Violence while in Utero: The Impact of Assaults During Pregnancy on Birth Outcomes*

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Abstract

Determining the social costs of crime, particularly violent crime, plays a critical role in assessing the cost-effectiveness of policy interventions in the justice system. Current leading estimates rely on an assumption that the impacts of victimization are fully understood, yet causal evidence on the effects of crime on its victims is relatively sparse. This study presents novel quasi-experimental evidence on the effects of reported violent crime on pregnancy and infant health outcomes, using a unique source of linked administrative data from New York City. We merge birth records that contain information on maternal residential addresses to the locations of reported crimes, and focus on mothers who experience a reported assault in their homes. Our empirical strategy compares the outcomes of women who have a reported assault in their home during pregnancy to those who have one shortly after. We find consistent evidence that assault in the 3rd trimester significantly increases rates of very low birth weight (less than 1,500 grams) and very pre-term (less than 34 weeks gestation) births, possibly through higher likelihood of induced labor. We also find a reduction in the take-up of Special Supplemental Program for Women, Infants, and Children (WIC) benefits among women with an assault during pregnancy. We show that our results are robust to multiple choices of control groups and to using maternal fixed effects models. As infant health is a strong predictor of life-long well-being, our results raise important new evidence on the long-term and intergenerational social costs of violent crime.

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1 Introduction

Crime is considered a canonical example of a negative externality because of its large costs to society. Measuring these costs is imperative for assessing the cost-effectiveness of interventions that reduce crime, and informing the justice system more broadly. While a large literature in economics is devoted to understanding the determinants of criminal behavior (Becker, 1968; Erlich, 1973; Freeman, 1999; Chalfin and McCrary, 2015) and an active area of work examines the impacts of criminal sanctions on the offenders themselves (Agan and Starr, 2018; Bounanno and Raphael, 2013; Aizer and Doyle, 2015; Hawken and Kleiman, 2009; Mueller-Smith, 2015; Dobbie *et al.*, 2018), comparably less is known about the causal effects of crime on *victims*. Yet current leading approaches to estimating the social costs of crime rely on either jury award estimates (Miller *et al.*, 1996) or contingent valuation studies (Cohen *et al.*, 2004), which both assume that the impacts of victimization are fully understood.[1]

Estimating the causal effects of criminal victimization is challenging for at least two reasons. First, while credible administrative data on alleged offenders (with personally identifying information) is readily available through arrest and incarceration databases, corresponding victim identities are generally withheld due to confidentiality concerns.[2] Research on victims of crime is thus typically limited to self-reports in survey data, such as the National Crime Victimization Survey, which may be subject to non-random measurement error or recall bias (Ellsberg *et al.*, 2001). Social surveys also rely on increasingly selected samples of individuals willing to respond, with unknown consequences for data quality (Dillman *et al.*, 2014). Second, victimization—especially due to violent crime—is not a random event. For instance, poor women are much more likely to experience domestic violence than their more advantaged counterparts (Jewkes, 2002). There are also substantial differences in victimization rates across race and ethnicity (Lauritsen and White, 2001), and by mental health status (Desmarais *et al.*, 2014). Thus, it is difficult to isolate the causal effects of experiencing a violent crime from the influences of other (unobservable) factors.

This paper attempts to overcome these challenges to deliver new evidence on how violent crime affects the outcomes of some of the most vulnerable members of society—pregnant women and newborn children. We leverage a unique source of linked administrative data from New York City: birth records with information on maternal residential addresses merged to the exact locations and dates of reported crimes. To address the endogeneity concerns

[1]See Section 2.3 for a description of these methods.

[2]Arrests data are available from the Federal Bureau of Investigation (FBI) Uniform Crime Reporting program. Data on prisoners are available through the National Prisoner Statistics program at the Bureau of Justice Statistics.

associated with exposure to violent crime, we limit our primary analysis sample to mothers living in single-family homes in The Bronx, Brooklyn, and Queens, who have at least one reported assault at their residence. Our empirical strategy compares the outcomes of women who have a reported assault in their home in months 0 through 9 post-conception to those who have one in months 1 through 10 after the month of the estimated due date. We thus rely on an assumption that the exact timing of the assault affects infant health outcomes only through the assault itself. In support of this assumption, we present evidence that a wide range of maternal characteristics are not statistically different across our treatment and comparison groups. We further show that our results are robust to using an alternative control group of women who experience a reported assault in the 9 months before pregnancy, and to using maternal fixed effects models. Our approach is similar to that of Black *et al.* (2016) and Persson and Rossin-Slater (2018), who exploit the timing of deaths in the family to study the effects of *in utero* exposure to maternal bereavement on children's outcomes.

Our results show that assault during pregnancy reduces average birth weight by 51.8 grams. The effects are much larger at the lower tail of the birth weight distribution, with 66 and 39 percent increases in the shares of births that are very low birth weight (less than 1,500 grams) and very pre-term (less than 34 weeks gestation), respectively. We also document a 2.3 percentage point (50 percent) increase in the likelihood of a low 1-minute Apgar score.[3] The effects of assault are concentrated in the 3rd trimester, and appear to be driven by an increased likelihood of induced labor. Interestingly, we also find that while women who experience an assault during pregnancy have more interaction with healthcare providers prenatally (e.g., they have 0.3 more prenatal visits) than those who experience one postpartum, they are 4 percentage points (6 percent) less likely to receive Special Supplemental Program for Women, Infants, and Children (WIC) benefits. The decline in WIC take-up could be explained by a "chilling effect" on interaction with a government program because of New York's mandatory arrest law for domestic violence cases. In particular, police who are called for a potential domestic violence incident are required to arrest someone, and if there are multiple individuals present in the home, they can arrest all of them. Thus, pregnant women who call the police about experiencing an assault may place less trust in government programs if they end up getting arrested as a result.[4]

Since the majority of all violence against women is perpetrated by domestic partners (Tjaden and Thoennes, 2000), our paper relates to a broader literature on intimate partner

[3]The Apgar score is based on a doctor's observation of the baby's skin color, heart rate, reflexes, muscle tone, and breathing shortly after birth, and is reported on a 0-10 scale. Scores below 7 are considered low. See: `https://kidshealth.org/en/parents/apgar.html`.

[4]See `http://www.nytimes.com/2007/08/07/opinion/07iyengar.html` for a discussion of the unintended consequences of mandatory arrest laws.

violence (IPV). While economists have studied the determinants of IPV from the perspective of household bargaining models (Tauchen *et al.*, 1991; Lundberg and Pollak, 1993; Farmer and Tiefenthaler, 1997; Dee, 2003; Stevenson and Wolfers, 2006; Aizer, 2010), there is much less economic research on its causal impacts on the victims. Our analysis of effects of assaults during the fetal stage is particularly relevant in light of evidence that IPV can escalate during pregnancy (Cheng and Horon, 2010; Brownridge *et al.*, 2011), with estimates suggesting that between 16 and 23 percent of American women experience IPV while pregnant (Chambliss, 2008), and that IPV-related homicide is a leading cause of death among pregnant women (Palladino *et al.*, 2011). As pointed out by Newberger *et al.* (1992), violence during pregnancy can affect infant health through a direct physical channel resulting from blunt trauma to the maternal abdomen, which in turn can result in early onset of labor due to placental abruption, or other complications such as the rupture of the mother's uterus. There may also be indirect channels, including elevated stress, exacerbation of existing chronic illnesses, changes in access to prenatal care or other services, and engagement in adverse behaviors such as smoking or poor nutrition.

A number of studies have documented a negative correlation between prenatal IPV and pregnancy and birth outcomes (Newberger *et al.*, 1992; Cokkinides *et al.*, 1999; Murphy *et al.*, 2001; Campbell, 2002; Valladares *et al.*, 2002; Coker *et al.*, 2004; Silverman *et al.*, 2006; Sarkar, 2008). To the best of our knowledge, only one prior study has used a quasi-experimental method to identify the impacts of IPV on infant health: Aizer (2011) uses linked hospitalizations and births data from California to estimate the effect of hospitalization for assault during pregnancy with a control function approach (Heckman, 1979) based on geographic and time variation in the enforcement of laws against domestic violence.[5] She finds that hospitalization for assault during pregnancy is associated with a 163 gram reduction in birth weight, with the largest impacts for assaults in the 1st trimester of pregnancy. We build on this path-breaking research in three primary ways: First, we examine a different set of assaults by including all assaults reported to the police instead of focusing on those resulting in hospitalization. Second, our research design does not rely on policy or enforcement-related variation at the aggregate level (which could potentially impact infant health through channels other than direct victimization). Third, in addition to birth outcomes, we also study effects on pregnancy-related behaviors, such as WIC benefit take-up.

We further contribute to a literature on the relationship between violence—either due to criminal activity or more global events such as wars and terrorist attacks—and infant health, which examines neighborhood or community-level exposure (Berkowitz *et al.*, 2003;

[5] Specifically, Aizer (2011) uses the ratio of arrests for domestic violence to the number of 911 calls to the police reporting domestic violence in the previous year, which varies across counties and over time.

Lederman *et al.*, 2004; Lauderdale, 2006; Messer *et al.*, 2006; Eskenazi *et al.*, 2007; Masi *et al.*, 2007; Camacho, 2008; Metcalfe *et al.*, 2011; Mansour and Rees, 2012; Brown, 2013; Torche and Villarreal, 2014; Torche and Shwed, 2015). Since these studies do not measure actual victimization, they typically argue that maternal stress during pregnancy is the main channel by which exposure to violence can affect infant health. We instead deliver new estimates that can speak to the direct consequences of violent crime on the victims and their unborn children. Indeed, our results are consistent with a direct physical channel by which birth outcomes are impacted—mothers who are assaulted in the 3rd trimester of pregnancy are more likely to need to have their labor induced prematurely, and consequently deliver babies with very low birth weights.

Our findings, combined with prior research on the lasting consequences of early-life health on adult health, human capital, and labor market outcomes (Almond *et al.*, 2018; Aizer and Currie, 2014; Currie and Almond, 2011; Currie, 2011; Barker, 1990), provide important new evidence about the large and intergenerational social costs of violent crime. Moreover, since poor pregnant women are much more likely to experience an assault than their more advantaged counterparts (Aizer, 2011), our research points to an additional mechanism by which early-life health disparities perpetuate persistent economic inequality in adulthood and across generations.

The rest of the paper unfolds as follows. Section 2 provides background information on assaults against women, police responses in New York City, and current approaches to estimating the social cost of crime. Section 3 describes our administrative data sources, while Section 4 discusses our empirical approach. Section 5 presents our results. Finally, Section 6 offers some conclusions.

2 Background

2.1 Intimate Partner Violence in the United States and New York City

Recent estimates from the National Intimate Partner and Sexual Violence Survey (NIPSVS) by the Centers for Disease Control and Prevention indicate that 32 percent of U.S. women experience physical intimate partner violence (IPV) at some point in their lifetimes (Smith *et al.*, 2017). This number represents an increase from a mid-1990s estimate from the National Violence Against Women Survey, which reported that 22 percent of women experienced IPV (Tjaden and Thoennes, 2000). There is also substantial heterogeneity in lifetime IPV prevalence rates across states, ranging from 25 to 42 percent. Point-in-time estimates from the National Crime Victimization Survey indicate there have been between 600,000 and 1,100,000 IPV events per year over the last decade (Bureau of Justice Statistics).

Moreover, violence originating from an intimate partner accounts for over one seventh of all violent crime. Indeed, violent crime in the U.S. most commonly occurs between two individuals with a known relationship (see Panel A of Figure 1). Survey estimates also suggest that less than half of all violent crime is ever reported to police (see Panel B of Figure 1).

In New York City—the setting for our paper—survey evidence shows that about 69,000 adult women feared IPV in 2004-2005 (New York City Department of Health and Mental Hygiene, 2008). Administrative records additionally indicate that women between the ages of 20 to 29 are at greatest risk of severe IPV, whether measured as female IPV-related homicide, female IPV-related hospitalizations, or female IPV-related emergency department visits (New York City Department of Health and Mental Hygiene, 2008).[6] Black and Hispanic women, as well as those living in low-income neighborhoods, are at heightened risk.[7]

Studies further show that pregnancy elevates the risk of IPV. Reported prevalence rates of physical or sexual abuse among pregnant women range between 7 and 23 percent (Helton and Snodgrass, 1987; Amaro *et al.*, 1990; McFarlane *et al.*, 1992; Johnson *et al.*, 2003; Chambliss, 2008), with more recent studies documenting relatively higher rates.

2.2 Police Responses to Domestic Violence in New York City

Since 1994, New York state law requires that police investigate all reports of domestic violence. In 2017, the New York City Police Department (NYPD) responded to almost 200,000 domestic assault incidents, with over half including an intimate partner (New York Police Department, 2017). Fourteen percent of all felony-level complaints included a domestic incident, making it one of the most common complaints to the NYPD.[8]

State law classifies domestic violence into three distinct categories depending on the severity of the offense. Felony domestic assault requires that a crime resulted in serious bodily injury (e.g., a broken bone) or involved a weapon that led to substantial prolonged pain or physical impairment. Misdemeanor offenses are crimes that result in substantial pain or impairment of physical condition, but not over a sustained period. Violations, also

[6]The second age group at greatest risk was women aged 30-39.

[7]Black and hispanic women have a 150% to 770% higher risk of severe IPV relative to non-Hispanic white women, depending on the specific measure.

[8]There are a number of other resources available to domestic violence victims in New York City as well. A 24-hour domestic violence hotline can connect victims with support programs. In addition, victims can receive free and confidential assistance at any of the five NYC Family Justice Centers, which are located in each NYC borough. These include case management services, psychological counseling, income and work support programs, and legal assistance. The city is regularly engaging in new initiatives to strengthen its strategy to discourage domestic violence and support victims (NYC Mayor's Office to Combat Domestic Violence, 2018).

known as petty offenses, include verbal threats and physical acts that do not result in injury.[9] Convicted offenders face increasingly severe sanctions as the gravity of the crime increases.

The NYPD has over 400 domestic violent prevention officers, investigators, and supervisors (New York Police Department, 2018). Prevention officers receive additional training to confront the potentially unpredictable situations associated with domestic violence. Additionally, New York state has had a "mandatory arrest" law since the passage of the Family Protection and Domestic Violence Intervention Act in 1994. This law implies that police officers must make an arrest when there is probable cause of either a felony or a misdemeanor offense committed by one "member of the same family or household" against another. Members of the same family include spouses, former spouses, individuals who have a child together, individuals who are related by blood, and individuals who are either in or were previously in an intimate relationship together.[10]

Importantly, based on the NYC Confidentiality Policy (Bloomberg, 2003a,b), undocumented immigrants who are victims of crime (including IPV) should not be subject to questions regarding immigration status by NYPD officers. The goal is to ensure that victims come forward, regardless of their immigration status, to help identify their offenders and receive support. As such, we do not expect there to be under-reporting of violence against immigrants in our data.[11]

2.3 Estimating the Social Costs of Crime

Scholars have sought to quantify the cost of crime to society for a long time, dating back to at least the Wickersham Commission on Law Observance and Enforcement (Anderson *et al.*, 1931). There are a variety of factors to consider, including (but not limited to) property loss or destruction,[12] administrative costs of running the justice system, victims' mental and physical health, and victims' potential lost productivity.[13] Determining how to quantify these impacts with a common unit of measurement (monetary costs) is not trivial, but is crucial for using cost-effectiveness to base policy decisions.

Researchers have developed a variety of strategies ranging in the degree of sophistication to meet this need (Cohen, 2005). A number of efforts have been made in the tradition of a "cost of illness" analysis (Hodgson and Meiners, 1982; Malzberg, 1950), wherein tangible

[9]Our analysis pools felony and misdemeanor assaults together. We do not examine violations in order to focus on instances when injury was sustained by the mother.

[10]See `http://www.opdv.ny.gov/help/fss/policecourts.html` for more details.

[11]Most relevant to our research design, we do not see any statistically significant differences in the shares of mothers who are foreign-born between mothers who have a reported assault during pregnancy and those who have one in the postpartum period. See Table 6 and the discussion in Section 4 below.

[12]Whether to consider property theft a social loss or transfer remains an open debate in the field.

[13]See Table 9B.2 in Donohue (2009) for an extensive discussion of potential costs of crime.

impacts of crime on specific outcomes are quantified using the best available information and assigned prices (McCollister *et al.*, 2010). These approaches typically rely on self-reported information from victims about the costs they faced after victimization. Closely related is the jury-award approach (Miller *et al.*, 1996), which assesses the total social cost of criminal events based on reviewing actual compensation awards provided in civil personal injury cases.

Other work uses hedonic pricing to estimate the social costs of crime (Thaler, 1978). In this approach, the cost of crime is estimated using capitalization of local crime rates in housing prices, generating a measure of both tangible and intangible costs. Contingent valuation studies utilize a similar logic. Surveys ask respondents about their willingness to pay to avoid various criminal acts, which theoretically can also provide a measure of tangible and intangible costs (Cohen *et al.*, 2004; Cook and Ludwig, 2000).[14]

All these methods share an assumption that the impacts of crime are known and unbiased. If crime has an unknown impact on society (either to the researcher, a jury, a homebuyer, or a survey respondent), then estimates of potential social costs will be biased towards zero. At the same time, if impact estimates are based on flawed priors (especially possible given the paucity of causal evidence on victimization), cost estimates could be further under- or overestimated.

Table 1 reports commonly used upper and lower bound estimates on the costs of several major crime types. Assaults are found to generate between approximately $16,000 and $90,000 in social costs per victim. While there is quite a range, existing estimates consistently indicate that violent crime leads to social costs substantially above all other types of offenses. As a consequence, small changes to violent crime rates can be influential in cost-effectiveness analyses. Benefit-cost calculations have become standard in analyses of interventions that influence criminal activity. It is therefore highly important to generate estimates of the costs of violent crime that meet two criteria: first, that they accurately reflect causal effects, and second, that they fully account for the full range of potential impacts.

Our analysis aims to inform cost estimates of violent crime by generating new evidence on the causal impacts of assault on pregnancy and infant health outcomes, which are typically not incorporated into existing calculations. We discuss our estimates of costs associated with these effects further in Section 6.

[14]Often cost strategies are complemented with estimates from the statistical value of life literature, which relies on a compensating wage differential framework to assess the impacts of severe violent crimes (Viscusi and ALDY, 2003).

3 Data

We merge three restricted administrative data sets from New York City for our analysis: the universe of birth records, the universe of reported crimes (between 2004 and 2012), and a building characteristics database.

Crime data. The crime data come from administrative records from the New York Police Department (NYPD). The data cover all criminal complaints reported to the NYPD between 2004 and 2012.[15] Each record has information on the exact spatial longitude and latitude coordinates of where the event allegedly occurred, the date and time of the offense, the degree of the offense and a categorical description of the nature of the offense. The incidents do not necessarily mean that a criminal charge, much less a conviction, was brought in the case; instead, these represent the full universe of reported crimes in New York City over the study period.[16]

Table 2 demonstrates that close to one-fifth of all reported crimes in New York City between 2004 and 2012 were violent in nature. This category includes assaults, aggravated assaults, murder, manslaughter and robbery. Property crimes account for an additional third of the crime reports, mainly reflecting larceny, grand larceny, and burglary. The remaining categories are predominantly comprised of drug offenses, criminal mischief, and harassment.

Figure 2 shows the trends in violent crimes in New York City over the study period. Misdemeanor and felony assaults, which represent the majority of violent offenses and are the focus of this study, remain stable over the study period at close to 110,000 combined offenses per year. There is a notable decline in robberies over the study period, particularly in 2009, but we do not focus on robberies in the current study.

Births data. The births data come from administrative records held by the New York City Department of Health and Mental Hygiene's Office of Vital Statistics. The data contain detailed information about the child and the parents.[17] We observe a variety of birth outcomes, including child sex, birth order, plurality, birth weight in grams, gestation length in weeks, the Apgar score measured at 1, 5, and 10 minutes after birth, an indicator for

[15] Due to privacy concerns, sexual assault crimes were withheld from this database. Administrative records from the NYPD (New York Police Department, 2017) indicate that less than 0.2% of domestic assault incidents included a complaint of rape.

[16] While there may be some false complaints contained in these records, it is advantageous to see the uncensored set of criminal events, particularly if concerns about victim cooperation may lead a non-trivial share of these cases to not proceed further through the criminal justice system.

[17] The data come from two sources: medical data about the child, pregnancy, and delivery are recorded by the hospital of delivery, while information about maternal behaviors are self-reported by the mother in a questionnaire that she completes while in the hospital.

any abnormal conditions of the newborn, an indicator for any congenital anomalies, an indicator for whether the child was transferred to the Neonatal Intensive Care Unit (NICU) after birth, and an indicator for whether the child has died by the time the birth certificate is filed. We also have information about the delivery, including whether the birth occurred via cesarean section, whether labor was induced, and any complications of labor or delivery. Further, we have data on maternal behaviors during pregnancy and at childbirth, including the date of prenatal care initiation and the total number of prenatal care visits, the number of times the mother was hospitalized during pregnancy, whether the mother worked during pregnancy, whether the mother received WIC benefits, whether the mother smoked before or during pregnancy (and the average number of cigarettes per day), whether the mother used any illicit drugs during pregnancy, maternal pregnancy weight gain, whether the mother self-reports being depressed during pregnancy, and whether the child is breastfed or formula-fed at the time of birth certificate filing.[18]

Lastly, the data contain rich information about the mothers, including age, education level, marital status, race/ethnicity, nativity, and whether the mother has any pregnancy risk factors (such as diabetes, hypertension, preeclampsia, eclampsia, and whether any previous child was born pre-term, low-birth-weight, or small-for-gestational-age). We also have more limited information about the fathers, which we use as a proxy for father involvement at the time of childbirth: we create an indicator for whether the father information is missing from the birth certificate. Importantly, the data contain the mother's exact (self-reported) residential address and full maiden names and dates of birth, which allow us to match mothers to crimes occurring in their homes, and also to match siblings to the same mother, as we discuss below.

We calculate the estimated month and year of conception for each birth using information on the birth month and year and gestation length, and limit the data to conception years 2004 to 2012.

Building characteristics data. Our building characteristics file, the Primary Land Use Tax Lot Output (PLUTO) data, comes from the NYC Department of City Planning (NYC DCP). Our PLUTO data contains information on the tax lot and building characteristics (type of dwelling, number of floors, estimated value, etc.), as well as on geographic, political, and administrative districts as of 2009. Each property is uniquely identified by the Bureau,

[18] The birth certificate format changed in 2008, which is during our sample time frame. The following of the above listed variables are only available in the 2008+ data: the number of times the mother was hospitalized during pregnancy and whether the mother was depressed during pregnancy. The question about depression is asked on a 5-point scale, with possible answers being: 1= not depressed at all; 2= a little depressed; 3= moderately depressed; 4= very depressed and did not get help; 5= very depressed and got help. Our indicator for depression during pregnancy includes all mothers with answers 2 through 5.

Block, Lot (BBL) tax identifier, an identifier that is unique to New York City.

The data, for example, allows us to distinguish between single-family homes and large multi-unit apartment buildings. This file also contains information on the year 2000 census tract.

Data Merge. The first step in our data merge is attaching a unique locational identifier to each birth record that documents where the mother lived during her pregnancy. This draws on the mother's self-reported residential address from the birth certificate, but is standardized in the form of the BBL. We rely on a program known as "Geosupport" (specifically `NYCgbat.exe`), published by New York City Department of City Planning (NYC DCP), which is a customized "fuzzy matching" algorithm designed specifically for common matching challenges in New York City.[19] `NYCgbat.exe` reads in the recorded street address along with the borough of residence and returns the BBL on file at NYC DCP for the address. Once the BBLs are identified, they are then merged back onto the original birth records data.

The crime data, which in its raw form is geographically identified by latitude and longitude coordinates, is mapped onto BBLs using ArcGIS. Our BBL shapefile is published by NYC DCP, and allows us to calculate the minimum distance between a given crime and the surrounding BBLS. Crimes are assigned to the nearest BBL.[20]

The crime and births data are linked using the common BBL identifier, yielding a dataset that combines mothers with crimes that occurred at their building of residence. The PLUTO dataset, which already contained BBL identifiers, is also merged in at this stage.

Including the information from PLUTO is critical for qualifying exactly what "exposure" might mean in the linked data. Since our crime data is effectively recorded at the building level and not exact apartment number level (e.g., we cannot distinguish whether an assault happened in the mother's apartment or in another one down the hall), the PLUTO information allows us focus the analysis on locations where exposure is more likely to be directly linked to the mother's home (e.g., single-family homes).

Measurement Error. Our primary explanatory variables are likely to be measured with error, which could bias our estimates of effects of violent assaults during pregnancy on infant health and prenatal behaviors toward zero. In particular, our measure of assault exposure

[19] The issue at hand is that there are potentially many different spellings for the same street name or address, which need to be harmonized into one single ID. Specific boroughs (Brooklyn, Bronx, Manhattan, Queens, and Staten Island) have specific nuances in address formats, which is taken into account by Geosupport.

[20] We use a minimum distance measure to account for the fact that some crime reports are geocoded in the street in front of a building or residence, which would otherwise not be mapped to a BBL identifier.

could capture another household member, who is not the pregnant woman or new mother, being victimized. If we use data on women who reside in buildings other than single-family homes, we face the additional possibility that another residential unit at the address is affected. This is unfortunately unavoidable in our context because victim information is withheld in crime data.

To determine the degree of bias, we compare our counts of the total number of pregnant women impacted by assaults with NYC-specific estimates from the Pregnancy Risk Assessment Monitoring System (PRAMS). PRAMS data between 2004 and 2012 suggests that a total of 28,593 pregnant mothers suffered some form of physical abuse during pregnancy. Since the PRAMS survey includes offenses that are never reported to law enforcement, we scale this number down by 0.42, which is the average reporting rate between 2004 and 2012 in the NCVS for violent offenses from a known offender. We thus obtain an estimate of 12,009 *reported* episodes of abuse among pregnant mothers from PRAMS. Any counts that exceed this number in our records would suggest measurement error in our explanatory variable of interest.

Table 3 shows a variety of different scaling factors to account for measurement error depending on different assumptions. The raw geographic data merge (i.e., not limiting to our primary analysis sample as described below) suggests that in 25 percent of all births in our data, women resided in a building with a misdemeanor or felony assault during their pregnancy. An additional 25 percent of births had a woman who resided in a building with a harassment claim which would include physical altercations that did not result in serious injury (e.g., a slap, a push, etc).[21]

The very high exposure rate is driven by large apartment buildings with many units. These figures dramatically overstate the actual likelihood of exposure to assaults.[22] To account for this issue, we scale down the exposure counts by $\frac{1}{Residential\,Units\,in\,Building}$. This applies a strong assumption that each unit has an equal likelihood of victimization. The resulting prevalence rates of 0.011 (0.011) for felony and misdemeanor assaults (harassment) appear much closer to the consensus estimates in the literature.

The unit-adjusted estimates using only felony and misdemeanor assaults, put our count within 250 cases of the PRAMS estimate. Because there are types of physical altercations only covered in the harassment complaints though, we also report the larger 23,517 domestic violence incidents count based on all types of physical altercation. Assuming that the PRAMS estimate is accurate, the broader definition would imply that roughly half of our

[21]The PRAMS data does not differentiate by degree of abuse. Because harassment charges also include non-physical offenses, an accurate administrative count of exposure to physical violence likely lies somewhere between these two methods.

[22]It is for this specific reason that we focus on single family homes in our main analysis.

observations have an explanatory variable measured with error. An easy rule of thumb for scaling our estimates to account for measurement error would be to multiply our estimated coefficients by two.

Analysis Sample and Summary Statistics. After limiting our sample to births with conception years 2004 to 2012, we make the following restrictions. First, we focus on mothers who reside in single-family homes, since for them, we can be most sure that the reported assault actually occurred at their home. Second, we only consider mothers residing in The Bronx, Brooklyn, or Queens, leaving us with 68,399 observations. We drop mothers in Manhattan since there are very few who reside in single-family homes, and we drop mothers in Staten Island because they are less comparable with mothers in the other boroughs in terms of their demographic and socio-economic characteristics.[23] Lastly, we create our primary analysis sample by focusing only on women who have a reported (misdemeanor or felony) assault at their home in the month of conception or in the following 9 months (treatment group), or in months 10 through 19 post-conception (i.e., the months following the expected due date month).[24] These restrictions leave us with a sample of 1,941 births.

Table 4 presents selected mean maternal characteristics for three sub-groups of mothers residing in single-family homes in the Bronx, Brooklyn, and Queens over our analysis time frame. Column (1) uses all observations where the mother did not experience an assault at her home in months 0-19 post-conception (i.e., mothers who are neither in our treatment nor control group). Column (2) uses our treatment group observations, while column (3) uses our control group observations. Comparing column (1) to the other two columns makes it clear that exposure to assault is not random. Women who have an assault during or shortly after pregnancy are younger, less likely to be married, more likely to have the father's information missing from the birth certificate, more likely to be non-Hispanic black or Hispanic, have lower education, and are less likely to work during pregnancy than their counterparts without an assault during this time period. However, when we zoom in on mothers who experience an assault either during pregnancy or postpartum in columns (2) and (3), the differences become much less pronounced. We examine these differences in more detail in the next section.

[23] Additionally, we find some evidence of non-random selection into assault during pregnancy in Staten Island: women who experience an assault during pregnancy are more likely to be foreign-born and have lower education levels than those who have an assault after pregnancy. We do not find any evidence of such selection in The Bronx, Brooklyn, or Queens.

[24] We drop the 199 observations where a mother has an assault in her home *both* during months 0-9 post-conception and months 10-19 post-conception, since these cannot be clearly assigned to either the treatment or control group. Additionally, since in some of our robustness analyses we also include women with an assault in their home in the 9 months before conception, we analogously drop the 134 observations where a mother has an assault *both* during months 1-10 before conception and months 0-9 post-conception.

Table 5 examines which types of mothers are most likely to experience an assault during pregnancy in a slightly different way. We report the share of mothers with an assault in the home during months 0-9 post-conception within the sub-group defined in the left column. Mothers who are most likely to have an assault during pregnancy are young (less than 20 years old), have the father's information missing from the birth certificate, non-Hispanic black, and have less than a high school education. These patterns highlight the importance of using a quasi-experimental research design to separate the causal impacts of assaults during pregnancy from the influences of these other characteristics.

4 Empirical Design

Our goal is to estimate a causal relationship between exposure to an assault during pregnancy and infant health. Consider a stylized model of the form:

$$y_i = \gamma AssaultPreg_i + \mathbf{x}_i'\chi + u_i \tag{1}$$

for each mother-child pair i. y_i is an outcome of interest such as an indicator for very low birth weight, $AssaultPreg_i$ is an indicator that is equal to 1 for mothers who have a reported assault in their homes during pregnancy and 0 otherwise, $\mathbf{x}_i$ is a vector of observable determinants of y_i, and u_i is a vector of unobservable characteristics. Since assaults during pregnancy are not randomly assigned (see Tables 4 and 5), unobservable components in u_i are likely to be correlated with the treatment variable, leading to biased estimates of γ in equation (1).

Our empirical strategy aims to overcome this issue by generating a control group that enables us to approximate a randomized design to the best of our ability. We argue that women who experience an assault in their homes in a short time period *after* pregnancy serve as an appropriate control group to those who have one during pregnancy. In particular, consider a sample of women who either experience an assault during pregnancy or shortly after childbirth:

$$S = \{i : \mathbf{1}[c \leq Assault \leq b]_i = 1 \,|\, \mathbf{1}[b < Assault \leq b + w]_i = 1\},$$

where c denotes the month of conception, b denotes the month of childbirth, and w denotes a time window after childbirth (in months), so that $\mathbf{1}[c \leq Assault \leq b]_i = 1$ indicates that the assault occurred during pregnancy (including the month of birth), and $\mathbf{1}[b < Assault \leq b + w]_i = 1$ indicates that it occurred in the w months after the child's birth month, respectively.

For all $i \in \{S\}$, suppose we estimate:

$$y_i = \sigma \mathbf{1}[c \leq Assault \leq b]_i + \mathbf{x}_i' \eta + \epsilon_i, \tag{2}$$

Model (2) would represent a causal relationship between *in utero* exposure to assault and infant health if, for all $i \in \{S\}$, $E(\mathbf{1}[c \leq Assault \leq b]_i \epsilon_i) = 0$. However, as we show below, a central finding of our analysis is that assault during pregnancy reduces average gestation length by inducing very pre-term births. Thus, since the treatment variable in equation (2) is defined based on the actual month of childbirth, b, there is a violation of the excludability restriction. A related issue is that there is a mechanical correlation between the duration of pregnancy and the likelihood that an assault occurs during it.[25]

We address these concerns by redefining our treatment variable relative to the *expected* rather than actual month of birth. Specifically, define the expected month of birth: $e_b = c+9$, i.e., 9 months after the month of conception. Unlike the actual month of birth, the expected month of birth is pre-determined relative to the date of the assault.

Now, consider the sample:

$$S' = \{i : \mathbf{1}[c \leq Assault \leq e_b]_i = 1 \,|\, \mathbf{1}[e_b < Assault \leq e_b + 10]_i = 1\},$$

Rather than estimating equation (2), we estimate the following equation on the sample with $i \in \{S'\}$:

$$y_{iymr} = \beta_0 + \beta_1 \mathbf{1}[c \leq Assault \leq e_b]_{iymr} + \psi_y + \phi_m + \rho_r + \mathbf{x'}_i \boldsymbol{\beta_2} + \nu_{iymr}, \tag{3}$$

where $\mathbf{1}[c \leq Assault \leq e_b]_{iymr}$ is an indicator variable that takes the value of 1 if the assault occurs in or before the estimated month of birth (at full term), and 0 otherwise. We include conception year and month fixed effects, ψ_y and ϕ_m, respectively, as well as fixed effects for the three boroughs in our analysis, ρ_r. The vector $\mathbf{x}_i$ includes the following control variables: maternal age group dummies (<20, 20-24, 25-34, 35+, missing), indicator for the mother being married, indicator for the father's information being missing from the birth certificate, indicator for the mother being foreign-born, maternal race/ethnicity dummies (non-Hispanic white, Hispanic, non-Hispanic black, other, missing), maternal education dummies (less than high school and no diploma, high school or GED diploma, some college or associate's degree, bachelor's degree or more, missing), indicator for singleton birth, and parity dummies (1st, 2nd, 3rd+, missing). The key coefficient of interest, β_1, represents an estimate of the impact

[25] See Currie and Rossin-Slater (2013), Black *et al.* (2016), and Persson and Rossin-Slater (2018) for detailed discussions of these issues.

of exposure to an assault during pregnancy.

Identifying assumption. Our analysis relies on the assumption that the timing of assault within a 10-month bandwidth surrounding the expected month of birth is exogenous to our outcomes of interest. Put differently, we require that mothers in our treatment and control groups are not systematically different in a way that is correlated with infant health. While this assumption is inherently untestable, we present several indirect tests to examine its plausibility.

While columns (2) and (3) of Table 4 already demonstrated that mothers in our treatment and control groups are similar in terms of their observable characteristics, Table 6 presents a more formal examination. Specifically, we estimate model (3), using each of the background characteristics as a dependent variable, and omitting the vector $\mathbf{x}_i$. We report the estimates of β_1 from these regressions; out of 12 coefficients in Table 6, only one is marginally significant at the 10% level. We find that mothers in the treatment group are about half of a year older than mothers in the control group, a difference that is unlikely to drive our main effects on infant health.

One plausible unobservable (to us) difference between the treatment and control groups relates to the prevalence of assault that does not lead to involvement of the police. As we wrote in Section 2, most instances of violent crime are not reported to the police, and thus it is possible that women who experience an assault that we can detect in our crime data, had prior exposure to violence from a domestic partner. This would imply that our control group—which consists of women with a reported assault in their homes in the postpartum period—may have also experienced an (unreported) assault during pregnancy. Consequently, by comparing our treatment and control groups, we would be under-estimating the true magnitude of the effect of violent assault during pregnancy. We attempt to address this issue by testing the robustness of our results to incorporating women who experience an assault in the 9 months *before* conception into the control group in Section 5 below. In other words, we estimate equation (3) on an alternative sample:[26]

$$S^{''} = \{i : \mathbf{1}[c{-}10 \leq Assault < c]_i = 1 \,|\, \mathbf{1}[c \leq Assault \leq e_b]_i = 1 \,|\, \mathbf{1}[e_b < Assault \leq e_b + 10]_i = 1\}$$

We also try a difference-in-differences type model, where we compare the difference between mothers who experience an assault during pregnancy and those who have one in the months after relative to the analogous difference for mothers who experience any other type of crime during those two time periods (see Section 5 for details).

[26]We do not use women with an assault before pregnancy in our primary specification because conception and childbirth following violent assault pre-pregnancy is likely endogenous.

Lastly, we leverage the maternal identifiers in our birth records data to link siblings to the same mother, and use a maternal fixed effects model. Using a sample of all singleton sibling births by mothers who reside in single-family homes in the Bronx, Brooklyn, or Queens during the first pregnancy, we estimate:[27]

$$y_{iymk} = \kappa_0 + \kappa_1 \mathbf{1}[c \leq Assault \leq e_b]_{iymk} + \zeta_y + \delta_m + \sigma_k + \mathbf{x'}_i \kappa_{\mathbf{2}} + \mu_{iymk} \tag{4}$$

for each child i, conceived in year y and month m, born to mother k. σ_k is a maternal fixed effect, while the vector $\mathbf{x}_i$ now only includes characteristics that vary within each mother: maternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for mother being married, maternal education dummies (less than high school and no diploma, high school or GED diploma, some college or associate's degree, bachelor's degree or more, missing), parity dummies (1st, 2nd, 3rd+, missing), and birth interval dummies (1st birth, < 12 months from previous birth, 12-24 months from previous birth, 24-36 months from previous birth, 36-48 months from previous birth, 48+ months from previous birth). The key coefficient of interest, κ_1, is identified off the 451 children of 201 mothers who have at least one pregnancy exposed to an assault, and one unexposed pregnancy.[28] We cluster standard errors on the mother. As we show below, the results are remarkably robust to these changes in estimation technique.

5 Results

Descriptive evidence. Tables A.1 through A.4 report estimates from ordinary least squares (OLS) models that examine the correlations between experiencing assault during pregnancy and a range of maternal background characteristics, birth and delivery outcomes, and behaviors during pregnancy. Here, we include all births with conception years 2004 to 2012 and with mothers residing in single-family homes in The Bronx, Brooklyn, and Queens, including those where no assault (or any other crime) occurred at any point during our sample period.

We see that violent victimization during pregnancy is associated with adverse birth and delivery outcomes. The clear negative selection on maternal characteristics (Table A.1) and prenatal behaviors (Table A.4) raise doubt, however, as to whether the estimates can be interpreted as causal.

[27] We only condition on residence in the Bronx, Brooklyn, or Queens during the first pregnancy since subsequent mobility may be endogenous.

[28] We also include children of mothers who never have an assault during pregnancy (18,107 observations) and children of mothers who have an assault during every pregnancy (42 observations) to increase power in identifying coefficients on the other variables in the regression model.

Birth and delivery outcomes. Table 7 presents our main results from estimating equation (3) using the sample with $i \in \{S'\}$ (defined in Section 4), using the following outcomes: birth weight (in grams), an indicator for low birth weight (<2,500 grams), an indicator for very low birth weight (<1,500 grams), an indicator for high birth weight (>4,000 grams), gestation length (in weeks), an indicator for a pre-term birth (<37 weeks gestation), an indicator for a very pre-term birth (<34 weeks gestation), and indicators for low 1-minute and 5-minute Apgar scores (<7). Our estimates indicate that exposure to a violent assault during pregnancy causes a deterioration in newborn health.

We find a marginally significant 51.8 gram reduction in average birth weight, which represents a 1.6 percent decline at the sample mean. However, the impacts seem to be especially concentrated at the lower end of the birth weight distribution. We estimate that the share of births with very low birth weight increases by 1.7 percentage points, or 66.4 percent at the sample mean. The likelihood of high birth weight also falls by 1.7 percentage points, which represents a 34.6 percent decline at the sample mean.[29] We also find a reduction in average gestation length of about one quarter of a week, driven by a 1.7 percentage point increase in very pre-term births (39.4 percent effect at the sample mean). Apgar scores are also negatively impacted; the likelihood of a low 1-minute Apgar score increases by 2.3 percentage points, or 49.6 percent at the sample mean.[30]

We examine additional delivery-related and post-birth outcomes in Table 8: indicators for a birth by cesarean section, induction of labor, any complications of labor or delivery (e.g., premature ruptures of membranes), any abnormal conditions of the newborn (e.g., use of assisted ventilation or surfactant), admission to the NICU, any congenital anomalies, breastfeeding initiation, male sex, and death of the infant by the time the birth certificate is filed.[31] We do not find any statistically significant impacts on any of these outcomes, except for induction of labor. We estimate the assault during pregnancy leads to a 5.5 percentage point increase in the likelihood of labor being induced, a 25.5 percent rise at the sample mean.

[29] High birth weight (defined as more than 4,000 grams) is regarded as a negative health outcome, which is correlated with a greater incidence of obesity and other adverse conditions like diabetes in later life (see, e.g.: Cnattingius *et al.*, 2012). Thus, the reduced likelihood of a high-birth-weight birth can be seen as a small beneficial effect of prenatal exposure to assault. However, the substantial costs associated with increases in adverse outcomes at the lower ends of the birth weight and gestation length distributions likely outweigh any benefits arising from reductions in high-birth-weight births. See Section 6 for a more detailed discussion about the costs of very low birth weight.

[30] Accounting for measurement error in our explanatory variable would suggest doubling the estimated coefficients indicating an even larger impact on birth outcomes.

[31] We follow the literature by examining the child's sex as a signal of changes to miscarriage rates (see, e.g., Sanders and Stoecker, 2015; Halla and Zweimüller, 2013). Since male fetuses are more likely to miscarry, a reduction in male births may indicate an increase in miscarriages.

Mechanisms. We attempt to better understand the patterns of effects on birth and delivery outcomes by exploring differences in impacts across various periods of exposure in Figures 3 through 7. For these analyses, we include all mothers with an assault in the window from 10 months before conception month to 19 months after conception month (i.e., all $i \in \{S''\}$ defined in Section 4 above). The figures show the coefficients and the corresponding 90% and 95% confidence intervals from event-study models that include separate indicators for any assault occurring during the following periods: 8-10 months before conception month ("-3 Pre"), 5-7 months before conception month ("-2 Pre"), 1-4 months before conception month ("-1 Pre"), months 0-2 post-conception ("1 Tri"), months 3-5 post-conception ("2 Tri"), months 6-9 post-conception ("3 Tri"), months 13-15 post-conception ("2 Post"), and months 16-19 post-conception ("3 Post"). The omitted cateogry is months 10-12 post-conception (i.e., the 3 months after the expected month of delivery).

We document that the impacts on very low birth weight, very pre-term, low 1-minute Apgar score births, as well as induction of labor, are all driven by assault in the 3rd trimester of pregnancy. These results are suggestive of a direct physical mechanism driving our effects: pregnant victims of assault may be likely to go to the hospital because of the resulting physical trauma, where they need to have their labor induced prematurely and therefore deliver very pre-term and very-low-birth-weight babies. Our findings are less consistent with indirect channels (e.g., stress) driving our impacts on birth outcomes, which would arguably also materialize through exposure in earlier parts of the pregnancy (as in Aizer, 2011).

Table 9 examines mechanisms further by estimating model (3) using observable maternal pregnancy-related behaviors as outcomes. We consider: an indicator for first trimester prenatal care initiation and the total number of prenatal care visits, as well as indicators for whether the mother was hospitalized during pregnancy (not including hospitalization for childbirth), received any WIC benefits, smoked during pregnancy, used illicit drugs during pregnancy, was depressed during pregnancy, had too low pregnancy weight gain (<15 lbs), or had too high pregnancy weight gain (>40 lbs).[32]

Our results indicate that women with a reported assault during pregnancy are 5 percentage points more likely to initiate prenatal care in the 1st trimester and have 0.3 more prenatal care visits than their counterparts with a reported assault in the postpartum period. Figure 8 shows that the impact on the number of visits appears driven by exposure

[32] Medical recommendations for pregnancy weight gain depend on the woman's pre-pregnancy BMI. However, our births data only contain information on maternal pre-pregnancy BMI starting in 2008. In order to study pregnancy weight gain for the whole sample, we use the 15 and 40 lbs thresholds, since overweight women are advised not to gain less than 15 lbs, while underweight women are advised not to gain more than 40 lbs. See `https://www.acog.org/Clinical-Guidance-and-Publications/Committee-Opinions/Committee-on-Obstetric-Practice/Weight-Gain-During-Pregnancy`.

in the 2nd trimester, possibly consistent with a direct effect of violence requiring additional prenatal care check-ups (e.g., due to resulting bruises or injuries) that do not necessarily lead to an induction of labor (as we show for assault in the 3rd trimester above). These findings further suggest that women who are assaulted during pregnancy may engage in compensatory behaviors, implying that our impacts on birth outcomes are lower bounds.

We also find a statistically significant negative effect on the likelihood of WIC receipt of 3.8 percentage points, or 5.8 percent at the sample mean. The decline in WIC take-up could arise for a variety of reasons, which we unfortunately cannot observe. One possibility is related to the fact that perpetrators of IPV tend to engage in controlling behaviors that limit the choices of their victims.[33] Women who are abused during pregnancy may fear going to a government program office (e.g., WIC) because of the possible reactions by their abusers. It is possible that WIC staff may report suspicion of domestic abuse to law enforcement, triggering a mandatory investigation.[34] It is also possible that the effect on WIC is due to New York's mandatory arrest law for domestic violence cases, where police are required to arrest at least one person if they respond to a domestic violence incident. If the nature of the incident is unclear, then the police may arrest all individuals in the home, including the pregnant woman, who may consequently place less trust in government programs.

Robustness checks. To address concerns about possible differences in unreported assault rates across our treatment and control groups discussed in Section 4, we test the sensitivity of our results to including women who have an assault in their home during months 1 through 10 *before* the conception month in Appendix Tables A.5, A.6, and A.7. Although the selection issues are arguably different across the two control groups of women with assaults before and after pregnancy, our results on birth outcomes, induction of labor, prenatal care, and WIC take-up are remarkably similar to those reported in our main tables.

Additionally, if there any differences between women who experience an assault during pregnancy and those who experience one in the postpartum period that are related to aggregate trends in crime, we can account for them by using mothers with any other crime in their home either during or after pregnancy as an additional control group in a difference-in-difference style model. In particular, we use a sample of all women with any crime in months 0-19 post-conception, and augment equation (3) by including separate indicators for

[33]For more discussion of the role of control in IPV, see, for example: `http://www.opdv.ny.gov/professionals/abusers/coercivecontrol.html`.

[34]Although by New York State Law there is no mandatory reporting of adult domestic violence by social services workers, staff may choose to report certain suspicion of domestic violence. In addition, all injuries resulting from discharge of a firearm, and all potentially life-threatening injuries inflicted by a knife or other sharp object, and serious burns must be reported to the local officials. Similarly, suspicion of child abuse or maltreatment are required to be reported to child protective services.

assault during months 0-9 post-conception, assault during months 0-19 post-conception (i.e., either during or after pregnancy), and any other crime during months 0-9 post-conception. The omitted category is thus women with any other crime in months 10-19 post-conception. Appendix Tables A.8, A.9, and A.10 present the results, which are again quite similar to those from our primary specifications.

Finally, to limit the possibility that unobservable differences between the treatment and control groups are driving our results, we estimate a maternal fixed effects model on a sample of siblings. These analyses compare across siblings born to the same mother, thus accounting for any time-invariant differences across mothers who do and do not experience an assault during pregnancy. While we lose some power—there are only 451 children of 201 mothers who have at least one pregnancy exposed to an assault, and one unexposed pregnancy—the patterns in Appendix Tables A.11, A.12, and A.13 are generally consistent with our main results. We find increases in adverse birth outcomes (including the likelihood of very pre-term births and low 1-minute Apgar scores, as well as NICU admission), and a large reduction in WIC take-up. We do not see a statistically significant impact on the induction of labor, however, suggesting that the mechanisms driving results on infant health in the sibling sample may be slightly different than those in our main analysis.

6 Conclusion

Measuring the social costs of crime—and especially violent crime—is crucial for informing policy debates regarding the judicial system and programs that impact criminal behavior more broadly. Implicit in all approaches that estimate these costs is the assumption that the costs of victimization are fully captured. However, causal evidence on the effects of violent crime on victims is sparse due to substantial data constraints and endogeneity in exposure. In this paper, we break new ground by using linked administrative data from New York City to deliver new quasi-experimental estimates of the effects of violent assaults on an important segment of the population, pregnant women and newborn children.

Our research design leverages birth records data on children of mothers living in single-family homes in The Bronx, Brooklyn, and Queens, who have at least one reported assault at their residence, as reported in administrative crime data. We compare the birth and pregnancy outcomes of women who have a reported assault in their home in months 0 through 9 post-conception to those who have an assault in months 1 through 10 after the month of the estimated due date. We find that assault during pregnancy leads to a 51.8 gram reduction in average birth weight, as well as large and significant increases in the rates of very low birth weight, very pre-term, and low Apgar score births of 66, 39, and 50 percent, respectively. The effects appear driven by assaults in the 3rd trimester, for which we also observe an rise in the likelihood of induced labor. We further show that women with assaults during pregnancy are less likely to take-up WIC benefits than women with an assault in the postpartum period.

Our quasi-experimental results are remarkably similar to the observed descriptive relationship between violent victimization and birth/delivery outcomes, in spite of the documented strong negative selection among victims. We do, however, observe strikingly different impacts on maternal behaviors during pregnancy when using the quasi-experimental approach. The descriptive results that use all women without an assault in the comparison group document that assault during pregnancy is associated with an increase in WIC participation, smoking, and depression; our preferred estimates that use women with an assault in the postpartum period as the comparison group instead indicate a significant decline in WIC participation and no impact on smoking or depression. We further find increases in the number of prenatal care visits, which may indicate a compensatory behavioral response to minimize the damage of assaults among victims. If women were unable to access such support, the negative consequences on birth outcomes could be larger.

To shed light on the issue of measuring the social costs of crime, we can use our estimates to conduct back-of-the-envelope calculations. We rely on two well-known estimates in the literature for this exercise. First, Almond *et al.* (2005) find that a one standard deviation increase in birth weight is associated with a 0.08 standard deviation reduction in hospital costs (where the standard deviation in hospital costs reported in their data is $39,000 in year 2000 dollars). Second, Black *et al.* (2007) document that a 10 percent increase in birth weight leads to 1 percent increase in annual earnings in adulthood. Under the assumption of linear effects of birth weight, our 51.8 gram reduction in average birth weight (see Table 7) due to an assault during pregnancy translates into an increase in hospital costs of $361.49 and a loss in the present value of lifetime earnings of about $833.20 (both numbers reported in 2018 dollars).[35] Added together, these represent a non-trivial share of current lower bound estimates on the costs of assaults, especially after accounting for measurement error issues in our explanatory variables, which would suggest multiplying by a factor of 1.96.

To the extent that our findings provide new evidence that has not previously been incorporated into the literature on the social costs of crime, our estimates indicate that the

[35] We arrive at these numbers as follows: The 51.8 gram reduction represents 0.08 of a standard deviation in our sample (which is 622.6) and a 1.6 percent effect at the sample mean. So, for hospital costs, we calculate $0.08 \times 0.08 = 0.0064$ SD reduction in hospital costs, or $0.08\times$ $39,000, or $250 in 2000 dollars, or $362 in 2018 dollars. With regard to lifetime earnings, the 1.6 percent effect means a 0.16 percent increase in earnings. We use data from the March 2017 Current Population Survey to obtain mean wage earnings by age, and then calculate the mean present value of lifetime earnings at age zero using a 3 percent real discount rate, which amounts to $520,753. 0.16 percent of $520,753 is $833.2.

cost of assaults to pregnant women needs to be scaled up by 15 percent for lower bound jury award estimates and by 2.6 percent for upper bound contingent valuation estimates.

It is essential to highlight, however, that the approximate $2,342 in costs associated with prenatal exposure to an assault is likely an *extreme* lower bound. Our impacts are most pronounced for the lower end of the birth weight distribution, where healthcare costs are arguably non-linear. Our calculation also does not account for costs associated with elevated rates of disability, chronic disease, and special education needs among children who have very low birth weights or are born extremely prematurely (Hack *et al.*, 2002, 2005). We also do not capture all the costs associated with lower cognitive ability among children with lower birth weights when compared to their higher-birth-weight counterparts (Figlio *et al.*, 2014), or any latent effects of prenatal exposure to assault on life-long health and development that do not materialize through impacts on birth outcomes (Barker, 1990). Further, we do not capture any costs associated with the mother's health or well-being. Lastly, and importantly, we cannot estimate the costs associated with violent crimes during pregnancy that are not reported to the police.

Our results imply that interventions that can reduce violence against pregnant women can have meaningful consequences not just for the women (and their partners), but also for the next generation. Future research may explore longer-term consequences of prenatal exposure to assaults on child health and development, as well as on maternal well-being.

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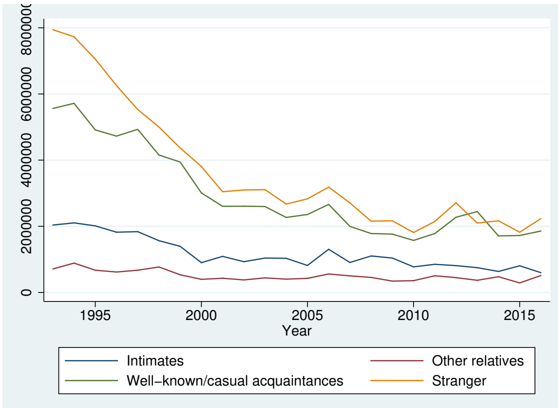
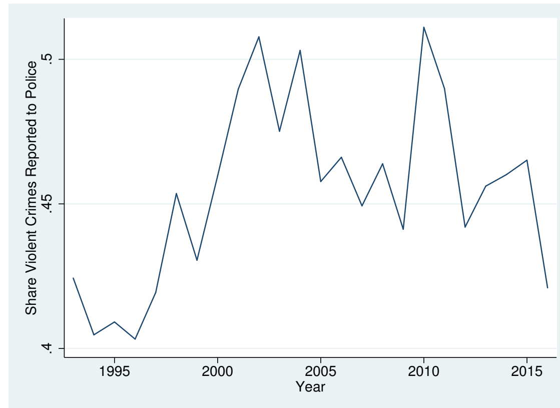
Figure 1: Trends on Violent Crimes in the National Criminal Victimization Survey

Panel A



Victim-Offender Relationship for Violent Crime

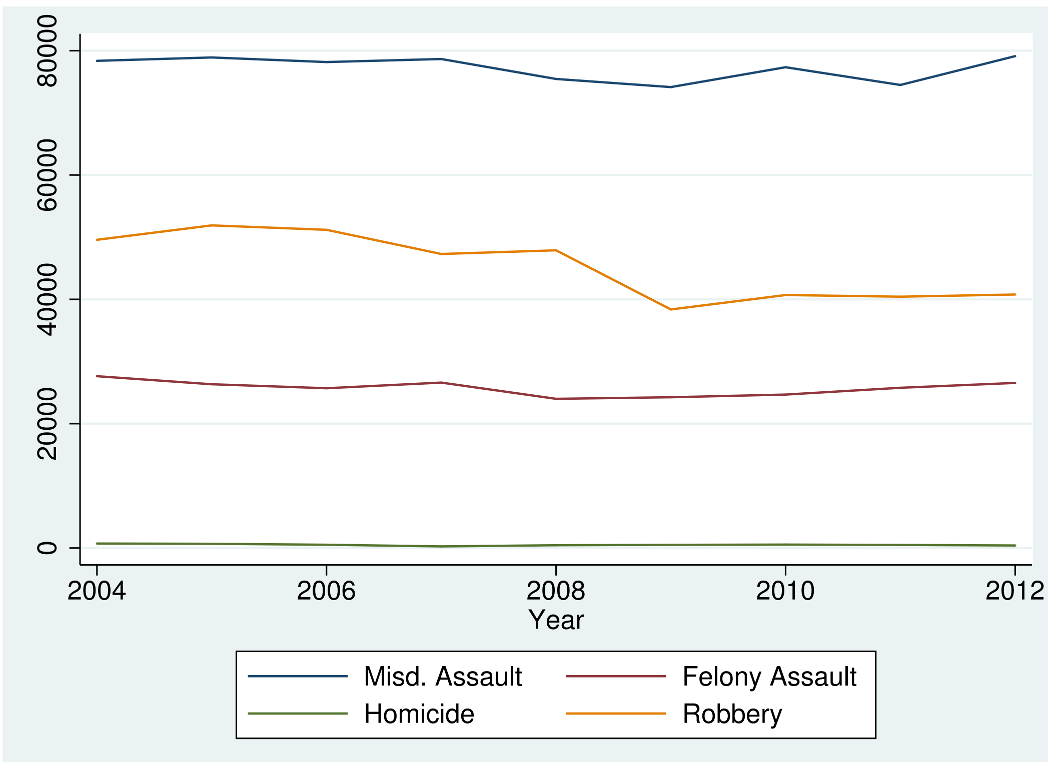
Panel B



Violent Crime Reporting Rate

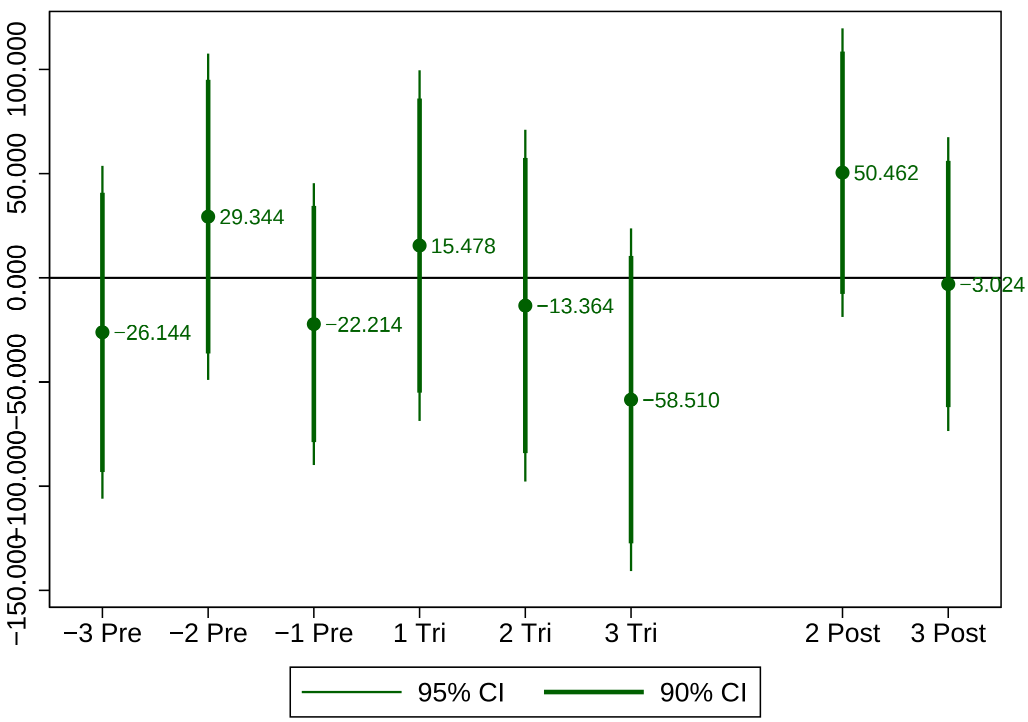
Notes: Authors calculations based on the Bureau of Justice Statistics National Crime Victimization Survey (1993-2016).

Figure 2: New York Violent Crime Trends (2004-2012)



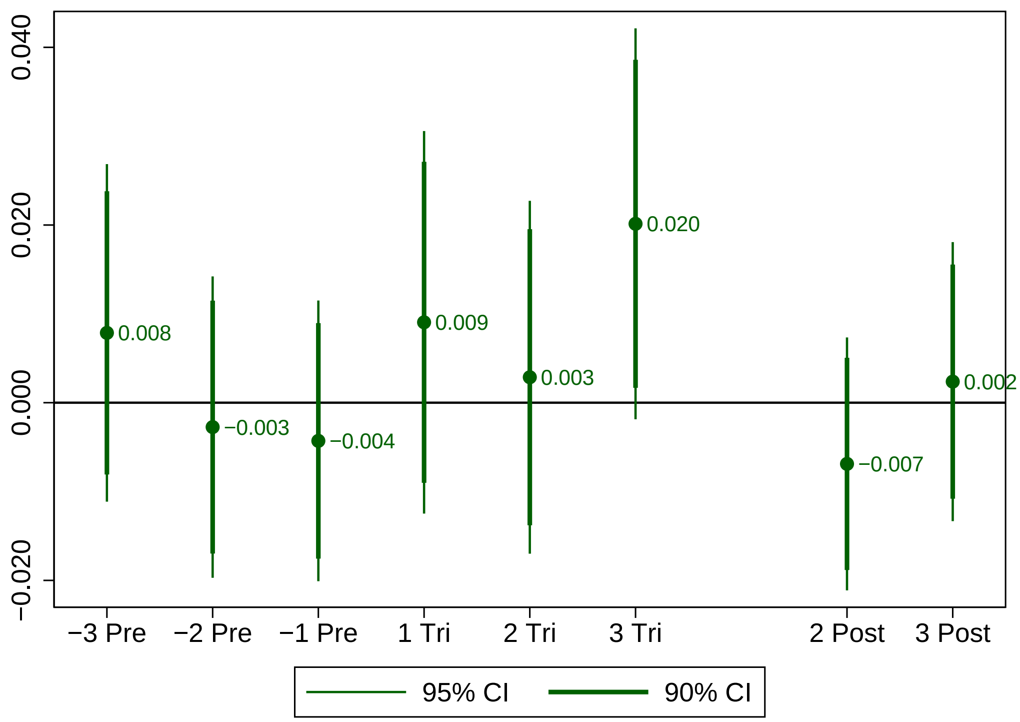
Notes: Authors' calculations based on administrative records from the New York Police Department.

Figure 3: Event Study: Birth Weight



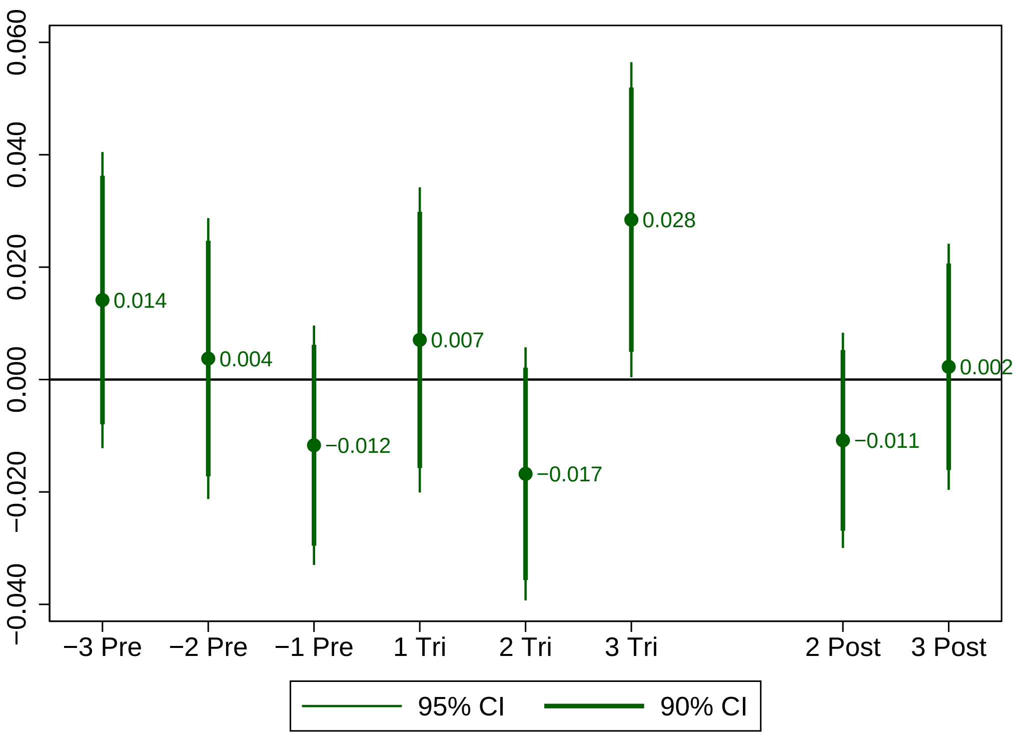
Notes: See notes under Table 7 for a description of the sample and control variables. This figure shows the coefficients and the corresponding 90 and 95% confidence intervals from event-study models that include indicators for any assault during the following windows: 8-10 months before conception month ("-3 Pre"), 5-7 months before conception month ("-2 Pre"), 1-4 months before conception month ("-1 Pre"), months 0-2 post-conception ("1 Tri"), months 3-5 post-conception ("2 Tri"), months 6-9 post-conception ("3 Tri"), months 13-15 post-conception ("2 Post"), and months 16-19 post-conception ("3 Post"). The omitted cateogry is months 10-12 post-conception (i.e., the 3 months after the expected month of delivery).

Figure 4: Event Study: Very Low Birth Weight



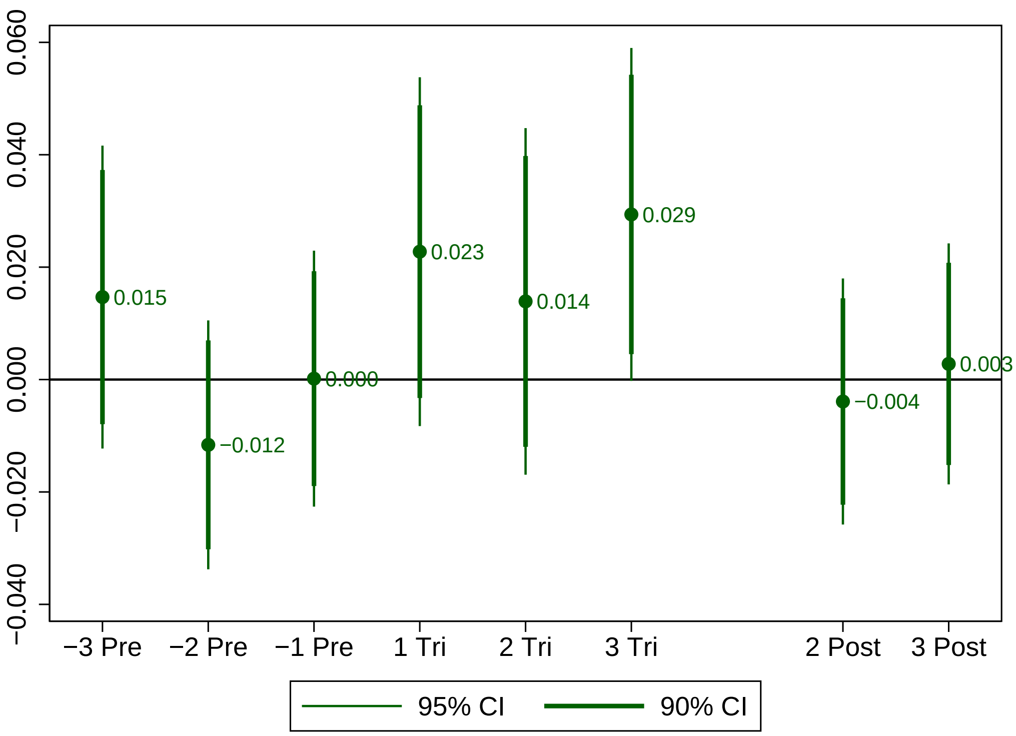
Notes: See notes under Table 7 for a description of the sample and control variables. This figure shows the coefficients and the corresponding 90 and 95% confidence intervals from event-study models that include indicators for any assault during the following windows: 8-10 months before conception month ("-3 Pre"), 5-7 months before conception month ("-2 Pre"), 1-4 months before conception month ("-1 Pre"), months 0-2 post-conception ("1 Tri"), months 3-5 post-conception ("2 Tri"), months 6-9 post-conception ("3 Tri"), months 13-15 post-conception ("2 Post"), and months 16-19 post-conception ("3 Post"). The omitted cateogry is months 10-12 post-conception (i.e., the 3 months after the expected month of delivery).

Figure 5: Event Study: Very Pre-Term



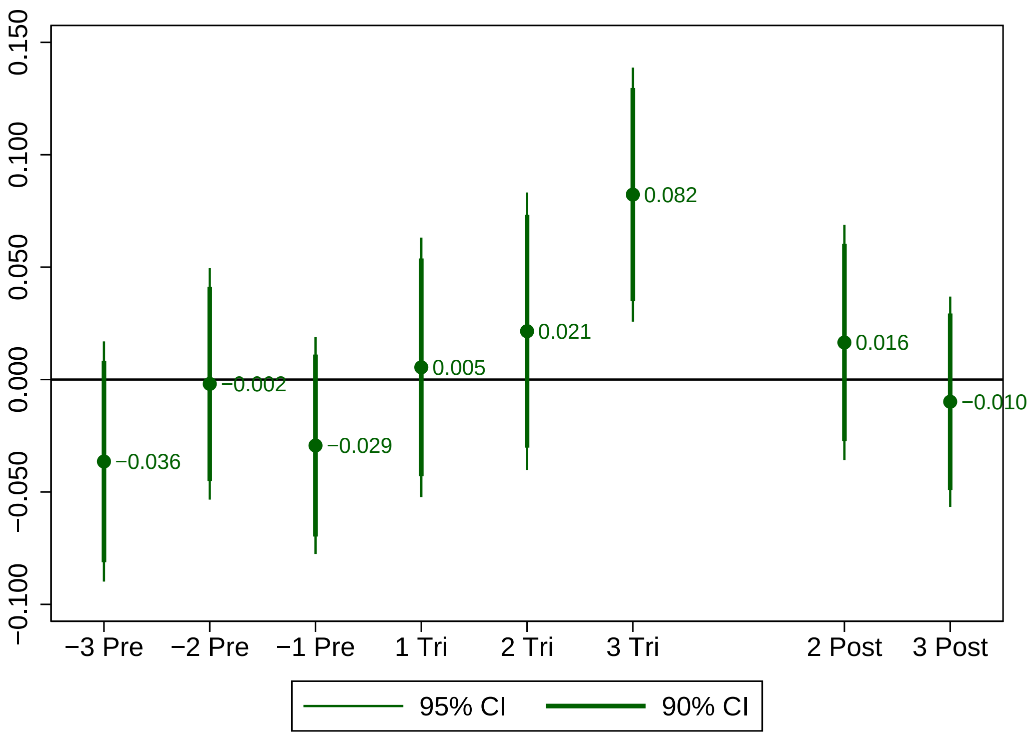
Notes: See notes under Table 7 for a description of the sample and control variables. This figure shows the coefficients and the corresponding 90 and 95% confidence intervals from event-study models that include indicators for any assault during the following windows: 8-10 months before conception month ("-3 Pre"), 5-7 months before conception month ("-2 Pre"), 1-4 months before conception month ("-1 Pre"), months 0-2 post-conception ("1 Tri"), months 3-5 post-conception ("2 Tri"), months 6-9 post-conception ("3 Tri"), months 13-15 post-conception ("2 Post"), and months 16-19 post-conception ("3 Post"). The omitted cateogry is months 10-12 post-conception (i.e., the 3 months after the expected month of delivery).

Figure 6: Event Study: Low 1-Min Apgar Score



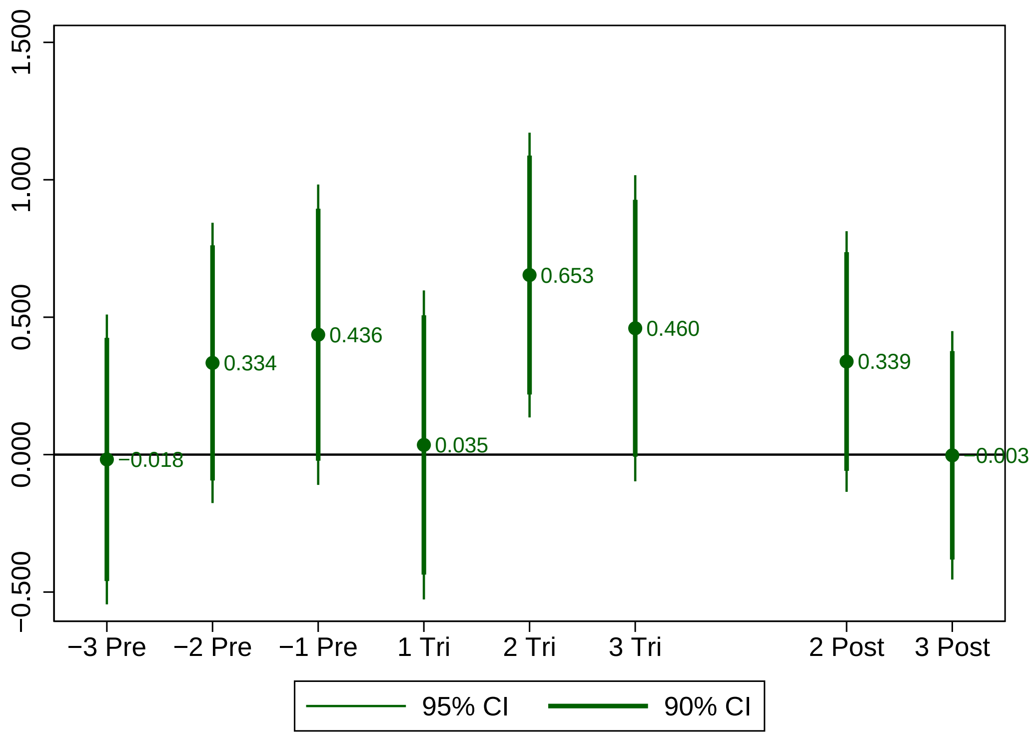
Notes: See notes under Table 7 for a description of the sample and control variables. This figure shows the coefficients and the corresponding 90 and 95% confidence intervals from event-study models that include indicators for any assault during the following windows: 8-10 months before conception month ("-3 Pre"), 5-7 months before conception month ("-2 Pre"), 1-4 months before conception month ("-1 Pre"), months 0-2 post-conception ("1 Tri"), months 3-5 post-conception ("2 Tri"), months 6-9 post-conception ("3 Tri"), months 13-15 post-conception ("2 Post"), and months 16-19 post-conception ("3 Post"). The omitted cateogry is months 10-12 post-conception (i.e., the 3 months after the expected month of delivery).

Figure 7: Event Study: Induction of Labor



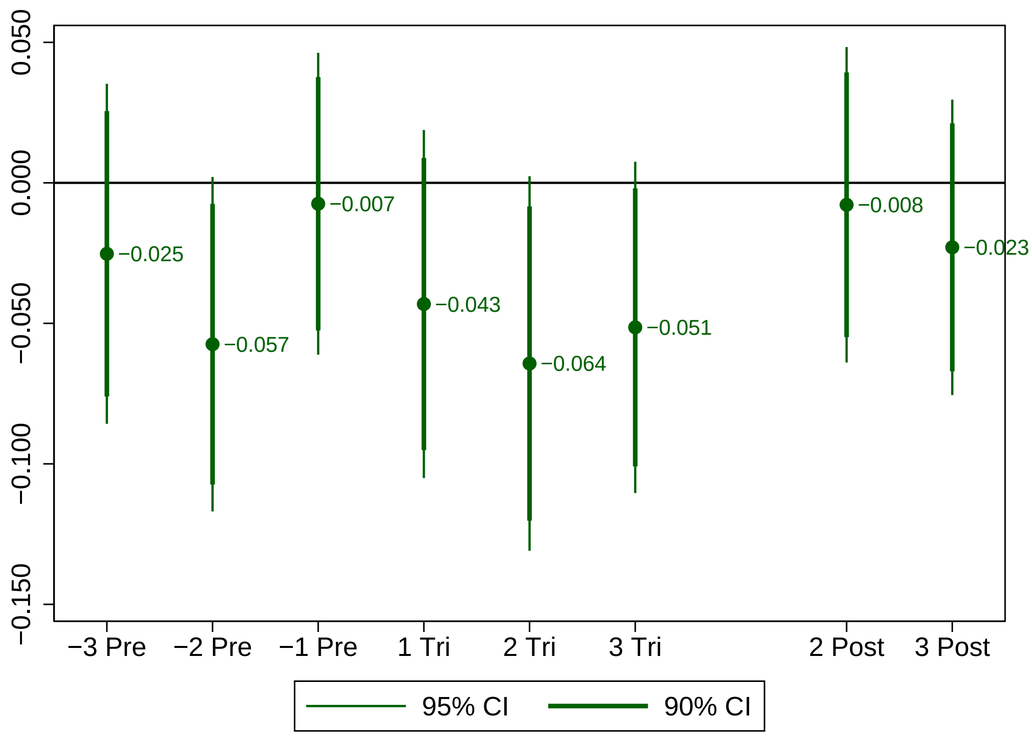
Notes: See notes under Table 7 for a description of the sample and control variables. This figure shows the coefficients and the corresponding 90 and 95% confidence intervals from event-study models that include indicators for any assault during the following windows: 8-10 months before conception month ("-3 Pre"), 5-7 months before conception month ("-2 Pre"), 1-4 months before conception month ("-1 Pre"), months 0-2 post-conception ("1 Tri"), months 3-5 post-conception ("2 Tri"), months 6-9 post-conception ("3 Tri"), months 13-15 post-conception ("2 Post"), and months 16-19 post-conception ("3 Post"). The omitted cateogry is months 10-12 post-conception (i.e., the 3 months after the expected month of delivery).

Figure 8: Event Study: Number Prenatal Visits



Notes: See notes under Table 7 for a description of the sample and control variables. This figure shows the coefficients and the corresponding 90 and 95% confidence intervals from event-study models that include indicators for any assault during the following windows: 8-10 months before conception month ("-3 Pre"), 5-7 months before conception month ("-2 Pre"), 1-4 months before conception month ("-1 Pre"), months 0-2 post-conception ("1 Tri"), months 3-5 post-conception ("2 Tri"), months 6-9 post-conception ("3 Tri"), months 13-15 post-conception ("2 Post"), and months 16-19 post-conception ("3 Post"). The omitted cateogry is months 10-12 post-conception (i.e., the 3 months after the expected month of delivery).

Figure 9: Event Study: WIC Receipt



Notes: See notes under Table 7 for a description of the sample and control variables. This figure shows the coefficients and the corresponding 90 and 95% confidence intervals from event-study models that include indicators for any assault during the following windows: 8-10 months before conception month ("-3 Pre"), 5-7 months before conception month ("-2 Pre"), 1-4 months before conception month ("-1 Pre"), months 0-2 post-conception ("1 Tri"), months 3-5 post-conception ("2 Tri"), months 6-9 post-conception ("3 Tri"), months 13-15 post-conception ("2 Post"), and months 16-19 post-conception ("3 Post"). The omitted cateogry is months 10-12 post-conception (i.e., the 3 months after the expected month of delivery).

Table 1: Common estimates on the social costs of crime

	Cohen, Miller and Wiersema (1996)	Cohen, Rust, Steen and Tidd (2004)
Murder	$4,980,360	$12,569,260
Rape	$147,378	$307,105
Robbery	$13,552	$300,626
Assault	$15,924	$90,706
Burglary	$2,372	$32,395
Motor Vehicle Theft	$6,268	*
Larceny	$627	*
Study Design	Jury Award	Contingent Valuation

Notes: Estimates have been converted to 2017 dollars. *Estimates not calculated in original article.

Table 2: Total NYPD Criminal Reports by Crime Type and Offense Level (2004-2012)

	Offense Level			
Crime Type	Felony	Misdemeanor	Violation	Total
Drug	128,248	552,351	1	680,600
Other	330,978	1,627,416	762,730	2,721,124
Property	1,175,072	1,132,586	0	2,307,658
Violent	644,117	694,638	0	1,338,755
All Types	2,278,415	4,006,991	762,731	7,048,137

Notes: Authors calculations based on administrative records from the New York Police Department.

Table 3: Assessing measurement error in the merged data

Total Affected Pregnancies	267,241	534,482	11,759	23,517
Share mismeasured relative to PRAMS baseline	0.96	0.98	-0.02	0.49
Implied Scaling Factor for Estimates	22.25	44.51	0.98	1.96
Types of Crimes included:				
Felony Assaults	×	×	×	×
Misdemeanor Assaults	×	×	×	×
Criminal Harassment		×		×
Reweighted according to # Residential Units in Building			×	×

Notes: Authors calculations based on administrative records from the New York City Department of Hygiene and Mental Health, the New York Police Department, and the New York City Department of City Planning. To determine the mismeasurement rate and implied scaling factor, we count all reports of physical abuse during pregnancy from the PRAMS data between 2004-2012 (28,593) scaled by the average violent crime reporting rate for known offenders (42%), which gives us a baseline target of 12,009 domestic violence episodes.

Table 4: Maternal Characteristics by Any Assault During/Post Pregnancy

	(1) No Assault	(2) Assault-Preg	(3) Assault-Post
Mother's Age	29.79	26.98	26.48
Mother Married	0.650	0.349	0.329
Father Info Missing	0.0714	0.136	0.135
Mother Non-Hispanic White	0.308	0.0998	0.113
Mother Hispanic	0.166	0.265	0.258
Mother Non-Hispanic Black	0.290	0.491	0.487
Mother Non-Hispanic Asian	0.212	0.118	0.101
Mother Foreign-Born	0.534	0.525	0.499
Mother's Education Less than HS	0.122	0.281	0.275
Mother's Education HS	0.249	0.305	0.295
Mother's Education Some College	0.273	0.273	0.275
Mother's Education College or More	0.355	0.135	0.151
Mother Worked During Pregnancy	0.506	0.383	0.376
Observations	66,458	872	1,069

Notes: Sample is limited to births by mothers who reside in single-family homes in the Bronx, Brooklyn, and Manhattan with conception years 2004-2012. Column (1) presents mean maternal characteristics for observations where the mother did not experience an assault in either the 10 months post conception month or 10 months post expected delivery months. Column (2) presents mean maternal characteristics for observations where the mother experienced any assault at her home during 10 months post conception month. Column (3) presents mean maternal characteristics for observations where the mother experienced any assault at her home during 10 months post expected delivery month, respectively.

Table 5: Shares of Mothers with Any Assault During Pregnancy Across Different Subgroups

Maternal Characteristic	Share with Any Assault During Pregnancy
Mother's Age <20	0.034
Mother's Age 35+	0.006
Mother is Married	0.006
Father's Info Missing	0.025
Mother is Non-Hispanic White	0.004
Mother is Hispanic	0.018
Mother is Non-Hispanic Black	0.021
Mother is Non-Hispanic Asian	0.007
Mother is Foreign-Born	0.012
Mother's Education Less than HS	0.029
Mother's Education HS	0.015
Mother's Education Some College	0.011
Mother's Education College or More	0.004
Mother Worked During Pregnancy	0.008

Notes: Sample is limited to births by mothers who reside in single-family homes in the Bronx, Brooklyn, and Manhattan with conception years 2004-2012. Column (2) presents the share of mothers who experienced an assault at her home during 10 months post conception month among those defined by the characteristic in the first column.

Table 6: Association Between Assaults During Pregnancy and Maternal Characteristics

	(1) Age	(2) Mar	(3) Dad Miss	(4) For	(5) Wh	(6) Hsp	(7) Bl	(8) LowEd	(9) HighEd	(10) Any Risk	(11) 1st Par	(12) Sngle
Assault During	0.494*	0.0260	0.0141	0.0349	-0.0117	0.0126	-0.00459	0.0194	-0.0208	0.0186	-0.0132	-0.00590
Pregnancy	[0.299]	[0.0223]	[0.0157]	[0.0236]	[0.0144]	[0.0206]	[0.0234]	[0.0232]	[0.0232]	[0.0226]	[0.0235]	[0.00703]
Dept. var mean	26.71	0.338	0.135	0.511	0.107	0.261	0.489	0.577	0.418	0.630	0.466	0.978
Indiv. obs.	1941	1941	1941	1933	1941	1941	1941	1941	1941	1941	1941	1941

Notes: Sample is limited to births by mothers who reside in single-family homes in the Bronx, Brooklyn, or Queens with conception years 2004-2012. Only observations where the mother experienced an assault at her home during either 10 months post conception month or 10 months post expected month of delivery are included. All regressions include conception year, conception month, and borough fixed effects. Robust standard errors.
Significance levels: * $p<0.1$ ** $p<0.05$ *** $p<0.01$

Table 7: Effects of Assault During Pregnancy on Birth Outcomes

	(1) Birwt	(2) LBW	(3) VLBW	(4) HBW	(5) Gest	(6) Pret	(7) V Pret	(8) Low 1m Apg	(9) Low 5m Apg
Assault During	-51.84*	0.0138	0.0172**	-0.0172*	-0.254**	0.00689	0.0167*	0.0234**	0.00275
Pregnancy	[28.78]	[0.0140]	[0.00770]	[0.00988]	[0.122]	[0.0148]	[0.00967]	[0.00996]	[0.00500]
Dept. var mean	3154.1	0.105	0.0259	0.0497	38.37	0.113	0.0424	0.0472	0.0125
Indiv. obs.	1933	1933	1933	1933	1933	1933	1933	1926	1925

Notes: Sample is limited to births by mothers who reside in single-family homes in the Bronx, Brooklyn, and Queens with conception years 2004-2012. Only observations where the mother experienced an assault at her home during either 10 months post conception month or 10 months post expected month of delivery are included. Regressions include controls for the following maternal characteristics: maternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for mother being married, indicator for father information missing, indicator for mother being foreign-born, maternal race/ethnicity dummies (non-Hispanic white, Hispanic, non-Hispanic black, other, missing), maternal education dummies (less than high school and no diploma, high school or GED diploma, some college or associate's degree, bachelor's degree or more, missing), indicator for singleton birth, parity dummies (1st, 2nd, 3rd+, missing). The regressions also control for conception year, conception month, and borough fixed effects. Robust standard errors.

Table 8: Effects of Assault During Pregnancy on Delivery Outcomes

	(1) C-Sec	(2) Ind	(3) Compl	(4) Abnor	(5) NICU	(6) Congen	(7) Bfeed	(8) Male	(9) Death
Assault During	-0.00102	0.0552***	-0.0149	0.0167	0.0177	-0.00189	-0.0103	-0.0231	0.00149
Pregnancy	[0.0219]	[0.0194]	[0.0153]	[0.0149]	[0.0156]	[0.00378]	[0.0235]	[0.0238]	[0.00282]
Dept. var mean	0.326	0.216	0.120	0.110	0.122	0.00580	0.819	0.499	0.00310
Indiv. obs.	1933	1925	1928	1914	1933	1896	1152	1933	1933

Notes: Sample is limited to births by mothers who reside in single-family homes in the Bronx, Brooklyn, and Queens with conception years 2004-2012. Only observations where the mother experienced an assault at her home during either 10 months post conception month or 10 months post expected month of delivery are included. Regressions include controls for the following maternal characteristics: maternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for mother being married, indicator for father information missing, indicator for mother being foreign-born, maternal race/ethnicity dummies (non-Hispanic white, Hispanic, non-Hispanic black, other, missing), maternal education dummies (less than high school and no diploma, high school or GED diploma, some college or associate's degree, bachelor's degree or more, missing), indicator for singleton birth, parity dummies (1st, 2nd, 3rd+, missing). The regressions also control for conception year, conception month, and borough fixed effects. Robust standard errors.

Table 9: Effects of Assault During Pregnancy on Pregnancy Behaviors and Outcomes

	(1) PNC 1Tri	(2) NVis	(3) Mom Hosp	(4) WIC	(5) Smoke	(6) Drugs	(7) Depr	(8) Low Wgt	(9) High Wgt
Assault During	0.0486**	0.301*	-0.00635	-0.0375*	0.00738	-0.00443	0.0187	0.0232	-0.0191
Pregnancy	[0.0223]	[0.181]	[0.0135]	[0.0208]	[0.00760]	[0.00402]	[0.0283]	[0.0155]	[0.0195]
Dept. var mean	0.641	10.02	0.0516	0.650	0.0280	0.00745	0.276	0.115	0.217
Indiv. obs.	1891	1900	1163	1924	1930	1880	1114	1910	1910

Notes: Sample is limited to births by mothers who reside in single-family homes in the Bronx, Brooklyn, and Queens with conception years 2004-2012. Only observations where the mother experienced an assault at her home during either 10 months post conception month or 10 months post expected month of delivery are included. Regressions include controls for the following maternal characteristics: maternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for mother being married, indicator for father information missing, indicator for mother being foreign-born, maternal race/ethnicity dummies (non-Hispanic white, Hispanic, non-Hispanic black, other, missing), maternal education dummies (less than high school and no diploma, high school or GED diploma, some college or associate's degree, bachelor's degree or more, missing), indicator for singleton birth, parity dummies (1st, 2nd, 3rd+, missing). The regressions also control for conception year, conception month, and borough fixed effects. Robust standard errors.

A Additional Results

Appendix Table A.1: Estimated OLS Relationship Between Assaults During Pregnancy and Maternal Characteristics

	(1) Age	(2) Mar	(3) Dad Miss	(4) For	(5) Wh	(6) Hsp	(7) Bl	(8) LowEd	(9) HighEd	(10) Any Risk	(11) 1st Par	(12) Sngle
Assault During	-2.698***	-0.285***	0.0634***	-0.0156	-0.185***	0.0848***	0.194***	0.208***	-0.212***	-0.0197	0.0273	0.0132**
Pregnancy	[0.215]	[0.0163]	[0.0113]	[0.0172]	[0.0112]	[0.0148]	[0.0172]	[0.0168]	[0.0167]	[0.0164]	[0.0169]	[0.00537]
Dept. var mean	29.70	0.641	0.0732	0.533	0.302	0.169	0.295	0.376	0.621	0.661	0.419	0.961
Indiv. obs.	68397	68399	68399	68234	68399	68399	68399	68399	68399	68399	68399	68399

Notes: Sample is limited to births by mothers who reside in single-family homes in the Bronx, Brooklyn, or Queens with conception years 2004-2012. All regressions include conception year, conception month, and borough fixed effects. Robust standard errors.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table A.2: Estimated OLS Relationship Between Assault During Pregnancy and Birth Outcomes

	(1) Birwt	(2) LBW	(3) VLBW	(4) HBW	(5) Gest	(6) Pret	(7) V Pret	(8) Low 1m Apg	(9) Low 5m Apg
Assault During Pregnancy	-52.54**	0.0141	0.0160**	-0.0144**	-0.262***	0.0134	0.0175**	0.0238***	0.00558
	[21.76]	[0.0104]	[0.00628]	[0.00678]	[0.0952]	[0.0108]	[0.00747]	[0.00831]	[0.00414]
Dept. var mean	3206.2	0.0918	0.0164	0.0607	38.55	0.0984	0.0293	0.0357	0.00784
Indiv. obs.	68232	68232	68232	68232	68234	68234	68234	68115	68116

Notes: Sample is limited to births by mothers who reside in single-family homes in the Bronx, Brooklyn, and Queens with conception years 2004-2012. Regressions include controls for the following maternal characteristics: maternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for mother being married, indicator for father information missing, indicator for mother being foreign-born, maternal race/ethnicity dummies (non-Hispanic white, Hispanic, non-Hispanic black, other, missing), maternal education dummies (less than high school and no diploma, high school or GED diploma, some college or associate's degree, bachelor's degree or more, missing), indicator for singleton birth, parity dummies (1st, 2nd, 3rd+, missing). The regressions also control for conception year, conception month, and borough fixed effects. Robust standard errors.

Table A.3: Estimated OLS Relationship Between Assault During Pregnancy and Delivery Outcomes

	(1) C-Sec	(2) Ind	(3) Compl	(4) Abnor	(5) NICU	(6) Congen	(7) Bfeed	(8) Male	(9) Death
Assault During	0.00925	0.0477***	0.00426	0.0188*	0.0185	-0.00301	-0.00152	-0.0222	0.00183
Pregnancy	[0.0160]	[0.0146]	[0.0108]	[0.0111]	[0.0115]	[0.00240]	[0.0165]	[0.0172]	[0.00232]
Dept. var mean	0.331	0.192	0.105	0.0935	0.104	0.00726	0.830	0.510	0.00210
Indiv. obs.	68234	68064	68072	67852	68234	67190	41935	68234	68232

Notes: Sample is limited to births by mothers who reside in single-family homes in the Bronx, Brooklyn, and Queens with conception years 2004-2012. Regressions include controls for the following maternal characteristics: maternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for mother being married, indicator for father information missing, indicator for mother being foreign-born, maternal race/ethnicity dummies (non-Hispanic white, Hispanic, non-Hispanic black, other, missing), maternal education dummies (less than high school and no diploma, high school or GED diploma, some college or associate's degree, bachelor's degree or more, missing), indicator for singleton birth, parity dummies (1st, 2nd, 3rd+, missing). The regressions also control for conception year, conception month, and borough fixed effects. Robust standard errors.

Table A.4: Estimated OLS Relationship Between Assault During Pregnancy and Pregnancy Behaviors and Outcomes

	(1) PNC 1Tri	(2) NVis	(3) Mom Hosp	(4) WIC	(5) Smoke	(6) Drugs	(7) Depr	(8) Low Wgt	(9) High Wgt
Assault During Pregnancy	0.00566	-0.0682	0.00229	0.0348**	0.0112*	0.0000871	0.0467**	0.0169	-0.00973
	[0.0160]	[0.139]	[0.00908]	[0.0151]	[0.00589]	[0.00235]	[0.0195]	[0.0115]	[0.0139]
Dept. var mean	0.736	10.59	0.0372	0.416	0.0155	0.00285	0.201	0.0899	0.204
Indiv. obs.	66646	67317	42263	67950	68179	66604	40982	67777	67777

Notes: Sample is limited to births by mothers who reside in single-family homes in the Bronx, Brooklyn, and Queens with conception years 2004-2012. Regressions include controls for the following maternal characteristics: maternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for mother being married, indicator for father information missing, indicator for mother being foreign-born, maternal race/ethnicity dummies (non-Hispanic white, Hispanic, non-Hispanic black, other, missing), maternal education dummies (less than high school and no diploma, high school or GED diploma, some college or associate's degree, bachelor's degree or more, missing), indicator for singleton birth, parity dummies (1st, 2nd, 3rd+, missing). The regressions also control for conception year, conception month, and borough fixed effects. Robust standard errors.

Table A.5: Effects of Assault During Pregnancy on Birth Outcomes: Using Mothers with Assaults Before Pregnancy as Additional Control Group

	(1) Birwt	(2) LBW	(3) VLBW	(4) HBW	(5) Gest	(6) Pret	(7) V Pret	(8) Low 1m Apg	(9) Low 5m Apg
Assault During	-32.60	0.0120	0.0152**	-0.0115	-0.195*	0.00610	0.0131	0.0254***	0.00248
Pregnancy	[25.39]	[0.0123]	[0.00702]	[0.00841]	[0.109]	[0.0129]	[0.00856]	[0.00917]	[0.00466]
Dept. var mean	3145.8	0.104	0.0250	0.0482	38.37	0.113	0.0431	0.0447	0.0124
Indiv. obs.	2758	2758	2758	2758	2758	2758	2758	2750	2749

Notes: Sample is limited to births by mothers who reside in single-family homes in the Bronx, Brooklyn, and Queens with conception years 2004-2012. Only observations where the mother experienced an assault at her home during either 10 months before the conception month, 10 months post conception month or 10 months post expected month of delivery are included. Regressions include controls for the following maternal characteristics: maternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for mother being married, indicator for father information missing, indicator for mother being foreign-born, maternal race/ethnicity dummies (non-Hispanic white, Hispanic, non-Hispanic black, other, missing), maternal education dummies (less than high school and no diploma, high school or GED diploma, some college or associate's degree, bachelor's degree or more, missing), indicator for singleton birth, parity dummies (1st, 2nd, 3rd+, missing). The regressions also control for conception year, conception month, and borough fixed effects. Robust standard errors.

Table A.6: Effects of Assault During Pregnancy on Delivery Outcomes: Using Mothers with Assaults Before Pregnancy as Additional Control Group

	(1) C-Sec	(2) Ind	(3) Compl	(4) Abnor	(5) NICU	(6) Congen	(7) Bfeed	(8) Male	(9) Death
Assault During	-0.00802	0.0534***	-0.00169	0.00862	0.00591	-0.00629*	-0.0104	-0.0344*	0.00170
Pregnancy	[0.0191]	[0.0172]	[0.0132]	[0.0132]	[0.0138]	[0.00345]	[0.0199]	[0.0208]	[0.00257]
Dept. var mean	0.331	0.207	0.113	0.114	0.128	0.00812	0.824	0.510	0.00326
Indiv. obs.	2758	2743	2750	2732	2758	2711	1683	2758	2758

Notes: Sample is limited to births by mothers who reside in single-family homes in the Bronx, Brooklyn, and Queens with conception years 2004-2012. Only observations where the mother experienced an assault at her home during either 10 months before the conception month, 10 months post conception month or 10 months post expected month of delivery are included. Regressions include controls for the following maternal characteristics: maternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for mother being married, indicator for father information missing, indicator for mother being foreign-born, maternal race/ethnicity dummies (non-Hispanic white, Hispanic, non-Hispanic black, other, missing), maternal education dummies (less than high school and no diploma, high school or GED diploma, some college or associate's degree, bachelor's degree or more, missing), indicator for singleton birth, parity dummies (1st, 2nd, 3rd+, missing). The regressions also control for conception year, conception month, and borough fixed effects. Robust standard errors.

Table A.7: Effects of Assault During Pregnancy on Pregnancy Behaviors and Outcomes: Using Mothers with Assaults Before Pregnancy as Additional Control Group

	(1) PNC 1Tri	(2) NVis	(3) Mom Hosp	(4) WIC	(5) Smoke	(6) Drugs	(7) Depr	(8) Low Wgt	(9) High Wgt
Assault During Pregnancy	0.0404**	0.192	-0.00509	-0.0341*	0.00523	-0.00382	0.0370	0.0132	-0.0124
	[0.0194]	[0.167]	[0.0111]	[0.0183]	[0.00677]	[0.00314]	[0.0236]	[0.0138]	[0.0169]
Dept. var mean	0.640	10.08	0.0490	0.650	0.0276	0.00708	0.256	0.120	0.214
Indiv. obs.	2691	2709	1695	2747	2755	2685	1624	2724	2724

Notes: Sample is limited to births by mothers who reside in single-family homes in the Bronx, Brooklyn, and Queens with conception years 2004-2012. Only observations where the mother experienced an assault at her home during either 10 months before the conception month, 10 months post conception month or 10 months post expected month of delivery are included. Regressions include controls for the following maternal characteristics: maternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for mother being married, indicator for father information missing, indicator for mother being foreign-born, maternal race/ethnicity dummies (non-Hispanic white, Hispanic, non-Hispanic black, other, missing), maternal education dummies (less than high school and no diploma, high school or GED diploma, some college or associate's degree, bachelor's degree or more, missing), indicator for singleton birth, parity dummies (1st, 2nd, 3rd+, missing). The regressions also control for conception year, conception month, and borough fixed effects. Robust standard errors.

Table A.8: "Difference-in-Difference" Effects of Assault During Pregnancy on Birth Outcomes, Relative to All Other Crimes

	(1) Birwt	(2) LBW	(3) VLBW	(4) HBW	(5) Gest	(6) Pret	(7) V Pret	(8) Low 1m Apg	(9) Low 5m Apg
Assault During	-53.30*	0.0136	0.0170**	-0.0174*	-0.213*	0.000746	0.0127	0.0291***	0.00369
Pregnancy	[28.82]	[0.0141]	[0.00759]	[0.0103]	[0.121]	[0.0149]	[0.00956]	[0.0103]	[0.00529]
Any Assault During	19.65	-0.00315	-0.00306	0.00482	0.0116	0.00465	-0.000754	-0.00652	0.00139
or Post Pregnancy	[18.82]	[0.00956]	[0.00443]	[0.00771]	[0.0751]	[0.0101]	[0.00606]	[0.00616]	[0.00331]
Any Crime During	6.660	-0.00369	0.00105	0.000529	-0.0356	0.00530	0.00428	-0.000615	0.000336
Pregnancy	[11.54]	[0.00575]	[0.00266]	[0.00476]	[0.0444]	[0.00590]	[0.00356]	[0.00385]	[0.00181]
Dept. var mean	3178.1	0.101	0.0198	0.0581	38.48	0.106	0.0361	0.0392	0.00866
Indiv. obs.	11226	11226	11226	11226	11226	11226	11226	11199	11198

Notes: Sample is limited to births by mothers who reside in single-family homes in the Bronx, Brooklyn, and Queens with conception years 2004-2012. Only observations where the mother experienced any crime at her home during either 10 months post conception month or 10 months post expected month of delivery are included. Regressions include controls for the following maternal characteristics: maternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for mother being married, indicator for father information missing, indicator for mother being foreign-born, maternal race/ethnicity dummies (non-Hispanic white, Hispanic, non-Hispanic black, other, missing), maternal education dummies (less than high school and no diploma, high school or GED diploma, some college or associate's degree, bachelor's degree or more, missing), indicator for singleton birth, parity dummies (1st, 2nd, 3rd+, missing). The regressions also control for conception year, conception month, and borough fixed effects. Robust standard errors.

Table A.9: “Difference-in-Difference” Effects of Assault During Pregnancy on Delivery Outcomes, Relative to All Other Crimes

	(1) C-Sec	(2) Ind	(3) Compl	(4) Abnor	(5) NICU	(6) Congen	(7) Bfeed	(8) Male	(9) Death
Assault During	0.00272	0.0403**	-0.0145	0.0144	0.0110	-0.000794	-0.00928	-0.0100	0.00215
Pregnancy	[0.0223]	[0.0196]	[0.0154]	[0.0148]	[0.0154]	[0.00358]	[0.0236]	[0.0241]	[0.00277]
Any Assault During	0.000909	0.0117	0.0192*	0.00238	0.000716	-0.000840	0.0100	-0.0104	0.000168
or Post Pregnancy	[0.0155]	[0.0130]	[0.0109]	[0.00984]	[0.0104]	[0.00268]	[0.0167]	[0.0167]	[0.00157]
Any Crime During	-0.0145	0.00961	0.00231	0.00228	0.00547	-0.00176	0.00162	-0.0126	0.000356
Pregnancy	[0.00925]	[0.00771]	[0.00618]	[0.00573]	[0.00616]	[0.00172]	[0.0104]	[0.0102]	[0.000899]
Dept. var mean	0.328	0.186	0.106	0.0963	0.113	0.00715	0.825	0.512	0.00214
Indiv. obs.	11226	11186	11188	11139	11226	11043	6253	11226	11226

Notes: Sample is limited to births by mothers who reside in single-family homes in the Bronx, Brooklyn, and Queens with conception years 2004-2012. Only observations where the mother experienced any crime at her home during either 10 months post conception month or 10 months post expected month of delivery are included. Regressions include controls for the following maternal characteristics: maternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for mother being married, indicator for father information missing, indicator for mother being foreign-born, maternal race/ethnicity dummies (non-Hispanic white, Hispanic, non-Hispanic black, other, missing), maternal education dummies (less than high school and no diploma, high school or GED diploma, some college or associate's degree, bachelor's degree or more, missing), indicator for singleton birth, parity dummies (1st, 2nd, 3rd+, missing). The regressions also control for conception year, conception month, and borough fixed effects. Robust standard errors.

Table A.10: "Difference-in-Difference" Effects of Assault During Pregnancy on Pregnancy Behaviors and Outcomes, Relative to All Other Crimes

	(1) PNC 1Tri	(2) NVis	(3) Mom Hosp	(4) WIC	(5) Smoke	(6) Drugs	(7) Depr	(8) Low Wgt	(9) High Wgt
Assault During	0.0345	0.199	-0.00707	-0.0381*	0.00855	-0.00541	0.0319	0.0284*	-0.0244
Pregnancy	[0.0225]	[0.185]	[0.0136]	[0.0209]	[0.00791]	[0.00411]	[0.0280]	[0.0154]	[0.0199]
Any Assault During	-0.0240	-0.251**	0.0119	0.0611***	0.00225	0.00307	0.00838	-0.00848	0.00457
or Post Pregnancy	[0.0158]	[0.122]	[0.00994]	[0.0144]	[0.00525]	[0.00326]	[0.0200]	[0.0102]	[0.0141]
Any Crime During	0.0117	0.0149	-0.00147	-0.00160	-0.00321	0.00139	-0.0194*	-0.00790	0.00446
Pregnancy	[0.00921]	[0.0759]	[0.00535]	[0.00880]	[0.00280]	[0.00161]	[0.0117]	[0.00590]	[0.00836]
Dept. var mean	0.691	10.35	0.0391	0.515	0.0199	0.00567	0.232	0.0970	0.217
Indiv. obs.	10957	11028	6315	11169	11216	10938	6086	11123	11123

Notes: Sample is limited to births by mothers who reside in single-family homes in the Bronx, Brooklyn, and Queens with conception years 2004-2012. Only observations where the mother experienced any crime at her home during either 10 months post conception month or 10 months post expected month of delivery are included. Regressions include controls for the following maternal characteristics: maternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for mother being married, indicator for father information missing, indicator for mother being foreign-born, maternal race/ethnicity dummies (non-Hispanic white, Hispanic, non-Hispanic black, other, missing), maternal education dummies (less than high school and no diploma, high school or GED diploma, some college or associate's degree, bachelor's degree or more, missing), indicator for singleton birth, parity dummies (1st, 2nd, 3rd+, missing). The regressions also control for conception year, conception month, and borough fixed effects. Robust standard errors.

Table A.11: Effects of Assault During Pregnancy on Birth Outcomes: Maternal Fixed Effects

	(1) Birwt	(2) LBW	(3) VLBW	(4) HBW	(5) Gest	(6) Pret	(7) V Pret	(8) Low 1m Apg	(9) Low 5m Apg
Any Assault During	-71.91	0.0247	0.0344	-0.0261	-0.552	0.0327	0.0559*	0.0484*	0.00956
Pregnancy	[66.32]	[0.0391]	[0.0244]	[0.0248]	[0.339]	[0.0364]	[0.0288]	[0.0277]	[0.0166]
Dept. var mean	3275.0	0.0611	0.00973	0.0679	38.75	0.0693	0.0178	0.0261	0.00684
Indiv. obs.	18599	18599	18599	18599	18600	18600	18600	18570	18570

Notes: Sample is limited to singleton sibling births by mothers who resided in single-family homes in the Bronx, Brooklyn, or Queens during the first pregnancy with conception years 2004-2012. Regressions include controls for the following time-varying maternal characteristics: maternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for mother being married, maternal education dummies (less than high school and no diploma, high school or GED diploma, some college or associate's degree, bachelor's degree or more, missing), parity dummies (1st, 2nd, 3rd+, missing), and birth interval dummies (1st birth, $<$ 12 months from previous birth, 12-24 months from previous birth, 24-36 months from previous birth, 36-48 months from previous birth, 48+ months from previous birth). The regressions also control for conception year, conception month, and mother fixed effects. Standard errors clustered on the mother.

Table A.12: Effects of Assault During Pregnancy on Delivery Outcomes: Maternal Fixed Effects

	(1) C-Sec	(2) Ind	(3) Compl	(4) Abnor	(5) NICU	(6) Congen	(7) Bfeed	(8) Male	(9) Death
Any Assault During Pregnancy	-0.0201	0.00117	-0.00706	0.0460	0.0710**	-0.00837	-0.0649	0.0459	0.0102
	[0.0309]	[0.0492]	[0.0388]	[0.0290]	[0.0340]	[0.0115]	[0.0770]	[0.0652]	[0.00955]
Dept. var mean	0.277	0.178	0.0957	0.0642	0.0704	0.00852	0.835	0.505	0.00167
Indiv. obs.	18600	18566	18558	18508	18600	18307	11743	18600	18599

Notes: Sample is limited to singleton sibling births by mothers who reside in single-family homes in the Bronx, Brooklyn, or Queens during the first pregnancy with conception years 2004-2012. Regressions include controls for the following time-varying maternal characteristics: maternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for mother being married, maternal education dummies (less than high school and no diploma, high school or GED diploma, some college or associate's degree, bachelor's degree or more, missing), parity dummies (1st, 2nd, 3rd+, missing), and birth interval dummies (1st birth, < 12 months from previous birth, 12-24 months from previous birth, 24-36 months from previous birth, 36-48 months from previous birth, 48+ months from previous birth). The regressions also control for conception year, conception month, and mother fixed effects. Standard errors clustered on the mother.

Table A.13: Effects of Assault During Pregnancy on Pregnancy Behaviors and Outcomes: Maternal Fixed Effects

	(1) PNC 1Tri	(2) NVis	(3) Mom Hosp	(4) WIC	(5) Smoke	(6) Drugs	(7) Depr	(8) Low Wgt	(9) High Wgt
Any Assault During	0.0143	-0.264	0.0108	-0.0935*	0.0129	-0.000330	-0.0220	0.0112	-0.0590
Pregnancy	[0.0593]	[0.486]	[0.0186]	[0.0542]	[0.0235]	[0.000523]	[0.0930]	[0.0365]	[0.0434]
Dept. var mean	0.781	10.67	0.0326	0.322	0.0120	0.00181	0.172	0.0858	0.176
Indiv. obs.	18162	18358	11835	18508	18586	18217	11527	18481	18481

Notes: Sample is limited to singleton sibling births by mothers who reside in single-family homes in the Bronx, Brooklyn, or Queens during the first pregnancy with conception years 2004-2012. Regressions include controls for the following time-varying maternal characteristics: maternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for mother being married, maternal education dummies (less than high school and no diploma, high school or GED diploma, some college or associate's degree, bachelor's degree or more, missing), parity dummies (1st, 2nd, 3rd+, missing), and birth interval dummies (1st birth, < 12 months from previous birth, 12-24 months from previous birth, 24-36 months from previous birth, 36-48 months from previous birth, 48+ months from previous birth). The regressions also control for conception year, conception month, and mother fixed effects. Standard errors clustered on the mother.