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IZA DP No. 13069

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ISSN: 2365-9793

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ABSTRACT

The Heterogeneous Effects of Conflict on Education: A Spatial Analysis in Sub-Saharan Africa*

In this paper, we identify under which conditions and to what extent armed conflicts harm the long-run educational attainment of children in rural Sub-Saharan Africa. By combining 66 rounds of DHS surveys with geo-coded conflict information, our study contextualizes the findings of a series of country-specific case studies on the effects of conflict on education, and provides evidence on the mechanisms through which these effects occur. Our main identification strategy compares educational losses of youth living within the same household, while also controlling for local weather shocks and countrywide dynamics in education. The effects of conflict on education are strongly context dependent. High-intensity conflicts reduce local educational attainment, on average, although this effect becomes insignificant in strong autocracies. By contrast, education is generally unaffected by localized low-intensity conflict. Human capital loss due to conflict is most severely felt in weak states, and in response to non-state based conflicts, highlighting the importance of state capacity in mediating the educational costs of local conflicts.

JEL Classification: I25, D74, O12

Keywords: education, years of schooling, conflict, Sub-Saharan Africa

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* We would like to thank Toke Aidt, Richard Bluhm, Johannes Croisier, Christopher Ellis, Andreas Fuchs, Robert Genthner, Andreas Kammerländer, Andreas Landmann, Jo Thori Lind, Matthew Rudh, Günther G. Schulze, Philip Verwimp, and seminar and workshop participants in Antwerp, Göttingen, Pontresina, Wageningen; at the EPCS conference in Budapest, ESPE conference in Glasgow, AEL Conference in Göttingen, and the HiCN workshop in Medellin, for their useful comments and discussions. All remaining errors are ours.

1 Introduction

Education is a basic human right and a crucial determinant of economic and social development. However, despite recent global development goals pushing towards universal education, around 61 million children worldwide are still unable to attend primary school (UNESCO, 2017). Strikingly, nearly half of these children live in conflict-affected countries, which raises the question about the role of conflict in deterring education (UNICEF, 2017).

The literature on the effects of conflict on education is substantial, yet the available case studies yield partly contradictory results (e.g. Akbulut-Yuksel, 2014; Arcand and Wouabe, 2009; León, 2012). They present a wide range of estimates, spanning from positive effects (0.2 years more of education per conflict year) to negative effects of 0.9 years of education lost (e.g., Akbulut-Yuksel, 2014; Arcand and Wouabe, 2009; Bertoni et al., 2019; León, 2012; Lee, 2014; La Mattina, 2018; Shemyakina, 2011). These country-specific studies focus on various types of conflicts with distinct intensities and actors within specific country contexts, and fail to generalize their results to a wider range of violent conflicts and their determinants.

This study contributes to close this gap by identifying under which conditions and to what extent conflict exposure during childhood influences the subsequent educational attainment of youth in rural areas of Sub-Saharan Africa. We consider conflict characteristics (conflict severity, as measured by the conflict's yearly death toll and the types of conflict actors), individual characteristics (age of exposure to conflict and gender) as well as country and location characteristics (political regime type, economic development, ethnic fractionalization, availability of natural resources) as important determinants of conflict effects. These conditions either follow the previous literature (e.g., León, 2012) or are closely linked to the concepts of state capacity and public goods provision – the two channels we investigate in detail in this paper. By extending the geographical scope of the analysis to a regional sample of 31 countries in Sub-Saharan Africa, our empirical study generalizes the findings of country-specific case studies on the costs of conflict in terms of lost education. Sub-Saharan Africa provides the optimal setting for such a study, being one of the most conflict-ridden regions in the world. Almost half of all armed conflicts during the past 40 years have taken place in Sub-Saharan Africa (Arcand and Wouabe, 2009). At the same time, education is still far from universal in many countries of this region.

Our empirical analysis combines data from 66 Demographic and Health Surveys (DHS) with geo-coded information on conflict events, provided by the Uppsala Conflict Data

Program (UCDP) (Sundberg and Melander, 2013). We determine potential conflict exposure through an exact measure of the distance between the current survey location of the 10 to 26 year old in the DHS sample and the geo-coded location of past conflict events, calculating potential exposure to violent events that have occurred within a 50 km radius of the survey location during different age periods. We measure only potential, and not actual, conflict exposure, as the DHS does not systematically record full migration histories. In order to mitigate the issue of wrongly attributed conflict history, we only focus on rural areas of Sub-Saharan Africa, as rural areas tend to have fewer migrants and the majority of displaced people either return to their homes after conflict or move to urban areas (IDMC, 2018; Awumbila, 2017). We further assess the scope of the potential bias by repeating our analysis for a sub-sample of data with migration information.

Any analysis of the link between conflict and education faces a central endogeneity problem: Omitted factors may both explain a higher local propensity to experience violent conflict at any given point in time and a lower quality of public goods provision or higher labor market incentives to drop out of school early. We address this potential omitted variable bias by including an extensive set of fixed effects along two main dimensions. First, household (or location) fixed effects absorb the geographic and socio-economic variation in the average propensity to experience conflict. Second, by including country-specific birth cohort fixed effects, we capture yearly changes in the national economic, conflict, and political environment. In our preferred specifications, we combine these two sets of non-nested fixed effects to control for common drivers of education and conflict exposure across households as well as over time within any country. We also account for time-variant local economic shocks by controlling for the local presence of weather shocks, which contribute to explaining conflict (Miguel et al., 2004; Harari and Ferrara, 2018), but also other local economic outcomes. We additionally validate our main results by using an interaction of weather shocks with the distance to the next ethnic border as an instrumental variable for past conflict exposure. In doing so, we control for the direct effects of weather shocks and ethnic diversity on schooling, and identify the effects of conflict exposure on education through the heterogeneous effect of extreme weather events due to ethnic fractionalization.

Our results show that exposure to low-intensity conflict cannot be robustly linked to educational attainment in rural Sub-Saharan Africa. But, conflicts of more substantial severity are robustly linked to average losses in local education. One additional conflict year of high intensity (with at least 1000 casualties) reduces average education by 1.4 months. The effects of conflict concentrate among the most exposed children: Among the upper 5% of children living in the most violence prone sample locations, the average losses from high-intensity conflicts reach up to 7 months.

Conflicts are especially harmful to education when state capacity is impaired. Our results show that especially conflicts by non-state actors, which are more likely to target schools during conflict, lead to educational losses. Strongly autocratic systems are more successful at sustaining public goods provision during periods of intense violence compared to weak states and strong democracies. Education in natural resource rich locations is also strongly affected by conflict. Rebel groups may strategically capture natural resources, financing their actions and harming public goods provision even in case of a conflict of similar intensity. When we compare exposure at different age periods, we find no differences on average, confirming both the early childhood as well as fetal origin theories. With respect to gender, high-intensity conflict seems to affect boys' education somewhat more negatively than girls' education.

The remainder of the paper is organized as follows. Section 2 offers a theoretical background, section 3 describes the data, while section 4 outlines the empirical strategy. Section 5 presents the empirical results and discusses issues of identification. Section 6 shows further robustness checks, while section 7 concludes.

2 Violent conflicts and education

Violent conflicts affect both the supply and demand for education through a wide variety of channels.

At a basic level, conflicts can directly affect the demand for education. In armed conflicts, pupils are often fighting, fleeing, or hiding instead of attending school (Sommers, 2002). Young boys are especially easy recruitment targets for armies and rebel groups, as they are easier to manipulate than adults and rarely require to be paid for their service. The fear of landmines or other physical dangers, as well as sexual violence, on the way to school often cause school drop-outs, especially among girls (Justino, 2011). Large-scale conflicts also provoke humanitarian crises, leading to massive displacement and migration that is usually very disruptive to education (Justino, 2011). Moreover, conflict affects school outcomes indirectly by reducing the incentives to invest into education. Epidemics or economic crises caused by conflicts can also inhibit necessary investments into skills (Cunha and Heckman, 2007). Physical destruction of schools increases the costs of education, through higher transportation costs of reaching the nearest school, for instance. Violent conflicts also shape the local labor market, reducing the returns to education.

The mental and physical health of school-aged children can be affected by conflict as well. Injuries, deaths, and traumatic experiences leading to psychological distress may cause children to stay home from school, compromising their cognitive and non-cognitive development (Justino, 2011; Sommers, 2002). Additionally, environmental shocks, both in utero and in early childhood, can affect neurological development and basic mental faculties that determine future abilities (Cunha and Heckman, 2007; Barker, 1995; Almond and Currie, 2011).[1] In this regard, conflict experiences before and immediately after birth may actually affect a child's future educational attainment in a number of ways.

From a supply standpoint, violent conflicts often destroy physical capital like school buildings and road infrastructure, reducing the state's ability to provide universal access to education. The loss of human capital through teacher absence further compromises the functioning of school systems and can lead to productivity losses in the long run (Lai and Thyne, 2007). During conflicts, public funds may be redirected from education towards expenditures on the military. Beyond these direct effects on education provision, a decrease in economic development from a violent conflict can lead to reductions in the demand for public goods, diminishing their marginal returns. Moreover, it can decrease tax revenues diminishing the financial resources of the government (Besley and Persson, 2014).

Education is one of the most common state-provided public goods (Daviet et al., 2016). Thus, the supply of education depends directly on state capacity, which includes the financial resources, administrative knowledge, and military and political power to establish and maintain well-functioning institutions that provide public goods and services. Countries with lower state capacity, i.e. those with poorer incomes or less quality of governance, provide fewer public goods on average, resulting in lower levels of human capital development (Besley and Persson, 2014). Moreover, the need to redirect funds from education to military activities in times of conflict is especially high when state capacity is limited (Lai and Thyne, 2007).

Violent conflicts and state capacity have common underlying roots and influence each other (Besley and Persson, 2010, 2014). While conflicts can limit state capacity, so too can the lack of adequate state capacity enable the spread of violence. The decision of a population to rebel is partially based on a state's capacity to repress insurgencies and

[1] See Currie and Almond (2011) and Bharadwaj and Vogl (2016) for reviews of the related literature.

accommodate grievances (Hendrix, 2010). Likewise, the under-provision of basic social security decreases the opportunity costs of fighting for the local population (Collier and Hoeffler, 2004). Hence, countries with lower state capacity are more likely to face revolts and at the same time have fewer resources to counteract them. Moreover, violent conflicts influence the decision to invest in state capacity in the future, preventing the establishment of robust and well-functioning institutions. This creates a volatile system of short-term rather than long-term goals, which raises the susceptibility to external shocks. Hence, we expect state capacity to be an important moderator in the relationship between conflict and education. Education in wealthier regions may be less affected by conflicts due to lower budget constraints.

Political systems face distinct incentives to invest in state capacity development: Democratic politicians direct policies to the median voter to gain the highest share of votes in order to stay in office (Downs, 1957), resulting in a high level of public goods provision following the interests of the majority. On the contrary, autocratic regimes favor transfers that target the elite (Deacon, 2009), resulting in a lower level of public goods provision. Strong states have a robustly functioning administrative system that is less susceptible to shocks. Additionally, they have more financial resources at their disposal, which protects funds appropriated to basic services from being redirected towards military engagements. Since autocratic regimes, generally speaking, direct higher amounts of public funds to armed forces rather than public goods and services provision (Deacon, 2009), the likelihood of redirection in the face of conflict is even smaller. Hence, we would expect that educational losses caused by violent conflicts are the most pronounced among weak states, followed by strong democracies, and are the least pronounced among strong autocracies.

Revenues from sources other than taxation, like natural resources, are often more volatile as they depend on the world market demand, which raises vulnerability and reduces long-term investments. As revenues from natural resources do not require investments into administrative capacity, resource-rich countries tend towards lower state capacity, on average (Besley and Persson, 2014). Moreover, natural resources are often targeted by rebels because of their easy extraction and high monetary value, providing a good source of financing. If rebel groups succeed capturing natural resources, governmental revenues drop sharply, further damaging public goods provision. Hence, the local presence of natural resources may fuel localized conflicts and further increase educational losses.

The level of public goods provision further depends on the ethnic diversity of a population (Habyarimana et al., 2007). A more heterogeneous population represents a greater variety of tastes and preferences, raising the cost of governance as well as the need for cooperation, and resulting in the possible under-provision of public goods. During conflicts, group identification is often used to mobilize fighters, which can further increase the existing social divide and reduce investments into state capacity, worsening the decline in educational provision during crisis.

3 Data and empirical strategy

3.1 Data sources

Our analysis relies on two main data sources: household data from the DHS program and the geo-referenced conflict event dataset from UCDP. The DHS program offers globally standardized, nationally representative household surveys for a large number of countries including all kinds of socio-demographic characteristics (ICF, 2016). We use the most recent rounds of the standard DHS surveys (2001 to 2016), which include geo-located data on survey location accurate to less than 5 km for most observations (ICF, 2016).[2] Since we measure conflict exposure by direct geodesic distance to the survey location and since distance translates to substantially different travel times in urban as compared to rural areas, we restrict our sample to rural areas of Sub-Saharan Africa only. More importantly, past migration experiences that could potentially bias our estimates downwards by causing us to assign past conflict history to unaffected youth are less prevalent in rural areas than urban areas, as the overall migration patterns in Sub-Saharan Africa show a clear rural to urban trend (Awumbila, 2017; IDMC, 2018).[3]

The UCDP geo-referenced Event Dataset (GED, Version 5.0) provides conflict event data for the years 1989 to 2015 (Sundberg and Melander, 2013). It contains information on the date and location of conflict events as well as the estimated number of fatalities. When restricted to Sub-Saharan African countries, the dataset reports 26,970 conflict events for the period of 1989 to 2015.

Additionally, we use precipitation anomalies (at a resolution of 0.5×0.5 decimal degrees) taken from the SPEIbase v.2.5 dataset to control for local weather extremes and the arising shocks to the local economy. The SPEIbase measures precipitation anomalies with

[2] DHS randomly displaces the GPS codes of survey locations to secure confidentiality. For most of the clusters, displacement occurs within a radius of 5 km. 1% of the rural clusters is displaced within a radius of 10 km.

[3] In the sub-sample that record past migration experience, 45% of urban youth live in households that have migrated within the past 25 years, whereas only 29% of rural youth are part of a household that has a migration history.

a standardized z-score based on monthly precipitation and potential evapotranspiration information (Vicente-Serrano et al., 2010). The geographical information on location of ethnic groups and regional capitals used in the instrumental variable approach is taken from the Nardov Mira geo-referenced ethnic groups (GREG) dataset (Weidmann et al., 2010) and the world map of national capital cities. The GREG dataset is a digital version of the Atlas Narodov Mira showing the geographical location of ethnic groups. It reports 8,969 polygons marking various ethnic homelands. We use the borders of the polygons to measure ethnic diversity and potential inter-ethnic tensions.

For the heterogeneity analysis, income per capita of a country is taken from the Word Development Indicators provided by the World Bank (WorldBank, 2016) and ethnic diversity of a country is measured by the ethnic fractionalization index proposed by Montalvo and Reynal-Querol (2005). To quantify the impact of political quality and democracy, we use the polity2 variable from the Polity IV Project, which ranks a political regime's form of government on a scale ranging from –10 (strong autocracy) to +10 (strong democracy), and is among the most widely used data sources in political science (Marshall et al., 2019). We proxy for local GDP and economic development using satellite data on intensity of nighttime lighting, gathered from the geographic data center of the National Oceanic and Atmospheric Administration's (NOAA) Earth Observatory Group (NOAA, 2019). We use version 4 of the DMSP-OLS Nighttime Lights Time Series, which provides yearly average visible stable lights at cloud free coverage for the years 1992 to 2013 and aggregate it to the resolution of 0.5×0.5 decimal degrees. To identify survey locations rich in natural resources, we utilize the major mineral deposit of the world dataset of the U.S. Geological Survey (USGS) (Schulz and Briskey, n.d.). The dataset provides the geographical location of deposits of major non-fuel mineral commodities.

3.2 Measurement

Our outcome variable of interest is educational attainment, which we measure by the reported number of completed school years in the DHS. We determine potential conflict exposure during childhood by combining an individual's birth year with their residence as reported in the survey. We restrict our dataset to individuals born between 1990 and 2003 and thus aged 10 to 26 years at the time of the survey for whom we can observe a full conflict history starting from their pre-birth year.[4]

For our main explanatory variable, we utilize the UCDP dataset. Based on the UCDP's

[4] We only include children starting from the age of 10 years as the multitude of factors determining delays in school entry would confound our regressions for younger children.

definition, we consider a conflict year to be one in which at least one conflict event took place within a 50 kilometer radius of the survey location.[5] According to this definition, about 12% of all childhood years in the sample were conflict years.

In order to distinguish between the effects of conflicts of varying intensities, we estimate a variety of models by gradually adjusting our definition of a conflict year according to the number of casualties in a given year (based on the best estimate category in the UCDP dataset). We re-define a conflict year as a one in which at least one conflict event has taken place within 50 km of the location of interest and in which the conflict events resulted in at least N deaths, with N ranging from 0 to 5,000.[6] We measure potential conflict exposure, C_{jct}, for an individual currently living in location j in country c and born in year t, as follows:

$$C_{jct} \quad = \sum_{\tau=t-1}^{t+12} \mathbb{1}(\text{No. Deaths}_{jc\tau} \geq N); \quad N \in [0, 5000], \tag{1}$$

where $\mathbb{1}$ indicates years in which battle-related deaths in the local neighborhood reached at least N. Total conflict exposure is measured as the sum of all conflict years over the full childhood period, beginning in utero and lasting until the age of 12. We then rank conflict exposure by intensity, using a threshold of 1000 battle-related deaths; conflict years with fewer than 1000 deaths are defined as "moderate-intensity" while years with 1000 or more battle-related deaths are defined as "high-intensity".[7]

For the heterogeneity analyses, we use an alternative measure of conflict intensity by summing up the total number of battle-related deaths and taking the logarithmic transformation, using the inverse hyperbolic sine function. For further analyses, we categorize conflict exposure years by the type of violence, as classified in the UCDP dataset, as well as the critical age periods at which the conflict occurs in an individual's life. The UCDP dataset recognizes three distinct categories of violent conflict: (1) state-based conflicts, directly involving a state government; (2) non-state based conflicts, involving violence between two non-governmental organized actors; and (3) one-sided violence against civilians, which can be perpetrated by any organized actor (Sundberg and Melander, 2013).

[5] Further robustness checks repeat our results for 25, 100 and 200 km.

[6] About 40.5% of all youth in the sample have potentially experienced at least one conflict year of any severity; 11% experienced at least one conflict year with 200 deaths; 5% for conflicts passing the 1,000 deaths threshold and about 2.4% of children experienced conflicts with 5,000 or more deaths.

[7] Only outright civil wars surpass the threshold of 1000 battle-related deaths. Thus, "moderate-intensity conflict" still includes instances of very substantial violence. We distinguish these from instances of starkly extreme violence, labelled as "high-intensity conflict".

Regarding "critical age periods", we follow the literature and distinguish between in utero (in the year preceding the birth year), early childhood (at age 0 to 3), pre-school age (age 4 to 6) and primary school age (age 7 to 12).[8]

We control for location-specific economic shocks by measuring extreme weather events. We base our extreme weather indicators on the SPEI index, measured at a 12-month scale. Months with SPEI values below –1.5 in a given grid-cell of 0.5 degrees are defined as beeing affected by a drought, and those with SPEI values above 1.5 are defined as being affected by a rainfall shock. The band of +/–1.5 standard deviations is based on the SPI classification system of McKee et al. (1993). We then calculate potential exposure to economic shocks during an individual's childhood as the sum of all past months during which the individual was subjected to drought or rainfall-shock periods separately. We link this grid data to our DHS dataset by choosing the grid cell with the closest centroid to the survey location (within a distance of 200 km).

To analyze the various channels through which conflict may affect education, we first classify countries based on their system of government; countries with polity2 scores below -5 for at least 10 of the included time periods are classified as strong autocracies, while those with polity2 scores above 5 for at least 10 years are classified as democracies. Next, we classify a location as being rich in natural resources if it is located within 50 km of a natural resource deposit. Geographic localities are further categorized based on whether they are above or below median values for ethnic fractionalization, income per capita, and local nighttime light intensity, on average, over time.

Restricting the sample to rural areas results in a dataset of 541,480 observations. Among these observations, 31 countries, 428 regions and 19,652 survey locations are represented. All included surveys are listed in table A1 in the appendix, and table 1 reports summary statistics. Youth in the sample have on average 3.8 years of schooling and about 1.6 years of exposure to any conflict during their childhood. Figure 1 maps the average number of conflict years during childhood in all survey locations, whereas figure 2 shows the average educational attainment per survey location. Detailed definitions of all variables are displayed in table A2 in the appendix.

[8] We use the year of birth, as no consistent information is available on the birth month. This introduces measurement error, in particular biasing our estimates on in utero conflict exposure downward.

3.3 Econometric model

We exploit the spatial and temporal variation in potential conflict exposure to infer the average effect of violent conflicts on educational attainment. Our econometric model regresses the number of years of completed education Y_{ihjct} of an individual i from household h, who is currently residing in survey location j, within country c, and was born in year t, on their potential conflict exposure during childhood C_{jct} (see eq. 1) and further controls:

$$Y_{ihjct} \quad = \quad \beta C_{jct} + \mathbf{S}'_{jct}\gamma + \mathbf{X}'_{ihjct}\theta + \lambda_{h\mathrm{J}} + \mu_{ct} + \epsilon_{ihjct}, \tag{2}$$

$\mathbf{S}_{jct}$ is a vector that measures the number of months that have resulted in locally relevant (negative) economic shocks. The vector $\mathbf{X}_{ihjct}$ captures a full set of gender-age fixed effects, whereas μ_{ct} denotes country-cohort and $\lambda_{h/j}$ household or location fixed effects. In all specifications, the coefficient β measures the loss in educational attainment due to one additional conflict year of a given severity that the individuals may have experienced during a given childhood period.

Since conflicts do not occur randomly across space and over time, but rather are driven by political and economic causes, these driving factors could themselves be related to educational outcomes. For instance, a weak local labor market reduces the potential outside income of the local population, thereby reducing the opportunity costs of fighting, yet it may also reduce the households' ability and willingness to invest in education and the quality of local public service delivery. If we do not control for the underlying causes of conflict (weak institutions, ethnic tensions, economic shocks, etc.), we may overestimate the disruptive effects of conflict on education.

We address factors driving general conflict dynamics by controlling for an extensive set of fixed effects and further time-variant local controls. The location fixed effects, λ_j, control for all time-invariant differences in local social and economic conditions and the local capacity to deliver education (for 19,652 locations). As several of these factors may also be linked to the likelihood of conflict (like ethnic composition, local institutions, geography, access to infrastructure), factoring out these effects should move us closer to measuring a causal effect. Alternately, household fixed effects, λ_h, are used instead of location fixed effects, restricting the variation even further (for a total of 232,890 households). These control for time-invariant household characteristics and identify the educational losses incurred through conflict by directly comparing youth of distinct ages residing within the same household. Additionally, the country-cohort fixed effects,

μ_{ct}, control for yearly changes in the macroeconomic and political environment of each country, including, among others, the country-wide determinants and consequences of violent conflicts and the overall trends in education provision.

Our preferred specifications rely on a combination of these two types of non-nested fixed effects. They identify the effects of conflict based on within-household variation, comparing the educational attainment of different cohorts, living within the same household, who were deferentially exposed to conflict, while at the same time factoring out all common time-varying dynamics that would affect the same cohort across all locations within a country. A remaining source of bias in our estimates comes from time-variant location-specific economic shocks, like major weather shocks, which could potentially affect different parts of a country to varying degrees and which may affect both conflict and educational outcomes. Therefore, in our preferred specifications, we also control for a series of weather shocks, denoted by the vector $\mathbf{S}_{jct}$.

We rely on robust standard errors clustered at the first sub-national administrative level. Alternative specifications in section 5 test the robustness of our results to other cluster specifications.

3.4 Issues of interpretation

We measure conflict exposure by linking past conflict incidence near a certain locality to the educational attainment of children and youth currently living in that locality. This measure does not capture direct individual exposure to conflict, but rather a location's exposure to conflict. Since particularly high-intensity conflicts are likely to induce massive (although potentially transitory) migration (e.g., Czaika and Kis-Katos, 2009), conflict-exposed youth may not still reside in the same place they lived during the specified age periods. Many may have moved to different locations, at least temporarily, and some of them may not have returned. If migrant youth coming from conflict-affected locations still lag behind in education in their new location, this will lead us to underestimate the effects of conflict on average. Contrary, if children with the best chances to complete their education (e.g., because of showing higher ability or coming from the wealthiest families) are more likely to leave in the face of conflict and/or are less likely to return after the conflict has subsided, our conflict coefficients may be overestimated.[9] Since DHS surveys do not collect information on migration systematically, we are left

[9] Children from better socio-economic background suffer more in terms of their education due to conflict (Akresh and De Walque, 2008). This makes it less likely that self-selection into migration drives the negative effects. Nonetheless, the evidence is inconclusive at best.

with a sub-sample of DHS surveys that distinguish between migrant and non-migrant households. In section 5, the role of migration in the conflict-education link is analyzed in these two sub-samples.

The potential presence of migration-induced measurement errors cautions us to not interpret our estimates as precise measures of the costs of conflict on education. These limitations notwithstanding, our results have an unambiguous interpretation on the local level. Taking a regional economic approach, our results show how the distribution of human capital in a locality changes with its past conflict history. Local development is most likely to suffer, even in the long run, due to a resulting gap in human capital.

4 Results

4.1 Exposure to conflicts of different severity

To establish a baseline result, we follow the bulk of the literature by relating individual educational attainment to past exposure to localized conflict events in general. Table 2 depicts results from regressing completed years of education on conflict exposure during childhood with a series of different specifications. Column 1 shows the baseline correlation between the years of education and the potential conflict exposure during childhood, including gender-age, location and birth cohort fixed effects as well as weather shock controls. The estimate on conflict shows a negative relationship between conflict exposure and the years of schooling. Exposure to one additional conflict year is linked to about 0.057 fewer years of schooling, or around 1.5% of a standard deviation. This coefficient is statistically significant, but of a rather modest magnitude, and does not suggest substantial losses in education due to conflicts, on average. In column 2 of table 2, we substitute the location fixed effects with household fixed effects to control for confounding household characteristics. In doing so, we identify the effects of conflict on education through the distinct potential conflict experiences of youth living within the same household. The results stay the same. Education outcomes may also vary by country and time, since government policies directly shape the school system. Public policies together with national labor market prospects, can be expected to drive individual education-investments decisions while at the same time being correlated with the likelihood of conflict. In order to mitigate this bias, in column 3 we exchange the birth cohort fixed effects for country-specific birth cohort fixed effects. With this approach, we increase the validity of our estimates but lose the ability to identify the average effect of a conflict in a country, as we only focus on within-country differences in conflict exposure driven by the proximity to localized conflicts. In our preferred and most restrictive model in column 4, we combine the country-specific birth cohort fixed effects with the household fixed effects. With the inclusion of country-specific time fixed effects, the conflict coefficient becomes insignificant. This indicates that the broader economic and political shocks that occur at the country level result in a more conflict-prone local environment, and also result in relatively poorer educational outcomes, without us being able to establish a separate link between the variation in local conflict exposure and education.

Our general conflict measure combined a wide range of different types of violence including low-level conflict of short duration (like a violent demonstration) as well as more protracted fighting. The latter can be expected to have a more disruptive effect on education than the former. In order to investigate the distinct effects of conflict on education by conflict intensity, we first classify conflict years into moderate- and high-intensity conflict years based on the threshold of 1000 casualties. The results in table 3 confirm our expectations. In the first two columns, coefficients on moderate- and high-intensity conflict are both negative and significant, but the point estimates on high-intensity conflicts are about three times larger than those of moderate-intensity conflicts. Once we focus on within-country variation (columns 3 and 4), the significance of moderate-intensity conflicts vanishes, but high-intensity conflicts still stay significantly negative. Our most restrictive model in column 4 shows that while conflict of relatively lower intensity has no effect on the within-country variation in education, a further high-intensity conflict year reduces the number of school years by 0.116 years or 1.4 months.

Figure 3 paints a more nuanced picture of the role of conflict severity for education by gradually adjusting our conflict definition to include only conflicts that pass a minimum threshold of yearly casualties (ranging from 1 to 5,000).[10] The left panel is based on the specification shown in column 3, whereas the right panel provides the estimates of our preferred regression model (column 4). The estimates show no negative relationship between moderate-intensity conflict and education. However, high-intensity conflict years reduce the number of years of school attainment. There is a strong gradient, as conflicts of larger severity disrupt education more strongly than conflicts of lower intensity. While exposure to one additional conflict year with at least 500 battle-related deaths reduces education by about 0.05 years, or around 1.6% of a standard deviation, the effect of a conflict year with at least 5,000 deaths amounts to 0.29 years of education lost (9.7%

[10] As only 2.39% of all children in the dataset were subject to a local conflict with more than 5,000 deaths per year, we take this as our upper conflict severity threshold.

of a standard deviation). The costs of conflict are disproportionately large for those children living in areas with the longest amount of past exposure to conflict. Children in the upper quartile of exposure to at least medium-intensity conflict (500 casualties) have experienced, on average, 2.4 years of such conflicts, resulting in an average education loss of $0.05 \times 2.4 = 0.12$ years, or 1.4 months. Among children in the upper 5% of high-intensity conflict exposure (5000 deaths), the loss amounts to $0.29 \times 2 = 0.58$ years, or around 7 months. These losses appear even more substantial in light of the rather low average educational attainment within our sample (3.8 years). As the difference between the two specifications is marginal, we will focus on the household fixed effects specification in subsequent estimations.

To consider an additional dimension of heterogeneity, we further differentiate the effects of conflict by age at exposure, dividing our conflict measure into those occurring within four distinct age periods in an individual's life: in utero, early childhood, preschool age, and primary school age. Figure 4 presents the estimation results. Table A3 in the appendix shows related results for age-group-wise regressions while distinguishing between moderate- and high-intensity conflicts. The results show that severe conflict experiences in all age periods are harmful for educational attainment, whereas exposure to moderate-intensity conflicts does not have statistically significant effects on the number of completed school years. Across the age periods, the effect sizes differ only marginally. Thus, the results reflect educational losses from direct effects of conflict, like school closure or student and teacher absence, and also point to the presence of indirect long-term consequences in line with the early childhood and fetal origin theory (Currie and Almond, 2011; Cunha and Heckman, 2007).

Boys' and girls' education is often found to respond differently to conflict (Shemyakina, 2011). To test for heterogeneity by gender, figure 5 shows a common baseline effect that turns more negative with conflict severity, and illustrates a differential effect for females. In our broader sample, high-intensity conflicts reduce the educational attainment of boys more strongly than that of girls. However, there is no marked difference with respect to the effects of moderate-intensity conflict years. Table A4 in the appendix shows regression results differentiating between moderate- and high-intensity conflict (at the 1000 deaths threshold) by gender. The results show a weakly positive relationship between moderate-intensity conflicts and boys' education, whereas the effect is fully nullified for girls. One additional year of high-intensity conflict leads to 0.19 years less education for boys, but only to a 0.04 years loss for girls.

4.2 Alternative sources of causal identification

Our models reduce the potential for omitted variable bias to a considerable extent, but time-varying local shocks could still drive conflict and education alike. In order to mitigate the possibility of unobservables driving the negative correlation between conflict and education, we extend our fixed effects specifications even further and substitute country-cohort fixed effects with region-cohort fixed effects. These regional birth-cohort fixed effects absorb a wide range of time varying regional characteristics like shocks to the regional labor market or changes in the regional provision of education. They factor out all common variations in both the propensity to experience a conflict and in educational outcomes across administrative regions within the same year. The remaining identifying variation comes from a comparison of locations within the same administrative region of tier one (GADM, 2011) that are within or outside of the direct range of a given conflict. One downside of these specifications is that, they will not be able to capture sufficient across-village variation in outcomes if the administrative units are small or the conflicts relatively far-reaching. Figure A1 in the appendix presents a full set of results. They show patterns similar to figure 3 but with slightly smaller education losses than our main results.

As a second consistency check, we implement an instrumental variable approach to validate our fixed effects results and deal with the potential endogeneity stemming from local time-varying factors. The instrument combines spatial variation in ethnic heterogeneity with the presence of location-specific weather shocks by interacting the distance to the nearest ethnic border with the number of extremely dry and wet months experienced in any location, combining two well-known streams of the literature on the causes of conflict. In economies based on rain-fed agriculture, extreme weather events proxy for economic shocks, determining the likelihood of conflict (Miguel et al., 2004; Harari and Ferrara, 2018). Both extremely dry as well as extremely rainy periods may reduce agricultural income and lower the opportunity costs of fighting. Lower incomes additionally result in lower tax revenues, weakening the state's capacity to fight against insurgencies (Harari and Ferrara, 2018). Unlike in the case of droughts, rainfall-shock months may have further direct effects on conflicts, since floods can act as a barrier to conflict by immobilizing people. Ethnic heterogeneity increases the likelihood that groups compete over power and public goods (Esteban et al., 2012). Often, one group dominates and discriminates against the others, resulting in grievances (Caselli and Coleman, 2013). Ideological differences and incompatible preferences between groups can add to the conflict potential. Moreover, group identification facilitates the mobilization of certain actors, making conflicts more likely.

Both weather shocks and ethnic diversity may directly affect the provision of local public goods, including education. Our regressions control for these direct effects by including localized weather shocks as predictors of education and by factoring out time-invariant spatial heterogeneities, including ethnic diversity, via location or household fixed effects. However, the interaction of weather shocks and ethnic fractionalization should not affect education directly, but rather through a differential effect on conflict potential only, providing us with a viable instrument. We base this instrument on Couttenier and Soubeyran (2013), who find that countries with a higher ethnic fractionalization are more prone to conflict when affected by a drought than less ethnically fractionalized countries, experiencing a similar drought. The instrument identifies the effects of conflict through the heterogeneous effect of a local economic shock, dependent on the risk of conflict due to ethnic fractionalization. The exclusion restriction behind this instrument requires that education be more negatively affected by weather shocks in ethnically heterogeneous regions due solely to the increased conflict potential arising from ethnic heterogeneity and not because of other factors. One potential confounding factor could arise if the distance to ethnic borders measures not only ethnic heterogeneity but also remoteness from district administrative offices, which could affect the government's ability to respond to localized shocks. To test for this alternative channel, we present results controlling for the distance between a location and the nearest administrative center, interacted with weather shocks.

The first and second stage regression results are reported in table 4. We use our baseline conflict measure (total number of conflict years of any severity) as weather shocks may result in conflicts of mild to moderate to severe intensity alike (Hendrix and Salehyan, 2012). Weather shocks can serve to trigger conflicts and are thus a good predictor of conflict occurrence. However, the role of weather shocks in influencing the quantity of conflict fatalities is weak. Hence, in this step we use instruments only to test the link between past conflict events and education, without distinguishing conflicts by their severity.

Column 1 presents results for regressions using gender-age, household, and birth-cohort fixed effects, whereas in column 2 birth-cohort fixed effects are replaced by country-specific birth-cohort fixed effects. Standard errors are clustered at the location level. In column 1 of the first stage, drought events increase the likelihood of conflict, on average, and the effect diminishes with the distance to an ethnic border, highlighting the heterogeneous effect of droughts as expected. By contrast, rainfall-shocks seem to act as a barrier to conflict, reducing the average conflict potential. This reduction is stronger in locations farther away from ethnic borders. The inclusion of country-specific birth-cohort fixed effects in column 2 absorbs the general effect of the extreme weather shocks, indicating that they have a national rather than a local scope. The interaction effect with rainfall-shock months still survives, showing that rainy periods reduce conflict potential more in ethnically homogeneous areas. Our results are closely related to O'Loughlin et al. (2012), which shows that climate conditions have effects at the local and national level. In columns 3 and 4, we add interaction terms of our weather variables with the distance to the regional capital to ensure that our instrument does not merely capture the effects of remoteness. In remote areas, the infrastructure is often less developed and aid programs may take longer to reach the affected population, increasing the impact of the weather effects. At the first stage, rainfall-shock months tend to increase the likelihood of conflict more in remote areas, underlining the immobility argument.

At the second stage, one further conflict year during childhood reduces educational attainment by 0.374 years (column 1) which is substantially larger than the OLS estimate (-0.059). The difference could arise from a reduction in measurement errors or omitted variables. When interpreting the IV estimates as local average treatment effects, educational losses are especially large for conflicts arising from agricultural shocks triggered by weather anomalies combined with ethnic fractionalization. The estimated effects are highly significant when only cohort-fixed effects are included. They stay of similar magnitude but lose their significance when we include country-specific birth cohort fixed effects. The inclusion of the interactions between weather events and the distance to the regional capital (column 3) does not change our estimates of conflict effects, making us more confident that the instruments measure the effects of conflict potential and not merely remoteness.

The IV estimates are of larger magnitude and still negative, supporting our OLS results. Once we control for country-specific variation over time, the relationship between our generic measure of conflict exposure and educational attainment is insignificant, showing that location-specific conflict occurrence does not affect educational attainment, on average. As weather shocks combined with ethnic fragmentation only provide a viable instrument for conflict occurrence and do not predict conflict intensity, we cannot use the same procedure to analyze the effects of high-intensity conflicts. However, we believe that our OLS results on higher intensity conflict are also more likely to underestimate the educational costs of conflict.

4.3 State capacity and further mechanisms

How much a conflict impairs the educational attainment of local children also depends on the actors involved in the conflict. As providers of education, governments have an incentive to maintain a functional education system, whereas non-state actors may destroy schools. Hence, we would expect that non-state conflicts have a stronger negative effect on education than state-based conflicts. Following the UCDP classification, we divide conflicts into state-based, non-state based, and one-sided violence. We regress the years of education on these three distinct types of conflict, including our standard controls. We focus on a summary measure of conflict intensity by using the inverse hyperbolic sine of the number of casualties of the respective conflict type. This helps us to reduce the number of presented interactions by incorporating the heterogeneous effects by conflict severity in a concise manner. Using our preferred specification, table 5 shows that on average the log-transformed number of casualties is not significantly related to the years of education. But, this measure still allows heterogeneous effects by the conflict actors. Most importantly, the loss of education seems to be caused by non-state conflicts, confirming our expectations. We find no significant effect of conflict severity on education for state-based conflicts and one-sided violence. A potential explanation for this latter null result is that attacks on civilians are, on average, of a lower intensity and usually accompany other conflicts involving state- or non-state actors.[11]

In a next step, we analyze a series of factors which potentially moderate the effects of conflict on education, distinguishing between a set of country and location characteristics. Results are presented in table 6. All characteristics are interacted with past conflict intensity (the inverse hyperbolic sine of the number of casualties) one-by-one first, whereas column 6 specifies the model to include all factors together.

We expect that educational losses from conflict vary with the form of government in power at the time of conflict. Classifying countries into strong autocracies, strong democracies, and others, column 1 of table 6 shows that the number of fatalities in a conflict decreases local school attainment both in weak states and strong democracies. As only few strong democracies exist in our sample, and these countries have not participated in large-scale conflicts, it is likely that these estimates are under-powered.[12] There is no educational loss in strong autocracies due to conflict confirming our hypothesis.

The provision of public goods may be more negatively impacted by conflict in ethnically

[11] The correlation of one-sided violence and state-based conflict is especially high with $\rho = 0.78$.

[12] We classified Benin, Lesotho, Madagascar, Malawi, Mali and Namibia as strong democracies. These countries did not experience any conflict of high intensity.

heterogeneous states than homogeneous ones, as cooperation costs generally increase with heterogeneity. Similarly, states with a poorer population (measured by lower average income) may face larger financial restrictions and be more susceptible to crisis due to their budget constraints. We test these channels in columns 2 and 3 of table 6. Both results turn out to be insignificant. Hence, conflict intensity is not significantly differentially linked to educational losses in more ethnically fractionalized or poorer states, on average.

At the local level, natural resources are often targeted by rebel groups because of their easy extraction and their high monetary value. If rebel groups succeed in capturing natural resources, government revenues drop sharply, reducing public goods provision. Consequently, the educational attainment of children from resource-rich regions is likely to suffer more due to conflict. Results in column 4 of table 6 support the resource curse argument. Educational losses significantly increase with conflict intensity in locations with natural resource deposits.

Local economic development may also be a relevant factor in education as the local demand for education and the local ability to provide public education can be expected to rise with local economic development. Similarly, wealthier regions with higher initial levels of educational attainment may face larger potential decreases if education is disrupted by conflict. Hence, the direction of the effect is a priori unclear. Our estimate in column 5 of table 6 shows a negative effect of conflict intensity in regions with higher economic prosperity. This indicates that, ceteris paribus, better-developed regions lose more education due to conflict.

Column 6 of table 6 includes all interactions, jointly testing heterogeneities at the country level and at the local level against each other. In this specification, our proxy for the state capacity channel turns out to be the most relevant differentiating factor. Education suffers substantially less from higher intensity conflicts in strong autocracies than under any other conditions. Regions with higher ethnic diversity are more strongly affected by conflict in terms of lost education, indicating that public service provision during times of conflict may be more limited in ethnically-heterogeneous countries. All else equal, geographic localities closer to natural resource deposits suffer substantially larger educational losses than geographic localities farther away. This indicates that a local resource curse not only serves to trigger conflicts, but also interacts with conflict intensity in affecting educational losses. Finally, the coefficient for local nighttime light intensity becomes insignificant in this joint test.

5 Further robustness issues

Our measurement strategy relies on the assumption that we can assess the past conflict exposure of an individual by measuring past conflict occurrences near the individual's current geographic location. Thereby, we neglect the potential measurement errors from migration described in section 3.4. Although we cannot analyze the role of migration in the whole sample, we observe migration patterns within a sub-sample of the population.[13] In this sub-sample, the share of migrants in the total youth population is positively correlated with years of past conflict exposure (over the last 25 years) for high intensity conflicts (see table A5 in the appendix), but are uncorrelated with conflict in general, or moderate-intensity conflicts. Hence, our estimates for high-intensity conflicts may be more prone to a bias due to incorrectly assigned birthplaces. With respect to self-selection into migration, migrants are, on average, better educated and tend to live in wealthier and better educated households than non-migrants (see A6 in the appendix), suggesting that there may be a downward bias in the high-intensity conflict estimates.

In the migration sub-sample, we can directly compare the conflict coefficients by migration status. Table A7 replicates our baseline results for the sub-sample of youth with migration information. Even though the sample is reduced substantially (to 68,110 observations), we can still see the same link between exposure to conflicts of high intensity and losses in education in column 1. In the model with household fixed effects, we run into power issues, as the sample is further reduced by half (column 2). The substantially smaller sample size renders the resulting coefficient estimates insignificant. However, the magnitude and direction of the effect are both comparable to the first specification. In columns 3 and 4, we estimate the general effect of past conflict exposure together with a differential effect for non-migrants. Migrants indeed show worse education outcomes than non-migrants in locations exposed to moderate-intensity conflicts, so migration may contribute to our estimate of the local costs of moderate-intensity conflict. However, in locations which have experienced a high-intensity conflict, non-migrants have decidedly fewer years of education on average, indicating that migration is not the only factor driving our results. Overall, migrants seem to be better off than locals. This could be because families who care more about education are more likely to relocate before the outbreak of a conflict, thereby reducing the human capital of the local population. Alternately, post-conflict locations might attract relatively better-educated households. In either case, migration tends to lead to an underestimation of the individual costs of high-intensity conflict.

[13] This sub-sample includes data from 20 countries.

Since the timing and location of conflicts are not exogenous, further placebo checks can help us to assess the potential role of pre-trends or other confounding factors driving local conflict and education. For this, we repeat our baseline regressions linking conflict exposure to education, but focus on conflicts that should not have directly affected children and youth in our sample, due to the nature of their timing. As a first test, we regress individual years of education on conflict occurring in the third and second year before the birth of a child. The results are shown in table A8. There is no residual correlation between late-life educational attainment and pre-utero exposure to conflict even among high-intensity conflicts. It follows that, the correlation between educational attainment and conflict exposure is unlikely to be driven by pre-trends. As a second placebo test, we examine educational attainment among those who were exposed to localized violence as adults, during a time when they were likely to have already finished their secondary education. For this, we focus on a sample of adults aged 26 to 47 (born between 1969 and 1986), and test for correlation between their potential for conflict exposure between the ages of 20 and 25 and their completed years of education. The results in column 3 and 4 of table A8 show no significant negative coefficients, and coefficients for moderate-intensity conflicts in fact depict a positive correlation, indicating that our previous findings are unlikely to be driven by common underlying trends.

Throughout all of our analyses, we reported estimates with standard errors clustered at the regional level, allowing for unspecified correlation between the residuals of individuals living in the same region. However, in our context, the correct level of clustering is debatable. Since we are measuring conflict exposure by location, measurement errors should similarly affect all individuals living within a certain location. Hence, standard errors should at least be clustered at the locality level. The presence of correlation at a larger level of analysis, for instance at the regional or even national level, is plausible and partially captured via our fixed effects. To check for the robustness of our results to the level of clustering, we compare our main specifications using three additional steps. Columns 1 and 2 in table A9 report regression results with standard errors clustered at the locality level, allowing only for correlation within specific locations. Columns 3 and 4 report results with two-way clustering at the locality level and among country-birth cohort cells. This latter method relies on asymptotics in the lowest level clusters, and is the most appropriate method to use when there is within-correlation on both dimensions and each dimension has many clusters (Cameron and Miller, 2015). Columns 5 and 6 report results using spatial standard errors, which correct for spatial dependence in the error terms. Since we measure conflict exposure within a 50 km radius around the location, neighboring locations will likely be affected by the same conflicts. In order to take this into account, we run regressions with spatially corrected standard errors based on Conley (1999), correcting for spatial correlation within 100 km to the survey location. The distinct versions of clustering do not change our main results substantially.

In our analysis, we assigned each conflict event to all survey locations located within a 50 km radius to the conflict. However, the average influence area of a conflict could be both farther or nearer. Therefore, we re-estimate the effects of conflicts using different severity thresholds for conflict influence zones, namely 25, 100 and 200 km. The results are shown in figure A2 in the appendix. Consistent with the literature (see e.g., Hallberg, 2012), the 25 km distance measure does not seem to sufficiently capture the full area of a conflict, and results yield substantially larger standard errors. However, effect sizes diminish when using wider distance measures of up to 200 km. As expected, the impact on educational attainment declines as the distance to the original conflict event increases.

6 Conclusion

We study the link between localized conflict occurrence and educational attainment of children in 31 Sub-Saharan African countries from 1989 to 2015. In doing so, we are able to generalize the results of a large number of case studies on this link, and investigate heterogeneous effects of different conflict types as well as context-specific conflict characteristics. For this purpose, we combine DHS surveys with the UCDP geo-referenced conflict dataset and link individual school attainment of youth to local occurrence of conflicts during four specific age periods during childhood. We address the endogeneity of conflict across time and space by including two-way fixed effects for households and country-specific birth cohorts, capturing time-variant shocks to conflict and education at the country level and time-invariant differences in the propensity of conflict across households. Additionally, we control for location-specific weather shocks and implement an instrumental variable approach to address further time-varying confounders.

Although we cannot identify robust effects from our generic conflict exposure, which measure includes every type of conflict event, the most severe and prolonged conflicts do result in substantial average costs of educational attainment. Educational losses occur mainly in states with weaker governance, which are more likely to experience declines in state capacity due to a conflict. Losses are also mainly triggered by non-state conflicts, which are more likely to destroy school infrastructures and thus harm the administrative capacity of the government. This highlights the crucial role of state capacity in mediating the effects of conflict on education. Moreover, conflict exposure during all age periods harms education, and the educational outcomes of boys are more strongly affected by high-intensity conflicts than those of girls.

The results document longer lasting losses of education among youth cohorts currently living in locations previously affected by a high-intensity conflict. Although we cannot distinguish direct disruptions to education from human capital losses through the channel of out-migration of those more likely to receive an education, we document shifts in the human capital composition of localities previously affected by severe conflict, leading to economic and social costs in the long run. Contextualizing the findings of previous case studies, the paper highlights the diverse effects of conflict on education. In order to achieve universal education for all, remedial policy interventions should target previously conflict-affected regions, especially in areas where state capacity is limited.

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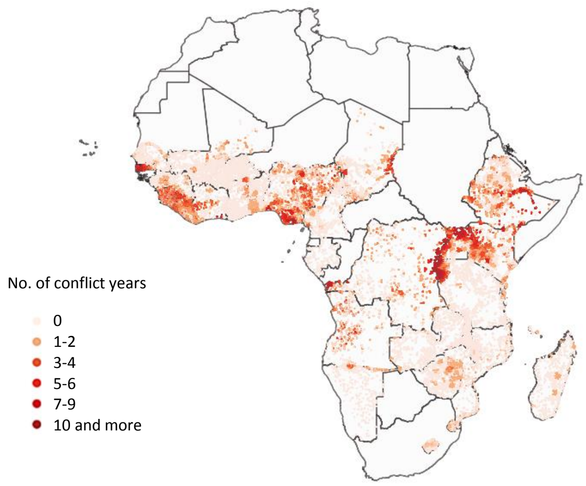
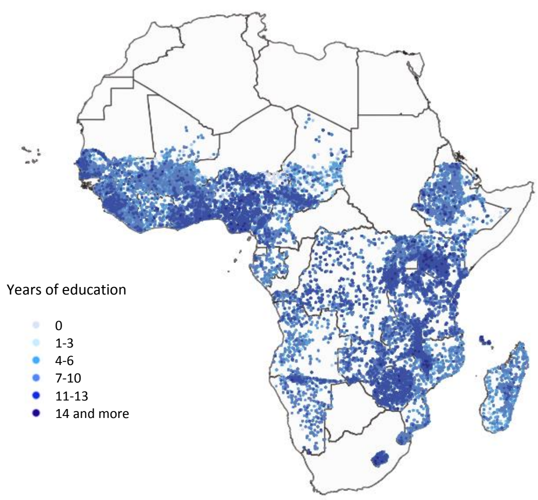
Figures

Figure 1: Average years of conflict exposure during childhood per survey location



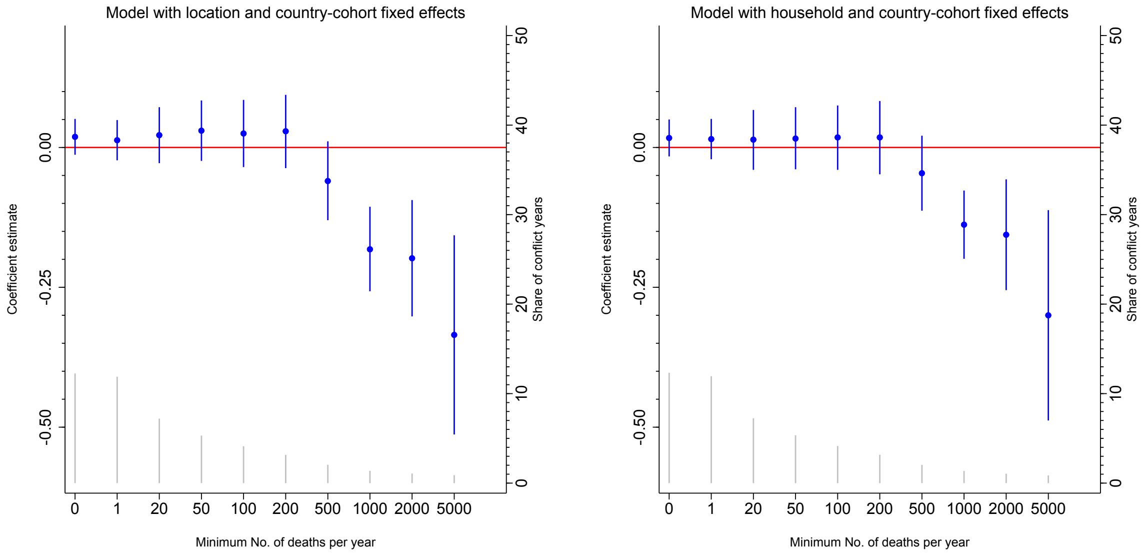
Note: Sources: DHS, UCDP, Map Library.

Figure 2: Average years of schooling per survey location



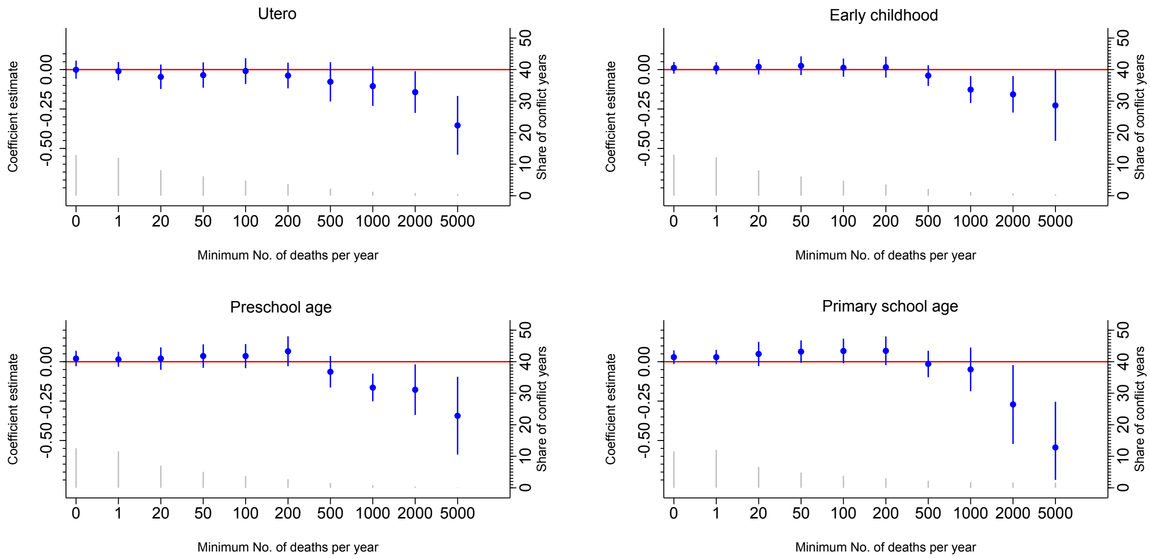
Note: Sources: DHS, Map Library.

Figure 3: The effects of past conflict exposure on education by conflict severity



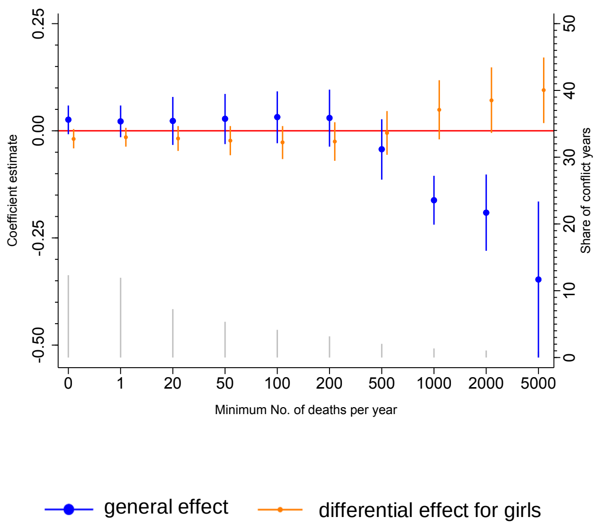
Note: The figure reports point estimates and 95% confidence intervals of education on conflict exposure during childhood within 50 km to the location. Regression specifications column 5 and 6 in table 2. The threshold that defines a severe conflict-year varies according to a minimum number of battle-related deaths displayed horizontally. The share of observations with at least one conflict year using the given deaths threshold is depicted by grey bars (second vertical axis). Standard errors are clustered at the level of administrative regions.

Figure 4: The effects of conflict on education by severity and age at exposure



Note: The figure reports estimates of the effect of conflict on education by age periods, whereby age period-specific conflicts enter the regression jointly. Specification as the right panel of figure 3.

Figure 5: Differential effects of conflict exposure during childhood by gender



Note: The figure reports point estimates and 95% confidence intervals of education on conflict exposure during childhood and the differential effects for girls. Specification as in the right panel of figure 3.

Tables

Table 1: Summary statistics

Variable	Mean	St. dev.	Min.	Max.
Dependent and main variables				
Years of education	3.81	3.11	0	18
Conflict years	1.64	3.04	0	14
Moderate-intensity conflict years	1.56	2.82	0	14
High-intensity conflict years	0.09	0.43	0	5
Conflict years in utero	0.12	0.33	0	1
Conflict years at age 0–3	0.51	1.08	0	4
Conflict years at age 4–6	0.37	0.82	0	3
Conflict years at age 7–12	0.64	1.37	0	6
Distance to border (in 100 km)	0.25	0.31	0	3
Drought months	11.17	9.87	0	119
Wet months	7.44	8.82	0	71
Age	14.39	3.67	10	26
Female	0.48	0.50	0	1
Heterogeneity analysis				
Asinh(Conflict deaths)	1.95	2.99	0	12.62
Asinh(State conflict deaths)	1.04	2.31	0	10.80
Asinh(Non-state conflict deaths)	0.72	1.75	0	9.06
Asinh(One-sided conflict deaths)	1.34	2.64	0	12.62
Strong democracy	0.24	0.43	0	1
Strong autocracy	0.31	0.46	0	1
Higher ethnic frac.	0.60	0.49	0	1
Higher income	0.47	0.50	0	1
Natural resources	0.22	0.41	0	1
More nightlights	0.50	0.50	0	1
Higher schooling	0.50	0.50	0	1

Note: Descriptive statistics refer to the full sample; $N = 541,480$.

Table 2: Baseline regressions: Conflict exposure and education

Dependent	**Years of education**			
	(1)	(2)	(3)	(4)
Conflict years	-0.057**	-0.059***	0.021	0.018
	(0.024)	(0.021)	(0.017)	(0.017)
Drought months	-0.000	-0.002	-0.006*	-0.006*
	(0.004)	(0.004)	(0.003)	(0.003)
Wet months	-0.010	-0.009	-0.006*	-0.003
	(0.008)	(0.007)	(0.003)	(0.003)
Observations	541,480	480,847	541,480	480,847
R-squared	0.528	0.757	0.550	0.772
Gender-age FE	Yes	Yes	Yes	Yes
Location FE	Yes		Yes	
Birth cohort FE	Yes	Yes		
Household FE		Yes		Yes
Country-cohort FE			Yes	Yes

Note: The table reports OLS estimates of education on the number of past conflict Standard errors are clustered at the level of administrative regions, ***,**,* denote significance at 1, 5 and 10%.

Table 3: Baseline regressions: Intensity of conflict and education

Dependent	**Years of education**			
	(1)	(2)	(3)	(4)
Moderate-intensity conflict years	-0.050** (0.024)	-0.054** (0.021)	0.019 (0.017)	0.017 (0.017)
High-intensity conflict years	-0.162** (0.073)	-0.135* (0.069)	-0.161*** (0.052)	-0.116*** (0.043)
Observations	541,480	480,847	541,480	480,847
R-squared	0.528	0.757	0.550	0.772
Gender-age FE	Yes	Yes	Yes	Yes
Weather controls	Yes	Yes	Yes	Yes
Location FE	Yes	Yes	Yes	Yes
Birth cohort FE	Yes	Yes	Yes	Yes
Household FE		Yes		Yes
Country-cohort FE			Yes	Yes

Note: The table reports OLS estimates of education on the amount of moderate- and high-intensity conflict years at the threshold of 1000 casualties. Standard errors are clustered at the level of administrative regions, ***,**,* denote significance at 1, 5 and 10%.

Table 4: Instrumental variable approach: conflict exposure and education

	First stage			
Dependent	**Conflict years**			
	(1)	(2)	(3)	(4)
Instruments:				
Distance to border ×	-0.024***	0.002	-0.021***	0.002
Drought months	(0.003)	(0.002)	(0.003)	(0.002)
Distance to border ×	-0.021***	-0.013***	-0.020***	-0.012***
Wet months	(0.004)	(0.003)	(0.003)	(0.002)
Controls:				
Drought months	0.015***	0.000	0.020***	0.000
	(0.002)	(0.001)	(0.002)	(0.002)
Wet months	-0.012***	0.001	-0.025***	-0.004
	(0.002)	(0.002)	(0.003)	(0.002)
Distance to admin. center ×			-0.006***	0.000
Drought months			(0.001)	(0.001)
Distance to admin. center ×			0.015***	0.006**
Wet months			(0.002)	(0.002)
R-Squared	0.954	0.977	0.954	0.977
Kleinbergen-Paap F-stat.	52.64	13.67	45.17	12.37
	Second stage			
Dependent	**Years of education**			
Conflict years	-0.374***	-0.456	-0.397***	-0.401
	(0.127)	(0.327)	(0.143)	(0.347)
Drought months	0.000	-0.005***	0.003	-0.001
	(0.002)	(0.002)	(0.003)	(0.002)
Wet months	-0.015***	-0.005**	-0.017***	-0.004
	(0.003)	(0.002)	(0.005)	(0.003)
Distance to admin. center ×			-0.003*	-0.004***
Drought months			(0.002)	(0.001)
Distance to admin. center ×			0.001	-0.001
Wet months			(0.003)	(0.003)
Observations	480,847	480,847	480,847	480,847
R-squared	0.753	0.767	0.752	0.769
Household FE	Yes	Yes	Yes	Yes
Gender-age FE	Yes	Yes	Yes	Yes
Birth cohort FE	Yes	Yes	Yes	Yes
Country-cohort FE		Yes		Yes

Note: The table reports the first and second stages from the IV regressions of education on the number of conflict years. Standard errors are clustered at the survey location. ***,**,* denote significance at 1, 5 and 10%.

Table 5: Heterogeneous effects of conflict intensity on education by conflict type

	Years of education				
	(1)	(2)	(3)	(4)	(5)
Asinh(Conflict deaths)	-0.006				
	(0.011)				
Asinh(State conflict deaths)		0.016			0.017
		(0.015)			(0.016)
Asinh(Non-state conflict deaths)			-0.048**		-0.049**
			(0.021)		(0.021)
Asinh(One-sided conflict deaths)				0.003	-0.001
				(0.014)	(0.015)
Observations	480,847	480,847	480,847	480,847	480,847
R-squared	0.772	0.772	0.772	0.772	0.772

Note: The table reports OLS estimates of education on the the inverse hyperbolic sine function of the number of casualties during childhood of distinct conflict types. Specifications include gender-age, country-cohort and household fixed effects and weather controls. Standard errors are clustered at the level of administrative regions. ***,**,* denote significance at 1, 5 and 10%.

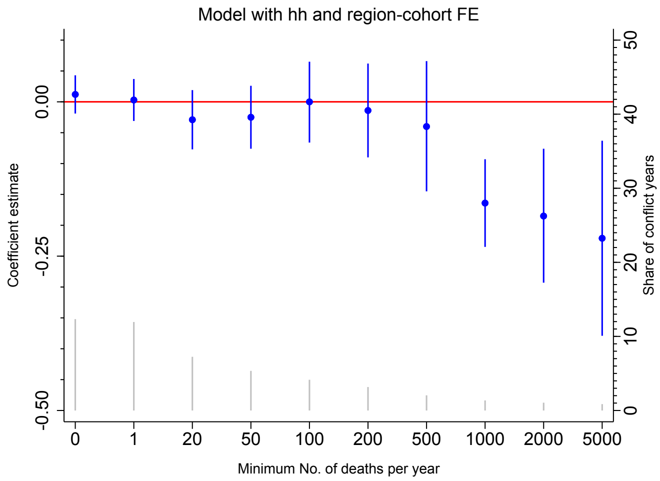
Table 6: Heterogeneous effects of conflict intensity on education by country and location characteristics

	Years of education					
	(1)	(2)	(3)	(4)	(5)	(6)
Asinh(Conflict deaths)	-0.067**	0.008	-0.008	-0.001	0.005	-0.036
	(0.034)	(0.013)	(0.011)	(0.012)	(0.013)	(0.033)
... × Strong democracy	-0.019					-0.030
	(0.020)					(0.020)
... × Strong autocracy	0.076**					0.078**
	(0.032)					(0.032)
... × Higher ethnic frac.		-0.022				-0.036*
		(0.020)				(0.019)
... × Higher income per capita			0.008			0.018
			(0.026)			(0.027)
... × Natural resources				-0.027*		-0.026*
				(0.014)		(0.014)
... × More nightlights					-0.020*	-0.015
					(0.012)	(0.011)
Observations	480,847	480,847	480,847	480,847	480,847	480,847
R-squared	0.772	0.772	0.772	0.772	0.772	0.772

Note: The table reports OLS estimates from education on the inverse hyperbolic sine function of the number of casualties during childhood and its interaction with country and location characteristics. Specifications as in table 5. ***,**,* denote significance at 1, 5 and 10%.

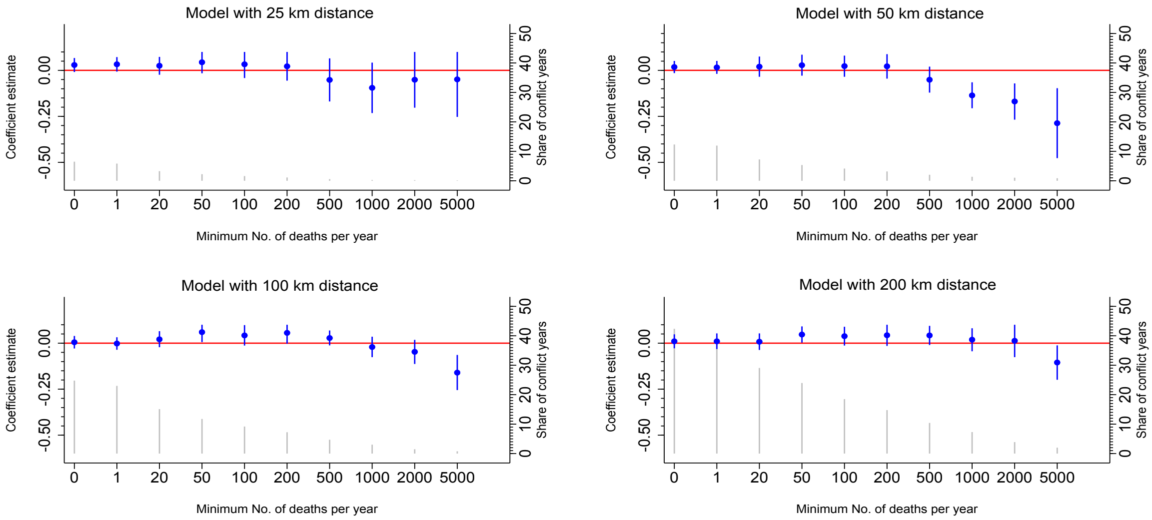
A Appendix

Figure A1: Robustness: The effects of past conflict exposure on education with region-cohort fixed effects



Note: The figure reports point estimates and 95% confidence intervals of education on conflict exposure (from in utero to age 12) within 50 km to the survey location. The regressions include weather controls, gender-age fixed effects and household combined with regional cohort fixed effects.. $N = 480,847$. The threshold that defines a severe conflict-year varies according to a minimum number of battle-related deaths displayed horizontally. The share of observations with at least one conflict year using the given deaths threshold is depicted by grey bars (second vertical axis). Standard errors are clustered at the level of administrative regions.

Figure A2: Robustness: The effects of past conflict exposure on education at 25, 50, 100 and 200 km distance



Note: The figure reports point estimates and 95% confidence intervals of the regression from education on conflict exposure (from in utero to age 12) within 25, 50, 100 and 200 km to the survey location. The regressions include weather controls, gender-age fixed effects as well as household and regional cohort fixed effects as stated in the graphs. $N = 480,847$. The threshold that defines a severe conflict-year varies according to a minimum number of battle-related deaths displayed horizontally. The share of observations with at least one conflict year using the given deaths threshold is depicted by grey bars (second vertical axis). Standard errors are clustered at the level of administrative regions.

Table A1: List of DHS surveys

Angola (2015/2016), Benin (2001, 2011/2012), Burundi (2010, 2012), Burkina Faso (2003, 2010), Cameroon (2004, 2011), Chad (2014/2015), Comoros (2012), DR Congo (2007, 2013/2014), Ethiopia (2005, 2011, 2016), Gabon (2012), Ghana (2003, 2008, 2014), Guinea (2005, 2012), Ivory Coast (2011/2012), Kenya (2003, 2008/2009, 2014), Lesotho (2009, 2014), Liberia (2007, 2013), Madagascar (2008/2009), Mali (2001, 2006, 2012/2013), Malawi (2000, 2004, 2010, 2015/2016), Mozambique (2009, 2011), Namibia (2000, 2006/2007, 2013), Nigeria (2003, 2008, 2013), Rwanda (2005, 2010, 2014/2015), Senegal (2005, 2010/2011, 2012/2013, 2015), Sierra Leone (2013), Swaziland (2006/2007), Tanzania (2010, 2015/2016), Togo (2013/2014), Uganda (2000/2001, 2006, 2011), Zambia (2007, 2013/2014), Zimbabwe (2005/2006, 2010/2011, 2015)

Table A2: Variable definitions

Dependent and main variables	
Years of education	Records the reported individual educational attainment in years.
Gender-age FE	Full set of interactions between age indicators and the reported gender.
Conflict years (in utero, at age of 0–3, 4–6, 7–12)	Measures conflict exposure of an individual in years, from the year before the birth year (in utero) until the age of 12 (or within the stated age brackets). A conflict year is defined as a year in which at least one conflict event has taken place within 50 km distance to the respective survey location.
Conflict years by severity (x deaths)	Measures conflict exposure of an individual in years, from the year before the birth year (in utero) until the age of 12 (or within the stated age brackets). A conflict year is defined as a year with at least x battle-related deaths occurring within 50 km distance to the respective survey location.
Moderate-intensity conflict years	Measures conflict exposure of an individual in years from the year before the birth year (in utero) until the age of 12 years. A moderate-intensity conflict year is a year with less than 1000 battle related deaths. Conflict events are counted within 50 km distance to the respective survey location.
High-intensity conflict years	Measures conflict exposure of an individual in years from the year before the birth year (in utero) until the age of 12 years. A high-intensity conflict year is a year with 1000 or more battle related deaths. Conflict events are counted within 50 km distance to the respective survey location.
Drought months (in utero, at age of 0–3, 4–6, 7–12)	Measures the exposure to drought events of an individual in months, from one year before the birth year until the age of 12 (or within the stated age brackets). A drought month is defined as a year when 12-months scale SPEI index is lower than -1.5.
Wet months (in utero, at age of 0–3, 4–6, 7–12)	Measures the exposure to extreme wet months of an individual from one year before the birth year until the age of 12 (or within the stated age brackets) in months. A wet month is defined as a year when the 12-months SPEI index was above 1.5.
Instrumental variable approach	
Distance to border	The variable gives the geographical distance to the nearest ethnic border in 100 km.
Distance to admin. center	The variable measures the geographical distance to the regional capital in 100 km (second administrative level).

Heterogeneity analysis	
Asinh(Conflict deaths)	Inverse hyperbolic sine function of the total number of battle-related deaths that occurred in 50 km distance to the survey location of an individual between one year before the birth and the age of 12.
Asinh(State conflict deaths)	Inverse hyperbolic sine function of the total number of battle-related deaths in conflicts where a state actor was involved. Conflict events are counted if occurred in 50 km distance to the survey location of an individual between one year before the birth and the age of 12. Conflict type is based on the UCDP classification.
Asinh(Non-state conflict deaths)	Inverse hyperbolic sine function of the total number of battle-related deaths in conflicts where no state actor was involved. Conflict events are counted if occurred in 50 km distance to the survey location of an individual between one year before the birth and the age of 12. Conflict type is based on the UCDP classification.
Asinh(One-sided conflict deaths)	Inverse hyperbolic sine function of the total number of battle-related deaths in conflicts with one-sided violence. Conflict events are counted if occurred in 50 km distance to the survey location of an individual between one year before the birth and the age of 12. Conflict type is based on the UCDP classification.
Higher ethnic frac.	Dummy variable that is one if the ethnic fractionalization index of the country is above the median.
Strong democracy	Dummy variable that is one if the polity2 score of the country is in more than 10 years of all years above 5.
Strong autocracy	Dummy variable that is one if the polity2 score of the country is in more than 10 years of all years below -5.
Higher income per capita	Dummy variable that is one if the average adjusted net national income per capita over 1989-2015 (current US$) is above the median
Natural resources	Dummy variable indicating that within 50 km of the survey location there is a natural resources deposit
More nightlights	Dummy variable that is one if the logarithm of the average nightlight intensity over time (1992-2013) of the grid is above the median.

Table A3: Intensity of conflict and education by age periods

	Years of education	
	(1)	(2)
Moderate-intensity conflict in utero	0.012	-0.002
	(0.027)	(0.028)
Moderate-intensity conflict years at age 0–3	0.011	0.010
	(0.020)	(0.019)
Moderate-intensity conflict years at age 4–6	0.010	0.015
	(0.024)	(0.026)
Moderate-intensity conflict years at age 7–12	0.028	0.026
	(0.024)	(0.022)
High-intensity conflict in utero	-0.171**	-0.127*
	(0.077)	(0.074)
High-intensity conflict years at age 0–3	-0.157***	-0.126***
	(0.054)	(0.047)
High-intensity conflict years at age 4–6	-0.226***	-0.145**
	(0.068)	(0.060)
High-intensity conflict years at age 7–12	-0.089	-0.022
	(0.117)	(0.083)
Observations	541,480	480,847
R-squared	0.550	0.772
Gender-age FE	Yes	Yes
Weather controls	Yes	Yes
Country-cohort FE	Yes	Yes
Location FE	Yes	Yes
Household FE		Yes

Note: The table reports estimation results from regressing education on the number of moderate- and high-intensity conflict years (between in utero and age 12) per age period. Conflict events are counted if occurred within 50 km to the survey. Moderate-intensity conflict years refer to years with less than 1000 deaths; high-intensity years to 1000 and more deaths. Standard errors are clustered at the level of administrative region. , ***,**,* denote significance at 1, 5 and 10%.

Table A4: Intensity of conflict and education by gender

	Years of education	
	(1)	(2)
Moderate-intensity conflict years	0.032*	0.033*
	(0.018)	(0.018)
Moderate-intensity conflict years × Female	-0.025**	-0.032***
	(0.010)	(0.010)
High-intensity conflict years	-0.222***	-0.185***
	(0.050)	(0.040)
High-intensity conflict years × Female	0.124**	0.143***
	(0.049)	(0.052)
Observations	541,480	480,847
R-squared	0.551	0.772
Gender-age FE	Yes	Yes
Weather controls	Yes	Yes
Country-cohort FE	Yes	Yes
Location FE	Yes	Yes
Household FE		Yes

Note: The table reports estimation results from regressing education on the number of moderate- and high-intensity conflict years (between in utero and age 12) as well as its interaction with a female dummy. Conflict events are counted if occurred within 50 km to the survey. Moderate-intensity conflict years refer to years with less than 1000 deaths; high-intensity years to 1000 and more deaths. Standard errors are clustered at the level of administrative region. , ***,**,* denote significance at 1, 5 and 10%.

Table A5: Robustness: Share of migrants and past conflict

	Share of migrants in location	
	(1)	(2)
Conflict years	0.004	
	(0.002)	
Moderate-intensity conflict years		0.003
		(0.002)
High-intensity conflict years		0.012***
		(0.003)
Observations	8,329	8,329
R-squared	0.087	0.088
Year FE	Yes	Yes

Note: The table reports estimation results from regressing the share of migrants among youth of age 10-26 in a survey location on the number of conflict years in the last 25 years, including further controls as stated in the table. Conflict events are counted if occurred within 50 km to the survey location. Moderate-intensity conflict years refer to years with less than 1000 deaths; high-intensity years to 1000 and more deaths. ***,**,* denote significance at 1, 5 and 10%.

Table A6: Robustness: Differences in socio-economic status by migration status

	Years of education	**Highest educ. in hh.**	**Poor hh.**	**Rich hh.**
	(1)	(2)	(3)	(4)
Non-migrant	-0.046*	-0.111***	0.031***	-0.051***
	(0.027)	(0.010)	(0.004)	(0.004)
Observations	68,110	68,110	68,110	68,110
R-squared	0.511	0.468	0.425	0.430
Gender-age FE	Yes	Yes	Yes	Yes
Location FE	Yes	Yes	Yes	Yes
Country-cohort FE	Yes	Yes	Yes	Yes

Note: The table reports estimation results from regressing socio-economic variables on the a dummy variable of non-migrants on a sub-sample with migration information. ***,**,* denote significance at 1, 5 and 10%.

Table A7: Robustness: Conflict exposure and education by migration status

	Years of education			
	(1)	(2)	(3)	(4)
Moderate-intensity conflict years	-0.035	-0.061	-0.132***	-0.193*
	(0.039)	(0.080)	(0.050)	(0.101)
High-intensity conflict years	-0.624***	-0.167	-0.213	-0.098
	(0.188)	(0.256)	(0.227)	(0.262)
Moderate-intensity conflict years ×			0.109***	0.148***
Non-migrant hh.			(0.032)	(0.048)
High-intensity conflict years ×			-0.509***	-0.087
Non-migrant hh.			(0.191)	(0.337)
Non-migrant hh.			-0.057	0.074
			(0.050)	(0.070)
Observations	68,110	37,962	68,110	37,962
R-squared	0.502	0.767	0.502	0.767
Gender-age FE	Yes	Yes	Yes	Yes
Weather controls	Yes	Yes	Yes	Yes
Country-cohort FE	Yes	Yes	Yes	Yes
Location FE	Yes	Yes	Yes	Yes
Household FE		Yes		Yes

Note: The table reports estimation results from regressing education on the number of moderate- and high-intensity conflict years (between in utero and age 12) for a sub-sample with data on migration experiences. Specifications include fixed effects and further controls as stated in the table. Conflict events are counted if occurred within 50 km to the survey location. Moderate-intensity conflict years refer to years with less than 1000 battle-related deaths; high-intensity years to 1000 and more deaths. ***,**,* denote significance at 1, 5 and 10%.

Table A8: Robustness: Placebo conflict exposure

	Years of education			
	(1)	(2)	(3)	(4)
Moderate-intensity conflict years in pre-utero	0.055	0.061		
	(0.043)	(0.043)		
High-intensity conflict years in pre-utero	-0.049	-0.053		
	(0.039)	(0.041)		
Moderate-intensity conflict years age 20-25			0.016	0.058***
			(0.012)	(0.020)
High-intensity conflict years age 20-25			0.066	-0.010
			(0.046)	(0.071)
Observations	441,773	363,817	652,003	362,665
R-squared	0.562	0.781	0.545	0.836
Gender-age FE	Yes	Yes	Yes	Yes
Weather controls	Yes	Yes	Yes	Yes
Country-cohort FE	Yes	Yes	Yes	Yes
Location FE	Yes	Yes	Yes	Yes
Household FE		Yes		Yes

Note: The table reports estimation results from regressing education on the number of moderate- and high-intensity conflict years (two years before in utero and during age 20-25). Specifications include fixed effects and further controls as stated in the table. Conflict events are counted if occurred within 50 km to the survey location. Moderate-intensity conflict years refer to years with less than 1000 deaths; high-intensity years to 1000 and more deaths. ***,**,* denote significance at 1, 5 and 10%.

Table A9: Robustness: Conflict exposure with differently clustered standard errors

Dependent:s	Years of education					
	(1)	(2)	(3)	(4)	(5)	(6)
Clustering standard errors:	Location		Location & country-cohort		Spatially corrected	
Moderate-intensity conflict years	0.019*	0.017	0.019	0.017	0.019	0.017
	(0.010)	(0.010)	(0.015)	(0.013)	(0.018)	(0.017)
High-intensity conflict years	-0.161***	-0.116***	-0.161***	-0.116***	-0.161***	-0.116**
	(0.036)	(0.036)	(0.050)	(0.043)	(0.054)	(0.051)
Observations	541,480	480,847	541,480	480,847	541,480	480,847
R-squared	0.550	0.772	0.550	0.772	0.031	0.029
Gender-age FE	Yes	Yes	Yes	Yes	Yes	Yes
Weather controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Location FE	Yes	Yes	Yes	Yes	Yes	Yes
Household FE		Yes		Yes		Yes

Note: The table reports estimation results from regressing education on the number of moderate- and high-intensity conflict years (between in utero and age 12). Specifications include fixed effects and further controls as stated in the table. Conflict events are counted if occurred within 50 km to the survey location. Moderate-intensity conflict years refer to years with less than 1000 battle-related deaths; high-intensity years to 1000 and more deaths. Models vary in the specification of standard errors. , ***,**,* denote significance at 1, 5 and 10%.