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ABSTRACT

UNDERSTANDING CONFLICT:

AN EXPLORATION OF THE RELATIONSHIP BETWEEN HORIZONTAL INEQUALITY AND VIOLENT CONFLICT

By

MECCA SAMARIA MUHAMMAD

DECEMBER 2020

Committee Chair: Dr. Volkan Topalli

Major Department: Criminal Justice and Criminology

In this dissertation, I examine the relationship between horizontal inequality and violent conflict dynamics. Mass violent conflict is a debilitating social issue associated with economic deprivation, health crises, and loss of life. Scholars across various disciplines have studied its determinants. Several authors suggest the importance of horizontal inequality, or relative inequality between groups, for determining the likelihood of violent conflict. However, data limitations have prevented researchers from confidently asserting the generalizability or certainty of horizontal inequality's potential effect on conflict. It is also possible that horizontal inequality could have an effect on the dynamics of a conflict, such as which groups become involved in conflict. However, few researchers have examined horizontal inequality's relationship with other conflict dynamics. I fill some of these gaps by using census data to examine the relationship between horizontal inequality and 1) violent conflict occurrence, and 2) group involvement in conflict. I calculate six measures of social and economic horizontal inequality for each relevant cultural group in eight countries using census data ranging from 1971-2011, and I combine these data with existing data on political horizontal inequality. I then analyze how indicators of horizontal inequality relate to the likelihood of conflict in a given year, and the likelihood of a given group engaging in conflict. Results support the hypothesis that horizontal inequality

correlates with conflict dynamics; however, the specific way in which a society is unequal affects whether conflict occurrence or group involvement are more or less likely. Just as relative inequality seems to affect conflict differently than absolute inequality, the specific factors that are unequal appear to have differential effects on conflict occurrence and group involvement. Results also suggest that horizontal inequality does not just affect conflict, but also conflict dynamics. It seems the relationship between horizontal inequality and conflict dynamics could be more complex than previously thought. Further attention should be dedicated to pursuing additional data and studying this topic. Policy implications include that reducing social, political, and economic inequality between groups might help mitigate violent conflict, and that ameliorating the most pernicious forms of inequality might have the most significant effect.

UNDERSTANDING CONFLICT:
AN EXPLORATION OF THE RELATIONSHIP BETWEEN HORIZONTAL INEQUALITY
AND VIOLENT CONFLICT

BY
MECCA SAMARIA MUHAMMAD

A Dissertation Submitted in Partial Fulfillment
of the Requirements for the Degree
of
Doctor of Philosophy
in the
Andrew Young School of Policy Studies
of
Georgia State University

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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Criminal Justice and Criminology in the Andrew Young School of Policy Studies of Georgia State University.

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DEDICATION

To my family, my friends, my ancestors, and everyone else whose shoulders I have stood on to get here.

I dedicate this work to you.

ACKNOWLEDGEMENTS

I could not and did not complete this doctoral program alone. I will never be able to thank my family, friends, advisors, and broader support system enough for their dedication to helping me through this arduous process. I love you all more than I can express. Thank you.

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CHAPTER I: INTRODUCTION

The formal study of mass violent conflict emerged in the 1960s (Galtung et al. 2013) with important works such as those of Galtung (1964), who is known for pioneering peace and conflict studies and examining sustainable peace. Even before then, however, scholars and practitioners have expressed strong interest in understanding and preventing the advent of inter- and intra-national violent conflict. Works ranging from those of Thomas Hobbes (1651) to Immanuel Kant (1795) explore why conflict develops when and how it does. For years, scholars have undertaken this question with the rationale that, if we better understand violent conflict, we will be better equipped to predict and prevent it.

Scholars have examined a variety of conflicts such as interstate war, civil war, etc., but, for the purposes of this work, I employ the term *conflict* to refer to non-traditional mass civil violent contested incompatibilities between a state and non-state actor, where both parties are armed groups, and where the conflict takes place within a country. These intrastate struggles require at least twenty-five annual battle-related deaths to qualify as a violent conflict, as is consistent with the extant literature and databases on conflict (e.g., Gleditsch 2002; Stewart 2002; Braithwaite et al. 2010a; Kreutz 2010).

The discussion of violent conflict has spanned several centuries and received attention from several disciplines. Criminologists, political scientists, and scholars from various other fields have a vested interest in preventing violence, preserving stability, and promoting security. In criminology and sociology, for example, some scholars have suggested there is a duty to address macro-level violent conflict across countries, as its violent nature and association with high rates of victimization make it both a relevant and urgent topic of study (Schoenfeld et al. 2009; Moon 2011). Criminologists who study conflict argue they have a unique skillset allowing

them to make sense of why violence happens in these cases and can offer suggestions on how to best prevent it. They contend that, despite the fact that political scientists are often seen as leading the discussion on macro-level conflict, criminologists should take a more prominent role in its study, prevention, and mitigation. This, they assert, is because of criminologists' focus on studying both the perpetrators and victims of violence as well as the underlying conditions that bring them together.

Researchers' explorations of conflict, peace, and their determinants have led to several notable developments. Specifically, researchers have articulated novel concepts that might help better explain conflict, and they have taken advantage of newly-developed data and methodologies to test the applicability of these concepts. One recently developed concept is *horizontal inequality*, which occurs when there are social, political, or economic disparities between two or more collective groups (i.e., groups that each have a shared identity) (Stewart 2000). *Horizontal* inequality is the relative inequality between groups, as opposed to *vertical* inequality, which is a society's absolute, or total, inequality (Stewart 2000). The literature on horizontal inequality and violent conflict is lacking. Still, existing studies suggest that horizontal inequality might be a key influencer in conflict (Stewart 2002; Ostby 2008; Mancini 2008; Cederman et al. 2013).

New research methods are also emerging in the field. Historically, persistent data limitations have forced researchers to impute missing data, or employ case studies where there is a dearth of macro-level data. While these studies have been immensely contributive to the field's theoretical developments, the potentially biased nature of the data also limits their ability to identify causal relationships or claim generalizable correlations. For reasons discussed in Chapter 2, these data and methods have not fully addressed the question of whether, and how, horizontal

inequality affects violent conflict. Consequently, scholars are exploring the mechanisms through which better data and methods can be implemented to further the understanding of conflict.

Researchers across various fields have contributed to the above developments, with their commonality being their mutual interest in the etiologies and prevention of violence and victimization. Each of these fields have contributed to the study of inequality and conflict. Because of this mutual interest (and potentially, the mutual good that can come from the such research), it is worthwhile to look past disciplinary boundaries and begin exploring what scholars from a range of areas offer on the relationship between inequality and conflict¹. It is the interest in better understanding and ameliorating inequality, instability, and violence that has led many researchers (myself included) to take up their area of study. It is therefore useful to recognize and take into consideration the works of these scholars, who emerge from many academic backgrounds. Doing so helps provide a fuller understanding of inequality and conflict, and also makes it more possible to address gaps in the literature and further the knowledge-base on the topic.

Today, our understanding of the relationship between inequality and conflict is not fully developed. We still do not fully understand how horizontal inequality, or other relevant potential influencers (e.g., collective framing, governmental repression, etc.), relate to violent conflict or its dynamics. For the most part this is because we have historically had limited capabilities² to assess how well variables of interest predict conflict, and even further limited data with which to

¹ To this end, a fuller discussion of the various disciplines' most relevant contributions to this paper can be found in Chapter 2, "Literature Review." A discussion of potential steps towards interdisciplinary collaboration in this area of study can be found in section 6.2, "Conclusions."

² Many studies of conflict determinants, which often include simple correlational analyses, have performed poorly when actually predicting conflict (Ward et al. 2010). In these cases, statistically significant independent variables have not translated into a better ability to understand and predict when and where civil conflict occurs (Ward et al. 2010). However, some complex statistical models that have performed better at predicting conflict suffer from a "black box" phenomenon, in which it is difficult to disentangle the effect of any given independent variable (Cederman and Weidmann 2017). This again limits researchers' ability to understand what causes conflict and why.

try. The literature on horizontal inequality and conflict is relatively new, as the study of horizontal inequality was only first applied to conflict by Stewart (2000). Because of this, there are still a host of research questions surrounding its relationship to conflict and conflict dynamics.

There are several gaps in the literature on conflict and peace. To maintain a digestible scope and help ensure analytical integrity, I address only a narrow range of them. The goal is not to draw a definitive overall conclusion on why, how, or when conflict develops, but rather, to better identify the role horizontal inequality plays in shaping conflicts between groups within national societies. To this end, I specifically focus on illuminating how variations of horizontal inequality (social, economic, and political) correlate with 1) conflict occurrence and 2) group involvement in conflict. In the current work, I use census data from various countries to assess whether these variables seem to be correlated with violent conflict and which groups engage in that conflict.

The first gap I address is the absence of a cross-country analysis on horizontal inequality and conflict that solely uses existing data. Cross-country data on horizontal inequality has consistently had gaps, particularly in countries where grouped differences have been associated with violent conflict (Canelas and Gisselquist 2018). This is because 1) data collection is not standardized across countries, leading to different available variables of study, 2) data explicitly documenting intergroup inequalities is often censored in countries where such differences have led to violence and instability, and 3) measuring and studying the effect of horizontal inequality on conflict is relatively new, largely coming to practice in the early 2000s with the work of Frances Stewart (2000). As such, several studies on horizontal inequality rely on data that was not actually observed, but was inferred via back projection, imputation, or other methods (e.g.,

Ostby 2008; EPDC 2015). While these studies have been highly contributive to the field, the assumptions researchers have had to make about how inequality trends over time to generate the previously missing data have been questionable. If these assumptions are not empirically supported, their findings could be inaccurate. Therefore, research that does not rely on such assumptions would be a useful addition to the discourse. The findings would provide insight as to how horizontal inequality and violent conflict relate to each other, as it would rely on data that is more reflective of what has been observed in practice. I therefore solely used existing data in this study.

My second contribution is to incorporate a previously unstudied dependent variable: group involvement in conflict. Previous studies of horizontal inequality and conflict have largely focused on *whether* conflict occurs (e.g., Mancini 2008; Tiwari 2010; Cederman et al. 2011). Because of the limitations of prior studies and available data, this examination is still a worthwhile venture, particularly with additional data. However, it is also possible that horizontal inequality is implicated in the *dynamics* of violent conflict, such as which groups become involved in the conflict. To explore this possibility, I examine whether conflict occurs and identify those groups that are involved in the conflict. In doing so, I hope to provide insight that will help researchers better identify paths for further examination in the study of horizontal inequality and conflict dynamics. The ultimate goal is that such works will eventually help us better understand and prevent the development of intranational conflict.

As a third innovation, I implement measures of economic and social inequality that differ from those others have employed in many previous studies. The most prominent source of information on horizontal inequality is demographic data gathered by national governments or intergovernmental organizations. However, because these data were not collected for the purpose

of measuring horizontal inequality and are not standardized, they are not optimal for the study of the relationship between horizontal inequality and conflict. Relatedly, likely because it is a relatively new concept with limited available data, original exploratory analyses only featured simple measures of horizontal inequality. In these cases, a single indicator was employed to comprehensively represent social or economic horizontal inequality (ex., Ostby 2008). In these ways, researchers have traditionally used limited measures of horizontal inequality.

Subsequently, the estimated effects of horizontal inequality could therefore be less accurate (Chesher 1991). Simpler measures of horizontal inequality do not capture the breadth of what inequality between groups looks like on the ground, and their use could lead to inaccurate estimations of the effect of horizontal inequality on conflict (Kreutz 2010). In response to this gap, I incorporated measures of horizontal inequality that include indicators (e.g., child mortality rates, unemployment rates, etc.) that have yet to be featured in previous cross-country analyses, to my knowledge. While incorporating such measures will not completely rectify the problem of having a limited capture of what inequality looks like on the ground, it should provide a fuller picture and facilitate a better understanding of the value horizontal inequality has for explaining variations in conflict and its dynamics.

The fourth gap I address is the lack of a quantitative cross-country analysis of social, economic, and political horizontal inequality. Perhaps surprisingly, explicitly measuring and studying the effect of horizontal inequality – as opposed to general (or *vertical*) inequality – on conflict is relatively new. As such, there is a lack of diverse studies on horizontal inequality and violent conflict. After searching the literature, I have found none that feature measures of political, social, and economic horizontal inequality together in a single cross-country analysis. This limits the ability of scholars to examine general effects, differences in these effects, and

independence of these effects. It also limits their ability to determine which forms of inequality account for the most variance in conflict developments.

Although many other gaps exist within the conflict and peace literature, I focus here on addressing the above four. To do so, I employed census data to calculate broader measures of horizontal inequality and assess how they seem to correlate with conflict occurrence and group involvement in conflict. I engaged this strategy to better understand 1) how more comprehensive measures of horizontal inequality relate to conflict, given existing data, and 2) how horizontal inequality relates to group involvement.

To this end, I address two focal research questions:

- 1) What is the extent to which social, economic, and political horizontal inequality correlate with violent conflict occurrence?
- 2) What is the extent to which social, economic, and political horizontal inequality correlate with group involvement in violent conflict?

As the study of conflict and inequality develops, policymakers and researchers have provided evidence of research on peace and conflict leading to subsequent advances in practical conflict prevention (Ackermann 2003; Braithwaite et al. 2010b). In this dissertation, I seek to contribute to that body of evidence by informing how we understand the impact of horizontal inequality on conflict occurrence and involvement. It is my hope that this research will help us better understand how horizontal inequality relates to violent conflict and its dynamics, with implications for how we might better address them.

CHAPTER II: LITERATURE REVIEW

2.1 An Introduction to the Literature

For centuries, scholars have been exploring what causes peace and conflict, in the hopes that they can eventually learn to prevent conflict and encourage peace (Ackermann 2003). This years-long pursuit has produced a number of seminal works on the topic (e.g., Kant 1795; Galtung 1964a) and spanned across disciplines including criminology, political science, and psychology.

Studying peace and conflict is in the interest of scholars from all of these disciplines, as its causes and effects overlap with theories and research areas across all of them. Political scientists are concerned with the political stability and power dynamics of a society. Psychologists have interests in both how people rationalize violence and how violence affects their psychological well-being. Criminologists are interested in violence and security, both at the macro- and micro-level. They frequently study why a given act of violence occurs, and how stable, secure societies can be equitably encouraged and maintained. The study of peace and conflict, therefore, bears relevance for and features scholars from each of these (and other related) fields³. However, despite the interdisciplinary nature and prominence of peace and conflict studies, researchers are still debating their determinants in siloed fashion. Consequently, there remains much about this topic that we do not fully understand.

A considerable portion of the research on conflict has focused on identifying factors that contribute to it. In recent years, concepts like *horizontal inequality* and *collective framing*⁴ have

³ It simultaneously should be given more attention from researchers in some of these fields, such as criminology, where, despite some attention, many scholars primarily focus on micro-level acts of violence that occur in localized spaces.

⁴ Collective framing is the process by which a collective group makes sense of a given situation. See Chapter 2.3.1, “Collective Framing,” for a fuller discussion.

emerged and gained traction in the literature as potential explanations (Stewart 2000; Cramer 2003; Ostby 2008; Hagan and Rymond-Richmond 2008; Cederman et al. 2011). Ideas like these have shown some promise in these studies, but conclusions researchers can definitively draw from them are still constrained. This is due to limitations that have historically restricted researchers' analytical approaches.

Before grappling with issues as complex as “why and how conflict happens” and “why and how peace happens,” it is helpful to review the literature on such topics. Upon delving into these works, it is important to recognize that these two questions are different, despite feeding into and relating to each other. While some factors might seem to instigate conflict (in which case, addressing them might help prevent it), others might contribute to peace after conflict has already begun. Researchers examining these questions, therefore, have branched out into different directions and created a body of literature on why different kinds of conflict initially start, and why different kinds of peace⁵ take hold when and where they do.

I cannot cover all of these contributions. Therefore, for the purposes of this discussion, I will start at the beginning of the issue, where prevention might be most promising. I focus here on studies that ask why violent conflict develops, as well as when, where, and how it does. I specifically focus on studies that examine the factors I ultimately hypothesize are most explanatory of conflict.

In this dissertation, I focus on the relationship between horizontal inequality and intrastate mass violent conflict. *Conflict* is defined as an armed incompatibility between a state and non-state actor resulting in at least twenty-five annual battle-related deaths (Pettersen et al.

⁵ Peace can be conceptualized in several ways (see Galtung 1964 for a fuller discussion), including the absence of violence (often termed “negative peace”) or the presence of cooperation between formerly opposing groups (“positive peace”). Which peace is the topic of study is left to the individual researcher.

2019; Gleditsch et al. 2002). This is the definition most commonly used in the existing literature and databases on intrastate conflict (e.g., Gleditsch 2002; Stewart 2002; Braithwaite et al. 2010a; Kreutz 2010; Petterson et al. 2019). I also remain consistent with the existing literature in my examination of *intrastate*, rather than interstate, conflicts, since my analysis accounts for the relationship between inequality between groups and violent conflict within a society. Conflict researchers have suggested that inequality between groups within a given society is likely to affect whether there is violence between those groups in that society (Stewart 2000; Stewart 2008; Willems 2012; Cederman et al. 2013). Therefore, intrastate conflict is the conflict of focus.

2.2 Inequality: The Ultimate “It Depends”

Debates on what causes conflict have focused on the importance of factors such as inequality (Stewart 2008), opportunity (Collier and Hoeffler 2004), anomie (Braithwaite et al. 2010), and framing (Hagan and Raymond-Richmond 2008). A particularly prominent discourse has been the “greed versus grievance” debate (Collier and Hoeffler 2004; Ostby 2008), situated largely within the political science literature. This debate has demonstrated the potential importance – and nuance – of inequality on conflict.

The “greed versus grievance” debate encompasses two camps of scholars, one of which asserts the importance of opportunism in conflict onset (e.g., Collier and Sambanis 2002; Collier and Hoeffler 2004), and the other of which asserts the importance of grievances and inequality (e.g., Ostby 2008; Cederman et al. 2011). Grievance scholars had long posited the relevance of inequality on mass violent conflict, referencing events like the Arab Spring, in which social inequalities and political non-representation played a major role (Ansani and Daniele 2012; Willems 2012), and the genocide in Rwanda, in which ethnic division and unequal treatment erupted into mass violence (Willems 2012; Dallaire 2010). While these cases suggest a relatively

straightforward relationship between inequality and conflict, empirical inquiry thus far indicates that the relationship between inequality and conflict is characterized by nuance.

Indeed, empirical tests of this relationship often returned null or negative findings (e.g., Collier and Sambanis 2002; Fearon and Laitin 2003; Collier and Hoeffler 2004). These results suggest that the relationship between inequality and conflict was, at best, uncommon or inconsistent. In light of these findings, many scholars instead offered that having the opportunity to acquire resources was more important to facilitating civil war (Fearon and Laitin 2003; Collier and Hoeffler 2004). Therefore, factors that facilitated opportunism (e.g., low average incomes, little rule of law, large populations), were more indicative of these kinds of conflict. Proponents of this “greed” framework argue that, when governmental capabilities to control rebellion are weak and resources for rebels to gain are maximized, combatants are more likely to mobilize; where conflict can happen, it will. These scholars suggest that leading actors are truly motivated by resources like land and loot⁶; though, they might employ revolutionary rhetoric and propaganda to organize foot soldiers to conduct attacks (Collier and Hoeffler 2004; Dallaire 2010). Researchers with this perspective highlight the importance of resource acquisition, but downplay the relevance of inequality.

However, since the early-to-mid 2000s, some scholars have argued those findings do not tell the entire story. Those findings, they contend, are muted by the researchers’ reliance on traditional measures of inequality. Those who suggest inequality does not share a strong relationship with conflict typically measure total, or absolute, inequality, and they typically use

⁶ It should be noted that loot, land, and other forms of resource acquisition are means to acquire power, which is also the objective of grievance-driven fighters. It could therefore be hypothesized that, even when people engage in violence for resource acquisition, they are rectifying underlying sources of inequality and “empowering” themselves. Dallaire (2010) speaks to this somewhat when he discusses how some disenfranchised Rwandan youths felt empowered through their recruitment and activity with their armed groups. Likewise, Collier, Hoeffler, and Rohner make room for the possibility of this in some of their later work (Collier et al. 2009). However, the exact process of grievance-into-acquisition, if it exists, has not yet been explicitly studied, and is left to speculation.

the GINI coefficient to do so. The GINI coefficient is a commonly-implemented measure to capture inequality, calculated by different major organizations such as the World Bank or the United State Census Bureau. It is a measure of dispersion of income across the entire income distribution (USCB 2019). The GINI coefficient is useful for describing how income is generally distributed across a given country (e.g., whether one person receives all the income, or whether it is evenly distributed amongst every person within a society). It is therefore a measure of absolute economic inequality.

However, the GINI coefficient does not help researchers understand much about how income (or any non-economic measure of inequality) is distributed between defined groups of people within a given country (Stewart et al. 2010). This can be problematic if it is intergroup inequality, or non-economic inequality, that is more relevant to conflict. The GINI coefficient is a measure of *vertical inequality*, or total inequality between individuals and/or households in a society, regardless of their cultural group affiliation (Stewart 2000). It is not, however, an accurate, or even very well-correlated, measure of *horizontal inequality* (Stewart et al. 2010), or relative inequality between two culturally-defined groups of people (Stewart 2008). This is notable because horizontal inequality seems to bear a different relationship with conflict in the literature. Studies comparing vertical and horizontal inequality's effect on conflict have overwhelmingly found vertical inequality to be uncorrelated with and non-predictive of conflict, and horizontal inequality to be correlated with and predictive of conflict (e.g., Cramer 2003; Mancini 2008; Ostby 2008; Cederman et al. 2013; EPDC 2015). Indeed, Gubler and Selway (2012) find that, when inequality is horizontal, rather than vertical, rebel groups are *twelve times* more likely to successfully recruit members.

It should be noted that horizontal inequality concerns social, economic, *and* political disparities between *culturally-defined* groups, not simply differences between economic haves and have-nots. Culturally-defined groups describe the ways that people organize and identify themselves within their societies: in the US, this often follows racial classifications, but in other societies, identity politics might more closely follow religious, linguistic, or ethnic lines (and often, these categories overlap with each other) (Gurr 1993). The specific way in which people in the society organize in-groups and out-groups (e.g., religious, racial, etc.) is not as important as the fact that such groupings exist and, often, dictate who has political power, economic opportunities, and social advantage (Gurr 1993). Measures of horizontal inequality address how well a given cultural group fares compared to others within its society. These measures are prompted from the understanding that, by merit of being members of a certain cultural group, such as a given ethnicity or religion, some people enjoy greater access to economic, political, and social benefits. Likewise, by merit of being members of a different cultural group, others do not.

Distinguishing between “vertical,” or total inequality, and “horizontal,” or between-group inequality, is not a completely novel concept in criminology⁷. It is also not simply an issue of measurement or semantics; rather, the two are conceptually different. Vertical inequality is the absolute inequality within a society (i.e., how far the most well-off household or individual is from the worst-off household or individual), while horizontal inequality is the relative inequality between groups in a society (i.e., how one cultural group is positioned as compared to another cultural group, on average). Therefore, the two are not simply alternate ways of measuring the same concept. While horizontal inequality statistics can be decomposed from vertical inequality

⁷ Stolzenberg et al. (2016), for example, study the effects of interracial versus total inequality on violent crime. They find that interracial inequality is predictive of violent crime, while total inequality does not have a strong association with offending rates.

statistics (and thus the two can have some correlation), a society's level of vertical inequality is not necessarily indicative of how much horizontal inequality there is (Stewart 2000). In fact, as Stewart et al. (2010) find, the level of vertical inequality in a society is not a reliable indicator of horizontal inequality, and the two are not strongly correlated.

To further observe the difference between vertical and horizontal inequality, it is worth considering examples demonstrating that the levels of vertical and horizontal inequality present in a society do not necessarily reflect each other. A society can have a large gap between its best-off and worst-off individuals, indicating a high level of vertical inequality, but the structural differences between groups can be relatively small, indicating a low level of horizontal inequality. This can be seen at times in Kenya, where elites from various ethnic groups might relate more to each other than worse-off members of their own ethnic groups (Stewart 2000). A society can also appear to be fairly equal in terms of its absolute, or vertical, inequality, but show strong cleavages across group lines where inequality exists, thus indicating a high level of horizontal inequality. This can be seen in Nepal, where vertical inequality indicators suggest that the society is fairly equal, while horizontal inequality indicators suggest large between-group differences (Tiwari 2010). Then, there are societies with high rates of absolute *and* relative inequality, such as Nigeria, where there are large overall disparities and significant disparities between cultural groups (Kuznar 2019). It is also possible for societies to have low rates of absolute and relative inequality, in which the total amount of inequality is very small and there are no systematic differences between groups. While there are societies with comparatively low vertical inequality, there are no societies without systematic differences between cultural groups. Scandinavian countries, for instance, which are often known for their large welfare states and might arguably provide the fairest example of this, still face clear divisions along ethnic and

immigrant lines for inequalities that exist socially, economically, and politically (Larsen 2011; Galloway et al. 2015; Mock-Munoz de Luna et al. 2019).

Graphical depictions of these possible combinations can be found below in Figure 1. Figure 1 provides example depictions of a society with 1) high vertical inequality but low horizontal inequality, 2) high vertical inequality and high horizontal inequality, 3) low vertical inequality and low horizontal inequality, and 4) low vertical inequality but high horizontal inequality. These depictions are simply examples to illustrate that vertical and horizontal inequality are not perfectly reflective of each other nor necessarily strongly correlated. While in Figure 1, the example variable measured is income in thousands, this choice is arbitrary and was made for the sake of the example. Literacy rates, unemployment rates, or any other measure could have been chosen. In box plots, the shaded box depicts the middle 50% of the observations, the lower “whisker” depicts the bottom 25%, and the upper “whisker” depicts the upper 25%. The line in the middle of the box is the 50th percentile. In societies with high vertical inequality, the distance from the lower and upper whiskers are considerably far, irrespective of how cultural groups compare to each other on average. In societies with high horizontal inequality, the better-off and worse-off people in the population (in this example, the wealthier 50% and the poorer 50%) are clearly divided along group lines. This is true regardless of whether there is much absolute (i.e., vertical) inequality. Likewise, in societies with low vertical inequality, there is not much distance between the wealthiest and poorest groups of people, regardless of whether any existing differences follow group lines. In societies with low horizontal inequality, groups do not fare much differently than each other on average, regardless of whether there is much absolute inequality present.

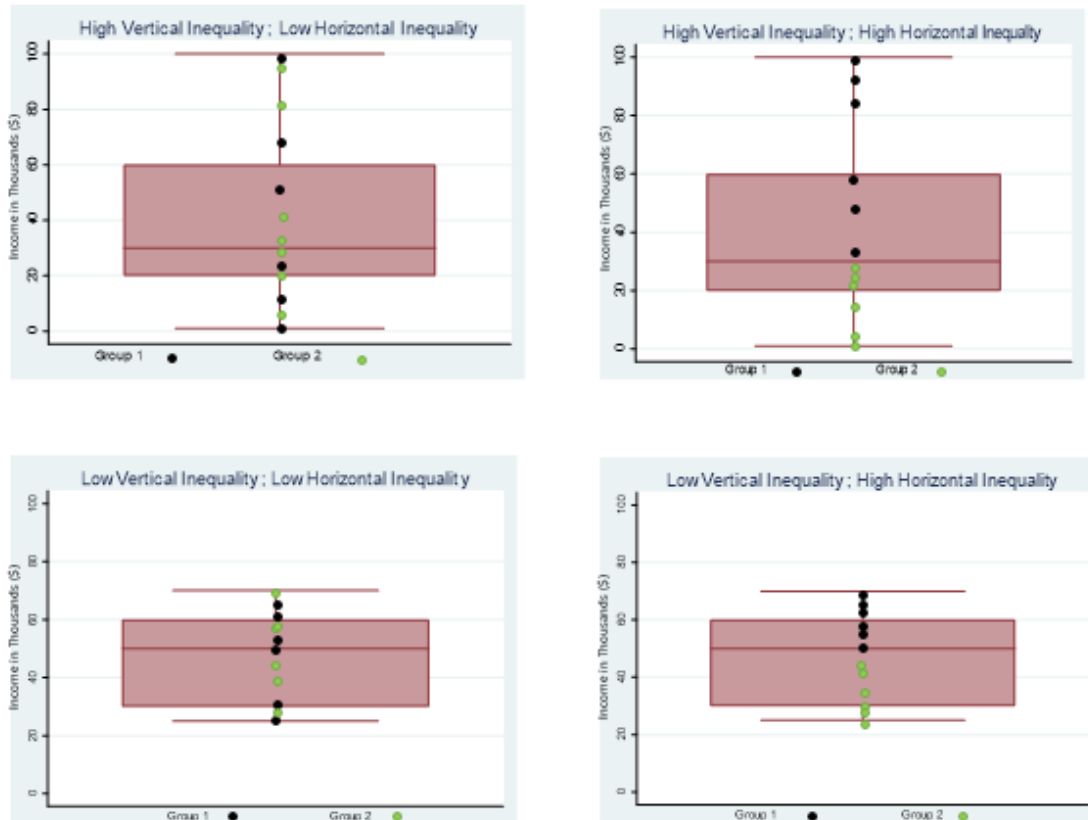


Figure 1: Illustration of Vertical (Total) versus Horizontal (Intergroup) Inequality

Stewart (2000) applied the idea of horizontal inequality to conflict. She and other subsequent authors (e.g., Stewart 2002; Stewart 2008; Ostby 2008; Cederman et al. 2013) explored the relationship between horizontal inequality and conflict, ultimately producing results that differed from those of previous scholars who measured vertical inequality. Rather than those scholars' null findings, Stewart (2000) and others measuring horizontal inequality (e.g., Ostby 2008; Mancini 2008; Cederman et al. 2013) found horizontal inequality to positively correlate with conflict. Likewise, Stewart et al. (2010) do not find vertical and horizontal inequality to be strongly correlated with each other. Murshed and Gates (2005) also note this lack of correlation in the case of Nepal, in which the GINI coefficient indicates a fairly equal society, as opposed to measures of grouped inequality, which indicate a much less equal country. From this literature, it appears, the greater the disparities between two collective groups, the more likely conflict is to

occur. This further suggests that differentiating inequality by type (i.e., horizontal versus vertical) is a worthwhile exercise.

Recognizing inequalities as “vertical” or “horizontal,” however, is not the only distinction that is relevant for understanding how inequality relates to conflict. It is also important to recognize the *types* of inequality that exist. These can be economic, such as in the case of the income measure historically used to capture inequality, or they can be social or political (Stewart 2000)⁸. This delineation is important, because how people perceive their status in comparison to others might depend on the kinds of inequality present within their society. That is, a social inequality that clearly goes along group lines, such as one group being barred from schools or healthcare institutions, might be more instigative than general differences in income that seem to indiscriminately affect people across a society. If it is important to recognize inequalities as horizontal or vertical, and also as social, political, or economic, then the GINI coefficient (a measure of vertical, economic inequality) is a limited capture of inequality on two fronts. To this point, Cramer (2003) argues that a measure of solely vertical, solely economic inequality is ill-prepared to capture the breadth of inequality and its effects.

Scholars, therefore, have studied not only horizontal inequality, but social horizontal inequality, economic horizontal inequality, and/or political horizontal inequality. Doing so has changed how we understand how inequality relates to conflict. One notable contribution is Ostby’s 2008 paper, which is the first large-N study to examine the relationship between civil conflict and empirical measures of horizontal inequality. Ostby (2008) finds that horizontal social inequality might be especially relevant to conflict onset. This could be because social or

⁸ Some scholars would also classify cultural inequalities as an independent analytical and theoretical kind of inequality (Stewart 2002). However, cultural inequalities could also be arguably encompassed within social inequalities. For the purposes of this paper, I define inequalities as social, economic, or political, and offer that cultural inequalities are a facet of social ones.

political inequalities across groups, such as disparities in access to education or the ability to participate in political proceedings, are more likely to have explicit *de jure* support (and more clearly so) than economic inequalities, such as differences in average income (Stewart 2008; Ostby 2008). Similarly, a commonly-shared group identity could serve as a rallying tool, unifying individuals and facilitating mobilization when inequality is present along this group line (Gurr 1993; Stewart 2008).

Relatedly, Cederman et al. (2013) test the ability of political and economic horizontal inequalities to predict conflict. They find political and economic horizontal inequalities both predict conflict considerably better than political and economic vertical inequalities. Further, they find political horizontal inequality to be a particularly strong predictor of conflict. Their work corroborates the above literature, and their observation regarding the importance of political horizontal inequality also contributes to the notion that *de jure* inequity could be particularly relevant to conflict. Cederman et al.'s (2013) paper is also important because of its implementation of horizontal political and economic inequality in an out-of-sample predictive model. Some analyses that have identified variables as important to conflict do not perform well when actually predicting it (Ward et al. 2013), so the ability of political and economic horizontal inequality to predict conflict is notable.

The above studies, while not definitive, offer evidence for the idea that horizontal inequality has the potential to inform whether mass violent conflict will occur. However, despite these advancements, there remain several gaps in our understanding of how inequality relates to conflict and why. First, several correlational analyses of horizontal inequality and conflict rely heavily on mathematically-generated missing data (e.g., Ostby 2008; EPDC 2015). This data is produced by taking assumptions that the authors make about what missing data should look like

and using them to calculate what missing data would be, if those assumptions are true. For example, Ostby (2008) assumes that inequality increases linearly over time in countries where there are two or more observed data points, but that inequality does not change over time in countries where there is only one observation. Assumptions such as these have previously been used to impute missing data in cases where it is unavailable, but needed to increase sample size and reduce type II error.

Correlational analyses that rely on imputed data can be informative, particularly as a literature is developing, but it is difficult to confidently establish unbiased, generalizable relationships from them (Kang 2013). This is because it is unclear from such analyses whether the detected effect is a result of the assumed nature of the imputed data. It is also unclear to what extent coefficients overestimate or underestimate relationships based on the imputed data, and whether detected effects can be taken at face value and generalized beyond the sample. Therefore, a cross-country analysis that exclusively relies on data that has been observed would be advantageous to the development of the literature.

Second, authors in the literature so far have typically used only one indicator of horizontal social or economic inequality to measure either. This could be because of 1) the preliminary nature of many studies on horizontal inequality and conflict, 2) the many data restrictions across countries on indicators relevant to horizontal inequality and group breakdowns (e.g., ethnic or religious breakdown, etc.), or 3) both. Some countries, particularly those with a history of civil conflict or an interest in censoring certain forms of data, restrict or outright ban data collection on indicators such as ethnicity breakdowns (see Canelas and Gisselquist 2018 for a fuller discussion). Thus, accessing sufficient data across enough countries for an analysis is a challenge. This can clearly make obtaining uniform data across countries of interest difficult,

subsequently reducing either the amount of possible variables that can be implemented in an analysis or severely compromising sample size. Often, therefore, particularly in cross-country studies, researchers use a single indicator for horizontal inequality. For example, they might include only educational attainment as the measure of social inequality, or only income as the measure of economic inequality.

The lack of comprehensive, cross-country measures of horizontal inequality is understandable given the above context, but it is not without consequence. The single indicators included in previous studies are not the only factors that contribute to economic or social power, and measuring them exclusively does not offer a complete picture of what horizontal inequality looks like on the ground. This is important, because the way a variable is measured can significantly alter the estimations of a given model (Chevitsky 1991; Kreutz 2010).

Implementing a more comprehensive measure of social and economic horizontal inequality should provide a more accurate picture of what grouped inequality looks like in a given society. Studying the relationship between these more comprehensive measures and conflict could also potentially provide different results from those noted in previous studies, and change what we understand the relationship between inequality and conflict to be.

The third gap is that tertiary aspects of conflict (e.g., which groups engage in conflict, conflict intensity, etc.) are understudied throughout the literature. Current research largely features studies in which the research question is whether horizontal inequality causes initial conflict onset. However, if horizontal inequality causes conflict initially, it is reasonable to hypothesize that it also affects the nature of conflict, such as whether a conflict occurs in any given year, or which groups engage in a conflict when it takes place. A study exploring horizontal inequality's effects on factors such as these, therefore, would be contributive to our

understanding of horizontal inequality's impact on not only conflict onset, but also conflict dynamics.

Fourth, it appears that, even if inequality is a compelling influencer of conflict, it does not operate in a vacuum. Other factors (e.g., governmental repression, collective framing, etc.) can and do work alongside inequality to encourage or discourage conflict. Inequality can potentially inform why some societies, rather than others, experience violent conflict. It might also inform why a given society has periods of conflict and periods of peace. However, inequality often changes incrementally over time, particularly when compared to violent outbreaks, which can quickly escalate and change in intensity. Relatedly, there are societies, such as the U.S., with considerable inequality, but that do not experience extended periods of civil conflict, as defined in this paper and the larger literature (i.e., a contested incompatibility between two armed groups, one of which is the state, resulting in 25 or more deaths within a calendar year). These issues suggest that other factors (e.g., governmental repression, collective framing, etc.) work alongside inequality to determine when a conflict occurs, and that inequality might be necessary, though insufficient, to explaining why conflicts develop how, when, and where they do. Examining confluent factors would help researchers better understand these outcomes. All of these gaps within the literature on inequality will have to be addressed before theory on inequality and conflict can be more complete.

2.3 Inequality in Practice: Mediating Factors and Other Causes of Conflict

Inequality might be a contributing factor to conflict, but it does not function unilaterally. In practice, it appears, horizontal inequality could be a necessary but insufficient condition for many civil conflicts. Other variables seem to work alongside inequality to either encourage or prevent it from escalating into violent conflicts. Thus, while horizontal inequality appears to be a

possible impetus for violent conflict, outside factors might still need to be “ideal” for it to occur. Specifically, groups that would be in conflict need to frame the society’s inequality in a way that begets violence as a response. Then, even after this framing has occurred, people need to have the capacity to mobilize and engage in conflict. This is largely dependent upon their society’s governmental structure, which must be sufficiently repressive to disallow legal means of rebellion (e.g., peaceful protest; voting in free and fair elections), but also sufficiently open to allow for groups to organize and mobilize (Gurr 1993; Muller 1985).

Some of these additional factors, which appear to mediate the effect of horizontal inequality on conflict, are considerably understudied. There are few studies in which authors directly relate collective framing (i.e., the process by which a collective group employs a common narrative to explain a given situation) to mass civil conflict (but see Gamson 1995, Oberschall 2002, and Hagan and Raymond-Richmond 2008 for examples), and even fewer data with which to study it. Likewise, more work on how governmental structure affects the horizontal inequality-conflict nexus would be useful. Works on this topic such as those of Gurr (1993) have been instrumental in bettering scholars’ understandings of these matters; however, there are still further opportunities for exploration.

2.3.1 Collective Framing

One factor that researchers have posited is important to the development of conflict is *collective framing*, popularized by Snow et al. (1986) to describe the bridge connecting people’s perceptions to their participation in a given movement. *Collective framing* is the phenomenon by which a collective, or a defined group of people, employs a common narrative to explain a given circumstance. In the context of violent conflict, collective framing is often implemented by a group to frame some sort of grievance – real or perceived – as the fault of a given entity. The

frame is constructed in such a way that it justifies conflict, such as through seeing the group as under attack or without other, more peaceful options.

Several authors (e.g., Hagan and Rymond-Richmond 2008; Oberschall 2002; Gamson 1995) have described collective framing as the process by which groups of everyday people rationalize participating in mass violent conflict. To do so, they often use case studies and qualitative data to examine the phenomenon, describing instances in which groups of people use common narratives to explain why they experience a given grievance. Notably, their work recognizes the importance of either real or perceived grievances. Where many researchers in the inequality literature measure objective inequality, authors of collective framing pieces often explicitly articulate the importance of *perceiving* inequality. To quote Gamson (1995),

Emotions other than indignation can be stimulated by perceived inequities...[However,] an injustice frame calls attention to a group of motivated human actors who carry some of the onus for bringing about harm and suffering. By defining a “they” who are responsible and can change things, adversarial frames supply the target for indignation and action in a way that aggregate frames cannot.

From a collective framing perspective, after a group experiences what they perceive to be an injustice of some sort, they frame it to make sense of why it has happened. In the context of conflict, this frame often attributes blame to a given entity, be it a government or another body. The aggrieved group sees the outside entity as having caused an unjust scenario, and often uses narratives of injustice or of being under attack to rationalize a response, which is frequently framed as self-defensive in nature or the only option left⁹.

Hagan and Rymond-Richmond (2008) document a compelling instance of collective framing giving way to genocide in their case study of Darfur in the early 2000s. While genocide

⁹ This is conceptually relevant to the notion in criminology of neutralization (Sykes and Matza 1957) and/or excuses and justifications (Scott and Lyman 1968).

often involves victimizing an unarmed group, constituting an act of violence against civilians rather than armed conflict as defined in this paper and much of the literature, the Darfur case is still useful for understanding how people can be motivated to engage in violence in response to perceived inequality. In the Darfur case, Sudanese Arabs, who were traditionally nomadic, and Sudanese blacks, who were traditionally settled and in control of water resources, had previously cooperated in distributing water (Hagan and Rymond-Richmond 2008). However, when the climate became drier and water sources depleted, narratives describing Sudanese blacks as “greedy” spread and were promulgated by the Sudanese Arab-led government (Hagan and Rymond-Richmond 2008). Formerly underdeveloped constructions of race became solidified, and with them stereotypes of Sudanese blacks as venal, aggressive, and antagonistic (Hagan and Rymond-Richmond 2008). These narratives then supported frames that Sudanese blacks were unfairly targeting Sudanese Arabs via resource distribution. Frames of injustice, victimization, and a need for self-defense cropped up and were popularized in everyday life, such as in marketplace squares (Hagan and Rymond-Richmond 2008). As these depictions became more entrenched, the water issue was framed as an attack on Sudanese Arab populations, which demanded a response in self-defense. The genocide began soon thereafter.

The Darfur case is useful because it illustrates several commonly-employed frames. It documents how collective frames can both build upon and encourage stereotyping, how violence can arise out of either real or perceived inequalities, and how frames can be used to attribute blame and imply the necessity of violence as a response. The case also demonstrates how important sentiments of injustice and persecution are for instigating violent responses. As Gamson (1995) states, “Only an injustice frame...taps the righteous anger that puts fire in the

belly and iron in the soul.” The Darfur case alludes to the importance of collectively framing group disparities to encourage mass violence.

The role collective framing plays in conflict onset is understudied, but it has been alluded to in some of the inequality literature. Cramer (2003), for example, writes that ideological mechanisms (amongst other factors like state strength¹⁰) are important for a society to transition from inequality into violent conflict. Stewart (2008) also posits that perceived inequalities, just like objective ones, can lead to conflict onset. In these pieces, authors imply that the way people make sense of their situation – not simply the presence of the situation in and of itself – is vital for conflict onset.

Cederman et al. (2011; 2013) also implicate framing as relevant to conflict onset, in addition to horizontal inequality. In their 2011 paper, they find when groups are both much richer and much poorer than the society’s average, they are more likely to engage in conflict. This could be indicative of groups framing the need to either protect their position in society or fight perceived injustices. Cederman et al. (2013) discuss several conditions under which they posit horizontal inequality must occur for conflict to potentially ensue. They argue that, after a given inequality manifests, the aggrieved group must designate that inequality as unjust, and “frame and blame” a given entity for its occurrence (Cederman et al. 2013). These conditions, which fit the collective framing orientation, could help engender conflict.

Collective framing can help researchers approach contexts under which objectively “better-off” groups commit acts of violence. Researchers can also employ the concept to hypothesize why acts of violence break out at particular moments. While currently, the exact mechanism by which collective framing works remains speculative, it could be that collective

¹⁰ Several other authors also posit that the structure of the state plays a pivotal role in the likelihood of conflict onset. See section 2.3.2 (“Governmental Structure”) for further discussion.

frames are built up and strengthened after key rallying events (e.g., a political election; the execution of a journalist). This could reinforce a given ideology (e.g., “We are unjustly treated,” or “There is no other option.”), encourage a sense of urgency, and motivate people to act (Black 2011). It could also be that collective frames gain traction cyclically, similarly to protest cycles (see Snow and Benford 1962 for a fuller discussion of protest cycles). In this case, groups would violently or non-violently express their dissatisfaction with a given situation, become fatigued from protesting after some amount of time or after they succeed in bringing about an intervention, and then mobilize again as frames suggest that the problem is still present. While the mechanism by which collective framing works is still unclear, several researchers (e.g., Gamson 1995; Oberschall 2002; Hagan and Raymond-Richmond 2008) have noted its putative important role in rationalizing and institutionalizing mass violence, including armed conflict. More work is needed to empirically test this notion and better understand how collective framing affects conflict.

It is worth considering how collective framing feeds into conflict, given its relevance in several case studies (e.g., Gamson 1995; Oberschall 2002; Hagan and Raymond-Richmond 2008). Still, there is much researchers do not know about collective framing and conflict. A thorough search of the literature reveals there are no aggregated empirical databases on collective framing. It is therefore difficult to assess the generalizability of conclusions researchers have drawn about its impact on violent conflict. There is also very little if any guidance on how to measure collective frames to rectify this gap in the conflict literature. These and related challenges make it difficult to explicitly measure collective frames, including in this current work (see Chapter 3.5 for a fuller discussion of study limitations). However, it is still important to recognize the theoretical importance of perceived inequality and collective framing. Doing so influences the

conclusions researchers draw from analyses (including the current one). It also allows researchers to better theorize how factors that *are* explicitly measured, like inequality, relate to conflict and under which conditions.

2.3.2 Governmental Structure

An additional factor that could alter the effect of horizontal inequality and be pivotal to conflict development is the governmental civil structure of a given society. In the context of this work, the governmental *civil structure* of a society refers to how repressive it is, and how inclusive are its institutions. A society's governmental civil structure can foment conflict if it is sufficiently repressive and disallows people from engaging in legitimate political mobilization (Ferguson 2006). It can also make conflict logistically possible, as the degree of governmental repression present determines whether groups can meet and mobilize. (Gurr 1993; Muller 1985). It is therefore important to consider a society's government when thinking about the development of violence.

A government's level of repression and the development of its institutions can make conflict more appealing for potential foot soldiers. Governmental repression and weak institutions can inspire people to make demands for change (Ferguson 2006). When the government attempts to quell these outbreaks and maintain its power with further repression, it can reinforce and sustain the rebel narrative, protracting the conflict in a cycle of repression and rebellion.

Thus far, the reviewed literature has been about conditions that make people want to engage in conflict. However, it is equally important to recognize that people must be *able* to engage in conflict. To this end, governmental civil structure is paramount. Muller (1985) finds that, when governments are either extremely repressive or not very repressive at all, conflict is

less likely. Instead, he demonstrates, moderately repressive regimes are most likely to experience violent outbreaks.

The reason for this has to do with both the willingness to engage in violence (which is increased by aforementioned horizontal inequalities and collective frames), and the ability to do so. When people live under regimes that recognize and respect freedoms, such as the right to peacefully protest or to hold free and fair elections, they are less likely to violently rebel, even when they have grievances against the regime. This could be because they perceive there to be a legitimate means by which they can participate in and change the political process, even if gradually. Messner and Rosenfeld (1994) support this idea; though, they apply it to micro-level actors and recommend “blocked opportunity theory” as a framework for understanding this phenomenon. They too write that, when individuals perceive there to be legitimate opportunities to access freedoms and pursue change, they are less likely to implement illegitimate means to do so. However, when there are no legitimate avenues for change, people pursue those which are illegitimate. This could help explain why non-repressive regimes lessen the effect of inequality on conflict.

On the opposite extreme, under highly repressive regimes, people are still less likely to rebel. Muller (1985) posits that this is because, even if they want to do so, extreme security measures and punishments set in place by the state render mobilization infeasible. In high-security societies, such as North Korea, for example, where organization is instantly (and often violently) quelled and people are encouraged to police each other, mobilization is far more dangerous. The high risk of mobilization disincentivizes individuals from doing so. Instead, it seems individuals are most likely to engage in violence under moderately repressive regimes, in which people do not have enough access to freedoms to effect change, but can still find ways to

meet with others and organize. This idea is supported by Acemoglu and Robinson (2013), who caution that, when regimes offer some access to freedoms (e.g., economic freedoms to access markets), but not others (e.g., political freedoms to engage in free and fair elections), instability and conflict are more likely.

Muller's (1985) and Gurr's (1993) findings are corroborated by other literature in which authors find that weak governmental institutions encourage conflict. Braithwaite et al. (2010), Collier and Hoeffler (2004), and Fearon and Laitin (2003) all speak to the relevance of poorly-developed governmental institutions, such as weak rule of law, on conflict. Through their use of case studies and statistical analyses, they all ultimately find that weak governmental institutions (and, in particular, weak rule of law) facilitate conflict by making its consequences less risky to participants and making conflict more feasible.

It should be noted that, over the years, a government can change its level of repression. That is, at some points in time, for example, a government might engage moderately repressive tactics to quell rebellion, whereas at other points, it might employ more repressive tactics to do so. It is important to remember in these discussions that governments can change the amount of repression they employ, and this can be subsequently change the propensity for conflict.

It is important to consider the qualities of a given society's government when examining how and when inequality causes conflict. Certain governmental civil structures render conflict both appealing and opportune. Conversely, the way some governments are organized makes conflict much more challenging, even when people might otherwise choose to engage in it. Authors analyzing the relationship between horizontal inequality and conflict, therefore, should assess a government's level of repression and the strength of its institutions, recognizing their potential to deter conflict, even in the face of inequality.

2.4 Why Violence? Why Here? Why Now?

The above research provides a guide for thinking about why conflicts happen, and when and where they take place. Across the literature, authors identify a host of variables that might factor into whether mass violent conflict occurs in a given society, and allude to some potential methods for prevention. The implications are notable. In societies with real or perceived horizontal inequality, with collective frames that assign a label of injustice to that inequality and attribute fault to a given entity, and with repressive enough governments to limit freedoms but still allow for mobilization, we can expect to see higher rates of mass violent conflict.

Importantly, in practice, none of the above factors operates in isolation. They all are important factors in determining conflict and its dynamics. Inequality seems to give way to societal grievances that demand to be addressed, particularly when it is present between groups and, potentially, as social or political inequality. Repressive or exploitative governmental civil structures might make conflict more or less feasible, depending on the level of repression. Collective frames can make sense of inequality and repression in ways that impact how the conflict develops. Thus, these factors are all important to consider when thinking about why a society experiences conflict. Recognizing the broad array of variables influencing conflict dynamics can help researchers better examine conflicts and think about how they might develop in the real world.

Relevantly, while these explanations might help researchers assess where, why, and when some conflicts break out, they might not work as explanations of why any given individual actor engages in conflict. As Matsueda (2017) notes, micro- and macro-level factors can cause each other. Still, even if horizontal inequalities and frames of injustice or self-defense motivate many individuals to engage in conflict, others could involve themselves in a given conflict for different

reasons, such as thrill, money, or identity. Instances of this have been described by Dallaire (2010), Braithwaite et al. (2010), Hagan and Rymond-Richmond (2008), and Collier and Hoeffler (2004). It is important to recognize that I focus on how the theorized key influencers discussed in this chapter affect actors at the macro-level. At the individual level, it is difficult within current research to clearly distinguish between someone who is motivated by, for example, income inequality, social isolation, or some other grievance, versus someone who is motivated by resources, identity, or some other benefit, since the latter motivations could manifest in similar ways and with similar propaganda as the former. It is also possible that individual motivations change as conflicts develop¹¹. I (and many researchers in this area of work) am interested in the causes of conflict at the macro-level, rather than as a micro-level decision-making process. These analyses use macro-level variables to understand macro-level outcomes, making up one of the possible mechanisms by which researchers can study macro-level phenomena (Coleman 1990). Despite the complexities of individual decision-making, the factors I discuss in this literature review are useful for thinking about the determinants of conflict at the societal level. They provide a lens through which scholars can examine cases of violence and non-violence. They may also be useful for theorizing about causes of conflict at the individual level, but that would require different forms of data to verify.

2.5 The Research Gaps

Both policymakers and researchers have a compelling interest in understanding, predicting, and mitigating conflict. Still, there are several research gaps in theory, data, methods, policy implications, and policy implementation in peace and conflict studies (Cederman and

¹¹ Possibilities such as this are, in part, why some discussions of how to instill peace post-conflict differ from discussions about what initially started the conflict (Braithwaite et al. 2010). It is also why, in some conflicts, it can become unclear even to warring parties what is motivating particular groups (Powell 2015).

Weidmann 2017; Hegre et al. 2017) that must be addressed. While researchers' efforts described above have made considerable headway in making sense of the relationship between inequality and conflict, the scientific community should neither claim full confidence in nor full understanding of this relationship. The data limitations permeating the field are acute, and, in many cases, persistent. Conceptual gaps still exist, and further study is required to deepen researchers' understandings of the manner in which inequality affects conflict dynamics. Addressing these limitations will help make studies on horizontal inequality and conflict more compelling. In this dissertation, I address several of these gaps.

2.5.1 Limited Conceptualizations of How Inequality Affects Conflict Dynamics

Perhaps as a result of poor data quality, limited measures of horizontal inequality, and the literature's relative newness, there is a lack of definitive, generalizable evidence on if and how horizontal inequality affects 1) conflict, or 2) the dynamics of conflict. It is unclear whether social, economic, and political horizontal inequality each have the same effect on conflict, or if their effects systematically differ. It is also unclear whether horizontal inequality between any groups causes conflict, or if horizontal inequality is only consequential between certain groups (e.g., the largest groups, the best- and worst-off groups, etc.), as has been discussed in passing¹² within the literature (Ostby 2008; Cederman et al. 2011). It is also unclear whether horizontal inequality simply causes conflict (assuming it does), or whether characteristics of the inequality also affect the nature of the conflict (e.g., who is involved in the conflict, how intense it is, etc.).

¹² Ostby (2008) examines differences between the two largest groups in a given society, positing that such differences are most likely to be consequential. Cederman et al. (2011) find that the richest and poorest groups in a society are both likely to engage in fighting where there is horizontal inequality, allowing for the suggestion that these two groups' differences could be particularly relevant to conflict. However, there is little systematic study of which groups' differences are most important to conflict, if any groups are special at all.

If researchers better understood whether and how the effects of different types of horizontal inequality vary, they could better identify situations that are most at-risk of escalating into violence, and tailor policies to be more effective. As well, if there were better evidence regarding whether and how horizontal inequality relates to conflict dynamics, policymakers would be better-positioned to focus attention on preventing and mitigating the most urgent situations.

I address some of these gaps in current conceptualizations of the relationship between horizontal inequality and violent conflict. I do so by incorporating social, economic, and political inequality into cross-country analyses; analyzing every relevant societal group's relative inequality; and examining the relationship between horizontal inequality and two conflict-related outcomes (conflict occurrence and group involvement in conflict). I also rely on existing data to limit the amount of methodological assumptions related to missing data, and implement more comprehensive measures of horizontal inequality than have been used in previous cross-country analyses.

2.5.2 Compelling Results with Questionable Data

Due to low sample size and missing data, some previous researchers have had to interpolate missing data on horizontal inequality statistics (e.g., Ostby 2008; EPDC 2015). These generated data were then used to estimate the relationship between horizontal inequality and conflict. While this is a useful route for examining difficult-to-study phenomena, it places limitations on what researchers should confidently assert is known about horizontal inequality. To this point, Ostby (2008) notes that, while her results suggest researchers should refrain from

considering the inequality-conflict relationship null¹³, her data have shortcomings and “must be regarded as preliminary” (Ostby 2008). Interpolated data requires considerable assumptions about how inequality between groups changes over time. For instance, the interpolation might be based on the assumption that horizontal inequality is constant, or that it changes linearly over time (Ostby 2008).

Researchers do not have enough information with which to confidently make such assumptions. As has been noted in the literature, we lack a clear understanding of what inequality between groups looks like on the ground (Canelas and Gisselquist 2018). This is especially true in the context of speculating about how inequality across groups changes over time and in different countries. Generated data are grounded in these kinds of assumptions.

The issues arising from generated data are particularly notable because, when analyses are replicated with the original data, many of the results do not hold (Ostby 2008; EPDC 2015). Given qualitative work demonstrating a relationship between horizontal inequality and conflict in several cases (e.g., Stewart 2002; Stewart 2008; Tiwari 2010), it is reasonable to speculate, as have the above authors, that these non-findings stem from poor power due to low sample size. However, researchers cannot rule out the possibility that data constructed using debatable assumptions could have generated biased findings. Results could inaccurately describe the substantive strength of the relationship, the significance of the relationship, or the direction of the relationship. Findings emerging from these data, therefore, cannot be considered definitive, despite the fact that such findings have driven subsequent research publications and policy briefs.

¹³ Ostby’s (2008) paper comes at a time in which much of the political science literature is disqualifying the relationship between inequality and conflict, due in large part, many scholars would argue, to the literature’s one-dimensional conceptualization and operationalization of inequality.

In some of these products, horizontal inequality is often described as definitively causing conflict, and policy recommendations are issued based off of this understanding.

In part because of these data limitations, it is important to continue exploring the relationship between horizontal inequality and conflict dynamics with varied data and methodological strategies to corroborate and build upon our scientific understanding of the topic. In the current work, I use available census data to study the relationships of interest. I also use cultural groups as the unit of analysis, helping to extend sample size without having to estimate missing data values (see Chapter 3, “Methodology,” for a fuller explanation). While these steps do not resolve the issue of limited data or small sample sizes, they offer another way to analyze the relationship between horizontal inequality and conflict dynamics. These perspectives, in combination with previous studies’, can help produce a fuller understanding of the relationship.

2.5.3 Measuring Horizontal Inequality

Researchers’ understanding of horizontal inequality has been adversely affected by their limited ability to measure it. In some countries, there are considerable restrictions on data access (Canelas and Gisselquist 2018). Concerns about reigniting conflicts, interests in maintaining a given power structure, and logistical barriers lessen how much data social scientists can acquire for analysis (Canelas and Gisselquist 2018). For example, in a society where there has historically been violence related to ethnic cleavages, the government might restrict access to data. Power allocation can be another concern: to prevent potential documentation of unequal power structures and lessen the likelihood of even peaceful movements to change that structure, powerbrokers might block access to data. Finally, logistical barriers can hinder data collection efforts. Researchers might not be able to access geographically- or linguistically-isolated groups, even when they would otherwise be allowed to do so. Canelas and Gisselquist (2018) offer

examples of when these obstacles have prevented researchers from adequately collecting horizontal inequality measures, such as in the examples of Tanzania or Rwanda, where governments have tried to encourage identification with national identities as opposed to ethnic ones.

The result of these obstacles has been that researchers who want to quantitatively analyze horizontal inequality's relationship with conflict have had to rely on data sources that were not collected for the purposes of measuring horizontal inequality. Because horizontal inequality studies were not their intended purpose, these data sources have fewer variables of interest. A general health survey, for example, which might be useful for studying grouped inequality on health outcomes, might not be useful for studying the effect of political or economic horizontal inequality. Readily available data also might not allow for optimal operationalization of certain variables. For instance, if a census only collects summary birth history (i.e., how many children a mother has ever had and how many are currently alive), calculating infant or child mortality rates for each relevant group could prove difficult. Data sources adapted for the study of horizontal inequality can also be limited in their applicability for cross-country analyses. For example, if census data is used to assess group differences, countries can only be included in the same analyses if both countries measure the same outcomes. If one country, however, collects household incomes and another does not, household incomes cannot be used for an analysis that features both countries.

Relatedly, while there are suggested practices for calculating inequality measures between groups (e.g., Stewart et al. 2010), none, to my knowledge, suggest practices regarding which indicators to include. For example, to measure economic inequality between groups, some authors have used differences in income, whereas others have used differences in ownership of

domestic assets (e.g., Ostby 2008; Cederman et al. 2013). Relatedly, researchers have often only used a single indicator to represent economic or social inequality comprehensively. This practice, however, risks depicting an incomplete picture of what inequality looks like on the ground.

Such restrictions have created a context in which there is limited data on horizontal inequality. There is consequently an incomplete understanding of its relationship with conflict. To address this, researchers would be well-served to reimagine currently available data into more comprehensive measures of horizontal inequality. This would mean including additional indicators of horizontal inequality into their analyses to help more accurately reflect what inequality looks like in reality.

It is possible for researchers to employ currently available data to calculate more comprehensive measures of horizontal inequality. I attempt to fill this gap in this dissertation by including several indicators of social and economic horizontal inequality, as opposed to a single one (see Chapter 3, “Methodology,” for a fuller discussion). Scholars such as Kreutz (2010) and Chesher (1991) have noted that the way in which a variable is measured can dramatically impact estimations of an effect. Implementing more comprehensive measures, therefore, could potentially change how we estimate and understand horizontal inequality’s effect on conflict dynamics.

Even though data is restricted to the countries that make it accessible, analyzing even just these countries can provide a foundation from which we can theorize about how horizontal inequality might affect conflict dynamics generally. Having different perspectives on and tests of a given concept provides more well-rounded evidence on how it relates to other variables, particularly when data is limited. Therefore, implementing more comprehensive measures of

horizontal inequality in a cross-country analysis of its effect on violent conflict dynamics would still improve our understanding of inequality and conflict.

There are several challenges to scholars' understanding of horizontal inequality and conflict. In this dissertation, I address several of them. I examine the effects of political, social, and economic horizontal inequality on two dependent variables, conflict occurrence and group involvement in conflict. I incorporate more comprehensive measures of horizontal inequality than have been used in previous cross-country analyses, and I rely exclusively on existing data to generate my results. By taking these steps, I am able to describe what the relationship between horizontal inequality and conflict looks like in cases where the two have been explicitly observed, inform theory on whether horizontal inequality affects not only conflict onset but also conflict dynamics, and contribute to the broader discussion of what causes violent conflict.

CHAPTER III: METHODOLOGY

3.1 Research Questions and Hypotheses

As research on conflict determinants has developed, scholars (e.g., Gurr 1993; Stewart 2002; Cederman et al. 2011) have suggested that inequality between culturally-defined groups could be particularly relevant to the onset of conflict. Researchers, therefore, have begun to study this particular manifestation of inequality and its relationship with conflict. Stewart (2008), Ostby (2008), and Cederman et al. (2011) have suggested that the effect of intergroup inequality might also change depending upon whether it is manifested as economic, social, and political inequality; therefore, it is important to study the effects of all of these.

Because of the youth of the literature and field-wide data limitations, it is useful to continue to study the relationship between horizontal inequality and conflict. It is also worthwhile to expand this area of research to more thoroughly consider the effects of horizontal inequality on the dynamics of conflicts, such as which groups become involved in conflict. To begin exploring this path, I employ census data to create several indicators each of social and economic horizontal inequality. I examine these indicators' effects on both whether conflict occurs in any given year and which groups become involved in that conflict. I do this to 1) provide evidence for whether, given existing data, there appears to be a correlation between different kinds of horizontal inequality and these conflict dynamics, 2) explore the possibility that horizontal inequality affects the nature of a conflict (e.g., which groups become involved), and 3) examine whether there appear to be differences in correlations depending upon the type of horizontal inequality present within a society and which indicators are used to examine it.

Researchers have incorporated very limited captures of horizontal inequality into cross-country analyses, and have not, to my knowledge, included political, economic, and social

horizontal inequality indicators into one cross-country analysis to note any differences in effect. There are also relatively few studies whose results are generated from unaltered data. Because of these limitations, it remains unclear whether the purported effects of horizontal inequality change depending on the nature of that inequality, or how strong the relationship between horizontal inequality and conflict is generally. Researchers have also not yet examined whether and how experiences of inequality affect which groups become involved in conflict. Finally, there is not a clear understanding of how other potential variables might work alongside horizontal inequality to influence conflict and its dynamics.

To address some of these gaps, I incorporate different, underutilized measures of horizontal inequality to analyze the relationship between political, social, and economic horizontal inequality and conflict dynamics. I also examine an understudied dependent variable, group involvement. Doing so helps determine 1) whether there is a relationship between horizontal inequality and conflict dynamics besides its initial onset, and 2) whether who gets involved in conflict is one of those dynamics. In these ways, I expand the knowledge-base on horizontal inequality and its subsequent effects on conflict dynamics.

To this end, I address two central research questions:

- 1) What is the extent to which social, and economic, and political horizontal inequality correlate with violent conflict?
- 2) What is the extent to which social, and economic, and political horizontal inequality correlate with group involvement in violent conflict?

These questions and the methods through which I address them differ from former studies in several ways. By incorporating “group involvement” as a dependent variable, I assess how horizontal inequality might affect how a conflict develops. By incorporating more

comprehensive measures of social and economic horizontal inequality, I attain a more holistic view of how inequality looks on the ground and how it relates to conflict. Addressing these issues in this way allows for a better understanding of how each type of inequality's effect on conflict dynamics differ from the others', if at all.

Herein, I address the following hypotheses:

H₁: Political, social, and economic horizontal inequality increase the likelihood of violent conflict.

H₂: Social and political horizontal inequality increase the likelihood of violent conflict more than economic inequality.

H₃: Groups that experience the most extreme horizontal inequality are most likely to engage in conflict.

I address the above hypotheses by reimagining available data, creating new aggregate measures of horizontal inequality, and using exclusively existing data. I then discuss these findings in the larger context of the peace and conflict literature, and analyze how we can use them to advance our understanding of why conflict develops in the manner it does.

3.2 Variables and Data Sources

For my analysis, I used some existing variables and created others. These data largely come from three sources, the Uppsala Conflict Data Program (UCPD)/Peace Research Institute of Oslo (PRIO) Armed Conflict Dataset (Pettersen et al. 2019; Gleditsch et al. 2002), IPUMS-International (Minnesota Population Center 2019a), and Ethnic Power Relations (EPR) Core Dataset 2018 (Vogt et al. 2015). As I enumerate the variables of interest in this section, I note and describe any additional data sources as needed. Important to note is that the unit of analysis for this study is cultural group, by country and by year. That is, each observation in the dataset is a given group in a given country in a given year.

3.2.1 Dependent Variables

There are two key dependent variables: conflict occurrence and group involvement in conflict. For the purposes of this study, and in line with the UPCD/PRIO Armed Conflict Dataset's definition, I define *conflict* 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 twenty-five battle-related deaths in a calendar year” (Pettersen et al. 2019; Gleditsch et al. 2002).

The first dependent variable is conflict occurrence. It is the outcome variable to address hypotheses one and two (“Political, social, and economic horizontal inequality increase the likelihood of violent conflict,” and “Social and political horizontal inequality increase the likelihood of violent conflict more than economic inequality,” respectively). Conflict occurrence is indicated as a 1 for years in which a conflict occurred, and 0 for years in which no conflict occurred. This variable is taken from the UCDP/PRIO Armed Conflict Dataset Version 19.1, which is a renowned dataset tracking when conflicts occur, as well as different qualities surrounding them (e.g., intensity, duration, involved parties) (Pettersen et al. 2019; Gleditsch et al. 2002). The UCDP/PRIO Armed Conflict Dataset is established as a reliable source of conflict data, featuring few if any errors in where and when conflicts occurred (Eck 2012). Its data has been used in prior studies exploring conflict onset (e.g., Ostby 2008).

Conflict occurrence is an indication of whether conflict (a contested incompatibility between a state and non-state actor resulting in 25+ deaths in a calendar year) occurred in a given year. To gather this data, the UCDP research team searched through media sources (e.g., news reports) and non-media sources (e.g., historical archives, truth commission reports, intergovernmental organizations' reports, etc.) (DPRC 2020). They used these sources to

estimate the number of battle-related deaths in a potential conflict in countries around the world. They recorded an indication of whether the country meets at least 25 battle-related deaths in a calendar year (yes or no). While the dataset's estimates are evaluated by multiple human coders and the dataset is widely implemented in the literature (e.g., Bosetti and von Einsiedel 2015; World Bank 2020), it should be noted that these are estimates and there can be discrepancies between this dataset's and others' estimates of direct battle deaths¹⁴.

The second dependent variable is group involvement in conflict. This variable is used to address hypothesis three ("Groups that experience the most extreme horizontal inequality are most likely to engage in conflict."). This variable is also created based on information from the UPCD/PRIO Armed Conflict Dataset (Pettersen et al. 2019; Gleditsch et al. 2002). It is a dichotomous variable, indicating whether a given group is involved (1) or not involved (0) in the conflict. Including this variable helps characterize the relationship between horizontal inequality and violent conflict, indicating whether certain kinds of differences are more relevant to conflict involvement than others.

To code this variable, I looked at the UCDP/PRIO's record of which warring armed groups were primarily involved in a conflict. I then verified which cultural groups these armed parties professed to represent and fight for, and assigned those cultural groups a 1 if they were a named primary party and a 0 if not. For example, Guatemala's Guerrilla Army of the Poor (EGP) was a warring party involved in the Guatemalan conflict in 1981 (Pettersen et al. 2019; Gleditsch et al. 2002), and it was largely made up of and fighting for indigenous Guatemalans and their

¹⁴ For example, in Colombia in 1980, when the conflict was comparatively less intense, the Colombian National Center for Historical Memory records less than 25 combatant deaths for the year 1980 in Colombia (CNMH 2013). However, because there is no inequality data for this year, possible discrepancies do not affect the final analysis, even with this year coded as a "0."

rights (Lawton 2015). Thus, Guatemala’s culturally-relevant ethnic group “indigenous” received a code of 1 in 1981 in Guatemala.

3.2.2 Independent Variables – Horizontal Inequality

The key independent variables for this analysis are captures of social, economic, and political inequality between groups. Political horizontal inequality is conceptualized as the degree of unequal power relations between groups within the political system. It captures the amount of political power that a given group has. Political horizontal inequality is coded as a categorical variable, whereby a group can hold all political power (1 – constituting a “monopoly”), most political power (2 – constituting a “dominance”), a share of political power (3 – constituting either a “senior partner” or “junior partner”), or be excluded from the political system (4 – constituting either “powerless,” “discrimination,” or “self-exclusion”). This variable comes from the EPR Core dataset 2018 (Vogt et al. 2015), which captures political horizontal inequality in every country around the world and details relevant cultural groups’ access to political power. Group categorizations can change in subsequent years as the political landscape develops.

The EPR Core dataset has historically been informative to studies examining political inequality between groups. Despite the name, groups in this dataset are not exclusively “ethnic” in more colloquial terms, whereby ethnicity indicates the groups’ countries of origin. The dataset extends “ethnic” to include ethnoreligious, ethnolinguistic, and racial groups, as is culturally relevant to the society (Vogt et al. 2015). The EPR Core Dataset tracks each relevant groups’ access to power, indicating for example whether a group has a monopoly of power, occupies most executive positions, occupies a minority of executive positions, holds no political power (but without explicit discrimination), is explicitly discriminated, or self-excludes from the

political process (for instance, by claiming independence from the central government) (Vogt et al. 2015) The categorizations were determined by an expert survey of roughly one hundred regional experts and then reviewed and evaluated by the dataset's management committee (Vogt et al. 2015). This dataset has been used in former studies, like that of Cederman et al.'s (2013), to examine political horizontal inequality's relationship with civil war.

Social horizontal inequality is conceptualized as differences between groups vis-à-vis communal benefits of the society and access to public services. Economic horizontal inequality is conceptualized as differences between groups regarding financial well-being. Researchers in peace and conflict studies have measured economic and social horizontal inequality in a host of ways, and there is not a consensus on which indicators are most appropriate to implement. There is also no well-established dataset measuring social and economic horizontal inequality: authors have historically created their own indicators based off of measures they think accurately capture social or economic status. One of the gaps I seek to fill is that of historically limited captures of social and economic horizontal inequality in cross-country analyses. As Stewart et al. (2010) write, including a wide range of indicators is preferable, since horizontal inequalities can differ across indicators, and which inequalities are most pertinent to a society can differ between societies¹⁵. Therefore, to gain a more complete picture of economic and social horizontal inequality in my analysis, I incorporate several indicators of each.

I implement three measures of social horizontal inequality (i.e., group differences in child mortality rates, literacy rates, and educational attainment) and three measures of economic horizontal inequality (i.e., group differences in unemployment, access to electricity, and home ownership), all of which are detailed below. Each measure is meant to capture an aspect of social

¹⁵ For example, land equity might be more consequential in Zimbabwe than it is in China (Stewart et al. 2010).

and economic status, respectively. Because it is unclear which, if any, indicator relates to violent conflict and to what degree (particularly with unaltered data¹⁶), I retain each indicator as its own variable for the purpose of the analysis. Doing so allows me to determine whether and to what extent estimated correlations differ when different measures of horizontal inequality are employed.

To measure each indicator of social and economic horizontal inequality, I almost¹⁷ exclusively use existing census data¹⁸ from each country. This data has been compiled and harmonized by IPUMS-International (Minnesota Population Center 2019a). IPUMS is a well-established project by the Minnesota Population Center. It collects, harmonizes, and distributes data collected by various organizations and countries. IPUMS-International (intuitively, its international branch) does this for census data gathered from around the world.

Inequality along the aforementioned indicators are all operationalized in the same way: as a given group's average distance from the national average. That is, each indicator is the absolute value¹⁹ of a group's average rate subtracted from the national average's rate. Therefore, if the

¹⁶ Some researchers (e.g., Ostby 2008; EPDC 2015) have found certain measures of horizontal inequality to have a positive relationship with violent conflict when using interpolated data, but this relationship becomes unapparent with unaltered data and smaller Ns.

¹⁷ The one exception to this is Bangladesh. To calculate Bangladesh's child mortality rates by group, I used IPUMS-Demographic and Health Survey's (DHS) data (Boyle et al. 2019), since number of children born and number of children survived were not gathered through Bangladesh's census. However, even this piece of data is collected, harmonized, and distributed by IPUMS, which is the data source that did so for every other social and economic indicator.

¹⁸ Because IPUMS provides a sample of each census, I calculated variables using the final weights indicated by IPUMS, even when the sample was indicated as a simple random sample. I then verified population estimates I generated against actual populations at the time of the census to ensure that they were consistent and ensure external validity. I also verified that group demographic breakdowns in my dataset were consistent with official reports as a way of ensuring external validity.

¹⁹ While I note that the relative position of a group (i.e., whether it is better- or worse-off than the national average) might have an effect on that group's involvement in conflict, I am hypothesizing that greater horizontal inequality increases the likelihood of involvement in conflict. Therefore, as an initial step on this exploratory topic, I am testing for whether there is evidence for or against this hypothesis. Future researchers might look to additional qualities surrounding the group, like whether a group is better- or worse-off, rather than simply how far from average it is (see Chapter 5 for a fuller discussion, and Appendix C for preliminary analyses).

national average literacy rate is, for example, coded as a value of 0.5, and the group average is 0.3, then educational attainment inequality for that group is coded at 0.2 for that year.

Social horizontal inequality is conceptualized as differences between groups regarding communal benefits of the society, such as healthcare or education. It concerns disparities in societal indicators of human development and access to services, rather than economic inequality, which revolves around financial disparities, or political inequality, which revolves around access to political power and expression. As noted, it is operationalized as a given group's distance from the national average on a given measure.

To capture social horizontal inequality, I measure differences in child mortality rates, literacy rates, and educational attainment. Child mortality rates have been used as an indicator of horizontal inequality once before, where it was found to be informative to violence in Indonesia (Mancini 2008). It could provide insight as to systematic differences in health between groups, since children do not pass of natural causes. However, it has never, to my knowledge, been included in any cross-country analysis of horizontal inequality and conflict. It is typically not included in analyses on the topic, and constitutes an understudied facet of horizontal inequality that might be informative to different aspects of violent conflict.

I also measure differences in literacy rates and educational attainment. Educational attainment has been used before as a gauge of social horizontal inequality (e.g., Ostby 2008; EPDC 2015), and is appropriate because of its indication of whether, on average, one group has received more schooling than other groups. Literacy rates are useful because they likely reflect quality of schooling. For example, even if everyone completed primary school (and thus had similar values for educational attainment), some groups could still have higher literacy rates than others because of differences in quality of instruction and educational infrastructure. To obtain

educational attainment for each country, I calculated the amount of people between ages 20-29 who had completed university²⁰ over the total amount of 20-29 year olds. Relatedly, I measure group differences in literacy rates by calculating the total amount of people who affirm that they can read and write over the total amount of people. Combined, these three indicators provide a broader picture of grouped social inequalities than have been previously captured in cross-country analyses on the topic. They also serve as a gauge for overall social disparities between relevant groups within a society.

Economic horizontal inequality is conceptualized as disparities between groups that occur across financial lines, including access to the consumer market or disparities in wealth. These inequalities largely concern financial well-being. It, too, is operationalized as a given group's distance from the national average on a given economic measure (i.e., unemployment rates, access to electricity, home ownership rates).

I employed three economic indicators to measure economic horizontal inequality: differences in unemployment rates, access to electricity, and home ownership rates. When combined, these indicators can provide a more comprehensive assessment of differences in economic outcomes between relevant groups in a society. Differences in access to electricity has been included in other horizontal inequality studies as a component of disparities in ownership of domestic assets (e.g., Ostby 2008). Home ownership also provides insight into how groups differ in their systematic possession of assets. Likewise, it can reflect systematic underlying differences

²⁰ I originally calculated educational attainment in three different ways to be able to compare slightly different operationalizations' effects. Therefore, I calculated the percentage of 20-29 year olds in the national population and in each cultural group that had completed primary school, high school, and university. In each operationalization, the detected effects were similar. Thus, I used only educational attainment as the percentage of 20-29 year olds who had completed university in my final models.

in wealth. Unemployment rates are included because they provide a way to get at differences in income and job opportunities.

To include indicators of economic and social horizontal inequality, I had to calculate them using census data harmonized by IPUMS-International (Minnesota Population Center 2019a). This is because such indicators are not widely and readily-available, as data on horizontal inequality is still quite limited and researchers are still relying on data that was not collected for the purpose of studying horizontal inequality. Some researchers have used data from the Demographic and Health Surveys for horizontal inequality measures; however, I employed IPUMS-International because it provides additional indicators that have not yet been incorporated in cross-country analyses. Including them thereby allowed me to expand horizontal inequality indicators beyond what the current literature features in many cross-country analyses.

Because of the size of the census sample datasets, I downloaded each country's census as a unique dataset. I calculated economic and social horizontal inequality indicators for each census dataset and then merged my calculated indicators into a final dataset for analysis. Dataset creation and analysis was conducted in Stata 15.1. Because IPUMS-International provides the questions and possible responses asked in each census, I was able to verify that each indicator included in the current study was gathered in a uniform way across countries and years. For example, because I calculated literacy rates, as opposed to having them provided by IPUMS-International, I was able to verify that "Can you read/write?" was the question asked in each country in each year, with possible responses of "Yes" or "No." IPUMS-International also maintains consistency through its harmonization process for questions such as those addressing educational attainment, where cultural contexts might make it difficult to achieve comparable responses in their raw form. For example, asking respondents in two different countries about

their highest level of schooling might be difficult to compare if respondents provide grade levels that do not exist as such in other countries' schooling systems. To make these variables comparable, IPUMS creates one "harmonized" variable, which takes a uniform coding scheme IPUMS designs (Minnesota Population Center 2019b). In the above example, both respondents could receive a 1, indicating "completed primary school," to indicate the equivalence of their schooling levels and facilitate comparability. IPUMS datasets also include the underlying, or "source," variables, which provide the respondents' original, un-harmonized answers, for reference by subsequent researchers. These factors contribute to the reliability of the data provided by IPUMS, as noted by Jeffers et al. (2017), and allowed me to ensure that my calculated rates were comparable and consistent across countries and years.

Social horizontal inequality indicators were measures of how far a given cultural group was from the national average in terms of its child mortality rates, literacy rates, and educational attainment. As noted, IPUMS-International provides a *sample* of censuses from countries around the world. While most of these samples are simple random samples (Minnesota Population Center 2020), some have, for example, differential weighting. IPUMS-International provides appropriate final weights to use in its datasets to calculate variables with minimal sampling error.

To calculate each indicator, I applied the final population weights provided by IPUMS-International to each census's dataset. This expanded each dataset to full-sized captures of each country's census. To remain consistent in my methods and ensure that I weighted the data correctly, I applied weights to every dataset, including simple random samples, which were representative of the population even before weighting (Minnesota Population Center 2020). As a way of checking my calculations and maintaining external validity, I used my applied population weights to calculate the total population of each country in each year in the dataset,

and verified my estimated population counts against each countries' official counts in each year to make sure they were similar. Congruently, I also calculated ethnic breakdowns of each country (see section 3.3 for a fuller discussion of how a country's relevant cultural groups were determined and coded) and compared them with official breakdowns to ensure they were similar and to ensure external validity.

After the data was weighted, I calculated the child mortality rates, literacy rates, and educational attainment of 1) each country in each year, and 2) each cultural group in each country in each year. To calculate child mortality rates, I was required to use summary birth history from census data. Censuses often record summary birth history, which details the total amount of children a woman has ever had and how many of them are alive at the time of the census, as opposed to full birth histories, which would also include the age at death of a given child (UN IGME 2013). With summary birth histories, calculating child mortality as simply the amount of deceased children divided by the total amount of children ever born could be problematic, since older women could answer for adult children. Therefore, to mitigate this issue and still capture an estimation of child mortality rates, I restricted the age of the mother to 20-29 at the time of census. This practice is consistent with the literature on this topic (Wilson and Wakefield 2018; UN IGME 2013), and makes it virtually impossible for mothers to count adult children in their responses. I therefore calculated child mortality rates by dividing the number of deceased children of women aged 20-29 over the number of total children ever born to these women. I did this at the national level to calculate national averages, as well as by groups in each country for each census year.

I originally calculated educational attainment in three different ways to be able to compare slightly different operationalizations' effects. That is, I calculated the percentage of 20-

29 year olds in the national population and in each cultural group that had completed primary school, high school, and university²¹. I then took the absolute value of the difference between each group's average rate and the national average, to see how distant a given group was from its country's average. The difference for each indicator was used in the final dataset as the indicator of horizontal inequality in terms of educational attainment. Likewise, to calculate literacy rates, I divided the total number of people who could read and write over the total number of people, both again for the national population and each cultural group in each country in each year. The absolute value of the difference between each group and its country's national average comprised the observation for that group in the given year.

Indicators for economic horizontal inequality were calculated in a similar manner as for social horizontal inequality. After weighting the data by using the process described above, I calculated the economic indicators: the absolute value of a group's difference from its country's national average in terms of its unemployment rates, home ownership rates, and access to electricity rates.

I calculated unemployment rates in a way that was consistent with the U.S. Bureau of Labor Statistics' (2015) measure and divided the number of people in the active workforce who were reported as unemployed at the time of the census over the total number of people in the active workforce. On the censuses, the "active workforce" is made up of those who are considered "employed" or "unemployed." The employed are those who worked for pay within the week of the census, maintained a family business, or were temporarily absent from their regular jobs because of some personal reason such as vacation (BLS 2015). The unemployed are

²¹ Each operationalization produced similar effects throughout the model-building process. Therefore, I only retained the operationalization of the percentage of 20-29 year olds who had completed university in my final analyses.

those who did not have a job at all during the week of census and had made at least one attempt to find a job within the past month, and those who were waiting to be called back to a job from which they had been laid off (BLS 2015). People who were not part of the active workforce, such as retirees, were not counted. Because calculations of unemployment rates are fairly consistent across sources, I was able to compare my calculated national unemployment rates with those reported by other official sources and verify their consistency as a measure of external validity.

I calculated home ownership rates by dividing the number of people who owned their home over the total number of possible homeowners (people who were eligible to receive the census question of whether they owned their dwelling), in the national population and in each cultural group. Finally, I calculated access to electricity by dividing the number of households with electricity over the total number of households, in the national population and in each cultural group. To get the final indicators used in the analyses, I took the absolute value of each cultural group's difference from the national average for each indicator in each year. These variables were included in the final dataset as the measures of economic horizontal inequality.

As noted, in this analysis, I operationalize social and economic horizontal inequality indicators as the absolute value of the difference between a given group's average and the national average. While there are several ways to operationalize horizontal inequality indicators, I chose this one for several reasons. The difference between a given group's average and the national average provides an indication as to whether or not the group is relatively better- or worse-off than the "average" person within their society. Because other groups are necessarily included in the national average, this operationalization allows me to gauge if the group of interest is generally better- or worse-off than most people in the society.

I took the absolute value of the difference between a group's average and the national average, as opposed to simply the difference, because of the hypotheses I seek to test. I am testing the hypothesis that the presence of inequality *itself* is correlated with conflict (H1, as discussed in section 3.2.1). I am also testing the hypothesis that the most unequal groups in a society are the most likely to engage in violence (H3, as discussed in section 3.2.1). This is not to suggest that being better- or worse-off does not have an effect on conflict and how it develops; rather, it is to test the notion that the presence of inequality itself is correlated with these outcomes. I seek to refrain from “immunizing” my analysis to null findings by testing several manifestations of inequality that could possibly be correlated with conflict and conflict dynamics. Instead, in this study, I test only what I explicitly hypothesize is correlated with conflict, that greater inequality is correlated with an increase in the likelihood of 1) conflict occurrence and 2) group involvement. I use underutilized measures of inequality and readily-available data to do so. The purpose of this exercise is to assess the viability of these measures and methods to study conflict and its dynamics, and to compare differences in effects of different indicators. Future researchers might expand upon these hypotheses and include additional qualities about the groups studied, such as whether a given group is better- or worse-off than the average person, and not just different²². Different operationalizations would be useful in future research, since there are still ongoing discussions (e.g., Stewart et al. 2010) regarding which indicators of horizontal inequality are best to include in analyses, and how to operationalize these concepts in ways that depict a fuller picture of how inequality manifests in different societies. As

²² I have conducted preliminary analyses on this front, examining the relationship between my independent variables without absolute values and the outcomes of interest (see Tables 12 and 13 in Appendix C). These analyses provide evidence for the idea that future research is warranted and are discussed further in Chapter 5 (“Research Implications”).

the field continues to develop, it is worth testing different ways of getting at relative inequality in analyses and noting any differences in effect.

Using the census samples to calculate horizontal inequality indicators in this way allowed me to include several indicators of social and economic horizontal inequality. Including such indicators provides a more comprehensive picture of how inequality manifests within each society. Including them also makes it possible to compare differences in effect between these various manifestations of inequality, and assess whether it is worthwhile for researchers to pursue incorporating different indicators of inequality into their analyses of conflict. Finally, it allows me to better assess horizontal inequality's relationships with both conflict and group involvement in conflict.

Notably, in societies where there is wide variation on group size, the largest group will contribute most to overall national averages, and would therefore have a relatively small deviation from the national average. This would mean that, even if the largest group fared considerably better than other groups, it would be close to "average." However, I test the theory that a group's relative inequality – how they fare socially, economically, and politically as compared to most people in their country – is correlated with conflict occurrence and group involvement in conflict. If a given group is the largest group in a society by a large margin, its social and economic indicators will be reflective of most people in that society. Likewise, even if a group is small, its average difference from the "average" person is able to be noted with this measure. Therefore, the use of distance from the national average to gauge inequality between groups is an appropriate measure, particularly for initial analyses.

It also should be noted that, while these types of indicators have been used outside of the conflict literature to indicate the societal well-being of nations and peoples, there is room for

critique on these measures. Specifically, inter-group differences on inequality measures do not always necessarily indicate unequal access to societal benefits. For example, while differences in home ownership could be the result of systematic unequal access to wealth and home-owning resources such as loans, it could also be the result of differences in how groups prioritize home ownership. One group could, for example, be settled and systematically desire home ownership at larger rates, while another group could be nomadic and see home ownership as a low priority.

While this critique is reasonable, reports and studies of each of the countries within this study's sample²³ indicate that differential access (not simply differential priorities) across group lines is present in each country (e.g., Baker 2001; Bennett et al. 2008; Bleckner 2015; MRGI 2018). These reports feature concrete illustrations of where and how systematic inequalities have led to group disparities, which indicate that the inequalities measured in this study are indeed reflections of unequal access, not simply different group attributes. For example, Bleckner (2015) recounts the government-mandated forced evictions, displacement, and torture of indigenous ethnic minorities in Bangladesh. In this case, the minorities were displaced in part so the country's ethnically most advantaged group, supported by the government, could settle in an area called the Chittagong Hills (Bleckner 2015). Unequal housing stability in this case is clearly the result of systematically unequal circumstances, not of differences in group characteristics such as being nomadic versus being settled. This example of systematic discrimination in Bangladesh is emblematic of circumstances in the other countries in the sample. It provides reason to believe that measures of inequality in this study are reflective of unequal access.

However, to the critique's point, differences across the social and economic inequality measures in this study still might not wholly reflect inequality on the ground in these countries.

²³ See Chapter 3.3, "The Sample," for a full description of the sample.

Likewise, in some other societies, certain measures might not be reflective of differential access, but rather differences in group attributes. With this critique in mind, it is still important to note the argument that, particularly in societies with demonstrated cleavages across group lines, group differences in social and economic indicators could reflect the ways in which inequality presents itself in the society.

As a final note, some of the literature (e.g., Hagan and Rymond-Richmond 2008; Stewart 2008; Cramer 2003) notes the relevance of perceived as well as objective inequalities. It would therefore be useful to include a measure of perceived inequality across societies, but data is currently still a major barrier (see Chapter 3.5, “Methodological Limitations,” for a fuller discussion). To ensure data quality and maintain a focused scope, I measure objective inequalities and study their relationship with conflict. In many cases, it is reasonable to expect these will coincide with perceived inequalities; however, this should not always be assumed to be the case, as the two can differ substantially (Miodownik and Nir 2016; Langer and Smedts 2013). Likewise, though the omission of perceived inequalities could make the effect of objective inequalities appear larger than they are, this potential for bias is unlikely to negate findings (see Chapter 3.5, “Methodological Limitations,” for a fuller discussion). I hypothesize that perceived inequalities, as well as the way they are framed, are relevant to conflict developments. However, I limit the scope of my study to assessing the relationship between objective inequalities, which I measure in the ways described above, and violent conflict dynamics.

3.2.3 Independent Variables – Controls

Horizontal inequality does not operate in a vacuum. Other factors can increase or decrease the risk of conflict, even in the face of horizontal inequality. Since I am only examining

the relationship between horizontal inequality and violent conflict, not every predictor of conflict should be accounted for. However, those that are likely to (moderately) correlate with horizontal inequality, violent conflict, and group involvement should be included in the analysis to mitigate omitted variable bias and better partial out the effect of horizontal inequality on violent conflict and group involvement.

One control variable is respective group size. Group size denotes each cultural group's share in the total population of the country. It is measured as a value ranging between 0 and 1, where 1 means that a group makes up 100% of the country. Including it is useful for understanding whether the detected effect of horizontal inequality is the result of group inequalities in and of themselves, or whether it is group size that actually drives response to horizontal inequality. Group size is publicly available data, compiled in the EPR Core dataset (Vogt et al. 2015).

Another important control is government repression. Government repression is a measure of how many civil liberties a government provides its people in a given country (e.g., access to peaceful protest; legal protections for investigative and journalistic entities; etc.). This variable is measured annually by Freedom House, who uses an extensive survey to determine each country's level of civil liberties and political rights. The survey measures factors related to freedom and government extraction, such as whether independent groups can count votes, rule of law, etc. (Puddington et al. 2017). Each answer generates a score, and the average score determines whether Freedom House ultimately categorizes the country as "free," "partly free," or "not free." Civil liberties are scored on a scale from 1 to 7, where 1 means that a country enforces no systematic repression of civil rights and 7 means that a country enforces grave systematic repression of civil rights.

Government repression is important to include, because if a country's rule of law and level of repression is incredibly high generally, it could negate the capacity to organize and form armed groups. As discussed in the literature review, this phenomenon has been evidenced in works such as those of Muller (1985), who finds repressive regimes to alter the effect of income inequality, and Gurr (1993), who finds repression to modify effects of marginalization. Likewise, literature on horizontal inequality and conflict has mentioned the need to include variables that get at military capacity (Mancini 2008). As such, including the measure of repression, which was used in Muller (1985), is an important step to parsing out the effects of horizontal inequalities.

I also included a trend variable as a control. This allowed me to determine if conflicts are simply becoming more intense or entrenched as a function of time (that is, conflicts get worse as they go on), or if it is as a result of changing horizontal inequality levels over time. Including this variable allowed me to distinguish the effects of horizontal inequality and any trend intrinsic to conflict onset or group involvement.

Finally, I included logged GDP and a variable called "neighbor" as controls. Logged GDP is included because economic development is a common indicator for conflict, and is often included in studies on the topic (Ostby 2008; Cederman et al. 2013). It is collected and disseminated by the World Bank data bank (World Bank 2020). "Neighbor" is a measure of how many bordering countries are also in conflict in a given year. It is included to capture the effect of spillover on a given country's conflict development.

3.3. The Sample

This study includes data from 28 cultural groups scattered across 8 countries, with data ranging from 1971-2011, for a span of 40 years. Each group, country, and year of census data included in the sample can be found listed in alphabetical order in Appendix A. The number of

cultural groups in each country ranges from 2 to 7, with the mean being 3.5 and the median being 3. Given that each group appears in each census year, the total sample size is 70.

Census years for each country are different (for instance, Bangladesh conducts a census in 1991 while Colombia does in 1993), and countries begin conducting censuses at different points in time (for instance, Indonesia's first available census is collected in 1971, where Mali's is 1987). As noted, Appendix A lists each of the census years for each of the countries. Each country collects two or three censuses, with the exception of Indonesia, which collects four censuses, as its censuses begin earliest. The mean amount of censuses conducted per country is 2.6 (median, 2.5).

Each country's relevant groups, as identified by a survey of regional experts (see Vogt et al. 2015), are the units of analysis. As noted, I calculated horizontal inequality measures for each group by calculating average rates (e.g., average child mortality rates, etc.) amongst groups. In most of the sample's cases, the census explicitly collected demographic information assigning individuals to the relevant ethnic groups identified in the EPR Core dataset. However, in the case of Indonesia, ethnicity information was not collected for most of the censuses. I instead use religious groupings, which are demonstrated by MRGI (2018), Ananta et al. (2015), Stewart et al. (2010), and Mancini (2008) to follow ethnic lines and serve as a reliable proxy in Indonesia. Likewise, in Mali and Peru, linguistic groupings are collected rather than explicit ethnic groupings. These are also demonstrated by Stewart et al. (2010) to serve as a reliable proxy of ethnicity in these contexts. It should also be noted that in Nepal, relevant cultural groups are identified as six castes, which are made up of various ethnic subgroups (ex., the Hill Brahman and Hill Chhetri ethnic groups are part of the larger caste, Caste Hill Hindu Elite). The Nepalese census conducted the census using these ethnic subgroups as categories, which I recoded back

into their larger caste categorizations by using Bennett et al.'s (2008) and Shreshta's (2002) guidelines for grouping. As an added measure to ensure external validity and confirm groupings from all of these countries matched up with demographic breakdowns in EPR Core and official records, I compared the percentage of the population that was in a given group in my dataset with the officially-recorded percentage of the population in that group. That is, for each of these countries, I ensured that my calculated ethnic breakdowns matched those in the EPR Core dataset and recorded by official records. In each case, the groupings matched. This step helps ensure that the group averages I calculated are reliable.

Within this sample, conflict occurs fairly often from year-to-year. In census years, Bangladesh experiences conflict in one of its three census years. Guatemala experiences conflict in two of its three census years, and Indonesia does in two of its four census years. Mali experiences conflict in one of its three census years. Colombia, Peru, and Uganda experience conflict in both of their census years. Additional information on the likelihood of conflict can be found in Chapter 4, "Results."

The field-wide limitations surrounding inequality data make it necessary to consider whether countries in the sample systematically differ from countries outside the sample. Some countries, such as Rwanda and Nigeria, do not ask people to identify with or allow for data collection on ethnicity, due to ethnicity's connections to historic violence (Canelas and Gisselquist 2018; Okolo 1999). Likewise, the particular countries included in this sample are included because they allowed people to identify with culturally- and politically-relevant groups in the given years. However, this difference does not necessarily mean that in-sample and out-of-sample countries have systematically different kinds of inequality.

Indeed, there is reason to believe that this issue does not negatively affect the sample. The census data was originally collected with the intention of capturing different aspects of life in the countries of interest: demographic breakdowns, maternal and child health, overall level of education, etc. (UNSD 2017). It was not specifically collected to provide a way to calculate horizontal inequality measures. Therefore, as Ostby (2008) argues, it is less likely that these kinds of data would have been hidden systematically by some countries for the purpose of improving horizontal inequality measures, and systematically presented by countries with more equal measures. This is not to imply that demographic breakdowns could not have factored into a country's choice of which data to collect or not collect; rather, it is to say that the issue is likely to be less severe with these data. There is, therefore, a logical basis for the idea that differences in inequality levels between in- and out-of-sample countries could be random. This is consequential, because non-random differences would make findings clearly less generalizable.

However, there still could be an issue of sample bias, in which correlates estimated in this analysis underestimate the worldwide relationships between horizontal inequality, conflict occurrence, and group involvement. This is because countries with any data at all could differ systematically from countries that do not have data, most notably in terms of their conflict types. Every country in the sample is a country in conflict. However, it is possible that countries excluded from IPUMS' data altogether, such as Central African Republic (CAR), are excluded due to particularly low governmental capacity or intense conflicts at times of what would have been census conduction or data collection. If this is true, it could suggest that findings are only generalizable to countries stable enough to allow adequate record-keeping of large geographical areas. That bias would underestimate the general relationship between horizontal inequality and conflict. If ethnic or religious conflicts, such as that of CAR's, for example, are excluded from

the sample because they are so intense or have had a systematically different effect, the general relationship between grouped inequalities and conflict could appear less dramatic than it is. Still, there is the question of how severe this issue is likely to be. The vast majority of countries conduct censuses, including countries in conflict (UNSD 2017). Therefore, while I cannot rule out the possibility of sample bias, findings would still be applicable to countries with some form of record-keeping capabilities, which is the overwhelming majority of them.

3.4 Model Specifications

Because of the preliminary nature of exploring the relationships of interest (either in and of themselves or with these variables specifically), it is most appropriate to test the relationships using several model specifications. Doing so makes it possible to assess the robustness of the parameter estimates against different specifications.

It should be noted that the models used to conduct this exercise provide preliminary examples of the analyses that can be done with this data. There are a number of adaptations and changes to these analyses researchers should consider in future studies, which are discussed more at length in Chapter 5 (“Research Implications”). For the purposes of this dissertation, it is simply useful to note if, when these operationalizations of horizontal inequality are incorporated into an analysis, there are correlations between each of the indicators and 1) conflict occurrence and 2) group involvement in conflict. The small sample size available from the census data employed inflates standard errors, but it provides considerable evidence if correlations are detected. While these preliminary analyses provide a useful glance into possible correlations, there is still room for further analysis to more definitively determine and compare effects, particularly since this analysis features several drawbacks (see sections 3.5 “Methodological Limitations” and 5.1 “Results Summarization” for a fuller discussion). Still, this (albeit limited)

treatment helps assess if examining variables in these ways (i.e., with the present indicators, operationalizations, dependent variables, and data) is a worthwhile exercise.

Before conducting analyses, I explore trends in the dependent variables themselves. Then, to examine correlations between horizontal inequality and conflict, I graphically depict bivariate relationships between horizontal inequality indicators and the predicted probability of conflict. I then conduct a simple linear probability model and logit model (see Chapter 4, “Results”). I run this analysis with group-level and country-level indicators, noting results in the adjoining discussion. These tests, while quite simple, allow me to take note of patterns between the variables using these data.

Because of its within-group variance, I am able to subject the relationship between horizontal inequality and group involvement to stricter tests, such as the random effects linear probability model (see Chapter 4, “Results,” for a fuller explanation). The data is paneled, and “unobservables” (e.g., historical events, cultural contexts, etc.) might mean that countries have differences in their original underlying propensities to engage in conflict. Allowing Beta coefficients to vary and implementing a random effects²⁴ linear probability model would help account for this. However, this analysis assumes that there is variation on the dependent variable within country-years. This assumption cannot be met when conflict is the dependent variable of interest, because if a country is in conflict in a given year, every cultural group in that country will receive a 1 for that year. Likewise, if a country is *not* in conflict in a given year, every cultural group will receive a 0 for that year. Therefore, there is no variation on the dependent variable. This is not the case when the dependent variable is group involvement in conflict, as

²⁴ A fixed effects analysis would also account for some of these factors; however, because there is a population beyond the sample about which I am interested in generalizing, the “group” effect can be thought of as random (Taylor 2019). To ensure this theoretical preference for random effects is appropriate, I estimated random and fixed effects models and ran a Wu-Hausman test, which confirmed the suitability of the random effects analysis.

some cultural groups can receive a 1 in years where other cultural groups in the same country receive a 0. When group involvement is the dependent variable, there is over-time within-country variation, which allows for a more appropriate use of the random effects analysis.

To study the relationship between horizontal inequality and group involvement, I first graphically depict bivariate relationships between horizontal inequality indicators and the predicted probability of group involvement. I then conduct a series of tests of the relationship, building the models from the simplest test (the linear probability model) to the strictest (the random effects linear probability model). Each of these models is specified in various ways, to assess how stable parameter estimates are against these different specifications. While a fuller exploration of each specification and its detected effects are found in Chapter 4 (“Results”), the random effects linear probability model can be written as

$$\hat{p}(\text{groupinv}_{it} = 1|x) = \widehat{\text{groupinv}}_{it} = \hat{\beta}_0 + \hat{\beta}_1 \text{polpwr}_{it} + \hat{\beta}_2 \text{abshicm}_{it} + \hat{\beta}_3 \text{abshilit}_{it} + \hat{\beta}_4 \text{abshiuni}_{it} + \hat{\beta}_5 \text{abshiunemp}_{it} + \hat{\beta}_6 \text{abshielec}_{it} + \hat{\beta}_7 \text{abshiown}_{it} + \hat{\beta}_8 \text{cl}_{it} + \hat{\beta}_9 \text{loggdp}_{it} + \hat{\beta}_{10} \text{neighbor}_{it} + \hat{\beta}_{11} \text{size}_{it} + \hat{\beta}_{12} \text{t}_{it} + \alpha_i + v_{it}$$

where *groupinv* is group involvement, *polpwr* is political inequality, *abshicm* is group difference in child mortality, *abshilit* is group difference in literacy, *abshiuni* is group difference in educational attainment, *abshiunemp* is group difference in unemployment, *abshielec* is group difference in electricity, *abshiown* is group difference in home ownership, *cl* is government repression of civil liberties, *loggdp* is logged GDP, *neighbor* is the number of bordering countries at war, *size* is group size, *t* is a time trend, and *a* and *v* are composite error terms²⁵. Specifying the models in various ways, building up to this one, allows me to examine the

²⁵ The term *a* represents time-invariant variables not included in the model, and the term *v* is allowed to differ between time periods and cases (Williams 2015).

robustness of estimates, particularly given the low sample size, and facilitates detecting patterns in the potential relationship between horizontal inequality and group involvement.

There are several model-building considerations that should be noted with regards to this exercise. The first is the presence of multicollinearity between some of the inequality indicators' correlation coefficients. Some forms of horizontal inequality are correlated with others. To examine multicollinearity, I gathered correlation coefficients for each of the independent and control variables (see Figure 8 in Appendix B). Many of these variables were not notably correlated; however, some were. Of the key independent variables (the horizontal inequality measures), three were moderately correlated (two in the 0.4s, and one in the 0.5s – see Figure 9 in Appendix B). As inequalities are difficult to disentangle and are likely related, multicollinearity is an issue within this analysis. It inflates standard errors and negatively impacts estimate consistency. However, this condition also means that, if there is a detected correlation between independent and dependent variables (particularly given the sample size), there would be considerably strong evidence against the null.

It is also important to consider the sample's balance. This analysis features panel data. As noted earlier in this chapter, observations within the data occur at slightly different frequencies and in different years due to the variable timing of census collection. One country, for example, might have conducted a census in 2001, whereas another one did so in 2002. However, this should not have negative implications for the analysis. Unbalanced panels are an issue when one unit occurs more or less in the sample for a reason related to data attributes, such as attrition due to changing horizontal inequality levels or changing conflict qualities. It does not appear this issue occurs in the sample, though, as no country “drops out” of the sample at any point: their census years are simply different, for reasons unrelated to the kind of inequality present in each

country. Likewise, though IPUMS harmonizes and distributes data from each country at different start times (for example, Indonesia's first census year in the IPUMS database is 1971, whereas it is 1987 for Mali), each country has collected censuses previous to their first recorded ones in the IPUMS database. Thus, every country collects censuses in similar time periods; no country is systematically different enough to have poor record-keeping that might occur, for example, as the result of a particularly dangerous conflict. This suggests that the sample, therefore, might not be systematically "unbalanced" in a way that would bias results. Finally, to protect against the possibility that unequal time intervals impact parameter estimates, I include two specifications of a trend variable (see Chapter 4, "Results," for further explanation). This allows for a better understanding of the robustness of parameter estimates and helps mitigate the potential consequences of unequal time intervals.

3.5 Methodological Limitations

It is important to consider the methodological limitations of this study. One limitation is the imperfect measure of horizontal inequality. In this study, I attempt to implement more comprehensive measures of horizontal inequality than have been implemented in many previous cross-country analyses. While the data I use to do so are reliable (Minnesota Population Center 2019a), the inequality indicators I use in this study are still not optimal measures of horizontal inequality in each society. Three indicators of social or economic inequality does not perfectly reflect the nature of inequality present within a society. While I intend for the indicators to serve as reliable proxies for horizontal inequality, societal differences in group statuses or treatment are not fully captured by the six indicators of social and economic inequality. Therefore, the measures could fail to pick up on the extent of group differences. Unfortunately, the data limitations are persistent, and unavailable data cannot be rendered available.

Another clear limitation is sample size, which decreases statistical power and increases the likelihood of type II error. As has been discussed extensively, data limitations within the field are acute, and this has a material impact on sample size. In part, using group-level indicators helps expand the sample size to $N=70$; though, this is still fairly small. A low sample size means that, while I can be sure that the relationships I examine are reflective of what occurred in the observation years, I cannot be sure that these relationships do not differ by chance from unobserved years, or that they are generalizable (see Chapter 4, “Results,” for a further discussion). It does mean, however, that there is considerable evidence for detected correlations, which must be apparent in the face of inflated standard errors and low statistical power.

Another methodological limitation of the study is omitted variable bias. Some of the literature (e.g., Hagan and Rymond-Richmond 2008; Stewart 2008; Cramer 2003) notes the relevance of perceived as well as “real” inequalities. It would be useful to include a measure of perceived inequality across societies, but data is currently still a major barrier to an adequate study of this. Perceived inequality is likely to positively correlate with real inequality and with conflict, resulting in an upward bias of the effect of the key independent variables in this study. I hypothesize that perceived inequalities, as well as the ways they are framed, are informative to conflict. However, due to limitations in measuring perceived inequality in these contexts, this study focuses on the relationship between objective inequalities and violent conflict.

Despite the fact that this upward bias exists, it is likely not severe enough to negate results. That is, the detected effects of objective horizontal inequality in this study are not likely solely attributable to what are, in fact, the effects of perceived inequality. Miodownik and Nir (2016) provide the first empirical analysis of the effects of objective and perceived horizontal inequality on protest behavior and receptivity to violence. The authors find both objective and

perceived horizontal inequalities to have a statistically significant positive effect on rebellion and receptivity to violence, supporting the idea that omitting perceived inequalities leads to upwardly biased estimations of objective inequalities.

However, Miodownik and Nir (2016) also find that objective inequalities have a larger substantive effect than perceived inequalities, and that this effect is robust, even when objective and perceived horizontal inequalities are included in the same model. These findings indicate two things. The first is that perceived and objective inequalities do not account for all of the same variance, which in turn means that objective inequalities have a unique effect on violence. Second, the larger substantive effect of objective horizontal inequalities indicates that, even when perceived inequality is omitted, most of the detected effect of objective inequality is its actual effect. Miodownik and Nir's (2016) findings are important, because they indicate that, despite the presence of an upward bias, the objective inequalities I examine in this analysis have a distinct effect on conflict, and their detected effects should be considered, at least in large part, as such.

Endogeneity is a methodological limitation, as well. I theorize that horizontal inequality is a cause of violent conflict, and that changes in horizontal inequalities could alter aspects of these conflicts. However, the reverse could be simultaneously true: violent conflict could lead to horizontal inequality. Simultaneity is likely a present issue with these data. Particularly in contexts where absolute loss is the same in warring groups, but one group has more people or resources, horizontal inequality could change as a result of violence. However, rather than asserting causality, I am exploring correlations and empirical regularities. These regularities can still be noted, even in the face of endogeneity.

While the purpose of a purely correlational analysis such as this one is not to resolve the issue of endogeneity, it is worthwhile to note that authors of previous studies have taken steps to examine and limit it. Cederman et al.'s (2011) analysis, for example, bolsters the idea of exogeneity in some conflicts (namely, in conflicts like those in this study, where both sides are armed, rather than instances of one-sided violence). In their study of horizontal inequality and conflict onset, Cederman et al. (2011) conduct several supplementary analyses to limit endogeneity between political and economic horizontal inequality and conflict onset. First, they drop all group years after an initial conflict begins, disallowing for the statistical possibility of conflict changing horizontal inequality. They also conduct a static model based on the country's first year in the dataset. In all supplementary analyses, the effects of horizontal inequality on conflict are robust (Cederman et al. 2011). This evidence reinforces the idea that horizontal inequality can cause conflict, and that findings are not simply estimates of how conflict impacts horizontal inequality. Still, despite these findings, endogeneity is a worthwhile concern, and impedes claims of causality.

When considering the possibility of simultaneity, it is also worth considering to what extent the kind of conflict studied in this analysis (and in studies such as Ostby's [2008], Mancini's [2008], Cederman et al.'s [2011], etc.) causes horizontal inequality theoretically. Mass violence is most likely to disproportionately affect one group if the conflict 1) is largely one-sided or 2) occurs exclusively on the territory of one group (which is also most likely in instances of one-sided violence, such as in the case of the Darfur genocide). However, as Raleigh et al. (2010), creators of the Armed Conflict Location and Event Dataset (ACLED), note, locations of fighting often extend to various regions of a country. They suggest that geographically-localized prolonged conflicts are not the norm. Likewise, one-sided violence,

where an armed group targets exclusively unarmed civilian populations (Eck et al. 2016) and which demonstrably begets horizontal inequality (Hagan and Rymond-Richmond 2008), is a different concept than the armed conflict of two warring groups (Vogt et al. 2015), as studied in this analysis and the abovementioned articles in the literature. Both forms of violence can still cause horizontal inequality; however, the extent to which this occurs likely differs.

As a final limitation, it is important to note that this study is restricted in scope. I do not attempt to determine a full account of what causes conflict. Rather, I simply attempt to illuminate what we know about whether horizontal inequality contributes to violent conflict and conflict development. As such, there are important facets to understanding conflict, such as those covered in the literature review, that I cannot fully address. I do not address the effect of, for example, collective framing on conflict.

Despite these limitations, it is worth considering the contributive merits of this piece. My goal is to help fill several research gaps in the field. Researchers' current assumptions about the relationship between horizontal inequality and violent conflict could be misguided, given the stubborn issues related to studying horizontal inequality discussed at length in Chapter 2.5, "The Research Gaps." Data issues, methodological limitations, and theory that is underdeveloped in some regards have stifled what researchers can confidently assert about the relationship between horizontal inequality and violent conflict. There are few cross-country analyses that exclusively rely on existing data or employ more than one indicator of economic and social horizontal inequality. Likewise, there are very few studies examining horizontal inequality's effect on conflict dynamics, such as who engages in conflict. Despite these field-wide limitations, some authors have discussed the relationship between horizontal inequality and violent conflict as clearly demonstrated and causative. However, if the understanding of the relationship is

misguided or incomplete, this could adversely affect policy aimed at rectifying either inequality or violence.

In this dissertation, I attempt to fill some of these gaps. By exclusively relying on existing data, expanding measurements of social and economic horizontal inequality, and exploring possible correlations between horizontal inequality and group involvement, I am able to more closely examine horizontal inequality's relationships with the outcomes of interest. By specifying models in several ways, I am better able to discern the robustness of parameter estimates. These contributions help inform how we can study horizontal inequality in the future and what we think about whether and why it affects violent conflict dynamics.

CHAPTER IV: RESULTS

4.1 A Summary of the Results

Ultimately, analyses showed promise of potential relationships between horizontal inequality and 1) conflict occurrence and 2) group involvement in conflict. However, these relationships might be more complex than previously postulated. While definitive conclusions cannot be drawn from the exploratory analyses conducted here, such analyses suggest that further investigation into the relationship between horizontal inequality and conflict dynamics (most notably, beyond simply whether conflict initially occurs) is warranted. It will be particularly useful to pursue additional data for these investigations, which can help elucidate potential complexities in the relationships that remain muddled.

Findings pointed to several horizontal inequality indicators that seemed to have a notable relationship with the dependent variables of interest, and supported the notion that further study would be useful. In particular, grouped inequality vis-à-vis educational attainment, access to electricity, unemployment, and home ownership seemed to have relationships with conflict dynamics. Some indicators were positively correlated with the outcomes, which was consistent with hypotheses. Others, however, appeared to have an inverse correlation with outcome variables, challenging our current understanding of how horizontal inequality relates to conflict dynamics. Possible explanations for these detected effects are explored in Chapter 5 (“Research Implications”).

There were also several control variables with notable relationships with conflict occurrence and group involvement. Consistent with the literature (see Chapter 2.3.2 “Governmental Structure”), groups appear less likely to become involved in conflicts as the government moves from moderate to extreme repression, likely because extreme repression

prevents large-scale mobilization (Muller 1985; Gurr 1993). They are also less likely to become involved in conflicts as logged GDP increases, likely because a more well-off society is associated with better development outcomes and less desire to rebel (Ostby 2008).

Corroborating previous studies (Mancini 2008; Ostby 2008; Tiwari 2010; Cederman et al. 2013), horizontal inequality seems to have a relationship with conflict. However, the current analysis also provides new evidence that this relationship might be more complex than previously thought. Likewise, it provides evidence that horizontal inequality might affect conflict dynamics, such as which groups are involved in the conflict. Whether violent conflict and group involvement are more, less, or just as likely seems to depend upon the specific type of horizontal inequality present in a society (e.g., whether it is unequal in terms of access to electricity, for example, versus in terms of home ownership). These findings, if substantiated in future analyses, would have notable theoretical, policy, and research implications, discussed in Chapter 5 (“Research Implications”).

When taking these results into consideration, it is again important to keep in mind the preliminary nature of these analyses. The purpose of these models is to establish a preliminary assessment of the relationship between horizontal inequality and conflict dynamics using the variables created for this analysis and the strategies implemented to help isolate the relationships. There are several limitations to the analyses that should be kept in mind (see section 5.1, “Results Summarization”).

Arguably the most notable takeaway from the analyses is that further investigation into the relationship between horizontal inequality and conflict dynamics using these kinds of data and indicators is warranted. As it appears from these results, there might be differential effects on conflict dynamics depending upon the way horizontal inequality manifests within a society.

This notion is worth continued exploration. Similarly, results provide support for the idea that it is worthwhile to continue down the path of 1) identifying better data and data sources, 2) taking into consideration additional aspects of the way inequality manifests within a society (e.g., indicating which groups are better- or worse-off, incorporating various indicators of social and economic inequality, etc.), and 3) examining horizontal inequality's effect on not only conflict onset, but also its various dynamics.

While the analyses presented in the current study are preliminary, they reinforce the idea that accessing various kinds of data, incorporating various indicators into cross-country analyses, and examining the effect of these indicators on additional dependent variables are useful strategies for future research. These strategies could provide further insight into the relationship between horizontal inequality and conflict dynamics.

4.2 Key Independent Variables

The key independent variables included in this study are measures of horizontal inequality. Specifically, they are group distances from the national average in terms of a group's child mortality rates, literacy rates, educational attainment, unemployment rates, access to electricity, and home ownership rates. They also include a categorical variable indicating a given group's access to political power, where 1 is a monopoly and 4 is exclusion from any power. Examined relationships between horizontal inequality and 1) conflict occurrence and 2) group involvement are delineated below.

4.2.1 Horizontal Inequality and Conflict Occurrence

Before examining the direct relationship between horizontal inequality and conflict, it is important to understand trends in the variables themselves. To this end, it is worthwhile to consider several descriptive statistics of these variables.

Conflict occurs fairly often within the sample countries. As noted in section 3.3, “The Sample,” conflict occurs in 62.8% of the 70 observations in the sample. From 1971-2011, the likelihood of conflict is roughly 55%. The likelihood of conflict occurring in the sample is higher than it is from 1971-2011. These differences in means, which describe the proportion of time countries are in conflict, can be seen in Table 1, below:

	Mean within Sample (N=70)	SD within Sample (N=70)	Mean from 1971-2011 (N=1148)	SD from 1971-2011 (N=1148)
conflict	.6285714 (.0581688)	.486675	.554007 (.0146771)	.4972913

Table 1: Proportion of Time Countries are in Conflict (1971-2011) (Data source: Petterson et al. 2019; Gleditsch et al. 2002)

From 1971-2011, trends in conflict varied considerably by country. While in Guatemala, for example, conflict became less likely over time, it become more likely in Nepal. To obtain these trends, I calculated the predicted probability of conflict. I regressed conflict on interaction variables of country-year, which allowed the slopes and intercepts to vary. I then used the results to predict the probability of y (conflict). Individual country trends in conflict from 1971-2011 can be seen in Figure 2, below, which shows how the predicted probability of conflict changes over time for each country.

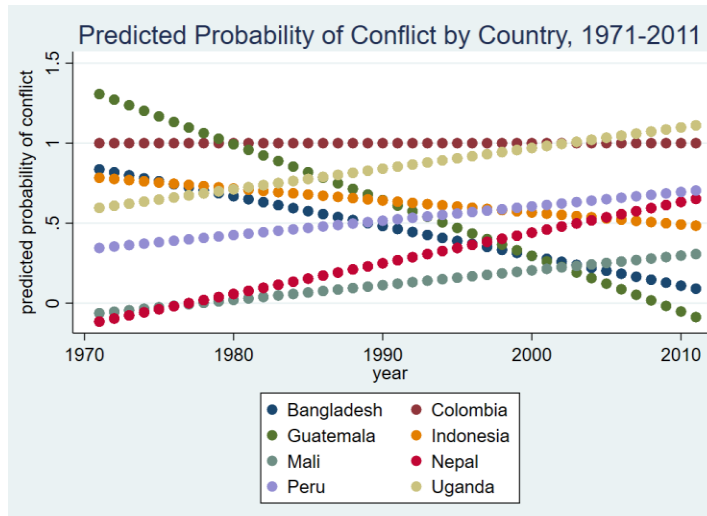


Figure 2: Predicted Probability of Conflict, 1971-2011 (Data source: Petterson et al. 2019; Gleditsch et al. 2002)

Likewise, trends in conflict can be seen by country in sample years (see Figure 3 below). I again used the predicted probability of conflict described above to depict these trends. Differences in trends between sample years and all years (1971-2011) can be noted by comparing Figures 2 and 3. In sample years, conflict becomes less likely over time, consistent with observations by several researchers (e.g., Lacina and Gleditsch [2005]), who note that major international and civil conflicts have declined since the Cold War.

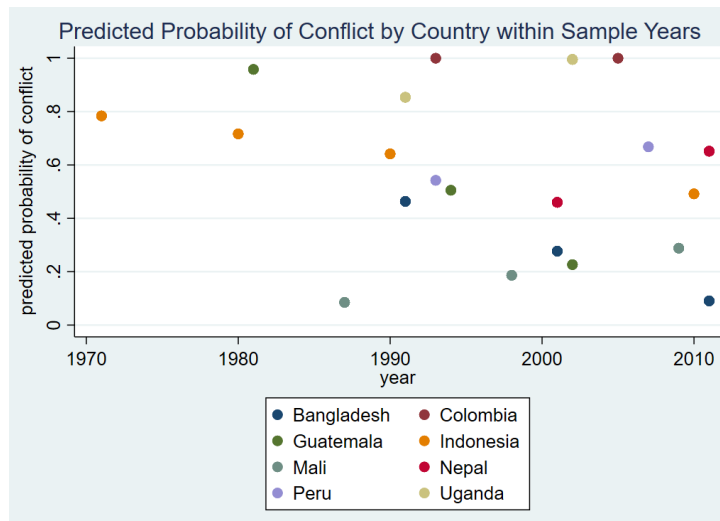


Figure 3: Predicted Probability of Conflict within Sample Years (Data source: Petterson et al. 2019; Gleditsch et al. 2002)

Regarding the independent variables, means, standard errors, and standard deviations for each can be found below in Table 2 (N=70²⁶). It should be kept in mind, when interpreting results of analyses detailed further in this chapter, that the social and economic horizontal inequality measures are produced as proportions rather than whole percentages. For example, the mean of the variable *abshicm* (the absolute value of a group’s distance from the national average in terms of its child mortality) is 0.0296461. Because this is a proportion, it should be interpreted that, on average, groups had a difference of roughly 3% from the national average.

	Mean	Standard Deviation
abshicm	.0296461 (.0067929)	.0568332
abshilit	.0736306 (.0072224)	.0574125
abshiuni	.010357 (.0015066)	.0126052
abshiunemp	.0057659 (.0011089)	.0092777
abshielec	.0955242 (.0147775)	.1236374
abshiown	.0409507 (.0063986)	.0535346
polpwr	3.228571 (.1154188)	.9656631
size	24.98843 (3.339343)	27.93895
gini	40.69104 (.9493808)	7.771017
gdp	64.47829 (18.25321)	152.7173
loggdp	2.891383 (.1791202)	1.498627
cl	4.295775 (.1193449)	1.005618
neighbor	1.3228571 (.0890981)	1.228571

Table 2: Means of Independent Variables (Data source: aggregated sources – see section 3.2²⁷)

It should also be noted that sufficient variation on the independent variables is necessary to be able to detect an effect, if it exists. The ratio of the standard deviation to the mean on each

²⁶ The GINI coefficient has 67 observations, as there was not a GINI coefficient calculated for Bangladesh in 2011. The GINI coefficient is not a variable in any main analysis, and is only used for a sensitivity analysis (see section 4.3).

²⁷ As discussed in section 3.2 (“Variables and Data Sources”), I created or used existing independent variables from various data sources, including census data from IPUMS (Minnesota Population Center 2019; Boyle et al. 2019), the EPR Core dataset (Vogt et al. 2015), the World Bank (2020), and Freedom House (Puddington et al. 2017).

independent variable is generally greater than one and, in some cases, greater than two. These ratios indicate sufficient variation.

To examine the relationship between horizontal inequality and conflict occurrence, I initially estimated a random effects linear probability model of conflict occurrence on horizontal inequality. This allowed me to take into account the paneled nature of the data. It also allowed Beta coefficients to vary, which could be consequential if unobservables, such as historical events or cultural contexts, might change the propensity for conflict. Because panel groups in this sample would constitute part of a larger population, as Taylor (2019) discusses, their effects can be thought of as random.

However, when estimating random effects models, there is an assumption of within- and between-group variance, so that the estimated intraclass correlation coefficient (ICC) does not equal zero. When conflict (a country-level variable) is the dependent variable, every group from one country has the same value for conflict coded in the same year. That is, in Bangladesh in 1991, for example, all three groups have a 1 coded to indicate that conflict happened that year. Because of this, there is not sufficient variation to estimate an ICC. Using country-level measures (e.g., average group differences in Bangladesh in 1991) might introduce more variance, but doing so reduces the sample size to 21, which is insufficient for a random effects analysis. Therefore, a different analysis examining the relationship between horizontal inequality and conflict is appropriate.

“Aggregated sources” refers to these sources for independent variables, and UPCD (Pettersson et al. 2019; Gleditsch et al. 2002) for dependent variables. More information can be found on variables and their sources in section 3.2.

First, to visualize the relationships between the social and economic horizontal inequality measures²⁸ and conflict, I depict each measure in a scatter plot alongside the predicted probability of conflict. This can be seen in Figure 4 below:

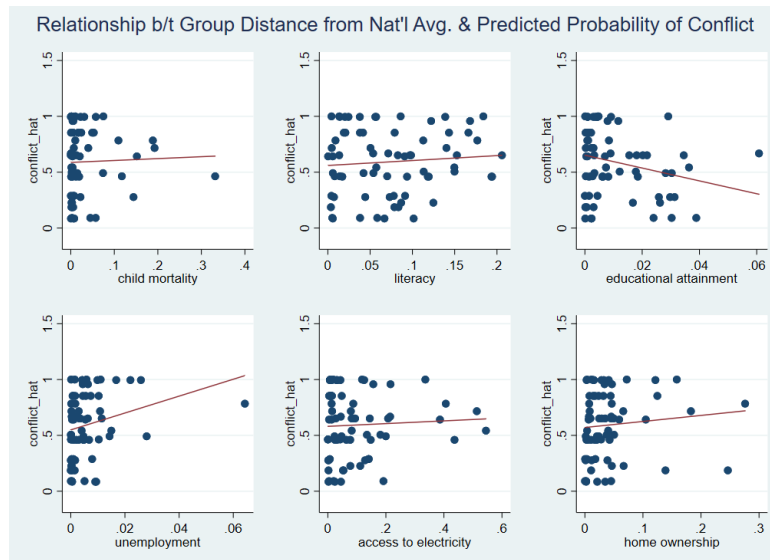


Figure 4: Relationship between Group Distance from Nat'l Avg. and Predicted Probability of Conflict (Data source: aggregated sources – see section 3.2)

The above graphs illustrate the relationship between each group-level measure and conflict, providing a picture of how group-level heterogeneity (i.e., relevant cultural groups' distances from the national average) correlates with the predicted probability of conflict. The starkest relationships can be seen in the educational attainment, unemployment, and home ownership graphs. Group differences in educational attainment appear to have a considerable inverse relationship with the predicted probability of conflict, while group differences in unemployment and home ownership appear to have positive relationships with conflict. If these findings hold, it would mean that as groups move farther from their country's national average in terms of unemployment and home ownership, conflict becomes more likely, as hypothesized.

²⁸ Because political horizontal inequality is a categorical variable, it is not appropriate to visualize in a scatter plot in the same way.

However, as groups move farther from their country’s national average in terms of educational attainment, conflict becomes *less* likely, which is the opposite of what was hypothesized.

Possible reasons for this relationship are explored in Chapter 5, “Research Implications.”

As noted above, country-level indicators cannot be used for a random effects analysis due to the insufficient amount of observations; however, they can still help inform researchers about the relationship between grouped inequality and conflict. In taking the average horizontal inequality value in each country in each year (e.g., Bangladesh’s average level of grouped inequality in 1991), I produced country-level indicators that can bolster group-level indicators. Bivariate depictions of their relationships with the predicted probability of conflict can be found in Figure 5 below:

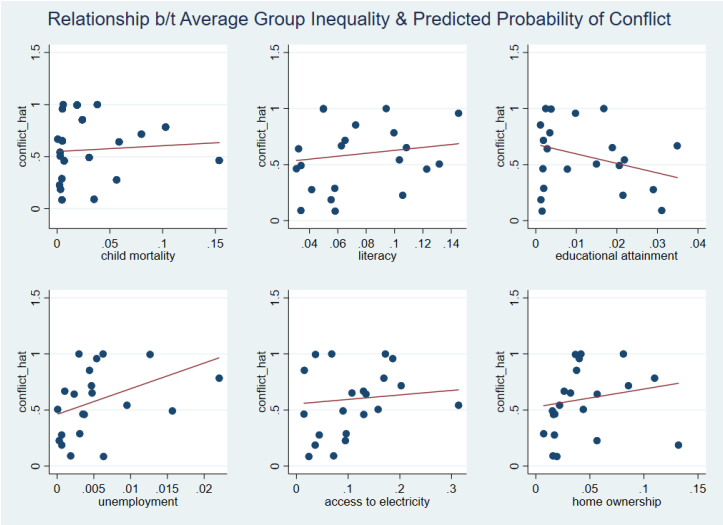


Figure 5: Relationship between Average Group Inequality and Predicted Probability of Conflict (Data source: aggregated sources – see section 3.2)

These relationships mirror those of the group-level indicators, as expected. Estimating the relationship between the indicators and conflict, though, points to some additional considerations worth taking into account. Table 3 below depicts the relationship between country-level horizontal inequality indicators and conflict using two model specifications, a linear probability model and a logit model.

conflict	LPM	Logit
m_abscm	.0523111 (2.850714)	-11.2194 (24.27365)
m_abslit	-1.814901 (3.704416)	-68.0533 (53.89318)
m_absuni	-27.1671** (10.26507)	-309.1977* (177.9725)
m_abselec	3.992047** (1.684154)	71.38793 (46.84442)
m_absunemp	-30.16842 (18.62365)	-334.5622 (211.4615)
m_absown	-6.528078* (3.274087)	-55.7486* (31.73241)
_cons	1.081843***	7.95588
N	21	21
r2	0.4744	

* p<0.1, ** p<0.05, *** p<0.01

Table 3: Relationship between Country-Level Indicators and Conflict

The model-building process for the linear probability model with country-level indicators can be found in Table 4, where I add each indicator one-by-one into the model²⁹.

conflict	M1	M2	M3	M4	M5	M6
m_abscm	0.141 (2.84)	1.004 (3.09)	-0.264 (3.23)	-1.305 (3.17)	-0.458 (3.11)	0.052 (2.85)
m_abslit		2.651 (3.52)	2.037 (3.51)	-1.845 (4.19)	-2.221 (4.05)	-1.815 (3.70)
m_absuni			-13.091 (10.76)	-16.877 (10.62)	-19.245* (10.36)	-27.167** (10.27)
m_abselec				2.871 (1.84)	3.467* (1.82)	3.992** (1.68)
m_absunemp					-30.304 (20.39)	-30.168 (18.62)
m_absown						-6.528* (3.27)
constant	0.615*** (0.14)	0.392 (0.33)	0.632 (0.38)	0.682* (0.37)	0.808** (0.36)	1.082*** (0.36)
N	21	21	21	21	21	21
r2	0.000	0.031	0.108	0.226	0.325	0.474

* p<0.1, ** p<0.05, *** p<0.01

Table 4: Model-Building Process for LPM with Country-Level Indicators

²⁹ The mean variance inflation factor (VIF) for this model was 1.47. Individual VIFs ranged from 1.15 (unemployment) to 1.84 (literacy). This supports previous discussions on multicollinearity (see section 3.5), which suggest a moderate correlation between different kinds of inequality, but not enough to warrant concern, even using conservative thresholds to identify highly collinear variables (Glen 2015).

As Table 4 indicates, the effects of (average) group differences in terms of educational attainment, access to electricity, and home ownership strengthen across specifications as the other horizontal inequality indicators are added. Regarding access to electricity, the regressions estimate an increase in the likelihood of conflict as groups move farther from their country's national average, on average. As estimated by the linear probability model, the relationship is significant at the $p < 0.05$ level ($p=0.033$), which is notable given the quite low sample size. This is consistent with the going hypothesis that horizontal inequality positively correlates with conflict. However, average group distances from the national average in terms of educational attainment and home ownership are correlated with a *lower* likelihood of conflict. This unexpected result runs contrary to H1 ("Political, social, and economic horizontal inequality increase the likelihood of violent conflict.").

While not statistically significant, the unemployment indicator's estimated Beta coefficient is robust when it is included in Models 5 and 6, remaining roughly -30. This suggests that the effects of horizontal inequality in terms of unemployment are independent of the effects of other forms of horizontal inequality. By comparison, the negative effects of educational attainment on conflict increase (i.e., become more negative) as other horizontal inequality indicators are included, as indicated in Models 4, 5, and 6. In Model 6, where all social and economic horizontal inequality indicators are included, the educational attainment parameter is the smallest it has been throughout the model-building process. This suggests that horizontal inequality in terms of educational attainment might mitigate conflict in the presence of other horizontal inequality indicators.

One should be cautious when interpreting the estimated coefficients presented and discussed above, and throughout the rest of this section. As noted earlier in this section, values

for social and economic horizontal inequality measures are coded as proportions (e.g., 0.05) instead of percentages (e.g., 5). Therefore, interpretations of the estimated coefficients for these measures should be moved to the hundredths place (e.g., where an estimated Beta coefficient of 20 is interpreted as 0.2). To take an example, the estimated effect of the access to electricity indicator in Table 4 is 3.992. This means that a one-unit change in a country's average access to electricity inequality from the mean is estimated to increase the likelihood of conflict from its base probability of ~0.63 within the sample (see Table 1) by 0.039 to roughly 0.67.

Notably, Figure 4 and Table 3 (above) portray different relationships between some horizontal inequality measures and conflict. Literacy, unemployment, and home ownership seem to have positive relationships in Figure 4, which contains bivariate scatterplots depicting relationships between the social and economic horizontal inequality measures and the predicted probability of conflict (the continuous specification). In Table 3, which estimates the relationships between these measures and conflict (the binary specification), the opposite is true. While one might first assume the signs of these indicators switched as additional indicators were added into the regression, such is not the case for the unemployment or home ownership variables³⁰. When estimating a linear probability or logit model, these indicators appear to have a negative relationship with conflict, and this is so throughout the model-building process. This discrepancy is indicative of a lack of robustness and again highlights 1) the potential complexity of the relationship, 2) the opacity of what horizontal inequality looks like in unobserved years, and 3) the need to acquire further data to test the relationships of interest with additional, more comprehensive measures.

³⁰ Literacy, which is never significant in this model and always has an estimated coefficient of around 0, switches signs as indicators are added (access to electricity, in Model 4 of Table 4).

I also estimated the effects of group-level horizontal inequality indicators (as opposed to the country-level indicators in Tables 3 and 4) on conflict. Educational attainment, access to electricity, and home ownership (the statistically significant indicators from Tables 3 and 4) are similar in terms of their direction of effects and likewise statistically significant when group-level indicators are used to predict conflict in Table 5 (below). Table 5 re-estimates Table 3 with group-level indicators (M1), with time as a linear variable added (M2), and finally with all independent variables added (M3). It should be noted that, though the relationship between political horizontal inequality and conflict was estimated, there were no statistically significant results in any model, again running contrary to the stated hypothesis.

	LPM M1	LPM M2	LPM M3
conflict			
abshicm	0.070 (1.08)	-0.000 (1.11)	-0.620 (1.55)
abshilit	0.411 (1.12)	0.416 (1.12)	1.905 (1.33)
abshiuni	-19.129*** (4.63)	-18.417*** (5.29)	-29.774*** (7.54)
abshielec	1.569** (0.60)	1.543** (0.62)	1.624** (0.69)
abshiunemp	-5.270 (6.91)	-5.117 (6.98)	-1.359 (7.17)
abshiown	-2.887** (1.34)	-2.932** (1.36)	-3.573** (1.45)
_t		-0.002 (0.01)	0.003 (0.01)
polpwr=1			0.000 (.)
polpwr=2			0.208 (0.30)
polpwr=3			0.188 (0.28)
polpwr=4			0.326 (0.26)
cl=3			0.000 (.)
cl=4			-0.114 (0.18)
cl=5			0.093 (0.28)
cl=6			-0.068 (0.26)
loggdp			0.061 (0.08)
neighbor			0.219* (0.12)
size			0.002 (0.00)
constant	0.793*** (0.10)	0.840*** (0.19)	0.008 (0.52)
N	70	70	66
r2	0.245	0.246	0.412

* p<0.01, ** p<0.05, *** p<0.01

Table 5: Relationship between Group-Level Indicators and Conflict

The detected relationships fall out of statistical significance when time is incorporated into the model as a series of dummy variables and all controls are included (see Table 6 below)³¹. Because incorporating sixteen dummy variables uses up degrees of freedom, one might expect this. However, the variables that were statistically significant in other model specifications (educational attainment, access to electricity, and home ownership – see Tables 3-5) are still consistent in terms of their estimated Beta coefficients.

```

-----
                                LPM, i._t
-----
conflict
-----
abshicm                        -0.389
                                (0.60)
abshilit                        0.153
                                (0.49)
abshiuni                       -4.407
                                (3.53)
abshielec                      0.328
                                (0.26)
abshiunemp                    -0.240
                                (3.26)
abshiown                       -0.416
                                (0.70)
_t=1                           0.000
                                (.)
_t=10                          1.659***
                                (0.18)
_t=11                          -0.124
                                (0.32)
_t=17                          -1.877***
                                (0.38)
_t=20                          1.515***
                                (0.19)
_t=21                          -0.693*
                                (0.33)
_t=23                          1.119***
                                (0.18)
_t=24                          0.812***
                                (0.17)
_t=28                          -2.492***
                                (0.43)
_t=31                          0.265**
                                (0.09)
_t=32                          0.169
                                (0.10)
_t=35                          0.405
                                (0.21)
_t=37
_t=39                          -0.853**
                                (0.29)
_t=40                          0.000
                                (.)
_t=41                          0.000
                                (.)
polpwr=1                       0.000
                                (.)
polpwr=2                      -0.007
                                (0.11)
polpwr=3                      -0.030
                                (0.11)
polpwr=4                       0.003
                                (0.09)

```

³¹ Specifying time this way allowed only the effects of the years for which census data exists to be estimated, in case the unequal census intervals affects parameters (see section 4.2.2 for a fuller explanation).

c1=3	0.000 (.)
c1=4	-1.438*** (0.29)
c1=5	-1.561*** (0.32)
c1=6	-0.800*** (0.16)
loggdp	-0.339*** (0.06)
neighbor	0.283*** (0.06)
size	-0.000 (0.00)
constant	2.361*** (0.44)

N	66
r2	0.953

* p<0.1, ** p<0.05, *** p<0.01	

Table 6: Conflict and Group-level Indicators with Time as a Series of Dummies

Overall, these preliminary findings provide mixed evidence for hypotheses 1 and 2. They suggest the relationship between horizontal inequality and conflict is perhaps more complex than a constant increasing effect of horizontal inequality on the likelihood of conflict. Instead, they indicate that while horizontal inequality could be correlated with conflict, the kind of horizontal inequality present in a society might have implications for whether conflict is more or less likely. I address possible explanations for these findings, as well as their impact on theoretical orientations, at length in Chapter 5.

4.2.2 Horizontal Inequality and Group Involvement

I also estimated the relationship between horizontal inequality and group involvement in conflict. The results supported the idea that horizontal inequality could have a considerable effect not just on whether conflict happens, but also on its dynamics. Results suggested that, even when held to the strictest test (a random effects linear probability model), horizontal inequality seemed to have a relationship with group involvement. However, whether a group was more or less likely to engage depended upon the way in which they were unequal. This suggests that the relationship between horizontal inequality and group involvement (and, perhaps, other dynamics

of conflict) is potentially more complex than is currently understood, and further investigation with improved data is warranted.

There are 28 cultural groups represented in the data. The proportion of groups engaged in conflict peaks in the 1980s and declines after then. The proportion of groups engaged in conflict, both from 1971-2011 and exclusively within sample years, can be seen in Figure 6 below.

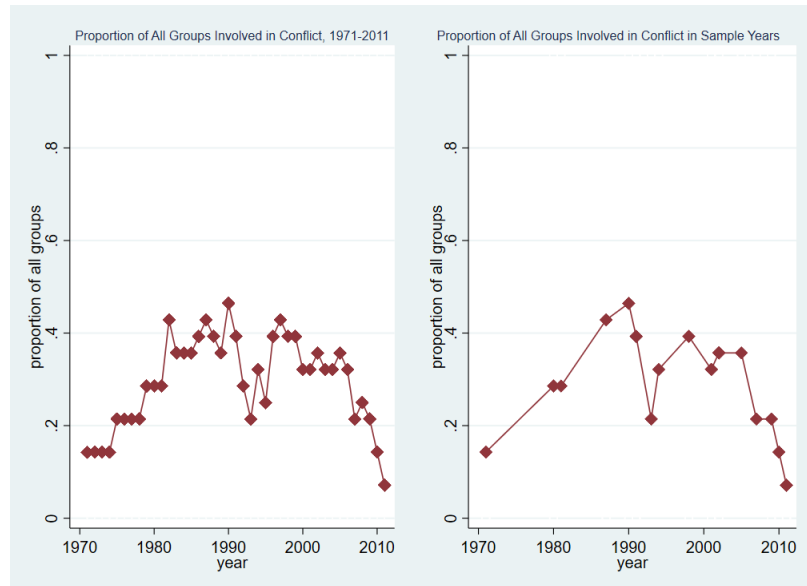


Figure 6: *Proportion of All Groups in Conflict* (Data source: Petterson et al. 2019; Gleditsch et al. 2002)

The average percentage of groups involved in conflict from 1971-2011 was roughly 29%, while the average in sample years was 38.6% (see Table 7).

	Mean within Sample (N=70)	SD within Sample (N=70)	Mean from 1971-2011 (N=1148)	SD from 1971-2011 (N=1148)
groupinv	.3857143 (.0585994)	0.4902782	.2952962 (.0134695)	.4563746

Table 7: *Percentage of Groups Involved in Conflict* (Data source: Petterson et al. 2019; Gleditsch et al. 2002)

Groups varied considerably in the proportion of sample years they engaged in conflict. Some groups engaged in conflict all the time, others never, and others still, a fraction of the time. The proportion of time each group was involved in a conflict in sample years differed somewhat

from the proportion of time they were engaged in conflict from 1971-2011. Illustrations of these proportions can be see below, in Figure 7.

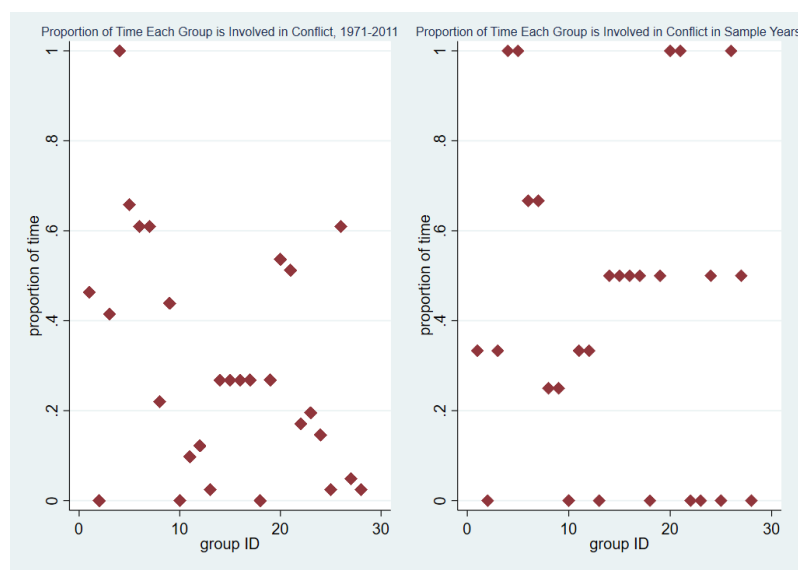


Figure 7: Proportion of Time Each Group is in Conflict (Data source: Petterson et al. 2019; Gleditsch et al. 2002)

Since there was within-group variation on group involvement, it was appropriate to build up to a random effects model to estimate the relationship between horizontal inequality and group involvement. Because of the exploratory nature of using these data to predict group involvement, I estimated the relationship using different model specifications. Doing so helped determine how robust the estimated effects were against various specifications.

Bivariate scatterplots of social and economic horizontal inequality measures and the predicted probability of group involvement can be found in Figure 7 below. Some correlations can be seen quite plainly in this figure (for example, the notably positive relationship between unequal access to electricity and the predicted probability of group involvement); however, the same caveat from the previous section must be kept in mind when interpreting this graph. Namely, models depicted later in this section estimate the relationship between horizontal inequality and group involvement *in census years*, where these bivariate graphs use the predicted

probability of group involvement over the 41 years from 1971-2011. Because of missing data, these relationships may not necessarily be reflective of each other, and the need for improved data is again made clear.

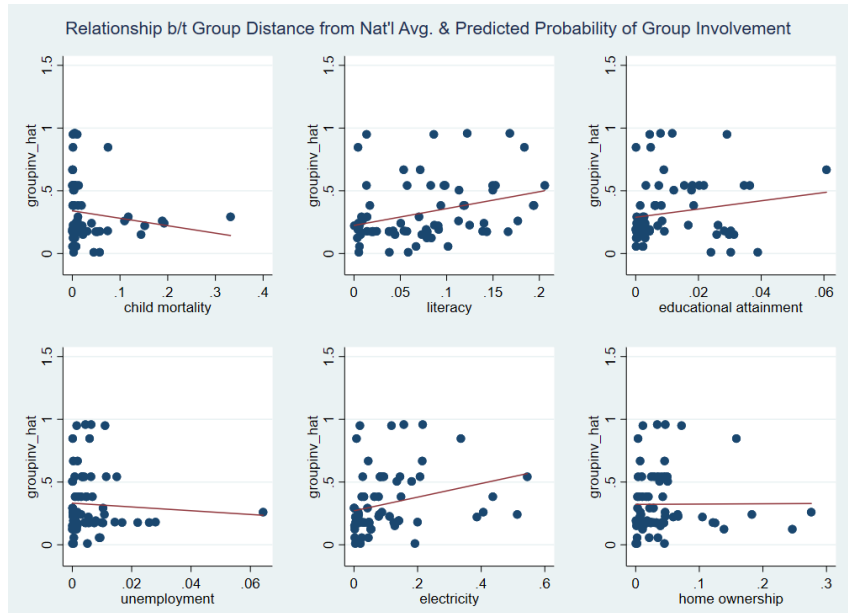


Figure 8: Relationship between Grouped Inequality and Predicted Probability of Group Involvement (Data source: aggregated sources – see section 3.2)

I estimated the relationships between horizontal inequality and group involvement using various model specifications, beginning with the simplest and building up in complexity. As can be seen in Table 8 (below), I first estimate the relationship between solely the social and economic horizontal inequality indicators and group involvement using simple linear probability and logit models. Grouped inequality in terms of educational attainment, access to electricity, and home ownership display statistically significant relationships with group involvement in conflict, which is notable given the sample size (N=70). According to these initial two models, as groups move farther from the national average (i.e., more unequal) in terms of their access to electricity, they are more likely to become involved in conflict. However, as groups move farther

from the national average in terms of their educational attainment and home ownership, they are less likely to become involved in conflict.

I then added the trend variable to these regressions using two different specifications. First, I included time as a linear trend variable, as is standard practice to control for the possibility that groups become more or less involved in conflict as a function of time. However, countries conduct their censuses at different time intervals (see Appendix A for a list of years that each country conducted a census included in this sample). Bangladesh, for example, conducts its census every ten years, whereas for Colombia, there is a twelve-year gap between censuses. It is unclear how these unequal time intervals might affect parameter estimates. Therefore, I also include time as a series of dummy variables for each data year (i.e., year in which data were available). Years received a 1 if they were a year in which data were available for that year or a 0 otherwise. This allowed only the effects of data years to be estimated (see Table 8). In terms of their effect sizes, grouped inequality in educational attainment, access to electricity, and home ownership remain fairly stable across the model specifications. When time is linear, the educational attainment indicator falls out of significance, and when time is dummied, only home ownership maintains statistical significance. This is unsurprising given that dummied time uses degrees of freedom. It is therefore helpful to take into consideration the general trend of these variables, recognizing the implication that these potential relationships warrant further attention.

groupinv	LPM	Logit	LPM + _t	LPM + i._t
abshicm	-0.537 (1.15)	-1.031 (5.87)	-0.669 (1.19)	0.265 (1.08)
abshilit	0.903 (1.19)	4.762 (5.58)	0.912 (1.20)	0.094 (1.15)
abshiuni	-8.245* (4.94)	-41.931* (25.47)	-6.913 (5.65)	-9.093 (5.61)
abshielec	1.526** (0.65)	9.076** (3.97)	1.476** (0.66)	0.414 (0.65)
abshiunemp	-4.390 (7.38)	-32.141 (50.20)	-4.103 (7.45)	8.453 (7.82)
abshiown	-3.368** (1.43)	-26.059** (12.14)	-3.454** (1.45)	-2.738* (1.62)
_t			-0.003 (0.01)	
_t=1				0.000 (.)
_t=10				0.395 (0.35)
_t=11				1.022** (0.39)
_t=17				-0.042 (0.34)
_t=20				0.381 (0.35)
_t=21				0.396 (0.28)
_t=23				1.044*** (0.34)
_t=24				1.138*** (0.40)
_t=28				0.307 (0.38)
_t=31				0.617** (0.30)
_t=32				0.251 (0.28)
_t=35				1.140*** (0.38)
_t=37				1.281*** (0.42)
_t=39				0.593* (0.35)
_t=40				0.009 (0.35)
_t=41				0.159 (0.31)
constant	0.438*** (0.11)	-0.131 (0.51)	0.525** (0.20)	0.039 (0.27)
N	70	70	70	70
r2	0.1505		0.1539	0.5637

* p<0.1, ** p<0.05, *** p<0.01

Table 8: Relationship between Horizontal Inequality and Group Involvement without Controls

Table 9 (below) contains model specifications in which all independent variables are included, again beginning with the simplest specifications and increasing in complexity to the

strictest test. After estimating a simple linear probability model with all independent variables included, I specified the model by clustering standard errors by group (referred to as “ID” in Table 9) and then by country. I find clustering by country to be the most theoretically appropriate choice, and therefore cluster by country in the random effects models, as well. However, I included the models clustered by group 1) to be able to examine how this affects standard errors and 2) because there is still some debate as to which variables to cluster by, especially depending on the research question.

I used guidance by Abadie et al. (2017), further discussed by McKenzie (2017), in my decision to make clustering by country the default. According to these authors, if there is a cohort from a locale (e.g., a village), it is most appropriate to cluster that locale (in this example, the village). This is because there are likely villages that the researcher is interested in drawing conclusions about beyond the sample. Likewise, there are not likely cohorts about which the researcher is interested in drawing conclusions that are excluded from the sample. In the current analysis, there are not culturally-relevant groups that have been excluded from the analysis, nor are there culturally-relevant groups outside of the sample about which I am interested in drawing conclusions. Instead, there are countries in the population beyond my sample, and I am interested in drawing conclusions about how horizontal inequality relates to conflict dynamics within them.

The fourth model specification in Table 9 is a random effects linear probability model, clustered by country. I estimated it to account for the panel data and allow Beta coefficients to vary. Because the panel “group” constitutes a subset of the larger population, the group effect can be thought of as random, as opposed to fixed (Taylor 2019). However, to ensure that this was an appropriate theoretical choice, I estimated a fixed effects model, as well, and ran a Wu-Hausman test, which confirmed the random effects model as the more appropriate specification.

The next four models are the same as the initial four, except time is specified as a series of dummy variables. This is again to allow for the possibility that the unequal time intervals of the censuses affect parameter estimates. Including time as a series of dummy variables allows for fuller comparison. As can be noted in Table 9, dummifying the time variable also added sufficient variation to calculate an ICC on the random effects model. While there is between- and within-group variation, it is still fairly small.

Of the horizontal inequality indicators, grouped inequality in terms of educational attainment and home ownership display a statistically significant relationship with group involvement in conflict when time is linear (column 4 of Table 9, “Random Effects”), and unemployment displays one when time is dummied (column 8 of Table 9, “Random Effects, i_t”). Access to electricity is statistically significant in the basic linear probability model and in the linear probability model clustered by cultural group (columns 1 and 2); though, it is insignificant in the remainders of the models. By comparison, the unemployment indicator becomes statistically significant in later models, when standard errors are clustered by country and time is dummied.

As groups move farther from the national average in their access to electricity and in their unemployment rates, they become more likely to engage in conflict. While the access to electricity indicator is only statistically significant in the first two specifications, it is worth noting, given the limited sample size and minimal degrees of freedom in later specifications. Because some variables are significant with small sample sizes, it is unclear whether these effects would remain significant with a larger sample size. I do *not*, therefore, attempt to argue that these effects would be statistically significant with additional data. Rather, I am noting that variables fall in and out of significance with these data, and additional data would help elucidate

which indicators (if any) seem to have more robust relationships, whether these effects are independent of other variables, etc. (see Chapter 5 for further discussion). If these relationships hold, they provide support for H3 (“Groups that experience the most extreme horizontal inequality are most likely to engage in conflict.”).

Grouped inequality in terms of educational attainment and home ownership appear to have an inverse relationship with group involvement, and comparably large effect sizes across specifications. That is, as groups get farther from their national average in terms of their educational attainment and home ownership rates, they become less likely to engage in conflict. These results do not support H3. Possible reasons for these relationships, if they exist, are discussed in Chapter 5, “Research Implications.”

Political horizontal inequality did not have a notable relationship with group involvement, either in terms of substantive or statistical significance. When time is linear, it is estimated to have a U-curve; when time is dummied, it is inverse, meaning that more marginalized groups are less likely to engage in conflict. However, the effect sizes are considerably small, and it never reaches statistical significance. This is not to suggest that political horizontal inequality is unimportant to group involvement; however, it is not evidenced in these specifications with this (limited) data.

Regarding control variables, government repression and logged GDP have relationships that are consistent with findings in the extant literature. Results indicate a generally decreasing likelihood of group involvement as governments progress from moderate to extreme measures of repression. This is consistent with Gurr’s (1993) and Muller’s (1985) findings, discussed in Chapter 2.3.2 (“Governmental Structure”), which demonstrate that countries with moderate repression experience conflict at higher rates than those with very severe or very little

repression³². Increases in a country's logged GDP are also associated with a decrease in group engagement in violence, which indicates that people engage in violent conflict less as their society's economy becomes more developed. This is unsurprising given economic development's consistent association with conflict (or lack thereof) (Hegre 2002; Ostby 2008; Cilliers & Schunemann 2013).

Notably, these findings do not evidence group size to have a statistically significant relationship with group involvement in conflict. Its substantive effect is also quite small. This runs contradictory to theoretical understandings noted in some other studies (e.g., Ostby 2008; Stewart et al. 2010), where the authors propose measuring horizontal inequality between the two largest or most politically competitive groups, as inequality between these groups might be most likely to cause conflict. The results of the current analysis, however, suggest that, if horizontal inequality has an effect on group involvement, it is present regardless of group size. This notion boosts the idea that horizontal inequality itself is informative to whether a country experiences conflict.

³² As noted previously, this is because governments with very little repression allow for legitimate means of social change, such as protest or voting, thereby discouraging people from violent means of social change. Likewise, countries that employ very severe forms of repression limit civil liberties to organize and mobilize, rendering rebellion infeasible. Moderately repressive regimes offer too few legitimate means to alleviate discontent, but still allow for enough mobilization to organize. Refer to Chapter 2.3.2 ("Governmental Structure") for a fuller discussion.

groupinv	LPM	LPM cluster(ID)	LPM cluster(ctry)	Random Effects	LPM i_t	LPM cluster(ID), i_t	LPM cluster(ctry), i_t	Random Effects, i_t
abshicm	0.052 (1.64)	0.052 (1.87)	0.052 (1.96)	0.052 (1.96)	0.535 (1.78)	0.535 (1.38)	0.535 (0.95)	0.404 (0.86)
abshilit	1.923 (1.41)	1.923 (1.59)	1.923 (1.35)	1.923 (1.35)	-0.356 (1.45)	-0.356 (1.71)	-0.356 (1.67)	-0.208 (1.54)
abshiuni	-9.078 (7.98)	-9.078 (7.97)	-9.078* (4.15)	-9.078** (4.15)	-2.605 (10.41)	-2.605 (6.77)	-2.605 (4.75)	-3.129 (4.46)
abshielec	1.289* (0.73)	1.289* (0.65)	1.289 (0.85)	1.289 (0.85)	0.096 (0.77)	0.096 (0.70)	0.096 (0.94)	0.065 (0.93)
abshiunemp	1.794 (7.59)	1.794 (6.35)	1.794 (3.16)	1.794 (3.16)	9.826 (9.61)	9.826 (6.80)	9.826* (4.76)	9.866** (4.15)
abshiown	-4.127*** (1.53)	-4.127*** (1.45)	-4.127** (1.35)	-4.127*** (1.35)	-2.443 (2.05)	-2.443 (2.42)	-2.443 (2.54)	-2.281 (2.31)
polpwr=1	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
polpwr=2	-0.052 (0.31)	-0.052 (0.17)	-0.052 (0.11)	-0.052 (0.11)	-0.110 (0.32)	-0.110 (0.15)	-0.110 (0.10)	-0.086 (0.10)
polpwr=3	-0.174 (0.29)	-0.174 (0.26)	-0.174 (0.23)	-0.174 (0.23)	-0.046 (0.32)	-0.046 (0.23)	-0.046 (0.13)	-0.053 (0.13)
polpwr=4	0.058 (0.27)	0.058 (0.21)	0.058 (0.13)	0.058 (0.13)	-0.018 (0.27)	-0.018 (0.09)	-0.018 (0.05)	-0.010 (0.06)
cl=3	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
cl=4	-0.423** (0.19)	-0.423* (0.21)	-0.423* (0.19)	-0.423** (0.19)	-0.343 (0.85)	-0.343 (0.57)	-0.343 (0.30)	-0.370 (0.26)
cl=5	-0.162 (0.30)	-0.162 (0.24)	-0.162 (0.28)	-0.162 (0.28)	-0.766 (0.96)	-0.766 (0.71)	-0.766* (0.37)	-0.799** (0.34)
cl=6	-0.396 (0.28)	-0.396 (0.26)	-0.396** (0.16)	-0.396** (0.16)	-0.638 (0.46)	-0.638 (0.49)	-0.638** (0.25)	-0.652*** (0.22)
loggdgdp	-0.086 (0.08)	-0.086 (0.07)	-0.086 (0.06)	-0.086 (0.06)	-0.198 (0.19)	-0.198* (0.12)	-0.198*** (0.04)	-0.196*** (0.04)
neighbor	0.021 (0.13)	0.021 (0.11)	0.021 (0.11)	0.021 (0.11)	-0.065 (0.18)	-0.065 (0.11)	-0.065 (0.06)	-0.054 (0.05)
size	0.005 (0.00)	0.005 (0.00)	0.005 (0.00)	0.005 (0.00)	0.002 (0.00)	0.002 (0.00)	0.002 (0.00)	0.002 (0.00)
_t	0.007 (0.01)	0.007 (0.01)	0.007 (0.01)	0.007 (0.01)				
_t=1					0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
_t=10					0.843 (0.54)	0.843** (0.37)	0.843*** (0.12)	0.845*** (0.11)
_t=11					0.925 (0.95)	0.925 (0.93)	0.925* (0.46)	0.898** (0.41)
_t=17					-0.305 (1.11)	-0.305 (1.00)	-0.305 (0.39)	-0.333 (0.35)
_t=20					0.944 (0.57)	0.944* (0.50)	0.944*** (0.12)	0.939*** (0.11)
_t=21					0.144 (0.99)	0.144 (0.80)	0.144 (0.38)	0.117 (0.34)
_t=23					1.256** (0.54)	1.256*** (0.29)	1.256*** (0.27)	1.267*** (0.27)
_t=24					1.264** (0.51)	1.264*** (0.22)	1.264*** (0.14)	1.246*** (0.14)
_t=28					-0.515 (1.26)	-0.515 (0.84)	-0.515 (0.59)	-0.561 (0.53)
_t=31					0.308 (0.25)	0.308 (0.21)	0.308 (0.35)	0.306 (0.35)
_t=32					-0.106 (0.29)	-0.106 (0.21)	-0.106 (0.22)	-0.117 (0.22)
_t=35					0.828 (0.62)	0.828* (0.41)	0.828*** (0.23)	0.797*** (0.21)
_t=39					0.003 (0.84)	0.003 (0.45)	0.003 (0.21)	-0.014 (0.18)
_t=40					0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
_t=41					0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)

constant	0.542 (0.55)	0.542 (0.41)	0.542 (0.50)	0.542 (0.50)	1.225 (1.28)	1.225 (0.80)	1.225*** (0.30)	1.227*** (0.27)
N	66	66	66	66	66	66	66	66
r2	0.3079	0.3079	0.3079	0.3079	0.5701	0.5701	0.5701	0.5696
sigma_u				0				0.1652
sigma_e				0.4783				0.4051
rho				0				0.1426

* p<0.1, ** p<0.05, *** p<0.01

Table 9: Relationship between Horizontal Inequality and Group Involvement with Controls

Overall, these results again find mixed support for H3. While they provide evidence for the notion that horizontal inequality might affect how conflicts develop and who becomes involved, they also caution against sweeping generalizations about the relationship between horizontal inequality and conflict dynamics. According to these results, the type of inequality matters. Possible explanations and contributions to theoretical orientations are further discussed in Chapter 5.

4.3 Sensitivity Analyses

Until this point, I have described the relationships illustrated in Tables 1 through 9, examining the relationship between horizontal inequality and 1) conflict occurrence and 2) group involvement. However, I also conducted sensitivity analyses to further elucidate the nature of these relationships.

I am primarily interested in exploring the relationship between horizontal inequality and conflict dynamics by determining whether these variables appear to be correlated. I am not primarily interested in either comparing the relationships of horizontal and vertical inequality on conflict, or in dispelling the importance of vertical inequality on conflict. However, I still re-estimated linear probability models including the standard measure of vertical inequality, the GINI coefficient, to be able to note whether there were any differences in effect on the likelihood of conflict³³. This is similar in practice to former studies (e.g., Ostby 2008; Cederman et al. 2013). Tables 10 and 11 in Appendix C display the models. Table 10 re-estimates the linear probability model of Tables 3 and 4 (where country-level indicators are used to predict conflict as the dependent variable) with the GINI variable included. Table 11 re-estimates linear

³³ Correlation coefficients between the GINI coefficient and the horizontal inequality measures can be found in Figure 10 of Appendix B.

probability models that include group involvement as the dependent variable (clustering by ID and country, and incorporating both specifications of time).

Regarding Table 10, the addition of the GINI coefficient renders every social and economic horizontal inequality indicator statistically significant. The GINI coefficient itself is also statistically significant. This suggests that the GINI coefficient was a negative confounder for the horizontal inequality indicators, and that including it made more apparent the relationship between horizontal inequality and conflict. It is also notable that, in Table 10, the effect size of the GINI coefficient is considerably small compared to each of the horizontal inequality indicators. This sensitivity analysis further supports the idea horizontal inequality might affect the likelihood of conflict in a way that is particularly informative, and that the way in which it does so depends upon how that inequality manifests within the society.

Table 11 depicts several linear probability models, specified with different clustered standard errors and time specifications. Across these models, grouped inequality in terms of educational attainment, unemployment, and home ownership remain substantively consistent with the main analysis results, shifting in and out of significance depending upon the specification. In this table, the GINI coefficient is statistically significant when time is linear and becomes insignificant when time is dummied. In every model specification, its detected effect size is smaller than those of the horizontal inequality indicators. This sensitivity analysis also supports the idea that horizontal inequality is informative for understanding conflict dynamics. It also suggests that further attention should be paid to understanding inequality, how it manifests in a society, and how that relates to conflict dynamics.

The GINI coefficient's detected effect size is similar across model specifications in both Tables 10 and 11. The models show that, as the GINI coefficient approaches 100 (perfect

inequality), the likelihood of conflict increases. This fits into the literature's (albeit mixed) findings regarding the relationship between inequality and conflict. Namely, it is that, as inequality increases within a society, conflict becomes more likely.

Sensitivity analyses support the notion that inequality and the way in which it manifests in a society is important for determining conflict and its dynamics. They suggest that continued investigation into the potential complexities of these relationships can be useful for better understanding conflicts and how they develop.

CHAPTER V: RESEARCH IMPLICATIONS

5.1 Discussion of Results

The results indicate evidence that horizontal inequality is related to both conflict occurrence and group involvement in conflict. They also indicate that different forms of horizontal inequality could have differential effects. The complex relationships between horizontal inequality and conflict dynamics demonstrated in these analyses provide several implications for our theoretical understanding of horizontal inequality and violent conflict, if they hold. These results also prompt opportunity for discussion on the steps researchers and policymakers can take to improve our ability to study these relationships further and address both inequality and conflict in the applied world.

My results suggest that there is reason to continue exploring new indicators, data sources, and ways of understanding the relationship between horizontal inequality and conflict dynamics. Still, there are drawbacks and benefits to the analyses conducted with these data, discussed below, and results must be considered tentative. Results must also be taken as preliminary given the limited amount of data available, which has led to a small sample size and increased the likelihood of Type II error (while simultaneously providing considerable evidence for indicators that did appear to have a relationship). Current explorations demonstrate that there could be an effect, and it could be more complex than we understand.

5.1.1 Drawbacks and Directions for Future Research

One drawback of this type of analysis is that it relies on relatively scarce census data from various countries. As noted in section 3.4, “Model Specifications,” these countries each conduct censuses at different times, leading to some irregularity in intervals. For example, while Bangladesh’s censuses span every ten years, Uganda’s span eleven. One way to make time

intervals between observation years equal would have been to interpolate what inequality looks like every ten years for countries without ten-year censuses, but this method would then suffer similar drawbacks to other studies in which interpolation was used³⁴. If this path were chosen, I would not be able to describe the relationship between horizontal inequality and conflict as it has been observed. I also would have to rely on assumptions about the nature of horizontal inequality in missing years, which introduces bias, particularly given that not much is understood about horizontal inequality trends over time in the sample.

However, by not interpolating, I have somewhat irregular time intervals. There is thus a tradeoff here between bias and efficiency. In part because there are studies that take advantage of interpolating methods, which favor efficiency in the bias-efficiency tradeoff if the imputations are biased, I elected to supplement these studies by analyzing relationships as they have been observed. It should be noted, as discussed in section 3.4, “Model Specifications,” that there does not appear to be any systematic cause of the uneven time intervals that is relevant to the nature of the relationship between horizontal inequality and conflict. That is, the fact that Bangladesh conducts its censuses every ten years, and Uganda does every eleven, does not seem to be as a result of the presence or kind of inequality or conflict. Therefore, it is worth considering the impact of this irregularity. Still, I have also included time as a dummy variable to further test the robustness of estimates and have a fuller understanding of the potential relationships between horizontal inequality and the outcomes of interest.

Another drawback to consider is the possibility of simultaneity, as discussed in section 3.5, “Methodological Limitations.” Statistically, it is difficult with these data to disentangle

³⁴ It should be noted that, when time is linear, the analysis is subject to automatic non-response weighting. Including time as a dummy variable helps mitigate this, despite using additional degrees of freedom, and tests the robustness of parameter estimates.

whether inequality or conflict is the precursor to the other, especially because of the considerable restriction present in this analysis of studying them in the same year. In this case, inequality present in one year must theoretically lead to conflict in that same year, assuming causality. To some degree, examining the relationship between horizontal inequality and the predicted probability of conflict (see Figure 3) helps ease this restriction. This is because the predicted probability assesses how likely conflict *generally* is over a period of time, easing the restriction of inequality leading to conflict in one particular year and further elucidating potential correlations. Likewise, as discussed in section 3.5, “Methodological Limitations,” there is theoretical reason to believe that horizontal inequality is causative of conflict. However, this drawback should still be noted. Estimations of the relationship hold the restriction. This is despite the fact that it is reasonable to believe the potential effects of inequality could take time to settle in.

The preliminary analyses presented here are intended to assess whether there appear to be empirical regularities and correlations between the indicators calculated and the outcomes of interest, not to establish causality. Future researchers might consider implementing lag variables of conflict to ease the restriction of this analysis (though, they would have to somewhat arbitrarily decide the time period it would take for inequality to “cause” conflict, be that five years, three years, etc.). They might also consider operationalizing their conflict outcome as the number of years within a given period of time (e.g., five years, three years, etc.) that have experienced conflict. Future researchers might also continue to use continuous predicted probabilities as their outcome of choice, or employ other methods to ease this restriction and further the discourse. Statistically, it is difficult to establish causality in this relationship, and that drawback is still present in the current study. Instead, it evidences that acquiring additional data,

incorporating various indicators of horizontal inequality, and exploring the relationships between horizontal inequality and additional aspects of violent conflict would be beneficial to the field.

Relatedly, the analyses presented in the current study are intended to assess whether the presence of inequality between groups itself (i.e., the absolute value of a group's distance from the national average) is associated with a change in conflict occurrence and group involvement (see section 3.2.2 for additional explanation of the rationale for using absolute values). However, it is possible that this relationship is further affected by other aspects of the inequality, such as whether the groups are better- or worse-off. On this front, I have conducted preliminary supplementary analyses examining horizontal inequality's relationship with conflict occurrence and group involvement. In these analyses, I estimate some of the models using the true values of horizontal inequality measures as opposed to their absolute values (see Tables 12 and 13 in Appendix C).

Interpretation of these models' coefficients is not as straightforward since values both above and below 0 mean that a group is moving farther away from the national average. It should be kept in mind that, when examining the supplementary analyses in Appendix C, the coefficients indicate the estimated change in the dependent variable as the measure of dispersion increases on the independent variable. For example, if an estimated coefficient is 5, then the likelihood of the dependent variable increases as groups become more unequal (regardless of whether they are trending above or below the national average). If a coefficient is -5, the likelihood of the dependent variable decreases as groups become more unequal. Coefficients themselves do not indicate differential effects between being better- or worse-off.

It is notable that, without taking the absolute value, grouped differences in literacy appear to positively correlate with group involvement. Likewise, home ownership appears to positively

(as opposed to negatively, as in the main analyses) correlate with the outcome variables. Taking the absolute value of horizontal inequality measures facilitates interpreting meaningful results and gets at the relationship between grouped inequality and conflict dynamics, but it truncates data and makes distinguishing differential effects between better- and worse-off groups infeasible. Examining how horizontal inequality relates to conflict and its dynamics with actual values, and delving further into how additional factors about the inequality manifests, such as whether a group is better- or worse-off, is a viable next step for researchers to take. Preliminary analyses in Appendix C suggest that such explorations might be fruitful.

There are several other takeaways from this dissertation that can inform the direction of future studies. This research project points to the idea that it is useful to include various indicators of economic, social, and political horizontal inequality, as these various indicators might have differential effects, even if they are all, for example, forms of economic inequality. Former researchers (e.g., Stewart 2008; Ostby 2008; Cederman et al. 2013) have suggested that there could be differential effects between social, economic, and political horizontal inequality; however, this analysis suggests that, even within these categories, different manifestations of horizontal inequality might have differential effects. It is important to pursue additional data and indicators from various data sources, as this might elucidate the relationship between horizontal inequality and conflict dynamics (see section 5.3 for a discussion of what additional data might ideally look like).

Additional data and data sources could also help researchers test whether the effects of certain inequalities are independent, or if they have given effects in the presence of other factors. As noted in sections 2.2 and 3.2.3, current understandings suggest that inequality does not operate in a vacuum. Discussions in Chapter 4 bolster this idea, indicating that some forms of

inequality seem to have robust and independent estimated effects, while others seem to strengthen in their effect as other variables are included in models. If researchers continue to explore these relationships with additional data, they will be able to better understand to what extent the effects of horizontal inequality are independent and to what extent the effects change in the presence of other variables.

Pursuing additional data will help researchers better understand which horizontal inequality indicators have a robust relationship with conflict dynamics, as well, and what the magnitude of those effects are. It is unclear whether the effects discussed in Chapter 4 will hold as sample size increases. Additional observation years would help elucidate this, as results would likely coalesce as more observations are included.

Finally, it seems important that researchers examine the effect of horizontal inequality on not only whether conflict initially occurs, but also the dynamics of that conflict. While there are papers examining ethnic cleavages and the duration of a conflict (e.g., Wucherpfennig et al. 2012) or its intensity (Murshed and Gates 2005), there is a need for further research that expands our understanding of how horizontal inequality affects other conflict dynamics. Results of this analysis suggest that such research could help inform conclusions on conflicts and their developments. These results, though preliminary, indicate that it is worth continuing down the path of pursuing additional data and expanding the kinds of indicators included in cross-cultural analyses of horizontal inequality and conflict dynamics.

Despite analytical limitations, I use this dissertation to build upon the going literature on inequality and mass violent conflict. I expand dependent variables to include not just initial conflict onset, but whether conflict happens in a given year and which groups become involved in conflict. I also use census data to include more granular measurements of economic and social

horizontal inequality than have been included in many previous cross-country analyses, and examine their observed effects on conflict occurrence and group involvement. I note that these could be informative if included as unique variables in an analysis, rather than as a composite score, because of their potentially complex effects. Finally, I look to further paths for studying horizontal inequality indicators and noting their possibly differential effects. Though results are preliminary, they point to the usefulness of expanding data on and indicators of horizontal inequality and its potential outcomes. They also provide several potential theoretical and methodological implications, detailed below.

5.2 Implications for Theory

The results in Chapter 4 can help contribute to the discourse on conflict determinants. Thus far, the going theories of conflict and inequality (discussed at length in Chapter 2) have asserted that, as horizontal inequality increases, the likelihood of conflict increases (Murshed and Gates 2005; Mancini 2008; Ostby 2008; Stewart et al. 2010; Cederman et al. 2013). Some of these theorists (e.g., Stewart et al. 2008; Cederman et al. 2013) have posited that social and political horizontal inequality have a larger effect on conflict than economic horizontal inequality, since political and social inequalities often have *de jure* support and follow group lines more explicitly than economic inequalities (see Chapter 2 for a fuller discussion). These theories have dominated the area since its inception. However, the results presented in the current study indicate that there could be a more complex relationship between horizontal inequality and conflict than understood.

One notable takeaway from this study is that horizontal inequality might be positively or inversely correlated with conflict dynamics depending upon the specific way in which a given society is unequal. To summarize the key findings of Chapter 4, horizontal inequality might be

associated with an increase in the probability of conflict if a society is unequal in terms of its access to electricity. Horizontal inequality is also, however, associated with a *decrease* in the probability of conflict, if the society is unequal in terms of its educational attainment and/or home ownership rates. Likewise, groups seem more likely to become involved in conflict as they become more unequal in their access to electricity and unemployment rates. They seem less likely to engage in conflict as they become more unequal in their educational attainment and home ownership rates. There are several possible explanations as to why this might be.

First, it could be that horizontal inequality increases the likelihood of civil conflict when the society is unequal in a way that prevents the attainment of a minimum threshold for a good quality of life. If someone faces a lack of access to electricity, for example, that would negatively impact their quality of life more significantly than whether they owned the home they lived in. Likewise, if someone were unemployed, they might be less able to afford basic goods and services than if they did not have a complete schooling, or a university degree. Groups, therefore, could be satisfied enough to eschew violence when they have attained benefits like access to electricity and employment, even without home ownership or significant educational attainment. This might help account for why certain grouped inequalities are positively correlated with conflict and group involvement, while others are inversely correlated.

It could also be that horizontal inequality increases the likelihood of civil conflict, unless one group is so much wealthier or more educated that a country experiences “brain drain,” leaving behind such a small group that remaining inequality is largely imperceptible by the population. It is important to keep in mind that, in this study, I did not distinguish between whether a group was above or below national average – just how unequal they were. The results,

therefore, estimate relationships between inequality in the most literal sense – how far a group is from average – and conflict. This includes groups that are far above average, as well.

The findings could, therefore, be attributable to groups with very high educational attainment and home ownership rates also being more likely to be considerably wealthy (as opposed to groups with average employment rates or with access to basic electricity, which are not as strongly dependent upon the attainment of considerable wealth). At the first sign of instability, therefore, which might generally occur as a result of horizontal inequality, large swaths of advantaged groups might flee the country, as they could afford to emigrate to more stable countries³⁵. The result would be a fairly small remaining group of well-off individuals, which makes inequality less noticeable on the ground and could lessen people's perceptions of high inequality. This phenomenon would look like any standard "brain drain," where very highly educated populations from peripheral and semi-peripheral countries leave those countries, actually decreasing its average educational attainment rate (Wong and Yip 1999). In this case (and similar to brain drain), well-off, highly-educated groups could leave countries as they destabilize, making conflict appear *less* likely as inequality increases, despite that the inequality itself was destabilizing. This is consistent with the fact that having disparities in electricity is associated with more conflict: having access to electricity would not necessarily be associated with very wealthy groups (even if relatively well-off in the given society) who would be able to flee at the first sign of instability.

It could also be that home ownership is a proxy for housing stability, and low housing stability indicates that people are moving more often and are less able to mobilize effectively.

³⁵ Examples of high-skilled, highly educated human capital fleeing as civil conflict ramps up can be seen in countries such as Liberia, where widespread instability and war-related deaths exacerbated human capital flight (Barclay 2002).

Lee et al. (1994) found that homeowners are less likely to move than home renters. Perhaps groups with very low home ownership rates are more mobile, and less able to organize with other members of their group as one cohesive unit. This would lessen the potential for overall conflict.

The above explanations might help us understand why horizontal inequality seems to have different effects depending upon which factor(s) in a society is/are unequal. Still, these explanations are speculative: it would be inaccurate to suggest that researchers understand precisely why horizontal inequality might affect conflict in the way that it appears to. While other scholars have found evidence that horizontal inequality's effects differ depending upon whether that inequality is social, economic, or political (e.g., Stewart 2002; Ostby 2008; Cederman et al. 2013 – see Chapter 2 for a fuller discussion), few researchers have explored whether effects of horizontal inequality differ at this level of granularity (e.g., access to electricity versus home ownership). Because these results are novel in that different inequality indicators seem to have different effects on conflict occurrence and group involvement, there is not much theoretical understanding to identify why these differences might exist. Researchers should look to corroborating these results and exploring possible theoretical explanations further in future studies.

Perhaps the clearest theoretical implication of these results, if they hold, is that the type of horizontal inequality present in a society can change how likely that society is to experience conflict. To better understand, predict, and prevent conflict, it is important to ascertain which inequalities might help cause conflict and why. The previous literature on inequality and conflict, as well as this study, have demonstrated that it is not *just* inequality that causes conflict, but rather, the type of inequality present that helps determine whether there is conflict. Horizontal

inequality is more informative to conflict than vertical inequality, as discussed extensively in Chapter 2 and again demonstrated in Chapter 4. In the same manner, the results of this study indicate that it is not just *any* horizontal inequality that affects conflict and its dynamics. This takeaway is pivotal as researchers think both theoretically and policy-wise about why violence happens, which kinds of inequalities policymakers should focus on, and which policies decision-makers should enact to preserve stability and peace. Recognizing the possibility of the complex relationship demonstrated in this study is important to these discussions.

Notably, I did not find a clear distinction between horizontal inequalities' effects in terms of whether they were social, economic, or political. Some of the literature suggests that economic horizontal inequality matters least (e.g., Stewart 2002; Ostby 2008; Cederman et al. 2013 – see Chapter 2 for a fuller discussion); though, I do not find statistical support for the role of political horizontal inequality, which was insignificant. Political horizontal inequality also typically had a smaller effect size on conflict than the economic or social horizontal inequality indicators did. It is also notable that, of the economic and social horizontal inequality indicators inversely associated with conflict occurrence and/or group involvement, one (educational attainment) was an indicator of social horizontal inequality and one (home ownership rates) was an indicator of economic horizontal inequality. Overall, these results suggest that it might not be about an inequality being social, economic, or political *per se*, but rather something else about the nature of the inequality (e.g., whether the inequality threatens a minimum quality of life, whether the inequality allows for brain drain, etc.) determining whether conflict is more likely and which groups become involved.

Examining these results can help us further understand the relationship between horizontal inequality and conflict. According to these results, the specific type of inequality is

important for determining both the likelihood of a conflict occurring as well as which groups are likely to be involved in that conflict. However, it is also worth noting that researchers currently understand very little about why these complexities might exist and how they affect violence.

5.3 Implications for Methodological Considerations

A perusal of the literature on horizontal inequality and conflict will make clear that the field is still suffering from considerable methodological and data limitations. As discussed in Chapter 2, data is severely lacking on horizontal inequality (Canelas and Gisselquist 2018), and several authors have circumvented this issue by interpolating missing data (e.g., Ostby 2008; EPDC 2015) or honing in on case studies (e.g., Stewart 2000). However, there are still many research gaps in the field, and better data is necessary to more accurately depict the relationship between horizontal inequality and conflict dynamics. Researchers would be well-served to improve data collection methods where possible to allow for the continued study of horizontal inequality and conflict. In the following section, I provide several suggestions to this end.

Improving data quality, specifically of horizontal inequality measures, is vital to the continued and improved study of horizontal inequality and conflict. There are two main issues surrounding data on horizontal inequality: its unavailability and its poor quality. Fixing these problems is not straightforward: for this reason, many of them remain unaddressed. Still, discussions on how to improve data collection and quality can help researchers consider possible next steps to the improved study of horizontal inequality and conflict.

5.3.1 Addressing Data Unavailability

Regarding data collection, it would be preferable if data were more widely available on horizontal inequality. Currently, the data that can be used to measure horizontal inequality is limited to countries that make it publicly available. The extent of any bias resulting from this

context is arguably small, given that the data used is typically not collected for the purpose of examining horizontal inequality, and countries included in the sample are less likely to have systematic differences in inequality than unexamined countries (see Ostby 2008 and Chapter 3's "The Sample" for a fuller discussion). However, the current data's advantage of "random" selection is also its downfall. Because data used to produce horizontal inequality measures was not collected for this purpose, it tends to be "messy," often needing to be reorganized and recalculated to come up with adequate measures.

There are two possible remedies to this problem. The first is that a data-aggregating entity, such as a research center with an interest in inequality, could maintain a public horizontal inequality database. Compilers of the database would aggregate *existing* data on each relevant group's standing in a given society and calculate horizontal inequality measures, ideally operationalized in several possible ways (e.g., a group's distance from the national average, a group score, etc.). The database would then need to be regularly updated. This database would allow researchers to readily conduct analyses with more standard measures. It would also remove the need for researchers to constantly recalculate horizontal inequality indicators from censuses that are already publicly available.

The second possible remedy is that researchers could begin collecting *novel* data for the specific purpose of studying horizontal inequality. This would allow researchers to record information on relevant groups' income levels, access to healthcare, and other pertinent variables that are not readily available for a host of countries. These data could also include measures of perceived inequality. If made public, these data would be readily available and would include more comprehensive measures of horizontal inequality than currently exist. They would therefore broaden the scope of what researchers are able to study about horizontal inequality.

Both of the above solutions have their own advantages and disadvantages. Regarding the first (i.e., aggregating existing data and using it to calculate horizontal inequality measures in a public database), optimizing existing data would maintain the advantage of low sample bias, since countries are less likely to censor data that is not collected for any purpose related to tracking inequality. However, the tradeoff is a considerably low sample size because horizontal inequality measures are generally calculated from census data or the Demographic and Health Surveys. However, the specific variables collected in the census or survey change depending upon a given country's interests. The result is a database with several variables available for a few countries, but few variables available for most countries. This limits the generalizability of any analysis employing these data.

Regarding the second solution (i.e., collecting new data for the purpose of studying horizontal inequality), data collection efforts would result in more comprehensive inequality measures and expand the research capabilities of the field. However, this solution could face considerable censorship problems: countries are less likely to allow researchers to collect data that would highlight the degree of inequality present in that country, particularly if the country experiences inequality-related violence. This could increase the amount of sample bias in the data, and subsequently inform biased analyses.

It is possible to combine these two solutions. Efforts to both optimize existing data and collect better data would be ideal, pending feasibility. This would likely be most easily undertaken if several research centers were to focus on advancing our understanding of horizontal inequality, with each research center focusing on one solution. It is also possible to compare analyses from newly-collected data with analyses from existing data (i.e., the analyses that have been presented in the literature thus far). Doing this would allow researchers to

compare differences between the analyses, if any, and readjust their focuses on collecting new data or optimizing existing data, if they assess bias from either source to be too strong.

Addressing persistent data issues is not straightforward logistically or ethically. As Canelas and Gisselquist (2018) have noted, in addition to the abovementioned logistical hurdles, there remains the question of the ethics of data collection on horizontal inequality. It is worth considering whether it is ethical to seek out data that, if made public, might be used to rally people and cause instability. Likewise, there is also the question of whether it is ethical to keep the same data hidden, so that people are less aware of their society's inequalities and therefore have less footing to challenge them. However, assuming that researchers decide to continue to seek out data, the potential to do so relies on optimizing "messier" existing data with potentially less bias, or attempting to collect cleaner and more comprehensive data where allowed and risking increased bias. These steps would further the study of horizontal inequality.

Related to the ethics of acquiring data is the ethics of conducting analyses on horizontal inequality and conflict, assuming data is available. Some researchers might have the ethical concern that analyses of conflict determinants could be abused to further repress vulnerable populations within societies if such analyses were misused by those in positions of political power. For example, the literature's common finding that not only the least repressive but also the *most* repressive governments experience the fewest instances of violent rebellion could be (mis)used to rationalize extreme forms of government repression. Likewise, discussions of which forms of inequality are most likely to cause conflict and between whom could be manipulated to allow for the most amount of "tolerable" inequality, or to profile and unfairly monitor groups of people who are determined "more likely" to engage in conflict.

These ethical considerations should be kept at the forefront of researchers' minds as they continue to pursue additional data and conduct more research on the relationship between inequality and conflict. Researchers should be careful to note that there are a host of social ills and ethical issues associated with inequality, government repression, and other factors separate of their potential effect on conflict likelihood. These ills and issues alone are sufficient to pursue the most equal and least repressive societies possible. Similarly, researchers should be careful to state what can and cannot be drawn from their work, as well as the implications of their work. Proactively discussing research and policy implications, and making clear the host of repercussions related to inequality and repression that are persistent regardless of their relationship with conflict, is an important step in reducing the risk of that research being abused to rationalize the mistreatment of others.

5.3.2 Addressing Data Quality

Thus far, this section has focused on the issue of data availability; however, strong data quality is also vital to informative studies on horizontal inequality and conflict. Data on horizontal inequality should maintain certain qualities to ensure they are most useful to future studies. As noted in Chapter 4, this study's results indicate that horizontal inequality might affect conflict and its dynamics differently, not just dependent upon whether it is social, economic, or political, but even at a more granular level. It therefore might not be preferable to lump a host of indicators together as one "social," "economic," or "political" inequality variable, but rather, to separately include each indicator in a database in case there are differences in their effects. Because of this, it is inadvisable for a given data-collecting entity to solely make available, for example, a social horizontal inequality "score." Researchers do not yet know enough about how various types of horizontal inequality differ in terms of their effect on conflict. Until we

understand this better, it is important to collect data at a fairly granular level, and conduct analyses that use each indicator as a variable. This will help researchers better understand how horizontal inequality is related to conflict, which will in turn sharpen theory and policymaking practices.

Likewise, because researchers understand so little about the potential complexity of the relationship between horizontal inequality and conflict, it is important to use existing data where possible. Previous studies (e.g., Ostby 2008; EPDC 2015) have had to interpolate missing data due to necessity: it would have been difficult to generate meaningful results with such small sample sizes. However, as has been discussed at length in Chapter 2, results can vary greatly depending upon factors such as interpolation, measurement, etc. (Chesher 1991; Kreutz 2010). Thus, it is preferable to use original data where possible, especially given that data and methodological capabilities are improving over time. It is important to remain cautious when claiming what researchers know and do not know about the relationship between horizontal inequality and conflict.

Finally, it should be acknowledged that efforts to improve data and methodological choices should be focused on measures of horizontal inequality, which have thus far been elusive. Available data on conflict and control variables commonly used in studies of horizontal inequality and conflict (e.g., GDP, governmental repression, etc.) are fairly straightforward and readily available. Likely as a result of this, these data have been collected in a clean, clear, and consistent way by sources such as the World Bank (2020), UCDP (2019), ACLED (2010), and Freedom House (2019), to name a few. Therefore, focusing on refining horizontal inequality measures will allow for considerable improvement in the study of inequality and conflict.

There are several steps researchers can take to make studies on horizontal inequality and conflict more informative. To better understand the connection between horizontal inequality and conflict, researchers should invest in data collection and aggregation and remain cautious about the conclusions they draw from their research, particularly given methodological and data constraints. This will be helpful in endeavors to better understand the relationship between horizontal inequality and conflict.

5.4 Conclusions about Theoretical and Methodological Implications

The results of this study have implications for both the theoretical orientations and methodological choices future researchers will take with regards to horizontal inequality and conflict. These implications should be noted to guide future study of the topic.

Regarding theoretical understandings of horizontal inequality and conflict, the most salient takeaway from this study is that horizontal inequality appears to be informative to both the presence of conflict and who is involved in that conflict. This study is unique in that its results indicate that how horizontal inequality affects conflict and its dynamics might depend upon the type of horizontal inequality present in the society, even down to a fairly granular level. The reason why certain inequalities seem to have the effects they do is still left to speculation, and deserves attention from further research.

CHAPTER VI: POLICY IMPLICATIONS AND CONCLUSIONS

Scholars have been questioning the origins of violent conflict for centuries (e.g., Hobbes 1651; Kant 1795). That investigation continues today (e.g., Cederman et al. 2013; Pettersson et al. 2019). It is my hope that this study contributes to these discussions.

This contribution represents the next phase in a long tradition of research on violent conflict. Recall that horizontal inequality is inequality between groups, as opposed to vertical, or absolute, inequality within a society. The results of this study indicate that horizontal inequality contributes to both the presence of violent civil conflict and which groups are involved in that conflict. The direction of effects – that is, whether conflict and group involvement are more or less likely – seem to depend upon the specific ways in which a given society is unequal. There are a host of policy implications and conclusions that can be drawn from these findings, assuming that these relationships hold in future studies.

6.1 Policy Implications

One of the most salient takeaways for policymakers is that, when determining risk for conflict, particularly in moderately repressive regimes, they should consider the state of inequality in a given country. The results of this study indicate that inequality might inform whether a country is likely to experience conflict. It also might be relevant for assessing which groups are most likely to become involved in the conflict. Therefore, if these findings hold, policymakers seeking evidence-based solutions for violent conflict should monitor and attempt to ameliorate the most pernicious kinds of inequality, enumerated below. These steps provide the most promising paths forward for understanding and preventing violent civil conflict.

First, it is important that policymakers assess the state of inequality between groups. There is consistent documentation throughout the literature that inequality between groups

within a society, or relative inequality, is pertinent to conflict (Stewart 2000; Ostby 2008; Mancini 2008; Cederman et al. 2013). This dissertation is no exception. I find that horizontal inequality has a considerably larger effect size on the likelihood of conflict than vertical inequality. Policymakers interested in preventing violent conflict, then, might specifically focus on monitoring inequality between groups if they hope to have the largest effect in reducing a country's likelihood of conflict.

Results of this study also indicate that it might not just be grouped inequalities, but specific kinds of grouped inequalities, that increase the risk of conflict and involvement in that conflict more than others. Different indicators of horizontal inequality had differential effects. Policymakers might therefore prioritize amelioration of these inequalities if they hope to decrease the likelihood of these outcomes.

The literature on horizontal inequality and conflict is still fairly young, and scholars are still exploring which inequalities encourage conflict the most and why. It is possible that there are underlying reasons why some indicators of horizontal inequality seem potentially more informative than others. Further exploration of these reasons will help tease out more specific policy implications. For example, if it is true that the most dangerous inequalities are grouped inequalities in which one group is not able to attain a threshold for a minimum quality of life, then we can extrapolate that the policy focus should be on such inequalities (of which access to electricity, for instance, would be an example). However, before this can be stated with any confidence, future studies should delve further into examining whether specific kinds of grouped inequalities make a society particularly at-risk, as discussed in Chapter 5 ("Research Implications"). Until then, policymakers should be aware that some grouped inequalities might

be more relevant than others, and that reducing grouped inequality generally is likely to pay dividends in terms of preventing instability.

Any inequality within a society is cause for concern: its presence can negatively affect quality of life at the individual and societal level (Braithwaite 2013; Neves et al. 2016). However, in this study, I provide evidence that some forms of inequality might have an especially pernicious effect, in that their presence might be more strongly correlated with violent conflict occurrence and group involvement. This difference, specifically at the level of granularity indicated in this study, has not been observed in previous studies. Policymakers should continue to consult researchers as theories on the most pertinent inequalities to violent conflict continue to emerge. Following these guidelines can help ensure that their societies are safe and stable.

6.2 Conclusions

Until the early 2000s, several authors (e.g., Collier and Sambanis 2002; Collier and Hoeffler 2004) believed the relationship between inequality and conflict was uncommon or inconsistent at best, and non-existent at worst. This discussion changed, however, with the work of Stewart (2000) and subsequent scholars (e.g., Ostby 2008; Cederman et al. 2013). They found relative (i.e., horizontal) inequality between groups was much more informative to the advent of conflict than absolute (i.e., vertical) inequality within a society. This distinction was important, and studies of horizontal inequality have since demonstrated a positive relationship between it and the likelihood of conflict (e.g., Mancini 2008; Ostby 2008; Tiwari 2010; Cederman et al. 2013).

Despite these advances, we still do not fully understand all of the complexities of the relationship between horizontal inequality and conflict dynamics. There has been a lack of

comprehensive data to adequately study this relationship, leading to chronically missing data and findings that might not be generalizable. Likewise, the literature's youth means that there are few studies exploring the effect of horizontal inequality on aspects of a conflict other than simply whether it occurred, such as who is involved, its intensity (but see Murshed and Gates 2005 for an example), etc. Finally, though some scholars (e.g., Stewart 2008; Cederman et al. 2013) have begun studying the issue, there is not much work exploring if and how different kinds of horizontal inequality (i.e., social, political, economic, or, as this study indicates, even more granular distinctions) affect conflict. The existence of these gaps means that scholars still have more to explore regarding the relationship between horizontal inequality and violent conflict.

I have attempted to rectify some of the aforementioned limitations with the current study. Employing census data spanning from 1971-2011 and across 8 countries, I have examined the relationship between horizontal inequality and violent conflict occurrence, as well as horizontal inequality and group involvement in conflict. Ultimately, I find evidence that there could be an effect of horizontal inequality on these outcomes, and that it is perhaps more complex than we have yet uncovered. The need for improved data, further research, and additional theory development to explain these results and help prevent future conflicts is paramount.

To conduct this study and build my theories of conflict, I have drawn from the criminology, political science, and public policy literatures. However, it is important to note that these literatures have largely been constructed independently: scholars from these fields often work with other scholars of their own respective disciplines. This is true despite each field's overlap with the others regarding inequality and conflict.

Many criminologists explore the relationship between inequality and crime (e.g., Hagan 1995; Lederman et al. 2002; Braithwaite 2013), and some are interested in determinants of large-

scale and/or political violence (e.g., Savelsburg 2010; Altier et al. 2014; LaFree and Feilich 2016). Findings from this study, and from political science as a whole, are edifying to such inquiries. I, as well as several political scientists (e.g., Ostby 2008; Cederman et al. 2013), find evidence that grouped inequality is important for determining the likelihood of violent civil conflict in which both sides are armed. Other criminologists, however, such as Hagan and Rymond-Richmond (2008), have found grouped inequality also to be important in case studies of international atrocity crimes. Studies of micro-level crime have also indicated that inequality between racial and ethnic groups could be causative of violence (e.g., Peterson & Krivo 2005; Stolzenberg et al. 2006; Hipp 2007). Stolzenberg et al. (2006) find interracial (i.e., grouped) inequality to be more relevant to violent crime than absolute inequality, mirroring findings in the horizontal inequality literature in political science.

Findings from the political science literature are generally supportive of the findings in the broader inequality and crime literature. However, there has not been much formal distinction in the criminological literature between vertical and horizontal inequalities and their different effects (Stolzenberg et al. [2006] provide an exception to this trend). The approaches scholars use in the political science and criminology literatures are complementary, as both suggest that relative deprivation is an important factor in driving violence. The scholars' interests are also well-aligned, as both camps of researchers study the determinants of violence (though, often at the micro-level, in the case of criminologists, and at the macro-level, in the case of political scientists). The combined approaches and findings of political scientists and criminologists indicate that continued efforts to reduce inequality, specifically between relevant cultural groups, could be helpful for ameliorating a host of violent crimes, not just civil conflict. Notably, if my findings apply to violence outside of civil conflict, as well, they would suggest that absolute

inequality in a society might have a smaller effect on violent crime than relative inequality between groups. They would also suggest that highlighting non-economic inequalities, such as social and political inequalities, could be important for the study of how inequality promotes violence. This is important, because it could help criminologists better understand the nature of a potential inequality-crime nexus, as has been done in the political science literature. My findings also provide evidence that these implications are not just relevant to criminologists studying crime in the US, but in other countries, as well.

Just as in criminology, many political scientists have a demonstrated interest in the role of inequality in producing mass violence (e.g., Collier and Sambanis 2002; Fearon and Laitin 2003; Stewart 2008; Cederman et al. 2013), to which criminological works can contribute. Political scientists could apply criminological and psychological research on collective framing and neutralization tactics (e.g., Sykes and Matza 1957; Oberschall 1995; Hagan and Raymond-Richmond 2008) to their works to possibly help explain how and why horizontal inequality causes several kinds of political violence (e.g., electoral violence, civil war, terrorism) when it does. This sort of application would go further than simply documenting the relationship between inequality and political violence, and instead help to produce more grounded theories as to why relationships between certain types of inequalities and violence exist, and how societies transition from being unequal to being unstable when they do (ex., through collective framing techniques). Criminological works can also inform the effect of perceived inequalities and group differences on political violence perpetrated against other groups, since these perceptions have been extensively studied in several criminological and psychological works (e.g., Gamson 1995; Holmes 2000; Wang 2012). As discussed in Chapter 2, section 2.3.1 (“Collective Framing”), these works suggest that inequality alone is insufficient for violence. Instead, groups must

collectively frame inequality – real or perceived – as unjust, and they must frame another entity (e.g., another ethnic group, the government, etc.) as responsible for that inequality. They also often frame violence as self-defensive in nature, or as the only option left. These conditions facilitate the transition of inequality into violence. The works on collective framing help explain when and why inequality transitions into violence. Much of the framing literature is situated in criminology, sociology, and psychology. These literatures can help develop theory in the political science literature. This could in turn augment conclusions drawn in the political science literature, where many of the discussions on mass violent conflict take place.

As well, academics in public policy have a vested interest in the use of the above-mentioned literatures to prevent and mitigate violent conflict. Public policy scholars often test and evaluate potential solutions to intractable social problems, including mass violence and conflict (e.g., Clayton et al. 2001; Hillman 2013). These studies can benefit conflict scholars in other fields, since program evaluations can help inform both the theory and practice of conflict prevention and conflict resolution. The program and policy evaluation literature is vast, and this approach can be helpful for determining the efficacy of potential solutions to conflict. Researchers from political science have produced works that imply potential solutions that have not yet been implemented or evaluated, but would benefit from the perspective of those in public policy. Likewise, criminologists and political scientists often study the etiology of violence, and this substantive expertise can be helpful to policy scholars specializing in evaluative mechanisms. Combining these approaches through collaboration could therefore advance both the study and practice of inequality and violence prevention.

Despite their overlapping interests, investigators from the above areas of study have largely conducted research on inequality and violence independently. Criminologists have

largely neglected the study of larger-scale political violence, as has been noted by several sociologists and criminologists (e.g., LaFree and Dugan 2004; Hagan and Raymond-Richmond 2008; Moon 2011). This is unfortunate, given that each of the above areas of study have subfields that have made considerable contributions to the discourse on inequality and violence. Combining them and working collaboratively could help expand perspectives on inequality and violence. It could usher in better understandings of which kinds of inequalities affect which kinds of crimes. This, in turn, could produce practical solutions to the reduction of violence globally.

Preventing and mitigating conflict requires continued collaboration between not only academics from various disciplines, but also between academics and policymakers. The value of peace and conflict studies lies in the field's potential to improve the human condition. The study of conflict resolution, therefore, requires an applied outlet, which policymakers are positioned to provide. Knowledge of what causes instability and what is likely to rectify it is relevant insofar as it is translated into a means to prevent and resolve such instability. Despite this, Mack (2002) notes that disconnects between researchers and policymakers permeate the field, leading to theory that does not affect practice and practice that is uninformed by theory. The very reason for the field's existence – to ameliorate existing social problems – cannot come to fruition in the absence of continued coordination between researchers and policymakers. It is important for scholars of peace and conflict studies to engage in collaboration with policymakers, making their works available and consumable to non-academic audiences, especially decision-makers, with this goal in mind. Researchers' findings on horizontal inequality and violent conflict provide bases from which social scientists can collaborate with policymakers to inform prevention and intervention efforts.

As mentioned above, criminology has shied away from the study of macro-level political violence. However, as Hagan and Raymond-Richmond (2008) argue, mass violence and international atrocity crimes are of clear relevance to criminology, a field made up of scholars with self-professed interests in understanding and preventing violence and victimization. Though large-scale violence falls well within this scope, criminologists have largely focused on micro-level violence. If more criminologists dedicated themselves and their resources to the study of such violence, they would be able to play considerable roles in helping prevent it. However, to have the tools and capabilities to study these kinds of violence and human rights abuses³⁶ and to simultaneously fail to do so is, therefore, a discipline-wide missed opportunity. For this, criminology “bears a disciplinary responsibility for neglect,” to quote Schoenfeld et al. (2008) in their reflections on criminology’s failure to help address conflict and atrocity crimes, aptly-titled “While Criminology Slept.”

The study of horizontal inequality and violence provides an opportunity for researchers from criminology, political science, and policy studies to collaborate. Criminologists have conducted a host of works on micro-level violence and inequality (e.g., Hagan 1995; Peterson & Krivo 2005; Stolzenberg et al. 2006; Hipp 2007), and their work applying collective framing concepts to violence (e.g., Gamson 2005; Hagan and Raymond-Richmond 2008) helps further understandings of how and when inequality transitions into violence. Political scientists conduct most of the studies on inequality and mass violent conflict (e.g., Stewart 2000; Fearon and Laitin 2003; Collier and Hoeffler 2004; Ostby 2008; Cederman et al. 2013). Their work on horizontal and vertical inequality provides a useful framework for predicting and theorizing about micro-

³⁶ Some, such as Schoenfeld et al. (2008), would even argue that criminologists have more expertise with which to study the advent of atrocity crimes and conflict than many other fields involved in their study, such as public health and international law. This context makes it especially concerning that criminologists have not yet pursued these topics in large numbers.

and macro-level violence. Finally, public policy scholars are well-equipped with evaluative techniques and a policy-oriented focus that can help bridge the gap between theory and practice. This specialization is helpful for the field, which is directly relevant to policy and violence prevention. Interdisciplinary collaborations between these areas of study can help enhance scholarly inquiry into violence and conflict and further efforts to prevent and mitigate violence and inequality.

Mass violent conflict is a particularly harmful social problem. It causes refugee crises, disease outbreaks, and human trafficking, to name a few (Gayer et al. 2007; Akee et al. 2010; Hayes et al. 2016). The literature is increasingly demonstrating that inequality, particularly between groups, could play a considerable role in determining whether a society will experience violent conflict (Stewart 2008; Ostby 2008; Tiwari 2010; Cederman et al. 2013). In this dissertation, I find support for that idea. I also find that different types of horizontal inequalities – not just horizontal inequality itself – might have different effects on conflict, and that these inequalities also inform conflict dynamics, such as which groups will engage in conflict.

Scholarly discoveries on horizontal inequality and conflict are still developing. Academics still have progress to make on explaining, predicting, and preventing violent conflict. With continued cooperation between disciplines and with policymakers, researchers can help inform theory and practice on conflict resolution. This pursuit can help produce safer, more stable societies.

APPENDIX A - SAMPLE

Below is the full sample included in the dataset in alphabetical and numerical order.

Country Name	Year	Relevant Groups
Bangladesh	1991	Buddhists Muslims Hindus
Bangladesh	2001	Buddhists Muslims Hindus
Bangladesh	2011	Buddhists Muslims Hindus
Colombia	1993	Blacks/Indigenous Whites
Colombia	2005	Blacks/Indigenous Whites
Guatemala	1981	Indigenous Non-indigenous
Guatemala	1994	Indigenous Non-indigenous
Guatemala	2002	Indigenous Non-indigenous
Indonesia	1971	Buddhists Christians Muslims
Indonesia	1980	Buddhists Christians Muslims
Indonesia	1990	Buddhists Christians Muslims
Indonesia	2010	Buddhists Christians Muslims
Mali	1987	Arabs/Moors Blacks Tuaregs

Mali	1998	Arabs/Moors Blacks Tuaregs
Mali	2009	Arabs/Moors Blacks Tuaregs
Nepal	2001	Adibasi Janjati Caste Hill Hindu Elites Dalits Madhesi Muslims Newars
Nepal	2011	Adibasi Janjati Caste Hill Hindu Elites Dalits Madhesi Muslims Newars
Peru	1993	Indigenous Non-indigenous
Peru	2007	Indigenous Non-indigenous
Uganda	1991	Baganda Banyarwanda Basoga Far North-west Nile Langi-Achola Southwesterners Teso
Uganda	2002	Baganda Banyarwanda Basoga Far North-west Nile Langi-Achola Southwesterners Teso

APPENDIX B – SUPPLEMENTARY STATISTICS

	abshicm	abshilit	abshiuni	abshie-c	abshiu-p	abshiown	polpwr	cl	loggdp	neighbor	size
abshicm	1.0000										
abshilit	0.0898	1.0000									
abshiuni	-0.0560	0.1318	1.0000								
abshielec	0.3324	0.4837	0.2725	1.0000							
abshiuemp	0.3724	0.2756	-0.0115	0.4231	1.0000						
abshiown	0.3923	0.3812	-0.1054	0.4804	0.5133	1.0000					
polpwr	0.3018	0.2294	0.0515	0.2066	0.2393	0.2082	1.0000				
cl	-0.0427	0.1829	-0.2342	0.0741	0.0136	0.0489	0.0382	1.0000			
loggdp	0.1965	-0.2266	0.5126	0.1631	0.0329	-0.0992	-0.1203	-0.3965	1.0000		
neighbor	-0.0280	-0.1996	-0.1492	-0.3694	-0.1650	-0.1312	0.0443	0.1469	-0.4954	1.0000	
size	-0.3237	-0.2653	0.0892	-0.1475	-0.2720	-0.2734	-0.6408	-0.0177	0.3446	-0.2958	1.0000
_t	-0.3227	-0.0840	0.4638	-0.1485	-0.1422	-0.3046	0.0304	-0.6410	0.2702	0.1169	-0.0993
		_t									
_t		1.0000									

Figure 9: Correlation Coefficients for Independent and Control Variables

	abshicm	abshilit	abshiuni	abshie-c	abshiu-p	abshiown	polpwr
abshicm	1.0000						
abshilit	0.1190	1.0000					
abshiuni	-0.0760	0.0919	1.0000				
abshielec	0.3391	0.5082	0.2805	1.0000			
abshiuemp	0.3783	0.2763	-0.0277	0.4066	1.0000		
abshiown	0.4061	0.4241	-0.0870	0.5158	0.4992	1.0000	
polpwr	0.3110	0.2541	0.1010	0.2456	0.2472	0.2434	1.0000

Figure 10: Correlation Coefficients for Key Independent Variables

	gini	abshicm	abshilit	abship-m	abshisec	abshiuni	abshie-c	abshiu-p	abshiown
gini	1.0000								
abshicm	-0.3937	1.0000							
abshilit	0.2221	0.1185	1.0000						
abshiprim	-0.0574	0.2535	0.3471	1.0000					
abshisec	-0.0034	0.0673	0.3309	0.3790	1.0000				
abshiuni	0.0970	-0.0884	0.1522	0.3284	0.8064	1.0000			
abshielec	0.0586	0.3360	0.5127	0.2357	0.4904	0.3322	1.0000		
abshiuemp	-0.0532	0.3804	0.2674	0.2442	0.0414	0.0084	0.4022	1.0000	
abshiown	0.0728	0.4082	0.4175	0.3150	0.0062	-0.0493	0.5101	0.4927	1.0000

Figure 11: Correlation Coefficients for Vertical and Horizontal Inequality Measures

APPENDIX C – SENSITIVITY & SUPPLEMENTARY ANALYSES

	GINI
conflict	
m_abscm	5.235* (2.52)
m_abslit	-5.985* (2.94)
m_absuni	-31.503*** (8.36)
m_abselec	4.492*** (1.25)
m_absunemp	-29.425* (13.74)
m_absown	-10.051*** (2.60)
gini	0.046*** (0.01)
constant	-0.496 (0.52)
N	20
r2	0.736
* p<0.1, ** p<0.05, *** p<0.01	

Table 10: Relationship between Country-Level Indicators and Conflict with GINI

groupinv	LPM+GINI cluster (ID)	LPM + GINI cluster (ctry)	LPM + GINI cluster (ID), i._t	LPM + GINI cluster (ctry), i._t
abshicm	1.519 (1.71)	1.519 (1.76)	1.158 (1.48)	1.158 (1.05)
abshilit	1.353 (1.32)	1.353 (1.22)	-0.441 (1.71)	-0.441 (1.66)
abshiuni	-10.383 (8.08)	-10.383* (4.63)	3.143 (6.35)	3.143 (4.45)
abshielec	1.052 (0.67)	1.052 (0.79)	-0.268 (0.68)	-0.268 (0.84)
abshiunemp	1.167 (7.08)	1.167 (3.83)	10.349 (6.80)	10.349** (3.89)
abshiown	-4.650*** (1.31)	-4.650*** (1.14)	-2.114 (2.38)	-2.114 (2.45)
polpwr=2	-0.270 (0.20)	-0.270** (0.11)	-0.165 (0.15)	-0.165 (0.14)
polpwr=3	-0.219 (0.19)	-0.219 (0.17)	-0.064 (0.23)	-0.064 (0.17)
polpwr=4	-0.056 (0.14)	-0.056 (0.09)	-0.061 (0.09)	-0.061 (0.09)
cl=4	-0.348* (0.20)	-0.348* (0.15)	-0.529 (0.39)	-1.079*** (0.27)
cl=5	0.043 (0.24)	0.043 (0.16)	-0.734 (0.52)	-1.601*** (0.44)
cl=6	-0.344 (0.26)	-0.344** (0.12)	-1.291** (0.62)	-0.994*** (0.23)
loggdp	-0.043 (0.08)	-0.043 (0.05)	-0.247*** (0.09)	-0.407*** (0.08)
neighbor	0.051 (0.13)	0.051 (0.11)	0.110 (0.27)	-0.157** (0.06)
size	0.004 (0.00)	0.004 (0.00)	0.003 (0.00)	0.003 (0.00)
_t	0.008 (0.01)	0.008 (0.01)		
gini	0.026*** (0.01)	0.026*** (0.01)	0.045 (0.03)	0.000 (.)
_t=1			0.000 (.)	0.000 (.)
_t=10			1.078** (0.41)	1.320*** (0.22)
_t=11			0.599 (0.87)	0.544 (0.49)
_t=17			-0.061 (0.77)	-0.850 (0.46)
_t=20			1.063** (0.41)	1.584*** (0.26)
_t=21			0.118 (0.62)	-0.319 (0.42)
_t=23			0.662 (0.73)	1.627*** (0.31)
_t=24			0.351 (0.90)	1.436*** (0.17)
_t=28			-1.450 (0.90)	-1.383* (0.59)
_t=31			0.008 (0.33)	0.388*** (0.09)
_t=32			-0.719 (0.57)	-0.076 (0.10)
_t=35			0.079 (0.68)	0.523* (0.23)
_t=39			0.000 (.)	-0.640** (0.23)
_t=40			0.000 (.)	0.000 (.)
_t=41			0.000 (.)	0.000 (.)
constant	-0.608 (0.47)	-0.608** (0.23)	-0.277 (1.60)	2.478*** (0.47)
N	63	63	63	63
r2	0.3988	0.3988	0.6000	0.6000

* p<0.1, ** p<0.05, *** p<0.01

Table 11: Group Involvement Full Models with GINI

conflict		LPM	Logit
m_hicm		2.138 (3.27)	56.803 (78.97)
m_hilit		0.607 (3.05)	12.668 (37.75)
m_hiuni		-23.988*** (8.05)	-350.004 (225.31)
m_hielec		1.652 (1.59)	-0.447 (20.34)
m_hiunemp		-20.065 (18.51)	-214.074 (177.81)
m_hiown		5.102* (2.88)	38.820 (48.09)
constant		0.714*** (0.13)	0.982 (1.12)
N		21	21
r2		0.472	

* p<0.1, ** p<0.05, *** p<0.01

Table 12: Relationship between Country-Level Indicators and Conflict without Absolute Values

groupinv	LPM	LPM + ctrls	Logit	Logit + ctrls
hicm	-0.408 (1.05)	-0.031 (1.51)	-1.283 (5.31)	0.864 (8.53)
hilit	2.158** (0.89)	2.336** (1.00)	14.057** (6.07)	13.120* (6.87)
hiuni	-10.771*** (3.93)	-2.777 (6.77)	-63.427** (27.47)	-18.008 (41.33)
hielec	-0.319 (0.60)	-0.260 (0.66)	-1.475 (3.90)	-0.314 (4.38)
hiunemp	-0.991 (6.21)	-3.310 (6.89)	-4.957 (41.51)	-28.673 (54.51)
hiown	2.214* (1.13)	2.471* (1.37)	18.900** (9.61)	17.858 (11.86)
polpwr=1		0.000 (.)		0.000 (.)
polpwr=2		-0.232 (0.30)		-1.290 (1.62)
polpwr=3		-0.270 (0.30)		-1.474 (1.53)
polpwr=4		0.058 (0.32)		0.334 (1.69)
cl=3		0.000 (.)		0.000 (.)
cl=4		-0.494** (0.22)		-2.646** (1.26)
cl=5		-0.092 (0.28)		-0.232 (1.58)
cl=6		-0.419 (0.28)		-1.833 (1.49)
loggdp		-0.169* (0.09)		-0.963* (0.52)
neighbor		-0.122 (0.12)		-0.729 (0.67)
size		0.004 (0.00)		0.024 (0.02)
_t		0.012 (0.01)		0.075 (0.08)
constant	0.443*** (0.06)	0.983* (0.53)	-0.241 (0.29)	2.275 (2.73)
N	70	66	70	66
r2	0.2126	0.3017		

* p<0.1, ** p<0.05, *** p<0.01

Table 13: Group Involvement Models without Absolute Values

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