CONFLICT AND THE FORMATION OF POLITICAL BELIEFS IN AFRICA

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ABSTRACT. We test whether living through conflict in childhood changes political beliefs and engagement. We combine data on the location and intensity of conflicts since 1945 with nationally representative data on political attitudes and behaviors from 17 sub-Saharan African countries. Exposure from ages 0 to 14 has a very small standardized impact on later attitudes and behaviors. This finding is robust to migration and holds across a variety of definitions, specifications, and sources of data. Our results suggest that at the population level, the "conflict trap" in Africa is not driven by shifts in political beliefs and engagement caused by conflict exposure in childhood.

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1. INTRODUCTION

Countries with histories of recent conflict face a significantly higher risk of future conflict. This phenomenon, known as "the conflict trap," has intensified over time (Collier et al., 2003; Collier and Sambanis, 2002). For example, every civil war that has broken out since 2003 has been a continuation of a past war (Collier et al., 2008; Hewitt et al., 2008). The problem is most acute in sub-Saharan Africa: a substantial fraction – 35 percent – of all repeat conflicts since 2000 are African (Suhrke and Samset, 2007; Walter, 2011).

This evidence prompts the question: does violence beget violence? The underlying premise of the conflict trap, that many of the structural causes of conflict – e.g., slow economic growth, weak institutions, marginalization of minorities – are themselves exacerbated during conflicts, implicates violence in past generations as the cause of violence in the next (Collier et al., 2008; Hegre et al., 2011).

We shed some light on one possible mechanism for this phenomenon that has received scarce attention: changes in political beliefs, attitudes, and engagement for individuals who grew up during a conflict. These attitudes matter for a wide range of political and economic outcomes. For example, more informed citizens turn out to vote at higher rates and choose better leaders (Banerjee et al., 2011). Civic engagement builds social capital, which in turn improves incomes (Guiso et al., 2004; Putnam, 2001). And democracy is positively linked with economic development (Barro, 1996; Tavares and Wacziarg, 2001).

Yet, whether these outcomes should change in response to childhood conflict exposure is *a priori* unclear. On one hand, we might expect political beliefs and engagement to shift, for at least three reasons. First, recent studies have demonstrated that children who live through conflict are less healthy, less educated, and have worse labor market outcomes as adults.[1] It may follow that beliefs and attitudes change as well.

Second, there is substantial evidence that childhood and adolescence are politically formative periods. The "child's political world" takes shape as early as ages 4 or 5, and restricts the political reality she later perceives (Easton and Hess, 1962; Verhulst et al., 2012). Childhood socialization

[1]See section 1.1, below.

– along with political context, economic circumstance, and events before adulthood – shapes later-life political attitudes (Giuliano and Spilimbergo, 2014; Jennings et al., 2009; Malmendier and Nagel, 2011; Margolis, 2012; Milburn et al., 1995; Sears and Valentino, 1997).

Third, effects on parents are likely to be transmitted to children: Parents' political beliefs exert a strong influence on children's views and their opinions of political actors (Glass et al., 1986; Jaros et al., 1968). Children rely on their parents for priors that shape their responses to new information (Achen, 2002), and look to them as role models for political participation (Wolbrecht and Campbell, 2007). For these reasons, it seems plausible that preferences, beliefs, and engagement may change significantly for children growing up during conflict.

On the other hand, research has shown that children growing up in adverse circumstances are surprisingly psychologically resilient.[2] For example, Garcıa-Ponce and Pasquale (2014) find that effects dissipate within a month of exposure to violence. Anthropological studies have often noted that individuals exposed to conflict display surprising resilience on average (Betancourt and Khan, 2008; Eggerman and Panter-Brick, 2010).

Beyond resilience, individuals often face social pressure to re-integrate and "forget" the past (Annan et al., 2009). Moreover, beliefs and behaviors may have a substantial genetic or physiological component, making them difficult to change (Alford et al., 2005; Fowler and Dawes, 2008; Hatemi et al., 2010; Oxley et al., 2008). And peer effects and socialization can push exposed individuals to converge to the common beliefs of their communities.

Finally, negative impacts may be counteracted by "post-traumatic growth." The struggle to cope with trauma can lead individuals to more fully appreciate life, form more intimate relationships, feel strength, recognize new possibilities, and develop spiritually. Communities may also mobilize to address their particular needs in the aftermath of a conflict, strengthening these individual effects (De Luca and Verpoorten, 2014). Empirically, this is consistent with results that social trust can recover rapidly after conflict ends (De Luca and Verpoorten, 2015). In all, then, it is not obvious that exposure to conflict actually breeds distrust, factionalism, or disengagement from the political system.

[2]See sections 1.1 and 5.2, below.

To evaluate the relevance of this mechanism, we combine data on political engagement and attitudes from the 2005 Afrobarometer, a set of nationally representative surveys from 17 sub-Saharan African countries, with spatial data from the Peace Research Institute Oslo (PRIO) on conflicts and their intensity since 1945. Using a difference-in-differences approach, we find that exposure to conflict for children between ages 0 and 14 has little effect on later-life political engagement or attitudes. Typically, we find that a one standard deviation increase in childhood conflict exposure has less than a tenth of a standard deviation impact on the multiple political outcomes we consider. This effect is smaller than many of the impacts found in case studies on war-affected populations in Africa. It is also smaller than the standardized effects of most other studies on shocks during formative periods and long-run political beliefs.

In order to show that our results are not due to misspecification, endogenous selection, or measurement error, we take several approaches. We show that our results are robust across a variety of specifications. We aggregate measures of political attitudes and engagement using several alternative methods, including mean effects and factor scores. We instrument for our principal measure of conflict exposure using alternative data sources. We show that our results cannot be explained by failings of either the Afrobarometer or PRIO data. To show that selective migration out of conflict-affected locations does not explain our findings, we remove areas that are likely destinations for migrants, we remove individuals whose locations do not match their ethnic identities, and we redefine treatment at country-by-year and ethnicity-by-year levels that do not permit out-migration. Finally, we control for adult (ages 15-30) exposure to account for the possibility that our effects are underestimated due to the fact that we are comparing those exposed as children to those exposed as adults.

Our findings have two principal implications. First, existing studies of conflict exposure have focused on the most affected groups within populations exposed to conflict. Our results show that the typical effect at the population level is smaller. Contrasting our "intent to treat" estimates with "treatment on the treated" effects from existing studies reveals that it may only be the most affected members of a conflict-exposed cohort whose political outcomes change. For the cohort as a whole, beliefs and engagement do not change in any substantial way. Second, we find limited

evidence that what treatments exist immediately after exposure tend to dissipate with age. This is not necessarily "resilience," or recovery from adverse effects. Rather, for outcomes such as the refusal to pay bribes or the support of equality, we in fact find small and positive initial impacts of exposure that shrink over time.

The role of past conflicts in explaining Africans' participation in current politics, then, is limited on average across the population. Existing studies have suggested both that conflict disrupts trade and growth (Abadie and Gardeazabal, 2003; Glick and Taylor, 2009), and that violence begets violence, for example by provoking reprisals or undermining trust (Jaeger and Paserman, 2008; Rohner et al., 2013b). Our results suggest that, while these mechanisms may indeed help to explain Africa's growth tragedy, they do not operate through the formation of political beliefs and habits during childhood for the broader population. We discuss these literatures and our contributions in more depth below.

1.1. **Relevant literature.** We contribute to two broad literatures.

1.1.1. *Recovery from conflict.* The first literature to which we contribute focuses on long-run recovery from conflict. Much of this work has been reviewed by Blattman and Miguel (2010). At the macro-economic level, adverse effects of conflict can be persistent (Glick and Taylor, 2009), though there are many cases in which societies recover very quickly (Casey et al., 2012; Davis and Weinstein, 2002; Miguel and Roland, 2011; Voors and Bulte, 2014). At the micro-economic level, in addition to the studies cited above, several recent papers have shown that conflict negatively affects physical and mental health, human capital accumulation, and other measures of welfare. These studies find that exposure to violence shapes individuals' later preferences and behavior (Callen et al., 2014; Cecchi et al., 2016; Grossman et al., 2015; Kim and Lee, 2012; Miguel et al., 2011; Moya, 2012; Voors et al., 2012). Exposed individuals face trouble gaining income and are left poorer (Ibánez and Moya, 2010; Pellillo, 2012). They shift towards less-risky, worse-performing assets, and spend less (Rockmore, 2015a,b).

A sub-set of this literature has focused on politically-relevant outcomes. These studies provide an intriguing set of results. Individuals who are exposed to crime, conflict, violence and

genocide often have greater levels of political participation, are more trustworthy, vote more often, exhibit more pro-social behavior, are more empathetic towards refugees, and contribute more to public goods (Bateson, 2012; Becchetti et al., 2014; Bellows and Miguel, 2006, 2009; Carmil and Breznitz, 1991; Garcıa-Ponce and Pasquale, 2014; Gilligan et al., 2014; Glennerster et al., 2013; Gneezy and Fessler, 2011; Hartman and Morse, 2015; Shewfelt, 2009). Conversely, trust, inter-ethnic cooperation, and membership in associations decline during the course of a conflict, although social capital recovers rapidly afterwards (Alacevich and Zejcirovic, 2018; Cassar et al., 2013; De Juan and Pierskalla, 2016; De Luca and Verpoorten, 2015; Grosjean, 2014; Rohner et al., 2013a).

This literature remains small, and we contribute new evidence to it. As with other studies cited above, most studies of this type limit their investigation to the survivors of a single conflict. Our broad sample helps confront concerns about external validity. We are also able to deal with a wider variety of measures of political beliefs and engagement than these studies. Finally, the effects on children's political outcomes may differ substantially from effects on adults, a gap our study attempts to fill. We find – consistent with previous results that catch-up occurs quickly after conflict ends – that, at least at the population level, political attitudes and behaviors are not altered substantially in the long run by conflict exposure.

1.1.2. *Early life events.* Second, we add to existing knowledge on the long run effects of early life events. Initially focused on early life disease exposure (e.g. Almond (2006); Bleakley (2010)), this literature has since turned to look at conflict. Recent studies find small but significant long-run effects of early-life conflict on reintegration, focusing on channels such as education, health, earnings and psychological outcomes such as risk-preference. Independent evidence from conflicts in Burundi, Eritrea, Germany, Nigeria, Rwanda and Zimbabwe have shown that affected children have suffered malnutrition and other health shocks that have reduced their adult heights (Agüero and Deolalikar, 2017; Akresh et al., 2012a,b; Alderman et al., 2006; Bundervoet et al., 2009; Cox, 2015; Minoiu and Shemyakina, 2014). Similarly, both exposure to conflict and participation as child soldiers disrupt children's schooling and hence their later labor market outcomes (Akresh and De Walque, 2011; Blattman and Annan, 2010; Leon, 2012; Shemyakina, 2011). By contrast, while

exposed children have experienced psychological problems, they are resilient in their capacity to re-integrate (Annan et al., 2011) and show more cooperativeness in lab-in-field experiments (Bauer et al., 2017).

We contribute to this literature in several ways. First, most of the previous work has not measured the impacts of childhood exposure on political outcomes in adulthood.[3] Second, previous studies often focus on intensely affected individuals (e.g., those who were victimized, attacked, suffered deaths in their families, and so on), and on specific country and conflict contexts. We contribute by estimating impacts at the population level, and across 17 African countries with recent histories of conflict.

1.2. **Outline.** In section 2, we outline the difference-in-difference strategy we use to uncover the effects of conflict exposure on political attitudes and behaviors. In section 3, we describe our sources of data and detail the methods we use to aggregate disparate measures of political outcomes into informative indices. We present our main results in section 4. Although the bulk of our robustness exercises are reported in appendix B, we outline these checks in this section. In section 5, we contrast our results with those of other studies. We discuss mechanisms from the literatures in psychology and political science that can explain our results. We present evidence that the initially small impacts of exposure are not universally negative, and tend to dissipate with time. In section 6, we conclude.

2. Empirical strategy

We use a difference-in-difference approach to identify the effects of early life conflict exposure on later political outcomes. Our principal specification is:

$$y_{ir} = \beta exposure_{ir} + x'_{ir}\gamma + \delta_r + \eta_t + \epsilon_{ir}. \tag{1}$$

Here, y_{ir} is a measure of political engagement or attitudes for individual i, living in sub-national region r. These "regions" are roughly equivalent to provinces. The treatment variable, $exposure_{ir}$,

[3]Two recent exceptions are Blattman (2009), who finds that former child soldiers exhibit greater rates of voting and community leadership in Uganda, and Humphreys and Weinstein (2007), who find that child soldiers have trouble reintegrating in Sierra Leone.

is the respondent's exposure to conflict between the ages of 0 and 14. We will measure this several ways, described below. Although we show that we can obtain similar results by modifying these cutoffs, our focus on this age range has two justifications. First, while a large literature exists on the effects of early life conflict exposure on health outcomes, the literature on conflict and political outcomes has largely neglected children. Exceptions include Kim and Lee (2012) and the studies of child soldiering by Annan and Blattman. Second, in Section 5.2, we cite evidence suggesting that childhood is a formative period for political views. Bauer et al. (2014), for example, find that the age band from 7 to 20 is the only one in which war exposure affects play in their behavioral experiments.

δ_r is a dummy variable for current region of residence. η_t is a dummy variable for year of birth. x_{ir} is a vector of additional controls. In our baseline, this will include dummies for female, urban, own living standards, level of education, and occupation, as well as a continuous measure of the share of the district's population coming from the respondent's ethnic group.

We estimate our baseline results using ordinary least squares (OLS). Because our spatial data on conflict exposure vary at the level of survey clusters within any given year, we cluster our standard errors by survey cluster. These clusters are roughly equivalent to towns or villages, though some larger cities such as Accra may contain several clusters. In almost all specifications, we report "standardized" coefficients – the estimated effect of a one standard deviation increase in conflict exposure on the political outcome of interest, also measured in standard deviations.

3. DATA

3.1. **Political engagement and controls.**

3.1.1. *Raw political outcomes.* We construct our measures of political engagement using individual level data from the third round of the Afrobarometer surveys. These are nationally representative surveys of the voting-age populations of 18 sub-Saharan countries. Of the 25,397 total respondents, we have access to geographic coordinates necessary to compute conflict exposure for 21,360 observations. These coordinates are taken from Nunn and Wantchekon (2011). Because we are not able to compute conflict exposure for individuals whose childhood predates the beginning

of our data on conflict, our base sample contains 18,222 respondents from 17 countries. These are: Benin, Botswana, Ghana, Kenya, Lesotho, Madagascar, Malawi, Mali, Mozambique, Namibia, Nigeria, Senegal, South Africa, Tanzania, Uganda, Zambia, and Zimbabwe.

The Afrobarometer asks respondents dozens of questions related to their political activities, group membership, attitudes, and knowledge. Details on these questions and how we recode them for the analysis are contained in appendix A. Most of these questions fall into three categories. The first set of questions presents respondents with two alternative statements, and asks with which statement they agree more. For example, question 22 asks respondents:

> Which of the following statements is closest to your view? Choose Statement A or Statement B.
>
> A: All people should be permitted to vote, even if they do not fully understand all the issues in an election.
>
> B: Only those who are sufficiently well educated should be allowed to choose our leaders.

We recode respondents' answers as follows: "Agree Very Strongly with A" is 1, "Agree with A" is 2, "Agree with Neither" is 3, "Agree with B" is 4, "Agree Very Strongly with B" is 5, and "Don't Know" is treated as missing. To make the results easier to interpret, we recode these variables so that option "B" reflects greater support for democracy, pluralism, non-violence, and equality. The above example is one such question, and so in our final coding, option "B" becomes the statement beginning with "[a]ll people should be permitted to vote." Thus, larger scores on these recoded variables reflect what we anticipate most readers will view as "good" outcomes.

The second set of variables measure factual knowledge. For example, question 43A2 asks respondents:

> Can you tell me the name of: Your Member of Parliament/National Assembly Representative?

Answers are recorded as "know but can't remember," "incorrect guess," "don't know," or "correct." We code "correct" as "1," and take all other outcomes as "0".

The third set of variables includes measures of agreement or disagreement with a single statement along one dimension. For example, question 36A asks respondents:

> There are many ways to govern a country. Would you disapprove or approve of the following alternatives: Only one political party is allowed to stand for election and hold office?

Respondents can select an answer along a five-point scale ranging from "strongly disapprove" to "strongly approve." As before, we recode these responses so that larger scores reflect "good" outcomes.

3.1.2. *Aggregation of political outcomes.* Because of the large number of variables available in the Afrobarometer, we aggregate these into ten indexes that we treat as our main dependent variables. We begin by dividing outcome measures from the Afrobarometer into ten groups. These are: voting, collective action, contact with political figures, refusal to pay bribes, interest in politics, deference to authority, support for democracy, support for equality, support for the rule of law, and trust. The variables that make up these groups are listed in appendix A. In our baseline, we use mean effects to aggregate these, following an approach similar to Glennerster et al. (2013). We take each component of the broader index, and convert it into a standard normal variable. The sum of these standardized components is then used as the outcome in equation (1). We report summary statistics for these in Table 1B.

In appendix B, we show that similar results obtain when factor analysis is used to aggregate these measures. We use principal components with quartimax rotation to extract the first principal component from each group of variables. We also show in appendix B that similar results are obtained using the dis-aggregated outcomes.

3.1.3. *Other controls from the Afrobarometer.* We also use the Afrobarometer as a source for control variables. Our main controls are a set of dummy variables. These control for year of birth, region of residence, gender, self-reported standard of living, level of education, occupation, and urban. In addition, we also control for the share of the respondent's ethnicity in the district. We report summary statistics for these in Table 1A.

FIGURE 1. Individuals born in 1965 treated by at least 1,000 battle deaths

Black squares are individuals treated by at least 1,000 battle deaths. White squares are individuals treated by fewer deaths.

3.2. **Conflict Exposure.** We measure conflict exposure as the number of battle deaths occurring in conflicts that overlapped with the respondent's place of residence at specific ages. In alternative specifications, we normalize this measure by population and by area. This requires data on the timing, location, spatial extent, and severity of conflict, and on African population densities over the past several decades. Details of these data are given in appendix A. We report summary statistics for these in Table 1A.

Data on conflict are taken from the Uppsala Conflict Data Program (UCDP) / International Peace Research Institute, Oslo (PRIO) Armed Conflict Dataset, Version 4 - 2006, hereafter the "PRIO data." The base list of conflicts and the years they occurred was first constructed by Gleditsch et al. (2002). The data cover conflicts that occurred between 1946 and 2008 in Africa. For the main analysis, we do not distinguish conflicts by type (e.g. civil, ethnic).

An extension to the PRIO data by Raleigh et al. (2006) gives each conflict a coordinate in latitude and longitude, and a radius in kilometers of the area affected by the conflict. If the respondent lives in a survey cluster that overlaps with a conflict that occurred during the respondent's childhood, we code the respondent as "treated" by that conflict. Detail on the construction of these data are provided by Raleigh et al. (2006) and Hallberg (2012). They assign each conflict a latitude and longitude coordinate defined as the mid-point of all known locations of battles. The radius is constructed in 50 km intervals to encompass all such battle locations, excluding sporadic violence distant from the affected territory. Because it is possible that these coordinates and radii only measure conflict locations with error, we will present instrumental variables estimates using alternative sources of data and we will report results using measures of conflict exposure that consider treatment at the region level or ethnicity level.

An additional extension to the PRIO data by Lacina and Gleditsch (2005) includes high, low, and best estimates of the number of deaths that occurred during each conflict. We use the best estimate in our analysis. If no best estimate is available, we use the mean of the high and low estimates. If the battle deaths estimate is missing part-way through a conflict, we impute it using the number of deaths from the previous year. Detail on the construction of these data are provided by Lacina and Gleditsch (2005) and Lacina and Uriarte (2008). They draw on existing lists of casualty statistics, conflict monitoring products, SIPRI yearbooks, consultations with regional experts, archival materials, media sources, published studies, and original demographic work.

We normalize these measures, combining them with population data from the United Nations Environment Programme. These raster data report population densities every ten years on a finely-spaced grid. We interpolate years between these reports exponentially. Taking the average of the densities at the grid points within a conflict's radius as the population density within the conflict zone, we convert battle deaths into per-capita figures.

Figure 1 shows an example – our treatment measure of deaths over the age range 0-14 for respondents born in 1965. The impact of conflicts such as the Nigerian Civil War and Mozambique's war for independence on those whose early childhoods overlapped with them are visible in the figure.

Because the Afrobarometer records region of current residence, and not where the respondent lived during childhood, we are constrained to assume that the respondents lived close to where they grew up when they were interviewed. We discuss several methods for dealing with the possible migration bias below.

Because we measure the childhood exposure of individuals who were adults in 2005, standard sources that only have recent coverage are not suitable for our purposes. These include the Armed Conflict Location Events Database (ACLED) and the Uppsala Conflict Data Program's Georeferenced Event Dataset (UCDP GED).

4. RESULTS

4.1. **Main results.** We present our baseline estimates of (1) in Table 2. In the first row, we report the estimated effect of exposure to battle deaths on each of our ten indices of political outcomes. For most outcomes, these results are both small and insignificant. We make no corrections for multiple hypothesis testing, as this would only further highlight the general pattern of insignificant results. Only for *trust* is the impact of a one standard deviation increase in battle deaths greater than 0.10 standard deviations, and this is only in one specification (discussed below). Exposure to conflict during childhood has a statistically significant though small positive impact on deference towards authority. In our baseline, the standardized coefficient is roughly 0.05.

Similar results obtain if we normalize exposure to battle deaths by population (row 2) or by the area within which the conflict took place (row 3). The estimated effect of exposure on support for democracy becomes significant in one specification, but with a small magnitude.

Deference remains significant if treatment is normalized by area, with a modest normalized magnitude slightly below 0.05 standard deviations. Results do not differ largely when exposure is reported as a dummy variable (row 4).[4] Small but significant effects are found for voting and trust. Using years of exposure (row 5) leads to significant estimates of the effects on voting, collective action, refusal of bribes, and deference, but these are all small in magnitude. Our instrumental

[4]We can define similar dummies for exposure greater than a particular conflict intensity cutoff, e.g., greater than 1 standard deviation of battle deaths; results are very similar and so not reported here.

variables approach (row 6) and use of factor analysis to weight the components of our outcome indices (row 7) are discussed below.

The magnitudes of our estimates can also be compared to the effects of variables that other studies have found can alter political behavior. The impact we find of a one standard deviation increase in conflict exposure is notably less than the effects of: single-ballot elections on voting for third parties in Brazil (Fujiwara, 2011), a voter awareness campaign on female voter turnout in Pakistan (Giné and Mansuri, 2011), rain during the 2009 "Tax Day" protests on the vote share received by the Republican Party in the United States (Madestam et al., 2013), threatening to publicize an individual's failure to vote on voting in the United States (Gerber et al., 2008), and; electronic voting machines on the valid share of votes cast in Brazil (Fujiwara, 2015). In each of these examples, the treatment effect is greater than 0.2 standard deviations of the dependent variable.

Similarly, many studies have found effects that are moderately larger than ours. Examples include the effect of an anti-violence campaign on voting in Nigeria (Collier and Vicente, 2014), the direct effects and intra-household spillovers from a door-to-door "Get Out The Vote" campaign in the United States (Nickerson, 2008), and the impact of a text message campaign on voting in Mozambique (Aker et al., 2015). In each of these cases the treatment effect is between 0.1 and 0.2 standard deviations of the dependent variable. Funk (2010) finds that Swiss adoption of voting by mail increased turnout by roughly one fifth of a standard deviation. She interprets this as small, arguing that the anonymity of voting by mail reduced social pressures to vote.

The magnitudes that we find are more comparable to other studies that have estimated treatment effects between 0 and 0.1 standard deviations of the dependent variable. These include the impact of a one standard deviation increase in childhood cognitive ability on voting in Britain (Denny and Doyle, 2008), the effect of an additional rainy Fourth of July during childhood on voting for the Republican Party in the United States (Madestam and Yanagizawa-Drott, 2012) and the effect of text message vote reminders on voting in the United States (Dale and Strauss, 2009).

To gauge the precision of the estimated effects, we do the following exercise. We take the largest coefficient estimate (in absolute value) in each column, and compute the 95% confidence interval

around this point. We report the larger (in absolute value) of the confidence interval boundaries in the final column of Table 2. The point of the exercise is to determine effect sizes that can be ruled out with 95% confidence.

The results from this last row show that for most outcomes, we can confidently reject standardized effects larger than 0.15 SD. The two exceptions to this are the computed boundaries for the rule of law and trust, which are around 0.18-0.19 SD. Overall, we see that effects larger than fairly small standardized impacts can be rejected with confidence.

4.2. **Heterogeneity.** In Table 3, we test whether the generally null effects we find in the baseline mask nonzero treatment effects for particularly vulnerable groups within the larger population. First, we test whether the effect differs by whether the respondent is an ethnic minority by interacting treatment with the respondent's ethnic group's share of the district population. We normalize this measure to be standard normal, and so the main effect corresponds to an individual whose ethnic group forms 60% of the population (the mean). Few of the interactions are significant, but here are some differences of interest. The effect of exposure to conflict on refusal of bribes becomes positive for an hypothetical individual whose ethnic group share of the district population is at the mean. As his ethnic group gains in size, this treatment diminishes. To interpret the magnitude of this interaction, a one standard deviation change in this normalized measure of ethnic group share translates into a roughly 35 percentage point increase in ethnic group share. Similarly, the (insignificant) positive effects of exposure on democracy and equality are attenuated for members of more locally-predominant ethnic groups, becoming even closer to zero.

Contrasting women with men, more differences emerge. While exposure to conflict decreases collective action for men, it has no effect on women, though the effect on men is quantitatively small. The effect of conflict exposure on deference is nearly twice as large for men as it is for women.

We also test whether heterogeneity in treatment occurs across different types of conflict. We collect alternative measures of conflict exposure from the Marshall (2009) database of Major Episodes of Political Violence (MEPV). By country and year, this data source reports the intensity of wars of independence, international violence, international war, civil violence, civil war, ethnic violence,

and ethnic war. In Table 7, we test whether exposure to different forms of violence in childhood predicts different effects on political outcomes.

Two conclusions are apparent. First, the estimated effects remain small. Very few of our estimates give standardized coefficients greater than 0.10. The clear exception is that exposure to wars of independence have a large positive effect on deference. Wars of independence have large effects on many of the other outcomes, though their effects are only estimated imprecisely. Exposure to international conflict predicts reduced voting and support for democracy. Exposure to civil conflict predicts greater voting, interest in politics, and trust, while weakly reducing support for democracy. Ethnic conflict in particular has several effects. It significantly predicts greater voting, collective action, contact with leaders, willingness to pay bribes, and interest in politics.

It is possible that individuals' responses to conflict exposure depend on the quality of political institutions. We find little evidence of this in Table 3. We consider two measures of institutional quality. The first is the well-known Polity IV index of democracy, averaged over the period 1945 to 2005. Greater scores on this index imply greater levels of democracy. The second measure we use is the International Country Risk Guide Property Rights Protection Index (i.e. protection against expropriation risk), made available by Besley and Persson (2011). This measure is a cross-sectional variable and is defined for 129 countries in 1997. Again, larger values correspond to greater institutional quality. The only significant interaction we find is that the effect of conflict exposure on deference appears to be attenuated in countries in which there is greater protection against expropriation.

In Table A6, we test whether ethnic wars in particular have effects on attitudes that differ by whether an individual is part of a majority or minority ethnic group. As before, we convert the individual's ethnic share to a standard normal variable, so that "0" corresponds to a share of roughly 60%. The effects remain relatively small at the mean, at less than 0.10 standard deviations. Those that have significant interaction effects are generally attenuated as the respondent's ethnic group becomes more predominant. This pattern is apparent (albeit with uneven significance) for voting, collective action, contact, deference, democracy, and rule of law. For refusal of bribes, interest,

equality, equality, and trust, there are small reinforcing effects that are stronger for more predominant groups, though only interest has significant main and interaction effects.

We have also interacted our baseline measure of war exposure with several other variables, in order to test whether our small main effects mask larger heterogeneous treatments. Similarly, if exposure to conflict were only to matter contingent on other outcomes such as later conflict or aid flows, these would capture this heterogeneity. We have not, however, found any evidence of this. In particular, we have interacted conflict exposure with: imminent elections reported by Hyde and Marinov (2012); imminent conflicts in the PRIO data; colonizer identity; historical conflicts reported by Brecke (1999); battle deaths over the period 1945 to 2000; battle deaths over the period 2000 to 2005; democracy reported by Vanhanen (2000) for the year 2000. Results are available on request.

4.3. **Robustness.** In Table 2, we test two alternative approaches to the measurement of conflict exposure. First, we convert our continuous measure of conflict exposure into a dummy variable that equals one if the respondent was exposed to any conflict during childhood. As before, the results are largely small and insignificant, with two exceptions. First, a negative impact of conflict on voting now emerges, with a moderate impact just over 0.05 standard deviations. Second, a negative effect of conflict exposure on trust becomes apparent. Here, the effect is quantitatively small, equal to a reduction of roughly 0.05 standard deviations.

In Table 2, we also use an instrumental variables approach to correct for possible downward bias caused by measurement error. The MEPV data provides intensity measures for each of the seven types of conflict listed above. We use these seven indices as a set of instruments for our baseline measure of conflict exposure. In this specification, results typically remain small and insignificant, and deference remains positive and significant. The two notable exceptions are support for the rule of law and trust. Both become positive and significant, though they remain quantitatively modest.

Finally, we show in Table 2 that the main results remain small when the individual Afrobarometer variables are aggregated using factor analysis, rather than constructed as a sum of normalized components. The main exception is that the effect of exposure on deference vanishes, becoming

both small and insignificant. Although the refusal to pay bribes emerges with significance, the estimated impact remains quantitatively small.

4.4. **Adult exposure.** In Table 4, we investigate whether exposure to conflict in adulthood has a larger impact on the dimensions of political attitudes and behaviors we examine. We include a variable constructed analogously to the childhood exposure variable, but for adults (ages 15-30). Overall, the standardized coefficient magnitudes for the impacts of adult exposure are very small and essentially identical to childhood exposure.

Further, we have re-estimated our baseline specification for all ages in age bands of 5 years in tables 5 and 6. We present effect of exposure to battle deaths and of normalized exposure to battle deaths by population. As seen in these tables, a vast majority of coefficients are tightly bound around zero, suggesting that age bands (0-4, 5-9 ..., 25-29) do not drive the magnitudes we estimate of the effect of early exposure. Further, later exposure also does not produce large impacts.

4.5. **Additional robustness.** We have run several additional tests to verify that our results are not due to measurement error, mis-specification, or selective out-migration. We summarize these results here, but leave detailed presentation to appendix B. In addition, we use this section to discuss the validity of both the Afrobarometer and the PRIO data.

4.5.1. *Migration.* Because we are only able to observe an individual's current place of residence, rather than his place of birth, it is possible that our results could result from selective permanent migration out of former places of conflict by the most strongly treated individuals. Temporary migration during a conflict, by contrast, would be a means for individuals to resist treatment and a mechanism helping explain our results.

To address this, we take several approaches. First, we remove the three largest cities from each country. These are the likely destinations for migrants, and many of the individuals living in urban areas have lived elsewhere previously. While removing these individuals from the sample cannot eliminate the possibility that those who choose to not migrate are also selected, those who do not migrate are more likely to resemble the mean respondent in a treated area. We then extend this

by removing all urban areas from the sample. Second, we remove individuals who, on the basis of their ethnicity, appear to be in an unlikely location – one where less than 10% of their ethnic group lives. The intuition for this exercise is similar. Third, we define measures of conflict that vary at the country-by-year or ethnicity-by-year level, which does not take place of residence into account. Similarly, we also restrict the analysis with ethnicity-level treatment to the urban sample only, to capture those who might have moved in response to treatment.

4.5.2. *Specification.* We also show that the general pattern of null results remains across several specifications. Using country, ethnicity, district, or survey cluster fixed effects rather than region fixed effects gives very similar results to our baseline. Adding rainfall shocks experienced in childhood to the estimation also does not change our findings. We allow battle deaths to enter the estimation separately at each age between 0 and 14. Further, we use an additional wave of the Afrobarometer to show that immediate responses to conflict are also small. The effects remain small when disaggregated in this manner (not reported). We specify alternative functional forms, taking the natural logarithm of each of our main measures of conflict exposure. These too give quantitatively small results (not reported).

4.5.3. *Validity of the Afrobarometer.* If it were the case that the Afrobarometer failed to capture respondents' political views accurately, then it could spuriously lead us to find the null impacts reported in Table 2. However, several studies have validated the use of the Afrobarometer as source of data. It has been used to measure, *inter alia*, voting patterns (Barkan et al., 2006), inter-ethnic inequality (Baldwin and Huber, 2010; Dunning and Harrison, 2010), corruption (Vicente, 2010), and tax compliance (Cummings et al., 2009). It has been used to examine the correlates and determinants of ethnic voting, living standards and education (Eifert et al., 2010; Huber, 2012; Michalopoulos and Papaioannou, 2013), political attitudes (Nunn, 2010, 2011), political participation (Bateson, 2012), political knowledge (Mattes and Bratton, 2007), and trust (Berggren and Bjørnskov, 2011; Besley and Reynal-Querol, 2014). BenYishay (2013) has shown that a one standard deviation increase in early-life rainfall predicts a 0.03 standard deviation increase in trust in one's neighbors in a sub-sample of the same Afrobarometer wave that we use.

Some of these effects have been quantitatively large. The impact of a one standard deviation increase in slave exports in Nunn and Wantchekon (2011) is comparable to a 0.15 standard deviation reduction in trust. Randomized control trials conducted within the Afrobarometer's sampling frame have found sizable treatment effects of an anti-violence campaign on violence, turnout, and voting that range as high as 45 percentage points (Collier and Vicente, 2014; Fafchamps and Vicente, 2013).

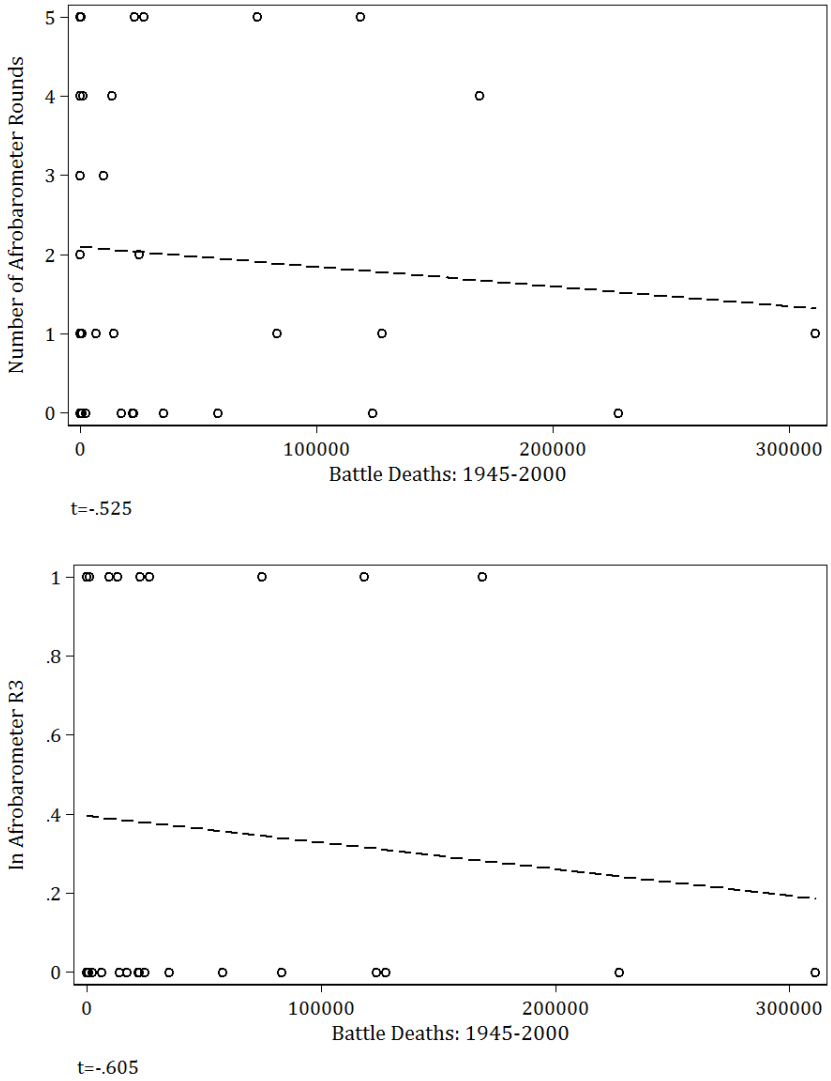
Within the Afrobarometer, the outcomes that we examine are correlated with other individual characteristics in intuitive ways. Higher levels of education and greater ownership of durable goods such as books, television, and radios, for example, predict voting, knowledge of the local MP, activity in a religious group, and support for democracy (not reported). We also find no evidence that countries that have experienced more violence are less likely to be surveyed in the Afrobarometer. There is a negative correlation between battle deaths over the period 1945 to 2000 and inclusion in the Arobarometer surveys, but this is not statistically significant (Figure 2). Any bias due to exclusion from the Afrobarometer would have been apparent in our earlier specifications interacting childhood conflict exposure with battle deaths over the period 1945 to 2000 and battle deaths over the period 2000 to 2005.

4.5.4. *Validity of the PRIO data.* Similarly, if the PRIO data failed to capture variation in the intensity of childhood exposure to conflict over time and space, it could lead to a spurious pattern of null results. A cursory look at Figure 1, however, suggests that the data are reasonable. We list some of the most destructive conflicts, their locations, and their intensity in appendix Table A9.

Like the Afrobarometer, these data have been used extensively by other researchers. Cunningham (2006) uses these data to measure the costliness of conflict. The severity of conflict captured by these data is systematically related to variables such as the Cold War, democracy, ethnic polarization, and arbitrary colonial boundaries (Lacina, 2006; Michalopoulos and Papaioannou, 2016).

We show that our treatment measures are strongly correlated with alternative measures of conflict exposure at the country level from the Integrated Network for Societal Conflict Research (INSCR) MEPV database. As reported in Table 2, results remain small if we instrument for the

FIGURE 2. Battle deaths and inclusion in the Afrobarometer



The fitted lines and t-statistics are the results of an OLS regression of the y-axis variable on the x-axis variable.

PRIO-based measure of treatment with the MEPV estimates. Similarly, the reduced form standardized coefficients when using the MEPV data directly are also small (coefficients not reported but available by request). The large first-stage F-statistics reported in Table 2 further validate our baseline measure of conflict exposure, since it and the MEPV indices are very strongly correlated.

In addition, we show in appendix B that our measure of conflict exposure is correlated with alternative measures whose limitations prevent them from being used in our baseline. These are the Armed Conflict Location and Event Dataset (Raleigh et al., 2012), and the ethnic-based measure of treatment used in Akresh et al. (2012a).

5. MECHANISMS

5.1. **Early life conflict exposure: magnitudes in the literature.** In order to understand the pattern of negligible results we have found, it is important first to point out that existing studies of the effect of conflict on the political behaviors and attitudes of adults or children who have been exposed to it have, with a few exceptions, found relatively modest effects, even where these are statistically significant. Bauer et al. (2016) provide a comprehensive meta-analysis of this literature. In Table 8, we report standardized coefficients drawn from several studies of this type that we have identified. For many of these studies, the effect of a one standard deviation increase in the measure of conflict exposure leads to a less than 0.1 standard deviation change in the outcome under study. Note that the studies in this table do not necessarily restrict treatment to children.

Two exceptions stand out. First, treatment effects are larger for child soldiers who have been abducted. Their experiences with violence leave larger effects in later life than for other children exposed to conflict (Blattman, 2009; Blattman and Annan, 2010). Second, general psychological outcomes that do not relate exclusively to politics respond more to treatment. Rohner et al. (2013a) and De Luca and Verpoorten (2015) find substantial adverse effects of civil war in Uganda on trust. This is similar to the effects Voors et al. (2012) find on altruism, risk-seeking, and time preference in Burundi or that Bauer et al. (2014) find in Georgia and Sierra Leone.

5.2. **Mechanisms and evidence.** As we discuss in section 1, a large prior literature shows that childhood plays an important role in the formation of adult political beliefs. This gives us reason to expect that conflict exposure in childhood will affect later beliefs, either directly or indirectly, and will continue to matter in later life. This is not what the data show. Psychological studies suggest two reasons why we do not find sustained negative effects of childhood conflict exposure on attitudes and behaviors. First, "resilience" enables individuals to recover from traumatic events. Second, "post-traumatic growth" counteracts their adverse impacts. Resilience is positive adaptation despite exposure to adversity (Luthar et al., 2000). Several empirical studies have noted that resilience is greater over time (Elder and Clipp, 1989), and when individuals are welcomed back into their families and communities (Christensen and Utas, 2008; King et al., 1998; Williamson, 2006). Indeed, the Garcıa-Ponce and Pasquale (2014) study cited in Table 8 finds that treatment

effects dissipate within 30 days of exposure to violence. Further, individuals face social pressures to re-integrate and "forget" the past (Annan et al., 2009).

Among child combatants, social reintegration and psychological recovery can occur rapidly (Annan et al., 2011; Blattman, 2009). Similarly, anthropologists who study populations exposed to conflict have frequently noted that, on average, individuals exposed to conflict display a surprising resilience (Betancourt and Khan, 2008; Eggerman and Panter-Brick, 2010).

Post-traumatic growth, by contrast, is "the experience of positive change that occurs as a result of the struggle with highly challenging life crises" (Tedeschi and Calhoun, 2004). The struggle to cope with trauma can lead individuals to more fully appreciate life, form more intimate relationships, feel strength, recognize new possibilities, and develop spiritually. This post-traumatic growth is strongest among those who are more religious, more optimistic, and extroverted (Carmil and Breznitz, 1991; Laufer and Solomon, 2006; Tedeschi and Calhoun, 1996).

In addition to these two broad mechanisms, several other variables mitigate the impact of events in childhood and adolescence on political outcomes. Beliefs and behaviors may have a substantial genetic or physiological component, making them difficult to change (Alford et al., 2005; Fowler and Dawes, 2008; Hatemi et al., 2010; Oxley et al., 2008). Further, peer effects and socialization can push exposed individuals to converge to the community mean if other members of community help shape their attitudes and behaviors. Communities may mobilize specifically to address their particular needs in the aftermath of a conflict, strengthening these effects (De Luca and Verpoorten, 2014). Empirically, this is consistent with results that social trust can recover rapidly after conflict ends (De Luca and Verpoorten, 2015).

Further, the studies cited in Table 8 typically provide evidence of the effect of treatment on the treated (TOT), while our estimates can be understood as intent-to-treat (ITT) estimates of the impact of conflict on the children within the affected region. For example, Bellows and Miguel (2009) report that 23% of households in their sample experienced a child being captured. Blattman and Annan (2010) report that "more than a quarter" of males in their sample were abducted for at least two weeks. Both estimates would suggest dividing their estimated effects by roughly four when applying them to the exposed populations in the regions they study. Both ITT and TOT

estimates are of interest. TOT estimates tell us what is to be expected by a victimized individual; ITT estimates tell us what is to be expected from a cohort that was at risk.

In Table 3, we provide direct evidence of the dissipation of treatment effects over time. We interact our measure of treatment with the time elapsed since treatment, and estimate:

$$y_{ir} = \beta exposure_{ir} + \gamma exposure_{ir} \times time_{it} + x'_{ir}\gamma + \delta_r + \eta_t + \epsilon_{ir}. \tag{2}$$

Here, all variables are as in (1). $time_{it}$ is time elapsed since treatment, i.e. current age, minus 14. For refusal of bribes and equality, there is a significant interaction effect that shows the initial treatment reverts towards zero over time. For both outcomes, the point estimates suggest that individuals return to normal within three years. Many of the other measures (voting, collective action, contact, deference, democracy, and rule of law) show similar patterns, although the coefficient estimates are not significantly different from zero. In all cases, the standardized coefficients on initial treatment remain small.

It is interesting to note that many of the initial treatments are not adverse. Ignoring significance, the results suggest that conflict exposure initially increases voting, refusal to pay bribes, support for democracy, and support for the rule of law. These are consistent with the results in several studies, including Bellows and Miguel (2009) and Blattman (2009) that, if conflict exposure has any effect, it is to make individuals more politically engaged. This is more consistent with post-traumatic growth than with resilience, though the magnitudes are modest and the results are often insignificant.

6. Conclusion

We have shown that exposure to conflict in childhood has negligible effects on a wide range of measures of political attitudes and activities. Our results are robust to several alternative specifications. They are not due to problems with either the data on conflict exposure or political outcomes, nor can they be explained away by measurement error in conflict exposure. These results are consistent with other recent findings on the effects of conflict exposure on political outcomes; though

these studies have found significant effects, these have been – with the exceptions of child soldiering and broader non-political psychological outcomes – quantitatively small.

This pattern of results is easily interpretable. First, anthropological work on survivor populations has underscored their remarkable resilience. Second, our quantitative estimates suggest that whatever effects conflict actually has dissipate rapidly with age. Third, our measure of conflict exposure captures the average treatment effect over the entire population of children within the conflict zone. This differs from other measures that compare those children who have been most acutely affected to those that have been less treated, within the same conflict.

Although existing studies use data that is finely targeted to individuals, it is unlikely that deeply personal experiences such as child soldiering are representative of the average treatment effect on the whole population that experiences its childhood in a location affected by conflict. Similarly, existing measures make it difficult to compare the intensity of treatment across conflicts. Effectively, the greater external validity and representativeness of our measure comes at the cost of specificity.

This suggests that our study captures an intent-to-treat estimate, rather than a measure of the effect of treatment on the treated. Interpretation of the results, then, should be limited by this fact. Further, our measure of conflict intensity is one of deaths occurring in battle. It will only capture other traumatic experiences in conflict, such as rape and disease, in-so-far as these are correlated with the intensity of combat. We do not have data that would allow us to test for heterogeneous effects according to the outcome of a conflict. Finally, not all African societies in our data are free and democratic. This dampens the variation in political participation that we are able to use for identification.

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Table 1A. Summary statistics: War exposure and controls

	(1) Mean	(2) s.d.	(3) Min	(4) Max	(5) N
PRIO Conflict Measures:					
Battle deaths (000s), ages 0-14	26.575	45.807	0	207.933	18,141
Battle deaths (000s), ages 0-14, per 1000 population	0.002	0.004	0	0.026	12,191
Battle deaths (000s), ages 0-14, per area	0.075	0.156	0	0.866	12,191
1(Battle deaths > 0, ages 0-14)	0.539	0.498	0	1	18,141
Years of conflict exposure, ages 0-14	4.053	5.045	0	15	18,141
Conditional on Exposure > 0					
Battle deaths (000s), ages 0-14	49.314	52.653	0.02	207.933	9,776
Battle deaths (000s), ages 0-14, per 1000 population	0.004	0.004	5.12E-07	0.026	6,348
Battle deaths (000s), ages 0-14, per area	0.134	0.191	2.55E-05	0.866	6,348
Years of conflict exposure, ages 0-14	7.522	4.599	1	15	9,776
MEPV Conflict Measures:					
War of Independence Score, ages 0-14 (avg)	0.137	0.544	0	3	17,756
International Violence Score, ages 0-14 (avg)	0.016	0.059	0	0.4	16,491
International War Score, ages 0-14 (avg)	0.026	0.078	0	0.267	16,491
Civil Violence Score, ages 0-14 (avg)	0.002	0.012	0	0.067	16,491
Civil War Score, ages 0-14 (avg)	0.254	0.795	0	4.8	16,491
Ethnic Violence Score, ages 0-14 (avg)	0.319	0.585	0	2	16,491
Ethnic War Score, ages 0-14 (avg)	0.287	0.730	0	2.8	16,491
Controls:					
Year of birth	1968	14.8	1875	1987	25,110
Age heaping	0.28	0.45	0	1	25,397
Female	0.50	0.50	0	1	25,397
Urban	0.38	0.49	0	1	25,397
Ethnic share in district	0.61	0.35	0.0027	1	25,397
Living conditions fairly bad (very bad omitted)	0.27	0.45	0	1	25,308
Living conditions neither good nor bad	0.22	0.41	0	1	25,308
Living conditions fairly good	0.25	0.43	0	1	25,308
Living conditions very good	0.04	0.19	0	1	25,308
Informal schooling only (No formal schooling omitted)	0.04	0.20	0	1	25,305
Some primary schooling	0.20	0.40	0	1	25,305
Primary school completed	0.16	0.36	0	1	25,305
Some secondary school/high school	0.20	0.40	0	1	25,305
Secondary school completed/high school	0.15	0.35	0	1	25,305
Post-secondary qualifications, not univ	0.06	0.24	0	1	25,305
Some university	0.02	0.13	0	1	25,305
University completed	0.02	0.13	0	1	25,305
Post-graduate	0.01	0.07	0	1	25,305

Notes: Raw means and standard deviations of conflict meaures are reported above; in all regressions, conflict measures are standardized by subtracting the mean of the variable and then dividing by the standard deviation.

Table 1B. Summary statistics: Standardized Outcome Groupings

	(1) Mean	(2) s.d.	(3) Min	(4) Max	(5) N
Standardized Outcome Groupings (Mean Effects Analysis)					
Voting	0	1	-1.922	0.561	25,248
Collective action	0	1	-2.454	2.850	23,981
Contact	0	1	-0.698	5.266	22,571
Refusal of bribes	0	1	-5.438	0.873	24,677
Interest	0	1	-1.732	2.125	20,227
Deference	0	1	-2.715	2.036	20,766
Democracy	0	1	-3.813	1.591	17,127
Equality	0	1	-2.417	1.074	23,668
Rule of law	0	1	-3.269	1.654	21,842
Trust	0	1	-2.573	2.043	10,291

Notes: See Table A.1 for individual components of each grouping. To generate standardized outcomes, each component is first de-meaned and normalized by its standard deviation. All standardized components within an outcome grouping are then summed, creating a standardized group variable, which is used in mean effects analysis.

Table 2. Main Results: Effects of Early-life Conflict Exposure on Political Attitudes and Behaviors

	(1) Voting	(2) Collective action	(3) Contact	(4) Refusal of bribes	(5) Interest	(6) Deference	(7) Democracy	(8) Equality	(9) Rule of law	(10) Trust
Mean Effects:										
Battle Deaths	0.00611	-0.0126	-0.000315	0.0236*	0.0143	0.0498***	0.00991	0.00892	0.00261	0.0138
(0-14)	(0.0109)	(0.0117)	(0.0134)	(0.0128)	(0.0131)	(0.0138)	(0.0143)	(0.0139)	(0.0150)	(0.0171)
N	*17,780*	*16,950*	*16,621*	*17,372*	*14,287*	*14,914*	*12,470*	*16,638*	*15,573*	*7,847*
Battle Deaths	-0.0155	0.0332	0.0207	0.0372	0.0236	0.0283	-0.0133	0.0109	0.0223	-0.00406
(0-14) per 1000	(0.0177)	(0.0254)	(0.0259)	(0.0238)	(0.0216)	(0.0218)	(0.0238)	(0.0241)	(0.0232)	(0.0236)
N	*11,933*	*11,371*	*11,095*	*11,667*	*9,600*	*9,940*	*8,406*	*11,127*	*10,436*	*5,111*
Battle Deaths	-0.00297	0.0323	0.0289	0.0149	0.0130	0.0492**	-0.0368*	0.0103	0.0211	-0.0272
(0-14) per area	(0.0155)	(0.0244)	(0.0272)	(0.0240)	(0.0205)	(0.0228)	(0.0211)	(0.0261)	(0.0255)	(0.0254)
N	*11,933*	*11,371*	*11,095*	*11,667*	*9,600*	*9,940*	*8,406*	*11,127*	*10,436*	*5,111*
Exposure dummy	-0.0576***	-0.0116	0.0378	-0.000932	-0.0261	0.0275	0.0126	0.00917	0.00258	-0.0494*
(0-14)	(0.0206)	(0.0222)	(0.0240)	(0.0216)	(0.0221)	(0.0230)	(0.0254)	(0.0211)	(0.0220)	(0.0261)
N	*17,780*	*16,950*	*16,621*	*17,372*	*14,287*	*14,914*	*12,470*	*16,638*	*15,573*	*7,847*
Years of exposure	-0.00852***	-0.00501*	-0.00326	0.00779***	0.00110	0.00617**	-0.000354	0.000590	0.00142	-0.00440
(0-14)	(0.00260)	(0.00266)	(0.00282)	(0.00292)	(0.00266)	(0.00307)	(0.00296)	(0.00281)	(0.00304)	(0.00344)
N	*17,780*	*16,950*	*16,621*	*17,372*	*14,287*	*14,914*	*12,470*	*16,638*	*15,573*	*7,847*
IV Battle Deaths	-0.055	-0.018	-0.026	0.042	0.062*	0.077**	-0.056	-0.024	0.090**	0.118***
(0-14)	(0.041)	(0.043)	(0.039)	(0.048)	(0.034)	(0.039)	(0.043)	(0.041)	(0.046)	(0.035)
First stage F-stat	*103.59*	*102.21*	*104.71*	*101.84*	*101.18*	*105.73*	*89.06*	*110.36*	*104.71*	*109.28*
N	*14,094*	*13,484*	*13,352*	*13,777*	*11,910*	*12,008*	*9,938*	*13,384*	*12,455*	*6,227*
Factors:										
Battle Deaths	0.006	0.002	-0.002	0.022*	-0.005	0.017	0.009	-0.013	0.012	0.018
(0-14)	(0.011)	(0.011)	(0.013)	(0.013)	(0.011)	(0.017)	(0.013)	(0.013)	(0.014)	(0.018)
N	*17,780*	*16,950*	*16,621*	*17,372*	*14,287*	*14,914*	*12,470*	*16,638*	*15,573*	*7,847*
Effects > |X| rejected with 95% confidence	*-0.09798*	*0.08299*	*0.08484*	*0.13429*	*0.12865*	*0.15345*	*-0.14029*	*-0.10437*	*0.18017*	*0.18661*

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Each cell represents a regression of a political attitude/behavior (column headers) on a conflict measure (row headers). All regressions include dummies for region of residence, year of birth, female, responses to "current living standards", level of education, occupation, and urban. The share of the respondent's ethnicity in the district population is also used as a control. Standardized coefficients are reported. Final row of table reports the (larger) boundary of the 95% CI for the *largest coefficient estimate* in each column; effect sizes larger than this boundary value can be rejected with 95% confidence.

Table 3. Heterogeneous Effects of Childhood Conflict Exposure by Time Since Exposure, Minority Status, and Gender

	(1) Voting	(2) Collective action	(3) Contact	(4) Refusal of bribes	(5) Interest	(6) Deference	(7) Democracy	(8) Equality	(9) Rule of law	(10) Trust
Time Since Exposure:										
Battle Deaths	-0.021	0.003	0.012	-0.030*	0.034**	-0.010	-0.019	-0.031*	0.021	0.011
(0-14) x Age	(0.015)	(0.014)	(0.016)	(0.017)	(0.015)	(0.018)	(0.016)	(0.017)	(0.017)	(0.020)
Battle Deaths	0.032	-0.017	-0.015	0.061***	-0.028	0.062**	0.034	0.047*	-0.023	0.000
(0-14)	(0.024)	(0.021)	(0.023)	(0.023)	(0.023)	(0.027)	(0.026)	(0.024)	(0.026)	(0.031)
N	*17,780*	*16,950*	*16,621*	*17,372*	*14,287*	*14,914*	*12,470*	*16,638*	*15,573*	*7,847*
Minority Status:										
Battle Deaths	-0.000	0.012	-0.013	-0.021**	0.002	-0.010	-0.020*	-0.020**	-0.017	0.012
(0-14) x Ethnic Share in District	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.011)	(0.010)	(0.010)	(0.011)	(0.012)
Battle Deaths	0.006	-0.014	-0.001	0.026**	0.015	0.049***	0.011	0.007	0.004	0.015
(0-14)	(0.011)	(0.012)	(0.013)	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)	(0.015)	(0.017)
N	*17,780*	*16,950*	*16,621*	*17,372*	*14,287*	*14,914*	*12,470*	*16,638*	*15,573*	*7,847*
Gender:										
Battle Deaths	0.021	0.038**	0.001	-0.014	-0.024*	-0.034**	-0.005	-0.034**	0.010	-0.016
(0-14) x Female	(0.014)	(0.015)	(0.015)	(0.014)	(0.014)	(0.017)	(0.018)	(0.016)	(0.016)	(0.018)
Battle Deaths	-0.005	-0.032**	-0.001	0.031**	0.027*	0.067***	0.012	0.026	-0.002	0.021
(0-14)	(0.014)	(0.014)	(0.016)	(0.014)	(0.015)	(0.016)	(0.015)	(0.016)	(0.018)	(0.019)
N	*17,780*	*16,950*	*16,621*	*17,372*	*14,287*	*14,914*	*12,470*	*16,638*	*15,573*	*7,847*
Democratic institutions:										
Battle Deaths	-0.014	0.007	0.009	-0.006	0.010	-0.011	0.017	-0.004	0.013	-0.027
(0-14) x Polity IV score	(0.010)	(0.011)	(0.011)	(0.010)	(0.014)	(0.012)	(0.014)	(0.013)	(0.013)	(0.021)
Battle Deaths	0.003	-0.011	0.002	0.022*	0.018	0.047***	0.013	0.008	0.006	0.007
(0-14)	(0.011)	(0.012)	(0.014)	(0.013)	(0.014)	(0.014)	(0.014)	(0.014)	(0.015)	(0.019)
N	*17,780*	*16,950*	*16,621*	*17,372*	*14,287*	*14,914*	*12,470*	*16,638*	*15,573*	*7,847*
Expropriation Risk:										
Battle Deaths	-0.020	0.007	0.002	-0.003	-0.008	-0.041**	0.017	-0.011	0.006	-0.004
(0-14) x Expropriation Risk	(0.014)	(0.017)	(0.017)	(0.017)	(0.018)	(0.017)	(0.020)	(0.017)	(0.020)	(0.030)
Battle Deaths	0.012	-0.012	-0.002	0.022*	0.014	0.051***	0.009	0.009	0.004	0.013
(0-14)	(0.011)	(0.012)	(0.014)	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)	(0.015)	(0.018)
N	*15,984*	*15,246*	*14,866*	*15,640*	*12,881*	*13,317*	*11,483*	*14,853*	*14,104*	*7,008*

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Each column by pair of rows above represents a regression of a political attitude/behavior (column headers) on battle deaths interacted with either age, ethnic share in district, or a female dummy (see row headers). All regressions include dummies for region of residence, year of birth, female, responses to "current living standards", level of education, occupation, and urban. The share of the respondent's ethnicity in the district population is also used as a control. Standardized coefficients are reported.

Table 4. Heterogeneous Effects of Early-life Conflict Exposure by Conflict Type

	(1) Voting	(2) Collective action	(3) Contact	(4) Refusal of bribes	(5) Interest	(6) Deference	(7) Democracy	(8) Equality	(9) Rule of law	(10) Trust
War of Independence	0.261	-0.196	-0.595	0.0405	-0.123	0.571*	0.543	0.320	0.104	-0.229
	(0.300)	(0.345)	(0.363)	(0.348)	(0.282)	(0.335)	(0.366)	(0.317)	(0.352)	(0.548)
International Conflict	-0.0546***	-0.0190	-0.00628	0.0266	-0.0139	-0.00530	-0.0400**	-0.0202	0.0114	0.00394
	(0.0160)	(0.0155)	(0.0152)	(0.0164)	(0.0151)	(0.0180)	(0.0185)	(0.0168)	(0.0165)	(0.0166)
Civil Conflict	0.0458*	0.0194	0.0201	-0.0122	0.0493**	0.0307	-0.0553*	0.0150	0.0360	0.0776***
	(0.0265)	(0.0270)	(0.0246)	(0.0301)	(0.0237)	(0.0273)	(0.0298)	(0.0289)	(0.0297)	(0.0280)
Ethnic Conflict	0.0514***	0.0422**	0.0766***	-0.0649***	0.0587***	-0.00936	0.0134	0.0320	-0.00992	-0.0511
	(0.0186)	(0.0209)	(0.0199)	(0.0208)	(0.0195)	(0.0226)	(0.0229)	(0.0220)	(0.0227)	(0.0356)
N	14,182	13,567	13,438	13,864	11,988	12,076	10,007	13,467	12,530	6,227

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Each column above represents a regression of a political attitude/behavior (column headers) on a conflict measure (row headers). All regressions include dummies for region of residence, year of birth, female, responses to "current living standards", level of education, occupation, and urban. The share of the respondent's ethnicity in the district population is also used as a control. Standardized coefficients are reported.

Table 5. Effects From Childhood and Adult Exposure

	(1) Voting	(2) Collective action	(3) Contact	(4) Refusal of bribes	(5) Interest	(6) Deference	(7) Democracy	(8) Equality	(9) Rule of law	(10) Trust
Battle Deaths (0-14)	-0.0136	-0.0127	-0.00634	0.0341**	0.0225	0.0515***	0.00534	-2.01e-05	0.0135	0.0137
	(0.0126)	(0.0154)	(0.0193)	(0.0174)	(0.0165)	(0.0180)	(0.0175)	(0.0181)	(0.0195)	(0.0219)
Battle Deaths (15-30)	-0.00586	0.00886	-0.00120	0.0319**	0.0272*	0.00689	0.0162	0.0118	0.0170	0.0170
	(0.00968)	(0.0132)	(0.0159)	(0.0152)	(0.0145)	(0.0151)	(0.0154)	(0.0142)	(0.0155)	(0.0165)
N	11,933	11,371	11,095	11,667	9,600	9,940	8,406	11,127	10,436	5,111

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Each cell represents a regression of a political attitude/behavior (column headers) on a conflict measure (row headers). All regressions include dummies for region of residence, year of birth, female, responses to "current living standards", level of education, occupation, and urban. The share of the respondent's ethnicity in the district population is also used as a control. Standardized coefficients are reported.

Table 6. Later exposure to battle deaths

Mean Effects:	(1) Voting	(2) Collective action	(3) Contact	(4) Refusal of bribes	(5) Interest	(6) Deference	(7) Democracy	(8) Equality	(9) Rule of law	(10) Trust
Battle Deaths	0.0190	0.00481	0.000842	0.0539*	0.0122	0.0367	-0.0212	-0.0201	-0.0143	0.0141
(0-4)	(0.0241)	(0.0256)	(0.0278)	(0.0319)	(0.0276)	(0.0327)	(0.0311)	(0.0303)	(0.0336)	(0.0275)
N	11,933	11,371	11,095	11,667	9,600	9,940	8,406	11,127	10,436	5,111
Battle Deaths	-0.0267**	-0.0118	0.00305	0.0179	-0.00874	0.0132	0.00459	0.0134	0.00266	-0.00604
(5-9)	(0.0106)	(0.0127)	(0.0145)	(0.0132)	(0.0122)	(0.0143)	(0.0146)	(0.0136)	(0.0152)	(0.0177)
N	11,933	11,371	11,095	11,667	9,600	9,940	8,406	11,127	10, 436	5,111
Battle Deaths	0.00968	-0.00195	-0.0103	0.00223	0.0321***	0.0308**	0.00820	-0.00989	0.0185	0.0164
(10-14)	(0.00919)	(0.0114)	(0.0139)	(0.0130)	(0.0115)	(0.0145)	(0.0141)	(0.0131)	(0.0137)	(0.0168)
N	11,933	11,371	11,095	11,667	9,600	9,940	8,406	11,127	10, 436	5,111
Battle Deaths	-0.00282	0.00607*	0.00519	0.00250	-0.00250	-0.00945**	-0.00304	0.00340	-0.00219	-0.00168
(15-19)	(0.00236)	(0.00335)	(0.00394)	(0.00371)	(0.00343)	(0.00385)	(0.00392)	(0.00353)	(0.00356)	(0.00451)
N	11,933	11,371	11,095	11,667	9,600	9,940	8,406	11,127	10, 436	5,111
Battle Deaths	0.000321	-0.00254	-0.00408	0.00194	0.00329	0.00350	0.00436	0.000253	0.00140	0.00533*
(20-24)	(0.00178)	(0.00221)	(0.00271)	(0.00265)	(0.00253)	(0.00286)	(0.00283)	(0.00276)	(0.00282)	(0.00295)
N	11,933	11,371	11,095	11,667	9,600	9,940	8,406	11,127	10, 436	5,111
Battle Deaths	-0.00171	0.00245	0.00219	0.00654**	0.00583**	0.00270	0.00206	0.00248	0.00582**	-0.00149
(25-29)	(0.00164)	(0.00280)	(0.00306)	(0.00274)	(0.00263)	(0.00273)	(0.00272)	(0.00253)	(0.00274)	(0.00284)
N	11,933	11,371	11,095	11,667	9,600	9,940	8,406	11,127	10, 436	5,111

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Each cell represents a regression of a political attitude/behavior (column headers) on a conflict measure (row headers) for population in age bands of 5 years from 0 to 29 years. All regressions include dummies for region of residence, year of birth, female, responses to "current living standards", level of education, occupation, and urban. The share of the respondent's ethnicity in the district population is also used as a control. Standardized coefficients are reported.

Table 7. Later exposure to battle deaths by population

Mean Effects:	(1) Voting	(2) Collective action	(3) Contact	(4) Refusal of bribes	(5) Interest	(6) Deference	(7) Democracy	(8) Equality	(9) Rule of law	(10) Trust
Battle Deaths	0.00248	0.0221	0.00919	0.0118	0.00896	0.0142	-0.0145	-0.0162	-0.0113	0.00631
(0-5) per 1000	(0.0123)	(0.0137)	(0.0143)	(0.0151)	(0.0144)	(0.0161)	(0.0150)	(0.0154)	(0.0157)	(0.0137)
N	11,933	11,371	11,095	11,667	9,600	9,940	8,406	11,127	10, 436	5,111
Battle Deaths	-0.0172*	-0.00900	-0.00754	0.0311**	-0.0135	-0.00188	-0.00339	0.0180	0.00202	-0.00226
(5-9) per 1000	(0.00936)	(0.0126)	(0.0150)	(0.0133)	(0.0120)	(0.0137)	(0.0138)	(0.0138)	(0.0140)	(0.0134)
N	11,933	11,371	11,095	11,667	9,600	9,940	8,406	11,127	10, 436	5,111
Battle Deaths	0.00308	0.00741	-0.00234	-0.0137	0.0341***	0.0320**	0.00167	-0.00362	0.0156	0.00545
(10-14) per 1000	(0.00874)	(0.0124)	(0.0137)	(0.0131)	(0.0106)	(0.0130)	(0.0128)	(0.0126)	(0.0123)	(0.0139)
N	11,933	11,371	11,095	11,667	9,600	9,940	8,406	11,127	10, 436	5,111
Battle Deaths	-0.00228	0.0277**	0.0297**	0.0118	-0.0320**	-0.0415***	-0.0148	0.00801	-0.00822	-0.0197
(15-19) per 1000	(0.00834)	(0.0130)	(0.0141)	(0.0135)	(0.0129)	(0.0136)	(0.0133)	(0.0127)	(0.0122)	(0.0132)
N	11,933	11,371	11,095	11,667	9,600	9,940	8,406	11,127	10, 436	5,111
Battle Deaths	-0.00352	-0.00203	-0.0155	-0.000765	0.0226**	0.0265**	0.0145	0.00609	0.0167	0.0124
(20-24) per 1000	(0.00772)	(0.0105)	(0.0129)	(0.0114)	(0.0108)	(0.0110)	(0.0137)	(0.0120)	(0.0114)	(0.0123)
N	11,933	11,371	11,095	11,667	9,600	9,940	8,406	11,127	10, 436	5,111
Battle Deaths	0.000973	0.0113	0.0395***	0.0143	0.0204	-0.00532	-0.00662	-0.00433	0.0147	-0.0202
(25-29) per 1000	(0.00785)	(0.0119)	(0.0146)	(0.0137)	(0.0125)	(0.0119)	(0.0140)	(0.0128)	(0.0118)	(0.0141)
N	11,933	11,371	11,095	11,667	9,600	9,940	8,406	11,127	10, 436	5,111

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Each cell represents a regression of a political attitude/behavior (column headers) on a conflict measure (row headers) for population in age bands of 5 years from 0 to 29 years. All regressions include dummies for region of residence, year of birth, female, responses to "current living standards", level of education, occupation, and urban. The share of the respondent's ethnicity in the district population is also used as a control. Standardized coefficients are reported.

Table 8. Results in the literature

Author(s)	Paper	Outcome of Interest	RHS Variable of Interest	Coefficient of Interest	SD(LHS variable)	SD(RHS variable)	Standardized Coefficient
Annan, Jeannie, Christopher Blattman, Dyan Mazurana and Khristopher Carlson	Civil War, Reintegration, and Gender in Northern Uganda	Member of at least one group (women) (Table 2)	Abduction as a child soldier	-0.040	0.500	0.440	**-0.035**
		Member of at least one group (men) (Table 2)		0.010	0.500	0.500	**0.010**
Bauer, Michal, Alessandra Cassar, Julie Chytilova, and Joseph Henrich	War's Enduring Effects on the Development of Egalitarian Motivations and In-group Biases	Egalitarian play in two behavioural games (Fig 2)	Affected & Internally Displaced (Georgia)	0.255	0.402	0.427	**0.271**
		Egalitarian play in two behavioural games (Fig 2)	Most Affected Category (Sierra Leone)	0.184	0.374	0.414	**0.204**
Bellows, John and Edward Miguel	War and Local Collective Action in Sierra Leone	Did you attend any community meetings in the past year? (Table 3)	Conflict Victimization Index	0.078	0.490	0.340	**0.054**
Blattman, Christopher	From Violence to Voting: War and Political Participation in Uganda	Voted in 2005 (Table 5)	Violent acts witnessed	0.040	0.500	1.700	**0.136**
Blattman, Christopher and Jeannie Annan	The Consequences of Child Soldiering	Index of psychological distress (Table 7)	Index of violence experienced	0.150	2.400	3.100	**0.194**
Cassar, Alessandra, Pauline Grosjean, and Sam Whitt	Legacies of violence: trust and market development	Amount sent in trust game (Table 3)	Household member injured or killed	0.639	6.270	0.410	**0.042**
De Luca, Giacomo and Marijke Verpoorten	From vice to virtue? Civil war and social capital in Uganda	Generalized Trust (Table 5)	Battle days	-0.005	0.370	45.940	**-0.621**
De Luca, Giacomo and Marijke Verpoorten	Civil War and Political Participation: Evidence from Uganda	Attend Meeting (Table 4)	LRA Event Days 2001-2005	0.079	1.001	45.935	**3.626**
		Discuss Politics (Table 4)	LRA Event Days 2001-2005	0.061	0.665	45.935	**4.215**
		Voted (Table 4)	LRA Event Days 1996-2006	0.009	0.457	90.920	**1.792**
Garcia-Ponce, Omar and Benjamin Pasquale	How Political Violence Shapes Trust in the State	Trust for the President (Table 3)	Violence in the past 10 days	0.410	1.077	0.314	**0.120**
			Military force in the past 10 days	0.550	1.077	0.369	**0.188**
			Protests in the past 10 days	-0.490	1.077	0.485	**-0.221**
Kim, Young-Il and Jungmin Lee	Long Run Impact of Traumatic Experience on Attitudes toward Risk: Study of Korean War and Its Impact on Risk Aversion	Coefficient of Relative Risk Aversion (Table 5)	Early Childhood Exposure to Korean War	0.105	0.307	0.240	**0.082**
		Conservative Political Stance (Table 7)	Early Childhood Exposure to Korean War	0.687	3.060	0.240	**0.054**
Miguel, Edward, Sebastian Saiegh, and Shankar Satyanath	Civil War Exposure and Violence	Yellow cards (Table 2)	Years of civil war (country level)	0.008	2.730	4.740	**0.013**
Rohner, Dominic, Mathias Thoenig, and Fabrizio Zilibotti	Seeds of Distrust: Conflict in Uganda	Generalized Trust (Table 1)	All fighting	-0.002	0.466	45.960	**-0.203**

Notes: This review is limited to studies that estimate the association between conflict exposure and beliefs, attitudes, or behaviors related to politics. Studies estimating associations between war exposure and other outcomes are omitted, as are studies focusing on other determinants of political beliefs, attitudes, or behaviors. Studies in which standard deviations of the LHS or RHS variables of interest are not reported, and for which we could not obtain these numbers from the authors, are also omitted.

Appendix: Not for publication.

APPENDIX A. DATA APPENDIX

A.1. **Variables used from the Afrobarometer.** The Afrobarometer can be downloaded from `http://www.afrobarometer.org/`.

Variables re-ordered so that "good" outcomes are assigned higher values: q20 q21 q22 q24 q36a q36b q36c q38 q40 q42 q49 q50 q51 q53b q59.

"Agree with A/Agree with B" variables: q19 q20 q21 q22 q23 q24 q25 q26 q27 q38 q39 q40 q41 q42 q49 q50 q51 q59.

Other variables requiring recodes: For q53b, "10" was recoded as "2," and then 2 was subtracted from all values. For q37, "1" and "2" were recoded as "0," while "3" was recoded as "1".

The variables used to construct each of our ten principal indices are:

- *Voting*: q29 q30
- *Collective action*: q31a q31b q31c
- *Contact*: q32a q32b q32c q32d q32e q32f q32g
- *Refusal of bribes*: q57a q57b q57c q57d q57e q57f
- *Interest*: q16 q17 q18a q18b
- *Deference*: q19 q20 q25 q26 q27 q59 q40 q41 q42
- *Democracy*: q36a q36b q36c q37 q38 q39 q52a q53b
- *Equality*: q22 q21 q23 q24
- *Rule of law*: q49 q52d q52b q52c q50 q51
- *Trust*: q55a q55b q55c q55d q55e q55f q55g q55h q55i q55j

Our measure of self-reported living standards is q4b, "In general, how would you describe: Your own present living conditions?" Respondents were given choices ranging from "1 - Very Bad" to "5 - Very Good."

A.2. **Variables used from the PRIO data.** The PRIO data are downloaded from `http://www.prio.no/`

Conflicts assigned coordinates manually: The Uganda/Tanzania War (id 252) is given coordinates of -1,31.5 and a radius of 150. The Kivu Conflict (id 254) is given coordinates of -2.5, 28 and

a radius of 150. The Tuareg Rebellion (id 255) is given coordinates of 18, 6 and a radius of 150. The Djibouti-Eritrea Border Conflict (id 260) is given coordinates of 12.71, 43.13 and a radius of 50.

A.3. **Other Variables.** Population density is used to normalize battle deaths by area. This is taken from the United Nations Environment Programme, and is downloaded from `http://na.unep.net/datasets/datalist.php`.

APPENDIX B. ROBUSTNESS APPENDIX

B.1. **General robustness tests.** In the last two columns of Table A1, we present results in which the individual components of our aggregated outcome measures are used as dependent variables. The results here mirror the main results in Table 2 – the effects are generally small and statistically insignificant.

In Table A2, we change the definition of conflict exposure in three ways, and show the main effects remain small under these alternate definitions. In these specifications, we extend our measure of treatment so that a conflict that affects any part of a country or region now "treats" the entire region. For example, the battle deaths from the Nigerian Civil War are extended, in alternative specifications, to include all of Nigeria, or all of the provinces in Nigeria within the conflict's radius. Similarly, we define treatment by ethnicity. For any conflict-affected region, we code all members of an ethnic group who form at least 10% of the region's population as "treated" by the conflict, whether or not these individuals live within that region. This approach treats, for example, all Igbo in Nigeria as affected by the civil war, while treating all Hausa as un-affected. Results are again small and bounded tightly around 0.

Finally, we allow conflict exposure to enter (1) separately for three age groups between 0 and 14: $0-4$, $5-9$, and $10-14$. This mitigates any bias towards zero that could result from effects being concentrated at specific points in childhood, or having counter-veiling effects at different points before adolescence. Again, the effects are similar to the baseline results: small coefficients that are tightly bound around 0 (see Table A2).

In Table A3, we show that our null findings remain across different definitions of the fixed effects δ_r. We show that country, district, and survey cluster fixed effects all give similar results; results from a specification with no spatial fixed effect are also similar. In addition, adding ethnicity-specific fixed effects to the baseline, alongside the regional fixed effects, does not generally change the results. Finally, we also include fully flexible region x year of birth fixed effects, and find similarly small coefficients here as well.[5]

In Table A4, we show that including rainfall shocks experienced between ages 0 and 14 do not change the results. Rainfall data are taken from the standard Willmott, Matsuura and Collaborators' series, hosted by the University of Delaware.[6]

B.2. **Selective migration.** We also carry out checks specific to the concern that our results might be driven by selective migration out of conflict-affected areas.

In Table A5, we show that discarding the most populous district in each country, or the three most populous districts in each country; keeping only individuals in their "home" territories, as defined by their ethnicity as mapped by Murdock (1959); keeping only individuals living in communities where own ethnic group is larger than 10 percent of the population; and keeping only rural individuals does not appreciably change the results. These strategies are meant to remove the most likely destinations for migrants from the data, as well as restrict the sample to individuals who are unlikely to migrate given the ethnic homogeneity of their homeland or their rural status. Last, we exclude the *rural* sample and show results for ethnicity-level treatment on the urban sample, under the argument that the previous checks might exclude those individuals who would have been most affected by conflict exposure (as they were the ones who endogenously migrated).

B.3. **Immediate responses.** In Table A7, we show that immediate responses to conflict are also small. In order to do this, we incorporate data from the fourth round of the Afrobarometer. Because the questions included in both waves of the Afrobarometer differ, we cannot construct the same aggregate measures of political attitudes as in our baseline. Rather, we focus on a set of outcomes related to trust and deference that exist in both surveys. Similarly, because we only have latitude

[5]We also tried specifications with region-specific linear or quadratic trends, which are not reported in the Table A3 since they are subsumed by region x year of birth fixed effects, reported in the last row of the table.

[6]These are downloaded from `http://climate.geog.udel.edu/~climate/`.

and longitude coordinates for the third round of the Afrobarometer, we cannot join individuals to conflict by spatial location. Rather, we merge individuals to conflict using the district in which conflict occurred. We take spatial data on battle deaths in recent conflicts from the Uppsala Conflict Data Program.[7]. We use OLS to estimate:

$$y_{idt} = \beta exposure_{idt-1} + x'_{idt}\gamma + \delta_d + \eta_t + \epsilon_{idt} \tag{3}$$

Here, y_{idt} is a measure of trust or deference for individual i in district d in year t, standardized to have a $N(0,1)$ distribution. $exposure_{idt-1}$ is the number of battle deaths that occurred in the year prior to the survey in district d. This is 2004 for Round 3 and 2007 for Round 4. We also standardize this to have a $N(0,1)$ distribution. Controls in x_{idt} are age, age squared, female, urban, dummies for living standards, and dummies for education. Occupation was not asked in Round 4. δ_d and η_t are dummies for district and year, respectively. Standard errors are clustered by district. The standardized effects, as in our baseline estimates, are small.

B.4. **Missing values.** In Table A8, we report results of the main regressions (replicating the analyses in Table 2) using two different methods of addressing the problem of high rates of missing values that are differential across the various outcome aggregations we use.

The first method, reported in the first five sets of rows in Table A8, is to average over non-missing values within each outcome category. For example, if a respondent answered 5 out of 7 of the questions in the "Collective Action" grouping, we would create an average of those 5 questions with non-missing values, and ignore the answers to the missing two. Therefore, each respondent will have a non-missing value for the aggregated group variable as long as she answered at least one question within that grouping. The results in Table A8 show that this does not significantly alter the size or precision of the measured impacts.

In the second five sets of rows in Table A8, we report results of specifications for which values of missing outcomes were imputed using the mean of the particular grouping variable within integer

[7]UCDP Battle-Related Deaths Dataset v.5-2014, Uppsala Conflict Data Program, www.ucdp.uu.se, Uppsala University

age $\times$ exposure dummy cells. Again, we find that there are no significant changes in the size and precision of the coefficient estimates.

B.5. **Correlation with other measures of conflict.** In Table A10, we show that the location and intensity of conflict that we use is correlated with an alternative measure constructed from another source. The Armed Conflict Location and Event Dataset (ACLED) of Raleigh et al. (2012) has recorded the latitude and longitude coordinates of violent events in Africa since 1996. This limited time-span makes it unsuitable for our baseline regressions. We divide sub-Saharan Africa into 1° by 1° squares, and count the number of events occurring within each square for each year between 1996 and 2008. We then use our baseline PRIO data to count the number of battle deaths due to conflicts that overlap with these squares in the same year. We regress battle deaths on the number of events, square fixed effects, and year fixed effects in Table A10, and find a positive and significant correlation. This remains if binary indicators are used for both events and battle deaths.

Further, we show that the ethnic-based measure of exposure to the Nigerian Civil War used in Akresh et al. (2012a) is correlated with our baseline measure in the Nigerian sub-sample. Following their approach, we treat the Anang, Efik, Ekoi, Ibibio, Ijaw, Ogoni, Itsekiri, Isoko, Urhobo, and Igbo as "treated" ethnicities (the Adoni, Adun, Izon, and Ogori do not appear in our data). For these ethnicities, the measure of treatment is the number of years of life between ages 0 and 14 that overlap with the period 1967-1970. For all other ethnicities, exposure is zero. We regress battle deaths on this measure of exposure, region fixed effects, and year of birth fixed effects in Table A10, and find a positive and significant correlation.

Table A1. Summary statistics: All Components of Outcome Groupings

	(1) Mean	(2) s.d.	(3) Min	(4) Max	(5) N	(6) Coef.	(7) s.e.
Voting							
Registered to vote - binary	0.80	0.40	0	1	25,311	0.00926	(0.0106)
Voted in last elections - binary	0.75	0.43	0	1	25,278	0.00265	(0.0113)
Collective Action							
Attend community meetings	2.28	1.29	0	4	25,228	0.00916	(0.0110)
Join others to raise an issue	1.83	1.28	0	4	25,133	-0.00300	(0.0110)
Attend demonstration or protest march	0.71	0.94	0	4	24,692	0.00106	(0.0131)
Active in religious group	1.26	0.93	0	3	25,338	0.00465	(0.0122)
Active in trade union	0.35	0.75	0	3	25,053	-0.0154	(0.0121)
Active in professional association	0.22	0.62	0	3	24,937	-0.00109	(0.0134)
Active in community development/self help org.	0.47	0.85	0	3	25,042	-0.0336***	(0.0126)
Contact							
Contact local government councillor	0.48	0.91	0	3	24,207	0.00730	(0.0134)
Contact MP	0.18	0.58	0	3	24,206	-0.00189	(0.0134)
Contact ministry official	0.20	0.61	0	3	24,205	-0.0253*	(0.0136)
Contact political party official	0.28	0.72	0	3	24,242	-0.0152	(0.0130)
Contact religious leader	0.86	1.15	0	3	24,266	-0.000113	(0.0145)
Contact traditional ruler	0.49	0.97	0	3	22,958	0.0169	(0.0127)
Contact other influential person	0.37	0.82	0	3	24,067	0.0204	(0.0137)
Refusal of Bribes							
Refuse to pay bribe for document or permit	3.39	0.92	0	4	25,220	0.00578	(0.0135)
Refuse to pay bribe for school placement	3.54	0.77	0	4	25,223	0.00841	(0.0138)
Refuse to pay bribe for household service	3.46	0.80	0	4	25,108	0.0218*	(0.0121)
Refuse to pay bribe for medicine or medical attention	3.45	0.96	0	4	25,270	0.00484	(0.0137)
Refuse to pay bribe to avoid problem with police	3.39	0.95	0	4	25,199	0.0195	(0.0129)
Refuse vote buying in election	3.33	1.16	0	4	25,113	0.0378***	(0.0120)
Interest							
Interested in public affairs	1.89	1.07	0	3	25,114	0.00813	(0.0120)
Discuss politics often	0.93	0.73	0	2	25,085	0.000270	(0.0123)
Politics is too complicated	2.23	1.19	1	5	23,649	0.0321**	(0.0143)
People don't listen to respondent about politics	2.87	1.24	1	5	22,859	0.00734	(0.0135)
Knows MP	0.46	0.50	0	1	24,233	0.0216*	(0.0128)
Knows local councillor	0.45	0.50	0	1	23,523	0.0173	(0.0143)
Knows VP	0.52	0.50	0	1	24,236	0.000630	(0.0114)
Knows biggest party	0.65	0.48	0	1	24,294	-0.00984	(0.0117)
Knows term limits	0.45	0.50	0	1	24,254	0.0207*	(0.0120)
Knows constitutionality	0.16	0.37	0	1	24,221	-0.0148	(0.0120)
Deference							
Government v. people responsible for their well-being	3.03	1.63	1	5	24,161	0.0120	(0.0142)
Should question leaders v. respect them	3.68	1.45	1	5	23,997	0.0272**	(0.0132)
Free assembly v. government can ban organizations	3.40	1.46	1	5	24,374	0.0167	(0.0143)
Free assembly v. government can close newspapers	3.28	1.51	1	5	24,271	0.0163	(0.0130)
Free speech v. government can ban views	3.78	1.35	1	5	24,588	0.0257**	(0.0127)
Elected leaders should listen to people v. follow own ideas	4.14	1.16	1	5	23,905	0.0168	(0.0125)
Parliament represents people v. president represents people	3.76	1.29	1	5	23,850	0.0275**	(0.0128)
President bound by courts v. not constrained	3.72	1.36	1	5	24,253	0.0241*	(0.0129)
Should keep present system v. abandon it	3.25	1.51	1	5	24,264	0.0131	(0.0130)
Democracy							
Rejection of one party rule	3.88	1.33	1	5	24,509	0.0134	(0.0119)
Rejection of military rule	4.02	1.21	1	5	24,327	-0.00205	(0.0122)
Rejection of one-man rule	4.16	1.10	1	5	23,974	0.00370	(0.0117)
Democracy is preferable	0.74	0.44	0	1	21,414	0.0203	(0.0125)
Elections good v. elections bad	4.15	1.24	1	5	24,986	0.0139	(0.0132)
Many parties good v. many parties bad	3.48	1.50	1	5	24,715	0.0160	(0.0128)
Constitution represents hopes of nation	3.49	1.20	1	5	20,917	-0.00601	(0.0139)
Party competition does not lead to conflict	1.79	1.00	0	3	23,966	-0.0102	(0.0129)

Equality							
Leaders should treat all equally v. favor own group	4.23	1.19	1	5	24,126	-0.00145	(0.0132)
All should vote v. only educated should	3.74	1.48	1	5	24,021	0.0285**	(0.0136)
Women are equal v. subject to custom	2.23	1.50	1	5	24,191	0.00393	(0.0135)
Women are equal v. men better leaders	3.91	1.43	1	5	25,213	-0.0105	(0.0114)
Rule of Law							
Obey government no matter how voted v. only if voted for it	4.28	1.06	1	5	25,025	-0.0135	(0.0142)
Legal solutions v. immediate solutions	3.65	1.16	1	5	23,056	0.00720	(0.0135)
Violence never justified v. sometimes necessary	3.82	1.06	1	5	23,996	0.00640	(0.0134)
Court decisions are binding	3.81	1.09	1	5	24,520	0.0205	(0.0132)
Police can make people obey the law	4.04	1.21	1	5	24,960	-0.00439	(0.0146)
People must pay taxes	3.98	1.25	1	5	24,701	-0.0101	(0.0129)
Trust							
Trust the President	1.95	1.12	0	3	24,481	0.00409	(0.0115)
Trust Parliament/National Assembly	1.74	1.09	0	3	23,501	-0.00108	(0.0122)
Trust the Electoral Commission	1.71	1.11	0	3	22,753	0.00538	(0.0121)
Trust your local council	1.66	1.11	0	3	23,545	-0.000439	(0.0115)
Trust the ruling party	1.73	1.15	0	3	24,101	0.0109	(0.0124)
Trust opposition political parties	1.19	1.07	0	3	23,447	-0.00224	(0.0134)
Trust the military	1.90	1.10	0	3	24,246	-0.00731	(0.0113)
Trust the police	1.69	1.12	0	3	24,633	0.00255	(0.0115)
Trust courts of law	1.89	1.04	0	3	23,895	0.00311	(0.0118)
Trust government broadcasting service	2.03	0.99	0	3	22,944	-0.00135	(0.0130)
Trust independent broadcasting services	1.98	0.99	0	3	19,979	-0.00950	(0.0146)
Trust government newspapers	1.86	1.02	0	3	15,574	0.00313	(0.0148)
Trust independent newspapers	1.91	0.99	0	3	19,373	-0.0114	(0.0142)
Most people can be trusted	0.17	0.37	0	1	23,810	0.00260	(0.0118)
Trust relatives	2.22	0.94	0	3	24,279	-0.00748	(0.0124)
Trust neighbors	1.74	1.01	0	3	24,224	-0.0130	(0.0128)
Trust people from your ethnic group	1.68	1.00	0	3	22,870	-0.0242*	(0.0137)
Trust people from other ethnic groups	1.37	0.99	0	3	22,662	-0.0303**	(0.0133)

Table A2. Robustness: Changing the Definition of Conflict Exposure

	(1) Voting	(2) Collective action	(3) Contact	(4) Refusal of bribes	(5) Interest	(6) Deference	(7) Democracy	(8) Equality	(9) Rule of law	(10) Trust
Deaths by Region										
Battle Deaths	-0.0597***	0.00539	0.0172	0.0307**	0.0103	0.0418**	0.0172	-0.0273*	-0.00676	0.00970
(0-14)	(0.0128)	(0.0144)	(0.0150)	(0.0153)	(0.0143)	(0.0169)	(0.0170)	(0.0151)	(0.0165)	(0.0174)
N	*16,577*	*15,812*	*15,434*	*16,217*	*13,300*	*13,907*	*11,775*	*15,449*	*14,582*	*7,209*
Deaths by Ethnicity										
Battle Deaths	-0.0102	0.00614	0.0229**	0.00371	0.00988	0.0106	0.00137	-0.0137	0.00361	0.0237**
(0-14)	(0.00923)	(0.0103)	(0.00939)	(0.0105)	(0.00978)	(0.0111)	(0.0109)	(0.00992)	(0.0100)	(0.0115)
N	*16,894*	*16,075*	*16,621*	*16,506*	*14,287*	*14,914*	*11,898*	*16,638*	*14,781*	*7,847*
Deaths by Age Cat.										
Battle Deaths	0.0397***	0.00635	0.0131	0.00907	0.00170	0.0171	0.00671	0.00835	-0.0160	-0.00369
(0-4)	(0.0120)	(0.0101)	(0.0110)	(0.0110)	(0.0107)	(0.0128)	(0.0122)	(0.0114)	(0.0121)	(0.0133)
Battle Deaths	-0.0148	-0.0124	-0.000465	0.0171*	-0.00210	0.0245**	-0.00271	0.00339	-0.000838	0.00695
(5-9)	(0.00937)	(0.00934)	(0.0111)	(0.0104)	(0.0105)	(0.0106)	(0.0116)	(0.0112)	(0.0122)	(0.0149)
Battle Deaths	-0.00656	-0.00803	-0.0108	0.00488	0.0210**	0.0262**	0.0119	0.00147	0.0184*	0.0144
(10-14)	(0.00745)	(0.00877)	(0.00949)	(0.00954)	(0.00881)	(0.0102)	(0.0108)	(0.0102)	(0.0106)	(0.0115)
N	*17,780*	*16,950*	*16,621*	*17,372*	*14,287*	*14,914*	*12,470*	*16,638*	*15,573*	*7,847*

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Each cell represents a regression of a political attitude/behavior (column headers) on a conflict measure (row headers). All regressions include dummies for region of residence, year of birth, female, responses to "current living standards", level of education, occupation, and urban. The share of the respondent's ethnicity in the district population is also used as a control. Standardized coefficients are reported.

Table A3. Robustness: Changing the Level of Fixed Effects

	(1) Voting	(2) Collective action	(3) Contact	(4) Refusal of bribes	(5) Interest	(6) Deference	(7) Democracy	(8) Equality	(9) Rule of law	(10) Trust
No FE										
Battle Deaths	0.0259**	0.0367***	0.0480***	0.0343***	0.00164	0.00516	-0.0715***	0.0471***	-0.0165	0.143***
(0-14)	(0.0103)	(0.0109)	(0.0117)	(0.0128)	(0.0133)	(0.0134)	(0.0133)	(0.0142)	(0.0135)	(0.0162)
Country FE										
Battle Deaths	-0.00256	0.00350	0.0135	0.0100	-0.00249	0.0428***	0.00993	0.0155	0.00827	0.0444***
(0-14)	(0.0107)	(0.0112)	(0.0125)	(0.0117)	(0.0127)	(0.0143)	(0.0137)	(0.0124)	(0.0138)	(0.0160)
Region FE + no controls										
Battle Deaths	0.0104	-0.00155	0.00592	0.0225*	0.0199	0.0510***	0.0138	0.00897	0.00499	0.0127
(0-14)	(0.0108)	(0.0121)	(0.0135)	(0.0128)	(0.0141)	(0.0139)	(0.0144)	(0.0139)	(0.0149)	(0.0175)
District FE										
Battle Deaths	0.0121	-0.0124	-0.00125	0.0115	-0.00158	0.0540***	0.00296	-0.00677	-0.0116	0.0143
(0-14)	(0.0123)	(0.0122)	(0.0146)	(0.0134)	(0.0139)	(0.0155)	(0.0152)	(0.0149)	(0.0162)	(0.0190)
Cluster FE										
Battle Deaths	0.0240*	0.00206	0.000751	0.00882	-0.00807	0.0520***	-0.00110	-0.00126	-0.0106	0.0139
(0-14)	(0.0138)	(0.0143)	(0.0173)	(0.0161)	(0.0170)	(0.0189)	(0.0187)	(0.0180)	(0.0195)	(0.0231)
Region and Ethnicity FE										
Battle Deaths	0.00213	-0.0154	-0.00388	0.0121	0.00720	0.0528***	0.0101	0.00335	0.000159	-0.00189
(0-14)	(0.0111)	(0.0117)	(0.0134)	(0.0125)	(0.0133)	(0.0139)	(0.0147)	(0.0140)	(0.0154)	(0.0114)
Region x year of birth FE										
Battle Deaths	-0.0219	-0.0400*	-0.0242	0.0397	0.0486**	0.0452*	0.0349	0.0371	0.0201	-0.00569
(0-14)	(0.0197)	(0.0221)	(0.0211)	(0.0253)	(0.0201)	(0.0249)	(0.0259)	(0.0238)	(0.0252)	(0.0319)
N	17,780	16,950	16,621	17,372	14,287	14,914	12,470	16,638	15,573	7,847

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Each cell represents a regression of a political attitude/behavior (column headers) on a conflict measure (row headers). All regressions include dummies for region of residence, year of birth, female, responses to "current living standards", level of education, occupation, and urban. The share of the respondent's ethnicity in the district population is also used as a control. Mean effects are reported.

Table A4. Robustness: Controlling for Rain Shocks in Period of Exposure

	(1) Voting	(2) Collective action	(3) Contact	(4) Refusal of bribes	(5) Interest	(6) Deference	(7) Democracy	(8) Equality	(9) Rule of law	(10) Trust
Battle Deaths (0-14)	0.00670	-0.0108	0.000232	0.0243*	0.0146	0.0498***	0.0104	0.00953	0.00475	0.0138
	(0.0109)	(0.0118)	(0.0134)	(0.0129)	(0.0132)	(0.0139)	(0.0143)	(0.0139)	(0.0152)	(0.0171)
Rainfall Shocks (0-14)	-0.00583	-0.0170*	-0.00631	-0.00685	-0.00350	-0.000694	-0.00445	-0.00718	-0.0202**	-0.000178
	(0.00914)	(0.00954)	(0.0101)	(0.00979)	(0.00972)	(0.0100)	(0.0105)	(0.00933)	(0.0101)	(0.0108)
N	17,780	16,950	16,621	17,372	14,287	14,914	12,470	16,638	15,573	7,847

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Each cell represents a regression of a political attitude/behavior (column headers) on a conflict measure (row headers). All regressions include dummies for region of residence, year of birth, female, responses to "current living standards", level of education, occupation, and urban. The share of the respondent's ethnicity in the district population is also used as a control. Standardized coefficients are reported.

Table A5. Robustness: Addressing Selective Migration During Conflict

	(1) Voting	(2) Collective action	(3) Contact	(4) Refusal of bribes	(5) Interest	(6) Deference	(7) Democracy	(8) Equality	(9) Rule of law	(10) Trust
Excluding Most Populous District in Each Country										
Battle Deaths	0.007	-0.005	0.012	0.021	0.016	0.045***	0.016	0.006	0.000	0.013
(0-14)	(0.011)	(0.012)	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)	(0.015)	(0.016)	(0.018)
N	15,998	15,243	14,970	15,622	12,828	13,413	11,173	14,988	14,002	6,943
Excluding 3 Most Populous Districts in Each Country										
Battle Deaths	0.007	-0.008	0.009	0.010	0.013	0.047***	0.014	0.006	0.005	0.008
(0-14)	(0.012)	(0.013)	(0.015)	(0.014)	(0.015)	(0.016)	(0.016)	(0.016)	(0.018)	(0.019)
N	13,564	12,922	12,704	13,218	10,864	11,357	9,393	12,715	11,883	5,831
Keeping Only Individuals in Home Territory (Defined by Murdock's Map)										
Battle Deaths	0.0542***	-0.0168	-0.0130	0.0644***	0.0280	0.0526**	0.0434*	0.0243	0.0371	0.0285
(0-14)	(0.0183)	(0.0241)	(0.0226)	(0.0238)	(0.0223)	(0.0245)	(0.0257)	(0.0221)	(0.0256)	(0.0271)
N	7,025	6,678	6,916	6,865	5,774	6,182	4,806	6,930	6,079	3,324
Keeping Only Individuals in Home Region (Defined by Ethnicity)										
Battle Deaths	0.030**	-0.008	-0.014	0.065***	0.020	0.055***	0.006	0.027	0.005	0.017
(0-14)	(0.014)	(0.017)	(0.017)	(0.018)	(0.016)	(0.017)	(0.020)	(0.018)	(0.019)	(0.022)
N	9,022	8,634	8,915	8,859	7,779	8,020	6,318	8,908	7,902	3,727
Keeping Only Individuals Living in Communities where Own Ethnic Group >10%										
Battle Deaths	0.023*	-0.003	-0.009	0.042***	0.014	0.050***	0.012	0.011	0.011	0.025
(0-14)	(0.013)	(0.014)	(0.014)	(0.015)	(0.013)	(0.015)	(0.017)	(0.015)	(0.016)	(0.018)
N	14,186	13,499	13,976	13,862	11,986	12,528	9,967	13,965	12,431	6,483
Keeping Only Rural Communities										
Battle Deaths	0.012	-0.014	-0.003	0.012	0.004	0.046***	0.005	-0.003	-0.019	0.007
(0-14)	(0.013)	(0.014)	(0.016)	(0.014)	(0.016)	(0.017)	(0.018)	(0.017)	(0.018)	(0.020)
N	10,994	10,426	10,220	10,700	8,562	9,033	7,244	10,228	9,382	4,218
Ethnicity Level Treatment, Keeping Only Urban Communities										
Battle Deaths	0.00776	0.00941	0.0236*	0.00598	0.0187	0.0213	0.00863	-0.00520	0.0251*	0.0259
(0-14)	(0.0142)	(0.0157)	(0.0139)	(0.0169)	(0.0139)	(0.0157)	(0.0160)	(0.0143)	(0.0138)	(0.0169)
N	6,498	6,239	6,401	6,391	5,725	5,881	5,003	6,410	5,919	3,629

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Each cell represents a regression of a political attitude/behavior (column headers) on a conflict measure (row headers). All regressions include dummies for region of residence, year of birth, female, responses to "current living standards", level of education, occupation, and urban. The share of the respondent's ethnicity in the district population is also used as a control. Standardized coefficients are reported.

Table A6. Heterogeneous Effects of Ethnic Conflict Exposure by Ethnic Share in District

	(1) Voting	(2) Collective action	(3) Contact	(4) Refusal of bribes	(5) Interest	(6) Deference	(7) Democracy	(8) Equality	(9) Rule of law	(10) Trust
Ethnic War (0-14) x Ethnic Share in District	-0.0216**	-0.0237**	-0.0199*	-0.00900	0.0229**	0.0138	-0.0133	0.00551	0.0295***	-0.00619
	(0.00976)	(0.0103)	(0.0104)	(0.0118)	(0.00965)	(0.0111)	(0.0114)	(0.0101)	(0.0108)	(0.0119)
Ethnic War	0.0239	0.0338*	0.0688***	-0.0511***	0.0431**	-0.0176	0.0105	0.0221	-0.0156	-0.0819***
	(0.0175)	(0.0190)	(0.0191)	(0.0191)	(0.0182)	(0.0215)	(0.0209)	(0.0213)	(0.0212)	(0.0291)
Ethnic Share in District	0.0350***	-0.00459	0.0236**	-0.0233*	0.0121	0.00868	-0.00648	0.00599	-0.00139	0.0187
	(0.0117)	(0.0122)	(0.0108)	(0.0119)	(0.0107)	(0.0131)	(0.0138)	(0.0116)	(0.0123)	(0.0141)
N	14,182	13,567	13,438	13,864	11,988	12,076	10,007	13,467	12,530	6,227

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Each column by pair of rows above represents a regression of a political attitude/behavior (column headers) on battle deaths interacted with either age, ethnic share in district, or a female dummy (see row headers). All regressions include dummies for region of residence, year of birth, female, responses to "current living standards", level of education, occupation, and urban. The share of the respondent's ethnicity in the district population is also used as a control. Standardized coefficients are reported.

Table A7. Robustness: Immediate Responses to Conflict

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Ethnic group's economic conditions	Ethnic group's political influence	Ethnic group treated unfairly	Ethnic or national identity	Trust the President	Trust Parliament / National Assembly	Trust the Electoral Commission	Trust your local council	Trust the ruling party	Trust opposition political parties
Battle Deaths	-0.0108	0.00511	0.00887	-0.00206	-0.0316***	0.00340	-0.0186*	0.0132	-0.0144	0.0187*
	(0.00690)	(0.0159)	(0.00638)	(0.00792)	(0.0107)	(0.0136)	(0.0109)	(0.0107)	(0.0110)	(0.0109)
N	35,753	34,682	34,017	36,594	39,085	37,739	36,759	37,634	38,450	37,448
	Trust the police	Trust courts of law	Trust relatives	Question actions of leaders vs. respect authority	Government bans organizations vs. join any	Government close newspapers vs. free to publish	Govt. suppress expression vs. people speak minds	Parliament makes laws vs. president does	Time to deal with problems vs. try another form	President free to act vs. obey the laws and courts
Battle Deaths	0.0186	0.0275**	0.00689	0.0558***	0.00287	-0.00210	0.000851	-0.0134	-0.0203***	0.00774
	(0.0133)	(0.0133)	(0.0127)	(0.0152)	(0.0106)	(0.00759)	(0.0102)	(0.0136)	(0.00668)	(0.00845)
N	39,321	38,198	39,371	38,729	38,757	38,766	38,867	37,875	38,238	38,381

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Each cell represents a regression of a political attitude/behavior (column headers) on a conflict measure (row headers). All regressions include dummies for district, round, female, responses to "current living standards", level of education, and urban. Age and age squared are also used as controls. Standardized coefficients are reported.

Table A8. Imputed Missing Values: Effects of Early-life Conflict Exposure on Political Attitudes and Behaviors

	(1) Voting	(2) Collective action	(3) Contact	(4) Refusal of bribes	(5) Interest	(6) Deference	(7) Democracy	(8) Equality	(9) Rule of law	(10) Trust
Averaging Over Non-Missing Values within Each Outcome Category										
Battle Deaths	0.00579	-0.00898	0.00997	0.0231*	0.0155	0.0481***	0.0117	0.0106	-0.000112	-0.00479
(0-14)	(0.0108)	(0.0115)	(0.0119)	(0.0126)	(0.0108)	(0.0118)	(0.0121)	(0.0123)	(0.0140)	(0.0111)
N	*17,866*	*17,866*	*17,866*	*17,866*	*17,866*	*17,866*	*17,866*	*17,866*	*17,866*	*17,866*
Battle Deaths	-0.0150	0.0341	0.0210	0.0337	0.00906	0.0224	-0.0114	0.00983	0.0152	0.0151
(0-14) per 1000	(0.0177)	(0.0251)	(0.0249)	(0.0233)	(0.0187)	(0.0211)	(0.0225)	(0.0243)	(0.0227)	(0.0183)
N	*11,996*	*11,996*	*11,996*	*11,996*	*11,996*	*11,996*	*11,996*	*11,996*	*11,996*	*11,996*
Battle Deaths	-0.00294	0.0295	0.0304	0.0153	0.00902	0.0433*	-0.0287	0.0113	0.0172	-0.0151
(0-14) per area	(0.0155)	(0.0244)	(0.0275)	(0.0235)	(0.0202)	(0.0231)	(0.0199)	(0.0267)	(0.0246)	(0.0212)
N	*11,996*	*11,996*	*11,996*	*11,996*	*11,996*	*11,996*	*11,996*	*11,996*	*11,996*	*11,996*
Exposure dummy	-0.0568***	-0.0124	0.0298	-0.00434	-0.0406*	0.0282	0.0141	0.0114	-0.00904	-0.0121
(0-14)	(0.0206)	(0.0217)	(0.0237)	(0.0212)	(0.0211)	(0.0217)	(0.0215)	(0.0211)	(0.0206)	(0.0198)
N	*17,866*	*17,866*	*17,866*	*17,866*	*17,866*	*17,866*	*17,866*	*17,866*	*17,866*	*17,866*
Years of exposure	-0.00845***	-0.00506*	-0.00266	0.00748**	-0.000218	0.00591**	0.000896	0.000809	0.000497	0.000841
(0-14)	(0.00259)	(0.00262)	(0.00267)	(0.00290)	(0.00233)	(0.00287)	(0.00262)	(0.00265)	(0.00281)	(0.00245)
N	*17,866*	*17,866*	*17,866*	*17,866*	*17,866*	*17,866*	*17,866*	*17,866*	*17,866*	*17,866*
Imputing Missing Values Using Means within Age x Exposure Dummy Cells										
Battle Deaths	0.00625	-0.0121	-0.000270	0.0247*	0.00723	0.0425***	0.00409	0.0116	-0.000150	0.107***
(0-14)	(0.0108)	(0.0114)	(0.0121)	(0.0127)	(0.0111)	(0.0120)	(0.0126)	(0.0121)	(0.0143)	(0.0101)
N	*17,866*	*17,866*	*17,866*	*17,866*	*17,865*	*17,866*	*17,865*	*17,866*	*17,866*	*17,852*
Battle Deaths	-0.0153	0.0331	0.0198	0.0381	0.0120	0.0329	-0.0287	0.0104	0.0216	-0.00917
(0-14) per 1000	(0.0177)	(0.0252)	(0.0260)	(0.0238)	(0.0188)	(0.0223)	(0.0236)	(0.0244)	(0.0233)	(0.0191)
N	*11,996*	*11,996*	*11,996*	*11,996*	*11,995*	*11,996*	*11,995*	*11,996*	*11,996*	*11,982*
Battle Deaths	-0.00303	0.0336	0.0296	0.0168	0.0113	0.0528**	-0.0451**	0.0102	0.0238	-0.00484
(0-14) per area	(0.0155)	(0.0241)	(0.0285)	(0.0242)	(0.0219)	(0.0239)	(0.0192)	(0.0268)	(0.0256)	(0.0222)
N	*11,996*	*11,996*	*11,996*	*11,996*	*11,995*	*11,996*	*11,995*	*11,996*	*11,996*	*11,982*
Exposure dummy	-0.0576***	-0.00896	0.0433*	-0.000863	-0.0228	0.0264	-0.0228	0.00905	0.000862	0.0511**
(0-14)	(0.0205)	(0.0217)	(0.0246)	(0.0216)	(0.0219)	(0.0225)	(0.0225)	(0.0213)	(0.0210)	(0.0216)
N	*17,866*	*17,866*	*17,866*	*17,866*	*17,865*	*17,866*	*17,865*	*17,866*	*17,866*	*17,852*
Years of exposure	-0.00849***	-0.00511*	-0.00282	0.00808***	0.000152	0.00606**	-0.00330	0.000782	0.00125	0.0124***
(0-14)	(0.00259)	(0.00261)	(0.00274)	(0.00292)	(0.00242)	(0.00289)	(0.00260)	(0.00264)	(0.00291)	(0.00246)
N	*17,866*	*17,866*	*17,866*	*17,866*	*17,865*	*17,866*	*17,865*	*17,866*	*17,866*	*17,852*

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Each cell represents a regression of a political attitude/behavior (column headers) on a conflict measure (row headers). All regressions include dummies for region of residence, year of birth, female, responses to "current living standards", level of education, occupation, and urban. The share of the respondent's ethnicity in the district population is also used as a control. Standardized coefficients are reported.

Table A9. List of 10 Most Severe Wars

Location	Start	End	Battle Deaths/Year
Algeria	1954	1962	20281
Nigeria	1967	1970	18750
Eritrea, Ethiopia	1998	2000	16667
Madagascar	1947	1947	9566
Mozambique	1977	1992	8768
Ethiopia	1962	1991	8751
Chad, Libya	1987	1987	8500
Angola	1961	1974	5643
Angola	1975	2002	5296
Cameroon	1960	1984	4640

Table A10. Robustness: Correlation with other measures of conflict

	(1)	(2)	(3)	(4)	(5)	(6)
	Battle deaths		Any deaths		Battle deaths	
	Panel of 1 X 1 squares				*Nigerian sample*	
Battle events	593.693**	305.620**				
	(246.648)	(136.027)				
Any event			0.328***	0.117***		
			(0.016)	(0.010)		
Akresh et al. (2012) measure					1.067***	0.953*
					(0.156)	(0.518)
Observations	28,015	28,015	28,015	28,015	1,792	1,792
Fixed effects	None	Square + Year	None	Square + Year	None	Region + Y.O.B.
Standard errors clustered by	Square				Survey Cluster	

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. All regressions estimated by OLS.