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Inequality and political violence: A review of the literature

Gudrun Østby

Peace Research Institute Oslo, Norway

Abstract

Does economic inequality breed political violence? For almost half a century, scholars have tried to test this assumption, finding little empirical support for a statistical relationship between the two variables. This article provides a critical review of this literature, starting out with the link between so-called vertical (inter-individual) inequality and conflict. I argue that the lack of empirical results can largely be attributed to the almost exclusive focus on individual-level differences in terms of income or land. Group identity is critical to recruitment and maintaining allegiance to a military organization. Hence, we should focus the attention on the relevant form of inequality—that between groups, or so-called horizontal inequalities. In contrast to the studies focusing on vertical inequality, an emerging quantitative literature on horizontal inequalities and conflict has found a positive link, which is more in line with the evidence from several case studies. However, measuring horizontal inequalities is a clear challenge, and there is a need for additional studies to qualify the initial findings. I conclude by suggesting some avenues for future research.

Keywords

Conflict, horizontal inequality, political violence, surveys, vertical inequality

Introduction

Three key features of the national income distribution are central to policy debates—the mean (or average) of the distribution, the spread of the distribution and the lower tail of the distribution. The rate of change of the first is simply the rate of *growth*; the second is captured by various measures of *inequality* (such as, e.g., the Gini coefficient); and the third tries to delineate *poverty*. To measure the latter we need to specify the poverty line, or a cutoff that defines the poor (Kanbur, 2007: 1). In theory, inequality can both rise and fall when average growth is rising and poverty is falling.¹

The starting point linking economic development, or the growth of such, and income inequality dates back to the well-known work of Nobel Prize Winner Simon Kuznets. Kuznets (1955) developed his basic idea of an inverted U-relationship between economic growth and income inequality. In short, he hypothesized that, in the process of industrialization, inequality would first increase,

Corresponding author:

Gudrun Østby, Peace Research Institute Oslo, Hausmannsgate 7, N-0186 Oslo, Norway.

Email: gudrun@prio.no

because of the shift from agriculture and the countryside to industry and the city, and then decrease as returns across sectors equalized. Today there is no empirical consensus as regards the link from development to inequality, but most recent contributions seem to cast doubt on the Kuznets curve, mostly failing to find an effect from growth to inequality (Fields, 2001).²

It seems to be generally accepted, though, that poverty breeds *conflict*. However, while in a general sense it seems plausible that poverty can create the desperation that fuels conflict, the precise nature of the causal linkage is not quite so evident. There are poor societies that are remarkably peaceful, and richer societies that are mired in violence. Moreover, within countries it is not necessarily the case that the wealthier areas are any less immune from political violence (Buhaug et al., 2011). Finally, there is evidence that those who direct extreme forms of violence are not themselves particularly impoverished (see Østby and Urdal, 2010: 17–22). Hence, although it is generally true that violent conflict is a feature of poorer rather than richer societies, wealth can provide the means for conflict as much as take away the reason for it, and the balance of forces is delicate and country specific (Kanbur, 2007).

The links between inequality and conflict are subtle as well, and subject to much greater debate, which is the focus of the current article. In the following I offer a review of the relationship between socio-economic inequality and political violence. This is a response to a major puzzle that has long featured in the academic literature on political violence. I refer to this puzzle as the *quantitative–qualitative mismatch*.

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

In contrast to this statistical rejection of the inequality–conflict link, a case-based literature has emerged, spearheaded by the Oxford-based development economist Frances Stewart. She focuses on the role of “horizontal inequalities” (HIs), or systematic economic and political inequalities between ethnic, religious or regional groups, in affecting conflict likelihood and conflict dynamics (see e.g. Stewart, 2002, 2008). The concept of horizontal inequality differs from the “normal” definition of inequality, often referred to as “vertical inequality”, because the latter type lines individuals up vertically and measures inequality over the range of individuals rather than groups. Furthermore, HIs are conceived of as inherently multidimensional, encompassing economic, social and political dimensions,³ unlike previous accounts and measures of inequality that seem to concentrate exclusively on economic inequality (usually operationalized as income inequality or inequality in land distribution). In brief, the horizontal inequality argument states that inequalities coinciding with cultural cleavages may enhance group grievances, which in turn may facilitate mobilization for conflict.

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

If the statistical studies are right, the contradictory evidence from some case studies should be viewed as anecdotal and cannot be generalized further. On the other hand, if the findings from case studies do actually reflect a more universal relationship, then the majority of the statistical inequality–conflict studies must have missed the target with their exclusive focus on inequality between individuals rather than groups.

The article proceeds as follows: in the next section I review the extensive literature on *vertical* inequality and conflict, focusing mostly on large-*N* statistical studies. I then present a theoretical framework that links *horizontal* inequalities with political violence through various mechanisms relating to both motivation and opportunities. This is followed by an evaluation of the existing empirical evidence on the HI–conflict relationship. I conclude by suggesting some avenues for future research.

Vertical inequality and civil conflict

Ideas about human frustration and responses to grievances are inescapably part of the rationale for believing that there is a relationship between inequalities and political violence. Such ideas are not of recent origin. Explanations of aggression and relative deprivation have deep roots in the history of thought. At least since Aristotle (e.g. 350 BC/1984), political theorists have believed that political discontent and its consequences—protest, instability and violence—depend not only on the absolute level of economic wealth, but also on its distribution, that is, inequality between the rich and poor (Nagel, 1974). A remarkably diverse literature, both ancient and modern, theoretical as well as empirical, has coalesced on the proposition that political violence is a function of economic inequality.

In this section I review the most central theoretical arguments and empirical studies of vertical inequality and conflict.⁴ I discuss some of the problems associated with this extensive literature, and suggest how a reconceptualization of inequality may be a solution to the empirical confusion in the field.

Theoretical arguments and empirical findings

Different theoretical approaches to inequality and conflict include Marxist theory of class struggle and revolution (Marx, 1887/1967),⁵ relative deprivation theory (e.g. Davies, 1962; Feierabend and Feierabend, 1966; Gurr, 1970) and theories of ethnic conflict and structural inequality (e.g. Galtung, 1964; Gurr, 1993, 2000; Hechter, 1975; Horowitz, 1985). What these theories have in common is the interpretation of conflict as a result of widely felt grievances among the relatively disadvantaged in society.

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

As Marx had articulated in the nineteenth century the discontent arising from political oppression and economic exploitation, psychologist Sigmund Freud provided a theory expanding such ideas in the direction of frustration and alienation. Freud (1920/1950) regarded the tendency to seek pleasure and avoid pain as the basic goal for individuals. Frustration was expected to occur whenever pleasure-seeking or pain-avoiding behavior was blocked. He believed that the natural reaction to this state of affairs would be aggression, normally directed toward those persons or objects that were perceived as the source of the frustration.

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

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

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

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

Early on, the theory of relative deprivation attracted criticism from advocates of what has come to be called the “resource mobilization”– or “mobilization opportunity” approach to the explanation of collective violence and protest (e.g. Snyder and Tilly, 1972; Tilly, 1978). They reject grievance explanations hypothesises for the reason that inequality and discontent are more or less always present in practically all societies (see also Skocpol, 1979). Hence, they believe that the most direct and influential explanatory factors are not perceived grievances, but rather financial and political *opportunities* for mobilizing a rebel organization. Furthermore, a series of statistical studies challenged the results pertaining to income inequality, which was usually seen as the main indicator of relative deprivation (Weede, 1981). In theory there are five possible relationships between economic inequality and political conflict: positive, negative, convex (inverted U-shaped), concave (U-shaped) or null. The literature includes examples of all.⁶

The pioneering cross-national research on the inequality–violence relationship was Russett’s (1964), who documented moderate correlations between inequality in land tenure systems and political instability in 47 countries. A subsequent study by Parvin (1973) came to the opposite conclusion. Working with a sample of 26 predominantly Western nations, he found that inequality

proved to be only marginally significant and even inversely related to political unrest. Nagel (1974) tried to combine the two assumptions and resolve the contradiction. The discontent triggered by inequality, Nagel believed, consisted of the tendency of individuals to compare wealth (a tendency he assumed was *inversely* related to the amount of objective inequality), and the extent of the grievance resulting from such comparisons (a direct function of inequality). Combining these two factors multiplicatively, Nagel suggested that the inequality–violence relationship resembled an inverse U-curve, with political violence most likely at intermediate levels of inequality. He found some support for this assumption in a study of Vietnamese provinces, but not with a cross-national sample.⁷

Sigelman and Simpson (1977) were the first to have access to personal income data. They assumed that income inequality would have greater violence potential than land inequality because in many societies—particularly those at higher development levels—life chances are not so closely connected to land ownership. They found some support for a linear relationship between the Gini index⁸ for personal income inequality and internal war, but concluded that “the overall level of societal well-being is a more critical determinant of political violence than is income inequality” (Sigelman and Simpson, 1977: 124). The latter was supported in a subsequent study by Weede (1981), who found a strong impact of average income, but no effect of inequality on collective violence.

Integrating relative deprivation theory with the resource mobilization approach, Muller and Seligson (1987) postulated that, whereas a high level of income inequality nationwide would significantly raise the probability that at least some dissident groups would be able to organize for violent collective action, a high level of agrarian inequality would not have the same effect on collective violence because it would be more difficult to mobilize people in the countryside. They found support for a positive relationship between income inequality and domestic conflict, whereas land inequality was found to be relevant only to the extent that it was associated with the nationwide distribution of income inequality.

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

Overall, relative deprivation theory remains the most prominent explanation that connects inequality (as well as other grievance-related factors) with conflict. However, despite the persistence of the theme, grievance models have not fared well in the contemporary empirical literature on inequality and conflict (see Blattman and Miguel, 2010). In the mid-1990s, World Bank researchers Deininger and Squire (1996) presented a new dataset on income inequality, which was later expanded into *The World Income Inequality Database* (UNU/WIDER and UNDP, 2000). These data represent a great improvement in terms of quality and spatio-temporal coverage compared with previous datasets, and soon became the standard source of inequality data. Subsequently, in virtually all cross-country regressions of civil conflict, economic inequality is not significant.⁹

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

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

Problems with the inequality–conflict literature

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

Methodological objections. It has been argued that the contradictory inequality–conflict results are due to variations among the studies in all aspects of research design (see e.g. Cramer, 2001, 2003; Lichbach, 1989; Zimmerman, 1983). Various critics have suggested that the inconsistent conclusions arise from a lack of essential control variables, from the different cases and time frames in which the effects of conflict are examined, and not least from poor data and inadequate level of analysis.

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

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

One of the most serious objections to previous empirical studies concerns the poor data on income inequality and the high level of missing observations. Before the Deininger and Squire (1996) dataset, and the recent appearance of the World Income Inequality Database (UNU/WIDER

and UNDP, 2000), cross-national data on inequality were distressingly scarce and imprecise. Yet, with this progress, the problem of a very large amount of missing data is still present. Deiniger and Squire include inequality data from quite a limited number of countries and years.¹⁰

Worse than the problem of poor and missing data itself is the problem that arises when the pattern of missing data is non-random or biased. Many countries do not have any inequality data at all. Trying to locate income inequality for these countries, Strand and Gates (2002) put a request to the Scientific Study of International Processes¹¹ listserv for information, and got the following answer from Phil Schrodtt (quoted in Strand and Gates, 2002: 5–6):

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

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

The point is that, in a situation in which we are less likely to have inequality data for conflict-ridden countries, this bias could imply that we infer that the effect of inequality on conflict is weaker than it actually is (see e.g. Gates, 2004).

A final methodological caveat relates to the level of analysis. Civil wars often take place within limited areas within countries. Since features of wealth and income distributions tend to vary considerably within countries, the use of national level indicators of inequality to explain variations in civil conflict is likely to be inappropriate (see e.g. Buhaug and Lujala, 2005; Buhaug et al., 2011). Based on a similar reasoning, Cramer (2001: 3) describes the national-level Gini coefficient as a “superficial outward sign of inequality”. His point is illustrated by the examples of Indonesia and Rwanda, which are commonly regarded as two countries with low Gini coefficients. Cramer claims that drawing from the published data on inequality to conclude that either of these two countries is a low-inequality country would be misleading, or even absurd:

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

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

Conceptual and theoretical objections

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

The standard (technical) critiques about lacking control variables, restricted samples, poor data and inappropriate level of analysis may apply to most of the studies reviewed. However, I contend that there is a more fundamental problem that produces the conflicting results concerning the inequality–conflict relationship: one of the most important flaws of the quantitative studies of inequality and conflict may be conceptual. My first conceptual objection is that, in the inequality–conflict literature, most attention has been focused on inequality between individuals. However, the topic of interest, violent conflict, is a group phenomenon, not situations of individuals randomly committing violence against each other. Group identity is critical to recruitment and maintaining allegiance to a military organization. Hence, we should focus the attention on the relevant form of inequality—that between groups.¹²

Such reasoning is supported by psychological experiments. For example, Brewer (1991: 478–479) concludes that individuals derive value from the group to which they belong. The willingness of individuals to make any sacrifice for group action is predicted more by a sense of collective rather than individual relative deprivation. Improvement of the group's condition, in other words, may be a more powerful motivation to participate in collective actions than improvement of the individual's condition. I recognize that ethnic or religious groups are to some extent socially constructed, sometimes with fluid membership. Nevertheless, the relative performance of identity groups is an important source of individual welfare, and can hence cause serious conflicts where structural economic or political differences coincide with cultural cleavages (see e.g. Stewart, 2002). Also, as demonstrated in the first section, vertical and horizontal inequalities do not necessarily overlap.

My second conceptual objection concurs with Sen (1992) and Stewart's (2002) complaint that most studies of the relationship with inequality have exclusively focused at *economic* inequality (usually measured by income). I have consistently talked about horizontal *inequalities* in plural. This choice of words is not incidental. In order to fully explore the inequality–conflict nexus, one should study various dimensions of inequality in addition to the strictly economic dimension operationalized as income. Sen (1992) asks an essential question: "Equality of what?" Given the fact that the human population is different in many respects, it is important to remember that inequality can be much more than just income inequality measured, for example, by the Gini index. Sen (1992, 2006) focuses on three different categories (or "spaces") of equality—equality of income or other financial assets; equality of welfare; and equal rights and liberties—and argues that the various categories of equality cannot be combined perfectly, since the differences in environmental factors and human capacities influence the final outcome. Stewart (2008) also stresses that horizontal inequalities are multidimensional—with political, economic and social elements (as indeed are vertical inequalities, but they are rarely measured in a multidimensional way).¹³

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

For the reasons presented above, we cannot conclude from the extant literature that inequality is unrelated to political violence. On the contrary, I argue that the rejection of the inequality–conflict nexus is at best premature. Indeed, it could be the case that vertical inequality in a homogenous population, despite the class differences it engenders, does not seriously increase the risk of conflict, but that could still leave a role for group inequality. Yet, most of the contributions reviewed here ignore how different dimensions of inequality are institutionalized and shaped by history and

Table 1. Typology of different forms of relative deprivation.

Time perspective	Aggregation level	
	Individual	Collective
Diachronic	(a) Intra-individual	(b) Intra-group
Synchronic	(c) Inter-individual	(d) inter-group

various social and cultural cleavages, and how such inequalities can be translated into collective violence. A more promising avenue to capture the inequality–conflict link has been taken by Stewart (e.g. 2000, 2002, 2008) and her collaborators, who focus on the role of horizontal inequalities, or “inequalities in economic, social or political dimensions or cultural status between culturally defined groups” (Stewart, 2009: 3). In the next section I outline the theoretical framework for studying horizontal inequalities and political violence showing that HIs can spur violent group mobilization through both grievance-based and opportunity-based mechanisms.

Theorizing horizontal inequalities and political violence

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

Less commonly discussed, but more important for empirical studies of inter-ethnic conflict is what Boswell and Dixon (1990: 542) refer to as synchronic relative deprivation, which is usually measured in terms of income distribution. Add to this that the literature has distinguished between individual vs collective relative deprivation. According to social identity theory, individuals’ investment in their membership group and the salience of group boundaries increase the likelihood that relative deprivation will be experienced in its collective form (Walker and Smith, 2002). Yet, most studies of inequality and conflict operationalize relative deprivation at the individual level by various measures of vertical inequalities, such as the Gini coefficient.

If we combine the distinctions diachronic/synchronic¹⁴ and individual/collective relative deprivation in a 2×2 matrix (see Table 1), alternative (d)—collective synchronic relative deprivation—comes closest to the concept of horizontal inequalities. However, there is one important feature that distinguishes the HI approach from that of relative deprivation (see Stewart, 2008). Whereas relative deprivation theory by definition focuses on the motives of the disadvantaged in society, the HI thesis stresses that it is not only resentment among the deprived that may cause political instability—although this clearly seems to be the case in many disputes (e.g. the Hutus vs Tutsis in Rwanda or race riots in industrialized countries). The relatively privileged can also attack the unprivileged (or the state) as a reaction to what they may perceive of as unfair redistribution, or out of fear that the relatively deprived may demand more resources and political power (e.g. the Biafra war in Nigeria, or the Basque conflict in Spain).¹⁵

Origins of horizontal inequalities

There can be many causes and origins of systematic differences between different ethnic, religious groups or regions. They relate to different factors such as ecological and climatological differences, the distribution of natural resource endowments, the differential impacts of colonialism, as well as various economic policies (Brown and Langer, 2010: 30). Horizontal inequalities often have their origin in historical circumstances—often colonial policy which privileged some groups over others. Sometimes, however, horizontal inequalities are not caused by deliberate agency at all but simply become evident, for example when traditional peoples on the periphery of modernizing societies are drawn into closer contact with the more powerful and technologically proficient groups (see Gurr, 2000). Furthermore, as shown in Østby (2008b), HIs tend to reproduce over time, sometimes lasting for decades. An initial advantage often leads to long-term cumulative advantages, as resources and education allow the more privileged groups to secure further advantages (Stewart, 2009). For example, children growing up in poor communities usually have less access to good schooling and must travel further, in social and geographical terms, to raise their own children out of poverty. According to HI theory, the risk of violent group mobilization should be higher when people are convinced that their socio-economic deprivation is caused by deliberate discrimination by the state. Conversely, if a country's government introduces policies designed to reduce HIs, this may reduce the political salience of the prevailing HIs, even when the actual redistributive effect is rather limited (Brown and Langer, 2010: 31).

How do HIs spur collective action?

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

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

As mentioned above, conflict can be initiated by both relatively deprived and relatively privileged groups, although the literature tends to focus mostly on the former. According to Horowitz (1985), in stratified social systems, social comparison reflecting superiority or inferiority should be particularly likely to trigger conflict. Gurr (2000) found that ethnic groups are in fact often subject to economic discrimination to the extent that their members have been systematically limited in access to desirable economic goods and conditions. Whether such inequalities are due to overt discrimination or not, unequal access to economic resources by different groups can provoke

collective grievances. In the words of Petersen (2002: 40) “resentment is the feeling of being *politically* dominated by a group that has no right to be in a superior position”. Conversely, groups that are relatively advantaged may also experience collective grievances owing to their fear that the deprived groups may gain political power and demand more resource redistribution, or turn to armed aggression to redress their grievances. For example, inter-regional inequality is often associated with inter-regional transfers from richer to poorer regions. If richer regions view these transfers as too large, this increases grievances, and they may seek to secede or they can push poorer regions to exit by insisting on a lower transfer rate (Sambanis and Milanovic, 2001: 12). In a study of conflict in Indonesia, Tadjoeeddin (2007: 23) refers to the demands of the richer regions for a degree of community welfare that corresponds to their relative high regional prosperity as “aspiration to inequality”.

It is clear that collective grievances do not automatically lead to violent action. Without resources and organization, grievances as such can do little to challenge powerful defenders of the status quo (Tilly, 1978). Even Gurr (1970) admitted that affect and frustration are insufficient to create rebellion. In his rational-actor model of political violence, Tilly (1978) argued that only when resources, organization and opportunity become available will people mobilize for collective action, including rebellion, if they calculate that it is in their interest to do so. For Tilly (1978: 7), opportunity concerns the relationship between a group and the world around it. It may come from a government weakness or an opposition organization’s calculation of its own strength. Gurr (1993: 130), on the other hand, distinguishes between *internal* and *external* opportunities for a group to mobilize. Opportunity factors internal to the group are the salience of group identity, networks among its members and the extent of common grievances. Opportunity factors external to the group include the character of the state and its resources, and whether the group has transnational kindred.¹⁶ According to Gurr’s theory (2000: 95), the factors of identity and frustration are innate in a group’s internal opportunities to mobilize.¹⁷ Internal opportunities are seen as the elements from which skillful leaders and ethnic entrepreneurs build collective movements for political action. However, the timing of action and the choices of strategies of participation, protest or rebellion depend largely on political and economic opportunities *external* to the group, such as the repressiveness of the state (see also Østby and de Soysa, 2008).

Whereas political scientists such as e.g. Gurr (2000) tend to focus mainly on the *political* opportunities external to the group (e.g. regime type), economists such as, for example, Collier and Hoeffler (2004), focus more on *economic* opportunities (e.g. in terms of the extortion of lootable natural resources), treating the objective of rebellion as financial gain. I argue that the level of socio-economic horizontal inequalities may influence a rebel group’s calculation of its own strength and thus serve as an indicator of the internal opportunities for a group to mobilize. Soldiers must be paid, and the cost of recruiting is related to their income forgone by enlisting as rebels. By definition, richer groups are more capable of supporting a rebel group with economic contributions and other kinds of material resources. On the other hand, members of the relatively disadvantaged groups are more likely to enlist as rebel recruits owing to lower opportunity costs, and because perceptions of injustice generate grievances that serve as a strong tool for recruitment. A final source of opportunity, or the ability to take coordinated action, is strong group cohesion, which in turn is often strengthened by commonly felt grievances. Figure 1 summarizes my argument, visualizing how various aspects associated with horizontal inequalities influence the risk of violent group mobilization.¹⁸

In short, Figure 1 shows that HIs imply both relative deprivation (a) and relative privilege (b). relative deprivation may lead directly to collective grievances (c), whereas relative privilege is assumed to lead to grievances via fears of potential or real redistribution (d, e). Grievances of both

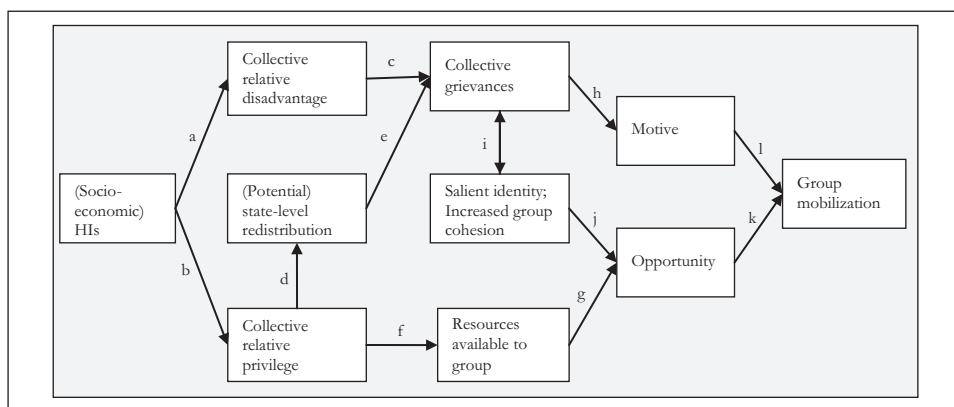


Figure 1. Mechanisms linking horizontal inequalities to group mobilization.

types are likely to constitute a strong motive for collective action (h). Furthermore, richer groups have more material resources (f), which implies better opportunities to establish and sustain a rebellion (g). However, opportunity can also increase as a result of strong group cohesion (j), which in turn is reinforced by and reinforces collective grievances (i). Finally, violent group mobilization is assumed to result from a combination of opportunity (k) and motive (l). Many of these individual mechanisms (arrows) have not yet been tested empirically, but together they form the theoretical basis from which various hypotheses can be developed.

Clearly, horizontal inequalities do not exist in a vacuum. If HIs are really as static and persistent as they are often considered to be (see e.g. Ahluwalia, 1976; Østby, 2008b; Williamson, 1965), there is good reason to assume that contextual factors play a significant role in translating HIs into conflict, particularly with regard to state regulation and the absolute economic level. Such mediating aspects of relevance can be: political conditions (regime type, electoral system, political exclusion; see e.g. Østby, 2008b), economic conditions (natural resources; see e.g. Østby et al., 2009) and demographic factors (population growth; see e.g. Østby et al., 2011).

HIs and different types, features and locations of political violence

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

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

It is beyond the ambition of this article to construct a thorough typology of political violence and discuss and test whether and how HIs are associated with each possible variant of this broad phenomenon. Nevertheless, the literature offers some discussion and insights as to whether HIs are

more likely to produce some forms of violence rather than others. For example, in Østby et al. (2011) we postulate that HIs should have stronger effect on episodic violence than on routine violence. The rationale behind this expectation is that low-scale “routine” violence usually has little or nothing to do with ethnic groupings. In line with this, Tadjoeeddin et al. (2010: 6) hold that “horizontal inequality is more appropriately located in the context of secessionist and ethnic conflicts”, an argument that resonates with recent findings by case studies (e.g. Sambanis, 2004). Also, in a quantitative analysis of subnational regions in Sub-Saharan African countries, Fjelde and Østby (2010) find evidence for a positive relationship between horizontal inequalities between ethnic groups and the onset of communal conflict, that is, conflicts that do not directly involve the state as one of the belligerents.

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

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

HIs and conflict: Measurement and empirical evidence

As we have seen, so far, most researchers of the horizontal inequality–conflict relationship have relied on qualitative case studies rather than large-*N* comparisons (e.g. Humphreys and Mohamed, 2005; Langer, 2005; Sambanis, 2004; Stewart, 2002, 2009). The main picture that emerges from these works is that horizontal inequalities are indeed associated with increased risks of political violence, ranging from occasional racial riots (e.g. Malaysia) to full-blown civil war (e.g. Uganda, Sri Lanka, South Africa). Another conclusion from this research is that both disadvantaged and advantaged groups have a higher likelihood of getting involved in internal conflict than groups closer to the national average (Stewart, 2009). There is also a handful of quantitative case studies that have addressed the HI–conflict link (e.g. Barron et al., 2009; Mancini, 2008; Østby et al., 2011 on Indonesia; Hegre et al., 2009 on Liberia; and Murshed and Gates, 2005 on Nepal), and these studies largely support the conclusions from the qualitative literature.¹⁹

Although Stewart (2000) was the first analyst to explicitly use the term “horizontal inequalities”, a handful of scholars have attempted to study structural differences between ethnic groups on a cross-national (or cross-group) basis. In a pioneering test Barrows (1976) analyzed the

determinants of political instability in 32 African states south of the Sahara during the 1960s. In a multiple correlation analysis he found that inequality was a consistent predictor of political instability when measured along a scale of “ethnic group inequality” based on “the size of ethnic groups and their share of political power and/or other values [wealth, education and the like]” (Barrows, 1976: 154–155). Barrow’s study is particularly noteworthy since it appears to be the first attempt at measuring horizontal inequalities quantitatively. A major problem with his index, however, is that his personal judgment was the only source for determining the group inequality scores for each country. Another early strand of research on HIs and violence is Blau and Blau (1982) and others’ work on Black/White relationships in US cities, which explores whether riot incidence is related to economic and social characteristics of the cities, including horizontal inequalities. Although some of these studies have led to mixed results (see Balkwell, 1990: 54–55), the general finding is a positive relationship between racial inequality and violence.

More recently, Gurr (2000) has developed an index of political, economic and cultural disparities for some 275 minority groups in 116 countries. Based on the “Minorities at Risk” (hereafter MAR) database, he found that where there are strong identities *together with* large group grievances (i.e. major political, economic, or cultural differences/discrimination), protest is more likely. Gurr’s data, thus, provide strong support to the hypothesis that horizontal inequalities are liable to lead to political violence.²⁰

A couple of cross-national investigations by Østby (2008a, b) represent the first attempts at measuring objective HIs based on data from a set of national Demographic and Health Surveys (DHS).²¹ The main conclusion from these and subsequent disaggregated studies (e.g. Østby et al., 2009, 2011; Hegre et al., 2009), as well as a recent paper by Condra (2009)²² largely support the validity of the positive relationship between various forms of HIs and conflict.

Further support for the HI-political conflict nexus is reported by Cederman et al. (2011), who provide a new global dataset on economic HIs, combining their newly coded data on ethnic groups’ settlement areas (Wucherpfennig et al., 2011) with Nordhaus et al.’s (2006) G-Econ dataset on local economic activity. In short, they find that, in highly unequal societies, both rich and poor ethnic groups fight more often than those groups whose wealth lies closer to the country average. In a related study Deiwi et al. (2012) adopt a spatial approach, combining the G-Econ data with geo-coded data on administrative units in 31 federal states between 1991 and 2005. They find that, in highly unequal federations, both relatively developed and underdeveloped regions are more likely to be involved in secessionist conflict than regions close to the country average.

In the following I discuss some strengths and weaknesses of the survey-based approach to studying the HI–conflict link compared with that to that of Cederman et al. (2011). First, however, I comment briefly on some important aspects pertaining to the collection of HI data: potential data sources, measurement issues and level of analysis.

Data and measurement issues

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

It is a formidable challenge to get at objective and comparable data on horizontal inequalities. Horizontal inequalities (and vertical ones alike) can be politically sensitive, and national and sub-national governments are likely to report biased data, if any. One solution to the problem has been to construct HI data based on national surveys which include information on both socio-economic well-being and ethnic/religious/regional group affiliations (see e.g. Østby, 2008b).²³

Starting in 1985, the DHS surveys now cover over 85 countries, providing accurate, nationally representative data on fertility, family planning, maternal and child health, gender, HIV/AIDS, malaria and nutrition. The DHS surveys were conducted primarily to provide researchers and policy-makers with comprehensive and comparable data on fertility and child health and their determinants, and the DHS project has become the gold standard of survey data in the population and health sector in developing countries. Apart from health and nutrition indicators, most of the surveys also include a host of question relating to socio-economic background factors such as the possession of various household assets (such as electricity, a radio and a refrigerator) and education levels. Furthermore, the surveys include information about the region of residence of the respondents, and sometimes, but not always, information on ethnic and religious affiliation. In a typical DHS survey, a sample of households is selected throughout the entire country and then interviewed using a household questionnaire to collect housing characteristics. Women between the ages 15 and 49 are interviewed using a women's questionnaire to collect information mainly on background characteristics, children and women's health and other issues, such as education level. During the last decade, the DHS has begun to include detailed information about the geographical location of each enumeration area. Together, these surveys provides a very rich dataset, from which one can construct reliable and valid group inequality indicators. The DHS surveys can be used to generate HI measures based on different group indicators: ethnicity, religion, region or locality of residence and migrant status. They also allow measurement of inequalities along various dimensions, using variables pertaining to household assets, educational levels and infant mortality rates.

Biased information on HIs is very unlikely when data are generated from national surveys like the DHS, as the original intention behind these was far from assessing socio-economic inequalities between ethnic groups. Finally, the aggregation of survey data ensures descriptive rather than evaluative data. That is, researchers do not need to rely on their personal judgment as the sole source for determining group inequality scores (as opposed to, e.g., the Minorities at Risk Project).

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

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

where worst refers to the average share of some asset owned by members of the poorer group and best refers to the average share of the asset owned by members of the richer group. The measure potentially ranges from 0 (perfect equality) to 1 (horizontal inequality with the richer group owning all the assets). In Østby (2008a: 151) I propose an application of this formula calculating HIs between the two largest ethnic groups per country, disregarding the status of smaller ethnic groups, and also compare HI measures with measures of polarization. Although Stewart (2009: 41) points to the problem of how to deal with intra-group inequalities, Mancini et al. (2008: 90) state that "we wish to separate our measure of HIs from what is happening within the group". Also, in an earlier paper, Stewart (2002: 12) stresses that "we need to measure intra-group as well as inter-groups differentials in order to explore how intra-group differentials affect the consequences of HIs". To

the best of my knowledge, there still is no ideal HI measure which fully combines information on both inter- and intra-group inequalities as well as relative group sizes.

In general, Stewart and her team focus mostly on aggregate situations, taking an overall (as opposed to a group-specific) perspective on measuring HIs. Along similar lines, Murshed and Tadjoeeddin (2009) insist that inequality has to be measured at the level of the nation state. However, any country-level measure of HI risks failing to capture the relevant groups in society. In fact, the horizontal inequality argument only requires one under-privileged group to predict conflict. If the rest of the population in the country is homogenous or has small income differences, a country-level measure would be attenuated and unable to capture this. This problem is present in any country-level study of HIs and conflict. This problem can be minimized by disaggregating the study of conflict below the national level (see e.g. Østby et al., 2011). In this way one can simply compare the group (or region) of interest with some other unit (e.g. the capital or the national average), and hence stick to simple ratio measures or group-specific measures. With a disaggregated design it is also easy to add an individual variable for intra-group inequality into the model (see e.g. Kuhn and Weidmann, 2013). In addition, there are other good reasons to disaggregate the study of conflict, not least the fact that it is rarely the case that a conflict engulfs an entire country (see e.g. Buhaug and Gates, 2002; Buhaug and Rød, 2006). The fact that DHS surveys now tend to include the exact geographical coordinates for each sampled village or town has opened up new possibilities for measuring spatial inequalities within countries.

Evaluating the evidence: Geographical scope vs data quality

Cederman et al. (2011) provide a new global dataset on economic HIs, combining their newly coded data on ethnic groups' settlement areas (Wucherpfennig et al., 2011) with Nordhaus et al.'s (2006) G-Econ dataset on local economic activity. The G-Econ dataset tries to assemble the best available data on local economic activity within countries for geographical grid cells, and convert these to comparable figures in purchasing power parity to allow for meaningful comparisons. The resolution of the spatially explicit data set is 1° grid cells. The data are constructed from a variety of sources, including regional gross product data for the lowest available political subdivision, estimates of regional income by industry, and estimates of rural population and agricultural income. The specific methodologies differ by countries and data availability (see Nordhaus et al., 2006 for a detailed discussion). The database has global coverage, but the temporal scope is limited to a single year, 1990.

Cederman et al. (2011: 483) admit that the DHS “offer a relatively direct measure of well-being”, but point to a number of limitations afflicted with the use of DHS to create HI measures. Most importantly, they point to the restricted geographical coverage, and the focus on developing countries. They also mention potential problems associated with representativeness at the subnational level and potential response biases, such as the possibility that poorer individuals might overstate (or understate) their assets. As discussed in Østby (2008a) and Østby et al. (2009), these problems should not be too severe, though.

Cederman et al. (2011) are of course right that the DHS cannot be used to evaluate the role of HIs on a global basis. However, this is not to say that the Nordhaus-based HIs data are necessarily superior to the DHS-based HI data for the countries that are included in both databases. First, the Nordhaus data cannot account for the informal economy, which very often benefits groups engaged in agriculture. This is particularly relevant for African and Asian countries where large segments of the population still depend on agricultural livelihoods. Any measure of economic productivity is a “flow measure”, and hence an imperfect proxy for the actual level of income or wealth.

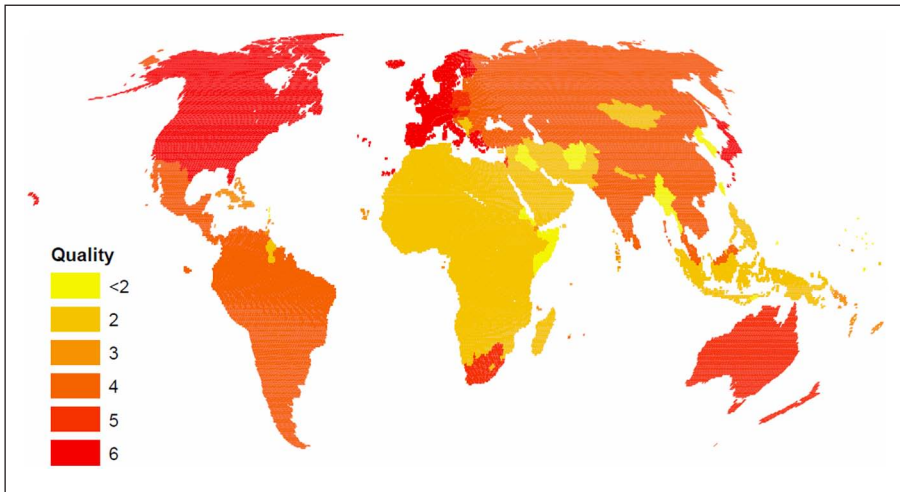


Figure 2. Quality of the G-Econ data.

Source: Map generated by author on the basis of Nordhaus et al.'s (2006) G-Econ data and PRIO-GRID (Tollefsen et al., 2012). Quality designation: -999 = some defect not yet determined; -99 = very small or zero area, to be set at zero area (non-existent); 1 = lowest quality (disputed, essentially non-existent data); 2 = low quality and some regional data; 3 = small islands; 4 = medium quality, developing country; 5 = high quality but not complete or poor resolution regional data; 6 = highest quality data and regional resolution.

Second, and far more serious, a closer inspection of the documentation of the Nordhaus data reveals that the overall data quality is indeed very poor for large parts of the developing world (where most conflicts occur)—exactly where the DHS surveys are conducted. The world map in Figure 2, which depicts the quality of the G-Econ data, with darker shades indicating top quality data, speaks for itself. With the exception of South Africa, the entire continent of Africa has “low quality or *some* regional data”²⁴ (my emphasis). Also parts of Asia, such as Indonesia, have equally poor data. According to Cederman et al. (2011: 483–484) “in some countries the official data may be of such poor quality that the variable is suppressed and accuracies over survey reports may be questionable”. This is at best an understatement.

How well do the two data sources correspond to each other? In Africa, the continent with the poorest data from Nordhaus, the correlation between the survey-generated asset indicator and the G-Econ-based GDPpc (Gross Domestic Product per capita) is $r = 0.61$ at the national level ($N = 38$), compared with 0.41 ($N = 517$) at the subnational, regional level.²⁵ Admittedly, a correlation above 0.6 is not so bad (especially since the variables measure slightly different phenomena). However, the fact that the correlation drops when we go below the national level could very well be a result of the lack of regional variation in the G-Econ data (i.e. that the data are to a certain extent geographically extrapolated). Adding this to the arguments presented above, G-Econ data do not come across as superior to the DHS data for the purpose of measuring horizontal inequalities in developing countries. Although Cederman et al. (2011) manage to construct a global dataset on HIs, their data are nevertheless of poor quality for most of the developing world, which highlights the acute need for high-quality economic data for these countries. Nonetheless, the combination of new geographic data is truly innovative should inspire future attempts along similar lines. However, if King (2001: 505) is right that “good data beats better methods every time”, we still have a lot of work to do. It seems that basic socio-economic indicators from household surveys are indeed a

fruitful starting point for measuring economic HIs in developing countries.²⁶ In any case, the work that relies on survey data (e.g. Østby, 2008; Østby et al., 2009) and that of Cederman et al. (2011) do reach the same overall conclusion: HIs do matter for political violence.

Conclusions and avenues for future research

The main motivation behind this article was to provide a systematic evaluation of the relationship between structural inequalities between groups—horizontal inequalities—and political violence in order to transcend and supplement the broad qualitative case-study literature in the field.

First, I have demonstrated that, in order to grasp the complex relationship between inequality and conflict, we need to move away from a single-tracked concept of inequality, distinguishing vertical from horizontal inequalities, and considering other dimensions than purely income inequality. Traditional inequality studies tend to ignore this diversity.

A second main conclusion from this review is that the political, economic and demographic context matters for the HI–conflict relationship. The articles reviewed above have tested the impact of three kinds of contextual factors which are all found to influence the HI–conflict nexus: political conditions, natural resources and population pressure. In sum there seems to be several relevant contextual factors that could indeed influence the HI–conflict nexus (see Stewart, 2008 for a more comprehensive discussion on this).

A third insight offered by the work presented herein is that subnational studies largely confirm the main conclusion that HIs matter for various forms of political violence. More importantly, disaggregated designs make it possible to unpack the HI measure and single out the separate effects of being relatively deprived and relatively privileged with regard to conflict risk. The disaggregated study of African regions reveals that the overall finding that HIs influence conflict is really driven by the effect from the relatively deprived regions.

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

One obvious challenge for future research in this field is to collect more and better data on HIs for various group identifiers and dimensions expanding the present spatio-temporal domain covered in this project. In addition to general, cross-national datasets, there is a need for carefully designed micro-level studies, which can help us better understand the mechanisms linking HIs to political violence. A wish list for future data collection projects would include designing new household surveys so as to include data on various group affiliations, and objective as well as perceived inequalities along economic, social, political and cultural dimensions. There is also a need for better temporal data on HIs. As time passes on there will be a constant supply of new household surveys from DHS and other sources, which will contribute to better temporal data.²⁷ A related point, noted by Blattman and Miguel (2010), is that it would be extremely useful to have follow-up surveys of the same respondents in post-conflict settings. At present, I am aware of one such survey which was conducted before and after the genocide in Rwanda (Verwimp, 2005).

There is also room for improvement with regard to the measurement of HIs. Existing work by, for example, Esteban and Ray (2011), Mancini et al. (2008) and Zhang and Kanbur (2001) has

provided some useful guidelines and starting points for measuring HIs, but we have yet to see the ideal HI formula which accounts for both group size, intra- and inter-group inequalities. Probably it is not even possible to construct such a summary measure of HI that makes perfect sense, but it is clearly worthwhile to explore various possibilities. Related to this, there is a need for more disaggregated studies of HI and political violence—not only in the spatial sense, but also dissertating by, for example, ethnic groups regardless of whether the group is clustered geographically or not. The Minority at Risk project is currently being expanded to include groups that are judged to be “not at risk” (Birnir and Inman, 2010).

Future efforts should explore the conflict potential of cross-cutting cleavages when it comes to the combination of horizontal inequalities across various dimensions (e.g. social, economic, political; see e.g. Langer, 2005; Østby, 2008b), but also with regard to various group identifiers. For example, when spatial cleavages are reinforced by other social divisions such as ethnicity and religion, the threat of armed conflict should be much greater (see Gubler and Selway, 2012; Rokkan, 1967).²⁸ I would assume that the coincidence between multiple group identifiers and structural socio-economic and political inequalities could be particularly dangerous. The conflict between the rural-based Mayan groups and the urban-richer mestizo in Guatemala and between the poorer Muslim areas of Mindanao and the Sulu Archipelago and the richer Christian areas of the Philippines serve as good examples of this.

Furthermore, as alluded to above, future research within this portfolio should pay more attention to the dependent variable. Previous studies (quantitative as well as qualitative), including those which focus on vertical and horizontal inequalities respectively, have used very different dependent variables, ranging from crime to full-scale civil war. We need a better, more systematic understanding of if and how certain types of inequality relate to different types of political (and non-political) violence.

Finally, as there seems to be quite a robust statistical relationship between HI and conflict, one of the most important challenges for future research should be to better account for the causal mechanisms underlying this relationship. This requires extensive theorizing and carefully selected micro-level studies. In particular we need to better understand the relationship between objective and perceived inequalities.²⁹ Stewart (2009: 16) holds that “people take action because of perceived injustices rather than because of measured statistical inequalities of which they might not be aware”. In general, it is reasonable to assume there to be a high correlation between perceived and observed HIs. However, it is important to study perceptions—and their determinants too—since leaders, the media and educational institutions can affect individuals’ judgment of inequality and their own relative position in society, even when the actual situation remains unchanged.

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Notes

1. The mechanics of growth, inequality, and poverty as defined above are quite straightforward. Holding inequality constant, when growth happens, poverty falls. Holding the mean of the distribution constant, when inequality increases, poverty increases. In this sense, hence, growth is good for poverty reduction

- and increasing inequality is bad for poverty reduction. If one could get growth without the inequality increase, or inequality reduction without a reduction in growth, or both growth and inequality reduction, then poverty would go down (Kanbur, 2007: 2)
2. Some studies with very long time-series for a few countries dating back to the nineteenth century, do find support for the relationship (e.g. Morrison and Murtin, 2007).
 3. This review focuses mostly on various socio-economic dimensions of inequality, such as income, land distribution and education. However, for an introduction to political (horizontal) inequalities, see, for example, Cederman et al. (2011), Østby (2008b) and Stewart (2008).
 4. Several qualitative studies (including some which combine qualitative and quantitative methods) have provided significant theoretical contributions to the literature on the inequality–conflict nexus (see e.g. André and Platteau, 1998; Kling, 1956; Moore et al., 1996; Nagel, 1974; Sambanis, 2004; Stewart, 2008). The present review, however, focuses on the *quantitative* literature. That said, to the best of my knowledge, a thorough review of the qualitative contributions to the inequality–conflict literature is yet not available.
 5. Despite the importance of Marx’s theory of class struggle and revolution, surprisingly little attention has been devoted to class exploitation in cross-national studies of violent political conflict. Exceptions include Boswell and Dixon (1993) and Schock (1996).
 6. Lichbach (1989: 436–439) identified 17 studies which posit that the inequality–conflict nexus is positive. He found only a handful of scholars who anticipate that the relationship will be negative. As regards curvilinear relationships, Lichbach identified six studies, four of which suggest that it is convex and two which suggest that it is concave. Finally, several of the studies failed to find a significant inequality–conflict nexus at all.
 7. Davis (1948) offered another curvilinear interpretation of the inequality–violence relationship, diametrically opposed to that of Nagel. A narrow concentration of incomes, Davis believed, would spur mass resentment and lead to revolution, while a wide dispersion would endanger elite dissatisfaction and ultimately cause civil war. To the best of my knowledge, this has not been demonstrated empirically.
 8. The most common measure of income inequality is the Gini coefficient—an index between 0 and 1 (or 0 and 100) where 0 implies an egalitarian distribution (perfect equality) and 1 (or 100) indicates total concentration (perfect inequality). The Gini coefficient is defined graphically as the area of concentration between the Lorenz curve and the line of perfect equality. The Lorenz curve is a graphical representation of the proportionality of a distribution (the cumulative percentage of the values) (Lorenz, 1905). See Sen (1997) for an overview of inequality measures.
 9. The one exception I am aware of is Auvinen and Nafziger (1999). However, see Humphreys (2002: 3).
 10. Deiniger and Squire (1996) use an objective and valid operationalization of income inequality, but, as noted by Székely and Hilgert (1999), the observations are not always comparable because they do not necessarily refer to the same notion of income.
 11. See Scientific Study of International Processes homepage: <http://www.isanet.org/ssip/>
 12. Another conceptual problem with the previous literature on vertical inequalities and conflict, and which complicates the comparison between the studies, is the heterogeneity in terms of the dependent variable. For example, whereas Collier and Hoeffler (2004) focus on civil war, Auvinen and Nafziger (1999) look at how inequality impacts the much broader category of “complex humanitarian emergencies”. Despite using the same inequality data (Deininger and Squire, 1996) then, they reach different conclusions as to whether inequality breeds conflict.
 13. One study of the relationship between vertical inequality and conflict, however, stands out in this regard: de Soysa and Wagner (2003) test the effect of vertical schooling inequality rather than income inequality on conflict, using data from Castelló and Doménech (2002) on differences in educational attainment. The authors even argue that this vertical measure captures some of the logic of horizontal inequalities, owing to the assumption that dominant ethnic groups control state resources and often use education policies to discriminate against minorities.
 14. The labels “diachronic” and “synchronic” RD stem from Boswell and Dixon (1990: 542).
 15. In fact, Gurr’s (2000) minorities at risk also include advantaged minorities like the Sunni Arabs of Iraq and the overseas Chinese of Southeast Asia, but his focus is on relative deprivation, that is, that these groups are vulnerable to challenges from disadvantaged groups.

16. In a later work, Gurr (2000) uses a different terminology, distinguishing between capacities (similar to internal opportunities) and opportunities (similar to external opportunities) for group action.
17. According to Gurr (2000: 65–95) the salience of group identity is partly attributed to previous collective disadvantages. Collective grievances and identities in their turn form the basis of a group's capacity, or internal opportunities for mobilization. Gurr (2000: 74) admits that there is no single answer to this "chicken-and-egg" problem concerning the root causes of ethnic conflict.
18. For the sake of simplicity, I have kept the boxes in Figure 1 to a minimum, although one could have envisioned additional boxes and arrows. For example the figure only includes socio-economic HIs, despite the fact that I have stressed the multidimensionality of HIs. However, the two types of HI are often likely to be interrelated and the main focus in the current review is on economic HIs. See also Langer (2005), who argues that political HIs are of great importance to leaders, whereas socio-economic HIs matter more for the masses.
19. The one exception I am aware of is Barron et al.'s (2009) study of local conflict in Indonesia, which found that educational HIs were associated with *lower* levels of conflict in rural areas.
20. Despite its wide use and great potential, however, MAR suffers from some fundamental flaws (see e.g. Østby, 2011).
21. These surveys are available from www.measure.dhs.com.
22. However, Condra's (2009) results suggest that it is not the poorest groups in the country that are at highest risk of rebellion, but rather those that are relatively better off.
23. The issue of ethnicity is a fundamental regarding the formation of identity groups and the salience of horizontal inequalities (see Østby, 2011: 26–31).
24. See G-Econ webpage: <http://gecon.yale.edu/data-and-documentation-g-econ-project>.
25. The corresponding correlation coefficients when replacing the DHS asset index with education years are even lower: 0.429 at the national level and 0.250 at the subnational, regional level.
26. As for data on political horizontal inequalities, see Cederman et al. (2011).
27. Furthermore, Baten and Fraunholz (2004), and Moradi and Baten (2005) have used anthropometric data from the DHS surveys to construct measures of inequality for pre-survey decades.
28. This has gained some recent empirical support by Gubler and Selway (2012), who based on data for 100 countries found that civil war onset is an average of nearly 12 times less probable in societies where ethnicity is crosscut by socio-economic class, geographic region and religion.
29. Some researchers have already started to investigate this issue quantitatively for selected countries (see e.g. Langer and Mikami, 2012; Rustad, 2012).

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