no evidence of a relationship between vertical inequality and conflict, horizontal inequalities do make developing countries more susceptible to civil conflict. This applies across various group identifiers (ethnic, religious and regional groups), although the most robust cross-national evidence is found when horizontal inequalities are measured between subnational regions. With the larger sample (Østby, 2008b) [Chapter 3], the relationship also holds for both economic and social dimensions of inequality, measured in terms of household assets and educational attainment respectively. Because the DHS surveys do not include any questions pertaining to political status of the respondents, I have only been able to test a general measure of political exclusion derived from the Minorities at Risk dataset (Gurr, 2000).[40] The effect of political exclusion has no separate effect on

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[40] A more recent source of political HIs are Cederman, Min & Wimmer's (2010) data on 'Ethnic Power Relations' (EPR). See Fjelde & Østby (2010) for an application of the geo-referenced version of these data (Wucherpfennig et al., 2010) with regard to non-state conflict in Africa.

conflict risk in my models, but there is a strong positive interaction effect between economic HIs and political exclusion, which confirms Stewart's multidimensional notion of HI, and supports her assumption that consistent HIs are particularly conflict-provoking.

A second main conclusion from this project is that the political, economic and demographic context matters for the HI–conflict relationship. The articles included here have tested the impact of three kinds of contextual factors which are all found to influence the HI–conflict nexus: political conditions, natural resources and population pressure. Due to the apparent inertia of HIs, it should be especially relevant to consider mediating factors that influence when and how HIs lead to political violence. First, I find that political conditions such as regime type condition the HI–conflict relationships, indicating that democracies and semidemocracies may facilitate the transformation of horizontal inequalities into conflict, by permitting protest. Second, economic conditions are also found to matter: The positive effect of relative deprivation on conflict at the regional level is particularly strong for regions with valuable natural resource endowments. The explanation for this can be that violent group mobilization may be a result of grievances if people do not feel that they benefit as much as they should from the revenues generated from natural resources in their own region. Finally, in the Indonesia study (Chapter 6) we find a strong interaction between provincial population growth and inter-religious HIs. Demographic pressure and inequality seem to have little effect in isolation, but in provinces where population growth is high, greater levels of inequality between religious groups appear to increase the violence risk. In sum there seems to be several relevant contextual factors that could indeed influence the HI–conflict nexus (see Stewart, 2008 for a more comprehensive discussion on this).

A third insight offered by the work presented herein is that subnational studies largely confirm the main conclusion that HIs matter for various forms of political violence. More importantly, disaggregated designs make it possible to unpack the HI measure and single out the separate effects of being relatively deprived and relatively privileged with regard to conflict risk. The disaggregated study of African regions reveals that the overall finding that HIs influence conflict is really driven by the effect from the relatively deprived regions. In this sense, I fail to provide supporting evidence for my assumption that both richer and poorer regions have increased conflict risk. A new

research paper (Fjelde & Østby, 2010) reaches similar conclusions with regard to non-state conflict in Africa, i.e. that it is the economically marginalized regions that see the higher risk of armed conflict between groups, whereas the regions that are economically privileged compared to the national average are not particularly at risk. An exception to this is the Liberia study (Hegre, Østby & Raleigh, 2009) in Chapter 5. It shows that conflict events were more frequent in the relatively richer locations of Liberia.[41] This could be due to reporting bias of events in richer, more urban areas. But it could also be an indication that horizontal inequalities do not exert similar effects on conflict onset and incidence. Finally, one could of course question whether inequality between purely geographically defined units (grid cells) are sufficiently good proxies for inequalities between 'real' identity groups. Furthermore, Cederman, Gleditsch & Weidmann (2010) in their global test of ethnic HIs, find that both affluent and poor regions (relative to the national mean) are more likely to engage in conflict. Deiwiks, Cederman & Gleditsch (2010) reach a similar conclusion for administrative regions in a global analysis of federations, but this study only includes four African countries.

A final general conclusion from this dissertation is that horizontal inequalities seem to matter across various types of political violence – not only civil conflict, although the evidence is less clear. The quantitative case study of Indonesian provinces demonstrates that the separate effect of HIs is not significant for neither routine nor episodic, ethno-communal violence, but the former has an effect in a context of high population growth. There is also some evidence in the final study of urban violence that HIs between rural-urban migrants and others have a positive effect on urban social disorder which includes various forms of political violence, such as riots, strikes, terrorism and also civil war.

In sum, my general and brief response to all the research questions 1–4 introduced in the first section is 'yes': Societies with severe HIs are more prone to internal conflict (RQ1); this relationship is mitigated by various contextual factors, such as political environment, natural resources and population pressure (RQ2); the conflict

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[41] A global test of spatial inequalities between 100km*100km grid cells (Buhaug et al., 2011) finds no general effect of either positive or negative income deviations. However, it finds some evidence that pockets of relative wealth within very low income countries are more likely to see conflict outbreaks.

provoking effect of HIs applies across various forms of political violence (RQ3) and varies at the subnational level (RQ4). These findings are of considerable theoretical importance and should inspire a revival of interest in grievance-based explanations of conflict in general. Although I argue that both grievances and opportunities contribute to linking HIs to political violence, we cannot easily separate between these to explanations. If it were the case that grievances do not matter for conflict due to their alleged omnipresence, then we should not be able to detect a statistically significant relationship between HIs and conflict. However, more research is needed to clarify the potential different effects of HIs on various types of political violence.

Three particular caveats should be added to the overall conclusion. First, due to the restricted number of countries covered by the DHS project, I cannot offer global generalizations of my findings. What I *can* do, however, is say something about the relationship between HIs and conflict in developing countries. And, as argued above, this is not necessarily inferior to any global assessment, particularly since global studies are often conducted at the expense of the quality of data for the more war-prone countries. Second, there is the ever-present problem of endogeneity: Is it HIs that breed conflict, or vice versa? Because some of the DHS countries only have survey data for recent years, there are reasons to be concerned that endogeneity could have distorted the results. However, most of the chapters address the endogeneity of HIs by controlling for past conflict history.[42] Finally, even with the disaggregated tests and unpacked (group-specific) HI measures, the risk of ecological fallacy is still there. For example, even if a region is relatively deprived overall, we do not know whether it is the more deprived individuals who actually commit the violence.[43] This should inspire en effort to collect new survey data which includes questions on both economic status and perpetration of – or attitudes to – political violence.

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[42] Hegre, Oneal & Russett (2010) notes that advanced methods to account for endogeneity rarely makes a difference.

[43] See Østby & Urdal (2010) for more discussion relating to education and political violence.

6.1. Future research

One obvious challenge for future research in this field is to collect more and better data on horizontal inequalities for various group identifiers and dimensions expanding the present spatio-temporal domain covered in this project. In addition to general, cross-national datasets, there is a need for carefully designed micro-level studies, which can help us better understand the mechanisms linking HIs to political violence. A wish list for future data collection projects would include designing new household surveys so as to include data on various group affiliations, and objective as well as perceived inequalities along economic, social, political and cultural dimensions. There is also a need for better temporal data on HIs. As time passes on there will be a constant supply of new household surveys from DHS and other sources, which will contribute to better temporal data.[44] A related point, noted by Blattman & Miguel (2010) is that it would be extremely useful to have follow-up surveys of the same respondents in post-conflict settings. At present, I am aware of one such survey which was conducted before and after the genocide in Rwanda (Verwimp, 2005).

There is also room for improvement with regard to the measurement of HIs. Existing work by e.g. Mancini, Stewart & Brown (2008); Esteban & Ray (2005); Zhang & Kanbur (2001) have provided some useful guidelines and starting points for measuring HIs, but, as pointed out in Chapter 2 in this dissertation, we have yet to see the ideal HI formula which accounts for both group size, intra- and inter-group inequalities. Probably it is not even possible to construct such a summary measure of HI which makes perfect sense, but it is clearly worthwhile to explore various possibilities. Related to this, there is a need for more disaggregated studies of HI and political violence – not only in the spatial sense, but also dissertating by e.g. ethnic groups regardless of whether the group is clustered geographically or not. The Minority at Risk project is currently being expanded to include groups which are judged to be 'not at risk' (Birnir & Inman, 2010).

This dissertation has tested the individual effects of HIs along various group identifiers (ethnicity, religion, region, migrant status) and dimensions (economic, social, political). Future efforts should explore the conflict potential of cross-cutting cleavages –

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[44] Furthermore, Baten & Fraunholz (2004), and Moradi & Baten (2005) have used anthropometric data from the DHS surveys to construct measures of inequality for pre-survey decades.

not only with regard to various dimensions, but also with regard to various group identifiers.[45] For example, when spatial cleavages are reinforced by other social divisions such as ethnicity and religion, the threat of armed conflict should be much greater (Rokkan, 1967). I would assume that the coincidence between multiple group identifiers and structural socioeconomic and political inequalities could be particularly dangerous. The conflict between the rural-based Mayan groups and the urban-richer mestizo in Guatemala and between the poorer Muslim areas of Mindanao and the Sulu Archipelago and the richer Christian areas of the Philippines serve as good examples of this.

Furthermore, as there seems to be quite a robust statistical relationship between HI and conflict, one of the most important challenges for future research should be to better account for the causal mechanisms underlying this relationship. This requires extensive theorizing and carefully selected micro-level studies. In particular we need to better understand the relationship between objective and perceived inequalities. Stewart (2009: 16) holds that 'people take action because of perceived injustices rather than because of measured statistical inequalities of which they might not be aware'. In general, one would expect there to be a high correlation between perceived and observed HIs. Hence, it is important to study perceptions, and their determinants too, since leaders, the media and educational institutions can affect individuals' judgment of inequality and their own relative position in society, even when the actual situation remains unchanged. Perception surveys for a selection of countries have already been undertaken by researchers at Stewart's Centre for Research on Inequality, Human Security and Ethnicity (see e.g. Stewart, 2009).

Finally, since there is growing evidence that HIs promote conflict, future efforts should focus on exploring the root causes of various HIs and also look at the reverse causality, including the effects of conflict on HIs. There are some examples of such studies with regard to the determinants of vertical inequalities (e.g. Bircan, Brück & Vothknecht, 2010; Bussmann, de Soysa & Oneal, 2005), but I am not familiar with similar systematic studies of the causes of HIs.

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[45] See also a paper by Han & Mahoney (2010), for an argument about how groups to compare their economic status both relative to the state mean and relative to kin groups in other countries.

6.2. Policy implications

Over the last couple of years, the research on horizontal inequalities and conflict has started to gain speed. The general finding that HIs breed political violence has proven to be remarkably robust – across very different data sources, measurements and research designs. In addition to Stewart's qualitative case studies, this dissertation, as well as some other recent statistical evidence (e.g. Cederman, Gleditsch & Weidmann, 2010), have demonstrated that HIs are positively related to political violence. Despite the growing academic interest in HIs, the international community has paid little attention to HIs (Stewart & Wang, 2006). Yet, the finding that HIs breed political violence has direct policy relevance.

Where HIs are found to be severe, policies are needed to correct them. In our increasingly pluralistic societies, the reduction of horizontal inequalities should be part of development policy for *all* countries, not only the ones which are currently, or have recently been, in conflict. Such policies should include both the elimination of discrimination, and affirmative action providing positive bias in favor of certain relatively deprived groups. Furthermore, policies reducing HIs should cover economic, social and political areas.

On the socio-economic side such policies might involve actions like public investment designed to reduce HIs, public (and private) sector employment and education policies to include group distribution requirements and policies toward land reforms. It should be stressed, however that policies developed to correct economic HIs can be tricky and in the worst case have the opposite effect with regard to conflict (Stewart, 2008). Tadjoeddin (2007) for example found that economic redistribution from richer to poorer regions has actually triggered conflict in Indonesia. Such examples demonstrate that successful redistributive policies are hard to design. Without economic growth, redistribution by definition cannot be pareto-optimal.[46] Hence one should put much energy and resources in promoting more inclusive and sustainable *growth*. However, the effects of redistribution policies may also depend on other contextual

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[46] Given a set of alternative allocations of goods or outcomes for a set of individuals, a change from one allocation to another that makes at least one individual better off without making any other individual worse off is called a Pareto improvement or a Pareto-optimal move.

factors. Ross (2007) explores various measures to reduce regional HIs within the context of mineral-rich states. He discusses various strategies, such as direct distribution, decentralization of mineral revenues, and government actions. For example, he argues, distributional issues are more likely to spur violence when the police or the military engages in predatory behavior. Various activities can curtail such actions, such as holding extensive consultations with local communities and NGOs.

Politically, there is a need for inclusion of minority groups to avoid the monopolization of political power by one group or another. This may be among the most difficult changes to bring about, since it is not an automatic result of democracy. Majority rule may lead to permanent rule by one ethnic group in situations where one group is in a strong numerical majority. Sri Lanka and Northern Ireland are examples of conflicts that occurred in democratic contexts. Democracy in sharply divided societies thus calls for some *constraints* to ensure an inclusive system, such as alternative voting systems or other forms of proportional representation, and decentralizing of governments to ensure power-sharing. Naturally, any appropriate policies depend on the main variants and root sources of the HIs in each society.

6.3. Closing remarks: peace by piece

In the first part of this introductory chapter, I introduced three main puzzles pertaining to both the general conflict literature, and more specifically to the inequality–conflict nexus: (I) 'the quantitative–qualitative mismatch'; (II) the 'ethnic paradox'; and (III) 'the key and the lamppost'. To what extent has this dissertation contributed to filling the immense knowledge gap resulting from these three puzzles? In my own view, I think the joint insights from the various chapters have largely contributed to solving the first puzzle: The reason for the previous conflicting findings between the qualitative case literature and the quantitative literature as regards the inequality–conflict nexus is that the latter has largely missed the target. It is not vertical inequalities that matter for political violence, but horizontal inequalities.

As regards the second, more general, challenge of explaining the causes of ethnic conflict, this project has at least added some critical pieces to the puzzle, demonstrating that systematic inequalities between groups can be one factor that distinguished peaceful from belligerent multiethnic societies. However, there may of course still be other

possible omitted variables which could better account for the phenomenon of ethnic violence.

The final puzzle relates to the general problem of low quality or missing data for the poorest, most conflict-ridden countries of the world. All global studies of economic factors and conflict that I am aware of suffer more or less from this problem. In this dissertation, my focus on generating reliable and valid HI data for developing countries has hopefully contributed to 'shedding some more light' on some countries which often either drop out of large-N global regressions due to missing data on vital indicators, or which are analyzed despite low quality data on the same. For this reason, I close this introduction chapter with a general plea for better data.

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7

Internal Migration, Inequality and Urban Social Disorder:
Evidence from African and Asian Cities

Internal Migration, Inequality and Urban Social Disorder

Evidence from African and Asian Cities[*]

Abstract:

Insecurity has become a fact of life for urbanites in African and Asian cities. By the mid-century, two-thirds of the world population will reside in urban areas. The bulk of this urban growth will take place in developing countries. Rural-urban migration is often seen as a consequence of climate change and increasing population pressure in the countryside. Rapid urbanization puts significant demands on city governments' ability to provide public services like adequate housing, electricity, water supply, health care, education, and employment. Whereas average living standards are usually higher in urban areas, economic growth does not result in prosperity for all. Inequality among city dwellers is a potential source of frustration which could increase the potential for political radicalization and unrest – especially if certain groups are underprivileged and suffer from social exclusion. Drawing on household surveys the paper provides new indicators of internal migration, poverty and inter-individual and inter-group inequality for 34 cities in Africa and Asia for the period 1986–2006. These data are linked with a newly developed dataset on social disorder events in African and Asian cities. The results suggest that it is not the actual movement of rural people into the cities that creates social upheaval. Rather, overall poor and unequal educational opportunities as well as socioeconomic marginalization of rural-urban migrants are found to spur increased levels of lethal urban political violence. This implies that urban planners and decision-makers should relax efforts at reducing the magnitude of cityward migration, and focus instead on promoting inclusive urban governance.

1. Introduction

In low and middle-income countries, hundreds of millions of urban dwellers are at risk from the direct and indirect effects of climate change. Whereas extreme weather events in recent years exemplify the direct vulnerability of urban populations (Satterthwaite et al., 2007), climate change may also spur rapid urbanization, which in turn puts significant demands on cities' ability to provide public services like electricity, water supply, health care, education, and employment.

The majority of the world population now lives in cities. By 2050, two-thirds of the world population will reside in urban areas and the bulk of overall population growth will be absorbed by cities (UN, 2008). Most of this growth is driven by urbanization in parts of the world where enormous development challenges remain, particularly Africa and Asia. Climate change is likely to exacerbate this trend further. Rising sea levels and more extreme weather patterns could permanently displace an additional several hundred million people within the mid-century. Although the exact figures remain contested, researchers agree that much of this population movement will consist of rural-urban migration.

The trend towards increasing urbanization plays an important role in the literature on environmental security. One reason for this is that rural-urban migration is usually seen as a consequence of high and increasing population pressure in the countryside, leading to rural scarcity of renewable resources like cropland, forests, and freshwater (Homer-Dixon, 1999). Various forms of environmental degradation, including desertification, prolonged droughts, and soil salinization, are other factors that might deteriorate agricultural livelihoods and push people to the cities. For example, in India there are certain villages that are now situated in the rain shadow of mountains, so that the monsoon rain can no longer provide a basis for rain-fed agriculture (Gruber et al., 2005). Whereas opportunities for employment are usually better in urban areas, labor markets cannot absorb fast-growing populations. Moreover, there has been an increasing trend of political violence in developing cities (Urdal, 2008).

As indicated above, an underlying assumption in this paper is that climate change affects cityward migration. The main focus here is on whether rural-urban migration, poverty, and inequality affect the magnitude of urban social disorder. Urban violence has

become a major preoccupation of policymakers, planners and development practitioners in cities around the world. In recent years, massive public protest and riots in cities such as New Delhi, Karachi, and Johannesburg have resulted in significant loss of life and widespread poverty. These disturbances have at times been triggered by immediate economic circumstances, such as rising food prices (see e.g. Hendrix, Haggard & Magaloni, 2009), and in some cases, ethnic tensions have resulted in even higher death tolls and destruction (e.g. clashes between Hindus and Sikhs in New Delhi, and Mahajirs and Pathans in Karachi). However, according to Brennan (1999: 17), 'urban crime and violence in the world's largest cities is generally not a spontaneous occurrence, but rather the product of inequality and social exclusion.'

With the exception of some anecdotal evidence from case studies (e.g. Percival & Homer-Dixon, 1998), the links between various forms of inequality and poverty and urban violence have so far not been systematically tested in statistical studies. In fact, there has been a general scarcity of research concerning political and criminal armed violence in urban settings (Jütersonke, Krause & Muggah, 2007). To the best of my knowledge, only a handful of quantitative large-N studies have systematically addressed political instability and violence at the city level (Buhaug & Urdal, 2009; Hendrix, Haggard & Magaloni, 2009; Urdal & Hoelscher, 2009), but none of these have looked explicitly at the violence potential of migration-induced urbanization or urban poverty and inequality.

In an effort to fill this knowledge gap, this paper explores to what extent rural-urban migration and social exclusion of individuals and groups affect the patterns of social disorder and violence in urban centers. I use national surveys to construct new city-level data on a wide range of indicators relating to migration, household deprivation, education levels, as well as access to basic public services like drinking water, sanitation, and electricity. I also construct various measures of inequalities between individuals and groups. Combined with existing data on social urban disorder in 34 large cities in Africa and Asia I test various hypotheses related to the violence potential of urbanization, poverty and inequality (both between individuals and between migrants and non-migrants).

The paper proceeds as follows. The following section presents the theoretical framework and derives some hypotheses about the expected relationships between rural-

urban migration, poverty, inequality and urban violence. Next, the research design demonstrates how national surveys can be used to generate city-level measures of demographic and socioeconomic factors with the aid of GIS software. Section 4 presents the empirical tests and Section 5 concludes. The analyses provide no evidence for the claim that greater levels of rural-urban migration are associated with urban social disturbance. However, it is shown that various forms of both urban poverty and inequality are associated with increased levels of disorder. Notably, socioeconomic marginalization of rural-urban migrants seems to increase the number of lethal events of urban political violence.

2. Migration, Socioeconomic Status, and Political Violence

The broad literature on social movements and collective action emphasizes two main factors that may lead to group mobilization and violence: motive and opportunity. The main issues of concern in this study, migration, poverty and inequality, are usually associated with grievances and hence relate to what is generally referred to as the motive tradition in empirical studies of political violence. However, they could also increase the opportunity for violence (see e.g. Percival & Homer-Dixon, 1998).

Many theoretical approaches provide valuable insights as to how increasing urban population pressure could transform into political violence. Possible explanations range from ethnic hatreds and associated security dilemmas (e.g. Horowitz, 1985; Posen, 1993) via modernization-based arguments of radicalization of aggrieved, unemployed youths and migrants (e.g. Huntington, 1996; Gizewski & Homer-Dixon, 1995) to theories of relative deprivation and inter-group-, or horizontal inequalities (e.g. Gurr, 1970; Stewart, 2008). Common to all of these contributions is their attention to the distribution of opportunities and privileges among the urban population. Below I discuss the potential harmful consequences of rural-urban migration as well as three factors closely associated with urbanization: poverty and inequality, and social exclusion of migrant populations.

2.1. Rural-urban migration

There is a plethora of studies that link cityward migration – both within and across national boundaries – to social unrest.[1] Rural-urban migration can influence both grievances and opportunities for violent mobilization. Various explanations include relative deprivation and radicalization of migrants, reduced opportunity costs, enhanced social communication networks, as well as ethnic frictions. Although urban in-migration consists of both urban-urban and rural-urban streams of migration, I focus on the latter, because people that live in rural areas are presumably more vulnerable to the immediate effects of climate change.

First, as argued by Gizewski & Homer-Dixon (1995), the rapid influx of rural-urban migrants tends not to be accommodated by public or private sectors. In turn, mobility expectations are not met, and the proximity of privileged elites among the old urban residents raises the migrants' awareness of their marginal role in society. Hence, migrants are likely to experience rising relative deprivation (both relative to their own expectations and relative to others), which in turn is assumed to increase their likelihood to engage in violent political activities. However, this is not to say that political protest and violence are necessarily instigated by the migrants themselves. In India, for example, poor integration and assimilation methods have also fueled anti-migrant political movements among 'sons of the soil' who perceive that their urban status is threatened (Huang & Keepper, 2009).

Second, migrants may have problems adjusting socially to the new life in the city. The disruption of old living habits and customs along with cultural conflicts may cause personal identity crises and primary group breakdown. As a result, traditional social control on deviant behavior is weakened, and for this reason, some scholars hold that migrants tend to be more easily recruited into radical movements (e.g. Gizewski & Homer-Dixon, 1995). The literature provides both older and more recent contributions that strongly oppose such portrayals of the 'radicalized migrant' (see e.g. Cornelius, 1972; Gilbert, 1999; Nelson, 1970). The main contrasting argument is that migrants are often

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[1] Due to data restrictions, this paper exclusively tests the violence potential of *internal* cityward migration, but there is no obvious reason why the argument might not apply to international migration as well.

politically passive, and that the threat to political stability may not lie with the newcomers, but with those deprived slum dwellers who are long-standing urban residents. For example, Indian cities have witnessed sharp urban violence, often in the poorest slums, and sometimes directed at the new migrants from the countryside (Homer-Dixon, 1994: 27–28).

A third factor linking cityward migration to political violence is that the urban environment includes high levels of social communication, implying intense competition for access to services and jobs among migrants and local urbanites (e.g. Reuveny, 2007). People concentrated in slums can communicate more easily than those scattered in rural villages. This may reinforce grievances and reduce coordination problems, thereby increasing the opportunities for engaging in collective mobilization.

Forth, as a result of urban in-migration, cities can be socially, culturally, ethnically, and religiously diverse and this mixing of ethnicities and shifting demographic composition of urban centers is often cited as a major destabilizing factor (see e.g. Beall, Gua-Khasnobis, & Kanbur, 2010), especially when migrants and long-standing urban dwellers belong to different ethnic groups. All the processes discussed above somehow relate to group grievances, whereas the third and forth processes expand the opportunities for these grievances to be articulated and acted upon. The four processes could occur simultaneously, and they all lead to the same basic assumption that high urbanization rates are likely to lead to greater risks of urban political violence.

There is some empirical evidence from particular cases supporting the urbanization-violence nexus (e.g. Huntington, 1996; Kahl, 2006; Percival & Homer-Dixon, 1998), but to my knowledge, only one quantitative study (Buhaug & Urdal, 2009) has tested the link between urbanization and urban violence. They find no support for what they refer to as the 'urbanization bomb.' A limitation of Buhaug & Urdal's study, however, is that they only test the impact of urban population growth (based on national data), and hence cannot distinguish between city-level in-migration and natural population growth.[2] Based on the discussion above, I propose the following hypothesis:

H1: Increased levels of rural-urban in-migration are associated with higher levels of urban violence.

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[2] Urbanization is a product of natural growth, migration and reclassification of rural land.

2.2. Absolute deprivation

Despite the above discussion, it should be stressed that migrants are also often valued for their skills and contributions to society, and can benefit absorbing areas in several ways, including increasing the workforce and tax-base (Nordås & Gleditsch, 2008; Reuveny, 2007). Indeed, the general link between urbanization and socioeconomic development is rarely disputed. However, in many developing countries around the world, economic growth has not resulted in prosperity for all. Absolute deprivation, or poverty, has long been recognized as an important risk factor associated with crime and violence in urban areas (UN Habitat, 2008). For example, Brennan (1999: 18) holds that 'when considering the linkages between urbanization, environment, and security, clearly, the missing link is poverty'. Yet, Beall, Gua-Khasnobis, and Kanbur (2010: 11–12) argue that the development literature has been marked by a general lack of focus on or understanding of urban poverty issues. Mitlin (2004) argues that urban poverty has been relatively ignored by development specialists, and shows that nearly all Poverty Reduction Strategy Papers (PRSPs) have s a strong emphasis on the relative importance of rural poverty whereas most show a general lack of focus on urban poverty issues, although there is evidence that 'poverty is becoming more urban' (Beall, Guha-Khasnobis & Kanbur, 2010: 7).

In the coming decades, increasing numbers of cities in developing countries will have a high proportion of their population living in poverty, and will also suffer from severe environmental degradation. Poor environmental conditions are most likely to affect the poor residents in megacities. Although citywide violence will have worldwide consequences, raising concerns for regional stability and financial markets, it will more frequently consist of 'the poor preying upon the poor' (Brennan, 1999: 18). Of the three billion urban residents in the world today, one billion live in slums. The rapid growth of urban slums – although many of them can be vibrant communities – tends to be associated with poverty and crime (e.g. Davis, 2007). The struggle to survive can lead to social fragmentation, creating grievances that can manifest themselves in crime and political violence. Also, there should be less opportunity costs involved for the poor associated with engaging in violent activities. This substantiates a second hypothesis:

> *H2: Higher levels of socioeconomic deprivation in urban populations are associated with higher levels of urban violence.*

2.3. Individual relative deprivation

A particularly important debate concerns the extent to which crime and urban political violence are causally rooted in poverty and/or *inequality*. The dynamics of poverty is arguably different in cities than in rural areas. Whereas poverty exists virtually everywhere, inequality is primarily an urban phenomenon (Hamdi, 2007). At least, income inequalities are generally more marked in urban than in rural areas (Moser, 2004). Intra-city inequalities have risen as the gap between rich and poor has widened, e.g. as a result of key exclusionary factors relating to unequal access to employment, education, health and basic infrastructure. Perceptions of declining living conditions in big cities have been buttressed by literature that documents substantial and growing inequality within cities (Brockerhoff & Brennan, 1998). The problem, hence, is not necessarily urbanization or poverty per se, but could rather relate to the fact that it has not resulted in a more equal resource distribution.

It is in relation to basic urban services that urban inequalities are often most evident, with poor slum-dwellers paying water vendors up to fifty times more for clean water than a resident living a stone's throw away in a neighborhood which is fully serviced (Beall, Gua-Khasnobis & Kanbur, 2010). As Stephens (1996: 13) bluntly observed, 'the poor pay more for their cholera'. Such patterns of urban inequality have provoked ominous visions of future cities. For example, Massey (1996: 410) argues that 'urban inequality entails escalating crime and violence punctuated by sporadic riots and increased terrorism as class tensions rise'. This perspective derives from a rich body of social conflict theory (often associated with Marx), and from a common idea that the disadvantaged of large cities will challenge the established urban social order violently. In order to test this argument I propose the following hypothesis:

H3: Higher levels of inequality in urban populations are associated with higher levels of urban violence.

2.4. Social exclusion of migrants

Where access to various goods and opportunities is shaped by underlying social cleavages, such as class, religion or cultural, historical and geographical origin, political violence is more likely to unfold. Stewart refers to such group-based differentials as 'horizontal inequalities' (see e.g. Stewart, 2008). Rural-urban migration can offer

individuals or groups opportunities to improve their situation, thereby potentially reducing nationwide inequalities. However, although migration might improve the socioeconomic situation of the migrants relative to their region of origin, it may create a new set of unequal relations and mechanisms in the recipient cities.

For example, cases in which rural-urban migrant populations live in informal settlements under poor environmental and economic conditions can cause social segmentation 'as subgroups within the community withdraw into themselves to protect their own interests' (Percival & Homer-Dixon, 1998: 290). The basic thesis is that macro-social patterns of inequality lead to social isolation and ecological concentration of the relatively disadvantaged (most likely the migrants), which in turn gives rise to structural barriers and cultural adaptations that undermine social organization and the control of violence (Sampson & Wilson, 2005: 178).

In a developing country marked by a dual economy, people living in a city may not be sensitive to the overall level of inequality (measured e.g. by the Gini coefficient) at the city level. Rather, as argued by Jiang, Lu and Sato (2008) segmented social groups in a city can be more concerned with the welfare status within their own reference group, and with the welfare gap between different social groups. They find support for this hypothesis, demonstrating a high correlation between inter group inequality and lack of happiness in Chinese cities. Jiang, Lu and Sato (2008) conclude that in China, the major form of the urban-rural divide has been transformed from a traditional dual economy between urban and rural sectors to a 'dual society' between urban residents and migrants in urban areas.

Recent statistical studies have demonstrated a positive impact of horizontal inequalities on the risk of armed civil conflict at the national, regional and ethnic group level (e.g. Cederman, Gleditsch & Weidmann, 2010; Østby, 2008; Østby, Nordås & Rød, 2009). As civil wars typically take place in rural areas,[3] this raises questions as to whether various kinds of inequality may have similar effects on urban violence. Sociological research in the United States has long identified racial inequalities as an important explanatory factor behind interracial violence in urban areas. A prominent example of this is Blau & Blau (1982) who in a study of American metropolitan areas, found that

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[3] For a study of (exceptional) urban insurgencies, see Staniland (2010).

socioeconomic inequality between races, as well as economic inequality generally, increases rates of urban violence. Apart from these studies of US cities, there has not been much research on inter-group inequality and urban violence. This does not imply that such inequalities should be of less importance. On the contrary, Brown (2008: 262) argues that:

> …the *experience* of horizontal inequality is rooted in locality and day-to-day interactions. The sense of relative deprivation experienced by a marginalized ethnic periphery vis-à-vis a distant and unvisited capital region, for instance, may be of much less political importance than inequalities relative to local residents who are seen as 'representative' of the dominant ethnic group, even if these latter inequalities are less severe.

The majority of rural-urban migrants tell researchers that they have moved to the city because of better opportunities for employment (Gilbert, 1999). If such expectations are not met, migrants may feel frustration and despair, particularly if they feel discriminated against due to their origin. This justifies the final hypothesis tested here:

> H4: *Greater relative deprivation of migrants compared to non-migrants is associated with higher levels of urban violence.*

3. Research Design

The study covers all cities which are both included in the Urban Social Disorder dataset (Urdal & Hoelscher, 2009) and whose country has hosted at least one Demographic and Health Survey (DHS), which will be described below. According to these criteria I obtain a sample consisting of 34 major cities in 31 different countries. As shown in Figure 1, most of the cities are located in developing countries, and the study covers 21 cities in Sub-Saharan Africa and 13 cities in Central- and East Asia. In total, I use information from 124,534 individual urban respondents from 89 different DHS surveys (covering 97 individual city-survey-years)[4], which equals an average of 1,284 respondents per city for each survey, and close to 3 surveys per city for the entire period. The unit of analysis is the city-year, covering the years 1986–2006.[5]

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[4] Two DHS countries have more than one city which is part of the present study: India (Calcutta, Mumbai, and New Delhi); Pakistan (Islamabad and Karachi). Hence the different numbers for surveys and city-survey-years.

[5] See Appendix D for a list of the DHS surveys used in the analyses.

Figure 1. Cities Covered in the Analyses

The new PRIO dataset on Urban Social Disorder (USD) (Urdal, 2008; Urdal & Hoelscher, 2009) is compiled from electronic news reports in the 'Keesing's Record of World Events' (KRWE). The dataset covers different forms of both violent and non-violent politically motivated disorder, including demonstrations, rioting, terrorism and armed conflict. Each event is coded with precise date and location. In this study I use two count measures of urban social disorder aggregated to the annual level for each city. Due to emerging evidence that lethal and nonlethal urban violence are not necessarily explained by the same factors (Buhaug & Urdal, 2009; Urdal & Hoelscher, 2009), I distinguish between *lethal events,* including only those events reported to have resulted in at least one death, and *non-lethal events*, which include only the events where no deaths were reported.[6]

For the spatio-temporal domain covered in this study (34 cities in 1986–2006), a total of 1,056 events are registered in the USD dataset, of which 476 (45.1%) are reported to have lead to fatalities. Correspondingly, 39.5% of all city-years covered here experienced non-lethal events, while 34.1% of the observations hosted at least one lethal

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[6] See Buhaug & Urdal (2009) for a discussion of potential caveats associated with the USD data.

event.[7] According to the USD data there has been a slight increase in the average number of urban disorder events (both lethal and non-lethal) over the last 50 years. This weak trend is also evident for the 34 cities and the time-period covered in the current study, although there is strong inter-annual variation (see also Urdal, 2008).

The map in Figure 2 displays the accumulate number of lethal and non-lethal events by city for the period 1986–2006. Larger pies indicate a higher number of urban social disorder events, and dark grey shade refers to lethal events, whereas light grey shade refers to non-lethal events. The map reveals that the cities with the highest prevalence of events are located in Asia, such as e.g. Dhaka, Jakarta, Karachi, and New Delhi but there are also a number of African cities which are among the hotspots of urban political violence, such as Johannesburg, Kinshasa, Lagos, and Nairobi.

Figure 2. Accumulated Urban Social Disorder Events by Type in 34 Cities, 1986–2006
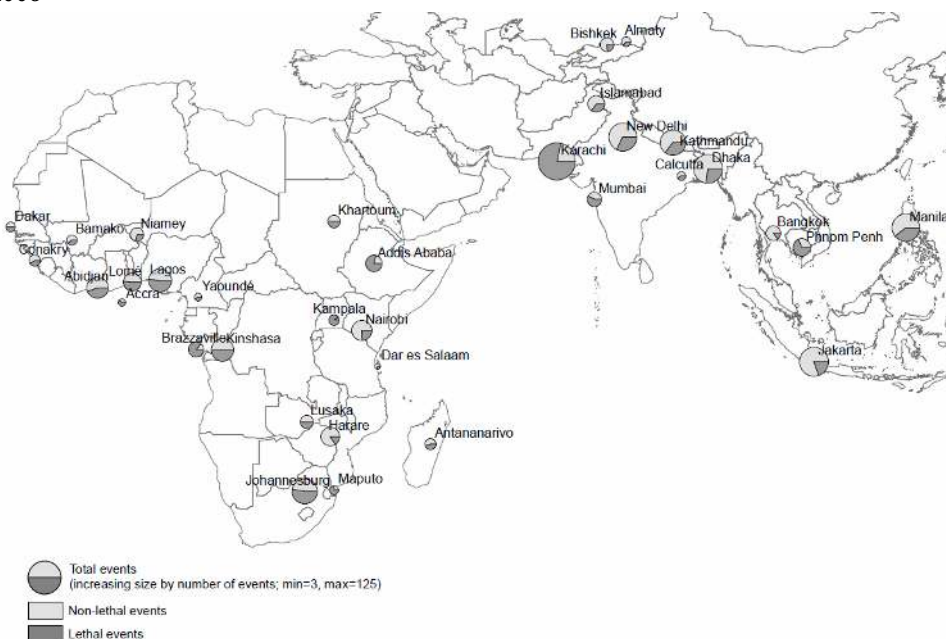
The main explanatory variables (used to test Hypotheses 1–4) are rural-urban in-migration, and various forms of absolute deprivation, inequality and migrant relative

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[7] See Appendix B for event counts by city.

deprivation. To my knowledge, this is the first study of urban violence to include such variables explicitly measured for each individual city. My new city-level data on in-migration and socioeconomic indicators are computed from the Demographic and Health Surveys (DHS).[8]

MEASURE DHS is a worldwide ongoing research project providing data and analysis on the population, health, and nutrition of women and children in developing countries. Since 1984, the DHS project has carried out more than 240 surveys in 85 countries. In a DHS, a sample of households is selected throughout the entire country. Women between the ages of 15 and 49 are interviewed about health, nutrition, and other issues, such as ethnicity, education, housing quality, and access to public services like drinking water and sanitation facilities. Typically, DHS surveys cover more than 10,000 respondents representing urban and rural areas and provinces/states. DHS surveys are conducted every four to five years in most countries, with the same questions asked in each survey to facilitate comparisons across time and space. This provides a rich data source from which one can construct indicators of in-migration as well as absolute and relative poverty at the city level. The DHS hence provide a unique opportunity for generating new data that are hard to find elsewhere, such as measures of inequality and social exclusion, which tend to be politically sensitive.

The process of matching the data on urban disturbances with survey data on individuals and households from the DHS implies some challenges. The first step before I could generate the explanatory variables from DHS was to identify the respondents for each city in question. In the standard DHS survey form, not even the city name is provided for urban residents; it only includes a variable for the region of residence of each respondent. Furthermore, the DHS definition of 'region' is often broader and cruder than the official first-level administrative units. But recently the DHS has begun to include detailed information about the exact location of each sample cluster, providing geographical coordinates for each surveyed location (village/town/city). This opens up the possibility to aggregate specific city-level measures, which is described in the following procedure:

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[8] See www.measuredhs.com.

First, each file with DHS survey coordinates was imported into ArcGIS and overlaid with shapefiles of African and Asian cities (Cederman, Buhaug & Rød, 2009) and first level administrative units (ESRI, 1998). I then identified all the coordinates which fall inside the administrative unit in which the city in question is located,[9] as illustrated in Figure 3 below. The figure shows a map of Nigeria with administrative units, zooming in the national capital region of Lagos. The black dots indicate the location of the DHS survey clusters. I selected the dots which fall within the same polygon as Lagos city.[10] Next, I merged each survey file to the corresponding geographic file with the cluster coordinates so that each respondent is associated with a specific geographical location. I censored all rural respondents, so that the basis for constructing city variables truly reflected the situation of city dwellers, not including rural respondents in the surrounding areas. However, in most cases, this was unnecessary, as all respondents within the small region of each city were actually urban residents. Finally, when the city dweller respondents had been identified for each survey,[11] I calculated aggregate city-level measures as described below.

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[9] The reason for doing this rather than selecting only the coordinates that overlap with the city coordinates, is to maximize the number of coordinates (and hence the number of respondents).

[10] In some cases this procedure required some manual corrections (see Appendix C).

[11] For surveys lacking GIS data I selected respondents from the DHS-defined region surrounding the city.

Figure 3. Distribution of DHS Survey Sample Points in Nigeria, 1990
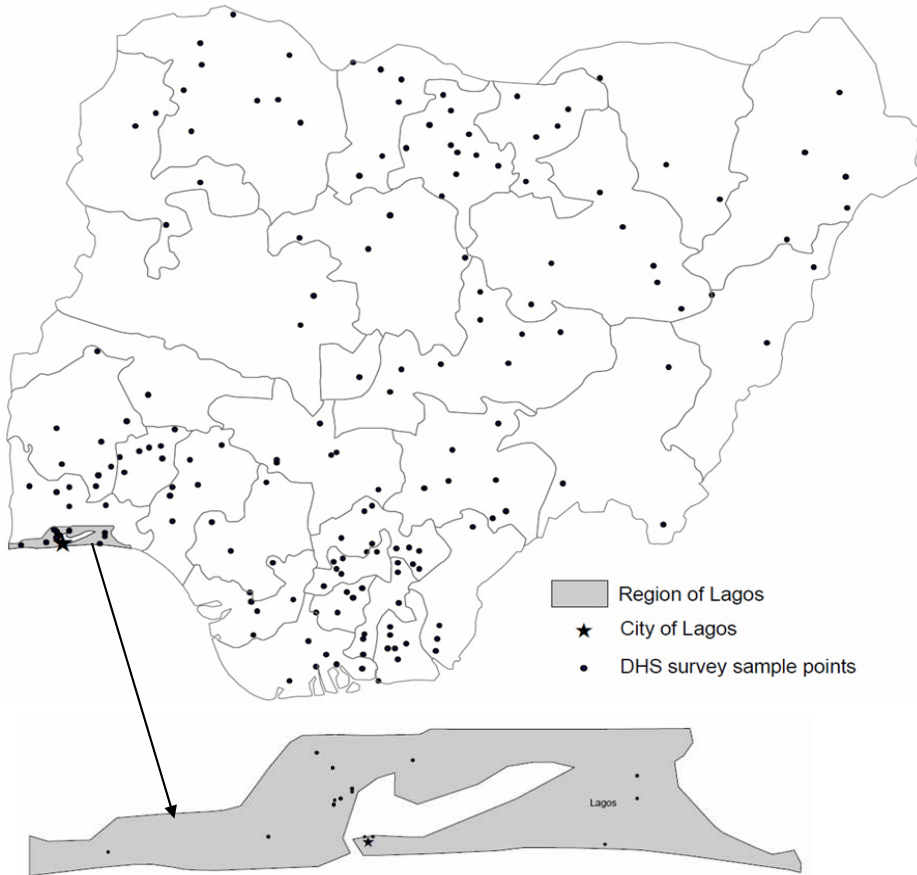
3.1. *Generating the main explanatory variables*

In order to test the first hypothesis I construct a variable for rural-urban in-migration (rural-urban migration). A big advantage with the DHS surveys is that they report the number of years each respondent has lived in the village, town, or city where s/he was interviewed, as well as information about the previous place of residence. This facilitates generating migration-induced urbanization for each city, which constitutes a refinement compared to previous studies, which rely on overall urban population growth to proxy urbanization. More specifically, following Omariba & Boyle (2009), the term for rural-urban migration is calculated as the percentage of city dwellers who have resided in the city in question less than five years, and whose previous area of residence was characterized as 'countryside' For comparison, I also calculate a more general in-

migration term not distinguishing between the migrants who previously lived in other cities, towns or the countryside. Respondents who are just visitors to the city are excluded for both terms. As shown in Appendix A, the average share of total in-migration to a city is app. 22%, whereas the average share of rural-urban migration is app. 7%. Hence, at least for my sample, the majority of in-migrants actually come from other towns or cities.

In the current development debate, poverty is often associated with the lack of both tangible and non-tangible assets (e.g. Sen, 1992; Stewart, 2008). Based on the assumption that various environmental and socioeconomic factors could have different violence potential, I include a wide set of indicators for absolute and relative deprivation. The DHS surveys do not contain information on incomes or consumption expenditure, but I rely on information on consumer durables. In poor countries, where most people are part of the informal sectors, household goods might better capture variations in welfare than GDP per capita (Filmer & Pritchett, 2001). Hence, I first construct an additive index of household standard of living in cities based on ownership of *household assets*: TV, radio, refrigerator, bicycle, car, and motorcycle. I then calculate the average ownership of such assets per city. The measure potentially reaches from 0 (no one owns a single asset) to 1 (everyone possesses all assets). My second indicator of city-level welfare is average *years of education* completed for city dwellers. Finally, I add a third dimension: *Access to public services (PS).* This measure is an additive index composed of three variables: whether each household has piped water, a flush toilet, and electricity*).*[12] This is constructed the same way as the asset measure, so that higher values indicate more welfare. I argue that such indicators of public services can be used as a basic measure of urban quality of life, and they provide a fundamental reflection of equity in policy.

In order to test the third hypothesis, I also calculate Gini coefficients[13] of intra-city inequality between individuals with regard to the three dimensions assets, education

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[12] Conceptually, this index bears resemblance to the UN's definition of a slum household (see Thomas, 2008: 65).

[13] The Gini coefficient ranges from 0 (totally egalitarian distribution) to 1 (complete concentration).

and PS. Finally, for the purpose of testing Hypothesis 4, I combine the information on respondents' well-being and settlement history to construct measures on inequalities between rural-urban migrants and non-migrants. Using the formula provided by Østby, Nordås & Rød (2009) I calculate rural-urban *migrant relative deprivation (MRD)* as the relative performance of each group of rural-urban migrants compared to the performance of the rest of the city population:

$$RRD = -1\left(\ln\left[\sum_{i=1}^{M} \frac{A_{i1}/A_{i2}}{M} \right] \right) \tag{1}$$

where M is the maximum number of household assets and A_1 refers to mean asset score of the migrant population and A_2 is the corresponding mean score of other residents in the city. The value '0' indicates perfect equality, negative values indicate relative deprivation of migrants and positive values indicate relative privilege of migrants. In fact if one compares all migrants (including both rural-urban and urban-to urban migrants) there is hardly any difference at all between this group and the rest of the city population (which can be seen from the near to zero averages for these terms in Appendix A). However, the differences are much stronger between rural-urban migrants and the 'born' city dwellers, usually in favor of the latter group.

Summing up, in addition to the pure rural-urban migration measure, I construct measures if overall absolute deprivation; intra-individual inequality and migrant relative deprivation along the three dimensions assets, education, and public services.[14]

3.2. Control variables and statistical model

Following Buhaug & Urdal (2009) I include the following controls: *city population size* (UN, annual); *level of (national) development,* represented by log-transformed real GDP per capita (World Bank, 2009), *(national) economic growth* (annual growth in GDP per capita); *regime type* (Marshall & Jaggers, 2003). Also, by the aid of ArcGIS I construct a variable measuring the distance in kilometers from each city to the closest armed conflict in the year of observation in order to control for potential spillover effects of ongoing armed

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[14] Since the DHS surveys are not conducted annually, I interpolate, and extrapolate as appropriate to maximize the number of observations.

conflicts.[15] More specifically, I measured the shortest distance from each city point to the border of the closest armed conflict zone. The latter information (geographical coordinates for conflict centre as well as radius corresponding to conflict circle) is available from the PRIO Conflict Site dataset.[16] Appendix A provides descriptive statistics for all the variables included in the analyses.

The dependent variable is an integer count of events, within a cross-sectional time-series structure. I use negative binomial regression to analyze the data. Based on the residuals from a panel-corrected OLS model, auto-correlated errors do not seem to be a problem in this sample.[17] Lagged dependent variables are therefore not included. The negative binomial regression is able to correct for data series where the variance is different from the mean. However, in this sample, over-dispersion is city-specific. Hence, fixed effects are used to produce regression estimates conditional on each city's unique over-dispersion.[18]

4. Results

This section reports the results from the multivariate empirical evaluations of the proposed Hypotheses 1–4, which are conducted by means of three sets of regression models represented in Tables 1–3 below. All the models include the terms for city population, the country-level controls for GDP per capita, economic growth, and regime type, as well as the distance from each city to the nearest ongoing armed conflict. Finally, each model includes one of the main explanatory variables: the percentage of rural-urban migration, measured at the city-level. Since the other explanatory variables are closely associated with each other, both conceptually and correlationally, the terms for poverty,

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[15] For generating the distance measure I apply the Winkel-Tripel map projection, which, according to Goldberg & Gott (2007) is the best overall whole-earth map projection known, producing very small distance errors, small combinations of ellipicity and area errors, and the smallest skewness of any map.

[16] See http://www.prio.no/CSCW/Datasets/Armed-Conflict/Conflict-Site/.

[17] Using the Stata 11.0 xtregar procedure, Durbin/Watson values were consistently within the acceptable interval for a number of different model specifications (Baltagi & Wu, 1999).

[18] It is only the over-dispersion parameter that is city-specific. The intercept is estimated as normal.

inequality and migrant relative deprivation are introduced in separate models. Since lethal and non-lethal urban disturbances are reported to be associated with slightly different patterns (Urdal & Hoelscher, 2009), I use two different dependent variables, so that each model is tested with regard to both lethal and non-lethal events.

Table 1 displays the results of the basic model including the term for rural-urban migration as well as the three city-level measures of absolute deprivation (average level of household asset ownership; education years, and access to public services). Contrary to Hypothesis 1, there seems to be no effect of the level of rural-urban migration on the frequency of political disorder, be it lethal or non-lethal. All the coefficients are positive, but the effect never reaches significance in Models 1a–3b. Buhaug & Urdal (2009) find similar results for their more crude measure of urbanization (overall urban population growth).[19]

The terms for urban absolute deprivation, or poverty, produce some mixed findings. Overall, Models 1a–3b yield little support to Hypothesis 2, assuming a positive relationship between poverty and urban violence. The one exception to this is Model 2a, which reveals a slightly significant negative effect of education on the number of lethal events. In other words, cities with less educated inhabitants are likely to see more events of lethal urban disorder. Contrastingly, the coefficients for the other two alternative measures of absolute deprivation have signs in the opposite direction, and the positive effect of public services is even statistically significant.

Whereas the finding that cities with better overall access to basic public services see more events of non-lethal violence is hard to qualify, the control variables provide a clearer picture as to what explains urban violence. The term for city population is constantly associated with disorder, although it never reaches statistical significance.[20]

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[19] I also tried to replace the term for rural-urban migration with all urban in-migration (including urban-urban migration). This also enables me to include four additional cities in the analysis: Colombo, Hanoi, Saigon, and Tashkent (since the DHS in these cities do have information on years lived in the present location, although they lack sufficient information to distinguish between rural-urban and urban-urban migrants). However, the coefficient for migration remained statistically insignificant, regardless of the inclusion of these four cities (results not reported here).

[20] Buhaug & Urdal (2009) and Urdal & Hoelscher (2009) find a positive and significant effect of city population with a bigger sample.

Somewhat counter-intuitively, higher national income levels are associated with increased levels of political violence in all models below, although the effects are only statistically significant for non-lethal disturbance. This could be interpreted so that increasing development implies more to loot or fight over among city dwellers. However, since the term is measured at the national level it is hard to draw any clear conclusions.[21] Again, coinciding with the findings of Urdal & Hoelscher (2009) and Buhaug & Urdal (2009), the negative squared term for regime type indicates that semi-autocratic regimes appear to have higher levels of both lethal and non-lethal events than both autocracies and democracies. Furthermore, there seems to be a clear negative effect of distance to nearest armed conflict on the frequency of lethal events.

Table 1. Rural-Urban Migration, Absolute Deprivation and Urban Social Disorder

	1a Lethal	1b Non-lethal	2a Lethal	2b Non-lethal	3a Lethal	3b Non-lethal
R-U migration	2.242	3.312	1.687	1.608	0.751	2.787
	(4.403)	(3.210)	(4.026)	(3.0800)	(3.644)	(2.842)
Assets	2.011	2.076				
	(2.169)	(1.487)				
Education			−0.156*	0.059		
			(0.093)	(0.086)		
Public services					1.246	2.389***
					(0.834)	(0.722)
City population	0.0024	0.0023	0.0035	0.0018	0.0027	0.00062
	(0.0042)	(0.0037)	(0.0039)	(0.0038)	(0.0040)	(0.0038)
GDP capita	0.191	0.566**	0.386	0.579**	0.216	0.603**
	(0.308)	(0.258)	(0.274)	(0.273)	(0.305)	(0.270)
Growth	−6.096***	−6.614***	−6.051***	−6.491***	−5.825***	−6.014***
	(1.423)	(1.222)	(1.434)	(1.235)	(1.470)	(1.308)
Polity	0.0066	−0.0033	0.013	−0.0019	0.0094	−0.0085
	(0.014)	(0.012)	(0.013)	(0.012)	(0.013)	(0.012)
Polity, sq.	−0.013***	−0.0076**	−0.014***	−0.0083***	−0.014***	−0.0083***
	(0.0032)	(0.0029)	(0.0032)	(0.0029)	(0.0032)	(0.0029)
Distance conf.	−0.00045***	−0.00025*	−0.00046***	0.00022	−0.00041***	0.00017
	(0.00016)	(0.00014)	(0.00016)	(0.00014)	(0.00016)	(0.00013)
Constant	−0.871	−3.824**	−0.257	−3.202**	−0.983	−4.489***
	(1.795)	(1.500)	(1.548)	(1.508)	(1.795)	(1.550)
χ^2	51.34	47.09	52.11	44.79	52.24	53.04
Observations	693	693	693	693	693	693
Number of cities	34	34	34	34	34	34

Negative binomial regression with fixed effects. Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

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[21] I also tested the effect of a more disaggregated measure of economic output based on Nordhaus' (2006) G-Econ dataset, which records economic output per 1 x 1 degree grid cell (for details, see http://gecon.yale.edu/) This also returned a positive, although insignificant, coefficient for nonlethal events, and a negative insignificant coefficient for lethal events.

In Table 2, the models are estimated with the same controls as in Table 1. However, here I introduce three measures of intra-individual inequality along the three dimensions of household welfare, education, and access to basic public services. The results are a bit unclear. Contrary to Hypothesis 3, the inequality terms are actually negative in 4 out of 6 models, and even significantly so for the case of asset inequality and non-lethal disturbance. On the other hand, with regard to lethal disturbance events, there seems to be a positive impact of inequality in terms of access to education, as shown in Model 5a. That is, in cities where the inhabitants have very unequal access to education, the prevalence of lethal violence is higher. This provides some support for Hypothesis 3, and indicates that city governments should aim to increase the educational opportunities to include the less privileged strata of society.

Table 2. Testing the Effect of Inequality on Urban Social Disorder

	4a Lethal	4b Non-lethal	5a Lethal	5b Non-lethal	6a Lethal	6b Non-lethal
R-U Migration	0.778	4.276	2.527	1.785	0.907	1.854
	(3.965)	(3.046)	(4.123)	(3.058)	(4.045)	(3.027)
Gini (Assets)	−0.124	−4.205**				
	(2.610)	(2.013)				
Gini (Education)			2.587**	−0.168		
			(1.239)	(1.209)		
Gini (PS)					0.259	−0.458
					(1.069)	(0.967)
City population	0.0031	0.00060	0.0023	0.0024	0.0031	0.0024
	(0.0042)	(0.0039)	(0.0040)	(0.0037)	(0.0041)	(0.0037)
GDP capita	0.252	0.512*	0.396	0.609**	0.261	0.598**
	(0.311)	(0.273)	(0.266)	(0.272)	(0.304)	(0.264)
Growth	−6.040***	−6.491***	−6.048***	−6.527***	−6.043***	−6.523***
	(1.425)	(1.217)	(1.437)	(1.239)	(1.421)	(1.242)
Polity	0.010	−0.0070	0.010	−0.0011	0.010	−0.0018
	(0.014)	(0.013)	(0.013)	(0.012)	(0.013)	(0.012)
Polity, sq.	−0.014***	−0.0081***	−0.014***	−0.0077***	−0.014***	−0.0077***
	(0.0032)	(0.0029)	(0.0032)	(0.0029)	(0.0032)	(0.0029)
Distance conf.	−0.00043***	−0.00024*	−0.00047***	0.00022	−0.00044***	0.00021
	(0.00015)	(0.00014)	(0.00016)	(0.00014)	(0.00016)	(0.00014)
Constant	−0.334	−1.042	−2.368	−2.994*	−0.486	−2.917*
	(2.223)	(1.836)	(1.734)	(1.806)	(1.799)	(1.534)
χ2	50.72	49.58	53.21	44.39	50.91	44.33
Observations	693	693	693	693	693	693
Number of cities	34	34	34	34	34	34

Negative binomial regression with fixed effects. Standard errors in parentheses.* significant at 10%; ** significant at 5%; *** significant at 1%.

Finally, in Table 3, I test the forth hypothesis, introducing to the model the terms for relative deprivation of in-migrants coming to the city from rural areas. Again, the measures are calculated along the three dimensions of general household welfare, education, and access to public services. Two result stand out: First, Model 7a reveals

that there seems to be a stronger violence potential for relative household deprivation of city dwellers that have migrated to the city from rural areas. In other words, systematic deprivation of rural-urban migrants with regards to household assets is positively associated with lethal urban disorder.

Table 3. Testing the Effect of Migrant Relative Deprivation on Urban Social Disorder

	7a Lethal	7b Non-lethal	8a Lethal	8b Non-lethal	9a Lethal	9b Non-lethal
R-U Migration	0.533	1.655	–0.140	1.880	–0.116	1.397
	(3.848)	(3.116)	(3.827)	(3.075)	(3.956)	(3.114)
MRD (Assets)	2.169**	0.165				
	(0.881)	(0.728)				
MRD (Education)			0.681	–0.060		
			(0.505)	(0.351)		
MRD (PS)					1.305*	0.416
					(0.765	(0.676
City population	0.0011	0.0023	0.0025	0.0025	0.0026	0.0023
	(0.0044)	(0.0037)	(0.0040)	(0.0037)	(0.0041)	(0.0037)
GDP capita	0.285	0.630**	0.239	0.619**	0.239	0.624**
	(0.337)	(0.265)	(0.295)	(0.260)	(0.324)	(0.263)
Growth	–6.035***	–6.529***	–5.986***	–6.535***	–6.000***	–6.520***
	(1.431)	(1.241)	(1.416)	(1.239)	(1.425)	(1.240)
Polity	0.0086	–0.0010	0.0092	–0.00091	0.011	–0.0011
	(0.013)	(0.012)	(0.013)	(0.012)	(0.013)	(0.012)
Polity, sq.	–0.014***	–0.0077***	–0.014***	–0.0077***	–0.013***	–0.0077***
	(0.0032)	(0.0029)	(0.0032)	(0.0029)	(0.003)	(0.0029)
Distance conf.	–0.00046***	0.00022	–0.00043***	0.00022	–0.00046***	–0.00024*
	(0.00016)	(0.00014)	(0.00016)	(0.00014)	(0.00016)	(0.00014)
Constant	–0.771	–3.223**	–0.409	–3.128**	–0.316	–3.148**
	(1.921)	(1.508)	(1.725)	(1.460)	(1.862)	(1.474)
χ2	56.33	44.33	52.55	44.34	53.84	44.80
Observations	693	693	693	693	693	693
Number of cities	34	34	34	34	34	34

Negative binomial regression with fixed effects. Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

The map in Figure 4 displays the mean level of relative deprivation and accumulated number of lethal events for each of the 34 cities over the entire period, with white circles representing the number of lethal events and black circles representing the level of migrant relative deprivation. The level of overlap (indicated by gray shade) suggest in which cities this relationship indeed seems to play out, such as New Delhi and Johannesburg.

Figure 4. Migrant Relative Deprivation and Total Lethal Events in 34 Cities, 1986–2006
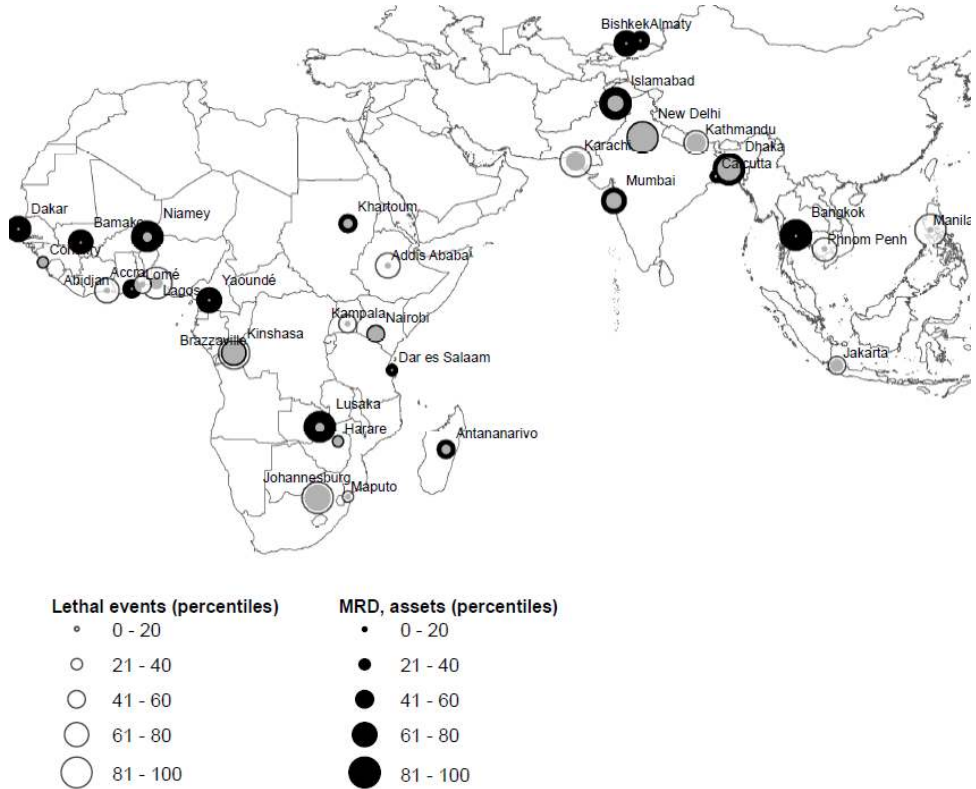
The predictive power of Model 7a is visualized in Figure 5 using the Clarify package (King, Tomz & Wittenberg, 2000). The vertical axis shows the number of estimated lethal events per year, and the horizontal axis represents the level of migrant relative deprivation. The line represents the best, or the most frequent, estimate, while the bars represent an 80% confidence interval.[22] As we see the number of lethal events is relatively low in the right side of the graph. The gradual increase of the curve gains momentum when the relative deprivation of migrants is larger than .3, and on the right hand side of the scale, we expect at least 3 events per year, most likely 6 and maybe as many as 14, with 80% certainty, whereas the parallel figures for a situation with parity would be 2, 3 and 4. These estimates are simulated for a city with all other parameters equal to that of New Delhi.

[22] The 80% interval was chosen to illustrate the effect, not to test any hypothesis. Using the normal 95% confidence interval, the illustration would be more dramatic and less effective.

Figure 5. Estimated Number of Annual Violent Events by Migrant Relative Deprivation
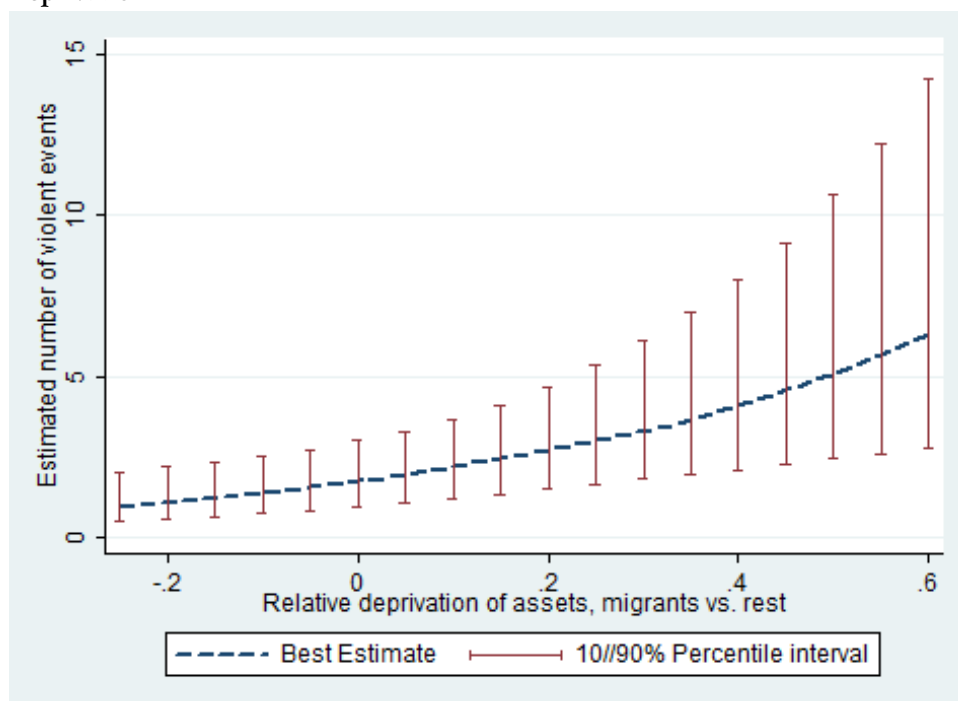
The positive coefficient for migrant relative deprivation holds when the term for household assets is replaced with public services in Model 9a. Migrant relative deprivation with regard to education has opposite signs (but neither is significant) with regard to lethal and non-lethal events.[23] The lack of effect with regard to educational marginalization of migrants (Model 8a), is perhaps not so surprising. Assuming that a fair amount of the people who migrate from the rural areas to the cities have already passed the typical age for undertaking education, it is a less valid grievance indicator that born city dwellers are on average more highly educated. However, if moving to the city does not lead to higher living standards in terms of actual household welfare and access to basic social services, this could arguably generate more frustration. This underscores the need for increased public sector investments to ensure inclusive basic service delivery in urban areas.

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[23] Replacing my MRD measure with Deiwiks, Cederman & Gleditsch' (2010) formula for 'low' inequality (Migrant score (g) /Non-migrant score (G) if g < G, 0 otherwise) does not change the results.

The findings reported above are robust to alternative specifications, outlier analysis, and the inclusion of the lagged dependent variable. Also, models with a binary dependent variable[24] produce similar results. Finally, there could be an endogeneity problem with my analysis given that some of the independent variables are based on surveys which were carried out late in the sample period. In order to account for this potential problem, I reran the tests using only the years in which a DHS survey was conducted and allowing for interpolation between these. The results remained largely unchanged.

5. Conclusions

Urban populations, particularly in developing countries, will continue to grow over the next decades. Much of this urban growth is due to increasing rural-urban migration, which is partly caused by climate change and environmental scarcity in the countryside. Simultaneously, the amount of urban violence has been increasing since the 1960s (Urdal, 2008), putting large numbers of urban dwellers at risk. Citywide violence may also have serious global effects, such as destabilizing worldwide financial markets and destroying infrastructure, thereby affecting already fragile national economies, or igniting violence in entire regions (Rosan, Ruble & Tulchin, 2000). Hence, there is a great need for studies enhancing our knowledge on the determinants of urban disturbances.

To my knowledge, this is the first study to systematically investigate the relationship between in-migration, socioeconomic well-being and political violence with city-level data. Applying GIS tools on national demographic and Health Surveys (DHS), I have generated city-level measures of absolute and relative deprivation along three dimensions: household assets; educational attainment; and access to basic public services, such as drinking water, sewage, and electricity. Based on these three dimensions I have tested the violence potential of absolute deprivation, intra-individual inequality (Gini), as well as inter-group inequality, or relative deprivation of rural-to urban migrants compared to the rest of the city population.

Existing quantitative research on the security consequences of high urban population growth has produced little support for the widespread concern over

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[24] The alternative variable takes the value 1 if a city-year experiences >1 lethal event.

increasing urban violence. With a more nuanced measure of rural-urban migration, this study adds momentum to the general conclusion that urbanization does not seem to foster social urban disturbance in itself. This should relax official attempts to reduce the magnitude of cityward migration per se, as it is not the movement of people as such that seems to create problems. I do find, however, that both inter-individual and inter-group inequalities between migrants and non-migrants seem to matter for lethal forms of urban political violence, which entails some clear policy implications. City governments and other decision-makers are best advised to aim at facilitating more equitable access to education and basic social services among city dwellers. Establishing formal institutions to help migrants assimilate into the social and economic life of the city should mitigate social fragmentation and reduce the level of urban violence.

Admittedly, the present study is associated with some shortcomings and challenges, which should inspire future efforts within this research portfolio. For example, one might question whether the DHS surveys are sufficiently representative at the city level (see e.g. Montgomery, 2003). However, because the sample of cities consists of national capitals and other large cities, this should not present a significant problem, at least regarding the poverty and inequality terms. But in the absence of national census data, the migration figures should be interpreted with some caution (see e.g. Bhagat, 2005). Furthermore, the DHS data only cover a limited number of cities and years. Future studies should look for other potential sources for coding city-level variables on socioeconomic well-being.

The combination of GIS tools and survey data opens up a lot of possibilities in terms of new data generation. However, while aggregated city level studies may yield important insights into systematic patterns between structural explanatory variables like migrant relative deprivation and urban violence, such studies (including this one) involve critical assumptions about individual-level behavior that may be potentially flawed. Even where inequalities are associated with violence, assuming that it is the personally most aggrieved individuals who are engaging in violence could, in principle, constitute an ecological fallacy. National surveys like the DHS do not usually provide information about perceptions of (or involvement in) political violence, or reasons for migrating to the city. This underscores the need for carefully designed micro-level studies.

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Appendices

APPENDICES Chapter 1

Appendix A1. New Tests with Updated Data on HIs

The current updating of the DHS project makes it possible to continuously update the HI data. Many new countries and surveys have been included since the two first chapters in this dissertation (Østby, 2008a,b) were published. In order to check whether my original results remain robust in a larger sample of countries, I have updated the data and rerun some basic tests of the effect from HIs to civil conflict onset. The updated study includes data from 177 national surveys in 73 countries, with more than 11,000 respondents per survey on average. In fact, the analyses in this paper are based on 2,016,932 individuals (women). Appendix A2 provides a complete list of the surveys (countries and survey years) used to generate the HI variables. As demonstrated in Figure A1 (taken from Østby & Strand, 2010), the results from Chapters 1 & 2 remain largely the same. Based on this updated dataset I have also singled out the effects for the African countries, to investigate whether HIs may be particularly relevant in Africa (Østby & Strand, 2010).

Figure A1. Six Horizontal Inequality Measures Compared, Global vs. SSA Sample
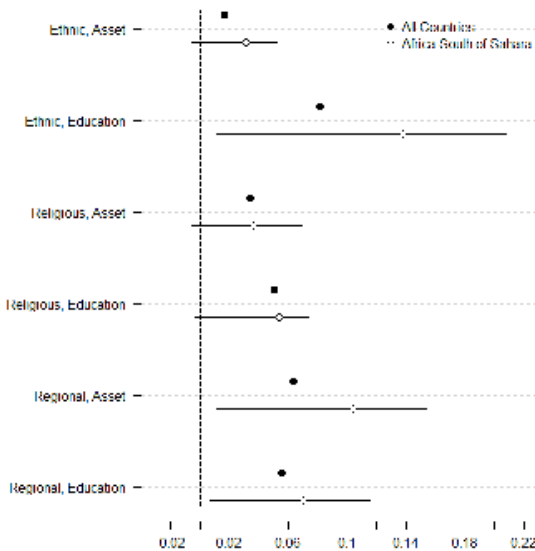
See Appendix A3 for full multiple regression model output. Control variables are economic development, population size and conflict history.

Figure A1 reports the effects of socioeconomic horizontal inequalities (measured in terms of household assets and education years) with regard to conflict onset. I test the impact of HIs between ethnic, religious, and regional groups respectively for the Sub Saharan Africa sample vs. the global sample. The figure summarizes the results from 12 regression models with six different measures of horizontal inequalities and two different samples (see Appendix A3 for full regression model output). I look at two dimensions of inequality (household assets and education) which are both calculated for three different group identifiers (ethnic, religious, and regional groups). The six measures are applied to both the global sample of countries with DHS surveys and Africa South of Sahara (SSA), and all models include control variables are economic development, population size and conflict history. The dots in Figure A1 refer the estimated difference in the risk of conflict onset between a country with very low and very high horizontal inequalities (5th and 95th percentile on the corresponding HI measure) when both of these countries are otherwise similar and typical for the sample median (quite similar to Guinea for the SSA sample). The lines are the 95% confidence intervals for this difference of conflict risk.

Overall, Figure A1 reveals that three of the HI measures stand out as particularly strong and significant for the global sample: Ethnic inequality in terms of education and both of the measures of regional divides (i.e. inequality in terms of both household assets and education between the capital region and other regions) Religious divides do not appear to be very important (although both effects are significant at the 10 and 5 percent level of significance, respectively, as can also be seen in Appendix A3). Ethnically based asset inequality is positive but the effect is not statistically significant. Both asset and education yield strong and substantially important effects for regional inequality, which also is estimated on the largest number of cases.[1] While the uncertainty regarding the estimates for ethnic- and religious-based asset inequality is larger than the accepted level of 95%, the over-all impression from Figure A1 is that horizontal inequality is strongly linked with armed conflict.

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[1] The reason for the differences in N is the different availability of data. All the DHS surveys include questions regarding regional affiliation, but several surveys exclude questions about ethnic and religious affiliations.

Appendix A2. DHS Surveys Used to Generate the HI Variables

No.	GWno	GWname	Year(s)	DHS code	Respondents
1	41	Haiti	1994-95	HTIR31	5,356
2	41	Haiti	2000	HTIR42	10,159
3	41	Haiti	2005-06	HTIR52	10,757
4	42	Dominican Republic	1986	DRIR01	7,645
5	42	Dominican Republic	1991	DRIR21	7,320
6	42	Dominican Republic	1996	DRIR32	8,422
7	42	Dominican Republic	1999	DRIR41	1,286
8	42	Dominican Republic	2002	DRIR4A	23,384
9	42	Dominican Republic	2007	DRIR52	27,195
10	52	Trinidad and Tobago	1987	TTIR01	3,806
11	70	Mexico	1987	MXIR00	9,310
12	90	Guatemala	1987	GUIR01	5,160
13	90	Guatemala	1995	GUIR34	12,403
14	91	Honduras	2005-06	HNIR51	19,948
15	93	Nicaragua	1998	NCIR31	13,634
16	93	Nicaragua	2001	NCIR41	13,060
17	100	Colombia	1986	COIR01	5,329
18	100	Colombia	1990	COIR22	8,644
19	100	Colombia	1995	COIR31	11,140
20	100	Colombia	2000	COIR41	11,585
21	100	Colombia	2005	COIR51	41,344
22	110	Guyana	2005	GYIR50	4,300
23	130	Ecuador	1987	ECIR01	4,713
24	135	Peru	1986	PEIR01	4,999
25	135	Peru	1991-92	PEIR21	15,882
26	135	Peru	1996	PEIR31	28,951
27	135	Peru	2000	PEIR41	27,843
28	135	Peru	2004-08	PEIR50	12,465
29	140	Brazil	1986	BRIR01	5,892
30	140	Brazil	1991	BRIR21	6,223
31	140	Brazil	1996	BRIR31	12,612
32	145	Bolivia	1989	BOIR01	7,923
33	145	Bolivia	1994	BOIR31	8,603
34	145	Bolivia	1998	BOIR3B	11,187
35	145	Bolivia	2003	BOIR41	17,654
36	150	Paraguay	1990	PYIR21	5,827
37	359	Moldova	2005	MBIR52	7,440
38	369	Ukraine	2007	UAIR51	6,841
39	371	Armenia	2000	AMIR42	6,430
40	371	Armenia	2005	AMIR53	6,566
41	373	Azerbaijan	2006	AZIR51	8,444
42	432	Mali	1987	MLIR01	3,200
43	432	Mali	1995-96	MLIR32	9,704
44	432	Mali	2001	MLIR41	12,849
45	432	Mali	2006	MLIR52	14,583
46	433	Senegal	1986	SNIR02	4,415
47	433	Senegal	1992-93	SNIR21	6,310
48	433	Senegal	1997	SNIR32	8,593
49	433	Senegal	2005	SNIR4H	14,602
50	434	Benin	1996	BJIR31	5,491
51	434	Benin	2001	BJIR41	6,219
52	434	Benin	2006	BJIR50	17,794
53	436	Niger	1992	NIIR22	6,503

54	436	Niger	1998	NIIR31	7,577
55	436	Niger	2006	NIIR51	9,223
56	437	Cote D'Ivoire	1994	CIIR35	8,099
57	437	Cote D'Ivoire	1998-99	CIIR3A	3,040
58	438	Guinea	1999	GNIR41	6,753
59	438	Guinea	2005	GNIR52	7,954
60	439	Burkina Faso (Upper Volta)	1993	BFIR21	6,354
61	439	Burkina Faso (Upper Volta)	1998-99	BFIR31	6,445
62	439	Burkina Faso (Upper Volta)	2003	BFIR43	12,477
63	450	Liberia	1986	LBIR01	5,239
64	450	Liberia	2007	LBIR51	7,092
65	452	Ghana	1988	GHIR02	4,488
66	452	Ghana	1993	GHIR31	4,562
67	452	Ghana	1998	GHIR41	4,843
68	452	Ghana	2003	GHIR4A	5,691
69	452	Ghana	2008	GHIR5H	4,916
70	461	Togo	1988	TGIR01	3,360
71	461	Togo	1998	TGIR31	8,569
72	471	Cameroon	1991	CMIR22	3,871
73	471	Cameroon	1998	CMIR31	5,501
74	471	Cameroon	2004	CMIR44	10,656
75	475	Nigeria	1990	NGIR21	8,781
76	475	Nigeria	1999	NGIR41	9,810
77	475	Nigeria	2003	NGIR4B	7,620
78	475	Nigeria	2008	NGIR51	33,385
79	481	Gabon	2000	GAIR41	6,183
80	482	Central African Republic	1994-95	CFIR31	5,884
81	483	Chad	1996-97	TDIR31	7,454
82	483	Chad	2004	TDIR41	6,085
83	484	Congo	2005	CGIR50	7,051
84	490	Congo, Democratic Republic of (Zaire)	2007	CDIR50	9,995
85	500	Uganda	1988-89	UGIR01	4,730
86	500	Uganda	1995	UGIR33	7,070
87	500	Uganda	2000	UGIR41	7,246
88	500	Uganda	2006	UGIR51	8,531
89	501	Kenya	1989	KEIR03	7,150
90	501	Kenya	1993	KEIR33	7,540
91	501	Kenya	1998	KEIR3A	7,881
92	501	Kenya	2003	KEIR41	8,195
93	510	Tanzania/Tanganyika	1991-92	TZIR21	9,238
94	510	Tanzania/Tanganyika	1996	TZIR3A	8,120
95	510	Tanzania/Tanganyika	2003	TZIR41	4,029
96	510	Tanzania/Tanganyika	2004	TZIR4I	10,329
97	510	Tanzania/Tanganyika	2007-08	TZIR50	16,318
98	516	Burundi	1987	BUIR01	3,970
99	517	Rwanda	1992	RWIR21	6,551
100	517	Rwanda	2000	RWIR41	10,421
101	517	Rwanda	2005	RWIR52	11,321
102	530	Ethiopia	2000	ETIR41	15,367
103	530	Ethiopia	2005	ETIR50	14,070
104	541	Mozambique	1997	MZIR31	8,779
105	541	Mozambique	2003	MZIR41	12,418
106	551	Zambia	1992	ZMIR21	7,060
107	551	Zambia	1996	ZMIR31	8,021
108	551	Zambia	2001-02	ZMIR42	7,658
109	551	Zambia	2007	ZMIR50	7,146

110	552	Zimbabwe (Rhodesia)	1988	ZWIR01	4,201
111	552	Zimbabwe (Rhodesia)	1994	ZWIR31	6,128
112	552	Zimbabwe (Rhodesia)	1999	ZWIR41	5,907
113	552	Zimbabwe (Rhodesia)	2005-06	ZWIR51	8,907
114	553	Malawi	1992	MWIR22	4,849
115	553	Malawi	2000	MWIR41	13,220
116	553	Malawi	2004	MWIR4C	11,698
117	560	South Africa	1998	ZAIR31	11,735
118	565	Namibia	1992	NMIR21	5,421
119	565	Namibia	2000	NMIR41	6,755
120	565	Namibia	2006-07	NMIR51	9,804
121	570	Lesotho	2004	LSIR41	7,095
122	572	Swaziland	2006-07	SZIR51	4,987
123	580	Madagascar (Malagasy)	1992	MDIR21	6,260
124	580	Madagascar (Malagasy)	1997	MDIR31	7,060
125	580	Madagascar (Malagasy)	2003-04	MDIR41	7,949
126	581	Comoros	1996	KMIR32	3,050
127	600	Morocco	1987	MAIR01	5,982
128	600	Morocco	1992	MAIR21	9,256
129	600	Morocco	2003-04	MAIR43	16,798
130	616	Tunisia	1988	TNIR02	4,184
131	625	Sudan	1989-90	SDIR02	5,860
132	640	Turkey/Ottoman Empire	1993	TRIR31	6,519
133	640	Turkey/Ottoman Empire	1998	TRIR41	8,576
134	640	Turkey/Ottoman Empire	2003	TRIR4H	8,075
135	651	Egypt	1988	EGIR01	8,911
136	651	Egypt	1992	EGIR21	9,864
137	651	Egypt	1995	EGIR33	14,779
138	651	Egypt	2000	EGIR41	15,573
139	651	Egypt	2005	EGIR51	19,474
140	651	Egypt	2008	EGIR5A	16,527
141	663	Jordan	1990	JOIR21	6,461
142	663	Jordan	1997	JOIR31	5,548
143	663	Jordan	2002	JOIR42	6,006
144	663	Jordan	2007	JOIR51	10,876
145	678	Yemen (Arab Republic of Yemen)	1991-92	YEIR21	6,010
146	703	Kyrgyz Republic	1997	KYIR31	3,848
147	704	Uzbekistan	1996	UZIR31	4,415
148	705	Kazakhstan	1995	KKIR31	3,771
149	705	Kazakhstan	1999	KKIR41	4,800
150	750	India	1992-93	IAIR22	89,777
151	750	India	1998-99	IAIR42	90,303
152	750	India	2005-06	IAIR51	124,385
153	770	Pakistan	1990-91	PKIR21	6,611
154	770	Pakistan	2006-07	PKIR52	10,023
155	771	Bangladesh	1993-94	BDIR31	9,640
156	771	Bangladesh	1996-97	BDIR3A	9,127
157	771	Bangladesh	1999-00	BDIR41	10,544
158	771	Bangladesh	2004	BDIR4J	11,440
159	771	Bangladesh	2007	BDIR50	10,996
160	780	Sri Lanka (Ceylon)	1987	LKIR02	5,865
161	790	Nepal	1996	NPIR31	8,429
162	790	Nepal	2001	NPIR41	8,726
163	790	Nepal	2006	NPIR51	10,793
164	800	Thailand	1987	THIR01	6,775
165	811	Cambodia (Kampuchea)	2000	KHIR42	15,351

166	811	Cambodia (Kampuchea)	2005	KHIR51	16,823
167	816	Vietnam, Democratic Republic of	1997	VNIR31	5,664
168	816	Vietnam, Democratic Republic of	2002	VNIR41	5,665
169	840	Philippines	1993	PHIR31	15,029
170	840	Philippines	1998	PHIR3A	13,983
171	840	Philippines	2003	PHIR41	13,633
172	850	Indonesia	1987	IDIR01	11,884
173	850	Indonesia	1991	IDIR21	22,909
174	850	Indonesia	1994	IDIR31	28,168
175	850	Indonesia	1997	IDIR3A	28,810
176	850	Indonesia	2002-03	IDIR41	29,483
177	850	Indonesia	2007	IDIR51	32,895
Total respondents					2,016,932

Surveys from country years censored from the analyses due to ongoing civil conflict highlighted in grey.

Appendix A3. Logit Regression of Civil War Onset and Various Horizontal Inequality Measures, (All DHS ctrs. vs. SSA), 1986–2008

	1 Global	2 SSA	3 Global	4 SSA	5 Global	6 SSA	7 Global	8 SSA	9 Global	10 SSA	11 Global	12 SSA
Ethnic HI (As.)	0.597	1.153										
	-1.034	-1.142										
Ethnic HI (Ed.)			1.962**	2.656***								
			-0.932	-1.012								
Religious HI (As.)					1.938*	1.675						
					-1.143	-1.265						
Religious HI (Ed.)							1.523**	1.402*				
							-0.676	-0.803				
Regional HI (As.)									2.388***	3.790**		
									-0.842	-1.75		
Regional HI (Ed.)											1.603***	1.957**
											-0.611	-0.89
Population (ln)	-0.028	-0.23	-0.148	-0.374*	0.038	-0.203	0.068	-0.143	0.163	-0.228	0.13	-0.185
	-0.155	-0.183	-0.166	-0.214	-0.159	-0.164	-0.165	-0.162	-0.138	-0.151	-0.14	-0.124
GDP pc (ln, t-1)	-0.545***	-0.772**	-0.477***	-0.734**	-0.353	-0.705**	-0.244	-0.578	-0.213	-0.509	-0.286*	-0.592
	-0.206	-0.328	-0.21	-0.372	-0.218	-0.347	-0.209	-0.367	-0.167	-0.337	-0.17	-0.364
Conflict Prox.	1.109**	1.036*	0.955**	0.7	1.337***	1.247*	1.267***	1.121*	1.099***	0.753	1.170***	0.921*
	-0.454	-0.54	-0.409	-0.496	-0.496	-0.645	-0.486	-0.612	-0.389	-0.498	-0.397	-0.489
Constant	0.732	5.184	1.778	6.686**	-1.831	4.094	-3.13	2.209	-5.239**	1.983	-4.019	2.565
	-2.799	-3.397	-2.701	-3.224	-3.506	-3.746	-3.346	-3.252	-2.599	-2.891	-2.643	-2.793
LL	-175.21	-124.3	-171.37	-119.54	-182.51	-117.26	-181.05	-116.25	-243.56	-140.15	-244.97	-142.05
Pseudo R²	0.0589	0.0824	0.0796	0.1175	0.0606	0.0756	0.0681	0.0836	0.0789	0.1085	0.0736	0.0964
# Conflicts	50	38	50	38	50	34	50	34	68	43	68	43
# Countries	43	27	43	27	50	28	50	28	67	33	67	33
N	787	513	787	513	921	539	921	539	1256	634	1256	634

Note: Logit regression coefficients, robust standard errors are in parentheses. *p < 0.10; **p ≤ 0.05; ***p ≤ 0.01.

APPENDICES Chapter 2

Appendix D. Robustness Checks: The Impact of Outliers

The maximum likelihood fit of a logistic regression is extremely sensitive to outliers (Pregibon, 1981). Given that the number of onsets relative to the number of control cases is quite skewed, it is not unlikely that the findings reported in this article could be due to the inclusion of some highly influential cases. Perigbon (1981) presents an influence statistic for logistic regression, 'dbeta', which behaves parallel to the more commonly used Cook's D in OLS. This statistic measures each observation's influence on the coefficients in the model. The norm is to remove observations with an influence higher than 1 (Hamilton 1992: 132). Tables AI and AII are identical to Tables II and III in the article respectively, except that outliers (i.e. observations with dbeta >1) are excluded from the analysis. In general, the results reported in the article are robust to the removal of influential outliers.

Table AI. Re-estimation of Models in Table II, Outliers Excluded

	(A1)	(A2)	(A3)	(A4)	(A5)	(A6)	(A7)
Vertical economic inequality	2.63						
	(2.36)						
Vertical social inequality		4.13*					
		(2.15)					
Economic polarization			10.87**				
			(5.02)				
Social polarization				4.62*			
				(2.72)			
Economic bipolarization					11.79***		
					(4.01)		
Social bipolarization						4.33*	
						(2.61)	
Ethnic polarization							0.32
							(1.06)
GDP per capita (ln)	-0.97*	-1.20*	-0.44	-0.62	-0.72	-0.61	-0.83**
	(0.55)	(0.67)	(0.52)	(0.51)	(0.43)	(0.52)	(0.40)
Population (ln)	-0.28	-0.45	-0.38	-0.41	-0.48	-0.40	-0.18
	(0.34)	(0.29)	(0.24)	(0.30)	(0.24)	(0.30)	(0.35)
Constant	3.27	5.22	0.76	2.88	4.12	2.82	3.06
	(6.13)	(6.96)	(5.21)	(5.26)	(4.33)	(5.29)	(5.36)
Log likelihood	-60.56	-59.57	-69.24	-69.13	-69.24	-69.18	- 71.80
Pseudo R^2	0.23	0.24	0.18	0.18	0.18	0.18	0.15
N	515	515	517	517	517	517	517
Number of countries	36	36	36	36	36	36	36
Number of civil wars	18	18	20	20	20	20	20

Logit estimates with robust z-statistics clustered on countries in parentheses. *p < 0.10; **p ≤ 0.05; ***p ≤ 0.01. Estimates for peaceyears and three natural cubic splines not reported. Observations with dbeta>1 (ouliers) excluded from the analysis.

All coefficients remain with the same sign, but effects are generally stronger and more significant. Exceptions are 'vertical economic inequality' and pure ethnic polarization, which remain insignificantly associated to conflict, as demonstrated in Table AI. With regard to the terms combining ethnic and socioeconomic aspects (Table AII), the results become much stronger when outliers are removed.

Table AII. Re-estimation of Models in Table III: Outliers Excluded

	(A8)	(A9)	(A10)	(A11)
Horizontal economic inequality	4.95***			
	(1.64)			
Horizontal social inequality		3.17**		
		(1.34)		
Ethnic/economic polarization			38.41**	
			(15.39)	
Ethnic/social polarization				7.90
				(9.27)
GDP per capita (ln)	-1.52***	-2.12***	-1.38**	-0.94**
	(0.58)	(0.81)	(0.54)	(0.44)
Population (ln)	-1.07***	-0.78***	-0.82**	-0.37
	(0.34)	(0.29)	(0.34)	(0.32)
Constant	14.24**	16.14	11.59**	5.09
	(5.67)	(5.85)	(5.71)	(5.09)
Log likelihood	-59.10	-59.85	-60.26	-64.92
Pseudo R^2	0.24	0.23	0.23	0.20
N	515	515	515	516
Number of countries	36	36	36	36
Number of civil wars	18	18	18	19

Logit estimates with robust z-statistics clustered on countries in parentheses. *p < 0.10; **p ≤ 0.05; ***p ≤ 0.01. Estimates for peaceyears and three natural cubic splines not reported. Observations with dbeta>1 (outliers) excluded from the analysis.

Most notably, the coefficient for horizontal economic inequality almost triples and becomes significant at 5% (Model 8). The effect of Horizontal Social Inequality also increases, with about 50%, and significance jumps from 10% to 5% (Model 9). The outliers are four conflict onsets in three countries: Cote d'Ivoire in 2002; Trinidad & Tobago in 1990; and Uzbekistan in 2000 and 2004. These are outliers because they represent unlikely conflict onsets. Economic development is much higher than for the rest of the sample, and levels of horizontal social inequality are very low for Trinidad & Tobago and Uzbekistan. Finally, the effect of one of the composite polarization terms, ethnic/economic polarization, almost doubles (Model 10) and reaches significance at the 5% level. Ethnic/social polarization becomes slightly stronger, but remains insignificant (Model 11). In sum, the outliers are *not* driving the results reported in the article. On the contrary, they actually prevent results from being statistically stronger.

APPENDICES Chapter 3

Appendix 7B. DHS Used in the Analysis

Country	Year	Country	Year	Country	Year
Armenia	2000	Guatemala	1998	Peru	1996
Bangladesh	1993	Guinea	1999	Peru	2000
Bangladesh	1996	Haiti	1994	Philippines	1993
Bangladesh	1999	Haiti	2000	Philippines	1998
Benin	1996	India	1992	Rwanda	1992
Benin	2001	India	1998	Rwanda	2000
Bolivia	1989	Indonesia	1987	Senegal	1986
Bolivia	1994	Indonesia	1991	Senegal	1992
Bolivia	1998	Indonesia	1994	Senegal	1997
Brazil	1986	Indonesia	1997	South Africa	1998
Brazil	1991	Indonesia	2002	Sri Lanka (Ceylon)	1987
Brazil	1996	Kazakhstan	1995	Sudan	1990
Burkina Faso	1992	Kazakhstan	1999	Tanzania/Tanganyika	1992
Burkina Faso	1998	Kenya	1998	Tanzania/Tanganyika	1996
Burkina Faso	2003	Kenya	2003	Tanzania/Tanganyika	1999
Burundi	1987	Kenya	1989	Thailand	1987
Cameroon	1991	Kenya	1993	Togo	1988
Cameroon	1998	Kyrgyz Republic	1997	Togo	1998
Central African Rep.	1994	Liberia	1986	Trinidad and Tobago	1987
Chad	1996	Madagascar (Malagasy)	1997	Tunisia	1988
Colombia	1986	Malawi	1992	Turkey/Ottoman Emp.	1993
Colombia	1990	Malawi	2000	Turkey/Ottoman Emp.	1998
Colombia	1995	Mali	1987	Uganda	1988
Colombia	2000	Mali	1995	Uganda	1995
Comoros	1996	Mali	2001	Uganda	2000
Cote d'Ivoire	1998	Mexico	1987	Uzbekistan	1996
Cote d'Ivoire	1994	Morocco	1987	Vietnam, Dem. Rep.	1997
Dominican Republic	1986	Morocco	1992	Vietnam, Dem. Rep.	2002
Dominican Republic	1991	Mozambique	1997	Yemen (Arab Rep. of Y.)	1991
Dominican Republic	1996	Namibia	1992	Zambia	1992
Dominican Republic	1999	Namibia	2000	Zambia	1996
Dominican Republic	2002	Nepal	1996	Zambia	2001
Ecuador	1987	Nepal	2001	Zimbabwe (Rhodesia)	1988
Egypt	1992	Nicaragua	1997	Zimbabwe (Rhodesia)	1994
Egypt	1995	Nicaragua	2001	Zimbabwe (Rhodesia)	2000
El Salvador	1985	Niger	1992		
Ethiopia	2000	Niger	1998		
Gabon	2000	Nigeria	1990		
Ghana	1988	Nigeria	1999		
Ghana	1993	Nigeria	2003		
Ghana	1998	Pakistan	1990		
Ghana	2003	Paraguay	1990		
Guatemala	1987	Peru	1986		
Guatemala	1995	Peru	1992		

* In some countries the survey was conducted over a two-year period. In these cases the table reports the first year only.

Appendix 7C. Logit Regression of Civil War Onset, GDP/Capita and Population Size, Various Samples

	Model A1 (Dev. ctrs. only)	Model A2 (Dev. ctrs. only)	Model A3 (Global sample)
HI_Educ. (Regions)	2.18***		
	(3.04)		
Population (ln)	0.17	0.045	0.22**
	(0.63)	(0.20)	(2.13)
GDP per capita (ln) $_{t-1}$	0.051	-0.29	-0.42***
	(0.20)	(-1.20)	(-3.13)
Peaceyears	-0.11	-0.16	-0.18
	(-0.58)	(-0.89)	(-1.24)
Constant	-6.37	-0.87	-2.49
	(-1.21)	(-0.20)	(-1.15)
LL	-137.13	-140.73	-275.48
Pseudo R²	0.059	0.034	0.101
# Conflicts	36	36	69
# Countries	55	55	147
N	777	777	2186

Note: Logit regression coefficients, z-values are in parentheses. Estimates for three natural cubic splines not shown in table. *p < 0.10; **p ≤ 0.05; ***p ≤ 0.01.

APPENDICES Chapter 4

This online appendix includes a selection of alternative specifications to the Models 1–14, Tables 1–4, presented in Østby, Nordås, and Rød (2009). In Appendix C, we replicate Models 1–14 using only the years after the survey was conducted in each country. For example, since the DHS survey was conducted in Côte d'Ivoire in 1994; we used the years 1994–2004 for Côte d'Ivoire in the replication. This provides an unbalanced panel. Appendix D reports the results for Models 1–14 using random effects models. In Appendix E, we introduce country-level controls for GDP per capita; GDP growth, Regime type, and Population size. Finally, Appendix F reports a full model including all the core explanatory variables that proved significant predictors of regional conflict onset in Tables 1–4 in the article. In general, the main results hold, with the exception of the term for education Gini, which drops to insignificance. However, including these terms together implies collinearity problems, particularly regarding the terms for education and education Gini. Running Model 15 with these two variables separately they both come out highly significant in line with the findings in the article. Overall, the results reported in the article are robust to all the alternative specifications reported in Appendices C–F below.

Appendix C: Models with Restricted Sample

Table C1. Onset of Conflict by Absolute Welfare, African Regions, 1986-2004, Restricted Sample

	(C1)	(C2)	(C3)
Distance to internal conflict[a,b]	0.007	0.008	0.018
	(0.14)	(0.16)	(0.39)
Distance to neighb. conflict[a,b]	-0.116**	-0.120**	-0.116**
	(-2.09)	(-2.01)	(-2.13)
Int. border	-0.622	-0.584	-0.784*
	(-1.46)	(-1.47)	(-1.92)
Population size[b]	-0.167	-0.160	-0.130
	(-1.45)	(-1.34)	(-1.08)
Ethnic difference	0.425	0.420	0.496
	(1.27)	(1.28)	(1.29)
Diamonds	0.713***	0.699***	0.736***
	(3.23)	(3.10)	(3.10)
Oil producer	0.400	0.402	0.651*
	(0.86)	(0.82)	(1.76)
Household assets		0.788	
		(0.40)	
Education years			-0.294***
			(-2.70)
Constant	-0.001	-0.209	-0.236
	(-0.00)	(-0.12)	(-0.12)
Pseudo R^2	.120	.120	.137
Observations	3,241	3,241	3,241
# Countries	22	22	22
# Regions	351	351	351
# Regionyears with onset	104	104	104

Note: Logit coefficients with robust z statistics clustered on countries in parenthesis. Estimates for peaceyears and three cubic splines not reported. [a]Lagged 1 year. [b]Logged. *p<.1; ** p<.05; ***p<.01.

Table C2. Onset of Conflict by Relative Deprivation, African Regions, 1986-2004, Restricted Sample

	(C4)	(C5)	(C6)	(C7)
Distance to internal conflict[a,b]	0.006	0.007	0.005	0.007
	(0.13)	(0.14)	(0.10)	(0.14)
Distance to neighb. conflict[a,b]	-0.118**	-0.116**	-0.116**	-0.115**
	(2.14)	(2.08)	(2.07)	(2.08)
Int. border	-0.609	-0.617	-0.622	-0.617
	(1.45)	(1.44)	(1.47)	(1.44)
Population size[b]	-0.162	-0.167	-0.171	-0.167
	(1.35)	(1.45)	(1.53)	(1.46)
Ethnic difference	0.450	0.432	0.466	0.427
	(1.31)	(1.25)	(1.32)	(1.18)
Diamonds	0.687***	0.707***	0.751***	0.707***
	(3.17)	(3.33)	(3.28)	(3.34)
Oil producer	0.365	0.390	0.390	0.395
	(0.74)	(0.81)	(0.80)	(0.83)
Rel.depr. (assets)	-0.190			
	(0.69)			
Rel.depr. (educ.)		-0.033		
		(0.19)		
Rel.depr. (assets)[c]			-0.363	
			(1.56)	
Rel.depr. (assets), sq[c]			0.548***	
			(3.23)	
Rel. depr. (educ.)[c]				-0.018
				(0.09)
Rel. depr. (educ.),sq[c]				-0.010
				(0.16)
Constant	0.018	0.011	-0.064	-0.001
	(0.01)	(0.01)	(0.04)	(0.00)
Pseudo R^2	0.120	0.120	0.124	0.120
Observations	3,241	3,241	3,241	3,241
# Countries	22	22	22	22
# Regions	351	351	351	351
# Regionyears with onset	104	104	104	104

Note: Logit coefficients with robust z statistics clustered on countries in parenthesis. Estimates for peaceyears and three cubic splines not reported. [a]Lagged 1 year. [b]Logged. [c]Centered. *p<.1; ** p<.05; ***p<.01. Rel. Depr. (assets): relative deprivation in household assets for region versus rest of country; Rel. Depr. (educ.): relative deprivation in education years for region versus rest of country.

Table C3. Onset of Conflict by Intra-Regional Inequality, African Regions, 1986-2004, Restricted Sample

	(C8)	(C9)	(C10)	(C11)
Distance to internal conflict[a,b]	0.007	0.019	0.008	0.019
	(0.14)	(0.40)	(0.15)	(0.38)
Distance to neighb. conflict[a,b]	-0.099*	-0.114**	-0.116**	-0.103*
	(-1.79)	(-2.10)	(-2.10)	(-1.82)
Int. border	-0.747*	-0.782*	-0.654	-0.887**
	(-1.81)	(-1.91)	(-1.56)	(-2.35)
Population size[b]	-0.201*	-0.136	-0.174	-0.167
	(-1.74)	(-1.12)	(-1.48)	(-1.38)
Ethnic difference	0.436	0.421	0.437	0.416
	(1.23)	(1.05)	(1.34)	(1.05)
Diamonds	0.734***	0.749***	0.694***	0.744***
	(3.24)	(3.13)	(3.09)	(3.19)
Oil producer	0.397	0.674*	0.398	0.624
	(0.94)	(1.80)	(0.81)	(1.59)
Gini (assets)	2.205*			1.577
	(1.65)			(1.06)
Gini (educ.yrs)		2.958***		2.701***
		(2.87)		(2.69)
Regional ELF			0.092	0.057
			(0.17)	(0.11)
Constant	-1.065	-2.990	0.062	-3.447*
	(-0.59)	(-1.54)	(0.03)	(-1.71)
Pseudo R^2	.127	.140	.120	.144
Observations	3,241	3,241	3,211	3,211
# Countries	22	22	22	22
# Regions	351	351	346	346
# Regionyears with onset	104	104	104	104

Note: Logit coefficients with robust z statistics clustered on countries in parenthesis. Estimates for peaceyears and three cubic splines not reported. [a]Lagged 1 year. [b]Logged. *p<.1; ** p<.05; ***p<.01.

Table C4. Onset of Conflict by Relative Deprivation and Natural Resources, African Regions, 1986-2004, Restricted Sample

	(C12)	(C13)	(C14)
Distance to internal conflict[a,b]	0.006	0.006	0.000
	(0.11)	(0.10)	(0.00)
Distance to neighb. conflict[a,b]	-0.119**	-0.132**	-0.128**
	(-2.14)	(-2.21)	(-2.30)
Int. border	-0.611	-0.658	-0.633
	(-1.43)	(-1.55)	(-1.44)
Population size[b]	-0.147	-0.160	-0.148
	(-1.23)	(-1.35)	(-1.07)
Ethnic difference	0.435	0.489	0.426
	(1.27)	(1.37)	(1.25)
Rel.depr. (assets)[c]	-0.232	-0.309	-0.243
	(-0.88)	(-1.11)	(-1.05)
Diamonds	0.633***		
	(3.29)		
Rel. depr. (assets)[c] * Diamonds	0.673		
	(0.93)		
Oil producer		0.184	
		(0.47)	
Rel. depr. (assets)[c] * Oil producer		1.390	
		(1.35)	
Nat. resources			0.550**
			(2.23)
Rel. depr. (assets)[c] * Nat. resources			1.173
			(1.64)
Constant	-0.177	0.312	-0.066
	(-0.10)	(0.18)	(-0.03)
Pseudo R^2	.120	.116	.124
Observations	3,241	3,241	3,241
# Countries	22	22	22
# Regions	351	351	351
# Regionyears with onset	104	104	104

Note: Logit coefficients with robust z statistics clustered on countries in parenthesis. Estimates for peaceyears and three cubic splines not reported. [a]Lagged 1 year. [b]Logged. *p<.1; ** p<.05; ***p<.01.

Appendix D: Random Effects

Table D1. Onset of Conflict by Absolute Welfare, African Regions, 1986-2004, Random Effects

	(D1) Random effects	(D2) Random effects	(D3) Random effects
Distance to internal conflict[a,b]	0.013	0.013	0.025
	(0.44)	(0.44)	(0.88)
Distance to neighb. conflict[a,b]	-0.087***	-0.082***	-0.080***
	(4.81)	(4.43)	(4.71)
Int. border	-0.209	-0.279	-0.525**
	(0.98)	(1.27)	(2.57)
Population size[b]	-0.013	-0.032	0.021
	(0.21)	(0.48)	(0.31)
Ethnic difference	0.051	0.049	0.095
	(0.25)	(0.24)	(0.48)
Diamonds	0.789***	0.766***	0.637***
	(3.29)	(3.20)	(2.92)
Oil producer	0.717**	0.699**	0.738**
	(2.30)	(2.24)	(2.55)
Household assets		-1.283	
		(1.40)	
Education years			-0.398***
			(7.08)
Constant	-2.751***	-2.270**	-2.225**
	(2.95)	(2.28)	(2.31)
Wald chi^2	96.64	99.25	174.55
Rho	.109**	.105**	.013
Observations	6208	6208	6208
# Countries	22	22	22
# Regions	354	354	354
# Regionyears with onset	144	144	144

Note: Random effect logit coefficients with absolute values for z statistics in parenthesis. Estimates for peaceyears and three cubic splines not reported. [a]Lagged 1 year. [b]Logged. *p<.1; ** p<.05; ***p<.01.

Table D2. Onset of Conflict by Relative Deprivation, African Regions, 1986-2004, Random Effects

	(D4) Random effects	(D5) Random effects	(D6) Random effects	(D7) Random effects
Distance to internal conflict[a,b]	0.015	0.013	0.015	0.014
	(0.50)	(0.46)	(0.52)	(0.47)
Distance to neighb. conflict[a,b]	-0.083***	-0.087***	-0.083***	-0.087***
	(4.53)	(4.82)	(4.51)	(4.79)
Int. border	-0.247	-0.226	-0.238	-0.229
	(1.14)	(1.06)	(1.10)	(1.07)
Population size[b]	-0.019	-0.012	-0.021	-0.011
	(0.29)	(0.18)	(0.32)	(0.17)
Ethnic difference	-0.002	0.020	-0.002	0.012
	(0.01)	(0.09)	(0.01)	(0.06)
Diamonds	0.802***	0.795***	0.862***	0.794***
	(3.34)	(3.32)	(3.55)	(3.31)
Oil producer	0.742**	0.739**	0.763**	0.746**
	(2.37)	(2.36)	(2.44)	(2.38)
Rel.depr. (assets)	0.328			
	(1.47)			
Rel.depr. (educ.)		0.115		
		(0.98)		
Rel.depr. (assets)[c]			0.139	
			(0.60)	
Rel.depr. (assets), sq[c]			0.534*	
			(1.96)	
Rel. depr. (educ.)[c]				0.143
				(0.85)
Rel. depr. (educ.),sq[c]				-0.017
				(0.23)
Constant	-2.770***	-2.801***	-2.830***	-2.751***
	(2.95)	(2.99)	(3.02)	(2.94)
Wald chi^2	99.20	97.70	102.09	97.84
Rho	.107**	.108**	.108**	.108**
Observations	6,208	6,208	6,208	6,208
# Countries	22	22	22	22
# Regions	354	354	354	354
# Regionyears with onset	144	144	144	144

Note: Random effect logit coefficients with absolute values for z statistics in parenthesis. Estimates for peaceyears and three cubic splines not reported. [a]Lagged 1 year. [b]Logged. [c]Centered. *p<.1; ** p<.05; ***p<.01.

Table D3. Onset of Conflict by Intra-Regional Inequality, African Regions, 1986-2004, Random Effects

	(D8) Random effects	(D9) Random effects	(D10) Random effects	(D11) Random effects
Distance to internal conflict[a,b]	0.017	0.024	0.013	0.025
	(0.57)	(0.84)	(0.45)	(0.88)
Distance to neighb. conflict[a,b]	-0.065***	-0.078***	-0.089***	-0.067***
	(3.43)	(4.57)	(4.90)	(3.74)
Int. border	-0.436**	-0.485**	-0.281	-0.645***
	(1.98)	(2.36)	(1.30)	(3.07)
Population size[b]	-0.082	0.017	-0.022	-0.029
	(1.22)	(0.25)	(0.35)	(0.44)
Ethnic difference	0.035	0.004	0.006	-0.083
	(0.17)	(0.02)	(0.03)	(0.41)
Diamonds	0.692***	0.654***	0.775***	0.619***
	(2.86)	(3.00)	(3.22)	(2.82)
Oil producer	0.605*	0.778***	0.755**	0.743**
	(1.91)	(2.71)	(2.41)	(2.53)
Gini (assets)	3.796***			1.968***
	(5.04)			(2.80)
Gini (educ.yrs)		3.630***		3.276***
		(7.19)		(6.21)
Regional ELF			0.566	0.702
			(1.25)	(1.63)
Constant	-4.191***	-5.674***	-2.777***	-6.255***
	(4.21)	(5.35)	(2.98)	(5.82)
Wald chi^2	121.03	169.94	95.57	177.14
Rho	.104**	.013	.108**	.013
Observations	6,208	6,208	6,113	6,113
# Countries	22	22	22	22
# Regions	354	354	349	349
# Regionyears with onset	144	144	144	144

Note: Random effect logit coefficients with absolute values for z statistics in parenthesis. Estimates for peaceyears and three cubic splines not reported. [a]Lagged 1 year. [b]Logged. *p<.1; ** p<.05; ***p<.01.

Table D4. Onset of Conflict by Relative Deprivation and Natural Resources, African Regions, 1986-2004, Random Effects

	(D12) Random effects	(D13) Random effects	(D14) Random effects
Distance to internal conflict[a,b]	0.016	0.016	0.013
	(0.54)	(0.55)	(0.44)
Distance to neighb. conflict[a,b]	-0.081***	-0.091***	-0.086***
	(4.43)	(4.97)	(4.71)
Int. border	-0.270	-0.286	-0.251
	(1.26)	(1.31)	(1.16)
Population size[b]	0.009	-0.011	-0.004
	(0.15)	(0.17)	(0.07)
Ethnic difference	-0.017	0.037	-0.020
	(0.08)	(0.17)	(0.10)
Rel.depr. (assets)[c]	0.310	0.265	0.321
	(1.39)	(1.19)	(1.42)
Diamonds[c]	0.719***		
	(2.97)		
Rel. depr. (assets) * Diamonds[c]	0.596		
	(0.93)		
Oil producer[c]		0.612*	
		(1.91)	
Rel. depr. (assets) * Oil producer[c]		1.324*	
		(1.81)	
Nat. resources[c]			0.753***
			(3.53)
Rel. depr. (assets) * Nat. resources[c]			1.015*
			(1.94)
Constant	-2.925***	-2.607***	-2.748***
	(3.13)	(2.76)	(2.94)
Wald chi^2	96.00	92.59	102.73
Rho	.109**	.116***	.109**
Observations	6,208	6,208	6,208
# Countries	22	22	22
# Regions	354	354	354
# Regionyears with onset	144	144	144

Note: Random effects. Logit coefficients with absolute values of z statistics in parenthesis. Estimates for peaceyears and three cubic splines not reported. [a]Lagged 1 year. [b]Logged. [c]Centered. *p<.1; ** p<.05; ***p<.01.

Appendix E: Adding Country-Level Controls

Table E1. Onset of Conflict by Absolute Welfare, African Regions, 1986-2004, Controlling for Country-Level Variables

	(E1)	(E2)	(E3)
Distance to internal conflict[a,b]	0.051*	0.047	0.054**
	(1.77)	(1.58)	(2.33)
Distance to neighb. conflict[a,b]	-0.078***	-0.073**	-0.081***
	(2.66)	(2.20)	(2.87)
Int. border	-0.387	-0.466*	-0.746**
	(1.45)	(1.68)	(2.50)
Population size[b]	0.062	0.065	0.310
	(0.34)	(0.33)	(1.25)
Ethnic difference	0.306	0.331	0.458
	(0.84)	(0.85)	(1.08)
Diamonds	0.756***	0.732***	0.743***
	(2.75)	(2.60)	(3.15)
Oil producer	0.686	0.679	0.872***
	(1.40)	(1.56)	(3.40)
GDP per capita[a,b]	-0.431	-0.430	-0.070
	(1.29)	(1.24)	(0.20)
GDP growth[a]	-0.095***	-0.096***	-0.098***
	(3.35)	(3.45)	(3.73)
Regime type[a]	-0.050	-0.047	-0.023
	(0.71)	(0.72)	(0.35)
Regime type, squared[a]	-0.010	-0.009	-0.001
	(0.94)	(0.85)	(0.09)
Population size[b]	-0.374	-0.401	-0.574
	(0.98)	(0.94)	(1.40)
Assets		-1.203	
		(0.48)	
Education			-0.450***
			(5.07)
Constant	2.987	3.382	-0.439
	(0.98)	(0.90)	(0.14)
Pseudo R^2	0.145	0.146	0.198
Observations	5,702	5,702	5,702
# Countries	22	22	22
# Regions	354	354	354
# Regionyears with onset	142	142	142

Note: Logit coefficients with robust z statistics clustered on countries (Huber-White) in parenthesis. Estimates for peaceyears and three cubic splines not reported. [a]Lagged 1 year. [b]Logged. *p<.1; ** p<.05; ***p<.01. Country-level variables in italics.

Table E2. Onset of Conflict by Relative Deprivation, African Regions, 1986-2004, Controlling for Country-Level Variables

	(E4)	(E5)	(E6)	(E7)
Distance to internal conflict[a,b]	0.051*	0.051*	0.052*	0.051*
	(1.85)	(1.80)	(1.87)	(1.81)
Distance to neighb. conflict[a,b]	-0.075**	-0.078***	-0.074**	-0.077***
	(2.55)	(2.66)	(2.55)	(2.64)
Int. border	-0.427	-0.397	-0.440	-0.402
	(1.61)	(1.51)	(1.62)	(1.54)
Population size[b]	0.086	0.075	0.127	0.077
	(0.43)	(0.41)	(0.61)	(0.43)
Ethnic difference	0.285	0.296	0.310	0.286
	(0.77)	(0.81)	(0.79)	(0.76)
Diamonds	0.770***	0.762***	0.811***	0.761***
	(2.83)	(2.73)	(3.07)	(2.75)
Oil prod.	0.721	0.702	0.750	0.711
	(1.54)	(1.44)	(1.58)	(1.48)
GDP per capita[a,b]	-0.421	-0.428	-0.415	-0.426
	(1.27)	(1.27)	(1.26)	(1.26)
GDP growth[a,b]	-0.094***	-0.095***	-0.093***	-0.095***
	(3.32)	(3.36)	(3.28)	(3.36)
Polity scale	-0.048	-0.048	-0.052	-0.048
	(0.71)	(0.70)	(0.76)	(0.71)
Polity scale, squared	-0.009	-0.010	-0.010	-0.010
	(0.87)	(0.92)	(0.90)	(0.91)
Population size[b]	-0.415	-0.389	-0.496	-0.391
	(1.02)	(1.01)	(1.16)	(1.02)
Rel.depr. (assets)	0.271			
	(0.94)			
Rel.depr. (educ.)		0.067		
		(0.58)		
Rel.depr. (assets) [c]			0.104	
			(0.42)	
Rel.depr. (assets), sq[c]			0.546***	
			(2.67)	
Rel. depr. (educ.) [c]				0.109
				(0.64)
Rel. depr. (educ.),sq[c]				-0.024
				(0.57)
Constant	2.854	2.902	2.904	2.918
	(0.96)	(0.96)	(0.99)	(0.97)
Pseudo R^2	0.146	0.145	0.149	0.145
Observations	5,702	5,702	5,702	5,702
# Countries	22	22	22	22
# Regions	354	354	354	354
# Regionyears with onset	142	142	142	142

Note: Logit coefficients with robust z statistics clustered on countries in parenthesis. Estimates for peaceyears and three cubic splines not reported. [a]Lagged 1 year. [b]Logged. [c] Centered. *p<.1; ** p<.05; ***p<.01. Country-level variables in italics.

Table E3. Onset of Conflict by Intra-Regional Inequality, African Regions, 1986-2004, Controlling for Country-Level Variables

	(E8)	(E9)	(E10)	(E11)
Distance to internal conflict[a,b]	0.046*	0.058**	0.051*	0.054**
	(1.70)	(2.40)	(1.79)	(2.30)
Distance to neighb. conflict[a,b]	-0.058*	-0.077***	-0.078***	-0.067**
	(1.85)	(2.75)	(2.64)	(2.21)
Int. border	-0.614**	-0.684**	-0.413	-0.820***
	(2.14)	(2.44)	(1.55)	(2.94)
Population size[b]	0.101	0.265	0.038	0.248
	(0.46)	(1.03)	(0.20)	(0.98)
Ethnic difference	0.381	0.347	0.296	0.284
	(0.91)	(0.84)	(0.83)	(0.65)
Diamonds	0.639**	0.742***	0.750***	0.690***
	(2.34)	(2.86)	(2.68)	(2.89)
Oil producer	0.625*	0.880***	0.677	0.810***
	(1.84)	(2.95)	(1.26)	(2.80)
GDP per capita[a,b]	-0.341	-0.206	-0.446	-0.161
	(1.04)	(0.62)	(1.34)	(0.47)
GDP growth[a]	-0.090***	-0.099***	-0.094***	-0.095***
	(3.15)	(3.63)	(3.34)	(3.28)
Regime type[a]	-0.062	-0.022	-0.048	-0.033
	(0.91)	(0.34)	(0.69)	(0.49)
Regime type, sq[a]	-0.007	-0.001	-0.010	0.000
	(0.69)	(0.10)	(0.94)	(0.03)
Population size[b]	-0.502	-0.557	-0.340	-0.577
	(1.16)	(1.31)	(0.89)	(1.32)
Gini (assets)	3.375***			1.845
	(2.67)			(1.40)
Gini (educ.yrs)		4.125***		3.826***
		(5.08)		(4.64)
Regional ELF			0.109	0.368
			(0.26)	(0.72)
Constant	0.777	-3.114	3.086	-4.053
	(0.26)	(1.15)	(1.04)	(1.35)
Pseudo R^2	0.162	0.196	0.144	0.200
Observations	5,702	5,702	5,632	5,632
# Countries	22	22	22	22
# Regions	354	354	349	349
# Regionyears with onset	142	142	142	142

Note: Logit coefficients with robust z statistics clustered on countries in parenthesis. Estimates for peaceyears and three cubic splines not reported. [a]Lagged 1 year. [b]Logged. *p<.1; ** p<.05; ***p<.01. Country-level variables in italics.

Table E4. Onset of Conflict by Relative Deprivation and Natural Resources, African Regions, 1986-2004, Controlling for Country-Level Variables

	(E12)	(E13)	(E14)
Distance to internal conflict[a,b]	0.056*	0.050*	0.049*
	(1.75)	(1.77)	(1.76)
Distance to neighb. conflict[a,b]	-0.073***	-0.084***	-0.080***
	(2.61)	(2.65)	(2.87)
Int. border	-0.431	-0.491*	-0.436
	(1.63)	(1.85)	(1.58)
Population size[b]	0.072	0.103	0.081
	(0.37)	(0.52)	(0.40)
Ethnic difference	0.260	0.335	0.259
	(0.72)	(0.88)	(0.70)
GDP per capita[a,b]	-0.484	-0.492	-0.486
	(1.43)	(1.45)	(1.36)
GDP growtha	-0.097***	-0.093***	-0.096***
	(3.37)	(3.39)	(3.54)
Regime type[a]	-0.043	-0.049	-0.043
	(0.64)	(0.70)	(0.67)
Regime type, sq[a]	-0.008	-0.010	-0.009
	(0.73)	(0.94)	(0.84)
Population size[b]	-0.357	-0.455	-0.381
	(0.93)	(1.07)	(0.98)
Rel.depr. (assets) [c]	0.234	0.154	0.215
	(0.82)	(0.54)	(0.90)
Diamondsc	0.682**		
	(2.27)		
Rel. depr. (assets) * Diamonds[c]	0.971		
	(1.46)		
Oil producer[c]		0.599	
		(1.49)	
Rel. depr. (assets) * Oil producer[c]		1.145	
		(1.29)	
Nat. resources[c]			0.707**
			(2.41)
Rel. depr. (assets) * Nat. resources[c]			1.230***
			(2.80)
Constant	3.078	3.803	3.250
	(1.03)	(1.21)	(1.07)
Pseudo R^2	0.144	0.140	0.150
Observations	5,702	5,702	5,702
# Countries	22	22	22
# Regions	354	354	349
# Regionyears with onset	142	142	142

Note: Logit coefficients with absolute values of z statistics in parenthesis, clustered on country. Estimates for peaceyears and three cubic splines not reported. [a]Lagged 1 year. [b]Logged. [c]Centered. *p<.1; ** p<.05; ***p<.01. Country-level variables in italics.

Appendix F: Full Model

Table F1. Onset of Conflict by Absolute Welfare, Relative Deprivation, Intra-Regional Inequalities, and Natural Resources, African Regions, 1986-2004

Distance to internal conflict[a,b]	0.015
	(0.26)
Distance to neighb. conflict[a,b]	-0.070*
	(1.67)
Int. border	-0.878***
	(2.74)
Population size[b]	-0.014
	(0.11)
Ethnic difference	0.300
	(0.78)
Education years	-0.599***
	(3.86)
Nat. resources[c]	0.438**
	(2.48)
Rel.depr. (assets) [c]	-2.191***
	(5.17)
Rel.depr. (assets), sq[c]	0.919***
	(3.72)
Rel. depr. (assets) * Nat. resources[c]	0.996**
	(2.10)
Gini (assets)	5.149***
	(3.31)
Gini (educ.yrs)	-0.687
	(0.59)
Constant	-3.939**
	(1.96)
Pseudo R^2	0.178
Observations	6,208
# Countries	22
# Regions	354
# Regionyears with onset	144

Note: Logit coefficients with robust z statistics clustered on countries in parenthesis, Huber-White. Estimates for peaceyears and three cubic splines not reported. [a]Lagged 1 year. [b]Logged. [c]Centered. *p<.1; ** p<.05; ***p<.01.

APPENDICES Chapter 6

Appendix A. Identification of Outliers

The maximum likelihood fit of a logistic regression is extremely sensitive to outliers. Given that the number of violent provinceyears relative to the number of control cases is quite skewed, it is not unlikely that the findings reported in this article could be due to the inclusion of some highly influential cases. Perigbon (1981) presents an influence statistic for logistic regression, 'dbeta', which behaves parallel to the more commonly used Cook's D in OLS. This statistic measures each observation's influence on the coefficients in the model.

We identified two outliers with the dbeta diagnostic, using the quite conservative threshold of 1.5, one observation for each of the dependent variables: Riau, 1996 for routine violence, and Jakarta, 1998 for episodic violence. These observations are removed from the models reported in the manuscript. More radical thresholds only strengthen our main results.

Appendix B. Indonesian Provinces and Province-Years Included in Analysis

ID	Province	Start year	End year	Years included in analysis
101	Nanggroe Aceh Darussalam	1959	-	1990–2001
102	Sumatera Utara (North Sumatra)	1950	-	1990–2001
103	Sumatera Barat (West Sumatra)	1957	-	1990–2001
104	Riau	1957	-	1990–2003*
105	Kepulauan Riau (Riau islands)	2002	-	-
106	Jambi	1957	-	1990–2001
107	Bengkulu	1967	-	1990–2001
108	Sumatera Selatan (South Sumatra)	1950	-	1990–2001
109	Bangka-Belitung	2001	-	-
110	Lampung	1964	-	1990–2001
111	Jakarta	1957	-	1990–2003†
112	Banten	2000	-	2000–2003
113	Jawa Barat (West Java)	1950	-	1990–2003
114	Jawa Tengah (Central Java)	1950	-	1990–2003
115	Yogyakarta	1950	-	1990–2001
116	Jawa Timur (East Java)	1950	-	1990–2003
117	Bali	1958	-	1990–2001
118	Nusa Tenggara Barat (West Nusa Tenggara)	1958	-	1990–2003
119	Nusa Tenggara Timur (East Nusa Tenggara)	1958	-	1990–2003
120	Kalimantan Barat (West Kalimantan)	1956	-	1990–2003
121	Kalimantan Tengah (Central Kalimantan)	1958	-	1990–2003
122	Kalimantan Selatan (South Kalimantan)	1956	-	1990–2001
123	Kalimantan Timur (East Kalimantan)	1956	-	1990–2001
124	Sulawesi Utara (North Sulawesi)	1960	-	1990–2001
125	Gorontalo	2000	-	-
126	Sulawesi Tengah (Central Sulawesi)	1964	-	1990–2003
127	Sulawesi Tenggara (South East Sulawesi)	1964	-	1990–2001
128	Sulawesi Selatan (South Sulawesi)	1960	-	1990–2003
130	Maluku	1950	-	-
131	Maluku Utara (North Maluku)	1999	-	-
132	Papua	1963	-	-
133	Irian Jaya Barat (West Irian Jaya)	2003	-	-
138	Timor Timur (East Timor)	1976	2002	-

Note: *: the observation for Riau in 1996 is not included in models of routine violence due to excessive influence. †: the observation for Jakarta in 1998 is not included in models of episodic violence due to excessive influence.

Appendix C. Descriptive Statistics for Sample Used in Analysis

Variable	Obs	Mean	Std. Dev.	Min	Max
Routine deaths (min 5)	313	0.20	0.40	0.00	1.00
Episodic deaths (min 5)	313	0.09	0.29	0.00	1.00
Pop. Growth	313	1.83	0.97	0.17	4.35
Pop. growth (c)	313	0.04	0.97	-1.62	2.56
Land scarcity (ln)	313	1.33	1.60	-0.85	7.51
Land scarcity (ln) (c)	313	0.11	1.60	-2.07	6.29
Urban share (%)	313	33.72	18.19	10.80	100.00
Polarization (rel)	313	0.33	0.28	0.02	0.97
Polarization (rel) (c)	313	-0.03	0.28	-0.35	0.61
Horizontal inequality (IMR)	313	0.20	0.12	0.03	0.68
Horizontal inequality (IMR) (c)	313	0.01	0.12	-0.17	0.49
Vertical Inequality (assets)	313	0.36	0.11	0.18	0.79
Polarization (rel) * Pop. Growth	313	0.01	0.19	-0.58	0.40
Horizontal inequality * Pop. growth	313	0.01	0.11	-0.25	0.63
Horizontal inequality * Land scarcity	313	0.03	0.14	-0.68	0.50
GDP /capita (ln)	313	-6.30	0.59	-7.20	-4.68
Population (ln)	313	15.36	0.94	13.98	17.48
Routine deaths (min 5), lagged	313	0.18	0.39	0.00	1.00
Episodic deaths (min 5), lagged	313	0.08	0.27	0.00	1.00
Neighb. routine deaths (min 5)	313	0.24	0.43	0.00	1.00
Neighb. episodic deaths (min 5)	313	0.12	0.33	0.00	1.00

Note: (ln) denotes that the variable is log-transformed. (c) refers to centred variables. The figures refer to the sample used for routine violence. Figures for episodic violence are largely the same (since only one observation differ from the sample used for routine violence).

Appendix D. Constructing Inequality Measures: Some Details

Horizontal Inequality

The dimension we use as a basis of constructing a HI measure for inequality between religious groups is the infant mortality rate (IMR), which we calculate for all religious groups within each province for all the survey years in the Indonesian DHS. We apply Newell's (1988: 64) definition and formula of IMR:

$$IMR = \left(\text{Deaths under age 1 in year} \Big/ \text{Live births in year} \right) * 1000$$

Though it is common to calculate IMR for only one year at the time, we chose to follow Brockerhoff and Hewett (2000: 35) and generate the IMR measures based on births in the 10 years preceding each DHS survey, in order to get sufficiently large samples for the disaggregated religious groups within the provinces.

Mancini, Stewart and Brown (2008) review different approaches to measuring HIs and conclude that an ideal measure should: a) be in so far as possible descriptive, not evaluative, b) be sensitive to group size, and c) measure inequality between groups as

such, without conflating it with vertical inequality and/or demographic polarization. Following Tadjoeddin, Suharyo and Mishra (2003) we use a measure of horizontal inequality which meets these criteria, namely the group-based coefficient of variance (*GCOV*), which is described in the article.

Vertical Inequality

Since the IMR described above is by definition a group measure (and hence cannot be broken down to individuals), we use another variable as the basis for constructing vertical, or inter-individual inequality, which we believe also is a good indicator of general welfare; namely an *additive household asset index*. This index is constructed on the basis of the following variables from the DHS surveys: *v119–v125* (dummies for whether or not each household has electricity, a radio, a television, a refrigerator, a bicycle, a motorcycle and/or a car).

We then calculate a province level Gini coefficient for vertical inequality in terms of the household asset index, using the **ineqdec0** command in Stata. The Gini coefficient is an index between 0 and 1 where 0 implies an egalitarian distribution (complete equality) and 1 indicates total concentration (complete inequality).

Appendix E. Robustness Checks

The main results reported in this article are robust to alternative specifications of the dependent variable (e.g. if we alter the violence threshold from a minimum of 5 to 1 death(s) per province-year), and to different constellations of the control variables.

Table A-I provides evidence that our main results reported in Models 7 and 8 are robust both to the exclusion of Jakarta (Models A1 and A2) and when we introduce year dummies (Models A3 and A4). The low *N* in these models is partly due to the exclusion of Jakarta, and partially due to the fact that some years have no variation on the dependent variable, leading to perfect collinearity and subsequently elimination from the analyses with year dummies.

Table A-I. Excluding Jakarta from Main Models, and Including Year Dummies

	Excluding Jakarta		Including Year Dummies	
	Model A1 (r)	Model A2 (e)	Model A3 (r)	Model A4 (e)
Pop. Growth (c)	-0.118	0.216	0.391	0.525
	(0.482)	(0.340)	(0.483)	(0.344)
Land scarcity (ln) (c)	-1.089*	-1.380**	1.155*	-0.157
	(0.573)	(0.702)	(0.655)	(0.849)
Urban share (%)	0.126***	-0.028	-0.024	-0.177***
	(0.048)	(0.025)	(0.072)	(0.066)
Polarization (rel)	-0.203	0.143	2.146***	0.931
	(1.028)	(0.993)	(0.803)	(1.112)
HI (IMR) (c)	0.687	-4.324**	-0.994	-0.674
	(2.208)	(1.952)	(2.181)	(2.268)
HI * Pop. growth	5.412***	3.793***	6.546***	2.832*
	(2.013)	(1.270)	(2.062)	(1.699)
GDP /capita (ln)	-3.207**	-0.251	-0.821	1.710**
	(1.477)	(0.539)	(1.234)	(0.847)
Population (ln)	1.802***	1.687***	2.557***	1.437*
	(0.530)	(0.595)	(0.500)	(0.741)
Temporal lag	1.898***	1.278*	1.387**	0.380
	(0.535)	(0.729)	(0.673)	(0.745)
Neighbouring violence	-0.604	0.886*	-2.616***	0.003
	(0.645)	(0.516)	(0.801)	(0.498)
Constant	-53.472***	-28.429***	-70.618	-27.628
	(16.554)	(10.239)	(0.000)	(0.000)
Log Pseudolikelihood	-76.69	-74.61	-56.66	-56.67
Pseudo R2	0.46	0.18	0.63	0.31
N	299	300	290	218

Note: ***: p-value<0.01. **: p< 0.05. * p<0.10 (two-sided tests). (ln) denotes that the variable is log-transformed. (c) refers to centred variables in models with interactions that include the specific variable. (r) and (e) refer to routine and episodic violence respectively. Figures are coefficients and estimated robust standard errors clustered on province (in parentheses). Year dummies in Models A3 and A4 not shown.

Although the correlation between vertical and horizontal inequality is not strong (and actually it is negative: r = –.17) it could be questioned whether the two variables should be included in the same model because, theoretically, they are somehow functions of each other (see Stewart, 2002). We tried to remove both terms in turn and the results remained largely unchanged (results not shown).

Furthermore, there is an increasing discussion over the use of a lagged dependent variable, the main concern being that it 'may soak up so much of the variance that it masks potential causal factors explained by the other independent variables of interest' (de Soysa and Nordås, 2007: 934; see also Plümper, Troeger and Manow, 2005). A province which experienced routine violence in a given year actually has almost 47.1 per

cent risk of experiencing it again in the subsequent year. It could be questioned then whether this effect is so strong that it disturbs the effects of other potential predictors of violence by putting these on too strict a test. However despite this strong control we do find some robust results on other variables, and if we remove the temporal lag these results remain largely the same, only stronger (results not shown).

Appendix F. Alternative Controls for Spatial Autocorrelation

The issue of spatial regression in cross-sectional time series is not straight forward, and the fact that we utilize a maximum likelihood model effectively limits our options. Spatial autocorrelations is most commonly specified either as spatial error or spatial lags (Anselin, 1988). The spatial error model is not very compatible with our general research design, but we present a test with a number of limitations. We also test a local regression model, geographically weighed regression, based on running a number of regressions with each unit as the point of origin (*i*), and then assigning weights to all other observations (*j*) inversely proportional to the distance *ij*.

In our view, the spatial lag model is the best alternative given the data limitations we have present. The spatial lag model allows us to explicitly model the mechanism behind spatial autocorrelation.

In an OLS framework, the spatial error model can be written as $Y = \beta X + \lambda We + \mu$, where μ is the homoskedastic and uncorrelated errors, and We is the spatial component of the error-term. λ indicates the strength of the error term, and this parameter should not be outside of the inverse range of the eigenvalues of the spatial weights matrix W .

The alternative formulation is called the spatial lag model and the OLS version is given by $Y = \rho WY + \beta X + \mu$, where ρ denotes the spatial autoregressive coefficient. Since the true error-term of the logistic regression model is unobserved, it is pointless to specify a spatial error logistic model, so we are limited to a spatial lag model. We have implemented this in our analysis, through a spatial lag of violence. Our spatial lag function returns 1 if one or more neighbors experience 5 or more casualties and 0 if that is not the case.

The spatreg package for Stata (Pisati, 2001) does not support the logistic regression model; does not support observations without neighbors; nor does it support

a panel data structure. Testing the spatial error model is therefore quite difficult, and it requires a number of adjustments to our model:

- We use OLS instead of the Logistic model in this test. This is not very problematic. The dependent variable is fairly balanced, and the most likely bias is that OLS standard errors become larger than they should have been.

- Spatial errors are more likely to be present the more variance there is in the dependent variable. Since we cannot analyze time series, we have chosen the most violent year as the test case, 1999.

- In order to include the isolated regions, we make the assumption that regions without neighbors are influenced by an unobserved factor, and we therefore make all isolated regions an idiosyncratic neighborhood. This is a heroic assumption, but the alternative is to either drop them; alter our specification of the spatial mechanism towards a distance-based continuous measure, or make another equally heroic assumption. With 27 regions in 1999, dropping the 6 isolated cases is a quite unattractive option.

- We impute missing observations with sample average to avoid listwise deletion. One unit has four missing values, and two units miss one value each.

Below, we report six models, all based on Model 1 in the article:

- The original logistic model without spatial and temporal lags.
- An OLS version of the original model
- A Spatial error model with all covariates from Model 1
- A Spatial lag model with all covariates from Model 1
- A Spatial error model with no covariates (constant only)
- A Spatial lag model with no covariates (constant only)

Table A-II describes the results from this exercise for routine violence and Table A-III provides corresponding results for episodic violence. Most importantly, the spatial parameters, λ and ρ are never outside their acceptable range. This suggests that neither the spatial lag nor the spatial error models outperform the parsimonious option for the year 1999.

The coefficients do not change very much between the various models. The largest deviance is the coefficient for gcovrel_imr in Table A-II, Model A7, which becomes positive and insignificant instead of negative and insignificant.

We believe these results support our choice to use the logistic regression model with a spatial lag component. Overall, since the spatial error model seems unsuited for our purpose, we suggest that we do not report these tests in the online appendix.

Table A-II. Robustness Tests with Alternative Spatial Controls, Routine Violence

	Mod A5	Mod A6	Mod A7	Mod A8	Mod A9	Mod A10
Pop. Growth (c)	2.470	0.174	0.208**	0.214*		
	(1.40)	(1.34)	(2.80)	(2.21)		
Land scarcity (ln) (c)	1.303	0.127	0.0298	0.205		
	(0.74)	(0.78)	(0.24)	(1.64)		
Urban share (%)	0.0362	0.00570	0.0246	0.00622		
	(0.24)	(0.34)	(1.66)	(0.51)		
Polarization (rel)	1.447	-0.131	-0.0316	-0.157		
	(0.27)	(-0.30)	(-0.10)	(-0.49)		
HI (IMR) (c)	-10.43	-0.399	0.0556	-0.154		
	(-0.71)	(-0.39)	(0.08)	(-0.20)		
Vertical inequality (as)	9.064	1.879	2.947**	2.194		
	(0.55)	(1.07)	(2.59)	(1.69)		
GDP /capita (ln)	-2.404	-0.178	-0.386	-0.223		
	(-0.97)	(-0.76)	(-1.66)	(-1.29)		
Population (ln)	2.806	0.227	0.171	0.210*		
	(1.64)	(1.88)	(1.86)	(2.35)		
Constant	-68.39	-5.430	-6.992**	-5.646**	0.299**	0.264*
	(-1.77)	(-2.01)	(-3.02)	(-2.84)	-3.02	-2.5
Lambda	-0.778***	0.117				
_cons	(-3.55)	(0.55)				
Sigma	0.264***	0.297***	0.453***	0.453***		
_cons	(6.25)	(7.05)	(7.33)	(7.33)		
Rho	-0.459*	0.117				
_cons	(-2.06)	(0.55)				
N	27	27	27	27	27	27

NOTE: t statistics in parentheses: * p<0.05, ** p<0.01, *** p<0.001

Table A-III. Robustness Tests with Alternative Spatial Controls, Episodic Violence

	Mod A11	Mod A12	Mod A13	Mod A14	Mod A15	Mod A16
Pop. Growth (c)	-1.017	-0.0675	-0.00197	-0.0753		
	(-0.47)	(-0.59)	(-0.03)	(-0.84)		
Land scarcity (ln) (c)	4.236	0.243	0.312**	0.222		
	(0.90)	(1.68)	(3.16)	(1.95)		
Urban share (%)	-0.671	-0.0394*	-0.0385***	-0.0363**		
	(-1.27)	(-2.67)	(-3.44)	(-3.09)		
Polarization (rel)	10.08	0.539	0.891***	0.598*		
	(1.22)	(1.41)	(3.43)	(1.98)		
HI (IMR) (c)	-2.042	-0.853	-0.631	-0.598		
	(-0.18)	(-0.95)	(-1.11)	(-0.82)		
Vertical inequality (as)	-14.42	-1.147	-1.343	-0.761		
	(-0.96)	(-0.74)	(-1.43)	(-0.61)		
GDP /capita (ln)	10.07	0.543*	0.450*	0.501**		
	(1.31)	(2.63)	(2.49)	(3.04)		
Population (ln)	0.196	-0.0245	-0.0911	-0.0628		
	(0.15)	(-0.23)	(-1.20)	(-0.71)		
Constant	78.56	5.577*	5.693**	5.693**	0.222**	0.232*
	(1.43)	(2.33)	(3.04)	(3.04)	-2.9	-2.49
Lambda	-0.791***	-0.0435				
_cons	(-4.57)	(-0.21)				
Sigma	0.220***	0.280***	0.415**	0.415***		
_cons	(6.56)	(7.24)	(7.35)	(7.35)		
Rho	-0.282	0.0435				
_cons	(-1.29)	(0.21)				
N	27	27	27	27	27	27

NOTE: t statistics in parentheses: * p<0.05, ** p<0.01, *** p<0.001

We have also experimented with Geographically Weighted Regressions models. The literature suggests taking the coordinates of the shape centroid for each region, but it made more sense to us to code the region capital as the location of each state. Since GWR does not take the panel data structure into account, we have moved these capitals slightly each year to avoid distances of 0. This is obviously not a sufficient solution to the problem. We have tried to run the model for a cross section (all observations from 2002), which did not give any interesting results. We found no significant bandwidths, but the number of observations became very low indeed.

Computing the GWR model on Stata is non-trivial, and the program does not accept negative coordinates. Indonesia was hence moved to the northern hemisphere for

this test. A cursory look at the literature indicates that this is the first application of this Stata routine on the southern hemisphere.

The GLM-based module failed to converge. Stata refused to provide ML estimates of the local regression, and there is not much we can do about that, except accept the fact that we are pushing our data to the point where it is an open question whether the answers received are produced by the model or the data. The R documentation of this procedure (Bivand 2010, 6) notes that "[t]he use of GWR on GLM is only at the initial proof of concept stage, nothing should be treated as an accepted method at this stage". However, we were able to produce results with the OLS-based module. The initial model is presented in Table A-IV and the final output from the Monte Carlo simulations is presented in Table A-V.

Table A-IV. Initial Regression for GWR Bandwidth Estimation

	Coef.	Std. Err.	T	P>|t|
Pop. Growth (c)	.0456813	.0262409	1.74	0.083
Land scarcity (ln) (c)	.0087332	.0290385	0.30	0.764
Urban share (%)	.0036697	.0027931	1.31	0.190
Polarization (rel)	.0296134	.0736641	0.40	0.688
HI (IMR) (c)	-.0647313	.1820656	-0.36	0.722
Vertical inequality (as)	.2505794	.1580074	1.59	0.114
GDP /capita (ln)	-.0808122	.0507611	-1.59	0.112
Population (ln)	.1191229	.0283054	4.21	0.000
Temporal lag	.4848093	.0521748	9.29	0.000
Neighboring violence	.0256236	.0459442	0.56	0.577
Constant	-2.377293	.6254359	-3.80	0.000

Table A-V. Significance Tests for Non-Stationarity

Variable	Si	P-Value
Pop. Growth (c)	1.6258	0.000
Land scarcity (ln) (c)	0.0247	0.610
Urban share (%)	0.0514	0.010
Polarization (rel)	0.0050	0.010
HI (IMR) (c)	0.1429	0.020
Vertical inequality (as)	0.1828	0.430
GDP /capita (ln)	0.1670	0.460
Population (ln)	0.1012	0.010
Temporal lag	0.0612	0.020
Neighboring violence	0.0560	0.710
Constant	0.0732	0.100

The overall statistic implies that the local regression beats the global regression with a long distance, as the model bandwidth was large and highly significant (p<0.0005). The factors that come out significant largely relate to population and population density. This is not very surprising. The most densely populated regions are also the smallest regions, such as the capital region, which therefore are most proximate to other regions and will be the most influenced. Furthermore, each of these local regressions are very much influenced by the other observations from the same region. The best indicator of this being a problem is that the constant term is the most affected variable in the equation. We added fixed effects to the regression to check for this factor.

First, the difference between the GWR and the global regression is no longer significant when we include region dummies. Adding fixed effects has removed most of the variance that was attributed to the spatial weights. Our assumption is that this is due to the undue weights assigned to observations from the same region but from different years. Also, most of the other significant variances are now insignificant. Both the temporal and the spatial lags are unaffected by the GWR, which leaves us very optimistic with regard to their effectiveness as autocorrelation controls.

Table A-VI. Significance Tests for Non-Stationarity

Variable	Si	P-Value
Constant	0.7968	0.805
_Iprovid_102	0.1145	0.439
_Iprovid_103	0.0943	0.195
_Iprovid_104	0.2636	0.000
_Iprovid_106	0.0558	0.976
_Iprovid_107	0.2262	0.439
_Iprovid_108	0.1155	0.195
_Iprovid_110	0.1432	0.415
_Iprovid_111	0.3468	0.463
_Iprovid_112	0.3565	0.024
_Iprovid_113	0.2719	0.195
_Iprovid_114	0.2329	0.341
_Iprovid_115	0.1811	0.463
_Iprovid_116	0.1793	0.463
_Iprovid_117	0.0946	0.463
_Iprovid_118	0.2333	0.268
_Iprovid_119	0.1102	0.829
_Iprovid_120	0.1504	0.244
_Iprovid_121	0.1583	0.341
_Iprovid_122	0.0304	0.902
_Iprovid_123	0.2170	0.220
_Iprovid_124	0.1326	0.610
_Iprovid_126	0.1111	0.854
_Iprovid_127	0.2259	0.512
Pop. Growth (c)	0.0911	0.659
Land scarcity (ln) (c)	0.1148	0.000
Urban share (%)	0.0979	0.122
Polarization (rel)	0.0014	0.854
HI (IMR) (c)	0.2211	0.024
Vertical inequality (as)	0.0807	0.610
GDP /capita (ln)	0.0207	1.000
Population (ln)	0.1174	0.512
Temporal lag	0.0265	0.951
Neighboring violence	0.0128	0.927
Constant	0.0084	0.732

APPENDICES Chapter 7

Appendix A: Descriptive Statistics

Variable	Obs.	Mean	Std. Dev.	Min	Max
Non-lethal events	693	0.83	1.58	0	13
Lethal events	693	0.68	1.37	0	15
In-migration (all)	693	0.24	0.11	0.08	0.66
Rural-Urban migration	693	0.09	0.05	0.03	0.30
Assets (city mean)	693	0.39	0.10	0.21	0.64
Education (city mean)	693	6.81	2.31	2.92	12.00
Public services (city mean)	693	0.59	0.20	0.12	0.95
Gini (Assets)	693	0.35	0.08	0.14	0.52
Gini (Education)	693	0.38	0.17	0.10	0.72
Gini (Public Services)	693	0.26	0.14	0.03	0.77
MRD, all (Assets)	693	0.07	0.12	-0.24	0.44
MRD, all (Education)	693	0.04	0.20	-0.37	0.70
MRD, all (Public Services)	693	0.01	0.09	-0.21	0.54
MRD, Rural-Urban (Assets)	693	0.20	0.18	-0.29	0.57
MRD, Rural-Urban (Education)	693	0.37	0.40	-0.36	1.52
MRD, Rural-Urban (Public Services)	693	0.11	0.14	-0.29	1.00
City population	693	32.38	36.01	2.04	163.68
GDP per capita (ln)	693	6.00	0.72	4.39	8.18
GDP per capita growth	693	0.01	0.05	-0.22	0.14
Regime type	693	0.61	6.06	-9	9
Regime type, squared	693	37.08	26.12	0	81
Distance to nearest conflict (km)	693	1.51	2.40	0	17

Appendix B: Cities Covered in Analysis, Event Count (1986–2006)

City	Country	No. events	No. fatal	% fatal
Abidjan	Côte d'Ivoire	41	18	43.9
Accra	Ghana	5	3	60.0
Addis Ababa	Ethiopia	24	19	79.2
Almaty	Kazakhstan	8	3	37.5
Antananarivo	Malagsy Republic	11	5	45.5
Bamako	Mali	10	4	40.0
Bangkok	Thailand	20	3	15.0
Bishkek	Kyrgyz Republic	17	4	23.5
Brazzaville	Congo	21	18	85.7
Calcutta	India	7	3	42.9
Conakry	Guinea	12	5	41.7
Dakar	Senegal	8	4	50.0
Dar es Salaam	Tanzania	3	1	33.3
Dhaka	Bangladesh	77	21	27.3
Harare	Zimbabwe	32	5	15.6
Islamabad	Pakistan	26	9	34.6
Jakarta	Indonesia	76	15	19.7
Johannesburg	South Africa	57	30	52.6
Kampala	Uganda	10	9	90.0
Karachi	Pakistan	125	98	78.4
Kathmandu	Nepal	59	21	35.6
Khartoum	Sudan	14	7	50.0
Kinshasa	Zaire	45	22	48.9
Lagos	Nigeria	47	24	51.1
Lomé	Togo	29	15	51.7
Lusaka	Zambia	16	8	50.0
Manila	Philippines	71	28	39.4
Maputo	Mozambique	7	5	71.4
Mumbai	India	19	11	57.9
Nairobi	Kenya	37	9	24.3
New Delhi	India	72	23	31.9
Niamey	Niger	17	5	29.4
Phnom Penh	Cambodia	28	19	67.9
Yaoundé	Cameroun	5	2	40.0
Total		**1,143**	**534**	**46.7**

Appendix C. Relocation of DHS Survey Sample Points

Sometimes, a few DHS survey sample points seem slightly dislocated (i.e. they are not located on the right side of the border of the administrative unit). For example, in the file for Senegal 1992–93 (SNGE22FL), one point was located in the sea, and not in the capital region of Dakar. In order to include the survey respondents who belong to this location in my calculations, I simply relocated this point using the Editor in ArcGIS, as shown in Figure C1 below

Figure C1. Relocation of DHS Survey Sample Point, Example from Senegal 1992–93
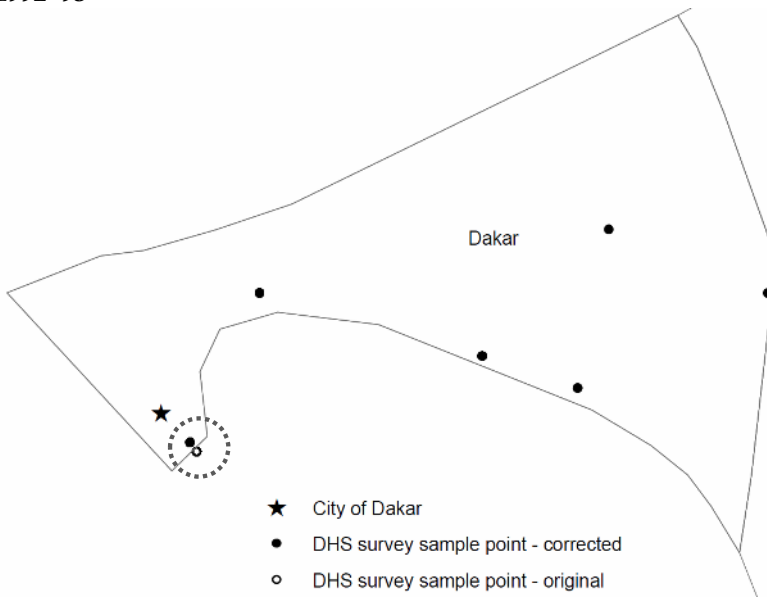
Appendix D: DHS Surveys Used to Generate the Migration and Inequality Measures

City	Country	DHS survey year	City respondents (N)
Abidjan	Ivory Coast	1994	1,432
Abidjan	Ivory Coast	1998[a]	1,250
Accra	Ghana	1988	530
Accra	Ghana	1993[b]	547
Accra	Ghana	1998	617
Accra	Ghana	2003[b]	731
Accra	Ghana	2008	622
Addis Ababa	Ethiopia	2000	2,015
Addis Ababa	Ethiopia	2005	1,812
Almaty	Kazakhstan	1995	615
Almaty	Kazakhstan	1999	636
Antananario	Malagasy Republic	1992	1,092
Antananario	Malagasy Republic	1997[c]	1,446
Antananario	Malagasy Republic	2003	2,301
Bamako	Mali	1987	503
Bamako	Mali	1995	1,265
Bamako	Mali	2001	2,067
Bamako	Mali	2006	2,011
Bangkok	Thailand	1987	1,248
Bishkek	Kyrgyz Republic	1997	893
Brazzaville	Congo	2005	2,165
Calcutta	India	1992[c]	898
Calcutta	India	1998	1,947
Calcutta	India	2005	3,642
Conakry	Guinea	1999[a]	1,337
Conakry	Guinea	2005[b]	941
Dakar	Senegal	1986	1,298
Dakar	Senegal	1992	1,506
Dakar	Senegal	1997	1,017
Dakar	Senegal	2005	1,354
Dar es Salaam	Tanzania	1991	900
Dar es Salaam	Tanzania	1996	666
Dar es Salaam	Tanzania	2003[c]	335
Dar es Salaam	Tanzania	2004	386
Dar es Salaam	Tanzania	2007[c]	717
Dhaka	Bangladesh	1993	583
Dhaka	Bangladesh	1996	558
Dhaka	Bangladesh	1999	970

Dhaka	Bangladesh	2004	1,125
Dhaka	Bangladesh	2007	1,085
Harare	Zimbabwe	1988[c]	345
Harare	Zimbabwe	1994[a]	608
Harare	Zimbabwe	1999	562
Harare	Zimbabwe	2005	1,395
Islamabad	Pakistan	1990	995
Islamabad	Pakistan	2006[a]	1,597
Jakarta	Indonesia	1987	3,251
Jakarta	Indonesia	1991[a]	1,813
Jakarta	Indonesia	1994[a]	1, 805
Jakarta	Indonesia	1997[a]	1, 784
Jakarta	Indonesia	2002[a]	1, 882
Jakarta	Indonesia	2007[a]	1, 722
Johannesburg	South Africa	1998	1,034
Kampala	Uganda	1988	527
Kampala	Uganda	1995	1,151
Kampala	Uganda	2000	597
Kampala	Uganda	2006	846
Karachi	Pakistan	1990	1,059
Karachi	Pakistan	2006 [a]	1,212
Kathmandu	Nepal	1996	471
Kathmandu	Nepal	2001	305
Kathmandu	Nepal	2006	390
Khartoum	Sudan	1989	1,037
Kinshasa	Zaire	2007	1,666
Lagos	Nigeria	1990	1,369
Lagos	Nigeria	1999[b]	715
Lagos	Nigeria	2003	3,040
Lagos	Nigeria	2008	1,946
Lomé	Togo	1988[c]	750
Lomé	Togo	1998	1,417
Lusaka	Zambia	1992	1,042
Lusaka	Zambia	1996	943
Lusaka	Zambia	2001	771
Lusaka	Zambia	2007	660
Manila	Philippines	1993	1,882
Manila	Philippines	1998	1,490
Manila	Philippines	2003	2,168
Maputo	Mozambique	1997	381
Maputo	Mozambique	2003	723
Mumbai	India	1992[c]	1,699
Mumbai	India	1998	3,191

Mumbai	India	2005	6,394
Nairobi	Kenya	1989	859
Nairobi	Kenya	1993	367
Nairobi	Kenya	1998	419
Nairobi	Kenya	2003	1,169
New Dehli	India	1992[c]	3,189
New Dehli	India	1998	2,287
New Dehli	India	2005	3,106
Niamey	Niger	1992	1,347
Niamey	Niger	1998	1,048
Niamey	Niger	2006	1,329
Phnom Penh	Cambodia	2000	621
Phnom Penh	Cambodia	2005	624
Yaoundé	Cameroun	1991	766
Yaoundé	Cameroun	1998	786
Yaoundé	Cameroun	2004	919
Total			**124,534**

All surveys listed are used to generate the terms for relative deprivation and inter-individual inequality. However some surveys could not be used to create the figures for in-migration and migrant relative deprivation. The reasons for the latter are the following: [a]Survey lacks information on settlement history (years lived in city and previous residence type) [b]Survey has too few rural-urban migrant respondents in the city in question to create meangful figures (<15) [c]Survey has information on years lived in city, but lacks info on previous residence type.

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[*] This is a complete 'meta-list' with references for all the seven chapters in this dissertation including appendices. Since most of the chapters were published as journal articles at various stages some works have multiple references here due to various versions cited in the different chapters, and in some instances due to various citation rules. For exact cross-references, please see the individual chapters.

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