

# Violence While in Utero: The Impact of Assaults During Pregnancy on Birth Outcomes\*

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## Abstract

We study the effects of prenatal exposure to violent crime on infant health, using New York City crime records linked to mothers' addresses in birth records data. We address endogeneity of assault exposure with three strategies and find that *in utero* assault exposure significantly increases the incidence of adverse birth outcomes. We calculate that the annual social cost of assault during pregnancy in the US is more than \$3.8 billion. Since infant health predicts long-term wellbeing and disadvantaged women are disproportionately likely to be domestic abuse victims, violence *in utero* may be an important channel for intergenerational transmission of inequality.

**Keywords:** domestic violence, infant health, victimization, social cost of crime

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

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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 (e.g., Becker, 1968; Freeman, 1999; Chalfin and McCrary, 2015) and the impacts of criminal sanctions on offenders (e.g., Buonanno and Raphael, 2013; Aizer and Doyle, 2015; Mueller-Smith, 2015; Bhuller *et al.*, 2016; Dobbie *et al.*, 2018; Agan and Starr, 2018) less is known about the causal effects of crime on *victims*. Estimates of the social cost of crime rely on jury awards (Miller *et al.*, 1996) or contingent valuation studies (Cohen *et al.*, 2004), both of which assume that the impacts of victimization are fully understood. Although these methods yield a wide range of estimates,<sup>1</sup> they serve a critical role in policy evaluation. Appendix Table A.1 lists examples of studies in which cost of crime estimates are used to evaluate a variety of interventions. Clearly, improving estimates of the costs of crime on victims can help to inform cost-benefit analyses of a wide range of policies, both within and outside of the criminal justice system.

Intimate partner violence (IPV) is a crime that is endemic in countries all around the world (Campbell *et al.*, 2004; Devries *et al.*, 2010), and accounts for over one seventh of all violent crimes in the United States (see Appendix Figure A1.a). Economists have studied the determinants of domestic violence from the perspective of household bargaining models (e.g., Lundberg and Pollak, 1993; Stevenson and Wolfers, 2006; Aizer, 2010), but 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 damaging as it can also affect the unborn child. The reported prevalence of physical or sexual abuse among pregnant and postpartum women ranges between 7 and 23 percent (Johnson *et al.*, 2003; Charles and Perreira, 2007; Chambliss, 2008), and IPV-related homicide is a leading cause of death during pregnancy

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<sup>1</sup>See Section 2 for a description of these methods. The most influential studies in this area differ by a factor of up to 22.

(Palladino *et al.*, 2011).

Estimating the causal effects of criminal victimization is challenging. While administrative data on alleged offenders (with personal identifying information) is readily available through arrest and incarceration databases, victim identities are generally withheld for confidentiality reasons.<sup>2</sup> Research on crime victims is thus typically limited to self-reports in surveys, such as the National Crime Victimization Survey, which may be subject to non-random measurement error or recall bias (Ellsberg *et al.*, 2001). And victimization—especially due to violent crime—is not a random event. 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 by race and ethnicity (Lauritsen and White, 2001), and by mental health status (Desmarais *et al.*, 2014). It is therefore difficult to isolate the causal effects of experiencing a violent crime from the influence of other (often unobservable) factors.

This paper attempts to overcome these challenges in order to offer new evidence on how violent crime affects some of the most vulnerable members of society—pregnant women and their 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.

We use three different empirical strategies, each of which relies on different identifying assumptions. First, we compare the outcomes of women who report an assault in the nine months after conception to those who experience an assault one to ten months after the estimated due date.<sup>3</sup> This comparison relies on the assumption that the determinants of IPV are similar during the pregnancy and postpartum periods, which is consistent with the

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<sup>2</sup>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.

<sup>3</sup>This 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

empirical literature (Charles and Perreira, 2007; Bailey, 2010). This assumption is supported by balancing tests for differences in maternal and paternal characteristics across the treatment and control groups, smoothness in sample density around the expected birth date, and a supplementary analysis of parental characteristics and family dynamics during the pregnancy and postpartum periods using data from the Fragile Families and Child Wellbeing Study. We also use a calculation proposed by Oster (2017) to investigate the extent to which these estimates may be biased by differential selection into IPV during the pregnancy and postpartum periods.

Second, we estimate a difference-in-differences (DD) model, in which 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 report any other type of crime during those two time periods. The DD model allows for there to be a difference in reporting rates between women who experience an assault during pregnancy and women who experience one postpartum, but assumes that this difference is similar to any difference in the reporting rate for other crimes.

Third, we estimate models with maternal fixed effects, which account for any time-invariant differences across mothers who do and do not experience an assault during pregnancy. These models compare two pregnancies of the same mother, where one was affected by assault and the other was not. Thus, these estimates focus exclusively on the pregnancy period and do not involve comparisons with IPV exposure after pregnancy. Put differently, whereas the control groups in the first two strategies contain mothers who may still be experiencing violence, the control group in the fixed effects model is not experiencing violence. Hence, one might expect the estimated effects of assault during pregnancy to be larger in this specification. Consistent with this prediction, the point estimates in the maternal fixed effects design are larger than those obtained using the first two strategies. The siblings sample also allows us to do a placebo test, in which we drop all mothers who ever experienced maternal bereavement on children's outcomes.

an assault during pregnancy, and instead estimate maternal fixed effects models using an indicator for assault in months one to ten *after* the estimated due date as the treatment variable. We do not find any evidence of placebo impacts on infant health outcomes, suggesting that the results from our baseline maternal fixed effects models are not confounded by unobservable differences across siblings around the period of childbirth that are correlated with exposure to assault.

All three strategies indicate that experiencing assault during pregnancy has adverse consequences for infant health. Focusing on the relatively conservative estimates from the “pregnancy vs. postpartum assault exposure” and DD strategies (which are remarkably similar), mothers with an assault during pregnancy have at least a 0.08 standard deviation (SD) higher summary index of severe adverse birth outcomes, compared to mothers who report an assault in the postpartum period and mothers who experience any other crime during either period. This result is driven by 1.5 and 2.1 percentage point (61 and 46 percent) higher rates of very low birth weight births (less than 1,500 grams) and low 1-minute Apgar score births, respectively.<sup>4</sup> These impacts stem mainly from assaults in the 3rd trimester of pregnancy. These assaults are also associated with a higher probability of induced labor, a likely medical response to injuries sustained by pregnant victims of abuse. Indeed, we find some evidence that victims of assault during pregnancy are generally more likely to use medical services.

We conduct a back-of-the-envelope calculation to provide an estimate of the average social cost of assault during pregnancy to infants. We use the conservatively estimated 1.5 percentage point increase in the likelihood of a very low birth weight birth, and account for six types of short and long-term costs: higher rates of infant mortality, increased medical costs during and immediately following birth, increased childhood disability, increased adult

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<sup>4</sup>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>.

disability, decreases in adult income, and reductions in life expectancy. We calculate an average social cost of at least \$36,857 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 to affected infants of at least \$3.8 billion in the U.S. alone. The cost becomes even larger if one applies the estimate we obtain from maternal fixed effects models (\$85,999 per assault; \$8.9 billion per year), which compare affected to unaffected pregnancies, as discussed above.

Importantly, our cost calculation is for affected infants and not mothers. Santos (2013) uses British survey data and models the cost of domestic violence as compensating variation in a life satisfaction regression, producing a cost estimate ranging from £27,000 (\$43,200 in 2013) to £70,000 (\$112,000 in 2013). In ongoing work, Bindler and Ketel (2019) use Dutch administrative data to study the impacts of criminal victimization in an event-study framework, finding that victims experience large reductions in earnings and increases in public benefit receipt that last up to 8 years following the crime. The full cost of IPV during pregnancy includes these costs to mothers in addition to the costs to infants, who are the focus of our paper.

Prior research suggests that shocks in the fetal period can have life-long consequences for adult health, human capital, and labor market outcomes (Almond *et al.*, 2018; Aizer and Currie, 2014; Barker, 1990). Since poor pregnant women are much more likely to be victims of assault than their more advantaged counterparts (Jewkes, 2002; Aizer, 2011), and since 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 for the persistence of economic inequality across generations.

## 2 Background

**Relationship of Our Work to Previous Research on IPV and Violence during Pregnancy.** Prior studies, which compare women who experience IPV during pregnancy to those who do not, document a negative association between prenatal IPV and pregnancy and birth outcomes (see, e.g., Newberger *et al.*, 1992; Murphy *et al.*, 2001; Campbell, 2002; Silverman *et al.*, 2006, among others).<sup>5</sup> To the best of our knowledge, only one other paper has addressed non-random selection into IPV in order to identify the impacts of IPV on infant health: Aizer (2011) uses a control function approach with linked hospitalizations and births data from California to estimate the effect of hospitalization for assault during pregnancy. In her study, variation in IPV comes from geographic and time variation in the enforcement of laws against domestic violence.<sup>6</sup> She finds that hospitalization for assault during pregnancy is associated with a 163 gram reduction in birth weight.

We build on this path-breaking research in three ways: First, we include all assaults reported to the police instead of focusing only on those resulting in hospitalization. Second, we develop several alternative research designs. Third, in addition to birth outcomes, we examine the use of medical interventions and prenatal behaviors in an attempt to understand the mechanisms driving the estimated effects on infant health.

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<sup>5</sup>There is also a small literature on interventions targeting IPV during pregnancy that have been evaluated using randomized controlled trials and suggest that interventions that lower IPV improve birth outcomes (Kiely *et al.*, 2010; El-Mohandes *et al.*, 2011). These studies are limited by small sample sizes: e.g., Kiely *et al.* (2010) report that out of the 150 pregnant women exposed to IPV in their treatment group, only one had a very low birth weight birth.

<sup>6</sup>Specifically, Aizer (2011) uses the ratio of arrests for domestic violence to the number of 911 calls reporting domestic violence in the previous year as an instrument for hospitalization for assault.

We also contribute to the literature on the relationship between violence—either due to local criminal activity or due to more global events such as wars and terrorist attacks—and infant health (see, e.g., Berkowitz *et al.*, 2003; Mansour and Rees, 2012; Torche and Villarreal, 2014, among others). These studies typically measure potential exposure rather than actual victimization, and focus on maternal stress during pregnancy as an important channel through which potential exposure to violence can affect infant health. Our estimates instead speak to the direct consequences of violent crime on victims.

**Intimate Partner Violence in New York City and Beyond.** Intimate partner violence is shockingly common. One study from the World Health Organization, which used data from twelve countries, reports that women’s lifetime risk of physical or sexual violence ranges from 15 percent (Japan) to 71 percent (Ethiopia) (Garcia-Moreno *et al.*, 2005). Prevalence rates of IPV during pregnancy have been found to be up to 57 percent, with typical risk for women in industrialized countries ranging between 3.4 and 11 percent (Campbell *et al.*, 2004; Devries *et al.*, 2010).

In the United States, recent estimates from the National Intimate Partner and Sexual Violence Survey by the Centers for Disease Control and Prevention indicate that 32 percent of U.S. women experience physical IPV at some point in their lifetimes (Smith *et al.*, 2017). As shown in Appendix Figure A1.a, violent crime where the perpetrator is a stranger has gone down substantially from 1993 to 2016. Instead, violent crime most commonly occurs between two individuals with a known relationship (intimate partners, other relatives, or acquaintances).

In New York City, surveys show that about 69,000 adult women feared IPV in 2004-2005 (New York City Department of Health and Mental Hygiene, 2008). Administrative records indicate that women in their peak childbearing ages of 20 to 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 visits, and that Black and Hispanic women are



at greater risk than their non-Hispanic white counterparts (New York City Department of Health and Mental Hygiene, 2008).

As previously noted, reported prevalence rates of physical or sexual abuse among pregnant and postpartum women range between 7 and 23 percent, with more recent studies documenting relatively higher rates. Thus, pregnant women and their unborn children represent a significant fraction of violent crime victims. Newberger *et al.* (1992) point out that violence during pregnancy can affect infant health through blunt trauma to the maternal abdomen, which in turn can result in early onset of labor due to placental abruption, or other complications such as the rupture of the mother's uterus. There may also be indirect channels, including elevated stress, exacerbation of existing chronic illnesses, changes in access to prenatal care or other services, and engagement in adverse behaviors such as smoking or poor nutrition as a coping mechanism.

## 2.1 Police Responses to Domestic Violence in New York City.

Since we use police reports to measure crimes, 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.<sup>7</sup>

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<sup>7</sup>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. While statistics specific to domestic violence incidents are unavailable, 67 percent of felony and misdemeanor assault suspects were arrested in 2017 (O'Neill, 2018). 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, and whether to release the defendant (with or without bail). According to NYS Division of Criminal Justice Services (2018),

State law breaks domestic violence into three distinct categories depending on its severity. 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. In this study, we focus specifically on reported instances of misdemeanor and felony assaults (including aggravated assaults) and exclude violations because less serious offenses have lower reporting rates than misdemeanor and felony assaults (Morgan and Kena, 2017).

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. New York state has had a “mandatory arrest” law since the passage of the Family Protection and Domestic Violence Intervention Act in 1994. This law implies that police officers must make an arrest when there is probable cause of either a felony or a misdemeanor offense committed by one “member of the same family or household” against another. The fact that felonies and misdemeanors are treated similarly by the police is another reason to exclude violations, where the officer has more discretion about whether to make an arrest.<sup>8,9</sup>

between 2013 and 2017, 23 percent of violent felony arrests were convicted and sentenced to incarceration. New York state does not report separate estimates for domestic violence offenses or statistics on the length of sentence.

<sup>8</sup>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.

<sup>9</sup>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 vio-

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 cases that were reported to the NYPD, and not only cases that resulted in a conviction, means that we have a more representative sample of assaults during pregnancy than some previous studies. However, issues of mis-measurement and under-reporting of IPV may still exist in our data, as discussed in Sections 3 and 4, as well as in Appendix C.

**Estimating the Social Costs of Crime.** Attempts to measure the social cost of crime date back to at least the Wickersham Commission on Law Observance and Enforcement (Anderson *et al.*, 1931). Costs include (but are not limited to) property loss or destruction, the administrative costs of the justice system, victims' mental and physical health, and victims' potential lost productivity.<sup>10</sup> Quantifying these impacts in terms of a common unit of measurement (e.g., dollars) is not trivial, but is crucial for evaluating policy. In fact, many economic analyses use such estimates to assess the potential cost-effectiveness of public programs (see Appendix Table A.1).

Several strategies have been developed to meet this need (Cohen, 2005).<sup>11</sup> The "cost of illness" tradition (Hodgson and Meiners, 1982; Malzberg, 1950) attempts to quantify the 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 victim self-reports. The jury-award approach measures the social cost of crime using actual compensation awards from civil personal injury cases (Miller *et al.*, 1996).

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  
lence against immigrants in our data.

<sup>10</sup>See Table 9B.2 in Donohue (2009) for an extensive discussion of potential costs of crime.

<sup>11</sup>Soares (2015) provides a review of these methods for an economic audience and discusses how the approaches measure inherently different theoretical parameters.

housing prices. Contingent valuation studies use a similar logic: Surveys ask respondents about their willingness to pay to avoid being the victim of various crimes, which theoretically provides a measure of both tangible and intangible costs (Cohen *et al.*, 2004; Cook and Ludwig, 2000).<sup>12</sup> All of these methods assume that the impacts of crime are fully known. If the impact is unknown to the researcher, a jury, a home buyer, or a survey respondent, then these estimates of the social cost of crime will be biased towards zero. Conversely, if people have exaggerated fears of crime, then survey methods will overstate the cost of crime.

Appendix Table A.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 other offenses. 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. Our analysis aims to provide new evidence on the cost of assault during pregnancy on infant health outcomes, which are typically omitted from existing calculations. We discuss the costs associated with our estimated impacts on affected infants' health in Section 6.

### 3 Data

We merge three restricted administrative data sets from New York City: the universe of reported crimes between 2004 and 2012, the universe of birth records, and a building characteristics database. We merge these data sets using information on exact crime locations, maternal residential addresses, and unique building identifiers, as described in detail in Appendix B.

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<sup>12</sup>Often cost strategies are complemented with estimates from the statistical value of life literature, which relies on a compensating wage differences (Viscusi and ALDY, 2003).

**Crime data.** The crime data come from NYPD records. These data cover all criminal complaints reported between 2004 and 2012.<sup>13</sup> Each record includes the exact longitude and latitude of the event, the date and time, the degree of the offense, and a categorical description of the nature of the offense. The incidents represent the full universe of reported crimes in New York City over the study period, whether the cases proceeded further or not. Appendix Table A.3 demonstrates that close to one-fifth of these reported crimes were violent in nature.

Appendix Figure A.1b shows 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, remained stable at close to 110,000 offenses per year.

**Births data.** Births data come from administrative records held by the New York City Department of Health and Mental Hygiene’s Office of Vital Statistics. These data include detailed information about both the child and the parents.<sup>14</sup> We observe 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

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<sup>13</sup>Sexual assaults are not included in 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. We were permitted to access only a subset of the variables contained in the data system, which was less than what is publicly available in geospatially coarsened data (see <https://data.cityofnewyork.us/Public-Safety/NYPD-Complaint-Data-Historic/qgea-i56i>).

<sup>14</sup>These data come from two sources: medical information 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.

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 child-birth, including the date of prenatal care initiation and the total number of prenatal care visits, whether the mother received Special Supplemental Program for Women, Infants, and Children (WIC) benefits, whether the mother smoked before or during pregnancy, whether the mother used any illicit drugs during pregnancy, maternal pregnancy weight gain, and whether the mother reports being depressed during pregnancy.<sup>15</sup>

These data include 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 the following information about fathers: age, education level, race/ethnicity, and nativity.<sup>16</sup> The births data include maternal residential addresses, full maiden names, and dates of birth, which allow us to match mothers to crimes occurring in their homes, and also to match siblings born 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.<sup>17</sup>

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<sup>15</sup>Information about depression during pregnancy is only available from 2008 onward.

<sup>16</sup>About 14 percent of the observations in our sample are missing all father information from the birth certificate. We create indicators for missing paternal characteristics which we include in our regression models in order to retain observations with missing father information. There is no statistically significant difference in the likelihood of missing father information between our treatment and control groups (see Table 2).

<sup>17</sup>There are two gestation length variables available in the vital statistics data. The first is based on the conventional computation of gestation using the date of the last menstrual period. This variable is at times missing or may be measured with error, so the records also include a clinical estimate of gestation, which takes account of all of the information available to the clinician (e.g., ultrasound measurements). We use this latter measure.

**Building characteristics data.** Building characteristics come from the NYC Department of City Planning’s (NYC DCP) Primary Land Use Tax Lot Output (PLUTO) data. The PLUTO data include 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.

**Analysis sample and summary statistics.** After merging these three data sets (see Appendix B), we make the following additional restrictions to the sample for our main analysis. First, we focus on mothers who reside in single-family homes because for them, we can be sure that a crime occurred in their home (rather than in another apartment in the same building). Second, we consider mothers residing in The Bronx, Brooklyn, or Queens, leaving us with 68,704 observations. We drop mothers in Manhattan since there are few who reside in single-family homes, and we drop mothers in Staten Island because they are less comparable to mothers in the other boroughs in terms of their demographic and socioeconomic characteristics.<sup>18</sup>

To create the analysis sample for our first estimation strategy, we identify women for whom the first reported (misdemeanor or felony) assault that we observe occurred in the month of conception or in the following 9 months, and women for whom the first such assault occurred in months 10 through 19 post-conception (i.e., the months following the expected due date month).<sup>19</sup> We use the expected rather than the actual month of birth to define

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<sup>18</sup>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 non-random selection in The Bronx, Brooklyn, or Queens as discussed further below.

<sup>19</sup>For most women we only observe one assault. There are 181 observations where a

these groups because realized gestation length is a potential outcome. A related issue is that the longer the pregnancy lasts, the more time there is for the woman to be assaulted during pregnancy.<sup>20</sup> These restrictions leave us with a sample of 2,045 births.

Tables 1 and 2 present mean maternal/child and paternal characteristics, respectively, for three sub-groups of parents that are relevant to our first identification strategy. In addition, to assess the comparability of our sample to births from the rest of the U.S., column (1) of each of the two tables presents mean characteristics in national vital statistics data for 2006–2013 births. Column (2) uses observations from our New York City sample where the mother did not experience a first assault at her home in months 0-19 post-conception (i.e., those who are neither in our treatment nor control group). Column (3) uses treatment group observations, column (4) uses control group observations, while column (5) reports the difference in means between the treatment and control groups.

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mother experiences assaults both during months 0-9 post-conception and months 10-19 post-conception; these women are included in our treatment group since they experienced their first observed assault during the prenatal period. Our results are similar if we drop these women. There are 201 observations where a mother has assaults during months 1-10 before conception and months 0-9 post-conception, and 77 observations where a mother has assaults during months 1-10 before conception, months 0-9 post-conception, and months 10-19 post-conception; these women are dropped from our main analysis sample since their first assault occurs before pregnancy, but they are included in a robustness analysis that uses mothers with first assaults before pregnancy as an alternative control group.

<sup>20</sup>See Currie and Rossin-Slater (2013), Black *et al.* (2016), and Persson and Rossin-Slater (2018) for detailed discussions of these issues. Note that there are 228 observations that are included in the treatment group (i.e., assault in months 0 through 9 relative to conception) where the assault actually happened after the birth. Thus, our estimates represent intent-to-treat effects, and suggest that the treatment-on-the-treated effects of assault during the actual duration of pregnancy are even larger.



Comparing column (1) to the other columns shows that the parents in our New York City data are much less likely to be non-Hispanic white and more likely to be foreign-born than the parents of the average child in the U.S. There are differences in other characteristics as well. Moreover, comparing column (2) to columns (3) and (4) in both tables demonstrates 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 other mothers. The fathers of their children are younger, less educated, and more likely to be minorities. However, when we zoom in on mothers who experience a first assault either during pregnancy or in the postpartum period in columns (3) and (4), the differences in parental characteristics are all statistically indistinguishable from zero at the 5% level of confidence. We explore the potential differences between the treatment and control groups for our first identification method further in Section 4 below.

**Measurement error.** In principle, our measure of exposure to assault could capture the victimization of another household member, who is not the pregnant woman or new mother. It is also possible that women move during pregnancy, so that they were not living at the home in which the assault occurred at the time when it occurred. The scope for these misclassification errors is limited, however. The victim is unlikely to be the child—estimates from Maston *et al.* (2011) indicate that only 0.6 percent of assaults are situations in which the victim-offender relationship is child-parent. Moreover, Census data show that only 12.7 percent of mothers of young children living in single-family homes in The Bronx, Brooklyn, or Queens have another non-partner female adult living in their home, implying that the vast majority of women in our data are the sole adult females in their households and therefore the likely victims of the assaults we observe. Similarly, only 11.3 percent of these mothers have moved in the last year.<sup>21</sup> Moreover, our estimate of the number of pregnant women

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<sup>21</sup>Estimates based on authors' calculations using pooled 2000 and 2010 Census 5% Public Use Microdata Samples. Mothers of young children are defined as women with children less

assaulted matches well with survey data from the Center for Disease Control's Pregnancy Risk Assessment Monitoring System (PRAMS), as shown in Appendix C.<sup>22</sup>

**Outcomes.** To address the issue of multiple hypothesis testing, we group our outcomes into four indices: (1) *severe birth outcomes index*, which includes indicators for the following severely adverse outcomes: 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 of the newborn, and death by the time of birth certificate filing; (2) *broad birth outcomes index*, which includes all outcomes included in the severe birth outcomes index, as well as other less severe measures: continuous birth weight in grams, indicator for low birth weight (<2,500 grams), gestation in weeks, and indicator for pre-term birth (<37 weeks);<sup>23</sup> (3) *use of medical services index*, than 2 years old in the household.

<sup>22</sup>Another data problem is that IPV tends to be under-reported to police. Notably, although IPV is under-reported, victims of IPV are as likely as other victims of assault to call the police (Felson *et al.*, 2002). In addition, Miller and Segal (2018) demonstrate that a higher rate of female representation in the police force increases the reporting rate of domestic violence to law enforcement. Our inclusion of conception year and month fixed effects accounts for this potential source of selection into reporting. We further attempt to limit the scope of potential under-reporting bias through two sample restrictions. First, we use a sample of mothers who have all had a reported assault 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, which are less likely to be under-reported than more minor offenses (Morgan and Kena, 2017). Importantly, Table 1 shows that the share of felony assaults in our sample is not significantly different between women in our treatment and control groups.

<sup>23</sup>We have also examined an index that additionally includes indicators for high birth weight (>4,000 grams) and a male child, with very similar results. Since male fetuses are

which includes: indicator for first trimester prenatal care initiation, number of prenatal care visits, 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); (4) *maternal behavioral and well-being index*, which includes indicators for: mother smoking during pregnancy, mother using illicit drugs during pregnancy, mother being depressed, low pregnancy weight gain (<15 lbs), high pregnancy weight gain (>40 lbs),<sup>24</sup> and the mother not receiving WIC benefits.

To create the indices, we first orient each outcome such that a higher value either represents a more adverse outcome (for indices 1, 2, and 4) or more use of medical services (for index 3), and then standardize each oriented outcome by subtracting the control group mean and dividing by the control group standard deviation. For the first analysis, the control group is defined as mothers for whom the first assault that we observe is in months 10 through 19 post-conception. For the maternal fixed effects analysis, 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).

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more likely to miscarry, a reduction in male births may indicate an increase in miscarriages (see, e.g., Sanders and Stoecker, 2015). We do not find any evidence for changes in the sex ratio at birth.

<sup>24</sup>Our births data only contain information on maternal pre-pregnancy body mass index 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>.

## 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:

$$y_i = \gamma \text{AssaultPreg}_i + \mathbf{x}'_i \omega + u_i \quad (1)$$

for each mother-child pair  $i$ .  $y_i$  is an outcome of interest such as the severe birth outcomes index,  $\text{AssaultPreg}_i$  is an indicator that is equal to 1 for mothers who have a first 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, unobservable components in  $u_i$  are likely to be correlated with the treatment variable, leading to biased estimates of  $\gamma$  in equation (1).

We aim to overcome this issue by using control groups that enable us to approximate a randomized design. We adopt three research designs, which rely on different identifying assumptions: (1) comparing the outcomes of women who report an assault in the nine months after conception to those who experience an assault one to ten months after the estimated due date, (2) a difference-in-differences (DD) model, in which 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 report any other type of crime during those two time periods, and (3) a maternal fixed effects approach, which accounts for any time-invariant differences across mothers who do and do not experience an assault during pregnancy.

**During Versus Post-Pregnancy Model.** The literature suggests that women who experience an assault in their homes in a short time period *after* pregnancy serve as an appropriate control group for women who have one during pregnancy, and we provide further evidence regarding the comparability of these two groups below.

In particular, consider the sample:

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

where  $c$  denotes the month of conception and  $e_b$  denotes the *expected* month of birth (i.e., the 9th month after the month of conception). Thus,  $\mathbf{1}[c \leq Assault \leq e_b]_i = 1$  indicates that the assault occurred during the expected length of the pregnancy, while  $\mathbf{1}[e_b < Assault \leq e_b + 10]_i = 1$  indicates that it occurred in the 9 following months.

We estimate the following model on the sample with  $i \in \{S\}$ :

$$y_{iymr} = \beta_0 + \beta_1 \mathbf{1}[c \leq Assault \leq e_b]_{iymr} + \psi_y + \phi_m + \rho_r + \mathbf{x}'_i \delta + \nu_{iymr}, \quad (2)$$

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 expected month of birth, and 0 otherwise. We include conception year and month fixed effects,  $\psi_y$  and  $\phi_m$ , respectively, as well as fixed effects for the three boroughs in our analysis,  $\rho_r$ .<sup>25</sup> The vector  $\mathbf{x}_i$  includes the following control variables: maternal and paternal age group dummies (<20, 20-24, 25-34, 35+, missing), indicator for the mother being married, indicators for maternal and paternal nativity status (U.S.-born, foreign-born, missing), maternal and paternal race/ethnicity dummies (non-Hispanic white, Hispanic, non-Hispanic Black, other, missing), maternal and paternal 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 a single rather than multiple birth, and parity dummies (1st, 2nd, 3rd+, missing).

The key coefficient of interest,  $\beta_1$ , represents an estimate of the impact of exposure to an assault during pregnancy. The coefficient  $\beta_1$  may not capture the stress associated with

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<sup>25</sup>Conception month fixed effects account for potential seasonality in births and non-random sorting into different months of birth (see, e.g., Bound *et al.*, 1995; Buckles and Hungerman, 2008).

being in a violent relationship, however. 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 before, during, and after pregnancy that may, from time to time, culminate 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 these estimates capture the effects of the more serious physical assault itself. As noted above, this is a distinction between our work and previous work on the impact of stressful events during pregnancy. However, as discussed, the maternal fixed effects estimates are likely to incorporate both the effects of physical assault and the effects of the stress associated with being at risk of violence; see below for more details on this strategy.

This first analysis relies on the assumption that the timing of assault within a 10-month window 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 selected in systematically different ways that are correlated with infant health. Evidence from a study of the determinants of IPV during and after pregnancy by Charles and Perreira (2007) provides support for this assumption. Using rich longitudinal survey data from the Fragile Families and Child Wellbeing Study (FFCWS)—which includes detailed information on parental demographics, family structure, measures of social support, stress, and substance abuse—they attempt to predict whether violence occurs during pregnancy or postpartum and find that only one out of 26 predictors is statistically significant.

We examine the plausibility of the identifying assumption in our data as well. Tables 1 and 2 have already shown that mothers and fathers in the treatment and control groups are similar in terms of their observable characteristics. Figure 1 depicts how the number and composition of births varies with the distance between the month of assault and the conception month. In panel (a), we plot the total number of births in which the first assault that we observe occurs, from 10 months before conception to 19 months after (months -10

to 19). In panel (b), we predict the severe birth outcome index using the maternal, paternal, and child characteristics included in vector  $\mathbf{x}_i$  (described above), and then plot the mean of the predicted index by the month of assault relative to conception month. The vertical red lines separate the figure into observations with assaults pre-pregnancy (months -10 to -1), assaults during the expected length of pregnancy (months 0 to 9, i.e., our treatment group), and assaults after the expected month of birth (months 10 to 19, i.e., our main control group). There are no jumps in either the number or the composition of births at these markers, suggesting no observable change in selection into our treatment or control groups.

In Appendix Table A.4, we show that the lack of significant differences in the observable characteristics of mothers and fathers between the treatment and control groups holds up when we include borough, conception year, and conception month fixed effects, as in our main model (2). Specifically, we estimate model (2), using each of the background characteristics as a dependent variable, and omitting the vector  $\mathbf{x}_i$ . We report the estimates of  $\beta_1$  from these regressions; out of 19 coefficients in Appendix Table A.4, only two are marginally significant at the 10% level. We find that mothers and fathers in the treatment group are less than a year older than mothers and fathers in the control group, a difference that is unlikely to drive the estimated effects on infant health. We have also used all of the characteristics in  $\mathbf{x}_i$  to predict our treatment variable. The  $p$ -value from an  $F$ -test of the joint significance of these regressors is 0.92.

In summary, we cannot detect any systematic differences between the treatment and control groups along margins observable in our administrative data. But other factors not recorded in our data—such as parental relationship quality or dynamic patterns in family income or poverty—may vary across women who experience assault during pregnancy and those who experience assault as new mothers. Moreover, the likelihood of calling the police when an assault occurs may differ between the treatment and control groups.

Bindler and Ketel (2019) show that both earnings and government benefit receipt in the

Netherlands are stable in the years leading up to a domestic assault incident, which reduces concerns about dynamic selection in our study. To shed more light on these concerns in the U.S. context, we turn to survey data from FFCWS. While the FFCWS sample is substantially smaller than our linked administrative data set, it includes more detailed information on parental relationships and evolving family circumstances and does not rely on reports of IPV to the police. We compare women whose partners initiated IPV during pregnancy and those whose partners initiated IPV in the first year after childbirth. We find no significant differences in a variety of individual and household measures (e.g., baseline relationship status, household composition, and household income). We also find that the timing of IPV initiation is uncorrelated with factors such as whether the baby resembles the father, the child's sex, and changes in the household's income and poverty ratio. Appendix D provides more details and presents these results.

In addition, we follow Oster (2017) to provide a lower bound on our estimated treatment effect after adjusting for selection on unobservable characteristics, and to examine how large selection on unobservables would have to be to make our estimated treatment effect go to zero. We further test the robustness of our results to incorporating women who experience a first assault in the 9 months *before* conception into the control group.

**Difference-in-Difference Model.** As a second estimation strategy, we estimate a DD model, in which 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. For these analyses, our sample includes all women with any crime in their home in months 0-19 post-conception; i.e., our DD analysis sample is:

$$S' = \{i : \mathbf{1}[c \leq Crime \leq e_b]_i = 1 \mid \mathbf{1}[e_b < Crime \leq e_b + 10]_i = 1\}.$$



Here,  $\mathbf{1}[c \leq Crime \leq e_b]_i = 1$  indicates that the crime occurred during the expected length of the pregnancy, while  $\mathbf{1}[e_b < Crime \leq e_b + 10]_i = 1$  indicates that it occurred in the 9 following months.

We then estimate the following model on the sample with  $i \in \{S'\}$ :

$$y_{iymr} = \pi_0 + \pi_1 \mathbf{1}[c \leq Assault \leq e_b]_{iymr} + \pi_2 \mathbf{1}[c \leq Assault \leq e_b + 10]_{iymr} + \pi_3 \mathbf{1}[c \leq Crime \leq e_b]_{iymr} + \psi_y + \phi_m + \rho_r + \mathbf{x}'_i \delta + \varepsilon_{iymr}, \quad (3)$$

Thus, we augment model (2) by including separate indicators for assault during months 0-9 post-conception ( $\mathbf{1}[c \leq Assault \leq e_b]_{iymr}$ ), assault during months 0-19 post-conception (i.e., either during or after pregnancy,  $\mathbf{1}[c \leq Assault \leq e_b + 10]_{iymr}$ ), and for experiencing any other crime during months 0-9 post-conception ( $\mathbf{1}[c \leq Crime \leq e_b]_{iymr}$ ). The omitted category is women who experienced some other type of crime in their home during months 10-19 post-conception. The rest of the variables are as defined in equation (2). We are interested in the coefficient  $\pi_1$ , which is the estimate of the effect of exposure to assault during pregnancy, relative to exposure to assault post-pregnancy, and relative to exposure to any other crime during either period.

The DD model allows for there to be differences in the characteristics of women who report an assault during pregnancy and those who report one in the postpartum period, but assumes that those differences are similar to the differences between women who report other crimes across these two time periods. Moreover, this model allows us to separate any differential effect of assault during pregnancy relative to the effect of exposure to another crime, such as burglary, which is likely to also be a stressful event. As one indirect test of the identifying assumptions underlying the DD design, we show results from the DD regression models with maternal and paternal characteristics as outcomes in Appendix Table A.5. We observe no systematic differences between mothers who report an assault during versus after pregnancy relative to the differences between mothers who report other crimes during versus

after pregnancy.

**Maternal Fixed Effects Model.** In a third estimation strategy, we leverage the maternal identifiers in our birth records data to link siblings born 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:<sup>26</sup>

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

for each child  $i$ , conceived in year  $y$  and month  $m$ , born to mother  $k$ .  $\pi_k$  is a maternal fixed effect, while the vector  $\mathbf{x}_i$  now only includes characteristics that vary within each mother: maternal and paternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for mother being married, maternal and paternal 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), paternal race/ethnicity and nativity dummies (to account for possible different fathers across pregnancies), child 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,  $\kappa_1$ , is identified using the 451 children of 201 mothers who have at least one pregnancy exposed to an assault, and one unexposed pregnancy.<sup>27</sup> In these models, we cluster standard errors on the mother. The use

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<sup>26</sup>We only condition on residence in The Bronx, Brooklyn, or Queens during the first pregnancy since subsequent mobility may be endogenous.

<sup>27</sup>We also include children of mothers who never have an assault during pregnancy (18,107 observations) and children of mothers who have an assault during every pregnancy (42 observations) to increase power in identifying the coefficients on the other variables in the regression model.

of mother fixed effects alleviates concerns about mothers who suffer assault during pregnancy being differentially selected from those who experience assaults in the postpartum period.

One potential issue for the maternal fixed effects model is that that subsequent fertility could be endogenous with respect to exposure to assault, leading to selection into the sibling sample. However, Appendix Table A.6 demonstrates that experiencing an assault during the first pregnancy observed in our data is not correlated with either the likelihood of having a future child or with the total number of subsequent children. Thus, we conclude that our sample of siblings is not subject to selection bias.

Another concern is that unobservable differences across siblings around the period of childbirth may be correlated with exposure to assault (e.g., changes in maternal employment or family structure). To examine this issue, we estimate placebo fixed effects models, in which we drop mothers who ever experience an assault during pregnancy, and instead use as the treatment variable an indicator for assault *after* pregnancy. As we discuss in more detail below, we find no evidence of a significant correlation between post-pregnancy exposure to assault and our main outcomes of interest, assuaging concerns about unobservable time-varying factors confounding the maternal fixed effects estimates.

## 5 Results

**Main results.** Table 3 summarizes the estimated results for the outcome indices, using our three empirical strategies. The first row reports results from estimating equation (2) using the sample with  $i \in \{S\}$  defined in Section 4 (i.e., the model that compares mothers who experience an assault during pregnancy with those who experience one in the postpartum period). In the second row, we show the treatment coefficient estimate from the DD model, in which we use mothers who experience any other crime in their home during or after pregnancy as an additional control group (note that the full set of DD coefficients is presented in Appendix Table A.7). The final row presents estimates from the maternal fixed effects model (equation (4)).

Across all three methods, the results indicate that exposure to assault during pregnancy causes a deterioration in newborn health. Column (1) shows that an assault during pregnancy is associated with a statistically significant 0.08 SD increase in the severe birth outcome index in both the “during versus post-pregnancy” and DD models. The effect on the broader birth outcome index, which includes less extreme outcomes and continuous birth outcome measures, is a 0.06 to 0.07 SD increase, which is marginally significant at the 10% level (column 2). The similarity of the results between the assault timing and DD models suggests that while there is a significant adverse effect of assault during pregnancy on infant health, there appears to be no significant impact of exposure to other crimes.<sup>28</sup>

The maternal fixed effects results for the severe and broader birth outcome indices in the third row of Table 3 columns (1) and (2) also show a worsening of newborn health. As expected, the estimated effects are larger in the fixed effects specifications than in the other two models. Recall that in the first two models, we are comparing assault during pregnancy to assault postpartum, meaning that all of the women in the sample are in violent, stressful relationships around the time of childbirth. In the maternal fixed effects model, we are instead comparing a pregnancy subject to violence to a pregnancy at a different time, when the woman might not be in a violent relationship, so the contrast is greater.

The results for the individual birth outcomes rather than the indices are presented in Appendix Tables A.8 and A.9.<sup>29</sup> The overall rise in the severe birth outcomes index is driven by 1.5 and 2.1 percentage point increases in the likelihoods of very low birth weight births and low 1-minute Apgar scores, respectively, which constitute 60.6 and 46.4 percent

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<sup>28</sup>We have also estimated regression model (2) 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.

<sup>29</sup>The results for individual outcomes from the DD models are very similar to those from the “during versus post-pregnancy” models and available upon request.

effects when evaluated at the sample means of the dependent variables. In the maternal fixed effects models, we find increases in the rates of very pre-term and low 1-minute Apgar score births (the latter is marginally significant at the 10% level), as well as a substantial increase in the likelihood of NICU admission (significant at the 5% level). The coefficient for very low birth weight is positive and large in the maternal fixed effects model, but not significant at conventional levels ( $p$ -value of 0.15).

**Mechanisms.** To shed some light on the mechanisms driving the effects on birth outcomes, we examine the mother's use of medical services during pregnancy and delivery in column (3) of Table 3. Results from the assault timing and DD models (rows 1 and 2) indicate that women with a reported assault during pregnancy are significantly more likely to use medical services than their counterparts with a reported assault in the postpartum period. Panel C of Appendix Table A.8, which shows results for the individual components of the use of medical services index, suggests that this effect operates in part through a 6.0 percentage point higher likelihood of initiating prenatal care in the first trimester and 0.33 more prenatal care visits (marginally significant). 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 suggests that women who experience violence during pregnancy may engage in compensatory behaviors, making our impacts on birth outcomes lower bounds. Column (3) of Panel C of Appendix Table A.8 also shows a strong impact on induction of labor—assault during pregnancy is associated with a 5.6 percentage point increase in the likelihood of labor being induced, a 25.7 percent rise at the sample mean.

However, when we examine the use of medical services with the maternal fixed effects specification, we find no statistically significant results (see Panel C of Appendix Table A.9). It is possible that there is less scope for variation across pregnancies within the same mother (i.e., a mother who initiates prenatal care during the first trimester in one pregnancy is also likely to initiate prenatal care at that time during another pregnancy, regardless of assault

exposure).

Column (4) of Table 3 examines mechanisms further by studying impacts on the maternal behavioral and well-being index. We do not find any significant effects on this index. When examining the individual outcomes in Panels D of Appendix Tables A.8 and A.9, we only observe one significant coefficient—women exposed to assault during pregnancy are less likely to receive WIC benefits.<sup>30</sup>

**Timing of effects.** In Appendix Figure A.2, we explore differences in impacts across various periods of exposure using a variant of our first estimation strategy. Here, we include all mothers with a first assault in the window from 10 months before conception month to 19 months after conception month. 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).

Sub-figures (a) and (b) of Appendix Figure A.2 suggest that the impacts on our two birth outcome indices are strongest for assaults in the 3rd trimester of pregnancy. Sub-figure (c) documents significant effects of exposure to assault in the second and third trimesters on the medical services index, while sub-figure (d) indicates a significant effect of second trimester exposure for the maternal behavioral and well-being index.<sup>31</sup> In supplementary

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<sup>30</sup>A decline in WIC take-up could arise because perpetrators of IPV engage in controlling behaviors that limit the choices of their victims. See: <http://www.opdv.ny.gov/professionals/abusers/coercivecontrol.html>. But this one significant coefficient could also be a statistical fluke.

<sup>31</sup>We find that the second trimester effect on the maternal behavioral and well-being index

analyses of individual outcomes (available upon request), we find that third trimester effects are particularly pronounced for very low birth weight, very pre-term, and low 1-minute Apgar score births, as well as for induction of labor.

The literature suggests that the fetus is differentially vulnerable to different types of shocks at various points in pregnancy. Assault during pregnancy may affect birth outcomes through two mechanisms. The first is ongoing maternal stress, and the second is through direct physical injury that requires induction of delivery or triggers labor. In the study most closely related to ours, Aizer (2011) examines mothers who have been hospitalized as a result of assault. She finds that the largest effect on birth weight is among women who were hospitalized in their first trimester but went on to deliver some months later. Our sample is different because it also includes women who were assaulted but not injured severely enough to be hospitalized. In this broader sample, we find that the effects on birth outcomes are concentrated among women assaulted in the third trimester. This may be because in addition to enduring very stressful circumstances, women assaulted in their third trimester are more likely to have their labor induced prematurely and therefore to deliver very pre-term and very low birth weight babies.

**Addressing additional concerns about selection.** As discussed in Section 4, a central concern about interpreting estimated effects from our first identification strategy as causal is possible differences in unreported assaults and selection into assault across the treatment and control groups. We have presented evidence that the observable characteristics of parents in the treatment and control groups are statistically indistinguishable. Further, the lack of significant coefficients on exposure in the “2 Post” and “3 Post” periods in the graphs in Appendix Figure A.2 suggests that mothers exposed to assault during distinct 3-month intervals after pregnancy are not differentially selected. At the same time, the occasionally is driven entirely by a decrease in WIC benefit take-up, as in Panels D of Appendix Tables A.8 and A.9, which we discuss above.

significant coefficients in the “-3 Pre” and “-2 Pre” periods suggest that women exposed *before* the pregnancy may be negatively selected due to potential non-random selection into conception.

In Panel A of Appendix Table A.10, we examine the robustness of our estimates to including women with an observed assault in their home up to 10 months prior to the conception month (i.e., before pregnancy). Like women in the treatment group, these women do not (yet) have newborn children, and may therefore have similar reporting behaviors. Using this augmented sample of assaults, we continue to see a significant increase in the summary index of severe adverse birth outcomes.

Several additional analyses are described in Appendix E. First, we implement Oster (2017)’s suggested procedure for developing bounds on the extent to which the estimates using our first identification strategy are likely to be biased by unobservable variables. We find that even conservatively estimated bounds indicate that IPV worsens birth outcomes. Second, we conduct a placebo test using the siblings sample, in which we drop mothers who experienced an assault during pregnancy and estimate models using assault in the 10 to 19 months after conception as the treatment variable. We find no significant effect of assault on birth outcomes in this sample, suggesting that our effects are not driven by unobservables associated with IPV around the time of childbirth. Finally, we estimate models using families in multi-family dwellings, which requires us to compute a probability that a mother in a given building suffered an assault during pregnancy. We continue to see a negative effect of IPV on birth outcomes in this alternative sample.

## 6 Conclusion

Measuring the social cost of crime—and especially violent crime—is crucial for policy debates about the judicial system and programs that impact criminal behavior more broadly. Existing approaches assume that all of the costs of victimization are fully captured. However, causal evidence on the effects of violent crime on victims is sparse due to data constraints and



the potential endogeneity of exposure. We break new ground by using linked administrative data from New York City to deliver new estimates of the effects of assault on an important segment of the population, newborn children.

We use three different research designs, which rely on novel administrative data that link birth records for children of mothers living in single-family homes in The Bronx, Brooklyn, and Queens boroughs of New York City to reported crimes that occur in their homes. In the first strategy, 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 estimated due date. We find that assault during pregnancy leads to increases in the rates of very low birth weight and low 1-minute Apgar score births of 61 and 46 percent, respectively. The effects appear to be driven by assaults in the 3rd trimester, for which we also observe an increase in the likelihood of induced labor. We show that our results are very similar when we instead estimate a DD model, which compares the difference in outcomes between women who experience an assault in their homes during pregnancy and in the postpartum period to the difference between women who experience other types of crimes across these two periods. We also present results from a third specification, which controls for maternal fixed effects, and thus compares two pregnancies of the same mother, where one was affected by assault and the other was not. As the control group in the maternal fixed effects model is less likely to be experiencing any violence (whereas the control groups in the first two specifications are exposed to violence in the period surrounding childbirth), the point estimates for the effects on infant health from this third method are larger than those from the first two methods.

What do our findings imply for the measurement of the social cost of crime? We conduct a back-of-the-envelope calculation, focusing on the estimates of the effect of assault on 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. This calculation—presented in detail in Appendix F and using the estimates from our first empirical model—generates an average social cost of \$36,857 per assault during pregnancy. If we use the larger maternal fixed effects estimates, we get an average social cost of \$85,999. Appendix Figure F.1 shows that this cost is largely driven by the higher likelihood of infant mortality and decreased life expectancy among very low birth weight children. We emphasize that these numbers likely underestimate the full social cost of assault on pregnant women for at least five reasons: measurement error in our crime data, under-reporting of IPV to the police, the fact that we do not measure the effects of assault on maternal well-being, possible compensatory responses on the part of mothers that may reduce the damage to the fetus from assault, and the fact that women subject to IPV are likely to be living with high levels of ambient stress, which is also known to affect fetal development.

As noted in Section 2, existing work suggests that assaults generate between \$16,000 and \$90,000 in social costs per victim depending on the methodological approach. However, we are not aware of any prior attempts to calculate these costs specifically for the population of newborn infants, whose mothers constitute a shockingly large and vulnerable subgroup of assault victims. The current methods of evaluating these effects—jury award estimates and contingent valuation studies—largely focus on costs that manifest immediately at the time of the violent episode and are not well-suited for including longer-term costs, such as those resulting from the adverse health and human capital consequences on unborn children. Incorporating our new findings into these estimates yields a total social cost of assaulting a pregnant woman in the range of \$52,781 to \$127,563, representing an increase of 41 to 231 percent over prevailing figures. Moreover, our calculation suggests that the overall cost for the “average” assault (i.e., regardless of whether it involves a pregnant woman or not) should be increased by 1 to 5 percent (see Appendix F).

With an average of 3,177 pregnant women between 2004 and 2012 in New York City suffering physical abuse, our estimates imply that the total social costs previously unaccounted

for in New York City alone are approximately \$117.1 to \$273.2 million per year. Across the United States, we estimate an annual social cost of around \$3.78 to \$8.82 billion based on the best available nationwide victimization estimates for pregnant women, and the fact that there are approximately 3.9 million births per year.<sup>32</sup>

Our results imply that interventions that 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 exploring the longer-term consequences of prenatal exposure to assaults on child health and development, as well as on maternal well-being is indicated.

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<sup>32</sup>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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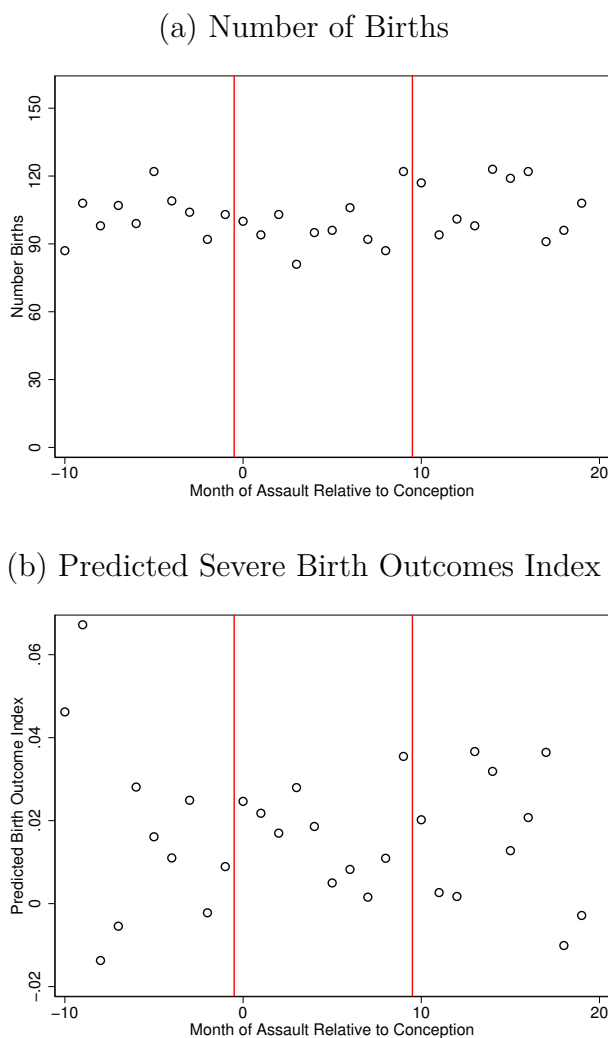
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Figure 1: Number of Births and Predicted Severe Birth Outcome Index By Month of Assault Relative to Conception



*Notes:* 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, who experience an assault in months -10 to 19 relative to conception (N=3,074). The top figure plots the total number of births by the month of assault relative to the conception month. The bottom figure plots mean predicted severe birth outcomes index by the month of assault relative to the conception month. See text for details on the construction of the predicted severe birth outcomes index. The vertical red lines in each graph denote the month of conception and month 9 of pregnancy, respectively.

Table 1: Mean Maternal &amp; Child Characteristics

	(1)	(2)	(3)	(4)	(5)
	US Births	Bronx-Brooklyn-Queens Births			
		No Assault	Assault-Preg	Assault-Post	Diff (3)-(4)
Mother's Age	27.64	29.79	27.01	26.48	0.524*
Mother's Age <20	0.094	0.044	0.122	0.140	-0.018
Mother's Age 35+	0.111	0.238	0.137	0.126	0.011
Mother Married	0.601	0.650	0.355	0.329	0.025
Mother Non-Hisp. White	0.539	0.308	0.121	0.113	0.008
Mother Hispanic	0.239	0.166	0.256	0.258	-0.002
Mother Non-Hisp. Black	0.146	0.290	0.488	0.487	0.000
Mother Non-Hisp. Asian	0.058	0.211	0.108	0.101	0.007
Mother Foreign-Born	0.239	0.532	0.495	0.498	-0.003
Mother's Ed <HS	0.178	0.122	0.269	0.275	-0.006
Mother's Ed HS	0.239	0.249	0.299	0.295	0.005
Mother's Ed Some Coll.	0.233	0.273	0.275	0.275	-0.000
Mother's Ed Coll.+	0.242	0.355	0.152	0.151	0.001
First Parity	0.399	0.418	0.451	0.478	-0.027
Singleton Birth	0.966	0.961	0.973	0.980	-0.007
Felony Assault	—	—	0.269	0.245	0.024
Observations	37,005,729	66,659	976	1,069	

*Notes:* Column (1) uses national vital statistics data for the universe of U.S. births in years

2006-2013 and presents sample means. In the other columns, 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. Column (2) presents mean 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 (3) presents mean characteristics

for observations where the mother experienced any assault at her home during 10 months

post conception month. Column (4) presents mean characteristics for observations where

the mother experienced any assault at her home during 10 months post expected delivery

month. Column (5) presents the difference between (3) and (4), where significance levels are

denoted as: \*  $p < 0.1$  \*\*  $p < 0.05$  \*\*\*  $p < 0.01$

Table 2: Mean Paternal Characteristics

	(1)	(2)	(3)	(4)	(5)
	US Births	Bronx-Brooklyn-Queens Births			
		No Assault	Assault-Preg	Assault-Post	Diff (3)-(4)
Father's Age	30.61	33.41	30.83	30.20	0.625
Father's Age <20	0.036	0.012	0.038	0.046	-0.008
Father's Age 35+	0.229	0.373	0.223	0.224	-0.0008
Father Non-Hisp. White	0.473	0.304	0.110	0.100	0.010
Father Hispanic	0.209	0.137	0.188	0.194	-0.006
Father Non-Hisp. Black	0.109	0.230	0.337	0.356	-0.019
Father Non-Hisp. Asian	0.050	0.192	0.102	0.0861	0.016
Father Foreign-Born	—	0.530	0.514	0.505	0.009
Father's Ed <HS	0.120	0.107	0.175	0.167	0.009
Father's Ed HS	0.202	0.261	0.295	0.299	-0.004
Father's Ed Some Coll.	0.179	0.228	0.183	0.185	-0.002
Father's Ed Coll+	0.193	0.287	0.103	0.108	-0.004
Father Info Missing	0.135	0.0717	0.140	0.135	0.006
Observations	37,005,729	66,659	976	1,069	

*Notes:* See notes under Table 1. Information on father's birth place is not available in the national vital statistics data.

Table 3: Summary of Estimated Effects of Assault During Pregnancy on Summary Index Outcomes

Research Design	Severe Birth Outcomes Index	Broad Birth Outcomes Index	Medical Services Index	Behavioral/ Wellbeing Index	Observations
<i>During vs. Post Pregnancy Model:</i>	0.077** [0.036]	0.066* [0.035]	0.057*** [0.022]	0.025 [0.019]	2,045
<i>Difference-in-Difference Model:</i>	0.077** [0.036]	0.064* [0.035]	0.040* [0.022]	0.032 [0.020]	11,566
<i>Maternal Fixed Effects Model:</i>	0.30** [0.13]	0.25** [0.12]	-0.013 [0.063]	0.040 [0.057]	18,600

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*Notes:* Each coefficient in each row is from a separate regression. The outcomes are four summary indices: severe birth outcomes index, broad birth outcomes index, use of medical services index, and a maternal behaviors and well-being index. See text for more details. 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. In the first row, 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, while the second row includes mothers who experienced any crimes at their homes during these periods. In the third row, 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. See text for more details about regression specifications and controls. Robust standard errors in the first two rows; standard errors are clustered on the mother in the third row. Significance levels: \*  $p < 0.1$  \*\*  $p < 0.05$  \*\*\*  $p < 0.01$