Unequal We Fight: The Impact of Economic Inequality Within Ethnic Groups on Conflict Initiation

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Abstract

Rebel mobilization is a crucial component of conflict initiation, but the existing civil war literature has not given adequate attention to this element in an ethnic group’s decision to fight. In this paper we look at the mobilization process by considering the impact of intra-group economic inequality on conflict. We argue that an ethnic group’s economic inequality is negatively related to its mobilization costs and therefore increases the likelihood that the group initiates a conflict. Confronted by the collective action problem inherent to rebel recruitment, rebel leaders in highly unequal ethnic groups face lower costs in creating the necessary material and/or purposive incentives to overcome freeriding and form a potent rebel movement. To assess our claim empirically, we developed a new measure of intra-group economic inequality by combining high resolution satellite images of light emissions, spatial population data, and geocoded ethnic settlement areas. After validating our measure against alternative measures of economic inequality we include it in a statistical model of conflict initiation and find considerable for our theoretical prediction: greater economic inequality within a group significantly increases the risk of conflict, especially if the group is excluded from political power and above or below the country average in terms of per-capita income.
1 Introduction

Civil war requires the mobilization of rebel forces. Why citizens choose to leave their homes, abandoning their families and livelihoods to join armed rebellion—an activity associated with potentially enormous risks to personal safety—has long been a central question in the civil war literature. Moreover, participation and rebel recruitment often serve as key causal mechanisms linking explanatory factors to dimensions of conflict. Unfortunately, however, these processes are often formulated in vague terms and are seldom theoretically developed. In the early civil war literature, participation was the self-evident mechanism connecting various independent variables, such as economic inequality, to conflict (e.g., Russett 1964; Huntington 1968; Gurr 1971; Paige 1975). Since then little has changed. Among today’s most frequently cited formal models of civil war onset (e.g., Fearon 1993; Powell 2006) mobilization processes remain excluded and most empirical research is plagued by problems of equifinality, as multiple distinct causal mechanisms are posited to explain the observed correlations. Take GDP per capita, for example. While Fearon and Laitin (2003) argue that low levels of economic development is a proxy for state weakness and is therefore positively related to conflict, Collier and Hoeffler (2004) explain the same relationship in terms of an individual’s opportunity costs of joining armed rebellion.

This paper studies the impact of rebel mobilization on conflict initiation. It does so by focusing on economic inequality within ethnic groups, a group-specific structural factor linked directly to participation and rebel recruitment. We argue that an increase of intra-group economic inequality loosens the rebel’s mobilization constraint and thus increases the risk of conflict. As within-group inequality increases, the proportion of poor citizens rises and/or the income of the poor decreases. This expands the pool of potential recruits and/or reduces the opportunity costs of the poor to enlist as fighters, allowing rebel leaders to recruit more fighters per dollar, which increases the rebels probability of victory. Hence, all else equal, higher levels of within-group economic inequality is positively associated with conflict.

The formation of a rebel group, however, is only a necessary and not a sufficient condition for conflict initiation. We argue that ethnic elites will only engage in the costly action of forming a rebel group and initiate a conflict if they are sufficiently motivated and see no alternative way of achieving their goal. As previous research has shown, groups with strong economic motives (wealthier or
poorer than the average national group) and political motives (excluded from political power) have greater incentives to form rebel organizations and initiate conflict (Cederman, Weidmann and Gleditsch, 2011). Therefore, we expect the positive association between intra-group economic inequality and conflict initiation to be significantly larger if an ethnic group is excluded from power and/or significantly better or worse off economically than the average ethnic group in that country.

In order to empirically evaluate our predictions, we develop a spatially flexible measurement approach of economic inequality. Following existing economic research that uses night lights as a proxy of wealth (e.g., Henderson, Storeygard and Weil, 2011), we combine geocoded data on ethnic settlement areas with high resolution satellite imagery of night lights and population estimates to create a proxy of within-group inequality. We validate our measurement approach against existing measures of economic inequality at the country-level and with geocoded survey data at the group-level for Sub-Saharan Africa. We then add our proxy of intra-group economic inequality to an existing statistical model of conflict initiation and find considerable support for our theoretical predictions.

The paper offers several important insights for conflict research. It stresses the importance to distinguish between different types of economic inequality. By distinguishing explicitly between inter- and intra-group inequality, we not only show that these different types of economic inequality contribute to conflict initiation in different ways (i.e., intra-group inequality affects rebel mobilization, inter-group inequality affects rebel motivation), but also that they interact in complex ways. This may explain why previous quantitative studies, using some overall measure of economic inequality, have failed to find significant correlations. Moreover, it provides a possible explanation for why price and trade shocks may not have a consistent effect on conflict initiation (Bazzi and Blattman, 2011). Our findings suggest that the effect of price and trade shocks will, among other things, depend on how they affect economic inequality both between and within ethnic groups.

The remainder of the paper is organized as follows. The next section reviews and situates the paper within the previous literature on economic inequality and conflict. Thereafter we link within-group economic inequality to rebel mobilization and develop our theory of conflict initiation. The fourth section presents our measurement approach and evaluates our measure of economic inequality against a variety of existing measures. Section five discusses the data and our empirical research design, before we present and discuss our findings. Finally, we conclude and discuss some avenues
for future research.

2 Inequality and Conflict

The connection between economic inequality and violence has long been at the center of conflict studies. Grounded theoretically in the writings by Plato, Marx, de Tocqueville, and others, the proponents of this relationship claim that nations with an unequal distribution of wealth are more likely to experience political violence of various forms. The key concept linking inequality to conflict is relative deprivation, which captures the extent to which people's expectations about what they should be achieving exceeds their actual levels of achievement. Relative deprivation leads to frustration and aggression, which motivate individuals to participate in rebellion (Davies, 1962). The greater the level of relative deprivation, the stronger the motivational base for political violence, and the greater the magnitude of violence (Gurr, 1971, 9).

Decades of subsequent empirical research on the economic inequality-political conflict nexus has led to mixed results. While political and economic grievances have been repeatedly linked to conflict processes in the qualitative literature on civil wars (e.g., Gurr, 1971; Davies, 1997; Sambanis, 2005), the plethora of quantitative studies (over 43 according to Lichbach (1989)) found no robust relationship between economic inequality and conflict (Midlarsky, 1988; Lichbach, 1989). In fact, today's most influential quantitative studies of civil war outright reject grievance-based explanations, based on the lack of a statistically significant correlation between country-level measures of individual wealth distributions and conflict (e.g., Fearon and Laitin, 2003; Collier and Hoeffler, 2004). Most recently, however, several quantitative studies have emerged, showing that the previous quantitative literature's failure to detect a robust connection between inequality and conflict is due to inappropriate conceptualization and measurement problems rather than the lack of a relationship. Cederman, Wimmer and Min (2010), for example, argue that conflict processes between ethnic groups should not be studied at the country- but at the group-level and show empirically that politically relevant ethnic groups excluded from power are significantly more likely to initiate a conflict. Moreover, Østby (2008), Stewart (2009), and Cederman, Weidmann and Gleditsch (2011) all provide robust quantitative evidence that horizontal economic inequality between ethnic groups, as compared to the previously used vertical measure of inequality between individuals, significantly
increase the risk of conflict.

We build upon this recent line of research, but focus on the impact of *intra-* rather than *inter-*group economic inequality on conflict. Although rebel mobilization is at the center of grievance-based arguments, its theoretical treatment is often vague. Even the most recent empirical studies within this line of reasoning remain largely implicit when it comes to participation and rebel recruitment strategies. In this paper we argue that economic inequality within ethnic groups contributes to conflict by loosening the constraints rebel leaders face when forming a rebel organization. We therefore explicitly focus on the rebel mobilization process. Moreover, by conceptualizing an ethnic group’s relative inequality vis-à-vis other ethnic groups as a motivational rather than a mobilizational factor, we are able to show how the different types of economic inequality interact in the production of conflict.

To date little research on within-group economic inequality and conflict exists. The sparse research that does exist provides two opposing views on the association of intra-group inequality and conflict. The first focuses on identity formation as the basis for group mobilization, arguing that mobilization requires a common identity and a collective unifying structure among its members (Tilly 1978, 84). A high level of economic inequality within an ethnic group may create resentment, which undermines cohesiveness and the group’s ability to take collective action, as the different social classes within the group identify more closely with their counterparts in other ethnic groups rather than identifying with the member of different social classes within their own group (Stewart 2000). Related to this, Sambanis and Milanovic (2011) argue that intra-regional inequality will undermine mobilization, since any income gains from victory will have to be distributed among the population. Thus, an increase in economic inequality within an ethnic group should reduce the likelihood that this group initiates a conflict. The exact opposite view has come out of economic theories of rebel mobilization and conflict. Esteban and Ray (2011) propose a model of ethnic conflict, in which discriminatory government policy or social intolerance are responsible for various forms of ethnic activism, including violence. Mobilization is considered to be costly and militants need to be compensated accordingly. Allowing for both financial and human contributions to conflict they show how an increase in within-group inequality weakens the rebel’s mobilization constraint by reducing the opportunity costs and thereby the compensation rate of fighting. Thus, rather than undermining group cohesion and mobilization, these models suggest that the emer-
gence of economic and cultural elites are a crucial explanatory factor regarding conflict initiation. Similarly, Gates (2002) models the costs of recruitment and allegiance for rebel groups as a function of the outside options of payoffs for recruits. As intra-group inequality increases, outside payoffs decrease, which reduces the rebel’s recruitment costs.

Our paper may be seen as the first empirical evaluation of these opposing views at the ethnic group-level. We argue along the lines of the second approach, but take a broader view with regard to the impact of intra-group inequality on rebel mobilization. We argue that within-group inequality does not only lower the opportunity costs for voluntary participation, but also lowers the costs associated with forceful recruitment, abduction, and indoctrination. Our empirical results indicate that there is a positive robust association between intra-group inequality and conflict initiation, providing support for the second view.

Empirical research on the relationship between within-group inequality and conflict is equally rare. In a recent working paper, Huber and Mayoral (2012) look at the impact of different forms of economic inequality on conflict. Using over 200 individual-level surveys from 89 countries they decompose general inequality into between-group inequality, within-group inequality, and overlap at the country-level (see Pyatt [1976] on the decomposition of the Gini index). Consistent with grievance-based arguments, they find a positive relationship between inter-group inequality and conflict, but this finding rarely surpasses conventional levels of statistical significance. In line with the second theoretical view above, they find a strong, robust positive association between within-group inequality and civil war. Although their finding with regard to within-group inequality is qualitatively similar to ours, our empirical approach differs in two important aspects. First, rather than country-year, our level of analysis is a group-year, which follows the conceptual innovation of previous research (e.g., Cederman, Wimmer and Min [2010]) and we believe is the most appropriate to study the impact of within-group inequality on conflict initiation. Second, by using a country-year as their unit of observation, Huber and Mayoral (2012) are forced to create an aggregate measure of within-group inequality across all ethnic groups of a country, risking aggregation bias (Signorino and Xiang, 2011). By performing a group-level analysis we are able to avoid this risk, include a direct measure of an ethnic group’s economic inequality, and thereby get a tighter match between theory and empirics. Finally, our empirical analysis suggests that the effect of within-group inequality on conflict initiation is conditional on the group’s political and economic status
relative to other groups in a country. In their analysis, Huber and Mayoral (2012) only consider unconditional effects of the various Gini components, missing relevant interaction effects among the different types of economic inequality.

More closely related to our empirical research design are Østby, Nordás and Rød (2009). They study the impact of socioeconomic inequalities between and within subnational regions in 22 Sub-Saharan African countries. By combining geocoded responses to Demographic and Health Surveys (DHS) of the US State Agency of Interantional Development (USAID) with geographical data on the location of conflict zones for 1986-2004, they find that conflict onset is more likely in regions with strong relative deprivation and strong intraregional inequalities. While we agree with their disaggregate empirical approach, some aspects remain problematic. First, they proxy ethnicity by region. While subnational regions may be a good proxy for the primary settlement area of some groups in some countries, it is far from ideal for others. Moreover, the number of ethnic groups in a country is rarely identical to the number of subnational regions. By using geocoded ethnic settlement areas our research design avoids this heroic assumption. Second, they assess whether a correlation exists between the level of inequality among survey respondents in a subnational region and the risk of conflict in that region. We do not limit our research design in that way. Our dichotomous dependent variable takes the value one if a rebel group initiated a conflict against the state on behalf of a certain ethnicity, independent from the location of both the ethnic group’s settlement region and the conflict zone. Finally, they ignore interactions among the different types of economic inequality. As noted above, our theoretical argument clearly indicates a conditional relationship between inter- and intra-group economic inequality and we find considerable empirical evidence supporting it.

3 Intra-Group Inequality, Rebel Mobilization, and Conflict Initiation

In this section we describe the link between economic inequality within ethnic groups and conflict initiation. We argue within-group inequality affects conflict initiation via rebel mobilization: as intra-group inequality increases, a rebel leader’s costs to overcome the collective action problem inherent in forming a powerful rebel organization. After discussing through which causal mecha-
isms intra-group inequality reduces rebel mobilization costs, we integrate this relationship into a 
more general theoretical framework of conflict initiation.

Rebel mobilization is generally discussed within the collective action framework (Lichbach, 1998). This theoretical framework treats conflict victory as a public good, which provides individuals with strong incentives to freeride (i.e., abstain from participation), since anyone can partake in the spoils of victory, regardless of whether they fought with the rebels or not. The solution to the freerider problem within the rational choice framework are selective incentives, which are gains that occur exclusively to those individuals taking part in the rebellion. Conceptually, the literature distinguishes between three types of selective incentives: material, social, and purposive. Since we focus on ethnic conflict and our unit of analysis is an ethnic group, we ignore social incentives as they are present within all groups and therefore cannot explain the variation in conflict initiation among ethnic groups. Throughout the subsequent discussion we follow Balcells and Rohner (2008) in assuming that a successful insurgency requires a certain number of rebels. We now proceed to show how within-group inequality affects the provision of material and purposive incentives.

Material Incentives

Of the different selective incentives, material incentives have received the most attention. Several economic models have been proposed, treating participation in a rebel organization as one employment option in the labor market. In these models rebel recruitment is therefore in competition with other employment options over labor. Gates (2002), for example, sets the cost per fighter in direct relationship to expected wages on the regular labor market and Grossman (1991) stresses the private returns to insurgents from fighting, such as booty taken in a successful insurrection. In a recent model, Esteban and Ray (2011) study rebel mobilization in an ethnic group with income heterogeneity. Allowing for both capital and labor contributions, they show that rich individuals

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1 An alternative view is that rebel mobilization is not a collective action problem as in Olson (1965), but a coordination problem as in Schelling (1980). Although important, we believe that this subtle theoretical difference does not affect our following theoretical discussion.

2 There are exceptions to the view that participation in rebel groups is subject to the freerider problem. Kalyvas and Kocher (2007), for example, argue that a crucial flaw of applying the collective action framework to rebel mobilization is that it assumes non-participation to be costless. But because civilians are often victimized in conflicts, joining the rebels may be one way to minimize potential costs of being harmed. We do not discuss this view explicitly, but believe that a positive relationship between intra-group inequality and rebel mobilization exists here as well. Since the livelihood of poor citizens is at greater risk during war, a high level of intra-group inequality increases the number of potential recruits, which reduces mobilization costs.
will opt to pay rather than serve, since their income from regular labor exceeds the offered compensation rate for fighting, which rebel leaders set to maximize their probability of victory. Poor citizens, however, will choose to fight rather than pay, since their wage from their labor. In all these models, an increase in economic inequality within an ethnic group reduces the costs of rebel mobilization. As intra-group inequality increases the number of individuals that are unemployed and/or work in low income jobs increases, lowering their opportunity costs and thereby increasing their likelihood of joining the rebels at a given compensation rate. This allows rebel leaders to recruit more fighters and thereby increase their chance of winning at a given compensation rate or hire the same amount of rebels at lower cost.

The mobilization strategies of Hutu rebels during Burundi’s civil war 1993-2005 nicely illustrate this causal mechanism. Burundi is poor and densely populated, with over four-fifths of the population engaged in subsistence agriculture. The small modern sector is largely based on the export of the cash crops coffee and tea. Society consists of two major ethnic groups: the Hutu, who constitute an estimated 85% of the population, and the Tutsi, who dominate educated society, historically held power, and still control the armed forces [Ngaruko and Nkurunziza, 2000]. The assassination of Burundi’s first democratically elected Hutu president, Melchior Ndadaye, on October 21, 1993, by Tutsi extremists within the army caused violence to erupt between the two ethnic groups, resulting in the death of 50,000 to 100,000 people within a year. The massacre was followed by an ethnic civil war, which lasted until 2005. Since then several studies have emerged that provide strong empirical evidence for the importance of material incentives in the recruitment strategies of the Hutu rebel movements. In a pilot survey of 350 ordinary civilian and “rank-and-file” former civil war combatants, Mvukiyehe, Samii and Taylor (2006) found that the vast majority (75%) of combatants participated freely, either seeking contact to the rebels themselves or being recruited in the village. The vast majority of them also expected some form of monetary benefit from participation, either in cash or kind. An empirically more sophisticated account of the opportunity cost mechanism in the rebel recruitment process is provided by Nillesen and Verwimp (2010). Instrumenting income short falls by insufficient rain, they provide strong empirical evidence that villages with above mean incidents of insufficient rain were significantly more likely to have experienced recruitment activities than others. They find similar results using recall information on recruitment in a 13-year panel data set. Moreover, in a related paper Verwimp and Bundervoet...
find that joining an armed rebel group was a lucrative decision: households of which at least one member joined an armed group experienced a 41% higher growth in welfare in the final years of the war (1999-2007).

So far we have focused on positive material incentives. Yet, as several studies — in particular with regard to child soldiers — point out, participation in rebellion might be coerced, that is non-voluntary and the result of negative incentives (e.g., Humphreys and Weinstein 2008, Andvig and Gates 2009, Beber and Blattman 2010). Recruiting individuals by force is not cheap. Aside from the costs of recruitment and retention, mobilization by force creates a particular third set of costs associated with shirking. Since they have not voluntarily joined the rebels, their willingness to fight is likely to be low, which limits the rebels performance on the battle field. Moreover, forcing citizens to join them at gun point and abducting their children carries high reputation costs with civil society. For all these reasons mobilization by force is likely to be rare, especially at the initial stage of conflict, and more likely to be used by resource-poor rebel groups (Eck 2008). In order to minimize the costs associated with coercive recruitment, rebels tend to employ this mobilization strategy at night in rural areas or slums outside of urban centers, where the state is weak. Their ideal target is young, male, and poor, since they yield decent fighters and are relatively easy to control and manipulate. Poor citizens are less able to protect themselves or their loved ones from threats and abduction, they have less to gain from trying to escape and are thus easier to retain, and they are often less educated and therefore easier to persuade and manipulate. Examining the characteristics of non-combatants against voluntary and abducted participants in the Revolutionary United Front (RUF) in the Sierra Leon civil war (1991-2002), Humphreys and Weinstein (2008) provide some support for this conjecture. RUF abductees are significantly less educated, felt significantly less safe inside the rebel group, and were significantly less likely to have friends that were RUF members when they joined than RUF volunteers and non-combatants. Moreover, their results suggest that RUF volunteers were significantly older than RUF abductees or non-combatants, which supports the view that age is a target variable in recruitment by force. Finally, they find evidence that both RUF volunteers and abductees are significantly poorer than non-combatants, but they find no significant difference in wealth between RUF volunteers and abductees, which is consistent with recruitment through a combination of positive and negative material incentives.
How does an increase in intra-group economic inequality affect rebel mobilization by force? Although we do not believe that negative material incentives play a major role in the initial stage of rebel mobilization, we believe that an increase in within-group inequality weakens the constraints rebels face in recruitment by force. As within-group economic inequality increases, the proportion of poor increases, which expands the rebel’s reservoir of potential recruits. A larger candidate pool makes recruitment easier, reducing rebel mobilization costs. Macours (2010) provides some evidence in support of the positive link between within-group inequality and violent recruitment. By combining newspaper accounts of abductions with survey-based measures of district-level economic inequality, she finds that Maoist insurgents in the Nepalese civil war appear to have targeted the districts with the fastest recent growth in income inequality for recruitment. Thus, as with positive material incentive, although possibly to a smaller degree, we expect a positive association between within-group inequality and rebel mobilization through the use of force.

**Purposive Incentives**

Within the collective action framework, scholars have also discussed so-called purposive incentives, which are usually conceptualized as internalized norms and values in which the person’s self-esteem depends on doing the right thing (Oliver, 1993). One way to think of this type of incentive in a rational choice framework is a “good” of inherent value that individuals consume by performing some action, such as fighting. Such an approach is closely related to a branch of the voting literature that suggests the collective action problem inherent in electoral participation of established democracies is overcome by the value some individuals place on the act of voting itself (e.g., Dhillon and Peralta, 2002; Feddersen, 2004). To date, this type of incentives has received relatively little attention in the conflict literature. Prominent exceptions are Scott (1976) and Wood (2003), who argue that moral outrage led people to rebel against relative deprivation during economic modernization in South Asia and government abuses in El Salvador, respectively. In their view, emotional and moral motives were essential to the emergence and consolidation of collective action. Peasants joined the rebellion not for material or social benefits, but “because they took pride, indeed pleasure, in the successful assertion of their interests and identity” (Wood, 2003, 18), something Wood (2003) calls ‘pleasure of agency’.

This ‘pleasure of agency’ may pre-exist, but can also be created by the rebel leadership through
education and communication. [Wood (2003)] points out that many of the peasants fighting in the rebellion had previously participated in a social movement calling for economic reform and political inclusion, through which they had become convinced that social justice was God’s will and that to acting righteously was to participate in the rebellion. In a different study, [Eck (2012)] argues that one of the key strategies used by the Communist Party of Nepal-Maoist (CPN-M) was indoctrination. Based on a series of interviews with rebel leaders, foot soldiers, academics, as well as members of civil society and the international community in Nepal, she concludes that recruitment through propaganda and indoctrination was the main form of initial mobilization. The Maoists spent up to a year prior to the onset of conflict sending so-called political-cultural teams into villages to educate the masses on the aims of the Maoists and the necessity of using armed force in exacting political change. The teams used various forms of propaganda, ranging from mass meetings, cultural campaigns, and political classes, to the distribution of pamphlets, posters, and newspapers. The Maoist campaign had a powerful effect on rural villagers, who were not accustomed to being addressed with respect by individuals in positions of power. By addressing the villagers, discussing their problems, showing how the CPN-M’s agenda may support their goals, and requesting their assistance, the Maoist’s encouraged the villagers to be active political agents, a radical departure from the villagers’ previous experiences of marginalization. The Maoists’ ideology was especially appealing to rural individuals since it matched well with their own local agendas and grievances, providing them with the emotional and moral motives to join the rebellion.

As in the case of material incentives, an increase in intra-group economic inequality is associated with a decrease in the rebel’s cost of mobilization via purposive incentives. Providing the emotional and moral motives for an individual to fight on behalf of a rebel group, requires an extensive propaganda and educational effort of the rebel leadership. Whether indoctrination and propaganda work and if so how much effort it takes in creating a ‘pleasure of agency’ in support of the rebel movement, depends on an individual’s education level. All else equal, less educated citizens are easier to indoctrinate than highly educated individuals. Educational inequality is highly correlated with economic inequality, especially in developing countries where compulsory education does not exist. Hence, the greater within-group inequality is, the higher educational inequality, raising the number of citizens with no or very little formal education. This increases the effectiveness of propaganda and indoctrination, which reduces the rebels’ mobilization costs.
Hypotheses

Above we have argued that intra-group economic inequality reduces the rebel’s costs of mobilizing a sufficiently large number of fighters to win a conflict against the state. Thus, our first hypothesis is the following:

**Hypothesis 1:** There is a positive association between an ethnic group’s level of economic inequality and the likelihood that it will initiate a conflict.

The ability to mobilize a large number of fighters, however, is merely a necessary and not a sufficient condition for an ethnic group to initiate a conflict. Within the context of a broader theory of conflict onset, we believe that the effect of intra-group economic inequality on conflict initiation is conditional on factors affecting the willingness of ethnic elites to engage in a costly conflict against the state. Two of these factors are the group’s relative economic and political status vis-à-vis other ethnic groups in a country. As noted earlier, proponents of grievance-based explanations for conflict have argued that those groups feeling deprived of their fair share of the country’s wealth should be more likely to initiate a conflict. We agree with this line of reasoning, but treat the relative economic position of a group as merely a motivational factor with little impact on mobilization. A similar impact has the group’s political status. Given that conflict is a costly action with an uncertain outcome, ethnic elites will generally prefer to initiate policy changes through the political process. Whether it is possible and to what extent they will succeed, depends on their political status. Obviously, if the group controls power, the elites are free to implement their preferred policy. If the group is part of a ruling coalition, they will have to engage in bargaining, which they may prefer to conflict. But, if the group is excluded from power, the elites often have no other option than to initiate a conflict in order for the state to consider their political demands (Lacina, 2011). This leads us to our second hypothesis:

**Hypothesis 2:** The positive association between an ethnic group’s level of economic inequality and the likelihood of conflict initiation is particularly strong for those groups that are excluded from power and/or significantly better or worse off economically than the average ethnic group.
4 Measuring Within-Group Economic Inequality

Our analysis requires estimates of intra-group inequality at the level of ethnic groups, which are difficult to obtain. In this section, we introduce a new way to measure intra-group inequality using nightlight emissions. We first describe the data sources and the computation of the new measure, and then present our attempts to validate it.

4.1 Using Nightlight Emissions to Measure Inequality

Most indicators of intra-group inequality such as the frequently-used Gini coefficient rely on survey data. Survey-based inequality estimation is typically applied at the national level, where income (or consumption) scores of a national sample of respondents are aggregated to obtain an estimate of within-country inequality. While this approach is not without problems (for example, surveys measure income differently, which impedes cross-national comparisons), the main requirement—a large enough sample of respondents that is representative at the national level—applies in most cases. This is different when we move the level of analysis down to the group. Few surveys start with a country’s list of groups, and then create representative samples for each of them. If surveys include ethnic categories, they do so by relying on a respondent’s self-reported membership in a group. Not only does this procedure fail to ensure representativeness at the group level (rather, we have to assume it), it also makes it difficult to aggregate estimates at the group level since self-reported categories may differ even if individuals are from the same group.

For these reasons, we resort to a different way of obtaining intra-group inequality estimates, but use survey data to validate our new measure (see below). Similar to [Cederman, Weidmann and Gleditsch, 2011], we use a spatial procedure to measure variation in wealth among the members of a group. The general idea of this procedure is to combine maps of economic activity with those about the settlement regions of ethnic groups. By finding out if group regions coincide with locations of high or low economic activity, [Cederman, Weidmann and Gleditsch, 2011] are able to compute wealth estimates at the group level, which are then used to determine the group’s relative economic status in the country. While our procedure largely follows this approach and relies on...
similar types of geographic data, the fact that we are examining within-group rather than between-
group variation requires us to adapt the earlier procedure. The economic maps used in Cederman,
Weidmann and Gleditsch (2011) are based on the G-Econ dataset (Nordhaus 2006). G-Econ maps
economic activity at the level of grid cells of size $1^\circ$ by $1^\circ$, which corresponds to about 110 km by
110 km at the equator. G-Econ picks up variation between these cells, but obviously not within.
With many group regions being covered by only one or a few G-Econ cells, we would not be able
to capture within-group variation at a sufficiently detailed level.

The requirement of fine-grained economic data leads us to abandon G-Econ in favor of another
type of data with high resolution, but a less perfect match to the concept we want to measure:
nightlight emissions. The amount of light radiating out from a particular location on earth can
be measured by satellites with extremely high spatial precision. We use data from the Defense
Meteorological Satellite Program (DMSP), which are available at a 30 arc-minute resolution (ap-
prox. 1 km at the equator). The US National Geophysical Data Center (NOAA) archives and
cleans the data, and makes them available for download. In particular, the data have cloud covers
and unsteady sources of light (such as fires) removed, and only contain stable emissions.


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We use
the first available year (1992) for our computation. For data on ethnic settlement regions, we use
the GeoEPR dataset (Wucherpfennig et al. 2011), which is compatible with the group list from
the Ethnic Power Relations dataset (Cederman, Wimmer and Min 2010). In short, our procedure
for calculating intra-group inequality works as follows:

1. Divide up the geographic space into small cells of equal size ($1/12$ of a degree, about 10 km).
2. For each raster cell, compute (i) the total nightlight emissions from the DMSP-OLS data,
   and (ii) the total population using the Landscan high-resolution dataset (Oak Ridge National
   Laboratory 2008)[4]
3. For each raster cell, compute the cell wealth as nightlights per capita.
4. After ordering all cells in a given group’s settlement region by per capita wealth, compute
   the Gini coefficient.

[4] There exist alternative population raster datasets. In order to make sure that our results do not depend on the
Landscan data, we have also tested the Afripop dataset (Tatem et al. 2007) and achieved virtually identical results.
Based on this procedure, we end up with inequality measures at the group level, ranging from 0.05 (very little inequality) to 1 (perfect inequality). In the next section, we discuss potential problems associated with the measure, and present our attempts to validate it.

4.2 Validating the New Measure

Since our approach is a new attempt to measure economic inequality, we need to take a closer looks at its validity. At first glance, there are two potential sources of error. First, nightlight may not be a good proxy for economic wealth at the individual level. This may be (i) because nightlights could be a poor proxy for economic wealth in general, or (ii) because the satellite image is unable to distinguish between individuals of different incomes at a given location. The first concern is somehow alleviated by the finding that light emissions correlate highly with economic performance. Henderson, Storeygard and Weil (2011) use nightlight emissions and their changes over time to measure national-level economic growth and find that they serve the purpose relatively well, in particular in regions where other, more accurate measurements are difficult to get (see also Chen and Nordhaus 2011). Still, at the subnational level, nightlight emissions could be driven by state-sponsored development and electrification [Min 2010], which would be problematic for our approach that relies on subnational variation on light emissions. The second concern results from the fact that our inequality indicator captures variation in light emissions between larger cells, rather than variation between individuals. By assigning everybody living in a particular cell to have that cell’s wealth level, we may be unable to capture considerable variation in wealth that exists, for example, among people living an urban environment. The second source of error is related to our procedure of approximating group settlement regions with polygons. Using the GeoEPR dataset as described above, everybody living within one of these polygons is assigned to a particular group. Again, urban areas with a high degree of ethnic mixing would be particularly prone to errors, since our data is unable to capture variation in ethnic groups at a particular location.

In short, there are different potential problems that could make our nightlights-based indicator a problematic choice. For that reason, we subject our measure to a series of validation steps. First, we address the first source of error discussed above and compare national-level Gini coefficients based

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5 This concern is less relevant to our analysis, since patronage would predominantly generate differences in nightlight emission between groups, whereas we are interested in within-group variation.
on nightlights to conventional, survey-based indicators. Related to the second source of error, we test the quality of the GeoEPR group regions and assess the extent to which they correspond with individual-level data on ethnicity obtained from surveys. Finally, we conduct a comparison at the group level between survey-based Gini coefficients and those obtained from nightlights.

Validation Using National-level Gini Coefficients Conventional Gini coefficients are typically computed at the national level and aim to measure inequality among a country’s citizens. With a slight modification, we can use our procedure introduced above to produce national-level, rather than group-level, estimates of inequality: Instead of computing the Gini coefficient across all cells in a group region (step 4), we calculate the Gini across all cells in a country, and thus obtain a spatial inequality indicator at the national level. This indicator can then be compared to existing, survey-based indicators. For the validation exercise, we compute spatial inequality estimates for 1992, and compare them to Gini values from the World Inequality Indicator Database (WIID) for roughly the same time period.\(^6\)

The correlation between the raw values of the WIID estimates and our spatial ones is 0.51 ($p = 0.00$), which suggests that the latter pick up much of what the survey-based Gini coefficient measures. However, as we have argued above, one potentially huge source of error is that the nightlights-based indicator cannot pick up the tremendous variation in income within urban environments. For that reason, we test if and how excluding those regions affects the correlation between our measure and the survey-based indicators. In order to do this, we first classify the grid cells used for our inequality computation (see step 1 in the above procedure) according to their level of urbanization. This is done using the GRUMP dataset, which gives us the proportion of a cell’s area that is ‘urban’. We then exclude cells based on different thresholds of this proportion from our analysis. Results from this exercise confirm our expectation. Excluding cells that are 90% urban results in an increase of the WIID-nightlights correlation to 0.57 and to 0.59 when excluding cells that are 80% urban (see Figure 1). In essence, implementing this simple adjustment of our nightlights-based estimation procedure improves the correlation with conventional inequality indicators by 20%.

\(^6\)Since the WIID has a considerable number of missing values for 1992 (our year of comparison), we include all values from 1990 to 1994 to increase our sample size. This results in a cross-section of $N = 111$ countries.
Figure 1: Scatter plot of nightlights-based national-level Gini coefficients and those based on surveys (WIID).

**Validating GeoEPR Settlement Regions** We have verified that the nightlights-based Gini coefficient is able to pick up basic patterns of inequality across countries, but does it also identify these patterns across groups? As argued above, one major source of error could result from the fact that our spatial procedure assigns all individuals living in a group region to one group. Thus, ethnically mixed populations cannot be identified correctly, which may constitute a major problem for our estimation procedure. Luckily, there is a way to validate the GeoEPR coding of settlement regions again using surveys as an alternative data source on ethnicity. The Demographic and Health Surveys (DHS) are a collection of standardized surveys conducted for a large number of African countries [United States Agency for International Development (USAID) [2013]]. In addition to the survey responses, more recent editions of the DHS also include the ethnicity of respondents, and their location in geographic coordinates. This information allows us to test whether a respondent’s self-reported ethnicity matches his/her ethnicity as predicted by GeoEPR based on location.

The sample for this validation exercise consists of 208,687 individual respondents from DHS surveys between 1986 and 2011. We include all geocoded DHS surveys for Sub-Saharan Africa, giving us data on over 17 different countries at various points in time. Data coverage is obviously far

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7 This sample excludes respondents that do not report their group, those without geographic coordinates, and those from groups coded as ‘dispersed’, i.e. without a clearly recognizable settlement region according to GeoEPR.

from comprehensive, but we believe it represents a decent sample of Sub-Saharan African countries. We manually match the group given in the DHS to the GeoEPR group categories and then compute the GeoEPR group for each respondent. This is simply done by taking the geographic location of a respondent (longitude/latitude coordinates) and looking up the GeoEPR group polygon that exists for that location and the given year. The results from this exercise are overwhelmingly positive. Out of the 208,687 individuals included in our sample, we can correctly predict their ethnic group for more than two-thirds (142,871, 68.5%). Similar to the nightlights measure, we may again be concerned that GeoEPR may be inaccurate in particular in urban areas with high degrees of ethnic mixing. This expectation is confirmed: if we exclude respondents living in ‘urban’ areas as measured by GRUMP, the percentage of respondents where GeoEPR correctly predicts ethnicity goes up to 74.5% (102,789 correctly predicted respondents out of 138,038 respondents living in rural regions). This finding is another reason for excluding urban areas in our main analysis below, as it is in these regions where the spatial measurement procedure seems to be most prone to errors.

**Group-level Comparison Using DHS Surveys**  
Finally, we conduct a validation exercise at the group level using again data from the DHS. Recent research has used this and other cross-national surveys to measure group-level inequality indicators (Huber and Gore, 2011). In comparison with other surveys such as Afrobarometer or the World Value Survey, DHS seems to be particularly well-suited to our task here, since (i) the national sample sizes are large, leaving us with large enough subsamples at the group level, and (ii) DHS employs a fine-grained measure of wealth, which allows for a precise computation of a group-level Gini coefficient. We use again DHS surveys for Sub-saharan Africa between 1986 and 2011. As described above, we match individuals to EPR groups and compute the Gini coefficient across all individuals of a group. This is done by constructing a wealth index based on the series of household asset questions, including questions on the type and quality of housing and the ownership of various household appliances and vehicles. After breaking the wealth index into quintiles, we calculated the Gini coefficient by numerical approximation for each ethnic group using the trapezoid method. We drop again groups without a

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9 The following variables from the DHS surveys were used whenever available: existence and type of toilet facility (v116), status of electrification (v119), ownership of a radio (v120), ownership of a television (v121), ownership of a refrigerator (v122), ownership of a bicycle (v123), ownership of a motorcycle/scooter (v124), ownership of a car/truck (v125), main floor material (v127), main wall material (v128), main roof material (v129), and ownership of a telephone (v153).
territorial representation and those with small DHS sample sizes (30 and below). This leaves us with a set of 251 groups for which we can compare survey- and nightlights-based Gini values.

For the groups in our sample, the correlation between the Gini based on DHS surveys and the one based on nightlights is 0.42 (p=0.00). This is not very strong, but indicates that our nightlights measure can pick up not only national-level, but even group-level inequality to a certain extent. Again, we test whether the lack of precision of our spatial procedure in urban areas is partly responsible for the result. This expectation is confirmed; excluding cells that are at least 80% urban (as explained above) improves the correlation by roughly 10% to 0.46. This correlation may seem low at first, but we have to keep in mind that we are not comparing our nightlights-based measure to the ‘true’ level of inequality. The survey-based indicator may equally be prone to errors; for example, the DHS surveys are not designed to be representative at the group level, so there could be selection bias. Moreover, since the DHS questionnaires were not designed to capture economic inequality, they do not contain a very sophisticated set of questions regarding household income and consumption. We proxy wealth by a series of relatively crude household asset questions, which is obviously far from perfect.

In sum, our validation exercise has proven to be moderately successful. We have seen that the nightlights-based inequality indicator is able to pick up much of what survey-based indicators measure, both at the national and the group level. Also, we were able to confirm that GeoEPR’s encoding of ethnic group regions allows us to predict the true ethnicity of individuals to a very high extent. Of course, our spatial measurement technique is far from perfect. However, since it is largely free from reporting and selection biases that could exist in surveys and is the same time globally applicable, we believe that there are considerable advantages to using it in a large-N analysis such as ours. In the next section, we introduce the design of our analysis that relies on our new indicator.

5 Research Design and Results

We are now ready to proceed to an empirical test of our theoretical propositions presented above. Using regression analysis, we assess the explanatory impact of our nightlights-based inequality indicator on ethnic conflict onset. Our approach relies on Cederman, Weidmann and Gleditsch...
and amends their base model. The sample consists of politically marginalized ethnic groups, which have limited inclusion in their country’s government, or are completely excluded from political influence. The group list and the level of political participation is taken from the EPR dataset (Cederman, Wimmer and Min 2010). Since our nightlights-based indicator only goes back as far as 1992, we include annual observations from 1993 to 2010. This results in a sample of 7,286 group/years, which is slightly larger than the one in Cederman, Weidmann and Gleditsch (2011, 6,438 observations) due to the longer time period (1993-2010 as compared to 1991-2005).

5.1 Variables

Our dependent variable is the onset of ethnic conflict and is taken from the ETH Zurich’s GROWup data portal (http://growup.ethz.ch). The variable takes a value of ‘1’ in those years where an ethnic groups becomes involved in armed conflict against the state. It is generated according to the procedure presented in Wucherpfennig et al. (2012), where politically marginalized ethnic groups are coded as “in conflict” if an armed organization fights on their behalf in a civil war as defined by the Uppsala/PRIO Armed Conflict Dataset (Gleditsch et al., 2002).

The main independent variable is intra-group inequality based on nightlight emissions data for 1992, as introduced in the previous section. This is the earliest available release of these data, and we use it to predict conflict in the years 1993 and onwards. Since our validation has clearly demonstrated that urban areas introduce error into our inequality estimate, we exclude these areas from the computation following the procedure described above. We use one estimate of inequality, since inequality is a quantity that changes only slowly over the years. Thus, what drives our results is the cross-sectional variation between groups, which we believe can be captured sufficiently well using the nightlights-based indicator.

We include the same set of independent variables as Cederman, Weidmann and Gleditsch (2011). First, we measure the relative economic status of a group as compared to the country average using the lineq2 measure. High values indicate that a group is either rich or poor as compared to the country average. As Cederman, Weidmann and Gleditsch (2011) have shown, high levels of this horizontal inequality are related with a higher risk of conflict onset. Next, in addition to these

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10 The slow changes in inequality are confirmed by our nightlights indicator, where estimates for 1995 (2010) are correlated with those for 1992 at 0.96 (0.84).
economic motives for conflict, we control for political motives using a dummy for whether a group is excluded from political power, according to the EPR dataset. A last group-level variable is the demographic power balance between the group and the group(s) in power, which following Cederman, Weidmann and Gleditsch (2011), is included both as a linear term and in a squared transformation to account for non-linearities in its effect on conflict onset. Last, we control for time trends by including the calendar year of observation and model temporal dependence in our dependent variable by including the number of peace years as a control variable, as well as its squared and cubed transformation (Carter and Signorino, 2010).

5.2 Results

We use logit models to test the impact of intra-group inequality on ethnic conflict. Our models first replicate the base model from Cederman, Weidmann and Gleditsch (2011) and then add our intra-group inequality variable (H1). Next, we test for conditional effects by interacting intra-group inequality with our variables for economic motives (relative inequality) and political motives (exclusion). Table 1 shows the results of our analysis.

Model 1 replicates the results from Model 3 in Cederman, Weidmann and Gleditsch (2011). Since we rely on a different sample, the results differ to a certain extent. However, our analysis confirms again the main effects found in the original paper and show that relative inequality of a group has a strong positive effect on conflict onset. Groups that are richer or poorer as compared to the national average face a higher risk of large-scale political violence. This economic effect operates together with a political one, as shown by the positive effect of the exclusion dummy: lack of political participation is a strong predictor of conflict. The remaining control variables receive largely the same effects as in the original model, but some fail to reach conventional levels of significance.

In Model 2, we add our intra-group inequality measure to the base model. As we hypothesized above, intra-group inequality exhibits a positive and significant effect, indicating that ceteris paribus, more unequal groups have a higher risk of engaging in violent political conflict. The size of the effect is also significant in substantive terms; increasing intra-group inequality from 0.322 (5th percentile) to 0.984 (95th percentile) changes the predicted likelihood of conflict from 0.18 percent to 1.5 percent, an eight-fold increase.
<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
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<td>Intra-group inequality (IGI)</td>
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<td>0.633</td>
<td>2.439***</td>
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<td></td>
<td>(0.522)</td>
<td>(1.485)</td>
<td>(0.665)</td>
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<td>Relative Inequality (lineq2)</td>
<td>0.447***</td>
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<td></td>
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<td></td>
<td></td>
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<td>(0.290)</td>
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<td>1.337***</td>
<td>0.062</td>
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<td>(0.340)</td>
<td>(1.159)</td>
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<td>-6.711</td>
<td>-6.937</td>
<td>-6.625</td>
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<td>GDP per capita (log)</td>
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<td>-0.166</td>
<td>-0.179</td>
<td>-0.160</td>
<td>-0.173</td>
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<td></td>
<td>(0.154)</td>
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<td>(0.165)</td>
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<tr>
<td>No. excluded groups</td>
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<td>0.006</td>
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<td></td>
<td>(0.020)</td>
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<td>Constant</td>
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<td>83.274</td>
<td>84.399</td>
<td>81.868</td>
<td>83.372</td>
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<td></td>
<td>(60.615)</td>
<td>(60.789)</td>
<td>(60.594)</td>
<td>(59.370)</td>
<td>(59.173)</td>
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<td>p</td>
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<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
</tr>
<tr>
<td>AUC</td>
<td>0.824</td>
<td>0.830</td>
<td>0.831</td>
<td>0.830</td>
<td>0.831</td>
</tr>
</tbody>
</table>

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 1: Regression results. Temporal controls (calendar year/peace years) not shown.
In Models 3 through 5, we test the conditional effects of intra-group inequality. As argued above, the effect of intra-group inequality should be stronger if both political and economic motives for conflict are high. Model 3 tests the former by interacting the exclusion dummy with intra-group inequality. As shown in Ai and Norton (2003), interpreting interaction effects in non-linear models cannot be done based on the coefficients shown in the table. Thus, in Figure 2 we plot the average simulated effect of intra-group inequality both for included and excluded groups. The plot confirms our expectation. For politically included groups, intra-group inequality seems to have no effect. For excluded groups, however, the effect is positive and significant.

![Figure 2: Average simulated effects of intra-group inequality on conflict onset, for included (left) and excluded (right) groups.](image)

Model 4 interacts intra-group inequality with relative inequality compared to the national average. We plot the interaction effect in Figure 3. Relative inequality is represented along the x-axis and ranges from 0 (no inequality) to 2 (groups that are either about 1/4 as wealthy as the country average, or about four times as wealthy). Although less pronounced as in Model 3, we find that the effect of intra-group inequality increases as economic motives for conflict increase; over the range of relative inequality, there is a marginal increase in the predicted probability of conflict onset by about 25%, from 0.02 to 0.025.

Model 5 adds both interactions at the same time. We visualize the interaction effect by separating out included groups (Figure 4, left panel) and excluded groups (Figure 4, right panel). As we expected, for included groups and irrespective of their relative inequality status, there is

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112 corresponds roughly to groups that are 4 times as wealthy, or 1/4 as wealthy, as the country average: \( \log(4) = \log(0.25) = 1.92 \). See definition of the \( \text{lineq2} \) measure in Cederman, Weidmann and Gleditsch (2011, p.9).
no effect of intra-group inequality, as the confidence interval in the left plot includes the 0. This is different, however, for included groups, where we see a positive effect of intra-group inequality that increases as relative inequality goes up. In sum, these results are evidence supporting our hypothesis above: Intra-group inequality seems to increase the risk of conflict, but only if political and economic motives are present.

We also tested the impact of intra-group inequality on the predictive accuracy of the model (Ward, Greenhill and Bakke 2010). In models with a binary outcome variable, this is typically done by ways of ROC analysis. Here, predictive accuracy is measured by the “area under the curve”, an indicator ranging from 0.5 (no value added as compared to random prediction) to 1 (perfect prediction). The AUC values for each of the models are given in the last row in Table 1.
We can see that the baseline model (Model 1) already has a relatively high AUC score. Adding intra-group inequality, the AUC score does increase, but only marginally so, demonstrating a weak improvement over the base model.

5.3 Robustness Checks

Alternative operationalizations and specifications  We amend the models presented above with a series of additional checks, in order to ensure that the effect of intra-group inequality we find above remains robust across different model specifications. Our robustness checks include the baseline model testing the unconditional effect (Model 2) and the model with two interaction terms (Model 5). Detailed results of this exercise will be made available in an online appendix.

First, we run the models with regional dummies, taken from Fearon and Laitin (2003). The effect of intra-group inequality remains positive and strongly significant in the unconditional model. The interaction with economic motives shows again that for higher values of relative inequality, the effect of intra-group inequality increases for excluded groups, but there is almost no effect for included groups. Second, we control for group wealth, taken from Cederman, Weidmann and Gleditsch (2011). This test ensures that the effect of intra-group inequality we find is not due to systematic differences in wealth across groups. Our results remain fully robust to controlling for group wealth, and all our main effects of interest retain their signs and significance levels. In our model, group wealth is not significantly related to conflict onset. Third, we control for the presence of natural resources in the country, in particular oil. Oil production has been shown to affect the risk of conflict, but may also contaminate our nightlights-based measure since it is associated with particular night emissions (illumination of oil fields and exhaust flames). Our dummy for oil production is based on Ross (2013). All our results remain robust to the inclusion of this variable. Last, we test an alternative measure of state strength. As many other examples in the literature (e.g. Fearon and Laitin 2003), our model uses GDP per capita to capture state strength, which may, however, capture many different factors. Therefore, we replace this variable with the incidence of coup attempts in the two years prior to the year of observation, based on data from Powell and Thyne (2011). The occurrence of coup attempts is an alternative indicator of a weak state and may open a window of opportunity for rebellion. As we would expect, this indicator is positively related to conflict risk, but remains insignificant and does not alter the effects of our main independent
Table 2: Results of the Altonji et al. (2005) assessment of bias from unobservables.

<table>
<thead>
<tr>
<th>Controls in restricted set</th>
<th>Controls in full set</th>
<th>Ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td>None</td>
<td>Full set from Model 2</td>
<td>14.5</td>
</tr>
<tr>
<td>Peace years controls</td>
<td>Full set from Model 2</td>
<td>3.2</td>
</tr>
<tr>
<td>Peace years and country-level controls</td>
<td>Full set from Model 2</td>
<td>35.4</td>
</tr>
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</table>

Assessing Bias from Unobservables  
Despite our attempts to control for many potential confounders, we cannot rule out that unobservable factors are correlated both with our independent and dependent variables, and lead us to mistakenly identify an effect of intra-group inequality on conflict. In order to assess whether our model specifications are prone to selection bias, we follow a procedure proposed by Altonji, Elder and Taber (2005), which was later modified by Bellows and Miguel (2009) and used in Nunn and Wantchekon (2011). The core idea is to assess how much stronger, in relation to observed controls, bias from unobserved covariates would have to be to explain away the effect of our main independent variable, intra-group inequality. The Altonji et al. (2005) approach works by comparing the coefficient estimated in a full model with all controls to the one obtained in a restricted model with no (or few) controls. The result of this calculation is a ratio, where high values indicate that selection based on unobservables would have to be high as compared to the one on observed controls for the effect to be zero.\(^\text{12}\) Since it is difficult to employ this approach to models with interaction terms, we restrict this analysis to Model 2. We test three different restricted models, one with no controls, one that includes only the peace years controls, and one with peace years and country level controls. As shown in Table 2, we find consistently high values for the ratio (3 and higher), which gives us some confidence that the positive association between within-group inequality and conflict is unlikely to be solely due to selection on unobservables.

6 Conclusion

The capability to mobilize a sufficient number of fighters is a crucial factor in explaining why some ethnic groups rebel and others do not. However, the existing literature on civil war initiation has

\(^{12}\) As proposed in Bellows and Miguel (2009), we compute this ratio based on linear probability models.
not given adequate attention to this mechanism. In this paper we have focused on this aspect of an ethnic group’s decision to engage in violence against the state. We have done so by looking at the impact of intra-group economic inequality, a group-specific structural factor that we argue is directly linked to the groups mobilization capacity. By considering the impact of this factor in light of existing theories of participation and rebel recruitment, we claim that there is a positive association between the level of economic inequality within an ethnic group and its probability of conflict initiation. In order to evaluate this claim empirically, we develop a new spatial approach to measuring economic inequality using nightlight satellite imagery, population rasters, and geocoded ethnic settlement areas. After validating our new measure, we add our indicator to existing statistical models of conflict onset and find considerable evidence in support of our theoretical prediction: greater economic inequality within a group significantly increases the risk of conflict, especially if the group is excluded from political power and above or below the country average in terms of per-capita income.

Although our results are far from definitive, leaving plenty of room for future theoretical and empirical research, we believe they provide important insights for conflict research. First, we extend the sparse literature on intra-group inequality and conflict by taking a broader theoretical look at how within-group inequality may affect mobilization. Second, to our knowledge, this is the first empirical study of the impact of within-group economic inequality on conflict at the global level. Although our theoretical discussion goes beyond the opportunity cost mechanism in Esteban and Ray (2011), our empirical analysis can also be seen as the first empirical test of their model. Third, this paper also makes a significant contribution to the ‘Greed and Grievances’ debate in the civil war literature. By distinguishing between inter- and intra-group economic inequality, we show that these different types of economic inequality matter for conflict initiation in distinct and previously ignored ways. While inter-group inequality affects an ethnic group’s willingness to rebel, intra-group inequality affects the group’s opportunity to launch a successful rebellion, suggesting that the overall effect of economic inequality on conflict initiation is more complex than previous studies claimed. Both a sufficiently high level of inter- and a sufficiently high level of intra-group economic inequality are necessary conditions, but only high levels of both inter- and intra-group inequality are sufficient for an ethnic group to initiate a conflict. Finally, our study offers one possible explanation for the inconsistent effect of commodity price shocks on conflict onset. If
ethnic groups are associated with specific economic activities (e.g., dominant in the production of a
certain cash crop), then price shocks may have a short term impact on both between- and within-
group inequality. Depending on how the shock affects these two types of economic inequality the
probability of conflict may rise or fall.
References


