The Effect of Conflict on Education: Evidence from Sierra Leone

Hoenig, Tillman

LSE

2018

The Effect of Conflict on Education: Evidence from Sierra Leone

Tillman Hönig*

October 2018

Abstract

This study investigates the long-run effects of the civil war in Sierra Leone on education. Similar to the literature in other contexts, I exploit cohort and conflict variation within Sierra Leone from micro-level data in the spirit of a difference-in-difference (DID) design. Since schooling typically takes place at a particular age for people, using school age cohorts in such a way is a very natural method to estimate a plausibly causal effect of higher conflict intensity on the education of those affected by it. I find that a one standard deviation increase in conflict intensity led to a 0.3-0.5 year reduction in the amount of schooling for school aged children during the war by 2011, that is, ten years after the civil war ended. Some suggestive evidence on channels indicates that a major driver of this effect may be supply side factors.

JEL classification: D74, I25, O12

*London School of Economics & Political Science, Email: t.hoenig@lse.ac.uk

The author would like to acknowledge and thank Statistics Sierra Leone (www.statistics.sl) for the provision of the underlying data.
I would also like to thank Tim Besley for helpful comments and discussions.

1. Introduction

Conflict has undoubtedly extremely destructive consequences on various life outcomes for the people affected by it, at least in the short run. In particular when we consider per capita measures of economic performance which may arguably reflect living standards, many studies demonstrate this relationship. Earlier cross-country macro studies (including Alesina & Perotti (1996); Barro (1991); Collier (1999)) show a clear negative link between conflict and economic performance and direct cost estimations (Ali (2013); Arunatilake et al. (2001) as well as more recent micro-empricial studies (such as Lopez & Wodon (2005); Serneels & Verpoorten (2015)) suggest that there is a negative causal effect as well. To what extent such effects are persistent in the long run or there is convergence in line with the logic of the Solow model is less clear though. Studies carrying out research to this end do not deliver conclusive evidence. While some estimates suggest convergence, others find the opposite. In particular, some direct evidence on GDP (Chen et al., 2008; Gates et al., 2012; Miguel & Roland, 2011) and city size (Brakman et al., 2004; Davis & Weinstein, 2002) hints at convergence. By contrast, other direct evidence on GDP (Akbulut-Yuksel, 2014; Galdo, 2013; Islam et al., 2016) and a range of studies on education and health outcomes[1] that should affect economic performance according to standard theory suggests that there is no convergence. I contribute to literature on education outcomes in the long run with evidence from Sierra Leone which suffered an atrocious civil war between 1991 and 2001.

Beyond having likely implications for living standards in the long run, educational attainment may also be an outcome of interest in and of itself. In theory, several channels are conceivable how conflict affects education. On the demand side, reduced financial resources may lead to lower (consumption of) education through an income channel.[2] In addition, in particular in settings where schools are distant, the cost of education increases through the destruction of infrastructure and increased uncertainty and insecurity when leaving the house to attend public institutions. Displacement may be another leading reason for loss of education. Destinations may provide less or potentially even more access to schools, for example in displacement camps, and the cost of displacement has implications for the household's financial resources. On the supply side, the destruction of

[1] Focussing on education, the object of study in this investigation, civil war, as well as long-run outcomes at least 10 years after the end of war, these include Akresh & De Walque (2011); Justino & Verwimp (2013); La Mattina (2017); Leon (2012); Saing et al. (2017).

[2] This income channel may be particularly important for very poor households when pushed close to or below the subsistence level and a substitution between education and income-generating work may realise. Another involuntary education-labour substitution may arise from child soldiering.

schools and killings of teachers clearly reduces education supply during and after conflict.

The supply side channels have clear policy implications for post-war reconstruction work. If addressed, this should reduce the impact that conflict has on educational attainment. In this sense, Sierra Leone is an interesting setting to look at because the government implemented school construction programmes soon after the war (e.g. the SABABU education project). I evaluate the impact of the conflict on educational attainment ten years after the end of the war, using both cohort and geographic conflict variation in the spirit of a difference-in-difference (DID) design. Unlike most of the studies analysing this link using similar methods in different contexts, I consider not only primary school education, but also secondary education – and indeed find significant results for secondary school aged children. To be precise, I consider four potentially affected cohorts: those born during the war and pre-school, primary school and secondary school aged students at the onset of war. To deal with potential concerns around the key identification assumption of parallel trends in education across cohorts between chiefdoms[3] that are more or less severely affected by conflict, I perform two robustness checks. First, I investigate whether parallel trends hold between older cohorts and second, I present some analysis pertaining to displacement and migration that make strong biasses through spillover effects less likely. This also presents a contribution to the existing papers, most of which do not seem to consider potential spillover effects in their setting which may be very likely in light of non-negligible displacement that conflict typically causes.

I find that a one standard deviation in conflict intensity reduces the educational attainment of pre-school, primary school and secondary school aged children in Sierra Leone by between 0.3 and 0.5 years. This translates into a reduction in educational attainment by between one and almost two years of schooling when moving someone from one of the chiefdoms least affected by conflict to one of the most highly affected chiefdoms. Such a large effect tends to be rather among the largest effects that other studies find in long-run studies in different countries. The direction and size of the effect is confirmed in two additional pieces of analysis. The first one exploits the conflict measure at the household as opposed to the chiefdom level. The second one is a siblings comparison. Within chiefdoms or households, children at school age are essentially compared to their older siblings who were beyond school age.

The paper then proceeds to investigate two potential channels of the sizeable effect:

[3]Chiefdoms are the third-level units of administration in Sierra Leone, following districts and the national level. There are 161 chiefdoms in my data, including the chiefdoms on the Western peninsula where the capital Freetown lies.

Displacement and asset loss reflecting reduced material resources of the household. Incorporating data on these variables in the main analysis does not mediate the conflict effect significantly and does not even have a significant effect itself. Therefore, it seems that other demand side factors such as destroyed infrastructure or uncertainty on the way to school or particularly supply side factors may be a main driving force of how conflict affects the education of children at school age during the war.

The following section will explore the context of the Sierra Leonean war in some more detail while section 3 discusses the data used in this study. I explain the identification strategy and empirical analyses performed in section 4, present the results in section 5 and conclude in section 6.

2. The Civil War in Sierra Leone

Sierra Leone suffered a civil war between 1991 and 2002 that caused some 70,000 casualties, displacement of over half the population and left many people injured, maimed and raped (UNDP, 2006). It started as an insurgency by the Revolutionary United Front (RUF) in 1991 entering the country from Liberia in the south. The RUF was a small rebel group at the onset of war with the political goal of overthrowing the ruling one-party regime, led by the All People's Congress (APC) party under Joseph Saidu Momoh (Richards, 1996). Their insurgency was supported by the National Patriotic Front for Liberia (NPFL), involved in the ongoing Liberian civil war. The RUF remained mainly active in the southern parts of Sierra Leone between 1991 and 1995 and recruited young people who were discontent with their lack of access to education and opportunities. A mixture of this low opportunity cost and coercion under threats led to many young people joining the RUF movement.

The Sierra Leonean Army (SLA) were the government's armed forces that acted as main opposing belligerent facing the RUF in the beginning of the war. Poor pay and conditions led to a military coup by a group of officers within the army under Captain Strasser who became the leader of the newly established military government called National Provisional Ruling Council (NPRC) in 1992. With a larger and better supplied army, the RUF was successfully pushed back to the Gola forests at the very border region with Liberia by the end of 1993. With young fighters trained in bush camps, however, the RUF launched a series of raids in a new offensive that was characterised by a large degree of violence against civilians in the form of looting, killing, maiming and raping. As a response

and out of an increasing distrust of the SLA which increasingly engaged in such violence as well, many communities organised their own fighting groups (usually based on traditional hunting groups and embedded in this culture), later collectively known as Civil Defence Forces (CDF). By 1995, the RUF had managed to make progress westwards heading towards the capital Freetown. In 1996, elections followed another coup from within the army and the newly elected President Kabbah started peace talks with the RUF. At the same time, the CDF started getting centrally co-ordinated and the leader of the Kamajor group, the largest hunter group, was made Deputy Defence Minister. In late 1996, the Abidjan Peace Accord was negotiated, but fighting resumed quickly. An ensuing coup in 1997 led to the establishment of the Armed Forces Revolutionary Council (AFRC) which invited the RUF to share with them. As a result, the Nigerian armed forces increased their support in the ECOWAS[4] Monitoring Group (ECOMOG), eventually driving the AFRC from power and reinstating President Kabbah in 1998. RUF/AFRC forces were forced out of Freetown and – through operations called "Operation Pay Yourself" and "Operation No Living Thing" – brought violence against civilians to unprecedented extents. Discussions between the government and RUF were revived in 1999 and resulted in the signing of the Lome Peace Agreement in July of the same year. A Disarmament, Demobilisation and Reintegration (DDR) programme was started. However, following abductions of UN peacekeeping forces and the imprisonment of RUF leaders, violence broke out again. Following a large deployment of UN peacekeepers and the intervention of UK armed forces, hostilities eventually became more sporadic and an agreement to reactivate the ceasefire was signed in Abuja in November 2000. Disarmament and demobilisation continued throughout 2001 before peace was finally declared on 18 January 2002 (Fithen & Richards, 2005; NPWJ, 2004).

As Richards (1996) argues, the element of political grievances played a great role as a cause of the civil war. In particular young people felt discontent with a patrimonial system in which a small group of patrons rules and decides on the allocation of opportunities and transfers arbitrarily. They felt disenfranchised and robbed of education and opportunities. The RUF's ideological roots lied in an idea of egalitarianism which initially helped in recruiting disenfranchised youth – however, as knowledge of the atrocities committed by the group spread, recruitment by capture became more necessary and common.

One of the atrocious features of the Sierra Leonean civil war was the extreme degree of violence against civilians, in particular all the community looting operations as well

[4]Economic Community of West African States

as the raping, killing and maiming that characterised the war. These acts of violence were not only committed by the RUF, but also by the SLA throughout the war, often by so-called "Sobels" who were soldiers by day and rebels by night, taking on an identity under which it was more legitimate and less consequential to engage in these activities. Such opportunistic behaviour and in particular the role switching demonstrates that there was an element of "greed" to the civil war that also became increasingly prevalent in the illicit mining of smuggling of diamonds. The diamond wealth resulting from these activites helped funding the war and provided incentives to prolong it (Keen, 2005; Richards, 2004).

Another interesting feature is the lack of ethnic or religious divisions as a key driver of war, as Bellows & Miguel (2009) point out. No ethnic group seemed to be disproportionately victimised and there seems to be no evidence that violence against a particular civil community was more pronounced if the community and the fighting group have largely differing ethnicities.

3. Data

This study's main source of data is the Sierra Leonean Integrated Household Survey (IHS) 2011 which is a general representative individual-level survey. I use questions on accomplished grades and degrees in school to construct years of schooling as a measure of education. For control variables, I also take an individual's sex, sector (urban vs. rural) as well as the household head's education, age and sex from this dataset.

Regarding conflict data, the survey contains a section with a number of questions on the impact of conflict on individuals and households that I make use of. As a main conflict measure, I follow Bellows & Miguel (2009) in constructing a victimisation index. This index is the share of "yes" answers to eight binary questions in the survey that cover how households were affected by the war along the following dimensions: (1) whether the household lost property or assets, (2) whether the house was burnt, (3) whether household members were killed, (4) whether relatives were killed, (5) whether household members lost limbs, (6) whether household members were molested or raped, (7) whether household members were displaced, (8) whether the war had any other effect on the household.[5] Given that the extreme degree of violence against civilians was a feature of the Sierra Leonean civil war, a victimisation index seems to be a sensible measure of the

[5]In fact, Bellows & Miguel (2009) use very similar questions in a survey carried out by the Institutional Reform and Capacity Building Project (IRCBP) to construct their victimisation index in the same way.

intensity of conflict.

Lastly, the survey contains some questions on asset loss, displacement, return and migration that I can employ for an analysis of the mechanisms and robustness checks. In particular, some of the questions allow me to track whether someone has been displaced during the war, has returned since then (i.e. within ten years after the end of the war) and if they ever lived in a different place.

4. Empirical Design

My empirical strategy exploits both geographical conflict variation and birth cohort variation in the spirit of a difference-in-difference design. Since schooling typically takes place at a particular age for children, the civil war will almost exclusively only affect the education of those who are young enough to be at school age during the war. Framing my empirical approach in terms of treatment and control groups, I therefore define four treatment cohorts: *T1. Born during the war* – People who were born between 1991 and 2001; *T2. Young children at the onset of war* – those born between 1986 and 1990; *T3. Primary school aged children at the onset of war* – those born between 1979 and 1985; *T4. Secondary school aged children at the onset of war* – those born between 1973 and 1978.[6] I further define two control groups: *C1. 19-25 year olds at the onset of war* – those born between 1966 and 1972; *C2. 26-30 year olds at the onset of war* – those born between 1961 and 1966. For most of my empirical analysis, these two cohorts will serve as a joint control group, representing young people who are just beyond school age when the civil war breaks out. However, I will make use of the distinct definitions of *C1* and *C2* for a placebo analysis in which I treat the former group as a placebo treatment group and the latter group as a control group to lend support to my identification assumption.

As for the geographical variation of conflict, I use the victimisation index that is defined at the chiefdom level.

[6]The oldest people to be considered part of the treatment group, that is, affected in their education by the civil war are aged 18 at the onset of war. Of course, there were some people who were older and still underwent schooling when the war broke out. However, this group is first of all small. The share of tertiary education students in my 2011 data is 2.2% (which is probably larger than the same share 20 years earlier when the war broke out). Out of all the people who report being above the age of 18 and still currently attending school (again, in 2011), a significant number (28%) report being in junior secondary school (the equivalent of middle school in many countries) which seems too implausible to be an accurate reflection of their education situation rather than a simple measurement error. This would leave less than 10% of those attending school who are actually above the age of 18. Secondly, people in this situation are likely to be about to finish their schooling and not that heavily affected in their education in the first few years of the war. Thirdly, including the 18-25 year olds at the onset of war as a treatment group turns out to just make my results for younger cohorts stronger. Therefore, if anything, my specification of treatment cohorts may result in a lower bound of the actual effect of civil war on education.

4.1. Main Specification

Using both conflict variation at the chiefdom level and cohort variation and controlling for the relevant fixed effects as well as imposing a linear district-level trend, I estimate the following regression equation:

$$yrsch_{ikt} = \alpha_k + \beta_t + (\gamma_d \times t) + \delta(cohort_c \times conflict_k) + \mu X_{ikt} + \epsilon_{ikt} \quad (1)$$

The outcome variable is years of schooling for individual i in chiefdom k (located in district d), born in year t (and thus belonging to cohort c). The two relevant fixed effects are chiefdom fixed effects α_k which capture all shocks common to everyone in the same chiefdom[7] and year-of-birth fixed effects β_t that contain fixed characteristics common to all people born in a given year. Beyond these fixed effects, I am imposing a linear trend in education development across age that is specific to districts, the chiefdoms' next higher level of administration.[8] The vector X_{ikt} contains some potentially relevant control variables, including an individual's sex and sector (urban or rural) as well as the household head's education, age and sex. The effect of interest is captured by δ which, I argue, reflects the causal impact of increased conflict hitting a particular cohort on the cohort's educational attainment ten years after the war. The essential identification assumption is that the change in education between school aged children and older cohorts in chiefdoms experiencing low conflict intensity is a valid counterfactual for what would have happened to education between the cohorts in high-conflict chiefdoms in the absence of conflict (conditional on the control variables and beyond a linear trend at the district level).

Two endogeneity concerns immediately spring to mind when considering the relationship between conflict and education in this setting. First, there is a likely selection of chiefdoms for violent activity. However, since my setting exploits both cohort and conflict variation, controlling for chiefdom fixed effects and a district-level linear trend, such a selection would need to happen on the cohort *trend* of education within the affected chiefdoms and to go beyond a simple district-level linear trend. This seems considerably less plausible. Instead of only relying on a plausibility assessment, however, I will also address this point in a placebo analysis. The placebo analysis considers the education trend between two cohorts that are both beyond school age and verifies that this trend is

[7] Note in particular that these chiefdom fixed effects would control for diamond mining activity before the war which may be a relevant factor for the choice of fighting.
[8] There are 14 districts in Sierra Leone.

not systematically different for chiefdoms that are more heavily affected by conflict.

A second concern are spillover effects. It is quite plausible to believe that conflict hitting a particular chiefdom not only affects the educational attainment of young people in the same chiefdom, but also in other chiefdoms through various channels. The most prominent channel is arguably displacement. Movement of people in times of conflict means that the social and economic situation changes in the destinations which poses a violation of the assumption that low-conflict chiefdoms are unaffected by conflict beyond their own (low-intensity) experience of conflict.[9] I will deal with this type of concern in robustness checks exploiting data on migration and displacement.

4.2. Additional Analyses

Further to these robustness checks, I run two additional specifications in order to provide some credibility for the results I find. The first one is an analysis in which I employ household-level measures of conflict instead of chiefdom-level averages. For this specification, the victimisation index is defined at the household level in the following regression:

$$yrsch_{ikt} = \alpha_k + \beta_t + (\gamma_d \times t) + \lambda conflict_h + \delta(cohort_c \times conflict_h) + \mu X_{ikt} + u_{ikt} \quad (2)$$

This regression contains two modifications relative to the main specification. The victimisation index is defined at the household level h and included in the regression as a level effect $conflict_h$ and in the interaction term $(cohort_c \times conflict_h)$. The remaining variables are the same as above. Essentially, this means that instead of comparing individuals across chiefdoms that are differently affected by conflict, we now compare individuals across households with different experiences of conflict. While this specification delivers similar results to the main specification, spillover effects are likely to be much more pronounced here. Any spillover across households within the same chiefdom puts the causal identification of this specification at risk. Such spillovers are considerably more likely than cross-chiefdom spillovers that would pose a problem for the main specification. Therefore, this analysis shall serve as supplemental evidence only with the main specification remaining the preferred one.

The second additional piece of analysis I perform to identify the effect of interest is a siblings comparison. Within families, I identify sets of siblings in the data where at least

[9] In Rubin's causal framework, this would be a violation of the Stable Unit Treatment Value Assumption (SUTVA).

one of them was at school age (below the age of 18), at least one of them was beyond school age and who are no more than ten years apart from each other in age. For these siblings, I run the following regressions:

$$yrsch_{ikt} = \alpha_{h/k} + \beta_t + \delta SchoolAge_{ikt} + \mu X_{ikt} + v_{ikt} \tag{3}$$

The treatment effect in this regression is the variable $SchoolAge_{ikt}$ indicating whether the person was at school age during the war. I run two different versions of this, a within-household comparison (with household fixed effects α_h) and a within-chiefdom comparison (with chiefdom fixed effects α_k). The remaining variables are the same as above. While such a comparison may get rid of some selection problems by the comparisons of siblings within the same families who happened to be of different ages during the war, the main issue with this specification is a power issue. The number of siblings with the desired age variation within families in my data is only 276 which is a small sample size relative to the number of chiefdom or household fixed effects included in the regression. Again, this is a reason why this analysis shall only serve as supplemental evidence to the main specification.

Apart from these two additional specifications, I also provide some suggestive evidence of mechanisms of conflict. In particular, data on whether households were displaced during the war and on the extent of asset loss they experienced allows me to verify whether these factors are drivers of the effect of conflict on education. Asset loss is reflective of a reduction of material resources of households which should affect their demand for education. Displacement is another channel through which conflict may affect the accumulation of schooling. I test for these potential channels by including the respective variables and their interactions with conflict in the main specification:

$$\begin{aligned} yrsch_{ikt} &= \alpha_k + \beta_t + (\gamma_d \times t) + \delta(cohort_c \times conflict_k) \\ &\quad + \zeta_d displc_h + \theta_d(cohort_c \times displc_h) + \mu X_{ikt} + w_{ikt} \qquad (4) \\ yrsch_{ikt} &= \alpha_k + \beta_t + (\gamma_d \times t) + \delta(cohort_c \times conflict_k) \\ &\quad + \zeta_a assets_h + \theta_a(cohort_c \times assets_h) + \mu X_{ikt} + z_{ikt} \qquad (5) \end{aligned}$$

The added term $displc_h$ is an indicator whether the household h (of individual i) was displaced during the war and the variable $assets_h$ is the number of item categories in which the household lost assets.

5. Results

5.1. Main Results

5.1.1. Main Specification

Table 1 demonstrates the results of the above regression equation with and without controls X. As a base case, the interaction term for the control cohorts (those born between 1961 and 1972) are left out – the four interaction terms for the four treatment cohorts are reported. The coefficients capture the loss in schooling that someone at school age experiences who lives in a chiefdom that was more affected by the conflict relative to someone else in the same cohort living in a chiefdom less affected by conflict. As can be soon, both the uncontrolled and controlled regression demonstrate a similar and sizeable effect of the war on the education of children at pre-school, primary school and secondary school age. In particular, for these three cohorts, a standard deviation increase in the intensity of conflict leads to a reduction in schooling attained by between 0.3 and 0.5 years. Focussing on primary school aged children as an example, moving a child from a chiefdom with one of the lowest intensities of conflict to a chiefdom at the top end of conflict intensity (consider, for example, the bottom and top 10% which are 3.28 standard deviations apart from each other) reduces their education by more than 1.5 years of schooling. This can certainly be considered a very sizeable effect and ranges among the largest effects in comparable studies.

5.1.2. Further Specifications

A first additional specification that I run is a household-level analysis. The results for this specification (equation 2) are provided in Table 2. Since the conflict variable is a victimisation index, taking values between 0 and 1, the coefficients are to be interpreted as the loss of schooling of a child at school age in a household with the strongest possible victimisation experience relative to living in a household that is not affected by the conflict at all. Indeed, all coefficients are negative and of great magnitude for pre-school, primary school and secondary school aged children. In particular the result for primary school aged children strongly confirms the main specification as the only significant result and with a very similar effect size.[10] The other results are indeed large as well, but very

[10]Recall that results in Table 5.1 are reported in standard errors which translates into a loss of schooling by 1.6 years if moving a child from one of the lowest conflict-intensity to one of the highest-conflict intensity chiefdoms.

imprecisely estimated and indeed insignificant. However, this specification is likely to suffer from spillover effects since households are compared. A household's victimisation is likely to have an impact on neighbouring households within the same community. The results should therefore be considered with caution and shall serve as confirming evidence to the more robust main specification at the chiefdom level. It is nevertheless reassuring to see the same direction of the effect and similar magnitudes.

In a similar way, a siblings comparison as a second additional piece of evidence confirms the main results in direction and magnitude. Table 3 reports the result of regression 3 where the attained level of schooling of children at school age during the war is compared to the education of their older siblings who were beyond school age during the war already. Specification (1) and (2) provide a within-chiefdom comparison of all sets of siblings where at least one was at school age and one beyond it at the onset of war and the age difference between siblings is not greater than ten years. Specification (3) and (4) provide the same analysis within households. The results to (2) and (4), the controlled regressions, are indeed quite similar and indicate that school aged children lost a year of education relative to their older siblings. Indeed, the results are insignificant, but the small sample size relative to the number of included fixed effects for chiefdoms or households has severe power implications driving large standard errors. Supplementing the main specification, however, these results again provide suggestive confirmation of large negative effects that the war had on the education of school aged children.

5.2. Robustness

5.2.1. Placebo Study

In order to lend support to the identification assumption of the main specification, I carry out a placebo analysis in which I compare the two control cohorts in their educational attainment which should both have been unaffected by the war since they were both beyond school age. This verifies that the trend in educational attainment across cohorts is the same (conditional on the controls and beyond a district-level linear trend) for people beyond school age and supports the idea that it would have continued to be the same for younger, school aged people in the absence of the war. To carry out this analysis, I run the regression equation (1) above again, restricting myself only to the control group (those born between 1961 and 1972) and allowing the younger control cohort *(C1)* to have a placebo treatment effect which is captured in the reported interaction term. Table 4

shows the result. There is no statistically significant differential effect of conflict on the educational attainment of cohort *(C1)* relative to the base cohort *(C2)*; the coefficient estimate is indeed even very close to zero for the second specification including the same control variables as main regression.

5.2.2. Spillover Effects

There are a number of ways in which low-conflict chiefdoms are affected by the civil war beyond their own direct experience of conflict – such effects arise from the fact that high conflict in some chiefdoms impacts on outcomes in low-conflict chiefdoms as well, in particular through temporary or permanent movement of people. For identification purposes, this may pose contamination problems, that is, the education outcome in low-conflict chiefdoms would no longer be an accurate reflection of what would have happened in chiefdoms with higher conflict intensity had they experienced less conflict (causal effect), but also captures the impact of conflict realisations in *other* (e.g. surrounding) areas on them (spillover effect). This section discusses different ways in which such spillover effects may occur and addresses these issues using data on migration and displacement from the household survey.[11]

A first spillover concern is that people who are more likely to be displaced from chiefdoms that are more heavily affected by the conflict and choose to stay in a different (low-conflict) chiefdom would be reported in the "false" chiefdom in 2011. If the influence of these people is large enough and their education outcomes systematically different from those who did not stay anywhere else, this may introduce bias. To provide some evidence against this possibility, note first that the share non-returnees is relatively low, at 7%.[12] Furthermore, restricting our sample to either only non-displaced people or only those who returned and re-running the main regression does not alter the results significantly. Table 5 shows the results. The coefficient on the interaction terms is not significantly different for any cohort in the two alternative specifications leaving out displaced or those who did not return.

[11] In particular, the answers to two yes-no questions are used, namely whether (i) someone was displaced as a result of the war and (ii) someone has returned from displacement since the end of the war.

[12] Note that the share of non-returnees is based on the household survey in Sierra Leone and can therefore by definition only capture internally displaced persons who did not return, but not refugees who fled abroad and chose not to return. There is not a lot of data on the number of refugees who returned to Sierra Leone over the years after the war, but two sources indicate that almost half a million refugees fled to the neighbouring countries Guinea and Liberia during the war that took the great majority of refugees. By 2004, 272,000 seem to have returned to Sierra Leone already and the number of Sierra Leonean refugees recorded by UNHCR was below 10,000 in 2009 (UNHCR, 2009; UNU-MERIT, 2017). Relative to the population of Sierra Leone, therefore, the share of refugees who fled abroad and have not returned by 2011 seems to be negligibly small.

Differential displacement intensity across chiefdoms that experience varying levels of conflict intensity may be another spillover concern. It can indeed be observed that conflict is unsurprisingly positively correlated with displacement rates at the chiefdom level. It is unclear, however, whether this is a positive or negative spillover that high-conflict chiefdoms impose on low-conflict chiefdoms. Several channels are theoretically conceivable. A net inflow of school aged children may mean local crowding-out effects, that is, the educational attainment of children in low-conflict chiefdoms is negatively affected through the spillover. However, a net inflow of older people who are pursuing their activities, including those in the education sector (e.g. teachers), may increase the education outcomes of children in low-conflict chiefdoms. What the net effect of these different channels is remains unclear. To provide some suggestive evidence that the net effect may actually be close to zero, I regress the displacement dummy on the conflict measure, cohort fixed effects and interaction terms between conflict and cohorts:

$$displ_{ikt} = conflict_k + cohort_c + \gamma(cohort_c \times conflict_k) + \xi_{ikt} \tag{6}$$

The idea is to gauge whether the net inflow rate of young people at school age (in treatment cohorts) is different from the net inflow rate of older people (in control cohorts). Table 6 shows the results. While the level effects capture that people are more likely to be displaced from chiefdoms experiencing more conflict and slightly less likely to be displaced at younger age (in treatment cohorts), the interaction terms are all fairly precise zero estimates. This means that the difference in the displacement rate across conflict intensities does not vary based on whether people are at school age or older, i.e. the net inflow rates for low-conflict chiefdoms are the same for varying age groups. Of course, this is no proof that opposing effects exactly cancel each other off, but it seems to suggest that there might not be very strong spillover effects in one direction or the other.

5.3. Evidence on Mechanisms

For a large reduced form effect, it is interesting and relevant for post-war policy making through what channel conflict affects education. The policy implications are naturally very different if the effect is mainly coming from demand side factors or supply side factors. Using data on displacement and asset loss that households experience, this section considers two particular channels. The first one is displacement. Including a variable indicating household displacement and its interaction with the cohort dummies in the

main regression allows us to see whether displacement mediates some of the conflict affects and has implications for the education of school aged children itself. Just like for the main conflict effect, the interaction term tells us to what extent the development of attained schooling between older cohorts and the given cohorts is different for displaced people relative to those who were not displaced. Table 7 shows the results in specification (2) (for this regression 4) in comparison with the main results reproduced in specification (1). As can clearly be seen, including the displacement terms does not change any of the original conflict effects significantly and displacement in and of itself does not seem to have an effect on the education of school aged children. It is noteworthy, however, that the coefficients are quite imprecisely estimated and positive for children younger than primary school age at the onset of war. The insignificant results may well mask some heterogeneity with some displaced people having no access to schools in their destinations and in particular very young children potentially gaining access to schools in displacement camps as they become old enough to go to school during the long war. These are of course speculations, but they serve as a word of caution to overhastily interpreting this as displacement having no implications for education.[13] In any case, it is clear that conflict still affects education in a major way other than through displacement.

Another channel that we can test for is a demand-side channel. Using the number of item categories in which household lost assets as additional element in the main regression in the same way as before, we can verify to what extent asset loss accounts for a reduction in attained schooling. Table 8 shows the results in specification (2) (for this regression 5) in comparison with the main results reproduced in specification (1). Similarly to the results for displacement, the conflict effects are not significantly changed, neither are any of the asset loss coefficients significant in and of itself. A caveat that should be kept in mind, however, is that the measure may only inaccurately and partly measure how conflict affects the financial resources of the household.

Taking these results together, it seems that other demand side factors such as infrastructure destruction or increased uncertainty on the way to school or particularly supply side factors such as the destruction of schools or killings of teachers play a large role in determining how conflict affects the educational attainment of children at school age during the war. This is in line with the evidence that Leon (2012) finds in Peru. Against this finding, the roll-out of post-war education projects in Sierra Leone targeting the supply

[13]Indeed, other studies focussing on this effect do not find conclusive evidence. See, for example, Fransen et al. (2018) and Fiala (2015).

side has likely been a good policy response. To what extent these policies have been successful (and in their absence the found conflict effects would have been even larger) remains unobserved in this study. However, the analysis does reveal that in spite of such approaches, the educational loss for school aged children during the war remains large, even ten years after the end of the war.

6. Conclusion

In this paper, I investigate the long-run impact of the Sierra Leonean civil war on the education of young people at school age during the conflict. In the spirit of a DID design, I exploit cohort and geographic conflict intensity variation to analyse this question. I find that pre-school, primary school and secondary school aged children suffer an educational loss of between 0.3 and 0.5 years of schooling per one standard deviation increase in conflict intensity. This means that children at that age who live in chiefdoms that were most heavily affected by conflict lose between one and almost two years of schooling relative to children of the same age living in chiefdoms that were least affected by the civil war. The result is robust to several robustness checks that lend support to the identification strategy and can be found in similar direction and extent in supplementary analyses.

Some evidence on channels suggests that displacement and asset loss on the demand side may not be major ways in which conflict affects educational outcomes. This lack of evidence suggests that how the supply of schools and teachers is affected by the war may be a very important force of how conflict affects education. The policy efforts for post-war reconstruction work in terms of education should therefore focus on supply side factors. The large effect found provide a solid justification for such policy efforts and further evidence that there may be long-run effects of conflict on life outcomes of those involved in light of the fact that education typically affects earnings and living standards.

References

Akbulut-Yuksel, M. (2014). Children of war: The long-run effects of large-scale physical destruction and warfare on children. *Journal of Human Resources*, 49(3), 634–662.

Akresh, R. & De Walque, D. (2011). Armed conflict and schooling: Evidence from the 1994 Rwandan genocide. *World Bank Policy Research Working Paper*.

Alesina, A. & Perotti, R. (1996). Income distribution, political instability, and investment. *European Economic Review*, 40(6), 1203–1228.

Ali, H. E. (2013). Estimate of the economic cost of armed conflict: A case study from Darfur. *Defence and Peace Economics*, 24(6), 503–519.

Arunatilake, N., Jayasuriya, S., & Kelegama, S. (2001). The economic cost of the war in Sri Lanka. *World Development*, 29(9), 1483–1500.

Barro, R. J. (1991). Economic growth in a cross section of countries. *The Quarterly Journal of Economics*, 106(2), 407–443.

Bellows, J. & Miguel, E. (2009). War and local collective action in Sierra Leone. *Journal of Public Economics*, 93(11), 1144–1157.

Brakman, S., Garretsen, H., & Schramm, M. (2004). The strategic bombing of cities in Germany in World War II and its impact on city growth. *Journal of Economic Geography*, 4(1), 201–218.

Chen, S., Loayza, N. V., & Reynal-Querol, M. (2008). The aftermath of civil war. *The World Bank Economic Review*, 22(1), 63–85.

Collier, P. (1999). On the economic consequences of civil war. *Oxford Economic Papers*, 51(1), 168–183.

Davis, D. R. & Weinstein, D. E. (2002). Bones, bombs, and break points: The geography of economic activity. *The American Economic Review*, 92(5), 1269–1289.

Fiala, N. (2015). Economic consequences of forced displacement. *The Journal of Development Studies*, 51(10), 1275–1293.

Fithen, C. & Richards, P. (2005). *No Peace, No War: An Anthropology of Contemporary Armed Conflicts*, chapter Making war, Crafting Peace: Militia Solidarities & Demobilisation in Sierra Leone, (pp. 117–136). James Currey.

Fransen, S., Vargas-Silva, C., & Siegel, M. (2018). The impact of refugee experiences on education: Evidence from burundi. *IZA Journal of Development and Migration*, 8(1), 6.

Galdo, J. (2013). The long-run labor-market consequences of civil war: Evidence from the Shining Path in Peru. *Economic Development and Cultural Change*, 61(4), 789–823.

Gates, S., Hegre, H., Nygård, H. M., & Strand, H. (2012). Development consequences of armed conflict. *World Development*, 40(9), 1713–1722.

Islam, A., Ouch, C., Smyth, R., & Wang, L. C. (2016). The long-term effects of civil conflicts on education, earnings, and fertility: Evidence from Cambodia. *Journal of Comparative Economics*, 44(3), 800–820.

Justino, P. & Verwimp, P. (2013). Poverty dynamics, violent conflict, and convergence in Rwanda. *Review of Income and Wealth*, 59(1), 66–90.

Keen, D. (2005). *Conflict and Collusion in Sierra Leone*. James Currey.

La Mattina, G. (2017). Civil conflict, domestic violence and intra-household bargaining in post-genocide rwanda. *Journal of Development Economics*, 124, 168–198.

Leon, G. (2012). Civil conflict and human capital accumulation: The long-term effects of political violence in Perú. *Journal of Human Resources*, 47(4), 991–1022.

Lopez, H. & Wodon, Q. (2005). The economic impact of armed conflict in Rwanda. *Journal of African Economies*, 14(4), 586–602.

Miguel, E. & Roland, G. (2011). The long-run impact of bombing Vietnam. *Journal of Development Economics*, 96(1), 1–15.

NPWJ (2004). Conflict mapping in sierra leone: Violations of international humanitarian law from 1991 to 2002. *Report by the No Peace Without Justice Project*.

Richards, P. (1996). *Fighting for the Rain Forest: War, Youth & Resources in Sierra Leone*. James Currey.

Richards, P. (2004). The political economy of internal conflict in sierra leone. *Working Paper*.

Saing, C. H., Kazianga, H., et al. (2017). The long-term impacts of violent conflicts on human capital: US bombing and, education, earnings, health, fertility and marriage in Cambodia. *Working Paper*.

Serneels, P. & Verpoorten, M. (2015). The impact of armed conflict on economic performance: Evidence from Rwanda. *Journal of Conflict Resolution*, 59(4), 555–592.

UNDP (2006). Case study sierra leone: Evaluation of undp assistance to conflict-affected countries. *Report*.

UNHCR (2009). Sierra leone global report 2008. *Report*.

UNU-MERIT (2017). Sierra leone migration profile. *Report*.

Appendix

A. Tables

Table 1: Main Results

	(1) Years of Schooling	(2) Years of Schooling
born 91-01 × conflict	0.020	-0.015
	(0.126)	(0.128)
born 86-90 × conflict	-0.425**	-0.424**
	(0.209)	(0.209)
born 79-85 × conflict	-0.596***	-0.508***
	(0.170)	(0.165)
born 73-78 × conflict	-0.307**	-0.279**
	(0.132)	(0.123)
Avg. Yrs. of sch.	5.6 (born 61-72, conflict = 0)	
Controls		✓
N	23983	23890
R^2	0.337	0.459

Note: All specifications include chiefdom and year-of-birth fixed effects and a district-level linear trend. Control variables in (2) are sex and sector (urban or rural) as well as the household head's education, age and sex. Standard errors are clustered at the chiefdom level and reported in brackets. The conflict measure is standardised. Avg. Yrs. of sch. provides the average educational attainment for people in the control group in 2011 who live in chiefdoms that experience a mean level of conflict. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 2: Household Level Results

	(1) Years of Schooling	(2) Years of Schooling
born 91-01 × conflict	-0.087	-0.140
	(0.369)	(0.357)
born 86-90 × conflict	-0.614	-0.698
	(0.565)	(0.573)
born 79-85 × conflict	-1.682***	-1.489***
	(0.518)	(0.479)
born 73-78 × conflict	-0.371	-0.427
	(0.413)	(0.394)
Avg. Yrs. of sch.	5.6 (born 61-72, conflict = 0)	
Controls		✓
N	23983	23890
R^2	0.335	0.458

Note: All specifications include chiefdom and year-of-birth fixed effects and a district-level linear trend. Control variables in (2) are sex and sector (urban or rural) as well as the household head's education, age and sex. Standard errors are clustered at the chiefdom level and reported in brackets. The conflict measure is standardised. Avg. Yrs. of sch. provides the average educational attainment for people in the control group in 2011 who live in chiefdoms that experience a mean level of conflict. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 3: Siblings Results

	(1) Yrs. of Sch.	(2) Yrs. of Sch.	(3) Yrs. of Sch.	(4) Yrs. of Sch.
School age	-1.451	-1.086	-0.990	-0.965
	(0.994)	(0.831)	(0.853)	(0.852)
Avg. Yrs. of sch.	3.7 (siblings beyond school age during the war)			
FE Level	Chiefdom	Chiefdom	Household	Household
Controls		✓		✓
N	276	276	276	276
R^2	0.594	0.735	0.846	0.848

Note: The reported regressor school age is a dummy indicating whether below the age of 18 during the war. All specifications include year of birth as a control variable. Additional control variables in (2) and (4) are sex and sector (urban or rural) as well as the household head's education, age and sex. Standard errors are reported in brackets. Avg. Yrs. of sch. provides the average educational attainment for people in the control group in 2011, i.e. those beyond school age who have siblings at school age. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 4: Placebo Results

	(1) Years of Schooling	(2) Years of Schooling
born 66-72 × conflict	0.248	0.066
	(0.133)	(0.601)
Avg. Yrs. of sch.	5.5 (born 61-65, conflict = 0)	
Controls		✓
N	4121	4107
R^2	0.286	0.615

Note: All specifications include chiefdom and year-of-birth fixed effects and a district-level linear trend. Control variables in (2) are sex and sector (urban or rural) as well as the household head's education, age and sex. Standard errors are clustered at the chiefdom level and reported in brackets. The conflict measure is standardised. Avg. Yrs. of sch. provides the average educational attainment for people in control group *(C2)* in 2011 who live in chiefdoms that experience a mean level of conflict. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 5: Subsample Results

	(1) Years of Schooling	(2) Years of Schooling	(3) Years of Schooling
born 91-01 × conflict	-0.015	-0.093	-0.054
	(0.128)	(0.131)	(0.127)
born 86-90 × conflict	-0.424**	-0.570***	-0.462**
	(0.209)	(0.216)	(0.204)
born 79-85 × conflict	-0.508***	-0.584***	-0.535***
	(0.165)	(0.157)	(0.166)
born 73-78 × conflict	-0.279**	-0.340**	-0.271**
	(0.123)	(0.141)	(0.126)
Avg. Yrs. of sch.	5.6	6.2	5.6
Sample	Full	No Displaced	Only Returned
Controls	✓	✓	✓
N	23890	14667	22869
R^2	0.459	0.491	0.464

Note: All specifications include chiefdom and year-of-birth fixed effects and a district-level linear trend. Control variables are sex and sector (urban or rural) as well as the household head's education, age and sex. Standard errors are clustered at the chiefdom level and reported in brackets. The conflict measure is standardised. Avg. Yrs. of sch. provides the average educational attainment for people in the control group in 2011 who live in chiefdoms that experience a mean level of conflict. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 6: Displacement Intensity

	(1) Displacement	
conflict	1.286***	(0.084)
born 91-01	0.002	(0.008)
born 86-90	-0.024*	(0.012)
born 79-85	-0.027**	(0.012)
born 73-78	-0.019	(0.012)
born 91-01 × conflict	0.001	(0.007)
born 86-90 × conflict	-0.017	(0.010)
born 79-85 × conflict	-0.006	(0.010)
born 73-78 × conflict	-0.004	(0.009)
Constant	-0.032	(0.027)
N	28445	
R^2	0.168	

Note: Standard errors are clustered at the chiefdom level and reported in brackets. The conflict measure is standardised. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 7: Displacement Channel

	(1) Years of Schooling	(2) Years of Schooling
born 91-01 × conflict	-0.015	-0.038
	(0.128)	(0.131)
born 86-90 × conflict	-0.424**	-0.469**
	(0.209)	(0.210)
born 79-85 × conflict	-0.508***	-0.484***
	(0.165)	(0.168)
born 73-78 × conflict	-0.279**	-0.277**
	(0.123)	(0.127)
born 91-01 × displacement		0.138
		(0.166)
born 86-90 × displacement		0.268
		(0.203)
born 79-85 × displacement		-0.110
		(0.157)
born 73-78 × displacement		0.001
		(0.182)
Avg. Yrs. of sch.	5.6 (born 61-72, conflict = 0)	
Controls	✓	✓
N	23890	23890
R^2	0.459	0.459

Note: All specifications include chiefdom and year-of-birth fixed effects and a district-level linear trend. Additional control variables are sex and sector (urban or rural) as well as the household head's education, age and sex. Standard errors are clustered at the chiefdom level and reported in brackets. The conflict measure is standardised. Avg. Yrs. of sch. provides the average educational attainment for people in the control group in 2011 who live in chiefdoms that experience a mean level of conflict. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 8: Assets Channel

	(1) Years of Schooling	(2) Years of Schooling
born 91-01 × conflict	-0.015	0.025
	(0.128)	(0.161)
born 86-90 × conflict	-0.424**	-0.527**
	(0.209)	(0.253)
born 79-85 × conflict	-0.508***	-0.549***
	(0.165)	(0.210)
born 73-78 × conflict	-0.279**	-0.354**
	(0.123)	(0.146)
born 91-01 × asset loss		-0.029
		(0.049)
born 86-90 × asset loss		0.086
		(0.060)
born 79-85 × asset loss		0.033
		(0.054)
born 73-78 × asset loss		0.061
		(0.040)
Avg. Yrs. of sch.	5.6 (born 61-72, conflict = 0)	
Controls	✓	✓
N	23890	23890
R^2	0.459	0.459

Note: All specifications include chiefdom and year-of-birth fixed effects and a district-level linear trend. Additional control variables are sex and sector (urban or rural) as well as the household head's education, age and sex. Standard errors are clustered at the chiefdom level and reported in brackets. The conflict measure is standardised. Avg. Yrs. of sch. provides the average educational attainment for people in the control group in 2011 who live in chiefdoms that experience a mean level of conflict. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.