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ABSTRACT

The Long-Run Labor-Market Consequences of Civil War: Evidence from the Shining Path in Peru*

This study exploits district-level variation in the timing and intensity of civil war violence to investigate whether early-life exposure to civil wars affects labor-market outcomes later in life. In particular, we examine the impacts of armed conflict in Peru, a country that experienced the actions of a tenacious, brutally effective war machine, the Shining Path, between 1980 and 1995. This study finds that the most sensitive period to early-life exposure to civil war violence is the first 36 months of life. A one standard deviation increase in civil war exposure leads to a four percent fall in adult monthly earnings. Neither fetal, nor pre-school, periods significantly affect long-run earnings. Substantial heterogeneity in the earnings impacts emerge when considering variation in the types of civil war violence. Sexual violations disproportionally affected the wages of women, while torture and forced disappearances disproportionally affected the wages of men. Evidence on intervening pathways suggests that health rather than schooling is the most important channel in connecting early-life exposure to civil war and adult earnings.

JEL Classification: I12, J13, O12, O15

Keywords: civil war, earnings, measurement error, instrumental variable approach, Shining Path

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1. Introduction

Violence against (and between) civilians is the most important attribute of civil wars. A minimum of eight out of ten people murdered in civil wars have been civilians (Kriger 1992). Unlike conventional wars, civil wars are associated with mass murder, forced disappearances, sexual assaults, and other types of extreme violence deliberately inflicted with the purpose of intimidating civilians through exemplary terror (Kalyvas 2006).

It is well documented the direct short-run effects of civil wars include the destruction of vital infrastructure, the collapse of institutions, a large number of displaced individuals, orphanage children, and a massive loss of life (Collier and Hoeffler 2003). Abadie and Gardeazabal (2003), for instance, illustrated the negative impact of terrorist violence in the Basque region of Spain on economic growth. Justino and Verwimp (2006) showed that one out of five Rwandans moved into poverty after the genocide. Yet, the long-run consequences of civil wars on human capital development, institutions, and social norms are still very unclear (see Blattman and Miguel 2009 for an excellent review of the literature).

In this study, we address whether early life exposure to civil war in Peru affects labor-market earnings later in life, following the critical-period programming theory (e.g., Godfrey and Barker 2000). For almost two decades, this Andean country experienced the actions of a tenacious, brutally effective war and political machine with no precedent in its modern history, the Shining Path. The social and economic losses from this spiral of violence were dramatic. Eight years after the beginning of the civil war, a special committee appointed by the Senate estimated the economic losses to be in US$9 billion dollars, equivalent to 66 percent of Peru's total foreign debt, or 45 percent of its GDP in 1988 (DESCO 1989). The production of violence

led to over 36,000 serious violence acts, including more than 24,000 documented killings and forced disappearances (TRC 2003).

A recent body of literature on the legacies of civil wars suggests that schooling (Chamarbagwala and Moran 2010, Shemyakina 2010, Leon 2009) and health outcomes (Akresh et al. 2007, 2009, Alderman et al. 2004) are negatively impacted by episodes of civil war; which, seen through the scope of human capital models, will inevitably affect total lifetime earnings of those affected. Direct evidence on the long-run labor-market consequences of civil wars, however, is still a missing gap in the literature. An exception is the work of Blattman and Annan (2007) who reported less schooling and work experience for former child soldiers in Uganda, and therefore, less success in their labor market outcomes as adults[1]. A somewhat different picture was obtained by Humphreys and Weinstein (2007), who found that increases in Sierra Leone combatants' violence exposure was weakly correlated with employability.

This study does not restrict labor-market analysis to former combatants, but rather uses a large national representative sample of civilians exposed to civil war at the very beginning of their lives. Moreover, we examine violence shocks several periods before and after birth to uncover evidence about the most sensitive or critical period: fetal, early childhood, and pre-school exposure[2]. Furthermore, this study uses a large number of cohorts in a representative sample of the national population that enables us to assess the long-run impacts of civil war violence, which improves over limited time horizons of most civil war datasets.

[1] This evidence is consistent with the findings in the U.S. and European literature for white veterans of conventional or international wars (see Hearst and Newman 1986; Angrist 1990).

[2] This paper has benefited from a recent body of research in economics that relates conditions in early life to outcomes in later life. This literature has helped in identifying shocks that have long-lasting effects, understanding the mechanisms of underlying shocks' persistence, and highlighting potential pathways connecting childhood and adult outcomes. So far, this work has primarily focused on the long-run effects of health or environmental shocks in early life on adult health (Maccini and Yang 2009, Alderman and Behrman 2006; Strauss and Thomas 1998) and schooling attainment (Glewwe and King 2001, Behrman and Rosenzweig 2004, Alderman et al. 2006)

The civil war in Peru began in the southern Sierra and spread to the rest of the country over a 15-year period. By exploiting regional variation in the timing and intensity of violence, we are able to identify its effects on labor-market earnings. Similar strategies were used in recent studies addressing the impact of civil war violence on human capital outcomes (e.g., Akresh et al. 2006, 2009; Chamarbagwala and Moran 2010; Shemyakina 2010).

We aim to advance on this literature by way of a systematic analysis of violence measures. First, we address measurement error in the violence dataset by using instrumental variables regressions where variables for violence measured in the closest 5 districts serve as instruments for violence in the individuals' district of birth. Measurement error, inherently affecting the majority of civil war datasets, has not been addressed in this literature (Blattman and Miguel 2009). Second, we use deviations, or transitory components, in the district-level violence around birth as an indicator of violence exposure early in life. This is a methodological advantage compared to the use of the actual intensity of violence that does not separate transitory shocks from secular trends in violence conditions. Likewise, the use of a very fine level of variation, at the district level, is an improvement over more aggregate (provincial and department) civil war datasets.

The distinction between civil war and violence in civil wars has been largely overlooked in the micro data analysis of civil wars (Kalyvas 2006). Accounting for civil war violence has primarily been based on a single specific measure of violence, including deaths (Chamarbagwala and Moran 2010) and abductions (Blattman and Annan 2009), length of exposure to civil war (e.g., Akresh et al. 2009, Leon 2009), and damage to household dwellings (Shemyakina 2010**).** In this study, we uncover evidence on the heterogeneity of civil war impacts by studying

separately five types of civil war violence: sexual violations, forced disappearances, abductions, killings, and forced detentions and torture.

We use two different sources of information to fulfill the data requirements of this investigation. To capture the socio-demographic and labor-market earnings of civilians, we use data from the 2006 and 2007 waves of the Peruvian household survey, Encuesta Nacional de Hogares (ENAHO), which interviewed approximately 22,000 households in both urban and rural regions in each year. The information on civil war measures comes from the Peruvian Truth and Reconciliation Commission (TRC), who collected a comprehensive dataset based on the reconstruction of the civil war period. We then link each individual in the household surveys to district-specific violence data according to the birth year of the respondent. In particular, we examine the earnings of working-age individuals born between 1974 and 1993, i.e., aged 14 to 34, in 2007, and thus, affected by the civil war violence during their first six years of life.

Five primary results emerge from this study. First, the most sensitive period to early-life exposure to civil war violence is the first 36 months of life. A one standard deviation increase in early childhood exposure to violence leads to a 4 percent fall in adult monthly earnings. Neither fetal, nor pre-school, periods are significantly related to adult earnings. Second, there is a large difference in the magnitude of the estimates emerging from standard linear and instrumental variables approach models, which suggests strong attenuation bias because of measurement error in the civil war dataset. Third, women are disproportionally affected by civil war violence relative to men. The magnitude of these differences reaches almost 4 percentage points. Likewise, civil war violence has disproportionally affected the long-run earnings of urban people, as compared to those living in rural locations.

Fourth, there is substantial heterogeneity in the impacts of civil war on adult earnings, depending on the type of civil war violence experienced early in life. Exposure to torture and forced disappearances yields the strongest negative impacts; sexual violations disproportionally affect the wages of women, while torture and forced disappearances disproportionally affect the wages of men. Focusing only on the most common types of violence, deaths and abductions, may underestimate the overall impact of civil war, as the psychological distress that attaches to other types of violence may have stronger long-lasting human capital impacts. Fifth, the analysis of the mechanisms connecting adult earnings and violence suggests that health, i.e., height, is the most important intervening channel. One standard deviation increase in armed violence during early childhood is significantly associated with one to two standard deviations lower height-for-age z-scores. On the other hand, schooling attainment does not explain the negative relationship between early-life exposure to civil war and adult earnings, as children are able to catch up over time.

The remainder of the paper is organized as follows. In section 2, we provide a background discussion about the Peruvian civil war. Section 3 describes the datasets and provides some descriptive statistics. In section 4, we discuss the empirical strategy along with the main results and sensitivity analyses. Section 5 addresses the study of violence on civil wars. Section 6 discusses potential pathways. Finally, Section 7 concludes.

2. Civil War Violence in Peru

In the earliest months of 1980, Peru witnessed the emergence of one of the world's deadliest terrorist groups, the Shining Path, a Maoist rebel group that was self-proclaimed as an

agent of a world history destined to conclude in a Communist Revolution[3]. The Shining Path initiated its actions as a focalized regional political movement in the Southern countryside of Peru by symbolically burning electoral ballots from the 1980 Presidential election in one of the poorest localities of the country. Unlike many other conflicts, the civil war in Peru does not follow the contest model's prediction in that armed conflicts flourish in resource rich regions because of the existence of more rents to fight over (e.g., Le Billon 2005). On the contrary, it follows Weinstein's (2006) typology of an 'activist rebellion,' where grievance trumped greed, participation was risky, short-term gains were unlikely, and highly-committed militants resembled investors dedicated to the cause of the organization and willing to make risky investments in return for the promise of future rewards (Weinstein 2006).

The initial response from the government was tardy and ineffective (Palmer 1992). The Peruvian Army began its operations against the Shining Path two years after the initial violent attack. Instead of using strategic force, along with rapid economic assistance, to bolster local economic conditions in the initially affected areas, police and military forces were accused of using indiscriminate violence against civilians (TRC 2003). This strategy did not cease, but rather fueled the expansion of the civil war[4]. Figures 1A to 1C illustrate the progression of the civil war: it began in the southern Sierra region and expanded northward and outward along the coastal cities. Three years after the Maoist group declared war against the "Old-State", the number of provinces affected by the political violence went from 37 to 72, including the capital,

[3] We follow the United Nations Security Council Report (2004) that defines terrorism as any act "intended to cause death or serious bodily harm to civilians or non-combatants with the purpose of intimidating a population or compelling a government or an international organization to do or abstain from doing any act." The analytical distinctiveness between rebel and terrorist groups is still an ongoing work in the literature.

[4] Kalyvas (2006) documents 100 studies and 45 historical cases where state violence against civilians provoked a greater insurgence violence, as a response.

Lima. By 1986, the civil war expanded to the jungle regions, as 46 percent of the total number of provinces was under the siege of political violence.

Beginning in August of 1987, the cycle of political violence worsened even more, when a new group, The Revolutionary Movement Tupac Amaru, began a cycle of violence against the government. Its actions were much less lethal than that of Shining Path, accounting for only 2 percent of the total number of killings and forced disappearances during the civil war period (TRC 2003). By 1989, the civil war expanded in all directions, covering 61 percent of the national territory. Finally, in 1995, almost 75 percent of the Peruvian provinces experienced the burden of civil war violence.

The end of civil war occurred before 1995 when Shining Path's founder and messianic leader, Abimael Guzman, was captured by a police intelligence operation. The Shining Path's political and war machine collapse abruptly. The end of the civil war in Peru illustrates new empirical evidence suggesting that insurgent leaders do matter (Guidolin and La Ferrara 2007).

Even though the Peruvian civil war seems to fit the 'activist rebellion' typology developed by Weinstein (2006), it does not follow its main prediction[5]. The Peruvian armed conflict was marked by deliberate indiscriminate violence against civilians (TRC 2003). The production of violence was provided by at least two competing parties, with the purpose of matching their opponent's violence to create fear in civilians to cut the other parties access to them. The number of serious violence acts, i.e., killings, forced disappearances, sexual

[5]The 'activist rebellion type' predicts that movements that arise in resource-poor contexts perpetrate into low levels of indiscriminate violence and employ violence selectively and strategically. On the other hand, the 'opportunistic rebellion type' predicts that civil wars emerging in rich natural resources areas tend to commit high levels of indiscriminate violence (Weinstein 2006). It highlights the limitations of building unified conceptual categories to describe diverse civil war movements. In this regard, we find useful the analytical boundaries of Kalyvas (2000), who defined a typology of civil war based on the interaction of two key elements of violence: its purpose and its production. According to this typology, the Peruvian armed conflict corresponds to 'civil war violence'.

violations, torture, forced recruitment, and abductions, climbed to over 36,000 violence events, including more than 24,000 documented killings and forced disappearances (TRC 2003).

This civil war violence evolved over the course of almost two decades, creating substantial regional variation in the timing and intensity of the violence. Figure 2 show the time series of these violence events. During the 1980-1982 period, the number of violence acts ranged between 100 and 800, then the intensity of the civil war increased vertiginously over the next three years, reaching its peak in 1984 with almost 6,000 violent acts. This was followed by a period of relative peace between 1985 and 1987, a period in which the violence intensity dropped almost five times, with respect to the peak year of 1984. In 1988, however, the cycle of violence erupted again, reaching its second peak in 1989-1990 and lasting until 1993. Starting in 1994, the number of violent acts decreased vertiginously, until it faded in 1997.

Figure 3 exhibits substantial regional variation in the intensity of civil war violence. While approximately 25 percent of the provinces never experienced a single act of violence between 1980 and 1995, 32 percent suffered fewer than 20 violent acts, 17 percent experienced between 100 and 500 violent attacks, and 9 percent were affected by over 500 acts of civil war violence. To put the numbers into perspective, if all provinces had suffered the same level of violence as the most affected ones, the national death toll would have been 450,000 instead of 25,000 (TRC 2003).

Therefore, the intensity of early life exposure to the cycle of armed violence depends on where and when the individuals were born. Some districts did not experience a single violent act, where other districts lost over 5 percent of their population. The years 1984 and 1989 were extremely violent, while 1986 was relatively peaceful. Some districts experienced their first deaths in 1981, while others experienced them in 1990. If one believes in the “fetal origins

hypothesis" (Barker 1998), the cohorts born in 1985, for instance, would be more adversely affected than the cohorts born in 1986. In the next few sections of this paper, we exploit these regional differences in the timing and intensity of violence to estimate the causal link between early-life exposure to violence and long-run labor-market earnings.

3. Data Sources

We used the 2006 and 2007 waves of the Peruvian household survey, Encuesta Nacional de Hogares (ENAHO), conducted yearly by Peru's national statistical agency, the Instituto Nacional de Estadistica e Informatica. The surveys provide current demographic, socio-economic, and labor-market information on a national representative sample of households and individual household members, including children.

During each year, approximately 22,000 households are interviewed across all 24 states in both urban and rural regions[6]. The ENAHO includes information on the district of birth and date of birth, to which we link civil war data. Individuals aged 14 and older were subject to the employment and earnings module, which provides detailed information about the current labor-market status and earnings of economically active individuals. We thus limit the analysis to working-age individuals aged 14 to 34 during the year of the survey (i.e., civilians born between 1974 and 1993), and thus, affected by civil war violence during their first six years of life.

Civil war data was obtained from the Peruvian Truth and Reconciliation Commission (TRC), a non-judicial temporary body established in June 2001, with the mandate to examine and collect information about the country's civil war period[7]. Its work focused on violence acts,

[6] The data, as well as the technical details, of each survey are publicly available at http://www.inei.gob.pe/

[7] More than 30 countries have used similar mechanisms over the past 25 years to assist them in the post-conflict transition toward democratic futures (Hayner 2006).

as long as they were imputable to terrorist organizations, state agents, or paramilitary groups (Supreme Decrees N^{o} 065-2001 and 101-2001). The TRC work, formally concluded on 2003, constitutes a "historical memory" that documents extensive, detailed information from victims, survivors, and other witnesses.[8]

The violence data collected by the TRC comes from the reconstruction of violence acts that took place in Peru between 1980 and 2000. Every single instance of civil war violence was coded as an event in a given space and time and placed systematically within a sequence of events. For each documented act of violence, there is information about the location, time, victim, and perpetrator. Overall, more than 36,000 violence events were reported.

We link each individual in the ENAHO data to the violence data for their district-specific birth year. For a given district in a given quarter, we define three explanatory variables: 'fetal exposure', defined as the sum of violent acts in the 4 quarters previous to the birth date; 'early childhood exposure,' defined as the sum of violent acts during the first 12 quarters of one's life and; 'pre-school exposure,' defined as the sum of violent acts between the 13 and 24 quarters of one's life. We compute these variables in the TRC dataset and merge this information with each birth-district/birth-date combination represented in the ENAHO sample.

Table 1 reports the summary statistics for the final matched sample. It is composed of 40,268 workers aged 14 to 34, after including non-remunerated family workers. The average individual in our sample is 22.7 years old with 9.4 years of schooling, although there is large schooling differences between urban (10.9) and rural (7.68) areas. Only 33.7 percent of the sample is married, which is roughly the same across all subsamples. Almost half of the individuals live in urban areas, where 10 percent do not speak Spanish as their first language. This percentage increases to 40 percent in rural areas. The average per capita household income

[8] The TRC's final report is available at http://www.cverdad.org.pe/ingles/ifinal/index.php.

is three times higher in urban areas than in rural areas. The proportion of working men is higher (55 percent), relative to that of women (45 percent). The mean monthly earnings are 277 soles (or about US$ 100), after inputting zero earnings for non-remunerated family workers. Large earnings gaps are observed between men and women and between urban and rural areas.

The second panel in Table 1 shows that the average individual in our sample was exposed to two, seven and nine violence shocks while in utero, early childhood, and pre-school periods, respectively. This distribution is primarily the same across all subsamples. One can also observe the (unconditional) mean monthly earnings by the intensity of violence in the last panel. There is a monotonic distribution for adult earnings and the intensity of violence when the violence measure is the early childhood exposure across all subsamples. This pattern is less obvious when considering fetal and pre-school violence measures.

4. Empirical Strategy and Results

4.1 Identification

The identification strategy relies on a difference–in-differences approach, where we test whether children born in districts affected by armed violence have more adverse labor-market earnings later in life, than their counterparts born before or after in the same district, relative to those who are born in other regions of the country during the same year. The same strategy was used in recent studies addressing the impact of civil war violence on schooling and health (Akresh et al. 2006, 2009; Leon 2009; Chamarbagwala and Moran 2010; Shemyakina 2010).

We aim to advance on this literature by way of a systematic analysis of the intensity of violence measures. To accomplish this, we first define the intensity of violence by deviations or

transitory components of armed violence from the mean for one's birth district. Mean district violence for a particular individual is calculated over the violence period, and excludes the violence experienced during the individual's birth period[9]. Measuring exposure to armed violence simply by the level, or number, of violence acts may be dominated by district-specific secular developments over time. This approach, pioneered by Bengtsson and Lindstrom (2000), in their study about famine and mortality, has the advantage of separating transitory shocks from secular trends in violent conditions within districts. At the individual level, transitory movements in armed violence involve unexpected shocks.

Second, the potential effects of attenuation bias, due to measurement error, are considered. While the work of the *TRC* constitutes the most comprehensive effort in the reconstruction of the civil war in Peru up to date, it is not free of measurement error. Unreported or undocumented violence acts, for instance, may cause the *TRC*'s measurements to be only imperfectly correlated with actual violence in the individuals' birth district. If this is the case, the standard least squares estimates will be biased and attenuated toward zero (Wooldridge 2005). Therefore, we instrument violence shocks with alternative measures of the same variable, measures whose errors are likely to be orthogonal to the original, instrumented variable under the assumptions of classical measurement error. A similar approach is used, for instance, in Maccini and Yang (2009), when addressing measurement errors in rainfall variables.

We implement a two-stage least squares model where, in the first step, the main regressor (V_{ijt}^{0}), the transitory shocks of violence for individual *i* in district *j* born in time *t*, is instrumented

[9] The mean early childhood exposure to violence is computed for two individuals born in the same district during the quarters *q* and *q+1*, as the average of the number of violent acts in quarters {*q+1,...,q+12}* and *{q+2,...,q+13}*, respectively. This means that there is no full overlap in the mean violence of individuals born in adjacent quarters and the differences increased over time. Thus, the overlap breaks completely for individuals born in quarter *q* and *q+13*, *q+1* and *q+14*, and so on. The same procedure is used when constructing fetal and pre-school transitory measures of violence.

with five analogous violence variables, measured during the same time span, but in the closest five districts (V_{ijt}^{v}):

$$V_{ijt}^{0} = \alpha_0 + \sum_{v=1}^{5} V_{ijt}^{v} + \mu_{ijt} \qquad (1)$$

This first stage regression for fetal, early childhood and pre-school exposures to birth district violence is reported in Appendix A. One can observe that the coefficients for all instruments have a positive and statistically significant relationship with the violence variable in the birth district. It is clear that the higher the geographic proximity with the birth district, the higher the correlation and significance of the instruments. Moreover, the test for the joint significance of the instruments passes easily conventional threshold levels used for detecting weak instruments in linear IV regressions (Staiger and Stock 1997, Stock and Yogo 2005)[10].

The estimation of the relationship between adult labor-market outcomes and early life exposure to violence is based on a reduced-form linear regression model for individual *i* in district *j* and birth year *t*,

$$y_{ijt} = \beta_1 + \beta_2 \tilde{V}_{ijt} + X_{ijt}'\beta_3 + \eta_j + \tau_t + \beta_4 TREND_{jt} + \varepsilon_{ijt}, \qquad (2)$$

where $\tilde{V}_{ijt}$ represents the instrumented transitory component of armed violence, η_j represents the fixed effects for district *j*, which controls for the persistent effects of violence on the districts where individuals are born, and τ_t represents the fixed effects for birth year *t* to control for the specific cohort-effects .The parameter of interest is β_2, which represents the impact of early-life exposure to civil war violence on adult labor-market earnings *y*. Identification of the impact comes from comparing individuals exposed to different shocks of violence, while isolating the

[10] Weak instruments in linear IV regressions can result in biased estimates and confidence intervals with actual coverage rates far from their nominal values. Staiger and Stock (1997) and Stock and Yogo (2005) provide specific statistical values to detect weak instruments. In the case of one endogenous regressor, for instance, instruments are deemed weak if the first stage F-test is less than ten.

persistent effects of violence in the birth district and birth cohort through district- and age-fixed effects. However, it may not identify the causal impacts, had the timing of the shocks followed a particular pattern in terms of district-level characteristics related to changes in the outcomes of interest over extended periods of time. For this reason, we include a district-specific linear trend ($TREND_{jt}$) to isolate variation in individuals' outcomes that diverge from long-running trends in one's birth district.

We also include a set of socio-demographic control variables (X) including gender, schooling attainment, marital status, region of actual residence (i.e., urban or rural), and ethnicity (i.e., mother's tongue is Spanish, Quechua or Aymara). ε_{ijt} is the idiosyncratic mean-zero error term and is assumed to be distributed independently of all η_j and τ_t. The errors might be correlated across time and space. To avoid potential biases in the estimation of the standard errors, we allow for an arbitrary covariance structure within districts by computing the standard errors clustered at the district level (Moulton 1986, Bertrand et al. 2004). Finally, we consider four separate subsamples, i.e., men, women, urban, and rural in the econometric analysis, given the evidence that some groups are more vulnerable to violence than others.

4.2 Long-Run Labor-Market Earnings Impacts

Table 2 reports the standard least squares estimates (henceforth, OLS-FE) for adult earnings. From the top of Table 2 down, we present five panels: the full sample, men, women, urban, and rural subsamples. Columns 1 to 3 depict the impact of fetal, early childhood, and pre-school exposure to violence, respectively. Clustered standard errors are shown in parentheses. For the interpretation of the results, this study focuses on the impact of one standard deviation in the transitory violence measures, which amounts to 10, 26, and 26 for fetal, early-childhood, and pre-school measures, respectively.

The OLS-FE estimates show that the most sensitive period to early-life exposure to civil war violence is the first 36 months of life. A one standard deviation increase in childhood exposure to violence leads to a reduction of 7 soles in monthly earnings, which is equivalent to a 2.5 percent fall (relative to the mean earnings of individuals aged 14 to 34). This result is statistically significant at the 1 percent level. At the same time, we find statistically insignificant impacts for both fetal and pre-school violence exposure, with point estimates close to zero. This result suggests that exposure to civil war while *in utero* or pre-school periods are less important predictors of labor-market success, with respect to violence shocks occurring during the weaning and post-weaning life periods. These results are consistent with the findings in Maccini and Yang (2009) that document that childhood, rather than fetal exposure to environmental conditions early in life, has the largest impacts on adult socioeconomic and health outcomes.

It is important to recognize, however, the potential role for the serial correlation of shocks in explaining these results. It is possible that the significant effects we find for early childhood exposure to violence reflects the correlation of violence measures across time. To address this point, we include all fetal, early childhood, and pre-school violence measures in the same regression. If the coefficients associated to early childhood change significantly, then there is evidence of a serial correlation over time, which undermines the evidence that early childhood exposure to armed violence matters *per se*. Column 4 shows the new point estimates. By looking at the sign and magnitude of the estimates, one confirms that exposure to early childhood violence significantly affects long-run labor-market earnings. The point estimates and corresponding standard errors are pretty similar to those observed in column 2, suggesting that early childhood exposure to violence shocks matters on its own.

Panels B and C report significant gender differences in the impacts of civil war on adult earnings. Women are more adversely affected than men. A one standard deviation increase in the intensity of violence in early childhood leads to a significant 4 percent reduction in monthly earnings for women, while the magnitude of the impact on men is almost half and not statistically significant. This result is consistent with several micro data analyses that illustrate a disproportional impact of civil war violence on schooling and health for civilian women across diverse geographical locations, including Asia (Shemyakina 2010), Africa (Akresh 2009), and Latin America (Chamarbagwala and Moran 2010, Leon 2009). On the other hand, neither fetal, nor pre-school, exposure to violence appears as relevant predictors for adult earnings.

Finally, Panels D and E show significant differences across urban and rural localities. Exposure to civil war in early childhood disproportionally affects the long-run earnings of civilians living in urban districts more than their rural counterparts. We provide three potential explanations for this finding. First, it is possible that children that experience the worst types of violence in rural areas migrated later to urban cities, where their parents feel relatively safer. They may have "imported" long-lasting physiological and structural effects of environmental conditions around birth to urban areas, which, along with their own difficulties, implies that the displacement process may have worsened their human capital endowment over time. Second, the likelihood of measurement error in the computation of civil war intensity may be greater in rural than urban areas. Therefore, the OLS-FE estimates for rural areas may reflect higher attenuation bias than the estimates for urban areas. The IV-FE estimates we present in Table 3 will shed some light on this issue. Third, the outcome of interest (earnings) has a higher variance in urban districts than rural ones; thus, the standard errors in the urban estimates are relatively larger. This, in turn, reduces the statistical significance of the point estimates for rural areas.

Table 3 reports the instrumental variable estimates (henceforth IV-FE) following the same logical arrangement as Table 2. Considering the measurement error in the violence data yields almost a two-fold increase in the magnitude of the point estimates with respect to the OLS-FE model, yielding an overall negative impact of about 4 percent in monthly adult earnings. This finding reveals considerable attenuation bias in the estimation of long-run earnings impacts when neglecting measurement error in the violence data. Moreover, the IV-FE estimates illustrates significant negative earnings impacts associated with early childhood exposure to civil war violence for all subsamples, including men and rural persons. The magnitude of the point estimates reveal that a one standard deviation increase in early childhood exposure to civil war is associated with a 6.5 and 2.7 percent fall in monthly earnings for women and men. These results are statistically significant at the 1 and 5 percent level.

Likewise, the long-run earnings of both urban and rural people are significantly affected by early childhood exposure to civil war, exposure that causes a 4 percent drop in monthly earnings for both groups. This suggests that accounting for measurement error may be more important for rural than urban areas. However, the statistical significance of the urban impacts is stronger (1 percent level) than that of rural areas (10 percent level).

Overall, the qualitative findings from the standard least squares model holds after accounting for the measurement error. In particular, the most sensitive period for early-life exposure to the civil war is during the first 3 years of life. Neither fetal, nor pre-school, periods appear as significant periods when estimating the impacts of violence on earnings later in life. Moreover, early childhood exposure to armed violence matters in and of itself as the point estimates do not change after simultaneously including all three measures of violence. Likewise,

women are disproportionally affected by civil war violence relative to men. The magnitude of this difference reaches almost 4 percentage points.

4.3 A Specification Test

A potential econometric concern is that early childhood exposure to violence shocks could be spuriously capturing the effect of unobservables correlated with idiosyncratic district-level characteristics that, in turn, affect individuals' labor-market productivity. Because this study focuses on the effects of early-life violence exposure following the extensive critical-period programming theory, we use, as a specification test, the cohort of adult workers who were exposed to civil war violence as adults but were old enough to have completed formal schooling, (non) cognitive development, and health risk factors before the civil war begins. We focus then on the cohort aged 17 and older in 1980. Thus, any significant long-run earnings impact for this particular group may suggest that some (unobservable) factors unrelated to environmental conditions around birth are also in play. If this is the case, the point estimates we reported in Tables 2 and 3 could be also capturing the effect of those uncontrolled variables.

Table 4 reports the IV-FE estimates for individuals aged between 44 and 65 in 2007, i.e., civilians born between 1942 and 1964. The variable of interest, intensity of violence exposure, is measured as the number of violence acts in one's birth district during the civil war period. Clustered standard errors are shown in parenthesis. Columns 1 and 3 in Table 4 show small and statistically not significant point estimates. This finding holds for all sub-samples and is not driven by potential measurement error, as the estimates are based on the instrumental variables approach model.

Unlike children exposed to violence around birth, adults are more mobile and can decide to change their place of residence over time. Thus, one potential problem with these results is that the violence measure may not reflect the actual exposure to violence for adult workers, as they could have migrated across localities during the civil war period. Unfortunately, we cannot observe the date of migration for each individual in the dataset, but we can identify whether individuals have migrated or not. With this caveat in mind, we analyze the relationship between migration status and civil war measures for adults aged 44 to 65. The results are reported in columns 2 and 4 in Table 4. One can observe that there is no systematic statistical relationship between migration and adult exposure to civil war after controlling for district fixed effects, birth year fixed effects, and a district-specific linear trend. This finding holds for all subsamples[11].

The differentiate impact between early childhood and adult exposure to civil war suggests that our results are not due to an omitted changing trend. Civil war could be spuriously capturing the effect of unobservables if those uncontrolled variables are only correlated with early childhood exposure to civil war, but not adult exposure.

4.4 More Sensitivity Tests

In this section, we use several alternative strategies to test the robustness of the impacts of early childhood exposure to civil war on adult earnings. First, we add to the baseline regressions a set of economic control variables. There is evidence that cyclical macroeconomic conditions during childhood can have effects on household income and mortality rates later in life (Van den Berg et al. 2006). In particular, we include two macro-economic indicators:

[11] We also estimated violence impacts for the subsample of adult workers aged 44-65 who, in 2007, were still living in the same localities that they were born. These unreported estimates do not change the picture a bit: the coefficients for labor-market earnings are negligible and not statistically significant for all subsamples.

inflation and economic growth rates, indicators that illustrate a large variance during the period of analysis.

Column 2 in Table 5 reports the IV-FE estimates for early childhood exposure to civil war after including economic control variables. Column 1 shows the baseline results for comparison purposes. The point estimates (and standard errors) for early childhood exposure to civil war do not change with respect to their baseline estimates. All quantitative findings of this study hold after including the macroeconomic covariates to our reduced-form model. It is clear that by using a very fine level of variation, districts, and by including a district-specific time trend, we adequately control other time varying factors that may be correlated with the civil war.

Second, the results for monthly earnings, so far, have been based on a sample of working individuals that include non-remunerated family workers. By default, the labor-market earnings of these individuals are inputted as zero. In column 3, we exclude non-remunerate family workers from the analysis, so that the estimates can better reflect the workers' market value productivity. One clear pattern emerges: the point estimates are significantly larger than the baseline ones for all subsamples but men and remain statistically significant. Relative to the mean earnings of remunerated workers, the magnitude of the estimates remain almost the same: 7 and 2 percent reduction in monthly earnings for women and men, and 4.8 and 3.4 percent reduction for civilians living in urban and rural districts.

Third, it is important to consider the potential effects of districts that did not experience a single act of political violence during the period under analysis. This may bias the results, as long as these districts may present idiosyncratic differences with respect to the rest of the country. We therefore exclude from the analysis districts that did not experience a single act of armed

violence during the civil war period. This approach allows us to exploit differences in the timing and intensity of violence within districts that experienced armed violence. Column 4 in Table 5 reports the new point estimates for early childhood exposure to civil war. Once again, all quantitative and qualitative findings hold. The main results do not change with all coefficient estimates significant at similar levels.

Finally, we explore how sensitive our results are to migration status. We expected low levels of migration (for children and their parents) around birth time. Yet, it is a possibility that we should consider in our estimates. In column 5, we use a dummy variable as the dependent variable, indicating whether a person migrated or not from her/his province of birth. The coefficients are negligible and statistically insignificant. There appears to be no systematic effect of early childhood exposure to armed violence on migration out of the province. This does not mean that individuals do not migrate at all, as many families could have migrated out of their localities over time.

5. Types of Civil War Violence

There is an analytical distinction between civil war and violence in civil wars. Violence against (and between) civilians is implemented through different channels, with the purpose of shaping individual behavior by attaching a cost to particular actions (Kalyvas 2006). For instance, forced disappearances may have stronger psychological and mental impacts than the number of deaths in combat, as it may prevent relatives of the disappeared persons from moving on with their lives. Likewise, sexual violations can produce the highest levels of stress, fear, and distrust in women. This distinction has largely been overlooked in the micro data analysis of civil wars because of lack of information. Accounting for civil war violence is mostly based on

measures of a single specific measure of violence, including deaths (Chamarbagwala and Moran 2010) and abductions (Blattman and Annan 2009), length of exposure to civil war (Akresh et al. 2009, Leon 2009), and damage to household dwellings (Shemyakina 2010**).**

In this section, we introduce some conceptual distinctions in the rationale and purpose of different types of violence exercised over the civilian population, with the aim of uncovering evidence about the heterogeneity of civil war violence on long-run earnings. We focus on five different types of violence: forced detentions and torture, forced disappearances, sexual violations, abductions, and killings. The qualitative evidence collected from thousands of oral testimonies by the TRC is illustrative on how these different types of violence have shaped individuals fate (TRC 2003).

Torture is a term with conceptual autonomy and is typically synonymous with 'torment'. Torture follows forced detentions. Its ultimate purpose depends on whether it is exercised by the State, or the insurgents. In the former case, the fundamental purposes are to extract information, force self-confessions and incentive denunciations of those individuals suspected of collaborating with the insurgents. In the latter case, the essential purpose is the 'exemplary punishment' of civilians who are not collaborating enough with them or are collaborating too much with the State. Shining Path, for instance, used the systematic torture of civilians in the so-called "popular judgments", in which the whole town, or village, was forced to observe the torture of people so that civilians perceive themselves as potential future victims.

The systematic practice of forced disappearances is considered a crime against humanity (United Nations 1992). The victims of such cases are commonly kidnapped, tortured, and then killed; with the body disposed in such a way that no one can prove that the victim is actually dead. It is often used as a tool to control and intimidate civilians through pure terror. It prevents

relatives of the victims from moving on with their lives, as the search for the bodies will become a regular activity, an activity that directly impacts the household's welfare. The psychological distress is overwhelmingly high, because of the uncertainty and the anxiety experienced, particularly for children with parents that have disappeared (United Nations 1992). Currently, Peru is one of the countries with the highest number of forced disappearances in the world (United Nations 2002); 65 percent of the disappeared people are still missing (TRC 2003).

Sexual violation is a common source of violence particularly exerted particularly on women in countries torn apart by civil wars. Effective violence requires control. Its purpose is primarily to express power and dominance over the civilian population. Its aim is to punish, shame, and coerce civilians who are suspected of collaborating with the other party. Sexual violence has a profound impact on physical and mental health, with long-term consequences (Swiss 1998). It may also affect the economic and social well-being of the victims and their families, as stigmatization and isolation by the community (and their families) follow the act itself (Mollica 1989).

Abductions are considered a war crime (International Criminal Court Statute, 1998). The purpose of this type of violence is three-fold: as a source of economic income, as a control of the civilian population, and as tool of political pressure. In the first case, the victims of abductions are wealthy individuals who endure extreme living conditions while in captivity. This type of violence 'grease the wheel' of insurgencies, as the liberation will depend on ransom payments. Abductions are also used by rebels to force poor young civilians to join the insurgency (Blattman and Annan 2009). It directly affects the accumulation of human capital of the victims of these actions. Finally, abductions are also used as a political and propaganda tool to pressure the

government and to impact public opinion. In Peru, 66 percent of abductions during the civil war period were motivated by economic purposes (TRC 2003).

Killings, including mass murdering, are the epitome of civil war violence on civilians: eight out of ten individuals killed in contemporary civil wars have been civilians (Kriger 1992). Yet, it is difficult to isolate a unique purpose of killings and mass murdering, as they summarize the intensity of civil war violence. In some cases, they are used as a signal to show strength and determination; they are also used to cause extreme fear in the civilian population to cut one's opponents access to civilians. Particularly important in the Latin America region were the purpose of killings and the mass murdering of civilians to match the other party's violence.

Figure 4 shows the distribution of types of violence over time. Three patterns emerge. Torture and killings are the most common types of violence in the Peruvian civil war, as they represent 75 percent of all violent acts reported by the TRC. They are followed by forced disappearances (11 percent) and abductions (12 percent). Sexual violations, on the other hand, are the least common type of violence in Peru (less than 2 per cent), suggesting strong measurement error from misreporting due to stigma. Second, all types of violence show the same evolution over time, with three well-defined peaks in the cycle of violence: 1983-1984, 1989-1990, and 1992-1993. For instance, the number of killings in 1983 shows a six-fold increase with respect to the previous year, an increase that is also observed across all types of violence. Third, all types of violence declined abruptly beginning in 1993, one year after the capture of Shining Path's leader Abimael Guzman.

To analyze the heterogeneous long-run impacts of types of civil war violence, we implement the same reduced-form approach implemented in section 4. All the econometric details hold, with the exception of defining violence measures separately, according to each

particular type of violence. Tables 6 and 7 report the OLS-FE and IV-FE estimates. In each table, we present five top down panels: the full sample, men, women, urban, and rural sub-samples. Each column shows the results for each type of violence estimated separately after including violence measures for all three periods: fetal, early childhood, and pre-school ones.

Table 6 depicts consistent results with the main finding in the previous section: the most sensitive period of early-life exposure to civil war is the first 36 months of life. This is common across all violence measures, and therefore, it is not driven by a particular type of civil war violence. Neither fetal, nor pre-school, periods show significant negative impacts on long-run earnings. Overall, forced disappearances and torture show the strongest negative impacts on earnings in the long-run. A one standard deviation increase in exposure to forced disappearances (and torture) in early childhood leads to a reduction of 2.6 (and 1.9) percent of monthly earnings later in life. Surprisingly, killings have fewer impacts on long-run earnings, as the magnitude of the impacts is less than half of that for torture or forced disappearances[12].

When turning our attention to the gender dimension in Panels B and C, one observes that diverse types of violence shape the fate of men and women differently. Sexual violations disproportionally affect women, while torture and forced disappearances disproportionally affect men. On average, a one standard deviation increase in sexual violations is associated with a reduction of over 2.6 percent of monthly earnings for women, while exposure to forced disappearances leads to a 1.8 percent reduction of monthly earnings for men. It is worth noticing that all types of violence exposure in early childhood show statistically significant negative effects for both women and men, something that we did not observe when analyzing the earnings impacts for men when using aggregate measures of violence.

[12] The standard deviations for transitory measures of violence are 5.95 for forced disappearances, 3.64 for killings, 3.37 for abductions, 2.54 for torture, and 0.45 for sexual violations.

Finally, when looking at Panels D and E, one observes strong heterogeneous impacts between urban and rural areas. The results suggest that the disproportionally negative impact we found in section 4 against individuals living in urban areas is not driven by a particular type of violence, as the econometric analysis shows significant earnings impacts across all types of violence. Within urban areas, forced disappearances (-2.0 percent), torture (-1.6 percent), and sexual violations (-1.4 percent) are the types of violence with the largest negative impacts on monthly earnings. In rural areas, torture (-2.1 percent) and abductions (-1.7 percent) yield the largest violence impacts.

Table 7 shows the counterpart IV-FE estimates for types of violence. Similar to the evidence presented in section 4 on aggregate measures of violence, the magnitude of the estimates by type of violence is substantially larger than those emerging from standard OLS-FE models. For example, the IV-FE estimates shows that a one standard deviation increase in forced disappearances and torture leads to a 4.1 and 6.4 percent reduction in monthly earnings later in life, respectively. These results illustrate the importance of considering attenuation bias in most civil war datasets because of the intrinsic measurement difficulties in the computation of violence in countries torn apart for civil wars.

Setting aside the magnitude of the estimates, all main qualitative findings emerging from the standard least squares model holds after correcting for measurement error: torture and forced disappearances yield the strongest negative impacts on earnings later in life; sexual violations disproportionally affect the wages of women (-9.5 percent), while torture (-6.0 percent) and forced disappearances (-2.6 percent) disproportionally affect the wages of men. Finally, the wages of individuals living in urban districts were more affected than those of rural ones with

sexual violations (-5.2 percent), torture (-5.4 percent), and forced disappearances (-3.5 percent), as the most sensitive types of violence for urban people.

Unlike the standard OLS estimates, however, the correction for measurement error causes an increase in the standard errors, which, in turn, reduces the statistical significance of some outcomes, particularly for abductions and killings. We did not observe this pattern when correcting for the measurement error in the aggregate measure of violence in section 4. A closer examination of the first stage regression shows a plausible explanation. The instruments are relatively weaker when splitting the violence measures for type of violence. The statistical significance of the instruments (as well as the F-test) is lower in this case, than when predicting the aggregate measure of violence.

By combining Tables 6 and 7, some lessons emerge. First, the significant negative impacts associated with early childhood exposure to civil war is not driven by measurement error, or by a particular type of violence, as the findings are robust through different models and types of violence. Second, there is substantial heterogeneity in the computation of adult earnings impacts, depending on the type of civil war violence experienced in early life. Focusing only on specific types of violence, mostly deaths and abductions, may underestimate the overall impact of civil war on long-run labor-market outcomes as the psychological distress attaches to other types of violence and, most importantly, the aggregate impact of all types of violence have stronger human capital impacts later in life. Third, the disproportionally negative impacts on individuals living in urban districts relative to rural ones are consistent across all types of violence, which suggest that this finding is not forced by specific types of violence that could have predominated in urban areas. Fourth, all these results point out policy implications for post-

conflict policy responses. Particular attention should be given to those individuals that were exposed to early-life episodes of sexual violations, forced disappearances, and torture.

6. Pathways

There are a number of potential pathways by which episodes of civil war early in life might have induced lower labor-market earnings later in life. Human capital theory suggests that health and schooling are two prime predictors of adult productivity (Mincer 1993). Because this study focuses on the effects of early-life exposure to civil war, a period before entry-school year, it is most plausible that the initial direct effect of civil war violence is on the health and nutrition of infants, which predisposed to diseases, contributes to the cognitive development, and affects the accumulation of human capital over time. In this regard, height is a well-established measure to reflect early-life adverse conditions (Maccini and Yang 2009, Strauss and Thomas 1998) and appears to be significantly affected by civil war episodes (Alderman 2004, Akresh 2007, 2009).

To analyze the health channel, we use the 1993 Peruvian school census on height for children aged 6 and 7, which includes all individuals born in 1986 and 1987 that are enrolled in school[13]. These particular cohorts were born in a particular period of the civil war: while in utero, they experienced one of the most peaceful periods in the civil war (i.e., 1985-1986). However, as infants aged 1 to 6, they experienced one of the most brutal periods of the civil war (i.e., 1987-1993) as described in Figure 4. Table 8 presents the short-run impacts of civil war violence on height-for-age z scores[14]. The similarity between the qualitative impacts of civil war on adult earnings and civil war on children's height is very telling. The most sensitive period of early-life

[13] This data was collected by Peru's Ministry of Education. It also includes data for children aged 8 and 9 years, but they are severely underrepresented. While the number of children aged 6-7 years is about 500,000, the number of children aged 9 was less than 37,000. For this reason, we do not include this particular cohort in the estimation.

[14] Height-for-age z-scores is defined as the difference between the child's height and the mean height of the same-aged reference group (U.S. population), divided by the standard deviation of the reference group.

exposure to civil war on height is during the first 36 months of life. Neither fetal, nor pre-school, periods are negatively related to children's height. One standard deviation increase in armed violence during early childhood is significantly associated with 0.35 (OLS-FE) and 2.0 (IV-FE) standard deviations lower height-for-age z-scores[15]. Likewise, the height of girls is disproportionally affected by civil war relative to that of boys. The point estimates show 0.38 and 0.28 standard deviations lower height-for-age z-scores for girls and boys in the OLS-FE model; while these scores reach 2.60 and 1.36 standard deviations in the IV-FE model. Furthermore, the height of children living in urban districts is also disproportionally affected, relative to rural ones, with the magnitude of these differences closely resembling the gender gap.

Table 9 shows the short-run height estimates by types of violence. Once again, the similarity of the civil war impacts on adult earnings and children's height is worth noticing. By looking at the standard OLS estimates, one observes that forced disappearances, torture, and sexual violations appear to be the most hurtful type of civil war violence on civilians, yielding 0.26, 0.22, and 0.17 height-for-age z-scores lower standard deviations, respectively[16]. Moreover, sexual violations disproportionally affect the height of girls (0.23 standard deviations), while torture (0.28) and forced disappearances (0.26) disproportionally affect the height of boys. Furthermore, children living in urban areas were most affected by forced disappearances (0.43) and torture (0.26), while those in rural districts were disproportionally affected by torture (0.21).

Accounting for measurement error does not change any of the qualitative results, except that the increase in the size of the standard errors leads to a loss in statistical significance for torture and killing violence measures, as shown in Panel B. The magnitude of the point estimates suggests, once again, a strong attenuation bias for the standard linear model as the overall

[15] The standard deviation is 11.5 for the transitory measure of early childhood violence for this particular cohort.
[16] The standard deviations for the transitory measures of violence for this particular cohort are 0.29, 2.59, 0.80, 0.92, and 1.76 for sexual violations, forced disappearances, abductions, killings, and torture, respectively.

negative effects for forced disappearances and sexual violations show a large change, from 0.26 to 0.77 and from 0.17 to 1.92 height-for-age z-scores lower standard deviations, respectively.

These results suggest that exposure to civil war early in life has strong impacts on children's health, which seen through the scope of standard human capital models, can strongly affect both cognitive and non-cognitive skill development (Cunha and Heckman 2007). Improved health status among children contributes to school enrollment (Alderman et al. 2001) and improves school performance (Glewwe and King 2001). Therefore, schooling may constitute a second subsequent intervening channel. In the Peruvian context, Leon (2009) reports that children aged 6 to 17 and exposed to civil war violence accumulate more years of school deficit as children and less years of schooling as adults[17]. The magnitude of the OLS point estimates is, however, weak, resulting in 0.07 less years of schooling as adults when using the aggregate number of violence acts as a measure for early childhood exposure to civil war[18].

To analyze whether these weak schooling outcomes are driven by measurement error or by particular types of violence, we estimate the IV-FE long-run impacts of early childhood exposure to civil war on schooling attainment. We use the ENAHO data and restrict the sample to individuals aged 17 or older in 2006-2007 and, therefore, old enough to have completed high school. Panel A in Table 10 reports these results. In column 1, we observe the overall impacts without differencing by type of violence. The point estimates have the expected sign, but are negligible and statistically not significant. Likewise, by looking at types of violence in columns 2-7, one observes no clear patterns, except for killing measure, which is the only type of violence

[17] Leon's findings were based on the combination of the 1993 census demographic data with the same violence dataset used in this study. The Peruvian census data is, however, not publicly available.
[18] The estimated impact on long-run schooling attainment is larger (0.12) when the measurement for violence is the number of years exposed to civil war.

that shows negative and statistically significant results, ranging from 0.015 to 0.052 less years of schooling for one standard deviation increase in the exposure to killings.

Overall, the magnitude of the schooling estimates is weak and consistent with Leon's (2009) findings. This robust pattern suggests that, in the long-run, years of schooling is not a significant channel to explain the negative relationship between early-life exposure to civil war and adult earnings as children are able to catch up. However, there is large evidence of the negative relationship between household welfare and the quality of the education received in Peru (Valdivia 1997). As the civil war intensified poverty and inequality, families and communities lost property, and military spending crowded-out necessary public investment in education, the quality of schooling received for those most affected would be negatively affected. We cannot test directly this 'quality' channel, which constitutes a topic of interest for future research. We can, however, indirectly address this issue by examining the impact of early-life exposure to civil war on long-run household wealth.

Therefore, we constructed a wealth index that uses, as inputs, household assets including characteristics of the household's dwelling collected in the ENAHO dataset. By aggregating over ten household assets through factor analytic methods, this index represents a proxy for long-run economic status, rather than a measure of current welfare or poverty (Filmer and Pritchett 2001). Panel B in Table 10 reports the estimates using the same IV-FE empirical approach as before. The magnitude of the estimated coefficient is small, but statistically significant: a one standard deviation increase in early childhood exposure to civil war is associated with a 0.08 lower asset index later in life for women and urban people. Consistent with our previous findings, forced disappearances, sexual violations, and torture yield the largest impacts on household wealth, while abductions and killings show the lesser impacts.

All being told, these results provide suggestive evidence that health is the most important intervening pathway between early childhood exposure to civil war violence and adult earnings. It is not trivial that the TRC placed post-conflict health-related problems on Peru's national agenda. One of the final recommendations of the TRC was the establishment of health reparations, after uncovering direct evidence from almost 17,000 people about a host of psychosomatic problems, including chronic depression, generalized anxiousness, and stress (TRC 2003)

7. Conclusions

Using detailed information about the timing and location of armed violence in Peru along with nationally representative household surveys collected in 2006 and 2007, this study shows a significant effect of early childhood exposure to armed violence on labor-market earnings later in life. On average, and keeping everything else constant, a one standard deviation increase in violence leads to a 4 percent decrease in adult earnings. This negative effect has disproportionally affected women and individuals living in urban areas and is robust to a variety of sensitivity checks and econometric details. On the other hand, we did not find any significant effect of either fetus or pre-school exposure to armed violence on adult earnings. Of particular importance is the large difference in the magnitude of the estimates between the standard linear and instrumental variables approach models, which may suggests a strong attenuation bias in studies that do not address issues of measurement error in violence datasets.

The analytical distinction between civil war and violence in civil wars (Kalyvas 2000), largely overlooked in the micro data analysis of civil wars, proves to be useful in uncovering substantial heterogeneity of civil war impacts on civilians' outcomes. Forced disappearances and

torture yields the strongest negative impacts on earnings later in life; sexual violations disproportionally affect the wages of women, while torture and forced disappearances disproportionally affect the wages of men. Finally, the wages of individuals living in urban districts were more affected than that of people in rural areas with sexual violations, torture, and forced disappearances as the most sensitive types of violence for urban people.

The analysis of pathways connecting adult earnings and early-life exposure to violence suggests that the most plausible explanation lies in infant health, due to long-lasting physiological and structural effects, as well as through household wealth restrictions. In particular, the strong similarity in the qualitative findings about the relationship between civil war and adult earnings and civil war and children's height is very telling. On the contrary, the weak and not significant impact of the civil war on long-run schooling attainment suggests that children were able to catch up over time.

These findings imply that, from a policy standpoint, it is particularly useful to focus on children aged zero to three living in regions affected by civil war episodes. Their long-run productivity may be significantly increased if their conditions are improved, for example, by way of food, shelter, and health care provisions. Understanding the heterogeneity of civil war impacts is also important in the design of appropriate post-conflict policy responses, including the targeting of individuals or groups that were disproportionally affected by an armed conflict. For instance, the current heated debate on policy and political circles in Peru about the exact number of murdered people during the civil conflict is of second order importance to improving the conditions of individuals that were affected by particular types of violence, including forced disappearances, torture, and sexual violations.

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Appendix A: First-Stage Least Squared Estimates
Violence in birth district on violence measures at the closest 5 districts

Panel A: Fetal Exposure to Civil War				
neighbor 1	neighbor 2	neighbor 3	neighbor 4	neighbor 5
0.382	0.477	0.226	0.202	0.108
(0.007)***	(0.008)***	(0.009)***	(0.008)***	(0.006)***
F-test	3469			
R^2	0.30			
Panel B: Early Childhood Exposure to Civil War				
neighbor 1	neighbor 2	neighbor 3	neighbor 4	neighbor 5
0.561	0.541	0.238	0.111	0.074
(0.007)***	(0.009)***	(0.009)***	(0.008)***	(0.006)***
F-test	4496			
R^2	0.36			
Panel C: Pre-School Exposure to Civil War				
neighbor 1	neighbor 2	neighbor 3	neighbor 4	neighbor 5
0.373	0.770	0.216	0.085	0.151
(0.006)***	(0.010)***	(0.009)***	(0.008)***	(0.006)***
F-test	4526			
R^2	0.36			

Notes: Standard errors in parenthesis. Estimation includes individuals aged 14 to 34 in the 2006-2007 ENAHO data.
Violence measures come from the Peruvian Truth and Reconciliation Commission (TRC)
The intensity of violence is defined by deviations or transitory components of armed violence from the mean of one's birth district. Mean district violence is calculated over the violence peri and excludes the violence experienced in the individual's birth period.
*** Statistically significant at the 1 percent level.

Figure 1A: Civil War in Peru, 1980-1995

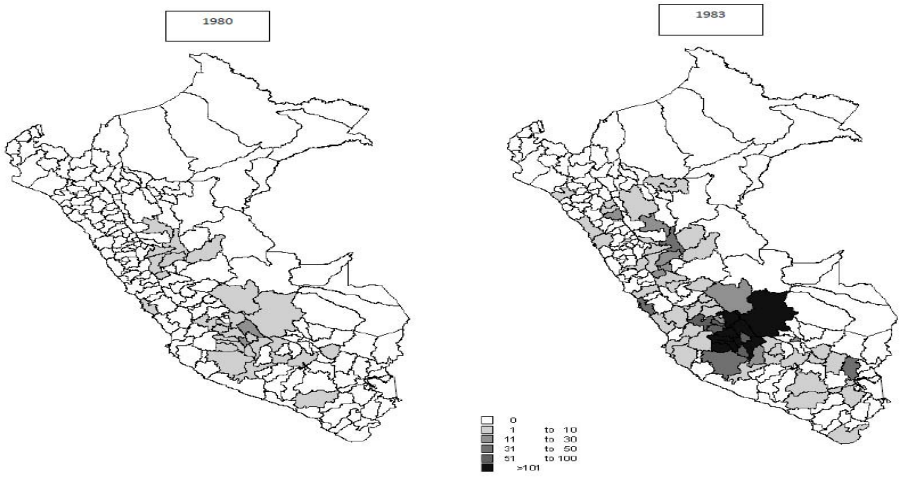


Figure 1B: Civil War in Peru, 1980-1995

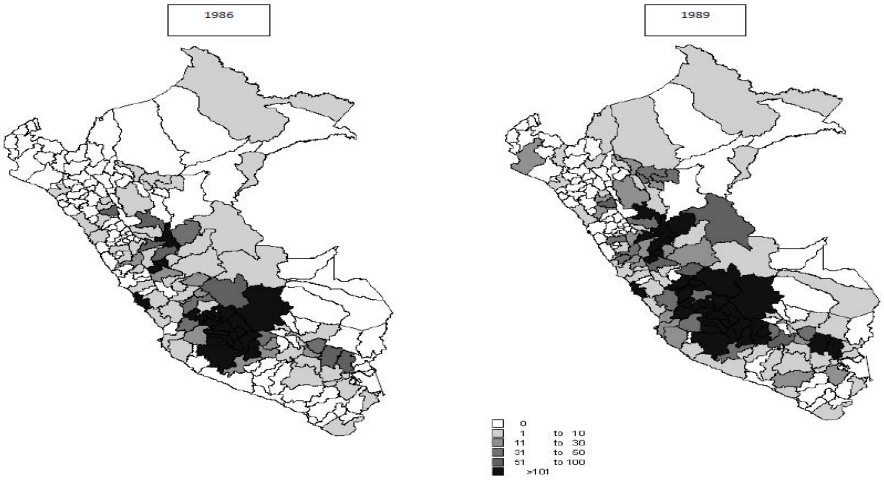


Figure 1C: Civil War in Peru, 1980-1995

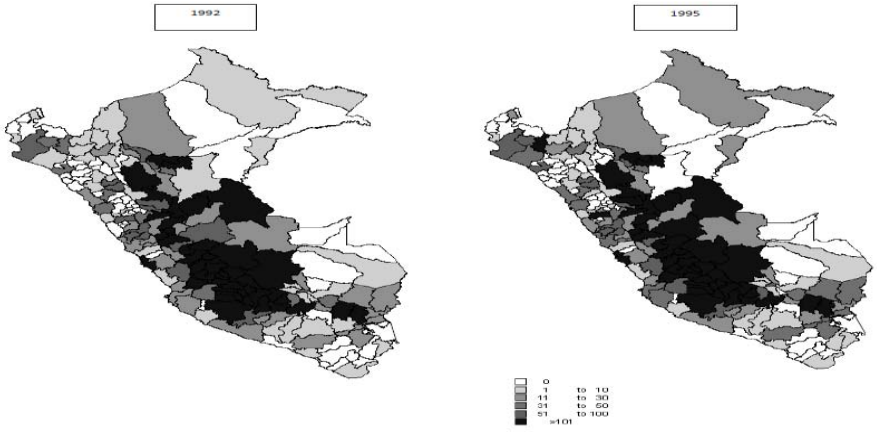
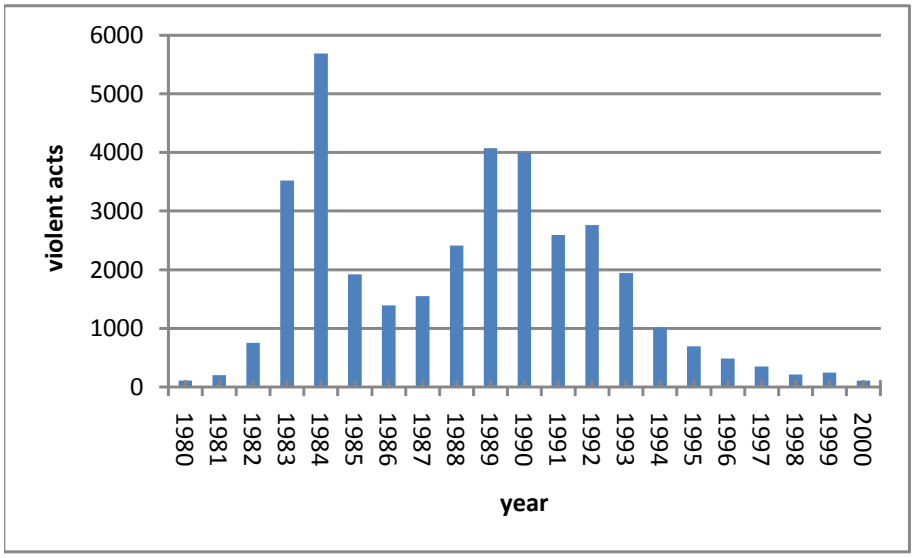
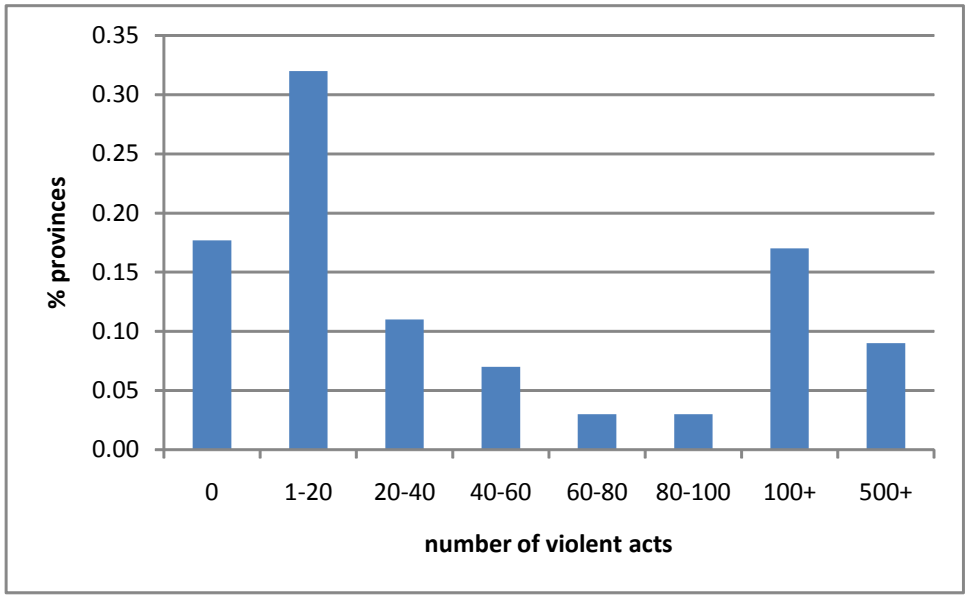


Figure 2: Number of Violent Acts by Year: Peru 1980-2000



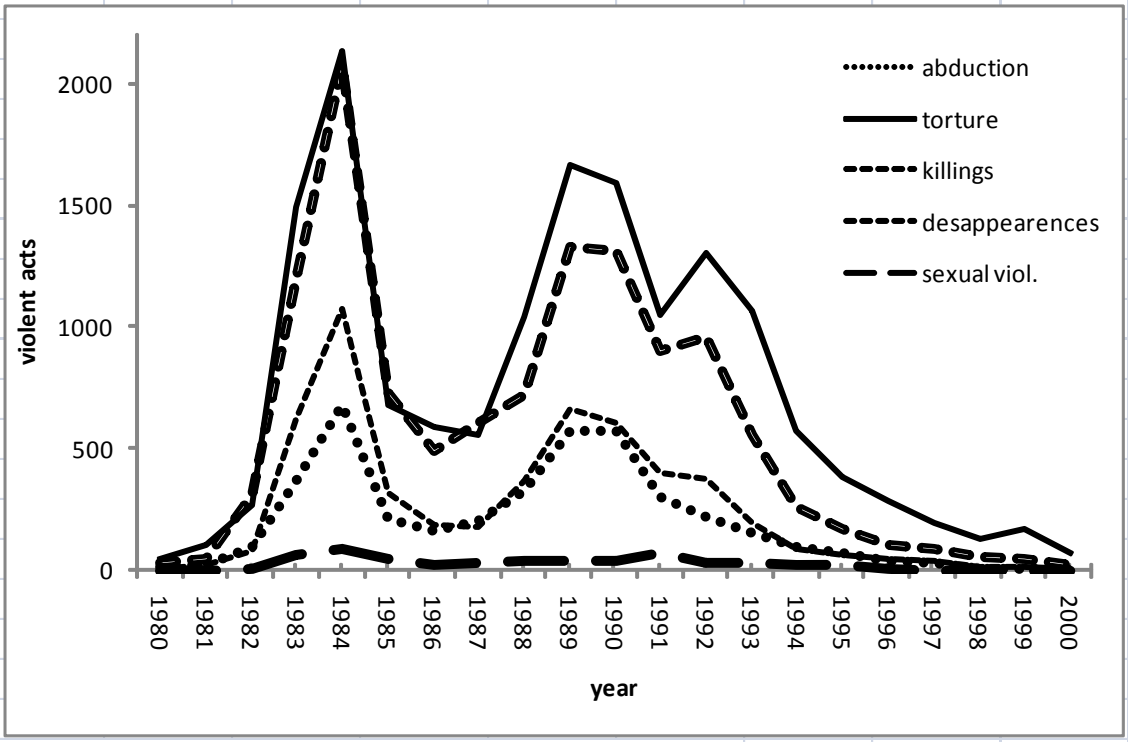
Source: Peruvian Truth and Reconciliation Commission Dataset.

Figure 3: Number of Violent Acts by Province, Peru 1980-2000



Source: Peruvian Truth and Reconciliation Commission Dataset.

Figure 4: Types of Violence by Year: Peru 1980-2000



Source: Peruvian Truth and Reconciliation Commission Dataset.

Table 1: Descriptive Statistics, ENAHO-Violence Matched Datasets, Workers Aged 14-34

	all	men	women	urban	rural
A. Socio-Demographics					
age	22.7 (5.5)	22.7 (5.5)	22.8 (5.5)	23.5 (5.2)	21.8 (5.7)
schooling	9.4 (3.5)	9.6 (3.2)	9.1 (3.8)	10.9 (3.0)	7.68 (3.25)
male (%)	55.0 (49.7)	-----	-----	54.4 (49.8)	55.9 (49.6)
married (%)	33.7 (47.2)	30.9 (46.2)	37.1 (48.3)	30.8 (46.1)	37.1 (48.3)
single (%)	62.7 (48.3)	67.4 (46.8)	56.9 (49.5)	64.5 (47.8)	60.4 (48.8)
rural (%)	45.6 (49.8)	46.3 (49.8)	44.7 (49.7)	-----	-----
Ethnicity					
castellano (%)	80.0 (39.1)	81.5 (38.8)	78.3 (41.2)	91.4 (27.9)	66.5 (47.1)
quechua (%)	16.1 (36.7)	14.6 (35.3)	17.8 (38.3)	7.0 (25.5)	26.9 (44.3)
aymara (%)	2.0 (13.6)	1.8 (13.4)	2.0 (14.0)	1.2 (11.3)	2.6 915.9)
Per capita annual household income (soles)	4342 (5857)	4257 (5559)	4448 (6202)	6084 (7207)	2267 (2316)
Monthy earnings (soles)	277 (473)	343 (510)	197 (411)	408 (561)	122 (268)
B. Violence Shocks					
Fetal exposure [-1 , 0 years]	2.0 (11.8)	2.0 (11.2)	2.1 (12.5)	2.1 (11.5)	2.0 (12.1)
Early childhood exposure [0 , 3 years]	7.3 (32.0)	7.4 (32.1)	7.3 (32.0)	7.9 (32.5)	6.7 (31.5)
Pre-school exposure [4 , 6 years]	8.6 (34.0)	8.5 (32.8)	8.7 (35.5)	9.9 (34.0)	6.9 (34.0)
C. Earnings by intensity of violence (soles)					
Fetal exposure					
[100, +)	106 (153)	141 (179)	66 (101)	168 (176)	45 (92)
[50, 99]	99 (176)	150 (217)	46 (94)	126 (204)	65 (126)
[10, 49]	123 (204)	148 (227)	90 (164)	167 (235)	69 (140)
[1, 9]	179 (289)	213 (306)	137 (262)	248 (330)	76 (167)
0	299 (500)	371 (538)	211 (435)	447 (593)	130 (282)
Early childhood exposure					
[100, +)	134 (224)	176 (234)	85 (202)	171 (265)	85 (140)
[50, 99]	152 9207)	181 (226)	111 (168)	193 (227)	92 (154)
[10, 49]	177 (282)	214 (303)	131 (247)	249 (317)	77 (184)
[1, 9]	228 (372)	276 (413)	168 (306)	307 (417)	93 (222)
0	311 (522)	387 (557)	219 (457)	219 (457)	134 (287)
Pre-school exposure					
[100, +)	246 (385)	293 (417)	187 (334)	312 (438)	133 (233)
[50, 99]	238 (310)	287 (344)	180 (251)	300 (339)	112 (280)
[10, 49]	249 (383)	299 (414)	186 (327)	333 (424)	98 (225)
[1, 9]	283 9485)	342 (495)	210 (463)	375 (555)	113 (242)
0	282 (489)	354 (537)	195 (407)	453 (598)	126 (281)
N	40268	22186	18082	21898	18370

Note: Matched sample includes individuals aged 14 to 34 in 2007. Fetal violence measure is defined as the number of violence acts in the four quarters right before birth. Early chilhood exposure is defined as the number of violence acts in the first 12 quarters after birth. Pre-school exposure is defined as the number of violence acts in the quarters 13 to 24 after birth. Socio-demographic variables comes from the 2006 and 2007 ENAHO datasets. Violence data comes from the Peruvian Truth and Reconciliation Commission. Earnings are measured in soles. The exchange rate is around 3 soles per 1 U.S$ dollar in 2006-2007.

Table 2: OLS-FE estimates of early-life exposure to civil war on long-run earnings

	(1)	(2)	(3)	(4)
Panel A: All sample				
fetal exposure [-1,0]	-0.100	-----	-----	0.074
	(0.188)	-----	-----	(0.130)
early childhood [0-3]	-----	-0.267	-----	-0.276
	-----	(0.105)***	-----	(0.093)***
pre-school [4-6]	-----	-----	0.008	0.009
	-----	-----	(0.054)	(0.051)
n=40268, R^2 =0.33				
Panel B: Men				
fetal exposure [-1,0]	-0.024	-----	-----	0.083
	(0.299)	-----	-----	(0.227)
early childhood [0-3]	-----	-0.208	-----	-0.219
	-----	(0.158)	-----	(0.142)
pre-school [4-6]	-----	-----	-0.067	-0.069
	-----	-----	(0.069)	(0.073)
n=22186 , R^2 =0.36				
Panel C: Women				
fetal exposure [-1,0]	-0.212	-----	-----	0.057
	(0.158)	-----	-----	(0.131)
early childhood [0-3]	-----	-0.303	-----	-0.308
	-----	(0.096)***	-----	(0.093)***
pre-school [4-6]	-----	-----	0.121	0.120
	-----	-----	(0.086)	(0.084)
n=18082, R^2 =0.33				
Panel D: Urban				
fetal exposure [-1,0]	-0.014	-----	-----	0.258
	(0.269)	-----	-----	(0.252)
early childhood [0-3]	-----	-0.357	-----	-0.378
	-----	(0.105)***	-----	(0.088)***
pre-school [4-6]	-----	-----	0.125	0.110
	-----	-----	(0.061)*	(0.062)*
n=21898, R^2 =0.29				
Panel E: Rural				
fetal exposure [-1,0]	-0.088	-----	-----	-0.015
	(0.135)	-----	-----	(0.113)
early childhood [0-3]	-----	-0.123	-----	-0.122
	-----	(0.084)	-----	(0.082)
pre-school [4-6]	-----	-----	0.019	0.019
	-----	-----	(0.072)	(0.075)
n=18370, R^2 =0.36				

Notes: Standard errors clustered by district of birth in parenthesis. In addition to district fixed effects, birth year fixed effect, and district-specific linear trends, OLS regressions include gender, schooling, sex, rural/urban indicator, and ethnicity. Estimation is based on individuals aged 14 to 34 in the 2006-2007 ENAHO data. The intensity of violence is defined by deviations or transitory components of armed violence from the mean for one's birth district. Mean district violence is calculated over the violence period and excludes the violence experienced in the individual's birth period.

*** Statistically significant at the 1 percent level; ** Statistically significant at the 5 percent level

* Statistically significant at the 10 percent level.

Table 3: IV-FE estimates of early life exposure to civil war on long-run earnings

	(1)	(2)	(3)	(4)
Panel A: All sample				
fetal exposure [-1,0]	-0.126	-----	-----	0.097
	(0.232)	-----	-----	(0.190)
early childhood [0-3]	-----	-0.427	-----	-0.429
	-----	(0.141)***	-----	(0.140)***
pre-school [4-6]	-----	-----	-0.231	-0.214
	-----	-----	(0.127)*	(0.132)
n=40246, R^2 =0.33				
Panel B: Men				
fetal exposure [-1,0]	-0.021	-----	-----	0.131
	(0.370)	-----	-----	(0.320)
early childhood [0-3]	-----	-0.343	-----	-0.355
	-----	(0.176)*	-----	(0.169)**
pre-school [4-6]	-----	-----	-0.172	-0.168
	-----	-----	(0.179)	(0.176)
n=22171 , R^2 =0.36				
Panel C: Women				
fetal exposure [-1,0]	-0.380	-----	-----	-0.091
	(0.274)	-----	-----	(0.268)
early childhood [0-3]	-----	-0.515	-----	-0.493
	-----	(0.171)***	-----	(0.178)***
pre-school [4-6]	-----	-----	-0.188	-0.171
	-----	-----	(0.226)	(0.236)
n=18075, R^2 =0.33				
Panel D: Urban				
fetal exposure [-1,0]	-0.466	-----	-----	-0.081
	(0.434)	-----	-----	(0.421)
early childhood [0-3]	-----	-0.687	-----	-0.677
	-----	(0.200)***	-----	(0.194)***
pre-school [4-6]	-----	-----	0.077	0.042
	-----	-----	(0.220)	(0.227)
n=21894, R^2 =0.29				
Panel E: Rural				
fetal exposure [-1,0]	0.040	-----	-----	0.127
	(0.218)	-----	-----	(0.244)
early childhood [0-3]	-----	-0.191	-----	-0.194
	-----	(0.101)*	-----	(0.105)*
pre-school [4-6]	-----	-----	-0.152	-0.134
	-----	-----	(0.116)	(0.121)
n=18352, R^2 =0.36				

Notes: Standard errors clustered by district of birth in parenthesis. In addition to district fixed effects, birth year fixed effect, and district-specific linear trends, regressions include gender, schooling, sex, rural/urban indicator, and ethnicity. Estimation is based on individuals aged 14 to 34 in the 2006-2007 ENAHO data. The intensity of violence is defined by deviations or transitory components of armed violence from the mean for one's birth district. Mean district violence is calculated over the violence period and excludes the violence experienced in the individual's birth period. Violence in individual's birth district is istrumented with violence shocks measured at the closest 5 districts to respondent's birth place.
*** Statistically significant at the 1 percent level; **Statistically significant at the 5 percent level;
* Statistically significant at the 10 percent level.

Table 4: Significance Test
IV-FE Impacts of Civil War on Long-Run Earnings of Adults, Aged 44-65

	Panel A: Men		**Panel B: Women**	
	earnings	migration	*earnings*	migration
	0.063	-0.0003	0.016	-0.0004
	(0.377)	(0.0003)	(0.242)	(0.0003)
N	14184	14184	11758	11758
R^2	0.22	0.35	0.49	0.36
	Panel B: Urban		**Panel D: Rural**	
	earnings	migration	*earnings*	migration
	0.347	-0.0002	-0.142	-0.0003
	(0.408)	(0.0003)	(0.201)	(0.0003)
N	14842	14842	11100	11100
R^2	0.28	0.36	0.16	0.39

Notes: Standard errors clustered by district of birth in parenthesis. In addition to district fixed effects, birth year fixed effect, and district-specific linear trends, regressions include gender, schooling, sex, rural/urban indicator, and ethnicity. Estimation is based on individuals aged 44 to 65 in 2006-2007 ENAHO data.
The intensity of violence is defined by deviations or transitory components of armed violence from the mean for one's birth district. Mean district violence is calculated over the violence period and excludes the violence experienced in the individual's birth period.
Violence in individual's birth district is istrumented with violence shocks measured at the closest 5 districts to respondent's birth place
*** Statistically significant at the 1 percent level;
** Statistically significant at the 5 level;
* Statistically significant at the 10 percent level.

Table 5: Sensitivity Tests, IV-FE Esimates
Early Childhood Exposure to Civil War on Long-Run Labor-Market Earnings

	Men					**Women**				
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*
early childhood civil war on earnings	-0.343	-0.343	-0.353	-0.307	----	-0.515	-0.515	-0.987	-0.506	----
	(0.176)*	(0.177)*	(0.184)*	(0.173)*	----	(0.171)***	(0.171)***	(0.281)***	(0.167)***	----
early childhood civil war on migration	----	----	----	-----	0.000027	-----	-----	-----	-----	0.000139
					(0.000248)					(0.000270)
N	22171	22171	16366	15692	22171	18075	18075	10095	12750	18075
R^2	0.36	0.36	0.32	0.33	0.30	0.33	0.33	0.33	0.30	0.32
	Urban					**Rural**				
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*
early childhood civil war on earnings	-0.687	-0.687	-0.953	-0.652	----	-0.191	-0.191	-0.325	-0.183	----
	(0.200)***	(0.201)***	(0.290)***	(0.195)***	----	(0.101)*	(0.102)*	(0.174)*	(0.105)*	----
early childhood civil war on migration	----	----	----	-----	-0.000102	----	----	----	-----	0.000121
					(0.000250)					(0.000158)
N	21894	21894	17455	17731	21894	18352	18352	9007	10711	18352
R^2	0.29	0.29	0.33	0.27	0.39	0.36	0.36	0.32	0.34	0.41
birth FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
district FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
district linear trend	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
macroeconomic covariates	no	yes	no	no	no	no	yes	no	no	no
excluding non-remunerated family workers	no	no	yes	no	no	no	no	yes	no	no
excluding districts with no armed violence	no	no	no	yes	no	no	no	no	yes	no

Notes: Standard errors clustered by district of birth in parenthesis. In addition to district fixed effects, birth year fixed effects, and district-specific linear trends, regressions include gender, schooling, sex, rural/urban indicator and ethnicity controls. The estimation includes individuals aged 14 to 34 in 2006-2007 ENAHO data.

The intensity of violence is defined by deviations or transitory components of armed violence from the mean for one's birth district. Mean district violence is calculated over the violence period and excludes the violence experienced in the individual's birth period.

Violence in individual's birth district is istrumented with violence shocks measured at the closest 5 districts to respondent's birth place.

*** Statistically significant at the 1 percent level;

** Statistically significant at the 5 level;

* Statistically significant at the 10 percent level.

Table 6: Types of violence on long-run earnings, OLS-FE Estimates

	sexual violations	disappearances	abductions	killings	torture
Panel A: All					
fetal exposure [-1,0]	-0.553	-0.098	0.051	0.000	0.244
	(2.828)	(0.277)	(0.319)	(0.269)	(0.751)
early childhood [0-3]	-8.630	-1.222	-1.082	-0.953	-2.047
	(4.528)*	(0.356)***	(0.254)***	(0.305)***	(0.638)***
pre-school [4-6]	-3.302	0.271	-0.035	-0.043	0.028
	(3.424)	(0.258)	(0.242)	(0.259)	(0.456)
N=40268, R^2 =0.33					
Panel B: Men					
fetal exposure [-1,0]	-0.224	-0.117	0.470	0.171	-0.130
	(2.971)	(0.219)	(0.281)*	(0.179)	(0.473)
early childhood [0-3]	-7.038	-1.058	-1.124	-0.951	-2.225
	(6.990)	(0.590)*	(0.474)**	(0.421)**	(1.062)**
pre-school [4-6]	-8.045	-0.001	-0.109	-0.065	0.366
	(5.183)	(0.294)	(0.297)	(0.269)	(0.930)
N=22186, R^2 =0.36					
Panel C: Women					
fetal exposure [-1,0]	-1.307	-0.163	-0.777	-0.396	0.557
	(7.301)	(0.599)	(0.550)	(0.555)	(1.360)
early childhood [0-3]	-11.572	-1.229	-0.517	-0.658	-1.564
	(4.179)***	(0.340)***	(0.303)*	(0.302)**	(0.498)***
pre-school [4-6]	0.924	0.706	0.290	0.221	-0.188
	(4.810)	(0.366)*	(0.331)	(0.409)	(0.343)
N=18082, R^2 =0.33					
Panel D: Urban					
fetal exposure [-1,0]	-6.203	-0.230	-0.647	-0.696	-0.069
	(5.051)	(0.502)	(0.332)*	(0.543)	(1.245)
early childhood [0-3]	-12.529	-1.358	-1.312	-1.373	-2.599
	(4.286)***	(0.264)***	(0.189)***	(0.214)***	(0.750)***
pre-school [4-6]	-4.876	0.464	0.519	0.782	0.801
	(5.418)	(0.289)	(0.386)	(0.385992)**	(0.987)
N=21898, R^2 =0.33					
Panel E: Rural					
fetal exposure [-1,0]	2.453	0.194	0.362	0.327	0.146
	(2.760)	(0.242)	(0.147)**	(0.161)**	(0.364)
early childhood [0-3]	-4.587	-0.615	-0.614	-0.391	-1.013
	(3.914)	(0.462)	(0.237)***	(0.230)*	(0.512)**
pre-school [4-6]	1.392	0.225	-0.181	-0.089	0.232
	(3.706)	(0.638)	(0.235)	(0.199)	(0.458)
N=18370, R^2 =0.33					

Notes: Standard errors clustered by district of birth in parenthesis. In addition to district fixed effects, birth year fixed effects and district-specific linear trends, OLS regressions include gender, schooling, sex, rural/urban indicator, and ethnicity. Estimation is based on individuals aged 14 to 34 in the 2006-2007 ENAHO data. The intensity of violence is defined by deviations or transitory components of armed violence from the mean for one's district. Mean district violence is calculated over the violence period and excludes the violence experienced in the individual's birth period.

*** Statistically significant at the 1 percent level; ** Statistically significant at the 5 percent level;

* Statistically significant at the 10 percent level.

Table 7: Types of violence on long-run earnings, IV-FE Estimates

	sexual violations	disappearances	kidnapping	killings	torture
Panel A: All					
fetal exposure [-1,0]	-12.832	0.084	-12.089	-0.894	-0.926
	(10.143)	(0.799)	(17.366)	(0.733)	(1.483)
early childhood [0-3]	-23.064	-1.919	-0.687	-0.189	-7.004
	(14.257)	(0.568)***	(2.202)	(1.319)	(2.062)***
pre-school [4-6]	-4.039	-0.693	-19.178	-0.529	-4.219
	(10.426)	(0.649)	(6.841)***	(1.471)	(1.085)***
N=40246, R^2=0.33					
Panel B: Men					
fetal exposure [-1,0]	-23.953	-0.432	9.894	0.511	-0.735
	(11.437)**	(0.613)	-13.015	(1.010)	(1.390)
early childhood [0-3]	-22.449	-1.502	6.649	-0.367	-8.334
	(20.853)	(0.761)**	(3.608)*	(2.630)	(3.057)***
pre-school [4-6]	15.566	-0.516	-19.227	0.115	-4.691
	(13.826)	(0.896)	(12.265)	(2.813)	(2.135)**
N=22171, R^2=0.36					
Panel C: Women					
fetal exposure [-1,0]	-6.395	1.009	13.311	-0.932	-0.400
	(18.342)	(1.923)	(16.853)	(1.604)	(2.285)
early childhood [0-3]	-41.681	-2.482	-3.941	1.126	-5.545
	(18.043)**	(0.603)***	(2.460)	(1.086)	(2.149)***
pre-school [4-6]	-11.108	-0.618	-17.394	-1.194	-3.428
	(19.971)	(1.450)	(19.572)	(1.303)	(1.294)***
N=18075, R^2=0.33					
Panel D: Urban					
fetal exposure [-1,0]	-20.820	0.293	-8.425	-3.122	-0.777
	(21.633)	(1.566)	(64.295)	(1.860)*	(2.747)
early childhood [0-3]	-47.835	-2.406	10.710	-3.349	-8.794
	(28.062)*	(0.714)***	(6.634)	-5.540	(1.874)***
pre-school [4-6]	39.309	0.687	-4.609	0.860	-3.024
	(26.245)	(1.056)	(28.822)	(3.395)	(2.153)
N=21894, R^2=0.29					
Panel E: Rural					
fetal exposure [-1,0]	-1.283	0.257	-12.956	1.074	-0.254
	(6.366)	(0.333)	(9.556)	(0.539)**	(0.852)
early childhood [0-3]	-3.598	-0.930	-1.744	-0.686	-2.914
	(12.524)	(0.489)*	(2.043)	(0.737)	(1.930)
pre-school [4-6]	-9.774	-0.854	-13.223	1.099	-1.655
	(10.288)	(0.687)	(10.850)	(1.973)	(1.273)
N=18352, R^2=0.36					

Notes: Standard errors clustered by district of birth in parenthesis. In addition to district fixed effects, birth year fixed effects and district-specific linear trends, regressions include gender, schooling, sex, rural/urban indicator, and ethnicity.
Estimation is based on individuals aged 14 to 34 in the 2006-2007 ENAHO data. The intensity of violence is defined by deviations or transitory components of armed violence from the mean for one's district. Mean district violence is calculated over the violence period and excludes the violence experienced in the individual's birth period.
Violence in individual's birth district is istrumented with violence shocks measured at the closest 5 districts to respondent's birth place.

*** Statistically significant at the 1 percent level; **Statistically significant at the 5 percent level;
* Statistically significant at the 10 percent level.

Table 8: Types of violence on height for children aged 6-7 in 1993

Panel A: OLS-FE

	All	Men	Women	Urban	Rural
fetal exposure [-1,0]	0.048	0.040	0.062	0.042	0.038
	(0.014)***	(0.015)***	(0.016)***	(0.014)***	(0.019)**
early childhood [0-3]	-0.032	-0.026	-0.035	-0.042	-0.023
	(0.005)***	(0.006)***	(0.006)***	(0.007)***	(0.005)***
pre-school [4-6]	0.005	0.001	0.010	-0.004	0.010
	(0.007)	(0.008)	(0.006)	(0.012)	(0.006)
N	472517	241471	231046	277091	195426
R^2	0.31	0.40	0.25	0.27	0.17

Panel B: IV-FE

	All	Men	Women	Urban	Rural
fetal exposure [-1,0]	0.060	0.047	0.096	0.030	0.038
	(0.087)	(0.087)	(0.103)	(0.166)	(0.084)
early childhood [0-3]	-0.186	-0.124	-0.236	-0.200	-0.150
	(0.023)***	(0.022)***	(0.029)***	(0.036)***	(0.023)***
pre-school [4-6]	-0.022	-0.049	0.007	-0.100	0.021
	(0.026)	(0.026)*	(0.028)	(0.044)**	(0.020)
N	471813	241123	230690	277047	194766
R^2	0.31	0.40	0.25	0.27	0.17

Notes: Standard errors clustered by district of birth in parenthesis. Regressions include district fixed effects, birth year fixed effetcs, and district-specific linear trend. Estimation is based on individuals aged 6-7 in 1993. The height data comes from the 1993 Peruvian School Census on Height conducted by the Ministry of Education. The intensity of violence is defined by deviations or transitory components of armed violence from the mean for one's district. Mean district violence is calculated over the violence period and excludes the violence experienced in the individual's birth period.
The outcome variable is height-for-age z-scores and is defined as the difference between the child's height and the mean height of the same-aged reference group (U.S population), divided by the standard deviation of the reference group.
The mean and standard deviation in the reference group (in centimeters) are 118.9 and 0.7 for boys 6-year old; 125.9 and 0.6 for boys 7-year old; 117.9 and 0.6 for girls 6-year old; and 124.3 and 0.7 for girls 7-year old.
Violence in individual's birth district is istrumented with violence shocks measured at the closest 5 districts to respondent's birth place.

*** Statistically significant at the 1 percent level;

** Statistically significant at the 5 level;

* Statistically significant at the 10 percent level.

Table 9: Early childhood exposure to types of violence on height for children aged 6-7 in 1993

	sexual violations	disappearances	abductions	killings	torture
Panel A: OLS-FE					
ALL	-0.604	-0.101	-0.087	-0.21	-0.132
N=472517	(0.229)***	(0.030)***	(0.029)***	(0.045)***	(0.038)***
R^2=0.31					
Men	-0.338	-0.092	-0.054	-0.263	-0.084
N=241471	(0.276)	(0.029)***	(0.045)	(0.049)***	(0.044)*
R^2=0.40					
Women	-0.822	-0.100	-0.100	-0.139	-0.162
N=231046	(0.215)***	(0.034)***	(0.026)***	(0.045)***	(0.038)***
R^2=0.25					
Urban	-1.003	-0.167	-0.096	-0.300	-0.151
N=277091	(0.350)***	(0.036)***	(0.025)***	(0.067)***	(0.048)***
R^2=0.27					
Rural	-0.319	-0.052	-0.059	-0.154	-0.120
N=195426	(0.141)**	(0.023)**	(0.060)	(0.046)***	(0.048)**
R^2=0.17					
Panel B: IV-FE					
	sexual violations	disappearances	abductions	killings	torture
ALL	-6.637	-0.3	-2.788	-0.015	-0.366
N=471813	(1.763)***	(0.048)***	(0.746)***	(0.253)	(0.235)
R^2=0.31					
Men	-5.32	-0.205	-2.867	0.017	-0.314
N=241123	(1.791)***	(0.045)***	(0.628)***	(0.274)	(0.228)
R^2=0.40					
Women	-7.944	-0.377	-2.495	-0.015	-0.348
N=230690	(2.316)***	(0.062)***	(0.992)**	(0.264)	(0.307)
R2=0.25					
Urban	-4.233	-0.313	-2.168	-0.194	-0.306
N=277047	(2.551)*	(0.075)***	(1.639)	(0.289)	(0.263)
R^2=0.27					
Rural	-6.589	-0.250	-2.052	0.146	-0.588
N=194766	(1.696)***	(0.048)***	(0.763)***	(0.35)	(0.198)***
R^2=0.17					

Notes: Standard errors clustered by district of birth in parenthesis. Regressions include district fixed effects, birth year fixed effetcs, and district-specific linear trend. Estimation is based on individuals aged 6-7 in 1993. The height data comes from the 1993 Peruvian School Census on Height conducted by the Ministry of Education. The intensity of violence is defined by deviations or transitory components of armed violence from the mean for one's district. Mean district violence is calculated over the violence period and excludes the violence experienced in the individual's birth period.
The outcome variable is height-for-age z-scores and is defined as the difference between the child's height and the mean height of the same-aged reference group (U.S population), divided by the standard deviation of the reference group.
The mean and standard deviation in the reference group (in centimeters) are 118.9 and 0.7 for boys 6-year old; 125.9 and 0.6 for boys 7-year old; 117.9 and 0.6 for girls 6-year old; and 124.3 and 0.7 for girls 7-year old. Violence in individual's birth district is istrumented with violence shocks measured at the closest 5 districts to respondent's birth place.
*** Statistically significant at the 1 percent level; ** Statistically significant at the 5 percent level;
* Statistically significant at the 10 percent level.

Table 10: Early childhood exposure to civil war violence, IV-FE estimates

	All	sexual violations	disappearances	abductions	killings	torture
Panel A: Schooling Channel						
Men	-0.001	-0.085	0.001	-0.061	-0.057	-0.014
N=17268	(0.002)	(0.241)	(0.009)	(0.170)	(0.025)**	(0.032)
R^2=0.44						
Women	-0.001	0.410	-0.001	0.701	-0.017	0.007
N=14029	(0.003)	(0.375)	(0.011)	(0.087)***	(0.010)*	(0.042)
R^2=0.59						
Urban	-0.001	0.228	-0.002	0.169	-0.036	0.007
N=18465	(0.002)	(0.261)	(0.009)	(0.293)	(0.018)*	(0.028)
R^2=0.32						
Rural	-0.001	0.043	0.001	0.274	-0.021	-0.008
N=12832	(0.002)	(0.308)	(0.010)	(0.073)***	(0.008)**	(0.034)
R^2=0.45						
Panel B: Wealth Channel						
	All	sexual violations	disappearances	abductions	killings	torture
Men	-0.001	-0.071	-0.005	0.045	0.006	-0.013
N=21738	(0.001)	(0.083)	(0.002)**	(0.011)***	(0.008)	(0.012)
R^2=0.65						
Women	-0.003	-0.280	-0.009	-0.014	0.006	-0.040
N=17781	(0.000)***	(0.086)***	(0.002)***	(0.024)	(0.005)	(0.014)***
R2=0.64						
Urban	-0.002	-0.251	-0.008	0.107	0.016	-0.025
N=21239	(0.0007)***	(0.101)**	(0.001)***	(0.062)*	(0.020)	(0.014)*
R^2=0.52						
Rural	-0.001	-0.038	-0.003	-0.015	0.000	-0.019
N=18280	(0.0003)*	(0.044)	(0.001)**	(0.006)**	(0.002)	(0.008)**
R^2=0.39						

Notes: Standard errors clustered by district of birth in parenthesis. In addition to district fixed effects, birth year fixed effects, and district-specific linear trends, regressions include gender, schooling, sex, rural/urban indicator, and ethnicity. The estimates for schooling are based on individuals aged 17 to 34 in the 2006-2007 ENAHO data, while the estimates for wealth are based on individuals 14 to 34 in the 2006-2007 ENAHO data. Wealth index is the first principal component of several household assest including access to safe water, flush toilet, characteristics of the housing dwelling, computer, telephone, cellphone, internet, TV, and per capita household income. The intensity of violence is defined by deviations or transitory components of armed violence from the mean for one's birth district. Mean district violence is calculated over the violence period and excludes the violence experienced in the individual's birth period. Violence in individual's birth district is istrumented with violence shocks measured at the closest 5 districts to respondent's birth place. *** Statistically significant at the 1 percent level; **Statistically significant at the 5 percent level; *Statistically significant at the 1 percent level.