The Inequality–Conflict Nexus Re-Examined

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PhD Dissertation

The Inequality–Conflict Nexus
Re-Examined

Table of Contents

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Preface

This report summarizes the PhD project *The Inequality-Conflict Nexus Re-examined,* carried out at the Department of Political Science and Government, Aarhus University. The project investigates the relationship between inequality and intrastate armed conflict (or civil war) and attempts to answer the following questions:

1. Does inequality increase the likelihood of intrastate armed conflict?
2. What are the causal pathways through which inequality may increase the likelihood of intrastate armed conflicts?

The project consists of a dataset and four single-authored papers:
1. The Categorically Disaggregated Conflict (CDC) Dataset (v. 1.0);
2. Improving large-N analysis of conflict causes: A categorical disaggregation of intrastate armed conflict. Manuscript under review (hereafter Paper 1);
3. The inequality-conflict nexus re-examined: Income, education and popular rebellions. Forthcoming in *Journal of Peace Research* (Paper 2);
4. From non-violent to violent conflicts: Examining conflict militarization. Manuscript under review (Paper 3);
5. The inequality-conflict nexus re-examined: How does inequality cause conflicts? Working Paper (Paper 4).

The summary is structured as follows: Chapter One reviews the state of the art, presents the research problem, and summarizes the main arguments of the project; Chapter Two describes the relationship among the four papers (and the dataset) and the role of the papers in the overall project; Chapters Three through Seven summarize the dataset and the four papers; and finally, Chapter Eight discusses the implications of the main findings of the project and offers suggestions for future research.

Chapter One:
State of the Art, Research Problem
and Main Arguments

> A study of the EI-PC nexus [inequality-conflict nexus] raises the "big positive questions" in our discipline. EI-PC studies lead analysts to consider the connections between power and conflict, competition and participation, stratification and domination, and exploitation and control which interrelate with such big questions in political science as: Who wins and who loses? Why do people support authority structures? What determines the persistence and change of the institutions? The EI-PC puzzle has thus attracted the attention of some of the great political theorists of all time: Aristotle, Plato, Machiavelli, de Tocqueville, Marx, and Madison. It has also been examined by some of the major figures in contemporary political science: Lipset, Dahl, and Huntington (Lichbach, 1989: 433).

Armed conflict is commonplace in today's world. 248 armed conflicts[1] have been recorded since 1946 (Themnér & Wallensteen, 2012: 565). There has not been 'a single year void of war over the last two hundred years, and perhaps for much longer than that' (Min & Wimmer, 2007: 67) – and this does not seem likely to change in the near future: 37 armed conflicts in 30 locations were recorded in 2011 (Themnér & Wallensteen, 2012: 565).[2] The toll, according to conservative estimates, amounts to 19.5 million dead (for 1945-1999) (Fearon & Laitin, 2003: 75). These numbers exclude victims of terrorist attacks, genocides, and mass political executions – events that often precede, accompany or succeed armed conflicts – as well as victims of poverty, malnutrition, and the spread of diseases occurring 'long after the shooting stops' (Ghobarah, Huth, & Russett, 2003; see also Collier et al., 2003).

It is not surprising, therefore, that armed conflict has attracted considerable interest among social scientists. Recently, the greatest focus has centred on *intrastate* armed conflicts (or civil wars) (e.g., Collier & Hoeffler, 2004; Fearon & Laitin, 2003; Hegre et al., 2001), as most of today's armed conflicts take place *within*, rather than between, states (see Figure 1). As a matter of fact, of the

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[1] Defined as a 'contested incompatibility that concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths' (Themnér, 2011: 1).
[2] This constitutes a substantial increase from the 31 conflicts recorded in 2010 (Themnér & Wallensteen, 2012: 565).

above-mentioned 37 armed conflicts recorded in 2011, 36 were intrastate (though nine of these saw international involvement) (Themnér & Wallensteen, 2012: 566).[3]

Figure 1. Armed conflicts by type, 1946-2011 (source: Themnér & Wallensteen, 2012: 568)
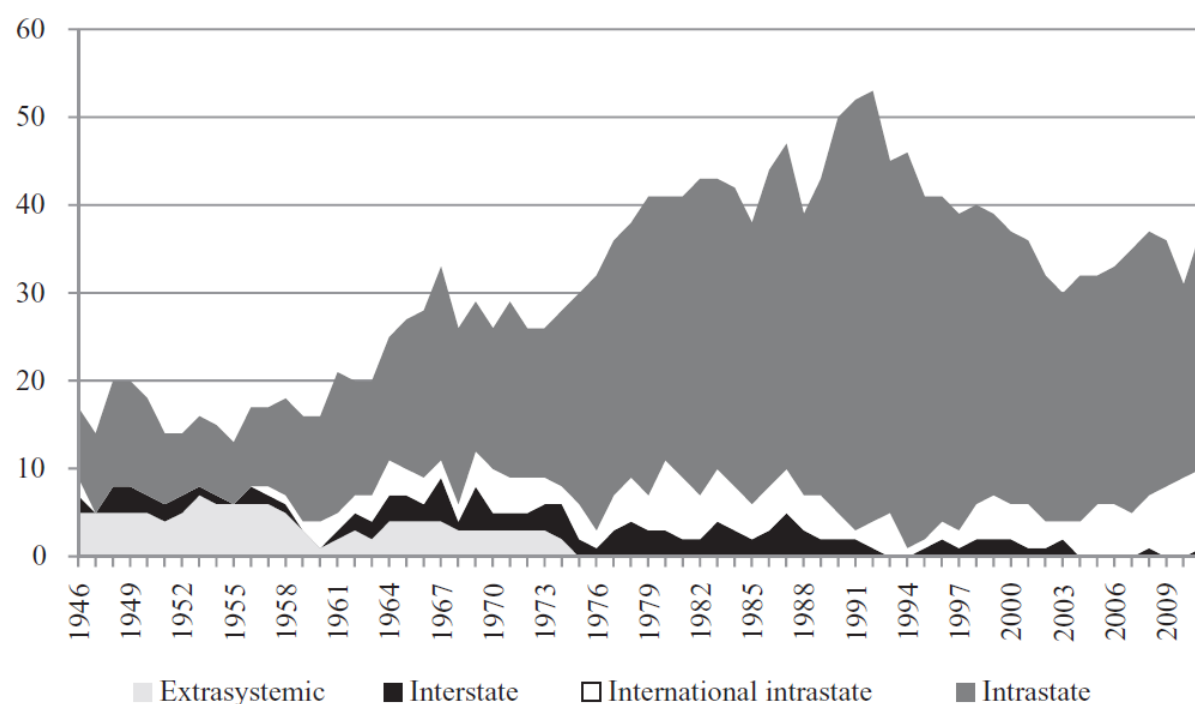
The outbreak of intrastate armed conflict (hereafter conflict) has been linked to a number of demographic (Homer-Dixon, 1994), ethnic (Horowitz, 1985), economic (Collier & Hoeffler, 2004), institutional (Hegre et al., 2001), and geographic (Buhaug & Gates, 2002) factors (for a book-length treatment of the causes of conflicts – both interstate and intrastate – see Levy & Thompson, 2010; for an overview of specific variables linked to intrastate conflicts, see Dixon, 2009). One variable among these has attracted particular attention: inequality.[4, 5]

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[3] Contrary to popular belief, this does not constitute a new trend: intrastate armed conflict has been the dominant form of armed conflict since at least 1816 (Sarkees, Wayman, & Singer, 2003: 61).
[4] Hereby, inequality is simply defined as an unequal distribution of certain goods (e.g., income or land) in a given society (for full definition see Paper 2, pp. 6–7, 9–10).
[5] According to Lichbach, there have been at least 43 quantitative studies of the inequality-conflict nexus between 1964–1989.

The Inequality-Conflict Nexus

Does unequal distribution of resources increase the risk of conflict? Quantitative conflict research has pursued this question ever since Bruce Russet published his *Inequality and Instability* in 1964. The results of the early research on the inequality-conflict nexus were mixed: different studies found a positive relationship between inequality in income or land tenure and conflict (Nagel, 1976; Prosterman, 1976; Russet, 1964; Sigelman & Simpson, 1977; Tanter & Midlarsky, 1967), no relationship (Hardy, 1979; Weede, 1981, 1987), a negative relationship with conflicts most likely in egalitarian societies (Mitchell, 1968; Parvin, 1973), and a concave (inverted-U) relationship with conflicts most likely at the intermediate levels of inequality (Nagel, 1974).

With the end of the Cold War, the focus of conflict researchers shifted to other variables such as ethnic diversity (Ellingsen, 2000), natural resources (Collier & Hoeffler, 2004), state capacity (Fearon & Laitin, 2003), and regime type (Hegre et al., 2001). Nevertheless, some studies in the 1990s and 2000s analysed the role of inequality as well. Once again, findings were mixed: Alesina & Perotti (1996) and Auvinen & Nafzinger (1999) found a positive relationship between inequality and conflict, while Hegre, Gissinger, & Gleditsch (2003) found that such a relationship is non-existent. On the whole, quantitative cross-national research has thus failed to establish a robust relationship between inequality and conflict.

The non-findings of the cross-national quantitative research, however, stand in contrast to extensive theoretical work arguing that inequalities are among the major causes of conflicts (e.g., Gurr, 1970; Davies, 1962; Runciman, 1966). As Midlarsky puts it,

> Theories of the relationship between inequality and political violence have been far more decisive in their assertions. From Aristotle to Marx and Tocqueville, the nexus between inequality and political violence has been claimed to be strong and direct, with little or no quibbling as to the ultimate outcome – in most cases revolution (1988: 492).

The non-findings of the cross-national literature also stand in contrast to numerous case studies that have demonstrated that sharp inequalities have had a major impact on the outbreak of a number of present-day conflicts (Booth, 1991; Boyce, 1996; Midlarsky & Roberts, 1985; Stewart, 2002, 2008).

What can potentially explain this mismatch between the quantitative, cross-national literature on the one hand and the theoretical and case study work on the other? Some scholars have argued that the non-findings of the cross-national studies are caused by methodological flaws such as measure-

ment error or poor quality of inequality data (e.g., Lichbach, 1989; Sambanis, 2005). Others have argued that the non-findings of the cross-national studies are caused by inadequate operationalization of dependent and independent variables (e.g., Buhaug, Cederman, & Gleditsch, 2013). Previous studies have mainly used proxies of inequality in the total population (i.e., vertical inequality), which does not necessarily overlap with inequality between particular groups (horizontal inequality):

> In practice, a country can have large income inequalities between groups (His) [horizontal inequalities], 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 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 inequality at the same time (e.g. South Africa), or it can score low on both (e.g. Switzerland) (Østby, 2011: 9).

This suggests that vertical and horizontal inequalities potentially have non-uniform effects on ethnic and non-ethnic conflicts. Non-ethnic conflicts, unlike ethnic ones, transcend ethnic boundaries. Such conflicts often involve participation of non-ethnically differentiated masses whose share of resources is closely related to the overall distribution of resources, which – as stated above – does not necessarily apply to ethnic groups.

Thus, as Sambanis puts it, there 'may exist a relationship between [vertical] inequality and popular revolutions or class conflict…But ethnic or secessionist wars should, in theory, be driven more by group-based inequality…than by interpersonal inequality' (2005: 328). As a matter of fact, Besancon (2005) has found that vertical income inequality is positively related to the onset of (non-ethnic) 'revolutions', but negatively to the onset of 'ethnic wars'. This suggests that the study of the inequality-conflict nexus must consider the distinction between horizontal and vertical inequalities on the one hand and ethnic and non-ethnic conflicts on the other. This is where the main arguments of the present project come in.

Argument One

This project argues that by aggregating the dependent variable (i.e., including ethnic and non-ethnic conflicts in the same category) and independent variable (i.e., making no distinction between vertical and horizontal inequalities), previous research on the inequality-conflict nexus has tried, in Buhaug, Cederman, & Gleditsch's words, 'to push square pegs through round holes' (2013: 1), which has resulted in inadequate empirical models that could not

appropriately account for the theoretical arguments on the inequality-conflicts nexus.

In turn, this project claims that to properly test the theoretical arguments on the inequality-conflict nexus, conflict research needs to disaggregate both independent variables and dependent variables, and link horizontal inequalities to ethnic conflicts and vertical inequalities to non-ethnic ones. Correctly specified models – where horizontal inequality is empirically linked to ethnic conflicts (Østby, 2008; Cederman, Weidmann, & Gleditsch, 2011) and vertical inequality is linked to non-ethnic ones (Paper 2) – demonstrate that inequalities significantly increase the likelihood of conflict onset. The project substantiates this argument not only in a large-N setting, but also in 16 qualitative case studies (Paper 4).

Argument Two

This project argues that inequalities (and their consequents) are *motivational* factors – not facilitating (or opportunity) factors. Therefore, inequalities are primarily related to conflict *initiation* – not to conflict *militarization.* Sharp inequalities between socioeconomic or ethnic groups can thus only account for why conflicts start, but, in most cases, cannot explain why conflicts become violent (i.e., militarize). Therefore, full explanation of conflict onset needs to go beyond mere effects of inequalities and integrate factors that *facilitate* armed violence. Consequently, the project claims that the factors that explain why conflicts become violent are linked to the military capacity of the conflicting parties (i.e., the state and the rebels) (Paper 3).

The two arguments are developed in full in the four papers. The following chapters of this summary describe the role of the papers and the dataset in the overall project (Chapter 2), summarize the papers and the dataset (Chapters 3–7), and provide a discussion of the implications of the main findings of the papers and offer suggestions for future research (Chapter 8).

Chapter Two:
The Relationship among the Four Papers (and the Dataset) and Their Role in the Overall Project

Paper 1 lays the conceptual and empirical ground for the project. It starts from an observation that conflicts are typically analysed as homogeneous phenomena in large-N research. No matter whether ethnic or non-ethnic, governmental or territorial, conflicts are often packed into one category and tested in empirical models. Subsequently, Paper 1 claims that certain variables may have non-uniform effects across conflict sub-categories, implying that the aggregate models (where all conflict categories are packed under one heading) may under- or overestimate the effects of predictors on particular conflict categories. The paper tests this claim systematically and finds that a number of commonly used conflict predictors indeed have non-uniform (and even opposite) effects on (1) ethnic governmental, (2) ethnic territorial, (3) non-ethnic governmental, and (4) non-ethnic territorial conflicts.

As a part of the empirical analysis, Paper 1 introduces the CDC dataset. While initially introduced as part of Paper 1, the CDC now constitutes an individual contribution to the dissertation and stands as separate data project that can be used for multiple purposes. The CDC is directly employed in Papers 1 and 2, and indirectly in Papers 3 and 4.

The findings of Paper 1 (on the differences in the effects of conflict predictors on ethnic governmental, ethnic territorial, non-ethnic governmental, and non-ethnic territorial conflicts) suggest that the main analysis of the inequality-conflict nexus needs to account for potential differences in the effects of inequality proxies on different conflict categories. Paper 2 finds significant evidence that vertical inequalities increase the likelihood of popular rebellion (which is, as described in Chapter Five, a particular form of the third conflict category, non-ethnic governmental conflict, data on which is taken from the CDC).

Paper 3 provides an additional test of the findings presented in Paper 2. It introduces a new approach to conflict analysis that focuses on conflict *militarization* (as opposed to the traditional focus on conflict *onset*). Instead of comparing countries 'at peace' with countries 'at war', Paper 3 compares countries at 'non-violent (or non-armed) conflict' with countries at 'violent (or armed)

conflict'. While I do not include inequality among the explanatory variables in the final version of Paper 3, I utilise the approach to conflict analysis developed in Paper 3 to test the effects of inequality proxies as part of the overall project.[6] I find that inequalities fail to account for variation in non-violent and violent conflicts (both ethnic and non-ethnic), suggesting that the inequality-conflict nexus is a consequence of inequality effects on conflict *origination* (i.e., outbreak of [as yet] non-violent conflict), and not of inequality effects on conflict *militarization* (i.e., the violent turn in non-violent conflicts).

Finally, Paper 4 addresses the 'how' question in the inequality-conflict nexus. Building on the covariational evidence presented in the previous papers, Paper 4 sets out to assess the causal pathways through which inequalities are thought to generate conflicts. Paper 4 assesses the causal pathways by matching the predictions of the causal pathways to the actual data in 16 qualitative case studies. The paper finds considerable support for the main causal pathways proposed in the conflict literature, some of which are employed in Paper 2. Paper 4 therefore provides qualitative support to the covariational patterns established in Paper 2. The link between the four papers (and the dataset), and the role the papers play in the overall project, are summarized in Table I.

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[6] Paper 3 has been submitted to a journal with a limited word count. As inequality proxies failed to significantly predict conflict militarization, and (in the framework of Paper 3) were not among the main explanatory variables, I have excluded inequality from the final version of Paper 3. However, I report the results of the inequality proxies on conflict militarization in this summary (see Chapter Six, Table VIII).

Table I. The role of the four papers and the dataset in the overall project

	Main argument/finding	Role in the overall project
CDC		Provides data for Papers 1-4.
Paper 1	'The effects of conflict predictors are non-uniform across different conflict categories.'	Suggests that the main analysis of the inequality-conflict nexus (Paper 2) needs to consider the fact that the effects of inequality can also be non-uniform across different conflict categories.
Paper 2	'The non-findings of the previous research on the inequality-conflict nexus are a consequence of the 'over-aggregation' of the dependent (conflict) and independent (inequality) variables – a correctly specified model, where vertical inequality is linked to non-ethnic conflicts, demonstrates that inequality significantly increases the likelihood of conflict onset.'	Provides covariational evidence on the inequality conflict nexus. It demonstrates that (vertical) inequality, indeed, significantly affects (non-ethnic) conflicts.
Paper 3	'The reasons for why conflicts start and why they militarize (i.e., become violent) are different – inequality explains why conflicts start, but fails to account for why conflicts militarize.'	Provides an additional (covariational) test of the inequality-conflict nexus. It demonstrates that inequality fails to account for why conflicts militarize.
Paper 4	'Evidence gathered in 16 qualitative case studies suggests that the main causal pathways through which inequalities are thought to lead to conflicts are plausible.'	Provides qualitative evidence on the inequality-conflict nexus. Assesses the theorised causal pathways (through which inequalities are thought to lead to conflicts) in qualitative case studies.

Chapter Three:
The Categorically Disaggregated
Conflict (CDC) Dataset (v.1.0)

As mentioned in the previous chapter, the CDC stands as a separate data project that can be used for multiple purposes. The primary purpose of the CDC is to provide data for a systematic analysis of the four conflict categories: ethnic governmental, ethnic territorial, non-ethnic governmental, and non-ethnic territorial. The CDC improves upon existing categorically disaggregated datasets in a number of ways. Most notably:

1. The CDC employs explicit definition and coding criteria, allowing systematic empirical comparison of the conflict categories deemed important in recent conflict research;
2. It provides coding of the key component variables (used to categorize conflicts into the four categories), allowing potential users to alter the coding of the conflict categories to fit alternative definitions;
3. It provides coding descriptions that document coding choices (along with references to the primary and secondary literature) for every conflict, allowing potential users to track individual coding decisions;
4. It considers the actual patterns of confrontation between conflicting parties (as opposed to just composition or proclamations of the conflicting parties), which allows more precise coding of the conflict categories.

In the following paragraphs of this chapter, I briefly describe the conceptual framework and coding criteria employed in the CDC (for a critique of similar data projects and an account on the need for such a dataset, see Paper 1).

The CDC uses the UCDP/PRIO dataset as a base; therefore, it employs the UCDP/PRIO's definition of an aggregate conflict (see footnote 1). The CDC also relies on the UCDP/PRIO's coding of incompatibility (*Incomp*) to distinguish between governmental conflicts ('incompatibility concerning type of political system, the replacement of the central government, or the change of its composition') and territorial conflicts ('incompatibility concerning status of a territory, [...] e.g., secession or autonomy'). For full definitions see Themnér (2011).

Subsequently, the CDC classifies these conflicts as ethnic or non-ethnic. Following Gurr and Harff, the CDC defines 'ethnic groups' as those

composed of people who share a distinctive and enduring collective identity based on shared experiences and cultural traits… [who]…may define themselves, and be defined by others, in terms of any or all of the following traits: life ways, religious beliefs, language, physical appearance, region of residence, traditional occupations, and a history of conquest and repression by culturally different peoples (1994: 190).

Based on this definition, the CDC classifies every conflict between two or more groups whose majorities represent different ethnicities as 'ethnic' and every conflict between groups whose majorities represent the same ethnicity as 'non-ethnic'. Conflicts in which individuals of one ethnicity compose a substantial part of both opposing groups are considered 'non-ethnic'. As the CDC was established to study conflict *onset*, conflicts in the CDC are classified based on their characteristics recorded in the initial phase. Thus, conflicts that start between non-ethnic groups but develop into inter-ethnic clashes are considered 'non-ethnic conflicts' and conflicts that start between ethnic groups but develop into clashes between members of the same ethnicity are considered 'ethnic conflicts'.

For reasons described in Paper 1, the CDC disregards the motivation of conflicting groups and focuses *solely on their composition.* No matter the reason a conflict is fought, it is considered an 'ethnic conflict' if the conflicting parties are composed of different ethnic groups. The CDC assumes that conflicts fought between ethnic groups are, indeed, fought over ethnic issues. Equally, the CDC assumes that conflicts involving systematic fighting and killing between individuals of the same ethnicity are non-ethnic conflicts.

The following is the exact description of how the coding of 'ethnic' and 'non-ethnic' conflicts was implemented in the CDC:
  – First, identification of *the parties to a conflict.* For this purpose, the CDC used the UCDP/PRIO's 'SideA' and 'SideB' variables.
  – Second, determination of *the composition of the parties to a conflict.* The coding of the composition of SideA and SideB was based on my own reading of primary and secondary sources. SideA is always the government of a state, and to determine its composition I focused on (i) the executive branch (i.e., presidents, prime ministers, members of the cabinet), (ii) military leadership, and (iii) foot soldiers taking part in the conflict. In many cases, I coded the composition of SideA based on the composition of its *de facto* leaders, assuming that the formal composition of the government (especially in autocracies) may not represent the actual power distribution within the executive. Similarly, to determine

the composition of SideB, I focused on the composition of (i) political and/or military leadership and (ii) foot soldiers.

- Third, determination of the *ethnic differences of the parties to a conflict*. 'Ethnic differences' in the CDC were operationalized by differences in language, religion and 'race'.[7] I coded SideA and SideB as 'ethnically different' if their members were distinct in *at least one* of the three characteristics.[8] It is important to note that the CDC treats language, religion, and 'race' merely as *proxies* of the concept of 'ethnicity' – and not as constitutive parts of 'ethnicity'. The CDC concurs with the widely-held view that ethnic affiliations are, to a great extent, socially constructed, and acknowledges that some groups may define their ethnicity on the basis of other attributes. Yet language, religion, and 'race' are the qualities that can be more or less unambiguously observed and used as proxies of the directly unobservable 'ethnicity'. These are also the qualities that (taken separately) help – surprisingly well – to empirically distinguish between groups widely perceived as distinct 'ethnic groups'.[9]

- Fourth, determination of *the pattern of confrontation between parties to a conflict*. In this step, I attempted to ascertain whether a conflict involved systematic fighting between (and killing of) members of the same ethnic group.

To make the coding explicit, I provide coding descriptions (along with references to the primary and secondary literature) in the CDC, documenting coding choices along the four steps for all (331) conflicts.

The UCDP/PRIO dataset (v.4-2011) contains 368 separate onsets of aggregate intrastate armed conflicts and internationalized intrastate armed conflicts. In line with previous research, the CDC applied the two-year intermitten-

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[7] Note, therefore, that conflict coding into 'ethnic' and 'non-ethnic' in the CDC did not merely rely on the labels attached to groups (e.g. 'Christian Maronites' or 'Kurds'), but on actual linguistic, religious and 'racial' characteristics.

[8] To distinguish between separate languages and two dialects of the same language I used Ethnologue (Lewis, Simons, & Fennig, 2013). To determine the religion of particular ethnic groups I used World Christian Database (Johnson, 2007). Note that followers of the main branches of Islam (Shia and Sunni) as well as members of the main groupings of Christianity (Catholicism, Protestantism, and Orthodoxy) are considered members of different ethnic groups. The CDC provides coding for all three characteristics so that potential users could easily apply other combinations of the three characteristics to match alternative definitions of ethnic conflicts.

[9] Indeed, there are very few groups widely considered to be 'distinct ethnic groups' that the coding criteria introduced in the CDC fail to distinguish – most notably, Tutsis and Hutus in Rwanda and Burundi and, less-well known, the Lulua and Luba in DRC.

cy rule (e.g., Buhaug, 2006: 698–699). Therefore, the final number of onsets in the CDC amounts to 331: 59 ethnic governmental, 128 ethnic territorial, 124 non-ethnic governmental, and 20 non-ethnic territorial.

The CDC comes in two formats:
– .xls (contains a codebook, coding descriptions, and a full list of variables in a spreadsheet);
– .pdf (contains a codebook, coding descriptions, and *key* variables in a text document).

See Appendix A for the CDC codebook. Appendices B, C, D, and E provide examples of coding descriptions for each of the four conflict categories.

Chapter Four:
Improving Large-N Analysis of Conflict Causes: A Categorical Disaggregation of Intrastate Armed Conflict (Paper 1)[10]

Research Problem

Intrastate armed conflicts are often analysed as homogeneous phenomenon in large-N research (e.g., Collier, Hoeffler, & Rohner, 2009; Esteban, Mayoral, & Ray, 2012; Montalvo & Reynal-Querol, 2005). Conflicts, no matter whether governmental or territorial, ethnic or non-ethnic, are often packed under one heading ('civil war' or 'intrastate armed conflict') and tested against dozens of predictor variables in regression models. Several studies suggest, however, that certain variables have non-uniform effects on different conflict categories. Buhaug (2006) has shown, for example, that country size and ethnic fractionalization only affect territorial conflicts. Besancon (2005) has demonstrated that inequality has an opposite effect on ethnic and non-ethnic conflicts. This implies that aggregate models that include all conflict categories under the same heading could under- or overestimate the effects of predictors on particular conflict categories.

What the Paper Does

The paper tests this observation systematically. First, it disaggregates conflicts into four sub-categories based on two commonly used criteria: (i) the aims over which the conflict is fought and (ii) the ethnic composition of conflicting parties. The first criterion differentiates between conflicts fought over territories and conflicts fought over governments. The second criterion distinguishes between conflicts between ethnic groups and conflicts between members of the same ethnic group. The simultaneous application of these two distinctions results in the disaggregation of conflict into following categories (Table II).

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[10] Previous versions of Paper 1 were presented at internal seminars in the International Relations Research Section of the Department of Political Science and Government, Aarhus University; at the Centre for the Study of Civil War (Peace Research Institute Oslo); and at the 20th Norwegian National Conference in Political Science, Trondheim, Norway.

Table II. The four conflict sub-categories

		Fought over	
		Government	Territory
Fought between	Groups of different ethnicity	(1) Ethnic governmental	(2) Ethnic territorial
	Groups of the same ethnicity	(3) Non-ethnic governmental	(4) Non-ethnic territorial

Second, the paper develops a theoretical framework exploring states' proneness to particular conflict categories. Without attempting to explain the 'root causes' of different conflict categories, the paper delineates some of the structural, country-level characteristics that may predispose countries to one rather than another conflict category.

Third, to test the theoretical expectations developed in the theoretical framework, the paper introduces a new dataset that categorizes conflicts into the four categories – the Categorically Disaggregated Conflict (CDC) Dataset.

Finally, the paper tests the four conflict categories in multinomial regression models of conflict onset against country size, population size, ethnic fractionalization, ethnic polarization, federal structure, new state, and previous conflict experience.

Theory

The paper argues that large and populous countries create more favourable conditions for territorial rather than governmental conflicts (and vice versa), and that ethnically heterogeneous countries create favourable conditions for ethnic conflicts, but few opportunities for non-ethnic conflicts. The form *ethnic* conflicts take in ethnically heterogeneous countries depends on the size (both relative and absolute) of the ethnic groups: because of domestic (establishing and maintaining a new state) and international factors (recognition), small ethnic groups find secession less plausible than large ones.

Further, the paper argues that previous experience of ethnic conflict creates favourable conditions for new ethnic conflict (and the form of previous ethnic conflict determines the form of new ethnic conflict), but previous experience of non-ethnic conflict does not do so for non-ethnic conflict.

Finally, the paper suggests that a federal structure – especially in newly-established states – will contribute to secessionist mobilization and thus increase the risk of territorial conflicts. Combining these arguments, the paper proposes four sets of hypotheses on the link between country size, population

size, ethnic fractionalization, polarization, federal structure, new state and previous conflict on the one hand, and the four conflict categories on the other.

Research Design

The paper relied on standard country-year logit and multinomial logit regressions covering the time period 1946–2009. The sample included all annual observations of states as defined by Gleditsch and Ward (1999). Conflict onsets were coded with nominal categories: ethnic governmental conflicts 1, ethnic territorial 2, non-ethnic governmental 3, non-ethnic territorial 4 and countries without conflicts 0 (reference category). Since the paper focused on the outbreak of conflict, country-years after the year of onset were set to 0. The final number of conflict *onsets* in the model amounted to 331: 59 ethnic governmental, 128 ethnic territorial, 124 non-ethnic governmental, and 20 non-ethnic territorial.

Main Findings and Conclusions

Paper 1 demonstrates that most of the predictors (country size, population size, ethnic fractionalization, ethnic polarization, federal structure, and previous conflict experience) have substantially different effects on the four conflict categories (Table III).[11]

More importantly, the paper finds that some variables even have an opposite effect on different conflict categories. In accord with these findings, the paper concludes that inferences based on categorically aggregate models should be treated with particular caution. Consider the following example of the effects of ethnic heterogeneity for illustration.

Previous studies have largely failed to establish a significant relationship between ethnic heterogeneity and onset of aggregate conflict (e.g., Collier & Hoeffler, 2004; Fearon & Laitin, 2003). As shown in Table III (Model 1), Paper 1 finds that the relationship between ethnic heterogeneity, proxied by Ethnic Fractionalization Index (Alesina et al., 2003), and aggregate conflicts is indeed insignificant. Figure 2 (upper part) shows why this is the case. Ethnic heterogeneity has opposing (and significant) effects on ethnic and non-ethnic conflicts.

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[11] Data on country size, population size, ethnic fractionalization, and ethnic polarization were taken from the World Bank (2011), Maddison (2010), and Alesina et al. (2003), respectively. Data on federal structure was compiled by the author using World Constitutions Illustrated (HeinOnline, 2012).

Table III. Logit and multinomial logit estimates of conflict onset (Paper 1, main results)

	Model(1)	Model(2)			
	Aggregate conflict	Ethnic governmental	Ethnic territorial	Non-ethnic governmental	Non-ethnic territorial
MAIN MODEL					
Surface area(ln)	.021(.052)	**.308**(.118)**	-.140(.115)	-.008(.070)	.188(.223)
Population size(ln)	**.247**(.061)**	-.171(.136)	**.799**(.128)**	.089(.080)	-.192(.267)
Fractionalization index	.266(.339)	**3.741**(.750)**	.219(.520)	**-1.401*(.576)**	-1.735(1.502)
Polarization index	**.834*(.333)**	**-2.109**(.754)**	**2.170**(.580)**	**1.829**(.521)**	1.937(1.524)
Federal state	.219(.192)	-.245(.515)	-.599(.362)	.293(.311)	**2.314**(.725)**
New state	**.568*(.264)**	.680(.519)	.778(.522)	.320(.422)	**2.003**(.734)**
Previous conflict	**.514**(.173)**	**.786*(.373)**	.663(.389)	.343(.252)	**1.446*(.586)**
GDP per capita(ln)	**-.405**(.075)**	-.264(.212)	-.276(.145)	**-.512**(.103)**	**-.821*(.382)**
Peace years	**.380*(.166)**	-.501(.371)	**1.335**(.299)**	-.135(.282)	-.550(.847)
Constant	**-8.364**(.870)**	**-8.368**(1.811)**	**-12.761**(.996)**	**-5.038**(.788)**	**-7.963**(2.421)**
N	7544	7544			
Wald chi2	187.54	395.37			
DISAGGREGATED PREVIOUS CONFLICT					
Previous_e_gov		**1.511**(.408)**	-.572(.393)	-.432(.407)	.923(.805)
Previous_e_ter		.433(.503)	**1.784**(.346)**	-.068(.311)	1.680(.943)
Previous_ne_gov		-.682(.402)	.023(.275)	.455(.244)	.713(.522)
Previous_ne_ter		.909(.475)	.199(.275)	.127(.342)	.838(.721)

Logit coefficients (β) with robust standard errors in parentheses; * p <.05; ** p <.01.

Figure 2. Estimated probabilities (with 95% confidence intervals) of conflict onset as a function of Ethnic Fractionalization Index (Alesina et al., 2003), holding other variables at their mean values. The probabilities were estimated using *CLARIFY* software (Tomz, Wittenberg, & King, 2003).
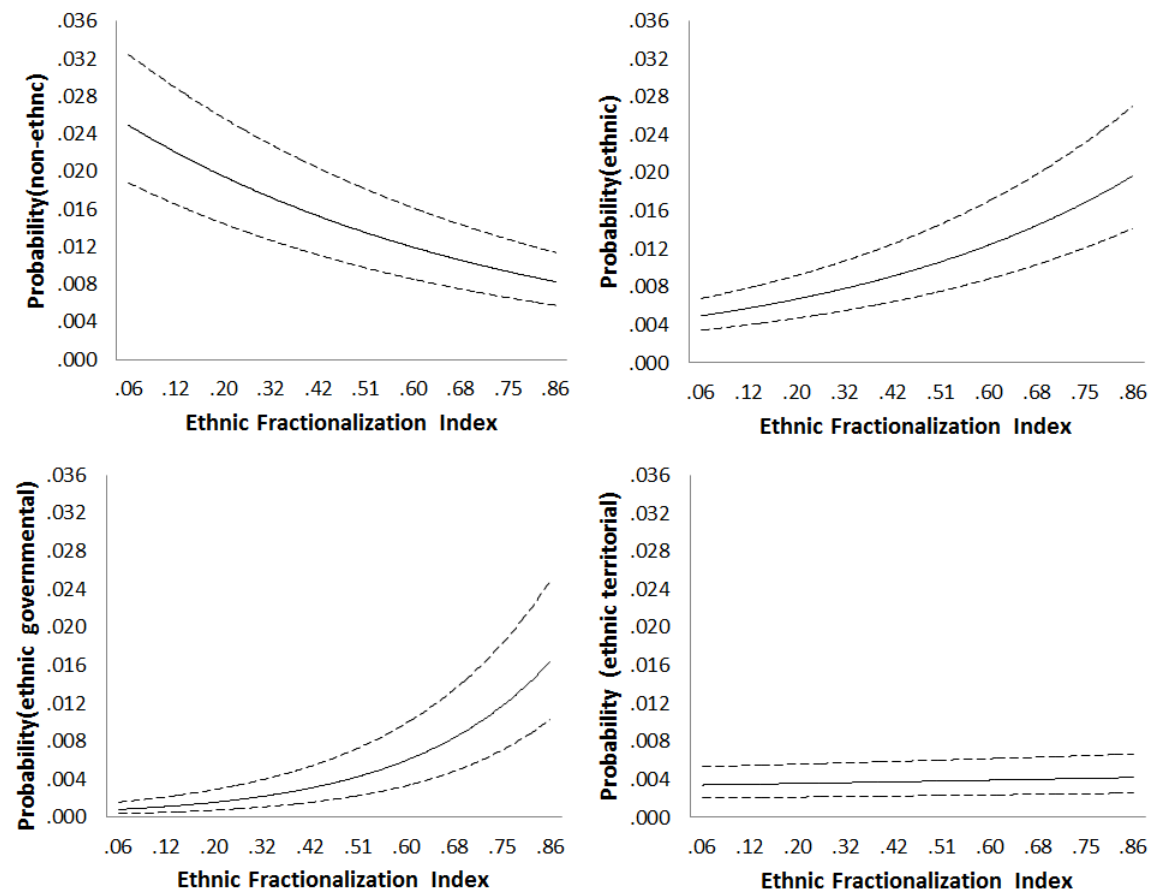

Figure 2 (lower part) also shows that ethnic fractionalization has non-uniform effects on ethnic governmental and ethnic territorial conflicts. Thus, the inference, for example, that 'ethnic heterogeneity increases the risk of ethnic conflict' is not precise – ethnic fractionalization, as shown in the figure, only increases the risk of ethnic *governmental* conflicts.

Chapter Five:
The Inequality-conflict Nexus Re-examined: Income, Education and Popular Rebellions (Paper 2)[12]

Research Problem

The impact of inequality on the outbreak of conflicts has recently attracted considerable interest in conflict research. In contrast to previous research that has focused on the distribution of resources in the total population (vertical inequality), recent studies have focused on the distribution of resources among certain groups of people (horizontal inequality) (Stewart, 2002; 2008). The recent studies have found that inequality significantly increases the likelihood of conflict onset (Cederman, Weidmann, & Gleditsch, 2011; Østby, 2008). However, most of the recent studies of the inequality-conflict nexus have focused on conflicts fought between ethnic groups (e.g., Cederman, Weidmann, & Gleditsch, 2011; Østby, 2008). The relationship between inequality and other (non-ethnic) categories of conflict has attracted less attention.

What the Paper Does

The paper attempts to address this gap. First, it provides a theoretical analysis of the relationship between vertical inequality and one particular form of non-ethnic conflict – *popular rebellion.* Popular rebellion is a form of non-ethnic governmental conflict, in which (1) mobilization transcends ethnic boundaries (i.e., conflicting parties are not limited to particular ethnic groups) and (2) hostilities involve popular participation (i.e., conflict is not limited to an elitist struggle among the incumbents).[13]

Second, the paper implements a statistical analysis of the relationship between vertical inequality and popular rebellion onset. The study employs two

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[12] Previous versions of Paper 2 were presented at an internal seminar in Centre for the Study of Civil War, Peace Research Institute Oslo; and at the 53rd Annual Convention of the International Studies Association, San Diego, CA, United States.
[13] Good examples of popular rebellions include the Salvadoran Civil War (1979–1992), the Guatemalan Civil War (1960–1996) or the Peruvian Civil War (1980–2000). For an elaborate definition see Paper 2, pp. 7–9.

recently introduced datasets on income and educational inequality (see below) that significantly expand upon previous inequality data.

Theory

Paper 2 argues that inequalities result in relative deprivation among the disadvantaged (e.g., Gurr, 1968, 1970; Davies, 1962; Runciman, 1966), which triggers the frustration-aggression mechanism predisposing individuals to commit violence (Dollard et al., 1939; Berkowitz, 1989). Further, the paper argues that inequalities strengthen the salience of social identities of the disadvantaged, which facilitates mobilization of solitary individuals for collective action (Gurr, 2000; Van Zomeren, Postmes, & Spears, 2008). In essence, Paper 2 explains the vertical inequality-popular rebellion nexus based on the 'Individual relative deprivation' and 'Social identity' pathways described in Paper 4.

Research Design

The paper relied on a standard country-year logit regression analysis covering 1961–2009. The sample included all annual observations of states as defined by Gleditsch & Ward (1999). The data on the outcome variable was taken from the CDC. The paper employed Category 3 ('non-ethnic governmental') as a base for coding popular rebellions. Since this paper focused on the outbreak of popular rebellion, country-years after the year of onset were set to 0. The analysis encompassed 77 onsets of popular rebellion.

Inequality was proxied by two indices representing the distribution of income and educational attainment in the total population: the Gini Index of Net Income Inequality (t-1) (hereafter income Gini) (Solt, 2009) and the Gini Index of Educational Inequality (t-1) (education Gini) (Benaabdelaali, Hanchane, & Kamal, 2012). Income Gini represents inequality in the net income in the total population and education Gini inequality in the educational attainment (proxied by schooling years) in the total adult population (age 15 and above).

Main Findings and Conclusions

Paper 2 demonstrates that inequality significantly predicts popular rebellion onset (see Tables IV and V). In addition, the paper reveals that proxies of inequality consistently outperform proxies of the absolute level of income – the effect of GDP per capita, one of the most robust predictors of (aggregate) conflict onset (Dixon, 2009; Hegre & Sambanis, 2006), becomes insignificant when

income and educational inequality are controlled for in the model of rebellion onset. This finding challenges the widely-established 'opportunity' approach (Collier & Hoeffler, 2004) and corroborates the theory of relative deprivation (Gurr, 1970), suggesting that it is relative, not absolute, well-being that ultimately motivates people to rise up in arms.

Table IV. Logistic regression estimates of popular rebellion onset (Paper 2, main results)

	(1.1)	(1.2)	(1.3)	(1.4)	(1.5)	(1.6)	(1.7)	(1.8a)	(1.8b)	(1.9a)	(1.9b)
Income Gini	3.276* (1.482)		3.186† (1.817)	3.403† (1.846)	3.372† (1.859)	3.411† (1.939)	3.685† (1.901)	3.076† (1.781)	29.805† (17.568)		
Income Gini^2									-30.679 (20.945)		
Education Gini		1.833*** (.494)	2.330** (.776)	2.050* (.933)	2.089* (.914)	2.341* (1.133)	2.224† (1.138)			1.203 (1.022)	11.683* (5.458)
Education Gini^2											-9.1873* (4.470)
Xpolity scores				-.006 (.046)	.109 (.245)	.127 (.244)	.113 (.235)	-.106 (.253)	-.167 (.244)	.225 (.212)	.180 (.203)
Xpolity scores^2					-.007 (.014)	-.008 (.014)	-.007 (.014)	.005 (.015)	.009 (.014)	-.014 (.013)	-.011 (.012)
GDP per capita(ln)						.086 (.285)	.053 (.283)	-.253 (.247)	-.215 (.241)	-.054 (.244)	-.004 (.226)
GDP per capita growth							.063 (.044)	.052 (.034)	.050 (.034)	.039 (.037)	.038 (.036)
Population size(ln)	.216* (.094)	.265*** (.060)	.265* (.106)	.270* (.110)	.271* (.111)	.266* (.117)	.259* (.108)	.204* (.104)	.202† (.104)	.214** (.079)	.193** (.075)
Peace years	.638† (.098)	.131 (.322)	.449 (.429)	.509 (.480)	.502 (.474)	.516 (.478)	.554 (.489)	.502 (.430)	.433 (.416)	.282 (.389)	.154 (.412)
Constant	-9.819*** (1.842)	-9.947*** (1.024)	-11.693*** (2.199)	-11.705*** (2.573)	-12.062*** (2.694)	-12.255*** (3.209)	-12.330*** (3.048)	-8.875*** (2.434)	-14.221*** (4.406)	-9.474*** (1.888)	-11.678*** (2.263)
Wald χ²	18.87	38.24	36.86	29.63	28.89	37.17	38.84	50.29	46.59	29.50	30.29
N	4130	6200	3682	3199	3199	3149	3149	3462	3462	5004	5004
N of popular rebellions	36	57	31	28	28	28	28	32	32	46	46

Logit coefficients (β) with robust standard errors in parentheses. † p <.10; * p <.05; ** p <.01; *** p <.001.

34

Table V. Logistic regression estimates of popular rebellion onset (imputed data) (Paper 2, main results)

	(2.1)	(2.2)	(2.3)	(2.4)	(2.5)	(2.6)	(2.7)	(2.8a)	(2.8b)	(2.9a)	(2.9b)
Income Gini	2.590** (.825)		1.922* (.900)	1.995* (.902)	1.900* (.905)	1.773† (.951)	1.811† (.950)	1.959* (.951)	12.890 (9.450)		
Income Gini^2									-11.940 (11.056)		
Education Gini		1.813*** (.455)	1.595*** (.489)	1.303* (.586)	1.302* (.579)	1.107 (.692)	1.095 (.689)			1.192† (.681)	11.782** (4.320)
Education Gini^2											-9.150* (3.756)
Xpolity scores				-.032 (.0314)	.209 (.183)	.193 (.187)	.184 (.185)	.149 (.181)	.144 (.174)	.188 (.188)	.178 (.183)
Xpolity scores^2					-.015 (.011)	-.014 (.011)	-.013 (.011)	-.012 (.011)	-.011 (.010)	-.013 (.011)	-.012 (.011)
GDP per capita(ln)						-.094 (.167)	-.116 (.174)	-.226 (.152)	-.212 (.148)	-.165 (.169)	-.085 (.158)
GDP per capita growth							.028 (.024)	.028 (.023)	.028 (.023)	.027 (.023)	.026 (.022)
Population size(ln)	.236*** (.054)	.232*** (.053)	.260*** (.056)	.267*** (.055)	.271*** (.054)	.267*** (.055)	.263*** (.054)	.251*** (.052)	.248*** (.053)	.236*** (.051)	.227*** (.050)
Peace years	.240 (.259)	.079 (.274)	.061 (.275)	.090 (.276)	.051 (.278)	.020 (.287)	.040 (.290)	.091 (.287)	.047 (.285)	.030 (.292)	-.072 (.299)
Constant	-9.476*** (1.022)	-9.226*** (.917)	-10.349*** (1.082)	-10.125*** (1.158)	-10.801*** (1.262)	-10.473*** (1.446)	-10.452*** (1.436)	-9.534*** (1.206)	-11.829*** (2.392)	-9.312*** (1.243)	-11.903*** (1.628)
F	8.99	12.47	10.38	8.73	8.26	8.08	7.10	7.84	5.74	7.63	6.33
Average RVI	.008	.034	.020	.072	.094	.085	.081	.082	.168	.084	.079
Imputed datasets	5	5	5	5	5	5	5	5	5	5	5
N	7471	7471	7471	7471	7471	7471	7471	7471	7471	7471	7471
N of popular rebellions	77	77	77	77	77	77	77	77	77	77	77

Logit coefficients (β) with robust standard errors in parentheses. † p <.10; * p <.05; ** p <.01; *** p <.001.

Chapter Six:
From Non-violent to Violent Conflicts:
Examining Conflict Militarization
(Paper 3)[14]

Research Problem

A number of present-day states have experienced internal political conflicts that had the potential of escalating into an armed conflict. Yet there have been more states that have managed to solve these crises through the use of non-violent means than those that have not. What explains the violent turn in non-violent conflicts? More specifically, why do some conflicts turn violent (i.e., militarize), while others do not? Previous large-N research has identified a number of variables that increase the risk of armed conflict onset (e.g., Dixon, 2009; Hegre & Sambanis, 2006). Yet we know little about which of these variables account for why conflicts start (*originate*) and which account for why conflicts become violent (*militarize*).

What the Paper Does

This paper presents a new approach to intrastate conflict analysis. In contrast to the traditionally posed question, 'What are the causes of armed conflicts?' this paper asks, 'What are the factors that lead to conflict militarization?' In place of commonly used comparisons of countries 'at peace' with countries 'at war', this paper compares countries 'at non-violent (or non-armed) conflict' with countries 'at violent (or armed) conflict'. The paper provides two contributions to conflict research. First, it delineates a theoretical framework that facilitates isolation of the factors that contribute to conflict militarization (see below). Second, using the newly introduced Conflict Information and Analysis System (CONIAS) dataset, the study shows that the likelihood of conflict militarization depends significantly on the military capacity of conflicting parties.

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[14] Previous versions of Paper 3 were presented at an internal seminar in the Comparative Politics Research Section of the Department of Political Science and Government, Aarhus University; at the 44th Annual meeting of the Danish Political Science Association, Vejle, Denmark; and at the 54th Annual Convention of the International Studies Association, San Francisco, CA, United States.

Theory

The paper argues that the reasons for why conflicts start and why they militarize are different. Conflict *origination* is related to 'underlying causes' that motivate conflicts (such as grievances over ethnic discrimination or political repression). Conflict *militarization* is related to 'facilitating causes' that facilitate conflicts (such as availability of arms and recruits). The paper suggests therefore that conflict research needs to adopt a more dynamic, two-stage framework to analyse conflict onset (see Figure 3). The first stage needs to be primarily focused on underlying causes and the turn from 'peace' to 'non-violent conflict' (Arrow 1) and the second on facilitating causes and the turn from 'non-violent conflict' to 'violent conflict' (Arrow 2).

Figure 3. Framework for conflict analysis
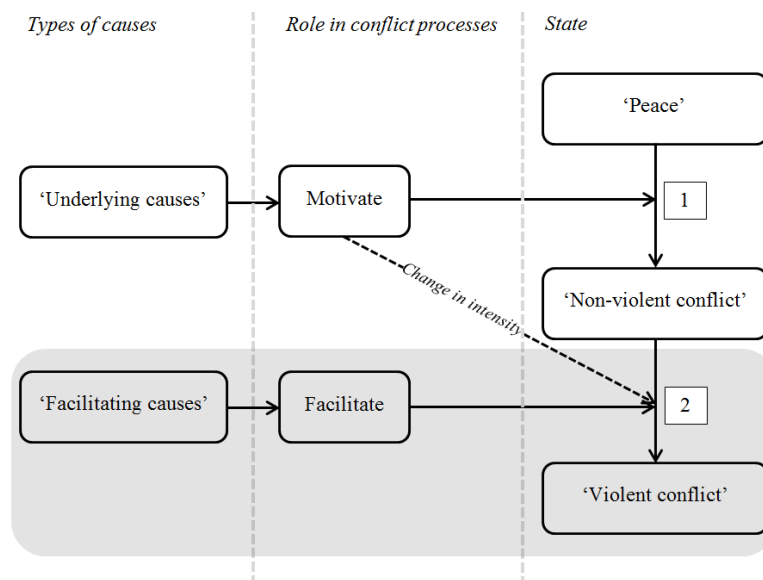
Subsequently, the paper claims that conflict militarization (Paper 3 does not deal with conflict origination) significantly depends on the military capacity of conflicting parties. First, the military capacity of a state determines the extent of the state's control over its territories and population. The stronger the state's control, the higher its capacity to identify potential rebels, and thus to prevent the would-be rebels from establishing a rebel organization. In addition, the stronger the state's control, the higher its capacity to track down illegal flows of arms and, thus, the higher its ability to prevent the equipping of would-be rebels.

Second, the military capacity of a state (and the potential military capacity of would-be rebels), determines the would-be rebels' decision to start an armed conflict. The would-be rebels' decision to take up arms and join a col-

lective action that involves high risks is based on the calculation of the chances of success and potential costs. Thus, the stronger the state and the weaker the rebels, the lower the chances of rebel success – and, conversely, the weaker the state and the stronger the rebels, the higher the chances of rebel success.

Research Design

The paper relied on a standard logistic regression analysis covering all conflict dyad-years in the world recorded from 1961-2008. The primary unit of analysis was a conflict dyad-year, which was coded with '1' if the conflict reached the level of 'violent conflict' and '0' if the conflict remained 'non-violent'. Given that the paper was primarily focused on the onset of violent conflict, ongoing years of violent conflict were dropped. To construct the proxy of the outcome variable, the paper employed the newly introduced Conflict Information and Analysis System (CONIAS) Dataset (Schwank et al., 2013). CONIAS classifies conflicts into 'disputes', 'non-violent crises', 'violent crises', 'limited wars', and 'wars'. Paper 3 aggregated disputes, non-violent crises, and violent crises into the category of 'non-violent conflict' and limited wars and wars into the category of 'violent conflict'. In turn, Paper 3 operationalized conflict militarization as a change in the conflict category from non-violent conflict to violent conflict. Military capacity of conflicting parties was proxied by the size of military personnel, military expenditures, simultaneous conflict, previous conflict, 'youth bulges', GDP per capita and size of the internally displaced persons (IDP) population.[15]

Main Findings and Conclusions

The paper finds that the likelihood of conflict militarization significantly depends on the military capacity of the conflicting parties (see Tables VI and VII). In particular, non-violent conflicts are more likely to militarize if (1) the military resources of a state are either scarce or abundant; if a state has (2) recently experienced, or (3) is currently experiencing, an armed conflict with another actor; and if rebel recruitment is aided by (4) youth bulges and (5) a significant population of internally displaced persons. In addition, the study reveals that

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[15] Data on military personnel and expenditures, 'youth bulges', GDP per capita, and IDP population was taken from Singer (1987), United Nations Department of Economic and Social Affairs, Population Division (2011), Maddison (2010), and Marshall (2008) respectively. Data on federal structure was compiled by the author using World Constitutions Illustrated (HeinOnline, 2012).

population size and GDP per capita – variables that have repeatedly been shown to increase the likelihood of armed conflict onset (e.g., Dixon, 2009; Hegre & Sambanis, 2006) – fail to account for why non-violent conflict militarize. The same applies to inequality proxies (see Table VIII) – the paper finds that inequalities fail to predict conflict militarization (though, as shown in Paper 2, inequalities significantly increase the risk of [non-ethnic] conflict onset).[16] This implies that the reasons for why conflicts start and why they militarize are different.

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[16] As mentioned above (Chapter Two), the effects of the inequality proxies on conflict militarization are only reported in this summary (they are not presented in the final version of Paper 3).

Table VI. Logistic regression estimates of conflict militarization (military personnel)

	(1.1)	(1.2)	(1.3)	(1.4)	(1.5)	(1.6)	(1.7)	(1.8)
Military personnel (ln)	-.234** (.091)	-.271** (.090)	-.282** (.095)	-.109 (.092)	-.055 (.090)	-.037 (.095)	-.041 (.096)	-.561** (.207)
Military personnel (ln)^2								.063** (.024)
Previous conflict		.762*** (.169)	.650*** (.190)	.643*** (.187)	.599*** (.188)	.600** (.198)	.613** (.199)	.639** (.206)
Simultaneous conflict			.670*** (.195)	.651*** (.191)	.680*** (.197)	.721*** (.198)	.696*** (.199)	.767*** (.202)
Youth bulges				.080*** (.015)	.069*** (.017)	.063*** (.018)	.065*** (.018)	.074*** (.018)
GDP per capita (ln)					-.128 (.103)	-.179 (.112)	-.150 (.118)	-.112 (.119)
IDP						.013† (.008)	.012 (.007)	.016* (.008)
Ethnic conflict							.238 (.163)	.246 (.164)
Population size (ln)	.144 (.094)	.122 (.092)	.105 (.097)	-.018 (.094)	-.076 (.094)	-.072 (.099)	-.071 (.097)	-.139 (.098)
Peace years (decay)	.042 (.168)	-.318† (.183)	-.706*** (.218)	-.895*** (.214)	-.962*** (.228)	-1.104*** (.246)	-1.129*** (.246)	-1.103*** (.245)
Constant	-3.609*** (.618)	-3.501*** (.602)	-3.250*** (.630)	-5.166*** (.777)	-4.337*** (.866)	-4.192*** (.928)	-4.419*** (.911)	-3.224*** (1.036)
N	5753	5753	5753	5729	5527	4997	4997	4997
Wald χ^2	9.78	32.79	38.40	64.30	62.49	68.62	76.64	83.25

Logit coefficients (β) with standard errors clustered on countries in parentheses; † = $p < .10$; * = $p < .05$; ** = $p < .01$; *** = $p < .001$.

41

Table VII. Logistic regression estimates of conflict militarization (military expenditures)

	(2.1)	(2.2)	(2.3)	(2.4)	(2.5)	(2.6)	(2.7)	(2.8)
Military expenditures (ln)	-.184*** (.055)	-.189*** (.055)	-.188*** (.056)	-.055 (.063)	.020 (.068)	.047 (.078)	.030 (.079)	-.862** (.305)
Military expenditures (ln)^2								.034** (.012)
Previous conflict		.653*** (.171)	.556** (.188)	.569** (.186)	.545** (.189)	.573** (.198)	.593** (.200)	.649** (.207)
Simultaneous conflict			.560** (.198)	.558** (.192)	.582** (.199)	.637*** (.201)	.613** (.202)	.664*** (.202)
Youth bulges				.074*** (.016)	.067** (.017)	.063*** (.018)	.065*** (.018)	.076*** (.020)
GDP per capita (ln)					-.163 (.113)	-.240* (.120)	-.195 (.129)	-.172 (.128)
IDP						.007 (.011)	.007 (.012)	.009 (.011)
Ethnic conflict							.239 (.168)	.279† (.169)
Population size (ln)	.114 (.073)	.068 (.073)	.048 (.076)	-.054 (.078)	-.126 (.082)	-.126 (.092)	-.116 (.090)	-.141 (.087)
Peace years (decay)	-.106 (.178)	-.421* (.192)	-.746*** (.228)	-.847*** (.221)	-.904*** (.232)	-1.065*** (.252)	-1.092*** (.253)	-1.115*** (.253)
Constant	-1.881*** (.460)	-1.605*** (.494)	-1.369** (.520)	-4.320*** (.837)	-4.185*** (.867)	-4.347*** (.963)	-4.480*** (.957)	.992 (2.016)
N	5694	5694	5694	5670	5475	4946	4946	4946
Wald χ^2	15.32	32.35	34.90	54.85	57.12	64.35	69.46	75.24

Logit coefficients (β) with standard errors clustered on countries in parentheses; † = $p < .10$; * = $p < .05$; ** = $p < .01$; *** = $p < .001$.

Table VIII. Logistic regression estimates of conflict militarization (income Gini and education Gini)

	(1.1)	(1.2)	(1.3)	(1.4)	(1.5)	(1.6)
Income Gini	-.818 (1.653)	-.685 (1.735)			-1.517 (1.878)	-1.436 (2.052)
Education Gini			.447 (.620)	.305 (.574)	.442 (.894)	.280 (.869)
Military personnel (ln)	**-.902** **(.302)**		**-.690** **(.240)**		**-1.046*** **(.326)**	
Military personnel (ln)^2	**.099** **(.033)****		**.077** **(.029)****		**.118** **(.035)****	
Military expenditures (ln)		**-.952*** **(.393)**		**-.917** **(.347)**		**-1.051*** **(.460)**
Military expenditures (ln)^2		**.033*** **(.015)**		**.036** **(.014)**		**.037*** **(.018)**
Previous conflict	.423 (.278)	.431 (.285)	**.601*** **(.235)**	**.572*** **(.235)**	.440 (.326)	.442 (.326)
Simultaneous conflict	**.869*** **(.272)**	**.791** **(.262)**	**.798*** **(.231)**	**.697** **(.233)**	**.983*** **(.299)**	**.872** **(.293)**
Youth bulges	**.079** **(.028)**	**.068*** **(.031)**	**.091*** **(.021)**	**.097*** **(.022)**	**.086** **(.033)**	**.079*** **(.035)**
GDP per capita (ln)	.047 (.185)	.011 (.191)	.045 (.143)	-.044 (.156)	.157 (.214)	.126 (.227)
IDP	.019 (.017)	.013 (.019)	**.019** **(.007)**	.010 (.012)	.026 (.018)	.018 (.021)
Ethnic conflict	.224 (.221)	.228 (.221)	.217 (.180)	.247 (.182)	.160 (.234)	.161 (.235)
Population size (ln)	-.145 (.164)	-.023 (.164)	-.089 (.123)	-.075 (.115)	-.192 (.189)	-.003 (.195)
Peace years (decay)	**-.837** **(.287)**	**-.901** **(.297)**	**-.994*** **(.266)**	**-1.014*** **(.270)**	**-.857** **(.308)**	**-.950** **(.318)**
Constant	-2.527 (1.543)	1.423 (2.832)	**-4.422*** **(1.472)**	-.315 (2.463)	-2.087 (1.905)	1.666 (3.379)
N	3379	3376	4341	4295	2966	2966
Wald χ²	47.73	41.20	70.94	61.05	46.22	38.85

Logit coefficients (*β*) with standard errors clustered on countries in parentheses; † = p < .10; * = p < .05; ** = p < .01; *** = p < .001. The effects of income Gini and education Gini were similarly insignificant when ethnic conflicts were altogether removed from the sample (not reported here).

Chapter Seven:
The Inequality-conflict Nexus
Re-examined: How Does Inequality
Cause Conflicts? (Paper 4)[17]

Research Problem

In recent years, a number of studies have investigated the relationship between inequality and conflict. It has been shown that inequalities between ethnic groups (Buhaug, Cederman, & Gleditsch, 2013; Cederman, Weidmann, & Gleditsch, 2011; Østby, 2008), social classes (Bartusevičius, 2013a; Besancon, 2005), regions (Buhaug et al., 2011; Murshed & Gates, 2005; Østby, Nordås & Rød, 2009), and administrative units in federations (Deiwiks, Cederman, & Gleditsch, 2012) significantly contribute to the outbreak of conflicts. However, most of the recent studies on the inequality-conflict nexus have been limited to covariational research focused on 'What is the relationship between X and Y?' (or 'What are the causal effects of X on Y?'). The question of 'How X causes Y' has attracted less attention.

What the Paper Does

The paper attempts to address the 'how' question in the inequality-conflict nexus by testing the causal pathways through which *economic* inequalities are thought to lead to conflict in a number of qualitative case studies.[18] First, the paper reviews the literature on the inequality-conflict nexus and identifies three main causal pathways that have been used to explain the relationship between economic inequality and conflict. Second, the paper specifies the observable implications (or predictions) of the three causal pathways. Finally, the paper assesses the (non-)congruence between the observable im-

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[17] A previous version of Paper 4 was presented at the Peace Science Society-International Studies Association Joint International Conference, Budapest, Hungary.

[18] The paper focuses on economic inequalities, as previous large-N research on the inequality-conflict nexus has mainly analysed distribution in economic goods (mostly income and land).

plications of the three causal pathways and actual data in 16 cases of conflict onsets.

Theory

Based on a survey of conflict literature, the paper identifies three main causal pathways through which inequalities are thought to lead to conflict:

– *Individual relative deprivation pathway.* Inequalities are thought to generate feelings of relative deprivation among the disadvantaged (Auvinen & Nafziger, 1999; Davies, 1962; Gurr, 1968, 1970; Nagel, 1974; Runciman, 1972; Sigelman & Simpson, 1977).[19] Relative deprivation is thought to predispose individuals to commit violence, because of the frustration-aggression mechanism (Gurr, 1970: 33-37).[20] Individuals predisposed to commit violence are, in turn, more susceptible to rebel mobilization, which leads to higher risk of armed conflict.[21]

– *Group relative deprivation pathway.* Feelings of relative deprivation develop not only from comparison of oneself to others, but also from comparison of one's own *group* to others (e.g., Runciman, 1972: 37). Thus, just as with inequalities between individuals, inequalities between groups (ethnic or socioeconomic) can trigger the frustration-aggression mechanism described above (or aid the collective action), and thus lead to a higher risk of armed conflict.

– *Separatists' greed pathway.* Territorial conflicts (conflicts over autonomy or secession) can be initiated by the advantaged as well (Buhaug et al., 2011: 817–821; Cederman, Weidmann, & Gleditsch, 2011: 483; Horowitz, 1985: 249–254; Østby, Nordås, & Rød, 2009: 306). This is often the case when the advantaged, residing in wealthier regions, perceive that they 'are subsidizing poorer regions' (Horowitz, 1985: 250) or that 'state-level redistribution denies them the fruits of their success' (Cederman, Weidman, & Gleditsch, 2011: 483). Higher wealth differentials between

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[19] 'A person is relatively deprived of X when (i) he does not have X; (ii) he sees some other person or persons, which may include himself at some previous or expected time, as having X, (iii) he wants X, and (iv) he sees it as feasible that he should have X' (Runciman, 1972: 11).

[20] For the original formulation of frustration-aggression hypothesis see Dollard et al. (1939: 1). For an updated version of the frustration-aggression hypothesis see Berkowitz (1989).

[21] Alternatively, the link from relative deprivation to conflict is sometimes explained by higher susceptibility to collective action among the disadvantaged (e.g., Van Zomeren, Postmes, & Spears, 2008: 505–506).

regions can therefore lead to secessionism claims, and thus increase the risk of conflict.[22]

Research Design

The paper tests the three causal pathways in congruence analysis (Blatter & Blume, 2008). First, the paper specifies observable implications (predictions) of the three causal pathways. Second, it randomly selects one-fourth of ethnic governmental, ethnic territorial, non-ethnic governmental and non-ethnic territorial conflict onsets recorded in the post-Cold War period (1992–2010). Finally, the paper tests the (non-)congruence of the specified observable implications of the three causal pathways to the actual data in particular conflicts.

Main Findings and Conclusions

The paper finds considerable evidence to support the three causal pathways. In ~45% of the cases (in ~20% of ethnic conflicts and ~70% of non-ethnic ones) the paper finds evidence to support the 'individual relative deprivation pathway'. In ~55% of the cases (in ~80% of ethnic conflicts and ~30% of non-ethnic ones) the paper finds evidence to support the 'group relative deprivation pathway'. Finally, in ~45% of territorial conflicts (in 20% of ethnic territorial and 100% of non-ethnic territorial)[23] the paper finds evidence to support the 'separatists' greed pathway'.

In addition, as indicated by the numbers presented above, the paper finds that the three causal pathways are associated with different conflict categories. Specifically, individual relative deprivation and separatists' greed are linked to non-ethnic governmental and non-ethnic territorial conflicts (respectively), while group replative deprivation is linked to ethnic conflicts.

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[22] The three causal pathways are not exhaustive (though, according to my survey of the conflict literature, they have been referred to most commonly). I was able to identify at least two more – the 'Social identity pathway' and the 'Defending status quo pathway.' (I briefly describe these pathways in Paper 4, though I do not test them in the case studies).

[23] Note that the case studies included only seven territorial conflicts (two non-ethnic and five ethnic).

Chapter Eight:
Implications and Suggestions
for Future Research

This PhD project has attempted to re-examine the relationship between inequality and conflict, which, until very recently, has been considered weak or even non-existent in conflict research. Using disaggregated concepts of inequality and conflict – and newly constructed data – this project has demonstrated that vertical inequalities significantly contribute to the likelihood of (non-ethnic) conflict onset. In combination with recent work on horizontal inequalities and ethnic conflicts, this study concludes that the relationship between inequality and conflict is significant and robust.

Subsequently, the project has shown that, although inequalities well account for (non-ethnic) conflict onset, they fail to predict conflict militarization. This suggests that the inequality-conflict nexus is primarily a consequence of the relationship between inequality and conflict origination, rather than a consequence of the relationship between inequality and conflict militarization.

In addition to the empirical patterns established in the quantitative analyses, this project has attempted to assess the causal theories used to explain the inequality-conflict nexus. Based on 16 qualitative case studies, the project has demonstrated that the main causal pathways through which inequalities are thought to generate conflicts (the 'individual relative deprivation pathway', the 'group relative deprivation pathway', and the 'separatists' greed pathway') are largely plausible.

The inequality-conflict nexus now seems to have been substantiated theoretically, in the large-N setting as well as within a number of case studies. In the following paragraphs of this chapter, I provide a summary of the specific implications of the theoretical and empirical analyses presented in the four papers and broad suggestions for future research.

Field-specific Implications

Perils of Aggregation (Paper 1)

The effects of conflict predictors can be non-uniform across different conflict categories. In fact, the effects of conflict predictors can be *opposite* across different conflict categories. This implies that the aggregate conflict models

that pack all conflict categories under the same heading can under- or overestimate the effects of predictors on particular conflict categories. Insignificant estimates, therefore, should not be rejected based on such categorically aggregate models. Conversely, significant coefficients derived in aggregate models should be treated with particular caution.

'Square Pegs in Square Holes, Round Pegs in Round Holes' (Papers 2 and 4)

The effects of vertical and horizontal inequalities are non-uniform on ethnic and non-ethnic conflicts. This potentially explains the non-findings of previous research on the inequality-conflict nexus that has packed ethnic and non-ethnic conflicts under the same heading. Correctly specified models – where horizontal inequalities are empirically linked to ethnic conflicts and vertical inequalities to non-ethnic ones – demonstrate that inequalities significantly increase the likelihood of conflict onset. This conclusion is now supported not only in the large-N setting, but also within a number of case studies.

Genuine Effects (Paper 2)

The relationship between vertical inequality and non-ethnic conflicts (or, more precisely, popular rebellions) is largely independent of regime type, absolute level of income or economic growth. This implies that inequality has a non-spurious effect on states' proneness to (non-ethnic) conflicts. While inequality could ultimately be rooted in one of these factors, the immediate effects of inequality seem to outperform the immediate effects of regime type, absolute level of income, and economic growth.

'Relative' Matters More than 'Absolute' (Paper 2)

Distribution of income can play a more important role in conflict onset than the absolute level of income, implying that would-be rebels' decision to join a rebellion may depend on their relative and not on their absolute level of income. This suggests, then, that what may ultimately motivate people to rise up in arms is not lower opportunity costs, but grievances over unequal distribution of income. Therefore, further studies should consider controlling for the distribution of income whenever the role of absolute income in conflict is analysed.

Conflict Origination versus Conflict Militarization (Paper 3)

While inequalities can explain why conflicts start, they cannot account for why conflicts turn violent. Factors that account for conflict militarization are linked to the military capability of conflicting parties. Conflict researchers, therefore, need to recognize the fact that reasons for why conflicts start and why they turn violent are not necessarily the same. Appreciation of the fact that factors accounting for the origination and militarization of conflicts are different could potentially help us to arrive at better-specified empirical models, as well as more explicit (and thus falsifiable) hypotheses.

Ambiguous Effects of Conflict Predictors (Paper 3)

Conflict researchers also need to consider the fact that the effect of the same variable could be non-uniform (including opposite) on conflict origination and conflict militarization. The sum effect of such variables can thus mistakenly be taken as significant or insignificant in those analyses that do not distinguish between non-violent and violent conflicts. This, in turn, suggests that we should not accept or reject our hypotheses based on significant/insignificant results derived in models of armed conflict onset that lump non-violent and violent conflicts into 'conflict' (or 'non-conflicts' and 'non-violent conflicts' into 'peace').

Suggestions for Future Research

Making Sense of Successful Quantitative Analyses

As noted above, until very recently the relationship between inequality and conflict was thought to be weak or non-existent. Over the last two or three years, however – owing to a number of (categorically and geographically) disaggregated quantitative studies – a consensus seemingly emerged among conflict researchers: inequalities, especially horizontal, are significant predictors of conflict. While the link between vertical inequalities and non-ethnic conflicts still needs to be scrutinized, the link between horizontal inequalities and ethnic conflict appears to be robust. One could therefore argue that it is time to 'make sense of successful quantitative social inquiry' (Johnson, 2006: 239–240) – which, in the context of inequality-conflict nexus, means finding out *how* inequalities lead to conflict. Only by specifying the processes through which inequalities lead to conflicts we will be able to generate concrete guidelines for policy makers aimed at preventing conflicts.

Scaling Down

Arguably, the most appropriate way to make sense of our quantitative findings on the inequality-conflict nexus is to explore the 'how' question at the micro-level. This means, most aptly, employing process tracing techniques (e.g., Beach & Pedersen, 2013; George & Bennet, 2005: 205–232; Gerring, 2007: 172–185) focused on each of the causal steps in the theorised causal chains. Paper 4 has implemented a number of basic tests of the causal theories at the meso level and found that the main causal pathways used to explain the inequality-conflict nexus are largely plausible. The next step is to assess these causal pathways at a lower level of analysis focused on *processes* leading to conflicts.

In-depth analyses of the micro processes in particular conflicts are not without challenges, however. Process tracing – which, in many cases, implies fieldwork – incurs additional logistical costs that are non-existent in secondary or 'desk' research. Data collection in micro-level analyses involves a number of practical difficulties as well. Reaching conflict participants for interviews, for example, can often be difficult or impossible. Gathering data in conflict zones can also be physically dangerous.

Given these trade-offs, conflict researchers could consider 'non-field' alternatives, including survey and laboratory experiments. Despite limited external validity, manipulation of variables linked to particular aspects of the causal theories in a simulated setting could potentially result in valuable insights into how the inequality-conflict nexus operates at the micro level. Theoretical and methodological developments within social psychological research, which has analysed the relationships among relative deprivation, collective action, and conflict for at least as long as conflict research analysed the inequality-conflict nexus (e.g., Jost & Kay, 2010: 1134–1136; or Van Zomeren, Postmes, & Spears, 2008: 505–507), would be of particular use.

Exploring Interactions

Surprisingly little research has been done on potential interactions between inequalities and other variables in their effects on conflicts. While it has commonly been observed that sharp inequalities have not always led to conflict, little effort has been made to investigate *under what conditions* inequalities generate conflicts. The effects of inequalities on conflicts can be contingent on a number of variables: the degree to which inequalities are perceived as 'just' or 'legitimate', the extent to which inequalities are blamed on governments, the degree to which people can alleviate inequalities through non-violent means, etc. Interacting vertical and horizontal inequali-

ties with regime type or corruption measures, for example, in disaggregated models of conflict onset would be some of the potential ways to find out under what conditions inequalities have conflict-inducing effects.

As suggested in Paper 3, inequalities alone (and motivational factors in general) can hardly explain the outbreak of conflict, however. To fully explain the outbreak of conflict, we also need to account for factors that facilitate violence. One way to do this is to analyse conflict origination and conflict militarization in two steps, as suggested in Paper 3. The other is to interact inequalities with facilitating variables in the standard models of conflict onset, taking care to provide precise theoretical explanations on the ways through which inequalities, in combination with other factors, could lead to *violent* conflicts.

Dynamics in Inequality Levels

Few studies within the literature on the inequality-conflict nexus have analysed the dynamics in inequality levels. While inequalities tend to reproduce and rarely change over short periods, in the longer term, rising inequalities could have conflict-inducing effects. Indeed, in his original theory, Gurr emphasized patterns of changes in relative deprivation, claiming that 'because RD [relative deprivation] is a psychically uncomfortable condition, men tend over the long run to adjust their value expectations to their value capabilities' (Gurr, 1970: 46). This suggests that empirical models accounting for changes in inequality levels could even better account for conflict than the models employing static measures of inequality.

Gurr's theory then also suggests that relative deprivation arises not only from comparison of one's own situation to others', but also from comparison of one's own situation to his situation in the past or potential situation in the future. Discrepancies between what Gurr termed 'value expectations' and 'value capabilities' in the form of 'decremental', 'aspirational', or 'progressive' deprivation – combined with the patterns between vertical and horizontal inequalities on the one hand and ethnic and non-ethnic on the other – constitute potential avenues for future research on the inequality-conflict nexus.

Beyond Inequality-Conflict Nexus

Finally, the theoretical and empirical insights on vertical and horizontal inequalities presented in this project might be fruitfully applied in other areas of social research. Armed conflict is just one of the many (and, perhaps, one of the least common) outcomes of sharp inequalities among individuals or social groups. Research on inequality's effects on domestic violence, protest,

regime change, repression, democratization, economic development, and many other social phenomena could potentially benefit from incorporating into theoretical and empirical models the findings on differences in the effects of vertical and horizontal inequalities revealed in this project.

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Appendix A.
The Categorically Disaggregated
Conflict (CDC) Dataset Codebook
(Version 1.0, 2013.03)

The *Categorically Disaggregated Conflict* (CDC) *Dataset* provides a categorization of 331 intrastate armed conflicts recorded between 1946 and 2010 into four categories:

1. Ethnic governmental;
2. Ethnic territorial;
3. Non-ethnic governmental;
4. Non-ethnic territorial.

The dataset uses the UCDP/PRIO Armed Conflict Dataset v.4-2011, 1946 – 2010 (Themnér & Wallensteen, 2011; also Gleditsch et al., 2002) as a base (and thus is an extension of the UCDP/PRIO dataset). Therefore, the dataset employs the UCDP/PRIO's operational definition of an aggregate armed conflict:

> a contested incompatibility that concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths (Themnér, 2011: 1).

As described in Paper 1, the dataset contains only *internal* and *internationalized internal armed conflicts* listed in the UCDP/PRIO dataset. Internal armed conflict 'occurs between the government of a state and one or more internal opposition group(s) without intervention from other states' (Ibid.: 9). Internationalized internal armed conflict 'occurs between the government of a state and one or more internal opposition group(s) with intervention from other states (secondary parties) on one or both sides'(Ibid.). For full definitions and further details please consult the codebook of the UCDP/PRIO dataset (Ibid.) and the website of the Department of Peace and Conflict Research, Uppsala University: http://www.pcr.uu.se/research/ucdp/definitions/.

The categorization of the aggregate intrastate armed conflicts into the four categories follows the coding criteria described in Paper 1. The dataset contains the following variables:

A. All original variables contained in the UCDP/PRIO Armed Conflict Dataset v.4-2011, 1946–2010 (Themnér & Wallensteen, 2011; also Gleditsch et al., 2002).

B. Variables introduced in the CDC.

For 'A' variables please consult the codebook of the original dataset (Themnér, 2011). 'B' variables are described below.

1) 'SideAName' – Full name in English of 'SideA' as coded in the UCDP Actor Dataset v. 2.1-2011 (2011) (variable 'Name_Orig_FullEng').

2) 'SideBName' – Full name in the original language and English (in parentheses) of 'SideB' as coded in the UCDP Actor Dataset v. 2.1-2011 (2011) (variables 'Name_Orig_Full' and 'Name_Orig_FullEng').

3) 'Difference' – the variable identifies whether SideA and SideB were ethnically different (as defined in Paper 1), and (if yes) what were the differences (1 – Language; 2 – Religion; 3 – 'Race'). The names of the languages and religions are taken from the *Ethnologue* (Lewis, Simons, & Fenning, 2013) and *World Christian Database* (Johnson, 2007) (hereafter WCD).

4) 'Language' – the variable takes the value of 1 if SideA and SideB spoke different *native* languages and 0 if SideA and SideB spoke the same native language. To distinguish between two dialects of the same language and two separate languages CDC uses Ethnologue's listing of languages. Ethnologue follows ISO 639-3 inventory of identified languages (http://www-01.sil.org/iso639-3) as the basis for the listing of languages. The primary criterion for distinguishing between individual languages and dialects of the same language in *Ethnologue* is mutual intelligibility. See *Ethnologue's* website for further details (http://www.ethnologue.com/about/problem-language-identification). Note that CDC considers individual languages composing one 'macrolanguage' as representing the same language.

5) 'Religion' – the variable takes the value of 1 if SideA and SideB followed different religions and 0 if SideA and SideB followed the same religion.

6) 'Race' – the variable takes the value of 1 if SideA and SideB represented different 'races' and 0 if SideA and SideB represented the same 'race'.

7) 'Religion2' – same as 'Religion' but disregards confessional differences within main religions (i.e., Sunni Islam and Shia Islam; Orthodox Christianity, Catholicism, and Protestantism).

8) 'Ethnic' – the variable takes the value of 1 if SideA and SideB are different in at least one of the three characteristics (i.e., language, religion

and 'race',) and 0 if SideA and SideB are the same in all three characteristics.

9) 'Ethnic2' – the variables takes the value of 1 if SideA and SideB are different in at least two of the three characteristics (i.e., language, religion, and 'race') and 0 if SideA and SideB are the same at least in two of the three characteristics.

10) 'Ethnic2 (religion2)' – same as 'Ethnic2' but disregards confessional differences within main religions (i.e., Sunni Islam and Shia Islam; Orthodox Christianity, Catholicism and Protestantism).

11) 'Category' – identifies the category of the conflict: 1 – ethnic governmental; 2 – ethnic territorial; 3 – non-ethnic governmental; 4 – non-ethnic territorial.

12) 'Category_r2' – same as 'Category' but applying 'Religion2' criterion.

13) 'Category_e2' – same as 'Category' but applying 'Ethnic2' criterion.

14) 'Category_e2r2' – same as 'Category' but applying 'Religion2' and 'Ethnic2' criterion.

15) 'Coding description' – provides a detailed description of the four coding steps described in the paper:
   1. Identification of the parties to a conflict (i.e., the names of SideA and SideB);
   2. Determination of the composition of the parties to a conflict (i.e., individuals and groups constituting SideA and SideB);
   3. Determination of the ethnic differences of the parties to a conflict (i.e., linguistic, religious, and 'racial' differences between groups represented by SideA and SideB);
   4. Determination of the pattern of confrontation between parties to a conflict (i.e., determination of whether conflict involved intra-ethnic fighting; and [if yes] determination of the scale of the intra-ethnic fighting).

16) 'Uncertainty' ('1' – coding is deemed highly certain; '2' – coding is deemed uncertain due to availability of data; '3' – coding is deemed uncertain due to the nature of conflict [described in the coding descriptions]; '4' – coding is deemed ambiguous due to availability of data and the nature of conflict [described in the coding descriptions]).

17) 'EPRcodes' – identifies the composition of the government as coded in the Ethnic Power Relations (EPR) dataset (Cederman, Min, & Wimmer, 2009). The variable lists groups (and their statuses) represented in the government (SideA), i.e., 'Monopoly', 'Dominant', 'Senior partner' and 'Junior partner' and groups represented in SideB. This variable provides an opportunity to compare the coding of SideA based on the author's

identified primary and secondary sources and the coding of SideA based on country experts as described in the EPR dataset.

*Please note that this project is **ongoing**. The coding descriptions and coding sources are constantly updated. The information contained in the coding descriptions will be more extensive as new data becomes available.*[24] *Comments, suggestions, and updates from area/country experts are especially welcome and should be sent to:* henrikas@ps.au.dk.

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[24] Please also note that, while the coding of conflicts in the CDC has been finalised, the coding descriptions, for some conflicts, remain unavailable. Every single conflict has been carefully considered, and the material (together with sources) used to inform the coding of every case is kept in the author's personal notes. Potential users willing to know the individual coding decisions for cases where coding descriptions are temporarily unavailable are welcome to contact the author.

Appendix B.
An example of coding description (Category 1):

ID: 65

Location: Laos

SideBName: Lao Resistance Movement

Startdate2: 12/1/1989

Difference: (1) Language (Lao vs. Hmong) (2) Religion (Buddhists vs. Ethnoreligionists)

Category: 1

Uncertainty: 1

EPRcodes: Lao (incl. Phuan) 'Senior partner', Hmong 'Junior partner' in 1989

Coding description:

1. SideA: Government of Laos; SideB: Lao Resistance Movement (LRM).

2. SideA: Since the Communist takeover in 1975, a number of Lao Theung and Lao Sung people (see above, ID: 65, year 1959) were installed in the state administration (Savada, 1995: Chapter 2: Population: Ethnic Diversity). Thus, the composition of the state apparatus became somewhat more ethnically heterogeneous. However, Lao Loum continued to dominate the government and key administrative positions (Ireson & Ireson, 1991: 925; Savada, 1995: Chapter 4) (as mentioned above, the leadership of Pathet Lao, who took over the power in 1975, were Lao Loum, see ID: 65, year 1959). The Lao People's Revolutionary Party (LPRP) – which has dominated and controlled political life in Laos since its Communist takeover – was also predominantly Lao Loum (Ibid.: Chapter 4: The Lao People's Revolutionary Party: Party Structure).

SideB: LRM was composed of Hmong people (Uppsala Conflict Data Program, 28 November 2012). EPR codes Hmong as a 'Junior partner' in the LPRP controlled government. So far, however, I have been unable to confirm whether this was indeed the case. It is known that some 20% of Hmongs fought on the side of the Communists during the Laotian Civil War (i.e., the conflict between Pathet Lao and the Royalist Government – see above, ID:

65, year 1959) (the rest were fighting on the CIA-supported anti-Communist side) (Duffy et al., 2004: 6). Thus, it is likely that after the Communist takeover some Hmongs (who are the sub-group of the Lao Sung – see above, ID: 65, year 1959), were installed in the state administration. Nevertheless, their numbers in the executive and their de facto power remain unclear.

As mentioned above, Lao Loum dominated the LPRP, which has controlled the state since 1975. Lao Loum composed the vast majority of the LPRPs Central Committee – the second most important body in the hierarchy of the party (the first being the Party Congress), which was charged with leading the party between the congresses and included key government ministers, leading generals of the army, secretaries of provincial party committees, and chairpersons of mass organizations (Savada, 1995: Chapter 4: The Lao People's Revolutionary Party: Party Structure). The Fifth Party Congress held in 1991 elected only four non-Lao Loum members into the 59-member Central Committee (ibid.). To my knowledge, the only high-ranked Hmong officials in Lao People's Democratic Republic (LPDR) were Faydang Lobliayao and his brother Nhiavu Lobliayao (both of whom were members of Pathet Lao leadership during the civil war). In 1975, Faydang was appointed Vice-President of the Supreme People's Assembly and Nhiavu Chairman of the Nationalities Committee. However, their role was largely ceremonial, without any administrative or decision-making power (Lee, 1982). It is known that the first time Hmong entered the Politburo (the leadership of LPRP) was only in 2006 (Amnesty International, 2007: 4). Further, it is known that Hmong people have been systematically persecuted under Communist rule. The reason for this was Hmong involvement in the 'secret army' and support for the Royalist side during the Laotian Civil War. Right after Pathet Lao took power, thousands of Hmongs were imprisoned or fled the country (Lee, 1982; Savada, 1995: Chapter 5: Threats to National Security: Internal Threats and Resistance Movements: The Hmong; also Chapter 4: Challenges to the Regime: Insurgents; Amnesty International, 2007).

3. Lao Loum speak Lao and mainly follow Theravada Buddhism (93% according to WCD). Hmong include two groups: Hmong Der (White Hmong) and Hmong Leng (Blue Hmong) (Duffy et al., 2004: 2), who speak Hmong Daw and Hmong Njua, respectively – two mutually intelligible languages (Hmong Daw and Hmong Njua fall under macrolanguage Hmong in Ethnologue). Both Hmong Der and Hmong Leng are animists (99% according to WCD).

4. So far, I have been unable to find any information suggesting that the conflict involved intra-Hmong or intra-Lao Loum fighting.

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[25] Note that references in the CDC are provided in a single list at the end of the document.

Appendix C.
An example of coding description
(Category 2):

ID: 116

Location: Pakistan

SideBName: Mukti Bahini (Liberation Force)

Startdate2: 26/03/1971

Difference: (1) Language (Heterogeneous vs. Bengali)

Category: 2

Uncertainty: 1

EPRcodes: Punjabi 'Senior partner', Bengali 'Discriminated', Pashtuns 'Junior partner', Sidhi 'Junior partner', Ahmadis 'Junior partner' in 1971

Coding description:

1. SideA: Government of Pakistan; SideB: Mukti Bahini.
2. SideA: since independence in 1947 the government (as well as the civil service and the army) has largely been dominated by Punjabis (e.g., Alavi, 1989: 1527; Blood, 1995: Chapter 2: Social Structure: Punjabis; Talbot, 2004: 53-54), the largest of Pakistan's ethnic groups (after 1971). However, other major Pakistani ethnic groups – Bengalis (the largest until 1971), Pakhtuns, Sindhis, Muhajirs, and Baloch – have always been represented (to a greater or lesser degree) in the executive, bureaucracy and/or the army. It is known, for example, that at least until the mid-1970, Punjabis shared executive power with Muhajirs – they were represented in the highest governmental offices and the bureaucracy (e.g., Haq, 1995: 991; Alavi, 1989: 1527). It is also known, for example, that Pakhtuns were well-represented among the military officers and foot soldiers (e.g., Haq, 1995: 991; Ziring, 1974: Footnote 8). Further, East Pakistanis, most of whom were Bengali, composed 36% of the members of the elite Civil Service in 1968 (Blair, 1971: 2557). The degree to which these ethnic groups were represented in the executive, civil service and the army varied at different periods and their de facto power remains unclear, however.

When the conflict started (March 1971), Pakistan was under a military regime led by Yahya Khan (the president and the Commander-in-Chief of the Pakistan Army). Yahya and other members of the military junta effectively controlled the government (e.g., Blood, 1995: Chapter 1: Yahya Khan and Bangladesh). According to Ziring, Yahya's regime cannot be defined as a personal military dictatorship, because other members of the junta were involved in the actual decision making process: '...Yahya was neither an Ayub Khan [previous president] nor a potential dictator. In a word, he was absolutely dependent on his brother officers, who remained outside the public spotlight but very much involved in the decision making process' (1974: 406-407).

The prominent members of the ruling junta included: General Abdu Hamid Khan, the Army Chief of Staff and Deputy Chief Martial Law administrator; Lieutenant General S. M. G. Peerzada, staff officer and virtual military Prime Minister; Lieutenant General Gul Hassan Khan, Chief of the General Staff; Major General Ghulam Omar, Secretary of the Pakistan National Security Council; Major General Mohammad Akbar Khan, the head of military intelligence; and Abubaker Osman Mitha, the Quartermaster General, head of special Pakistani commando force (Special Service Group) (Ziring, 1974: 411).

Though often considered Pakhtun, Yahya Khan was member of Qizilbash (Amin, 2000: Footnote 11), a Persian-speaking Shia sect. Abdu Hamid Khan was Punjabi (Ziring, 1974: 411). S. M. G. Peerzada's ethnicity is unknown (so far I have only been able to establish that he was born in Bombay) (Ibid.). Gul Hassan Khan was Pakhtun (Shah, 1997: 176). Ghulam Omar was Punjabi (Amin, 2000: Footnote 11). Mohammad Akbar Khan was also Punjabi (Ziring, 1974: 411). Abubaker Osman Mitha was Memoni.

SideB: Mukti Bahini represented (and was composed of) Bengalis (e.g., Blood, 1995: Chapter 1: Yahya Khan and Bangladesh). Its nucleus was formed from the (defected) East Pakistan Rifles, provincial police, and Bengali members of the Pakistan Army stationed in the East Pakistan (Ziring, 1974: 418; LaPorte, 1972: 102).
3. SideA: Thus, at the time of the conflict, SideA was linguistically (Panjabi [Western], Pashto languages, Urdu, Persian, Memoni and others) and religiously (Sunni Islam and Shia Islam) heterogeneous (though Punjabis were numerically dominant).
SideB: East Wing (i.e., Bangladesh) Bengalis spoke Bengali and were predominantly Sunni Muslims.

4. The government forces employed in the conflict against Mukti Bahihi were dominated by Punjabis (in general, according to Amin [2000], by 1966 the foot soldiers of the Army were predominantly Punjabi), but also included Pakhtuns, Balochis, and Sindhs (e.g., Bose, 2005: 4469).

The highest command of the operation was mainly Punjabi as well. For example, Major General Rao Farman Ali, one of the top planners of Operation Searchlight (the military operation that started the conflict on 26 March 1971) and commander of the Pakistani forces in Dhaka during the operation, was Punjabi. Khadim Hussain Raja, another top planner of Operation Searchlight and commander of the 14th Division of the Army (the main army unit employed during the first phase of the operation) was born in Jhelum District (Punjab province), and thus, was most likely Punjabi as well. General Tikka Khan, the commander of (all) Pakistani forces in the East Wing during the operation, was also Punjabi (Amin, 2000: Footnote 11).

While Bengalis were excluded from the central government (see above), they were represented (though very underrepresented) in the army, both among officers and foot soldiers. It is known, however, that when the fighting broke out, there was a widespread mutiny among the Bengali officers and soldiers (e.g., Bose, 2005: 4465). Indeed, as mentioned above, the nucleus of Mukti Bahihi was formed from the Bengali mutineers from the Pakistan Army (Ziring, 1974: 418; LaPorte, 1972: 102). Yet Bose notes that 'While many Bengali army officers and police personnel eventually mutinied and joined the battle for liberation of Bangladesh, some Bengali officers and men remained loyal to a united Pakistan, fighting to the end for that cause and becoming POWs in India' (Bose, 2011: 398). As yet, however, I have been unable to establish the proportion of Bengalis fighting on the side of the government against Mukti Bahihi.

As the conflict evolved the government also formed paramilitary forces from the local East Wing (i.e., Bangladesh) population, the so called 'Razakars' (Razakars formed two groups, the 'Al Badr' and the 'Al-Shams'). Bose claims that Razakars included local Bengalis (Bose, 2005: 4476 and Footnote 35). Macdermot states that in 'areas where there were Biharis [Urdu-speaking Muslims from Bihar, India], the Razakars were recruited largely from the Biharis, but in many areas they were recruited from Bengalis who were loyal to the West Pakistan' (1973: 478). So far, however, I have been unable to establish the scale of their involvement in the conflict. Note also that Razakars were formed only in the later stages of the conflict (April-May). There is also an indication that part of the local Bengalis collaborated with the government (e.g., Bose, 2005: 4467-4468; 2011: 398); once again, however, I

have been unable to find any precise information on the scale of such collaboration.

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Appendix D. An example of coding description (Category 3):

ID: 63

Location: Lebanon

SideBName: Lebanese National Movement

Startdate2: 9/2/1975

Difference: SideA included members of ethnic group constituting SideB

Category: 3

Uncertainty: 1

EPRcodes: Maronite Christians 'Senior partner', Sunnis (Arab) 'Senior partner', Armenian Orthodox 'Junior partner', Druze 'Junior partner', Eastern Orthodox Christians 'Junior partner', Greek Catholics 'Junior partner', Shi'a Muslims (Arab) Junior Partner in 1975

Coding description:

1. SideA: Government of Lebanon; SideB: Lebanese National Movement.
2. SideA: The conditions introduced in the National Pact were formally upheld during the years of the Lebanese Civil War (1975-1989) as well; thus, during the first year of the conflict, the Lebanese Government was formally ethnically heterogeneous (see above, ID: 63, year 1958). Note, however, that the Lebanese Government cannot, in precise terms, be regarded as representing SideA, as members of the same cabinet at most times contained elements of some or all of the parties fighting each other (Uppsala Conflict Data Program, 22 November 2012). Indeed, UCDP/PRIO notes that it does not regard the entire Lebanese Government as constituting one party to a conflict (i.e., SideA). Instead, it specifies that SideA, in this particular episode (at the outbreak of conflict in 1975), is regarded as represented by the following actors: 'President Frangieh, the militias under his control (the Zghorta Liberation Army) and the national forces of the army, police, and gendarmerie. On the side of the status quo [i.e., the government] can also be found the forces of such politicians as Pierre Gemayel (the Phalangists) and Camille Chamoun (the National Liberal Party and its 'Tigers' militia). The latter groups are, however, not representatives of the government at this stage' (Ibid.).

SideA was predominantly Christian. The president and his Zghorta Liberation Army (also called Marada Brigade) were Christians (Makdisi & Sadaka, 2005: 63). The army and the security forces, at the start of the conflict, included both Christians and Muslims (see above). Gemayel and his Phalangists, as well as Chamoun and his 'Tigers', were all Christians (Salibi, 1958: 374; Collelo, 1989: Appendix B).

SideB: According to UCDP/PRIO, SideB – in 1975 – was 'Kamal Jumblatt's Lebanese National Movement (LNM), an umbrella group including parties such as the Druze-based Progressive Socialist Party (PSP) of Jumblatt himself, the Independent Nasserite Organization (or Mourabituoun, one of the main rebel groups of the 1958 crisis), the Syrian Social Nationalist Party (SSNP) and the Lebanese Communist Party (LCP), to mention just a few of the larger groups. Several Palestinian organizations also joined the LNM, such as the PFLP (Popular Front for the Liberation of Palestine) and the DFLP (Democratic Front for the Liberation of Palestine). The PLO - the largest Palestinian group - was never a member, but at times supported the LNM in combat' (Uppsala Conflict Data Program, 2012/11/22).

The majority of LNM members were Muslims: the PSP was predominantly Druze; the Independent Nasserite Organization was mainly Muslim; and the Palestinian organizations (PFLP and DFLP) were Muslim as well. However, two significant units within LNM were Christian: LCP, a 3,000-strong organization, was composed of Orthodox and Armenian Christians; and SSNP, an organization of the same size (which in the 1958 episode fought on the side of Chamoun and Christians, see above) was Christian as well (Collelo, 1989: Appendix B).

3. Thus, while this conflict has often been portrayed as an exemplary case of 'sectarian' conflict (i.e., Christians versus Muslims), it appears that members of the same religious community (i.e., Christians) were represented on both sides of the conflict. There were no linguistic or 'racial' differences between the parties to the conflict.

4. Exactly the first clashes between Zghorta Liberation Army (which, as mentioned above, was primarily Christian) and LNM (which, as noted above, included a substantial proportion of Christians) in early September are treated as the start of the conflict by UCDP/PRIO. Thus, initial stages of conflict involved intra-Christian fighting.

The conflict was eventually (mid-September) joined by the Lebanese Army and Security forces. While it is known that the army split along religious lines in January, 1976 (into the Christian-Led Lebanese Army [LA] and the Muslim Lebanese Arab Army [LAA]) (Collelo, 1989: Chapter 5: The 1975 Civil War: The Early Stages of Combat), I was unable to establish with certainty

what its role was in the initial phase of the conflict and whether Muslim units of the army (the Lebanese Army included both Christians and Muslims, see above) were used to confront the LNM in late 1975.

In the later stages of conflict (1976-1979) there were also intra-Christian clashes within SideA. Chamoun's National Liberal Party (NLP) confronted Gemayel's Phalangists in mid-1976 (Uppsala Conflict Data Program, 23 November 2012). Clashes between NLP 'Tigers' and Phalangists took place in 1977–1979 as well (Uppsala Conflict Data Program, 24 November 2012).

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Appendix E.
An example of coding description (Category 4):

ID: 52

Location: South Vietnam

SideBName: Front national de libération (National Liberation Front)

Startdate2: 4/1/1955

Difference: SideA included members of ethnic group constituting SideB

Category: 4

Uncertainty: 4

EPRcodes: Kihn (Vietnamese) 'Dominant' in 1955

Coding description:

1. SideA: Government of South Vietnam; SideB: National Liberation Front (NLF).
It is not clear which conflict UCDP/PRIO refers to here. UCDP/PRIO codes the start of the conflict April 1955 (the coding precision is set to '3' which means that the month and the year are coded precisely). The problem is that the NLF was not established until 20 December 1960 (Cima, 1989: Chapter One: Second Indochina War). While the residue of Viet Minh Communists operated in South Vietnam after the Geneva Accord in 1954, to my knowledge, before 1959 there were no serious military encounters between Communists and the Government (see, for example, Gravel, 1971: 314-346).
    There was another conflict in South Vietnam at the time: precisely in April 1955, the Battle of Saigon took place (the first military encounter took place on 31 March 1955) – a conflict that was fought, in the initial phase, between the Government of South Vietnam and Binh-Xuye (Fall, 1955: 252).
2. SideA: In 1955, the Government of South Vietnam was (de facto) in the hands of Ngo Dinh Diem (who had been Prime Minister since June 1954) and his family members (Sacks, 1967: 516; also Jacobs, 2010: 154) – the so called 'Ngo clan'. In October 1955 Diem assumed the position of Chief of State and by doing so completely deposed the Emperor Bao Dai from power (who, until October 1955 [note that conflict started in April 1955], had still played some role in the executive, being himself the Chief of State).

Diem was a devout (or even fanatical) Christian (Roman Catholic) (Jacobs, 2001: 599-601). His family members were Christians as well (Jacobs, 2004: 28). It is known that the 'government shamelessly favoured Catholics: a disproportionate share of U.S. aid went to [Catholic] refugees [fleeing from North Vietnam]; Northern Catholics held privileged positions in the Vietnamese National Army (VNA) and state bureaucracy' (Jacobs, 2001: 619) (see also Gunn & Slighoua, 2011: 47). Thus, it seems that the Government was dominated by Christians, who composed only 10% of Vietnam's total population at the time (Ibid.: 612). It is also known that Buddhists, who were the majority in South Vietnam (figures range from 70% to 85%, e.g., Roberts, 1965: 242), were discriminated against under Diem's dictatorship (Ibid.).

It is also known, however, that Buddhists were not excluded from the executive. For example, on 19 November 1954, Diem appointed Nguyen Van Vy, a Buddhist, the Chief of Staff of NVA (Jacobs, 2006: 67). Several key figures in the NVA were Buddhists as well: for example, General Duong Van Minh (who was the Minister of Defence in Diem's cabinet until late March in 1955, and who led the NVA in the battle of Saigon) or General Tran Van Don (who was at one point appointed Chief of Staff of the NVA). Diem's cabinet also included Buddhists: Tran Van Do (Foreign Minister), as well as the above-mentioned Doung Van Minh were Buddhists. I cannot, however, confirm with certainty the proportions of Christians and Buddhists in the executive. I also cannot ascertain their de facto power.

In racial and linguistic terms, SideA was predominantly Vietnamese (Diem and the 'Ngo clan' were Vietnamese). In general, since 1954, South Vietnam was in the hands of a Vietnamese elite (e.g., Cima, 1989: Chapter 2: Society in the 1954-75 period: South Vietnam). It is also known that ethnic minorities – not just Buddhists – were systematically discriminated against under Diem's dictatorship (Ibid.).

SideB: If UCDP/PRIO refers here to the Battle of Saigon, then SideB was composed of Binh-Xuye – an organized crime enterprise or, as Crozier puts it, 'a band of near gangsters of whom the Prime Minister [i.e., Diem] disapproved because of their unsavoury connections with gambling and prostitution' (1955: 51). The leader of Binh-Xuye was half Chinese, half Vietnamese. So far I have been unable to establish what the composition of the Binh-Xuye troops was. However, it seems that it did not represent any particular linguistic or religious group: 'Although the Binh Huyen often is spoken of as a "sect", it has no religious basis. Headed by a gang of ex-river pirates, it can only be described as "Murder, Inc."' (Grant, 1956: 439). And, given the fact that Binh-Xuyen consisted of some 5,000 troops (Crozier, 1955: 51), it must

have included a significant number of Vietnamese; of whom, as noted above, a majority were Buddhists.

If UCDP/PRIO refers here to the initial phase of the conflict between Communists in South Vietnam and the Government, then NLF was made of the residue of the Viet Minh (e.g., Gravel, 1971: 134-346), most of whose members were Vietnamese Buddhists (Jacobs, 2001: 606.).

3. Thus (regardless which conflict the UCDP/PRIO refers to), SideA must have included members of ethnic groups also constituting SideB. The coding, however, remains ambiguous. As shown above, Christians were favoured over Buddhists within the Government and thus it is likely that the former were dominant over the latter. On the other hand, even if Buddhists were dominated (and discriminated against) by Christians, their members held some of the key positions in the executive and thus have significantly contributed to the fighting effort against SideB, which (whether NLF or Binh Xuyen) included Vietnamese Buddhists as well.

4. If UCDP/PRIO refers here to the Battle of Saigon, then the conflict, in the initial phase, was largely limited to the confrontation between NVA soldiers and Binh Xuyen troops (Fall, 1955: 252).

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Summary

This PhD project investigates the relationship between inequality and intra-state armed conflict (or civil war) and attempts to answer the following questions: (1) Does inequality increase the likelihood of conflict? (2) What are the causal pathways through which inequality may increase the likelihood of conflict?

The project contributes to the conflict literature in several ways. First, it argues that empirical analyses of conflict onset need to consider potential differences in the effects of conflict predictors on conflict sub-categories. To demonstrate this, the project introduces a new dataset – The Categorically Disaggregated Conflict (CDC) Dataset – that categorizes conflicts into ethnic governmental, ethnic territorial, non-ethnic governmental and non-ethnic territorial. Subsequently, the project tests the four conflict categories in empirical analysis against a number of conflict predictors and finds that the effects of most of the predictors are non-uniform (including sometimes opposite).

Second and directly related to the first, the project argues that the failure to establish an empirical relationship between inequality and conflict in previous large-N studies is an outcome of a categorical aggregation of the dependent (conflict) and independent (inequality) variables. To properly test the inequality-conflict nexus, we need to distinguish between vertical inequalities and horizontal inequalities on the one hand, and ethnic and non-ethnic inequalities on the other. Correctly specified models, where vertical inequalities are linked to non-ethnic conflicts and horizontal inequalities to ethnic ones, show that inequalities significantly increase the risk of conflicts. The project substantiates this claim both in a large-N setting and within 16 qualitative case studies.

Third, the project presents a new approach to conflict analysis that focuses on conflict militarization. In place of commonly used comparisons of countries 'at peace' with countries 'at war', the project compares countries 'at non-violent (or non-armed) conflict' with countries at 'violent (or armed) conflict.' It finds that conflict militarization significantly depends on the military capacity of conflicting parties. In addition, the project finds that inequalities fail to account for conflict militarization, suggesting that the reasons for why conflicts start and why they become violent are not the same.

Finally, the project implements a number of case studies testing the causal pathways theorized in the literature on the inequality conflict nexus. The case studies largely support the theories and empirical patterns established in the large-N studies. Specifically, the case studies suggest that ine-

qualities lead to conflicts through generation of individual relative deprivation, group relative deprivation and motivation to secede.

The four contributions are presented in four single-author papers that have been published in or prepared for publication in peer-reviewed journals. The CDC dataset has been adjusted for further use in quantitative conflict research and made publicly available online.

Resumé

Denne ph.d.-afhandling undersøger sammenhængen mellem ulighed og borgerkrig og forsøger at besvare følgende spørgsmål: (1) Øger ulighed sandsynligheden for konflikt? (2) Igennem hvilke kausalstier øger ulighed sandsynligheden for konflikt?

Projektet bidrager til konfliktlitteraturen på flere måder. For det første argumenteres der for, at empiriske analyser af konfliktudbrud har brug for at tage højde for, at der muligvis er forskel på virkningen af konfliktprædiktorer på tværs af konflikttyper. Med henblik på at vise dette introducerer projektet et nyt datasæt – The Categorically Disaggregated Conflict (CDC) Dataset – som kategoriserer konflikter i fire typer: etnisk-regeringsorienterede, etnisk-territorielle, ikke-etnisk-regeringsorienterede og ikke-etnisk-territorielle. Efterfølgende tester projektet sammenhængen mellem de fire konfliktkategorier og en række konfliktprædikatorer i en empirisk analyse og finder, at virkningen af de fleste prædikatorer er uensartet (herunder modsatrettet).

For det andet – og direkte relateret til det første bidrag – argumenterer projektet for, at tidligere forsøg på at etablere et empirisk forhod mellem ulighed og konflikt i store-N-studier har været fejlslagne pga. brugen af aggregerede forståelser og målinger af henholdsvis de afhænige (konflikt) og uafhængige (ulighed) variable. For at gennemføre en ordentlig test af uligheds-konflikt-sammenhængen er vi nødt til på den ene side at skelne mellem vertikale uligheder og horisontale uligheder og på den anden side etniske og ikke-etniske uligheder på den anden side. Korrekt specificerede modeller, hvor vertikale uligheder er koblet til ikke-etniske konflikter og horisontale uligheder til etniske konflikter viser, at uligheder bidrager signifikant til at øge risikoen for konflikt. Projektet underbygger dette både gennem analyser med store-N-analyser og 16 kvalitative casestudier.

For det tredje præsenterer projektet en ny tilgang til konfliktanalyse, som fokuserer på konfliktmilitarisering. I stedet for den valige praksis med at sammenligne fredelige lande med borgerkrigsramte lande, sammenligner projektet lande med ikke-voldelige konflikter med lande med voldelige konflikter. Resultaterne viser, at konfliktmilitarisering afhænger af de brydende parters militære kapacitet. Derudover viser projektet, at uligheder ikke er forklaringsdygtige, når det gælder konfliktmilitarisering. Dette indikerer, at årsagerne, hvorfor konflikter opstår, og hvorfor de bliver voldelige, ikke er identiske.

Endelig gennemfører projektet en række casestudier med henblik på at analysere de kausalstier, som er teoretiseret i litteraturen om sammenhæn-

gen mellem ulighed og konflikt. Casestudierne giver generelt støtte til teori-erne og de empiriske fund fra store-N-analyserne. Mere konkret indikerer ca-sestudierne, at uligheder fører til konflikt ved at skabe individuel relativ depri-vation, relativ deprivation på gruppeniveau og motivation til at løsrive sig.

De fire bidrag præsenteres i fire eneforfattede artikler, som enten er pub-liceret i eller er forberedt til publicering i peer-reviewede tidsskrifter. CDC-datasættet er tilpasset til at kunne blive brugt i kvantitativ konfliktforskning og er gjort offentligt tilgængeligt.