The Mexican Drug War and Early-Life Health: The Impact of Violent Crime on Birth Outcomes[1]

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ABSTRACT

This research examines the toll the current drug war in Mexico is having on the early-life health of the next generation. Specifically, this analysis, through the study of a sudden and violent event and use of rich longitudinal data that allows sibling comparisons, measures the birth outcome impact of exposure to increased local conflict, while examining and controlling for behavioral responses (migration and family planning) to that violent environment. The estimates, across multiple samples and specifications, consistently indicate that exposure, early in gestation, to the average increase in local violent crime between the pre-escalation of violence period and 2009 in Mexico leads to substantial decreases in birth weight (75 grams and a ~40% increased risk of being <2,500 grams) that are exacerbated for mothers of low socioeconomic status (~120-125 grams).

1. Introduction

Since 2008, rates of crime and violence in Mexico have risen at a dramatic and unprecedented pace. According to official data reported by the National Institute of Statistics and Geography (INEGI), homicide figures in Mexico had been stable and declining from the mid-1990's until 2007, but between 2007 and 2010 the number of reported murders almost tripled (from 8,845 in 2007 to 25,000 in 2010, Figure 1 displays yearly homicide totals and Figure 2 provides monthly homicide rates from 2000 - 2011). While the specific causes of this substantial change in the criminal environment are still being debated, what is undeniably clear is that Mexico is suffering one of the most sudden and deadly internal conflicts in recent history.

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Given the magnitude of this outbreak of violence many scholars have already begun to examine the short and long-term effects this ongoing tragedy will have on the citizens of Mexico (Dell, 2011; Brown and Velasquez, 2013; Robles et al., 2013, Velasquez, 2013, among others). The purpose of the current study is to add to this growing set of research by rigorously investigating the impact of the rise in violence on a group of individuals that is particularly vulnerable and, in terms of Mexico's future, very important: infants. Specifically, the goal of this research is to estimate the extent to which a pregnant mother's exposure to violence can restrict human capital accumulation at its earliest stage, in-utero, by examining the impact of the escalation of the Mexican drug war on the birth outcomes of its citizens.

This research question is motivated by several potential mechanisms that may connect maternal experience of crime and conflict to the early-life health of the in-utero child. For one, the magnitude and conspicuous nature of the drug war violence in Mexico has made psychological exposure essentially unavoidable in highly affected areas and maternal mental stress has been associated with intrauterine growth and gestational length restrictions of the exposed in-utero child (Beydoun and Saftlas, 2008; Camacho, 2008; Brown, 2013). Additionally, researchers have found that individuals experiencing increasing levels of violence are suffering poorer economic outcomes (Dell, 2011; Robles et al., 2013; Velasquez, 2013). A loss in family resources driven by local conflict has the potential to hamper the development of the fetus through decreased consumption of nutritious foods and vitamins and/or restricted use of prenatal care.

Discovering the validity and magnitude of the relationship between violence from the Mexican drug war and birth outcomes is of particular interest as economists continue to identify a strong and persistent association between birth weight and later life outcomes such as IQ, height, educational attainment, and wages (Behrman and Rosenzweig, 2004; Black et al., 2007; Figlio et al., 2013). Additionally, while much of the early work examined this relationship in developed countries, subsequent research has provided evidence of this link in more diverse settings (Bharadwaj et al., 2010 and Torche and Echevarria, 2011 in Chile and Rosenzweig and Zhang, 2012 in China).

Generating a causal link between the fetal health consequences of exposure to local violence, though, presents numerous challenges. For example, differential regional conflict levels may be correlated with pre-existing differences or trends in various local factors that are also correlated with the robustness of the population of mothers. Furthermore, behavioral responses to rising crime, such as migration and family planning, may cause a child's exposure level to be correlated with observed and unobserved characteristics of the mother, which in turn are also related to fetal health. This study relies on the timing, level of detail, and persistent tracking efforts of the Mexican Family Life Survey (MxFLS) to explicitly examine and address these concerns in a more rigorous way than any previous study of this topic.

The MxFLS is a nationally representative longitudinal study that has been conducted in Mexico since 2002 and spans both the pre-escalation of violence and escalation periods while maintaining very high levels of survey retention. Along with detailed individual- and household-level economic, health, and migration data, the MxFLS also contains a particularly useful feature for this study: reproductive histories. This module provides information on the birth weight, prenatal care usage, pregnancy complications, and location of delivery for all births since the last interview. This rich survey is then paired with the month and municipality-level homicide data collected by INEGI to generate estimates of the impact of increased local violence in key gestational periods on the birth outcomes of exposed children.

Using data, which allows comparison of pre-conflict and conflict period cohorts, inclusion of sibling fixed effects, and removal of endogenous migration through an intent-to-treat approach, this study finds that children exposed to local violence in their first trimester of gestation had a sizable and significant decrease in their birth weight (75 grams) and a substantial increase in the probability of being designated low birth weight (3-5% out of a base of 7-10%).[2] Moreover, children of lower socioeconomic status families suffered even larger adverse outcomes, with the magnitude of the first trimester exposure effect on birth weight doubling in size.

To put these results in context, the magnitude of the birth weight effect is considerably larger than estimates of the positive impact on birth weight of federal nutrition programs such as the Supplemental Nutrition Program for Women, Infants, and Children (WIC) and the Food Stamp Program (FSP) in the United States and are about one-third to two-thirds the size of the adverse consequences of maternal smoking.[3] Furthermore, amongst lower socioeconomic status families the adverse effect of exposure in early gestation to the escalated violence in Mexico is equal to the positive impact of the large-scale conditional cash program *Oportunidades (PROGRESA)* on birth outcomes.[4] Overall, the consistency and scale of these findings suggest that the deleterious effect of the Mexican drug war on the population of Mexico may reverberate for an entire generation.

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[2] These magnitudes were calculated based on the average increase in the 3-month homicide rate between 2009 and the pre-escalation of violence period of 2005-2007 (.25 additional homicides per 10,000 inhabitants).

[3] Hoynes et al. estimate a 2 gram effect of WIC on the average population and a 18-29 gram impact amongst participants (2011). With regard to FSP, Almond et al. suggest the program led to birth weight increases of 2-5 grams in general and 15-40 grams amongst the treated (2011). There have been many types of studies of maternal smoking's effect on birth weight with a consensus forming around a magnitude of about 200-230 grams and 100-130 grams for heavy (11+ cigarettes a day) and light smokers, respectively (Rosenzweig and Schultz, 1983; Sexton and Hebel, 1984; Brooke et al., 1989; Wilcox, 1993; Almond et al., 2005; Ward et al., 2007).

[4] Barber and Gertler estimate a 127 gram increase in the birth weight of children born to mothers participating in *Oportunidades* (2008).

2. Motivation

2.1 Organized Crime's Leading Role in Violence in Mexico

The sudden change in the magnitude and subtleties of conflict related to the drug trade in Mexico, as well as the increasing spillover of violence onto civilian non-actors, has put an international spotlight on the Mexican "war on drugs".

This increased interest has led to an in-depth study of and vigorous debate about its causes (Guerrero, 2011; Rios and Shirk, 2012; Robles et al., 2013, provide excellent holistic descriptions of the conflict's history and actors). The general consensus though, with varying opinions to the magnitude of each factor, is that the spike in homicides is a byproduct of three interrelated events. One aspect is the increased success of the USA-Colombia fight to reduce the flow of drugs between the two countries, giving Mexican drug cartels extra incentive to control the increasingly profitable drug trade (Castillo et al., 2012).

A second major influence was former president, Felipe Calderón's, strategy of increased federal military opposition to OCGs (Molzahn et al., 2012; Guerrero, 2011). The military approach taken by Calderón was to unilaterally challenge all OCGs, regardless of the size or location of the territory they controlled. As would be expected, this tactic has resulted in increased and geographically dispersed conflict throughout Mexico (Guerrero, 2011).

Lastly, the changes in military policy during the past few years have fostered an unexpected and unintended alteration in the overall picture of crime in Mexico. When the military succeeded in capturing or killing a high-ranking cartel member this would regularly result in intense fighting within the group to fill the power vacuum and eventually the fracturing of the original OCG into several new crime organizations. Guerrero finds that between 2006 and 2010 the number of OCGs grew by a factor of more than 2.5 (2011). The increased number of crime groups operating in a limited space and competing over finite profits has amplified violence between these groups. Moreover, this fighting has changed the conflict environment for non-combatants as the increased use of intimidation and scare tactics through conspicuous violence and criminal activities targeting innocents has not been merely a negative externality of the OCGs actions, but also a targeted agenda goal, as the OCGs seek to reduce the willingness for citizens to mobilize or cooperate with the police or rival cartels.

Another major negative spillover from the war on drugs has been an increase in non-drug related crimes that target non-combatants. As profits from drug running are reduced in size, due to military interference and the need to split the proceeds between more groups, OCGs have increasingly turned to crimes perpetrated on law-abiding citizens such as extortions, kidnappings, and car thefts (Molzahn et al., 2012; Guerrero, 2011).

In summary, the pregnant women in Mexico that form the population of interest for this study faced an environment in the late 2000's that was in stark contrast to the world they had lived in just a few years before. In particular there was a dramatic and larger rise in the potential for physical, mental, and financial harm. This study is interested in analyzing how a rise in this type of potential victimization can hinder the early life health of the next generation.

2.2 Conflict and In-Utero Human Capital Development: Pathways

While generating a clear causal link has been difficult, a growing literature has been building a consensus that health as early as birth can have significant consequences for later life economic, educational, and health outcomes (Strauss and Thomas, 2007, as well as, Almond and Currie, 2011 provide overviews of the current literature). Moreover, a set of studies has linked a specific birth outcome, birth weight, to the longrun accumulation of human capital such as height, IQ, earnings, and education (Behrman and Rosenzweig, 2004; Black et al., 2007; Figlio et al., 2013 among others). This link between birth weight and long run economic outcomes suggests that if the Mexican drug war is hindering fetal health, its impact on Mexican society will linger into the next generation. Unfortunately, there are compelling reasons to believe that the violence in Mexico has the potential to harm in-utero health and birth outcomes through several pathways of vulnerability including maternal anxiety, restriction of resources, and reduced access or willingness to utilize prenatal health services.

A growing literature has emerged that rigorously examines the impact of maternal anxiety on the birth outcome of the in-utero child. Using theoretical models, animal experiments, and small sample human studies the medical literature has biologically mechanized and repeatedly correlated maternal stress with, among other birth outcomes, restricted intrauterine growth and shortened gestational length (de Catanzaro and Macniven, 1992; Wadhwa et al., 1993, 2001, and 2004; Mulder et al., 2002 provides a review).

Specifically, one theorized mechanism is that the body produces excess cortisol, norepinephrine, and epinephrine when confronting ``worry, anxiety, and cognitive preparation for a threat'' (McEwen 1998) and this reaction stimulates the supply of corticotropin-releasing hormone (CRH), which is strongly linked to intrauterine growth and parturition timing (Wadhwa et al., 1993, 2004; Mancuso et al., 2004 and others). An additional channel suggested by Mulder et al. is the stress induced arousal of the sympathetic nervous system, which can cause restricted blood flow to the fetus and result in decreased intrauterine growth (2002). Some research has also indicated that the timing of the stress exposure is paramount in determining its impact on fetal health.

Several studies have posited that as a pregnancy progresses the fetus is less and less at risk to fluctuations in maternal CRH levels because the mother is less reactive and has dampened sensitivity to stressful events (Schulte et al., 1990; de Weerth and Buitelaar, 2005). Furthermore, CHR and cortisol levels naturally increase throughout pregnancy, which may in turn work to insulate the fetus from later term maternal anxiety shocks.

Establishing a causal link with regards to the overall effect of maternal stress as well as the importance of the exposure timing has proven quite challenging. Recently a few studies, relying on natural experiment techniques, have been able to more credibly identify the impact of acute stress from events such as landmine explosions (Camacho, 2008), terrorist attacks (Brown, 2013), and earthquakes (Torche, 2011). These papers have provided a consistent picture: maternal anxiety exposure in early pregnancy leads to significantly poorer birth outcomes.

This literature as a whole suggests that the anxiety from the fear and victimization that has been all too present in Mexico in the last few years has a direct biological pathway in which it can damage the early life health of the exposed children. Moreover, there are additional mechanisms that are not explicitly related to the impact of increased cortisol on the regulation of the fetus that may link increased fear and anxiety to poor birth outcomes.

Terror and depression induced by exceedingly high levels of realized or potential victimization may cause pregnant mothers to alter their behavior in several harmful ways. The experience of increased stress and loss of control may lead to the escalation of risky behaviors such as smoking and drinking. Additionally, fear may cause women to be less likely to access prenatal health care, which has been associated with the quality of the birth outcome in both developed and developing countries (Rosenzweig and Schultz, 1983, United States; Jewell and Triunfo, 2006, Uruguay; Jewell, 2007, Bolivia, Brazil, Colombia, and Peru; Wehby et al., 2009, Argentina). In addition to mental hardship, these mothers may also be experiencing direct insults to nutrition through a tightened resource constraint.

Recently a few papers have examined the impact of the Mexican outbreak of violence on the income and earnings of the Mexican population (Dell, 2011; Velasquez, 2013; Robles et al., 2013). Each study, using a different identification strategy, has found that the conflict has had a negative impact on the labor market participation and earnings of Mexican workers. If this reduction in financial resources leads to a change in consumption patterns, the health of the in-utero child may suffer. For example, Almond and Mazumder suggest that children exposed during the gestational period to a mother who is restricting their food intake due to observance of Ramadan are born significantly smaller (2011). Furthermore, if the restricted income potential impacts a family's ability to afford prenatal care, this would further put the fetus's health at risk.

2.3 Conflict and Human Capital Development In-Utero: Prior Evidence

What is clear is that there are many reasons to think that violence and conflict may have significant consequences for the birth outcomes of exposed children, yet very few studies have rigorously attempted to identify the magnitude of the effect. While studies of the impact of maternal anxiety have had some success by using plausibly exogenous shocks of psychological stress for identification, these papers do not provide an apt substitute for the study of the impact of crime and victimization on in-utero health as they typically rely on infrequent or short-term events and identify only one of the channels (maternal anxiety) by which conflict may hinder proper fetal development.

Most of the literature that specifically analyzes the impact of conflict on birth outcomes has relied on persistent variation between localities in rates of violence, which may be correlated with other unobserved or omitted factors that differ between the regions and are correlated with early life health. Furthermore, these studies, by using locations with fairly well established and constant levels of violence over time, are subject to conflating systematically taken behavioral responses, such as residential sorting and family planning, with the actual impact of violence on birth outcomes. Recently, one study combined an unexpected outbreak of violence with the identification strength of within family comparisons to produce results, which remove many of the concerns presented in the previous literature.

In 2012, Hani Mansour and Daniel Rees published their work examining conflict and birth weight in Gaza and the West Bank during the al-Aqsa Intifada. The al-Aqsa Intifada started with the contentious visit by Ariel Sharon to the Temple Mount in September 2000 that sparked 4 months of violence, followed by a reduction in conflict for 8 months, at which point clashes intensified again until the summer of 2002, with some level of residual violence lasting into 2005. Mansour and Rees evaluate the impact of fatalities caused by Israeli troops, at the month and district level, on the birth outcomes of children born between April 2001 and June 2004. They find that an increase in fatalities in the district of birth 9-6 months before birth lead to a statistically significant increase in the probability that the child will be born low birth weight (LBW, <2,500 grams). The strength of their analysis lies in the fact that they can exploit temporal and geographic variation in conflict intensity and, as some mothers had two children between April 2001 and June 2004, they can control for time-invariant characteristics of the mother using sibling fixed effects. This paper and its identification strategy is a major contribution to the current literature and remains the seminal work in the area of conflict and birth outcomes.

One issue this analysis faces, though, is that all of the data was collected and almost all of the children under study were conceived *after* the conflict was initiated. Thus, the study loses the baseline, or non-conflict, level of maternal characteristics and fertility rates in each district. The authors must rely on the assumption that family planning decisions and behaviors were the same within a district during the lulls in conflict (which is their control cohort) as they were before conflict was ever initiated. It is highly plausible that the fertility demographics and behaviors during truly non-conflict times are quite different than during the temporary and unsustainable moments of low conflict used to generate variation in the study.

In addition to family planning behavioral responses, migration patterns cannot be assessed using this data. While the authors suggest migration was relatively low in general during this period, by citing that 94% of the births were to women living continuously in the same municipality since September 2000, this does not account for all the women (and thus their children) who out-migrated from the survey area due to this intense conflict. Moreover, if the 6% that reported having moved did so systematically in a way correlated with potential fetal health (for example, mothers with more means moved to safer areas), this could lead to biased point estimates of the impact of violence on birth outcomes.

The authors are able to help assuage some of these concerns by focusing on those families that had two children born during the 3-year sample window. By comparing siblings, any potential biases between families arising from time-invariant characteristics are no longer relevant, as all comparisons are within the family. This strategy greatly increases the identification strength of the study, but raises some additional concerns when using this data in this setting.

Since the data window is so short, the sample remaining when only using mothers that gave birth twice in a three-year period has the potential to not reflect the general population. Additionally, since the conception decision for almost all of these births was made after the conflict had begun, and thus the potential for violence exposure was largely anticipated, the selectivity of the sample of mothers choosing to have more than 1 child during this time is plausibly exaggerated. Finally, while sibling fixed effects is an effective strategy to control for time-invariant characteristics of families/mothers, when used in a setting in which the conflict is not an unexpected shock, the potential for time-varying heterogeneity within a family driving the timing and health of a birth is exacerbated. With these limitations in mind, this work is still the strongest analysis of this topic in the literature, and a jumping off point for this study.

A second paper that should be mentioned is a working paper by Florencia Torche and Andres Villarreal that, like this paper, is interested in the question of how the surge in crime in Mexico has affected the birth outcomes of the exposed (2012). To examine this issue, they utilize INEGI monthly homicide data at the municipality level, pairing it with the birth certificate data of all children born between January 2008 and December 2010. The results from Torche and Villarreal's analysis are quite surprising and strikingly different than what has previously been found in the literature.

Their estimates suggest that children in Mexico exposed to additional homicides during the first few months of gestation show *increased* birth weight and *lower* probability of being born low birth weight. They suggest that it is increased prenatal care behavior, spurred on by early gestation exposure to conflict, specifically amongst the urban, low socioeconomic status mothers, which is driving these counter-intuitive findings. The strength of this work is that it utilizes official birth certificate data and the large number of observations allows for robust subsample analysis. While the richness of this study's data is quite useful, the time period under study and the lack of longitudinal data with sibling linkages makes it very hard to interpret the counter-intuitive findings as causal.

The researchers, due to lack of birth weight information in the birth certificate data before 2008, are limited to studying a sample in which the majority of the births were conceived after the violent surge had already begun. As mentioned in relation to Mansour and Rees's work, this is potentially very problematic when considering the systematic behavioral responses families may make during a time of conflict. Selective migration is one potential reaction to increased violence (or threat of violence). When the authors take a look at the impact of homicides on out-migration of childbearing aged women using 2005 and 2010 population counts, they find that women with exactly 9 years of education are more likely to out-migrate during times of greater violence. Additionally, this sensitivity analysis is only able to account for migration between 2005 and 2010, while temporary migration during pregnancy and delivery, which may be more reactive to local violence, cannot be captured.

A second possible response to violence is changing fertility behavior. Unfortunately, the authors are unable to rigorously analyze whether there is a fertility response to violence because the data is all post-conflict initiation and thus a true baseline level is not available for comparison. That said, using data from 2008 to 2010 they find a positive relationship between municipality level homicide rates and birth rates. Thus, it seems that theoretically as well as in practice, using data that does not allow for control of systematic migration or between family unobserved heterogeneity to analyze the impact of violence on birth outcomes in Mexico, particularly when data is only available for the post conflict initiation period, leads to a selected sample and potentially misleading results.

3. Data

Properly estimating the impact of violence on individual health outcomes faces numerous empirical obstacles. In order to take on these challenges, this study will pair the INEGI monthly homicide data at the municipal level with the fortuitously timed and rich Mexican Family Life Survey (MxFLS).

The INEGI data provides information on all official reports of intentional homicides. A helpful feature of the data is that the label of "homicide" is assigned to a death using the World Health Organization's guidelines (ICD-10, Instituto Nacional de Estadística), which should shield the data from regional differences in the classification or rate of reporting of a deceased as a homicide victim. One potential concern with this data is that it contains information only on registered homicides. If rates of reporting are significantly lower for cartel related homicides this data may serve as a very poor proxy for local conflict levels. Previous research has addressed this issue by comparing the INEGI data to organized crime related homicide data collected by the President's Office and found that the INEGI data captures the same trends found in the more explicit OCG-related President's data (Velasquez 2013). The advantage to and reason for using the INEGI data throughout the rest of the analysis is its longer collection period, 1990-2011. Using data that spans both the pre-conflict and conflict periods allows the temporal variation of homicide rates in Mexico to be combined with the panel nature of the MxFLS.

The MxFLS is an ongoing longitudinal data set that is representative of the Mexican population in 2002. During the 2002 baseline survey, MxFLS1, information was collected on approximately 8,440 households and 35,600 individuals among 150 communities and 16 states throughout Mexico. The second wave, MxFLS2, was conducted in 2005-2006 and the third wave, MxFLS3, was started in 2009 and is currently in the final stages of fieldwork. The MxFLS was designed to follow all individuals in baseline households (and children born to these individuals since baseline) and has had great success in keeping low levels of attrition; with over 89% of the panel respondents being re-interviewed in MxFLS2 and similar re-contact rates are anticipated for MxFLS3.

More than just pointing to the low attrition rate in this survey though, it is of first order importance to test whether the sample of interest (childbearing aged women) are more likely to attrite from the sample due to a rise in violence in their municipality of residence. In order to formally test whether attrition amongst our population of interest is being driven by potential violence exposure, an analysis is conducted on women aged 7-42 in MxFLS1 (thus approximately14-49 years old and eligible for the birth history component by MxFLS3) that estimates the relationship between the change in the homicide rate from 2002 to 2009 in the respondent's MxFLS1 municipality of residence and their likelihood of attriting from the MxFLS. Similarly, an analysis using women aged 10-45 in MxFLS2 examines if the change in violence between 2005 and 2009 in the respondent's MxFLS2 municipality predicted their future attrition. In addition to simply seeing if attrition is related to pending increases in local violence in general, specifications are also conducted in which the homicide rate variable is interacted with MxFLS1 or MxFLS2 characteristics of the respondent to determine if attrition amongst subgroups of the population is predicted by increased conflict.

Examination of this issue reveals that attrition was quite low amongst this group of respondents: 13.4 percent between baseline and the third wave and less than 9% between the second and third wave (Table 1). Furthermore, no statistically significant relationship is detected in each analysis; suggesting that potential exposure to conflict was not a determining factor of attrition from the MxFLS3 sample by childbearing age women.

One particularly valuable aspect of the MxFLS, for the purposes of this study, is the fact that the timing of the survey waves provides a useful snap shot of Mexico before and during the major rise in conflict. The first follow-up was conducted between 2005 and 2006, a period of low levels of violence, and the second follow-up was performed from 2009 to 2013, during times of extremely elevated violence.

Maps 1-4 show the municipality homicide rate per 10,000 inhabitants for 2002, 2005, 2007, and 2009. Maps 1-3 provide a view of the conflict landscape during MxFLS1, MxFLS2, and an intermediate year between MxFLS2 and MxFLS3. The picture painted by these three maps is one of heterogeneous rates of violence, with homicides mainly concentrated across the United States border, a main drug trade route running along the Sinaloa-Durango and Sonora-Chihuahua borders up to the U.S.-Mexico border, and in the southern coasts of Michoacán and Guerrero. By 2009 though, as seen in Map 4, the image of violence in Mexico was much different. Map 4 shows that the conflict had intensified and spread across Mexico, and areas like the interior of Durango and southern Sonora, which previously were off the main drug trade routes and thus shielded from most of the violence, were now at the center of the drug war.

While the magnitude of the conflict has risen significantly in the last few years across Mexico, the level of the change across municipalities varies a great deal. For example, between 2005 and

2009 the range of growth rates in homicides between municipalities spanned from a dreadful 30-fold increase in one area to an 80% *decrease* in another. Thus, along with the temporal variation in violence, this analysis will also be able to exploit the geographic distribution of conflict exposure across municipalities. Given that we have a great deal of variation in conflict intensity growth between municipalities, an open question is whether this violence heterogeneity actually reflects underlying trends in other municipality characteristics. If this were the case, it would raise concerns that an analysis of the impact of violence on birth outcomes would actually be picking up the effect of some other municipality trend on in-utero health. While it seems unlikely, due to the suddenness and well-documented origins of the change in the conflict environment, that some other municipality characteristic trend would be generating the temporal and geographic heterogeneity in violence seen in Mexico, we will formally explore this concern.

To examine this question a rich set of pre-escalation of violence trends of the 135 baseline MxFLS municipalities are used to predict each municipality's 2009 homicide rate, as well as the change in homicide rate in each municipality between 2005 and 2009. Trends were created using the IPUMS samples of the 2000 and 2005 Mexican censuses and the MxFLS1 and MxFLS2 survey waves.[5] Table 2 displays the results of these analyses. In both specifications, the estimates

[5] From the Census the municipality trend between 2000 and 2005 are included for, the share of households with basic utilities (piped water, sewage system, electricity, television), the share of working age adults (21-65 years old) with particular levels of education ("less than primary" and "at least high school grad"), the ability to speak the indigenous language, and literacy, as well as, the overall share of different age groups (<18 and 18 to 65) and average educational attainment. From the MxFLS the municipality trend between MxFLS1 and MxFLS2 are included for, the share of adults (older than 18) that are married, are employed (by gender), are self-employed (by gender), live in a rural area, have a relative in the U.S., have thoughts of future migration, have fear during the day, have fear in the night, as well as, the average household size, hourly earnings (by gender), and per capita household expenditure. Also from the MxFLS municipality trends between MxFLS1 and MxFLS2 are included for the average report of increased domestic violence, the presence of vandalism, the level of police presence, the number of schools at various education levels (primary, junior high, and high school), and the rate of "poor" households.

strongly suggest that pre-conflict trends in municipality characteristics were unrelated to future homicide rates.[6]

An additional advantage of the MxFLS is the comprehensive set of variables it collects about its participants, including information about the individual's economic, social, and health outcomes and behavioral histories (migration, fertility, marriage). Furthermore, information of household expenditure and asset ownership is gathered as well. Moreover, by using the panel nature of the data, the very serious potential biases from selective and endogenous fertility and migratory patterns can be examined.

Most importantly for this project, though, are the detailed reproductive histories collected in the MxFLS. For example, in MxFLS3, all household member women between 14 and 49 are asked to provide information such as date of birth, birth weight, prenatal care behavior, and place and locality of delivery on all pregnancies that have occurred since the MxFLS2 interview.

Focusing on panel member women in order to maintain representativeness provides a sample of 1,850 live births since MxFLS2 to 1,608 women (Table 3, Column 1).[7] As mentioned in the previous section, properly identifying the impact of conflict on birth outcomes will be greatly improved by the use of sibling comparisons, for which there are 471 sibling births to 229 mothers (Table 3, Column 2).

Two potential concerns exist, though, when considering the use of these samples as the main populations of examination. First, since some of the interviews in MxFLS3 took place in late

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[6] While there is 1 coefficient in Column 1 and 3 coefficients in Column 2 significant at the 10% level, given the number of dependent variables (64), this is even fewer significant estimates than what would be expected by chance. Moreover, joint tests for each regression are non-significant.

[7] There are 2,087 live births since MxFLS2. To create the analysis sample, 128, were removed due to lack of birth date information. Additionally, 84 births were dropped due to missing birth weight data. Lastly, 20 twins and 5 duplicates were not included in the sample.

2009 and beyond, some of these pregnancies were conceived after the surge in violence could no longer be thought of as a shock or unanticipated. With the conflict intensity no longer being exogenous to family planning behavior, the children conceived during this time might be part of families that are significantly different in observed and unobserved ways that are also correlated with fetal health. While the use of sibling comparisons will help alleviate this issue to some extent, time-varying family characteristics that may lead a mother to have an additional child during a predictably high homicide period still present an issue for identification.

To combat this problem, analysis can be conducted on a sample that only includes births before July 1, 2009 (labeled as born before 3Q2009, Table 3, Columns 3 and 4). Using this sample, all of the births will have been conceived, approximately, at latest in the third quarter of 2008, eliminating around one-sixth of the births from the full sample and one-third of the births from the sibling sample. The reasoning for the timing of this sample selection is that, at this point, homicide rates had been elevated for only a few months, which generate variation in a key period of gestation, first trimester, but the violence was still relatively new and less predictable. In order to make sure issues of selective fertility based on anticipated violence does not continue to drive the results even after making this sampling choice, analyses limiting the observations further, to those born before the second quarter of 2009 or those born before 2009, are also conducted.

The second issue that exists for both of the sibling samples mentioned previously is that they rely on mothers that gave birth to multiple children in a 3-4 year period. This is problematic for two reasons. Firstly, this restriction leads to a fairly small number of observations, which serves to reduce the power of the analysis, constrain the number of additional controls that can be used, and limit the number of stratifications that can be run in order to parse out any heterogeneity in the estimated effect. Secondly, by only being able to include mothers that gave birth to multiple children in a relatively short time span, the sibling sample mothers end up having significantly different characteristics than those in the full sample. For example, when comparing baseline characteristics of the mothers with only one child in column 3 with those with multiple births in column 4 it is apparent the sibling sample is significantly different in a few ways. Single-birth mothers are statistically significantly older and more likely to be married in 2002, and though not statistically significantly so, they are also earning more per month and living in households with higher per capita expenditure.[8] As such, the sibling sample may be drawn from lower on the socioeconomic status continuum.

To make the sibling and full sample more comparable, any birth since baseline (2002) to a mother in the MxFLS3 birth history is included, while continuing to exclude births after the second quarter of 2009 (Table 3, Columns 5 and 6). This sampling choice adds 320 births to the full sample and triples the size of the sibling sample. Importantly, now the only observed characteristic that significantly differs between mothers in the full sample and mothers in the sibling sample is that mothers with multiple births have significantly more education at baseline (though this difference is eliminated once age at baseline is controlled). This final sample (Table 3, Columns 5 and 6) will be treated as the preferred population because, along with being more representative, the increased size allows the use of additional controls and for the heterogeneity of the impact of violence to be explored through stratification.[9]

[8] Conducting the same comparison of single birth mothers in Column 1 to multiple birth mothers in Column 2, finds that the single birth mothers are significantly older, more likely to be married, earn more, and live in higher per capita expenditure households at baseline.

[9] Analysis using the sample focused solely on the MxFLS3 birth history births before the third quarter of 2009 (Table 3, Column 3 and 4) provides qualitatively similar and quantitatively larger estimates of the effect found when using the preferred sample. Estimates of the analysis using this alternative sample are found in Tables A2 and A3.

Using the nationally representative MxFLS, combined with INEGI's monthly homicide data at the municipality level, this study will be able to take advantage of large and unanticipated variations in violence exposure across regions and time and pair them with a sibling fixed effect identification strategy.

4. Empirical Strategy and Results

4.1 Behavioral Responses: Migration and Fertility

A violent conflict of the scale currently faced by Mexican citizens will almost surely result in systematic behaviors being taken by a selected group of the exposed in order to alleviate the potential negative spillovers to their well-being. Recognizing, analyzing, and accounting for these responses is imperative to any study of the Mexican drug war's impact on individual outcomes. Specifically, in the case of studying the effect of in-utero exposure to violence on fetal health, two behavioral responses must be addressed: migration and family planning/fertility.

Systematic migration as a result of a realized or impending surge in crime has the potential to change the composition of individuals exposed to violence and lead to biased results. For example, if mothers with a larger preference for safety are more likely to move away from high crime areas, and this safety preference also leads these mothers to take additional pro-health behaviors, the high crime areas would disproportionately be left with less healthy mothers and thus lower quality births without violence exposure being the cause. As such, it is important to determine whether migration decisions are being driven by potential exposure to violence.

In order to examine this issue, three measures of migration behavior will be analyzed. The first measure is simply an indicator for whether the interview municipality in MxFLS3 is different than the interview municipality in MxFLS2, which is the case for approximately 7% of the mothers (Table 4, Columns 1 and 2). The second identifier of migration is an indicator of whether the respondent has answered that they have moved from their MxFLS2 locality for longer than one year at any time between the MxFLS2 interview and the MxFLS3 interview, which represents approximately 16% of the women (Table 4, Columns 3 and 4). Finally, the last and most sensitive measure of migration is an indicator of whether the respondent answered that they have moved from their MxFLS2 locality for longer than one month at any time between the MxFLS2 interview and the MxFLS3 interview, which accounts for about 17% of the mothers in each sample (Table 4, Columns 5 and 6).

Table 4 presents results of regressions on our sample of mothers that test whether the change in violence between 2005 and 2009 in the mother's municipality of residence in MxFLS2 was predictive of their decision to migrate. To estimate the relationship between migration and potential exposure as carefully as possible, and to avoid spurious correlations, each of these regressions additionally controls for various individual and household characteristics in MxFLS2 (age fixed effects, education, marital status, earnings, employment, rural status, household size, and household per capita expenditure), the municipality characteristic trends presented in Table 2, MxFLS2 state of residence fixed effects, as well as, year and month of MxFLS3 interview fixed effects.

$$y_{ij} = \gamma + \delta_1 HOM_j + \beta' X_i + \pi' \rho_j + \alpha_{YOI} + \tau_{MOI} + \sigma_{STATE} + u_{ij} \qquad (1)$$

This specification is represented in equation 1, where *y* is the migration decision of individual *i*, that resides at baseline in municipality *j*, HOM_j captures the change in the homicide rate between 2005 and 2009 in municipality *j*, X_i is a vector of individual characteristics measured in MxFLS2, ρ_j is a vector of municipality characteristic trends, α_{YOI} are indicators for the year of interview in MxFLS3, τ_{MOI} are indicators for the month of interview in MxFLS3, and σ_{STATE} are indicators for the state of residence in MxFLS2.

Moreover, it is also important to examine if the migration behavior of certain subgroups of the population was sensitive to local violent conflict. If potential violent crime exposure caused particular groups of mothers to migrate, and this systematic behavior was unaccounted for, it would create bias in the estimates of the impact of violence on birth outcomes. To explore if this is a concern, equation 1 is also estimated with characteristics of the mother (age, age squared, education, marital status, earnings, employment, rural status, household size, and household per capita expenditure) measured in MxFLS2 interacted with the change in the local homicide rate.[10]

The analyses of both versions of equation 1 are presented in Table 4. Column 1 provides estimates using the measure of movement typically used when analyzing migration in birth outcome studies: whether the mother resides in the same place as the previous wave (or in some studies some specified prior date). Examination of the relationship between this measure and potential future homicide exposure suggest that future violence does not predict migration. Moreover, even adding more detail to the analysis, by exploring if within certain subgroups future local conflict predicts being interviewed in a different municipality than in the previous wave (Column 2), there appears to be no relationship between violence and migration behavior.

Using a migration measure that is more inclusive and short-term in nature (any migration away from MxFLS2 locality for at least a year) and possibly more relevant for pregnant women, provides a different interpretation of the relationship between migration and conflict exposure. Column 3 of Table 4 provides evidence that future local violence increases migration behavior

[10] Both versions of equation 1 are also run using measures of migration between baseline and MxFLS3, these results are provided in Table A1.

amongst the mothers in the sample, with a 1 in 10,000 rise in the homicide rate increasing the probability of migration by 1.5%. Moreover, Column 4 of Table 4 suggests that the influence of violence on migration was particularly strong amongst mothers from rural areas, mothers that earned more, and mothers living in more wealthy households.[11]

This analysis strongly implies that not measuring short-term migration may provide misleading conclusions about the relationship between local conflict and migratory behavior. Additionally, since migration does appear to be a behavioral response to potential violence exposure and that the response is systematically taken by subgroups within the population of mothers, failing to control for migration in an analysis of the impact of violent crime on birth outcomes would produce non-trivial bias in the estimates.[12] Given this serious threat to identification, the issue of endogenous migration will be addressed directly when developing the methodology to test the effect of local homicides on birth outcomes in the next subsection.

Turning to fertility behavior, exposure to local violence has the potential to impact birth rates in the effected area in a few first order ways. First, certain families may actively try not to conceive a child during a time of intense conflict. They may see this environment as dangerous for the health of the mother, dangerous for the health of the child, or infeasible due to a loss in resources. If these families are drawn from a specific part of the birth outcome distribution, not accounting for this composition change will bias results. An additional possibility is that local birth rates are being driven by selective migration, with families more or less likely to conceive

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[11] These results hold when using an even more liberal measure of migration, any movement for at least a month from the MxFLS2 locality, found in Columns 5 and 6.

[12] One additional note, even if the decision to migrate was not impacted by potential violence, the destinations of those movements may be changed in a systematic way. If, rather than level of migration, the location of migratory flows changed in a way correlated with both anticipated future homicide exposure as well as fetal health, this would skew results, as well.

choosing to move away from local crime. Finally, birth rates may be impacted directly by violence exposure. If the anxiety and/or resource restrictions caused by local violence are severe enough, fetal health may deteriorate to a point that a non-marginal number of pregnancies may be lost.

To examine the impact of local violence on monthly birth rates of MxFLS1 municipalities the following regression was estimated:

$$BR_{jmy} =$$
$$\gamma + \sum_{i=10}^{15} \pi_i \, (HOM, i\ months\ before\ birth)_{jmy} +$$
$$\pi_1 \, (HOM, 9-7\ months\ before\ birth(_{jmy} + \pi_2 \, (HOM, 6-4\ months\ before\ birth)_{jmy} +$$
$$\pi_3 \,)HOM, 3-1\ months\ before\ birth(_{jmy} + \alpha_y + \rho_m + \sigma_j + \tau_{STATE,y} +$$
$$u_{jmy} \qquad (2)$$

where, *BR*, is the birth rate of the relevant group in month *m*, year *y*, and municipality *j*, $\sum_{i=10}^{15} \pi_i \, (HOM, i\ months\ before\ birth)_{jmy}$, represents the municipality homicide rate for each month from 10 to 15 months before the outcome birth rate, $(HOM, 9-7\ months\ before\ birth(_{jmy}$, $)HOM, 6-4\ months\ before\ birth(_{jmy}$, $(HOM, 3-1\ months\ before\ birth)_{jmy}$, represent the municipality homicide rates over an approximation of the first, second, and third trimester of the outcome birth rate, respectively, and α_y, ρ_m, σ_j, and $\tau_{STATE,y}$ represent fixed effects at the year, month, municipality, and state times year level.

The numerator for the outcome birth rate, *BR*, was calculated as the number of births in the MxFLS reproductive history. To create the denominator for the birth rate, the number of women

14 to 49 years old (and thus eligible to complete the reproductive history) in each wave of the MxFLS were counted and considered the base January population in the year following the initiation of that wave of the MxFLS. Thus, the count for MxFLS1 was used for January 2003, the count for MxFLS2 was used for January 2006, and the count for MxFLS3 was used for January 2010. Then a linear imputation method was used to fill in the months in between waves. The same strategy was used when constructing birth rates for women with less than 9 years of education and for women with at least 9 years of education (compulsory level). The time period of this analysis runs to June 2009, as this is the latest date for which the MxFLS3 reproductive history should contain all births up to that point, as no MxFLS3 interview took place before June 2009.

Table 5 displays the findings from estimation of equation 2. The results from Column 1 suggest that local homicide rates before conception and all the way through pregnancy did not significantly change overall birth rates. Additionally, when looking at a lower education subgroup (<9 years of education), there does not appear to be a statistical relationship between violence just before or during gestation on birth rates (Table 5, Column 2). Interestingly though, for higher educated women (≥9 years of education), even when controlling for a great deal of temporal, geographic, and even geographic time trend variation through the inclusion of fixed effects, it appears there is a response in birth rates to conflict.

The estimates in Column 3 of Table 5 suggest that educated women, when faced with increasing violence during the time leading up to a potential conception date, are less likely to reproduce. Moreover, increased homicide exposure in the area of the second trimester also seems to lower birth rates. This second finding may be caused by families being able to forecast conflict intensity and deciding not to have children when violence is expected to rise during the 4 to 6 months before birth, or perhaps, within the subgroup of higher educated women, homicide exposure in the second trimester has a non-trivial impact on being able to produce a live birth.

This finding serves to underline the importance of using a sibling fixed effects identification strategy in order to estimate the impact of local homicide rates on fetal health. While the current alternative in the field is to simply control for temporal and geographic heterogeneity through time and location fixed effects, the results in Table 5 suggest this strategy would contain biased estimates. In this specific case it appears a method of time and location fixed effects would not control for a compositional change amongst the second trimester exposed group of educated women and perhaps wrongly attribute a reduction in birth outcomes to violence exposure.

4.2 Birth Outcomes: General Results

This section presents results of an evaluation of the impact of local homicide rates during gestation, constructed as 1 to 9 months before birth, on birth outcomes. This analysis is an extension and contribution to previous work on conflict and in-utero health as it utilizes a sudden increase in violence, data that spans the non-conflict and conflict periods, and a longitudinal survey, which helps to address some of the potential concerns raised by systematic behavioral responses by different types of mothers.

Issues related to each of the two behavioral responses discussed in section 4.1 inform and guide the identification strategy used to estimate the impact of local homicide rates on birth outcomes. As mentioned previously, migration decisions for mothers in the sample were significantly influenced by exposure to conflict. In order to take on this issue of systematic behavioral response directly, the identification strategy employed in this analysis takes an intent-to-treat approach. Specifically, exposure intensity during gestation for each birth will be calculated as the homicide rate during that time period in the mother's baseline (MxFLS1) municipality of residence, rather than the municipality of residence at birth. While this approach may attenuate the estimate of the impact of local violence on birth outcomes, it alleviates concerns that endogenous migration behavior is driving the results.

Also, as mentioned in section 4.1, there appears to be a reduction in birth rates amongst more educated women when they experience higher conflict intensity prior to conception and around the second trimester, even after controlling for regional and temporal fixed effects. This bias causing systematic behavior is an example of the composition issues that may exist when only temporal and geographic heterogeneity is controlled. Moreover, as with any survey, the amount of covariates available to use as controls between mothers is limited. This limitation may lead to the misidentification of heterogeneity in health related preferences and behaviors of mothers differentially exposed to local homicide rates as an effect of violence on birth outcomes.

These issues of composition change and omitted variable bias are ever-present in studies of fetal health. The strategy that will be used in this study to address these concerns is the use of sibling comparisons. By only making comparisons within a family, time invariant characteristics or preferences of the mother/household are controlled. Additionally, in an attempt to limit the potential for time-varying within-family behavioral changes related to violence exposure biasing the results, samples that are limited to births conceived before violence levels could be predictably anticipated are utilized. Finally, available time-varying characteristics (mother's education, age at birth, employment status, earnings per month, and marital status, as well as, household size, rural status and per capita expenditure) between baseline and MxFLS2 are included. Since these characteristics are not measured at the time of birth (except for age at birth) they are assigned as follows (except for age at birth): if a birth is from the MxFLS3 reproductive history they are assigned the time-varying characteristic reported in MxFLS2 and if the birth is from the MxFLS2 reproductive history they are assigned the time-varying characteristic reported in MxFLS1.

Standard temporal (month of birth, year of birth, month of interview, and year of interview) and geographic (municipality of birth) fixed effects are also included in some specifications to control for any spurious relationship between the date of interview, as well as, the time and place of the birth and the birth outcome, which is unrelated to violence exposure. Furthermore, when sample sizes are large enough, state of birth interacted with year of birth fixed effects are included to remove additional state-year trend unobserved heterogeneity from the analysis.

Finally, local homicide rates from before conception and after birth will be included as controls. The results from section 4.1 make it clear that behaviors related to family planning are being impacted by local violence several months before the potential conception month. To account for these fertility behaviors as well as other behaviors related to violence that may change the composition of maternal characteristics, even in a within-family comparison, the local homicide rates for the 6 months before conception, 15 to 10 months before birth, are included. In addition, it may be the case that local violence in the time surrounding a birth has a relationship to birth outcomes that is unrelated to its impact on the fetal health of the child. As such, the homicide rate for the 6 months after birth are additionally added to the regression, as these homicides should be related to the local homicide rate during pregnancy but unrelated to birth outcomes and serve to control for any additional spurious relationship.

The empirical strategy is illustrated in the following regression framework:

$$b_{ijtmk} = \gamma + \nu_m + \alpha_{YOB} + \partial_{MOB} + \varphi_{YOI} + \omega_{MOI} + \rho_j + \tau_{STATE,YOB} + \beta' X_{ijtmk} + \sum_{i=10}^{15} \pi_i \,(HOM, i\ months\ before\ birth)_{kt} + \pi_1 (HOM, 9-7\ months\ before\ birth)_{kt} + \pi_2 (HOM, 6-4\ months\ before\ birth)_{kt} + \pi_3 (HOM, 3-1\ months\ before\ birth)_{kt} + \sum_{i=1}^{6} \eta_i \,(HOM, i\ months\ after\ birth)_{kt} + u_{ijtm} \qquad)3)$$

Where *b* is the birth outcome of individual *i* born in municipality *j* in time *t* to mother *m*, that resided in municipality *k* at baseline, ν_m captures sibling fixed effects, α_{YOB} are indicators of the year of birth, ∂_{MOB} are indicators of the month of birth, φ_{YOI} are indicators of the year of interview, ω_{MOI} are indicators of the month of interview, ρ_j are municipality of birth fixed effects, $\tau_{STATE,YOB}$ are state of birth interacted with year of birth fixed effects, X_{ijtmk} is a vector of individual (gender, birth order fixed effects, and survey wave) and time-varying mother/household characteristics, and $(HOM, i\ months\ before\ birth)_{kt}$, $(HOM, 9-7\ months\ before\ birth)_{kt}$, $(HOM, 6-4\ months\ before\ birth)_{kt}$, $(HOM, 3-1\ months\ before\ birth)_{kt}$, and $(HOM, i\ months\ after\ birth)_{kt}$ are homicide rates in the municipality of mother's baseline residence, *k,* during specific periods before, during and after gestation of individual, *i*.

Table 6 presents the estimates from specifications that build up to equation 3. In Column 1, using a specification without sibling fixed effects, it appears that local violence in the middle of gestation, 4 to 6 months before birth, is negatively related to a loss in birth weight, while exposure during the rest of gestation is non-significant. This estimate though may be driven by the change in fertility behavior presented in Table 5. Table 5 suggested that the comparison group of mothers exposed to violence in the second trimester is composed of too few births to women with at least compulsory education, which would lead to downward biased estimates. Even after adding the full set of controls other than the sibling fixed effects (Column 2) and moving to the sibling sample (Column 3), the magnitude of the estimate on exposure 4 to 6 months before birth is essentially unaffected and exposure in all other parts of gestation continue to have no relationship to birth outcomes.

In Column 4 the sibling fixed effects are introduced and have a dramatic effect on the results. First of all, once maternal fixed effects are included, the sign of the effect from exposure 4 to 6 months before birth is reversed and the absolute magnitude is greatly diminished. Additionally, the estimates now suggest that experience of increased local violence 7 to 9 months before birth leads to statistically significantly reduced birth weight. This non-trivial change in the overall set of results points to the misleading conclusions that can be drawn when an analysis of crime on birth outcomes fails to control for unobserved heterogeneity between mothers/families. Moreover, as additional controls are added (Columns 5-7) the magnitude of the early gestation effect only grows.

To give some guidance to interpreting the results, the average homicide rate in Mexico between the pre-escalation of violence period of 2005-2007 and 2009 rose by approximately 1 homicide per 10,000 in MxFLS1 municipalities, which would produce a rise of around .25 homicides per 10,000 in the 3 month homicide rate. Thus, the results in Table 6 estimate that the loss in birth weight resulting from the average 3-month increase in violence in Mexico between the pre-conflict period and 2009 is 75 grams (300*.25).[13]

[13] Moreover, 2009 was just the beginning of the Mexican drug war surge in violence. By 2011, the annual rate had risen by around an additional .5 homicides per 10,000.

An alternative way to conceptualize the estimates is to calculate the impact on birth weight of 1 additional homicide in a representative municipality. The median 2009 population amongst MxFLS1 municipalities is around 60,000 people, thus according to estimates in Table 6, one extra homicide during early gestation in a municipality of this size would generate a 50 gram decrease in birth weight amongst the exposed.[14]

While Table 6 restricts births to those born before July 2009 in order to shield the estimates from births in which local violence was easily predictable by the families, it is important to check that this restriction has gone far enough. To test the robustness of the initial findings, Table 7 contains the results of the full specification from equation 3 using samples limited to children born before April 2009 and before January 2009, respectively. Columns 2 and 3 of Table 7 display these findings. These estimates suggest that the inclusion of children born between January and June 2009 is not driving the main results, as these more restrictive samples produce very similar results.

Additionally, since all the birth weight estimates are based on self-reports, it may be the case that the birth weight measures of children born outside a medical facility contain a great deal of error that is correlated with local violence exposure. Thus, an additional analysis was conducted on only the subsample of children born in a hospital or clinic (Table 7, Column 4). While the level of significance on the impact of early gestation exposure to violent crime is reduced, due to the increased size of the standard errors, the point estimate remains marginally statistically significant and the magnitude of the estimate is even larger (>15%) than the result in Table 6.[15]

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[14] Calculated as (-300*10,000)/60,000.

[15] As an additional way to provide evidence of whether measurement error/recall bias in the reported birth weights is correlated with violence exposure, equation 3 was also estimated using a measure of stacking as the dependent variable. Stacking was measured as having a birth weight that ended in .0 or .5 kilograms. Conducting

In summary, the analysis of the impact of increased local conflict on birth weight consistently confirms that early gestation exposure leads to significantly poorer outcomes.

To provide even more information about how local conflict is impacting birth outcomes, analysis on the probability of a birth falling into one of the commonly used categories of poor fetal health, low birth weight (<2,500g) is conducted and presented in Table 8. Column 1 provides the estimates of the probability of being less than 2,500 grams at birth and, due to stacking at 2,500 grams in the data, Column 2 presents the results when those listed as weighing exactly 2,500 grams are included as low birth weight as well. Both estimates are in concordance with the previous findings as they suggest that exposure to greater local violence in the first few months of gestation lead to an increase in the probability of falling into the category of low birth weight.

Using a 3-month shock in the early gestation homicide rate of .25/10,000 as a base, these results suggest that the probability of being designated as low birth weight rises by 3-5%, which relative to a base of 7-10% represents a 40-50% increase in this risk factor.[16]

Another important aspect of a birth is its gestational length. Unfortunately, not all mothers were asked the question regarding the number of weeks each pregnancy lasted. The only available information about gestational length for all births comes from the following question asked to mothers: “Was your child from pregnancy #x born premature?” The rate of answering this question “Yes” was quite high, 16.5% for the sample under examination, while the expected rate of prematurity in Mexico is closer to 7-10%. The estimates when using this measure as the dependent variable in equation 3 are found in Column 3 of Table 8. Surprisingly there is a

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this analysis provides no evidence that stacking in birth weights is related to violence exposure. Table A4 displays these results.

[16] Alternatively, the impact of one additional homicide, during early gestation, in a municipality of 60,000 people is a 2-4% increase in the probability of being born low birth weight. Which is an effect size of 25-33% when scaled against the mean low birth rate incidence.

predicted reduction of self-assigned premature births with increased local homicide rates in mid pregnancy, as well as a potential increased risk if exposure is late in pregnancy. Given the lack of clarity in what the premature birth self-assignment measure is capturing, though, it is quite difficult to interpret this result.

Lastly, the MxFLS asks the mothers various details of the delivery of each child, including a question of whether there were any complications or difficulties with each pregnancy. Column 4 of Table 8 provides the results of estimating equation 3 using this measure as the dependent variable. Evaluating this analysis there does not appear to be any relationship between gestational violence exposure and pregnancy complications.

In summary, children born to mothers that were exposed in early gestation to the recent surge in conflict caused by the Mexican drug war have substantially and statistically significantly poorer birth outcomes. Moreover, the general findings are consistently replicated over several samples and multiple layers of additional controls.

4.3 Birth Outcomes: Effect Heterogeneity

This section will be focused on exploring whether the effect of local violence on birth outcomes is significantly larger for families/mothers of lower socioeconomic status. There are several reasons to think this group would be particularly susceptible to the impact of violence on birth outcomes. First of all, there is research that suggests that the most disadvantaged groups are the most likely targets of cartel victimization (Diaz-Cayeros et al., 2011). Moreover, due to this group's limited means, they may be less able to compensate for negative health shocks. Specifically, the analysis that is conducted in this paper is an examination of the impact of local violence on families in the bottom 50% of the per capita expenditure distribution at baseline or births to mothers with less than 9 years of education (compulsory level), respectively (Table 9).[17]

For this analysis, mother's education was assessed in two ways: educational attainment at baseline and educational attainment in MxFLS3. The advantage to using baseline education is that it is insulated from the potential endogenous effect of local violence exposure. On the other hand, by using education in MxFLS3 the measure will reflect completed education for the younger mothers in the sample. Results are similar when using either measure.

Columns 2-4 in Table 9 provide clear evidence that the birth weights of children born to families/mothers with lower socioeconomic status are impacted substantially harder by exposure to local violence early in gestation. In both cases the magnitude of the coefficient is more than double the size of the equivalent estimate (found in Column 1). Using .25/10,000 as a base for a 3-month early gestation impact of the Mexican drug war shock, this translates to a reduction in birth weight of around 120-125 grams.[18,19]

4.4 Prenatal Care

An additional analysis that can be explored in this data is the impact local violence exposure had on the number and timing of a mother's prenatal care visits during each pregnancy. Tables 10 and 11 present the results of using the number of prenatal care visits and the probability of initiating prenatal care in the first two months of pregnancy as the dependent variable in equation 3, respectively. In addition, each table includes analysis limited to the socioeconomic status subgroups introduced in the previous section.

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[17] Due to the reduced sample size, state of birth interacted with year of birth fixed effects are omitted.

[18] Alternatively, this estimate suggests an 80-84 gram drop in birth weight due to early gestation exposure to one additional homicide in a municipality with a population of 60,000 for lower socioeconomic status mothers.

[19] Analysis of births to the corresponding higher socioeconomic mothers/families is provided in Table A5.

In Column 1 of Table 10 the estimates of equation 3 suggest that violence exposure early in gestation led to significantly fewer overall prenatal care visits. The heterogeneity of this result, though, is less clear, as those born to families in the bottom 50 percent of the per capita expenditure distribution in MxFLS1 or born to mothers with less than 9 years of education in MxFLS3 (Columns 2 and 4) have an exacerbated effect of early gestation local violence on prenatal care visits, while mothers with less than compulsory education in MxFLS1 (Column 3) face qualitatively the same impact. Interestingly, there appears to be some evidence that elevated exposure to increased local homicide rates late in pregnancy leads to additional uptake of prenatal care amongst the low socioeconomic status subgroups. This finding may reflect either an increase in the compensatory behavior of these mothers or an increase in the number of pregnancy-related health complications these women face.

The sheer number of prenatal care visits, though, is only one component of prenatal care utilization, as several studies have shown that birth outcomes are significantly improved when prenatal care is initiated earlier in pregnancy (Rosenzweig and Schultz, 1983; Jewell and Triunfo, 2006; Jewell, 2007; Wehby et al., 2009). Table 11 contains the results of estimation of equation 3 when using the probability of prenatal care initiation in the first two months as the dependent variable.

As in Table 10, we see that exposure to local violence early in pregnancy led to poorer prenatal care, with mothers statistically significantly delaying prenatal care initiation, though this effect is not consistently stronger for low socioeconomic status mothers. Also there is evidence that the forward looking expectation of greater violence exposure late in pregnancy led to pro-health behaviors early in pregnancy amongst lower socioeconomic status families.

While these results display a very interesting behavioral response to local violence exposure, they do not seem to be the primary mechanism driving the poorer birth outcomes of exposed mothers, as the inclusion of both prenatal care measures as independent variables in equation 3 only partially (5-22%) attenuates the results from Tables 6 and 8. Specifically, including the number of prenatal care visits and whether prenatal care initiation began in the first two moths of pregnancy into equation 3 reduces the estimate of the effect of increased conflict exposure 9 to 7 months before birth from 299.9 to 281.1, 10.7% to 8.3%, and 21.2% to 20.1% for birth weight, being born <2,500 grams, and being born ≤2,500 grams respectively.[20]

5. Discussion

5.1 Relative Size of the Effect

The analysis presented in this paper makes a strong and consistent statement that exposure to local violence in early pregnancy leads to statistically significantly smaller births. Placing the size of the effect in context, though, is of first order importance, as it provides guidance for determining the severity of this concealed cost of crime and conflict.

One place to start when assessing the real toll on society caused by violence on birth outcomes is neonatal mortality.[21] Neonatal mortality has been shown to have a strong relationship to birth size. Almond, Chay, and Lee's estimate that a 100 gram increase in birth weight leads to a reduction of 1.5 neonatal deaths per 1,000 births (2005). Applying this to the findings presented previously suggests that exposure to a .25 per 10,000 increase in the homicide rate in early

[20] Results are found in Table A6.
[21] Neonatal mortality is typically defined as the death of a live birth within 28 days.

gestation results in more than 1 additional neonatal death per 1,000 births, or an increase of almost 2 deaths per 1,000 births amongst mothers of lower socioeconomic status.[22,23]

A different way to provide perspective on the magnitude of the adverse impact of the Mexican drug war on fetal health is to compare its effect to commonly cited drivers of birth outcomes such as nutrition and maternal smoking. The Supplemental Nutrition Program for Women, Infants, and Children (WIC) and the Food Stamp Program (FSP) in the United States have been evaluated in terms of their positive effect on birth weight by Hoynes et al. and Almond et al., respectively (2011).

Hoynes et al. estimate a 2 gram effect of WIC on the average population and a 18-29 gram impact amongst participants, and Almond et al. find that FSP led to birth weight increases of 2-5 grams in general and 15-40 grams amongst the treated (2011). These effect sizes suggest that the impact of these programs, even amongst the highly selected participants, is considerably smaller than the harmful influence of being in utero during high levels of local violence.

Smoking, the most commonly accepted environmental risk factor of fetal health, provides an additional way to contextualize the size of the impact of the Mexican drug war. Taking the estimates produced from multiple strategies to assess the effect of maternal smoking on birth weight provides a general finding of around a 200-230 gram and a 100-130 gram loss in birth weight for children born to women that were heavy (11+ cigarettes a day) and light smokers during pregnancy, respectively (Rosenzweig and Schultz, 1983; Sexton and Hebel, 1984; Brooke

[22] Additionally, using the estimates in Almond, Chay, and Lee as a guide, the results presented in this paper suggest exposure in early gestation to a conflict shock of the size of the shift between the pre-escalation of violence period of 2005-2007 and 2009 in Mexico leads to more than a 1.5 and 2.5 increase in deaths per 1,000 births in the first year of life for all and lower socioeconomic mothers, respectively (2005).

[23] It should be noted that if the relationship between birth weight and neonatal morality depends on the cause of the birth weight variation, this extrapolation might be an over or underestimate.

et al., 1989; Wilcox, 1993; Almond et al., 2005; Ward et al., 2007). This suggests that the impact of the rise in violence in Mexico between the pre-conflict period of 2005-2007 and 2009 on birth weight was at least one-third and two-thirds the size of the effect of having a heavy and light smoking mother during gestation, respectively. If focusing on the larger harm done to the birth outcomes of the mothers of lower socioeconomic status, the violence in Mexico had an effect half the size of having a heavy smoking mother or was equivalent to being exposed to a lightly smoking mother during gestation.

A final way to evaluate the magnitude of the effect of the Mexican drug war on fetal health is to compare its adverse impact to the gains achieved in Mexico by *Oportunidades/PROGRESA*, a government social assistance program partially designed to improve birth outcomes of participating women. *Oportunidades* (formerly *PROGRESA*) is a large-scale conditional cash transfer (CCT) program in Mexico that targets poorer families and ties compensation to investment in the education and health of the household's children. One component of the program was a condition that pregnant women needed to complete a prearranged prenatal care plan, acquire specific nutritional supplements, and attend meetings that focus on pregnancy health education (Barber and Gertler 2008). Evaluation of the impact this program had on the birth outcomes of participating mothers suggests that the children exposed to *Oportunidades* in utero were born 127 grams heavier (Barber and Gertler 2008). This estimate mirrors the magnitude of the negative impact on birth weight of exposure to the Mexican drug war in early gestation for lower socioeconomic status mothers (the group most similar to the *Oportunidades* sample), suggesting this recent conflict could be eliminating the gains of one of the oldest and largest CCTs in existence.

5.2 Mechanisms

One area in which this analysis is unable to make particularly definitive statements is in terms of the relative importance of each of the potential pathways through which local violence impacts birth outcomes. The main avenues suggested by the literature for an effect of local violence on birth outcomes are: biological reaction to anxiety, poorer health behaviors (e.g. smoking, less exercise), decreased use of health care, or constrained nutrient intake. Given the results mentioned in subsection 4.4 and shown in Table A6, while it appears prenatal care was reduced and delayed due to exposure to violence, this is not the primary cause of the reduction in birth weight, accounting for only around 10% of the effect.

Another mechanism worth considering is reduced nutrition. There is considerable evidence that increased local homicide rates led to poorer economic outcomes (employment, earnings, hourly wages) for the exposed adults (Dell, 2011; Velasquez, 2013; Robles et al., 2013). If this shock to economic outcomes served to restrict the budget constraint of the household, leading to less nutrients being consumed by a pregnant family member, this may directly impact the fetal health of the in utero child. Almond and Mazumder find that a fetus, provided limited nutrients due to the mother's experience of Ramadan, is statistically significantly reduced in size at birth (2011). This effect, though, is not restricted to early gestation exposure. Reduced nutrient intake due to Ramadan reduced birth weight amongst children exposed in the first trimester (20 grams), as well as, the second trimester (26 grams). The wider temporal scope of the effect of restricted nutrient intake, as compared to the limited timing of the effect of local homicides, suggests that this may not be the primary pathway through which violence is impacting birth outcomes, although it does not completely rule out its contribution. For instance, it may be the case that when a family experiences a shock in earnings in mid gestation, they reallocate resources specifically to avoid the pregnant mother facing reduced nutrition, whereas this is not the case when a family is faced with financial deficits early in a women's pregnancy.

A more compelling argument that nutrition is not the main driver of the results is that the effect size estimated by Almond and Mazumder, as well as, the various studies of nutrition programs mentioned previously, such as WIC (2-5 grams) and FSP (2-5 grams), is significantly smaller than those found in this study. Given that those studies focused directly on nutrition and found much smaller effects, it is less likely that the hypothesized potential nutrient restriction from Mexican drug war exposure would be the leading mechanism resulting in the large negative estimates found in this paper. Unfortunately, without detailed individual-level consumption data over the gestation period, the level, timing, and impact of potential nutrient restrictions cannot be assessed.

The mechanism most commonly thought to be operating on birth outcomes during conflict is the fetus's exposure to maternal anxiety. The timing of the effect estimated in this study, early gestation, matches up with the findings from the medical and economics literature on the impact of maternal mental distress on fetal health (Schulte et al., 1990; de Weerth and Buitelaar, 2005; Camacho, 2008; Torche, 2011; Brown, 2013). Moreover, while the results in this paper are much larger than those found in Camacho or Brown (9 and 15 grams, respectively), they are of the magnitude of those reported by Torche (51 grams).[24] This suggests that if maternal stress is the

[24] One potential reason for the large disparity in the results between this study and the works by Camacho and Brown is that they may have been using stress shocks of a smaller magnitude. For instance, the exogenous stressor used in Camacho's work, landmine explosions, is arguably so random that it may not be a particularly strong signal of conflict intensity in that area at that moment and thus the 9 gram estimate is a reflection of anxiety only due to the recent detonations but not to any fear of personal harm or victimization.
In the case of Brown's examination of the impact of the 9/11 terrorist attacks on the birth outcomes of exposed mothers, his estimate is attenuated as it must exclude mothers living in areas that were directly attacked (New York City and Washington D.C. metro area) in order to avoid confounding factors such as pollution and the subsequent reduction in economic activity.

first order pathway being exploited in this study, the violence and victimization caused by the Mexican drug war may be leveling the same amount of anxiety on its population as a natural disaster such as an earthquake.

A final potential mechanism that may be triggering the large influence local violence is having on birth outcomes is a change in maternal health behaviors. If exposure to local conflict spurs mothers to engage in risk-taking and/or health-reducing behaviors such as smoking or reduced exercise, this would have a non-trivial impact on the health of the fetus. While the MxFLS does not have data on tobacco consumption or physical activity with timing information at the level of detail needed to test if these behaviors saw an uptick in expression during each pregnancy, analysis can be conducted to determine if exposure to local violence changes the smoking and exercise behavior of the mothers in this sample.

Utilizing the longitudinal nature of the MxFLS, a respondent's level of smoking or exercise when measured in MxFLS2 can be compared to the same respondent's behavior in MxFLS3. Specifically, it is possible to determine if the amount of local violence experienced in the year leading up to the interview significantly changed health oriented behaviors while controlling for all time-invariant heterogeneity at the individual level. As before, in order to eliminate the issue of endogenous migration, homicide rate level is assigned based on the pre-escalation of violence location of residence, in this case the MxFLS2 municipality of residence.

To examine this relationship the following equation was estimated:

$$y_{iwjk} = \gamma + v_i + \rho_j + \varphi_{YOI} + \omega_{MOI} + \beta' X_{iwjk} + (HOM, 1 - 12\ months\ before\ interview)_k + u_{iwjk} \qquad (4)$$

where, y, is the health behavior of mother i, in survey wave w, interviewed in municipality j, and residing in municipality, k, in MxFLS2. $(HOM, 1-12\ months\ before\ interview)_k$, represents the homicide rate over the 12 months prior to the interview in the municipality the respondent resided in at MxFLS2 interview, v_i, φ_{YOI}, ω_{MOI}, and ρ_j represent fixed effects at the individual, year of interview, month of interview, and municipality of interview level, and, X_{iwjk} is a vector of time-varying individual (age, marital status, educational attainment, monthly earnings, and employment status) and household (household size, rural status, and per capita expenditure) characteristics.

Columns 1, 2, and 3 of Table 12 display the results of estimating equation 4 using the number of cigarettes smoked per week, the number of exercise days between Monday and Friday, and the amount of exercise time per day as the dependent variable, respectively. The results provide evidence that these mothers were not spurred to change health behavior in a negative way due to local violence exposure. While these results indicate that the negative impact on birth outcomes due to local conflict exposure was not a result of increased smoking or decreased exercise, the analyses are not detailed enough to detect changes during pregnancy periods and can only be taken as suggestive evidence.

6. Conclusion

The sudden and horrific internal conflict in Mexico has cost the country thousands of lives and disseminated a widespread sense of insecurity amongst the non-combatants. Research has documented some of the explicit effects of the violence such as increased victimization (extortions, kidnappings) and losses of earnings and employment. What has been left unexplored is the toll the increased violence may have on the well-being of the next generation.

Medical and economic research has continually produced a link between birth outcomes and markers of long run health, education, and employment outcomes. This set of facts, paired with the potential mechanisms at play in Mexico that may effect fetal health (maternal stress, resource restriction, reduced prenatal care), provides reason to believe that the Mexican drug war can adversely impact the long-term trajectories of those exposed in utero.

Evaluating the effect of conflict in any region on individual-level health outcomes always faces the challenges of separating out spurious relationships, as well as, tracking and correcting for behavioral responses. With these concerns in mind, this analysis is conducted on a violent conflict that escalated swiftly and with a great deal of heterogeneity, using data that contains the non-conflict and conflict periods, allows for the documentation and control of the potential concerns raised by systematic behavioral response, and can take advantage of sibling comparisons.

This research finds that, once migration, fertility behavior, and between mother heterogeneity is accounted for, estimates of the effect of local violence on birth outcomes are remarkably consistent and large. Specifically, the analysis finds that an increase in local violence in early gestation of the scale experienced in Mexico between the pre-escalation of violence period of 2005-2007 and 2009 leads to a 75 gram reduction in birth weight and a 40-50% increased risk being born low birth weight. The large magnitude of the effect on birth weight is further exacerbated amongst those of lower socioeconomic status, with the children of this group of exposed mothers facing decreases of around 120-125 grams on average.

These estimates suggest that exposure to the Mexican drug war causes damage to birth weight 10 times larger than the gains found in U.S federal nutrition programs and about one-half the size of being born to a mother that smokes during pregnancy. Moreover, for children of lower socioeconomic households, the adverse effect of the recent exposure is equivalent to the gains scene in the birth weight of pregnant mothers enrolled in the large and successful Mexican conditional cash transfer program, *Oportunidades.* Most disturbingly, the homicide rate in many municipalities in Mexico has only continued to rise since 2009, suggesting these effect sizes are in fact lower bounds of the overall toll this internal war has taken on the next generation of Mexicans citizens.

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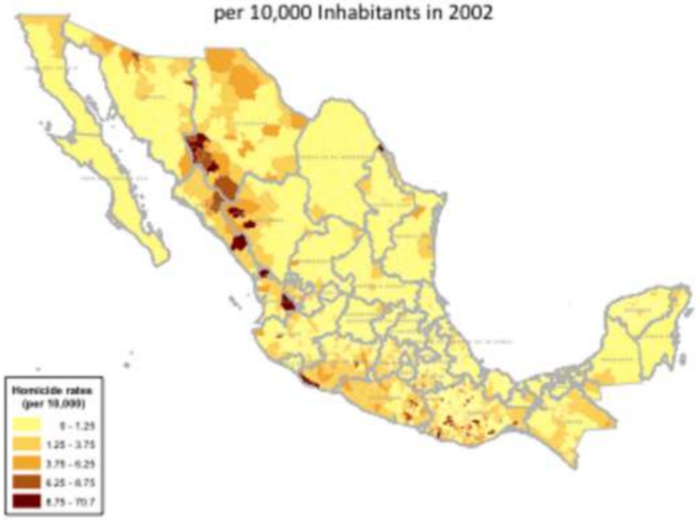
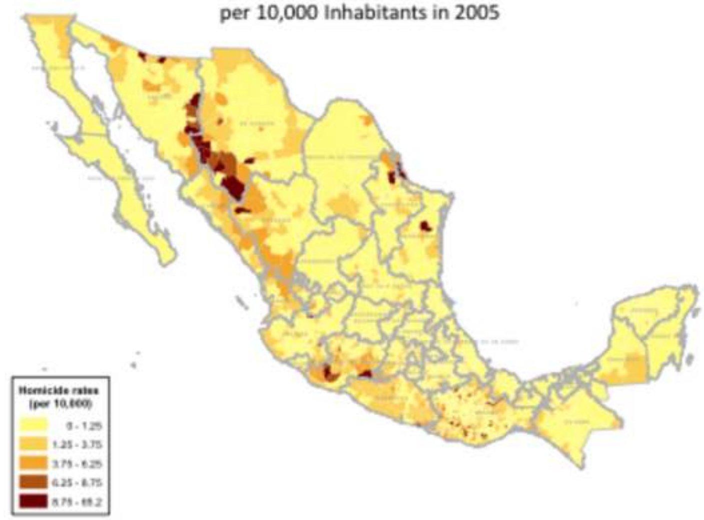
Map 1: Municipality Homicide Rates
per 10,000 Inhabitants in 2002

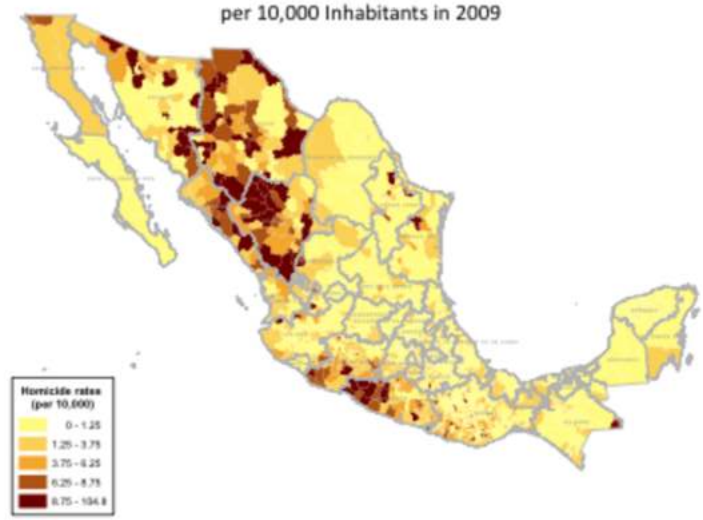


Map 2: Municipality Homicide Rates
per 10,000 Inhabitants in 2005



Map 3: Municipality Homicide Rates
per 10,000 Inhabitants in 2007

Map 4: Municipality Homicide Rates
per 10,000 Inhabitants in 2009



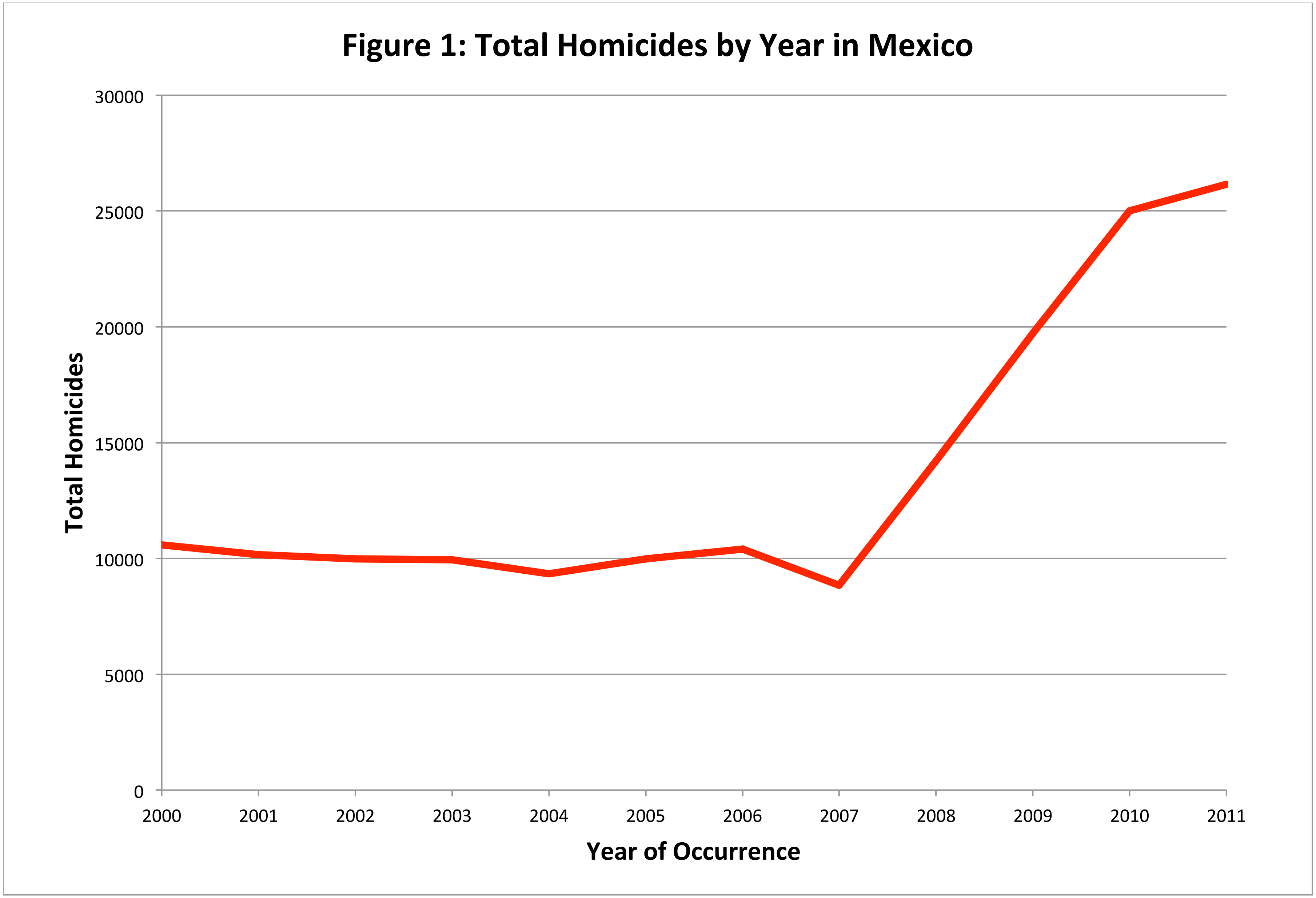


Figure 1: Total Homicides by Year in Mexico
Total Homicides
30000
25000
20000
15000
10000
5000
0
2000
2001
2002
2003
2004
2005
2006
2007
2008
2009
2010
2011
Year of Occurrence


Figure 2: Homicide Rate by Year and Month in Mexico (per 10,000)
Homicide Rate (per 10,000)
0.25
0.2
0.15
0.1
0.05
0
2000
2001
2002
2003
2004
2005
2006
2007
2008
2009
2010
Year and Month of Occurrence

Table 1
Respondent Attrition and Potential Future Homicide Rate Exposure
for Women Aged 7 to 42 in MxFLS1 or Women Aged 10 to 45 in MxFLS2

Homicide Rate	**(1)**	**(2)**	**Homicide Rate**	**(3)**	**(4)**
Change Between 2002 & 2009	0.34%		Change Between 2005 & 2009	0.14%	
	(0.21)			(0.16)	
MxFLS1 Characteristics Interacted with Homicide Rate Change Between 2002 & 2009:			*MxFLS2 Characteristics Interacted with Homicide Rate Change Between 2005 & 2009:*		
Age		0.07%	Age		-0.04%
		(0.09)			(0.07)
Age Squared		0.00%	Age Squared		0.00%
		(0.00)			(0.00)
Lived in Rural Locality		0.07%	Lived in Rural Locality		-0.27%
		(0.52)			(0.38)
Year of Education		0.03%	Year of Education		0.00%
		(0.05)			(0.05)
Employed		0.00%	Employed		0.00%
		(0.00)			(0.00)
Earnings per Month (1,000 Pesos)		-0.18%	Earnings per Month (1,000 Pesos)		0.08%
		(0.11)			(0.07)
PCE of HH (10,000 Pesos)		0.37%	PCE of HH (10,000 Pesos)		-0.04%
		(0.42)			(1.07)
Married		0.00%	Married		0.00%
		(0.00)			(0.00)
Household Size		-0.10%	Household Size		0.14%
		(0.11)			(0.08)
Observations	11,511	11,511	Observations	10,429	10,429
Mean of Dependent Variable	13.7%	13.7%	Mean of Dependent Variable	8.4%	8.4%

Notes:
***, ** denote significance at the 1 and 5% level, respectively
Homicide rates are per 10,000. Standard errors are clustered at the municipality level
Regressions include respondent's years of education, marital status, employment status, earnings per month,
household per capita expenditure and size, an indicator for whether the municipality is rural, and fixed effects for age, interview year, and month.

Table 2

Previous Municipal Trends and Levels of Characteristics' Relationship to Current Homicide Rate

	Municipal Homicide Rate (per 10,000)	
	Level in 2009	**Change From 2005 to 2009**
Municipality Characteristics	**(1)**	**(2)**
CENSUS: Change in Share of Households Between 2000-2005 with:		
Televisions	-6.73	-5.33
	(6.13)	(7.76)
Piped Water	-1.59	4.53
	(5.01)	(5.59)
Sewage System	1.52	-4.62
	(3.74)	(4.44)
Electricity	1.60	7.97
	(9.81)	(11.05)
CENSUS: Change in Share of 21-65 Year Olds Between 2000-2005 with:		
Less Than Primary Education	-0.15	-18.64 *
	(8.27)	(9.97)
At Least High School Diploma	-11.19	-29.55 *
	(13.40)	(15.20)
Speak Indigenous Language	-3.47	-10.25
	(6.91)	(6.76)
Illiterate	-13.38	-22.81
	(27.76)	(31.62)
CENSUS: Change Between 2000-2005 in Share of:		
Less Than 18 Year Olds	7.83	-3.16
	(18.72)	(22.58)
18 to 65 Year Olds	3.79	-10.17
	(27.07)	(29.57)
CENSUS: Change Between 2000-2005 in:		
Average Educational Attainment	1.12	1.02
	(1.46)	(1.61)
MxFLS: Change in Share of Older than 18 Year Olds Between MxFLS1-MxFLS2:		
Married	-4.43	-6.36
	(6.12)	(6.77)
Employed Females	-0.88	1.31
	(4.40)	(4.64)
Employed Males	0.27	1.46
	(4.69)	(4.63)
Self-Employed Females	-2.42	-5.18
	(4.66)	(4.73)
Self-Employed Males	2.89	4.20
	(3.76)	(3.98)
Rural	1.63	2.07 *
	(1.02)	(1.10)
Have Relative in the U.S.	-2.96 *	-2.73
	(1.64)	(1.68)
Have Thoughts of Future Migration	-2.15	-0.12
	(3.34)	(3.55)
Have Fear in the Day	-1.15	0.41
	(6.39)	(6.95)
Have Fear in the Night	-4.70	-6.23
	(6.44)	(6.69)
MxFLS: Change Between MxFLS1-MxFLS2 in:		
Average Household Size	0.03	-0.10
	(0.68)	(0.69)
Log Hourly Earning of Females Older than 18 (Pesos)	0.29	-0.01
	(0.46)	(0.48)
Log Hourly Earning of Males Older than 18 (Pesos)	0.78	0.35
	(0.71)	(0.70)
Log Household Per Capita Expenditure (Pesos)	0.86	1.16
	(1.05)	(1.22)
MxFLS: Change in Share of Localities Between MxFLS1-MxFLS2 with:		
Increased Domestic Violence	-0.05	-0.13
	(0.44)	(0.44)
Presence of Vandalism	0.50	0.35
	(0.38)	(0.43)
Presence of Police	0.19	0.13
	(0.40)	(0.43)
MxFLS: Change Between MxFLS1-MxFLS2 in Localities:		
Number of Primary Schools	0.00	0.00
	(0.00)	(0.00)
Number of Junior Highs	-0.01	0.00
	(0.01)	(0.01)
Number of High Schools	0.01	0.00
	(0.01)	(0.01)
Rate of Poor Households	0.00	-0.01
	(0.01)	(0.01)
Observations	135	135
Mean of Dependent Variable	1.89	0.97
F test: Jointly 0; Prob>F	0.20	0.23

Notes:

***, **, * denote significance at the 1, 5, and 10% level, respectively. Robust standard errors provided.

Table 3
Descriptive Statistics

Homicide Rate	All Births to Panel Members Since MxFLS2: Full Sample	All Births to Panel Members Since MxFLS2: Sibling Sample	All Births Before 3Q2009 to Panel Members, Since MxFLS2: Full Sample	All Births Before 3Q2009 to Panel Members, Since MxFLS2: Sibling Sample	All Births Before 3Q2009 to Panel Members, Adds Siblings Born Since Baseline: Full Sample	All Births Before 3Q2009 to Panel Members, Adds Siblings Born Since Baseline: Sibling Sample
	(1)	(2)	(3)	(4)	(5)	(6)
Birth Outcomes:						
Birth weight in grams (g)	3,241.5	3,205.8	3,249.8	3,238.0	3,241.6	3,249.4
	(619.2)	(600.2)	(624.9)	(595.8)	(609.7)	(585.5)
Low birth weight (<2,500g)	7.7%	7.6%	7.6%	6.2%	7.7%	7.1%
	(26.7)	(26.6)	(26.5)	(24.1)	(26.7)	(25.6)
Low birth weight+ (≤2,500g)	10.6%	12.1%	10.8%	11.1%	10.9%	10.7%
	(30.8)	(32.6)	(31.0)	(31.4)	(31.1)	(30.9)
Premature Birth	19.6%	21.7%	19.8%	19.9%	18.7%	16.5%
	(39.7)	(41.2)	(39.9)	(40.0)	(39.0)	(37.1)
Any Pregnancy Complication	8.1%	5.1%	8.3%	3.6%	8.4%	6.5%
	(27.3)	(22.1)	(27.6)	(18.7)	(27.7)	(24.7)
Male	51.4%	50.7%	50.4%	50.8%	50.3%	50.8%
	(50.0)	(50.0)	(50.0)	(50.1)	(50.0)	(50.0)
Medical Care:						
Born in Hospital	88.9%	85.6%	88.6%	85.0%	88.7%	88.0%
	(31.5)	(35.2)	(31.8)	(35.7)	(31.7)	(32.5)
Number of Prenatal Visits	7.9	7.2	7.9	7.3	7.7	7.3
	(4.1)	(3.3)	(4.0)	(3.4)	(4.0)	(3.7)
Prenatal Care, First 2 Months	66.4%	65.8%	67.5%	68.7%	66.5%	64.3%
	(47.3)	(47.5)	(46.9)	(46.4)	(47.2)	(47.9)
Mother's Characteristics at Baseline:						
Age	19.2	18.0	19.6	18.3	19.6	20.4
	(6.7)	(5.9)	(6.7)	(5.6)	(6.7)	(5.9)
Lived in Rural Locality	51.1%	55.0%	51.1%	55.0%	51.1%	51.6%
	(50.0)	(49.9)	(50.0)	(49.9)	(50.0)	(50.0)
Year of Education	7.5	7.2	7.6	7.4	7.6	7.9
	(3.5)	(3.3)	(3.4)	(3.1)	(3.4)	(3.1)
Employed	31.6%	31.5%	31.0%	29.4%	31.0%	30.0%
	(46.5)	(46.6)	(46.3)	(45.8)	(46.3)	(45.9)
Earnings per Month (Pesos)	575.2	399.3	524.3	380.8	524.3	583.6
	(1,966.5)	(913.1)	(1,899.7)	(897.7)	(1,899.7)	(2,839.3)
PCE of HH (Pesos)	1161.9	946.6	1185.3	974.2	1185.3	1350.2
	(4031.8)	(1176.0)	(4302.2)	(1193.3)	(4302.2)	(5434.6)
Married	28.6%	17.8%	30.0%	19.5%	30.0%	30.2%
	(45.2)	(38.4)	(45.8)	(39.8)	(45.8)	(46.0)
Number of Births	1,850	471	1,548	307	1,868	891
Number of Mothers	1,608	229	1,392	151	1,392	415

Notes:
Standard deviation in parenthesis.

Table 4
Respondent Migration Between MxFLS2 and MxFLS3 and Potential Future Homicide Rate Exposure for Panel Women that Gave Birth Since MxFLS2 and before 2Q2009

Homicide Rate	Interviewed in Different Municipality (1)	Interviewed in Different Municipality (2)	Migrated for at Least 1 Year (3)	Migrated for at Least 1 Year (4)	Migrated for at Least 1 Month (5)	Migrated for at Least 1 Month (6)
Change Between 2005 & 2009	1.18%		1.55% **		1.39% *	
	(0.75)		(0.72)		(0.76)	
MxFLS2 Characteristics Interacted with Homicide Rate Change Between 2005 & 2009:						
Age		-0.03%		0.09%		0.11%
		(0.25)		(0.18)		(0.18)
Age Squared		0.00%		0.00%		0.00%
		(0.00)		(0.00)		(0.00)
Lived in Rural Locality		-0.05%		2.16% **		2.69% ***
		(0.82)		(0.99)		(1.02)
Year of Education		0.05%		-0.05%		-0.07%
		(0.07)		(0.11)		(0.10)
Employed		-0.05%		-0.01%		-0.04%
		(0.10)		(0.23)		(0.21)
Earnings per Month (1,000 Pesos)		0.35%		0.66% *		0.75% *
		(0.23)		(0.37)		(0.44)
PCE of HH (10,000 Pesos)		1.47%		7.02% **		6.62% *
		(2.55)		(2.91)		(3.91)
Married		0.05%		-0.17%		-0.15%
		(0.10)		(0.26)		(0.28)
Household Size		0.03%		-0.23%		-0.16%
		(0.13)		(0.16)		(0.18)
Observations	1,346	1,346	1,277	1,277	1,277	1,277
Mean of Dependent Variable	6.8%	6.8%	15.9%	15.9%	17.4%	17.4%

Notes:
***, **, * denote significance at the 1, 5, and 10% level, respectively. Homicide rates are per 10,000. Standard errors are clustered at the municipality level
Regressions include controls for respondent's MxFLS2 education, age fixed effects, employment status, earnings per month, household size, marital status, household's per capita expenditure, and rural status. Additionally all regressions control for all municipality characteristic trends and levels shown in Table 2, as well as, year of interview, month of interview, and MxFLS2 state of residence fixed effects.

Table 5
Impact of Local Homicide Rate on Birth Rate at the Municipality-Month Level from January 2003 to June 2009

	Birth Rate of 14-49 Yr. Old Women (per 1,000)		
	All	<9 Yrs. Education	≥9 Yrs. Education
Homicide Rate	(1)	(2)	(3)
15 Months Before Birth	-0.23	0.08	-0.55 **
	(0.19)	(0.37)	(0.25)
14 Months Before Birth	0.11	0.80	-0.50
	(0.27)	(0.52)	(0.25)
13 Months Before Birth	0.15	0.68	-0.31
	(0.26)	(0.48)	(0.24)
12 Months Before Birth	-0.14	0.17	-0.57 **
	(0.26)	(0.38)	(0.24)
11 Months Before Birth	-0.17	0.40	-0.17
	(0.20)	(0.43)	(0.26)
10 Months Before Birth	-0.10	-0.10	-0.22
	(0.28)	(0.44)	(0.24)
9 to 7 Months Before Birth	0.00	0.20	0.19
	(0.14)	(0.22)	(0.25)
6 to 4 Months Before Birth	-0.19	0.04	-0.36 **
	(0.16)	(0.25)	(0.17)
3 to 1 Months Before Birth	0.12	0.38	0.11
	(0.18)	(0.38)	(0.27)
Mean Monthly Birth Rate (1,000)	2.7	3.8	2.7
Municipality-Months	10,452	10,452	10,451

Notes:

***, ** denote significance at the 1 and 5% level, respectively. Homicide rates are per 10,000.

Robust standard errors provided. Regressions include year, month, municipality, and state interacted with year fixed effects.

Table 6
Impact of Local Homicide Rate on Birth Weight
for All Births to Panel Members Since MxFLS2 and Before July 2009, Adding Siblings Born Since MxFLS1

Homicide Rate	(1)	(2)	(3)	(4)	(5)	(6)	(7)
9 to 7 Months Before Birth	-0.9	7.3	-46.9	-120.3 **	-222.5 **	-226.9 **	-299.9 ***
	(34.4)	(56.8)	(75.1)	(60.3)	(97.4)	(91.5)	(106.0)
6 to 4 Months Before Birth	-61.5 **	-66.0	-89.4	31.7	53.9	38.9	118.3
	(29.7)	(40.3)	(78.2)	(58.8)	(71.4)	(69.7)	(84.8)
3 to 1 Months Before Birth	35.6	-2.8	15.9	21.8	121.4	67.7	6.2
	(35.1)	(48.6)	(75.6)	(59.2)	(103.1)	(79.0)	(86.1)
Sibling Sample	NO	NO	YES	YES	YES	YES	YES
Sibling Fixed Effects	NO	NO	NO	YES	YES	YES	YES
Municipality Fixed Effects	NO	YES	YES	NO	YES	YES	YES
Pre/Post Gestation Hom Rates	NO	YES	YES	NO	NO	YES	YES
State and YOB Fixed Effects	NO	YES	YES	NO	NO	NO	YES
Mean of Dependent Variable	3241.6	3241.6	3,249.4	3,249.4	3,249.4	3,249.4	3,249.4
Observations	1,868	1,868	891	891	891	891	891
Number of Mothers				415	415	415	415

Notes:
***, **, * denote significance at the 1, 5, and 10% level, respectively. Homicide rates are per 10,000.
Standard errors are clustered at the municipality level. Regressions additionally include controls for the gender of the child, maternal (age at birth, age at birth squared, years of education, employment status, earnings per month, and marital status) and household characteristics (size, per capita expenditure, and rural status), as well as, year of birth, month of birth, year of interview, month of interview, birth order, and survey wave fixed effects.

Table 7
Impact of Local Homicide Rate on Birth Weight
for All Births to Panel Members Since MxFLS2, Adding Siblings Born Since MxFLS1

	All Births to Panel Members Since MxFLS2 and Before3Q2009			
	All	**Before 2Q2009**	**Before 2009**	**In Clinic or Hospital**
Homicide Rate	**(1)**	**(2)**	**(3)**	**(4)**
9 to 7 Months Before Birth	-299.9 ***	-282.0 **	-324.0 **	-348.4 *
	(106.0)	(122.5)	(133.5)	(197.6)
6 to 4 Months Before Birth	118.3	32.8	44.4	91.3
	(84.8)	(91.2)	(97.0)	(152.5)
3 to 1 Months Before Birth	6.2	34.9	-55.0	93.0
	(86.1)	(125.1)	(121.5)	(159.2)
Sibling Sample	YES	YES	YES	YES
Sibling Fixed Effects	YES	YES	YES	YES
Municipality Fixed Effects	YES	YES	YES	YES
Pre/Post Gestation Hom Rates	YES	YES	YES	YES
State and YOB Fixed Effects	YES	YES	YES	YES
Mean of Dependent Variable	3,249.4	3,257.0	3,248.6	3,251.3
Observations	891	809	735	727
Number of Mothers	415	377	343	339

Notes:
***, **, * denote significance at the 1, 5, and 10% level, respectively. Homicide rates are per 10,000.
Standard errors are clustered at the municipality level. Regressions additionally include controls for the gender of the child, maternal (age at birth, age at birth squared, years of education, employment status, earnings per month, and marital status) and household characteristics (size, per capita expenditure, and rural status), as well as, year of birth, month of birth, year of interview, month of interview, birth order, and survey wave fixed effects.

Table 8
Impact of Local Homicide Rate on Birth Outcomes
for All Births to Panel Members Since MxFLS2 and Before July 2009, Adding Siblings Born Since MxFLS1

	LBW (<2,500g)	LBW+ (≤2,500g)	Child Premature	Complications
Homicide Rate	**(1)**	**(2)**	**(3)**	**(4)**
9 to 7 Months Before Birth	10.7% **	21.2% ***	4.2%	1.7%
	(5.0)	(7.7)	(6.7)	(5.8)
6 to 4 Months Before Birth	-4.1%	-5.1%	-9.5% **	-3.5%
	(3.7)	(5.4)	(4.4)	(3.7)
3 to 1 Months Before Birth	-0.3%	3.4%	11.4% *	0.5%
	(4.2)	(7.4)	(6.5)	(5.5)
Sibling Sample	YES	YES	YES	YES
Sibling Fixed Effects	YES	YES	YES	YES
Municipality Fixed Effects	YES	YES	YES	YES
Pre/Post Gestation Hom Rates	YES	YES	YES	YES
State and YOB Fixed Effects	YES	YES	YES	YES
Mean of Dependent Variable	7.1%	10.7%	16.5%	6.5%
Observations	891	891	891	886
Number of Mothers	415	415	415	413

Notes:

***, **, * denote significance at the 1, 5, and 10% level, respectively. Homicide rates are per 10,000.

Standard errors are clustered at the municipality level. Regressions additionally include controls for the gender of the child, maternal (age at birth, age at birth squared, years of education, employment status, earnings per month, and marital status) and household characteristics (size, per capita expenditure, and rural status), as well as, year of birth, month of birth, year of interview, month of interview, birth order, and survey wave fixed effects.

Table 9
Impact of Local Homicide Rate on Birth Weight
for Births to Subgroups of Panel Members Since MxFLS2 and Before July 2009, Adding Siblings Born Since MxFLS1

	All		Bottom 50% PCE in MxFLS1		<9 Yrs. Education in MxFLS1		<9 Yrs. Education in MxFLS3	
Homicide Rate	**(1)**		**(2)**		**(3)**		**(4)**	
9 to 7 Months Before Birth	-226.9	**	-501.9	**	-500.5	***	-475.1	***
	(91.5)		(240.1)		(129.7)		(162.2)	
6 to 4 Months Before Birth	38.9		38.2		38.6		188.7	
	(69.7)		(126.5)		(103.6)		(147.1)	
3 to 1 Months Before Birth	67.7		-60.7		-45.4		-85.6	
	(79.0)		(157.1)		(126.4)		(173.4)	
Sibling Sample	YES		YES		YES		YES	
Sibling Fixed Effects	YES		YES		YES		YES	
Municipality Fixed Effects	YES		YES		YES		YES	
Pre/Post Gestation Hom Rates	YES		YES		YES		YES	
State and YOB Fixed Effects	NO		NO		NO		NO	
Mean of Dependent Variable	3,249.4		3,231.1		3,214.5		3,221.6	
Observations	891		432		487		364	
Number of Mothers	415		203		224		165	

Notes:
***, **, * denote significance at the 1, 5, and 10% level, respectively. Homicide rates are per 10,000. Standard errors are clustered at the municipality level. Regressions additionally include controls for the gender of the child, maternal (age at birth, age at birth squared, years of education, employment status, earnings per month, and marital status) and household characteristics (size, per capita expenditure, and rural status), as well as, year of birth, month of birth, year of interview, month of interview, birth order, and survey wave fixed effects.

Table 10
Impact of Local Homicide Rate on Prenatal Care Visits
for All Births to Panel Members Since MxFLS2 and Before July 2009, Adding Siblings Born Since MxFLS1

	All	Bottom 50% PCE in MxFLS1	<9 Yrs. Education in MxFLS1	<9 Yrs. Education in MxFLS3
Homicide Rate	**(1)**	**(2)**	**(3)**	**(4)**
9 to 7 Months Before Birth	-1.2 **	-2.7 ***	-1.7	-2.7 **
	(0.6)	(0.8)	(1.1)	(1.1)
6 to 4 Months Before Birth	-0.3	-0.5	0.2	0.4
	(0.5)	(0.6)	(0.4)	(0.7)
3 to 1 Months Before Birth	-0.1	2.2 *	2.1 **	2.8 ***
	(0.5)	(1.2)	(1.0)	(1.0)
Sibling Sample	YES	YES	YES	YES
Sibling Fixed Effects	YES	YES	YES	YES
Municipality Fixed Effects	YES	YES	YES	YES
Pre/Post Gestation Hom Rates	YES	YES	YES	YES
State and YOB Fixed Effects	YES	NO	NO	NO
Mean of Dependent Variable	7.3	6.9	7.0	6.9
Observations	886	429	482	361
Number of Mothers	413	202	222	164

Notes:
***, **, * denote significance at the 1, 5, and 10% level, respectively. Homicide rates are per 10,000. Standard errors are clustered at the municipality level. Regressions additionally include controls for the gender of the child, maternal (age at birth, age at birth squared, years of education, employment status, earnings per month, and marital status) and household characteristics (size, per capita expenditure, and rural status), as well as, year of birth, month of birth, year of interview, month of interview, birth order, and survey wave fixed effects.

Table 11

Impact of Local Homicide Rate on Prenatal Care Initiation in the First Two Months of Pregnancy for All Births to Panel Members Since MxFLS2 and Before July 2009, Adding Siblings Born Since MxFLS1

	All	Bottom 50% PCE in MxFLS1	<9 Yrs. Education in MxFLS1	<9 Yrs. Education in MxFLS3
Homicide Rate	**(1)**	**(2)**	**(3)**	**(4)**
9 to 7 Months Before Birth	-29.4% ***	-36.0% ***	-15.8%	-9.6%
	(9.8)	(11.2)	(16.5)	(19.8)
6 to 4 Months Before Birth	13.7%	-10.6%	10.3%	-8.4%
	(8.7)	(10.4)	(9.2)	(14.0)
3 to 1 Months Before Birth	3.9%	38.3% **	16.3%	38.2% **
	(9.2)	(16.1)	(13.1)	(17.1)
Sibling Sample	YES	YES	YES	YES
Sibling Fixed Effects	YES	YES	YES	YES
Municipality Fixed Effects	YES	YES	YES	YES
Pre/Post Gestation Hom Rates	YES	YES	YES	YES
State and YOB Fixed Effects	YES	NO	NO	NO
Mean of Dependent Variable	64.3%	63.1%	60.9%	60.1%
Observations	889	432	485	362
Number of Mothers	414	203	223	164

Notes:

***, **, * denote significance at the 1, 5, and 10% level, respectively. Homicide rates are per 10,000. Standard errors are clustered at the municipality level. Regressions additionally include controls for the gender of the child, maternal (age at birth, age at birth squared, years of education, employment status, earnings per month, and marital status) and household characteristics (size, per capita expenditure, and rural status), as well as, year of birth, month of birth, year of interview, month of interview, birth order, and survey wave fixed effects.

Table 12
Impact of Local Homicide Rate on Maternal Health Behaviors for Mothers of the Sibling Sample used in Table 8

Homicide Rate	Number of Cigarettes Smoked Per Week (1)	Number of Exercise Days Between Monday and Friday (2)	Amount of Exercise Time Per Day (Mins) (3)
1- 12 Months Before Interview	-0.15	0.06 *	1.35
	(0.14)	(0.04)	(1.41)
Individual Fixed Effects	YES	YES	YES
Municipality Fixed Effects	YES	YES	YES
Mean of Dependent Variable	0.5	0.35	9.21
Observations	768	768	768
Number of Mothers	384	384	384

Notes:
***, **, * denote significance at the 1, 5, and 10% level, respectively. Homicide rates are per 10,000.
Standard errors are clustered at the municipality level. Regressions additionally include controls for time-varying individual (age, years of education, marital status, employment status, earnings per month) and household characteristics (size, per capita expenditure, rural status),
as well as, year of interview and month of interview fixed effects.

Table A1
Respondent Migration Between MxFLS1 and MxFLS3 and Potential Future Homicide Rate Exposure
for Panel Women that Gave Birth Since MxFLS2 and before 2Q2009

	Interviewed in Different Municipality		Migrated for at Least 1 Year		Migrated for at Least 1 Month	
Homicide Rate	**(1)**	**(2)**	**(3)**	**(4)**	**(3)**	**(4)**
Change Between 2002 & 2009	1.3%		1.0%		0.5%	
	(0.82)		(0.79)		(0.87)	
MxFLS1 Characteristics Interacted with Homicide Rate Change Between 2002 & 2009:						
Age		0.1%		0.5%		-0.1%
		(0.37)		(0.80)		(0.88)
Age Squared		0.0%		0.0%		0.0%
		(0.01)		(0.02)		(0.02)
Lived in Rural Locality		1.2%		0.1%		0.0%
		(1.05)		(0.92)		(0.97)
Year of Education		0.1%		-0.2%		-0.3%
		(0.08)		(0.18)		(0.17)
Employed		-0.2% **		-0.1%		0.1%
		(0.10)		(0.21)		(0.22)
Earnings per Month (1,000 Pesos)		0.8%		0.4%		0.6%
		(0.56)		(0.64)		(0.59)
PCE of HH (10,000 Pesos)		0.3%		-0.8%		0.0%
		(2.47)		(3.17)		(3.27)
Married		0.0%		0.2%		0.2%
		(0.11)		(0.15)		(0.18)
Household Size		0.0%		-0.1%		0.0%
		(0.17)		(0.24)		(0.28)
Observations	1,392	1,392	1,358	1,358	1,358	1,358
Mean of Dependent Variable	8.6%	8.6%	22.4%	22.4%	24.9%	24.9%

Notes:
***, **, * denote significance at the 1, 5, and 10% level, respectively. Homicide rates are per 10,000. Standard errors are clustered at the municipality level
Regressions include controls for respondent's MxFLS1 education, age fixed effects, employment status, earnings per month, household size, marital status, household's per capita expenditure, and rural status. Additionally all regressions control for all municipality characteristic trends and levels shown in Table 2, as well as, year of interview, month of interview, and MxFLS1 state of residence fixed effects.

Table A2
Impact of Local Homicide Rate on Birth Weight
for All Births to Panel Members Since MxFLS2 and Before July 2009

Homicide Rate	**(1)**	**(2)**	**(3)**	**(4)**
9 to 7 Months Before Birth	-5.8	-306.1 ***	-401.3 ***	-538.9 ***
	(40.1)	(117.5)	(126.1)	(140.4)
6 to 4 Months Before Birth	-38.5	22.7	-36.3	-55.9
	(46.0)	(77.8)	(120.0)	(150.8)
3 to 1 Months Before Birth	28.2	-103.2	-9.6	131.9
	(39.7)	(85.4)	(104.3)	(135.6)
Sibling Sample	NO	YES	YES	YES
Sibling Fixed Effects	NO	YES	YES	YES
Municipality Fixed Effects	NO	NO	YES	YES
Pre/Post Gestation Hom Rates	NO	NO	NO	YES
State and YOB Fixed Effects	NO	NO	NO	NO
Mean of Dependent Variable	3,249.8	3,238.0	3,238.0	3,238.0
Observations	1,548	307	307	307
Number of Mothers		151	151	151

Notes:

***, **, * denote significance at the 1, 5, and 10% level, respectively. Homicide rates are per 10,000.

Standard errors are clustered at the municipality level. Regressions additionally include controls for the gender of the child, maternal (age at birth, age at birth squared, years of education, employment status, earnings per month, and marital status) and household characteristics (size, per capita expenditure, and rural status), as well as, year of birth, month of birth, year of interview, month of interview, birth order, and survey wave fixed effects.

Table A3
Impact of Local Homicide Rate on Birth Weight
for All Births to Panel Members Since MxFLS2

	All Births to Panel Members Since MxFLS2		
	Before 3Q2009	**Before 2Q2009**	**Before 2009**
Homicide Rate	**(1)**	**(2)**	**(3)**
9 to 7 Months Before Birth	-306.1 ***	-292.7 **	-366.3 ***
	(117.5)	(127.0)	(130.9)
6 to 4 Months Before Birth	22.7	1.9	-58.4
	(77.8)	(86.5)	(122.9)
3 to 1 Months Before Birth	-103.2	-123.2	-60.1
	(85.4)	(135.9)	(149.6)
Sibling Sample	YES	YES	YES
Sibling Fixed Effects	YES	YES	YES
Municipality Fixed Effects	NO	NO	NO
Pre/Post Gestation Hom Rates	NO	NO	NO
Mean of Dependent Variable	3,238.0	3,248.6	3,238.5
Observations	307	257	205
Number of Mothers	151	127	101

Notes:
***, **, * denote significance at the 1, 5, and 10% level, respectively. Homicide rates are per 10,000.
Standard errors are clustered at the municipality level. Regressions additionally include controls for the gender of
maternal (age at birth, age at birth squared, years of education, employment status, earnings per month,
and marital status) and household characteristics (size, per capita expenditure, and rural status), as well as,
year of birth, month of birth, year of interview, month of interview, birth order, and survey wave fixed effects.

Table A4
Impact of Local Homicide Rate on Birth Weight Stacking
for All Births to Panel Members Since MxFLS2 and Before July 2009, Adding Siblings Born Since MxFLS1

Homicide Rate	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
9 to 7 Months Before Birth	2.0%	4.6%	9.4%	12.3%	13.4%
	(2.9)	(5.9)	(8.7)	(7.9)	(10.4)
6 to 4 Months Before Birth	-0.8%	-6.0%	-5.4%	-10.5%	-4.3%
	(3.6)	(4.9)	(6.1)	(7.7)	(7.3)
3 to 1 Months Before Birth	3.2%	-0.8%	-1.9%	-2.4%	2.7%
	(2.7)	(6.6)	(11.0)	(8.3)	(9.0)
Sibling Sample	NO	YES	YES	YES	YES
Sibling Fixed Effects	NO	YES	YES	YES	YES
Municipality Fixed Effects	NO	NO	YES	YES	YES
Pre/Post Gestation Hom Rates	NO	NO	NO	YES	YES
State and YOB Fixed Effects	NO	NO	NO	NO	YES
Mean of Dependent Variable	23.7%	24.4%	24.4%	24.4%	24.4%
Observations	1,868	891	891	891	891
Number of Mothers		415	415	415	415

Notes:

***, **, * denote significance at the 1, 5, and 10% level, respectively. Homicide rates are per 10,000.

Birth weight stacking measured as having a birth weight that ended in .0 or .5 kilograms.

Standard errors are clustered at the municipality level. Regressions additionally include controls for the gender of the child, maternal (age at birth, age at birth squared, years of education, employment status, earnings per month, and marital status) and household characteristics (size, per capita expenditure, and rural status), as well as, year of birth, month of birth, year of interview, month of interview, birth order, and survey wave fixed effects.

Table A5
Impact of Local Homicide Rate on Birth Weight
for Births to Subgroups of Panel Members Since MxFLS2 and Before July 2009, Adding Siblings Born Since MxFLS1

	All	Top 50% PCE in MxFLS1	≥9 Yrs. Education in MxFLS1	≥9 Yrs. Education in MxFLS3
Homicide Rate	**(1)**	**(2)**	**(3)**	**(4)**
9 to 7 Months Before Birth	-226.9 **	-144.5	-73.2	-113.1
	(91.5)	(142.9)	(120.3)	(94.5)
6 to 4 Months Before Birth	38.9	61.0	-224.4	-101.9
	(69.7)	(95.0)	(278.5)	(121.7)
3 to 1 Months Before Birth	67.7	18.1	95.1	69.2
	(79.0)	(137.2)	(182.8)	(132.5)
Sibling Sample	YES	YES	YES	YES
Sibling Fixed Effects	YES	YES	YES	YES
Municipality Fixed Effects	YES	YES	YES	YES
Pre/Post Gestation Hom Rates	YES	YES	YES	YES
State and YOB Fixed Effects	NO	NO	NO	NO
Mean of Dependent Variable	3,249.4	3,266.9	3,291.5	3,271.6
Observations	891	441	404	516
Number of Mothers	415	203	191	245

Notes:
***, **, * denote significance at the 1, 5, and 10% level, respectively. Homicide rates are per 10,000. Standard errors are clustered at the municipality level. Regressions additionally include controls for the gender of the child, maternal (age at birth, age at birth squared, years of education, employment status, earnings per month, and marital status) and household characteristics (size, per capita expenditure, and rural status), as well as, year of birth, month of birth, year of interview, month of interview, birth order, and survey wave fixed effects.

Table A6
Impact of Local Homicide Rate on Birth Outcomes
for All Births to Panel Members Since MxFLS2 and Before July 2009, Adding Siblings Born Since MxFLS1
Additionally Controlling for Prenatal Care Visits and Prenatal Care Initiation in the First 2 Months of Pregnancy

Homicide Rate	Birth Weight (1)	LBW (<2,500g) (2)	LBW+ (≤2,500g) (3)
9 to 7 Months Before Birth	-268.2 **	8.3%	20.1% **
	(116.2)	(5.1)	(8.2)
6 to 4 Months Before Birth	109.8	-3.4%	-4.3%
	(85.9)	(3.6)	(5.3)
3 to 1 Months Before Birth	15.7	-0.3%	3.4%
	(84.8)	(4.1)	(7.4)
Sibling Sample	YES	YES	YES
Sibling Fixed Effects	YES	YES	YES
Municipality Fixed Effects	YES	YES	YES
Pre/Post Gestation Hom Rates	YES	YES	YES
State and YOB Fixed Effects	YES	YES	YES
Mean of Dependent Variable	3,249.4	7.1%	10.7%
Observations	891	891	891
Number of Mothers	415	415	415

Notes:
***, **, * denote significance at the 1, 5, and 10% level, respectively. Homicide rates are per 10,000.
Standard errors are clustered at the municipality level. Regressions additionally include controls for the gender of the child, maternal (age at birth, age at birth squared, years of education, employment status, earnings per month, and marital status) and household characteristics (size, per capita expenditure, and rural status), as well as, year of birth, month of birth, year of interview, month of interview, birth order, and survey wave fixed effects.