Spatial dimensions of social unrest and armed violence

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To Janina

Abstract

Social unrest and armed violence rewind development achievements in the fight against poverty. This thesis examines various factors contributing to social unrest and armed violence in different parts of a country. A common method of this thesis is the empirical regression analysis of geo-spatial data to explain social unrest and armed violence. I find four results. First, social unrest occurs more likely in areas where droughts coincide with existing ethnic grievances. Second, to identify these grievances, we develop a novel spatial inequality measure between and within ethnic groups. Validation of the inequality measure against perceived differences in identity groups' economic conditions shows that individuals feel ethnic grievances. Third, competition between armed groups causally increases the level of violence. Finally, there is no evidence that development aid increases civil wars, but Chinese aid seems to increase state repressions and a higher tolerance for autocratic rule. This thesis shows that spatial characteristics can help understand and explain social unrest and armed violence within countries.

Keywords: Political economy, social unrest, economic development, political violence, ethnic inequality

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Chapter 1

Introduction

Sustainable economic development is the key driver to improve the livelihood of all people around the globe and ending poverty. Peace and prosperity for the people and the planet are the shared vision of the United Nations formulated in 17 distinct Agenda 2030 goals. Although much has been achieved already and poverty rates decreased in the past, a certain group of countries is left behind. Countries with high levels of Fragility, Conflict, and Violence (FCV) did not see a decrease in poverty in the past decades, and the World Bank estimates that half of the world's extreme poor will live in FCV countries by 2030 (Corral et al., 2020).

Conflict-affected countries lack behind in their fight against poverty and witness detrimental impacts on development gains, such as losses in educational attainment, losses in life-time earnings, and deeper gender disparities (Buvinić et al., 2013). Conflicts have a long-term impact on economic growth, as well. For example, the World Bank (2017) documents strong GDP growth in Syria's non-oil sectors, but the Arab Spring triggered protests in Syria that eventually escalated to the Syrian civil war. GDP losses between 2011 and 2016 due to the war are estimated to be four times Syria's 2010 GDP. However, long-term cumulative losses are estimated to be 20 higher than capital losses due to disrupted economic networks and institutions that cannot be build up as quickly as capital investments.

Social unrest and armed conflicts appear to be distinct forms of political action but have a common motivator for group mobilization. Collective grievances are one major factor with the potential to bring together individuals to voice their dissatisfaction and fight for political change and better livelihood. Hence, it is not surprising that protests may also escalate to armed violence, especially if participants who are willing to use force do not observe any progress of their cause (Ryckman, 2019). When groups of citizens feel severely disadvantaged and have limited means to be heard, protest and even taking up arms seem to be the only options left.

The marginalization of ethnic people proves to be a powerful mobilizer for social unrest and maybe a reason to join armed groups. Conflict in late 2007 erupted in Kenya after the Kikuyu group members won the election over members of the Luo, Luhya, and Kalenjin. Within two months, almost 1000 people died, mostly from the Kikuyu tribe, who are perceived to be disproportionately better-off (Wrong, 2010). There exist various armed groups in Balochistan that fight for more autonomy of their region, since Balochistan is one of Pakistan's most resource-rich regions but remains economically backward compared to other regions in Pakistan. The Euskadi Ta Askatasuna group, better known as ETA, is responsible for over 800 deaths over the course of 30 years in their fight for an autonomous Basque country (Abadie and Gardeazabal, 2003). Thus, ethnic grievances are present and relevant in countries across all income levels.

This thesis contributes to understanding how economic exclusion of ethnic groups may increase social unrest and armed violence, and how official development aid may increase government repression. Social unrest received surprisingly little attention, given the vast literature on armed violence. This thesis lays out the construction of a novel inequality measure to track ethnic grievances. These grievances interact with external climate shocks and may be expressed as social unrest as the social contract between the state, and its citizens is crumbling. Moreover, one ethnicity maybe represented by multiple political or armed groups. A group-level study in Pakistan shows that competition effects among one specific set of armed ethnic groups causally increases violence levels. Moreover, the thesis develops a method to combine two well-known dataset of armed violence, that eliminates double-counting of events and allows to approximate for government counter-insurgency measures. Additionally, the thesis scrutinizes how development aid contributes to more stability in civil war settings, more government repression, and deteriorating norms on democracy depending on conditions set by the donor.

The thesis is organized as follows. Chapter 2 shows that droughts interplay with ethnic inequality in triggering social unrest and protests. Protests may occur throughout a country but tend to be locally confined. A counter-example would be a civil war where fighting units traverse from one location to the next, thereby expanding their control area. Given the local dimension of protests, the chapter devices a novel local inequality measure that is decomposable, allows for various group definition, and can be applied to any spatial resolution. The chapter applies the local inequality measure to satellite nightlight emission within ethno-linguistic regions. The results synthesises the literature of inequality as well as climate impacts on social unrest and violence. The interaction of climate shocks and ethnic between-group inequality affect social unrest. Droughts increase the probability of social unrest, when they occur in areas with high levels of ethnic inequality.

Why does the interaction of climate shocks and inequality leads to more conflict? Chapter 3 argues that the perception of ethnic inequality lead to grievances. A validation of the local inequality measure of chapter 2 helps to understand if inequality measured from outer space maps grievances felt by individuals on the ground. Afrobarometer data on individual perceptions of ethnic group inequality correlate with the local measure of ethnic between-group inequality. Moreover, economic distress such as droughts seems to increase the salience of ethnic groups. Thus, droughts and ethnic inequality together seem to contribute to individual inequality perception. The results validate the relevance of the novel inequality measure and suggest that droughts increase social tensions not via economic distress alone but by a higher perception of group differences.

Chapter 4 focuses on competition among groups of one specific ethnicity in Pakistan to analyze the internal determinants of armed violence. Focusing on a specific group allows tracking their activity closely across space and pinpoint specific events to an upsurge in overall violence. The chapter explores the unique setting in which the number of armed groups increases through a split of the Baloch Liberation Army (BLA) into the BLA and United Baloch Army (UBA). The Marri family leads the BLA, and leadership disputes between two brothers broke out when their father died of a natural cause. The death eventually led to the group split, each led by one of the brothers. The chapter shows that the BLA split increases the number of incidents within Balochistan compared to the districts outside Balochistan by 3.5, which is an increase of roughly 130%. The study does not only contribute to understanding the dynamics of competition between armed groups. The chapter also describes a data-driven approach in identifying double-counts of incidences of two well-known datasets of armed violence. Moreover, the chapter provides a dataset of mergers and splits of armed groups in Pakistan for researchers to use in their group-level analysis.

Social unrest and armed violence is also determined by government action. Chapter 5 focuses on the role of development aid on armed conflict and government repression. Development projects are aimed at assisting lower-income economies to strengthen their economic development. Development assistance is not undisputed, however. Researchers and policymakers criticize aid as fungible, subject to elite capture, and contribute to conflict financing and corruption (Isaksson and Kotsadam, 2018a). China, as the emerging donor, stirs up this literature. China's values of non-interference from other states and respecting sovereignty are in stark contrast with Western donors' values of human rights and democracy. Neither of these values can be ranked objectively over the other, but they eventually lead to different aid impacts. The analysis compares the impact of Chinese aid as an emerging donor and World Bank aid as a traditional donor. On the one hand, systematic tests show a lack of evidence that aid affects armed conflict for World Bank and Chinese aid. On the other hand, the relationship seems to be driven by the donor's different tolerance to state repression. While there is no evidence that World Bank aid affects government repression, regions that receive Chinese aid experience more state repression incidences. Using governance questions of the Afrobarometer reveals that individuals living in regions that receive Chinese aid report a significantly lower identification with democratic ideals. In contrast, the opposite is true for regions receiving World Bank aid.

The final chapter briefly puts the thesis' findings into the broader picture of how social unrest and armed violence may be viewed from a policy perspective.

Chapter 2

Triggering social unrest

Ethnic inequality intensifies drought effects in Africa[*]

2-1 Introduction

A commonly held belief is that social unrest is linked to high levels of inequality. Media narratives suggest that the protests during the 'Arab spring' were in part so intense because of a prevailing sense of inequity. Similarly, the vigorous protests sweeping Latin America in 2019 have been associated with high levels of inequality in the region. Protests are typically not started by high levels of inequality, instead they are often incited by changes in food prices or government subsidies and potentially become more intense in places where the underlying grievances are strong.

In this paper, we analyze whether droughts trigger social unrest in Africa and whether local inequalities between ethnic groups intensify such unrest. Put differently, we ask if severe weather shocks interact with ethnic grievances, and if ethnic groups compare themselves to nearby tribes or to the entire nation. To answer this question, we design a new sub-group decomposable index of local spatial inequalities. We combine this new index with geospatial data to empirically analyze a large set of protests and violent riots between 1992 and 2013 across the entire African continent. Our identification strategy leverages exogenous temporal and spatial variation in the occurrence of severe droughts and its interaction with slowly-changing local inequalities. This allows us to identify a conditional estimate which compares the effect of a drought in regions with high ethnic inequality to the effect in regions with low ethnic inequality.

A key building block is that we motivate and derive an index of local spatial inequalities that can be decomposed into between and within-group components. "Horizontal" inequality between ethnic groups or administrative regions is typically measured at the national level, so that we know very little about whether the association of inequality with various forms of conflict is driven by localized tensions or nation-wide grievances. To overcome this issue, we define a local entropy index which is similar to those proposed in the literature on spatial segregation (e.g., Reardon and O'Sullivan, 2004). Our index essentially compares the local distribution of income to the local distribution of the population in some predefined radius (or neighborhood) and spatially weights the contribution of each unit to the index.

[*]This chapter is based on joint work with Richard Bluhm

Contrary to non-spatial indexes of inequality, it allows us to study inequality between and within groups at very local scales where the definition of "local" is determined by the available data. Hence, we can ask whether the inequality that is observed locally matters more or less than income comparisons to individuals and groups located further away. Our measure coincides Theil's entropy coefficient (Theil, 1967) if the neighborhood is made arbitrarily large and the weighting scheme is uniform.

We employ this measure to capture local inequality between and within ethnic groups in Africa (but it could be useful in a number of contexts). Building on Alesina et al. (2016), who measure nationwide inequalities between ethnic groups with geospatial data, we use nighttime lights as a proxy for economic activity together with high-resolution population data to estimate the components of our index. To this end, we first partition the space into a lattice of units that are less than 10 km wide and then consider a range of neighborhood sizes for the weighting function, from 50 km to 200 km. Maps of the spatial distribution of ethno-linguistic groups help us to identify the ethnic affiliation of each spatial unit. For all neighborhood sizes, the within-ethnic group component is larger than the between-ethnic group component, and local inequalities tend to be smaller than comparisons which include far away units. We then aggregate the data to the standard grid of $0.5° \times 0.5°$, which is typically used in conflict research, and add data on protests and droughts. Our main specification studies the interaction of droughts and local inequalities at various neighborhoods, after purging confounding variation that is constant across grid cells or country-years.

Our results highlight the critical role economic differences between ethnic groups play in mediating the effect of droughts on social unrest. Droughts primarily trigger protests and violence in places with underlying grievances between ethnic groups. An increase of local inequality between ethnic groups by two standard deviations during drought years increases the protest likelihood by 0.26 percentage points. This effect resembles about a third of the average protest likelihood in Africa. Moreover, the estimates we provide are strictly decreasing in the size of the neighborhood, roughly halving if we double the size from 50 km to 100 km and becoming indistinguishable from zero at 200 km. We interpret this as evidence that droughts trigger protests and violence in places where members of one ethnic group can observe another close-by ethnic group which appears to be better (or worse) off. Directly observing group-level inequities in income and, perhaps also, differences in public service provision thus appears to be a prerequisite to food-price related social unrest in Africa.

We present a number of extensions to better understand this finding. First, we examine whether within-group inequality has a similar amplifying effect on the propensity to protest. Within-group inequality can help to fund mobilization and overcome a resource problem of poor groups. Protests, however, are low cost events and often spontaneous so that this aspect may play less of a role in our context. In line with the latter, we find little evidence suggesting that inequality within groups interacts with droughts, especially once we allow interactions with both types of inequality. Second, we study if horizontal inequality defined along administrative regions is a relevant between-group cleavage in Africa. We find little support in the data that regional inequalities interact with droughts, even though administrative divisions sometimes overlap with ethnic homelands. Third, we analyze the role of ethnic distances (as opposed to physical distances). Groups whose languages are more closely related may feel less antagonism towards one another in spite of local inequalities compared to groups who are linguistically distant (and thus more likely to be perceived as an "out-group"). We find some evidence supporting this notion. The interaction of droughts and local inequality between groups is weakly increasing in linguistic distance, but these differences are not statistically significant. Finally, a number of robustness checks show that our main results do not depend on particular estimation choices.

Our paper relates to several larger bodies of work. Even though climate shocks are often thought of as a trigger of social unrest, there are surprisingly few studies on climate as a driver for protests and riots (Hendrix and Salehyan, 2012).[1] The majority of climate disaster-related studies focus on armed conflict or outright civil war and typically explains local conflicts via sub-national climate variation (Burke et al., 2015, 2009; Hsiang et al., 2013, 2011; Bohlken and Sergenti, 2010). Our work is closest to two recent studies. Harari and Ferrara (2018) show that droughts decrease agricultural income and increases armed conflict in grid cells across Africa, while Almer et al. (2017) find that droughts intensify competition for water and trigger riots. Other studies highlight that there is little evidence of a robust relationship between climate variability and a broader definition of conflict (Bergholt and Lujala, 2012; Buhaug, 2010; Buhaug et al., 2014; Gleditsch, 2012; Theisen, 2012; Theisen et al., 2012; van Weezel, 2019). In line with this, our results show little association between droughts and (violent) protests when inequality is not taken into account.

A large literature is focused on the effect of income inequality on conflict. The theoretical case is well-developed. Within economics, the identification-alienation approach (Esteban and Ray, 1994, 2011) offers a framework of how social stratification along income lines is related to conflict intensity. A key insight in this line of work is that greater inequality between groups leads to greater alienation vis-à-vis other groups, while ethnic markers can be used to achieve stronger identification with the group. If poorer groups can expropriate richer groups, then greater income disparity between groups implies larger spoils of victory. This conjecture finds support within political science. Stewart (2008) and Østby (2008), for example, argue that greater 'horizontal' inequalities, that is, inequalities between ethnically or culturally defined groups, can bring about (non-)violent conflicts. In turn, within-group inequalities can help overcome a resource problem: universally poor groups may lack the means to fight. Empirically linking income inequalities to social unrest and conflict has proved difficult. This is partly due to conceptual issues (e.g. 'What type of inequality matters? Among whom?') and in part due to limited data availability.[2] New geospatial data permit the construction of group-specific income proxies across a wide range of geographies (e.g. Cederman et al., 2011, 2015; Alesina et al., 2016). We follow this recent literature in how we

[1] Although a larger literature focuses on the link between food prices and social unrest (see e.g. Bellemare, 2015; Hendrix and Haggard, 2015).

[2] Collier and Hoeffler (2004a), for example, argue that most political and social variables hold little explanatory power when it comes to conflicts. As in most early studies, their measure is a household survey aggregate of inequality among all individuals in the country. Group-level economic disparities were only recently captured using household surveys. Østby (2008) uses Demographic and Health Surveys (DHS) from 36 developing countries to show that horizontal social inequalities are related to conflict. Huber and Mayoral (2014) assemble data from more than 200 household surveys to study the role of vertical and horizontal inequalities between groups. They decompose total national inequality into within and between group inequalities to show that a) national comparisons can be deceiving, and b) within group inequalities could be more important than between group inequalities.

construct our income proxy and contribute to it by studying differences between and within groups based on different group delineations.

Although grievances are often thought to be local in nature, they are seldom measured that way. The social deprivation literature in economics and sociology recognized this local component from the start. Galbraith, for example, defined poor people as those whose "income, even if adequate for survival, falls markedly behind that of the community" (1958, p. 252). Townsend (1962, p. 219) puts it this way: "individuals and families whose resources, over time, fall seriously short of the resources commanded by the average individual or family in the community in which they live, whether that community is a local, national or international one, are in poverty." This notion of relative poverty is very close to our concept of local inequality. In essence, we argue that this is how communities and ethnic groups perceive themselves vis-à-vis other groups. Still, we do not know if the relevant comparison is the immediate neighborhood or the larger nation and treat this part as an empirical question.

A nascent literature on the economic consequences of local inequality suggests a key mechanism behind local tensions may be exclusion from markets and government services. Gulati and Ray (2016) study access to basic services, such as health care or schooling, and emphasize a trade-off between provision and prices. Initially, some inequality may help to increase the provision and availability of services demanded by richer neighbors. However, as inequality increases, the poor are eventually priced out of the market. Araujo et al. (2008) show that social investments in Ecuador designed to exclusively benefit the poor within a neighborhood are provided less often to comparatively unequal communities. Their explanation is that local elites who rule these localities block projects which do not benefit them privately. Thus, local inequality may also reduce the benefits that poorer individuals may receive from the government. Our study highlights that external shocks appear to interact with a pre-existing perception of injustice or a lack of government service. This is sometimes described as as a fulfillment failure of the social contract which creates fertile ground for social unrest (Patel and McMichael, 2009).

The remainder of this paper is organized as follows. Section 2-2 introduces our sub-group decomposable local inequality measure and discusses its properties. Section 2-3 describes the data and our empirical strategy. Section 2-4 presents our main results, including several extensions and robustness checks. Section 2-5 concludes.

2-2 Measuring local inequality

The first objective of this paper is design an index of spatial inequalities at the local level that summarizes the experience of those living in a particular neighborhood. The two criteria which make inequality local are that observations close by receive greater weight than those afar, and that the definition of between-group inequality should depend on the underlying spatial structure, such as, borders separating ethnic groups or political regions. Drawing on insights from the spatial segregation literature (Reardon and O'Sullivan, 2004), we motivate and derive a spatially-weighted Theil index which shares these two properties (Theil, 1967).

In addition to capturing local inequality, an index of spatial inequality that is suitable for our application should satisfy several principles of inequality measurement, such as scale independence and the population principle, but not anonymity (Cowell, 2011). Since location matters, it it not desirable to require that inequality should not increase or decrease when people, cells, or any other underlying unit switch positions in two-dimensional space. Instead, the index should capture how spatial inequality varies depending on where particular values cluster.[3] Finally, the index should be additively decomposable, so that we can examine spatial inequality between different ethnic groups in the application.[4] This implies that the index has to be part of the class of generalized entropy indexes (Bourguignon, 1979; Shorrocks, 1984). There are other approaches in the literature, such as the spatial Gini proposed by Andreoli et al. (2018), but these measures are not subgroup decomposable without a residual.

To fix ideas, we now introduce some notation together with the relevant concepts. Our proposed local inequality measure is defined for a vantage point, p, within a *neighborhood*, N. Each unique neighborhood comprises of multiple locations denoted by c. We may think of such a neighborhood as a collection of relatively small regions, such as census tracts, or locations on a regular grid.

Spatial proximity between locations is given by some function $\psi(p, c)$ which denotes the inverse distance between the vantage point p to location c, for all possible (unordered) pairs of $\{p, c\}$ in the neighborhood. For now, we impose little structure on $\psi(p, c)$, apart that it is non-negative and symmetric when p and c are switched. We will later specify common spatial weighting functions, such as uniform or Bartlett kernels based on inverse geodesic distances. Let the cumulative mass of proximities around p be $\Psi_p = \int_{c\in N} \psi(p, c)dc$.

Based on these ingredients, we define the population density in the neighborhood of location p as

$$\tilde{\omega}_p = \Psi_p^{-1} \int_{c\in N} \psi(p, c)\omega_c dc, \tag{2-1}$$

where ω_c defines the population density in location c. The total population around p is simply the integral over ω_c.

As in Reardon and O'Sullivan (2004), this defines a smooth surface of population densities for all different vantage points at a given neighborhood size. The surface forms the basis of the weights used in the index and all spatially-weighted neighborhood quantities will be denoted by '$\tilde{}$'.

Inequality between groups in the neighborhood: We start with the between group component of the spatially-weighted Theil index. Let there be G mutually exclusive population subgroups, which later will denote ethnic groups. The population density of a particular group g around p is $\tilde{\omega}_p^g$, defined implicitly by substituting ω_c^g for ω_c in eq. (2-1). Similarly, the spatially-weighted per capita income of group g in the neighborhood around p is $\tilde{\bar{y}}_p^g = {\Psi_p}^{-1} \int_{c\in N} \psi(p, c)y_c^g dc$, where y_c^g denotes the per capita income of group g in c. The spatially-weighted mean income of the entire neighborhood, $\tilde{\bar{y}}_p$, can be then be obtained by aggregating

[3] Unfortunately, anonymity is also a requirement for establishing the transfer principle, which therefore cannot be satisfied by our index.

[4] Note that throughout the chapter we will treat ethnic groups as distinct population subgroups. Another approach would be to explicitly incorporate some notion of ethnic distance together with physical distance, as Hodler et al. (2017) propose in the case of spatial segregation.

incomes via the spatially-weighted population shares $(\tilde{\omega}_p^g/\tilde{\omega}_p)$.[5]

Between-group entropy in the neighborhood is

$$\tilde{T}_p^b = \sum_{g\in G} \frac{\tilde{\omega}_p^g}{\tilde{\omega}_p} \frac{\tilde{\bar{y}}_p^g}{\tilde{\bar{y}}_p} \ln\left(\frac{\tilde{\bar{y}}_p^g}{\tilde{\bar{y}}_p}\right) = \sum_{g\in G} \frac{\tilde{Y}_p^g}{\tilde{Y}_p}\left[\ln\left(\frac{\tilde{Y}_p^g}{\tilde{Y}_p}\right) - \ln\left(\frac{\tilde{\omega}_p^g}{\tilde{\omega}_p}\right)\right], \tag{2-2}$$

where Y_p (Y_p^g) denotes the total income (of each group) in p.

The index follows the familiar structure of Theil (1967). Contrary to an index which would only consider inequality directly at location p, e.g. a grid cell or some small administrative regions, it additionally takes the neighborhood around p into account but places more emphasis on locations close to p. The second equality shows that we are essentially comparing spatially-weighted income shares to spatially-weighted population shares for each group in the neighborhood. Inequality will be zero if these two are the same. This is precisely how this index differs from measures of spatial segregation where only the second component plays a role.[6]

Inequality within groups in the neighborhood: For each ethnic group, local inequality within the group is defined similarly to that of non-spatial Theil indexes. The only difference is that we have to take the spatial weights into account when constructing the index and the relevant income aggregates.

Let $N_g \subset N$ be the set of locations in N in which group g has a positive density, then the spatially-weighted Theil for group g in the neighborhood of p is

$$\tilde{T}_p^g = (\Psi_p^{N_g})^{-1} \int_{j\in N_g} \psi(p,j) \frac{a_j \omega_j^g y_j^g}{\tilde{a}_p^{N_g} \tilde{\omega}_p^{N_g} \tilde{\bar{y}}_p^{N_g}} \ln\left(\psi(p,j) \frac{a_j \omega_j^g y_j^g}{\tilde{a}_p^{N_g} \tilde{\omega}_p^{N_g} \tilde{\bar{y}}_p^{N_g}}\right) dj \tag{2-3}$$

where a_j is the area of the location and all other quantities with an N_g superscript are defined as before but integration now runs over the subset of locations in N_g.

Since more than one ethnic group may be present in a particular area, the local income shares are then used to aggregate the index for each group to a within-group component for the entire neighborhood around p

$$\tilde{T}_p^w = \sum_{g\in G} \frac{\tilde{\omega}_p^g}{\tilde{\omega}_p} \frac{\tilde{\bar{y}}_p^g}{\tilde{\bar{y}}_p} \tilde{T}_p^g. \tag{2-4}$$

To arrive at overall inequality, we only need to add a weighted Theil index for inequality within each group to the between group component (Bourguignon, 1979; Shorrocks, 1984), that is, $\tilde{T}_p = \tilde{T}_p^b + \tilde{T}_p^w$.

In most of the remainder we will emphasize the between group component, $\tilde{T}_p^b$, as it provides us with an estimate of how different ethnic groups are in the space around p. The within component, $\tilde{T}_p^w$, will play a subordinated role but allow us to test which of these two aspects of inequality is driving our results. As both components can be analyzed directly, we do not use $\tilde{T}_p$ at all. While our paper uses this index to study differences among groups that occupy different areas within a country and uses aggregate income proxies, the index could be useful in a variety

[5] Note that $\sum_{g\in G} \tilde{\omega}_p^g/\tilde{\omega}_p = 1$, so that $\tilde{\bar{y}}_p = \sum_{g\in G} \frac{\tilde{\omega}_p^g}{\tilde{\omega}_p} \tilde{\bar{y}}_p^g$.

[6] In fact, with $\tilde{Y}_p^g = \tilde{Y}_p$ the expression collapses to the spatial segregation index defined in Reardon and O'Sullivan (2004).

of circumstances where groups are defined according to different criteria and the underlying data is observed at the individual level.

2-3 Data and empirical strategy

We use our local inequality indexes to study how spatial inequality between and within ethnic groups magnifies the effects of on social unrest in Africa. The unit of analysis is a $0.5° \times 0.5°$ grid cell (roughly 56×56 km at the equator) for 45 countries. This data structure follows similar papers studying conflict in Africa (e.g. see McGuirk and Burke, 2020; Berman and Couttenier, 2015a; Berman et al., 2017a; Harari and Ferrara, 2018). Here we focus on our main variables of interest, listed below. The summary statistics for all variables used in the main analysis and robustness checks are provided in Table 2-A1 in the Appendix.

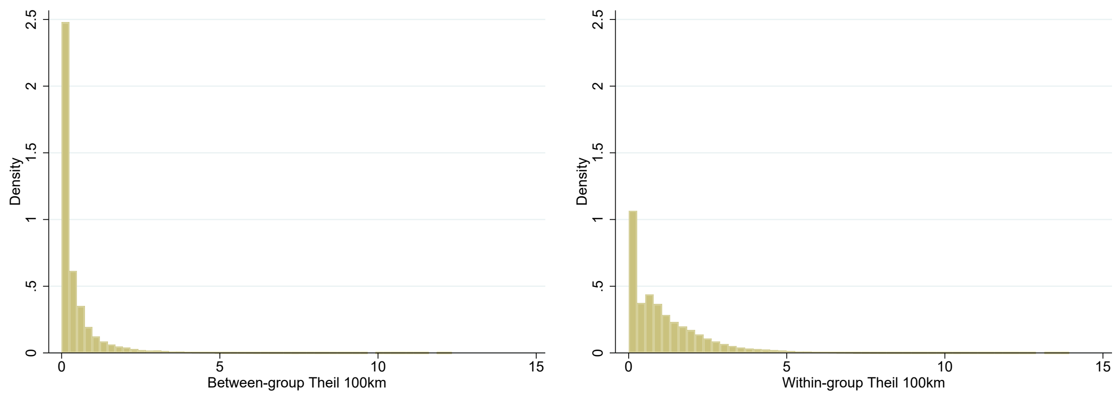
Ethnic inequality. Our primary measure of grievances is local spatial inequality between ethnic groups. Creating this measure poses two challenges. First, household surveys are not undertaken regularly in most of Africa. Second, even if they were, many surveys do not record the ethnic affiliation of the respondent and are not representative at smaller spatial scales. We overcome these challenges by following an burgeoning literature which uses nighttime lights as a proxy for income at the local level together with maps reflecting the spatial distribution of ethnic groups (see, e.g., Cederman et al., 2011; Michalopoulos and Papaioannou, 2013; Alesina et al., 2016). Our approach is closely related to Alesina et al. (2016), who construct Gini coefficients for ethnic inequality across countries based on nighttime lights per capita and ethnic maps. We use their blueprint to construct income proxies and assign them to ethnic territories but then take these data as inputs to our own measures of local spatial inequalities. We proceed in three steps.

We first define the spatial and political entities for which we want to measure between-group differences. Our baseline measures are based on the *Ethnologue* project, which documents the contemporary spatial distribution of about 7000 living language groups across the globe. The project also collects information about each language's ancestors that can be used to construct linguistic trees and aggregate homelands that have a shared language history (more on this below). For robustness checks, we also use data from the Geo-referencing of Ethnic Groups (GREG) data—the digital counterpart of the *Atlas Narodov Mira* assembled by Soviet ethnographers in the early 1960s—and administrative boundaries from the Database of Global Administrative Areas (GADM). As these maps differ in their accuracy and will be combined with other geospatial data at varying resolutions, we first create a medium resolution grid of 5×5 arc minutes, or about 9.3×9.3 km near the equator. We then intersect this grid with country boundaries and the various political entities to obtain a unique attribution of each cell to a particular country and (ethnic) region.[7]

[7]Note that this creates smaller "squiggly" cells at the borders of a country or borders between (ethnic) regions which we take as the unit of observation for the inequality measures. Both *Ethnologue* and *GREG* sometimes assigns an area to multiple groups, while administrative boundaries are non-overlapping. Whenever an area is shared between two groups or more groups, we duplicate the cell for each group. This has the same effect as distributing light and population in equal shares across these areas.

Figure 2-1 – Theil distribution with 100km distance cutoff

(**a**) Ethnologue Between-group Theil 100km (**b**) Ethnologue Within-group Theil 100km



Second, we calculate nighttime lights and population to construct luminosity per capita—our welfare measure—for each cell in the medium resolution grid. The nighttime lights data are from the Defense Meteorological Satellite Program Operation Linescan System (DMSP-OLS) provided by the National Atmospheric and Oceanic Administration (NOAA, 2015). The data are available for every year from 1992 until 2013. The DMSP-OLS system records luminosity as six bit digital numbers ranging from 0 to 63.[8] Although they are well-known problems with these data in terms of bottom and top-coding (Jean et al., 2016; Bluhm and Krause, 2019), they measure luminosity in a consistent manner around the globe. The population data are from the Global Human Settlement Layer (GHSL) project (Pesaresi et al., 2019a). The GHSL data are available for 1975, 1990, 2000 and 2015. We linearly interpolate these data over time to obtain annual estimates of population. We then sum nighttime lights and population using the medium resolution grid.[9]

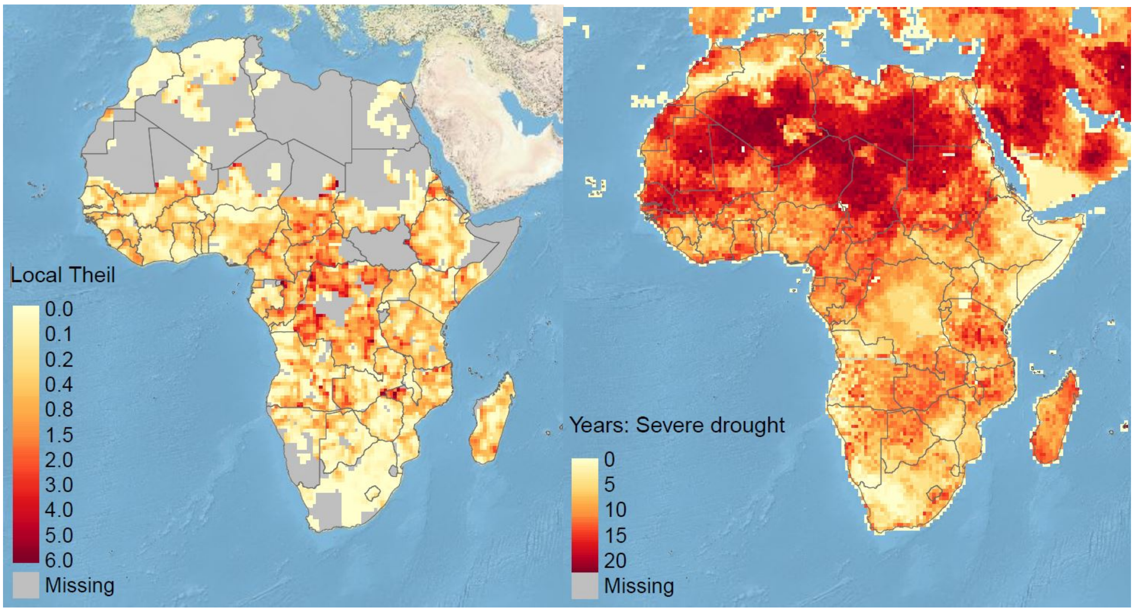
Third, we construct our measures of local inequality between and within ethnic homelands (or regions) for all of Africa over the period from 1992 to 2013. Running these calculation at the medium resolution grid before aggregating to $0.5° \times 0.5°$ using area-weighted averages makes sure that local inequality does not get "averaged out" in the process.[10] A novel feature of our inequality measure is the flexibility of the definition of the neighborhood or, more precisely, the weighting function $\phi(p, q)$. It allows us to determine how far people "look" in their assessment of which inequalities to consider. We do not have a strong prior on how large or small these neighborhood should be. We typically report results for all cells within a radius of 50 km, 100 km,

[8]These images are annual composites of various cloud-free satellite pictures taken twice a day around the globe between 8:30 and 10:00 PM local time.

[9]The native resolution of the DMSP-OLS data is 30 arc seconds, whereas the GHSL data is available at a 250 meter or 1 km resolution with an equal-area projection.

[10]Average light per capita in two $0.5° \times 0.5°$ cells may be exactly the same even though the underlying distribution of light within these cells differs. We preserve inhabited regions with no light by setting their local Theil contribution to zero. Since the local Theil contains a log of the income share, results for zero income shares are undefined. However, we do not observe exact zeros due to bottom-coding and noise in the DMSP-OLS data. All we really know is that the income share of that region becomes vanishingly small. Applying L'Hôpital's rule to the Theil component of such a region, suffices to show that the limit is zero. This is equivalent to replacing observed light or light per capita by arbitrarily small values for regions where zero light is observed. The aggregation to $0.5° \times 0.5°$ cells is done with area weights.

or 200 km. We assume that $\phi(p, q)$ is decreasing in the geodetic distance from cell p to q, leading us to adopt a Bartlett with linearly decreasing weights in the baseline calculations.



(a) 100km Between Theil at ˜55km^2 resolution

(b) SPEIbase drought years

Figure 2-2 – Ethnic inequality and droughts

Figure 2-1 shows that the tail of the within-group component of inequality tends to be wider than the distribution of between-group inequalities. Moreover, changes of at least one unit are well within the range of the data. The left panel of Figure 2-2 presents a map of the average between-group component from 1992 until 2012. It shows that there is substantial spatial heterogeneity in the between-group component. Both are based on a Bartlett kernel with a 100 km distance cutoff. Missing values occur when less than two groups live within the specified proximity.

Droughts. Our primary measure of droughts is the Standardized Precipitation Evapotranspiration Index (SPEI) by Beguería et al. (2010). The SPEI takes the standardized difference between precipitation and the historic average of precipitation, while accounting for potential moisture evaporation of the soil. It is a widely used indicator to capture droughts that allows comparing droughts in rain-rich and rain-poor areas.[11] Droughts are usually classified as "severe" if the SPEI is below -1.5. We use a binary indicator for droughts that is 1 if a cell experienced a month with SPEI values below -1.5 (Tollefsen et al., 2012).

[11]The SPEI has two particular advantages compared to other drought measures. First, unlike the Standard Precipitation Index (SPI) the SPEI is an extension of the SPI by accounting for temperature as well to model evapotranspiration of the soil. While the SPI is suitable for meteorological droughts, droughts driven by warming are unaccounted (Beguería et al., 2013). Thus, the SPEI is a suitable measure for our sample in Africa by taking into account moisture evaporation of the soil. Second, our baseline SPEI index is considered to have a more accurate evapotranspiration model compared than its counterpart reported by Global Drought Monitor, especially when considering climate change (Beguería et al., 2013; Yang et al., 2017).

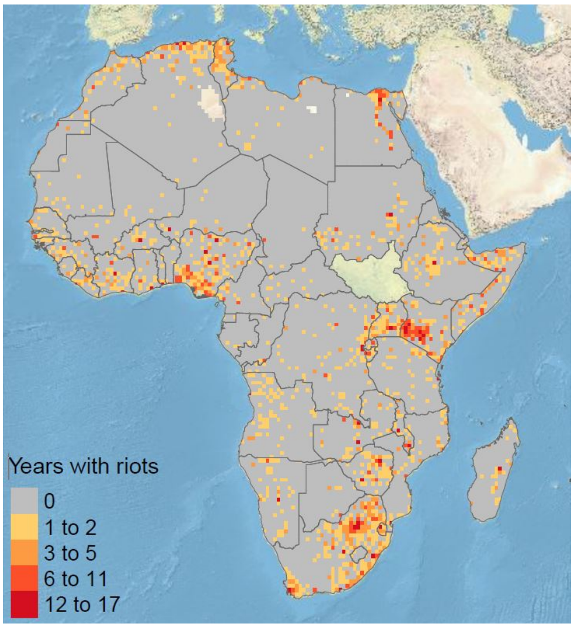


Figure 2-3 – Locations with years of protests

Figure 2-3 illustrate which locations experienced a drought in any month within a calendar year for the entire sample period between 1992 and 2013. The greatest area that is affected by droughts is within the sparsely populated Sahara. However, droughts affect the entire continent and only few areas are considered drought-free.

Riots. Our primary measure of social unrest are 'demonstrations' and 'riots' as recorded by the Social Conflict Analysis Database or SCAD version 3.3 (Salehyan et al., 2012). The database includes protests, riots, strikes, government repression, communal violence, and other forms of unrest. SCAD tracks online news from the Associated Press (AP) and Agence France Presse (AFP) for African countries with a population above 1 million. In this manner, SCAD tracks social disturbances which attract enough people to be reported on in the media. While this will certainly undercount small scale events, it is the only database which consistently collects and geocodes protests in Africa.

Two features make these data better suited for our proposes than other leading databases. First, SCAD does not impose a battle-death threshold. The project classifies events that are not well tracked by other major conflict databases. We use the categories demonstrations and violent riots from their data, no matter if these were planned or spontaneous.[12] These events have in common that they are directed toward members of a distinct group or direct against the government. Second, SCAD geo-references each conflict event to provide an approximate latitude and longitude of its location. The 75% of these events are coded with a precision reflecting the town or settlement level (we present results using subsets of different precision codes in the robustness checks). Table 2-1 shows that protests occur only in a small fraction, less than 1%, of all cell-years but the standard deviation is sizable. Figure 2-2 adds that the protests are spread out over the entire continent with some higher concentration in Nigeria, Kenya, and South Africa.

[12]Note that we often drop the distinction between demonstrations and riots in the remainder and simply refer to them as 'protests'.

Table 2-1 – Summary statistics of selected variables

	Mean	Std. Dev.	Min	Max	Observations
Protests and riots (SCAD)	0.0086	0.0924	0	1	262,108
Between Theil (50 km)	0.3962	0.7247	0.00	12.42	106,908
Between Theil (100 km)	0.4104	0.7396	0.00	12.34	152,241
Between Theil (200 km)	0.4005	0.6854	0.00	13.63	189,530
Within Theil (50 km)	0.7619	1.0263	0.00	12.62	231,308
Within Theil (100 km)	1.1927	1.3007	0.00	13.93	231,308
Within Theil (200 km)	1.3644	1.2481	0.00	14.35	231,308

Empirical strategy. Our analysis exploits two types of identifying variation: within-cell changes in local spatial inequality and within-cell changes in the local effect of income shocks. Droughts are exogenous to riots, but local ethnic inequality (whether within or between groups) could, at least in theory, respond to endogenously to outbreaks of protests and violence. We circumvent this issue by focusing directly on the interaction of income shocks with lagged local inequality levels. This gives rise to a conditional interpretation, where we compare the effect of a drought in regions with high ethnic inequality to the effect in regions with low ethnic inequality. Bun et al. (2014) show that interactions of exogenous shocks with level shifters are identified under mild conditions, without the need for external instruments. Spatial inequality is persistent so that lagging might not fully solve the issue of simultaneous causality. While returning to this issue below, we note here that a strong endogenous response is more plausible in the case of civil wars than for localized protests and riots.

Our specifications of interest are variants of the following form

$$P_{ijt} = \beta_1 D_{ijt} + \beta_2 \tilde{T}^b_{i,j,t-1} + \beta_3 \left(D_{ijt} \times \tilde{T}^b_{i,j,t-1} \right) + \mathbf{x}'_{ijt}\boldsymbol{\delta} + \mu_{ij} + \lambda_{jt} + \epsilon_{ijt} \tag{2-5}$$

where P_{ijt} is an indicator of protests and riots in cell i of country j at time t, $\tilde{T}^b_{i,j,t-1}$ is a measure of between-group inequality in some pre-defined neighborhood (or within-group inequality with superscript w), $\mathbf{x}_{ijt}$ is a vector of controls, μ_{ij} are cell fixed effects and λ_{jt} are country-year effects. We allows for dependencies in ϵ_{ijt} over space and time by clustering the standard errors by a "supergrid" of 2° × 2° cells. In other words, 56 km × 56 km cells near the equator are grouped together in an area which is about 222 km × 222 km. The resulting estimates are robust to heteroskedasticity and serial correlation, spatial correlation, and general variance-covariance misspecification (Lu and Wooldridge, 2017).

Our primary coefficient of interest is β_3. We do not attempt to interpret β_2 causally. We are only interested in β_1 in terms of its size and relation to β_3. There is a separate literature which deals with the unconditional effects of such shocks on conflict. The baseline controls include average log light intensity (per capita) and the log of population. This isolates the neighborhood's economic distribution from the level of income (light) in the cell and the cell-level population. Moreover, using only within-cell variation and allowing for country-year fixed effects purges a lot of potential confounding variation. Our results are net of the effects of time-invariant factors, such as local ethnic heterogeneity or segregation, and economic or political shocks hitting an entire country in a particular year. In fact, the only type of unobserved variation that threatens identification in this context are time-varying omitted variables that affect local protests and droughts simultaneously *and* have different effects in regions with high and low local inequality. It seems implausible that any such variation could be jointly determining the location and duration of droughts together with changes in local inequality. However, we cannot entirely rule out feedback from protests to inequality.

2-4 Results

Baseline results. We report our main results using local inequality between ethnic groups as the relevant cleavage and the incidence of riots as the outcome of interest in Table 2-2. We always present three "neighborhood sizes," doubling from 50 km over 100 km to 200 km. We begin with the regressions without interaction terms in columns (1) to (3). Regardless of the neighborhood size, we find hardly any evidence in favor of the hypotheses that droughts or between-group inequality are associated with an increase in the probability of protests and riots.This is in line with many recent contributions focusing on conflict and climate or inequality more broadly (see e.g., Buhaug et al., 2014, on climate and conflict or Huber and Mayoral, 2019, on between-group vs. within-group inequality and conflict).

Table 2-2 – Local ethnic inequality and protests

	Theil distance					
	50 km	100 km	200 km	50 km	100 km	200 km
Drought (D_t)	-0.0000	-0.0003	-0.0003	-0.0008	-0.0007	-0.0004
	(0.0008)	(0.0006)	(0.0005)	(0.0009)	(0.0007)	(0.0006)
Between Theil ($\tilde{T}^b_{t-1}$)	0.0010	0.0006*	0.0003	0.0001	0.0001	0.0003
	(0.0006)	(0.0003)	(0.0003)	(0.0006)	(0.0004)	(0.0004)
Interaction				0.0018**	0.0010**	0.0002
($D_t \times \tilde{T}^b_{t-1}$)				(0.0009)	(0.0005)	(0.0004)
Cell FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	103,133	147,926	186,395	103,133	147,926	186,395

Notes: The table reports fixed effects regression results. The dependent variable is binary indicator for demonstrations and riots. Standard errors clustered by $2° \times 2°$ cells are reported in parenthesis. Significant at * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

Column (4) to (6) turn to our conditional hypotheses that droughts trigger low-level protest events when ethnic inequality between groups is high. Here we find substantial evidence in favor of such an interaction, especially at smaller neighborhood sizes. Column (4) shows that a two standard deviation increase in between-group inequality measured in the surrounding 50 km increases the probability of experiencing a riot by 0.26 percentage points. Recall that protest incidence across all cells is very low. Only about 0.86% of all cell-years experience a protest or riot so that this is a substantial portion of the baseline effect. The effect also appears to be relatively local, in the sense that it is highest at neighborhood sizes of 50 km and decreases in size and significance towards 200 km. At 100 km, a two standard deviation increase in between-group inequality leads to a 0.15 percentage point higher probability of protest. All in all, the first results suggest that decreased incomes due to droughts may lead to more riots in areas with greater inequality between groups. Droughts thus seem to trigger protests and violence in places with underlying grievances between groups.

2-4.1 Extensions and mechanisms

Within versus between group inequalities. We now examine the role of between-group vs. within-group inequality. Esteban and Ray (2011) show that the intensity of conflict can be linked to inequality (polarization) between and inequality within groups. They show that inequality within groups has two opposing effects. On the one hand, it makes coordination more difficult due to lower group cohesion. On the other hand, it allows for specialization within groups so that richer members can finance the violence carried out by poorer members. In the context of civil wars, they suggest that the latter is more important than the former. Huber and Mayoral (2019) provide evidence from household surveys that within-group inequality is associated with the onset and intensity of civil wars. Our index allows us to study these two components separately and jointly for various neighborhood sizes.

Table 2-3 presents results where we include only the interaction of droughts with inequality within ethnic groups in columns (1) to (3) and then both measures and their interactions in column (4) to (6). There is very little evidence that a higher capacity to organize violence, as proxied by within-group inequality, interacts with droughts. If anything, column (1) suggests a negative interaction which is not robust to also including between-group inequality in column (4). Column (5) and (6) suggest that there could be a minimal effect of inequality within groups on protests in years without severe droughts. However, the interaction of droughts with inequality between ethnic groups remains robust, and its predominantly local effect is virtually unaffected by the inclusion of within-group inequality. This seems plausible in our context. Financing constraints might not be an issue for protests and riots which are inexpensive to carry out. A lack of within-group cohesion, however, is likely to make mobilization more difficult.

Regional "horizontal" inequality. So far, we have assumed that ethnic differences between groups are the prevailing cleavage in Africa. This assumption draws on Alesina et al. (2016) who show that ethnic inequality is an important dimension of underdevelopment and the broader literature on ethnic conflict. A separate literature argues that broader "horizontal" inequalities between regions are important, or potentially more important, for civil conflict and civil unrest (Østby, 2008; Østby et al., 2009; Fjelde and Østby, 2014). Our inequality index can be computed for any regional partition to easily test these arguments in the context of local inequalities and their interaction with droughts.

Table 2-4 shows the corresponding results. We find some evidence of an overall effect of between-region inequality on protest in column (1) to (3), which is also present for non-drought years in columns (5) and (6). However, as before, we are

Table 2-3 – Within-group vs. between-group inequality

	Theil distance					
	50 km	100 km	200 km	50 km	100 km	200 km
Drought (D_t)	-0.0002	-0.0007	-0.0006	-0.0006	-0.0011	-0.0002
	(0.0005)	(0.0005)	(0.0006)	(0.0013)	(0.0010)	(0.0008)
Within Theil ($\tilde{T}^w_{t-1}$)	0.0001	0.0001	0.0003	-0.0001	0.0004*	0.0008**
	(0.0002)	(0.0001)	(0.0002)	(0.0004)	(0.0003)	(0.0003)
Interaction	-0.0006**	0.0001	-0.0000	-0.0001	0.0003	-0.0001
($D_t \times \tilde{T}^w_{t-1}$)	(0.0003)	(0.0002)	(0.0002)	(0.0005)	(0.0003)	(0.0003)
Between Theil				0.0001	0.0003	0.0005
($\tilde{T}^b_{t-1}$)				(0.0006)	(0.0004)	(0.0004)
Interaction				0.0018**	0.0011**	0.0002
($D_t \times \tilde{T}^b_{t-1}$)				(0.0009)	(0.0005)	(0.0004)
Cell FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	229,845	229,845	229,845	103,133	147,926	186,395

Notes: The table reports fixed effects regression results. The dependent variable is binary indicator for demonstrations and riots. Standard errors clustered by $2° \times 2°$ cells are reported in parenthesis. Significant at * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

Table 2-4 – Regional inequalities

	Theil distance					
	50 km	100 km	200 km	50 km	100 km	200 km
Drought (D_t)	-0.0010	-0.0006	-0.0005	-0.0009	-0.0006	-0.0002
	(0.0009)	(0.0006)	(0.0005)	(0.0009)	(0.0007)	(0.0005)
Between Theil	0.0006	0.0010**	0.0009	0.0006	0.0011**	0.0015*
($\tilde{T}^b_{t-1}$)	(0.0005)	(0.0005)	(0.0007)	(0.0006)	(0.0005)	(0.0008)
Interaction				-0.0002	-0.0001	-0.0013**
($D_t \times \tilde{T}^b_{t-1}$)				(0.0010)	(0.0008)	(0.0006)
Cell FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	102,148	156,303	205,948	102,148	156,303	205,948

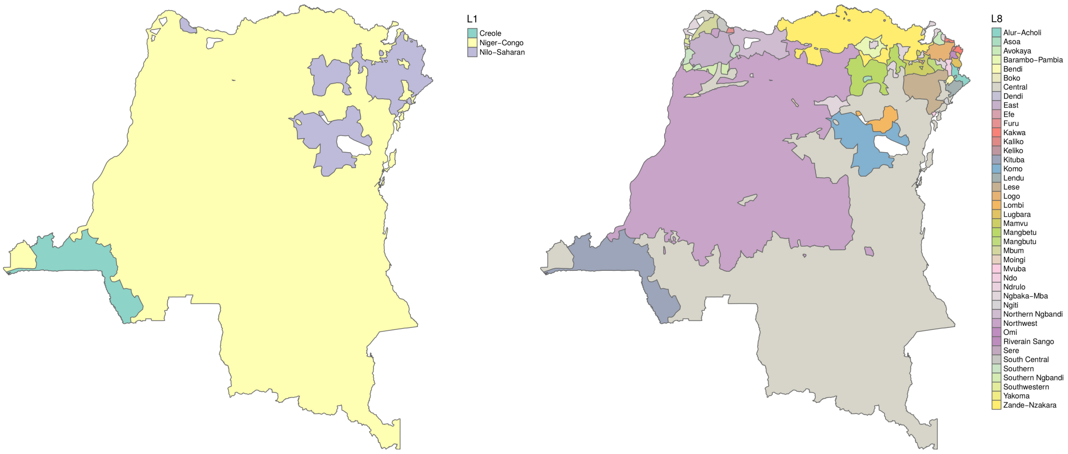
Notes: The table reports fixed effects regression results. The dependent variable is binary indicator for demonstrations and riots. Standard errors clustered by $2° \times 2°$ cells are reported in parenthesis. Significant at * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

hesitant to interpret the inequality variables on their own, as various confounding factors could influence them. The interactions with droughts are less likely to be confounded and paint the opposite picture. If anything, inequality between regions seems to weaken the likelihood of protest, although this only manifests itself at large neighborhood sizes. Local inequality between ethnic groups thus seems to capture underlying grievances in Africa whereas inequality between regions captures something else.

Ethno-linguistic distances. Conflict theory, including the identification-alienation framework by Esteban and Ray (1994), emphasizes differences between groups along identity lines. Contact theory instead states that inter-group contact can reduce the potential for prejudice and conflict, as long as it takes place among groups that enjoy “equal status” (Allport, 1954; Pettigrew, 1998). Apart from physical distance, which is the dimension we have exploited thus far, groups can also differ in terms of their “ethno-linguistic distance”, that is, how related their languages are and when they have separated from one another. Both frameworks predict that more linguistically distant groups, which are less likely to understand each other, are more likely to be in conflict. They are more likely to be perceived as a separate identity group in the identification-alienation framework or as an out-group in contact theory. In line with this, Desmet et al. (2012) provide country-level evidence that group fractionalization (not inequality) at more distant levels of the language tree seems to be more predictive of conflict than fractionalization among less distant groups.

Figure 2-4 – Linguistic homelands of the Congo, DRC

(a) At $l = 1$ **(b)** At $l = 8$

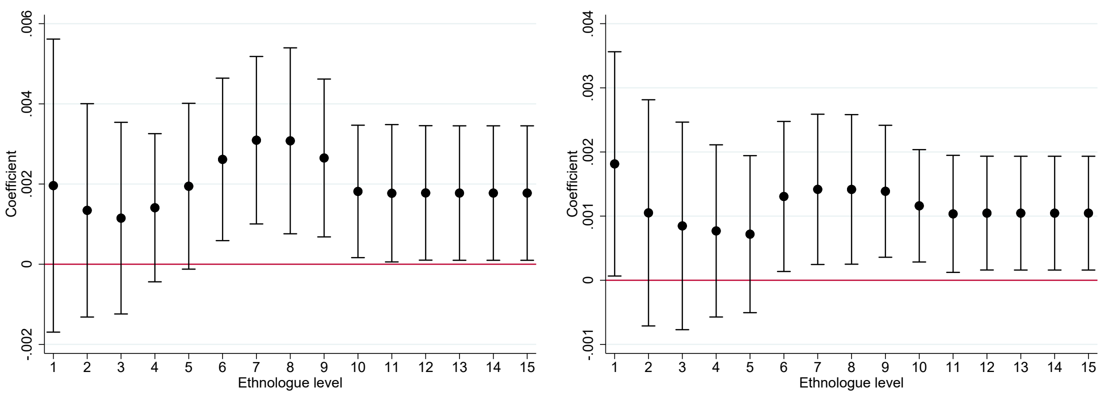


Our approach allows us to account for ethnic distances, in addition to physical distances indirectly. Each pixel in the medium resolution grid not only belongs to a contemporary ethnic group, but the *Ethnologue* project also provides us with the entire phylogenetic tree. The linguistic tree has 15 levels, where the highest level is the least aggregated level of all contemporary divisions, which we have been using throughout our baseline results. As our measure can be sub-group decomposed into any non-overlapping grouping, we aggregate the pixel identities at different levels of the language tree. Figure 2-B2 in the Appendix illustrates the first eight levels of the language tree in the Democratic Republic of the Congo and Figure 2-4 show continuous versions of the ethnic maps we are implicitly constructing in this process.

We run our baseline model with the interaction of droughts and between-group inequality at all 15 levels of the tree to test whether we observe a pattern of increasing or decreasing effect sizes. Figure 2-5 shows that our results support the notion that inequalities among groups that are more distant linguistically, yet live close by, is more strongly associated with protests and riots during severe droughts. At both 50 km and 100 km, the estimated effect is approximately constant for levels 15 to 10 but rises substantially before it becomes indistinguishable from zero at the highest

Figure 2-5 – Coefficient plots of interaction terms

(a) 50 km (b) 100 km



aggregation levels. This last part is an artifact of the data. As we aggregate up, the number of homelands decreases, but their average size increases, making it less and less likely that linguistically distance groups actually live close by. Thus, we take this as tentative evidence that there is indeed a conflict gradient that is increasing in "deeper" ethnic cleavages.

2-4.2 Robustness

Endogeneity of spatial inequality. One concern with our identification strategy is that the interaction term is identified in the presence of omitted variable bias but not when there is feedback from protests to ethnic inequality (Bun et al., 2014). While we have argued that such feedback is more likely with larger-scale conflict, it is possible that destructions from large scale riots and/or subsequent repressions by the government create higher spatial inequality.

Table 2-5 – Initial and average inequality

	Inequality measured in year(s) ...					
	1992			*1992–1996*		
	50 km	100 km	200 km	50 km	100 km	200 km
Drought (D_t)	-0.0002	-0.0007	-0.0004	-0.0010	-0.0013	-0.0008
	(0.0011)	(0.0008)	(0.0006)	(0.0011)	(0.0009)	(0.0008)
Interaction ($D_t \times \tilde{T}_0^b$)	0.0005	0.0010**	0.0003	0.0013**	0.0010**	0.0004
	(0.0007)	(0.0005)	(0.0003)	(0.0006)	(0.0005)	(0.0004)
Cell FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	81,598	129,866	181,302	87,397	122,060	152,116

Notes: The table reports fixed effects regression results. The dependent variable is binary indicator for demonstrations and riots. Standard errors clustered by $2° \times 2°$ cells are reported in parenthesis. Significant at * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

In Table 2-5, we construct two simple tests that rule out the possibility of reverse causality and leverage the persistent nature of ethnic inequality. Given that inequality changes slowly, we can construct time-invariant proxies for spatial inequality based on periods early in the sample and then study subsequent protests and riots. We always include fixed effects for each cell so that the fixed effects absorb the initial level of inequality, and only the interaction is identified. Columns (1) to (3) use only the initial period of the lights data to isolate the persistent component. This is less than ideal, since fewer cells are illuminated early in the sample than later on, and we use only one data point to estimate the static component. Column (4) to (6) improve this by using the average over the first five years of data and then studying protests and riots after 1996. The second set of results is very similar to our baseline estimates, which suggests that reverse causality plays a subordinate role.

Droughts and growing seasons. The literature on droughts and conflict has not settled on a single best way of capturing the effects of droughts. Harari and Ferrara (2018) emphasize the importance of focusing on the growing season of all crops, whereas most contributions in the literature typically use the SPEI index for the entire year (e.g. Almer et al., 2017). While it seems ex ante intuitive to focus on the former, it is not clear that the effect of droughts should be limited to the growing season for several reasons. First, African smallholder farmers tend to grow a diversity of crops (e.g. McCord et al., 2015; Bellon et al., 2020), so that it is difficult to attribute shocks to a single main crop. Second, different crop types have a very different susceptibility when a drought needs to occur to severely affect their growing cycle and what effects a drought has on their yield (groundnuts versus maize). Third, pastoral conflict over water will be unrelated to the growing season (e.g. Almer et al., 2017).

Table 2-6 – Different drought measures, 100 km neighborhood size

	SPEIbase drought measured by ...					
	Start season	End season	Dummy all	Dummy crop	Share all	Share crop
Drought (D_t)	0.0004	-0.0002	-0.0007	-0.0014*	-0.0083	-0.0031
	(0.0004)	(0.0004)	(0.0007)	(0.0008)	(0.0062)	(0.0041)
Between Theil ($\tilde{T}^b_{t-1}$)	0.0006	0.0006	0.0001	0.0004	0.0002	0.0005
	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Interaction	-0.0005*	-0.0001	0.0010**	0.0006	0.0071**	0.0011
($D_t \times \tilde{T}^b_{t-1}$)	(0.0002)	(0.0003)	(0.0005)	(0.0005)	(0.0035)	(0.0023)
Cell FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	143,077	141,086	147,926	147,926	143,092	129,696

Notes: The table reports fixed effects regression results. The dependent variable is binary indicator for demonstrations and riots. Standard errors clustered by $2° \times 2°$ cells are reported in parenthesis. Significant at * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

Table 2-6 reports a variety of perturbations of our primary drought measure. Columns (1) and (2) focus on SPEI values during the start and end of the rainy season. Recall that a SPEI of -1.5 is considered a severe drought. In line with

Almer et al. (2017), we find little evidence that just one specific aspect of droughts is driving our results. The negative coefficients imply that negative SPEI values at the start of the rainy season increase the likelihood of protests, although the effects are weaker. This in line with the notion that a drought presents a bigger problem to farmers when it occurs instead of the anticipated rain. Conversely, droughts at the end of the rainy season are not strongly related with protests as the timing implies a lower susceptibility to crop failure when the rain is expected to abate. Columns (3) repeats the baseline results and column (4) uses our baseline indicator of an SPEI value below -1.5 during the growing season of the cell's main crop. The absence of results suggests that adverse effects on diversified farms and other water-related conflicts seem to matter. We obtain a weaker association between the interaction of interest and protests if we focus only on the main crop in a cell.[13] Columns (5) and (6) mirror these results using the share of months in which the cell experienced a drought (for all crops and the main crop). The effects are strongest with the share measure for all crops. Here a two standard deviation increase in the Theil index in a cell that has experienced droughts in 3 out of 12 months raises the probability of a protest by 0.26 percentage points (or by about 1/3 of the unconditional probability).

Other tests. We conduct a battery of other robustness checks whose detailed results we relegate to the Appendix. Here we only briefly summarize the findings. *i)* Using different conflict data and categories of conflict shows that our effect is specific to non-state actions initiated by civilians and does not predict the involvement of the government (Table 2-C1), *ii)* the results are similar if we use a subset of more precise conflict locations (Table 2-C2), *iii)* they are robust at 100 km for ethnic regions obtained via an alternative ethnic mapping (from GREG) but not significant at 50 km due to the substantially larger size of GREG homelands (Table 2-C3), *iv)* they are robust to using a uniform weighting scheme in the computation of the inequality indexes as opposed to a Bartlett kernel with linearly decreasing weights (Table 2-C4), and *v)* droughts do not capture broader weather shocks, using temperature or precipitation in lieu of droughts does not reveal any conditional or unconditional effects of these variables on protests (Table 2-C5).

2-5 Concluding remarks

This paper shows that droughts trigger social unrest in Africa, but primarily in location with a high degree of local ethnic inequality between groups. We construct a novel spatial inequality measure that calculates between and within-group differences at each location within a given radius. Such spatial variation allows assessing the importance of different reference groups in the formation of grievances.

We find that droughts are more likely to trigger social unrest in locations with high levels of ethnic between-group inequality. Locations with two standard deviations higher inequality levels between ethnic groups during drought years have a significantly higher protest incidence than drought affected locations with no between-group inequality. Second, income differences within smaller geographic

[13]The main crop is defined as having the highest harvested area in a cell (Tollefsen et al., 2012; Portmann et al., 2010)

areas seem to be more relevant in explaining social unrest than differences between groups which are further apart. This suggests that individuals evaluate their deprivation relative to other groups in their vicinity and not to some broader population which they do not directly observe. Third, not all kinds of inequality matter equally. We show that interactions of droughts with inequality between administrative regions or within ethnic regions are not associated with more protest in our data. Finally, the interaction of droughts with inequality between ethnic groups is weakly increasing in the degree of linguistic distance. This suggests that groups speaking languages from different branches of the language trees are more likely to be perceived as out-group members than groups speaking more closely-related languages.

Satellite data of nighttime luminosity enable the application of the local inequality measure on a large scale but also has several weaknesses. Ideally, we would have used georeferenced micro data on the ethnic affiliation or spoken language, income and protest experience of individuals or households over a similar time horizon. Such data is not available for all of Africa. However, data is available for many other potential applications of our local inequality measure. For example, the index could be used to capture intra-urban inequality at the block level.

The IPCC projects that the frequency and magnitude of drought will rise in many parts of the world if global temperature increases by 2° Celsius instead of 1.5° (Hoegh-Guldberg et al., 2018). Our study suggests that this implies social unrest could increase in areas with large economic differences between ethnic groups. Climate change adaptation measures can help people cope with climate change impacts, but could also contribute to lower ethnic grievance by targeting high inequality regions. Some ethnic grievances can be directly addressed by a more equal provision of basic services and public goods.

2-6 Data appendix

2-A1 Additional summary statistics

Table 2-A1 – Summary statistics

	Mean	Std. Dev.	Min	Max	Observations
Panel A. Conflict variables					
Protests and riots (SCAD)	0.0086	0.0924	0	1	262,108
Conflict incidence (GED)	0.0256	0.1578	0	1	263,010
Nonstate conflict (GED)	0.0056	0.0748	0	1	263,010
Statebased conflict (GED)	0.0150	0.1214	0	1	263,010
Onesided conflict (GED)	0.0108	0.1034	0	1	263,010
Panel B. Ethnologue variables					
Between Theil (50 km)	0.3962	0.7247	0.00	12.42	106,908
Between Theil (100 km)	0.4104	0.7396	0.00	12.34	152,241
Between Theil (200 km)	0.4005	0.6854	0.00	13.63	189,530
Within Theil (50 km)	0.7619	1.0263	0.00	12.62	231,308
Within Theil (100 km)	1.1927	1.3007	0.00	13.93	231,308
Within Theil (200 km)	1.3644	1.2481	0.00	14.35	231,308
Log luminosity (DMSP)	-0.9141	5.1575	-4.61	12.31	231,308
Log population (GHSL)	6.6708	6.0881	-4.61	16.62	231,308
Panel C. First-order region variables					
Between Theil (50 km)	0.2276	0.5106	0.00	11.57	108,371
Between Theil (100 km)	0.2372	0.5260	0.00	12.21	165,071
Between Theil (200 km)	0.2428	0.4860	0.00	9.54	216,926
Within Theil (50 km)	0.7454	1.0492	0.00	13.61	262,966
Within Theil (100 km)	1.2083	1.3678	0.00	14.42	262,966
Within Theil (200 km)	1.4421	1.3488	0.00	18.89	262,966
Log luminosity (DMSP)	-1.2554	4.9926	-4.61	12.31	262,966
Log population (GHSL)	5.8537	6.4101	-4.61	16.62	262,966
Panel D. GREG variables					
Between Theil (50 km)	0.2999	0.5974	0.00	12.22	94,302
Between Theil (100 km)	0.2803	0.5663	0.00	11.72	144,024
Between Theil (200 km)	0.2516	0.5178	0.00	11.13	188,084
Within Theil (50 km)	0.7667	1.0631	0.00	13.55	250,712
Within Theil (100 km)	1.2449	1.3763	0.00	14.76	250,712
Within Theil (200 km)	1.4960	1.3483	0.00	18.60	250,712
Log luminosity (DMSP)	-1.1245	5.0587	-4.61	12.84	250,712
Log population (GHSL)	6.1824	6.2869	-4.61	16.62	250,712
Panel D. Other variables					
Log temperature (GHCN+CAMS)	24.6376	3.9257	7.51	39.53	260,626
Log precipitation (GPCC)	701.3178	609.7708	0.12	3,275.41	263,010

2-B2 Additional figures

Figure 2-B1 – Illustration of linear distance weighting for local Theil measure

(a) Group location **(b)** Cell weights

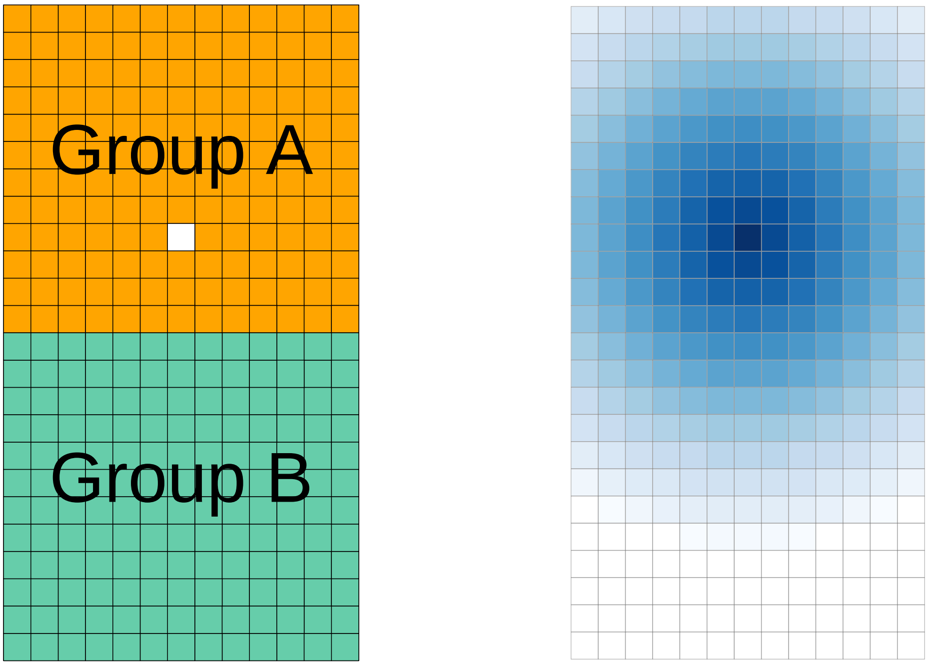
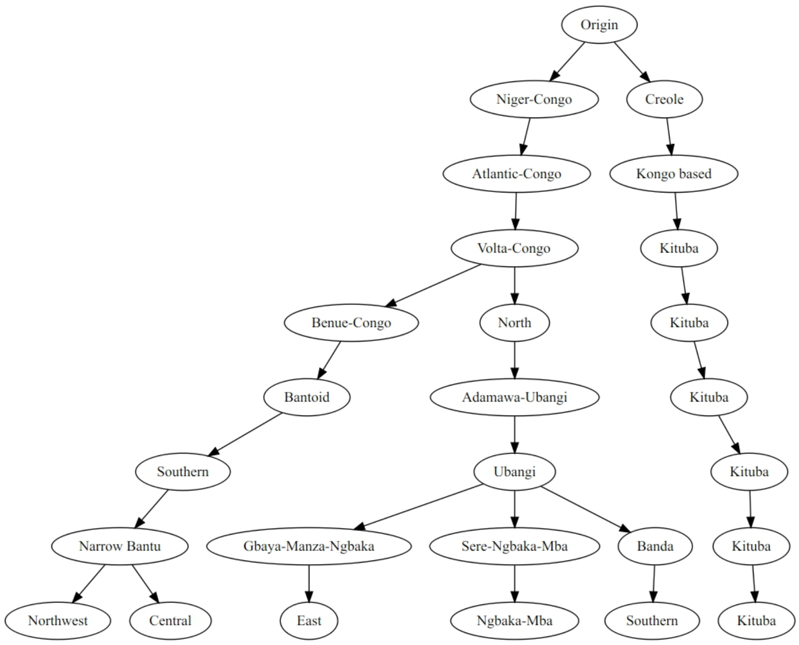


Figure 2-B2 – Linguistic tree of Congo (DRC) up to $l = 8$



Note: The figure shows the linguistic tree of the Democratic Republic of Congo from level 1 to 8. We have added a level 0 (the origin) for display purposes only.

2-7 Analytical appendix

Table 2-C1 – Conflict type, 50 km neighborhood size

	Conflict database and measure ...				
	SCAD repression	GED overall	GED nonstate	GED statebased	GED onesided
Drought (D_t)	0.0068	-0.0001	-0.0015**	0.0014	-0.0016
	(0.0213)	(0.0016)	(0.0007)	(0.0014)	(0.0011)
Between Theil ($\tilde{T}^b_{t-1}$)	-0.0756	0.0011	-0.0013	0.0028**	-0.0004
	(0.0673)	(0.0013)	(0.0008)	(0.0011)	(0.0010)
Interaction ($D_t \times \tilde{T}^b_{t-1}$)	-0.0175	-0.0007	0.0017**	-0.0023*	-0.0006
	(0.0496)	(0.0015)	(0.0008)	(0.0014)	(0.0009)
Cell FE	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Observations	1,776	103,362	103,362	103,362	103,362

Notes: The table reports fixed effects regression results. The dependent variable varies by column. Standard errors clustered by 2° × 2° cells are reported in parenthesis. Significant at * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

Table 2-C2 – SCAD protest with different precision codes

	SCAD precision codes ...					
	1–4			*1–5*		
	50 km	100 km	200 km	50 km	100 km	200 km
Drought (D_t)	-0.0004	-0.0006	-0.0003	-0.0005	-0.0007	-0.0004
	(0.0009)	(0.0007)	(0.0006)	(0.0009)	(0.0007)	(0.0006)
Between Theil	0.0003	0.0001	0.0004	0.0004	0.0001	0.0001
($\tilde{T}^b_{t-1}$)	(0.0004)	(0.0003)	(0.0003)	(0.0005)	(0.0004)	(0.0003)
Interaction	0.0013*	0.0010***	0.0003	0.0013*	0.0010**	0.0003
($D_t \times \tilde{T}^b_{t-1}$)	(0.0007)	(0.0004)	(0.0003)	(0.0008)	(0.0004)	(0.0003)
Cell FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	103,133	147,926	186,395	103,133	147,926	186,395

Notes: The table reports fixed effects regression results. The dependent variable is binary indicator for demonstrations and riots. Standard errors clustered by 2° × 2° cells are reported in parenthesis. Significant at * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

Table 2-C3 – Between-group inequality based on GREG (Atlas Narodov Mira)

	Theil distance					
	50 km	100 km	200 km	50 km	100 km	200 km
Drought (D_t)	-0.0001	-0.0002	-0.0002	-0.0004	-0.0005	-0.0003
	(0.0008)	(0.0006)	(0.0005)	(0.0009)	(0.0007)	(0.0006)
Between Theil ($\tilde{T}^b_{t-1}$)	0.0002	0.0001	-0.0001	-0.0003	-0.0003	-0.0001
	(0.0005)	(0.0003)	(0.0003)	(0.0006)	(0.0003)	(0.0004)
Interaction				0.0010	0.0010**	0.0000
($D_t \times \tilde{T}^b_{t-1}$)				(0.0007)	(0.0005)	(0.0004)
Cell FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	89,246	136,813	179,078	89,246	136,813	179,078

Notes: The table reports fixed effects regression results. The dependent variable is binary indicator for demonstrations and riots. Standard errors clustered by 2° × 2° cells are reported in parenthesis. Significant at * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

Table 2-C4 – Uniform weighting in distance function

	Theil distance					
	50 km	100 km	200 km	50 km	100 km	200km
Drought (D_t)	-0.0000	-0.0003	-0.0003	-0.0009	-0.0007	-0.0004
	(0.0008)	(0.0006)	(0.0005)	(0.0009)	(0.0007)	(0.0006)
Between Theil ($\tilde{T}^b_{t-1}$)	0.0013	0.0012**	0.0007	0.0001	0.0006	0.0006
	(0.0008)	(0.0005)	(0.0004)	(0.0008)	(0.0007)	(0.0006)
Interaction				0.0024**	0.0011*	0.0002
($D_t \times \tilde{T}^b_{t-1}$)				(0.0012)	(0.0007)	(0.0005)
Cell FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	103,133	147,926	186,395	103,133	147,926	186,395

Notes: The table reports fixed effects regression results. The dependent variable is binary indicator for demonstrations and riots. Standard errors clustered by 2° × 2° cells are reported in parenthesis. Significant at * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

Table 2-C5 – Climate

	Climate shock measured by ...					
	Log Temperature			*Log Precipitation*		
	50 km	100 km	200 km	50 km	100 km	200 km
Shock (S_t)	0.0005	0.0003	0.0005	-0.0000	-0.0000	-0.0000
	(0.0006)	(0.0005)	(0.0005)	(0.0000)	(0.0000)	(0.0000)
Between Theil ($\tilde{T}^b_{t-1}$)	0.0052	-0.0011	0.0026	-0.0003	0.0008	0.0012
	(0.0037)	(0.0027)	(0.0031)	(0.0019)	(0.0010)	(0.0007)
Interaction ($S_t \times \tilde{T}^b_{t-1}$)	-0.0002	0.0001	-0.0001	0.0000	-0.0000	-0.0000
	(0.0002)	(0.0001)	(0.0001)	(0.0000)	(0.0000)	(0.0000)
Cell FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	102,217	146,323	184,436	103,133	147,926	186,395

Notes: The table reports fixed effects regression results. The dependent variable is binary indicator for demonstrations and riots. Standard errors clustered by $2° \times 2°$ cells are reported in parenthesis. Significant at * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

Chapter 3

Local inequality

Validating a spatial measure

3-1 Introduction

A large body of literature argues that inequality relates to subjective well-being or creates grievances that contribute to armed conflicts and civil wars (Ray and Esteban, 2017). Especially horizontal inequality, such as inequality between ethnic groups, are critical factors to violence (Østby, 2008). Household-level surveys show clear evidence of inter-personal income differences, also known as *vertical inequality*. However, it is unclear if individuals perceive these objective measures of inequality.[1] What is more, it is unclear how individuals internalize the more abstract concept of horizontal inequality, that is, inequality between identity groups.

This chapter analyzes whether individuals perceive inequality that we measure in the data. Chapter 2 of this thesis presents the construction of a local inequality measure based on differences of nighttime light across ethnic regions. I regress individual responses of perceived inequality on the local measure of ethnic inequality to assess if individuals also recognize ethnic inequality. Moreover, I will test the relationship of perceived inequality and increasing group salience through economic distress in two ways. First, I test if individuals with ethnic self-identification are more likely to observe local inequality than individuals without a clear ethnic awareness. Second, I test if ethnic inequality is more meaningful to inequality perception if individuals are affected by droughts. In this context, droughts could exacerbate existing ethnic grievances, as individuals experience economic distress.

The results suggest that local ethnic between-group inequality mirrors perceived measures of inequality between ethnic groups. A one-standard-deviation increase in the ethnic between-group inequality increases the probability of respondents perceiving their identity group as worse off, or much worse off. Moreover, the relationship between local ethnic inequality relates stronger to inequality perception if individuals have a clear ethnic identity. Finally, droughts worsen the evaluation of group inequality potentially due to increased group alienation or unequal distribution of scarcer resources.

This study contributes to the literature on ethnic inequality and inequality perception by bridging the gap between the two strands. Research on inequality

[1] In this chapter, *objective* or *measured* inequality refers to inequality measures based on observed income data, while *subjective* inequality refers to respondents' evaluation

consequences assumes that actors observe these differences and act accordingly, such as voting for redistribution (Meltzer and Richard, 1981) or participating in violent conflict (Østby, 2008; Cederman et al., 2013; Esteban et al., 2012; Lessmann and Steinkraus, 2019). However, it is unclear how individuals perceive inequality, and if they experience such grievance from objective ethnic inequality measures at all. The results of this study lend faith that individuals assessment of their group's income position is correlated with objective measures of inequality.

Still, inequality perception is subject to a large degree of errors. There seems to be a consensus that individuals' valuation of their income ranking is mostly wrong (Gimpelson and Treisman, 2017) or "self-enchanced", an effect where individuals rank themselves better than the average person (Loughnan et al., 2011). Furthermore, the social milieu or immediate neighborhood are crucial reference groups of an individual's income ranking (Cruces et al., 2013; Evans, 2004). Meanwhile, high inequality perception is correlated with stronger demands for government redistribution (Gimpelson and Treisman, 2017), as well as higher acceptance for violence (Miodownik and Nir, 2015). The relationship between an objective measure of ethnic inequality and perceived ethnic group inequality is not clear-cut, given the systematic bias in inequality assessment of individuals on a personal or group level. Understanding the relationship between ethnic inequality measures by remote-sensing data and survey-based perception values enables further analysis of the influence of ethnic inequality on economic development in a data-scarce environment.

The analysis of inequality perception needs to be distinguished from the literature of subjective well-being, even though the concepts are related. Subjective well-being alone assesses the individual's quality of life (Diener, 1984). Subjective well-being correlates positively with economic inequality for individuals who have a high fairness perception as they attribute inequality with social mobility (Bjørnskov et al., 2013). However, perceived inequality evaluates the experienced difference in livelihood between individuals or identity groups. Thus, a low subjective well-being evaluation may be the outcome of high perceived inequality that eventually could lead to widespread social unrest (Devarajan and Ianchovichina, 2017).

The rest of this chapter is organized as follows. Section 3-2 introduces the underlying data. The empirical strategy is described in Section 3-3. Section 3-4 shows the results on explaining perceived personal and group level inequality. Section 3-5 concludes.

3-2 Data

The unit of observation is the individual level respondent of the Afrobarometer. The Afrobarometer is a survey about public attitudes about different aspects of development, such as governance and livelihood. Afrobarometer conducts these surveys since 1999 for up to 36 African countries depending on the survey round.[2] As such, the Afrobarometer is a repeated cross-sectional dataset on opinions and attitudes, including the perception of livelihood.

I match the individual-level responses to inequality perception with ethnic inequality measures local to the respondent's location, i.e., the grid cell. Table 3-A2

[2]Table 5-A7 provides a full list of countries included in each survey round.

shows the descriptive statistics for the explanatory variables across rounds 1–5.

Inequality perception. The Afrobarometer asks three different questions on perceived inequality. The first question asks about a comparison of their personal living condition compared to other citizens of their country. Similarly, the second and third questions ask about the respondent's economic conditions or ethnic and tribal group compared to other identity or ethnic and tribal groups. Respondents answer both questions on a scale between 1–5. The responses have been harmonized. Thus, the values from 1–5 correspond to "much worse", "worse", "same", "better", "much better". Table 3-1 show the descriptive statistics of the dependent variables along with some control variables. [3]

Table 3-1 – Descriptive statistics of ordered variables

	Ordered opinion variables					
	Much worse	Worse	Same	Better	Much better	Obs
In general, how do you rate: Your living conditions compared to those of other [countrymen]?	0.10	0.28	0.34	0.25	0.03	105,828
Is your [identity group]'s economic conditions worse, the same as, or better than other groups?	0.12	0.23	0.41	0.20	0.05	65,511
In general, how would you describe: Your own present living conditions?	0.19	0.30	0.22	0.26	0.04	93,250
When you look at your life today, how satisfied do you feel compared with one/five year(s) ago	0.11	0.25	0.30	0.30	0.05	109,960
When you look forward at your life's prospects, how satisfied do you expect to be in one year's time?	0.09	0.13	0.16	0.43	0.18	96,038

Interpersonal comparison of livelihood is available for all rounds of the Afrobarometer but conceptionally relates vaguely to the measure of local inequality within or between ethnic groups. The Afrobarometer question asks about a general comparison of the respondent's livelihood to other "countrymen." Thus, the reference point is neither an ethnic group nor any other group type (e.g., occupation, class). However, one possibility to test the relationship between perceived interpersonal inequality and local spatial inequality is to use within-region administrative inequality.

The measure of local ethnic between-group inequality relates most likely to the question on economic conditions compared to other ethnic and tribal groups. Respondents were asked about their identity group and ethnic belonging in an open question without any pre-defined group names. However, the specific framing of the question in rounds 1–2 makes them unusable for this study's purpose.

Rounds 1–2 asks about their identity group's economic conditions compared to others, while rounds 3–4 ask explicitly about the ethnic or tribal economic

[3]Table 3-A1 lists all the questions used from Afrobarometer for each round.

conditions. A closer look at the question on the identity reveals that the questions are not aiming for ethnic identification.[4] The identities stated in rounds 1–2 are about 30% "Language/Tribe/Ethnic," and while identities such as "Religion," "Occupation," and "Class" comprise about 55%.

Rounds 3–4 pose more specific questions on ethnicity and tribal belonging. The specific question on their identity is, "What is your tribe? You know, your ethnic or cultural group." Not surprisingly, answers to this question are much more diverse. While rounds 1–2 have about 16 unique categories (e.g., "Religion," "Occupation," and "Class"), round 3 alone lists about 290 tribal and ethnic identities. The estimates on between-group inequality will be based mostly on questions in rounds 3–4, given these conceptual differences and the fact that round 5 does not ask about the economic situations of a tribe or ethnic group,

Local ethnic inequality. The local inequality measure within and between administrative regions and ethnic groups is elaborated in detail in Chapter 2. Briefly, we develop a local Theil measure that accounts for income per capita within a defined radius of an arbitrary vantage point on a $55 \times 55km^2$ grid. The measure has three features: First, using distance weighting, locations farther away from the vantage point receive less weight than nearby locations. Distance weighting assumes that closer locations stipulate a more meaningful comparison group. Second, the distance threshold is flexible; that is, we test if locations within 50, 100, or 200 km are more meaningful reference points. Third, to proxy for income and population, we use satellite nightlight data and population grids from NOAA (2015); Pesaresi et al. (2019b).[5] The local inequality measure on the grid cell is attributed to each respondent within that cell.

Economic distress. Drought events at the beginning of the rain season are used for evaluating the impact of economic distress on group salience. Beguería et al. (2010) offers a comprehensive database for the Standardized Precipitation Evapotranspiration Index (SPEI) around the globe.[6] The SPEI index calculates standardized differences in precipitation compared to historical average values. This measure is similar to the Standardized Precipitation Index (SPI) but additionally accounts for the evapotranspiration of soil moisture. Hence, the SPEI considers temperature variation as well to compute the severeness of droughts instead of relying on precipitation alone.[7]

[4]The following question is asked before any identity-related question. For example, the specific question for Nigeria is: "We have spoken to many Nigerians and they have all described themselves in different ways. Some people describe themselves in terms of their language, ethnic group, or religion, and others describe themselves in economic terms, such as working class, middle class, or a farmer. Besides being Nigerian, which specific group do you feel you belong to first and foremost?"

[5]The population grid is available for from 1990 until 2015 in a five-year interval. We interpolate the population data linearly and calculate the sum of light and the total population in each cell to get a proxy variable for income per capita.

[6]The SPEI index is available on a spatial resolution of $55 \times 55km^2$ from Tollefsen et al. (2012)

[7]There exist two SPEI measures. One is called SPEIbase with the Penman-Monteith evapotranspiration model, while the SPEI Global Drought Monitor uses the Thornthwaite model of moisture loss. Yang et al. (2017) analyzes the sensitivity of these different measures and conclude that the Thornthwaite is becoming less applicable under climate change situations. Hence, the analysis relies on the SPEIbase.

3-3 Empirical strategy

The analysis aims to measure the degree of how ethnic inequality experienced by an individual is associated with perceived inequality. The main model for Ordinary Least Squares (OLS) and ordered probit regressions is specified as follows:

$$\tilde{Y}_{ipt} = \beta \tilde{T}^{b}_{i,p,t} + \gamma \tilde{T}^{w}_{i,p,t} + \mathbf{x}'_{ipt}\boldsymbol{\delta} + \mu_{it} + \epsilon_{ipt} \tag{3-1}$$

$\tilde{Y}_{ipt}$ is an latent ordered variable on opinion, $\tilde{T}^{b}_{i,p,t-1}$ ($\tilde{T}^{w}_{i,p,t}$) is a measure of between (within) region or group inequalities within an area of 50km, 100km, or 200km from an individual's cell location. μ_{it} are country-year effects. $\mathbf{x}'_{ipt}$ is a vector of controls including cell population, cell light, respondent's gender, age, age^2, urban location indicator, as well as, livelihood in current, past and future situation.

Controlling for two sets of living conditions affecting the Theil variable and the dependent variable itself is crucial for the analysis. First, changes in light amount and population could leverage reporting on how individuals perceive inequality. Controlling for both factors allows an interpretation of the Theil as differences between ethnic groups regardless of increases in per capita light. Second, past, current, and expected livelihood may affect the reporting of perceived inequality as well. For example, respondents may state that they have the same living conditions as others in anticipation and expectation of economic growth even though they may have worse living conditions.

Standard errors are clustered at the cell level.[8] The independent variables are standardized to facilitate interpretation. Thus, each unit increase represents an increase of the respective variable by one standard deviation.

3-4 Results

Each table reports regression coefficients for Theil indices with 50, 100, and 200km distance cut-offs. Given the coding laid out in Table 3-1, a negative sign of the coefficient corresponds to a marginal effect with a worsening of the outcome variable.

Theoretically, individual level and group level inequality may have different relationships between a perceived and an objective measure. In principle, an identity group's actual economic conditions (for example, an ethnic identity group) are expected to be negatively related to perceived differences in economic conditions. That is, an individual may have better living conditions than most of her peer-group, but systematic ethnic differences are likely to be visible. Observing the systematic differences may form some degree of grievance, leading to a lower evaluation of the group's relative economic conditions. In contrast, a high ethnic within-group inequality may be positively related to the perceived evaluation of the group's economic condition. Such a relationship is consistent with attributing the wealth of individuals of the same ethnicity to a collective understanding of ethnic kinship.

[8]Clustering at the cell level accounts for the possibility that individuals from the cell report similar answers to a survey questions over time. Such a pattern would otherwise decrease the information content of each observation within a cell. In contrast, clustering at a higher regional dimension may be undesirable, since each observation has a legitimate perception of their living conditions even if it resides close to each other (e.g., in a neighboring cell). Thus, the information content of each respondent across geographical cells should not be discounted.

Similarly, the relationship between measured and perceived inequality on a personal or within-inequality level is ambiguous as well. On the one hand, higher within-inequality measured objectively could negatively relate to perceived personal inequality because more respondents from the lower end of the income distribution are likely to be sampled in an area with high inequality. On the other hand, an individual could report feeling much better off than the rest of other citizens in an environment with high inequality, because they are aware that they contribute to higher within inequality or *self-enhance* their ranking. Hence, the relationship between relative well-being measured objectively on the individual level and perceived within-inequality could be positive.

3-4.1 Unconditional results

Table 3-2 shows that the relationship between measured and perceived inequality seems to be straightforward. The first three columns report the results on the individual-level assessment of relative living conditions for Theil indices, and columns 4 to 6 show the results on the relative assessment of the economic living conditions of the respondent's identity group compared to other groups. Generally, the perceived differences in livelihood and economic conditions on the personal and group level negatively correlate with the between-group and within-group Theil over the three different distance cut-off points. On the personal level (column 1 to 3), both ethnic inequality types significantly explain the perceived differences. The stronger correlation of the between-group Theil highlights that the general question of comparing their livelihood against "other countrymen" may be understood as a between-identity-group comparison.

Between-group differences seem to explain perceived group differences in economic conditions. Columns 4 to 6 show that isolating identity groups as reference points effectively reduces noise in the variable of perceived inequality. The within-Theil do not exhibit strong explanatory power anymore. In contrast, a one standard deviation increase in the between-group Theil worsens the respondent's evaluation of their group's economic conditions compared to other groups.

OLS regressions, though convenient to interpret, may not be the ideal estimator to explain ordered variables. Hence, Table 3-3 shows the results applying ordered probit estimations. The statistical significance level is nearly identical, which confirms that the OLS results are not subject to biases related to different functional form estimations.

A key advantage of ordered probit regressions is the appropriate marginal effects representation of the explanatory variables. Table 3-4 reports the marginal effects of the ordered probit estimation for the between-group Theil indices only. The last column shows that a one standard deviation increase in the 200km between-group Theil increases the probability of a respondent saying that their identity group is "much worse" off than other groups by 0.5%. This coefficient is about 4% of the probability that respondents rate their group's economic conditions as "much worse."

The estimated relationship appears to have a relatively small economic significance. The literature on inequality perception documents that individuals themselves are unable to identify their relative position in the income distribution. Hence, it is not unsurprising that various factors determine the perception of inequality.

Table 3-2 – Ethnic inequality and perceived relative living conditions (OLS)

	Economic conditions vs. others on ...					
	personal level			*group level*		
Theil distance	50km	100km	200km	50km	100km	200km
Between-Theil$_t$	-0.0152***	-0.0216***	-0.0306***	-0.0231**	-0.0192	-0.0278**
	(0.0057)	(0.0055)	(0.0054)	(0.0116)	(0.0132)	(0.0129)
Within-Theil$_t$	0.0005	-0.0148**	-0.0170**	-0.0050	-0.0221*	-0.0177
	(0.0069)	(0.0065)	(0.0066)	(0.0120)	(0.0126)	(0.0166)
Sum of light$_t$	0.0048	0.0131	0.0192	0.0119	0.0113	0.0124
	(0.0126)	(0.0124)	(0.0125)	(0.0244)	(0.0239)	(0.0236)
Population$_t$	0.0037	-0.0045	-0.0082	0.0266	0.0212	0.0219
	(0.0101)	(0.0100)	(0.0100)	(0.0271)	(0.0260)	(0.0263)
N	62,254	68,658	70,732	37,420	40,924	41,833
Adj. R^2	0.29	0.29	0.29	0.10	0.10	0.10
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes: Dependent variables are ordered variables on respondent's valuation of their economic conditions compared to others (personal-level) or other identity groups (group-level). Standard errors clustered at the cell level in parenthesis. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

Table 3-3 – Ethnic inequality and perceived relative living conditions (Ordered probit)

	Economic conditions vs. others on ...					
	personal level			*group level*		
Theil distance	50km	100km	200km	50km	100km	200km
Between-Theil$_t$	-0.0182**	-0.0264***	-0.0374***	-0.0248*	-0.0206	-0.0298**
	(0.0072)	(0.0068)	(0.0068)	(0.0127)	(0.0144)	(0.0141)
Within-Theil$_t$	0.0009	-0.0186**	-0.0210**	-0.0047	-0.0237*	-0.0190
	(0.0087)	(0.0081)	(0.0082)	(0.0130)	(0.0136)	(0.0180)
Sum of light$_t$	0.0069	0.0178	0.0255	0.0136	0.0129	0.0142
	(0.0168)	(0.0163)	(0.0162)	(0.0265)	(0.0260)	(0.0257)
Population$_t$	0.0029	-0.0078	-0.0125	0.0284	0.0225	0.0232
	(0.0129)	(0.0127)	(0.0127)	(0.0291)	(0.0280)	(0.0284)
N	62,254	68,658	70,732	37,420	40,924	41,833
Pseudo R^2	0.12	0.12	0.12	0.04	0.04	0.04
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes: Dependent variables are ordered variables on respondent's valuation of their economic conditions compared to others (personal-level) or other identity groups (group-level). Standard errors clustered at the cell level in parenthesis. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

3-4.2 Salience of ethnicity

The overall result shows that perceived group inequality can be mapped by the local measures of spatial inequality, regardless of the individual's ethnicity awareness. However, the comparison between ethnic inequality with an equivalent subjective measure must also be related to ethnicity.

I analyze two hypotheses in turn. First, the relationship between the ethnic

Table 3-4 – Ordered probit marginal effects for between-group Theil indices

	Marginal effect: economic conditions vs. others on ...					
	personal level			*group level*		
$T_{Between}$	50km	100km	200km	50km	100km	200km
Much worse	0.0025**	0.0036***	0.0051***	0.0045**	0.0037	0.0053**
	(0.0010)	(0.0009)	(0.0009)	(0.0022)	(0.0025)	(0.0025)
Worse	0.0032**	0.0046***	0.0065***	0.0042*	0.0035	0.0051**
	(0.0013)	(0.0012)	(0.0012)	(0.0022)	(0.0025)	(0.0024)
Same	-0.0005**	-0.0007***	-0.0010***	-0.0013**	-0.0011	-0.0015**
	(0.0002)	(0.0002)	(0.0002)	(0.0007)	(0.0007)	(0.0007)
Better	-0.0040**	-0.0058***	-0.0082***	-0.0049*	-0.0041	-0.0059**
	(0.0016)	(0.0015)	(0.0015)	(0.0025)	(0.0029)	(0.0028)
Much better	-0.0011**	-0.0017***	-0.0024***	-0.0024*	-0.0020	-0.0029**
	(0.0004)	(0.0004)	(0.0004)	(0.0012)	(0.0014)	(0.0014)

Notes: Dependent variables are ordered variables on respondent's valuation of their economic conditions compared to others (personal-level) or other identity groups (group-level). Only the marginal effects of the Between-Theil variables are shown. Standard errors clustered at the cell level in parenthesis. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

Theil measure and the perception measure may be more accurate for persons with an awareness of their ethnicity. One would only expect a relationship between perceived ethnic group differences with a measure of ethnic inequality. Second, the salience of ethnicity may be higher during economic distress. This may be when agents highlight between-group ethnic differences as they experience the unequal effects of a particular shock. Such an increase in animosity has been observed by Schilling et al. (2012). The author finds that droughts were the main reason for higher communal cattle-raiding in the poorest and most marginalized county of Kenya. These attacks eventually lead to stronger distrust between two particular community groups.

The Afrobarometer enables to test the relationship of ethnic awareness and perceived group differences. Round 3 and 4 include questions on the respondent's tribal belonging or ethnic identity. Respondents stated their identity without any pre-defined lists. Respondents stating national identification without regards to ethnicity most likely introduce measurement error as they do not observe ethnic differences. Thus, it is expected that individuals with self-identification to an ethnic or tribal group exhibit a stronger relationship between perceived group differences and ethnic between-group Theil.

Columns 1 to 3 of Table 3-5 report the results to the subset of round 3 and round 4 of the Afrobarometer. Compared to Table 3-2, the unconditional relationship of the 200km Between-Theil is about twice as large for the subset with a precise framing of ethnic and tribes as groups. The variable *Ethnic identification* for columns 4 to 6 is defined as 1 if the respondent self-identifies to a specific tribe or ethnic group. The base variable Between-Theil is statistically insignificant and shows that respondents without tribal or ethnic identification do not connect local ethnic inequality with perceived group differences.[9] In contrast, the interaction term

[9]Non-ethnic identification includes age, gender, occupation, or class, for example.

shows that respondents with an ethnic identification have a strong and statistically significant association between the local ethnic between-group Theil and worse economic conditions for their ethnic group.

Table 3-5 – Ethnic identification

	Economic conditions vs. others on group level					
Theil distance	50km	100km	200km	50km	100km	200km
Between-Theil$_t$ (T_t)	-0.0178	-0.0341***	-0.0457***	-0.0255	0.0088	0.0130
	(0.0132)	(0.0128)	(0.0144)	(0.0177)	(0.0207)	(0.0231)
Ethnic identification (E)				0.0951**	0.1099**	0.1130***
				(0.0441)	(0.0428)	(0.0428)
$T_t \times E$				0.0100	-0.0462**	-0.0624**
				(0.0222)	(0.0213)	(0.0247)
Within-Theil$_t$	0.0044	-0.0052	-0.0119	0.0037	-0.0058	-0.0131
	(0.0131)	(0.0138)	(0.0185)	(0.0131)	(0.0137)	(0.0184)
N	25,976	28,454	29,115	25,976	28,454	29,115
Adj. R^2	0.12	0.12	0.13	0.12	0.12	0.13
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Light and population	✓	✓	✓	✓	✓	✓

Notes: Dependent variable is ordered variable on respondent's valuation of their economic conditions compared to other identity groups (group-level). Standard errors clustered at the cell level in parenthesis. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

The lack of evidence above that respondents without a clear ethnic identification connect perceived inequality with local inequality may be driven by an insufficient number of respondents with non-ethnic identification. Table 3-6 test if local inequality based on administrative regions interplays with ethnic identification. The first three columns show that between-group administrative inequality (i.e., local inequality based on administrative region 1 instead of ethnic regions) shows a strong negative relationship with the respondent's identity group perceived economic well-being. This relationship is in line with ethnic regions overlapping with administrative regions. Crucially, columns 4 to 6 show that the interaction term is statistically insignificant. That is, respondents with ethnic self-identification are not more likely to relate local inequality between administrative regions with worse economic conditions of their ethnic group. However, respondents without a clear ethnic identification still observe administrative between-group inequality as the constituent term for the between-Theil is negative, large, and statistically significant. This highlights that perceived inequality is not only ethnically observed, but the administrative region can also serve as a meaningful reference group for perceived spatial inequality without any reference to ethnicity.

Given that ethnic identification plays a key role in perceived inequality, how fluid is the salience of ethnicity? Economic distress can lead to inter-ethnic tensions and distrust (Schilling et al., 2012). I test this hypothesis by regressing the perceived inequality on the interaction of droughts and local between-group inequality. The marginal effect of droughts is:

$$\frac{\partial Y}{\partial Drought} = \beta_2 + \beta_3 \tilde{T}^b_{i,p,t}$$

Since a drought value of -1.5 indicates a severe drought, $\beta_2 + \beta_3 \tilde{T}^b_{i,p,t} > 0$ would

Table 3-6 – Ethnic identification with GADM1 regions

	Economic conditions vs. others on group level					
Theil distance	50km	100km	200km	50km	100km	200km
Between-Theil$_t$ (T_t)	-0.0083	-0.0704***	-0.1407***	-0.0084	-0.0807**	-0.0960**
	(0.0153)	(0.0212)	(0.0318)	(0.0437)	(0.0354)	(0.0398)
Ethnic identification (E)				0.0928**	0.0933**	0.0835**
				(0.0422)	(0.0390)	(0.0386)
$T_t \times E$				-0.0000	0.0108	-0.0483
				(0.0443)	(0.0327)	(0.0395)
Within-Theil$_t$	-0.0019	-0.0184*	-0.0382***	-0.0021	-0.0180*	-0.0381***
	(0.0132)	(0.0109)	(0.0131)	(0.0132)	(0.0109)	(0.0132)
N	25,046	27,922	29,540	25,046	27,922	29,540
Adj. R^2	0.12	0.13	0.13	0.12	0.13	0.13
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Light and population	✓	✓	✓	✓	✓	✓

Notes: Dependent variable is ordered variable on respondent's valuation of their economic conditions compared to other identity groups (group-level). Standard errors clustered at the cell level in parenthesis. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

indicate that respondents report worse economic conditions of their ethnic group compared to other groups, vice versa.

Table 3-7 shows that there is no evidence that droughts increase the perception of group inequality. However, a Chow test for structural break reveals that the Afrobarometer rounds 1 and 2 are significantly different from round 3 and 4 and may not be estimated in a pooled regression. Moreover, a Chow test does not reject the null hypothesis that there is no structural break between rounds 3 and 4. The structural break is created through the different framing of asking about the differences in the economic conditions of the identity groups. The question in round 1 and 2 are less distinct about ethnic differences, whereas round 3 and round 4 explicitly ask about the respondent's ethnic and tribal identification. Therefore, splitting the sample is justified due to the high measurement error introduced by the broad definition of identity groups in the first two rounds of the Afrobarometer.

There is no evidence that droughts and local ethnic inequality worsen the perception of group differences in the first 4 columns. However, the last two columns show that droughts ($SPEIvalues < 0$) are associated with worse perceived economic conditions of the own identity group compared to other groups. This effect is even stronger, the higher the ethnic between-group inequality.

Both findings suggest that perceived inequality relates to ethnicity in two ways. First, ethnic inequality measured by structural nightlight differences is mirrored in people's perception of ethnic differences. Second, the perception of inequality is not static, but maybe heightened by economic distress. Alternatively, economic distress may increase perceived inequality by an actual increase in ethnic inequality. For example, an economic shock may have unequal effects across ethnic groups in a country. All in all, the results suggest that perceived inequality is a fluid concept.

3-4.3 Sensitivity analysis

Dependent variable. Table 3-B1 further facilitates the interpretation of the effect size. The binary dependent variable is 0 if respondents state that they are

Table 3-7 – Drought impact on group salience

	Group-level economic conditions vs. others					
	rounds 1–2			*rounds 3–4*		
Theil distance	50km	100km	200km	50km	100km	200km
Between-Theil$_t$ (T)	-0.0258*	0.0026	0.0017	-0.0119	-0.0348***	-0.0517***
	(0.0147)	(0.0155)	(0.0175)	(0.0143)	(0.0129)	(0.0152)
Drought$_t$ (D)	-0.0140	-0.0082	-0.0076	0.0703***	0.0589***	0.0492***
	(0.0276)	(0.0263)	(0.0260)	(0.0189)	(0.0179)	(0.0168)
$T_t \times D_t$	0.0196	-0.0128	-0.0058	0.0059	0.0241**	0.0448***
	(0.0182)	(0.0129)	(0.0174)	(0.0125)	(0.0106)	(0.0121)
Within-Theil$_t$	-0.0358**	-0.0575***	-0.0356**	-0.0004	-0.0120	-0.0205
	(0.0158)	(0.0157)	(0.0144)	(0.0133)	(0.0141)	(0.0191)
N	20,317	22,442	22,974	25,148	27,562	28,213
Adj. R^2	0.12	0.12	0.12	0.10	0.11	0.11
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	partial	partial	partial	partial	partial	partial
Light and population	✓	✓	✓	✓	✓	✓

Notes: Dependent variable is ordered variable on respondent's valuation of their economic conditions compared to other identity groups (group-level). The control variable do not include the respondents current absolute evaluation of living standards to include Afrobarometer 1 in the analysis. Table 3-B8 shows that the results are very similar if including the full set of control variables but dropping round 1. Standard errors clustered at the cell level in parenthesis. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

"much worse" or "worse" off than other groups, and 1 for answering "same," "better," and "much better." The last column highlights that a one standard deviation increase in the 200km between-group Theil increases the probability that respondents feel much worse or worse off compared to other groups by around 1.4%. The results of Table 3-B7 account for the structural break in the perceived group-level inequality between rounds 2 and 3–4. The interpretation of the unconditional effect of Table 3-2 does not change.

Ethnic inequality. The results are robust to a different definition of ethnicity by using ethnic homelands defined by GREG, a digital version of the Soviet Atlas Narodov Mira (Table 3-B5). Moreover, the main results are also robust to a uniform weighting scheme of the local inequality variable. This means that the results of Table 3-B6 include local ethnic inequality measures where each cell receives an equal weight within the distance cut-off point instead of a linear decay.

Control variables Table 3-B2, Table 3-B3, and Table 3-B4 are correspond to Table 3-2, Table 3-3, and Table 3-4. The regressions include the additional control variable of the binary indicator of the respondent's ethnic self-identification. The magnitudes are generally greater due a reduction on measurement error.

Drought measurement. Table 3-B9 uses the SPEI global drought monitor data with the Thornthwaite model of moisture loss. The interaction relationship is generally weaker but remains significant for the 200km ethnic-between group inequality. Table 3-B10 adds a control variable for the ethnic identification for results on the SPEIbase and SPEIgdm drought indicator. Controlling for ethnicity increases the size of the coefficients by a small degree.

3-5 Concluding remarks

This chapter aims to understand if and how objective an ethnic inequality measure mirrors individuals' inequality perception. Moreover, I test if the relationship between the objective and subjective inequality measures change with individuals' awareness of their ethnicity and experience of economic distress.

The results show that an increase of the local between-group within the individual's living environment is negatively correlated with the individual's evaluation of her ethnic group's economic condition compared to other groups. The relationship appears to be relatively small in economic terms. However, an objective measure of inequality between ethnic groups will not reflect inequality between groups if the group identity is not ethnic in itself. The results show that ethnic self-identification is a crucial factor for such a relationship. Finally, the relationship between the objective and subjective measures tends to be higher during drought periods at the start of the raining season. This finding is consistent with the loss of crops or increased tensions due to water scarcity and higher salience of ethnicity as a group marker.

The results highlight that ethnicity is an important group marker and that the link between the objective and subjective measure of ethnic inequality becomes stronger with economic distress. This finding has some particular implications for the so-called grievance literature that links horizontal inequality to social unrest. It is not only the case that ethnic inequality can materialize as an actual grievance for ethnic individuals. However, there is the potential that economic shocks deepen the division between groups as resources become scarcer (see Döring, 2020; Schilling et al., 2012). Therefore, this study suggests that future research on ethnic inequality and social unrest should not discard the opportunity costs channel. Economic shocks, including climate change-induced natural disasters, may increase the tension between ethnic groups.

3-6 Data appendix

Table 3-A1 – Overview of Afrobarometer questions by questionnaire rounds

		Afrobarometer round ...				
No.	Question	1	2	3	4	5
1	In general, how do you rate: Your living conditions compared to those of other [countrymen]?	pfeerd	q2B	q5	q5	q4
2	Are [members of your identity group]'s economic conditions worse, the same as, or better than other groups in this country?	pfegrp	q55	-	-	-
3	Think about the condition of [respondent's ethnic group] Are their economic conditions worse, the same as, or better than other groups in this country?	-	-	q80A	q80	-
4	What is your tribe? You know, your ethnic or cultural group	-	-	q79	q79	q84
5	In general, how would you describe: Your own present living conditions?	-	q1B	q4B	q4B	q3B
6	When you look at your life today, how satisfied do you feel compared with one/five year(s) ago	pfepas	q3B	q6B	q6B	q5B
7	When you look forward at your life's prospects, how satisfied do you expect to be in one year's time?	pfefut	q4B	q7B	q7B	q6B
8	Respondent's age	age	q80	q1	q1	q1
9	Urban (1=Urban)	urbrur	urbrur	urbrur	urbrur	urbrur
10	Respondent's gender (1=Male)	gender	q96	q101	q101	q101

3-7 Analytical Appendix

3-B1 Sensitivity of main results

Table 3-A2 – Descriptive statistics

	Mean	Std. Dev.	Min	Max	Obs
Ethnologue					
Between T_{50km}	-0.00	1.00	-0.56	13.87	92,261
Between T_{100km}	-0.00	1.00	-0.63	14.57	101,482
Between T_{200km}	-0.00	1.00	-0.83	11.76	104,245
Within T_{50km}	-0.00	1.00	-1.26	8.33	110,150
Within T_{100km}	0.00	1.00	-1.35	9.05	110,150
Within T_{200km}	0.00	1.00	-1.54	14.33	110,150
Sum of cell light	-0.00	1.00	-0.40	9.56	110,150
Total cell population	0.00	1.00	-0.56	12.27	110,150
GREG					
Between T_{50km}	0.00	1.00	-0.50	14.38	77,578
Between T_{100km}	0.00	1.00	-0.53	21.50	90,658
Between T_{200km}	-0.00	1.00	-0.62	32.90	99,021
Within T_{50km}	0.00	1.00	-1.26	8.04	109,846
Within T_{100km}	-0.00	1.00	-1.34	10.06	109,846
Within T_{200km}	-0.00	1.00	-1.51	14.49	109,846
Sum of cell light	-0.00	1.00	-0.32	10.61	109,846
Total cell population	-0.00	1.00	-0.53	11.05	109,846
ADM1					
Between T_{50km}	-0.00	1.00	-0.60	12.89	95,814
Between T_{100km}	-0.00	1.00	-0.67	15.90	105,287
Between T_{200km}	-0.00	1.00	-0.78	6.72	110,151
Within T_{50km}	-0.00	1.00	-1.22	9.12	110,524
Within T_{100km}	-0.00	1.00	-1.27	11.50	110,524
Within T_{200km}	0.00	1.00	-1.41	13.43	110,524
Sum of cell light	0.00	1.00	-0.39	9.72	110,524
Total cell population	0.00	1.00	-0.55	12.99	110,524
Afrobarometer variables					
Respondent's age	36.36	14.42	0	130	109,433
Urban indicator	0.39	0.49	0	1	110,331
Respondent's gender	0.50	0.50	0	1	111,101
Ethnic self-identification	0.49	0.50	0	1	73,674
Drought indicator					
SPEIbase at start period	-0.00	0.93	-3.92	2.80	103,493
SPEIgdm at start period	-0.17	1.01	-3.92	5.92	106,395
SPEIbase at end period	-0.07	0.98	-2.68	2.73	99,486
SPEIgdm at end period	-0.24	1.03	-2.95	4.26	106,403

Table 3-B1 – Main with dummy dependent variable

	Economic conditions vs. others on ...					
	personal level			*group level*		
Theil distance	50km	100km	200km	50km	100km	200km
Between-Theil$_t$	-0.0111***	-0.0134***	-0.0197***	-0.0097*	-0.0089*	-0.0138**
	(0.0027)	(0.0028)	(0.0030)	(0.0050)	(0.0053)	(0.0057)
Within-Theil$_t$	0.0003	-0.0048	-0.0067**	-0.0108**	-0.0146***	-0.0108
	(0.0031)	(0.0030)	(0.0033)	(0.0053)	(0.0056)	(0.0073)
Sum of light$_t$	0.0014	0.0045	0.0063	-0.0029	-0.0028	-0.0030
	(0.0048)	(0.0051)	(0.0054)	(0.0083)	(0.0081)	(0.0080)
Population$_t$	0.0051	0.0024	0.0008	0.0155	0.0140	0.0151
	(0.0050)	(0.0050)	(0.0048)	(0.0116)	(0.0113)	(0.0113)
N	62,254	68,658	70,732	37,420	40,924	41,833
Adj. R^2	0.23	0.23	0.23	0.11	0.11	0.12
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes: Dependent variables are a binary variables indicating 0 if respondents value their economic conditions 'much worse' or 'worse' and 1 if 'same', 'better', or 'much better' compared to others (personal-level) or other identity groups (group-level). Standard errors clustered at the cell level in parenthesis. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

Table 3-B2 – Main with tribe control

	Economic conditions vs. others on ...					
	personal level			*group level*		
Theil distance	50km	100km	200km	50km	100km	200km
Between-Theil$_t$	-0.0184***	-0.0266***	-0.0382***	-0.0163	-0.0328**	-0.0441***
	(0.0069)	(0.0069)	(0.0064)	(0.0133)	(0.0129)	(0.0145)
Within-Theil$_t$	-0.0035	-0.0192***	-0.0242***	0.0037	-0.0056	-0.0124
	(0.0080)	(0.0072)	(0.0074)	(0.0131)	(0.0138)	(0.0185)
Sum of light$_t$	0.0147	0.0216	0.0308**	0.0131	0.0080	0.0114
	(0.0151)	(0.0150)	(0.0157)	(0.0290)	(0.0291)	(0.0288)
Population$_t$	-0.0019	-0.0095	-0.0153	0.0132	0.0092	0.0045
	(0.0109)	(0.0109)	(0.0109)	(0.0313)	(0.0307)	(0.0306)
N	49,111	54,304	56,098	25,976	28,454	29,115
Adj. R^2	0.28	0.28	0.28	0.12	0.12	0.13
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes: Dependent variables are ordered variables on respondent's valuation of their economic conditions compared to others (personal-level) or other identity groups (group-level). Standard errors clustered at the cell level in parenthesis. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

Table 3-B3 – Ordered probit with tribe

	Economic conditions vs. others on ...					
	personal level			*group level*		
Theil distance	50km	100km	200km	50km	100km	200km
Between-Theil$_t$	-0.0223**	-0.0332***	-0.0472***	-0.0186	-0.0380**	-0.0513***
	(0.0088)	(0.0086)	(0.0081)	(0.0154)	(0.0150)	(0.0168)
Within-Theil$_t$	-0.0043	-0.0243***	-0.0303***	0.0049	-0.0059	-0.0138
	(0.0102)	(0.0091)	(0.0094)	(0.0151)	(0.0159)	(0.0214)
Sum of light$_t$	0.0203	0.0295	0.0412**	0.0167	0.0106	0.0147
	(0.0192)	(0.0191)	(0.0199)	(0.0334)	(0.0336)	(0.0333)
Population$_t$	-0.0041	-0.0142	-0.0216	0.0142	0.0096	0.0040
	(0.0139)	(0.0138)	(0.0139)	(0.0357)	(0.0351)	(0.0350)
N	49,111	54,304	56,098	25,976	28,454	29,115
Pseudo R^2	0.12	0.12	0.12	0.05	0.05	0.05
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes: Dependent variables are ordered variables on respondent's valuation of their economic conditions compared to others (personal-level) or other identity groups (group-level). Standard errors clustered at the cell level in parenthesis. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

Table 3-B4 – Ordered probit marginal effects with tribe

	Marginal effect: economic conditions vs. others on ...					
	personal level			*group level*		
$T_{Between}$	50km	100km	200km	50km	100km	200km
Much worse	0.0032**	0.0047***	0.0066***	0.0031	0.0063***	0.0085***
	(0.0012)	(0.0012)	(0.0011)	(0.0026)	(0.0024)	(0.0028)
Worse	0.0039**	0.0057***	0.0082***	0.0032	0.0065**	0.0087***
	(0.0015)	(0.0015)	(0.0014)	(0.0027)	(0.0026)	(0.0029)
Same	-0.0010**	-0.0013***	-0.0018***	-0.0012	-0.0023***	-0.0031***
	(0.0004)	(0.0003)	(0.0003)	(0.0010)	(0.0009)	(0.0010)
Better	-0.0049**	-0.0073***	-0.0103***	-0.0036	-0.0073**	-0.0098***
	(0.0019)	(0.0019)	(0.0018)	(0.0030)	(0.0029)	(0.0032)
Much better	-0.0012**	-0.0018***	-0.0026***	-0.0016	-0.0032**	-0.0044***
	(0.0005)	(0.0005)	(0.0005)	(0.0013)	(0.0013)	(0.0015)

Notes: Dependent variables are ordered variables on respondent's valuation of their economic conditions compared to others (personal-level) or other identity groups (group-level). Only the marginal effects of the Between-Theil variables are shown. Standard errors clustered at the cell level in parenthesis. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

Table 3-B5 – Main table with GREG regions

	Economic conditions vs. others on ...					
	personal level			*group level*		
Theil distance	50km	100km	200km	50km	100km	200km
Between-Theil$_t$	-0.0125**	-0.0255***	-0.0244***	-0.0009	-0.0204*	-0.0382**
	(0.0059)	(0.0056)	(0.0061)	(0.0173)	(0.0124)	(0.0174)
Within-Theil$_t$	0.0010	-0.0109	-0.0205**	-0.0136	-0.0211	-0.0205
	(0.0081)	(0.0072)	(0.0080)	(0.0160)	(0.0152)	(0.0177)
Sum of light$_t$	-0.0125	-0.0041	-0.0024	0.0075	0.0088	0.0038
	(0.0119)	(0.0108)	(0.0107)	(0.0207)	(0.0198)	(0.0205)
Population$_t$	0.0247**	0.0140	0.0127	0.0181	0.0132	0.0189
	(0.0121)	(0.0113)	(0.0114)	(0.0280)	(0.0248)	(0.0256)
N	53,237	61,616	67,281	31,708	36,575	39,123
Adj. R^2	0.29	0.29	0.29	0.10	0.10	0.10
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes: Dependent variables are ordered variables on respondent's valuation of their economic conditions compared to others (personal-level) or other identity groups (group-level). Standard errors clustered at the cell level in parenthesis. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

Table 3-B6 – Main results with uniform distance weigthing

	Economic conditions vs. others on ...					
	personal level			*group level*		
Theil distance	50ukm	100ukm	200ukm	50ukm	100ukm	200ukm
Between-Theil$_{t-1}$	-0.0169***	-0.0239***	-0.0314***	-0.0211*	-0.0174	-0.0229*
	(0.0054)	(0.0054)	(0.0059)	(0.0119)	(0.0131)	(0.0130)
Within-Theil$_{t-1}$	0.0025	-0.0148**	-0.0132**	-0.0058	-0.0226*	-0.0036
	(0.0069)	(0.0064)	(0.0064)	(0.0132)	(0.0130)	(0.0147)
Sum of light$_t$	0.0048	0.0134	0.0189	0.0118	0.0108	0.0085
	(0.0126)	(0.0124)	(0.0127)	(0.0245)	(0.0240)	(0.0235)
Population$_t$	0.0034	-0.0051	-0.0065	0.0262	0.0223	0.0324
	(0.0101)	(0.0101)	(0.0105)	(0.0271)	(0.0263)	(0.0269)
N	62,254	68,658	70,732	37,420	40,924	41,833
Adj. R^2	0.29	0.29	0.29	0.10	0.10	0.10
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes: Dependent variables are ordered variables on respondent's valuation of their economic conditions compared to others (personal-level) or other identity groups (group-level). Standard errors clustered at the cell level in parenthesis. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

Table 3-B7 – Main with split sample

	Group-level economic conditions vs. others					
	rounds 2			*rounds 3–4*		
Theil distance	50km	100km	200km	50km	100km	200km
Between-Theil_{t}	-0.0374**	0.0125	0.0180	-0.0178	-0.0341***	-0.0457***
	(0.0183)	(0.0190)	(0.0226)	(0.0132)	(0.0128)	(0.0144)
Within-Theil_{t}	-0.0328	-0.0673***	-0.0297	0.0044	-0.0052	-0.0119
	(0.0222)	(0.0221)	(0.0244)	(0.0131)	(0.0138)	(0.0185)
Sum of light_{t}	0.0026	0.0059	-0.0011	0.0122	0.0073	0.0108
	(0.0359)	(0.0353)	(0.0352)	(0.0290)	(0.0291)	(0.0288)
Population_{t}	0.0696	0.0650	0.0877	0.0153	0.0108	0.0058
	(0.0661)	(0.0649)	(0.0660)	(0.0312)	(0.0306)	(0.0305)
N	11,444	12,470	12,718	25,976	28,454	29,115
Adj. R^2	0.07	0.07	0.07	0.12	0.12	0.13
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes: Dependent variables are a binary variables indicating 0 if respondents value their economic conditions 'much worse' or 'worse' and 1 if 'same', 'better', or 'much better' compared to others (personal-level) or other identity groups (group-level). Standard errors clustered at the cell level in parenthesis. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

3-B2 Sensitivity test of interaction results

Table 3-B8 – Drought impact on group salience with full control set

	Group-level economic conditions vs. others					
	round 2			*rounds 3–4*		
Theil distance	50km	100km	200km	50km	100km	200km
Between-Theil$_t$ (T)	-0.0416*	0.0094	0.0110	-0.0146	-0.0363***	-0.0525***
	(0.0226)	(0.0190)	(0.0241)	(0.0137)	(0.0125)	(0.0146)
Drought$_t$ (D)	-0.0096	0.0057	0.0014	0.0660***	0.0554***	0.0466***
	(0.0419)	(0.0413)	(0.0407)	(0.0179)	(0.0169)	(0.0159)
$T_t \times D_t$	-0.0071	-0.0250*	-0.0295	0.0067	0.0240**	0.0432***
	(0.0275)	(0.0150)	(0.0221)	(0.0120)	(0.0104)	(0.0116)
Within-Theil$_t$	-0.0350	-0.0714***	-0.0355	-0.0021	-0.0154	-0.0242
	(0.0227)	(0.0224)	(0.0251)	(0.0129)	(0.0137)	(0.0184)
N	11,243	12,224	12,472	25,085	27,494	28,141
Adj. R^2	0.07	0.07	0.07	0.12	0.13	0.13
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Light and population	✓	✓	✓	✓	✓	✓

Notes: Dependent variable is ordered variable on respondent's valuation of their economic conditions compared to other identity groups (group-level). Standard errors clustered at the cell level in parenthesis. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

Table 3-B9 – Drought impact on group salience (SPEIgdm)

	Group-level economic conditions vs. others					
	round 1–2			*rounds 3–4*		
Theil distance	50km	100km	200km	50km	100km	200km
Between-Theil$_t$ (T)	-0.0268**	0.0034	0.0097	-0.0105	-0.0251**	-0.0377**
	(0.0136)	(0.0145)	(0.0169)	(0.0137)	(0.0126)	(0.0150)
Drought$_t$ (D)	-0.0315*	-0.0238	-0.0244	0.0395**	0.0315**	0.0271**
	(0.0176)	(0.0161)	(0.0159)	(0.0158)	(0.0148)	(0.0138)
$T_t \times D_t$	0.0038	-0.0128	-0.0137	0.0139	0.0154	0.0218**
	(0.0120)	(0.0121)	(0.0135)	(0.0104)	(0.0096)	(0.0094)
Within-Theil$_t$	-0.0298*	-0.0505***	-0.0302**	0.0045	-0.0027	-0.0056
	(0.0160)	(0.0158)	(0.0139)	(0.0130)	(0.0138)	(0.0188)
N	21,028	23,180	23,721	26,042	28,497	29,162
Adj. R^2	0.11	0.12	0.12	0.10	0.10	0.11
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	partial	partial	partial	partial	partial	partial
Light and population	✓	✓	✓	✓	✓	✓

Notes: Dependent variable is ordered variable on respondent's valuation of their economic conditions compared to other identity groups (group-level). The control variable do not include the respondents current absolute evaluation of living standards to include Afrobarometer 1 in the analysis. Standard errors clustered at the cell level in parenthesis. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

Table 3-B10 – Drought impact on group salience

	Group-level economic conditions vs. others					
	SPEIbase			*SPEIgdm*		
Theil distance	50km	100km	200km	50km	100km	200km
Between-Theil$_{\mathrm{t}}$ (T)	-0.0129	-0.0347***	-0.0506***	-0.0115	-0.0249**	-0.0375**
	(0.0138)	(0.0127)	(0.0148)	(0.0134)	(0.0122)	(0.0146)
Drought$_{\mathrm{t}}$ (D)	0.0652***	0.0547***	0.0461***	0.0390**	0.0309**	0.0268**
	(0.0180)	(0.0170)	(0.0159)	(0.0153)	(0.0142)	(0.0133)
$\mathrm{T_t} \times D_{\mathrm{t}}$	0.0062	0.0236**	0.0430***	0.0160	0.0165*	0.0212**
	(0.0119)	(0.0104)	(0.0116)	(0.0101)	(0.0097)	(0.0093)
Within-Theil$_{\mathrm{t}}$	-0.0026	-0.0157	-0.0245	0.0012	-0.0076	-0.0106
	(0.0129)	(0.0137)	(0.0184)	(0.0125)	(0.0135)	(0.0181)
Ethnic identification	0.1119***	0.0916**	0.0824**	0.1031**	0.0884**	0.0774*
	(0.0408)	(0.0396)	(0.0397)	(0.0413)	(0.0398)	(0.0397)
N	25,085	27,494	28,141	25,976	28,426	29,087
Adj. R^2	0.12	0.13	0.13	0.12	0.12	0.13
Country-Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Light and population	✓	✓	✓	✓	✓	✓

Notes: Dependent variable is ordered variable on respondent's valuation of their economic conditions compared to other identity groups (group-level). Standard errors clustered at the cell level in parenthesis. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

Chapter 4

Armed groups in conflict

*Competition and political violence in Pakistan**

4-1 Introduction

The proliferation of armed groups is often associated with a rise of organized political violence[1] and failing states. Prominent examples include Libya and Syria since 2011, or the Democratic Republic of Congo during the Great War of Africa.[2] An additional armed group can destabilize the status quo by threatening the influence of incumbent groups and the government. The threat can be amplified if the additional group claims to fight for the same cause as an established group. In such cases, the additional group not only challenges the monopoly of violence from existing players but threatens their distinct support base, e.g., financial supporters and recruits. A prominent example is the appearance of Hamas in the Gaza strip and the West Bank challenging the Palestinian Liberation Organization (PLO) as the sole agent of the Palestinians.[3]

Empirical evidence is still limited, despite strong priors that more armed groups intensify political violence. Currently, the literature can only report positive correlations between the number of armed groups and the frequency and severity of political violence.[4] The main problem in estimating the causal effect of an increase in the number of armed groups on political violence is that the number of armed groups within a given geographic area is endogenous. First and foremost, groups most likely actively select themselves into given areas (Gaibulloev, 2015). The selection in turn reasonably depends on the strength of incumbent actors as well as attributes inert to the area in question.[5] Second, groups have different goals and strategies, respond

*This chapter is based on joint work with Martin Gassebner and Paul Schaudt (Gassebner et al., 2020).

[1]We use the term organized political violence as a general term for politically motivated violence, such as civil war, terrorism, and counter-insurgencies.

[2]Taken to the extreme, proliferation of armed actors means a war of everyone against everyone, famously making life "solitary, poor, nasty, brutish, and short" (Hobbes, 1969).

[3]Outbidding theory predicts that the amount and severity of violence will increase as the competing groups aim to demonstrate that they are the most effective agent for the political goals of a particular population (Kydd and Walter, 2006). The logic behind outbidding theory is that groups would like to commit less costly attacks as long as they can obtain their political goals.

[4]See Findley and Young (2012); Nemeth (2014); Conrad and Greene (2015).

[5]Such as weak state capacity (Fearon and Laitin, 2003a) resulting from a higher distance to

to incentives differently, and might have different support groups (see Kis-Katos et al., 2014; Toft and Zhukov, 2015). Hence, new groups might be formed to cater to previously neglected interests and grievances. Finally, political violence itself affects the number of armed groups, as some groups bleed out during a conflict, or are attracted by the fighting itself, e.g., hunting their enemies across locations.

This paper provides causal evidence on the effect of group proliferation on the frequency and intensity of organized political violence. We exploit a unique setting in which the number of armed groups increases through a split of a separatist group that is plausibly exogenous to the conflict dynamics. Specifically, we exploit the split of the Baloch Liberation Army (BLA) into the BLA and United Baloch Army (UBA), operating primarily within the Balochistan Province in Pakistan. The split between the BLA and UBA goes back to a leadership dispute between two brothers who, in short, could not agree on the organization's leadership. While disagreement between the brothers could be related to some unobserved conflict dynamics, the split of the BLA has the additional feature that the groups only effectively split after the father of the two brothers died of natural causes following a relatively short and severe illness.[6]

The exogenous timing of the father's death and the geographically concentrated area of operations of both groups allow us to specify difference-in-difference (diff-in-diff) specifications. We estimate the causal effect of an additional armed group on the quantity and lethality of political violence within the districts of Pakistan.[7] We argue that the main channel underlying this effect is competition (for publicity, recruits, and/or financing) between armed groups. Furthermore, we estimate semi-elasticities between the number of armed groups and political violence using the geographically differential impact of the father's death as an instrument for the number of armed groups. Additionally, concerns about unobserved confounders explaining group formation are alleviated since the general goals, target audience, primary opponent, and tactics of the BLA and UBA are similar.[8]

The empirical analysis combines data from multiple publicly available data sources on political violence committed by the various armed groups within Pakistan. To measure the number of armed groups correctly, we systematically document all mergers and splits of armed groups in Pakistan between 1990 and 2018. Thus, we provide a unified analysis of organized political violence, including terrorism, guerilla warfare, as well as more symmetric forms of political violence. This allows us to test if armed groups change their strategies in response to increased competition. Recent theoretical and empirical work highlights that groups alter their strategies in response to changing constraints, of which increased competition could be an important factor.[9]

the capital (Campante et al., 2019), the presence of lootable resources (Berman and Couttenier, 2015b), and the hostility or acceptance of the local population (Berman et al., 2011).

[6]Khair Baksh Marri died within five days after being admitted to the hospital (Khan, 2014; News International, 2014).

[7]We introduce the setting and the involved actors in detail in Section 4-2.

[8]Looking at raw data shows that on the district-year level 21 % of BLA attacks do not cause bodily harm, while this number 26 percent for the UBA. Both groups conduct a single severe attack in 52 % of the district years in which they are active. Regarding targets both groups target private citizens only one third of the time and businesses about 20% (BLA) and 23% (UBA), respectively.

[9]For a theoretical model see Bueno de Mesquita (2013). For empirical evidence showing how different groups use different strategies, see Stanton (2013); Fortna (2015). For the varying impact of shocks and support groups on different groups see Dube and Naidu (2015); Toft and Zhukov

Combining data on terrorism from the Global Terrorism Database (GTD) (START, 2019) and political violence more broadly from the UCDP Georeferenced Event Dataset (GED) (Sundberg and Melander, 2013) allows us to increase coverage and to proxy for government counter-insurgency efforts. We differentiate insurgency from counter-insurgency by exploiting the different inclusion criteria of events for each database.

Our results show that, on average, the BLA split increases the number of incidents within districts in Balochistan compared to the districts outside of Balochistan by 3.5. This corresponds to an increase of roughly 130%, both in the reduced form specifications and in the 2SLS models in which we estimate the effect of an additional armed group. To put this effect into perspective, an increase in the number of armed groups by one is, on average, an increase in the number of armed groups operating within a given district by 37 %. Accounting for counter-insurgency measures highlights that the violence is driven by armed groups and not a reaction of the government to the split of the BLA.

We contribute to various strands of the literature. Our results confirm that the proliferation of armed groups increases organized political violence, adding additional insight to a vast literature on the determinants of political violence (see for excellent overviews Hegre and Sambanis, 2006; Blattman and Miguel, 2010; Gaibulloev and Sandler, 2019). Note that group proliferation is a potential omitted variable in many studies and cannot be captured by fixed effects in monadic settings. Furthermore, the issue cannot be resolved by focusing on smaller units such as grid-cells.[10]

Methodologically, we provide a novel approach of incidence matching to combine two widely used datasets on armed groups. The main problem in combining different data sources, such as the GTD and GED, is the possibility of double-counting incidents and thus the introduction of measurement error. As we highlight in Section 5-3.1, the definitions of political violence and inclusion criteria used by the different datasets overlap. Hence, multiple data sources potentially include the same incidences of some groups in some regions. Such double-counting would distort the relative activity of a specific group. We provide a data-driven solution for addressing the threat of double-counting when using multiple data sources of political violence. Specifically, we implement an uncertainty-based measure applying spatial and temporal buffers surrounding each incident within the GTD database and flag all incidents included in the GED dataset involving the same group that fall within the joint buffer. Adjusting the buffer sizes allows trading off false-positive vs. false-negative assignments of double-counts. Double-counting in Pakistan applies to 5% to 10% of the GED incidents for reasonable parameters, but our main results are robust to the issue over a wide range of thresholds.[11] The cleaned match further allows to identify incidents with government involvement which are not treated as terrorist incidents by the GTD. Hence they are plausibly initiated by the government and can be used to proxy for counter-insurgency efforts. Our approach is general and allows other authors to analyze directional incidents of political violence, previously

(2015).

[10]See Buhaug and Rød (2006); Tollefsen et al. (2012); Besley and Reynal-Querol (2014); Condra et al. (2018) for prominent examples.

[11]The range between 5% and 10% is based on the procedure including name matches and distance thresholds no bigger than 32km (see Section 4-10, which we find most reasonable).

limited to specific settings (Jaeger and Paserman, 2008; Abrahams et al., 2019).

We also provide evidence of interactions between groups in a unique setting, as there are several armed groups in Balochistan fighting the government, attacking civilians and businesses, but only sporadically each other. What is more, there are up to five established separatist groups operating in Balochistan. These groups are supported by local financiers to some degree and strife for a more autonomous Balochistan. Thus, our setting is markedly different from related studies in which the conflict parties, however defined, are engaged in a battle royal, try to loot local resources in areas that they capture (as in Morelli and Rohner, 2015; Adhvaryu et al., 2018; Gehring et al., 2019), and engage in shifting alliances (König et al., 2017).

Finally, we provide new time-variant data on the armed group level, including mergers and splits for armed groups in Pakistan. The field has made tremendous strides identifying the effects of local conditions and temporal shocks, and static group characteristics affect political violence.[12] Still, the literature is mostly silent on changes within the actors that organize, political violence on a broader scale; the armed groups themselves. Two notable exceptions are the contributions by König et al. (2017) and Trebbi and Weese (2019) that document observed and unobserved coalition structures over time.

The remainder of the chapter is structured as follows: Section 4-2 introduces our setting in detail. Section 4-3 presents our data, and the definition of our core variables. Section 4-4 discusses our empirical strategy. Section 4-5 reports our main results and discusses threats to identification. Section 4-6 explores alternative mechanisms, and estimates semi-elasticities for an additional armed group. Finally, Section 4-7 provides a brief overview of the robustness tests and Section 4-8 concludes.

4-2 Background: Setting

The Balochistan conflict is an ethnic dispute concentrated in the Balochistan province[13] of Pakistan[14]. It started in 1948 when newly independent Pakistan annexed the autonomous Baloch state of Kalat. Since the start, there have been several violent waves between Pakistan and Balochi insurgents: 1958-59, 1962-63, 1973-77, and ongoing since the early 2000s (Times of India, 2016). One of the most important figures that emerged during the 1970s insurgency was Kahir Bakhsh Marri (KBM), who led the Balochistan People's Liberation Front (BPLF). After concessions from the government, the conflict burned out, although it smoldered beneath the surface until it flared up again in the early 2000s. Most current insurgent groups (the BPLF no longer exists) call for an independent Balochistan. Among the many reasons for the insurgency is systemic repression and marginalization of Baloch people as well as the exploitation of natural resources without improvements in local

[12]See Berman et al. (2011); Dube and Vargas (2013); Berman and Couttenier (2015b) for examples on conditions and shocks, while Kis-Katos et al. (2014); Fortna (2015); Polo and Gleditsch (2016) examine characteristics.

[13]One of the four provinces in Pakistan which form the first sub-national layer together with two autonomous territories and the Federal Territory of Islamabad.

[14]Traditional Balochistan has been divided between Iran, Afghanistan and Pakistan following the colonial period.

living conditions, an issue that has continuously been raised since the 1960s.[15] As Dashti (2017, chapter 1) puts it: "[t]he Baloch are considered the poorest people while their land is amongst the richest in the world." The recent development follows a vicious cycle of violence: Pakistan follows a "pick up and dump strategy" whereby the Baloch opposition is rounded up and subsequently tortured and killed (Rashid, 2014). The insurgents initially attacked the military, but they have also turned against non-Baloch natives recently.

The BLA is one of the key players in the insurgency movement, led by the Marri tribe. It was founded around 2000 by the eldest son of KBM. Other Baloch insurgency groups exist, such as the Baloch Liberation Front (BLF), Baloch Republican Army (BRA), Balochistan Liberation United Front (BLUF), or United Baloch Army (UBA). The groups' area of operation is concentrated on locations within Balochistan. All of the Baloch insurgency groups are considered terrorist organizations by the Pakistani government (NACTA, 2020).

Despite the similarity of the groups, Baloch insurgency groups are distinct entities that compete against each other. Groups primarily compete for attention, financial backers and recruits within the Baloch Province, but only rarely fight each other. Hence, visibility is key for each group. A decrease in media attention decreases the attention pay-offs for a group, which may reduce the group's capabilities (Jetter, 2017). Attacks on protected government institutions and incidences with high fatalities demonstrate the capability of a group and will generate more attention. This logic seems especially crucial in this setting since the established insurgency groups of Balochistan have similar platforms. Furthermore, Baloch insurgency groups rely heavily on financing from other governments, wealthy individuals, and middle-class Baloch (Economist, 2012).

The set of Baloch insurgency groups, apart from the appearance of the UBA, has remained constant since 2005. Let us now discuss how the UBA came to enter the conflict, and if it is plausible that its appearance is exogenous with respect to the local conflict dynamics. Baloch groups usually do not openly communicate who their leaders are. In the case of the BLA, KBM seems to be the person who has been calling the shots. In 2007, the previous leader of the BLA, Balach Marri, was killed in action (Dawn.com, 2014). Balach Marri is one of six sons of KBM and BLA leadership passed to the next born brother, Hyrbyair Marri. His younger brother Mehran Marri was in dispute with Hyrbyair regarding leadership and strategy. Personal correspondence with Baloch journalist Malik Siraj Akbar revealed that the BLA recruited from non-Marri tribes starting from 2006 onwards. Some members did not agree with recruiting people that are outside their tribe. Mehran Marri supposedly stole weapons and money as well to form his own group, the UBA. The UBA seems to be part of the BLA as a faction, given that KBM asked the BLA leader to pardon his younger brother's theft and uprising (Ali, 2015; Nabeel, 2017). A joint truce statement of the BLA and UBA in 2018 reveals that leadership disputes lead to the formation of the UBA in 2011(Balochistan Post, 2018).

The real split and alienation occurred after the death of the brothers' father.

[15]The Baloch region is abundant, among other things, in natural gas, copper, and gold (Shah, 2017). It also provides access to the Straits of Hormuz. De Luca et al. (2018) document that while most of Pakistan's gas is produced in Balochistan, the central government charges lower prices for it and pays fewer royalties compared to gas from other regions.

Only five months after the death of KBM, both groups started to attack each other (see START, 2019; Sundberg and Melander, 2013). We later address anticipation effects of KBM's death and find no evidence that groups acted independently beforehand. The critical question is whether KBM died of a natural cause. All accounts support this: he died at age 86 due to a brain hemorrhage in June 2014 (Khan, 2014). Such cerebral bleed occurs suddenly, and the most frequent reason for such bleeding types is high blood pressure. He was admitted to the hospital, and physical damage to his head is unlikely to go unnoticed and under-reported given his popularity. Thus, we conclude that the splitting of the BLA and UBA is driven by the natural death of the leader's father.

In summary, the timing of the actual split between the BLA and UBA is not driven by the competition of the already established groups, nor by some external factors influencing the political violence within Balochistan. As such, we are confident that the group split provides exogenous variation in the number of armed groups operating within Balochistan.

4-3 Data

The units of observation are the districts of Pakistan between 1994 and 2018.[16] Districts in Pakistan correspond to the the third administrative layer. The main results are mostly based on a balanced panel for 141 districts between 1995 and 2018 (we lose 1994 due to differencing of some variables). The primary variables of interest are the level of organized political violence and the number of armed groups accounting for group mergers, group splits, and naming conventions (e.g., "Al-Qa'ida" vs. "Al-Qaida").

4-3.1 Dependent variable: Organized political violence

Our dependent variable is organized political violence. We take the number of incidents committed by armed groups to measure the frequency of organized political violence, and the number of fatalities to measure the severity of political violence.

Data is primarily taken from the "Global Terrorism Database" (GTD) (START, 2019), and complemented by information from the "UCDP Georeferenced Event Dataset" (GED) (Sundberg and Melander, 2013). The GTD, officially tracking terrorism, is our preferred source due to two reasons.[17] First, since our armed groups of interest are classified as terrorist organizations, the coverage of incidents in which they have been involved turns out to be most comprehensively tracked by the GTD. The GTD codes more than 500 incidents committed by either the BLA or UBA, while alternative open source databases such as the GED or the "Armed Conflict Location & Event Data Project" (ACLED) (Raleigh et al., 2010), contain far fewer incidents (333 and 90) in which one of the two groups is involved.[18] Second,

[16]We cannot start our sample earlier as GTD does not provide data for the year 1993, due to a loss of data (see `https://www.start.umd.edu/gtd/about/`). Our approach needs an uninterrupted time-series.

[17]The GTD defines a terrorist attack as: "the threatened or actual use of illegal force and violence by a non-state actor to attain a political, economic, religious, or social goal through fear, coercion, or intimidation."(START, 2019)

[18]Since most events are purely domestic the ITERATE database is not applicable.

the GTD does not have a fatality threshold to include incidents – as is the case for the GED – or has known geographic biases in the recording of incidents – as has been shown for ACLED (Eck, 2012). The GTD is, however, likely to suffer from under-reporting as is common to all open source database relying on news reports to track organized political violence (Van der Windt and Humphreys, 2016a). This last issue is less of a problem in our setting since our identifying variation will come from relative changes in the amount of political violence committed in treated vs. untreated districts. To the best of our knowledge, the under-reporting bias does not change differently between the treatment and control group over time and is thus unlikely to bias our results. Note that we can only use incidents from the GTD and GED which contain information on the district where they occur. This leads to a loss of 95 incidents in the GTD and 180 incidents in the GED, leaving us with 14,063 and 5,611 incidents in the respective database.

Counting fatalities deserves some special consideration. First, fatalities in the databases are recorded with considerable uncertainty. Incidences are always reported if there is newspaper coverage. On the contrary, fatalities may not be stated if the source is too vague or may not state how many people died during an incident. Most notably, the most recent source is used for the fatality estimate. If several newspapers report fatalities for an incident, the modal figure will be included in the database. Second, the number of fatalities is subject to a larger degree of randomness. While armed groups may conduct their attacks with certain expectations with regard to how "big" an attack should be, there are a couple of factors that contribute to the actual number of deaths. In case of a specific assassination, collateral damage may be acceptable depending on how reliant the group is on public support by the affected civilians (as in Toft and Zhukov, 2015). Moreover, the perpetrators are included in the death toll. For example, a suicide attack resulting solely in the perpetrators' death is coded as a fatal attack. Even though fatality rates are difficult to predict, they are informative on the group's intention and capabilities.

A downside of the GTD database is its' focus on terror attacks. Although the applied definition of terrorism is rather broad, it is not clear if a "proper" battle between an armed group and the Pakistani government on a "clearly defined" battlefield would be coded. It should not as this constitutes symmetric warfare. Furthermore, the GTD does not code counter-insurgency operations by the government. An example would be an airstrike in northwest Pakistan killing 20 militants by the Pakistani government reported on 28th of June 2015, which is included in the GED but not the GTD. To answer our research question, we need to ensure to capture these types of events as well. Thus we supplement the GTD data with data from the GED. Specifically, we complement it with GED data on internal armed conflict and one-sided violence against civilians.[19] Using both databases also allows us to test if groups switch from more symmetric political violence, i.e., actions against government troops (coded predominantly by the GED), to more asymmetric types of violence, i.e., against civilians or employing hit and run tactics (the bulk of incidents included in the GTD).

Employing two databases that track organized political violence comes at a cost,

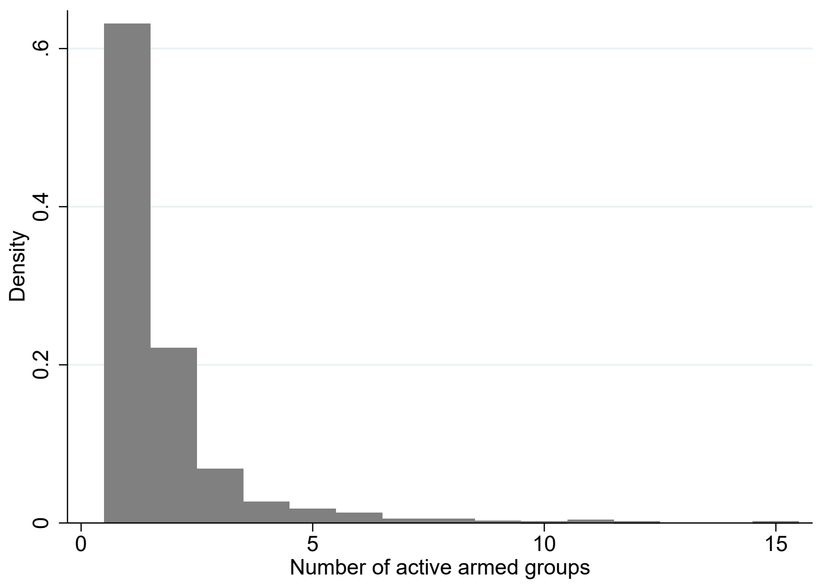
[19]The GED defines an event as: "an incident where armed force was by an organized actor against another organized actor, or against civilians, resulting in at least 1 direct death at a specific location and a specific date" (Stina, 2019).

however. The risk of double-counting incidents introduces potential measurement errors. Double-counting arises if both the GED and GTD code the same incidents for the same groups. We propose to address this issue by assigning an uncertainty measure for double-counting to each incident in the GED dataset. Specifically, we introduce several temporal and spatial buffers around each incident in the GTD database and flag GED incidents that fall within the buffer. Thus, the reader may decide with which buffer she is comfortable. The sole assumption necessary for this approach to work is that double-counting is only an issue between databases but not within them. Section 4-10 discusses our approach and how double-counting affects our results in detail.

4-3.2 Independent variable: Number of armed groups

Our primary independent variable is the number of armed groups. We use these to measure the degree of competition. We consider all actors contained in the GTD and GED as armed groups if they have an individual name. That means we exclude actors such as gunmen or tribesmen.[20] After independently cleaning the data, we compare our groups with the groups reported in (Hou et al., 2020) and find no omissions. In our baseline specifications, we use the active number of armed groups. We define any group as active within a district if it commits at least one attack during the year in that district. The number of active armed groups is then just the count of those groups.

Figure 4-1 – Distribution: Number of armed groups



Notes: Depicts the distribution of the number of *active* groups in district-years with at least one active group. *Active* means conducting at least one attack in a given year and district.

Figure 4-1 depicts the distribution of the number of armed groups over district years that have a positive amount of armed groups operating within them, which

[20] We also exclude so-called one-hit wonders (Blomberg et al., 2010), which are groups that only commit a single attack. We test for the sensitivity of our results to including them in the robustness section. A full list of all armed groups is provided in Section 4-13.

are about 15% of the district-years. That is not to say that most districts never experience group activity. Of the 141 districts included in our sample, only 25% do not experience any activity during the sample period.[21]

Counting groups only as active in a district if they commit an attack during a year is by no means the only way how to think about group presence. For one, it ignores the strategic choice of locality (Marineau et al., 2020). Hence, we employ alternative measures of the number of active groups, such as the potential number of active groups. That is, we set existing groups as potentially active in all districts in which they ever have been active in any year if they are active somewhere in Pakistan in a given year. Groups that cease to exist cannot be potentially active in a district. The idea behind the potential active group measure is that a group reveals the set of districts in which it competes to us only over time while other groups are already aware of them. Furthermore, we are ambivalent about the exact locational choice in a specific year that might be driven by operational or strategic concerns which we are not able to observe.[22]

Other issues when counting the number of independent armed groups are splits and mergers of armed groups and related measurement error within our source databases. The GTD and GED do not track the split and mergers of different armed groups but assign the perpetrator or conflict party of a given incident based on who claimed involvement in an incident, or a third party that attests the identity of the included actors. Hence there is the potential to attribute an incident to a group called X-A which is simply a faction of A, but might later become an independent group. Much like in the case of the BLA and UBA. Note that both the GTD and GED change past entries in their databases if they receive new information, and it is not clear if they also backward correct specific names. To minimize the problem without reducing our sample too much, we use only data until 2018, assuming that most corrections occur within the first year, rather than later on.

To address the issue of potential splits and mergers, we conduct an in-depth analysis of all armed groups within Pakistan and track if they split or merge with other groups during our sample period. The analysis is based on full-text online searches of major media outlets. Figure 4-2 provides an overview of the timing of all splits and mergers occurring in our sample, while Section 4-14 provides detailed documentation of each case. We can then reassign incidents to the corresponding pre-merger or post-split groups and adjust the number of groups for each district, to reflect splits and mergers correctly. Note that we will not use the other splits or mergers to identify the competition effect since we cannot rule out that the timing of the mergers and splits are endogenous to the conflict dynamics within Pakistan. However, controlling for the number of groups allows us to proxy for changes in the degree of competition within our treatment and control groups unrelated to our treatment. Table 4-A1 shows that most group splits would neglect the increase in groups within our control group, i.e., districts outside of Balochistan. Full descriptive statistics for our variables of interest are reported in Table 4-A2.

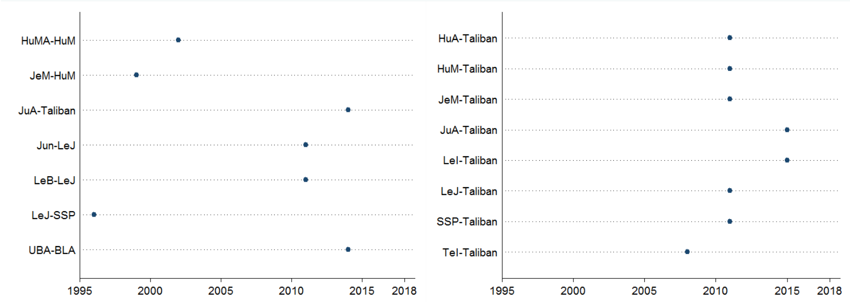
How unique is Pakistan as a case study for our proposed mechanism? To get an

[21]Figure 4-A1 reports the active group distribution for districts in and outside of the Baloch province separately. The distribution of the number of armed groups is skewed slightly more to the right for districts within Balochistan compared to those outside of Balochistan.

[22]Section 4-11 shows how this changes the landscape of active groups and its effect on our core results.

Figure 4-2 – Armed groups splits and mergers

(a) Group splits **(b)** Group mergers

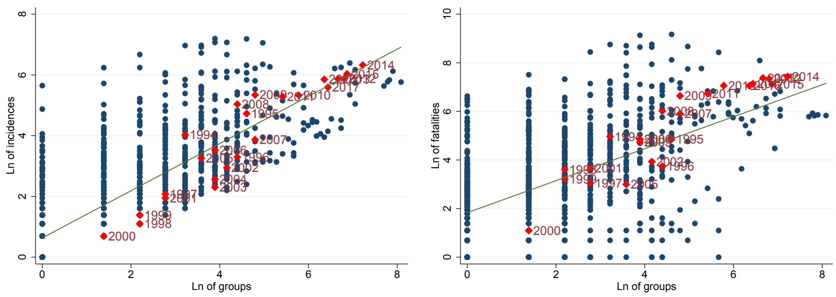


Notes: Reports the year in which groups split (panel A) or merge (panel B). Baloch Liberation Army (BLA), Harakat ul-Mujahidin Al-Almi (HuMA), Harakat ul-Mujahidin (HuM), Jaish-e-Mohammad (JeM), Jamaat-ul-Ahrar(JuA), Jundullah (Jun), Lashkar-e-Balochistan (LeB), Lashkar-e-Islam (LeI), Lashkar-e-Jhangvi (LeJ), Sipah-e-Sahaba/Pakistan (SSP), Tehrik-e-Islami (TeI), United Baloch Army (UBA)

initial idea, we plot the elasticity between aggregated incidents and fatalities on the number of active armed groups at the country level for all countries included in the GTD. Figure 4-3 shows the results, highlighting Pakistan-Year observations in red.

Figure 4-3 – Armed groups and political violence

(a) Incidents-Group-Elasticity **(b)** Fatalities-Group-Elasticity



Notes: Depicts a scatter plot of unfiltered (log of) groups vs (log of) incidents & fatalities created by these groups. The unit of observation is country-year. Pakistan is represented in red. The line illustrates the best linear fit using the global GTD sample between 1994 and 2018.

The first impression is quite stark. First, there is an apparent correlation between the number of armed groups active within a country and the amount of organized political violence perpetrated. Second, Pakistan is not located in the extremes but seems to fit the linear fitted line quite well. Of course, this is only suggestive evidence on the country level, but it is supportive of notion that more armed groups lead to more political violence.[23]

[23]The pattern is similar if we demean the measures by country and year (see Figure 4-A2).

4-4 Empirical Strategy

4-4.1 Estimation Framework

We are interested in the causal effect of an increase in the number of active groups on the amount of political violence within Pakistani districts. As already mentioned in the preceding sections, we exploit the fact that Khair Bakhsh Marri (KBM) died of a natural cause, which leads to the break up of the BLA into the BLA and UBA, to obtain exogenous variation in the number of competing armed groups within districts.

In the spirit of Draca et al. (2011), we will use two complementary identification strategies to answer our question. We will run panel difference-in-difference estimations in which we regress political violence on the interaction of districts in Balochistan with the post KBM death period, as well as 2SLS regressions where we use the same interaction to predict the number of armed groups active within a district. The idea behind the 2SLS approach is that the split in the BLA due to KBM's death leads to plausible exogenous variation in the number of armed groups within Balochistan compared to districts outside of Balochistan. This second strategy allows us to estimate the semi-elasticity of an additional armed group on political violence. We return to the 2SLS specification in the mechanism section of the paper and start with the reduced form specification (diff-in-diff) here.

The intuition behind the diff-in-diff specification is that the death of KBM splits the BLA. This affects the 28 districts in Balochistan more compared to districts outside of Balochistan since both the BLA and UBA almost exclusively operate in Balochistan (close to 95% of the incidents).[24] Our baseline specification is defined as follows:

$$Y_{it} = \beta(Baloch_i \times PostKBM_t) + \mathbf{X}'_{\mathbf{it}}\text{-} + \eta_i + \gamma_t + \epsilon_{it} \tag{4-1}$$

where Y_{it} is the amount of political violence (either incidents or fatalities) perpetrated in district i during year t. $Baloch_i \times PostKBM_t$ is the interaction between districts located in the Baloch province with the *KBM post-mortem* period, $\mathbf{X}'$ is a vector of control variables we use to control for potentially unobserved confounders between the control and treatment districts over time. We include the log of the population to normalize the count of incidents relative to the local population. We also employ a set of geographic characteristics interacted with time to proxy for changes in state capacity within districts over time (Fearon and Laitin, 2003a; Buhaug et al., 2009).[25] η_i and γ_t are district and year fixed effects, and ϵ_{it} is an error term assumed to be well behaved.

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[24] See Table 4-A1 for the incidences of BLA and UBA within and outside of Balochistan.

[25] The log of population density is calculated based on the GWP (CIESIN, 2016). Note that the GWP is only provided every five years and only provides detailed spatial population estimates for the reference years 1990, 1995, 2000, 2010, and 2015. We linearly interpolate and extrapolate the population data between those reference years and 2018, the last year of our sample.

We use several geographic characteristics to proxy for the government's capability to project power within a given district. We include the log of ruggedness, elevation and road density, as well as the count of airports located within a district. Note that we interact all of those time-invariant variables with year fixed effects to allow for a distinct impact each year.

We also specify an event study to test for possible violations of the common trend assumption. We define:

$$Y_{it} = \sum_{s}^{S} \beta_s b_{it}^{s} + a_{it} + b_{it} + \mathbf{X'_{it}}\text{-} + \eta_i + \gamma_t + \epsilon_{it} \tag{4-2}$$

where b_{it}^{s} is our time-varying treatment indicator $(Baloch_i \times PostKBM_t)$ across the sequence s.[26] s is a sequence $s = (-3, -2, 0, 1, 2, 3)$ centered at the treatment year 2014 for each district. We bin the endpoints of our treatment effect window with two dummies labeled a and b. a is a dummy that is unity for each district in the Baloch province following the post-treatment sequence under consideration (in our case 2018), while b is a dummy that is unity for all years before the sequence starts, i.e., 2010 or earlier. $\mathbf{X}'$ is a vector of control variables containing the same variables as in equation 4-1. The coefficients of interest are β_s, which report the difference-in-difference estimate separately for each year within the sequence (apart from 2013 or $t-1$, which is our omitted category). This stacked or event study approach has several advantages compared to standard difference-in-difference methods.

First and foremost, event studies provide the best insight into potential pre-treatment trends since we obtain direct estimates for them. Second, event-studies can depict the dynamics of the treatment effect and provide insight into how long it lasts.[27] However, there are also downsides. Ideally, one would have a clear idea of the timing of the effect ex-ante, i.e., for how long it should last and design the post-treatment period accordingly, but we lack such a clear prior. Note that we are also constrained since we only have four post-treatment years of data available to us. Furthermore, the estimation method is also more demanding. Borusyak and Jaravel (2017) recommend, for example, to drop the pre-treatment coefficients if the null of no pre-trends cannot be rejected in the initial event study.

4-4.2 Identifying Assumptions

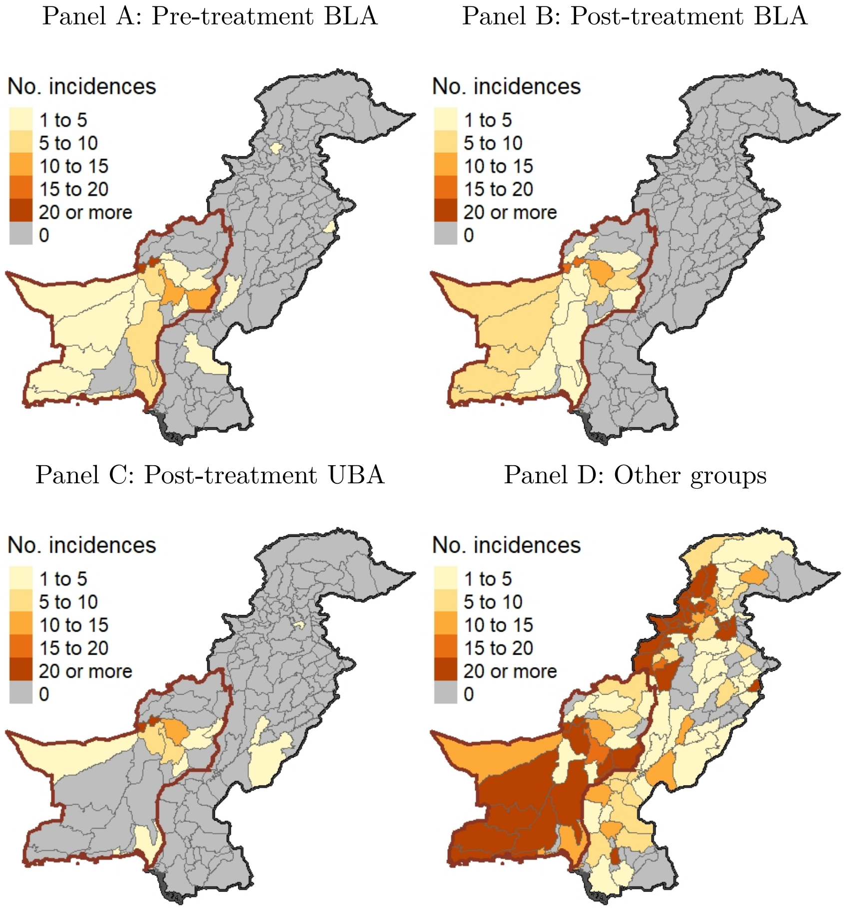
For standard DiD and event-study estimates to be valid, they need to satisfy two central assumptions. First, we need to assume common trends, or bias stability, which means that differences in the expected potential non-treatment outcomes over time are not related to the subsequent treatment groups. Second, we need to assume exogeneity of our control variables and pre-treatment outcomes. This implies that the timing of the treatment is exogenous. If both assumptions are satisfied, we can recover the average treatment effect on the treated (ATT).

The exogenous timing of our treatment is the main advantage of our unique setting. As outlined above, KBM died in a hospital from a brain hemorrhage. Thus, as long as we agree to assume that the brain hemorrhage was not the result of more intensive competition between the groups within the Baloch province, we have little problems in that regard. The anticipation of his death is, however, another issue. We acknowledge that KBM was already comparably old, which could have led to some of his sons anticipating his death. In such a case, one could argue that the UBA and BLA already compete before his death, i.e., when the UBA

[26] The treatment indicator is simply the previous introduced interaction.

[27] Additionally, recent studies have shown that symmetry in the pre- and post periods bring DiD estimates closer to experimental benchmarks (Chabé-Ferret, 2015).

Figure 4-4 – Terrorism distribution across Pakistan

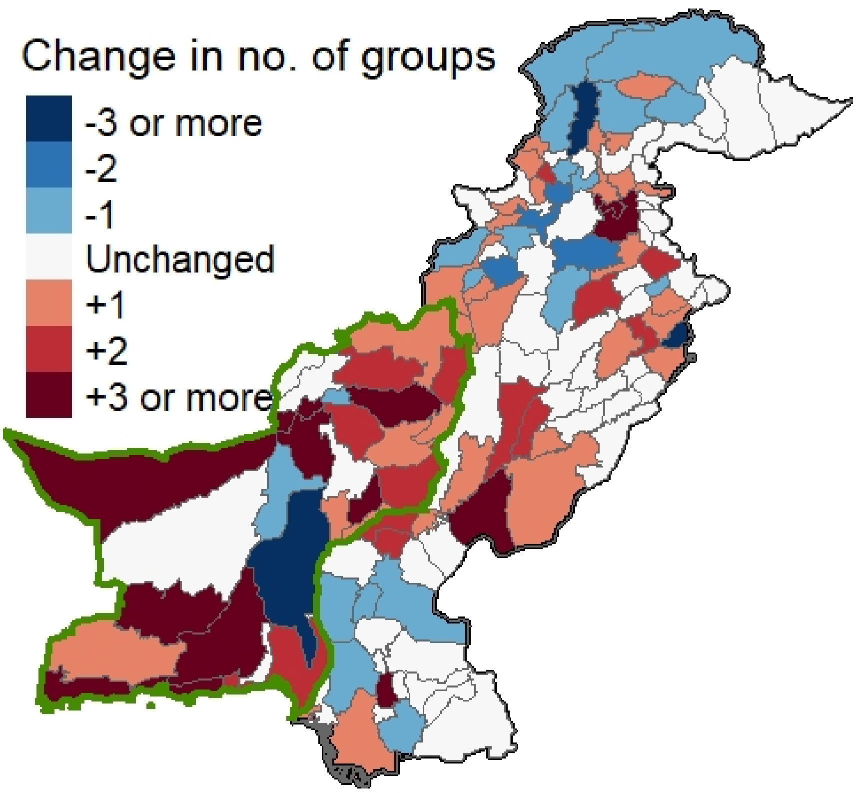


Notes: Illustrates the average number of terror events. The bold red line represents the borders of the Baloch province. The darkest shade within Balochistan in Panels A-C is Quetta, the capital.

faction formed. Our analysis below will show, however, that this seems not to be the case. The composition of the treatment groups is another crucial issue. In our main specification, we define districts in Balochistan as the treatment group, since it is the region in which the BLA has traditionally operated. If, for example, we would find that the UBA and BLA never overlap after their split, our mechanism of an exogenous increase in the number of groups could be jeopardized. Figure 4-4 suggests that we do not face this problem, since the main area of operation remains within Balochistan. Additionally, there is a high degree of operational overlap, since almost all districts with UBA activities see BLA activities as well.

Panel A of Figure 4-4 shows the spatial distribution of BLA attacks in the pre-treatment period, while panel B depicts the post-treatment period distribution. The activity within Balochistan has been relatively stable, while a few districts outside of Balochistan have not experienced any further violence by the BLA. In panel C, we plot the spread of UBA incident in the post-treatment period. Comparing the spatial spread of the UBA to the BLA shows that the UBA also engages in some minor activity outside of the Baloch province, but both groups share their most active districts. Finally, the last panel shows the overall distribution of incidents by other groups. It should be apparent that while Balochistan is a major hotspot for

Figure 4-5 – Change in number of groups



Notes: Reports the change in group numbers between 2009-2013 (pre-treatment) and 2014-2017 (post-treatment). The bold green line represents the borders of the Baloch province.

terrorism, there are other districts with a similar level of violence, e.g., the districts within the Federal Tribal Areas in the north-west (see panel D).

We provide additional suggestive evidence that our treatment operates via an increase in the number of armed groups in Figure 4-5. Figure 4-5 plots the difference in the number of groups for the five years before treatment to the five years after treatment. As expected, after KBM's death the number of active armed groups increases most within districts where the BLA has been active. Furthermore, districts that already host more groups in the first place see a higher group count after KBM's death (Figure 4-A3).[28]

Finally, let us briefly discuss the parallel trends assumption and potential anticipation effects. Given that the UBA faction established itself already in 2011, and KBM was already relatively old, one could argue that the UBA faction was already affecting competition between groups before KBM's death. We address the issue in the next section. To preview our findings, neither parallel trends, the faction creation, nor anticipation effects seem to be a problem in our setting.

[28]We directly test for the mechanism of increases in the group count after the group split using 2SLS in Table 4-9.

4-5 Results

4-5.1 Baseline results

Table 4-1 reports our baseline results for the difference-in-difference specifications. The coefficient in column 1 is highly statistically significant, regardless of clustering on the district or the division level, and when allowing for spatial dependence in the error terms[29]. The estimate suggests that districts within Balochistan experience an increase of around 3.5 incidents in the post-treatment period compared to districts outside Balochistan. The results for fatalities are sizeable as well, although less statistically significant. The treatment districts experience roughly 5.5 fatalities more compared to the non-treatment districts annually during the post-treatment period.

Table 4-1 – Baseline results

	Incidents (1)	Fatalities (2)	Ln Incidents (3)	Ln Fatalities (4)
$Baloch_i \times Post_t$	3.5904	5.5364	2.1067	1.5800
	(0.9554)***	(2.8711)*	(0.3787)***	(0.4420)***
Controls	Yes	Yes	Yes	Yes
District-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Obs	3144	3144	3144	3144
Adj. Within-R^2	0.235	0.164	0.136	0.101

Notes: The controls include log of population and geographical characteristics interacted with year dummies (see footnote 25). Standard errors clustered at the district in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

In columns 3 and 4, we take logs of our dependent variables to address the skewness of our dependent variables. Note that we add a small constant of 0.01 to both incidents and fatalities in order to keep district-year observation in the sample that experiences no activity. Column 3 suggests that our treatment effect leads to an increase of roughly 210% in incidents, while column 4 suggests an increase in fatalities of around 160%. Since those estimates are potentially biased due to the constant that we add we also replicate the estimations using Poisson and negative binomial regressions, which produce somewhat lower effects from around 100% for incidents and between 50% to 90% for fatalities (see Table 5-B38 and Table 4-A6). The statistical significance is relatively stable throughout the specification.[30]

[29]Divisions are the second administrative layer of Pakistan (below the provinces). There are 30 divisions in total, six of which lie within the Baloch Province. We also allow for arbitrary spatial dependence following Colella et al. (2019). Figure 4-A4 reports the results using the "acreg" command to estimate our baseline specification using Conley HAC standard errors (Conley, 1999) across several distance cutoffs for the spatial dependence. We find that our estimates remain highly statistically significant and stable even above a cutoff of 1,500 km. Table 4-A4 reports the alternative standard errors for Table 4-1.

[30]We also run log specifications without adding the constant, thus focusing on the intensive margin. Results are similar to the Poisson and negative binomial results (see column 1 Table 4-A7).

The qualitative effects remain stable if we employ more conservative specifications by adding district-decade fixed effects and linear time trends for each district (see Table 4-A8).[31] What is more, our incident results are not driven by nonlethal events, but are comparable if we focus on incidents in which at least one person was wounded or killed (Table 4-A10).

Table 4-2 – Controlling for other groups

	Incidents (1)	Fatalities (2)	Ln Incidents (3)	Ln Fatalities (4)
Panel A: Other groups in changes				
$Baloch_i \times Post_t$	3.6534	5.8153	2.1867	1.6526
	(0.9790)***	(2.9540)*	(0.3857)***	(0.4454)***
Δ No. other groups	0.9081	4.0179	1.1531	1.0458
	(0.1420)***	(0.8920)***	(0.1298)***	(0.1449)***
Adj. Within-R^2	0.249	0.180	0.239	0.166
Panel B: Other groups in levels				
$Baloch_i \times Post_t$	2.2596	0.1187	1.2159	0.6821
	(0.7291)***	(2.1189)	(0.2817)***	(0.3137)**
No. other groups	2.5476	10.3715	1.7054	1.7189
	(0.2179)***	(1.4648)***	(0.2407)***	(0.2683)***
Adj. Within-R^2	0.381	0.300	0.426	0.328
Controls	Yes	Yes	Yes	Yes
District-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Obs	3144	3144	3144	3144

Notes: The controls include log of population and geographical characteristics interacted with year dummies (see footnote 25). Standard errors clustered at the district level in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

One potential unobserved factor is the number of other armed groups active within districts. The number of other groups active within treatment and control districts could bias our estimates in either direction. If, for some reason, competition declines within the control group over time, we would get an upward bias in our estimates, while our estimates would suffer from a downward bias if group activity increase for some reason within our control group. The same holds true if either development occurs within our treatment districts, or within treatment and control districts.

To address these concerns, we include the change in the number of other active groups in each district from $t-1$ to t, and alternatively, the number of other active groups as additional control variables.[32] Table 4-2 replicates the specifications of Table 4-1 including the change of other active groups in Panel A, and the amount

[31] Note that those additional fixed effects and trends can partly capture discoveries of natural resources for which we do not have data for our treatment period. Results are also similar if we exclude all controls (see Table 4-A9). Furthermore, our results are not driven by any specific district. In Figure 4-A5 we report the baseline coefficients of column 1 dropping one district at the time.

[32] Other active groups are the active number of groups excluding the BLA and UBA from the count. If we do not exclude the BLA and UBA from the count, our treatment would be perfectly

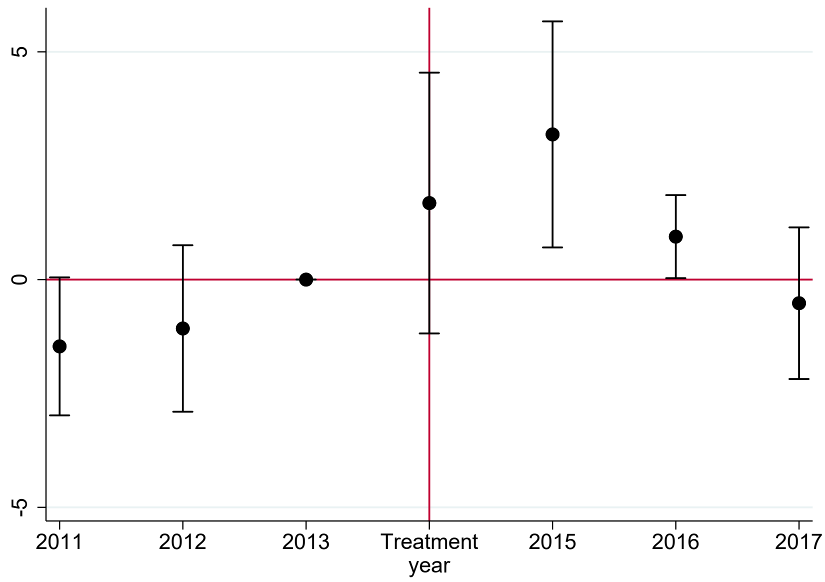
of other active groups within the district in Panel B, as additional control variables. Columns 1 to 4 in Panel A show that our results are virtually unchanged if we control for the change in the number of active groups within districts. When controlling for the level of other active groups our coefficients are roughly halved, except for the fatality specification in which the coefficient drops to 0.1. Note that the level specification of other groups performs significantly better in terms of model fit, which is why we slightly prefer this specification. What is more, our log estimations move much closer to the nonlinear estimates (see Table 5-B38 and Table 4-A6).

4-5.2 Threats to identification

As outlined in Section 4-4.2 our identification requires the death of KBM to be exogenous and unanticipated. Furthermore, districts in Balochistan need to exhibit parallel pre-treatment trends in violence compared to the rest of Pakistan.

We directly test for violations of the pre-trends and anticipation effect using an event-study with the full set of baseline controls and the number of other active groups. Figure 4-6 reports diff-in-diff coefficients and their corresponding point-wise 95% confidence intervals, using incidents as the dependent variable.

Figure 4-6 – Baseline effect



Notes: Reports the coefficient and their accompanying 95% CIs for an event study specification corresponding to column 1 in Table 4-2. The controls include log of population and geographical characteristics interacted with year dummies (see footnote 25), as well as dummies binning the endpoints of the event window.

The statistically insignificant coefficients in the pre-treatment period in Figure 4-6 lead us to not reject the null of common trends or no pre-trends. Furthermore, it highlights that our effect is rather short-term. The point estimates we have obtained previously seem to be driven by competition between groups during the two

colinear with the number of active groups. We use the number of active groups including the BLA and UBA in the instrumental variable regressions in Section 4-6.

years after treatment occurs. Note that the incident coefficients become statistically significant only from 2015 onward, which is not surprising given that KBM died in June of 2014 and the UBA split did not materialize for the first half year. The effect vanishes in $t+3$.

Another issue is the correct timing of our treatment. As discussed in Section 4-2, the UBA faction formed during 2011, due to disagreements about strategy and competing leadership aspirations of two of the Mari sons.[33] Hence, it could be possible that the presence of the UBA faction already affects competition between groups that we currently neglect. We test for this possibility directly and include the following additional interaction term $Baloch_i \times UBAfaction_t$ that represents the counterfactual effect in Baloch districts from 2011 onward instead of after 2014. The results are reported in Table 4-3.

Table 4-3 – UBA faction vs. UBA group

	Incidents (1)	Fatalities (2)	Ln Incidents (3)	Ln Fatalities (4)
$Baloch_i \times UBAfaction_t$	0.4716	-1.6000	0.2465	0.0506
	(0.6338)	(4.9944)	(0.3265)	(0.4155)
$Baloch_i \times Post_t$	1.8651	1.4571	1.0097	0.6398
	(0.5578)***	(3.5682)	(0.3243)***	(0.3825)*
No. other groups	2.5402	10.3968	1.7015	1.7181
	(0.2172)***	(1.4412)***	(0.2412)***	(0.2691)***
Controls	Yes	Yes	Yes	Yes
District-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Obs	3144	3144	3144	3144
Adj. Within-R^2	0.381	0.300	0.426	0.328

Notes: The controls include log of population and geographical characteristics interacted with year dummies (see footnote 25). Standard errors clustered at the district level in parenthesis. The results are similar when using the Δ of No. other groups instead of the level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

In line with our argument, we find that the coefficient of $Baloch_i \times UBAfaction_t$ is statistically insignificant throughout all specifications. In turn, our coefficient of interest $Baloch_i \times Post_t$ is statistically significant and comparable in size to the estimates of our previous specifications and supports our previous reasoning in the incident specifications. The fatality specifications are more fickle.

4-6 Extensions and alternative channels

In this section we further scrutinize how our treatment affects competition between armed groups. We explore if the type of organized political violence changes through the treatment and to which part our results are explained by the government's

[33] Note that if the split between the BLA and UBA has already really occurred in 2011 due to a dispute over leadership between the two brothers, this would have been an alternative identification strategy. At least as long as the struggle for leadership was not driven by the conflict itself, but by personal ambitions of the brothers.

response. Furthermore, we scrutinize how the BLA itself was affected by the split of the UBA and if the government changes its response to the BLA compared to other groups. Lastly, we obtain semi-elasticities of an additional armed group on the quantity and quality of violence within districts.

4-6.1 Targets of armed groups

We think that our effect runs via an increase in the number of armed groups within Baloch districts that compete for attention, as visibility facilitates the attraction of financiers and recruits. We find this effect to be particularly strong in terms of the number of incidents and weaker regarding the number of fatalities. Consistent with this we do not find a statistically significant result when using fatalities per incident as the dependent variable.[34] This indicates that there seems to be no strategy change towards more lethal attacks. However, our findings so far could also be a result of increased infighting between groups, i.e., armed groups attacking each other. If KBM was a unifying figure, he might have stopped different groups from attacking each other (such as his sons).

Our data allow us to test this alternative explanation directly. The GTD list the target type of an incidents, e.g., Terrorists/Non-State Militia or Violent Political Party among others. We create an alternative incident count using only incidents that target either of those categories and rerun our core specification.

Table 4-4 – Targets

	Incidents vs. other Groups (1)	Incidents vs. Gov. (2)	Incidents vs. public Infrastructure (3)	Incidents vs. Business (4)	Incidents vs. citizens (5)
$Baloch_i \times Post_t$	-0.0527	1.4151	0.9838	0.4582	0.8959
	(0.0416)	(0.5738)**	(0.3688)***	(0.1365)***	(0.3394)***
Controls	Yes	Yes	Yes	Yes	Yes
District-FE	Yes	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes
Obs	3144	3144	3144	3144	3144
Adj. within-R^2	0.246	0.482	0.222	0.327	0.295

Notes: The definition of the dependent variables (GTD target type) are listed in Table 4-A3. The controls include log of population and geographical characteristics interacted with year dummies (see footnote 25). Standard errors clustered at the district level in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Column 1 of Table 4-4 shows that our treatment does not affect infighting in the treatment vs. the control group. Hence, KBM's death does not seem to have increased infighting between groups. To further verify that group competition is, in fact, not directed against each other, we examine the target of Baloch insurgency groups. In contrast to the GTD, the GED systematically lists both conflict parties. Before the BLA splits in 2014, there is no recorded incident of a Baloch group against another Baloch insurgency group. However, after 2014, we find that the BLA and

[34] Results available upon request.

UBA did attack each other once. The incident reassures us that the groups' real alienation started only after the death of KBM, while at the same time infighting is not the driver of our result.

We use the target type information provided by the GTD to qualify further what kind of competition effect we are observing, specifically if the type of political violence has changed. Columns 2 to 5 in Table 4-4 replicate our baseline specification, using incidents against the government, against public infrastructure, business, and private citizens as the dependent variable.[35] We find that the coefficient on attacks against the government is most sizable. However, we cannot reject that the increase of attacks against the government, public infrastructure, businesses, and private citizens are not different from one another. In summary, it seems that the type of competition relating to the targets that groups pick, has not changed due to the treatment.

4-6.2 Government response: Counter-insurgency

Is our observed effect of increased political violence in fact driven by counter-insurgency efforts of the government? One alternative explanation of our proposed mechanism is that the government sees the death of KBM as a chance to crack down on separatist groups within Balochistan. Thus, the increase in political violence could simply be increased counter-insurgency activity by the government.

We test for this possibility by analyzing if our effect sizes change if we explicitly include counter-insurgency efforts of the government of Pakistan. To do so, we need a proxy for incidents instigated by the government against armed groups. Obtaining a suitable proxy is not without problems. Recall that the GTD only codes terrorists events and hence misses counter-insurgency operations, such as the airstrike mentioned in Section 4-3.1. The GED on the other hand, codes event dyads but those are not directional, i.e., there is no indicator variable informing us if the incidents was initiated by the government or an armed group.

To proxy for counter-insurgency activity by the government, we use the incidents between the government and armed groups that are reported in GED but not in GTD. The assumption is that if we subtract the incidents between the government and any armed group included in the GTD and thus identified as a terrorist activity by the GTD, the events left can be used as reasonable proxies of operations instigated by the government.

The main operational obstacle is to deal with measurement uncertainty between the two databases. We tackle this issue with our proposed double counting procedures, which we explain in detail in Section 4-10. In short, we draw a buffer of 25km around each GED event and flag it as a potential double count if the GTD codes an event of the same armed group during the same day.[36] Events that are flagged as potential double counts are excluded form the analysis, which leaves us with a set of incidents that will use as our counter-insurgency proxy (roughly 60% of government incidents). Our count of counter-insurgency incidents obtained from this procedure is roughly 47% of all incidents in which the government is involved (46 % within the Baloch province and 48% outside of the Baloch province).

[35] Table 4-A3 provides the specific definitions for each of the measures.

[36] We use only events for which the geographic precision provided by the GED is 1 to 25km for this exercise.

Table 4-5 – Government response

	Incidents (1)	Incidents Gov. (2)	Incidents vs. Gov. (3)
$Baloch_i \times Post_t$	3.5422	0.1442	1.5593
	(0.9647)***	(0.4342)	(0.7340)**
Inci Gov.	0.2050		
	(0.0574)***		
Controls	Yes	Yes	Yes
District-FE	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes
Obs	3144	3144	3144
Adj. Within-R^2	0.291	0.0106	0.0525

Notes: Column 1 uses our baseline dependent variable. Column 2 uses our counter-insurgency proxy as the dependent variable. Column 3 uses the count of incidents against the government (column 2 Table 4-4) and counter-insurgency measures by the government as the dependent variable. The controls include log of population and geographical characteristics interacted with year dummies (see footnote 25). Standard errors clustered at the district level in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Column 1 of Table 4-5 replicates our baseline reduced form effect (column 1 of Table 4-1) adding counter-insurgency efforts of the government as an additional variable. While counter-insurgency is positively correlated with violence (e.g., revenge) this does not alter our initial finding. Note that the effect of government counter-insurgency on political violence is theoretically ambiguous (Bueno De Mesquita, 2005). In column 2 we change our dependent variable. We now explicitly look at the determinants of counter-insurgency. We see that our treatment does not affect government-induced actions. In column 3, we add counter-insurgency events to the attacks against the governments by organized groups. We get a significant treatment effect but if we compare it to column 2 of Table 4-4, we see that adding counter-insurgency leaves the treatment effect virtually unchanged. We take the results reported in Table 4-5 as evidence that the increase in violence is primarily driven by increased activity of the armed groups, and not by counter-insurgency efforts in the wake of KBM's death.

4-6.3 Capacity effect and strategy changes

Our reduced-form effect has multiple potential channels that can explain the increase in political violence. First and foremost, the split changes the absolute and relative capacity of the BLA itself, as well as the relative capacity of other groups within the districts. Furthermore, the changes in relative capacity could lead the government to treat the BLA and UBA differently post treatment, i.e., targeting them more or less. We test for both possibilities. First, we analyze how the BLA as well as BLA and UBA jointly compare to other well established separatist groups within Balochistan. Second, we test if the government targets its counter-insurgency efforts more towards the BLA (and UBA) compared to other well established separatist groups in the Baloch province.

Ex-ante, we would expect that the amount of violence perpetrated by the BLA should decrease compared to other Baloch separatists groups (at least in relative terms). That is, if the UBA split did indeed significantly reduce BLA's assets. However, it could also simply change the composition of political violence perpetrated by the BLA without changing the absolute violence level, e.g., switching from symmetric to asymmetric warfare, i.e., targeting civilians (Bueno de Mesquita, 2013). We test for both possibilities, running a diff-in-diff specification on the sample of Baloch separatist groups active within districts, defined as:

$$Y_{ijt} = \delta(BLA_{ij} \times PostKBM_t) + \eta_{ij} + \gamma_{it} + \epsilon_{ijt} \tag{4-3}$$

where Y_{ijt} is political violence committed by group j within district i at time t , $(BLA_{ij} \times PostKBM_t)$ is the interaction of out post-treatment period with the BLA, η_{ij} are district-group fixed effects controlling for the time-invariant capacity a group has within a district, γ_{it} are district-year fixed effects controlling for competition between groups, as well as government capacity within a district-year, and ϵ_{ijt} is the error term. δ is the diff-in-diff coefficient between the BLA and the other well established Baloch separatist groups, specifically the BRA, BLF, and BLUF. Note that the UBA is excluded for now, but we return to the UBA momentarily. Note further that we restrict the control group to the other big Baloch separatist groups since they are present both in the pre- and post-treatment periods and often operate in the same districts as the BLA. These last two points are of importance since our fixed effects restrict our identifying variation to differences in the outcome of within-district groups over time while also partialling out any district-specific time variation common to all groups. Thus a district-group-year observation is only informative if the BLA and at least one of the other groups operate in a district during the same year.[37]

Table 4-6 – DiD: Within Baloch separatist groups

	Inci	Inci all	Inci civil	Fatal	Fatal all	Fatal civil
	(1)	(2)	(3)	(4)	(5)	(6)
$BLA_{ij} \times Post_t$	-0.0795	-0.2202	-0.1073	0.0348	-0.1143	-0.0802
	(0.0565)	(0.0917)**	(0.0519)**	(0.0788)	(0.1815)	(0.1056)
District-Group-FE	Yes	Yes	Yes	Yes	Yes	Yes
District-Year	Yes	Yes	Yes	Yes	Yes	Yes
Obs	7896	7896	7896	7896	7896	7896
Adj. within-R^2	0.374	0.378	0.295	0.291	0.328	0.237

Notes: Columns (1) and (4) include our standard GTD measure, (2) and (5) combine GTD and GED, (3), and (6), combines GTD and GED events against civilians. Standard errors clustered at the district level in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The results, presented in Table 4-6, provide some evidence that the BLA reduces its activity compared to the other armed Baloch groups. While we find no statistically significant effect using our standard measure (column 1), the combined GTD and all GED events (column 2), and violence against civilians exclusively

[37]Since we change the treatment and control groups, we have to reconfirm the common trend assumption. We do so using an event study as before. The results are reported in Figure 4-A6 and show that also in this setting the null of no pre-treatment trends cannot be rejected.

(column 3) show a reduction.[38] Note that the reduction in attacks against civilians (column 3) is only about half the magnitude compared to the reduction in all attacks (column 2), which is consistent with a strategy shift by the BLA due to reduced capacity. With respect to inflicted fatalities we cannot reject that the ratio of fatalities between the BLA and other armed Baloch groups remains constant.[39] We take this as evidence that the additional violence in our baseline results is likely not exclusively driven by an increase in BLA activities, but also from the increasing activity of other well established armed groups competing for the top spot. The within-group analysis highlights that the BLA commits somewhat fewer incidents compared to other established groups within Balochistan. However, this does not necessarily mean that BLA commits fewer incidents overall. If we run a first-difference analysis on the BLA exclusively, we cannot find robust evidence that the BLA changed the number of incidents it committed.[40]

Moreover, we find that the UBA is compensating for the decrease in BLA activities somewhat. Table 4-A11 in the Appendix replicates Table 4-6 with the sole difference that incidences from the UBA are added to the BLA, simulating that the groups did not split. The negative effect on all incidents is both lower in magnitude and statistically significance, while the effect on violence against civilians vanishes. We also find some evidence that the BLA and UBA jointly increase their activity compared to all other groups.[41]

We now test if the government focuses its counter-insurgency efforts to a higher degree on the BLA (and UBA) following its split. The assumption would be that the government sees the death of KBM as a chance to finish the group off. In such a case our reduced form effect would exhibit an alternative channel besides the competition effect we have in mind.

To test for this possibility we generate group specific proxies for counter-insurgency efforts by the government and regress these proxies on the $BLA \times Post_t$ interaction. Note that the same restrictions apply as in Section 4-6.2. We only use incidents from the GED as proxies for counter-insurgency efforts that do not plausible appear in the GTD database, following our introduced double counting thresholds. We also include the respective amount of incidents and fatalities committed by each group as additional control variables, in order to partial out any government response driven by the time-varying activity levels of the groups.

Table 4-7 reports the results of this specification using both counter-insurgency incidents as well as fatalities as the dependent variable. Column 1 and 2 show that we cannot reject the null that the government does not change its focus towards the BLA following KBM's death compared to the other separatist groups within Balochistan. The same holds true in columns 3 and 4 in which we treat the UBA

[38] In column 3 we add up GTD incidents and GED incidents against civilians.

[39] Even when estimating non-state incidences and fatalities, e.g., group against another group, the results are largely unchanged. That is, non-state incidences are statistically lower in the post-treatment period for the BLA and insignificant for fatalities.

[40] Results not reported but available upon request.

[41] To further verify that it is indeed the BLA and other Baloch separatist groups that primarily seem to increase their violence, we flip the specification of Table 4-6 and use all other groups as a control group. Note that since the comparison group is now more heterogeneous, we control for group ideology following Kis-Katos et al. (2014). We find some evidence that the UBA and BLA jointly commit more violence compared to other groups following the split (see Table 4-A12). Note, however, that the common support of incidents in districts during the same years is much more limited as mentioned above.

Table 4-7 – DiD: Government action against BLA/UBA

	BLA only		*BLA & UBA*	
	Inci	Fatal	Inci	Fatal
	(1)	(2)	(3)	(4)
$BLA_{ij} \times Post_t$	0.0036	0.0034	0.0030	-0.0108
	(0.0035)	(0.0250)	(0.0034)	(0.0302)
Inci (GTD)	0.0187		0.0144	
	(0.0047)***		(0.0074)*	
Fatal (GTD)		0.1901		0.1185
		(0.1161)		(0.0779)
District-Group-FE	Yes	Yes	Yes	Yes
District-Year	Yes	Yes	Yes	Yes
Obs	7896	7896	7896	7896
Adj. within-R^2	0.0973	0.136	0.0702	0.0851

Notes: The dependent variable is counter-insurgency. Inci(GTD) and Fatal(GTD) are terror attacks and fatalities by major Baloch groups. Standard errors clustered at the district level in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

and BLA as the same group. Hence, it seems unlikely that the government changed its behavior towards our core groups of interest following KBM's death. Finally, more terror attacks by the Baloch terror groups positively relates to government counter-insurgency efforts.

The within group analysis shows that there seem to be capacity effects that one would expect following Bueno de Mesquita (2013) and that the government does not seem to be particularly affected by the split itself. Rather, it seems the case that the government reacts to group activity in general, which poses no challenge for our preferred interpretation of the effect, as competition between groups.

4-6.4 Semi-elasticity of armed groups on political violence

Our previous findings are consistent with competition effects between armed groups resulting from group proliferation. Thus, we are interested in the marginal effect of an additional armed group on the amount of political violence. We estimate 2SLS models where we use our previous interaction as an instrument to predict the number of active groups within a district. Note that by definition, we get at least one additional active group within most districts located in the Baloch province, namely the UBA.

Before we present the results, let us be transparent about a caveat. Using our interaction as an instrument assumes that the death of KBM only has a differential effect within Baloch districts compared to districts outside of Balochistan via an increase in the number of armed groups, specifically the UBA. Hence, we need to assume that the additional violence committed by the other well-established groups within Balochistan is a reaction to the additional competition and not driven by some other issue related to KBMs death. While our previous analysis is in line with this assumption, we cannot rule out alternative explanations entirely. Apart from this drawback, there is also a major benefit of the 2SLS analysis, namely addressing potential measurement error in the number of active groups.

Table 4-8 presents the results form the 2SLS estimation. The first stage result

Table 4-8 – 2SLS evidence

	Second Stage			
	Incidents	Fatalities	Ln Incidents	Ln Fatalities
	(1)	(2)	(3)	(4)
No. active groups	4.0887	6.3049	2.3991	1.7993
	(0.8452)***	(2.5344)**	(0.3652)***	(0.2851)***
Avg. no. act grps in act districts:	*Treatment group:* 2.3509		*Control group:* 1.7669	
Std dev no. act groups:	*Treatment group:* 1.6395		*Control group:* 1.6595	
	First Stage DV: No. of active groups			
$Baloch_i \times Post_t$	0.8781			
	(0.2035)***			
Controls	Yes	Yes	Yes	Yes
District-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Obs	3144	3144	3144	3144
Adj. Within-R^2	0.3095	0.3004	0.3789	0.3576
F-stat IV	18.59			
First stage adj. R^2	0.698			

Notes: Reports the 2SLS results using $Baloch_i \times Post_t$ as an instrument for the number of active groups operating within a district. The controls include log of population and geographical characteristics interacted with year dummies (see footnote 25). Standard errors clustered at the district level in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

in the lower panel shows that, on average, the number of active groups rises by just short of one. A point estimate of below one is to be expected, given that the UBA does not operate in all districts of Balochistan. Instrument power also seems fine, given that our first stage F-stat passes the conventional threshold comfortably.

The second stage results in column 1 and 2 are similar to our reduced form effects. The semi-elasticities reported in columns 3 and 4 are also close to our reduced form. An additional group increases the number of incidents by roughly 240% and fatalities by around 180%. On average, we have 2.4 active groups within a district[42]. This means that on average, the number of groups increases by roughly 37 %. As in the reduced form setting, the incident coefficients decrease if we use non-linear instrumental variable specifications Table 4-A13. Poisson and negative binomial methods coupled with control functions, provide estimates of between a 128% and 176% for incidents. In the case of fatalities, Poisson estimations obtain point estimates corresponding to an increase of 118%, while the negative binomial results point to an increase of 280%. We trust the conservative incidents estimates the most, since they are consistent with our reduced-form evidence and less volatile compared to the fatality estimates, nor as dependent on the exact model specification. Furthermore, we only include population as control variable in the negbin models due to convergence issues. Nevertheless, even the conservative estimates are quite sizeable given that an increase by one group – an increase in armed groups of on average 37% – leads to an increase in political violence of 135%. Additionally, the first stage residuals in Table 4-A13 point towards a sizable downward bias if one does not control for the endogeneity of the number of groups.

How heterogeneous is the effect across districts? Did other groups see the BLA's

[42]Conditional on having at least one attack in our sample.

Table 4-9 – 2SLS evidence: Heterogeneous treatment effects?

	Second Stage			
	Incidents	Fatalities	Ln Incidents	Ln Fatalities
	(1)	(2)	(3)	(4)
No. active groups	4.6398	10.1287	1.7409	1.5004
	(1.0095)***	(3.0336)***	(0.3502)***	(0.3111)***
	First Stage DV: No. of active groups			
$Treatshare_i \times Post_t$	5.2300			
	(0.8993)***			
Controls	Yes	Yes	Yes	Yes
District-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Obs	3144	3144	3144	3144
Adj. within-R^2	0.219	0.282	0.429	0.335
F-stat IV	33.77			
First stage adj. R^2	0.710			

Notes: Reports the 2SLS results using $Treatshare_i \times Post_t$ as an instrument for the number of active groups operating within a district. The controls include log of population and geographical characteristics interacted with year dummies (see footnote 25). Standard errors clustered at the district level in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

split as a chance to enter its areas of operation? We test this hypothesis by exploiting the intensive margin of BLA and UBA activity within our instrument. Table 4-9 shows a similar 2SLS regression as in Table 4-8. The first stage's interaction term is the post-treatment indicator times the share of BLA and UBA presence in a particular district. That is $treatshare_i$ ranges between 0 and 1. A $treatshare_i$ value of 1, indicates that the BLA or UBA launched at least one attack in the particular district in every year of the sample period. The maximum value of $treatshare_i$ is about 0.5. This corresponds to attacks every other year, on average. In these districts, the model predicts two additional groups, on average. The results support the notion that other groups might have seen the split of the BLA as a chance to enter districts with a high BLA presence.

4-7 Robustness tests

We perform several additional tests to understand the sensitivity of our findings, which we report briefly here and in greater detail in the Appendix.

We start by testing how our results are affected if we create our dependent variables from two separate datasets in Section 4-10. Section 4-B1 shows that using both incidents from GTD and GED increases the point coefficients of our baseline specification, although they are not statistically different from one another. We take this as suggestive evidence that our results are not driven by a change in strategy of the groups operating in the treatment districts towards events more likely to be covered by the GTD. Section 4-B2 investigates the problem of "potential" double-counting, which is introduced if one uses information from two datasets that, in theory, have an overlap of events that they code. Our probability-based approach to assess the likelihood of potential double counts suggests that double counting is around 10% for realistic scenarios in our case, which might explain the higher point coefficients in Section 4-B1. However, our results do not change if we exclude those potential double counts from our estimation.

We also further probe our concept of active armed groups (see Section 4-11). Section 4-C1 introduces the concept of potential active groups, defined as groups that are active anywhere in the country, and have been active at least once in a specific district. This approach loosens the requirement that a group needs to commit an attack in a specific district to be counted as active there, and in theory, allows for spatial choices of the armed groups that we do not observe. Again, our results remain remarkably robust. Note that the measure of potential active groups and active armed groups are reasonably well correlated (77%). The overlap highlights another property of our setting. Specifically, the armed groups in our sample seem to have well-defined areas of operation. We also extended the potential active armed groups measure to cover all districts falling within the convex hull of a groups incidents. Again our results remain stable. Section 4-C2 test if the inclusion of "one-hit-wonders" (Blomberg et al., 2010) in our measure of active armed groups affects our results. Note that "one-hit-wonders" are counted identically in both potential armed groups and armed groups counts since they commit only a single incident. Once more, our results remain stable.

Finally, we test if our effects are driven by the ethno-political representation of the ethnic groups living across our treatment and control districts (see Section 4-12). Given that the conflict in Balochistan has an ethnic origin, one might expect that our estimates could be biased by changing political representation of ethnic groups across our treatment and control districts (Bormann et al., 2019). Proxying for the ethno-political representation of both districts and provinces using data from the geocoded version of the Ethnic Power Relations dataset (GeoEPR) (Wucherpfennig et al., 2011; Vogt et al., 2015), we can reject this threat to our identification.

4-8 Conclusion

This paper studies the causal effect of an increase in the number of independent armed groups on political violence. While the arguments in favor of such a mechanism have long been present in the literature, we are the first paper to provide causal evidence on the matter.

Our estimates predict that one additional active armed group (corresponding to a 37 % increase in the number of groups within our sample) more than doubles organized political violence. The result highlights that the elasticity between armed groups and political violence is about two. These significant effects are consistent with competition between armed groups for local dominance. In a communication to the Indian newspaper *The Hindu* (Bhattacherjee, 2019), the BLA indicated that "they are planning to intensify the struggle against Pakistan as they remain 'the most popular' militant organization in Balochistan."

Exploiting group mergers and splits is a promising avenue to analyze how the number of armed groups involved in a conflict interacts with other determinants of conflict. The main advantage of exploiting group splits and mergers, is that the number of combatants (or foot soldiers) should remain constant in the short-run and that there are no selection effects in and out of the combat zone. Hence, researchers can better isolate how economies of scale and increased competition affect the amount of political violence perpetrated by armed groups. This is not to say that the situation in which a new group enters into a combat zone are of no interest. Quite the opposite, we believe it vital to compare our results to situations in which a new actor actively selects into a combat zone, such as the Islamic State starting operations within Afghanistan.

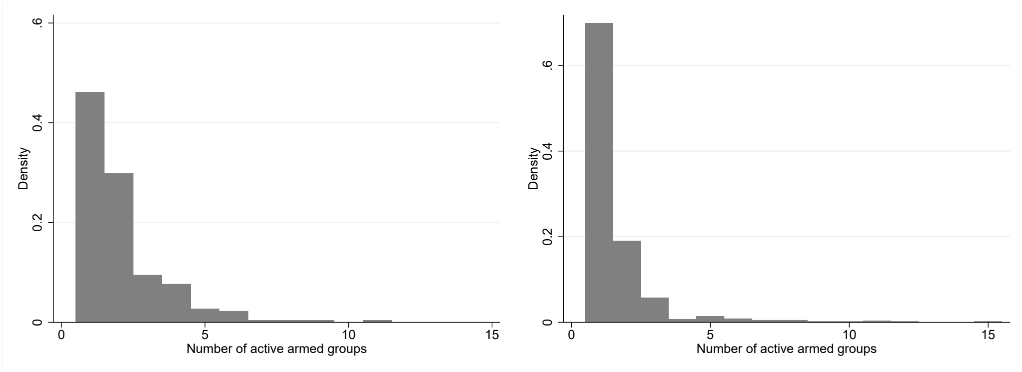
Finally, mergers and spits of armed groups have in general received little attention so far, which makes them a promising topic of future studies. Future research needs to trace the reasons why new groups form or split up and encroach upon the territories of other groups. Understanding within group dynamics is largely absent from the literature so far. We believe this to be a major obstacle when it comes to policy recommendations. Consider the evaluation of counter-insurgency efforts against a specific group for example. It is impossible to evaluate whether the policy can reduce political violence if we ignore how other groups are indirectly affected. Our study offers a toolkit to engage in those kind of studies by providing a method to calculate proxies for counter-insurgency efforts, by combining the GTD and GED databases. What is more, matching of incidences between the GTD and GED datasets enables researchers to holistically analyze political violence of armed groups and increase coverage.

4-9 Appendix

4-A1 Figures

Figure 4-A1 – Distribution: Number of armed group in and outside of Balochistan

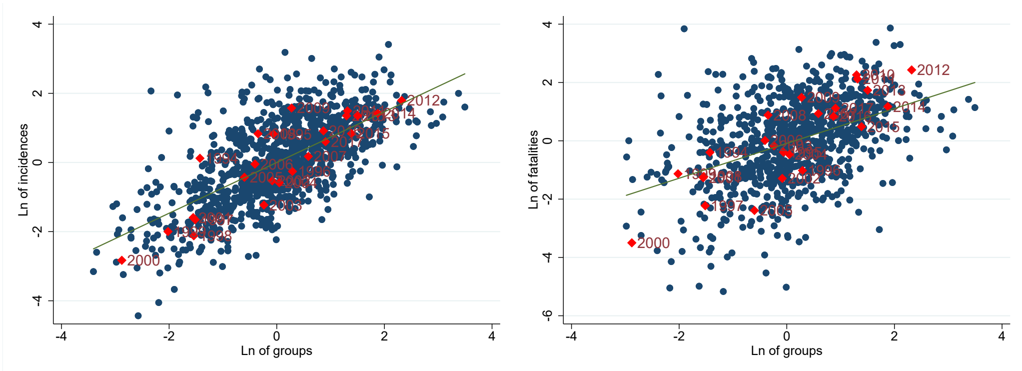
(a) Baloch province **(b)** Outside Baloch province



Notes: Panel A depicts the distribution of the number of *active* groups in district-years with at least one active group in Balochistan. Panel B depicts the distribution of the number of *active* groups in district-years with at least one active group in outside of Balochistan. *Active* means conducting at least one attack in a given year and district.

Figure 4-A2 – Armed groups and political violence (demeaned by country & year)

(a) Incidents-Group-Elasticity **(b)** Fatalities-Group-Elasticity

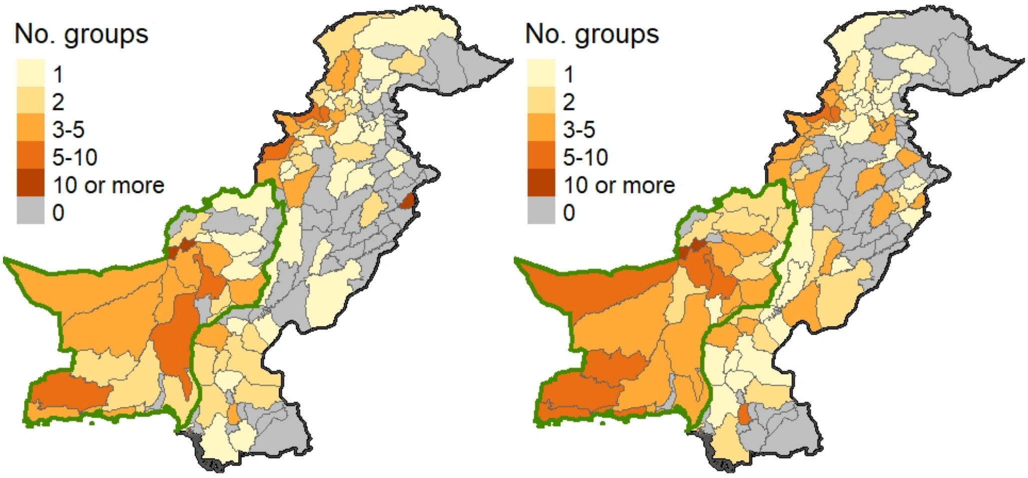


Notes: Depicts a scatter plot of the (log of) groups vs (log of) incidents & fatalities created by these groups, demeaned by country and year. The unit of observation is country-year. Pakistan is represented in red. The line illustrates the best linear fit using the global GTD sample between 1994 and 2018.

Figure 4-A3 – Number of armed groups across districts

(a) Number of groups within districts (pre-treatment)

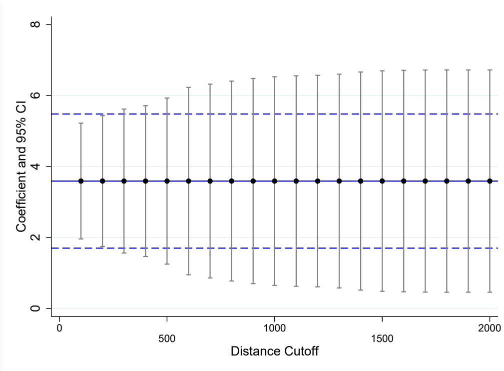
(b) Number of groups within districts (post-treatment)



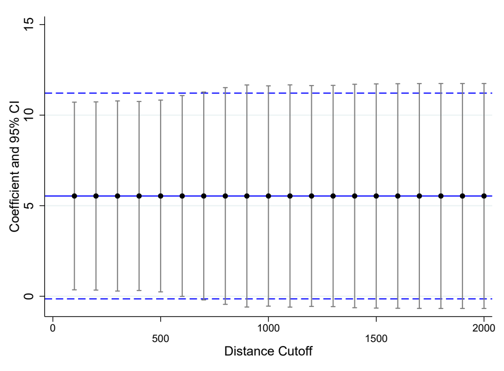
Notes: Panel A reports the number of groups active in districts in the pre-treatment period. Panel B plots the number of groups active within a district in the post-treatment group. The bold green line represents the borders of the Baloch province.

Figure 4-A4 – Baseline estimate: Arbitrary spatial clustering

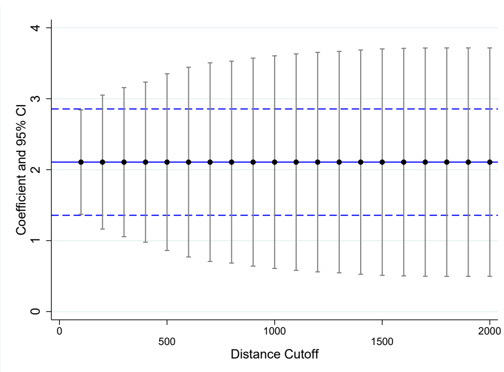
(a) Incidents



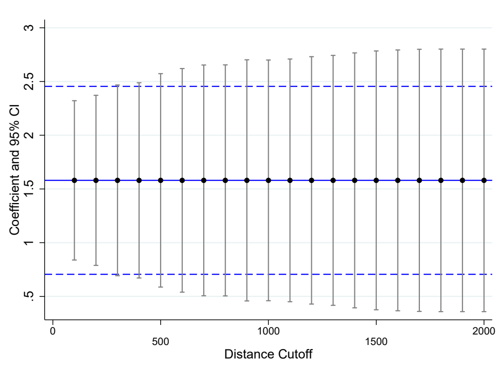
(b) Fatalities



(c) Ln Incidents



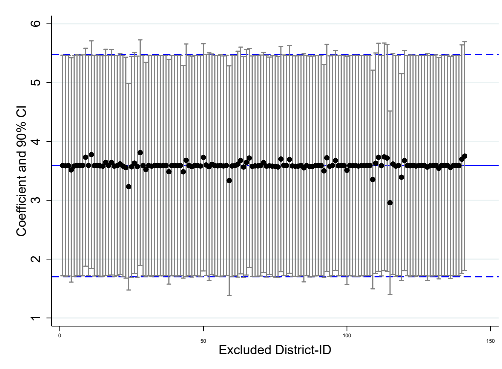
(d) Ln Fatalities



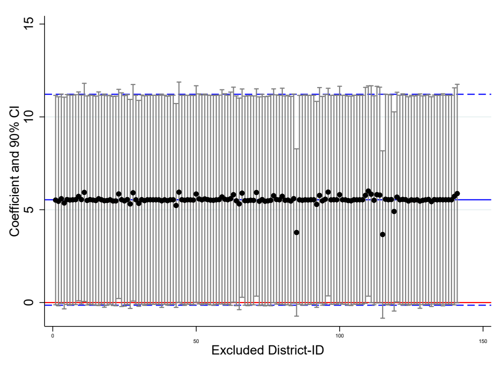
Notes: Reports the point coefficient and their accompanying 95% CIs for different distance thresholds of spatial clustering corresponding to columns 1 to 4 of Table 4-1. The horizontal line depict that actual baseline estimate and its accompanying 95% CIs from clustering on the district level.

Figure 4-A5 – Leave one out test: Districts

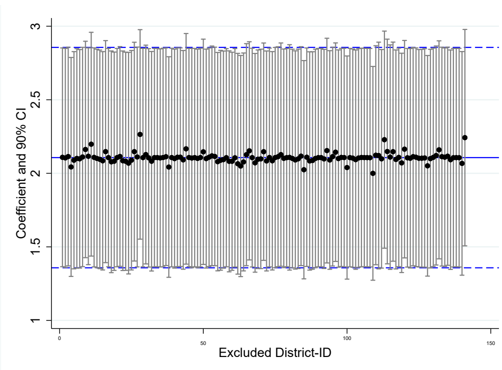
(a) Incidents



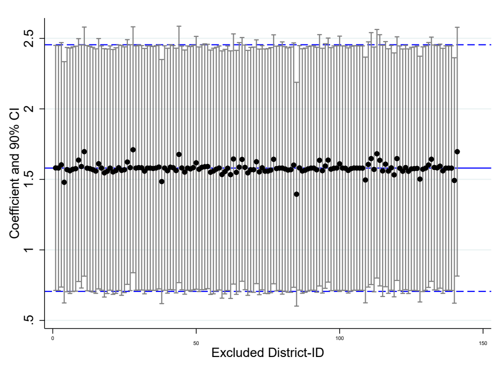
(b) Fatalities



(c) Ln Incidents

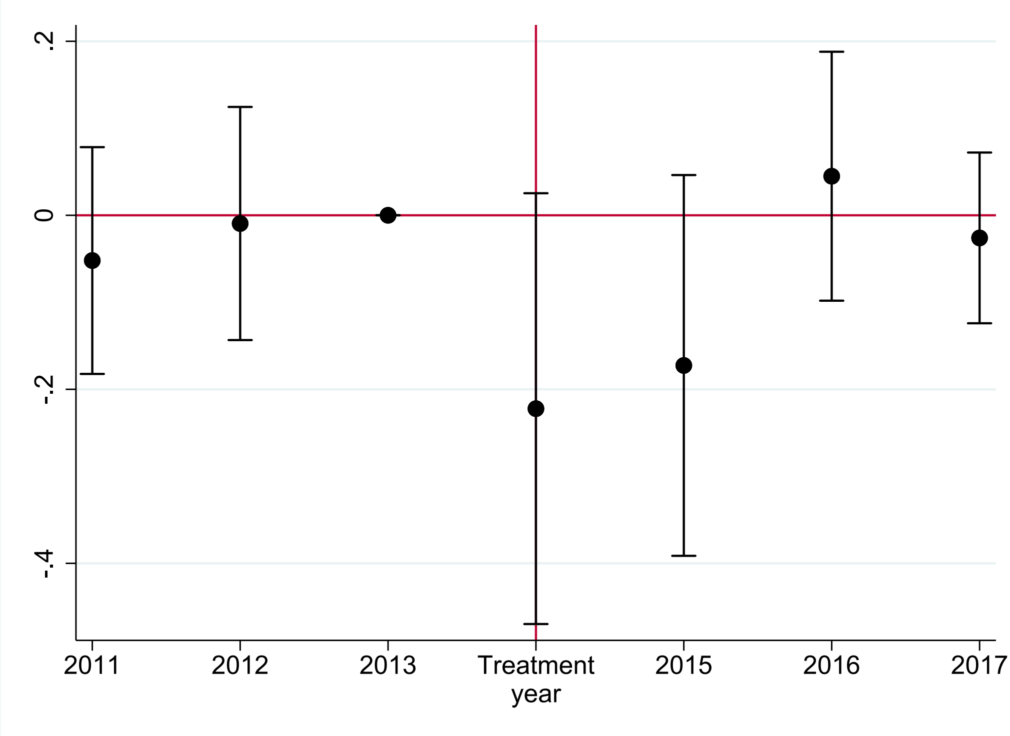


(d) Ln Fatalities



Notes: Reports the point coefficients of our baseline specifications in Table 4-1, dropping one district at the time together with their 95% CI. The blue horizontal line is the respective baseline coefficient (full sample) with its 95% CIs (dashed lines).

Figure 4-A6 – Event Study: Baloch secession groups



Notes: Reports the point coefficient and their accompanying 95% CIs for column 1 of Table 4-6. The controls include dummies binning the endpoint of the event window.

4-A2 Tables

Table 4-A1 – Treated groups (mergers and splits) and location of incidents and fatalities

Group name	Rest of Pakistan		Balochistan	
	Incidences	Fatalities	Incidences	Fatalities
Baloch Liberation Army (BLA)	11	17	195	433
United Baloch Army (UBA)	7	35	84	138
Harakat ul-Mujahidin Al-Almi (HuMA)	1	3	0	0
Harakat ul-Mujahidin (HuM)	1	6	0	0
Jaish-e-Mohammad (JeM)	3	5	0	0
Jamaat-ul-Ahrar (JuA)	19	93	0	0
Jundullah	22	311	3	14
Lashkar-e-Balochistan (LeB)	2	5	29	3
Lashkar-e-Jhangvi (LeJ)	97	738	81	805
Lashkar-e-Islam (LeI)	129	422	0	0
Taliban	1,442	6,330	88	640
Sipah-e-Sahaba/Pakistan (SSP)	15	59	1	1

Table 4-A2 – Descriptive statistics

	Mean	SD	Min	Max	N
Dependent variables					
Incidents	0.93	4.02	0.00	71.00	3,144
Incidents (at least one wounded)	-3.59	2.20	-4.61	4.26	3,144
Incidents (at least one fatality)	3.27	17.34	0.00	398.00	3,144
Counter insurgency	-3.63	2.43	-4.61	5.99	3,144
Ln fatalities	13.50	78.01	0.00	1,543.00	3,144
Fatalities GED+GTD	-3.10	3.03	-4.61	7.34	3,144
Ln Fatalities GED+GTD	0.65	3.00	0.00	61.00	3,144
Fatalities per incidence	0.55	2.58	0.00	45.00	3,144
Incidents vs. other Groups	0.63	4.46	0.00	124.00	3,144
Incidents vs. Gov.	0.06	0.55	0.00	22.00	3,144
Incidents vs. Infrastructure	0.40	2.04	0.00	43.00	3,144
Incidents vs. Business	0.11	0.89	0.00	31.00	3,144
Incidents vs. Citizens	0.08	0.49	0.00	9.00	3,144
	0.25	1.40	0.00	27.00	3,144
Treatment variables					
Treatshare (UBA or BLA)	0.04	0.09	0.00	0.52	3,144
Control variables					
Nr. active groups	0.41	1.06	0.00	15.00	3,144
Nr. other groups	0.34	0.93	0.00	14.00	3,144
Δ nr. other groups	0.01	0.49	-4.00	5.00	3,013
Nr. potential other groups	1.11	1.64	0.00	20.00	3,144
Δ nr. potential other groups	0.05	0.50	-5.00	6.00	3,013
Incidence concentration	0.15	0.33	0.00	1.00	3,144
Fatalities concentration	0.12	0.31	0.00	1.00	3,144
Ln population	13.49	1.11	8.66	16.07	3,144
Airport presence	0.07	0.25	0.00	1.00	3,144
Ln ruggedness	10.45	2.14	6.03	14.07	3,144
Ln elevation (in meters)	5.83	1.47	2.20	8.50	3,144
Ln road density	-9.50	0.80	-11.60	-6.57	3,144
Area share of discriminated ethnic groups (district)	0.15	0.30	0.00	1.00	2,921
Area share of junior partner ethnic groups (district)	0.25	0.39	0.00	1.00	2,921
Area share of powerless ethnic groups (district)	0.20	0.37	0.00	1.00	2,921
Area share of senior partner ethnic groups (district)	0.40	0.47	0.00	1.00	2,921
Group-level variables					
Incidents GTD	0.08	0.82	0.00	37.00	9,870
Incidents GTD + GED	0.13	1.22	0.00	47.00	9,870
Incidents asymmetric	0.09	0.87	0.00	37.00	9,870
Incidents non-state	0.08	0.82	0.00	37.00	9,870
Fatalities GTD	0.10	1.12	0.00	36.00	9,870
Fatalities GTD + GED	0.23	2.35	0.00	89.00	9,870
Fatalities asymmetric	0.12	1.36	0.00	51.00	9,870
Fatalities non-state	0.10	1.12	0.00	36.00	9,870

Table 4-A3 – Definition of the dependent variables in Table 4-4

Incidence against...	GTD targtype coding rule
Other groups	Terrorists/Non-State Militia
	Violent Political Party
Government	Military
	Police
	Government (Diplomatic)
	Government (General)
Public infrastructure	Airports & Aircraft
	Food or Water Supply
	Telecommunication
	Transportation
	Utilities
Business	Business
	Tourists
Citizens	Private Citizens & Property

Table 4-A4 – Baseline results: Alternative standard errors

	Incidents (1)	Fatalities (2)	Ln Incidents (3)	Ln Fatalities (4)
$Baloch_i \times Post_t$	3.5904	5.5364	2.1067	1.5800
	(1.1315)***	(2.8108)*	(0.3803)***	(0.4671)***
	[1.5985]**	[3.1686]*	[0.8213]**	[0.6232]**
Controls	Yes	Yes	Yes	Yes
District-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Obs	3144	3144	3144	3144
Adj. Within-R^2	0.235	0.164	0.136	0.101

Notes: Reports the baseline results in Table 4-1. Standard errors clustered at the division level in parenthesis, and allowing for spatial dependence with a distance cutoff of 2000 in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-A5 – Baseline results: PPML

	Incidents			*Fatalities*		
	(1)	(2)	(3)	(4)	(5)	(6)
$Baloch_i \times Post_t$	0.9903	1.0780	0.6847	0.9216	1.0364	0.4163
	(0.2283)***	(0.2284)***	(0.1639)***	(0.3191)***	(0.3120)***	(0.2514)*
Δ No. other groups		0.2256			0.2855	
		(0.0461)***			(0.0650)***	
No. other groups			0.4341			0.4779
			(0.0718)***			(0.0685)***
Controls	Yes	Yes	Yes	Yes	Yes	Yes
District-FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	2640	2640	2640	2448	2448	2448
R^2	0.7806	0.7779	0.7878	0.6552	0.6565	0.6756

Notes: Reports the baseline results in Table 4-1 using Poisson estimation method. Standard set of controls (population and geographical characteristics interacted with year dummies, see footnote 25) included but not reported. Standard errors clustered at the district level in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-A6 – Baseline results: Negative Binomial

	Incidents		*Fatalities*	
	(1)	(2)	(3)	(4)
$Baloch_i \times Post_t$	1.2092	1.2504	0.9838	0.9900
	(0.1710)***	(0.1584)***	(0.2161)***	(0.2097)***
Δ No. other groups		0.3271		0.5389
		(0.0599)***		(0.0909)***
District-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Obs	2592	2592	2328	2328
Log likelihood	-1697	-1655	-2237	-2188

Notes: Reports the baseline results in Table 4-1 using negative binomial estimation method. Standard set of controls is reduced to log of population due to convergence issues. Other group level specification is left out due to convergence issues. Standard errors are based on 250 bootstraps. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-A7 – Log DV intensive margin

	Ln Incidents			*Ln Fatalities*		
	(1)	(2)	(3)	(4)	(5)	(6)
$Baloch_i \times Post_t$	0.9577	0.9891	0.8326	0.7246	0.7892	0.5649
	(0.1781)***	(0.1743)***	(0.1385)***	(0.2655)***	(0.2754)***	(0.2786)**
Δ No. other groups		0.0983			0.1294	
		(0.0300)***			(0.0622)**	
No. other groups			0.2488			0.2468
			(0.0410)***			(0.0653)***
Controls	Yes	Yes	Yes	Yes	Yes	Yes
District-FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	561	561	561	434	434	434
Adj. within-R^2	0.192	0.205	0.267	0.0371	0.0457	0.0712

Notes: Reports the baseline results in Table 4-1 using a logarithmic transformation of the dependent variable. The controls include log of population and geographical characteristics interacted with year dummies (see footnote 25). Standard errors clustered at the district level in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-A8 – Baseline results: Additional fixed effects

	Incidents	Fatalities	Ln Incidents	Ln Fatalities
	(1)	(2)	(3)	(4)
$Baloch_i \times Post_t$	3.4631	3.4597	1.5282	1.2294
	(0.9192)***	(3.8167)	(0.3961)***	(0.4398)***
Controls	Yes	Yes	Yes	Yes
District-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
District-Decade-FE	Yes	Yes	Yes	Yes
District-trend	Yes	Yes	Yes	Yes
Obs	3144	3144	3144	3144
Adj. within-R^2	0.0302	-0.0240	0.0459	0.0279

Notes: Reports the baseline results in Table 4-1 but includes additional fixed effects. The controls include log of population and geographical characteristics interacted with year dummies (see footnote 25). Standard errors clustered at the district in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-A9 – Baseline results: no controls

	Incidents	Fatalities	Ln Incidents	Ln Fatalities
	(1)	(2)	(3)	(4)
$Baloch_i \times Post_t$	3.9227	6.2560	2.4174	1.9353
	(1.2703)***	(3.2534)*	(0.3948)***	(0.4625)***
Controls	No	No	No	No
District-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Obs	3144	3144	3144	3144
Adj. within-R^2	0.0339	0.00453	0.0570	0.0280

Notes: Reports the baseline results in Table 4-1 but excludes all control variables. Standard errors clustered at the district in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-A10 – Baseline results: severe and fatal incidents

	Incidents severe (1)	Incidents fatal (2)	Ln Incidents severe (3)	Ln Incidents fatal (4)
$Baloch_i \times Post_t$	1.6786	1.2365	1.5031	1.2911
	[0.7169]**	[0.6236]**	[0.3805]***	[0.3677]***
Controls	Yes	Yes	Yes	Yes
District-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Obs	3144	3144	3144	3144
Adj. Within-R^2	0.270	0.254	0.114	0.111

Notes: Reports the baseline results in Table 4-1 using only incidents in which at least one person got wounded (severe) or died (fatal). The controls include log of population and geographical characteristics interacted with year dummies (see footnote 25). Standard errors clustered at the district in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-A11 – DiD: Within Baloch separatist groups (BLA & UBA)

	Inci (1)	Inci all (2)	Inci civil (3)	Fatal (4)	Fatal all (5)	Fatal civil (6)
$BLA_{ij} \times Post_t$	-0.0211	-0.1618	-0.0630	0.1721	0.0230	0.0528
	(0.0679)	(0.0850)*	(0.0535)	(0.1056)	(0.1875)	(0.1084)
District-Group-FE	Yes	Yes	Yes	Yes	Yes	Yes
District-Year	Yes	Yes	Yes	Yes	Yes	Yes
Obs	7896	7896	7896	7896	7896	7896
Adj. within-R^2	0.374	0.424	0.337	0.288	0.357	0.291

Notes: BLA and UBA are treated as one group. Standard errors clustered at the district level in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-A12 – DiD: Within Non-Baloch separatist groups (BLA & UBA)

	Inci (1)	Inci all (2)	Inci civil (3)	Fatal (4)	Fatal all (5)	Fatal civil (6)
$BLA_{ij} \times Post_t$	0.0741	-0.0952	-0.0145	0.1404	-0.0366	-0.0298
	(0.0308)**	(0.0810)	(0.0420)	(0.0872)	(0.1894)	(0.1139)
Ideology Controls × year	Yes	Yes	Yes	Yes	Yes	Yes
District-Group-FE	Yes	Yes	Yes	Yes	Yes	Yes
District-Year	Yes	Yes	Yes	Yes	Yes	Yes
Obs	57246	57246	57246	57246	57246	57246
Adj. within-R2	0.0529	0.299	0.353	0.273	0.248	0.223

Notes: BLA and UBA are treated as one group. Ideology controls are extensive dummies created from the three ideology dimension in Kis-Katos et al. (2014). Standard errors clustered at the district level in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-A13 – Nonlinear instrumental variable results

	Second Stage			
	Poisson		*Negative Binomial*	
	Incidents	Fatalities	Incidents	Fatalities
	(1)	(2)	(3)	(4)
No. active groups	1.280***	1.186**	1.763***	2.834***
	(0.440)	(0.525)	(0.475)	(0.847)
Residual 1st-stage	-0.942**	-0.749	-1.239***	-0.731
	(0.407)	(0.580)	(0.452)	(0.760)
	First Stage DV: No. of active groups			
$Baloch_i \times Post_t$	0.9569		0.9031	
	(0.2117)***		(0.2214)***	
Controls	Yes	Yes	Yes	Yes
District-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Obs	3384	3384	3384	3384
Log likelihood	-1495	-5580	-1915	-2526
F-stat IV	20.432		16.652	
First stage adj. $Within - R^2$	0.4330		0.2783	

Notes: Standard errors of the second stage are based on 250 bootstraps. First stage is estimated with OLS. Standard set of controls is reduced to population due to convergence issues. Note that we use population instead of ln(population) due to the nonlinear transformation introduced by Poisson and negative binomial second stages. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

4-10 Counting incidents and fatalities

We take a closer look at how the incidents and fatalities reported by the different databases GTD and GED enter our specifications. We start by testing the robustness of our results regarding different issues in the data, such as if an armed group has officially claimed an event. We then proceed and discuss further what problems arise if one combines events of organized political violence across databases and how we deal with those issues.

4-B1 Counting all forms of organized political violence

The main specifications primarily rely on incidents and fatalities provided by the GTD. The reason is that the GTD has the best coverage for most of the actors we are interested in (see Section 5-3.1). However, having the best coverage for our actors of interest among the available databases has the potential to bias our results. The GTD, by its definition, focuses on organized political violence that fits their definition of terrorism. Thus, if following our treatment, all groups would switch to committing more violence fitting the GTD definition, it could be that GTD provides us the best coverage, without substantially changing the overall violence level. For example, following treatment, the actors could be involved in fewer events that fulfill the criteria of internal armed conflict, which is the primary focus of the GED database. We test for the aggregated effect across databases in Table 4-B1, where we replicate our baseline table including all events committed by armed groups from the GTD, and GED.[43]

Table 4-B1 – All incidents and fatalities

	Incidents	Fatalities	Ln Incidents	Ln Fatalities
	(1)	(2)	(3)	(4)
$Baloch_i \times Post_t$	4.7563	19.4801	1.7329	1.5295
	(1.3502)***	(14.4061)	(0.3549)***	(0.4600)***
Controls	Yes	Yes	Yes	Yes
District-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Obs	3144	3144	3144	3144
Adj. within-R^2	0.303	0.110	0.496	0.460

Notes: Replicates our 2SLS specifications combining incidents and fatalities form GTD and GED. Standard set of controls (population and geographical characteristics interacted with year dummies, see footnote 25) included but not reported. Standard errors clustered at the district level in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-B1 shows that the size and statistical significance of our point estimates remains roughly constant across our specifications.[44] Thus, the competition effect, which we obtained in our main specifications, does not seem to be driven by a switch in a strategy that is over-reported by a specific database.

A related issue is our classification of an armed group. Recall that we only count actors as armed groups if they have a unique name that identifies them; hence, we

[43]See Section 5-3.1 for our definition of an armed group.

[44]An exception is the fatality count, which tends to be less stable across different specifications.

exclude events from actors such as "Tribesmen" or "Gunmen". However, excluding the events committed by those actors might also bias our results in unexpected ways. Furthermore, given that both the GTD and GED rely on publicly available data, those names could also reflect uncertainty about the actual perpetrator of the event. Fortunately, the GTD codes if an armed group has officially claimed an event, which allows us to test if our results are driven by uncertainty about events or our group definition.

In Table 4-B2, we replicate our core specification on incidents and fatalities, using all events included in the GTD in column 1 and 2, those claimed by a group in column 3 and 4, and finally all events included in the GTD and GED in column 5 and 6.

Table 4-B2 – All events

	All GTD Events		*Claimed GTD Events*		*All GTD & GED Events*	
	Incidents	Fatalities	Incidents	Fatalities	Incidents	Fatalities
$Baloch_i \times Post_t$	6.6975	6.7272	3.4439	4.7646	7.8635	20.6708
	(2.0678)***	(3.3198)**	(0.9636)***	(2.8339)*	(2.2933)***	(14.8812)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
District-FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	3144	3144	3144	3144	3144	3144
Adj. Within-R^2	0.202	0.166	0.296	0.178	0.158	0.0144

Notes: The controls include log of population and geographical characteristics interacted with year dummies (see footnote 25). Standard errors clustered at the district level in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Again, we obtain economically more significant effects in columns 1, 2, 5, and 6, which suggests that our competition effect also influences less organized actors. Furthermore, columns 3 and 4 show comparable estimates to columns 1 and 2 of Table 4-1, highlighting that the armed groups that we identify seem to claim a lot of the incidents they commit publicly. Summing up, our results seem not to be driven by GTD specific coding rules.

4-B2 Double counting organized political violence

Our larger estimates using both GTD and GED events of organized political violence could be a result of double-counting. Double counting arises if both the GED and GTD code the same incidents for the same groups. Even though they have different definitions of organized political violence, this is not implausible, at least for a subset of events that might fit both definitions.

Testing for double counting is not straight forward, due to two reasons. First, GTD and GED have slightly different group names and different levels of aggregation. Generally, the level of aggregation is usually higher in the GED compared to the GTD. For example, GED will code a group "X", and the GTD will code the same group "X" as "$X - 1$" referring to some faction within "X" and "$X - 2$", referring to another faction within "X". Second, each event is coded based on publicly available source material subject to human interpretation. Thus, GTD and GED may attribute events to different actors due to conflicting and or different source material. This may also lead to alternative coding decisions concerning the day or exact location of a specific event.

The first issue is easily solved by harmonizing the group names. In practice, we aggregate the GTD group names up to the GED group name for all matches.[45] The second issue has no clear solution. Hence, we allow for varying levels of time and location precision in both datasets.

With these limitations in mind, we propose a solely uncertainty based approach to address the issue. Specifically, we quantify the likelihood that a GED event is a potential double count of a GTD event, depending on the distance both in space and time from one GED event committed by group i to all GTD events also committed by group i. The advantage of our approach compared to other possibilities, i.e., checking the source material of each incident, is the scalability for samples containing more than 10,000 events, as in our setting. Furthermore, it is transparent and allows the reader to decide with which buffer size she is comfortable with, instead of crosschecking our individual coding decisions.

The procedure of incidence-matching is simple. We classify a GED event committed by group i as being a potential double-count, if the temporal distance to any GTD event committed by the same group i is within a bandwidth t starting from $+/-$ one day and simultaneously below a certain distance threshold d starting from one kilometer. To allow for multiple different events within a close proximity at the same time period, we require the number of people killed reported by the GTD to fall within the bounds (low and high estimate) of the fatality count provided by GED.[46] Note that the matching of fatalities becomes more important as soon as the temporal and distance buffer increase, as the likelihood of false positives increases with wider buffers. Note that for this approach to be valid, we need to assume that double counting is only an issue between databases but not within. We do not find this assumption problematic since both databases conduct internal quality control, and it seems unlikely that they systematically misinterpret the set of sources they judged to be meaningful. Finally, the approach may also be performed without matching groups and fatalities from both databases. The only effect this has is that one is more likely to classify GED events as potential double counts, which in truth are not. In general, the approach has a clear trade-off between the likelihood of committing type 1 vs. type 2 errors, depending on the thresholds and the same-group criterion.

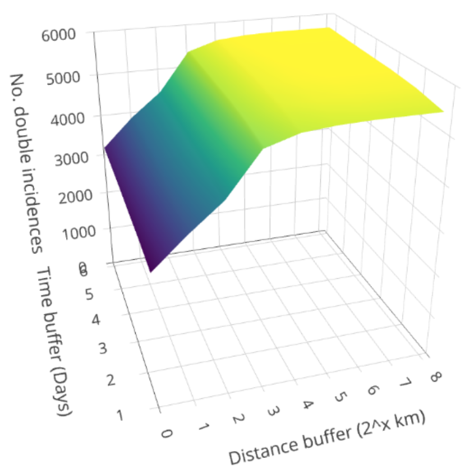
We illustrate the impact on the double-counts for each criterion. Panel A of Figure 4-B1 shows the the naive approach (excluding the same group name and fatality criteria). The number of GED events that are considered potential double counts of GTD events for parameter constellations of daily temporal bandwidth $t = (1, 2, 3, 4, 5, 6)$, and a distance threshold ranging from $d = (2^0, 2^1, 2^2, 2^3, 2^4, 2^5, 2^6, 2^7, 2^8)$, which corresponds to 1, 2, 4, 8, 16, 32, 64, 128, and 256 kilometers, respectively. Note that the temporal threshold affects the potential double count status of a GED event much less, compared to the distance threshold. As soon as we use a distance threshold of 32km, every GED event is a potential

[45]The specific matching of group names between the GTD and GED dataset is documented in Section 4-13

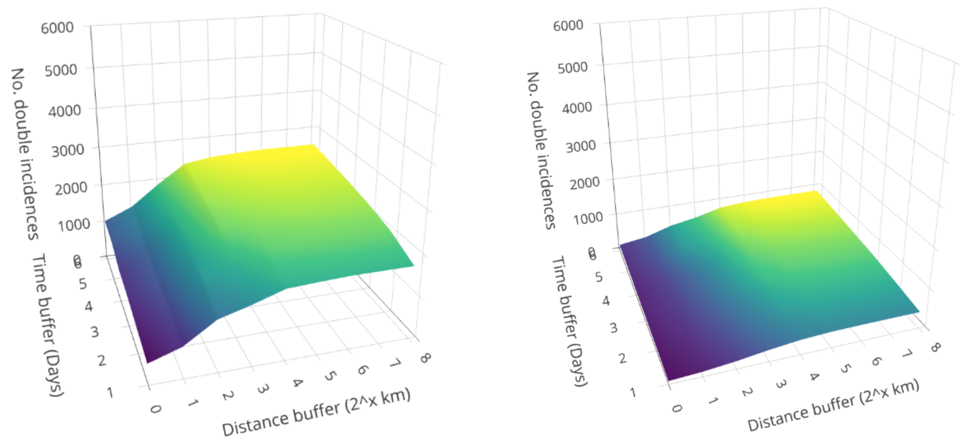
[46]Both the GED and GTD count all fatalities related to a specific incidents. If our approach is applied to sources using different methods for counting fatalities, this criteria should be dropped from the procedure.

Figure 4-B1 – Nr. of incidences of GTD and GED double coding

(a) Without group name matching **(b)** With group name matching



(c) With fatality matching **(d)** With group name and fatality matching



double count. In other words, whenever a GED event is coded in any day during our sample period, there is at least 1 GTD event coded within 32km distance of that GED event.

Panel B of Figure 4-B1 replicates the approach but applies the same-group name criterion. The picture becomes much more nuanced. Raising the distance threshold affects the number of assigned double counts beyond 32km. Furthermore, the interaction between the temporal and distance buffer is more pronounced. Lastly, the maximum amount of GED incidents flagged as potential double counts is 65% of those in the naive approach.

Panel C and D show that the inclusion of the fatality-match decreases the number of assigned double-counts dramatically. The maximum amount of assigned double counts falls to just above 2000 without name matching and below 1000 if one includes the name match. The general effect of increasing the distance and temporal thresholds remains similar to the respective approach without fatality matching.

We conclude that the naive approach may involve too many type 1 errors. Type

1 errors are likely to be high if one ignores the same name criterion, given a high number of armed groups active within Pakistan. Hence, the name-matching seems a necessary condition for a meaningful application of the approach, in our setting. The fatality criterion seems more optional if one already includes the same-name requirement and keeps the distance thresholds moderate. Still, it is unclear when the trade-off between type 1 and type 2 errors is minimized for the temporal and distance buffers.

Next, we assess the stability of our core results concerning double-counting. Since we have no clear guidance regarding the optimal thresholds for t and d we test the stability of our results for all combinations of t and d introduced above. Note that there is no upper limit on the combination one could test. Nevertheless, we restrict ourselves to the introduced set for brevity. We start with the naive approach for illustrative purposes.

Panels A to F of Figure 4-B2 plot the point coefficients of our baseline specification (column 1 Table 4-1) for increasing distance thresholds across different time buffers, starting with $+/-$ one day in Panel A, in the upper part of each panel and the fraction of GED events in use (not flagged as a potential double-count) in the lower panel. Furthermore, each panel plots the baseline estimate of column 1 Table 4-1 with its 95% CIs (black solid and dashed lines), as well as the estimate using all events attributed to armed groups in GTD and GED (corresponding to column 1 Table 4-B1) with its 95% CIs (blue solid and dashed lines).

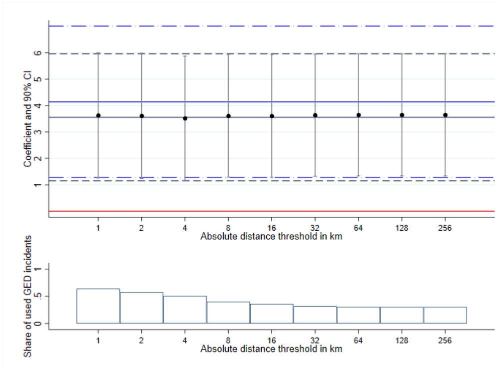
Figure 4-B2 shows that our core results seem relatively robust to double-counting since we cannot reject that they are identical to our baseline effect of the effect from Table 4-1. Note that we also fail to reject that the baseline effect is the same when using only GTD compared to using both GTD and GED events.

Panels A to F of Figure 4-B3 report the results for the more nuanced approach relying on the same-name criterion. Again the results remain remarkably stable. The major difference between the two approaches is that the fraction of included GED events is much more stable in Figure 4-B3 compared to Figure 4-B2. The stable result is to be expected since the same-name criterion drastically reduces the set of potential double counts regardless of the buffers. This pattern does not change if one adds the fatality criteria to either approach. In fact the only major difference is that both approaches use a higher fraction of the GED events, while the decline in usable GED events in the distance and temporal buffer remain similar.[47] In summary, the results make us confident that our combined results using the full set of available events of organized political violence, is not biased by some systematic measurement error due to double counting.

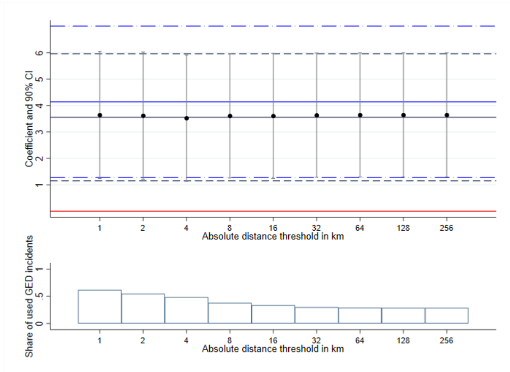
[47] Results nor reported but available upon request.

Figure 4-B2 – Potential double coding: GTD & GED

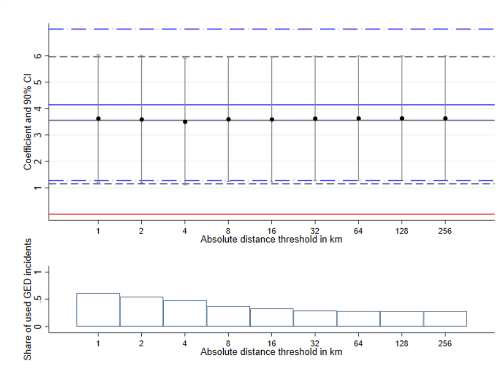
(a) A: +/- 1 days



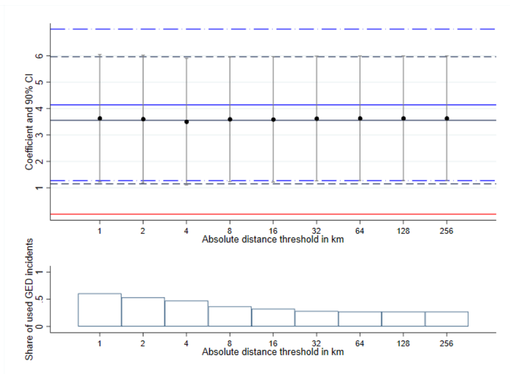
(b) B: +/- 2 days



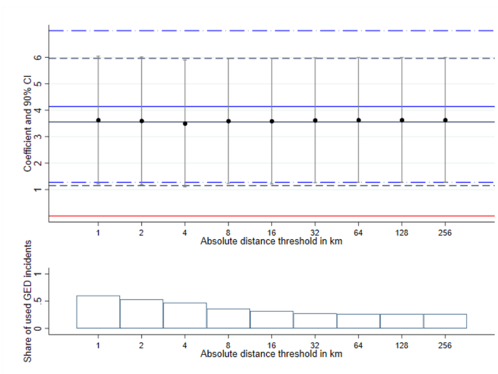
(c) C: +/- 3 days



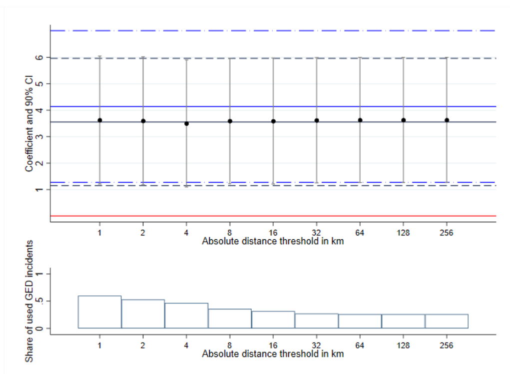
(d) D: +/- 4 days



(e) E: +/- 5 days



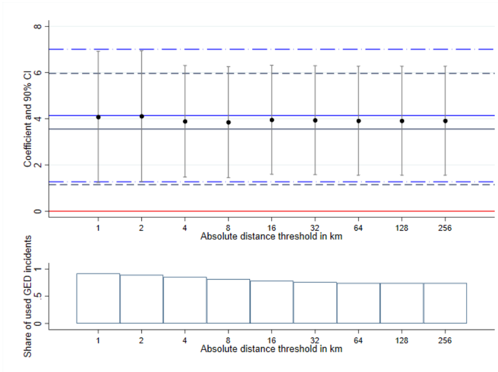
(f) F: +/- 6 days



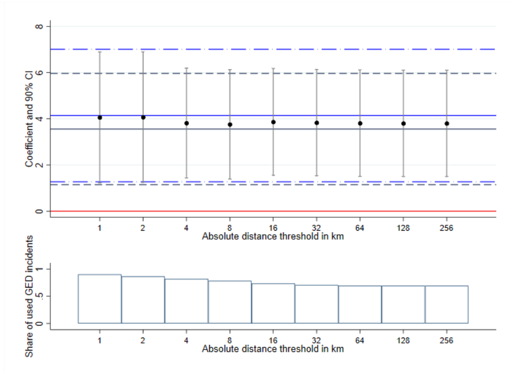
Notes: Reports the point coefficient and their accompanying 95% CIs for different potential double-counting thresholds both in time and space. The horizontal dark-grey solid line is the baseline coefficient using only GTD incidents. The two horizontal dark-grey dashed lines are the accompanying 95% CIs. The horizontal blue solid line is the coefficient using all GED and GTD incident ignoring potential double counting. The two horizontal blue dashed lines are the accompanying 95% CIs.

Figure 4-B3 – Potential double coding (Same Names): GTD & GED

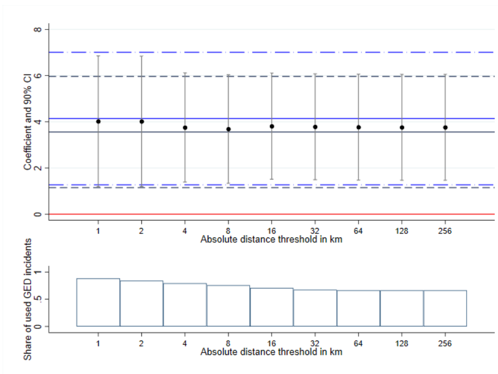
(a) A: +/- 1 days



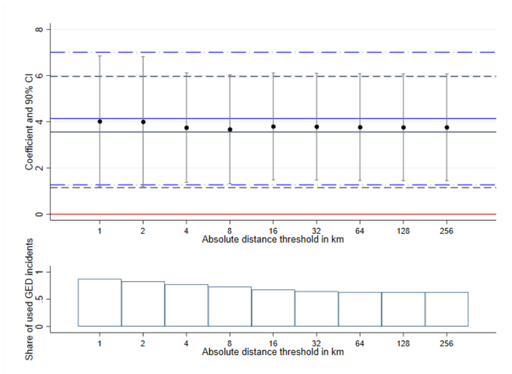
(b) B: +/- 2 days



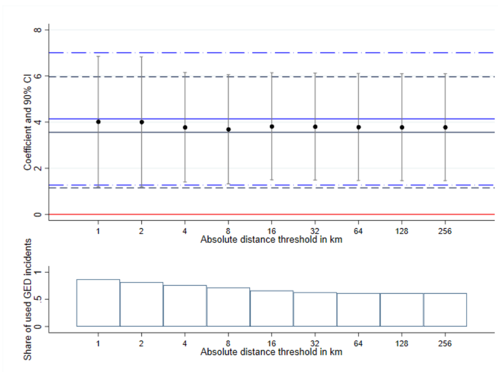
(c) C: +/- 3 days



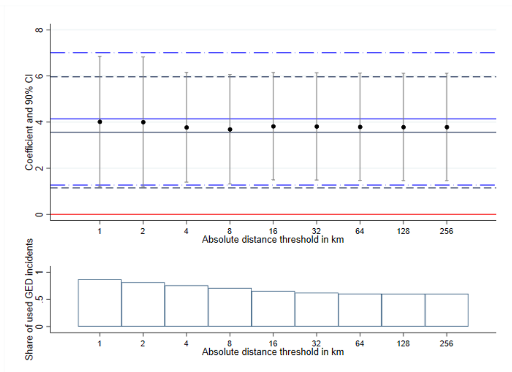
(d) D: +/- 4 days



(e) E: +/- 5 days



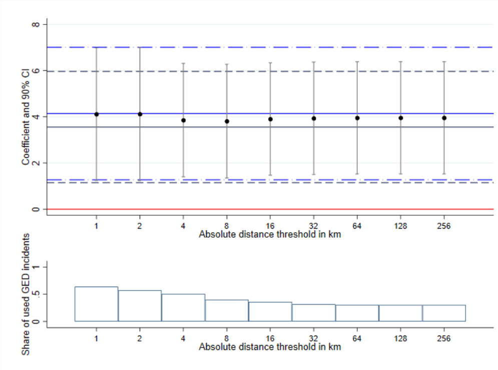
(f) F: +/- 6 days



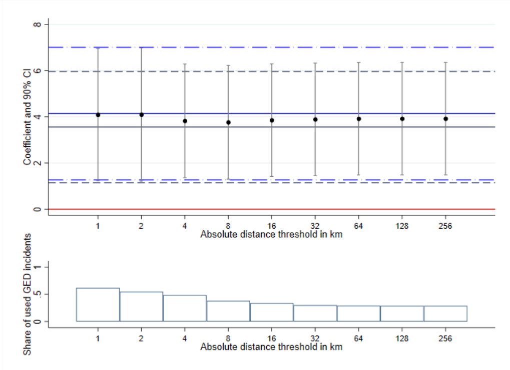
Notes: Reports the point coefficient and their accompanying 95% CIs for different potential double-counting thresholds both in time and space. The horizontal dark-grey solid line is the baseline coefficient using only GTD incidents. The two horizontal dark-grey dashed lines the accompanying 95% CIs. The horizontal blue solid line is the coefficient using all GED and GTD incident ignoring potential double-counting. The two horizontal blue dashed lines the accompanying 95% CIs.

Figure 4-B4 – Potential double coding (matching fatalities): GTD & GED

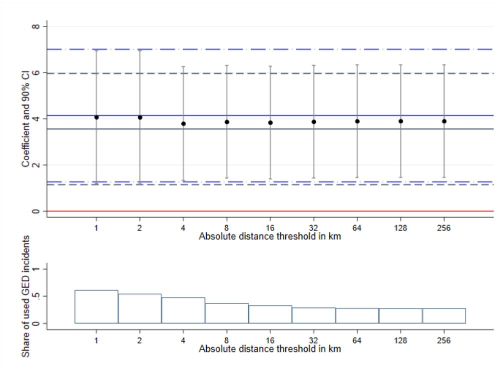
(a) A: +/- 1 days



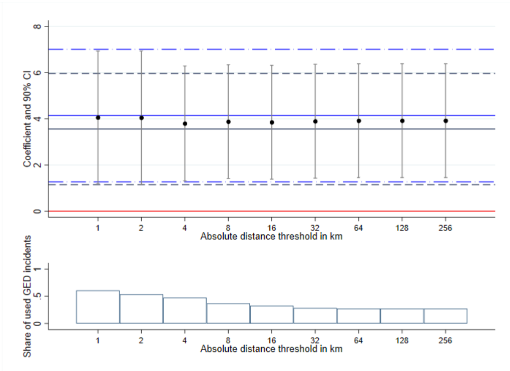
(b) B: +/- 2 days



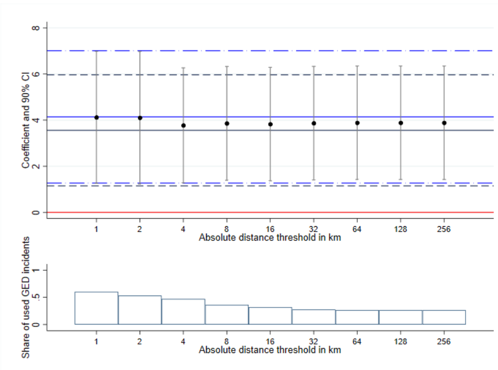
(c) C: +/- 3 days



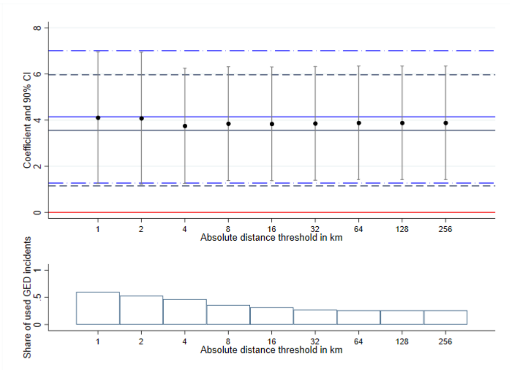
(d) D: +/- 4 days



(e) E: +/- 5 days

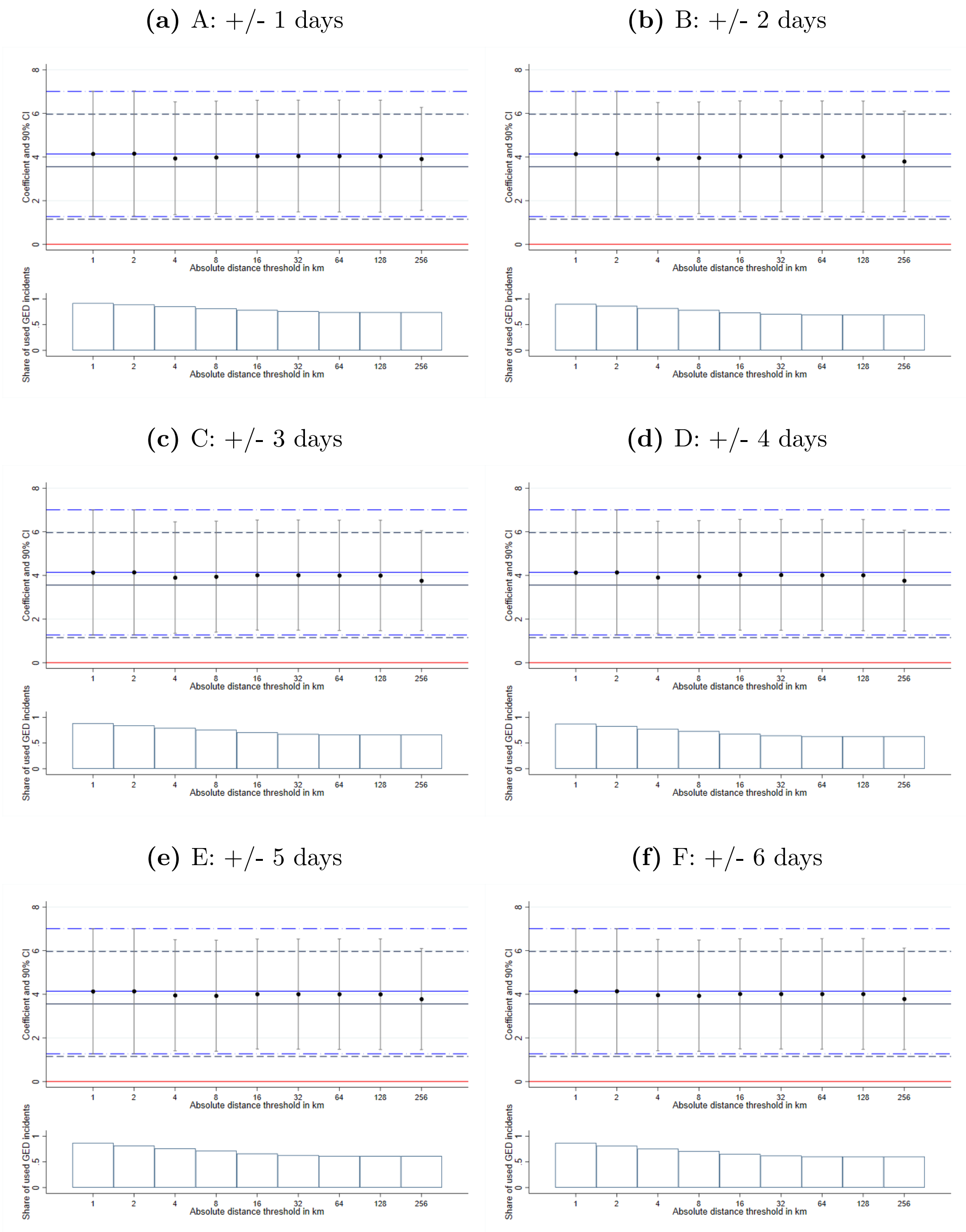


(f) F: +/- 6 days



Notes: Reports the point coefficient and their accompanying 95% CIs for different potential double-counting thresholds both in time and space. The horizontal dark-grey solid line is the baseline coefficient using only GTD incidents. The two horizontal dark-grey dashed lines are the accompanying 95% CIs. The horizontal blue solid line is the coefficient using all GED and GTD incident ignoring potential double counting. The two horizontal blue dashed lines are the accompanying 95% CIs.

Figure 4-B5 – Potential double coding (Same Names & matching fatalities): GTD & GED



(a) A: +/- 1 days

(b) B: +/- 2 days

(c) C: +/- 3 days

(d) D: +/- 4 days

(e) E: +/- 5 days

(f) F: +/- 6 days

Notes: Reports the point coefficient and their accompanying 95% CIs for different potential double-counting thresholds both in time and space. The horizontal dark-grey solid line is the baseline coefficient using only GTD incidents. The two horizontal dark-grey dashed lines the accompanying 95% CIs. The horizontal blue solid line is the coefficient using all GED and GTD incident ignoring potential double-counting. The two horizontal blue dashed lines the accompanying 95% CIs.

4-11 Counting independent groups

How many groups are competing within a district at any point in time? Given that our competition argument is based primarily on the number of active armed groups within a local market of violence, this question is of paramount importance. However, as in the case of measuring organized political violence, measurement choices are abundant, theoretical guidance is limited, and empirical best practice absent.

Recall, the number of active groups in our main specifications is a simple count of the armed groups that commit at least one incident within a district in a given year. Yet, one might argue that the actual incident committed is a strategic choice that maximizes utility over several dimensions for the group, e.g., ease of committing the attack vs. potential payoff (Marineau et al., 2020). If strategic considerations drive actual attacks, counting groups only as active within a district if they commit an attack during a year is an imperfect measure of their presence.[48] In turn, this will lead to an imperfect count of active groups and thus an imperfect proxy for group competition. It seems plausible that group effort is a result of the actual competition and not the imperfect perceived one. In general, we assume that local groups, as well as the government, have better information about the currently active groups within a district. Another issue closely related is the treatment of "one-hit wonders" Blomberg et al. (2010), defined as armed groups that only commit one attack during our sample. In our main analysis, we exclude those armed groups entirely. Nevertheless, it seems prudent to test if our coding of active groups is sensitive to them.

4-C1 Active armed groups & potential active armed groups

In this section, we propose an alternative measure of active groups, which we call "potential active groups". We define a group as potentially active in all districts in which it has ever been active (during our sample) if it commits at least one incident in any district in the current year. The number of potentially active groups is then simply the sum of all potential groups within a district. Note that this measure will, by definition, always be greater or equal to the number of active groups within a district, since being active in one district automatically assumes potential group activity in any other district where the group has ever been active during our sample period.

Table 4-C1 replicates Table 4-2 with the difference that the change of other groups and the number of other groups in the upper and lower panel is replaced by the change and number of potential active groups. Once more, our results are remarkably stable. None of the coefficients is statistically different from the once using the active number of groups instead of the potential one, either in changes or levels.

Instead of counting a group as active in all districts in which it ever operates, we can also define an area of operation in which we count a group as active whenever it is active somewhere. We follow König et al. (2017) and define a group's area of operation as the convex hull drawn around its incidents over the sample period. Specifically, we treat all districts as belonging to the group's area of operation if the

[48] Groups could also have the goal to be unpredictable (Jaeger and Paserman, 2008).

Table 4-C1 – Controlling for potential other groups

	Incidents (1)	Fatalities (2)	Ln Incidents (3)	Ln Fatalities (4)
$Baloch_i \times Post_t$	2.2487	0.3628	1.6310	1.1495
	(0.7815)***	(2.1562)	(0.3555)***	(0.4063)***
Δ No. potential other groups	1.4739	5.7693	0.5294	0.4879
	(0.2418)***	(1.3040)***	(0.1012)***	(0.1219)***
Adj. Within-R^2	0.325	0.241	0.187	0.134
	Potential other groups in levels			
$Baloch_i \times Post_t$	3.6176	5.6771	2.1174	1.5930
	(0.9699)***	(2.9068)*	(0.3807)***	(0.4448)***
No. potential other groups	0.6692	1.9767	0.1643	0.0797
	(0.1454)***	(1.2948)	(0.0883)*	(0.0840)
Adj. Within-R^2	0.240	0.167	0.137	0.101
Controls	Yes	Yes	Yes	Yes
District-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Obs	3144	3144	3144	3144

Notes: The controls include log of population and geographical characteristics interacted with year dummies (see footnote 25). Standard errors clustered at the district level in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

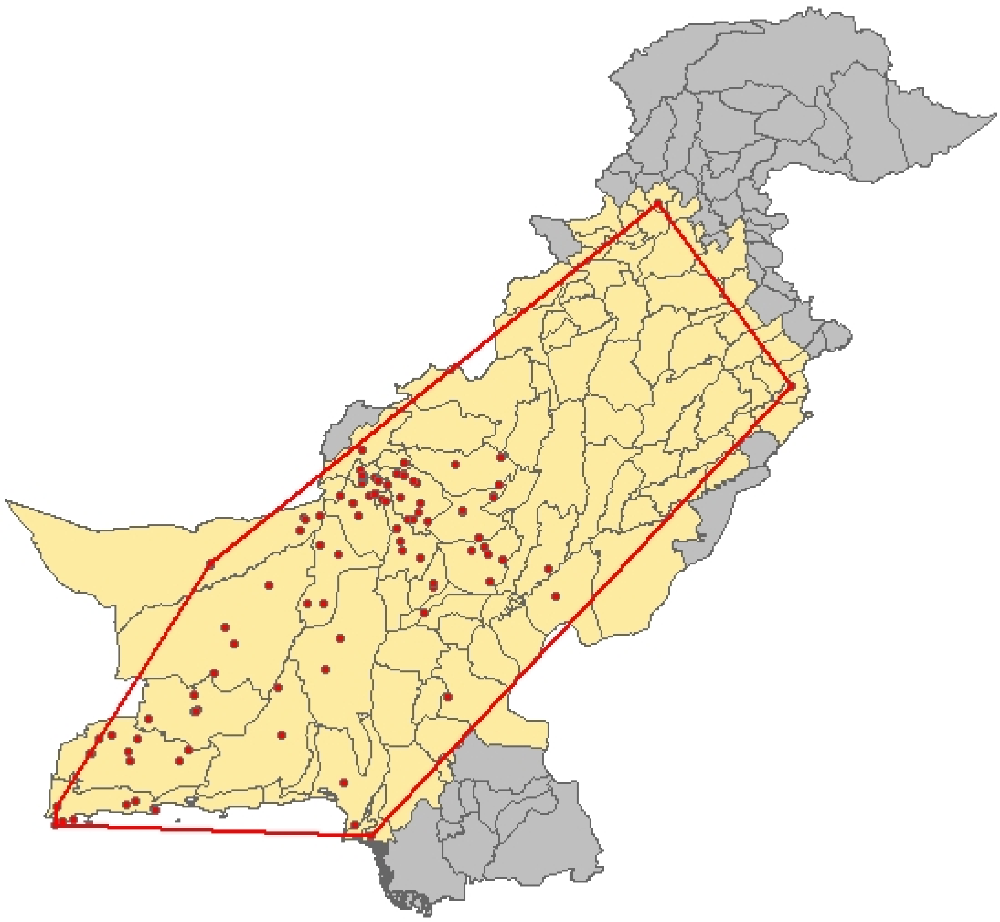
convex hull of incidents intersects them. Figure 4-C1 below illustrates the area of operations for the BLA based on this definition. Note that the convex hull is at the opposite end of our standard measure of group presence. In the case of the BLA three single attacks outside Baluchistan are enough to flag many districts outside of Balochistan with a BLA presence.

Table 4-C2 replicates Table 4-C1 using the area of operations instead of the districts in which a group ever operates to count the number of potential other groups. As before, we cannot reject that the coefficients are the same. Note, however, that the coefficient of the other group count is no longer statistically significant, which is likely driven by the districts that fall within the convex hull of a group but that never experience organized political violence perpetrated by that group.

4-C2 Active armed groups: One-hit wonders

We now include "one-hit wonders" Blomberg et al. (2010) into our measure of potential active groups. Note that one-hit wonders affect the measure of active and potential active groups in the same way since, by definition, they only commit one incident in one district in one year. Table 4-C3 reports the results of another replication of Table 4-2, including the one-hit-wonders in the number of potential groups. Columns 1 to 4 of Table 4-C3 show that our results are not sensitive to the inclusion of one-hit wonders.

Figure 4-C1 – Convex hull of BLA incidents



Notes: Depicts the convex hull of all BLA incidents (red line) and the incident locations of the BLA (red dots). Districts intersected by the convex hull are colored and counted as the area of operations for the BLA following this approach.

Table 4-C2 – Controlling for potential other groups area of operation (convex hull)

	Incidents (1)	Fatalities (2)	Ln Incidents (3)	Ln Fatalities (4)
$Baloch_i \times Post_t$	3.6119	4.9624	2.1067	1.5583
	(0.9979)***	(2.7818)*	(0.3831)***	(0.4444)***
Δ No. potential other groups	0.1099	-1.5900	0.0016	-0.0801
	(0.1914)	(1.1137)	(0.0829)	(0.0944)
Adj. Within-R^2	0.234	0.165	0.135	0.101
	Potential other groups in levels			
$Baloch_i \times Post_t$	3.1656	5.0426	1.8002	1.4120
	(0.7836)***	(2.3005)**	(0.3816)***	(0.4380)***
No. potential other groups	0.1862	0.2288	0.1402	0.0805
	(0.1561)	(0.7824)	(0.0640)**	(0.0703)
Adj. Within-R^2	0.236	0.164	0.140	0.102
Controls	Yes	Yes	Yes	Yes
District-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Obs	3144	3144	3144	3144

Notes: The controls include log of population and geographical characteristics interacted with year dummies (see footnote 25). Standard errors clustered at the district level in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-C3 – Controlling for other groups: Counting one-hit wonders

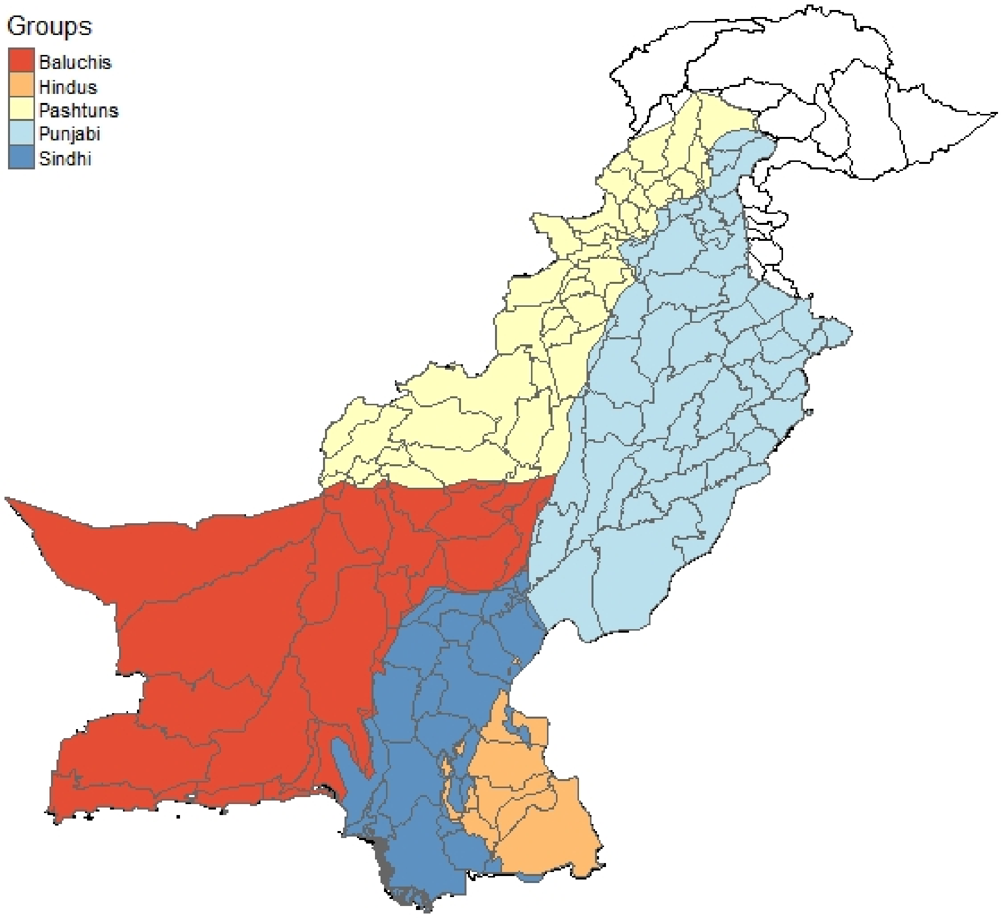
	Incidents (1)	Fatalities (2)	Ln Incidents (3)	Ln Fatalities (4)
$Baloch_i \times Post_t$	3.6128	5.6977	2.1590	1.6355
	(0.9773)***	(2.9259)*	(0.3865)***	(0.4460)***
Δ No. potential other groups	0.9368	3.5604	1.2074	1.0956
	(0.2036)***	(0.5533)***	(0.1375)***	(0.1542)***
Adj. Within. R^2	0.248	0.175	0.235	0.164
	Potential other groups in levels			
$Baloch_i \times Post_t$	2.2923	0.3220	1.2348	0.7108
	(0.7544)***	(2.1246)	(0.2839)***	(0.3166)**
No. potential other groups	2.6158	10.6708	1.7814	1.7923
	(0.2472)***	(1.3799)***	(0.2395)***	(0.2664)***
Adj. Within. R^2	0.374	0.294	0.421	0.324
Controls	Yes	Yes	Yes	Yes
District-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Obs	0.248	0.175	0.235	0.164

Notes: Standard errors clustered at the district level in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

4-12 Ethnopolitical representation

Could our effects be driven by differing ethno-political representations of the people living across Pakistani districts (Bormann et al., 2019)? Given that the conflict in Balochistan is historically a conflict between the tribes of a specific ethnicity (i.e., the Baluchi with the central government), it seems a plausible source for omitted variable bias. We address this issue directly and control for the representation of the ethnic groups within districts over time using the geocoded version of the Ethnic Power Relations (GeoEPR) (Wucherpfennig et al., 2011; Vogt et al., 2015).

Figure 4-D1 – Politically relevant ethnic groups in Pakistan (GeoEPR)



Notes: Reports the intersect between our district shape and the GeoEPR shape.

The EPR tracks the political access of political relevant groups to the central state. It codes the political power of groups with some exceptions on an ordinal scale. The set of group power status are: “Monopoly”, “Dominance, “Senior Partner”, “Junior Partner”, “Powerless”, “Discriminated”, “Self-exclusion” and “Irrelevant” (see Vogt et al., 2015). The first two categories imply that a group has more or less exclusive access to power, while the “partner” categories are assigned to groups that share power in government. The remaining groups are (apart from the Self-exclusion category) self-explanatory. In our sample, we only observe groups being either junior or senior partner (the Punjabi for the entire period), discriminated against (the Baluchi for most of the time), or powerless. Figure 4-D1 reports the ethnic groups across our districts.

Two issues arise when using the GeoEPR in our setting. First, the GeoEPR measures access to political power for ethnic groups and not for districts. Hence, we need to aggregate the representation of the groups living within a district to the district level. Second, the GeoEPR's time coverage only extends to 2017, and has no data for several districts.[49] Thus we do not include the GeoEPR variables in our baseline specifications.

We aggregate group representation to the district level by creating a set of dummies representing each power category in our sample and weight each group's power with its area share in the respective districts. Hence we have 3 variables ranging between 0 and 1 as additional control variables for our specifications. Note that the senior partner category is left out since it is time invariant. We are aware that this procedure is not suitable to estimate the effect of political representation on violence directly or provide meaningful estimates for specific ethnic power relations. Yet, it should be sufficient to proxy for potentially unobserved changes in the political representation across districts over time.

Table 4-D1 – Ethnopolitical representation across districts

	Incidents (1)	Fatalities (2)	Ln Incidents (3)	Ln Fatalities (4)
$Baloch_i \times Post_t$	2.5522	3.9648	1.9981	1.4050
	(0.8023)***	(2.4389)	(0.3788)***	(0.4550)***
Area share discriminated	0.0970	-3.3771	0.2111	0.0634
	(0.4437)	(1.4641)**	(0.3119)	(0.3463)
Area share powerless	-0.4502	-2.5694	-0.5743	-0.3272
	(0.3596)	(0.9233)***	(0.1725)***	(0.2018)
Controls	Yes	Yes	Yes	Yes
District-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Obs	2921	2921	2921	2921
Adj. within-R^2	0.388	0.172	0.151	0.110

Notes: The controls include log of population and geographical characteristics interacted with year dummies (see footnote 25). Standard errors clustered at the district level in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-D1 replicates Table 4-1 adding the EPR-based variables. Note that we use the junior partner status as the omitted category. The main results remain stable. We refrain interpreting our power dummies. Interpretation of the power dummies is not straight forward due to potentially opposing effects. Consider that the share of discriminated increases. This might make the discriminated group more likely to support an armed group within a districts. At the same time, it reduces the share of the junior partner group which is a potential target, in turn suppressing violence within a district. Hence, what we observe is a net correlation.

Given that our treatment group consists of all districts located within the Baloch Province we can also weight the political representation of ethnic groups on the

[49]The white areas in Figure 4-D1.

Province level (see Table 4-D2). Again, our coefficient of interest remains stable throughout all specification.

Table 4-D2 – Ethnopolitical representation across provinces

	Incidents (1)	Fatalities (2)	Ln Incidents (3)	Ln Fatalities (4)
$Baloch_i \times Post_t$	4.1426	3.6403	2.0263	1.5053
	(0.9919)***	(2.0383)*	(0.3789)***	(0.4455)***
Area share discriminated	-0.4056	-3.2752	-0.2037	-0.3093
	(0.6529)	(2.2391)	(0.3770)	(0.4134)
Area share powerless	-0.1994	-2.6305	-0.4414	-0.1108
	(0.3921)	(1.1759)**	(0.1992)**	(0.2175)
Controls	Yes	Yes	Yes	Yes
District-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Obs	2921	2921	2921	2921
Adj. Within-R2	0.243	0.172	0.146	0.109

Notes: The controls include log of population and geographical characteristics interacted with year dummies (see footnote 25). Standard errors clustered at the district level in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

In summary we confident that changing political representation of ethnic groups across districts and provinces does not bias our estimates.

4-13 Matching groups between GTD and GED

No.	GTD Group Name	GED Group Name	Matched Group Name
1		Abdullah Azzam Brigades	Abdullah Azzam Brigades
2		Abu Hafs Katibatul al-Ghurba al-Mujahideen	Abu Hafs Katibatul al-Ghurba al-Mujahideen
3		al-Intiqami al-Pakistani	Al-Intiqami al-Pakistani
4		Al-Intiqami al-Pakistani	Al-Intiqami al-Pakistani
5		Al-Jihad	Al-Jihad
6		Al-Mansoorian	Al-Mansoorian
7		Al-Qa'ida	Al-Qaida
8		Al-Qaida	Al-Qaida
9	Al-Qaida	Al-Qaida	Al-Qaida
10		Al-Qaida in the Indian Subcontinent	Al-Qaida
11		Al-Qa'ida in the Indian Subcontinent	Al-Qaida
12		Amr Bil Maroof Wa Nahi Anil Munkir	Amr Bil Maroof Wa Nahi Anil Munkir
13	Ansaar ul-Islam		Ansaar ul-Islam
14		Ansarul Islam (Pakistan)	Ansaar ul-Islam
15		Ansar Al-Mujahideen	Ansar Al-Mujahideen
16		Ansar al-Sharia	Ansar al-Sharia
17			Ansar Wa Mohajir
18		Ansar Wa Mohajir (Pakistan)	Ansar Wa Mohajir
19		Ahle Sunnat Wal Jamaat (ASWJ-Pakistan)	ASWJ
20		Sipah-e-Sahaba/Pakistan (SSP)	ASWJ
21		Baba Ladla Gang	Baba Ladla Gang
22	Baloch Ittehad		Baloch Ittehad
23		Baloch Militant Defense Army	Baloch Militant Defense Army
24		Baloch Mussalah Diffah Tanzim (BMDT)	Baloch Mussalah Diffah Tanzim (BMDT)
25		Baloch National Liberation Front	Baloch National Liberation Front
26		Baloch Liberation Army (BLA)	BLA
27	BLA		BLA
28		Baloch Liberation Front (BLF)	BLF
29	BLF		BLF
30		Baloch Liberation Tigers (BLT)	BLT
31		Balochistan Liberation United Front (BLUF)	BLUF
32		Baloch Republican Army (BRA)	BRA
33		Baloch Republican Party	BRA
34	BRA		BRA
35		Baloch Republican Guards (BRG)	BRG

Continued on next page

Appendix 4-E1 – continued from previous page

No.	GTD Group Name	GED Group Name	Matched Group Name
36		Baloch Waja Liberation Army (BWLA)	BWLA
37		Baloch Young Tigers (BYT)	BYT
38	Fedayeen Islam		Fedayeen Islam
39	Forces of Momin Afridi		Forces of Momin Afridi
40	Forces of Shah Sahib		Forces of Shah Sahib
41	Forces of Turkestan Bhittani		Forces of Turkestan Bhittani
42		Free Balochistan Army (FBA)	Free Balochistan Army (FBA)
43	Government of Afghanistan		Government of Afghanistan
44	Government of India		Government of India
45	Government of Iran		Government of Iran
46	Government of Iraq		Government of Iraq
47	Government of Pakistan		Government of Pakistan
48	Government of United States of America		Government of United States of America
49		Hafeez Brohi Group	Hafeez Brohi Group
50		Hafiz Gul Bahadur Group	Hafiz Gul Bahadur Group
51		Halqa-e-Mehsud	Halqa-e-Mehsud
52		Haqqani Network	Haqqani Network
53		Harakat ul-Mujahidin (HuM)	Harakat ul-Mujahidin (HuM)
54		Harakat ul-Mujahidin Al-Almi	Harakat ul-Mujahidin Al-Alami
55		Harkatul Jihad-e-Islami	Harkatul Jihad-e-Islami
56	Hizb-i Islami-yi Afghanistan		Hizb-i Islami-yi Afghanistan
57		Islami Jamiat-e-Talaba (IJT)	IJT
58	IMU		IMU
59		Islamic Movement of Uzbekistan (IMU)	IMU
60	IS		IS
61		Khorasan Chapter of the Islamic State	IS
62		Jaish al-Umar (JaU)	Jaish al-Umar (JaU)
63		Jaish as-Saiyouf (Army of Swords)	Jaish as-Saiyouf (Army of Swords)
64		Jaish Usama	Jaish Usama
65		Jaish-e-Islam	Jaish-e-Islam
66	Jaish-ul-Islam		Jaish-ul-Islam
67		Jamaat-E-Islami	Jamaat-E-Islami
68	Jamaat-ul-Ahrar		Jamaat-ul-Ahrar
69		Jamaat-ul-Ahrar	Jamaat-ul-Ahrar
70		Jeay Sindh Qaumi Mahaz (JSQM)	Jeay Sindh Qaumi Mahaz (JSQM)
71		Jaish-e-Khorasan (JeK)	JeK
72		Jaish-e-Mohammad (JeM)	JeM
73	Jondullah		Jondullah
74		Jundallah (Pakistan)	Jondullah

Continued on next page

Appendix 4-E1 – continued from previous page

No.	GTD Group Name	GED Group Name	Matched Group Name
75		Khatm-e-Nabuwat (KeN)	Khatm-e-Nabuwat (KeN)
76		Lashkar-e-Balochistan	Lashkar-e-Balochistan
77	Lashkar-e-Islam		Lashkar-e-Islam
78		Lashkar-e-Islam (Pakistan)	Lashkar-e-Islam
79		Lashkar-e-Jarrar	Lashkar-e-Jarrar
80		Lashkar-e-Omar	Lashkar-e-Omar
81		Lashkar-e-Taiba (LeT)	Lashkar-e-Taiba (LeT)
82		Lashkar-e-Jhangvi	LeJ
83	LeJ		LeJ
84		Majlis-e-Askari	Majlis-e-Askari
85		Majlis-e-Lashkari	Majlis-e-Lashkari
86		Mohajir National Movement	MQM
87	MQM		MQM
88		Muttahida Qami Movement (MQM)	MQM
89	MQM-H		MQM-H
90		Mujahideen Ansar	Mujahideen Ansar
91		Mutahida Majlis-e-Amal	Mutahida Majlis-e-Amal
92		Afghans	NA
93		Baloch Nationalists	NA
94		Bandits	NA
95	Bangesh		NA
96		Bhittani tribe	NA
97		Brelvi Muslims	NA
98	Civilians		NA
99		Gunmen	NA
100		Individual	NA
101	Kachai sub-tribe of Bangesh		NA
102		Kalpar Tribesmen	NA
103	Kashmir insurgents		NA
104	Lashkar of Akakhel tribe		NA
105	Lashkar of Akakhel tribe		NA
106	Lashkar of Kukikhel clan		NA
107	Lashkar of Mohmand tribe		NA
108	Lashkar of Orakzai tribe		NA
109	Lashkar of Salarzai tribe		NA
110	Lashkar of Zakakhel tribe		NA
111	Lashkhar of Wazir tribe		NA
112	Laskhar of Masozai Qaumi tribe		NA
113		Mahsud Tribe	NA
114	Mangal		NA
115		Militants	NA
116		Miscreants	NA
117	Mishti		NA
118	Mohajir		NA
119		Muslim Extremists	NA
120		Muslim extremists	NA
121		Muslim Fundamentalists	NA
122		Muslim Militants	NA

Continued on next page

Appendix 4-E1 – continued from previous page

No.	GTD Group Name	GED Group Name	Matched Group Name
123		New People's Army (NPA)	NA
124		Orakzai Freedom Movement	NA
125		Other	NA
126	Pashtun		NA
127		Separatists	NA
128		Shia Muslim extremists	NA
129		Shiite Muslims	NA
130	Sindhi		NA
131		Sunni Muslim extremists	NA
132		Sunni Muslims	NA
133	Supporters of MQM		NA
134	Supporters of PPP		NA
135	Supporters of Yousaf Ali Khan Magsi		NA
136	Supporters of Zulfikar Ali Khan Magsi		NA
137		Tribal Group	NA
138		Tribesmen	NA
139	Turi		NA
140		Youths	NA
141		Pakistani People's Party (PPP)	Pakistani People's Party (PPP)
142		People's Amn Committee	People's Aman Committee
143		Qari Kamran Group	Qari Kamran Group
144		Sindh Liberation Front	Sindh Liberation Front
145		Sindh Revolutionary Army	Sindh Revolutionary Army
146		Sindhu Desh Liberation Army (SDLA)	Sindhu Desh Liberation Army (SDLA)
147		Sindhudesh Revolutionary Army (SRA)	Sindhudesh Revolutionary Army (SRA)
148		Sipah-I-Mohammed	Sipah-I-Mohammed
149		Punjabi Taliban	Taliban
150	Taleban		Taliban
151		Taliban	Taliban
152		Tehrik-i-Taliban Pakistan (TTP)	Taliban
153	TTP		Taliban
154		Tanzeem al-Islami al-Furqan	Tanzeem al-Islami al-Furqan
155	Tawheed ul-Islam		Tawheed ul-Islam
156	Tawheed ul-Islam		Tawheed ul-Islam
157		Tawheedul Islam	Tawheed ul-Islam
158		Tehrik-e-Khilafat	Tehrik-e-Khilafat
159		Tehrik-e-Taliban Islami (TTI)	Tehrik-e-Taliban Islami (TTI)
160		Tehrik-e-Tuhafaz	Tehrik-e-Tuhafaz
161		Tehrik-e-Tuhafaz (Pakistan)	Tehrik-e-Tuhafaz

Continued on next page

Appendix 4-E1 – continued from previous page

No.	GTD Group Name	GED Group Name	Matched Group Name
162		Tehrik-e-Nifaz-e-Aman Balochistan-Jhalawan Brigade (TNAB-Jhalawan Brigade)	TNAB-Jhalawan Brigade
163		Tehreek-e-Nafaz-e-Shariat-e-Mohammadi (TNSM)	TNSM
164		Tehrik-e-Nafaz-e-Shariat-e-Mohammadi (TNSM)	TNSM
165	TTP-Islahi		TTP-Islahi
166	TTP-KM		TTP-KM
167	TTP - MR		TTP-MR
168	TTP - MT		TTP-MT
169	TTP-SM		TTP-SM
170	TTP - TA		TTP-TA
171	UBA		UBA
172		United Baloch Army (UBA)	UBA
173		Uzair Baloch Gang	Uzair Baloch Gang
174		Zehri Youth Force (ZYF)	Zehri Youth Force (ZYF)

4-14 Splits & mergers of armed groups within Pakistan

Table 4-F1 – Observed groups splits and reasons to split

Child group	Split year	Mother group	Reason to split	Details	Source
United Baloch Army (UBA)	2014	Baloch Liberation Army (BLA)	Natural death of leader	The UBA split from the BLA after the father of both group leaders died. The group leaders are brothers and could not agree who to lead the BLA.	Stanford-Mapping Militants
Jaish-e-Mohammad (JeM)	2000	Harakat ul-Mujahidin (HuM)	Lost funding by ISI	Some sources claim that ISI [Pakistan's Inter-Services Intelligence] lost interest in funding HuM after Khalil's [founder of HuM] 1998 decision to join hands with Bin Laden. ISI may have offered Azhar assistance and funding to establish JeM following his release from prison.	Stanford-Mapping Militants
Harakat ul-Mujahidin Al-Almi	2002	Harakat ul-Mujahidin (HuM)	Dispute over organizational affairs.	There was reportedly some pressure on the HuM after its proscription in Pakistan in 2001 to merge with the Jamiat-ul-Mujahideen. This plan met with stiff resistance from within the HuM and reportedly, the dissent led to a group breaking away from the parent outfit and calling itself the Harkat-ul-Mujahideen Al-alami.	SATP
Jundullah	2011	Lashkar-e-Jhangvi (LeJ)	No reason found		TRAC
Lashkar-e-Balochistan	2011	Lashkar-e-Jhangvi (LeJ)	No reason found		TRAC
Lashkar-e-Jhangvi (LeJ)	1996	Sipah-e-Sahaba/Pakistan (SSP)	Ideology conflicts	Former SiS [Sipah-i-Sahaba] militants Riaz Basra, Malik Ishaq, and Akram Lahori founded LeJ in 1996 after breaking away from SiS, claiming that SiS had deviated from its founder's teachings.	Stanford-Mapping Militants
Jamaat-ul-Ahrar	2014	Tehrik-i-Taliban Pakistan (TTP)	Leadership dispute after the killing of former leader.	JA split from the TTP under the leadership of former TTP commander Omar Khalid Khorasani. This separation was a result of growing tensions between Khorasani and the then leader of the TTP, Maulana Fazlullah. The broadness of the TTP's coalition also presents challenges, however, and has threatened the group's cohesion. For instance, after the death of its former amir Hakimullah Mehsud in a U.S. drone strike in November 2013, the jihadist leaders of several key TTP factions failed to reach a consensus over who should head the group.	Stanford-Mapping Militants, UNHCR-Refworld
Jundallah (Pakistan)	2014	Tehrik-i-Taliban Pakistan (TTP)	"Self-reinvigoration through ISIS" and anti-Shi'a ideals.	When ISIS captured Mosul in July 2014, Jundullah was one of the first organizations that pledged allegiance to Abu Bakr al-Baghdadi. Due to Jundullah's strong ties to al-Qaeda, their decision to shift alliances was probably a difficult one. However, because the group lost most of its core leadership due to severe actions taken by Pakistani law enforcement, this move indicates a policy of self-reinvigoration through ISIS. Jundallah is likely to be partly comprised of cadres from banned sectarian Deobandi tafkiri groups like LeJ or Ahle-Sunnat-Wal-Jamat (ASWJ), which consider Shi'a Muslims to be kafirs[meaning: infidel or disbelievers], underlining that the group already had strong sectarian leanings even before the advent of the Islamic State.	Washington Institute, UNHCR-Refworld

Table 4-F2 – Observed groups mergers and reasons to merge

Lead group	**Merge year**	**Merging group**	**Reasons to merge**	**Details**	**Source**
Tehrik-i-Taliban Pakistan (TTP)	2011	Harakat ul-Mujahidin (HuM)	Common aims and enemies and concentration of power	Media reports on January 5, 2011, indicated that five terrorist groups had joined the TTP and were working under its umbrella TTP. With common aims and enemies, LeJ, SSP, JeM, HuM and Harkat-ul-Ansar (HuA) had 'merged' with TTP. TTP spokesman Azam Tariq declared, 'We have not forced anyone to join TTP, and the leaders and activists of the banned religious organisations have united themselves under the umbrella of the TTP on their own choice.' The sole objective of the Shura meeting was to unite the small militant fractions under the leadership of TTP against NATO forces in Afghanistan and to wage a defensive jihad against Pakistani forces.	SATP
Tehrik-i-Taliban Pakistan (TTP)	2011	Jaish-e-Muhammad (JeM)			
Tehrik-i-Taliban Pakistan (TTP)	2011	Lashkar-e-Jhangvi			
Tehrik-i-Taliban Pakistan (TTP)	2011	Sipah-e-Sahaba/Pakistan (SSP)			
Tehrik-i-Taliban Pakistan (TTP)	2015	Jamaat-ul-Ahrar (JuA)	Reconcilition through government operations and leadership dispute resolution	The government's commencement of the Zarb-e-Azb operation in North Waziristan district, and supplementary operations in other districts of tribal areas, served to soften the TTP's differences over the leadership and to bind these groups together against a common enemy. In addition, Fazlullah carrying out the December 16, 2014 attack on the Army Public School in Peshawar, in which 141 people (a large number of them children) were killed, outclassed all other competing jihadist groups, and Fazlullah thereby proved his mettle to rule TTP.	UNHCR-Refworld
Tehrik-i-Taliban Pakistan (TTP)	2015	Lashkar-e-Islam (LeI)	Re-organisation as a result of significant gains by security forces	Militant organisation Lashkar-i-Islam (LI) has merged with the TTP as part of a re-organisation plan. The decision to unify the militant groups was taken at a meeting attended by TTP leaders Mulla Fazlullah, Omar Khalid Khurasani and LI leader Mangal Bagh. The militants announced the unification at a time when security forces are making significant gains against them in military operations in North Waziristan and Khyber Agency (government's Operation Khyber I), which were once considered their bastions.	

Chapter 5

Development Aid and Conflict

*China and the World Bank - How Contrasting Development Approaches affect the Stability of African States**

5-1 Introduction

As part of what the Economist describes as the “The new scramble for Africa,” emerging donors, in particular, China (Dreher and Fuchs, 2015; Dreher et al., 2018), are challenging the predominance of traditional donors in affecting African development. The big question is whether this time, as the magazine asks, African countries will be the benefactors of foreign engagement? Africa is a central focus of traditional donors as well as a key target region for China. While there was a considerable drop in global poverty rates thanks to rapid growth, mostly in Asian countries, many African states lag behind. In particular conflict-prone states plagued by re-igniting battles pose a challenge, which is why those are also labeled as the ”new frontier of development.”[1]

While the literature on the growth effects of aid converges towards an on average small, positive effect (Clemens et al., 2011; Dreher and Langlotz, 2019; Galiani et al., 2017; Kilby, 2015), researchers are divided about the impact of aid on stability and conflict (e.g., Bluhm et al., 2021; Child, 2018; Crost et al., 2014; Nunn and Qian, 2014). Some perceive Chinese aid as a crucial step forward that brings growth and stability to Africa, while others regard it as a big risk that narrowly focuses on Chinese self-interest, enriches elites (Dreher et al., 2019), fosters conflict, and exports repression, along with surveillance tactics and autocratic norms (Kishi and Raleigh, 2016).[2] We are shedding light on this crucial question by systematically contrasting

*This chapter is based on joint work with Kai Gehring and Lennart Kaplan (Gehring et al., 2019).

[1]See Economist (2019) and Economist (2017).

[2]See Washington Post (2015) and Council on Foreign Relations (2018) for the direct citation. See Freedom House (2018) on East African states adapting Chinese internet censorship policies, New York Times (2019) on exporting the surveillance state, and US News (2018) about China’s web surveillance model, and Council on Foreign Relations (2018) about Zimbabwe using Chinese large-scale facial recognition software in its capital Harare. See Economist (2018) about China training foreign officials and bureaucrats, and promoting ”their political model” as an alternative to democracy.

the Chinese approach to development with that of the World Bank (WB), one of the most important traditional donors, and analyzing their effect on stability in recipient regions.

Moving beyond the partly subjective public rhetoric, we argue that Chinese foreign aid needs to be considered with all its nuances. China's "no strings attached" approach to development differs sharply from the expert-driven, conditional approach of traditional donors like the WB and many other Western DAC donors. Both donors are interested in growth, but while the World Bank regards democracy, transparency, and human rights as a critical part of prosperity, the Chinese model highlights social and political stability as the key ingredient to development. To comprehensively understand the impact of these approaches, requires a holistic definition of stability. This chapter defines stability as a broad continuum ranging from outright conflicts with at least a certain number of battle-related deaths, to lower level-conflict events like citizen protests and government repression, as well as attitudes related to stability.

Although the more economic growth and stability-oriented perspective of China and the rule- and expert-based democratic perspective of the World Bank might be seen as two ends of the spectrum of development policies, their impact on stability is complex. Even if its motive would be mere self-interest, China also has an incentive to protect its investments as well as its workers in Africa. One should not expect China to turn a blind eye on recipient governments starting outright conflicts. Both donors will try to stop recipient governments from engaging in conflicts that they deem avoidable or unnecessary, and given their size have some leeway over recipient governments.[3] At the same time, when regarding stability more broadly than just focusing on the outright conflicts, China is likely to build on its own domestic development experience, which combines growth with an autocratic and stability-oriented rule. Therefore, there are good reasons to believe that China would be more willing to accept recipient governments' use of autocratic policies and non-lethal repression to enhance stability, while the WB emphasizes democracy and humanitarian values more strongly. This chapter does not take a normative stance which approach is ultimately superior from the perspective of a developing country. But it carefully carves out the most important conceptual differences between the two donors and their potential effect on state stability.

To then investigate the causal effects of the two donors on stability, we precisely link new detailed geo-referenced datasets on development projects by China (Strange, Dreher, Fuchs, Parks, and Tierney, 2017) and the WB (Dreher, Fuchs, Parks, Strange, and Tierney, 2017) with geo-referenced measures of stability at the sub-national level in Africa. Our dataset allows us to match the location of aid projects and conflicts more precisely than earlier studies, and enables us to flexibly eliminate potential biases arising from, for instance, unobserved conflict trends, region-specific time-invariant factors, and country-level time-varying factors. Moreover, our Bartik-type identification strategy adapts an established instrumental variable (IV) approach by Nunn and Qian (2014). Our instrument is the interaction

[3]Based on the different approaches, the distribution of aid also differs. Chinese aid goes more often directly to governments and the home regions of ethnic leaders (Dreher et al., 2019). Its distribution is associated with more corruption (Isaksson and Kotsadam, 2018a) and weaker labor unions (Isaksson and Kotsadam, 2018b). Still, its less bureaucratic approach was also found to lead to more evenly distributed economic activity within countries (Bluhm, Dreher, Fuchs, Parks, Strange, and Tierney, 2020).

between exogenous temporal variation in the WB's IDA liquidity (Dreher et al., 2017) and in Chinese domestic (over-)production of commodities with the pre-determined probability of a region to receive aid with (Dreher et al., 2017; Bluhm et al., 2020).

Our results provide several important insights. First, a wide range of fixed effects (FE) and IV specifications reject the idea that aid by either donor, on average, fuels conflict at the sub-national level. In the FE specification, a one standard deviation change in WB aid associates with about 1.6 percentage points lower conflict likelihood. The effect remains negative and of similar magnitude, but becomes statistically insignificant when using IV. When studying China, both strategies yield negative and small, insignificant coefficients.

We move beyond this main IV effect by considering the actors involved in and responsible for a conflict. Both WB and Chinese engagement consistently leads to a reduction in lethal violence by governments against civilians. For both donors, we also find no positive effect on protest events like demonstrations, riots, and strikes. At the same time, there are crucial differences concerning how stability is secured. Among the two, only Chinese aid is associated with more government repression in recipient regions. Afrobarometer responses suggest that both donors have different effects on measures of security, democratic norms and attitudes, as well as on perceptions of government behavior. While WB aid is linked to higher perceived security and stronger support for democratic values, Chinese aid tends to result in a stronger emphasis on rule following behavior and a higher acceptance of autocratic regimes.

This chapter contributes in several ways to better understand the role of donors in influencing recipient country stability, as well as the channels and mechanisms linking aid to various types of conflict. We combine the strengths of existing approaches on the country level (e.g., Bluhm, Gassebner, Langlotz, and Schaudt, 2021; Nielsen, Findley, Davis, Candland, and Nielson, 2011; Nunn and Qian, 2014), with the advantages of studies focusing on sub-national aid data in specific sectors in selected countries (e.g., Berman, Shapiro, and Felter, 2011; Child, 2018; Crost, Felter, and Johnston, 2016; Sexton, 2016; Van Weezel, 2015). The aim is to deliver the best possible compromise between using micro-data with causal identification strategies and estimating externally valid results for more than one country. Truly randomly allocated aid projects in individual countries possess a higher internal validity. Still, their findings could be driven by the particular country context or the specific type of aid, and it is impossible to replicate them at large scale for a full continent. We apply identification strategies that are well established in the literature, consider a broad set of all aid-eligible countries, and our results can be meaningfully interpreted beyond the context of an individual country.

Besides using new data and providing more precise estimates about the causal effect of aid on more comprehensive measures of stability, we want to emphasize three main contributions. First, the chapter adds to the scarce evidence on the incentives for different actors and their choices created by development projects. Crost et al. (2014), for instance, focus on how aid changes the incentives for rebel groups.[4] Our finding of a significant reduction in lethal violence enacted by recipient

[4]As a robustness test, we also show results on sectoral differences, which augment previous results on intersectoral differences within specific countries (e.g., Child, 2018; Crost et al., 2016; Berman et al., 2011).

governments against civilians supports the idea of the “cost of shame” (Lebovic and Voeten, 2009). The fear of losing aid money changes the incentives of recipient notably.

Second, we shed some light on the hopes and fears associated with emerging donors (Asmus, Fuchs, and Müller, 2017; Fuchs and Vadlamannati, 2013). In particular, China’s increased global engagement, like the Belt and Road initiative and the intense China-Africa Cooperation, is one of the crucial geopolitical changes in the last two decades. These changes will continue to create tensions in the future. Existing chapters have either focused on outright conflict, or the impact of, for instance, Chinese aid in Latin America on attitudes towards China (Brückner, Eichenauer, and Fuchs, 2018). Still, convincing causal evidence that provides a comprehensive picture of the impact of Chinese aid on stability in a broad sense was missing.

Third, by contrasting Chinese aid with the World Bank as a prototypical example of a traditional, multilateral donor that involves development experts and accounts for democracy and humanitarian values, we provide a useful reference point. Western newspapers and NGOs have widely complained about the active export of Chinese surveillance technology and policies, as well as about the potentially detrimental impact that the apparent success of the Chinese approach to development has on developing countries. Our results paint a more nuanced picture. China’s engagement is not associated with an increase in outright conflict, and even with more stability when considering less lethal conflicts by governments against civilians. Still, it comes along with increased government repression and a higher prevalence of autocratic norms. Hence, the approach makes a difference in some critical dimensions. How to assess these differences depends on the normative perspective of the observer.

The chapter proceeds as follows. Section 5-2 summarizes the existing literature and how the two approaches are linked to different measures of stability. Section 5-3 explains the data and the corresponding sources and provides descriptive statistics. Section 5-4 presents the specification and empirical strategy. Section 5-5 shows and discusses the results, and Section 5-6 concludes.

5-2 Theoretical considerations and related literature

This section first defines our concept of stability, and then contrasts the policies by China and the WB, and how they may affect outright conflict as well as individual dimensions of stability. To understand the impact of the two approaches to development in a comprehensive way requires a holistic definition of stability. More specifically, we think of outright conflict as a lethal fight that caused at least a specific number of battle-related deaths. Besides the average effects on conflict, we distinguish between the actors involved in a conflict, either government-related or non-state actors like rebels. Each of those can be engaged in a two-sided conflict with the respective other group, or start a one-sided conflict against civilians. Besides these lethal conflict events, lower-level conflicts like citizens protests against governments, and government policies against potential rebels or other minority groups in the country also characterize stability. Finally, attitudes are both reflecting the results of these other events, but are also themselves signs of stability, for instance, beliefs about the quality of democratic processes or rule-following behavior.

Outright conflict: Aid can lower conflict if it raises income and hence, the opportunity costs of fighting. In that regard, the aid effectiveness literature converges towards either a null effect (Doucouliagos and Paldam, 2009), or small positive effects (Galiani et al., 2017) of aid on growth. Berman, Felter, Shapiro, and Troland (2013) hypothesize that projects are more successful in reducing violence if they require the integration of development experts. Minasyan, Nunnenkamp, and Richert (2017) demonstrate the importance of donor quality. The WB built up vast expertise over the decades since its foundation, which may increase the effectiveness of its projects in raising income.

At the same time, traditional donors have also been criticized for lack of "ownership" and underutilizing local knowledge in recipient countries. Scholars writing about emerging donors like India (Fuchs and Vadlamannati, 2013) and China (Humphrey and Michaelowa, 2018) also highlight less complicated, bureaucratic processes with quicker implementation times. Hence, China's flexibility and emphasis on economic "mutual benefits" may boost growth even more than the WB approach (Dreher et al., 2017). Thus, in these dimensions both donors could reduce the incentives to engage in conflict by fostering growth in their own ways.[5]

Besides growth effects, the distribution of potential gains is vital as the literature on resource-related income shocks highlights (e.g., Berman et al., 2017b; Dube and Vargas, 2013; Gehring et al., 2018). Whether potential gains from aid are used for short-term consumption, invested in fostering development, or end up in the foreign bank accounts of government officials, affects the impact on conflict. If the projects contribute to rising inequality, this could trigger conflicts. WB projects were found to be less politically motivated than other types of aid (e.g., Dreher, Sturm, and Vreeland, 2009). The Bank aims for aid allocation in line with conflict prevention policies accounting for humanitarian aspects and security. In contrast, Dreher et al. (2019) find that Chinese projects in Africa are more likely to benefit the birth regions of the respective leader, corresponding to the literature on how leader changes affect the bilateral allocation of aid (Rommel and Schaudt, 2020; Faye and Niehaus, 2012).[6] Isaksson and Kotsadam (2018a) suggest that Chinese engagement is associated with higher local corruption, which could increase inequality and lower trade union membership (Isaksson and Kotsadam, 2018b). Such effect could decrease the labor share of profits. However, Chinese infrastructure projects are particularly found to lead to an equal distribution of economic activity (Bluhm et al., 2020). Hence, the theoretical predictions are, to some degree contradictory, leaving this an empirical question.

[5]The literature also describes "aid as a price" that can be acquired as a result of winning a fight or conflict. This "aid as a price" theory has both a direct goods-related and a political dimension. Regarding goods, Nunn and Qian (2014) show that US food aid leads to more conflict, as it can be looted. Expensive equipment associated with investments in healthcare and communication infrastructure can also be sold on black markets. To remedy these issues, some traditional donors like the WB seek to "conflict-proof" their aid by avoiding projects that provide lootable/fungible resources over which warring parties may fight. They instead provide aid in a more discrete manner, such as social programs (Berman et al., 2013; Crost et al., 2014; Imai et al., 2018). We investigate aid in different sectors separately in a robustness test.

[6]There is evidence that, patronage networks are also an influential domestic phenomenon in China (Jiang and Zhang, 2020).

Finally, traditional Western donors often impose conditions and require specific processes in aid-receiving countries. The WB often uses conditions regarding governance, equality, anti-discrimination, among others. The Bank is also considered to be a global leader in “conflict-sensitive programming” (Van der Windt and Humphreys, 2016b; World Bank, 2011), based on the idea that conflict-management can mitigate conflict (Gonzalez and Neary, 2008). This involves the identification of conflict escalators using a detailed Conflict Analysis Framework (CAF) (Wam, 2006) to help WB staff to understand country-specific sources of conflicts. The WB’s Operational Procedures instruct WB staff on how to act within a conflict-affected country (World Bank, 2001; Bannon, 2010).

Officially, Chinese aid has fewer strings attached.[7] Still, even for skeptical observers who assume China is largely interested in securing resources and providing employment for Chinese workers, it is implausible that China would welcome recipient governments engaging in unprovoked, avoidable conflicts. This would endanger existing investments and the health not only of African but also a large number of Chinese workers in Africa (officially 227,407 by 2016). Moreover, stability is a crucial part of the Chinese development model; in a speech at the 2008 central party congress Hu Jintao mentioned the word stability alone 21 times (freedom did not appear a single time).[8]

To sum up, while politicians, newspapers and some scholars raise concerns about specific aspects of Chinese aid that could give rise to more conflict than the rule-driven approach of the WB, we argue that the net impact on outright conflict is less straightforward. There are reasons to expect WB aid could be more successful in lowering the average risk of conflict. But based on its self-interest and emphasis on stability, China has incentives to “unofficially” set conditions to avoid instability as well.[9]

Actors and types of conflict: Two-sided conflicts between governments and rebels could be affected differently by aid than one-sided conflicts against civilians. Generally, neither donor should be interested in outright conflict. They can threaten to withhold future aid payments to prevent recipients from engaging in conflicts they deem harmful and unnecessary. Lebovic and Voeten (2009) label this the “cost of shame.” We argue that this threat is less likely to matter for two-sided conflicts as it is much easier for both sides to justify their actions as a necessary reaction to the other side. One-sided conflict actions against civilians, in contrast, are harder to justify. If governments use excessive violence against citizens, public pressure in donor countries can stop in particular international organizations like the WB from aid payments (Tir and Karreth, 2018). At the same time, China, as an autocratic one-party state where decision-making is less constrained could be able to threaten to cut aid payments more credibly. Generally, both donors have the incentives and means to exert pressure on recipient governments to avoid unprovoked one-sided conflicts, while their direct influence on rebel groups is limited.[10]

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[7]Anthony Germain on CBC, “China in Africa: No strings attached,” last accessed 31.01.2019.

[8]Anthony Germain on BJ Review, last accessed 31.06.2019.

[9]The Guardian also postulates that “Chinese aid to Africa is going to come with all sorts of strings attached, despite the ”no-conditionality”” rhetoric (The Guardian: “The west has no right to criticise the China-Africa relationship” last accessed 31.01.2019.)

[10]Donors may also encourage rebels to fight an opposed regime as in the case of covert aid to Angolan UNITA under president Reagan (Lagon, 1992). Our data cover almost exclusively projects

Lower level conflict - Government policies and protests: Besides outright, lethal conflict, we are also interested in lower-level types of conflict that are not necessarily directly causing a significant number of casualties. The lower-level conflict has two dimensions. Protest events like strikes, riots or demonstrations can be understood as bottom-up actions by citizens against governments (see, e.g., Sangnier and Zylberberg, 2017). In contrast, government policies like repression are top-down measures by governments to avoid conflicts and such protests. The latter affects the costs of the former, and empirically, we are only able to observe the equilibrium outcome of both dimensions.

WB and Chinese aid can affect the reasons as well as the costs of protest by fostering state capacity. On the one hand side, infrastructure projects like highways, bridges, railroads, and ports strengthen the capacity of the state by extending the spatial reach of its monopoly. Agents of the state – e.g., police officers, judges, and tax collectors – can use their increased capacity in different ways. If they wield it to enforce the rule of law impartially, levy taxes, and deliver public services, improvements in capacity and legitimacy may result in a “virtuous circle” of better state capability (Levi, Sacks, and Tyler, 2009), conflict reduction (Berman et al., 2011) and less reasons to protest. On the other hand, if state agents exploit their increased capacity to enrich themselves, favor some groups over others, or weaken political opponents (Wig and Tollefsen, 2016), this can trigger protests.[11]

The WB uses an independent “Inspection Panel” to investigate complaints about human rights abuses or local conflict provoked by the WB (Zvogbo and Graham, 2018). It pursues an approach to actively build trust and social cohesion in post-conflict and conflict-affected countries (Bannon, 2010). This approach includes, for example, projects with a focus on community-driven development, and capacity building with regards to accountability and public service delivery. The Kecamatan Development program in Indonesia, for instance, attempted to reduce protests via transparency through a particularly participatory approach (Gibson and Woolcock, 2005; Barron, Diprose, and Woolcock, 2011).[12] To the best of our knowledge, China does not have an analogous set of policies, institutions, or operational tools in place to encourage conflict-sensitive development programming.[13] Hence, all else equal, Chinese projects could be related to more protests.

However, the equilibrium impact of both donors is more complex. Citizens deciding whether to engage in protests also weigh the costs against the benefits of these actions. WB policies that foster democratic participation and transparency may be linked, all else equal, to a higher likelihood to protest. Better informed citizens may be more willing to politically engage in more democratic states where the political costs of opposing and the fears of its consequences are lower.

At the same time, Chinese aid could increase the costs of organizing protests, as it decreases trade union membership (Isaksson and Kotsadam, 2018b). Moreover,

implemented in accordance with the government, so this aspect should be of lesser importance.

[11]For instance, insurgents may sabotage projects if they would not benefit sufficiently and government success weakens their support in the population (Crost et al., 2014).

[12]This community-driven development approach inspired the National Solidarity Program - a large scale development program, which was evaluated to increase governmental support in conflict-ridden Afghanistan (Beath, Christia, Egorov, and Enikolopov, 2016).

[13]China only established its first specialized aid Agency CIDCA with a centralized evaluation mandate in 2018. Heiner Janus on DIE, “Next Steps for China’s New Development Agency,” last accessed 22.02.2019.

the enhancement of state capacity also affects the ability to handle protests. China emphasizes social stability as part of its growth model domestically, including the use of force to constrain opposition forces or protesters. Such repression can incite anger and unrest, but also enhance stability via a deterrence effect. An article, for instance, describes how "Chinese officials use advances in facial recognition technology and big data to identify potential troublemakers and reduce the risk of large-scale public demonstrations."[14]

The country is also accused of financially supporting repressive governments in Africa and exporting such repression to recipient countries (Kishi and Raleigh, 2016). For example, Uganda could turn to China after Western donors protested against strict "anti-gay" laws in the country.[15] Several reports describe how China exports its approaches regarding surveillance and censorship. One describes how China "propagate its model abroad by conducting large-scale training of foreign officials" of 36 mostly developing countries.[16] Many of those like Angola, Ethiopia, The Gambia, Kenya, Libya, Morocco, Nigeria, Rwanda, South Africa, Sudan, Zambia, Zimbabwe are in Africa. Another article describes how Uganda and Tanzania introduced cybersecurity laws that are similar to Chinese law after attending training sessions.[17] Freedom House emphasizes how Chinese support helps governments in Sub-Saharan Africa to censor the internet and social media.[18]

China's projects may thus provide more reasons to protest, but repressive policies raise the cost of protests. WB policies may provoke fewer protests due to the implemented safeguards, but stronger democratic standards and less fear of expressing opinions in public make protests more likely. Hence, we expect an increase in repressive government policies related to Chinese aid, but the equilibrium impact of both donors regarding protests remains an empirical question.

Attitudes: The Chinese government regards stability as central for development, and portrays itself as a "rock of stability."[19] However, it does not regard democracy, democratic participation, or equal democratic rights as necessary to achieve stability, or sometimes even sees them as an obstacle to that. Dagong, a Chinese rating agency, writes that "centralized political power enabled [East-Asian countries] to concentrate on solving the most urgent issues in the economic reform step by step," while "countries copying the western system encountered many political obstacles in maintaining stability."[20] A Chinese scholar describes the common perception that developing countries experiencing "chaotic" democratization "inevitably plunge into a chaotic situation marked by soaring prices, shortage of essential supplies, frequent violent conflicts and a precarious state of life and property." This also entails that "the ability to establish and maintain an effective internal order [...] is

[14]See Nikkei.com, last accessed 31.04.2019.

[15]Washington Post, "When China gives aid to African governments, they become more violent," last accessed 31.01.2019.

[16]See US News, last accessed 31.04.2019.

[17]See Nextgov.com, last accessed 31.04.2019.

[18]See Freedom House, accessed 31.04.2019. In addition to training, China reportedly exports surveillance technology like cameras but also advanced artificial intelligence technology. For instance, China signed an agreement with Zimbabwe, Angola, and Ethiopia to deploy a new facial recognition software to monitor its population.

[19]The Economist, last accessed 31.01.2019.

[20]QZ.com, last accessed 31.01.2019.

the most important of all national capacities" , with higher priority than "democratic accountability in a country's political development process."[21]

China is keen on spreading its development model and emphasizing its advantages. In exchange for financial support, Chinese development projects sometimes require that partners broadcast Chinese radio or TV to win "African hearts and minds."[22] For instance, a radio station set up in Kenya reserves a specified amount of hours to promote Chinese culture and values, China supplies text books for schools in Liberia, Ghana and Tanzania, and organizes cultural events in South Africa. Cultural centers aim at spreading Chinese culture and values. Note that this is not good or bad per se; Western donors and the WB are engaging in the same efforts to spread the values and norms they want to propagate. The motivation to do so may be mere self-interest or the honest conviction that the respective development model is the best to raise developing countries out of poverty. Empirically, we are interested whether WB projects are related to more positive perceptions of democracy and governance, and if citizens in regions receiving Chinese aid are more likely to accept autocratic, strong states and strict rules to achieve prosperity.

5-3 Data

5-3.1 Aid Data: World Bank and China

We consider all African countries with more than one million inhabitants on the OECD's DAC recipient list in 1995, the initial year of our sample period. We focus on disbursements by IDA, the WB's arm for development aid. For China, we use the media-based data set on Chinese ODA-like commitments from Dreher et al. (2019), geo-referenced by Strange et al. (2017).[23] All financial flows are thus considered that qualify as aid by having a significant concessionary component.[24]

Our unit of observation is the country-region-year, with regions as the unit of analysis referring to the first level sub-national administrative division – ADM1: "provinces," "states," or "regions" (data from Hijmans et al., 2010). This level is the most suitable choice, as it allows us to distinguish between considerable sub-national variation, while still capturing over 90% of the overall spending by China and the WB (see Figure 5-1).[25] Moreover, this administrative level is also highly relevant for aid allocation. Many projects are assigned to specific regions, and the regional governments can influence how, or where, to spend the funds.

Our approach to assigning aid projects to regions is the following. Precisely geo-referenced projects, as well as projects where we possess information about the first

[21] CGTN, last accessed 31.01.2019.

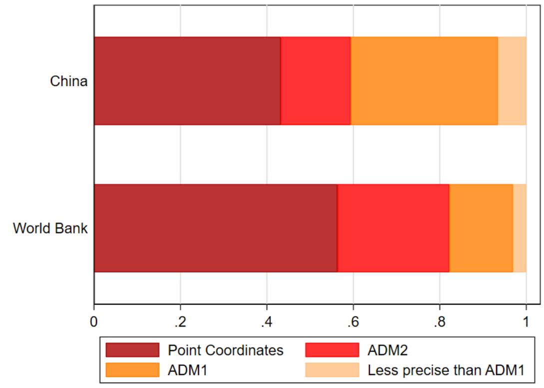
[22] See LA Times, last accessed 30.07.2019.

[23] The data was compiled using the TUFF methodology, which covers a broad set of quality and triangulation steps. Due to the reliance on media, politically controversial projects may be under-reported in regimes with low press freedom (Kilby, 2017). This may induce a downward bias when using conflict as the outcome and those projects would be more likely to lead to conflict.

[24] Other official finance (OOF) flows in China's finance portfolio has less of a development focus. The WB's International Bank for Reconstruction and Development (IBRD) also provides development finance in the form of loans with interest rates closer to market rates.

[25] Lower level administrative regions (ADM2) would only capture between 60 and 80%. Using smaller grid cells would require solely relying on projects with exact data on latitude and longitude, which is only about 50% for the WB and less than 50% for China.

Figure 5-1 – Disbursement/Commitment Amounts by Precision Codes



and second-order subnational level, are assigned to the respective ADM1 region. To cope with the fact that most projects have several project locations, we assume that aid is distributed equally across locations, following Dreher and Lohmann (2015). This means that for a project implemented in 10 locations, with four locations in region A and six in region B, 40% of the project volume would be assigned to A and 60% to B. This procedure ignores projects with lower precision, mostly direct support for governments, but their average effect would be captured by country-year fixed effects. The data appendix provides more details.[26]

Table 5-1 compares aid projects by the two donors that we can assign to the ADM1 level. WB disbursements sum up to USD 29.4 billion, distributed over 1,472 projects in 25,041 locations in Africa. Since graduating from IDA eligibility in 1999 (Galiani et al., 2017), China's overseas portfolio of grants, loans, and export credits has also rapidly expanded as part of its 'Going Out' strategy. In Africa, Chinese aid amounts to USD 13.2 bn, from 333 projects in 1,308 locations. Hence, the WB finances a larger number of projects than China, and each project tends to have more project locations. China finances fewer projects, but spends almost twice as much per project, and nearly ten times as much per project location. Even though aid characteristics differ by donor, both have a comparable propensity in disbursing and committing aid to regions with ongoing conflict events. It is also not the case that Chinese aid is committed largely to autocratic states (based on Polity IV). Finally, the set of recipient countries is similar, since about 76% of the recipient countries receive aid from both donors

[26] 23% of Chinese projects focus on one location, while 95% of WB projects have more than one location.We distribute aid according the following way: $Aid_{pijt} = \frac{Aid_{pit}}{\int Locations_{pi}} * \int Locations_{pj}$, where p is the project, i is the country, j is the region, and t is the period for which we estimate the allocation shares. For robustness, Tables A 5-B45 and 5-B46 display the main results using population weights. For instance, if a project has project locations in two regions of a country, two million inhabitants reside in region A, and three million reside in region B, 40% of project funds are allocated to region A and 60% to region B. Here, the aid attribution formula is $Aid_{pijt} = \frac{Aid_{pit}}{\int Population_{pi}} * Population_{pj}$. Population data are from the gridded population data provided by the Center for International Earth Science Information Network (CIESIN) Columbia University (2016). As a robustness test, we show results using the ADM2 regions and assign project locations with less precise location information than ADM1 to the capital region.

Table 5-1 – Donor Comparison: WB vs. China

	WB Aid	Chinese Aid
Total Disbursements/Commitments (USD):	29.4bn	13.2bn
Active in No. of Countries:	35	41
Number of Projects:	1,472	333
Number of Locations:	25,041	1,308
Mean Number of Locations per Project	17	4
Mean per Project (USD):	19.97m	39.63m
Mean per Location (USD):	1.17m	10.09m
Average number of locations per project:	41	9
Conflict occurrence in recipient region:	9.87%	9.09%
Share of aid to democracies:	19.8%	38.2%
Share of countries that receive aid from both donors:	76%	

Notes: Aid is measured in constant 2011 USD.

5-3.2 Stability Measures

To measure outright conflict, we follow the literature and create a binary *conflict incidence* measure based on the number of battle-related deaths (BRD). The data is taken from the Uppsala Conflict Data Program's (UCDP) geo-referenced Event Dataset (GED) (Croicu and Sundberg, 2015). GED provides a reliable and comprehensive source of geo-referenced conflict events based on media and NGO reports, as well as secondary sources like field reports and books. The database also includes information about the type of conflict and the groups that were involved.[27] Table 5-2 shows descriptive statistics for all stability measures, with the incidence measures scaled as either 0 or 100. Figure 5-2b shows a map with all conflict events in our sample period, distinguishing between conflict with less than 5 BRD, with between 5 and 25 BRD, and more than 25 BRD. Studies at the country level usually use thresholds of 25 or 1000 to define a conflict. As our research is at the smaller first-order sub-national level, we choose 5 BRD per country-region-year as the threshold in our main specification. For robustness tests, we also use 25 BRD, as well as the log of BRD as a continuous conflict intensity indicator. We also use GED to code whether an outright conflict was a two-sided fight between government-related groups and non-state actors (rebels), or a one-sided action by either of those sides against civilians.

To examine protests and repression, we make use of the Social Conflict Analysis Database – SCAD (Salehyan et al., 2012). It provides reliable and detailed geo-referenced information for Africa. We also define a binary *protests incidence* indicator. It takes the value one if there was at least one event in either of the categories demonstrations, strikes or riots, as well as an indicator for *government repression.* [28] Government repression includes, for instance, increased surveillance

[27] Alternatives are the ACLED and PRIO datasets, which rely on similar primary data as UCDP. One issue with PRIO Gridded data is that neighboring cells in a 50km radius are also coded as conflict-affected, which may lead to erroneous conflict coding of neighboring administrative and ethnic regions (Tollefsen et al., 2012). ACLED is broader in coverage than UCDP data, but is criticized for its partly ambiguous inclusion criteria and vague geo-coding (Eck, 2012).

[28] SCAD defines government repression as a "Distinct violent event waged primarily by

activities like in Niger, where "after conducting one month of surveillance, the government arrested 9 military officers said to be planning a coup." Figure A5-A4 illustrates the spatial distribution of protests and repression across Africa. Finally, we use selected questions from Afrobarometer Data (2018) to measure perceptions of security, democratic norms and attitudes, as well as government responsiveness and repression. The first round started in 12 African countries between 1999 and 2001, while round 6 covers 36 countries between 2014 and 2015. Table A5-A7 provides details.

5-3.3 Control Variables

Even though we will not decisively rely on control variables due to the bad control problem, we provide specifications using the most important aspects highlighted in the previous literature. Initial regional development is proxied using nighttime light (Henderson et al., 2012). Regional population matters for aid allocation. Population calculation is based on the *Gridded Population of the World* dataset (Center for International Earth Science Information Network (CIESIN) Columbia University, 2016). From the PRIO Gridded data (Tollefsen et al., 2012), we use several natural resource indicators including oil, gold, gemstones, and narcotics, as well as measures on temperature and precipitation, that can be linked to conflict (Miguel et al., 2004). To match the gridded data to the respective region-year, we intersect the PRIO-Grid with the AMD1 shapefile and calculate area-weighted averages for each region. Robustness tests use data from Cederman et al. (2014) and Wucherpfennig et al. (2011) about the distribution of ethnic groups. Table A5-A4 in the data appendix provides a more detailed overview of all variables used at any part of the chapter.

Table 5-2 provides summary statistics. The final sample comprises 728 ADM1 regions in 45 countries. WB aid is, on average, higher per region-year than Chinese aid: USD 2.2 million versus USD 1.4 million, respectively. Figure 5-2a illustrates that both donors are active in a large number of countries and regions. Figure 5-2b reveals sufficient cross-sectional variation in conflict events across as well as within countries to estimate a demanding FE model.

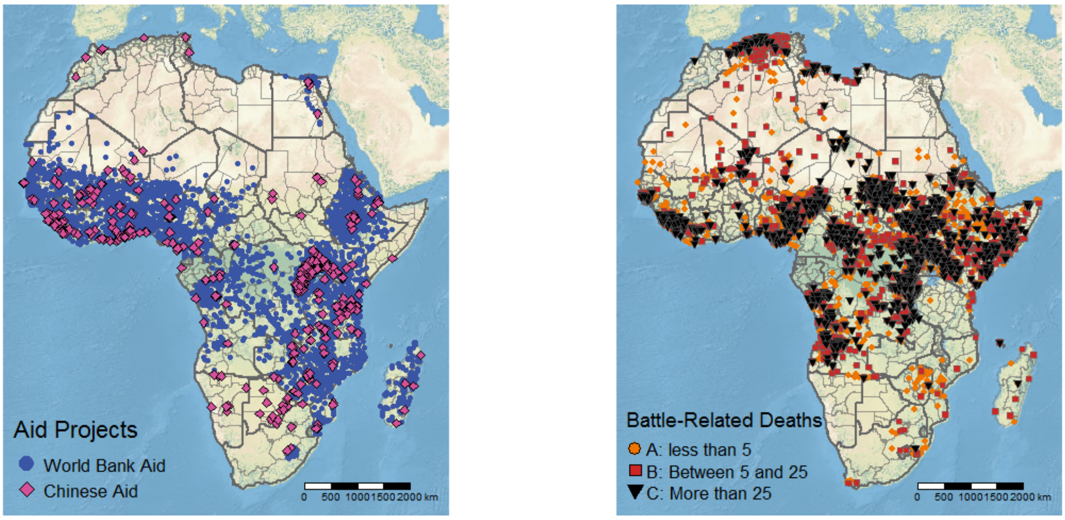
While the information for aid disbursements by the WB's IDA is available from 1995 to 2012, information on Chinese aid commitments in Africa is constrained to the years 2000 to 2012. Both the WB and China are active in most African countries – the WB in 35 countries, and China in 41 countries. There is a significant overlap in their presence between countries, but prior research found no evidence of one donor systematically affecting the allocation choices of the other (Humphrey and Michaelowa, 2018). Hence, we can run our regressions separately for each donor to exploit the full sample period for which we have WB data, without fearing a strong systematic bias in results.

government authorities, or by groups acting in explicit support of government authority, targeting individual, or "collective individual," members of an alleged opposition group or movement." (Salehyan et al., 2012). The coded events include, for instance, "Police arrested a prominent opposition lawyer," "Police arrested four members of comedy group who make videos making fun of the government" or "Militant youths allied with Malawi's ruling party to attack a newspaper photographer" (Salehyan et al., 2012). Repression is distinguished from government conflict against civilians by being associated with less than 5 BRD.

Table 5-2 – Descriptive statistics - ADM1 Region

	Mean	SD	Min	Max
World Bank Aid	2,240,340	8,991,909	0	488,643,178
ln(WB Aid)	6	9	-5	20
Chinese Aid	1,391,272	22,843,120	0	900,000,000
ln(Chinese Aid)	-4	4	-5	21
Riots, Strikes, Demonstrations in Perc.	14	34	0	100
Repression Incidence in Perc.	1	11	0	100
Conflict Incidence in Perc.	12	32	0	100

Notes: Descriptive statistics for our main variables. ln(Aid) is based on aid +0.01USD.



(a) (b)

Figure 5-2a Chinese (2000-2012) and WB (1995-2012) development aid. Authors' depiction based on AidData (2017) and Dreher et al. (2019).
Figure 5-2b Conflict 1996-2014. Authors' depiction based on Croicu and Sundberg (2015). Category 1 (binary) = B+C, Category 2 (binary) = C, Category 3 (continuous) = {A, B, C}
Notes: Depicted borders refer to countries (thick line) and first administrative divisions (thin line).

5-4 Empirical Strategy

5-5 Empirical Strategy

Of course, the aid projects shown on the map above are not randomly allocated. Donors may be more or less likely to select a region based on its conflict potential, which causes concerns about endogenous selection. Over the long term, reverse causality may also cause problems if regions formerly plagued by conflict receive more aid afterward. Considering Figures 5-2a and 5-2b helps to understand our two different approaches to identification. The first approach utilizes the sub-national data and condition step-by-step on more and more observables and unobservables through various fixed effects, time trends, and controls.

First, precise coding helps to precisely link aid and stability. Angola, for instance, receives more aid projects in regions that also experience more conflict. In contrast, the regions in Sudan that often receive aid are not the ones that experience conflict. Country-level studies, in contrast, would code both countries as cases where a country received aid and also experienced conflict. Second, the correlation between aid and conflict is affected by unobserved region-specific factors that can make both receiving aid projects and conflict more likely. Region-fixed effects can eliminate time-invariant differences that affect this joint likelihood of receiving aid and experiencing conflict.

Third, country times year (from now on country-year) fixed effects eliminate the effect of any spurious event at the country-year level that could affect conflict and, by chance, coincides with changes in aid allocation, like a political regime change. Very restrictive specifications may eliminate too much variation and falsely conclude that there is no conflict-fueling effect of aid. For that reason, we eliminate potentially biasing variation step-by-step for transparency. In the following,we assess the direction of possibly remaining bias and propose an IV strategy for each donor.

5-5.1 Fixed effects, time trends and control variables

Our two baseline empirical specifications are

$$C_{i,c,t} = \beta_1 A_{i,c,t-1/t-2} + \delta_i + \tau_t + \Delta_i T + X'^{Ex}_{i,c,t}\beta_2 + \epsilon_{i,c,t}, \tag{5-1}$$

$$C_{i,c,t} = \beta_1 A_{i,c,t-1/t-2} + \delta_i + \tau_t + \Delta_i T + X'^{Ex}_{i,c,t}\beta_2 + \kappa_{c,t} + \epsilon_{i,c,t}, \tag{5-2}$$

where $C_{i,c,t}$ is our conflict indicator of interest in region i, in country c and year t. $A_{i,c,t-1/t-2}$ is log of per capita aid. Note, that we distinguish between World Bank aid *disbursements* and Chinese aid *commitments*. We lag WB aid disbursements by one year. For Chinese aid commitments, we use a lag of two years. On average, Chinese aid is disbursed one year after financial commitments (following Dreher et al., 2019, 2017). Thus, the assumed time lag from disbursements to conflict in the next year is the same for both donors.[29]

Our specifications includes region, and time fixed effects, δ_i and τ_t. Furthermore, we add regional linear time trends $\Delta_i T$ to control for any differing linear conflict trends across regions. Including country-year fixed effects $\kappa_{c,t}$ asks a subtly different question: conditional on whether the whole country is involved in a conflict or not in a particular year, how did previous aid receipts affect the conditional likelihood of a particular region to also be in conflict? For that reason, the following sections always consider one specification without (eq. 1) and one with country-year fixed effects (eq. 2).

We distinguish between three types of control variables. First, exogenous controls such as climatic shocks. Second, we account for the effect of time-invariant controls like elevation or ruggedness of terrain by interacting them with year dummies. These first two sets are contained in $X^{Ex}_{i,c,t}$, as they are not at risk of being bad controls. Third, we twice lag potentially "bad controls" like nighttime light (as a proxy for economic activity), or population, $X^{End}_{i,c,t-2}$, which can be affected directly by aid

[29]AidData cannot distinguish exactly how much money from the Chinese commitments is disbursed in a particular year for all projects, but where the information exists one year fits the data best (see also Dreher et al., 2017). An examination of further lags in Table A5-B2 suggests that this timing is not driving the subsequently reported results.

projects. Using "pre-determined" values solves the bad control issue only if we assume sequential exogeneity. For that reason, those variables are tested but not part of our preferred specifications. The error term is denoted as $\epsilon_{ir,t}$.

Standard errors are two-way clustered at both the country-year and the regional level (Cameron et al., 2011). This allows for arbitrary correlation within a country and year, which is important as conflicts often have a strong spatial component and tend to spill over. Also allowing for correlation within a region over time is important as conflict also tends to exhibit strong persistence over time. Tables A5-B41 and A5-B42 show that the results are similar for other clustering options.

5-5.2 Instrumental Variable approach

Our IV strategies exploit the heterogeneous impact of a plausibly exogenous time-series, which affects the amount of aid allocated, depending on a pre-determined cross-sectional difference in the probability to receive aid. The probability is computed by dividing the number of years a region i has received aid by the number of years passed until year $t-1$.[30][31] As in any Difference-in-Difference (DiD) setup, both regression stages control for the main constituting terms forming the interaction; only the interaction term is used as the conditionally exogenous instrument in the first stage. The identifying assumption is that, in the absence of a change in the time series, there would be common trends in aid allocation in low and high aid probability recipient regions. The IV for WB aid and Chinese aid use the same idea but differ in the donor-specific probability and in the time-varying factor T_t that induces variation over time.

Application to WB aid

Based on discussions with WB staff, as well as, recipient country personnel, the mechanism we exploit and document for identification is the following. We exploit the heterogeneous effect of yearly variation in the availability of additional "free" IDA resources on regions with an initially lower or higher likelihood of receiving aid.[32] If there are more funds available, the Bank may exhaust the funds and allocate them to recipient countries. Countries and regions already involved in projects may receive a larger share of the additional funds, partly due to lower costs of information screening and other preparation costs.[33]

[30] If beginning in 1995, and a region received aid in three out of five years, the aid probability in 1999 would be 0.6. If aid receipts stop in 1999, the probability declines to 0.5 in 2000 as the region received aid in three out of six years. Nizalova and Murtazashvili (2016) show that if the heterogeneity of interest (here the probability to receive aid) is independent from the treatment (here the donor's global aid budget) the interaction of exogenous and endogenous variables can be interpreted as exogenous when controlling for the endogenous factor (in this case the probability to receive aid). Using initial or pre-determined values allows us to relax these assumptions, compared to using a constant probability as in Nunn and Qian (2014) or (Bluhm et al., 2020).

[31] Nunn and Qian exploit temporal variation in US wheat production, interacted with a constant probability to receive US food aid.

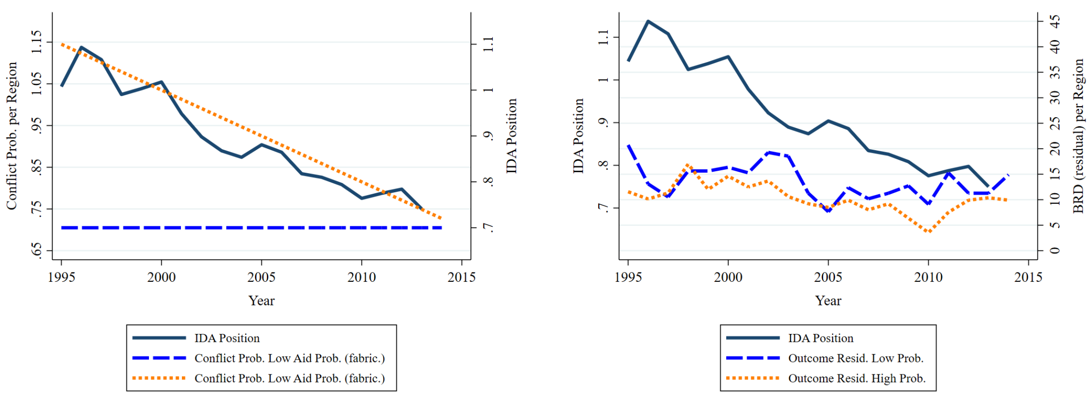
[32] The idea is based on Lang (2016) and Gehring and Lang (2018), who employ such a supply-push identification approach using variation in the IMF's liquidity.

[33] Galiani et al. (2017) use Gross National Income (GNI) as a threshold for IDA eligibility. We prefer the liquidity over graduation for three reasons. First, the continuous liquidity treatment covers a less specific LATE. Few countries only graduate and experience reductions in WB aid afterward. Second, Kerner et al. (2017) suggest that countries have leeway to postpone graduation

Variation in the funding position, defined as “the extent to which IDA can commit to new financing of loans, grants, and guarantees given its financial position” (World Bank, 2015), can be caused by internal adjustments, shareholders’ timing of payments, and repayments by large borrowers like India. It should be exogenous to stability in any individual sub-national African region, in particular, conditional on country or even country-year fixed effects.[34]

The IDA funding position is obtained by Dreher et al. (2017) from 1995 to 2007 and the World Bank’s annual financial reports from 2008 onwards.[35] This is interacted with the region’s pre-determined probability to receive aid, $p_{i,c,t-2}$, to capture that higher probability regions should profit more from higher funding positions. For simplicity, we do not display fixed effects, time trends, and control variables here, so that the equation becomes

$$Aid_{i,c,t-1} = \alpha_1 p_{i,c,t-2} + \alpha_2 IDA_{t-1} + \alpha_3 p_{i,c,t-2} IDA_{t-1} + \epsilon_{i,c,t-1} \tag{5-3}$$



(a) Problematic trends (fabricated) in outcomes

(b) Actual trends in outcomes

Figure 5-2 – WB- IDA funding position and conflict outcomes for low and high probability regions.

Note: Figure (a) displays the temporal variation we use in our interacted instrument, the IDA Funding Position (solid line), along with fabricated trends in the conflict outcomes for low (long-dashed line) and high probability (short-dashed line) recipient regions. The trends are fabricated to illustrate potentially problematic trend differences that could induce a spurious correlation. Figure (b) displays the IDA Funding Position (solid line), along with the actual trends in the conflict outcomes for low (long-dashed line) and high probability (short-dashed line) recipient regions. The displayed outcomes in (b) is the probability of experiencing regional conflict of more or equal to five battle-related deaths per year.

by reporting lower GNI estimates. In our sample, we find that the threshold does not always imply a strict reduction in IDA allocations.

[34]One worry is a correlation with the global level of conflict At the same time, a stronger correlation with conflict in high than in low probability regions. Controlling for global conflict levels interacted with the probability in Tables A5-B13 and A5-B14 does not affect the first or second stage results.

[35]Because the WB’s fiscal year ends in June, the reported position in the fiscal years t and t-1 can both affect disbursements in t-1. Using only the position in t-1 is a viable alternative and also works well in first stage estimations, which is demonstrated in Table A5-B7. Using both fiscal years t and t-1 to compute the funding position appears more coherent and is applied subsequently.

One potential problem associated with approaches like this is that, even if the temporal variation is plausibly exogenous, trends in the time series may overlap with differing trends in the outcome variable, leading to a spurious IV effect. This risk is exacerbated if the time series is relatively short and dominated by long-term trends (Christian and Barrett, 2017). The left-hand side of Figure 5-2 shows how systematic differences in the long term conflict trends between low and high probability regions could bias estimates. The right-hand side figure then shows that the relevant variation in outright conflict exhibits no such trends. Despite a general decline in the funding position, there is sufficient year-on-year variation.

Application to China

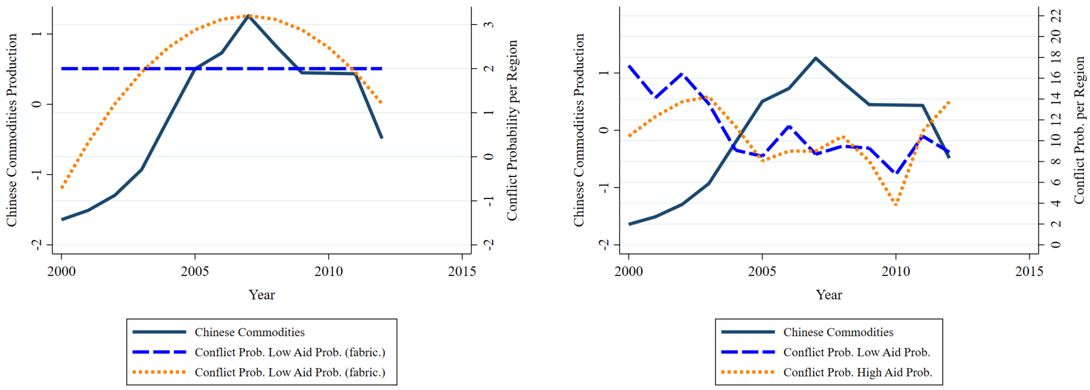
Regarding China, we make use of the fact that the economic structure and political incentives frequently lead to excess domestic commodity production. To clear markets and protect domestic companies from potential losses, China commits to more aid projects abroad (Dreher et al., 2017; Bluhm et al., 2020). This pattern is not entirely unknown from European agricultural overproduction. These additional projects are often large-scale infrastructure projects that directly use overproduced commodities as inputs (Bräutigam, 2011), but Bluhm et al. (2020) show that commodity (over-)production also induces variation in other sectors like education or health. It thus captures a local average treatment effect, but seems to trigger variation in the sectors that are overall representative of Chinese aid.

Chinese "mega-deals" (Strange et al., 2017) cannot easily be duplicated or scaled within regions, and the country tries to strongly expand its influence during our sample period. Thus, additional projects are more often implemented in low probability regions that had initially no or very few projects.

We follow Bluhm et al. (2020) and use principal component analysis to construct a time series on Chinese domestic commodity over-production, $T_{i,c,t}$. The time-varying variable is interacted with the region's pre-determined probability to receive aid, $p_{i,c,t-3}$. This captures that lower probability regions should profit more from Chinese commodity overproduction. The first stage equation is

$$Aid_{i,c,t-2} = \alpha_1 p_{i,c,t-3} + \alpha_2 Commodity_{t-3} + \alpha_3 p_{i,c,t-3} Commodity_{t-3} + X^{Ex}_{i,c,t}\alpha_4 + \epsilon_{i,c,t-2} \tag{5-4}$$

The left-hand side of Figure 5-3 illustrates differing long-term conflict trends in low and high probability regions, which would lead to biased estimates. The commodity time series variable is inverse U-shaped. The IV results may be spurious if conflict trends in either low or high probability regions would, for other reasons, also follow such a pattern. The right-hand side graph, however, assures us that this is not the case.



(a) Problematic trends (fabricated) in outcomes

(b) Actual trends in outcomes

Figure 5-3 – China: Chinese commodities production and conflict outcomes for low and high probability regions.

Notes: Figure (a) displays the temporal variation we use in our interacted instrument, the Chinese Commodities Production (solid line), along with fabricated trends in the conflict outcomes for low (long-dashed line) and high probability (short-dashed line) recipient regions. The fabricated trends illustrate potentially problematic trend differences that could induce a spurious correlation. Figure (b) displays the Chinese commodity (over-)production (solid line), along with the actual trends in the conflict outcomes for low (long-dashed line) and high probability (short-dashed line) recipient regions. The displayed outcomes in (b) is the probability of experiencing regional conflict of more or equal to five battle-related deaths per year.

5-6 Results

5-6.1 Outright conflict – OLS, fixed effects and time trends

To allow readers to evaluate a potential trade-off between eliminating bias and over-controlling, we begin by showing simple correlations. We then add fixed effects, time trends, and different categories of control variables step-by-step. Beginning with WB aid in Table 5-3, we find that the raw correlation with conflict incidence is negative. Adding country and year fixed effects shifts the coefficient upward (column 2); adding country-specific linear and quadratic trends to capture country-specific conflict dynamics moves the coefficient slightly downward to -0.05 (column 3). When adding region fixed effects, which capture region-specific, time-invariant attributes, that can explain heterogeneity within countries, the point estimate nearly quadruples in size to -0.21 and becomes statistically significant at the 1%-level (column 4).

Adding exogenous controls, and time-invariant region characteristics, interacted with year dummies to capture their potentially time-varying influence (column 5), as well as adding region-specific linear time trends, changes the coefficient only slightly (column 6). Column 8 goes one step further by controlling for country-year fixed effects. The remaining variation then is only due to differences in aid across regions within country-years, conditional on whether the country as a whole experience a conflict. Despite the restrictive specification, the robust negative relationship between WB aid and conflict does not disappear and remains significant at the 5%-level. The coefficient of -0.1772 suggests that a one standard deviation change in log WB aid is associated with a decrease in the conflict likelihood of $9 \times 0.1772 \approx 1.59$ percentage points. To put this into perspective, the average of conflict incidence with our threshold of five battle-related deaths (BRD) is 12 percent; accordingly, this is small, however, it is a non-trivial change. The coefficient becomes insignificant when controlling for lagged values of factors that are potentially endogenous controls (columns 7 and 9), but remains negative. Although these are only conditional correlations, the fact that 8 out of 9 coefficients are negative suggests that there is no conflict-fueling effect of WB aid, on average.

Turning to China, our theoretical prior was that certain arguments suggest a positive relationship with conflict to be more likely when involved with Chinese aid. Nonetheless, the raw correlation with conflict is also negative. The coefficient drops drastically in size when adding country and time fixed effects, as well as country-specific time trends (columns 2 and 3), but loses significance. Overall, the coefficients are much smaller and closer to zero than those for the WB. Remarkably, however, there is not a single positive coefficient, also suggesting no signs of a conflict-inducing effect of Chinese aid. Our preferred specifications in columns 6 and 8 indicate that increasing log Chinese aid by one standard deviation is associated with a decrease in the conflict likelihood by $4 \times 0.0654 \approx 0.26$ percentage points.

Table 5-3 reveals the degrees of freedom researchers possess in selecting their preferred specification in such a setting. We find it reassuring that throughout these different specifications, there is no sign of a conflict-inducing effect for either WB or Chinese projects. Relating to the ideas about assessing coefficient changes when moving towards more restrictive specifications in Altonji et al. (2005), we see that the effect of adding additional FE, trends, and covariates neither suggests a strong systematic upward, nor a downward bias.

The confidence interval comprises negative, zero, and some positive effects. Still, considering the rich set of specifications we examined, it seems highly unlikely that other unobserved factors would push the average effect towards an economically meaningful and statistically significant conflict-fueling effect. Even if there were substantial changes in Chinese aid, they would not fuel conflict by much compared to the average likelihood of conflict of 12 percent. The following uses our preferred specifications in columns 6 and 8.

Table 5-3 – OLS results - Aid and conflict likelihood

Panel A: WB Aid	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\ln(World\,Bank\,Aid_{\text{t-1}})$	-0.1918*	0.0010	-0.0496	-0.2129***	-0.2057***	-0.1608**	-0.1314	-0.1772**	-0.1756**
	(0.0989)	(0.0776)	(0.0683)	(0.0659)	(0.0651)	(0.0717)	(0.0831)	(0.0816)	(0.0894)
N	13104	13104	13104	13104	13050	13050	11699	13050	11699
Panel B: Chinese Aid									
$\ln(Chinese\,Aid_{\text{t-2}})$	-0.1753**	-0.0233	-0.0026	-0.1090*	-0.0663	-0.0654	-0.0682	-0.0347	-0.0441
	(0.0865)	(0.0705)	(0.0642)	(0.0572)	(0.0644)	(0.0726)	(0.0725)	(0.0883)	(0.0917)
N	9464	9464	9464	9464	8700	8700	8254	8700	8254
Country FE	No	Yes	Yes	–	–	–	–	–	–
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	–	–
Time Trends	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls × Year FE	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Linear Regional Trends	No	No	No	No	No	Yes	Yes	Yes	Yes
Lagged Endogeneous Controls	No	No	No	No	No	No	Yes	No	Yes
Country × Year FE	No	No	No	No	No	No	No	Yes	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD$\geq$5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5-6.2 Outright conflict – Instrumental Variables

Table 5-4 shows the IV results with and without country-year fixed effects. Overall, the first stage works better for the WB (F= 99/86) than for China (F=36/31); all F-statistics, however, are well above the critical value of 10. The interaction term between the prior probability to receive aid with the IDA position, respectively Chinese commodity production, is highly significant in the first stage, and the signs of the coefficients align with our priors. Regions with a higher pre-determined probability profit more from a higher WB liquidity, regions with an initially lower probability profit more from an expansion of the Chinese aid budget. Table A5-B3 and A5-B4 indicate that the WB first stage effect works both through the extensive and intensive margin. High probability regions have a higher likelihood to profit by receiving aid in a particular year, and conditional on receiving aid in a given year, the size of the disbursements also becomes larger. For China, Table A5-B3 reveals that as expected the first stage relationship is mainly driven by the extensive margin, e.g., the likelihood of having at least one active project in a specific region-year. Regions without pre-existing projects are more likely to receive a project as the Chinese development budget expands.

The second stage results largely confirm the OLS results. Both specifications yield negative coefficients for the WB and China. The coefficients for the WB are somehow smaller (larger) in the specification without (with) country-year FE, and become statistically insignificant. The coefficients for China become much more negative, however, they remain insignificant. There is again no evidence for a conflict-fueling effect of aid projects for either of the two donors. While being insignificant, the coefficient would imply that increasing log WB aid by one standard deviation decreases the conflict likelihood by about $9 \times 0.2252 \approx 2.03$ percentage points. Similarly, raising log Chinese aid by one standard deviation would decrease conflict by $4 \times 0.1886 \approx 0.75$ percentage points.

By definition, IV estimates are identified using a particular kind of variation in the variable of interest that is caused by the excluded instrument (local average treatment effect (LATE)). Comparing the IV point estimates with OLS shows no difference with regard to the direction of the effects, but minor variations in size. To check whether the direction of the changes is plausible, Table A5-B2 shows OLS specifications with three lags, the contemporaneous value, and a lead term of the treatment variable. For the WB, there are no clear indications of a pre-trend that would signal selection bias, in line with the IV estimate being very close to the OLS estimate. For China, the lead term is positive, indicating that it is more likely to select into regions that will experience conflict in the future.[36] This potential selection effect would suggests an upward bias in China OLS coefficients. The fact that the IV coefficients for China are more negative suggests that the IV helps to address this concern.

[36] Any concerns that the effect for China would be biased as it generally tends to go into countries which are transitioning towards autocracy are addressed in the specifications with country-year fixed effects.

Table 5-4 – IV results - Aid and conflict likelihood at the ADM1 level

Panel A: World Bank Aid	(1)	(2)
IV Second Stage: World Bank		
ln($World\,Bank\,Aid_{\text{t-1}}$)	-0.1014	-0.2252
	(0.3752)	(0.4192)
N	12325	12325
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	99.639	86.724
IV First stage: World Bank		
$IDA\,Position_{\text{t-1}} \times \text{Cum. Prob}_{\text{t-2}}$	70.9363***	80.8832***
	(7.1065)	(8.6854)
N	12325	12325
Panel B: Chinese Aid	(1)	(2)
IV Second Stage: China		
ln($Chinese\,Aid_{\text{t-2}}$)	-0.2582	-0.1886
	(0.4282)	(0.5256)
N	7975	7975
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	36.578	31.190
IV First stage: China		
$Chinese\,Commodity_{\text{t-3}} \times \text{Cum. Prob}_{\text{t-3}}$	-14.0193***	-12.6964***
	(2.3180)	(2.2734)
N	7975	7975
Country-Year FE	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Specification is identical to identical to Table 3, column 8. Probability is included but the coeffcient not displayed to save space (full results in Table A 5-B5. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5-6.3 Results - Types of Conflict and Actors

Table 5-5 shows the results using these distinctions with and without country-year FE. The coefficients for two-sided conflict action by government or rebels against each other (column 1 and 2), or between different rebel groups (column 3 and 4) are partly of an economically significant size, but are all far from being statistically significant for both donors. In accordance with our theoretical priors, we find that in a region that receives more WB or Chinese aid, there are, however, significantly less conflicts with at least five battle-related deaths (BRD) by the government against civilians (column 5 and 6). A one standard deviation change in log WB aid decreases the likelihood of violence against civilians with at least 5 BRD by $9\times0.2939 \approx 2.61$ percentage points. This is plausible as the WB is known to punish human rights violations by governments. For instance, suspending aid payments in Indonesia to push the government towards finding peaceful bargaining solutions in Timor (Tir and Karreth, 2018).

Although Tir and Karreth (2018) focus their arguments on international organizations like the WB, which impose strong conditionality. The fact that we also find the same significant effect, even larger in size, for China, validates our prior that China also informally has the incentives and ability to stop recipient governments from engaging in conflicts that may be deemed undesirable from the donors perspective. Changing log Chinese aid by one standard deviation decreases the likelihood of this type of conflict substantially by $4 \times 0.5673 \approx 2.27$ percentage points. The value China attributes to social stability, business interests and the widespread presence of Chinese workers may be reasons to convince recipient governments to abstain from engaging in actions that cause civilian casualties and endanger stability. Tir and Karreth (2018) argue that the prospect of gaining access to aid could also constrain rebels. But we find no equivalent significant reduction in rebel violence against civilians (column 7 and 8).

Table 5-5 – Aid and conflict types by actors

Panel A: World Bank Aid - IV	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	State vs. N-State		N-State vs. N-State		State vs. Civilians		N-State vs. Civilians	
$\ln(World\ Bank\ Aid_{\text{t-1}})$	-0.4177	-0.4319	0.1252	0.1488	-0.3579*	-0.2939*	-0.0961	-0.1417
	(0.3174)	(0.2630)	(0.2096)	(0.2447)	(0.1885)	(0.1739)	(0.2072)	(0.2704)
N	12325	12325	12325	12325	12325	12325	12325	12325
Kleibergen-Paap underidentification test p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Kleibergen-Paap weak identification F-statistic	99.639	86.724	99.639	86.724	99.639	86.724	99.639	86.724
Panel B: Chinese Aid - IV								
	State vs. N-State		N-State vs. N-State		State vs. Civilians		N-State vs. Civilians	
$\ln(Chinese\ Aid_{\text{t-2}})$	0.2749	0.2200	0.2462	0.4178	-0.5336**	-0.5673**	-0.3273	-0.3553
	(0.2104)	(0.2280)	(0.1924)	(0.2637)	(0.2300)	(0.2877)	(0.2520)	(0.3066)
N	7975	7975	7975	7975	7975	7975	7975	7975
Kleibergen-Paap underidentification test p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Kleibergen-Paap weak identification F-statistic	36.578	31.190	36.578	31.190	36.578	31.190	36.578	31.190
Country-Year FE	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Specification is identical to identical to Table 3, column 8. ”State vs. N-State” refers to state-based violence against non-government actors, ”N-State vs. N-State” refers to non-government violence against the other organized non-state groups, and ”State vs. Civilians” refers to one-sided violence versus civilians by the government and ”N-State vs. Civilians” refers to one-sided violence versus civilians by non-state actors. The categories are mutually exclusive. Standard errors in parentheses, two-way clustered at the country-year and regional level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Table A5-B25 depicts corresponding OLS results.

5-6.4 Results - Protest and government repression

Panel A of table 5-6 shows the results for our two main specifications, but now, replacing the outcome variable with an indicator, measuring whether at least one demonstration, riot, or strike took place.[37] For the WB, both specifications yield a negative coefficient but remain statistically insignificant. Regarding China, we observe negative coefficients, which are of modest size (100% more aid increase the likelihood of riots by 0.07%) and remain statistically insignificant. Accordingly, despite reports indicating increasing protests against the presence of Chinese business (Wegenast et al., 2017), we find no clear relationship between Chinese aid and citizen protests over our sample period.[38]

Recipient governments may achieve this absence of protests and outright conflict by intensifying non-lethal repression. Panel B of table 5-6 tests whether aid is related to more reports of non-lethal government repression. In the underlying SCAD data events range from the repression of opposition lawyers to constraining anti-government artists in Egypt and media restrictions in Malawi (Salehyan et al., 2012). [39] The results indicate neither a positive nor significantly negative relationship for the WB. The results for China contrast our previous findings and establish that repression intensifies in regions where China is present. A 100% increase in Chinese aid increases the likelihood of experiencing repression by about 0.77%, which is significant, considering an average of 2.26%.

[37] Table A5-B27 depicts corresponding OLS results. Tables A5-B19, A5-B20 and A5-B21 show OLS regressions separately for demonstrations, riots and strikes; Table A5-B22 separate IV estimates. None of them turns out significant once region FE are included.

[38] See, for instance, The Telegraph, last accessed 02.02.2019.

[39] Table A5-B24 reports results for a count variable of non-lethal pro-government violence events, which are robust to this change in the outcome variable. Table A5-B23 verifies that this is driven by events recorded in SCAD that are distinct from the UCDP events, by coding only those region-years as a one that did not experience lethal government violence against civilians according to UCDP.

Table 5-6 – Protests and non-lethal government repression [SCAD]

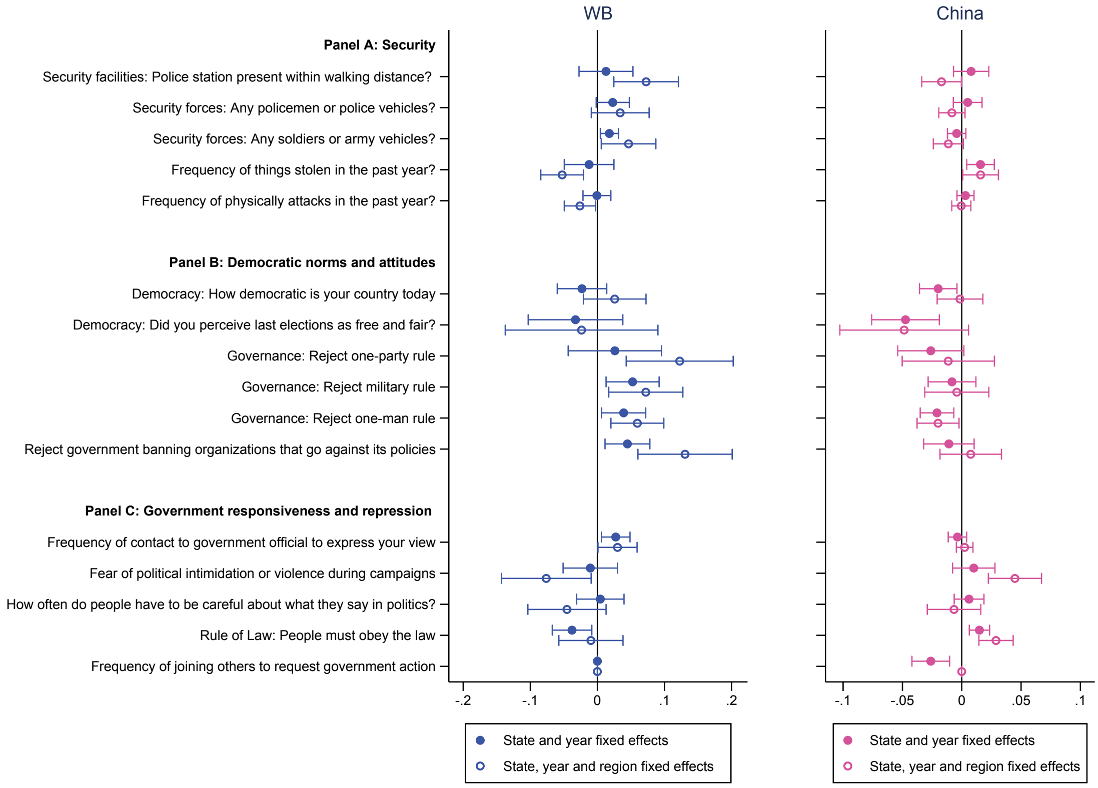
Panel A: Riots, demonstrations or strikes	(1)	(2)
IV Second Stage: World Bank		
ln($World\,Bank\,Aid_{\text{t-1}}$)	-0.3854	-0.2032
	(0.3092)	(0.3362)
N	12325	12325
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	99.639	86.724
IV Second Stage: China		
ln($Chinese\,Aid_{\text{t-2}}$)	-0.1599	-0.0742
	(0.3964)	(0.4452)
N	7975	7975
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	36.578	31.190
Panel B: Non-lethal Government Repression		
IV Second Stage: World Bank		
ln($World\,Bank\,Aid_{\text{t-1}}$)	0.1543	0.0885
	(0.1042)	(0.1177)
N	12325	12325
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	99.639	86.724
IV Second Stage: China		
ln($Chinese\,Aid_{\text{t-2}}$)	0.6103**	0.7696**
	(0.2873)	(0.3439)
N	7975	7975
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	36.578	31.190
Country-Year FE	No	Yes

Notes: The dependent variables are binary protest and government repression incidence indicators, taking on the value 1 if there was at least one event in the respective category. The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China).Specification is identical to identical to Table 3, column 8. Standard errors in parentheses, two-way clustered at the country-year and regional level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5-6.5 Results - Attitudes

Examining the associated mechanisms for all effects is beyond the scope of this chapter. Still, we can present some correlational evidence using geo-referenced Afrobarometer data to investigate the plausibility of some of our results. To do so, we match data, from all Afrobarometer waves to the regions and years in our sample, and compute the region-year level average of each question we use. Details are provided in Appendix Table A5-A6. Note that the survey covers varying subsets of all African countries in selective years so that the resulting dataset comprises an unbalanced panel with gaps. The temporal variation is not sufficient for a strong first stage using the IV. We can only use less restrictive sets of fixed effects than in our main specifications. Figure 5-4, thus, plots the coefficients from individual OLS regressions of selected relevant questions on WB and Chinese aid: model 1 uses country and time FE, model 2 region and time FE.

Figure 5-4 – OLS regressions on mechanisms using Afrobarometer for WB and China



Notes: The figure shows coefficient plots along with 90% confidence intervals of individual OLS regressions of log WB and log Chinese aid on the respective questions from Afrobarometer. All outcome question responses were standardized with mean zero. Respondents were matched to the ADM1 regions using the provided geocoordinates. Table A5-B37 provides the full regression results. Afrobarometer surveys were conducted in the years 1999-2015 for a varying number of 12 to 36 countries, resulting in an unbalanced panel with uneven gaps between years.

The results are grouped into three categories. Panel A refers to questions signaling the presence of state security forces as a measure for state capacity within the area, and the ability to maintain a monopoly of violence. Moreover, we use two questions asking whether respondents or their families were the victims of robbery or physical attacks in the past year. The results suggest that the WB engagement is associated with an increase in security forces and a reduction in crimes. There is no such increase for China. However, one needs to keep in mind that these are conditional correlations, and China may select into regions more likely to experience conflict and a deterioration in state capacity.

Panel B examines democratic norms and attitudes. The results are not necessarily causal, but differences stand out that reflect the differential approaches of both donors. There are indications that the perception of democracy, and the fairness of elections, deteriorate in regions with Chinese aid projects. The WB seems to have a consistently positive impact on democratic norms and a neutral to a positive effect on stability. Respondents are more likely to reject one-party rule, military rule, and one-man rule, which is not the case for China. With the coefficients being consistently significant in both models regarding one-man rule, respondents are less likely to reject these authoritarian governance forms. This could indicate that China helps some authoritarian regimes to stay in power. In a more detailed examination, Isaksson and Kotsadam (2018a) also find deterioration in norms, and an increase in local corruption, associated with Chinese projects.

Panel C examines questions indicating the way the government interacts with its citizens and its use of repression. In regions with more WB aid, people report being more apt to contact their government officials and express their views frequently. There is no such effect for China. In regions with WB aid, the fear of political intimidation or violence is lower, while it is higher in regions with Chinese aid activities. At the same time, there is no apparent difference in whether people think they have to be careful what they say privately about politics. Finally, two results stand out. In regions with more Chinese aid, respondents state much more clearly that people must always obey the law. Moreover, there is a negative correlation between Chinese aid and the willingness to join others to request government action. These correlations correspond to the different norms and conditions of the WB and China that we described above.

Importantly, all of these results on mechanisms need to be interpreted cautiously and do not necessarily signal causality. Still, they underline that the different approaches taken by the two donors matter. It is important to reconsider that aid by both donors is, if anything, leading to less conflict. The results on mechanisms suggest that, WB aid goes with improved democratic norms and security provision by the government. For China, one interpretation is that the country is exporting stability which results in a reduction in the likelihood of certain types of conflict. Still, this increase of stability seems to come at the cost of increased government repression in addition to a weakening of democratic processes.

5-6.6 Sensitivity

Modifiable area unit problem - different aggregation levels: First, we aggregate at the country level. This allows us to see the aggregate impact of potential spill-overs to other regions and enables us to compare our main results to studies at the country level. We show results both with and without controlling for the share of aid projects that could not be assigned to a particular ADM1 region. These are, to a large extent, projects where money flows directly to the central government. The coefficients are also negative for both donors in both specifications. Thus, our results at the local level do not seem to be driven by choosing a particular spatial unit.[40]

In Table A5-B33 (A5-B34), we move towards OLS (IV) regressions at a lower level of aggregation, the ADM2 level. Note that we are capturing a smaller share of all projects at this level due to the precision level in the georeferencing. The OLS results for the WB and China are both similar to the ones at the ADM1

[40]Point estimates for the less precisely coded aid can be found in Table A5-B36. Although the coefficient for non-geocoded WB aid at the country level turns positive it remains small and insignificant. This supports that there is also a null effect at the country level. OLS and IV point estimates for geo-coded aid aggregated at the country level are shown in Table A5-B35. The coefficients remain small and insignificant, as well.

level. All coefficients are insignificant, and the majority are negative, especially, when conditioning on more restrictive fixed effects. The IV point estimates differ somehow but never become statistically significant.

Choice of conflict indicator: As we discuss in the data section, there is no "correct" coding of the dependent variable, just more and less plausible choices. Table A5-B15 (A5-B16) presents alternative regression results with a higher conflict threshold of at least 25 BRD per region-year using the OLS (IV) specifications. Table A5-B17 (Table A5-B18) considers the log of battle-related deaths (+0.01) as a continuous measure of conflict intensity instead of looking at a binary indicator of conflict incidence using OLS (IV). We find largely negative OLS coefficients for the WB and slightly positive ones for China. However, with IV, both coefficients turn negative, in line with previous results.

Instrumental variable: We conduct the majority of robustness tests with regard to our IV strategy. As outlined, we take the concern serious that our instrumental variable may intersect with a spurious trend as suggested by Christian and Barrett (2017). In this regard, when taking non-stationarity (Table A5-B8) of the time series into account by taking first differences of conflict, aid and our liquidity indicator. The second stage results in Table A5-B9 remain clearly indistinguishable from zero. While it is arguably unlikely that conflict in a specific sub-national region determines our global liquidity indicators, we also assess the robustness of the instrumental variables by controlling for global conflict levels in Tables A5-B13 and A5-B14. The strength of the instruments remains virtually unaffected and the point estimates remain negative and statistically insignificant.

The second component of the IV, the probability term, may be computed in different ways. We test various plausible options. The cumulative probability is advantageous, as it only uses pre-determined values; yet, it could create problems if the probability in the first year(s) is not sufficiently informative. Table A5-B10 drops the first year of the corresponding panel (starting at 1998 for the WB's IDA, and 2003 for Chinese Commodity Production). Thus, the first probability is based on at least two observations. Table A5-B11 uses a constant probability from the third year of the respective sample onwards. Table A5-B12 drops the 10 highest leverage region-year observations. Figures A5-B1 and A5-B2 display the IV estimates dropping country-by-country, to avoid the possibility of the relationship being driven by one particular state. Both first and second stage results are robust to all these choices and specifications.

Moreover, Table A5-B6 reports reduced-form estimates. Table A5-B1 uses a lead of aid as a placebo treatment in the first stage, which always shows up as statistically insignificantly. Table A5-B5 reports the first stage, including the coefficient for the probability.

Political Systems: Development aid may have differential impacts across political systems due to different allocation decisions and distributional aspects. As a further sensitivity exercise, we consider heterogenous effects across democratic and autocratic systems based on the distinction due to the Polity IV data (Marshall et al., 2014). The WB disburses 20% of its aid to democratic countries, where 38% of Chinese commitments go to regions of democratic states.[41] Considering the results

[41]On a first view this allocation patterns may seem surprising. Yet, a selection mechanism may prevail, where both donors give more to the opposed political system, e.g., to foster regime change (Aidt et al., 2018).

in Tables A5-B31 and A5-B32 , we find that effects across democratic and autocratic subsamples are similar in sign and statistical significance as average effects. While the effects for the WB are insignificant, repression significantly increases in regions that receive Chinese aid both in autocracies and democracies. Thus, we do not find evidence for heterogenous effects across regimes.[42]

Both donors in same specification: One trade-off was whether to show both donors over the same period and in the same equation. This should not be decisive, as China is only active in 6% of the region-years that also feature WB projects. Moreover, Humphrey and Michaelowa (2018) find no systematic relationship between the selection of locations by the two donors at the country-level. Still, accounting for aid from one donor as a potential omitted variable in the other donor's equation is a potential issue. Table A5-B47 (Table A5-B48) shows that the OLS (IV) results also suggest no conflict-fueling effects when including both donors jointly. In joint IV specifications for both donors, the K-P F-statistics for the WB becomes smaller than 10 (Table A5-B48), giving rise to concerns about a weak IV. Still, the table shows that both instruments capture distinct variation: the interaction instrument for the WB is still significant in explaining variation in WB aid, and the IV for China is still significant in explaining variation in Chinese aid. With the caveat of a weak IV in mind, the table still indicates no conflict-fueling effects for both donors.

Non-linear estimators: In line with Berman et al. (2017b), we also run a Poisson Pseudo Maximum Likelihood estimation in Table A5-B38, which is suitable for binary outcomes with a large fraction of zeros. Moreover, we implement a negative binomial estimation in Table 5-B39. The results are generally in line with the main findings in terms of coefficient signs. However, note that the models only converge when restricting us to the use of year fixed effects.

Temporal dependence: As conflict may be highly persistent over time, we include a lagged dependent variable in Table A5-B40. The results are very similar, with mostly negative and partly significant coefficients for the WB and China.

5-7 Conclusion

China constantly increases its range of development projects in Africa. This raises both hopes and rejections among political and academic observers. The big question is whether African countries will benefit or suffer from this foreign engagement? To answer this question, we compare the effect of Chinese aid on state stability to a donor that represents a strongly contrasting approach to development -- the World Bank (WB). China is the major emerging donor, emphasizing mutual economic benefits without official economic or political conditions for recipient governments and has no specific guidelines to manage potential conflict risks (Asmus et al., 2017; Hernandez, 2017). In contrast, the WB is a traditional, multilateral donor that emphasizes human right conditions, expert knowledge, and engages explicitly in conflict-sensitive programming. Without taking a normative stance, we compare the effects of those different development approaches on a comprehensive set of stability measures. The chapter contributes to the literature by providing, as we hope, the

[42] We also try to capture changes in the aid approach by traditional donors like the WB by splitting the sample in two periods. The results in Table A5-B43 support the main finding that aid on average does not effect outright conflict in either sample.

most comprehensive analysis of the causal effect of development aid projects on a comprehensive set of stability measures in a multi-country analysis at the subnational level this far.

Our results using aid projects and outright conflict in the same region show no signs of a conflict-fueling effect. The WB tends to have a conflict-reducing effect in some fixed effects specifications, but when using instrumental variable strategies, estimates for both donors are negative and insignificant, on average. Looking at heterogeneity with regard to actors and types of conflict, we find that the threat of losing out on future aid payments leads to a reduction in lethal violence by governments against civilians related to both Chinese and WB projects.

In contrast to a substantial amount of media reports, we also find no net effect of Chinese aid projects on civilian unrest and protests in Africa. At the same time, we do, however, observe that in regions in which China is engaged the likelihood of government repression against targeted individuals or groups increases. Thus we cannot say with certainty whether the non-significant result on protest reflects the higher costs of protesting due to repression or that there is no reason to protest. WB aid has neither a significant net effect on protests or government repression.

Nonetheless, when considering attitudes from Afrobarometer surveys, our results suggest that WB aid has positive effects on perceived safety, democratic norms, and democratic values. Chinese aid is associated with attitudes related to stability like a higher adherence to the rule of law, but also with a higher acceptance of autocratic approaches. The results suggest a rationale where China is eager to export stability and avoid violent conflict that endangers its workers and investment. China may also be more supportive of repression and autocratic rule than traditional Western-influenced donors like the WB.

5-8 Data Appendix

5-A1 Sources

Table 5-A1 lists descriptions and sources of our independent, dependent and control variables.

Table 5-A1 – Data Sources

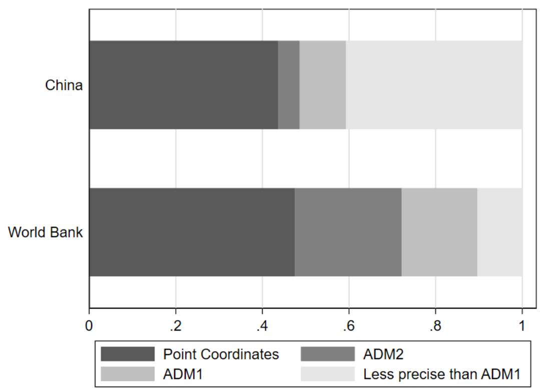
Variable Name	*Variable Description*	Time Period	*Variable Source*
WB Aid	log of WB Aid disbursements per region-year	1995-2012	Strandow et al. (2011)
Chinese Aid	log of Chinese Aid commitments per region-year	2000-2012	Dreher et al. (2017)
Strikes, Riots, Demonstrations	Binary indicator (100;0) if any violent event of this type in a given region-year took place	1995-2012	Salehyan et al. (2012)
Intensity 1/2	Binary indicator (100;0) 1 if ¿=5/¿=25 persons were killed in a given region-year	1995-2014	Croicu and Sundberg (2015)
Population	Continuous indicator of regional population	1995-2014	(CIESIN 2016)
Drought (end of rainseason)	SPI value of drought severity of the region’s rainy season	1995-2014	Tollefsen et al. (2012); Guttman (1999)
Drought (start of rainseason)	SPI value of drought severity during the first month of the region’s rainy season	1995-2014	Tollefsen et al. (2012); Guttman (1999)
Temperature	Mean temperature (in degrees Celsius) per region-year	1995-2014	Tollefsen et al. (2012); Fan and Van den Dool (2008)
Precipitation	Total amount of precipitation (in millimeter) per region-year	1995-2014	Tollefsen et al. (2012); Schneider et al. (2015)
Chinese Commodity	Chinese Commodity production (factor, standardized)	1999-2013	Dreher et al. (2017); Bluhm et al. (2020)
IDA Funding Position	“Bank‘s net investment portfolio & its non-negotiable, non-interest-bearing demand obligations (on account of members‘ subscriptions and contributions)” divided by “sum of the Bank‘s undisbursed commitments of development credits and grants.”	1995-2012	Dreher et al. (2017)
Elevation	Standard deviation of regional elevation as an indicator of ruggedness of terrain	Constant	USGS Global 30 Arc-Second Elevation (GTOPO30)
Ocean, Rivers, Lakes	Binary indicator of region’s presence of rivers, lakes or ocean	Constant	Natural Earth, from Natural Earth.com
Landarea	Area of a given region	Constant	Hijmans et al. (2010)
Travel Time (Mean)	Gives the mean regional estimate of the travel time to the nearest major city	Constant	Tollefsen et al. (2012); Uchida and Nelson (2009)
Borders	Binary indicator if a region borders another country	Constant	Own estimations based on Hijmans et al. (2010)

5-A2 Independent Variables (Development Aid)

WB's IDA & IBRD disbursements

For our analysis, we draw on the "WB IBRD-IDA, Level 1, Version 1.4.1" provided by the AidData consortium, which covers approved loans under the IBRD-IDA lending line between 1995 and 2014.[43] These data correspond to project aid disbursed from 5,684 projects in 61,243 locations. The data builds on the information provided by the WB, including the disbursement dates, project sectors, and disbursed values. These values are deflated to 2011 values. In an effort to allow for more fine-grained analysis of aid projects, AidData's coders filtered the location names from aid project documentation and assigned these to specific locations. Some projects include exact locations on latitude and longitude. Other projects, which had a more policy or regulation oriented purpose, could only be assigned to an administrative level (e.g., the first level of sub-national regions (provinces) or the second level (districts). To include as many disbursements as possible, but to be also able to grasp the advantages of geo-referenced data, we focus our analysis on these administrative levels. For our administrative boundaries, we build on the GADM dataset constructed by Hijmans et al. (2010). One difficulty with this data is that for some countries, including more populous nations like Armenia, more fine-grained administrative distinctions are missing. As the size of administrative regions is not fixed by size across countries, we assume in these cases that our ADM1 regions would be ADM2 regions.

Figure 5-A1 displays the development finance locations coded by donor, distinguishing all projects (precision 1-8), projects coded at least at the first administrative level (precision 1-4), projects coded at least at the second administrative level (precision 1-3) and projects coded more precise (precision 1-2).



(a)

Figure 5-A1 – No. of Project Locations by Precision Codes

One challenge arises in projects with a multitude of locations, where it is not possible to derive a distinct value of disbursements. In this regard, we suggest two solutions.

[43] As the number of documented projects declines steeply after 2012, we focus on the 1995-2012 period.

First, we allocate disbursements by the number of locations. In line with previous research by Dreher and Lohmann (2015), we assume that aid is distributed equally across locations and allocate aid proportionally to the locations per region. For instance, for a project with 10 locations, where 4 locations are in region A and 6 locations are in region B, 40% of project disbursements would be accounted in region A and 60% in region B.

Second, we calculate population-weighted disbursements. Here, we assume that aid is allocated based on the regional population shares. For instance, if a project would have project locations in two regions of a country, where two million inhabitants would reside in region A and three million would reside in region B, 40% of project disbursements would be accounted in region A and 60% in region B. Here, the aid attribution formula would write as follows: $Aid_{pijt} = \frac{Aid_{pit}}{\int Population_{pi}} * Population_{pj}$, where p is the project, i is the country, j is the region and t is the period for which we estimate the allocation shares.

Finally, our dataset comprises development finance from IBRD and IDA. However, only IDA disbursements classify as Official Development Assistance. For this purpose, disbursements are disentangled into IDA (development aid) and IBRD (development finance) disbursements.

Allocation scheme (more detailed)

Location weighting

The WB geocoded data release comes in the format of projects and several corresponding locations. For instance, a typical project report would mention the transaction amounts, the project purpose as well as different project locations. The latter can be classified in different degrees of precision (e.g., precision codes smaller than 4 correspond to locations that refer to an ADM2 region or even more precise, while precision code 4 corresponds to locations at the ADM1 level). When allocating the development aid across locations on the ADM1 and ADM2 level, we make the following assumptions based on a three-step procedure.[44] First, we subtract the share of development aid, which corresponds to locations, which are coded less precise than ADM1 (e.g., large geographic regions or aid at the country level). For example, if three out of 10 locations in a project are coded less precise than ADM1, further analysis focuses on the remaining 70% of development aid. Second, we then allocate all aid with precision codes 1-3 to the corresponding ADM2 regions. This is done by taking the location share (either by equal or population weights) of the transaction amount per location. A certain ADM2 region may have several locations per project or even several projects; we collapse our data by ADM2 region. Third, we then allocate all aid with precision code 4 to the corresponding ADM1 regions. This is done by taking the location share (either by equal or population weights) of the transaction amount per location. A certain ADM1 region may have several locations per project or even several projects, we collapse our data by ADM1 region. To allow for inference on the ADM2 level, we assume that transactions coded with precision 4 are attributable equally to all corresponding ADM2 regions. In practice, this is done by merging the ADM1 regions with all corresponding ADM2 regions

[44]Throughout the chapter, we allocate the aid either assuming equal weights per location or weighting each location by population.

and then splitting the aid with location or population weights. Finally, data with precision codes 1-3 and precision code 4 can be simply added upon the ADM2 level yielding our treatment variable of interest. For inference on the ADM1 level, totals of ADM2 level development assistance are created on the geounit-year level.

Population weighting

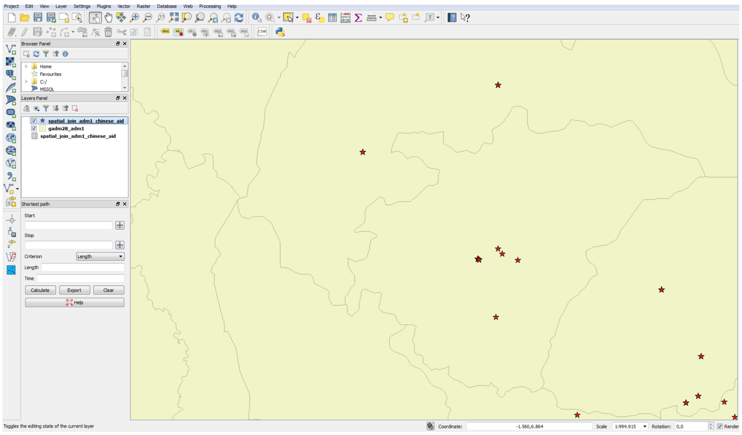
Analogous to the location weighted aid, we also distribute aid with population weights. Our population data are from the Center for International Earth Science Information Network (CIESIN) Columbia University (2016). However, some projects only consist of locations without population estimates (e.g., deserts). In this case, we assume a population of one citizen per location to be able to distribute those aid disbursements. We then consequently attribute the population of an ADM1 regions to project locations, which are coded at the ADM1 level (precision 4), and ADM2 populations to project locations, which are coded at least as precise as the ADM2 level (precision 1-3).

Similar to the location-weighing, we construct the total population of each project-year $pop_{project}$. For the projects coded with precision 4, we then attribute disbursements via the regional share in population pop_{ADM1}. This is then divided by $pop_{project}$ and multiplied with the project disbursements $TransactionValue_{proj}$ in each year: $ADM1Precision_4 = \frac{pop_{ADM1}}{pop_{proj}} * TransactionValue_{proj}$. As there may be several active projects per ADM1 region, we aggregate the disbursements on the ADM1 level. In order to break those numbers down to the ADM2 level, we merge all corresponding ADM2 regions to the ADM1 regions. We then divide the population in each ADM2 region by the population in each ADM1 region and multiply this share with the yearly disbursements per region, $ADM2Precision_4 = \frac{pop_{ADM2}}{pop_{ADM1}} * ADM1Precision_4$. For the precision codes 1-3 (at least coded as precise as the ADM2 level), we then attribute disbursements via the regional share in population divided by $pop_{project}$. This is then multiplied with the project disbursements in each year: $ADM2Precision_{123} = \frac{pop_{ADM2}}{pop_{proj}} * TransactionValue_{proj}$. As there may be several active projects per ADM2 region, we aggregate the disbursements on the ADM2 level. Finally, we merge the precision code 1-3 and 4 data on the ADM2 level to obtain our variables of interest. Those can then be aggregated on the ADM1 level.

Chinese Aid (ODA-like and OOF flows)

To create our data on the ADM2 and ADM1 level, we make use of the feature that aid can be defined on the ADM2 level and then aggregated to the ADM1 level. One challenge with the data is, however, that we lack information on the ADM2 regions for some countries (as there are no ADM2 regions in small countries). Therefore, we create two spatial joins of ADM1 and ADM2 regions from the GADM dataset with Chinese aid point features. This yields matches of the specific project locations with the administrative regions as depicted in Figure 5-A2.

To create our data, we first load our ADM2 data into Stata and drop the ADM0 and ADM1 identifiers to be later able to rely on the identifiers from the ADM1-Aid spatial join. The next step involves merging the ADM2-Aid spatial join with the ADM1-Aid spatial join by the target-fid, which uniquely identifies the points from the Dataset "aiddata_china_1_1_1.xlsx" by (Dreher et al., 2019) and Strange et al. (2017). Based on this data, we create unique identifiers for all ADM1 and ADM2



Notes: The figure depicts the locations of Chinese aid projects (stars) within administrative boundaries. Both information can then be spatially matched. Graphical depiction based on Quantum GIS.

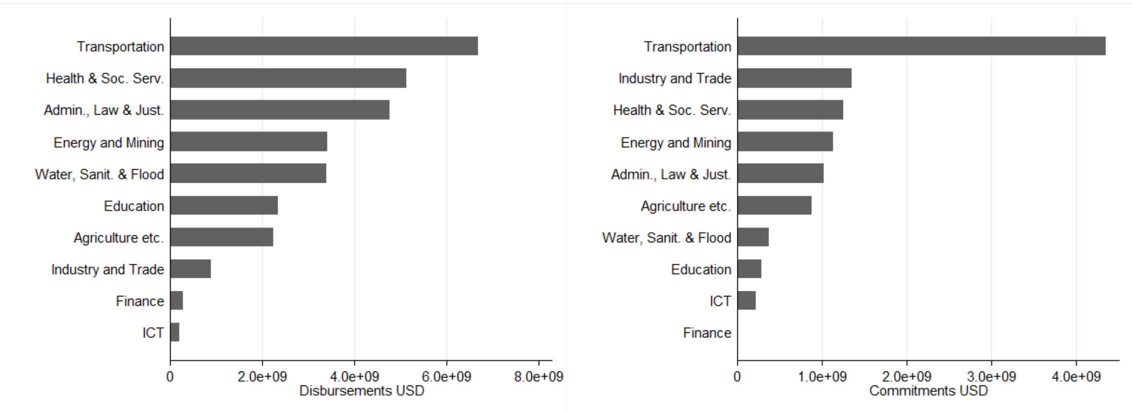
Figure 5-A2 – Chinese Aid ADM1 Spatial Join

regions, whereby we treat ADM1 regions as ADM2 regions in cases that ADM2 regions are missing (e.g., in Cape Verde). This assumption can be made as sizes of administrative regions are somewhat arbitrary and several ADM2 regions are larger than other countries' ADM1 regions. After getting the regional identifiers right, we can merge (a) the spatial joins of ADM regions & Chinese aid locations with (b) data on flows of Chinese aid. In a first step, we clean these data from entries that only relate to pledges of Chinese aid (information is from the variable status254). Although the data on Chinese finance to Africa also contain information on official investment, the focus of this chapter is on development aid. Thus, we focus on flows, which correspond to "ODA-like" funds as those would compare closest to development aid (following individual correspondence with the authors of Strange et al. (2017)). The data are then merged with population data from the gridded population of the world data to allocate financial flows with population weights in case one project had commitment locations in different administrative regions. Yet, one further challenge has to be resolved before allocating the commitments to regions. The Chinese aid commitments are coded like WB disbursements with different precision (e.g., some are coded only for geographic features. Such aid involve several administrative regions or are funds which go to central ministries or the government). For our commitment allocation, we only consider those projects, which are at least coded at the ADM1 level. This means that we proportionally exclude commitments, which provide information on the central level and sub-regional levels as indicated before. We furthermore distinguish between projects, which are coded only at the ADM1 level and ones that provide information on the ADM2 level (or more precise). The former are proportionally split over the underlying ADM2 regions. Although the latter can be precisely traced back to the ADM2 region, projects may have commitments in several ADM2 regions. In this case, we also split the commitments proportionally by locations or population, as indicated earlier.

To exploit sectoral variation in development finance both for the WB and China, we make use of the information provided by Strange et al. (2017) on Chinese aid's sectoral allocation using the OECD's Creditor Reporting System (CRS) codes. To achieve comparability with the broad sectors indicated for the WB, we assign sectors as follows: "Agriculture, Fishing and Forestry" (CRS-310:"Agriculture, Forestry and Fishing"), "Public Administration, Law and Justice" (CRS-150), "Information and communication" (CRS-220: "Communications"), "Education" (CRS-110: "Education"), "Finance" (CRS-240: "Banking and Financial Services"), "Health and other social services"(CRS-120: "Health," CRS-160: "Other Social infrastructure and services"), "Energy and mining" (CRS-230: "Energy Generation and Supply"), "Transportation" (CRS-210: "Transport and Storage"), "Water, sanitation and flood protection"(CRS-140: "Water Supply and Sanitation"), "Industry and Trade" (CRS-330: "Trade and Tourism," CRS-320: "Industry, Mining, Construction").

Sectoral distribution of aid disbursements

We use additional information on the financier for each disbursement for each project. Based on this information, we can construct sectoral distributions of aid flows. While both donors are investing heavily in transportation across Africa, further priorities differ. The WB supports Health and Social Services strongly, whereas China commits a large share of its funds to Industry & Trade.



(a) **(b)**

Figure 5-A3 – Sectoral Distribution of Aid: (a) WB's IDA; (b) China

5-A3 Dependent Variables (Conflict data)

Table 5-A4 provides an overview about the different conflict outcomes considered in this chapter. The construction of the data and sources are described in more detail in the subsequent paragraphs.

Table 5-A3 – Aid Allocation Formula Example

Example of Weighted Aid Allocation										
ID	Year	Aid Value	Loc. ID	ADM1 ID	ADM2 ID	Prec. Code	ADM1 Weight	Prec.4 Aid to ADM2	Prec. 1–3	Total Aid
	1995	100	2	1	1	1	1/7		14.29	14.29
1	1995	100	3	1	2	2	1/7		14.29	14.29
1	1995	100	4	2	1	4	1/7	14.29		14.29
1	1995	100	5	3	1	3	1/7		14.29	14.29
1	1995	100	6	3	2	1	1/7		14.29	14.29
1	1995	100	6	3	3	4	(1/7)*(1/3)	4.76		4.76
1	1995	100	6	3	1	4	(1/7)*(1/3)	4.76		4.76
1	1995	100	7	3	2	4	(1/7)*(1/3)	4.76		4.76
1	1995	100	8	4	1	4	1/7	14.29		14.29
Totals:								42.86	57.14	100.00

Table 5-A4 – Descriptive statistics - ADM1 Region

	Mean	SD	Min	Max
Conflict Incidence	11.65	32.08	0	100
State Based Conflict	7.01	25.54	0	100
Non-State Based Conflict	3.74	18.97	0	100
State Violence vs Civilians	1.83	13.39	0	100
Non-State Violence vs Civilians	3.41	18.14	0	100
Riots, Strikes & Demonstrations	13.59	34.27	0	100
Riots	8.08	27.26	0	100
Strikes	7.53	26.40	0	100
Demonstrations	2.92	16.83	0	100
Non-lethal Pro-GVMT Violence	1.16	10.71	0	100

Notes: Descriptive statistics for our main outcome variables. The sample period is 1995-2014 in order to account for the different lag structures. Click here to go back to section 5-3.2.

UCDP Data

AidData and UCDP use the same coding framework, so we can use similar coding rules and restrict us to events coded at least at the ADM1 level (precision codes 1-4). For the more precise data (precision codes 1 and 2), we again use a point to polygon analysis on the ADM level. As one conflict event is always coded in one discernible location (Croicu and Sundberg, 2015), we do not need to make additional distributional assumptions by location number or population size for conflict data, because we do not face issues of multiple project locations, which we had in the aid data. Yet, for conflict observations on the ADM1 level (precision code 4), we do not distribute battle-related deaths by population weights across ADM2 regions.

A useful feature of the UCDP data is the possibility to discern three different types of violence. Those are the government against organized groups (type 1), organized non-governmental groups versus the government (or against another non-governmental group) (type 2), and one-sided violence by the government against civilians (type 3 governmental) and by non-governmental groups against civilians (type 3 non-governmental).[45] UCDP data can be considered as comprehensive for our 1995 to 2012 sample. Hence, all missing values are treated as zeros. For Syria, information on battle-related deaths are not reported and is not part of our analysis.

SCAD data

UCDP data focus on organized violence with lethal outcomes. However, along with the different theories, it could be hypothesized that discontent and aid appropriation do not necessarily need to be linked to full-fledged conflict. What is more, recent empirical work by Bluhm et al. (2021) underscores the role of aid in conflict dynamics. Thus, we also consider social conflict as a further outcome, in terms of demonstrations and repressions, based on the Social Conflict Analysis Database (Salehyan et al., 2012). SCAD involves demonstrations, riots, strikes, coups, pro-, anti- and extra-government violence, which can, but do not necessarily have to involve casualties. In this way, SCAD complements the UCDP data.[46] SCAD mainly builds on data compiled by the Lexis-Nexis services from searches of Agence France Presse and Associated Press. Based on the available information, data are geo-referenced by web searches of the locations mentioned in the event reports. Analogous to UCDP data, precision codes are provided, which are used to allocate events similarly.

Matching EPR to GREG

To measure ethnic homelands, we use the GREG dataset (Weidmann et al., 2010). It is a geo-referenced version of the initial locations of ethnic homelands based on the Soviet Atlas Narodov Mira. The information about the power status comes from the time-variant Ethnic Power Relations (EPR) dataset (Vogt et al., 2015). Wherever possible, we match the group power status from EPR in a particular year to one of the time-invariant GREG group homelands. The original dataset assigns

[45] For a more detailed description of the different types of violence, please consult Croicu and Sundberg (2015).

[46] Prior to 2014 armed conflict was not included in SCAD data and is now also distinguished from "social disturbances."

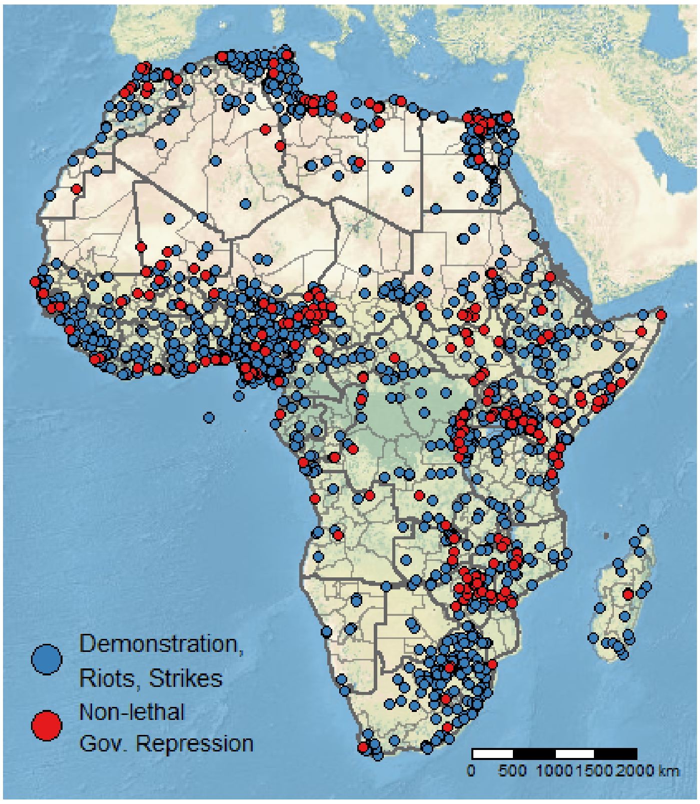


Figure 5-A4 – SCAD Data for precision codes 1-4

eight different power statuses to groups. The differences are sometimes marginal and hard to interpret. To minimize measurement error we only use the more precise information on whether a group was part of the governing coalition or not. We then intersect the ethnic group polygons with the administrative regions to classify regions as one of the three categories.

5-A4 World Bank Aid in the Financial Sector

A more profound classification exercise on the World Bank's financial sector aid reveals that sectoral reforms may play a crucial part in mitigating conflict. To classify IDA projects targeted at the financial sector, we select projects with at least 10% disbursements in the recipient's financial sector. Moreover, we restrict the classification to projects with traceable money flow to ADM1 regions. Finally, we obtain the reports for each project and develop a classification of IDA aid in the financial sector based on the project goals and descriptions.

Table 5-A5 shows that 50% of all financial sector aid are aimed at sectoral reforms. These projects support existing government reforms and development but mainly include new projects launched outside the government's initiative.

5-A5 Afrobarometer

Measures on people's norms about democracy are taken from Afrobarometer Data (2018). The geo-coded individual responses are matched with the administrative region and the response values to the respective questions are averaged on the first administrative level to allow a matching with regional aid flows.

Class.	Classification Name	Share of Projects	Description
I.	Support services to enterprises	15%	Financial and non-financial support to (selected) enterprises or enterprise sectors
II.	Support services to NGOs	2.5%	Financial and non-financial support to NGOs or welfare organisations
III.	Support services to individuals or groups	15%	Financial and non-financial support to individuals, socio-economic or geographical groups
IV.	Capacity building	10%	Capacity building in socio-economic or geographical groups or supporting other capacity building projects
V.	Sectoral reforms	50%	New projects or support of existing government efforts that primarily target sectoral adjustment and reforms
VI.	Environmental Protection	2.5%	Projects aimed at protecting or improving the environment or wildlife
VII.	Emergency support	2.5%	Projects providing emergency support
VIII.	Research support	2.5%	Research or evaluation focused projects

Specific project examples		
Class.	Project Number	Project goals
I.	P083082 Micro, Small and Medium Enterprise Project, Nigeria	Increase performance and employment levels of micro, small and medium enterprises in selected non-oil industry sub-sectors + 3 targeted states of the country through i.) Improving access to financial services, ii.) Developing the market for business development services, iii.) Development of business climate etc. **http://documents.worldbank.org/curated/en/333691474574170700/pdf/000020051-20140625225024.pdf**
III.	P052186 Microfinance Project, Madagascar	Improve income and living standards of low-income Malagasy by i.) Establishing appropriate legal, regulatory and supervisory framework for microfinance, ii.) Expanding micro-financial skills and iii.) Developing strong and sustainable local institutions. **http://documents.worldbank.org/curated/en/933341474899762755/pdf/000020051-20140625070634.pdf**
V.	P035620 Financial Institutions Development Project, Tanzania	i.) Restructuring and privatizing the National Bank of Commerce and restructuring the smaller People's Bank of Zanzibar for competition and efficiency in the banking sector, ii.) Continuation of strengthening of Bank Supervision Directorate, iii.) Improving payments system, iv.) Creating a private credit information bureau, v.) Developing the insurance industry and capital markets. **http://documents.worldbank.org/curated/en/899741468311395554/pdf/multi-page.pdf**

Table 5-A5 – World Bank Aid in the Financial Sector

Table 5-A6 – Afrobarometer - Labels, questions and sources

Variable Name	Variable Description	Availability	Code
Panel A: Security			
Security facilities: Police station present within walking distance?	Are the following facilities present in the primary sampling unit/enumeration area, or within easy walking distance: Police station?	2008-2009, 2011-2014	ea-fac-c
Security forces: Any policemen or police vehicles?	Are the following facilities present in the primary sampling unit/enumeration area, or within easy walking distance: Police station?	2008-2009, 2011-2014	ea-sec-a
Security forces: Any soldiers or army vehicles?	In the PSU/EA, did you (or any of your colleagues) see: Any soldiers or army vehicles?	2008-2009, 2011-2014	ea-sec-b
Frequency of things stolen in the past year?	During the past year, have you or anyone in your family: Had something stolen from your house?	2002-2006, 2008-2009, 2011-2014	q11a-x
Frequency of physical attacks in the past year?	During the past year, have you or anyone in your family: Been physically attacked?	2002-2006, 2008-2009, 2011-2014	q11b-x
Panel B: Democratic norms and attitudes			
Democracy: How democratic is your country today?	In your opinion how much of a democracy is your country today?	1999-2006, 2008-2009, 2011-2014	q40
Democracy: Did you perceive last elections as free and fair?	On the whole, how would you rate the freeness and fairness of the last national election, held in your country?	1999-2001, 2005-2006, 2008-2009, 2011-2014	q22-x
Governance: Reject one-party rule	There are many ways to govern a country. Would you disapprove or approve of the following alternatives: Only one political party is allowed to stand for election and hold office?	1999-2006, 2008-2009, 2011-2014	q28a
Governance: Reject military rule	There are many ways to govern a country. Would you disapprove or approve of the following alternatives: The army comes in to govern the country?	1999-2006, 2008-2009, 2011-2014	q28b
Governance: Reject one-man rule	There are many ways to govern a country. Would you disapprove or approve of the following alternatives: Elections and Parliament are abolished so that the president can decide everything?	1999-2006, 2008-2009, 2011-2014	q28c
Reject government banning organizations that go against its policies	Which of the following statements is closest to your view? Choose Statement 1 or Statement 2. Statement 1: Government should be able to ban any organization that goes against its policies. Statement 2: We should be able to join any organization, whether or not the government approves of it.	2005-2006, 2008-2009, 2011-2014	q16-x
Panel C: Government responsiveness and repression			
Frequency of contact to government official to express your view	During the past year, how often have you contacted any of the following persons about some important problem or to give them your views: An official of a government agency?	1999-2006, 2008-2009, 2011-2014	q24c-x
Fear of political intimidation or violence during campaigns	During election campaigns in this country, how much do you personally fear becoming a victim of political intimidation or violence?	2008-2009, 2011-2014	q49-x
How often do people have to be careful about what they say in politics?	In your opinion, how often, in this country: do people have to be careful of what they say about politics?	2002-2006, 2008-2009, 2011-2014	q51a-x
Rule of Law: People must obey the law	For each of the following statements, please tell me whether you disagree or agree: The police always have the right to make people obey the law.	2002-2006, 2008-2009, 2011-2014	q42b
Frequency of joining others to request government action	Here is a list of actions that people sometimes take as citizens when they are dissatisfied with government performance. For each of these, please tell me whether you, personally, have done any of these things during the past year. If not, would you do this if you had the chance: Joined others in your community to request action from government.	2014	q27a

5-9 Analytical Appendix

5-B1 Instrumental Variable

Motivation of Instrumental Variable

To reduce the risk of the instrument being subject to spurious trends and correlations, we need to understand the underlying mechanisms. This section is dedicated to providing a more detailed description. In a first step, Table 5-B2 shows OLS correlations of our conflict measure with one lead term and three lag terms of aid. The second lead of Chinese aid is correlated with conflict, suggesting China selects into post-conflict settings. This correlation may also correspond to a geographically more selective allocation of Chinese funds as described in Figure 5-2a. We also test more formally if the instrument is suitable to tackle the selection bias, by regressing conflict on an instrumented lead term and find no significant relationship in Table 5-B1. The instrumental variable approach is, thus, warranted to reduce selection bias.

Table 5-B3 suggest that the instrumental variables for both donors affect the extensive margin (e.g., the probability to have at least one active aid project in a given region-year). Table 5-B4, in turn, indicates that for the WB the intensive margin matters as well (e.g., provided that at least one active aid project, how much funds does a region receive?).

Table 5-B6 depicts the reduced form estimates. In line with the main results, both interacted instruments are not significantly correlated with lethal conflict outcomes at the regional level.[47] For transparency, Table 5-B5 displays the first stage including the constituent probability term, which, however, is not an instrument itself as we control for it in the second stage (see Section 4).

[47]While the constituent probability term enters significantly, it is not part of the instrument, and we control for it in the second stage.

Table 5-A7 – Afrobarometer - Questionnaire rounds and countries

	Round 1	Round 2	Round 3	Round 4	Round 5	Round 6
Algeria	–	–	–	–	2013	2015
Benin	–	–	2005	2008	2011	2014
Botswana	1999	2003	2005	2008	2012	2014
BurkinaFaso	–	–	–	2008	2012	2015
Burundi	–	–	–	–	2012	2014
Cameroon	–	–	–	–	2013	2015
Cape Verde	–	2002	2005	2008	2011	2014
Cote d'Ivoire	–	–	–	–	2013	2014
Egypt	–	–	–	–	2013	2015
Ethiopia	–	–	–	–	2013	–
Gabon	–	–	–	–	–	2015
Ghana	1999	2002	2005	2008	2012	2014
Guinea	–	–	–	–	2013	2015
Kenya	–	2003	2005	2008	2011	2014
Lesotho	2000	2003	2005	2008	2012	2014
Liberia	–	–	–	2008	2012	2015
Madagascar	–	–	2005	2008	2013	2015
Malawi	1999	2003	2005	2008	2012	2014
Mali	2001	2002	2005	2008	2013	2014
Mauritius	–	–	–	–	2012	2014
Morocco	–	–	–	–	2013	2015
Mozambique	–	2002	2005	2008	2012	2015
Namibia	1999	2003	2006	2008	2012	2014
Niger	–	–	–	–	2013	2015
Nigeria	2000	2003	2005	2008	2013	2015
Sao Tome/ Principe	–	–	–	–	–	2015
Senegal	–	2002	2005	2008	2013	2014
Sierra Leone	–	–	–	–	2012	2015
South Africa	2000	2002	2006	2008	2011	2015
Sudan	–	–	–	–	2013	2015
Swaziland	–	–	–	–	2013	2015
Tanzania	2001	2003	2005	2008	2012	2014
Togo	–	–	–	–	2012	2014
Tunisia	–	–	–	–	2013	2015
Uganda	2000	2002	2005	2008	2012	2015
Zambia	1999	2003	2005	2009	2013	2014
Zimbabwe	1999	2004	2005	2009	2012	2014

Source: Afrobarometer Data (2018)

Table 5-B1 – ADM1 - Absence of Pre-Trends with IV. Regression with Instrumented Lead of Aid

	(1)	(2)
Panel A: WB Aid		
Placebo (Lead): World Bank		
$\ln(World\,Bank\,Aid_{\,\mathrm{t+1}})$	0.2299	0.2332
	(0.3586)	(0.3704)
N	12325	12325
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	99.481	86.444
Panel B: Chinese Aid		
Placebo (Lead): China		
$\ln(Chinese\,Aid_{\,\mathrm{t+1}})$	0.0396	-0.3753
	(0.2888)	(0.3351)
N	8700	8700
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	34.263	29.941
Exogeneous Controls	Yes	Yes
Exogeneous Controls $\times$ Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country $\times$ Year FE	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.6.

Interpretation: The lead terms indicate that our instrumental variable method suceeds in addressing the identified selection bias of Table 5-B2.

Table 5-B2 – ADM1 - Leads and further Lags

	(1)	(2)
Panel A: WB Aid		
Leads and Lags: World Bank		
$\ln(World\,Bank\,Aid_{t+1})$	-0.0059	0.1559
	(0.1236)	(0.1124)
$\ln(World\,Bank\,Aid_{t})$	-0.1089	-0.2128**
	(0.1047)	(0.0984)
$\ln(World\,Bank\,Aid_{t-1})$	0.0214	-0.0933
	(0.0893)	(0.0900)
$\ln(World\,Bank\,Aid_{t-2})$	0.0516	0.1424
	(0.0876)	(0.1015)
$\ln(World\,Bank\,Aid_{t-3})$	-0.0811	-0.0535
	(0.0878)	(0.1000)
N	10150	10150
Panel B: Chinese Aid		
Lead and Lag: China		
$\ln(Chinese\,Aid_{t+1})$	0.1681	0.2083*
	(0.1239)	(0.1239)
$\ln(Chinese\,Aid_{t})$	-0.0127	0.0231
	(0.1263)	(0.1367)
$\ln(Chinese\,Aid_{t-1})$	-0.0086	-0.0481
	(0.1518)	(0.1562)
$\ln(Chinese\,Aid_{t-2})$	0.0121	-0.0506
	(0.1156)	(0.1285)
$\ln(Chinese\,Aid_{t-3})$	0.0572	-0.0308
	(0.0978)	(0.1117)
N	6525	6525
Exogeneous Controls	Yes	Yes
Exogeneous Controls × Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country × Year FE	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.2.
Interpretation: The lead terms of the table indicate that World Bank aid does not exhibit a selection effect, while China seem to select into regions more likely to experience conflict in the future. Table 5-B1 indicates that our instrumental variable approach succeeds in reducing this selection bias.

Table 5-B3 – ADM1 IV (First Stage - Extensive Margin (Likelihood of at least one active project))

	(1)	(2)
Panel A: WB Aid		
IV FS Extensive Margin: IDA Position		
$IDA\,Position_{\,\text{t-1}} \times \text{Cum. Prob}_{\,\text{t-2}}$	4.0782***	4.8249***
	(0.4140)	(0.5238)
N	12325	12325
Panel B: Chinese Aid		
IV FS Extensive Margin: Chinese Commodity		
$Chinese\,Commodity_{\,\text{t-3}} \times \text{Cum. Prob}_{\,\text{t-3}}$	-0.7267***	-0.6591***
	(0.1205)	(0.1163)
N	7975	7975
Exogeneous Controls	Yes	Yes
Exogeneous Controls × Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country-Year FE	No	Yes

Notes: The table displays regression coefficients the first stage of the IV regression, when instead of the aid amount a binary indicator of aid receipts is used. The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. The constituent term of the probability is depicted in the appendix. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-5.2.

Interpretation: The table shows differences in how both donor allocate aid to regions. An increase in the IDA position increases a region's probability to receive more aid projects if the region already received had a WB project in the past. On the contrary, an increase in Chinese overall aid linked to commodity (over-)production increases the region's probability to receive aid more for regions that did not receive aid in the past. This is in line with China's strategic aim of expanding to new regions.

Table 5-B4 – ADM1 IV (First Stage - Intensive Margin)

	(1)	(2)
Panel A: WB Aid		
IDA Position$_{\text{t-1}}\times$ Cum. Prob$_{\text{t-2}}$	4.4155	8.5243**
	(3.3348)	(3.7926)
N	7091	7081
Country-Year FE	No	Yes
Regional Time Trend	Yes	Yes
Country Time Trend:	Yes	Yes
$CountryTimeTrend^2$:	Yes	Yes
Panel B: Chinese Aid:		
Chinese Commodity$_{\text{t-3}}\times$ Cum. Prob$_{\text{t-3}}$	-0.6974	0.0592
	(1.5012)	(2.3391)
N	232	232
Country-Time Trends	No	Yes

Notes: The table displays regression coefficients the first stage of the IV regression, when constraining the sample only on recipient regions. The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. All regressions include exogenous controls, region fixed effects and year fixed effects. Country-Year fixed effects and more rigid time trends are not included for Chinese Aid due to the more limited variation. The constituent term of the probability is depicted in the appendix. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-5.2.
Interpretation: For the World Bank, the first stage effect seems partly also related to the intensive margin, i.e. the expansion of existing projects. That is, regions that already received aid in the past are likely to larger amounts of aid if more additional funds are available. For China, there is no evidence in favor of a change at the intensive margin. The first stage effect seems to be driven by extensive margin changes.

Table 5-B5 – ADM1 IV (First Stage with probability constituent term)

Panel A: WB Aid	(1)	(2)
IV First stage: World Bank		
$IDA\,Position_{\text{ t-1}} \times \text{Cum. Prob}_{\text{ t-2}}$	70.9363***	80.8832***
	(7.1065)	(8.6854)
$Cum.\,Prob_{\text{ t-2}}$	-72.7723***	-82.0994***
	(7.7291)	(9.2698)
N	12325	12325
Panel B: Chinese Aid		
IV First stage: China		
$Chinese\,Commodity_{\text{ t-3}} \times \text{Cum. Prob}_{\text{ t-3}}$	-14.0193***	-12.6964***
	(2.3180)	(2.2734)
$Cum.\,Prob_{\text{ t-3}}$	-43.8804***	-39.5225***
	(4.7041)	(4.4175)
N	7975	7975
Exogeneous Controls	Yes	Yes
Exogeneous Controls × Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country-Year FE	No	Yes

Notes: The table displays regression coefficients the first stage of the IV regression, displaying additionally the constituent term of the probability, which was not shown in Table 5-4. This table display the constituent term for completeness. The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.6.

Table 5-B6 – ADM1 Reduced Form

	(1)	(2)
Panel A: WB Aid		
Reduced Form: IDA Position		
$Cum. Prob_{\text{t-2}}$	10.8281	19.2994
	(27.3795)	(33.4583)
$IDA\,Position_{\text{t-1}} \times \text{Cum. Prob}_{\text{t-2}}$	-7.1921	-18.2132
	(26.5498)	(33.5818)
N	12325	12325
Panel B: Chinese Aid		
Reduced Form: Chinese Commodity		
$Cum. Prob_{\text{t-3}}$	-8.1658	-14.3840
	(9.7637)	(10.2361)
$Commodity,_{\text{factor1}} \times \text{Cum. Prob}_{\text{t-3}}$	6.6166	5.1407
	(6.6138)	(7.1640)
N	7250	7250
Exogeneous Controls	Yes	Yes
Exogeneous Controls × Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country × Year FE	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.6.

Interpretation: Both interacted instruments are not significantly correlated with the conflict outcome and are in line with the main results in Table 5-4.

Robustness of Instrumental Variable

The main specification uses the rolling average of the WB's IDA position (e.g., averaging across t and $t-1$) because the Bank's fiscal year ends already in June. For robustness, Table 5-B7 depicts instrumental variable results using only the variation in $t-1$. The results are largely unchanged.

Moreover, there are several degrees of freedom regarding the definition of the interacted probability term. We indicate the robustness of an insignificant conflict-aid link when dropping the first year of the respective panel (starting at 1998 for the WB's IDA, and 2003 for Chinese Commodity Production) Table 5-B10 or using an interacted instrument based on an initial probability from the first three sampling years (1995 to 1997 for the WB's IDA; 2000 to 2002 for Chinese Commodities) in Table 5-B11.

Finally, first stage results may be susceptible to a small share of very influential observations. Table 5-B12 indicates that results are qualitatively unchanged if we exclude the ten high leverage region-years from the sample. Figures 5-B1 and 5-B2 display the first stage relationship leaving out single countries, suggesting that there are no individual states driving the relationship.

A (non-)linear trend in our outcome, treatment and instrumental variable may render the panels non-stationary and lead to spurious findings. The Hadri test assesses the null hypothesis "All Panels are (trend) stationary". Table 5-B8 indicates that there are at least some panels being non-stationary and may include a trend. For this reason, we correct for the non-stationarity and take first differences of outcome, treatment and instrumental variables. Results in Table 5-B9 remain robust and support the main findings.

Table 5-B7 – ADM1 IV (IDA-Position$_{t-1}$)

	(1)	(2)
Panel A: WB Aid		
IV Second Stage: World Bank (t-1)		
$\ln(World\,Bank\,Aid_{\,\text{t-1}})$	-0.1294	-0.0251
	(0.3976)	(0.3868)
IV FS: IDA Position (t-1)		
$IDA\,Position_{\,\text{t-1}} \times \text{Cum. Prob}_{\,\text{t-2}}$	51.3655***	65.1984***
	(5.6627)	(6.9103)
N	12325	12325
Exogeneous Controls	Yes	Yes
Exogeneous Controls × Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country-Year FE	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. Instead of a running sum of IDA funding position in "t" and "t-1" only the variation in "t-1" is used. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-5.2.

Interpretation: The instrumental variable is nearly unaltered even if changing the sample: Here, probabilities are not based on a rolling average but based on the last year's IDA position, $t - 1$.

Table 5-B8 – Test for (Trend) Stationarity - Hadri type

	(1)	(2)
ln(World Bank Aid$_t$)	125.8488***	89.4980***
	(0.0000)	(0.0000)
ln(Chinese Aid$_t$)	2.9868***	1.1684
	(0.0014)	(0.1213)
IDA Position$_{t-1}\times$ Cum. Prob$_{t-2}$	145.3093***	121.1980***
	(0.0000)	(0.0000)
Chinese Commodity$_{t-3}\times$ Cum. Prob$_{t-3}$	98.1532***	65.5592***
	(0.0000)	(0.0000)
Conflict	56.8260***	23.5170***
	(0.0000)	(0.0000)
Linear Trend	No	Yes

Notes: Conflict refers to category 1 binary conflict indicator (100 if BRD≥5, 0 if BRD¡5). Hadri type test coefficient estimates for the five variables indicated in rows. p-values in columns refer to the null hypothesis ”All panels are (trend) stationary.” * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.6.

Interpretation: The test shows that (trend) stationarity is rejected, hence robustness tests under a weak dependence assumption is conducted in Table 5-B9.

Table 5-B9 – ADM1 IV (First Difference WB & Chinese aid)

	(1)	(2)
Panel A: WB Aid		
IV Second Stage: World Bank		
L.d1lnaid	0.7606	0.2460
	(1.1439)	(1.2420)
N	12325	12325
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	64.200	35.064
IV First stage: World Bank		
$IDA\,Position_{\text{(t-1)-(t-2)}} \times \text{Cum. Prob}_{\text{t-2}}$	18.7864***	29.3949***
	(2.3424)	(4.9589)
Panel B: Chinese Aid		
IV First stage: China		
L2.d1lnaid_c	-0.3690	-0.5025
	(0.4856)	(0.6253)
N	7975	7975
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	20.972	13.774
First Stage: Chinese Commodity		
$Commodity,_{\text{(t-3)-(t-4)}} \times \text{Cum. Prob}_{\text{t-3}}$	-13.5621***	-10.5846***
	(2.9568)	(2.8471)
Exogeneous Controls	Yes	Yes
Exogeneous Controls × Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country-Year FE	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. The constituent term of the probability is depicted in the appendix. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-5.2 or section 5-6.6.
Interpretation: The Hadri test in Table 5-B8 shows that despite a certain degree of time persistence, the results of the first stages using first differences are nearly unchanged under the assumption of weakly dependent time series.

Table 5-B10 – ADM1 IV (Without first year)

	(1)	(2)
Panel A: WB Aid		
IV Second Stage: World Bank ln($World\,Bank\,Aid_{\text{t-1}}$)	-0.2904	-0.2681
	(0.4172)	(0.3975)
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	80.438	78.004
	68.5810***	88.1297***
	(7.6467)	(9.9784)
N	11600	11600
Panel B: Chinese Aid		
IV Second Stage: China		
ln($Chinese\,Aid_{\text{t-2}}$)	-0.5634	-0.5104
	(0.5786)	(0.7241)
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	22.620	16.927
IV First stage: China		
	-11.7436***	-10.0728***
	(2.4692)	(2.4483)
N	7250	7250
Exogeneous Controls	Yes	Yes
Exogeneous Controls × Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country-Year FE	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The constituent term of the probability is depicted in the appendix. Click here to go back to section 5-6.6.
Interpretation: The instrumental variable is nearly unchanged when dropping the first year. This accounts for a potentially overly high leverage of the first year in influencing the cross-sectional probability terms.

Table 5-B11 – ADM1 IV (Initial Probability)

	(1)	(2)
Panel A: WB Aid		
IV Second Stage: World Bank		
$\ln(World\,Bank\,Aid_{\text{t-1}})$	0.2253	-0.3389
	(0.7469)	(0.6206)
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	27.090	26.027
IV First stage: World Bank		
$IDA\,Position_{\text{t-1}} \times \text{Con. Prob}_{98}$	43.4391***	61.1537***
	(8.3419)	(11.9769)
N	11600	11600
Panel B: Chinese Aid		
IV Second Stage: China		
$\ln(Chinese\,Aid_{\text{t-2}})$	-1.4689	-1.2846
	(1.3446)	(1.4723)
Kleibergen-Paap underidentification test p-value	0.001	0.002
Kleibergen-Paap weak identification F-statistic	13.035	9.925
IV First stage: China		
$Chinese\,Commodity_{\text{t-3}} \times \text{Con. Prob}_{03}$	-5.8046***	-5.7207***
	(1.6061)	(1.8130)
N	7250	7250
Exogeneous Controls	Yes	Yes
Exogeneous Controls × Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country-Year FE	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. The probability is based on the third year in the corresponding sample (1998 for the WB's IDA; 2003 for Chinese Commodities) and held thereafter constant. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.6.

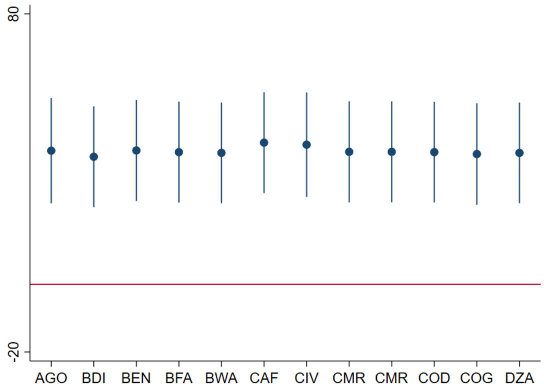
Interpretation: An alternative to our cumulative, updated probability is a constant probability. Computing this over the whole sample period is potentially problematic, but we can exclude the first third of the sample to compute a constant, but pre-determined probability. When doing this, the signs of the instrumental variable in the first stage remain unchanged, they are only smaller in magnitude.

Table 5-B12 – ADM1 IV (Without high leverage region)

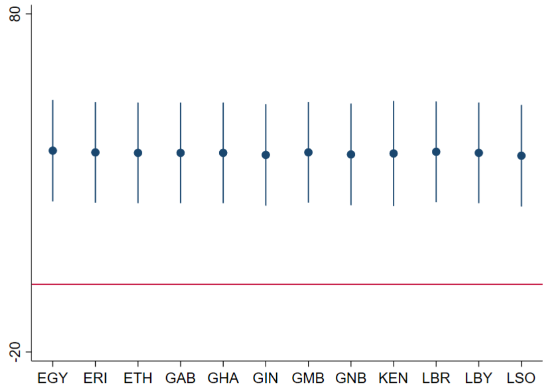
	(1)	(2)
Panel A: WB Aid		
IV Second Stage: World Bank		
$\ln(World\,Bank\,Aid_{\text{t-1}})$	-0.0990	-0.2268
	(0.3761)	(0.4197)
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	99.363	86.752
IV First stage: World Bank		
$IDA\,Position_{\text{t-1}} \times \text{Cum. Prob}_{\text{t-2}}$	70.8414***	80.8936***
	(7.1068)	(8.6851)
N	12317	12291
Panel B: Chinese Aid		
IV Second Stage: China		
$\ln(Chinese\,Aid_{\text{t-2}})$	-0.2592	-0.1934
	(0.4281)	(0.5251)
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	36.571	31.181
IV First stage: China		
$Chinese\,Commodity_{\text{t-3}} \times \text{Cum. Prob}_{\text{t-3}}$	-14.0197***	-12.6973***
	(2.3183)	(2.2739)
N	7974	7974
Exogeneous Controls	Yes	Yes
Exogeneous Controls × Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country-Year FE	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.6.

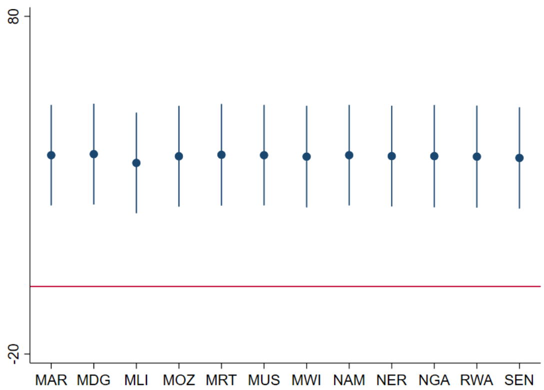
Interpretation: One concern is that the predictive power of the instrumental variables is driven by a few regions that received a lot of aid in the past. The table shows that when dropping the 10 region-year observations receiving most aid our results still hold.



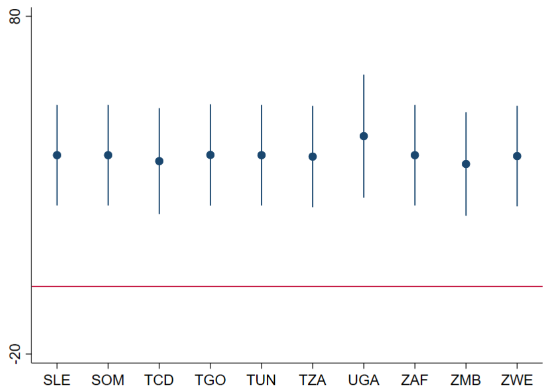
(a) Leave one out (i)



(b) Leave one out (ii)



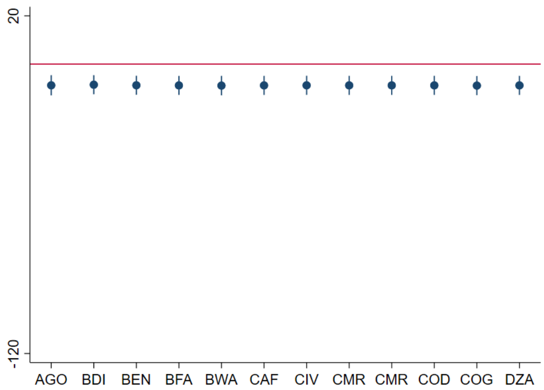
(c) Leave one out (iii)



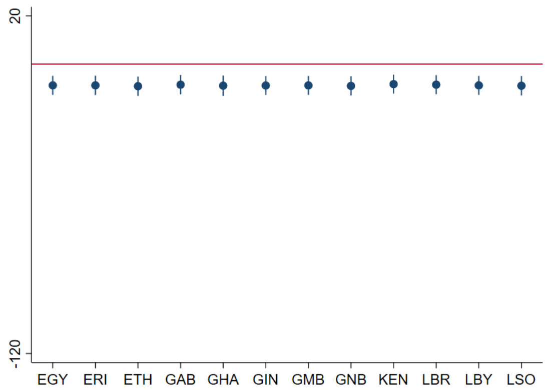
(d) Leave one out (iv)

Figure 5-B1 – Robustness of first stage for World Bank Aid - Leaving one country out

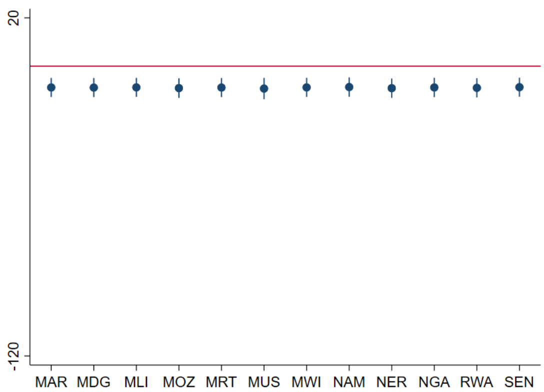
Note: Results depict coefficients of the instrumental variable $probability_{i,c,t-2} \times IDAPosition_{t-1}$ for different regressions leaving one country out from the estimation. Labels in the graph refer to ISO codes of recipients. Click here to go back to section 5-6.6.
Interpretation: To rule out that one particular country drives the results, we run a series of regressions, each leaving out one country. The graph shows that the results are not affected when doing that for any particular country.



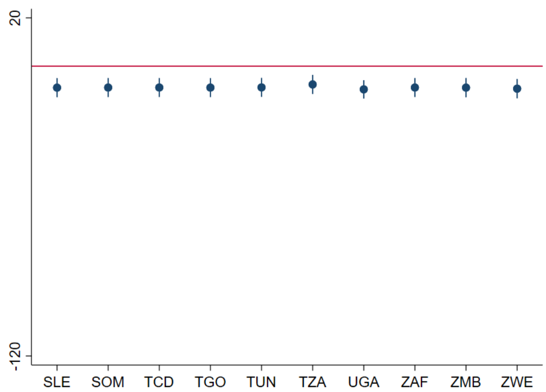
(a) Leave one out (i)



(b) Leave one out (ii)



(c) Leave one out (iii)



(d) Leave one out (iv)

Figure 5-B2 – Robustness of first stage for Chinese Aid - Leaving one country out

Note: Results depict coefficients of the instrumental variable $probability_{i,c,t-3} \times ln(Chinese\,Commodity_{t-3})$ for different regressions leaving one country out from the estimation. Labels in the graph refer to ISO codes of recipients. Click here to go back to section 5-6.6.
Interpretation: To rule out that one particular country drives the results, we run a series of regressions, each leaving out one country. The graph shows that the results are not affected when doing that for any particular country.

Table 5-B13 – ADM1 IV (WB - Global Time Series)

	Glob. Conflict	Glob. Conflict
Panel A: WB Second Stage		
ln(World Bank Aid$_{t-1}$)	-0.1484	-0.0605
	(0.3637)	(0.3788)
N	12325	12325
Kleibergen-Paap under-ID p-val.	0.000	0.000
Kleibergen-Paap weak ID F-stat	99.771	99.659
Panel B: WB First Stage		
IDA Position$_{t-1}\times$ Cum. Prob$_{t-2}$	76.5677***	95.7504***
	(7.6586)	(9.5846)
ln(Global BRD$_{t-1}$)$\times$Cum. Prob$_{t-2}$	-0.9489	-2.9929***
	(0.5929)	(1.0281)
N	12325	12325
Exogeneous Controls	Yes	Yes
Exogeneous Controls $\times$ Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country-Year FE	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. All regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. Click here to go back to section 5-6.6. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Interpretation: One potential concern is that the time-varying WB liquidity and overall amount of WB aid react to overall global conflicts in a way that affects low and high probability regions differently. The table shows that controling for an interaction of global battle-related deaths with the probability to receive aid neither changes the first nor second stage coefficients to a noticeable degree.

Table 5-B14 – ADM1 IV (China - Global Time Series)

	Glob. Conflict	Glob. Conflict
Panel A: China Second Stage		
ln(Chinese Commodity$_{t\text{-}2}$)	-0.2070	-0.1037
	(0.4766)	(0.5948)
N	7975	7975
Kleibergen-Paap under-ID p-val.	0.000	0.000
Kleibergen-Paap weak ID F-stat	31.432	26.756
Panel B: China First Stage		
Chinese Commodity$_{t\text{-}3}\times$ Cum. Prob$_{t\text{-}3}$	-13.0574***	-11.8020***
	(2.3254)	(2.2784)
ln(Global BRD$_{t\text{-}3}$)$\times$ Cum. Prob$_{t\text{-}3}$	2.6516	2.1932
	(2.3520)	(2.3782)
N	7975	7975
Exogeneous Controls	Yes	Yes
Exogeneous Controls $\times$ Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country-Year FE	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD$\geq$5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. All regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. Click here to go back to section 5-6.6. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Interpretation: One potential concern is that the time-varying commodity (over-)production and overall amount of Chinese aid react to overall global conflicts in a way that affects low and high probability regions differently. The table shows that controling for an interaction of global battle-related deaths with the probability to receive aid neither changes the first nor second stage coefficients to a noticeable degree.

5-B2 Alternative Outcome Variables

Robustness of results on lethal violence (UCDP measures)

As thresholds of five battle-related deaths or one incidence per region-year are arbitrary, we depict for robustness also other intensity thresholds. First, aid could matter for rather more intense conflicts in line with the evidence on conflict dynamics made by (Bluhm et al., 2020). Tables 5-B15 (OLS) and 5-B16 (IV) indicate for a higher threshold of 25 battle-related deaths mainly insignificant coefficients, which also remain negative for the few significant OLS results. While the IV specifications indicate medium-sized negative for the WB and small positive coefficients for China, both stay insignificant. Second, this also holds in Tables 5-B17 (OLS) and 5-B18 (IV) when using a continuous measure of logarithmized battle-related deaths.

Robustness of results on non-lethal violence (SCAD)

The measurement of conflict is non-trivial and in this respect, we display in the main part beyond lethal violence measures of social conflict based on (Salehyan et al., 2012). Both anecdotal evidence and research studies alike suggest increased social conflict linked to Chinese investment activities. We take these concerns seriously by disentangling the results from Table 5-6 from the main part. We consider the effects on demonstrations, riots and strikes separately with OLS in Tables 5-B19 ,5-B20 and 5-B21 as well as using IV in Table 5-B22. Results do not correspond to a statistically significant positive effect of aid on neither riots, demonstrations, and strikes. An explanation could be that these accounts mostly cover commercial investment activities, which are not conflict sensitively programmed (Wegenast et al., 2017; Christensen, 2017).

Additionally, we consider the robustness of the main results relating to repression fueling effects of Chinese aid. First, to separate clearly between regions with lethal pro-government and non-lethal pro-government activities, we constrain the sample on regions, which *did not* encounter any one-sided violence by the government registered in the UCDP dataset. Results in Table 5-B23 support a robust link between Chinese aid and repression. Second, when using instead of a dichotomous repression measure from SCAD a continuous indicator, a consistently positive effect of Chinese aid on repression is suggested by the IV estimates of Table 5-B24.

Table 5-B15 – ADM1 OLS results (Intensity 2)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: WB Aid									
ln($World\ Bank\ Aid_{\text{t-1}}$)	-0.1061	-0.0440	-0.0703	-0.1810***	-0.1522***	-0.1528**	-0.1156*	-0.1386**	-0.1513**
	(0.0659)	(0.0551)	(0.0536)	(0.0528)	(0.0532)	(0.0596)	(0.0656)	(0.0673)	(0.0708)
N	13104	13104	13104	13104	13050	13050	11699	13050	11699
Panel B: Chinese Aid									
ln($Chinese\ Aid_{\text{t-2}}$)	-0.0917	-0.0209	0.0184	-0.0285	-0.0140	0.0059	-0.0021	-0.0022	-0.0096
	(0.0614)	(0.0504)	(0.0378)	(0.0446)	(0.0510)	(0.0521)	(0.0531)	(0.0566)	(0.0605)
N	9464	9464	9464	9464	8700	8700	8254	8700	8254
Country FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Trends	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls × Year FE	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Linear Regional Trends	No	No	No	No	No	Yes	Yes	Yes	Yes
Lagged Endogeneous Controls	No	No	No	No	No	No	Yes	No	Yes
Country × Year FE	No	No	No	No	No	No	No	Yes	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD$\geq$25, 0 if BRD¡25). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.6.

Interpretation: To address concerns that the results hold only for a conflict measure of at least 5 battle-related deaths, the table shows that the results are nearly unchanged compared to Table 5-3 if considering a conflict threshold of at least 25 battle-related deaths. In addition, the coefficients for our preferred specification (8) becomes insignificant in the IV setting of Table 5-B16.

Table 5-B16 – IV (Intensity 2)

	(1)	(2)
Panel A: WB Aid		
IV Second Stage: World Bank		
$\ln(World\,Bank\,Aid_{\text{t-1}})$	-0.1437	-0.4581
	(0.3075)	(0.3301)
IV First stage: World Bank		
$IDA\,Position_{\text{t-1}} \times \text{Cum. Prob}_{\text{t-2}}$	70.9363***	80.8832***
	(7.1065)	(8.6854)
N	12325	12325
Panel B: Chinese Aid		
IV Second Stage: China		
$\ln(Chinese\,Aid_{\text{t-2}})$	0.1289	0.1652
	(0.2757)	(0.3140)
N	7975	7975
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	36.578	31.190
IV First stage: China		
$Chinese\,Commodity_{\text{t-3}} \times \text{Cum. Prob}_{\text{t-3}}$	-14.0193***	-12.6964***
	(2.3180)	(2.2734)
N	7975	7975
Exogeneous Controls	Yes	Yes
Exogeneous Controls × Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country-Year FE	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥25, 0 if BRD¡25). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. The constituent term of the probability is depicted in the appendix.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.6.
Interpretation: To address concerns that the results hold only for a conflict measure of at least 5 battle-related deaths, the table shows that the results are nearly unchanged compared to Table 5-4 if considering a conflict threshold of at least 25 battle-related deaths.

Table 5-B17 – OLS results (Battle-related Deaths)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: WB Aid									
ln($World\ Bank\ Aid_{\text{t-1}}$)	-0.0164*	-0.0014	-0.0025	-0.0174***	-0.0165***	-0.0142**	-0.0106	-0.0142*	-0.0131
	(0.0092)	(0.0071)	(0.0065)	(0.0060)	(0.0059)	(0.0065)	(0.0077)	(0.0074)	(0.0082)
N	13104	13104	13104	13104	13050	13050	11699	13050	11699
Panel B: Chinese Aid									
ln($Chinese\ Aid_{\text{t-2}}$)	-0.0119	0.0034	0.0068	-0.0055	-0.0008	0.0004	0.0001	0.0034	0.0025
	(0.0087)	(0.0065)	(0.0054)	(0.0048)	(0.0055)	(0.0057)	(0.0057)	(0.0063)	(0.0064)
N	9464	9464	9464	9464	8700	8700	8254	8700	8254
Country FE	No	Yes	Yes	–	–	–	–	–	–
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	–	–
Time Trends	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls × Year FE	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Linear Regional Trends	No	No	No	No	No	Yes	Yes	Yes	Yes
Lagged Endogeneous Controls	No	No	No	No	No	No	Yes	No	Yes
Country × Year FE	No	No	No	No	No	No	No	Yes	Yes

Notes: The table displays regression coefficients with the log of battle-related deaths + 0.01 as dependent variable (category 3). Standard errors in parentheses, two-way clustered at the country-year and regional level. The sample includes African countries for the sampling period of 1995-2012 for the WB and 2000-2012 for Chinese Aid. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.6.

Interpretation: To address concerns that the results hold only for a dichotomous conflict measure, the table shows that the results are nearly unchanged compared to Table 5-3 if considering log of battle-related deaths as the dependent variable. In addition, the coefficients for our preferred specification (8) become insignificant in the IV setting of Table 5-B18.

Table 5-B18 – IV (Battle-Related Deaths)

	(1)	(2)
Panel A: WB Aid		
IV Second Stage: World Bank		
$\ln(World\,Bank\,Aid_{\text{t-1}})$	-0.0179	-0.0340
	(0.0340)	(0.0358)
N	12325	12325
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	99.639	86.724
IV First stage: World Bank		
$IDA\,Position_{\text{t-1}} \times \text{Cum. Prob}_{\text{t-2}}$	70.9363***	80.8832***
	(7.1065)	(8.6854)
N	12325	12325
Panel B: Chinese Aid		
IV Second Stage: China		
$\ln(Chinese\,Aid_{\text{t-2}})$	-0.0312	-0.0180
	(0.0337)	(0.0419)
N	7975	7975
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	36.578	31.190
IV First stage: China		
$Chinese\,Commodity_{\text{t-3}} \times \text{Cum. Prob}_{\text{t-3}}$	-14.0193***	-12.6964***
	(2.3180)	(2.2734)
N	7975	7975
Exogeneous Controls	Yes	Yes
Exogeneous Controls × Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country-Year FE	No	Yes

Notes: The table displays regression coefficients for the log of battle-related deaths +0.01 as dependent variable (category 3). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. The constituent term of the probability is depicted in the appendix.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.6.
Interpretation: To address concerns that the results hold only for a dichotomous conflict measure, the table shows that the results are nearly unchanged compared to Table 5-4 if considering log of battle-related deaths as the dependent variable.

Table 5-B19 – OLS results (Demonstrations)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: WB Aid									
ln($World\,Bank\,Aid_{\text{t-1}}$)	0.0578	0.1247*	0.3399***	0.0514	0.0414	0.0491	0.0272	0.0390	0.0260
	(0.0684)	(0.0708)	(0.0705)	(0.0472)	(0.0454)	(0.0569)	(0.0640)	(0.0633)	(0.0700)
N	13104	13104	13104	13104	13050	13050	11699	13050	11699
Panel B: Chinese Aid									
ln($Chinese\,Aid_{\text{t-2}}$)	0.7830***	0.8995***	0.9203***	-0.1090	-0.0865	-0.0781	-0.0711	-0.1094	-0.0955
	(0.1899)	(0.1649)	(0.1700)	(0.0766)	(0.0864)	(0.0964)	(0.0983)	(0.1188)	(0.1213)
N	9464	9464	9464	9464	8700	8700	8254	8700	8254
Country FE	No	Yes	Yes	–	–	–	–	–	–
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	–	–
Time Trends	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls × Year FE	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Linear Regional Trends	No	No	No	No	No	Yes	Yes	Yes	Yes
Lagged Endogeneous Controls	No	No	No	No	No	No	Yes	No	Yes
Country × Year FE	No	No	No	No	No	No	No	Yes	Yes

Notes: The table displays regression coefficients with a binary indicator for demonstrations as dependent variable. The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.4.

Interpretation: To address concerns that our non-finding for the OLS results on protests in Table 5-B27 is driven by the aggregation of riots, demonstrations, and strikes, we consider each protest type by itself. The table shows that our preferred specifications (6) and (8) still exhibit no statistically significant relationship between demonstrations and aid.

Table 5-B20 – OLS results (Riots)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: WB Aid									
ln($World\,Bank\,Aid_{\text{t-1}}$)	0.0920	0.0037	0.2350***	0.0129	-0.0060	-0.0060	-0.0203	-0.0853	-0.0864
	(0.0620)	(0.0856)	(0.0617)	(0.0533)	(0.0510)	(0.0584)	(0.0635)	(0.0710)	(0.0771)
N	13104	13104	13104	13104	13050	13050	11699	13050	11699
Panel B: Chinese Aid									
ln($Chinese\,Aid_{\text{t-2}}$)	0.4258***	0.5248***	0.5289***	0.0006	0.0399	0.0316	0.0411	0.0424	0.0524
	(0.1482)	(0.1261)	(0.1292)	(0.0814)	(0.0933)	(0.0985)	(0.0995)	(0.1175)	(0.1195)
N	9464	9464	9464	9464	8700	8700	8254	8700	8254
Country FE	No	Yes	Yes	–	–	–	–	–	–
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	–	–
Time Trends	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls × Year FE	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Linear Regional Trends	No	No	No	No	No	Yes	Yes	Yes	Yes
Lagged Endogeneous Controls	No	No	No	No	No	No	Yes	No	Yes
Country × Year FE	No	No	No	No	No	No	No	Yes	Yes

Notes: The table displays regression coefficients with a binary indicator for riots as dependent variable. The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.4.

Interpretation: To address concerns that our non-finding for the OLS results on protests in Table 5-B27 is driven by the aggregation of riots, demonstrations, and strikes, we consider each protest type by itself. The table shows that our preferred specifications (6) and (8) still exhibit no statistically significant relationship between riots and aid.

Table 5-B21 – OLS results (Strikes)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: WB Aid									
ln($World\,Bank\,Aid_{\,\text{t-1}}$)	0.0020	0.0302	0.1288***	-0.0197	-0.0252	-0.0377	-0.0374	-0.0717	-0.0704
	(0.0310)	(0.0391)	(0.0377)	(0.0309)	(0.0333)	(0.0415)	(0.0454)	(0.0503)	(0.0555)
N	13104	13104	13104	13104	13050	13050	11699	13050	11699
Panel B: Chinese Aid									
ln($Chinese\,Aid_{\,\text{t-2}}$)	0.1611*	0.1832**	0.1931**	-0.1785**	-0.2042**	-0.1845**	-0.1817*	-0.1620	-0.1654
	(0.0847)	(0.0810)	(0.0846)	(0.0712)	(0.0804)	(0.0938)	(0.0959)	(0.1046)	(0.1114)
N	9464	9464	9464	9464	8700	8700	8254	8700	8254
Country FE	No	Yes	Yes	–	–	–	–	–	–
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	–	–
Time Trends	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls × Year FE	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Linear Regional Trends	No	No	No	No	No	Yes	Yes	Yes	Yes
Lagged Endogeneous Controls	No	No	No	No	No	No	Yes	No	Yes
Country × Year FE	No	No	No	No	No	No	No	Yes	Yes

Notes: The table displays regression coefficients with a binary indicator for strikes as dependent variable. Standard errors in parentheses, two-way clustered at the country-year and regional level. The sample includes African countries for the sampling period of 1995-2012 for the WB and 2000-2012 for Chinese Aid. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.4.
Interpretation: To address concerns that our non-finding for the OLS results on protests in Table 5-B27 is driven by the aggregation of riots, demonstrations, and strikes, we consider each protest type by itself. The table shows that our preferred specifications (6) and (8) still exhibit no statistically significant relationship between strikes and aid. This is not the case for specification (6), which is addressed in table 5-B22.

Table 5-B22 – IV (Riots, Demonstrations & Strikes [SCAD])

Panel A: WB Aid	(1)	(2)	(3)	(4)	(5)	(6)
IV Second Stage: World Bank						
	Demonstr.	Demonstr.	Riots	Riots	Strikes	Strikes
ln($World\,Bank\,Aid_{\text{t-1}}$)	-0.2232	-0.1458	0.0106	-0.1950	0.0289	-0.0184
	(0.2514)	(0.2808)	(0.2543)	(0.2294)	(0.1793)	(0.1463)
N	12325	12325	12325	12325	12325	12325
Kleibergen-Paap underidentification test p-value	0.000	0.000	0.000	0.000	0.000	0.000
Kleibergen-Paap weak identification F-statistic	99.639	86.724	99.639	86.724	99.639	86.724
Panel B: Chinese Aid						
IV Second Stage: China						
	Demonstr.	Demonstr.	Riots	Riots	Strikes	Strikes
ln($Chinese\,Aid_{\text{t-2}}$)	0.0498	0.0686	-0.0629	0.0424	-0.1489	-0.0776
	(0.4018)	(0.4707)	(0.3622)	(0.4312)	(0.4183)	(0.5076)
N	7975	7975	7975	7975	7975	7975
Kleibergen-Paap underidentification test p-value	0.000	0.000	0.000	0.000	0.000	0.000
Kleibergen-Paap weak identification F-statistic	36.578	31.190	36.578	31.190	36.578	31.190
Country-Year FE	No	Yes	No	Yes	No	Yes

Notes: The table displays regression coefficients for any violence of these three types as dependent variable. The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. OLS results are depicted in the appendix. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.4

Interpretation: The table shows the intrumental variables results for specification (6) and (8) of Tables 5-B19, 5-B20, and 5-B21. There is no evidence that aid by either donor is statistically significantly affecting demonstrations, riots, and strikes individually in Africa during our sample period.

Table 5-B23 – ADM1 IV (Repression (non-lethal) - Regions with UCDP violence against civilians coded as zero)

	(1)	(2)
Panel A: WB Aid		
IV: IDA Position - Actors		
$\ln(World\,Bank\,Aid_{\text{t-1}})$	0.1543	0.0885
	(0.1042)	(0.1177)
N	12325	12325
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	99.639	86.724
Panel B: Chinese Aid		
IV: Chinese Commodity - Actors		
$\ln(Chinese\,Aid_{\text{t-2}})$	0.6103**	0.7696**
	(0.2873)	(0.3439)
N	7975	7975
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	36.578	31.190
Exogeneous Controls	Yes	Yes
Exogeneous Controls × Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country-Year FE	No	Yes

Notes: The table displays regression coefficients for a binary pro-governmental violence indicator as dependent variable. The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. The constituent term of the probability is depicted in the appendix. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.4.

Interpretation: One concern is that our findings in Table 5-6, based on the SCAD repression data are somehow affected by repression overlapping with lethal conflicts recorded in the UCDP-GED data. To test the robustness of the repression results, this table shows that theynearly unchanged even if the repression indicator ignores cases that also feature recorded UCDP violence against civilians. Repression is thus distinct from large scale conflict against civilians events.

Table 5-B24 – Non-lethal Repression [SCAD] - Continuous measure

Panel A: WB Aid	(1)	(2)
IV Second Stage: World Bank		
$\ln(World\,Bank\,Aid_{\text{t-1}})$	0.0011	0.0012
	(0.0014)	(0.0013)
N	12325	12325
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	99.639	86.724
Panel B: Chinese Aid		
IV Second Stage: China		
$\ln(Chinese\,Aid_{\text{t-2}})$	0.0072**	0.0092**
	(0.0032)	(0.0045)
N	7975	7975
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	36.578	31.190
Country-Year FE	No	Yes

Notes: The table displays regression coefficients for a continuous measure of non-lethal pro-government violence as dependent variable. The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.4.

Interpretation: The table addresses the concern that the results of Table 5-6 only hold for the binary dependent variable of non-lethal government repression. The relationship of Chinese aid on non-lethal government repression remains positive and statistically significant.

Comparison with OLS estimates

To test if results substantially change when using OLS, we consider the results corresponding to the IV estimates on actors (Table 5-5) and the aggregated outcome for riots, demonstrations, and strikes (Table 5-6). Table 5-B25 suggests mostly neutral effects, while significantly negatively coefficients of WB aid occur for state-based and non-state violence. Regarding riots, demonstrations, and strikes, Table 5-B27 shows that the different actors' results become insignificant once we condition on regional level fixed effects.

Table 5-B25 – Actors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: WB Aid - OLS								
	State vs. N-State		N-State vs. N-State		State vs. Civilans		N-State vs. Civilians	
OLS: WB - Actors ln($World\,Bank\,Aid_{\text{t-1}}$)	-0.1229**	-0.1365**	-0.0348	-0.0784	-0.0596	-0.0372	-0.1040**	-0.0979**
	(0.0580)	(0.0615)	(0.0417)	(0.0526)	(0.0373)	(0.0384)	(0.0427)	(0.0473)
N	13050	13050	13050	13050	13050	13050	13050	13050
Panel B: Chinese Aid - OLS								
	State vs. N-State		N-State vs. N-State		State vs. Civilans		N-State vs. Civilians	
OLS: China - Actors ln($Chinese\,Aid_{\text{t-2}}$)	-0.0009	0.0122	-0.0162	0.0016	-0.0702	-0.0625	-0.0338	-0.0334
	(0.0491)	(0.0591)	(0.0529)	(0.0659)	(0.0427)	(0.0454)	(0.0292)	(0.0373)
N	8700	8700	8700	8700	8700	8700	8700	8700
Country-Year FE	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. Exogenous (time-varying) controls are included in all regressions. Time Trends included, consist of linear and squared country-specific time trends as well as linear regional time trends. "State vs N-State" refers to state-based violence against non-government actors, "N-State vs N-State" refers to non-government violence against the other organized non-state groups, and "State vs Civilians" refers to one-sided violence versus civilians by the government and "N-State vs. Civilians" refers to one-sided violence versus civilians by non-government (NG) actors. The categories are mutually exclusive. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to Table 5-5.

Interpretation: For state-based violence against civilians, the coefficients are negative and statistically insignificant, while the IV coeficients in Table 5-5 were also statistically significant.

Table 5-B26 – OLS results (Protests: Riots, Demonstrations & Strikes [SCAD])

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: WB Aid									
$\ln(World\,Bank\,Aid_{\text{t-1}})$	0.1194	0.1291	0.4360***	0.0106	-0.0140	-0.0035	-0.0443	-0.0092	-0.0270
	(0.0912)	(0.1028)	(0.0885)	(0.0641)	(0.0635)	(0.0779)	(0.0845)	(0.0897)	(0.0993)
N	13104	13104	13104	13104	13050	13050	11699	13050	11699
Panel B: Chinese Aid									
$\ln(Chinese\,Aid_{\text{t-2}})$	0.8761***	1.0301***	1.0445***	-0.1026	-0.0468	-0.0182	-0.0041	0.0141	0.0330
	(0.2247)	(0.1888)	(0.1939)	(0.0880)	(0.0973)	(0.1005)	(0.1022)	(0.1265)	(0.1296)
N	9464	9464	9464	9464	8700	8700	8254	8700	8254
Country FE	No	Yes	Yes	–	–	–	–	–	–
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	–	–
Time Trends	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls × Year FE	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Linear Regional Trends	No	No	No	No	No	Yes	Yes	Yes	Yes
Lagged Endogeneous Controls	No	No	No	No	No	No	Yes	No	Yes
Country × Year FE	No	No	No	No	No	No	No	Yes	Yes

Notes: The table displays regression coefficients with a binary indicator for any violence of these three types as dependent variable. The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.4.

Interpretation: The table display the corresponding OLS results of Table 5-6. The results are in line with the instrumental variable estimates since they are mostly negative, but statistically insignificant.

Table 5-B27 – OLS results (Non-lethal Government Repression)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: WB Aid									
$\ln(World\,Bank\,Aid_{\text{t-1}})$	0.0406**	0.0645***	0.0955***	0.0474**	0.0301	0.0327	0.0200	0.0139	-0.0022
	(0.0177)	(0.0217)	(0.0231)	(0.0193)	(0.0191)	(0.0209)	(0.0232)	(0.0289)	(0.0287)
N	13104	13104	13104	13104	13050	13050	11699	13050	11699
Panel B: Chinese Aid									
$\ln(Chinese\,Aid_{\text{t-2}})$	0.2144***	0.2116***	0.2248***	0.0279	0.0185	0.0126	0.0151	0.0079	0.0116
	(0.0814)	(0.0702)	(0.0712)	(0.0476)	(0.0521)	(0.0552)	(0.0564)	(0.0660)	(0.0674)
N	9464	9464	9464	9464	8700	8700	8254	8700	8254
Country FE	No	Yes	Yes	–	–	–	–	–	–
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	–	–
Time Trends	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls × Year FE	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Linear Regional Trends	No	No	No	No	No	Yes	Yes	Yes	Yes
Lagged Endogeneous Controls	No	No	No	No	No	No	Yes	No	Yes
Country × Year FE	No	No	No	No	No	No	No	Yes	Yes

Notes: The table displays regression coefficients with a binary indicator for non-lethal government repression as dependent variable. The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.4.

Interpretation: The table display the corresponding OLS results of Table 5-6. The results are in line with the IV estimates in Table 5-6. The estimate for China is positive and significant.

5-B3 Channels - Aid Sectors

Aid in different sectors could be more or less likely to fuel or calm down a conflict. We examine aid projects in eight subcategories with and without country-year FE. For the WB, the IV strategy works well using sector-specific probabilities. For China, we only show OLS resultsdue to severe weak IV problems caused by limited observations in certain sectors.

Aid in different sectors exhibit different effects on conflict. Table 5-B28 shows that there are positive coefficients of WB (Chinese) aid in a few categories, though, statistically insignificant. The insignificant negative average effects in previous tables seem to be driven by significant conflict-reducing effects for the sectors ”finance” (WB only) and ”transportation” (WB and China). A 100% increase in WB finance aid is associated with a 1.59 percentage point reduction in the conflict likelihood – relative to the baseline likelihood of 12 percent. Examining a sample of the 1,361 finance projects shows that they typically support both existing and new projects to induce structural or sectoral reforms. These projects provide technical assistance and consulting, in topics of regulation and finance, or business services.[48] As monetary disbursements are rather small, the main impact must stem from the knowledge transfer and technical support to modernize and develop capital markets, banks and insurances, as well as technical assistance to enhance transparency and regulation.

Regarding the transportation sector, a 100% increase in WB (Chinese) aid is associated with a 6.7 (3.4) percentage points reduction in the conflict likelihood. This sector has many large-scale infrastructure projects with large disbursements in dollar terms. The negative effect suggests that high transportation costs were significant obstacles for exchange, consumption, public goods provision, and eventually economic growth (see also **??**). This seems to dominate both potentially negative effects on corruption (Isaksson and Kotsadam, 2018a), and disputes over land usage. It is in line with (Bluhm et al., 2020), who show that Chinese infrastructure projects reduce economic inequality and, hence, potential reasons for conflict.[49]

Overall, the heterogeneities across aid categories are a first explanation for the relatively broad confidence interval when studying the average effect of WB and Chinese aid. We find no significant conflict-fueling effect on any aid sector for neither donor. The overall negative relationship does not seem to mask strong conflict-fueling effects in certain sectors.[50]

[48]Out of 40 projects, 26 were in one of those categories. Appendix section 5-A4 documents how we retrieve detailed information on World Bank aid in the finance sector.

[49]Improvements in transportation infrastructure are likely linked to higher accessibility for the media and correlate with mobile phone coverage. This would induce an upward bias to our estimates (Weidmann, 2016; Von Borzyskowski and Wahman, 2019).

[50]Table A5-B44 presents WB OLS and ChineseIV results. The results differ slightly, but there is no significant positive effect in any sector. One caveat of these regressions is that high collinearity and insufficient power make regressions infeasible where all individual sectoral aid variables are jointly included.

Table 5-B28 – Aid sectors and conflict

World Bank Aid Sectors - IV	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: No Country-Year FE	AX	BX	CX	EX	FX	JX	LX	TX	WX	YX
ln($World\,Bank\,Aid_{\text{t-1}}$)	0.2179	-0.2102	0.3423	0.5525	-1.6744**	0.2773	-0.1658	-0.7843**	0.5021	-0.4463
	(0.3572)	(0.4195)	(0.3016)	(0.4572)	(0.7877)	(0.4321)	(0.2858)	(0.3323)	(0.5593)	(0.3647)
Kleibergen-Paap underidentification test p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Kleibergen-Paap weak identification F-statistic	58.309	80.342	39.353	50.568	16.781	73.307	33.666	64.555	40.026	31.887
Panel B: Country-Year FE										
ln($World\,Bank\,Aid_{\text{t-1}}$)	0.4793	-0.4087	0.2652	0.2253	-1.5963*	0.2952	-0.1206	-0.6667*	-0.2726	-0.3717
	(0.3152)	(0.4445)	(0.2709)	(0.4771)	(0.9361)	(0.4020)	(0.2764)	(0.3570)	(0.6850)	(0.3299)
N	12325	12325	12325	12325	12325	12325	12325	12325	12325	12325
Kleibergen-Paap underidentification test p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Kleibergen-Paap weak identification F-statistic	59.949	61.188	56.632	31.111	12.238	73.686	36.219	28.587	23.180	33.957
Chinese Aid Sectors - OLS										
Panel C: No Country-Year FE	AX	BX	CX	EX	FX	JX	LX	TX	WX	YX
ln($Chinese\,Aid_{\text{t-2}}$)	-0.3165	-0.2123	0.1770	-0.0830		-0.0168	0.3516	-0.2780*	-0.2974	0.8388
	(0.2001)	(0.1446)	(0.1321)	(0.1584)		(0.1604)	(0.2681)	(0.1633)	(0.1842)	(0.8914)
Panel D:Country-Year FE										
ln($Chinese\,Aid_{\text{t-2}}$)	-0.1946	-0.1881	0.1281	-0.0484		0.0287	0.3241	-0.3378*	0.0377	0.7787
	(0.2307)	(0.1405)	(0.1252)	(0.1635)		(0.1533)	(0.2792)	(0.1946)	(0.2148)	(0.7926)
N	8700	8700	8700	8700		8700	8700	8700	8700	8700

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD$\geq$5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. Regressions account for (time-varying) exogenous controls and time trends. Time Trends include linear and squared country-specific time trends as well as a linear regional trend. AX - "Agriculture, fishing, and forestry" BX - "Public Administration, Law, and Justice" CX - "Information and communications" EX - "Education" FX - "Finance" JX - "Health and other social services" LX - "Energy and mining" TX - "Transportation" WX - "Water, sanitation and flood protection" YX - "Industry and Trade" Standard errors in parentheses, two-way clustered at the country-year and regional level: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Interpretation: One may be concerned about the existence of a certain conflict-increasing type of aid that is masked in our overall aid measure. To address this concern, we find that aid in any sectors is, if anything, significantly negatively related to conflict. However, there is a fair bit of heterogeneity. This also highlights that some types of aid may have a different effect in the short and in the long run. Generally, this suggests ample room for future research to explore these results in more detail.

5-B4 Channels - Ethnic groups and governing coalition

Conflicts are not only driven by economic considerations but often strongly influenced by existing cleavages between groups. Ethnic identities are among the most salient traits and ethnicities constitute a very important reference group in most African countries. To measure ethnic homelands, we use the GREG dataset (Weidmann et al., 2010). This dataset is a geo-referenced version of the initial locations of ethnic homelands based on the Soviet Atlas Narodov Mira. These locations were determined before our sample, and, even though immigration becomes more important over time, prior studies suggest that a large share of Africans still live in their ethnic home region (Nunn and Wantchekon, 2011). This makes those group polygons a noisy, but still informative measure.

The first important question is whether the effect of aid projects differs between more and less ethnically fractionalized regions. Theoretically, one may expect more potential for dissatisfaction about an unequal allocation of projects or the distribution of the associated benefits in ethnically fractionalized regions. We compute standard fractionalization measures in line with the literature (Alesina and Ferrara, 2005; Fearon and Laitin, 2003b), and split the sample between countries in regions with fractionalization above or below the median. Appendix Table 5-B30 shows no large differences. When including country-year FE, the negative relationship between aid and conflict becomes even a bit stronger, but the difference is small. Even in the more fractionalized regions, it does not turn positive. [51]

More important than considering ethnic cleavages, in general, is to define which ethnic groups are allies and form a joint coalition and which groups are outside that coalition. To classify administrative regions, our unit of analysis, we distinguish whether all groups (Coalition), at least one group (Mixed), or no group (N-Coalition) in a region is part of the governing coalition in a particular year. The information about the power status comes from the time-variant Ethnic Power Relations (EPR) dataset (Vogt et al., 2015). Wherever possible, we match the group power status from EPR in a particular year to one of the time-invarying GREG group homelands. The original dataset assigns eight different power statuses to groups. The difference is sometimes marginal and hard to interpret, which is why we only use the more precise information on whether a group was part of the governing coalition or not. We then intersect the ethnic group polygons with the administrative regions to classify regions as one of the three categories.

This distinction aims at testing the plausibility of the existing results, and at uncovering heterogeneous effects that may be hidden in the averages. For instance, it may be that there is no conflict-inducing effect on average. However, assuming that aid project benefit governing groups more often, existing tensions and conflict may be fueled especially in mixed districts where other groups observe these distributional differences. In contrast, rapacity theory would predict that governing coalition regions with large aid inflows become more attractive for rebels to capture.

We find several interesting differences in Table 5-B29. The results for the WB always change signs depending on the inclusion of country-year fixed effects. Nonetheless, there is again never a significant conflict-inducing effect. For China,

[51] Note that for individual aid sectors, the IV does not perform sufficiently well for China when splitting the samples. Therefore, we show the OLS specifications for all the sample splits for China. We intend to conduct a more in-depth analysis of aid inequality and ethnic groups in an accompanying chapter.

both coefficients for mixed regions are positive. However, all coefficients are statistically insignificant. Even when considering governing coalition structures, on average Chinese aid does not increase conflicts with at least 5 BRDs.[52] Moreover, we control in all regressions for fractionalization, which we define in this case as $1-\sum s^2$, where s is the ethnic groups area share in the administrative region. To account for the important role that ethnic fractionalization takes in the politico-economic literature (e.g., Alesina et al., 2003), we consider also a sample split at the median of ethnic fractionalization in Table 5-B30. In the subsample the instrumental variable retains strength. Although coefficients change signs, when considering the more fractionalized regions, results support robustness of the neutral effects.

[52]This finding is robust to defining the coalition only as the more powerful senior, dominant or monopoly groups and excluding junior partners. Results are available upon request from the authors.

Table 5-B29 – ADM1 results (Power status - Member of Coalition Group)

Panel A: WB - IV	(1)	(2)	(3)	(4)	(5)	(6)
Conflict in region belonging to ...	N-Coalition	N-Coalition	Coalition	Coalition	Mixed	Mixed
$\ln(World\,Bank\,Aid_{\text{t-1}})$	-0.7052	0.2016	0.0686	-0.6372	0.1552	-0.3712
	(0.9362)	(1.3680)	(0.4500)	(0.4716)	(0.5181)	(0.5339)
N	2144	2075	3750	3651	4569	4537
Kleibergen-Paap underidentification test p-value	0.000	0.003	0.000	0.000	0.000	0.000
Kleibergen-Paap weak identification F-statistic	35.086	18.726	41.902	26.417	63.396	66.952
Panel B: China- IV:						
Conflict in region belonging to ...	N-Coalition	N-Coalition	Coalition	Coalition	Mixed	Mixed
$\ln(Chinese\,Aid_{\text{t-2}})$	-0.6513	0.7011	-0.7345	-1.2272	0.6919	1.1403
	(1.0808)	(3.4968)	(0.5935)	(0.7612)	(0.6681)	(0.9162)
N	1335	1285	2487	2420	2944	2924
Kleibergen-Paap underidentification test p-value	0.033	0.055	0.000	0.000	0.000	0.000
Kleibergen-Paap weak identification F-statistic	15.575	3.709	59.921	46.322	22.702	19.653
Country $\times$ Year FE	No	Yes	No	Yes	No	Yes
Control for Fractionalization	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD$\geq$5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. Both regressions include (time-varying) exogenous controls, year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends as well as linear regional time trends. Columns (1) & (2) refer to all regions without members of the governing coalition, whereas columns (3) & (4) to mixed regions with some groups in and out of the coalition, and columns (5) & (6) to regions that contain groups exclusively from the coalition. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Interpretation: The table addresses concerns that our analysis masked a conflict-increasing effect of aid flowing to regions with different levels of political power. There is no evidence of such an effect of aid.

Table 5-B30 – Sample-split: Median Fractionalization

Panel A: WB Aid - IV:				
$\ln(World\,Bank\,Aid_{\text{t-1}})$	-0.2585	-0.6189	0.1471	-0.0455
	(0.4163)	(0.4904)	(0.5688)	(0.7054)
N	5474	5474	4998	4998
Kleibergen-Paap underidentification test p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap weak identification F-statistic	71.721	49.454	75.067	65.391
Panel B: Chinese Aid - IV:				
$\ln(Chinese\,Aid_{\text{t-2}})$	-0.4831	-0.5251	0.0510	0.7714
	(0.5695)	(0.7265)	(0.6113)	(0.7163)
N	3542	3542	3234	3234
Kleibergen-Paap underidentification test p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap weak identification F-statistic	51.569	38.166	23.501	20.763
Country $\times$ Year FE	No	Yes	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD$\geq$5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. The sample is split in regions, which are below the country level median/mean of ethnic fractionalization (0) [columns (1) & (2)] or above the median/mean (1) [columns (3) & (4)]. Ethnic fractionalization is based on $1-\sum s^2$, where s is the ethnic groups area share in the administrative region. Conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. Both regressions include (time-varying) exogenous cont+rols, year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends as well as linear regional time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Interpretation: Does aid increase the conflict likelihood if a region is highly fractionalized due to ethnic tensions? The table shows that there is no evidence for such an effect.

5-B5 Regime Types

Development aid may have differential impacts across political systems due to different allocation decisions and distributional aspects. As a further sensitivity check, we consider heterogeneous effects across regime types. Based on the Polity IV data by Marshall et al. (2014), we distinguish democracies (Polity Score *geq* +7) and autocracies (Polity Score ¡+7). Results are depicted for outright conflict in Table 5-B31 and for repression in Table 5-B32.

Table 5-B31 – IV results - Aid and conflict across regime types

Panel A: World Bank Aid	(1)	(2)	(3)	(4)
	Autocracy		Democracy	
$\ln(World\,Bank\,Aid_{\text{t-1}})$	-0.3335	-0.3267	4.0175	1.0187
	(0.4762)	(0.5092)	(5.0638)	(2.0015)
N	10411	10411	1914	1914
Kleibergen-Paap underidentification test p-value	0.000	0.000	0.255	0.103
Kleibergen-Paap weak identification F-statistic	71.845	66.353	1.238	2.867
Panel B: Chinese Aid	**(1)**	**(2)**	**(3)**	**(4)**
	Autocracy		Democracy	
$\ln(Chinese\,Aid_{\text{t-2}})$	-0.3292	-0.4861	0.1872	0.2647
	(0.6014)	(0.7091)	(0.4943)	(0.9213)
N	6409	5521	1556	1311
Kleibergen-Paap underidentification test p-value	0.000	0.000	0.017	0.044
Kleibergen-Paap weak identification F-statistic	42.309	33.295	3.960	19.005
Country-Year FE	No	Yes	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include exogenous (time-varying) controls. Year and region fixed effects as well as time trends are included in all regressions. Time Trends include linear and squared country-specific time trends and a linear regional trend. Click here to go back to section 5-6.6. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Interpretation: One may be concerned that not differentiating between regime types may have masked an aid-conflict relationship. After all, there is the tendency that accountability principles are weaker and power concentration higher in autocratic countries. The table does not provide evidence that aid in autocratic or democratic countries has a significant relationship with conflict.

Table 5-B32 – IV results - Aid and repression across regime types

Panel A: World Bank Aid	(1)	(2)	(3)	(4)
	Autocracy		Democracy	
ln($World\,Bank\,Aid_{\text{t-1}}$)	0.0936	0.0338	2.1565	0.6255
	(0.1160)	(0.1293)	(2.3762)	(0.6082)
N	10411	10411	1914	1914
Kleibergen-Paap underidentification test p-value	0.000	0.000	0.255	0.103
Kleibergen-Paap weak identification F-statistic	71.845	66.353	1.238	2.867
Panel B: Chinese Aid	(1)	(2)	(3)	(4)
	Autocracy		Democracy	
ln($Chinese\,Aid_{\text{t-2}}$)	0.6312	0.8240*	0.7814**	1.0828***
	(0.4112)	(0.4851)	(0.3267)	(0.3016)
N	6409	5521	1556	1311
Kleibergen-Paap underidentification test p-value	0.000	0.000	0.017	0.044
Kleibergen-Paap weak identification F-statistic	42.309	33.295	3.960	19.005
Country-Year FE	No	Yes	No	Yes

Notes: The dependent variable is a binary indicator on occurrence of non-lethal repression (pro-government violence). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include exogenous (time-varying) controls. Year and region fixed effects as well as time trends are included in all regressions. Time Trends include linear and squared country-specific time trends and a linear regional trend. Click here to go back to section 5-6.6. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Interpretation: One may be concerned that not differentiating between regime types may have masked a potential relationship between government repression and aid. In line with Table 5-6, only Chinese Aid does correlate with higher government repression incidences. This result holds for autocratic and democratic countries, although the relationship seems to be stronger in democratic countries.

5-B6 Spatial Dimension (Spill Overs and Aggregation Levels)

Aggregation levels

Despite the many advantages of geospatial analysis (e.g., precision, geographical control variables), robustness is subject to the modifiable area unit problem (MAUP). More specifically, other conflict mechanisms can be at play when considering different levels of aggregation. Testing robustness on different spatial levels, hence, reduces the risk of ecological fallacy (Maystadt et al., 2014). This is specifically relevant in the aid-conflict nexus where different political entities may appropriate funds to engage in violent or peace-building activity. For this reason, we consider conflict and aid in the subordinate ADM2 regions both with OLS (IV) in Table 5-B33 (5-B34). Results are generally consistent with the main finding of a neutral effect of aid on conflict. Although the IV estimates for China turn positive, they do not attain statistical significance at any conventional level.

Additionally, we turn to an analysis on the country level as conflict may not manifest on the regional level, but spill over to other localities. Also on the country-level Table 5-B35 does provide neither for the WB nor for China any evidence of a significant link between aid and conflict. All OLS and IV estimates in Table 5-B35 are negative and for China even statistically significant using OLS method. Thus, the analysis on the country level is in line with the results on the regional level. The negative and non-significant effects, which are not in line with previous literature on the country level (for instance, Collier and Hoeffler, 2004b), may be due to our focus on aid flows, which are geocoded (see Section 5-3.1). To address concerns that our analysis misses non-geocoded aid flows of the two donors, we make use of the feature that we can include those flows on a country-level. Consistently, results in Table 5-B36 indicate significantly negative to neutral effects.[53] Hence, even when accounting for non-geocoded aid the main conclusion holds that there is no evidence that aid is positively related to conflict.

[53]Ideally, we would have liked to consider results in Table 5-B36 also via an instrumental variable approach, which was not possible due to weak IV concerns in the first stage.

Table 5-B33 – ADM2 level OLS results (Intensity 1)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: WB Aid									
ln($World\ Bank\ Aid_{\text{t-1}}$)	0.0288	0.0188	0.0068	-0.0740***	-0.0674***	-0.0580**	-0.0354	-0.0627**	-0.0535**
	(0.0209)	(0.0196)	(0.0219)	(0.0245)	(0.0228)	(0.0231)	(0.0256)	(0.0246)	(0.0263)
N	105354	105354	105354	105354	105214	105214	91333	105214	91333
Panel B: Chinese Aid									
ln($Chinese\ Aid_{\text{t-2}}$)	0.0105	0.0104	0.0579*	-0.0392	-0.0499	-0.0410	-0.0455	-0.0501	-0.0500
	(0.0407)	(0.0402)	(0.0331)	(0.0318)	(0.0388)	(0.0319)	(0.0327)	(0.0438)	(0.0454)
N	76089	76089	76089	76089	70132	70132	64482	70132	64482
Country FE	No	Yes	Yes	–	–	–	–	–	–
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	–	–
Time Trends	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls × Year FE	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Linear Regional Trends	No	No	No	No	No	Yes	Yes	Yes	Yes
Lagged Endogeneous Controls	No	No	No	No	No	No	Yes	No	Yes
Country × Year FE	No	No	No	No	No	No	No	Yes	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes second order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.6.

Interpretation: One concern may be that the results in Table 5-3 only hold for the aggregated data at the first sub-national administrative level, but there may be conflict-fueling effects at lower levels. We address this concern by validating the ADM1 results with an alternative lower-level of aggregation, the second-order administrative sub-divisions (ADM2). The table shows that once region fixed effects are included all coefficient signs are unchanged and the magnitude becomes stronger, on average.

Table 5-B34 – ADM2-level IV (Intensity 1)

	(1)	(2)
Panel A: WB Aid		
IV Second Stage: World Bank		
$\ln(World\,Bank\,Aid_{\text{t-1}})$	0.2599	0.1522
	(0.1644)	(0.1171)
N	99367	99367
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	82.851	67.210
IV First stage: World Bank		
$IDA\,Position_{\text{t-1}} \times \text{Cum. Prob}_{\text{t-2}}$	62.4924***	69.9580***
	(6.8656)	(8.5333)
N	99367	99367
Panel B: Chinese Aid		
IV Second Stage: China		
$\ln(Chinese\,Aid_{\text{t-2}})$	0.0517	0.0496
	(0.2007)	(0.2748)
N	64285	64285
Kleibergen-Paap underidentification test p-value	0.001	0.001
Kleibergen-Paap weak identification F-statistic	12.896	13.020
IV First stage: China		
$\text{Commodity}_{\text{t-3}} \times \text{Cum. Prob}_{\text{t-3}}$	-14.2846***	-12.8430***
	(3.9779)	(3.5593)
N	64285	64285
Exogeneous Controls	Yes	Yes
Exogeneous Controls × Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country-Year FE	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes second order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.6.

Interpretation: One concern may be that the results in Table 5-3 only hold for the aggregated data at the first sub-national administrative level, but there may be conflict-fueling effects at lower levels. We address this concern by validating the ADM1 results with an alternative lower-level of aggregation, the second-order administrative sub-divisions (ADM2). The table shows that while the first stage results are nearly identical, the coefficients in the second stage remain small and statistically insignificant. We test the consequences of higher levels of aggregation in Table 5-B35 and 5-B36.

Table 5-B35 – Country level aggregation with OLS and IV

Cross-Country Analysis				
ln($WB\,Aid_{\text{t-1}}$)	-0.2157	-2.4586		
	(0.2638)	(3.9577)		
ln($Chinese\,Aid_{\text{t-2}}$)			-0.2056*	-1.0947
			(0.1041)	(0.7621)
N	836	792	792	528
Kleibergen-Paap underidentification test p-value		0.101		0.000
Kleibergen-Paap weak identification F-statistic		2.743		22.130
Estimation method:	OLS	IV	OLS	IV

Notes: Dependent variable is a binary conflict indicator (100 if BRD≥25, 0 if BRD¡25). Columns (1) and (2) depict OLS/IV coefficients for WB geocoded aid aggregated at the country level. Columns (3) and (4) depict OLS/IV coefficients for Chinese geocoded aid aggregated at the country level. This includes aid, which is coded at least at the ADM1 level (refer to Figure 5-1). The sample includes African countries for the sampling period of 1995-2012 for the WB and 2000-2012 for Chinese Aid. Conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. All regressions include year and country fixed effects, as well as a linear country-trend. Standard errors in parentheses are clustered at the level of the country. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.6.

Interpretation: To test if a higher levels of data aggregation leads to a different conclusion about the effect of aid on conflict, the estimates refer to the country level, where aid and battle-related deaths were aggregated at the country level. The coefficients are very small and statistically insignificant. Even though the Kleibergen-Paap F-statistic is well below the critical value of 10 for the World Bank estimations, the other results indicate that there is no evidence that aid fuels conflict also when aggregating our date on the country level.

Table 5-B36 – Country level aggregation with inclusion of non-geocoded projects

Panel A: WB Aid	Geocoded only	Non-Geocoded aid incld.
WB Aid_{t-1}	-0.2157	-0.1491
	(0.2638)	(0.3263)
WB Aid_{t-1} non-geocoded		-0.1649
		(0.3675)
R^2	0.757	0.757
N	836	836
Panel B: Chinese Aid		
Chinese Aid_{t-2}	-0.2056*	-0.2016*
	(0.1041)	(0.1075)
Chinese Aid_{t-2} non-geocoded		-0.0735
		(0.1986)
R^2	0.763	0.763
N	792	792

Notes: Dependent variable: Category 2 binary conflict indicator (100 if BRD≥25, 0 if BRD¡25). Estimates refer to the country level, where aid and battle-related deaths were aggregated at the country level. The first column depicts coefficients for geocoded aid aggregated at the country level. The second column controlls for non-geocoded aid, which is aid coded less precise than the ADM1 level (refer to Figure 5-1). The sample includes African countries for the 1995-2012 (WB) and the 2000-2012 period (China). The regression includes country and year fixed effects, as well as a linear country-trend. Standard errors in parentheses are clustered at the level of the country. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.6.
Interpretation: One potential issues is that the aid measures in Table 5-B35 ignore projects that were not geo-coded or at least assigned to an ADM1 regions. This table shows that the main coefficients remain negative and do not change much when controlling for non-geocoded aid at the country level. Hence, omitting non-geocoded aid does not bias our results.

5-B7 Mechanisms - Afrobarometer

Table 5-B37 – Mechanisms - Afrobarometer

	WB	WB	China	China
Panel A: Security				
Security facilities: Police station present within walking distance?	0.001	0.008*	0.002	-0.004***
	(0.003)	(0.003)	(0.002)	(0.003)
Security forces: Any policemen or police vehicles?	0.002	0.004	0.001	-0.002
	(0.002)	(0.003)	(0.002)	(0.002)
Security forces: Any soldiers or army vehicles?	0.002*	0.005***	-0.001	-0.003
	(0.001)	(0.003)	(0.001)	(0.002)
Frequency of things stolen in past year?	-0.001	-0.006**	0.004*	0.004***
	(0.002)	(0.002)	(0.002)	(0.002)
Frequency of phsysical attacks in the past year?	-0.000	-0.003***	0.001	-0.000
	(0.001)	(0.002)	(0.001)	(0.001)
Panel B: Democratic norms and attitudes				
Democracy: How democratic is your country today?	-0.002	0.003	-0.005*	-0.000
	(0.002)	(0.003)	(0.002)	(0.003)
Democracy: Did you perceive last elections as free and fair?	-0.003	-0.003	-0.012**	-0.012
	(0.005)	(0.007)	(0.004)	(0.008)
Governance: Reject one-party rule	0.003	0.013*	-0.006	-0.003
	(0.005)	(0.005)	(0.004)	(0.006)
Governance: Reject military rule	0.006*	0.008*	-0.002	-0.001
	(0.003)	(0.004)	(0.003)	(0.004)
Governance: Reject one-man rule	0.004*	0.006*	-0.005*	-0.005***
	(0.002)	(0.003)	(0.002)	(0.003)
Reject government banning organizations that go against its policies	0.005*	0.014**	-0.003	0.002
	(0.002)	(0.005)	(0.003)	(0.004)
Panel C: Government responsiveness and repression				
Frequency of contact to government official to express your view	0.003*	0.003***	-0.001	0.001
	(0.001)	(0.002)	(0.001)	(0.001)
Fear of political intimidation or violence during campaigns	-0.001	-0.008***	0.003	0.011**
	(0.003)	(0.004)	(0.003)	(0.003)
How often do people have to be careful about what they say in politics?	0.000	-0.005	0.002	-0.002
	(0.002)	(0.004)	(0.002)	(0.003)
Rule of Law: People must obey the law	-0.004*	-0.001	0.004**	0.007**
	(0.002)	(0.003)	(0.001)	(0.002)
Frequency of joining others to request government action			-0.006**	
			(0.002)	
Country FE	Yes	Yes	Yes	Yes
Region FE	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Significance levels: * 0.10 ** 0.05 *** 0.01. Click here to go back to section 5-6.5.

5-B8 Estimations - Miscellaneous

Estimation approach

Data sets with many zero outcome observations can ask for different estimation approaches (Silva and Tenreyro, 2006). Therefore, we also consider a Poisson Pseudo Maximum Likelihood (PPML) estimator in Table 5-B38. In line with the main findings results are mostly non-significant and have a negative sign if turning statistically significant.[54] Due to the persistent nature of conflicts, the use of lagged dependent variables is a recurring topic in the conflict literature (e.g., Bazzi and Blattman, 2014). Table 5-B40, thus, presents the results including a lagged dependent variable, extending the main model by a lagged conflict indicator:

$$C_{i,c,t} = \beta_1 A_{i,c,t-1/t-2} + \beta_2 C_{i,c,t-1} + \lambda_c + \tau_t + \delta_i + \lambda_c T + \lambda_c T^2 + X^{Ex}_{i,c,t}\beta_2 \\ +\delta_i T + X^{En}_{i,c,t-2}\beta_3 + \kappa_{c,t} + \epsilon_{i,c,t} \tag{5-B1}$$

None of the coefficients in Table 5-B40 are positive, stressing the robustness of our main findings.

Although less often considered, the choice of standard error clustering can affect results substantially. Tables 5-B41 and 5-B42, thus, depart from our use of two-way clustering on the country-year and regional level, but only cluster on the region. Despite this adaptation, the results assure us that the insignificant findings are not driven by our choice of standard error clustering.

[54] A clear caveat is that we can only use year fixed effects with PPML in our setting due to convergence issues. Thus, as results do not differ substantially, we rely in the main part on OLS and instrumental variable estimators.

Table 5-B38 – PPML

	(1)	(2)	(3)
Panel A: WB Aid			
ln($World\,Bank\,Aid_{\text{t-1}}$)	-0.0005	0.0178	-0.0171
	(0.0063)	(0.0149)	(0.0173)
N	6246	1476	7344
Panel B: Chinese Aid			
ln($Chinese\,Aid_{\text{t-2}}$)	-0.0128*	0.0023	-0.0328*
	(0.0076)	(0.0131)	(0.0189)
N	3783	962	4589

Notes: Dependent variables- In column (1) a binary conflict indicator (100 if BRD≥5, 0 if BRD¡5), in column (2) a binary indicator if any event of non-lethal pro-government violence took place, in column (3) a continuous measure of logged battle-related deaths. Standard errors in parentheses, clustered at the regional level. The sample includes African countries for the sampling period of 1995-2012 for the WB and 2000-2012 for Chinese Aid. All regressions include year fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.6.

Interpretation: To address concerns that OLS regression is not an optimal method for binary dependent variables and dependent variables with many zeros we verify our results with Poisson pseudo-maximum likelihood estimation (Silva and Tenreyro, 2011). The results are in line with our main specification since the majority of the coefficients are not statistically significant or statistically significant but negative.

Table 5-B39 – Negative Binomial

	(1)	(2)
Panel A: WB Aid		
	Intensity 1 (Dummy)	Non-lethal repression (Dummy)
ln($World\,Bank\,Aid_{\text{t-1}}$)	-0.0150***	-0.0086
IRR:	0.9851***	-0.9914
	(0.0033)	(0.0117)
N	6246	1476
Panel B: Chinese Aid		
	Intensity 1 (Dummy)	Non-lethal repression (Dummy)
ln($Chinese\,Aid_{\text{t-2}}$)	-0.0177*	0.0266**
IRR	0.9825*	1.0269**
	(0.0101)	(0.0135)
N	3783	962

Notes: Dependent variables- In column (1) a binary conflict indicator (100 if BRD$\geq$5, 0 if BRD¡5) and in column (2) a binary indicator if any event of non-lethal pro-government violence took place. Standard errors in parentheses, clustered at the regional level. The sample includes African countries for the sampling period of 1995-2012 for the WB and 2000-2012 for Chinese Aid. All regressions include year fixed effects. Due to convergence issues standard control variables are not included. * p < 0.1, ** p < 0.05, *** p < 0.01 Click here to go back to section 5-6.6.

Interpretation: To address concerns that OLS regression is not an optimal method for binary dependent variables, we verify our results with a negative binomial estimator. IRR coefficients refer to incidence-rate ratios, where a coefficient <1 implies a negative effect and a >1 implies a positive effect.

Table 5-B40 – OLS results: Lagged dependent variable

Panel A: WB Aid	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\ln(World\ Bank\ Aid_{\text{t-1}})$	-0.0844	-0.0069	-0.0173	-0.1659***	-0.1575***	-0.1406**	-0.1149	-0.1647**	-0.1652*
	(0.0520)	(0.0551)	(0.0458)	(0.0585)	(0.0586)	(0.0680)	(0.0795)	(0.0780)	(0.0862)
N	13104	13104	13104	13104	13050	13050	11699	13050	11699
Panel B: Chinese Aid									
$\ln(Chinese\ Aid_{\text{t-2}})$	-0.0965*	-0.0300	-0.0082	-0.0983*	-0.0634	-0.0661	-0.0686	-0.0345	-0.0437
	(0.0563)	(0.0589)	(0.0588)	(0.0589)	(0.0660)	(0.0725)	(0.0721)	(0.0889)	(0.0925)
N	9464	9464	9464	9464	8700	8700	8254	8700	8254
Country FE	No	Yes	Yes	–	–	–	–	–	–
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	–	–
Time Trends	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls × Year FE	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Linear Regional Trends	No	No	No	No	No	Yes	Yes	Yes	Yes
Lagged Endogeneous Controls	No	No	No	No	No	No	Yes	No	Yes
Country × Year FE	No	No	No	No	No	No	No	Yes	Yes

Notes: Dependent variable is a binary conflict indicator (100 if BRD≥5, 0 if BRD¡5). Standard errors in parentheses, two-way clustered at the country-year and regional level. The sample includes African countries for the sampling period of 1995-2012 for the WB and 2000-2012 for Chinese Aid. Applying the lag structure of our regression equation, this means that conflicts are considered for the WB from 1996 to 2013 and for China from 2002 to 2014. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.6.
Interpretation: To address concerns of conflict persistence, these regressions control for the first lag of the binary indicator. The coefficients are nearly unchanged compared to Table 5-3.

Table 5-B41 – ADM1 OLS results (Clustering at regional level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: WB Aid									
$\ln(World\,Bank\,Aid_{\text{t-1}})$	-0.1918***	0.0010	-0.0496	-0.2129***	-0.2057***	-0.1608**	-0.1314*	-0.1772**	-0.1756*
	(0.0709)	(0.0643)	(0.0666)	(0.0611)	(0.0624)	(0.0672)	(0.0771)	(0.0799)	(0.0895)
N	13104	13104	13104	13104	13050	13050	11699	13050	11699
Panel B: Chinese Aid									
$\ln(Chinese\,Aid_{\text{t-2}})$	-0.1753**	-0.0233	-0.0026	-0.1090**	-0.0663	-0.0654	-0.0682	-0.0347	-0.0441
	(0.0761)	(0.0664)	(0.0676)	(0.0540)	(0.0605)	(0.0680)	(0.0687)	(0.0743)	(0.0757)
N	9464	9464	9464	9464	8700	8700	8254	8700	8254
Country FE	No	Yes	Yes	–	–	–	–	–	–
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	–	–
Time Trends	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls × Year FE	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Linear Regional Trends	No	No	No	No	No	Yes	Yes	Yes	Yes
Lagged Endogeneous Controls	No	No	No	No	No	No	Yes	No	Yes
Country × Year FE	No	No	No	No	No	No	No	Yes	Yes

Notes: The table displays regression coefficients with low Intensity Conflict (¿5 battle-related deaths) as dependent variable. The sample includes African countries for the sampling period of 1995-2012 for the WB and 2000-2012 for Chinese Aid. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-5.1.

Interpretation: One may be concerned that our null finding is based on too conservative clustering of our standard errors. To address this, the standard errors in parentheses are clustered at the regional level. As expected, the standard errors become slightly smaller which increases the statistical significance of the negative coefficients.

Table 5-B42 – ADM1 IV (Clustering at Regional Level)

	(1)	(2)
Panel A: WB Aid		
IV Second Stage: World Bank		
ln($World\,Bank\,Aid_{\text{t-1}}$)	-0.1014	-0.2252
	(0.3276)	(0.3899)
N	12325	12325
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	237.269	132.466
Panel B: Chinese Aid		
IV Second Stage: China		
ln($Chinese\,Aid_{\text{t-2}}$)	-0.2582	-0.1886
	(0.4169)	(0.5231)
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	55.897	41.160
Exogeneous Controls	Yes	Yes
Exogeneous Controls × Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country-Year FE	No	Yes

Notes: Dependent variable is a binary conflict indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes African countries for the sampling period of 1995-2012 for the WB and 2000-2012 for Chinese Aid. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. The constituent term of the probability is depicted in the appendix.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-5.1.
Interpretation: One may be concerned that our null finding is based on a clustering of our standard errors that is too conservative. To address this, the standard errors in parentheses are clustered at the regional level. The standard errors even become slightly smaller, but the negative coefficients remain statistically insignificant.

Time Contingency

Policy changes over time may affect the conflict implications of foreign aid, e.g., via conditionality or choice of different projects. This could, for instance, be a change in Western or US policy paradigms that also affected WB strategies and decisions. To allow for heterogeneity over time, we split the sample in an early period (for the WB 1995-2003 and for China 2000-2005) and a late period (for the WB 2004-2012 and for China 2006-2012). Coefficients remain insignificant, which provides further evidence that neutral effects on average are not driven by a specific time period.

Definition of aid (Sectors and weighting scheme)

Table 5-B44 reports the OLS/IV estimates corresponding to sectoral aid in Table 5-B28. Although significance is affected the negative signs in the transport and finance sectors are retained.

Populations weighting is an alternative allocation assumption, as opposed to location distribution. Tables 5-B45 and 5-B46 indicate that results are not driven by this assumption.

Table 5-B43 – ADM1 IV (WB Aid - Time Split)

Panel A: WB Aid		
ln($World\,Bank\,Aid_{\text{t-1}}$) $^{\text{¡=2003}}$	0.7770	0.2329
	(0.6407)	(0.6338)
ln($World\,Bank\,Aid_{\text{t-1}}$) $^{\text{¿2003}}$	-1.1608	-0.8405
	(0.9499)	(1.0267)
N	12325	12325
Kleibergen-Paap underid. test p-value	0.000	0.000
Kleibergen-Paap weak id. F-statistic	19.628	10.073
Panel B: Chinese Aid		
ln($Chinese\,Aid_{\text{t-2}}{}^{\text{¡=2005}}$)	-0.1320	0.0335
	(0.6197)	(0.5777)
ln($Chinese\,Aid_{\text{t-2}}{}^{\text{¿2005}}$)	-0.2266	-0.1853
	(0.7862)	(0.9107)
N	7975	7975
Kleibergen-Paap underidentification test p-value	0.022	0.026
Kleibergen-Paap weak identification F-statistic	7.204	6.972
Exogeneous Controls	Yes	Yes
Exogeneous Controls × Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country-Year FE	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes first order subnational regions in African countries. The sample includes African countries for the sampling period of 1995-2012 for the WB and 2000-2012 for Chinese Aid. Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.6.

Interpretation: Are our results driven by different policy regimes over time? To address this concern, the table splits the sample into different time periods, which does not alter our main conclusion.

Table 5-B44 – ADM1 - Aid Subtypes

WB Aid Subtypes - OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: No Country-Year FE	AX	BX	CX	EX	FX	JX	LX	TX	WX	YX
ln($World\,Bank\,Aid_{\text{t-1}}$)	0.0293	-0.1873**	0.1229	0.0215	-0.0958	-0.1575**	0.0236	-0.1479**	-0.0339	-0.1125
	(0.0734)	(0.0832)	(0.1526)	(0.0759)	(0.0886)	(0.0688)	(0.0855)	(0.0689)	(0.0816)	(0.0933)
Panel B: Country-Year FE										
ln($World\,Bank\,Aid_{\text{t-1}}$)	-0.0617	-0.2672***	0.0048	-0.0209	-0.0912	-0.1667*	-0.0317	-0.1137	0.0013	-0.2080*
	(0.0872)	(0.0953)	(0.1737)	(0.0990)	(0.1352)	(0.0896)	(0.0935)	(0.0898)	(0.1010)	(0.1067)
N	13050	13050	13050	13050	13050	13050	13050	13050	13050	13050
Chinese Aid Subtypes - IV										
Panel C: No Country-Year FE	AX	BX	CX	EX	JX	LX	TX	WX	YX	
ln($Chinese\,Aid_{\text{t-2}}$)	-1.4578**	-2.5352	-0.5066	0.7578	-0.1554	1.0265**	-0.4216	0.1977	-7.0545	
	(0.7314)	(1.6254)	(0.3691)	(0.6847)	(0.3838)	(0.4776)	(0.3463)	(0.5326)	(102.9015)	
Kleibergen-Paap underid. test p-value	0.176	0.015	0.472	0.120	0.062	0.214	0.028	0.554	0.101	
Kleibergen-Paap weak id. F-statistic	1.712	11.768	0.484	3.225	4.727	1.718	6.006	0.318	7.075	
Panel D: Country-Year FE										
ln($Chinese\,Aid_{\text{t-2}}$)	-1.0048	-2.2549	-0.3010	0.8156	0.1259	0.9374*	-0.4798	0.5000	1.3334	
	(0.8080)	(1.8415)	(0.3837)	(0.7330)	(0.4240)	(0.5226)	(0.4277)	(0.6383)	(2.8884)	
N	8700	8700	8700	8700	8700	8700	8700	8700	8700	
Kleibergen-Paap underidentification test p-value	0.232	0.011	0.674	0.095	0.064	0.530	0.043	0.626	0.173	
Kleibergen-Paap weak identification F-statistic	1.107	10.909	0.156	2.936	4.322	0.369	4.282	0.212	2.467	

Notes: Dependent variable: Category 1 binary conflict indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes African countries for the sampling period of 1995-2012 for the WB and 2000-2012 for Chinese Aid. Conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. Regressions account for (time-varying) exogenous controls and time trends. Time Trends include linear and squared country-specific time trends as well as a linear regional trend. AX - ”Agriculture, fishing, and forestry” BX - ”Public Administration, Law, and Justice” CX - ”Information and communications” EX - ”Education” FX - ”Finance” JX - ”Health and other social services” LX - ”Energy and mining” TX - ”Transportation” WX - ”Water, sanitation and flood protection” YX - ”Industry and Trade” Standard errors in parentheses, two-way clustered at the country-year and regional level: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-B3.

Interpretation: The table shows the corresponding OLS and IV estimation of Table 5-B28. Despite changes in coefficient size, the two estimation largely agree with one another in coefficient sign.

Table 5-B45 – OLS results: Population Weighted Aid Allocation

Panel A: WB Aid	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\ln(World\,Bank\,Aid_{\text{t-1}})$	-0.1898*	0.0062	-0.0440	-0.2217***	-0.2153***	-0.1664**	-0.1357	-0.1867**	-0.1829**
	(0.1005)	(0.0788)	(0.0692)	(0.0667)	(0.0663)	(0.0732)	(0.0840)	(0.0833)	(0.0909)
N	13104	13104	13104	13104	13050	13050	11699	13050	11699
Panel B: Chinese Aid									
$\ln(Chinese\,Aid_{\text{t-2}})$	-0.1776**	-0.0246	-0.0037	-0.1137**	-0.0718	-0.0696	-0.0723	-0.0390	-0.0482
	(0.0865)	(0.0704)	(0.0648)	(0.0576)	(0.0648)	(0.0728)	(0.0726)	(0.0891)	(0.0925)
N	9464	9464	9464	9464	8700	8700	8254	8700	8254
Country FE	No	Yes	Yes	–	–	–	–	–	–
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	–	–
Time Trends	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls × Year FE	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Linear Regional Trends	No	No	No	No	No	Yes	Yes	Yes	Yes
Lagged Endogeneous Controls	No	No	No	No	No	No	Yes	No	Yes
Country × Year FE	No	No	No	No	No	No	No	Yes	Yes

Notes: Dependent variable is a binary conflict indicator (100 if BRD≥5, 0 if BRD¡5). Standard errors in parentheses, two-way clustered at the country-year and regional level. The sample includes African countries for the sampling period of 1995-2012 for the WB and 2000-2012 for Chinese Aid. Conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-3.1.

Interpretation: To assign aid projects to each ADM1 region we assumed equal aid distribution across project localities in Table 5-3. This table shows that a weighting scheme based on the region's population size does not alter the coefficient significantly. Thus, the results are not based on specific aid allocation assumptions.

Table 5-B46 – ADM1 IV: Population Weighted Aid Allocation

Panel A: WB Aid	(1)	(2)
IV Second Stage: World Bank		
ln($World\,Bank\,Aid_{\text{t-1}}$)	-0.1026	-0.2286
	(0.3798)	(0.4256)
N	12325	12325
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	100.841	88.424
Panel B: Chinese Aid	**(1)**	**(2)**
IV Second Stage: China		
ln($Chinese\,Aid_{\text{t-2}}$)	-0.2613	-0.1903
	(0.4332)	(0.5305)
N	7975	7975
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	36.887	31.502
Country-Year FE	No	Yes

Notes: Dependent variable is a binary conflict indicator (100 if BRD≥5, 0 if BRD¡5). Standard errors in parentheses, two-way clustered at the country-year and regional level. The sample includes African countries for the sampling period of 1995-2012 for the WB and 2000-2012 for Chinese Aid. Both regressions include exogenous (time-varying) controls. Year and region fixed effects as well as time trends are included in all regressions. Time Trends include linear and squared country-specific time trends and a linear regional trend. The constituent term of the probability is depicted in the appendix. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Click here to go back to section 5-3.1.
Interpretation: To assign aid projects to each ADM1 region we assumed equal aid distribution across project localities in Table 5-3. This table shows that a weighting scheme based on the region's population size does not alter the coefficient significantly. Thus, the results are not based on specific aid allocation assumptions.

Both donors

Comparing both donors jointly comes at the disadvantage of losing five years of observations for the WB and - linked to this - a reduction of IV strength. Although the coefficients remain largely negative or insignificant in Tables 5-B47 (OLS) and 5-B48 (IV), the effects for the WB becomes less negative. Tables 5-B47 (OLS) and 5-B48 (IV) indicate that this is mostly driven by the different sampling years, rather than attributable to strong interactions between the two donors. It is important to see in Table 5-B48 that the respective first stages for both donors become weaker when trying to estimate them simultaneously, but the exogenous instruments remains significant for the respective donor (column 2). This further supports that the interaction terms capture a specific variation linked to the allocation process of the two donors, instead of general trends or conflict patterns in the receiving regions. Still, the K-P F-statistics of 3.5 in our preferred specification with country-year FE underlines why we chose to estimate both first stages separately.

Table A5-B49 and Table A5-B50 show that the OLS and IV results also hold when restricting the WB results to the years of Chinese aid data availability.

Table 5-B47 – OLS results - Both Donors

WB & Chinese Aid	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\ln(World\,Bank\,Aid_{\text{t-1}})$	-0.1460	0.0571	0.0808	-0.0603	-0.0973	0.0661	0.0674	-0.0793	-0.0948
	(0.1194)	(0.0951)	(0.0913)	(0.0864)	(0.0859)	(0.0866)	(0.0887)	(0.0884)	(0.0925)
$\ln(Chinese\,Aid_{\text{t-2}})$	-0.1278	-0.0291	0.0070	-0.1060*	-0.0660	-0.0656	-0.0644	-0.0345	-0.0367
	(0.0854)	(0.0700)	(0.0590)	(0.0595)	(0.0644)	(0.0727)	(0.0735)	(0.0884)	(0.0898)
N	8736	8736	8736	8736	8700	8700	8261	8700	8261
Country FE	No	Yes	Yes	–	–	–	–	–	–
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	–	–
Time Trends	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls $\times$ Year FE	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Linear Regional Trends	No	No	No	No	No	Yes	Yes	Yes	Yes
Lagged Endogeneous Controls	No	No	No	No	No	No	Yes	No	Yes
Country $\times$ Year FE	No	No	No	No	No	No	No	Yes	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD$\geq$5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.1.

Interpretation: The table shows the results when simultaneously including WB aid and Chinese aid in the regression equation. The coefficient for the WB aid remain negative, but are somehow smaller compared to Table 5-3 and lose statistical significance. The coefficients for Chinese aid remain largely unchanged. Table 5-B49 shows that the change for the WB is mainly driven to reducing the sample period by 5 years.

Table 5-B48 – ADM1 IV - Both Donors (Intensity 1)

	(1)	(2)
IV Second Stage: World Bank		
ln($World\,Bank\,Aid_{\text{t-1}}$)	-0.7029	-2.3839
	(1.0780)	(1.6965)
ln($Chinese\,Aid_{\text{t-1}}$)	-0.2482	-0.1655
	(0.4319)	(0.5415)
Kleibergen-Paap underidentification test p-value	0.000	0.005
Kleibergen-Paap weak identification F-statistic	11.573	3.489
IV First stage: World Bank		
$IDA\,Position_{\text{t-1}} \times \text{Cum. Prob}_{\text{t-2}}$	57.3235***	63.8053***
	(12.0425)	(24.1932)
$Chinese\,Commodity_{\text{t-3}} \times \text{Cum. Prob}_{\text{t-3}}$	-0.2181	-0.1051
	(0.6571)	(0.6166)
N	7975	7975
IV First stage: China		
$IDA\,Position_{\text{t-1}} \times \text{Cum. Prob}_{\text{t-2}}$	-17.9057*	-10.1067
	(9.3878)	(13.2890)
$Chinese\,Commodity_{\text{t-3}} \times \text{Cum. Prob}_{\text{t-3}}$	-13.9921***	-12.7060***
	(2.3178)	(2.2742)
N	7975	7975
Exogeneous Controls	Yes	Yes
Exogeneous Controls × Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country-Year FE	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. The constituent term of the probability is depicted in the appendix. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Click here to go back to section 5-6.1.
Interpretation: The table shows the results when simultaneously including WB aid and Chinese aid in the regression equation. The coefficient for the WB aid differ in size, but not in sign and significance, compared to Table 5-4. The coefficients for Chinese aid are largely unchanged. Table 5-B50 shows that the difference for the WB this is mainly due to reducing the sample period by 5 years.

Table 5-B49 – OLS results: (WB Aid - Same Years as Chinese Aid)

Panel A: WB Aid	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ln($World\ Bank\ Aid_{\text{t-1}}$)	-0.1505	0.0559	0.0811	-0.0606	-0.0976	0.0657	0.0717	-0.0795	-0.1004
	(0.1197)	(0.0949)	(0.0910)	(0.0864)	(0.0859)	(0.0865)	(0.0886)	(0.0884)	(0.0944)
N	8736	8736	8736	8736	8700	8700	8254	8700	8254
Panel B: Chinese Aid									
ln($Chinese\ Aid_{\text{t-2}}$)	-0.1753**	-0.0233	-0.0026	-0.1090*	-0.0663	-0.0654	-0.0682	-0.0347	-0.0441
	(0.0865)	(0.0705)	(0.0642)	(0.0572)	(0.0644)	(0.0726)	(0.0725)	(0.0883)	(0.0917)
N	9464	9464	9464	9464	8700	8700	8254	8700	8254
Country FE	No	Yes	Yes	–	–	–	–	–	–
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	–	–
Time Trends	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Exogeneous Controls × Year FE	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Linear Regional Trends	No	No	No	No	No	Yes	Yes	Yes	Yes
Lagged Endogeneous Controls	No	No	No	No	No	No	Yes	No	Yes
Country × Year FE	No	No	No	No	No	No	No	Yes	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Conflicts are considered for the WB from 2002 to 2013 due to the lag structure. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Interpretation: The table shows the results when restricting our estimations for WB aid to the years with available information for Chinese aid, which reduces the sample by 5 years. The coefficient for the WB aid are different compared to Table 5-3 and lose statistical significance.

Table 5-B50 – ADM1 IV (WB Aid - Same Years as Chinese Aid)

	(1)	(2)
Panel A: WB Aid		
IV Second Stage: World Bank		
$\ln(World\,Bank\,Aid_{\text{t-1}})$	-0.6227	-2.3417
	(1.0568)	(1.6897)
Kleibergen-Paap underidentification test p-value	0.000	0.005
Kleibergen-Paap weak identification F-statistic	22.619	6.960
IV First stage: World Bank		
$IDA\,Position_{\text{t-1}} \times \text{Cum. Prob}_{\text{t-2}}$	57.2759***	63.9080***
	(12.0429)	(24.2241)
N	7975	7975
Panel B: Chinese Aid		
IV Second Stage: China		
$\ln(Chinese\,Aid_{\text{t-2}})$	-0.2582	-0.1886
	(0.4282)	(0.5256)
N	7975	7975
Kleibergen-Paap underidentification test p-value	0.000	0.000
Kleibergen-Paap weak identification F-statistic	36.578	31.190
IV First stage: China		
$Chinese\,Commodity_{\text{t-3}} \times \text{Cum. Prob}_{\text{t-3}}$	-14.0193***	-12.6964***
	(2.3180)	(2.2734)
N	7975	7975
Exogeneous Controls	Yes	Yes
Exogeneous Controls × Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country-Year FE	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD¡5). The sample includes first order subnational regions in African countries for the 1995-2012 (WB) and the 2000-2012 period (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year and region fixed effects as well as time trends. Time Trends include linear and squared country-specific time trends. The constituent term of the probability is depicted in the appendix. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Interpretation: The table shows the results when restricting our estimations for WB aid to the years with available information for Chinese aid, which reduces the sample by 5 years. The coefficient for the WB aid are similar to those when including Chinese aid jointly, indicating that using both aid variables in separate equations in our main approaches does not introduce a large bias.

Chapter 6

Conclusion

This thesis analyzes different determinants of social unrest and violence of armed groups. More specifically, it asks whether climate shocks leverage grievances of economically disadvantaged groups. It also poses the question how competition between groups representing one ethnic group affects violence levels. Finally, it addresses the question if development aid promotes peace or fuel conflict.

The results show that ethnic inequality matters. Droughts are especially likely to trigger social unrest in regions with high ethnic between-group inequality but not so much for non-ethnic inequality measures. A validation study shows that individuals' perception of economic differences between their ethnicity and other groups correlates with our measure of spatial inequality between groups. Focusing on one specific ethnic group in Pakistan shows that armed violence is not conducted by one representative group, but group competition among the same ethnicity increases armed violence. Finally, there is no evidence that development aid of the World Bank and China contributes to civil war. However, Chinese aid seems to increase state repression and higher acceptance of autocratic rule.

Moreover, the thesis has two methodological contributions. First, we develop a novel inequality measure that can be decomposed into inequality between and within groups. We apply the measure to ethnic regions, but the index is flexible to accommodate any kind of groups and spatial dimensions. Second, the thesis develops a method to combine two well-known datasets of armed violence. The procedure eliminates double-counting of events and allows to approximate for government counter-insurgency measures.

The thesis points out that ethnic grievances are a crucial factor for social unrest and armed violence that is likely to worsen with climate change trends. A natural way to reduce the risk of social unrest induced by climate change is to shield the economy from these shocks. Climate change adaptation is a strategy that governments, bilateral, and multilateral donors promote along with climate change mitigation measures. Climate change mitigation and adaptation are necessary measures for countries, but completely eliminating the exposure from natural hazards is infeasible and, in most cases, economically inefficient.

Subsequently, addressing grievances are important as well. However, this thesis did not set out how to address these grievances. Also, social unrest and armed violence may occur because citizens see the social contract broken, as they cannot see that the government is providing them any benefits. In contrast, state repression may undoubtedly be able to smother social unrest but may not address underlying tensions. Without the possibility to vent anger, these tensions could build up over time and potentially lead to (armed) violence. Moreover, economic inequality is merely one facet that constitutes ethnic grievances. For example, an entire literature exists on the cause and consequences of ethnic exclusion from political participation.

Democratization is a process that many western donors like to see but could backfire in individual settings. Chua (2003) argues that, in an attempt to win voters, politicians may stir ethnic hatred against rich and market-dominant ethnic minorities when market liberalization and democratization occur simultaneously. Examples include ethnic violence against Indians in Southeast Africa or Chinese in the Philippines and Indonesia. In contrast, a community-driven approach in the design of development projects may reduce protests by building up accountability and improved delivery of public services (Barron et al., 2011). Therefore, democratic norms and values are a way to address grievances, including ethnic grievances. However, there are no one-size-fits-all solutions to address ethnic inequality and grievances. Fruitful future research may identify what dimension of the social contract is broken that stirs up social unrest and how to shape policies to empower ethnic regions that governments are willing to undertake to promote stability.

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