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

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Abstract

This paper provides empirical evidence of the persistent effect of exposure to political violence on human capital accumulation. I exploit the variation in conflict location and birth cohorts to identify the long- and short-term effects of the civil war on educational attainment. Conditional on being exposed to violence, the average person accumulates 0.31 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, exposure to violence during early childhood leads to permanent losses. I also explore the potential causal mechanisms.

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1 Introduction

Civil conflicts have been widespread throughout the world in the post-WWII period. During the past decade, economists have analyzed the consequences of these conflicts, with particular attention to their welfare effects. The short-run impacts 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 impact on human capital is persistent, particularly if exposure to conflict happens early in life. Specifically, the average person exposed to political violence before school age (in-utero, early childhood, and pre-school age ranges) has accumulated 0.31 fewer years of schooling upon reaching adulthood, with stronger short- than long-term effects. In contrast, individuals who experience the shock after starting school fully catch up to peers who were not exposed to violence

Understanding the scale and persistence of civil conflict on economic development is key, especially in developing countries, where most of the conflicts in the second half of the 20^{th} century have occurred. Economic growth theory suggest that, after a shock, the economy returns to its steady state level (as does human capital), but these models offer very little insight on the pace of recovery. Empirical cross-country and cross-regional studies suggest that countries see a steep decline on a variety of welfare indicators as a cosequence of war. They also show that there is significant recovery in most of these dimensions, but that this process varies in its duration.[1] As Blattman and Miguel (2009) suggest, beyond the trends revealed by cross-country or cross-regional evidence, it is hard to draw conclusions on how violence affects individual and household welfare, for which we need detailed individual-level analyses.

Micro-level studies have gone further in unveiling the relationship between civil conflict and individual welfare. Research in this area has focused on the immediate effects of conflict on health and educational outcomes. Several authors have found that there are significant effects of exposure to violence on education and health outcomes.[2] If the findings from the cross-country literature hold at the individual level, we

[1] 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. Miguel and Roland (2011) 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.

[2] 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

should observe that people are able to recover from these shocks after a certain period of time. If this were the case, the studies cited above would only be measuring the short-term consequences of violence, while neglecting the fact that these effects will disappear as time goes by. Further, the pace of recovery might be different across groups of the population and some of them might even face irreversible losses. For example, evidence suggests that other type of shocks (notably related to health) experienced in-utero or during early childhood are persistent and may even determine the income gradient.[3] These potential long-run outcomes have deep implications for policy design for post-conflict societies.

This paper contributes to the literature relating civil conflict to 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 over 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 Peruvian civil conflict 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 the extent to which supply and demand shocks can account for the persistent effect of violence.

Using data from the 2007 and 1993 national census in Perú, 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

fewer 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 0.047 sd's. Arcand and Wouabe (2009) analyze the 27-year-long Angolan 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 otherwise directly affected) on education and labor market outcomes, Annan and Blattman (2010) find that educational losses are closely associated with length of time abducted, while those reporting the most psychological distress have been exposed to the most severe war violence and are disproportionately, but not exclusively former combatants. Outside of Africa, Shemyakina (2011) analyzes 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. Ichino and Winter-Ebmer (2004) and Akbbulut-Yuksel (2009) look at the long-term effects of WWII on educational outcomes, finding similar effects. In Latin America, Camacho (2009) shows that women's exposure to the Colombian conflict during pregnancy causes children to be born with lower weight.

[3] Barker (1988) gave rise to the "fetal origins hypothesis", which has been used to refer to the critical period programming caused by conditions experienced in the fetal stage. Case and Paxon (2010 and 2011) show that health conditions in the early life determine the income gradients in the long-run. Mancini and Yang (2011) find that weather shocks in the early life have long lasting consequences in health, education and income among Indonesian girls. Almond and Currie (2011) provides a comprehensive review on economist's work on this topic.

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 attempt at controlling 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 school-age (during in-utero, early childhood, and pre-school age) accumulated 0.31 fewer years of schooling upon reaching adulthood. The short-term effects are larger than in the long-run, particularly if exposure to conflict happened early in life. Shocks in the pre-birth/in-utero period have a similar effect in the short- and long-run. Those who experience the shock in early childhood or preschool age on average only partially recover, while individuals who are exposed to violence once they have started their schooling cycle fully catch up to peers who were not exposed to violence. This suggests that children who are affected during very early childhood (pre-birth/in-utero) suffer irreversible effects of violence. Those who experience the shock in early childhood or pre-school age partially recover, while people exposed to violence once they have started their schooling cycle are able to fully catch up with their peers who did not experience violence in this period.

To put these results in context, Duflo (2001) finds that the effect of the massive school construction program in Indonesia on school attainment in the long-run is of a slightly smaller magnitude, but in the opposite 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 fewer years of primary education, about half of what I find in my short-term estimates.

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

Overall, the results in this paper show that shocks during the early stages of one's life have long-run irreversible consequences on human welfare. Relief efforts should thus be targeted to pregnant mothers and young children, and then to children in the early stages of their schooling cycle in order to minimize the long-term welfare losses for society.

In the next section, I present a historical perspective of the Peruvian civil conflict and describe the data used. Section 3 provides a simple theoretical model to help us understand the potential causal channels, as well as the empirical strategy. Section 4 presents the main results of the paper, discussing additional suggestive evidence about the causal channels. Finally, Section 5 summarizes and discusses the results.

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 fighting between the rebel group Partido Comunista del Perú-Sendero Luminoso (PCP-SL) and the national army.[4] The Peruvian Truth and Reconciliation Commission (CVR, for its acronym in Spanish) estimates that this conflict caused the death of about 69,290 people (about 0.31% of the population), making the Peruvian case one of the longest and most brutal conflicts in Latin America.

Toward the end of the 1970's, Perú was transitioning to democracy. On May 17^{th}, 1980, the night before the presidential election, the PCP-SL made its first attack: a group of five men broke into the voter registration office in the district of Chuschi, Ayacucho (in the southern Andes) and burned the ballot boxes and the registry. No injuries were reported, but on that day the PCP-SL formally declared war on the Peruvian state (CVR, 2004).[5]

Between 1970 and 1992, Perú experienced a deep economic collapse. This decline hit peasants in the rural highlands particularly hard, worsening regional inequalities (Weinstein, 2007).[6] At the same time, education was expanding while employment opportunities for educated individuals remained stagnant. This expansion of the educational sector created an illusion of progress in the population, which was not matched by job opportunities for the newly educated workforce. 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 the unemployment rate for those with other levels of education (McClintock, 1998).

The CVR considers this *"status inconsistency"* the main breeding ground upon which the PCP-SL was able to spread its ideas during the late 1970's in Ayacucho. In this area, the rebel group was able to build

[4] Additional armed groups participated in the conflict as well. The main ones were the Movimiento Revolucionario Túpac Amaru (MRTA), paramilitaries, and government-led militias (especially during the 1990's).

[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] In the southern highlands – where the PCP-SL emerged – the infant mortality rate was 128/1000 births, while the nationwide rate 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.

a critical mass of young and relatively educated supporters, who established the ideological foundations of the war and recruited the initial army. Importantly, this motivation was relevant in the initial stages of the war and in its original location. The expansion of the conflict during the 1980's, on the other hand, was motivated by political and strategic reasons.

The armed conflict started in the region of Ayacucho, where most of the PCP-SL's activity was concentrated between 1980 and 1982. The political strategy of the PCP-SL was inspired by the Chinese revolution and consisted of war advancing from rural areas to the cities. Thus, the main strategic target was Lima, the capital city. Additionally, the PCP-SL aimed to control the coca-producing region in the Amazon. This strategic movement of the war is depicted in Figure 1.[7]

As Figure 2 shows, there were two clear peaks in violent activities. The first 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 were popular leaders and landholders. The civil population was also severely threatened by the rebels: whenever a village declared itself opposed to the revolution, it was brutally punished. Victims of roadside attacks for collection of supplies and food for the army were mostly traders and farmers. Attacks on the civil population are presented in Table 1.[8] Public infrastructure was also a frequent target of the attacks; unfortunately, I only have data on human rights violations. For my purposes, it is important to note that school infrastructure was not affected by either of the parties involved in the conflict.

In September 1992, when violence in the country was at its peak and 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 decreased significantly and its power within the country was limited.[9]

Overall, there were fatalities reported in all but two departments (out of 25) of Perú at some point. The CVR estimates that 54% of the deaths can be attributed to the PCP-SL; the Movimiento Revolucionario

[7] One potential concern is that, if the violence started in places where educational levels were high, there is a correlation between pre-violence levels of education and violence incidence. Following the argument above, this would affect only the initial period and location of the war. To address this, in Column (2) Table 15 in the appendix, I exclude Ayacucho from the regressions and the results hold. In any case, the correlation between violence and education should be positive, and this would lead to an underestimation of my results.

[8] The data included in Table 1 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.

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

Túpac Amaru (MRTA) was responsible for 1.5% of the deaths; and the remaining 43.5% were 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 collected by the Truth and Reconciliation Commission (CVR), which has detailed records of every human rights violation reported during the period of violence. Particularly, the information used in this paper corresponds to illegal detentions, kidnapping, murder, extrajudicial executions, torture, or rape. Individual-level records from the 2007 and 1993 censuses allow me to identify the year and district of birth of each individual. I merge the violence data with the census, thus I can identify the number of human rights violations that took place in the district and year of birth, as well as in every year before and after. Additionally, I use data from the 1992 DHS to analyze the potential causal pathways through which the observed effect is acting.

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 and establishing responsibility over human rights violations in that period.[10] The CVR was a flagship program of the transition government, and it was declared one of its priorities. It was well resourced, with a total budget of about US$19 million over two years of operation, provided by the government and aid agencies. Apart from designating reputable commissioners, the CVR also recruited top academics and young professionals for the two years it operated.

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.[11] 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, location and date when the act took place, and individual characteristics. 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 Red Cross, the judiciary, NGOs, and the ombudsman's office. In this process, the CVR identified approximately 45,000 cases. After dropping double-coded cases and those that could

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

[11] Public audiences were widely advertised in the locality where the audience was going to be held, as well as in neighboring localities. The main locations where the audiences were held were determined based on previous reports of the incidence of violence from human rights organizations, the ombudsman, or the press. Additionally, communities could ask for an audience to be held in their town. There were no complaints at the time that the CVR emphasized politically active or unstable areas.

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 coded the testimonies and previous reports of violent acts, which include detention, kidnapping, torture, and rape, among others; in this data set, each of the 12,807 observations represent one violent act recorded. The final dataset that I work with is an aggregate of these. Overall, I have 36,019 unique reports of violent acts.

One of the drawbacks of the CVR information 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 attended the public hearings to tell their stories. Due to this fact, I use the presence of violence in the district, rather than intensity, where presence of violence is the occurrence of at least one violent act in the district. The intensity of violence is more subject to bias if particular unobserved characteristics lead to higher reporting in some areas. Section 4.2.1 further discussess the potential biases implied by the sample and my variable definition choice.

The intensity of violence is more subject to bias if particular unobserved characteristics lead to higher reporting in some areas. Other effects of the sample composition are further discussed later in the paper.

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 is helpful when trying to pin down the causal channels. It is important to note that the data set only includes human rights violations and not attacks on public infrastructure; hence, I am only identifying the effect of being exposed to violence against human beings in the immediate environment (within the district), and not the effect of the destruction of economic infrastructure or public utilities.

The individual-level information used in the analysis comes from a 2% random samples of the 2007 and 1993 national censuses. Importantly, respondents reported their age and the district where their mothers lived at the moment of birth (or the district where the respondent was born). 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 respondent's life, in his/her district of birth. It is worth mentioning that errors in the reported age or district of birth may lead to an erroneous assignment of violence exposure. The wrong assignment of the year in which the respondent was exposed to violence is of special concern here. Errors are due to missing information on the month of birth, and people making mistakes reporting age, which could lead me assign violence exposure with a margin of error. To minimize this potential problem, violence exposure will be analyzed during certain sensitive periods of life, rather than assigning it to specific years.[12]

The outcome of interest is educational achievement. To measure the long-term effects of violence, I use the number of years of primary and secondary schooling accumulated during one's lifetime.[13] This effect can only be measured among people who are old enough to have finished their schooling cycle by the time of the data collection. Hence, I use the information from 2007 and restrict the 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 timeline 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 from the 1993 census, which was taken right at the point when political violence started declining. The advantage of using a sample of people in school age (6 to 17 years of age) is that we can assume that most of them still live in the same household, and therefore we can compare siblings who were exposed to violence at different stages of their life, holding constant all district-specific and household-specific characteristics.

The main independent variable is the number of years of exposure to violence during different stages of early life. The stages of life that I consider are: in-utero/pre-birth (1 to 2 years before birth), early childhood (0 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 year s old). The definition of the periods in life that I use is purposefully broad, and it responds (i) to the potential errors in reported age, and (ii) to the fact that I want to capture the effects of violence exposure during pregnancy.[14]

Table 2 presents descriptive statistics, by violence exposure status. On average, people in the 2007 sample have about 9.4 years of primary and secondary education (out of a maximum of 11). People who were ever exposed to violence in the relevant period in their districts have, on average, one more year of education (9.7) compared to those whose birth district was never exposed to violence while they were children (8.7 years). On the other hand, when I compare the educational achievement of children in school age observed in the 1993 census, those born in districts never exposed to violence in their birth

[12] The age reports in the Peruvian census presents bunching at ages that are mutiple of 5 (and less so in ages exactly contiguous to 5, 10, 15, etc.). This is a common problem of using self reported age data. The bunching causes me to wrongly assign the violence to different ages, and attenuates my coefficients. On the other hand, as long as this problem is present in all of the cohorts, and across regions, it should not bias the estimated coefficients.

[13] 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.

[14] Given that age is reported, rather than birth date, part of prenatal period could be part of the first year of life. For example, a person who is a week away from her 20^{th} birthday at the time of the census will report her age as 19, in which case the prenatal period will cover all but one week of the first year of life (I thank an anonymous referee for this graphic example). On top of this, given that I want to capture violence shocks on household welfare or maternal health during pregnancy, I define the pre-birth/in-utero period as going back two years before birth. This is consistent with Camacho (2009). Further evidence on this is presented in the last section of the paper.

districts have attended school for 4.5 years, while those in districts ever exposed have 0.25 fewer years of education. All covariates shown are balanced between people born in violent and non-violent districts. Table 3 shows a more formal test of balance to analyze whether violence took place in districts with particular predetermined characteristics. The results show that districts/years in which violence took place have no statistically different pre-determined characteristics than those districts/years that were peaceful.

3 Theoretical Framework and Empirical Strategy

Consider a typical education production function model in the spirit of those discussed in Hanushek (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 ($N_1, ..., N_t$), 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 individuals born in the same year, those in the 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):

$$S_{ijt} = \alpha + \sum_{\tau=t-2}^{t+17} \beta_\tau Violence_{j\tau} + \gamma_p(t) + \delta X_i + \eta_j + \nu_t + \epsilon_{ijt} \tag{3}$$

where S_{ijt} is the number of years of schooling achieved by individual i born in district j, and in year

t. X_i is a vector of individual time-invariant characteristics, such as gender or ethnicity. The district of birth fixed effects (η_j) control for any specific characteristics of all children born in the same locality. Similarly, the year of birth fixed effects (ν_t) absorb any shock common to all children born in the same year. $\gamma_p(t)$ is a flexible province-specific trends, which is included in all the regressions to account for the differential developments of each province of the country through time, as for example, 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 outcome in deviation from the long-run trend in his/her birth province. Finally, ϵ_{ijt} is a random error term.

One must bear in mind the inclusion of this large set of fixed effects when interpreting the results, since they do not represent the impact of violence on schooling at the national level, but rather the average effect with respect to local averages and year averages, and purged of province-specific flexible trends. Further, the estimates should be interpreted as conservative, since the district fixed effects are eliminating some valuable cross-sectional variation in the violence data.

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 allow for an arbitrary variance-covariance structure within birth district by clustering them.

$Violence_{j\tau}$ represents the exposure to violence in the birth district j, during year $\tau = t-2, .., , t+17$, where t is the year of birth. The focus of the estimation is thus on β_τ. Given that I am interested in detecting the effects of exposure in different periods of life, I aggregate these indicators into variables that capture the number of years exposed during each relevant period: pre-birth/in-utero(1 to 2 years before birth), early childhood (0 until 3 years old), pre-school age (4 to 6 years old), primary school age (7 to 12 years old), and high school age (13 to 17 years old).

Consistent with the model presented above, exposure to violence can affect individual endowments $(E_1, ..., E_t)$ through several channels. For example, violent attacks can affect E_0^h by killing or otherwise affecting 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 through food availability, for example. Other potential pathways are the nutrition of the mother, or of the child herself once she is born, which may cause irreversible consequences for her future school attainment through long-lasting effects on cognitive abilities. Camacho (2009) presents evidence suggesting that violence-related stress before birth 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 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 community educational resources (C_0). 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 in the revolution, which is clear from the great influence they had on the teachers' union. Schools are a highly valued asset within a community; thus, if the army was to gain the support of the community to fight the terrorists, it did not have an incentive to destroy school infrastructure. A consistent series of information about the number of schools or the number of teachers at the district level is not available. On the other hand, knowing the close relationship between the teachers and the rebels, one channel through which violence affected C_t was the capture or even murder of the local teacher by the national army: about 3% of the reported human rights violations were against teachers (see Table 1). It was not an easy task to replace a teacher in a violent area.

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.

One way of checking if there is a selection problem 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 4 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 for a period of 1 to 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, since my identification strategy hinges on the fixed effect, I don't only need to see that the pre-violence levels of education balanced, but more importantly, that cohort differences are as well. Panel B in Table 4 shows the difference in educational attainment between different cohorts, across districts with different levels of violence exposure. People born in a non-violent district did not attain significantly more education than those born in violent districts.[15]

Another threat to the identification assumption is that the characteristics of the population change as a function of the timing of the violent attacks. This means that the characteristics of the population settled in a particular district are similar across violent and non-violent years. One way to test this assumption is to see whether these pre-determined characteristics in each districtXyear cell are a function of the presence of violence. I run this test on the 2007 and 1993 data in Table 3. One important concern is that fertility choices are determined by the presence of violence. If this were the case, the size of cohorts exposed to violence would be smaller than in peaceful years. Results show that cohorts affected by violence were not smaller than the non-affected ones in neither 1993 nor 2007. For the 2007 sample, I see that there are marginal differences in average age and the percentage of native speakers, with violent districts having older people (though the difference is close to zero), and smaller indigenous populations. On the other hand, the gender composition and migration in violent districts do not seem to be different by violence status.

In 1993, there are no significant differences in cohort size, gender composition, migration rates, percentage of indigenous population, or education of the household head. There is a difference in wealth, with violent districts being slightly poorer .[16]

4 Results and Discussion

4.1 Long-term Consequences of the Conflict

Table 5 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: in-utero/pre-birth (1 to 2 years before birth), early childhood (0 to 3 years of age), pre-school (4 to 6), primary school (7 to 12), and secondary school age (13 to 17).[17] Being exposed to violence before entering school -that is, during the pre-birth/in-utero years, early childhood, or pre-school

[15] As an additional test for the identification assumption I also run regressions to see whether incidence or presence of violence in the district predict pre-war education levels (or cohort differences). I find an insignificant relationship, and the coefficients are very close to zero. These results are omitted, but are available upon request.

[16] I thank an anonymous referee for suggesting this test.

[17] The results are robust to the choice of years grouped together. Figure 3 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 and errors in assigning violence due to age reporting.

age- has a statistically and economically significant effect on long-run human capital accumulation. As shown in column (1) in Table 5, an additional year of exposure to violence before birth implies that the person will accumulate 0.07 fewer years of education; if the shock happens during the early childhood or in pre-school age, it reduces long-term educational achievement by 0.05 years for each year of exposure to violence, respectively.[18] On the other hand, living in a district affected by violence during 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, or about to conceive, 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 in-utero/pre-birth period. As expected, I do not find any statistically significant effect for these variables.[19][20]

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.[21] The coefficients associated with violence in the in-utero, early childhood, and pre-school years are still significant at the conventional levels and their magnitude is slightly increased compared to those shown in column (1). [22]

The average child affected by violence in each of the periods analyzed, in-utero, early childhood, or pre-school, had about 1.4, 2.2 and 1.9 years of exposure, respectively. This means that the average child exposed to violence in-utero/pre-birth accumulated about 0.10 fewer years of schooling than his/her peers who lived in peaceful districts or were born in peaceful years. For the average child affected during either early

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

[19] In Table 13 I explore the heterogeneous impacts of violence by gender and ethnicity. The point estimates for the exposure to violence in all periods are larger for girls than those found in my benchmark specification in the first column, and statistically significant for exposure during the early childhood, and in the pre-school period. On the other hand, for men only the exposure to violence during the early childhood seems to be an important determinant of future schooling, and the coefficient is smaller in magnitude. Meanwhile, I find that the effect for native speakers is larger than for Spanish speakers. Though, these results are not statistically significant due to the reduced sample size.

[20] Galdo (2010) estimates the effects of violence exposure on labor market outcomes in Peru using an instrumental variables approach. I run similar regressions using my specification, and using the data from the Peruvian household survey (ENAHO). The results that I obtain in my fixed effects approach are of a similar magnitude. Being exposed to an additional year of violence during in utero or early childhood leads to a decrease in wages of about 2%. These results are displayed on Table 12 in the appendix.

[21] The F-test of joint significance for exposure to violence three or more years before birth fails to reject at the 10% the null of being jointly zero in column (2) and column (3).

[22] As an additional robustness test, I run the regressions excluding the locations where the conflict was more persistent, and the main insights of the paper hold. I show these results in Table 15 in the appendix.

childhood or pre-school age, the effects of an additional year of exposure are 0.11 and 0.10, respectively.[23] 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 3 shows these results in a graphical way. Consistent with Table 5, the effect of 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.[24]

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 similar magnitude, but in the opposite direction: each school constructed per 1,000 children led to an increase of 0.12 to 0.19 years of education.

4.2 Potential Biases and Concerns

4.2.1 Sample Composition

The violence data coming from the CVR is mainly self-reported, which can raise a number of problems. First, it is plausible that the under-reporting in the data comes from the group that was more affected by violence, i.e. those for whom verbalizing the incidents in front of the Commission was more difficult, such as the victims of sexual violence. Second, 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 was higher) were not able to report human rights violations. Further, it is more likely the the under reporting was more pronounced for violent acts that took place at the initial stages of the conflict, in localities that were least affected, or in places where the population was less proactive about the reporting process. 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 should be interpreted as a lower bound. To partially overcome this issue, I rely on a measure of presence of violence, rather than intensity. For the presence of violence variable to turn on, it is sufficient to observe one act of violence; hence, any under-reporting is minimized by this variable.[25]

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

[23] An alternative way of thinking about these results is as the treatment on the treated effect for the direct experience of war on educational achievement.

[24] 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 15.

[25] I have run all the regressions in the paper using an intensity, rather than presence of violence measure, and the magnitude and significance of the main results are consistent with the ones shown. These results are available in Table 11 in the Appendix.

induced by the fatal victims again introduces a downward 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 in 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, nor the reasons to leave one's hometown. Not knowing the exact location where each respondent was located each year can cause me to wrongly assign her exposure to violence. The bias implied by this wrong assignment cannot be signed a priori.

There is anecdotal evidence that people who migrated from violent areas were discriminated against in the cities and thus denied 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 many more employment opportunities and 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 those 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 5 and 6 would be overstating the impact of violence.

There is no clear way to determine the migration bias other than empirically. One indirect way to deal with this issue is to restrict the sample to non-migrants, or those who were living there five years before the 2007 census, and compare the point estimates of the original sample with those of the non-migrants. Table 6 presents the estimation of the model splitting the sample between those who report living in their birth-place and those who migrated at different points in their lives. The results show that the effect of violence for the non-migrants is higher than in the full sample, especially for exposure during the in-utero/pre-birth period, for which we observe an effect on those still living in their birth districts of about 0.09, while for migrants it is small and not statistically significant. If exposure happened at other periods of the early life, there are no differences between migrants and non-migrants. The results are very similar when we differentiate between migration at any point in life (Columns (2) and (4)) and when I exclude from this category those who lived in their birth district until 2002 (Columns (3) and (5)). These findings are in line with Escobal and Flores (2009), who document that mothers who migrate out of violent districts have children with higher nutritional statuses when compared to their peers who stayed, but they find no differences in cognitive abilities.

An additional concern might be that people who migrate are different from the ones who stay. For example, they might be richer, more educated, more forward-looking, etc. However, this seems not to be the case in the data I am working with. The evidence shown in Table 3 shows that the presence of violence does not influence the composition of the cohorts living in the district, in terms of their size, gender composition, average age, average education, percentage of native speakers, education of the parents, wealth, or even migration. [26]

In sum, the effect observed in Tables 5 and 6 does not contain a significant migration bias. If anything, the bias exists for people affected by the conflict in the in utero period, and in any case it is an downward bias.

4.3 Short-term Results and the Persistence of Violence

The results in the previous section show 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.[27] In this section, I explore the short-term impacts of political violence on schooling, and compare them with the long-terms effects estimated above. I estimate equation (3) on the data from the 1993 census. Given 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. This also allows for a tighter identification strategy, since I can not only compare children within the same district, but I can also compare children within the same household who are affected by violence at different stages of their lives.

The results are shown in Table 7. Being exposed to violence during the in-utero period, early childhood, pre-school age, or primary school age has a statistically and economically significant effect on schooling. Considering that the average child exposed to violence in each of these periods has about 1.4, 2.0, 1.8, and 2.9 years of violence exposure, respectively, the overall effect for the average child affected by violence in

[26] 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. These results are shown in Table 14 in the appendix. Once I include district-specific intercepts, 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.

[27] See for example, Miguel and Roland (2011), Davis and Weinstein (2002), Brakman, Garretsen and Schramm (2004), Cerra and Saxena (2008).

each of these periods is 0.98 fewer years of schooling.

Given that the vast majority of children in school age still live in their parents' household, I am able to 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 (2) of Table 7. The sibling difference model gives very similar results in terms of magnitude and statistical significance. 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 are basically unchanged when I account for time-invariant household characteristics allows me to rule out the hypothesis that the causal pathway through which experiencing violence affect educational achievement is not a persistent shock to household welfare, or other time-invariant household characteristic.

Recall from Table 5 that, for people observed in 2007, the average child exposed to violence accumulates 0.31 fewer years of education.[28] Comparing these results, we see that the coefficients associated with shocks in the pre-birth/in-utero period are similar between the long- and short-term estimations (-0.07 and -0.102, respectively), while the coefficients associated with violence exposure in the early childhood or pre-school periods are about three times as large in the short-term than the long-term.

Further, in the estimation of the short-term effects, I see that violence during the primary school age is significant, and the magnitude of the coefficient is non-negligible, while in the long-run, violence in this period does not seem to have an effect on school attainment. This suggests that the effect of violence on human capital accumulation is mitigated as time goes by. More importantly, children who are affected by violence during the very early childhood (pre-birth/in-utero) suffer irreversible effects of violence. On the other hand, those who experience the shock during early childhood or pre-school age partially recover. Finally, people exposed to violence once they have started their schooling cycle are able to fully catch up with their peers who didn't experience violence in this period. This evidence is consistent with the extensive literature about economic shocks and the critical-period programming (Almond and Currie, 2011; Alderman, Hoddinott, and Kinsey, 2006; Maccini and Yang, 2009, among others).

Comparing the magnitude of these results with the ones available in the literature, Akresh and de Walque (2010) found that four years after the Rwandan genocide, children (directly) exposed to violence accumulate 0.5 fewer years of primary education, about half of what I find in my short-term estimates.

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

4.4 Possible Causal Pathways

As shown in the model above, one potential mechanism behind the observed effect might be a supply side shock: if a teacher was directly affected by violence, it may have made it harder for children to go to school. The CVR recorded the occupation of the victims, thus I can directly test this hypothesis by including in my benchmark regressions a dummy variable for whether a teacher was attacked in the district of birth within each period of the student's life. These results are shown in Table 8. In the short-term, conditional on being exposed to violence, an attack on a teacher during the in-utero, early childhood or pre-school periods leads to a significant decrease in schooling of about 0.55, 0.48 and 0.28 years, respectively, as shown in Columns (1) and (2). The fact that the effect is significant for any period before the child was old enough to enter school suggests that the injury or death of a teacher delayed the entrance to school. Further, when I look at the long-term effects of this particular type of violence, I find that having a teacher attacked does not significantly affect the long-term accumulation of human capital. Taken together, these results suggest that violence against teachers leads to a delay in school entrance but does not lower educational achievement in the long-run.

On the other hand, I can also present some suggestive evidence on whether the effect is driven by a demand-side shock, such as effects on health, which affect child's cognitive development (Camacho, 2009). Using data from the 1992 DHS, and a between-siblings difference model, 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 9. I indeed find some evidence that the occurrence of a shock between two years before birth and the first year of life has a negative effect on health status. The reduced sample size and the high data demand of the identification based on household fixed effects limits my ability to do statistical inference in this case. Nevertheless, the magnitude of the coefficients for the years -2 through 1 is an order of magnitude larger than the ones associated with violence experience after the first year of life.

One other potential mechanism through which violence exposure might affect future educational outcomes is through household 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 provide some suggestive evidence. In Column (1) of Table 10, I run an OLS regression of an asset index (Filmer and Pritchett, 2001) on whether there was violence in the district during the years preceding the survey and some relevant controls. The results suggest that violence did not differentially affect asset tenure at the household level. Further, using a similar strategy, I can determine whether 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 just before the survey is correlated with lower body mass index of the women in reproductive age.[29]

To summarize, I find suggestive evidence of two potential channels through which violence affects educational achievement: (i) 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 occurring between one year before birth and one year after birth decreases a person's health status. Finally, this effect does not seem to operate through shocks to household asset tenure, but rather through maternal health.

5 Summary and Conclusions

Civil conflict is a widespread phenomenon around the world, with about three-fourths of countries 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 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 of violence on human capital accumulation for people exposed to it in the early stages of life. 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

[29] 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.

average losses in educational achievement in the long-term, relative to local averages and year averages, and purged from province flexible trends.

The results show that the average person exposed to political violence before school age (in-utero, early childhood, and pre-school age ranges) has accumulated 0.31 fewer years of schooling upon reaching adulthood, with stronger short- than long-term effects. In contrast, individuals who experience the shock after starting school fully catch up to peers who were not exposed to violence. Concerns with the sample composition and migration issues lead us to think that these results ought to be interpreted as lower bounds of the estimated effects.

Short-term effects show that the persistence of the shock depends on the moment in life when the child was exposed to violence. Shocks in the pre-birth/in-utero period have a similar effect in the short and long-run. Those exposed during early childhood or pre-school age experience effects that are three times larger in the short-run, while violence exposure once the schooling cycle has started only has a short-term effect. This suggests that children who are affected by violence during the very early childhood (pre-birth/in-utero) suffer irreversible effects of violence. Those who experience the shock in the early childhood or pre-school age partially recover, while people exposed to violence once they have started their schooling cycle are able to fully catch up with their peers who did not experience violence in this period. This result contrasts with the cross-country findings that the effects of violence vanish over time.

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 decrease the educational achievement of children, mainly by delaying school entrance, but this effect is not persistent. On the demand side, violent events occurring within a year before or after 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 contribute to the evidence that shocks during the early stages of one's life have long-term irreversible consequences on human welfare. This suggests that relief efforts should be targeted to pregnant mothers and young children, and then children in the early stages of their schooling cycle, if we want to minimize the long-term welfare losses for the society.

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

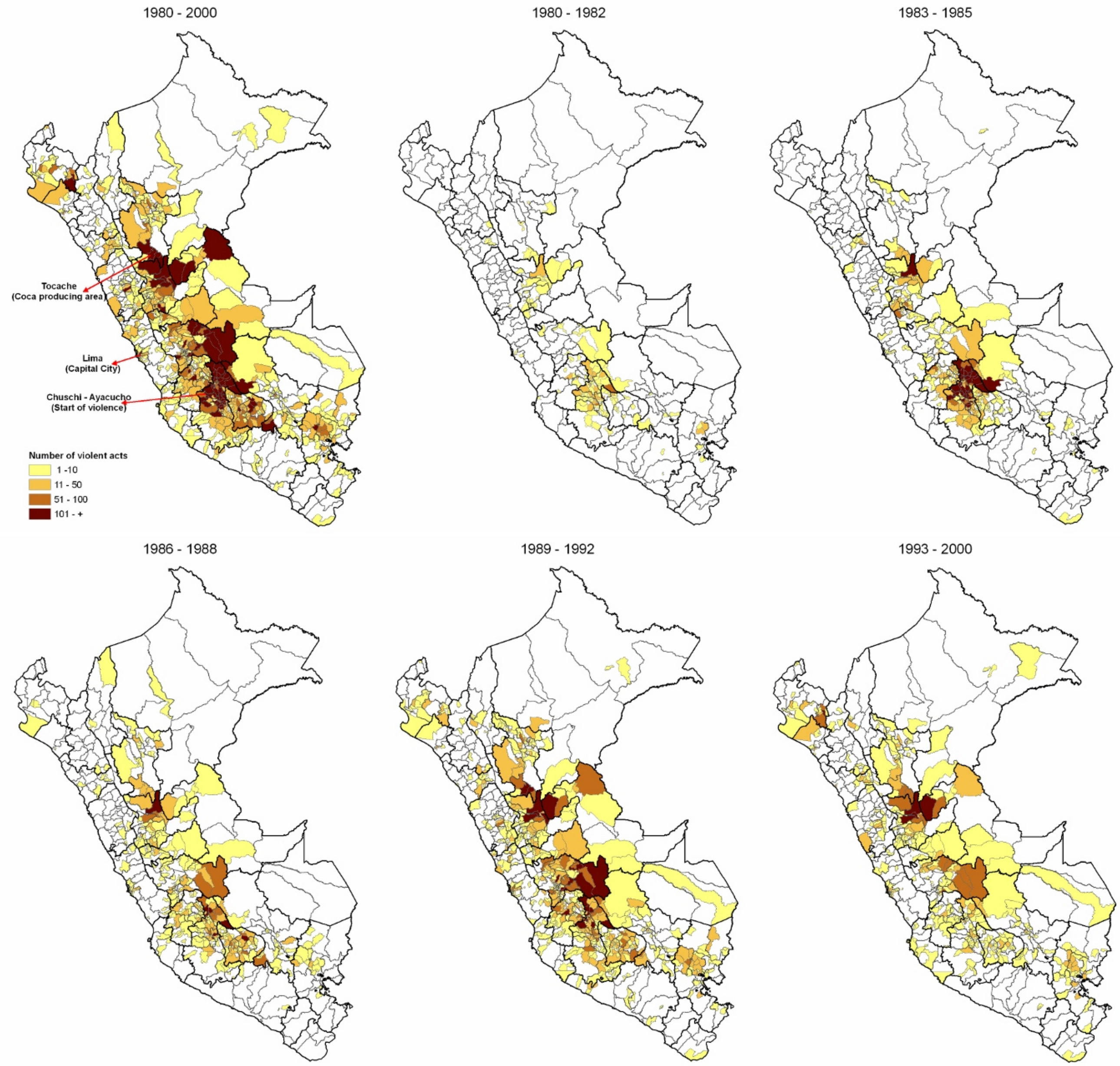
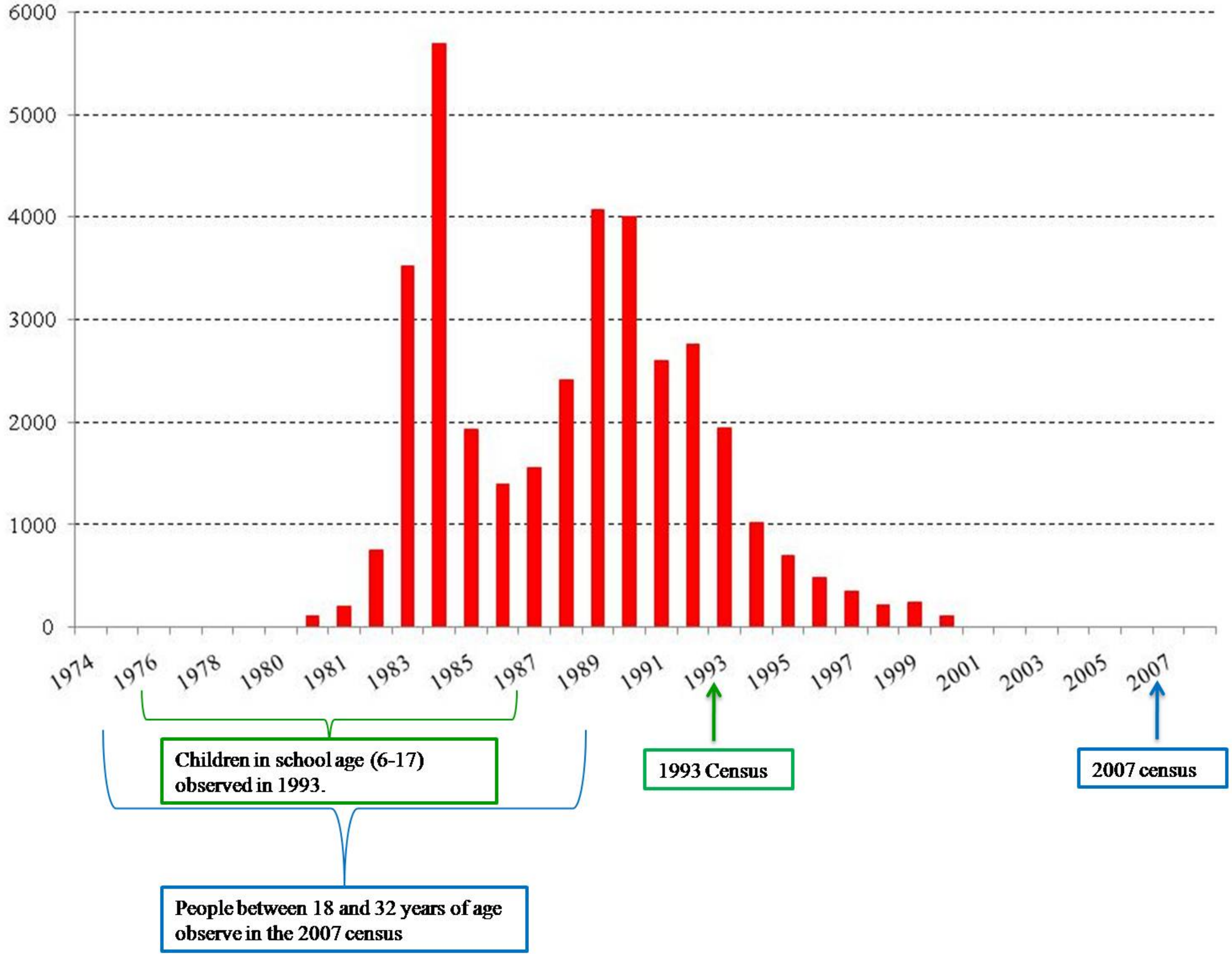


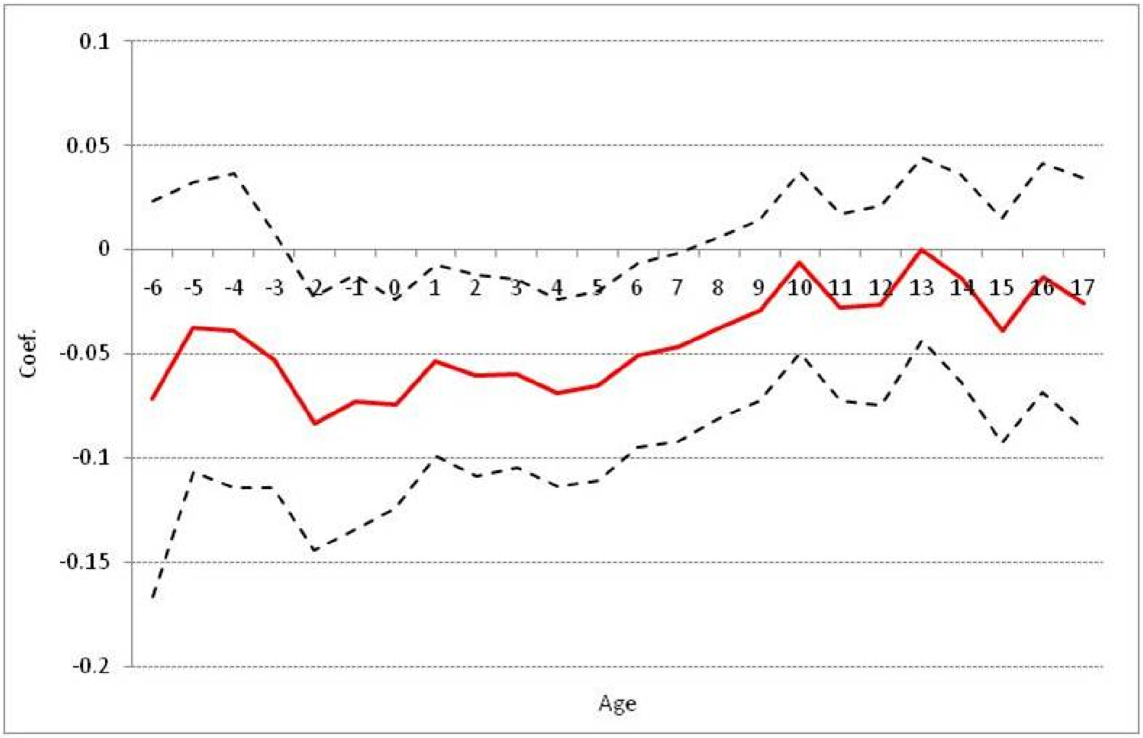
Figure 2: The Timing of the Conflict and Structure of the Data: # of Violent Events Reported to the CVR, by Year of Occurrence



Source: CVR, 2004.

Note: The figure shows the number of 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: The effect of Violence Exposure on Human Capital Accumulation, 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: 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 4: Pre-Violence Average Years of Education, by Violence Exposure

Panel A: Average years of education, by year cohorts			
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: Difference 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 high school 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 2: 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	75314	4.36	3.23	0	11
Gender (=1 male)	139446	0.49	0.50	0	1	75314	0.51	0.50	0	1
Mothers' language (=1 native)	139446	0.13	0.34	0	1	75314	0.21	0.41	0	1
Migrant (1=migrated)	139446	0.39	0.49	0	1					
No. of years exposed to violent events (in utero)	139446	0.21	0.53	0	2	75314	0.14	0.44	0	2
No. of years exposed to violent events (early childhood)	139446	0.71	1.19	0	4	75314	0.50	1.03	0	4
No. of years exposed to violent events (pre-school age)	139446	0.82	1.09	0	3	75314	0.64	1.00	0	3
No. of years exposed to violent events (primary school age)	139446	1.70	1.97	0	6	75314	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	31852	4.50	3.29	0	11
Gender (=1 male)	40086	0.49	0.50	0	1	31852	0.51	0.50	0	1
Mothers' language (=1 native)	40086	0.15	0.36	0	1	31852	0.20	0.40	0	1
Migrant (1=migrated)	40086	0.38	0.48	0	1					
No. of years exposed to violent events (in utero)										
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	43462	4.25	3.18	0	11
Gender (=1 male)	99360	0.49	0.50	0	1	43462	0.51	0.50	0	1
Mothers' language (=1 native)	99360	0.12	0.33	0	1	43462	0.22	0.41	0	1
Migrant (1=migrated)	99360	0.40	0.49	0	1					
No. of years exposed to violent events (in utero)	99360	0.29	0.61	0	2	43462	0.23	0.56	0	2
No. of years exposed to violent events (early childhood)	99360	1.00	1.30	0	4	43462	0.86	1.23	0	4
No. of years exposed to violent events (pre-school age)	99360	1.15	1.13	0	3	43462	1.10	1.10	0	3
No. of years exposed to violent events (primary school age)	99360	2.39	1.95	0	6	43462	2.54	1.87	0	6
No. of years exposed to violent events (high school age)	99360	1.36	1.64	0	5					

Notes: 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: in utero, 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 the ones observed in 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: in utero, early childhood, pre-school or primary school age.

Table 3: Balancing tests: Violence exposure on Predetermined Characteristics

	(1) Log(cell size)	(2) % Male	(3) Avg. Age	(4) % migrants	(5) % Native speakers	(6) Avg. Yrs. of educ. hh head	(7) Avg. Asset index
Panel A: 2007 Census Sample							
Presence of violence	0.026	0.001	0.000	0.011	-0.009		
	(0.010)**	(0.007)	(0.000)***	(0.007)	(0.005)*		
Constant	1.206	0.488	37.000	0.401	0.295		
	(0.015)***	(0.010)***	(0.000)***	(0.010)***	(0.007)***		
District of fixed effects	Yes	Yes	Yes	Yes	Yes		
Year fixed effects	Yes	Yes	Yes	Yes	Yes		
Province specific cubic trend	Yes	Yes	Yes	Yes	Yes		
Observations	51792	51792	51792	51792	47724		
Number of districts	1825	1825	1825	1825	1825		
R-squared	0.12	0.01	1.00	0.24	0.05		
Panel B: 1993 Census Sample							
Presence of violence	0.004	0.000	-0.000	0.002	0.002	-0.040	-0.046
	(0.013)	(0.009)	(0.000)*	(0.006)	(0.005)	(0.061)	(0.019)**
Constant	1.132	0.498	27.891	0.518	0.338	-2.766	-0.871
	(0.020)***	(0.014)***	(0.000)***	(0.012)***	(0.008)***	(0.107)***	(0.042)***
District of fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province specific cubic trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32220	32220	32220	32220	32220	32153	31755
Number of districts	1781	1781	1781	1781	1781	1780	1776
R-squared	0.10	0.02	1.00	0.22	0.04	0.06	0.10

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. Each observation in this regression represents a districtXyear cell, and the variable "Presence of violence" is an indicator equals to one whenever there was at least on violent event in the district in that particular year. The sample includes all people born after 1970 interviewed in each sample.

Table 5: 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	-0.072
		(0.041)	(0.048)
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	-0.040
		(0.037)	(0.038)
Exposed to violent events in his/her year -3		-0.051	-0.059
		(0.031)	(0.035)
No. of years exposed to violent events (in utero)	-0.071		-0.079
	(0.021)***		(0.023)***
No. of years exposed to violent events (early childhood)	-0.051		-0.061
	(0.014)***		(0.016)***
No. of years exposed to violent events (pre-school age)	-0.051		-0.063
	(0.015)***		(0.016)***
No. of years exposed to violent events (primary school age)	-0.019		-0.032
	(0.014)		(0.016)**
No. of years exposed to violent events (high school age)	0.000		-0.019
	(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	8.613	8.589	8.658
	(0.066)***	(0.054)***	(0.071)***
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 defined 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). The F-test of joint significance for the coefficients before year -2 in Columns (2) and (3) fails to reject the null that they are jointly equal to zero. In Column (2), the F-test is 1.94 (p-value=0.1013), while in Column (3), the F-test is 1.86 (p-value=0.1157).

Table 6: Violence and Human Capital, by Migration Status

	(1)	(2)	(3)	(4)	(5)
	Years of education				
	Full Sample	Non-Migrants		Migrants	
		Lives in birth district	Lived in birth dist. 5 years ago	Doesn't live in birth district	Didn't live in birth dist. 5 years ago
No. of years exposed to violent events (pre-birth)	-0.071	-0.095	-0.094	-0.023	-0.027
	(0.021)***	(0.028)***	(0.027)***	(0.029)	(0.031)
No. of years exposed to violent events (early childhood)	-0.051	-0.052	-0.055	-0.052	-0.038
	(0.014)***	(0.018)***	(0.018)***	(0.021)**	(0.019)**
No. of years exposed to violent events (pre-school age)	-0.051	-0.054	-0.046	-0.059	-0.075
	(0.015)***	(0.019)***	(0.018)**	(0.022)***	(0.024)***
No. of years exposed to violent events (primary school age)	-0.019	-0.024	-0.021	-0.018	-0.019
	(0.014)	(0.018)	(0.017)	(0.021)	(0.023)
No. of years exposed to violent events (high school age)	0.000	-0.009	-0.001	0.004	0.005
	(0.013)	(0.017)	(0.016)	(0.020)	(0.022)
Gender (male=1)	0.438	0.485	0.476	0.395	0.393
	(0.030)***	(0.040)***	(0.038)***	(0.028)***	(0.029)***
Mother's language (native=1)	-1.747	-1.909	-1.774	-1.151	-1.232
	(0.064)***	(0.086)***	(0.078)***	(0.061)***	(0.067)***
Constant	7.674	7.061	8.244	7.190	9.735
	(0.052)***	(0.066)***	(0.067)***	(0.088)***	(0.110)***
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
Observations	139446	84884	96812	54562	42634
R-squared	0.06	0.07	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 17 and 32 years old interviewed in the 2007 national census. Migrants in columns (2) and (4) are defined as those who currently live in a district different from their birth district. In columns (3) and (5), I exclude from the group of migrants those who live in a different district from where they were born, but five years ago lived in the same district where they were born

Table 7: Violence and Human Capital: short-term Effects

	(1)	(2)
	Years of Education	
No. of years exposed to violent events (in utero)	-0.102	-0.087
	(0.036)***	(0.042)**
No. of years exposed to violent events (early childhood)	-0.140	-0.146
	(0.028)***	(0.031)***
No. of years exposed to violent events (pre-school age)	-0.133	-0.139
	(0.030)***	(0.032)***
No. of years exposed to violent events (primary school age)	-0.113	-0.111
	(0.027)***	(0.027)***
Gender (male=1)	0.137	0.128
	(0.021)***	(0.022)***
Mother's language (native=1)	-1.000	-0.445
	(0.054)***	(0.118)***
Constant	6.835	134.858
	(229.997)	(1.114)***
Household fixed effects	No	Yes
District of birth fixed effects	Yes	No
Year of birth fixed effects	Yes	Yes
Province specific cubic trend	Yes	Yes
Mean dependent variable	4.36	4.35
Observations	75314	63888
R-squared	0.50	0.54

* 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.

Table 8: Supply Side Shocks and Human Capital

	(1)	(2)	(3)
	Years of Education		
	Short-term		Long-term
No. of years exposed to violent events (in utero)	-0.063	-0.043	-0.059
	(0.036)*	(0.041)	(0.022)***
No. of years exposed to violent events (early childhood)	-0.107	-0.100	-0.042
	(0.029)***	(0.032)***	(0.016)***
No. of years exposed to violent events (pre-school age)	-0.112	-0.105	-0.043
	(0.032)***	(0.033)***	(0.016)***
No. of years exposed to violent events (primary school age)	-0.101	-0.093	-0.015
	(0.028)***	(0.027)***	(0.015)
No. of years exposed to violent events (high school age)			-0.001
			(0.014)
Teacher was a victim (in utero)	-0.528	-0.554	-0.141
	(0.164)***	(0.155)***	(0.075)*
Teacher was a victim (Early childhood)	-0.331	-0.480	-0.061
	(0.097)***	(0.106)***	(0.047)
Teacher was a victim (pre-school age)	-0.174	-0.281	-0.036
	(0.084)**	(0.085)***	(0.041)
Teacher was a victim (primary school age)	-0.056	-0.125	0.002
	(0.065)	(0.072)*	(0.030)
Teacher was a victim (high school age)			0.051
			(0.036)
Gender (male=1)	0.136	0.129	0.437
	(0.021)***	(0.023)***	(0.030)***
Mother's language (native=1)	-1.000	-0.448	-1.747
	(0.054)***	(0.118)***	(0.064)***
Constant	6.941	134.740	8.604
	(967.574)	(1.120)***	(0.066)***
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	4.35		9.4
Observations	75314	63888	139446
R-squared	0.51	0.54	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. For column (3), the sample includes all people between 18 and 32 years old interviewed in the 2007 national census.

Table 9: 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 10: Demand Side Shocks and Human Capital: Asset Accumulation and Mother's Health

	(1) Household 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.

APPENDIX[30]

Table 11: Violence and Human Capital Accumulation: 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	-0.00403
		(0.00219)	(0.00227)
Log(No. of violent event per pop in the dist/year(t-5) of birth)		-0.00073	-0.00132
		(0.00166)	(0.00173)
Log(No. of violent event per pop in the dist/year(t-4) of birth)		-0.00074	-0.00166
		(0.00192)	(0.00194)
Log(No. of violent event per pop in the dist/year(t-3) of birth)		-0.00269	-0.00353
		(0.00150)	(0.00151)*
Log (No. of violent events) (in utero)	-0.00548		-0.00574
	(0.00134)***		(0.00134)***
Log (No. of violent events) (early childhood)	-0.00431		-0.00492
	(0.00133)***		(0.00135)***
Log (No. of violent events) (pre-school age)	-0.00370		-0.00427
	(0.00121)***		(0.00122)***
Log (No. of violent events) (primary school age)	-0.00262		-0.00294
	(0.00155)		(0.00155)*
Log (No. of violent events) (high school age)	-0.00193		-0.00246
	(0.00143)		(0.00143)
Gender (male=1)	0.43729	0.43742	0.43721
	(0.03036)***	(0.03037)***	(0.03038)***
Mother's language (native=1)	-1.74779	-1.74734	-1.74706
	(0.06387)***	(0.06385)***	(0.06384)***
Constant	8.22879	8.43130	7.94169
	(0.09499)***	(0.08897)***	(0.13175)***
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.

[30]Not intended for publication.

Table 12: 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	-0.018	0.006	0.006	0.004	0.004	-0.034	-0.033
(in utero)	(0.046)	(0.011)	(0.011)	(0.011)	(0.012)	(0.031)	(0.030)
No. of years exposed to violent events	-0.058	-0.008	-0.010	-0.019	-0.019	-0.026	-0.018
(early childhood)	(0.032)*	(0.006)	(0.006)	(0.006)***	(0.006)***	(0.021)	(0.020)
No. of years exposed to violent events	-0.005	-0.010	-0.010	-0.008	-0.008	-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	-0.002	-0.002	-0.007	-0.007	-0.021	-0.022
(primary school age)	(0.036)	(0.007)	(0.007)	(0.006)	(0.006)	(0.021)	(0.021)
No. of years exposed to violent events	0.016	-0.008	-0.006	-0.005	-0.005	0.032	0.032
(high school age)	(0.032)	(0.007)	(0.006)	(0.007)	(0.007)	(0.019)*	(0.018)*
Gender (male=1)	0.505	0.112	0.125	0.187	0.188	0.486	0.505
	(0.052)***	(0.008)***	(0.009)***	(0.007)***	(0.007)***	(0.025)***	(0.023)***
Mother's language (native=1)	-1.680	0.158	0.088	0.091	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	10.612	0.179	0.550	0.644	0.678	7.074	6.036
	(1.937)***	(0.326)	(0.299)*	(0.223)***	(0.223)***	(1.035)***	(0.977)***
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 13: 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	-0.071	-0.087	-0.063	-0.177	-0.051
(in utero)	(0.021)***	(0.029)***	(0.025)**	(0.083)**	(0.020)***
No. of years exposed to violent events	-0.051	-0.059	-0.049	-0.034	-0.043
(early childhood)	(0.014)***	(0.020)***	(0.016)***	(0.054)	(0.013)***
No. of years exposed to violent events	-0.051	-0.079	-0.020	-0.063	-0.044
(pre-school age)	(0.015)***	(0.023)***	(0.017)	(0.058)	(0.015)***
No. of years exposed to violent events	-0.019	-0.007	-0.029	-0.044	-0.009
(primary school age)	(0.014)	(0.018)	(0.018)*	(0.051)	(0.014)
No. of years exposed to violent events	0.000	-0.001	0.004	-0.038	0.004
(high school age)	(0.013)	(0.019)	(0.017)	(0.057)	(0.013)
Gender (male=1)	0.438			1.656	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	8.613	7.862	6.939	-1.759	8.190
	(0.066)***	(0.086)***	(0.078)***	(0.280)***	(0.059)***
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	0.13	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 14: Migration and Exposure to Violence

	(1)	(2)
	Migration status (=1 migrant)	
Exposed to violent events in his/her year -6	-0.000	-0.030
	(0.020)	(0.011)***
Exposed to violent events in his/her year -5	0.008	-0.009
	(0.014)	(0.009)
Exposed to violent events in his/her year -4	0.021	-0.000
	(0.016)	(0.008)
Exposed to violent events in his/her year -3	0.007	-0.001
	(0.014)	(0.007)
No. of years exposed to violent events (in utero)	0.007	-0.011
	(0.008)	(0.004)**
No. of years exposed to violent events (early childhood)	0.020	0.001
	(0.006)***	(0.003)
No. of years exposed to violent events (pre-school age)	0.016	0.002
	(0.005)***	(0.003)
No. of years exposed to violent events (primary school age)	0.005	-0.001
	(0.005)	(0.003)
No. of years exposed to violent events (high school age)	0.007	-0.003
	(0.006)	(0.003)
Gender (male=1)	-0.027	-0.026
	(0.003)***	(0.003)***
Mother's language (native=1)	-0.183	-0.177
	(0.017)***	(0.017)***
Constant	0.336	-0.407
	(0.010)***	(0.013)***
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	0.07	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 15: 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	-0.069	-0.069	-0.068	-0.069	-0.127	-0.067	-0.066
(in utero)	(0.022)***	(0.022)***	(0.021)***	(0.022)***	(0.028)***	(0.022)***	(0.022)***
No. of years exposed to violent events	-0.053	-0.053	-0.048	-0.051	-0.099	-0.055	-0.048
(early childhood)	(0.014)***	(0.014)***	(0.014)***	(0.014)***	(0.020)***	(0.015)***	(0.014)***
No. of years exposed to violent events	-0.050	-0.054	-0.043	-0.050	-0.084	-0.053	-0.042
(pre-school age)	(0.015)***	(0.015)***	(0.015)***	(0.015)***	(0.021)***	(0.015)***	(0.015)***
No. of years exposed to violent events	-0.020	-0.019	-0.013	-0.017	-0.044	-0.020	-0.011
(primary school age)	(0.014)	(0.015)	(0.014)	(0.014)	(0.020)**	(0.015)	(0.014)
No. of years exposed to violent events	-0.003	-0.002	0.006	-0.000	-0.014	-0.005	0.006
(high school age)	(0.013)	(0.013)	(0.013)	(0.013)	(0.019)	(0.013)	(0.013)
Gender (male=1)	0.422	0.412	0.418	0.440	0.574	0.395	0.420
	(0.030)***	(0.030)***	(0.030)***	(0.031)***	(0.032)***	(0.030)***	(0.031)***
Mother's language (native=1)	-1.745	-1.760	-1.706	-1.751	-1.782	-1.759	-1.710
	(0.068)***	(0.069)***	(0.067)***	(0.064)***	(0.067)***	(0.075)***	(0.067)***
Constant	7.699	7.691	8.592	7.715	7.347	8.655	7.713
	(0.052)***	(0.053)***	(0.066)***	(0.052)***	(0.062)***	(0.069)***	(0.052)***
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	135502	133092	136140	136050	106226	129148	132744
R-squared	0.05	0.05	0.05	0.06	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.