Long-term Pre-conception Exposure to Local Violence and Infant Health[†]

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Abstract

This paper studies the effects of mothers' long-term pre-conception exposure to local violence on birth outcomes. Using administrative data from Mexico and two different empirical strategies, our results indicate that mothers' long-term exposure to local violence prior to conception has detrimental effects on infant health at birth. The results suggest that loss of women's human capital and deterioration of mental health are potential underlying mechanisms behind the adverse effects, highlighting intergenerational consequences of exposure to local violence. Our findings shed light on the welfare implications of local violence that are not captured in in-utero exposure to violence.

Keywords: Birth outcomes; local violence; pre-conception; maternal stress
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1 Introduction

One distressing aspect of local crime is its repeated shocks, which negatively affect an individual's physical and mental well-being by inflicting fear and anxiety (Dustmann and Fasani, 2016).[1] Violence-related fear and psychological stress could affect infant health at birth through biological and behavioral changes of women of childbearing age (Mark and Torrats-Espinosa, 2022; Currie et al., 2023). To date, researchers have extensively studied the effects of prenatal exposure to various random shocks on infant health and its long-lasting impacts on later-life outcomes, highlighting intergenerational consequences of exposure to negative shocks (Currie and Rossin-Slater, 2013; Black et al., 2016; Koppensteiner and Manacorda, 2016; Brown, 2018; Persson and Rossin-Slater, 2018; Currie et al., 2022). However, there is scarce evidence on how long-term, cumulative exposure to negative shocks shapes individuals' various outcomes,[2] particularly infant health at birth, the initial health endowment, which affects individuals' future outcomes (Currie and Hyson, 1999; Black et al., 2007; Oreopoulos et al., 2008; Figlio et al., 2014; Bharadwaj et al., 2018; Maruyama and Heinesen, 2020).[3]

In this study, we investigate whether women's repeated exposure to local violence leading up to conception affects infant health at birth. If any, understanding this dimension of local violence exposure may shed light on the welfare implications of violence not captured in in-utero exposure to local violence.[4] Estimating the causal effects of long-term pre-conception

[1]Dustmann and Fasani (2016) show that the mental distress effects of local crime are larger for females in the UK. Rossin-Slater et al. (2020) and Bharadwaj et al. (2021) study the mental health effects of high-profile (mass) shootings in the U.S. and Norway, respectively.

[2]The exceptions include Bishop et al. (2023), who examine the effect of long-term, cumulative exposure to $PM_{2.5}$ for up to a decade on the likelihood of being diagnosed with Alzheimer's disease, and Anderson (2020), who studies the effect of long-term exposure to air pollution on mortality.

[3]Almond and Currie (2011) provide a review of the fetal origins hypothesis, which emphasizes the importance of fetal health in shaping individuals' future trajectories in human capital and health.

[4]The literature on developmental and behavioral medicine documents the relationship between pre-conception stress and pregnancy outcomes. Animal models provide evidence that unpredictable stress prior to conception affects the neurodevelopment of rat offspring (Keenan et al., 2018). The authors state that "Repeated or chronic increases in stress exposure, and hence chronically increased plasma cortisol, can lead to responses that have pathological and illness-inducing consequences for the offspring." Growing evidence from human studies in the fields of psychiatry, psychology, and behavioral medicine also indicate a significant relationship between maternal pre-conception stress and birth outcomes. For example, Mahrer et al. (2021)

exposure to local violence on infant health faces three main challenges. First, exposure to local violence is typically not random; it is possible, for example, that disadvantaged areas are more prone to violent crimes, and children born in those areas have poor health at birth. Second, women endogenously migrate in response to local violence (Currie et al., 2023; Padilla-Romo and Peluffo, 2023b). Finally, exposure to local violence could affect fertility decisions for different types of women, changing the composition of women giving birth or the composition of live births.

To overcome these empirical challenges and to estimate the effects of interest, we combine administrative records of all births registered in Mexico during 2009-2019 (except teenage births) with incident-level homicide records based on death certificates for the period of 1990-2020, using *mothers' municipality of birth*. This allows us to construct various measures of women's exposure to local homicides—arising from a large, exogenous, and unpredictable increase in local violence during the onset of Mexico's war on drugs—prior to conception, as well as postpartum exposure to local violence.[5]

We use two empirical strategies to estimate the effects of long-term exposure to local violence, each of which hinges on different identifying variations. First, we leverage within-municipality variation in the homicide rate over time. Specifically, we count the number of months in which the monthly homicide rate is at least two standard deviations above the municipality's historical mean of monthly homicide rates (hereafter referred to as "violent"

find that maternal stress—such as life events, financial strain, interpersonal violence, and discrimination—before conception is associated with reduced gestation length. The authors note that "when stressors become more severe and possibly traumatic, as with IPV [interpersonal violence] or more lifelong and chronic discrimination, a woman's ability to cope effectively may be overtaxed and her physiology adversely affected, thus posing a risk for her baby." Similarly, Witt et al. (2014) document that pre-conception exposure to stressful life events, such as divorce and deaths of parents, leads to an increase in the likelihood of having a very low birth weight baby. The authors also find that the negative effect on birth weight increases with respect to an additional stressful event. Lastly, Harville et al. (2010) find that women who experienced violence (indicated by physical neglect, maladjustment, mental subnormality in family, experienced bullying, or contact with social services) in their childhood, particularly in adolescence, are more likely to have a low birth weight and preterm birth baby.

[5] Anderson (2020), who studies the effect of long-term exposure to air pollution on mortality, offers valuable insights into measuring the effect of long-term exposure to such random shock. The author states that "the identifying variation in air pollution thus needs to be cross-sectional in nature (or a very long panel), exogenous, and yet subtle enough not to induce migration."

month).[6] One can interpret this measure as a random shock from a local violence distribution and think of this variable as the total number of months with a large exogenous shock to local violence. We then define mothers' long-term exposure to local violence as the total number of violent months to which mothers are exposed during a 15-year window, that is, from 16 years before conception to 1 year before conception.[7] This estimation strategy relies on the assumption that the variation in the number of violent months to which mothers are exposed is as good as random, after controlling for municipality fixed effects and year-month of conception fixed effects. We further include municipality-by-month of conception fixed effects, which controls for municipality-specific seasonality in conception or birth outcomes. The latter fixed effects also control for municipality-level seasonality in homicides that might be related to the drug trade or cultivation of drugs, which in turn affects the economic conditions of a municipality.

Second, we depart from the aforementioned identifying variation and exploit instead the variation in the timing of a structural break in the number of violent months in a municipality aggregated by year-month of conception. This approach allows us to precisely identify the timing of an increase in local violence from a local violence distribution and capture the cumulative, increasing effect of long-term exposure to violence. We then estimate a difference-in-differences model and show the dynamic effects of the structural break on birth outcomes by comparing birth outcomes of mothers in municipalities that have a structural break, which indicates an increase in local violence exposure, to those of mothers in municipalities that do not have the structural break. The identifying assumption of this research

[6] Researchers have used this approach for measuring extreme weather/temperature shocks (McKee et al., 1993; Marchiori et al., 2012; Kim et al., 2021; Ibáñez et al., 2022). For example, Ibáñez et al. (2022), who study the effects of extreme temperature on agricultural production, define temperature shock as the number of weeks in which the temperature is at least two standard deviations above the historical mean. We provide results for using different thresholds ($1.5\sigma_m$, $2\sigma_m$, $2.5\sigma_m$, $3\sigma_m$) that define any given month as violent in Section 4.6.

[7] We use the timing of conception rather than the timing of the mother's own birth, as mothers who give birth at an older age are mechanically exposed for a longer period of time to local violence. We also define in-utero exposure as 1-9 months after conception, irrespective of the actual gestational length listed on birth certificates following the literature. For example, Persson and Rossin-Slater (2018) define the period of in-utero exposure as 280 days (40 weeks) after the date of conception. Importantly, the data allows us to measure violence exposure for up to 18 years before conception.

design is that absent the structural break, birth outcomes of mothers in treated municipalities would have followed the same trends as those of mothers in non-treated municipalities. We provide empirical evidence to support this assumption, showing that these two groups followed parallel trends prior to the structural break. We further show that the estimates are robust to the staggered timing of the structural break across municipalities.

Both empirical strategies indicate that long-term exposure to local violence before conception has adverse impacts on birth outcomes. Using our first empirical strategy, we estimate that an additional violent month during the 15-year window is associated with approximately a 0.6-gram decrease in birth weight, a 0.002-week decrease in gestational age, and a 0.01-centimeter decrease in infant height. Given that the average mother in our sample is exposed to approximately five violent months during the 15-year window, long-term violence exposure lowers birth weight by 3 grams, gestation length by 0.01 weeks, and birth height by 0.05 centimeters.[8] The estimates from our second empirical strategy show that the negative effects on infant health increase in absolute value over time after the structural break, as exposure to local violence sharply increases after the structural break. Moreover, the estimated effects on infant health prior to the break are close to zero and statistically insignificant, supporting our identifying assumption.

We provide suggestive evidence of the underlying mechanisms behind our results. Specifically, we show that violence exposure decreases women's educational attainment and increases the likelihood of feeling depressed and anxious among women but not among men in the same age group. Furthermore, we show that our results are robust to alternative definitions of long-term pre-conception exposure to local violence, different specifications, and alternative samples where we relax the restrictions imposed on the analysis sample.

[8]Comparing our results to the findings of other related studies, Hoynes et al. (2011) find that the introduction of the Supplemental Program for Women Infants and Children in the 1970s increased average birth weight by approximately 2 grams. Similarly, Almond et al. (2011) find that the rollout of the Food Stamp Program during the 1960s-1970s increased average birth weight by approximately 2 grams. Furthermore, Kose et al. (2022) find that the introduction of Community Health Centers in the 1960s-1980s increased average birth weight by 3-6 grams. Additionally, Black et al. (2016) and Persson and Rossin-Slater (2018) find that a death in the family during pregnancy reduces the birth weight by 21 grams and 11 grams, respectively.

Importantly, we show that long-term violence exposure does not affect birth rates, mothers' age at conception, marital status, the gender of the infant, or the probability of having a stillbirth, suggesting that our results are unlikely to be driven by selection. We also show that our results are robust to different time frames for long-term exposure, such as adjusting the duration from 7-17 years to 1-24 months prior to conception.[9] One potential concern may be that our estimation strategy captures variability in violence levels rather than an increase in local violence. To alleviate this concern, we demonstrate that using an absolute measure of local violence—specifically, the sum of the monthly homicide rates over the 15-year window—yields consistent results. Additionally, when we substitute our measure of long-term exposure with the number of "safe" months—defined as months with homicide rates 1.5 SD *below* the municipality's historical mean—our findings imply an improvement in infant health.

This study contributes to the growing literature that studies the effects of exposure to violence on infant health at birth. Recent studies document adverse effects of direct and indirect *prenatal* exposure to violence, specifically, intimate partner violence and the sniper attacks in the U.S. (Currie et al., 2022, 2023). In contrast to these papers that investigate violent events that are high-stakes yet infrequent or disproportionately affect a smaller portion of the population, we focus on local violence (mostly drug-related) that *repeatedly* affects a broader range of civilians. In this respect, our paper is closely related to Koppensteiner and Manacorda (2016), who study the effects of *prenatal* exposure to homicides (occurring in the public way) on infant health in Brazil.[10] Considering that violence, such as firearm violence, has become prevalent even in developed countries (Rossin-Slater et al., 2020; Currie et al., 2023), the evaluation of social costs and welfare implications associated with cumulative effects of violence exposure is crucial. Our paper builds upon the findings of the aforementioned papers and advances the literature by providing evidence on the effects of long-term

[9] See Section 4.7 for a detailed discussion on alternative time durations of long-term exposure.

[10] The authors estimate that an additional homicide during the first trimester of pregnancy increases the likelihood of low birth weight by 0.6 percentage points (8 percent relative to the sample mean). This effect, however, is limited to municipalities with a population lower than 5,000 people.

pre-conception exposure to local violence on infant health. Additionally, providing evidence on violence-deteriorated women's educational attainment and mental health as the potential underlying mechanisms enriches the existing literature.

Our study contributes to the broader literature on the effects of in-utero or early-life exposure to environmental factors on fetal or children's health. The literature has explored different sources of exogenous shocks to mothers' physical and psychological stress.[11] For example, considering shocks that affect infants' physiological development through the physical impact on mothers, such as air and water pollution, Mouganie et al. (2023) study the effect of open-air waste burning on birth outcomes in Lebanon, and find that at least one incident of waste burning during the first two trimesters of pregnancy increases the likelihood of low birth weight by 5 to 8 percentage points. Additionally, Rangel and Vogl (2019) find that prenatal exposure to smoke from agricultural fires in Brazil, particularly during the third trimester of pregnancy, reduces birth weight and gestational length. Using water pollution and river flow direction, Dias et al. (2023) show that areas receiving water containing the toxic herbicide glyphosate experience adverse birth outcomes in Brazil.[12] Given that persistent exposure to air pollution adversely affects human health (Bishop et al., 2023; Anderson,

[11]Focusing on maternal stress due to family bereavement, Black et al. (2016) find that the death of the mother's parent during pregnancy reduces the birth weight by 21 grams, with a similar magnitude of the effect across different trimesters of pregnancy in Norway. Similarly, exploring the differential effects of a death in the family by different relative types using Swedish data, Persson and Rossin-Slater (2018) find that exposure to a death in the family during pregnancy lowers the birth weight by 11 grams and increases the likelihood of low birth weight and preterm birth by 12 percent each. The authors further show that prenatal exposure to a family death leads to an increase in the likelihood of children consuming mental health drugs, such as ADHD, anti-anxiety, and depression medications in both middle childhood (ages 9-11) and adulthood. Guldi and Hamersma (2023) show that reduced post-birth maternal depression, due to the expansion of pregnancy-related Medicaid is one potential mechanism behind the improved early childhood outcomes.

[12]Other related studies include, Jones (2020) who finds that prenatal exposure to dust storms (elevated $PM_{2.5}$ concentrations) in the U.S. leads to an increase in the likelihood of low birth weight by 1.4 percentage points. Additionally, Dave and Yang (2022) find that prenatal exposure to lead-contaminated drinking water reduces the birth weight by 31 grams. Kim et al. (2017) show that infants born closest to the epicenter of California's 1994 Northridge earthquake were 0.2 percentage points more likely to be born with low birth weight. Using policy interventions, Da Mata and Drugowick (2023) find that a smoke-free law in Brazil increases the birth weight by approximately 30 grams, and Charris et al. (2024) document that trade-induced positive economic shock in Brazil reduces infant mortality. In Brazil, Koppensteiner and Menezes (2023) find that maternal dengue infections lower birth weight by 27 grams, with the effect being driven by dengue infections in the third trimester.

2020),[13] our findings of a link between pre-conception violence exposure and infant health imply that estimating the prenatal exposure to shocks without considering the potential cumulative effect of long-term exposure leading up to conception might underestimate the full impacts, in a context where women are repeatedly exposed to unexpected shocks. Our findings also suggest that women who are persistently exposed to negative shocks, such as violence, need to be recognized sooner for remediation in order to mitigate intergenerational consequences of exposure to shocks.

2 Data

We use administrative data on birth and homicide records as well as three survey datasets. Birth and homicide records are from Mexico's Ministry of Health and the National Institute of Statistics and Geography (INEGI), respectively. Census survey data are from the 2010 Population and Housing Census and the 2015 Intercensal Survey from INEGI, retrieved from IPUMS International. Well-being survey data are from the 2013-2022 Basic Module of the Self-reported Welfare Survey (BIARE) conducted by INEGI.

Birth Records

The birth records include all births from 2009-2019 reported in the Birth Information Subsystem (SINAC). The data contains information on the municipality of birth, year and month of birth, birth weight (in grams), birth height (in centimeters), gestational age (in weeks), infant gender, and other birth outcomes such as APGAR score at 5 minutes, types of delivery, and whether the birth is singleton or multiple births.[14] The data also contains

[13] Bishop et al. (2023) examine the effect of long-term, cumulative exposure to air pollution for up to a decade on the likelihood of being diagnosed with Alzheimer's disease.

[14] Our main analysis focuses on outcomes of birth weight, height, and gestational length. These variables have missing information for 5.23%, 2.45%, and 0.52% of the observations, respectively. In Table A.1 and Figure A.1, we show that the probability of attrition for each of these variables is not correlated with our different measures of local violence exposure prior to conception.

information on the municipality of the mother's birth, age of the mother,[15] marital status at birth, education level, occupation, prenatal care, number of pregnancies, and number of children born alive to the mother.

Homicide Records

Homicide reports are based on death certificates from 1990 to 2020, where the cause of death is registered as an "alleged homicide." The data includes information at the incident level on the date and municipality where the homicides occurred. For each municipality-year-month combination, we count the number of alleged homicides and use municipality-level population estimates from the National Council of Population (CONAPO) to put the information into per capita rates.

To construct our main measure of mothers' long-term pre-conception exposure to local violence, we count the number of months in a municipality during the 16 to 1 years prior to conception (192 to 12 months prior to conception) in which the homicide rate is at least two standard deviations above the municipality's average monthly homicide rate calculated over the period of 1990-2020 (hereafter referred to as "historical mean").[16] That is, we define a municipality m as violent in year-month t as follows:

$$Violent_{mt} = \mathbb{1}(HomicideRate_{mt} \geq HomicideMean_m + 2\sigma_m) \tag{1}$$

where $HomicideRate_{mt}$ is the homicide rate of municipality m in year-month t, and $HomicideMean_m$ is the historical mean homicide rate of municipality m and σ_m is its standard deviation. Intuitively, $Violent_{mt}$ can be interpreted as a random shock from a local violence distribution. We then define mothers' long-term pre-conception exposure to local violence in municipality

[15] Age of mother at conception is calculated using birth year and month of the mother and conception year and month.

[16] This approach has been used in the literature to measure extreme weather shocks (McKee et al., 1993; Marchiori et al., 2012; Kim et al., 2021; Ibáñez et al., 2022). In Section 4.7, we show that our results are qualitatively similar across different thresholds ($1.5\sigma_m$, $2\sigma_m$, $2.5\sigma_m$, $3\sigma_m$) that define any given month as violent.

m at year-month of conception t as follows:

$$LongTerm_{mt} = \Sigma_{i=12}^{192} Violent_{mt-i} \tag{2}$$

where $Violent_{mt-i}$ equals one, if municipality m experience a local violence shock i months before conception at year-month t.[17] We repeat this procedure to construct our measure of in-utero exposure to local violence, where we count the number of months during pregnancy (1 to 9 months after conception) in which the monthly homicide rate is at least two standard deviations above the municipality's historical mean.

Figures 1 and 2, respectively, show the geographic distribution of mothers' long-term pre-conception exposure to local violence and in-utero exposure by year-month of conception. Figure 3 shows the municipalities' average exposure by year-month of conception. Overall, local violence is widespread throughout the country and within states, and varies significantly over time. We leverage both sources of variation in our identification strategy, which we describe in Section 3.

Census Survey Data

Census survey data is used to understand potential selection into giving birth and the underlying mechanisms driving our main results. These data contain individual-level information on gender, municipality of residence, age, the number of children, marital status, and years of schooling. We include all women aged between 20 and 49 to explore how long-term exposure to local violence affects the probability of having children and being married, as well as their years of schooling.

[17] We show that our main results are robust to using alternative measures of violent (i.e., $2.5\sigma_m$ and $3\sigma_m$ above the historical mean homicide rate) and alternative starting and ending points for our window of long-term exposure to local violence (e.g., 204-12 months, 180-12 months, 168-12 months, 204-1 months, 192-1 months, 180-1 months, among others).

Well-Being Survey Data

Well-being survey data is used to explore a potential mechanism behind the adverse effect of exposure to local violence on infant health. We use the basic questionnaire of BIARE, which is a individual-level quarterly survey that measures the subjective dimension of well-being. The survey is conducted in the first month of each quarter, and the sample is representative of the urban population. We use the survey section that captures survey respondents' emotional states the day before the interview. We focus on questions regarding mental distress, specifically, feeling depressed (depressed, sad, or afflicted), anxious (worried, anxious, or stressed), exhausted (tired or devitalized), and irritated (bad mood) for women aged between 20 to 49. The survey records the fraction of the day respondents remained in different emotional states to generate measures of emotional balance. Specifically, for each emotional state, the survey respondents are given a 0 to 10 rating scale, where 0 indicates the absence of such emotion, 5 indicates the presence of such emotion in half a day, and 10 indicates a whole day. We use 6 or above to define any survey respondents as having such emotional symptoms following standards from the National Institute of Mental Health (NIMH) used in screening for mental distress.[18] In our analysis, since survey respondents only report symptoms experienced the day before the survey, we identify individuals who are more likely to experience mental distress by focusing on those who reported having two symptoms simultaneously, following standard screening tests (Kroenke et al., 2007, 2003).[19,20] According to our definition, 1.9 percent of the sample reported feeling depressed and anxious, 1.2 percent depressed and irritated, 1.5 percent depressed and exhausted, 1.8 percent anxious and

[18] These guidelines suggest that an individual is more likely to experience depression if they exhibit symptoms for most of the day, nearly every day during the reporting period. Source: https://www.nimh.nih.gov/health/topics/depression.

[19] For example, the Patient Health Questionnaire (PHQ-2) is a self-administered test that inquires about two symptoms—depressed mood and anhedonia—to screen for depression. The Generalized Anxiety Disorder (GAD-2) is a self-administered screening test for generalized anxiety disorder that asks about two symptoms: feeling anxious and being unable to stop worrying (Kroenke et al., 2007, 2003).

[20] We also aggregate three or four symptoms together to increase the precision in identifying individuals who suffer from mental distress, but doing so significantly decreases the proportion of people identified as having mental distress.

irritated, 2.2 percent anxious, and exhausted, and 1.2 percent exhausted and irritated the previous day.[21]

Sample Restrictions

We focus on women who give birth to a live child for the first time, including stillbirths.[22] We further restrict our data to singleton births to avoid pregnancy complications related to multiple births (Black et al., 2016; Dave and Yang, 2022). We include only live births with birth weight of at least 500 grams, gestation of at least 23 weeks, and birth height of at least 27 centimeters.[23] We keep municipalities with at least 10,000 total population to alleviate concerns related to extreme outliers in the homicide rate driving the results.[24] We test the robustness of our results in Section 4.7 when relaxing these sample restrictions. Table 1 presents summary statistics for the analysis sample. The average mother in our sample was exposed to 5.12 violent months in the 15-year window before conception and 0.62 months during pregnancy.

3 Empirical Strategy

We implement two different empirical strategies to estimate the effects of long-term exposure to local violence on infant health. Each strategy exploits different sources of variation and relies on different identifying assumptions. The first strategy leverages variation in the

[21] In our sample, 3.1 percent of women aged 20 to 49 report experiencing depression. The Mexican Institute of Public Health indicates a national prevalence of 5.8 percent among women in Mexico. Furthermore, the report highlights that depression rates tend to increase with age, based on data from the 2002-2003 National Assessment Performance Survey (Belló et al., 2005).

[22] Currie and Moretti (2003), who study the relationship between mothers' schooling and pregnancy outcomes, focus their analyses on first births because of differences in average outcomes associated with birth order.

[23] Black et al. (2016) employ similar sample restrictions: birth weight of at least 500 grams and gestation of at least 26 weeks.

[24] We use the average total population calculated over the period of 1990-2020 using municipality-level population estimates from CONAPO. Out of 2,454 municipalities, 1,097 municipalities have an average total population of less than 10,000, with a mean and median of 4,229 and 3,693 people, respectively. Table A.8 Panel A shows that our main results shown in Table 2 remain unchanged when these sample restrictions are relaxed.

intensity of long-term exposure to local violence prior to conception, and the second one exploits variation in structural breaks of the intensity measure in a difference-in-differences framework.

3.1 Two-way Fixed Effects (TWFE) Model

To estimate the effects of mothers' long-term exposure to local violence on infant health, we exploit within-municipality variation in long-term exposure to local violence over time. The level of long-term exposure to local violence experienced by a mother depends on the municipality of their own birth and the year-month of conception. We estimate the following baseline specification:

$$Y_{imt} = \alpha_m + \gamma_t + \delta LongTerm_{mt} + X_{it}\beta + \epsilon_{imt}, \tag{3}$$

where Y_{imt} represents different infant health outcomes of the first live birth of mother i who was born in municipality m and conceived at year-month t, including a low birth weight indicator (<2,500 grams), birth weight (in grams), a preterm birth indicator (<37 weeks), gestational age (in weeks), and height (in centimeters); α_m and γ_t are mothers' municipality of birth and year-month of conception fixed effects.[25] In our preferred specification, we additionally control for mothers' municipality of birth-by-month of conception fixed effects to account for municipality-specific seasonality in birth outcomes. $LongTerm_{mt}$ is measured as in Equation (2) and denotes the sum of violent months from 16 years before conception (192 months) to 1 year before conception; X_{it} include during pregnancy (1-9 months after conception) and post-birth exposure to local violence (10-21 months after conception); and ϵ_{imt} is an error term that we allow to be correlated within mothers' municipality of birth. We do not include infant biological sex or maternal characteristics observed at the time of giving birth, such as age at conception, educational level, and marital status, since exposure

[25]The timing (year-month) of conception is recovered using the gestational age in weeks.

to local violence is likely to affect these outcomes (Brown and Velásquez, 2017; Lindo and Padilla-Romo, 2018; Koppensteiner and Menezes, 2021; Chang and Padilla-Romo, 2023; Padilla-Romo and Peluffo, 2023a).[26]

The coefficient of interest, δ, provides the marginal effect of one additional violent month during the 15-year window, as defined in Equation (2). It can be interpreted as causal when there are no other shocks, such as unobserved municipality-level interventions, that affect both levels of local violence within a municipality and our different measures of infant health at birth, and when there is no selection into a live birth. To support the validity of our identification strategy, we show that postpartum exposure to high levels of local violence does not affect infant health at birth. We also show that both long-term and in-utero exposure to local violence does not affect the gender of the infant or the probability of having at least one stillbirth.

We use the mother's municipality of birth as a proxy for the municipality where she has spent the last 17 years before the birth of her child. While we could have used the mother's municipality of residence, we chose not to in order to avoid introducing endogeneity problems, given that exposure to local violence induces migration into safer areas, especially among individuals with high socioeconomic status (Padilla-Romo and Peluffo, 2023b).[27] In doing so, we should think of δ as a lower bound of the true effect.

3.2 Difference-in-Differences (DID) Model

As an alternative estimation strategy, we estimate a modified version of our preferred specification, where we leverage the variation in the timing of structural breaks in the number of violent months in a municipality by year-month of conception. To do so, we first test for a structural break in each municipality's time series of the number of violent months and identify the timing of the most significant break following the procedure developed by

[26] We show in Section 4.7 that our main result is not sensitive to the inclusion of mothers' characteristics.

[27] In Table A.5, we show that mothers exposed to high levels of local violence in their municipality of birth during pregnancy are more likely to reside in a municipality that is different from their municipality of birth.

Andrews (1993). That is, for each municipality, we identify either a single structural break or no break.[28] Among those municipalities with a structural break, we distinguish between a positive break and a negative break: a positive structural break indicates an increase in local violence exposure, whereas a negative break indicates a decrease in violence exposure.

Figure 4 shows the timing of these empirically identified positive breaks for a sample of four municipalities in Mexico: Ciudad Juárez, Culiacán, Guadalajara, and Monterrey. Note that there is significant variation in the levels of exposure to violence before and after the break and in the timing of the structural break. For this analysis, we drop municipalities with a negative break. In doing so, we compare infant health outcomes of children born to mothers in municipalities with a sharp increase in violence exposure to those born to mothers in municipalities without significant changes in local violence exposure.[29]

We estimate the following event study specification:

$$Y_{imt} = \alpha_m + \gamma_t + \theta_{mm_c} + \sum_{p \in \{0-11, 12-23, 24-35, \ldots, 84+\}} \delta_p Break_{pmt} + X_{it}\beta + \nu_{imt}, \tag{4}$$

where $Break_{pmt}$ is an indicator of municipality m having a structural break in mothers' long-term exposure to local violence p months before the conception year-month t, and θ_{mm_c} are the mother's municipality of birth by month of conception fixed effects. We additionally include indicators for the three years prior to the structural break. That is, 12 to 1, 13 to 24, and 25 to 36 months prior to the structural break. Our coefficients of interest, δ_p, capture the average effect of having a structural break p months before the conception year-month and are relative to changes in infant health outcomes between treated and non-treated municipalities 37 or more months prior to the structural break.

The identifying assumption of this difference-in-differences research design is that in the absence of the structural break, birth outcomes of mothers in treated municipalities

[28] Our control group consists of municipalities that are identified as having no break, have little to no violent months, and have little changes in the number of violent months over the 15-year window (i.e., the bottom 10 percent of the distribution). We find consistent results when using municipalities that have no break and have zero violent months over the 15-year window.

[29] We show in Section 4.3 that the negative structural break has null effects on infant health at birth.

would have followed the same trends as those of mothers in non-treated municipalities. We provide empirical evidence to support this assumption, showing that these two groups followed parallel trends prior to the break and that there are no anticipation effects.

Considering that municipalities had structural breaks at different times, we further estimate Equation (4) using the Imputation Estimator developed by Borusyak et al. (2021), which produces estimates that are robust to staggered treatments and dynamic effects.

4 Results

4.1 Effects on Infant Health: TWFE Model

We present our main results on birth outcomes using Equation (3) in Table 2. Odd columns show the baseline specification in Equation 3. Even columns present our preferred specification, which additionally controls for the municipality of the mother's own birth-by-month of conception fixed effects to account for municipality-specific seasonality in birth outcomes. The dependent variables in columns (1)-(2), (3)-(4), (5)-(6), (7)-(8), and (9)-(10) are an indicator variable for low birth weight (<2,500 grams), birth weight in grams, an indicator variable for a preterm birth (<37 weeks), gestational age in weeks, and birth height in centimeters, respectively. The coefficient of interest, δ, provides the marginal effect of experiencing one additional violent month during the 15-year window, specifically 192 to 12 months before conception.[30] We focus on the results provided by our preferred specification.

Table 2 Panel A shows that long-term exposure to local violence has adverse impacts on birth outcomes. The estimates in columns (2), (4), (6), (8), and (10) show that having one additional violent month over the period defined above, conditional on having controlled for homicide exposure during pregnancy, increases the likelihood of low birth weight by 0.01 percentage point, decreases birth weight by 0.6 grams, increases the likelihood of preterm

[30] We show in Section 4.7 that changing the definition of long-term exposure yields similar results across different measures and that using alternative thresholds ($1.5\sigma_m$, $2\sigma_m$, $2.5\sigma_m$, $3\sigma_m$) that define any given month as violent provides consistent results.

birth by 0.01 percentage point, and decreases gestational age by 0.002 weeks and birth height by 0.01 centimeter, respectively. The estimated effect on the likelihood of preterm birth is statistically insignificant. The average mother in our sample is exposed to approximately 5 violent months during the 15-year window. Hence, long-term violence exposure increases the likelihood of low birth weight by 0.8 percent relative to the sample mean and decreases birth weight by approximately 3 grams, gestation length by 0.01 weeks, and birth height by 0.05 centimeters. Furthermore, the point estimate for postpartum violence exposure (defined as 10-21 months after conception) is statistically insignificant, serving as a falsification test.[31]

To explore whether prenatal exposure to local violence has differential effects across different periods of pregnancy, we estimate in Panel B of Table 2 the in-utero effects of local violence exposure by each trimester of pregnancy. Each trimester is defined as a 3-month exposure window, starting with the first trimester being the first three months after conception (i.e., 1-3 months after conception). Column (4) shows that exposure to one additional violent month during the first trimester of pregnancy decreases birth weight by approximately 1 gram. Since the average mother in our sample is exposed to 0.2 violent months during the first trimester of pregnancy, violence exposure during this period lowers birth weight by 0.2 grams.[32] For ease of presentation, we use the single measure of in-utero exposure in the rest of the paper, as shown in Panel A of Table 2, since long-term pre-conception exposure to local violence is our focus.

To explore treatment heterogeneity in the timing of exposure prior to conception, we estimate the effects after dividing our main treatment variable into six 30-month bins. In Table 3, we show that the effects shown in Table 2 are mostly driven by mothers' exposure to local violence occurring approximately 1 to 9 years before conception.

We further estimate the effects on other birth outcomes using our preferred specification

[31] In Section 4.7, we show that using an extended period to measure postpartum exposure to local violence yields insignificant effects on birth outcomes.

[32] For comparison, Koppensteiner and Manacorda (2016), Mouganie et al. (2023), and Currie et al. (2023) document significant adverse birth effects stemming from the first or the first two trimesters of pregnancy. In contrast, Black et al. (2016) document consistent negative effects across trimesters of pregnancy.

and report the results in Table A.2. In column (1), we find that exposure to 5 violent months during the 15-year window is associated with a 0.4 percentage point (0.7 percent) increase in the likelihood of having a cesarean section (C-section). In column (2), we find an insignificant effect of long-term violence exposure on the 5-minute APGAR score.[33]

4.2 Effects on Infant Health: DID Model

As discussed in detail in Section 3.2, we use a different source of variation by identifying a structural break in long-term exposure to violence for each municipality and use the variation in the timing of the structural break in a difference-in-differences specification, as described in Equation (4).

The estimated effects for all our different measures of infant health at birth are shown in Figure 5. Panel (a) shows the estimated effects on the intensity of treatment after the structural break. It is worth noting that the small increase in the number of violent months *prior* to the timing of the break does not compromise the validity of our research design or contradict the identifying assumption of parallel trends—As shown in Figure 4, in some municipalities, the number of violent months started to increase even before the timing of the most significant, empirically identified break. Overall, the number of violent months a mother is exposed to in the 15-year pre-conception window increases by approximately 7 months, 0-11 months after the structural break; this effect increases to approximately 15 months, 84 or more months (84+) after the structural break.

Panels (b)-(f) show the estimated effects on the probability of low birth weight, birth weight, the probability of preterm birth, gestational age, and birth height, respectively. Consistent with the increasing effect of the number of violent months mothers are exposed to prior to conception, the estimated negative effects on all birth outcomes are increasing in absolute value over the months after the structural break. Importantly, the estimated effects

[33]For comparison, Black et al. (2016) find that experiencing the death of the mother's parent during pregnancy increases the likelihood of having a cesarean section but does not affect the 5-minute APGAR score. Both Koppensteiner and Manacorda (2016) and Dave and Yang (2022) find null effects on these outcomes.

on birth outcomes before the structural break are close to zero and statistically insignificant at conventional levels, providing supportive evidence of the validity of the research design.

Considering the staggered structural breaks across municipalities, we further estimate Equation (4), but instead of allowing for dynamic effects, we include a post-structural-break indicator as the treatment variable and use the estimation method developed by Borusyak et al. (2021), which produces estimates that are robust to staggered treatments. These estimated effects are shown in Table 4. For reference, Panel A presents the estimates using the OLS estimator, and Panel B presents the estimates using the imputation estimator. Column (1) shows the estimated increase in the number of violent months, and columns (2)-(6) show the effects on our different measures of infant health at birth.

The results in Panel B column (1) indicate that the number of violent months (i.e., the intensity of treatment) increases by approximately 9 months after the structural break. As discussed earlier, the small and significant increase in the number of violent months before the break (1-5 months prior) reflects the increasing trend in the number of violent months prior to the timing of the structural break in some municipalities (see Figure 4). As the number of violent months increases by 9 months after the structural break, the probability of low birth weight increases by 0.2 percentage points, birth weight decreases by approximately 6 grams, gestational age decreases by 0.04 weeks, and height decreases by 0.09 centimeters.[34] These results indicate that the staggered treatment of mothers' municipality of birth does not pose a threat to our identification strategy, and if any, our main estimates are biased towards zero. Moreover, the estimated effects on birth outcomes (columns (2)-(6)) for the pre-treatment months are not significant at conventional levels, providing support for our identifying assumption.

[34] To compare to our main results in Table 2, a one-month increase in the number of violent months is associated with a 0.7-gram decrease in birth weight, a 0.005-week decrease in gestational age, and a 0.01-centimeter decrease in height.

4.3 Effects of Exposure to Non-violent Months

Our main analysis uses standard deviations from the municipality's historical mean to measure long-term exposure to local violence. Thus, it is possible that our TWFE model captures deviations from the average levels of violence in a municipality rather than increases in exposure to local violence. To alleviate this concern, we estimate the effects of exposure to safe months or a decrease in violence exposure on infant health at birth. We estimate our preferred specification in Table 2, but replace our measure of long-term exposure with the number of months with homicide rates 1.5 SD *below* the municipality's historical mean, which we refer to as the number of very safe months.[35] The results in Table 5 indicate that exposure to very safe months does not affect infant health at birth. The point estimates are not statistically significant at conventional levels, and, if anything, the point estimates would imply improvements in infant health. This finding indicates that our main estimates are unlikely to merely capture deviations from the average levels of violence.

Similarly, the DID analysis, which utilizes variations in the structural breaks, focuses on comparisons between municipalities that have a positive structural break and those that do not have significant changes in the number of violent months. By using this approach, it is possible that we capture the effects of changes in the trends rather than increases in exposure to local violence. To address this concern, we perform an analysis that is analogous to Figure 5, but we compare municipalities that have a *negative* structural break (i.e., a reduction in long-term pre-conception exposure to local violence) to municipalities that do not have a break. The estimates in Figure 6 panel (a) indicate that the number of violent months a mother is exposed to in the 15-year window decreases by 4 to 7 following the negative structural break, while the estimates in panels (b)-(f) show that the negative structural break has no effect on our different measures of infant health at birth. The point estimates before and after the break are close to zero and statistically insignificant at conventional

[35] We use 1.5 SD instead of 2 SD as in our main analysis, because only seven municipalities in our sample ever have a month with a homicide rate 2 SDs below their historical mean.

levels. One might expect positive effects on infant health, given that the negative break analysis provides reverse experiments. However, the decrease in violence over time does not necessarily indicate a symmetric improvement in infant health if the accumulated adverse effect of violence exposure persists to the extent that affects a woman's physical and/or mental well-being afterward.[36] Alternatively, it is also possible that our findings of null effects on infant health stem from the relatively weaker intensity of treatment associated with a negative break (Figure 6 panel (a)), compared to a positive break (Figure 5 panel (a)): it is only half of the intensity of treatment for a negative break. These findings together support the validity of our empirical strategies.

4.4 Selection

Exposure to local violence may influence women's fertility decisions and endogenously change the composition of both women giving birth and their newborns. To explore this possibility, we conduct three analyses to examine the scope of selection by estimating the effects of long-term exposure to violence on fertility rates, the likelihood of a female birth, and maternal characteristics using both approaches discussed in Section 3.1 and 3.2.

Fertility

To examine whether violence in a municipality affects birth rates, we use a municipality-level panel dataset and estimate the effects of long-term exposure to violence in a municipality on the birth rate, considering the mother's municipality of her own birth. The estimated effects using the TWFE model are shown in Table A.3. Column (1) shows the estimated effects on birth rate, and column (2) shows the effects on the log of birth rate. Considering the count nature of the birth data, column (3) presents estimates from a Poisson model estimated by pseudo-maximum likelihood. As in our preferred specification, all models

[36] While it is a different context, Barreca et al. (2018) find that exposure to hot temperatures (above 80 $^{\circ}F$) leads to a decline in birth rates in the US, while cold temperatures (below 70 $^{\circ}F$) have insignificant effects on birth rates.

include municipality, year-month of conception, and municipality-by-month of conception fixed effects. The results show that regardless of the model, exposure to local violence does not affect birth rates.[37] All coefficients are close to zero and not significant at conventional levels. We additionally estimate the effect on fertility (i.e., number of births) using the DID model discussed in Section 3.2 and find no evidence of endogenous fertility, as shown in Figure A.2.

Selection Into Live Birth

The sex ratio of newborns can be altered by psychological stress caused by severe life events around the time of conception, due to differential conception or differential abortion of male embryos (Hansen et al., 1999). To test the possibility of the change in the gender composition of newborns due to long-term exposure to local violence, we estimate the effect on the likelihood of a female birth, following the literature (Black et al., 2016; Persson and Rossin-Slater, 2018; Dave and Yang, 2022; Dias et al., 2023).[38] Table A.4 column (1) shows no evidence of any statistically significant effect of long-term homicide exposure on the likelihood of an infant being female.[39] Additionally, in column (2), we test whether long-term violence exposure affects the likelihood of having at least one stillbirth and find no such evidence. These results, taken together, imply that our results are not driven by selection into live births. Repeating the same analysis using the DID model yields the same result, as shown in Figure A.3.

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[37] These results are consistent with Floridi et al. (2023), who show that increases in homicides in Mexico do not affect the fertility rate.

[38] Dave and Yang (2022) note "the biological fragility of the male fetus to negative health shocks is often used to indirectly test for miscarriages, which would result in a greater likelihood of observing a female birth."

[39] We use the approach discussed in Section 3.1 and our preferred specification to be consistent with our main results.

Composition of Mothers Giving Birth

Next, we examine whether our treatment variable is correlated with mothers' observable traits by conducting a balancing test. Specifically, we estimate the effect of long-term exposure to local violence on each of the observed mother's characteristics: mother's age at the time of conception, mother being married at the time of giving birth (1/0), whether the mother has moved out of the municipality where she was born ("mover", 1/0), and mother having completed high school education or higher (1/0).

Table A.5 shows that our measure of long-term violence exposure prior to conception is not correlated with the mother's age at the time of conception (column (1)) nor her marital status (column (2)). In column (3), we find that our current measure of long-term violence exposure during the 15-year window is not correlated with mothers' moving status. Meanwhile, in-utero violence exposure (i.e., 1 to 9 months after conception) increases the probability of migrating to a different municipality. However, column (4) shows that long-term violence exposure prior to conception is negatively associated with mothers' educational level (i.e., the likelihood of mothers having completed high school education or higher at the time of giving birth). To understand the timing of violence exposure, we estimate the results by dividing our main treatment variable into six 30-month bins. Table A.6 column (4) indicates that violence exposure is systematically related to mothers' educational attainment, consistent with the literature of violence and human capital development (Brown and Velásquez, 2017; Koppensteiner and Menezes, 2021; Chang and Padilla-Romo, 2023; Padilla-Romo and Peluffo, 2023a). While the point estimates are small (0.2-0.3 percent relative to the sample mean), we explore this negative correlation between treatment and maternal education in more detail in the following section, using census survey data and by drawing on the related literature.

Finally, we conduct the same analysis of balancing tests using the DID model discussed in Section 3.2, and display the results in Figure A.4. In panels (a) and (b), we find that as time elapses since the structural break, mothers' age at conception decreases while the likelihood of being married at the time of giving birth increases, although most of the coefficients are statistically insignificant at conventional levels. Both of these factors may have positive impacts on infant health at birth. In panel (c), we find little evidence of crime-induced migration. Furthermore, in panel (d), consistent with our TWFE model, the likelihood of completing high school education or higher is negatively associated with exposure to local violence.

4.5 Mechanism

We consider two potential mechanisms behind our results: changes in women's human capital and mental well-being.

Human Capital

In this section, we revisit one of the findings from the balancing tests shown in Table A.5: the negative correlation between our measure of long-term exposure to local violence and maternal education, as reported in column (4) of Table A.5. To understand the relationship between the two, we employ the census survey data described in Section 2. The idea is that since we only observe women who give birth in our birth data, rather than the general population of women of childbearing age, it is unclear whether the negative correlation between violence exposure and maternal education indicates changes in the composition of women giving birth due to violence exposure.

Focusing on all women aged between 20 and 49, based on census data from 2010 and 2015, we estimate the effect of long-term exposure to local violence on years of schooling using the TWFE model, where the treatment is the number of violent months aggregated at the municipality level, up to one month before the timing (year-month) of the survey.[40] In Figure A.5, we find consistent evidence that local violence exposure lowers women's educational

[40]Given that we do not have information on the municipality of their birth, we use municipality of residence for this analysis. We use municipalities with a population of 10,000 or more to be consistent with our analysis with birth data.

attainment.[41] In addition to this finding, we rely on the economics literature to posit that the negative relationship between local violence exposure and maternal education may be an underlying mechanism behind the adverse effects of mothers' long-term exposure to local violence on their newborns' health at birth.[42] Specifically, in the context of Mexico, Chang and Padilla-Romo (2023) find that exposure to violent crimes lowers only female students' test scores, which results in a placement of female middle school students into less-preferred and lower-quality high schools.[43] While the context is different, the economics literature on intergenerational transmission of maternal (parental) education documents positive effects of maternal (parental) schooling on infant health. For example, Currie and Moretti (2003) and Chou et al. (2010) find that maternal college education in the U.S. and parental high school education in Taiwan, respectively, improved birth outcomes, such as reduced incidence of low birth weight and infant mortality. Lastly, Dursun et al. (2024) find that expanding the curriculum for a high school diploma in the U.S. reduced the likelihood of low birth weight and preterm birth.

Mental Health

We further estimate the effects of exposure to local violence on the mental well-being of women aged between 20 and 49 using well-being survey data described in Section 2 and variation in the structural breaks.[44] As discussed in Section 2, we focus on individuals who report having any two of the following conditions simultaneously: feeling depressed, anxious, exhausted, and irritated. In Figure A.6, we find that, in general, local violence

[41]Furthermore, we estimate the effects of long-term exposure to local violence on the likelihood of being married and having children using the census data. In Figure A.5, we find insignificant effects on these outcomes, validating our findings regarding non-endogenous fertility in tables A.3 and A.5.

[42]We refer to the literature that documents the adverse effects of crime exposure on human capital development (Brown and Velásquez, 2017; Koppensteiner and Menezes, 2021; Chang and Padilla-Romo, 2023; Padilla-Romo and Peluffo, 2023a) and the positive effects of maternal education on infant health (Currie and Moretti, 2003; Chou et al., 2010; Dursun et al., 2024).

[43]The authors find that crime-induced concentration problems among female students are an underlying channel through which violence exposure affects female students' test scores.

[44]We use municipalities with a population of 10,000 or more to be consistent with our analysis using birth data.

exposure increases the likelihood of having emotional or psychological discomfort.[45] We find insignificant (or smaller) effects for males aged between 20 and 49, as shown in Figure A.7. These results are consistent with Dustmann and Fasani (2016), who find that local crimes, particularly property crime, increase depression and anxiety, and that the effects are greater for female residents in the UK.

4.6 Heterogeneous Effects

By Intensity of Violence Exposure

Our measure of long-term violence exposure utilizes the variation in violence from a local violence distribution. We explore heterogeneity by the magnitude of violence exposure since a greater intensity of violence exposure may have larger adverse effects on birth outcomes. Using our preferred specification of the TWFE model, we change thresholds that define any given month as violent. As discussed in Section 2, our main analysis uses two standard deviations above the municipality's historical mean of monthly homicide rates ($2\sigma_m$) as a cutoff to determine whether any month is considered "violent" (see Equation (1)). In Figure 7, we replicate the results in Table 2 using other thresholds, such as $1.5\sigma_m$, $2.5\sigma_m$, and $3\sigma_m$ above the historical mean, and display the estimates of long-term exposure. We find that the magnitude of the effects increases in absolute value as we use more extreme measures of violence exposure. We also consider the non-linearity in the effects of long-term violence exposure by dividing our main treatment variable into quintiles and using the first quintile as a reference group. Figure A.8 shows that the magnitude of the effects increases non-linearly as the intensity of violence escalates.[46]

Furthermore, we conduct a similar analysis using the variations in the structural breaks. Specifically, we compare the birth outcomes in municipalities with a large structural break

[45] We find consistent results when using three or four symptoms together. The results are available upon request.

[46] In Figure A.8, the 2nd quintile includes 1-2 violent months, the 3rd quintile includes 3-5 violent months, the 4th quintile includes 6-9 violent months, and the 5th quintile includes 10-32 violent months.

to those that have no break,[47] and in municipalities with a small structural break to those that have no break. Figure 8 panel (a) shows that the intensity of violence exposure in municipalities that have a large break (blue) is significantly greater than those that have a small break (red), and the difference in the intensity of violence exposure between the two increases over time. In panels (b)-(f), we find significantly greater adverse effects on infant health at birth in municipalities that have a large structural break than those in municipalities that have a small break.

By Gender of Infant

Considering that recent studies document gender differences in the effects of negative shocks on infant health, we explore heterogeneity based on the biological sex of the infant. For example, Black et al. (2016), who study the effects of mothers' psychological stress caused by the death of their parent during pregnancy on infant health at birth, document larger adverse effects for male than for female infants. Similarly, Dave and Yang (2022), who study prenatal exposure to lead in drinking water on birth outcomes, find a significant adverse effect for males only.[48] Using our preferred specification of the TWFE model, we show in Figure 9 that the impact of long-term violence exposure on birth weight is greater for male than for female infants. We do not find the same pattern of effects on other birth outcomes, such as gestational length and height. When using variation of the structural breaks, we find more pronounced gender differences in birth outcomes, as shown in Figure 10. While the mothers of both male (blue) and female (red) infants are exposed to the same number of

[47] We consider that a municipality has a large break if the difference in the sum of the number of violent months in the year before and after the break is in the top 5 percent of the distribution when the difference is greater than zero.

[48] Dave and Yang (2022) state "one robust and striking pattern is that the adverse impact of prenatal lead exposure on fetal health appears to be concentrated among male fetuses, which is consistent with the fragile male hypothesis (Eriksson et al., 2010; Kraemer, 2000)." In contrast, Koppensteiner and Manacorda (2016), who study prenatal exposure to homicides in Brazil, find a marginally significant effect only for female infants on the likelihood of low birth weight (≤2,500 grams) during the first trimester of pregnancy. The authors find no effect for male infants and no effect for subsequent trimesters of pregnancy.

violent months after the break (panel (a)), the adverse effects on birth weight and gestational length are larger for male than for female infants.

4.7 Robustness

We conduct a number of robustness checks to bolster the validity of our empirical strategies.

Alternative Durations of Long-Term Exposure to Local Violence

Our definition of mothers' long-term exposure to local violence appears to lack a clear rationale, other than the availability of long-spanning homicide data. Hence, we first test if our main results are sensitive to alternative durations of long-term exposure to local violence. To do so, we iteratively estimate Equation (3) using our preferred specification after altering both the *starting* and *ending* point-in-time of exposure to local violence. We provide results in Figure 11, focusing on birth weight for the purpose of brevity.[49] In Figure 11 panel (a), we change the starting point-in-time of exposure to homicide—from 84 to 204 months before conception, with an increment of 12 months—after holding the ending point-in-time of exposure fixed at 12 months before conception. The results show that adopting alternative starting points of violence exposure provides a similar magnitude of the birth weight effects across different measures of long-term violence exposure. In Figure 11 panel (b), for each of the three starting points, that is—180, 192, and 204 months before conception—we vary the ending points of violence exposure from 1 to 24 months before conception, with an increment of 6 months. The results show that the birth weight effects are similar across different measures of long-term violence exposure.

Alternative Treatment Variable

We replicate our main results in Table 2 using a different measure of long-term exposure to local violence: the sum of the monthly homicide rates over the 15-year window (i.e., an

[49] A full set of results using all birth outcomes considered in Table 2 are available upon request.

absolute measure of local violence). The estimated effects, shown in Table A.7, are consistent with our main results reported in Table 2: infant health worsens as long-term pre-conception exposure to local violence increases.

Other Robustness Checks

We perform additional robustness checks to alleviate concerns regarding the sample restrictions on our analysis sample, the duration of postpartum exposure, and the inclusion of individual control variables in Table A.8.

We first relax the restrictions imposed on the analysis sample: we excluded births in municipalities with an average population of less than 10,000 people, a birth weight of less than 500 grams, a gestational age of less than 23 weeks, and a birth height of less than 27 centimeters. Panel A of Table A.8 shows that our main results, shown in Table 2, are not sensitive to the sample restrictions.

We also test whether our main results are sensitive to an alternative definition of postpartum exposure to violence, which considers homicides occurring 10 months after conception (10+ months after conception) rather than homicides occurring 10-21 months after conception.[50] Panel B of Table A.8 displays results that are consistent with the main results in Table 2.

Lastly, we test if the main results are sensitive to the inclusion of the infant's biological sex and mother's characteristics, such as the age of the mother at the time of conception, whether the mother is married at the time of giving birth, and whether the mother has completed high school education or above. Panel C of Table A.8 shows that the inclusion of infant sex and maternal characteristics does not affect our main results.

[50] Specifically, it captures homicides occurring 10-200 months after conception.

5 Conclusion

A large literature studying the effects of random shocks on infant health at birth focuses on in-utero exposure to such shocks (Currie and Rossin-Slater, 2013; Black et al., 2016; Koppensteiner and Manacorda, 2016; Brown, 2018; Persson and Rossin-Slater, 2018; Currie et al., 2022; Dave and Yang, 2022). However, we know little about the effects of long-term exposure to negative shocks on infant health at birth. Focusing on local violence and using administrative birth data from all newborns registered in Mexico between 2009 and 2019, we estimate the effects of mothers' long-term exposure to local violence before conception on different measures of infant health at birth.

We find that mothers' long-term pre-conception exposure to local violence has adverse impacts on infant health at birth. Our findings are consistent across two different empirical strategies. We show that our results are robust to alternative definitions of long-term exposure to local violence, different specifications, and alternative samples where we relax the restrictions imposed on the analysis sample. We further show that our results are not driven by selection into live births or by changes in the composition of mothers giving birth.

This study contributes to the growing literature on the effects of prenatal or early-life exposure to environmental factors on fetal or children's health by providing the first evidence of the impact of long-term pre-conception exposure to local violence on infant health. Our findings highlight intergenerational consequences of exposure to local violence and shed light on the costs of local violence not captured in in-utero exposure alone.

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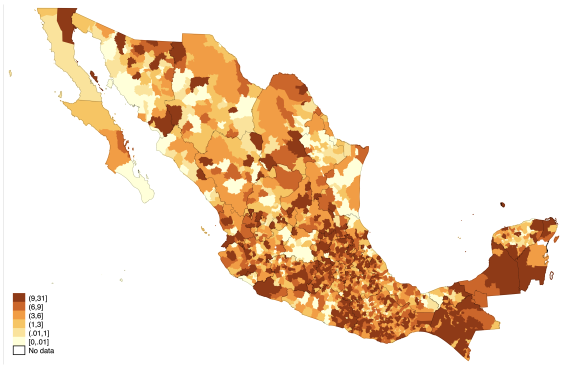
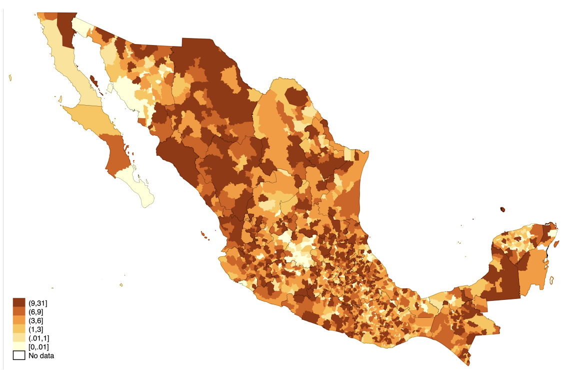
Figure 1: Geographic Distribution of Long-Term Pre-Conception Exposure to Local Violence by Year-Month of Conception

(a) January 2010



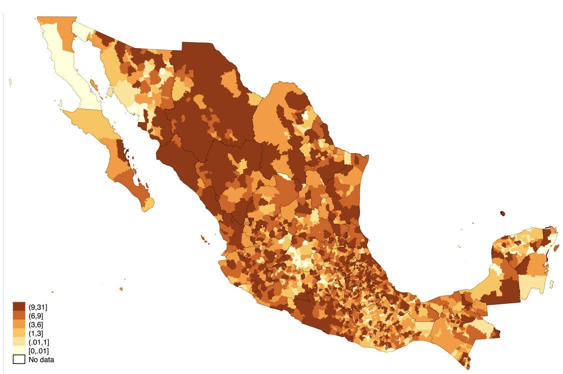
(b) January 2012

(c) January 2014



(d) January 2016

(e) January 2018

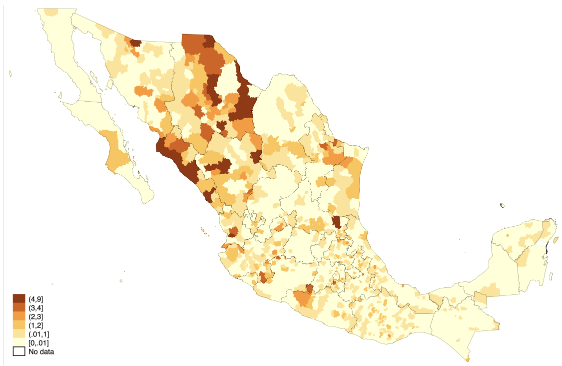


(f) January 2020

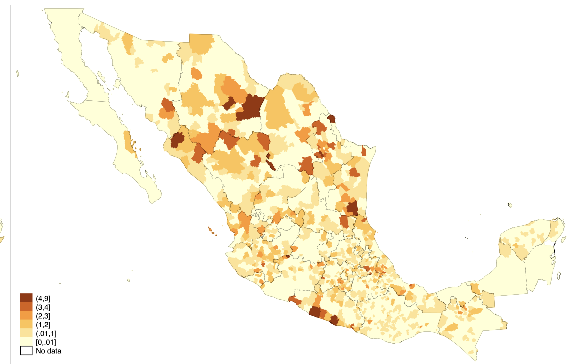
Notes: Pre-conception exposure to local violence is defined as the number of months from 192 to 12 months before the conception year-month with a homicide rate two standard deviations above the municipality's historical mean.

Figure 2: Geographic Distribution of In-Utero Exposure to Local Violence by Year-Month of Conception

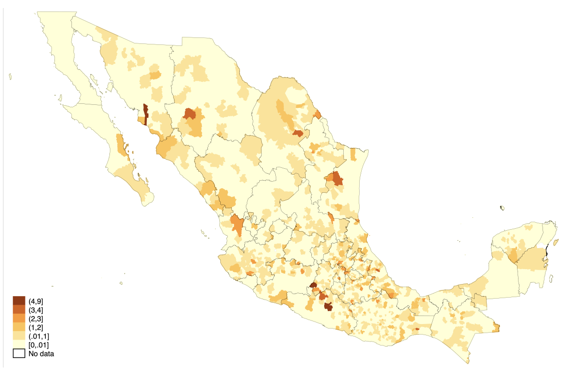
(a) January 2010



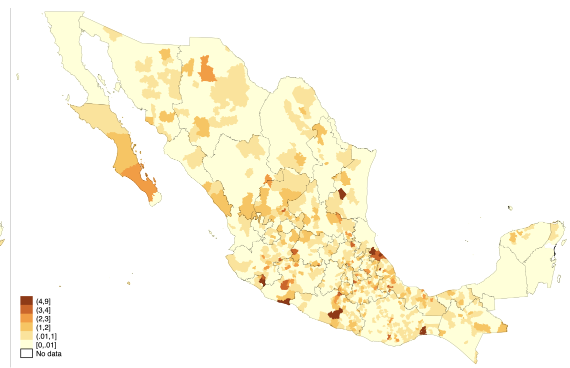
(b) January 2012



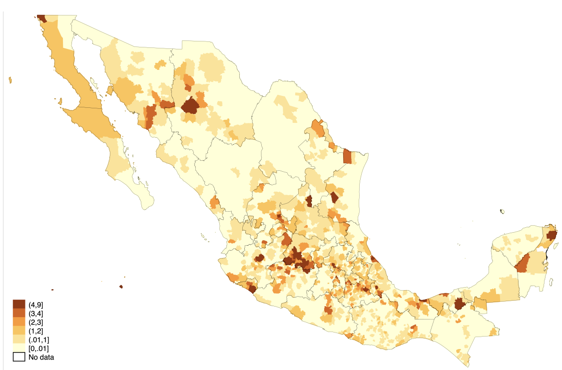
(c) January 2014



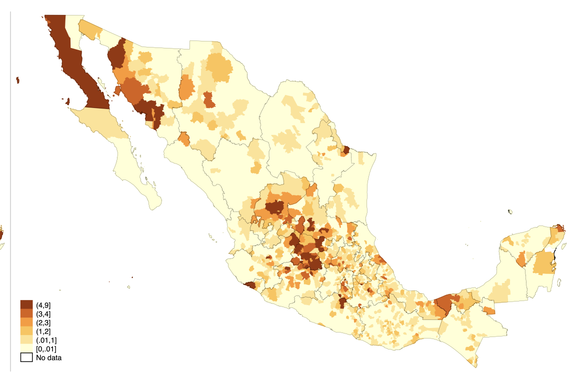
(d) January 2016



(e) January 2018



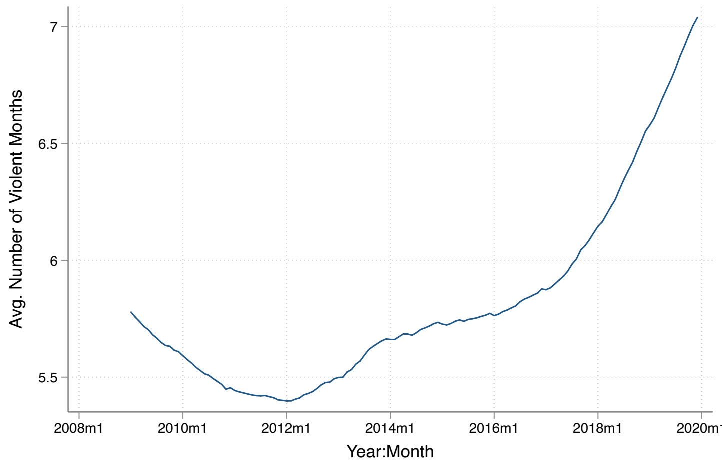
(f) January 2020



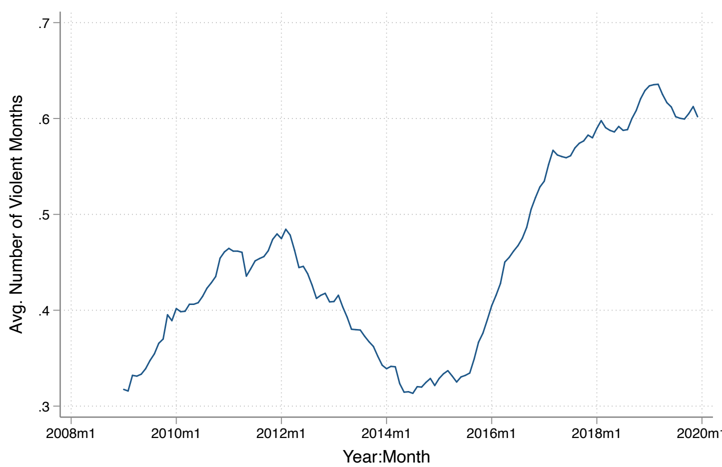
Notes: In-utero exposure to local violence is defined as the number of months from 1 to 9 months after the conception year-month with a homicide rate two standard deviations above the municipality's historical mean.

Figure 3: Municipalities' Average Long-Term Pre-Conception and In-Utero Exposure to Local Violence by Year-Month of Conception

(a) Pre-Conception



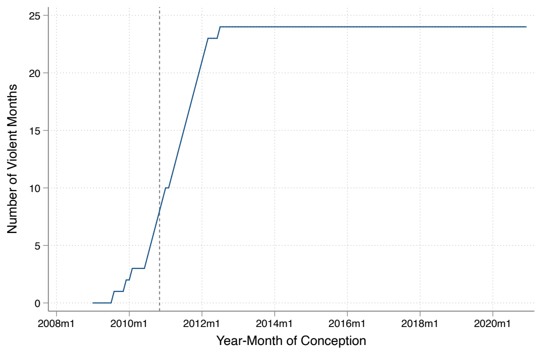
(b) In-Utero



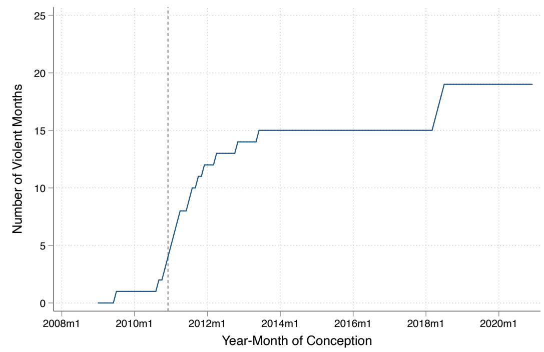
Notes: Panels (a) and (b) show the municipality-level average pre-conception and in-utero exposure to local violence by the year-month of conception. Pre-conception exposure to local violence is defined as the number of months from 192 to 12 months before the conception year-month with a homicide rate two standard deviations above the municipality's historical mean. In-utero exposure to local violence is defined as the number of months from 1 to 9 months after the conception year-month with a homicide rate two standard deviations above the municipality's historical mean.

Figure 4: Structural Breaks in Selected Cities

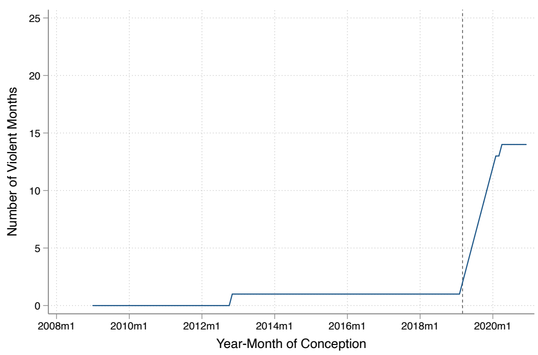
(a) Ciudad Juárez



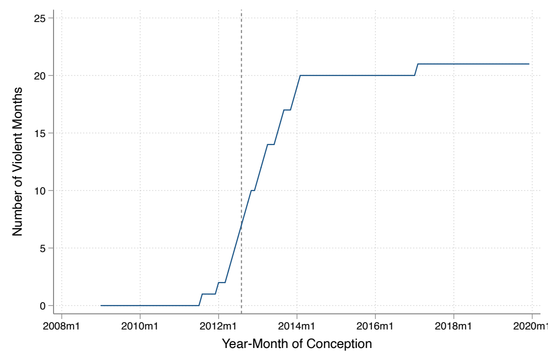
(b) Culiacán



(c) Guadalajara



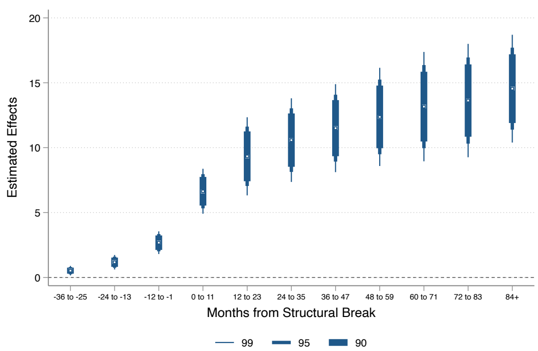
(d) Monterrey



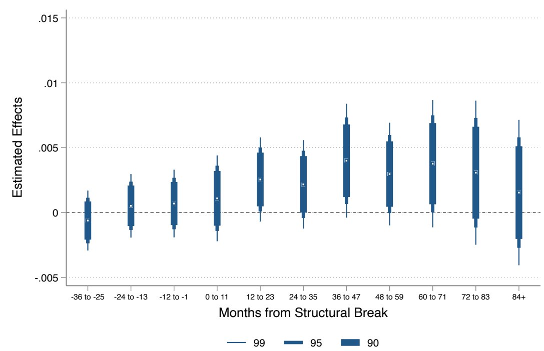
Notes: Each panel presents the time series of the number of violent months by year-month of conception for a sample of four municipalities. The vertical dashed line indicates the month of the structural break.

Figure 5: Effects of Long-Term Exposure to Local Violence on Birth Outcomes By Time From Structural Break

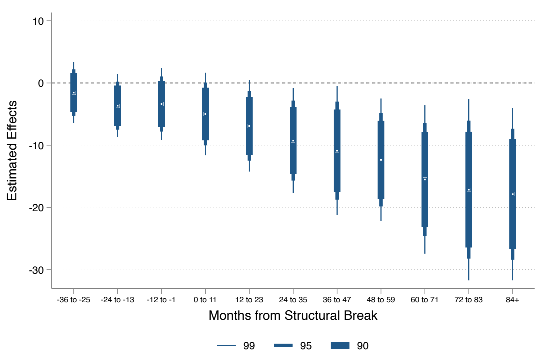
(a) Violent months



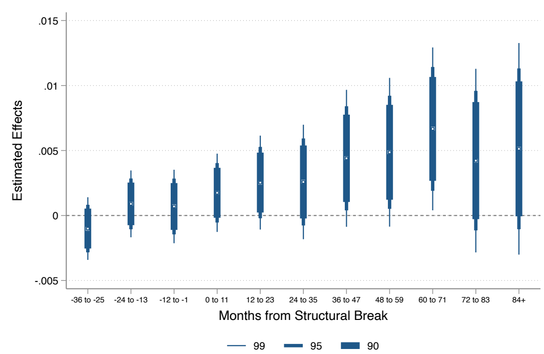
(b) Low birth weight (1/0)



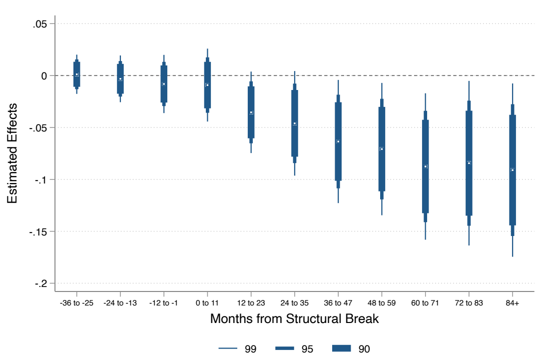
(c) Birth weight (in grams)



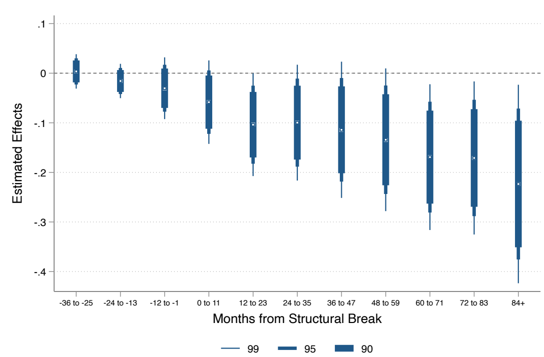
(d) Preterm birth (1/0)



(e) Gestation (in weeks)



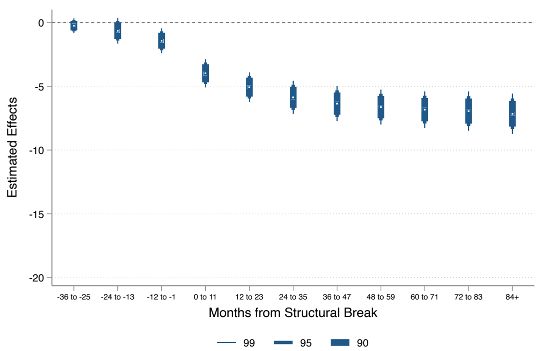
(f) Birth height (in centimeters)



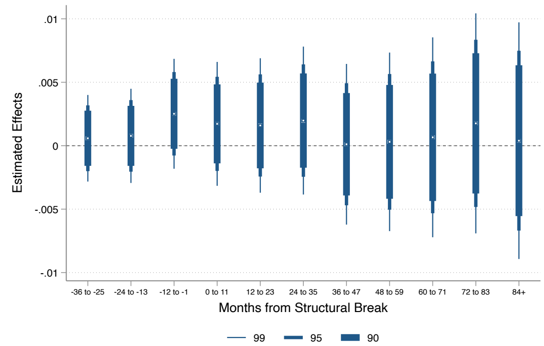
Notes: All coefficients from each panel come from the same regression that includes mothers' municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors for confidence intervals are clustered at the municipality level.

Figure 6: Effects of Long-Term Exposure to Local Violence on Birth Outcomes By Time From Negative Structural Break

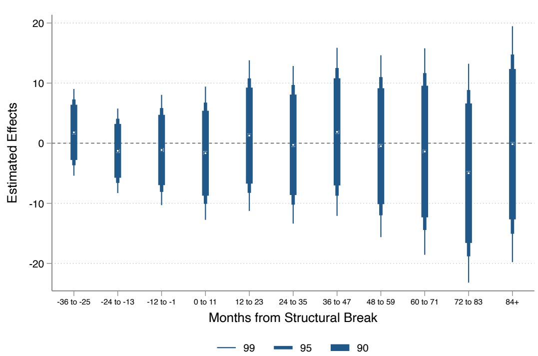
(a) Violent months



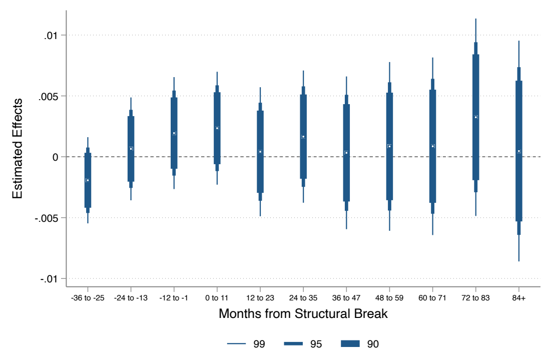
(b) Low birth weight (1/0)



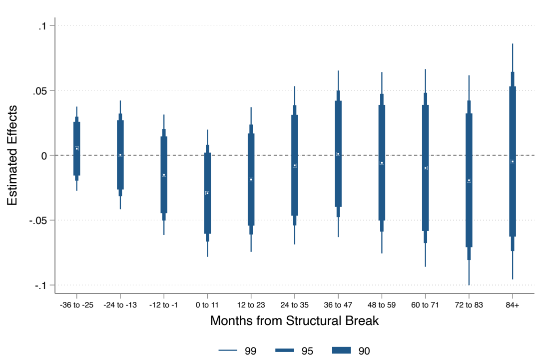
(c) Birth weight (in grams)



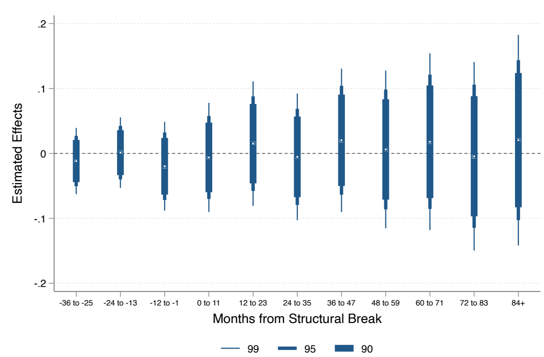
(d) Preterm birth (1/0)



(e) Gestation (in weeks)



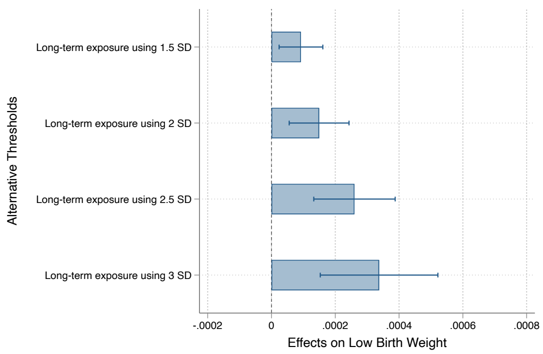
(f) Birth height (in centimeters)



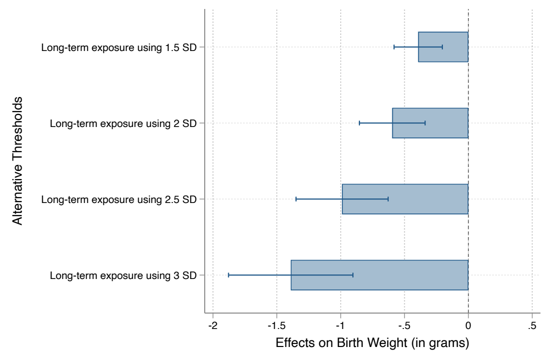
Notes: All coefficients from each panel come from the same regression that includes mothers' municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors for confidence intervals are clustered at the municipality level.

Figure 7: Effects of Long-Term Exposure to Local Violence on Birth Outcomes: Alternative Thresholds ($1.5\sigma_m$, $2\sigma_m$, $2.5\sigma_m$, $3\sigma_m$)

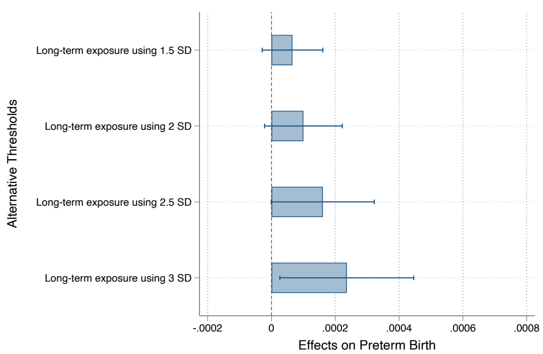
(a) Low birth weight (1/0)



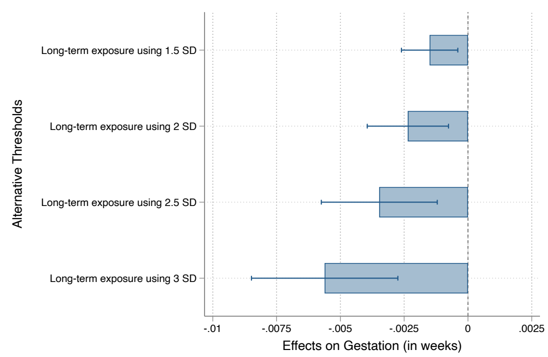
(b) Birth weight (in grams)



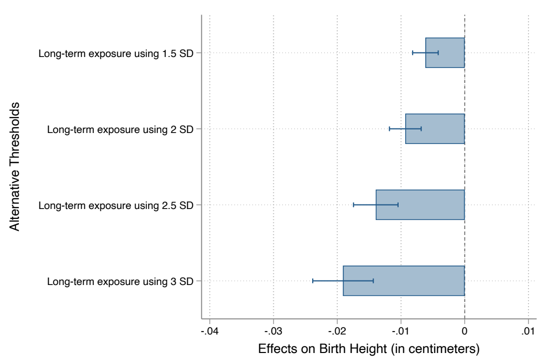
(c) Preterm birth (1/0)



(d) Gestation (in weeks)



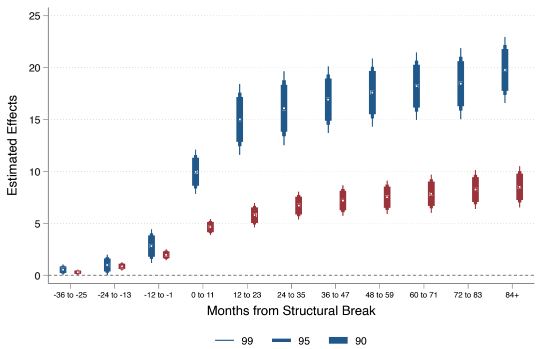
(e) Birth height (in centimeters)



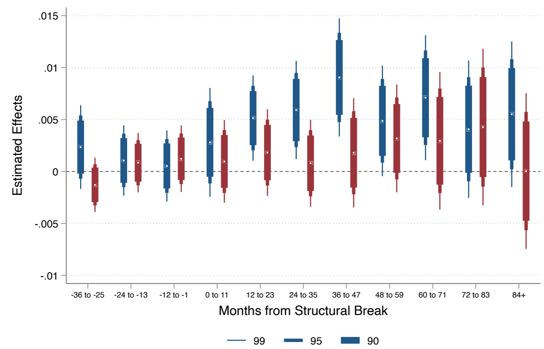
Notes: All figures show estimated coefficients and their 95% confidence intervals. Each estimate comes from a different regression. All regressions include mothers' municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors for confidence intervals are clustered at the municipality level.

Figure 8: Effects of Long-Term Exposure to Local Violence on Birth Outcomes By Time From Structural Break: By Intensity of Violence Exposure: Large Structural Break (blue) vs. Small Structural Break (red)

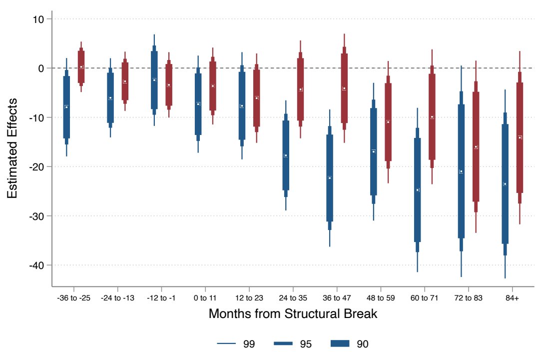
(a) Violent months



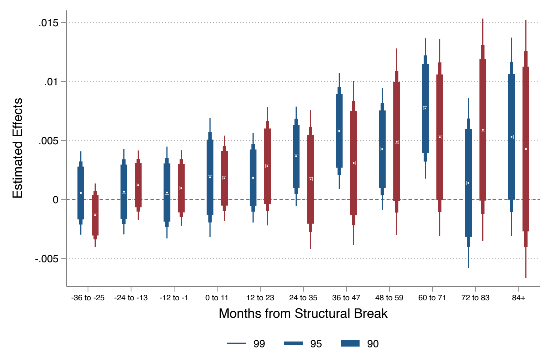
(b) Low birth weight (1/0)



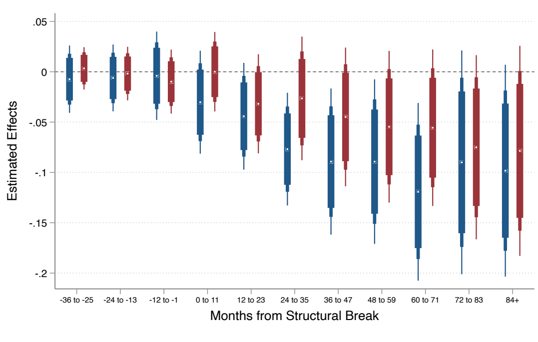
(c) Birth weight (in grams)



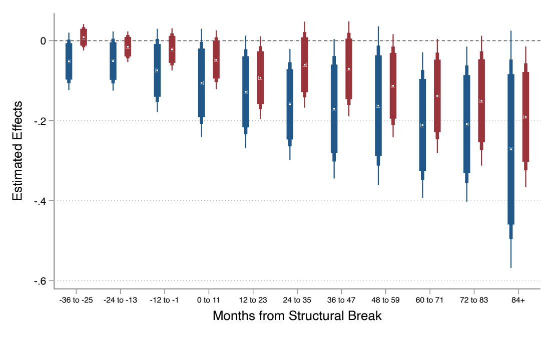
(d) Preterm birth (1/0)



(e) Gestation (in weeks)



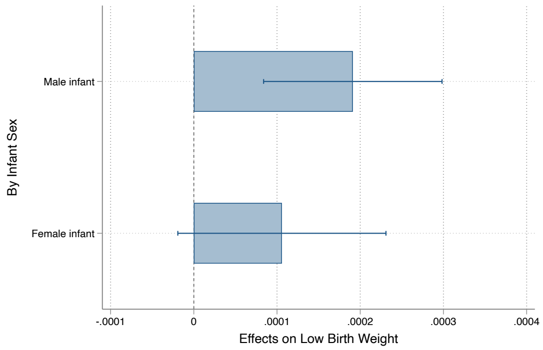
(f) Birth height (in centimeters)



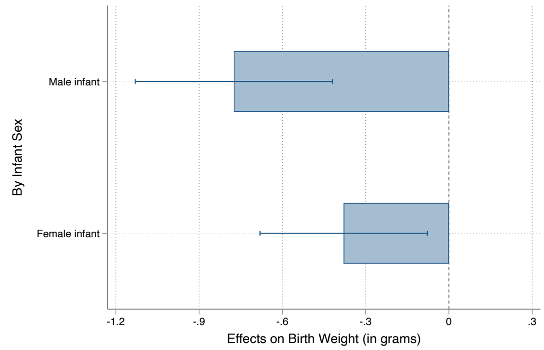
Notes: All coefficients for each group of municipalities (large vs. small break) from each panel come from the same regression that includes mothers' municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors for confidence intervals are clustered at the municipality level.

Figure 9: Effects of Long-Term Exposure to Local Violence on Birth Outcomes: By Infant Sex

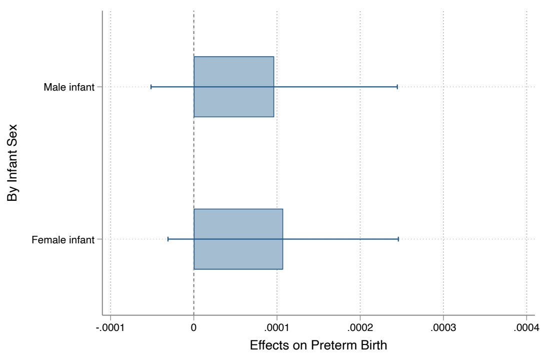
(a) Low birth weight (1/0)



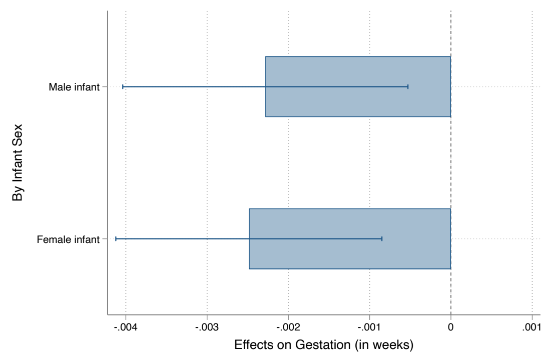
(b) Birth weight (in grams)



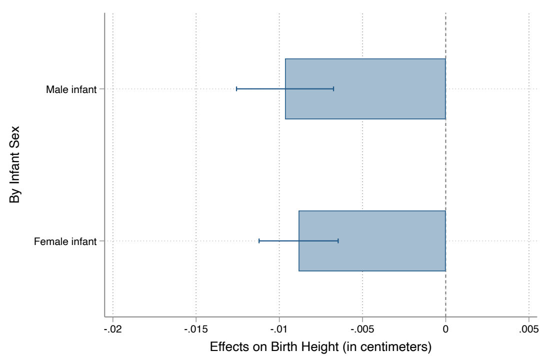
(c) Preterm birth (1/0)



(d) Gestation (in weeks)



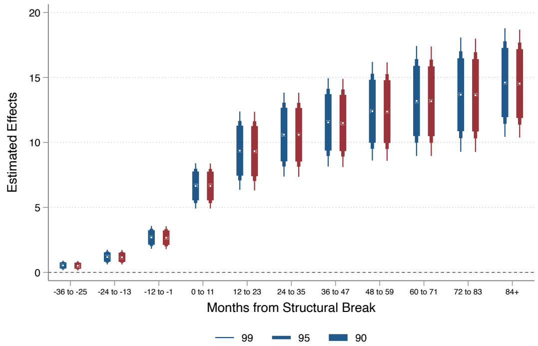
(e) Birth height (in centimeters)



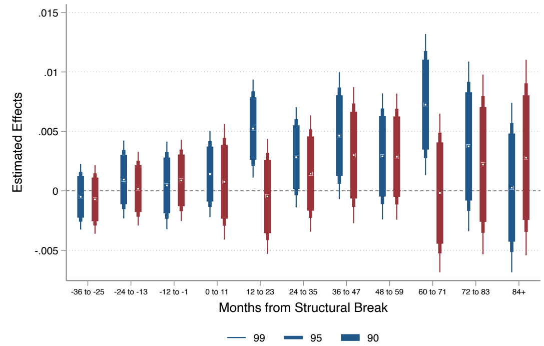
Notes: All figures show estimated coefficients and their 95% confidence intervals. Each estimate comes from a different regression. All regressions include mothers' municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors for confidence intervals are clustered at the municipality level.

Figure 10: Effects of Long-Term Exposure to Local Violence on Birth Outcomes By Time From Structural Break: By Gender of Infants: Male (blue) vs. Female (red)

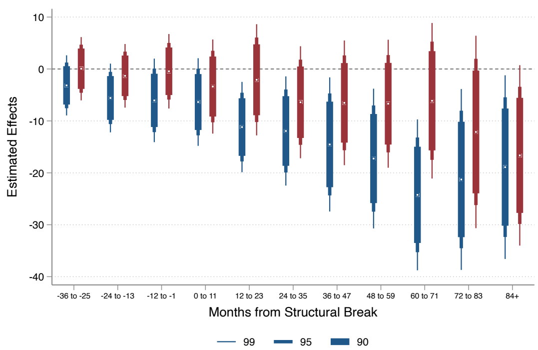
(a) Violent months



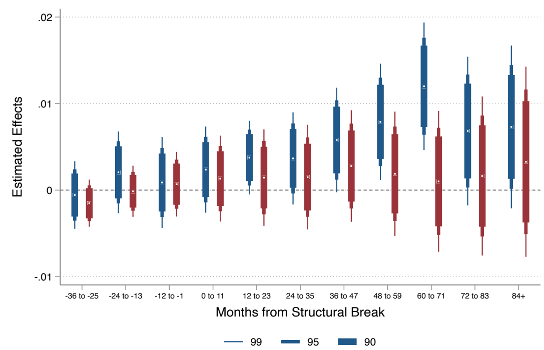
(b) Low birth weight (1/0)



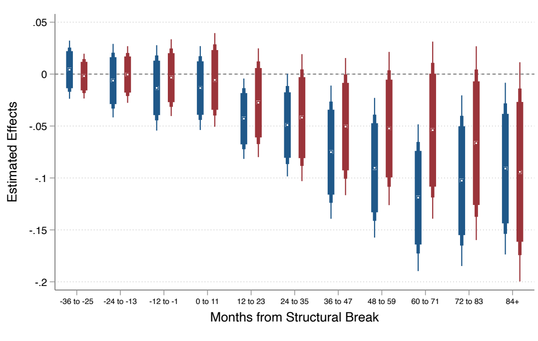
(c) Birth weight (in grams)



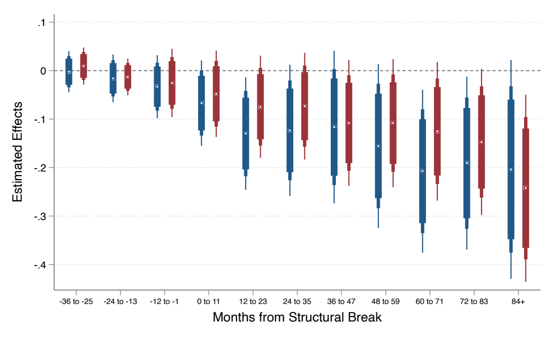
(d) Preterm birth (1/0)



(e) Gestation (in weeks)



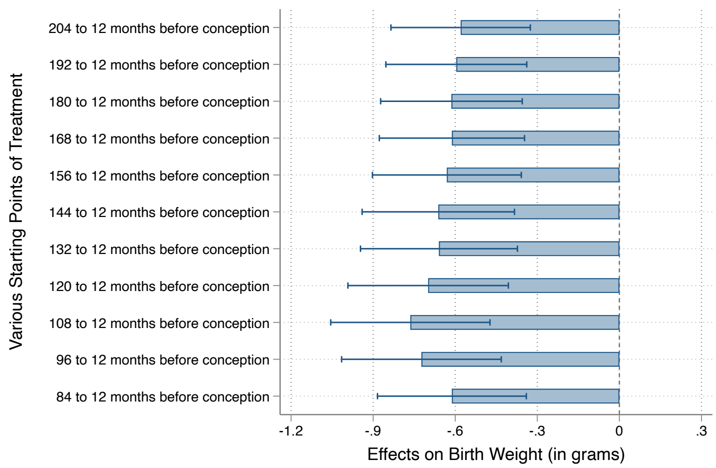
(f) Birth height (in centimeters)



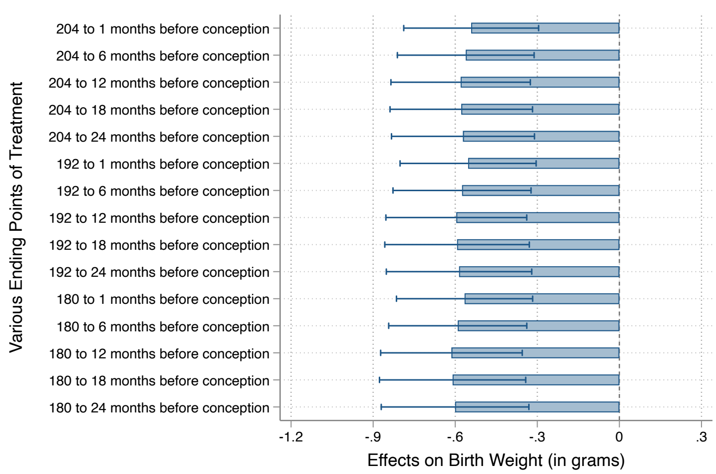
Notes: All coefficients for each gender of infants from each panel come from the same regression that includes mothers' municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors for confidence intervals are clustered at the municipality level.

Figure 11: Effects of Long-Term Exposure to Local Violence on Birth Outcomes: Alternative Starting - Ending Points of Exposure

(a) Alternative starting points



(b) Alternative ending points



Notes: All figures show estimated coefficients and their 95% confidence intervals. Each estimate comes from a different regression. All regressions include mothers' municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors for confidence intervals are clustered at the municipality level.

Table 1: Summary Statistics

	(1)
Low Birth Weight (1/0, birth weight less than 2,500 grams)	0.06
	(0.24)
Birth weight (in grams)	3,131.11
	(466.36)
Preterm birth (1/0, gestational length less than 37 weeks)	0.07
	(0.25)
Gestational length (in weeks)	38.76
	(1.78)
Height (in centimeters)	49.85
	(2.66)
Female Infant (1/0)	0.49
	(0.50)
Mother being married at birth (1/0)	0.45
	(0.50)
Age of mother at conception	25.08
	(4.72)
Mother having completed a high school education or higher (1/0)	0.61
	(0.49)
C-section delivery (1/0)	0.58
	(0.49)
APGAR 5-minute score	8.88
	(0.83)
Stillborn (1/0)	0.12
	(0.33)
Mover (1/0)	0.46
	(0.50)
Long-term (192 to 12 months before conception)	5.12
	(5.37)
During pregnancy (1-9 months after conception)	0.62
	(1.31)
Post-birth (10-21 months after conception)	0.93
	(1.80)
First trimester (1-3 months after conception)	0.20
	(0.54)
Second trimester (4-6 months after conception)	0.21
	(0.55)
Third trimester (7-9 months after conception)	0.21
	(0.57)

Notes: Each cell shows the mean of the listed variables. Standard deviations are shown in parentheses. Long-term indicates the number of violent months a mother is exposed to in the 15-year window prior to conception.

Table 2: Effects of Long-Term Exposure to Local Violence on Birth Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variables:	Low birth weight (1/0)		Birth weight (in grams)		Preterm birth (1/0)		Gestation (in weeks)		Height (in centimeters)	
Panel A										
Long-term (192 to 12 months before conception)	0.0002***	0.0001***	-0.6148***	-0.5961***	0.0001	0.0001	-0.0023***	-0.0024***	-0.0094***	-0.0093***
	(0.0000)	(0.0000)	(0.1299)	(0.1312)	(0.0001)	(0.0001)	(0.0008)	(0.0008)	(0.0013)	(0.0013)
During pregnancy (1-9 months after conception)	-0.0001	-0.0001	0.0939	0.1110	-0.0002	-0.0002	0.0018	0.0017	-0.0003	-0.0002
	(0.0001)	(0.0001)	(0.2563)	(0.2601)	(0.0002)	(0.0002)	(0.0013)	(0.0014)	(0.0028)	(0.0029)
Post-birth (10-21 months after conception)	-0.0001	-0.0001	-0.2339	-0.2332	-0.0001	-0.0001	0.0003	0.0004	-0.0014	-0.0013
	(0.0001)	(0.0001)	(0.2322)	(0.2314)	(0.0001)	(0.0001)	(0.0011)	(0.0011)	(0.0022)	(0.0022)
Observations	4239974	4239956	4239974	4239956	4470964	4470947	4470964	4470947	4365165	4365147
Municipality of mother's birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-month of conception FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-month of conception FE		Yes		Yes		Yes		Yes		Yes
Panel B: By trimester of pregnancy										
Long-term (192 to 12 months before conception)	0.0002***	0.0002***	-0.6173***	-0.6011***	0.0001	0.0001*	-0.0023***	-0.0024***	-0.0094***	-0.0094***
	(0.0000)	(0.0000)	(0.1302)	(0.1314)	(0.0001)	(0.0001)	(0.0008)	(0.0008)	(0.0013)	(0.0013)
First trimester (1-3 months after conception)	0.0000	0.0001	-0.5650	-1.0688**	0.0003	0.0004	-0.0011	-0.0023	-0.0050	-0.0064
	(0.0002)	(0.0002)	(0.4715)	(0.5046)	(0.0003)	(0.0003)	(0.0021)	(0.0023)	(0.0042)	(0.0043)
Second trimester (4-6 months after conception)	-0.0001	-0.0000	0.7321	0.7957	-0.0004	-0.0004	0.0031	0.0032	0.0013	0.0013
	(0.0003)	(0.0003)	(0.5117)	(0.5666)	(0.0003)	(0.0003)	(0.0022)	(0.0023)	(0.0040)	(0.0043)
Third trimester (7-9 months after conception)	-0.0002	-0.0004	0.0741	0.5855	-0.0004	-0.0005*	0.0033	0.0042*	0.0029	0.0045
	(0.0003)	(0.0003)	(0.5376)	(0.5293)	(0.0003)	(0.0003)	(0.0025)	(0.0025)	(0.0048)	(0.0048)
Post-birth (10-21 months after conception)	-0.0001	-0.0001	-0.2520	-0.2910	-0.0001	-0.0001	0.0002	0.0001	-0.0017	-0.0017
	(0.0001)	(0.0001)	(0.2269)	(0.2234)	(0.0001)	(0.0001)	(0.0011)	(0.0011)	(0.0021)	(0.0021)
Observations	4239974	4239956	4239974	4239956	4470964	4470947	4470964	4470947	4365165	4365147
Municipality of mother's birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-month of conception FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-month of conception FE		Yes		Yes		Yes		Yes		Yes

Notes: All regressions include mother's municipality of birth fixed effects, year-month of conception fixed effects. Standard errors in parentheses are clustered at the municipality level. *, **, *** significant at the 10%, 5%, and 1% level, respectively.

Table 3: Effects of Long-Term Exposure to Local Violence on Birth Outcomes: 30-month Windows of Violence Exposure

Dependent variables:	(1) Low birth weight (1/0)	(2) Birth weight (in grams)	(3) Preterm birth (1/0)	(4) Gestation (in weeks)	(5) Height (in centimeters)
Long-term (191 to 162 months before conception)	0.0003	-0.6420*	0.0000	-0.0013	-0.0074**
	(0.0002)	(0.3356)	(0.0002)	(0.0021)	(0.0029)
Long-term (161 to 132 months before conception)	0.0000	-0.3577	-0.0001	-0.0011	-0.0090**
	(0.0002)	(0.3932)	(0.0002)	(0.0022)	(0.0037)
Long-term (131 to 102 months before conception)	-0.0002	0.0852	-0.0001	-0.0007	-0.0128***
	(0.0001)	(0.3777)	(0.0002)	(0.0016)	(0.0034)
Long-term (101 to 72 months before conception)	0.0001	-0.7636***	0.0002	-0.0037**	-0.0107***
	(0.0001)	(0.2243)	(0.0001)	(0.0014)	(0.0021)
Long-term (71 to 42 months before conception)	0.0002***	-0.8256***	0.0003***	-0.0045***	-0.0117***
	(0.0001)	(0.1805)	(0.0001)	(0.0013)	(0.0021)
Long-term (41 to 12 months before conception)	0.0002**	-0.4851***	0.0001	-0.0011	-0.0071***
	(0.0001)	(0.1520)	(0.0001)	(0.0010)	(0.0022)
During pregnancy (1-9 months after conception)	-0.0001	0.0261	-0.0001	0.0009	-0.0012
	(0.0001)	(0.2790)	(0.0001)	(0.0012)	(0.0025)
Post-birth (10-21 months after conception)	-0.0001	-0.2838	-0.0001	-0.0001	-0.0018
	(0.0001)	(0.2136)	(0.0001)	(0.0009)	(0.0021)
Observations	4239956	4239956	4470947	4470947	4365147
Municipality of mother's birth FE	Yes	Yes	Yes	Yes	Yes
Year-month of conception FE	Yes	Yes	Yes	Yes	Yes
Municipality-month of conception FE	Yes	Yes	Yes	Yes	Yes

Notes: All regressions include mother's municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors in parentheses are clustered at the municipality level. *, **, *** significant at the 10%, 5%, and 1% level, respectively.

Table 4: Effects of Long-Term Exposure to Local Violence on Birth Outcomes: OLS vs. Imputation Estimator

Dependent variables:	(1) Long-Term Exposure (in months)	(2) Low birth weight (1/0)	(3) Birth weight (in grams)	(4) Preterm birth (1/0)	(5) Gestation (in weeks)	(6) Height (in centimeters)
Panel A: OLS Estimator						
Post Structural Break	7.5461***	0.0020**	-4.7377***	0.0026*	-0.0336**	-0.0764**
	(0.8840)	(0.0009)	(1.7718)	(0.0015)	(0.0146)	(0.0298)
Observations	3165008	3008297	3008297	3163456	3163456	3087149
Panel B: Imputation Estimator						
Post Structural Break	8.8485***	0.0022***	-6.2563***	0.0027	-0.0427***	-0.0938***
	(0.3512)	(0.0007)	(1.5329)	(0.0019)	(0.0159)	(0.0280)
1 month prior	3.0779***	0.0030	-0.9432	-0.0020	0.0174	0.0025
	(0.2707)	(0.0021)	(4.4055)	(0.0028)	(0.0175)	(0.0310)
2 months prior	2.5213***	0.0005	1.7593	0.0008	0.0205	-0.0157
	(0.2336)	(0.0023)	(4.5415)	(0.0026)	(0.0190)	(0.0365)
3 months prior	2.1906***	-0.0009	2.0373	-0.0008	0.0085	-0.0067
	(0.2083)	(0.0021)	(4.8777)	(0.0030)	(0.0214)	(0.0380)
4 months prior	1.8076***	0.0029	-0.9729	-0.0018	0.0231	-0.0044
	(0.1803)	(0.0022)	(4.6062)	(0.0029)	(0.0207)	(0.0296)
5 months prior	1.4296***	0.0003	5.2200	-0.0024	0.0277	0.0119
	(0.1522)	(0.0021)	(4.1684)	(0.0029)	(0.0183)	(0.0368)
Observations	3164871	3008150	3008150	3163326	3163326	3087019
Municipality of mother's birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-month of conception FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-month of conception FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Regressions include mothers' municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors in parentheses are clustered at the municipality level. *, **, *** significant at the 10%, 5%, and 1% level, respectively.

Table 5: Effects of Exposure to Safe Months on Birth Outcomes

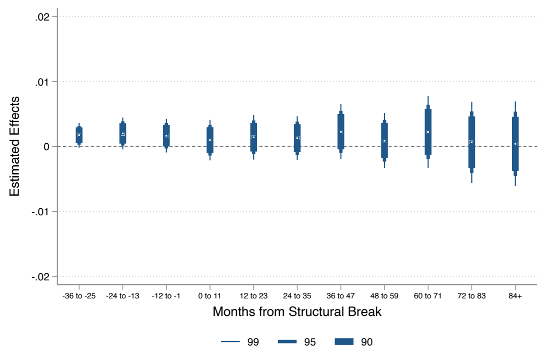
Dependent variables:	(1) Low birth weight (1/0)	(2) Birth weight (in grams)	(3) Preterm birth (1/0)	(4) Gestation (in weeks)	(5) Height (in centimeters)
Long-term (192-12 months before conception)	-0.0005	0.2898	-0.0022	0.0220	0.0407
	(0.0009)	(1.3147)	(0.0020)	(0.0180)	(0.0370)
During pregnancy (1-9 months after conception)	0.0007	0.1356	0.0074	-0.0544	-0.0583
	(0.0011)	(1.9147)	(0.0063)	(0.0499)	(0.0503)
Post-birth (10-21 months after conception)	-0.0011**	1.8145	-0.0002	0.0008	0.0049
	(0.0004)	(1.2961)	(0.0007)	(0.0095)	(0.0087)
Observations	4239956	4239956	4470947	4470947	4365147
Municipality of mother's birth FE	Yes	Yes	Yes	Yes	Yes
Year-month of conception FE	Yes	Yes	Yes	Yes	Yes
Municipality-month of conception FE	Yes	Yes	Yes	Yes	Yes

Notes: All regressions include mother's municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors in parentheses are clustered at the municipality level. *, **, *** significant at the 10%, 5%, and 1% level, respectively.

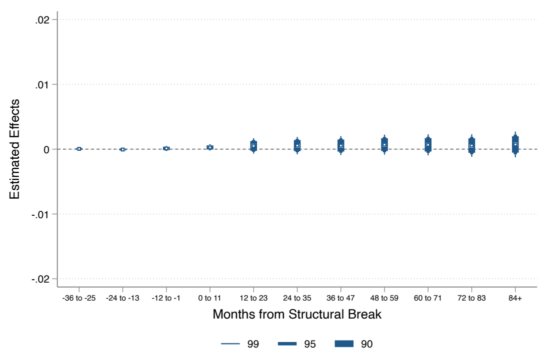
Appendix A

Figure A.1: Effects of Long-Term Exposure to Local Violence on Attrition By Time From Structural Break

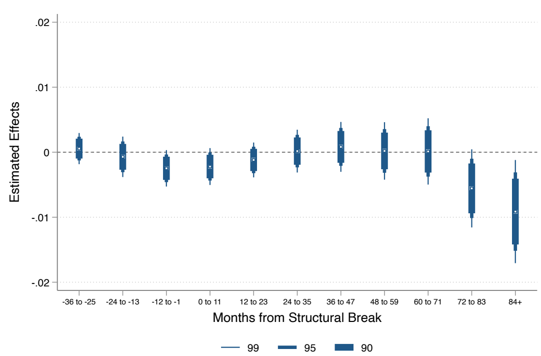
(a) Birth weight (1/0)



(b) Gestation (1/0)

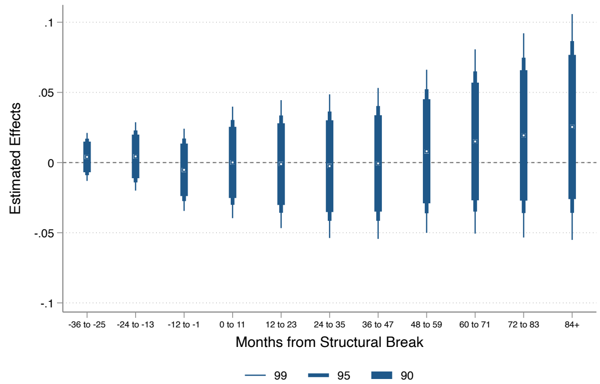


(c) Birth height (1/0)



Notes: All coefficients from each panel come from the same regression that includes mothers' municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors for confidence intervals are clustered at the municipality level.

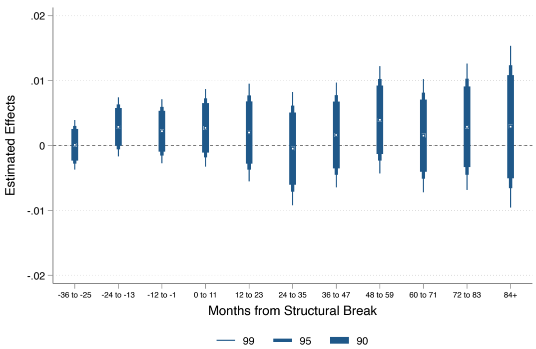
Figure A.2: Effects of Long-Term Exposure to Local Violence on the Number of Births By Time From Structural Break



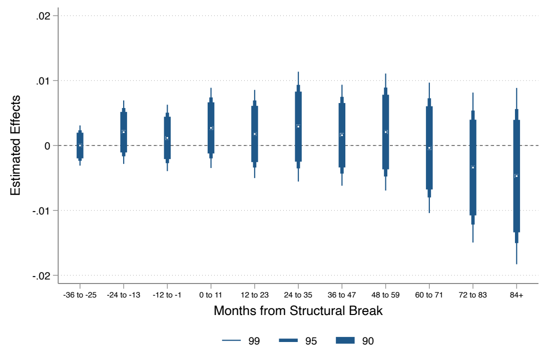
Notes: All coefficients come from the same regression that includes mothers' municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Estimates come from a Poisson model estimated by Pseudo-Maximum Likelihood that uses municipalities' female population as the exposure variable. Standard errors for confidence intervals are clustered at the municipality level.

Figure A.3: Effects of Long-Term Exposure to Local Violence on the Likelihood of a Female Birth and Stillbirth By Time From Structural Break

(a) Female infant (1/0)



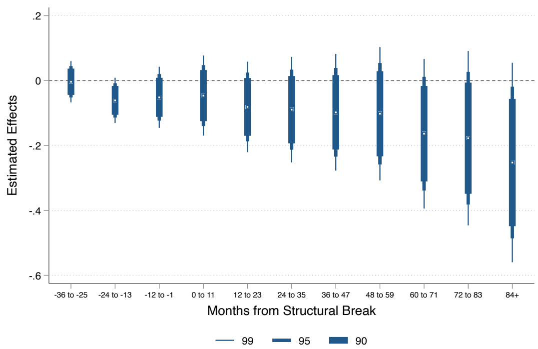
(b) Stillbirth (1/0)



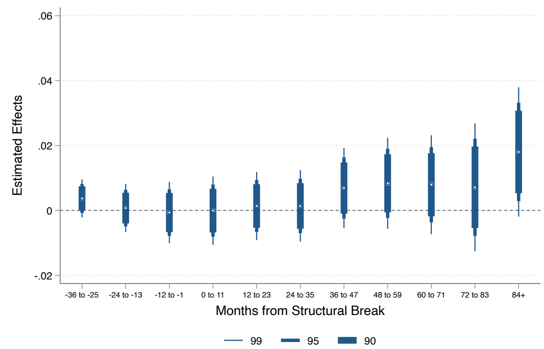
Notes: All coefficients from each panel come from the same regression that includes mothers' municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors for confidence intervals are clustered at the municipality level.

Figure A.4: Effects of Long-Term Exposure to Local Violence on Maternal Characteristics By Time From Structural Break

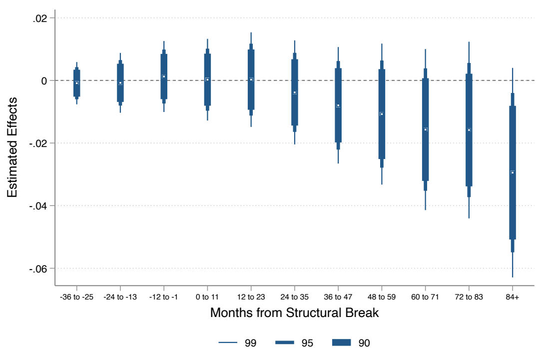
(a) Age at conception



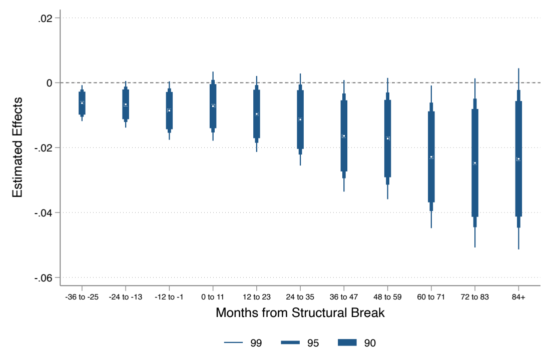
(b) Married (1/0)



(c) Mover (1/0)

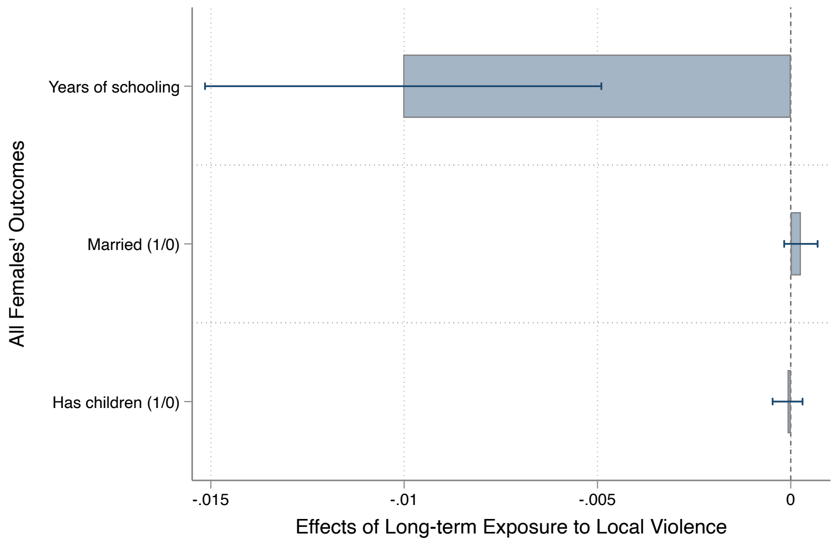


(d) High school & above (1/0)



Notes: All coefficients from each panel come from the same regression that includes mothers' municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors for confidence intervals are clustered at the municipality level.

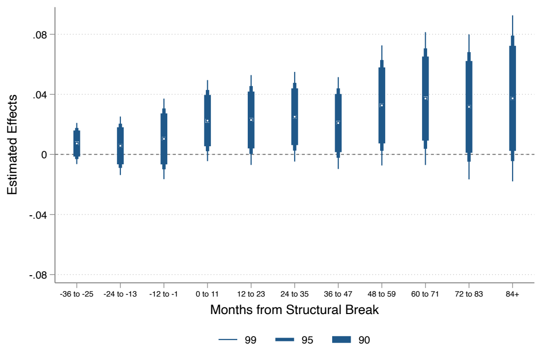
Figure A.5: Effects of Long-Term Exposure to Local Violence on Females Characteristics



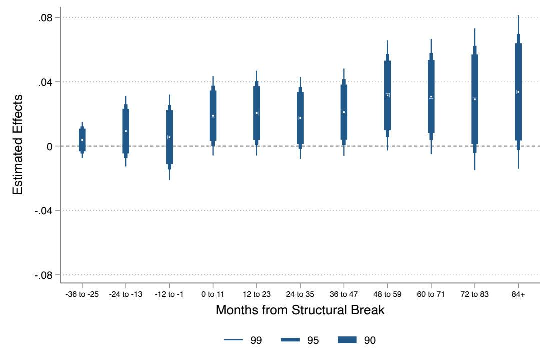
Notes: The figure shows estimated coefficients and their 95% confidence intervals. Each estimate comes from a different regression. All regressions include females' municipality of residence fixed effects and year fixed effects. Standard errors for confidence intervals are clustered at the municipality level.

Figure A.6: Effects of Long-Term Exposure to Local Violence on Mental Health By Time From Structural Break: Females Only

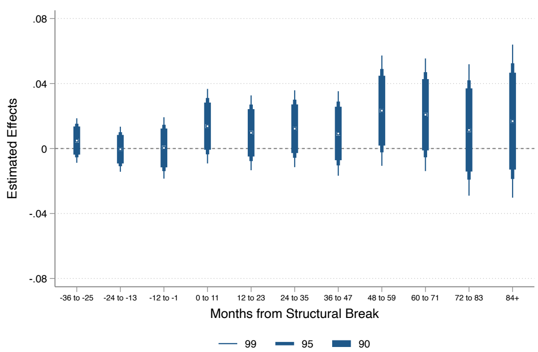
(a) Depression and Anxiety



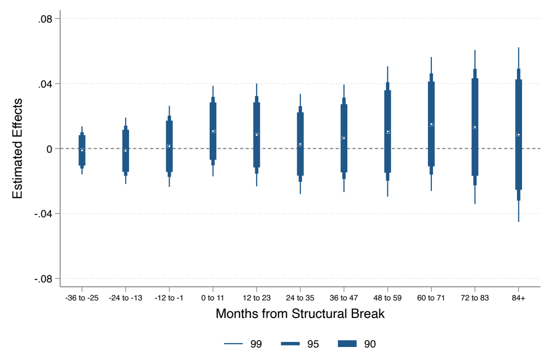
(b) Depression and Irritated



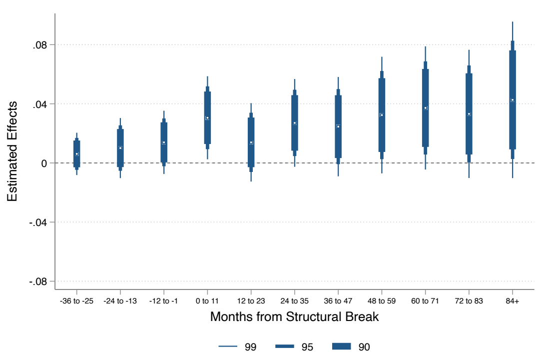
(c) Depression and Exhausted



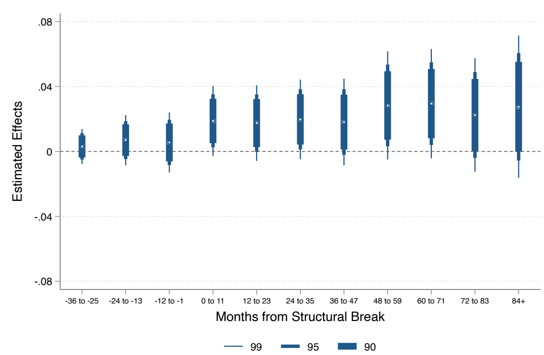
(d) Anxiety and Irritated



(e) Anxiety and Exhausted



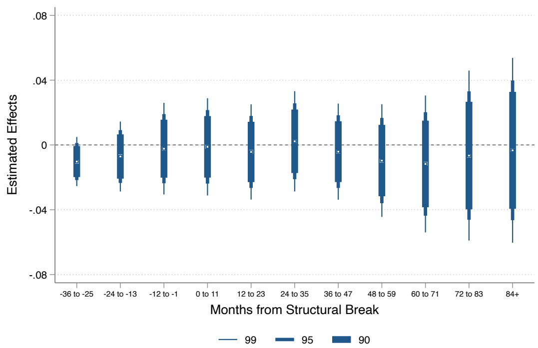
(f) Exhausted and Irritated



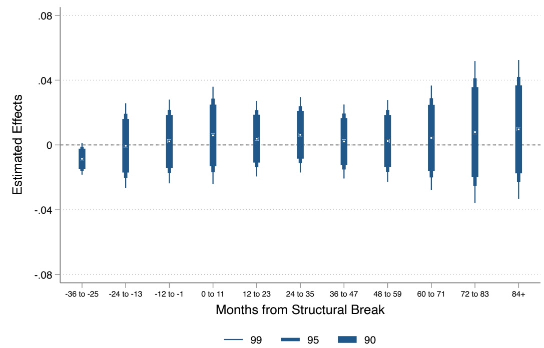
Notes: All coefficients from each panel come from the same regression that includes survey respondents' municipality of residence fixed effects and calendar year-month fixed effects. Standard errors for confidence intervals are clustered at the municipality level.

Figure A.7: Effects of Long-Term Exposure to Local Violence on Mental Health By Time From Structural Break: Males Only

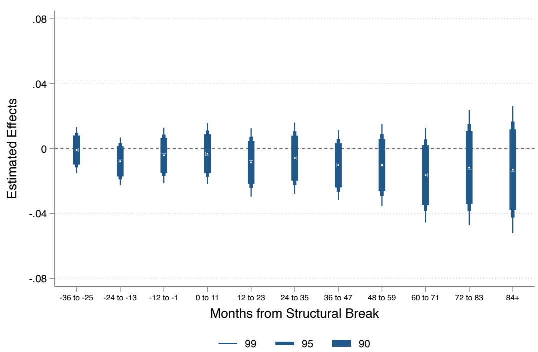
(a) Depression and Anxiety



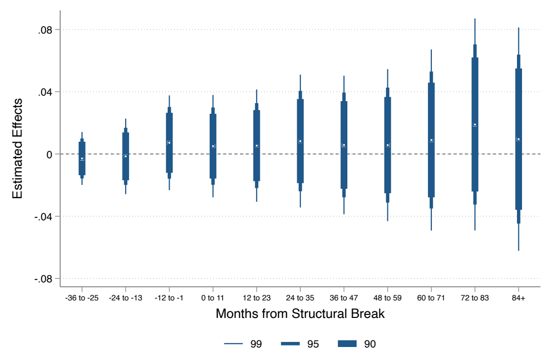
(b) Depression and Irritated



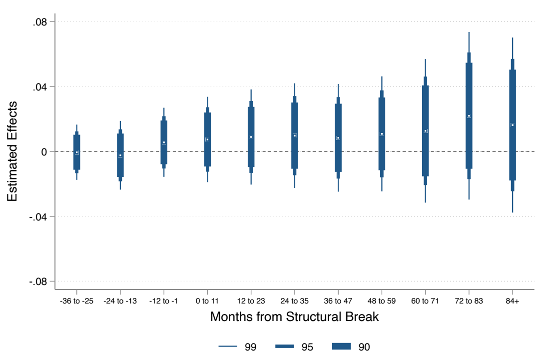
(c) Depression and Exhausted



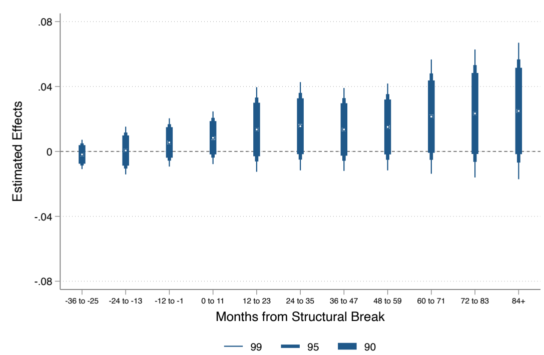
(d) Anxiety and Irritated



(e) Anxiety and Exhausted



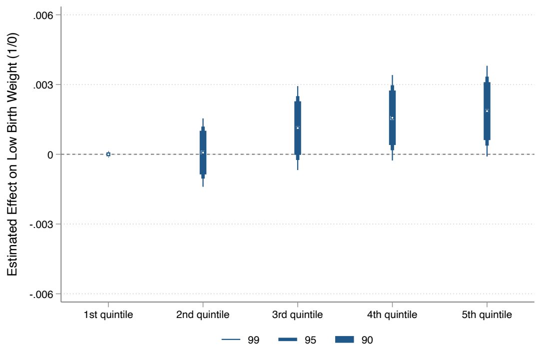
(f) Exhausted and Irritated



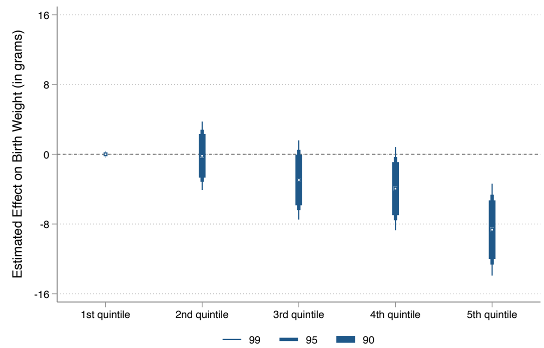
Notes: All coefficients from each panel come from the same regression that includes survey respondents' municipality of residence fixed effects and calendar year-month fixed effects. Standard errors for confidence intervals are clustered at the municipality level.

Figure A.8: Effects of Long-Term Exposure to Local Violence on Birth Outcomes: By Quintile

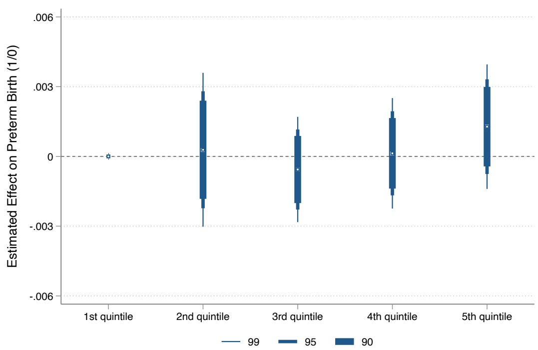
(a) Low birth weight (1/0)



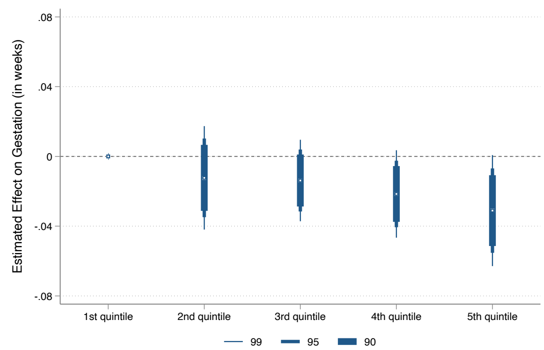
(b) Birth weight (in grams)



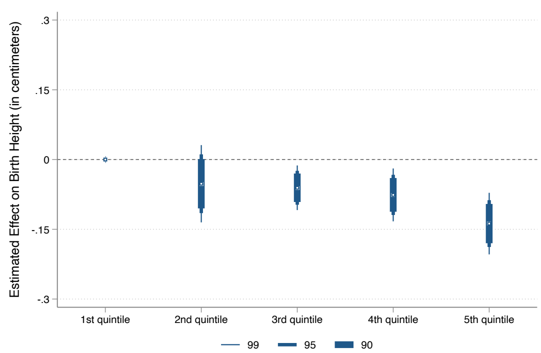
(c) Preterm birth (1/0)



(d) Gestation (in weeks)



(e) Birth height (in centimeters)



Notes: All regressions include mother's municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors for confidence intervals are clustered at the municipality level.

Table A.1: Effects of Long-Term Exposure to Local Violence on Attrition

Dependent variables:	(1) Birth weight (1/0)	(2) Gestation (1/0)	(3) Birth height (1/0)
Long-term (192 to 12 months before conception)	-0.0001	0.0000	-0.0001*
	(0.0001)	(0.0000)	(0.0001)
During pregnancy (1-9 months after conception)	0.0002	-0.0000	-0.0001
	(0.0001)	(0.0000)	(0.0002)
Post-birth (10-21 months after conception)	0.0003***	-0.0000	0.0000
	(0.0001)	(0.0000)	(0.0001)
Observations	4472878	4472878	4472878
Municipality of mother's birth FE	Yes	Yes	Yes
Year-month of conception FE	Yes	Yes	Yes
Municipality-month of conception FE	Yes	Yes	Yes

Notes: All regressions include mother's municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors in parentheses are clustered at the municipality level. *, **, *** significant at the 10%, 5%, and 1% level, respectively.

Table A.2: Effects of Long-Term Exposure to Local Violence on Other Birth Outcomes

	(1)	(2)
Dependent variables:	C-section (1/0)	5-min APGAR
Long-term (192 to 12 months before conception)	0.0008***	-0.0001
	(0.0003)	(0.0008)
During pregnancy (1-9 months after conception)	-0.0007	-0.0000
	(0.0006)	(0.0013)
Post-birth (10-21 months after conception)	0.0004	-0.0022
	(0.0005)	(0.0014)
Observations	4466232	4433917
Municipality of mother's birth FE	Yes	Yes
Year-month of conception FE	Yes	Yes
Municipality-month of conception FE	Yes	Yes

Notes: All regressions include mother's municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors in parentheses are clustered at the municipality level. *, **, *** significant at the 10%, 5%, and 1% level, respectively.

Table A.3: Effects of Long-Term Exposure to Local Violence on Birth Rates

Dependent variables:	(1) Birth Rate	(2) Log of Birth Rate	(3) # of Births
Long-term (192 to 12 months before conception)	-0.0007	-0.0003	-0.0012
	(0.0007)	(0.0004)	(0.0011)
During pregnancy (1-9 months after conception)	-0.0003	-0.0002	-0.0004
	(0.0016)	(0.0009)	(0.0012)
Post-birth (10-21 months after conception)	0.0005	0.0002	0.0009
	(0.0010)	(0.0006)	(0.0010)
Observations	173696	173696	173578
Municipality of mother's birth FE	Yes	Yes	Yes
Year-month of conception FE	Yes	Yes	Yes
Municipality-month of conception FE	Yes	Yes	Yes

Notes: All regressions include mother's municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Estimates in columns (1) and (2) are weighted by the population of females in the municipality. Estimates in Column (3) are estimated by Pseudo-Maximum Likelihood using municipalities' female population as the exposure variable. Standard errors in parentheses are clustered at the municipality level. *, **, *** significant at the 10%, 5%, and 1% level, respectively.

Table A.4: Effects of Long-Term Exposure to Local Violence on the Likelihood of a Female Birth and Stillbirth

	(1)	(2)
Dependent variables:	Female Infant (1/0)	Stillbirth (1/0)
Long-term (192 to 12 months before conception)	0.0001	-0.0000
	(0.0001)	(0.0001)
During pregnancy (1-9 months after conception)	0.0004*	0.0001
	(0.0002)	(0.0003)
Post-birth (10-21 months after conception)	0.0001	-0.0002
	(0.0002)	(0.0002)
Observations	4469732	4439961
Municipality of mother's birth FE	Yes	Yes
Year-month of conception FE	Yes	Yes
Municipality-month of conception FE	Yes	Yes

Notes: All regressions include mother's municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors in parentheses are clustered at the municipality level. *, **, *** significant at the 10%, 5%, and 1% level, respectively.

Table A.5: Correlation Between Exposure to Local Violence and Maternal Characteristics

Dependent variables:	(1) Age at conception	(2) Married (1/0)	(3) Mover (1/0)	(4) High school & above (1/0)
Long-term (192 to 12 months before conception)	-0.0009	-0.0000	0.0003	-0.0010***
	(0.0024)	(0.0002)	(0.0003)	(0.0003)
During pregnancy (1-9 months after conception)	0.0010	-0.0004	0.0010*	-0.0011**
	(0.0040)	(0.0005)	(0.0005)	(0.0005)
Post-birth (10-21 months after conception)	0.0022	0.0000	0.0004	0.0000
	(0.0036)	(0.0002)	(0.0004)	(0.0003)
Observations	4456957	4391428	4459022	4414982
Municipality of mother's birth FE	Yes	Yes	Yes	Yes
Year-month of conception FE	Yes	Yes	Yes	Yes
Municipality-month of conception FE	Yes	Yes	Yes	Yes

Notes: All regressions include mother's municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors in parentheses are clustered at the municipality level. *, **, *** significant at the 10%, 5%, and 1% level, respectively.

Table A.6: Correlation Between Exposure to Local Violence and Maternal Characteristics: 30-month Windows of Violence Exposure

	(1)	(2)	(3)	(4)
Dependent variables:	Age at conception	Married (1/0)	Mover (1/0)	High school & above (1/0)
Long-term (191 to 162 months before conception)	-0.0043	-0.0001	-0.0010	-0.0001
	(0.0056)	(0.0004)	(0.0006)	(0.0006)
Long-term (161 to 132 months before conception)	0.0033	-0.0003	0.0007	0.0002
	(0.0091)	(0.0006)	(0.0007)	(0.0006)
Long-term (131 to 102 months before conception)	0.0026	-0.0002	0.0001	-0.0005
	(0.0069)	(0.0005)	(0.0006)	(0.0006)
Long-term (101 to 72 months before conception)	-0.0047	0.0004	-0.0000	-0.0020***
	(0.0042)	(0.0004)	(0.0004)	(0.0005)
Long-term (71 to 42 months before conception)	0.0007	0.0001	0.0004	-0.0016***
	(0.0035)	(0.0004)	(0.0003)	(0.0004)
Long-term (41 to 12 months before conception)	-0.0006	-0.0002	0.0014***	-0.0010***
	(0.0030)	(0.0002)	(0.0004)	(0.0003)
During pregnancy (1-9 months after conception)	0.0007	-0.0003	0.0010*	-0.0015***
	(0.0038)	(0.0004)	(0.0005)	(0.0005)
Post-birth (10-21 months after conception)	0.0020	0.0001	0.0006	-0.0003
	(0.0033)	(0.0002)	(0.0004)	(0.0004)
Observations	4456957	4391428	4459022	4414982
Municipality of mother's birth FE	Yes	Yes	Yes	Yes
Year-month of conception FE	Yes	Yes	Yes	Yes
Municipality-month of conception FE	Yes	Yes	Yes	Yes

Notes: All regressions include mother's municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors in parentheses are clustered at the municipality level. *, **, *** significant at the 10%, 5%, and 1% level, respectively.

Table A.7: Effects of Long-Term Exposure to Local Violence on Birth Outcomes: Sum of Monthly Homicide Rates

Dependent variables:	(1) Low birth weight (1/0)	(2) Birth weight (in grams)	(3) Preterm birth (1/0)	(4) Gestation (in weeks)	(5) Height (in centimeters)
Long-term (192 to 12 months before conception)	0.0001**	-0.2628***	0.0001	-0.0014**	-0.0046***
	(0.0000)	(0.0787)	(0.0001)	(0.0005)	(0.0010)
During pregnancy (1-9 months after conception)	-0.0001	0.0991	-0.0001	0.0014	0.0019
	(0.0001)	(0.2577)	(0.0002)	(0.0012)	(0.0023)
Post-birth (10-21 months after conception)	0.0000	-0.2583	-0.0000	-0.0007	-0.0018
	(0.0001)	(0.2217)	(0.0001)	(0.0011)	(0.0025)
Observations	4239956	4239956	4470947	4470947	4365147
Municipality of mother's birth FE	Yes	Yes	Yes	Yes	Yes
Year-month of conception FE	Yes	Yes	Yes	Yes	Yes
Municipality-month of conception FE	Yes	Yes	Yes	Yes	Yes

Notes: All regressions include mother's municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. Standard errors in parentheses are clustered at the municipality level. *, **, *** significant at the 10%, 5%, and 1% level, respectively.

Table A.8: Effects of Long-Term Exposure to Local Violence on Birth Outcomes: Robustness Checks

Dependent variables:	(1) Low birth weight (1/0)	(2) Birth weight (in grams)	(3) Preterm birth (1/0)	(4) Gestation (in weeks)	(5) Height (in centimeters)
Panel A: Without restrictions on sample					
Long-term (192 to 12 months before conception)	0.0001***	-0.6009***	0.0001*	-0.0024***	-0.0093***
	(0.0000)	(0.1293)	(0.0001)	(0.0008)	(0.0013)
During pregnancy (1-9 months after conception)	-0.0001	0.0909	-0.0002	0.0022	0.0006
	(0.0001)	(0.2573)	(0.0002)	(0.0017)	(0.0033)
Post-birth (10-21 months after conception)	-0.0001	-0.2195	-0.0001	0.0008	-0.0010
	(0.0001)	(0.2278)	(0.0001)	(0.0013)	(0.0025)
Observations	4419549	4419549	4663786	4663786	4552215
Municipality of mother's birth FE	Yes	Yes	Yes	Yes	Yes
Year-month of conception FE	Yes	Yes	Yes	Yes	Yes
Municipality-month of conception FE	Yes	Yes	Yes	Yes	Yes
Panel B: Alternative definition of post-birth exposure					
Long-term (192 to 12 months before conception)	0.0001*	-0.4981***	0.0000	-0.0016	-0.0099***
	(0.0001)	(0.1798)	(0.0001)	(0.0010)	(0.0018)
During pregnancy (1-9 months after conception)	-0.0002	0.1261	-0.0004	0.0032	-0.0021
	(0.0002)	(0.3502)	(0.0003)	(0.0024)	(0.0056)
Post-birth (10+ months after conception)	-0.0000	0.1181	-0.0002	0.0014	-0.0013
	(0.0001)	(0.2076)	(0.0001)	(0.0012)	(0.0035)
Observations	4239956	4239956	4470947	4470947	4365147
Municipality of mother's birth FE	Yes	Yes	Yes	Yes	Yes
Year-month of conception FE	Yes	Yes	Yes	Yes	Yes
Municipality-month of conception FE	Yes	Yes	Yes	Yes	Yes
Panel C: Inclusion of mother's characteristics					
Long-term (192 to 12 months before conception)	0.0001***	-0.6070***	0.0001	-0.0024***	-0.0093***
	(0.0000)	(0.1347)	(0.0001)	(0.0008)	(0.0012)
During pregnancy (1-9 months after conception)	-0.0001	0.1521	-0.0001	0.0015	0.0003
	(0.0001)	(0.2591)	(0.0002)	(0.0014)	(0.0029)
Post-birth (10-21 months after conception)	-0.0001	-0.1595	-0.0001	0.0006	-0.0009
	(0.0001)	(0.2473)	(0.0001)	(0.0011)	(0.0022)
Observations	4099997	4099997	4322409	4322409	4221010
Municipality of mother's birth FE	Yes	Yes	Yes	Yes	Yes
Year-month of conception FE	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes
Municipality-month of conception FE	Yes	Yes	Yes	Yes	Yes

Notes: All regressions include mother's municipality of birth fixed effects, year-month of conception fixed effects, and mothers' municipality of birth-by-month of conception fixed effects. In Panel C, we additionally include infant biological sex and maternal characteristics, such as the age of the mother at the time of conception, whether the mother is married at the time of giving birth, and whether the mother has completed high school education or above. Standard errors in parentheses are clustered at the municipality level. *, **, *** significant at the 10%, 5%, and 1% level, respectively.