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# The Effects of Armed Conflict on Educational Attainment and Inequality

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## Abstract

We exploit the variation in the timing of conflict between countries using a difference-in-differences matching strategy to identify the impacts of armed conflict on years of schooling and educational inequality. We draw upon data from the Uppsala Conflict Data Program and the Ethnic Power Relations databases, which enable us to distinguish between ethnic and non-ethnic conflicts. Further, we are able to identify the effect of conflict onset as well as the incidence of conflict in years following onset. Our results provide evidence that the introduction of any conflict worsens educational attainment and exacerbates pre-existing inequalities thereof. This paper also shows that conflict effects are more pronounced when ethnic in nature and that attainment and inequality outcomes worsen as conflicts persist over time. Our results are robust to different regression specifications and propensity score matching algorithms.

JEL Classification: F51, I24, I25

***Keywords***: Armed Conflict, Ethnic Conflict, Educational Attainment, Educational Inequality, Difference-in-Differences, Propensity Score Matching

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## 1 Introduction

Over half the world's countries have participated in at least one conflict since the end of World War II. Every conflict has a unique set of circumstances and causal factors that contribute to its onset. Prolonged, high-intensity conflicts have profound consequences for human development, leaving deep marks on the economy, political systems and social fabric of the countries ravaged by them. The effects of conflict on education systems are well documented – one does not need to go deep in the research literature to understand the effect of damaged school infrastructure, loss of human capacity, and the risks to safety and security of students and staff in a situation of active conflict.

However, what we seek to understand is whether conflict affects everyone in the same way, or whether it exacerbates disparities that already exist between the various groups in a society affected by it. Earlier work by Omoeva and Buckner (2015) found that the risk of conflict is substantially higher at higher levels of educational inequality, while all other known predictors of conflict are held equal. So, our question is - does conflict generally correct for these inequalities over time, or does it exacerbate them?

Akresh and De Walque (2008) investigate the impacts of the 1994 Rwandan genocide and find that education levels among non-poor boys have declined relative to poor boys, thus lowering the inequality between poor and non-poor groups. This disparity is explained as being likely due to conflict affecting resources among the non-poor disproportionately leading to lower education enrollment and attainment. Valente (2011) who examines the impacts of civil war in Nepal between 1996 and 2006 argues that the conflict leads to higher migration rates among the most educated and affluent groups, whereas the poorest are more likely to remain. Thus, overall educational attainment declines, however the gaps between the most and least educated are narrower. On the other hand, several studies argue and provide some evidence that conflict exacerbates inequalities in education at the subnational level (Kibris, 2015; Shemyakina, 2011; and Aguero and Majid, 2014), between gender groups (Justino, 2010), and between ethnic groups (Østby and Urdal, 2014).

As a result, theoretical predictions of the effect of conflict on education inequality are ambiguous as several mechanisms can run counter to each other. In this study, we disambiguate the effect of conflict on educational attainment and inequality by estimating the net causal effect. We employ a quasi-experimental design to identify the net causal effect of violent conflict on the distribution of educational attainment.[i] Specifically, we employ a propensity score matching difference-in-differences (DD) approach to identify the causal effect of armed conflict on education using a comprehensive panel dataset including 100 countries spanning over 50 years from 1960 to 2010, by comparing the pre- and post-conflict outcomes between similar country-year observations who differ only in terms of their conflict experience. We disaggregate the effects of conflict by nature of the conflict (ethnic and non-ethnic), intensity of the conflict (minor and major), and duration of the conflict.

Our empirical results demonstrate that conflict undermines educational opportunity for some more than others, but that effects are considerably nuanced and context-dependent, at times even contradictory when viewed across the universe of conflicts, with impacts borne disproportionately by poorer or wealthier families, girls or boys, or ethnic or religious groups depending on the setting. In terms of educational attainment, our findings show robust evidence that years of schooling decrease with conflict. More interesting are the results from ascertaining the distribution of education attainment in the event of armed conflict. To that end, we find that the incidence of armed conflict, on average, worsens disparities in education between wealth groups, gender groups, and overall inequality at the national level. The disaggregated results show that the conflict effect is most prominent among ethnic conflicts that last at least six years and worsens over time. In addition, we find that countries whose observable demographic, economic, and political characteristics predict a high likelihood of conflict are affected most by the incidence of such events.

## 2 Conceptual framework: The impacts of internal conflict on education

Characterized as "development in reverse" (Collier et al., 2003, p. 2), internal conflict disrupts educational access and provision and, consequently, can affect human capital stocks in the long-term. Cross-national analyses have established that educational participation often wanes during periods of conflict (Lai & Thyne, 2007; Shields & Paulson, 2015; Stewart, Huang, & Wang, 2000; Poirer, 2012). Case study evidence corroborates these findings, with evidence for declines in educational attainment among conflict-affected populations in Bosnia (Swee, 2009), Tajikistan (Shemyakina, 2011), Rwanda (Agüero & Majid, 2014; Akresh & De Walque, 2008), Colombia (Rodriguez & Sanchez, 2009), Croatia (Kecmanovic, 2012), Zimbabwe (Alderman, Hoddinott, & Kinsey, 2006), and Guatemala (Chamarbagwala & Moran, 2011).[ii]

Internal conflict undermines public service delivery, including that of education, through numerous mechanisms. In the realm of public health, Ghobarah, Huth, and Russett (2003) identify several ways that conflict compromises outcomes in the short- and long-term, including through: (a) reductions in resources due to economic slowdowns, loss of trained professionals, and destruction of key infrastructure; (b) shifts in spending from public services to security or other war-induced investments; and (c) complications to service delivery resulting in less efficient use of resources (e.g., greater difficulty supplying medicine where transportation infrastructure is reduced). Gates, Hegre, Nygård, and Strand (2012) argue that these mechanisms extend more generally to the reduction of public services, including education, following violent conflict. Collier et al. (2003) group these mechanisms under two channels: funding decreases and "technical regress," including the destruction of infrastructure and displacement, that makes it hard for individuals to maintain health (2003, p. 26) and, similarly, to education. Put otherwise, conflict impacts both (a) macro-level national public services planning and resourcing[iii] and (b) micro-level determinants of access to services, with the cumulative effects of micro-level outcomes amounting to technical regress.

Justino (2016) fleshes out the micro-level channel for education in a review of research on conflict effects to education. Justino identifies supply-side constraints to education in the wake of conflict, namely harm to infrastructure, such as schools, crucial to the provision of education and displacement to camps, where the availability of schooling opportunities is more limited and factors, like overcrowding, reduce quality. Along these lines, educators may be targets of violence, as in the case of the long conflict in Colombia (Novelli, 2010). These resonate with the Ghobarah et al. (2003) mechanisms but are detailed specifically for education. Justino also identifies demand-side barriers to educational participation during conflict, specifically that some youth who would otherwise be in school during periods of stability are not during conflict because (a) they participate—often involuntarily—in rebel groups or the military and (b) they are kept out of school for their safety or because the relevance of education to job markets lessens. Two additional demand-side mechanisms, diminished health, nutrition, and psycho-social wellbeing and greater poverty during conflict, both of which are regular results of conflict (Gates et al., 2012; Chen, Loayza, & Reynal-Querol, 2008), have indirect effects on education (e.g., Alderman, Hoddinott, & Kinsey, 2006). In these situations, households may struggle with the cost (or opportunity cost) of sending a child to school and children may suffer malnutrition, which limits cognitive abilities in the short-term and long-term, especially when experienced in early childhood.

### 2.1 Impacts of internal conflict on the distribution of education in a society

As Justino (2016) observes, the disruptive nature of war often reshapes the social fabric of a country, and shifts have the potential to systematically impact some more than others. Furthermore, the severity and intensity of violence during internal conflict varies within a country and, as such, the consequences of war for education are likely to be distributed unevenly. When the negative consequences of conflict are borne disproportionately by already disadvantaged groups, inequality would rise. In a cross-national analysis spanning 1960-2016, Bircan, Bruck, and Vothknecht (2016) demonstrate that income inequality increases following conflict. This finding suggests that conflict might lead to increased *educational* inequality, as households suffering greater economic hardship may be less able to fund their children's education or to provide the nutritional staples that underpin strong cognitive development and educational outcomes.

A few case studies suggest that this mechanism may be playing out in several contexts. Kibris (2015) finds that conflict exposure of applicants was associated with lower scores on university entrance exams in Turkey. In an example of rising gender inequality, Shemyakina (2011) shows that educational attainment among women, particularly poor women, suffered more than that of men during internal conflict, reinforcing preexisting gender gaps in education in Tajikistan. In Guatemala, Chamarbagwala and Morán (2011) find that average years of schooling drops for Mayan youth, historically an educationally disadvantaged population.

Alternatively, the well-documented declines in educational attainment from conflict may *reduce* inequality for the perverse reason that educational drops are larger among the elite. For example, in Burundi during the 1993 violence, wealthier individuals were more likely to be adversely affected (Bundervoet, 2009). In Liberia, conflict incidents were more prevalent in wealthier areas (Hegre, Ostby, and Raleigh, 2009), which may have resulted in particularly severe consequences for wealthier populations. Violence during internal conflict may even *target* elites, as with Rwandan and Cambodian genocides (de Walque, 2006; Justino, 2016), leading to lower inequality. Lower inequality may also be a conflict outcome where education is affected for all, but where disadvantaged groups have low initial levels of education so that a floor effect limits the extent of their declines. For instance, in Rwanda, Akresh and De Walque (2008) find education levels among men and the non-poor were most reduced during the genocide and suggest this may be because they had more to lose—education among women and the poor was already low at the outset of the genocide.

Because for educational inequality can rise or fall because of internal conflict, a central aim of this study is to examine whether one of those patterns has dominated conflicts experienced over the past half century. While cross-national research on the outcomes of different types of internal conflicts has been limited, there is reason to expect that ethnic conflicts *could* be particularly influential on inequality. Ethnic conflicts are defined by Wimmer, Cederman, and Min in their Ethnic Power Relations (EPR) dataset, which we employ in this study, as “conflicts over ethnonational self-determination, the ethnic balance of power in government, ethnoregional autonomy, ethnic and racial discrimination (whether alleged or real), and language and other cultural rights” (2009, online appendix para 20). With ethnic affiliation and ethnic motives shaping some conflicts, it follows that outcomes of conflict may differ along group lines.

Costalli, Moretti, and Pischedda (2017) examined the impact of ethnic conflict on economic outcomes and found it to be no more influential than ideological ones; however, few quantitative analyses have looked at the unique effects of ethnic conflict on education or educational inequality. Yet educational inequalities may occur through political mechanisms in which education systematically discriminates against ethnic, religious, gender, and other groups, with new—potentially discriminatory mechanisms—sometimes instituted as a direct reaction to conflict. Bush and Saltarelli (2000) explain that the winners of conflict may adapt the education system to favor their group, changing the language of instruction or curriculum or even restricting the educational opportunities of certain groups, for example in apartheid in South Africa. Davies (2010) offers Sri Lanka, Northern Ireland, and Bosnia-Herzegovina as examples where education has been segregated along ethnic or religious lines. Finally, ethnic conflicts may also have unique but indirect impacts on the education of certain groups, such as when some groups are more vulnerable to displacement and, therefore, fewer educational opportunities. Czaika and Kis-Katos (2009) find, for example, that migration was more likely to occur from villages with larger ethnic Javanese in the 1999-2002 conflict in Aceh, Indonesia and explain that this may be due to the threats to directed towards Javanese populations during conflict.

In sum, internal conflicts reshape the distribution of education in a society through the multitude of mechanisms established in literature examining conflict and its outcomes, with ethnic internal conflicts having the potential to be particularly influential. This study builds on the current literature on the topic by empirically identifying the net effect of these forces on educational inequality in internal conflicts in 100 countries from 1960 to 2010 and whether there are differential effects under ethnic versus non-ethnic conflicts.

# 3 Methods

## 3.1 Identification Strategy

### 3.1.1 Difference-in-Differences

We employ a difference-in-differences (DD) strategy to determine the effect of conflict on educational outcomes, by comparing the change in outcomes in pre- and post-conflict periods between treatment and control country-year observations. Formally, we write the DD regression equation as follows.

$$y_{ct} = \gamma_c + \lambda_t + \phi f(p_t) + \delta(T_c.Post_t) + X\beta + \varepsilon_{ct} \tag{1}$$

where $Y_{ct}$ represents mean years of schooling among youths aged 15-24 years old or inequality in education in country $c$ observed in year $t$. We measure educational inequality using the Gini coefficient evaluated at the national (within-country) level, between-ethnic/religious groups, and between-wealth decile groups. Additionally, we measure educational inequality between gender groups using the gender parity index. $\gamma_c$ and $\lambda_t$ represent country and year fixed effects, respectively. $T_c$ is a dummy variable that denotes the treatment group and takes on a value of one if country $c$ has experienced armed conflict at least once since 1946, zero otherwise. $Post_t$ is a dummy variable that denotes the incidence of conflict in year $t$. Therefore, variation in the interaction of $T_c$ and $Post_t$ identifies the treatment effect of armed conflict on the outcome $y_{ct}$, $\delta$.

$X$ denotes a matrix of observable time-varying country-level characteristics, lagged by five years, that are known correlates of educational attainment and inequality.[iv] Specifically, we include variables that account for the level of economic activity (real GDP per capita), oil production per capita, total population, youths aged 15-24 as a proportion of total population, indicator variables for whether countries are considered a democracy, anocracy, or autocracy, and the number of ethnic/religious groups present in each country. Omoeva and Buckner (2015) show that countries with lower levels of economic development are more likely to experience conflict and tend to have lower levels of educational attainment.[v] Similarly, democratic political structures are positively correlated with educational attainment and negatively correlated with conflict (Persson, 2014). Fearon and Laitin (2003) show that the number of ethnic groups, in other words the degree of ethnic fractionalization, are a significant predictor of conflict.[vi] As such, these covariates are included in the analysis to mitigate omitted variable biases when estimating $\delta$. Finally, $f(p_t)$ is a quadratic time trend during peacetime that enables us to control for pre-conflict onset time trends in mean years of schooling and inequality in educational attainment.

In addition to the basic DD setup described in equation [1] that estimates the average effect of conflict on mean years of schooling and educational inequality, we test for heterogeneity in the treatment effect. First, we test whether the effect of conflict varies by nature of the conflict, ethnic or non-ethnic. Equation [1] becomes:

$$y_{ct} = \gamma_c + \lambda_t + \phi f(p_t) + \sum_{j=1}^{2} \delta_j \left(T_c^j . Post_t\right) + X\beta + \varepsilon_{ct} \qquad [2]$$

In this case, $T_c^1$ represents a non-ethnic conflict in country $c$ in year $t$, while $T_c^2$ represents an ethnic conflict for the same country-year observation. The parameters of interest are $\delta_1$ and $\delta_2$ that denote the average treatment effect of non-ethnic and ethnic conflicts, respectively, as identified in the DD framework.

Collier, Hoeffler, and Soderbom (2004) examine the duration of civil wars and show that certain economic and political factors and inequalities therein increase the likelihood of lengthening these conflicts. As such, we allow the average treatment effect, as identified in equation [2], to vary over the duration of the conflict spell. This enables us to test for the heterogeneity of the treatment effect over time and determine whether conflict creates a shock on the educational system that attenuates over time or is permanent. In addition to the estimating the treatment effect of conflict over time, we estimate the effect on educational outcomes following the conclusion of said conflict, as follows:

$$y_{ct} = \gamma_c + \lambda_t + \phi f(p_t) + \sum_{j=1}^{2} \delta_j \left(T_c^j . Post_t . f(w_t)\right) + X\beta + \varepsilon_{ct} \qquad [3]$$

To estimate the conflict effect over the duration of the conflict, we interact the term $T_c^j . Post_t$ with a parametric function $f(w_t)$, where $w_t$ represents the number of years of the conflict as of year $t$. Therefore, $\delta_j$ becomes the slope of the change in educational outcomes over time for conflict countries relative to non-conflict countries.

In addition to satisfying OLS assumptions, the DD strategy requires that pre-conflict trends in schooling attainment and inequality to be parallel for both the treatment and control groups to produce causal effects.[vii] We address the "parallel" trends assumptions in our basic specification that includes a quadratic time trend during peacetime (pre-conflict). However, it is still likely that our estimates of $\delta$ may not yield causal estimates due to a systematic mismatch in both observed and unobserved time-varying country characteristics, since assignment of conflict is non-random. As such, we pursue a strategy that combines the advantages of the DD technique with matching to correct for potential selection bias.

### 3.1.2 Propensity Score Matching

We follow the methods proposed by Heckman, Ichimura, and Todd (1997, 1998) to match treatment and control countries based on their pre-conflict attributes. Moreover, we implement several variants of propensity score matching and weighting schemes to construct a synthetic control group that best resembles the treatment sample in pre-conflict onset periods. We model the assignment of the treatment, conflict, by estimating the propensity score for each country-year observation using a logit model of the probability of experiencing conflict as a function of quadratic peace years, $f(p_t)$, and time varying country-level observable characteristics, $X$. Since we are concerned with selecting the control group based on pre-conflict characteristics, we estimate propensity scores using as follows.

$$\ln\left(\frac{P(w_{ct})}{1-P(w_{ct})}\right) = \gamma_c + \lambda_t + \phi f(p_t) + X\beta + \upsilon_{ct} \qquad [4]$$

where $P(w_{ct})$ denotes the probability that country $c$ in year $t$ experiences conflict and $w_{ct} = T_c.Post_t$. As a result, equation [4] yields estimates of the propensity score, which will be the basis for our propensity score matching strategy. Propensity scores are therefore nothing other than the predicted value of $P(w_{ct})$. For simplicity, we will denote the propensity score as $\hat{p}_{ct} = \hat{P}(w_{ct})$. Keeping in line with Heckman, Ichimura, and Todd (1997, 1998), we compute the DD matching estimator using a kernel/local linear weighting matching algorithm. The counterfactual outcome is constructed as the kernel-weighted average of the outcome of all country-year observations in the control group. To mitigate the concern that 'bad' matches may be used, we enforce the common support condition by trimming the propensity score distribution between the treatment and control groups. The DD matching estimator under kernel based weighting is computed as follows:

$$\hat{\delta}^{k} = E[w_{ct}(y_{ct} - y_{ct'}) - (1 - w_{ct})W(\hat{p})(y_{ct} - y_{ct'})] \qquad [5]$$

where $W(\hat{p})$ denotes kernel weights according to the following

$$W(\hat{p}) = \frac{K\left(\frac{\hat{p}_j - \hat{p}_i}{h_n}\right)}{\sum_j^{n_c} K\left(\frac{\hat{p}_j - \hat{p}_i}{h_n}\right)} \qquad [5a]$$

$K$ is a kernel function, $h_n$ is a bandwidth parameter, $j$ is an index for countries in the control group and $n_c$ is the total number of observations in the control group.

We also estimate the average treatment effect, or the average effect of conflict, on mean years of schooling and/or inequality in educational attainment using propensity scores as inverse probability weights (IPW) as follows:

$$\hat{\delta}_{ATE}^{ipw} = E\left[\frac{w_{ct}}{\hat{p}}(y_{ct} - y_{ct'}) - \frac{(1 - w_{ct})}{1 - \hat{p}}(y_{ct} - y_{ct'})\right] \qquad [6]$$

Using the same IPW matching algorithm, the average treatment effect on the treated, or the average effect of conflict on conflict-affected countries, is estimated as:

$$\hat{\delta}_{ATT}^{ipw} = E\left[w_{ct}(y_{ct} - y_{ct'}) - \frac{\hat{p}}{1 - \hat{p}}(1 - w_{ct})(y_{ct} - y_{ct'})\right] \qquad [7]$$

The DD matching estimator, similar to the simple DD estimator, requires that the parallel trends condition hold in pre-treatment periods for the treatment and control groups to produce causal estimates. The second condition for the DD matching estimator to produce causal estimates of $\delta$, is that the propensity of receiving the treatment, or experiencing conflict, be strictly between 0 and 1. The third condition requires that the treatment and control samples be balanced along their covariates. Finally, it is important that the propensity score distributions for the treatment and control groups overlap, or fall under a common support. Heckman, Ichimura, and Todd (1997, 1998) show that a violation of common support, or comparing observations that are incomparable, can lead to biased estimates of $\delta$.

With both the simple DD and matching DD strategies, it is possible that the differences between treatment and control observations include unobserved characteristics that may influence educational outcomes, in addition to the obvious difference in treatment assignment. For both strategies to produce valid causal effects, the unobserved characteristics of both the treated and control observations are assumed static, pre- and post-treatment. However, available data do not allow us to empirically test this assumption. As such, a potential drawback to our estimation strategy is that we must assume that the unobservables are static. In case this assumption does in fact fail, our estimates of the treatment effect are influenced by changes in unobservables as well, yielding a net effect of conflict on educational outcomes through the various mechanisms described in the conceptual framework provided earlier in this paper.

## 4 Data and Descriptive Analysis

We construct the analytic dataset for this paper by combining aggregate country-level data as well as micro-level data that we aggregated to the country-level from various publicly available sources. The remainder of this section describes the dataset construction by outcome variables, conflict (treatment assignment) variables, and control variables. Finally, we compare the mean values for treatment and control observations and test for statistical significance of the mean differences. Comparing outcomes by treatment status denotes simple difference in means between conflict and non-conflict observations. However, comparing mean differences in the covariates serves as our test for sample balance prior to propensity score matching via inverse probability weights.

## 4.1 Educational Attainment and Inequality

### 4.1.1 Dataset Construction

We draw educational attainment data from three public-use household survey data: Multiple Indicator Cluster Survey (MICS) administered by UNICEF, the Demographic and Health Surveys (DHS) program funded by USAID and administered by ICF international, and the Integrated Public-Use Microdata Series-International (IPUMS-I) as collected and maintained by the Minnesota Population Center at the University of Minnesota. From these data, we compute the mean years of schooling for all individuals aged 15 years or older in 10-year increments and compute aggregate means at the national, ethnic/religious, gender, and wealth decile levels. In addition, we compute education Gini coefficient, Theil index, and coefficient of variation across these same dimensions for all available country-year observations.[viii]

We follow a method similar to Barro and Lee (2010) to fill in missing country-year observations using a logical backward projection technique in 10-year increments. We stratify our projections by age group, gender, and by 5-year schooling bins. However, unlike

Barro and Lee (2013), we empirically measure the amount of additional schooling accumulated by individuals over 25 years after the age of 25. We find that individuals with fewer than 5 years of schooling accumulate approximately zero additional years of schooling after age 25 years. Conversely, men and women with more than 10 years of schooling accumulate between 0.3 and 0.7 years of schooling after age 25 years. In addition to computing the amount of schooling gained after 25 years of age, we adjust for the national mortality rate by age group and gender. Thus, we are able to extrapolate backwards the mean value of years of schooling for all age cohorts as far back as four decades prior to the administration of each household survey. Additionally, we create back-projections for specific ethnic, religious, wealth, and gender subgroups of each country that enables us to accurately estimate inequality measures where data were originally missing.

The final step in completing the construction of our educational attainment and inequality dataset, we fill in all missing country-year observations by interpolating between observed and/or backward extrapolated data points, separately for each country. In this case, we use simple linear interpolation to determine an approximate value for the missing years of schooling. Finally, the educational attainment and inequality dataset yields 4,650 country-year observations, which covers 95 countries over 68 years (1946-2013). However due to small sample sizes among the oldest and newest years, we restrict the final analytic sample to data points prior to (and including) 2010.

#### 4.1.2 Inequality Measurement

The measurement of inequality is highly contested in the literature as different measures pose some comparative advantages over others.[ix] To measure inequality in attainment between ethnic/religious groups, wealth groups, or nationally, we use the Gini coefficient due to its popular use in research and the gender parity index to convey disparities between males and females. We do not assert the use of any particular measure over another as the superior metric. However, to ensure the robustness of our findings we employ alternative measures of educational inequality, the Theil index and the coefficient of variation. We define the educational outcome variables of interest as follows.

Mean years of schooling, by country,

$$\bar{y}_{ct} = \frac{\sum_{i=1}^{n} y_{ict}}{n} \quad [8a]$$

overall (within-country) Gini coefficient,

$$g_{ct} = \frac{1}{2n^2 \bar{y}_{ct}} \sum_{i=1}^{n} \sum_{j=1}^{n} |y_{ict} - y_{jct}|; \quad [8b]$$

between-group Gini coefficient for each country,

$$gg_{ct} = \frac{1}{2\bar{y}_{ct}} \sum_{r=1}^{R} \sum_{s=1}^{S} p_r p_s |\bar{y}_{rct} - \bar{y}_{sct}|; \quad [8c]$$

within-country gender parity index in mean years of schooling:

$$gpi_{ct} = \frac{\bar{y}_{ct}^{f}}{\bar{y}_{ct}^{m}}; \quad [8d]$$

Finally, as a robustness check for the use of the Gini coefficient, we also compute the within-country Theil index and between-group Theil index,

$$t_{ct} = \frac{1}{n} \sum_{i=1}^{n} \frac{y_{ict}}{\bar{y}_{ct}} \ln\left(\frac{y_{ict}}{\bar{y}_{ct}}\right) \text{ and } gt_{ct} = \sum_{r}^{R} p_r \frac{\bar{y}_{rct}}{\bar{y}_{ct}} \ln\left(\frac{\bar{y}_{rct}}{\bar{y}_{ct}}\right); \quad [8e]$$

and the coefficient of variation for within-country and between-group:

$$cv_{ct} = \frac{\sigma_{y_{ct}}}{\bar{y}_{ct}} \text{ and } gcv_{ct} = \frac{\sigma_{\bar{y}_{rct}}}{\bar{y}_{ct}} \quad [8f]$$

#### 4.1.3 Sample Statistics and Trends

Table 1 presents the mean and sample size for years of schooling, gender parity ratio for years of schooling[x], Gini coefficient, Theil index, and the coefficient of variation computed at the national, ethnic/religious group, and wealth decile group levels. The sample distributions are further stratified by geographic region. The table shows that our overall sample size varies between 3,956 and 4,579 country-year observations across all educational outcomes except for inequality at the national level.[xi] The sample mean years of schooling for the 95 countries, over the past 50 years is 5.73 years with a mean gender parity ratio of 0.74, and a mean within-country Gini coefficient of 0.43 (out of 1). In contrast, average educational inequality between ethnic/religious group yields a group Gini of 0.11, while the group Gini between wealth deciles is 0.23, on average. This means that wealth inequality in our sample is more prominent relative to the inequality based on ethnic or religious affiliation.

Educational attainment, in terms of mean years of schooling, has more than doubled, globally, over the past half century (Barro and Lee, 2013). This trend holds true for developing and emerging economies. Figure 1 plots trends in education inequality as defined by ethnic/religious groups, wealth decile, and nationally.[xii] We can see that, although trends in years of schooling have been steadily increasing, the progress made in terms of inequality is not as evident. The national education Gini coefficient has remained stable over the past 50 years hovering around 0.4 (out of 1), whereas the ethnic/religious and wealth decile group Gini coefficients have only declined by 0.07 and 0.13 points over the same time period, respectively.

Finally, Figure 2 plots mean inequality across all countries in our sample and across gender, ethnic/religious, wealth decile, and national dimensions of inequality. We can also see, from the figure, that all four inequality measures are correlated with each other: countries with high levels of inequality in one measure also have high levels of inequality on all other measures.[xiii] Countries with high inequality or gaps in education on one dimension tends to have high inequality on all other dimensions as well. The figure also confirms the notion that educational inequality between wealth groups explains a larger portion of total national inequality than between ethnic/religious groups. Moreover, the mean vertical Gini coefficient across all countries is about .43, while the mean wealth decile group Gini coefficient is about .23 and the mean ethnic/religious group Gini coefficient is approximately .11. Across all countries we find that the group Gini across wealth deciles is consistently larger in magnitude than between ethnic/religious groups. Lastly, the disparity between females and males is highest in countries exhibiting high levels of education inequality nationally, between ethnic/religious groups, and between wealth deciles.[xiv]

### 4.2 Incidence of Conflict

In the 70-plus years since the end of World War II, there have been 254 recorded armed conflicts, 114 of which passed the threshold of 1,000 battle-related deaths to be considered wars (Themner and Wallensteen, 2014). Of these conflicts, about 88 percent have been intra-state, half of which are ethnic in nature (Gleditsch et al., 2002; Pettersson and Wallensteen, 2015; Wimmer, Cederman, and Min, 2009).[xv]

Figure 3 displays the proportion of countries experiencing state and ethnic conflict over time, as recorded by the Uppsala Conflict Data Program (Themner and Wallensteen, 2014). As the figure shows, incidence of state-related armed conflict peaked in the 1980s and 1990s where about a quarter of the sample was in a state of conflict. However, ethnic conflict was most prevalent in the 1990s and 2000s.

Although most countries have experienced a form of conflict at any given time in the past century, there is variability in the timing and duration of the occurrence of conflict, providing us with a basis for our DD estimation strategy. This is illustrated by Figure 4, which plots the proportion of all conflict-affected countries by the number of years of ethnic and non-ethnic conflict showing that different countries experience spells of armed conflict over varying durations, some for spells up to 50 years.

### 4.3 Control Variables

The final phase of the analytic dataset construction is linking educational outcomes and conflict incidence data with country-year level economic, demographic, and political characteristics. First, we draw upon data from the Penn World Tables (PWT) to determine real GDP per capita and from Ross and Mahdavi (2015) to obtain oil and gas production per capita, both of which are coupled to proxy for the macroeconomic level of economic production. Second, total and youth population size data are extracted from the United Nations Population Division (UNPOP). Finally, we incorporate political climate indicators from the Polity IV database (Marshall, Gurr, and Jaggers, 2014). We use the country Polity IV index scores to determine whether a country is a democracy, anocracy, or autocracy as follows. A country is defined as an autocracy if $-10 \leq polity \leq -6$, anocracy if $-5 \leq polity \leq 5$, or a democracy if $6 \leq polity \leq 10$.[xvi]

### 4.4 Sample Balance

To support our propensity score matching strategy, we perform a simple test for sample balance between conflict and no conflict observations, our treatment and control groups. Table 2 displays the mean value of all covariates used in determining the likelihood of conflict incidence as well as the mean difference between conflict and no conflict states twice, once under no matching, i.e. the original unweighted sample, and again using kernel-based weights. In the unmatched sample, we find that conflict and no conflict observations are systematically and statistically different along almost all of the observed characteristics included. Educational inequality across all dimensions are higher among conflict countries. Oil production and GDP per capita are lower, population sizes are larger, and the likelihood of being an anocracy is higher among conflict countries than non-conflict countries.

When applying propensity score kernel weighting to our treatment and control groups as well as restricting to observations within the common support of the propensity score distribution, we see that almost all statistically significant mean differences are much smaller in magnitude and statistically insignificant. The only variable that remains significantly different is the five-year lagged education Gini coefficient across ethnic/religious groups. Although the difference is statistically significant, we argue that the magnitude of the difference is relatively small at 1.2 points (out of 100) and is unlikely to greatly impact our subsequent estimates of the effect of conflict on educational attainment and inequality.

Overall, the kernel-based matching algorithm appears to balance the treatment and control groups along observable country characteristics, successfully.

# 5 Findings and Robustness Checks

## 5.1 Empirical Results

### 5.1.1 Effect of Conflict

Table 3 displays the results from estimating equation [1] with log mean years of schooling-($\ln(\bar{y}_{ct})$), gender parity index ($gpi_{ct}$), national Gini coefficient ($g_{ct}$), ethnic/religious group Gini coefficient ($gg_{ct}^{ethn}$), and wealth decile group Gini coefficient ($gg_{ct}^{wealth}$) as outcomes. Further, the table presents the regression results under three different matching algorithms. The first panel presents the results from the simple difference-in-differences (DD) estimator with no matching. The second panel presents the results from the DD matching estimator using kernel-based weights. The final panel presents the results from the DD matching estimator with propensity scores used as inverse probability weights. We note that all matching estimators use weighting schemes to identify average treatment effects on the treated (ATT). In other words, we apply propensity score weighting to determine the effects of conflict on conflict-affected countries, rather than the average effect of conflict on any given country, which would be the average treatment effect (ATE).

Table 3 shows that, generally, the simple DD estimator understates the effect of conflict on mean years of schooling and educational inequality and that estimates are somewhat less precise in comparison to either of the DD matching estimates. As such, even when employing a DD strategy, we show that selection into the treatment group is still a valid source of bias when estimating the treatment effect. The estimates of $\hat{\delta}^{K}$ and $\hat{\delta}^{IPW}$, the average effect of conflict on conflict-affected countries under both matching techniques, are similar across all outcomes. This indicates that the estimates of the conflict effect are robust to the matching algorithm. We find that mean years of schooling are only moderately negatively affected by conflict, on average, and that the estimated effects are not statistically significant. We estimate that conflict lowers mean years of schooling by 0.6 to 1.5 percent for countries that have ever experienced conflict. Relative to the mean years of schooling among conflict affected countries, the incidence of conflict lowers attainment by between 3 percent and 7.6 percent of a year of schooling.

However, in terms of the effect of conflict on various dimensions of educational inequality, we find that the GPI, the national Gini coefficient, and the wealth decile group Gini coefficient increase following the incidence of conflict. Therefore, conflict, on average, lowers GPI by 3.3 to 3.5 points (out of 100). Relative to a mean GPI for countries that have ever experienced conflict of 69.6 points, conflict lowers the GPI by approximately 5 percent. Similarly, we find that the incidence of conflict leads to higher education inequality at the national level, where, on average, conflict increases the national education Gini coefficient by 0.7 to 0.9 points (out of 100). This estimate translates to a 2 percent increase in inequality as measured by the Gini coefficient, relative to an average national Gini of 45.4 points.

The effects on education inequality are further corroborated when examining the impacts of conflict on education inequality between wealth deciles. We estimate that conflict increases the education inequality between wealth groups by 1.1 to 1.3 points (out of 100). Relative to the average wealth group Gini coefficient of 24 points, this effect translates to a

5.4 percent increase in education inequality between wealth deciles. Finally, when we examine the effects of conflict on education inequality between ethnic/religious groups, we find similar effects that are not statistically significant at the conventional levels. Nevertheless, we find that conflict (regardless of type) increases education inequality between ethnic/religious groups by 0.4 to 0.5 points (out of 100). However, relative to the average group Gini at the ethnic/religious group level, this effect translates to a 3.9 percent increase in inequality.

### 5.1.2 Effect of Conflict, by Type (Ethnic and Non-Ethnic)

In the following analysis, we disaggregate the effect of conflict to determine whether conflicts have differential effects on educational outcomes in terms of ethnic and non-ethnic armed conflicts. Table 4 presents the results from estimating equation [2] on mean years of schooling and inequality in education, nationally, by gender groups, by ethnic/religious groups, and by wealth deciles. Similar to Table 3, we also present the estimation results under different matching algorithms. Across all specifications, we consistently find that the effects of conflicts are more pronounced when the conflict is ethnic in nature than when it is not. Additionally, we find that the simple DD estimator produces smaller effect sizes relative to the DD matching estimators, which means that it is likely that the DD estimator is comparing countries that are incomparable to identify the effect of conflict.

The first column of Table 4 shows that non-ethnic conflicts have little to no effect on mean years of schooling, while ethnic conflicts lower mean years of schooling by 2.2 to 2.7 percent. The pattern is duplicated when examining the effects on education inequality along gender groups. Ethnic conflicts lower GPI by 5.3 to 5.5 points, which relative to the average

GPI of 69.6 points is an 8 percent decrease in gender parity. On the other hand, non-ethnic conflicts have a relatively smaller and statistically insignificant effect on GPI by between 1.7 and 1.9 points (2.7 percent decrease).

The third column of Table 4 shows that overall inequality is higher during the incidence of conflict and more so when the conflict is ethnic by nature. We estimate that the overall Gini coefficient for education inequality increases by 1.2 to 1.7 points (2.7 to 3.8 percent) during ethnic conflicts and only increases by 0.4 to 0.5 points during non-ethnic conflicts (1 to 1.1 percent). However, we note that these estimates are not statistically significant at the conventional levels. When examining the effect of conflict on education inequality along wealth decile groups, we again find that non-ethnic conflicts have a small and insignificant effect on inequality while ethnic conflicts have a larger and statistically significant effect on inequality. Moreover, we estimate that ethnic conflicts increase inequality in education between wealth groups by 2.2 to 2.3 points. Relative to a mean group Gini coefficient of 24 points, ethnic conflict increases inequality using this measure by 9.2 to 9.6 percent.

Surprisingly, when estimating the heterogeneous impact of conflict, by type, on ethnic/religious inequality in education, we find that non-ethnic conflicts tend to exacerbate inequality along ethnic/religious lines while ethnic conflicts have almost no effect. Specifically, we estimate that the ethnic/religious group Gini coefficient for educational attainment is increased during non-ethnic conflicts by 0.9 points. This effect size translates to a 7 percent increase in the ethnic/religious group Gini coefficient relative to an average group Gini coefficient of 12.7 points.

### 5.1.3 Effect of Conflict, by Type and Duration

Table 5 displays the results from estimating equation [3], where we estimate the heterogeneous effect of conflict by type and by the duration of conflict. Again, we find that non-ethnic conflicts generally exhibit smaller effects relative to ethnic conflicts across all education outcomes. Ethnic conflicts, by contrast, show detrimental effects on outcomes for the duration of unrest, reaching a plateau as the conflict winds down. Figure 5 plots the marginal effects of ethnic conflict over time prior to the onset of, during, and following the conclusion of conflict.

Effects of non-ethnic conflicts are statistically insignificant across the board in the first five years after onset. In the following years (6-10 years since onset), non-ethnic conflict exacerbates ethnic inequality in education by 1.6 points, and this effect grows in magnitude with longer duration conflicts: 1.4 points 11-15 years since onset, and by 2.9 points when the duration of the conflict exceeds 16 years. In terms of percentage change in ethnic/religious group Gini, we estimate that the Gini coefficient increases by 12.6 percent in years 6 through 10, by 11 percent in years 11 through 15, and by 22.8 percent in years 16 and onward.

On all education outcomes, other than between-ethnic/religious group inequality, ethnic conflicts exhibit a larger magnitude effect that increases with the duration of conflict. Moreover, we find that mean years of schooling increase by 2 percent during the first five years of ethnic conflict, but decrease in all periods afterward before dropping significantly after at least 16 years of conflict. The effect of ethnic conflict between years 6 and 10 of the conflict is virtually zero, but decreases by 2.2 percent in years 11 through 16 of the conflict.

More noticeable, is the effect of ethnic conflict on mean years of schooling after at least 16 years of conflict, where we estimate an 18.2 to 18.7 percent decrease in mean years of schooling. Relative to a mean of 5.1 years, this effect translates to about 0.95 years of schooling among countries that have ever experienced conflict.

In terms of gender inequality, ethnic conflicts tend to have a substantial impact relatively early on. We find that GPI decreases by about one point in the first five years, by 7.3 to 7.4 points in years 6 through 10, by 10.5 points in years 11 through 15, and by 9.3 points in the years that follow. Relative to the mean GPI, this translates to a gender parity discrepancy in attainment by 1.4 percent in the first five years, by 10.6 percent in the second five years, by 15.1 percent in the eleventh through fifteenth year of the conflict, and by 13.5 percent after at least 16 years of ethnic conflict.

For overall inequality, we find a similar pattern, however, the effect sizes are relatively small. We estimate that the within-country Gini coefficient for education increases by 1 point in the first 10 years of ethnic conflict, by 1.2 points in the five years that follow, and by 2.2 points after at least 16 years. Relative to the mean national Gini of 69.6 points, these effects are equivalent to an increase of 2.2 percent in the first 10 years, 2.6 percent in years 11 through 15 of the conflict, and by 4.8 percent after 16 years or more of ethnic conflict. However, we find mostly small and statistically insignificant results in terms of the Gini coefficient between ethnic/religious groups.

Finally, we find that ethnic conflict does not affect inequality between wealth decile groups within the first five years of the conflict. We argue that it is likely due to our choice of using attainment as the measure of educational outcomes, where certain portions of the population will be unaffected by conflict if their education is already complete. On the other hand, children who are in school at the time of the incidence of conflict are those who would be affected by the time they are 15-24 years old. As such, we find that ethnic conflict exacerbates pre-existing education inequality between wealth groups by 2.3 points in years 6 through 10 of the conflict, by 3.7 points in years 11 through 15, and by 5.3 points in the years following the fifteenth year of the conflict. This is equivalent to an increase in the between-wealth group Gini coefficient by 9.6 percent in years 6 through 10 of the conflict, by 15.4 percent in years 11 through 15, and by 22 percent after at least 16 years of ethnic conflict.

## 5.2 Robustness Checks

In the following subsection of the analysis, we perform several robustness checks to test for potential misspecification in our regression models and, consequently, in the results reported in the previous sections. We first test for the validity of the DD common trends assumption prior to the incidence of conflict (the treatment). Second, we perform a placebo test using only pre-treatment data with a randomly assigned treatment date for countries who have ever experienced armed conflict between 1946 and 2010. Since the first two checks test for the validity of the DD identification strategy, we run the same analysis as reported in Table 3 using alternative measures of inequality nationally, and along gender, ethnic/religious, and wealth decile groups.

### 5.2.1 Common pre-treatment trends

To test for parallel trends during pre-treatment periods for both the treatment and control groups, we estimate a variant of equation [1] whereby we include country-specific quadratic peace year trends. Additionally, we replicate the analysis under the new specification and under no matching, kernel matching, and propensity scores as inverse probability weights. This strategy will enable us to test whether differences in pre-treatment trends exist and confirm whether the DD common trends assumption holds. As such, if the estimated treatment effects alter significantly when using country trends, then we would reject the null hypothesis that the treatment and control groups share parallel or common pre-conflict trends. Our objective in this analysis is to ascertain the validity of the DD approach and that the treatment and control groups did in fact follow similar trends prior to the incidence of conflict.[xvii] This approach tests whether identification of the treatment effect is via within-country changes in conflict incidence rather than a function of diverging trends in educational outcomes.

Table 6 presents the estimation results for the specification including country trends. We can see that the estimated effects of any conflict are largely the same as those presented in Table 3, where the specification did not include country trends. This result holds true under no matching, kernel matching, and propensity scores as inverse probability weights. Across all specifications and matching algorithms, the magnitude, direction, and statistical significance are unaltered between results from Table 3 and Table 6. Although, the estimated effects from including country trends are slightly smaller, although the differences are not statistically significant. As a result, we can reject the hypothesis that countries in the treatment and control groups had pre-conflict trends that were different which supports the validity of the DD estimates.

### 5.2.2 Falsification test

We perform an additional check to the robustness of the DD estimates using a falsification test. Using only data from pre-conflict years for the treatment group and maintaining all data from control group, we randomly assign a false conflict onset date for the treated countries and extend the post-treatment period to every year following the false onset year. This enables us to run the DD strategy and test whether pre-conflict trends between the treatment and control countries diverged, thus violating the common trends assumption. Additionally, the false onset year doubles as a placebo test as countries assigned to the false conflict should not be affected. Therefore, if the test shows that the false DD effect is statistically significant then the DD strategy suffers from pre-treatment trends that are not parallel as well as potentially spurious correlations between conflict and education attainment and inequality.

The results of the falsification test are presented in Table 7 where we estimate the DD equation [1] using only pre-conflict data on mean years of schooling, the gender parity index, national Gini, ethnic/religious group Gini, and wealth group Gini. Across all outcome variables, we find no statistically significant estimates of the false conflict. Additionally, the direction of the DD estimate for each educational outcome runs counter to the actual DD estimates. If the DD estimates presented in Tables 5 through 9 were biased, the bias would be positive for mean years of schooling and gender parity index, and negative for all Gini outcomes. This means that, at worst, the DD strategy underestimates the true effect of conflict, and represents a lower bound to the effect of conflict. Given the results of the falsification test, we reject the hypotheses that the treatment and control groups do not have common pre-treatment trends, and that the DD estimates are spurious.

#### 5.2.3 Alternative inequality measures

The final robustness check that we perform deals with the choice of the gender parity index and Gini coefficient as the main measures of educational inequality nationally and between groups. To check for the sensitivity of the gender parity results we run the same analysis as presented in Table 3 but using gender attainment gap (measured in years) as the inequality measure. To test the sensitivity of our results for the Gini coefficient, we replicate the analysis using the Theil index and coefficient of variation as the measures of within-country and between-group inequality.

The results of the replication analysis with alternate inequality measures are presented in Table 8 under no matching, kernel matching, and propensity scores as inverse probability weights. The estimated DD effects under the different matching algorithm mirror the findings from the original inequality measures in terms of direction and statistical significance. Specifically, we find that the gender education gap and the within-country inequality in education increases in response to the incidence of conflict. Further, we find almost no effect on ethnic/religious group inequality via the Theil index or the coefficient of variation. We also find that between-wealth group inequality increases when using the coefficient of variation. However, the estimated effect of conflict is less precise using the group coefficient of variation, whereas the group Gini coefficient appears to produce smaller standard errors.

### 5.3 Conclusion and Policy Implications

The objective of this paper is to investigate and ascertain the links between the incidence of violent conflict and inequality in education, building empirical support for the relationship that has so far had theoretical grounding, but limited empirical evidence. We find that conflict, in general, lowers mean attainment by about 7.6 percent of a year of schooling, increases inequality at the national level where the Gini coefficient increases by approximately 2 percent, lowers the gender parity ratio by 5 percent, and increases the educational inequality between wealth decile groups by 5.4 percent as measured by the between-group Gini coefficient.

Furthermore, we take a more nuanced investigation of the effects of conflict on educational outcomes by disaggregating the conflict effect by type (ethnic and non-ethnic) and by type and duration. Across all levels of stratification presented in this paper, we find that ethnic conflicts are relatively more harmful than non-ethnic ones, and chronic ethnic conflicts are more harmful than temporary conflicts of any sort. More importantly, in modeling the trends of education inequality prior to, during, and post-conflict, we find that while education inequality declines in post-conflict years of peace, its levels tend to plateau or decline slowly, and potentially never reaching pre-conflict values depending on the duration of the conflict.

On average, ethnic conflict lowers mean years of schooling by 0.14 years, widens the gap between boys and girls by about 8 percent, increases the national Gini coefficient for education by 3.8 percent, and increases the Gini coefficient between wealth groups by 9.6 percent. Ethnic conflicts that last longer than 16 years lower mean years of schooling almost by a full year (0.95 years), increases the gender gap by 13.5 percent, increases overall inequality at the individual level by 4.8 percent, and widens the education gap between wealth deciles by 22 percent on the group Gini coefficient. These findings are consistent with the hypothesis that conflict exacerbates pre-existing levels of education inequality between groups, as well as inequality across individuals. It is important to note that potential inequality reducing effects may still exist at the individual country level – as our literature review indicates. However, this study shows that the impact of conflict is detrimental on net, and the levels of inequality in education cannot be expected to return to pre-conflict levels on their own, especially after a protracted conflict.

These findings provide additional support to the argument that education in conflict and post-conflict contexts does not merely remain the same or worsen for all groups, and that cycles of inequality may deepen, thereby creating the conditions for increased conflict risk, and potentially setting off a vicious cycle. This provides an impetus for greater attention to equity in education, particularly in conflict-affected and fragile settings – with expanding the metrics beyond outcome proxies (such as schooling completed or learning outcomes) to measures of inequality in education resource allocation. Programming and policy in education should also refer to this study as additional support for decisions that favor an equitable – though not always equal – resource distribution in education, particularly in favor of females and groups at the lower end of the socioeconomic spectrum.

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# Tables

## Table 1. Summary Statistics – Education Attainment and Inequality

| | National | | | | | Ethnic/Religious Groups | | | Wealth Decile Groups | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | $\bar{y}$ | $gpi$ | $g$ | $t$ | $cov$ | $gg$ | $gt$ | $gcov$ | $gg$ | $gt$ | $gcov$ |
| East Asia and Pacific | 6.27 | 0.84 | 0.31 | 0.22 | 0.58 | 0.08 | 0.02 | 0.22 | 0.16 | 0.06 | 0.32 |
| | (443) | (440) | (177) | (177) | (177) | (430) | (430) | (386) | (440) | (440) | (440) |
| Europe and Central Asia | 10.23 | 0.96 | 0.15 | 0.06 | 0.29 | 0.04 | 0.01 | 0.10 | 0.07 | 0.01 | 0.14 |
| | (473) | (470) | (56) | (56) | (56) | (491) | (491) | (431) | (470) | (470) | (470) |
| Latin America and Caribbean | 7.46 | 0.96 | 0.38 | 0.31 | 0.70 | 0.09 | 0.03 | 0.22 | 0.17 | 0.06 | 0.33 |
| | (978) | (950) | (616) | (616) | (616) | (858) | (858) | (836) | (931) | (931) | (917) |
| Middle East and North Africa | 5.74 | 0.74 | 0.45 | 0.55 | 0.90 | 0.05 | 0.01 | 0.12 | 0.21 | 0.11 | 0.41 |
| | (160) | (160) | (49) | (49) | (49) | (77) | (77) | (77) | (160) | (160) | (160) |
| North America | 11.68 | 1.02 | 0.12 | 0.04 | 0.24 | 0.01 | 0.00 | 0.03 | 0.02 | 0.00 | 0.05 |
| | (51) | (51) | (51) | (51) | (51) | (51) | (51) | (51) | (41) | (41) | (41) |
| South Asia | 3.72 | 0.43 | 0.60 | 0.76 | 1.14 | 0.17 | 0.06 | 0.39 | 0.30 | 0.18 | 0.64 |
| | (216) | (187) | (42) | (42) | (42) | (215) | (215) | (215) | (215) | (215) | (215) |
| Sub-Saharan Africa | 4.19 | 0.60 | 0.56 | 0.79 | 1.22 | 0.14 | 0.05 | 0.32 | 0.29 | 0.19 | 0.64 |
| | (1,732) | (1,725) | (521) | (521) | (521) | (1,692) | (1,692) | (1,588) | (1,732) | (1,732) | (1,675) |
| **Total** | **6.04** | **0.76** | **0.43** | **0.47** | **0.85** | **0.11** | **0.04** | **0.25** | **0.22** | **0.12** | **0.46** |
| | **(4,053)** | **(3,983)** | **(1,512)** | **(1,512)** | **(1,512)** | **(3,814)** | **(3,814)** | **(3,584)** | **(3,989)** | **(3,989)** | **(3,918)** |

Notes: Numbers in cells represent mean values for mean years of schooling ($\bar{y}$), gender parity index ($gpi$), Gini coefficient ($g$), Theil index ($t$), coefficient of variation ($cov$), between-group Gini ($gg$), between-group Theil ($gt$), and between-group coefficient of variation ($gcov$). Numbers in parentheses represent the number of country-year observations in each cell. Top column headings denote group-level dimensions.

## Table 2. Treatment and Control Group Sample Balance, Before and After Matching

| | Unmatched | | | Matched | | |
|---|---|---|---|---|---|---|
| | Conflict | No Conflict | Difference | Conflict | No Conflict | Difference |
| Gini - ethnic/rel. groups | 0.127 | 0.108 | 0.019* | 0.111 | 0.099 | 0.012* |
| | (0.003) | (0.002) | (0.003) | (0.003) | (0.002) | (0.004) |
| Gini - wealth decile groups | 0.240 | 0.223 | 0.017* | 0.214 | 0.207 | 0.007 |
| | (0.005) | (0.003) | (0.005) | (0.005) | (0.003) | (0.006) |
| Gini - national | 0.454 | 0.420 | 0.034* | 0.398 | 0.422 | -0.024 |
| | (0.011) | (0.006) | (0.013) | (0.013) | (0.007) | (0.015) |
| ln(Oil production per capita) | 2.136 | 2.263 | -0.127 | 3.497 | 3.099 | 0.397 |
| | (0.101) | (0.061) | (0.118) | (0.213) | (0.115) | (0.242) |
| ln(Real GDP per capita) | 7.545 | 7.712 | -0.167* | 8.132 | 8.115 | 0.017 |
| | (0.037) | (0.017) | (0.041) | (0.078) | (0.039) | (0.087) |
| ln(Total population) | 9.615 | 8.662 | 0.953* | 10.184 | 10.309 | -0.125 |
| | (0.045) | (0.025) | (0.051) | (0.091) | (0.050) | (0.104) |
| Pct. age 15-24 years | 0.190 | 0.188 | 0.002* | 0.194 | 0.194 | 0.001 |
| | (0.001) | (0.000) | (0.001) | (0.001) | (0.001) | (0.001) |
| Democracy | 0.270 | 0.280 | -0.010 | 0.512 | 0.461 | 0.052 |
| | (0.017) | (0.009) | (0.019) | (0.035) | (0.017) | (0.039) |
| Anocracy | 0.348 | 0.299 | 0.048* | 0.294 | 0.318 | -0.025 |
| | (0.018) | (0.009) | -0.020 | (0.032) | (0.016) | -0.036 |
| **Observations** | **584** | **2,282** | **2,866** | **579** | **2,282** | **2,861** |

Notes: All covariates listed in this table are lagged by 5 years. Numbers in cells reflect simple mean values, while numbers under the heading "Difference" denote the difference between the mean for the conflict and no conflict groups ($\mu_c - \mu_{nc}$). Numbers in parentheses denote standard errors. Difference in means are tested for statistical significance using a simple t-test.
* denotes statistical significance at the 10% level.

## Table 3. Average Effect of Conflict on Attainment and Inequality

| | $ln(\bar{y}_{ct})$ | $gpi_{ct}$ | $g_{ct}$ | $gg_{ct}^{ethnic}$ | $gg_{ct}^{wealth}$ |
|---|---|---|---|---|---|
| *Mean Outcome - Treatment Group* | *5.078* | *0.696* | *0.454* | *0.127* | *0.240* |
| **DD - No Matching** | | | | | |
| Conflict | -0.013 | -0.023* | 0.008 | 0.003 | 0.006 |
| | (0.019) | (0.013) | (0.006) | (0.004) | (0.006) |
| Observations | 2,866 | 2,866 | 1,371 | 2,517 | 2,866 |
| **DD Matching - Kernel** | | | | | |
| Conflict | -0.006 | -0.033** | 0.007** | 0.004 | 0.011* |
| | (0.013) | (0.013) | (0.003) | (0.004) | (0.006) |
| Observations | 2,861 | 2,861 | 1,042 | 2,507 | 2,861 |
| **DD Matching - IPW** | | | | | |
| Conflict | -0.015 | -0.035*** | 0.009* | 0.005 | 0.013** |
| | (0.015) | (0.013) | (0.005) | (0.004) | (0.006) |
| Observations | 2,866 | 2,866 | 1,121 | 2,512 | 2,866 |

Notes: Under both propensity score matching algorithms, the analytic sample is restricted to be within the common support. Matching weights are applied to estimate the average treatment effect on the treated (ATT). Numbers in parentheses denote standard errors clustered at the country-treatment level. All regression specifications include the full list of covariates that are lagged by five years, quadratic peace years, country fixed effects, and year fixed effects. Asterisks denote statistical significance as follows.
*** p<0.01, ** p<0.05, and * p< 0.10

**Table 4. Average Effect of Conflict on Attainment and Inequality, by Type**

| | $ln(\bar{y}_{ct})$ | $gpi_{ct}$ | $g_{ct}$ | $gg_{ct}^{ethnic}$ | $gg_{ct}^{wealth}$ |
|---|---|---|---|---|---|
| *Mean Outcome - Treatment Group* | *5.078* | *0.696* | *0.454* | *0.127* | *0.240* |
| **DD - No Matching** | | | | | |
| Non-Ethnic Conflict | -0.003 | -0.010 | 0.004 | 0.005 | 0.000 |
| | (0.023) | (0.013) | (0.008) | (0.005) | (0.006) |
| Ethnic Conflict | -0.025 | -0.040* | 0.013 | 0.000 | 0.014 |
| | (0.027) | (0.023) | (0.012) | (0.006) | (0.011) |
| Observations | 2,866 | 2,866 | 1,371 | 2,517 | 2,866 |
| **DD Matching - Kernel** | | | | | |
| Non-Ethnic Conflict | 0.006 | -0.017 | 0.004 | 0.009* | 0.002 |
| | (0.016) | (0.013) | (0.005) | (0.005) | (0.005) |
| Ethnic Conflict | -0.022 | -0.053** | 0.012 | 0.000 | 0.022** |
| | (0.018) | (0.022) | (0.010) | (0.005) | (0.010) |
| Observations | 2,861 | 2,861 | 1,042 | 2,507 | 2,861 |
| **DD Matching - IPW** | | | | | |
| Non-Ethnic Conflict | -0.006 | -0.019 | 0.005 | 0.009** | 0.005 |
| | (0.019) | (0.012) | (0.006) | (0.005) | (0.005) |
| Ethnic Conflict | -0.027 | -0.055** | 0.017 | -0.001 | 0.023** |
| | (0.019) | (0.023) | (0.012) | (0.005) | (0.010) |
| Observations | 2,861 | 2,861 | 1,042 | 2,507 | 2,861 |

Notes: Under both propensity score matching algorithms, the analytic sample is restricted to be within the common support. Matching weights are applied to estimate the average treatment effect on the treated (ATT). Numbers in parentheses denote standard errors clustered at the country-treatment level. All regression specifications include the full list of covariates that are lagged by five years, quadratic peace years, country fixed effects, and year fixed effects. Asterisks denote statistical significance as follows.
*** p<0.01, ** p<0.05, and * p< 0.10

**Table 5a. Average Effect of Conflict, by Type and Duration (No Matching)**

| | $ln(\bar{y}_{ct})$ | $gpi_{ct}$ | $g_{ct}$ | $gg_{ct}^{ethnic}$ | $gg_{ct}^{wealth}$ |
|---|---|---|---|---|---|
| *Mean Outcome - Treatment Group* | *5.078* | *0.696* | *0.454* | *0.127* | *0.240* |
| **DD - No Matching** | | | | | |
| ***Non-Ethnic Conflict:*** | | | | | |
| 1-5 Years | 0.019 | 0.006 | -0.001 | -0.001 | -0.006 |
| | (0.022) | (0.015) | (0.011) | (0.006) | (0.008) |
| 6-10 Years | 0.000 | -0.025 | -0.002 | 0.012 | 0.006 |
| | (0.039) | (0.021) | (0.017) | (0.010) | (0.011) |
| 11-15 Years | -0.007 | 0.011 | 0.012 | 0.002 | -0.018 |
| | (0.054) | (0.031) | (0.011) | (0.010) | (0.012) |
| 16+ Years | -0.014 | -0.004 | 0.003 | 0.021 | -0.004 |
| | (0.043) | (0.026) | (0.012) | (0.014) | (0.016) |
| ***Ethnic Conflict:*** | | | | | |
| 1-5 Years | 0.014 | -0.004 | 0.012 | -0.007 | -0.003 |
| | (0.028) | (0.017) | (0.012) | (0.006) | (0.009) |
| 6-10 Years | -0.005 | -0.071* | 0.009 | 0.002 | 0.022 |
| | (0.041) | (0.038) | (0.010) | (0.008) | (0.018) |
| 11-15 Years | -0.030 | -0.103* | 0.001 | 0.004 | 0.037 |
| | (0.038) | (0.057) | (0.014) | (0.006) | (0.025) |
| 16+ Years | -0.181*** | -0.079 | 0.025* | 0.015 | 0.049* |
| | (0.066) | (0.062) | (0.015) | (0.015) | (0.025) |
| Observations | 2,866 | 2,866 | 1,371 | 2,517 | 2,866 |

**Table 5b. Average Effect of Conflict, by Type and Duration (Kernel Matching)**

| | $ln(\bar{y}_{ct})$ | $gpi_{ct}$ | $g_{ct}$ | $gg_{ct}^{ethnic}$ | $gg_{ct}^{wealth}$ |
|---|---|---|---|---|---|
| *Mean Outcome - Treatment Group* | *5.078* | *0.696* | *0.454* | *0.127* | *0.240* |
| **DD Matching - Kernel** | | | | | |
| ***Non-Ethnic Conflict:*** | | | | | |
| 1-5 Years | 0.029 | 0.012 | -0.007 | -0.001 | -0.007 |
| | (0.024) | (0.016) | (0.009) | (0.006) | (0.007) |
| 6-10 Years | 0.016 | -0.022 | 0.009 | 0.016* | 0.002 |
| | (0.020) | (0.017) | (0.012) | (0.009) | (0.009) |
| 11-15 Years | -0.002 | -0.006 | 0.019*** | 0.014 | -0.010 |
| | (0.043) | (0.027) | (0.007) | (0.009) | (0.010) |
| 16+ Years | 0.018 | -0.034 | 0.009 | 0.029** | 0.006 |
| | (0.043) | (0.025) | (0.008) | (0.012) | (0.015) |
| ***Ethnic Conflict:*** | | | | | |
| 1-5 Years | 0.021 | -0.009 | 0.009 | -0.010* | 0.004 |
| | (0.023) | (0.016) | (0.009) | (0.006) | (0.008) |
| 6-10 Years | -0.001 | -0.073** | 0.010 | 0.002 | 0.023* |
| | (0.030) | (0.029) | (0.007) | (0.007) | (0.014) |
| 11-15 Years | -0.022 | -0.102** | 0.012* | 0.007 | 0.037* |
| | (0.032) | (0.048) | (0.007) | (0.005) | (0.020) |
| 16+ Years | -0.182** | -0.095* | 0.022** | 0.013 | 0.053** |
| | (0.078) | (0.049) | (0.011) | (0.013) | (0.024) |
| Observations | 2,861 | 2,861 | 1,042 | 2,507 | 2,861 |

**Table 5c. Average Effect of Conflict, by Type and Duration (Propensity Score IPW)**

| | $ln(\bar{y}_{ct})$ | $gpi_{ct}$ | $g_{ct}$ | $gg_{ct}^{ethnic}$ | $gg_{ct}^{wealth}$ |
|---|---|---|---|---|---|
| *Mean Outcome - Treatment Group* | *5.078* | *0.696* | *0.454* | *0.127* | *0.240* |
| **DD Matching - IPW** | | | | | |
| ***Non-Ethnic Conflict:*** | | | | | |
| 1-5 Years | 0.028 | 0.012 | -0.008 | 0.000 | -0.008 |
| | (0.025) | (0.016) | (0.010) | (0.006) | (0.007) |
| 6-10 Years | 0.001 | -0.025 | -0.002 | 0.017** | 0.005 |
| | (0.026) | (0.016) | (0.014) | (0.008) | (0.009) |
| 11-15 Years | -0.007 | -0.008 | 0.003 | 0.013 | -0.009 |
| | (0.045) | (0.026) | (0.009) | (0.009) | (0.010) |
| 16+ Years | 0.012 | -0.033 | -0.011 | 0.027** | 0.006 |
| | (0.045) | (0.026) | (0.013) | (0.012) | (0.015) |
| ***Ethnic Conflict:*** | | | | | |
| 1-5 Years | 0.023 | -0.011 | 0.011 | -0.010* | 0.004 |
| | (0.023) | (0.017) | (0.009) | (0.006) | (0.008) |
| 6-10 Years | 0.002 | -0.074** | 0.001 | 0.002 | 0.023 |
| | (0.032) | (0.031) | (0.008) | (0.007) | (0.014) |
| 11-15 Years | -0.021 | -0.105** | -0.004 | 0.006 | 0.037* |
| | (0.033) | (0.049) | (0.012) | (0.006) | (0.020) |
| 16+ Years | -0.187** | -0.093* | 0.025 | 0.013 | 0.051** |
| | (0.077) | (0.052) | (0.018) | (0.014) | (0.024) |
| Observations | 2,861 | 2,861 | 1,042 | 2,507 | 2,861 |

Notes: Under both propensity score matching algorithms, the analytic sample is restricted to be within the common support. Matching weights are applied to estimate the average treatment effect on the treated (ATT). Numbers in parentheses denote standard errors clustered at the country-treatment level. All regression specifications include the full list of covariates that are lagged by five years, quadratic peace years, country fixed effects, and year fixed effects. In addition to the regular specification, we include quadratic post-treatment trends to assess lingering post-conflict effects. Asterisks denote statistical significance as follows.
*** p<0.01, ** p<0.05, and * p< 0.10

**Table 6. Average Effect of Conflict with Country Pre-Treatment Trends**

| | $ln(\bar{y}_{ct})$ | $gpi_{ct}$ | $g_{ct}$ | $gg_{ct}^{ethnic}$ | $gg_{ct}^{wealth}$ |
|---|---|---|---|---|---|
| *Mean Outcome - Treatment Group* | *5.078* | *0.696* | *0.454* | *0.127* | *0.240* |
| **DD - No Matching** | | | | | |
| Conflict | -0.020 | -0.017* | 0.007 | 0.000 | 0.008* |
| | (0.014) | (0.009) | (0.005) | (0.004) | (0.005) |
| Observations | 2,866 | 2,866 | 1,371 | 2,517 | 2,866 |
| **DD Matching - Kernel** | | | | | |
| Conflict | -0.011 | -0.027*** | 0.007*** | 0.004 | 0.012** |
| | (0.011) | (0.010) | (0.003) | (0.003) | (0.005) |
| Observations | 2,861 | 2,861 | 1,042 | 2,507 | 2,861 |
| **DD Matching - IPW** | | | | | |
| Conflict | -0.017 | -0.029*** | 0.007** | 0.004 | 0.014*** |
| | (0.013) | (0.011) | (0.003) | (0.004) | (0.005) |
| Observations | 2,866 | 2,866 | 1,121 | 2,512 | 2,866 |

Notes: Numbers in parentheses denote standard errors clustered at the country-treatment level. All regression specifications include the full list of covariates that are lagged by five years, quadratic peace years, country fixed effects, year fixed effects, and quadratic country peace years. Asterisks denote statistical significance as follows.
*** p<0.01, ** p<0.05, and * p< 0.10

## Table 7. Results from Falsification (Placebo) Test

| | $ln(\bar{y}_{ct})$ | $gpi_{ct}$ | $g_{ct}$ | $gg_{ct}^{ethnic}$ | $gg_{ct}^{wealth}$ |
|---|---|---|---|---|---|
| **False Incidence:** | | | | | |
| Placebo | 0.029 | 0.027 | 0.009 | -0.006 | -0.010 |
| | (0.044) | (0.027) | (0.007) | (0.023) | (0.012) |
| **Economic Activity:** | | | | | |
| ln(Real GDP per capita) | -0.001 | -0.007 | -0.011 | -0.056** | 0.022 |
| | (0.080) | (0.038) | (0.017) | (0.023) | (0.020) |
| ln(Oil production per capita) | -0.001 | -0.003 | -0.001 | -0.006 | -0.004 |
| | (0.011) | (0.009) | (0.003) | (0.006) | (0.003) |
| **Demographics:** | | | | | |
| ln(Total population) | 0.366* | -0.034 | -0.111* | -0.130*** | -0.073 |
| | (0.193) | (0.103) | (0.058) | (0.032) | (0.056) |
| Percent 15-24 years | -3.957** | -0.475 | -0.082 | -0.537 | 1.04*** |
| | (1.572) | (0.730) | (0.456) | (0.524) | (0.354) |
| **Political Structure:** | | | | | |
| Democracy | 0.069 | -0.039 | -0.02*** | 0.002 | -0.003 |
| | (0.043) | (0.033) | (0.004) | (0.008) | (0.016) |
| Anocracy | 0.040 | -0.048** | -0.013** | 0.013* | 0.004 |
| | (0.033) | (0.022) | (0.005) | (0.007) | (0.010) |
| **Quadratic Peace Years:** | | | | | |
| Peace years | -0.04*** | -0.013 | 0.014* | 0.012** | 0.006 |
| | (0.008) | (0.014) | (0.007) | (0.005) | (0.005) |
| Peace years squared | 0.000 | -0.000 | -0.000** | -0.000** | 0.000** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Constant | -1.201 | 0.782 | 1.452*** | 1.630*** | 0.522 |
| | (1.472) | (0.993) | (0.514) | (0.338) | (0.454) |
| Observations | 976 | 976 | 340 | 861 | 976 |

Notes: Numbers in parentheses denote standard errors clustered at the country-treatment level. All regression specifications include the full list of covariates that are lagged by five years, quadratic peace years, country fixed effects, year fixed effects, and quadratic country peace years. Asterisks denote statistical significance as follows.
*** p<0.01, ** p<0.05, and * p< 0.10

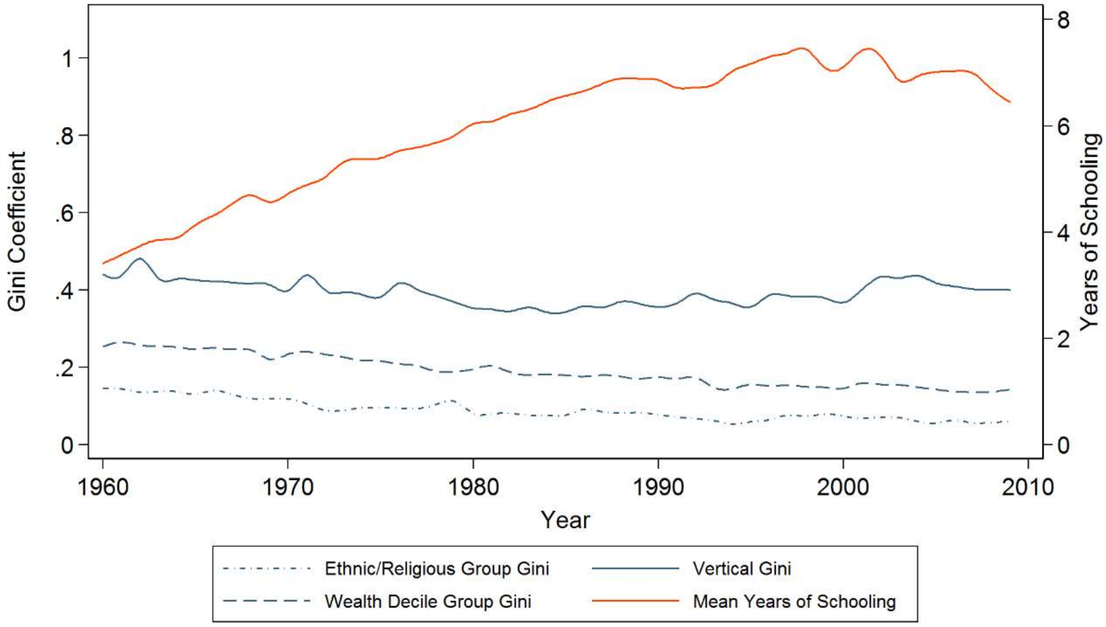
## Table 8. Average Effect of Conflict, under Alternate Measures of Inequality

| | $gap_{ct}$ | $t_{ct}$ | $gt_{ct}^{ethnic}$ | $gt_{ct}^{wealth}$ | $cov_{ct}$ | $gcov_{ct}^{ethnic}$ | $gcov_{ct}^{wealth}$ |
|---|---|---|---|---|---|---|---|
| **DD - No Matching** | | | | | | | |
| Conflict | 0.136 | 0.037** | 0.001 | -0.002 | 0.051** | 0.006 | 0.003 |
| | (0.135) | (0.018) | (0.003) | (0.007) | (0.025) | (0.009) | (0.014) |
| Observations | 2,866 | 1,371 | 2,517 | 2,866 | 1,371 | 2,517 | 2,866 |
| **DD Matching - Kernel** | | | | | | | |
| Conflict | 0.300** | 0.025*** | 0.001 | 0.000 | 0.027** | 0.011 | 0.013 |
| | (0.127) | (0.009) | (0.002) | (0.005) | (0.010) | (0.008) | (0.012) |
| Observations | 2,861 | 1,042 | 2,507 | 2,861 | 1,042 | 2,507 | 2,861 |
| **DD Matching - IPW** | | | | | | | |
| Conflict | 0.292** | 0.039*** | 0.001 | 0.003 | 0.046*** | 0.012 | 0.017 |
| | (0.128) | (0.014) | (0.002) | (0.005) | (0.016) | (0.009) | (0.012) |
| Observations | 2,861 | 1,042 | 2,507 | 2,861 | 1,042 | 2,507 | 2,861 |

Notes: Column headings refer to the gender gap in years of schooling ($gap_{ct}$), Theil index ($t_{ct}$), group Theil index ($gt_{ct}$), coefficient of variation ($cov_{ct}$), and group coefficient of variation ($gcov_{ct}$). Numbers in parentheses denote standard errors clustered at the country-treatment level. All regression specifications include the full list of covariates that are lagged by five years, quadratic peace years, country fixed effects, and year fixed effects. Asterisks denote statistical significance as follows.
*** p<0.01, ** p<0.05, and * p< 0.10

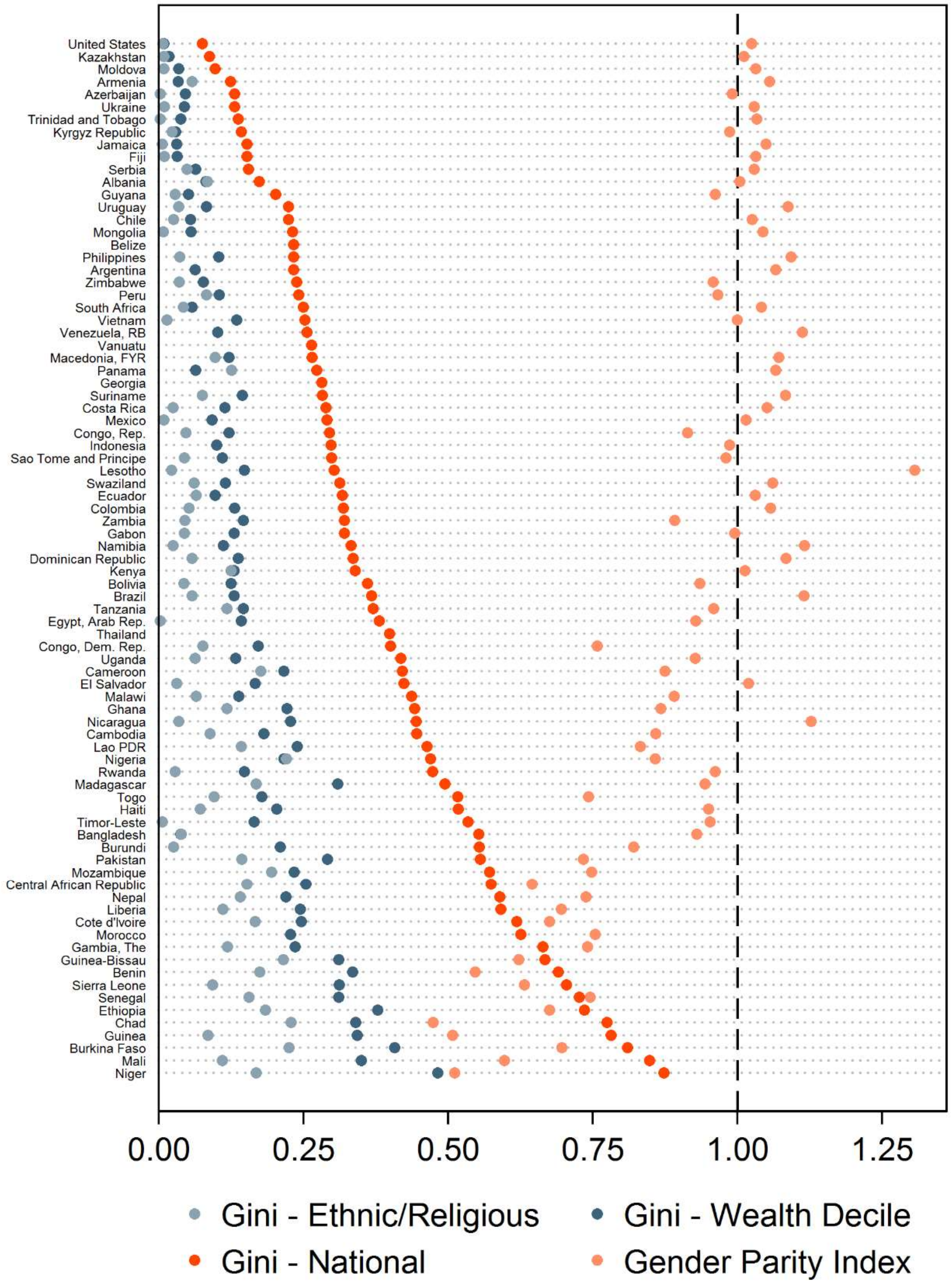
## Figures

### Figure 1. Trends in Education Attainment and Inequality



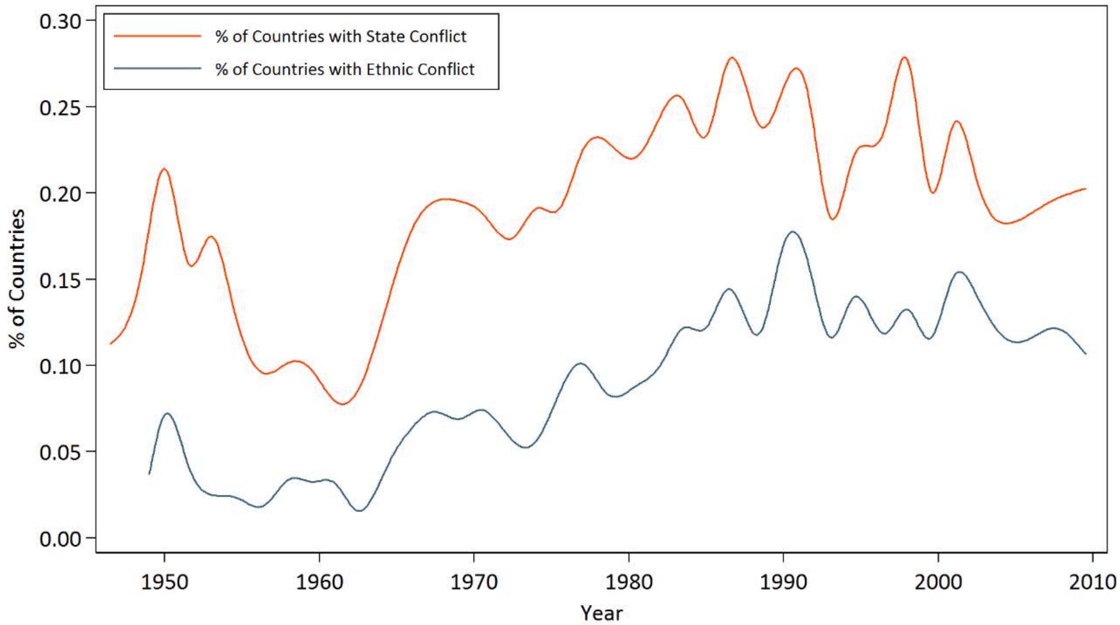

Source: Authors' calculations and EPDC, FHI 360 (2016)

**Figure 2. Cross-Country Trends in Education Inequality**



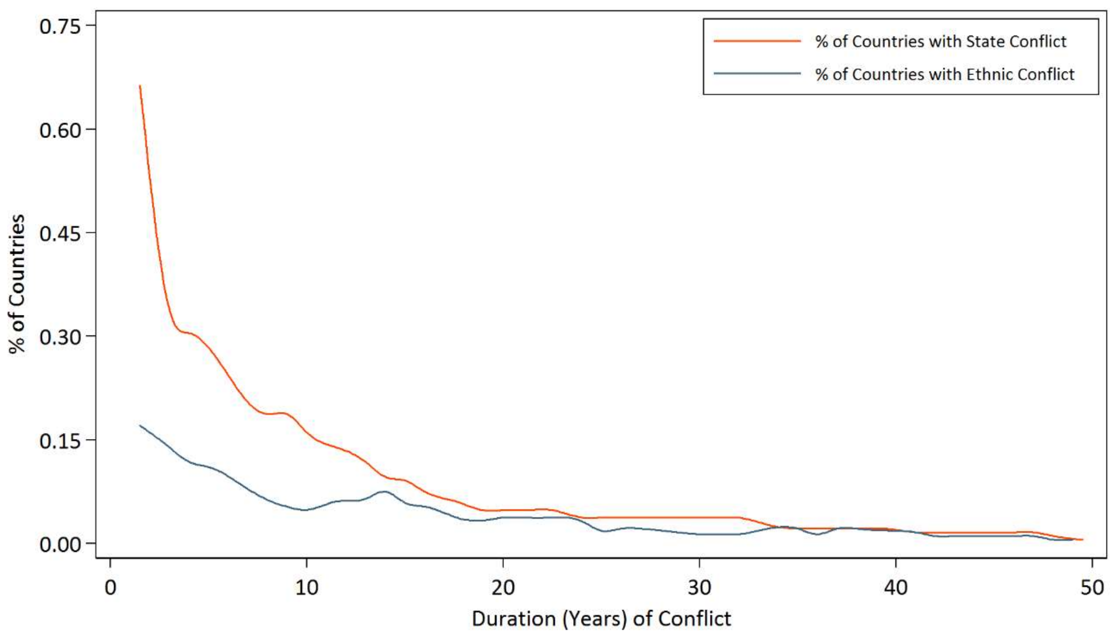

Source: Authors' calculations and EPDC, FHI 360 (2016)

**Figure 3. Proportion of Countries Experiencing State and Ethnic Conflict**



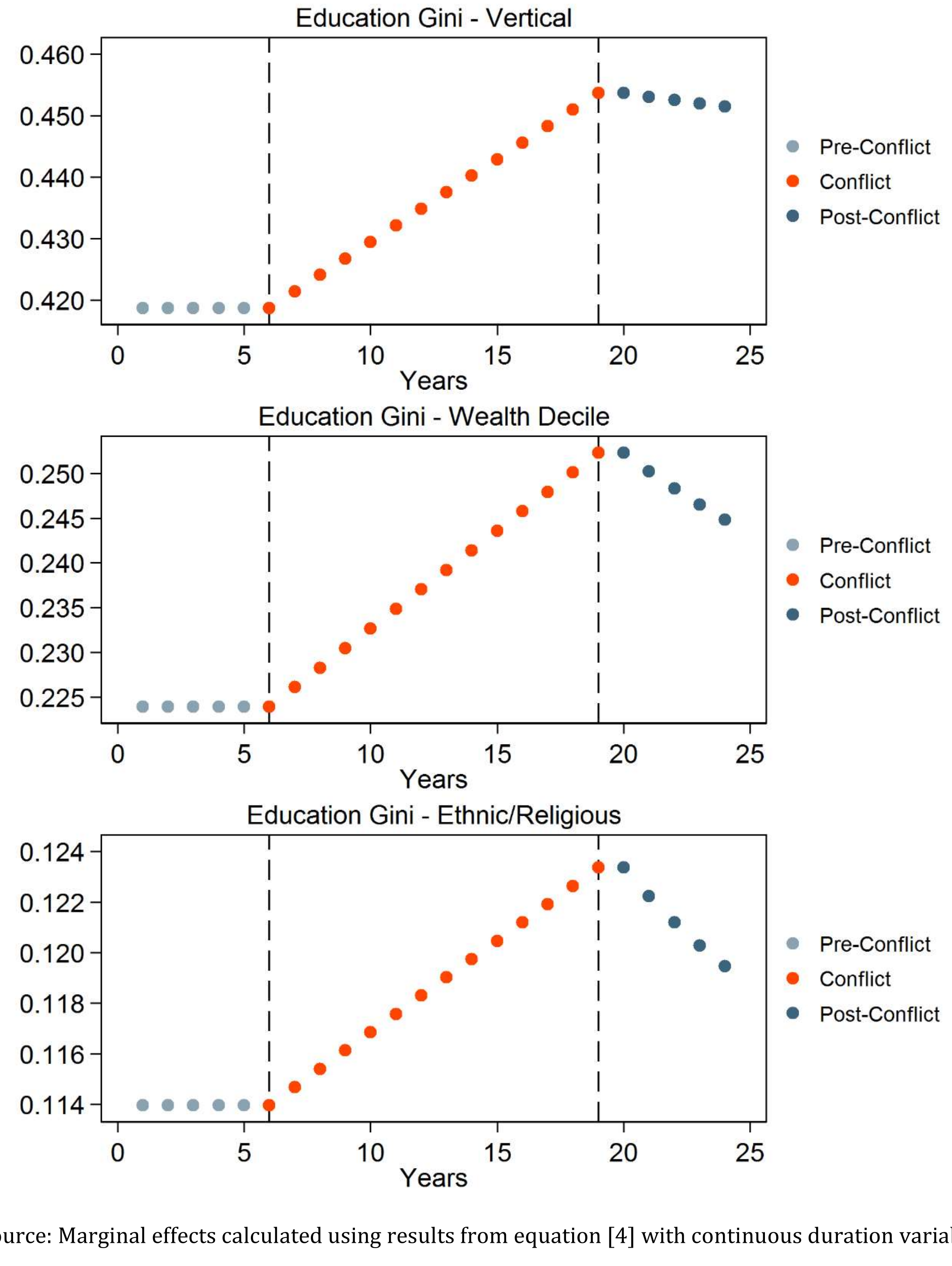

Source: UCDP/PRIO armed conflict database (2015)

**Figure 4. Distribution of State and Ethnic Conflict Duration**




Source: UCDP/PRIO armed conflict database (2015)

**Figure 5. Predicted Inequality Pre-, During, and Post-Ethnic Conflict**




Source: Marginal effects calculated using results from equation [4] with continuous duration variables

## Notes

[i] For the remainder of the paper, we define educational attainment as (mean) years of schooling completed.

[ii] While the bulk of evidence points to conflict's negative consequences on education, a few studies have found positive changes in education during conflict. For example, in a study of the conflict-affected Basque region, de Groot and Göksel (2011) find that educational levels for those from conflict-affected regions increase more than those in other regions of Spain. The authors suggest that very low levels of conflict – conflict in which the supply of education is uninterrupted – may create an incentive for individuals to improve educational qualifications so they can migrate and work in other Spanish regions.

[iii] See Lai and Thyne (2007) for cross-national evidence on reductions in educational expenditures during civil war.

[iv] Covariates are lagged by five years to mitigate potential simultaneity between the treatment and the covariates as well as the covariates and the outcome variables.

[v] See Lucas (1988) and Mankiw, Romer, and Weil (1992) for theoretical discussion of educational attainment and economic development and growth.

[vi] See Fearon (2003) for a detailed discussion of ethnic fractionalization and its definition.

[vii] For a discussion on the use and assumptions of difference-in-differences methods, refers to Ashenfelter (1978), Abadie (2005), Imbens and Wooldridge (2009), and Angrist and Pischke (2009).

[viii] Ethnic or religious identity groups comprising less than five percent of the total population were reclassified into an "other" category.

[ix] Inequality measurement in economics whether it concerns income, health, or educational inequality has been debated for more three decades. For reference see Cowell and Kuga (1981), Yaari (1988), Silber (1999), Cowell and Flachaire (2007), and Ferreira and Gignoux (2011) among many others.

[x] Value of years of schooling for females as a proportion of the value of years of schooling for males.

[xi] We are unable to construct a similarly sized sample for our national inequality measures because we are unable to create backwards extrapolations of individual-level data. As a result, we are constrained to only having national inequality data points between observed values, rather than projected values.

[xii] Wealth deciles are determined by computing a wealth index, which is comprised of certain household possessions and divided into 10 groups of equal size, for each household in each country-year observation.

[xiii] Simple correlations between the different measure show a high level of association between .68 and .94.

[xiv] The correlation coefficient between the vertical Gini and the gender parity index is -.84.

[xv] Definitions and estimates of the incidences of conflict and civil conflicts are based on the Uppsala Conflict Data Program (UCDP) and the Peace Research Institute Oslo (PRIO) armed conflict database that were created by Gleditsch et al. (2002) and updated by Pettersson and Wallensteen (2015). Wimmer, Cederman, and Min (2009) identify conflicts as ethnic or non-ethnic.

[xvi] Refer to Tables A1 and A2 in the appendix for a summary of the data sources and political regime variable construction.

xvii See Besley and Burgess (2004), Wolfers (2006), and Angrist and Pischke (2009) for a discussion of DD identification and including country/state trends to test for common pre-treatment trends.