Long-Run Labor-Market Consequences of Civil War:

Evidence from Shining Path in Peru

Jose Galdo[*]
Carleton University and IZA

Abstract

This study investigates whether early life exposure to civil wars affects labor-market outcomes later in life. In particular, we investigate the impacts of armed conflict in Peru, a country that between 1980 and 1995 experienced the actions of a tenacious, brutally effective war machine with no precedent in its modern history: the Shining Path. Over a 15-year period, the civil war evolved from the southern Sierra to the rest of the country which allow us to exploit regional variation in the timing of violence shocks to identify its effects. The main results show that a one standard deviation increase in early childhood exposure to armed violence leads to a 5 percent fall in monthly earnings. Individuals living in urban areas and Spanish speakers experienced a larger negative effect compared to rural people and indigenous-descendant Quechua speakers. We find, on the other hand, no significant impacts of fetus exposure to armed violence on long-run labor-market outcomes.

[*]Address: 1011 Dunton Tower, Carleton University, Ottawa, ON K1S5B6. E-mail: jose_galdo@carleton.ca.

1. Introduction

There are no more painful events in the history of modern nations than episodes of civil war that confront regions against regions, younger against older generations, civilians against governments, and civilians against civilians. After all, civil wars are associated to murder, forced disappearances, kidnapping, sexual assaults, and many other sources of human suffering that have implications beyond the individual (Coghlan et al. 2007, Justino 2007).

Episodes of civil war are, unfortunately, extremely common as they have afflicted a third of all nations and two thirds of African countries in the last 50 years (Harbom and Wallensteen 2007). The burden of economic costs is overwhelmingly high for countries torn apart for armed conflicts because of physical and human capital destruction (World Bank 2003). The direct short-run effects of internal conflicts are very palpable: destruction of vital infrastructure, collapse of institutions, a large number of displaced individuals, orphanage children, and a massive loss of life (Collier and Hoeffler 2003). Yet the long-run consequences of civil wars on human capital development and individual's well-being 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 outcomes later in life. Between 1980 and 1995 this Andean country experienced the actions of a tenacious, brutally effective war machine with no precedent in its modern history: the Shining Path. The total number of serious violence acts i.e., killings, forced disappearances, sexual violations, torture, forced recruitment, and kidnapping, climbed to more than 35,000, including more than 24,000 killings and forced disappearances (TRC 2003). The social and economic losses from this spiral of violence were enormous. Eight years after the start of the civil war, a special committee appointed by the Senate estimated the economic losses 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). Preliminary estimations suggest that the total economic losses reached US$25 billion dollars by the end of 1995 (Puican 2003).

The civil war started in the southern Sierra and spread to the rest of the country over a 15-year period. By exploiting regional variation in the timing of violence acts, we are able to identify the effects of exposure to violence on labor outcomes. In particular, we use micro-level analysis and data to examine the effects of violent shocks around the time of birth on earnings and employment for individuals aged 14 to 27 years in 2007.

This paper is related to a recent and growing body of research in economics that has uncovered some evidence on the long-run legacies of civil wars[1]. This small literature has mainly focused on two outcomes: anthropometric measures (Alderman, Hoddinott and Kinsey 2004, Akresh, Bundervoet and Verwimp 2009) and schooling attainment (Shemyakina 2006, Leon 2009); outcomes that affect individuals' labor-market productivity. 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) that reports less schooling and work experience for former child soldiers in Uganda, and therefore, less success in their labor markets outcomes as adults[2]. A somewhat different picture is obtained by Humphreys and Weinstein (2007), who find that increases in Sierra Leone combatants' violence exposure is weakly correlated with employability.

---

[1] This paper has also benefited from a recent body or 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 much of this work has focused primarily 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; Maluccio et al. 2007, Almond et al. 2007)

[2] 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, Imbens and van der Klaauw 1995)

Several features distinguish this paper from the existing research on the legacy of civil wars. First, this study does not restrict the labor-market analysis to former combatants but rather uses a large national representative sample of individuals that were exposed to civil war at the very beginning of their life. Second, we examine violence shocks several periods before and after birth to uncover evidence about the most sensitive or critical period –fetal or early childhood exposure. Third, 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 number of violence acts that does not separate transitory shocks from secular trends in violence conditions within districts. Likewise, the use of a much finer level of variation by considering more than 1500 districts (the smallest administrative units after the villages) is an improvement over provincial and department level data.

Fourth, the treatment indicator is not a binary variable but rather a continuous one, which reflects the intensity of exposure to civil conflict as there is substantial time variation in the levels of violence across districts. Thus our results are not driven solely by the contrast between war-affected and non war-affected localities. Fifth, to deal with potential measurement error in the violence data, this study uses instrumental variables regressions where variables for violence measured at the closest 5 districts serve as instruments for violence in the individuals' district of birth. Sixth, this study uses a large number of cohorts in a representative sample of the national population that improves over limited time horizons of most civil war datasets. Finally, this study focuses on the civil war legacy in an Andean country, expanding the literature that so far has mainly focused on African countries and, therefore, may raise concerns about generalizability.

We use two different sources of information to fulfill the data requirements of this investigation. To capture the socio-demographic and labor-market information of individuals we use 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 violence events during the 1980s and 1990s as part of its mandate to investigate and document the civil war period. We link then each individual in the household surveys to district-specific violence data for their birth year.

The econometric estimates show statistical significant effects of early life exposure to civil war on adult earnings after controlling for district fixed effects, birth year fixed effects, and a district-specific linear trend. A one standard deviation increase in childhood exposure to violence shocks leads to a 5 percent fall in monthly earnings. This negative result does not affect everyone with the same intensity. In the long-run, the Peruvian civil war has disproportionally affected the adult earnings of non-indigenous people and individuals living in urban localities. We find, on the other hand, no impacts of fetus exposure to armed violence on labor-market outcomes later in life. The results indicate 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. Understanding the economic legacies of conflict is also important to the design of the appropriate post-conflict policy responses including the targeting of subgroups that were disproportionally affected by the armed conflict.

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 importance of subgroup impacts. Section 6 discusses potential pathways. Finally, Section 7 concludes.

2. <u>Peru's Civil War: 1980-1995</u>

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 self-proclaimed as an agent of a world history destined to conclude in Communist Revolution[3]. The Shining Path initiated its actions as a focalized regional political movement in the Southern countryside of the country by symbolically burning electoral ballots from the 1980 Presidential election in one of the poorest localities of the country. Unlike many African political conflicts, the civil war in Peru does not follow the contest model's prediction that armed conflicts flourish in resource rich regions because of the existence of more rents to fight over (e.g., Le Billon 2001, 2005). On the contrary, its roots are related to political grievances, institutional quality, and poverty (Stern 1998, Strong 1991).

The initial response from the government was tardy and ineffective (Palmer 1992). The Peruvian Army started its operations against the Shining Path after almost two years of the initial terrorist 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 (TRC 2003). This strategy did not stop but rather fueled an

---

[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 though.

expansion of the civil war to the rest of the country[4]. Figures 1A to 1C show the progression of the civil war: it started 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 passed from 37 to 72, including the capital, 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 1987, the cycle of political violence worsened even more when a new group –The Revolutionary Movement Tupac Amaru- started a cycle of violence against the government. Its actions were much less lethal than that from Shining Path, accounting by for 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, by 1995, almost 75 percent of the Peruvian provinces experienced the burden of a civil war with no precedent in its modern history.

The end of civil war was marked before 1995 though. In September of 1992, Shining Path's founder and messianic leader Abimael Guzman, was captured by a police intelligence operation. The Shining Path's effective political and war machine fell as a castle of cards. The man who inspired the strength, discipline, and commitment to its dogmatic militants was at the same time its Achilles Heel (see Gorriti 1999 and Palmer 1992 for exhaustive studies about the Shining Path). The end of the civil war in Peru illustrates new empirical evidence suggesting that insurgent leaders do matter (e.g., Jones and Olken 2007, Guidolin and La Ferrara 2007).

The Peruvian armed conflict was marked by a deliberately indiscriminate violence against civilians (TRC 2003). Between 1980 and 1995, the civil war period, the number of

---

[4] Amnesty International noted that the Peruvian government had the worst human rights record in the World in terms of forced disappearances in the late 1980s (Palmer 1992). Kalyvas (2006) documents 100 studies and 45 historical cases where state violence against civilians provoked greater insurgence violence as a response.

serious violence acts i.e., killings, forced disappearances, sexual violations, torture, forced recruitment, and kidnapping climbed to more than 35,000, including more than 24,000 killings and forced disappearances (TRC 2003). This violence evolved over a course of 15 years creating substantial regional variation in the timing of the violence shocks. Figure 2 show the time series of these violence events. While in 1980-1982 period the number of violent acts ranged between 100 and 800, it climbed vertiginously to almost 6,000 in 1984. After a period of stabilization between 1985 and 1988 (with 2,000 violence acts in each year), the intensity of civil war increased in the late 1980s when the number of violence acts rose to 4,000 in 1989 and 1990. Starting in 1993, the number of violence acts decreased vertiginously until fading up in 1997.

Likewise, Figure 2 shows substantial regional variation in the intensity of the violence shocks. Whereas approximately 25 percent of provinces never experienced a single act of political 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 more than 500 random acts of violence during the civil war period. To put in perspective these numbers, 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. If one believes, for instance, in the "fetal origins hypothesis" (Barker 1998), the cohort born in 1985 would be more adversely affected than the cohort born in 1984. Similarly, individuals born in areas that experienced more than 100 violence acts may have been more adversely affected those born in the areas that experienced less than 5 acts. Thus, the research question we address in the next sections is whether these early-life

differences in the intensity of exposure to civil war violence are causally related to individuals' labor-market outcomes later in life.

3. Data Sources

We use the 2006 and 2007 waves of the Peruvian household survey, Encuesta Nacional de Hogares (ENAHO), which is 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.

In each year, approximately 22,000 households are interviewed across all 24 states and in both urban and rural regions.[5] The ENAHO includes information on district of birth and date of birth to which we link the civil war data. Individuals aged 14 and older are subject to the employment and earnings module, which provides detailed information (93 questions) about the current labor-market status and earnings of economically active individuals. We therefore limit the analysis to working-age individuals who were born during the civil war period, or individuals aged 14 to 27 in the year of the survey. The ENAHO sample consists of 47,447 individuals born between 1980 and 1994, from which 29,408 individuals were either employed or seeking employment.

The 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[6]. Its work focused on violence acts as long as they were imputable to terrorist organizations, state agents, or

---

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

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

paramilitary groups (Supreme Decrees N° 065-2001 and 101-2001). Its work was formally concluded on 2003, when presented its final report to the President, Chairman of the Congress, Chairman of the Supreme Court, and the Ombudsman. The TRC final report constitutes a "historical memory" that documents extensive, detailed information from victims, survivors, and other witnesses, through thousands of detailed statements[7].

The violence data collected by the TRC comes from the reconstruction of violence acts that took place in Peru between 1980 and 2000. For each documented violence act, there is information about the location, time, victim, perpetrator, and type of act. Overall, 35,019 violence events were reported between 1980 and 1995, from which 32 percent corresponds to killings, 22 to forced detentions, 18 percent to torture, and 15 percent to forced disappearances[8].

We link each individual in the ENAHO data to the violence data for their district-specific birth year. We use the data for working-age individuals 14 to 27 years old (corresponding to years of birth, 1980-1994). For a given district in a given year, we define two explanatory variables: 'early childhood exposure to violence' that is defined as the sum of violent acts in the first 12 quarters of one's life and 'fetal exposure to violence' defined as the sum of violent acts in the 4 quarters previous to the birth date. We compute these variables in the TRC dataset and merge this information to each birth-district/birth-date combination represented in the ENAHO sample.

Table 1 reports summary statistics for the final sample set composed by 44,983 individuals aged 14 to 27 after discarding observations with incomplete information. The average individual in our sample is a 19.3 years old with 9.5 years of schooling. Only 18 percent of the sample is married whereas the proportion of men and women is evenly distributed in the

---

[7] The TRC final report is available at http://www.cverdad.org.pe/ingles/ifinal/index.php
[8] Violent acts were classified as killing, forced disappearances, forced detentions, forced recruitment, kidnapping, sexual violations, torture, and other grave injuries.

sample. Almost 4 out of 10 individuals live in rural areas and 16 percent do not speak Spanish as their first language. About 60 percent of individuals aged 14 to 27 years old were employed in 2006 and 2007, from which 59 percent had remunerated jobs, while 41 percent worked as non-remunerated family workers. The average earnings in this particular subsample of young adult workers were 201 soles (or US$70) per month after inputting zero earnings for non-remunerated family workers.

The average individual in our sample was exposed to 12 violence shocks in her early childhood and 3 shocks while she was in utero. One can also observe unconditional mean earnings across violence shocks measures in the lower panel of Table 1. There is a clear monotonic distribution for adult earnings as those who were not exposed to violence shocks in early life have 1.56, 1.45, 1.20, and 1.02 times higher earnings than those who experienced +100, 50-100, 10-49, and 1-9 violence shocks, respectively. Similar patterns are observed when looking at fetal violence measures.

4. Empirical Strategy and Results

4.1 Identification

The identification strategy builds on this observation: it is a difference–in-differences approach where we test whether children born in districts affected by shocks of armed violence have adverse labor-market outcomes later in life than their counterparts born before or after in the same districts, relative to those who are born in other regions in the same year.

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

$$y_{ijt} = \eta_j + \tau_t + X_{ijt}{}'\beta_1 + \beta_2 V_{ijt} + \beta_3 TREND_{jt} + \varepsilon_{ijt} , \qquad (1)$$

where η_j represents the fixed effects for district j which controls for 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 specific cohort-effects . V_{ijt} represents deviations or transitory shocks of armed violence from the mean for one's birth district. Mean district violence for a particular individual is calculated over the 1980-1995 period, and excludes the violence experience in the individual's birth period. This approach has the advantage to separate transitory shocks from secular trends in violence conditions within districts. At the individual level, transitory movements in armed violence involve unexpected shocks. On the contrary, measuring exposure to armed violence simply by the level or number of violence acts may be dominated by district-specific secular developments over time.

The parameter of interest is β_2, which represents the impact of early childhood (or fetal) exposure to shocks of violence on adult labor-market outcomes y. Identification of the impact comes from comparing individuals exposed to different shocks (deviations) of violence while isolating persistent effects of violence in the birth district and birth cohort thorough district- and age-fixed effects. Yet it may not identify the causal impacts had the timing of the shocks followed a particular pattern in terms of district-level characteristics that are 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 diverges 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 residence (i.e., urban and rural), and ethnicity (i.e., mother's tongue is Spanish or aboriginal ones including Quechua and Aymara), which are considered prime variables in predicting labor-market outcomes (Mincer 1993). 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. For example, the persistence of local labor-market conditions could induce time-series correlation at the district level. 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).

4.2 Labor-Market Impacts

Table 2 reports the main results for both earnings and employment outcomes. Panel A shows the results for early childhood exposure to civil war while Panel B focus on the estimates for fetus exposure. Clustered standard errors are shown in brackets. For interpretation of the results, this study focuses on the impact of one standard deviation in the (transitory) violence measure (V), computed in the 1980-1995 period (for those aged 14 to 27 in 2007).

The regression results show that early life exposure to civil wars affects labor-market earnings later in life. A one standard deviation increase in childhood exposure to violence acts i.e., 25, leads to a reduction of 6.5 soles in monthly earnings, which is equivalent to 3.2 percent fall in monthly earnings (relative to the mean earnings of individuals aged 14 to 27). This result is statistically significant at the 5 percent level. On the other hand, and not surprisingly, we find no long-run employment effects for civil war exposure early in life. In contexts of intense

poverty and weak (or absent) social security systems, people have to work (or invent their own job) to meet subsistence needs[9]. In Peru, 69 percent of the poor, for example, act as entrepreneurs (Banerjee and Duflo 2007).

The second panel of Table 2 considers the effects of fetus exposure to armed violence later in life. For all outcomes of interest we find statistically not significant impacts on both earnings and employment outcomes. The point estimates for labor-market earnings, for example, is almost close to zero after controlling for district specific linear trends. This result suggests that civil war shocks *in utero* are less important predictors of labor-market success in comparison to shocks occurring during the weaning and post-weaning life periods.

It is important to recognize, however, the potential role for serial correlation of shocks in utero and shocks in the months following birth. It is possible that the significant effects we find for early childhood exposure to violence shocks just reflect correlation of fetal exposure to violence. To address this point, we re-estimate model (1) to include both fetal and early childhood violence shocks measures in the same regression. If the coefficients in Panel A change significantly, then there is evidence of serial correlation over time, which undermines the evidence that early childhood exposure to armed violence matters *per se*.

Panel C in Table 2 shows the new results. By looking at the estimates, one confirms that exposure to childhood violence affect significantly long-run labor-market earnings. The point estimates and corresponding standard errors are pretty similar to those observed in Panel A, suggesting that early childhood exposure to violence shocks matters *per se.* At the same time, the differentiate impact between fetal and early childhood exposure to civil war on long-run labor-

---

[9] Peru does not have, for instance, an unemployment insurance system.

market earnings is another element suggesting 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 correlated only with early childhood exposure to civil war, but not with fetal exposure. These results are consistent with the findings in Maccini and Yang (2009) that documents that childhood rather than fetal exposure to environmental conditions early in life has the largest impacts on adult socioeconomic and health outcomes.

4.3 An IV Approach

A potential caveat with the OLS point estimates is measurement error in the violence data, something that is rarely addressed (Blattman and Miguel 2009). While the work of the Peruvian Truth and Reconciliation Commission (*TRC*) constitutes the most comprehensive effort in the reconstruction of the civil war in Peru up to date, it may not be free of measurement error of the actual number of violence acts at the district-level. 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 OLS estimates in Table 2 are biased and attenuated toward zero (Wooldridge 2005). To deal with classical measurement error, we instrument violence shocks with alternative measures of the same variable, measures whose errors are likely to be orthogonal to the original, instrumented variable.

We therefore modify equation (1) to incorporate an instrumental variables regression where the main regressor (transitory shocks of armed violence from the mean for one's birth district) is instrumented with five analogous violence variables measured at the same time span but at the closest five districts. A similar approach is used, for instance, in Maccini and Yang

(2009) when addressing measurement errors in rainfall variables. The first stage regressions predicting both fetal and childhood exposure to birth district violence are 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 join significance of the instruments passes with amplitude conventional threshold levels used for detecting weak instruments in linear IV regressions (Staiger and Stock 1997, Stock and Yogo 2005)[10].

Table 3 shows the instrumental variables results. Panel A shows the results for early childhood exposure to civil war, Panel B focus on the estimates for fetus exposure and Panel C reports the estimates after incorporating both violence measures in the same regression. Standard errors appear in parenthesis and clustered standard errors in brackets. Panel A shows that early childhood exposure to civil war affects significantly labor-market earnings later in life. The IV estimates are modestly larger that the corresponding OLS point estimates: a one standard increase in the exposure to civil war lead to a fall in 10 soles in monthly earnings, equivalent to 5 percent drop in monthly earnings (relative to the mean earnings of individuals aged 14 to 27). This result is statistically significant at the 5 percent level. In terms of employment, the IV regression results are also consistent with the OLS estimates showing statistically insignificant effects.

---

[10] Weak instruments in linear IV regression can result in biased estimates and confidence intervals with actual coverage rates far from their nominal values. Staiger and Stock's (1997) suggested rule of thumb is that, in the case of one endogenous regressor, instruments be deemed weak if the first stage F-test is less than ten. Stock and Yogo's (2005) test proposed a refinement to Staiger and Stock's (1997) rule-of-thumb by providing specific statistic values to compare vis-à-vis the first stage F-test.

Panel B in Table 3 shows the IV regression estimates for fetal exposure to civil war. The results are, again, pretty consistent with the OLS estimates: there are not statistically significant effects for fetal exposure to armed violence. Both earnings and employment outcomes show negligible impacts, reinforcing the previous OLS results that highlight significant differences between childhood and fetal exposure to civil war on long-run labor-market outcomes. Finally Panel C in Table 3 show the IV estimates when both violence measures are included in the same specification. The results are, once again, consistent with the previous OLS estimates, suggesting that early childhood exposure to armed violence matters *per se*. The coefficient for earnings does not change a bit after including measures of fetal exposure to civil war.

4.4 A Specification Test

This study focuses on the effects of violence shocks experienced early in life following the extensive critical-period programming theory that stresses the long-lasting physiological and structural effects of environmental conditions around birth (see the reviews in Godfrey and Barker 2000 and Elo and Preston 1992). A potential econometric concern is that early childhood (or fetal) exposure to violence shocks could be spuriously capturing the effect of unobservables that are correlated with idiosyncratic district-level characteristics that in turn affect individuals' labor-market earnings.

Since adult workers in Peru, defined as those aged older than 16 in 1980, were largely unaffected by the civil war as children but experienced its burden as adults, this comparison provides a specification test of the validity of our empirical strategy. It is very likely that the labor-market prospects for these adult workers were affected during the time conflict or even in

the immediate aftermath of the war as capital destruction and lack of private investment were the norm. Yet it is much less clear how these effects would persist after 15 years the civil war ended. After all, these adult workers already completed their cognitive development, health risks factors, and formal years of education before the civil war starts. Thus any significant labor-market effects 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 labor-market estimates to civil war exposure for individuals aged between 43 and 65 in 2007 i.e., individuals born between 1942 and 1964. Both ordinary least squares and instrumental variables regressions are estimated following the specification in equation (1). We define a new violence exposure measure defined as the number of violence acts in one's birth district during the 1980-1995 civil war period. Conventional standard errors are shown in parenthesis while robust standard errors in brackets. Two main results emerge. First, the point estimates for employment and labor-market earnings are very small in magnitude and statistically significant equal to zero. Second, these not significant results hold after estimating the corresponding instrumental variables approach regressions. In particular, the point estimate for labor-market earnings is negligible and changes of sign to 0.067 (std. error=0.198).

One potential problem with these results is that the civil war exposure measure may not reflect the actual exposure to violence for adult workers as they could have migrated across localities over time. Unfortunately, we cannot observe the date of migration for each individual in the data, but we can identify whether individuals have migrated or not. With this caveat in mind, we, therefore, estimate the relationship between migration status and civil war measures for adults aged 43 to 65. The results are reported in column 3 of Table 4. One can observe that

there is no 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. Moreover, we also re-estimate equation (1) only for the subsample of adult workers who in 2007 were still living in the same localities where 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. The OLS regression results, for instance, show a point estimate equal to -0.012 with robust standard error equal to 0.420.

4.5 More Sensitivity Tests

In this section, we use several alternative strategies to test the robustness of early childhood exposure to civil war on long-run labor-market 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: inflation rates and economic growth rates, indicators that show large variance during the period of analysis. An episode of high inflation, for instance, characterized the Peruvian economy between 1988 and 1990, which was followed by an economic recession in the early 1990s. Column 3 and 4 in Table 5 report the estimates when including economic control variables while columns 1 and 2 show 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 the baseline estimates. All quantitative findings of this study hold after including 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 control adequately 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 columns 7-8, we exclude non-remunerate family workers from the analysis so that the point estimates can reflect better the productivity market value. The results, again, do not change much. The new point estimates change marginally from -0.257 to -0.277 for OLS and from -0.405 to -0.366 for IV estimates, and they remain statistically significant. The negative effects of early childhood exposure to armed violence on labor-market earnings later in life are thus independent of whether we included or not non-remunerated family workers.

Third, it is important to consider also the potential effects of districts that did not experience a single act of political violence during the period under analysis. It may bias the results as long as these districts may present idiosyncratic differences with respect to the rest of country. We therefore exclude from the analysis districts that did not experience a single act of armed violence during the 1980-1995 period. This approach allows us to exploit only differences in the timing of violence shocks within districts that experienced armed violence. Columns 7-8 report the new point estimates for early childhood exposure to civil war. Once again, all our quantitative and qualitative findings hold. The main results do not change and the coefficient estimates and statistically significance levels are similar.

Finally, we explore how sensitive are our results to migration status. One expects low levels of migration (for children and their parents) around birth. Yet it is a possibility that we

should consider in our estimates. In columns 9-10, we use as dependent variable a dummy variable indicating whether a person migrate or not from her province of birth. The coefficients are negligible and statistically not significant. There appears to be no 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. It is very likely that several families have migrated out of their localities over time.

5. Heterogeneous Effects

The burden of armed conflicts does not affect everyone in the same way or with the same intensity. There is evidence that some particular groups are more vulnerable than others. In the short-run, women, for instance, are more adversely affected than men in terms of anthropometric measures (Akresh et al. 2009), life expectancy (Plümper and Neumayer 2007), and health outcomes (Hazem et al 2008). Understanding which groups will be disproportionally affected in the long-run has important policy implications in countries torn-apart for civil conflicts.

To address the importance of subgroup impacts on labor-market earnings in Peru, we estimate variations of equation (1) through the interaction of the violence measures with three individual and locality characteristics: gender, ethnicity, and urban/rural locality. The estimated equation is described by

$$y_{ijt} = \eta_j + \tau_t + \beta_1 C_i + \beta_2 V_{ijt} + \beta_3 V_{ijt} \times C_i + X_{ijt}'\beta_4 + \beta_5 TREND_{jt} + \varepsilon_{ijt} \qquad (2)$$

where the new term C_i represent the characteristic of interest and $V_{ijt} \times C_i$ the interaction between the violence variable and the characteristic of interest. The interpretation of the coefficients is as

follows: for example, in the specification that tests for heterogeneous impacts by gender, where the variable C_i takes the value 1 for men and 0 otherwise, β_2 is the violence effect for women whereas the corresponding effect for boys is given by the sum of the coefficients $\beta_2 + \beta_3$. If β_3 is statistically significant different to zero, there is evidence of long-run heterogeneous effects of civil war by gender.

Table 6 reports the subgroup impacts to childhood exposure to civil war. Standard errors appear in parenthesis and clustered standard errors in brackets. In row 1, when the violence shock measure is interacted with gender, there is no evidence of gender effects. The interaction term parameter is not statistically significant and, thus, there is no evidence about heterogeneous earnings impacts between men and women.

The second row of Table 6 shows, on the other hand, significant long-run heterogeneous impacts on labor-market earnings between urban and rural localities. The IV estimates suggest that one standard deviation in childhood exposure to civil war decreases monthly earnings in 23 soles in urban areas (equivalent to 11 percent drop relative to the mean); whereas in rural areas the negative effects is very small (1 sol). These results hold whether we estimate separate regressions for urban and rural subsamples or whether we consider only salaried individuals in the sample.

Rows 3 and 4 of Table 6 show heterogeneous estimates by ethnicity. Two main groups are considered: whether the individual mother's tongue is 'Spanish', the majority of the population, and whether the individual mother's tongue is 'Quechua', the main minority indigenous-descendant group. The results show that the long-run earnings of 'Spanish' group individuals have been more affected than that for the indigenous-descendant 'Quechua' group.

This result is in line with the urban/rural estimates as the majority of the "Quechua" group individuals live in rural areas. More important, this result is consistent with Leon (2009) who finds using the Peruvian Census that the civil war has disproportionally affected the schooling attainment of Spanish speakers rather than that of indigenous speakers.

In summary, the reduced-form evidence uncovered in this study suggest that, in the long-run, the Peruvian civil war has disproportionally affected the labor-market earnings of non-indigenous people and individuals living in urban localities. These results do not indicate that indigenous individuals or those living in rural areas are better off in terms of labor-market earnings than non-indigenous ones or those living in rural areas. In fact, there is still a large gap in terms of labor and social measures between rural and urban areas in Peru, or between indigenous and non-indigenous groups. This result only suggest that exposure to civil war early in life have affected disproportionally the labor-market earnings of some subgroups later in life.

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. First, it is plausible that the initial effect of early childhood exposure to civil war is on the health and nutrition of infants which predispose to diseases and affect the accumulation of human capital over time. It is not trivial that the Peruvian Truth and Reconciliation Commission (TRC) placed the 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 for almost 17,000 people through private and public testimonies (TRC 2003). The TRC's final report emphasized, for

instance, a host of psychosomatic problems including chronic depression, generalized anxiousness, stress, and loss of confidence, among others.

Our current data, however, do not enable us to directly analyze health measures. For this reason, we use as auxiliary source the 2005 Peruvian Demographic and Health Survey (DHS), a nationally representative household survey, which focuses on health measures of women, aged 15 to 49 and their children aged 6 and younger. There are two caveats, however, with this micro-data analysis. First, the 2005 DHS survey collects information on health outcomes only from adult women born on or before 1990. Thus, we do not have information neither for adult men nor for women born between 1990 and 1995, the last 5 years of the armed conflict. Second, the number of women with health measures is relatively small. Out of 5,114 women aged 15 to 27 in 2005 only around 2,000 were randomly selected to report information on anthropometric health measures. As a result, the empirical analysis is implemented using department-level variation rather than the much finer district-level variation. We, therefore, should take these quantitative results with caution and only as suggestive evidence.

Based on the (almost) same methodology presented in section 3, we measure the impact of early childhood exposure to armed conflict on height for women aged 15 to 27 in 2005. Adult height is a well-established measure to reflect early-life resource availability (Maccini and Yang 2009, Schultz 2002 and 2005, Strauss and Thomas 1998, Steckel 1995). Column 1 in Table 7 shows the estimated coefficient. It suggests that one standard deviation increase in armed violence during early childhood is associated with 0.26 centimeters lower height for women. This point estimate, however, is not statistically significant at conventional levels due to the small sample size.

Improved health and nutrition status among children contributes to school enrollment (e.g., Alderman et al. 2001) and improves school performance later in life (e.g., Glewwe and King 2001). Glewwe and King, for instance, conclude that malnutrition that persists into the second year of life is critical for cognitive development. In the context of the Peruvian civil war, Leon (2009) shows, after combining census demographic data with the same violence dataset, that children exposed to armed violence accumulates between 0.12 and 0.19 less years of education as adults. Seen through the scope of standard human capital models, these results provide suggestive evidence about potential pathways by which episodes of civil war early in life might have induce lower labor-market earnings later in life[11].

One complementary channel through which early exposure to civil wars might affect adult labor-market success is via household wealth. Because we use a young cohort of individuals age 14 to 27, it is very likely that most of them are still living with their parents. Therefore, we construct a wealth index that uses as inputs not only the total household income per capita but also a set of household assets including characteristics of the household's dwelling. By aggregating 15 household assets through factor analytic methods, this index represents a proxy for long-run economic status rather than a measure either of current welfare or poverty. Evidence for some developing countries suggests that this approach is a robust measure of household wealth (Filmer and Pritchett 2001) and less variable and sensitive to transitory fluctuations (Fields 1998).

Using the same empirical approach and reduced-form specification presented in section 3, column 2 in Table 7 reports early childhood exposure to civil war on household wealth. The magnitude of the estimated coefficient is small but statistically significant: a one standard

---

[11] The Peruvian census data is not publicly available. For that reason, it was not possible to incorporate it in this study.

increase in exposure to civil war is associated with 0.03 standard deviations lower asset index later in life. In the short-run, however, this negative effect is expected to be larger, leading to lower household income and, therefore, poorer health for infants.

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, our empirical analysis 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 shocks lead to a 5 percent decrease in adult earnings. This negative effect has disproportionally affected non-indigenous individuals living in urban areas and is robust to a variety of sensitivity checks and econometric details. On the other hand, we do not find any significant effect of fetus exposure to armed violence on adult earnings.

Some patterns in our data analysis suggest that the most plausible explanation for these results is that exposure to violence shocks early in life has a negative impact on infant health due to physiological and structural effects as well as through household wealth restrictions. In turn, these human capital factors affect educational achievement and adult health. Adult health and educational achievement can then have direct effects on labor-market earnings.

These results are in accordance with other studies on the legacy of civil wars in developing countries. This small but growing literature has documented that certain human capital impacts appear to be persistent (Blattman and Miguel 2009). Evidence on poorer schooling attainment (Shemyakina 2006, Akresh and de Walque 2009, Leon 2009) and health outcomes (Akresh, Bundervoet and Verwimp 2009, Camacho 2009) seem robust across

countries and datasets; which, seen through the scope of standard human capital models, will inevitable affect the labor-market outcomes of individuals later on life.

The results 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, sheltering, and health care provision. Understanding the economic legacies of conflict is also important to the design of the appropriate post-conflict policy responses including the targeting of individuals or groups that were disproportionally affected by the armed conflict.

References

Akresh, R, and D. de Walque. (2009)."Armed Conflict and Schooling: Evidence from the 1994 Rwandan Genocide," *Review of Economics and Statistics*, Forthcoming.

Akresh, R., T.Bundervoet and P.Verwimp. (2009) "Health and Civil War in Burundi." Journal of Human Resources, 44(2): 536-563.

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

Alderman, H. and J. Behrman. (2006). "Reducing the Incidence of Low Birth Weight in Low-Income Countries Has Substantial Economic Benefits," World Bank Research Observer, 21(1):25-48.

Alderman, H., J.Hoddinott and B.Kinsey. (2006). "Long Term Consequences of Early Childhood Malnutrition," Oxford Economic Papers, 58(3): 450-474.

Almond, D. (2006). "Is the 1918 Influenza Pandemic Over? Long-term Effects of In Utero Influenza Exposure in the Post-1940 U.S. Population." Journal of Political Economy, 114: 672-712.

Angrist, J. (1990). "Lifetime Earnings and the Vietnam Era Draft Lottery: Evidence from Social Security Administrative Records." The American Economic Review, 80(3): 313-36.

Banerjee, A., E.Duflo, G. Postel-Vinay, and T.M. Watts. (2007). "Long Run Health Impacts of Income Shocks: Wine and Phylloxera in 19th Century France." National Bureau of Economic Research Working Paper 12895.

Barker, D. (1998). Mothers, Babies, and Health in Later Life. Edinburgh, U.K.: Churchill Livingstone.

Behrman, J. and M. Rosenzweig (2004). "Returns to Birthweight." Review of Economics and Statistics: 86 (2): 586-601.

Blattman, C.and J.Annan (2007). "The Consequences of Child Soldiering." HiCN Working Paper, 22.

Blattman, C. and E. Miguel. (2009). "Civil War" *Journal of Economic Literature*, Forthcoming.

Camacho, A. (2009). "Stress and Birth Weight: Evidence from Terrorist Attacks". American Economic Review: Papers & Proceedings. 98(2), 511-515.

Case, A., D. Lubotsky and C. Paxson (2002). "Economic Status and Health in Childhood: The Origins of the Gradient." The American Economic Review, 92 (5): 1308-1334.

Coghlan, B, P. Ngoy, F. Mulumba, C. Hardy, V. Nkamgang, T. Stewart, J. Lewis, and R. Brennan. (2007). "Mortality in the Democratic Republic of Congo: An Ongoing Crisis", International Rescue Committee, New York.

Collier, Paul, and A. Hoeffler. (1998) "On Economic Causes of Civil War" Oxford Economic Papers, 50(4), 563-573.

Currie, J. and R. Hyson (1999). "Is the Impact of Health Shocks Cushioned by Socioeconomic Status? The Case of Low Birthweight." American Economic Review Papers and Proceedings, 89(2): 246-250.

Currie, J. and E. Moretti (2005). "Biology as Destiny? Short and Long-Run Determinants of Intergenerational Transmission of Birthweight." NBER Working Paper 11567.

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

Elo, I. and S. Preston (1992). "Effects of Early-Life Conditions on Adult Mortality: A Review." Population and Development Review, 13 (1): 77-100.

Filmer, D, and L. Prichett. (2001). "Estimating Wealth Effects Without Expenditure of Data - Or Tears: An Application to Enrollments in States of India". Demography.

Glewwe, P., and E. King (2001). "The Impact of Early Childhood Nutrition and Academic Achievement: Does the Timing of Malnutrition Matter?" World Bank Economic Review, 15(1): 81-114.

Gorriti G. (1999). "The Shining Path: A History of the Millenarian War in Peru"

Hazem H.A, A. Ghobarah, P. Huth, and B. Russett. (2008). "The Post-War Public Health Effects on Civil Conflict"

Hearst, N., T. Newman and S. Hulley. (1986). "Delayed effects of the military draft on mortality. A randomized natural experiment." New England Journal of Medicine, 314(10): 620-24.

Imbens, G. and W. van der Klaauw. (1995). "Evaluating the Cost of Conscription in The Netherlands." Journal of Business and Economic Statistics, 13(2):207–15.

Johnson, R. and R. Schoeni (2007). "Early-Life Origins of Adult Disease: The Significance of Poor Infant Health and Childhood Poverty." Mimeo, Goldman School of Public Policy, University of California, Berkeley.

Justino, P. 2007. "On the Links between Violent Conflcit and Household Poverty: How Much do We Really Know", MICROCON, Research Working Paper 1.

Leon, G. (2009) "Civil Conflict and Human Capital Accumulation: The Long Term Effects of Political Violence in Peru" Bread Working Paper No 245.

Lindeboom, M., G.J. Van den Berg, and F. Portrait (2006). "Economic Conditions Early in Life and Individual Mortality." The American Economic Review, 96(1):290-302.

Maccini, S. and D. Yang (2009) "Under the Weather: Health, Schooling, and Economic Consequences of Early-Life Rainfall" The American Economic Review, 99(3):1006-1026

Maluccio, J.A., J. Hoddinott, J.R. Behrman, A. Quisumbing, R. Martorell and A. D. Stein (2007). "The Impact of Nutrition during Early Childhood on Education among Guatemalan Adults." Mimeo, University of Pennsylvania, IFPRI, and Emory University.

Palmer, D. (1992) Shining Path of Peru. New York: St. Martin's Press.

Plümper T, and E. Neumayer. (2007) "The Unequal Burden of War: The Effect of Armed Conflict on the Gender Gap in Life Expectancy"

Puican, N.. (2003). "Los Costos Economicos de la Violencia Politica: Una Vision desde el Campo Economico productivo". CVR, Lima.

Royer, H. (2009). "Separated at Girth: Estimating the Long-Run and Intergenerational Effects of Birthweight Using Twins." American Economic Journal: Applied Economics, 1(1): 49–85.

Shemyakina, O. (2006). "The Effect of Armed Conflict on Accumulation of Schooling: Results from Tajikistan." Households in Conflict Network Working Paper, 12.

Strauss, J. and D. Thomas (1998). "Health, Nutrition, and Economic Development." Journal of Economic Literature, 36 (2): 766-817.

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

1980

1983

☐	0
☐	1 to 10
▨	11 to 30
▨	31 to 50
▨	51 to 100
■	>101

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

1986

1989

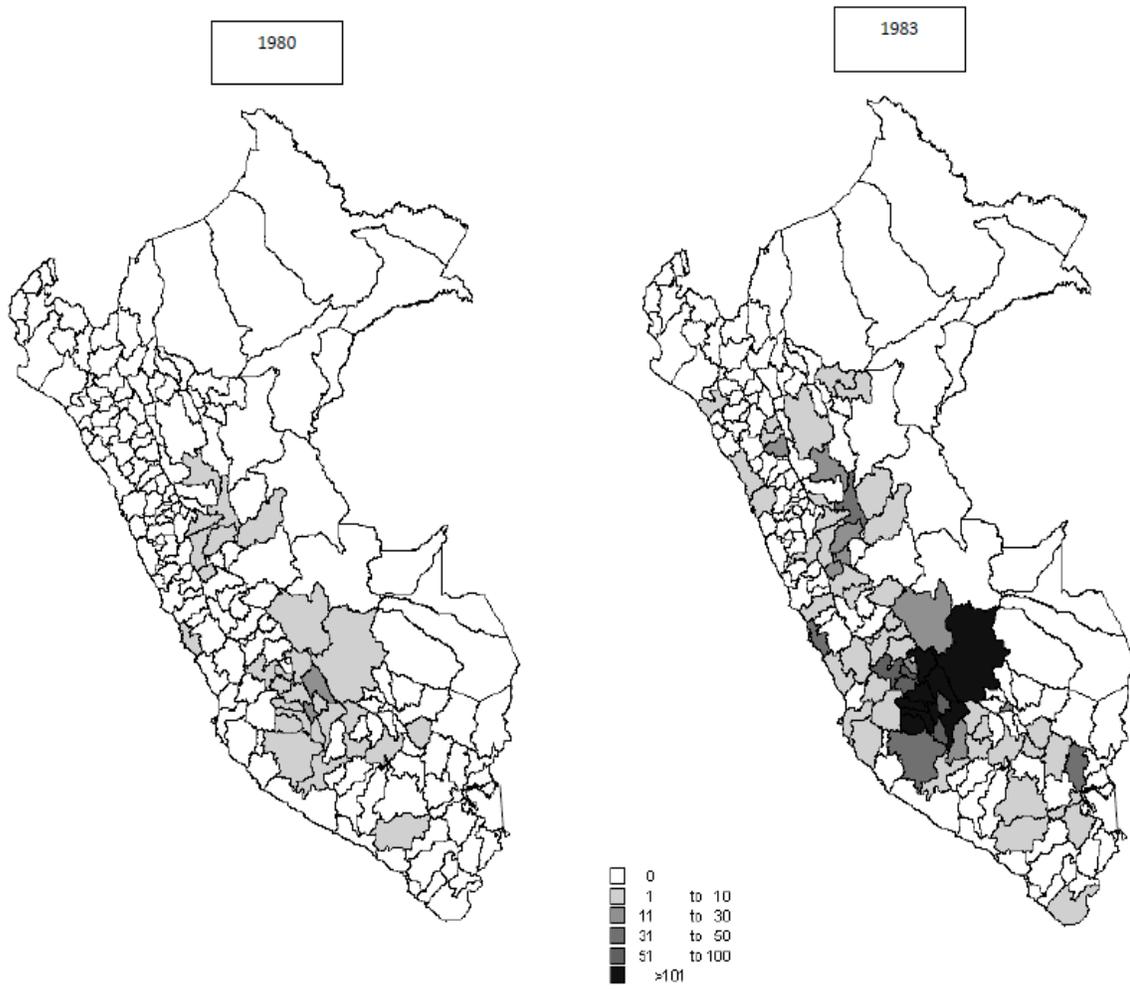|		
☐	0	
	1	to 10
	11	to 30
	31	to 50
	51	to 100
■	>101	
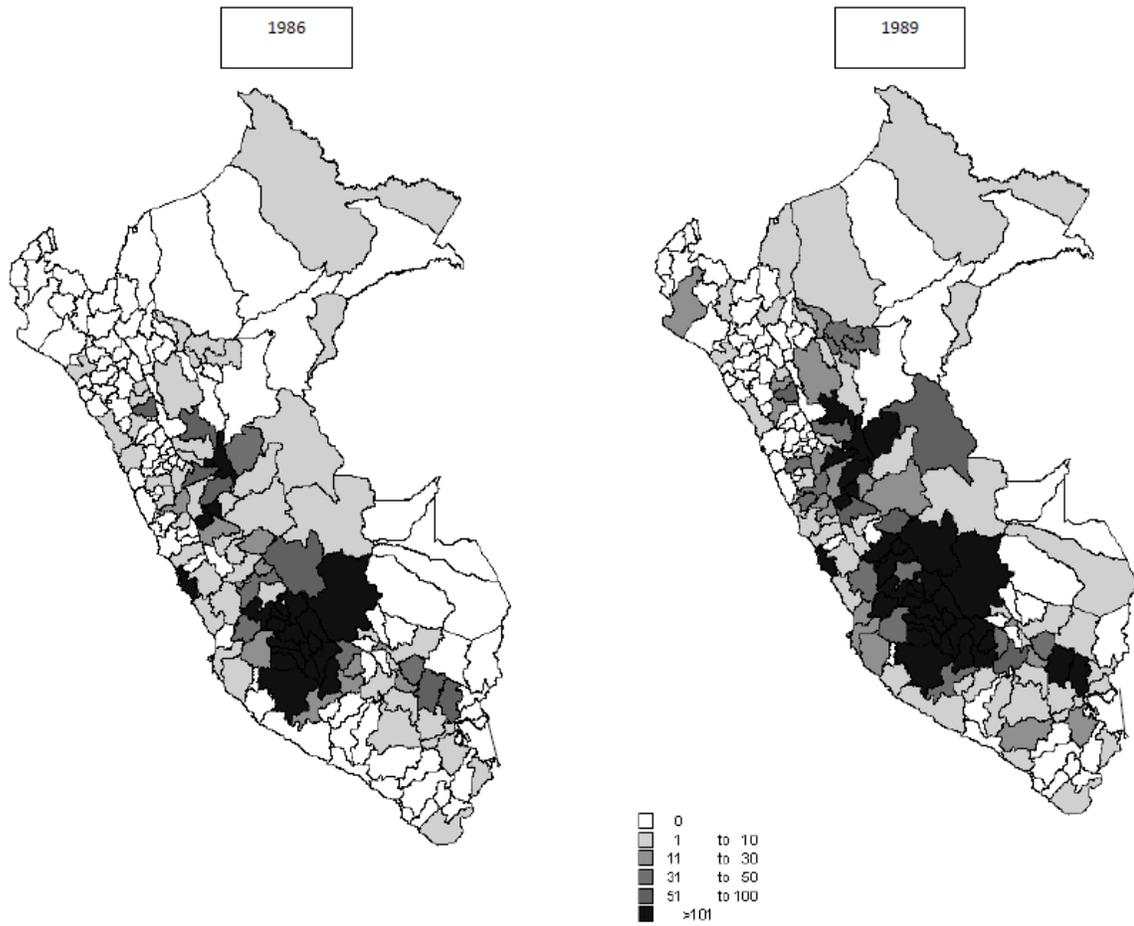
Figure 1C: Civil War in Peru, 1980-1995

1992

1995

	0
	1 to 10
	11 to 30
	31 to 50
	51 to 100
	>101

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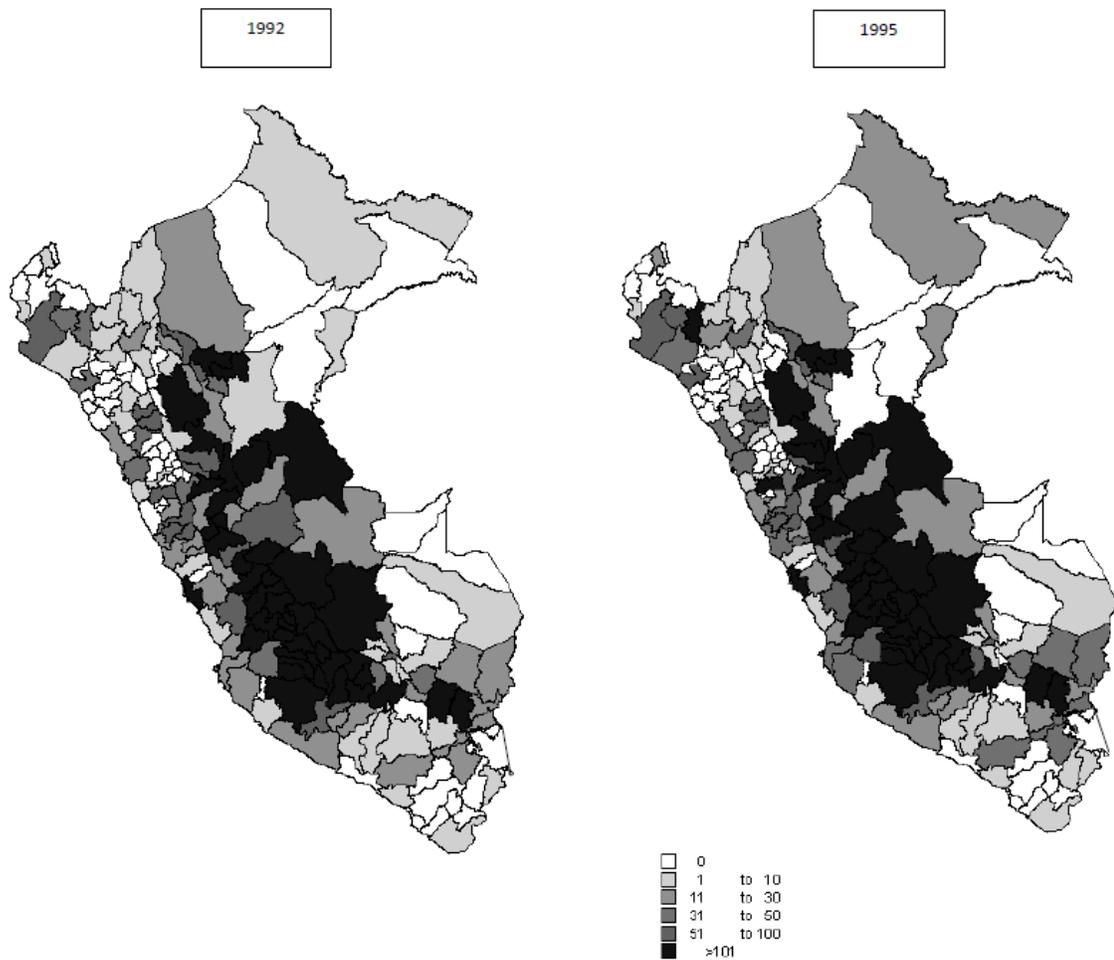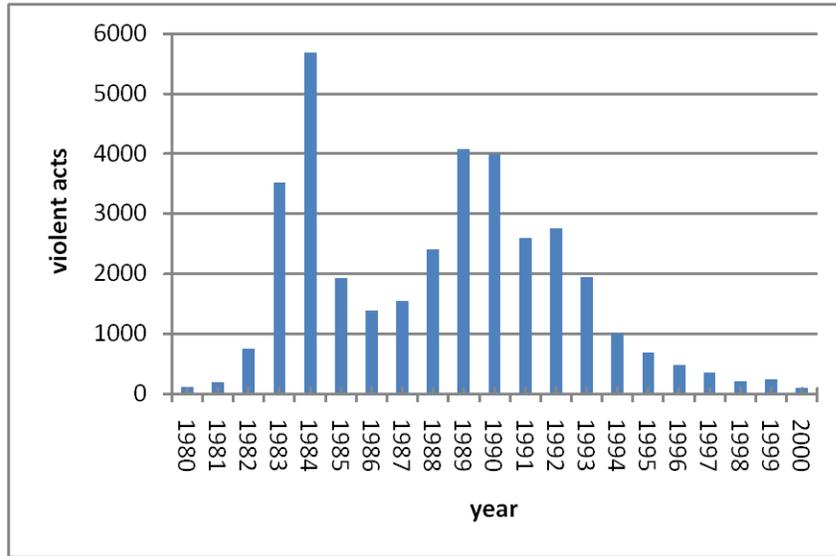

Figure 3: Number of Violent Acts by Province, Peru 1980-2000
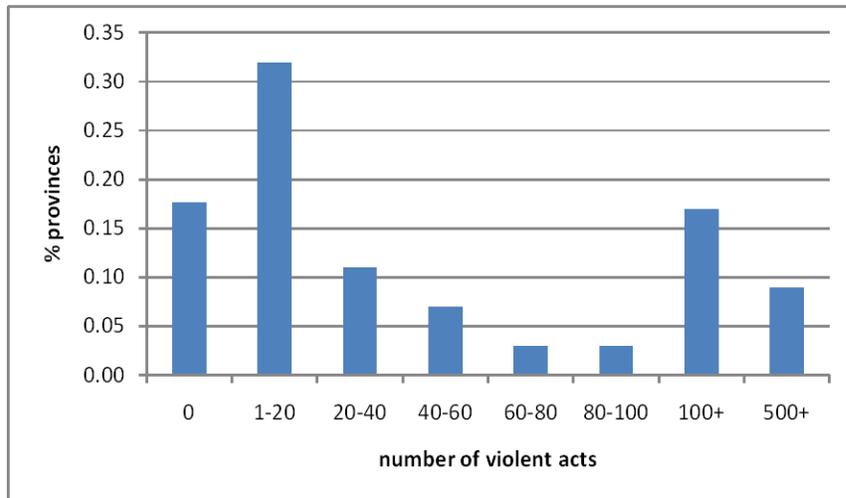
Appendix A: First-Stage Least Squared Estimates
Violence in birth district on violence measured at the closest 5 districts

Panel A: Early Childhood Exposure to Civil War

neighbor 1	neighbor 2	neighbor 3	neighbor 4	neighbor 5
0.515	0.567	0.211	0.141	0.072
(0.007)***	(0.009)***	(0.008)***	(0.007)***	(0.006)***

F-test	4466
R^2	0.33

Panel B: Fetal Exposure to Civil War

neighbor 1	neighbor 2	neighbor 3	neighbor 4	neighbor 5
0.364	0.473	0.222	0.140	0.127
(0.006)***	(0.008)***	(0.008)***	(0.007)***	(0.006)***

F-test	3422
R^2	0.28

Notes: Standard errors in parenthesis. Estimation includes individuals aged 14 to 27 on 2007.

Violence data comes from the Peruvian Truth and Reconciliation Commission (TRC)

*** Statistically significant at the 1 percent level.

Table 1: Descriptive Statistics

	mean	std. dev.	# n
A. Socio-Demographics			
age	19	4	44,983
schooling	9.5	3	44,983
male (%)	50	50	44,983
married (%)	18	38	44,983
single (%)	80	39	44,983
rural (%)	38	48	44,983
annual household income (soles)	4312	5231	44,983
Ethnicity			
castellano (%)	84	36	44,983
quechua (%)	12	33	44,983
aymara (%)	1	11	44,983
B. Violence Shocks			
Childhood exposure	12	38	44,983
Fetal exposure	3	14	44,983
C. Earnings (in soles) and violence shocks			
Childhood exposure			
[100, +)	134	229	28156
[50, 99]	144	199	28156
(10, 49]	174	277	28156
(1, 9]	206	321	28156
0	210	381	28156
Fetal exposure			
[100, +)	116	157	28156
[50, 99]	98	177	28156
(10, 49]	112	191	28156
(1, 9]	179	289	28156
0	213	368	28156

Note: Data include individuals aged 14 to 27 in 2007. Early childhood violence measure
is defined as the sum of violence acts in the first 12 quarters after birth. Fetus violence measure
is defined as the sum of violence acts in the 4 quarters right before birth. Socio-demographic
variables comes from the 2006 and 2007 ENAHO datasets. Violence data comes from the
Peruvian Truth and Reconciliation Comission. Earnings are measured in soles. The exchange rate
is around 3 soles per 1 U.S dollar.

Table 2: Impacts of Civil War on Long-Run Labor-Market Outcomes, OLS-FE

	Panel A. Early Childhood Exposure to Civil War			
	employment		*earnings*	
civil war	-0.000215	0.000062	-0.404	-0.257
	(0.000203)	(0.000161)	(0.140)***	(0.109)**

	Panel B. Fetus Exposure to Civil War			
	employment		*earnings*	
civil war	-0.000105	0.000165	-0.285	-0.031
	(0.000285)	(0.000175)	(0.208)	(0.139)

	Panel C. Early Chidhood + Fetus Exposure to Civil War			
	employment		*earnings*	
civil war (child)	-0.000223	0.000053	-0.400	-0.258
	(0.000194)	(0.000163)	(0.127)***	(0.103)**
civil war (fetus)	0.000054	0.000156	-0.025	0.020
	(0.000217)	(0.000181)	(0.170)	(0.125)
N	44983		28156	
birth FE	yes	yes	yes	yes
district FE	yes	yes	yes	yes
district linear trend	no	yes	no	yes

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, each regression includes gender, schooling, sex, rural/urban
indicator, and ethnicity. Data include individuals aged 14 to 27 in 2007. Violence measures are defined
by deviations of violence acts in birth district from 1980-1995 district mean violence.
*** Statistically significant at the 1 percent level;
 ** Statistically significant at the 5 level;
* Statistically significant at the 10 percent level.

Table 3: Impacts of Civil War on Long-Run Labor-Market Outcomes, IV-FE

	Panel A. Early Childhood Exposure to Civil War			
	employment		*earnings*	
civil war	-0.000253	0.000035	-0.452	-0.405
	(0.000233)	(0.000283)	(0.152)***	(0.180)**

	Panel B. Fetus Exposure to Civil War			
	employment		*earnings*	
civil war	-0.000214	-0.000276	-0.103	-0.071
	(0.000401)	(0.000441)	(0.237)	(0.256)

	Panel C. Early Chidhood + Fetus Exposure to Civil War			
	employment		*earnings*	
civil war (child)	-0.000044	-0.000283	-0.477	-0.405
	(0.000233)	(0.00044)	(0.145)***	(0.175)**
civil war (fetus)	-0.000248	0.000045	0.217	0.002
	(0.000381)	(0.000283)	(0.213)	(0.245)
N	44983		28140	
birth FE	yes	yes	yes	yes
district FE	yes	yes	yes	yes
district linear trend	no	yes	no	yes

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, each regression includes gender, schooling, sex, rural/urban
indicator, and ethnicity. Data include individuals aged 14 to 27 in 2007. Violence measures are defined
by deviations of violence acts in birth district from 1980-1995 district mean violence.
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 4: Significance Test
Impacts of Civil War on Long-Run Labor-Market Outcomes of Adults, Aged 43-65

	Panel A: OLS-FE Estimates		
	employment	*earnings*	migration
civil war	0.000109	-0.115	-0.000179
	(0.000077)	(0.276)	(0.00013)
N	33390	27795	33390
	Panel B. IV-FE Estimates		
	employment	*earnings*	migration
civil war	-0.000018	0.067	-0.000206
	(0.000119)	(0.198)	(0.000205)
N	33318	27749	33281
birth FE	yes	yes	yes
district FE	yes	yes	yes
district linear trend	yes	yes	yes

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, each regression includes gender, schooling, sex, rural/urban
indicator, and ethnicity. Estimation is based on individuals aged 43 to 65 in 2007. Violence measures are defined
by deviations of violence acts in birth district from 1980-1995 district mean violence.
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
Early Childhood Exposure to Civil War on Long-Run Labor-Market Earnings

	OLS (1)	IV (2)	OLS (3)	IV (4)	OLS (5)	IV (6)	OLS (7)	IV (8)	OLS (9)	IV (10)
civil war on earnings	-0.257 (0.109)**	-0.405 (0.180)**	-0.257 (0.109)**	-0.405 (0.180)**	-0.277 (0.118)**	-0.364 (0.220)*	-0.245 (0.106)**	-0.366 (0.178)**	-----	-----
civil war on migration	----	----	-----	-----	-----	-----	-----	-----	0.00006 (0.00014)	0.000278 (0.000191)
N	28156	28140	28156	28140	16470	16462	19915	19905	44983	44983
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	no	yes	yes	no	no	no	no	no	no
excluding non-remunerated family workers	no	no	no	no	yes	yes	no	no	no	no
excluding districts with no armed violence	no	no	no	no	no	no	yes	yes	no	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, each regression include gender, schooling, sex, rural/urban indicator and ethnicity. This data include individuals aged 14 to 27 in 2007. Violence measures are defined by deviations of violence acts from the 1980-1995 district mean violence. Violence shocks in individual's birth district is istrumented with violence shocks measured at the closest 5 districts to respondent's birth district.

*** Statistically significant at the 1 percent level;

 ** Statistically significant at the 5 level;

* Statistically significant at the 10 percent level.

Table 6: Heterogeneous Impacts
Impacts of Civil War on Long-Run Labor Earnings

	OLS	IV
civil war	-0.167	-0.335
	(0.176)	(0.239)
civil war*sex	-0.155	-0.119
	(0.166)	(0.266)
civil war	-0.465	-0.916
	(0.129)***	(0.246)***
civil war*rural	0.463	0.892
	(0.205)**	(0.289)***
civil war	-0.123	-0.139
	(0.092)	(0.232)
civil war*spanish	-0.261	-0.561
	(0.187)	(0.308)*
civil war	-0.367	-0.614
	(0.173)**	(0.231)***
civil war*quechua	0.235	0.465
	(0.186)	(0.318)
N	28156	28140

Notes: Standard errors clustered by district of birth in parenthesis.
In addition to district fixed effects, birthyear fixed effect, and district-specific
linear trends, each regression includes gender, schooling, sex, rural/urban
indicator, and ethnicity. Data data include individuals aged 14 to 27 in 2007.
Violence measures are defined by deviations of violence acts in birth district
from 1980-1995 district mean violence.
*** Statistically significant at the 1 percent level;
** Statistically significant at the 5 level;
* Statistically significant at the 10 percent level.

Table 7: Pathways
Impacts of Civil War on Long-Run Labor Earnings

	height	wealth index
civil war	-0.0045	-0.00063
	(0.0046)	(0.00016)***
N	1890	27762

Notes: The civil war effect on height is estimated using the 2005
Peruvian Demographic and Health Survey (DHS), while the
wealth index coefficient is estimated using the 2006-2007 ENAHO
survey. The wealth index is the first pricipal component of 15
household assests including: flush toilet, drinking water,
refrigerator, computer, telephone, cellphone, own house, invaded land,
microwave, video recorder, music recorder, wash machine, car, bicycle
and household income per capita.
*** Statistically significant at the 1 percent level;
 ** Statistically significant at the 5 level;
* Statistically significant at the 10 percent level.