The Hidden Costs and Lasting Legacies of Violence on Education: Evidence from Colombia

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Abstract

This paper studies the medium- to long-term effects of exposure to violence in early childhood (in-utero to age 5) on educational outcomes. To study this question, this paper exploits the massive escalation of violence and crime in Colombia in the 1980s and 1990s. Results show that one standard deviation increase in the homicide rate, in at least one year during early life, is associated with 0.1 fewer years of education, with a 1-percentage-point decline in school enrollment and a 3-percentage-point increase in the probability that a child is behind grade. The findings also show that in utero and early-childhood exposure to violence has a more pronounced impact on human capital attainment than exposure at other stages of the life course (i.e., school age, adolescence). Furthermore, extensive robustness tests provide little evidence that selective sorting, mobility, fertility, or survival could drive estimates of violence on education.

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One of the most pressing problems faced by developing countries is high crime and violence. In Latin America, for example, crime and violence have reached "epidemic levels"—the homicide rate is over 0.1 homicides per thousand inhabitants—making it the world's most violent region (United Nations, 2013). A decline in human capital is a particularly destructive consequence of violence on individuals—extending beyond direct human losses, injuries, and property damage—that can lead to poverty and inequality through its effects on wages and productivity (Mincer, 1974; Schultz, 1961; Becker, 1962). Therefore, studying how violence affects human capital is important.

Early childhood is especially relevant to study as education during the first years of life sets children on their path for future outcomes. Violence exposure in the early stages could lead to large deficits that are difficult to reverse. Although the evidence on the short- (and medium-) term impacts of violence exposure on education and health is growing, there is a relative dearth of empirical evidence on how these shocks extend over time.

This study examines the long-term impacts of exposure to crime and violence, from the prenatal period to age five, on an individual's educational attainment. I focus on Colombia, which has experienced enormous changes in its homicide rate in the last three decades, in both urban and rural areas, due to the proliferation of organized crime (from the late-1980s to mid-1990s) and the recrudescence of its 50-year-long low-intensity armed conflict (from the mid-1990s to mid-2000s). From 1980 to 2002, the homicide rate in Colombia went from 0.2 homicides per every 1,000 inhabitants to almost 0.9 (an increase of more than 300%), and from 2002 to 2010 it declined to approximately 0.4 homicides per every 1,000 inhabitants (a decline of approximately 55%).

The identification strategy I use in this paper exploits the temporal and geographic variation in cohorts of individuals exposed to high crime and violence in Colombia in their early lives. Using Census-2005's detailed information on the date and municipality of birth, and long-run outcomes (i.e., education) for each individual, I am able to identify the violence to which a person was exposed in utero and in early childhood (as well as in later stages). I estimate models that control for a rich set of variables including geographic and time fixed-effects and trends, as well as models that account for sibling fixed-effects, to provide rigorous evidence of the effects of violence on education. Results show that an increase of one standard deviation in violence in at least one year in early-life, is associated with 0.1 fewer years of schooling, a 1-percentage-point decline in school enrollment, and a 2-percentage-point increase in the probability that a child is behind grade. So an individual who is exposed to more than one year of high homicide rate in early-life can experience up to 0.4 fewer years of schooling as an adult, than someone who was not exposed. Similarly, school enrollment could decrease by almost 4 percentage points and being behind grade could increase up to 6 percentage points. These findings are important considering the huge inequality in education in developing countries.

Results are robust to an extensive set of falsification tests and analyses of potential sources of selection bias. I find no placebo effects of homicide rates three (and four) years prior to birth, providing support for the validity of the identification strategy. While the results are conditional on survivorship and on individuals remaining in the country, they do not seem to be driven by selection on sorting, mobility, fertility, survival, or other unobserved time-varying factors associated with violence that could affect educational attainment.

The study contributes to previous research in several ways. First, it provides new findings on the relatively understudied link between violence exposure in the first years of life and long-term human capital outcomes, as most studies have focused on school-age exposure to violence (6–17 years of age Shemyakina, 2011; Akresh and de Walque, 2011; Chamarbagwala and Morán, 2011; Rodriguez and Sanchez, 2012). Focusing on exposure in early-life is important given recent research showing that adverse experiences in utero and in early childhood affect an individual's long-term socioeconomic success (Barker, 1998; Cunha and Heckman, 2007; Almond and Currie, 2011a,b). Second, by exploring how violence over the life course, from in utero to adolescence, affects education, this paper shows that in-utero and early-childhood exposure has a more pronounced impact on human capital than exposure at other stages (i.e., school-age, adolescence, etc.). This finding is particularly relevant considering that, although prior research has demonstrated the negative impact that exposure to violence has on individuals, to date, no research has studied whether violence at a particular developmental time has more pronounced impacts on later-life outcomes. From a policy perspective, targeting interventions to children who were exposed to violence in early childhood might be more efficient in terms of costs and benefits than targeting them at other stages (Doyle et al., 2009). Third, I use an arguably more objective measure of violence—the homicide rate– than those employed in previous literature that usually lack temporal or geographic variation, or that rely on victims self-reports of damages and loses during wars. By using homicide rates, I can identify with more precision changes in violence exposure in key developmental stages such as in-utero and in each year of an individual's life from age 0 to 5 (and up to age 17). Fourth, while most of the previous literature has focused on civil wars or internal conflicts, this paper also considers the effects of violence originating from the escalation of urban crime—associated with the consolidation of the drug cartels in the late 80s and early 90s that mostly affected urban populations in Colombia (Levitt and Rubio, 2000). Thus, the results of this paper could extend to other settings with high crime and violence but without massive forms of violence such as a civil war. One example is the case of Mexico, which has recently experienced a dramatic increase in the level of crime associated with the drug-business.

The remainder of the paper is organized as follows. Section 1 presents the theoretical predictions and a brief description of the existing literature and describes the nature of the violence in Colombia. Sections 2 and 3 present the data and the empirical strategy, respectively. Section 4 shows the findings on the long-term impacts of violence on education. Lastly, Section 5 performs some robustness tests to show that the results can be interpreted as lower-bound estimates of the impact of violence, and Section 6 provides a brief conclusion.

1. Background

In this section, I describe some of the literature on the effects of violence exposure on educational outcomes and on the potential mechanisms through which violence could affect education. I also present a brief description of violence in Colombia.

1.1. Early-Life Violence Exposure and Human Capital

A growing body of evidence documents that adverse conditions before age six can have persistent impacts on later-life well-being (Barker, 1998; Cunha and Heckman, 2007; Almond and Currie, 2011a,b). Several studies have shown that exposure to famines and malnutrition, weather shocks, disease and pandemics, radiation fallout, and pollution in-utero and during the first years of life reduce adult health, educational attainment, and wages (Aguilar and Vicarelli, 2012; Almond, 2006; Almond et al., 2009; Maccini and Yang, 2009; Almond and Mazumder, 2011; Van den Berg et al., 2007, 2012). Based on this evidence, it is reasonable to expect that cohorts exposed to the huge increase in homicide rates in Colombia that occurred during their early lives would experience a decline in later-life human capital.

Table 1 provides an overview of the studies investigating the relationship between violence shocks and education. As the table shows, these studies have consistently found a negative association. By comparing children exposed to wars (or conflicts) in the area of residence with non-exposed children, Akresh and de Walque (2011) found effects around 0.5 fewer years of education (Rwanda); Shemyakina (2011) found 0.6 (Tajikistan); Rodriguez and Sanchez (2012) found 0.2 (Colombia); Chamarbagwala and Morán (2011) found impacts between 0.12 and 1.09 (Guatemala); Akbulut-Yuksel (2011) found 0.4 (Germany), and Leon (2012) estimated an effect of at least 0.3 fewer years of schooling (Peru). Others have found that violence reduces school enrollment (Bosnia) Swee (2009) and increases grade failure (Mexico) Caudillo and Torche (2014).

Moreover, previous literature has mostly focused on armed conflicts or civil wars that represent huge episodes of mass mortality (see column 2 in Table1). While these are important shocks to analyze, little research has explored the effects of other forms of less-extreme violence such as urban crime that can also impose huge costs to individuals and that are relatively more common. In addition, the measures used in previous literature to model changes in violence (also shown in column 2), have mostly relied on either geographic or temporal variation, or on self-reports of victims of war that have a number of limitations. Lastly and most importantly, these studies have exclusively focused on exposures that occur after age 5 (see column 3 in Table1).

Compared with previous literature, this paper makes several contributions: First, this is one of the first papers to study the long-term effects of early-life exposure to violence on future educational outcomes and provide evidence that in-utero and early childhood violence shocks have a more pronounced impact than shocks experienced at other developmental stages of life.[1]. This paper is also the first to study the links between early-life exposure and long-term educational outcomes using an objective measure of violence—the homicide rate.[2] Several studies have used victim's own reports of human rights violations during periods of war—which are likely to suffer from bias as individuals who self-report are likely to be selective—or have employed violence measures that provide little spatial or temporal variation which limits their ability to identify the effects of shocks of this nature from other confounding factors (Leon, 2012; Shemyakina, 2011). By using municipal-level homicides in a given year, I am able to examine the effect of violence in each specific year over the life course, providing more accurate evidence of the relationship between education and violence than provided by the prior research. I also add to the literature by using data on Colombia that incorporates two sources of violence—urban crime and armed conflict—so I am able to exploit greater variation in the homicide rate across geographic units and years. Lastly, this paper moves forward from previous research by performing an extensive analysis of selection related to endogenous sorting, mobility, fertility, and survival, as well as falsification tests that allow me rule out the presence of confounding factors, trends, and potential sources of bias.

[Table 1 around here]

1.2. Potential Pathways through Which Violence Could Affect Educational Outcomes

The literature has documented several mechanisms through which violence exposure in early life could reduce educational outcomes. First, it could limit the amount and quality of resources in the local community (supply-side mechanisms) available for child human capital formation. For example, high violence can disrupt the economy (i.e., reduce household economic resources Minoiu and Shemyakina, 2014), destroy infrastructure (e.g., hospitals, schools Akbulut-Yuksel, 2011), reduce the quality of public services (exodus of medical doctors, teachers Leon, 2012; Rodriguez and Sanchez, 2012), and limit investments (e.g., during wars, resources are shifted away from education and health to military spending).

[1] Another group of studies has explored the effects of violence shocks on health outcomes, concentrating on the in-utero and early-childhood stages (Camacho, 2008; Mansour and Rees, 2012; Valente, 2011; Akresh et al., 2012). However, as in the case of the literature on education, little is known about the long-term consequences of this early-life exposure.

[2] Homicides are the most common offense associated with a conflict and with urban crime, and they are the primary threat to the civilian population. Homicide rates are highly correlated with the occurrence of other terrorist actions, such as attacks, explosions, landmine explosions, and abductions (Sanchez and Nunez, 2007), and they are more reliable than other measures of crime, which are not always registered by the police (Levitt and Rubio, 2000). Alderman et al. (2006) also explore the effects of early-life exposure to violence. However, the focus of their paper is to analyze the link between early-life malnutrition and future human capital. They employ an instrumental-variable approach that models child malnutrition with civil war and drought exposure in Zimbabwe.

Second, since this paper focuses on violence in early life, an important mechanism by which violence could reduce education operates through maternal health, nutrition, and stress. The fetal-origins hypothesis predicts that changes in the prenatal environment can "program" the fetus in ways that can affect adult health (Barker, 1998). Nutritional deprivation and chronic stress in pregnancy can lead to significant declines in fetal and newborn health and cognitive outcomes through changes in the immune and behavioral systems, which may in turn lead to permanent alterations in the body's systems (Denckel-Schetter, 2011; Gluckman and Hanson, 2005). For instance, Aizer et al. (2012) found that in-utero exposure to elevated levels of cortisol, negatively affects the cognition, health, and educational attainment of the offspring.

Third, stress at later stages may compromise the family environment through its effects on parental mental health and family relationships, which are likely to affect human capital outcomes (Campbell et al., 1991). Fourth, violence could also increase the risk of malnutrition. Recent studies have shown that early exposure to war reduces birth weight (e.g. Camacho, 2008; Mansour and Rees, 2012; Brown, 2014) and height-for-age (Valente, 2011; Akresh et al., 2012; Bundervoet et al., 2009). Others have found that malnutrition in childhood (i.e., low height-for-age) is associated with delayed school enrollment, fewer grades attained, and lower probability of enrolling in school (Glewwe et al., 2001; Bundervoet, 2012; Alderman et al., 2006).

1.3. Colombia's Recent Violent History

Colombia's 50-year armed conflict started in the 1960s with the creation of its communist guerrillas *Fuerzas Armadas Revolucionarias de Colombia* (FARC) and *Ejercito de Liberación Nacional* (ELN), but it was not until the 1980s that violence in Colombia massively escalated. At that time, there was an unprecedented rise in the homicide rate that was triggered by the emergence and consolidation of drug cartels that responded, in part, to an increase in the international demand for cocaine. These cartels operated in the largest urban areas, Bogota, Medellin, and Cali. Representing just 30% of the total population, these areas accounted for nearly 40% of the national homicides (Levitt and Rubio, 2000). By 1991, the country's level of violence peaked with 0.7 homicides per every 1,000 inhabitants. The country achieved the highest homicide rate in Latin America, and Medellin became the most dangerous city in the world (Borrell, 1988, see Figures 1 and 2).

Apart from an increasing international demand for drugs, particularly from the U.S. and Europe, other factors contributed to explain the emergence and consolidation of drug cartels. These included the breakdown of Colombia's criminal justice system and the enormous profits derived from the drug trade (Vargas, 1999; Levitt and Rubio, 2000; Bagley, 2001; Sanchez and Nunez, 2007). In the early 1990s, police efforts dismantled the Medellin and Cali cartels, but the power vacuum was rapidly filled by other violent nonstate actors: the paramilitaries—groups that emerged to protect wealthy landowners from guerrilla threat—and the aforementioned FARC and ELN guerrillas. Both paramilitaries and guerrillas took over Colombia's cocaine market, extending it to many rural areas, and escalated the armed conflict to one of its bloodiest stages during the latter half of the 1990s and early 2000s.

From 2002 to 2010, the dynamic of the conflict changed. After the failure of the peace dialogues with FARC in 2001, the government implemented new military strategies to combat the guerrillas and destroy their illicit crops (Pizarro, 2011). Many combatants from FARC quit fighting, the government gained control in many rural areas, and the paramilitaries demobilized. The new government actions lead to a drop in the homicide rate that can be seen in Figures 1 and 2. In spite of significant military and police improvements, by the end of the 2000s Colombia was still a violent country, ranked fifth in the world by violent deaths per capita (Declaration, 2011). In this study, I exploit the large variation in violence across municipalities and over time to estimate the effect of violence on education.

2. Data

To investigate the effects of violence on education, I use 2005 Census data obtained from IPUMS-International (MinnesotaPopulationCenter2011). The Census includes a representative sample of four million individuals, accounting for 10% of the total population, and provides rich demographic and socioeconomic information on each member of the household. One virtue of using the Census is that it reports the municipality[3] and date of birth for each individual, and the municipality where an individual's mother lived while she was pregnant. This information enables me to identify the level of violence to which a person was exposed in-utero. Since I cannot identify the individual's exact location after birth, I assume that a person's location during early childhood is the same as his/her municipality of birth. I discuss how selective migration might affect my results in the Robustness Checks section (5).

Data on violence come from the National Police Department and include all homicides that occurred in each of the 1,100 municipalities in Colombia since 1979.[4] The homicide rate is defined as total number homicides per thousand inhabitants in a municipality in a year.

The outcome of interest in this study is educational attainment. I consider three measures:

1. Years of schooling, a discrete variable, indicates the total number of completed years of education that an individual has attained. This outcome is measured for adults between 19 and 23 years of age. I focus on this

[3]Colombia has 1,100 municipalities, less than half of which have populations of less than 20,000 inhabitants. The Census groups these low-population municipalities into larger entities, making the total number of municipalities in the Census 532. Combining municipalities in this manner does not represent a problem for data analysis since each municipality in the grouped areas is clearly identified.

[4]This study uses data from the Vital Statistics Death Records and contrasts these numbers with homicide records from the National Police Department, thereby making the violence measure used here more reliable.

age group (the oldest age group in my sample) since the data on violence begins in 1979.

2. School enrollment, a dummy variable, takes the value of one when a child is reported to be attending an academic institution and zero otherwise. This outcome is measured for children aged 14 to 18 years.
3. Child is behind grade, a dummy variable, takes the value of one when a child's age minus his/her years of education is more than six (the age at which by law all children must be enrolled in first grade of primary education General Education Law in Colombia, 1994); and zero otherwise. This outcome is measured conditional on being enrolled in school for those between 14 and 18 and it reflects whether a child is not on track in the school cycle.

Table 2 presents descriptive information on the sample composition of young adults and children. Results show that the average years of schooling for the sample 19 to 23 years of age is 8.3 and 63% of those between 14 and 18 are enrolled in school, of whom 40% more than half are behind grade. In terms of their demographic and socioeconomic characteristics, 81% are White, 10% are Black, and 6.5% are indigenous populations. Table 2 also provides information on mothers' characteristics for the sample of 14- to 18-year-olds—those more likely to live with their parents. Mothers are 44 years of age, 82% are White, 11% are Black, and 7% are of indigenous descent. Less than 45% of mothers are married, 33% cohabitate, 8% are single, and 14% are either divorced or widowed. As expected from the general population, the sample is highly urban, almost 60% of mothers live in urban areas. Lastly, Table 2 shows the GDP per capita (1.35 million pesos[5]), the average years of schooling of education of the adult population (those of age 30) in each municipality and year (6.21), and the average homicide rate to which children and young adults in the sample were exposed to in their early life (0.51 homicides per 1,000 inhabitants).

[Table 2 around here]

Table 2 also shows the descriptive characteristics by whether children were exposed to high or low violence in early life. High (low) violence is defined as whether an individual was exposed to an average homicide rate above (below) the average homicide rate in the period of interest which was 0.51 homicides per 1,000 inhabitants. These differences suggest that children tend to have better outcomes if they were exposed to less violence in their early-life. For example, young adults report more years of schooling and children are more likely to be enrolled in school if they lived in less-violent areas decades ago. Children are also more likely to be behind grade if they were exposed to low-violent in their early lives. Mothers of children exposed to high homicide rates are more likely to be White and less likely to be Black or a member of an indigenous group. Perhaps the most striking difference is that mothers of high-violence-exposed

[5]The exchange rate is approximately 2,000 pesos = $1 US.

children are more educated and are more likely to be single. Lastly, differences at the regional level indicate that violent areas are also wealthier and have a slightly less-educated population (although this difference is very small). These differences in the raw data point to the importance of controlling for individual and maternal characteristics (in addition to time and geographic factors) in order to identify the effect of violence on education.

3. Methods

In the absence of experimental data, this study relies on exploiting the temporal and spatial variation in cohorts exposed to Colombia's violent conflict. The validity of this method depends on the assumption that changes in violence within municipality–year are uncorrelated with other determinants of educational outcomes. While this assumption cannot be tested directly, I will provide indirect evidence that this assumption holds and that it is feasible to estimate the impact of violence on education using this approach.

I use two empirical models. The first is a linear model that controls for a rich set of covariates and municipality, year, and month of birth fixed-effects, and department (equivalent to a state in the U.S.) time trends. The second accounts for sibling fixed-effects. Controlling for sibling fixed-effects helps remove the potential bias associated with both the probability that a family resides in areas prone to violence and with low parental investment in education. For the binary outcomes, school enrollment and whether a child is behind grade, I employ a linear probability model and a linear probability model with sibling fixed-effects, respectively.[6] The following equation describes the linear specification:

$$Y_{imjdtb} = \beta_0 + \gamma_k \, Violence_{j,t+k} + X'_{im}\beta_1 + Z'_{jdt}\beta_2 + \alpha_j + \alpha_t + \alpha_b + \mu_d(t) + \varepsilon_{imjdtb}, \quad (1)$$

where the subscript i refers to an individual, m to the mother, j municipality of birth, d department of birth, t year of birth, and b month of birth. The variable $\boldsymbol{Y}$ denotes the educational outcome. The term *Violence* represents the homicide rate observed in municipality of birth j and year of birth t for each individual and will denote the level of violence to which that individual was exposed to at birth. The subscript k takes the value of -3, -2, -1, 0, 1, 2,..., 5, representing each year of age, from (prior to) birth to age 5.[7] So for instance, the homicide rate to which an individual was exposed to at age 1 will be given by the homicide rate observed in year $t+1$ where k takes the value of 1. $\boldsymbol{X}$ is a matrix of individual and maternal characteristics that includes dummies for child's gender, child's and mother's race (White, Black, indigenous, unknown), mother's age (less than 26, 26–35, 36–45, 46–55, more than 55), education (less than primary, primary,

[6] Using other functional forms such as a logistic regression provided marginal effects that were almost identical in magnitude to those obtained from the linear probability models.

[7] The period $t = -2$ will capture the effects of a violence shock experienced prior to conception on the mother's health or on the household's welfare, which in turn may affect health during pregnancy (Camacho, 2008; Leon, 2012).

secondary, more than secondary), marital status (married, cohabiting, divorced, widowed, single, unknown), and an indicator for whether the household is urban versus rural. $\boldsymbol{Z}$ is a vector that includes economic controls such as the GDP per capita at the department level[8] and a proxy for investments in education at the municipality level.[9] These regional measures help to account for differences in economic development and educational investments across municipalities and departments. The terms α_j, α_t, and α_b are vectors of municipality, year, and month-of-birth fixed-effects, respectively, and ε is the random error term. The municipality fixed-effects control for any time-invariant municipality level factors correlated with both violence conditions and with the provision and quality of education. The year and month of birth fixed-effects will absorb year and month-specific factors that could affect both the whole economy and an individual's education. They also control for national time trends in the dynamics of violence. $\mu_d(t)$ is a flexible department-specific (cubic) time trend intended to capture differences in long-term development and in the outcomes over time and across departments. Errors are clustered at the year and municipality levels to account for the within-municipality-year serial correlation in the observations. The key coefficient of interest is γ_k. I estimate Equation 1 separately for each specific year of exposure to violence in early-life.

To estimate sibling fixed-effects models, I use Equation 2. The only covariates included in this model are child's gender and age (in matrix $\boldsymbol{X}$), the indicators for economic development and investments in education (in vector $\boldsymbol{Z}$), the municipality, year, and month of birth dummies (the vectors α_j, α_t and α_b, respectively), and the department time trends.

$$Y_{imjdtb} = \beta_0 + \gamma_k \, Violence_{j,t+k} + X'_{im}\beta_1 + Z'_{jdt}\beta_2 + \alpha_j + \alpha_t + \alpha_b + \mu_d(t) + \varepsilon_{imjdtb}, \tag{2}$$

In addition to the effects of violence during the period of interest, I run augmented versions of Equations 1 and 2 in which I also include the impact of the homicide rate two and three years before birth ($t = -2$ and $t = -3$). Including these prior-to-conception homicide rates serves as a placebo test for the identification strategy; in theory, they should not be associated with an individual's later-life educational attainment.

4. Results

4.1. Long-Term Effects of Violence

Figures 3–6 show the effects on education of violence experienced in early-life. Results are presented as profiles, where I plot the effect of violence associated with each year of exposure from age -1 (the year an individual was in-utero)

[8]The GDP per capita is only available at the department level for the period of interest.

[9]This proxy is constructed as the average years of education of the adult population in each municipality and year. Here I assume that the adult population, those over 30 years of age, has completed their investments in education. Using other reference ages for the adult population (e.g., 27, 35) does not affect the coefficient of interest β_t.

to age 5. In each figure, the dot represents the coefficient β_t obtained from Equation 1 (and Equation 2). The 95% confidence intervals are depicted by the standard error bars.

4.2. Years of Schooling

Figures 3 and 4 present the effects of violence on years of schooling using the baseline specification (Equation 1) and the model with sibling fixed-effects (Equation 2). Results show that exposure to violence has a significant and negative impact on future years of education. I find that a one standard deviation increase in homicide rates (i.e., 0.61 homicides per 1,000 inhabitants) while in-utero (at $t = -1$) reduces the years of education by 0.05 and that exposure during childhood (ages one to five) is associated with a 0.08 year decline for each year of exposure. 0.08 years of schooling is equivalent to a 1.0% with respect to the mean or a 0.02 of a standard deviation; however, an individual who is exposed to more than one year of high homicide rate in early-life can experience up to 0.4 fewer years of schooling as an adult. To put these results in perspective, Barro and Lee (2013) document that the average years of schooling in Latin America increased by just 1.07 from 2000 to 2010. Moreover, the figures also show that placebo test (homicide rates two and three years prior to birth) shows no impact of violence on years of schooling providing some evidence on the validity of the identification strategy.

My estimates are consistent to those found in previous studies. For example, Akresh and de Walque (2011) found effects around 0.5 fewer years of education (Rwanda); Shemyakina found 0.6 (Tajikistan); Rodriguez and Sanchez (2012) found 0.2 (Colombia); and Chamarbagwala and Morán (2011) found effects between 0.12 and 1.09 (Guatemala). Leon (2012) also focused on the prenatal and early-childhood periods and found that violence in Peru reduced years of schooling by at least 0.3.

The findings from the sibling fixed-effects model show consistent but larger estimates, which could suggest that controlling for unobserved family time-invariant characteristics is important to identify the effect of homicide rates on education as it helps remove the potential bias associated with both the probability that a family resides in areas prone to violence, and with low parental investment in education.[10] Almond et al. (2009) found that impacts of exposure to radioactive fallout from Chernobyl were stronger within families, comparing an exposed with an unexposed sibling. They interpreted this finding as evidence that parents could have reinforced the negative impact of the childs human-capital shock through lower parental investment. I find that a one standard deviation increase in the homicide rate is associated with a 0.09 to 0.18 decline in years of schooling.

Although the sample I used to estimate the sibling fixed-effects models corresponds to only 45% of the total sample of young adults, this group has significantly better socioeconomic characteristics than those in the full sample. For

[10]The GDP per capita is only available at the department level for the period of interest.

example, they are more likely to have more-educated mothers and to live in families where parents are more likely to be married (these summary statistics are not shown here), which is consistent with the idea that in Colombia many young adults after age 18 stay at home with their parents to attend college. Thus, the effects of violence on education for this group may result in a lower-bound estimate of homicide rates. In what follows, I focus the discussion on the sibling fixed-effects models given that they provide more compelling evidence for the effect of violence on education.

[Insert Figures 3 and 4 around here]

4.3. School Enrollment and Being Behind Grade

Figures 5 and 6 show findings on school enrollment and on whether a child is not on track for those between 14 and 18 years old, using linear probability models with sibling fixed-effects. The results indicate that educational outcomes are particularly sensitive to violence exposure around birth. For example, I find that children living in areas with a high homicide rate around birth (i.e., a one standard deviation increase in violence) experience a decline in the probability of school enrollment by almost 0.01 and that the probability that a child is behind grade, conditional on being enrolled in school, increases by 0.02. These estimates are equivalent to a 1.6% and a 4.7% decline with respect to the mean. The effect of violence on whether a child is behind grade could be considered as a milder impact than that on school enrollment since it is conditioned on this outcome being one.

These results provide some information on the potential mechanisms by which violence operates. Since the effect is concentrated around birth and not particularly after, maternal conditions during pregnancy such as stress, health, or nutrition are likely to play a significant role.

[Insert Figures 5 and 6 around here]

5. Robustness Checks

5.1. Continuous Exposure to Violence over the Life Course

So far in this study, I have investigated the unconditional impact of violence on education in each year of an individual's early-life. I now ask whether these effects are robust to exposure at other stages (i.e., primary-school and high-school ages). Exploring how violence affects different stages over the life cycle is important because individuals could be continuously exposed to violence or be exposed in different stages in their lives. Thus, identifying sensitive periods could help inform policy-makers on how to target efforts and resources.

To test for whether the effects found in this study are robust to the inclusion of posterior exposure to violence, I estimate Equation 3.

$$Y_{imjtb} = \beta_0 + \sum \gamma_k \, Violence_{j,t+k} + X'_{im}\beta_1 + Z'_{jdt}\beta_2 + \alpha_j + \alpha_t + \alpha_b + \mu_d(t) + \varepsilon_{imjdtb} \quad (3)$$

The only difference with respect to Equation 1 is that I now condition each individual exposure to violence at a given age, to previous and subsequent exposure to violence. The term $\sum \gamma_k \, Violence_{j,t+k}$ adds each individual vector of violence from in-utero (t = -1) to a year prior to 2005 (year in which the Census was collected). So for instance, the educational attainment of a 20-year-old person in 2005 is regressed on the homicide rate he or she was exposed to while in-utero, at birth, at age 1, and so on until the homicide rate experienced at age 19. By including all homicide rates at different years, this model also tests for the presence of serial correlation in homicide rates within municipalities. Figure 7 shows the results of Equation 3 on years of schooling. The findings indicate that exposure to violence during early-ife has the most pronounced impacts on future years of education, compared to exposure at other stages. The magnitude of the coeffients are consistent to those found in Figure 3; for each year of exposure to a high homicide rate in early-life (an increase of one standard deviation), years of schooling decline by 0.09. [11] Leon (2012) also finds stronger impacts of violence when exposure occurs prior to school entry and small or no effects when individuals are exposed after preeschool-age.

[Insert Figure 7 around here]

5.2. Effects of Violence on Health

A question that I also ask is whether violence exposure could affect individual health as previous studies have shown (Akbulut-Yuksel, 2011; Camacho, 2008; Valente, 2011; Brown, 2014). Using Census data that include a battery of health questions, I construct an indicator for whether an individual suffers from a permanent disability,[12] and I test whether changes in exposure to homicide rates are associated with changes in this outcome. I found that exposure to violence during the first years of life increased the probability of having this condition by 0.3%. While having a permanent disability is a very extreme health condition, this result could suggest that even more subtle health problems might also be associated with violence exposure. Results are not shown but are available upon request.

5.3. Sources of Selection Bias

To correctly estimate the effect of exposure to violence on education, this paper analyzes four potential sources of selection bias: geographic sorting, mobility, fertility, and survival.

[11] Results on school enrollment and on the likelihood that a child is behind grade provide similar findings to those observed in Figures 6 and 7. Even after controlling for exposure to violence at other stages in life, violence around birth has the most pronounced impact on these outcomes. Results on school enrollment and child being upgraded are not shown here but are available upon request.

[12] The battery of health questions in the Census data ask about specific disabilities, such as, blindness, deafness, muteness, and disabilities affecting upper extremities, lower extremities, personal care, mobility, mental health, and psychological limitations. Using these extreme conditions, I constructed a dummy variable that indicated whether the individual suffers from any disability. Approximately 3% of the sample reports to have any permanent disability.

Geographic sorting. Table 2 shows statistically significant differences in the sample composition by violence exposure in early life. Although these differences could be due in part to violence, there are other factors correlated with violence that could also affect them. For instance, violence-prone municipalities could be disadvantaged in other ways: They might be poor or provide few public goods. A way to control for these differences is to include municipality and year fixed-effects in the regression. However, if these differences are time-varying, or if there are differences into who is affected by violence within municipalities, then the fixed-effects would not account for these differences and there will be selective sorting within areas.

To test for the presence of selective sorting, I performed a simple test comparing the characteristics of mothers exposed to violence during pregnancy (and during their child's early years) to those not exposed in these periods. In the presence of selective sorting, the association between violence exposure and mother characteristics should be significant even after controlling for municipality and year fixed-effects. Equation 4 presents the model I used to test for selective sorting.

$$X_{imjtb} = \beta_0 + \gamma_k \, Violence_{ij,t+k} + \alpha_j + \alpha_t + \alpha_b + \varepsilon_{imjtb}, \tag{4}$$

where $\boldsymbol{X}$ represents a set of dummy variables for mother's age, race, education, and marital status as described in Equation 1;[13] *Violence* refers to the homicide rate observed during the year of pregnancy and during each of the child's early years; α_j, α_t, and α_b represent fixed-effects for the child's birth municipality, year, and month, respectively.

Appendix Table 1 shows the results on selective sorting. In general, the coefficients on violence show little evidence on selective sorting based on mother's age, race, and education after accounting for municipalities fixed-effects. While some coefficients are significant, for example, mother's age, being a minority, or having less than a high school education are negatively associated with violence, results show that after controlling for these geographic time-invariant characteristics (as well as year and month FE) absorbs a big portion of this potential source of selection bias.

Mobility. Another potential source of selection bias is related to endogenous migration in response to (or in expectation of) high violence. Selective migration could be a problem for my analysis if households who migrate due to high violence are different from those who do not migrate in ways that could potentially affect a child's education. For instance, if families who migrate are wealthier or more educated than those who stay, then I could overestimate the negative effect of violence.

Since the Census does not provide information on households' migration history, which would allow me to test with high precision a family's migration

[13]The sample only focuses on the mothers of children between 14 to 18 because this group of children still live with their parents and so I can control for their mothers' characteristics in the regression models.

response due to violence, I examine whether some families are more or less likely to change their place of residence after being exposed to a high homicide rate in particular moments in time. In order to perform this test, I select all the mothers in my sample (the mothers of the sample of children and young adults I analyze here), and I examine whether a mother experiencing high violence during her pregnancy (or during her child's early years) predicts that she moves to another municipality.[14]

Equation 5 describes the model I use to test for the presence of selective migration.

$$Y_{imjtb} = \beta_0 + \gamma_{k1}\, Violence_{ij,t+k} + \gamma_{k2}\, Violence_{ij,t+k} \times X_{im} + \beta_1 X_{im} + \alpha_j + \alpha_t + \alpha_b + \varepsilon_{imjtb} \quad (5)$$

where $\boldsymbol{Y}$ is a dummy indicator that takes the value of 1 when a mother m changes her place of residence, and 0 otherwise. So for example, a mother would be classified as a migrant mother (the outcome equals 1) if she gives birth to her child in Bogota and then she is sampled in Medellin in 2005.[15]

Violence is measured during pregnancy, and in child's age 1, 3, and 5. $\boldsymbol{X}$ includes mother characteristics as in the main specification. The terms α_j, α_t, and α_b control for the first child's municipality of birth fixed-effects (i.e., baseline municipality of a mother's residence), and for a child's year and month of birth fixed-effects. The coefficient of interest, γ_{k2}, identifies whether there is selective migration on that specific mother characteristic. Appendix Table 2 shows results on selective mobility for both the sample of mothers of young adults and those of children, when exposure to violence occurs in pregnancy, and in child's age 1, 3, and 5. In general, I find little evidence of selective migration in terms of mother characteristics. This is shown by the lack of significance in almost all the interaction terms in the regression and by the test of joint significance of the model (the F-statistic is relatively high, 12 or above).

While these tests for selective migration find little evidence for endogenous migration, it is true that internal forced displacement in Colombia has been one of the most dramatic consequences of the armed conflict.[16] Identifying internally displaced populations (IDP) in the Census 2005 is only possible for those who have recently been forced to move (in the last 5 years).[17] Although

[14]For mothers with two or more children between 14 and 23 in 2005, I focus on how exposure to violence during a mother's pregnancy of her first child (or during the early stages of her first child) predicts that she moves to another municipality before she gives birth to her second child.

[15]For mothers with two or mother children, the outcome would take the value of 1 if, for example, a mother's first child is born in Bogota and her second child is born in Medellin. I restrict the sample to those cases who report to be living in their child's municipality of birth.

[16]The total displaced population in the country reached over 3.5 million since 1997, which is equivalent to 8% of the total population (United Nations High Commissioner for Refugees, 2010). Displaced groups commonly have very low socioeconomic indicators, including educational attainment and health status.

[17]This is consistent with the fact that forced migration is a relatively recent consequence of the conflict. Statistics from Acción Social reveal forced displaced populations since the 1990s.

I cannot identify IDPs prior to year 2000, the number of IDPs in the Census is small (they represent 5% of the recent migrants and are approximately 2% of the total sample), providing some additional evidence that internal migration in the sample is not particularly driven by violence.

In addition to these two pieces of evidence—the multivariate analysis and the fact that IDPs are a relatively small portion of the Census—I also test whether the estimates of violence on educational outcomes are robust to controlling for the proportion of IDPs that was expelled from each municipality and year in Colombia.[18] Results indicate that controlling for IDPs in Equation 5 does not substantially change the estimates of homicide rates on education. Lastly, I estimate Equation 5 for both the sample of movers (those who were born in a different municipality to where they were sampled in 2005) and non-movers. Results are remarkably similar, suggesting that the estimates of violence on education may not suffer, substantially, from selective migration. These findings are not shown here due to space limitations, but they are available upon request.

Fertility. Violence can also affect a woman's fertility decisions by either reducing the number of children she wants to have or by delaying her decision to become pregnant. If violence affects some women more than others, this may result in a biased estimate of the impacts of violence on education.[19] For example, if more educated women are less likely to have an additional child in the presence of high violence, then those more likely to have children are the less educated mothers who are less likely to invest in their children's education even in the absence of violence.

In order to test for the presence of this potential source of selection bias, I examine whether violence is associated with the number of children a mother has, by observable characteristics. Equation 6 describes the model used to test this hypothesis.

$$X_{imjtb} = \beta_0 + \gamma_{k1}\, Violence_{ij,t+k} + \gamma_{k2}\, Violence_{ij,t+k} \times Nchild_m \\ + \beta_1 Nchild_m + \alpha_j + \alpha_t + \alpha_b + \varepsilon_{imjtb} \quad (6)$$

where *Nchild* represents a mother's report on her number of children in 2005; the interaction term between violence and the number of children indicates whether there exists selective fertility on that specific maternal characteristic. The term α_j stands for oldest (first) child's municipality of birth FE, whereas α_t and α_b represent each child's year and month of birth FE, respectively. I should note that these results are conditional on both child and mother surviving in 2005, and children living in a household with their mothers.

[18]This information was obtained from the National Agency Acción Social—Cumulative IDP's (Registro Unico de Población Desplazada por la Violencia).

[19]An example of violence inducing negative selection on fertility was shown by Valente (2011). The author found that high-caste and highly educated women in Nepal were relatively less likely to become pregnant after being exposed to the violent conflict, or when they anticipated more violence during the pregnancy.

Results are presented in Appendix Table 3, and they show that black mothers or those with fewer years of education (i.e., mothers with less than primary education) are more likely to have an additional child in the presence of high violence during their child's early-life. While this is evidence of (negative) selective fertility, I claim that this bias is actually small and does not represent a huge concern for my long-term estimates of violence on education. My claim focuses on the magnitude of the coefficient that acompanies the interaction term relative to the magnitude of the association between the violence and household size. For instance, in the case of years of education –perhaps the most important measure of SES –, selection on fertility only accounts for a tenth to a half of the magnitude of β_1, net fertility, suggesting that on the margin, while this estimate is statistically significant and may indice negative selection due to violence, it is very small.

Survival. Lastly, I analyze how violence affects the probability of survival. Selective survival can occur if, for instance, violence increases the probability of mortality of the frailer fetuses or children, leading to healthier babies surviving. In this case, the effects of violence on education would result in a lower-bound estimate on the true impact.

One way to test for the presence of selective survival in the Census is to explore whether violence during pregnancy (for example) affects the sex ratio or the cohort size at birth. Several studies have shown that boys are biologically weaker and more susceptible to diseases and premature death than girls (Naeye et al., 1971; Waldron, 1985), and that they are more vulnerable to environmental factors (Pongou, 2013). Hence, exploring the relationship between violence and the cohort size or the sex ratio can help understand whether there exists selection on mortality. Equation 7 shows the model used to test for this type of threat.

$$Y_{jt} = \beta_0 + \gamma_k \, Violence_{j,t+k} + \alpha_j + \alpha_t + \varepsilon_{jt,} \tag{7}$$

where $\boldsymbol{Y}$ is measured using the sex ratio[20] or cohort size[21] in municipality j and in year t; *Violence* is measured in $t + k$ as in equation 1. Appendix Table 4 shows the associations between violence in-utero, at birth, and in ages 1 through 5, and the sex ratio and cohort size. The findings show that, except for a few cases where violence is positively associated with an increase in cohort size, in general, violence does not seem to be associated with changes in the sex ratio or in the cohort size, which could suggest little evidence for selective mortality.

6. Discussion and Conclusions

A growing body of literature has shown that shocks in the prenatal, infancy, and childhood periods can have long-term consequences on future health, education, and wages. Despite of this growing evidence, most studies investigating

[20] The ratio of males to females.

[21] The count of all births that occurred in a given municipality and year.

the effects of violence on education have focused on individuals who were exposed at school age, and little is known about how violence exposure in other life stages impacts long-term individual outcomes.

I investigate the consequences of exposure to high violence, from in-utero to current age, on future educational attainment. I moved beyond previous research by focusing on early-life exposure and by using a more precise measure of violence—the homicide rate—than used in most previous studies. My focus on Colombia provides an interesting case to study due to the large variation in crime and violence associated with the proliferation of drug cartels and the recrudescence of its armed conflict. I found that exposure to homicide rate while in-utero reduced the years of schooling by 0.05, and that exposure during childhood (ages one to five) was associated with 0.08 fewer years of schooling for each year of exposure. So an individual who lived throughout his/her early life in a violent municipality is likely to attain approximately 0.4 fewer years of schooling as an adult, than someone who was not exposed. Similarly, I found that children exposed to a high homicide rate around birth are less likely to be enrolled in school (by 4 percentage points) and more likely to be behind grade (by 6 percentage points). Most important, results show that exposure during the prenatal to childhood periods have the most pronounced impacts in terms of future losses in education, compared to exposure at other stages in life.

While the results for years of schooling and for child school enrollment were obtained from two different samples (individuals between 19 and 23 years of age and children between 14 and 18) and from to two different waves of violence, they can be reconciled. The estimated effect of exposure to violence on school enrollment around birth (a decline of between 1 and 2 percentage points) is equivalent to an expected loss in 0.1 years of education, which is within the estimated impact of violence around birth on an individual's future years of schooling. Some back-of the-envelope calculations using Galdo's (2013) estimates on the effects of violence on adult monthly earnings for the case of Peru's conflict suggest that the decline in years of schooling in Colombia could be associated with a 2.5% in future wages.

This study faces several limitations. First, since the data on homicide rates is at the yearly level, I cannot identify with more precision critical periods of an individual's life that can be affected by violence. For example, I cannot explore the effect of a high homicide rate in each trimester while in-utero, which could provide some suggestive evidence for potential mechanisms (e.g., maternal stress, maternal malnutrition; see Camacho, 2008; Mansour and Rees, 2012; Brown, 2014). Second, while Census data contain rich information on a large and representative sample of Colombians, they do not include data on where the children lived throughout their early life (nor information on household migration). Nevertheless, I have provided evidence suggesting that results are not driven by selective migration, and in any case, the bias seemed to be small, resulting in a lower-bound estimate of the true effect. Third, although homicide rates are highly correlated with the occurrence of other terrorist actions (e.g., massacres, landmine explosions, etc.) and they are more reliable than other measures of crime that are not always registered by the police, it is also true that exposed cohorts were likely to be exposed to other terrorist attacks during their early life, which could have caused significant declines in their well-being, accentuating the effects of municipality homicide rates. Since I did not account for other violent events, it is likely that the estimates of homicide rate could overestimate the impact of violence on education.

The results shown in this paper have important public-policy implications. First, they contribute to the growing evidence suggesting that conditions experienced during early life have a long reach; second, they offer some suggestive evidence that could help inform policy-makers of when in the life course interventions targeted to mitigate impacts of violence can be most effective. Future research that investigates the link between violence and human capital should focus on determining the relevant pathways of transmission by which violence affects human capital accumulation, particularly those operating at the household/individual level—about which less is known—and on identifying effective public policies that help mitigate the negative impacts of violence. Another extension would be to explore the effects of violence on other domains of human capital, for example, on cognitive and non-cognitive skills.

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Table 1: Effects of Violence on Education

Study	Source of violence / Violence measure	Age of exposure	Outcome / Findings
Akbulut-Yuksel (2011) Data: GSOEP; $N = 3{,}744$ Methods: Difference-in-difference; interaction between city-level intensity of WWII destruction and dummy for being school-aged during WWII	Germany during the WWII Bombing intensity across regions	Children 6 and older (includes those who were of primary school age and upper secondary school age during the war years)	School-aged children exposed to WWII have 0.4 fewer years of schooling in adulthood, with those in the most affected cities completing 1.2 fewer years.
Akresh and de Walque (2011) Microdata: DHS 1992 and 2000; $N = 45{,}642$ Methods: Difference-in-difference; Treatment group: School-aged children in year 2000 Control: School-aged children in year 1992	Rwanda's 1994 genocide Exposure to the genocide is measured using a dummy for year (exposed if observed in DHS 2002 and 0 o.w.); Genocide intensity is measured at the geographic level using the number of deaths.	Children 6 and older	School-age children exposed to the genocide experienced (a) a drop in 0.5 years of schooling and (b) a 15 pp decline in the likelihood of completing 3rd or 4th grade.
Caudillo and Torche (2014) Data: School Census panel data of all elementary schools in Mexico (1990–2010); $N = 84{,}404$ schools Methods: Linear model with rich controls; GSIS models that account for group-specific intercepts/slopes	Mexico's recent increase in violence (period of analysis 1990–2010) Homicide rates	Children 6–11	An increase in 1 homicide per 1,000 inhabitants increased grade failure in primary education by 0.003.

table continues

Study	Source of violence / Violence measure	Age of exposure	Outcome / Findings
Chamarbagwala and Morán (2011) Data: Census 2002; $N =$ 4.5 million Methods: Difference-in-difference Treatment: Indiv who were at school age in high conflict zones during the 3 periods of the conflict; Control group: Those who lived in low conflict areas	Guatemala 36-year conflict (1960–1996) Using victim's self-reports two measures of exposure to the conflict were constructed: 1) the number of human rights violations in each department; 2) the number of victims in each department (province).	Children 7–17	Being exposed to the conflict (a) reduced the years of schooling for all and (b) most affected were the Mayan and female rural populations: declines up to 1.09 in years of schooling for males and up to 1.17 for females.
Ichino and Winter-Ebmer (2004) Data: See Appendix Table A1 in their paper. Methods: Test the existence of significant breaks in the evolution of deviations from the trend in education across countries (Germany and Austria versus Sweden and Switzerland)	WWII Living in Germany or Austria vs. Sweden or Switzerland during the WWII	Children at age 10	Exposed children achieved 0.2 fewer years of schooling.
Leon (2012) Data: Census 1997 and 2003; $N \sim 200{,}000$ Methods: Linear model with rich controls, sibling FE	Peru's 1980–1993 conflict Dummy variable for whether a victim reported any human right violation in the district	-2 to adulthood	Conflict exposure (a) prior to school-age leads to a 0.31 decline in years of schooling (in the short-term, effects are 0.98 fewer years of schooling) and (b) after age 6 has little effect.

table continues

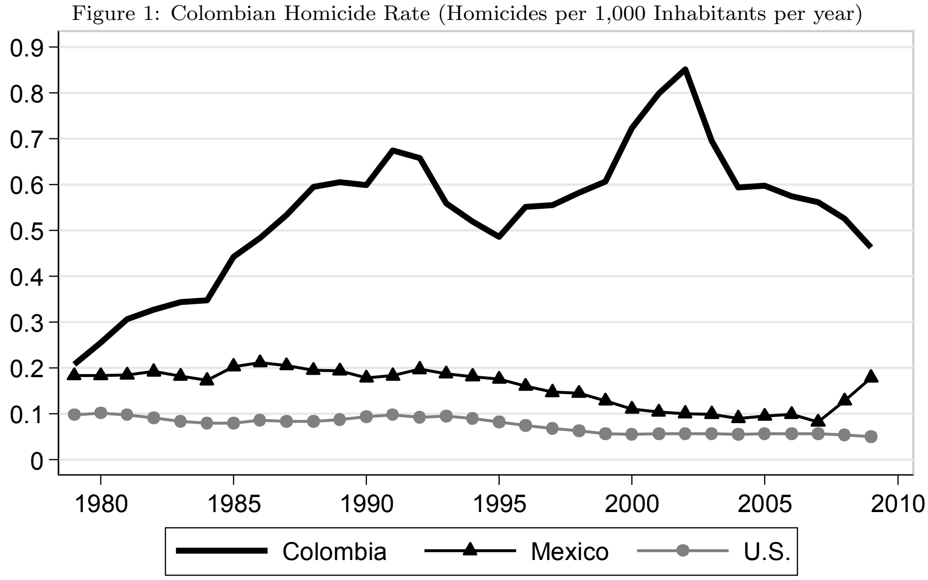
Study	Source of violence / Violence measure	Age of exposure	Outcome / Findings
Rodriguez and Sanchez (2012) Data: Living Standards Measurement Survey (LSMS); N = 20,642 Methods: Duration model and bi-probit; instrumental variables	Colombia's armed conflict 1960–present (period analyzed: 1992–2003) Accumulated violence exposure measured by the N of total attacks a child was exposed to since age 6	Children 6–17	An increase of 1-SD in the armed-conflict exposure increased the joint probability of school drop-out and child labor by 13%.
Shemyakina (2011) Data: LSMS 1999 and 2003; $N \sim 11{,}000$ Methods: Difference-in-difference Treatment group: Those who were school age (7–17) during the conflict and lived in conflict-affected areas Control group: Those that should not have been significantly affected by conflict (i.e., lived in low-conflict areas)	Tajikistan's 1992–1998 conflict Victim's self-reports on damages: 1) if a household reported damage to their own dwelling; 2) dummy indicator for whether the area had a dwelling damage; 3) dummy for whether the district experienced high vs. low violence.	Children 7–17	Conflict exposure is associated with (a) negative effects on girls and no effect on boys, (b) women achieving 0.7 fewer years of schooling and girls who were of school age had 0.5 fewer years, and girl's reduced likelihood of being enrolled in school by 12.3%.
Swee (2009) Data: Bosnian LSMS; N 5,000 Methods: Difference-in-difference Treatment group: School-aged indiv living in high areas with high war intensity Control group: Similar school-aged indiv living in low-conflict areas	Bosnian War (1992–1995) Violence exposure: N of war casualties per capita in municipality	Children 7–19	A 1-SD increase in the N of war casualties per capita reduces the likelihood of secondary school attainment by 4 pp.

Table 2: Sample Descriptive Statistics by Violence Exposure

	Full	Violence Low	Violence High	Diff
Outcomes:				
Years of schooling (M)	8.27	8.34	8.08	0.26***
	(4.012)	4.04	3.92	
N	300,221	222,001	78,220	
School enrollment (%)	62.66	63.56	61.33	2.23***
N	370,589	221,320	149,269	
Child is behind grade[a] (%)	40.28	41.44	38.49	2.95***
N	232,218	140,671	91,547	
Individual characteristics:				
Age	18.18	18.53	17.51	1.02***
	(2.89)	(2.92)	(2.71)	
Gender	49.53	49.49	49.6	−0.11
White	80.86	78.85	84.81	−5.96***
Black	10.33	12.06	9.23	2.83***
Indigenous	6.47	1.92	1.95	−0.03***
Unknown/other	2.34	7.17	4.01	3.16***
Mother characteristics:				
Age	44.18	44.59	43.42	1.17***
	(7.81)	(7.88)	(7.60)	
White	81.51	79.39	85.47	−6.08***
Black	10.98	11.91	9.23	2.68***
Indigenous	7.00	8.13	4.89	3.24***
Unknown/other	0.51	0.57	0.41	0.16*
Years of schooling	5.25	5.24	5.27	−0.03*
	(4.23)	(4.29)	(4.11)	
Married	44.13	43.96	43.36	0.60***
Cohabiting	33.26	33.91	31.72	2.19***
Divorced	8.55	8.24	8.89	−0.65***
Widowed	5.87	5.48	6.46	−0.98***
Single	8.05	7.45	9.01	−1.56***
Urban household	59.32	58.06	59.93	−1.87***
Municipality or department characteristics:				
GDP per capita	1.35	1.27	1.51	−0.24***
	(0.54)	(0.55)	(0.48)	
Proxy-investments in education[b]	6.21	6.23	6.18	0.05***
	(2.17)	(2.27)	(1.96)	
Homicide rate	0.51	0.21	1.11	−0.90***
	(0.61)	(0.15)	(0.71)	
N	670,810	583,992	319,036	

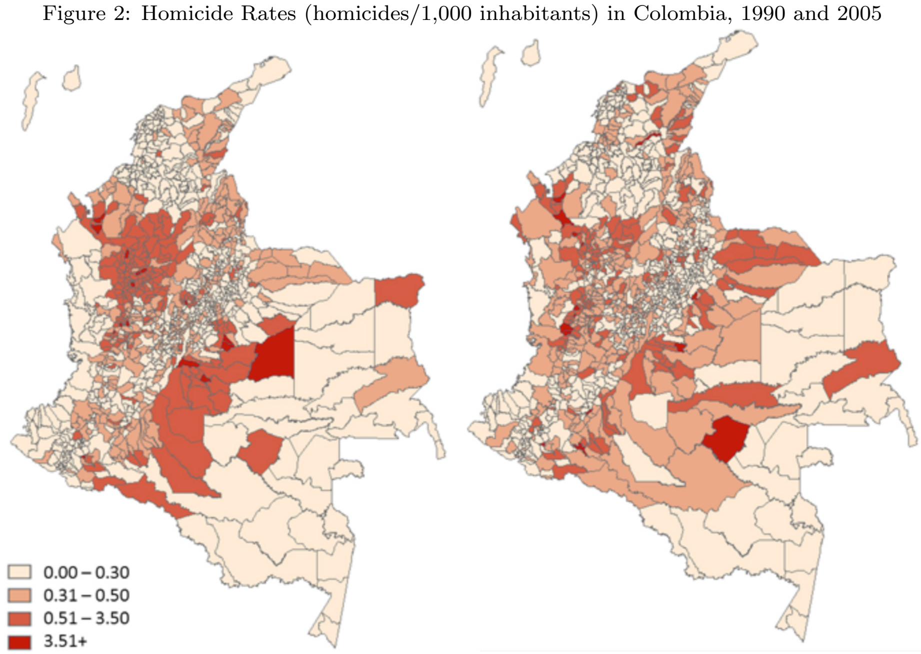
Note: N = 670,810. Years of schooling is defined for adults 19-23, and school enrollment and child is behind grade for children 14-18. Sample of mothers includes mothers of children/adults 14–23. High (low) violence refers to whether an individual was exposed to an average homicide rate above (below) the average homicide rate in the period of interest, which was 0.51 homicides per 1,000 inhabitants. Standard deviations in parenthesis. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. [a] Conditional on being enrolled in school. [b] Average years of education of the 30-year-old population in each municipality and year.

Figure 1: Colombian Homicide Rate (Homicides per 1,000 Inhabitants per year)



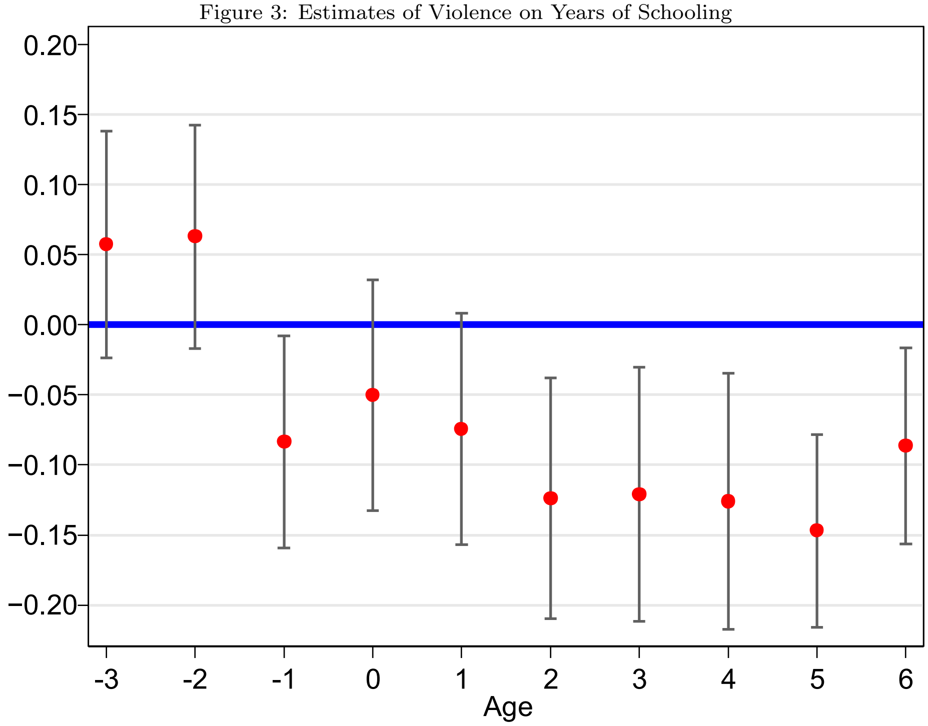
Sources: National Police Department (Colombia), Instituto Nacional de Estadística y Geografía (INEGI; Mexico), and Bureau of Justice Statistics (BJS; U.S.)

Figure 2: Homicide Rates (homicides/1,000 inhabitants) in Colombia, 1990 and 2005



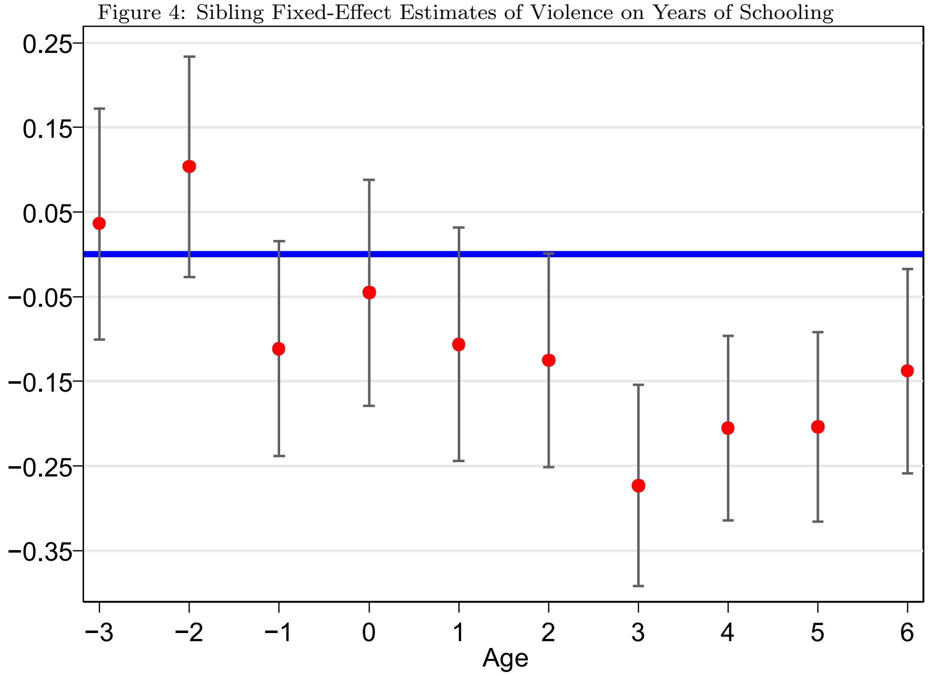
Source: Author's calculations.

Figure 3: Estimates of Violence on Years of Schooling



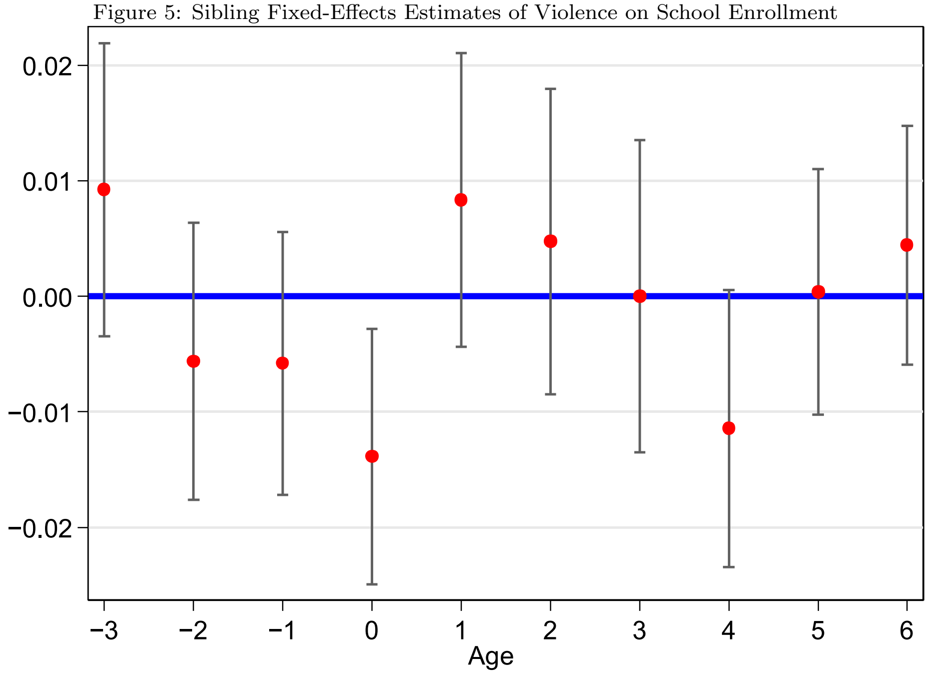
Note: $N = 300.221$. Sample includes individuals between 19 and 23 years of age. Models control for individual characteristics including: dummies for gender, child's and mother's race, mother's age, education, and marital status, an indicator for urban household, department GDP per capita, a proxy for municipal investments in education, municipality, year, and month of birth child's FE. The dots represent the effect of homicide rate on years of schooling; the standard error bars represent the 95% confidence intervals. Each coefficient was obtained from a separate regression (Equation 1).

Figure 4: Sibling Fixed-Effect Estimates of Violence on Years of Schooling



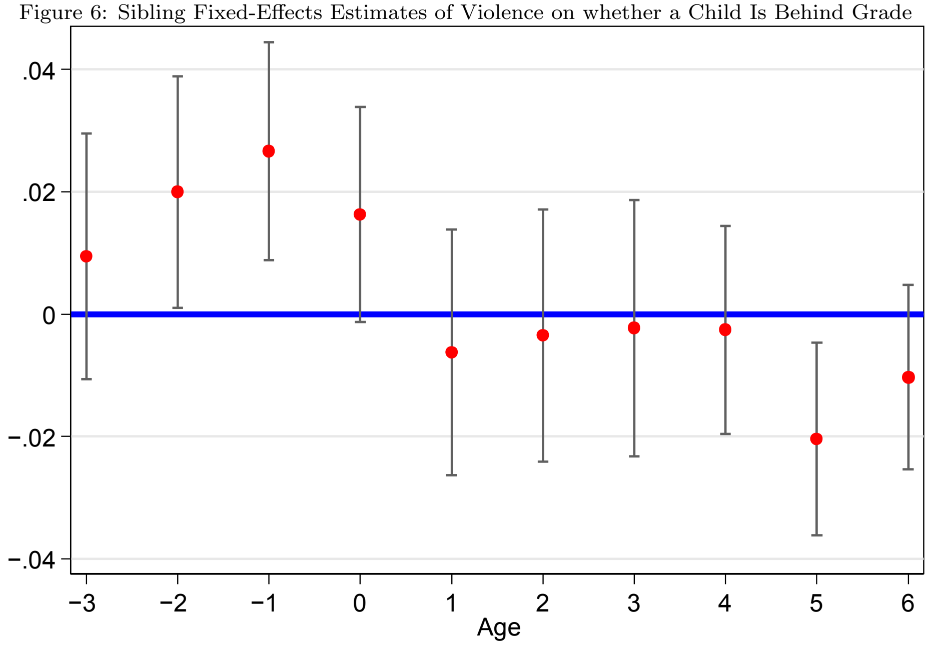
Note: $N = 135.100$. Sample includes individuals between 19 and 23 years of age who live with their parents and siblings. Models control for gender, department GDP per capita, a proxy for municipal investments in education, municipality, year, and month of birth FE, and department time trends. The dots represent the effect of homicide rate on years of schooling; the standard error bars represent the 95% confidence intervals. Each coefficient was obtained from a separate regression (Equation 2).

Figure 5: Sibling Fixed-Effects Estimates of Violence on School Enrollment



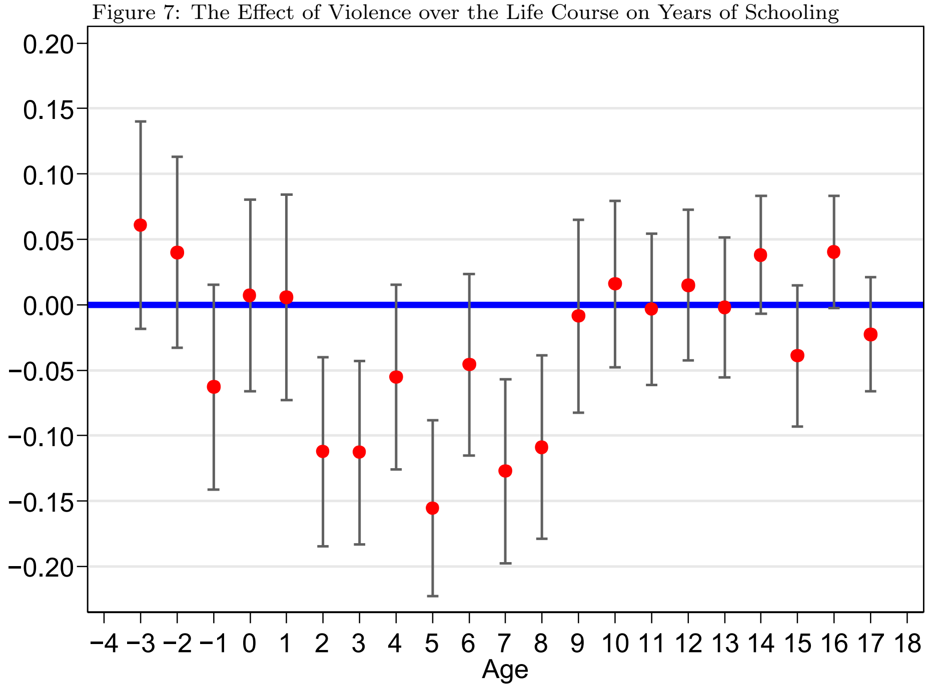
Note: $N = 370.589$. Sample includes children between 14 and 18 years of age. Models control for gender, department GDP per capita, a proxy for municipal investments in education, municipality, year, and month of birth FE, and department time trends. The dots represent the effect of homicide rate on school enrollment; the standard error bars represent the 95% confidence intervals. Each coefficient was obtained from a separate regression (Equation 2).

Figure 6: Sibling Fixed-Effects Estimates of Violence on whether a Child Is Behind Grade



Note: $N = 232.218$. Sample includes children between 14 and 18 years of age. Models control for gender, department GDP per capita, a proxy for municipal investments in education, municipality, year, and month of birth FE, and department linear time trends; models are conditioned on child being enrolled in school. The dots represent the effect of homicide rate on school enrollment; the standard error bars represent the 95% confidence intervals. Each coefficient was obtained from a separate regression (Equation 2).

Figure 7: The Effect of Violence over the Life Course on Years of Schooling



Note: N = 300.221. Sample includes individuals between 19 and 23 years of age. Models control for individual and maternal characteristics, including dummies for gender, child and mother's race, mother's age, education, and marital status, an indicator for urban household, department GDP per capita, a proxy for municipal investments in education, municipality, year, and month of birth FE, and department time trends. The dots represent the effect of homicide rate on years of schooling; the standard error bars represent the 95% confidence intervals. The effects of violence in a given age are conditioned on exposure at other ages as described in Equation 3.

Appendix Table 1: Violence and Selective Geographic Sorting

	Age	Black	Indigenous	Years of education	Less than primary education	Married
A. Without Municipality Fixed-Effects						
Violence in-utero	-0.190^{***}	-0.017^{***}	-0.016^{**}	0.095	-0.0093	-0.001
	(0.0412)	(0.0052)	(0.0067)	(0.1458)	(0.0139)	(0.0057)
Violence at child's birth	-0.182^{***}	-0.019^{***}	-0.021^{***}	0.144	-0.014	0.003
	(0.0401)	(0.0050)	(0.0059)	(0.1363)	(0.0130)	(0.0054)
Violence at child's age 1	-0.177^{***}	-0.017^{***}	-0.032^{***}	0.301^{**}	-0.032^{***}	0.004
	(0.0380)	(0.0045)	(0.0047)	(0.1187)	(0.0113)	(0.0051)
Violence at child's age 3	-0.163^{***}	-0.017^{***}	-0.040^{***}	0.499^{***}	-0.052^{***}	0.007
	(0.0384)	(0.0044)	(0.0046)	(0.0993)	(0.0094)	(0.0048)
Violence at child's age 5	-0.136^{***}	-0.015^{***}	-0.045^{***}	0.575^{***}	-0.060^{***}	0.007
	(0.0355)	(0.0043)	(0.0045)	(0.0879)	(0.0085)	(0.0046)
B. With Municipality Fixed-Effects						
Violence in-utero	-0.038	0.001	-0.002	-0.013	0.000	0.003
	(0.0346)	(0.0013)	(0.0010)	(0.0265)	(0.0002)	(0.0023)
Violence at child's birth	-0.043	0.001	-0.001	-0.022	0.003	0.005^{**}
	(0.0340)	(0.0012)	(0.0009)	(0.0236)	(0.0028)	(0.0023)
Violence at child's age 1	-0.055	0.000	-0.002^{**}	-0.026	0.004	0.004
	(0.0363)	(0.0014)	(0.0009)	(0.0224)	(0.0026)	(0.0025)
Violence at child's age 3	-0.016	0.003^{**}	-0.001	-0.054^{**}	0.011^{***}	0.004
	(0.0412)	(0.0015)	(0.0009)	(0.0247)	(0.0029)	(0.0029)
Violence at child's age 5	0.111^{***}	0.00	-0.001	-0.021	0.004	0.003
	(0.0400)	(0.0018)	(0.0010)	(0.0273)	(0.0031)	(0.0027)
N	399,090	399,090	399,090	399,090	399,090	399,090

Note: N = 399,090. Sample includes all mothers of children and adults between 14 and 23 years of age. Each coefficient is obtained from a separate regression. All regressions include fixed-effects for child's birth year and month. The regressions in Panel B also include fixed-effects for child's birth municipality. Robust standard errors are clustered at the municipality–year level. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.001$.

Appendix Table 2: Violence and Mother's Selective Mobility

	Mother Moved				
	In-utero	Age 0	Age 1	Age 3	Age 5
	(1)	(2)	(3)	(4)	(5)
Violence at ...	−0.025	−0.007	0.002	0.008	−0.027
	(0.0170)	(0.0120)	(0.0275)	(0.0265)	(0.0269)
Violence at age ...	0.023	0.010	0.0009	−0.009	0.025
× *Age 26–35*	(0.0160)	(0.0191)	(0.0267)	(0.0259)	(0.0262)
Violence at age ...	0.034**	0.020**	0.011**	0.003**	0.032**
× *Age 36–45*	(0.0157)	(0.0189)	(0.0266)	(0.0258)	(0.0261)
Violence at age ...	0.032**	0.020**	0.014**	0.009**	0.039**
× *Age 46–55*	(0.0160)	(0.0191)	(0.0268)	(0.0259)	(0.0262)
Violence at age ...	0.041**	0.029**	0.023**	0.021**	0.051**
× *Age* > 55	(0.0163)	(0.0194)	(0.0270)	(0.0261)	(0.0264)
Violence at age ... × *Black*	0.005	0.004	0.004	0.007	0.009
	(0.0054)	(0.0050)	(0.0049)	(0.0047)	(0.0041)
Violence at age ...	−0.014**	−0.016**	−0.015**	−0.011**	−0.013**
× *Indigenous*	(0.0055)	(0.0051)	(0.0053)	(0.0057)	(0.0052)
Violence at age ... × *Other*	−0.105	−0.025	−0.039	0.089	0.163
	(0.0683)	(0.0867)	(0.0749)	(0.1345)	(0.1274)
Violence at age ...	−0.0006	−0.002	−0.004	−0.008	−0.0105
× *Primary education*	(0.0033)	(0.0032)	(0.0031)	(0.0030)	(0.0030)
Violence at age ...	−0.004	−0.005	−0.010	−0.015	−0.020
× *Secondary education*	(0.0046)	(0.0043)	(0.0040)	(0.0043)	(0.0049)
Violence at age ...	−0.015**	−0.014**	−0.022**	−0.027**	−0.030**
× *University education*	(0.0072)	(0.0067)	(0.0062)	(0.0064)	(0.0072)
Violence at age ... × *Married*	0.000	−0.002	−0.002	−0.001	−0.002
	(0.0048)	(0.0047)	(0.0042)	(0.0039)	(0.0037)
Violence at age ...	0.011**	0.008**	0.009**	0.007**	0.006**
× *Cohabiting*	(0.0048)	(0.0045)	(0.0042)	(0.0039)	(0.0036)
Violence at age ... × *Divorced*	0.020***	0.015***	0.015***	0.011***	0.007***
	(0.0062)	(0.0060)	(0.0053)	(0.0052)	(0.0051)
Violence at age ... × *Widowed*	0.009	0.008	0.008	0.005	0.006
	(0.0058)	(0.0054)	(0.0049)	(0.0048)	(0.0051)
N	269,713	269,713	269,713	269,713	269,713
R^2	0.044	0.040	0.0405	0.0405	0.0405
F-statistic	12.59	16.34	15.89	15.40	15.38

Note: $N = 269{,}713$. Sample includes mothers of children and adults between 14 and 23 years of age. Results shown in each column are obtained from a separate regression. All regressions include controls for mother characteristics (age, race, education, and marital status), fixed-effects for the first child's birth municipality, and for each child's year and month. Robust standard errors are clustered at the municipality–year level. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.001$.

Appendix Table 3: Violence and Selective Fertility

	Age	Black	Indigenous	Years of education	Less than primary education	Married
Violence in-utero	0.116	−0.003	0.005***	0.029	−0.016***	0.009*
	(0.1071)	(0.0026)	(0.0019)	(0.0927)	(0.0045)	(0.0047)
Violence in-utero	0.020	0.002***	−0.002***	−0.051*	0.008***	−0.002
× *Nchild*	(0.0221)	(0.0007)	(0.0006)	(0.0276)	(0.0012)	(0.0012)
Nchild	−1.679***	−0.0006	−0.010***	−0.233***	0.026***	0.0009
	(0.0204)	(0.0006)	(0.0006)	(0.0297)	(0.0010)	(0.0010)
Violence at child's birth	0.186	−0.002	0.004**	0.1005	−0.009**	0.014***
	(0.1025)	(0.0025)	(0.0017)	(0.0877)	(0.0042)	(0.0045)
Violence at child's birth	0.012	0.002***	−0.001**	−0.050*	0.007***	−0.002*
× *Nchild*	(0.0218)	(0.0007)	(0.0006)	(0.0273)	(0.0012)	(0.0011)
Nchild	−1.676***	−0.0008	−0.011***	−0.229***	0.026***	0.001
	(0.0205)	(0.0005)	(0.0006)	(0.2289)	(0.0010)	(0.0009)
Violence at child's age 1	0.145	−0.004	−0.0005	0.182**	−0.003	0.012***
	(0.1017)	(0.0026)	(0.0016)	(0.0874)	(0.0042)	(0.0046)
Violence at child's age 1	0.006	0.002***	0.000	−0.102***	0.007***	−0.0008
× *Nchild*	(0.0217)	(0.0007)	(0.0006)	(0.0252)	(0.0011)	(0.0011)
Nchild	−1.672***	−0.0008	−0.012***	−0.202***	0.026***	0.0005
	(0.0210)	(0.0006)	(0.0006)	(0.0308)	(0.0010)	(0.0010)
Violence at child's age 3	0.109	−0.003	−0.005***	0.168*	0.005	0.007
	(0.1082)	(0.0026)	(0.0018)	(0.0928)	(0.0047)	(0.0051)
Violence at child's age 3	0.013	0.002***	0.002***	−0.099***	0.005***	0.0007
× *Nchild*	(0.0220)	(0.0007)	(0.0005)	(0.0264)	(0.0011)	(0.0012)
Nchild	−1.676***	−0.0009	−0.012***	−0.201***	0.027***	0.000
	(0.0215)	(0.0006)	(0.0006)	(0.0324)	(0.0010)	(0.0010)
Violence at child's age 5	0.067	−0.009***	−0.007***	0.298***	−0.006	−0.0005
	(0.1062)	(0.0028)	0.003***	(0.0915)	(0.0048)	(0.0051)
Violence at child's age 5	−0.001	0.003***	−0.013***	−0.077***	0.005***	0.002
× *Nchild*	(0.0221)	(0.00075)	(0.0006)	(0.0268)	(0.0011)	(0.0012)
Nchild	−1.668***	−0.001**	−0.013***	−0.214***	0.027***	−0.0008
	(0.0214)	(0.0006)	(0.0006)	(0.0324)	(0.0010)	(0.0010)
N	222,176	222,176	222,176	222,176	222,176	222,176

Note: N = 222,176 mothers with at least a child between 14 and 18 years of age. *Nchild* is defined as the number of children a mother has. Each column in each panel is obtained from a separate regression. All regressions include fixed-effects for the first child municipality of birth, and for each child's year and month of birth. Robust standard errors are clustered at the municipality–year level. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.001$.

Appendix Table 4: Violence and Selective Survival

	Sex ratio		Cohort size	
	Children 14–18	Adults 19–23	Children 14–18	Adults 19–23
	(1)	(2)	(3)	(4)
Violence in-utero	-0.015	-0.022	1.885	0.937^{*}
	(0.0209)	(0.0222)	(1.4034)	(1.1087)
Violence at child's birth	-0.003	-0.001	3.747^{***}	-0.125
	(0.0212)	(0.0222)	(1.3791)	(1.0986)
Violence at child's age 1	0.061^{***}	-0.014	4.073^{**}	0.020
	(0.0196)	(0.0219)	(1.7120)	(1.2011)
Violence at child's age 3	-0.032	-0.010	2.066	-0.851
	(0.0227)	(0.0233)	(1.5642)	(0.8614)
Violence at child's age 5	-0.003	-0.019	-0.782	-2.071^{**}
	(0.0197)	(0.0199)	(1.1132)	(0.9027)
N	2,660	2,660	2,660	2,660

Note: $N = 2{,}660$ municipality–year observations. Each coefficient is obtained from a separate regression. All regressions control for municipality and year fixed-effects. Robust standard errors are clustered at the municipality–year level. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.001$.