

INEQUALITY AND IDENTITY- BASED CONFLICT: THE STATE OF THE EVIDENCE

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The Asia Foundation

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I. INTRODUCTION

It has long been suggested that inequality is a determinant of identity-based conflict. But while the idea may at first seem intuitive, empirical support for this relationship has been hard to come by. Decades of research on vertical inequality (VI)—inequality among individuals or households—led to inconclusive findings before a turn to horizontal inequality (HI)—inequality among groups—took center stage. Today, policy briefs on just about every conflict in the world seem to reference horizontal inequality as a likely cause.

Policymakers should treat these findings on HI with caution, however. An extensive review of the literature reveals that, at this stage, we can really be certain only that national-level economic and political HIs among ethnic groups increase the likelihood of civil war. Indeed, under some conditions—when considering other categories or aspects of conflict, for example, measuring HI along different axes of inequality or across different dimensions of identity, or performing different levels of analysis—there is no evidence that HI increases conflict, and there is some evidence that improving equality among groups can actually exacerbate conflict. In short, broad appeals to “inequality” and “conflict” are too vague to be of practical use.

When the existing literature is broken down by type, the simple story that HI causes conflict reveals its limitations, and a more nuanced understanding becomes possible. In this paper, I break down existing findings by (1) type of inequality, including which domain we consider (economic, political, or socioeconomic) and which dimension of identity (ethnic, linguistic, religious, etc.); (2) type of conflict, including category (civil war, riot, interpersonal violence, etc.) and aspect (onset, magnitude, frequency, and duration); and (3) unit of analysis (national, regional, subregional, or group level). This typology leads us to much more limited conclusions than the recent fervor has suggested, but this kind of analysis is essential if we really want to understand the microprocesses underlying these conflicts—who are the participants (elites and combatants), what motivates them, what resources they have, and what tools they use. Indeed, this subtler and more variegated landscape may prevent policymakers from making serious mistakes by jumping on a bandwagon we do not yet fully comprehend.

So, what do we learn by organizing existing findings into these types?

Most studies focus on high-level conflict, civil wars, or ethnic civil wars and find that political and economic horizontal inequality among ethnic and regional groups increases the likelihood of onset. There is some evidence that they might also increase the intensity of these types of violence, but there has been no work on the frequency or duration of high-level conflict. Many of these studies have examined country-level variation, but we also have evidence that subnational horizontal inequality determines which groups within a country will be involved in the conflict, as well as some evidence suggesting which regions of a country will experience civil war.

For medium-level conflict such as ethnic riots, the findings are much less conclusive, such that at no level of analysis, and for no aspect of conflict, do we have consistent results one way or the other, even within the same country. Sometimes HI increases conflict, other times it has no effect or even decreases conflict.

For low-level conflict (interpersonal violence) there are so few studies outside of the US context that no real conclusions can be drawn at this stage, and a number of studies that conflate violence types make it difficult to say how HI is playing a role at all.

This essay proceeds as follows. The next section summarizes the literature on vertical inequality. Proceeding chronologically, it maps the development of empirical findings over the last fifty years or so

and identifies a major theoretical weakness: the failure to address the collective-action problem. Section 3 then analyzes the literature on horizontal inequality. Following an overview of over fifty single- and multi-country studies, it breaks down the findings first by conflict type and then by type of inequality and type of identity. A discussion of underlying conflict processes is woven throughout. Section 4 then considers the way forward, introducing a set of testable hypotheses for each conflict type. The final section concludes the paper.

II. VERTICAL INEQUALITY

In this section, I trace the development of studies on vertical inequality and conflict in the post-war era. The major finding is a lack of consistency in empirical studies linking these two variables, with a need to respond theoretically to the collective-action problem—how does grievance get translated into conflict?

Early Studies

In the decades following WWII, Western social scientists dedicated themselves to understanding what brought about social stability in a country, with each discipline separately identifying vertical inequality as a central factor. They were motivated by a desire to prevent the “twin evils” of fascism and communism among both the weakened states of Europe and the scores of newly independent countries emerging from European colonialism. In political science, Kort (1952) identified the rich and poor as key actors: “When a critically high concentration of income prevails in a society, a revolution [i.e., a disturbance initiated by the underprivileged minority] is likely to occur ... when income is dispersed beyond a certain critical minimum of concentration, a civil war [i.e., a disturbance initiated by a privileged minority] is likely to take place” (p. 491).¹ In economics, Kuznets (1955) asserted that as an economy starts on the path of economic growth, levels of income inequality will increase before later decreasing again as the economy fully matures. The development of what became known as the Kuznets Curve, an inverted U, spawned a huge literature, one branch of which was dedicated to investigating whether economic growth (and thus greater inequality) inevitably led to conflict. Sociologists, meanwhile, resurrected the concept of relative deprivation, a term coined by Merton (1938) but which drew on Durkheim’s (1893) concept of anomie.²

Perhaps the most famous of these writings on relative deprivation, however, was by Ted Gurr in his magnum opus, *Why Men Rebel* (1970). Gurr defined relative deprivation as the “perceived discrepancy between value expectations and value capabilities,” where values include welfare, security, self-actualization, and so forth. Relative deprivation, in Gurr’s theory, leads to frustration, which in turn leads to violence. Rich in examples, *Why Men Rebel* is nevertheless short on the conditions that turn frustration into violence, a critique that would come to characterize much of the VI-conflict literature.

¹Kort was applying Aristotle to a theory of revolution. Indeed, as Lichbach (1989) summarizes, the question of inequality and conflict has “attracted the attention of some of the great political theorists of all time: Aristotle, Plato, Machiavelli, de Tocqueville, Marx, and Madison.

²Merton drew on the writings of French sociologist Emile Durkheim and his concept of anomie, which referred to a breakdown of social bonds between an individual and the community—e.g., unruly scenarios resulting in fragmentation of social identity and rejection of moral guidance. One of the primary determinants of anomie was the forced division of labor, which Durkheim described in terms hinting at economic inequality. Forced division of labor for Durkheim involved power holders, driven by a desire for profit, and people doing work they are unsuited for, which caused them to be unhappy and want to change the system. Anomie leads to antisocial behaviors including crime and suicide. Thus, in many ways, the social science of the 1960s through the 1980s on economic inequality and conflict was a resurrection of Durkheim’s late 19th century work applied to higher-level forms of anomie, rebellion, civil war, ethnic conflict, etc.

Snyder and Tilly (1972) heavily criticized Gurr: why does relative deprivation lead to violence in some cases but not others?³ Is it only the manifestation of violence that demonstrates the strength of the underlying frustration? If so, the theory suffers from circular logic and is highly deterministic. Frustration may provide the fuel, but conflict requires somebody to gather up all the fuel and concentrate it in a particular location, provide a spark to ignite the fire, subsequently shelter it from the elements, and ensure a continual flow of fuel to sustain the fire.

Lichbach Review, the First Forty Years

Two decades later, Lichbach (1989) surveyed 43 quantitative studies, both subnational and cross-national, finding little agreement among them.⁴ Some found evidence in favor of inequality leading to conflict,⁵ while others found the opposite. To use Lichbach's words, "Under moderate economic inequality, some are unhappy; but under pure economic equality, everyone is unhappy."⁶ Others still find that the relationship is U shaped (lower conflict at intermediate levels),⁷ inverted-U shaped (more conflict at intermediate levels),⁸ or nonexistent.⁹ Lichbach remarks, "In sum, two decades of empirical research in conflict studies have challenged the conventionally accepted view that a strong positive relationship exists between economic inequality and political conflict." He identifies differences in measures of inequality used, what aspect of economics they are measuring, how political conflict is measured (and which cases are included), the time frames employed, and what other variables are controlled for. "The lack of agreement among studies using the same data does not inspire confidence in the possible existence of an EI-PC law or laws," he wrote.¹⁰

Perhaps more frustrating to Lichbach than the empirical disagreement was that the mechanisms underlying various economic-inequality–political-conflict propositions had typically been neglected. Lichbach states:

Virtually no one using this approach has suggested what characteristics of dissident movements are influenced by economic inequality! There are no speculations, for example, about the impact of economic inequality on an opposition group's size (number of dissidents), geographic scope of activity, participants (involvement by different types of actors), duration of activity, cohesiveness, ability to attract allies, radicalism of aims and goals, feelings of legitimacy and alienation from government, coercive capacity, and perhaps most important, tactics and form of attack (mass demonstrations or elite coups). It is equally amazing that no one, in all of this literature, has suggested what aspects of government policies and structures that are associated with dissidents are influenced by economic inequality. There are no speculations, for example, about the impact of economic inequality on governmental accommodation and repression of dissent, or on the growth of party systems and federal structures to institutionalize dissent. Thus, no one has bothered to suggest propositions about how both government and opposition groups respond to the EI-PC nexus (p.450).

³ As would Collier and Hoeffler (2000, 2004) three decades later, Snyder and Tilly (1972) pointed to opportunity-based mobilization as opposed to grievances.

⁴ Although, in his own words, "some of them are perhaps best forgotten" (p. 436).

⁵ (Mitchell 1969, Paranzino 1972, Clark 1973, Gurr 1968, Gurr and Duvall 1973, Gurr and Lichbach 1979, Barrows 1976, Muller 1985).

⁶ (Mitchell 1969, Parvin 1973).

⁷ (Davis 1954, Havrilesky 1980)

⁸ Nagel (1974) finds evidence in South Vietnam, though Sigelman and Simpson (1977) find no evidence in a cross-national study.

⁹ (Hardy 1979, Weede 1981, 1987, Duff and McCamant 1976, Powell 1982, Russo 1972, McAdam 1982, Spilerman 1971)

¹⁰ It seemed to Lichbach that "The relevant question to most statistical modelers was not 'Why does economic inequality breed political conflict?' Rather, it appears to have been 'What variables must be controlled in order to see if economic inequality really causes political dissent?'"

Lichbach himself still felt unclear as to why grievances should translate into conflict at all, especially given the writings of Mancur Olson (1971), who highlighted the role of what he termed the collective-action problem in group decision-making. Who would take the lead on organizing conflict? Why would individuals not just free ride on the efforts of others? Conflict does, after all, potentially cost the life of the participant. Lichbach suggested more attention be paid to institutions, such as the regime type of the country or the nature of the dissident organization.

Post-Lichbach, the Last Three Decades

With the downfall of the Communist bloc, questions about the economic determinants of various social, economic, and political phenomena began to give way to concerns about identity and culture. Huntington (1997), with his famous Clash of Civilizations thesis, had put race and religion at the forefront of international politics, but scholars had long realized that civil wars, most of which were identity based, had become the primary form of conflict in the post-WWII era. This, combined with the growing sense that vertical inequality had reached a dead-end in conflict research, contributed to the turn in inequality-conflict studies.¹¹ In truth, however, the inequality link to conflict had been dead for some time.¹² There were three main responses to this roadblock in the literature: revisiting how inequality was measured, respecifying the relationship between inequality and conflict, and directly addressing the collective-action problem.

A first type of response was to follow Lichbach's advice and think more carefully about how inequality is measured and how it relates to conflict. Building on work by earlier social scientists working on rural societies (Moore 1966, Scott 1976), Binswanger, Deininger, and Feder (1995), for example, distinguished national income inequality from land-distribution inequality. Relying on a diverse set of cases, from El Salvador to Mozambique, they claimed that as well as incurring static and dynamic efficiency costs, large farms and the land inequality that goes with them entail social costs, including unrest and civil war (p. 2,060).¹³ This argument, of course, was not able to explain urban unrest, but it also met opposition in terms of explaining separatist insurgencies, which tend to happen in rural settings but which are not motivated primarily by land disputes of this nature. In a 2004 piece, Collier and Hoeffler (2004) use measures of both national income inequality (Gini coefficient) and land-distribution inequality, finding no effect of either on the onset of civil war. While this initial response, following Lichbach's advice, was valuable, the debate fizzled out—billed again as inconsistent results—whereas the real failure was perhaps more in failing to develop theory relating to specific conflict types.

A second response, chiefly by economists, began exploring an indirect effect of inequality on conflict, through economic growth. One finding that has been consistent across nearly all past studies is the negative relationship between GDP per capita and civil war: civil wars are much more likely in poor countries. Scholars linked vertical inequality to this relationship by arguing that VI suppresses economic

¹¹ Shortly after Lichbach's review, in the 1990s and early 2000s, Collier and Hoeffler began suggesting (à la Snyder and Tilly) that opportunistic behavior rather than grievances was what led to the onset of civil war. Their much-cited *Greed and Grievance in Civil War* (2000, 2004), a follow-on to their earlier "On Economic Causes of Civil War" (1998), signaled the nail in the coffin of vertical inequality studies. In another well-known study from the early 2000's, Fearon and Laitin (2003) also find no relationship between inequality and the onset of civil war.

¹² Or, more accurately, had continued to produce conflicting findings. For example, a study by Nafziger and Auvinen (1997) found a positive association between national income inequality and conflict (both onset and battle deaths). Alesina and Perotti (1996) found that inequality was correlated with the number of political murders annually. In an earlier version of their 1998 piece, Collier and Hoeffler (1996) actually found that income inequality reduced the likelihood of civil-war onset. Thus, a decade to a decade and a half after Lichbach's review, empirical results were still contradictory, and a review by Cramer (2003) concludes: "Econometric analysis appears capable of supporting completely contrasting causal mechanisms for the same variable through modest changes in the specification of models."

¹³ A similar argument had been made by Russett (1964).

growth by misallocating investment in education. The wealthy commit resources to high-quality education of their offspring irrespective of their innate intelligence, while bright children of poor families cannot afford proper schooling. Poor labor-market opportunities and a lack of incentives for entrepreneurial innovation among the disadvantaged reinforce this private aversion to investing in education (Cramer 2003). Ultimately, inconsistent empirical results again hampered this line of research.¹⁴

The third response has contributed to the largest and most successful scholarship on inequality and conflict. In his 1989 review essay, Lichbach briefly touched on the question of horizontal inequality. “Who is to be economically equal to whom?” he asks. “When there are several groups in a nation, the subject class of economic equality is no longer straightforward.” Cramer, in his 2003 review essay, argued that inequality should be reviewed relationally, and that individuals care more about how their locality fares in comparison to other parts of the country, thus implying a geographically-based horizontal inequality (p. 405). A decade after Lichbach, and around the same time as Cramer’s review, Frances Stewart would kick off a vast research agenda that began to tackle horizontal inequality head on.

III. HORIZONTAL INEQUALITY

Stewart first intimated the role of horizontal inequalities in conflict in a 1998 Oxford University working paper (Stewart 1998). She defined horizontal inequality as “inequality between groups, where groups are defined by region/ethnicity/class/religion, according to the most appropriate type of group identification in the particular society” (p. 18). She would later distinguish among economic, political, social, and cultural dimensions of inequality (Stewart 2008). Since that time, there have been hundreds of studies, both quantitative and qualitative, investigating horizontal inequalities, from their technical measurement, to data compilation, to their application to numerous social, economic, and political phenomena. Conflict has remained one of the most studied topics within this huge literature.

So, what do we know about horizontal inequality and conflict after almost two decades of research on the topic? To answer this question, let me turn to the first quantitative study on HI and conflict, Barrows’s (1976) analysis of 32 sub-Saharan African countries. Though more rudimentary in its measure of HI, Barrows usefully distinguished among conflict categories: he found that “ethnic group inequality” was a significant predictor of communal instability (violence among linguistic, ethnic, religious, or regional groups) and turmoil (riots, strikes, and demonstrations), but not elite instability (e.g., military coups).¹⁵ Moreover, he identified the underlying dynamics of these conflict types, such as where they took place, the different types of actors involved and their motivations and actions, and these actors’ relations with the government (a modifying factor). Barrows also discusses data limitations. Unable to generate city-level measures for the “turmoil” category, the most appropriate unit of analysis for this conflict type, he nevertheless discusses his predictions were he able to gather such data.

¹⁴ As Cingano (2014) summarizes from his review of 27 empirical studies: “There is no consensus on the sign and strength of the relationship; furthermore, few works seek to identify which of the possible theoretical effects is at work.”

¹⁵ Barrows produced a measure of horizontal inequality based on his own subjective scoring on a 1–9 scale, where a 9 means a small group controls a disproportionately large share of political power, wealth, education, and other variables.

BOX 3.1. Overview by the Numbers

- **Level of analysis.** Of the fifty-one studies, twenty-nine are multicountry studies and twenty-two are single-country studies.
- **Conflict type.** Twenty-eight of the studies address high-level conflict (civil wars, ethnic wars, secessionist wars, rebellions, etc.), twelve examine medium-level conflict (ethno-communal violence, riots, etc.), and just two explore low-level conflict (protests, demonstrations, etc.). Eight studies analyze multiple types of conflict combined into a general conflict category, while four study terrorism, which, as I discuss in more detail below, should be treated as a tactic rather than a distinct conflict type. Lastly, there are several stand-alone analyses on violent crime, coups, political violence, support for violence, and homicides, and one on the type of conflict (ethnic or class).
- **Aspect of conflict.** Of the studies addressing high-level conflict, most examine the conflict's onset. There are only five studies on the intensity/magnitude of civil wars, one on frequency, and none on conflict duration or re-onsets (breakdowns of settlements or ceasefires). Studies exclusively on ethnic civil war are surprisingly sparse. And though there are more studies on ethnic conflict at the medium level, most focus on intensity/magnitude rather than onset, duration, etc.
- **Type of HI.** Most studies employ a measure of *economic* horizontal inequality. There are a few that examine political HI, but hardly any investigate socioeconomic HI. None measure cultural HI.
- **Unit of analysis.** Ten studies are at the country level, eight at the ethnic-group level, and the rest at the subnational level of analysis, including nine of the multicountry studies. Among the subnational studies, we see an array of units of analysis, ranging from region, to province, to district, all the way down to the village.
- **Modifying variables.** In some cases, the effect of HI is dependent on some other factor, such as national wealth, petroleum production, within-group inequalities, high decentralization, poverty level of the ethnic group, and the level of ethnic fractionalization. In one case, HI is the modifying variable of another factor.

In what follows, I provide an overview of more than 50 quantitative studies, both cross-national and single-country analyses, summarizing the state of the field's knowledge of horizontal inequality's effect on various types of conflict. Building on several of the issues Barrows and Stewart raised in their pioneering works, I lay out four categorizations along which I review the HI-conflict literature:

1. **Type of conflict.** First, I divide this overview into three levels of conflict: high-level conflict (e.g., civil wars), medium-level conflict (e.g., riots), and low-level conflict (e.g., interpersonal violence). Then, within each level of conflict, I analyze both the category (civil war, riots, interpersonal violence, etc.) and the aspect (onset, magnitude, frequency, and duration) of conflict.

2. **Aspect of conflict:** onset (occurrence or outbreak of conflict), magnitude (numbers of deaths, injuries, property destruction), frequency (number of onsets per period, or time between onsets), and duration (how long the conflict lasts).
3. **Type of horizontal inequality.** Again, within each level of conflict (high, medium, and low), I examine both the dimension of inequality (economic, political, or socioeconomic) and the horizontal category of identity (ethnic, linguistic, religious, etc.).
4. **Unit of analysis.** I explore how both the scope of the study (multicountry or single country) and the units of analysis (regions, districts, groups) shape theoretical predictions.

In each section, I also detail (5) which factors condition our current findings on HI and conflict, and conclude by discussing (6) the actors involved—who they are, what they want, their capabilities, and specifically how these three aspects of actors are shaped by HI and thus lead to conflict.

HIGH-LEVEL CONFLICT (CIVIL WAR)

Category of Conflict

There are several types of high-level conflict examined in the literature, all with similar findings on the effect of HI. By high-level conflict, I refer to several forms of conflict found in the literature, including civil wars, ethnic civil wars, separatist wars, revolutions, and guerilla warfare, all of which involve conflict where one party is the state and the other a nonstate armed group.¹⁶ The type of nonstate armed group and its aims are what distinguish these various subtypes in the literature. Perhaps the biggest distinction is between civil wars and ethnic civil wars (almost all studies focus on one or both of these categories), the latter being a subtype of the former in most studies, defined by the nonstate group representing a particular ethnic group. Overall, the main finding on category of conflict is as follows:

- The findings for all types of high-level conflict are similar: horizontal inequality increases high-level conflict.

Civil war, ethnic civil war, separatism, and revolution can all be distinguished by the end goal of the nonstate armed group. This distinction may not be meaningful to some researchers, but to the extent it is, we cannot say much about the latter two categories of conflict in terms of the role of HI. Guerilla warfare, however, is a strategy that can actually be used across these various conflict categories. See Morel (2016) for an excellent discussion on categorizing conflict.

Aspect of Conflict

We can also examine different aspects of conflict, namely onset, intensity, frequency, and duration, of which only onset has a consistent and significant relationship with HI. Most studies have focused on the question of conflict onset—does conflict occur at all? There are some studies on intensity (once a conflict starts, how bad does it become?) and one on frequency (repetition of onset), but to date, there are none on the durations of civil wars. Following are the main findings by aspect of conflict:

- All of the cross-national studies on onset find a positive relationship to HI.
- The findings on intensity (fatality count) are mixed. Wibbels and Bakke (2006) find no effect, while Koos (2013) and Lessman (2016) both find a positive and significant

¹⁶ Overall and yearly death counts normally accompany this definition.

relationship, but each study differs in important ways. Wibbels and Bakke's sample is restricted to federations,¹⁷ while Koos, who like Lessman has a much broader sample of countries, is restricted to just a couple of years of analysis. Wibbels and Bakke's and Lessman's HI measure is interregional inequality, but Koos uses an ethnic-based measure. We cannot, then, draw strong conclusions about these mixed findings on intensity.

- Lastly, Raleigh (2014) measures civil-war frequency using an event count, finding a positive effect of political HIs.

Importantly for future work, none of these studies present a theory of civil-war intensity separate from that of onset. The one study on frequency likewise turns to the same generic logic of grievances. But these various conflict aspects surely have different underlying mechanisms, and thus, HI must interact with them differently. How does horizontal inequality shape the magnitude of violence once it has already begun? Do people simply fight more fiercely, or are more people participating in the violence? Can we go on to separate intensity from the duration of the conflict? Tailoring theory to specific aspects of each conflict type seems equally important as differentiating by conflict type. Such questions represent a rich research agenda going forward.

Unit of Analysis

Next, researchers must decide which unit of analysis is the most appropriate and ensure that our empirical choice matches the theoretical story we tell. The two main global studies take two very different approaches to HI measurement. Selway's (2009, 2011) global dataset aggregates group differences in income at the national level.¹⁸ Using this data, Gubler and Selway (2012) find that HI is positively related to civil-war onset,¹⁹ especially when ethnic groups are also concentrated in their own geographic regions and belong to different religious groups. In a smaller sample of developing countries, Østby (2008a) uses a similar nationally-aggregated dataset and finds a similar positive effect of HI on civil-war onset.

In contrast, Cederman, Weidmann, and Gleditsch (2011) employ geocoding techniques to produce group-level estimates of GDP per capita and likewise find a positive and significant relationship.²⁰ This different unit of analysis allows them to propose and test additional hypotheses—e.g., both advanced and backward ethnic groups are more likely to experience conflict than groups whose wealth lies closer to the national average. This group-based empirical procedure more precisely lines up with the rebel-group story that scholars have developed to link HI and high-level conflict. In short, HI helps rebel leaders in recruitment (more in "Actors" section below). The conflict data codes whether a group has links to a rebel organization that was actively involved in fighting in one of the conflicts included in the UCDP/PRIO dataset. While greater country-level HIs à la Gubler and Selway (2012) make it more likely that any one of the aggrieved groups might turn to violence—and quantitative analysis is a probabilistic method—if a

¹⁷ Federations may act differently to modify HI's effect on intensity, and certainly this institution is the most thoroughly studied in the literature.

¹⁸ Selway uses the term crosscutting cleavages, and measures relations between various dimensions, not just ethnicity and income. The dataset currently has 155 countries, though in his published work with Gubler on ethno-income crosscuttingness, he uses an older version with just 102 countries. The dataset relies primarily on public opinion surveys, including World Values Surveys and various regional barometers.

¹⁹ They use the term ethno-income crosscuttingness, which has the reverse effect of HI, given that higher levels of crosscuttingness equate to more equality. The conflict data are from three sources: PITF State Failure Problem Set (Bates et al. 2003), Uppsala Conflict Data Program (UCDP) / Peace Research Institute Oslo (PRIO) Armed Conflict Data Set (Gleditsch et al. 2002), and Major Episodes of Political Violence (Marshall 2005).

²⁰ The dataset matched ethnic-group maps with geocoded data on wealth. The geocoded wealth data is derived from data on local economic activity within countries for geographical grid cells converted to comparable figures in purchasing power parity (Nordhaus 2006).

country's score is driven by groups that did not rebel, a group-based measure is better.²¹ Ultimately, the choice of unit of analysis will come down to the research question or theory.

Two additional cross-national studies extend down to the subnational unit of analysis (Brown 2010, Lessman 2016). They measure interregional horizontal inequality using reported data to compare a region's wealth or income to the national average. The findings echo those of national-level and group-level HI measures, but these studies contain many fewer cases (as few as 31 in one study). Moreover, further work is required on the theoretical side in terms of why region alone (separate from ethnicity) matters as an identity. In sum, the findings on HI by unit of analysis are as follows:

- National-level measures of ethnic/linguistic HI increase the likelihood of high-level conflict onset.
- Group-level measures of ethnic/linguistic HI increase the likelihood of high-level conflict onset.
- Interregional measures of HI increase the likelihood of high-level conflict onset in multicountry studies, though there are so few countries in these studies that we cannot confidently draw cross-country conclusions at this stage.
- HI measures at the district and village levels, and even within nonpolitical or administrative geographic units, have been found to increase conflict, but these findings are too rudimentary to generalize at this point.

The subnational focus is a fruitful avenue of research. It is important, however, to match theory with empirics. Vadlamannati's (2011) study on civil war in the nine northeastern states of India tells a story of ethnic civil war and separatism. The measure used is one of interregional horizontal inequality, however, and with over 400 ethnic groups among the nine states, the question whether any one group identifies with state-level outcomes becomes especially important. Why would one group care about the welfare of other groups? For example, would a wealthy group whose own income was equal to or even higher than the country average care about the state's poverty level? Vadlamannati provides little detail on the groups, the actual conflicts, or the role horizontal inequalities played in the process. Group-level measures would be much more appropriate in this study. There is a similar mismatch in the three studies on Nepal's civil war. While much of the story revolved around identity groups—religion, caste, language, and ethnicity—only one study generated group-based measures of horizontal inequality, the other two employing *spatial* (by which the literature means geographical) HI.²²

Under-theorized choice of unit of analysis seems to be especially problematic for single-country studies. Of the two Nepalese studies that focus on "region" (Joshi and Mason 2010), one uses village-level HI (Nepal, Bohara, and Gawande 2011) and the other district-level HI (Murshed and Gates 2005). Neither satisfactorily justifies the unit of analysis they employ, though the findings are similar. They thus miss the opportunity to develop our understanding of conflict processes. Did the population care about inequality between groups within the village, how its village fared compared to other villages within the district, or even how the district compared to other districts around the country? The latter unit of analysis seems to accord with a story of a single national Maoist movement, but this tell us little about how recruitment

²¹ Other country-level studies include Buhaug, Cederman, Gleditsch (2014) who compute country-level averages of Cederman, Weidmann and Gleditsch (2011) and Cederman, Weidmann, Bormann (2015) who incorporate satellite data on night lights (rather than GDP) as the horizontal inequality measure. Both studies find a positive and significant association between HIs and civil war onset.

²² I found several more that claimed to test the theory quantitatively, but the studies were of poor quality and are not included in this review.

occurred, whether rebel groups formed in local cells, and who and where the target of attacks were. If HI at two different units of analysis correlates with high levels of violence, what does that mean about the underlying conflict process.

An important aspect of grievances is linking individual psychology to the unit of analysis of choice. Hegre, Østby, and Raleigh's (2009) study of civil-war violence in Liberia analyzes horizontal inequality and violence at the smallest geographic aggregation of any study I was able to find—8.5km by 8.5km units. The unit of analysis was chosen based on detailed knowledge of a war in which vast portions of the country and most ethnic groups were caught up in the fighting. As such, if HIs were going to matter at all, it would have to be very localized relative deprivation driving support for rebel groups. Had the measure been an ethnic-based measure of HI, or a regional spatial measure, there likely would not have been any findings. This study raises an important point about the arbitrary nature of reported and survey data, which are both measured within official administrative units. When do political boundaries matter for conflict? The authors provide strong justification for why they were not appropriate for Liberia's conflict. Some studies, however, make no attempt to address this issue, though it would seem to be a vital one in the conflict process. Other studies try to make connections between official boundaries and ethnic homelands, which are rough at best. Perhaps the most sophisticated type of study on this point deals with federalism. Federal boundaries are large and are often drawn around ethnic groups. Stories of nonethnic boundaries, however, assume that the boundary itself creates a sense of identity among the citizens, and that they begin looking to unit-level outcomes in their decision-making. That connection seems to require testing, especially where ethnicity is not salient. Detailed qualitative accounts within single countries might assuage the reader, but this could also be tested through attitudinal surveys or behavioral experiments. Extending the study of horizontal inequalities to the microbehavioral realm seems like a promising avenue for future research.

Dimensions of HI

Most studies thus far have looked at economic HIs, with some controlling for political HIs. Various studies examine different aspects of economics, however. Selway and Gubler's (2015) measure is based on income; Cederman, Weidmann, and Gleditsch's (2011) on GDP per capita; and Østby's (2008a) on household assets. Would other dimensions generate different hypotheses? Theoretically, loss of income for a year could mean not being able to feed a family and might entail stronger grievances, whereas lack of household assets such as a radio or television might be frustrating, but it would not result in grievances as strong. A contrasting logic might be that individuals see inequality in assets as more entrenched. Income can vary, but in the long-run, being able to afford assets such as "a radio, a television, a refrigerator, a bicycle, a motorcycle and/or a car" (*ibid.* p.9), reflect something more inherently unfair and unchangeable. Thus, assets measures might result in stronger grievances than income.

Socioeconomic HIs have been rarely studied. Østby's (2008a) HI measures employ data on education in addition to household assets. While assets and education are likely highly correlated, their link to conflict is potentially different. Assets are economic, while education is socioeconomic, which dimensions have been differentiated in more recent work on HIs and conflict. Brown and Langer (2010), for example, theoretically discuss educational HIs and conflict. They argue that HIs in education are indirectly connected to conflict by perpetuating long-time socioeconomic inequalities. This logic is suggestive of entrenched grievances, but does not engage specifics of various conflict processes. Brown (2008), however, suggests that state interventions, if seen to favor one group over another, can transform communal conflict into antistate rebellion. With educational horizontal inequalities, the upper echelons of the state bureaucracy are more likely to be the preserve of the more educated group, thus making state

interventions more likely to favor one group over another. Another possible connection is the networks created by the education system. A group's exclusion from tertiary education, for example, eliminates the possibility of creating crosscutting ties with elites from other groups. Instead, the disadvantaged group may have networks from secondary school, which are more likely to be located in ethnic regions and thus be more homogeneous in nature. Further, the most educated members of the disadvantaged group will not be engaged in the types of professions that might dissuade them from forming rebel groups and their own independent state, in which higher-education opportunities would be more available to their ethnic group.

There has also been significant work done on political HIs, with the results largely echoing those of economic HIs: more inequality leads to more conflict. This class of studies began with Gurr (1993) and his Minorities at Risk (MAR) dataset, an expert-coded dataset, in the vein of Barrow, on political as well as economic horizontal inequalities. MAR classified 233 politicized communal groups in 93 countries according to political, economic, and ecological differences, finding that most groups suffering horizontal inequalities had taken some action to assert group interests, ranging from nonviolent protest to rebellion. A more recent effort is the Ethnic Power Relations (EPR) dataset of (Cederman, Wimmer, and Min 2010), also expert-coded at the group level, which finds a positive and significant relationship with civil-war onset. Other studies that rely on MAR or EPR include Koos (2013); Koubi and Böhmelt (2014), who find that political HI depends on national wealth—it only matters in poor countries; Han, O'mahoney, and Paik (2014); Raleigh (2014), who argues that rebellion is more likely to occur for excluded groups if the group's elites are in direct competition with the regime; and Asal et al. (2016), who find that the effect of political HIs is conditioned by the amount of petroleum produced.

Lastly, there needs to be more explicit discussion about the different dimensions of HI studied. In the three single-country studies on Nepal's civil war, the dimensions of inequality all differed: expenditures, human development, and education. One interpretation of this is that horizontal inequality in any category is equally likely to lead to civil war, but it might be that the fact that all three of these dimensions reinforced horizontal inequality means that Nepal was more prone to civil war in the first place. To date, there has been no work looking at the cumulative effect of various types of HI, and this seems like a fruitful avenue for future research.

We can summarize the findings along dimensions of HI as follows:

- Economic horizontal inequalities (night lights, GDP per capita, income, household assets) contribute to the onset of high-level conflict.
- Political horizontal inequalities contribute to the onset of high-level conflict.
- There is preliminary evidence that socioeconomic horizontal inequalities (education) contribute to the onset of high-level conflict, but much more work needs to be done in this area.

Moderating Factors

Moderating factors make an already complicated picture of HI and conflict even more complex. In some studies, the moderating factor simply affects the strength of HI's effect, but in others HI has no effect unless the condition is present (or absent). The most common moderating factor analyzed in past studies, whether by choice or by virtue of data availability, is country type. Scholars have examined federations, developing countries, and specific geographic regions (mostly sub-Saharan Africa).

Federalism seems to intensify the effect of HIs on civil-war onset. Wibbels and Bakke (2006) argue that federalism "provide[s] regional groups the opportunity both to collect resources and to create a network

of institutions through which to mobilize.” Deiwiks, Cederman, and Gleditsch (2012) use country-level averages of Cederman, Weidmann, and Gleditsch’s (2011) geocoding technique to analyze secessionist warfare in federal states. Their results shed more light on the underlying mechanisms at play in federations: an ethnic group’s access to regional institutions while suffering exclusion from central state power increases the risk of secessionist conflict. These measures of political HIs reinforce the already conflict-inducing effect of economic HIs within federations.

HI may also have a stronger effect on conflict in developing countries. Østby (2008b) focused on developing countries (55 in total), using data from the Demographic and Health Surveys (DHS). Probably the earliest cross-national effort, Østby provided strong evidence for the theories of Stewart that linked horizontal inequalities to higher levels of conflict in war.²³ The idea of restricting analysis to developing countries is potentially important: protracted armed conflicts rarely occur in wealthy countries, so the scarcity of resources makes a particularly potent environment for horizontal inequality. The findings of this single study align closely with the global samples cited earlier, but examining a more complete set of developing countries, or considering different dimensions of HI, might reveal different patterns.

Another modifying factor is region, with scholars having focused on sub-Saharan Africa separately. While the scholars no particular theoretical justification for looking at this region, other than their own interest and expertise, other studies have claimed that Africa has a unique ethnic landscape—it is more ethnically diverse than other regions, or it has experienced more colonial intrusion in the form of crude border demarcations that gave no consideration to ethnic groups and their historical homelands. Nevertheless, the two studies on this region do not use an explicitly ethnic measure of HI, only a spatial (i.e. geographic) one. Perhaps this explains why Østby, Nordås, and Rød (2009) find no effect of HI on civil-war onset, though they justify this by positing that “subnational regions frequently correspond to ethnic-group demarcations, with each region being dominated by a particular ethnic group” (p. 304).

Other moderating factors include other dimensions of ethnic structure—e.g., the geographic distribution of ethnic groups (Gubler and Selway 2012), the degree religion cuts across ethnicity (Gubler and Selway 2012), levels of within-group inequality (Houle and Bodea 2017, Kuhn and Weidmann 2015), and the existence of political HIs (Lacina 2015).

²³ The conflict data stem from the Uppsala/PRIO Armed Conflict Dataset (2011), which uses a threshold of 25 battle-related deaths per year.

BOX 3.2. Summary of High-Level Conflict Findings

- **Conflict type.** There is strong evidence to suggest that HIs increase the likelihood of civil-war and ethnic civil-war onset. There is only preliminary evidence that HIs increase the intensity and frequency of civil and ethnic civil wars. There is insufficient evidence for the link between HI and the duration of high-level conflict.
- **Type of HI.** There is strong evidence to suggest that both economic HIs and political HIs increase the likelihood of high-level conflict. However, there is insufficient evidence linking socioeconomic or cultural HIs and high-level conflict.
- **Unit of analysis.** There is strong evidence that national-level ethnolinguistic, group-level, and interregional HIs increase the likelihood of high-level conflict. There is very preliminary evidence from a couple of countries for HI at lower levels (district, village, etc.) increasing conflict (onset, fatalities, frequency).
- **Modifying Variables.** Political institutions (federalism), level of development, geographic distribution of ethnic groups, ethnoreligious crosscutting, within-group inequality, and political HIs.

Actors: Instigators and Participants

To conclude, I briefly summarize what we have learned about the underlying connection between HI and conflict. The most common mechanism examined in existing studies begins with grievance on the part of potential nonstate combatants. These potential combatants compare themselves to members of other groups, be they geographic or ethnic groups. While group membership goes some way in addressing the collective-action problem that Lichbach (1989) identified in his seminal review, the theoretical story detailing the creation of the combatant group is still lacking in these accounts.

Some studies go further in detailing the mechanisms of HI and conflict. Civil wars require coordination at a high level to draw in large numbers of participants. Accordingly, Østby (2008a) argues that when the HI is due to discrimination, they tend to spur ethno-political organizations to address the discrimination. HI also helps strengthen the group's identity, which the group can then use symbolically to build group loyalty. In addition, rebel groups have lower costs when HI is high, because the alternative income for potential recruits is lower. Gubler and Selway (2012) build on this rebel-group logic, arguing that HI enhances identification with groups goals, facilitates social control, and improves in-group communication.

At minimum, however, future research should test the specific mechanisms outlined in existing theories on horizontal inequalities and civil-war onset. Koos (2013) comes closest to this. She regresses both conflict intensity and grievances on a measure of horizontal inequality. While some sort of mediation analysis or two-stage least squares would have examined the causal chain more precisely, the study demonstrates the potential for testing the most common logic cited in every study: horizontal inequality leads to conflict because grievances are stronger.

MEDIUM-LEVEL CONFLICT (E.G., RIOTS AND ETHNO-COMMUNAL VIOLENCE)

Category of Conflict

Findings on the effect of HI on medium-level conflict depend significantly on which category of conflict is examined. By medium-level conflict, I refer to episodes in which both parties are nonstate groups. While high-level conflicts differ mainly in their end goals, the forms of medium-level conflict can range from spontaneous riots to planned armed warfare. There is no consistent term for this level of conflict in the literature, except perhaps among scholars of ethnicity, who use the term ethno-communal violence. In addition, there are usually no magnitude requirements, and fatalities can sometimes be more numerous than in a civil war. For example, the ethno-communal conflict in Maluku (Indonesia) lasted for four years and claimed a minimum of 5,000 lives, easily passing the threshold for the strictest definitions of civil war (Spyer 2002). Riots are perhaps the most studied form of medium-level conflict, but most of that literature tends to be qualitative in nature. Moreover, conflicts at this level involve a variety of forms (or tactics), such as pitched intervillage battles, organized terrorist attacks, and even lynchings or assassinations. State involvement is also ambiguous: orchestration of violence by state officials can occur, or security services can be biased against minority groups. In short, a necessary first step in this literature is to provide clearer definitions. Nevertheless, we can summarize the findings as follows:

- The three studies on riots or urban social disorder—one at the national level, one at the city level but cross-national, and one at the city level in Kyrgyzstan—all find that HI increases medium-level conflict.
- There is very little agreement, however, among the several studies on ethno-communal violence, (both multicountry and single country), which almost exclusively examine intensity.²⁴
- Three studies look at nonethnic conflict. Theisen and Slettebak (2011), Østby et al. (2011) and Tadjoeeddin et al. (2015) distinguish “routine” violence (group-based vigilante violence, popular justice and intergroup violence, and neighborhood brawls) from ethnic violence. All three find that HI has no effect on onset or intensity of routine violence.

Aspect of Conflict

When we analyze the results purely in terms of aspects of conflict, the findings at this level of conflict seem even less convincing. Of the four cross-national studies, two examine the onset of conflict, and two use event-count data to capture conflict frequency. The twelve single-country studies all analyze the number of fatalities to capture conflict intensity. None of the studies considers conflict duration. The two studies on conflict onset, Fjelde and Østby (2014) and Raleigh (2014), come to opposing conclusions on the effect of HIs on conflict. The two studies looking at conflict frequency, Østby (2016) and Selway and Gubler (2015), both find a positive effect under some conditions, but negative or no effects under other conditions. There is likewise little consistency on conflict intensity, even among studies on the same country. Three of the eight studies on Indonesia, for example, find a positive and significant effect of HI on violence. An additional one finds no relationship (Østby et al. 2011). In contrast, two studies find that HI decreases violence (Tajima 2013, Gubler, Selway, and Varshney 2016), and one finds a negative but insignificant relationship (Theisen and Slettebak 2011). Lastly, Deters and Nimeh (2014) find positive,

²⁴ Sometimes referred to just as ethnic violence or communal violence in the literature.

negative, and no effects depending on which model they use (more detail below). In short, the Indonesia case reveals the complexity of horizontal inequality's relationship with conflict:

- There is insufficient evidence that HI increases the likelihood of the onset of medium-level conflict.
- There is conflicting evidence on the effect of HI on the frequency of medium-level conflict.
- There are no consistent findings (or there is insufficient evidence) on the effect of HI on medium-level conflict intensity.
- There is no evidence that HI affects medium-level conflict duration

Unit of Analysis

A first point that emerges from the literature on medium-level conflict is about choosing a unit of analysis that matches qualitative accounts of the conflict. Brueck, Kroeger, and Vothknecht (2012) test an indirect measure of violence on the 2010 interethnic clashes between the Uzbek and Kyrgyz populations in southern Kyrgyzstan.²⁵ The descriptions of the violence and its causes are all at the national or regional level: issues over the constitution, the place of the Uzbek language, the role of Uzbeks in business compared to a lack of political power, etc. However, the analysis is done at the community level (120 communities in the country compared to seven oblasts), with the results suggesting that localized HI was the cause.

HI at higher levels of analysis may also play a role in some aspects of conflict. Yes, medium-level conflicts tend to be highly localized, but Selway and Gubler (2015) investigate riots at the country level, which choice they justify by pointing out that riots often spread across a country, or pull in people from other parts of the country. Bussing coethnics in from other parts of the country to fight in a local riot is a common feature of most riots (in the case of the Indonesia archipelago, they even shipped them in).

Lastly, many of the seeming inconsistencies in the literature might be easily explained by unit of analysis, the choice of which has theoretical implications that, to date, nobody has taken advantage of. One of the two studies on conflict frequency, which have opposing findings, explains variation at the country level (Selway and Gubler 2015) while the other is at the city level (Østby 2016). But the unit of analysis at which the measure of HI is constructed is equally important. Fjelde and Østby (2014) and Raleigh (2014) both analyze the onset of communal violence in Africa (multicountry), but their different findings could be explained by one employing a measure of HI that compares a region to the rest of the country, whereas the other compares local HIs. In sum, the findings are as follows:

- National-level HI measures have not been studied enough to support broad conclusions. The one study (Selway and Gubler 2015) suggests that HI's effect is conditional on ethnic fractionalization.
- There have not been any group-level HI measures examined to date.
- There have not been any interregional HI measures examined to date.
- Intraregional HI measures have not been studied enough to make broad conclusions. The one study (Fjelde and Østby 2014) suggests that HI increases medium-level conflict.
- Provincial HI measures are associated with no effect on medium-level conflict, but this conclusion reflects just two studies, which rely on the same measure of HI (Østby et al. 2011, Theisen and Slettebak 2011).

²⁵ The information is derived from surveys and asks "Do you know anybody who was displaced as a result of the violence?"

- District/county HI measures have mixed findings. One study suggests it increases conflict (Tadjoeddin 2013), another that it decreases conflict (Gubler, Selway, and Varshney 2016).
- Village HI measures have not been studied enough to make broad conclusions. The one study (Tajima 2013) suggests that HI increases medium-level conflict.

Dimensions of HI

Different dimensions of horizontal inequality can have unique effects on medium-level conflict, demonstrated by the divergent findings, in the same region (Africa) and over a similar period of time, of the Fjelde and Østby (2014) and Raleigh (2014) studies. Raleigh finds that political HI actually reduces conflict and also that an income-based measure of horizontal inequality along geographic lines has no effect. Fjelde and Østby analyze economic HI (household assets) and socioeconomic HI (education) among ethnic groups. Contrary to Raleigh, they find that HI increases communal violence.²⁶ these differences suggest that some dimensions of HI may increase conflict, while others may have no effect, or even reduce it.

Of the several studies on Indonesia, no two use the same dimensions of HI, perhaps explaining the divergent findings within a single case. The HI dimensions range from health outcomes to consumer spending, education, and income. There are also differences in whether the identity groups along which horizontal inequality is calculated are defined religiously, ethnically, or geographically. Deters and Nimeh (2014) attempt to explore the effect of these differences in HI dimensions. They use models with numerous measures and apply them by both linguistic and religious group. They find that HI of water source increases conflict, while HI of malnutrition and of flooring decreases it.²⁷ They suggest that water, as a commodity provided by either the government, the community, or an NGO, represents an identifiable public symbol of unequal treatment or opportunity, but that malnutrition and flooring, which depend largely on personal income, are more private or personal. This sort of logic represents a fruitful avenue for future studies. Deters and Nimeh also report a more significant effect when groups are identified by the linguistic dimension rather than the religious one, though they offer no reasons for this finding.

Changes in HI over time (i.e., temporal variance) may also be important. Most cross-national studies employ static measures of HI. However, violence often occurs in places that may have had none for many years. It makes sense, then, even for the basic logic of grievances, that a change in levels of violence might have been precipitated by changes in HI. The alternative is that the precipitating grievances are always simmering in society at a static (and high) level. Deters and Nimeh (2014) is the only study to address this issue. They find that for “access” indicators such as unemployment or enrolment rates of schoolchildren, violence increases as groups become less equal. Conversely, for “achievement” indicators such as malnutrition, underpaid employment, or household consumption, violence rises as groups become more equal. This is a fruitful avenue of exploration, and the authors make some progress in grouping the empirical findings into categories that have potential theoretical significance.

The findings on dimensions of HI can be summarized as follows:

- Socioeconomic HI tends to be positively correlated with medium-level conflict.

²⁶ These two studies are not completely comparable in terms of unit of analysis. Fjelde and Østby measure HI at the regional level, and Raleigh at the national level.

²⁷ In developing countries, the difference between a bare-earth floor versus some kind of covering is a meaningful measure of wealth

- Economic HI can be weakly correlated, uncorrelated, or negatively correlated with medium-level conflict.
- HI among linguistic groups is likely to lead to medium-level conflict, but HI among religious groups seems to dampen conflict.
- There has been insufficient work on political or cultural HI.

Moderating Factors

Studies on medium-level conflict identify moderating factors that the literature on high-level conflict does not explore. First, returning to the idea of temporal variance, time may matter in other ways than just changes in HI. Specifically, in the study of Indonesian violence, we can distinguish two distinct periods: 1999–2003, in the immediate aftermath of the fall of Suharto, when there were periods of extremely high violence, and the years since then, when levels of violence fell dramatically. Now, we might simply categorize the violence in the latter period as low-level conflict, but regardless, there is something about that time period that moderated the effect of HI on conflict.

Level of democracy is one possibility: a government that distributes goods and services more democratically may mitigate group antagonisms and the impulse towards violence, even where HI persists. Indeed, 2004, when levels of violence began their dramatic fall, is the year Indonesia improved from a six to an eight on Polity's scale of democracy, and it has remained at eight ever since (Marshall and Gurr 2013).

The size and economic dominance of the majority ethnic group seems to be an important moderating factor. Selway and Gubler (2015) find a positive correlation between HI and riots, but only in heterogeneous countries. In countries with a dominant ethnic majority, they find that HI reduces riots, echoing findings on other conflict types wherein advantaged groups initiate secessionism, for example. Fjelde and Østby (2014) find that HI's positive effect on conflict is especially great in regions where the largest ethnic group is severely disadvantaged compared to other groups.

Actors: Instigators and Participants

The distinctive characteristics of medium-level conflict mean that we cannot uncritically apply the rebel-group theory of high-level conflict. First, the aims of medium-level conflict are quite different from the aims of civil war, especially ethnic or secessionist wars, where the goal is political autonomy. In addition, planning by all sides of the conflict is more decentralized in medium-level conflict, and participants usually do not give up their regular employment to engage in full-time violence. Medium-level conflict, though not entirely unplanned, tends to be more spontaneous. Participants don't face the difficult cost-benefit calculations of high-level conflict, and social control is less likely to play a role given more limited time to enforce non-compliance. Group saliency and the use of myths and symbols will certainly continue to play a role, but here the traditional story of grievances seems much more applicable: grievances make incidents more likely to be seen in group terms. Thus, a spark such as an attack on a group member by a nonmember is seen as a group-based threat, and revenge or punishment becomes a central goal of the violence.

If we are going to develop models of magnitude or duration, we need a similar actor-centered story. For example, consider Gross's (2011) cascade model of rioting. He argues that every individual has a threshold level of rioting by other people at which he or she will decide to participate. Potential rioters assess the circumstances based on the availability of information that other people are rioting (information

cascades), and they will join if the number of people already rioting reaches a critical mass (tipping point). Higher HI may amplify information cascades, or they may lower the tipping point. This has implications for how intense the violence becomes.

BOX 3.3. Summary of Medium-Level Conflict Findings

- **Conflict type.** There is evidence that HI increases riots, but that it has no effect on routine violence. The findings on ethno-communal violence are mixed. By aspect of conflict, we have mixed results on conflict frequency and intensity, and we need more studies on onset and duration.
- **Type of HI.** The findings suggest that socioeconomic and language-based HIs are more strongly associated with increasing conflict than economic or religious HIs. These results should be treated with caution, however, since they rely so heavily on the Indonesian case.
- **Unit of analysis.** Many units of analysis are used across this literature, making it difficult to draw broad conclusions.
- **Modifying variables.** Democracy, the size and economic dominance of the majority group, and population growth are all identified as modifying variables in the literature on medium-level conflict.

LOW-LEVEL CONFLICT (DEMONSTRATIONS, INTERPERSONAL VIOLENCE)

Category and Aspect of Conflict

There is very little work on the relationship between horizontal inequality and forms of low-level conflict. By low-level conflict, I refer to incidents that have low fatality levels, including violent protests and interpersonal violence. Violent protests differ from riots in their magnitude, though there is no accepted standard in the literature. However, they are more than just peaceful gatherings. Injuries are a minimum occurrence and property damage is likely.

Just two studies, Wibbels and Bakke (2006) and Brown (2009), tackle violent (ethnic) protests, with contrasting results. Wibbels and Bakke find that HI increases ethnic protest, though the sample is restricted to federal countries. Brown, in a small sample of democracies from 1983 to 2005, finds that horizontal inequality has no effect on ethnic protests.²⁸ Clearly there is more work to be done in this area,

²⁸ Only wealthy regions are likely to experience ethnic protests, he finds, though this is further moderated by type of federalism (self-rule mitigates protest in wealthy regions). Self-rule can also induce ethnic protests in low-wealth regions.

especially on nonethnic protests. Even with regard to ethnic protests, however, other aspects, such as duration and onset, could be explored along with simple frequency.

I could find only one study explicitly on horizontal inequality and interpersonal violence, which covers a vast array of incidents, including violent crime, gender-based violence, and personal issues. Tadjoeeddin et al. (2015) finds no effect of horizontal inequality on violent crime in Indonesia. He does, however, link violent crime with vertical inequality. An established literature on racial income inequality and crime does exist, however, mainly in the context of the United States, but also other countries such as South Africa.²⁹ McCall and Parker (2005) examine changes in racial inequality between the 1980s and 1990s in urban areas, finding that areas with increased levels of racial inequality have higher homicide rates. Blau and Blau (1982) also examine violent crime, while LaFree and Drass (1996) explore interracial crime, and Kim et al. (2016) explain arrest rates. These four studies, chosen from over three decades of research, demonstrate the variety of interpersonal violence outcomes studied in this literature.

In sum, the findings are as follows:

- There are too few studies on HI and violent protest to draw any broad conclusions at this stage.
- An established literature on racial inequality and crime exists in the United States context, which is not fully explored in this essay, but which at first glance seems to indicate that higher racial HI increases interpersonal violence.

Unit of Analysis

The two studies on ethnic protests use a measure of interregional HI and, again, have mixed findings, such that we can draw no conclusions concerning unit of analysis in violent protests at this stage. I found no analysis of violent protest that uses national- or group-level HIs, nor any inspection of intraregional HIs or HIs at lower levels of analysis, such as the district, city, or village. As with medium-level conflict, going down to smaller units of analysis seems more theoretically appropriate, but perhaps, like riots (Selway and Gubler 2015), violent protests also have a countrywide element to them.

In the U.S. studies on interpersonal violence, findings based on various units of analysis all seem to agree, though there has been theoretical discussion as to which units are most appropriate. Bourguignon (2001) insists that neighborhood is the correct unit of analysis. The pioneering study by Blau and Blau (1982), examining the 125 largest metropolitan areas in the United States, finds a positive effect of racial HI. On the other hand, a neighborhood-level study in South Africa by Demombynes and Özler (2005) finds no relationship. It could be that individuals evaluate group characteristics at larger levels of aggregation: one's group could be relatively poor locally but less so at the overall city level, for example. Alternatively, while city-level inequality might increase grievances, local inequality might deter violence in other ways—e.g., by weakening social capital.

We are thus able to conclude the following from the literature:

- There are not enough studies to evaluate the effect of unit of analysis on studies of violent protest.

²⁹ What I present here is not an exhaustive review of this literature. Demombynes and Özler (2005) state that the use of group-based inequality in the analysis of crime was rare just over a decade ago (p.271).

- In the U.S. context, unit of analysis does not seem to affect the results (all find that racial HI increases low-level conflict), but other cases (Indonesia, S. Africa) do not have significant findings for HI using some units of analysis.

Dimensions of HI

To date, only economic HIs have been explored for both violent protests and interpersonal violence—again with mixed results on violent protests, but more consistent results for interpersonal violence. Going forward, it might be useful to consider whether other dimensions might not induce violence. Some forms of socioeconomic HI, especially if they are not directly linked to government fiscal decisions, might not lead to protests, or instead might lead to peaceful protests. There is a glaring gap in the literature on this front, both empirically and theoretically.

Moderating Factors

Again, given the sparseness of studies at this level of conflict, there is ample room to explore moderating factors. Both studies on ethnic protests contain moderating factors. Wibbels and Bakke (2006) restrict their analysis to federal countries, which presupposes certain institutional arrangements. Brown's (2009) study, which examines types of federalism, stands as a model for any future analysis of federalism. Not all federal arrangements are the same, and they may differ in the ways they prevent or provoke protests. Additionally, Brown argues that HI only increases ethnic protests in rich regions.

Actors: Instigators and Participants

Lastly, moving forward, we need an actor-centered theory that elucidates where in the conflict process HIs shape both violent protests and interpersonal violence. Since violent protests and riots may start out as identical phenomena, it is essential to ask why some events turn violent, and are thus coded as riots, when others do not? Using microdata to distinguish protests that mostly cause property damage from those that produce injuries or low levels of fatalities could help define the phenomena to begin with (peaceful protests, violent demonstrations, riots), which is a vital first step in laying out an actor-based theory.

In terms of interpersonal violence, HI-induced grievances would be the logical place to begin. The more grievances in society, the more likely some crime will be committed. Theory would have to differentiate HI from VI, however. Should HI be expected to increase non-group-based types of crime? They may correlate with wealth levels, for example, though wealth might not be the true underlying mechanism.

BOX 3.4. Summary of Low-Level Conflict Findings

- **Conflict type.** There are too few studies on HI and violent protest to draw broad conclusions at this stage. An established literature on racial inequality and crime exists in the United States context, which is not fully explored in this essay, but which at first glance seems to indicate that higher racial HI increases interpersonal violence.
- **Type of HI.** Economic HIs increase interpersonal violence, but there are not enough studies to evaluate violent protests. More work needs to be done on political, socioeconomic, and cultural HIs.
- **Unit of analysis.** There are not enough studies to evaluate the effect of unit of analysis on the analysis of violent protest. In the U.S. context, unit of analysis does not seem to affect the results (all studies find that racial HI increases low-level conflict), but other cases (Indonesia, S. Africa) do not have significant findings for HI under some units of analysis.
- **Modifying variables.** Federalism has been explored as a modifying variable of HI in violent protests.

OTHER TYPES OF CONFLICT

To conclude this overview of past literature, I describe several works that do not fit neatly into the three levels of conflict laid out above, and that therefore seem less useful in evaluating what we know about HI and conflict.

Terrorism

Terrorism has been treated as a separate conflict type in the literature, though because it is used across several conflict types, including civil war and ethno-communal violence, it is difficult to justify as a separate category. However, because it has been the focus of several studies on horizontal inequality, I review it here. As defined in these accounts, terrorism often involves few attackers but numerous victims. The perpetrators may or may not take credit for the attack, and as the name suggests, the goal is part of a long-term strategy that relies on creating an atmosphere of fear.

There are two cross-national studies that examine the effect of horizontal inequalities on terrorism, but their findings disagree. In both cases, the dependent variable is the number of terrorist events. While Hendrix and Young (2014) find no effect of ethnic horizontal inequality (income) in a global sample of 137 countries,³⁰ Ezcurra and Palacios (2016) find a positive and significant effect of economic (GDP) HI along geographic lines in a smaller sample of 48 countries.

Studies on HI and terrorism also suffer from theoretical murkiness, stemming in part from the imprecision of treating terrorism as a distinct type of conflict. For Hendrix and Young, horizontal inequality is just a control variable, and they simply state that it is “a source of grievances that spur violent conflict.” Ezcurra

³⁰ Their data come from Cederman, Weidmann, and Gleditsch (2011).

and Palacios advance the logic much further. In countries with ethno-regions, interregional inequality will lead to ethnically defined parties, they argue, using “animosity and distrust between the various groups to broadcast messages that target other ethnic groups” (p. 62). In nonethnicized countries, “richer regions will tend to want more autonomy, and conflict may arise due to differences between desired and actual levels of sovereignty” (p. 62).

Ezcurra and Palacios’ logic, however, falls short of explaining why these actors choose terrorism rather than other forms of conflict or even peaceful solutions to their problems. Indeed, elsewhere they posit that “sovereignty demands can ultimately give rise to the development of secessionist movements in the richer regions of the country, which may be supported by terrorist organizations” (p. 62). Not all terrorist activities, however, are linked to sovereignty issues or accompanied by secessionist movements. And terrorist activities in countries with ethno-regions are not necessarily instigated solely by ethnic parties.

The two single-country studies on terrorism, in India and Pakistan, both find that horizontal inequality increases the number of terrorist attacks per year. Gomes (2015) looks at 360 districts in India. He finds that as the income growth of “scheduled tribes” decreases, the number of attacks decreases. Syed, Saeed, and Martin (2015) report similar results in Pakistan. Comparing five regions between 1980 and 2010, they too find a positive and significant effect of HI on the number of terrorist attacks. As mentioned above, terrorism is a tactic that can occur in most types of conflict, rather than a distinct category, but these studies offer important lessons. First, examining the income growth of a single group does not actually measure horizontal inequality. We do not know anything about the wealth of other groups in India. Both studies also lack a tight theoretical connection to terrorism, again demonstrating the difficulty with terrorism as a distinct conflict category.

Combined Conflict

The problem with studies that combine conflict at various levels is that the underlying mechanisms of conflict at each level are so different that it is difficult to know how to interpret the results. Four studies across two countries combine high-level forms of conflict with lower-level forms. Regardless of their individual usefulness, the studies identify some interesting moderating factors and raise important issues about unit of analysis.

Beginning with Indonesia, Østby et. al. (2011) find that horizontal inequality in infant mortality rates between religious groups increases violence, but only in areas where population growth is high. For their conflict variable, they combine separatism with ethno-communal violence, labeling them together as “episodic.”³¹ The authors refer to the grievances mechanism, but note that only where there was an additional spark created by demographic changes were these inequalities perceived. Population pressure creates resource scarcity, which makes horizontal inequalities particularly salient.³² While grievances may play a part in all these types of conflict, as noted throughout this essay, more precision on how grievances are linked to specific conflict types is needed.

Barron, Kaiser, and Pradhan (2009) examine all violence in Indonesia (ethnic and nonethnic as well as low, medium, and high levels of conflict) in a single measure, finding that HI actually decreases conflict, though only in rural areas. This modifying variable distinguishing rural from urban areas is potentially an

³¹ This same study was also reviewed in the medium-conflict section above, in which Østby’s useful category of “routine” violence was evaluated.

³² An alternative explanation is that population pressure may be an additional source of grievance, and it is the presence of multiple grievances that triggers violence.

important one. Not only do the forms of violence seem to differ in these two areas, but social interactions (themselves based heavily on location, density, and demographics patterns in housing) also differ.

Turning to China, Cao, Duan, Liu, and Wei (2016) and Cao et al. (2016) explore the ethno-religious violence in Xinjiang Province, finding a direct, positive effect of horizontal inequality. Forms of violence in their conflict measure include terrorism, civil-war insurgency, riots, demonstrations/protests, and assassinations. The common link among them is some kind of ethnic motivation. Again, Cao, Duan, Liu, and Wei (2016), refer to “widely felt grievances” among the relatively disadvantaged in the society. In this case, they note that this is manifested in urban-rural inequality, as Uighurs tend to settle in rural areas and Han Chinese in urban areas. Although a strong theoretical justification is not provided for analysis at the county level, presumably the proximity of different groups is an important factor in ethnic minorities observing their relative deprivation. This lends credibility to mainstream media accounts, which suggest that country-wide horizontal inequalities provoke the violence. The authors show, however, that Uighurs who live in more equal counties do not engage in violence. Cao et al. (2016), show that this further depends on the density of religious institutions: mosques reduce violence by providing local public goods and information flows that provide a bridge between the local population and the government.

Combining conflict types makes it difficult to identify responsible actors and outline the conflict process. This is particularly a problem in the two studies cited above by Cao et al. Civil war and demonstrations are too different for us to simply accept that increased grievances make an individual more likely to engage in either. What is missing in all these studies is a reference to the actors that initiated each type of activity and how horizontal inequalities brought the violence about. Do HIs increase the number of individuals likely to initiate each type of activity? Do they increase the likelihood that individuals will participate? Additionally, as we have learned in other studies, it is sometimes the advantaged group that starts the conflict. Lumping together demonstrations and insurgencies does not seem to be a fruitful avenue of future research if we are to truly understand how to prevent HIs from turning into violence.

One study (Han, O’mahoney, and Paik 2014) place them along a spectrum of magnitude, with terrorism at the bottom and civil war at the top. The theory linking horizontal inequality to conflict is, for two of the studies (Han, O’mahoney, and Paik 2014, Lessmann 2016), Gurr’s general grievances story and, for the other two (Langer et al. 2017, van Staveren and Pervaiz 2017) one of social cohesion. Social cohesion argues that horizontal inequality leads to the failure to develop or the breakdown of social ties between groups. These social ties are what prevent stereotypes of other groups from taking root; they also foster trust instead of suspicion and fear. In the absence of social cohesion, conflict is more likely. In many ways, social cohesion stands in theoretical proximity to grievances as a link between horizontal inequality and conflict. There is still no explanation for how specific conflict processes unfold, or how social cohesion affects the formation of groups, aids the instigators of violence, or shapes the decision to participate. In short, studies that combine types of violence tend to be the least theoretically satisfying of all.

Political Violence

Kniss’s (2010) study finds a positive and significant relationship between HIs and election violence in Kenya. The study is unique in its focus on the constituency as a meaningful unit of analysis across which people compare their relative inequality. Indeed, the use of constituency as the unit of analysis is much more justifiable at election time, when people are likely making such comparisons in order to choose a candidate to support. Kniss measures HI using landholding, education, jobs, and political power, with the implicit comparison being a country-level mean. It is not clear that comparison to the country mean is the right unit of analysis, however. Perhaps horizontal inequality within the constituency is what matters. Much of this may depend on the level of elections or the nature of the party system.

Election violence may be a useful category to add into one of the main categories in this essay, but which one? There is simply not enough detail in the Kniss piece to know what types of violence occurred. Were there political murders or violent protests—forms of low-level conflict—or were there riots and armed intergroup warfare?

Other Conflict Types

Two studies, one on the likelihood of coups and another on which form a conflict will take, identify another type of inequality that scholars are beginning to examine, within-group inequality, which interacts with HI in interesting ways. Houle and Bodea (2017) examine the likelihood of coups. Focusing on sub-Saharan Africa, with ethnic groups as the unit of analysis, the authors argue that horizontal inequality increases the chances that an ethnic group will stage a coup, but only when within-group inequality is low. If the members of the ethnic group are highly divided along class lines, they cannot unite behind a coup effort, or they fight amongst themselves along class lines. Siroky and Hechter (2010) argue that when horizontal inequality is high but within-group inequality is low, ethnicity will be the dominant form of conflict. This dimension of inequality is certainly an important moderating factor in considering the effect of horizontal inequality, because it helps identify which members of an ethnic group will engage in conflict.

A final study examines a possible mechanism of conflict: support for conflict at the individual level. The data is derived from surveys in a study of conflict in Kenya (2017). The measure of horizontal inequality is access to basic services, education, and employment, by ethnicity, and the data are calculated at the regional level. The findings suggest that areas in which horizontal inequality is higher have higher levels of support for violence. This would make it more likely, perhaps, for individuals to participate in conflict, or perhaps support it in some other way (providing resources, rewarding political leaders, etc.).

IV. FUTURE RESEARCH AGENDA

The goal of this section is to generate more precise hypotheses for future research into the four categorizations that constitute the focus of this essay: type and aspect of conflict, type of HI, and unit of analysis. I continue to distinguish between high, medium, and low levels of conflict. Then, in addition to distinguishing among aspects of conflict (onset, frequency, intensity, and duration), I separate conflicts based on ethnicity from those based on nonethnic issues.³³ Additionally, I consider conflicts based on economic resources and politics as differing types. Next, I distinguish among dimensions of identity used to create HI measures, as well as types of inequality (economic, political, socioeconomic, and cultural). I also glean different units of analysis. Finally, I incorporate some of the modifying variables identified in the existing literature. I do not generate any hypotheses for combined violence or terrorism for reasons laid out in the previous section.

HIGH-LEVEL CONFLICT

³³ I follow Horowitz (1981), and indeed, most political scientists, in using the term “ethnicity” to refer to a sense of collective belonging, which could be based on language, religion, history, culture, race, or common descent.

Civil war is the most studied high-level conflict type in the literature, and ethnic civil war its most studied subtype.³⁴ With very few studies on other categories of high-level conflict (e.g., separatism, revolution) it makes sense to focus on these two categories. As noted by Sambanis (2001), ethnic and nonethnic civil wars have distinct causes, and this should be taken into consideration when generating hypotheses related to HI.

ETHNIC CIVIL WARS

Box 4.1 presents five hypotheses for the study of horizontal inequality and civil war. Hypotheses 1 through 3d deal with the question of which subnational region is most likely to witness the onset of civil war. While civil wars tend to be seen as countrywide phenomena—inasmuch as the state governs the entire country and draws upon it for the financial and human resources to fight—many conflicts are geographically quite confined. To date, much of the literature has studied civil war at the national level, however. Thus, a first focus of a future research agenda would be to understand whether horizontal inequality at lower levels of analysis helps predict which areas of a country will experience ethnic civil war. We know that interregional measures of HI tend to increase civil war, but unless geography is correlated with ethnicity, we should be investigating ethnic-based HI at the subnational level.

Hypotheses 1 and 2a simply point out that country-level measures are inappropriate to this question. Hypothesis 1 is the most obvious case, since a single country-level measure would generate the same prediction for every region. While this may seem like a straw-man hypothesis, it pushes us away from the not uncommon practice in multilevel, cross-national statistical analysis of using a measure from a higher unit of analysis in the absence of one at the level of analysis under consideration.³⁵

Hypotheses 2a posits that comparison among regions should not provoke an ethnic civil war, though it may provoke a nonethnic civil war, unless regions are ethnically defined (see box 4.2). But then, as hypothesis 2b states, why not just compute an intergroup measure of horizontal inequality. This intergroup measure differs from the country-level measure in hypothesis 1, which is a single measure incorporating information from all groups. In contrast, the intergroup measure produces a different score for each group relative to one or more of the other groups in the country. The violence data, of course, should also be group based.

Hypothesis 2b has already been extensively tested, with group-based HI being found to increase the likelihood a group will engage in armed conflict. However, there are limitations to this approach. First, we do not know which subgroup of a particular ethnic group initiates the movement. For example, an ethnic group may be spread across several regions of a country: why does conflict begin in one region as opposed to others? A second limitation is on the conflict side of these analyses, which generally rely on two databases: Ethnic Power Relations (EPR) (Rono 2009) and Minorities at Risk (MAR) Sambanis (2001). EPR and MAR rely on identifying armed groups and matching them with ethnic groups. This approach has two limitations. First, it is not clear how large the armed group has to be in order to be counted, nor how many fatalities they have caused, how long they have been around, and the degree they are supported by the majority of the ethnic group in question. Second, this approach generally limits us to questions of conflict

³⁴ In the literature review above, I did not separate these out into different sections, mainly because most of the authors analyze both together as a single category of “civil war”. There are exceptions, of course. Group-based analyses, by nature of their set-up, are always exclusively about ethnic civil war (e.g. Cederman, Weidmann, and Gleditsch 2011). Other country-level analyses examine ethnic and non-ethnic civil war separately (e.g. Gubler and Selway 2012). Very few treat it as a separate and sole focus of the study (e.g. Wibbels and Bakke 2006) . Regardless, there has been lots of evidence found in these various studies, especially at the country or group level.

³⁵ This effectively inflates the sample size and introduces bias in the estimates, though there are some methodological techniques that attempt to adjust for that.

onset. Intensity is a particular problem, barring additional information, but to date the data has not been converted to allow analysis of frequency or duration.

Hypothesis 3a examines HIs within units of analysis: intraregional if the unit of analysis is regions, intradistrict if examining districts, or intravillage if exploring all the way down to the village level. For ethnic civil war, however, these low-level inequalities may not be enough to generate a high-level form of violence involving a group and the government. The regional level is the most likely to do this. Regions are usually large enough to give groups the feeling of deserving political and economic control, and comparison with another local group or groups may produce such dynamics. For example, the civil war in Thailand might be attributed to Malays in the south comparing themselves to ethnic Thais in that region. If they were to compare themselves to Thais in other regions, most notably the Northeast, they might feel less aggrieved. It is all about local relative inequality. The Thai case, however, suggests that intraregional tensions are conditional on one of the groups being geographically concentrated in a region. If the Malays also inhabited the Northeast of Thailand, for example, we would likely not see civil war emerge there, due to greater parity in that region. Hypothesis 3b, introduces this condition.

Hypothesis 3c extends it further, positing that federalism would make geographically concentrated groups even more likely to wage an ethnic civil war in the presence of high HIs. This works whether the minority group is relatively rich or relatively poor. In the case of a poor minority group, federalism provides the idea that further political autonomy may improve their situation, while for rich minority groups (think Catalans in Spain) federalism is a way to protect their wealth from being redistributed by the central government. Contrast this to hypothesis 2c, where federalism is not expected to make a difference at the country level.

There may be limitations to this logic of local comparisons, however. HI within a single district or village, no matter how bad, is unlikely to spur a civil war, due to limitations on recruiting armed combatants to fight the power of the central state. Hence, hypothesis 3b.

As identified in the literature review, there have been very few studies on civil-war intensity and none on duration. Hypothesis 4a simply says that hypotheses 1a–2d should apply to those aspects of civil war in addition to onset. Hypotheses 4b and 4c call for applying to the dimensions of intensity and duration the same cross-country and group-level models that have been so widely connected to ethnic-civil-war onset. The predictions in 4b and 4c are based on the grievances logic, allowing future researchers to develop other predictions based on more in-depth theoretical work.

BOX 4.1. Ethnic Civil War Hypotheses

Units of Analysis

1. Country-level measures of ethnic horizontal inequality does not determine the likelihood of ethnic civil-war onset within a region.
- 2a. Interregional horizontal inequality is **not** associated with an increased likelihood of ethnic civil-war onset within a region **unless** regions are ethnically defined.
- 2b. Intergroup horizontal inequality **is** associated with an increased likelihood of ethnic civil-war onset.
- 2c. Federalism has no cross-national modifying effect on intergroup horizontal inequality, though it may increase or decrease the effect of intergroup HI in specific countries.
- 3a. Intraregional, intradistrict, or intravillage HI (along ethnic lines) is **not** associated with an increased likelihood of civil-war onset within the region, district, or village, respectively.
- 3b. Interregional, ethnic HI increases the likelihood of ethnic civil war if and only if the ethnic groups are isolated in separate regions.
- 3c. Federalism generally has no effect on the predictions in 3a, but may exacerbate 3b, especially if a minority group is rich.

Intensity and Duration

- 4a. The same relationships above also applies to the intensity (number of injuries and fatalities) and duration of civil wars.
- 4b. Country-level indices of HI should also predict the increase of intensity and duration
- 4c. Group-level measures of HI should also predict the increase of intensity and duration

Types of HIs

5. Education-based horizontal inequality has a larger effect on the likelihood of onset of civil war (of any type) than income-based HI.
6. Health-based horizontal inequality has a smaller effect on the likelihood of onset of civil war (of any type) than income-based HI.

Lastly, in hypotheses 5 and 6, I turn to other types of HIs. Most of the empirical support thus far has relied on income or other measures of wealth or asset inequality. There have only been one or two studies that include models of health or education. There is thus ample room for developing theories regarding other types of HIs. One dimension of these different HIs is who provides them—government or private entities. Given that education is most clearly a government-provided good, we might think it would be the most salient in terms of anger against the government. Health, in contrast, like income, is fuzzier regarding the government's role. There is also the issue of immediacy. Education tends to be a long-term phenomenon, whereas income and assets are more immediately felt. Money buys food in the short term, even if education has the potential to contribute more to long-term, future welfare. Health, on the other hand, while immediately felt, is intermittent. Only some people get sick, and then, mostly, they get better. Thus, these two dimensions predict that education might have an equal or slightly greater effect on civil war than income, but health would have a weaker one. These hypotheses are mostly to be tested in the same cross-national models as before; however, they could also be explored in connection with hypotheses 1–3c.

NONETHNIC CIVIL WARS

Hypothesis 1 states that there is no reason to expect country-level measures of ethnic HI to predict nonethnic civil war. Though many previous studies have found a positive correlation between country-level ethnic HI and civil war, their datasets on civil war also included ethnic civil wars, so they are not a test of this hypothesis. This logic is repeated in hypothesis 3a for lower levels of analysis.

Hypothesis 2a identifies the most important subnational predictor of civil-war onset: inter-regional horizontal inequality. This is an important point, since civil war necessitates some type of group entity. Vertical inequality might lead to the formation of ideologically based civil wars, though the evidence for this is thin, as noted in the literature review, above. Horizontal inequality along regional lines, however, is quite likely to engender the identity-like feelings needed to underpin a sense of grievance and aid recruitment by overcoming the collective-action problem. Perhaps this only leads to a certain type of nonethnic civil war—secessionist civil war—though this is an empirical question at this point. A prominent example of interregional HI leading to civil war is the Colombian conflict (1964–2016), in which the rebel regions acted like an ethnic group for all intents and purposes.

Hypothesis 2a thus predicts that the regions that are most divergent from the national average are the ones that will experience civil war. This question has still not been resolved in the literature, with large differences in the sample sizes and locations (federations, sub-Saharan Africa, etc.) and, to date, no global sample of countries in a cross-national study. Furthermore, individual countries may exhibit unique dynamics, and this question therefore bears asking in each country separately. This has only been done for India, regarding civil-war onset, though civil-war intensity has been studied in Liberia and Nepal. In short, there are many questions still to be answered about subnational, interregional HIs and civil-war onset.

Hypothesis 2b picks up on the theme of federalism again, with the prediction again left uncertain for individual countries.

Hypothesis 2c explores an area of civil-war research that has received too little quantitative study: the mediating dynamics discussed in the theoretical literature. First, many theories focus on HIs lowering the costs and thus increasing the ability of rebels to recruit. This should lead to higher average numbers of

recruits. Second, the much-used logic of grievance means that a society should generate more violence sparks.

Hypothesis 3a states that intraregional HIs, as well as inter- and intradistrict HIs, should not be enough to spur civil-war onset. First, as stated above, ethnic or religious HIs should not matter for nonethnic civil wars. However, geographic HIs also will not be enough to spur armed conflict. Even within regions, intraregional HI would essentially be a measure of how equal districts are within a region. If they were highly unequal, would a single district have enough grievance to enable recruitment of rebel groups? Other types of conflict may occur, but a high-level conflict is unlikely.

Hypotheses 5 and 6, on health and education HIs, mirror those of the predictions for ethnic civil war, so I do not repeat that discussion here.

BOX 4.2. Nonethnic Civil War Hypotheses

Units of Analysis

1. Country-level ethnic HI has no effect on the likelihood of nonethnic civil-war onset.
- 2a. Interregional HI is associated with an increased likelihood of nonethnic civil-war onset within a region.
- 2b. Federalism has no cross-national modifying effect on interregional HI, though it may increase or decrease its effect in specific countries.
- 2c. The number of sparks and the number of recruits should also be higher in regions far from the national average.
- 3a. Intraregional, intradistrict, or intravillage HIs (along geographic or ethnic lines) are **not** associated with an increased likelihood of civil-war onset within the region, district, or village, respectively.

Intensity and Duration

- 4a. The relationships described above should also apply to the intensity (number of injuries and fatalities) and duration of civil wars.
- 4b. Country-level indices of HIs should also predict the increase of intensity and duration
- 4c. Group-level measures of HI should also predict the increase of intensity and duration

Types of HIs

5. Education-based horizontal inequality has a larger effect on the likelihood of onset of civil war (any type) than income-based HI.
6. Health-based horizontal inequality has a smaller effect on the likelihood of onset of civil war (any type) than income-based HI.

DATA CONSIDERATIONS

Cross-national datasets on both conflict and horizontal inequality are sophisticated and abundant. Thus, hypotheses 4a–6 should be fairly simple to test cross-nationally. Measures of HI along other dimensions might also need to be computed, but the raw data exists to do that. Hypotheses 1–3c are a little more challenging. On the violence side, there are geocoded datasets on civil war that can be merged with any unit of analysis. However, computing HIs at low levels of aggregation requires different

techniques. Even the largest surveys that gather information on health—e.g., the Demographic Health Surveys (DHS)—are not designed to be representative at low levels of aggregation. For wealth, the use of night lights merged with ethnic maps is one way to go, but we would need much more accurate ethnicity maps than the ones used by Cederman, Weidmann, and Bormann (2015), which rely on data from the Soviet *Atlas Narodov Mira* (1964).

MEDIUM-LEVEL CONFLICT

ETHNO-COMMUNAL MEDIUM VIOLENCE

As with civil war, we should be careful to distinguish between ethno-communal types of low-to-medium violence and non-ethno-communal types. For example, the United Kingdom and Korea have very high incidence of riots, only very few of which are ethnic based in the former, and none of which are in the latter. Although, like civil war, there is much written on this medium level of violence, the results are much less in agreement regarding the effect of horizontal inequality on violence. Separating out the ethno-communal from the non-ethno-communal is certainly a first step in clearing up these seeming inconsistencies.

Box 4.3 displays seven hypotheses that future research could explore. Hypotheses 1–2c deal with the problem of unit of analysis. Past studies have analyzed HIs at the regional level, district level, and village level. But which level is the most appropriate? Rather than decide which level to use empirically, based on strengths of correlations, this question should be answered theoretically. Given that this type of violence is highly localized, one possible hypothesis is that comparisons with other regions should not cause this type of violence. Of course, while mobilization may begin locally, motivating issues may be broader in nature, and participants may then be drawn in from other regions, as in Selway and Gubler (2015). We should keep in mind, however, that unless regions are ethnically defined, geographical HIs should not affect ethno-communal violence. Thus, hypothesis 1a is that interregional HI has no effect on any aspect of ethno-communal violence.

Hypothesis 1b, however, references the effect of country-level, ethnic-based HIs, which in multicountry studies should affect the likelihood of ethno-communal violence (onset and frequency), as well as its intensity and duration once conflict has begun.

Intraregional HI, or HI at any other lower-level units, such as intradistrict, or intravillage, will be much more certain to spark ethno-communal violence (hypothesis 2a) as well as affect the intensity of the violence. The presence of more local HIs simply increases the saliency of inequality. It can be observed on a daily basis, as it could not if the other group lived far away. Moreover, people come into physical contact with members of the other group, meaning that day-to-day incidents can be painted in ethnic terms, creating a higher frequency of flashpoints.

Hypothesis 2b makes an argument about the cumulative effect of horizontal inequality at different levels of analysis—the more inequality there is at different levels, the more likely is conflict to happen. We have to keep in mind who the actors are as well as the nature of the violence. Since this type of violence is more spontaneous, the actors are making much quicker decisions about participation. “Should I go riot? Well, I just heard they are building a new pump in the other group’s neighborhood and we are already worse off in the village, but the situation across the district more generally is not biased toward the other group, so maybe there is something I can do about it other than turn to violence.” Alternatively, “in addition to the pump in their village, the other group is getting more government jobs in the district, I have to do something about it right now.” In short, these notions can heavily influence snap decisions.

BOX 4.3. Ethno-Communal Medium Violence Hypotheses

Unit of Analysis

- 1a. Interregional HI has no effect on any aspect of ethno-communal violence.
- 1b. Country-level ethnic HI, in multicountry analyses, increases all aspects of ethno-communal violence.
- 2a. Intraregional, intradistrict, and intravillage ethnic HI increases the likelihood of onset and intensity of ethno-communal violence. The effect of HI is greater the larger the units of analysis.
- 2b. A combination of two or more intraregional, intradistrict, and intravillage ethnic HI will have a greater effect on ethno-communal violence.
- 2c. Political HI concerning regional power will increase ethno-communal violence, especially in more decentralized systems.

Type of HIs

3. Types of horizontal inequality—e.g., education, income, health, assets—can be differentiated by degree of government provision and immediacy. More immediate types will be more heavily associated with ethno-communal violence, but government provision may not apply so much for this low-to-medium type of violence.

Moderating Effects

- 4. HIs have a higher positive effect on the onset of ethno-communal violence in urban areas, but not on the intensity.
- 5. HIs only matter where population growth is high.

Hypothesis 2c really just emphasizes the role of political HI, though I have not specified in the hypotheses to this point which categories—social, economic, cultural, or political—matter (I have assumed they all do). However, it is worth emphasizing that political HI at the national level should not come into play here, just those at the regional level. Moreover, the higher the level of decentralization, the more citizens see this as a local problem—with local actors responsible for the horizontal inequality—and thus one to be solved with this more localized type of violence.

Hypothesis 3 repeats much of the logic from hypotheses 5 and 6 from the previous two sections. It just bears saying here that the government-provided distinction may matter less at the local level, because

any type of horizontal inequality at the local level leads to grievances that are acted out in the heat of the moment.

Lastly, I present two conditional hypotheses derived from existing studies. First, HI matters more in urban areas (hypothesis 4). Taken from the empirical findings in Barron, Kaiser, and Pradhan (2009), the presumed logic is something about the less personal nature of social interactions in urban areas making violence against anonymous targets easier to psychologically process. The increase of HI's effect in urban areas may also be caused by actual physical proximity, which is higher in urban areas. This connects somewhat with hypothesis 5 on population density, taken from Østby et al. (2011), though the argument there focuses on competition for scarce resources. Regardless, both these hypotheses are worth testing again among these proposed future studies that pay closer attention to unit of analysis, type of HI, etc.

NON-IDENTITY-BASED CONFLICT

Like ethno-communal violence, non-identity violence at the medium level of conflict can assume various forms, from riots or violent protests, to gang warfare, to farmers attacking landowners. The scale of the violence, however, is not large. Groups may take up weapons, but they are not permanently armed, and the target is not the entire central state apparatus, though it could be local government bodies or officials. Within this form of violence, I turn to two possible categories: disputes over economic resources, and political violence. While electoral violence, a subcategory of political violence, has been studied briefly in the literature, other forms of political violence have not appeared in any meaningful way, but they generate distinct hypotheses regarding the role of horizontal inequality.

ECONOMIC RESOURCES

Violence over economic resources includes competition over land, natural resources (water, forests, mineral resources, etc.), access to or control of development infrastructure such as roads, bridges, dams, etc., and access to economic markets, as well as labor disputes. This would also include violence targeting the state over governance issues such as corruption, tenders, and the awarding of public contracts; the quality of public services and access to these services; commodity availability, prices, and subsidies; and the delivery of development and aid programs.

I begin with types of inequality, considering first vertical inequality. It is not immediately clear what vertical inequality would signal to individuals who are likely to engage in violence over economic grievances. Is it simply a good proxy for general political inequality (something we do not have good measures for), which would make resolving such economic issues more difficult? Is it that lack of access or control is resulting in lower incomes? In that case, we would not expect to see vertical inequality in health or education play much of a role here, except inasmuch as they correlate with income (hypotheses 1a and 1b). Many of these disputes may not fall along identity lines, so we should expect to see some correlation between vertical income inequality and violence over economic resources.

It is also not clear what the role of horizontal inequalities should play in our theory, regardless of the type (income, health, education), unless the groups fighting for access—and they are often ethnic minorities in remote areas—are defined by ethnicity. Thus, the effect of income HI will be conditioned by the geographic concentration of ethnic groups (hypotheses 2a and 2b). The resolution of these issues also relies heavily on equality in political power, so, again, given sufficient geographic concentration of ethnic groups, political HIs should increase this violence type (hypothesis 2c).

Next, I turn to unit of analysis, which will depend heavily on the size and distribution of the resource and the level of government at which policy is made. Some of the issues may be determined by state officials,

others by national politicians. And this will vary by country. Thus, both regional (including district and village) and national HIs should be investigated as an empirical exercise before hypotheses are formulated (hypothesis 4). However, in federal systems, lower-level government bodies will have more power in determining these outcomes, and thus we should expect to see more violence, especially of the kind motivated by governance issues.

BOX 4.4. Hypotheses about Violence over Economic Resources and Governance

Types of Inequality

- 1a. Vertical income inequality increases violence over economic resources.
- 1b. Vertical health/education inequality has no effect on violence over economic resources.
- 2a. Horizontal income inequality increases violence over economic resources, but only if ethnic groups are geographically concentrated.
- 2b. Horizontal health/education inequality has no effect on violence over economic resources.
3. Political HI increases violence over economic resources, given sufficient geographic concentration of ethnic groups.

Unit of Analysis

4. Horizontal inequalities at all levels of analysis have the potential to increase violence over economic resources.
5. Subnational-level measures of HI will be more important in federal systems, especially for violence over governance issues.

POLITICAL VIOLENCE

All of the studies to date on horizontal inequality and political violence have focused on electoral violence. Other forms of political violence remain understudied, such as violence between rival political parties or associated organizations (e.g., youth wings, student political organizations), competition over nonelective positions and influence within the government, army, police, etc., and contestation over the constitution or institutional (e.g., federal) arrangements. Political violence in this category, to emphasize, does not involve identity. Thus, ethnicized party systems, and therefore violence among ethnic parties, violence over an ethnic group's representation in the bureaucracy, etc., should not be considered here.

BOX 4.5. Political Violence Hypotheses

Types of Inequality

1. Vertical inequality increases political violence.
2. Horizontal inequality along geographic lines increases political violence.
3. Horizontal inequality along ethnic lines has no effect on (nonethnic) political violence.
4. Types of horizontal inequality—e.g., education, income, health, assets—can be differentiated by degree of government involvement in its provision and immediacy. More immediate types will be more heavily associated with ethno-communal violence, but government involvement in provision may not apply so much for this low-to-medium type of violence.

Unit of Analysis

5. The type of election (national, state, local) determines which level of HI will matter for electoral violence.
6. In national elections, the type of electoral rules also determines how much local HI increases electoral violence. Majoritarian (FPTP) systems magnify the effect of local HI.
7. The particular form of political violence also determines which HIs matter.

Moderating Effects

8. Federal systems and highly decentralized systems will experience more political violence as a result of HI.

Vertical inequality may matter, especially in party systems divided by economic ideology, the familiar Left-Right dimension (hypothesis 1, box 4.5). This could result in violence during elections, especially if the Left believes the Right monopolizes democracy via its financial resources, for example. Such inequality may motivate violence among associated organizations outside of elections, too, however. Horizontal inequality along geographic lines is the most likely to matter here. If a certain region feels politically shut out or much worse off along socioeconomic dimensions, it may resort to violence to rectify the inequality (hypothesis 2). Otherwise, we should not expect to see an effect for ethnic HI in this category (hypothesis 3). Other dimensions of inequality should also matter here. As emphasized above, education is most tied to political decision-making, followed by income and then health. So, we should expect the effects of these different types of HI on political violence to display a corresponding hierarchy of magnitudes, whether we look at vertical or horizontal HI (hypothesis 4).

Finally, in terms of unit of analysis, we should expect to see national HI matter in national electoral contests, regional HI in state elections, and district/local HI in lower-level elections (hypothesis 5). Additionally, electoral rules shape how much local HI also drives political violence: majoritarian (FPTP) systems, which tend to have smaller electoral districts, will magnify the effect of local HI. Other forms of political violence are also affected by HIs at different levels of analysis. At the other end of the spectrum, subnational-level HIs should not matter for contestation over the constitution or institutional (e.g., federal) arrangements.

Finally, federal systems and highly decentralized systems will tend to experience more political violence, given the more localized nature of political competition.

DATA CONSIDERATIONS

Data on the dependent variable is notoriously a challenge at the medium level of conflict. There is a reliable cross-national dataset that counts the number of riots, but it does not disaggregate by type of riot, nor provide any information on injuries, fatalities, or property damage. Even so, that dataset—the Cross-National Time-Series Data Archive (Banks and Wilson 2017)³⁶—has not been sufficiently utilized. Some of the general conflict datasets do have information on ethno-communal violence, but they do not include riots. MAR has data on ethnic riots, but it is reported by ethnic group, and its ordinal coding severely restricts analysis on intensity while making duration studies nigh impossible.

Projects monitoring local violence, such as those supported by The Asia Foundation in Thailand, Myanmar, Nepal, and other places, are starting to collect the type of disaggregated data we need to get at many of these hypotheses.³⁷ Not only do they break down injuries, fatalities, and property damage for each conflict, but they painstakingly code information on the type of conflict (motivation, tactics, etc.).

The same issue of constructing measures of HI at low levels of aggregation applies here. I add that some censuses are able to provide such information. This explains why we have so many studies on Indonesia in the existing literature.

LOW-LEVEL CONFLICT

INTERPERSONAL VIOLENCE

Lastly, we turn to interpersonal violence. The literature on horizontal inequality contained only two studies on interpersonal violence, both examining homicide rates. These studies do not distinguish between different types of interpersonal violence, and they tend to conflate gender-based violence or personal disputes with violent crime. Turning to a related field of study, interracial income inequality and crime in the United States, we see a similar combining of homicide motivations into more general crime or murder rates.³⁸ However, trying to formulate hypotheses about all types of interpersonal violence quickly runs into problems.

³⁶ The transparency of the dataset is also low, being a proprietary data source, though the original academic version relied on national newspapers.

³⁷ <http://asiafoundation.org/publication/violent-incidents-monitoring-systems-methods-toolkit/>.

³⁸ Due to data limitations, these hypotheses have rarely been tested outside of the U.S. context.

Consider this proposition: if identity is not part of the motivation for this type of crime, then ethnic HI has no effect on interpersonal violence, but vertical income inequality is predicted to increase interpersonal violence. The idea underlying the role of vertical income inequality might be one of grievances: an aggrieved person with low income is more likely to turn to robbery or organized crime to support himself. But is this really about poverty, then, rather than horizontal inequality? Would a rich person, aggrieved by high HI and, as per past theories, his coerced support of the poor through taxes and redistribution, be more likely to turn to organized crime, robbery, or kidnapping for ransom? That feels like a harder proposition to support. However, either type of inequality could simply increase overall levels of grievance in a society. In short, this line of thinking leads to the idea that crime is not affected by either horizontal or vertical inequality, but simply by poverty. These are crimes of desperation rather than grievance.

In contrast, gender-based violence or personal disputes are more crimes of passion, acts of frustration or anger. I purposely do not use the term aggrieved for these actors, so as not to confuse their violent acts with those directed towards individuals or groups that are perceived to be responsible for the underlying HI. Thus, the first two hypotheses in box 4.6 are that (1) both vertical and horizontal inequality lead to crimes of passion, but (2) neither affects crimes of desperation.

BOX 4.6. Interpersonal Violence Hypotheses

Types of Interpersonal Violence

1. Horizontal and vertical inequality both **increase** rates of crimes of passion (gender-based violence, personal disputes, or extrajudicial responses to crime).
2. Neither horizontal or vertical inequality have an effect on crimes of desperation (kidnapping for ransom, robbery or violent theft, or crimes related to organized crime or illegal trade).

Unit of analysis

3. HI at local levels will have stronger effects on crimes of passion

One characteristic that ties these various categories together is the locality of the violence. Both perpetrator and victim will usually be from the same area, so it makes sense to concentrate on localized measures of horizontal inequality. Comparisons with groups or individuals at higher units of analysis, in regions one has never visited or among groups one has never met, generate less anger and frustration than seeing neighbors or fellow townsfolk who are better off than you. Thus, hypothesis 3 states that HI at local levels will result in higher rates of crimes of passion.

DATA CONSIDERATIONS

Not having found any cross-national studies on horizontal inequality and interpersonal violence, I find it difficult to comment on the future viability of such studies. However, most countries make crime data readily available, so it may not be too onerous to construct such a dataset. An important issue going forward, however, would be how each country defines and reports certain types of crimes. In terms of data on horizontal inequality, there is clearly high-quality data in the United States and South Africa, right down to the neighborhood level. Identifying those sources and seeing if they exist in other countries is the next step.

V. CONCLUSION

The scholarship on inequality and conflict is at an exciting stage of its development. After five decades of quantitative analysis, we now have high-quality data on the inequality side of the equation, especially at the national level. With the use of nationally representative public-opinion surveys and innovative geocoding techniques, we now have robust, cross-country measures of horizontal inequality in addition to vertical inequality. Some of this data has been aggregated at the level of ethnic group and even subnational administrative units. Some work remains to be done, however, constructing inequality datasets along noneconomic dimensions. At the national level, this could be done using surveys, most of which contain some question on educational attainment, for example. However, most surveys are not designed to be representative at subnational levels, which is where some of the hypotheses in this paper push. The Demographic and Health Surveys (DHS) have rich data on health outcomes, but mostly in developing countries. The surveys are large enough to be representative at some levels of analysis, especially when multiple years are combined, but they still leave many gaps. Censuses are one possible solution to this. Not all censuses gather data on identity, however; indeed, some countries strictly forbid it. There are some possible geocoding solutions to this issue via the intersection of data reported at different levels of analysis.

On the conflict side of the equation, there have also been promising developments. The most sophisticated datasets on civil war provide precise geocoded data that can be used at numerous levels of analysis. Some of the hypotheses in this paper simply require an examination of other aspects of civil war (e.g., intensity or duration) or of the role of moderating factors in the conflict process. There is also the opportunity to investigate intervening mechanisms, such as those related to rebel-group recruitment. Other forms of conflict have lower quality data. At the medium level of analysis, there is one cross-national dataset on riots, but it does not disaggregate by ethnic and nonethnic violence. Violence monitoring projects such as those supported by The Asia Foundation³⁹ produce high-quality, granular data that can be a model going forward, but only in a few countries or subnational regions. Nevertheless, these data might be among the most promising resources for testing the array of hypotheses in this paper. Since they provide counts of fatalities, injuries, and property destruction as well as recording the tactics used and the motives for the violence, there is an opportunity to construct categories, such as distinguishing between violent protests and higher-scale riots. Moreover, the data are collected at very low levels of aggregation, allowing for testing many of the subnational hypotheses in this paper.

The data innovations on both the inequality and conflict sides of the equation present the discipline with the opportunity to fill in the numerous gaps I identified in the literature review. To visualize the limits of

³⁹ <http://asiafoundation.org/publication/violent-incidents-monitoring-systems-methods-toolkit/>.

the findings of these 51 studies regarding HIs and conflict, imagine a grid with conflict down one axis. If we distinguish the three levels of conflict (high, medium, and low), categorize them as either ethnic or nonethnic, and examine four different aspects (onset, magnitude, event count, and duration), this will give us 24 potential categories on just this one axis. Then, along the other axis, we would have our measures of horizontal inequality, which we could distinguish by unit of analysis (country, region, group, district, village), type of inequality (economic, socioeconomic, or political), and dimension of identity (ethnic, religious, geographic): forty-five categories on this side, giving us 1,080 possible cells in the grid. What we have so far is 51 studies, some of which may fill up four or five cells at best, and some of which may occupy the same cell. In other words, we have hundreds of empty cells yet to be filled. We are left with a picture of how little we yet know.

Thus, to say that quantitative studies demonstrate that horizontal inequality increases conflict is a crude oversimplification of the full picture. At best, we can say that some forms of horizontal inequality along some dimensions (mostly economic) and at some levels of analysis increase some aspects of some types of conflict at some levels of analysis. But some forms of horizontal inequality along some dimensions and at some levels of analysis have no effect or even decrease some aspects of some types of conflict at some levels of analysis. We are most confident about national-level economic and political HIs among ethnic groups increasing the likelihood of civil war, especially ethnic civil war, at the national level. The findings for national-level HIs among geographic regions give us a little less confidence; we do not know much at all about HIs among religious groups, nor about HIs along noneconomic or nonpolitical dimensions. Nor do we know much about other aspects of civil war, such as intensity or duration. Lastly, we do not know much about the effect of any HIs on other types of conflicts, let alone the different aspects of those types. However, the good news is that data exist with which to explore many of these gaps in the literature at the national level.

The subnational findings to date are much more inconsistent regarding any dimension of horizontal inequality increases or conflict type. At this level, we face both data and theoretical issues. It is not a foregone conclusion that measures constructed at any level of aggregation will matter for conflict. Perhaps horizontal inequality at lower levels of analysis is more fitting for the type of conflict considered, but the different results cast doubt on any role of HIs in the conflict process. If grievances constitute the most important mechanism, then HI at any unit of analysis might be grievance causing. If recruitment or other processes matter more, then theory might help us establish which level of analysis should matter. Regardless, comparing the single-country studies to the cross-national ones illuminates a possible role of horizontal inequalities at lower levels of analysis and opens a dialogue with the cross-national studies: Which unit of analysis is the most relevant? Is more than one unit of analysis relevant, and if so, how do they connect theoretically to various conflict types? Absent theory, it is simply an empirical exercise whose value is uncertain.

This same undertaking of exploring lower levels of analysis might be a fruitful avenue for the study of vertical inequality too. And speaking of other types of inequality, scholars have recently begun constructing measures of within-group inequality that potentially shed light on how inequality affects different members of a group. When the advantaged group instigates a riot, for example, does this tend to involve only the more disadvantaged members of that group? Might elites actually push against the conflict, having the most to lose? Conversely, Donald Horowitz argues that poor regions are more disposed to secession because their educated elites will benefit from the creation of a new state even if the region as a whole suffers Østby et al. (2011). In short, an emphasis of this essay has been to more precisely identify the actors, at what levels of analysis they come into play, etc., and within-group inequality offers a whole new dimension along which to advance theory.

The set of hypotheses presented in this essay is simply a beginning. They are certainly incomplete and undertheorized, and perhaps even wrong. However, they represent fruitful avenues for research in the near future. Greater investments in supporting the collection of granular data on violence, such as the work done by The Asia Foundation, would be necessary to fully test many of these hypotheses.

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TABLE 1: PUBLISHED PAPERS, QUANTITATIVE STUDIES OF HORIZONTAL INEQUALITY & CONFLICT, CROSSNATIONAL SCOPE

Study's Author(s)	Country Coverage	Horizontal Inequality Data	Conflict Data	Findings
Bakke and Wibbels (2006)	22 federal states, ~350 regions, 1978-2000	Reported data. Regional gross domestic product (gdp) per capita	Civil war intensity, UCDP/PRIO, MAR indicator for ethnic rebellion (magnitude)/protest.	<i>HI increases conflict</i> --Result depends on high decentralization. Where decentralization low: HI no effect on ethnic rebellion, and reduces ethnic protest.
Østby (2007, 2008a, b)	55 developing countries	Surveys. DHS	Civil conflict onset, UCDP/PRIO	<i>HI increases conflict</i>
Gubler/Selway (2008, 2012)	102 countries in a global sample	Surveys. WVS, CSES, various regional barometers	Civil war onset, UCDP/PRIO, PITF, MEPV	<i>HI increases conflict</i>
Brown (2009)	Contemporary democracies, 1983-2005	Reported data. Spatial. Regional relative wealth.	Ethnic Protest, MAR	<i>HI has no effect on conflict</i> --in relatively wealthy regions ethnic protest more likely
Østby, Nordås, and Rød (2009)†	22 countries in Sub-Saharan Africa, 1896-2004	Surveys. Demographic and Health Surveys (DHS) on welfare (household asset index) and socioeconomic inequalities (education) between and within subnational regions	Civil War onset. UCDP/PRIO Armed Conflict Database	<i>HI has no effect on conflict</i>
Brown (2010)†	31 countries in a global sample, >600 subnational units	Reported data, subnational. Ratio of subnational GDP to national.	Secessionist/civil conflict onset, UCDP/PRIO	<i>HI increases conflict</i>
Cederman, Wimmer, Min (2010, 2009)§	Global sample	Expert coding. EPR	Ethnic/civil conflict onset, UCDP/PRIO	<i>HI increases conflict</i>
Cederman/Weidmann/Gleditsch (2011)§	~450 groups from a global sample of countries	Geo-coding. GREG and subnational measures of GDP per capita as ratio to country average.	Civil War onset. Non-state Actors (NSA) Dataset, based on UCDP/PRIO	<i>HI increases conflict</i>
Rustad et al. (2011)†	All countries in East, Southeast, and South Asia	Geocoding. Geo-referenced data on aspects of social, economic, and political exclusion, as well as endemic poverty and physical geography	Intra-state conflict onset, UCDP/PRIO	<i>HI increases conflict</i> --Produces map of where violence predicted to happen. Generally corresponds to where known conflicts are today. No statistical analysis.

Deiwiks, Cederman, and Gleditsch (2012) [†]	Administrative units within 31 Federal states, 1991-2005	Geocoding. Geocode economic wealth and ethnic settlements. Exclusion from central state power plus access to regional institutions.	Secessionist conflict onset, UCDP/PRIO	<i>HI increases conflict</i> --In highly-developed and underdeveloped regions, secessionist conflict more likely than in regions close to average.
Agnes Koos (2013) [§]	Ethnic groups from a global sample, 2004-5	Expert coding. Combined MAR and EPR, plus imputation to groups not in these datasets.	Conflict intensity (CONIS ⁴⁰ and UCDP/PRIO), political, cultural, economic grievances (MAR).	<i>HI increases conflict</i>
Koubi and Böhmelt (2014)	A global sample of all countries	Expert coding. Excluded groups (EPR)	Civil conflict onset, UCDP/PRIO	<i>HI increases conflict</i> --conditional on national wealth
Han, O'mahoney, and Paik (2014) [§]	141 ethnic groups from 79 countries in a global sample	Expert coding. Minority At Risk (MAR) dataset from years 1945 to 2000 combined with geocoding using GeoEPR	Civil Conflict Onset, also have rough measure of conflict intensity (1=terrorism, 7=full civil war), MAR Rebellion Index	<i>HI increases conflict</i>
Buhaug, Cederman, Gleditsch (2014)	Global sample	Geocoding. Country-level versions of Cederman/Weidmann/Gleditsch APSR article	Civil War onset. Non-state Actors (NSA) Dataset, based on UCDP/PRIO	<i>HI increases conflict</i>
Hendrix and Young (2014)	137 countries in a global sample	Geocoding. Data from Cederman/Weidmann/Gleditsch (2011)	Annual count of terror attacks committed by dissident groups, Global Terrorism Database	<i>HI has no effect on conflict</i> --Used as control, but no significance.
Fjelde and Østby (2014) [†]	Sub-Saharan Africa, 1990–2008	Surveys. A series of household surveys: inequality in terms of household welfare and education between individuals (vertical inequality) and between ethnic groups (horizontal inequality), DHS	Communal conflict (non-state) onset	<i>HI increases conflict</i> --Especially regions in which the largest ethnic group is severely disadvantaged
Raleigh (2014) [†]	Sub-Saharan African states with pop > 100,000 (10km ×	Expert coding. Governance relationships are derived from the Ethnic Relations Dataset (EPR)	Civil war, militia actions, communal violence onset, ACLED's 60,000	<i>HI multiple effects</i> --Political HI increases civil war, but decreases political militia and communal violence; economic HI increases both civil

⁴⁰ CONIS is a conflict database maintained by the University of Heidelberg, previously known as KOSIMO

	10km units), 1997 to 2011		disaggregated African event points	war and political militia, but no effect on communal violence.
Selway and Gubler (2015)	Global sample	Surveys. WVS, CSES, various regional barometers. National-level indicators.	Riots	<i>HI increases conflict</i> --Only in highly fractionalized societies. In majority-dominant (homogenous) societies, effect is negative and significant.
Kuhn and Weidmann (2015)§	Global sample	Geocoding. A new global measure of economic inequality by combining high-resolution satellite images of light emissions, spatial population data, and geocoded ethnic settlement areas	Onset of ethnic conflict and is taken from the ETH Zurich's GROWup data portal (http://growup.ethz.ch)	<i>HI increases conflict</i> --Within group inequality increases risk of conflict if political/economic inequalities between groups provide a motive
Cederman, Weidmann, Bormann (2015)§	Global sample	Geocoding & Survey. Geocoded data from the G-Econ project with night lights emissions data from satellites with survey estimates in order to arrive at an improved measure of group-level inequality	Onset of civil violence, UCDP/PRIO Armed Conflicts Database	<i>HI increases conflict</i> --horizontal economic inequality drives conflict in the case of groups that are relatively poor compared to the country average
Lacina (2015)§	Global sample	Expert coding. Regional political inequality (power in the capital). Comparison of EPR separatist groups that regionally concentrated to most significant neighbor.	Separatist war onset, EPR	<i>HI increases conflict</i> --when government moderately favors one ethnic group in the periphery, moderate political inequality is correlated with approximately 5–8 times higher conflict odds
Lessmann (2016)†	56 countries (835 subnational regions), 1980-2009. Cross section dataset for year 2005, covers 110 countries	Reported data. Country-level regional inequalities, Lessmann (2014) and Gennaioli et al. (2013). OECD or EUROSTAT.	Civil war onset, intensity (deaths), UCDP/PRIO; Event count of strikes, assassinations, guerilla war, purges, revolutions, riots, demonstrations, government crises, terrorism	<i>HI increases conflict</i>
Siroky and Hechter (2016)	102 countries in a global sample	Surveys, Selway (2011) data	Occurrence of ethnic or class war, UCDP/PRIO, PITF	<i>HI increases conflict</i> --When between-group inequalities high & within-group low, ethnicity the

				dominant principle of group solidarity and primary basis of group conflict
Asal et al. (2016)	Global sample	Expert coding. GeoEPR	Civil war onset, UCDP/PRIO, and using Cederman, Wimmer, Min (2010) ethnic coding of it	<i>HI increases conflict</i> --Depends on amount of petroleum produced
Langer (2017)	19 African countries, 2005-2012	Surveys. Social Cohesion Index from Afrobarometer (extent of perceived inequalities, level of societal trust, strength of adherence to national identity). Also, one that accounts for variation across ethnic groups.	"a range of different violent conflict events", ACLED	<i>HI increases conflict</i> --No relationship found with non-violent conflicts, e.g. protests.
Ezcurra and Palacios (2016)†	48 countries, 1990–2010	Reported data, interregional inequality, Sources: OECD Territorial Statistics, Cambridge Econometrics, and various national statistics	Count of domestic terrorist attacks, Global Terrorism Database	<i>HI increases conflict</i>
Østby (2016)†	34 cities in Africa and Asia, 1986–2006	Surveys. Inequalities between rural– urban migrants and non-migrants based on DHS data.	Urban social disorder events	<i>HI increases conflict</i>
van Staveren and Pervaiz (2017)	82 countries in a global sample	Surveys. Inclusion of Minorities Index based on survey questions like: “Have you ever experienced discrimination?”	Social Cohesion – includes all levels of violence, from riots to terrorism, to rebellion. Indices of Social Development database.	<i>HI increases conflict</i>
Bodea and Houle (2017)§	32 sub-Saharan African countries and 141 ethnic groups, 1960-2005	Surveys. Between- and within- group inequality measures are constructed based on survey data from the Afrobarometer and the Demographic and Health Surveys	Coup onset, Roessler (2011).	<i>HI increases conflict</i> --Between-ethnic-group inequality (BGI) increases likelihood that an ethnic group stages a coup only when within-ethnic-group inequality (WGI) is low

§Unit of analysis is ethnic group

†Unit of analysis is subnational unit (region, province, district, etc.

TABLE 2 PUBLISHED PAPERS, QUANTITATIVE STUDIES OF HORIZONTAL INEQUALITY & CONFLICT, SUBNATIONAL SCOPE

Study's Author(s)	Geographic Coverage	Horizontal Inequality Data	Conflict Data	Findings
Indonesia				
Mancini (2008)	district-level data: 14 provinces, but "remaining 91 districts are treated as "zero" conflict cases."	Reported data. Child Mortality rates.	Ethno-communal violence onset. UNSFIR Database II (14 provinces, 1990–2003).	<i>HI increases conflict</i>
Barron, Kaiser, and Pradhan (2009)	Lowest level administrative level in Indonesia, 2003. Exclude highest conflict areas.	Surveys. Census - education attainment. Best vs. least endowed groups, poverty/ inequality within village.	All conflict. Indonesian gov't Village Potential Statistics (PODES), 2003.	<i>HI multiple effects</i> --HI decreases violent conflict in rural areas. No effect in urban areas.
Østby et al. (2011)	Provinces, 1990 – 2003. Possible 394 province-years (33 provinces), but due to missing information 314 (25 provinces).	Surveys. Demographic and Health Surveys (1987, 1991, 1994, 1997, and 2000–2001. The variable GCOV is based on infant mortality rates (IMR) between religious groups.	Violence onset— <i>Routine</i> : group-based vigilante violence/popular justice and inter-group/ neighborhood brawls. <i>Episodic</i> : ethno-communal and separatist violence, UNSFIR Database I (26 provinces, 1990–2001), UNSFIR Database II.	<i>HI multiple effects</i> --Where population growth is high, religious HI increases violence risk. No effect where population growth low.
Theisen and Slettebak (2011)	Provinces 1990-2003	Surveys. Data from Østby et al. (2011). Disasters is main IV.	"low-level violence", dummy variable of "collective" violence.	<i>HI has no effect on conflict</i> --religious horizontal inequality negatively related to fatal episodes . . . [but] increases risk of routine violence. "Both effects are undistinguishable from zero however."
Tadjoeddin (2013)	Provinces, 1997-2002	Reported data. Spatial inequality (district relative to region) – figures of religious HIs overtime in Poso and Ambon.	Severity index of ethnic violence (any type) across districts, UNSFIR I and II	
Tajima (2013)	Indonesian villages	Surveys. 2000 Census. Educational attainment, ethnic and religious	Communal violence, PODES 2003, 2006 -- Sep 2001 to Aug 2002; Jun 2004 to May 2005.	<i>HI decreases conflict</i> --Result not in main paper, but in Appendix G of online material

		groups, village, subdistrict, and district levels.		
Deter and Nimeh (2014)	Indonesia (1993-1997)	Surveys. HI indicators across five dimensions: health, employment, education, housing and network connectivity. RAND Indonesian Family Life Survey 1993, 1997, 2000 and 2007. IFLS survey covers thirteen (of the thirty-four) Indonesian provinces.	Deadly ethno-communal violence, NVMS. The dependent variable measures whether deadly ethno-communal violence took place in the district during the period 1998-2003.	<i>HI increases conflict</i> --Plays larger role for linguistic groups than religious groups. "Converging" HI positively related to <i>access</i> indicators (under- or unemployed and child enrolment). For <i>achievement</i> indicators (malnutrition, underpaid employment, and household consumption) "diverging" HI matters.
Tadjoeddin (2015)	Indonesian districts and provinces	Reported data. District level; Gini index of consumption expenditure, based on Susenas data; group Gini and weighted group coefficient of variation (wGCOV) of years of schooling across ethnic/religious groups, census	NVMS, 2005–2012, inverse measure of societal stability including routine & ethnic violence (incidents and fatalities), and violent crime (number of incidents)	<i>HI increases conflict</i> --vertical inequality explains routine violence and violent crime, HI explains ethnic violence
Gubler, Selway, and Varshney (2016)	post-Suharto Indonesia, districts, 1997-2014	Surveys. Income, DHS based on Selway (2011) crosscuttingness measure.	Muslim/Christian communal violence	<i>HI decreases conflict</i>
Tadjoeddin (2010)	Indonesia, districts	Reported data. District education rank compared to district income/welfare rank (not ethnic HI)	NVMS, ethnic violence (onset, severity index, number deaths)	<i>HI multiple effects</i> --explains onset, but not persistence
Other Asia				
Murshed and Gates (2005)	Nepal	Reported data. HDI indicators and landlessness – all spatial	Civil war fatalities (intensity of conflict)	<i>HI increases conflict</i> --Discusses caste HI in the paper, but they not feature in model
Nepal, Bohara, and Gawande (2011)	3857 Nepalese villages 1996-2003	Surveys. Nepal Living Standard Survey (NLSS) and census.	Civil war fatalities per village	<i>HI increases conflict</i>
Joshi and Mason (2010)	Nepal district-level	Reported data. Same measures as Murshed and Gates – district vs. national HDI (spatial)	level of civil war violence (fatalities)	<i>HI increases conflict</i> --Those with resources use violence against peasants who might support insurgency

Cao et al. (2016)	county level in Xinjiang from 1990 to 2005	Surveys. Interethnic inequality, education—secondary school completion (Census), prefecture level (not county)	Ethnic Violence (terrorism, insurgency, riots, demonstrations/protests, and assassinations) onset, Ethnic Violence in China (EVC) database: the Xinjiang Region event data set	<i>HI increases conflict</i> --higher inter-ethnic inequalities are associated with increased ethnic violence only in areas with low and medium levels of mosque density.
Cao et al. (2016)	county level in Xinjiang from 1990 to 2005	Surveys. Interethnic inequality, education—secondary school completion (Census), prefecture level (not county)	Ethnic Violence (terrorism, insurgency, riots, demonstrations/protests, and assassinations) onset, Ethnic Violence in China (EVC) database: the Xinjiang Region event data set	<i>HI increases conflict</i>
Vadlamannati (2011)	9 NE Indian states. 1970-2007	Reported data. Poverty relative to national levels, economic and political discrimination. Economic and Political Discrimination, MAR.	Civil war onset, UCDP/PRIO	<i>HI increases conflict</i>
Gomes (2015)	India, 360 districts	Reported data. Income; socioeconomic and geography data from myriad sources	Terrorist attacks per year, Global Terrorism databases	<i>HI increases conflict</i> --Growth of incomes of Scheduled Tribes decreases intensity of conflict.
Syed, Saeed, and Martin (2015)	5 Pakistani regions 1980-2010 (capital + 4 provinces)	Abstract does not specify	Terrorist attacks	<i>HI increases conflict</i>
Brueck, Kroeger, and Vothknecht (2012)	Kyrgyzstan	Surveys. household and individual survey data	Riots & ethnic violence: Violent incidences during the June riots, as proxied by the share of displaced households.	<i>HI increases conflict</i>
Africa				
Hegre, Østby, and Raleigh (2009)	Liberia local level	Geo-coding. DHS, geographical cells of approximately 76 km ² as units of analysis	ACLED dataset, events include battles, headquarter or base establishment, violence to civilians, and rebel presence. Events can occur between any political actor (i.e.,	<i>HI has no effect on conflict</i> --War events were more frequent in richer locations. Better support for “opportunity” explanations than for “relative deprivation” theories.

			government, rebels, and militias). 90% are civil war events.	
Rono (2009)	Kenya	Surveys. Afrobarometer: Income polarization, regional mean income, changes in land inequality; Access to basic services	Support for violence, Afrobarometer	<i>HI increases conflict</i>
Kniss (2010)	Kenya	Reported data. Constituency-level indicators (land, education, jobs, political power) obtained from Kenya Central Bureau of Statistics	Election violence, number killed/wounded, Jones and Silverstein, "Kenya Dataset: April-January, 2008".	<i>HI increases conflict</i>
North America				
McCall and Parker (2005)	US urban areas	Surveys. Racial competition and racial inequality, census, changes between 1980 and 1990	Average number of homicides involving an offender and victim of different races	<i>HI increases conflict</i>