Conflict, Educational Attainment, and Structural Transformation: La Violencia in Colombia

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I. Introduction

Violent civil conflict is prevalent and widespread. Blattman and Miguel (2010) report that over the past 50 years, one-third of all countries have experienced civil war (violent conflicts with at least 1,000 battle-related deaths per year), and nearly one-half have been involved in internal conflict (with at least 25 annual deaths). Some negative effects of these conflicts, such as the destruction of physical assets, are relatively visible and can be countered with large financial investments. Other nonphysical costs of violent conflict, however, are less apparent, and it is harder to recover from them, as they involve changes in household decisions and opportunities that may reduce future long-run income. Within this set of effects, lower human capital accumulation stands out as one of the main potential long-term consequences of exposure to conflict.

In this paper, we estimate the effects of violence on human capital accumulation (measured as educational attainment) and explore the possible causal mechanisms. We exploit within-country variation using data from Colombia's bipartisan political conflict in the mid-twentieth century known as La Violencia.

In addition, we also trace implications for the labor market. First, we find that sectors that tend to have very educated workers are smaller (workers participate less in these sectors) in affected areas. Second, workers are less likely to participate in manufacturing and services in violent areas relative to agriculture.

Focusing on nonmigrants, our empirical strategy compares the difference in the education of individuals who were young enough to be in school (or deciding whether to go to school) during La Violencia with that of older individuals from the same municipality. Next, we compare the size of this gap or

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trend in human capital accumulation in violent (or treated) versus peaceful (or control) municipalities.

Our main result is that individuals exposed to violence, especially the younger cohorts, have a lower educational attainment. For prebirth/in utero individuals (those 1–2 years before birth in 1948), early childhood (0–3 years old in 1948), and preschool (4–6 years old in 1948), we find a reduction of 0.2–0.3 years of education. Effects on the primary school (7–12 years old in 1948) and high school (13–17 years old in 1948) cohorts are also present, but the absolute decreases in years of schooling are roughly one-third and one-fifth as large, respectively. We find no differences in attainment for cohorts older than 17 in 1948, reflecting common trends in educational attainment for cohorts too old to have changed their schooling decisions because of La Violencia, which helps validate our approach. While not comparable to the largest effects found in previous work, the impact we find is economically meaningful, especially taking into account the coarse measure of violence we have available and the long-run nature of the impact we study.[1]

As noted, these results refer to nonmigrants. We find no effects for migrants. This may partly be due to measurement error in exposure to violence, since we do not know whether they moved before violence broke out. But since we observe a similar noneffect for even the very young migrant cohorts that could not have moved before being affected, we conclude that, on average, migration helped attenuate the impact of conflict for these households. While this unveils a positive side of migration, it also underscores the inequities that violence brings about: only households fortunate enough to migrate were able to avoid the costs of violence.

To explore mechanisms, we present a set of complementary outcomes. While these produce indirect and therefore tentative evidence on the causal channels, they are consistent with the importance of the demand side in explaining the effects. Heterogeneous effects are concentrated on boys: we find

[1] Comparing the size of these effects with those in the existing literature is not straightforward, given the different measures of violence used (e.g., simple indicator variables for exposure vs. intensity measures) and the various outcomes measured (e.g., years of schooling, probability of grade completion). Looking at studies that use approaches more comparable to ours suggests a wide range of effects, depending on context and the affected population (boys vs. girls, and younger/in utero vs. older kids). Akresh and de Walque (2008) find an average impact of 0.5 fewer years of schooling due to conflict exposure in Rwanda, an 18.3% drop relative to the mean. Rodríguez and Sánchez (2012) report decreases in contemporary Colombia of 10%–20% relative to the mean. The estimates of Chamarbagwala and Morán (2011) are very sensitive to the group affected, ranging from 6% to 30%. Akbulut-Yuksel (2014) finds decreases of 0.4–1.2 years of education in a population with an average of 11 years of schooling. Estimates in León (2012) are close to 3%, with 0.3 fewer years of schooling in a sample that had around 9 years of schooling on average.

no effects on the probability of having no schooling, and we find no effects on entry into the teaching profession.

Another important (and, to the best of our knowledge, novel) aspect of our analysis is that we also show that this impact had long-term consequences that extended beyond human capital accumulation. In particular, we focus on the working population in 1973 and verify the sector in which each individual is employed. We use the average educational levels of employees in the sector as a measure of how human capital intensive it is. Using this measure as our dependent variable, we show that sectors that tend to hire more qualified workers shrink (in terms of employment) in affected areas. Again, the effects are concentrated among the relatively younger cohorts, and the size of the effects is economically meaningful.[2] We also examine the implications for the transition from agriculture to manufacturing and to services, and we find that violence reduces the likelihood of transitioning. One caveat that applies to this analysis is that unlike with education (which should not be altered for individuals who were sufficiently old to have completed their formative years when the violence broke out), employment could respond for all cohorts. In other words, older cohorts are not pure controls: their employment can respond since violence can influence local economies. Younger cohorts are affected by this effect plus any impact that education has on their job prospects.

A number of papers have also exploited within-country variation (and variation in birth cohorts, in some cases) to study the consequences of violence. Our main contribution to the literature is in two key dimensions. First, we explore the longer-term effects of internal conflict on education. The bulk of the literature on the human capital effects of conflict has focused on the implications on health (Bundervoet, Verwimp, and Akresh 2009; Agüero and Deolailkar 2012). While some examine the effects of violence on education, they typically focus on the short-run effects.[3] León (2012), who studies the Peruvian civil war, is one notable exception looking at a comparatively longer horizon (14 years), but we go beyond this and examine impacts over three decades later. Also, while Ichino and Winter-Ebmer (2004) look at long-term impacts, their

[2] Cohorts in the early childhood stage take the largest hit in terms of the human capital level of the jobs they are able to secure, with 0.31 fewer years of education as a result of violence. This is 5.5% of the human capital content of the average job for individuals in this cohort.

[3] In the Colombian case, Duque (2014) also looks at education effects, but she focuses on the increases in general violence (as captured by the homicide rate) starting in the 1980s with the intensification of guerrilla warfare. Duque also presents a useful review and summary table of some of the recent papers examining the impact of war or conflict on human capital, including years of schooling (Akresh and de Walque 2008; Chamarbagwala and Morán 2011; Shemyakina 2011; Rodríguez and Sánchez 2012; Akbulut-Yuksel 2014), school enrollment (Swee 2015), and grade failure (Caudillo and Torche 2014).

focus is on an international war of epic proportions.[4] Second, we contribute by exploring the implications on the labor market and the likely role of education in explaining those effects. Here the evidence is limited and also often focused on the short run, as in, for example, Kondylis (2010), Bozzoli, Brück, and Wald (2013), and Calderón-Mejía and Ibáñez (2015). We are not aware of other papers that explore the implications for transitions into more modern sectors of production, with likely consequences for the process of structural transformation.

We also contribute by examining the impact of violence on the first years of life, where the literature is thinner even though there is mounting evidence of the importance of early-life events and conditions for long-run results (in particular, health; see Currie and Vogl 2013). Finally, to our knowledge, this is the first paper to examine the effects of La Violencia in Colombia on human capital accumulation, using an empirical strategy that aims to overcome the usual identification problems.

Our results are important to understand the implications of conflict beyond the immediate economic effects. The implications on schooling for affected cohorts are of course irreversible, and we suggest that they matter not just because they affect the welfare of affected families but also because they slow down the entire process of sectoral recomposition. This underscores a possible persistent impact on aggregate development in Colombia and on regional inequality. By reducing worker participation in sectors often associated with the structural transformation that occurs as income grows (Herrendorf, Rogerson, and Valentinyi 2014), violence might have influenced Colombia's development process in affected regions.[5]

Our results on the likely mechanisms—albeit more suggestive, given the limitations of the historical data—are also important from a policy perspective, as they suggest that in order to attenuate long-term effects, policy makers should focus their attention on helping families maintain the conditions that will let them persist in their educational investments and not just on rebuilding infrastructure.

[4] They exploit variation across countries by looking at school age individuals in countries affected by World War II (Austria and Germany) vs. those in nonwar countries (Sweden and Switzerland), finding sizable effects on education and earnings.

[5] This impact on local economies does not imply that structural transformation did not occur. For instance, these sectors might have flourished even in these regions by substituting capital for labor. However, it seems very unlikely that substitution of capital for labor could have made up for these obstacles in Colombia, particularly in the context of very thin financial markets (see, e.g., Villar, Salamanca, and Murcia 2005). Unfortunately, given data limitations, we cannot trace the patterns of capital accumulation, industrial composition, or economic growth of the regions affected by violence (to contrast with the trends of peaceful areas) to more rigorously examine the general equilibrium effects.

The rest of the paper is organized as follows. Section II briefly discusses the main possible channels of influence of violence on education and their relevance in the particular historical context. Section III presents our empirical approach and data sources. Section IV presents our main results and robustness checks (additional robustness exercises are relegated to the appendixes, available online). Finally, section V explores the possible causal mechanisms underlying the main results. Section VI concludes.

II. La Violencia and Its Likely Impact on Education

There are various reasons why conflict may influence human capital accumulation. Violence can have several direct effects on the supply of education. The disorder, chaos, and diversion of resources (e.g., to devote them to military expenditure) can reduce public goods provision, including schooling. If teachers are targeted with violence, this can also reduce effective supply. Finally, even when educational inputs (physical or human resources) are not directly affected, complementary inputs may be destroyed.

The demand-side potential effects are also numerous. Education is a long-term investment, and increased uncertainty and lower life expectancy may reduce households' incentives to make it. Also, if violence destroys wealth and directly reduces income, households may be forced to withdraw children from school so they can work. Moreover, individuals (especially boys) may abandon school to engage directly in the conflict. Violence also brings insecurity, especially in rural areas, creating an additional obstacle to go to school. It also produces distress that affects parenting, influencing children's development and causing stress that may affect fetal and children development. These disadvantages may be reflected in lower student achievement and retention.

The historical literature suggests that La Violencia affected human capital accumulation through various of these mechanisms.[6] On the supply side, violence reduced the provision of education by destroying schools, killing teachers, and causing the total collapse of the state in certain regions (Guzmán, Fals-Borda, and Umaña 1964; Canal 1966; Oquist 1980).

On the demand side, children dropped out of school for several reasons. Some, particularly boys, abandoned school to work after the family means of support were destroyed or their fathers died. Reports from a school that was heavily affected by La Violencia estimated that 30% of the children had lost their fathers (Canal 1966). Other children dropped out of school because of fear of being victimized (Canal 1966). Narratives about children abandoning

[6] Appendix A presents some basic context on this period. For key references on additional aspects of La Violencia in Colombia, see, e.g., Bushnell (1993, chap. 9), Safford and Palacios (2002, chap. 14), Chacón (2004), and Chacón, Robinson, and Torvik (2011).

school and joining armed groups were widespread (Guzmán, Fals-Borda, and Umaña 1964; Canal 1966; Sánchez and Meertens 1983). Many adolescent victims of violence joined local bandit groups (*bandoleros*) after 1958 and claimed their desire for revenge (Sánchez and Meertens 1983). Adolescents also became combatants after active recruitment from armed groups (Guzmán, Fals-Borda, and Umaña 1964). Also, given the nature of violence, the psychological consequences of victimization could also influence education. Fear and posttraumatic stress led children and their families to retreat into their private lives and could have also affected children development (Lipman and Havens 1965; Sánchez and Meertens 1983).

In short, the available historical record suggests that violence could have had an important effect on human capital accumulation both by reducing the demand for education and by curtailing the supply. It also highlights that the impact was likely different for the migrant and nonmigrant populations. We provide systematic evidence of the quantitative importance of these alleged effects.

III. Empirical Approach

A. Data and Key Variables

Our main dependent variables come from Colombia's 1973 national census. First, we consider years of schooling as of 1973. We observe years of schooling from age 6 onward. We assume a maximum of 17 years of education (which corresponds to 12 years of primary plus secondary schooling and 5 years of professional training). As we show below, this is quite a conservative threshold that applies to the vast majority of Colombians in 1973. Hence, to focus on individuals who are no longer studying, in our years of schooling regressions we look at only those who are at least $17 + 6 = 23$ years of age in 1973. Since La Violencia started in 1948, this implies examining individuals who were -2 years old or older in 1948. In most of our specifications, we aggregate these cohorts into groups by stages in the education process (similar to León 2012). Our youngest cohort includes prebirth/in utero individuals in 1948 (-2 to -1 years; i.e., 1–2 years before birth), followed by early childhood (0–3 years), preschool (4–6 years), primary school (7–12 years), high school (13–17 years), university (18–23 years), and beyond university (24–29 years).

Our second dependent variable evaluates whether cohorts exposed to violence not only attain lower educational levels but also, if they are employed, find jobs in sectors that typically hire less qualified individuals. We focus on the working population in 1973 and determine in which of 33 sectors each individual is employed. Next, we calculate the average education of workers in each sector. To reduce the likelihood that this is influenced by violence directly, we compute it out of sample, that is, for municipalities unaffected by violence or their neighbors.[7] Finally, we assign to each individual the average years of education of the sector in which he or she works. This *mean schooling in job sector* is a measure of the average human capital intensity of various jobs.[8]

Each sector is depicted in figure 1 with its corresponding average years of education. Some of the sectors with the highest human capital content are social and related community (a result likely driven by medical doctors), real estate and business services (lawyers), insurance and financial institutions, and crude petroleum and natural gas production (engineers). Other sectors with a relatively high human capital content include several industrial activities. The sectors with the lowest levels of formal education include fishing, agriculture and hunting, and forestry and logging.

This contrast between sectors with high and low human capital content suggests that if violence reduced employment in sectors with high human capital content, it can slow down the process of structural transformation that may accompany the process of economic development. Changes in the composition of employment are often a key part of the process of economic development, with both demand- and supply-side factors moving economies from predominantly agricultural sectors to services and manufacturing (for a review of this extensive literature, see Acemoglu 2009, chap. 20–21). As a complementary exercise, we also look directly at participation in agriculture, manufacturing, and services as an outcome variable.

The distributions of our two main dependent variables are presented by cohort in figure 2. Panel a shows the average years of schooling by age in 1973. It is clear that younger cohorts have higher educational attainment than older cohorts, which simply reflects the country's progress in education over time. Panel b shows that the average schooling in the job sector follows a similar pattern, increasing almost monotonically with younger generations. Similarly, figure 3 presents the cross-sectional distribution of these variables, differentiating by sex. Notice that even though men and women were similarly educated, women were employed by sectors with a lower average level of schooling than men.

Our main independent variable, La Violencia incidence, is coded on the basis of *Revista Criminalidad*, an annual publication of the National Police Department of Colombia. We use information from the first and second editions of the publication, in which the police recorded which areas were most affected by bipartisan conflict during 1948–53 (Policía Nacional de Colombia 1958, 1959).[9] The presence of La Violencia is then coded at the municipality

[7] This is yet another reason to focus our analysis on municipalities affected by violence and their neighbors.

[8] The estimation sample in this case includes only the working population older that 10 years in 1973 reporting information on the sector of employment.

[9] See app. B for more details on our violence measure and other variables. All variables are described and their sources specified in table B2.

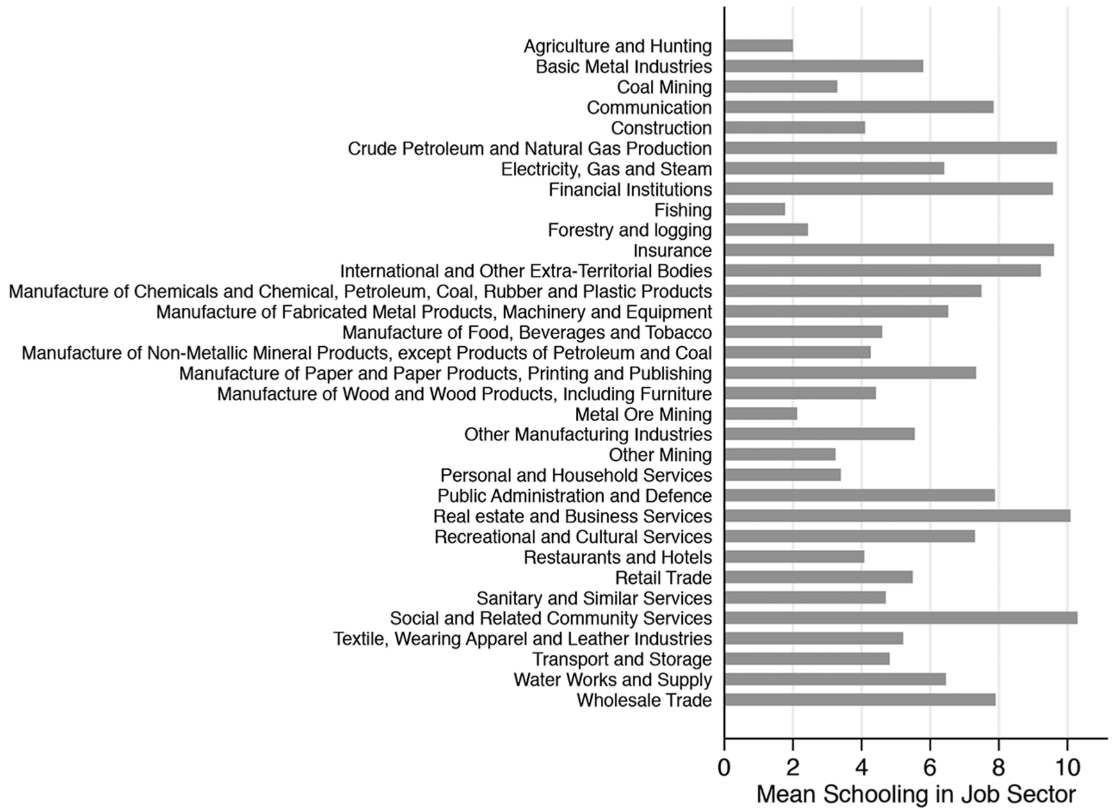

Figure 1. Average years of schooling of employees by sector in 1973. The average is computed out of sample, that is, for individuals who were born in municipalities not affected by violence or their neighbors. Source: 1973 Colombian census.

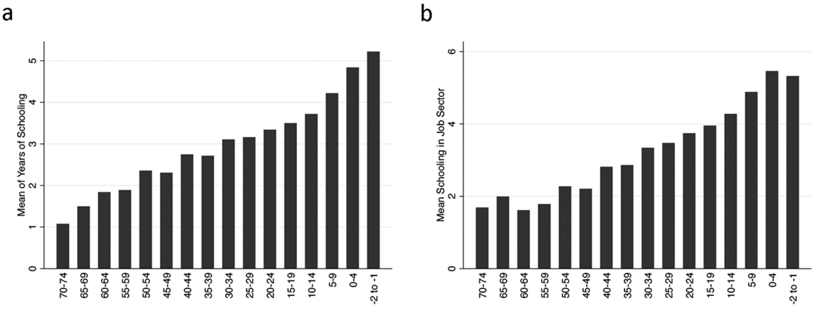

Figure 2. Years of schooling and mean schooling in job sector. Years of schooling (panel a) is the average years of schooling (in 1973) of each of the cohorts listed on the horizontal axis (by their age in 1948). Mean schooling in job sector (panel b) is computed only for the working population in 1973 and refers to the average years of education of employees involved in the sector in which the individual works. To reduce the likelihood that this average education is directly influenced by violence, it is computed out of sample for individuals who were born in municipalities not affected by violence or their neighbors. Source: 1973 Colombian census.

level with a dummy variable that equals 1 if the municipality was affected by bipartisan conflict during 1948–53 and 0 otherwise.

We observe long-term migration by comparing whether the municipality of residence in 1973 coincides with the municipality of birth. In our baseline regressions, we focus on nonmigrants, as it is more likely that these individuals, if born in a violent municipality, were exposed to violence. Yet similar to León (2012), we cannot take individuals' migration histories into account. Thus, we may wrongly assign exposure to violence to individuals who, having been born and currently living in a violent (peaceful) municipality, nonetheless lived in a peaceful (violent) municipality in the interim. However, if anything, this

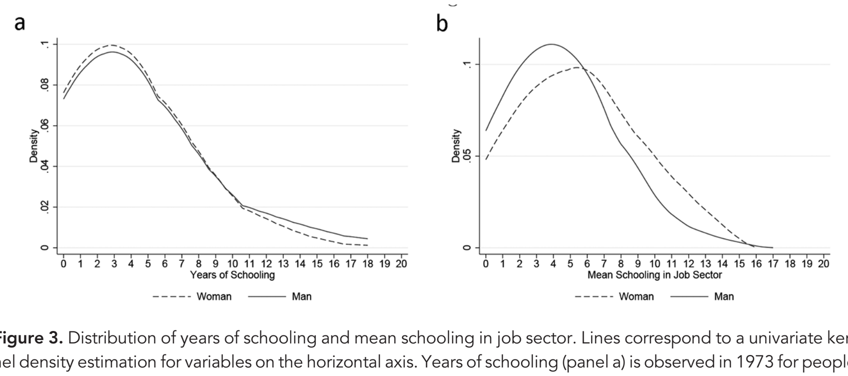

Figure 3. Distribution of years of schooling and mean schooling in job sector. Lines correspond to a univariate kernel density estimation for variables on the horizontal axis. Years of schooling (panel a) is observed in 1973 for people older than 6 years. Mean schooling in job sector (panel b) is computed only for the working population in 1973 and refers to the average years of education of employees involved in the sector in which the individual works. To reduce the likelihood that this average education is directly influenced by violence, it is computed out of sample for individuals who were born in municipalities not affected by violence or their neighbors. Source: 1973 Colombian census.

should bias the exercise against finding effects, as some individuals assumed to be treated were in fact away during violence. Below, we discuss other likely implications of focusing on the nonmigrant population.

Finally, for controls and to explore mechanisms, we also take into account the individual's gender (from the 1973 census) and a set of municipality-level variables $\mathbf{C_m}$. We include variables that, according to existing research, are likely to affect the incidence of violence in general (and La Violencia in particular) and that could also influence educational attainment. Thus, a first set of controls includes geographical variables, such as altitude, terrain, and precipitation. These variables, especially rough terrain (Hegre and Sambanis 2006; Aguirre 2016) and precipitation level (Miguel, Satyanath, and Sergenti 2004), have been highlighted in the literature on violence. Moreover, Colombia is a geographically diverse country, with the Andes breaking into three parallel ranges in its territory, and controlling for potential effects of this topography on both conflict and education may be important.

Also included is a set of historical institutional variables that comprise the earliest municipal foundation date from Bernard and Zambrano (1993) and indigenous population presence from 1535 to 1540 from Melo (1996; coded in Fernández 2010). We also add some variables capturing historical local conflict and politics: land conflicts from 1901 to 1931 from LeGrand (1986) and Liberals' vote share in the 1946 presidential election by collecting the official records from the national electoral authority.

Table 1 presents descriptive statistics for the main dependent variables in our sample of municipalities (years of schooling and the mean schooling in job sector) as well as for individual migration status, gender, and the municipal-level variables used in the analysis. Years of schooling is close to 4.1 on average in all municipalities and 4.3 on average in peaceful municipalities, and it falls to 3.6 for individuals exposed to violence. The pattern is similar for the average schooling in job sector: it averages 4.6 in all municipalities, 4.76 in peaceful municipalities, and 4.3 in violent ones. Also, within violent municipalities there is more migration (68% of individuals born in violent municipalities resided elsewhere in 1973) than among peaceful ones (54%), hinting that migration is a response to violence. The gender composition (53% female) is similar across all types of municipalities. Finally, in our sample of 449 municipalities, 38% were affected by La Violencia.

B. Empirical Strategy

The empirical strategy follows Duflo (2001) and exploits the differential impact of La Violencia across municipalities and cohorts in Colombia. Our main estimation equation is

TABLE 1
SUMMARY STATISTICS

	Mean	Standard Deviation	Minimum	Maximum	*N*
	A. Individual-Level Variables				
Years of schooling	4.1	3.65	0	18	2,706,067
Mean schooling in job sector	4.62	2.62	0	11.56	1,352,115
Migration	.59	.49	0	1	2,706,067
Sex (woman = 1)	.53	.5	0	1	2,706,067
Teacher	.001	.029	0	1	1,069,041
Sector of employment (1 = agriculture, 2 = manufacturing, 3 = services):					
Definition 1	1.321	.561	1	3	352,482
Definition 2	1.375	.569	1	3	383,171
Affected by violence:					
Years of schooling	3.62	3.34	0	18	820,638
Mean schooling in job sector	4.31	2.54	0	11.56	408,951
Migration	.68	.47	0	1	820,638
Sex (woman = 1)	.53	.5	0	1	820,638
Teacher	.001	.025	0	1	251,115
Sector of employment (1 = agriculture, 2 = manufacturing, 3 = services):					
Definition 1	1.105	.343	1	3	96,365
Definition 2	1.14	.378	1	3	100,254
Not affected by violence:					
Years of schooling	4.31	3.76	0	18	1,885,429
Mean schooling in job sector	4.76	2.64	0	11.56	943,164
Migration	.54	.5	0	1	1,885,429
Sex (woman = 1)	.53	.5	0	1	1,885,429
Teacher	.001	.03	0	1	817,926
Sector of employment:					
Definition 1	1.402	.604	1	3	256,117
Definition 2	1.459	.601	1	3	282,917
	B. Municipal-Level Variables				
Violence	.379	.486	0	1	449
Colonial institutions:					
Indigenous population	.414	.493	0	1	449
Early foundation date	1,774.94	125.013	1,530	1,975	449
Geographical covariates:					
Log (altitude of municipality head)	6.671	1.304	.693	8.023	447
Rainfall (mm)	1,942.881	932.168	640	7,750	447
Roughness (altitude [standard deviation])	490.962	284.04	1.756	1,349.764	449
Historical conflict:					
Conflict of land tenure (1901–31)	.078	.268	0	1	449
Politics:					
Liberals' vote share in 1946	61.371	27.29	.097	100	374
Additional robustness:					
Bandoleros	.196	.397	0	1	449
Years of late violence	.937	1.56	0	6	381

Note. The table shows descriptive statistics for the main sample, including violent municipalities and their neighbors. "Years of schooling" represents the years of education of individuals (older than 6 years) in 1973. "Mean schooling in job sector" is the average years of schooling of the employees in the sector in which the individual works. The average is calculated using the sectors in fig. 1 (International Standard Industrial Classification of All Economic Activities, revision 2) and is computed out of sample, i.e., for individuals who were born in municipalities not affected by violence or their neighbors. "Violence" is a dummy variable that equals 1 for municipalities affected by violence during La Violencia. "Migration" is a dummy variable that equals 1 if the respondent's residence was different from his municipality of birth. Definition 1 of sector of employment uses the following subsectors as categories: (1) Agriculture, Hunting, Forestry, and Fishing; (2) Manufacturing; and (3) Financing, Insurance, Real Estate, and Business Services and missing otherwise. Definition 2 uses (1) Agriculture, Hunting, Forestry, and Fishing; (2) Manufacturing, Construction, and Electricity, Gas, and Water; and (3) Financing, Insurance, Real Estate, and Business Services. Teacher is a binary indicator for those individuals who report the sectors of Education Services and Research and Scientific Institutes as their sectors of employment. For more details on the construction and sources of all variables, see table B2.

$$y_{imk} = \alpha_m + \lambda_k + \sum_k (V_m \times d_{imk})\gamma_k + \sum_k (\mathbf{C_m} \times d_{imk})\delta_k + \mathbf{X_i}\beta_i + \epsilon_{imk}, \quad (1)$$

where i indexes individuals, m municipalities (of birth), and k cohorts. Hence, y_{imk} is outcome y for individual i from cohort k born in municipality m. We include municipality and cohort fixed effects, α_m and λ_k, respectively.[10] A dummy variable, V_m, indicates whether municipality m was exposed to bipartisan violence during 1948–53. Finally, d_{imk} is a dummy variable that equals 1 if individual i born in m belongs to cohort k in 1948 and 0 otherwise.

The coefficients of interest are the γ_k, the difference-in-differences estimates of the average impact of La Violencia on birth cohort k.[11] The γ_k for cohorts k in their schooling years are unbiased measures of the impact of violence if there are no omitted time-varying and municipality-specific effects correlated with violence incidence. We verify the plausibility of this assumption by checking whether La Violencia helps explain the change in educational attainment for cohorts that were too old for their schooling decisions to have been influenced by the violence.

We include individual controls $\mathbf{X_i}$ and differential trends parametrized as functions of municipal characteristics $\mathbf{C_m}$ (the term $\Sigma_k(\mathbf{C_m} \times d_{imk})$). This robustness check is key since the main threat to our identification strategy is the possibility that, even absent the outbreak of violence, cohorts in municipalities affected by La Violencia would have trended away from those in peaceful ones for other reasons. By including these interaction terms, we allow individuals born in municipalities with traits that are presumably connected with violence to behave differentially. Finally, since education across cohorts within a municipality is likely correlated, we cluster standard errors at the municipality level.

IV. Main Results and Robustness

A. Years of Schooling

We start by estimating equation (1) without any controls other than the individual's gender and the set of cohorts k by educational stage (the beyond university cohort, 24–29 years, acts as the excluded comparison cohort). Table C1

[10] Data availability precludes including household fixed effects.

[11] Recall that in the sample the average years of schooling is 4.1, with a maximum of 18. Moreover, the 80th, 90th, and 95th percentiles are 7, 9, and 11 years, respectively. Therefore, even allowing for long delays and a fairly late entry into formal education, our estimates should capture the total amount of education ever obtained for our sample in the overwhelming majority of cases. We cannot test whether delayed schooling and subsequent failure to retain students was one of the main mechanisms, since we do not observe students in the key years in between La Violencia and 1973 to check whether they are starting school at relatively older ages.

(tables B1, B2, C1–C6 are available online) shows that when considering the full sample of individuals, none of the interactions between violence and cohorts are significant and that this noneffect is entirely driven by the migrant population.

There are several possible explanations for this. First, since we do not know the migration history, some of these individuals might have been unaffected by violence if they left their birthplace before the violence. By erroneously assigning exposure, the effect of violence on these individuals is thus attenuated. This explanation, however, cannot be the full story because it does not apply to the youngest cohorts who were clearly born in a municipality affected by violence even if they migrated soon after. The second possible explanation is that migrants are able to make up for any potential disadvantage of being born in a violent area. For example, they may move to richer areas and places with better public goods provision. Migrants from violent areas could also be discriminated against in their new locations and experience worse outcomes than nonmigrants. In this case, something else would explain their relatively better performance in our regressions. For instance, a third reason could be that there is positive selection into migration, with people escaping violence being particularly well educated. But again, this is not the only possibility: there could be negative selection, with the most vulnerable households migrating as a result of violence.

Regardless of the underlying reason, this discussion underscores that a variety of factors make it particularly hard to identify the true effect of violence on schooling for migrants. Instead, we can have a more precise estimate of the true effect of violence on nonmigrants. We therefore narrow the comparison to nonmigrants (i.e., individuals born in a given municipality and residing in that same place in 1973).

In column 1 of table 2, we find that all the relatively younger cohorts (from in utero to high school) were negatively affected by violence. The larger effects are concentrated among the relatively younger of these groups. The estimated effect is a fall of 0.3 years of education for individuals in the early childhood stage. By contrast, the impact on those in high school is only 0.06 years. In utero, early childhood, preschool, and even primary school individuals were more likely to be attending school than their older counterparts (regardless of the political situation), and this decision could have been altered by major shocks, such as the outbreak of violence. There is also growing evidence that adverse events in the very early stages, including in utero, are particularly strong (for reviews, see Currie 2011; Currie and Almond 2011; Currie and Vogl 2013). Individuals at more advanced stages of schooling may also have more resiliency when faced by shocks: after all, the fact that they remained in school

TABLE 2

MAIN RESULTS AND ROBUSTNESS: YEARS OF SCHOOLING (DEPENDENT VARIABLE: YEARS OF SCHOOLING)

	(1)	(2)	(3)	(4)	(5)	(6)
Violence × cohort in 1948:						
Violence × in utero	−.291***	−.182**	−.234**	−.297***	−.353***	−.264***
	(.102)	(.0909)	(.0978)	(.104)	(.106)	(.0993)
Violence × early childhood	−.309***	−.179**	−.249***	−.309***	−.384***	−.246***
	(.114)	(.0757)	(.0934)	(.113)	(.121)	(.0851)
Violence × preschool	−.269***	−.180***	−.219***	−.269***	−.321***	−.200***
	(.0958)	(.0598)	(.0754)	(.0948)	(.107)	(.0681)
Violence × primary school	−.121***	−.107***	−.103**	−.121***	−.142***	−.113***
	(.0392)	(.0364)	(.0403)	(.0393)	(.0435)	(.0415)
Violence × high school	−.0647**	−.0604**	−.0512*	−.0650**	−.0880***	−.0765**
	(.0295)	(.0300)	(.0301)	(.0293)	(.0309)	(.0318)
Violence × university	−.0325	−.0403	−.0327	−.0323	−.0423	−.0442
	(.0261)	(.0248)	(.0270)	(.0259)	(.0286)	(.0289)
Sex (woman = 1)	−.351***	−.351***	−.352***	−.351***	−.359***	−.360***
	(.131)	(.131)	(.132)	(.131)	(.137)	(.137)
Controls × cohort	No	Institutions	Geography	Historical conflict	Politics	Full
Municipality fixed effects	✓	✓	✓	✓	✓	✓
Cohort fixed effects	✓	✓	✓	✓	✓	✓
Observations	1,121,935	1,121,935	1,117,605	1,121,935	1,062,066	1,057,736
R^2	.348	.348	.349	.348	.344	.345

Note. Robust standard errors in parentheses are clustered at the municipality level. "Years of schooling" represents the years of education of individuals (older than 6 years) in 1973. Cohorts are defined by age in 1948: prebirth/in utero individuals (1–2 years before birth), early childhood (0–3 years), preschool (4–6 years), primary school (7–12 years), high school (13–17 years), university (18–23 years), and beyond university (24–29 years), which acts as the excluded comparison cohort. The set of municipal controls interacted with cohort fixed effects is marked in each column and includes the following: presence of indigenous population from 1535 to 1540 and foundation date of the municipality (institutions), altitude of municipality head, rainfall and roughness (geography), conflicts of land tenure from 1901 to 1931 (historical conflict), and Liberals' vote share in the 1946 presidential election (politics). "Full" refers to the entire set of controls from cols. 2–5. For more details on the construction and source of all variables, see app. B2.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

at a time when average educational attainment was much lower reflects a strong decision to pursue more education.

Finally, relative to the comparison group of individuals older than 24 years, those at the university stage have no differential schooling achievement by type of municipality. The interaction coefficient for this cohort is small (−0.03) and not statistically significant. This reflects the common trend in educational attainment for cohorts too old to have changed their schooling decisions because of La Violencia and helps validate the empirical approach.

The size of these effects is economically meaningful. For instance, individuals in the early childhood stage attained close to 4.7 years of schooling on average (fig. 2). Thus, the estimated impact of violence on their human capital accumulation is close to 6.6% of the mean attainment. The absolute fall in education for preschool age children is lower, but since they attain on average proportionally less education, the magnitude of the effect is similar to that of the early childhood cohort. In contrast, the effect on individuals old enough to be in primary school and high school is relatively smaller (0.12 and 0.06 fewer years of education, respectively) even after considering their relatively lower educational level (effects are close to 3% and 1.5% of average attainment, respectively). Finally, consistent with the growing evidence of the importance of prebirth and in utero conditions for human capital accumulation and other development outcomes (see, among others, Mansour and Rees 2012; Duque 2014, 2017), we find sizable effects for these cohorts, with an absolute fall of 0.29 years of education (or 7% of the mean in the sample, 4.1 years).[12]

While the parallel trends of older cohorts supports our identification assumption that violent municipalities would have exhibited trends similar to peaceful ones had it not been for La Violencia, for extra robustness we now directly control for trends based on observable municipal variables. We first examine the variables likely to influence violence in general and La Violencia in particular, according to existing research. These are described in table 3 and grouped by colonial institution, geography, historical conflict, and politics. As expected, a number of these variables are significantly different for peaceful and violent municipalities.[13] But the *t*-tests reveal that, with the single exception of colonial institutions variables (indigenous population and foundation date), the difference in observables between violent and peaceful neighboring

[12] We also estimate eq. (1) for all cohorts $k \in \{-2, -1, 0, \dots, 28, 29\}$, where k represents the age of individuals in 1948 (the excluded cohort is individuals aged 29). The resulting interaction terms γ_k are plotted in fig. 5, verifying that our results are not an artifact of our choices when grouping cohorts into categories by educational stage.

[13] Peaceful neighboring municipalities have more indigenous people, recent foundation dates, less rainfall, and lower vote shares for the Liberals.

TABLE 3

DIFFERENCES BETWEEN VIOLENT AND NONVIOLENT MUNICIPALITIES

Variable	Peaceful Neighbor (1)	Peaceful Nonneighbor (2)	Violent (3)	Peaceful Neighbor vs. Violent: Difference (4)	Peaceful Neighbor vs. Violent: *t*-Statistic (5)	Peaceful Nonneighbor vs. Violent: Difference (6)	Peaceful Nonneighbor vs. Violent: *t*-Statistic (7)
Generalized standardized index:							
Aggregate index	6.668	6.407	7.038	−.367	−.645	−.600	−1.112
A. Colonial institutions:							
Indigenous population	.465	.398	.329	.136	2.868	.069	1.66
Early foundation date	1,762.341	1,790.225	1,795.618	−33.277	−2.755	−5.393	−.452
B. Geographical covariates:							
Log (altitude of municipality head)	6.749	6.010	6.540	.208	1.643	−.530	−3.393
Rainfall (mm)	1,791.658	1,890.638	2,191.639	−399.980	−4.492	−301.001	−3.133
Roughness (altitude [standard deviation])	480.825	354.666	507.597	−26.772	−.968	−152.930	−5.916
C. Historical conflict:							
Conflict of land tenure (1901–31)	.064	.031	.100	−.035	−1.360	−.068	−3.860
D. Politics:							
Liberals' vote share in 1946	56.260	55.094	70.849	−14.588	−5.094	−15.754	−5.944
Additional robustness:							
Years of late violence	.774	.216	1.240	−.466	−2.807	−1.024	−9.634
Bandoleros	.150	.046	.270	−.120	−3.135	−.224	−9.517

Note. The aggregate index is defined for each municipality *m* as $I_m = \Sigma_{j\in C}(z\text{Covariate}_{j,m})^2$, where $z\text{Covariate}_{j,m}$ is the standardized value of covariate *j* across municipalities and *C* is the set of covariates listed in panels A–D. Violent municipalities are those affected by La Violencia. For more details on the construction and source of all variables, see app. B2.

municipalities (col. 4) is smaller than the same difference relative to peaceful but not adjacent areas (col. 6).[14]

We control for any time-invariant influence of these features in our regressions by including municipality fixed effects. Indeed, this is one of the strengths of the difference-in-differences approach. However, it is relevant to verify that these characteristics did not have a differential impact across cohorts. Table 2

[14] We report an individual *t*-test but also a compound index of standardized variables that captures the overall difference in this set of observables. This value is defined for each municipality *m* as $I_m = \Sigma_{j\in C}(z\text{Covariate}_{j,m})^2$, where $z\text{Covariate}_{j,m}$ is the standardized value of covariate *j* across municipalities and *C* is the set of covariates listed in table 3.

reports the results in columns 2–5. Column 2 interacts the institutional variables, column 3 the vector of geographical controls, column 4 the historical conflict variables, and column 5 the political controls. In column 6, the entire set is included, thus allowing for a very flexible specification that lets municipalities with a wide range of characteristics potentially connected to the incidence of violence have different education results.

The results are very similar across columns in terms of not just statistical significance but also the magnitude of the coefficients, suggesting that results reflect the impact of violence specifically and not other variables correlated with conflict. To summarize, there is a statistically significant, robust, and economically meaningful impact of violence on nonmigrants. Before proceeding with a look at labor market outcomes, however, we must recognize that migration creates challenges to identify the impacts even on the nonmigrant population. Specifically, selection rather than direct effects of violence may be driving our main results if parents who put a higher value on children's education migrate because of the violence. Moreover, migration is more common from violent places (table 1). The diverging levels of migration are particularly problematic if they also affect the composition of migrants: that is, if violence differentially expels the most (or least) selective parents compared with people migrating out of nonviolent places. To examine this possibility, we run a specification identical to our baseline regression where the outcome is a dummy variable for migration. The results are succinctly presented in figure 4. Unlike the analogous figure for schooling (fig. 5), in this case there is no discernible pattern for the plotted interaction terms between cohorts groups and exposure to violence. Most interactions are small (around 0.01 vs. a migration mean of 0.39) and not statistically significant. This contradicts the possibility that families with children in their schooling years or younger migrated differentially because of the violence. Thus, it lends further support to our conclusion that the observed impact on schooling and employment is the result of conflict directly and not of selective migration.[15]

[15] Admittedly, these findings are reassuring but insufficient to fully rule out selective migration effects. Migration of affected cohorts could conceivably be uniform in magnitude yet vary in aspects such as parents' education and other unobservables correlated with educational attainment. If this is the case, violence has an effect on capital accumulation in affected municipalities but for different reasons than the ones we emphasize. While we acknowledge this possibility, we also highlight that the complementary results in sec. V suggest impacts on the children's decision to go to school and therefore also add to the evidence that effects do not merely reflect selective migration. Also, col. 4 of table C6 shows that migrants from violent places are not particularly more or less educated than migrants from any other place. Thus, at least relative to that reference group, they do not appear to be very different, which again suggests (similar to the estimates in fig. 4) that there is a level effect on migration without discernible effects on composition. We discuss this result in more detail in app. C.5.

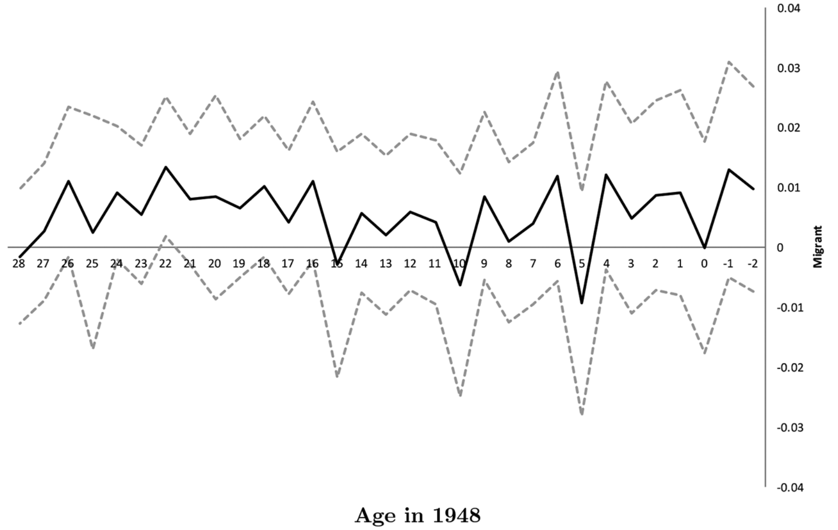

Figure 4. Difference-in-differences estimates of the effect of violence on migration by age in 1948. The figure plots coefficients and 90% confidence intervals for γ_k in the following regression model: $y_{imk} = c + \alpha_m + \lambda_k + \Sigma_k(V_m \times d_{imk})\gamma_k + \epsilon_{imk}$, where y_{imk} is the status of migration (1 if migrant, 0 otherwise) of individual i from cohort k born in municipality m; α_m and λ_k are municipality and cohort fixed effects, respectively; V_m equals 1 if municipality m was exposed to bipartisan violence during 1948–53; and d_{imk} is a dummy variable that equals 1 if individual i belongs to cohort $k \in \{-2, -1, 0, \ldots, 28, 29\}$, where k represents the age of individuals in 1948 (the excluded cohort in the d_{imk} set is individuals aged 29).

B. Sector of Employment

We now run our basic regression from equation (1), where the dependent variable is the average years of schooling of the sector in which the individual is employed. In this regression, a negative γ_k implies that individuals in cohorts k exposed to violence are engaged in activities that typically demand less qualified workers than those who were not exposed.[16]

The results are in table 4. Violence has a significant effect on the human capital content of individual employment for cohorts that, in table 2, exhibited an effect on education. Thus, exposure to violence also stops individuals from moving into the more educated (and hence presumably more productive) sectors. This suggests an impact on development and regional inequality in Colombia.

The larger effects are again among relatively younger individuals, and the size of the effects is economically meaningful. Cohorts in the early childhood stage take the largest hit, with 0.31 fewer years of education as a result of violence. This is 5.5% of the average (fig. 2) and roughly one-third of the distance

[16] Consistent with the findings for education, table C1 shows the results by migration status, finding no effect in the entire sample, a result driven by migrants.

Figure 5. Difference-in-differences estimates of the effect of violence on years of schooling by age in 1948. The figure plots coefficients and 90% confidence intervals for γ_k in the following regression model: $y_{imk} = c + \alpha_m + \lambda_k + \Sigma_k(V_m \times d_{imk})\gamma_k + \epsilon_{imk}$, where y_{imk} is years of schooling of individual i from cohort k born in municipality m; α_m and λ_k are municipality and cohort fixed effects, respectively; V_m equals 1 if municipality m was exposed to bipartisan violence during 1948–53; and d_{imk} is a dummy variable that equals 1 if individual i belongs to cohort $k \in \{-2, -1, 0, \ldots, 28, 29\}$, where k represents the age of individuals in 1948 (the excluded cohort in the d_{imk} set is individuals aged 29).

between the average worker in the agriculture and hunting sectors relative to the manufacturing of food and beverages sectors (fig. 1).[17]

Finally, since we find no significant difference among older cohorts, this effect does not appear to be driven by preexisting differential trends between the different types of municipalities. Moreover, in principle, the type of employment of older cohorts could have been affected by violence (unlike schooling decisions, which should not be affected by violence for older cohorts). This result suggests that the direct effect of violence on the sector of employment—such as the destruction of physical infrastructure of more advanced industries or financial capital flight—are not as important in slowing down the transition to more modern sectors as the human capital accumulation of the workers themselves. Indeed, if these other mechanisms were crucial, there is no a priori reason to expect the effect to be concentrated solely in individuals of school age when violence broke out.[18] This finding is also consistent with other studies showing that countries are able to recover from relatively large

[17] To show that group cohorts do not affect the main results, in fig. 6 we also plot the γ_k from estimating equation (1) for all cohorts $k \in \{-2, -1, \ldots, 28, 29\}$.

[18] Recall from sec. II that one of the sectors suffering most from violence directly was the agricultural sector, which also points in the direction of direct destruction being less important than worker qualifications, since it appears that workers transition more slowly out of agriculture in violent places, not more rapidly as would be expected if the dominant force is that the sector faces a direct economic loss from violence.

TABLE 4

MAIN RESULTS AND ROBUSTNESS: STRUCTURAL CHANGE (DEPENDENT VARIABLE: MEAN YEARS OF SCHOOLING IN JOB SECTOR)

	(1)	(2)	(3)	(4)	(5)	(6)
Violence × cohort in 1948:						
Violence × in utero	−.207**	−.104	−.210**	−.215**	−.241**	−.156*
	(.0963)	(.0774)	(.0969)	(.0998)	(.101)	(.0869)
Violence × early childhood	−.310**	−.155*	−.288***	−.313**	−.363***	−.215**
	(.123)	(.0865)	(.107)	(.125)	(.129)	(.0944)
Violence × preschool	−.292***	−.154**	−.258***	−.294***	−.337***	−.176**
	(.111)	(.0721)	(.0917)	(.113)	(.118)	(.0793)
Violence × primary school	−.183***	−.111**	−.168***	−.185**	−.212***	−.125**
	(.0706)	(.0496)	(.0631)	(.0728)	(.0781)	(.0530)
Violence × high school	−.0830**	−.0545	−.0727*	−.0841**	−.0937**	−.0487
	(.0397)	(.0362)	(.0396)	(.0409)	(.0442)	(.0383)
Violence × university	−.0163	−.0251	−.0297	−.0171	−.0215	−.0399
	(.0241)	(.0254)	(.0264)	(.0240)	(.0262)	(.0290)
Sex (woman = 1)	.955***	.952***	.955***	.955***	.944***	.942***
	(.152)	(.152)	(.153)	(.151)	(.154)	(.155)
Controls × cohort	No	Institutions	Geography	Historical conflict	Politics	Full
Municipality fixed effects	✓	✓	✓	✓	✓	✓
Cohort fixed effects	✓	✓	✓	✓	✓	✓
Observations	554,657	554,657	552,589	554,657	526,420	524,352
R^2	.425	.426	.426	.425	.421	.422

Note. Robust standard errors in parentheses are clustered at the municipality level. Mean years of schooling in job sector is the average years of schooling of the employees in the sector in which the individual works. Cohorts are defined by age in 1948: prebirth/in utero individuals (1–2 years before birth), early childhood (0–3 years), preschool (4–6 years), primary school (7–12 years), high school (13–17 years), university (18–23 years), and beyond university (24–29 years), which acts as the excluded comparison cohort. The set of municipal controls interacted with cohort fixed effects is marked in each column and includes the following: presence of indigenous population from 1535 to 1540 and foundation date of the municipality (institutions), altitude of municipality head, rainfall and roughness (geography), conflicts of land tenure from 1901 to 1931 (historical conflict), and Liberals' vote share in the 1946 presidential election (politics). "Full" refers to the entire set of controls from cols. 2–5. For more details on the construction and source of all variables, see app. B2.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

Figure 6. Difference-in-differences estimates of the effect of violence on structural change by age in 1948. The figure depicts the difference-in-differences estimates of the impact of violence on mean years of schooling of sector of employment for each cohort by age in 1948. It plots coefficients and 90% confidence intervals for γ_k in the following regression model: $y_{imk} = c + \alpha_m + \lambda_k + \Sigma_k(V_m \times d_{imk})\gamma_k + \epsilon_{imk}$, where y_{imk} is mean schooling in job sector for individual i from cohort k born in municipality m; α_m and λ_k are municipality and cohort fixed effects, respectively; V_m equals 1 if municipality m was exposed to bipartisan violence during 1948–53; and d_{imk} is a dummy variable that equals 1 if individual i belongs to cohort $k \in \{-2, -1, 0, \ldots, 28, 29\}$, where k represents the age of individuals in 1948 (the excluded cohort in the set of dummy variables d_{imk} is individuals aged 29). Mean schooling in job sector is computed only for the working population in 1973 and refers to the average years of education of employees involved in the sector in which the individual works. To reduce the likelihood that this average education is directly influenced by violence, it is computed out of sample for individuals who were born in municipalities not affected by violence or their neighbors.

shocks to physical capital and infrastructure, such as large natural disasters and war.[19]

In columns 2–6 of table 4, we explore robustness to the inclusion of key observable variables, as in table 2 for education. Some of the interactions with violence are somewhat sensitive to the inclusion of differential trends based on municipal characteristics. In particular, when including the interaction of institutional variables with cohort dummies in column 2, the coefficient on the interaction between violence and the in utero and high school generations becomes smaller in magnitude and is no longer statistically significant at conventional levels. For individuals from early childhood to primary school, however, the estimation still indicates a negative impact of violence that is both sizable

[19] Miguel and Roland (2011) is a well-known study of the Vietnam bombings. These authors find no evidence of long-run poverty traps. They also find, unlike us, no impact on education. However, their measure is literacy, which has an average of 88% in their sample, so the minor impacts may be due to scarce variation in this particular basic level of education. One example of a recent paper finding long-run recovery after natural disasters (in this case, earthquakes) is Gignoux and Menéndez (2016).

and statistically significant. Moreover, in the remaining columns—and, importantly, in column 6, where all controls are interacted with the cohort dummies—the results are in line with our baseline results reproduced in column 1 of the table. The only exception refers to the high school generation, where we find no significant effects once all controls are included.

Thus, for this outcome we must add the caveat that the impact on some generations in our baseline results—specifically, the in utero and high school cohorts—could reflect trends for different municipalities rather than violence. However, given the robustness of the results for the early childhood, preschool, and primary school groups, we again conclude that the reduction in the level of human capital of these individuals' jobs likely reflects the impact of violence and not some other variables correlated with conflict.

C. Robustness: Differential Fertility and Survival

In this section, we discuss the extent to which differential fertility and marriage patterns or selective survival may contaminate our results.[20]

First, it may be the case that rather than capturing the direct impact of violence, our estimates result from selection in the types of women who survive violence. For example, the lower educational attainment of treated cohorts could simply reflect that mothers who survive have a stronger preference for quantity over quality of children. Alternatively, instead of selective survival of certain types of mothers, they could reflect their decision to change their fertility in the face of violence. For instance, if mostly less-educated mothers are willing to have children in the presence of high levels of violence, we could also observe the documented patterns.

We examine these possibilities by verifying whether reproduction and marriage patterns differ according to violence exposure for the sample of women we observe in 1973 (focusing on those between the ages of 15 and 45 in 1948, thus of reproductive age when potentially exposed to violence). The results are presented in table 5 for an indicator variable for having any children, the number of children (total live births), and an indicator variable that equals 1 for married women as a dependent variable in a regression analogous to our

[20] Other possible sources of selection bias are unfortunately untestable with our data. For instance, we cannot directly compare the characteristics of mothers exposed to violence with those who were not exposed for two reasons. First, our examination of longer-term results implies that few of our children (in 1948) are (in 1973, as adults) still living with their mothers. Therefore, we have little information (and information only for a selected sample, those choosing to live with their mothers) on mothers' characteristics. Second, for the sample of mothers who we can match, we do not have information about the order and place of birth of their children. This discussion also explains why we cannot control for mother fixed effects.

TABLE 5

SELECTIVE FERTILITY IN BASELINE SAMPLE

	Dependent Variable					
	Dummy = 1 If Had Any Children (1)	Number of Children (2)	Dummy = 1 If Married (3)	Years of Schooling (4)	Years of Schooling (5)	Years of Schooling (6)
Violence × age in 1948:						
Violence × 15–19 years	−.00393	−.0547	.000334	.0305	.0189	.00605
	(.00699)	(.123)	(.00986)	(.0773)	(.0772)	(.0817)
Violence × 20–24 years	−.000199	.0424	.0102	.0569	.0505	.0449
	(.00635)	(.111)	(.00951)	(.0704)	(.0677)	(.0734)
Violence × 25–29 years	−.000618	−.141	−.00134	.0679	.0767	.0498
	(.00652)	(.105)	(.00874)	(.0645)	(.0654)	(.0659)
Violence × 30–34 years	.00362	.0473	.00441	.0592	.0560	.0462
	(.00668)	(.108)	(.0104)	(.0569)	(.0597)	(.0558)
Violence × 35–39 years	−.00436	−.0900	−.00195	.0495	.0528	.0454
	(.00597)	(.0957)	(.00862)	(.0500)	(.0495)	(.0529)
Number of live births						−.0709***
						(.0184)
Constant	.888***	5.599***	.803***	2.254***	2.148***	2.667***
	(.00215)	(.0415)	(.00283)	(.0221)	(.0229)	(.112)
Municipality fixed effects	✓	✓	✓	✓	✓	✓
Full controls × cohort	✓	✓	✓	✓	✓	✓
Sample	All women	All women	All women	All women	Women with children	All women
Observations	265,100	236,854	265,100	255,520	233,898	229,493
R^2	.016	.073	.014	.308	.312	.321

Note. The sample includes women between 15 and 45 years old. Robust standard errors in parentheses are clustered at the municipality level. Cohorts are defined by age in 1948. "Full" controls refers to the following municipal variables, which are interacted with cohort fixed effects: presence of indigenous population from 1535 to 1540 and foundation date of the municipality (institutions), altitude of municipality head, rainfall and roughness (geography), conflicts of land tenure from 1901 to 1931 (historical conflict), and Liberals' vote share in the 1946 presidential elections (politics). For more details on the construction and source of all variables, see app. B2.

*** $p < .01$.

basic regression equation. After controlling for municipality and cohort fixed effects (we group cohorts in 5-year intervals), we find no differential marriage or fertility patterns for women exposed to violence during their fertile years. While the likelihood of having any children or of being married is on average larger than 80% (notice the constant in the regressions in cols. 1 and 3), the interaction effects are typically smaller than 1 percentage point, virtually negligible. Similarly, for an average of roughly 5.5 children (col. 2), the point estimate of the interaction terms ranges from 0.04 to 0.14 in the absolute number of children. In short, there is no evidence that women of reproductive age married or had children at different rates in violent areas. If they did differ, this could signal decisions to delay/precipitate these decisions as a result of violence exposure.

Admittedly, one could observe similar marriage and fertility rates while the type of mothers in violent and peaceful areas differ, so this evidence remains suggestive. However, to explore this possibility, the last three columns in table 5 use years of schooling as the dependent variable to examine whether women of reproductive age in general (col. 4) and actual mothers in particular (any women of these ages with children; col. 5) differ in educational attainment in violent versus nonviolent municipalities. Column 6 also shows a specification for all women, controlling for the number of children. We do not find systematic or large differences in levels of education for any cohort, suggesting that women in violent and nonviolent municipalities tend to be similarly educated.

Second, our estimates could also be contaminated by differential survival rates among treated cohorts. For example, our impact on schooling for the prenatal cohort could be underestimated if only sufficiently strong fetuses can survive the prenatal stress associated with violence. Older children in conflict areas could also be affected by harsher conditions, leading to malnourishment or disease, or even suffer from violence directly, leading to (plausibly positive) selection for those we observe in 1973. In table 6, we therefore examine whether exposure to violence can predict the absolute size of the treated cohorts and their sex ratio. For the youngest cohorts, the sex ratio is revealing, as girls are biologically stronger than boys. For relatively older cohorts, boys could have been more directly involved in or exposed to violence. Panel A tests whether the size of each of our baseline cohorts is different in treated versus control municipalities, after including our basic controls.[21] The constants in the regression give a rough estimate of average cohort size in peaceful municipalities,

[21] Since some cohorts include more years than others, we divide cohort size by the number of years per cohort to facilitate interpretation.

TABLE 6

SELECTIVE SURVIVAL IN BASELINE SAMPLE

	Beyond University (1)	University (2)	High School (3)	Primary School (4)	Preschool (5)	Early Childhood (6)	In Utero (7)
	A. Dependent Variable: Cell Size Divided by Number of Years in Cohort						
Violence	−28.52*	−35.46	−43.03	−50.22	−57.96	−77.80	−104.8
	(15.90)	(22.78)	(30.39)	(36.67)	(41.76)	(57.76)	(76.16)
Constant	353.1*	433.8	587.8	711.1	839.7	1279	1749
	(185.8)	(277.6)	(379.3)	(487.7)	(575.9)	(835.9)	(1154)
R^2	.063	.054	.052	.049	.047	.045	.040
	B. Dependent Variable: Share of Boys in Cohort						
Violence	.00337	−.000722	−.00155	−.00397	.00980**	3.71E−05	−.00407
	(.00448)	(.00477)	(.00480)	(.00339)	(.00455)	(.00407)	(.00526)
Constant	.463***	.439***	.454***	.421***	.442***	.473***	.477***
	(.0298)	(.0334)	(.0273)	(.0233)	(.0331)	(.0264)	(.0338)
R^2	.061	.024	.019	.039	.025	.023	.056
Observations	372	372	372	372	372	372	372
Controls	✓	✓	✓	✓	✓	✓	✓

Note. Robust standard errors in parentheses are clustered at the municipality level. Cohorts are defined by age in 1948: prebirth/in utero individuals (1–2 years before birth), early childhood (0–3 years), preschool (4–6 years), primary school (7–12 years), high school (13–17 years), university (18–23 years), and beyond university (24–29 years). Panel A regresses the size of each cohort (divided by the number of years per cohort) in each municipality on the violence dummy. Panel B regresses the share of boys in each cohort in each municipality on the violence dummy. All municipal controls are also included: presence of indigenous population from 1535 to 1540 and foundation date of the municipality (institutions), altitude of municipality head, rainfall and roughness (geography), conflicts of land tenure from 1901 to 1931 (historical conflict), and Liberals' vote share in the 1946 presidential election (politics). For more details on the construction and source of all variables, see app. B2.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

and the violence coefficient indicates whether cohorts are larger or smaller in treated municipalities. Notice that there is a large variation in the size of cohorts across municipalities, as the standard error on the average size of cohorts is very large. However, this variation is typically not significantly associated with violence exposure. Only once—for individuals beyond university—is the violence coefficient significant. In this case, the size of the coefficient, a fall in almost 29 individuals, is close to 8% of the constant. The coefficients in the regressions for the other cohorts are, as stated, typically not significant, although we must add the caveat that they are always negative and not very precisely estimated. Importantly, the magnitude of the point estimates is similar for all cohorts, implying sizes 6%–8% smaller in violent areas. Panel B, on the other hand, shows more precisely estimated nonresults. The ratio of boys (roughly 45%) in each cohort is not smaller in violent municipalities, and the size of coefficients is close to zero. Indeed, even the sole significant coefficient (for the preschool cohort) is nonetheless quantitatively small: just 0.98 percentage points.

Overall, the evidence that treated cohorts are simply smaller, that there was selective survival by gender, or that they reflect differing mother characteristics is much weaker than the overall results, suggesting that these cohorts were directly affected by violence in their schooling choices and outcomes.

D. Robustness: Employment Selection and Sectoral Definition

We now explore two related robustness tests for our sector of employment regressions. First, one concern is that we observe the sector of employment only for those who work. Since employed people do not constitute a random sample of the whole population, our baseline estimates in table 4 may be biased. Consequently, in table 7, we present our results for the sector of employment using the two-step Heckman correction procedure (Heckman 1979). This method predicts and controls for the probability of participation in the labor market using individual characteristics relevant to the decision to work (which must not be directly affected by violence). We use municipality fixed effects, cohort fixed effects, and gender to predict the employment status in a simple probit model. In a second step, we compute the predicted probability of participation and run

$$\begin{aligned} y_{imk} = {} & \alpha_m + \lambda_k + \sum_k (V_m \times d_{imk})\gamma_k + \sum_k (\mathbf{C_m} \times d_{imk})\delta_k \\ & + \mu\left[\frac{\phi(\widehat{\text{Work}_{imk}})}{\Phi(\widehat{\text{Work}_{imk}})}\right] + \mathbf{X_i}\beta_i + \epsilon_{imk}, \end{aligned} \tag{2}$$

TABLE 7

RESULTS FOR YEARS OF SCHOOLING IN JOB SECTOR USING HECKMAN'S CORRECTION (DEPENDENT VARIABLE: MEAN YEARS OF SCHOOLING IN JOB SECTOR)

	(1)	(2)	(3)	(4)	(5)	(6)
Violence × cohort in 1948:						
Violence × in utero	−.224***	−.109*	−.224***	−.231***	−.265***	−.170**
	(.0551)	(.0600)	(.0577)	(.0552)	(.0615)	(.0722)
Violence × early childhood age	−.325***	−.164***	−.303***	−.328***	−.383***	−.227***
	(.0501)	(.0540)	(.0521)	(.0501)	(.0560)	(.0650)
Violence × preschool age	−.303***	−.165***	−.273***	−.305***	−.351***	−.190***
	(.0562)	(.0608)	(.0585)	(.0563)	(.0628)	(.0732)
Violence × primary school age	−.192***	−.119**	−.179***	−.194***	−.223***	−.134**
	(.0497)	(.0532)	(.0514)	(.0497)	(.0557)	(.0640)
Violence × high school age	−.0895*	−.0618	−.0793	−.0908*	−.101*	−.0536
	(.0521)	(.0555)	(.0538)	(.0522)	(.0585)	(.0669)
Violence × university age	−.0218	−.0327	−.0358	−.0227	−.0269	−.0453
	(.0535)	(.0570)	(.0552)	(.0536)	(.0600)	(.0686)
Mills ratio	5.527***	5.573***	5.580***	5.529***	5.808***	5.898***
	(.146)	(.147)	(.148)	(.146)	(.162)	(.165)
Controls × cohort	No	Institutions	Geography	Historical conflict	Politics	Full
Municipality fixed effects	✓	✓	✓	✓	✓	✓
Cohort fixed effects	✓	✓	✓	✓	✓	✓
Observations	1,162,192	1,162,192	1,162,192	1,162,192	1,162,192	1,162,192

Note. Mean years of schooling in job sector is the average years of schooling of the employees in the sector in which the individual works. Given that we observe sector decisions just for those who work, these estimations use Heckman's correction for self-selection into the labor force using an estimation in two stages. The selection equation or first stage uses cohort of birth, municipality fixed effects, and sex as variables for the selection. Cohorts are defined by age in 1948: prebirth/in utero individuals (1–2 years before birth), early childhood (0–3 years), preschool (4–6 years), primary school (7–12 years), high school (13–17 years), university (18–23 years), and beyond university (24–29 years), which acts as the excluded comparison cohort. The set of municipal controls interacted with cohort fixed effects is marked in each column and includes the following: presence of indigenous population from 1535 to 1540 and foundation date of the municipality (institutions), altitude of municipality head, rainfall and roughness (geography), conflicts of land tenure from 1901 to 1931 (historical conflict), and Liberals' vote share in the 1946 presidential election (politics). "Full" refers to the entire set of controls from cols. 2–5. For more details on the construction and source of all variables, see app. B2.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

where $\phi(w)/\Phi(w)$ is the inverse Mills ratio—which, in the spirit of a control function approach, corrects for the endogeneity generated by the selection problem—and $\widehat{\text{Work}}_{imk}$ is the predicted probability of being a worker (from the probit model). If μ is significant, the model supports the concern of a sample selection, which is the case in all our estimates of table 7. This correction, however, does not affect our qualitative results. Moreover, we obtain larger and more precise estimates compared with our specifications in table 4. In column 1, for instance, for those individuals in utero in 1948, we observe a decrease of 0.224 years of schooling of the sector of employment. This is 4.84% of the average human capital content of individuals in this cohort.[22]

Second, while we have argued that the human capital content of sectors of employment measured out of sample provides a valid metric for the level of sophistication of these sectors, one concern is that this also relates directly to the level of education of workers themselves, which we have shown is affected by violence. Also, this approach to classify sectors not only is not standard but also does not translate directly into the common transition from agriculture to manufacturing to services emphasized in the literature on structural transformation (although fig. 1 does show broad patterns consistent with a correlation between such sectors and an increasing human capital content). Thus, in table 8 we consider our baseline specification with a dependent variable that takes integer values from 1 to 3 depending on the sector of employment (1 for agriculture, 2 for manufacturing, and 3 for services). To make sure results are not sensitive to the exact components of what gets included in each sector, particularly in manufacturing, the definition in columns 1 and 2 uses the following subsectors to build the categories: (1) Agriculture, Hunting, Forestry, and Fishing; (2) Manufacturing; and (3) Financing, Insurance, Real Estate, and Business Services and missing otherwise. A second definition in columns 3 and 4 also includes Construction and Electricity, Gas, and Water in manufacturing.

Results for this specification (which is fully independent of workers' observed human capital in or out of the sample) also reveals that violence reduces the likelihood of transitioning to more modern sectors for treated cohorts. The effects are especially clear once we control for possible selection in the even columns. The more sizable effects, regardless of the sector's definition used, are concentrated on those in their early childhood or at the preschool

[22] The procedure in two stages leads to consistent but not asymptotically efficient estimates (for more details on the estimation procedure, see Heckman and Robb 1985). In our tables, we adjust standard errors that account for the two-step procedure (using the corrected routine programmed in Stata). Finally, notice that we report the number of total observations used in the first step and therefore more than in table 4.

TABLE 8

SECTOR OF EMPLOYMENT AND SELECTION INTO LABOR FORCE: EXPLORING STRUCTURAL CHANGE (DEPENDENT VARIABLE: SECTOR OF EMPLOYMENT)

	Definition 1		Definition 2	
	Ordinary Least Squares (1)	Heckman (2)	Ordinary Least Squares (3)	Heckman (4)
Violence × cohort in 1948:				
Violence × in utero	−.0124	−.0126**	−.0148	−.0145**
	(.0133)	(.00594)	(.0133)	(.00597)
Violence × early childhood	−.0199*	−.0198***	−.0193	−.0194***
	(.0119)	(.00527)	(.0119)	(.00530)
Violence × preschool	−.0176*	−.0173***	−.0203*	−.0205***
	(.0104)	(.00589)	(.0104)	(.00593)
Violence × primary school	−.00920	−.00906*	−.0101	−.0102*
	(.00857)	(.00518)	(.00875)	(.00521)
Violence × high school	−.00332	−.00316	−.00219	−.00241
	(.00608)	(.00540)	(.00649)	(.00543)
Violence × university	.00348	.00357	.00251	.00234
	(.00488)	(.00557)	(.00566)	(.00560)
Sex (woman = 1)	.242***	.365***	.225***	.0742***
	(.0477)	(.0248)	(.0367)	(.0275)
Mills ratio		−.0967***		.113***
		(.0194)		(.0205)
Municipality fixed effects	✓	✓	✓	✓
Cohort fixed effects	✓	✓	✓	✓
Observations	352,482	1,162,192	383,171	1,162,192
R^2	.588		.573	

Note. Sector of employment is a variable that takes integer values from 1 to 3. Definition 1 of sector of employment uses the following subsectors as categories: (1) Agriculture, Hunting, Forestry, and Fishing; (2) Manufacturing; and (3) Financing, Insurance, Real Estate, and Business Services and missing otherwise. Definition 2 uses (1) Agriculture, Hunting, Forestry, and Fishing; (2) Manufacturing, Construction, and Electricity, Gas, and Water; and (3) Financing, Insurance, Real Estate, and Business Services. Given that we observe sector decisions just for those who work, estimations in cols. 2 and 4 use Heckman's correction for self-selection into the labor force using an estimation in two stages. The selection equation or first stage uses cohort of birth, municipality fixed effects, and sex as variables for selection. Cohorts are defined by age in 1948: prebirth/in utero individuals (1–2 years before birth), early childhood (0–3 years), preschool (4–6 years), primary school (7–12 years), high school (13–17 years), university (18–23 years), and beyond university (24–29 years), which acts as the excluded comparison cohort.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

in 1948. For them, La Violencia implied an approximate reduction of 0.02 points on the sector of employment variable (roughly 1.5% of the average sector of work used by both definitions).[23]

[23] The interpretation of this coefficient is somewhat misleading, given the nature of the outcome variable. We acknowledge that a better strategy in this scenario is an ordered probit with Heckman correction. That strategy, however, relies heavily on the distributional assumptions of the unobservables

E. Additional Robustness

In appendix C (apps. A–C are available online), we discuss a number of additional robustness exercises to rule out possible alternative interpretations of our findings. First, we verify that the findings reported above reflect the period of intense bipartisan violence commonly known as La Violencia and not simply the impact of persisting violence thereafter. Second, we examine the possible implications of focusing on our baseline sample with only neighboring municipalities. Specifically, we study whether (negative or positive) spillovers from violent neighbors could affect the outcomes of our peaceful control municipalities. We find little indication of significant spillovers, lending further credibility to our baseline results. We show that violence only in the municipality of birth, not in municipalities neighboring violent places, helps predict migration in the full sample of individuals; being born in a violent municipality affects the probability of moving to a neighboring location negatively, diminishing the concern that we have spillovers stemming from a relatively large proportion of (potentially positively or negatively selected) treated individuals arriving in our control municipalities; migrants from violent areas are not particularly positively (or negatively) selected relative to any regular migrant; and results are similar when running our baseline specification for years of schooling with the full set of municipalities. These findings on the limited spillovers between municipalities resonate with the fragmented nature of Colombian jurisdictions, as highlighted by, among others, Safford and Palacios (2002). Finally, we demonstrate that our results are robust to including department-specific temporal trends.

V. Mechanisms: Supply or Demand?

Our results uncover a significant and quantitatively important reduction in human capital accumulation and restriction in the types of jobs for cohorts exposed to violence. But what mechanisms underlie this relationship?

To assess the likely relevance of different mechanisms, table 9 presents our basic regressions for years of schooling, restricting the sample to boys (cols. 1, 3) and girls (cols. 2, 4), with the full set of controls from tables 2 and 4.

When examining the impact of years of schooling, we find that the results are concentrated on boys rather than girls. Indeed, the coefficients for in utero, early childhood, and preschool cohorts are significant and negative for both

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and their correlation between the selection equation and the baseline specification. For this reason, we prefer the simpler Heckman model also as a more direct comparison to ordinary least squares. Our results, nevertheless, are robust to this alternative strategy.

TABLE 9
MECHANISMS: SEX

	Dependent Variable			
	Years of Schooling		Mean Schooling in Job Sector	
	Boys (1)	Girls (2)	Boys (3)	Girls (4)
Violence × in utero	−.352***	−.179*	−.168**	.329**
	(.127)	(.0913)	(.0827)	(.150)
Violence × early childhood	−.325***	−.174**	−.219**	.222
	(.102)	(.0845)	(.0902)	(.160)
Violence × preschool	−.279***	−.124*	−.200**	.117
	(.0868)	(.0685)	(.0814)	(.157)
Violence × primary school	−.172***	−.0669	−.134**	−.00774
	(.0555)	(.0474)	(.0574)	(.136)
Violence × high school	−.0935**	−.0621	−.0703*	.0928
	(.0471)	(.0393)	(.0395)	(.121)
Violence × university	−.0953	.000546	−.0651	.0920
	(.0904)	(.0403)	(.0696)	(.108)
Controls × cohort	Full	Full	Full	Full
Municipality fixed effects	✓	✓	✓	✓
Cohort fixed effects	✓	✓	✓	✓
Observations	510,766	546,970	424,002	100,350
R^2	.359	.334	.433	.284

Note. Robust standard errors in parentheses are clustered at the municipality level. “Years of Schooling” represents the years of education of individuals (older than 6 years) in 1973. “Mean Schooling in Job Sector” is the average years of schooling of the employees in the sector in which the individual works. Cohorts are defined by age in 1948: prebirth/in utero individuals (1–2 years before birth), early childhood (0–3 years), preschool (4–6 years), primary school (7–12 years), high school (13–17 years), university (18–23 years), and beyond university (24–29 years), which acts as the excluded comparison cohort. “Full” refers to the following municipal variables, which are interacted with cohort fixed effects: presence of indigenous population from 1535 to 1540 and foundation date of the municipality (institutions), altitude of municipality head, rainfall and roughness (geography), conflicts of land tenure from 1901 to 1931 (historical conflict), and Liberals’ vote share in the 1946 presidential election (politics). For more details on the construction and source of all variables, see app. B2.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

genders but roughly twice as large for boys than for girls. Thus, it seems that violence affected both genders in these age groups, but the largest effects were suffered by boys. Moreover, while violence reduced education in the primary school and high school age groups for boys, we find no significant effects for girls in these age groups.

When looking at sectors of employment, the difference is even clearer. Only boys have significant negative interactions between the age groups and violence. Interactions for girls are typically not significant, with the sole exception of in utero individuals, where in fact the coefficient is positive. That is, violence reduced the access of boys—not girls—to sectors of employment typically demanding more qualified workers.

One reason for this is that since girls have lower educational attainment, their levels of schooling simply cannot fall as much. This plus the fact that the labor participation of girls was smaller than that of boys may help explain why violence did not affect the jobs that girls obtain.[24]

But there are other reasons why La Violencia may have affected boys more than girls, and these may reveal the mechanisms whereby conflict influenced education and development. On the one hand, boys are more likely to have participated directly in violent events. On the other hand, even when not participating directly, if their fathers were killed or engaged in violence, or if their fathers' job opportunities or incomes were reduced by the fighting, it is more likely that boys would have had to withdraw from school to support the family. This withdrawal would have pushed them into the labor market earlier, with the implication of halting their education and obtaining lower-quality jobs.

This difference between boys and girls therefore suggests that demand-driven mechanisms partly explain our main result for years of schooling. Indeed, if all the effects were supply driven (school destruction, teacher migration, and so on), they should affect both boys and girls.

Another way to explore the mechanisms is to look at the probability of having no schooling at all rather than years of schooling as the dependent variable. Presumably, if schools are destroyed and this explains a large part of the overall effect on years of schooling, we should observe an effect on the probability of simply having no schooling. We ran our basic regression using an indicator variable that equals 1 for individuals with no schooling as the dependent variable and found no robust effect for our treated cohorts. Indeed, the only significant coefficients—although not robust to the addition of controls—are negative for the youngest controls. In general, as shown in table 10, we find zero effect on the probability of having no schooling.

To further examine mechanisms, we also run our basic specification where the dependent variable is a binary indicator for teacher (based on the sector of employment). One caveat is that unlike our baseline specification for schooling, in this case older cohorts could be affected if by violence (e.g., if schools are destroyed and they must abandon their careers). However, older cohorts should

[24] That some of the coefficients for girls are positive and, in the case of the in utero cohort, statistically significant is a surprising finding. This may be explained by selection issues, which might be particularly pressing for women given the low female labor force participation in this period. Indeed, in regressions as our baseline specification with labor force participation for women as the dependent variable, we find that participation falls for affected cohorts. Consistent with this, results from a Heckman selection model imply that after controlling for selection, the positive coefficients for female cohorts are no longer present.

TABLE 10

MECHANISMS: PROBABILITY OF NO SCHOOLING (DEPENDENT VARIABLE: DUMMY = 1 IF NO SCHOOLING)

	(1)	(2)	(3)	(4)	(5)	(6)
Violence × cohort in 1948:						
Violence × in utero	−.0389*	−.00841	−.0287*	−.0381*	−.0378	.00660
	(.0222)	(.0144)	(.0163)	(.0214)	(.0257)	(.0159)
Violence × early childhood	−.0302*	−.00872	−.0253*	−.0301*	−.0281	.00391
	(.0180)	(.0124)	(.0145)	(.0179)	(.0213)	(.0142)
Violence × preschool	−.00685	.00884	−.00549	−.00705	−.00769	.0140
	(.0138)	(.0103)	(.0120)	(.0140)	(.0162)	(.0121)
Violence × primary school	−.00711	.00555	−.00739	−.00739	−.00837	.00921
	(.0114)	(.00842)	(.0102)	(.0116)	(.0134)	(.00999)
Violence × high school	.000239	.00386	−.00243	6.28E−05	.00198	.00883
	(.00698)	(.00637)	(.00683)	(.00715)	(.00857)	(.00717)
Violence × university	−.00483	−.00183	−.00555	−.00488	−.00489	.000244
	(.00604)	(.00552)	(.00607)	(.00611)	(.00699)	(.00620)
Sex	.0209***	.0210***	.0210***	.0210***	.0183***	.0183***
	(.00307)	(.00307)	(.00309)	(.00307)	(.00297)	(.00299)
Constant	.290***	.289***	.290***	.290***	.282***	.280***
	(.00693)	(.00515)	(.00652)	(.00701)	(.00750)	(.00594)
Controls × cohort	No	Institutions	Geography	Historical conflict	Politics	Full
Municipality fixed effects	✓	✓	✓	✓	✓	✓
Cohort fixed effects	✓	✓	✓	✓	✓	✓
Observations	1,162,192	1,162,192	1,157,696	1,162,192	1,099,801	1095305
R^2	.161	.161	.161	.161	.161	.162

Note. Robust standard errors in parentheses are clustered at the municipality level. Cohorts are defined by age in 1948: prebirth/in utero individuals (1–2 years before birth), early childhood (0–3 years), preschool (4–6 years), primary school (7–12 years), high school (13–17 years), university (18–23 years), and beyond university (24–29 years), which acts as the excluded comparison cohort. The set of municipal controls interacted with cohort fixed effects is marked in each column and includes the following: presence of indigenous population from 1535 to 1540 and foundation date of the municipality (institutions), altitude of municipality head, rainfall and roughness (geography), conflicts of land tenure from 1901 to 1931 (historical conflict), and Liberals' vote share in the 1946 presidential election (politics). "Full" refers to the entire set of controls from cols. 2–5. For more details on the construction and source of all variables, see app. B2.

* $p < .10$.

*** $p < .01$.

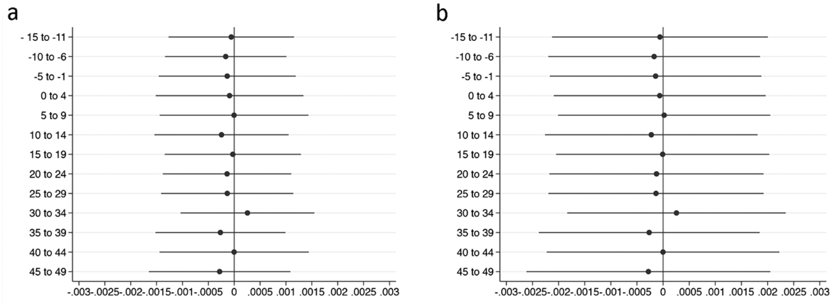

Figure 7. Impact on likelihood of being a teacher. Panel a depicts the interaction coefficients (and 95% confidence intervals) of the violence dummy and the specified cohorts in 1948 in a regression for an indicator for teacher in 1973. This variable, based on sector of employment, uses codes 931 and 932 of the Clasificación Industrial Internacional Uniforme (CIIU) revision 2 (Education Services and Research and Scientific Institutes). The econometric model includes cohort and municipality fixed effects. The excluded cohort is defined by the individuals who were 50–54 in 1948. Robust standard errors are clustered at the municipality level. Panel b depicts the interaction coefficients (and 95% confidence intervals) of the violence dummy and the specified cohorts in 1948 in a regression for an indicator for teacher in 1973. This variable, based on sector of employment, uses codes 931 and 932 of the CIIU revision 2 (education services and research and scientific institutes). The econometric model includes cohort and municipality fixed effects. The excluded cohort is defined by the individuals who were 50–54 in 1948 using the two-step Heckman correction procedure. The selection equation uses sex, municipality fixed effects, and cohort fixed effects to predict participation in the labor force.

be less affected than younger ones who are starting their career paths and who might avoid entering the profession in the first place. Thus, it is still informative to examine whether entry into the teaching profession decreases for younger cohorts in violent areas. The results (shown graphically in figure 7), reveal no significant effects whether we correct (panel B) or do not correct (panel A) for selection into employment.[25]

These last two findings are in line with the findings by sex, since they underscore that demand factors (instead of the direct destruction of supply) are the more likely mechanisms for our overall effect. Of course, we must nonetheless admit that evidence in this section is not entirely conclusive, as these are indirect ways of assessing the likely mechanisms absent more direct data on both household behavior and schooling supply.

VI. Conclusions

La Violencia in Colombia had important implications for economic development that extended beyond the most immediate and obvious effects on physical capital. In particular, it reduced the human capital accumulation of cohorts exposed to violence, an effect that persisted 25 years after the outbreak of bipartisan violence. Moreover, this slowed the growth of employment sectors

[25] Point estimates are very close to zero, although confidence intervals are quite wide relative to the very low average fraction (less than 1%) of teachers in the population.

typically demanding highly qualified workers and, specifically, the transition from agricultural to manufacturing and service jobs, thus retarding the transition of the labor force to more modern sectors of production. This likely slowed down the structural transformation often associated with the process of development and arguably exacerbated regional inequalities.

These effects highlight the importance of devising policy strategies to attenuate the impact of violence. Our findings point to the importance of further investigating policies to counteract the decisions that households in situations of distress must make (such as increasing child labor participation) and alleviate the harsh conditions that could produce obstacles to sustained educational investment (not just financial but also constraints on early childhood development). Indeed, our results suggest that demand-driven mechanisms partly explain the reduction in human capital accumulation, so an approach based on restoring the supply of public education, such as building schools or increasing the number of teachers, is insufficient.

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