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Seeds of distrust: conflict in Uganda

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Abstract We study the effect of civil conflict on social capital, focusing on Uganda's experience during the last decade. Using individual and county-level data, we document large causal effects on trust and ethnic identity of an exogenous outburst of ethnic conflicts in 2002–2005. We exploit two waves of survey data from Afrobarometer (Round 4 Afrobarometer Survey in Uganda, 2000, 2008), including information on socioeconomic characteristics at the individual level, and geo-referenced measures of fighting events from ACLED. Our identification strategy exploits variations in the both the spatial and ethnic intensity of fighting. We find that more intense fighting decreases generalized trust and increases ethnic identity. The effects are quantitatively large and robust to a number of control variables, alternative measures of violence, and different statistical techniques involving ethnic and spatial fixed effects and instrumental variables. Controlling for the intensity of violence during the conflict, we also document that post-conflict economic recovery is slower in ethnically fractionalized counties. Our findings are consistent with the existence of a self-reinforcing process between conflicts and ethnic cleavages.

Keywords Acholi · Afrobarometer · Causal effects of conflict · Civil war · Ethnic conflict · Identity · Satellite light · Trust · Uganda

JEL Classification C31 · C36 · H56 · N47 · O55 · Z10

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1 Introduction

This paper investigates the effects of civil conflict on social capital, focusing on the experience of Uganda during the last decade. Civil conflicts have persistent devastating effects on economic development (Collier and Hoeffler 2004; Collier et al. 2009), and their legacy involves more than physical and human capital destruction. Civil conflicts often entail the persistent breakdown of civic and economic cooperation within society. We are motivated here by our recent theoretical work (Rohner et al. 2013), arguing that war leads to a collapse of trust and social capital which in turn sows the seeds of more ethnic conflict. Yet, there are also instances in which wars appear to cement rather than destroy cooperation. Historically, wars promoted nation building in Europe (Tilly 1975). The aftermath of World War II in Western Europe was characterized by strong institutional development involving social cooperation, renewed national identity and sustained high economic growth (Eichengreen 2008). Interestingly, Osafo-Kwaako and Robinson (2013) find no evidence that warfare is associated with future state building or political centralization in Africa. However, at a more micro level, Bellows and Miguel (2009) report evidence of positive social capital developments in Sierra Leone after the devastating civil conflict of 1991–2002.[1] The goal of this paper is to address two questions: First, is there evidence of causal effects of war on inter-ethnic trust? Second, how do such effects differ across different dimensions of trust and social capital?

We document causal effects of ethnic conflict on trust and ethnic identity using individual, county-level and district-level data from Uganda. An ethnic mosaic consisting of more than 50 groups, Uganda is a natural environment for such a micro-study. Ethnic conflicts have been pervasive since independence in 1962. Since 1985, Uganda has been ruled by the National Resistance Movement (NRM) led by Yoweri Museveni, whose main constituency is the Bantu-dominated South. His government has faced opposition and armed rebellion in several parts of the country, especially in the "Acholiland" region (in North Uganda), where the Lord's Resistance Army (LRA) was active until 2006, and close to the border with the Democratic Republic of Congo, where the insurgency led by the Allied Democratic Forces (ADF) was been active until 2004.

Our empirical strategy exploits an exogenous change in the policy against internal insurgency that occurred in 2001, after the September 11 terrorist attack. The declaration of the "war on terror" was a turning point. In earlier years, the international community had tried to promote negotiated settlements of the Ugandan conflicts.[2] In 2001, the US Patriot Act declared the LRA and the ADF terrorist organizations. Fearing retaliation, the ruling Sudanese National Islamic Front that had offered sanctuary and military help to the LRA until then, withdrew its support to the rebel army. Museveni's government seized this opportunity to launch a military crackdown on rebel armies in different fronts, particularly in the regions neighboring Sudan where the LRA had lost the logistic support from its basis in Sudanese territory. The ADF was soon annihilated and ceased any significant military activity within Uganda after 2004. Military action against the LRA started in March 2002, when the army launched "Operation Iron Fist" against the rebel bases in South Sudan. The LRA responded by attacking villages and government forces in Northern Uganda. Military activity and reprisals peaked in 2003. In 2005, the LRA moved its bases to the Democratic

[1] Bellows and Miguel (2009) use a household survey to analyze whether people who have been victimized in the civil war in Sierra Leone are affected in their post-war behavior. In particular, they find that more victimized people are more likely to "attend community meetings", and to "join social and political groups".

[2] An example of this strategy is the Amnesty Act of 2000, by which the Government of Uganda granted amnesty to all rebels who would abandon violence, renouncing to criminal prosecution or punishment for offenses related to the insurgency.

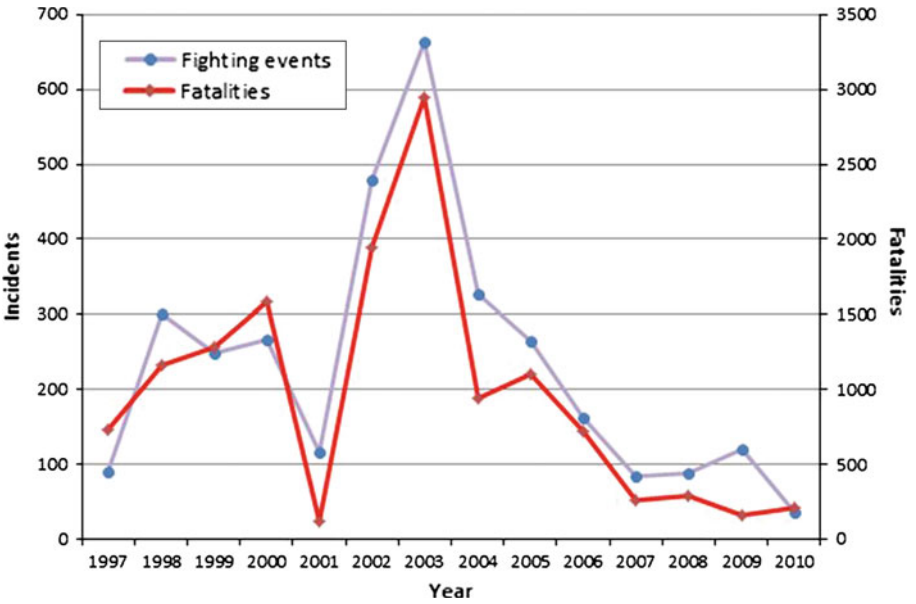

Fig. 1 The figure shows the annual number of fighting events (*left-hand scale*) and the number of fatalites (*right-hand scale*) in Uganda during 1997–2010. *Source*. ACLED (2011)

Republic of Congo, while the International Criminal Court issued an arrest warrant for its leader Joseph Kony. A cease-fire between the LRA and the government of Uganda was signed on September 2006, with the mediation of the autonomous government of South Sudan.

Figure 1 shows the total number of geo-referenced fighting events and of fatalities related to the conflict between 1997 and 2010 from Armed Conflicts Location Events Data (ACLED). Between 2000 and 2008 ACLED reports nearly 2,500 fighting events resulting in almost 10,000 fatalities. Consistent with the narrative above, there was a sharp increase in 2002–2005, followed by a decline, and very low levels of violence have been recorded after 2006. The escalation of violence in 2002–2005 is not merely an Acholi phenomenon. A large number of conflict episodes was recorded all over Uganda in this period (see Fig. 2).

We are interested in assessing the effects of this surge in violence on different measures of trust and ethnic identity. To do so, we exploit two waves of survey data from Afrobarometer (2000, 2008), a repeated cross section including information on various measures of trust and socioeconomic characteristics at the individual level.[3] Our strategy is to regress individual trust in year 2008 on spatial measures of intensity of fighting during 2000–2008, controlling for a large number of individual, ethnic and spatial characteristics. Most important, we control for the average trust at the district level in 2000, in order to filter out cross-district heterogeneity resulting from long-standing factors.[4]

[3] Although Afrobarometer also ran a survey in 2005, we decided to use the 2008 data for a variety of reasons. First, the number of conflicts was still large in 2005 (see Fig. 1). Second, we are interested in persistent effects of conflict on trust rather than in emotional reactions that may arise while the conflict is still ongoing. Last but not least important, there were still many refugees in 2005. This raises two issues. On the one hand, poor living conditions in refugee camps may affect trust reported by respondents. On the other hand, many people could be living in camps outside of their counties, rendering our identification strategy invalid.

[4] The district of the respondent is the most disaggregated geographical information provided by the 2000 Afrobarometer.

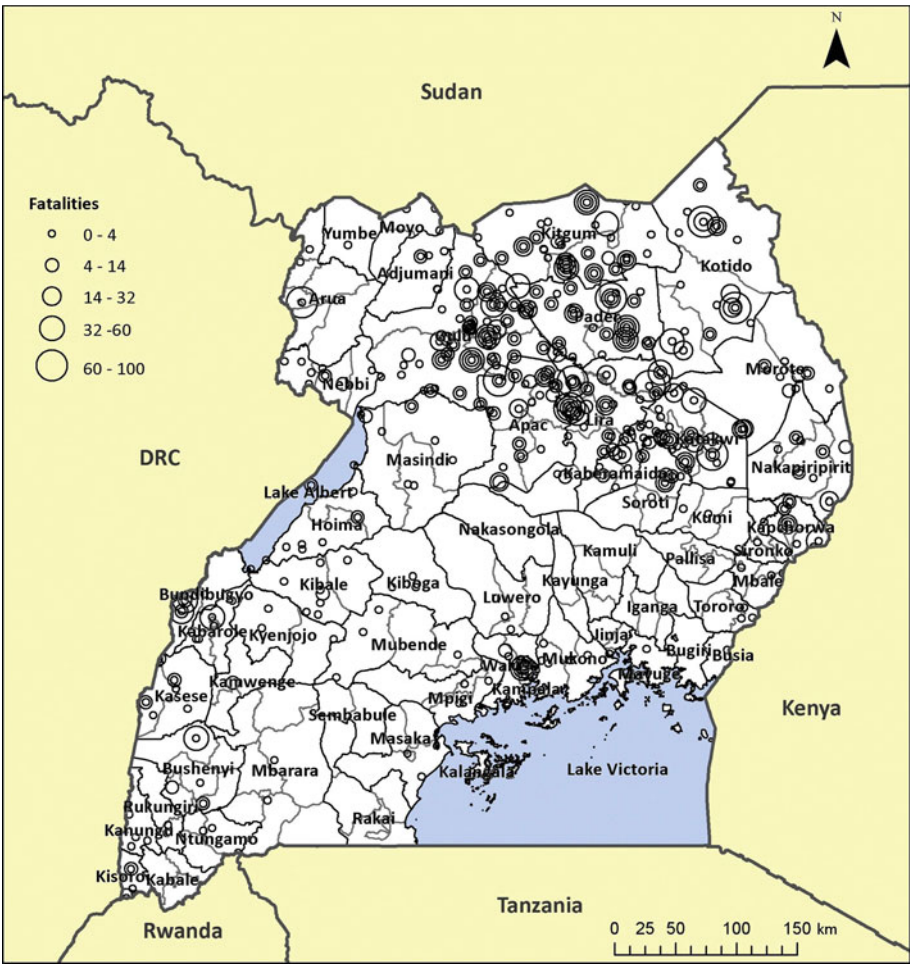

Fig. 2 The figure shows the districts and counties of Uganda and the location of fighting events. The *bold black lines* display the district borders whose names are also listed in the *map*. The *thin grey lines* show the county borders. The *circles* correspond to the locations of fighting events between 2000 and 2008 (from ACLED 2011)

We address concerns about reverse causality and omitted variables through two complementary strategies. First, we adopt an instrumental variables strategy. Our identification relies on an external political shock—i.e., the US declaring the main rebel movements of Uganda to be terrorist organizations, and the Khartoum government withdrawing support of those groups—affecting the intensity of fighting, but having no direct effect on trust measures. Since political shock affected the probability of fighting differentially across geographical areas, with a larger escalation of violence being observed close to the Sudanese border, we use the distance of each county from Sudan as an instrument for the number of fighting events.[5]

[5] Although this instrument is time invariant, our identification relies on the fact that such geographical characteristics affected the intensity of fighting after the September 11, 2001 shock. So, in a sense, our instrument captures an interaction between the political shock and the geographic characteristic.

We also consider an alternative strategy where the identification relies on the within-county variation in conflict intensity involving different ethnic groups. ACLED provides information about the rebel groups and ethnic militias that were involved in each conflict event. In most cases, these groups can be linked to ethnic affiliations. We can then regress our measures of trust on the number of fighting events involving different ethnic groups within each county, controlling for both county and ethnic group fixed effects. Our hypothesis is that respondents should be affected particularly by local events involving their own ethnic group.

Our main finding is that the intensity of fighting has a negative and statistically significant effect on "trust towards other people from Uganda". The estimated effect is quantitatively large, and robust to instrumenting fighting intensity by distance to Sudan. A one-standard-deviation increase in fighting (corresponding to 45 additional episodes of violence) translates into a 46 % standard deviation decrease in trust (corresponding to 22 percentage points). This is a very large effect, corresponding to about half of the difference between the Netherlands, the eighth most trusting country in the world, and the three countries with the lowest trust levels (Peru, Brazil and the Philippines).

The effect is stronger when fighting events involve the respondent's ethnic group. Fighting has no significant effects on "trust in known people" and on "trust in relatives", suggesting that fighting induces distrust mainly towards people outside the ordinary social network. Moreover, people living in counties experiencing more fighting report a large increase in a self-reported measure of "ethnic identity", i.e., they identify themselves more strongly with their own ethnic group relative to other forms of affiliation, including Ugandan nationality. This result is robust to the inclusion of ethnicity fixed effects. The response is stronger for people owning a radio, who are likely to be better informed about events associated with the conflict. Moreover, the results are not driven by the Acholi region, the most tormented by the conflict between the LRA and the government. Excluding all counties of core Acholiland does not affect the estimates.

In Rohner et al. (2013), we argue that by undermining trust, conflict hinders economic cohesion in ethnically divided societies. Although a thorough empirical investigation of this question would require a longer time span of data, in an extension we consider the economic effects of ethnic conflicts. Ideally, one would like to study how the dynamics of GDP per capita at the county level are affected by exposure to conflict. However, regional GDP data are not available for Uganda. We resort to proxying these with the average intensity of nighttime light recorded by U.S. meteorological satellites at the county level. We document an interesting interaction effect: given the intensity of fighting, post-conflict economic recovery depends on the ethnic fractionalization of each county. Fighting has a negative effect on the economic situation of highly fractionalized counties 4 years after the end of the conflict outburst, but has no effect on less fractionalized counties.

1.1 Related literature

This paper is part of a large literature on inter-ethnic conflict. Earlier contributions focus on characteristics of the political process (see, e.g., Horowitz 2000), while more recent formal theories study the effect of population characteristics (see, e.g., Esteban and Ray 2011; Rohner 2011). Different from these papers, our study suggests that ethnic identity may be endogenous relative to the conflict dynamics.[6]

[6] In this sense our paper is related to a recent literature studying endogenous ethnic and political identity in various contexts (see Balcells 2012; Caselli and Coleman 2013; Choi and Bowles 2007; Fryer and Levitt 2004; Posner 2004).

While we examine the effect of conflict on social capital, over the last decade a large empirical literature has studied the opposite channel, i.e., how different measures of ethnic diversity predict the outbreak of civil wars.[7] However, there is also a growing number of micro-level studies dealing with the impact of conflicts on human capital, in particular the educational attainment of cohorts exposed to war, in different countries (see Akresh and de Walque 2010; Blattman and Annan 2010; Leon 2012; Shemyakina 2011; Swee 2008). There is also a literature in medicine documenting that child soldiers or children who experienced war are more likely to experience depression and post-traumatic stress or anxiety (see Barenbaum et al. 2004; Dyregrov et al. 2000, and Derluyn et al. 2004).

The studies above focus on human rather than social capital. More directly related to our work is the recent literature on the effect of individual war experience on political participation and local collective action (see, e.g., Bellows and Miguel 2009; Blattman 2009, and Humphreys and Weinstein 2007). Besley and Reynal-Querol (2012) study the historical legacy of pre-colonial conflict in Africa and find that historical conflict is negatively correlated with trust levels today.

There is also a conflict-related literature based on lab and field experiments, including Fearon et al. (2009), Gilligan et al. (2010), Miguel et al. (2011), Voors et al. (2012), and Whitt and Rick (2007). Cassar et al. (2013) run experiments in Tajikistan and find that conflict exposure reduces trusting and fair behavior, especially in interactions with people from the same area. They explain this finding as due to the nature of the Tajik war, in which clear frontlines were absent and much violence took place within villages.

Our paper is related also to the literature linking trust and social capital in communities to past history and ethnic fragmentation.[8] While Alesina and La Ferrara (2000) finds that participation in social activities is lower in ethnically heterogeneous communities, Alesina and La Ferrara (2002) shows that a recent history of traumatic experiences and discrimination, poverty, low education and ethnic diversity correlate with low trust. Ashraf and Oded (2011) and Ashraf and Galor (2013) link cultural diversity of societies to their long-run development. They find that genetic diversity has a hump-shaped effect on comparative economic development: on the one hand diversity results in more distrust, lower coordination, less cooperation and more social unrest; on the other hand, a wider spectrum of traits makes is easier to implement advanced technological paradigms.

Using Afrobarometer and historical data, Nunn and Wantchekon (2011) find that individuals in sub-Saharan African countries whose ancestors belonged to ethnicities that were subject to a high intensity of enslavement report lower trust levels today. Our results are complementary to theirs. While they emphasize persistent effects of events that occurred long time ago, we show that large contemporaneous shocks can change beliefs and social capital. In a similar vein, Guiso et al. (2009) document that bilateral trust across countries depends on the number of years in which the two countries have been in war during the last millennium. Different aspects of the relationship between trust and growth are studied by Algan and Cahuc (2010) and Giuliano and Spilimbergo (2009). A number of papers document that business links are more stable between people of the same ethnic groups (Fafchamps 2000; Fisman 2003). These papers are related to the findings in our paper

[7] See Fearon and Laitin (2003), Collier and Hoeffler (2004), Collier and Rohner (2008), Collier et al. (2009), Montalvo and Reynal-Querol (2005) and Esteban et al. (2012).

[8] For a general discussion of the origins and effects of trust and social capital on economic development, see the survey articles of Doepke and Zilibotti (2013), Fehr (2009), Guiso et al. (2006), and Sobel (2002).

that fighting appears to have larger post-war economic effects in ethnically fractionalized counties.

Finally, our paper is related to the limited literature on the consequences of the conflict in Uganda. Bozzoli et al. (2011) analyzes the effect of conflict on individual expectations in Northern Uganda. Their paper is complementary to ours insofar as it documents the effect of differential exposure to conflict. However, they use a different dataset (the Northern Uganda Livelihood Survey) which covers only the population living in six Northern districts. This survey is only available for 2007, so pre-conflict attitudes cannot be controlled for. Using also data from Northern Uganda, Fiala (2013) analyses the economic consequences of being internally displaced. A recent paper by De Luca and Verpoorten (2011)—carried out independently, and posterior to the first version of our paper—studies the effect of conflict in Uganda on associational membership and trust.[9] Deininger (2003) analyzes household survey data for Uganda and finds that households more heavily affected by civil strife are less likely to engage in (non-farm) enterprise expansion or startup and are more likely to close down an existing enterprise. Vargas Hill et al. (2008) document that in Uganda agricultural "cooperatives were much less likely (…) to exist in communities that had recently experienced civil conflict".

Section 2 provides an overview of the historical context of the Ugandan conflict. Section 3 describes the data and empirical strategy. Section 4 discusses the main empirical results regarding the effect of conflict on measures of trust and ethnic identity. Section 5 performs some robustness checks. Section 6 analyzes two important extensions focusing, respectively, on spatial×ethnic variation in violence, and the economic effects of ethnic conflict. Section 7 concludes. A number of additional statistics and robustness tests and a detailed data description are found in the Appendix.

2 Context of conflict in Uganda

Since pre-colonial times the area of what is Uganda today has been characterized by a great ethnic diversity. The main dividing line runs between the Nilotic people of the North, and the Bantu-dominated South. These ethnic identities were fostered by the British colonization as part of a divide-and-rule strategy. For instance, the colonial administration restricted inter-ethnic movements. While Nilotic ethnic groups (and in particular the Acholi) were over-represented in the army, they were under-represented in the administration and white-collar jobs, and generally discriminated against (Nannyonjo 2005).

Even after independence in 1962, Ugandan politics remained dominated by ethnicity, with each leader favored some groups, and repressed others. Uganda's first prime minister, Milton Obote, was overthrown by Idi Amin in 1971, whose regime was hostile to Acholi soldiers, perceived to be Obote's agents. After Amin, it was again the turn of Obote to rule the country, who was followed by Acholi officer Tito Okello. During this period, the dominant position of northerners in the army was re-established, only to be dismantled again when Okello lost power in 1986 to the former rebel leader of the National Resistance Army (NRA) and current President of Uganda, Yoweri Museveni, a southerner (Finnström 2008). The northern (and in particular, Acholi) ex-officers and

[9] This study uses a different econometric specification that does not control for past trust (which play a key role in our identification), nor does it consider ethnic identity. It is based on Afrobarometer 2005, whereas we prefer to use Afrobarometer (2008) for reasons explained in detail below. Finally it emphasizes different outcome variables, and does not link fighting events to specific ethnic groups.

soldiers of the Ugandan army fell again from grace, and have since played an important role in the various Northern-based rebel movements. In 1987 Joseph Kony started his own militia drafting mostly Acholi deserters. This movement eventually became, in 1994, the most important and persistent rebel movement of Uganda, under the name of LRA.

Although over time the LRA has intensified criminal activities and often attacked villages inhabited by people from their own ethnic background—either to prosecute alleged traitors, or to force the recruitment of child soldiers—the conflict has ethnic roots.[10] According to Nannyonjo (2005, p. 475), "the current conflict in the Acholi and Lango sub-regions between the LRA and the Ugandan government has deep historical roots resulting from ethnic hostilities…". This view is echoed by Finnström (2008, pp. 74–75), "the majority of people in central Uganda perceived Museveni's war as a war against a regime of northerners, rather than the war for democracy. (…) In Museveni's war propaganda, the enemy was alleged to be northerners in general and Acholi in particular". Similarly, the Women's Commission (2001, p. 81) argues that "the current conflict in northern Uganda has its roots in ethnic mistrust between the Acholi people and the ethnic groups of central and southern Uganda as well as in the religious and spiritual beliefs of the Acholi people and the manipulation of these beliefs." The civil population in the North suffered abuses from both the LRA and the government troops (Dolan 2009).[11] Interestingly, the primary blame and grievances kept being directed mostly against the Kampala government and the southern Bantu-speaking ethnicities that it represents (Finnström 2008).

The role of Sudan is especially important. Since the early 1990s, the Khartoum government had provided the LRA with logistic support and military equipment, allowing it to hold base camps in southern Sudan. In exchange, the LRA helped the Sudanese army to fight the south Sudanese rebels. The Ugandan government, in turn, supported the Sudan People's Liberation Army. Reciprocal accusations led the two governments to sever diplomatic relationships in 1995. In early 1999, former US President Jimmy Carter chaired negotiations to restore these ties (see Neu 2002). Progress was slow until September 11, 2001, when the Sudanese government came under heavy international pressure. In 2002 Uganda and Sudan restored diplomatic relations and signed a protocol giving the Ugandan army the right to enter southern Sudan and attack the LRA.

Besides this major violent conflict between the southern government and the northern rebels of the LRA, in recent years there have been several other smaller-scale ethnic conflicts in Uganda. For example, the rebels of ADF have been fighting the government in southwestern Uganda, and there has been widespread ethnic violence in the northeastern Karamoja region triggered by cattle raiding (Nannyonjo 2005; Finnström 2008).

[10] According to Finnström (2008), the Museveni government has tried hard to frame the LRA as non-politically motivated criminals who attack their own people. In particular, "the rhetoric of a local northern conflict in which Acholi kill fellow Acholi like cannibalistic grasshoppers, reflects a more general Ugandan conception of the Acholi as violent and war-prone" (Finnström 2008, p. 107).

[11] "The conduct of the Museveni's troops (…) soon deteriorated. Killings, rape, and other forms of physical abuse aimed at noncombatants became the order of the day soon after the soldiers established themselves in Acholiland, which was foreign territory for them" (Finnström 2008, p. 71).

3 Econometric analysis

3.1 Data sources

Our main data source is the Afrobarometer (2008) survey on Uganda, in which 2431 subjects were surveyed between July and October 2008, in 55 districts and 125 counties of Uganda.[12] Each respondent is associated with a district and a county of residence, as well as with an ethnic group. We also use information from Afrobarometer (2000). Note that the smallest geographical unit included in the 2000 survey is the district. Thus, we can only construct our control variables from this data source (particularly, past trust and living conditions) at the district level.

The other major data source is the ACLED (Armed Conflict and Location and Event Dataset 2011) dataset, which provides precise geo-location of various categories of fighting events. In Afrobarometer, we ignore the precise geo-location of respondents. Using ArcGIS, we consequently aggregate fighting events both at the county- and district-level and match them with the county and district of residence of Afrobarometer respondents.

All variables are described in detail in the Data Appendix, and the descriptive statistics of all variables used are contained in Table 16 in the Appendix. We describe here the main variables.

3.2 Main variables

3.2.1 Dependent variables

We use mainly two questions from Afrobarometer (2008) to construct the following dependent (binary) variables at the individual level:

- *Generalized trust*: "How much do you trust each of the following types of people: Other Ugandans?" (question Q84C). The variable takes the value one if the respondent answers either "I trust them somewhat" or "I trust them a lot". Otherwise, the value is set to zero.
- *Ethnic identity*: "Let us suppose that you had to choose between being a Ugandan and being a _ [R's Ethnic Group]. Which of the following best expresses your feelings?" (question Q83). The variable takes the value one if the respondent answers either "I feel only (R's ethnic group)" or "I feel more (R's ethnic group) than Ugandan". Otherwise, the value is set to zero.

In Sect. 5.4, we also consider the following two alternative questions:

- *Trust in known people*: "How much do you trust each of the following types of people: Other people you know?" (question Q84B). The variable takes the value one if the respondent answers either "I trust them somewhat" or "I trust them a lot". Otherwise, the value is set to zero.
- *Trust in relatives*: "How much do you trust each of the following types of people: Your relatives?" (question Q84A). The variable takes the value one if the respondent answers either "I trust them somewhat" or "I trust them a lot". Otherwise, the value is set to zero.

[12] Afrobarometer selects samples in the following way: "The sample is designed as a representative cross-section of all citizens of voting age in a given country. The goal is to give every adult citizen an equal and known chance of selection for interview. We strive to reach this objective by (a) strictly applying random selection methods at every stage of sampling and by (b) applying sampling with probability proportionate to population size wherever possible (…). The sample is stratified by key social characteristics in the population such as sub-national area (e.g. region/province) and residential locality (urban or rural)" (Afrobarometer 2008).

In Sect. 4 we denote our dependent variable by $TRUST^{08} \in \{$*Generalized trust, Ethnic identity, Trust in known people, Trust in relatives*$\}$. In Sect. 6.2, we run a regression where the dependent variable is a proxy for the level of economic activity. In particular, we use *satellite nightlight*, a county-level measure of the average nighttime light intensity. We constructed this measure with the help of ArcGIS, using the geo-referenced county border and the geo-referenced Satellite Nightlight Data from the National Oceanic and Atmospheric Administration (2010). These data have been used in recent research as a proxy for economic activity (see, e.g., Henderson et al. 2012; Hodler and Raschky 2011).

3.2.2 Main explanatory variables

We use four alternative explanatory variables with variation at the county-level (at the district-level in several specifications), $FIGHTING_c^{00-08} \in \{$*All Fighting, Violence Against Civilians, Battles, Internally Displaced People*$\}$. All variables code fighting events taking place between the last day of the Afrobarometer (2000) survey (on June 26, 2000) and the first day of the Afrobarometer (2008) survey (on July 27, 2008).

- *All Fighting* (main explanatory variable): Total amount of all violent events in a county. It corresponds to the sum of the events of the following "Event Type" in ACLED: "Battle-Government regains territory", "Battle-No change of territory", "Battle-Rebels gain territory", "Riots/Protests", and "Violence against civilians".
- *Violence Against Civilians*: Total number of events coded as "Violence against civilians" in ACLED.[13]
- *Battles*: Total number of events coded as "Battle-Government regains territory", "Battle-No change of territory", and "Battle-Rebels gain territory" in ACLED.
- *Internally Displaced People (IDP)*: Total number of IDP per district in 2006 from UNHCR (2006).

As default in most specifications, we focus on the *number of events* of the three fighting variables above (All Fighting, Violence Against Civilians, and Battles), we also run as robustness checks the corresponding regressions for these three fighting categories, but focusing on the *number of fatalities* taking place in the fighting events of a given category.

In an alternative specification (Sect. 6.1), we use the information provided by ACLED to match (whenever feasible) each event coded in *All fighting* to a particular ethnic group according to the classification of Afrobarometer (2008) (Q79). In this alternative specification, *All fighting* varies on the ethnic group level, and corresponds to the total number of violent events linked to a group.

3.2.3 Primary control variables

We define as "primary" control variables the ones that have a key role in our identification strategy, since (as explained below) these allow us to filter out heterogeneity in the pre-treatment stage. The primary control variables is a vector of trust/identity variables from Afrobarometer (2000), denoted by $\mathbf{TRUST^{00}} = \{$*Generalized trust 2000, Trust in Known People 2000, Trust in relatives 2000, Ethnic identity 2000*$\}$. The variation of $\mathbf{TRUST^{00}}$ is at the district level.

[13] Examples of violence against civilians in the ACLED database for Uganda include e.g. different ethnic clans attacking each other in cattle raids, rebel ambushes of passenger vehicles, or rebel raids against villages supposed to support the enemy.

The questions asked in Afrobarometer (2000) were not identical to those asked in Afrobarometer (2008). The exact construction of the 2000 variables is deferred to Appendix B. In Sect. 6.2, the dependent variable is *satellite nightlight,* and we control for its analogue in year 2000.

3.2.4 Ethnic control variables

In some tables, we also control for a number of ethnic-specific time-invariant control variables:

- *Slavery* is borrowed from Nunn and Wantchekon (2011). It measures the number of people who were enslaved during the slave trade period (1400–1900) in each ethnic group, normalized by the area of land inhabited by the group during the nineteenth century. This is Nunn and Wantchekon's preferred measure of slave trade incidence.
- *Hunting* indicates the traditional ethnic-specific dependence on hunting (including trapping and fowling). This variable is borrowed from Michalopoulos and Papaioannou (2013)—as are the three variables listed below. It corresponds to variable v2 of the Ethnographic Atlas of Murdock (1967). The variable is measured on a cardinal scale between 0 and 9, where a larger number means more dependence (the same scale is used for the three variables listed below).
- *Fishing* indicates the traditional ethnic-group specific dependence on fishing (including shell fishing and the pursuit of large aquatic animals). It corresponds to variable v3 of the Ethnographic Atlas of Murdock (1967).
- *Animal husbandry* indicates the traditional ethnic-group specific dependence on animal husbandry. It corresponds to variable v4 of the Ethnographic Atlas of Murdock (1967).
- *Agriculture* indicates the traditional ethnic-group specific dependence on agriculture (including penetration of the soil, planting, tending the growing crops, and harvesting). It corresponds to variable v5 of the Ethnographic Atlas of Murdock (1967).

Note that together with the omitted category "Gathering", the scores of the activities "Hunting", "Fishing", "Animal husbandry" and "Agriculture" sum up to 100 % of the traditional food dependence.

3.2.5 Other control variables

All regressions include a vector of individual sociodemographic controls (**X**) from Afrobarometer (2008), consisting of age, education, employment status, gender, rural/urban location, religion and ownership of a radio and of a TV; and a vector of district-level controls (**Z**) including population, urbanization rate, demographic structure, share of manufacture, share of subsistence farming, net migration, fertility, number of micro-enterprises, and unemployment, all of which are from the Census of the Ugandan Bureau of Statistics (2002). These data are not available at the county level. Further, we use the Geo-Referenced Ethnic Group (GREG) dataset, which allows us to compute ethnic fractionalization measures at the county level (Weidmann et al. 2010). Finally, we proxy for living conditions in 2000 using the county-level average satellite nightlight intensity, computed based on data from satellites of the National Oceanic and Atmospheric Administration (2010).

3.3 Empirical strategy

We consider the following benchmark econometric model:

$$\mathbb{P}(TRUST_{i,c,e}^{08} = 1) = \Phi\big[a_0 + a_1 FIGHTING_c^{00-08} + \mathbf{TRUST}_d^{\mathbf{00}\prime}\alpha + \mathbf{ETHNIC}_e'\beta + \mathbf{X}_i'\boldsymbol{\gamma} + \mathbf{Z}_c'\boldsymbol{\delta}\big] \quad (1)$$

where i denotes an individual, c a county (where a county is a sub-unit of a district, d), and e an ethnic group.

We will estimate (i) Probit maximum likelihood models and (ii) linear probability models using either the ordinary least squares (OLS) or the two-stage least squares (2SLS) estimator, in presence of instrumental variables. Hence, Φ in Eq. (1) is either the *cdf* of a standard normal distribution (in the Probit model) or the identity function. $TRUST^{08}$ yields the different measures of trust/identity from Afrobarometer (2008). $FIGHTING_c^{00-08}$ is our main explanatory variable. In the set of tables below, we always report the estimated coefficient a_1 capturing the effect of county-level fighting on trust/identity. In some specifications we change the scale of analysis by considering $FIGHTING_d^{00-08}$, a measure of fighting at the district rather than at the county level. We also consider an alternative independent variable, $FATALITIES_c^{00-08}$, counting the number of casualties (as opposed to the number of fighting events) for the same categories of violence as for the $FIGHTING_c^{00-08}$ variable. $FATALITIESc^{00-08}$ may be a more precise treatment measure, since it is correlated with the conflict intensity.

The primary control variables $\mathbf{TRUST}_d^{\mathbf{00}}$ (a vector) is designed to filter out heterogeneity in the pre-treatment measures of trust at the geographic or ethnic group level. This variable plays a key role in our identification strategy. Ideally, since our aim is to identify the causal effect of shocks taking place between the two Afrobarometer surveys, we would like to control for individual measures of trust in 2000. However, this is not possible since Afrobarometer is not a panel at the individual level. Filtering out the effect of past trust at the district level, $\mathbf{TRUST}_d^{\mathbf{00}}$ yields the best approximation of this ideal specification. Since part of the time-invariant heterogeneity may be rooted at the ethnic rather than at the geographical level, we filter out heterogeneity in long-term trust across ethnic groups by a set of ethnic-specific control variables $\mathbf{ETHNIC}_e$. These include *Slavery* following Nunn and Wantchekon (2011) who show that an ethnic history of enslavement has a large and significant explanatory power on the average level of trust exhibited by people belonging to different ethnic groups in Afrobarometer 2005. In addition, we control for the traditional ethnic-specific dependence on the traditional activities of *hunting, fishing, animal husbandry* and *agriculture* from Michalopoulos and Papaioannou (2013), as described above. Finally, in some specifications we include ethnic fixed effects. In this case, we omit ethnic controls, since these are collinear with the fixed effects.

We introduce a set of additional individual sociodemographic control variables ($\mathbf{X}_i$) and county- (when available) or district-level controls ($\mathbf{Z}_c$) to filter out additional sources of heterogeneity. All district-level controls are from the Census 2002, and therefore are measured before the outburst of conflict in 2002–2005. This reduces concerns about their endogeneity. We also control for ethnic fractionalization and for nightlight measured using satellites for the year 2000 at the county level. We allow for intracluster correlation of the error terms both in the spatial and ethnic dimensions.

OLS and Probit regressions may yield inconsistent estimates of a_1, due to either reverse causality or omitted variables bias. We address this concern through an instrumental variable strategy. Concern about reverse causality is mitigated by the fact that our dependent variable

is measured in 2008, 3 years past the end of active fighting. This is one of the reasons why we do not focus on Afrobarometer 2005, which surveys Ugandan people while fighting is either still ongoing or a very recent experience (see Fig. 1). However, reverse causality cannot be ruled out completely if variables are serially correlated. Perhaps more importantly, unobservable shocks occurring after year 2000 may be driving both trust and fighting. To this aim, we instrument $FIGHTING_c^{00-08}$ by a county-level geographic characteristic that is correlated with the fighting intensity, while having, plausibly, no direct effect on trust. We focus in particular on the *Distance to Sudan*.[14] This is a natural instrument, since Southern Sudan played a crucial role in the 2002–2005 military escalation. In particular, before 2001 this region used to be a safe haven for rebel movements—most notably for the LRA. However, the events following September 11 forced the Sudanese government to withdraw its support of the LRA and to let the Ugandan army attack the LRA bases in Sudanese territory. This triggered the response of the LRA with repeated incursions, looting and engagements with the army within the Ugandan territory.[15]

Our exclusion restriction requires the error term to be uncorrelated with the instrument. In this respect, it is important to remember that our primary control variables ($\mathbf{TRUST}_d^{00}$), and the ethnic controls, should filter out the long-run correlation between our instrument and potential omitted factors. For instance, if counties (or ethnicities) neighboring Sudan were less inclined to trust and cooperation due to unobserved historical or cultural factors, these factors might have a direct effect on $TRUST^{08}$. However, they would also affect $\mathbf{TRUST}_d^{00}$, and as long as their influence did not change after 2000 (other than due to fighting), the instrument would be uncorrelated with the omitted variables conditional on the observables—which include $\mathbf{TRUST}_d^{00}$. To the opposite, problems would arise if the error term included time varying shocks that are correlated with the geographical variables. An example might be a weather shock during the period 2000–2008. However, we could not find evidence of any such remarkable event. In Sect. 6.1 below, we consider a more demanding identification where we control for ethnic and county-level fixed effects.

Finally, a possible concern is conflict-induced migration: in 2008, some people may be living in different counties from where they lived at the time of the conflict when massive forced population displacements occurred. However, this issue is quantitatively minor. First, by 2008 the majority of displaced people had returned to their home villages (see UN 2009; UNHCR 2010). In contrast, the problem would have been important if we had used Afrobarometer 2005, since the number of people living in refugee camps peaked at 1.8 millions in 2005. This is one of the main reasons why we rely on Afrobarometer 2008. Second, most movements took place within counties. People were forced to move from rural areas to so-called "protected villages" established mostly in local trading centers (UNOCHA 2002; Médecins sans frontiéres 2004). As a result, cross-county migration is altogether modest. Given that our main explanatory variable is defined at the county-level, the results are unlikely to be contaminated by cross-county conflict-induced migration.

[14] We construct this variable by computing with ArcGIS the minimum distance between the geo-referenced border of a given county and the geo-referenced border of Sudan.

[15] If we had a longer span of data and a full dynamic model, the instrument would be the interaction between September 11 and "distance to Sudan". Note that "distance to Sudan" could have a direct permanent effect on trust (if, e.g., Acholi people trust the Kampala government less than do people in the rest of Uganda). However, this effect is filtered out by $TRUST_d^{00}$. See the discussion below.

Table 1 Effect of fighting on generalized trust in 2008

	Dependent variable: generalized trust in 2008					
	(1)	(2)	(3)	(4)	(5)	(6)
All fighting	−1.97***	−1.06**	−0.85*	−0.49***	−0.32***	−0.28***
	(0.50)	(0.52)	(0.51)	(0.12)	(0.10)	(0.10)
Fighting variable	Events	Events	Events	Fatalities	Fatalities	Fatalities
Ethnic controls	No	Ethnic variables	Ethnic FE	No	Ethnic variables	Ethnic FE
Method	Probit	Probit	Probit	Probit	Probit	Probit
Observations	2,242	2,131	2,234	2,242	2,131	2,234
Pseudo R^2	0.101	0.116	0.146	0.102	0.118	0.148

The unit of observation is an individual. Robust standard errors in parenthesis (adjusted for clustering at county level). Significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. All specifications control for unreported individual sociodemographics (Age, Education, Employed, Gender, Rural, Own TV, Own Radio, 17 Religion Fixed Effects), districts characteristics at the beginning of the period (Past Generalized Trust, Past Trust in Own Group, Past Ethnic Identity, Population, Urbanization, AgeDependencyRatio, Share of Manufacture, Share of Subsistence Farming, Net Migration, Number of MicroEnterprises, Adjusted Total Fertility Rate, Unemployment Rate), and county characteristics at the beginning of the period (Ethnic Fractionalization, Nightlight)

4 Results

Table 1 presents the results of a set of probit estimations. We only report the estimated marginal effects of the main coefficient of interest, i.e., $FIGHTING_c^{00-08}$ in columns (1)–(3), and $FATALITIES_c^{00-08}$ in columns (4)–(6). The regressions in columns (1) and (4) control only for county (or district) and individual characteristics. The results show a significant negative effect of fighting on general trust. Controlling for time-invariant ethnic characteristics that can affect trust reduces the estimated effect from −1.97 to −1.06 in column (2) and from −0.49 to −0.32 in column (5). Finally, in columns (3) and (6) we control for ethnic fixed effects. This is a very demanding specification because it identifies exclusively within-ethnicity effects of conflict, while in reality an important effect of conflict may be to exacerbate ethnic rivalries. Not surprisingly, the marginal effects are now smaller: ethnic fixed effects absorb about half of the effects in columns (1) and (4). However, the estimated coefficients remain statistically significant, at the 10 % level in the case of fighting and at the 1 % level in the case of fatalities.

The effect of our primary control variable, $\mathbf{TRUST}_d^{00}$, (coefficients not reported in Table 1) is interesting. *Generalized trust* is highly positively correlated with its district-level counterpart in Afrobarometer (2000) (which is, recall, a component of the vector $\mathbf{TRUST}_d^{00}$): the regression coefficient of "*Generalized trust 2000*" ranges between 0.79 and 1.44 across the different specifications, and is always significant at the 1 % level. Such a high autocorrelation is reassuring, as it suggests that $\mathbf{TRUST}_d^{00}$ indeed filters out well the pre-conflict level of trust.[16]

[16] The coefficient of *Slavery* in columns (2) and (5) is, as expected, consistently negative: individuals belonging to groups highly exposed to enslavement in the eighteenth century report a lower *Generalized trust* in 2008, *ceteris paribus*. The point estimates range between −0.65 and −0.66, being on the margin of standard levels of statistical significance (the p-values range between 0.116 and 0.128 across the different specifications). The fact that the effect of slavery is smaller than in Nunn and Wantchekon (2011) is not surprising, since our regressions control for trust in 2000 which filters out most of the long-term variation. Consistent with this interpretation, *Slavery* becomes statistically significant if we omit $\mathbf{TRUST}_d^{00}$.

In summary, the probit regressions show that people living in counties where fighting has been more intense and has caused a higher number of fatalities turned on average less trustful towards other Ugandans relative to year 2000. The effect is robust to the inclusion of several controls and ethnic fixed effects.

Since there are concerns of reverse causality or omitted variables bias (as discussed above), Table 2 reports the results of an instrumental variable method. We only report the estimated marginal effects of the main coefficient of interest, i.e., $FIGHTING_c^{00-08}$ in Panel A, and $FATALITIES_c^{00-08}$ in Panel B. In columns (1)–(3) of Panel A (Panel B) we report the results of the same specification as in columns (1)–(3) (columns (4)–(6)) in Table 1, using a OLS regression. The coefficients of the OLS regressions are similar in magnitude to the corresponding marginal effects of the Probit model. Note that using OLS and 2SLS also allows us to two-way cluster standard errors at the ethnic group and county level, which we do throughout the paper for all OLS and 2SLS regressions. Columns (4)–(6) of Panel A (Panel B) run the same specification as in columns (1)–(3) (columns (4)–(6)) in Table 1, but using a 2SLS regression. Columns (7)–(8) in Panel A (Panel B) report the results from 2SLS regressions using different measures of the intensity of fighting (fatalities) as the primary regressors. Further, in column (9) of Panel A we use IDP as primary regressor (since this regression has no counterpart with fatalities, Panel B has only eight columns).

The coefficients of *All fighting* in the 2SLS regressions are negative and significant. The results are robust to the alternative measures of fighting, including *Violence Against Civilians* (column (7) of Panel A) and *Battles* (column (8) of Panel A). It is also robust to using the same measures, though it counts the number of fatalities involved as opposed to the number of events, as shown in Panel B. Finally, in column (9) of Panel A we show that the results are also robust to replacing the measure of fighting intensity with the number of IDP.[17] We interpret the larger coefficients in the 2SLS specifications with respect to their OLS counterparts as originating from two related sources. First, the OLS may suffer from an attenuation bias in the OLS regressions due to measurement error. Second, the OLS coefficient corresponds to the average effect of the number of fighting events, $FIGHTING_c^{00-08}$. However, trust and ethnic identity are likely to respond to the intensity of the treatment (violence), which varies across counties. For instance, the county-level average fatalities per fighting event is highly negatively correlated with our instrumental variable, distance to Sudan, the correlation coefficient being −0.29. This observation suggests that even other non-observable dimensions of violence intensity (such as looting, kidnapping, permanently injured people, etc.) are likely to be correlated with our geographical instrument. More generally, if each fighting event (even each fatality) is associated, on average, with more intense violence in counties close to Sudan, this can explain why the 2SLS coefficients are larger than the OLS ones, which are based on an average effect.[18]

We also checked for possible selection problems following the procedure suggested by Altonji et al. (2005) aimed to gauge the amount of selection on unobservable characteristics based on the amount of selection on the observed explanatory variables.[19] This allows one

[17] We include IDP for two reasons: First, they are a proxy of fighting intensity. Second, forced displacements can be viewed as a deliberate military strategy in conflict (cf. Esteban et al. 2011). Indeed, some authors see the protected villages for IDP in Uganda as part of an aggressive military strategy pursued by the Museveni government to control and oppress the civilian population in the North (Finnström 2008; Dolan 2009).

[18] Consistent with this interpretation, the bias of the OLS coefficient is smaller when we measure violence by the number of fatalities than when we use the number of fighting episodes, see Panel b of Table 2. The reason is that fatalities is a better (albeit imperfect) measure of the intensity of violence.

[19] We run two regressions: one with a restricted set of control variables and one with a full set of controls. The restricted set of controls consists of the primary controls, $\mathbf{TRUST}_d^{\mathbf{00}}$ and $\mathbf{ETHNIC}_e$ (i.e., we exclude

to assess how severe the omitted variable bias should be for the effect of fighting to be driven fully by unobserved characteristics. We find no indication that our results arise, spuriously, from a selection on unobservables. To the opposite, adding control variables appears to increase (in absolute value) the size of the estimated coefficient, suggesting that our result would be strengthened if we could control for more unobservable variables.

4.1 Ethnic identity

To test more directly whether conflicts affect inter-ethnic attitudes, we replace our measure of trust by *Ethnic identity*, i.e., the proportion of respondents who identify themselves primarily with their ethnic affiliation. The results are reported in Tables 3 and 4, corresponding to Tables 1 and 2, respectively. The estimated coefficient of interest is always positive and in most cases highly significant.[20] As in the case of *Generalized trust*, the coefficients in the 2SLS regressions are significantly larger than their OLS counterpart. Violence strengthens the identification of Ugandans with their own ethnic group.

4.2 First stage regression

Table 5 reports the coefficients of the excluded instruments in the first-stage regressions of 2SLS specifications from Table 2. In particular, columns (1)–(5) (columns (6)–(10)) in Table 5 correspond to the regressions of columns (4)–(8) in Panel A (Panel B) of Table 2; finally column (11) in Table 5 corresponds to the regression of columns (9) in Panel A of Table 2. In all cases the IV coefficients have the expected sign and are highly significant. Robust (Kleibergen–Paap) F-statistics accounting for clustered residuals are large, and in most cases above the conventional threshold for weak instruments. The specifications with ethnicity fixed effects are generally more problematic, and the F-statistics show in some cases possible weak instrument problems. This is not surprising, since ethnic groups are spatially clustered, limiting the explanatory power of the geographical excluded instrument in the first-stage regression. Nevertheless, it is reassuring that the F-statistics are well above ten when the intensity of fighting is measured by the number of fatalities (columns (6)–(10)).

One should recall here, though, that the standard Stock-Yogo critical values for weak instruments are calibrated for the case of i.i.d. residuals, and do not apply to the case of clustered standard errors (see, e.g., Bun and de Haan 2010). Therefore, the F-statistics provide no precise diagnostic of the weak instrument problem.

Footnote 19 continued
$\mathbf{X}_i$ and $\mathbf{Z}_d$ in Eq. 1)—both are essential constituents of our econometric specification. Then, we calculate the ratio $|\hat{a}_1| / \left(\left|\hat{a}_1^R\right| - |\hat{a}_1|\right)$, where $\hat{a}_1$ is the estimated coefficient with the full set of controls and the alternative options for $\mathbf{ETHNIC}_e$ (columns 1–3 in Table 2), while $\hat{a}_1^R$ is the estimated coefficient with the restricted set of controls. In absence of ethnic controls we obtain $\hat{a}_1^R = -1.02$, implying that $\left|\hat{a}_1^R\right| < |\hat{a}_1|$ (since $\hat{a}_1 = -2.10$). With ethnic covariates we get $\hat{a}_1^R = -0.73$ ($\hat{a}_1 = -1.12$) and with ethnic fixed effects $\hat{a}_1^R = -0.45$ ($\hat{a}_1 = -0.94$). In none of the three cases is the point estimate attenuated by the inclusion of the full set of controls. In fact, such inclusion increases the absolute value of the point estimate.
Note that the power of this robustness test depends on the explanatory power of the observable characteristics that are included. In our case, 17 out of the 34 additional control variables are significant at the 5 % level and their inclusion increases the R^2 by 0.04 (with small variations across the alternative options for $\mathbf{ETHNIC}_e$).

[20] We repeated the Altonji et al. (2005) procedure to detect problems of selection on unobservables. The restricted regression yields with no ethnic control $\hat{a}_1^R = 0.33$ ($\hat{a}_1 = 0.74$ in column (1) of Table 4), with ethnic covariates $\hat{a}_1^R = 0.35$ (with $\hat{a}_1 = 0.43$ in col. 2), and with ethnic fixed effects, $\hat{a}_1^R = 0.25$ (with $\hat{a}_1 = 0.49$ in col. 3). Thus, again, selection on unobservables does not appear to drive our results.

Table 2 Effect of fighting on generalized trust in 2008 (second stage)

	Dependent variable: generalized trust in 2008								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Events									
All fighting	−2.10***	−1.12*	−0.94*	−4.34***	−4.08*	−4.70**			
	(0.75)	(0.64)	(0.53)	(1.22)	(2.23)	(2.27)			
Violence civil.							−11.37**		
							(5.53)		
Battles								−7.70**	
								(3.83)	
IDP									−0.87**
									(0.38)
Ethnic controls	No	Ethn. var.	Ethnic FE	No	Ethn. var.	Ethnic FE	Ethnic FE	Ethnic FE	Ethnic FE
Method	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Observations	2,252	2,141	2,252	2,252	2,141	2,252	2,252	2,252	2,252
R^2	0.128	0.145	0.181	0.112	0.125	0.155	0.154	0.155	0.186
	Dependent variable: generalized trust in 2008								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Panel B: fatalities									
All fighting	−0.53***	−0.35**	−0.32*	−0.99***	−0.66*	−0.90**			
	(0.17)	(0.16)	(0.16)	(0.30)	(0.35)	(0.42)			
Violence Civil.							−1.63**		
							(0.81)		
Battles								−1.95**	
								(0.90)	
Ethnic controls	No	Ethn. var.	Ethnic FE	No	Ethn. var.	Ethnic FE	Ethnic FE	Ethnic FE	
Method	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	
Observations	2,252	2,141	2,252	2,252	2,141	2,252	2,252	2,252	
R^2	0.128	0.148	0.183	0.118	0.144	0.171	0.172	0.160	

The unit of observation is an individual. Robust standard errors in parenthesis (adjusted for two-way clustering at county and ethnicity level). Significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. All specifications control for unreported individual sociodemographics (Age, Education, Employed, Gender, Rural, Own TV, Own Radio, 17 Religion Fixed Effects), districts characteristics at the beginning of the period (Past Generalized Trust, Past Trust in Own Group, Past Ethnic Identity, Population, Urbanization, AgeDependencyRatio, Share of Manufacture, Share of Subsistence Farming, Net Migration, Number of MicroEnterprises, Adjusted Total Fertility Rate, Unemployment Rate), and county characteristics at the beginning of the period (Ethnic Fractionalization, Nightlight)

As additional diagnostics, we follow the procedure suggested by Angrist and Pischke (2009, pp. 212–213). The results are in Appendix A. Table 10 reports the coefficient of *All fighting* in the second stage regression, along with a number of statistics of the first-stage regressions from a variety of specifications and estimation techniques. Columns (1)–(3) show the robustness of the benchmark second-stage estimates (columns (4)–(6) in Panel A of Table 2; columns (1)–(3) in Table 5) to the use of a LIML estimator. This estimator is less efficient, but also less biased when instruments are weak. The fact that the results are

Table 3 Effect of fighting on ethnic identity in 2008

	Dependent variable: ethnic identity in 2008					
	(1)	(2)	(3)	(4)	(5)	(6)
All fighting	0.69*	0.39	0.45*	0.18**	0.14**	0.12**
	(0.36)	(0.30)	(0.27)	(0.08)	(0.07)	(0.06)
Fighting variable	Events	Events	Events	Fatalities	Fatalities	Fatalities
Ethnic controls	No	Ethnic variables	Ethnic FE	No	Ethnic variables	Ethnic FE
Method	Probit	Probit	Probit	Probit	Probit	Probit
Observations	2,256	2,145	2,217	2,256	2,145	2,217
Pseudo R^2	0.056	0.072	0.087	0.056	0.072	0.087

The unit of observation is an individual. Robust standard errors in parenthesis (adjusted for clustering at county level). Significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. All specifications control for unreported individual sociodemographics (Age, Education, Employed, Gender, Rural, Own TV, Own Radio, 17 Religion Fixed Effects), district characteristics at the beginning of the period (Past Generalized Trust, Past Trust in Own Group, Past Ethnic Identity, Population, Urbanization, Age-Dependency-Ratio, Share of Manufacture, Share of Subsistence Farming, Net Migration, Number of Micro-Enterprises, Adjusted Total Fertility Rate, Unemployment Rate), and county characteristics at the beginning of the period (Ethnic Fractionalization, Nightlight)

almost identical suggests no bias due to weak instruments. In columns (4)–(6), we run a reduced-form regression. The coefficient of the excluded instrument has the expected sign and is statistically significant, which is again reassuring. Finally, in column (7) we report the results of a specification where we collapse all variables to the county level. We include the standard set of district and county controls (but drop all individual controls). The results are similar to the benchmark specification using individual level variables. In this specification, standard errors are not clustered, allowing us to compute standard Cragg–Donald Wald F-statistics for i.i.d. residuals which can be compared to the Stock-Yogo bounds. We obtain $F = 17.4$. We conclude that our analysis is not subject to a weak instrument problem. In the two panels of Table 11 in the Appendix we display the analogues of the first-stage results as in Tables 5 and 10 when the dependent variable is ethnic identity.[21]

4.3 Exclusion restriction

We run a number of placebo tests on the credibility of our exclusion restriction.

Consider, first, Fig. 3. The first panel displays counties characterized by a positive number of fighting episodes, while the second panel shows counties in which no fighting occurred. Each figure plots on the horizontal axis the distance from Sudan, and on the vertical axis the county-level average of generalized trust filtered by the set of control variables (without including the large battery of religion and ethnic fixed effects due to the relatively small number of observations). Remarkably, the relationship is positive and highly significant across counties experiencing violence, but is insignificant across those experiencing no violence. Though not a formal test of the validity of our exclusion restriction, this falsification analysis

[21] In the Appendix Table 15 we also report the benchmark IV estimates of *Generalized trust* (Panel A of Table 2) and *Ethnic identity* (Panel A of Table 4)—with and without ethnic fixed effects—using IV-Probit, which leads to very similar results as in Tables 2 and 4.
Finally, our main results also hold when the generalized trust variable is not coded as a binary variable, but left in its original ordinal scale. In this case, one can use an Ordered Probit estimator. However, the results of this specification are not robust to the inclusion of ethnic fixed effects.

Table 4 Effect of fighting on ethnic identity in 2008 (second stage)

	Dependent variable: ethnic identity in 2008								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: events									
All fighting	0.74**	0.43**	0.49**	2.94***	4.05***	4.23***			
	(0.37)	(0.21)	(0.22)	(1.03)	(1.54)	(1.29)			
Violence civil.							10.26***		
							(2.52)		
Battles								6.93***	
								(2.39)	
IDP									0.79***
									(0.19)
Ethnic controls	No	Ethn. var.	Ethnic FE	No	Ethn. var.	Ethnic FE	Ethnic FE	Ethnic FE	Ethnic FE
Method	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Observations	2,259	2,148	2,259	2,259	2,148	2,259	2,259	2,259	2,259
R^2	0.059	0.076	0.094	0.039	0.036	0.060	0.059	0.060	0.088
	Dependent variable: ethnic identity in 2008								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Panel B: fatalities									
All fighting	0.19**	0.15**	0.12***	0.67***	0.66***	0.81***			
	(0.08)	(0.07)	(0.03)	(0.20)	(0.19)	(0.22)			
Violence Civil.							1.47***		
							(0.44)		
Battles								1.76***	
								(0.53)	
Ethnic controls	No	Ethn. var.	Ethnic FE	No	Ethn. var.	Ethnic FE	Ethnic FE	Ethnic FE	
Method	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	
Observations	2,259	2,148	2,259	2,259	2,148	2,259	2,259	2,259	
R^2	0.059	0.077	0.094	0.043	0.061	0.071	0.070	0.064	

The unit of observation is an individual. Robust standard errors in parenthesis (adjusted for two-way clustering at county and ethnicity level). Significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. All specifications control for unreported individual sociodemographics (Age, Education, Employed, Gender, Rural, Own TV, Own Radio, 17 Religion Fixed Effects), district characteristics at the beginning of the period (Past Generalized Trust, Past Trust in Own Group, Past Ethnic Identity, Population, Urbanization, Age-Dependency-Ratio, Share of Manufacture, Share of Subsistence Farming, Net Migration, Number of Micro-Enterprises, Adjusted Total Fertility Rate, Unemployment Rate), and county characteristics at the beginning of the period (Ethnic Fractionalization, Nightlight)

suggests that distance from Sudan has an effect on trust in 2008 only through the channel of recent violence. In peaceful counties, distance from Sudan is uncorrelated with trust. The same relationship holds for ethnic identity (see Fig. 4 in the Appendix).

Second, the distance from Sudan could be correlated with pre-conflict levels of trust or with other characteristics affecting trust. Although we control for district-specific average levels of trust, one might worry that this filter of the effect of past trust is imperfect. To address this concern, we run a large number of placebo regressions whose results are shown

Table 5 First stage of benchmark regressions on generalized trust in 2008

Dep. var:	All fight. (1)	All fight. (2)	All fight. (3)	Viol. Civ. (4)	Battles (5)	All fight. (6)	All fight. (7)	All fight. (8)	Viol. Civ. (9)	Battles (10)	IDP (11)
Dist. from Sudan	−0.12***	−0.07***	−0.09***	−0.04***	−0.06***	−0.52***	−0.43***	−0.49***	−0.27***	−0.23***	−0.51***
	(0.03)	(0.02)	(0.03)	(0.01)	(0.02)	(0.07)	(0.08)	(0.14)	(0.08)	(0.07)	(0.16)
Fighting variable	Events	Events	Events	Events	Events	Fatal.	Fatal.	Fatal.	Fatal.	Fatal.	IDP
Ethnic controls	No	Eth. var.	Ethn. FE	Ethn. FE	Ethn. FE	No	Eth. var.	Ethn. FE	Ethn. FE	Ethn. FE	Ethn. FE
Method	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Observations	2,252	2,141	2,252	2,252	2,252	2,252	2,141	2,252	2,252	2,252	2,252
R^2	0.746	0.794	0.832	0.793	0.803	0.687	0.707	0.748	0.674	0.749	0.950
F stat. (Kl.–Paap)	19.875	12.232	8.574	10.922	7.034	59.843	32.477	14.587	13.050	10.113	9.024

Standard errors in parenthesis (robust, two-way clustered at the county and ethnicity level). Significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

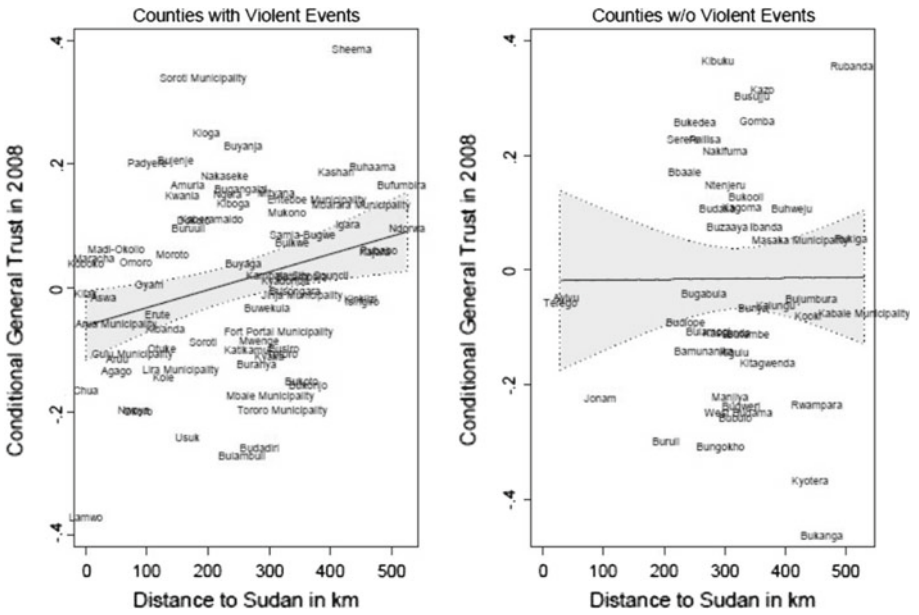

Fig. 3 The scatter plots display the distance from Sudan (*horizontal axis*), and the county-level average of generalized trust filtered by the set of control variables (*vertical axis*). The *left panel* displays counties characterized by a positive number of fighting episodes, while the *right panel* displays counties with no fighting episodes

in Table 6. In Panel A we start by running individual-level regressions whose dependent variable is the survey measure of generalized trust (columns (1)–(2)) or ethnic identity (columns (3)–(4)) in 2000. These are regressed on the county specific measure of distance from Sudan. In all panels we include no control variables in odd columns, while we include the full set of controls and fixed effects in even columns. From column (5) onwards we show the analogous placebo regressions for our standard control variables, i.e. the district characteristics at the beginning of the period (Population, Urbanization, Age-Dependency-Ratio, Share of Manufacture, Share of Subsistence Farming, Net Migration, Number of Micro-Enterprises, Adjusted Total Fertility Rate, Unemployment Rate), and the county characteristics at the beginning of the period (Ethnic Fractionalization, Nightlight). The estimated coefficient is in most cases statistically insignificant, providing reassurance that distance to Sudan does not capture spurious effects of historical differences in trust or other covariates.

4.4 Quantitative effects

The magnitude of the estimated effects is large.[22] The dependent variable, *Generalized trust*, has a sample mean equal to 0.31 with a standard deviation of 0.46. *All fighting* ranges between 0 and 227 violent events with a standard deviation of 45 events. In Table 2, an estimated coefficient of −4.70 in the main 2SLS regression with ethnicity fixed effects (column (6), Panel A) means that a one-standard-deviation increase in *All fighting* (i.e., 45 additional episodes of violence) translates into a 46 % decrease in the standard deviation of *Generalized*

[22] In all the tables, the fighting variables have been rescaled by a factor 10^3 in order to improve readability of their estimated coefficients.

Table 6 Placebo regressions

Dep. var:	Gen. trust 2000		Ethnic identity 2000		Population		Urbanization		Age-dependency-ratio	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A										
Dist. from Sudan	−0.20	0.19	−0.10	−0.10	7.27***	0.99	13.77	−5.30	7.20	−7.35
	(0.18)	(0.12)	(0.08)	(0.06)	(2.72)	(1.60)	(22.37)	(13.55)	(12.66)	(8.50)
All standard controls + FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2,279	2,259	2,279	2,259	2,431	2,259	2,431	2,259	2,431	2,259
R^2	0.064	0.814	0.026	0.900	0.116	0.953	0.007	0.948	0.004	0.926
Dep. var:	Sh. manufacture		Sh. subsistence farm.		Net Migration		Nr. micro enterprises		Adj. total fertility	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel B										
Dist. from Sudan	2.92	2.30*	25.87	−5.45	−1.52	−14.02	39.03	−0.32	−1.11	−0.91
	(1.93)	(1.38)	(24.33)	(5.57)	(6.87)	(9.08)	(25.94)	(8.50)	(0.90)	(0.71)
All standard controls + FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2,431	2,259	2,431	2,259	2,431	2,259	2,431	2,259	2,431	2,259
R^2	0.039	0.868	0.027	0.983	0.001	0.777	0.053	0.983	0.023	0.918

Table 6 continued

Dep. var:	Unemployment rate		Ethnic fractionaliz.		Nightlight	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel C						
Dist. from Sudan	2.54	1.28	−0.05	0.19	2.60	0.59
	(4.05)	(1.89)	(0.14)	(0.20)	(1.89)	(0.65)
All standard controls + FE	No	Yes	No	Yes	No	Yes
Observations	2,431	2,259	2,431	2,259	2,431	2,259
R^2	0.011	0.872	0.001	0.405	0.037	0.937

The unit of observation is an individual. OLS regressions in all columns. Robust standard errors in parenthesis (adjusted for two-way clustering at county and ethnicity level). Significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The specifications (2), (4), (6) control for unreported individual sociodemographics (Age, Education, Employed, Gender, Rural, Own TV, Own Radio, 17 Religion Fixed Effects, 28 Ethnicity Fixed Effects), district characteristics at the beginning of the period (Past Generalized Trust, Past Trust in Own Group, Past Ethnic Identity, Population, Urbanization, Age-Dependency-Ratio, Share of Manufacture, Share of Subsistence Farming, Net Migration, Number of Micro-Enterprises, Adjusted Total Fertility Rate, Unemployment Rate), and county characteristics at the beginning of the period (Ethnic Fractionalization, Nightlight)

trust (i.e., 22 percentage points). This is a very large effect, at about half the magnitude of the difference between the Netherlands (0.48), the eighth most trusting country in world, and the three countries with the lowest trust levels (Peru, Brazil and the Philippines (0.06)).[23] The estimated effect between the least and most conflictive counties is a 107 percentage point increase in *Generalized trust*. With the more conservative main OLS estimate (column (3), Panel A, Table 2) we obtain that a one-standard-deviation increase in *All fighting* leads to a 9.2 % standard deviation decrease in generalized trust; the "maximum" effect of moving from counties with no violence to the county with the highest violence corresponds to a 21 percentage point decrease in trust towards other Ugandans. The quantitative effects are similar when alternative measures of violence are considered.

In Table 4, an estimated coefficient of 4.23 in the main 2SLS regression with ethnicity fixed effects (column (6), Panel A) means that a one-standard-deviation increase in *All fighting* translates into a 48 % standard deviation increase in ethnic identity (i.e., 19 percentage points). The estimated effect between the least and most conflictive districts is a 96 percentage point increase in ethnic identity. With the more conservative OLS estimate (column (3), Panel A, Table 4) we get that a one-standard-deviation increase in *All fighting* leads to a 7.1 % standard deviation increase in ethnic identity. The quantitative effects are similar when alternative measures of violence are considered.

5 Robustness

In this section we perform some robustness checks. To limit the number of tables, we hone in on the specification with ethnic fixed effects, the most demanding one. Also, we usually report only the results of regressions where the intensity of fighting is measured by the count of events rather than the number of fatalities.

5.1 Cross-district versus cross-county variations

Table 7 reports the results of a subset of the regressions of Tables 2 and 4 when *All fighting* is measured at the district rather than at the county level. This specification has the advantage of measuring the dependent variable at the same level as the lagged dependent variable. Its disadvantage is that it disposes with some information available in the data (i.e., the cross-county variation in trust within each district in Afrobarometer 2008). Columns (1)–(6) of Panel A reproduce the columns (1)–(6) of Panel A of Table 2 on *Generalized trust*. All estimated coefficients are negative, and all but one are statistically significant. However, they are smaller in magnitude (in absolute value) than in the cross-county regression, and the 2SLS estimate with ethnic fixed effects becomes marginally insignificant. Analogously, columns (7)–(12) of Panel A reproduce the columns (1)–(6) of Panel B of Table 2, focusing on fatalities rather than events. The results are very similar.

Panel B analogously reproduces the columns (1)–(6) of both Panels of Table 4 on *Ethnic identity*. All coefficients have in this case the expected positive sign, and are all highly significant.

While the regressions of Panels (a) and (b) retain the variation of the dependent variable at the individual level, in Panel C all information is collapsed at the district level. For this

[23] These figures correspond to the average percentage of respondents answering "Most people can be trusted" to the World Values Survey Question A165 "Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?". We use the average scores over the first three waves of the World Values Survey (2009).

purpose, we exclude individual control variables from the right hand side of equation (1) and collapse all the other variables (both on the right and on the left hand sides) at their district average level. The resulting sample consists of only 49 observations (i.e., districts), implying a low number of degrees of freedom. In columns (1)–(4) we have *Generalized trust* as the dependent variable, and display again the results for both events and fatalities. Both the OLS and the 2SLS coefficients are negative, but only the OLS coefficient is highly significant. Columns (5)–(8) report the results of the corresponding regressions for *Ethnic identity*. Here, all coefficients have the expected positive sign, and are highly significant.

The regressions of Table 7 rule out the cross-county variation. In principle, it is possible also to run the regressions at the county level including district fixed effects. In this case, the coefficients are estimated exploiting the within-district variation. This specification is very demanding, since Uganda has 125 counties and 55 districts. Thus, controlling for district fixed effects reduces significantly the sources of variation in the data from which the coefficients of interest are estimated. Moreover, the within-district variation of the instrument is very limited, since the distance from Sudan of two contiguous counties is often similar, leading to a severe weak instrument problem. Not surprisingly, the results are often insignificant.

In conclusion, this section shows that our results hinge on cross-district variation, although the results are stronger when one exploits also the cross-county variation. There is a small albeit positive contribution of the within-district variation.

5.2 Acholiland

One might suspect that the results above are driven by Acholiland, the troubled region in the North where most of the fighting between the government and the LRA took place. In fact, this is not the case. In Appendix Table 12 we focus on the robustness of the benchmark 2SLS estimates of *Generalized trust* (Panel A of Table 2) and *Ethnic identity* (Panel A of Table 4) when the identifying power of Acholiland is mitigated. Columns (1)–(4) refer to the regression for *Generalized trust*. Starting out without ethnic fixed effects, in column (1) we remove the counties classified as Acholi by the GREG dataset (Weidmann et al. 2010).[24] In column (2) we remove from the sample the counties classified as Acholi by the Ethnologue (ETHN) definition of Acholiland (Lewis 2009). Columns (3) and (4) are analogous to columns (1) and (2), but including ethnic fixed effects. In neither case are the results significantly different from the benchmark specifications of Panel A of Table 2, although with ethnic fixed effects the standard errors increase, and the significance level is just below the 10 % threshold. In columns (5)–(8) we perform the corresponding analysis for *Ethnic identity*. The results are again robust.

5.3 Additional controls

In Appendix Table 13 we show that the results are robust to the inclusion of additional controls, namely past fighting events from 1997 to 1999, "trust in president" in 2008, and insecurity perceived at the individual level during the last year before the 2008 survey. We explain these variables in detail in the Data Appendix. We do not include these regressors in our main specifications since (i) past fighting is measured imprecisely as it covers only 3 years, due to data limitations; (ii) trust in president is likely to be endogenous, and could even be regarded as an outcome variable; (iii) insecurity may suffer from selection-into-

[24] In particular, this dummy codes as one all counties where Acholis are the largest ethnic group everywhere in the territory according to GREG.

Table 7 Robustness to constructing fighting measures and collapsing at the district level

	Dependent variable: generalized trust in 2008											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: generalized trust in 2008[a]												
All fighting	−1.06***	−0.61***	−0.77***	−1.12***	−0.85**	−1.95	−0.25***	−0.13***	−0.19***	−0.28***	−0.17**	−0.49
	(0.22)	(0.18)	(0.18)	(0.34)	(0.42)	(1.19)	(0.07)	(0.05)	(0.05)	(0.10)	(0.09)	(0.32)
Fighting variables	Events	Events	Events	Events	Events	Events	Fatalities	Fatalities	Fatalities	Fatalities	Fatalities	Fatalities
Ethnic controls	No	Ethn. var.	Ethnic FE	No	Ethn. var.	Ethnic FE	No	Ethn. var.	Ethnic FE	No	Ethn. var.	Ethnic FE
Method	OLS	OLS	OLS	2SLS	2SLS	2SLS	OLS	OLS	OLS	2SLS	2SLS	2SLS
Observations	2,252	2,141	2,252	2,252	2,141	2,252	2,252	2,141	2,252	2,252	2,141	2,252
R^2	0.133	0.146	0.183	0.133	0.146	0.174	0.128	0.146	0.183	0.128	0.145	0.173
	Dependent variable: Ethnic Identity in 2008											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel B: ethnic identity in 2008[a]												
All fighting	0.51***	0.46***	0.26**	0.80***	0.92***	1.95**	0.15***	0.13***	0.09***	0.20***	0.19***	0.49**
	(0.16)	(0.07)	(0.10)	(0.26)	(0.28)	(0.85)	(0.04)	(0.02)	(0.03)	(0.06)	(0.05)	(0.20)
Fighting variables	Events	Events	Events	Events	Events	Events	Fatalities	Fatalities	Fatalities	Fatalities	Fatalities	Fatalities
Ethnic controls	No	Ethn. var.	Ethnic FE	No	Ethn. var.	Ethnic FE	No	Ethn. var.	Ethnic FE	No	Ethn. var.	Ethnic FE
Method	OLS	OLS	OLS	2SLS	2SLS	2SLS	OLS	OLS	OLS	2SLS	2SLS	2SLS
Observations	2,259	2,148	2,259	2,259	2,148	2,259	2,259	2,148	2,259	2,259	2,148	2,259
R^2	0.062	0.078	0.094	0.060	0.076	0.068	0.063	0.080	0.094	0.063	0.079	0.072

Table 7 continued

Dep. var.:	Generalized trust in 2008				Ethnic Identity in 2008			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel C: collapsed at the district level[b]								
All fighting	−1.18***	−0.81	−0.30***	−0.20	0.44*	0.73**	0.14***	0.18**
	(0.27)	(0.60)	(0.07)	(0.15)	(0.23)	(0.36)	(0.05)	(0.08)
Fighting variables	Events	Events	Fatalities	Fatalities	Events	Events	Fatalities	Fatalities
Method	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Observations	49	49	49	49	49	49	49	49
R^2	0.637	0.627	0.629	0.619	0.352	0.336	0.368	0.364

[a] The unit of observation is a district. Robust standard errors in parenthesis (adjusted for two-way clustering at district and ethnicity level). [b] The unit of observation is a district. Robust standard errors in parenthesis. Significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. All specifications control for unreported districts characteristics at the beginning of the period (Past Generalized Trust, Past Trust in Own Group, Past Ethnic Identity, Population, Urbanization, Age-Dependency-Ratio, Share of Manufacture, Share of Subsistence Farming, Net Migration, Number of Micro-Enterprises, Adjusted Total Fertility Rate, Unemployment Rate, Ethnic Fractionalization, Nightlight)

victimization bias and covers only the period 2007–2008. However, we find that our results are robust to the inclusion of these variables.

5.4 Other dimensions of trust

Finally, Appendix Table 14 replaces the dependent variable of Panel A of Table 2 (in particular, the specification with ethnic fixed effects of columns (3), (6), (7), (8) and (9)) by *Trust in known people* (columns (1)–(5)) and *Trust in relatives* (columns (6)–(10)), respectively. The estimates are in all but one case insignificant. Interestingly, there is some evidence in the 2SLS estimates of a positive effect of fighting on trust in relatives, although this is never statistically significant. This result is partially different from Nunn and Wantchekon (2011), who find that a past history of enslavement has a negative effect on all dimensions of trust, including trust in relatives. Our finding suggests that the effect of local ethnic conflicts is less pervasive and mostly confined to the inter-ethnic dimension.[25]

The findings that conflict leads to a stronger ethnic identity, and that it has a strong negative and significant impact on generalized trust, while having only a weak and non-significant effect on trust in family, are consistent with the theoretical literature on the emergence of parochialism and within-group bias in the face of inter-group conflict (cf. Bowles and Gintis 2004; Choi and Bowles 2007).

6 Extensions

In this section, we consider two important extensions of the main specification.

6.1 Spatial–ethnic variation in violence

So far the analysis has shown that violence across Ugandan counties is associated with a decrease in trust towards other Ugandans and an increase in ethnic identity. In this section, we propose complementary empirical strategies to address two related issues. First, we would like to cast more light on the mechanism linking violence to the erosion of trust. The evidence we present could be driven by the effects of inter-ethnic violence on trust and ethnic identity, or simply by the mere exposure of people to conflict and violence, regardless of any ethnic dimension. Our theoretical research in Rohner et al. (2013) links, more specifically, the effect of war on social capital to inter-ethnic relationships. According to this view, people's beliefs should respond to violence targeting their own ethnic group rather than to generic violence occurring within their own county. We would like to test whether there is more direct evidence of the ethnic channel. Second, the cross-county identification is subject to the *caveat* that counties may have been subject to unobservable shocks correlated with both a high incidence of conflict and low trust. For example, during the period under consideration the government might have reduced transfers or public goods to districts (or counties) populated by hostile ethnic groups. Unfortunately, no direct measure of such government policies are available to us.

[25] We also find that "Trust in known people" is more negatively affected in ethnically diverse areas. In particular, in OLS regressions we find that, when we split the sample, in low-fractionalization counties the relationship between trust and fighting is insignificant, whereas in highly fractionalized areas it is negative and highly significant. This is consistent with a large proportion of known people being from other ethnic groups in fractionalized areas. However, these results are not robust to TSLS where, due to very large standard errors, the differences between high- and low-fractionalization areas are insignificant. Since these results (which are available upon request) are not robust, we do not emphasize them.

Table 8 Ethnic Fighting, Generalized Trust and Ethnic Identity

Dep. var.:	Generalized Trust			Ethnic Identity		
	(1)	(2)	(3)	(4)	(5)	(6)
Fight(OtherEth,Cou)	−1.07			−0.21		
	(0.68)			(0.56)		
Fight(Eth,Cou)	−0.71			0.78***		
	(0.75)			(0.27)		
Fight(Eth)*Fight(Cou)		−0.31			1.83**	
		(0.67)			(0.89)	
Fight(Eth)*Radio			−0.08**			0.07**
			(0.04)			(0.03)
Fixed Effects	Ethnic	County, ethnic	County*ethnic	Ethnic	County, ethnic	County*ethnic
Method	Probit	Probit	Probit	Probit	Probit	Probit
Observations	2234	2341	2162	2217	2280	2136
R^2	0.146	0.204	0.155	0.087	0.118	0.107

The unit of observation is an individual. Standard errors in parenthesis (robust, clustered at county level). Significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All specifications control for unreported individual sociodemographics (Age, Education, Employed, Gender, Rural, Own TV, Own Radio, 17 Religion Fixed Effects), and columns (1) and (4) for district characteristics at the beginning of the period (Past Generalized Trust, Past Trust in Own Group, Past Ethnic Identity, Population, Urbanization, Age-Dependency-Ratio, Share of Manufacture, Share of Subsistence Farming, Net Migration, Number of Micro-Enterprises, Adjusted Total Fertility Rate, Unemployment Rate), and county characteristics at the beginning of the period (Ethnic Fractionalization, Nightlight)

To make progress in this direction, we exploit *spatial*×*ethnic* variations in violence. We use the information provided by ACLED about the nature of each episode of conflict event, each being classified as involving specific rebel groups or ethnic militias, civilians, or the Ugandan army. Many rebel groups have a main ethnic affiliation, e.g. if the ACLED data lists a "battle" between "Bafumbira Ethnic Militia" and "Batooro Ethnic Militia", this event would be linked to both the Bafumbira and the Batooro ethnic groups, and, for example, episodes involving the LRA can be linked to the Acholi group. Therefore, we can associate most events with one or more ethnic groups involved, as well as with the counties where they occurred.[26] Having constructed such a variable, we identify the effect of violence on trust and ethnic identity out of the within-county variation in the number of events involving different ethnic groups, possibly after controlling for both county and ethnic group fixed effects.

To begin with, column (1) and column (4) of Table 8 yield the results of the Probit specification of Column (3) in Tables 1 and 3 after splitting the main independent variable *All Fighting* at the county-level into events involving (i.e. *Fight(Eth,Cou)*) and not involving (i.e. *Fight(OtherEth,Cou)*) the respondent's ethnic group. In column (1) both estimated coefficients are insignificant. Interestingly, in column (4) the coefficient of *Fight(EthCou)* (0.78) is positive and highly significant while the coefficient of *Fight(OtherEth,Cou)* is negative and insignificant. This regression shows that fighting episodes linked to a respondent's own

[26] We have followed a conservative matching strategy, only linking events that can be attributed with a very high confidence to particular groups. The results are similar when a more aggressive matching strategy is used, or when particular rebel groups are removed. The matching table is available from the authors upon publication.

ethnic group have a stronger effect on *Ethnic identity* than do events involving other ethnic groups.

We consider, next, a specification including both county and ethnic fixed effects, where the effect of violence is identified by the interaction between the number of fighting events in the respondent's county and the number of fighting events throughout Uganda involving the respondent's ethnic group: *Fight(Eth)*Fight(Cou)*. The hypothesis we test is that, within each ethnic group, ethnic identity (trust) is stronger (weaker) in counties that are subject to more intense fighting. Or identically: within each county, ethnic identity (trust) is stronger (weaker) among people belonging to ethnic groups more actively involved in fighting nationwide.[27] The results are presented in columns (2) for *Generalized trust* and (5) for *Ethnic identity*. The point estimates of the interaction effects are, as expected, negative (−0.31) and positive (1.83), respectively, although only the coefficient in the regression for *Ethnic identity* is statistically significant (at the 5 % level).

In the main specification of the previous section, we focused on the effects of violence that occurred in the respondent's county. This is a plausible assumption, since our *All fighting* variable codes even minor episodes about which knowledge is unlikely to be shared by all Ugandans. However, well-informed individuals may be affected by news of ethnic violence involving their group anywhere in Uganda. To test this hypothesis, we include an interaction between the ownership of a radio and the number of fighting events nationwide involving the respondent's group (*Fight(Eth)*Radio*). This interaction enables us to run an even more demanding specification controlling for the interaction between ethnic and county fixed effects. The results are shown in n columns (3) and (6). As expected, the coefficient of *Fight(Eth)*Radio* is negative and significant in the case of *Generalized trust,* and positive and significant in the case of *Ethnic identity.* People owning a radio are more reactive to the news of violence involving their own ethnic group anywhere in Uganda. This result is related to the growing literature on the politico-economic effects of mass media pioneered by Strömberg (2004). Recent applications to ethnic conflict include DellaVigna et al. (2011), and Yanagizawa-Drott (2012), focusing respectively on partisan radio broadcasting in the Serbo-Croatian and Rwandan conflicts. These papers show that an exogenous increase in the exposure to radical news affects attitudes towards ongoing conflicts. Note, though, that we do not try to identify exogenous variation in the exposure to radio broadcasting. Thus, the effect identified by our regression could reflect, in part, some self-selection of individuals in the decision to own a radio.

In conclusion, this extension shows that the ethnic channel plays an important role. The within-county results rule out that the increase in ethnic identity is driven by targeted government policies, e.g., the government spending less on hostile districts or counties.

6.2 The heterogeneous effects of conflict on economic activity

In this extension, we study the effect of violence on economic performance. The ideal dependent variable would be GDP per capita at the county (or district) level, but these data are not available in Uganda. Therefore, we proxy GDP by light intensity nighttime according to Satellite Nightlight Data from the National Oceanic and Atmospheric Administration (2010).

[27] The main effects of Fight(Eth) and Fight(Cou) are now absorbed by the county and ethnic fixed effects and cannot be estimated separately. If we omit the fixed effects, the estimated coefficients of Fight(Eth) and Fight(Cou) are negative and significant at the 95 % level (−1.27, s.e. 0.50, and −0.12, s.e. 0.06, respectively) in the case of general trust, and positive but insignificant (0.55, s.e. 0.34, and 0.03, s.e. 0.04) in the case of ethnic identity. If one adds the interaction term Fight(Eth)*Fight(Cou) to this specification without fixed effects, the estimated main effects Fight(Eth) and Fight(Cou) remain negative and significant (positive and insignificant) for the case of general trust (ethnic identity), while the interaction coefficient is in both cases insignificant.

Nightlight data have been used in recent research as a proxy for economic activity (see, for example Henderson et al. 2012; Hodler and Raschky 2011). We include the details of the data construction are in the Appendix.

The focal point of our analysis is the extent to which post-conflict recovery is heterogeneous across counties of different ethnic fractionalization. In particular we hypothesize that if conflict destroys trust and forges strong ethnic identities, the more fractionalized counties would suffer stronger and more persistent economic effects because of their heavier reliance on inter-ethnic business relations that are disrupted by the erosion of trust.

Since *satellite nightlight*, the dependent variable, is measured at the county level we cannot condition on any individual-level information.[28] We estimate the following specification

$$NIGHTLIGHT_c^{08} = \alpha_0 + \alpha_1 NIGHTLIGHT_c^{00} + \alpha_2 FIGHTING_c^{00-08} + \alpha_3 FRAC_c + \alpha_4 FIGHTING_c^{00-08} \times FRAC_c + u_c. \quad (2)$$

We use a Tobit regressor rather than an OLS, since satellite light data are censored at zero. In all specifications, the main coefficient of interest is α_4.

The results are reported in Table 9. Column (1) shows that the main effect of *All fighting* on satellite light in 2008 is negative, but statistically insignificant. Column (2) shows that there is a negative and significant interaction effect: Fighting affects *Satellite light* negatively in highly ethnically fractionalized counties. Since the main effects are measured at a zero level of fractionalization, the insignificant coefficient on *All fighting* indicates that violence has no economic effect in non-fractionalized counties.

As usual, it is difficult to instrument the interaction term. To make progress in this direction, we follow Besley and Persson (2011) and split the sample into high- and low-fractionalization counties, instrumenting in each specification *All fighting* with the same geographic characteristics as before. Since 47 % of the counties have zero fractionalization, and 75 % have a measure of fractionalization below 23 %, we set the threshold at the top quartile. Thus, the sample of low-fractionalization (high-fractionalization) counties consists of the three lowest quartiles (respectively, top quartile). The coefficients of interest are now the main effects of *All fighting,* separately for low- and high-fractionalization counties, in columns (3)–(4) of Table 9, respectively. Fighting is associated with a large and significant decline in living conditions in high-fractionalization counties (column (4)), and with no significant effect in less fractionalized counties (column (3)).[29] The coefficient of *All fighting* in high-fractionalization counties is more than thirteen times larger. In the last three columns of Table 9 we show that the results are similar for alternative measures of fighting.[30]

The finding that ethnic violence dating back to 2002–2005 has a negative effect on economic outcomes measured in 2008 in ethnically fractionalized counties, is consistent with the view that conflict hinders economic cooperation in ethnically divided societies. The evidence suggests that violence has weaker effects on economic cooperation when violence does not involve ethnic cleavages. In other words, violence appears to have more persistent effects in ethnically divided areas.

In the working paper version of this study (Rohner et al. 2012), we show the results of regressions using an alternative proxy of living standards as the dependent variable. In

[28] Note that in this regression we cannot control for ethnic fixed effects, since the dependent variable is measured at the county level.

[29] The small sample size in the split sample reduces the power of the first-stage regression. The Kleibergen–Paap F-stats are well below 10, raising a concern of a weak-instrument bias.

[30] The results are very similar if one controls for the district-averages of our past trust and ethnic identity variables from the 2000 Afrobarometer survey.

Table 9 Explaining Living Conditions proxied by Satellite Nightlight in 2008

Dependent variable: Nightlight in 2008							
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Nightlight (2000)	0.83***	0.84***	0.81***	0.94***	0.84***	0.84***	0.82***
	(0.09)	(0.09)	(0.11)	(0.09)	(0.09)	(0.09)	(0.09)
All fighting	−0.72	−0.44	−1.08	−15.00***			
	(1.32)	(1.32)	(1.78)	(5.21)			
Ethnic frac.	0.04	0.15	3.15	0.24	0.14	0.12	0.11
	(0.13)	(0.13)	(1.95)	(0.23)	(0.13)	(0.13)	(0.13)
Fighting*Frac		−29.83**					
		(13.67)					
Civ. viol.					−0.54		
					(3.05)		
Civ.*Frac					−68.43**		
					(30.26)		
Battles						−0.54	
						(2.06)	
Battles*Frac						−47.26*	
						(27.12)	
IDP							−0.10
							(0.15)
IDP*Frac							−10.40***
							(3.97)
Method	Tobit	Tobit	IVTobit	IVTobit	Tobit	Tobit	Tobit
Sample	All	All	Low Frac.	High Frac.	All	All	All
Observations	125	125	75	43	125	125	125
Log Pseudolikelihood	−21.64	−19.18	150.26	154.07	−19.03	−19.82	−18.18

The unit of observation is a county. Robust standard errors in parenthesis. Significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All specifications control for district characteristics at the beginning of the period (Population, Urbanization, Age-Dependency-Ratio, Share of Manufacture, Share of Subsistence Farming, Net Migration, Number of Micro-Enterprises, Adjusted Total Fertility Rate, Unemployment Rate), and county characteristics at the beginning of the period (Ethnic Fractionalization, Nightlight)

particular, we use individual responses to a question contained in Afrobarometer (2008) about perceived living conditions. As we note in that version, the main limitation of this alternative proxy is its subjective nature. It may easily be biased by non-economic determinants of well-being, including the state of inter-ethnic relationships within local communities. The results we obtained with the alternative proxy line up with those outlined here.[31]

7 Conclusions

In this paper, we have studied the effect of civil conflict on social capital, focusing on the experience of Uganda during the last decade. Using individual and county-level data, we document causal effects of an outburst of civil conflict in 2002–2005, driven by an exogenous shock linked to US foreign policy, on post-conflict trust and ethnic identity. We find that the extent of fighting has a strong and statistically significant negative impact on *Trust towards other Ugandans* between 2000 and 2008. The estimated effect is quantitatively large and robust to a number of control variables, alternative measures of violence and different statistical techniques. People living in districts experiencing more violence also report a strong increase in *Ethnic identity*, i.e., they identify themselves more strongly with their own ethnic group relative to alternative affiliations. Thus, conflict appears to strengthen within-ethnic group ties. This finding is consistent with the evidence in other studies that social capital is fueled by external wars: countries acquire a stronger internal cohesion.

Our results are robust to various specifications, including instrumental variable strategy. Further, the findings overall all robust to a demanding identification strategy relying on the variation within each district in the ethnic violence involving different ethnic groups.

We also study post-conflict economic recovery. Four years after the end of the major conflict, the intensity of fighting had negative economic effects in highly fractionalized counties, but no effects in lowly fractionalized counties. This observation is suggestive of a negative effect of ethnic conflict on inter-ethnic economic cooperation, and is consistent with the predictions of our companion paper (Rohner et al. 2013).

In future work, we plan to extend the approach in this paper to the study of civil conflicts in other African countries, and to consider the role of alliance networks between combatant groups.

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[31] In particular, fighting affects negatively living standards in ethnically fractionalized counties. In contrast, violence has no effect in non-fractionalized counties. When ethnic fixed effects are included, all interaction effects have the expected sign, but most are statistically insignificant. The fact that the specification using the subjective measure of living standards yields less robust results is not surprising, given the noisier nature of this variable.

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