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

Gianmarco León *

gianmarco@berkeley.edu

University of California, Berkeley

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Abstract

This paper provides empirical evidence of the long- and short-term effects of exposure to political violence on human capital accumulation. Using a novel data set that registers all the violent acts and fatalities during the Peruvian civil conflict, I exploit the variation in conflict location and birth cohorts to identify the effect of the civil war on educational attainment. Conditional on being exposed to violence, the average person accumulates 0.21 less years of education as an adult. In the short-term, the effects are stronger than in the long run; these results hold when comparing children within the same household. Further, children 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 in the long run. I explore the potential causal mechanisms, finding that supply shocks delay entrance to school but don't cause lower educational achievement in the long-run. On the demand side, suggestive evidence shows that the effect on mother's health status and the subsequent effect on child health is what drives the long-run results.

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1 Introduction

Throughout the world, civil conflicts have been widespread in the post-WWII period. During the past decade, economists have started analyzing the consequences of these conflicts, with particular attention to their welfare effects. The short-run impact of civil conflicts are clearly catastrophic. However, recent analyses provide mixed evidence on the persistence of the effects of conflict on human capital accumulation.

Using data from the Peruvian civil conflict, this paper provides estimates of the effect of exposure to civil conflict on short- and long-run educational achievement, showing that the effect on human capital is not only persistent, but more so if exposure to conflict happens during certain periods of early life. The results show that the average person exposed to political violence before starting school (in early childhood and in pre-school age) accumulates 0.21 less years of education as an adult. The short-term effects are stronger than in the long-run, showing that the persistence of the shock depends on the moment in life when the child is exposed to violence. Further, the results are robust to comparisons between siblings in the same household who were exposed to violence in different periods of their lives.

Pioneering theoretical work in this area links armed conflicts and economic performance from a macroeconomic perspective.[1] These studies were followed by empirical cross-country applications, which suggest that countries see a steep decline on a variety of welfare indicators when the war ends, yet there is significant recovery in most of these dimensions.[2] Studies exploiting variation within regions of the same country arrive at similar conclusions.[3] However, as Blattman and Miguel (2009) point out, beyond the trends revealed by cross-country evidence, it is hard to draw conclusions about how does violence affects individual and household welfare, for which we need detailed country analysis.

[1] See: Collier and Hoeffler 1998, Collier (1999), Collier and Sambanis 2005, among others

[2] 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.

[3] 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, Garretsen 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 further in unveiling the relationship between individual welfare and civil conflict. The research in this area has focused on the immediate effects of conflict on health and educational outcomes. Using micro data, exploiting the variations in geographical incidence of conflicts, and using different cohorts as control groups, several authors have found that there are economically and statistically significant effects of exposure to violence on education and health outcomes.[4] 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, while neglecting the fact that these effects will disappear as time goes by.

This paper contributes to the literature relating civil conflict and human welfare in several respects. First, I provide the first micro-estimates in the literature about the short- and long term effects of civil conflict on educational attainment, showing that the effects of violence are persistent in time. Second, I use a high quality data set representative at the national level which contains the universe of human rights violations reported during the civil conflict in Perú across districts and years. Further, the structure of the data allows me to estimate the short-term effects of violence by comparing siblings exposed to conflict at different stages of their lives. Finally, using alternative data sets, I determine whether supply or demand shocks are more relevant for the persistent effect of violence exposure on adult educational attainment.

[4] Akresh and de Walque (2010) 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 (2010) 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 lower height for age z-scores. Using a similar research design, Akresh, Bundervoet and Verwimp (2010), 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. Arcand and Wouabe (2009) analyze the Angolan 27-year long civil conflict, finding that in the short-run, conflict intensity worsens child health, does not significantly affect household expenditures, increases school enrollment and decreases fertility, as would be predicted by a Neoclassical unitary household model. The long term impacts found in this study are significantly different from those documented for the short term. In one of the only studies that is able to identify the impact of a direct exposure to violence (either by being abducted or directly affected) on education and labor market outcomes, Annan and Blattman (2010) find that educational losses are closely associated with time abducted, while those reporting the most psychological distress have been exposed to the most severe war violence and are disproportionately, but not only, former combatants. 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. Likewise, Swee (2009) finds that living in a municipality exposed to the Serbian-Bosnian conflict decreases the likelihood of completing secondary education. In Latin America, Camacho (2009) shows that women exposure to the Colombian conflict during pregnancy causes children to be born with lower weight.

My identification strategy exploits the variation in the temporal and geographical incidence of the conflict, relying on a large set of geographic and time fixed effects, along with province specific time trends. After partialing out district and year specific variation, I argue that the incidence of violence is not correlated with any determinant of educational achievement: the geographical and temporal expansion of the conflict followed clear political and strategic guidelines from the rebel group, taking the war from rural areas in the highlands to the rich coastal districts (to try to control Lima, the capital city), and the coca region in the jungle (to secure sources of financing).

The results show that the average person exposed to political violence before starting school (in the early childhood, and pre-school age) accumulates about 0.21 less years of education as an adult. This effect is more important for women than for men, and for indigenous language speakers than for Spanish speakers. The short-term effects are found to be stronger than the long-run, showing that the persistence of the shock depends on the moment in life in which the child was exposed to violence. The effects on educational achievement are also observed on labor market outcomes.

Seen through the lens of a classic education production function model, the evidence suggests that the exposure to violence affects adult human capital accumulation through both a supply and a demand side effect. On the supply side, having a teacher killed in the community has a strong impact on educational deficit by delaying school entrance. However, this effect does not have a long-term impact on educational attainment. On the demand side, suggestive evidence shows that the effect observed is not explained by short- or long-term shocks on household's wealth, but I observe a persistent decrease on mothers' health status after a violence shock, which translates into lowering childrens' health.

Overall, the results in this paper reinforce the idea that shocks during the early stages of one's life have long term irreversible consequences on human welfare. Further, relief efforts in the case of violence shocks should be targeted first to pregnant mothers and young children, and then to children in the early stages of their schooling cycle if we want to minimize the long term welfare losses for society.

2 Historical Overview and the Data

2.1 The Civil Conflict in Perú.

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) estimates that this conflict caused the death of about 69,290 people (CVR, 2004), making the Peruvian case one of the longest and most brutal political conflicts in Latin America.

Towards the end of the 1970's, Perú was about to return to democracy after a 12-year military dictatorship. On the night before the national elections, on May 17^{th} 1980 the PCP-SL made its first symbolic attack: a group of five hooded men broke in to the voter registration office in the district of Chuschi, Ayacucho and burned the ballot boxes and the registry. No injuries were reported, but on that day the PCP-SL formally declared the war to the Peruvian state (CVR, 2004).[5]

Between 1970 and 1992, Perú experienced a deep economic collapse.[6] This economic decline hit peasants in the rural highlands particularly hard, making regional inequalities even worse. For example, in the southern highlands (where the PCP-SL emerged) the infant mortality rate was 128/1000 births, while nationwide it was about 92/1000; more than 80% of the population in the area lack access to drinking water, and the ratio of people per doctor was astronomically high (17,000 per doctor), while the nationwide ratio was 1,255 (Weinstein, 2007).

During that time, education was expanding,[7] while employment opportunities for educated individuals (and especially for university graduates), was stagnant: university enrollment more than doubled from 1970 to 1990 (from 19% to 40%), while the unemployment rate for university graduates in the early 1990's was more than double relative to that of other levels of education (McClintock, 1998). This expansion of the educational sector, 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 *"status inconsistency"* as the main breeding ground in which the PCP-SL was able to spread its ideas and gain supporters among poor peasants in the highlands and urban

[5] 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.

[6] The average annual growth rate during the 1970's was 0.7%, and it declined to -3.2% in the 1980's.

[7] 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 by the early 1980's.

marginal population.[8]

The armed conflict started in the department of Ayacucho, in the south-central highlands, where most of the activity of the PCP-SL was concentrated between 1980 and 1982, as shown in Figure 1. The political strategy of the PCP-SL was inspired by the Chinese revolution and consisted of war advancing from the rural areas to the cities.[9] The organization of the movement was highly decentralized, with regional committees formed of a very small number of trained cadres and financed by local resources. The main focus in the early stages of the revolution was on political activity: agitation, mobilization, and popular education. Violence was only used to impose discipline in PCP-SL communities: militants identified the most hated members of the community, and implemented "popular justice". As the war progressed, violence extended to executing all representatives of the establishment. As Figure 2 shows, there were two clear peaks in violent activities. The first one started in 1983, when the government launched their anti-terrorist activities. The second period of intense violence was triggered by the decision of the central committee of the PCP-SL in its first congress (1988) to prioritize the war in the cities (Weinstein, 2007 and CVR, 2004).

Among the victims of the PCP-SL attacks, we find popular leaders and land holders. However, the civil population was also severely threatened by the terrorists: whenever a village declared itself against the revolution, it was brutally punished. Moreover, victims from roadside attacks for collection of supplies and food for the revolutionary army were mostly traders and farmers. Attacks on the civil population were a common episode during the war, as can be seen in Table 2.[10] Public infrastructure was a frequent target of the attacks; unfortunately, my data set does not capture these episodes since it only focuses on human rights violations. For my purposes, it is important to note that school infrastructure was not hit by either of the parties involved in the conflict.

In September 1992, when the violence in the country was at its peak and the attacks in the cities were frequent, the head of the revolutionary army, together with most of the central committee of the party, were captured and incarcerated. From that point on, violent attacks from the PCP-SL

[8]On average, the areas where the PCP-SL started their actions had a higher educational level than other areas of the highlands. As a robustness check for my results, I also run all the analysis excluding these areas (see Table 14). The main insights of the paper remain unchanged if we do this.

[9]The geographical evolution of the war is shown in Figure 1, from which we can clearly distinguish the expansion pattern towards Lima (the capital), as well as the higher incidence of violence in the coca area.

[10]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.

decreased significantly and its power within the country was under control.[11]

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,[12] out of which 54% were attributed to 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 used in this paper corresponds to illegal detentions, kidnapping, murder, extra judiciary executions, torture, or rapes. On the other hand, individual level records from the 2007 and 1993 census allows me to identify the year and district of birth of each individual. I can merge the violence data with the census data, thus identifying the number of human rights violations that took place in the district of birth during the year when each person was born, as well as in every year before and after birth.[13]

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 which agents were responsible for human rights violations that took place in that period.[14] One of the main tasks of the CVR was to travel around the country holding public hearings during which they gathered testimonies from victims, relatives, witnesses, and survivors to report any act of violence between 1990 and 2000.[15] 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,

[11]Even though after the capture of Abimael Guzman, reports of human rights violations are still 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.

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

[13]In later sections, I also merge the violence information with data from the 2007 National Household Survey (ENAHO), and the 1992 DHS using a similar aggregation level.

[14]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.

[15]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.

MRTA, etc.), identity of the victim and individual characteristics.[16] Importantly, the reported occupation of the victims allows me to identify whether a teacher was a victim of violence in a particular year and district, which will be helpful when trying to pin down the causal channels of the observed effect. It is important to note that the data set only includes human rights violations and not attacks to public infrastructure, hence 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. In both rounds of the census people were asked to provide basic demographic information, language, educational attainment and the year and district of birth. Therefore, my final data set is at the individual level, and includes individual information, as well as variables recording the number of human rights violations in each year of the individual's life in his/her district of birth.

The outcome of interest is educational achievement. To measure the long term effects of violence, my outcome variable will be defined as the number of the years of primary and secondary schooling accumulated during one's lifetime.[17] This effect can only be measured on a group of people who are observed after the period of violence was over and who are old enough to have finished their schooling cycle. Therefore, I use the information collected in 2007, and restrict my analysis to people who were at least 18 years old at the time of the interview. Also, in order to have a suitable control group, I include people who were born in a period without violence (after 1975). Figure 2 explains the time-line of the conflict intensity and the overlap periods with the individual level sample.

On the other hand, when analyzing the short term effects of violence I use the information gathered in the 1993 census, which was applied right at the point when political violence started its decline. In this case, people exposed to the political violence are mostly of school age, and the

[16]The data gathered from this process were 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, 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.

[17]For this reason, I truncate the education variable at 11 years, which corresponds to the completion of the secondary schooling cycle in Perú. The main results from the paper are unchanged if the dependent variable is not truncated.

number of years of education is not an accurate measure of human capital accumulation. Hence, I use the educational deficit as my outcome of interest. This variable measures how far behind the child is falling with respect to the mandatory age of school entrance and normal progress.[18] The population included in the sample is children of school age (6-17).

The main independent variable will be the number of years of exposure to violence during different stages of the early life in the district of birth. The stages of life that I consider are: early childhood (-2 until 3 years old), pre-school (4 to 6 years old), primary school age (7 to 12 years old), and high school age (13 to 17 years old).[19] Similarly, I will also consider the number of human rights violations at each stage, to get an estimate of the effects of the intensity of violence.

Table 1 presents the descriptive statistics of the main variables used in the analysis, by violence exposure status. On average, people in the 2007 sample have about 9.4 years of primary and secondary education (out of 11). Further, when I split the sample by violence exposure status, people who were ever exposed to violence in the relevant period in their districts have, on average, one more year of education (9.7) when compared to those whose birth district was never exposed to violence while they were children (8.7 years). Likewise, when I look at the educational deficit among school aged children in the 1993 census, those who were exposed to violence in their birth districts are 1.02 years behind in school, while children who had no experience with war fall back in school by an additional 0.54 year of school. This evidence is consistent with the *"status inconsistency"* argument presented in the previous section, according to which violence arises in areas where the population is more educated and thus more frustrated by the poor economy. All covariates shown are balanced between people born in violent compared to non-violent districts.

3 Theoretical Framework and Empirical Strategy

Consider a typical education production function model in the spirit of those discussed in Hanusheck (1979), where the stock of education (S_t) for an individual in period t is a function of her endowments in each period ($E_1, ..., E_t$), the history of educational inputs to which she had access ($N_1, ..., N_t$),

[18] 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.

[19] In the early childhood period I choose to include the second year before the child was born, which reflect the fact that violence shocks have an effect on household welfare, which in turn affects mother's health status during pregnancy, therefore negatively affecting their children. This is consistent with Camacho (2009). Further evidence on this is presented in the last section of the paper.

factors related to the (time-invariant) demographic characteristics X (i.e. gender, language), and community characteristics $(C_1, ..., C_t)$.

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

The endowment at each period of time, $E_1, ..., E_t$, is determined by both, demand and supply side factors. Among the former, there are genetic factors (G), household's endowments (E_0^h), and environmental experiences and conditions at the start of each period (V_t). The supply side factors to be considered are denoted by C_t, and one can think of them as school supply, or number of teachers available in the community:

$$E_t = g(G, E_0^h, V_t, C_t) \tag{2}$$

The date and location of birth jointly determine the exposure of any given child to the violence. Hence, 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 reduced form equation to be estimated directly follows equations (1) and (2):

$$S_{ijt} = \alpha + \sum_t \beta_t Violence_{jt} + \gamma_p Trend_t + \delta X_i + \eta_j + \nu_t + \epsilon_{ijt} \tag{3}$$

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_i is a vector of individual time invariant characteristics, such as gender, or ethnicity.

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} is 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.

From equation (3), the focus is 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), 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 ($E_1, ..., E_t$) 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 through 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 s/he is born is through traumatic experiences that affect the mothers 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 (Grimard and Lazlo, 2010).

Violence could also affect the community educational resources (C_0), as in the Sierra Leonean and Ugandan cases (Bellows and Miguel, 2009, Annan and Blattman, 2010). However, the destruction of educational infrastructure during the conflict by any of the parties involved was not an issue in Perú:

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. Sadly, a consistent series of information about the number of schools, or the number of teachers at the district level is not available. As suggestive evidence, Figure 3 shows the pattern in public expenditures in education during the period 1970-1997. It is clear from the figure that despite the small dip in expenditures between 1983-85, there is an increasing trend in the public expenditures in the educational sector. Since the influence of the rebels on the teachers was known, one channel through which violence affected C_t was the capture or even murder of the local teacher on the hands of the national army: 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.

4 Results and Discussion

4.1 Long term consequences of the conflict

My main goal is to estimate the causal impact of exposure to violence on schooling. In order to do so, I use a large set of district and year fixed effects, as well as province specific time trends. 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 absorb any shock common to all households with children born in the same year. The flexible province-specific trends included in all the regressions account for the differential developments of each province of the country through time. One 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 of province flexible trends. Further, I consider the estimates presented as conservative, since the district fixed effects are eliminating some valuable cross-sectional variation in the violence data.

The main identifying assumption needed 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 districts affected by violence were also those with lower growth of educational achievement. However, as explained in Section 2, and illustrated in Table 1, one of the main breeding grounds for the PCP-SL to get popular support was the *"status inconsistency"*, that is, the localities where the PCP-SL started where those with relatively high levels of education and low opportunities for social mobility. For my 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 education.

One way of checking if there is a selection problem in my sample is to compare pre-violence levels and trends of education between the districts that were affected by violence, and those that are used as controls. In the 1993 census I can compare 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. Panel A of Table 3 shows the average years of education of the cohort of people who were between 17 and 22 in 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). Likewise, those born in districts with higher levels of exposure have about 7.3 years of education. None of the differences between these groups of districts are statistically significant, and the same pattern holds for previous cohorts. Further, not only are the pre-violence levels of education are balanced, but cohort differences are as well. Panel B in Table 3 shows the difference in educational attainment between different cohorts, across districts with different levels of violence exposure. People born in a non-violent district were not increasing their educational level significantly more than those born in violent districts.[20]

Table 4 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 (-2 to 3), pre-school (4 to 6), primary

[20] As an additional test for the identification assumption I also run regressions to see whether pre-war education levels (or cohort differences) predict the intensity of violence, finding a positive correlation between pre-war educational attainment and the intensity of violence, which reinforces the arguments above. The results are ommitted, but are available upon request.

school (7 to 12), and secondary school age (13 to 17).[21] Being exposed to violence before entering school, this is, during the early childhood or in pre-school age has a statistically and economically significant effect on long-run human capital accumulation. As shown in column (1) in Table 4, an additional year of exposure to violence between two years before the birth of a child and the time s/he was three years of age implies that s/he will accumulate 0.06 less years of education, while being exposed to a similar shock during pre-school age (4 to 6), leads to about 0.05 less years of education per each year of exposure to violence.[22] 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 long run educational achievement. Further, I 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, I do not find any statistically significant effect for these variables.[23]

One potential concern with the results shown in Column (1) is that the time-series correlation in the exposure of violence might be affecting my estimates. One way to indirectly test this is to include in the same regression the indicator variables for the violence exposure before birth, as well as those 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.[24] The coefficients associated with violence in the critical period of early life (two years before birth until 3 years old), and pre-school years (ages 4 through 6) are still significant at the conventional levels and their magnitude is slightly increased compared to those shown in column (1).

On average each child who was ever affected by violence during early childhood, and in the pre-school years had about 2.6 and 1.9 years of exposure, respectively. This means that the average

[21] The results are robust of the choice of the years grouped together. 4 show the results year by year. The inclusion of the years before birth in the "early childhood" period reflects the fact that violence shocks can have persistent effects on the mother's health status, which would negatively impact children's development. Evidence along these lines is provided in the last section of the paper.

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

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

[24] I test whether the coefficients of violence exposure before two years before birth are jointly statistically significant. An F-test fails to reject the null that all the coefficients are statistically different from zero at the 10% in column (2) and column (3) of Table 4.

child who was exposed to violence in his/her early childhood accumulated about 0.14 less years of schooling than his/her peers in peaceful districts, or born in peaceful years; while if s/he was exposed to violence between 4 and 6 years old, the effect for the average child exposed to violence is 0.09.[25] Moreover, I 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 4, violence exposure before 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.[26]

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. In the context of war exposure, Akresh and de Walque (2010) found that four years after the Rwandan genocide children (directly) exposed to violence accumulate 0.5 less years of primary education.

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 6 replicates the regressions presented in Table 4 replacing the variables used in equation (3) with the number of violent events reported per 1,000 inhabitants living in the district in 1993.[27] 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. The coefficients are not statistically significant from each other, and all of them are between 0.003 and 0.005.

The observed effect of educational achievement also has a long term correlate on labor market outcomes. Using data from the 2007 national household survey (ENAHO), and relying on a similar

[25] 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.

[26] As a robustness check, I run the same specification excluding regions of the country that had a particularly high and continuous presence of violence (Ayacucho and Huancavelica), those that were close to the coca producing areas (Huanuco and San Martin), and the capital city (Lima), which is significantly more urban and rich than the rest of the country. The results are very similar, and are shown in Table 14.

[27] Sadly, yearly information about the population per district is not available for each year. If there was higher migration out of the violent districts, the results shown in this table are be biased upwards. This bias does not affect the results of the previous table.

specification strategy, Table 5 shows the effect of being exposed to violence on the probability of being part of the informal sector, the probability of being employed, and on labor income. In column (1), I run the same specification as the one shown in Table 4, for comparison purposes, and find a similar effect in terms of magnitude and statistical significance. However, in this case I only find that exposure to violence is relevant for shocks during the early childhood. While I don't observe that violence has an effect on informality [NB: What do you mean by informality? -JR], people who were exposed to an additional year of violence during the early childhood are 1.3% less likely to be employed. Moreover, people who have a job and had experiences with violence in their close environment earn about 2% less in the labor market.[28]

4.2 Heterogeneity of the effect

Maccini and Yang (2009) in their study on 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[29].

Following these ideas, Table 7 divides the sample by gender and ethnicity to see if there are differential impacts in these particular subgroups of the population. Consistent with the findings mentioned above, Table 7 shows that the results are mostly driven by the effect for women. The point estimates for the exposure to violence in all periods are larger for girls than those found inmybenchmark specification in the first column, 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 early childhood translates into 0.07 fewer years of education, while during the pre-school period it means about 0.08 less education as adult women. On the other hand, for men only exposure to violence during early childhood seems to be an important

[28]When I split the sample by migration status and gender, I find that the effect is basically driven by people who still live in their birth district, and it is more important for men than for women. These results are available upon request of the interested reader. Galdo (2010) provides estimates of the effects of violence on labor market outcomes that are consistent with my results, even after correcting for the attenuation bias though an IV procedure.

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

determinant of future schooling, and the coefficient is smaller in magnitude. Meanwhile, I find that the effect for indigenous language speakers is larger than for Spanish speakers; however, these results are not statistically significant due to a smaller sample size.

4.3 Potential biases and concerns

4.3.1 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, since people voluntarily approached to the public hearings to tell their stories. 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.

It is plausible that the under-reporting present in the data is coming from the group that was more affected by violence, i.e. those who had a harder time verbalizing the incidents in front of the Commission. Further, the testimonies were collected in relatively big towns, which implies that some of the most vulnerable populations (for whom the opportunity cost of reporting the violence were higher) were not able to report human rights violations. This possible selective under-reporting of the violence data is likely to lead to an under-estimation of my results. Hence, the point estimates found have to be interpreted as a lower bound. In any case, if this bias exists, it is more likely to show up in the regressions where I use the number of human rights violations, as opposed to those in which I include the number of years exposed to violence in each period. In the regressions using years exposed to violence, 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.[30]

Another possible bias in my result may come from the fact that the fatality victims of violence are not on the census. However, these people were those most affected by violence. Hence, the selection problem induced by the fatal victims again introduces a negative bias in the estimation of the effects of violence.

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

4.3.2 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. I do not observe the actual migration history, or the reasons to leave one's hometown. The bias implied by the migration history cannot be signed a priori.

There is anecdotal evidence that people who migrated from violent areas were discriminated against in larger cities, denying them 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 are likely to go to larger 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 the results: if people who were able to escape from the violent districts were those at the top end of the income distribution, had they stayed, the effect on their human capital accumulation would have been higher. If that were the case, the estimates in Tables 4 and 7 would be overstating the impact of violence.

There is no clear way to disentangle the sources of migration bias, other than to address the question empirically. One indirect way to deal with this issue is to restrict the sample to people who still live in their birth districts, and compare the point estimates of the original sample with those of the non-migrants. Table 8 report on the estimation of the model, 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 of violence for the non-migrants is slightly higher than 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 0.064 less years of schooling, while if they experienced violence when they were between 4 and 6 years old, they would accumulate 0.055 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.

For those who were no longer live 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 0.044 less years of education if the violence happened during the early childhood, and about 0.058 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.

In an additional robustness check, I regress the probability of migration on the incidence of violence in each year before and after birth, time invariant individual characteristics, a set of year specific effects, and a province time trend. Column (1) on Table 9 shows that violence exposure during the early childhood, or during pre-school age increases the probability of migration. However, once I include district specific intercepts in Column (2), there is no significant association between migration status and exposure to violence. These results support the idea that migration is higher in the districts affected by violence, but this migration responds to a structural, time invariant characteristic of those districts, and the timing and location of violence does not differentially affect the likelihood of migration.

Summing up, the effect observed in Tables 4 and 7 do not contain a significant migration bias, and if anything, this effect will bias the estimates downwards.

4.4 Short term effects and possible causal pathways

So far I've shown that living through violent periods during critical periods of life causes lower school achievement in the long run. This finding contrasts with other studies which document that, after suffering civil wars, countries are able to recover in most areas of development, such as nutrition, education, economic growth, etc.[31] In this section, I explore the extent to which the effect is vanishing over time by looking at the short-term impacts of political violence on schooling. Using a similar methodology as above, I estimate equation (3) on the data coming from the 1993 census. The dependent variable in this case is the educational deficit, rather than school attainment. This variable is defined for children in school age (6-17) as the difference between their age, the mandatory age for

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

entering school (6 years old in Perú), and the number of years completed in school. For example, a child who is 16 years old and has completed primary education (6 years of education) has a deficit of 4 years, since she should have been in her fourth year 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 1993 in the location where they were born.

The results are shown in column (1) of Table 10. Being exposed to violence either in early childhood, pre-school age, or primary school age has a statistically and economically significant effect on school deficit. An additional year of violence in the district of birth during any of these periods of life causes a child to fall behind in school in about 0.1 years. Considering that the children exposed to violence in this sample were exposed to 2.4, 1.8, and 2.9 years of violence in each of the periods mentioned, respectively, this implies an average effect of 0.50 more years of school deficit for the average child exposed to violence in these periods. Recall from Table 4 that, for people observed in 2007, the average child exposed to violence accumulate 0.21 less years of education.[32] Together, these results suggest that the effect of violence on human capital accumulation is mitigated as time goes by. Children who are affected by violence once they have already started their school life are able to catch up, while the ones 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). In Column (2) I use the intensity of violence as the main independent variable. The patterns mirror those observed with years of exposure.

The structure of the sample allows a tighter identification strategy. Given that the vast majority of children in school age still live in their parents' household, I could exploit the variation in the timing of violence exposure between siblings to identify the parameters of interest, keeping constant all time invariant household characteristics. Results are reported in Columns (3) and (4) of Table 10. The sibling difference model gives very similar results. Taken together, these results shed some light on the potential mechanisms that might be working behind the observed effect. The fact that the point estimates slightly increase when I account for household time invariant characteristic allow me to rule out the hypotheses that the causal pathway through which experiencing violence in the

[32] In this case, I am unable to test whether shocks during high school affect schooling outcomes, since in my sample I don't observe children who are old enough to be in high school, and have suffered violence.

environment affect educational achievement are any persistent shocks to household welfare, or other household time invariant characteristic. If that were the case, I should observe a decrease in the effect of violence exposure on educational deficit.

One potential mechanism behind the observed effect might be a supply side shock: if a teacher was directly affected by violence, it can make it harder for children to go to school. The CVR recorded the occupation of the victims,[33] thus I can directly test this hypothesis by including into my benchmark regressions a dummy variable for whether a teacher was attacked within each period of the students' life. These results are shown in Table 11. In the short term, conditional of being exposed to violence, having a teacher attacked during early childhood or pre-school age leads to an increase in the school deficit of about 0.30 and 0.17 years, respectively, as shown in columns (1) and (2). The fact that the effect is significant whenever these events happened before the child was old enough to enter school, suggests that having a teacher injured or dead in the community delayed the entrance to school. Further, when I look at the long term effects of this type of violence, I find that having a teacher attacked does not significantly affect the long term accumulation of human capital. These results suggest that violence against teachers lead to a delay in school entrance, but do not cause a lower educational achievement in the long run.

On the other hand, I can also explore whether the effect is driven by a demand side shock. A violence shock in the district can have an effect on health status, which in turn has long term irreversible cognitive effects. Using data from the 1992 DHS, and a similar specification as before, I can test whether violence exposure has an effect on the weight for height, or height for age z-scores. These results are shown in Table 12. I indeed find suggestive evidence that having a shock between two years before birth, and the first year of life has a negative effect on health status. However, the reduced sample size limits my ability to do statistical inference in this case.

One other potential mechanism through which violence exposure might affect future educational outcomes is through household's wealth, which in turn has an effect on children's cognitive development. Even though I am not able to directly test this channel, I can use the information contained in the 1992 DHS to give some suggestive evidence. In column (1) of Table 13, I run an OLS regression of an asset index (Filmer and Prichett, 2001) on whether there was violence in the district during

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

the years preceding the survey, and some relevant controls. The results suggest that violence did not differentially affect asset tenure at the household level.[34] Further, using a similar strategy, I can see if the health status of the mothers in the sample is affected by violence. Column (2) illustrates this point, showing that exposure to violence the year before the survey was done is correlated with lower body mass index of the women in reproductive age.[35]

Summarizing, I find suggestive evidence of two potential channels through which violence affects educational achievement are: (i) a supply side shocks, specifically attacks against teachers increase the educational deficit of children exposed to the shock; and (ii) on the demand side, violent events between one year before the child was born and when she was one year old decreases her health status. Finally, this effect does not seem to go through shocks to household asset tenure, but through maternal health.

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

[34] It is important to have in mind the fact that the measure of violence only represents human rights violations, and does not capture any destruction of productive infrastructure, which are the most likely determinants of household economic status.

[35] The fact that violence shocks one or two years before birth have an effect on maternal and child health speaks to the results shown in the previous section, where I observe that shocks preceding birth significantly affect educational attainment.

level are followed at the micro level, one might expect these effects to vanish over time.

In this paper, 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 about 69,290 people, as well as huge economic losses. Using a novel 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 an average person exposed to political violence during the early childhood has 0.14 less years of education as an adult, 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. This result is more important for women than for men, and for indigenous populations than for Spanish-speakers. 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 shows that the long-term effects are smaller than the short-term effects. This result contrasts with the cross-country findings that the effects of violence vanish over time. An interesting result of the paper is that children affected by violence are able to catch up if they are exposed to violence after they have already started their schooling cycle, while if they experience violence earlier in life, the effect is persistent.

Finally, I look at the potential causal channels though which this effect is working, finding suggestive evidence for two of the hypothesized mechanisms. On the supply side, attacks against teachers increase the educational deficit of children exposed to the shock, mainly by delaying school entrance, but this effect does not have a correlate in long-term educational achievement. On the demand side, violent events in the two years around birth decrease the child's health status. This effect does not seem to go through shocks to household asset tenure, but through maternal health.

Overall, the results in this paper reinforce the idea that shocks during the early stages of one's life have long term irreversible consequences on human welfare. Further, relief efforts in the case of violence shocks should be targeted first to pregnant mothers and young children, and then to children in the early stages of their schooling cycle if we want to minimize the long term welfare losses for society.

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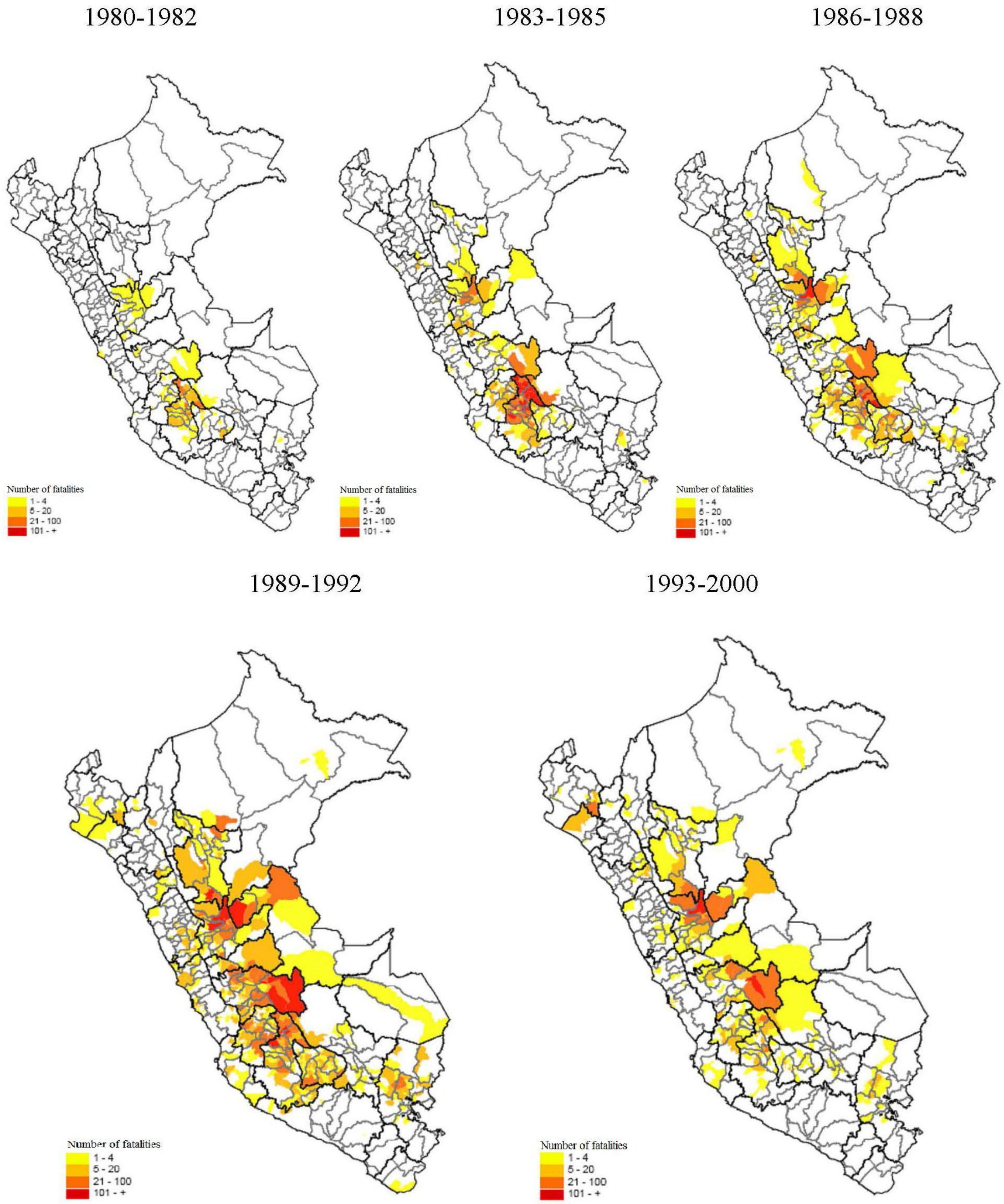
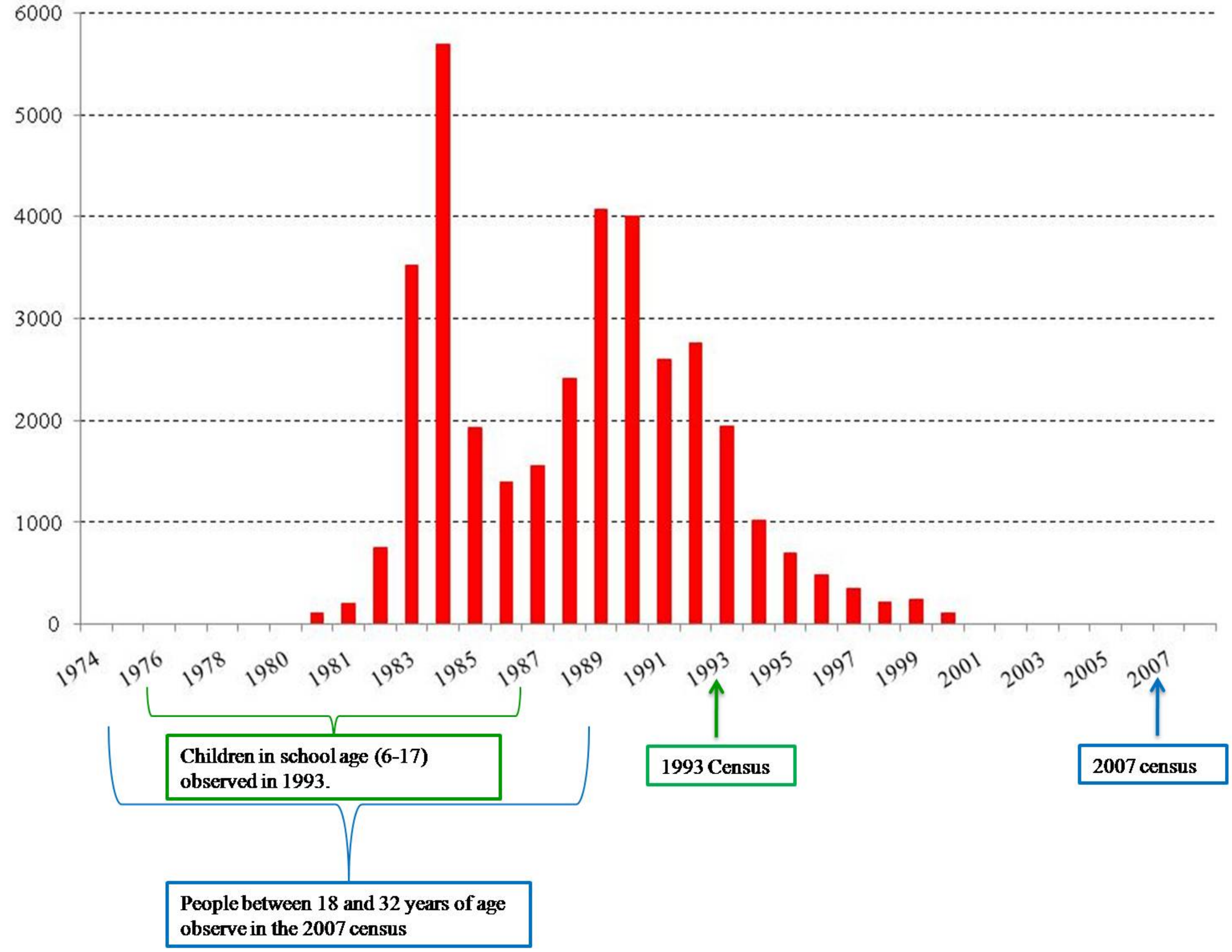
Figure 1: Geographical Expansion of the Conflict: # of Fatalities Reported to the CVR, by District



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



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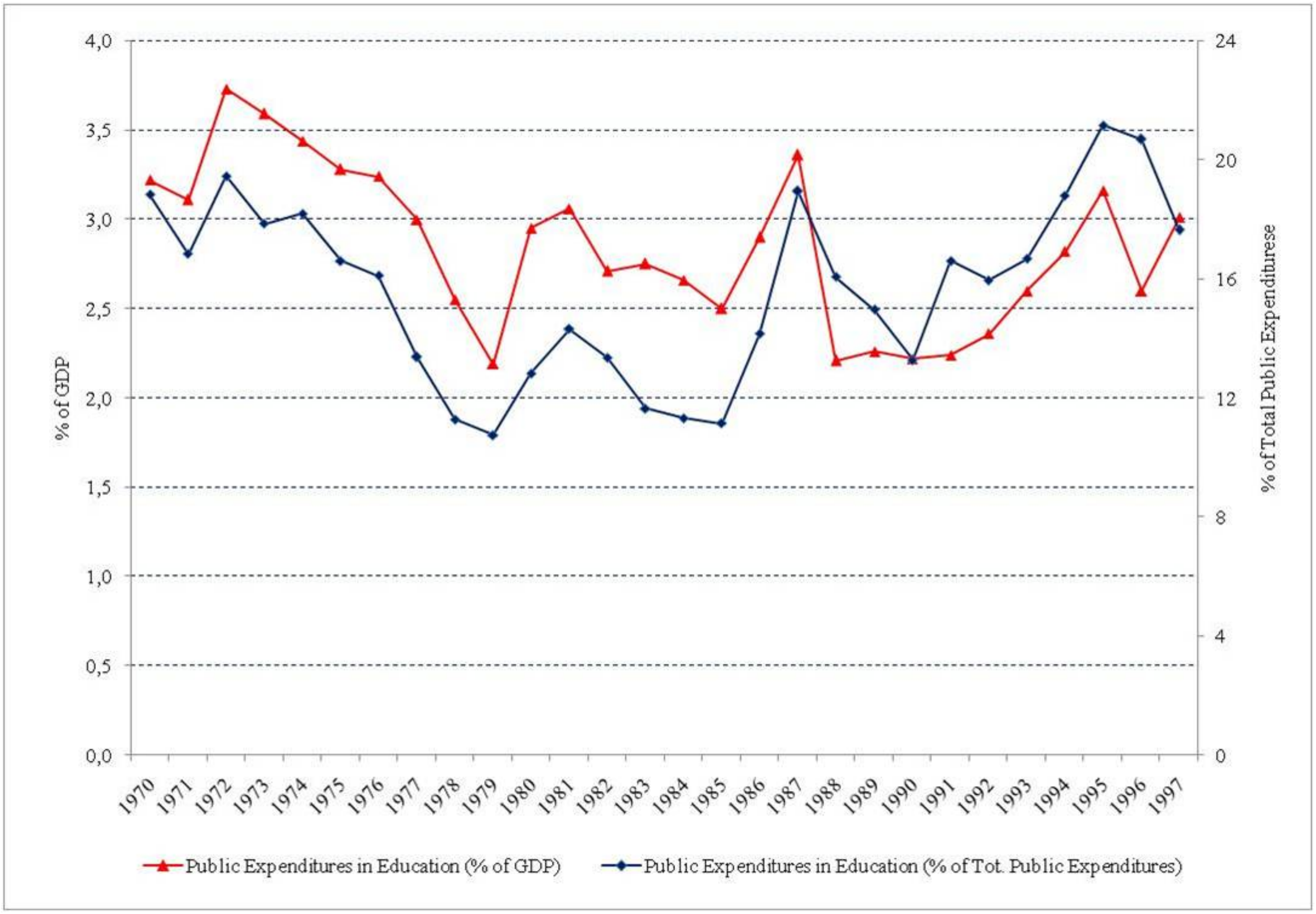
Source: CVR, 2004.

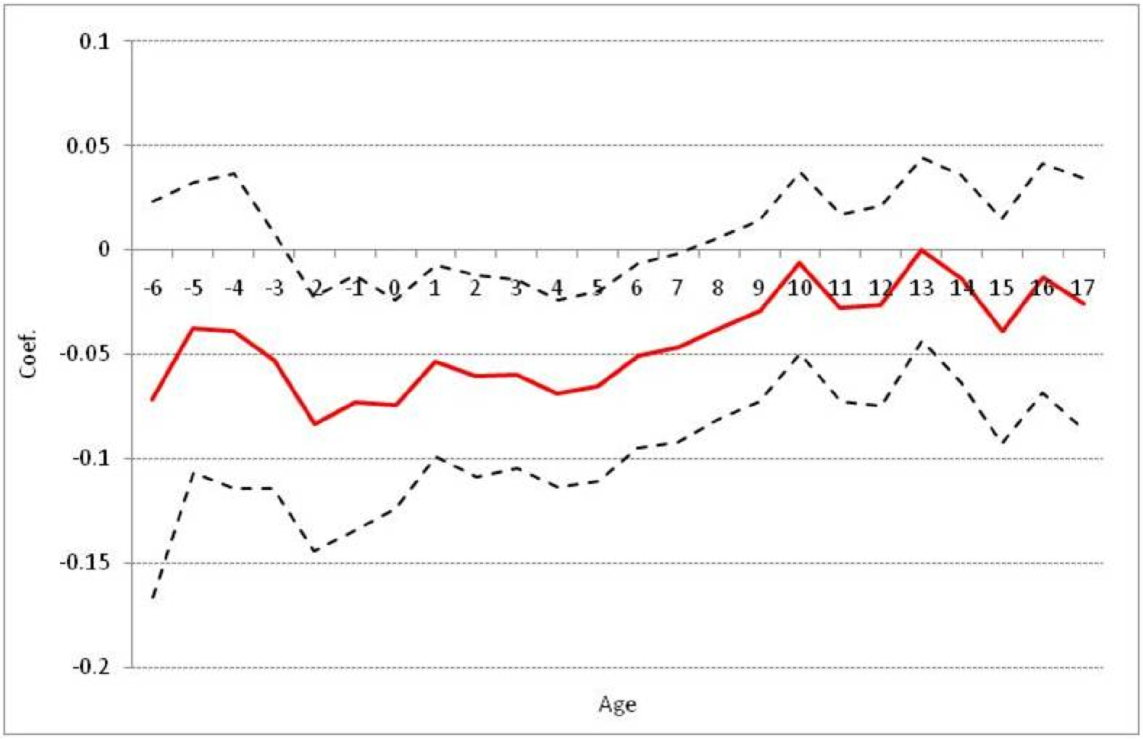
Note: The figure shows the number of human rights violations recorded by year, as well as the structure of the data used in the analysis. From the 2007 census, I consider all people between 18 and 32 years old (born between 1975 and 1989). The observations from the 1993 census correspond to all children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

Figure 3: Public Expenditures in Education 1970-1997



Source: World Bank (2001).

Figure 4: Long Term 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 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, I 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 2: Demographic Characteristics of the Victims of Human Rights Violations

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

Genderof the victim	%
Male	79,0
Female	21,0
Total	100,0
Educational level of the victim	
No education	16,4
Primary	46,5
Secondary	24,6
Higher	12,5
Total	100,0
Languageof the victim	
Native	70,9
Spanish	29,1
Total	100,0

Source: CVR, 2004.

Table 3: Pre-Violence Average Years of Education, by Violence Exposure

Panel A: Average years of education, by year of birth

No. of years exposed to violence	1958-1963	1953-1957	1948-1952
0	7.4	6.7	6.1
1 - 3	7.5	6.9	6.2
4 - 6	7.3	6.5	5.5
>6	7.0	6.3	5.4
Total	7.4	6.7	6.0

Panel B: Changes in educational attainment between cohorts

No. of years exposed to violence	[1958-1963] - [1953-1957]	[1953-1957] - [1948-1953]
0	0.70	0.65
1 - 3	0.73	0.66
4 - 6	0.76	0.96
>6	0.75	0.80
Total	0.72	0.70

Source: CVR, 2004 and and National Census 1993.

Notes: Panel A displays the average years of education by the cohort of people born between 1958-62, 1953-57, and 1948-53, who were old enough to have finished highschool by the time the violence started. Panel B shows the differences between cohorts. None of the differences by levels of exposure to violence are statistically significant.

Table 4: Violence and Human Capital Accumulation: Long Term Effects

	(1)	(2)	(3)
		Years of education	
Exposed to violent events in his/her year -6		-0.055	-0.070
		(0.041)	(0.047)
Exposed to violent events in his/her year -5		-0.019	-0.034
		(0.031)	(0.035)
Exposed to violent events in his/her year -4		-0.021	-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)	-0.056		-0.066
	(0.014)***		(0.016)***
No. of years exposed to violent events (pre-school age)	-0.052		-0.064
	(0.015)***		(0.016)***
No. of years exposed to violent events (primary school age)	-0.020		-0.032
	(0.014)		(0.016)**
No. of years exposed to violent events (high school age)	-0.000		-0.020
	(0.013)		(0.016)
Gender (male=1)	0.438	0.437	0.437
	(0.030)***	(0.030)***	(0.030)***
Mother's language (native=1)	-1.747	-1.747	-1.747
	(0.064)***	(0.064)***	(0.064)***
Constant	9.007	8.978	9.059
	(0.047)***	(0.032)***	(0.055)***
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. The periods of life considered are difined as follows: early childhood (-2 until 3 years old), pre-school (4 to 6 years old), primary school age (7 to 12 years old), and high school age (13 to 17 years old).

Table 5: Violence and Human Capital: Long Term Effects on the Labor Market

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Yrs. Of education	Work in the informal sector		Currently working		Log(Monthly wage)	
No. of years exposed to violent events	-0.047	-0.004	-0.006	-0.013	-0.013	-0.028	-0.022
(early childhood)	(0.027)*	(0.005)	(0.005)	(0.006)**	(0.006)**	(0.017)*	(0.016)
No. of years exposed to violent events	-0.004	-0.010	-0.010	-0.007	-0.007	-0.007	-0.010
(pre-school age)	(0.038)	(0.008)	(0.008)	(0.007)	(0.007)	(0.024)	(0.023)
No. of years exposed to violent events	-0.007	-0.001	-0.002	-0.006	-0.006	-0.021	-0.022
(primary school age)	(0.036)	(0.007)	(0.007)	(0.007)	(0.007)	(0.021)	(0.021)
No. of years exposed to violent events	0.015	-0.008	-0.006	-0.006	-0.005	0.032	0.032
(high school age)	(0.032)	(0.007)	(0.007)	(0.008)	(0.008)	(0.019)	(0.018)
Gender (male=1)	0.505	0.112	0.125	0.187	0.188	0.486	0.505
	(0.051)***	(0.008)***	(0.009)***	(0.007)***	(0.007)***	(0.025)***	(0.023)***
Mother's language (native=1)	-1.680	0.158	0.088	0.092	0.085	-0.362	-0.183
	(0.110)***	(0.016)***	(0.015)***	(0.015)***	(0.016)***	(0.052)***	(0.049)***
Years of education			-0.029		-0.003		0.079
			(0.001)***		(0.001)**		(0.004)***
Constant	9.461	0.299	0.630	0.774	0.803	7.065	6.020
	(1.716)***	(0.274)	(0.257)**	(0.214)***	(0.214)***	(1.031)***	(0.973)***
District of birth fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province specific cubic trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dependent Variable	9.28	0.55		0.71		6.38	
Observations	22083	22083	22083	22083	22083	12232	12232
R-squared	0.11	0.06	0.09	0.12	0.12	0.15	0.19

Source: Encuesta Nacional de Hogares 2007. * 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 2007. The dependent variable in columns (2) and (3) is defined as those workers who are not openly unemployed (work less than 35 hrs. per week), or work in a firm that is not registered, or does not keep accounting books (self reported); in columns (4) and (5), I consider occupied those workers who declare being employed, and working more than 35 hrs. per week; the log of the monthly income is taken over the labor income of those who declare being employed.

Table 6: Violence and Human Capital Acummulation: Long Term Effects

	(1)	(2)	(3)
		Years of education	
Log(No. of violent event per pop in the dist/year(t-6) of birth)		-0.00270	-0.00387
		(0.00219)	(0.00229)
Log(No. of violent event per pop in the dist/year(t-5) of birth)		-0.00073	-0.00144
		(0.00166)	(0.00170)
Log(No. of violent event per pop in the dist/year(t-4) of birth)		-0.00074	-0.00180
		(0.00192)	(0.00196)
Log(No. of violent event per pop in the dist/year(t-3) of birth)		-0.00269	-0.00360
		(0.00160)	(0.00159)*
Log (No. of violent events (early childhood)	-0.00491		-0.00570
	(0.00141)***		(0.00143)***
Log (No. of violent events (pre-school age)	-0.00330		-0.00389
	(0.00120)***		(0.00121)***
Log (No. of violent events (primary school age)	-0.00235		-0.00270
	(0.00154)		(0.00155)*
Log (No. of violent events (high school age)	-0.00178		-0.00234
	(0.00142)		(0.00143)
Gender (male=1)	0.43743	0.43742	0.43735
	(0.03036)***	(0.03037)***	(0.03038)***
Mother's language (native=1)	-1.74815	-1.74734	-1.74743
	(0.06384)***	(0.06385)***	(0.06381)***
Constant	8.74524	8.81968	8.45792
	(0.07407)***	(0.07942)***	(0.11564)***
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 7: Violence and Human Capital, by Gender and Ethnicity

	(1)	(2)	(3)	(4)	(5)
	Years of education				
	Full sample	Women	Men	Native speakers	Spanish speakers
No. of years exposed to violent events	-0.056	-0.066	-0.053	-0.070	-0.045
(early childhood)	(0.014)***	(0.020)***	(0.015)***	(0.048)	(0.013)***
No. of years exposed to violent events	-0.052	-0.080	-0.021	-0.062	-0.045
(pre-school age)	(0.015)***	(0.023)***	(0.017)	(0.058)	(0.015)***
No. of years exposed to violent events	-0.020	-0.008	-0.030	-0.043	-0.009
(primary school age)	(0.014)	(0.019)	(0.018)*	(0.051)	(0.014)
No. of years exposed to violent events	-0.000	-0.001	0.004	-0.039	0.004
(high school age)	(0.013)	(0.019)	(0.017)	(0.056)	(0.013)
Gender (male=1)	0.438			1.657	0.256
	(0.030)***			(0.054)***	(0.025)***
Mother's language (native=1)	-1.747	-2.238	-1.213		
	(0.064)***	(0.081)***	(0.065)***		
Constant	9.007	11.694	11.041	6.376	9.191
	(0.047)***	(0.150)***	(0.066)***	(0.213)***	(0.046)***
District of birth fixed effects	Yes	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes	Yes
Province specific cubic trend	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	9,40	9.19	9.63	7,74	9,65
Observations	139446	71412	68034	18287	121159
R-squared	0.06	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 8: 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 9: Migration and Exposure to Violence

	(1)	(2)
	Migration status (=1 migrant)	
Exposed to violent events in his/her year -6	0.000	-0.028
	(0.020)	(0.011)**
Exposed to violent events in his/her year -5	0.007	-0.009
	(0.014)	(0.009)
Exposed to violent events in his/her year -4	0.019	-0.001
	(0.017)	(0.008)
Exposed to violent events in his/her year -3	0.004	-0.003
	(0.015)	(0.007)
No. of years exposed to violent events (early childhood)	0.017	-0.003
	(0.006)***	(0.002)
No. of years exposed to violent events (pre-school age)	0.017	0.002
	(0.005)***	(0.003)
No. of years exposed to violent events (primary school age)	0.005	-0.001
	(0.004)	(0.003)
No. of years exposed to violent events (high school age)	0.005	-0.003
	(0.006)	(0.003)
Gender (male=1)	-0.027	-0.026
	(0.003)***	(0.003)***
Mother's language (native=1)	-0.172	-0.177
	(0.016)***	(0.017)***
Constant	0.306	0.478
	(0.010)***	(0.010)***
District of birth fixed effects	No	Yes
Year of birth fixed effects	Yes	Yes
Province specific cubic trend	Yes	Yes
Mean dep. var.	0.39	
Observations	139446	139446
R-squared	0.06	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 10: Violence and Human Capital: Short Term Effects

	(1)	(2)	(3)	(4)
	Educational deficit			
No. of years exposed to violent events (early childhood)	0.116		0.123	
	(0.024)***		(0.026)***	
No. of years exposed to violent events (pre-school age)	0.114		0.130	
	(0.028)***		(0.027)***	
No. of years exposed to violent events (primary school age)	0.097		0.106	
	(0.024)***		(0.022)***	
Log (No. of violent events (early childhood)		0.00663		0.00650
		(0.00183)***		(0.00200)***
Log (No. of violent events (pre-school age)		0.00408		0.00464
		(0.00182)**		(0.00170)***
Log (No. of violent events (primary school age)		0.00677		0.00757
		(0.00191)***		(0.00187)***
Gender (male=1)	-0.146	-0.14575	-0.139	-0.13867
	(0.016)***	(0.01634)***	(0.017)***	(0.01675)***
Mother's language (native=1)	0.890	0.88903		
	(0.043)***	(0.04257)***		
Constant	3.743	4.21852	3.727	4.23324
	(0.076)***	(0.13617)***	(0.079)***	(0.13651)***
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		1.25	
Observations	75314	75314	63888	63888
R-squared	0.35	0.35	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 11: Supply Side Shocks and Human Capital

	(1)	(2)	(3)
	Educational deficit		Years of Education
No. of years exposed to violent events (early childhood)	0.089	0.095	-0.049
	(0.026)***	(0.027)***	(0.015)***
No. of years exposed to violent events (pre-school age)	0.097	0.111	-0.046
	(0.029)***	(0.028)***	(0.016)***
No. of years exposed to violent events (primary school age)	0.091	0.099	-0.017
	(0.025)***	(0.022)***	(0.015)
No. of years exposed to violent events (high school age)			-0.002
			(0.014)
Teacher was a victim (Early childhood)	0.290	0.305	-0.037
	(0.081)***	(0.081)***	(0.048)
Teacher was a victim (pre-school age)	0.149	0.165	-0.021
	(0.071)**	(0.074)**	(0.041)
Teacher was a victim (primary school age)	0.030	0.055	0.014
	(0.057)	(0.060)	(0.031)
Teacher was a victim (high school age)			0.061
			(0.036)
Gender (male=1)	-0.146	-0.140	0.438
	(0.016)***	(0.017)***	(0.030)***
Mother's language (native=1)	0.889		-1.747
	(0.043)***		(0.064)***
Constant	3.744	3.700	8.997
	(0.075)***	(0.079)***	(0.047)***
Household fixed effects	No	Yes	No
District of birth fixed effects	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes
Province specific cubic trend	Yes	Yes	Yes
Mean dependent variable	1.25	1.25	9.4
Observations	75314	63888	139446
R-squared	0.35	0.42	0.06

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. The sample in columns (1) and (2) 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. For column (3), the sample includes all people between 18 and 32 years old interviewed in the 2007 national census.

Table 12: Demand Side Shocks and Human Capital: Child Health

	(1) Weight for age z-score	(2) Haight for age z-score
Exposed to violent events in his/her year -2	-0.064	-0.095
	(0.112)	(0.096)
Exposed to violent events in his/her year -1	-0.170	-0.144
	(0.100)*	(0.098)
Exposed to violent events in his/her year 0	0.079	0.003
	(0.122)	(0.126)
Exposed to violent events in his/her year 1	-0.034	-0.183
	(0.114)	(0.109)*
Exposed to violent events in his/her year 2	0.142	-0.089
	(0.096)	(0.101)
Exposed to violent events in his/her year 3	-0.024	-0.073
	(0.091)	(0.091)
Exposed to violent events in his/her year 4	0.122	0.078
	(0.101)	(0.093)
Gender	-0.026	-0.074
	(0.041)	(0.049)
Constant	-1.571	-7.140
	(1.128)	(1.081)***
Household fixed effects	Yes	Yes
Year of birth fixed effects	Yes	Yes
Province specific cubic trend	Yes	Yes
Observations	7696	7696
R-squared	0.27	0.33

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. The sample includes all children between zero and five years of age interviewed at the DHS 1992.

Table 13: Demand Side Shocks and Human Capital: Asset Accumulation and Mothers' Health

	(1) Household's Asset Index	(2) Mother's Body Mass index
Events 1988	-0.225	-0.070
	(0.266)	(0.202)
Events 1989	-0.215	-0.049
	(0.211)	(0.220)
Events 1990	0.070	0.256
	(0.208)	(0.217)
Events 1991	-0.224	-0.489
	(0.239)	(0.197)**
Events 1992	0.388	-0.236
	(0.267)	(0.201)
Constant	-4.040	21.342
	(0.373)***	(0.600)***
Observations	6221	2972
R-squared	0.42	0.10

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. Source: DHS 1992. Point estimates are from OLS regressions in all cases. Regression in column (1) is at the household level. Controls include age of the household head, dummies for the maximum educational level in the household, number of members of the household, and a dummy for urban areas. In column (2), the unit of observation are mothers between 14 and 49 years of age with children between zero and five years old. Controls include dummies for the educational level, age, an indicator for whether the mother is currently pregnant, number of household members, the asset index, and a dummy for urban areas.

Table 14: Robustness - Long Term Effects Excluding Different Regions

	(1) Excluding Lima	(2) Excluding Ayacucho	(3) Excluding Huancavelica	(4) Excluding Huanuco	(5) Excluding San Martin	(6) Excluding Ayacucho and Huancavelics	(7) Excluding Huanuco and San Martin
				Years of education			
No. of years exposed to violent events	-0.106	-0.057	-0.057	-0.053	-0.056	-0.058	-0.053
(early childhood)	(0.017)***	(0.014)***	(0.014)***	(0.014)***	(0.014)***	(0.014)***	(0.014)***
No. of years exposed to violent events	-0.084	-0.051	-0.054	-0.044	-0.050	-0.053	-0.042
(pre-school age)	(0.021)***	(0.015)***	(0.015)***	(0.015)***	(0.015)***	(0.015)***	(0.015)***
No. of years exposed to violent events	-0.044	-0.020	-0.020	-0.013	-0.018	-0.020	-0.011
(primary school age)	(0.020)**	(0.014)	(0.015)	(0.014)	(0.014)	(0.015)	(0.014)
No. of years exposed to violent events	-0.015	-0.004	-0.002	0.006	-0.001	-0.006	0.006
(high school age)	(0.019)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Gender (male=1)	0.574	0.422	0.412	0.418	0.441	0.395	0.420
	(0.032)***	(0.030)***	(0.030)***	(0.030)***	(0.031)***	(0.030)***	(0.031)***
Mother's language (native=1)	-1.783	-1.745	-1.760	-1.706	-1.752	-1.759	-1.711
	(0.067)***	(0.068)***	(0.069)***	(0.067)***	(0.064)***	(0.075)***	(0.067)***
Constant	8.664	9.027	9.026	9.005	9.978	9.048	9.958
	(0.057)***	(0.046)***	(0.048)***	(0.046)***	(0.059)***	(0.047)***	(0.058)***
District of birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province specific cubic trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dependent variable	9.07	9.42	9.42	9.42	9.42	9.44	9.44
Observations	106226	135502	133092	136140	136050	129148	132744
R-squared	0.06	0.05	0.05	0.05	0.06	0.05	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 18 and 32 years old interviewed in the 2007 national census. Lima is the capital city of Perú, and also the most urbanized, and developed. Ayacucho and Huancavelica are regions where the conflict started, and where it has been more persistent in time. Huanuco and San Martin are two regions in which the coca cultivation has been particularly active.