GROUP INEQUALITY AND CIVIL CONFLICT

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ABSTRACT

This paper explores empirically the role of economic inequality at the ethnic group level in both the onset and intensity of civil conflict. Drawing on a new data set using surveys to measure inequality, we provide evidence of a strong and robust relationship between within-group inequality and the intensity as well as incidence of civil war. There is no relationship, however, between within-group inequality and the onset of conflict. These results are consistent with recent theoretical research arguing that groups that are unequal internally are most likely to sustain civil conflict because they have both the labor and capital necessary to keep fighting. Our analysis also casts doubt on existing research suggesting that inequality between ethnic groups causes civil wars to begin. There are good reasons to doubt such theoretical arguments, and we find no evidence of an empirical relationship between inequality across groups and conflict onset, even using the same data that has been previously used to establish this relationship. Our findings therefore support theories of civil conflict emphasizing the capacity to fight rather than those emphasizing a causal role of economic grievances between groups.

Keywords: Ethnicity, within-group inequality, horizontal inequality, civil conflict.

JEL: D63, D74, J15, O15

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1. INTRODUCTION

The tragic civil war in Syria, which broke out in 2011 and which pits a range of ethnic and sectarian groups against the government, has cost hundreds of thousands of lives and displaced millions. While it is a particularly depressing case, Syria joins a growing list of countries that have suffered from serious intrastate conflicts, which have replaced inter-state wars as the nexus for large scale violence in the world.[2] Understanding why groups start civil wars is therefore a crucial step towards limiting the devastation of such conflicts. However, as the violent events following the Arab Spring illustrate, conflicts vary a great deal in their intensity and duration, with some conflicts leading to large numbers of fatalities and others to a small number, and some lasting many years, and others ending quickly. It is thus equally important to understand how groups sustain such conflicts.

This paper examines empirically the role of economic inequality in both the onset and sustainment of civil war. The analysis has two features worth noting. First, it is at the ethnic group level. Much work on economic inequality and civil conflict has been influenced by long-standing arguments by Karl Marx, Dahrendorf (1959) and Gurr (1970, 1980), who argue that inequality breeds grievances which in turn lead to civil violence. However, national level studies have not found empirical support for this idea, leading some to conclude that economic grievances do not cause conflict (e.g., Lichbach 1989, Fearon and Laitin 2003 and Collier and Hoeffler 2004). But most civil conflict is rooted in ethnic, sectarian or other identity groups (Doyle and Sambanis 2006, Fearon and Laitin 2003), making it important to consider arguments about how inequality measured with respect to groups affects the propensity of groups to participate in civil wars (e.g., Cederman, Weidmann and Gleditsch 2011, Cederman, Gleditsch and Buhaug 2013).[3]

Second, the analysis distinguishes between arguments about group-level inequality and the *onset* of civil conflict, on one hand, and group-level inequality and the *intensity* or *incidence* of civil

[2]Gleditsch et al. (2002), for example, find that since WWII, there were 46 interstate conflicts with more than 25 battle-related deaths per year, 22 of which have killed at least 1,000 over the entire history of the conflict. Over the same period, there were 181 civil conflicts with more than 25 battle-related deaths per year, and almost half of them have killed more than 1,000 people.

[3]See Esteban, Mayoral and Ray (2012) and Arbatli, Ashraf and Galor (2015) for recent evidence on the connection between ethnic structure and conflict.

conflict, on the other. With respect to conflict onset, a prominent line of research argues that horizontal inequality – economic differences *across* groups – leads groups to start civil wars (Stewart 2002, Cederman et al., 2011). Such inequality exists when a group is particularly poor or particularly rich, so relatively poor and rich groups should be the most likely to initiate conflicts. These arguments about horizontal inequality and civil war onset, however, have little to say about the incidence of civil wars – that is, about the ability of groups to sustain the fight. Small and powerless groups might spark a conflict, but then be quickly crushed by the government. Arguments about inequality and the incidence of civil wars have therefore focused on a different dimension of inequality. In particular, recent theoretical research has argued that groups that are unequal internally are most likely to sustain civil conflict because such groups have both the labor (poor people) and the capital (rich people) necessary to keep fighting (Esteban and Ray, 2011).

By distinguishing between conflict incidence and conflict onset, this paper makes two contributions to studies of inequality and civil war. First, drawing on a new data set using surveys to measure inequality, we provide evidence of a strong and robust relationship between within-group inequality and the *incidence* as well as the *intensity* of civil war. Consistent with theoretical arguments emphasizing the role played by income heterogeneity in sustaining conflict, our results also show that the connection between within group-inequality and conflict onset is much weaker. Second, at both the theoretical and empirical level, we contend that arguments about horizontal inequality and conflict onset are flawed. Theoretically, it is not at all clear that the relative wealth of a group should be systematically related to its propensity to spark civil conflict. Empirically, using the same data employed in previous studies, we find no evidence of a robust empirical relationship between horizontal inequality and conflict onset.

The picture our paper paints of the role of inequality in civil conflict is thus quite different than previous research. In contrast to group level research on conflict onset but consistent with national-level research on overall inequality, we find no support for the argument that economic grievances lead to civil war. The reality is probably that grievances can take many forms, not only economic but also political and cultural, and that whether they trigger violent conflict is due to idiosyncratic events – like regional contagion in the Middle East – for which it is difficult to provide systematic evidence. But in contrast to the national level research dismissing the role of inequality, our group level analysis finds that inequality can play an important part in understanding civil conflict, albeit one unrelated to grievances. Instead, unequal groups are associated with a higher incidence and intensity of civil conflict, not with a higher probability of conflict onset, as claimed in previous research (Kuhn and Weidmann, 2015). Group-level inequality therefore seems related to a group's capacity to sustain conflict, opening a link to the level of inequality in a group and the group's on-going participation in civil wars. Our findings therefore support theories claiming that the critical factors that foster civil unrest are those that increase the capacity to fight rather than those claiming that economic grievances are a direct cause of conflict.

The paper is organized as follows. Section 2 reviews the existing arguments and empirical evidence that represent the point of departure for our analysis, discussing first the relationship between horizontal inequality and conflict onset and then discussing the relationship between intra-group inequality and conflict intensity. Section 3 discusses data and measures, and including a new data set that uses individual-level surveys to measure within-group inequality. The section also compares the survey-based measures to a measure that integrates information about groups' spatial locations and economic activities in these locations. The empirical analysis is found in the two sections that follow. Section 4 focuses on conflict incidence or intensity as the dependent variables, and section 5 focuses on the onset of civil war. Section 6 concludes. Additional results are included in the Appendices.

2. Ethnic inequality and conflict

This section discusses the theoretical arguments as well as the existing empirical evidence linking civil conflict and two dimensions of ethnic inequality, *within*- and *between*-group. In so doing it is important to distinguish two different aspects of conflict, its *outbreak* and its *intensity* or *incidence*. This distinction is frequently overlooked in the empirical literature, but as emphasized recently by Bazzi and Blatmann (2014), the *onset* and the *intensity* (or *incidence*) of conflict should be expected to follow different underlying logics, and thus to rest on different empirical foundations. This is clearly the case for different aspects of group-based inequality.

2.1. **Horizontal inequality and conflict outbreak.** Research on inequality between groups – usually referred to as horizontal inequality – is rooted in the concept of grievance, and it connects grievances with the *outbreak* of civil conflict. The basic argument is that when a group has large economic differences with other groups, the group is more likely to *initiate* conflict. Central to this notion is the idea of "relative deprivation" (e.g., Cederman, Gleditsch and Buhaug 2013, Stewart 2000 and Stewart 2002).[4] Poorer groups have incentives to initiate conflict in order to expropriate the rival's resources: the larger the income gap between the groups, the greater the potential prize, and hence the greater the incentive for conflict by the poorer group (e.g., Acemoglu and Robinson 2005, Cramer 2003, Stewart 2002 and Wintrobe 1995). The fact that inequalities are linked to ethnic identities is important for two reasons. First, identity-based inequalities are easily politicized by elites, and such politicization typically involves a framing strategy where an afflicted group blames one or more other groups for injustice. Second, a central problem in initiating conflict is mobilization, and ethnic identities facilitate solutions to the collective action problem associated with waging civil war. Thus, horizontal inequalities create feelings of grievance among members of poor groups, the link between grievance and ethnic identity aids in the type of mobilization necessary for conflict, and relatively poor ethnic groups are therefore more likely to initiate civil strife.

This is only one-half of the story about horizontal inequality and ethnic civil conflict: if only relatively poor groups had incentives to initiate conflict, it would be quite difficult to distinguish the role of ethnic inequality in civil war onset from the role of poverty. But arguments about horizontal inequality also emphasize the incentives of rich groups, who have more to lose if conflict breaks out (e.g., Cederman, Gleditsch and Buhaug 2013, pp. 97-98, and Cederman, Weidmann and Gleditsch 2011, p. 478). Rich groups should be concerned that the government will expropriate their wealth, giving such groups incentives to engage in pre-emptive attacks and/or secession wars to diminish threats against them. Relatively rich and poor groups should therefore be most likely to initiate conflict, creating a link between horizontal inequality and civil war onset. Using

[4]For a detailed theoretical account of the link between horizontal inequality and civil conflict, as well as a review of the relevant literature, see Cederman, Gleditsch and Buhaug (2013, chapter 3).

a large cross-national dataset, Cederman, Weidmann and Gleditsch (2011), Cederman, Gleditsch and Buhaug (2013), Cederman, Weidmann and Bormann (2015) and Kuhn and Weidmann (2015) provide group-level empirical support for the link between horizontal inequality and conflict onset. These empirical results are described and re-examined in Section 5 below.

Several observations about the horizontal inequality argument deserve highlighting. First, if one considers the strategic incentives of groups, it is not clear that a group should be most likely to initiate conflict when it is relatively rich or poor. A relatively poor group is less likely to succeed in conflict because it lacks the resources necessary for success. If a very poor group is going to be crushed when it initiates conflict, it is reasonable to ask why it will attack in the first place. A rise in the income of a group might even enhance its capacity to fund militants and thus its probability of success in conflict. As a result, the closing of the income gap between two groups – rather than its widening – might ignite conflict.[5] Similar strategic considerations should apply to rich groups: if rich groups understand that they will be difficult to defeat, it is not clear why relatively rich groups will not need to undertake preemptive attacks. Indeed, if we introduce uncertainty about the relative military strength of rich and poor groups, we might expect that conflict will be highest when economic differences are most modest, as these will be the cases where the relatively poor group has the strongest belief it can win and the relatively rich group has the most to gain from pre-emption. While it is not our purpose to develop a theory about how the potential gains and loses from the outcomes of civil conflict interact with strategic considerations regarding expectations of winning, we would underline that these strategic considerations work against the arguments about horizontal inequality and are notably absent from them.

Finally, the *logic* linking *poor groups* to the outbreak of civil conflict could be the opposite of that posited in horizontal inequality arguments about grievance. As described above, such arguments emphasize what poor groups have to *gain* from conflict – the poorer they are, the more they have to gain. But it might be that poorer groups initiate conflict because they have *little to lose*. This is the type of argument made in research on civil war at the national level, where

[5]This point is compatible with the abundant evidence showing that economic modernization might instigate rather than moderate ethnic conflict (Tellis, Szayna, and Winnefeld 1998, Chua 2003). See Esteban and Ray (2011) for a discussion of this issue.

for example Coellier and Hoeffler (2004) argue that poorer countries are more likely to have civil conflict because the opportunity cost of fighting for poor people in such countries is relatively low, and thus their labor can be bought relatively cheaply.[6] If we applied this logic at the group level, we might expect relatively poor groups to be involved in initiating conflict because their members have the lowest opportunity cost of doing so.

Although both the inequality and opportunity cost perspectives suggest poorer groups should be more likely to initiate conflict, these two perspectives yield different expectations for rich groups. From the "poverty" perspective, relatively rich groups will not engage in conflict; in fact, they would have the highest opportunity cost of doing so. From the "horizontal inequality perspective," both relatively rich and relatively poor groups have incentives to initiate conflict. Thus, one way to sort out empirically whether inequality or poverty is related to a group's incentive to start a civil war is to consider the role of rich groups. If only poor groups are more likely to initiate civil wars, it is difficult to attribute this to inequality: it could be because of grievance, lower opportunity cost, or some combination of the two. But if both relatively rich and relatively poor groups are likely to initiate conflict, it is difficult to attribute this to opportunity cost, and the role of inequality becomes central.

2.2. **Within-group inequality and conflict intensity.** The arguments about horizontal inequality and conflict have focused on why civil wars begin. Conflicts, however, vary a great deal in their intensity and duration, with some civil wars leading to large numbers of fatalities and others a small number, and some lasting many years, and others ending quickly. It is therefore important to understand not only the relationship between inequality and the outbreak of war, but also the link between inequality in the intensity or incidence of civil war. To this end, theoretical research has argued that groups that are unequal internally are most able to sustain civil conflict.

Esteban and Ray (2008, 2011) (henceforth "ER") provide the most precise theory about how group-based attributes are related to the incidence of civil conflict. Their argument focuses on the role of rich and poor within a group. Effectiveness in conflict requires various inputs, most notably financial support and labor (i.e, fighters). Maintaining conflict therefore has at least two

[6]See also Dal Bó and Dal Bó (2011), Dube and Vargas (2014) and Bazzi and Blattman (2015).

opportunity costs: the cost of contributing resources and the cost of contributing one's labor to fight. Economic inequality within a group simultaneously decreases both opportunity costs. When the poor within a group are particularly poor, they will require a relatively small compensation for fighting, and when the rich within a group are particularly rich, the opportunity cost of resources to fund fighters will be relatively low. Thus, groups with high income inequality should have the greatest capacity to sustain civil conflict.

There is ample anecdotal evidence supporting the link between within-group inequality and conflict but to the best of our knowledge, this is the first paper that analyzes empirically the link between within-group inequality and conflict *incidence* and *intensity* at the group level.[7] ER (2011) provide examples from Africa, Asia and Europe to illustrate the causal mechanisms in their theory. In their survey of the literature on ethnic conflict, Fearon and Laitin (2000) also describe examples where the elites promote ethnic conflict and combatants are recruited from the lower class to carry out the killings, including Bosnia (the "weekend warriors," a lost generation who sustained the violence by fighting during the weekends and going back to their poorly-paid jobs in Serbia during the week), Sri Lanka (where the ethnic war on the ground was fought on the Sinhalese side by gang members), and Burundi. They conclude,

> [O]ne might conjecture that a necessary condition for sustained *ethnic violence* is the availability of thugs (in most cases young men who are ill-educated, unemployed or underemployed, and from small towns) who can be mobilized by nationalist ideologues, who themselves, university educated, would shy away from killing their neighbors with machetes. (p. 869)

A more recent example can be found in Ukraine, where Rinat Akhmetov, its richest man, sent thousands of his own steelworkers to establish control of the streets in Eastern Ukraine in opposition to pro-Kremlin militants. The case of the Rwandan genocide is also suggestive. In the spring

[7] Previous studies have focused instead on conflict onset, which is not the ideal outcome variable to test this theory. Østby, Nordås and Rød (2009) find a positive association between within-region inequality and conflict onset in 22 countries in Sub-Saharan Africa and Kuhn and Weidmann (KW, 2015) obtain similar results using a global dataset that has the ethnic group – rather than the region– as unit of analysis.

of 1994, the Hutu majority carried out a massacre against the Tutsi minority where 500,000 to 800,000 Tutsi and moderate Hutus that opposed the killing campaign were assassinated. In the years immediately prior to the genocide, Rwanda suffered a severe economic crisis motivated by draughts, the collapse of coffee prices, and a civil war. Verwimp (2005) documents an increase in within-group inequality among the Hutu population prior to the genocide: on the one hand, a sizable number of households that used to be middle-sized farmers lost their land and became wage workers in agriculture or low skilled jobs. On the other, rich farmers with access to off-farm labor were able to keep and expand their land. This new configuration encouraged the Northern Hutu elites to use their power to instigate violence. Backed by the Hutu government, these elites used the radio (particularly RTLM) and other media to begin a propaganda campaign aimed at fomenting hatred of the Tutsis by Hutus (Yanagizawa-Drott, 2012). The campaign had a disproportionate effect on the behavior of the unemployed and on delinquent gang thugs in the militia throughout the country (Melvern 2000), individuals who had the most to gain from engaging in conflict (and the least to lose from not doing so). Importantly, the campaign made it clear that individuals who engaged in the ethnic-cleansing campaign would have access to the property of the murdered Tutsi (Verwimp, 2005). Thus, the rich elites "bought" the services of the recently impoverished population by paying them with the spoils of victory, something that was more difficult to undertake prior to the economic crisis.[8]

Although the ER theory provides a clear rationale for why intra-group inequality should be positively related to the incidence of conflict, the opposite argument has also been made. Such intra-group inequality could create resentment among the poor and reduce group cohesiveness (Sambanis and Milanovic, 2011) which, in turn, could have a negative impact on conflict.[9] Thus,

[8] It is also worth noting that micro-level studies of participation emphasize that richer elites recruit the poor to fight. Brubaker and Laitin (1998), for example, argue that most ethnic leaders are well-educated and from middle-class backgrounds while the lower-ranking troops are more often poorly educated and from working-class backgrounds. In their study of Sierra Leone's civil war, Humphreys and Weinstein (2008) find that factors such as poverty, a lack of access to education, and political alienation are good predictors of conflict participation and that they may proxy, among other factors, for a greater vulnerability to political manipulation by elites. Justino (2009) also emphasizes that poverty is a leading factor in explaining participation in ethnic conflict.

[9] ER also have an argument about why this argument about a negative relationship might be unlikely to hold. In their model of how social coalitions form for civil conflict, ER (2008) show that in the absence of bias favoring either type of conflict, ethnicity will be more salient than class. This is because a class division creates groups with strong economic homogeneity. Thus, while the poor may have the incentives to start a revolution, conflict might be extremely difficult

it is important to understand empirically the direction of the relationship between within-group inequality and the incidence of conflict.

3. Measuring ethnic inequality at the group-level

There are two dimensions related to measuring various forms of ethnic inequality at the group level. The first is the nature of the data on economic well-being: some studies rely on surveys and others on geo-coded data of economic activity. The second is the nature of the measures themselves – the way that the data is used to create a variable measuring some form of inequality. This section reviews the central approaches in the existing literature and describes how we use surveys to measure within-group inequality. In so doing, we would emphasize that our focus on *group-level* measures means that measures that aggregate information across groups into a single variable are not relevant here.[10]

3.1. **Horizontal Inequality: Data and measures.** Testing group-level arguments about horizontal inequality and civil conflict requires data on the economic well-being of groups. To this end, the dominant approach in existing group-level studies relies not on survey data, but rather on geo-referenced data on the geographic location of ethnic groups along with geo-referenced estimates of economic development. There are a number of data sets on the geographic location of groups, including the GREG, the GeoEPR and the Ethnologue.[11] The group-level studies of horizontal inequality and conflict have relied on the GeoEPR dataset, described in Wucherpfennig et al. (2011), which utilizes an expert survey to determine the identity and location of politically relevant ethnic groups. The spatial data on group locations is linked to spatial data on economic output, for example using Nordhauss (2006) G-Econ data set (the approach taken by Cederman, Weidmann and Gledistch 2011, CWG henceforth), or using satellite images of light density at night. Research

for the poor to sustain because of the high cost of resources. But even if the poor are able to overcome these constraints, class conflict may be difficult to start or sustain because when the rich foresee a class alliance that can threaten their status, they can propose an ethnic alliance (to avoid the class one) that will be accepted by the poor ethnic majority, planting the seeds of ethnic conflict.

[10]Østby (2008), for example, develops various country-level measures of horizontal inequality and polarization that are useful in studies where a geographic space like a country or region (rather than the group) is the unit of anlaysis.

[11]The GREG dataset (Weidmann, Rob and Cedarman 2010) is based on the Soviet Atlas Narodov Mira. The *Ethnologue* provides information on the spatial location of linguistic groups in much of the world.

using these data to study the connection between group inequality and civil conflict include CWG, Cederman, Gleditsch and Buhaug (2013) and Cederman, Weidmann and Bormann (2015).

One variable that has been used in group-level studies of horizontal equality and conflict is

$$\text{LINEQ2} = (log\frac{g}{G})^2,$$

where g is the GDP per capita of the group and G is the *unweighted* average GDP per capita of all groups in society.[12] The variable has the important feature that it takes a larger value as a group becomes either relatively rich or relatively poor, Thus, if the variable has a robust relationship with civil conflict the relationship cannot be easily attributed to group poverty rather than horizontal inequality.

This measure, however, has important flaws. Most importantly is the specification of G as the unweighted average of all group GDP's per capita. This definition implies that when calculating LINEQ2 for a group j, one is partially comparing that group's wealth to itself (because group j's GDP is included in G). A much more direct measure of grievance would be to compare a group's GDP to that of *other* groups. Another problem associated to this measure is that the definition of G in LINEQ2 ignores group size.[13] Finally, it is worth noting that LINEQ2 has a peculiar functional form: the log of a ratio, squared. This highly non-linear function is one way to ensure that the measure grows larger as a group becomes either richer or poorer than average, but it is not a particularly intuitive way to do so: the functional form is extremely sensitive to small changes in the data, and it imposes asymmetries in the treatment of rich and poor groups.[14] And the highly non-linear functional form also means that variable will be highly skewed, inviting the possibility of high leverage by outlying observations (see Section 5 below).

[12] See Cederman et al. (2011) and Kuhn and Weidmann (2015).

[13] Consider an example with three groups, group 1 is a poor group with an income of 1, group 2 is a somewhat less poor group with an income of 1.5, and group 3 is a rich group with an income of 5. The measure will be the same for group 1 regardless of whether group 2 is extremely large (in which case the rest of the population group 1 faces is relatively poor, like group 1) or extremely small (in which case the rest of the population that group 1 faces is relatively rich, unlike group 1).

[14] Suppose, for example, there are only two groups in society: a poor group with an income of 1 and a rich group with an income of 5. Then (no matter the size of the two groups) LINEQ2 is .26 for the rich group, and is 1.21 for the poor group.

It is therefore useful to consider whether empirical results regarding horizontal inequality are robust to alternative measures of the concept. We consider two simple possibilities. The first is the absolute difference between the GDP per capita of group g and the GDP per capita of those in other groups:

$$\text{HI(ABS)} = |g - \bar{G}|,$$

where $\bar{G}$ is the weighted average (by group size) of the GDP per capita for all groups *other than* the group in the numerator. This variable provides a simple comparison of a group's economic well-being with the economic well-being of other groups. Second, to attenuate the skewness of this variable and the influence of outliers, HI(LN) is the natural log of HI(ABS). It turns out that the correlation between LINEQ2 and the HI variables is very small (see Table B.2 in Appendix B). Given the straightforward interpretation of the HI variables, these low correlations cast doubt on the adequacy of LINEQ2 as a measure of horizontal inequality.

In addition to LINEQ2, previous research has considered a second approach to measuring horizontal inequality, one that creates separate variables for rich and poor groups. For example, Cederman, Weidmann and Bormann (2015) define LOW $= \frac{g}{G}$ if $G > g$ and 1 otherwise, and HIGH $= \frac{G}{g}$ if $G < g$ and 1 otherwise. The goal of developing this measure is to distinguish whether the onset of civil wars is associated more strongly with a group being relatively poor or relatively rich.

There are two issues to bear in mind when using HIGH and LOW to test arguments about horizontal inequality. First, like LINEQ2, LOW and HIGH define G to include all groups and it weights these groups equally, independently of their size. In this regard, these two variables have some of the same shortcomings discussed above with respect to LINEQ2.[15] Second, when HIGH and LOW are used instead of LINEQ2, arguments about horizontal inequality are supported only if *both variables* are associated with civil war onset. If, for example, only LOW has a significant association, we cannot disentangle the role of poverty from the role of inequality.

[15]Fjelde and Østby (2014) invoke a variant of LOW and HIGH that does not suffer this limitation.

3.2. **Within-group inequality: data and measures.** As opposed to horizontal inequality, measuring intra-group inequality is not a conceptually difficult task, as standard inequality measures like the well-known Gini coefficient can be calculated at the group level. What it is difficult in this case is to obtain the right data to compute such a measure. As a result, there is little previous research that measures inequality within groups for a large number of countries. Østby, Nordås and Rød (2009), for example, use survey data from the Demographic Health Surveys in 22 countries in Sub-Saharan Africa. Their study calculates the Gini coefficient for each region and their analysis finds that regions with higher levels of inequality are most likely to experience the onset of conflict. Kuhn and Weidmann (KW, 2015) use geo-coded rather than survey data. KW calculate a Gini coefficient for each group by first dividing ethnic homelands into cells of equal size (about 10 km), focusing on non-urban areas.[16] They then compute nightlight emissions per capita for each cell and all cells occupied by a group are used as inputs (i.e., as if they were data on individuals) to calculate the group's Gini coefficient.

This approach has the merit that it applies a consistent criterion for measuring economic output across space, potentially reducing problems with cross-national comparisons of economic measures. However, it also has clear limitations for measuring within-group inequality. First, to calculate a group's gini, the approach must assume either that particular geo-coded areas are occupied by only one group, or that individuals from different groups in the same geo-coded area have the same income. Neither assumption is attractive. On one hand, there is substantial variation in the regional segregation of groups, and Morelli and Rohner (2015) link this segregation itself to civil conflict. On the other hand, if one assumes that individuals from different groups occupy the same geo-coded area, one also has to assume that individuals from these different groups all have the same income, even though there is ample reason to expect that incomes might vary across groups. Second, over half the world's population live in urban areas (Angel 2012), the fact that urban cells are discarded is likely to influence the estimates since a huge source of within-group inequality is dismissed. Additionally, the urbanization of a country maybe correlated with other

[16] KW point out that groups might be relatively geographically segregated in the countryside, this is unlikely to be the case in urban areas. Thus including urban cells introduces measurement error.

factors that are related to civil war, raising concerns that the biases may be correlated with conflict. Finally, measuring WGI using the nightlight in geographic cells will yield results that are sensitive to cell size: the larger the size of the cell, the smaller the resulting within-group measure, and in the limit, if the whole territory is assigned to one cell, within-group inequality must be zero. But the choice of cell size is arbitrary. It is not surprising, then, that KW's own analysis shows a weak relationship between group Gini's based on nightlights and group Ginis based on (DHS) surveys, and that the relationship between these variables further weakens when urban areas are included.[17] To avoid these drawbacks of using geo-coded data, we have created a new group-level dataset that uses individual-level surveys.

3.3. **Measuring WGI with surveys.** Our measure of WGI uses individual level surveys containing data on both relevant ethnic identity and economic well-being. Since the surveys vary in their measures of economic well-being, we address the issue of their comparability by applying two existing methodologies. This section sketches the general approach and further details are in Appendix A.

The surveys. In order to have a broad cross-national sample of countries, we include a variety of surveys that fall into one of three categories. The first category, "HES" (for "Household Expenditure Survey"), includes the best surveys available in the world for calculating inequality: the Luxembourg Income Study, the Living Standards Monitoring Surveys, other similar household expenditure surveys, and a handful of national censuses. The second category uses household income data in a form that is less precise than HES surveys. These include the World Values Surveys (WVS), which typically has about 10 household income categories per country, and the Comparative Study of Elections Surveys (CSES), which reports income in quintiles. The third type of survey, typically conducted in relatively poor countries, does not have household income data, but rather has information on various assets that households possess. Such measures of assets are used in countries where there are many poor individuals who do not make substantial cash transactions, and thus for whom individual income cannot be used to meaningfully measure

[17]Their analysis also shows that when we compare a country-level Gini based on nightlights with a standard measure of the Gini, there is tremendous variation in the nightlight Gini at every level of the standard Gini; see KW Figure 1.

economic well-being. In such cases, social scientists often use an array of asset indicators (such as the type of housing, flooring, water, toilet facilities, transportation, or electronic equipment the household possesses) to determine the relative economic well-being of households. For these surveys, which include the Demographic Health Surveys (DHS) and the Afrobarometer Surveys, we use the household assets to measure economic well-being.[18]

To identify the relevant groups in a country and their size, we rely on Fearon (2003), who provides a set of clear and reasonable criteria for identifying the socially relevant ethnic, religious, racial and/or linguistic groups across a wide range of countries that is widely used in the literature. We discard surveys that do not include survey questions that allow us to adequately identify the Fearon groups. Specifically, if there exist one or more groups on Fearon's list that we cannot identify in the survey, we sum the proportion of the population that these groups represent per Fearon's data. If this sum is greater than .10, we do not utilize the survey.[19] In the end, we have data from 232 surveys in 89 countries from 1992-2008. Appendix A.2 provides further details about data coverage and includes a map of the countries with useful surveys, while Appendix A.3 discusses whether the resulting sample of countries is representative. A list of all the surveys employed is provided in Appendix A.5.

Adjusting the group-level Gini coefficients. The survey information on ethnicity and "income" in the surveys to compute group-level Gini coefficients. But since the surveys vary in their measures of economic well-being, we face the problem of comparability in inequality measures across surveys. This is a standard challenge: the observations in Deininger and Squire's classic (1996) data set, for instance, differ in many respects (most significantly, in their income definitions and their reference units) and are rarely comparable across countries or even over time within a single

[18]For the DHS surveys, which contain a large number of asset indicators (typically around 13), we follow Filmer and Pritchett (2001) and McKenzie (2005) and run a factor analysis on the asset variables to determine the weights of the various assets in distinguishing household well-being. We then use the factor scores, and the responses to the asset questions, to measure the household "wealth" of the respondent. The Afrobarometer surveys have a much smaller number of asset questions, typically 5 or less, and so we simply sum the assets.

[19]As an example, consider the Afrobarometer survey for Nigeria in 2003, for which it is possible to use a language variable to map to many of Fearon's groups. But one of his groups is "Middle Belt," and it is not possible to identify these individuals in the Afrobarometer survey. Since Fearon's data suggest they represent 18 percent of the population (which exceeds our threshold), we exclude this survey.

country. Its successor, the World Income Inequality Database (WIID), faces identical challenges. Thus, if scholars wish to conduct cross-national research on inequality using such measures, they must adopt methodologies to adjust the measures from different surveys to make them comparable.

We consider two approaches. The first is the *Intercept approach*, which shares the same spirit as the original Deininger and Squire (1996) exercise. To remove average differences due to different survey methodologies, this approach involves first calculating the Gini coefficient for each group using the surveys and then regressing these Ginis on survey, time and country dummies, with HES as the omitted category (because the HES surveys are the best-available estimates of income distributions in the world). The coefficients on the survey dummies are then used to adjust the group inequality measures to remove average differences that could be traced to different survey types.

The second procedure, the *Ratio approach*, is similar to the methodology employed by Solt (2009) to construct the Standardized World Income Inequality Dataset (SWIID).[20] This approach uses external data on the Gini – the SWIID – to construct country-level adjustment factors based on the ratio of country-level Ginis based on surveys to SWIID Ginis. These adjustment factors based on the ratios are then applied to the group Ginis to obtain the adjusted group Ginis. Appendix A.1 has further details of this procedure.

Since our time period is relatively short, we define our measures of group Gini as the average of the adjusted group Ginis from all the available surveys, adjusted with the Intercept approach (G^I) or the Ratio approach (G^R). The approach yields data on within group inequality for 446 groups in 89 countries. As a practical matter, the two approaches of adjusting the data yield similar results. The correlation of G^I and G^R is .73, although G^I has a somewhat higher mean (.45) than that of G^R (.38). Figure A.1 in Appendix A.2 depicts this relationship.

3.4. **Surveys vs. nighlights in measuring WGI.** The survey-based measure of WGI avoids some of the shortcomings we have described with respect to using geo-referenced data to compute this variable. For instance, there is no need to discard information from urban areas, or to assume an

[20]The SWIID provides comparable (country-level) Gini indices of gross and net income inequality for 173 countries from 1960 to the present and is one of the most thorough attempts to tackle the comparability challenge.

individual's ethnic identify based on a geographic location. We also do not have to make arbitrary assumptions about "cell" size. The survey-based data, however, also presents problems vis-à-vis the geo-referenced approach, such as the fact that it covers less countries (and, therefore, the sample could be biased), or the fact that there is heterogeneity in the sources of income.

Appendix A.4 provides a discussion of the strengths and weaknesses of the survey and geo-coded WGI measures. The main conclusions are as follows. First, the sample of countries for which useful surveys exist is very similar to a global sample of countries with respect to the variables of central interest in the analysis, such as income, conflict level, ethnic diversity, level of democracy, etc. (see Table A.1), diminishing concerns that results are biased by a lack of sample representativeness. Second, although the sample of countries for which useful surveys exist is smaller (89 versus 128 countries in the geo-coded approach), group coverage by country is considerably better in the survey than the geo-coded sample, as the survey data contains information for 90% of the groups in the countries studied, versus 70% for the geo-coded approach (446 and 558 groups, respectively). Third, although there exists a positive association between both sets of measures, the divergences are substantial. The correlation between IGI and the survey-based Gini coefficients is .41 for G^I and .34 for G^R (see Figure A.3 in Appendix A.4 for a graphical comparison). Fourth, the divergence between two two sets of measures seems due in part to systematic biases in the geo-coded data. In particular, the geographic characteristics of the ethnic homeland are correlated with the nightlight measures, which are systematically higher when ethnic homelands are heterogeneous in elevation. The same is not true when country-level Gini coefficients or survey-based group-level Ginis are considered. As a result, the correlation between the geo-coded and the survey-based measures is around 45% to 60% larger when the ethnic homeland has a low or moderate variability in elevation than when variability is high. (See Appendix A.4 for more details.) We therefore feel that considerable caution must be used when interpreting any results from analyses that use nightlights to measure WGI.

4. Empirical analysis of group inequality and the intensity of conflict

We now turn to the substantive focus of our paper, which is to analyze empirically the relationship between the group-level inequality measures and the measures of group involvement in civil conflict, beginning in this section with a focus on conflict incidence and intensity and then moving on to the outbreak of civil conflict. To measure group involvement in conflict in a given year, we use the Ethnic Power Relations data set (EPR, Cederman et al. 2009).[21] Ethnic groups are coded as engaged in conflict if a rebel organization involved in the conflict expresses its political aims in the name of the group, and a significant number of members of the group participate in the conflict (see Wucherpfennig et al. 2012 for details).

The theory in Esteban and Ray (2011) argues that within-group inequality affects a group's capacity to wage and sustain conflict. Our primary dependent variable for testing this argument is INTENSITY, which captures conflict incidence and severity. The variable takes the value 0 if a group is at peace in a given year, the value 1 for each year in which an ethnic group is involved in armed conflict against the state resulting in more than 25 battle-related deaths, and the value 2 if the resulting number of battle deaths that year is larger than 1000. We also test the argument using INCIDENCE, a binary measure that takes the value 1 for each year in which an ethnic group is involved in armed conflict against the state resulting in more than 25 battle-related deaths.

All empirical models include (unreported) country and year indicator variables as well as a lagged dependent variable (to capture conflict persistence). Control variables are described as they appear in the text. Detailed definitions of all variables as well as a table of summary statistics are provided in Appendix D.

Table 1 presents models where we regress INTENSITY on G^R using ordered logit models with standard errors clustered at the country level. Model 1 includes only G^R, with no other controls (except the country and year fixed effects, and the lagged dependent variable). The coefficient for G^R is positive and very precisely estimated.[22] Model 2 adds several controls. At the group

[21] The data are accessed through the ETH Zurich's GROWup data portal (`http://growup.ethz.ch`). In order to merge this data with the WGI survey-data, we matched first the EPR groups to the Fearon groups.

[22] The adjusted inequality measures are generated regressors and therefore standard errors that do not take this fact into account are generally invalid since they ignore the sampling variation in such regressors. Nevertheless, for the purpose

Table 1. Within group inequality and the intensity of conflict

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
G^R	6.574***	6.003**	4.708**	4.776**	4.810**		
	(0.009)	(0.017)	(0.029)	(0.030)	(0.031)		
G^I						5.361**	
						(0.029)	
G^U							6.055**
							(0.012)
HI(LN)			-0.342	-0.337	-0.337	-0.338	-0.320
			(0.156)	(0.164)	(0.161)	(0.174)	(0.195)
GROUP GDP			-1.489	-1.402	-1.288	-1.505*	-1.480
			(0.114)	(0.129)	(0.177)	(0.100)	(0.105)
GROUP ELEV. (SD)			-0.001	-0.001	-0.001	-0.001	-0.001
			(0.159)	(0.185)	(0.253)	(0.222)	(0.267)
GIP			-0.854	-0.868	-0.875	-0.849	-0.819
			(0.131)	(0.117)	(0.115)	(0.125)	(0.126)
GROUP SIZE		-2.661	-1.826	-1.794	-1.847	-1.836	-1.857
		(0.173)	(0.436)	(0.441)	(0.429)	(0.425)	(0.422)
POP		-2.930	-1.559	-2.240	-1.208	-2.155	-2.177
		(0.166)	(0.579)	(0.432)	(0.717)	(0.452)	(0.445)
XPOLITY		-0.094*	-0.132**	-0.132**	-0.132**	-0.133**	-0.133**
		(0.059)	(0.044)	(0.046)	(0.032)	(0.044)	(0.044)
GDP		0.436	2.135				
		(0.738)	(0.190)				
POVERTY2					0.037		
					(0.264)		
INTENSITY(LAG)	4.032***	4.114***	4.280***	4.282***	4.294***	4.294***	4.285***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
R^2	0.640	0.663	0.706	0.705	0.704	0.704	0.705
Obs	6675	6368	4887	4887	4744	4887	4887

Note. The dependent variable is INTENSITY. All models contain country and year dummies. Estimation is by maximum likelihood in an ordered logit model. p-values based on robust standard errors clustered at the country level are in parentheses. $^{*}p < 10$, $^{**}p < .05$, $^{***}p < .01$.

level, it adds GROUP SIZE, the group's relative size, as measured by Fearon (2003). In addition, it adds three country-level controls: POP is the (lagged) log of the country's population; XPOLITY is a country's (lagged) democracy score based on Polity IV;[23] and GDP is the log of the country's GDP per capita (lagged) from the Penn World Tables (PWT). The coefficient for G^R remains positive and precisely estimated when these controls are included. Model 3 includes four additional

of testing whether the inequality variables are significantly different from zero, the sampling variation in the generated regressors can be ignored (at least asymptotically). See Wooldridge (2002), chapters 6 and 12 for additional details.

[23] Our measure of POLITY is XPOLITY (Vreeland 2008), which combines 3 out of the 5 components of Polity IV and leaves out the two components (PARCOMP and PARREG) that are constructed using political violence in their definition.

group-level controls. GROUP GDP is the (lagged) log of the per capita GDP of the group (using the G-ECON data); GIP is a dummy variable indicating whether the group has access to power; and GROUP ELEV. (SD) is the standard deviation of the elevation of the ethnic homeland. Finally, although horizontal inequality arguments have been applied to civil war onset, not to intensity or incidence, every onset contributes to the measures of intensity or incidence. It is therefore important to examine the robustness of the WGI results when we control for this variable. We therefore also include HI(LN), our preferred measure of horizontal inequality (lagged).[24] Model 3 shows that when the group-level controls are included, the coefficient for G^R remains positive and precisely estimated, though smaller in magnitude. Note also that the group-level GDP variable is negative and somewhat precisely estimated (as arguments about poverty and wealth would suggest) but the country-level GDP has the wrong (positive) sign and is far from being significant. Although it is standard to include country-level GDP in country-level regressions, at the group level it is less clear it should be included when controlling for group GDP, which captures much of the information in the country-level GDP measure. Indeed, the correlation for the group GDPs and the country GDP is .90. Model 4, our baseline specification henceforth, re-estimates model 3 without GDP, making it possible to include observations for which we are missing the PWT measure of GDP. The results for within-group inequality are essentially unchanged.

One might be concerned that conflict is associated with poverty, not with the existence of both poor *and* rich individuals within the same group. This concern is addressed in part by controlling for a group's GDP per capita in all specifications, but group GDP is an imperfect proxy for poverty. Unfortunately, data on poverty at the group level is not available, but model 5 adds a country-level variable: POVERTY2 measures the percentage of the population with income lower than 2 dollars a day. The results for G^R are robust to the inclusion of this variable.[25]

Models 6 and 7 consider alternative survey-based measures of WGI. In model 6, G^I is the survey-based Gini coefficient adjusted using the intercept approach and in model 7 G^u is the unadjusted survey-based group Gini. The results for WGI are robust to using either measure.

[24]We show below that the results are very similar when using LINEQ2.

[25]We also used POVERTY4, which measures the percentage of the population with income lower than 4 dollars a day, and again the results for within-group inequality are robust.

Model 7 suggests that the relationship between WGI and conflict incidence exists in the raw data and is not being driven by decisions with respect to adjusting the heterogenous surveys, although the heterogeneity in the surveys obviously warrants making such adjustments.

The coefficients for the WGI variables are not only precisely estimated, they are substantively large. We can use the estimates from our baseline specification (column 4 in Table 1) to give an idea of their magnitude. For instance, moving from the median value of G^R (.37) to the value in the 90th percentile (.54), while holding all other variables at their means, increases the predicted probability of conflict (i.e, the probability of observing strictly positive values of INTENSITY) by 225%. To put this effect into perspective, we have performed similar calculations for the other variable that is always significant in Table 1, XPOLITY. In this case, moving from the median value of XPOLITY (4) to the value in the 90th percentile (7), while holding all other variables at their means, reduces the predicted probability of conflict by 37%.

4.1. **Further robustness tests.** One way to further assess robustness is to use INCIDENCE as an alternative measure of conflict. We estimated each of the models in Table 1 using INCIDENCE as the dependent variable and find the same strong results for the WGI variables. The results are in Table C.1 in Appendix C. We also estimated the models using subsets of the data. Conflict is a relatively low frequency event, and to determine if the results are due to the leverage of a particular country, we re-estimated model 4 in Table 1, but omitting each country. The coefficient for G^R is fairly precisely estimated when any country is excluded, and the results are weakest when India is excluded (p=.10). In addition, as noted above, the measures of WGI in the previous tables are based on the average of the various surveys for a group, and this average is applied to all years, even those that precede the date of the survey.[26] We therefore re-estimated the models in Table 1 using only data during or after the earliest year for which we have survey data. Considerably less data is available, but the results are robust, despite the fact that the standard errors for the WGI variables are slightly larger (see Table C.2). We have also re-estimated models 3-7 in Table 1, substituting LINEQ2 (measured with the G-Econ data) for HI(LN). The results for WGI remain robust (see Table C.3).

[26]Below we present evidence that civil war does not have an effect on within-group inequality.

In sum, the empirical models consistently estimate a precise association between within-group inequality and the intensity and incidence of civil conflict. This association exists when we include country and year indicators as well as a lagged dependent variable. The results are robust to different measures of conflict intensity and incidence, different methods for adjusting survey-based within-group inequality measures, different sets of control variables, and different subsets of the data. In addition, the effect of WGI on conflict is not only statistically significant, it is also substantively large, as the previous section shows.

4.2. **Correlation versus causality.** While these results provide robust support for the ER theory, they obviously do not imply causation. As usual, the two central concerns are omitted variable bias and reverse causality. We discuss the two in turn.

Omitted variable bias. We face the standard concern that omitted variables may be affecting both the heterogeneity of incomes within a group and its propensity to be involved in conflict. Our identifying assumption – i.e., that within-group inequality is close to random conditional on observable characteristics – will not hold if there is a systematic relationship between group Ginis and other unobserved country or group characteristics. Our analysis partly attenuates this problem by introducing country-fixed effects in all specifications, thereby relying exclusively on within-country variation to identify the parameters. Our specifications also control for a large number of group-level characteristics.

Nonetheless, the possibility of omitted variable bias always exists. We can investigate its relative importance by estimating how the coefficients of interest change with the inclusion of the additional explanatory variables. To the end, we employ a method recently developed by Oster (2016), which builds on the work by Altonji, Elder and Taber (2005). The method is based on the fact that omitted variable bias is proportional to coefficient movements computed in models with and without controls, scaled by the change in R-squared when controls are included. The idea is that if including controls substantially attenuates the coefficient estimates on WGI, then it is possible that inclusion of more controls would reduce the estimated effect even further. But if the inclusion of controls has no effect on the magnitude of coefficient estimates, we can be more confident in suggesting a causal interpretation for the estimated relationship. As emphasized by Oster (2016), scaling by R-squared movements is key to diagnosing the quality of the added controls because adding an irrelevant control might not have an effect on the coefficient of interest, making that coefficient look stable despite the fact that the omitted variable bias can be large. Thus, by scaling by movements in R^2's, it's possible to take into account the quality of the controls.

We have computed the amount of correlation between the unobservables and WGI, relative to the correlation of the observables and WGI, that would be necessary to explain away our key result (i.e., to make the coefficient of WGI equal to zero). In its simplest formulation, this value, denoted by δ, can be computed as follows (see Oster, 2016):[27]

$$\delta = \frac{\beta_c}{\beta_{nc} - \beta_c} \frac{R_c^2 - R_{nc}^2}{R_{\max}^2 - R_c^2},$$

where β_c and β_{nc} are the coefficients of WGI in a model that contains all the observable controls and one with no or a few controls, respectively, and R_c^2 and R_{nc}^2 are the R^2's associated with those regressions. Finally, $R_{\max}^2$ is one's assumption about the maximum R^2 that could be attained if all the relevant controls were observed.

A value of $\delta = 2$, for example, would suggest that the unobservables would need to be twice as important as the observables to produce a treatment effect of zero. Altonji et al. (2005) and Oster (2016) suggest that values of δ larger than 1 in absolute value can be interpreted as evidence that omitted variable bias is unlikely to explain the observed result. A value of 1 (or larger) means that the unobservables would need to be at least as important as the observables to produce a treatment effect of zero. Since researchers typically choose the controls they believe *ex ante* to be the most important (Angrist and Pischke, 2010), situations where the effect of the unobservables is larger than that of the controls are deemed unlikely.

To assess the importance of the omitted variable bias we have computed the value of δ using Oster's technique. Table C.4 in Appendix C presents our results. The full model corresponds

[27]This definition of δ corresponds to the case where there is a single observable control, see Oster (2016) for details on the more general case

to our baseline specification (column 4 in Table 1). Restricted models I, II and III correspond to models with no controls, with country fixed effects and with country and year fixed effects, respectively. We consider three different values of $\mathrm{R}_{\mathrm{max}} = \{1, 0.9, 0.8\}$. The figures in Table C.4 correspond to the values of δ for each of the 9 cases considered. In most cases we obtain values of δ that are larger than 1 in absolute value, which suggests that it is not likely that the significance of WGI is due to omitted variable bias. Only in two of the nine combinations (when we consider a model with no controls – Restricted model I – and very high values of $\mathrm{R}_{\mathrm{max}}$ (equal to 1 or 0.9) do we obtain values of δ that are smaller than 1. Thus, for any of our models that include even only country fixed effects, it is unlikely according to this test that the causal effect of WGI could be zero.

Reverse causality. Social conflict can disrupt the economy and affect the distribution of income within groups through its effects on wages and employment, destruction of infrastructure, or the confiscation of assets, among other things. But while conflict can clearly affect inequality, it is unclear whether or why this effect should be systematic. A civil war, for example, may hurt the richest within groups (lowering intra-group inequality) or hurt the poorest (increasing intra-group inequality). Though we cannot offer a full theoretical treatment of this issue, we can examine whether civil war influences group-level Ginis in our data because for some countries we have data on inequality at different points in time. Thus, we can measure changes in inequality within groups over time to see if these changes differ with the incidence of civil conflict.

We therefore created a data set where the unit of observation is a country-group, and for each group, we have measured how within-group inequality changes (using both oth G^R and G^I) between the first and last year for which we have data. This "change in group Gini" variable is the dependent variable that we regress on the proportion of years in which there was civil conflict after the first year for which we have data for a group. The regressions include two control variables: one for the last year for which we have data, and another for the total number of years between our first and last survey for the group. We estimated the model with and without country dummy variables, and with and without groups that were in conflict for the first year we have data. In none of the eight models we estimated is the coefficient for the proportion of years in conflict at all precisely estimated, and it switches signs across various models. Although based on a relatively small amount of data, this analysis suggests there is no evidence that past civil conflict has a systematic positive or negative relationship with the level of inequality within a group. The full results are in Table C.5.

Table 2. Within-group inequality (measured with surveys) and conflict onset

	(1)	(2)	(3)	(4)	(5)	(6)
G^R	2.499	2.149	2.086		1.986	
	(0.565)	(0.609)	(0.627)		(0.692)	
G^I				4.641		4.458
				(0.300)		(0.388)
GROUP GDP		-1.539	-1.518	-1.499	-1.598	-1.562
		(0.198)	(0.186)	(0.188)	(0.186)	(0.163)
HI(LN)		-0.239	-0.236	-0.249		
		(0.610)	(0.619)	(0.596)		
LINEQ2					-0.005	0.081
					(0.998)	(0.970)
GROUP SIZE		-3.044	-3.062	-3.256	-2.963	-3.123
		(0.428)	(0.424)	(0.419)	(0.428)	(0.425)
GROUP ELEV. (SD)		-0.002	-0.002	-0.001	-0.002	-0.001
		(0.274)	(0.283)	(0.329)	(0.311)	(0.369)
GIP		-2.522***	-2.509***	-2.442***	-2.502***	-2.437***
		(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
GDP		0.665				
		(0.867)				
POP		-3.117	-3.311	-3.706	-4.011	-4.450
		(0.742)	(0.733)	(0.695)	(0.674)	(0.629)
XPOLITY		-0.100	-0.099	-0.100	-0.092	-0.093
		(0.548)	(0.550)	(0.537)	(0.542)	(0.528)
c	-5.915***	-5.847	0.280	-0.927	1.099	-0.024
	(0.004)	(0.864)	(0.969)	(0.901)	(0.883)	(0.998)
R^2	0.096	0.225	0.225	0.228	0.222	0.225
Obs	1183	820	820	820	820	820

Note. The dependent variable is ONSET. All models contain country and year dummies. Estimation is by maximum likelihood using logit. p-values based on robust standard errors clustered at the country level are in parentheses. $^{*}p < 10$, $^{**}p < .05$, $^{***}p < .01$.

5. Empirical analysis of group inequality and the onset of conflict

The analysis above focused on the intensity and incidence of civil conflict, which the ER theory suggests should be linked to within-group inequality. This section focuses on the *onset* of conflict, which has been central to studies emphasizing horizontal inequality. Before turning to the role of horizontal inequality, however, it is instructive to consider the relationship between within-group inequality and conflict onset. Esteban and Ray's argument does not specifically link WGI to the start of civil wars: conflict might break out for a variety of reasons, but labor and capital should influence whether it can be sustained and how fiercely it can be fought. But even though within-group inequality might not spark conflict, it may lower the costs of starting one when some spark occurs. In addition, every onset of conflict contributes to measures of intensity or incidence. Thus, although finding a relationship between WGI and conflict onset would not be inconsistent with ER's theory, the fact that years of pure incidence are censored from the sample is likely to reduce the explanatory power of WGI in onset regressions (notice that onset observations are only around 17% of all conflict observations). We therefore begin by revisiting this relationship, which is also the focus of KW's analysis.

5.1. **WGI and conflict onset.** The dependent variable in our onset regressions, also taken from EPR, is ONSET, which equals 1 during the first year in which a group is involved in an armed conflict that results in more than 25 battle-related deaths.[28] Model 1 in Table 2 is the same as model 1 in Table 1, with only country and year indicators as controls, and without the lagged dependent variable (since there is no concern about persistence when estimating onset models). Model 2 adds all group-level and country level controls (as in model 3 of Table 1) and model 3 removes the country-level GDP from model 2. Model 4 uses G^I as the measure of within-group inequality, and models 5 and 6 re-estimate models 3 and 4, replacing HI(ln) with LINEQ2 (which has been used in previous research on horizontal inequality and conflict onset). The results suggest a very weak relationship between WGI and conflict onset: the parameter estimates for the WGI variables are always positive but always estimated with substantial error. We also estimated these 6 models by excluding observations for years prior to our first survey. Although the small amount

[28]This variable is called ONSET_DO_FLAG in the EPR data set, and it is missing in years where a group is involved in conflict during the years immediately following the year when conflict is initiated. Thus, the data include group-years where a group either begins involvement in conflict or is not involved in conflict. See Wucherpfennig et al. 2012 for details on conflict measures.

Table 3. WITHIN-GROUP INEQUALITY (MEASURED USING NIGHTLIGHTS) AND CONFLICT ONSET

	(1)	(2)	(3)	(4)	(5)	(6)
IGI	1.921*	1.026	1.457	1.007	1.654	1.155
	(0.082)	(0.380)	(0.222)	(0.354)	(0.166)	(0.371)
LINEQ2(t)	0.210	0.407*	0.216	0.241	-0.620	-0.619
	(0.210)	(0.091)	(0.259)	(0.170)	(0.314)	(0.350)
EXCLUDED GROUP	1.002**	1.019**	1.046**	1.190**	0.984**	1.042**
	(0.017)	(0.015)	(0.011)	(0.012)	(0.025)	(0.015)
GROUP GDP(t)		0.405				
		(0.253)				
GROUP SIZE$_{\text{EPR}}$		0.694				
		(0.869)				
GROUP ELEV. (SD)		0.001**	0.001**	0.001		0.001**
		(0.023)	(0.042)	(0.133)		(0.039)
POW. BALANCE	-2.465	-4.341	-3.938	-3.423	-2.663	-4.289
	(0.435)	(0.250)	(0.286)	(0.420)	(0.391)	(0.240)
POW. BALANCE^2	1.509	3.137	3.212	3.015	1.527	3.396
	(0.695)	(0.591)	(0.465)	(0.571)	(0.695)	(0.441)
GDP(t)	-0.105	-0.244	-0.099	-0.527	-0.237	-0.243
	(0.902)	(0.736)	(0.909)	(0.695)	(0.780)	(0.778)
N. EXCL. GROUPS	-0.001	0.006	0.006	0.068	0.028	0.033
	(0.988)	(0.923)	(0.919)	(0.619)	(0.717)	(0.675)
POP				0.782*		
				(0.055)		
XPOLITY				-0.033		
				(0.749)		
YEAR	-0.071	-0.080*	-0.076	-0.047	-0.068	-0.073
	(0.126)	(0.074)	(0.103)	(0.450)	(0.163)	(0.134)
PEACEYRS	-0.284	-0.269	-0.280	-0.252	-0.311*	-0.302*
	(0.106)	(0.141)	(0.114)	(0.238)	(0.074)	(0.087)
SPLINE1	0.010	0.008	0.010	0.007	0.012	0.011
	(0.483)	(0.572)	(0.511)	(0.684)	(0.400)	(0.442)
SPLINE2	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.691)	(0.798)	(0.728)	(0.900)	(0.595)	(0.648)
SPLINE3	0.000	0.000	0.000	-0.000	0.000	0.000
	(0.817)	(0.924)	(0.854)	(0.964)	(0.714)	(0.765)
R^2	0.192	0.200	0.197	0.223	0.185	0.191
Obs	2982	2977	2977	2591	2956	2951

Note. The dependent variable is ONSET. All models contain country dummies. Estimation is by maximum likelihood using logit. p-values based on robust standard errors clustered at the country level are in parentheses. $^{*}p < 10$, $^{**}p < .05$, $^{***}p < .01$.

of remaining data for onset regressions is quite small, again we find no relationship between WGI and conflict.

These results contrast sharply with KW's finding of a positive and significant relationship between conflict onset and their measures of WGI based on nightlight emissions data. The differing results could be due to a number of factors, including that the surveys result in fewer observations and have different input data on income. In addition, the KW analysis uses a different set of control variables. It is therefore worth revisiting KW's analysis using that paper's measure of within-group inequality, called IGI (for intra-group inequality).[29] Model 1 in Table 3 replicates model 4 in Table 1 of KW's paper, which relies on the same set of control variables as in CWG, but which (unlike CWG) includes country fixed effects. Since we are considering all years (starting in 1992) for which EPR data is available, our data merging process results in more observations than in KW's original paper. However, the results are similar to those presented in KW: the coefficient for IGI is positive and significant at the 10% level, though not as precisely estimated as in KW's data.[30]

This result for IGI, however, is not at all robust. Omitted variable bias is one concern. We explored the robustness of the results to omitted variables employing the technique developed by Oster (2016), as in Section 4.2 above, computing the values of δ in a setup where the full model is that of column 1 and the three restricted models contain (i) no controls, (ii) country fixed effects and (iii) country and year dummies. In all cases the values of δ were close to zero (ranging between 0.09 and 0.33), raising concerns that the estimates for IGI in model 1 could suffer omitted variable bias.[31] This concern is clear from the model in column 2, which adds three group-level control variables that are present in the models in Section 4 and that are taken directly from the EPR data set: GROUP GDP, GROUP ELEV.(SD) and GROUP SIZE.[32] Though the coefficient for IGI remains

[29]See Section A.4 for a comparison of the survey and the nighlight WGI measures.

[30]The models presented in the previous tables use lags of the economic variables in order to diminish concerns about reverse causality. For consistency with the KW and CWG approaches, variables in Table 3 are not lagged, and thus are noted with T in parentheses. The results are substantively similar regardless of whether the variables are lagged. Model 1 also omits 15 observations by using exclusion rules commonly used in research that employs EPR data to estimate models of civil war onset. In particular, groups are excluded if they are judged to be dominant, to have a monopoly on power, or if they are geographically dispersed (see discussion in CWG 2011). The results are essentially identical if these exclusion rules are ignored.

[31]KW also consider in their robustness checks an omitted variable bias analysis in a similar vein as the one discussed in the text, and obtain values of δ that suggest that results are robust to omitted variable bias. However, they use the technique introduced by Bellows and Miguel (2009) that does not incorporate the movements in R^2 in the estimation of δ. As discussed at length in Oster (2016), the omitted variable bias is proportional to the movement in coefficients *only* if movements in R^2's are also taken into account and, thus, it is critical to introduce this term in order to have accurate results.

[32]GROUP SIZE is SIZE (EPR) from the EPR data, and is the "group's population size as a fraction of the ethnically relevant population of this group's country."

positive, it is now estimated with considerable error (p=.38). Of the three group-level variables added to model 2, however, only one is precisely estimated, GROUP ELEV. (SD), so model 3 re-estimates model 2 without GROUP GDP and GROUP SIZE. The coefficient for IGI remains imprecisely estimated. Model 1 also lacks two country-level controls, POP and POLITY. When these variables are added to model 3, the coefficient for IGI remains very imprecisely estimated (see model 4), though the two country level variables also have imprecisely measured coefficients. Finally, it is useful to note that the results in model 1 require the presence of a particular group: the East Timorese in Indonesia. Model 5 presents results from re-estimating model 1 without this group and the coefficient for IGI now is insignificant. And when we add GROUP ELEV. (SD) to model 5 (see model 6), the p-value of IGI's coefficient is .37.

We therefore find little support for a robust association between within-group inequality and conflict onset, regardless of whether WGI is measured using nightlights data or surveys. While it is always possible that these null results are due to measurement error, we suspect that is not the issue here. Instead, the null results are not inconsistent with the ER argument, which emphasizes that WGI increases the capacity to fight rather creating incentives to do so.

5.2. **Horizontal inequality and conflict onset.** The arguments about horizontal inequality and conflict onset have found substantial support in group-level empirical tests, all of which rely on the EPR group-level data (see KW, CWG, Cederman, Weidmann and Bormann 2015, and Cederman, Gleditsch and Buhaug 2013). But in Tables 2 and 3, the coefficients for horizontal inequality vary widely in size and even in sign, and only in column 2 of Table 3 is the coefficient positive and somewhat precisely estimated (p=.09). One might worry, however, that each data set used above to measure WGI – using either surveys or nightlights – has considerably fewer observations than is the case when one measures horizontal inequality using the full EPR data set. It is therefore worth further probing the relationship between horizontal inequality variables and conflict onset by using all available EPR data set (and not just the EPR data that maps to KW's IGI variable, as in Table 3).

We begin by using the using EPR's G-ECON data on group economic well-being to create the three measures of horizontal inequality discussed in section 3.1: LINEQ2, HI(ABS) and HI(LN). The summary statistics for these variables as well as the correlations between them are in Tables B.1 and B.2 in Appendix B. The highly non-linear functional form of LINEQ2 results in a variable with a skewness of 6.7 and a kurtosis of 63.6, which is a potential concern since such skewed measures invite the possibility of high leverage by outlying observations. By comparison, the skewness of HI(ABS) and HI(LOG) are 3.4 and -0.4, and the kurtosis of the two variables are 21.3 and 3.3. Although the three measures aim to capture the same underlying concept, LINEQ2 is very weakly correlated with HI(ABS) (r=.20) and HI(LN) (r=.13). Given the straightforward interpretation of the HI variables, these low correlations underline concerns that LINEQ2 may not be a good measure for tapping the underlying concept of horizontal inequality.

As in the analysis above, we estimate logit models with robust standard errors clustered at the country level, and we include country indicator variables.[33] We use the set of controls used in KW and CWG, which as noted above are also the same as those used in Model 1 of Table 3.[34] We lag the economic variables to guard against reverse causation, and following CWG, the models exclude dominant groups, monopoly groups, and geographically dispersed groups.

Model 1 in Table 4 uses LINEQ2 as the measure of horizontal inequality, and the variable has a positive coefficient but one estimated with considerable error (p=.40). The GDP data from the Penn World Tables are missing for a number of observations for countries in the EPR data set, and it is possible to use the EPR country-level data to measure GDP per capita using the G-ECON data. The (lagged) log of GDP per capita using the country-level data, GDP(GECON), has a correlation of .93 with GDP. Model 2 re-estimates model 1 using GDP(GECON) as the country-level control for GDP and the number of observations increases from 4,135 to 4,564. The coefficient for LINEQ2 is now negative, though not precisely estimated.

[33]In estimating group-level regressions, it is crucial to include the country fixed effects in order to be confident that unobserved country-level variables are not driving the results, a point that is also emphasized by KW. CWG, Cederman, Weidmann and Bormann (2015) and Cederman, Gleditsch and Buhaug (2013) do not include country indicator variables.

[34]The control variables are obtained from the GROWup portal except GDP (the log of the country GDP per capita, constant dollars), which we obtain from the Penn World Tables.

Table 4. Horizontal inequality and conflict onset using EPR data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
LINEQ2	0.161	-0.145	0.222	-0.652	-0.135		
	(0.333)	(0.511)	(0.101)	(0.578)	(0.523)		
HI(ABS)						-1.261	
						(0.253)	
HI(LN)							0.087
							(0.399)
EXCLUDED GROUP	1.145***	0.935**	0.912*	0.885*	0.817**	0.991***	0.917**
	(0.002)	(0.012)	(0.082)	(0.096)	(0.025)	(0.010)	(0.014)
POW. BALANCE	-0.911	1.063	-2.347	-2.452	0.857	0.645	1.248
	(0.745)	(0.689)	(0.488)	(0.476)	(0.753)	(0.802)	(0.625)
POW. BALANCE^2	0.275	-1.768	0.891	0.954	-1.497	-1.313	-1.919
	(0.942)	(0.636)	(0.824)	(0.816)	(0.697)	(0.717)	(0.587)
GDP	0.126		-0.737	-0.760			
	(0.883)		(0.464)	(0.457)			
GDP(GECON)		0.346			0.347	0.465	0.377
		(0.478)			(0.479)	(0.347)	(0.449)
N. EXCL. GROUPS	-0.008	-0.009	-0.009	0.014	0.035	-0.010	-0.005
	(0.894)	(0.940)	(0.892)	(0.870)	(0.750)	(0.934)	(0.969)
YEAR	-0.045	-0.054*	-0.042	-0.037	-0.059*	-0.054*	-0.060*
	(0.319)	(0.087)	(0.396)	(0.471)	(0.097)	(0.088)	(0.063)
PEACEYRS	-0.371*	-0.401**	-0.463**	-0.480**	-0.355**	-0.402**	-0.384**
	(0.055)	(0.011)	(0.034)	(0.029)	(0.030)	(0.011)	(0.016)
SPLINE1	0.016	0.018	0.024	0.025	0.015	0.018	0.017
	(0.300)	(0.130)	(0.153)	(0.138)	(0.237)	(0.133)	(0.163)
SPLINE2	-0.000	-0.000	-0.000	-0.001	-0.000	-0.000	-0.000
	(0.454)	(0.247)	(0.229)	(0.211)	(0.388)	(0.255)	(0.294)
SPLINE3	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.556)	(0.328)	(0.271)	(0.254)	(0.477)	(0.340)	(0.381)
R^2	0.178	0.156	0.188	0.175	0.142	0.156	0.155
Obs	4053	4564	2210	2205	4180	4564	4564

Note. The dependent variable is ONSET. All models contain country dummies. Estimation is by maximum likelihood in a logit model. p-values based on robust standard errors clustered at the country level are in parentheses. $^{*}p < 10$, $^{**}p < .05$, $^{***}p < .01$.

CWG argue that for measurement reasons it makes sense to exclude groups with a population less than 500,000. Models 3 therefore re-estimates Model 1, but excluding such groups (which results in the loss of considerable data). The results provide more support than previous models for horizontal inequality arguments as the positive coefficient for LINEQ2 is now positive and somewhat precisely estimated (p=.10). This result, however, is very fragile. There is one group in the data set that results in an extreme outlier: the East Timorese in Indonesia appear in 5 years, they initiate conflict in two of these five years, and LINEQ2 for this group is roughly 14 standard deviations above the mean for this variable (the highest score of any group in the model). Model 4 re-estimates model 3 without these 5 observations and the coefficient for LINEQ2 changes sign to *negative* (but is not close to statistically significant). And when we re-estimate model 3 but use GDP(GECON) as the GDP control, the coefficient for LINEQ2 is also negative but imprecisely estimated (see model 5). Thus, there is no evidence in these models of a robust positive association between LINEQ2 and conflict onset.

Next consider the two HI variables. Model 6 re-estimates model 2 using HI(ABS) instead of LINEQ2 as the measure of horizontal inequality. The coefficient for HI(ABS) has the wrong sign but is not significant. When we exclude small groups from model 6 (results not reported), the coefficient for HI(ABS) remains negative and insignificant. Model 7 uses HI(LN) as the measure of horizontal inequality and its coefficient is positive but with a p-value of .40. When we exclude small groups (not reported) the results are similarly unsuppportive of arguments about horizontal inequality. We also used nightlights data to create the various horizontal inequality variables and then re-estimated all the models in Table 4. In each model, the coefficient for horizontal inequality is negative and imprecisely measured (see Table C.6). Thus, we find no evidence in these data for the horizontal inequality arguments.

Since the publication of CWG in 2011, Cederman, Gleditsch and Buhaug (2013) and Cederman, Weidmann and Bormann (2015) do not utilize LINEQ2, focusing instead on HIGH and LOW to assess arguments about inequality across groups. As discussed above (a) both LOW and HIGH are less direct measures of horizontal inequality than we would like (because g is included in the denominator and the denominator treats all groups as if they were equal in size), and (b) if results from this approach are strong for LOW but weak for HIGH, we cannot know if inequality or poverty might be the explanation. It is worth noting that in Cederman, Gleditsch and Buhaug (2013), only the coefficient for LOW is precisely estimated (see p. 108) and thus there is little evidence in that analysis supporting the horizontal inequality argument. In Cederman, Weidmann and Bormann (2015), both the LOW and HIGH coefficients are significant, but these models do not include country fixed effects. When such fixed effects are added to these results using the Cederman, Weidmann and Bormann (2015) replication materials, the results hold for LOW often hold but the coefficients for HIGH cease to be significant (see Table C.7). This suggests that poverty (rather than inequality) is associated with conflict onset since richer groups do not seem to display a higher propensity to start conflicts.[35]

Our analysis of the evidence regarding horizontal inequality and conflict onset thus suggests there is no robust relationship between horizontal inequality and conflict onset. Using the EPR data and the same measures of horizontal inequality as well as new ones, and using both G-ECON and nightlights data for groups, we find no evidence of any association between horizontal inequality and conflict. While this stands in contrast to previous conclusions using these data, we feel the null results should be unsurprising since the theoretical underpinnings of the horizontal inequality arguments seem to us both incomplete and unconvincing.

6. CONCLUSION

Studies of inequality and civil war have often emphasized the role of economic grievances. In country-level studies, measures of inequality are viewed as proxies for such grievances, and null-findings regarding inequality in cross-national empirical analyses have led scholars to marginalize the study of both inequality and grievances from the study of internal conflicts. In the past five years, however, research focusing on economic differences across groups has led to a renaissance in grievance-based arguments. Focusing on which types of groups initiate civil wars, these studies have found a central role for horizontal inequality, which is linked to economic grievances.

Our analysis suggests limitations in both the country-level and group-level conclusions about inequality. Unlike the previous country-level studies, we find it is a mistake to dismiss the role of inequality in efforts to understand civil wars. But unlike the previous group-level studies, our null findings regarding horizontal inequality suggest that it is also a mistake to link inequality either to the concept of grievance or to the outbreak of civil conflicts. Instead, following recent theoretical research, we test empirically the idea that inequality within a group is a proxy for the group's capacity to fight. The empirical analysis demonstrates that this capacity is linked not

[35] We also used the EPR data to calculate LOW and HIGH, using both nightlights and Gecon data and then re-estimated models 1-5 in Table 4. The results often yield a negative coefficient for one of the two variables, and results vary a great deal across the models in the table, but in general neither variable is significant

to the outbreak of civil wars, but rather to their incidence and intensity. Since civil wars vary considerably in their severity, we hope this analysis will not only refine our understanding of inequality and civil conflict, but also motivate additional studies aimed at shedding light on why some civil conflicts spin out of control and others do not.

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APPENDIX A. USING SURVEYS TO COMPUTE GROUP-LEVEL GINI COEFFICIENTS

This Appendix provides additional details about the survey-based group-level Gini coefficients and their construction. We first describe in more detail the two approaches used to deal with survey heterogeneity (Section A.1). Sections A.2 and A.3 discuss sample coverage and sample representativeness, respectively. Section A.4 compares the two existing datasets of within-group ethnic inequality, the one computing using nightlight emissions data and the one introduced in this paper that uses surveys. Finally, Section A.5 provides a list of all the surveys considered to construct our inequality measures.

A.1. **Data construction: The Intercept and The Ratio Approaches.**

i) The "intercept approach" to adjusting the survey measures of inequality. The first approach to adjusting the inequality measures is similar to that employed in the original Deininger and Squire (1996) exercise. The idea is to remove average differences due to different survey methodologies. To implement this approach, we first calculate the Gini coefficient for each group using the surveys. We then regress the group Gini on survey, time and country dummies, with HES as the omitted category. We use the HES as reference since these surveys are probably the best-available estimates of income distributions in the world. The shift coefficients on the survey dummies are then used to adjust the inequality measures so as to remove average differences that could be traced to different survey types.

ii) The "ratio approach" to adjusting the components of the Gini. The second approach draws on external data on the Gini – the Standardized World Income Inequality Dataset (SWIID)– to adjust the group-level measures of the Gini as well as the three components of the Gini decomposition. The SWIID (Solt 2009) provides comparable Gini indices of gross and net income inequality for 173 countries from 1960 to the present and is one of the most thorough attempts to tackle the comparability challenge (see Solt 2009 for details on the methodology). We use the SWIID data and a methodology similar to Solt (2009) to obtain (time-varying) adjustment factors for the overall country Gini from each country and year and then apply these factors to the group Ginis from the surveys. Let $G_{c,t,\mathrm{SWIID}}$ be the SWIID Gini for country c in year t and $G_{c,t,s}$ be the Gini from country c and year t using survey s. The *ratio* approach involves 4 steps. The first three are similar to employed by Solt (2009) to adjust country-level inequality measures. The fourth step applies the same adjustment procedure to thegroup-level Ginis.

Step 1: Whenever a survey Gini and the SWIID Gini are available for the same country and year, we compute their ratio, $R_{c,t,s} = \frac{G_{c,t,s}}{G^S_{c,t}}$.

Step 2: For the 201 available ratios, we regress $R_{c,t,s}$ on country (α_c) and year (δ_t) dummy variables. Specifically, we estimate:

(1) $$R_{c,t,s} = \alpha_c + \delta_t + \epsilon_{c,t,s}.$$

Step 3. For each survey we use the parameter estimates from eq. (1) to obtain the predicted values of the ratios, $\hat{R}_{c,t,s}$, for all surveys. For those surveys where ratios exist, the predicted ratios are of course very close to the actual ratios (r=.98), but the predicted ratios also can be derived from Eq. (1) for the 16 surveys where the SWIID Gini is missing. This is justified by the fact that the factors that affect these ratios tend to change only slowly over time within a given country and, hence, the missing ratios can be predicted based on available data on the same ratio in the same country in proximate years. As in Solt (2009), the adjusted Gini coefficient for country c, time t and survey s, $\widehat{G_{c,t,s}}$, could be computed as the product of $\widehat{G_{c,t,s}}$= $\overline{R}_{c,t,s}G_{c,t,s}$.

Step 4. To obtain adjusted measures using the ratio approach, we take the product of the group Ginis from the surveys and the predicted ratios:

(2) $$\widehat{G_{g,c,t,s}} = \hat{R}_{c,t,s} * G_{g,c,t,s}.$$

where G_g denotes group-level Ginis. We justify this last step as follows. Remember that the country-level Gini coefficient can be written as:

(3) $$G = WGI_c + BGI_c + OV_c,$$

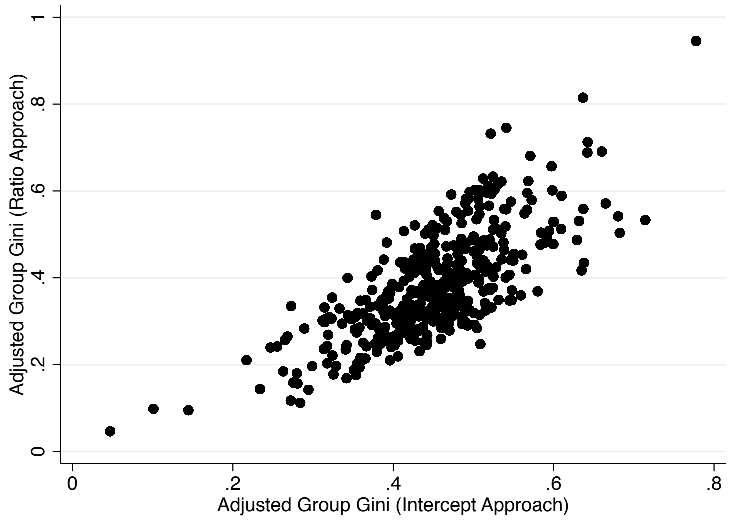


Figure A.1. G^R versus G^I

where WGI_c is the weighted average of group Ginis, BGI_c is a measure of the average difference in group mean incomes in a society and Ov_c (from overlap) is a residual term. It follows that $\widehat{G_{c,t,s}} = \hat{R}_{c,t,s} * G_{c,t,s} + \hat{R}_{c,t,s} * BGI_{c,t,s} + \hat{R}_{c,t,s} * Ov_{c,t,s}$. Since WGI is a weighted average of the group Ginis, it follows that by adjusting each of the group Ginis by the same factor $\hat{R}_{c,t,s}$, one obtains the "adjusted" WGI, $\widehat{WGI} = \hat{R}_{c,t,s} * WGI$. Thus, the adjusted country-level Gini and the adjusted group Ginis are internally consistent.

Step 4 yields the measures we use in our empirical analysis using the "ratio" approach. As in the intercept approach, time-invariant measures are computed by averaging all observations available for one group/country and assigning the average values to all years. Such measures are denoted by G^R.

Figure A.1 displays G^R vis-à-vis G^I and shows that there is a close relationship between the two sets of survey-based Gini coefficients (the correlation coefficient is .73). G^R has a lower mean (.38) and higher standard deviation (.12) than G^I (.45 and .9, respectively).

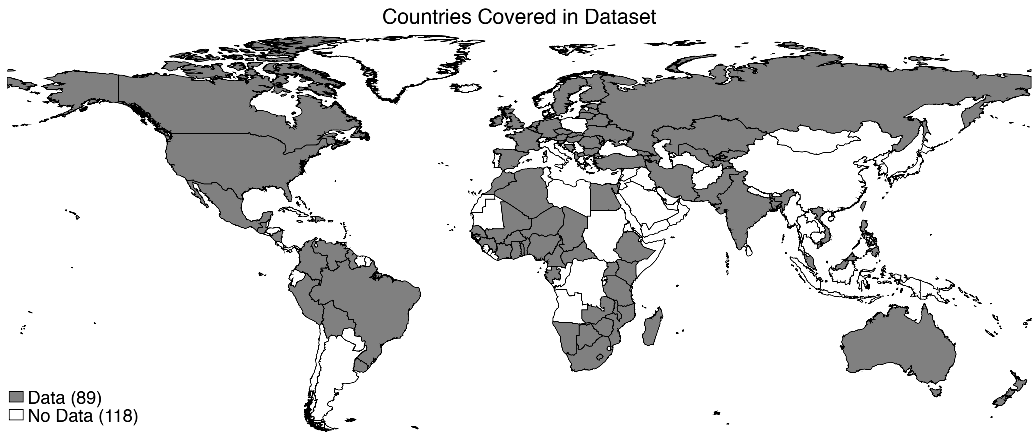


Figure A.2. Countries included in data set

A.2. **Coverage.** Using surveys, we have been able to compute Gini coefficiens for 446 groups in 89 countries. Figure A.2 depicts the countries for which we have data. Although our sample contains fewer countries than that based on nightlight emissions (which covers 558 groups in 128 countries), the coverage by country is better than in the nightlight data approach. Fearon's ethnic group classification contains 504 groups for those 89 countries, which means that almost 90% of Fearon's groups are in the survey sample. In contrast, for the 128 countries for which geo-coded group-level Gini coefficients are available, only around 70% of the groups are in the sample (i.e., 504 from the 767 groups listed in EPR).

A.3. **Sample Representativeness.** One additional limitation of the survey approach is that it can only be implemented in countries with useful surveys, and the set of such countries might be unrepresentative in important ways. In particular, one might worry that the countries where surveys exist might be correlated with ethnic conflict itself, or with variables related to ethnic conflict.

Table A.1 examines this issue empirically.[36] The table compares the sample of countries obtained from our surveys to a broader set of countries from the SWIID data set. We use it as a benchmark because it is the inequality dataset with broader coverage. The top half of Table A.1 describes the distribution of countries around the world using the SWIID and our survey data,

[36]In this analysis, we focus on 88 countries since data on some key controls are missing for one of the countries in our dataset (Bosnia) and, therefore, it never enters our regressions.

focusing on the post-1994 time period for which most of our survey data exists. There are 136 countries available in SWIID (taking into account that there are some countries in this data set for which conflict or other control variables do not exist) and 88 countries – or 64 percent of the SWIID – for which we have useful surveys. The table shows a slightly higher proportion of the countries in the survey data are from Central Europe, and a slightly higher proportion of the SWIID countries are from Latin America, but the distributions of countries across the regions are quite similar. Thus, there is little in the way of regional bias in the survey data.

The bottom half of the table provides descriptive data on key variables in the two data sets: GDP/capita, ethnic fractionalization (F), ethnic polarization (P), level of democracy (xPolity), level of inequality, and the incidence of civil conflict.[37] For each of these variables, the means for the set of countries in SWIID are quite similar to the means for the set of survey countries. We have also examined the incidence of surveys in country-years that are experiencing conflict and country-years that are not, and we do not find significant biases: Surveys exist in 19.0 percent of country-years that are not experiencing conflict, and they exist in 15.3 percent of country-years that are experiencing conflict. Thus, although there are limits to the number of countries we can analyze using surveys, the sample of countries obtained using surveys seems reasonably unbiased with respect to the variables of central interest in the analysis here.

Although this is reassuring evidence that neither the sample of countries nor the sample of groups from the surveys is particularly biased, the accuracy with which the surveys measure individual "income" can be a concern. One problem might be that in areas of a country with civil conflict, it will be difficult to conduct surveys. This concern is mitigated to a certain extent by the fact that surveys exist in countries with conflict in roughly the same proportion of cases as they exist in countries without conflict. This concern might also be particularly muted in relatively low intensity conflicts, where conducting surveys may not be particularly problematic. Such conflicts are a central (but not exclusive) focus of our analysis. The concern will also be less worrisome if we do not find that civil conflict has a systematic effect on group-level inequality, allowing us

[37]Precise variable definitions and sources are provided below.

Table A.1. Sample representativeness

	SWIID sample	Survey sample
Number of countries	136	88
Percentage of countries in:		
Central Europe	19.8	26.4
Latin America	16.2	12.5
Middle East	5.9	3.4
Africa	28.7	30.1
Neo-Europe	16.2	18.2
East Asia	8.8	5.7
South Asia	4.4	3.4
Average Real GDP/capita	\$9,836	\$10,288
Average F	.46	.50
Average P	.55	.58
Average xPolity	3.4	3.6
Average Gini (SWIID)	.38	.38
Percent of years with Prio25 civil conflict	.15	.17

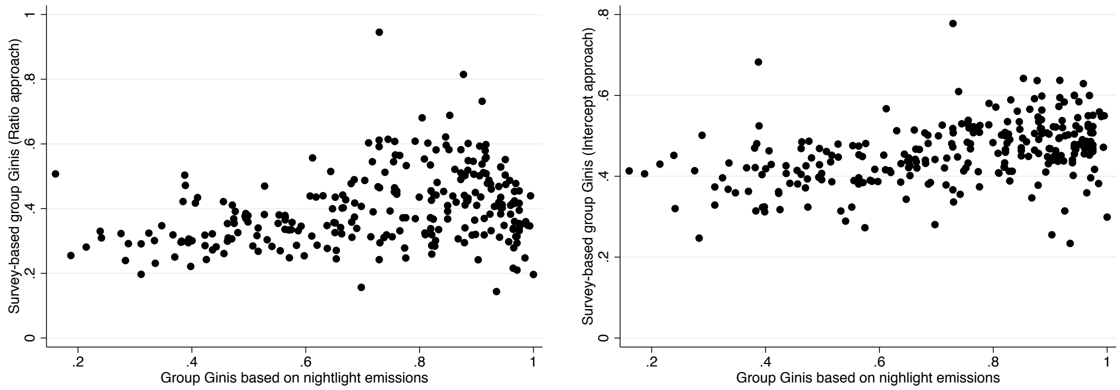
Notes. This table compares the sample of countries included in the dataset presented in this paper (88 countries) and the SWIID (137).

to more confidently aggregate measures over time. We find this null relationship in the data (see below).

A.4. **Comparing the survey and the nightlight-based group-level Gini coefficients.**

Figure A.3 compares Gini coefficients computed using nightlight data (IGI) and surveys (G^I and G^R). The graph shows that there is a positive association between both set of measures, although divergences are substantial. The correlation between IGI and the survey-based Gini coefficients is .41 and .34 for G^I and G^R respectively.

The fact that big disparities between the two datasets exist is not surprising, giving the very different methodologies employed it their elaboration. As mentioned in the main text, disparities can be due to a number of reasons, such as the degree of urbanization, group segregation, etc. The



(a) G^R vs. IGI　　**(b)** G^I vs. IGI

Figure A.3. Survey-based versus Nightlight group-level Gini Coefficients

Table A.2. The relation between inequality measures and ethnic homeland's elevation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ELEV	0.239***	0.243***	0.022	-0.015	0.226**	0.226**	-0.015	-0.001	0.010	0.008
	(0.000)	(0.000)	(0.829)	(0.898)	(0.019)	(0.019)	(0.762)	(0.984)	(0.515)	(0.583)
GDP_g		0.122		0.021				0.243***		0.024
		(0.133)		(0.843)				(0.000)		(0.629)
c	-0.543***	-0.186	-0.180	0.454	-0.090	-0.090	-0.015	0.701***	1.746***	0.028
	(0.000)	(0.471)	(0.305)	(0.285)	(0.581)	(0.581)	(0.786)	(0.000)	(0.000)	(0.569)
Dep.var.	IGI	IGI	G^R	G^R	IGI	IGI	HI(LN)	HI(LN)	CNTRY-GINI	CNTRY-GINI
R^2	0.655	0.698	0.678	0.661	0.807	0.807	0.686	0.709	0.876	0.881
Obs	470	418	211	188	211	211	417	417	374	333

Table A.3. THE RELATION BETWEEN GEOGRAPHY AND INEQUALITY MEASURES. This table regresses different inequality measures on the standard deviation of group's homeland elevation (denoted as ELEV to save space) and group's GDP (GDP_g, from G-Econ) . $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

geography of the homeland is another obvious source of divergence. Geographic characteristics might have an impact on nightlight data measurements which can lead to biases in those measures. Table A.3 examines the relation between the standard deviation of homeland's elevation and different inequality measures. It is easy to check that there is no correlation between country-level inequality measures and heterogeneity in elevations (see columns 9 and 10 in Table A.3). Similarly, at the group level we can think of no reason that a group will be more unequal if its members live at heterogeneous elevations than if they tend to live at similar elevations. But diversity in elevations might be correlated with diversity in nightlight readings. For example if travel is more costly at higher elevations, individuals might tend to live more closely together in such regions (affecting the concentration of nightlight) but be no more or less well off than individuals living at lower elevations.

Model 1 in Table A.3 shows the results from regressing IGI on GROUP ELEV (SD) –denoted in the table simply as ELEV to save space– along with country fixed effects, in a data set where a group is the unit of analysis. To facilitate interpretation, all variables in the table are standardized to have a mean of 0 and standard deviation of 1. We can see that the coefficient for ELEV is positive and large. A one standard deviation increase in ELEV is associated with a .24 standard deviation increase in IGI. Model 2 adds a control for the log of the group's GDP per capita and the result is essentially identical. Models 3 and 4 re-estimate models 1 and 2 using G^R as the dependent variable. The results are quite different, with the coefficient for G^R estimated very imprecisely.[38] Since there is data for more groups using nightlights than using surveys, models 3 and 4 have fewer observations than models 1 and 2. We therefore re-estimated models 1 and 2 using only the observation for which survey data is available. The results, in models 5 and 6, show that there is essentially zero difference with the estimates for ELEV in models 1 and 2. Finally, since measures of horizontal inequality obviously treat information from all cells for a group identically (rather than looking for heterogeneity within groups), there is little reason to expect this same relationship between measures of horizontal inequality and ELEV(SD). To see if this is true, models 7 and 8 re-estimate models 1 and 2 using HI(LN) as the dependent variable. There is no relationship between this variable and ELEV. Finally, models 9 and 10 use country-level Gini coefficients (from Solt) and show that there is no correlation between country-level inequality and elevation.

The results in Table A.3 suggest that the geo-coded measures of WGI could be biased upwards in places where variability in elevations is high. In fact, the correlation between the survey and the nightlight based WGI datasets increases considerably in places with low or moderate values of ELEV. The correlation between G_R (G^I) and IGI is .40 (.50) for ethnic homelands with a value of

[38] We also estimated these models with G^I and obtained substantively similar results.

ELEV below the median and it goes down to .27 (.32) when values of ELEV above the median are considered.

A.5. **List of Surveys.** Table A.4 lists each of the surveys employed in the elaboration of the group-level Gini coefficients.

Table A.4. The Surveys

Albania	2002(WVS) 2005(HES-LSMS)	Kyrgyz Rep	1997(DHS) 2003(WVS)
Algeria	2002(WVS)	Latvia	1996(WVS) 1999(WVS)
Armenia	1997(WVS) 2000(DHS)	Lithuania	1997(CSES, WVS)
Australia	1995(WVS) 1996(CSES) 2004(CSES) 2005(WVS)	Macedonia	1998(WVS) 2001(WVS)
Austria	2000(LIS)	Madagascar	2005(AFRO)
Azerbaijan	1995(HES-ASLC) 1997(WVS) 2006(DHS)	Malawi	2000(DHS) 2003(AFRO) 2004(DHS) 2005(AFRO)
Bangladesh	1996(WVS) 1997(DHS) 2000(DHS) 2002(WVS) 2004(DHS) 2007(DHS)	Malaysia	2006(WVS)
Belarus	1996(WVS) 2001(CSES)	Mali	1995(DHS) 2001(DHS) 2002(AFRO) 2005(AFRO) 2006(DHS)
Belgium	1999(CSES, WVS)	Mexico	1997(CSES, WVS) 2000(WVS) 2003(CSES)
Benin	1996(DHS) 2001(DHS) 2005(AFRO) 2006(DHS)	Moldova	1996(WVS) 1999(WVS) 2005(DHS) 2006(WVS)
Bolivia	2002(HES-MECOVI) 2003(DHS)	Morocco	2001(WVS) 2007(WVS)
Bosnia	1998(WVS) 2001(WVS) 2004(HES-LIBP)	Mozambique	2002(AFRO) 2005(AFRO)
Botswana	2003(AFRO) 2005(AFRO)	Namibia	2000(DHS) 2003(AFRO) 2006(AFRO)
Brazil	1996(DHS) 1997(WVS) 2002(CSES, HES-IPUMS) 2006(WVS, HES-PNAD)	Netherlands	1999(WVS)
Bulgaria	1995(HES-IHS) 1997(WVS) 2001(CSES) 2006(WVS)	New Zealand	1996(CSES) 1998(WVS) 2002(CSES)
Burkina Faso	1992(DHS) 1998(DHS, HES-EP2) 2003(DHS)	Nicaragua	2001(HES-EMNV)
Cameroon	1998(DHS) 2004(DHS)	Niger	1992(DHS) 1998(DHS) 2006(DHS)
Canada	1997(CSES, HES) 2000(WVS) 2001(HES-IPUMS) 2006(WVS)	Nigeria	2000(WVS) 2005(AFRO)
Central African Rep	1994(DHS)	Pakistan	2001(WVS)
Chad	1997(DHS) 2004(DHS)	Peru	2000(DHS) 2004(DHS, HES) 2008(WVS)
Colombia	1998(WVS)	Philippines	1993(DHS) 1998(DHS) 2003(DHS) 2008(DHS)
Cote d'Ivoire	1998(DHS)	Romania	1996(WVS, CSES) 1997(HES) 2005(WVS)
Cyprus	2006(WVS)	Russia	1995(WVS) 1999(CSES) 2000(CSES, HES) 2006(WVS)
Czech Rep	1996(CSES)	Senegal	1992(DHS) 2002(AFRO) 2005(AFRO, DHS)
Dominican Rep	1998(WVS)	Singapore	2002(WVS)
DRC	2007(DHS)	Slovakia	1998(WVS)
Egypt	1995(DHS) 2000(WVS) 2005(DHS) 2008(DHS)	Slovenia	1996(CSES)
Estonia	1996(WVS) 1999(WVS) 2000(HES)	Spain	1995(WVS) 1996(CSES) 2000(CSES, WVS) 2004(CSES) 2007(WVS)
Ethiopia	2000(DHS) 2005(DHS)	South Africa	1996(WVS) 1998(DHS) 2001(HES-IPUMS) 2002(AFRO) 2006(AFRO) 2007(WVS)
Finland	2003(CSES) 2004(HES) 2005(WVS)	Sweden	2005(HES) 2006(WVS)
France	1999(WVS) 2002(CSES) 2006(WVS)	Taiwan	1995(WVS) 1996(CSES) 2004(CSES)
Gabon	2000(DHS)	Tajikistan	1996(HES-LSS)
Georgia	1996(WVS)	Tanzania	1993(HES-HRDS)
Germany	1999(WVS) 2004(HES) 2006(WVS)	Togo	1998(DHS)
Ghana	1993(DHS) 1998(DHS) 2003(DHS) 2008(DHS)	Turkey	1993(DHS) 2007(WVS)
Guatemala	1995(DHS) 1998(DHS) 2000(HES-ENCOVI) 2005(WVS) 2006(HES)	Uganda	1995(DHS) 2005(AFRO)
Guinea	1999(DHS) 2005(DHS)	UK	2004(HES)
Guyana	2005(DHS)	Ukraine	1996(WVS) 1998(CSES) 2006(WVS)
Hungary	2002(CSES)	United States	1996(CSES) 1997(HES) 2000(WVS) 2004(CSES) 2005(HES-IPUMS) 2006(WVS)
India	1995(WVS) 2001(WVS) 2006(WVS)	Uruguay	1996(WVS) 2006(WVS)
Iran	2007(WVS)	Uzbekistan	1996(DHS)
Ireland	1999(WVS)	Venezuela	1996(WVS) 2000(WVS)
Israel	1995(HES-IPUMS) 2005(HES)	Vietnam	1997(DHS) 2002(DHS) 2005(DHS)
Kazakhstan	1995(DHS) 1999(DHS)	Zambia	1996(DHS) 2001(DHS) 2003(AFRO) 2005(AFRO) 2007(WVS, DHS)
Kenya	1993(DHS) 1998(DHS) 2003(DHS, AFRO) 2005(AFRO) 2008(DHS)	Zimbabwe	2001(WVS) 2004(AFRO) 2005(AFRO)

Appendix B. Summary tables related to horizontal inequality measures

This section provides summary statistics relative to the horizontal inequality variables discussed in Section 3.1. Table B.1 provides basic descriptive statistics (mean, standard deviation, skewness and kurtosis) and shows that LINEQ2 is highly skewed and has very large kurtosis. On the other hand, HI(LN) is basically symmetric and with moderate kurtosis values. Table B.2 shows that the correlation between LINEQ2 and the HI measures is very small. Given the straightforward interpretation of the HI variables, these low correlations underline concerns that LINEQ2 may not be a good measure for tapping the underlying concept of horizontal inequality.

Table B.1. Summary statistics of group-level economic variables from EPR

	Mean	SD	Skewness	Kurtosis
g	0.364	0.505	3.02	14.02
LINEQ2	0.388	1.02	6.73	63.6
HI(ABS)	0.18	0.281	3.49	21.3
HI(LN)	-2.72	1.58	-0.43	3.3

Note. This table provides summary statistics of g (the group GDP per capita measured using G-Econ) and different measures of horizontal inequality. Data from the Growup Portal (EPR) has been employed to compute these measures.

Table B.2. Correlations of horizontal inequality variables created from EPR data

	LINEQ2	HI(ABS)	HI(LN)
LINEQ2	1		
HI(ABS)	0.202	1	
HI(LN)	0.128	0.724	1

Note. This table provides correlations among the different horizontal inequality measures. There are 821 observations in each correlation.

APPENDIX C. ADDITIONAL RESULTS

C.1. **Group inequality and conflict incidence and intensity: robustness checks.** This Appendix contain regressions aiming to examine the robustness of the results in Section 4. Table C.1 replicates Table 1 in the main text using INCIDENCE as dependent variable. Table C.2 is also similar to Table 1 but excludes all observations prior to the first non-missing observation of the survey-based WGI variable. Table C.3 also replicates columns 3–7 in Table 1, using an alternative measure of horizontal inequality (LINEQ2). Table C.4 checks whether the results are robust to omitted variable bias using the technique introduced by Oster (2016) while C.5 examines whether changes in WGI are correlated with conflict (to address reverse causality concerns).

Table C.1. Within group inequality and conflict incidence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
G^R	7.091**	6.471**	4.822**	4.873**	4.877**		
	(0.014)	(0.027)	(0.038)	(0.038)	(0.040)		
G^I						5.439**	
						(0.035)	
G							6.034**
							(0.018)
GROUP SIZE		-3.086	-2.263	-2.249	-2.311	-2.285	-2.293
		(0.158)	(0.355)	(0.357)	(0.349)	(0.345)	(0.345)
HI(LN)			-0.291	-0.290	-0.293	-0.293	-0.275
			(0.276)	(0.281)	(0.272)	(0.289)	(0.316)
GROUP GDP			-1.605*	-1.575*	-1.443*	-1.676**	-1.651**
			(0.058)	(0.062)	(0.099)	(0.048)	(0.049)
GROUP ELEV. (SD)			-0.001*	-0.001*	-0.001	-0.001	-0.001
			(0.086)	(0.087)	(0.130)	(0.111)	(0.126)
GIP			-0.743	-0.752	-0.763	-0.738	-0.710
			(0.168)	(0.154)	(0.149)	(0.160)	(0.162)
POP		-2.946	-1.160	-1.642	-0.383	-1.510	-1.545
		(0.139)	(0.633)	(0.517)	(0.891)	(0.553)	(0.543)
XPOLITY		-0.098*	-0.133*	-0.134*	-0.135**	-0.135*	-0.135*
		(0.075)	(0.056)	(0.057)	(0.035)	(0.056)	(0.056)
GDP		-0.436	1.019				
		(0.731)	(0.485)				
POVERTY2					0.050		
					(0.160)		
INCIDENCE(LAG)	4.722***	4.773***	4.716***	4.715***	4.730***	4.731***	4.722***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
CONSTANT	-8.272***	-0.637	-14.077	-4.367*	-5.332*	-5.379*	-4.597*
	(0.000)	(0.952)	(0.284)	(0.097)	(0.060)	(0.082)	(0.093)
R^2	0.556	0.585	0.630	0.629	0.631	0.628	0.629
Obs	1854	1741	1483	1483	1483	1483	1483

Note. The dependent variable is INCIDENCE. All models contain country and year dummies. Estimation is by maximum likelihood in a logit model. p-values based on robust standard errors clustered at the country level are in parentheses. $^{*}p < 10$, $^{**}p < .05$, $^{***}p < .01$.

Table C.2. Estimating models in Table 1 omitting all group-years that precede the first year for which survey data exists

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
G^R	7.991**	6.986**	4.444*	4.520*	4.621*		
	(0.040)	(0.048)	(0.083)	(0.072)	(0.066)		
G^I						4.699*	
						(0.053)	
G							5.574**
							(0.031)
GROUP SIZE		-3.471	-1.938	-1.917	-1.917	-1.920	-1.971
		(0.101)	(0.404)	(0.408)	(0.412)	(0.393)	(0.387)
HI(LN)			-0.300	-0.284	-0.286	-0.290	-0.275
			(0.287)	(0.328)	(0.322)	(0.324)	(0.348)
GROUP GDP			-0.878	-0.783	-0.622	-0.851	-0.845
			(0.303)	(0.325)	(0.431)	(0.281)	(0.283)
GROUP ELEV. (SD)			-0.000	-0.000	0.000	-0.000	0.000
			(0.894)	(0.924)	(0.811)	(0.997)	(0.922)
GIP			-0.941	-0.931	-0.964	-0.938	-0.895
			(0.237)	(0.235)	(0.213)	(0.234)	(0.242)
POP		0.156	4.569	2.896	3.822	2.739	2.794
		(0.976)	(0.431)	(0.602)	(0.522)	(0.621)	(0.615)
XPOLITY		-0.234***	-0.149***	-0.161***	-0.138**	-0.165***	-0.163***
		(0.000)	(0.006)	(0.006)	(0.025)	(0.006)	(0.006)
GDP		-0.431	2.831				
		(0.838)	(0.258)				
POVERTY2					0.068		
					(0.104)		
INTENSITY(LAG)	5.304***	5.559***	4.689***	4.696***	4.710***	4.711***	4.694***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
CONSTANT 1	28.610***	24.356	59.388**	33.838**	40.391**	33.340**	32.784**
	(0.000)	(0.191)	(0.011)	(0.012)	(0.015)	(0.014)	(0.015)
CONSTANT 2			65.796***	40.199***	46.773***	39.671***	39.142***
			(0.006)	(0.004)	(0.007)	(0.005)	(0.005)
R^2	0.748	0.767	0.742	0.741	0.741	0.740	0.741
Obs	4852	4709	3682	3682	3583	3682	3682

Note. The dependent variable is INTENSITY. All models contain country and year dummies. Estimation is by maximum likelihood in a conditional logit model. p-values based on robust standard errors clustered at the country level are in parentheses. $^{*}p < 10$, $^{**}p < .05$, $^{***}p < .01$.

Table C.3. Re-estimating models 3-7 from Table 1 but substituting LINEQ2 for HI(LN).

	(1)	(2)	(3)	(4)	(5)
G^R	5.067**	5.182**	5.198**		
	(0.035)	(0.034)	(0.036)		
G^I				5.794**	
				(0.033)	
G					6.812**
					(0.012)
LINEQ2	0.598	0.928	0.783	0.757	0.922
	(0.778)	(0.658)	(0.713)	(0.724)	(0.657)
GROUP GDP	-1.291*	-1.135*	-1.074	-1.288*	-1.211*
	(0.086)	(0.083)	(0.112)	(0.055)	(0.062)
GROUP SIZE	-1.550	-1.490	-1.540	-1.536	-1.557
	(0.510)	(0.526)	(0.513)	(0.503)	(0.502)
GROUP ELEV. (SD)	-0.001	-0.001	-0.001	-0.001	-0.001
	(0.141)	(0.154)	(0.215)	(0.196)	(0.231)
GIP	-0.857	-0.874	-0.879	-0.850	-0.809
	(0.171)	(0.157)	(0.154)	(0.170)	(0.170)
POP	-1.421	-1.893	-0.765	-1.770	-1.857
	(0.561)	(0.462)	(0.806)	(0.494)	(0.472)
XPOLITY	-0.128**	-0.129**	-0.128**	-0.130**	-0.130**
	(0.044)	(0.045)	(0.035)	(0.043)	(0.043)
GDP	1.913				
	(0.210)				
INTENSITY(LAG)	4.297***	4.296***	4.307***	4.310***	4.298***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
POVERTY2			0.036		
			(0.293)		
CONSTANT 1	36.973***	21.171***	26.352***	21.699***	21.563***
	(0.001)	(0.001)	(0.005)	(0.001)	(0.002)
CONSTANT 2	42.884***	27.064***	32.273***	27.568***	27.465***
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
R^2	0.704	0.704	0.702	0.703	0.704
Obs	4989	4989	4828	4989	4989

Note. The dependent variable is INTENSITY. All models contain country and year dummies. Estimation is by maximum likelihood in an ordered logit model. p-values based on robust standard errors clustered at the country level are in parentheses. $^{*}p < 10$, $^{**}p < .05$, $^{***}p < .01$.

Table C.4. Assessing the importance of omitted variable bias

	RESTRICTED MODEL I	RESTRICTED MODEL II	RESTRICTED MODEL III
$R_{max} = .8$	-1.12	2.01	1.36
$R_{max} = .9$	-0.93	1.62	1.51
$R_{max} = 1$	-0.80	1.36	1.35

Note. This table applies Oster (2016) technique to assess how strong the correlation between the unobservables and WGI, relative to the correlation of the observables and WGI, has to be in order to explain away the significance of WGI. Calculations have been performed using the software *psacalc* provided by the author. The full model corresponds to our baseline specification (column 4 in Table 1) and the restricted models contain no controls, country dummies and country and year fixed dummies for models I, II and III, respectively. We consider three different values of $R_{max} = \{1, 0.9, 0.8\}$, the maximum R^2 that could be attained in all the relevant controls would be introduced in the regression. See the main text and Oster (2016) for details.

Table C.5. Reverse causation: Civil conflict and changes in WGI

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
%WAR YRS	-0.014	0.029	0.022	0.079	0.009	0.045	0.157	0.083
	(0.814)	(0.687)	(0.932)	(0.786)	(0.785)	(0.511)	(0.555)	(0.744)
TOTAL YEARS	-0.001	-0.003	-0.000	0.001***	-0.009**	-0.020***	-0.009**	-0.016***
	(0.722)	(0.293)	(0.935)	(0.000)	(0.016)	(0.000)	(0.028)	(0.000)
LAST YEAR	-0.001	-0.011***	-0.001	-0.010***	0.014**	0.020***	0.014**	0.021***
	(0.858)	(0.000)	(0.810)	(0.000)	(0.015)	(0.000)	(0.018)	(0.000)
CONSTANT	1.856	21.726***	2.561	19.747***	-27.774**	-40.302***	-27.987**	-42.127***
	(0.858)	(0.000)	(0.811)	(0.000)	(0.015)	(0.000)	(0.019)	(0.000)
Obs	251	251	243	243	251	251	243	243

Note. The unit of observation is a group and the dependent variable in models 1-4 (5-8) is the change in G^R (G^I) between the first year and last year for which we have different surveys measuring group inequality. %WAR YRS is the proportion of years that the group is in conflict over the period for which we have data for the group. TOTAL YEARS is the number of year from the first year for which we have group data to the last year for which we have group data. LAST YEAR is the last year for which we have group data. Models 1,2, 5 and 6 use all data whereas models 3, 4, 7 and 8 exclude groups that were in conflict during the first year for which we have surveys. Models 3, 4, 7 and 8 include country fixed effects whereas the other models do not. Models are estimated using OLS, and p-values based on robust standard errors clustered at the country level are in parentheses. $^{*}p < 10$, $^{**}p < .05$, $^{***}p < .01$.

C.2. **Group inequality and conflict onset : robustness checks.** Table C.6 replicates Table 4 in the main text, using nighlight emissions data to compute the horizontal inequality variables. It shows no relation between any of these variables and conflict onset. Table C.7 replicates some of the regressions in Cederman, Weidmann and Bormann (2015) and examines whether their results are robust to including country fixed effects. Columns 1 to 4 (4 to 7) in Table C.7 replicate Table II (Table III) in Cederman, Weidmann and Bormann (2015). This table shows that when country fixed effects are introduced in the regression only the variable LOW is robustly associated to conflict onset. However, HIGH is in general insignificant, which again cast doubt on the link between HI and conflict onset.

Table C.6. Horizontal inequality and conflict onset using nightlights data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
LINEQ(NIGHT)	-0.069	-0.037	-0.114	-0.114	-0.064		
	(0.185)	(0.481)	(0.295)	(0.278)	(0.303)		
HI(ABS)(NIGHT)						-8.946	
						(0.186)	
HI(LN)(NIGHT)							-0.092
							(0.428)
EXCLUDED GROUP	0.768**	0.774**	0.619	0.523	0.638*	0.821**	0.806**
	(0.044)	(0.048)	(0.251)	(0.358)	(0.094)	(0.032)	(0.035)
POW. BALANCE	-0.555	1.617	-2.895	-3.323	1.175	1.667	1.700
	(0.849)	(0.571)	(0.426)	(0.399)	(0.674)	(0.550)	(0.542)
POW. BALANCE^2	-0.680	-2.749	1.054	1.453	-2.412	-2.863	-2.815
	(0.888)	(0.532)	(0.823)	(0.769)	(0.573)	(0.500)	(0.510)
GDP	0.557		-0.095	-0.249			
	(0.591)		(0.938)	(0.843)			
GDP(GECON)		0.550			0.529	0.576	0.570
		(0.346)			(0.350)	(0.331)	(0.339)
N. EXCL. GROUPS	0.066	0.036	0.071	0.145	0.052	0.033	0.030
	(0.530)	(0.744)	(0.587)	(0.486)	(0.633)	(0.768)	(0.790)
YEAR	-0.058	-0.055	-0.064	-0.057	-0.058	-0.054	-0.054
	(0.360)	(0.105)	(0.325)	(0.385)	(0.100)	(0.120)	(0.118)
PEACEYRS	-0.346	-0.337*	-0.479*	-0.512**	-0.308*	-0.332*	-0.333*
	(0.153)	(0.063)	(0.061)	(0.049)	(0.092)	(0.067)	(0.064)
SPLINE 1	0.013	0.013	0.022	0.024	0.011	0.013	0.013
	(0.470)	(0.330)	(0.243)	(0.200)	(0.398)	(0.339)	(0.338)
SPLINE 2	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.629)	(0.508)	(0.364)	(0.308)	(0.575)	(0.514)	(0.519)
SPLINE 3	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.730)	(0.615)	(0.442)	(0.380)	(0.673)	(0.617)	(0.626)
R^2	0.187	0.151	0.198	0.195	0.133	0.153	0.152
Obs	3454	4094	1964	1960	3854	4094	4094

Note. Dependent variable is INCIDENCE. All models contain country fixed effects. Estimation has been carried out by maximum likelihood in a logit specification. Robust standard errors clustered at the country level have been computed. p-values are in parentheses. $^{*}p < 10, ^{**}p < .05, ^{***}p < .01$.

Table C.7. Horizontal Inequality and conflict onset: robustness to country fixed effects

Low (G-Econ)	1.385***						
	(0.000)						
High (G-Econ)	0.933						
	(0.510)						
Low (nightlights)		0.183**					
		(0.038)					
High (nightlights)		0.198					
		(0.498)					
Low (50/50 mix)			0.810***				
			(0.000)				
High (50/50 mix)			0.654				
			(0.300)				
Low (spatial only, nuanced mix)				0.538***	0.538***		
				(0.006)	(0.006)		
High (spatial only, nuanced mix)				0.647	0.647		
				(0.237)	(0.237)		
Low (average spatial/surveys)						0.647***	
						(0.002)	
High (average spatial/surveys)						1.252*	
						(0.058)	
Low (ovlp-wted avrg spatial/svys)							0.603***
							(0.002)
High (ovlp-wted avrg spatial/svys)							0.866
							(0.117)
Excluded	1.495*	1.195	1.255*	1.338*	1.338*	0.993	0.946
	(0.071)	(0.106)	(0.085)	(0.078)	(0.078)	(0.121)	(0.139)
Downgraded	1.795***	1.901***	1.870***	1.881***	1.881***	1.651***	1.615***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Relative group size	-1.196	-1.777*	-1.467	-1.528	-1.528	-1.520	-1.666
	(0.247)	(0.073)	(0.139)	(0.147)	(0.147)	(0.139)	(0.101)
Postwar	-0.115	-0.235	-0.210	-0.202	-0.202	-0.121	-0.093
	(0.721)	(0.491)	(0.556)	(0.561)	(0.561)	(0.669)	(0.732)
Ongoing conflict	-1.096**	-1.078**	-1.151**	-1.148**	-1.148**	-1.056***	-1.054***
	(0.023)	(0.025)	(0.014)	(0.015)	(0.015)	(0.007)	(0.007)
GDP (log, t-1)	-2.140**	-2.085**	-2.060**	-2.011**	-2.011**	-1.255	-1.236
	(0.018)	(0.028)	(0.033)	(0.038)	(0.038)	(0.206)	(0.221)
Population (log)	-2.742	-2.621	-2.675	-2.610	-2.610	-2.340	-2.454
	(0.150)	(0.154)	(0.147)	(0.149)	(0.149)	(0.204)	(0.179)
Peace years	0.151	0.126	0.144	0.132	0.132	0.087	0.085
	(0.257)	(0.293)	(0.246)	(0.264)	(0.264)	(0.351)	(0.366)
c	43.316*	45.189*	43.633*	43.067*	43.067*	33.020	34.802
	(0.075)	(0.052)	(0.065)	(0.064)	(0.064)	(0.162)	(0.135)
R^2	0.238	0.215	0.233	0.229	0.229	0.191	0.190
Obs	1966	1808	1966	1966	1966	2311	2311

Note. The dependent variable is ONSET. This table replicates Tables II and III in Cederman, Weidmann and Bormann (2015) including country fixed effects in regressions that are otherwise identical are those in that paper. See Cederman, Weidmann and Bormann (2015) for details and precise definitions of the Low and High variables. Estimation is by maximum likelihood in a logit model. p-values based on robust standard errors clustered at the country level are in parentheses. $^{*}p < 10$, $^{**}p < .05$, $^{***}p < .01$.

APPENDIX D. VARIABLE DEFINITIONS AND SUMMARY STATISTICS

This section provides detailed definitions for the variables employed in the empirical analysis as well as a table of summary statistics.

D.1. **Variable definition.**

Conflict variables.

INCIDENCE: "Group level Armed conflict". A binary measure taking a value of 1 for those years where an ethnic group is involved in armed conflict against the state resulting in more than 25 battle-related deaths. Ethnic groups are coded as engaged in conflict if a rebel organisation involved in the conflict expresses its political aims in the name of the group and a significant number of members of the group participate in the conflict. Source: Wucherpfennig et al. (2012).

INTENSITY: "Group level Conflict intensity". We assign a value of 0 if group G is at peace in a given year, a value of 1 if there are events satisfying CONFLICT25$_G$ and the total number of battle deaths that year does not exceed 1000, and a value of 2 if the number of battle deaths is larger than 1000. Source: Wucherpfennig et al. (2012).

ONSET: "Group level Conflict Onset". A binary measure reflecting the first year in which a group enters a conflict, as defined in CONFLICT25$_G$ above.

Inequality measures.

G^R: Group Gini coefficient, computed using survey data and adjusted using the Ratio approach, as described in Section 3.3 and Appendix A.1.

G^I: Group Gini coefficient, computed using survey data and adjusted using the Intercept approach, as described in Section 3.3 and Appendix A.1.

G^U: Group Gini coefficient, computed using survey data, unadjusted.

IGI: Group-level Gini coefficient computed using nighlight emissions. Source: KW.

LINEQ2: CWG measure of horizontal inequality, defined as $log\frac{(}{g}G)^2$, where g is group's GDP per capita and G is the (unweighted) average of GDP per capita of all groups. Source: Growup portal, https://growup.ethz.ch/

HI(ABS): Group-level measure of horizontal inequality defined as HI(ABS) $= |g - \bar{G}|$, where g is group's GDP per capita and $\bar{G}$ is the weighted average (by group size) of the GDP per capita for all groups *other than* g. Source: Growup portal, https://growup.ethz.ch/.

HI(LN): natural log of HI(ABS).

Controls.

GDP: log of real GDP per capita, lagged one year. The source is the Penn World Tables (2015).

POP: log of the population in millions, lagged one year, as reported by the Penn World Tables (2015).

XPOLITY: democracy score based on Polity IV, lagged one year. It combines 3 out of the 5 components of Polity IV (XCONST, XRCOMP, XROPEN) and leaves out the two components (PARCOMP and PARREG) that are related to political violence, and hence are likely to be endogeneous. It ranges from -6 (maximum level of autocracy) to 7 (maximum level of democracy). See Vreeland (2008) for details.

GROUP GDP: Group GDP per capita, lagged one year. Source: G-Econ (through the GrowUp portal, urlhttps://growup.ethz.ch/).

POVERTY2: Percentage of the total population with income lower than 2 dollars a day. Source: World Bank.

GIP: dummy variable indicating whether the group has access to power. Source: GrowUp portal, https://growup.ethz.ch/.

GROUP ELEV. (SD): the standard deviation of the elevation of the ethnic homeland. Source: GrowUp portal, https://growup.ethz.ch/.

POW. BALANCE. Demographic power balance between the group and the group(s) in power. Denoting the populations of the group and the group(s) in power as s and S, respectively, the power balance is defined as s/(s+S) if the group is excluded, and as s/S otherwise.

EXCLUDED: dummy variable indicating whether the group is excluded to power (i.e., it is defined as 1-GIP. Source: GrowUp portal, urlhttps://growup.ethz.ch/

D.2. **Summary statistics.** Tables D.8 and D.9 provide summary statistics for all the variables employed in the empirical analysis. Since the sample size reduces considerably when survey-based measures are considered, we provide summary statistics considering the full (EPR) sample (Table D.9) as well as the reduced (survey-based) sample (Table D.8).

Table D.8.

variable	Obs.	mean	sd	min	max
INCIDENCE	7070.00	0.03	0.17	0.00	1.00
INTENSITY	7070.00	0.03	0.20	0.00	2.00
ONSET	6816.00	0.00	0.07	0.00	1.00
G^R	8474.00	0.38	0.12	0.05	0.95
G^I	8474.00	0.45	0.09	0.05	0.78
LINEQ2	5654.00	0.20	1.41	0.00	33.01
HI(LN)	5548.00	-2.89	1.65	-9.46	0.92
GROUP GDP	5707.00	-1.41	1.28	-11.21	1.75
GROUP ELEV. (SD)	5849.00	315.34	265.16	5.77	1617.65
GIP	6539.00	0.64	0.48	0.00	1.00
POVERTY2	8090.00	45.63	34.80	0.00	99.45
POP	6895.00	2.89	1.46	-0.52	7.10
XPOLITY	7862.00	2.83	4.02	-5.00	7.00

Notes. This table presents summary statistics of the variables considered in Section 4 in the main text (survey sample).

Table D.9.

variable	**N**	**mean**	**sd**	**min**	**max**
ONSET	17102.00	0.01	0.08	0.00	1.00
INCIDENCE	17964.00	0.04	0.19	0.00	1.00
IGI	12349.00	0.71	0.22	0.04	1.00
LINEQ2	12983.00	0.35	1.24	0.00	33.01
HI(LN)	12786.00	-2.53	1.66	-9.46	3.02
GROUP ELEV. (SD)	13274.00	338.90	306.36	5.77	1925.56
GIP	15348.00	0.57	0.49	0.00	1.00
POW. BALANCE	15348.00	0.24	0.32	0.00	1.00
GDP	15830.00	8.42	1.16	5.23	11.72
GROUP GDP	12983.00	-1.32	1.30	-11.21	3.10
EXCLUDED GROUP	17706.00	7.60	11.71	0.00	55.00
POP	15824.00	2.96	2.01	-3.20	7.19
XPOLITY	14620.00	2.22	4.34	-6.00	7.00

Notes. This table presents summary statistics of the variables considered in Section 5 in the main text (EPR sample).