NBER WORKING PAPER SERIES

THE HIDDEN COST OF FIREARM VIOLENCE ON PREGNANT WOMEN AND THEIR INFANTS

Janet Currie
Bahadir Dursun
Michael Hatch
Erdal Tekin

Working Paper 31774
http://www.nber.org/papers/w31774

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
October 2023

The authors thank seminar participants at San Diego State University, Indiana University, University of Virginia, University of Kansas, Cornell University, University of Warwick, Royal Holloway, Newcastle University, University of Sussex, University of Edinburgh, and conference and workshop participants at the Southern Economic Association, INSANE, Public Attitudes and Individual Wellbeing Workshop, and the 12th Workshop on the Economics of Risky Behavior. The use of Virginia Vital Statistics Natality data for this project has been reviewed and approved by the Institutional Review Board of the Virginia Department of Health (Study # 40251). The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

The Hidden Cost of Firearm Violence on Pregnant Women and Their Infants
Janet Currie, Bahadir Dursun, Michael Hatch, and Erdal Tekin
NBER Working Paper No. 31774
October 2023
JEL No. I12,I18,J13,K40

ABSTRACT

Firearm violence is a pervasive public health crisis in the United States, with significant numbers of homicides involving firearms, including indiscriminate shootings in public spaces. This paper investigates the largely unexplored consequences of stress induced by these attacks on newborn health. We use two approaches to examine this question. First, we consider the "beltway sniper" attacks of 2002 as a natural experiment, using administrative birth records with maternal residential addresses in Virginia. The beltway sniper attacks, a series of random shootings in the Washington DC metropolitan area, led to significant terror and disruptions in daily life over a three-week period. We compare birth outcomes of children exposed to the attacks in utero due to timing or having a residential address near a shooting location to those who were not exposed. Second, we investigate the impact of in-utero exposure to mass shootings on infant health outcomes using restricted-access U.S. Vital Statistics Natality records from 2006 to 2019 and leveraging variation in the timing of mass shootings in counties where at least one shooting occurred.

Our findings reveal that mass shootings impose substantial, previously unconsidered costs on pregnant women and their infants. Exposure to the beltway sniper attacks during pregnancy increased the likelihood of very low birthweight and very premature birth by 25 percent. The analysis based on national data from mass shootings confirms these findings, albeit with smaller effect sizes. These results underscore the need to recognize the broader impact of violence on vulnerable populations when assessing the true costs of firearm violence. Calculations based on our estimates suggest significant economic burdens, with the additional costs of the beltway sniper attacks reaching $15.5 billion in 2023 dollars and mass shootings imposing annual costs of seven billion dollars. These findings suggest that pregnant women and their infants may require additional support in the aftermath of mass gun violence.

Janet Currie
Department of Economics
Center for Health and Wellbeing
185A Julis Romo Rabinowitz Building
Princeton University
Princeton, NJ 08544
and NBER
jcurrie@princeton.edu

Bahadir Dursun
5 Barrack Road
Newcastle University Business School
Newcastle on Tyne
United Kingdom
and IZA
Bahadir.Dursun@newcastle.ac.uk

Michael Hatch
School of Public Affairs
American University
4400 Massachusetts Avenue NW
Washington, D.C. 20016-8070
United States
mhatch@american.edu

Erdal Tekin
School of Public Affairs
American University
4400 Massachusetts Avenue NW
Washington, DC 20016-8070
and IZA
and also NBER
tekin@american.edu

1. Introduction

Firearm violence is a public health epidemic, affecting communities all over the United States. One of the many disturbing aspects of this epidemic is the persistent and increasing occurrence of homicides committed with a firearm. According to the Centers for Disease Control and Prevention (CDC), 19,592 people died from firearm homicides in the United States in 2022, accounting for 41 percent of total deaths from firearms, with the rest mainly accounted for by suicides.[1] In addition to these deaths, more than four times as many Americans each year sustain non-fatal firearm injuries (Kaufman et al., 2021). Between 2019 and 2020, the firearm homicide rate increased by about 35 percent, from 4.6 to 6.1 per 100,000 persons, reaching the highest recorded level in over 25 years (Kegler et al., 2022).

A particularly alarming symptom of the firearm violence epidemic is the increase in the frequency and lethality of incidents in which an individual shoots indiscriminately at people in a public area. These active shooter or mass shooting incidents[2] not only result in direct harm through loss of life and medical expenses, but also have the potential to cause psychological harm such as post-traumatic stress, fear, and depression among members of affected communities (Lowe and Galea, 2017; Rossin-Slater et al., 2020; Brodeur and Yousaf, 2022; Soni and Tekin, forthcoming). Furthermore, the severity of these effects appears to be positively related to the intensity and the physical proximity of the shooting incident (Thoresen et al., 2012; Shultz et al., 2014).

---

[1] See https://publichealth.jhu.edu/2023/cdc-provisional-data-gun-suicides-reach-all-time-high-in-2022-gun-homicides-down-slightly-from-2021.

[2] For example, there were 61 incidents designated as active shooter incidents in 2021 by the Federal Bureau of Investigation (FBI), which represents a 53 percent increase from 2020 and a 97 percent increase from 2017 (FBI, 2021). The FBI defines an active shooter as one or more individuals actively engaged in killing or attempting to kill people in a populated area. Implicit in the definition of this term is the use of firearms as the weapon of choice. "Mass shootings" are a related type of incident. Although there is no universally accepted or official definition of a mass shooting, the general criteria used to categorize an event as a mass shooting incident is the murder of four or more people (not including the shooter) with a firearm in a single incident that is not related to more conventionally motivated crimes such as armed robbery, gang shootings, or drug violence.

There has been a rise in high-fatality indiscriminate killings in public spaces in recent years (Agnich, 2015; Lowe and Galea, 2015, 2017; Webster, 2017; Lin et al., 2018), accompanied by increased media coverage (Roeder, 2016; Jetter and Walker, 2018). This greater media attention has led to increased public awareness despite the fact that random shootings are still rare and account for less than one percent of firearm deaths, sparking significant public concern and fear (Luca et al., 2020). A recent survey by the American Psychological Association shows that nearly 80 percent of adult Americans live with stress due to the possibility of a mass shooting, and about three-quarters report changing their behavior to avoid becoming a victim.[3] As a result of the staggering human toll, there are persistent calls for lawmakers to take substantive action to help curb this violence.[4]

There is a large literature in economics devoted to understanding the determinants of firearm violence and the effects of the policies and programs aimed at reducing its prevalence (Duggan, 2001; Edwards et al., 2018; Cook and Ludwig, 2000; Luca et al., 2017; McClellan and Tekin, 2017; Anderson et al., 2021). Yet relatively little is known about the impacts of firearm violence beyond the effects on those who are directly victimized. Information is especially lacking about the effects on some of the most vulnerable groups in society such as those who are pregnant and their infants.[5]

The goal of this paper is to examine the causal effect of stress induced by the fear of gun violence during pregnancy on newborn health. To accomplish this goal, we rely on two sources of data, which have complementary strengths. The first uses the 2002 "beltway sniper" attacks as

[3] See https://www.apa.org/news/press/releases/2019/08/fear-mass-shooting.
[4] The U.S. Senate recently voted 65 to 33 to pass a bipartisan gun control bill, the Safer Communities Act, which was considered to be the most significant legislation addressing guns in nearly 30 years. President Biden signed the bill into law on June 26, 2022.
[5] This is partly because victim identities are generally withheld for confidentially reasons.

a natural experiment and leverages administrative birth records with information on maternal residential addresses from Virginia. As explained in detail below, the beltway sniper attacks were a series of random shootings in the Washington DC metropolitan area and along Interstate 95 in Virginia between October 2nd and 22nd, 2002. The attacks left ten people dead and three people critically injured. During this three-week period, millions of people living in and around the areas that experienced shootings were terrorized and experienced major disruptions in their lives. Our primary empirical strategy involves comparing the birth outcomes of children in utero during the time and near the locations of the shootings to those of children who were not exposed to the attacks in utero because of timing or because their mothers resided farther from the attacks.

To provide context for the sniper attacks analysis and shed light on the broader implications of U.S. firearm violence, we perform a second analysis, examining the effect of exposure to mass shootings during pregnancy on birth outcomes using all U.S. Vital Statistics Natality records from 2006 to 2019. Specifically, we estimate the impact of in-utero exposure to mass shootings on infant health outcomes using a difference-in-differences research design, taking advantage of arguably exogenous variation in the occurrence of these incidents across counties and over time. We anticipate these estimates to be smaller than the estimated effects of the beltway sniper attacks. This is primarily due to the time-limited nature of mass shootings, in contrast to the three-week duration of the sniper attacks, and because counties represent large geographic units where not all residents are likely to have been equally affected by a shooting.[6] Nonetheless, examining the pattern of estimated effects of mass shootings on birth outcomes helps us to assess the external validity of the beltway sniper estimates.

[6] An additional reason is that the national Vital Statistics Natality data include month of birth but not date of birth so that the timing is less precise in this second analysis.

The goal of this paper is to examine the causal effect of stress induced by firearm violence during pregnancy on infant health. Our analysis indicates that extreme incidences of firearm violence have substantial costs on third parties that have not been considered previously. Exposure to the beltway sniper attacks during pregnancy increased the likelihood of very low birthweight (birthweight<1,500 grams) and extreme prematurity (gestational length<32 weeks) by 25 percent. The impact on birthweight is most pronounced for those exposed during the first and second trimesters, with effect sizes of 40 and 35 percent, respectively, while no discernible effect is observed for those exposed during the third trimester. For extreme prematurity, the effects are concentrated among those exposed during the first six months of pregnancy, especially during the second trimester, with an effect size of approximately 30 percent. The analysis based on national data from mass shootings confirms the finding that extreme incidences of firearm violence inflict significant costs on infants affected in utero, though as predicted the effect sizes in the national mass shooting analysis are smaller.

These findings highlight the importance of considering the broader impact of violence on vulnerable populations when assessing the true costs of such incidents. Calculations based on our findings suggest that the additional costs of the beltway sniper attacks because of harms to children in utero totalled $15.5 billion in 2023 dollars. Our estimates using national data imply that effects of mass shootings on children in utero impose costs of seven billion annually. These results suggest that pregnant women and their infants are likely to need additional support in the form of counselling and access to health care following incidents of mass gun violence and should be remembered when policies to help victims are implemented and in public health surveillance. More broadly, these additional costs of violence underline the need for effective policy responses to firearm violence.

The rest of the paper is laid out as follows. Section 2 provides background information about the effects of stress during pregnancy, while Section 3 describes relevant features of the beltway sniper attacks. The data are described in Section 4, and Section 5 describes our methods. Section 6 presents the results, followed by a series of robustness checks and placebo analyses in Section 7. Section 8 provides a discussion of mechanisms, while Section 9 provides a calculation of the costs of mass shootings on children in utero. Conclusions appear in Section 10.

2. Stress During Pregnancy and Birth Outcomes

Infant health at birth is an important predictor of an individual's success later in life. Low birthweight is associated with poorer cognitive development and educational attainment as well as lower employment and earnings (e.g., Black et al., 2007; Currie and Moretti, 2007; Oreopoulos et al., 2008; Currie et al., 2010; Figlio et al., 2014). Additionally, adverse conditions during pregnancy cause poor health among newborn children, which often persists over time and adversely affects human capital accumulation and labor market productivity. Most of the literature documenting these effects considers physical health insults, such as famines (Almond et al., 2010; Scholte et al., 2015), radiation (Black et al., 2019), extreme temperature (Bruckner et al., 2014), malnutrition (Almond and Mazumder, 2011), and air pollution (Chay and Greenstone, 2003, 2005; Currie and Walker, 2011; Currie and Schwandt, 2016).

There is a medical literature documenting a relationship between maternal prenatal stress and birth outcomes, but the causal interpretation of these results is hindered by the fact that exposure to firearm violence is not random but related to a myriad of individual and neighborhood factors such as economic deprivation, discrimination, social disorganization, and or poor health (Friedson and Sharkey, 2015; Sampson et al., 1997; Sampson, 2011).[7]

A growing number of economic and social science studies have sought to measure the causal effects of stress during pregnancy. Aizer et al. (2016) use a sample of pregnancies from the early 1960s in Providence and Boston to estimate the effect of cortisol levels during pregnancy on educational attainment in primary school. This study has the advantage of having a direct measure of stress (cortisol levels), and directly demonstrating a relationship between cortisol and future child outcomes. It does not however, focus on the impact of individual particularly stressful events like gun violence.

Black et al. (2016) and Persson and Rossin-Slater (2018) examine the effects of stress experienced in utero due to the death of close relatives in Norway and Sweden, respectively, using variations in the precise timing of bereavement relative to the child's birth. Similarly, Currie et al. (2022) explore the impact of intimate partner assaults during pregnancy on infant health outcomes, comparing the effects of incidents before or after pregnancy.

Several studies focus on stress-inducing events and experiences such as earthquakes (Glynn *et al*., 2001; Torche, 2011), hurricanes (Currie and Rossin-Slater, 2013), the terrorist attacks of 9/11 (Lauderdale, 2006; Brown, 2014), and the prevalence of landmines (Camacho, 2008) to make inferences about the impact of fetal exposure to stress on infant health. While these

[7] The association between maternal prenatal stress and adverse birth outcomes is well-described in the medical literature and several causal pathways have been hypothesized. For instance, stress stimulates the production of corticotrophin-releasing hormone (CRH), which has adverse effects on infant health outcomes (Hobel and Culhane, 2003; Grote et al., 2010). Similarly, both animal and human studies have shown that stress in utero may have re-programming effects on neurodevelopment and the hypothalamic-pituitary adrenal axis (HPA), which could affect a developing fetus (Weinstock, 2008; Glover et al., 2010). Furthermore, stress during pregnancy may suppress the mother’s immune system, and negatively affect the offspring’s developing immune system (Stott, 1973; Weinstock, 2008). Aside from these channels, adverse behavioral responses to stress, such as maternal cigarette smoking or alcohol consumption, can also have adverse effects on the health of the fetus.

studies overcome the challenge of non-randomness of exposure, it can be difficult to separate out the effect of stress from the other effect of other impacts of these events and experiences.

This paper contributes to the existing literature on the influence of maternal stress during pregnancy by focusing on psychological stress caused by firearm violence. Arguably our results mainly reflect the effects of stress per se rather than, for example, effects on family income and organization that could result from a stressful event, or direct personal exposure to hardship or physical violence.

There are two studies that take on the question of the relationship between prenatal exposure to firearm violence and birth outcomes, while also attempting to account for the potential endogeneity of exposure. Torche and Villarreal (2014) examine the effect of maternal exposure to local homicides on birthweight using monthly Mexican data at the municipality level from 2008 to 2010. The authors control for municipality fixed effects, implicitly assuming that violence is the only factor that varies over time within each city. Surprisingly, they find that exposure to homicides in the first trimester increases infant birthweight and reduces the proportion of low birthweight children. The authors argue that pregnant women exposed to local violence engage in health-enhancing behaviors such as increased use of prenatal care. However, in municipality-level data it is difficult to rule out other explanations such as differential migration in response to violence.

Goin et al. (2020) use data on birth, death, emergency department, and hospital records from California between 2007 and 2011 to estimate the association between living in a neighborhood with high firearm violence and preterm delivery. The authors use a propensity score approach combined with a machine learning algorithm to identify neighborhoods with similar characteristics but differing levels of firearm violence. They find that firearm violence is associated with an increased risk of preterm delivery, which is partially mediated by increases in infections and substance use.

Finally, two recent studies focus on the effect of school shootings on the mental health of a different vulnerable group: school-aged children. Rossin-Slater et al. (2020) look at the effects of 44 school shootings on prescriptions of antidepressants among children living within five miles of a school compared to those living 10 to 15 miles away. Bharadwaj et al. (2021) examine the effects of the mass shooting in Norway on July 22, 2011. They compare survivors to a matched control group and find that survivors have lower test scores, more health visits, more diagnoses of mental health problems, and ultimately fewer years of schooling and lower labor force participation. They also find effects on the mental health of parents and siblings of survivors.

3. The Beltway Sniper Attacks and the Definition of Mass Shootings

The beltway sniper attacks were a series of shootings that occurred in October 2002 in the Washington, D.C., metropolitan area and along Interstate-95 in Virginia. The attacks were perpetrated by two gunmen, who targeted seemingly random individuals in various public locations. Over a period of three weeks, between October 2 and 22, 2002, ten people were killed, and another three were critically injured by precisely aimed rifle shots fired from a distance. The shootings occurred in multiple locations, with five attacks in Virginia resulting in three deaths and two serious injuries, while the other eight incidents took place within several miles of the Virginia border in Maryland and the District of Columbia. A timeline for the shootings along with a short description of the victims is presented in Table 1.[8] The locations of these thirteen shootings are marked in the map displayed in Figure 1.

The beltway sniper attacks generated a significant amount of fear and anxiety among the residents of the affected areas. The victims were chosen indiscriminately without any apparent motive, shot down during their regular activities like mowing the lawn, pumping gas, shopping, or reading a book on a bench. The snipers targeted both men and women, showing no distinct preference for the race or age of the victims. The unpredictable and seemingly random nature of the shootings caused panic and lead to widespread behavioral changes, as people sought to protect themselves and their families from potential harm. Individuals altered their routines by skipping work and school, running or weaving through parking lots to avoid being targeted, canceling outdoor activities, and avoiding shopping centers and gas stations in the affected areas. (Coppola, 2004; Zivotofsky and Koslowsky, 2005; Mitchell, 2007). The attacks had detrimental effects on the mental health of individuals living in the communities bordering the affected areas. For example, Schulden et al. (2006), analyze contemporaneous data from a survey of 1,205 adults in the communities impacted by the shootings. They find that 44 percent of respondents experienced at least one symptom of traumatic stress, while 7 percent reported symptoms consistent with a diagnosis of post-traumatic stress disorder.

Many studies have shown that experiencing mass trauma can lead to high rates of psychiatric issues, impacting not only the individuals directly affected but also those indirectly exposed (Schultz et al., 2014; Soni and Tekin, forthcoming). For instance, a study of survivors of

[8] The initial shooting of the beltway sniper attacks occurred at 5:20 p.m. inside a craft store in Maryland, approximately an hour before the first deadly shooting took place several miles away near a food warehouse. This shooting is excluded from the analysis since no one was injured and it did not generate much public anxiety or panic. Including this incident as part of the beltway sniper shootings does not alter our results, which is not surprising given its close geographical proximity to a subsequent fatal shooting on the same day.

the Oklahoma City bombing found that over one third of those exposed to the blast developed post-traumatic stress disorder (PTSD) (North et al., 1999). Following the September 11 terrorist attacks, 7.5 percent of the overall Manhattan resident population exhibited PTSD symptoms, with this number rising to 20 percent for those living closest to the attacks (Galea et al., 2002).

The perpetrators used a modified car with a hidden compartment that allowed them to shoot from a concealed position, making it difficult for witnesses to identify the source of the gunfire. The shootings ultimately led to one of the largest criminal manhunts in U.S. history, involving federal, state, and local law enforcement agencies. Ultimately, the intensive search concluded in the arrest of the two gunmen at 3:15 a.m. on October 24, 2002 after police were tipped off by two 911 calls from a rest stop in Maryland where the two snipers were sleeping in their car. Two men, one aged 41 and the other aged 19 were found guilty of murder and weapon charges in 2003. The older one was executed in 2009, while the younger man was sentenced to life in prison without parole.

The beltway sniper shootings differed from some of the other mass trauma events studied in the literature in several ways that may have led to a higher mental toll. First, the sniper attacks took place over an extended period of approximately three weeks. This prolonged duration meant that the public was exposed to a sustained period of fear and uncertainty, which can exacerbate psychological distress. The elusive nature of the snipers and the difficulty authorities faced in apprehending the perpetrator could have intensified feelings of powerlessness and vulnerability in the community and amplified the emotional impact of the attacks.

Our analysis of national data on mass shootings uses a measure of mass shootings that aligns with the FBI's definition of mass murderers, in keeping with other recent studies (Krouse and Richardson, 2015; Luca et al. 2020; Yousaf, 2021; Brodeur and Yousaf, 2022; Soni and Tekin, forthcoming).[9] Specifically, the criteria for defining a mass shooting are as follows: (1) The incident resulted in the death of four or more individuals, excluding the perpetrator(s); (2) Firearms were the primary weapons used in the incident; and (3) The incident is unrelated to gang, drug, or other criminal activity. The last condition helps to identify incidents that a pregnant person might perceive as representing an increased threat to their own safety.

4. Data

The analysis of the beltway sniper attacks relies on the Vital Statistics Natality records from Virginia between 1998 and 2004. These data cover the universe of births in the state and include detailed information about health at birth and background information about the mother. A contractual agreement with the Virginia Department of Health, allowed us to use a restricted version of these records which includes the mother's residential address. These data also include the child's sex, birth order, plurality, birthweight in grams, an indicator for any congenital abnormalities, gestational length in weeks, the Apgar score, and maternal education. Furthermore, the data have information about the delivery, such as whether the birth was through cesarean section, if labor was induced, whether the mother experienced gestational hypertension, and a number of complications that may occur during labor or delivery (e.g., premature rupture of membranes). Lastly, they include information about maternal behaviors during pregnancy, including the frequency of prenatal care visits and whether the mother smoked either before or during pregnancy.

[9] The FBI's definition of a "mass murderer" is an individual who kills four or more people, not including themselves, in a single occurrence, typically in a single location, with no distinctive period between the murders (Federal Bureau of Investigation, 2008).

The original data set has 690,340 birth records. Over 91 percent (628,596) had addresses that were successfully geocoded.[10] The distance between each of the maternal addresses and each of the thirteen sniper shooting locations was calculated using the latitude and longitude of each location and assigning the shortest distance to be the most relevant shooting for each residential address. Mothers residing within a 10-mile radius of the closest shooting were considered part of the treatment group while those outside a 10-mile radius were the control group. We also present alternative estimates using radii of seven miles or five miles.

As a final restriction, the main analysis includes only mothers who lived within a 50-mile radius of any shooting. This restriction reduces the number of observations to 350,651. Virginia is a state with large variations between urban and rural areas concerning socioeconomic status, cultural norms, political perspectives, and attitudes towards gun ownership. Moreover, all of the shootings occurred in relatively urban areas.[11] Limiting the analysis sample to mothers who resided within a 50-mile radius of the closest shooting helps to create treatment and control groups that resemble each other more closely. However, we also present estimates based on a sample relaxing this restriction.

The primary measures of birth outcomes are an indicator for very low birthweight (below 1,500 grams) and very preterm (less than 32 weeks of gestation).[12] While we focus on these two

---

[10] The geocoding match rate is in line with the results found by Edwards et al. (2014) in their analysis of live births in the state of North Carolina in 2005. A substantial number of the unmatched addresses in our analysis were PO Boxes (28 percent) or began with an alpha character indicative of rural route numbers or combinations of route and box numbers (26 percent) indicating that they would likely be excluded from our main analysis sample of those within 50 miles of a shooting in any case.

[11] Ten of the 13 sniper shootings occurred in the DC metropolitan area encompassing urban neighborhoods in Northern Virginia or Maryland and DC areas within a several miles of Virginia. The remaining three incidents occurred in relatively urban areas alongside the Interstate-95 corridor nearby the cities of Fredericksburg and Richmond.

[12] In the vital statistics data, there are two variables related to gestation length. The first measure is based on the conventional calculation of gestation using the date of the last menstrual period. However, this variable is sometimes missing or may be measured with error. As an alternative, the records include a clinical estimate of gestation, which considers all available information, including ultrasound measurements (Currie et al., 2022). For our analysis, we use this clinical estimate as it provides a more reliable measure of gestation length and is available for all births.

severely adverse birth outcomes in the main analysis, results for birthweight in grams, low birthweight (below 2,500 grams) and preterm (34 or 37 weeks of gestation) are also presented in supplementary analyses. In addition, we construct indices of adverse birth outcomes in order to avoid possible pitfalls due to multiple hypothesis testing.

Finally, we make the following additional restrictions to the main analysis sample: Excluding babies with gestational length less than 23 weeks because not many of these babies survive and these values could be recorded in error; excluding mothers who were younger than age 18 at the time of delivery; and excluding women with a conception date after the last shooting occurred. The reason for this later exclusion is that women who were exposed to the shootings before conception might still be affected by trauma stemming from these incidents. Therefore, women with a conception date after the last shooting might not constitute a clean control group. However, as we demonstrate in the robustness analysis, the estimates remain stable when considering alternative control groups, including one that includes women who were exposed to the beltway sniper attacks after the completion of pregnancy or before conception. We also show that the results are also robust to including women younger than 18 and babies with less than 23 weeks of gestation in the estimation sample. Imposing these criteria generates an analysis sample of 264,446 observations. About 45 percent of these mothers lived within a 10-mile radius of a shooting and constitute the treatment group. The rest lived between 10 to 50 miles of a shooting and form the control group.

Descriptive statistics for the analysis sample as well as for the treatment and control groups separately are shown in Table 2. The average age of a sample mother is 29 with treated mothers being slightly older than the mothers in the control group. Mothers residing within a 10-mile radius of a sniper attack are more likely to be married and Hispanic, and less likely to be White or

Black, as well as being more educated than those with an address outside this bandwidth. With respect to the outcome variables, a little over one percent of infants are very low birthweight and approximately 1.3 percent were very premature within a 10-mile radius of a sniper shooting. In the control group, the comparable figures are higher at 1.4 percent and 1.5 percent within 10- to 50-mile of a shooting.

These means suggest that the treatment group is somewhat better off, at least with respect to expected birth outcomes, which means that any estimated effects of the sniper attacks could be regarded as a lower bound on the true effects. The final column of Table 2 displays the summary statistics for observations located beyond the 50-mile radius of the nearest shooting incident and shows that limiting the control group to people within the 50-mile radius does make the treatments and control groups more comparable. In what follows, we will assess the extent to which the treatment and control groups were evolving along parallel trends prior to the beltway sniper attacks.

The attacks could have prompted some pregnant mothers to give birth in other parts of Virginia, far removed from the areas where the shootings occurred, or possibly even in other states. If pregnant women stayed in Virginia but avoided delivering in hospitals near the shooting locations, then these births would still be captured in the Virginia Natality records. If women chose to leave Virginia to give birth elsewhere, then we would expect to see a decline in Virginian fertility rates in the nine months following the shootings. Another possibility is that extreme stress triggered by exposure to the sniper attacks could have resulted in miscarriages or stillbirths, leading to a decline in the number of live births in the months following the shootings. However, Appendix Figure 1 shows that there are no discernible changes in Virginian fertility patterns following the sniper attacks.

The FBI's Supplementary Homicide Reports (SHR) are the primary source of information about mass shootings nationally. The SHR provides a database of U.S. homicides based on reports from local law enforcement agencies in 49 states and the District of Columbia.[13] However, the UCR Program is voluntary and some law enforcement agencies do not consistently submit SHR forms to the FBI, resulting in potential limitations and inconsistencies in the data (Huff-Corzine et al., 2014). To ensure accuracy, each incident was verified using additional media organizations and government sources.[14] This process not only corrected inconsistencies in the SHR data but also helped to identify and include mass shootings that were not present in the SHR. In total, there were 113 incidents in 96 counties between 2006 and 2019.[15] Figure 2 shows the geographical variation and yearly number of incidents. Variations in the criteria used to define mass shootings lead to inconsistent assessments of both their frequency and lethality. For instance, the Gun Violence Archive incorporates incidents related to gangs and drug activities. By excluding conventional crimes, we focus on random and unpredictable shooting incidents, which we hypothesize are the most likely to induce stress in uninvolved third parties.

National data on infant health at birth were obtained from the restricted access version of the U.S. Vital Statistics Natality records maintained by the National Center for Health Statistics (NCHS) at the Centers for Disease Control and Prevention. These records cover all births registered in the 50 states and the District of Columbia and are based on information obtained from

[13] The SHR contains information on the victim and suspected offender's characteristics, such as age, sex, and race, as well as details about the incident, such as the location, number of victims killed, type of weapon used, relationship between the victim(s) and offender(s), and the circumstances surrounding the incident.
[14] These sources included the USA Today's Mass Killings List, the Washington Post's Mass Shooting List, the Mass Shootings in America Project, the FBI's Active Shooter Incidents in the USA Report, and Gun Violence Archive. These organizations differ in the criteria they use in constructing databases on mass shootings, including the minimum number of victims, the location of the incident, whether the shooting was associated with conventional crimes such as gang violence or robbery, and the relationship between the perpetrator and the victims.
[15] The analysis period starts in 2006 because that is when one of the main sources of information for mass shootings, USA Today, began tracking these incidents. The endpoint was chosen as 2019 in order to avoid major shifts in societal dynamics caused by the COVID-19 pandemic and its aftermath.

both mothers and hospitals. The data coverage is similar to what was described above for the Virginia data except that we do not observe the exact residential address or day of birth. We can observe the mother's county of residency and month of birth allowing us to connect birth records to the mass shooting database, but with more error than in the Virginia data. The variables used in the mass shooting analysis and the process employed to construct the sample align closely with the approach adopted in the analysis of the beltway sniper attacks. Our mass shooting analysis considers all counties that experienced at least one mass shooting between 2006 and 2019. Sample construction and descriptive statistics for the national sample are discussed in Appendix A.

5. Empirical Methods

In order to estimate the causal impact of exposure to the beltway sniper attacks on birth outcomes we adopt a difference-in-differences estimation strategy. The outcomes of infants born to mothers who lived near one of the shooting sites during their pregnancy during October 2002 are compared to the outcomes of infants born to mothers who lived further away, and this difference is compared to the analogous difference for infants born to mothers whose pregnancy did not overlap with October 2002.[16] Specifically, the treatment group includes babies who were in utero between October 2 and 23, 2002, and whose mothers lived within 10 miles of one of the shooting sites.[17] All other infants within 50 miles radius are in the control group. This estimation strategy is formalized in the following equation:

[16] The standard difference–in–differences estimator can produce biased results in the presence of heterogeneous treatment effects over time (e.g., Goodman–Bacon, 2021; de Chaisemartin and d'Haultfoeuille, 2020), but this issue is not a concern in this context because the timing of treatment does not vary over time.

[17] The last shooting incident took place on October 22, 2002, and shooters were arrested at 3:15 a.m. on October 24. We have selected October 23 as the last date of the treatment period because the snipers remained at large until the end of October 23. However, using October 22 or 24 as the endpoint of the treatment period does not alter the estimates.

$$y_{imdyc} = \alpha + \beta_1\, Close_{imdyc}{*}Tri^{1}{}_{imdyc} + \beta_2\, Close_{imdyc}{*}Tri^{2}{}_{imdyc} + \beta_3\, Close_{imdyc}{*}Tri^{3}{}_{imdyc} + \mu\, Close_{imdyc}$$

$$+\ \eta\, Close_{imdyc}{*}Coincide_{imdy} + \phi \boldsymbol{X}_{imdyc} + \lambda_{dmy} + \delta_c + \delta_c t + \varepsilon_{imdyc} \qquad (1)$$

where y_{imdyc} is a birth outcome of child *i* conceived in day *d*, in month *m*, and year *y* in county *c*. $Close_{imdyc}$ is a binary indicator for whether the child's mother lived close to one of the shooting sites during pregnancy. $\text{Tri}^{k}{}_{imdyc}$ (i.e., k = 1, 2, 3) are binary indicators representing the mother's trimester-specific exposure to the sniper attacks during pregnancy. "First trimester" is defined as 0 to 91 days post-conception, "second trimester" is 92-182 days post-conception, and "third trimester" is 183-280 days post-conception.[18] Equation (1) also includes an interaction term between the closeness indicator and a binary variable indicating that the expected delivery date coincided with the shooting period. This specification allows for a separate effect on the infants of women who had an expected delivery date between October 2 and October 23, 2002 since these women might have faced special obstacles getting to hospital, for example.[19] The vector $\boldsymbol{X}$ contains observable characteristics including indicators for the mother's age, marital status, educational attainment, race and ethnicity, child's gender, and indicators for parity and plurality. The vector δ_c represents county fixed effects and λ_{dmy} stands for day-month-year of conception fixed effects. Specifications using weekly or monthly rather than daily fixed effects are also reported below. Finally, the most comprehensive specifications include county-specific linear time trends ($\delta_c t$, at the conception month-year level) to control for common trends and shocks to infant health that

---

[18] The first trimester indicator is assigned a value of one if the conception dates are within the range of July 24, 2002, to October 23, 2002, and zero otherwise. The second trimester indicator is set to one if the conception dates fall between July 23, 2002, and April 23, 2002, and the third trimester indicator takes on a value of one if the conception dates are between April 22, 2002, and January 16, 2002, and zero otherwise.

[19] The estimated coefficients on this term are small and never statistically significant in any of our models. These 1,172 women were exposed to the shootings in the last few days of pregnancy and may have been exposed to between one and thirteen shootings. Including a separate indicator for them ensures that everyone in the sample that identifies the main effect of interest was exposed to all thirteen shootings.

might be correlated with our treatment indicators at the county level. The ε_{imdyc} is a child-specific idiosyncratic error term. Standard errors are clustered at the county level, making statistical inference robust to arbitrary forms of both heteroskedasticity and serial correlation within counties over time.

Reports about the shootings dominated the news cycle during this period with intense coverage from both the local and national media organizations.[20] Therefore, it is safe to assume that all of the women in our sample heard about the incidents and felt some level of anxiety and fear. By choosing as treated women those who lived close to a shooting, we aim to capture the fear caused by the risk of becoming a victim. However, since the definition of "close" is unclear a priori, we experiment with 10, 7, and 5-mile radii and show results from this analysis in the robustness section.

Given these considerations, the benchmark estimation sample can be described as:

S = {i=**1**| Conception date ≤ [October 2-23, 2002] ≤ Expected_Birthdate}$_i$ =1 | **1** [Birthdate < [October 2-23, 2002]$_i$ = 1}, where "Expected_Birthdate" is the expected birthdate of the child, calculated by adding 280 days to the date of conception. The "expected" rather than the "actual" birthdate is used because the stress caused by the shootings might have altered the actual birthdate by influencing gestational length. Using the expected birthdate, helps to guard against bias from the potential endogeneity of the actual birthdate.[21]

In equation (1), the coefficients β_1, β_2, and β_3 are the parameters of interest, representing the trimester-specific impacts of exposure to the sniper attacks in utero on birth outcomes. A causal interpretation of β_1, β_2, and β_3 in equation (1) hinges on the validity of the "parallel trends"

[20] Media coverage was so intense that over 50 articles appeared in the Washington Post alone during the three-week period of the shootings (Muzzatti and Featherstone, 2007). Furthermore, over 70 percent of citizens reported that they had followed the news more than usual during the weeks of the sniper attacks (Coppola, 2004; Mitchell, 2007).
[21] This approach follows that of Black et al. (2016), Persson and Rossin-Slater (2018b), and Currie et al., 2022).

assumption required by the DiD method. That is, although there may be pre-existing differences in birth outcomes between babies born to treated and control mothers as shown in Table 2, there must be no pre-existing differential trends between the two. The plausibility of this assumption can be tested in several ways. First, we perform a balance test to assess correlations between the treatment variables and several measures of maternal and child characteristics. If, for example, the racial or ethnic composition of Virginia was changing over time in a way that was correlated with proximity to the sniper attack sites, then this demographic change could potentially confound the estimates. Appendix Table 1 shows that treatment indicators do not predict the race, ethnicity, education, age, or marital status of mothers. Similarly, there is no correlation between the treatment indicators and the child's gender or the singleton birth indicator.[22]

Finally, the "parallel trends" assumption is directly tested by estimating an event-study specification that allows the treatment to have impacts across various periods of exposure. This test is performed by estimating a dynamic model which is specified as follows:

$$y_{imdyc} = \beta_o + \sum_{\substack{q=-4^{+} \\ q\neq -1}}^{3} \beta_q \, Close_{imdyc} Tri_{imdyc}^{t+q} + \eta \, Close * Coincide_{imdyc} + \mu \, Close_{imdyc} + \phi \boldsymbol{X}_{imdyc} + \lambda_{dmy} + \delta_c + \delta_c \, t + \varepsilon_{imdyc} \qquad (2)$$

In equation (2), the coefficients β_1, β_2, and β_3 are defined as in (1) as the trimester-specific impacts of exposure to the sniper attacks in utero on birth outcomes. Lagged parameters capture the effects of exposure to the sniper attacks during the *post-pregnancy* period. For example, β_{-1} corresponds to the impact of exposure to sniper shootings within the three months after the expected date of delivery. The other lagged parameters denote the impacts of exposure in the subsequent three-

[22] In our analysis, we also include county-specific linear time trends in the most comprehensive specification in order to control for any unobservable county-level compositional changes that are trending linearly over time and could be correlated with birth outcomes.

month periods. Because exposure after the pregnancy is over should have no effect on birth outcomes, this dynamic model allows us to investigate the presence of omitted variables which would violate the identification strategy. As shown below, the results are robust to alternative lag and lead structures.

In contrast to the beltway sniper attacks which occurred at one point in time, the research design for the analysis of the national mass shooting data is based on staggered treatment because the incidents occurred at different times in different counties. When treatment is staggered, standard DID estimates can be biased in the presence of heterogeneous treatment effects (Goodman-Bacon, 2021; de Chaisemartin and d'Haultfœuille, 2020; Sun and Abraham, 2021). To overcome this bias, we use the estimator proposed by Borusyak et al. (2023). A detailed description of the methodology used in the mass shooting analysis is provided in Appendix B.

6. Results

The Beltway Sniper Attacks

Estimates of equation (1) showing the impact of in utero exposure to the beltway sniper attacks on birth outcomes are shown in Table 3. Trimester-specific estimates appear in Panel A and overall estimated effects for any exposure during pregnancy are shown in Panel B. Columns 1 and 3 present estimates with conception day-month-year and county fixed effects as well as mother and child characteristics. Columns 2 and 4 add county-specific linear trends.

The estimates in column 1 suggest that exposure to mass shootings during the first and second trimesters of pregnancy increases the likelihood of very low birthweight by around 25 percent. When county-specific linear trends are included in column 2, the estimates still indicate that exposure to sniper attacks during the first and second trimesters increases the risk of very low birthweight and the effect size grows to approximately 35 to 40 percent of the baseline.

Turning to the estimates for prematurity in column 3, the estimates for the first and second trimesters are positive but are less precisely estimated than those for very low birthweight, perhaps because gestational age is often measured with error. However, in the specification with county-specific trends (column 4), the point estimates on very premature are similar to those on very low birthweight in column 2 and the estimated effect of exposure to the shootings during the second trimester is statistically significant. The point estimate suggests that exposure to the sniper attacks in the second trimester caused the likelihood of very premature birth to increase by about 32 percent. Panel B of Table 3 shows the overall effect of exposure to the shootings at any point in pregnancy. Here the model with county-specific trends suggests that exposure to the shootings during pregnancy increased both the incidence of very low birthweight and of extreme prematurity by about 25 percent.

The results from an event-study analysis in the form of equation (2) are presented in the top panel of Figure 3.[23] In order to maintain consistency with the categorization of pregnancy into trimesters, the pre-sniper attack period is also divided into three-month segments. The figure indicates that for women whose pregnancies ended prior to the first attack, there are no statistically significant differences in either very low birthweight or extreme prematurity between the treatment and control areas. Among women affected by the attacks in the 1st and 2nd trimesters of their pregnancies, the treatment group has a significantly higher probability of very low birthweight.

[23] The point estimates from the event-study analysis are presented in table format in Appendix Table 2.

Among women affected in the second trimester of their pregnancy, there is also an elevated risk of being very premature in the treatment group compared to the control.[24]

As discussed earlier, women with a conception date after the shootings ended might still have been traumatized. As a result, the benchmark control group includes only women who experienced the shootings after they became pregnant. Figure 4 shows event study estimates comparing women in the treatment and control areas who conceived after the shooters were captured: There is no statistically significant difference between the two groups in this period.

Taken together, the results show that exposure to the Beltway sniper during pregnancy increased the incidence of very low birthweight and extreme prematurity. Table 4 presents estimates using a wider spectrum of outcomes in order to gain insights into the broader ramifications of the sniper attacks and to develop a more comprehensive understanding of their effects. These models all include county-specific linear trends, making them most comparable to columns (2) and (4) of Table 3.

The results indicate that exposure to the shootings during pregnancy led to a 26-gram reduction in overall birthweight, representing a 0.7 percent decrease over the baseline. This effect is most prominent for babies born to mothers who experienced these incidents during the second trimester of their pregnancy, with a larger reduction in birthweight of approximately 40 grams. As shown in column 2, the effect on birthweight is more pronounced at the lower end of the distribution, with an approximately 12 percentage point increase in likelihood of giving birth to a

[24] The event-study estimates are robust to exclusion of county-specific linear trends (please see Appendix Figure 3). Specifically, the very low birthweight estimates are all positive and they are significant at the 95 percent level for the first and second trimester indicators. Likewise, the very premature estimates are all positive, and significant at the 90 percent level for the second trimester indicator. The event-study estimates without county-specific trends are, if anything, more precisely estimated than the semi-dynamic specification without the trends shown in Table 3. Additionally, the estimates remain quite stable and consistent when using different event-study specifications with alternative leads and lags. As an illustration, Appendix Figure 2 shows estimates from a model using five lags and three leads.

low birthweight baby (birthweight less than 2500 grams) and these effects appear to be entirely driven by exposures in the first and second trimesters. Column 3 displays estimated effects on gestational length in weeks. While exposure to sniper attacks during pregnancy is associated with a slight yet statistically significant decrease in gestation length overall, the impact is primarily attributable to exposures during the first and second trimesters.

Next, we present estimates on binary indicators of prematurity as defined by gestational cut-offs of 34 weeks (column 4) and 37 weeks (column 5). These estimates indicate that exposure to the sniper shootings during the first and second trimesters increases the likelihood of gestational lengths below 34 weeks by approximately 25 percent. However, as we move away from the tails of the distribution, and use a cut-off of 37 weeks of gestation, the effect dissipates and loses statistical significance. Column 6 examines the risk of having an abnormal condition at birth.[25] The estimate on the any exposure during pregnancy indicator suggest a roughly 8 percent higher risk, but the coefficient is not precisely estimated. Finally, column 7 shows that exposure to sniper attacks during pregnancy does not appear to impact the probability of having a low Apgar score.[26]

The estimates in Table 4 are consistent with those of Table 3 in that they show that exposure to the beltway sniper shootings increased the probability of adverse birth outcomes. However, concerns about multiple hypothesis testing arise when examining a spectrum of inter-related outcomes. To address this concern, the measures are grouped into two indices. The first, a *severe birth outcomes index*, is comprised of the following indicators: very low birthweight, very preterm, any abnormal conditions at birth (e.g., use of assisted ventilation), and low Apgar score. The

[25] These conditions include anemia, fetal alcohol syndrome, hyaline membrane disease, meconium aspirations syndrome, assisted ventilation, seizures, infectious condition, birth injury, and any unknown conditions. The composite variable available to us reflects the existence of any of these conditions, and therefore, we are unable to examine them individually, except for assisted ventilation, which was included as a separate variable.

[26] The Apgar score, reported on a scale of 0 to 10, is determined by a doctor's assessment of the baby's skin color, heart rate, reflexes, muscle tone, and breathing soon after birth. Scores below seven are categorized as low.

second is a *broad birth outcome index*, which adds all the remaining variables in Table 4: continuous birthweight in grams, low birthweight, an indicator for preterm based on a 34 weeks of gestation cutoff, an indicator for preterm based on 37 weeks of gestation cutoff, and gestation in weeks. To create these indices, each outcome variable is oriented so that a higher value represents a more adverse outcome. Then, each outcome is standardized by subtracting the mean of the control group and dividing by the control group's standard deviation before adding the components.

The estimates for the outcome indices are presented in columns 8 and 9 of Table 4. In the case of the *severe birth outcome index*, the estimates show that exposure to the beltway sniper shootings during pregnancy leads to a decline in newborn health as shown in column 8. The estimates are largest among babies born to mothers who experienced the sniper attacks during the first and second trimesters, with effect sizes of approximately 0.03 standard deviations for each trimester. When we consider the broader birth outcome index in column 9, the evidence once again suggests a decline in newborn health among exposed infants. The effect sizes are similar to those obtained for the severe birth outcome index.

We provide further evidence about the validity of these DiD estimates using the indices by performing an event-study analysis, shown in Figure 5, to examine the parallel trends assumption. The event-study estimates suggests that the assumption holds.

National Mass Shootings Analysis

This section shifts the focus to examining the impact of mass shootings on pregnancy outcomes at the national level between 2006 and 2019, exploiting within-county variation in the timing of mass shootings in a difference-in-differences framework. If the precise timing of mass shootings is not systematically related to within county time-varying unobserved factors determining infant health outcomes, then this empirical approach can yield causal estimates of the impact of mass shootings on birth outcomes.

The results of this analysis are presented in a way that aligns with the discussion of the beltway sniper attacks. Trimester-specific estimates are shown in Panel A of Table 5 and the overall effects for any exposure during pregnancy appear in Panel B. Column 1 presents estimates based on a specification that includes county fixed effects and conception quarter-by-year fixed effects, while column 2 also incorporates mother and child characteristics.

The point estimates from this analysis are smaller than those from the analysis of the beltway sniper shootings, as expected. Mass shootings tend to be brief, often lasting only a few minutes or hours, whereas the beltway sniper attacks persisted for a period of three weeks. Another difference lies in the fact that an individual could reside in the same county as a mass shooting but live more than 10 miles away from the location of the actual incident. The fact that we only observe county rather than exact residential location in the national data is likely to attenuate the estimated effects. A third issue is that we can connect shootings to pregnancy intervals only at the monthly level, so our measure of when someone was exposed during their pregnancy is also subject to error.

Estimates in column 2 of Panel A show that experiencing a mass shooting during first trimester of pregnancy is associated with an increased incidence of very low birthweight and extreme prematurity. The point estimates translate into effect sizes of 5.7 percent (0.069/1.219) for very low birthweight and 5.2 percent (0.073/1.394) for extreme prematurity. A causal interpretation of these estimates rests on the assumption that infant outcomes in a particular county were not already trending differently compared to other counties prior to a mass shooting, an assumption that is supported by the event-study evidence in Figure 6.

Table 6 presents the estimated effects of exposure to a mass shooting during pregnancy on the same set of outcomes examined in the sniper attacks analysis. The first two columns show that second-trimester exposure to a mass shooting during pregnancy is associated with a slight reduction in birthweight (3.4 grams) and a small 1.9 percent increase in the incidence of low birthweight. Column 3 indicates that second-trimester exposure to a mass shooting is associated with a decrease in gestation length of 1.3 weeks (approximately 3.4 percent). Columns 4 and 5 display estimates on the impact of prematurity obtained using gestation cut-offs of 34 and 37 weeks, respectively. Results using the 34-week cutoff imply that second-trimester exposure to a mass shooting increases the likelihood of prematurity 2.7 percent. Using the 37-week cutoff produces similar estimates. Furthermore, Column 6 shows that experiencing a mass shooting during the first and second trimesters of pregnancy raises the probability of the newborn having an abnormal condition by about 8 and 7 percent, respectively.[27] Column 7, shows that exposure in the third trimester results in a 6 percent increase in the probability that the newborn has a low Apgar score.[28]

Following the approach in the beltway sniper analysis, two composite indices were created using the outcome categories in Table 6. Estimates using these indices as dependent variables show that exposure to a mass shooting during the first, second, and third trimesters leads to an increase in the severe birth outcomes index of 0.008, 0.005, and 0.004 standard deviations, respectively (column 8). Focusing on the broader index, the results are similar with effect sizes of 0.005, 0.006,

[27] The conditions included in this category are assisted ventilation, admission to a neonatal intensive care unit, surfactant deficiency, use of antibiotics, seizures, and birth injury. There is a slight difference in the types of conditions included in this variable between the Virginia birth records and the U.S. natality data.

[28] The U.S. Vital Statistics Natality records used in our national analysis of mass shootings include a more comprehensive range of variables related to infant and maternal health compared to the Virginia data used in the analysis of sniper attacks. The findings from these additional variables largely align with the evidence emerging from our main analysis. For instance, exposure to a mass shooting during the first trimester is associated with a roughly 5.7 percent increased likelihood of an infant being admitted to a neonatal intensive care unit. Also the results are robust to the inclusion of this condition in the birth outcomes indices.

and 0.003 standard deviations (column 9). Furthermore, the results from the any exposure specifications shown in Panel B reveal that exposure to a mass shooting anytime during pregnancy leads to an increase in the likelihood of severe birth index by 0.006 standard deviation and the likelihood of broader birth index by 0.008 standard deviation. Event study versions of these results shown in Appendix Figure 4 support the assumption of parallel pretrends underlying these DiD estimates.

7. Robustness and Heterogeneity of the Estimates Across Subgroups

Table 7 probes the robustness of the beltway sniper analysis to changes in the way that the sample is constructed. For example, the results are almost identical if we include mothers younger than age 18 or eliminate any gestational age restrictions as shown in columns 1 through 4. It could be argued that some control variables such as marital status of the mother and child gender could be affected by maternal stress (e.g. if boys are more vulnerable to fetal loss). However, excluding these variables (columns 5 and 6) has a minimal impact on the estimates. The results from the event-study versions of these specifications are shown in Appendix Figures 5-7. As demonstrated by the figure, there is no evidence of any statistically significant pre-trends in the difference between the treatment and control units. Finally, the estimates obtained using a sample that includes births conceived after October 23, 2002 are shown in columns 7 and 8. These point estimates are slightly smaller than those shown in Table 3, which could reflect lingering trauma after the shootings.[29]

Table 8 explores heterogeneity in the estimates across demographic subgroups. We begin by presenting estimates for mothers with more than a high school education (columns 1 and 2) and

[29] The event-study estimates from the analysis including births conceived after October 23, 2002 is displayed in Figure 4.

those with a high school degree or less (columns 3 and 4). These estimates show that the effects are largest among relatively more educated mothers. Turning to estimates by race, columns 5 to 6 demonstrate that the estimates for white mothers closely align with the overall estimates in terms of magnitude and statistical significance. The effects among Black mothers (columns 7 and 8) are sizable but imprecisely estimated. Finally, we provide estimates for subgroups categorized by maternal age. These estimates indicate that the overall effects are primarily driven by mothers who are older than 30 years at the time of delivery. In summary, the beltway sniper shootings appear to have had a more significant impact among exposed pregnant women with relatively high socioeconomic status. It is possible that this group experienced greater increases in stress levels over their baseline levels compared to less advantaged women who may experience greater baseline levels of stress in their day to day lives.

Table 9 shows estimates using alternative geographic definitions of *closeness* to the sniper attacks as well different boundaries for the spatial coverage of the analysis area. The first two columns show estimates using a seven-mile radius around each shooting. The next two columns narrow the exposure area to a five-mile radius around a shooting. Using a seven-mile radius to define "close" produces the largest point estimates and suggests that both exposure in the first trimester and exposure in the second trimester increased very low birthweight and extreme prematurity. Using a five-mile radius produces estimates that are generally slightly smaller, but still highly statistically significant for the impact of exposure in the first and second trimesters.

The effect of relaxing the 50-mile restriction is shown in columns 5 to 8 of Table 9. Columns 5-6 show estimates from a sample that includes mothers within a 100-mile radius of any shooting while columns 7 and 8 present estimates using the full sample of Virginia births. Including everyone within 100 miles, or everyone in Virginia, produces smaller estimates of the impact of exposure to the shootings during pregnancy, perhaps because the broader treatment group pulled in by relaxing the 50-mile restriction is less comparable to the treatment group. However, the estimated effect of exposure in the second trimester is still statistically significant.

Next, we assess the sensitivity of our estimates to using an empirical specification with week or month fixed effects rather than daily fixed effects. As shown in Appendix Table 3, the estimates of the effects of exposure to the beltway sniper shootings during pregnancy are remarkably robust to these alternative specifications.

Placebo Analyses

We perform two placebo analyses to further assess the validity of our estimates of the effects of the beltway sniper attacks. In the first analysis, models are estimated assuming that the shootings occurred on the same dates but in a different year. As shown in Appendix Table 4, none of the results obtained for the years 1998, 1999, 2000, and 2001 are statistically significant. In the second exercise, we created 22-day windows partitioning the sample period which resulted in 70 possible "shooting" periods. The model was then estimated treating each of these windows as the period of the attacks.

Appendix Figure 8 plots the distribution of point estimates from these permutations for two outcomes: very low birthweight and very premature. The baseline estimates of the coefficients on each trimester indicator from column 2 and column 4 of Table 3 are shown with a vertical red line. The distribution of placebo estimates that are greater than the baseline estimates are shown in blue. We also perform this placebo analysis for both the specification with a single binary indicator for exposure to the attacks at any time during pregnancy (Appendix Figure 9). In both panels of the figure, the evidence closely aligns with what is reported in Table 3 and suggests that is unlikely that the effects we find were generated by chance. For example, the placebo estimates are greater than the actual estimates from Table 3 only 5.7 percent of the time for very low birthweight and none of the placebo estimates are larger in magnitude pertaining to very premature births for the specification with single binary indicator of exposure.

Robustness of the National Mass Shootings Data

In line with the Beltway sniper analysis, the estimated effects of mass shootings on birth outcomes remain remarkably robust to alternative sample constructions. These alternatives involve the inclusion of mothers younger than age 18, removing any gestational age restrictions, and excluding potentially endogenous characteristics like marital status and the sex of the child. These estimates and event-study figures are shown in Appendix Table 5 and Appendix Figures 10-12.

As in the beltway sniper analysis, Appendix Table 6 shows that the effects obtained in the mass shooting analysis appear to be primarily driven by mothers with more than a high school education, who are white, and who are older than age 30. These estimates reinforce the finding that it is the more socioeconomically advantaged women, who tend to experience lower levels of baseline stress, who bear the greatest burden from a sudden increase in stress, possibly because more disadvantaged women suffer more from stress on a day-to-day basis.

8. Potential Mechanisms

Given these findings, it is important to inquire about the potential mechanisms that may underlie these observed effects. One plausible mechanism is the profound impact of maternal stress during pregnancy. Chronic stress can lead to the release of stress hormones, such as cortisol, which, when elevated for prolonged periods, can negatively impact placental function and fetal growth (Mulder et al., 2002; Hobel and Culhane, 2003; Grote et al., 2010). Moreover, prenatal stress could compromise the mother's immune system, and adversely affect the developing immune system of the offspring (Stott, 1973; Ruiz and Avant, 2005; Weinstock, 2008).

Beyond these pathways, negative behavioral reactions to stress by pregnant women could also detrimentally affect fetal health. These data have information on an array of maternal behaviors, as well as conditions and medical procedures during pregnancy and delivery, which may help illuminate some of the underlying mechanisms driving the estimated effects on infant health. These variables are treated as outcome measures using our most comprehensive specification to assess whether they are affected by the shooting treatment indicators. The results of this analysis are presented in Table 10. The first column shows that exposed mothers had a 7.6 percent increased probability of having a procedure performed.[30] The procedures include amniocentesis, induction of labor, stimulation of labor, ultrasound, or any other unknown procedure, but the estimates in the second column suggest that most of the effect is explained by an increase in inductions. As shown in column 7, we do not see any significant changes in likelihood of C-Section delivery associated with exposure to the sniper attacks.

The third column shows that tobacco use during pregnancy was unrelated to exposure to the shootings. This finding appears to rule out increased smoking due to stress as a likely mechanism. Similarly, there is no significant overall change in the use of prenatal care (column 4). However, we do observe some effects on prenatal care at the extreme ends of the distribution (column 5). Specifically, women who were exposed to sniper attacks during the first trimester are more likely to receive more than nine prenatal care visits which is consistent with the findings of Torche and Villarreal (2014). Similarly, Currie et al. (2022) find that women who experienced an assault early in their pregnancy tend to visit the doctor sooner to monitor the fetus's health. If

[30] In some cases, we know that a procedure was performed but the specific type is labeled as unknown.

mothers take actions to protect their fetus from the harmful impacts of stress, then our results may represent a conservative estimate of the true impact of exposure to the sniper attacks during pregnancy on infant health.

Column 6 considers the effects of exposure to the beltway shootings on the incidence of gestational hypertension. Interestingly, women exposed in the third trimester have a 16 percent higher probability of experiencing gestational hypertension which has been linked to higher levels of stress hormones and inflammation (Landsbergis et al., 1996; Mulder et al., 2002). This finding demonstrates that stress from the shootings has a physiological impact on the body.

The measures considered in Table 10 are also available in the national data. The results from the analogue of Table 10 are shown in Appendix Table 7. These results do not show much association between exposure to a mass shooting and any of these maternal conditions and procedures during delivery. Perhaps this is not surprising considering the brief duration of mass shootings and the large geographic scope of counties, as highlighted earlier. The only exception is that women are again at a higher risk of developing gestational hypertension if they experienced a mass shooting anytime during pregnancy, but the effect appears to be strongest among those exposed in the first trimester. There is also a peculiar negative effect on the any tobacco use during pregnancy among those exposed in the third trimester, though the effect size is only 3 percent.

Taken together, the overall picture is consistent with the hypothesis that the cascade of biological changes triggered by exposure to extreme stress has detrimental effects on infant health.

9. The Costs of Exposure to Firearm Violence During Pregnancy

Estimates of the costs of firearm violence generally rely on willingness-to-pay approaches (Cohen et al., 2004) and jury awards (Miller et al., 1996), and implicitly assume that the consequences of firearm violence are fully understood. These methods rarely consider intangible costs such as the psychological pain and suffering caused by firearm violence, or the possible transmission of these costs to newborn children. Recent studies have highlighted the adverse economic consequences of mass shootings on local economies, leading to significant and persistent declines in individual earnings (Brodeur and Yousaf, 2022) and consumer confidence about the overall economic outlook (Lagerborg et al., 2023) among residents in counties affected by these tragic events. But these studies do not account for the potential toll resulting from the harmful impact of mass shootings on pregnancy outcomes.

To contextualize our estimates of the effects of gun violence on infant health, we conducted a back-of-the-envelope calculation of the additional economic costs resulting from exposure to shootings during pregnancy. We adopt a comprehensive approach that considers the costs associated with the increase in very low birthweight through six different channels: higher infant mortality rates, increased medical expenses at and shortly after birth, costs related to childhood disability, reduced adult income, increased adult disability, and diminished life expectancy.[31] The calculation of these costs are illustrated in Appendix Table 8. Adding the costs from each channel yields a total cost of very low birthweight of $2,977,477 (in 2023 dollars). Multiplying by the estimated effect of exposure to the sniper attacks during pregnancy on the probability of very low birthweight from Table 3 (0.316 percentage point), yields an additional social cost of $940,883 from an exposure to the Beltway sniper shootings during pregnancy. There were 16,506 Virginian women whose pregnancy overlapped with the sniper attacks and who lived within 10 miles of a

[31] Our calculation of the social costs of exposure to extreme firearm violence on birth outcomes follows the approach used in Currie et al. (2022). This comprehensive approach encompasses six channels including (i) costs due to infant death based on Mathews et al. (2015) and Cutler & Meara (2000); (ii) infant medical care cost based on Rogowski (1998); (iii) childhood disability cost based on Hack et al. (2002) and Stabile & Allin (2012); (iv) cost due to reduction in adult income based on American Communities Survey (2017) and Bharadwaj et al. (2018); (v) cost of adult disability based on Hack et al. (2002) and Anderson et al. (2010); and (vi) cost of long-term mortality risk (i.e., reduced life expectancy) based on Bharadwaj et al. (2018) and Lee et al. (2009). For other details, see Appendix F of Currie et al. (2022).

shooting. Multiplying this figure by $940,883 yields a total cost of $15.5 billion dollars from the increase in very low birthweight attributable to this tragic episode of firearm violence. As shown in Appendix Table 8, the bulk of this cost comes from the cost associated with increased likelihood of infant mortality (43 percent) and cost of long-term mortality risk (42 percent).

Additional perspective can be gained by using the results from the national mass shooting analysis. The estimates shown in Table 5 indicate that county-level exposure to a mass shooting during pregnancy results in a 0.028 percentage point increase in the likelihood of very low birthweight. Multiplying this estimate by the total cost of very low birthweight presented in Appendix 8 generates an average social cost of $83,369 per mass shooting during pregnancy for each woman. According to the U.S. Vital Statistics Natality records, 1,168,120 women were exposed to a mass shooting during their pregnancy between 2006 and 2019. Therefore, our estimates imply additional social costs of roughly 7 billion dollars per year, or a total of $97.4 billion dollars over this period. The results are based on 113 mass shootings and suggest that one additional mass shooting incident places an additional cost burden of approximately $862 million dollars on the children of pregnant women residing in the affected counties.

These estimates are likely to understate the true social cost of stress due to gun violence for several reasons. First, mass shootings make up only one percent of gun deaths and many women are affected by stress stemming from other types of gun violence. A possible reason why we see larger effects of exposure to mass shootings on women of relatively high socioeconomic status is that women of lower socioeconomic status may be more likely to live in circumstances in which they are already fearful of gun violence. Second, we do not measure the effects of gun violence on parents' psychological well-being, which is important not only for their own welfare but for their children's. Similarly, we do not measure the effects of trauma on children who were already born at the time of the attacks. Third, some of our estimates, such as those showing increases in the number of prenatal care visits after first trimester exposure, hint at possible compensatory responses on the part of parents that may reduce the harmful effects of exposure to gun violence.

The analysis suggests first that interventions to reduce gun violence would have positive effects on pregnant women and their children. However, even in the absence of such measures, it is possible that greater attention to the effects of gun violence on mother's mental health might help to mitigate the negative effects we identified. Screening pregnant women for depressive symptoms is simple and inexpensive. According to a recent study investigating the cost-effectiveness of various screening tools for identifying depression in early pregnancy in the United Kingdom, the cost of implementing the screening is $5.61 per pregnant woman using the Whooley two-item screener (Heslin et al., 2022). Since post-partum depression is also thought to have negative effects on infants, screening using an instrument like the 10-item Edinburgh Postnatal Depression Scale could also be helpful (Cox et al., 1987; Hewitt et al., 2009).[32]

Recently, the U.S. Preventive Services Task Force, an independent panel comprising national experts in disease prevention and evidence-based medicine, issued a recommendation that clinicians screen all adults between the ages of 19 and 64 for anxiety disorders, as well as screening all adults, including those aged 65 and older, for major depressive disorder (O'Connor et al., 2023).[33] The statement emphasized that these recommendations also extend to pregnant and

[32] The Whooley questions are a set of two questions developed as a screening tool to assess for depression (Whooley et al., 1997; Arroll et al., 2005). The two questions are: (1) During the past month, have you often been bothered by feeling down, depressed or hopeless?; (2) During the last month, have you often been bothered by having little interest or pleasure in doing things? If a woman answers 'yes' to either question, she can be referred for further mental health treatment. According to Heslin et al. (2022) the cost of the slightly more involved Edinburgh Postnatal Depression Scale is $11.63 per person.

[33] The Task Force assigned a "B" grade to the recommendation, implying that there was high certainty that the net benefit was moderate or there was moderate certainty that the net benefit was moderate to substantial.

postpartum adults. If these recommendations were widely followed, they might help to mitigate the toll of gun violence.

10. Conclusions

This study provides new evidence about the impact of exposure to severe and indiscriminate firearm violence during pregnancy on birth outcomes. We begin with an analysis of the effects of the beltway sniper attacks, a series of random shootings that unfolded in October 2002, using data from Virginia birth certificates between 1998 and 2004. This analysis is complemented by a nationwide exploration of the impact of mass shootings in the United States on birth outcomes, using county-level restricted access birth records from 2006 to 2019.

The findings indicate that exposure to a beltway sniper attack during pregnancy amplified the likelihood of very low birthweight and extreme prematurity by 25 percent. The impact on birthweight is most significant for those exposed during the initial and second trimesters, with effect sizes of 40 and 35 percent, respectively. Regarding extreme prematurity, once again, the effects are most pronounced among those exposed in the first six months of pregnancy, particularly in the second trimester, with an effect size of approximately of 30 percent. The analysis of nationwide data from mass shootings underscores the finding that severe, random firearm violence imposes considerable burdens on individuals indirectly influenced by these incidents, particularly infants affected in utero. The estimated effects of mass shootings in the national analysis are qualitatively similar though smaller. This aligns with the shorter duration of mass shootings compared to the three-week span of the sniper attacks and the larger geographic scope of counties as well as the fact that we are less able to precisely match mothers to shootings (in terms of location and timing) in the national data.

Evaluating the social cost of crime, especially violent crime involving firearms, is of crucial significance for shaping policy discussions. The urgency of reducing the violence has been intensified by the rising prevalence of mass shootings, in the United States. Existing methods for calculating the costs often assume that all victimization costs have been adequately captured. However, existing estimates do not include costs due to effects on children in utero. Our findings indicate that the beltway sniper attacks resulted in social costs of $15.5 billion that have not been factored into previous assessments. Estimates using the national data on mass shootings suggest that they impose $7 billion in societal costs annually due to their impacts on pregnant women and their infants. The results imply that implementing interventions to alleviate the stress that is generated by these events among pregnant women could have substantial advantages, benefiting not only the women themselves but also future generations and society as a whole.

References

Agnich, L. E. (2015). A comparative analysis of attempted and completed school-based mass murder attacks. *American Journal of Criminal Justice*, *40*, 1-22.

Aizer, A., Stroud, L., and Buka, S. (2016). Maternal stress and child outcomes: Evidence from siblings. *Journal of Human Resources*, *51*(3), 523-555.

Almond D, Edlund L, Li H, Zhang J. (2010). Long-term effects of the 1959–1961 China Famine: Mainland China and HongKong. InThe Economic Consequences of Demographic Change in East Asia, NBER-EASE, Ito T, Rose A (eds.), Vol.19 University of Chicago Press: Chicago; 321–350

Almond, D., & Mazumder, B. (2011). Health capital and the prenatal environment: the effect of Ramadan observance during pregnancy. *American Economic Journal: Applied Economics*, *3*(4), 56-85.

Anderson, W. L., Armour, B. S., Finkelstein, E. A., & Wiener, J. M. (2010). Estimates of state-level health-care expenditures associated with disability. *Public Health Reports*, *125*(1), 44-51.

Anderson, D. M., Sabia, J. J., and Tekin, E. (2021). Child access prevention laws and juvenile firearm-related homicides. *Journal of urban economics*, *126*, 103387.

Arroll, B., Smith, F. G., Kerse, N., Fishman, T., and Gunn, J. (2005). Effect of the addition of a “help” question to two screening questions on specificity for diagnosis of depression in general practice: diagnostic validity study. *Bmj*, *331*(7521), 884.

Bharadwaj, P., Lundborg, P., & Rooth, D. O. (2018). Birth weight in the long run. *Journal of Human Resources*, *53*(1), 189-231.

Bharadwaj, P., Bhuller, M., Løken, K. V., & Wentzel, M. (2021). Surviving a mass shooting. *Journal of Public Economics*, *201*, 104469.

Black, S. E., Devereux, P. J., & Salvanes, K. G. (2007). From the cradle to the labor market? The effect of birth weight on adult outcomes. *The Quarterly Journal of Economics*, *122*(1), 409-439.

Black, S. E., Devereux, P. J., and Salvanes, K. G. (2016). Does grief transfer across generations? bereavements during pregnancy and child outcomes. American Economic Journal: Applied Economics, 8(1), 193–223.

Black, S. E., Bütikofer, A., Devereux, P. J., & Salvanes, K. G. (2019). This is only a test? Long-run and intergenerational impacts of prenatal exposure to radioactive fallout. *Review of Economics and Statistics*, *101*(3), 531-546.

Borusyak, K., Jaravel, X., & Spiess, J. (2023). Revisiting event study designs: Robust and efficient estimation. *Review of Economic Studies*.

Brodeur, A., and Yousaf, H. (2022). On the economic consequences of mass shootings. *The Review of Economics and Statistics*, 1-43.

Brown, R. (2014). The intergenerational impact of terror: Does the 9/11 tragedy reverberate into the outcomes of the next generation?. *Households in Conflict Network. https://econpapers. repec. org/paper/hicwpaper/165. htm*.

Bruckner, T. A., Modin, B., & Vågerö, D. (2014). Cold ambient temperature in utero and birth outcomes in Uppsala, Sweden, 1915–1929. *Annals of epidemiology*, *24*(2), 116-121.

Camacho, A. (2008). Stress and birth weight: evidence from terrorist attacks. *American Economic Review*, *98*(2), 511-515.

Chay, K. Y., and Greenstone, M. (2003). The impact of air pollution on infant mortality: evidence from geographic variation in pollution shocks induced by a recession. *The Quarterly Journal of Economics*, *118*(3), 1121-1167.

Chay, K. Y., and Greenstone, M. (2005). Does air quality matter? Evidence from the housing market. *Journal of political Economy*, *113*(2), 376-424.

Cohen, M. A., Rust, R. T., Steen, S., & Tidd, S. T. (2004). Willingness-to-pay for crime control programs. *Criminology*, *42*(1), 89-110.

Cook, P. J., and Ludwig, J. (2000). *Gun violence: The real costs*. Oxford University Press on Demand.

Coppola, D. P. (2005). "Gripped by fear" Public risk (mis) perception and the Washington, DC sniper. *Disaster Prevention and Management: An International Journal, 14(1), 32-54.*

Cox, J. L., Holden, J. M., and Sagovsky, R. (1987). Detection of postnatal depression: development of the 10-item Edinburgh Postnatal Depression Scale. *The British journal of psychiatry*, *150*(6), 782-786.

Currie, J., & Moretti, E. (2007). Biology as destiny? Short-and long-run determinants of intergenerational transmission of birth weight. *Journal of Labor economics*, *25*(2), 231-264.

Currie, J., Stabile, M., Manivong, P., & Roos, L. L. (2010). Child health and young adult outcomes. *Journal of Human resources*, *45*(3), 517-548.

Currie, J., & Walker, R. (2011). Traffic congestion and infant health: Evidence from E-ZPass. *American Economic Journal: Applied Economics*, *3*(1), 65-90.

Currie, J. and Rossin-Slater, M. (2013). Weathering the storm: Hurricanes and birth outcomes. *Journal of Health Economics*, 32(3), 487–503.

Currie, J. and Schwandt, H. (2016). The 9/11 dust cloud and pregnancy outcomes: A reconsideration. Journal of Human Resources, 51(4), 805–831.

Currie, J., Mueller-Smith, M., and Rossin-Slater, M. (2022). Violence while in utero: The impact of assaults during pregnancy on birth outcomes. *Review of Economics and Statistics*, *104*(3), 525-540.

Cutler, D. M., & Meara, E. (2000). The technology of birth: Is it worth it?. In *Forum for Health Economics & Policy* (Vol. 3, No. 1). De Gruyter.

de Chaisemartin, Clement, and Xavier d'Haultfoeuille (2020). Two way fixed effects estimators with heterogeneous treatment effects. American Economic Review, 110(9): 2964–2996.

Dobkin, C., Finkelstein, A., Kluender, R., & Notowidigdo, M. J. (2018). The economic consequences of hospital admissions. *American Economic Review*, *108*(2), 308-352.

Duggan, M. (2001). More guns, more crime. *Journal of political Economy*, *109*(5), 1086-1114.

Edwards, S. E., Strauss, B., & Miranda, M. L. (2014). Geocoding Large Population-level Administrative Datasets at Highly Resolved Spatial Scales. *Transactions in GIS*, *18*(4), 586-603.

Edwards, G., Nesson, E., Robinson, J. J., and Vars, F. (2018). Looking down the barrel of a loaded gun: The effect of mandatory handgun purchase delays on homicide and suicide. *The Economic Journal*, *128*(616), 3117-3140.

Federal Bureau of Investigation (2008). Serial murder: Multi-disciplinary perspectives for investigators. http://www.fbi.gov/stats-services/publications/serial-murder/serial-murder-july-2008-pdf.

Federal Bureau of Investigation (2021). Active Shooter Incidents in the United States in 2021. Department of Justice, Washington, D.C.,

Figlio, D., Guryan, J., Karbownik, K., & Roth, J. (2014). The effects of poor neonatal health on children's cognitive development. *American Economic Review*, *104*(12), 3921-3955.

Friedson, M., and Sharkey, P. (2015). Violence and neighborhood disadvantage after the crime decline. *The Annals of the American Academy of Political and Social Science*, *660*(1), 341-358.

Galea, S., Ahern, J., Resnick, H., Kilpatrick, D., Bucuvalas, M., Gold, J., and Vlahov, D. (2002). Psychological sequelae of the September 11 terrorist attacks in New York City. *New England journal of medicine*, *346*(13), 982-987.

Glover, V., O'connor, T., and O'Donnell, K. (2010). Prenatal stress and the programming of the hpa axis. Neuroscience and Biobehavioral Reviews, 35(1), 17–22.

Glynn, L. M., Wadhwa, P. D., Dunkel-Schetter, C., Chicz-DeMet, A., & Sandman, C. A. (2001). When stress happens matters: effects of earthquake timing on stress responsivity in pregnancy. *American journal of obstetrics and gynecology*, *184*(4), 637-642.

Goin, D. E., Rudolph, K. E., Gomez, A. M., and Ahern, J. (2020). Mediation of firearm violence and preterm birth by pregnancy complications and health behaviors: Addressing structural and postexposure confounding. *American journal of epidemiology*, *189*(8), 820-831.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, *225*(2), 254-277.

Grote, N. K., Bridge, J. A., Gavin, A. R., Melville, J. L., Iyengar, S., and Katon, W. J. (2010). A meta-analysis of depression during pregnancy and the risk of preterm birth, low birth weight, and intrauterine growth restriction. *Archives of general psychiatry*, *67*(10), 1012-1024.

Hack, M., Flannery, D. J., Schluchter, M., Cartar, L., Borawski, E., & Klein, N. (2002). Outcomes in young adulthood for very-low-birth-weight infants. *New England Journal of Medicine*, *346*(3), 149-157.

Heslin, M., Jin, H., Trevillion, K., Ling, X., Nath, S., Barrett, B., ... and Byford, S. (2022). Cost-effectiveness of screening tools for identifying depression in early pregnancy: a decision tree model. *BMC health services research*, *22*(1), 1-17.

Hewitt, C. E., and Gilbody, S. M. (2009). Is it clinically and cost effective to screen for postnatal depression: a systematic review of controlled clinical trials and economic evidence. *BJOG: An International Journal of Obstetrics and Gynecology*, *116*(8), 1019-1027.

Hobel, C., & Culhane, J. (2003). Role of psychosocial and nutritional stress on poor pregnancy outcome. *The Journal of nutrition*, *133*(5), 1709S-1717S.

Huff-Corzine, L., McCutcheon, J. C., Corzine, J., Jarvis, J. P., Tetzlaff-Bemiller, M. J., Weller, M., and Landon, M. (2014). Shooting for accuracy: Comparing data sources on mass murder. Homicide Studies, 18(1), 105–124.

Jetter, M., & Walker, J. (2018). The effect of media coverage on mass shootings.

Kaufman, E. J., Wiebe, D. J., Xiong, R. A., Morrison, C. N., Seamon, M. J., and Delgado, M. K. (2021). Epidemiologic trends in fatal and nonfatal firearm injuries in the US, 2009-2017. *JAMA internal medicine*, *181*(2), 237-244.

Kegler, S. R., Simon, T. R., Zwald, M. L., Chen, M. S., Mercy, J. A., Jones, C. M., ... and Dills, J. (2022). Vital Signs: Changes in Firearm Homicide and Suicide Rates—United States, 2019–2020. *Morbidity and Mortality Weekly Report*, *71*(19), 656.

Krouse, W. J., & Richardson, D. J. (2015). Mass murder with firearms: Incidents and victims, 1999-2013.

Lagerborg, A., Pappa, E., and Ravn, M. O. (2023). Sentimental business cycles. *The Review of Economic Studies*, *90*(3), 1358-1393.

Landsbergis, P. A., and Hatch, M. C. (1996). Psychosocial work stress and pregnancy-induced hypertension. *Epidemiology*, *7*(4), 346-351.

Lauderdale, D. S. (2006). Birth outcomes for Arabic-named women in California before and after September 11. *Demography*, *43*, 185-201.

Lee, C. P., Chertow, G. M., & Zenios, S. A. (2009). An empiric estimate of the value of life: updating the renal dialysis cost-effectiveness standard. *Value in Health*, *12*(1), 80-87.

Lin, P. I., Fei, L., Barzman, D., & Hossain, M. (2018). What have we learned from the time trend of mass shootings in the US? *PloS one*, *13*(10), e0204722.

Lowe, S. R., & Galea, S. (2015). Posttraumatic stress in the aftermath of mass shootings. *Traumatic stress and long-term recovery: Coping with disasters and other negative life events*, 91-111.

Lowe, S. R., & Galea, S. (2017). The mental health consequences of mass shootings. *Trauma, Violence, & Abuse*, *18*(1), 62-82

Luca, M., Malhotra, D., and Poliquin, C. (2017). Handgun waiting periods reduce gun deaths. *Proceedings of the National Academy of Sciences*, *114*(46), 12162-12165.

Luca, M., Malhotra, D., & Poliquin, C. (2020). The impact of mass shootings on gun policy. *Journal of public economics*, *181*, 104083.

Mathews, T. J., MacDorman, M. F., & Thoma, M. E. (2015). Infant mortality statistics from the 2013 period linked birth/infant death data set.

Matsumoto, B. (2018). Family ruptures, stress, and the mental health of the next generation: Comment. *American economic review*, *108*(4-5), 1253-1255.

McClellan, C., & Tekin, E. (2017). Stand your ground laws, homicides, and injuries. *Journal of human resources*, *52*(3), 621-653.

Miller, T. R., Cohen, M. A., & Wiersema, B. (1996). Victim costs and consequences: a new look: US Department of Justice. *Office of Justice Programs, National Institute of Justice*

Miller, D. L. (2023). An Introductory Guide to Event Study Models. *Journal of Economic Perspectives*, *37*(2), 203-230.

Mitchell, A. (2007). Social impacts of fear: An examination of the 2002 Washington, DC sniper shootings. *Colorado State University, Boulder, CO*.

Mulder, E. J., De Medina, P. R., Huizink, A. C., Van den Bergh, B. R., Buitelaar, J. K., and Visser, G. H. (2002). Prenatal maternal stress: effects on pregnancy and the (unborn) child. *Early human development*, *70*(1-2), 3-14.

Muzzatti, S. L., & Featherstone, R. (2007). Crosshairs on our backs: The culture of fear and the production of the DC sniper story. *Contemporary Justice Review*, *10*(1), 43-66.

North, C. S., Nixon, S. J., Shariat, S., Mallonee, S., McMillen, J. C., Spitznagel, E. L., and Smith, E. M. (1999). Psychiatric disorders among survivors of the Oklahoma City bombing. *Jama*, *282*(8), 755-762.

O'Connor, E., Henninger, M., Perdue, L. A., Coppola, E. L., Thomas, R., & Gaynes, B. N. (2023). Screening for depression, anxiety, and suicide risk in adults: A systematic evidence review for the US preventive services task force.

Oreopoulos, P., Stabile, M., Walld, R., & Roos, L. L. (2008). Short-, medium-, and long-term consequences of poor infant health: An analysis using siblings and twins. *Journal of human Resources*, *43*(1), 88-138.

Persson, P. and Rossin-Slater, M. (2018a). Family ruptures, stress, and the mental health of the next generation. American Economic Review, 108(4-5), 1214–52.

Persson, P. and Rossin-Slater, M. (2018b). Family ruptures, stress, and the mental health of the next generation: Reply. American Economic Review, 108(4-5), 1256–63.

Roeder, Oliver. 2016. "The Phrase 'Mass Shooting' Belongs to the 21st Century." *FiveThirtyEight,* January 21. Available at: http://fivethirtyeight.com/features/we-didnt-call-them-mass-shootings-until-the-21st-century.

Rogowski, J. (1998). Cost-effectiveness of care for very low birth weight infants. *Pediatrics*, *102*(1), 35-43.

Rossin-Slater, M., Schnell, M., Schwandt, H., Trejo, S., & Uniat, L. (2020). Local exposure to school shootings and youth antidepressant use. *Proceedings of the National Academy of Sciences*, *117*(38), 23484-23489.

Ruiz, R. J., and Avant, K. C. (2005). Effects of maternal prenatal stress on infant outcomes: a synthesis of the literature. *Advances in Nursing Science*, *28*(4), 345-355.

Sampson, R. J., Raudenbush, S. W., & Earls, F. (1997). Neighborhoods and violent crime: A multilevel study of collective efficacy. *science*, *277*(5328), 918-924.

Sampson, Robert. 2011. "Neighborhood Effects, Causal Mechanisms, and the Social Structure of the City." Pp. 227–50 in Analytical Sociology and Social Mechanisms, edited by P. Demeulenaere. Cambridge, UK: Cambridge University Press.

Scholte, R. S., Van Den Berg, G. J., & Lindeboom, M. (2015). Long-run effects of gestation during the Dutch Hunger Winter famine on labor market and hospitalization outcomes. *Journal of health economics*, *39*, 17-30.

Schulden, J., Chen, J., Kresnow, M. J., Arias, I., Crosby, A., Mercy, J., ... & Blythe, D. (2006). Psychological responses to the sniper attacks: Washington DC area, October 2002. *American journal of preventive medicine*, *31*(4), 324-327.

Shultz, J. M., Thoresen, S., Flynn, B. W., Muschert, G. W., Shaw, J. A., Espinel, Z., ... and Cohen, A. M. (2014). Multiple vantage points on the mental health effects of mass shootings. *Current psychiatry reports*, *16*, 1-17.

Soni, A., and Tekin, E. (in press). How do mass shootings affect community wellbeing? *The Journal of Human Resources*.

Stabile, M., & Allin, S. (2012). The economic costs of childhood disability. *The future of children*, 65-96.

Stott, D. (1973). Follow-up study from birth of the effects of prenatal stresses. *Developmental Medicine and Child Neurology*, 15(6), 770–787.

Sun, L., & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, *225*(2), 175-199.

Thoresen, S., Flood Aakvaag, H., Wentzel-Larsen, T., Dyb, G., and Kristian Hjemdal, O. (2012). The day Norway cried: Proximity and distress in Norwegian citizens following the 22nd July 2011 terrorist attacks in Oslo and on Utøya Island. *European journal of psychotraumatology*, *3*(1), 19709.

Torche, F. (2011). The effect of maternal stress on birth outcomes: exploiting a natural experiment. *Demography*, *48*(4), 1473-1491.

Torche, F., and Villarreal, A. (2014). Prenatal exposure to violence and birth weight in Mexico: Selectivity, exposure, and behavioral responses. *American sociological review*, *79*(5), 966-992.

Webster, D.W. (2017). The True Effect of Mass Shootings on Americans. Annals of Internal Medicine, 166(10), 749-750.

Weinstock, M. (2008). The long-term behavioural consequences of prenatal stress. Neuroscience and Biobehavioral Reviews, 32(6), 1073–1086.

Whooley, M. A., Avins, A. L., Miranda, J., and Browner, W. S. (1997). Case-finding instruments for depression: Two questions are as good as many. *Journal of general internal medicine*, *12*(7), 439-445.

Wolfers, J. (2006). Did unilateral divorce laws raise divorce rates? A reconciliation and new results. *American Economic Review*, *96*(5), 1802-1820.

Yousaf, H. (2021). Sticking to one's guns: Mass shootings and the political economy of gun control in the United States. *Journal of the European Economic Association*, *19*(5), 2765-2802.

Zivotofsky, A. Z., & Koslowsky, M. (2005). Gender differences in coping with the major external stress of the Washington, DC sniper. *Stress and Health: Journal of the International Society for the Investigation of Stress*, *21*(1), 27-31.

APPENDIX A

Data and Sample Construction for the Mass Shooting Analysis

In alignment with our state-level analysis from Virginia, our primary birth outcome measures are indicators of *Very Low Birthweight* and *Very Premature*. These are defined as having a birthweight of less than 1,500 grams and a gestational length of less than 32 weeks. Additional variables that are considered include *Low Birthweight* (birthweight< 2500 grams), and *Premature* (gestational length< 34 and 37 weeks). Beyond birthweight and gestational length, the extensive information present in the Vital Statistics Natality records is used to create several additional measures of infant health. First, we create a binary indicator for any conditions at birth if the newborn had one of these conditions: assisted ventilation required immediately following delivery or for more than 6 hours, admission to NICU, newborn given surfactant replacement therapy, antibiotics received by the newborn immediately following delivery, seizure or serious neurological dysfunction, and birth injury. Second, a binary indicator is created to capture whether the newborn was admitted to the intensive care unit. Third, low 5-minute Apgar score indicator is generated when the 5-minute Apgar score registers below 7. Finally, measures of the number of prenatal visits and an indicator for C-Section delivery are constructed.

To calculate a mother's exposure to mass shootings during pregnancy, estimated conception month are generated by subtracting the gestational length from the month and year (Black et al., 2016; Persson and Rossin-Slater, 2018; Currie et al., 2022). Consistent with the sniper analysis, the analysis sample in the national data includes mothers who are between the ages 18 and 49 at the time of delivery. The estimation sample is also restricted to births with gestation of at least 23 weeks (Persson and Rosin-Slater, 2018).[34] The benchmark analysis focuses on counties

[34] Our results are robust to including women under the age of 18 and infants under 23 weeks of gestation in our estimation sample. Please see Appendix Table 5 and Appendix Figures 10 and 11 for details.

that experienced at least one mass shooting between 2006 and 2019. Imposing these restrictions yields a sample of approximately 3 million observations. However, as a robustness check the analysis is also performed using all counties in the United States including those where no mass shooting has occurred.

Appendix Table 9 presents the summary statistics for birth outcomes and selected control variables by mothers' exposure to mass shootings during pregnancy in counties that experienced at least one mass shooting. The mothers who are in the treatment group are slightly older, more likely to have lower education levels (i.e., less than high school degree at the time of delivery), and less likely to be black compared to mothers who are in the control group. The fraction of births that are in the Very Low Birthweight category is around one percent in treatment and control groups, respectively. The prevalence of Very Premature births follows a similar pattern. More specifically, the sample mean for Very Premature births is around 1.5 percent in both samples.

One consideration is whether the occurrence of mass shootings affects the composition of infant health outcomes, which could potentially introduce bias in our results. For instance, if the trauma caused by a mass shooting increases the likelihood of stillbirth, this could lead to a selection into the analysis sample. Some women may also decide to deliver their baby in a different county or go out of state in response to a mass shooting. Note that the case of moving to another county should not matter for our analysis as these babies would still be considered born in the same state. To investigate these issues, we estimate our model using the total number of births at the county level as the outcome variable. The estimates from this analysis are economically and statistically insignificant, suggesting that the occurrence of mass shootings did not have a significant effect on the number of births in the affected counties.[35] In addition, we visually inspect the per capita

[35] The estimates and standard errors are 35 and 66 for the specification with binary indicator of exposure. For the trimester-specific specification, they are 13 and 81 for the first trimester, 25 and 70 for the second trimester, and 60

quarterly county averages of fertility rates for up to seven quarters preceding a mass shooting and three quarters following the shooting. The results are presented in Appendix Figure 13, and they do not provide clear visual evidence of a significant shift in the county averages of fertility rates around the time of a mass shooting. Lastly, we investigate the possibility of changing locations in response to a mass shooting by making use of the indicators in the natality records that signify whether the state and county of occurrence of birth and residence are different. A virtual inspection of the proportion of these cases over time reveal no evidence of a shift in delivery location around the time of occurrence of a mass shooting. We also estimated our most comprehensive model using these indicators as outcomes. Again, this analysis resulted in no evidence to suggest that mass shootings induce pregnant women to move other states or counties.[36]

APPENDIX B

Empirical Method for the Mass Shooting Analysis

To obtain the causal estimates of the effect of exposure to mass shootings on infant health outcomes, we exploit the within-county variation in the timing of the mass shootings using a difference-in-differences framework. Unlike the Virginia analysis, nationwide Vital Statistics data do not contain exact date of birth. Instead, we possess information on the month and year of each birth. To calculate a mother's exposure to mass shootings during pregnancy, we first generate estimated conception quarter-year by subtracting the physician estimated gestational length from

and 64 for the third trimester. Aside from being statistically insignificant, these estimates are practically zero given the baseline number of births of 3,403 per quarter.

[36] The estimates on all three trimester indicators as well as the binary any exposure indicator are zero and statistically insignificant in this regression.

the actual quarter-year of delivery (Black et al., 2016; Persson and Rossin-Slater, 2018; Currie et al., 2022).[37]

Our empirical model can be specified as follows:

$$y_{ict} = \beta_o + \beta_1 Tri_{ct}^1 + \beta_2 Tri_{ct}^2 + \beta_3 Tri_{ct}^3 + X_{ict}^i \gamma_1 + \lambda_c + \nu_t + \varepsilon_{ict}, \qquad (3)$$

where y_{ict} is infant i's birth outcome (i.e., very low birthweight or very premature), whose mother's county of residence is c, and conceived in quarter-year t. Tri_{ct}^k (k=1, 2, 3) are the key treatment variables representing mother's trimester-specific exposure to a mass shooting. More specifically, Tri_{ct}^k are indicator variables, which take on the value of one for mothers who experienced a mass shooting in their county of residence c within the corresponding trimester k.[38] Consistent with the Sniper shooting analysis, X_{ict}^i is a vector of observable characteristics including indicators for the mother's marital status, parity and plurality categories, mother's age (<20, 20-24, 25-34, 35+, and missing), maternal education (less than high school degree, high school degree, some college degree, bachelor's degree or more, missing); male infant, mother's race/ethnicity categories; and birth order categories. λ_c and ν_t denote mother's county of residence fixed effects and conception quarter-year fixed effects. Finally, ε_{ict} is an idiosyncratic error term.

The set up above compares women who experienced mass shootings during pregnancy and those who experienced the mass shootings *before*, or *after* pregnancy. However, the women who experience mass shootings in their county of residence *before* conception might still be traumatized. Thus, we constitute my benchmark control group only with women who experience mass shootings *after* pregnancy (Persson and Rossin-Slater, 2018b; Matsumoto, 2018; Currie et al., 2022). This is consistent with our approach for the Virginia analysis. Nevertheless, our results

---

[38] We define first trimester as, 0 to 2 months post-conception, second trimester as 3 to 5 months post-conception, and third trimester as 6 to 8 months post-conception.

are robust to employing alternative control groups such as forming control groups with women who experienced mass shootings *before* or *after* pregnancy (as stated above), including women who give birth in counties never experienced mass shootings in the estimation period.

To maintain consistency with our Virginia analysis, we have defined the control and treatment groups based on the expected date of birth instead of the actual date of birth. This approach is taken to avoid violating the excludability requirement given the possibility that "realized gestational length" could be influenced by the treatment.

The validity of our research design relies on the assumption that the timing of mass shootings is not related to within-county unobserved factors that may potentially affect infant health outcomes. Any systematic pre-incident cross-county differences that influence incident timing and infant health outcomes simultaneously may bias estimates. Consistent with the beltway sniper analysis, our treatment indicators do not predict the race, ethnicity, education, age, or marital status of the mothers (Appendix Table 10). Similarly, there is no correlation between the treatment indicators and the child's gender or the singleton birth indicator. However, we also include county-specific linear time trends in our most comprehensive specifications in order to control for any unobservable county-level compositional changes that are trending linearly over time and could be correlated with birth outcomes.

To further assess the validity of this assumption, we estimate an event-study version of equation (3) expressed as follows:

$$y_{ict} = \phi_o + \sum_{\substack{q=-5 \\ q\neq-5}}^{3} \kappa_q Tri^i_{c,t+q} + X^i_{ict}\,\phi_2 + \lambda_c + \nu_t + \varepsilon_{ict}. \quad (4)$$

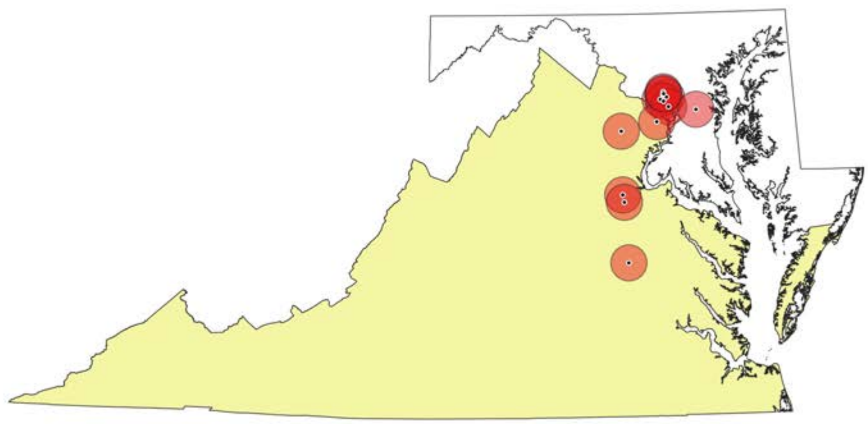
The set of coefficients $\{\kappa_q Tri^i_{c,t+q}\}$ for $q > 0$ are the variables of interest, estimating the trimester-specific exposure to mass shootings on infant health outcomes. The lags represent separate indicators for exposure to mass shootings in/during post-pregnancy periods (q = -5 is the reference category).

Note that the specifications in equations (3) and (4) do not include county-specific linear trends to avoid potentially distorting dynamic treatment effects as highlighted in Borusyak et al. (2023) and Miller (2023). Furthermore, these models are estimated with the sample of counties that experienced a mass shooting during our analysis period. As emphasized by Wolfers (2006), unit specific trends are justifiable when there is a sufficient sample period available before the treatment starts. In our case, we only include five pre-treatment periods for each mass shooting in the national analysis (i.e., τ=-5 is the reference category).

The research design for the mass shooting analysis employs staggered treatment, as mass shooting incidents occurred at various times across different counties. However, the two-way fixed effects models with staggered treatment can exhibit several forms of biases (i.e., under-identification and negative weighting) (Sun and Abraham, 2021; Borusyak et al., 2023). To address these concerns, we adopt the imputation estimator developed by Borusyak et al. (2023) in our analysis of mass shootings.

Appendix Table 11 presents a comparison of means for all child and mother characteristics based on treatment status (i.e., counties ever experienced mass shootings or "ever treated counties" vs. counties never experienced mass shootings during the estimation period or "never treated counties"). The final column of Appendix Table 11 provides the disparity in means and reports t-test outcomes for "ever treated counties" versus "never treated counties." As indicated in column 3, all observable characteristics, including the outcome variables, are systematically different in ever treated counties compared to never treated counties. Furthermore, ever treated counties might experience different trends for infant health outcomes compared to the counties that have never witnessed mass shootings. To reduce such confounding factors, mitigate any potential biases, and ensure the validity and reliability of our results we adopt the ever-treated counties as the primary sample for our main analysis (Dobkin et al., 2018; Sun and Abrahams, 2021). However, as shown in Appendix Table 12, our main results are robust to conducting the DiD analysis using both ever and never treated counties.

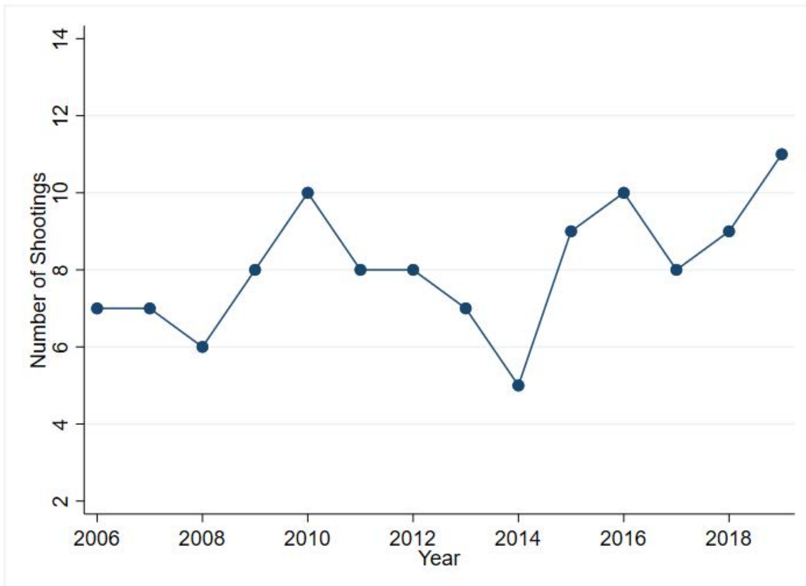
Figure 1: Geographic Distribution of Sniper Shootings



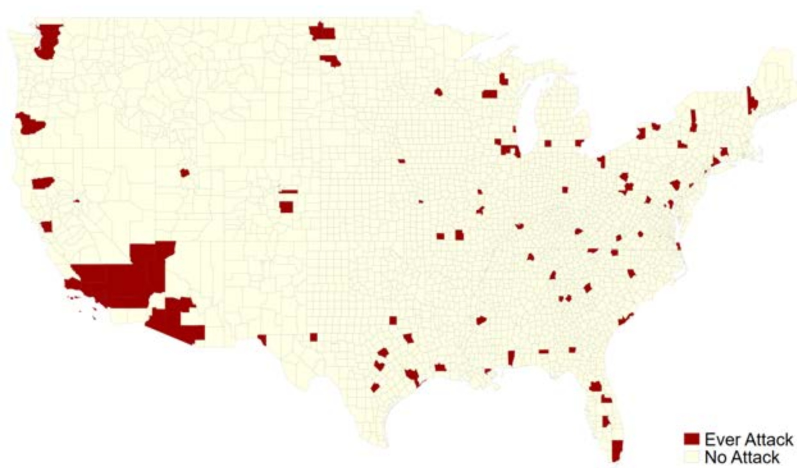
Notes: This figure illustrates the precise locations of the Sniper Shootings that occurred between October 2 and October 22, 2002, as listed in Table 1. Red circles on the map denote a 10-mile radius around each incident, calculated based on the latitudes and longitudes of the shootings.

Figure 2: Characteristics of National Level Mass Shootings

(a) Number of Mass Shootings by Year



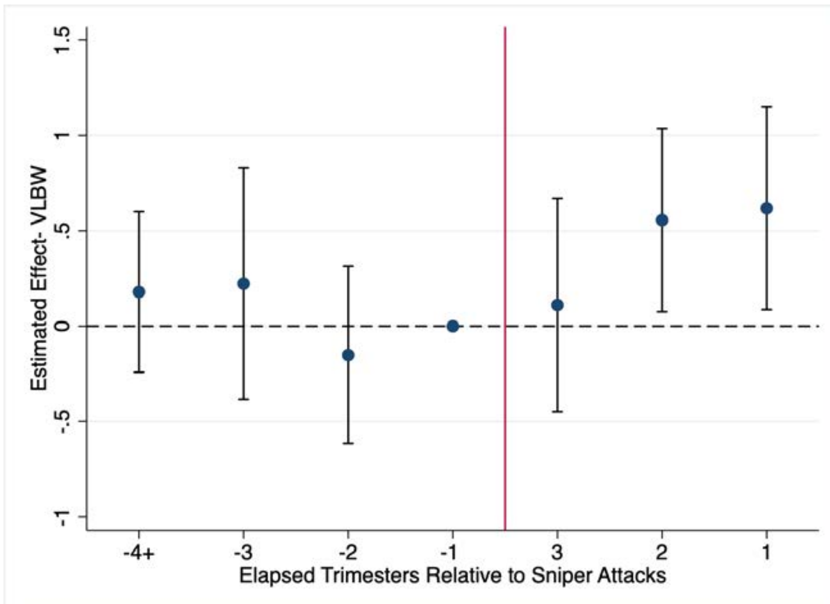
(b) Geographic Distribution of Mass Shootings (2006-2019)



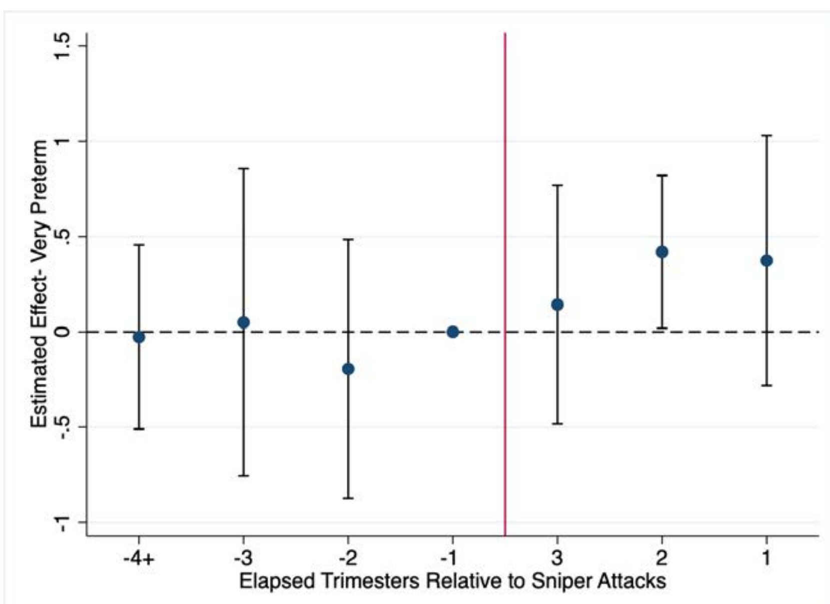
Notes: The top panel of this figure presents a graphical representation of the incidence of mass shootings between 2006 and 2019, as determined by the criteria outlined in Section 3. The data sources utilized for this analysis include the following references: SHR, USA Today (2023), The Washington Post's Mass Shooting List (2023), the Mass Shootings in America (MSA) Project prepared by Stanford University Geospatial Center and Stanford Libraries (2019), the FBI's Active Shooter Incidents in the USA Report (2017), and the Gun Violence Archive (2023). The bottom panel plots the geographic distribution of mass shootings between 2006 and 2019 based on the sources listed in Panel A. Counties in red highlight Ever Attacked counties.

Figure 3: Dynamic Effects of Sniper Shootings on Infant Health Outcomes

(a) Very Low Birthweight



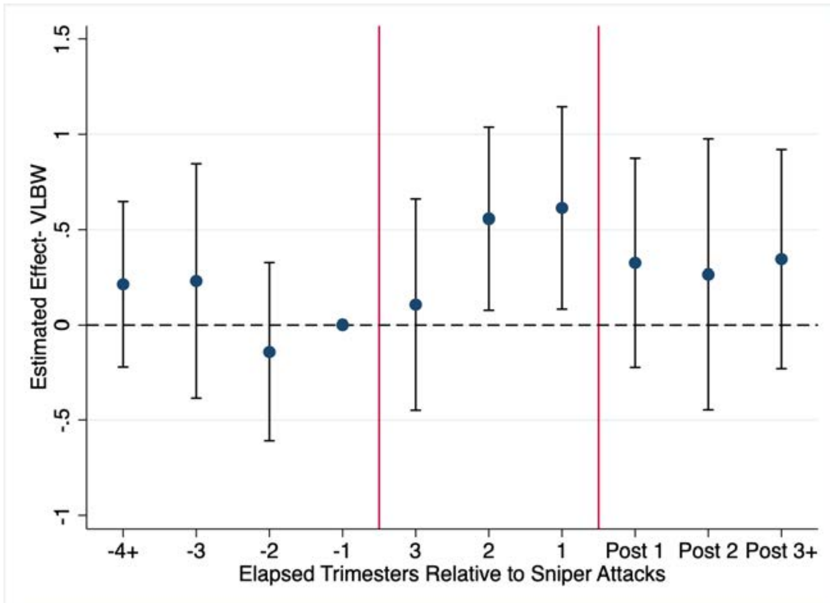
(b) Very Premature



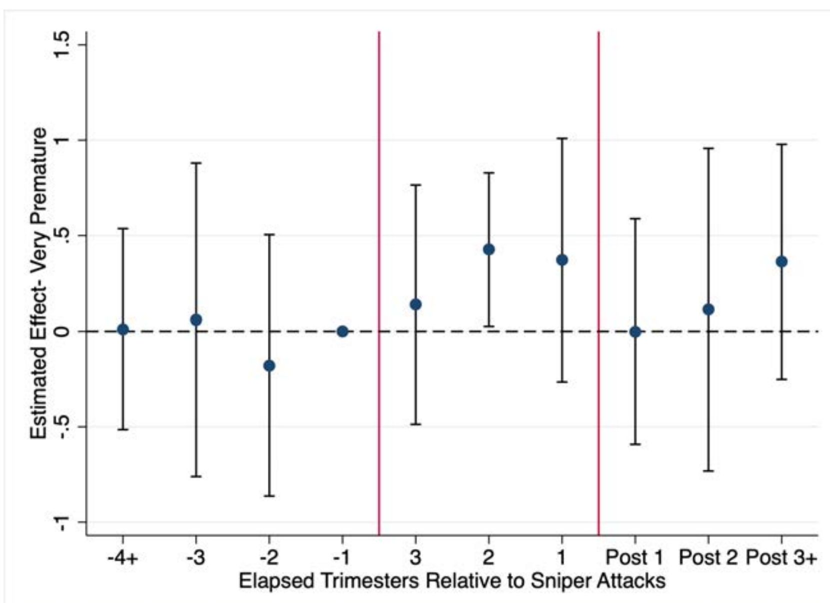
Notes: Each panel shows the coefficient estimates with 95% confidence intervals based on equation (2). "1" , "2", and "3" denote exposure to Sniper shootings within the first, second, and third trimesters of pregnancy, respectively. $q = -1$ is the reference category. Standard errors are clustered at the county level. Both outcomes are multiplied by 100 for ease of interpretation.

Figure 4: Dynamic Effects of Sniper Shootings on Infant Health Outcomes: Post Sniper Shootings Period Included

(a) Very Low Birthweight



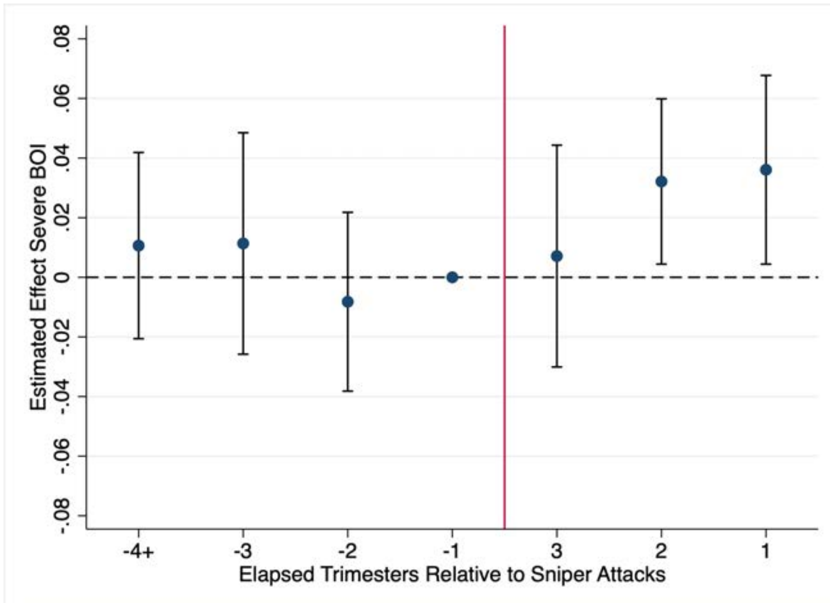
(b) Very Premature



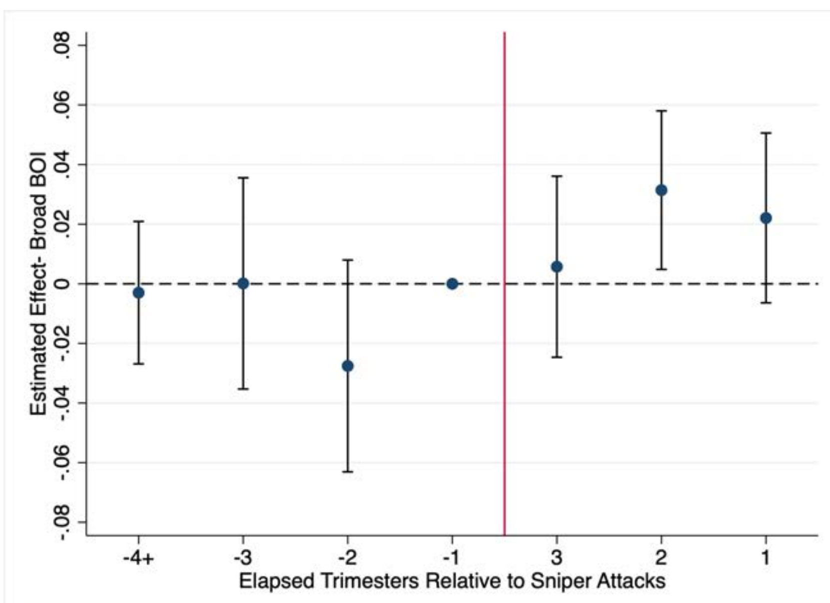
Notes: Each panel shows the coefficient estimates with 95% confidence intervals based on equation (2). "1" , "2", and "3" denote exposure to Sniper shootings within the first, second, and third trimesters of pregnancy, respectively. $q = -1$ is the reference category. Standard errors are clustered at the county level. Both outcomes are multiplied by 100 for ease of interpretation.

Figure 5: Dynamic Effects of Sniper Shootings on Infant Health Outcomes: Severe and Broad Birth Outcomes Indices

(a) Severe Birth Outcomes Index



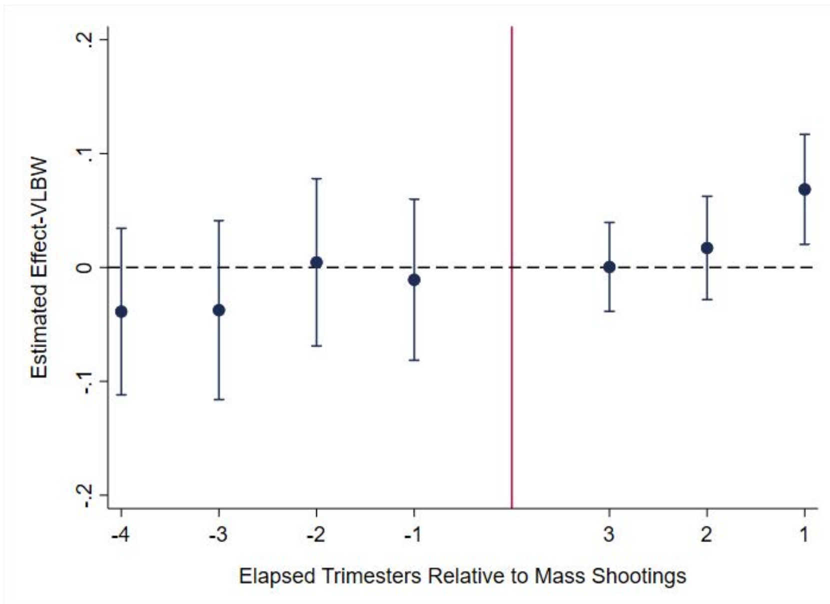
(b) Broad Birth Outcomes Index



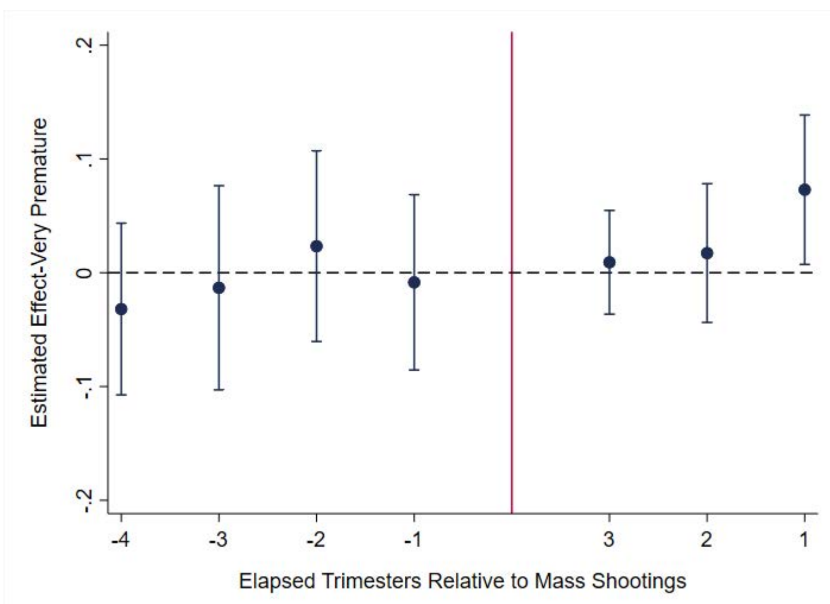
Notes: This figure shows event study estimates for Severe and Broad Birth outcome indexes. Each panel shows the coefficient estimates with 95% confidence intervals based on equation (2). "1" , "2", and "3" denote exposure to Sniper shootings within the first, second, and third trimesters of pregnancy, respectively. $q = -1$ is the reference category. Standard errors are clustered at the county level.

Figure 6: Dynamic Effects of Mass Shootings on Infant Health Outcomes

(a) Very Low Birthweight



(b) Very Premature



Notes: This figure shows event study estimates using the imputation estimator from Borusyak et al.(2022). Each panel shows the coefficient estimates with 95% confidence intervals based on equation (4). “1” , “2”, and “3” denote exposure to mass shootings within the first, second, and third trimesters of pregnancy, respectively. $q = -5$ is the reference category. Standard errors are clustered at the county level. Both outcomes are multiplied by 100 for ease of interpretation.

Table 1: Timeline of Beltway Sniper Shootings

Oct-02	A 55-year old man murdered while crossing a parking lot of a food warehouse
Oct-03	A 39-tear old man murdered while mowing grass
Oct-03	A 54-year old man murdered while pumping gas at a gas station
Oct-03	A 34-year old woman murdered while seated on a bench at a bus station, reading a book
Oct-03	A 25-year old woman murdered vacuuming her car at a gas station
Oct-03	A 72-year old man murdered while walking on a sidewalk
Oct-04	A 43-year old woman injured while loading her van at a shopping mall
Oct-07	A 13-year-old-boy critically injured at a school
Oct-09	A 53-year old man murdered while pumping gas
Oct-11	A 53-year old man murdered while pumping gas
Oct-14	A 47-year old woman murdered while loading packages into her car at a Home Depot parking lot
Oct-19	A 39-year old man injured in a parking lot outside a steakhouse
Oct-22	A 35-year old man murdered as he stood outside his vehicle

Table 2: Descriptive Statistics

	(1) Full Sample	(2) Within 10 Miles of Shootings	(3) Between 10 and 50 Miles of Shootings	(4) Beyond 50 Miles of Shootings
Panel A: Control Variables				
Mother Age	29.331	30.046	28.729	26.991
	(5.800)	(5.645)	(5.860)	(5.814)
Married	0.770	0.812	0.735	0.666
	(0.421)	(0.391)	(0.442)	(0.472)
White	0.718	0.729	0.708	0.675
	(0.450)	(0.445)	(0.455)	(0.468)
Black	0.174	0.118	0.221	0.277
	(0.379)	(0.323)	(0.415)	(0.448)
Other Race	0.107	0.151	0.070	0.047
	(0.309)	(0.358)	(0.255)	(0.212)
Hispanic	0.123	0.181	0.074	0.036
	(0.328)	(0.385)	(0.262)	(0.187)
High School or Less	0.373	0.337	0.403	0.503
	(0.484)	(0.473)	(0.491)	(0.500)
More than High School	0.616	0.646	0.590	0.494
	(0.487)	(0.478)	(0.492)	(0.500)
First Baby	0.414	0.432	0.399	0.404
	(0.493)	(0.495)	(0.490)	(0.491)
Singleton	0.966	0.966	0.965	0.969
	(0.183)	(0.181)	(0.184)	(0.173)
Male Child	0.512	0.512	0.512	0.511
	(0.500)	(0.500)	(0.500)	(0.500)
Panel B: Outcome Variables				
Very Low Birthweight	1.222	1.061	1.358	1.498
	(10.988)	(10.247)	(11.573)	(12.146)
Very Premature	1.428	1.280	1.553	1.643
	(11.865)	(11.240)	(12.365)	(12.713)
Observations	264,611	120,953	143,658	207,694

Notes: This table presents sample averages for several control and outcome variables using Vital Statistics Natality records from Virginia. Column 1 displays sample averages for the corresponding variables within a 50-mile radius of the closest shooting (full sample). Column 2 provides sample averages for mothers residing within a 10-mile radius of the closest shooting, constituting the treatment group. Column 3 presents the sample averages for the control group, specifically, mothers for whom the closest shooting is within 10-50 miles distance to their residence. Lastly, Column 4 displays the sample averages for mothers residing beyond a 50-mile radius from the closest shooting incident.

Table 3: The Effects of Sniper Shootings on Infant Health Outcomes

	Coefficient (× 100) (Standard Error)			
	(1) Very Low Birthweight	(2) Very Low Birthweight	(3) Very Premature	(4) Very Premature
Panel A:				
1st Trimester x 10 miles	0.346**	0.503**	0.163	0.410
	(0.163)	(0.197)	(0.270)	(0.285)
2nd Trimester x 10 miles	0.291**	0.438***	0.227	0.455**
	(0.116)	(0.132)	(0.178)	(0.184)
3rd Trimester x 10 miles	-0.148	-0.010	-0.039	0.178
	(0.182)	(0.223)	(0.192)	(0.208)
Panel B:				
Exposure to Shootings During	0.172*	0.316**	0.121	0.350**
Pregnancy x 10 miles	(0.098)	(0.143)	(0.110)	(0.132)
Mean DV	1.222	1.222	1.427	1.427
Conception Day-Month-Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes
County Specific Linear Trends	No	Yes	No	Yes
Observations	264,372	264,372	264,549	264,549

Notes: Each column in Panels A and B corresponds a separate regression. The outcomes in the first two columns pertain to cases of Very Low Birthweight, while the last two columns concern Very Premature births. The estimates presented in Panel A represent interaction terms involving trimester indicators reflecting exposure to Sniper shootings and the binary indicator variable *Close*. Estimates presented in Panel B replace the trimester indicators with an overall measure of exposure to shootings during pregnancy. See text for more details about regression specifications and controls. Both outcomes are multiplied by 100 for ease of interpretation. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: The Effects of Exposure to Sniper Shootings on Other Infant Health Outcomes

	(1) Birthweight	(2) Low Birthweight	(3) Gestational Length In Weeks	(4) Very Premature Less Than 34 Weeks	(5) Premature Less Than 37 Weeks	(6) Any Conditions At Birth Newborn	(7) APGAR5 Less Than 7	(8) Severe BO Index	(9) Broad BO Index
Panel A:									
1st Trimester x 10 miles	-12.509*	1.041**	-0.075**	0.575*	-0.213	0.658	0.028	0.029**	0.027**
	(7.359)	(0.392)	(0.032)	(0.302)	(0.410)	(0.422)	(0.154)	(0.014)	(0.011)
2nd Trimester x 10 miles	-40.482***	0.943**	-0.124***	0.685***	0.563	0.260	0.079	0.025**	0.037***
	(10.401)	(0.465)	(0.034)	(0.236)	(0.485)	(0.357)	(0.232)	(0.012)	(0.010)
3rd Trimester x10 miles	-24.537**	0.565	-0.038	0.313	-0.151	0.089	-0.212	0.000	0.011
	(11.221)	(0.432)	(0.038)	(0.302)	(0.460)	(0.326)	(0.222)	(0.013)	(0.013)
Panel B:									
Exposure to Shootings During	-25.789***	0.854***	-0.080***	0.527***	0.068	0.340	-0.032	0.018**	0.025***
Pregnancy x 10 miles	(5.820)	(0.238)	(0.025)	(0.197)	(0.248)	(0.255)	(0.122)	(0.008)	(0.007)
Mean DV	3335.159	7.006	38.588	2.546	9.609	4.479	1.088	0.000	0.000
Conception Day-Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Specific Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	264,372	264,372	264,549	264,549	264,549	264,174	264,035	264,549	264,549

Notes: Each column in Panels A and B corresponds to a separate regression. Column headings indicate dependent variables. The estimates presented in Panel A represent interaction terms involving trimester indicators reflecting exposure to Sniper shootings and the binary indicator variable *Close*. Estimates presented in Panel B replace the trimester indicators with an overall measure of exposure to shootings during pregnancy. See text for more details about regression specifications and controls. Outcomes are multiplied by 100 for ease of interpretation. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: The Effects of Mass Shootings on Infant Health Outcomes: National Level Mass Shootings Analysis

	Coefficient (× 100) (Standard Error)			
	(1) Very Low Birthweight	(2) Very Low Birthweight	(3) Very Premature	(4) Very Premature
Panel A:				
1st Trimester	0.077***	0.069***	0.083**	0.073**
	(0.027)	(0.025)	(0.035)	(0.034)
2nd Trimester	0.015	0.017	0.014	0.017
	(0.024)	(0.023)	(0.032)	(0.031)
3rd Trimester	-0.025	0.000	-0.021	0.009
	(0.021)	(0.020)	(0.024)	(0.023)
Panel B:				
Exposure to MS During Pregnancy	0.022	0.028	0.025	0.033
	(0.021)	(0.019)	(0.026)	(0.024)
Mean DV	1.219	1.219	1.394	1.394
Conception Quarter and Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Mother/Child Characteristics	No	Yes	No	Yes
Observations	2,930,950	2,930,950	2,933,866	2,933,866

Notes: This table presents estimates of the effect of exposure to mass shootings between 2006 and 2019 on Very Low Birthweight and Very Premature births. The coefficient estimates are obtained using imputation estimator from Borusyak et al.(2022). Each column in Panels A and B corresponds a separate regression. The estimates presented in Panel A are trimester indicators reflecting exposure to mass shootings. Estimates presented in Panel B replace the trimester indicators with an overall measure of exposure to shootings during pregnancy. See text for more details about regression specifications and controls. Both outcomes are multiplied by 100 for ease of interpretation. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: The Effects of Exposure to Mass Shootings on Other Infant Health Outcomes: National Level Mass Shootings Analysis

	(1) Birthweight	(2) Low Birthweight	(3) Gestational Length in Weeks	(4) Very Premature Less Than 34 Weeks	(5) Premature	(6) Any Conditions At Birth Newborn	(7) APGAR5 Less Than 7	(8) Severe BO Index	(9) Broad BO Index
Panel A:									
1st Trimester	-2.135	0.003	-0.004	0.029	0.145**	0.561**	0.038	0.008***	0.005**
	(1.535)	(0.047)	(0.007)	(0.039)	(0.071)	(0.220)	(0.039)	(0.003)	(0.002)
2nd Trimester	-3.422**	0.150***	-0.013**	0.068*	0.316***	0.470**	0.039	0.005*	0.006***
	(1.506)	(0.045)	(0.006)	(0.038)	(0.060)	(0.226)	(0.033)	(0.003)	(0.002)
3rd Trimester	0.282	0.018	-0.005	0.022	0.127**	0.249	0.080***	0.004**	0.003*
	(1.019)	(0.044)	(0.005)	(0.031)	(0.057)	(0.173)	(0.031)	(0.002)	(0.002)
Panel B:									
Exposure to Shootings During	-1.737	0.057	-0.007	0.040	0.196***	0.426**	0.053*	0.006***	0.004***
Pregnancy	(1.129)	(0.036)	(0.005)	(0.029	(0.053)	(0.183)	(0.028)	(0.002)	(0.002)
Mean DV	3268.88	7.700	38.510	2.549	9.501	7.001	1.352	0.000	0.000
Conception Quarter and Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,930,950	2,930,950	2,933,866	2,933,866	2,933,866	2,503,808	2,922,740	2,933,866	2,933,866

Notes: This table presents estimates of the effect of exposure to mass shootings between 2006 and 2019 on various infant health outcomes. Column headings indicate dependent variables. The coefficient estimates are obtained using imputation estimator from Borusyak et al.(2022). Each column in Panels A and B corresponds a separate regression. The estimates presented in Panel A are trimester indicators reflecting exposure to mass shootings. Estimates presented in Panel B replace the trimester indicators with an overall measure of exposure to shootings during pregnancy. See text for more details about regression specifications and controls. Outcomes are multiplied by 100 for ease of interpretation. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: Robustness Analyses: Sniper Shootings

	Include Mothers Below 18		**No Gestational Age Restrictions**		**No Marital Status and Gender of the Child Controls**		**Including Babies Conceived After October 23rd 2002**	
	(1) Very Low Birthweight	(2) Very Premature	(3) Very Low Birthweight	(4) Very Premature	(5) Very Low Birthweight	(6) Very Premature	(7) Very Low Birthweight	(8) Very Premature
1st Trimester x 10 miles	0.475**	0.366	0.540**	0.447	0.499**	0.408	0.381*	0.260
	(0.196)	(0.271)	(0.221)	(0.306)	(0.199)	(0.286)	(0.199)	(0.299)
2nd Trimester x 10 miles	0.450***	0.458**	0.451***	0.476**	0.442***	0.461**	0.326**	0.321*
	(0.131)	(0.178)	(0.139)	(0.203)	(0.132)	(0.184)	(0.127)	(0.190)
3rd Trimester x 10 miles	-0.063	0.109	-0.078	0.146	-0.007	0.185	-0.122	0.038
	(0.208)	(0.193)	(0.230)	(0.230)	(0.222)	(0.208)	(0.197)	(0.204)
Panel B:								
Exposure to Shootings During Pregnancy x 10 miles	0.293**	0.314**	0.310**	0.359**	0.317**	0.354**	0.203	0.210
	(0.132)	(0.122)	(0.146)	(0.137)	(0.143)	(0.133)	(0.127)	(0.143)
Mean DV	1.244	1.455	1.358	1.573	1.222	1.427	1.248	1.471
Conception Day-Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Specific Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	270,837	271,022	264,855	265,034	264,372	264,549	340,062	340,269

Notes: Each column in Panels A and B corresponds to a separate regression. The odd (even) columns employ Very Low Birthweight (Very Premature) as the dependent variable. The estimates presented in Panel A represent interaction terms involving trimester indicators reflecting exposure to Sniper shootings and the binary indicator variable *Close*. Estimates presented in Panel B replace the trimester indicators with an overall measure of exposure to shootings during pregnancy. See text for more details about regression specifications and controls. Outcomes are multiplied by 100 for ease of interpretation. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8: Heterogeneous Treatment Effects of Sniper Shootings by Education, Race/Ethnicity, and Maternal Age

	More than High School		High School or Less		Whites		Blacks		Mother's Age Less Than 30		Mother's Age More Than 30	
	(1) Very Low Birthweight	(2) Very Premature	(3) Very Low Birthweight	(4) Very Premature	(5) Very Low Birthweight	(6) Very Premature	(7) Very Low Birthweight	(8) Very Premature	(9) Very Low Birthweight	(10) Very Premature	(11) Very Low Birthweight	(12) Very Premature
1st Trimester x 10 miles	0.349	0.226	0.523	0.655	0.320*	0.196	0.823	1.318	0.368*	0.337	0.568*	0.514
	(0.221)	(0.295)	(0.322)	(0.429)	(0.190)	(0.270)	(0.704)	(1.033)	(0.199)	(0.254)	(0.317)	(0.436)
2nd Trimester x 10 miles	0.608**	0.871***	0.061	-0.266	0.375**	0.376**	0.594	0.433	0.135	-0.034	0.768***	0.988***
	(0.229)	(0.273)	(0.309)	(0.363)	(0.176)	(0.153)	(0.745)	(0.790)	(0.206)	(0.278)	(0.205)	(0.304)
3rd Trimester x 10 miles	-0.003	0.061	-0.060	0.365	-0.289	-0.056	1.267	0.682	-0.164	-0.052	0.036	0.293
	(0.293)	(0.212)	(0.314)	(0.440)	(0.206)	(0.190)	(0.796)	(0.861)	(0.261)	(0.272)	(0.283)	(0.298)
Mean DV	1.090	1.265	1.424	1.683	0.968	1.155	2.479	2.780	1.257	1.474	1.226	1.431
Conception Day-Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Specific Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	162,731	162,834	98,498	98,548	189,779	189,895	45,942	45,973	154,121	154,226	116,712	116,792

Notes: Each column corresponds to a separate regression. Column headings explain the corresponding sub-groups used for analysis. The odd (even) columns employ Very Low Birthweight (Very Premature) as the dependent variable. The estimates presented in Panel A represent interaction terms involving trimester indicators reflecting exposure to Sniper shootings and the binary indicator variable *Close*. Estimates presented in Panel B replace the trimester indicators with an overall measure of exposure to shootings during pregnancy. See text for more details about regression specifications and controls. Outcomes are multiplied by 100 for ease of interpretation. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 9: The Effects of Sniper Shootings with Alternative Treatment Radii

	Alternative Treatment 7 miles Radius		Alternative Treatment 5 miles Radius		Imposing 100 miles Restriction		No Outside Circle Restriction	
	(1) Very Low Birthweight	(2) Very Premature	(3) Very Low Birthweight	(4) Very Premature	(5) Very Low Birthweight	(6) Very Premature	(7) Very Low Birthweight	(8) Very Premature
1st Trimester x Radius	0.556**	0.456**	0.596**	0.439**	0.301**	0.209	0.212	0.123
	(0.236)	(0.215)	(0.238)	(0.203)	(0.138)	(0.197)	(0.138)	(0.180)
2nd Trimester x Radius	0.507***	0.586**	0.282**	0.340**	0.263*	0.381**	0.237*	0.408***
	(0.170)	(0.233)	(0.141)	(0.165)	(0.140)	(0.152)	(0.123)	(0.142)
3rd Trimester x Radius	0.193	0.260	0.294	0.316	-0.186	0.063	-0.186	0.038
	(0.252)	(0.235)	(0.268)	(0.229)	(0.166)	(0.157)	(0.157)	(0.152)
Panel B:								
Exposure to Shootings During	0.422**	0.436***	0.394**	0.366***	0.130	0.219**	0.091	0.191*
Pregnancy x 10 miles	(0.183)	(0.151)	(0.175)	(0.128)	(0.115)	(0.107)	(0.110)	(0.100)
Mean DV	1.222	1.427	1.222	1.427	1.357	1.547	1.343	1.522
Conception Day-Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Specific Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	264,372	264,549	264,372	264,549	402,059	402,315	471,861	472,146

Notes: Each column in Panels A and B corresponds to a separate regression. Column headings explain the corresponding alternative treatment radius used for analysis. The odd (even) columns employ Very Low Birthweight (Very Premature) as the dependent variable. The estimates presented in Panel A represent interaction terms involving trimester indicators reflecting exposure to Sniper shootings and the binary indicator variable *Close*. Estimates presented in Panel B replace the trimester indicators with an overall measure of exposure to shootings during pregnancy. See text for more details about regression specifications and controls. Both outcomes are multiplied by 100 for ease of interpretation. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 10: The Effects of Exposure to Sniper Shootings on Maternal Outcomes and Medical Procedures

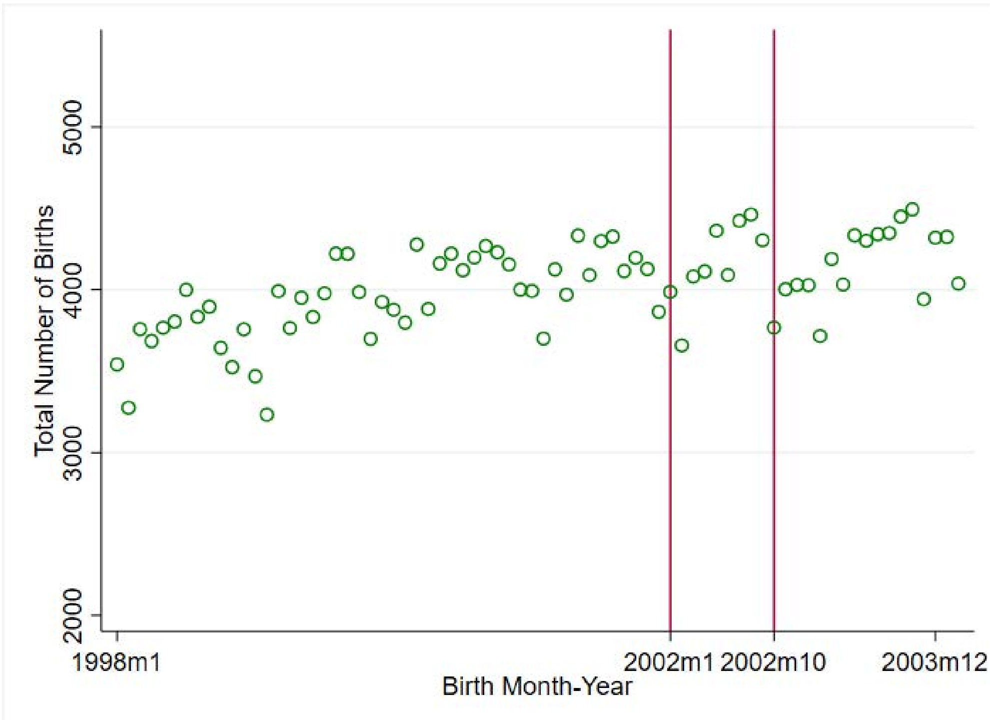
	(1) Any Obstetric Procedures	(2) Induction of Labor	(3) Tobacco Use During Pregnancy	(4) Number of Prenatal Care Visits	(5) 9 or More Prenatal Care Visits	(6) Gestational Hypertension	(7) C-Section
Panel A:							
1st Trimester x 10m	-0.335	-0.003	-0.020	0.158	1.885***	-0.307	-0.904
	(0.982)	(0.010)	(0.582)	(0.097)	(0.654)	(0.344)	(0.936)
2nd Trimester x 10m	0.254	0.003	0.252	0.018	0.844	0.441	0.475
	(1.360)	(0.014)	(0.654)	(0.090)	(0.665)	(0.380)	(0.679)
3rd Trimester x 10m	1.658**	0.017**	-0.067	0.076	0.309	0.634*	1.054
	(0.747)	(0.007)	(0.299)	(0.068)	(0.703)	(0.352)	(0.824)
Panel B:							
Exposure to Shootings During Pregnancy x 10m	0.507	0.005	0.056	0.084	1.026*	0.248	0.191
	(0.868)	(0.009)	(0.397)	(0.064)	(0.524)	(0.265)	(0.512)
Mean DV	19.847	0.198	5.272	11.766	86.695	3.959	16.656
Conception Day-Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Specific Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	264,549	264,549	264,526	260,524	260,524	264,549	264,516

Notes: Each column in Panels A and B corresponds to a separate regression. Column headings indicate dependent variables related to maternal health. The estimates presented in Panel A represent interaction terms involving trimester indicators reflecting exposure to Sniper shootings and the binary indicator variable *Close*. Estimates presented in Panel B replace the trimester indicators with an overall measure of exposure to shootings during pregnancy. See text for more details about regression specifications and controls. Outcomes are multiplied by 100 for ease of interpretation. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Online Appendix

The Hidden Cost of Firearm Violence on Pregnant Women and Their Infants

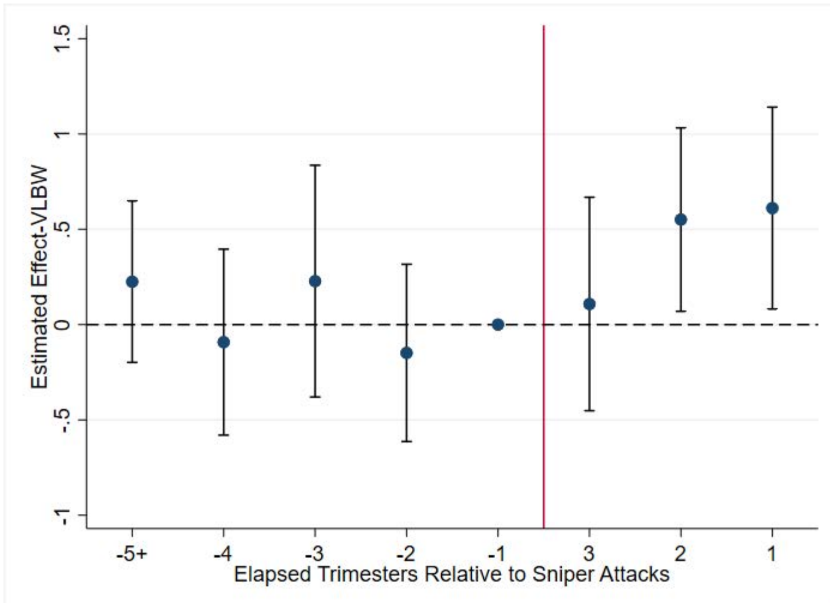
Figure A1: Total Number of Births by Birth Month-Year in Virginia



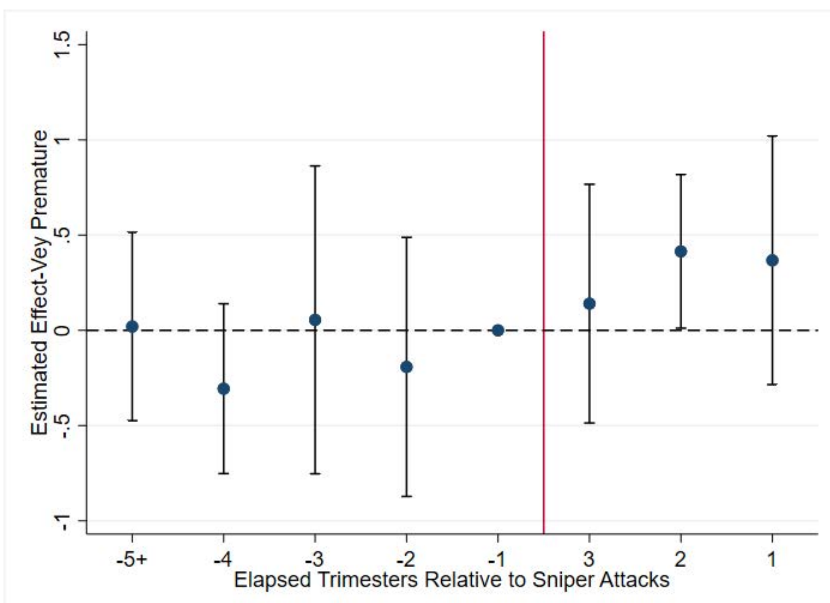
Notes: The figure shows the total number of births by birth month-year between 1998 and 2004 using the Vital Statistics Natality records from Virginia. The vertical red lines in graph denote January 2002 and October 2002, respectively.

Figure A2: Dynamic Effects of Sniper Shootings on Infant Health Outcomes: Alternative Number of Leads with 5 Pre-Treatment Indicators

(a) Very Low Birthweight



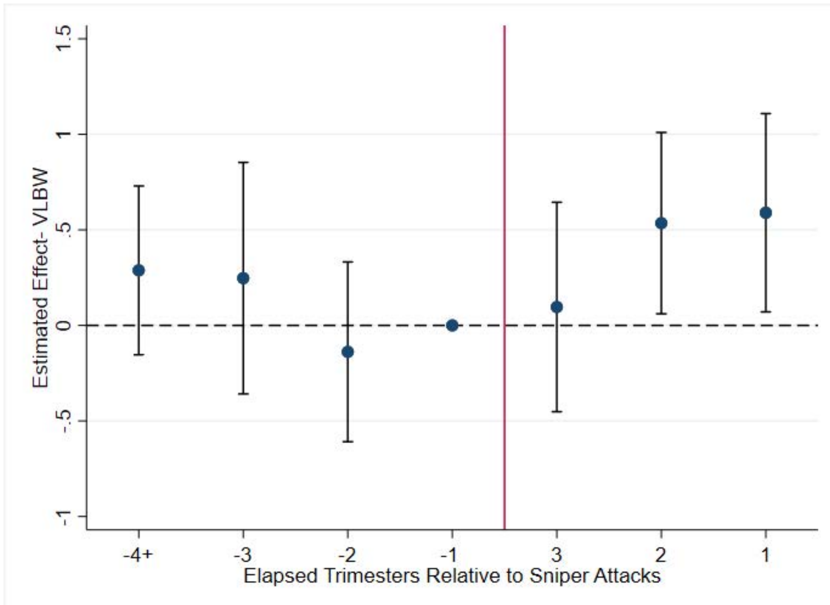
(b) Very Premature



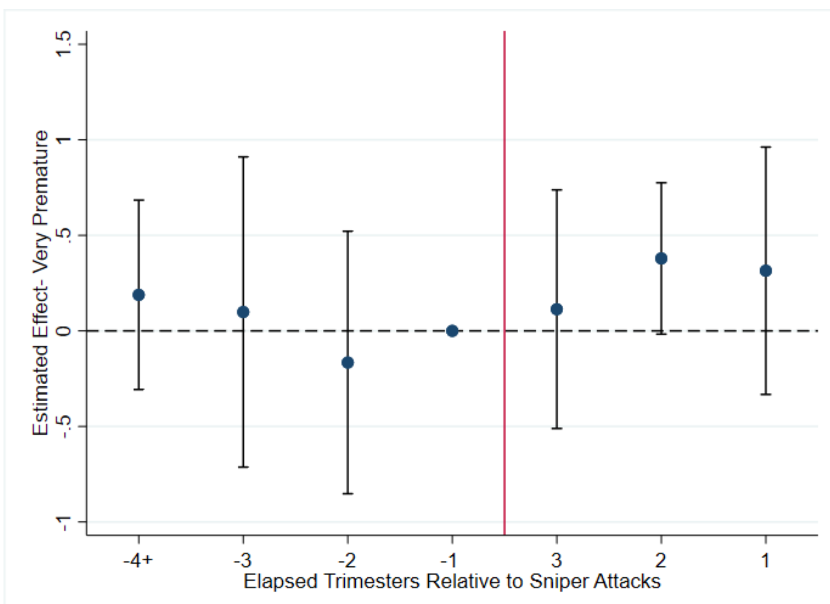
Notes: Each panel shows the coefficient estimates with 95% confidence intervals based on equation (2). “1” , “2”, and “3” denote exposure to Sniper shootings within the first, second, and third trimesters of pregnancy, respectively. $q = -1$ is the reference category. Standard errors are clustered at the county level. Both outcomes are multiplied by 100 for ease of interpretation.

Figure A3: Dynamic Effects of Sniper Shootings on Infant Health Outcomes: Excluding County Specific Linear Time Trends

(a) Very Low Birthweight



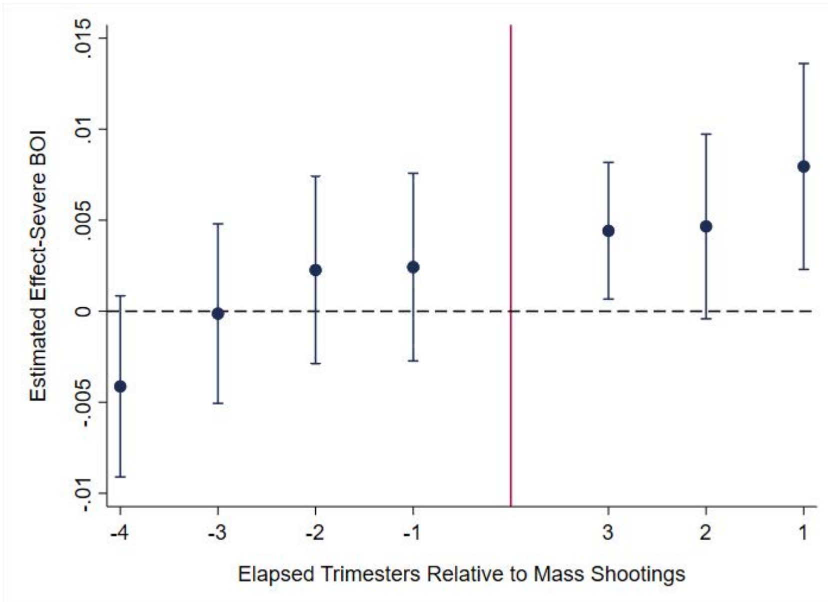
(b) Very Premature



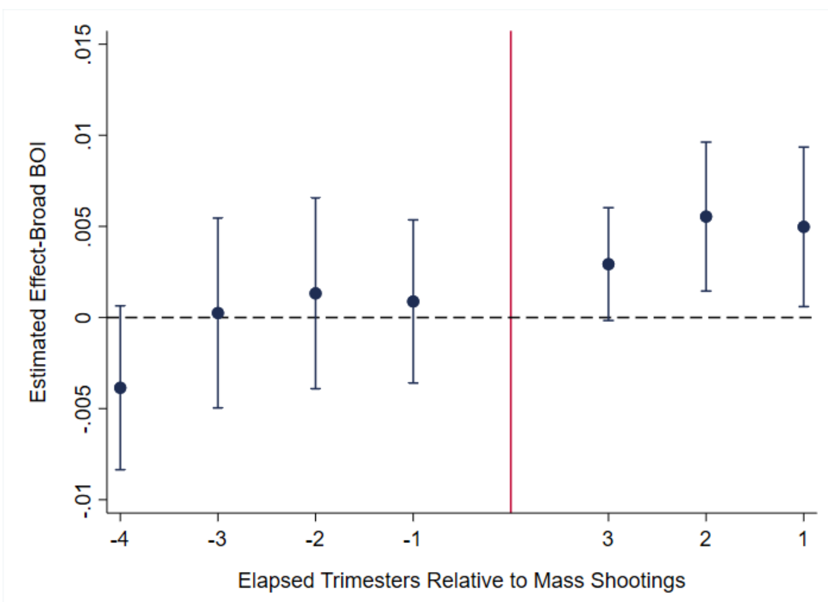
Notes: Each panel shows the coefficient estimates with 95% confidence intervals based on equation (2). "1" , "2", and "3" denote exposure to Sniper shootings within the first, second, and third trimesters of pregnancy, respectively. $q = -1$ is the reference category. Standard errors are clustered at the county level. Both outcomes are multiplied by 100 for ease of interpretation.

Figure A4: Dynamic Effects of Mass Shootings on Infant Health Outcomes: Severe and Broad Birth Outcomes Indices

(a) Severe Birth Outcomes Index



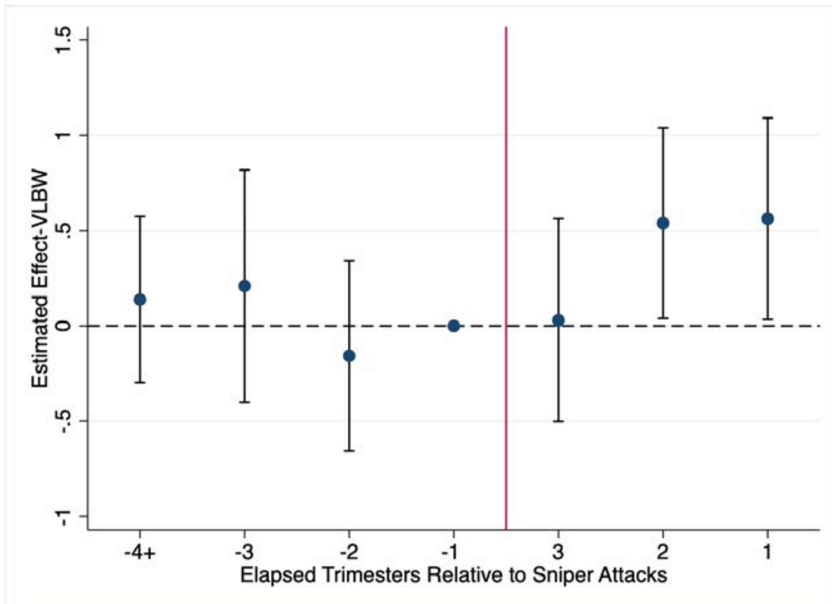
(b) Broad Birth Outcomes Index



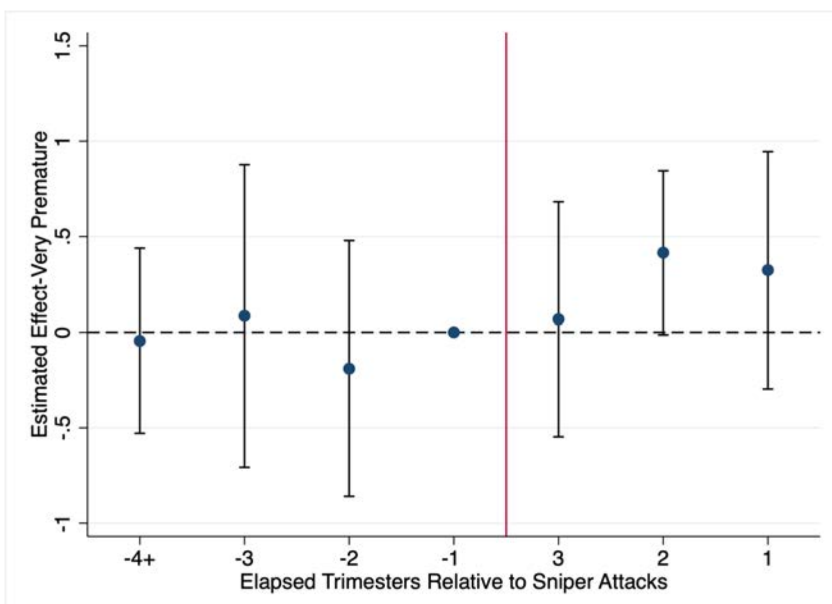
Notes: This figure shows event study estimates using the imputation estimator from Borusyak et al.(2022). Each panel shows the coefficient estimates with 95% confidence intervals based on equation (4). "1" , "2", and "3" denote exposure to mass shootings within the first, second, and third trimesters of pregnancy, respectively. $q = -5$ is the reference category. Standard errors are clustered at the county level.

Figure A5: Dynamic Effects of Sniper Shootings on Infant Health Outcomes: Including Mothers Below 18

(a) Very Low Birthweight



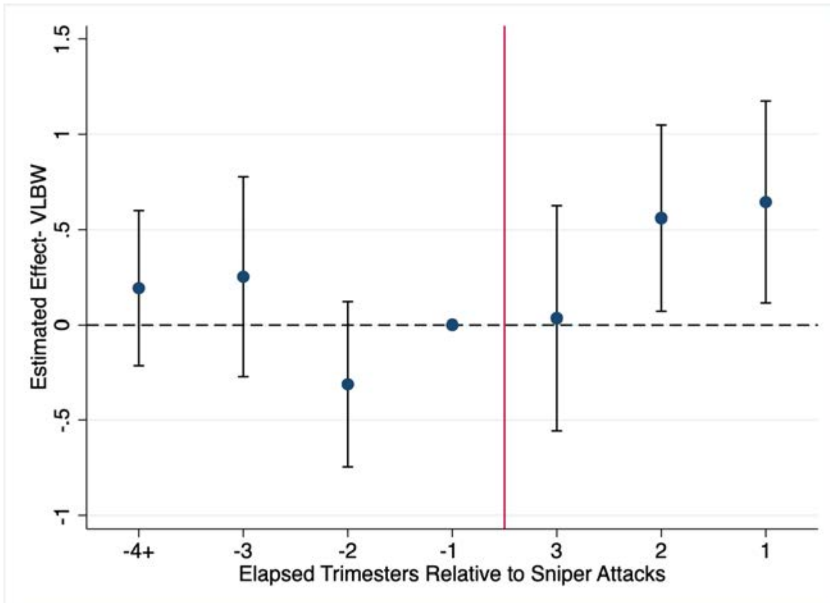
(b) Very Premature



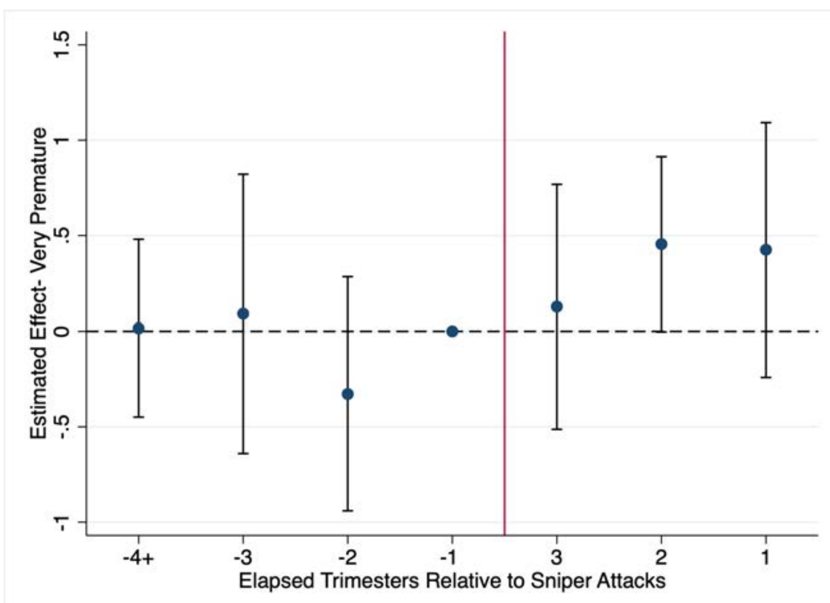
Notes: Each panel shows the coefficient estimates with 95% confidence intervals based on equation (2). "1" , "2", and "3" denote exposure to Sniper shootings within the first, second, and third trimesters of pregnancy, respectively. $q = -1$ is the reference category. Standard errors are clustered at the county level. Both outcomes are multiplied by 100 for ease of interpretation.

Figure A6: Dynamic Effects of Sniper Shootings on Infant Health Outcomes: No Gestational Age Restrictions

(a) Very Low Birthweight



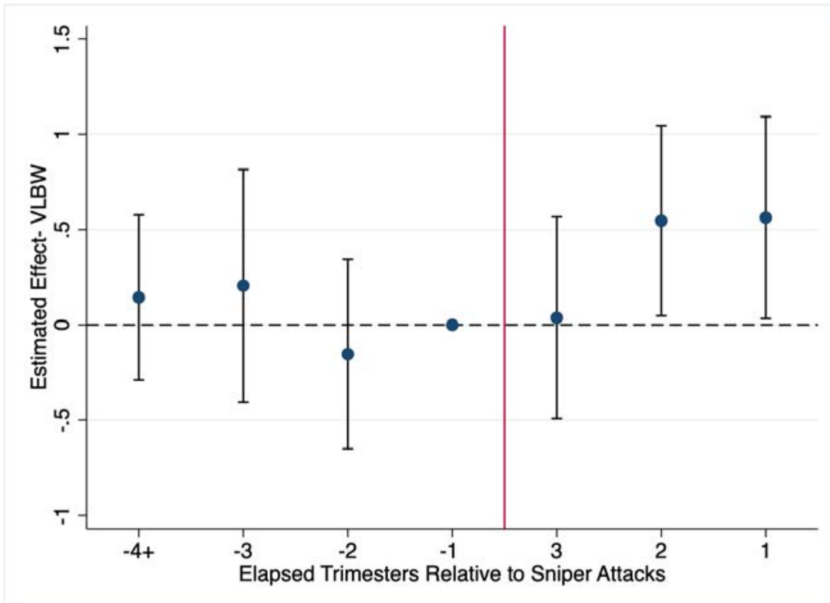
(b) Very Premature



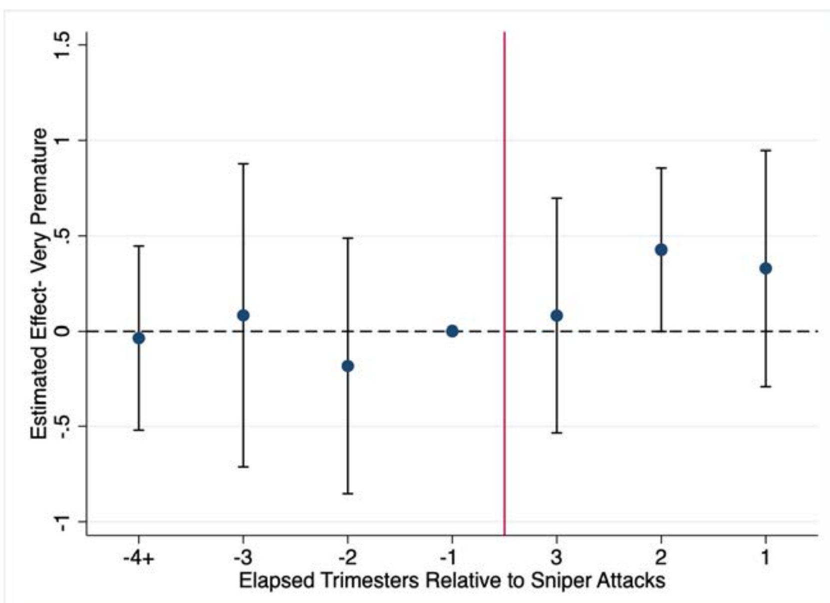
Notes: Each panel shows the coefficient estimates with 95% confidence intervals based on equation (2). "1" , "2", and "3" denote exposure to Sniper shootings within the first, second, and third trimesters of pregnancy, respectively. $q = -1$ is the reference category. Standard errors are clustered at the county level. Both outcomes are multiplied by 100 for ease of interpretation.

Figure A7: Dynamic Effects of Sniper Shootings on Infant Health Outcomes: No Marital Status and Gender of the Child Controls

(a) Very Low Birthweight

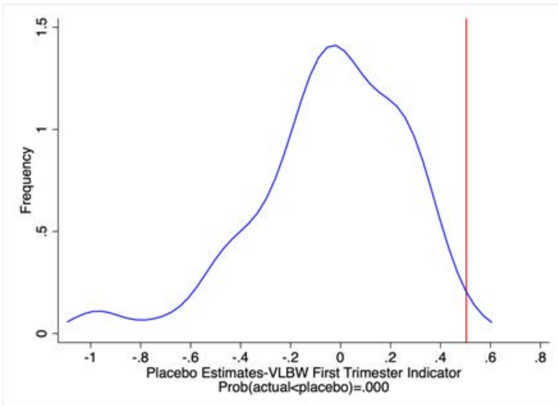


(b) Very Premature

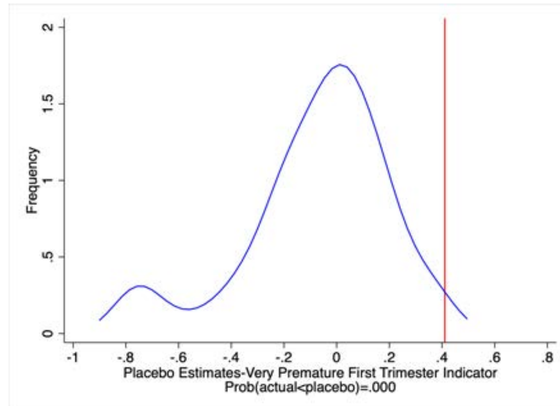


Notes: Each panel shows the coefficient estimates with 95% confidence intervals based on equation (2). "1" , "2", and "3" denote exposure to Sniper shootings within the first, second, and third trimesters of pregnancy, respectively. $q = -1$ is the reference category. Standard errors are clustered at the county level. Both outcomes are multiplied by 100 for ease of interpretation.

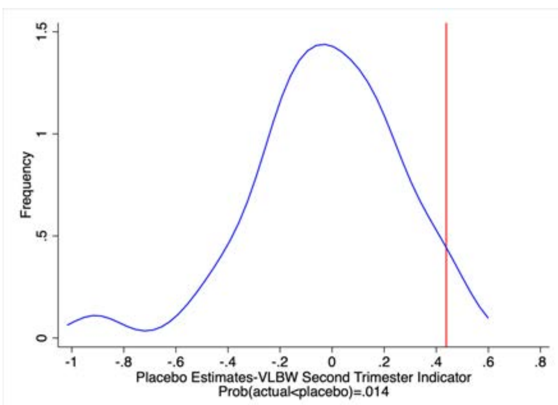
Figure A8: Placebo Estimates of Sniper Shootings: Magnitudes of the Trimester Indicators



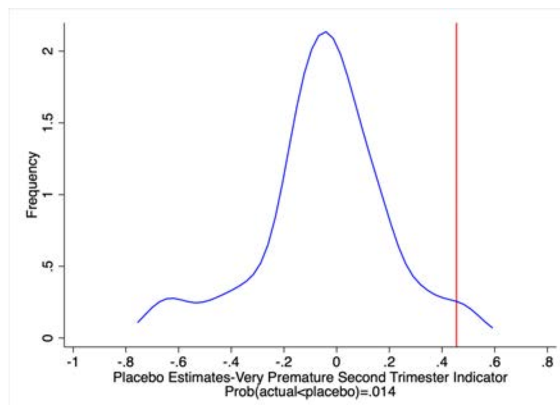
(a) VLBW- First Trimester



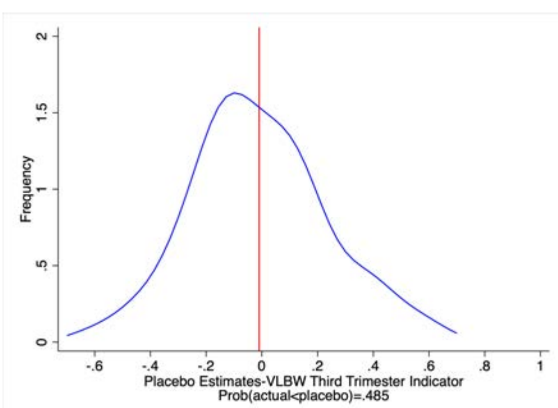
(b) Very Premature- First Trimester



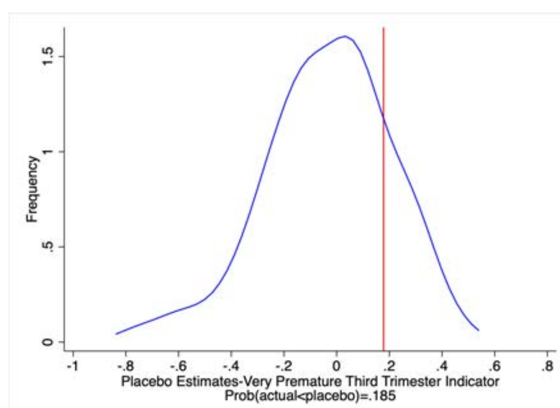
(c) VLBW- Second Trimester



(d) Very Premature- Second Trimester



(e) VLBW- Third Trimester

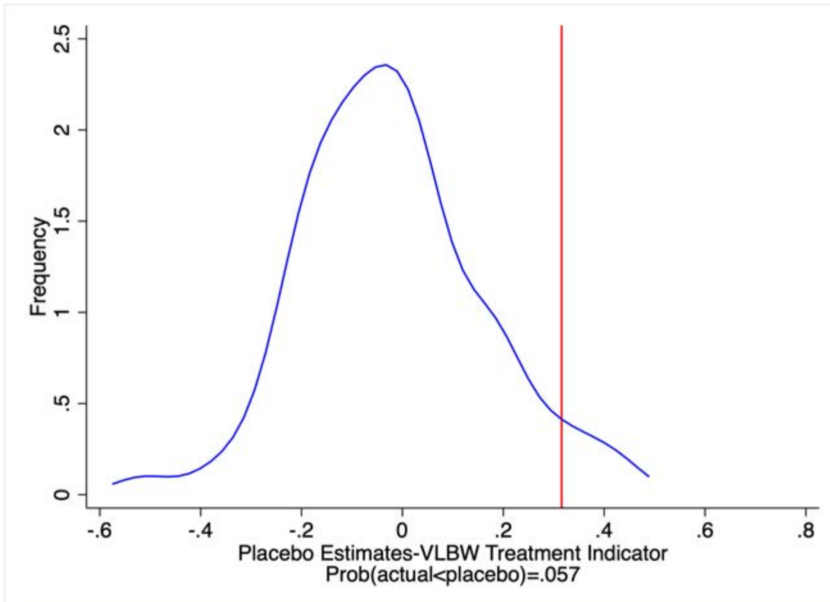


(f) Very Premature- Third Trimester

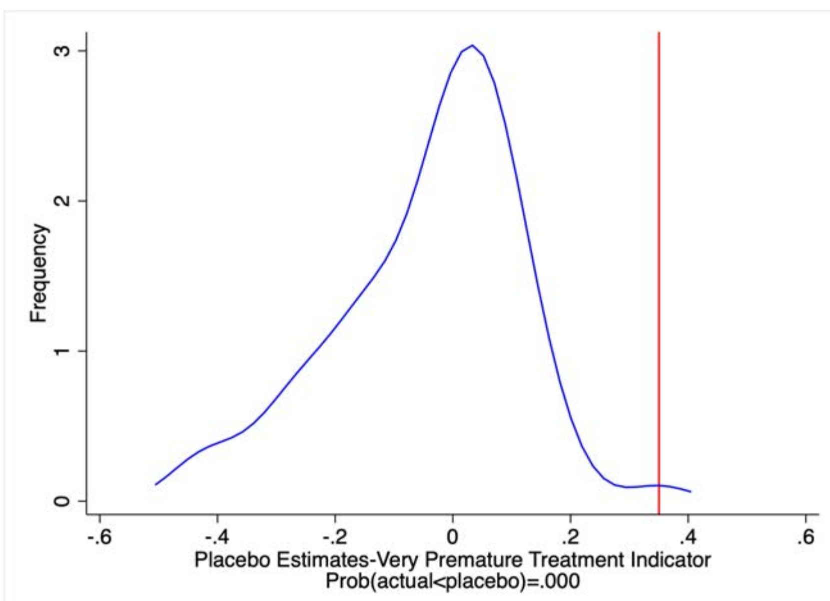
Notes: Each figure illustrates the distributions of coefficient estimates for each trimester indicator concerning the simulated shootings generated within 22-day windows partitioning the sample period . The vertical lines represent the actual estimates, and the x-axis of each plot displays the ratio of placebo estimates exceeding the actual estimates.

Figure A9: Placebo Estimates of Sniper Shootings: Magnitudes of the Treatment Indicators

(a) Very Low Birthweight



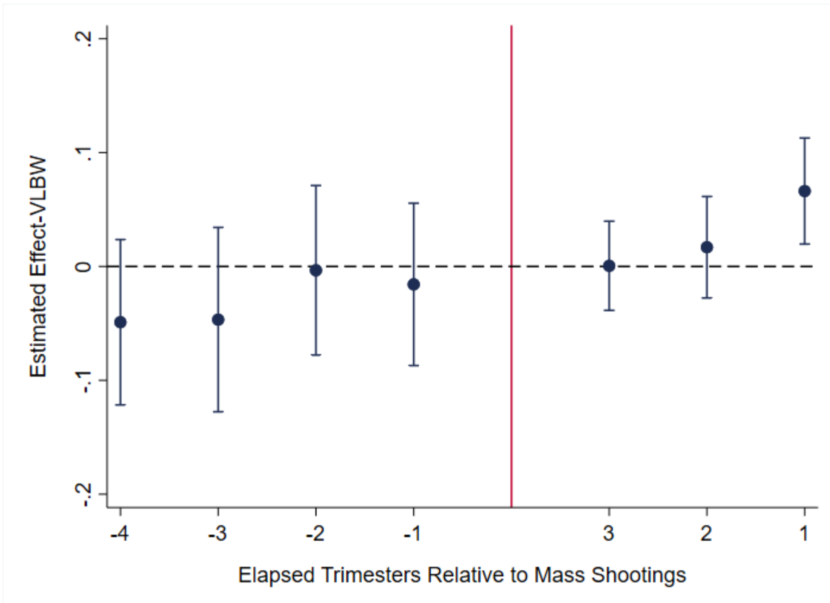
(b) Very Premature



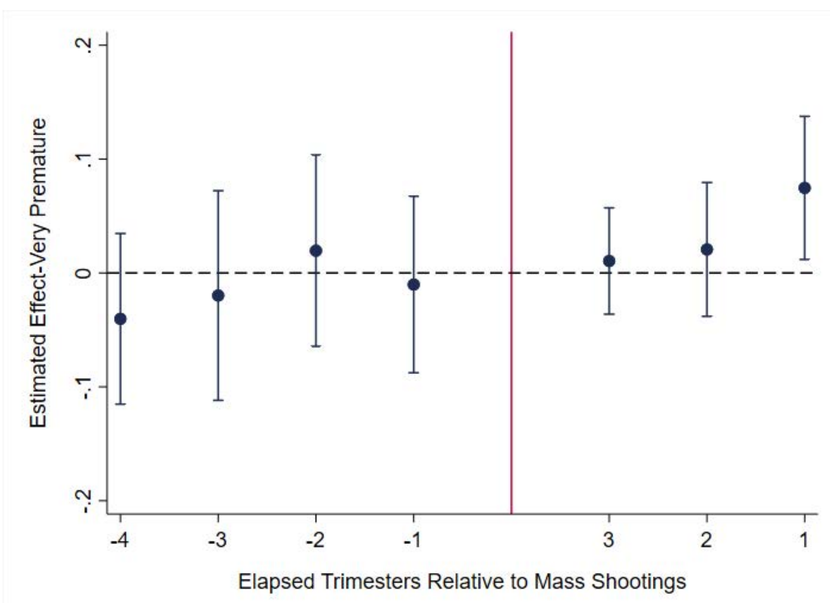
Notes: Each figure illustrates the distributions of coefficient estimates for single binary indicator for exposure to attacks concerning the simulated shootings generated within 22-day windows partitioning the sample period. The vertical lines represent the actual estimates, and the x-axis of each plot displays the ratio of placebo estimates exceeding the actual estimates.

Figure A10: Dynamic Effects of Mass Shootings on Infant Health Outcomes: Including Mothers Below 18

(a) Very Low Birthweight



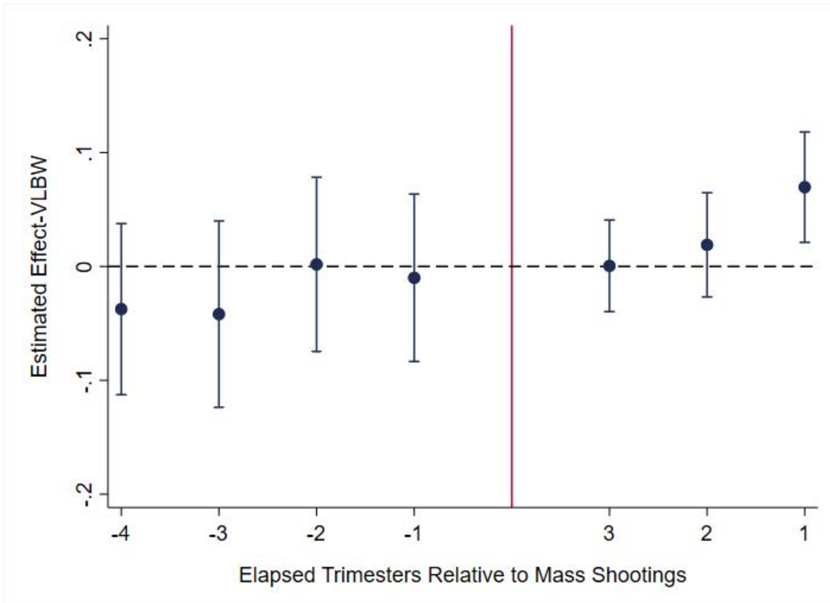
(b) Very Premature



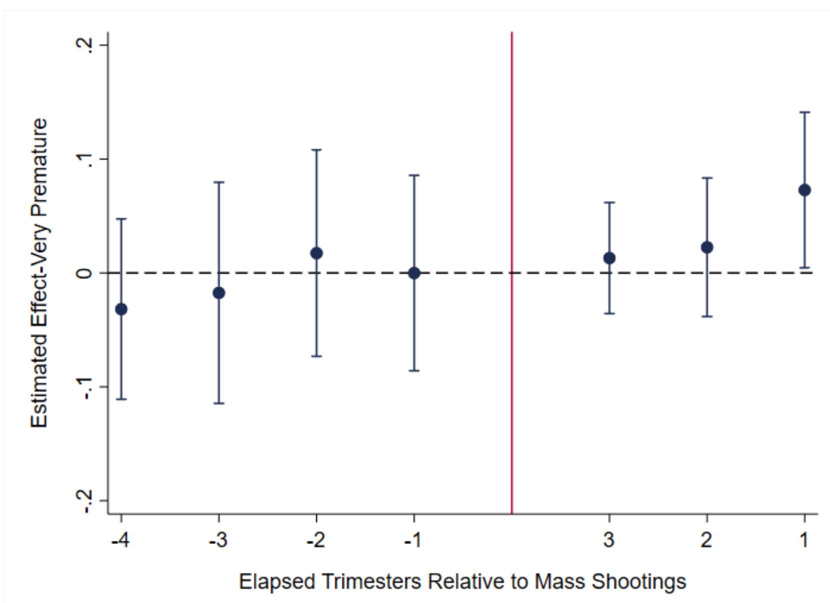
Notes: This figure shows event study estimates using the imputation estimator from Borusyak et al.(2022). Each panel shows the coefficient estimates with 95% confidence intervals based on equation (4). “1” , “2”, and “3” denote exposure to mass shootings within the first, second, and third trimesters of pregnancy, respectively. $q = -5$ is the reference category. Standard errors are clustered at the county level. Both outcomes are multiplied by 100 for ease of interpretation.

Figure A11: Dynamic Effects of Mass Shootings on Infant Health Outcomes: No Gestational Age Restrictions

(a) Very Low Birthweight



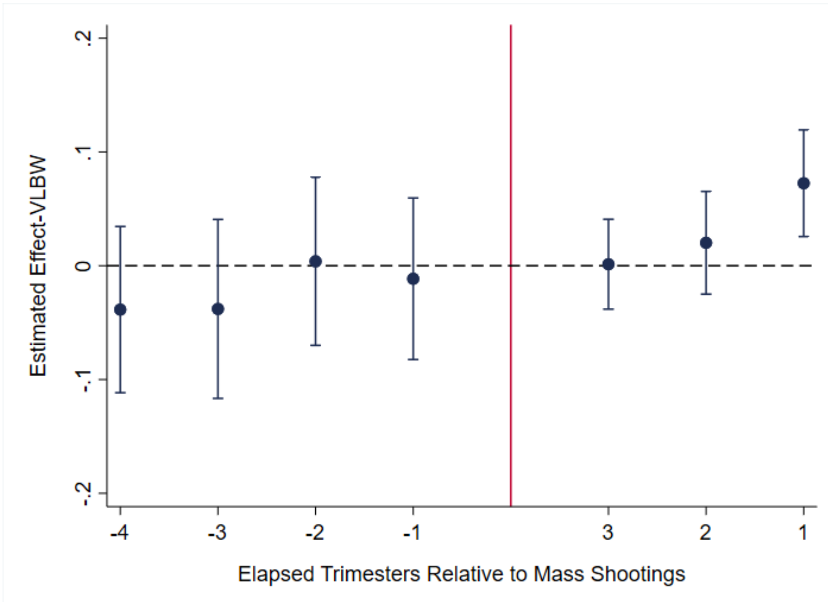
(b) Very Premature



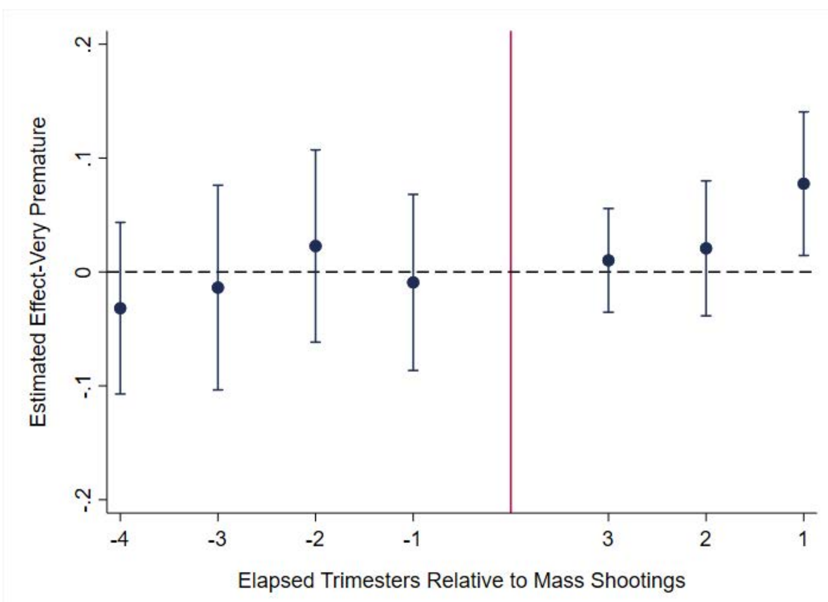
Notes: This figure shows event study estimates using the imputation estimator from Borusyak et al.(2022). Each panel shows the coefficient estimates with 95% confidence intervals based on equation (4). "1" , "2", and "3" denote exposure to mass shootings within the first, second, and third trimesters of pregnancy, respectively. $q = -5$ is the reference category. Standard errors are clustered at the county level. Both outcomes are multiplied by 100 for ease of interpretation.

Figure A12: Dynamic Effects of Mass Shootings on Infant Health Outcomes: No Marital Status and Gender of the Child Controls

(a) Very Low Birthweight

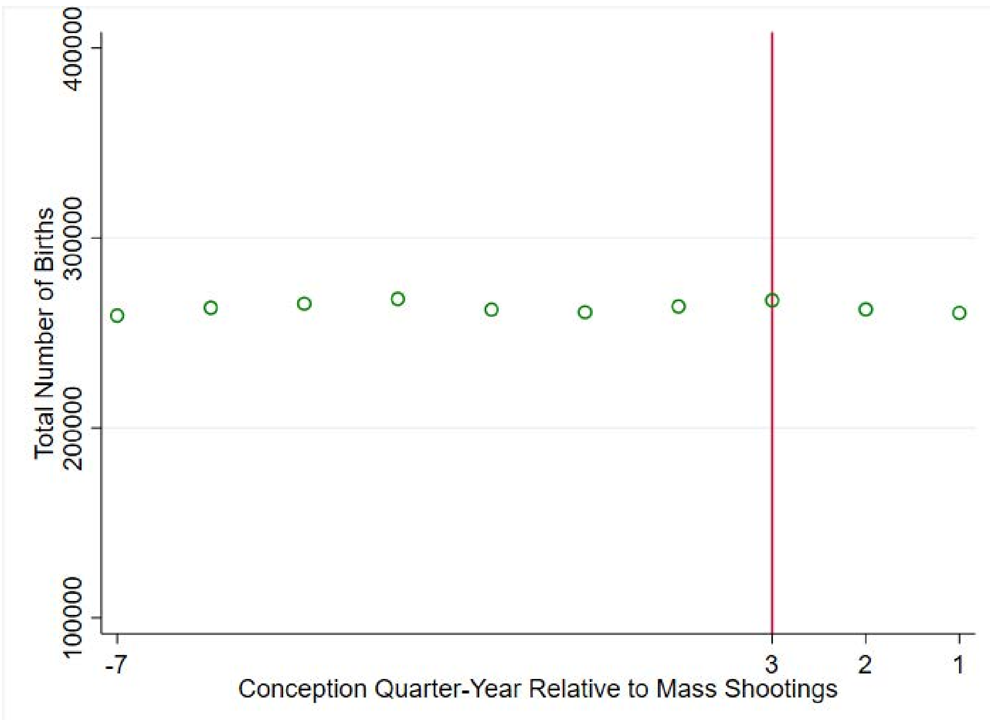


(b) Very Premature



Notes: This figure shows event study estimates using the imputation estimator from Borusyak et al.(2022). Each panel shows the coefficient estimates with 95% confidence intervals based on equation (4). "1" , "2", and "3" denote exposure to mass shootings within the first, second, and third trimesters of pregnancy, respectively. $q = -5$ is the reference category. Standard errors are clustered at the county level. Both outcomes are multiplied by 100 for ease of interpretation.

Figure A13: Birth Composition in National Data: Total Number of Births by Quarter-Year



Notes: This figure shows quarterly number of births by county up to seven quarters preceding a mass shooting and three quarters following the shootings (between 2006 and 2019) using U.S. Vital Statistics Natality records.

Appendix Table 1: Balancing Tests, Estimated Effects of Shooting Exposure on Maternal and Child Characteristics

	(1) Whites	(2) Blacks	(3) Other Race	(4) Mother's Age	(5) Gender of the Child: Male	(6) Singleton
Panel A:						
1st Trimester x 10 miles	0.004	-0.004	-0.013	-0.007	0.000	-0.000
	(0.004)	(0.004)	(0.014)	(0.031)	(0.000)	(0.000)
2nd Trimester x 10 miles	0.004	-0.004	-0.008	0.031	-0.000	0.000
	(0.009)	(0.009)	(0.011)	(0.045)	(0.000)	(0.000)
3rd Trimester x 10 miles	-0.006	0.006	0.006	-0.002	-0.000	0.000
	(0.009)	(0.009)	(0.010)	(0.040)	(0.000)	(0.000)
Panel B:						
Exposure to Shootings During	0.001	-0.001	-0.005	0.007	0.000	0.000
Pregnancy x 10 miles	(0.006)	(0.006)	(0.011)	(0.025)	(0.000)	(0.000)
Mean DV	0.718	0.174	0.107	29.331	0.512	0.965
Conception Day-Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
County Specific Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	264,549	264,549	264,549	264,549	264,549	264,552

Notes: Each column in Panels A and B corresponds to a separate regression. Column headings indicate dependent variables. The estimates presented in Panel A represent interaction terms involving trimester indicators reflecting exposure to Sniper shootings and the binary indicator variable *Close*. Estimates presented in Panel B replace the trimester indicators with an overall measure of exposure to shootings during pregnancy. See text for more details about regression specifications and controls. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table 2: The Effects of Sniper Shootings on Infant Health Outcomes Including Leads and Lags

	Coefficient (×100) (Standard Error)	
	(1) Very Low Birthweight	(2) Very Premature
-4 Pre x 10 miles	0.179	-0.027
	(0.210)	(0.241)
-3 Pre x 10 miles	0.223	0.050
	(0.302)	(0.403)
-2 Pre x 10 miles	-0.151	-0.195
	(0.232)	(0.339)
-1 Pre x 10 miles (Omitted)	(.)	(.)
1st Trimester x 10 miles	0.618**	0.375
	(0.265)	(0.327)
2nd Trimester x 10 miles	0.556**	0.420**
	(0.240)	(0.200)
3rd Trimester x 10 miles	0.111	0.143
	(0.279)	(0.313)
Conception Day-Month-Year FE	Yes	Yes
County FE	Yes	Yes
Mother/Child Characteristics	Yes	Yes
County Specific Linear Trends	Yes	Yes
Observations	264,372	264,549

Notes: Each column represents a separate regression based on equation (2). The dependent variablefor first (second) column is Very Low Birthweight (Very Premature). The estimates represent interaction terms involving trimester indicators reflecting exposure to Sniper shootings and the binary indicator variable *Close*. $q = -1$ is the reference category. See text for more details about regression specifications and controls. Both outcomes are multiplied by 100 for ease of interpretation. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table 3: The Effects of Shootings on Infant Health: Alternative Fixed Effects and Trends

	Weekly Fixed Effects and Weekly Trends		**Monthly Fixed Effects and Monthly Trends**	
	(1) Very Low Birthweight	(2) Very Premature	(3) Very Low Birthweight	(4) Very Premature
1st Trimester x 10 miles	0.507**	0.419	0.507**	0.413
	(0.202)	(0.295)	(0.200)	(0.294)
2nd Trimester x 10 miles	0.388***	0.386**	0.383***	0.380**
	(0.140)	(0.180)	(0.141)	(0.178)
3rd Trimester x 10 miles	-0.039	0.151	-0.036	0.150
	(0.227)	(0.209)	(0.229)	(0.208)
Panel B:				
Exposure to Shootings During Pregnancy x 10 miles	0.290*	0.321**	0.289*	0.314**
	(0.147)	(0.136)	(0.146)	(0.135)
Mean DV	1.222	1.427	1.222	1.427
Conception Week-Month-Year or Month-Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes
County Specific Linear Trends	Yes	Yes	Yes	Yes
Observations	264,374	264,551	264,374	264,551

Notes: Each column in Panels A and B corresponds a separate regression. The outcomes in the first two columns pertain to cases of Very Low Birthweight, while the last two columns concern Very Premature births. The estimates presented in Panel A represent interaction terms involving trimester indicators reflecting exposure to Sniper shootings and the binary indicator variable *Close*. Estimates presented in Panel B replace the trimester indicators with an overall measure of exposure to shootings during pregnancy. Both outcomes are multiplied by 100 for ease of interpretation. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table 4 : Estimates from Placebo Analysis for Sniper Shootings

	Coefficient (x 100) Standard Error							
	1998		**1999**		**2000**		**2001**	
	(1) Very Low Birthweight	(2) Very Premature	(3) Very Low Birthweight	(4) Very Premature	(5) Very Low Birthweight	(6) Very Premature	(7) Very Low Birthweight	(8) Very Premature
1st Trimester x 10 miles	0.030	-0.138	0.141	0.042	0.105	0.151	-0.304	-0.061
	(0.269)	(0.338)	(0.157)	(0.262)	(0.269)	(0.169)	(0.226)	(0.290)
2nd Trimester x 10 miles	0.359	-0.080	0.191	-0.084	-0.109	0.058	-0.087	-0.122
	(0.245)	(0.277)	(0.245)	(0.239)	(0.236)	(0.179)	(0.187)	(0.213)
3rd Trimester x 10 miles	-0.190	-0.160	-0.437*	-0.548	-0.208	-0.469	0.002	0.018
	(0.240)	(0.235)	(0.258)	(0.348)	(0.268)	(0.295)	(0.150)	(0.147)
Panel B:								
Exposure to Shootings During	0.027	-0.130	-0.064	-0.226	-0.080	-0.108	-0.128	-0.053
Pregnancy x 10 miles	(0.208)	(0.214)	(0.165)	(0.188)	(0.180)	(0.144)	(0.116)	(0.155)
Mean DV	1.130	1.330	1.160	1.351	1.227	1.424	1.250	1.447
Conception Day-Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Specific Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	69,314	69,385	117,281	117,396	166,726	166,858	216,003	216,162

Notes: Each column in Panels A and B corresponds to a separate regression. Column headings indicate the years of simulated shootings. The odd (even) columns employ Very Low Birthweight (Very Premature) as the dependent variable. The estimates presented in Panel A represent interaction terms involving trimester indicators reflecting exposure to Sniper shootings and the binary indicator variable *Close*. Estimates presented in Panel B replace the trimester indicators with an overall measure of exposure to shootings during pregnancy. See text for more details about regression specifications and controls. Outcomes are multiplied by 100 for ease of interpretation. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table 5: Robustness Analyses: National Level Mass Shootings

	Include Mothers Below 18		No Gestational Age Restrictions		No Marital Status and Gender of the Child Controls	
	(1) Very Low Birthweight	(2) Very Premature	(3) Very Low Birthweight	(4) Very Premature	(5) Very Low Birthweight	(6) Very Premature
1st Trimester	0.066***	0.075**	0.070***	0.073**	0.073***	0.077**
	(0.024)	(0.032)	(0.025)	(0.035)	(0.024)	(0.032)
2nd Trimester	0.017	0.021	0.019	0.023	0.020	0.021
	(0.023)	(0.030)	(0.023)	(0.031)	(0.023)	(0.030)
3rd Trimester	0.001	0.010	0.001	0.013	0.001	0.010
	(0.020)	(0.024)	(0.021)	(0.025)	(0.020)	(0.023)
Panel B:						
Exposure to Shootings During Pregnancy	0.028	0.035	0.029	0.036	0.031	0.036
	(0.019)	(0.023)	(0.019)	(0.025)	(0.019)	(0.024)
Mean DV	1.225	1.401	1.245	1.449	1.219	1.394
Conception Quarter and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes	Yes	No
Observations	2,992,503	2,995,481	2,932,874	2,936,373	2,930,950	2,933,866

Notes: This table presents estimates of the effect of exposure to mass shootings between 2006 and 2019 on various infant health outcomes. The coefficient estimates are obtained using imputation estimator from Borusyak et al.(2022). The odd (even) columns employ Very Low Birthweight (Very Premature) as the dependent variable. Each column in Panels A and B corresponds a separate regression. The estimates presented in Panel A are trimester indicators reflecting exposure to mass shootings. Estimates presented in Panel B replace the trimester indicators with an overall measure of exposure to shootings during pregnancy. See text for more details about regression specifications and controls. Outcomes are multiplied by 100 for ease of interpretation. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table 6: Heterogeneous Treatment Effects of Mass Shootings by Education, Race/Ethnicity, and Maternal Age

	More than High School		High School or Less		Whites		Blacks		Mother's Age Less Than 30		Mother's Age More than 30	
	(1) Very Low Birthweight	(2) Very Premature	(3) Very Low Birthweight	(4) Very Premature	(5) Very Low Birthweight	(6) Very Premature	(7) Very Low Birthweight	(8) Very Premature	(9) Very Low Birthweight	(10) Very Premature	(11) Very Low Birthweight	(12) Very Premature
1st Trimester	0.158***	0.177***	0.067	0.159	0.097***	0.073**	-0.074	-0.038	0.038	0.019	0.105***	0.136***
	(0.055)	(0.066)	(0.071)	(0.076)	(0.028)	(0.037)	(0.080)	(0.081)	(0.030)	(0.041)	(0.037)	(0.042)
2nd Trimester	0.106**	0.124**	0.067	0.114	0.008	0.003	-0.073	-0.065	0.013	-0.022	0.036	0.071**
	(0.048)	(0.059)	(0.069)	(0.084)	(0.020)	(0.027)	(0.073)	(0.082)	(0.028)	(0.036)	(0.028)	(0.035)
3rd Trimester	0.036	0.031	-0.040	0.040	0.001	0.004	-0.064	-0.101	-0.014	-0.014	0.033	0.052
	(0.043)	(0.055)	(0.067)	(0.061)	(0.019)	(0.022)	(0.081)	(0.075)	(0.029)	(0.031)	(0.031)	(0.035)
Mean DV	1.136	1.291	1.332	1.528	0.987	1.169	2.465	2.637	1.156	1.327	1.292	1.472
Conception Quarter and Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,432,767	1,434,423	1,084,808	1,085,640	2,135,443	2,137,221	468,610	469,466	1,573,356	1,574,823	1,357,594	1,359,043

Notes: This table presents estimates of the effect of exposure to mass shootings between 2006 and 2019 on various infant health outcomes for different sub-groups of population. The coefficient estimates are obtained using imputation estimator from Borusyak et al.(2022). Column headings explain the corresponding sub-groups used for analysis. The odd (even) columns employ Very Low Birthweight (Very Premature) as the dependent variable. The estimates presented are trimester indicators reflecting exposure to mass shootings. See text for more details about regression specifications and controls. Outcomes are multiplied by 100 for ease of interpretation. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table 7: The Effects of Exposure to Mass Shootings on Maternal Health Outcomes and Medical Procedures

	(1) Any Obstetric Procedures	(2) Induction of Labor	(3) Tobacco Use During Pregnancy	(4) Number of Prenatal Care Visits	(5) 9 or More Prenatal Care Visits	(6) Gestational Hypertension	(7) C-Section
Panel A:							
1st Trimester	0.459	0.046	-0.036	-0.026	-0.202	0209**	-0.207
	(0.390)	(0.213)	(0.104)	(0.045)	(0.230)	(0.096)	(0.184)
2nd Trimester	0.318	0.094	-0.092	0.014	-0.192	0.044	-0.067
	(0.457)	(0.197)	(0.082)	(0.036)	(0.196)	(0.093)	(0.168)
3rd Trimester	0.058	0.195	-0.125**	-0.012	-0.193	0.134	0.083
	(0.352)	(0.178)	(0.054)	(0.028)	(0.178)	(0.104)	(0.124)
Panel B:							
Exposure to Shootings During	0.276	0.112	-0.084	-0.008	-0.196	0.1290*	-0.062
Pregnancy	(0.346)	(0.172)	(0.072)	(0.034)	(0.182)	(0.075)	(0.146)
Mean DV	34.503	18.657	3.649	11.348	80.986	4.615	33.444
Conception Quarter and Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,506,760	2,929,952	2,441,946	2,808,143	2,808,142	2,505,673	2,930,405

Notes: This table presents estimates of the effect of exposure to mass shootings between 2006 and 2019 on various maternal health outcomes. The coefficient estimates are obtained using imputation estimator from Borusyak et al.(2022). Each column in Panels A and B corresponds to a separate regression. The estimates presented are trimester indicators reflecting exposure to mass shootings. See text for more details about regression specifications and controls. Outcomes are multiplied by 100 for ease of interpretation. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table 8: Calculating the Social Cost of Sniper Attacks and Mass Shootings on Pregnant Women

Channel		Estimate		Source
		Sniper Attacks	Mass Shootings	
Cost due to infant death	$1,295,006	$409,222	$36,260	Matthews et al. (2015) and Cutler and Meara (2000)
Infant medical care cost	$251,734	$79,548	$7,049	Rogowski (1998)
Childhood disability cost	$66,527	$21,022	$1,863	Hack et al. (2002) and Stabile and Allin (2012)
Cost due to reduction in adult income	$20,824	$6,581	$583	American Communities Survey (2017) and Bharadwaj et al. (2018)
Cost of adult disability (medical care)	$84,609	$26,736	$2,369	Hack et al. (2002) and Anderson et al. (2010)
Cost of long-term mortality risk	$1,258,778	$397,774	$35,246	Bharadwaj et al. (2018) and Lee et al. (2009)
Total Estimated Cost	$2,977,477	$940,883	$83,369	

Notes: This table presents the details of our calculation of the cost of exposure to sniper attacks and mass shootings. We use our estimated effects on the likelihood of very low birthweight from the binary exposure to sniper attacks (0.316 in Table 3) and mass shootings (0.028 in Table 5) models. Following Currie et al. (2022), we consider increased costs association with six 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. Dollar amounts have been inflation adjusted to 2023 values using the US consumer price index. To determine the present discounted value of lifetime earnings, we aggregate over the distribution of earnings data from ages 16 to 64 in the 2017 American Communities Survey, assuming that earnings are discounted using a 3 percent real rate (i.e., a 5 percent discount rate with 2 percent wage growth) back to age zero.

Appendix Table 9: Descriptive Statistics, National Level Mass Shootings Analysis

	(1)	(2)	(3)
	Full Sample	Treatment Group	Control Group
Panel A: Control Variables			
Mother Age	28.930	29.039	28.859
	(5.881)	(5.867)	(5.889)
White	0.729	0.723	0.732
	(0.445)	(0.447)	(0.443)
Black	0.159	0.162	0.158
	(0.366)	(0.369)	(0.364)
Other/Native	0.112	0.115	0.110
	(0.315)	(0.318)	(0.313)
Hispanic	0.383	0.381	0.384
	(0.486)	(0.486)	(0.486)
High School or Less	0.370	0.365	0.373
	(0.483)	(0.482)	(0.484)
More than High School	0.630	0.635	0.627
	(0.483)	(0.482)	(0.484)
First Baby	0.322	0.321	0.322
	(0.467)	(0.467)	(0.467)
Male Child	0.512	0.512	0.512
	(0.500)	(0.500)	(0.500)
Panel B: Outcome Variables			
Very Low Birthweight	0.012	0.012	0.012
	(0.110)	(0.110)	(0.110)
Very Premature	0.014	0.014	0.014
	(0.117)	(0.117)	(0.117)
Observations	2,969,057	1,168,120	1,800,937

Notes: This table presents sample averages for several control and outcome variables for mothers who reside in Ever Attacked counties, using U.S. Vital Statistics Natality records data for the mass shootings between 2006 and 2019. Column 1 displays sample averages for the corresponding variables for women who resides in Ever Attacked counties (full sample). Column 2 provides sample averages for mothers who were exposed to shootings during their pregnancies, constituting the treatment group. Column 3 presents the sample averages for the control group, including women who experienced mass shootings after their expected month of delivery.

Appendix Table 10: Balancing Tests, Estimated Effects of Mass Shootings Exposure on Maternal and Child Characteristics National Level Mass Shootings Analysis

	(1) Whites	(2) Blacks	(3) Natives or Other Race	(4) Mother's Age	(5) Gender of the Child: Male	(6) Singleton Baby
Panel A:						
1st Trimester	0.002	-0.002	-0.000	-0.003	0.001	-0.000
	(0.002)	(0.002)	(0.001)	(0.009)	(0.001)	(0.000)
2nd Trimester	-0.000	0.000	-0.001	0.007	0.000	-0.001
	(0.002)	(0.002)	(0.001)	(0.008)	(0.000)	(0.000)
3rd Trimester	0.000	-0.000	-0.001	0.009	0.000	-0.000
	(0.001)	(0.000)	(0.000)	(0.006)	(0.000)	(0.000)
Panel B:						
Exposure to MS During	0.000	-0.000	-0.001	0.005	0.001	-0.000
Pregnancy	(0.001)	(0.001)	(0.001)	(0.007)	(0.001)	(0.000)
Mean DV	0.729	0.159	0.112	28.930	0.512	0.967
Conception Month and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,933,866	2,933,866	2,933,866	2,933,866	2,933,866	2,933,866

Notes: The coefficient estimates are obtained using imputation estimator from Borusyak et al.(2022). Each column in Panels A and B corresponds a separate regression. The estimates presented in Panel A are trimester indicators reflecting exposure to mass shootings. Estimates presented in Panel B replace the trimester indicators with an overall measure of exposure to shootings during pregnancy. See text for more details about regression specifications and controls. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table 11: Characteristics of Counties Ever Experienced Mass Shootings vs. Never Experienced Mass Shootings

	(1)	(2)	(3)
Variables	Counties Ever Experienced Mass Shootings	Counties Never Experienced Mass Shootings	Difference
Very Low Birthweight	1.219	1.242	-0.023***
	(10.974)	(11.075)	(0.007)
Very Premature	1.394	1.421	0.027***
	(11.726)	(11.837)	(0.007)
First Baby	0.322	0.307	-0.015***
	(0.467)	(0.461)	(0.001)
Mother's Age	28.930	28.259	-0.670***
	(5.881)	(5.765)	(0.003)
Whites	0.728	0.768	0.040***
	(0.0445)	(0.422)	(0.001)
Blacks	0.159	0.149	-0.010***
	(0.366)	(0.356)	(0.001)
Others/Natives	0.112	0.080	-0.032***
	(0.315)	(0.272)	(0.001)
Hispanics	0.377	0.177	-0.199***
	(0.485)	(0.0382)	(0.001)
More than High School	0.630	0.667	0.035***
	(0.483)	(0.472)	(0.001)
High School or Less	0.370	0.334	-0.035***
	(0.483)	(0.472)	(0.001)
Married	0.573	0.609	0.036***
	(0.495)	(0.488)	(0.001)
County Population Size>1m	0.750	0.122	-0.628***
	(0.433)	(0.328)	(0.001)
County Population Size Between 500k and 1m	0.166	0.199	0.033***
	(0.372)	(0.399)	(0.001)
County Population Size Between 250k and 500k	0.050	0.181	0.131***
	(0.218)	(0.385)	(0.001)
County Population Size Between 100k and 250k	0.023	0.206	0.182***
	(0.151)	(0.404)	(0.001)
County Population Size Between 100k and 250k	0.010	0.292	0.282***
	(0.099)	(0.454)	(0.001)

Notes: This table compares the sample averages of several control and outcome variables for mothers who reside in Ever Attacked counties and Never Attacked counties using U.S. Vital Statistics Natality records data for the mass shootings between 2006 and 2019. Column 1 displays sample averages for the corresponding variables for women who resides in Ever Attacked counties. Column 2 provides sample averages for mothers who live in Never Attacked Counties. Column 3 presents the difference between first and second columns. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table 12: The Effects of Mass Shootings on Infant Health Outcomes
National Level Mass Shootings Analysis: Including Never Treated Counties

	(1) Very Low Birthweight	(2) Very Premature
Panel A:		
1st Trimester	0.045***	0.032
	(0.012)	(0.021)
2nd Trimester	0.005	0.019
	(0.010)	(0.017)
3rd Trimester	-0.017	-0.003
	(0.013)	(0.016)
Panel B:		
Exposure to MS During Pregnancy	0.011	0.016
	(0.009)	(0.016)
Mean DV	1.193	1.380
Conception Quarter and Year FE	Yes	Yes
County FE	Yes	Yes
Mother/Child Characteristics	Yes	Yes
County Specific Linear Trends	Yes	Yes
Observations	191,901	191,901

Notes: This table presents estimates of the effect of exposure to mass shootings between 2006 and 2019 on Very Low Birthweight and Very Premature births. The coefficient estimates are obtained using imputation estimator from Borusyak et al.(2022). The estimation sample includes the observations from Never Treated Counties as well. For computational efficiency purposes the estimation sample is collapsed at the conception quarter-year by county level. Regressions are weighted by number of women who gave birth at the conception quarter-year and county level. Each column in Panels A and B corresponds a separate regression. The estimates presented in Panel A are trimester indicators reflecting exposure to mass shootings. Estimates presented in Panel B replace the trimester indicators with an overall measure of exposure to shootings during pregnancy. See text for more details about regression specifications and controls. Both outcomes are multiplied by 100 for ease of interpretation. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.