Civil Conflict and Human Capital Accumulation: The Long Term Effects of Political Violence in Perú

Gianmarco León *

Abstract

I provide empirical evidence of the long- and short-term effects of political violence on human capital accumulation. Using a unique data set that registers all the violent acts and fatalities during the Peruvian civil conflict, merged with individual level census data, I identify the causal effect of exposure to the conflict on educational attainment. The results show that the average person exposed to political violence accumulates about between 0.12 and 0.19 less years of education as an adult. This effect is more important for women than for men, and for Spanish speakers than for native speakers. The short-term effects are found to be stronger than in the long run. Interestingly, children affected by violence are able to catch up if they experience violence once they have already started their schooling cycle, while if they are affected earlier in life, the effect persists.

JEL Classification Code: I20, J13, O12, O15

Keywords: Political violence, Human capital accumulation, Economic shocks, Perú

*Department of Agricultural and Resource Economics, UC Berkeley. I am very grateful for the patient guidance of Elisabeth Sadoulet during the process of this research. Insightful comments and suggestions by Catie Almirall, Michael Anderson, Max Auffhammer, Alberto Chong, Alain De Janvry, Javier Escobal, Valerie Koechlin, Jeremy Magruder, and Edward Miguel were extremely important. The personnel in the INEI were very helpful in providing the census data I work with and taking the time to answer all my questions; likewise, Daniel Manrique provided the violence data, and had infinite patience with my questions. Of course, all errors remain my own. Coordinates: Leó´n, Department of Agricultural and Resource Economics, 308 Giannini Hall #3310, University of California, Berkeley, CA 94720 USA. Fax: (510) 643-8911, Tel: (202) 288-0504. E-mail: Gianmarco@berkeley.edu.

1 Introduction

Between 1980 and 1993, Perú suffered an intense period of violence caused by constant fights between the rebel group Partido Comunista del Perú-Sendero Luminoso (PCP-SL) and the national army. The Peruvian Truth and Reconciliation Commission (CVR, for its acronym in Spanish) estimated that this conflict caused the death of about 69,200 people (CVR, 2004), making the Peruvian case one of the longest and most brutal political conflicts in Latin America. However, beyond the well know short term effects of civil conflicts, in terms of lives lost in the war, damages to the economic and social infrastructure, the deep pain for the families of those who died or disappeared during the conflict, very little is known about the long term repercussion of war on those who survived. In this paper I will try to fill this gap in the literature by looking at the effects of exposure to the conflict on human capital accumulation.

During the past few years, there has been a growing literature in economics investigating the causes and consequences of civil conflicts. Pioneering theoretical work developed by Collier (1999) links armed conflicts and economic performance from a macroeconomic perspective. This study was followed by further theoretical development along the same lines (Collier and Hoeffler 1998, Collier and Sambanis 2005), as well as empirical cross-country applications. Among the latter, Chen, Loayza and Reynal-Querol (2008) look at 41 countries that suffered civil conflicts between 1960 and 2003, finding that after the war ends, there is significant recovery in terms of economic performance, health, education and political development. Moreover, Cerra and Saxena (2008) find that most of the output losses due to conflict are recovered in a very short period of time. However, as Blattman and Miguel (2009) point out, even though the cross country empirical evidence is useful to understand the overall trends following armed conflicts, it is hard to draw conclusions about how does violence affects individual and household welfare, for which we need detailed country analysis. Miguel and Roland (2005) look at the long term consequences of the massive US bombings in Vietnam, finding that 27 years after the end of the war there was no detectable impact on poverty rates, consumption levels, literacy levels, infrastructure, or population density. Davis and Weinstein (2002) and Brakman, Garrtesen and Schramm (2004) arrive at similar conclusions based on evidence from the allied bombing in Japan and West Germany, respectively. In general, this literature concludes that the effects of severe periods of violence on economic outcomes and human welfare tend to vanish over time.

Micro-level studies have gone deeper into unveiling the relationship between individual welfare and civil conflict. Mostly due to data limitations, the research in this area has focused on the immediate effects of conflict on health and educational outcomes. Akresh and de Walque (2009) use micro data collected four years after the Rwandan genocide to assess its impact on school attainment of children exposed to the conflict. They find that children (directly) exposed to violence accumulate 0.5 less years of primary education. Akresh, Verwimp, and Bundervoet (2009) look at the effects of the same conflict on child stunting, comparing the effect of violence with economic shocks, concluding that girls and boys exposed to the conflict have height for age z-scores that are 0.30 and 0.72 standard deviations lower, respectively. Using a similar research design, Akresh, Bundervoet and Verwimp (2009), assess the effects of the civil war in rural Burundi on health outcomes shortly after the termination of the conflict, finding that an extra month of exposure to the conflict reduces the children's height for age z-scores by 0.047sd's. Outside of Africa, Shemyakina (2006) analyze the effect of the 1992-1998 civil conflict in Tajikistan, finding that children who had experienced violence related shocks are less likely to be enrolled in school. The effects found are stronger for girls than for boys. In Latin America, Camacho (2009) shows that women exposure to the Colombian conflict during pregnancy causes children to be born with lower weight.

Unlike the cross-country analysis, micro econometric research has gotten closer to a cleaner identification of the causal pathways through which violence affects human development in the short-run. However it might be the case that people exposed to conflict at some point of their lives suffer an initial negative shock, from which they recover after a certain time, mirroring the patterns observed in the cross country literature. If this were the case, the studies cited above are only measuring the short term consequences of violence. Arcand and Wouabe (2009) try to address this issue by analyzing the effects of the Angolan civil war on a broad range of welfare indicators, finding significant differences in the short and long term impacts.

In this paper I go further than the existing literature, estimating the short and long term effects of civil conflict on educational achievement. Using a high quality data set containing the number of human rights violations[1] during the civil conflict in Perú across districts and years, matched with individual level census information from 1993 and 2007, I am able to identify the presence and intensity of the violence shock in the district where each child was born. Furthermore, I consistently estimate the effect of exposure to the civil conflict during different periods of life on the the level of education that people accumulate as adults.

Seen through the scope of a classic education production function model, the evidence found suggests that the exposure to violence affects later human capital accumulation through the shock of violence to the initial endowment the household had before the child is born, as well as during the early stages of his/her life. The evidence shows that this effect is relevant when the shock affect children during the very early stages of their lives, and before they enter primary school. Moreover, the impacts are greater for women than for men, and for Spanish speakers than for indigenous populations. Additionally, I find that the short-term effect of violence on school deficit is large, which together with the estimated long-term effects, implies that people who suffer these negative shocks are able to partially recover. Furthermore, children affected by violence are able to catch-up if they were affected once they have already started their schooling cycle, while if they experience violence earlier in life, the effect is persistent. Some suggestive evidence about the possible causal channel of these effect, but further investigation of this issue is left as an open question for future research.

[1]Particularly, the CVR reports the number of illegal detention, kidnappings, murder, extra judiciary executions, torture, or rapes by district and year. Hence, a violent shock is here understood as the intensity of human rights violations in a particular year and district.

2 Historical Overview and the Data

2.1 The Civil Conflict in Perú.

Towards the end of the decade of 1970, Perú was about to return to democracy after a military dictatorship that lasted 12 years. In 1979, a Constitutional Assembly was elected to design a Constitution and call for national elections, which were held on May 1980. During this electoral process, the PCP-SL made its first attack against the state: in certain areas of the department of Ayacucho, the days before the election, citizens started seeing graffities in the streets, calling them to boycott the election. The night before, on May 17^{th}, militants of the PCP-SL publicly burnt the electoral ballots in the district of Chuschi (province of Cangallo, department of Ayacucho), a symbolic act through which the PCP-SL formally declared the war to the Peruvian State (CVR, 2004).[2]

In the late 1970s', Perú was still a lower-middle income country, with a per-capita income of US$2,180 (US$ from 2000) in 1978, about 37% of the population still living in rural areas, and a small industrial sector, which only contributed with 38% of the GDP (World Bank, 2009). Most of the economy was dependent on the exports of natural resources. At the same time, government expenditures towards the expansion of the educational sector had been steadily growing since the 1950's, resulting in almost universal access to primary education. This expansion created a widespread idea of progress in the population, which wasn't fulfilled by the capacity of the economy to absorb the newly educated workforce. The CVR considers this phenomena, which they labeled as *"status inconsistency"*, as the main breeding ground in which the PCP-SL was able to spread their ideas and gain supporters among poor peasants and urban marginal squatters.

The armed conflict started in the department of Ayacucho, in the south-central highlands of the country, and before 1982, most of the activity of the PCP-SL was concentrated in that area, as shown in Figure 1. The political strategy of the revolutionary army was centered

[2]It is important to note that before the war was formally declared on that date, there had been no previous violent political activity headed by the PCP-SL.

in three main objectives: first, the advance towards Lima, the capital; second, keep captive populations using ideological arms and a terror strategy based on intimidation; and finally, to control the coca circulation, tracing alliances with the drug lords in the area, who provided some part of the financing needed to keep the war going.[3]. The geographical evolution of the war is shown in Figure 1.

The year 1982 represented a break-point in the dynamics of the conflict. The PCP-SL attacked a jail in Ayacucho to rescue their militants. The response from the army was brutal, assassinating three terrorists injured in conflict in a local hospital. Since that point, the violence from both sides exponentially increased, reaching its peak in 1983-84 and then in a later escalate, in 1989-90. Figure 2 shows the time dimension of the conflict.

Some of the attacks by the PCP-SL were targeted towards popular leaders and land holders. However, the civil population was also threatened by the terrorists: whenever a village declared themselves against the revolution, they were brutally punished. Moreover, victims from roadside attacks and collection of supplies and food for the revolutionary army were mostly traders and farmers. As a result, attacks to civil population was not an uncommon episode during the war, as can be seen in Table 2.[4] Almost half of the victims of human rights violations were farmers, about 7% were local traders, 5% housewives, 5% independent workers, and so on. The probability of being a victim of violence in hands of either the army or the PCP-SL in a violent area was evenly distributed. A very often target of the attacks were public infrastructure, such as power stations and distribution centers, roads, water channels, etc. Our data set does not capture these episodes since it only focuses on human rights violations. For our purposes, it is important to note that, as I explain in the next section, school infrastructure was not hit by neither of the parties involved in the conflict.

By 1992, the PCP-SL had consolidated its presence in the central and southern highlands, and had significant influence in urban marginal areas in Lima, from where they planned selec-

[3]Other important sources of financing were based on targeted kidnappings, and "cupos" (fees) that they charged to farmers, entrepreneurs, and business in general to keep them "safe".

[4]The data included in Table 2 has to be interpreted carefully, since about 20% of the individual cases of human rights violations do not have information on the occupation of the victim.

tive attacks and bombings in the city. But in September 1992, the head of the revolutionary army, Abimael Guzman, together with most of the central committee of the party were captured and incarcerated. From that point on, violent attacks from the PCP-SL decreased significantly, and its power within the country was controlled.[5]

Overall, there were fatalities reported in all but two departments (out of 25) of Perú at some point. The CVR estimates that about 69,290 people were killed,[6] out of which 54% are responsibility of the PCP-SL; the Movimiento Revolucionario Túpac Amaru (MRTA) was responsible for 1.5% of the deaths; and the remainder of deaths was perpetrated by agents of the state (police, army, navy, etc.) or paramilitary groups.

2.2 The Data

Information about the presence and intensity of violence comes from the data set collected by the Peruvian Truth and Reconciliation Commission (CVR), which has detailed records of every human rights violation reported during the period of civil violence in Perú. Particularly, the information that I use in this paper correspond to illegal detentions, kidnapping, murder, extra judiciary executions, torture, or rapes. This data is merged with individual level information from the 2007 and 1993 censuses, where we can identify the year and district of birth of each individual.

In 2001, during the transition to democracy, the government appointed the CVR, which was in charge of shedding light on the violent period between 1980 and 2000, as well as to establish the responsible agents of the human rights violations that took place in that period.[7] One of the main tasks of the CVR was to travel around the country holding public hearings

[5]Even though after the capture of Abimael Guzman we still observe reports of human rights violations reported to the CVR, the vast majority of this violence was responsibility of the government of Alberto Fujimori. The ex-president was just convicted for some of these charges.

[6]This estimate was made based on a cross sample methodology.

[7]A total of thirteen commissioners were appointed. The CVR had to be politically impartial, thus Commissioners picked were representative public figures from the civil society, human rights organizations, academic sectors, the military, the church, and represented different political views. Even though there are claims that the left was over represented in the CVR, the public consensus is that the commissioners represented an impartial political view.

during which they gathered testimonies from victims, relatives, witnesses, and survivors to report any act of violence between 1990 and 2000.[8] All the testimonies were individually coded in order to identify the type of act (rape, murder, torture, etc), location, potential responsible group (armed forces, PCP-SL, MRTA, etc.), identity of the victim and individual characteristics (gender, age at death, birth place, educational level, language, occupation, and religion). The data gathered from this process was merged and cross tabulated by the identity of the victim with the original registry information from six other data sets gathered at different points in time by human rights organizations, the judiciary, NGO's, and the ombudsman's office. In this process, the CVR identified approximately 45,000 cases. After dropping double-coded cases and those that could not be cross-validated, the sample size drops to 23,149 individual fatalities (only disappeared or dead). Additionally, in a separate data set, the CVR also coded the testimonies as violent acts,[9] which include detention, kidnapping, murder, extra judiciary execution, torture, rape, among others; in this data set, each of the 36,019 observations represent one violent act recorded. The former constitutes a subset of the later, hence I use this information for the analysis. Also, it is important to note that the data set only includes human rights violations, and not terrorist attacks against public infrastructure, therefore we should be careful in the interpretation of the results, since I am only identifying the effect of being exposed to violence against human beings in the close environment (within the district), and not of the destruction of economic infrastructure or public utilities.

The individual level information used in the analysis comes from 2% random samples of the 2007 and 1993 national census, respectively. In each of these data sets, I can identify the year and district of birth of each individual, hence I can merge them with the aggregated violence data at this level. My final data set is at the individual level, and includes variables

[8]The public audiences were widely advertised in the locality where the audience was going to be held, as well as in the neighboring localities. Additionally, communities could ask for an audience to be held in their town.

[9]Due to budgetary and time constraints, this data set was coded based only on 70% of the testimonies received.

recording the number of human rights violations that happened in each year of the individual's life in his/her district of birth. Additionally, the data also includes basic demographic information: gender, language, etc., and more importantly to us, the number of years of education completed.

My interest variable is the number of years of education accumulated during the lifetime. More specifically, the years of primary and secondary schooling.[10] For this reason, I restrict the sample from the 2007 census only to those people that are old enough to have completed the last grade of secondary education (18 years old or older). Likewise, I include all individuals born after 1975 in order to have a suitable control group in the data. Figure 2 explains the time-line of the conflict intensity and the overlap periods with the sample. The main independent variable will be the number of years of exposure to violence during each stage of the early life in the district of birth. Similarly, I will also consider the number of human rights violations, to get an estimate of the effects of the intensity of violence.

When trying to understand the short term effects of violence I use the 1993 census, which was applied right at the point when political violence started to sharply decline in the country. In this case, people exposed to the political violence are mostly in school age, and hence the number of years of education would not be an accurate measure of human capital accumulation. Alternatively, I use the educational deficit as the outcome variable. This variable measures how far behind is the child falling with respect to the mandatory age of school entrance and normal progress.[11] The population included in the sample are all children in school age (6-17).

Table 1 presents the descriptive statistics of the main variables used in the analysis, by violence exposure status. On average, people included in the sample has about 9.4 years of primary and secondary education (out of 11). Further, when we split the sample by violence

[10] For this reason, we truncate the education variable at 11 years, which correspond to the completion of the secondary schooling cycle in Perú. The main results from the paper are unchanged if we didn't truncated the dependent variable.

[11] Formally, this variable is defined as the age of the person, minus the mandatory age to enter school (6), minus the number of years of education completed.

exposure status, people who were exposed to violence in their districts have, on average, higher educational attainment (9.6) than those whose birth district was never exposed to violence while they were children (8.7 years). This evidence is consistent with the *"status inconsistency"* argument presented in the previous section, according to which the main breeding ground for the PCP-SL to gain support was the discontent of relatively well educated people who did not have equal access to the market as the people living in bigger cities. All the other covariates shown in the table are balanced between people born in violent compared to non-violent districts. Likewise, when we look at the difference in educational deficit among school aged children in the 1993 census, those who were exposed to violence in their birth districts are 1.56 years behind in school, while children who didn't had experience with war fall 1.02 years behind in school.

Figure 3 further shows the average years of education for individuals born in districts exposed to violence, and

3 Theoretical Framework and Empirical Strategy

Consider a typical education production function model in the spirit of those discussed in Hanusheck (1979), where the overall stock of educational for each individual in period t (S_t) is a function of the individual's initial endowments when she starts her school life (E_0), the history of educational inputs ($N_1, ..., N_t$) to which the child has access to, factors related to the (time-invariant) demographic characteristics X (i.e. gender, language), and community characteristics ($C_1, ..., C_t$).

$$S_t = s(E_1, ..., E_t, E_0^h, N_1, ..., N_t, X, C_1, ..., C_t) \tag{1}$$

The endowment at each period of time, E_t, is determined by genetic factors (G), the endowments of the household where the child is born (E_0^h), in addition to environmental experiences and conditions in the early life (V_0), community educational resource (C_0), as well as other environmental characteristics that have a persistent effect of cognitive skills (R_0).

$$E_t = g(G, E_0^h, V_0, C_0, R_0) \tag{2}$$

The focus in this paper is to try to disentangle the effect of violence shocks that affect children in their early life and have an effect on future school achievement. Hence, I will focus on the component of the individual endowment that is affected by environmental conditions in this critical period (V_0). The reduced form of the model allows me to identify the deviation of an individual outcome from the ones born in the same year, as well as of one's birth district, and the long run trend in the expansion of education in the province. To be able to identify this effect, I exploit the exogenous variation provided by the moment when the civil conflict started, as well as its geographical localization. The date and location of birth jointly determine the exposure of any given child to the violence.

The reduced form equation to be estimated directly follows equations (1) and (2):

$$S_{ijt} = \alpha + \sum_t \beta_t Violence_{jt} + \gamma_{jt} Trend + \delta X_{ijt} + \eta_j + \nu_t + \epsilon_{ijt} \tag{3}$$

where S_{ijt} is the number of years of schooling achieved by individual i born in district j, and in year t. I include a province specific cubic time trend, which is intended to capture long-run changes, such as differentiated economic development, or the intensity in the construction of schools in a particular province. Further, this variable isolates the variation in a person's outcomes which diverge from the long run trends in his/her birth province. X_{ijt} is a vector of individual time invariant characteristics, such as gender, or language.

The inclusion of district of birth intercepts (η_j) controls for any district specific time-invariant characteristics. Similarly, I allow for cohort effects by including year of birth specific fixed effects (ν_t). Finally, ϵ_{ijt} represent a random error term. A particular problem arises due to the fact that educational achievement is a stock variable, hence districts with higher educational achievement in a given year will very likely have similar (or higher) educational achievement the following year. Likewise, there might be education spillover effects between districts. To deal with the spatial and time correlation in the error terms, standard errors will allow for an arbitrary variance-covariance structure within birth district by clustering them at the district of birth level.

From equation (3), our interest relies on the coefficients associated with $\sum_t Violence_{jt}$, which is a set of variables indicating the number of years of violence exposure in each period of one's life: early childhood (-2 until 3 years old),[12] pre-school (4 to 6 years old), primary school age (7 to 12 years old), and high school age (13 to 17 years old). These variables are intended to capture the effect of having the bad luck of being born in a violent period, and in a district exposed to violence. Additionally, in the next section I also use as independent variables of interest measures of the intensity of violence in each year.

Consistent with the model presented above, exposure to violence can affect individual endowments at the moment they start school through several channels. For example, violent attacks of the PCP-SL can affect E_0^h by killing a member of the household, which represent a direct income shock for the household that could last several years. Hence, if a household suffers from this shock some years before the child is born, it could still affect the nutrition of the child. Other potential pathways are the nutrition of the mother, or of the child himself once he is born, which may cause irreversible consequences for her/his future school attainment. Similarly, an economic shock that affects the household during the early stages of life affect the nutrition that the children receive, and hence has long lasting effects on cognitive abilities. On the other hand, Camacho (2009) presents evidence suggesting that violence related stress during pregnancy has negative effects on the child's birth weight, which in turn affects cognitive development. Another channel through which violence exposure could affect the child before she/he is born is through traumatic experiences that affect the

[12] In the early childhood period I choose to include the second year before the child was born because there are measurement errors in the date of birth: I only have information about the year when the child was born, but not the specific month. Likewise, the information from the CVR is also aggregated by year, and may contain some recall errors.

mother's parenting abilities, and thus the child's development. Finally, this effect can also be more direct, psychologically affecting the child himself, which will in turn affect his cognitive abilities.

Violence could also affect the community educational resources (C_0), as in the Sierra Leonean case (Bellows and Miguel, 2009). However, as was argued above, it is unlikely that educational infrastructure was destroyed during the conflict by any of the parties involved: the PCP-SL had strong beliefs about the role of education on the revolution, which is clear from the great influence they had on the teacher's union. On the other hand, schools are a very valued asset within a community, thus if the army was to gain the support of the community to fight the terrorists, they didn't had an incentive to destroy school infrastructure. However, since the influence of the rebels on the teacher's was known, the capture or even murder of the local teacher of the hands of the national army was not an uncommon episode: about 3% of the reported human rights violations were against teachers (see Table 2). Further, it was not an easy task to replace a teacher in a violent area. This effect would be captured by the violence indicators corresponding to the years before the child enters school, which in Perú happens at age 6.

4 Results and Discussion

4.1 Main Findings

My main goal is to estimate the causal impact of exposure to violence on schooling. In order to do so, we must first isolate the individual's schooling from the mean schooling in his/her birth district, as well as the mean schooling in the respective birth cohort. We must also account for the fact that the different governments that succeeded during the period of analysis put different emphasis on the development of each particular region, thus we must isolate individual's schooling from the long run trend of his/her birth province. The inclusion of a large set of fixed effects and province specific trends are intended to take care of these issues.

The district of birth fixed effects control for any specific characteristics of all children born in the same locality. Similarly, the year of birth fixed effects should absorb any shock common to all households with children born in the same year. The flexible province-specific trends included in all the regressions are meant to account for the differential developments of each province of the country through time. We must bear in mind the inclusion of this large set of fixed effects when interpreting the results, since they will not represent the impact of violence on schooling at the national level, but the average affect with respect to local averages, year averages, and purged from province flexible trends.

The main identifying assumption that I need to consistently estimate the causal effect of exposure to violence on educational achievement is that, after controlling for a broad set of district and year fixed effects, and a province specific time trend, the error term is uncorrelated with the incidence of violence. This assumption will be violated if there was a selection problem whereby the districts affected by violence were also those with lower levels of educational achievement. However, as explained in Section 2, one of the main breeding grounds for the PCP-SL to get popular support was the *"status inconsistency"*, this is, the localities where the PCP-SL started where those with relatively high levels of education and low opportunities for social mobility. For our purposes, this means that if anything, there should be a negative correlation between the localities where the conflict started and educational achievement. Further, as shown in Figure 1, the expansion of the conflict followed clear strategic and political objectives, which are uncorrelated with the distribution of educational.

One way of checking if there isn't a selection problem in our sample is to look at the baseline level of education between the districts that were affected by violence, and those that are used as controls. Unfortunately, there is no district specific data on education available for the years before the conflict started. However, in the 1993 census we can look at the educational level of the cohorts that, at the time of the start of the conflict, were old enough to have finished their educational cycle. Figure 3 shows the average years of education of the cohort of people who were between 17 and 22 by 1980, separating them by the number of years of violence exposure of their birth districts. People born in districts that were never affected by violence had about 7.4 years of education, while those who were born in a district that was exposed to violence between 1 and 3 years have slightly more education (7.5 years, on average). Likewise, those born in districts with higher levels of exposure have about 7.2 years of education. None of the differences between these groups of districts are statistically significant.

Table 3 shows the results of the main specification presented in equation (3). In all the specifications I use a set of variables indicating the number of years that each individual was exposed to violence during each period of the early life: early childhood, pre-school, primary school, and secondary school age. Being exposed to violence before entering school, this is, during the early childhood or in pre-school age has an statistically and economically significant effect on the long-run human capital accumulation. As shown in column (1) in Table 3, a household that was exposed to one year of violence between two years before the child was born[13] and the time when the child is three years old implies that the he/she will accumulate 0.06 less years of education, while being exposed to a similar shock during pre-school age (4 to 6), implies about 0.05 less years of education per each year of exposure to violence.[14] On the other hand, living in a district affected by violence while you are in primary or secondary school age does not have a significant impact on the long run educational achievement. Furthermore, we expect that any violent shock experienced by the household during the years before the mother was pregnant will not have any effect on the child's educational outcomes. As a robustness check, Column (2) tests this hypothesis by including indicators for the presence of violence in the district of birth during the years before the mother was pregnant, and as expected, we do not find any statistically significant effect for

[13]As I mentioned before, there are potential measurement errors in the date of birth (I only have the year of birth of each individual, but not the month), and in the violence reports. Hence, violence experienced two years before the child was born can be interpreted as a shock in-utero.

[14]The point estimates are not significantly different from each other.

these variables.[15]

One potential concern with the results shown in Column (1) is that the time-series correlation in the exposure of violence might be affecting our estimates. One way to indirectly test this is to include in the same regression the indicator variables for the years before pregnancy, and the variables indicating the number of years of exposure to violence in each period of the individual's life. I do this in Column (3), finding again no statistically significant results for the exposure to violence during the years before pregnancy.[16] The coefficients associated with violence in the critical period of early life (in-utero until 3 years old), and pre-school years are still significant at the conventional levels and their magnitude is slightly increased compared to those shown in column (1).

The effect of having an additional year of exposure to violence during the early childhood is 0.055, while for the period of pre-school is about 0.05. Even though this seems like a small effect, it is important to notice that on average each child that has ever been affected by violence in each of these periods had about 2.6 and 1.9 years of exposure, respectively. This means that the average child born who was exposed to violence in his/her early childhood will accumulate about 0.14 less years of schooling than his/her peers in peaceful districts, or born in peaceful years; while if he/she was exposed to violence between 4 and 6 years old, the effect for the average child exposed to violence is 0.09. To put these results in context, Duflo (2001) finds that the effect of the massive school construction program in Indonesia on school attainment is of about the same magnitude, but in the different direction: each school constructed per 1,000 children led to an increase of 0.12 to 0.19 years of education.[17] Furthermore, we can be fully flexible in the functional form assumed to fit equation (3), and include indicators for each year exposed to violence. Figure 4 shows these results in a graphical way. Consistent with the results shown in Table 3, violence exposure before

[15] As a robustness check, I also run these regressions only considering fatalities (number of deaths and disappeared). The results are very similar in magnitude and statistical significance.

[16] I also test if the coefficients if violence exposure before birth are jointly statistically significant. An F-test fails to reject the null that all the coefficients are statistically different from zero.

[17] An alternative way to think about the results shown is as the intention to treat effect for the direct experience of war on educational achievement.

the mother was pregnant (3 to 6 years before) is not statistically different from zero, while this effect is relevant while the child is between -2 and 6 years of age. The coefficients corresponding to older ages are again indistinguishable from zero.

We might be tempted to think than not only the presence of violence in the birth district during the sensitive period is relevant in determining future schooling outcomes, but also the intensity of violence matters. Table 4 replicates the regressions presented in Table 3 replacing the variables used in equation (3) with the number of violent events reported per 1,000 inhabitants living in the district in 1993.[18] The statistical significance of the coefficients exactly mirrors the results shown before: being exposed to a violent shock during the early childhood or in pre-school age has a negative causal effect on adult educational achievement. Further, the coefficients are not statistically significant from each other, and all of them are between 0.003 and 0.005, which means that an increase in the number of human rights violations in the district of birth, in a particular year, of one log point (about 2.7 more violent events per 1,000 inhabitants) leads to about 0.004 less years of education as an adult. Again, considering that the average district affected by violence had about 1.8 violent events per 1,000 inhabitants, the effect found translates into 0.07 less years of education.

Maccini and Yang (2009) in their study about the effects of early life rainfall on adult welfare find that experiencing a weather shock in early life has a negative persistent effect on welfare, but the effect is only significant for women. This suggests that, in times of hardship, there may be a different preferences for boys and girls, giving priority to the latter whenever the household faces a negative income shock. On the other hand, the CVR documented that about three fourths of the victims of violence during the war were indigenous[19]. Following these ideas, Table 5 divides the sample by gender and by language to see if there are differential impacts in these particular subgroups. Consistent with the findings mentioned above, Table 5 shows that the results are mostly driven by the effect for women. The point estimates

[18]Sadly, yearly information about the population per district is not available. If there was higher migration out of the violent districts, the results shown in this particular table can be biased upwards.

[19]Indigenous people are defined as those that have a native mother's language: Quechua, Aymara, or an amazonic language.

for the exposure to violence in all periods are larger for girls than those found in our benchmark specification in the first column of Table 3, and statistically significant for exposure during the early childhood, and in the pre-school period. This implies that on average an extra year of violence in the district of birth in during the former period translates into 0.067 fewer years of education, while in the latter it means about 0.079 less education as adult women. On the other hand, for men only the exposure to violence during the early childhood seems to be important determinant of future schooling, and the coefficient is smaller in magnitude. Surprisingly, I find that the effect of violence is only significant for Spanish speakers, while for the indigenous population there is no significant effect.

4.2 Potential biases and concerns

4.2.1 Sample composition

One of the drawbacks of using the information gathered by the CVR to measure the intensity of violence is that it comes from a non-random sample. The characteristics of the data gathering process make this a self-selected sample, which may introduce biases to our estimation. Moreover, based on a much debated statistical procedure, the commission concluded that the total number of fatalities during this period was about 69,290, which is almost four times the number of observations actually recorded. If there is any bias implied from the self-selection into reporting human rights violations to the CVR, it is plausible that the under reporting present in the data is coming from the group that was more affected by violence, those who had a harder time verbalizing the incidents in front of the commission. Further, the testimonies were collected in relatively bigger cities, which implies that for some of the most vulnerable populations -for whom the opportunity cost of reporting the violence were binding- were not able to report human rights violations. This possible selective under reporting of the violence data is likely to underestimate our results, hence the point estimates found have to be interpreted as a lower bound. Anyhow, if this bias exists, it is more likely to show up in the regressions where I use the number of human rights violations, whereas those in which I include the number of years exposed to violence in each period I code a year of exposure where there has been at least one violent event reported, therefore this bias is not likely to be picked up in this measure.[20]

Another possible bias in our result may come from the fact that the fatal victims of violence are not recorded in the census. However, one might think of these people as those who were most affected by the violence. Hence, the selection problem induced by the fatal victims again introduces a negative bias in the estimation of the effects of violence.

4.2.2 Migration

A more serious concern comes from the fact that the questions recorded on the census only ask about the district of birth, where the person lived five years before the interview, and the current location. We do not observe the actual migration history, or the reasons to leave one's hometown, hence it is not guaranteed that the presence of violence in the birth district in fact affected every individual in the full sample. The bias implied by the migration history can not be signed a priori. On the one hand, there is anecdotal evidence that people who migrated from violent areas are discriminated in larger cities, denying them the access to public services such as education or health care. If that were the case, the point estimates shown before would be overstating the effect of violence on schooling. On the other hand, people who migrate away from conflict areas is likely to go to bigger cities, where there are much more employment opportunities, better access to public services, and where they have a social network to support them. Hence, the development outcomes of people who migrated should be better than that of their peers left behind. In this case, including the migrants in the estimation would imply an underestimation of the effect. Additionally, positive or negative selection into migration could also bias our results: if people who were able to scape from the violent districts were those at the top end of the distribution, had they stayed, the

[20] I also run the same set of regressions using as a cut-off to define a violent district in a given year being above the 20th and 30th percentile of violent district. The basic patterns observed in the results in the tables shown are unchanged. These alternative tables are available for the interested reader upon request.

effect on their human capital accumulation wouldn't have been that bad. If that were the case, the estimates in Tables 3 and 5 would be overstating the impact of violence.

There is no clear way to disentangle the sources of the direction of the migration bias, but to address the question empirically. One indirect way to deal with this issue is to only take into account in the regression analysis those people who still live in their birth districts, and compare the point estimates of the original sample with the ones obtained for the non-migrants. This evidence will give us a sense of the direction and size of the bias in the results reported in Tables 3 and 5. Indeed, Table 6 shows the main results from Table 3, splitting the sample between those who report living in their birth-place and those who migrated at some point in their lives. The results show that the effect for the non-migrants is slightly higher than those found in the full sample. On average, an additional year of exposure to violence during the early childhood for those children who still live in their birth place leads to 0.062 less years of schooling, while if they experienced violence when they were between 4 and 6 years old, they would accumulate 0.052 less years of education. Overall, the effect for non-migrants who were affected in both periods of their lives translates into about one quarter of a year less schooling.

On the other hand, for those children were no longer living in their birth district by 2007, the effect of violence exposure is also statistically significant for both periods, but the coefficients are slightly smaller. Being born in a violent district for children who migrated and were affected by violence in both periods, causes them to accumulate about 0.046 less years of education if the violence happened during the early childhood, and about 0.059 if it happened between 4 and 6 years old. This result is consistent with the findings by Escobal and Flores (2009), who document that mothers who migrate out of violent districts have children with higher nutritional status, when compared to their peers who stayed, but they find no differences on cognitive abilities.

Summing up, the effect observed in Tables 3 and 5 ought to be interpreted as a lower bound of the effect of exposure to violence on educational achievement in future life.

4.3 Short term effects and possible causal pathways

So far we have shown that living through violent periods in the close environment during the critical period of life have a causal link to lower school achievement in the long term. However, this finding contrast with empirical findings which document that, after suffering civil wars, countries are able to recover in most areas of development, such as nutrition, education, economic growth, etc.[21] In this section, I go further in this direction and look at the short-term impact of political violence on school achievement. Using a similar methodology as above, and data from the 1993 census, I fit equation (3). Unlike previous section, we are not able to observe in 1993 people who has been exposed to violence and also who is old enough to have finished school. Therefore, a more accurate measure of school achievement for this population would be one that tells us how far behind children are falling in school. The educational deficit is defined only for children in school age (6-17) and is measured as the difference between the age, the mandatory age for entering school (6 in Perú), and the number of years completed in school. So, for example, a child who is 16 years old, and only has completed primary education (6 years of education) has a deficit of 4 years, since she should have been in her fourth grade of high school by that age. Given this definition, and the findings in the previous section, I focus on the children in school age who report living in the same location where they were born, interviewed in the 1993 census.

The results of this exercise are shown in column (1) in Table 7. In this case, being exposed to violence either in the early childhood, pre-school age, or primary school age has a statistically and economically significant effect on school deficit. Suffering from the presence of human rights violations in the district of birth during the early childhood, between 4 and 6 years old, or during primary education causes a child to fall behind in school in about 0.1 years, for exposure to violence in each of the periods of life. Considering that the average children exposed to violence in this sample was exposed to about 2.4, 1.8, and 2.9 years of violence in each of the periods mentioned, respectively, this implies an average effect of 0.78

[21] See for example, Miguel and Roland (2005), Davis and Weinstein (2002), Brakman, Garrtesen and Schramm (2004), Cerra and Saxena (2008).

more years of school deficit for the average child exposed to violence in these three periods. Recall from Table 3 that, for people observed in 2007, the average child exposed to violence accumulate 0.21 less years of education. Putting together the results found in this section with the ones in Table 3, they seem to suggest that the effect of violence on human capital accumulation is reduced as time goes by. Moreover, it seems like those children who are affected by violence once they have already started their school life are able to catch up, while those who experience violence before entering school are permanently affected. This evidence is consistent with the extensive literature about economic shocks and the critical-period programming (Alderman, Hoddinott, and Kinsey, 2006; Maccini and Yang, 2009, among others).

As I mentioned in Section 3, one of the hypothesized channels through which violence exposure might affect future educational outcomes is through household's wealth, which in turn has a significant effect on children's cognitive development. Given the small range of questions included in the census, we are not able to directly test this channel. However, one of the advantages of using a younger cohort and who hasn't migrated is that most of the individuals in the sample are very likely to still be living in their parent's house, then we can use the asset tenure information in the census to construct an asset index for the household where the child grew up. This measure has been suggested to be a good proxy of long term economic welfare (Filmer and Prichett, 2001). If it were the case that the effect of violence exposure on human capital accumulation went through the welfare effects of the household, the inclusion of the asset index in the regression presented in the first column of Table 7 would imply a reduction in the statistical significance and magnitude of the coefficients of violence exposure. We perform this exercise in Columns (2). However, the coefficients of violence exposure (and their standard errors) are hardly affected, which suggests that long term household wealth isn't one causal channel through which violence affects educational outcomes.[22]

[22]It is important to have in mind the fact that our measure of violence only represent human rights violations, and does not capture any destruction of productive infrastructure, which are the most likely

The structure of the sample allows us to do one further robustness check, we could exploit the variation of violence exposure between siblings to identify the parameters of interest, keeping constant all time invariant household characteristics. This model is shown in Columns (3) of Table 7. Surprisingly, the sibling difference model does not show much significant differences with the initial model. Taken together, these results allows me to rule out the hypotheses that the causal pathway through which experiencing violence in the environment affect educational achievement is household economic welfare, or any other household time invariant characteristic. Therefore, the effect has to go either through some short term shock to the household, or a time variant, unobserved environmental parameter.

One additional channel through which violence could affect directly the educational level of children could be if it was the case that a teacher was affected by the violence directly, and thus classes were interrupted abruptly. The CVR recorded the occupation of the victims whenever it was reported,[23] and thus we can directly test this hypothesis by including interactive terms in our main regression. These results are shown in the last column of Table 7. The results suggest that the direct effect of violence on teachers leads to a higher schooling deficit, and surprisingly this is especially true whenever this events happened before the child was old enough to enter school. The information available does not allow me to go further in the investigation of the exact causal pathways, which leaves open an interesting window for future research.

Finally, Table 8 presents supporting evidence of the findings from Table 7. In this case, we reproduce the regressions from this table, but now looking at the intensity of violence, as measured by the number of human rights violations per 1,000 population in the district. The results are consistent to those shown in the previous table.

determinants of household economic status.

[23]About 20% of the sample do not report the occupation of the victim, thus these results have to be interpreted carefully.

5 Summary and Conclusions

Civil conflict is a widespread phenomenon around the world, with about three fourths of the countries around the globe having experienced an internal war within the past four decades (Blattman and Miguel, 2009). The short term consequences of these conflicts are brutal in terms of lives lost, destruction of economic infrastructure, loss of institutional capacity, deep pain for the families of the people who died in the war, etc. However, the economic literature so far has had little to say about the long term effects of these conflicts on those who survived, but still were exposed to them. In this paper I address this issue, looking at the long- and short-term consequences of political violence on educational achievement in Perú.

The empirical literature dealing with the effects of civil conflicts, especially at the macro level, shows robust evidence that those countries exposed to severe violence are able to catch up after a certain period of time, recovering their pre-conflict levels in most development indicators. On the micro side, several papers document the very short-term consequences of conflicts on human development, especially on nutrition and education. However, if the trends observed at the macro level are followed at the micro level, one might expect these effects to vanish over time.

In this paper, I go further than the existing literature, looking at the long- and short-term effects of the exposure to violence in the early stages of life. Particularly, I analyze the Peruvian case, in which the constant struggles between the army and the rebel group PCP-SL lasted over 13 years, causing the death of more than 69,000 people, as well as huge economic losses. Using a unique panel data set collected by the Peruvian Truth and Reconciliation Commission (CVR), which registers all the violent acts and fatalities during this period, merged with individual level census data from 1993 and 2007, I quantify the long term effects that violence had on people exposed to it in the early stages of their life on human capital accumulation. The identification strategy used in the analysis exploits the exogenous nature of the timing and geographic localization of violence, which allow me to identify the average losses in educational achievement in the long term, with respect to local averages, year averages, and purged from province flexible trends.

The results show that a average person exposed to political violence during the early childhood has 0.14 less years of education, while if the violence was experienced before entering school, but after the early childhood, this effect is of about 0.09 less years of education as an adult. This result is more important for women than for men, and for Spanish speakers than for indigenous populations. Some concerns with the sample composition and migrations issues leads us to think that these results ought to be interpreted as lower bounds of the estimated effects.

I also provide evidence that tells us that the short term effects are stronger than the long term effects. This finding contrasts with the cross-country findings that the effects of violence vanishes over time, since we still find significant effects in the long-run. An interesting finding of the paper is that children affected by violence are able to catch up if they are affected once they have already started their schooling cycle, while if they experience violence earlier in life, the effect is persistent.

The structure of our data also allows me to test for possible causal pathways through which which exposure to violence might affect adult educational outcomes: I am able to rule out that the effect found goes through household long term wealth or any other household time invariant characteristics. We must be careful when interpreting these results, since our data set does not capture the violent acts that are related to the destruction of productive and physical infrastructure. Interestingly, I find that whenever a teacher was affected directly by violence, the educational deficit is reduced, and this is especially true when the teacher was affected during the period before the schooling cycle of the child started. These findings still leave a window open for future research in trying to disentangle the causal pathways through which violence affects adult welfare. However, the evidence presented here is useful to propose some policy recommendations for post-conflict societies, suggesting that people directly affected by violence in the early stages of life should be compensated for their losses in educational achievements, for example through technical training.

References

Akresh, Richard, Damien de Walque. 2009. “Armed Conflict and Schooling: Evidence from the 1994 Rwandan Genocide”. *Review of Economics and Statistics, forthcoming.*

Akresh, Richard, Verwimp, Philip, and Bundervoet, Tom. 2009. “Civil War, Crop Failure and Child Stunting in Rwanda”. *Economic Development and Cultural Change, forthcoming.*

Akresh, Richard, Tom Bundervoet and Philip Verwimp. 2009. “Heath and Civil War in Rural Burundi”. *Journal of Human Resources*, forthcoming

Alderman, H., J.R. Behrman, V. Lavy, and R. Menon. 2001. "Child Health and School Enrollment: A Longitudinal Analysis." *Journal of Human Resources.* Vol. 36 (1), 185-205.

Alderman, Harold, Hoddinott, John, and Kinsey, Bill. 2006. “Long Term Consequences of Early Childhood Malnutrition.” *Oxford Economic Papers*, 58(3), 450-474.

Arcand, Jean-Louis, and Eric Dijemeu Wouabe. 2009. “Households in the Time of War: Evidence for Angola”. *The Graduate Institute, Mimeo.*

Blattman, Christopher and Edward Miguel. 2009. “Civil War”. *Journal of Economic Literature*, Forthcoming.

Brakman, S., Garretsen, H., and Schramm, M. 2004. “The Strategic Bombing of Cities in Germany in World War II and it Impact on City Growth”. *Journal of Economic Geography*, 4(1), 1-18.

Camacho, Adriana. 2009. “Stress and Birth Weight: Evidence from Terrorist Attacks”. *American Economic Review: Papers & Proceedings.* 98(2), 511–515.

Castillo, Marco, and Regan Petrie. 2007. “Discrimination in the War place: Evidence from a Civil War in Perú”. *Georgia Institute of Technology Mimeo.*

Cerra, V., and Saxena, S. C. 2008. “Growth Dynamics: The Myth of Economic Recovery”. *American Economic Review*, 98(1), 439-457.

Chen, Siyan, Loayza, Norman V., and Reynal-Querol, Marta. 2008. “The Aftermath of Civil War.” *The World Bank Economic Review.* Vol 22(1), 63.

Collier, Paul. 1999. “On Economic Consequences of Civil War.” *Oxford Economic Papers*, 51(1), 168-83.

Collier, Paul, and Hoeffler, A. 1998. “On economic causes of civil war”. *Oxford Economic Papers*, 50(4), 563-573.

Collier , Paul, and N. Sambanis (eds.) 2005. “Understanding Civil War: Evidence and Analysis”. *The World Bank, Washington, D.C.*

Comisión de la Verdad y Reconciliación (2004). Informe Final.

Davis, D. R., and Weinstein, D. 2002. “Bones, Bombs, and Breakpoints: The Geography of Economic Activity”. *American Economic Review*, 92(5).

Duflo, Esther. 2001. "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from and Unusual Policy Experiment." *American Economic Review*, Vol 91(4): 795-813.

Escobal, Javier, and Eva Flores. 2009. “Maternal Migration and Child Well-Being in Perú”. *GRADE,* Mimeo.

Filmer, Deon, and Prichett, Lawrence. 2001. Estimating Wealth Effects Without Expenditure of Data - Or Tears: An Application to Enrollments in States of India. *Demography.*

Hanusheck, Eric. 1979. “Conceptual and Empirical Issues in the Estimation of Educational Production Functions”. *Journal of Human Resources*. Vol. 14(3): 351-388.

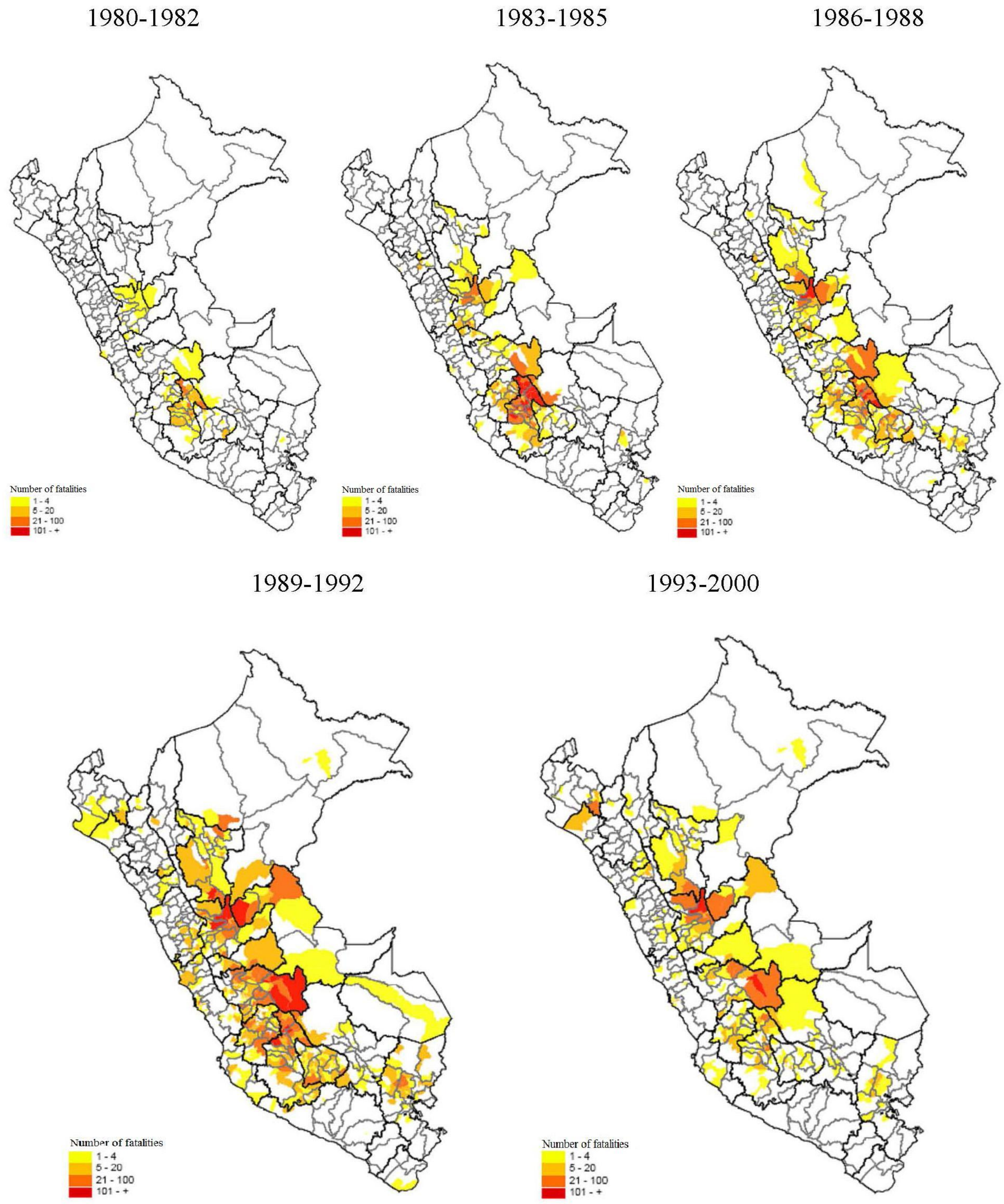
Maccini, Sharon, and Dean Yang. 2009. “Under the Weather: Health, Schooling, and Economic Consequences of Early-Life Rainfall”. *American Economic Review, forthcoming.*

Miguel, Edward and Roland, Gerard. 2005. “The Long Run Impact of Bombing Vietnam.” *University of California, Berkeley, Mimeo.*

Shemyakina, Olga. 2006. “The Effect of Armed Conflict on Accumulation of Schooling: Results from Tajikistan.” *HICN Working Paper 12.*

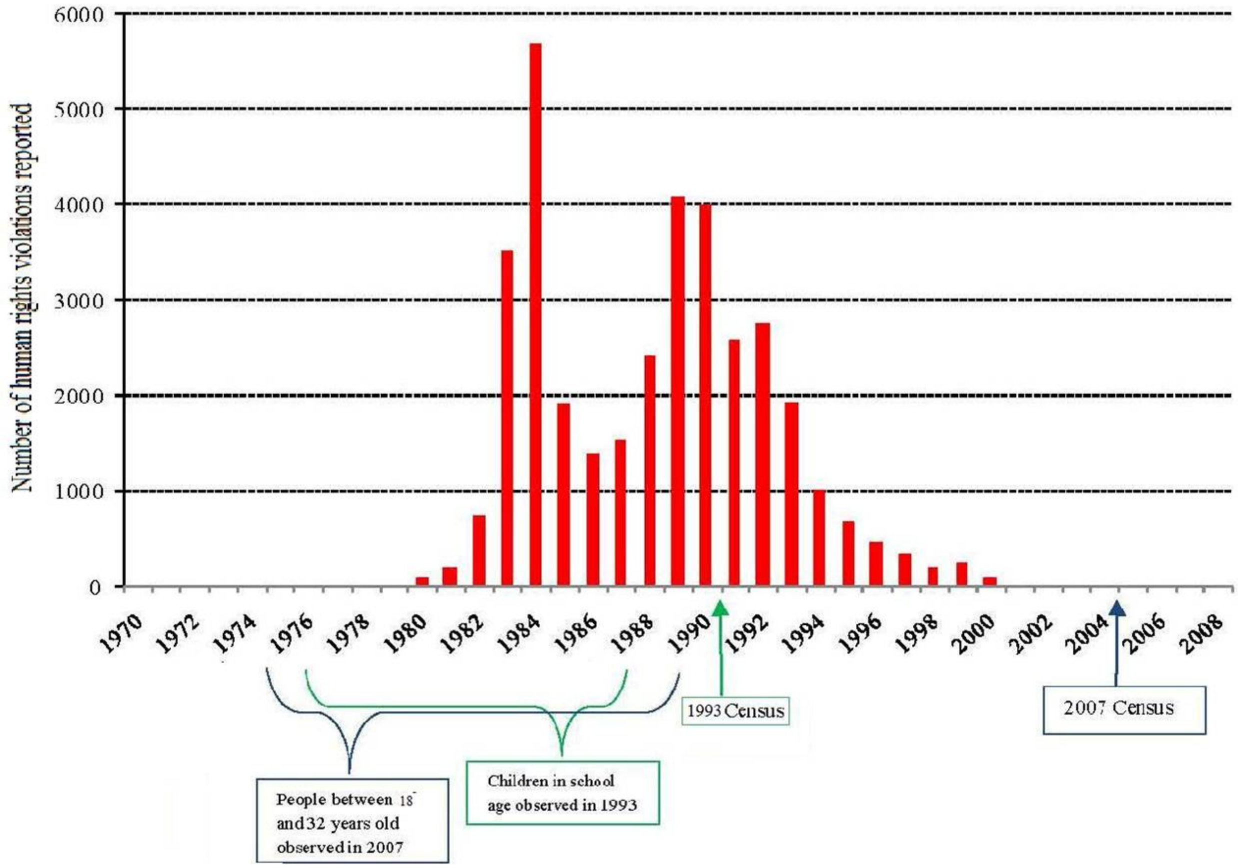
World Bank (2009). World Development Indicators. CD ROM.

Figure 1: Geographical Expansion of the Conflict: # of Fatalities Reported to the CVR, by District



Source: CVR (2004).

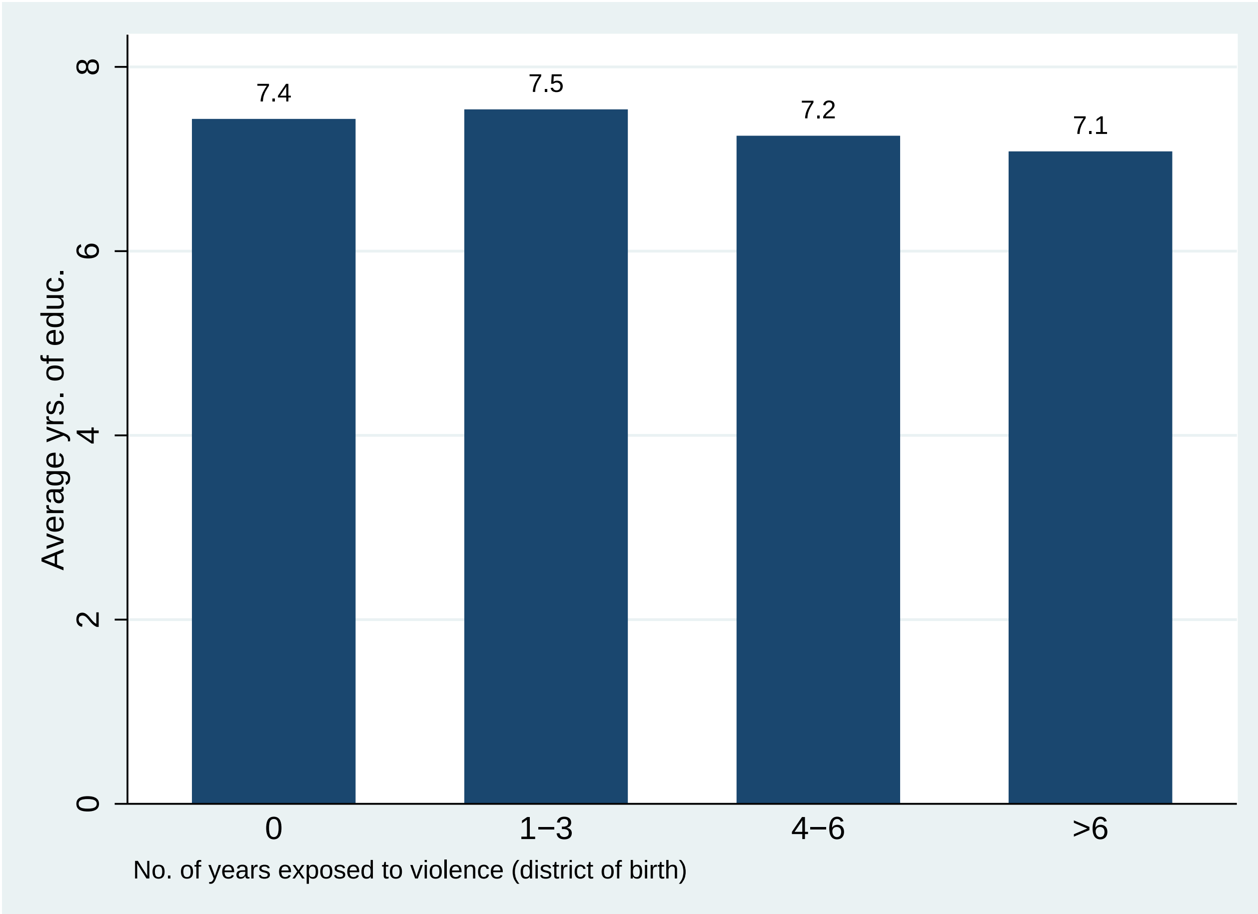
Figure 2: The Timing of the Conflict and Structure of the Data: # of Violent Events Reported to the CVR, by Year of Occurrence



Source: CVR, 2004.

Note: The figure shows the number of humanrights violations recorded by year, as well as the structure of the data used in the analysis. From the 2007 census, I consider all people between 18 and 32 years old (born between 1975 and 1989). The observations from the 1993 census correspond to al children in school age (born between 1976 and 1987).

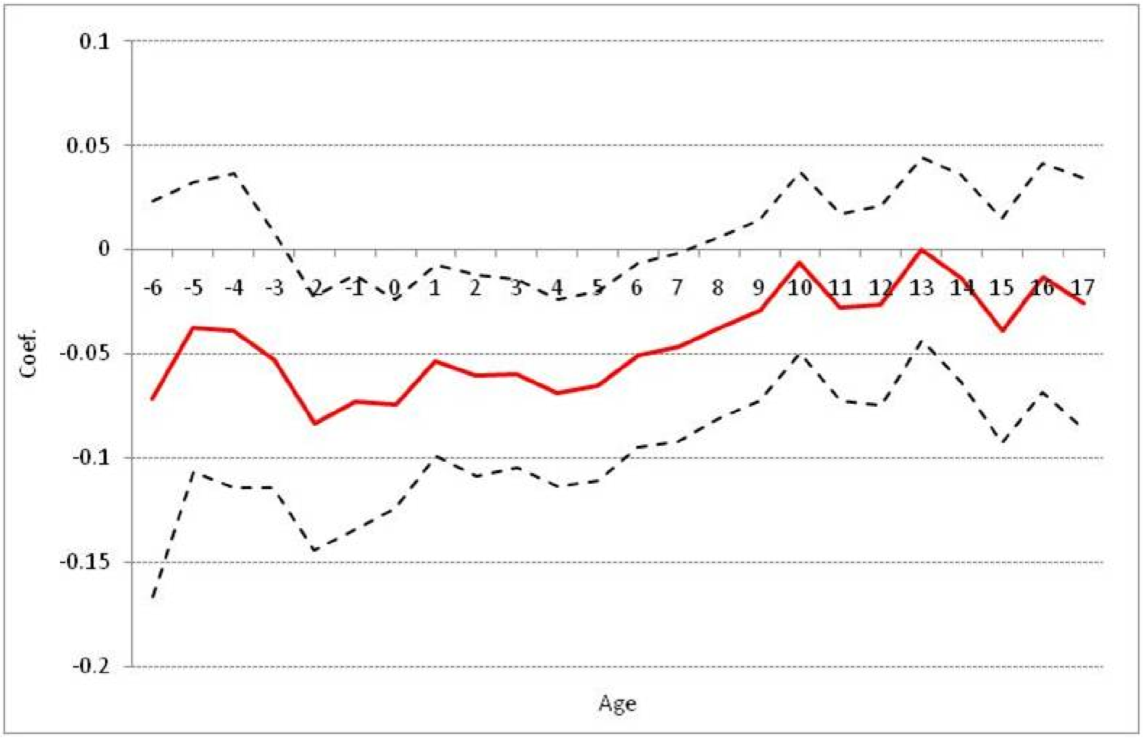
Figure 3: Pre-Violence Average years of education, by Violence exposure



Source: CVR 2004 and National Census 1993.

Notes: The Figure displays the average years of education accumulated by the cohort of people born between 1958 and 1963, who were old enough to have finished highschool by the time the violence started. The X-axis displays the number of years to which the district of birth of each individual was exposed to violence. None of the differences are statistically significant at the 5%.

Figure 4: The effect of Violence Exposure on Human Capital Acummulation, by age



Note: The figure presents the coefficients (and confidence intervals) for exposure to violence between 6 years before birth until 17 years old. The control variables included in the equation are gender, mother's language, district fixed effects, year of birth fixed effects, and a province level cubic trend.

Table 2: Victims of human rights violations, by occupation

Occupation of the victim	
Farmer	47.8
Local authorities	18.4
Sales person, trader	6.9
Housewives	5.5
Independent workers	5.2
Student	3.5
Teacher	3.4
Dependent employees	3.0
Other	2.2
Army	1.8
Manual laborer	1.6
Professionals or intelectual	0.6
Total	100.0

Source: CVR, 2004

Table 1: Summary Statistics

Variable	2007 Census					1993 Census				
	Obs.	Mean	S.d.	Min.	Max.	Obs.	Mean	S.d.	Min.	Max.
Full Sample										
Years of education	139446	9.40	2.82	0	11					
Educational deficit						75312	1.25	2.00	0	11
Gender (=1 male)	139446	0.49	0.50	0	1	75312	0.51	0.50	0	1
Mothers' language (=1 native)	139446	0.13	0.34	0	1	75312	0.21	0.41	0	1
Migrant (1=migrated)	139446	0.39	0.49	0	1					
Asset index						72675	-0.59	1.68	-2.54	11.12
No. of years exposed to violent events (early childhood)	139446	0.92	1.57	0	6	75312	0.63	1.34	0	6
No. of years exposed to violent events (pre-school age)	139446	0.82	1.09	0	3	75312	0.64	1.00	0	3
No. of years exposed to violent events (primary school age)	139446	1.70	1.97	0	6	75312	1.47	1.90	0	6
No. of years exposed to violent events (high school age)	139446	0.97	1.51	0	5					
Never exposed to violence										
Years of education	40086	8.70	3.22	0	11					
Educational deficit						31830	1.56	2.22	0	11
Gender (=1 male)	40086	0.49	0.50	0	1	31830	0.51	0.50	0	1
Mothers' language (=1 native)	40086	0.15	0.36	0	1	31830	0.20	0.40	0	1
Migrant (1=migrated)	40086	0.38	0.48	0	1					
Asset index						31001	-0.86	1.38	-2.54	10.67
No. of years exposed to violent events (early childhood)										
No. of years exposed to violent events (pre-school age)										
No. of years exposed to violent events (primary school age)										
No. of years exposed to violent events (high school age)										
Exposed to violence at least once										
Years of education	99360	9.69	2.59	0	11					
Educational deficit						43482	1.02	1.78	0	11
Gender (=1 male)	99360	0.49	0.50	0	1	43482	0.51	0.50	0	1
Mothers' language (=1 native)	99360	0.12	0.33	0	1	43482	0.22	0.41	0	1
Migrant (1=migrated)	99360	0.40	0.49	0	1					
Asset index						41674	-0.39	1.85	-2.54	11.12
No. of years exposed to violent events (early childhood)	99360	1.29	1.72	0	6	43482	1.10	1.62	0	6
No. of years exposed to violent events (pre-school age)	99360	1.15	1.13	0	3	43482	1.11	1.10	0	3
No. of years exposed to violent events (primary school age)	99360	2.39	1.95	0	6	43482	2.55	1.87	0	6
No. of years exposed to violent events (high school age)	99360	1.36	1.64	0	5					

Note: For the 2007 census, we include all people between 18 and 32 years old. People considered ever exposed to violence are those exposed to violence in any of the relevant periods of analysis: early childhood, pre-school, primary school age, or secondary school age. In the case of the 1993 census, the statistics presented are for all children in school age (6-17) who, at the moment of the interview, still lived in their birth district, and similar to those form the 2007 census, those considered affected by violence are the ones who had at least one episode of violence in their birth district during the any of the relevant periods of analysis: early childhood, pre-school or primary school age.

Table 3: Violence and Human Capital Accumulation: Long term effects

	(1)	(2)	(3)
		Years of education	
Exposed to violent events in his/her year -6		-0.052	-0.065
		(0.041)	(0.047)
Exposed to violent events in his/her year -5		-0.016	-0.030
		(0.031)	(0.034)
Exposed to violent events in his/her year -4		-0.022	-0.041
		(0.037)	(0.038)
Exposed to violent events in his/her year -3		-0.051	-0.052
		(0.029)	(0.030)
No. of years exposed to violent events (early childhood)	-0.055		-0.065
	(0.014)***		(0.016)***
No. of years exposed to violent events (pre-school age)	-0.050		-0.061
	(0.015)***		(0.016)***
No. of years exposed to violent events (primary school age)	-0.016		-0.029
	(0.014)		(0.016)*
No. of years exposed to violent events (high school age)	0.001		-0.018
	(0.013)		(0.016)
Gender (male=1)	0.438	0.437	0.437
	(0.030)***	(0.030)***	(0.030)***
Mother's language (native=1)	-1.748	-1.748	-1.747
	(0.064)***	(0.064)***	(0.064)***
Constant	-3.475	-1.146	-2.363
	(6.425)	(6.485)	(6.474)
District of birth fixed effects	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes
Province specific cubic trend	Yes	Yes	Yes
Mean dep. var.		9.40	
Observations	139446	139446	139446
R-squared	0.06	0.06	0.06

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. The sample includes all people between 18 and 32 years old interviewed in the 2007 national census.

Table 4: Violence and Human Capital Acummulation: Long term effects

	(1)	(2)	(3)
		Years of education	
Log(No. of violent event per 1,000 pop in the dist/year(t-6) of birth)		-0.00275	-0.00392
		(0.00219)	(0.00229)
Log(No. of violent event per 1,000 pop in the dist/year(t-5) of birth)		-0.00072	-0.00142
		(0.00166)	(0.00169)
Log(No. of violent event per 1,000 pop in the dist/year(t-4) of birth)		-0.00063	-0.00169
		(0.00193)	(0.00197)
Log(No. of violent event per 1,000 pop in the dist/year(t-3) of birth)		-0.00270	-0.00303
		(0.00160)	(0.00169)*
Log(No. of violent events per 1,000 pop (early childhood))	-0.00490		-0.00569
	(0.00140)***		(0.00142)***
Log(No. of violent events per 1,000 pop (pre-school age))	-0.00328		-0.00387
	(0.00120)***		(0.00121)***
Log(No. of violent events per 1,000 pop (primary school age))	-0.00222		-0.00258
	(0.00154)		(0.00154)*
Log(No. of violent events per 1,000 pop (high school age))	-0.00173		-0.00229
	(0.00142)		(0.00143)
Gender (male=1)	0.43736	0.43731	0.43727
	(0.03035)***	(0.03036)***	(0.03037)***
Mother's language (native=1)	-1.74828	-1.74747	-1.74758
	(0.06389)***	(0.06390)***	(0.06386)***
Constant	-3.45215	-1.36433	-3.29125
	(6.50500)	(6.47717)	(6.51066)
District of birth fixed effects	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes
Province specific cubic trend	Yes	Yes	Yes
Mean dep. var.		9.40	
Observations	139446	139446	139446
R-squared	0.06	0.06	0.06

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. The sample includes all people between 18 and 32 years old interviewed in the 2007 national census.

Table 6: Violence and Human Capital, by Migration Status

	(1)	(2)	(3)
		Years of education	
	Full Sample	Non-Migrants	Migrants
No. of years exposed to violent events (early childhood)	-0.055	-0.062	-0.046
	(0.014)***	(0.018)***	(0.019)**
No. of years exposed to violent events (pre-school age)	-0.050	-0.052	-0.059
	(0.015)***	(0.018)***	(0.022)***
No. of years exposed to violent events (primary school age)	-0.016	-0.020	-0.019
	(0.014)	(0.018)	(0.021)
No. of years exposed to violent events (high school age)	0.001	-0.008	0.006
	(0.013)	(0.017)	(0.020)
Gender (male=1)	0.438	0.485	0.394
	(0.030)***	(0.040)***	(0.028)***
Mother's language (native=1)	-1.748	-1.910	-1.152
	(0.064)***	(0.086)***	(0.061)***
Constant	-3.475	5.136	-11.775
	(6.425)	(8.728)	(8.289)
District of birth fixed effects	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes
Province specific cubic trend	Yes	Yes	Yes
Mean dep. var.	9.40	9.20	9.72
Observations	139446	84884	54562
R-squared	0.06	0.07	0.05

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. The sample includes all people between 17 and 32 years old interviewed in the 2007 national census.

Table 5: Violence and Human Capital by Gender and Race

	(1)	(2)	(3)	(4)
	Years of education			
	Women	Men	Native speakers	Spanish speakers
No. of years exposed to violent events (early childhood)	-0.067	-0.049	-0.067	-0.044
	(0.020)***	(0.015)***	(0.048)	(0.013)***
No. of years exposed to violent events (pre-school age)	-0.079	-0.017	-0.065	-0.041
	(0.023)***	(0.017)	(0.058)	(0.015)***
No. of years exposed to violent events (primary school age)	-0.005	-0.025	-0.037	-0.005
	(0.019)	(0.017)	(0.051)	(0.014)
No. of years exposed to violent events (high school age)	-0.000	0.005	-0.041	0.006
	(0.019)	(0.017)	(0.057)	(0.012)
Mother's language (native=1)	-2.237	-1.213		
	(0.081)***	(0.065)***		
Gender (male=1)			1.656	0.256
			(0.054)***	(0.025)***
Constant	-14.978	7.616	-30.596	-0.299
	(9.677)	(8.845)	(29.055)	(5.985)
District of birth fixed effects	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes
Province specific cubic trend	Yes	Yes	Yes	Yes
Mean dep. var.	9.19	9.63	7.74	9.65
Observations	71412	68034	18287	121159
R-squared	0.07	0.04	0.12	0.02

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. The sample includes all people between 18 and 32 years old interviewed in the 2007 national census.

Table 7: Violence and Human Capital: Short term effects

	(1)	(2)	(3)	(4)
	Educational deficit			
No. of years exposed to violent events (early childhood)	0.114	0.111	0.116	0.080
	(0.024)***	(0.024)***	(0.026)***	(0.027)***
No. of years exposed to violent events (pre-school age)	0.115	0.113	0.127	0.104
	(0.027)***	(0.027)***	(0.027)***	(0.028)***
No. of years exposed to violent events (primary school age)	0.099	0.101	0.104	0.098
	(0.023)***	(0.023)***	(0.022)***	(0.023)***
Teacher was a victim (Early childhood)				0.078
				(0.122)
Teacher was a victim (pre-school age)				-0.028
				(0.119)
Teacher was a victim (primary school age)				0.164
				(0.079)**
No. of years exposed to violent events (early childhood)*Teacher				0.071
				(0.034)**
No. of years exposed to violent events (pre-school age)*Teacher				0.084
				(0.044)*
No. of years exposed to violent events (primary school age)*Teacher				-0.029
				(0.020)
Asset index		-0.170		
		(0.011)***		
Gender (male=1)	-0.146	-0.147	-0.139	-0.139
	(0.016)***	(0.016)***	(0.017)***	(0.017)***
Mother's language (native=1)	0.891	0.810		
	(0.043)***	(0.043)***		
Constant	35.446	35.272	22.990	22.444
	(20.131)*	(21.076)*	(16.697)	(16.427)
Household fixed effects	No	No	Yes	Yes
District of birth fixed effects	Yes	Yes	No	No
Year of birth fixed effects	Yes	Yes	Yes	Yes
Province specific cubic trend	Yes	Yes	Yes	Yes
Mean dependent variable	1.25			
Observations	75312	72675	75312	75312
R-squared	0.35	0.37	0.42	0.42

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. The sample includes all people in school age (6-17) who still live in their birth district, interviewed in the 1993 national census. The dependent variable, educational deficit, is defined as: Age - Mandatory age to enter school (6) - Years of education completed.

Table 8: Violence and Human Capital: Short term effects

	(1)	(2)	(3)
		Educational deficit	
Log(No. of violent events per 1,000 pop (early childhood))	0.00692	0.00672	0.00637
	(0.00184)***	(0.00183)***	(0.00197)***
Log(No. of violent events per 1,000 pop (pre-school age))	0.00451	0.00467	0.00475
	(0.00174)***	(0.00174)***	(0.00168)***
Log(No. of violent events per 1,000 pop (primary school age))	0.00703	0.00706	0.00745
	(0.00187)***	(0.00187)***	(0.00188)***
Asset index		-0.17026	
		(0.01078)***	
Gender (male=1)	-0.14577	-0.14622	-0.13888
	(0.01634)***	(0.01644)***	(0.01674)***
Mother's language (native=1)	0.89002	0.80976	
	(0.04257)***	(0.04278)***	
Constant	37.79950	37.56215	25.15553
	(19.45622)*	(20.28895)*	(15.39673)
Household fixed effects	No	No	Yes
District of birth fixed effects	Yes	Yes	No
Year of birth fixed effects	Yes	Yes	Yes
Province specific cubic trend	Yes	Yes	Yes
Mean dependent variable		1.25	
Observations	75312	72675	75312
R-squared	0.35	0.37	0.42

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. The sample includes all people in school age (6-17) who still live in their birth district, interviewed in the 1993 national census. The dependent variable, educational deficit, is defined as: Age - Mandatory age to enter school (6) - Years of education completed.