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

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I. Introduction

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). While the direct short-run effects of civil wars on the number of displaced individuals, orphaned children, and murdered people are well documented (e.g., Collier et al. 2003), the long-run consequences of armed conflict on human capital development, institutions, and social norms are still unclear (see Blattman and Miguel [2010] for an excellent review of the literature).

In this study, we investigate whether early life exposure to civil war in Peru affects labor-market earnings later in life, following the critical-period programming theory that highlights the role of early life circumstances in determining long-run economic outcomes (e.g., Godfrey and Barker 2000). For almost 2 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 at US$9 billion, equivalent to 66% of Peru's total foreign debt, or 45% of its gross domestic product in 1988 (DESCO 1989).

Evidence on the long-run labor-market consequences of civil wars is still a missing gap in the literature. An exception is the work of Blattman and Annan (2010), who reported less schooling and work experience for former child soldiers in Uganda and, therefore, less success in their labor-market outcomes as adults. A somewhat different picture was obtained by Humphreys and Weinstein (2007), who found that increases in Sierra Leone combatants' vi-

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olence 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 preschool exposure.[1] 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 on labor-market earnings, a methodological advantage with respect to the limited time horizons of most civil war data sets. Finally, the use of a very fine level of variation, at the district level, is an improvement over more aggregate (provincial and departmental) civil war data sets.

A recent body of literature on the legacies of civil wars suggests that schooling (e.g., Shemyakina 2011) and health outcomes (e.g., Camacho 2008; Akresh, Bundervoet, and Verwimp 2009) are negatively affected by episodes of civil war, which, seen through the scope of human capital models, will inevitably affect total lifetime earnings of those affected. Two recent papers developed independently and in parallel to this study provide microempirical evidence on the negative effects of civil war on school achievement and health outcomes in Peru. Leon (2010) uses the 2007 Household Census to report that boys and girls aged 6–17 accumulate more years of school deficit as children and fewer years of schooling as adults. The second study, by Grimard and Laszlo (2010), shows negative long-run effects of Peru's armed conflict on women's anthropometric measures on the basis of the 2004–8 Demographic and Health Surveys (DHS).[2] Both studies exploited regional variation in the timing and intensity of violence to identify long-run effects via standard ordinary least squares (OLS) regressions. Similar strategies were used in recent studies addressing the impact of civil war violence on human capital outcomes (e.g., Akresh et al. 2009; Chamarbagwala and Moran 2011).

From a methodological point of view, we aim to advance this literature by way of a systematic analysis of violence measures. We address measurement error in the violence data set by using instrumental variables regressions in

[1] This article has benefited from a recent body of research in economics that relates conditions in early life to later outcomes (e.g., Strauss and Thomas 1998; Glewwe and King 2001; Behrman and Rosenzweig 2004; Maccini and Yang 2009).

[2] Leon's working paper appeared in September 2009 at http://ipl.econ.duke.edu/bread/papers/0909conf/Leon.pdf. At that time, the first version of this article was an ongoing work and submitted to the IZA/World Bank Conference in Development in November 2009 for consideration. The latest draft by Leon appeared in 2010 and includes child and mother anthropometrics using the 2002 DHS. Grimard and Laszlo's preliminary draft appeared in May 2010.

which variables for violence measured in the closest three districts serve as instruments for violence in the individual's district of birth. Measurement error, inherently affecting the majority of civil war data sets, has not been addressed in this literature (Blattman and Miguel 2010). Likewise, the distinction between civil war and violence in civil wars has been largely overlooked in the microdata analysis of civil wars (Kalyvas 2006). Accounting for civil war violence has primarily been based on a single specific measure of violence that includes deaths and forced disappearances (Grimard and Laszlo 2010; Leon 2010), abductions (Blattman and Annan 2010), length of exposure to civil war (Akresh et al. 2009), and damage to household dwellings (Shemyakina 2011). 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. These analyses constitute a contribution to the existing literature on civil wars.

We use two different sources of information to fulfill the data requirements of this investigation. To capture the sociodemographic 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), which collected a comprehensive data set 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 date of birth of the respondent. In particular, we examine the earnings of working-age individuals born between 1974 and 1993 (i.e., aged 14–34, in 2007) and, thus, affected by the civil war violence during their first 6 years of life.

Four 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 1 standard deviation increase in early childhood exposure to violence leads to a 5% fall in adult monthly earnings, 3.5% reduction in the probability of working in formal jobs, and 6% reduction in the probability of working in large firms. Neither fetal nor preschool periods are significantly related to adult earnings. This result is consistent with several studies that explain that the most important stage in life to determine the income gradient is early childhood (see Case and Paxson 2010).

Second, women are disproportionately affected by civil war violence relative to men. The magnitude of these differences reaches almost 5 percentage points for the earnings variable. This robust result adds to the literature of gender bias in developing countries that finds that, in times of crisis, the negative impacts of shocks are greater for girls than for boys (see Dreze and Sen [1989] for a review). Likewise, civil war violence has disproportionately affected the long-run earnings of urban people, as compared to those living in rural locations.

Third, 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 forced disappearances yields the strongest negative impacts in the long run; sexual violations disproportionately affect the wages of women, while torture and forced disappearances disproportionately affect the wages of men. Focusing only on the most common type of violence (i.e., deaths) 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.

Fourth, the analysis of the mechanisms connecting adult earnings and violence suggests that children's health (i.e., height) is an important intervening channel. A 1 standard deviation increase in armed violence during early childhood is significantly associated with 1.2 centimeters lower height in the short run. Health, along with school deficits and negative shocks in household wealth, might explain the negative relationship between early life exposure to civil war and adult earnings.

The remainder of the article is organized as follows. In Section II, we provide a background discussion about the Peruvian civil war. Section III describes the data sets and provides some descriptive statistics. In Section IV, we discuss the empirical strategy along with the main results and sensitivity analyses. Section V addresses the study of violence on civil wars. Section VI discusses potential pathways. Finally, Section VII concludes.

II. 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 a self-proclaimed agent of a world history destined to conclude in a communist revolution. 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" in which 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.[3]

The initial response from the government was tardy and ineffective (Palmer 1992). The Peruvian Army began its operations against the Shining Path 2 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. Figure 1 illustrates 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 share of provinces affected by the political violence went from 17% to 36%, including the capital, Lima. By 1986, the civil war expanded to the jungle regions, as 46% 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 terrorist group, the Revolutionary Movement Tupac Amaru, began a cycle of violence against the government. Its actions were much less lethal than those of the Shining Path, accounting for only 2% 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% of the national territory. Finally, in 1995, almost 75% of the Peruvian provinces experienced the burden of civil war violence.

The end of the 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 collapsed abruptly. The end of the civil war in Peru illustrates new empirical evidence suggesting that insurgent leaders do matter (Guidolin and La Ferrara 2007).

The Peruvian armed conflict was marked by deliberate indiscriminate violence against civilians (TRC 2003). The production of violence was provided

[3] Even though the Peruvian civil war seems to fit the activist rebellion typology developed by Weinstein (2006), it does not follow its main prediction. 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. However, the "opportunistic rebellion type" predicts that civil wars emerging in rich natural resources areas tend to commit high levels of indiscriminate violence (Weinstein 2006). In this regard, we find useful the analytical boundaries of Kalyvas (2006), who defined a typology of civil war on the basis of 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."

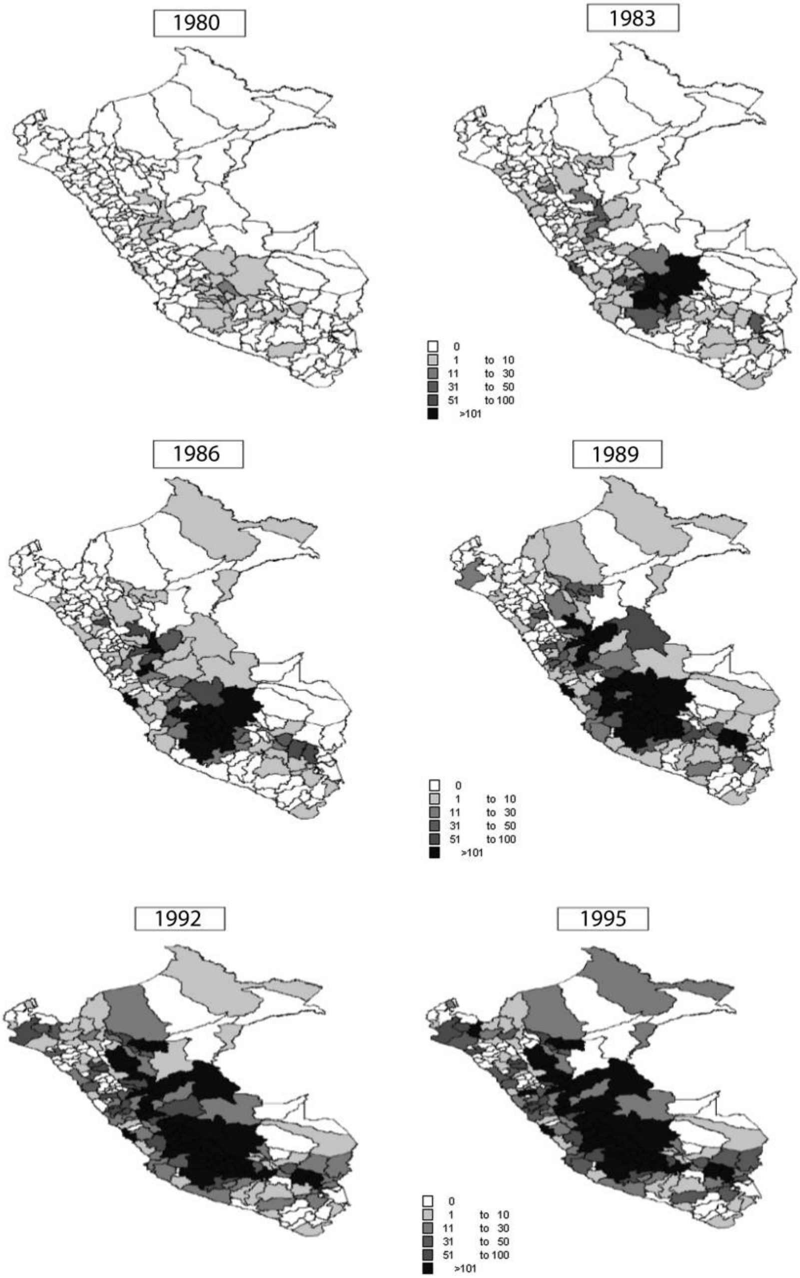

Figure 1. Peruvian civil war province-level distribution of violent events between 1980 and 1995

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

This civil war violence evolved over the course of almost 2 decades, creating substantial regional variation in the timing and intensity of the violence. During 1980–82, the number of violent acts ranged between 100 and 800, and then the intensity of the civil war increased vertiginously over the next 3 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 intensity of violence 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 1989–90 and lasting until 1993. Starting in 1994, the number of violent acts decreased abruptly, until it faded in 1997. Similarly, the Peruvian civil war exhibits substantial regional variation in the intensity of violence. While approximately 25% of the provinces never experienced a single act of violence between 1980 and 1995, 32% suffered fewer than 20 violent acts, 17% experienced between 100 and 500 violent attacks, and 9% 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 those most affected, the national death toll would have been 450,000 instead of 25,000 (TRC 2003).[4]

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, while other districts lost over 5% 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. In the next sections of this article, 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.

III. Data Sources

We used the 2006 and 2007 waves of the Peruvian household survey, ENAHO, conducted yearly by Peru's national statistical agency, the Instituto Nacional de Estadistica e Informatica. The surveys provide current demographic, socioeconomic, and labor-market information on a nationally representative sample of

[4] Figures A1 and A2 in the online appendix show the time and spatial variation in intensity of violence.

households and individual household members, including children.[5] During each year, approximately 22,000 households are interviewed across all 24 states in both urban and rural regions. 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 14–34 during the year of the survey (i.e., civilians born between 1974 and 1993) and, thus, affected by civil war violence during their first 6 years of life.

Civil war data were obtained from the Peruvian TRC, a nonjudicial temporary body established in June 2001, with the mandate to examine and collect information about the country's civil war period. Its work focused on violent acts, as long as they were imputable to terrorist organizations, state agents, or paramilitary groups (Supreme Decrees nos. 065-2001 and 101-2001). The TRC work, formally concluded in 2003, constitutes a "historical memory" that documents extensive, detailed information from victims, survivors, and other witnesses (TRC 2003).

The violence data collected by the TRC come from the reconstruction of violent 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 violent events were documented.

We link each individual in the ENAHO data to the violence data for their district-specific date of birth. 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 "preschool exposure," defined as the sum of violent acts between 13 and 24 quarters of one's life. We compute these variables in the TRC data set and merge this information with each birth district/birth date combination represented in the ENAHO sample. A total of 1,591 districts were identified in the ENAHO data after merging both data sets.

Table 1 reports the summary statistics for the final matched sample. It is composed of 40,268 workers aged 14–34, after including nonremunerated family workers. The average individual in our sample is 22.7 years old with

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

TABLE 1

DESCRIPTIVE STATISTICS: ENAHO-VIOLENCE MATCHED DATA SETS, WORKERS AGED 14–34

	Full Sample	Men	Women	Urban	Rural
Sociodemographic:					
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 (15.9)
Per capita annual household income (soles)	4,342 (5,857)	4,257 (5,559)	4,448 (6,202)	6,084 (7,207)	2,267 (2,316)
Monthly earnings (soles)	277 (473)	343 (510)	197 (411)	408 (561)	122 (268)
Formal jobs (1= formal, 0 otherwise; %)	18.9 (39.3)	20.0 (40.0)	17.5 (38.0)	29.9 (45.8)	5.7 (23.2)
Firm size (1 = 20+ workers, 0 otherwise; %)	15.2 (35.9)	17.5 (38.0)	12.4 (33.0)	22.7 (41.8)	6.3 (24.3)
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)
Preschool exposure [4, 6 years]	8.6 (34.0)	8.5 (32.8)	8.7 (35.5)	9.9 (34.0)	6.9 (34.0)
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 (207)	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)
Preschool 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 (485)	342 (495)	210 (463)	375 (555)	113 (242)
0	282 (489)	354 (537)	195 (407)	453 (598)	126 (281)
N	40,268	22,186	18,082	21,898	18,370

Note. Standard deviations in parentheses. Matched sample includes individuals aged 14–34 in 2007. Fetal exposure is defined as the number of violent acts in the four quarters right before birth. Early childhood exposure is defined as the number of violent acts in the first 12 quarters after birth. Preschool exposure is defined as the number of violent acts in quarters 13–24 after birth. Sociodemographic variables come from the 2006 and 2007 ENAHO (Encuesta Nacional de Hogares) data sets. Violence data come from the Peruvian Truth and Reconciliation Commission. The exchange rate is around 3 soles per US$1 in 2006–7.

9.4 years of schooling, although there is a large schooling difference between urban (10.9) and rural (7.68) areas. Only 33.7% of the sample is married, which is roughly the same across all subsamples. Almost half of the individuals live in urban areas, where 10% do not speak Spanish as their first language. This is 40% in rural areas. The average per capita household income is nearly three times higher in urban areas than in rural areas. The proportion of working men is higher (55%), relative to that of women (45%). The mean monthly earnings are 277 soles (or about US$100), after inputting zero earnings for nonremunerated family workers. Large earnings gaps are observed between men and women and between urban and rural areas.

Table 1 shows that the average individual in our sample was exposed to two, seven, and nine violence shocks while in utero, during early childhood, and during preschool 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 table 1. 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 preschool violence measures.

IV. Empirical Strategy and Results

A. Identification

The identification strategy relies on a difference-in-differences approach in which we test whether children born in districts affected by armed violence have more adverse labor-market earnings later in life than do their counterparts born before or after them in the same district, relative to those who are born in other regions of the country during the same year. This is a standard empirical approach in economics and has been used in recent studies addressing the relationship between civil wars and health (e.g., Akresh et al. 2009), schooling (e.g., Leon 2010), and climate (Burke et al. 2009), among others.

From a methodological perspective, we aim to advance this literature by considering the potential effects of attenuation bias due to measurement error. Unreported or undocumented violent acts, for instance, may cause the TRC's measurements to be only imperfectly correlated with actual violence in the individual's birth district. If this is the case, the least squares estimates will be biased and attenuated toward zero (Wooldridge 2005). When the problem is that the violence measure (V) is only an imperfect proxy for the true violence (V^*), there may be other imperfect proxies for V^* that are also available. Finding such proxy variables (Z) is not as difficult as in the standard instrumental variable (IV) approach in which the correlation with the error term is due to a deeper economic mechanism (Bound, Brown, and Mathiowetz 2001). Violence in an individual's birth district is therefore instrumented with the violent acts measured at the closest three districts to the respondent's birthplace.[6] Given that the work of the TRC in Peru is considered one of the most comprehensive efforts to recover extensive, detailed information on civil war violence, we instrument violent acts with alternative measures of the same variable, under the assumption that the measurement error in one proxy variable is uncorrelated with the measurement error in the other proxy variable. A similar strategy was used by Maccini and Yang (2009), who estimate the long-run socioeconomic impacts of environmental conditions in early life.

We implement a two-stage least squares model in which, in the first step, the main regressor (V_{ijt}^{0}), the shocks of violence for individual i in district j born in time t, is instrumented with three analogous violence variables, measured during the same time span but in the closest three districts (Z_{ijt}^{v}):

$$V_{ijt}^{0} = \alpha_0 + \sum_{v=1}^{3} \alpha_v Z_{ijt}^{v} + X_{ijt}' \alpha_4 + \eta_j + \tau_t + \mathrm{TREND}_{jt} + \mu_{ijt}. \qquad (1)$$

Because accounting for omitted variable bias is of particular concern, we use district fixed effects, η_j, to control for the persistent effects of violence on the districts where individuals are born, and birth-year fixed effects, τ_t, to control for the specific cohort effects. However, this may not identify the causal impacts if the timing of the violent acts followed a particular pattern in terms of district-level characteristics related to 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 their birth district. Finally, a set of sociodemographic control variables (X) is also considered, by 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).

This first-stage regression for fetal, early childhood, and preschool exposures to birth district violence is reported in the online appendix, available in the online version of *Economic Development and Cultural Change*. All coefficients have a positive relationship with the violence variable in the birth district and are significant at the 1% level. The magnitude of the coefficients reveals a strong correlation between birth district violence and measures of violence in

[6] The "closest" districts are by definition those that show the shortest geographic distance to the reference district. The distance is measured using the Universal Transversa Mercator, a standard system used worldwide for this type of analysis, through the Hawth's Analysis Tools for ArcGIS software.

the neighboring districts, particularly for the two closest districts. That explains the rationale for using only the closest three districts since including the fourth or fifth closest neighbors reduces the power of the instruments.[7] Moreover, the test for the joint significance of the instruments passes conventional threshold levels used for detecting weak instruments in linear IV regressions (Stock and Yogo 2002), as indicated by the value of the *F*-test and its *p*-value.

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

$$y_{ijt} = \beta_1 + \beta_2 \tilde{V}_{ijt} + X'_{ijt}\beta_3 + \eta_j + \tau_t + \mathrm{TREND}_{jt} + \varepsilon_{ijt}, \tag{2}$$

where $\tilde{V}_{ijt}$ represents the instrumented measure of civil war violence. The parameter of interest is β_2, which represents the impact of (instrumented) 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 persistent effects of violence in the birth district, age cohort, and long-running trends in one's birth district. The idiosyncratic mean zero error term is ε_{ijt}, which is assumed to be distributed independently of all η_j and τ_t.[8]

B. *Long-Run Labor-Market Earnings Impacts*

Table 2 reports the instrumental variables with fixed effects results (IV-FE) for adult earnings after considering four separate subsamples in the econometric analysis, given the evidence that some groups are more vulnerable to violence than others (Blattman and Miguel 2010). From the top of table 2 down, we present five panels: the full sample, men, women, urban, and rural. Columns 1–3 depict the impact of fetal, early childhood, and preschool exposure to violence, respectively. Clustered standard errors at the district level are shown in parentheses. For the interpretation of the results, this study focuses on the impact of

[7] The magnitude and statistical significance of the impacts do not change much, however, when implementing the IV approach with the five closest neighbors.

[8] A potential concern when using geographic data is the spatial error correlation that deals with "nuisance" data dependence due to the presence of unobserved local common shocks. Spatial error correlation is a special case of a regression with nonspherical error term, in which the off-diagonal elements of the covariance matrix express the structure of the spatial dependence (Anselin 1988). As such, spatial error correlation does not result in OLS biased estimates, but it would affect the standard error of the estimated parameters in the first-stage regression. The inclusion of spatial (district) fixed effects is considered the preferable strategy for addressing spatially correlated errors in cross-sectional data (Beron, Murdoch, and Thayer 2001; Kuminoff, Parmeter, and Pope 2010).

TABLE 2

IV-FE ESTIMATES OF THE IMPACTS OF EARLY LIFE EXPOSURE TO CIVIL WAR ON LONG-RUN EARNINGS

	(1)	(2)	(3)	(4)
Full sample:				
Fetal exposure [−1, 0]	−.157	. . .	. . .	.067
	(.243)	. . .	. . .	(.197)
Early childhood exposure [0–3]	. . .	−.432	. . .	−.431
	. . .	(.140)***	. . .	(.138)***
Preschool exposure [4–6]	. . .	. . .	−.239	−.224
	. . .	. . .	(.128)*	(.133)*
N	40,246			
R^2	.33			
Men:				
Fetal exposure [−1, 0]	−.026	. . .	. . .	.081
	(.389)	. . .	. . .	(.336)
Early childhood exposure [0–3]	. . .	−.336	. . .	−.344
	. . .	(.180)*	. . .	(.171)**
Preschool exposure [4–6]	. . .	. . .	−.181	−.180
	. . .	. . .	(.183)	(.181)
N	22,171			
R^2	.36			
Women:				
Fetal exposure [−1, 0]	−.409	. . .	. . .	−.113
	(.280)	. . .	. . .	(.276)
Early childhood exposure [0–3]	. . .	−.518	. . .	−.496
	. . .	(.167)***	. . .	(.173)***
Preschool exposure [4–6]	. . .	. . .	−.192	−.177
	. . .	. . .	(.227)	(.236)
N	18,075			
R^2	.33			
Urban:				
Fetal exposure [−1, 0]	−.511	. . .	. . .	−.132
	(.456)	. . .	. . .	(.439)
Early childhood exposure [0–3]	. . .	−.703	. . .	−.691
	. . .	(.202)***	. . .	(.194)***
Preschool exposure [4–6]	. . .	. . .	.064	.026
	. . .	. . .	(.220)	(.230)
N	21,894			
R^2	.29			
Rural:				
Fetal exposure [−1, 0]	.003	. . .	. . .	.094
	(.219)	. . .	. . .	(.248)
Early childhood exposure [0–3]	. . .	−.193	. . .	−.193
	. . .	(.099)*	. . .	(.102)*
Preschool exposure [4–6]	. . .	. . .	−.151	−.135
	. . .	. . .	(.119)	(.124)
N	18,352			
R^2	.36			

Note. Standard errors clustered by district of birth in parentheses. In addition to district fixed effects, birth-year fixed effects, and district-specific linear trends, regressions include gender, schooling, marital status, rural/urban indicator, and ethnicity controls. Estimation is based on individuals aged 14–34 in the 2006–7 Encuesta Nacional de Hogares data. The intensity of violence is measured as the number of violent acts for one's birth district in each period of analysis. Violence in individual's birth district is instrumented with the violent acts measured at the closest three districts to respondent's birthplace.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

1 standard deviation in the violence measures, which amounts to 12, 32, and 34 violent acts for fetal, early childhood, and preschool measures, respectively.

The IV-FE estimates show that the most sensitive period of early life exposure to civil war violence is the first 36 months of life. A 1 standard deviation increase in childhood exposure to violence leads to a reduction of 14 soles in monthly earnings, which is equivalent to a 5% fall (relative to the mean earnings of individuals aged 14–34). This result is statistically significant at the 1% level. At the same time, the result suggests that exposures to civil war while in utero or during preschool periods are less important predictors of labor-market success, relative to violent shocks occurring during the weaning and postweaning 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 affect 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 reflect the correlation of violence measures across time. To address this point, we include all fetal, early childhood, and preschool violence measures in the same regression. If the coefficients associated with 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. Table 2, 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 similar to those observed in column 2, suggesting that early childhood exposure to violence shocks matters on its own.

Table 2 reports significant gender differences in the impacts of civil war on adult earnings. Women are more adversely affected than men. A 1 standard deviation increase in the intensity of violence in early childhood leads to a significant 8% reduction in monthly earnings for women, while the magnitude of the impact on men is 3.2%. This result is consistent with several microdata analyses that illustrate a disproportionately large impact of civil war violence on schooling and health for civilian women across diverse geographical locations, including Asia (Shemyakina 2011), Africa (Akresh et al. 2009), and Latin America (Chamarbagwala and Moran 2011).

Finally, table 2 shows significant differences across urban and rural localities. Exposure to civil war in early childhood disproportionately affects the long-run earnings of civilians living in urban districts (−5%), while we observe less statistical power for their rural counterparts. We provide three potential explanations for this finding. First, it is possible that children who experience the worst types of violence in rural areas migrated later to urban cities, where their parents feel relatively safer. Second, the likelihood of measurement error in the computation of civil war intensity may be greater in rural than in urban areas. The IV-FE estimates we present in tables 3 and 4 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 rural estimates are relatively larger. This, in turn, reduces the statistical significance of the point estimates for rural areas.

It is important to highlight that considering measurement error in the violence data yields almost a twofold increase in the magnitude of the point estimates with respect to their OLS-FE counterpart estimates (reported in the online appendix). This finding reveals attenuation bias in the estimation of long-run earnings impacts when neglecting measurement error in the violence data. Overall, the qualitative findings from both OLS-FE and IV-FE models are consistent. 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 preschool periods appear as significant periods when estimating the impacts of violence on earnings later in life. This result is consistent with a stream of studies that pinpoint early childhood as the most important stage in life to determine the income gradient (Case and Paxson 2010).

C. *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 test whether our results are driven by the inclusion of nonremunerated family workers, which may play a role in explaining the large impacts we found for women. We thus exclude this category of workers from the analysis, so that the estimates can better reflect the workers' market-value productivity. Column 2 in table 3 shows the new point estimates for early childhood exposure to civil war violence (col. 1 reports the baseline results for comparison). One pattern emerges: the point estimates are almost the same for all subsamples but women and remain statistically significant. In particular, we observe even bigger point estimates for women when excluding nonremunerated family workers from the sample estimation. Relative to the mean earnings of remunerated workers, though, the magnitude of the impact estimates remains almost the same: an 8.7% and 2.2% reduction in monthly earnings for women and men, and a 4.6% and 2.7% reduction for civilians living in urban and rural districts. These results lead us to believe that the inclusion of non-

TABLE 3

SENSITIVITY TESTS: IV-FE ESTIMATES OF THE IMPACTS OF EARLY CHILDHOOD EXPOSURE TO CIVIL WAR ON LONG-RUN LABOR-MARKET EARNINGS

	Full Sample					Men					Women				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Early childhood civil war on earnings	−.432	−.479	−.403	−.401	. . .	−.336	−.33	−.301	−.30169	. . .	−.518	−.964	−.507	−.481	. . .
	(.140)***	(.152)***	(.138)***	(.146)***	. . .	(.180)*	(.181)*	(.177)*	(−.18399)	. . .	(.167)***	(.276)***	(.162)***	(.182)***	. . .
Early childhood civil war on birth-district birth-year cohort size	. . .	. . .	. . .	. . .	.0028	. . .	. . .	. . .	. . .	.0034	. . .	. . .	. . .	. . .	.0027
	. . .	. . .	. . .	. . .	(.0041)	. . .	. . .	. . .	. . .	(.0043)	. . .	. . .	. . .	. . .	(.0047)
N	40,246	26,461	28,442	40,246	64,176	22,171	16,366	15,692	22,171	32,264	18,075	10,095	12,750	18,075	31,912
	Urban					Rural									
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)					
Early childhood civil war on earnings	−.703	−.737	−.667	−.700	. . .	−.193	−.21	−.184	−.162	. . .					
	(.202)***	(.198)***	(.196)***	(.211)***	. . .	(.099)*	(.136)	(.101)*	(.096)*	. . .					
Early childhood civil war on birth-district birth-year cohort size	. . .	. . .	. . .	. . .	.0057	. . .	. . .	. . .	. . .	.0031					
	. . .	. . .	. . .	. . .	(.0065)	. . .	. . .	. . .	. . .	(.0023)					
N	21,894	17,455	17,731	21,894	39,355	18,352	9,006	10,711	18,352	24,821					
Birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Excluding nonremunerated family workers	No	Yes	No	No	No	No	Yes	No	No	No	No	Yes	No	No	No
Excluding districts with no armed violence	No	No	Yes	No	No	No	No	Yes	No	No	No	No	Yes	No	No
Including violence years before birth	No	No	No	Yes	No	No	No	No	Yes	No	No	No	No	Yes	No

Note. Standard errors clustered by district of birth in parentheses. In addition to district fixed effects, birth-year fixed effects, and district-specific linear trends, the baseline regression includes gender, schooling, marital status, rural/urban indicator, and ethnicity controls. The regression includes individuals aged 14–34 in 2006–7 Encuesta Nacional de Hogares data. The intensity of violence is measured as the number of violent acts in one's birth district. Violence in individual's birth district is instrumented with violent acts measured at the closest three districts to respondent's birthplace.

* Significant at 10%.

*** Significant at 1%.

remunerated family workers in the baseline estimation does not explain the larger violence impacts for women.

Second, 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 3 in table 3 reports the new point estimates for early childhood exposure to civil war. Once again, all quantitative and qualitative findings hold. The point estimates are very stable, with all coefficient estimates significant at similar levels.

Third, we include measures of exposure to violence in the years before birth to be able to rule out that the effects identified in the baseline model do not correspond to secular trends in the violent districts. Column 4 in table 3 shows the new point estimates after including in the same regression two measures of violence, corresponding to 1–3 years before birth and 4–6 years before birth, in addition to fetal, early childhood, and preschool violence measures. The emerging point estimates for the early childhood period vary marginally for all subsamples and remain statistically significant except for men. However, none of the coefficients for the violence measures in the years before birth are statistically significant.

Fourth, civil war conflict might induce selective mortality, and thus selection into the sample might confound the results. We therefore investigated whether the size of birth cohorts is affected by civil war violence, by regressing the number of individuals appearing in the ENAHO sample at the birth-district birth-year level on early childhood civil war violence. The point estimates reported in table 3, column 5, are not statistically significantly different from zero across all subsamples. The survival-based selection bias story seems more plausible for episodes of civil war characterized by genocide and ethnic cleansing in which the spatial extermination of large shares of the population is expected (Kalyvas 2006).

Fifth, we explore how sensitive our results are to migration status. Migrants are defined as individuals who at the time of the survey were residing in a district different from their district of birth (settled migrants).[9] We ex-

[9] The ENAHO data allow us to identify migration status as a onetime event. With these data it is not possible to identify return migrants, those who moved during the course of their lifetime but returned to their district of birth at the time of the survey. Return migrants, however, are a small percentage of migrants in Peru. Laszlo and Santor (2009) used the 1991 round of the Living Standard Survey and

pected 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 table 4, we use a dummy variable as the dependent variable indicating whether a person migrated or not from her or his district of birth. Column 1 shows the early childhood estimates without controlling for the intensity of violence before birth. The coefficients are negligible and statistically insignificant in all cases. Yet, it is important to consider also the intensity of civil war in the years before birth and after birth. Failing to do so could hide some important differences between migrants and nonmigrants, as well as within migrant groups (i.e., those who migrated early vs. late in the war). We thus include in the regressions measures of violence in the period corresponding to 6 years before birth through age 6. Column 2 in table 4 shows the point estimates. There appears to be no systematic effect of exposure to armed violence on migration early in life. Furthermore, it is possible that the relationship between migration and civil war violence in early childhood might be different, depending on the place of destiny. Thus, two additional measures of migration status are considered: migration to the capital Lima and migration to other urban areas. Unreported results show that the zero migration effects hold independently of the place of destiny.

Overall, this study finds that exposure to civil war violence in early childhood is not statistically related to migration status. A plausible explanation is that while this study focuses on exposure to civil war in early childhood (age 0–3), migration is more likely to occur at later ages. Families with newborns tend to stay in the same location during the years around birth because moving is typically more difficult, more costly, and more uncertain for these families than for families without children or older children (Bernard, Finnie, and St. Jean 2008).

Sensitivity of the main results to the definition of civil war violence is also assessed by expressing violence measures as a share of the population rather than as a count. The online appendix shows these IV-FE results after defining the violence variables per 10,000 people. The information on population size comes from the Peruvian census data in 1981. The point estimates are quite comparable with the results reported in table 2. Since this study is based on districts, the smallest administrative unit in Peru, and considers urban and rural districts separately, the point estimates are robust to the distinction between counts and share of the population measures.[10] Furthermore, we test whether

reported that 80% of migrants are settled migrants in Peru. Moreover, these authors showed that in most cases migration follows a rural-urban pattern (the reverse only happens in 4% of cases).

[10] Two caveats should be taken into account when analyzing these results. First, as violence intensity varies over time across districts, a better measure of violence per capita would require changing the denominator (the size of the population) for each district between 1974 and 1993. However,

TABLE 4

SENSITIVITY TESTS: IV-FE ESTIMATES OF THE IMPACTS OF CIVIL WAR VIOLENCE EXPOSURE ON MIGRATION

	Full Sample		Men		Women		Urban		Rural	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
[−6, −4] years before birth	. . .	−.000344	. . .	−.000324	. . .	−.000172	. . .	.000044	. . .	−.000124
	. . .	(.000289)	. . .	(.000231)	. . .	(.000357)	. . .	(.00038)	. . .	(.000226)
[−3, 1] years before birth	. . .	.000125	. . .	.000341	. . .	−.000193	. . .	.000365	. . .	−.000093
	. . .	(.0003)	. . .	(.000387)	. . .	(.000473)	. . .	(.000504)	. . .	(.000264)
Fetal exposure	. . .	−.000352	. . .	−.000517	. . .	−.000584	. . .	.000487	. . .	−.00062
	. . .	(.000313)	. . .	(.000556)	. . .	(.00064)	. . .	(.000514)	. . .	(.000337)*
Early childhood exposure	.000059	.000095	.000027	.000113	.000139	.000184	−.0001	−.000123	.000121	.000197
	(.000168)	(−.000181)	(.000248)	(.000255)	(.00027)	(.000312)	(.00025)	(.000267)	(.000158)	(.000166)
Preschool exposure	. . .	−.000079	. . .	.000211	. . .	−.000092	. . .	.000106	. . .	−.000064
	. . .	(.00016)	. . .	(.000205)	. . .	(.000254)	. . .	(.000241)	. . .	(.000214)
N	40,246	40,246	22,171	22,171	18,075	18,075	21,894	21,894	18,352	18,352
Birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Including violence years before birth	No	Yes	No	No	No	No	No	No	No	No

Note. Standard errors clustered by district of birth in parentheses. In addition to district fixed effects, birth-year fixed effects, and district-specific linear trends, regressions include gender, schooling, marital status, rural/urban indicator, and ethnicity controls. The estimation includes individuals aged 14–34 in 2006–7 Encuesta Nacional de Hogares data. The intensity of violence is measured as the number of violent acts in one's birth district. Violence in individual's birth district is instrumented with violence shocks measured at the closest three districts to respondent's birthplace.

* Significant at 10%.

the inclusion of schooling, marital status, and urban/rural control variables soaks up part of the civil war impacts on adult earnings because these variables may be intermediate channels. The inclusion of these control variables has marginal impacts in the point estimates of the Mincerian wage regressions. One additional analysis that we were not able to implement due to lack of information is whether time-variant changes in districts might be the ultimate causal factor behind both civil war violence and later life outcomes. Including lagged variables for district economic output as control variables would have added internal validity to the results.[11]

D. Other Labor-Market Outcomes

While this study mostly focuses on labor-market earnings, this section extends the analysis to two additional labor-market outcomes: employment and job quality characteristics. Civil war might affect the employability of individuals in the long run due to physical or mental disability. If this is the case, the impacts we are observing for the earnings variable might be underestimated since those who were more affected by violence may be out of the labor force. Thus, "selection" into the labor force might be an issue in these data. We therefore assess the effects of violence exposure on the probability of employment. Table 5 reports the IV-FE point estimates.[12] Results show zero impacts for the employment variable across all subsamples including women. The zero impact of civil war violence on employment is somewhat expected because in settings in which unemployment insurance systems are nonexistent, people have to work (or invent their own job) against great odds to survive. This result is in line with Humphreys and Weinstein's (2007) findings for Sierra Leone.

A more relevant variable to consider is the quality of employment, as there is a qualitative large difference between "having a job" and "having a quality job" in development settings (Fields 2001). Two widely used indicators of job quality are considered: formal jobs and firm size. The former refers to

district-level information on population over time is not available. Second, the number of districts considered in the 1981 Peruvian census is smaller with respect to the number of districts included in the ENAHO 2006–7 data. Thus, there is a difference in the number of observations considered in tables 2 (40,246) and A5 (35,993; available in the online appendix).

[11] Additional unreported sensitivity analyses exclude from the regressions individuals aged 14–16 because a large portion of individuals of this age would be enrolled at school, a situation that would originate potential selection issues. The point estimates change little, and the qualitative results hold. In addition, we include in the regressions the sample of individuals who were born between 1970 and 1974 and thus were not exposed to the armed violence as children (because they were already older than 7 when the war started), as they provide additional exposure variation. Once again, all qualitative results hold.

[12] The corresponding OLS-FE estimates are reported in the online appendix.

TABLE 5

IV-FE ESTIMATES OF THE IMPACTS OF EARLY LIFE EXPOSURE TO CIVIL WAR ON LABOR-MARKET OUTCOMES

	Full Sample	Men	Women	Urban	Rural
Employment	−.00029	−.00014	−.00035	−.00043	−.00003
	(.00020)	(.00022)	(.00027)	(.00031)	(.00020)
Formal employment	−.00032**	−.00033*	−.0002	−.00048*	−.0001
	(.00016)	(.00018)	(.00018)	(.00028)	(.00011)
Large firm	−.00028**	−.00019	−.00027	−.00047*	−.00007
	(.00014)	(.0002)	(.00019)	(.00028)	(.00012)

Note. Standard errors clustered by district of birth in parentheses. In addition to district fixed effects, birth-year fixed effects, and district-specific linear trends, regressions include gender, schooling, marital status, rural/urban indicator, and ethnicity controls. Estimation is based on individuals aged 14–34 in the 2006–7 Encuesta Nacional de Hogares data. The intensity of violence is measured as the number of violent acts for one's birth district in each period of analysis. Violence in individual's birth district is instrumented with the violent acts measured at the closest three districts to respondent's birthplace. Formal employment takes the value one if an individual works in formally registered firms or works with health insurance coverage or works in the public sector, zero otherwise. Large firm takes the value one for firms with 20+ workers, zero otherwise. $N = 40{,}246$.

* Significant at 10%.

** Significant at 5%.

jobs in which individuals work in formally registered firms, work with health insurance coverage, or work in the public sector. The latter defines large firms as those with 20 or more workers. Table 5 shows that individuals exposed to civil war violence early in life have lower probabilities of working in both formal jobs and large firms, after controlling for birth district, birth year, and district-specific linear time trends. For instance, the IV-FE results shows that a 1 standard deviation increase in civil war violence leads to a 0.89 percentage point reduction in the probability of working in large firms (equivalent to a 6% reduction) and a 1.02 percentage point reduction in the probability of working in formal jobs (equivalent to a 3.5% reduction). Overall, one observes the correct sign across all subsamples, although the statistical significance varies across them, particularly when analyzing individual subsamples.

V. Types of Civil War Violence

There is an analytical distinction between civil wars 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, victims of forced disappearances 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. This might have stronger psychological and mental impacts than the number of deaths in combat, as it prevents relatives of the persons who disappeared from moving on with their lives (TRC 2003). Likewise, sexual violation is a gender-based weapon of war exerted particularly on women in countries torn apart by civil wars. It can produce the highest levels of stress, fear, and distrust in women, with long-term consequences (Swiss and Gilles 1993). Thus, focusing only on the most common source of violence (i.e., deaths) might underestimate the overall impact of civil wars, as exposure to more extreme violence might have stronger human capital impacts later in life.

In this section, we focus on five different types of violence: forced detentions and torture, forced disappearances, sexual violations, abductions, and killings. Figure 2 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% of all violent acts reported by the TRC. They are followed by forced disappearances (11%) and abductions (12%). Sexual violations, however, are the least common type of violence in Peru (less than 2%), which may suggest 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: 1983–84, 1989–90, and 1992–93. For instance, the number of killings in 1983 shows a sixfold 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, 1 year after the capture of Shining Path's leader Abimael Guzman.[13]

To estimate the heterogeneous impacts of civil war violence, we follow the same reduced-form approach implemented in Section IV. All econometric details hold, with the exception of defining violence measures according to each particular type of violence. We acknowledge that this analysis imposes some limitations in the data, as some types of violence are more public, the knowledge about them more widespread, and their measure might be more adequately recorded, which may cause noisiness in the least common types of violence (e.g., sexual violations and abductions). Table 6 reports the IV-FE estimates, while the corresponding OLS-FE estimates are reported in the online appendix. Each column shows the results for each type of violence estimated separately after including violence measures for all three periods: fetal, early childhood, and preschool.

The top of table 6 depicts results consistent with the main finding in the previous section: the most sensitive period of early life exposure to civil war is

[13] Figure 2 suggests also a strong correlation between different types of violence exposure. By estimating district/year bivariate correlations between pairs of violence measures, one observes positive correlation across time and space for all measures of civil war violence. The magnitude ranges from 0.25 to 0.62, suggesting a moderate to strong correlation. The highest correlation is observed for torture and forced disappearances (0.62) and for killings and abductions (0.62). Correlations involving sexual violations, however, show the lowest coefficients.

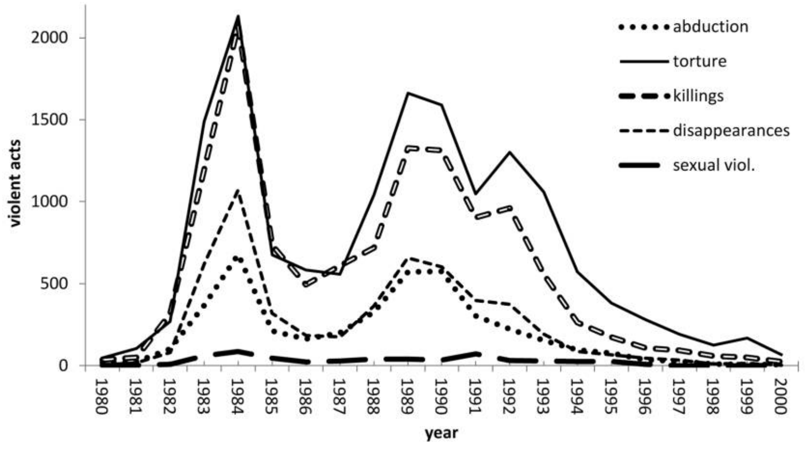

Figure 2. Types of violence by year: Peru, 1980–2000. Source: Peruvian Truth and Reconciliation Commission data set.

the first 36 months of life. Overall, forced disappearances and torture show the strongest negative impacts on earnings in the long run. A 1 standard deviation increase in exposure to forced disappearances (torture) in early childhood leads to a reduction of 5.1% (6.4%) of monthly earnings later in life. Surprisingly, killings have no significant affect on long-run earnings, as the magnitude of the impacts is less than half of that for torture or forced disappearances.[14]

The bottom of table 6 reports the point estimates for the first 36 months of life across different subsamples. One observes that diverse types of violence shape the fate of men and women differently. Sexual violations disproportionately affect women, while torture and forced disappearances disproportionately affect men. On average, a 1 standard deviation increase in sexual violations is associated with a reduction of over 9% of monthly earnings for women, while exposure to forced disappearances leads to a 3% reduction of monthly earnings for men. Similarly, one observes strong heterogeneous impacts between urban and rural areas. Within urban areas, forced disappearances (−4.5%) and torture (−5.6%) are the types of violence with the largest statistically significant negative impacts on monthly earnings, while in rural areas only forced disappearances show statistically significant effects.

Setting aside the magnitude of the estimates, some lessons emerge when comparing the IV-FE results with the OLS-FE results reported in the online

[14] The standard deviations for types 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.

TABLE 6
IV-FE ESTIMATES OF THE IMPACTS OF TYPES OF VIOLENCE ON LONG-RUN EARNINGS

	Sexual Violation	Disappearance	Kidnapping	Killing	Torture
Full sample:					
Fetal exposure					
[−1, 0]	−17.092	.089	−3.057	−1.095	−.974
	(12.908)	(.790)	(24.868)	(.830)	(1.518)
Early childhood					
exposure [0–3]	−27.035	−1.983	−.706	.720	−5.882
	(16.215)*	(.572)***	(2.779)	(.991)	(1.835)***
Preschool exposure					
[4–6]	−4.376	−.730	−19.973	−.455	−4.178
	(11.430)	(.642)	(7.053)***	(1.432)	(1.034)***
N	40,246				
R^2	.33				
Early childhood violence across subsamples:					
Men:	−25.739	−1.475	10.686	1.711	−7.087
	(24.142)	(.778)*	(5.674)*	(2.743)	(2.724)***
N	22,171				
R^2	.36				
Women:	−39.592	−2.637	−5.064	1.601	−4.519
	(19.999)*	(.581)***	(3.520)	(.817)*	(1.923)**
N	18,075				
R^2	.33				
Urban:	−54.970	−2.540	10.774	1.343	−7.458
	(33.488)	(.687)***	(6.928)	(4.989)	(1.827)***
N	21,894				
R^2	.29				
Rural:	−5.659	−.954	−1.535	−.835	−2.428
	(14.549)	(.506)*	(2.345)	(.563)	(1.601)
N	18,352				
R^2	.36				

Note. Standard errors clustered by district of birth in parentheses. In addition to district fixed effects, birth-year fixed effects, and district-specific linear trends, regressions include gender, schooling, marital status, rural/urban indicator, and ethnicity. Estimation is based on individuals aged 14–34 in the 2006–7 Encuesta Nacional de Hogares data. The intensity of violence is measured as the number of violent acts for one's birth district in each period of analysis and type of violence. Violence in individual's birth district is instrumented with violent acts measured at the closest three districts to respondent's birthplace.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

appendix. First, the significant negative impacts associated with early childhood exposure to civil war are not driven by measurement error, or by a particular type of violence, as the findings are robust through different methods and violence measures. Unlike the standard OLS-FE estimates, however, the correction for measurement error causes more variability in the estimates, particularly for the least common types of violence (i.e., sexual violations and abductions), which, in turn, reduces the statistical significance of some point estimates. We observe high instability of the point estimates for these two types of civil war violence, particularly after splitting the sample for gender and geography.

Second, there is substantial heterogeneity in the computation of earnings impacts, depending on the type of civil war violence experienced in early life. Overall, forced disappearances appear to be the most hurtful type of violence in the long run. This result is consistent with medical evidence that shows that children of those who disappeared and of those who were executed show different symptomatologies, as the former suffer from ongoing fear that translates into developmental disorders and behavioral alterations (Ugalde, Zwi, and Richards 1999). Currently, Peru is one of the countries with the highest number of forced disappearances in the world (United Nations 2002), where 65% of the people who disappeared are still missing (TRC 2003). Third, the disproportionately negative impacts on individuals living in urban districts, relative to rural ones, are consistent across most types of violence, which suggests that this finding is not forced by a specific type of violence that could have predominated in urban areas. It is possible that children who were exposed to extreme violence in rural areas migrated later to urban cities. They may have "imported" long-lasting physiological and structural effects, which, along with their own displacement difficulties, may have worsened their human capital endowment over time.

VI. 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. Since this study focuses on the effects of early life exposure to civil war before schooling plays any role, children's health is arguably the initial short-run channel due to adverse environmental factors. Children's health, seen through the scope of standard human capital models, can affect in turn school achievement and adult health, which along with adverse shocks in household wealth may affect adults' earnings power in the long run.

A. Short-Run Health Impacts

It is well established in the literature that exposure to material deprivation and stressful environments early in life affects the growth and cognitive development of children (e.g., Resnik 2002; Aizer, Stroud, and Buka 2009). In this regard, height is a well-established marker that reflects early life adverse conditions (e.g., Strauss and Thomas 1998; Maccini and Yang 2009). In particular, the period from birth to age 3 is identified as the most critical to adult height, as the speed of growth reaches its peak and nutritional needs are greatest at this point (Martorell, Khan, and Schroeder 1994; Martorell et al. 2005).

As the correlation between childhood height and adult height is high, taller children become taller adults. Once in the labor market, taller workers earn more, on average, than other workers (Steckel 1995; Case and Paxson 2006). This is explained by the positive relationship between height and the development of cognitive ability throughout childhood that is even present before any potential differential treatment of taller children in school (Case and Paxson 2008).

Microdata evidence on civil wars has uncovered a negative relationship between exposure to armed conflict and children's height (Alderman and Behrman 2006; Akresh et al. 2009). In this regard, a very recent working paper by Grimard and Laszlo (2010) provides evidence on the long-run effects of Peru's armed conflict on height and nutrition for women aged 15–49. After matching the 2004–8 DHS with the TRC violence data, the authors found that exposure to armed conflict at birth has negative long-run effects on height, while exposure to violence in early childhood has negative long-run impacts on anemia. A caveat with the DHS data is that they do not contain the location codes for birth district, and thus the authors proxy that with the current district of residence after restricting the sample to nonmigrants.

In this section we present analysis complementary to but different from Grimard and Laszlo (2010). First, we focus on the short-run impacts of civil war on children's height by using the 1993 Peruvian school census on height for children aged 6 and 7, data that include individuals born in 1986 and 1987 and who are enrolled in primary school.[15] Unlike adult height that also depends on the timing and duration of the adolescent growth spurt (Beard and Blaser 2002), short-run impacts are not affected by the potential impact of the household socioeconomic environment during puberty (the health selection hypothesis). Second, the school census data include information on birth districts for both boys and girls. 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., 1985–86). However, as infants aged 1–6, they experienced one of the most brutal periods of the civil war (i.e., 1987–93). Third, we are able to uncover evidence on the heterogeneity of the impacts by types of civil war violence.

Table 7 presents the IV-FE short-run impacts of civil war violence on height. For the interpretation of the results, this study focuses on the impact of 1 stan-

[15] These data were collected by Peru's Ministry of Education. They also include information for children aged 8 and 9, but they are severely underrepresented. While the number of children aged 6–7 is about 500,000, the number of children age 9 was fewer than 37,000. For this reason, we do not include this particular cohort in the estimation.

dard deviation in the violence measures for this particular cohort.[16] The top of table 7 shows, once again, that early childhood is the period most sensitive to violence. A 1 standard deviation increase in armed violence is significantly associated with 1.2 centimeters lower height for the full sample (col. 1). This result is statistically significant at the 1% level. Moreover, by looking at the preschool period, one can also observe negative but mainly not statistically significant impacts. Fetal exposure, however, is not significantly related to short-run height, as the point estimates are positive and imprecisely measured.

Furthermore, the height of boys and girls is significantly affected in the short run, with the magnitude of the point estimates almost the same for both groups (table 7, cols. 2 and 3). Similarly, while the height of urban and rural children is also negatively affected in the short run (cols. 4 and 5), the height of children living in urban districts is disproportionately affected, relative to rural children. The magnitude of this difference reaches 0.66 centimeters. These results are statistically significant at the 1% level.

The bottom of table 7 shows the IV-FE early childhood estimates by type of violence. By looking at the full sample in column 1, one observes that forced disappearances, sexual violation, and torture emerge again as the most hurtful types of violence, yielding 0.66, 0.81, and 0.99 centimeters lower height, respectively.[17] In this regard, the similarity between the qualitative impacts of these types of violence on adult earnings and on children's height is very telling. No significant differences are found, however, when comparing the impacts between girls and boys or between urban and rural areas. Overall, the magnitudes of these point estimates are comparable (or somewhat smaller) with respect to related research on the effects of civil war on height in Rwanda (e.g., Akresh and de Walque 2008) and the effects of early life rainfall on height in Indonesia (Maccini and Yang 2009).

B. Intermediate Outcomes

Results from the previous section 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 affect children's cognitive and noncognitive skill development (Cunha and Heckman 2007; Case and Paxson 2010). It is well established in the literature that there is a correlation between

[16] The standard deviations for this cohort reach 4, 19, and 27 for fetal, early childhood, and preschool violence measures, respectively.

[17] The standard deviations for violence measures for this particular cohort are 0.36, 3.67, 0.75, 1.12, and 2.25 for sexual violations, forced disappearances, abductions, killings, and torture, respectively.

TABLE 7

IV-FE IMPACTS OF CIVIL WAR VIOLENCE ON HEIGHT FOR CHILDREN AGED 6–7 IN 1993

	Full Sample	Men	Women	Urban	Rural
Estimates:					
Fetal exposure [−1, 0]	.040	.048	.034	.028	.036
	(.040)	(.037)	(.046)	(.075)	(.037)
Early childhood exposure [0–3]	−.064	−.065	−.063	−.083	−.048
	(.009)***	(.009)***	(.009)***	(.015)***	(.017)***
Preschool exposure [4–6]	−.014	−.015	−.014	−.073	.022
	(.017)	(.017)	(.017)	(.030)**	(.019)
N	471,813	241,123	230,690	277,047	194,766
R^2	.71	.72	.69	.63	.77
Early childhood estimates by type of violence:					
Sexual violation	−2.251	−2.564	−2.026	−2.851	−1.961
	(.984)**	(1.174)**	(.934)**	(1.908)	(.782)**
Killing	−.082	−.830	.688	−6.608	.177
	(2.971)	(3.063)	(3.071)	(12.441)	(2.152)
Abduction	−.044	−.018	−.066	−.016	.027
	(.067)	(.122)	(.080)	(.104)	(.108)
Disappearance	−.181	−.183	−.178	−.247	−.118
	(.033)***	(.031)***	(.036)***	(.050)***	(.025)***
Torture	−.441	−.320	−.564	−.211	−.329
	(.174)**	(.172)**	(.193)***	(−.290)	(.160)**
N	471,813	241,123	230,690	277,047	194,766
R^2	.71	.72	.69	.63	.78

Note. Standard errors clustered by district of birth in parentheses. In addition to district fixed effects and birth-year fixed effects, regressions include dummy variables for nutrition levels, gender, and rural/urban areas. Estimation is based on individuals aged 6–7 in 1993. The height data come from the 1993 Peruvian School Census on Height conducted by the Ministry of Education. Height is measured in centimeters. Violence in individual's birth district is instrumented with violent acts measured at the closest three districts to respondent's birthplace.

** Significant at 5%.

*** Significant at 1%.

children's health and schooling—the latter, a major determinant of adult earnings. For instance, improved health status among children contributes to school enrollment (Alderman et al. 2001) and improves school performance (Glewwe and King 2001). Therefore, schooling constitutes a second subsequent intervening channel between civil war early in life and earnings later in life.

In this regard, Leon (2010) provides evidence that children aged 6–17 who were exposed to the Peruvian civil war accumulate more years of school deficit as children and fewer years of schooling as adults. The short-run impacts for early childhood exposure to civil war reach 0.78 years of school deficit. In the long run, though, the magnitude of the impacts fades out to 0.21

fewer years of schooling, suggesting that children were able to catch up over time.[18]

To analyze the role of both measurement error and type of violence, we complement this analysis by estimating the IV-FE model of early childhood exposure to civil war on years of schooling separately for each type of violence for the full sample. We restrict the sample to individuals aged 17 or older in 2006–7 and, therefore, old enough to have completed high school. Column 1 in table 8 shows that the point estimates have the expected sign but are statistically not significant. Killing is the only type of violence exposure that shows statistically significant results. A 1 standard deviation increase in early childhood exposure to killings leads to a reduction of 0.10 years of schooling in the long run. This result is consistent with Leon's (2010) study that uses killings and forced disappearances as the measure for civil war in Peru.

Because the civil war intensified poverty and inequality, families and communities lost property, and military spending crowded out necessary public investment in education, it is plausible that the quality of schooling received rather than the years of schooling was most affected by civil war violence. Although we cannot test directly the "quality" channel, which constitutes a topic of interest for future research, we can indirectly address this topic by examining the long-run consequences of civil war on household wealth. In Peru, as in many developing countries with dysfunctional public school systems, wealth is the most important determinant of the quality of schooling received (Valdivia 1997).

We constructed a wealth index that uses, as inputs, household assets including characteristics of the household's dwelling collected in the ENAHO data set. By aggregating over 10 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). Column 2 in table 8 shows the IV-FE estimates for the long-run wealth impacts. The magnitude of the estimated coefficient is small but statistically significant: a 1 standard deviation increase in early childhood exposure to civil war is associated with a 0.07 lower asset index later in life for women and urban people. Consistent with our main findings, forced disappearances (0.035 lower asset index) and torture (0.049 lower asset index) yield the largest impacts on household wealth.

[18] Leon's findings were based on the combination of the 1993 census demographic data with the same violence data from the TRC. The Peruvian census data are, however, not publicly available.

TABLE 8
IV-FE ESTIMATES OF THE IMPACTS OF EARLY CHILDHOOD VIOLENCE ON INTERMEDIATE OUTCOMES: FULL SAMPLE

	Schooling (Aged 17–34)	Household Wealth	Work-Loss Days due to Health Condition	Chronic Health Condition	Marital Status
Early childhood violence	−.0014	−.0015	.0001	−.0001	.0000
	(.0015)	(.0004)***	(.0004)	(.00010)	(.0001)
By type of violence:					
Sexual violation	−.1364	−.1534	.0812	.0136	−.0151
	(.1752)	(.0635)**	(.1114)	(.0233)	(.0266)
Disappearance	.0000	−.0059	−.0004	−.000375	.0008
	(.0067)	(.0015)***	(.0016)	(.0003)	(.0007)
Abduction	.4560	.0111	−.0101	−.003784	−.0139
	(.6523)	−.011521	(.0067)	(.0069)	(.0085)
Killing	−.0268	.0030	−.0025	.000033	.0058
	(.0138)*	(.0028)	(.0029)	(.0005)	(.0021)***
Torture	−.0056	−.0195	.0012	−.001766	.0011
	(.0205)	(.0078)**	(.0064)	(.0018)	(.0022)
N	33,524	39,519	40,247	40,247	40,247
R^2	.46	.6200	.09	.11	.37

Note. Standard errors clustered by district of birth in parentheses. In addition to district fixed effects, birth-year fixed effects, and district-specific linear trends, regressions include gender, schooling, marital status, rural/urban indicator, and ethnicity. The estimates for schooling are based on individuals aged 17–34, while the estimates for the rest of the intermediate outcomes are based on individuals aged 14–34. Wealth index is the first principal component of several households assets including access to safe water, flush toilet, characteristics of the dwelling, computer, telephone, cell phone, Internet, TV, and per capita household income. The intensity of violence is measured as the number of violent acts in one's birth district. Violence in an individual's birth district is instrumented with violence shocks measured at the closest three districts to respondent's birthplace.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

Columns 3–5 in table 8 report other potential intermediate outcomes, including adult health and marital status. Relevant adult health outcomes such as height, mental health, body impairment, and self-reported good/bad health status, among others, are not collected in the ENAHO survey. There is some proxy information regarding adult health, though. Variables include information on the number of work-loss days due to health conditions in the last month, whether the person has a chronic health condition (asthma, tuberculosis, AIDS, cholesterol, diabetes), whether the person has felt sick in the previous month (flu, colitis), and whether the person has experienced some symptoms in the previous month (cough, fever, headache, diarrhea). We decided to focus on the first two health variables since the last two capture, in most cases, information on seasonal, random health conditions. Results for work-loss days due to a health condition (col. 3) show the correct sign, although the point estimates are imprecisely measured. The coefficients for a chronic health condition (col. 4) are also imprecisely measured, as they are not statistically significantly different from zero. Likewise, we find no significant impacts for marital status, except for one particular type of violence: killings. In this case, one can observe a positive and statistically significant impact.

All told, these results provide suggestive evidence that children's health, schooling, and household wealth are intervening pathways between early childhood exposure to civil war violence and adult earnings. In particular, the short-run negative impact on children's height is very telling. However, evidence for an intermediate role of adult health is much weaker.

A complementary analysis that would gauge the relative importance of a specific intermediate outcome variable considers regressing labor-market earnings on early childhood exposure to civil war and then adding adult health, schooling, and wealth index variables successively. The idea is to test whether the inclusion of a specific intermediate variable X leads to both declines in the coefficient of civil war and increases in the R^2. If that happens, it would suggest that variable X might be an important pathway toward labor-market earnings. Unreported results show that the point estimates for early life exposure to civil war are statistically significant at the 1% level after including adult health variables, schooling, and a household wealth index. However, the point estimates do not fall, and the R^2 does not increase abruptly after including any particular control variable. When comparing the relative impact of the intermediate variables, one observes that the highest decline in the coefficient for civil war violence is observed after including a household wealth index.

VII. Conclusion

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 1 standard deviation increase in violence leads to a 5% decrease in adult earnings, 6% decrease in the probability of working in large firms, and 3.5% decrease in the probability of working in formal jobs. This negative effect has disproportionately affected women and individuals living in urban areas and is robust to a variety of sensitivity checks and econometric details. However, we did not find any significant effect of either fetal or preschool exposure to armed violence on adult earnings. Of particular importance is the difference in the magnitude of the estimates between the standard linear and IV approach models, which may suggest attenuation bias in studies that do not address issues of measurement error in violence data sets.

The analytical distinction between civil war and violence in civil wars (Kalyvas 2006), largely overlooked in the microdata analysis of civil wars, proves to be useful in uncovering substantial heterogeneity of civil war impacts on civilians' outcomes. Forced disappearances yield the strongest negative impacts on earnings later in life; sexual violations disproportionately affect the wages of women, while torture and forced disappearances disproportionately affect the wages of men. Finally, the wages of individuals living in urban districts were more affected than were those of people in rural areas, with torture and forced disappearances as the most sensitive types of violence for urban people. These results are consistent with the qualitative evidence collected by the TRC from thousands of oral testimonies that illustrates how these different types of violence have shaped individuals' lives (TRC 2003). These results point out implications for postconflict policy responses. Particular attention should be given to those individuals who were exposed to early life episodes of forced disappearances, torture, and sexual violations.

The analysis of pathways connecting adult earnings and early life exposure to violence suggests that a plausible explanation lies in infant health and short-run school deficits, due to long-lasting physiological and structural effects, which along with negative impacts in household wealth have the power to affect adult earnings power. For instance, it is quite possible that stunted children dropped out from the school system in critical years, which in turn might have affected noncognitive skills that have substantial returns in the labor market. It is not trivial that the TRC placed postconflict health-related problems on Peru's national agenda.

These findings imply that, from a policy standpoint, it is particularly useful to focus on children aged 0–3 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 postconflict policy responses, including the targeting of individuals or groups that were disproportionately affected by an armed conflict.

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