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VIOLENCE WHILE IN UTERO:
THE IMPACT OF ASSAULTS DURING PREGNANCY ON BIRTH OUTCOMES

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

Evidence about the effects of violent crime on victims is sparse, but is necessary to measure the social costs of crime and the cost-effectiveness of policy interventions in the justice system. We present new evidence about the effects of violent crime on pregnancy and infant health outcomes, using unique linked administrative data from New York City. We compare mothers who lived in a home where an assault was reported during their pregnancies to mothers who lived in a home where an assault took place shortly after the birth. We find that assaults during pregnancy significantly increase the incidence of negative birth outcomes. Our results are robust to the use of alternative control groups and to using maternal fixed effects models. Based on these impacts, we calculate that the social cost per assault during pregnancy is at least $36,857, implying a total annual cost of around $3.8 billion. Since 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, violence in utero is an important potential channel for intergenerational transmission of inequality.

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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 the impacts of criminal sanctions on offenders (Hawken and Kleiman, 2009; 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.[1] In turn, estimates of the cost of crime serve a critical role in policy evaluation. Appendix Table B.1 lists examples of studies in which cost of crime estimates are used to evaluate a variety of interventions, including labor market programs, child development programs (such as high quality preschools), gun regulation, housing programs, and many others. 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) accounts for over one seventh of all violent crimes (see Appendix Figure B.1a). 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; Card and Dahl, 2011), 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 (Helton and Snodgrass, 1987; Amaro *et al.*, 1990; McFarlane *et al.*, 1992; Johnson *et al.*, 2003; Charles and Perreira, 2007; Chambliss, 2008), and IPV-related homicide is a leading cause of death during pregnancy (Palladino *et al.*, 2011).

Estimating the causal effects of criminal victimization is challenging for at least two reasons. First, while credible 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.[2] Research on crime victims is thus typically limited to self-reports in survey data, such as the National Crime Victimization Sur-

[1]See Section 2.4 for a description of these methods.

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

vey, which may be subject to non-random measurement error or recall bias (Ellsberg *et al.*, 2001).[3] Second, 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. 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.[4]

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 during pregnancy have a 0.08 standard deviation (SD) higher summary index of poor birth outcomes. This result is driven by 1.5 and 2.1 percentage point (61 and 46 percent) higher rates of births that are very low birth weight (less than 1,500 grams) and that have low 1-minute Apgar scores, respectively.[5] These impacts stem mainly from assaults in the 3rd trimester of pregnancy, which are also associated with a higher probability of induced labor, a likely medical response of injuries sustained by pregnant victims of abuse.

A key assumption in the research design is that women who experience assault during pregnancy are not selected differently from women who experience assault in the immediate postpartum period. Prior evidence from survey data on the determinants of IPV during pregnancy versus in the postpartum period is supportive of this assumption (Charles and Perreira, 2007). A recent review of the literature by Bailey (2010) notes that many national and international population-based studies have found that pregnant women and non-pregnant women of childbearing age are equally likely to experience IPV.

[3]A recent exception is an ongoing study by Blinder and Ketel (2018), who use register data from the Netherlands, and examine effects of criminal victimization on labor market outcomes. Social surveys also rely on increasingly selected samples of individuals willing to respond, with unknown consequences for data quality (Dillman *et al.*, 2014).

[4]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.

[5]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`.

In support of this understanding of the temporal pattern of IPV we: (1) show that rates of victimization and the average maternal and paternal characteristics available in our administrative data are statistically indistinguishable across the treatment and comparison groups, (2) demonstrate smoothness in sample densities and predicted adverse health risks by the timing of assault relative to the estimated due date, and (3) use supplementary survey data from the Fragile Families and Child Wellbeing Study to document that there are no statistically significant differences in a wider range of parental relationship and time-varying factors between women who report IPV initiation during pregnancy and those who report IPV up to one year post-childbirth.

However, since there may still be unobserved variables that affect IPV and which differ between the prenatal and postpartum periods, we also use a calculation proposed by Oster (2017) to investigate the extent to which our estimates may be biased by differential selection into our treatment and control groups. This calculation provides a lower bound on our main treatment effect estimate under the assumption that there is an equal degree of selection on unobservable and observable characteristics. This lower bound still suggests large harmful effects of IPV during pregnancy on infant health.

In addition, we examine alternative control groups. We find that our results are robust to including women who experience a reported assault in the 9 months before pregnancy. This result suggests that relative to the prenatal period, any important omitted variables would have to be the same in the pre-pregnancy and post-pregnancy periods in order to account for our results, which rules out a large number of omitted factors related to household dynamics in the presence of an infant, for example.

Our results are also robust to using maternal fixed effects models. These models compare two pregnancies of the same mother, where one was affected by assault and the other was not. By focusing exclusively on the pregnancy period, these estimates rule out the possibility that our results are driven by a lack of comparability between IPV during pregnancy and IPV either before or after pregnancy. Our maternal fixed effects models, which yield larger point estimates compared to our main results, also suggest that, if anything, our main specification may produce a lower bound estimate of the impact of IPV on infant health. This interpretation is consistent with the possibility that our main specification's control group of mothers who experience a reported assault during the postpartum period is in part contaminated by unreported IPV during their pregnancies.

We conduct a back-of-the-envelope calculation to estimate the average social cost of assault during pregnancy. We use the estimated 1.5 percentage point increase in the likelihood of a very low birth weight birth, and account for six types of costs: Higher rates of infant mortality, increased medical costs during and immediately following birth, increased childhood disability, increased adult 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 of at least $3.8 billion. The cost becomes even larger if one applies the estimate we obtain from maternal fixed effects models, which compare affected to unaffected pregnancies (see Sections 5 and 6).

These 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 intergenerational social costs 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 since the majority of all violence against women is perpetrated by domestic partners (Tjaden and Thoennes, 2000), these results suggest that intra-family conflict may be an important and previously understudied mechanism through which early-life health disparities perpetuate persistent economic inequality across generations.

The rest of the paper proceeds as follows. Section 2 discusses our paper in the context of the existing literature, and then provides background information about IPV, 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 the empirical approach. Section 5 presents the results. Finally, Section 6 presents our estimate of the implied social cost of crime and offers some conclusions.

2 Background

2.1 Relationship of Our Work to Previous Research on IPV and Violence during Pregnancy

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; Ahmed *et al.*, 2006; Silverman *et al.*, 2006; Sarkar, 2008).[6] However, to the best of our knowledge, only one other paper

[6] There is also a small literature on interventions targeting IPV during pregnancy that have been evaluated using randomized controlled trials. Consistent with our results, interventions that lower IPV also reduce the incidence of very low birth weight and very pre-term births (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 who were assigned to their treatment group, only one had a very low birth weight

has used a quasi-experimental method to identify the impacts of IPV on infant health: Aizer (2011) uses a control function approach (Heckman, 1979) and linked hospitalization and birth data from California to estimate the effect of hospitalization for assault during pregnancy. Variation in IPV comes from geographic and time variation in the enforcement of laws against domestic violence.[7] 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, and as discussed above, we develop an alternative research design based on comparing assault during pregnancy to assault in the nine months postpartum rather than asking whether the marginal reported assault during pregnancy is likely to result in an arrest. 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 due to more global events such as wars and terrorist attacks—and infant health. (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). 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 speak to the direct consequences of violent crime on victims.[8] Our results are consistent with a direct physical channel—mothers assaulted in the 3rd trimester of pregnancy are more likely to need to be induced prematurely, and deliver very low birth weight babies as a consequence.

2.2 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 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

birth.

[7]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.

[8]Duncan *et al.* (2016) show that fetal exposure to the Superbowl increases the probability of low birth weight. The authors argue that domestic violence is one channel through which this effect could arise, but do not have any direct data on the incidence of violence.

of women experienced IPV (Tjaden and Thoennes, 2000).

As shown in Appendix Figure B.1a, 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 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 visit (New York City Department of Health and Mental Hygiene, 2008).[9] Black and Hispanic women, as well as those living in low-income neighborhoods, are at heightened risk.[10]

As previously noted, reported prevalence rates of physical or sexual abuse among pregnant and postpartum women range between 7 and 23 percent (Helton and Snodgrass, 1987; Amaro *et al.*, 1990; McFarlane *et al.*, 1992; Johnson *et al.*, 2003; Charles and Perreira, 2007; 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. 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 to prenatal care or other services, and engagement in adverse behaviors such as smoking or poor nutrition as a coping mechanism.

2.3 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

[9] The second age group at greatest risk was women aged 30 to 39.

[10] 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.

included a domestic incident, making it one of the most common complaints to the NYPD.[11] 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. We focus specifically on reported instances of misdemeanor and felony assaults (including aggravated assaults) and exclude violations from our main analysis sample 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.[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

[11] 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, as well as whether to release the defendant (with or without bail) or to hold the defendant on remand. 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). Yet only a fraction of those who are arrested are convicted and sentenced to incarceration. 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.

[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.

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 are still likely to exist in our data, as discussed below in Sections 3 and 4.

2.4 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,[14] the 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, many economic analyses use such estimates to assess the potential cost-effectiveness of public programs (see Appendix Table B.1).

Several strategies have been developed to meet this need (Cohen, 2005).[16] 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 assign 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 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).[17] 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 B.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

[14] Whether to consider property theft a social loss or transfer remains an open question 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.

[17] 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).

estimates consistently indicate that violent crime is more costly than any 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. It is therefore critical to generate estimates of the cost of violent crime that accurately reflect causal effects, and that fully account for the full range of potential impacts. 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 health and use of medical care in Section 6.

3 Data

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

Crime data. The crime data come from NYPD records. These data cover all criminal complaints reported between 2004 and 2012.[18] Each record includes the exact longitude and latitude 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 represent the full universe of reported crimes in New York City over the study period.[19] Appendix Table B.3 demonstrates that close to one-fifth of these reported crimes were violent in nature. Violent crimes include 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. Most other crimes involve drug offenses, criminal mischief, or harassment. Appendix Figure B.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 combined offenses per year.[20]

[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. In addition, 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`). Any attempt to merge these additional variables into our analysis data set would be a violation of our data use agreements.

[19]While there may be some false complaints, it is advantageous to see the uncensored set of criminal reports, particularly when many cases may not proceed further due to victim non-cooperation.

[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.

Births data. Birth 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.[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 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.[22]

Lastly, 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.[23] 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.

[21] 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.

[22] The birth certificate format changed in 2008, and information about depression during pregnancy is only available from 2008 onward. Depression is coded on a 5-point scale: 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 includes mothers with answers 2 through 5.

[23] About 14 percent of the observations in our sample are missing all father information from the birth certificate. We create indicators for missing values for all paternal characteristics such that we can include them as controls in our regression models without dropping observations that do not have father information. There is no statistically significant difference in the likelihood of missing father information between our treatment and control groups (see Table 2).

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.

Data merge. First, 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), which is a customized "fuzzy matching" algorithm designed specifically to address common matching challenges in New York City.[24] `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 also includes latitude and longitude for each event, is mapped onto BBLs using ArcGIS. Our BBL shapefile is published by NYC DCP, and allows us to calculate the minimum distance between a crime and the surrounding BBLs. Crimes are assigned to the nearest BBL.[25] The crime and births data are linked using the common BBL identifier, yielding a data set that matches mothers with crimes that occurred at their building of residence.

The PLUTO data set is merged in at this stage using the BBL. Since our crime data is recorded at the building level and does not include an exact apartment number, we cannot tell whether an assault happened in the mother's apartment or in another one in the same building. PLUTO allows us focus our main analysis on single-family homes where this problem does not arise. Below, we investigate a larger sample of assaults in multi-family complexes as a robustness check.

Measurement error. In addition to the issue about single-family versus multi-family buildings, our measure of exposure to assault could capture the victimization of another

[24] 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. Geosupport also accounts for nuances in address formats in each borough.

[25] 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.

household member, who is not the pregnant woman or new mother.[26] These measurement problems are unfortunately unavoidable because victim information is withheld in the crime data. To determine the degree of bias that may arise from these issues, we compare our counts of the total number of pregnant women impacted by assaults with NYC-specific estimates from the Center for Disease Control's Pregnancy Risk Assessment Monitoring System (PRAMS). PRAMS starts with the universe of vital statistics natality records, and then surveys a random sample of new mothers. PRAMS data for 2004 to 2012 suggest that 28,593 NYC mothers suffered physical abuse during pregnancy. Since PRAMS may include some offenses that were never reported to law enforcement, we could scale this number down by 0.42, the average reporting rate in the NCVS for violent offenses from a known offender between 2004 and 2012 (Bureau of Justice Statistics). This scaling would yield 12,009 *reported* episodes of abuse among pregnant women from PRAMS.

Appendix Table B.4 shows a variety of scaling factors based on different assumptions about measurement error. In our data, 25 percent of births were to mothers who resided in a building with a reported felony or misdemeanor assault during months 0-9 post-conception. An additional 25 percent of births were to women who lived in a building with a harassment claim (harassment includes cases of physical altercations that did not result in serious injury, e.g., a slap, a push, etc.).[27] However, these figures dramatically overstate the actual likelihood that someone in the woman's own home was victimized. Assuming each residential unit in a building has an equal probability of having an incident of victimization, we can scale the counts down by the inverse of the number of residential units in each building. This simple correction results in prevalence rates of 0.011 for felony and misdemeanor assaults and 0.011 for harassment, respectively, which is much closer to estimates in the literature.

Remarkably, if we apply this unit-adjustment, and focus only on counts of felony and misdemeanor assaults, our count is within 250 cases of the 12,009 cases we estimated from PRAMS, under the assumption of a police reporting rate of 0.42. However, since PRAMS may include altercations that would be classified as harassment complaints, it may be that our count should include harassment reports, which yields a total of 23,517 cases (in the last column of Appendix Table B.4). If the higher estimate is accurate, it would imply that roughly half of our observations have an explanatory variable that is measured with error. In this case, the true estimated effects might be twice as large as those we report below.

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

[27]The PRAMS data does not differentiate by degree of abuse. Harassment charges can also include non-physical offenses.

Under-reporting of IPV. An additional data problem is that IPV tends to be under-reported to police.[28] We 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). Nevertheless, our estimates of β_1 may be subject to attenuation bias resulting from under-reporting.

Analysis sample and summary statistics. After limiting our sample to births with conception years 2004 to 2012, we make the following additional restrictions for our main analysis. First, we focus on mothers who reside in single-family homes, because we know the assault occurred in their home. 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.[29]

To create our primary analysis sample, 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).[30] These restrictions leave us with a sample of 2,045 births.

Tables 1 and 2 present mean maternal and paternal characteristics, respectively, for three sub-groups of parents. Column (1) uses all observations where the mother did not experience

[28]Although IPV is under-reported, victims of IPV are as likely as other victims of assault to call the police (Felson *et al.*, 2002).

[29]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. This finding may be in part due to the relatively small sample size in Staten Island. We do not find any evidence of non-random selection in The Bronx, Brooklyn, or Queens as discussed further below.

[30]For the majority of women in our primary analysis sample, we only observe one assault. There are 181 observations where a mother experiences assaults in her home both during months 0-9 post-conception and months 10-19 post-conception; these women are included in our treatment group since they experience their first observed assault during the prenatal period. Our results are similar if we instead drop women with multiple assaults across the prenatal and postpartum periods. Additionally, 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.

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 (2) uses treatment group observations, column (3) uses control group observations, while column (4) reports the difference in means between the treatment and control groups. Comparing column (1) to columns (2) and (3) 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 (2) and (3), the differences in parental characteristics are all statistically indistinguishable from zero at the 5% level of confidence. We explore the potential differences between our treatment and control groups further in Section 4 below.

Outcomes. To address the issue of multiple hypothesis testing, we group our outcomes into four indices: (1) *main birth outcomes index*, which includes 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 of the newborn, and death by the time of birth certificate filing; (2) *broad birth outcomes index*, which includes all outcomes included in the main birth outcomes index, as well as: 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;[31] (3) *use of medical services index*, which includes: indicator for first trimester prenatal care initiation, number of prenatal care visits, indicator for WIC benefit receipt, 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), and high pregnancy weight gain (>40 lbs).[32]

To create the indices, we first orient each outcome such that a higher value either rep-

[31]Since male fetuses are more likely to miscarry, a reduction in male births may indicate an increase in miscarriages (see, e.g., Sanders and Stoecker, 2015; Halla and Zweimüller, 2013).

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

resents 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 most of the 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 (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).

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 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 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. 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:

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

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 use the expected rather than the actual month of birth to define these samples 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. For both of these reasons, defining treatment using the actual month of birth may introduce bias into the model.[33]

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

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

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, 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 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, β_1, represents an estimate of the impact of exposure to an assault during pregnancy. The coefficient β_1 may not capture the stress associated with 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 our estimates capture the effects of the more serious assault itself. As noted above, this is a distinction between our work and previous work on the impact of stressful events during pregnancy.

Identifying assumptions. Our 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,

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

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 main 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 at 0 and 9 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). We find 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 B.5, we further 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 β_1 from these regressions; out of 19 coefficients in Appendix Table B.5, 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 our main 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.

The above discussion makes clear that we cannot detect any systematic differences between the treatment and control groups along margins observable in our administrative data. But one might worry that 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, under-reporting of IPV implies that the likelihood of calling the police when an assault occurs may differ between the treatment and control groups. To shed light on these concerns, we turn to survey data from FFCWS. While the FFCWS sample is substantially smaller than our linked administrative data, 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 on 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 C 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. We also estimate a difference-in-differences (DD) 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. The DD model allows for there to be 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 any difference in the reporting rate for other crimes. The idea is that other crimes—such as burglary—are also stressful events, but are less likely to involve direct physical harm to the mother compared to an assault. Thus, evidence of a differential negative impact of exposure to assault compared to exposure to other crimes would imply that the direct physical channel is important.

Lastly, 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:[34]

$$y_{iymk} = \kappa_0 + \kappa_1 \mathbf{1}[c \leq Assault \leq e_b]_{iymk} + \zeta_y + \chi_m + \pi_k + \mathbf{x'}_i \tau + \mu_{iymk} \tag{3}$$

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 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

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

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, 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.[35] In these models, we cluster standard errors on the mother. The use of mother fixed effects alleviates some of the concern about mothers who suffer assault during pregnancy being differentially selected from those who experience assaults in the postpartum period.

As we show below, the estimates are remarkably robust to these changes in estimation techniques and samples.

5 Results

Descriptive evidence. Appendix Tables B.6 and B.7 report estimates from ordinary least squares (OLS) models that examine the correlations between experiencing assault during pregnancy and a range of maternal and paternal 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, including those who experienced no assault (or any other crime) during our sample period.

We see that assault during pregnancy is associated with a range of adverse outcomes for infants and mothers. However, the clear negative selection on maternal and paternal characteristics shown in Appendix Table B.6 raises doubt about whether the estimates can be interpreted as causal.

Main results. Panel A of Table 3 presents our main results for birth outcomes, based on estimating equation (2) using the sample with $i \in \{S\}$ defined in Section 4. The estimates suggest that exposure to assault during pregnancy causes a deterioration in newborn health. We find that the share of births with very low birth weight increases by 1.5 percentage points, or 60.6 percent at the sample mean. The likelihood of a low 1-minute Apgar score increases by 2.1 percentage points, or 46.4 percent at the sample mean. While the signs of the treatment coefficients for other outcomes—very pre-term birth, NICU admission, the presence of abnormal conditions or congenital anomalies, and death—are also consistent

[35] 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.

with an adverse effect, they are not individually statistically significant. However, column (7) shows that an assault is associated with a 0.08 SD increase in the summary adverse birth outcome index, which is statistically significant at the 5% level. The effect on the broader birth outcome index, which includes less extreme outcomes and continuous birth outcome measures, is a 0.06 SD increase which is marginally significant at the 10% level (column 8 of Panel A).

Mechanisms. To shed some light on the mechanisms driving birth outcomes, we examine the mother's use of medical services during pregnancy and delivery in Panel B of Table 3. Column (1) indicates that women with a reported assault during pregnancy are 6.0 percentage points more likely to initiate prenatal care in the 1st trimester and have 0.33 more prenatal care visits (marginally statistically significant) 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 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 negative effect on the likelihood of WIC receipt of 4.4 percentage points (column 3). The decline in WIC take-up could arise for a variety of reasons. One possibility is related to the fact that perpetrators of IPV tend to engage in controlling behaviors that limit the choices of their victims.[36] Women who are abused during pregnancy may fear going to a government program office (e.g., a WIC clinic) because of the possible reactions of their abusers. It is possible that WIC staff may report suspicion of domestic abuse to law enforcement, triggering a mandatory investigation.[37] It is also possible that the effect on WIC is due to New York's mandatory arrest law for domestic violence cases, where police are required to arrest at least one person if they respond to a domestic violence incident. If the nature of the incident is unclear, then the police may arrest all individuals in the home, including the pregnant woman, who may consequently place less trust in government programs.

Column (4) of Panel B 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 analyze a summary

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

[37] Although by New York State Law there is no mandatory reporting of adult domestic violence by social services workers, staff must report all injuries resulting from discharge of a firearm, and all potentially life-threatening injuries inflicted by a knife or other sharp object, as well as serious burns, and may also choose to report other incidents.

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 3 examines mechanisms further by estimating model (2) using observable maternal pregnancy-related behaviors and well-being measures as outcomes. We do not find strong evidence of adverse impacts on these margins. This result suggests that the effects on birth outcomes are not driven primarily by changes in maternal behaviors.

As another way of investigating the 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. For these analyses, the sample includes all women with any crime in their home in months 0-19 post-conception. We augment equation (2) 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 for experiencing any other crime during months 0-9 post-conception. The omitted category is thus women who experienced some other type of crime in their home during months 10-19 post-conception. Appendix Table B.8 presents the results, which point to a significant adverse effect of assault during pregnancy on infant health, but show no significant impact of other crimes.[38]

In Appendix Figure B.2 we explore differences in impacts across various periods of exposure. 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).

The graphs 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

[38]We have also estimated our main 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.

mechanism driving our effects: pregnant victims of assault may be likely to go to the hospital because of the resulting physical trauma, where they may need to have their labor induced prematurely and therefore deliver very pre-term and very low birth weight babies.

Addressing concerns about selection. As discussed in Section 4, a central concern about interpreting our estimated effects as causal is about possible differences in unreported assaults and selection into assault across our treatment and control groups. We have already presented evidence that the observable characteristics of parents in our treatment and control groups—both in our administrative data from NYC and survey data from the FFCWS—are statistically indistinguishable. In Panel A of Table 4, 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). Just like women in our 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 adverse birth outcomes.[39]

Even if we do not observe any evidence of selection on observable characteristics, it remains possible that our effects are biased due to selection on unobservables. To assess the magnitude of this bias, we follow Oster (2017) and report a lower bound on our main treatment effect estimate under the assumption of equal degree of selection on unobservable and observable variables. We also calculate how large the degree of selection on unobservable characteristics would have to be to explain away the treatment effect. Columns (1) through (4) of Appendix Table B.9 show that our treatment effect on the adverse birth outcome index is remarkably stable as we include different sets of controls for maternal and paternal characteristics, and borough fixed effects. We also report two parameters: β, which is a lower bound on the treatment effect under the assumption of equal degree of selection on observed and unobserved variables, and δ, the degree of selection on unobservables relative to observables that would be necessary to get a treatment effect of zero. We find that the lower bound on the treatment effect for the adverse birth outcome index remains positive, and that selection on unobservables would have to be more than 1.3 times larger than selection on observables in order to explain away our treatment effect.[40] As our δ parameter is greater

[39] Additionally, under the assumption 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.8) further assuage concerns about differential reporting rates biasing our results.

[40] A central input into the Oster calculation is R^{max}, which is the R^2 from a hypothetical regression of the outcome on treatment and both observed and unobserved controls. Oster (2017) proposes two ways of choosing R^{max}. One way is to use the R^2 from a within-family regression model. Another way is to assume that R^{max} is equal to $1.3 \times \tilde{R}^2$, where $\tilde{R}^2$ is the R^2 from the regression with all observable controls (column 4 in Appendix Table B.9). We use the former method and set $R^{max} = 0.18$, which is the R^2 from our

than one, our results pass the rule-of-thumb threshold suggested by Oster (2017).

Multi-family homes. Panel B of Table 4 investigates whether our results hold when we expand beyond our single-family home analysis sample. Here, we consider births by mothers who reside in *multi-family* homes with three floors or less, where the mother experienced an assault in her building during either months 0-9 post-conception or the 9 months after the expected month of delivery. The treatment variable in Panel B is the probability of assault during pregnancy, assuming that the probability is equal to the number of reported assaults in the building divided by 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. The difference in results between the single- and multi-family home samples is likely driven by the difference in parental characteristics across the two groups. To assess this explanation, we have tried re-weighting the multi-family sample to more closely resemble our primary single-family home analysis sample on observable characteristics, and estimating our models using the re-weighted data. The resulting coefficients become more similar to those from our baseline models (results available upon request).

Maternal fixed effects models. Finally, Panel A of Table 5 presents results from a maternal fixed effects model using a sample of siblings. These analyses compare 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 (recall, there are only 451 children of 201 mothers who had one exposed and one unexposed pregnancy), we nevertheless observe a large and statistically significant increase in the summary index of adverse birth outcomes. Appendix Table B.10 presents results for individual outcomes, where 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 likewise positive and large, but not significant at conventional levels (p-value of 0.15).

The maternal fixed effects results are qualitatively similar to those discussed in Table 3 in that there are significant effects on the adverse birth outcome indices, but not on the medical services and behavioral indices. Quantitatively, the effects on the adverse birth

maternal fixed effects model that includes all time-varying maternal and paternal controls (equation (3)) for the adverse birth outcome index as the outcome. If we were to use the latter method, we would set $R^{max} = 1.3 \times 0.054 = 0.07$, obtaining even larger δ and β parameters. Thus, our choice of $R^{max} = 0.18$ is conservative.

outcome indices are larger in the fixed effects specifications, which is perhaps intuitive. In Table 3, we are comparing assault during pregnancy to assault postpartum, implying that all of the women in the sample are likely to be in violent relationships. In Panel A of Table 5 and in Appendix Table B.10, we are comparing women who were subject to violence during one pregnancy to themselves, at a point where they may well have left (or not yet entered) the violent relationship. Thus, the contrast between "treatment" and "control" is starker in the maternal fixed effects model.

The siblings sample also allows us to do a placebo test, in which we drop all mothers who ever experienced 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 correlation between assault post-pregnancy and our pregnancy and birth outcomes. Instead, Panel B of Table 5 shows insignificant treatment coefficients for both of our birth outcome indices (columns 1 and 2). The magnitudes of the coefficients are about one sixth of those reported in the analogous columns in Panel A. We do observe a significant correlation with the maternal behavioral and well-being index in column (3), which suggests positive selection on this set of outcomes. That is, a negative value means better behaviors in the postpartum assault group, which would tend to reduce the estimated difference between the effect of assault during pregnancy and the effect of assault postpartum.

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, pregnant women and newborn children.

Our research design leverages birth records for children of mothers living in single-family homes in The Bronx, Brooklyn, and Queens boroughs of New York City who suffered 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 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.

Descriptive estimates that include 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 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 A and using the estimates from our main regression 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 B.3 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 2.4, 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 pregnant women and their infants, who 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 incorporating longer-term costs, such as those resulting from the adverse health and human capital consequences on unborn children. 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.[41]

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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[41] 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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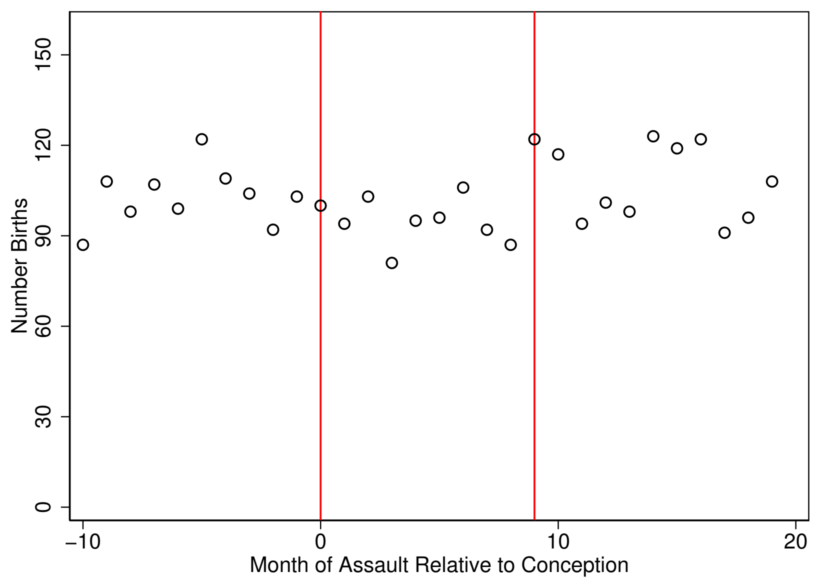
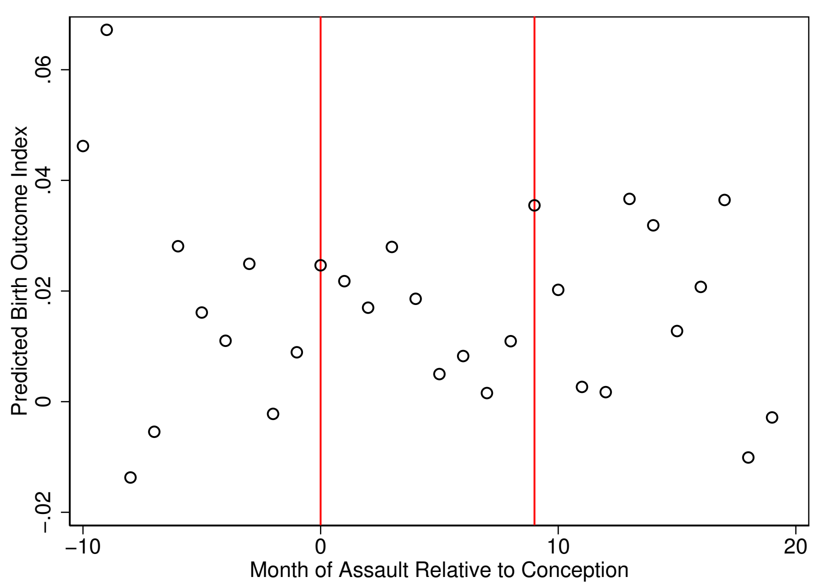
Figure 1: Number of Births and Predicted Adverse Birth Outcome Index By Month of Assault Relative to Conception

(a) Number of Births



(b) Predicted Adverse Birth Outcome Index



Notes: 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 adverse birth outcome index by the month of assault relative to the conception month. Predicted adverse birth outcome index is estimated from a regression of the index on the following characteristics: maternal and paternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for 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 singleton birth, parity dummies (1st, 2nd, 3rd+, missing). The vertical red lines in each graph denote the month of conception and month 9 of pregnancy, respectively.

Table 1: Maternal Characteristics by Any Assault During/Post Pregnancy

	(1) No Assault	(2) Assault-Preg	(3) Assault-Post	(4) Diff (2)-(3)
Mother's Age	29.79	27.01	26.48	0.524*
Mother's Age less than 20	0.0444	0.122	0.140	-0.0184
Mother's Age 35 or more	0.238	0.137	0.126	0.0110
Mother Married	0.650	0.355	0.329	0.0252
Mother Non-Hispanic White	0.308	0.121	0.113	0.00771
Mother Hispanic	0.166	0.256	0.258	-0.00204
Mother Non-Hispanic Black	0.290	0.488	0.487	0.000334
Mother Non-Hispanic Asian	0.211	0.108	0.101	0.00655
Mother Foreign-Born	0.532	0.495	0.498	-0.00278
Mother's Education Less than HS	0.122	0.269	0.275	-0.00556
Mother's Education HS	0.249	0.299	0.295	0.00451
Mother's Education Some College	0.273	0.275	0.275	-0.000433
Mother's Education College or More	0.355	0.152	0.151	0.00103
Observations	66,659	976	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. Column (4) presents the difference between (2) and (3), where significance levels are denoted as: * p<0.1 ** p<0.05 *** p<0.01

Table 2: Paternal Characteristics by Any Assault During/Post Pregnancy

	(1) No Assault	(2) Assault-Preg	(3) Assault-Post	(4) Diff (2)-(3)
Father's Age	33.41	30.83	30.20	0.625
Father's Age less than 20	0.0120	0.0379	0.0458	-0.00793
Father's Age 35 or more	0.373	0.223	0.224	-0.000213
Father Non-Hispanic White	0.304	0.110	0.100	0.00954
Father Hispanic	0.137	0.188	0.194	-0.00614
Father Non-Hispanic Black	0.230	0.337	0.356	-0.0193
Father Non-Hispanic Asian	0.192	0.102	0.0861	0.0164
Father Foreign-Born	0.530	0.514	0.505	0.00920
Father's Education Less than HS	0.107	0.175	0.167	0.00869
Father's Education HS	0.261	0.295	0.299	-0.00426
Father's Education Some College	0.228	0.183	0.185	-0.00182
Father's Education College or More	0.287	0.103	0.108	-0.00409
Father Info Missing	0.0717	0.140	0.135	0.00566
Observations	66,659	976	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. Column (4) presents the difference between (2) and (3), where significance levels are denoted as: * $p<0.1$ ** $p<0.05$ *** $p<0.01$

Table 3: 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	(8) Br. Index
Assault During Pregnancy	0.0154** [0.00732]	0.0136 [0.00922]	0.0214** [0.00941]	0.0185 [0.0150]	0.0124 [0.0146]	0.00186 [0.00279]	0.0768** [0.0361]	0.0556* [0.0289]
Dept. var mean	0.0254	0.0421	0.0461	0.124	0.116	0.00342	0.0446	0.0314
Indiv. obs.	2045	2045	2037	2045	2000	2045	2045	2045

B. Use of Medical Services

	(1) PNC 1Tri	(2) NVis	(3) WIC	(4) Induc	(5) Csec	(6) Compl	(7) Index
Assault During Pregnancy	0.0596*** [0.0215]	0.325* [0.174]	-0.0443** [0.0198]	0.0555*** [0.0187]	-0.0101 [0.0210]	-0.0147 [0.0148]	0.0317 [0.0198]
Dept. var mean	0.646	10.05	0.639	0.216	0.325	0.120	0.0129
Indiv. obs.	1975	2010	2036	2037	2045	2040	2045

C. Maternal Behaviors and Well-Being

	(1) Smoke	(2) Drugs	(3) Depr	(4) Low Wgt	(5) High Wgt	(6) Index
Assault During Pregnancy	0.00601 [0.00730]	-0.00369 [0.00421]	0.0126 [0.0270]	0.0155 [0.0146]	-0.00992 [0.0190]	0.0112 [0.0220]
Dept. var mean	0.0284	0.00854	0.272	0.112	0.221	0.00444
Indiv. obs.	2042	1990	1178	2021	2021	2045

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 7th column in Panel A reports results using an index outcome, which is an equally weighted average of z-scores for the outcomes reported in the panel. The 8th column in Panel A reports results using a broader index outcome, which includes the reported outcomes as well as: 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 outcomes in Panel B are: indicator for any prenatal care, 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. In Panels B and C, 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 and paternal characteristics: maternal and paternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for 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 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 4: Effects of Assault During Pregnancy on Summary Index Outcomes, Robustness

A. Include Mothers with Assaults Before Pregnancy				
	(1) Birth Out	(2) Broad Birth Out	(3) Med Serv	(4) Behav/Well
Assault During Pregnancy	0.0643** [0.0324]	0.0470* [0.0257]	0.0297* [0.0170]	0.0136 [0.0186]
Indiv. obs.	3074	3074	3074	3074
B. Multi-Family Home Sample				
	(1) Birth Out	(2) Broad Birth Out	(3) Med Serv	(4) Behav/Well
Prob. of Assault During Pregnancy	0.0261* [0.0151]	0.0288** [0.0129]	-0.0318*** [0.01000]	0.0163 [0.0122]
Indiv. obs.	29183	29183	29183	29183

Notes: Each coefficient in each panel is from a separate regression. The outcomes are four summary indices: 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. 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 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 (mothers in single-family homes are excluded). The treatment variable in Panel B is the probability of assault, assuming that the probability is equal to the number of assaults in the 10 months post conception divided by the number of units in the building. Controls are the same as in Table 3. Robust standard errors. Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table 5: Maternal Fixed Effects Models

A. Main Results, Maternal FE				
	(1) Birth Out	(2) Broad Birth Out	(3) Med Serv	(4) Behav/Well
Assault During Pregnancy	0.301** [0.133]	0.191* [0.105]	-0.0407 [0.0574]	0.00506 [0.0702]
Indiv. obs.	18600	18600	18600	18600

B. Placebo Effects, Maternal FE				
	(1) Birth Out	(2) Broad Birth Out	(3) Med Serv	(4) Behav/Well
Assault Post Pregnancy	0.0545 [0.126]	0.0334 [0.0984]	0.00584 [0.0660]	-0.174** [0.0852]
Indiv. obs.	18107	18107	18107	18107

Notes: Each coefficient in each panel is from a separate regression. The outcomes are four summary indices: 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 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 B, mothers who experience an assault in their home during any pregnancy are additionally dropped. Regressions include controls for the following time-varying maternal and paternal characteristics: 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 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 effects on the likelihood of very low birth weight births from our main model (1.3 percentage points, see Table 3) and from the maternal fixed effects model (3.4 percentage points, see Appendix Table B.10). 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[5]		
Pre/Post Pregnancy Assault Timing: $\boldsymbol{\Delta}^{\mathbf{VLBW}} \times [(1)+(2)+(3)+(4)+(5)+(6)] =$	**$36,857**	
Maternal FE: $\boldsymbol{\Delta}^{\mathbf{VLBW}} \times \underbrace{[(1)+(2)+(3)+(4)+(5)+(6)]}_{\text{Cost of VLBW}} =$	**$85,999**	

[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.

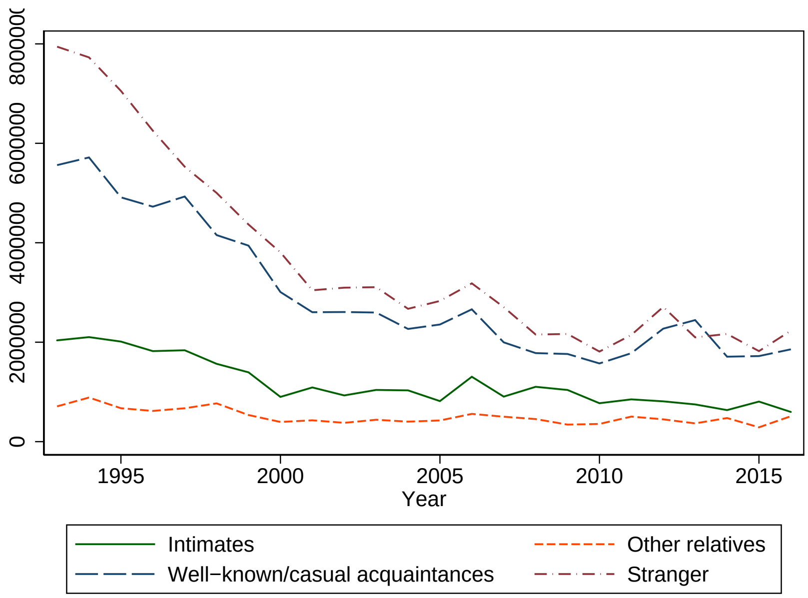
[4] We use the Social Security Administration's Period Life Table from 2015, and multiply the probability of death in each year of life following the first year by 2.8 based on the estimate from Bharadwaj *et al.* (2018). We exclude the first year to avoid double counting the impacts to infant mortality.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

[5] $\Delta^{VLBW}_{Pre/Post} = 0.015$; $\Delta^{VLBW}_{Mat.FE} = 0.035$

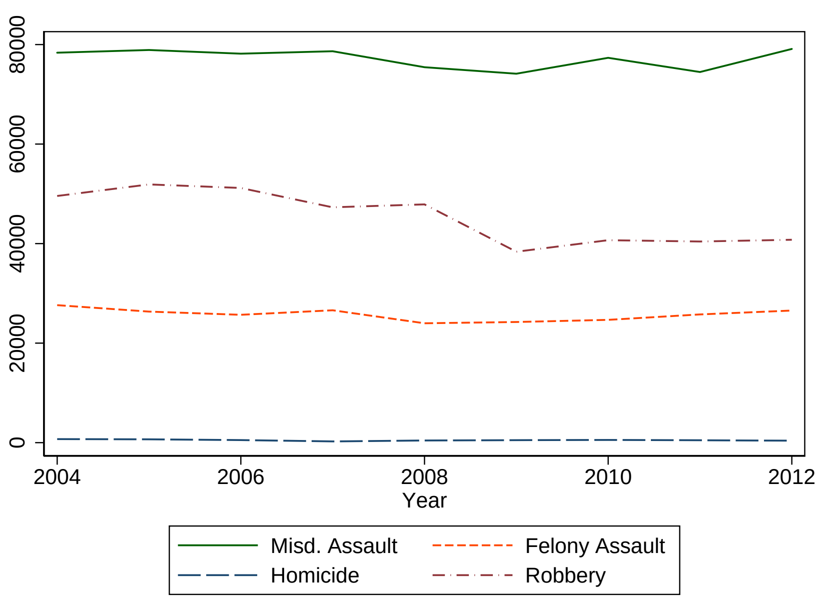
B Additional Results

Appendix Figure B.1: Trends in Violent Crimes

(a) By Victim-Offender Relationship in the NCVS

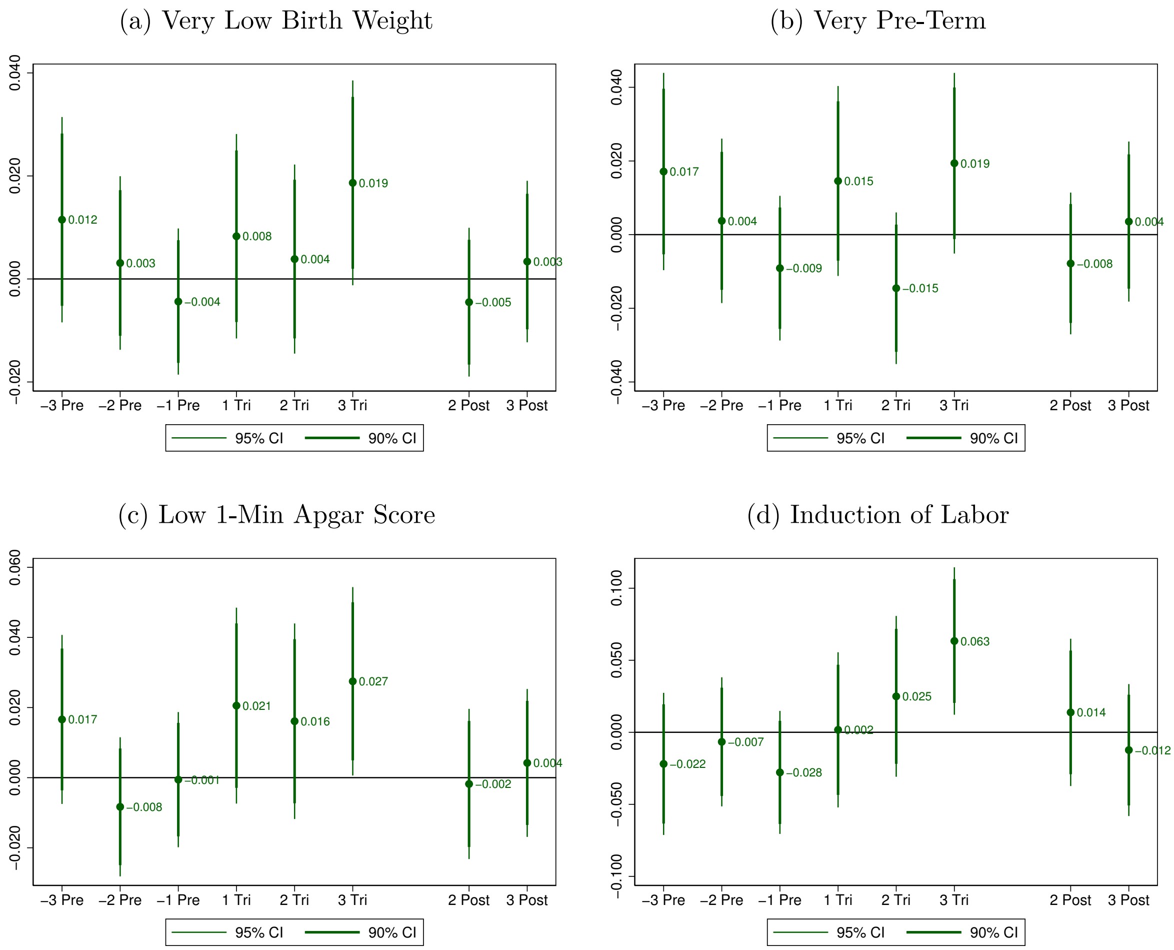


(b) By Type in New York City



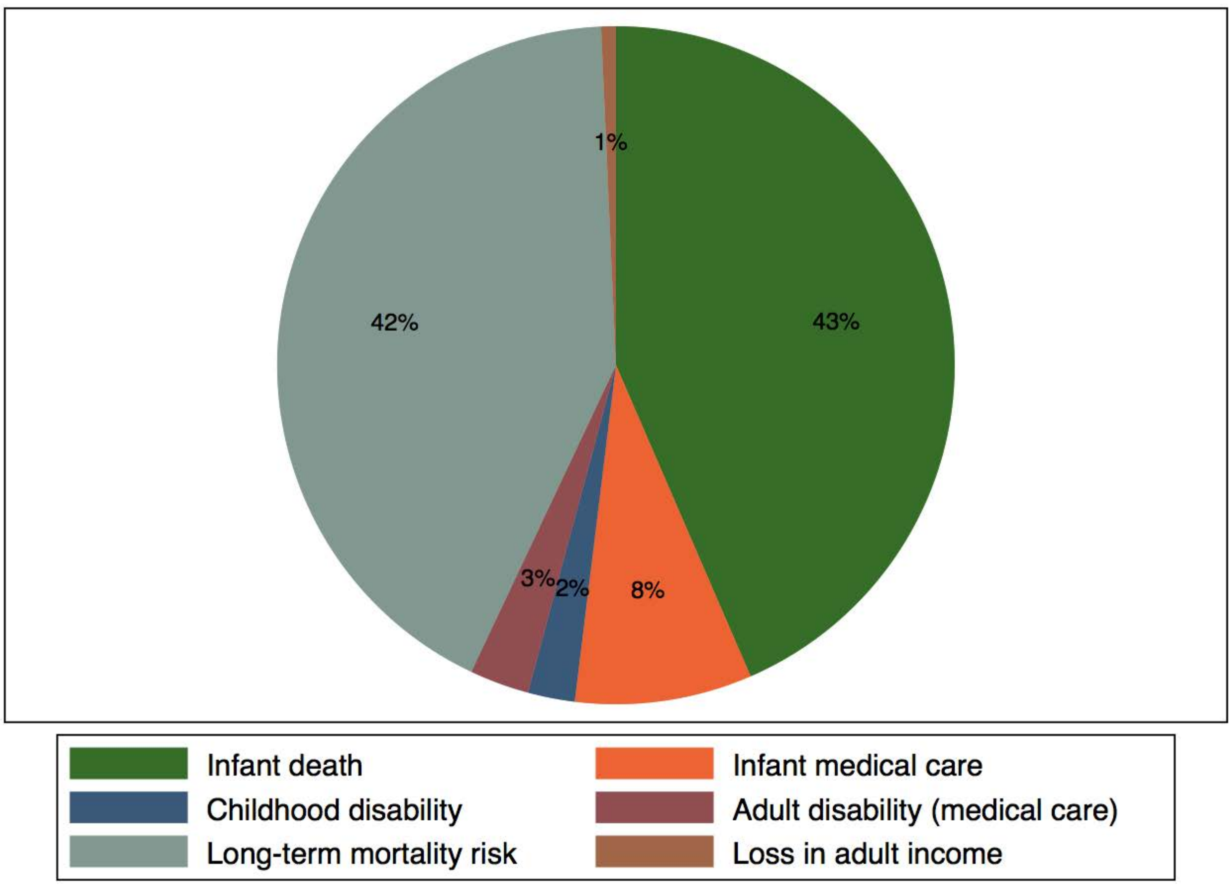
Notes: Authors' calculations based on the Bureau of Justice Statistics 1993-2016 National Crime Victimization Survey (panel a) and administrative records from the New York Police Department (panel b).

Appendix Figure B.2: Timing of Effects of Exposure to Assault



Notes: See notes under Table 3 for a description of the sample and control variables. Each graph 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).

Appendix Figure B.3: 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.

Appendix Table B.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.

Appendix Table B.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.

Appendix Table B.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.

Appendix Table B.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.

Appendix Table B.5: Association Between Assaults During Pregnancy, Maternal, and Paternal Characteristics, Main Analysis Sample

A. Maternal Characteristics

	(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	0.525*	0.0310	0.000309	0.0113	0.000830	-0.00400	-0.00291	0.00206	0.0199	-0.0161	-0.00780
Pregnancy	[0.291]	[0.0216]	[0.0228]	[0.0146]	[0.0198]	[0.0226]	[0.0226]	[0.0225]	[0.0218]	[0.0227]	[0.00692]
Dept. var mean	26.73	0.341	0.496	0.117	0.257	0.488	0.569	0.426	0.632	0.465	0.977
Indiv. obs.	2045	2045	2045	2045	2045	2045	2045	2045	2045	2045	2045

B. Paternal Characteristics

	(1) Age	(2) For	(3) Wh	(4) Hsp	(5) Bl	(6) LowEd	(7) HighEd	(8) Missing
Assault During	0.674*	0.00895	0.0121	-0.00479	-0.0263	0.00814	-0.0145	0.0171
Pregnancy	[0.408]	[0.0223]	[0.0140]	[0.0177]	[0.0215]	[0.0227]	[0.0205]	[0.0152]
Dept. var mean	30.50	0.510	0.105	0.191	0.347	0.468	0.290	0.137
Indiv. obs.	1565	2045	2045	2045	2045	2045	2045	2045

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

Appendix Table B.6: Estimated OLS Relationship Between Assaults During Pregnancy, Maternal, and Paternal Characteristics, Sample Includes Mothers with No Assaults

A. Maternal Characteristics

	(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	-2.659***	-0.277***	-0.0468***	-0.159***	0.0745***	0.190***	0.190***	-0.193***	-0.0152	0.0248	0.0115**
Pregnancy	[0.207]	[0.0155]	[0.0163]	[0.0115]	[0.0139]	[0.0162]	[0.0160]	[0.0160]	[0.0155]	[0.0160]	[0.00522]
Dept. var mean	29.70	0.641	0.531	0.302	0.169	0.296	0.377	0.621	0.661	0.419	0.961
Indiv. obs.	68701	68704	68704	68704	68704	68704	68704	68704	68704	68704	68704

B. Paternal Characteristics

	(1) Age	(2) For	(3) Wh	(4) Hsp	(5) Bl	(6) LowEd	(7) HighEd	(8) Missing
Assault During	-2.489***	-0.0245	-0.168***	0.0380***	0.101***	0.0977***	-0.217***	0.0658***
Pregnancy	[0.289]	[0.0160]	[0.0111]	[0.0124]	[0.0153]	[0.0161]	[0.0147]	[0.0108]
Dept. var mean	33.33	0.529	0.299	0.139	0.234	0.371	0.508	0.0736
Indiv. obs.	60714	68704	68704	68704	68704	68704	68704	68704

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

	(1) Birth Out	(2) Broad Birth Out	(3) Med Serv	(4) Behav/Well
Assault During Pregnancy	0.0766^{***} [0.0288]	0.0584^{***} [0.0226]	0.0391^{***} [0.0142]	0.0383^{**} [0.0155]
Indiv. obs.	68704	68704	68704	68704

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 and paternal characteristics: maternal and paternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for 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 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.8: "Difference-in-Difference" Effects of Assault During Pregnancy on Summary Index Outcomes, Relative to Any Crime

	(1) Birth Out	(2) Broad Birth Out	(3) Med Serv	(4) Behav/Well
Assault During Pregnancy	0.0767**	0.0522*	0.0182	0.0182
	[0.0360]	[0.0288]	[0.0201]	[0.0225]
Mis/Fel Assault During or Post Pregnancy	-0.0111	-0.00668	0.0202	0.00942
	[0.0222]	[0.0182]	[0.0141]	[0.0161]
Any Crime During Pregnancy	0.00912	0.00722	0.00440	-0.00932
	[0.0133]	[0.0109]	[0.00823]	[0.00871]
Indiv. obs.	11566	11566	11566	11566

Notes: The outcomes are four summary indices: 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. 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 and paternal characteristics: maternal and paternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for 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 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.9: Sensitivity of Estimates to Changing Control Variables, with Oster (2017) Calculation

	Outcome: Adverse Birth Outcome Index			
	(1)	(2)	(3)	(4)
Assault During Pregnancy	0.083** [0.037]	0.083** [0.037]	0.080** [0.036]	0.077** [0.036]
Conception Year & Month FE	Y	Y	Y	Y
Child Characteristics	Y	Y	Y	Y
Maternal Characteristics	N	Y	Y	Y
Paternal Characteristics	N	N	Y	Y
Borough FE	N	N	N	Y
R^2	0.040	0.048	0.052	0.054
Lower Bound Treatment Effect (β)	0.031	0.044	0.035	0.025
Null Effect Selection Ratio (δ)	1.411	1.759	1.556	1.361
No. of obs.	2045	2045	2045	2045

Notes: Each coefficient is from a separate regression, where the dependent variable is the adverse birth outcome index. 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. The controls for child characteristics are: indicator for singleton birth and parity dummies (1st, 2nd, 3rd+, missing). The controls for maternal characteristics are: maternal age dummies (<20, 20-24, 25-34, 35+, missing), indicator for mother being married, indicators for maternal nativity status (U.S.-born, foreign-born, missing), 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). The controls for paternal characteristics are: paternal age dummies (<20, 20-24, 25-34, 35+, missing), indicators for paternal nativity status (U.S.-born, foreign-born, missing), paternal race/ethnicity dummies (non-Hispanic white, Hispanic, non-Hispanic black, other, missing), 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). The reported β parameter is a lower bound on the treatment coefficient under the assumption of equal degree of selection on included control variables in the model and the unobservables ($\delta = 1$). The reported δ parameter is the ratio of the selection on unobservables to the selection on included control variables that would be required to obtain a treatment coefficient of zero ($\beta = 0$). The calculation assumes $R^{max} = 0.18$, which is the R^2 from our maternal fixed effects model that includes all time-varying maternal and paternal controls (equation (3)) for the adverse birth outcome index as the outcome. We follow Oster (2017) in using a within-family regression model to choose R^{max}. Note that Oster (2017) also proposes setting R^{max} to be equal to $1.3 \times \tilde{R}^2$, where $\tilde{R}^2$ is the R^2 from the regression with all observable controls (column 4). If we were to set $R^{max} = 1.3 \times 0.054 = 0.07$, we would obtain larger δ and β parameters, making our choice of $R^{max} = 0.18$ conservative. Robust standard errors. Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Appendix Table B.10: Effects of Assault During Pregnancy on Individual Outcomes, Maternal Fixed Effects Model

A. Birth Outcomes

	(1) VLBW	(2) V Pret	(3) Low 1m Apg	(4) NICU	(5) Abn/Con	(6) Death	(7) Index	(8) Br. Index
Assault During Pregnancy	0.0352 [0.0244]	0.0568** [0.0286]	0.0491* [0.0275]	0.0722** [0.0338]	0.0350 [0.0315]	0.0109 [0.00964]	0.301** [0.133]	0.191* [0.105]
Dept. var mean	0.00973	0.0178	0.0261	0.0704	0.0707	0.00167	0.00377	0.00294
Indiv. obs.	18599	18600	18570	18600	18279	18599	18600	18600

B. Use of Medical Services

	(1) PNC 1Tri	(2) NVis	(3) WIC	(4) Induc	(5) Csec	(6) Compl	(7) Index
Assault During Pregnancy	0.00663 [0.0594]	-0.259 [0.485]	-0.0895* [0.0538]	0.000829 [0.0491]	-0.0206 [0.0310]	-0.00622 [0.0387]	-0.0407 [0.0574]
Dept. var mean	0.780	10.67	0.322	0.178	0.277	0.0957	-0.000451
Indiv. obs.	18002	18358	18508	18566	18600	18558	18600

C. Maternal Behaviors and Well-Being

	(1) Smoke	(2) Drugs	(3) Depr	(4) Low Wgt	(5) High Wgt	(6) Index
Assault During Pregnancy	0.0128 [0.0235]	0.0000715 [0.000746]	-0.0196 [0.0910]	0.0131 [0.0362]	-0.0605 [0.0436]	0.00506 [0.0702]
Dept. var mean	0.0120	0.00181	0.172	0.0858	0.176	0.000933
Indiv. obs.	18586	18217	11527	18481	18481	18600

Notes: Each coefficient in each panel is from a separate regression. See notes under Table 3 for description of the outcomes. 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. Regressions include controls for the following time-varying maternal and paternal characteristics: 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 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

C Supplementary Analysis of Data from the Fragile Families and Child Wellbeing Study

An important concern for our research design is that our treatment and comparison groups differ on margins not observable in our administrative data set. To address this concern, we supplement our analysis with one of the few other data sources that has information on IPV exposure during pregnancy and in the postpartum period: The Fragile Families and Child Wellbeing Study (FFCWS). FFCWS is a panel survey data set that follows a cohort of new births in 20 large U.S. cities between 1998 and 2000. Although the survey yields a much smaller sample size than our administrative data from New York City, FFCWS oversamples births to unmarried parents, which leads to an increased prevalence rate of domestic violence among respondents. Importantly, FFCWS contains detailed information on the mothers, as well as their relationships and evolving family circumstances, which helps us provide a more thorough evaluation of the plausibility of our identifying assumptions. Specifically, we examine differences in available time-invariant and time-varying characteristics across mothers with exposure to IPV during pregnancy and those with exposure in the year following childbirth.

Among respondents who completed both the baseline survey and the year one follow-up survey, 295 (11%) of respondents report some degree of IPV either while pregnant or in the year following childbirth.[42] In Appendix Table C.1, we divide the sample into two groups: those who report IPV initiation in the baseline survey ("IPV Baseline") and those who report IPV initiation in the year one follow-up ("IPV 1-Year"). Note that these groups conform with the definitions that we use in our main analysis.

Appendix Table C.1 shows that the sample is roughly equally split between mothers for whom IPV initiated during the baseline (pregnancy) period and those for whom IPV initiated one year later, which is consistent with the fact that we observe similar levels of assault initiations in our treatment and comparison groups in the administrative data. The table also demonstrates balance on a range of fixed baseline characteristics observed in FFCWS, both factors that we can also measure in the administrative records as well as a number of additional individual and household characteristics only available in survey data (e.g., baseline relationship status, household composition, and household income).

Dynamic selection, however, is likely the larger concern given that we show balanced on fixed maternal characteristics in our main analysis sample. It may be that the determinants

[42]We measure this rate through combining several questions from the baseline and year one follow-up survey. From the mother's baseline survey, we use questions: B7B, B13B, and B25B. From the mother's year one follow-up survey, we use questions: D6H, D6I, D8H, D8I, D9A2, D9A3, E8H, E8I, E9A2, and E9A3. When questions ask about the frequency of occurrence, we code any positive response as experiencing IPV.

of IPV during versus after pregnancy are different, and that these omitted characteristics evolve over time and directly influence both IPV and birth outcomes. For example, some men may initiate IPV because of paternity uncertainty, which is amplified once the baby is born and has a physical resemblance to someone other than the father. Thus, the types of children exposed to IPV during gestation may be systematically different from those whose mothers experience IPV postpartum. Another possibility is that changes in household income or employment could both influence IPV and infant health.

Appendix Table C.2 examines these hypotheses. The first four rows evaluate whether the resemblance of the baby at baseline and the child's sex are correlated with the timing of IPV. The last two rows consider whether changes in the household's income and poverty ratio from the baseline survey to the one year follow-up influence the likelihood of post-natal IPV initiation. The results show only modest differences between those with IPV initiation during versus post pregnancy in each of these potential dynamic mechanisms. Across all of the comparisons, we cannot reject the null hypothesis that there is no difference in the sample means. We conclude that, even if this form of dynamic selection is a factor, it likely plays only a modest role in the overall determinants of IPV in the household.

Appendix Table C.1: Balance on Fixed Baseline Characteristics in the FFCWS Sample

Variable	IPV Baseline	IPV 1-Year	Difference
Race/Ethn. = Hispanic	0.350	0.310	-0.040
	(0.030)	(0.030)	(0.050)
Race/Ethn. = White, Not Hisp.	0.140	0.150	0.010
	(0.020)	(0.030)	(0.040)
Race/Ethn. = Black, Not Hisp.	0.470	0.530	0.060
	(0.040)	(0.040)	(0.050)
Age at Birth	23.98	23.2	-0.780
	(0.420)	(0.360)	(0.550)
Educ. = Less than High School	0.560	0.520	-0.040
	(0.040)	(0.040)	(0.050)
Educ. = High School	0.220	0.230	0.010
	(0.030)	(0.030)	(0.040)
Educ. = Post-Secondary Education	0.230	0.250	0.020
	(0.030)	(0.030)	(0.040)
Married	0.090	0.080	-0.010
	(0.020)	(0.020)	(0.030)
Cohabiting, Not Married	0.430	0.490	0.060
	(0.040)	(0.040)	(0.050)
Years knowing father before pregnancy	3.720	3.150	-0.570
	(0.270)	(0.230)	(0.360)
Total minors in Household	1.410	1.280	-0.130
	(0.090)	(0.100)	(0.140)
Total adults in Household	2.290	2.270	-0.020
	(0.080)	(0.070)	(0.100)
Baseline Household Income	21,185	23,596	2,411
	(1,380)	(1,672)	(2,163)
Observations	200	195	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table C.2: Potential IPV response to Birth Characteristics in the Fragile Families Sample

Variable	IPV Baseline	IPV 1-Year	Difference
Baby resembles Mother	0.260	0.330	0.070
	(0.040)	(0.040)	(0.050)
Baby resembles Father	0.570	0.520	-0.050
	(0.040)	(0.040)	(0.060)
Baby resembles non-parent	0.170	0.150	-0.020
	(0.030)	(0.030)	(0.040)
Baby is male	0.540	0.470	-0.070
	(0.040)	(0.040)	(0.050)
Δ Household Income	-1,558	-1613	-55
	(1,380)	(1,984)	(2,406)
Δ Household Poverty Ratio	-0.260	-0.390	-0.130
	(0.080)	(0.120)	(0.140)
Observations	200	195	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.