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ABSTRACT

Violence While in Utero: The Impact of Assaults during Pregnancy on Birth Outcomes*

Causal evidence of the effects of violent crime on its victims is sparse. Yet such evidence is needed to determine the social cost of crime and to evaluate the cost-effectiveness of policy interventions in the justice system. This study presents new evidence on the effects of violent crime on pregnancy and infant health outcomes, using unique linked administrative data from New York City. We merge birth records with maternal residential addresses to the locations of reported crimes, and focus on mothers who lived in a home where an assault was reported during their pregnancies. We compare these mothers to women who lived in a home with an assault that took place shortly after the birth. We find that assaults in the 3rd trimester significantly increase rates of very low birth weight (less than 1,500 grams) and very pre-term (less than 34 weeks gestation) births, possibly through a higher likelihood of induced labor. We show that our results are robust to multiple choices of control groups and to using maternal fixed effects models. We calculate that these impacts translate into a social cost per assault during pregnancy of $41,771, and a total annual cost of over $4.25 billion when scaled by the national victimization rate. As infant health is a strong predictor of life-long well-being, and women of lower socioeconomic status are more likely to be victims of domestic abuse than their more advantaged counterparts, our results suggest that in utero subjection to violent crime is an important new channel for intergenerational transmission of inequality.

JEL Classification: I14, I31, J12, J13, K14

Keywords: domestic violence, infant health, pregnancy, health inequality, social cost of crime

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1 Introduction

Crime is considered a canonical example of a negative externality because of its large cost to society. 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; Buonanno 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 cost 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]

Estimates of the cost of crime serve a critical role in policy evaluation. Table 1 lists examples of studies in which cost of crime estimates are used to assess a variety of interventions that impact crime rates, including labor market programs, child development interventions (such as high quality preschools), gun regulation, housing programs, and many others. Clearly, comprehensive understanding of the costs of crime on victims can inform cost-benefit analyses of a wide range of policies, both within and outside of the criminal justice system.

Intimate partner violence (IPV) is an important component of violent crime, accounting for over one seventh of all violent crimes (see Figure 1). Economists have studied the determinants of domestic violence 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), but consistent with the general scarcity of evidence on the effects of crime on victims, there is much less economic research on the impacts of IPV on victims. This paper focuses on the effects of assault on pregnant women, which 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). Newberger *et al.* (1992) point out that 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

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

to prenatal care or other services, and engagement in adverse behaviors such as smoking or poor nutrition as a coping mechanism.

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; Aizer, 2011). 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 (often 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. 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 experience an assault 1 to 10 months after the estimated due date. We rely on the 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 maternal characteristics are statistically indistinguishable in 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. [3]

We find that assault during pregnancy has adverse consequences for infant health. Compared to mothers who have an assault in the postpartum period, mothers with an assault

[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.

[3]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.

during pregnancy have a 0.08 standard deviation (SD) higher summary index of poor birth outcomes, driven by 1.7, 1.7, and 2.4 percentage point (66, 39, and 50 percent) higher rates of births that are very low birth weight (less than 1,500 grams), very pre-term (less than 34 weeks gestation), and have low 1-minute Apgar scores, respectively.[4] These impacts appear to be concentrated in the 3rd trimester of pregnancy. Additionally, we find that prenatal exposure to assault is associated with an increased likelihood of induced labor, which is likely a response of the healthcare system to injuries sustained by pregnant victims of abuse.

We conduct a back-of-the-envelope calculation to estimate the average social cost generated by assault during pregnancy. We use our estimated 1.7 percentage point increase in the likelihood of a very low birth weight birth, and account for costs that operate through six channels: higher rates of infant mortality, increased medical costs at and immediately following birth, increased costs associated with childhood disability, decreases in adult income, increased medical costs associated with adult disability, and reductions in life expectancy. We calculate an average social cost of $41,771 per assault during pregnancy. Assuming that 2.6 percent of pregnant women experience an assault—the national victimization rate estimated from survey data—this figure translates into a total annual social cost in excess of $4.25 billion.

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 new evidence about the large and intergenerational social cost of violent crime. Since poor pregnant women are much more likely to be victims of assault than their more advantaged counterparts (Jewkes, 2002; Aizer, 2011), and as the majority of all violence against women is perpetrated by domestic partners (Tjaden and Thoennes, 2000), our results suggest that intra-family conflict may be an important and previously understudied mechanism by which early-life health disparities perpetuate persistent economic inequality across generations.

A number of prior 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). However, 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,

[4]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`.

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 examine the use of medical interventions and prenatal behaviors in an attempt to understand the mechanisms driving our estimated effects on infant health.

We also contribute to a literature on the relationship between violence—either due to local 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; 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 through 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.

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 presents our estimate of the implied social cost of crime and offers some conclusions.

2 Background

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

Intimate partner violence is shockingly common. Recent estimates from the National Intimate Partner and Sexual Violence Survey (NIPSVS) by the Centers for Disease Control and

[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.

Prevention indicate that 32 percent of U.S. women experience physical 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).

As shown in Figure 1, violent crime where the perpetrator is a stranger has gone down substantially over 1993-2016. Violent crime most commonly occurs between two individuals with a known relationship (either intimate partners, other relatives, or acquaintances). And as noted above, violence originating from an intimate partner accounts for over one seventh of all violent crime.

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 and 29 are at greatest risk of severe IPV, whether measured as female IPV-related homicide, female IPV-related hospitalization, or female IPV-related emergency department visit (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. Thus, pregnant women and their unborn children represent a significant fraction of violent crime victims.

2.2 Police Responses to Domestic Violence in New York City

Since, as discussed further below, we use police reports to measure crimes in our analysis, it is useful to understand how police treat domestic violence in New York City. 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]

[6] The second age group at greatest risk was women aged 30 to 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

When a domestic violence complaint is made, the police may issue an appearance ticket or immediately arrest the accused depending on the degree of the offense.[9] If arrested, the accused are locally booked and should be arraigned before a judge within 24 hours. At the arraignment, the judge decides whether to issue an order of protection, as well as whether to release the defendant (with or without bail) or to hold the defendant on remand.

State law breaks 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 known as petty offenses, include verbal threats and physical acts that do not result in injury. Our analysis sample focuses specifically on reported instances of misdemeanor and felony assaults (including aggravated assaults). Offenses categorized as violations may also impact fetal health and birth outcomes. However, as less serious offenses have lower reporting rates than misdemeanor and felony assaults (Morgan and Kena, 2017), we believe they are especially susceptible to selective reporting, which may change before and after childbirth, thereby violating the assumptions of our research design (described in more detail in Section 4). As a consequence, we exclude violations from the analysis.

Convicted offenders face increasingly severe sanctions as the gravity of the crime increases,[10] yet only a fraction of those who are arrested are convicted and sentenced to incarceration.[11]

The NYPD has over 400 domestic violence prevention officers, investigators, and supervisors (New York Police Department, 2018). Prevention officers receive additional training in how 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

strategy to discourage domestic violence and support victims (NYC Mayor's Office to Combat Domestic Violence, 2018).

[9]While statistics specific to domestic violence incidents are unavailable, 67 percent of felony and misdemeanor assault suspects were arrested in 2017 (O'Neill, 2018).

[10]By New York law, individuals convicted of a misdemeanor assault can be sentenced to anywhere from 0 to 12 months of jail time. Felony assault sentences depend on the degree of the offense: Class B (5-25 years in prison), Class C (3.5-15 years in prison), Class D (2-7 years in prison), or Class E (1.5-4 years in prison or probation).

[11]According to NYS Division of Criminal Justice Services (2018), between 2013 and 2017, 23 percent of violent felony arrests were convicted and sentenced to a form of incarceration (prison, jail, time-served or jail and probation). The corresponding figure for all misdemeanor arrests was 15 percent. New York state does not report specific estimates for domestic violence offenses or statistics on the length of sentence.

misdemeanor offense committed by one "member of the same family or household" against another. The fact that felonies and misdemeanors are treated similarly by the police in terms of arrests is another reason to exclude violations, which are less likely to result in an arrest.[12][13]

In summary, domestic violence cases make up a large fraction of the NYPD's workload. Officers are mandated to respond to domestic violence complaints, and must arrest suspects in cases of felony or misdemeanor assault. The fact that we use data on all such cases that were reported to the NYPD, and not only on cases that resulted in a conviction, means that we likely have a more representative sample of assaults during pregnancy than some previous studies. However, some issues of mis-measurement and under-reporting of IPV likely still exist in our data, which we discuss in Sections 3 and 4 below.

2.3 Estimating the Social Costs of Crime

Scholars dating back to at least the Wickersham Commission on Law Observance and Enforcement (Anderson *et al.*, 1931) have sought to quantify the social cost of crime. Costs include (but are not limited to) property loss or destruction,[14] administrative costs of the justice system, victims' mental and physical health, and victims' potential lost productivity.[15] Quantifying these impacts in terms of a common unit of measurement (e.g., dollars) is not trivial, but is crucial for evaluating policy decisions. In fact, a wide range of economic analyses uses such estimates to assess the potential cost-effectiveness of interventions or public programs (see Table 1).

Several strategies have been developed to meet this need(Cohen, 2005).[16] One ("cost of illness") tradition (Hodgson and Meiners, 1982; Malzberg, 1950) attempts to quantify tangible impacts of crime on specific outcomes using the best available information and assigned prices (McCollister *et al.*, 2010). Often, the best available information comes from self-reports from victims about the costs of victimization. The jury-award approach is closely related (Miller *et al.*, 1996). It assesses the total social cost of crime using actual compensation awards from civil personal injury cases.

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

[13]The NYC Confidentiality Policy (Bloomberg, 2003a,b) mandates that police officers should not ask undocumented immigrants who are victims of crime (including IPV) about immigration status. This policy arguably mitigates concerns about under-reporting of violence against immigrants in our data.

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

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

[16]Soares (2015) provides a review of these methods for an economic audience and discusses how the approaches measure inherently different theoretical parameters.

Other work uses hedonic methods to estimate the social cost of crime (Thaler, 1978), assuming that both the tangible and intangible costs of crime are capitalized into local housing prices. Contingent valuation studies use a similar logic: Surveys ask respondents about their willingness to pay to avoid various criminal acts, which theoretically provides a measure of both tangible and intangible costs (Cohen *et al.*, 2004; Cook and Ludwig, 2000).[17]

All of these methods assume that the impacts of crime are fully known. If an impact of crime is unknown to the researcher, a jury, a home buyer, or a survey respondent, then estimates of the social cost of crime derived from these methods will be biased towards zero. Conversely, if impact estimates are based on biased priors (which is possible given the lack of causal evidence on victimization), then cost estimates could be either under- or overestimated. For example, if people have exaggerated fears of crime, then survey methods will overstate the cost of crime.

Table 2 reports commonly used upper and lower bound estimates of the cost of several major types of crime. According to these estimates, the social cost of assault is between approximately \$16,000 and \$90,000 per victim. While there is a wide range, available estimates consistently indicate that violent crime is more costly than any other type of offense. As a result, small changes in 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 critical to generate estimates of the cost of violent crime that meet two criteria: First, they must accurately reflect causal effects, and second, they must 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 omitted from existing calculations. We discuss our estimates of costs associated with these effects further in Section 6.

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.

[17]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).

Crime data. The crime data come from administrative records from the NYPD. The data cover all criminal complaints reported to the NYPD between 2004 and 2012.[18] 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.[19]

Table 3 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.[20]

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.[21] We observe a variety of birth outcomes, including child sex, birth order, plurality, birth weight in grams, gestation length in weeks, the Apgar score, an indicator for 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 an indicator for any complications of labor or delivery. Further, we have data on maternal behaviors during pregnancy and at childbirth, including the date of prenatal care

[18]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.

[19]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.

[20]The figure also shows a notable decline in robberies over the study period, particularly in 2009, but we do not focus on robberies in the current study.

[21]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.

initiation and the total number of prenatal care visits, 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, and whether the mother reports being depressed during pregnancy.[22]

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, pre-eclampsia, 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 maternal exact (self-reported) residential addresses 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 month and year of birth 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). The 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, Block, Lot (BBL) tax identifier, an identifier that is unique to New York City. Importantly, these data allow us to distinguish between single-family homes and large multi-unit apartment buildings.

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. We use the mother's self-reported residential address from the birth certificate, and standardize it 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),

[22]The birth certificate format changed in 2008, which is during our sample time frame. The information on whether the mother was depressed during pregnancy is only available from 2008 onward. 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.

which is a customized "fuzzy matching" algorithm designed specifically for common matching challenges in New York City.[23] `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.[24]

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

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 the estimated effects of violent assaults during pregnancy toward zero. In particular, our measure of assault exposure could capture another household member—who is not the pregnant woman or new mother—being victimized.[25] 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 problem is unfortunately unavoidable in our context because victim information is withheld in the 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 suggest that a total of 28,593 NYC mothers suffered some form of physical abuse during pregnancy. Since the PRAMS survey includes offenses that are never reported to law enforcement, we scale

[23]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 identifier. Specific boroughs (Brooklyn, Bronx, Manhattan, Queens, and Staten Island) have specific nuances in address formats, which is taken into account by Geosupport.

[24]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.

[25]Estimates from Maston *et al.* (2011) indicate only 0.6 percent of assaults are situations in which the victim-offender relationship is child-parent.

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 (Bureau of Justice Statistics). We thus obtain an estimate of 12,009 *reported* episodes of abuse among pregnant women from PRAMS. Any counts that exceed this number in our records would suggest measurement error in our explanatory variable of interest.

Table 4 shows a variety of different scaling factors to account for measurement error depending on different assumptions. In 25 percent of all births in our data, women lived in a building with a reported misdemeanor or felony assault during their pregnancy. An additional 25 percent of births were to women who lived in a building with a harassment claim. These would include cases of physical altercations that did not result in serious injury (e.g., a slap, a push, etc).[26]

This very high "exposure rate" is driven by large apartment buildings with many units. Hence, these figures dramatically overstate the actual likelihood of direct victimization. Assuming each residential unit in a building has an equal probability of direct victimization, we can scale down the exposure counts by $\frac{1}{Residential\,Units\,in\,Building}$. This simple correction results in prevalence rates of 0.011 for felony and misdemeanor assaults and 0.011 for harassment, which is much closer to the consensus estimates in the literature.

Remarkably, the unit-adjusted estimates using only felony and misdemeanor assaults, put our count within 250 cases of the PRAMS estimate. However, because there are types of physical altercations that are only covered in the harassment complaints, we also report the larger 23,517 domestic violence incidents count that includes harassment. Assuming that the PRAMS estimate is accurate, the broader definition would imply that roughly half of our observations have an explanatory variable measured with error. An easy rule of thumb then 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 for our main analysis. 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

[26]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.

other boroughs in terms of their demographic and socioeconomic characteristics.[27] 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).[28] These restrictions leave us with a sample of 1,941 births.

Table 5 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 treatment group observations, while column (3) uses 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 be non-Hispanic black or Hispanic, and have lower education levels 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 analyze these differences in more detail in the next section.

Table 6 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), 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.

Outcomes. We create four groups of outcomes that we examine in all of our analyses: (1) <u>main birth outcomes</u>, which are indicators for: very low birth weight (<1,500 grams), very pre-term birth (<34 weeks gestation), low 1-minute Apgar score (<7), NICU admission, any abnormal conditions (e.g., use of assisted ventilation or surfactant) or congenital anomalies

[27] 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.

[28] 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 analysis 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.

of the newborn, and death by the time of birth certificate filing; (2) use of medical services: indicator for first trimester prenatal care initiation, number of prenatal care visits, indicator for WIC take-up, indicator for induction of labor, indicator for delivery by c-section, and indicator for any complications during labor or delivery (e.g., premature rupture of membranes); (3) maternal behavioral and well-being outcomes, which are indicators for: mother smoking during pregnancy, mother using illicit drugs during pregnancy, mother being depressed, too low pregnancy weight gain (<15 lbs), too high pregnancy weight gain (>40 lbs), and the father information being missing from the birth certificate;[29] (4) additional birth outcomes relegated to Appendix B for ease of exposition: continuous birth weight in grams, indicator for low birth weight (<2,500 grams), indicator for high birth weight (>4,000 grams), gestation in weeks, indicator for pre-term birth (<37 weeks), and indicator for male child.[30]

An important concern for our analysis is that we may find spurious effects due to the number of outcomes we consider. We address the issue of multiple hypothesis testing by creating four outcome indices. We create three indices using the outcomes described in each of groups (1) through (3) above. The fourth index consists of the outcomes in group (1) and (4), i.e., all possible birth outcomes. To create the indices, we first orient each outcome such that a higher value either represents a more adverse outcome (for outcome groups 1, 3, and 4) or more use of medical services (for outcome group 2), and then standardize each oriented outcome by subtracting the control group mean and dividing by the control group standard deviation. For most of our analysis, the control group is defined as mothers who experience an assault in months 10 through 19 post-conception. For the maternal fixed effects analysis (described further in Section 4 below), the control group is all births with no assault in months 0-9 post-conception. We take an equally weighted average of the standardized outcomes as in Kling *et al.* (2007).

[29] 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`.

[30] 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.

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:

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

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 5 and 6), 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 the longer the pregnancy lasts, the more time there is for the woman to be assaulted during pregnancy.[31]

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 $i \in \{S^{'}\}$:

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

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 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 of exposure to an assault during pregnancy.

Under-reporting of IPV. A major problem with using crime data to study IPV is that it is under-reported to the police.[32] We attempt to limit the scope of potential bias from under-reporting through two sample restrictions. First, we use a sample of mothers who have all had an assault reported at their residence at some point in the months surrounding childbirth, indicating a willingness to report to law enforcement in both our treatment and control groups. Second, we focus on misdemeanor and felony offenses that resulted in some form of injury, which are less likely to be under-reported than more minor offenses (Morgan and Kena, 2017).

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

[32] Although there is evidence that IPV is under-reported, research shows that victims of IPV are as likely as other victims of (non-domestic) assault to call the police (Felson *et al.*, 2002).

Nevertheless, our estimates of β_1 may be subject to two forms of attenuation bias resulting from under-reporting.[33] The literature on IPV suggests that an assault where the police are called is unlikely to be a "one-off" event (Straus *et al.*, 2017). In many cases, there was a continuous pattern of abuse that culminates in a more serious assault in which law enforcement gets involved. Therefore, all of the women in our treatment and control groups are likely to be subject to high levels of stress, implying that our estimates capture the effects of the more serious assault itself rather than the (possibly chronic) stress associated with being in a violent relationship. Additionally, if the likelihood of reporting is inversely related to a woman's bargaining power in the relationship (Frieze and Browne, 1989; Herzberger, 1996), it is possible that those who suffer the worst abuse are the least likely to report. Our estimates of the impact of reported victimization would then underestimate the true impact of victimization unconditional on reporting.

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 5 already demonstrated that mothers in our treatment and control groups are similar in terms of their observable characteristics, Table 7 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 11 coefficients in Table 7, 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.[34]

As discussed above, under-reporting of IPV implies that one plausible unobservable difference between the treatment and control groups is in the likelihood of calling the police when an assault occurs. If, for example, mothers of newborns were more likely to involve the police after experiencing abuse than pregnant women, then we would face a violation of our identifying assumption. The fact that we do not observe significant differences in

[33] This form of bias is distinct from potential attenuation bias due to measurement error in our explanatory variable (resulting from the crime data being linked to mothers at the building level) already discussed in Section 3.

[34] We have also estimated these models using trimester-specific indicators for exposure to assault. Out of 33 possible coefficients, only two are statistically significant at the 5% level. Results available upon request.

the observable characteristics of women in our treatment and comparison groups, however, assuages concerns about this difference being substantial. To address this issue further, we test 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:[35]

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

We also estimate a difference-in-differences (DD) style 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). The DD model allows for a difference in the reporting rate between women who experience an assault during pregnancy and women who experience one postpartum, but assumes that this difference is similar to that in the reporting rate for other crimes.

Lastly, we leverage the maternal identifiers in our birth records data to link siblings to the same mother, and use a maternal fixed effects design. 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:[36]

$$y_{iymk} = \kappa_0 + \kappa_1 \mathbf{1}[c \leq Assault \leq e_b]_{iymk} + \zeta_y + \chi_m + \pi_k + \mathbf{x'}_i \tau + \mu_{iymk} \tag{4}$$

for each child i, conceived in year y and month m, born to mother k. π_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 using the 451 children of 201 mothers who have at least one pregnancy exposed to an assault, and one unexposed pregnancy.[37] We cluster standard

[35] 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.

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

[37] 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

errors on the mother. As we show below, the estimates are remarkably robust to these changes in estimation technique.

5 Results

Descriptive evidence. Appendix Tables B.1 and B.2 report estimates from ordinary least squares (OLS) models that examine the correlations between experiencing assault during pregnancy and a range of maternal background characteristics, as well as the four summary outcome indices described in Section 3. 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 who experienced no assault (or any other crime) at any point during our sample period.

We see that violent victimization during pregnancy is associated with a range of adverse outcomes for infants and mothers. The clear negative selection on maternal characteristics in Appendix Table B.1 raises doubt, however, as to whether the estimates can be interpreted as causal.

Main results. Panel A of Table 8 presents our main results for the first set of birth outcomes, based on estimating equation (3) using the sample with $i \in \{S'\}$ (defined in Section 4). Our estimates suggest that exposure to violent assault during pregnancy causes a deterioration in newborn health.

We find that the share of births with very low birth weight increases by 1.7 percentage points, or 66.4 percent at the sample mean. We also find a 1.7 percentage point increase in very pre-term births (39.4 percent effect at the sample mean). The likelihood of a low 1-minute Apgar score increases by 2.3 percentage points, or 49.6 percent at the sample mean. While the signs of the treatment coefficients for other outcomes—NICU admission, the presence of abnormal conditions or congenital anomalies, and death—are also consistent with an adverse effect, they are not statistically significant. The last column shows a 0.08 SD increase in the summary adverse birth outcome index, which is significant at the 5% level.

Appendix Table B.3 presents results for the other birth outcomes (described as group 4 in Section 3), as well as a summary index comprised of all 12 birth outcomes in this table and in Panel A of Table 8. We see that the increase in very low birth weight births translates into a marginally significant 52.1 gram reduction in average birth weight, while the rise in very pre-term births materializes as a decline in average gestation length of about one

identifying coefficients on the other variables in the regression model.

quarter of a week.[38] We do not observe any significant change in the share of male births, implying that our results are unlikely to be biased by differential selection into birth due to heightened miscarriage rates. Importantly, the effect on the broader summary index remains statistically significant.

Mechanisms. To shed some light on the mechanisms driving the estimated effects on birth outcomes, we examine outcomes related to the mother's use of medical services during pregnancy and delivery in Panel B of Table 8. Column (1) indicates that women with a reported assault during pregnancy are 5 percentage points *more* likely to initiate prenatal care in the 1st trimester than their counterparts with a reported assault in the postpartum period. It is plausible that women who are assaulted early in the pregnancy may go to the doctor sooner than they otherwise would have to check on the health of the fetus. This finding further suggests that women who experience violence during pregnancy may engage in compensatory behaviors, making our impacts on birth outcomes lower bounds.

We also find a marginally significant negative effect on the likelihood of WIC receipt of 3.8 percentage points, or 5.8 percent at the sample mean (column 3). The decline in WIC take-up could arise for a variety of reasons, which we 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.[39] Women who are abused during pregnancy may fear going to a government program office (e.g., a WIC clinic) 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.[40] 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

[38] The likelihood of high birth weight also falls by 1.7 percentage points, which represents a 34.6 percent decline at the sample mean (marginally significant at the 10% level). 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.

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

[40] 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 suspicions 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.

programs.

Column (4) of Panel B shows a strong impact on induction of labor—assault during pregnancy is associated with a 5.5 percentage point increase in the likelihood of labor being induced, a 25.5 percent rise at the sample mean. However, when we analyze a summary index of all outcomes related to maternal use of medical services in column (7), we do not find a statistically significant estimate. This result is perhaps not surprising as some of the significant impacts on these outcomes go in opposite directions (i.e., the increase in 1st trimester prenatal care initiation is regarded as an increase in services, while the decline in WIC take-up is treated as a decrease).

Panel C of Table 8 examines mechanisms further by estimating model (3) using observable maternal pregnancy-related behaviors and well-being measures as outcomes. We do not find strong evidence of adverse impacts on these margins.

In Figures 3 through 6 we explore differences in impacts across various periods of exposure. 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 category is months 10-12 post-conception (i.e., the 3 months after the expected month of delivery).

The figures suggest that the impacts on very low birth weight, very pre-term, and low 1-minute Apgar score births, as well as induction of labor, are strongest for assaults in the 3rd trimester of pregnancy. Taken together, these results are indicative 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).

As another way of investigating the stress mechanism, we use mothers who experience any other crime in their home during or after pregnancy as an additional control group in a DD style model. The idea is that other crimes—such as burglary—are stressful events, but are less likely to involve direct physical harm to the mother when compared to an assault. Thus, evidence of a differential impact of exposure to assault would imply that the direct physical channel is important. 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 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 Table B.4 presents the results, which point to a significant adverse effect of assault during pregnancy on infant health, but no significant impacts of other crimes.[41]

Robustness checks. We conduct a number of robustness tests using the four summary indices as outcomes. To address concerns about possible differences in unreported assault rates across our treatment and control groups discussed in Section 4, we include women who have an assault in their home during months 1 through 10 *before* the conception month in Panel A of Table 9. Although the selection issues are arguably different across the two control groups of women with assaults before and after pregnancy, we continue to see a significant increase in the summary index of adverse birth outcomes.[42]

In Panel B of Table 9, we attempt to limit the possibility that unobservable differences between the treatment and control groups are driving our results by estimating 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—we nevertheless observe a large and statistically significant rise in the summary index of adverse birth outcomes.

The siblings sample also allows us to do a placebo test, in which we drop all mothers who ever experience an assault during pregnancy, and instead estimate maternal fixed effects models using an indicator for assault in months 10-19 post-conception (i.e., *after* pregnancy) as the treatment variable. If our main results were driven by unobservable differences across siblings around the period of childbirth that are correlated with exposure to assault (e.g., changes in maternal employment or family structure), then we would expect to see a significant spurious correlation between assault post-pregnancy and our pregnancy and birth

[41] We have also estimated our main regression model (3) using a sample of mothers who experience a burglary instead of an assault in their homes either during or after pregnancy. We do not find any significant effects of exposure to burglary on our outcome indices. Results available upon request.

[42] Additionally, under the assumpation that any difference in the reporting rate between women who are subject to an assault during pregnancy and those who are subject to an assault postpartum is similar to the difference in the reporting rate for other crimes across these two groups, then the results from our DD model (Appendix Table B.4) further assuage concerns about differential reporting rates biasing our results.

outcomes. Instead, Appendix Table B.5 shows insignificant treatment coefficients for both of our birth outcome indices (columns 1 and 4). The magnitudes of the coefficients are about one sixth of those reported in the analogous columns in Panel B of Table 9. We do observe a significant correlation with the maternal behavioral and well-being index in column (3), but it points to positive selection on these outcomes (i.e., mothers appear to engage in fewer adverse behaviors and have higher well-being when they experience an assault in the months after expected birth than around the time of their other births).

Lastly, Panel C of Table 9 investigates whether our results hold when we expand beyond our single-family home analysis sample. Here, we instead consider births by mothers who reside in *multi-family* homes with three floors or less in the Bronx, Brooklyn, and Queens, where the mother experienced an assault in her building during either 10 months post conception month or 10 months post expected month of delivery. The treatment variable in Panel C is the probability of assault during pregnancy, assuming that the probability is equal to the inverse of the number of units in the building. We continue to see an increase in the adverse birth outcome summary index in this larger and more heterogeneous sample. We also observe a decline in the use of medical services index—unlike in the single-family home sample, mothers in multi-family homes with a higher probability of assault during pregnancy are less likely to initiate pre-natal care in the first trimester than those with an assault in the building in the postpartum period.

6 Conclusion

Measuring the social cost 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 this cost is the assumption that all 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 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 large and significant increases in the rates of very low birth weight, very pre-term, and low 1-minute 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.

Our quasi-experimental results are remarkably similar to the observed descriptive relationship between violent victimization and birth outcomes, in spite of the documented strong negative selection among victims. However, we document 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 show 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 decline in WIC participation and no impacts on smoking or depression. We further find an increase in first trimester prenatal care initiation, which may indicate a compensatory behavioral response among victims. If women were unable to access such resources, the adverse consequences on birth outcomes could be larger.

What do our estimates imply for the measurement of the social cost of crime? We conduct a back-of-the-envelope calculation, focusing on the 1.7 percentage point increase in very low birth weight births. We consider the best available evidence on costs arising through six channels: higher rate of infant mortality, increased medical costs at and immediately following birth, increased costs associated with childhood disability, decreases in adult income, increased medical costs associated with adult disability, and reductions in life expectancy. These costs do not all immediately manifest at the time of the violent episode, and are likely to have been omitted in prior efforts to calculate the social cost of assault. Our calculation—presented in detail in Appendix A—generates an average social cost of $41,771 per assault during pregnancy, an estimate that is midway between the estimated costs of an assault in the cost of crime literature.[43] Figure 7 shows that this figure is largely driven by the higher likelihood of infant mortality and decreased life expectancy among very low birth weight children.

As noted in Section 2.3, prior work suggests that assaults create between $16,000 and $90,000 in social costs per victim depending on the methodological approach. To the extent that our research produces new evidence that has not previously been incorporated into the literature on the social cost of crime, our estimates indicate that the cost of assault to pregnant women needs to be scaled up by 262 percent for jury award estimates and by 46 percent for contingent valuation estimates. With an average of 3,177 pregnant women between 2004 and 2012 in New York City suffering physical abuse, total social costs previ-

[43]Considering the measurement error issues in our explanatory variables raised in Section 3, the actual cost may be even higher.

ously unaccounted for in New York City would exceed $132.7 million per year. Across the United States, we estimate an annual social cost in excess of $4.25 billion based on the best available nationwide victimization estimate for pregnant women, and the fact that there are approximately 3.9 million births per year.[44]

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 and society as a whole. 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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[44]Pooling across all participating states in PRAMS 2011 (AR, CO, DE, GA, HI, MA, MD, ME, MI, MN, MO, NE, NJ, NM, NY, OK, OR, and PA), 2.6 percent of respondents reported being physically hurt by their husband or partner during pregnancy.

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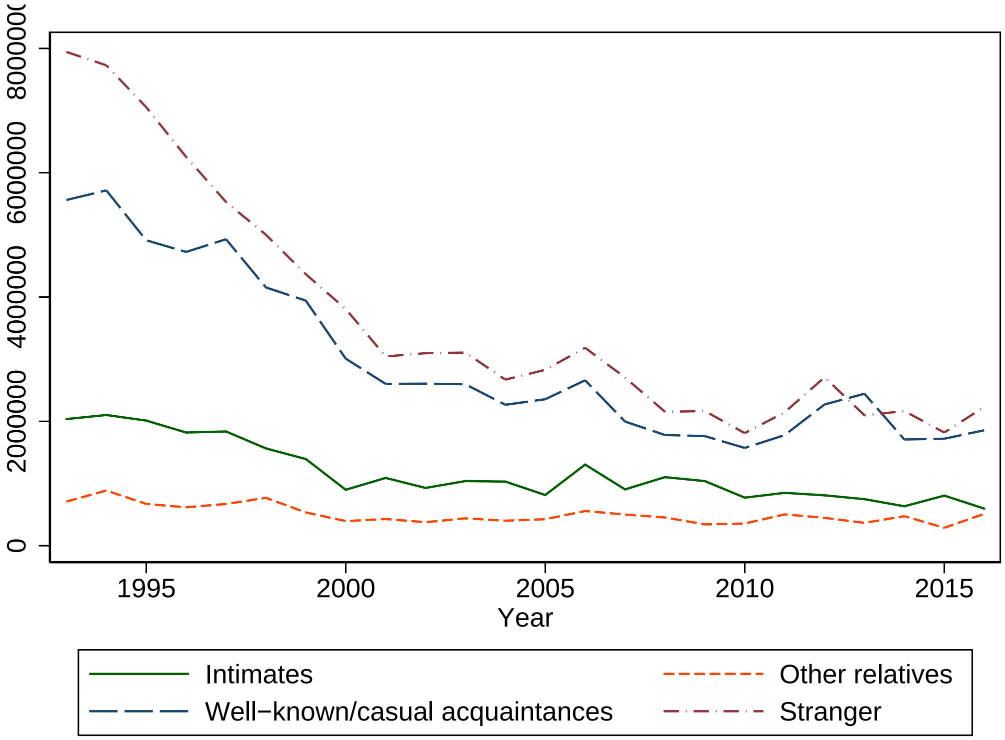
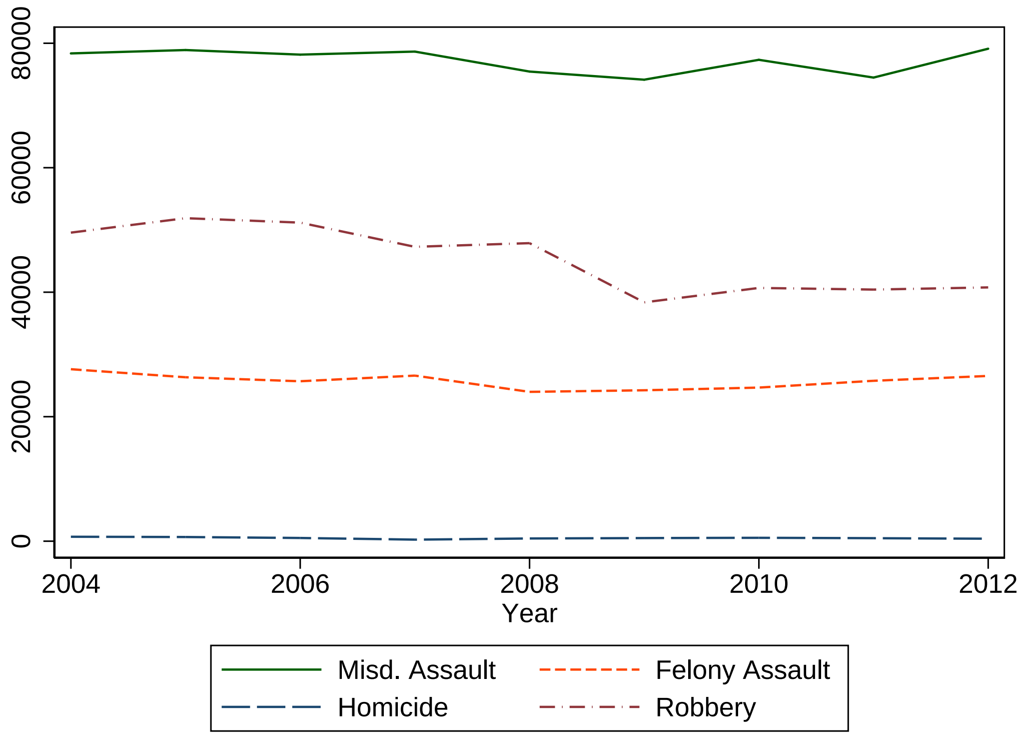
Figure 1: Trends in Violent Crimes by Victim-Offender Relationship in the National Criminal Victimization Survey



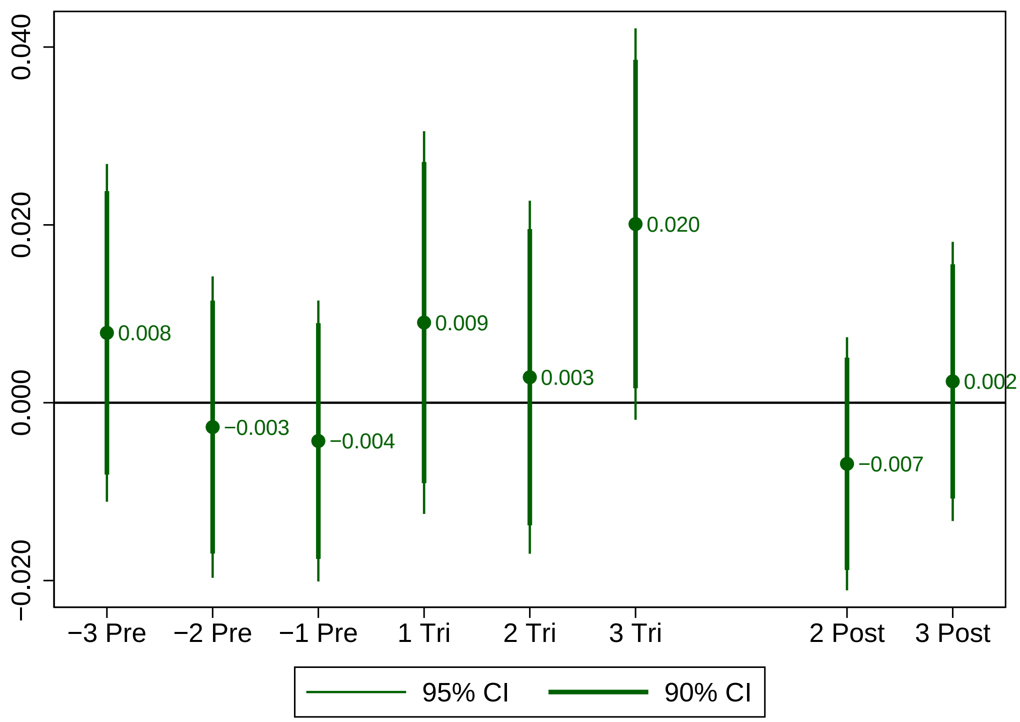
Notes: Authors calculations based on the Bureau of Justice Statistics National Crime Victimization Survey (1993-2016).

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



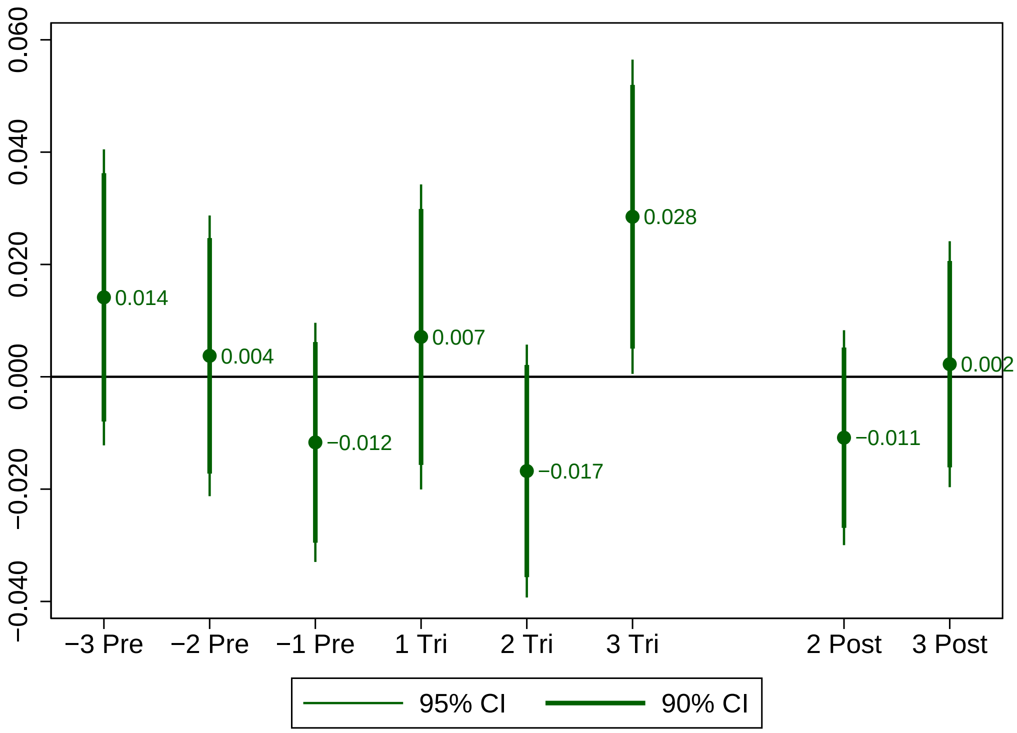
Notes: Authors' calculations based on administrative records from the New York Police Department.

Figure 3: Event Study: Very Low Birth Weight



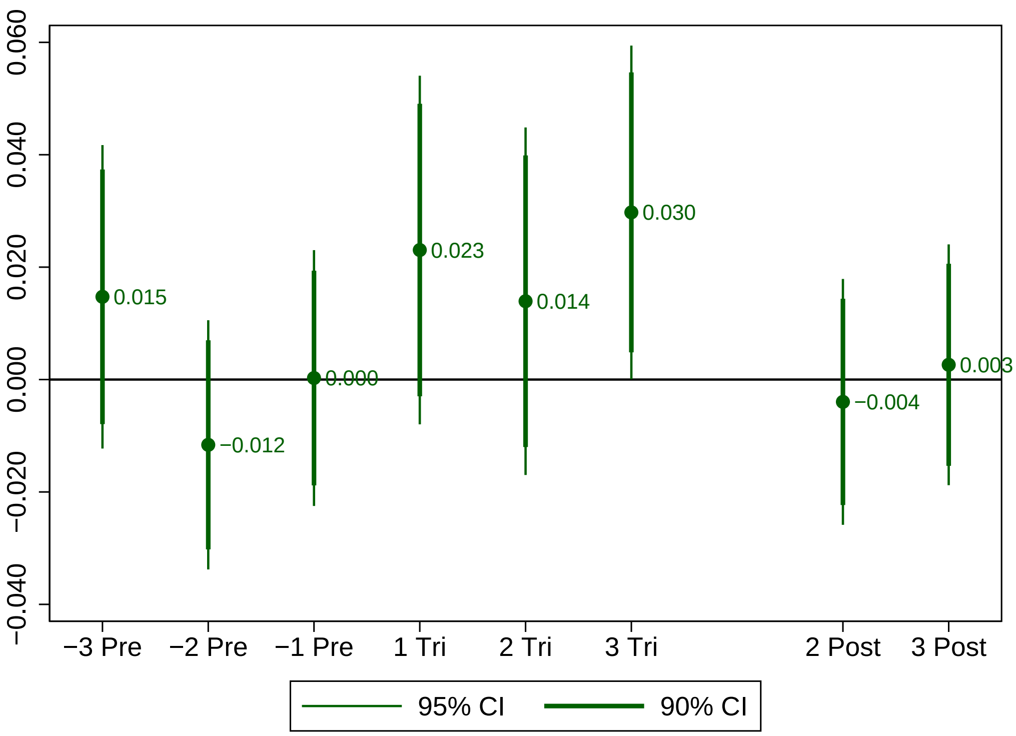
Notes: See notes under Table 8 for a description of the sample and control variables. This figure shows the coefficients and the corresponding 90 and 95% confidence intervals from event-study models that include indicators for any assault during the following windows: 8-10 months before conception month ("-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 Pre-Term



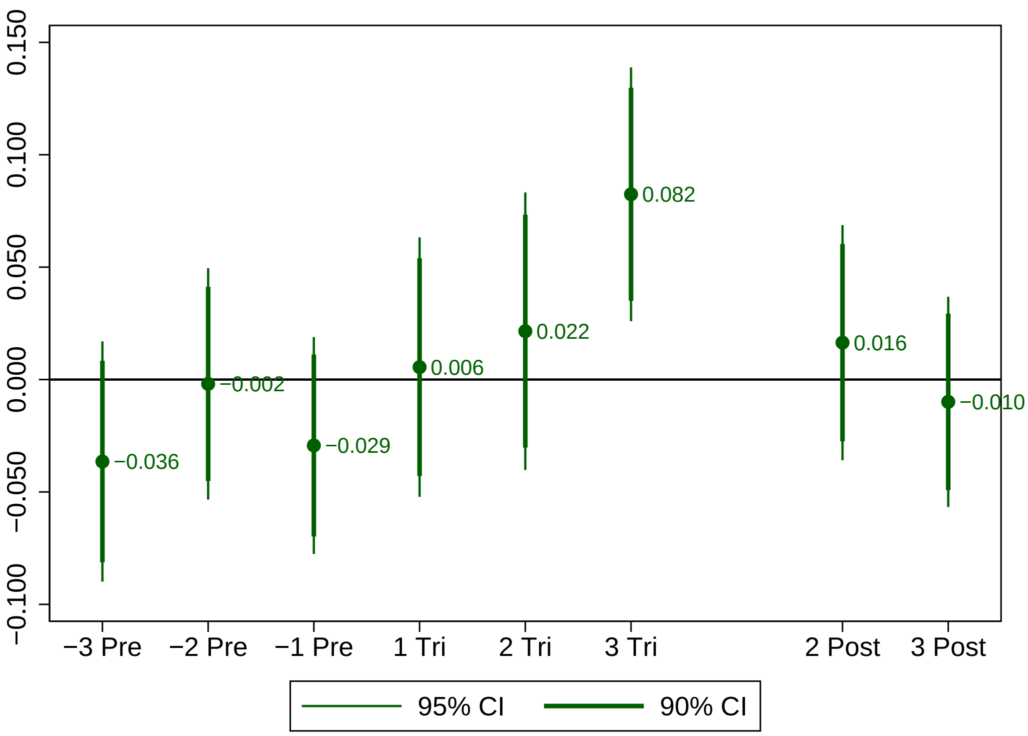
Notes: See notes under Table 8 for a description of the sample and control variables. This figure shows the coefficients and the corresponding 90 and 95% confidence intervals from event-study models that include indicators for any assault during the following windows: 8-10 months before conception month ("-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: Low 1-Min Apgar Score



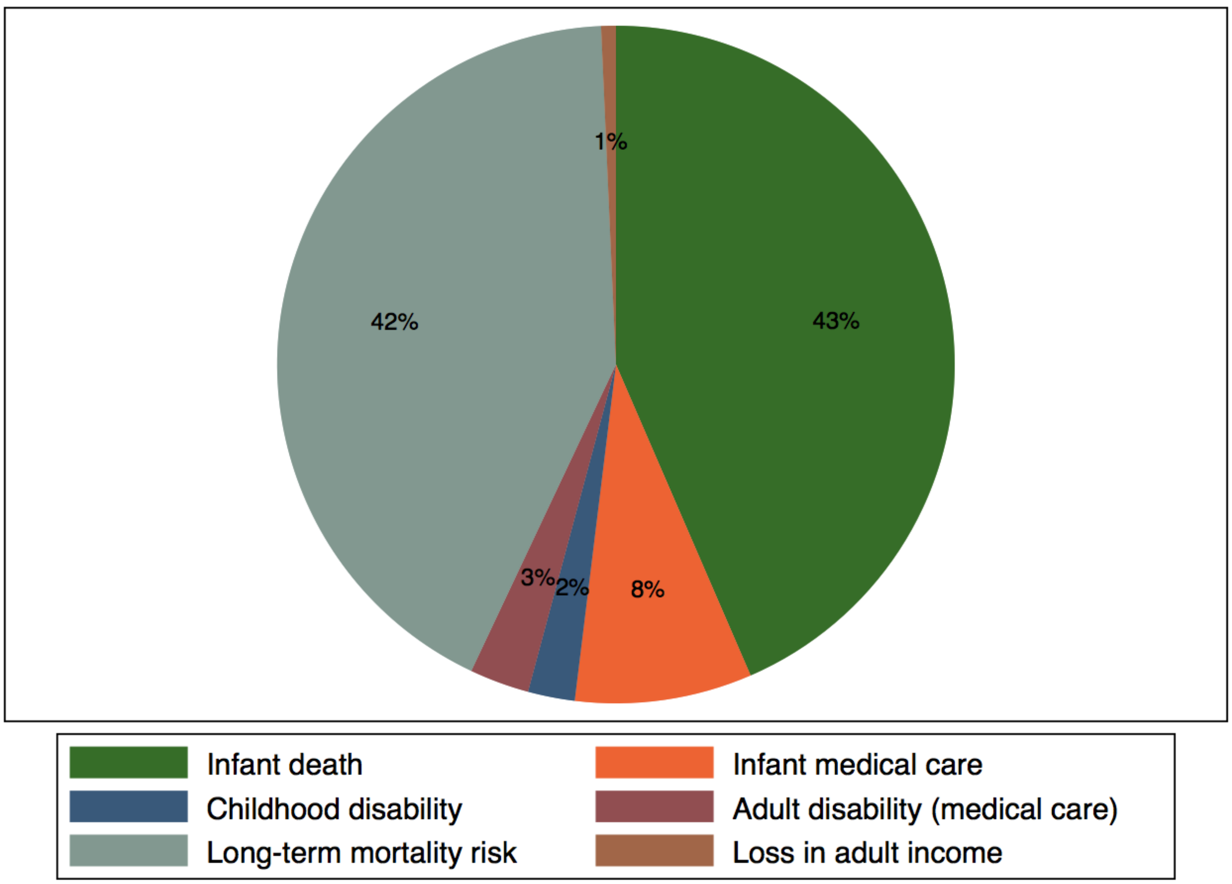
Notes: See notes under Table 8 for a description of the sample and control variables. This figure shows the coefficients and the corresponding 90 and 95% confidence intervals from event-study models that include indicators for any assault during the following windows: 8-10 months before conception month ("-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: Induction of Labor



Notes: See notes under Table 8 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: Distribution of the Total Social Cost of Assault by Source



Notes: We estimate the total social cost of assault during pregnancy at $41,771 in 2018 dollars. See Appendix A for details on this calculation.

Table 1: Examples of Economic Research Using Social Cost of Crime Estimates

Research Field	Illustrative Studies
Active Labor Market Programs	McConnell and Glazerman (2001); Redcross *et al.* (2012); Heller (2014)
Child Development	Belfield *et al.* (2006); Heckman and Masterov (2007); Currie and Tekin (2012)
Education	Lochner and Moretti (2004); Deming (2011)
Gun Regulation	Lott (1998); Lott and Whitley (2001); Donohue *et al.* (2017)
Justice and Law Enforcement	Levitt (1996); Evans and Owens (2007); Hjalmarsson (2009); Buonanno and Raphael (2013); Mueller-Smith (2015); Dobbie *et al.* (2018)
Media	Dahl and DellaVigna (2009)
Public Health	Carpenter and Dobkin (2011); Heaton (2012)
Urban Policy	Kling *et al.* (2005); Linden and Rockoff (2008); Cook and MacDonald (2011); Freedman and Owens (2011)

Notes: Studies listed in this table represent a non-exhaustive sample of economic research that uses social costs of crime estimates in the analysis.

Table 2: Common Estimates of the Social Cost 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 3: 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 4: 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 5: 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
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
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 6: 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
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

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 7: Association Between Assaults During Pregnancy and Maternal Characteristics, Main Analysis Sample

	(1) Age	(2) Mar	(3) For	(4) Wh	(5) Hsp	(6) Bl	(7) LowEd	(8) HighEd	(9) Any Risk	(10) 1st Par	(11) Sngle
Assault During	0.494*	0.0260	0.0349	-0.0117	0.0126	-0.00459	0.0194	-0.0208	0.0186	-0.0132	-0.00590
Pregnancy	[0.299]	[0.0223]	[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	0.511	0.107	0.261	0.489	0.577	0.418	0.630	0.466	0.978
Indiv. obs.	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 8: Effects of Assault During Pregnancy, Main Analysis Sample

A. Birth Outcomes

	(1) VLBW	(2) V Pret	(3) Low 1m Apg	(4) NICU	(5) Abn/Con	(6) Death	(7) Index
Assault During Pregnancy	0.0171** [0.00770]	0.0166* [0.00965]	0.0236** [0.01000]	0.0179 [0.0155]	0.0117 [0.0153]	0.00152 [0.00278]	0.0821** [0.0377]
Dept. var mean	0.0259	0.0424	0.0472	0.122	0.115	0.00310	0.0439
Indiv. obs.	1933	1933	1926	1933	1891	1933	1933

B. Use of Medical Services

	(1) PNC 1Tri	(2) NVis	(3) WIC	(4) Induc	(5) Csec	(6) Compl	(7) Index
Assault During Pregnancy	0.0478** [0.0223]	0.293 [0.181]	-0.0379* [0.0208]	0.0555*** [0.0194]	-0.000918 [0.0219]	-0.0147 [0.0153]	0.0321 [0.0204]
Dept. var mean	0.641	10.02	0.650	0.216	0.326	0.120	0.0153
Indiv. obs.	1891	1900	1924	1925	1933	1928	1933

C. Maternal Behaviors and Well-Being

	(1) Smoke	(2) Drugs	(3) Depr	(4) Low Wgt	(5) High Wgt	(6) Dad Miss	(7) Index
Assault During Pregnancy	0.00763 [0.00759]	-0.00400 [0.00403]	0.0188 [0.0282]	0.0229 [0.0154]	-0.0192 [0.0195]	0.0155 [0.0147]	0.0192 [0.0204]
Dept. var mean	0.0280	0.00745	0.276	0.115	0.217	0.132	0.00377
Indiv. obs.	1930	1880	1114	1910	1910	1933	1933

Notes: Each coefficient in each panel is from a separate regression. The outcomes in Panel A are indicators for: very low birth weight, very pre-term birth, low 1-minute APGAR score, NICU admission, any abnormal conditions or congenital anomalies of the newborn, death. The outcomes in Panel B are: indicator for first trimester prenatal care initiation, number of prenatal care visits, indicator for WIC take-up, indicator for induction of labor, indicator for delivery by c-section, indicator for any complications during labor or delivery. The outcomes in Panel C are indicators for: mother smoking during pregnancy, mother using illegal drugs during pregnancy, mother being depressed, too low weight gain, too high weight gain, father information missing from the birth certificate. In each panel, the last column reports results using an index outcome, which is an equally weighted average of z-scores for the outcomes reported in the panel. See text for more details. 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 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. Significance levels: * $p<0.1$ ** $p<0.05$ *** $p<0.01$

Table 9: Effects of Assault During Pregnancy on Summary Index Outcomes, Robustness

A. Include Mothers with Assaults Before Pregnancy

	(1) Birth Out	(2) Med Serv	(3) Behav/Well	(4) Broad Birth Out
Assault During Pregnancy	0.0681** [0.0345]	0.0295 [0.0179]	0.0142 [0.0174]	0.0525* [0.0275]
Dept. var mean	0.0466	0.0118	-0.000315	0.0318
Indiv. obs.	2758	2758	2758	2758

B. Maternal Fixed Effects Models

	(1) Birth Out	(2) Med Serv	(3) Behav/Well	(4) Broad Birth Out
Assault During Pregnancy	0.293** [0.134]	-0.0437 [0.0575]	-0.00579 [0.0598]	0.185* [0.105]
Dept. var mean	0.00377	0.000144	0.00248	0.00294
Indiv. obs.	18600	18600	18600	18600

C. Multi-Family Home Sample

	(1) Birth Out	(2) Med Serv	(3) Behav/Well	(4) Broad Birth Out
Prob. of Assault During Pregnancy	0.0375* [0.0225]	-0.0328** [0.0148]	0.00684 [0.0149]	0.0347* [0.0190]
Dept. var mean	0.00468	-0.000740	-0.00102	0.00431
Indiv. obs.	22191	22191	22191	22191

Notes: Each coefficient in each panel is from a separate regression. The outcomes are four summary indices: birth outcomes index, use of medical services index, maternal behaviors and well-being index, and a broader birth outcomes index. See text for more details. In Panel A, the sample is limited to births by mothers who reside in single-family homes in the Bronx, Brooklyn, and Queens with conception years 2004-2012, 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. In Panel B, the 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. In Panel C, the sample is limited to births by mothers who reside in multi-family homes with 3 floors or less in the Bronx, Brooklyn, and Queens with conception years 2004-2012, where the mother experienced an assault in her building during either 10 months post conception month or 10 months post expected month of delivery. The treatment variable in Panel C is the probability of assault, assuming that the probability is equal to the inverse of the number of units in the building. Controls and standard errors in Panels A and C are the same as in Table 8. Panel B 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). Panel B regressions also control for conception year, conception month, and mother fixed effects, and the standard errors are clustered on the mother.

Significance levels: * p<0.1 ** p<0.05 *** p<0.01

A Calculating the Social Cost of Assault on Pregnant Women

This appendix presents the details of our calculation of the social cost of assault on pregnant women. We use our estimated 1.7 percentage point increase in the likelihood of a very low birth weight birth as the starting point (see Table 8). This adverse infant health outcome is associated with increased costs through numerous channels, including: higher rate of infant mortality, increased medical costs at and immediately following birth, increased costs associated with childhood disability, decreases in adult income, increased medical costs associated with adult disability, and reductions in life expectancy. The details are presented below:

Channel	**Estimate**	**Source**
(1) Cost due to infant death =	$1,068,682	
Change in infant mortality per VLBW ×	0.206	Matthews *et al.* (2015)[1]
Cost of infant mortality	$5,184,000	Cutler and Meara (2000)[2]
(2) Infant medical care cost	$207,739	Rogowski (1998)[2]
(3) Childhood disability cost =	$54,900	
Change neurosensory disability per VLBW ×	0.10	Hack *et al.* (2002)
Cost of childhood disability (18 years)	$549,000	Stabile and Allin (2012)[2]
(4) Cost due to reduction in adult income =	$17,185	
Average lifetime income ×	$520,753	American Communities Survey[3]
Percent income loss from VLBW	0.033	Bharadwaj *et al.* (2018)
(5) Cost of adult disability (medical care) =	$69,822	
Change adult disability per VLBW ×	0.10	Hack *et al.* (2002)
Cost of adult disability medical care (ages 19 to 67)	$698,220	Anderson *et al.* (2010)[2]
(6) Cost of long-term mortality risk =	$1,038,786	
Average change in life expectancy ×	11.6	Bharadwaj *et al.* (2018)[4]
Statistical value of year of life	$89,551	Lee *et al.* (2009)[2]
Estimated cost of assault during pregnancy		
$\underbrace{[0.017]}_{\Delta^{\text{VLBW}}} \times \underbrace{[(1)+(2)+(3)+(4)+(5)+(6)]}_{\text{Cost}^{\text{VLBW}}} =$	**$41,771**	

[1] We conservatively assume that in the absence of the assault, the victims would face the infant mortality risk associated with low birth weight but not very low birth weight babies (i.e., birth weights in the range 1,500-2,499g).

[2] Dollar amounts have been inflation adjusted to 2018 values using the US consumer price index.

[3] In order to calculate the present discounted value of lifetime earnings, we sum over the distribution of earnings from ages 16 to 64 in the 2017 American Communities Survey, and assume that earnings are discounted at a 3 percent real rate (i.e., a 5 percent discount rate with 2 percent wage growth) back to age zero.

[4] We use the Social Security Administration's Period Life Table from 2015, and multiply the probability of death in each year of life following the first year by 2.8 based on the estimate from Bharadwaj *et al.* (2018). We exclude the first year to avoid double counting the impacts to infant mortality. We calculate the changes in life expectancy separately for men (12.3 years) and women (10.9 years), and then average them.

B Additional Results

Appendix Table B.1: Estimated OLS Relationship Between Assaults During Pregnancy and Maternal Characteristics, Sample Includes Mothers with No Assaults

	(1) Age	(2) Mar	(3) For	(4) Wh	(5) Hsp	(6) Bl	(7) LowEd	(8) HighEd	(9) Any Risk	(10) 1st Par	(11) Sngle
Assault During	-2.698***	-0.285***	-0.0156	-0.185***	0.0848***	0.194***	0.208***	-0.212***	-0.0197	0.0273	0.0132**
Pregnancy	[0.215]	[0.0163]	[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	0.533	0.302	0.169	0.295	0.376	0.621	0.661	0.419	0.961
Indiv. obs.	68397	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

Appendix Table B.2: Estimated OLS Relationship Between Assault During Pregnancy and Summary Index Outcomes, Sample Includes Mothers with No Assaults

	(1) Birth Out	(2) Med Serv	(3) Behav/Well	(4) Broad Birth Out
Assault During Pregnancy	0.0842***	0.0376**	0.0265*	0.0652***
	[0.0308]	[0.0149]	[0.0144]	[0.0245]
Dept. var mean	-0.0132	-0.0264	-0.0891	-0.0163
Indiv. obs.	68234	68234	68234	68234

Notes: The outcomes are four summary indices: birth outcomes index, use of medical services index, maternal behaviors and well-being index, and a broader birth outcomes index. See text for more details. 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 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.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Appendix Table B.3: Effects of Assault During Pregnancy on Additional Birth Outcomes, Main Analysis Sample

	(1) Birwt	(2) LBW	(3) HBW	(4) Gest	(5) Pret	(6) Male	(7) Index
Assault During Pregnancy	-52.05*	0.0137	-0.0174*	-0.253**	0.00688	-0.0225	0.0609**
	[28.73]	[0.0140]	[0.00989]	[0.122]	[0.0148]	[0.0237]	[0.0304]
Dept. var mean	3154.1	0.105	0.0497	38.37	0.113	0.499	0.0313
Indiv. obs.	1933	1933	1933	1933	1933	1933	1933

Notes: The outcomes are: birth weight in grams, indicator for low birth weight, indicator for high birth weight, gestation in weeks, indicator for pre-term birth, indicator for male child. The last column reports results using an index outcome, which is an equally weighted average of z-scores for the outcomes reported in this table as well as in Panel A of Table 8. See text for more details. 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 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.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Appendix Table B.4: "Difference-in-Difference" Effects of Assault During Pregnancy on Summary Index Outcomes, Relative to All Other Crimes

	(1) Birth Out	(2) Med Serv	(3) Behav/Well	(4) Broad Birth Out
Assault During Pregnancy	0.0810**	0.0184	0.0144	0.0555*
	[0.0377]	[0.0206]	[0.0206]	[0.0303]
Any Assault During or Post Pregnancy	-0.00951	0.0187	-0.00516	-0.00472
	[0.0223]	[0.0141]	[0.0146]	[0.0182]
Any Crime During Pregnancy	0.0107	0.00297	-0.00203	0.00865
	[0.0132]	[0.00827]	[0.00791]	[0.0110]
Dept. var mean	0.00736	-0.0206	-0.0400	0.00381
Indiv. obs.	11226	11226	11226	11226

Notes: The outcomes are four summary indices: birth outcomes index, use of medical services index, maternal behaviors and well-being index, and a broader birth outcomes index. See text for more details. 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 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.

Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Appendix Table B.5: Placebo Effects of Assault Post Pregnancy, Maternal Fixed Effects Models

	(1) Birth Out	(2) Med Serv	(3) Behav/Well	(4) Broad Birth Out
Any Assault Post Pregnancy	0.0521	-0.00162	-0.168**	0.0330
	[0.126]	[0.0651]	[0.0764]	[0.0984]
Dept. var mean	0.000142	-0.000764	-0.00232	-0.00102
Indiv. obs.	18107	18107	18107	18107

Notes: The outcomes are four summary indices: birth outcomes index, use of medical services index, maternal behaviors and well-being index, and a broader birth outcomes index. See text for more details. The 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. We drop mothers who experience an assault in their home during any pregnancy. 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, and the standard errors are clustered on the mother.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01