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Author

Jaskiewicz, Anna Maria

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Essays in Applied Microeconomics

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by

Anna Jaskiewicz

Committee in charge:

Professor Heather Royer, Chair
Professor Shelly Lundberg
Professor Kelly Bedard

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The Dissertation of Anna Jaskiewicz is approved.

Professor Shelly Lundberg

Professor Kelly Bedard

Professor Heather Royer, Committee Chair

May 2024

Essays in Applied Microeconomics

Curriculum Vitæ

Anna Jaskiewicz

Education

2024	Ph.D. in Economics (Expected), University of California, Santa Barbara.
2019	M.A. in Economics, University of California, Santa Barbara.
2018	B.A. in Economics, New York University, Shanghai.

Abstract

Essays in Applied Microeconomics

by

Anna Jaskiewicz

The first chapter studies the impact of anti-Black hate crimes on health outcomes of Black Americans. In 2019, hate crimes reported in the United States rose to the highest level in a decade. High exposure to race-motivated violence may induce psychological stress among Black individuals, contributing to racial disparities in health. In this paper, I conduct two separate yet complementary studies that document the adverse effects of anti-Black hate crimes on the health outcomes of Black infants and adults. First, I leverage a rich data set consisting of all nationwide birth records from the National Center for Health Statistics to show that in utero exposure to local anti-Black aggravated assaults is associated with lower birth weights and shorter gestation lengths among Black infants. Second, using restricted-access Emergency Department Data from the California Department of Health Care Access and Information, I find an increase in the volume of chest pain-related Emergency Department visits among Black adults following an anti-Black aggravated assault in their area of residence. In contrast to these results, I report that the effects on White infants and adults are negligible in magnitude and largely insignificant. Taken together, this suggests that stress associated with exposure to local anti-Black hate crimes may be a contributor to the racial health disparities present in the United States.

The second chapter, co-authored with Michael Topper, explores how in utero exposure to gunshot noise affects birth outcomes of mothers in California. Gun violence is ubiquitous across the United States, with gun-related deaths reaching an all-time high in 2021.

The prevalence of gunfire results in loud and potentially stress-inducing sounds, which may adversely affect critical stages of in utero development. However, gunfire is largely unreported, creating a unique challenge for researchers to understand its consequences. In this paper, we mitigate this shortcoming by leveraging data from ShotSpotter–an acoustic gunshot technology which uses an array of sensors placed on city structures to detect the sound of gunfire. We combine this unique data source with the universe of births from nine California cities, each matched to a mother's residence. Using the variation in gunfire detections from ShotSpotter at the census-block level, we employ a difference-in-differences methodology and find that gunshot noise creates substantial increases in very low birth weight ($<$ 1,500 grams) and very pre-term births ($<$ 32 weeks). These effects are driven by times of the day when mothers are likely to be at-home, and are particularly concentrated among mothers with low levels of education. These results suggest that gunshot noise is a major factor contributing to the income inequities in pregnancy outcomes.

The last chapter–joint work with Dingyue (Kite) Liu, Ruth Morales, and Jinglan (Caroline) Zhang–is a field experiment investigating if online leaderboards can positively shape student study behaviors. Procrastination is a common occurrence in everyday life, particularly among students. In this paper, we explore the implementation of a gamified leaderboard within an undergraduate economics course to assess its impact on class engagement and procrastination reduction. The leaderboard is integrated within weekly online assignments, auto-graded using an AI-assisted platform. Students achieving a full score and submitting their work earlier are ranked higher on the leaderboard. Our results suggest that the treated group, i.e., the group exposed to the leaderboard, exhibits earlier completion times relative to the control group, i.e., the group not exposed to the leaderboard. This points to the positive influence of gamified leaderboards on reducing procrastination tendencies and motivating students to complete tasks earlier.

Permissions and Attributions

1. The content of chapter 2 is the result of a collaboration with Michael Topper.
2. The content of chapter 3 and its appendix is the result of a collaboration with Dingyue (Kite) Liu, Ruth Morales, and Jinglan (Caroline) Zhang.

Contents

Chapter 1

Anti-Black Hate Crimes & Health Outcomes of Black Americans

1.1 Introduction

Disparities in health outcomes between Black and White Americans begin at birth and persist throughout the life course. Black infants are over twice as likely as White infants to die before their first birthday (Office of Minority Health, 2023a), and Black adults are 30% more likely to die from heart disease relative to their White peers (Office of Minority Health, 2023b). While earlier research often attributed these disparities to differences in socio-economic conditions, a substantial body of work points instead to the psychological stress experienced by minority individuals upon encountering discrimination (Geronimus, 1992; Lauderdale, 2006; Gemmill et al., 2019; Samari et al., 2020; Goin et al., 2021; Curtis et al., 2021; Vu et al., 2023). To this end, as of September 2023, 21 states have declared racism as a public health crisis, acknowledging that racism constitutes a major barrier to achieving health equity (American Public Health Association, 2023).

Hate crimes, i.e., crimes motivated by a bias against a specific social group, are an increasingly common manifestation of racism. There were over 7,000 hate crimes reported in the United States in 2019, the highest level in a decade, and anti-Black bias motivated a quarter of these incidents (see: Figure 1.1). In fact, in 2019, two out of three Black Americans resided in a county that reported at least one anti-Black hate crime, making exposure to nearby racially motivated violence a common experience for Black Americans.

This paper is the first one to study the impact of local anti-Black hate crimes on the health outcomes of Black Americans. I focus specifically on the impact of anti-Black aggravated assaults.[1] Aggravated assaults are the largest violent offense category among hate crimes,[2] and violent crimes are more likely to attract attention of news media (Marsh, 1991; O'Hear, 2020), allowing for information dissemination among non directly victimized individuals.

I provide evidence from two separate but complementary sets of analyses (see: Table 1.1). In the first one, I use the restricted-access natality data from the National Center for Health Statistics to examine the impact of in utero exposure to local anti-Black aggravated assaults on birth outcomes. In the second one, I use the restricted-access Emergency Department Data from the California Department of Health Care Access and Information to study how the volume of chest pain-related Emergency Department (ED) visits changes in the days following a local anti-Black aggravated assault.

This juxtaposition enables me to make two major contributions to the literature. First, I demonstrate that anti-Black hate crimes negatively affect health of Black Americans at multiple stages of the life course. Previously studied in utero shocks have been criticized for being "quirky" and "exotic", and their generalizability to other settings

[1] An aggravated assault is an "unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily injury" (Federal Bureau of Investigation, 2017).

[2] Violent crimes include murder and nonnegligent manslaughter, forcible rape, robbery, and aggravated assault (Federal Bureau of Investigation, 2010).

has been questioned (Almond et al., 2018). Studying the effects on adults, alongside the effects on infants, mitigates these concerns. Second, I report the adverse effects on Black individuals in a broad national context as well as in the specific context of a state with one of the highest shares of agencies reporting hate crimes (Kaplan, 2023). This alleviates concerns regarding under-reporting, which remains a frequent challenge when studying hate crimes.

In the first set of analyses, I compare birth outcomes of mothers who reside in counties reporting at least one anti-Black aggravated assault during pregnancy or after delivery. Mothers exposed to a stressor soon after delivery are thought to be the best control group for mothers exposed to the same stressor during pregnancy (Persson and Rossin-Slater, 2018; Currie et al., 2022). Among Black infants, in utero anti-Black aggravated assaults are associated with decreases in birth weight and gestational length as well as increases in the incidence of low birth weight and preterm delivery. Although these effects are small in magnitude in the baseline, they become substantial for in utero exposures to a relatively high number of anti-Black aggravated assaults.

In the second set of analyses, I report a significant increase in the number of chest pain-related Emergency Department visits among Black patients immediately following an anti-Black aggravated assault within the patients' ZIP code of residence. I report no long-term effects as the effects dissipate soon after the assault. At the same time, I provide descriptive evidence which indicates that information dissemination through local news media is a crucial factor mediating these effects. In particular, in areas with availability of daily local newspapers, the increases in the number of chest pain-related ED visits are the most immediate, suggesting that individuals learn about the assaults with minimal delay.

The effects of anti-Black aggravated assaults on health outcomes of White infants and adults are to a large extent not significant and smaller in magnitude than the effects among Black infants and adults. White individuals are less likely than Black individuals to identify with the victims of anti-Black hate crimes, and individuals not identifying with the victims are less affected by a crime than individuals strongly identifying with the victims (Powdthavee, 2005). These differences in reported effects suggest that anti-Black violence may be an important driver of the Black-White disparities in health outcomes.

I conduct a series of placebo tests to show that: (a) anti-Black aggravated assaults occurring after birth do not affect birth outcomes, (b) anti-Black aggravated assaults occurring after the visit day do not affect the number of chest pain-related ED visits, and (c) anti-Black aggravated assaults do not affect Emergency Department visits due to conditions not typically considered stress-related. I also show that anti-White aggravated assaults as well as non-hate-motivated aggravated assaults do not affect health outcomes of either Black or White mothers. This suggests a unique quality to anti-Black violence that sets it apart from non-hate-motivated violence and hate crimes against majority groups.

The paper's sections are organized as follows: Section (II) briefly summarizes the literature on stress, birth outcomes, and cardiovascular health, Section (III) describes the data used, Section (IV) reviews the empirical strategy, Section (V) discusses the results, and Section (VI) concludes.

1.2 Literature Review

In Utero Stress & Birth Outcomes. Prematurity is a leading cause of infant mortality (March of Dimes, 2023). However, health at birth is also associated with a range of health and economic outcomes later in life. Children delivered prematurely or born small are more likely to have worse adult health, lower educational attainment, and lower earnings (Black et al., 2005; Royer, 2009; Almond and Currie, 2011; Almond et al., 2018).

Worse health at birth is strongly linked to maternal exposure to stress during pregnancy (Aizer et al., 2012). Past research documents that earthquakes, bombings, rocket attacks, terrorist attacks, armed conflict, homicides, mass shootings, intimate partner violence, and family bereavement during pregnancy all carry the potential to increase the rate of preterm deliveries and low birth weights (Camacho, 2008; Torche, 2011; Mansour and Rees, 2012; Quintana-Domeque and Rodenas-Serrano, 2017; Brown, 2018; Persson and Rossin-Slater, 2018; Dursun, 2019; Lichtman-Sadot et al., 2022; Currie et al., 2022). However, even exposure to rather mild and less obvious in utero stressors, such as Super Bowl-related emotional arousal, can have a negative effect on birth outcomes (Duncan et al., 2016).

There are three primary pathways through which in utero stress can lead to preterm birth and low birth weight: hormonal, immune, and behavioral (Schetter, 2009). Corticotrophin-releasing hormone (CRH) is typically secreted by the pituitary gland in response to stress. During pregnancy, CRH is also released through the placenta and plays a critical role in coordinating maternal and fetal endorine events. Increases in CRH too early during the pregnancy (already during weeks 16 through 20) are associated with higher rates of pre-term deliviers, a phenomenon referred to as "placental clock" (McLean et al., 1995; Herrera et al., 2021). Stress is also known to supress body's natural immune responses, increasing susceptibility to infections. Mothers with urinary tract infections or with infections of more remote sites, such as gingivitis, tend to experience higher rates of pre-term deliveries and low birth weights (Webb et al., 2014). In fact, maternal infection is a leading cause of spontaneous pre-term births before the 32nd week of gestation (University of Utah Health, 2023). Finally, stress can alter behaviors around sleep, nutrition, substance use, physical activity, and healthcare utilization, all of which can strongly affect fetal health (Stults-Kolehmainen and Sinha, 2013; Kim et al., 2018).

Stress & Cardiovascular Health. Psychological stress is associated with elevated likelihood of cardiovascular morbidity in the long run but stress can cause worse cardiovascular outcomes also in the very short term. Earthquakes, terrorist attacks, and even soccer games were found to be related with increases in deaths due to adverse cardiac events on the day of initial exposure and during the days that followed (Trichopoulos et al., 1983; Meisel et al., 1991; Steinberg et al., 2004; Wilbert-Lampen et al., 2008). The onset of symptoms tends to occur very rapidly, with victims developing chest pain as early as within the first hour of exposure (Leor et al., 1996).

Stress activates the sympathetic nervous system and raises the blood levels of hormones such as epinephrine and cortisol, increasing heart rate and blood pressure. In addition, stress can induce a narrowing of blood vessels in the heart. Coronary vasoconstriction, in combination with heightened heart rate and blood pressure, leads to insufficient amount of oxygen-rich blood being supplied to the heart, causing chest pain and, in extreme cases, myocardial infraction, colloquially referred to as a heart attack (Levine, 2022)

1.3 Data

1.3.1 Hate Crime Data

Data on anti-Black hate crimes are extracted from the Federal Bureau of Investigation (FBI)'s Uniform Crime Reporting (UCR) Program. The FBI began collecting information on hate crimes in the United States in 1991, pursuant to the Hate Crime Statistics Act (Office of the Law Revision Counsel, 2021). Local agencies voluntarily submit reports of hate crime using a two-step procedure. At the first step, if the officer directly responding to a crime suspects a bias motivation, they forward the case details to a special unit at their local agency. At the second step, the special unit reviews the case file and, if the bias motivation is confirmed, submits incident information to the UCR Program (Federal Bureau of Investigation, 2022). The FBI compiles the reports submitted by local agencies and each year releases a consolidated report on hate crimes in the Unites States. For every incident, the data provides the Originating Agency Identification (ORI) code, the incident date and type (e.g., aggravated assault), as well as the bias category (e.g., anti-race) and subcategory (e.g., anti-Black). I focus on anti-Black aggravated assaults, which I define as any hate crime with a bias motivation which includes anti-Black or anti-African American and offense type which includes aggravated assault.

There are several challenges associated with using the hate crime data. First, as participation in the UCR Program is voluntary, local police agencies may choose not to contribute data to the Program (Kaplan, 2023). Figure 1.2 plots the geographic distribution of hate crimes reported in the United States from 1991 through 2019. While information on hate crimes is not available for approximately a quarter of all counties, these counties are to large extent rural and account for only 4% of the total population.[3] I exclude from my analyses all counties for which information on hate crimes is not available.

Second, local police agencies may modify their reporting behavior over time due to factors such as changes in the broader socio-political climate. In the first study, I address this challenge by limiting the sample to mothers residing in counties which report an anti-Black aggravated assault either during pregnancy or after delivery. It is less likely that reporting behavior will undergo substantial changes over relatively short periods of time. In the second study, I restrict my attention to California which is a state with one of the highest shares of reporting agencies (Kaplan, 2023), pointing to a culture which

[3] Author's calculations.

promotes hate crime reporting among victims and local agencies. Finally, I show that my results are robust to excluding the counties and ZIP codes most likely to change their reporting behavior over time (i.e., those reporting only one hate crime throughout the sample period).

Third, individuals may choose not to report hate crime victimization to local police agencies and, even if they do, police officers may not classify the reported crime as a hate crime. However, this worry is likely to be more relevant in the case of non-violent offenses, such as intimidation and vandalism. In the case of violent offenses, such as aggravated assault, individuals may be more inclined to report victimization and police officers involved in responding to the report may have more incentive to thoroughly examine potential bias motivation.

Panel A of Table 1.2 provides the summary statistics for hate crimes reported nationwide from 1991 through 2019. An average county reported nearly 86 hate crimes in total, out of which 29 hate crimes were motivated by an anti-Black bias and nearly four constituted anti-Black aggravated assaults.

1.3.2 Natality Data

Data on birth outcomes come from restricted access county-level natality files, which are provided by the National Center for Health Statistics (National Center for Health Statistics, 2021) and compiled using the information from birth certificates. The data span the universe of known births in the United States and include information on pregnancy characteristics (e.g., gestation length), mother's demographics (e.g., race, age), and infant outcomes (e.g., birth weight). Using standard classifications set by the World Health Organization, I construct four main birth outcome variables: *Birth weight*, *Low birth weight*, *Gestation length*, and *Pre-term delivery*. *Birth weight* reflects an infant's weight at birth expressed in grams; *Low Birth Weight* is an indicator variable that takes the value of one when an infant's birth weight is less than 2,500 grams (World Health Organization, 2014); *Gestation length* reflects the gestation length expressed in weeks; *Pre-term delivery* is an indicator variable that takes the value of one when the gestation length is less than 37 weeks (World Health Organization, 2018).

I concentrate on non-Hispanic Black and non-Hispanic White mothers between the ages of 15 and 49. I limit the baseline sample to singleton births as twin growth patterns differ from singleton growth patterns, and multiple pregnancies constitute only 3% of all live births (Centers for Disease Control and Prevention, 2023). I focus on viable and peri-viable births, i.e., births with gestation length longer than 22 weeks. Panel B of Table 1.2 summarizes the demographic characteristics as well as birth outcomes of Black and White mothers in the sample. Black mothers are younger, less likely to be married, and less likely to have a college degree than White mothers. Black mothers are also around twice as likely as White mothers to give birth preterm and to have infants with low birth weight.

1.3.3 Emergency Department Data

I obtained the restricted-access data on Emergency Department visits from the California Department of Health Care Access and Information (HCAI) (California Department of Health Care Access and Information, 2022). The data span the universe of face-to-face Emergency Department encounters in California from 2011 through 2019 and include information on the date of the visit, patient's demographic characteristics (race, gender, age category), patient's ZIP code of residence, and the International Classification of Diseases (ICD) codes for every diagnosis made during the visit.

I concentrate on non-Hispanic Black and non-Hispanic White patients residing in

California. Panel C of Table 1.2 summarizes the demographic characteristics for the patients in the sample. Both Black and White patients are balanced in terms of gender; however, Black patients tend to be younger than White patients. In an average ZIP code, Black patients make approximately five visits to the Emergency Department per day, including 0.263 chest pain-related visits, whereas White patients make about 13 visits to the Emergency Department per day, including 0.700 chest pain-related visits.

The sample covered by the ED data is a product of two types of selection processes. Individuals need to experience symptoms severe enough that make them seek a face-to-face encounter with a provider within the context of an Emergency Department. At the same time, the symptoms cannot severe enough to warrant hospital admission as the ED data does not include information on visits that result in same-hospital admission. Given this, I focus on ED visits related to chest pain. Chest pain is one of the primary symptoms of a myocardial infraction (National Center for Chronic Disease Prevention and Health Promotion, 2022), making patients more likely to seek immediate medical help upon experiencing chest pain. This is potentially not the case for other stress-related conditions, such as panic attacks, for which seeking care may be perceived as elective. Indeed, chest pain is the second most common reason for ED visits, after abdominal pain (Rui et al., 2013). At the same time, majority of patients presenting with chest pain in the Emergency Department are discharged without a cardiac diagnosis (Cleveland Clinic, 2022).

Chest pain-related ICD codes used to construct the main outcome variable are listed in Table A.1. I classify a visit as chest pain-related if chest pain is listed as either the principal diagnosis or other diagnosis. HCAI transitioned from using ICD-9 to using ICD-10 in 2015 but the day-of-year fixed effects included in the baseline model account for any differences in reporting chest pain due to the ICD-9 to IDC-10 transition.

1.3.4 Subsidiary Data

In addition to the three main data sources, several subsidiary data sources are also utilized. First, the Law Enforcement Agency Identifiers Crosswalk (Bureau of Justice Statistics, 2021) is leveraged to harmonize geographic identifiers across the hate crime statistics, the natality files, and the ED data. Second, the County Population Totals (United States Census, 2022) are used to construct county-level population variables. Third, the Uniform Crime Reporting (UCR) Program Data: Supplemental Homicide Reports (Kaplan, 2021) and Offenses Known and Clearances by Arrest (Kaplan, 2020) are used to obtain county-level crime levels.[4] Fourth, the Bureau of Labor Statistics data are used to extract county-level unemployment rates (U.S. Bureau of Labor Statistics, 2021). Finally, the News Desert Project data is leveraged for heterogeneity analyses splitting the sample by availability of daily local newspapers (UNC Hussman School of Journalism and Media, 2023).

1.4 Methods

1.4.1 Anti-Black Aggravated Assaults & Birth Outcomes

So as to isolate the effect of exposure to anti-Black aggravated assaults on infant health, I begin by restricting the sample to mothers residing in counties that report an anti-Black aggravated assault during pregnancy or up to ten months after birth. Mothers exposed to a stressor after delivery are the most suitable controls for mothers exposed to the same stressor during pregnancy (Persson and Rossin-Slater, 2018; Currie et al.,

[4]This data is supplemented with the Florida Supplemental Homicide Reports (Florida Department of Law Enforcement, 2021) to correct the temporal aggregation of crime reports in Florida for several years in the original data.

2022). My sample is therefore defined by the following set:

$$S = \{i : \mathbf{1}[e_c \leqslant Assault \leqslant e_b]_i = 1 \vee \mathbf{1}[e_b < Assault \leqslant e_b + 10]_i = 1\} \tag{1.1}$$

In equation (1), e_c denotes the expected month and year of conception (i.e., the month and year obtained by subtracting gestation length from the actual birth month and year) and e_b denotes the expected month and year of birth (i.e, the month and year obtained by adding nine months to the expected month and year of conception).[5] The final sample $i \in \{S\}$ includes all mothers residing in counties that report at least one anti-Black aggravated assault during or after the expected month and year of conception but before or during the expected month and year of birth. The sample also includes all mothers residing in the counties that report at least one anti-Black aggravated assault after the expected month and year of birth but up to ten months after the expected month and year of birth. The sample is defined in terms of expected, as opposed to actual, month and year of conception and birth as the actual month and year of birth can be endogenous to anti-Black hate crimes (i.e, the shorter the length of gestation, the smaller the likelihood of exposure to treatment (Currie et al., 2022)).

Using the sample $i \in \{S\}$, I estimate the following model:

$$Y_{cmy} = \alpha + \beta Assault_{cmy} + \gamma X_{cmy} + \delta_c + \theta_{my} + \epsilon_{cmy} \tag{1.2}$$

where Y_{cmy} denotes a birth outcome of mothers residing in county c, with children conceived in month m and year y, and $Assault_{cmy}$ represents the number of anti-Black aggravated assaults in county c during pregnancy that started in month m and year y. The set of covariates, X_{cmy}, includes controls for maternal and county characteristics

[5] As the restricted access natality files do not include the exact date of birth, all births are assumed to occur on the 15th day of the month.

likely affecting birth outcomes: the proportion of mothers in given age (<20, 20-24, 25-34, >34 years old) and education categories (less than high school degree, high school degree, some college degree, college degree or more), the proportion of births in given birth order categories, the proportion of male infants, the proportion of mothers married, total and Black population, unemployment rate, and the homicide rate in county c. The model is weighted using the number of births conceived in county c, in month m and year y.

County and month-by-year fixed effects are denoted by, respectively, δ_c and θ_{my}; ϵ_{cmy} is the error term, and the standard errors are clustered at the county level. Month-by-year fixed effects account for the factors that affect all infants conceived in the same month and year, such as nation-wide shifts in unemployment or inflation. County fixed effects account for the factors that affect all infants of mothers residing in the same county and that remain stable over time, such as county area.

Anti-Black aggravated assaults are parameterized as the raw count of anti-Black aggravated assault reports at a county-by-month level. Literature on in utero exposure to crime uses different parameterizations depending on the relative frequency of the crime being studied. Mass shootings have been parameterized as binary variables (Dursun, 2019), instances of fatal police violence have been parameterized as raw counts (Jahn et al., 2021), and homicides have been parameterized in terms of rates per thousand residents (Torche and Villarreal, 2014). While the raw count is my preferred parameterization for anti-Black aggravated assaults as anti-Black aggravated assaults occur more frequently than mass shootings but less frequently than homicides, the effects are robust to these alternative parameterizations.

The main coefficient of interest is β. It captures the effect of exposure to an additional anti-Black aggravated assault in utero as compared to the exposure after birth. However, it is worth noting that β does not capture the stress associated with living in a county experiencing significant anti-Black sentiment. As an anti-Black aggravated assault may be perceived as an extreme manifestation of anti-Black sentiment, the estimates reported in this work capture the effect of exposure to a more violent manifestation of anti-Black sentiment.

The effects for Black and White mothers are reported separately throughout the paper. White individuals are, by definition, not the targets of anti-Black violence. Given that individuals are more likely to be affected by the news of crime if they identify with the victim (Powdthavee, 2005), I hypothesize that the relationship between anti-Black aggravated assaults will be substantially weaker for White mothers than for Black mothers.

The identification strategy outlined above relies on an assumption that the timing of exposure to an anti-Black aggravated assault is exogenous to other factors that could also affect mother's birth outcomes. In Table 1.7, I provide evidence of the plausibility of this assumption by demonstrating that exposure to anti-Black aggravated assaults during pregnancy does not predict mother's demographic characteristics. Table 1.7 also shows that in utero exposure to anti-Black aggravated assaults has no effect on the number of births in a county. This suggests that there seems to be no evidence of compositional effects impacting the reported results.

To further verify the validity of the empirical strategy, I check whether anti-Black aggravated assaults *after* expected delivery affect mothers' birth outcomes. Intuitively, anti-Black aggravated assaults after expected delivery constitute a placebo treatment and should not affect birth outcomes, given that birth has already occurred. Figures 1.9 and 1.10 document the effects of anti-Black hate crime rates during pregnancy as well as 1-10 months and 11-20 months after delivery on birth outcomes. The anti-Black hate crime rates *after* delivery do not appear to affect birth outcomes of either Black or White mothers.

1.4.2 Anti-Black Aggravated Assaults & Emergency Department Visits

The strategy used to capture the effect of anti-Black aggravated assaults on birth outcomes is not perfectly suited for capturing the effect on chest pain-related Emergency Department visits. First, equation (2) is designed to model the intergenerational effects of anti-Black aggravated assaults at a county level rather than the very immediate effects in the days that follow an anti-Black aggravated assault within a ZIP code. Second, the Emergency Department data is more compressed temporally than the Natality Data (2011-2019 as opposed to 1991-2019) and restricted to a state with high hate crime reporting rates, which renders the sample restrictions imposed in the previous study less necessary.

Therefore, to estimate the effect of anti-Black aggravated assaults on the number of chest pain-related Emergency Department visits, I posit the following regression framework:

$$Y_{tz} = \alpha + \sum_{i=-3}^{0} \beta_{t_i} Assault_{t_i z} + \gamma X_{tz} + \delta_z + \theta_t + \epsilon_{tz} \tag{1.3}$$

where Y_{tz} denotes the number of chest pain-related ED visits on day t in ZIP code z, and AB_{tz} (for $i = -3, -2, -1, 0$) is a binary variable equal to one when at least one anti-Black aggravated assault is reported in ZIP code z either three, two, or one day before the visit day or on the visit day. The set of covariates, X_{tz}, includes available the patient- and ZIP-code level characteristics likely associated with chest pain-related ED visits: the proportion of female patients, proportion of patients over 55 years of age, the number of non-racially motivated hate crimes reported in ZIP code z up to three days before day t, and the total number of ED visits on day t in ZIP code z.

ZIP code and day-of-year fixed effects are denoted by, respectively, δ_z and θ_t; ϵ_{zt} is the error term. I cluster the standard errors at the ZIP code level. Day-of-year fixed effects account for the factors that affect all patients visiting the Emergency Department on the same day, such as holiday effects or weather conditions. ZIP code fixed effects account for the factors that affect all patients residing in the same ZIP code but remain stable over time, such distance to the nearest hospital.

The main coefficients of interest are β_0, β_{-1}, β_{-2}, and β_{-3} which correspond to the effects on the number of chest pain visits on the day of the assault, one day after the assault, two days after assault, and three days after the assault. Anti-Black aggravated assaults are parameterized as binary indicators given that, at a ZIP code-by-day level, they occur relatively infrequently. I focus on the effects up to three days after the assault as the lifespan of news stories related to violence and war tends to be limited to first three days following the triggering event (The Lifespan of News Stories, 2019), suggesting that individuals should be able to learn about the assault during this time frame.

The identification strategy outlined above relies on the assumption that the timing of exposure to an anti-Black aggravated assault is exogenous to other factors that could affect the number of chest pain-related Emergency Department visits. To verify the validity of this empirical strategy, I conduct two types of placebo tests. First, I show that anti-Black aggravated assaults reported *after* the visit day do not affect the number of chest pain-related visits (see: Figure 1.13). Second, I show that anti-Black aggravated assaults are not related to the number of ED visits due to conditions not typically thought of as stress related, such as flu (see: Table 1.10). This provides evidence indicating that trends in unobservable factors potentially impacting both anti-Black aggravated assaults and the number of chest pain ED visits do not seem to be driving the reported effects.

Analysis at a ZIP code-by-day level, although allowing for a identification of very local effects, posits important challenges. Importantly, crime data are not easily available at such a granular level. I control for hate crimes that are not racially motivated in equation

(3); however, it needs to be acknowledged that the changes in underlying crime levels can be a potential confounder.

1.5 Results

1.5.1 Anti-Black Aggravated Assaults & Birth Outcomes

Table 1.3 provides the effects of anti-Black aggravated assaults throughout pregnancy on birth outcomes of Black mothers (Panel A) and White mothers (Panel B), estimated using equation (2). Recall that equation (2) is estimated using the sample of mothers residing in counties which report at least one anti-Black aggravated assault during pregnancy as well as mothers residing in counties which report at least one anti-Black aggravated assault up to 10 months after expected delivery. In column 1, the outcome variable is average birth weight in grams; in column 2, the outcome variable is the average gestation length in weeks; in column 3, the outcome variable is the fraction of births with low birth weight; in column 4, the outcome variable is the proportion of births delivered preterm.

Across all outcome variables, the exposure to anti-Black aggravated assaults during pregnancy is associated with worse infant health among Black mothers but not among White mothers. Among Black mothers, an additional anti-Black aggravated assault during pregnancy is associated with a reduction in birth weight by 1.280 grams and an increase in low birth weights by 0.5% relative to the mean. An extra anti-Black aggravated assault during pregnancy is also associated with a 0.005 week shorter gestation length and a 0.5% increase in pre-term deliveries, relative to the mean.

The magnitude of the effects among White mothers is substantially smaller than the magnitude of effects among Black mothers; the effects are also to a large extent insignificant. As hypothesized, White individuals seem less affected by anti-Black aggravated assaults than Black individuals, and the effects of anti-Black aggravated assaults are thus are mostly diluted. Interestingly, in column 3 of Table 1.3, an additional anti-Black aggravated assault is actually associated with a slight decline in low birth weights among White mothers. In utero exposure to stress can occasionally improve birth outcomes through the channel of increased utilization of prenatal care (Torche and Villarreal, 2014). Table 1.4 shows how exposure to in utero anti-Black aggravated assaults affects prenatal care utilization among Black and White mothers. Among White (but not Black mothers), an additional anti-Black aggravated assault during the first trimester is associated with an increase in prenatal visits. At the same time, among Black mothers, an additional anti-Black aggravated assault during the first trimester of pregnancy is linked with lower rates of starting prenatal care in the first trimester. This suggests that Black mothers may choose to delay the onset of prenatal care in response to a local anti-Black aggravated assault early during the pregnancy.

The effects of anti-Black aggravated assaults on infant health are non-linear. Figure 1.3 shows the effects of in utero exposure to 1 or 2, 3 or 4, 5 or 6, 7 or 8, 9 or 10, and 11, 12, or 13 anti-Black aggravated assaults. Across the four main outcome variables, the higher the exposure, the higher the magnitude of reported effects. Most notably, exposure to over 11 anti-Black aggravated assaults in utero is related to an over 70 gram decrease in birth weight among Black mothers. The same trend is not present among White mothers (see: Figure 1.4), i.e., White mothers do not appear to be affected by exposures to anti-Black aggravated assaults regardless of levels of exposure.

Past scholarship on in utero stressors points out that the timing of exposure during pregnancy matters. Notably, Dursun (2019) reports the strongest effects for exposure to a mass shooting in the second trimester of pregnancy, which is consistent with the phenomenon of a *placental clock*, through which exposure to stress early during the pregnancy can "program" the fetus for pre-term delivery (McLean et al., 1995; Herrera et al., 2021). My findings echo this very closely. Table 1.5 shows the effects of anti-Black aggravated assaults reported during the first, second, and third trimester of pregnancy. Similar to Dursun (2019), I find that, among Black mothers, the effects on birth outcomes are largely concentrated in the first and second trimesters.

Figures 1.5 and 1.6 contrast the effects of exposure to anti-Black aggravated assaults to the effects of exposure to other crimes: anti-White aggravated assaults (i.e., aggravated assaults motivated by an anti-White bias), all hate motivated aggravated assaults (i.e., aggravated assaults regardless of bias motivation), all anti-Black hate crimes (i.e., anti-Black hate crimes regardless of offense type), and non-hate motivated aggravated assaults (i.e., aggravated assaults not classified as hate motivated). The effects among Black mothers are insignificant except for exposures to anti-Black aggravated assaults, suggesting that exposures to *hate-motivated* crimes targeting one's *in-group* are the most salient to individuals. Simultaneously, the finding at all anti-Black hate crimes (regardless of offense type) are not significantly related to the birth outcomes of Black mothers also indicates that hate crimes at large, in contrast to the more violent hate crimes, may not receive the media attention required to affect broader communities. As before, the effects among White mothers are largely insignificant throughout, even in case of exposures to anti-White hate crimes.

The main results persist across several robustness checks. Figures 1.7 and 1.8 show the effects on birth outcomes of Black and White mothers when the baseline specification and sample are modified. First, I include state-by-year fixed effects alongside the county and month-of-year fixed effects to account for state-specific factors that change over time and that could affect birth outcomes. Second, I exclude all controls for maternal demographics and county-level characteristics. Third, I exclude all counties reporting only one hate crime throughout the sample period and therefore likely changing their reporting behavior over time. Among both Black and White mothers, the estimates remain stable in magnitude across all these specifications. The reported effects are also robust to changing the parameterization of the assault variable from raw counts to a natural logarithm, binary indicator, and a rate per 10,000 residents (see: Table 1.6).

Lastly, I conduct a placebo test to provide evidence that the trends in unobservable variables do not seem to be driving the reported results on birth outcomes. I check whether anti-Black aggravated assaults *before* the expected conception month and year and *after* the expected delivery month and year affect mothers' birth outcomes. Intuitively, anti-Black aggravated assaults following the expected delivery month and year should not affect birth outcomes. While anti-Black aggravated assaults before the expected conception month and year may have an effect on birth outcomes, this effect would likely be smaller in magnitude that the effects of exposures during pregnancy. Figures 1.9 and 1.10 document the placebo test estimating the effects of anti-Black aggravated assaults 1-10 months before conception, during pregnancy, 1-10 months after delivery, and 11-20 months after delivery. Anti-Black aggravated assaults *before* conception and *after* delivery do not appear to affect birth outcomes of either Black or White mothers.

1.5.2 Anti-Black Aggravated Assaults & Emergency Department Visits

Building on the intergenerational effects on birth outcomes, let us now move on to the more immediate results on chest pain-related Emergency Department visits. Table 1.8 shows the effects of anti-Black aggravated assaults on the number chest pain-related Emergency Department visits on the day of the assault as well as one, two, and three days after the assault, estimated using equation (3). The model is reported separately for Black patients (Column 1) and White patients (Column 2). I find evidence of substantial immediate effects of anti-Black aggravated assaults among Black patients. In Column A, an anti-Black aggravated assault in the patient's ZIP code of residence is associated with 0.114 more chest-pain related ED visits on the day of the assault (around a 43% change relative to the mean). The immediacy of reported effects is consistent with existing literature on short-term effects of exposures to acute stressors, such as earthquakes (Leor et al., 1996).

Similarly to the effects on infant health, I report no significant relationship between anti-Black aggravated assault reports and chest pain-related ED visits among White patients. Moreover, White individuals do not appear to be affected by exposures to anti-White aggravated assaults (see: Figure 1.11), also echoing results from the study discussed above.

In Table 1.9, I split the sample by the number of local daily newspapers using the News Project Data. I provide suggestive evidence that availability of local news media is a crucial factor mediating the reported effects on chest pain-related Emergency Department visits. Among Black patients residing in areas with no availability of daily local newspapers, I find that the increases in the number of chest pain-related ED visits occur with a delay. At the same time, among Black patients residing in areas with high availability of daily local newspapers, the increases in the number of chest pain-related ED visits occur immediately. This provides suggestive evidence that individuals in these areas are able to learn about the assaults with minimal delay.

The immediate effects of anti-Black aggravated assaults on the number of chest pain-related ED visits among Black individuals persist across a range of robustness checks. Figure 1.12 provides the results when ZIP codes reporting only one hate crime throughout the sample period are excluded, when the day-of-year fixed effects are replaced with month-of-year and weekday fixed effects, and when patient demographic controls and ZIP code level controls are excluded. The effects among Black patients remain significant and stable in terms of magnitude across all these specifications, while the effects among White patients continue to be insignificant.

Lastly, I conduct two sets of placebo tests to further verify the validity of the results. First, I provide evidence that anti-Black aggravated assaults reported *after* the visit day do not affect the number of chest pain-related ED visits (see: Figure 1.13). Second, I check whether anti-Black aggravated assaults affect the number of flu-related Emergency Department visits (see: Table 1.10). Flu is not usually considered to be stress related and therefore one would not expect to see any effects of anti-Black aggravated assaults on the number of flu-related Emergency Department visits. Indeed, I find no evidence of anti-Black aggravated assaults resulting in increases in the volume of flu-related ED visits among either Black and White patients.

1.6 Discussion

The number of hate crimes reported in the United States has been growing in recent years. While we are rapidly increasing our understanding of what causes hate crimes (Muller and Schwarz, 2023; Cao et al., 2023), we still have relatively limited empirical evidence on how hate crimes affect outcomes of the targeted communities. To this end, this paper studies how local anti-Black aggravated assaults affect the health outcomes of Black infants and adults. Using restricted-use nation-wide natality data, I show that anti-Black aggravated assaults during pregnancy in the mother's county of residence are associated with decreases in birth weight and gestation length as well as increases in the incidence of low birth weights and pre-term deliveries among Black infants. I complement these results by leveraging the restricted-access Emergency Department (ED) data from California and providing evidence of increases in chest pain-related ED visits among Black patients following an anti-Black aggravated assault in their ZIP code of residence.

I find little evidence of exposures to local anti-Black aggravated assaults adversely impacting the health outcomes of White infants and adults. This underscores the salience of hate crimes specifically for the victimized groups. Anti-Black hate crimes remain the most frequently reported type of hate crime in the Untied States; however, given that hate crimes motivated by biases other than anti-Black have also been increasing, more future work should aim to document how various kinds of hate motivated violence impact the targeted communities.

Figures

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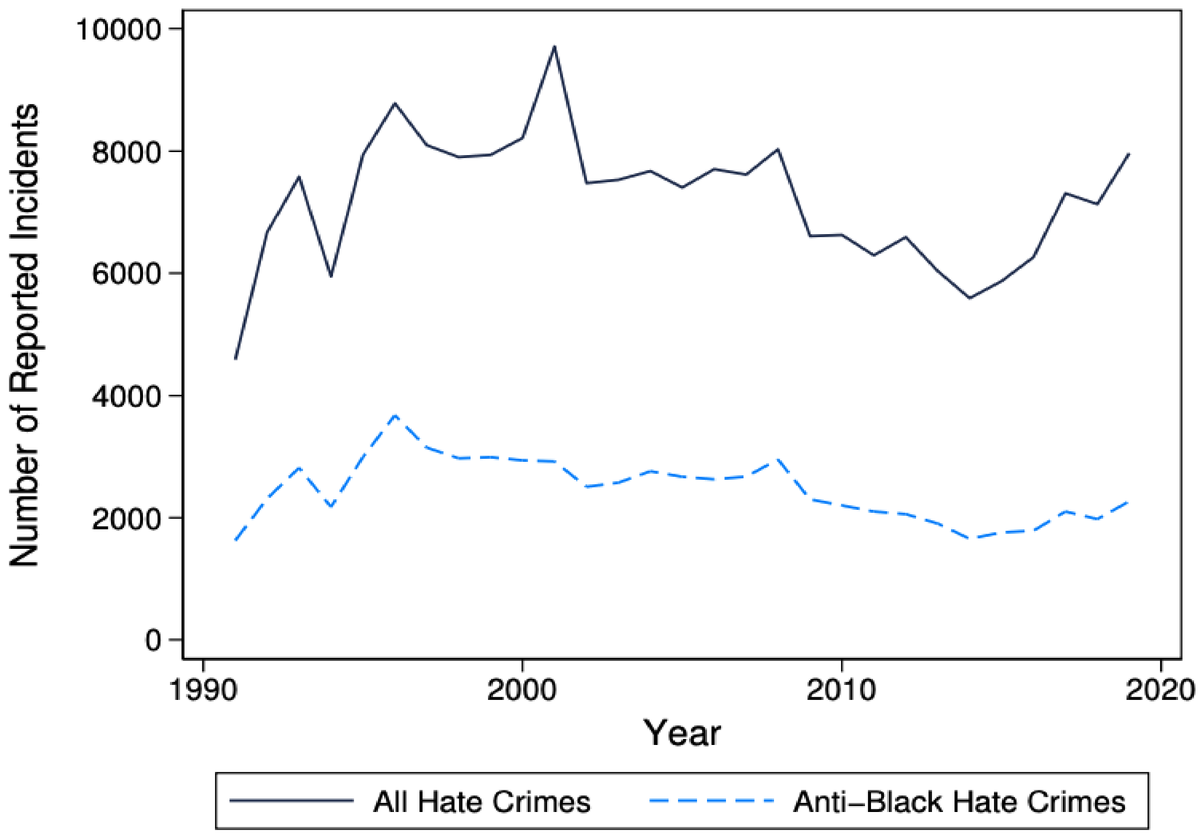


Figure 1.1: Hate Crimes Reported in the United States Per Year, 1991-2019
Note: "All hate crimes" include all hate crimes reported in the United States, regardless of bias motivation. "Anti-Black hate crimes" include all hate crimes motivated by a bias against Black or African American individuals. Incidents reported outside of the contiguous United States are excluded.

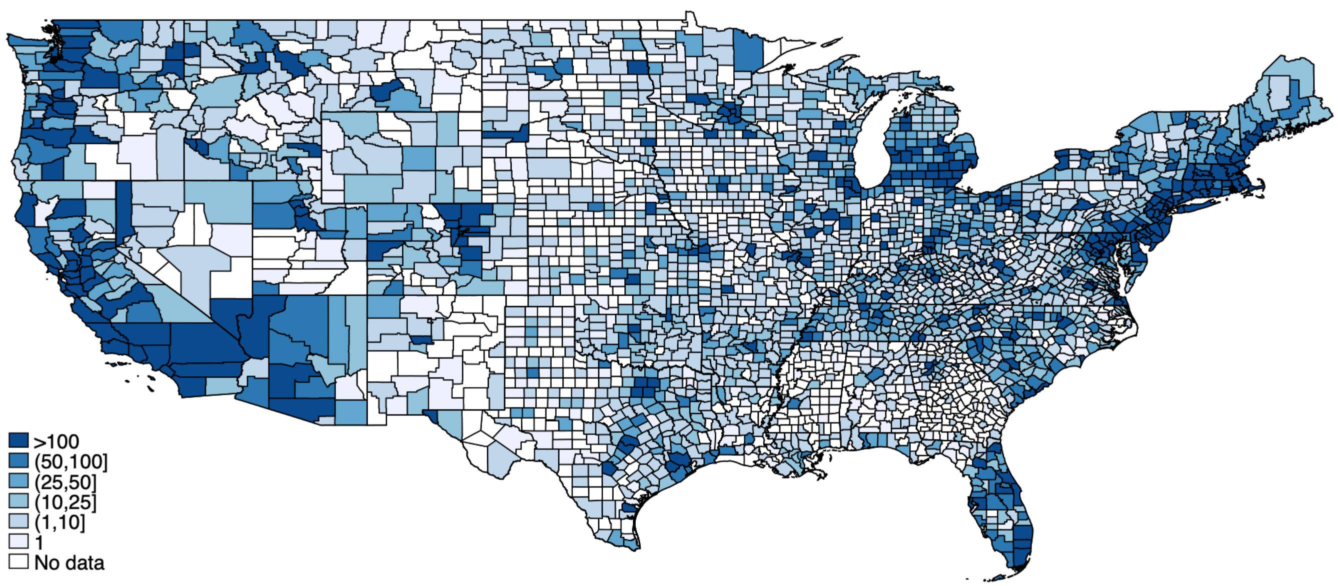
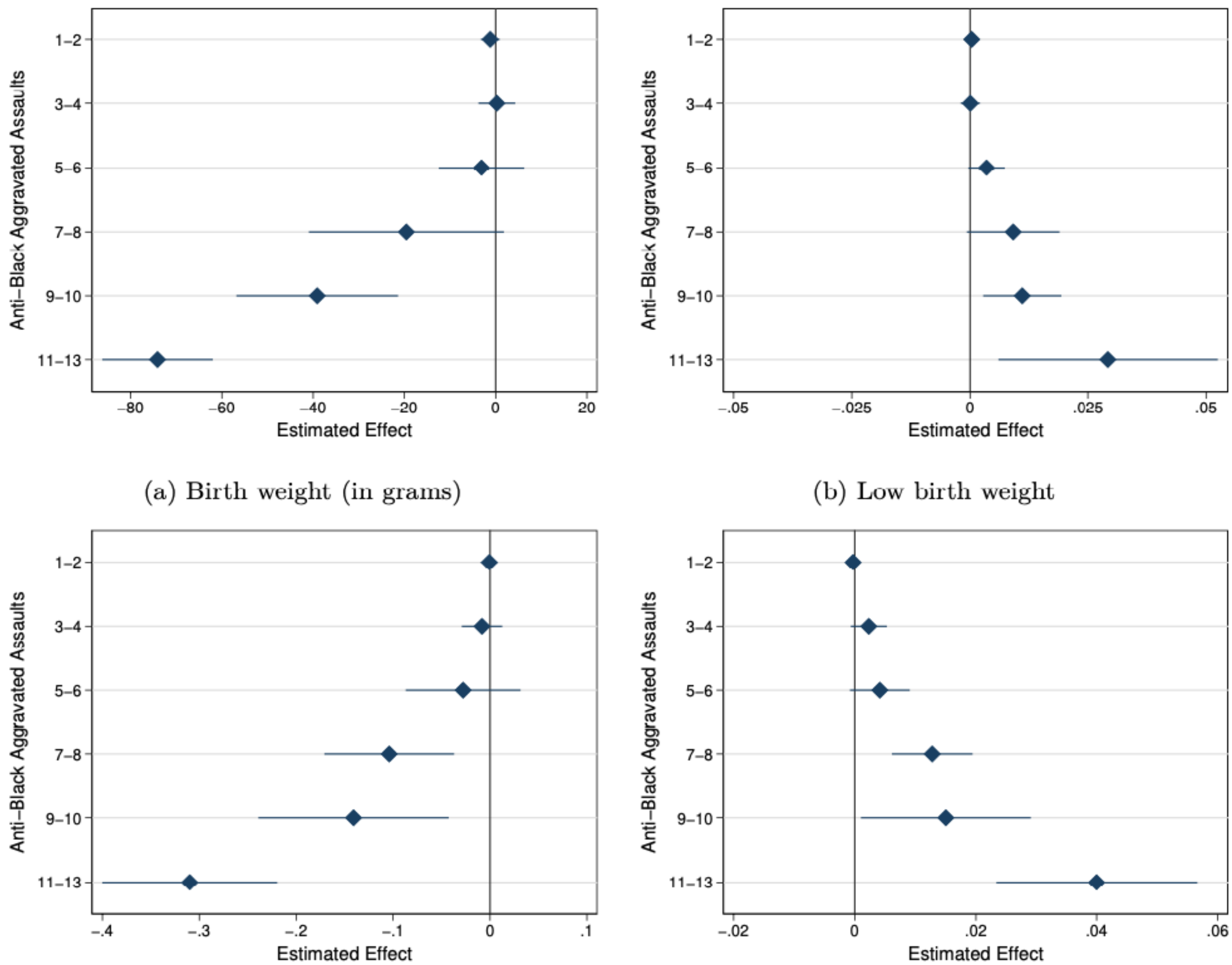


Figure 1.2: Geographic Variation in the Total Number of Hate Crimes Reported by Counties, 1991-2019
Note: “Total Number of Hate Crimes” refers to the sum of all hate crimes reported by agencies within a county from 1991 through 2019. Incidents reported outside of the contiguous United States are excluded.



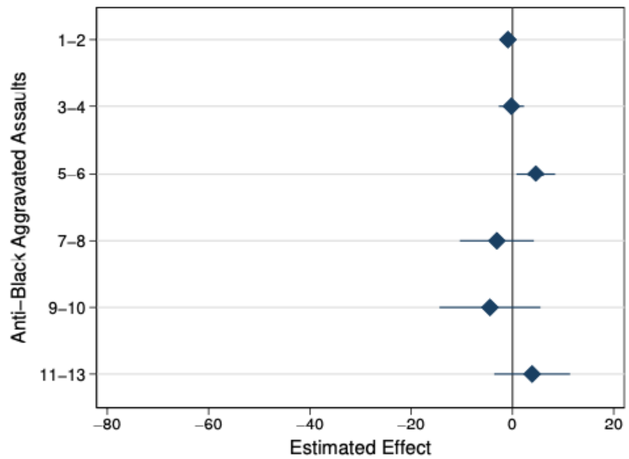
(a) Birth weight (in grams)

(b) Low birth weight

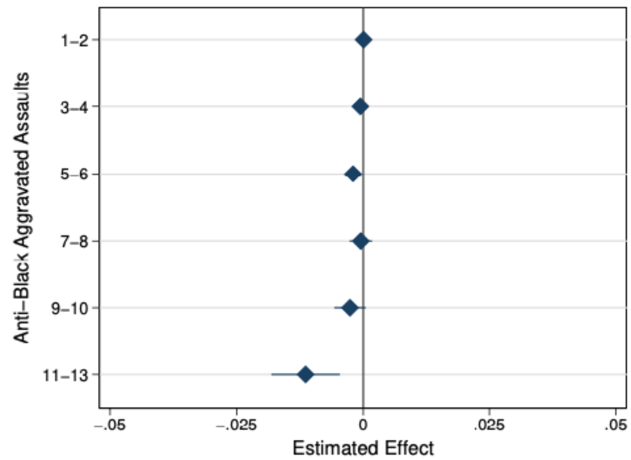
(c) Gestation length (in weeks)

(d) Pre-term delivery

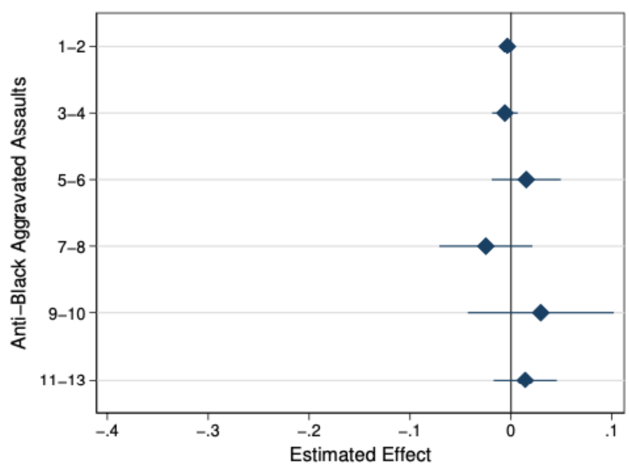
Figure 1.3: Black Mothers–Non-Linear Effects on Birth Outcomes
Note: Each diamond depicts a coefficient estimate and each whisker depicts the estimated 95% confidence interval. “1-2” is a binary variable equal to one when a mother is exposed to 1 or 2 anti-Black aggravated assaults during pregnancy. “3-4” is a binary variable equal to one when a mother is exposed to 3 or 4 anti-Black aggravated assaults during pregnancy. “5-6”, “7-8”, “9-10”, and “11-13” are binary variables which are defined analogously.



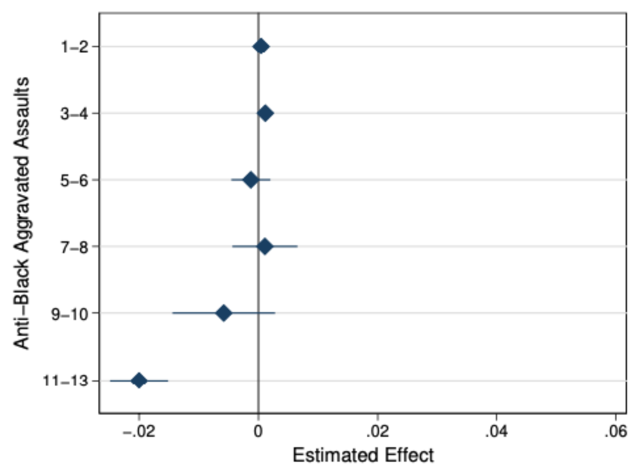
(a) Birth weight (in grams)



(b) Low birth weight



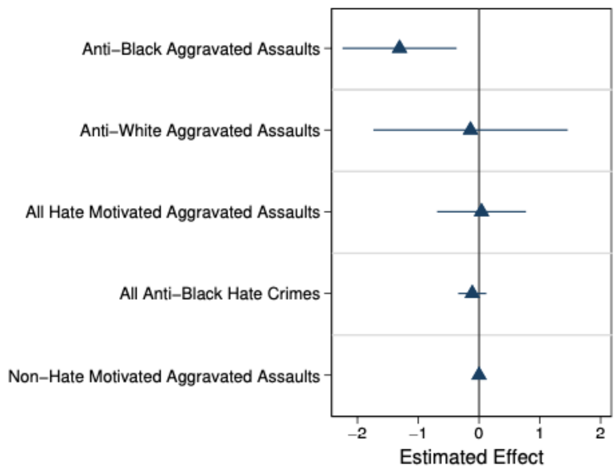
(c) Gestation length (in weeks)



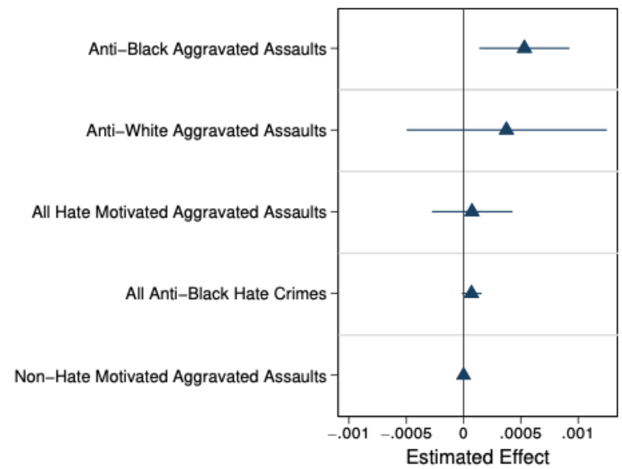
(d) Pre-term delivery

Figure 1.4: White Mothers–Non-Linear Effects on Birth Outcomes

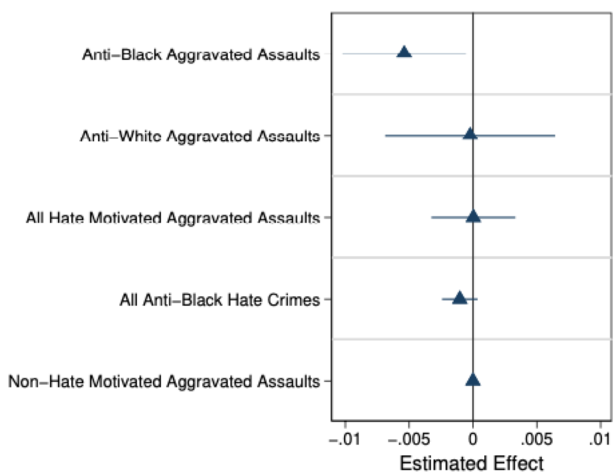
Note: Each diamond depicts a coefficient estimate and each whisker depicts the estimated 95% confidence interval. “1-2” is a binary variable equal to one when a mother is exposed to 1 or 2 anti-Black aggravated assaults during pregnancy. “3-4” is a binary variable equal to one when a mother is exposed to 3 or 4 anti-Black aggravated assaults during pregnancy. “5-6”, “7-8”, “9-10”, and “11-13” are binary variables which are defined analogously.



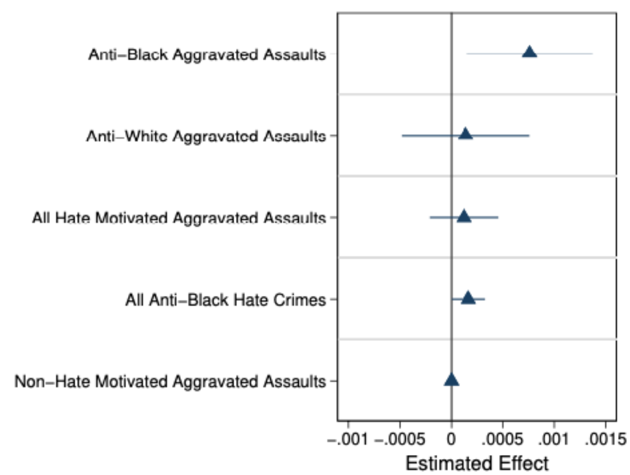
(a) Birth weight (in grams)



(b) Low birth weight



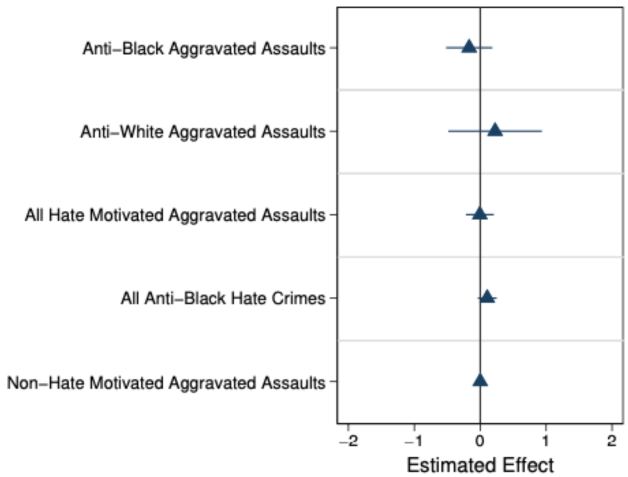
(c) Gestation length (in weeks)



(d) Pre-term delivery

Figure 1.5: Black Mothers–Effects of Other Crimes on Birth Outcomes

Note: Each triangle depicts a coefficient estimate from a separate regression model; each whisker depicts the estimated 95% confidence interval. "Anti-Black Aggravated Assaults" refer to aggravated assaults motivated by an anti-Black bias. "Anti-White Aggravated Assaults" refer to aggravated assaults motivated by an anti-White bias. "All Hate Motivated Aggravated Assaults" refer to hate-motivated aggravated assaults regardless of bias motivation. "All Anti-Black Hate Crimes " refer to all anti-Black hate crimes regardless of offense type. "Non-Hate Motivated Aggravated Assaults" refer to all aggravated assaults not classified as hate motivated.



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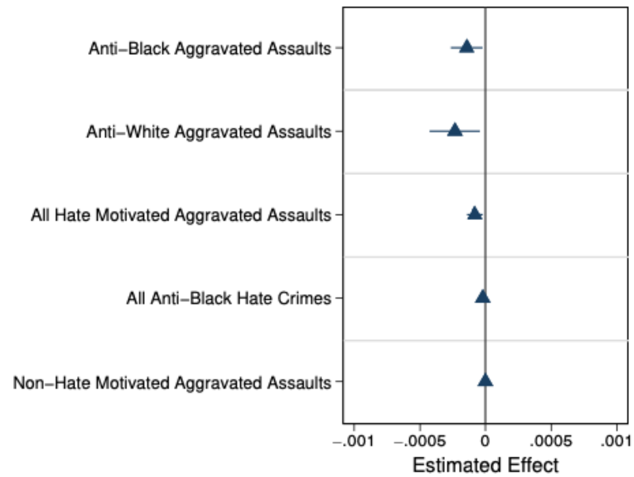
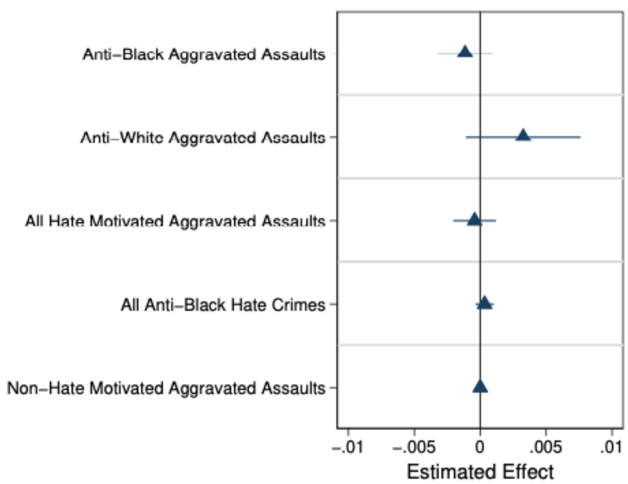
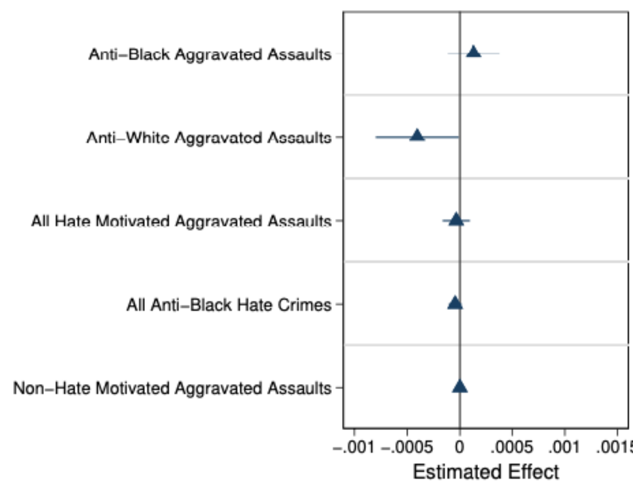


(b) Low birth weight



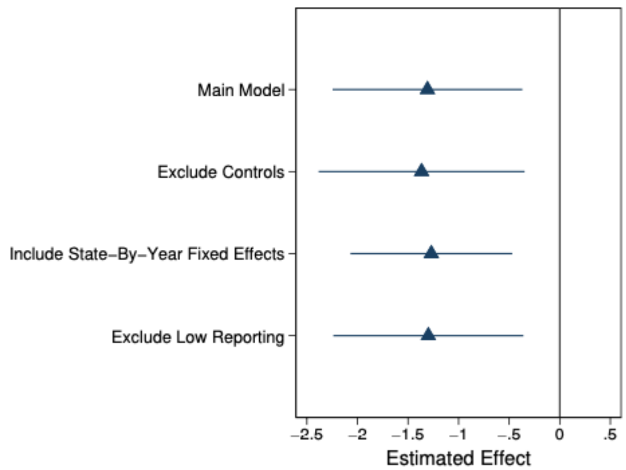
(c) Gestation length (in weeks)



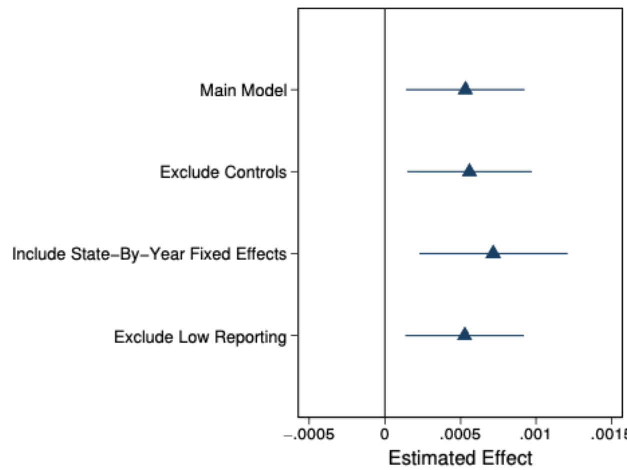
(d) Pre-term delivery

Figure 1.6: White Mothers–Effects of Other Crimes on Birth Outcomes

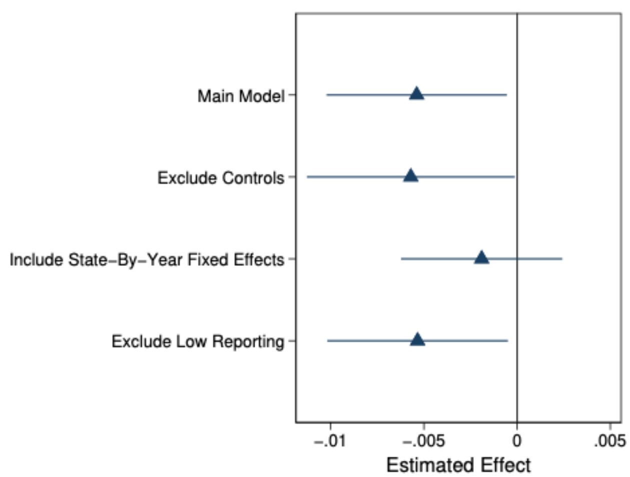
Note: Each triangle depicts a coefficient estimate from a separate regression model; each whisker depicts the estimated 95% confidence interval. "Anti-Black Aggravated Assaults" refer to aggravated assaults motivated by an anti-Black bias. "Anti-White Aggravated Assaults" refer to aggravated assaults motivated by an anti-White bias. "All Hate Motivated Aggravated Assaults" refer to hate-motivated aggravated assaults regardless of bias motivation. "All Anti-Black Hate Crimes " refer to all anti-Black hate crimes regardless of offense type. "Non-Hate Motivated Aggravated Assaults" refer to all aggravated assaults not classified as hate motivated.



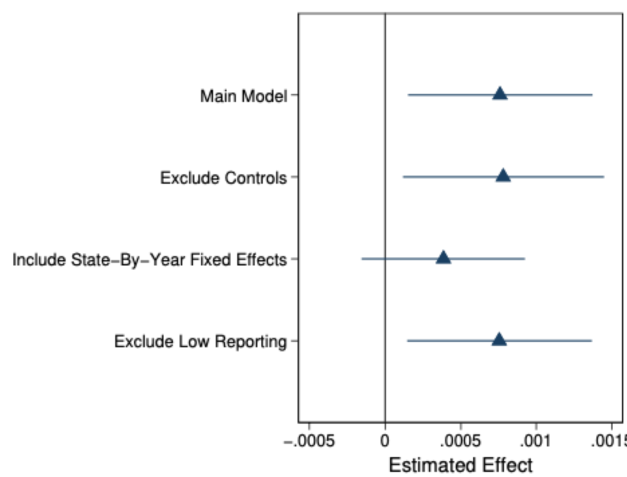
(a) Birth weight (in grams).



(b) Low birth weight.

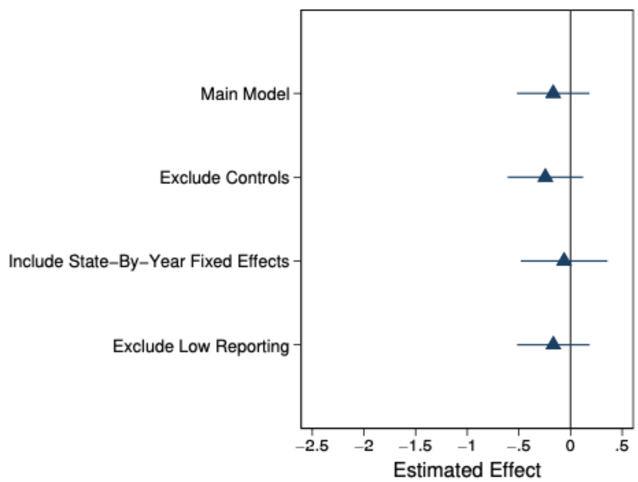


(c) Gestation length (in weeks).

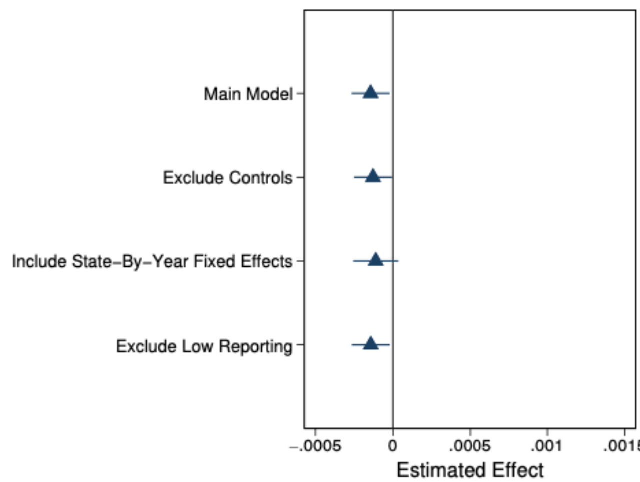


(d) Pre-term delivery.

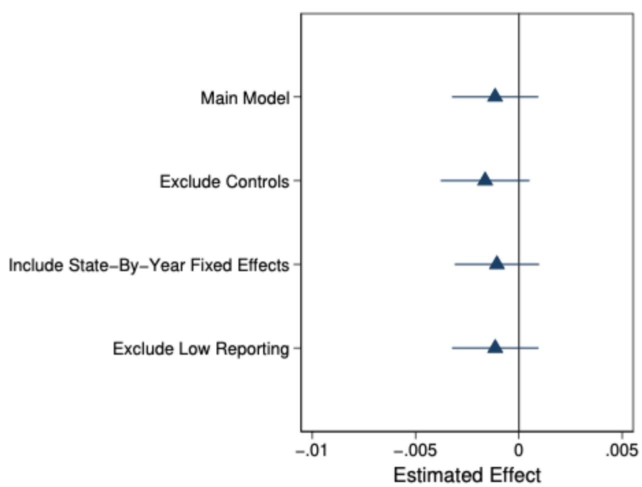
Figure 1.7: Black Mothers–Robustness to Changes in Model and Sample Specifications
Note: Each triangle depicts a coefficient estimate from a separate regression model; each whisker depicts the estimated 95% confidence interval. "Main Model" refers to the effect of in utero exposure to anti-Black aggravated assaults on birth outcomes estimated using equation (2). "Exclude Controls" refers to the effect estimated using equation (2) but excluding controls for maternal demographics and county-level characteristics. "Include State-By-Year-Fixed Effects" refers to the effect estimated using equation (2) but including state-by-year fixed effects alongside the county and month-by-year fixed effect. "Exclude Low Reporting" refers to the effect estimated using equation (2) but excluding the counties which report only one hate crime throughout the sample period.



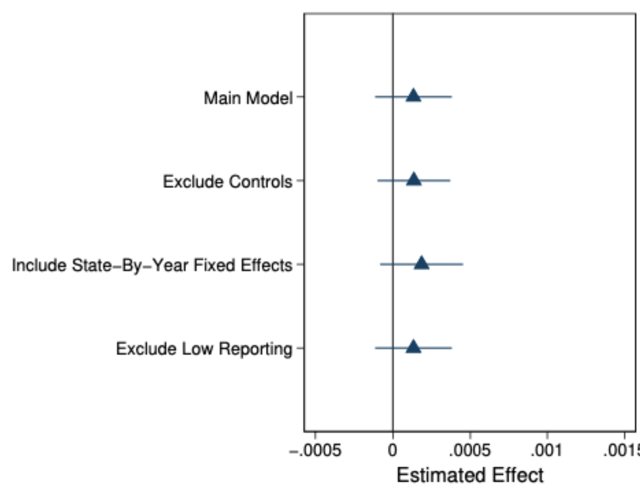
(a) Birth weight (in grams).



(b) Low birth weight.

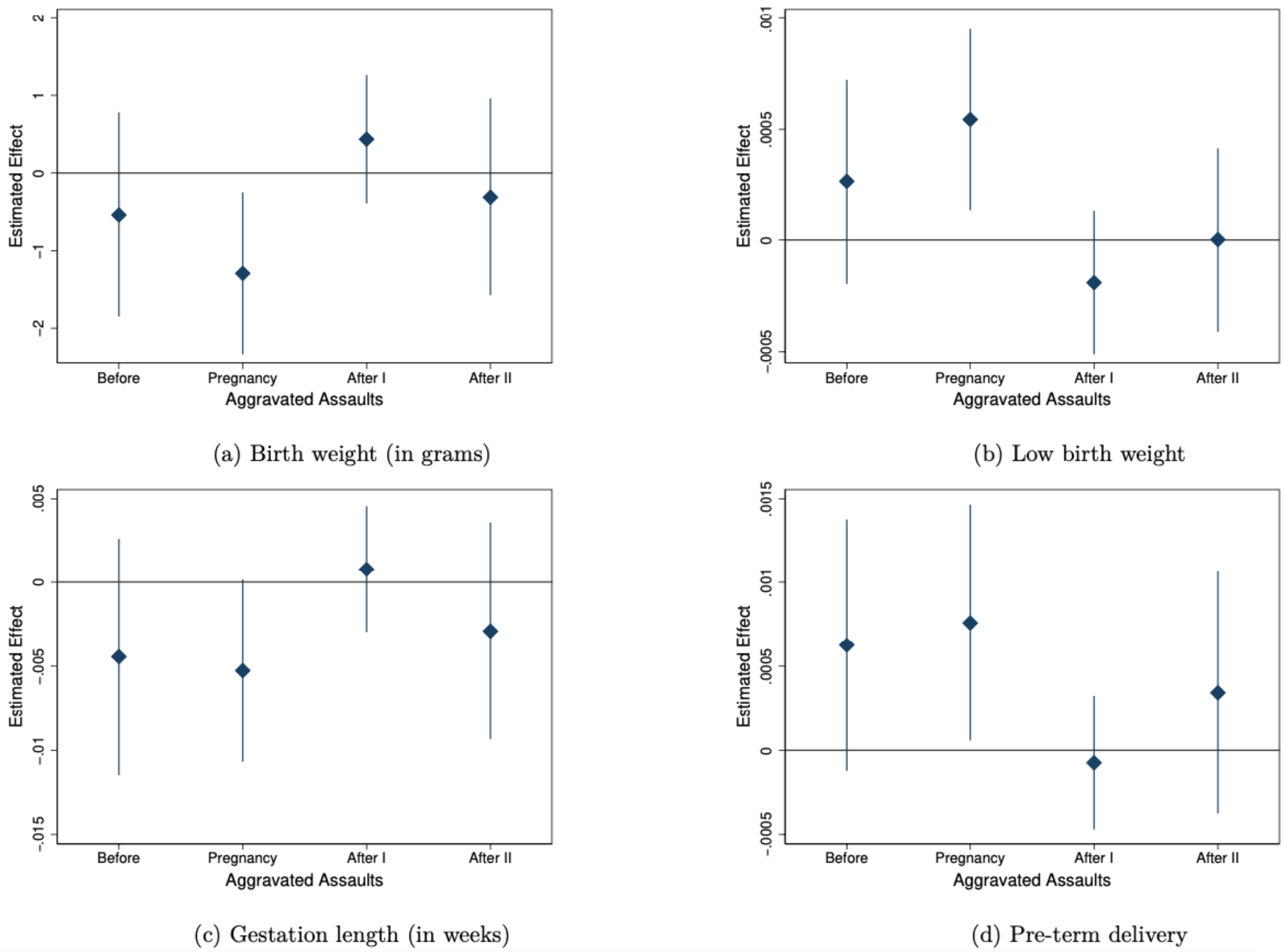


(c) Gestation length (in weeks).



(d) Pre-term delivery.

Figure 1.8: White Mothers–Robustness to Changes in Model and Sample Specifications
Note: Each triangle depicts a coefficient estimate from a separate regression model; each whisker depicts the estimated 95% confidence interval. “Main Model” refers to the effect of in utero exposure to anti-Black aggravated assaults on birth outcomes estimated using equation (2). “Exclude Controls” refers to the effect estimated using equation (2) but excluding controls for maternal demographics and county-level characteristics. “Include State-By-Year-Fixed Effects” refers to the effect estimated using equation (2) but including state-by-year fixed effects alongside the county and month-by-year fixed effect. “Exclude Low Reporting” refers to the effect estimated using equation (2) but excluding the counties which report only one hate crime throughout the sample period.



(a) Birth weight (in grams)

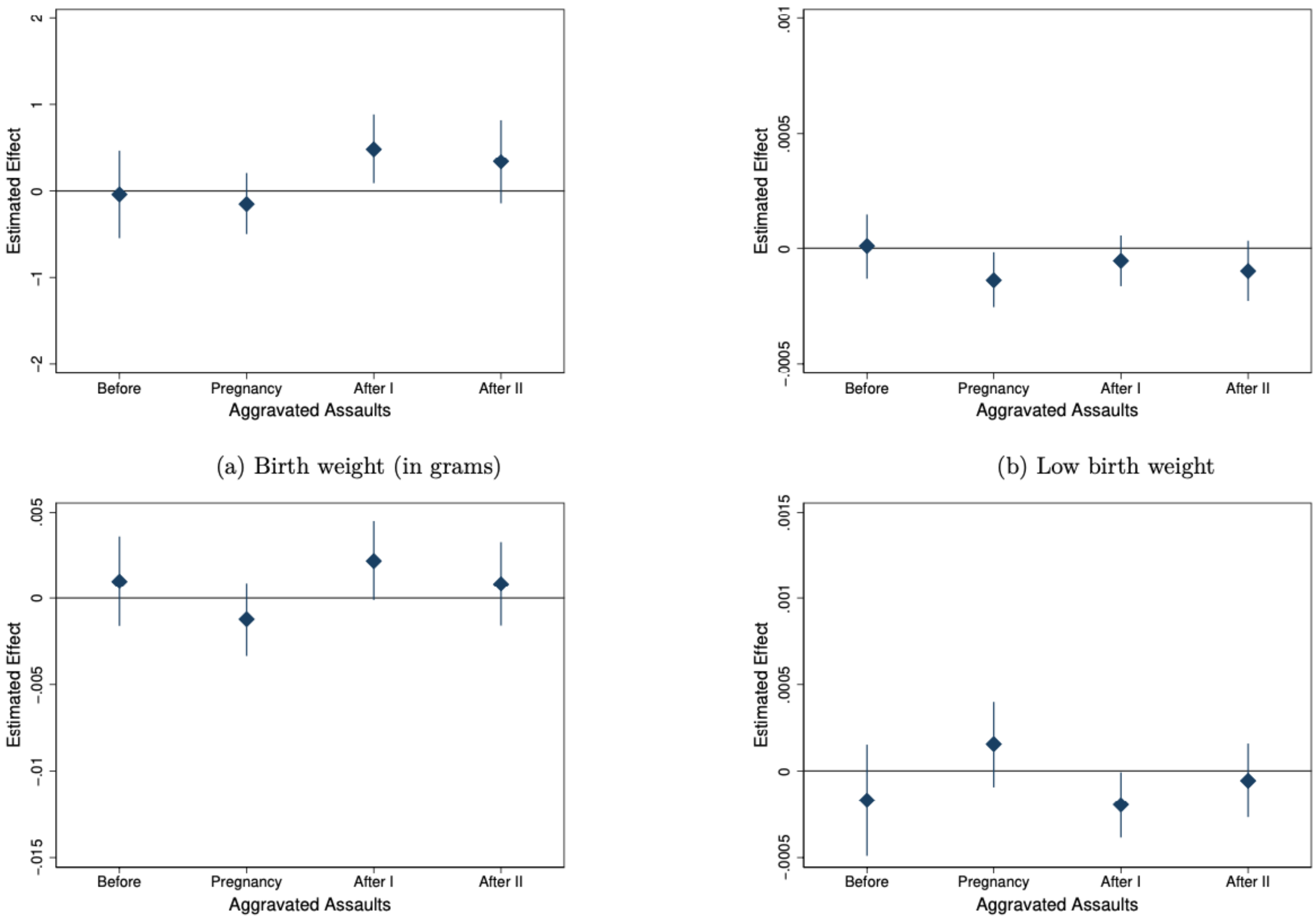
(b) Low birth weight

(c) Gestation length (in weeks)

(d) Pre-term delivery

Figure 1.9: Black Mothers–Placebo Effects of Anti-Black Aggravated Assaults Reported Before Conception and After Birth on Birth Outcomes

Note: Each diamond depicts a coefficient estimate and each whisker depicts the estimated 95% confidence interval. "Before" refers to the effect of exposure to anti-Black aggravated assaults up to 10 months prior to expected conception. "Pregnancy" refers to the effect of exposure to anti-Black aggravated assaults during pregnancy. "After I" refers to the effect of exposure to anti-Black aggravated assaults from 1 up to 10 months after to expected birth. "After II" refers to the effect of exposure to anti-Black aggravated assaults from 11 up to 20 months after expected birth.



(a) Birth weight (in grams)

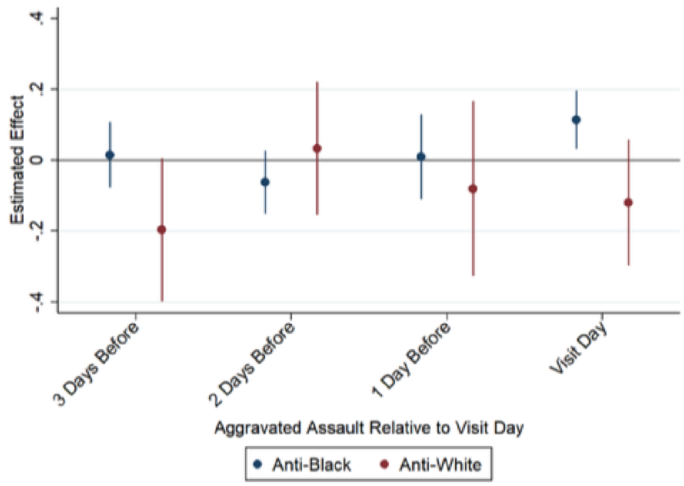
(b) Low birth weight

(c) Gestation length (in weeks)

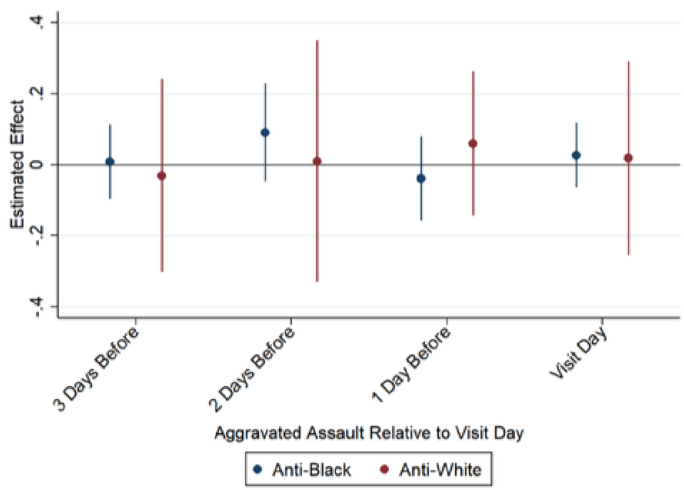
(d) Pre-term delivery

Figure 1.10: White Mothers–Placebo Effects of Anti-Black Aggravated Assaults Reported Before Conception and After Birth on Birth Outcomes

Note: Each diamond depicts a coefficient estimate and each whisker depicts the estimated 95% confidence interval. "Before" refers to the effect of exposure to anti-Black aggravated assaults up to 10 months prior to expected conception. "Pregnancy" refers to the effect of exposure to anti-Black aggravated assaults during pregnancy. "After I" refers to the effect of exposure to anti-Black aggravated assaults from 1 up to 10 months after to expected birth. "After II" refers to the effect of exposure to anti-Black aggravated assaults from 11 up to 20 months after expected birth.



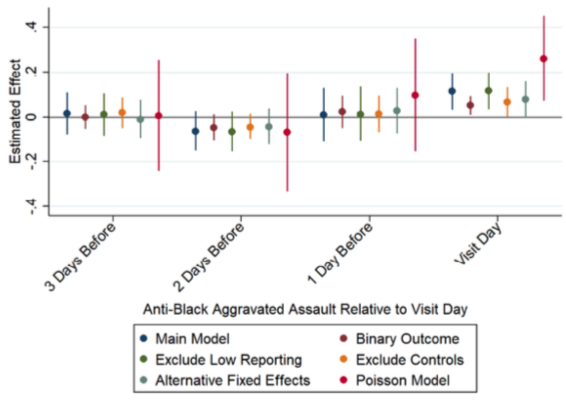
(a) Black Patients.



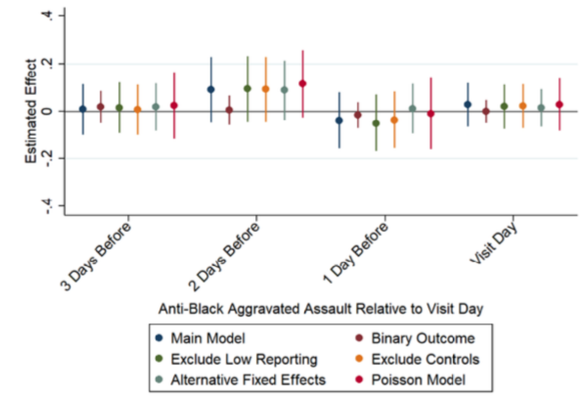
(b) White Patients.

Figure 1.11: Effects of Anti-Black and Anti-White Aggravated Assaults on the Number of Chest Pain Emergency Department Visits Among Black and White Patients

Note: Each circle depicts a coefficient and each whisker depicts the estimated 95% confidence interval. "3 Days Before", "2 Days Before", and "1 Day Before" refer to the effect of aggravated assaults reported three, two, and one day before the visit day. "Visit Day" refers to the effect of aggravated assaults reported on the visit day. "Anti-Black" refers to anti-Black aggravated assaults reported in the patient's ZIP code of residence; "Anti-White" refers to anti-White aggravated assaults reported in the patient's ZIP code of residence.



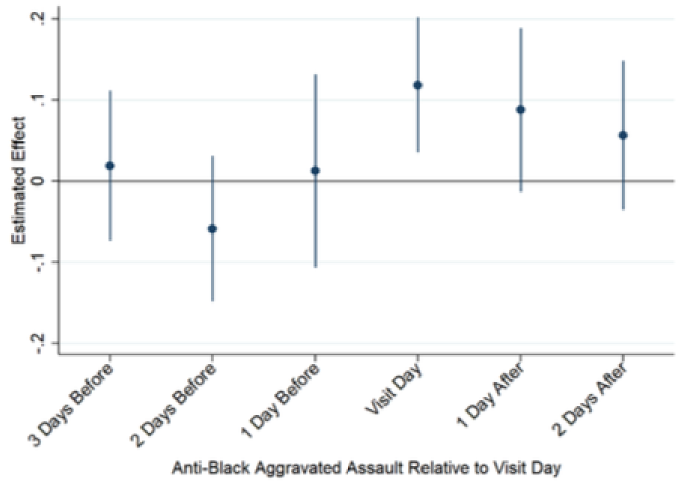
(a) Black Patients.



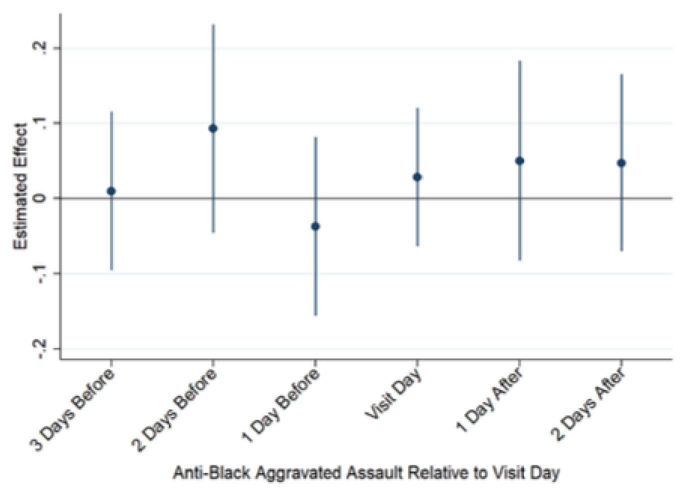
(b) White Patients.

Figure 1.12: Effects of Anti-Black Aggravated Assaults Reported Three, Two, and One Day Before Visit Day or on the Visit Day on the Number of Chest Pain Emergency Department Visits–Robustness to Model Specifications

Note: Each circle depicts a coefficient and each whisker depicts the estimated 95% confidence interval. "Main Model" refers to the baseline model estimated using equation (4). "Binary Outcome" refers to the baseline model estimated using equation (4) when the outcome variable is a binary indicator equal to one when at least one chest pain-related ED visit is made by patients in ZIP code z on day t. "Exclude Low Reporting" refers to the model estimated using equation (4) and excluding the ZIP codes which report only one hate crime throughout the sample period. "Exclude Controls" refers to the model estimated using equation (4) and excluding the patient demographic controls. "Alternative Fixed Effects" refers to the model estimated using equation (4) but including year, month, and weekday fixed effects instead of day-of-year fixed effects. "Poisson Model" refers to a Poisson model estimated using equation (4) and including year, month, and weekday fixed effects instead of day-of-year fixed effects. "3 Days Before", "2 Days Before", and "1 Day Before" refer to the effect of aggravated assaults reported three, two, and one day before the visit day. "Visit Day" refers to the effect of anti-Black aggravated assaults on the day of the visit.



(a) Black Patients.



(b) White Patients.

Figure 1.13: Placebo Effects of Anti-Black Aggravated Assaults Reported After the Visit Day on the Number of Chest Pain Emergency Department Visits Among Black and White Patients

Note: Each circle depicts a coefficient and each whisker depicts the estimated 95% confidence interval. “3 Days Before”, “2 Days Before”, and “1 Day Before” refer to the effect of anti-Black aggravated assaults reported three, two, and one day before the visit day. “Visit Day” refers to the effect of anti-Black aggravated assaults reported on the visit day. “2 Days After”, and “1 Day After” refer to the effect of anti-Black aggravated assaults reported two and one day after the visit day.

Tables

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Table 1.1: Comparison of the Two Studies Provided in This Paper

	Study 1	**Study 2**
Outcome	Infant health at birth	Adult chest pain
Data	Natality Records	Emergency Department Data
Data provider	National Center for Health Statistics	California Department of Health Care Access and Information
Effects	Intergenerational	Immediate
Sample	Nation-wide, 1991-2019	California, 2011-2019
Treatment level	County-by-month	ZIP code-by-day

Table 1.2: Summary Statistics

Panel A: Hate Crimes	Mean		St. Dev.	
All hate crimes	85.940		448.857	
Anti-Black hate crimes	29.358		134.888	
Anti-Black aggravated assaults	3.532		24.252	
	Black Mothers		*White Mothers*	
Panel B: Natality Data	Mean	St. Dev.	Mean	St. Dev.
Birth weight (grams)	3,130.446	88.596	3,398.254	69.298
Low birth weight	0.112	0.039	0.052	0.038
Gestation length (weeks)	38.378	0.403	38.997	0.290
Preterm delivery	0.153	0.050	0.083	0.026
Mother's age	25.760	3.106	27.912	2.074
Married	0.341	0.246	0.718	0.137
Less than high school degree	0.205	0.197	0.109	0.083
College degree or more	0.128	0.167	0.310	0.178
First child	0.382	0.215	0.421	0.086
Male child	0.509	0.217	0.513	0.076
Births per month	71.286	143.826	200.036	226.903
	Black Patients		*White Patients*	
Panel C: Emergency Department Data	Mean	St. Dev.	Mean	St. Dev.
Chest pain visits per day	0.263	0.632	0.700	1.022
All visits per day	4.504	6.654	12.720	10.365
Female	0.502	0.337	0.485	0.238
Over 55 years old	0.247	0.288	0.333	0.220

Note: Hate crimes reported as totals at a county level (1991-2019). Natality Data reported at a county-by-month level and available for the entire contiguous United States (1991-2019). Emergency Department Data reported at a ZIP code-by-day level and restricted to California (2011-2019).

Table 1.3: Effect of Anti-Black Aggravated Assaults During Pregnancy on Birth Outcomes

	Birth weight (grams)	**Gestation length (weeks)**	**Low birth weight**	**Pre-term delivery**
	(1)	(2)	(3)	(4)
			Coefficient × 100	
Panel A: Black Mothers				
Anti-Black aggravated assaults	-1.280***	-0.005**	0.052***	0.076**
	(0.487)	(0.002)	(0.020)	(0.031)
Observations	43,222	43,222	43,222	43,222
Mean of Dependent Variable	3,130.446	38.377	11.202	15.268
Panel B: White Mothers				
Anti-Black aggravated assaults	-0.174	-0.001	-0.014**	0.013
	(0.179)	(0.001)	(0.006)	(0.013)
Observations	51,722	51,722	51,722	51,722
Mean of Dependent Variable	3,398.254	38.997	5.246	8.267

Note: Coefficients in columns 3-4 multiplied by 100 for ease of interpretation. "Anti-Black aggravated assaults" refer to the number of anti-Black aggravated assaults reported in the mother's county of residence during pregnancy. Robust standard errors clustered at the county level. All models are weighted using the number of births in a county and include controls for mother's marital status, age, education level, infant's biological sex, parity, total and Black population, crime level, and unemployment rate as well as county and month-by-year fixed effects. *** indicates significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level.

Table 1.4: Effect of Trimester-Specific Exposure to Anti-Black Aggravated Assaults on Pre-Natal Care Utilization

	Number of prenatal visits	Start care in 1st trimester
	(1)	(2)
Panel A: Black Mothers		
1st trimester	-0.027	-0.003*
	(0.024)	(0.002)
2nd trimester	-0.000	0.000
	(0.020)	(0.001)
3rd trimester	0.005	0.0001
	(0.015)	(0.001)
Observations	43,110	43,110
Mean of Dependent Variable	10.430	0.648
Panel B: White Mothers		
1st trimester	0.021*	0.000
	(0.012)	(0.001)
2nd trimester	0.006	0.000
	(0.009)	(0.001)
3rd trimester	0.016	0.001
	(0.008)	(0.001)
Observations	51,718	51,718
Mean of Dependent Variable	11.817	0.822

Note: "1st trimester" refers to the effect of anti-Black aggravated assaults reported in the mother's county of residence during the first trimester of pregnancy; "2nd trimester" refers to the effect of anti-Black aggravated assaults reported in the mother's county of residence during the second trimester of pregnancy; "3rd trimester" refers to the effect of anti-Black aggravated assaults reported in the mother's county of residence during the third trimester of pregnancy. Robust standard errors clustered at the county level. All models are weighted using the number of births in a county and include controls for mother's marital status, age, education level, infant's biological sex, parity, total and Black population, crime level, and unemployment rate as well as county and month-by-year fixed effects. *** indicates significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level.

Table 1.5: Effect of Trimester-Specific Exposure to Anti-Black Aggravated Assaults on Birth Outcomes

	Birth weight (grams)	**Gestation length (weeks)**	**Low birth weight**	**Pre-term delivery**
	(1)	(2)	(3)	(4)
			Coefficient × *100*	
Panel A: Black Mothers				
1st trimester	-1.178	-0.008*	0.042	0.075
	(1.072)	(0.004)	(0.044)	(0.054)
2nd trimester	-1.701**	-0.013***	0.075**	0.121***
	(0.691)	(0.004)	(0.038)	(0.047)
3rd trimester	-1.055	0.003	0.044	0.043
	(0.686)	(0.003)	(0.028)	(0.038)
Observations	43,222	43,222	43,222	43,222
Mean of Dependent Variable	3,130.446	38.377	11.202	15.268
Panel B: White Mothers				
1st trimester	-0.197	-0.002	-0.006	-0.009
	(0.376)	(0.002)	(0.014)	(0.019)
2nd trimester	-0.390	-0.003*	-0.031**	0.034
	(0.315)	(0.002)	(0.013)	(0.023)
3rd trimester	0.009	0.001	-0.009	0.016
	(0.332)	(0.002)	(0.011)	(0.019)
Observations	51,722	51,722	51,722	51,722
Mean of Dependent Variable	3,398.254	38.997	5.246	8.267

Note: Coefficients in columns 3-4 multiplied by 100 for ease of interpretation. "1st trimester" refers to the effect of anti-Black aggravated assaults reported in the mother's county of residence during the first trimester of pregnancy; "2nd trimester" refers to the effect of anti-Black aggravated assaults reported in the mother's county of residence during the second trimester of pregnancy; "3rd trimester" refers to the effect of anti-Black aggravated assaults reported in the mother's county of residence during the third trimester of pregnancy. Robust standard errors clustered at the county level. All models are weighted using the number of births in a county and include controls for mother's marital status, age, education level, infant's biological sex, parity, total and Black population, crime level, and unemployment rate as well as county and month-by-year fixed effects. *** indicates significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level.

Table 1.6: Effect of In Utero Anti-Black Aggravated Assaults on Birth Outcomes–Robustness to Alternative Parameterizations of Anti-Black Aggravated Assaults

	Birth weight (grams)			Gestation length (weeks)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Black Mothers						
Anti-Black aggravated assaults	-24.542*	-2.035**	-1.513*	-0.093*	-0.007	-0.003
	(13.387)	(0.877)	(0.895)	(0.054)	(0.004)	(0.004)
Observations	43,222	43,222	43,222	43,222	43,222	43,222
Mean of Dependent Variable	3,130.446	3,130.446	3,130.446	38.377	38.377	38.377
Panel B: White Mothers						
Anti-Black aggravated assaults	-6.972*	-0.616	-0.794**	-0.029	-0.003	-0.000**
	(4.207)	(0.376)	(0.367)	(0.020)	(0.002)	(0.002)
Observations	51,722	51,722	51,722	51,722	51,722	51,722
Mean of Dependent Variable	3,398.254	3,398.254	3,398.254	38.997	38.997	38.997
Rate per 10,000	X	-	-	X	-	-
Log(Assault + 1)	-	X	-	-	X	-
Assault binary	-	-	X	-	-	X

Note: In Columns (1) and (4), anti-Black aggravated assaults are parameterized as a rate per 10,000 residents; in Columns (2) and (5), anti-Black aggravated assaults are log-transformed; in Columns (3) and (6), anti-Black aggravated assaults are parameterized as binary indicators equal to one when at least one anti-Black hate crime is reported in the mother's county of residence during pregnancy. Robust standard errors clustered at the county level. All models are weighted using the number of births in a county and include controls for mother's marital status, age, education level, infant's biological sex, parity, total and Black population, crime level, and unemployment rate as well as county and month-by-year fixed effects. *** indicates significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level.

Table 1.7: Associations between Anti-Black Aggravated Assaults and Maternal Demographic Characteristics

	Total births	Mother's age	Married	Less than HS	College or more	First child	Male child
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Coefficient × 100				
Panel A: Black Mothers							
Anti-Black aggravated assaults	0.464	0.005	-0.149***	-0.059	0.021	0.044	0.002
	(0.506)	(0.006)	(0.051)	(0.058)	(0.042)	(0.044)	(0.030)
Observations	43,222	43,222	43,222	43,222	43,222	43,222	43,222
Mean of Dependent Variable	71.286	26.033	30.314	20.563	13.193	38.048	50.830
Panel B: White Mothers							
Anti-Black aggravated assaults	-0.240	-0.001	-0.189*	0.003	-0.133	-0.055**	0.006
	(0.558)	(0.004)	(0.102)	(0.022)	(0.125)	(0.028)	(0.016)
Observations	51,722	51,722	51,722	51,722	51,722	51,722	51,722
Mean of Dependent Variable	200.036	28.866	76.072	8.561	38.322	42.979	51.324

Note: Coefficients in columns 3-7 multiplied by 100 for ease of interpretation. Robust standard errors clustered at the county level. All models except for column (1) are weighted using the number of births in a county. All models include controls for total and Black population, unemployment rate as well as county and month-by-year fixed effects. *** indicates significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level.

Table 1.8: Effects of Anti-Black Aggravated Assaults on the Number of Chest Pain-Related Emergency Department Visits

	Number of Chest Pain Related ED Visits	
	Black Patients	**White Patients**
	(1)	(2)
Visits on day of assault	0.114***	0.047
	(0.042)	(0.046)
Visits one day after assault	0.009	-0.039
	(0.061)	(0.060)
Visits two days after assault	-0.062	0.091
	(0.045)	(0.070)
Visits three days after assault	0.015	0.008
	(0.047)	(0.053)
Observations	18,181	25,637
Mean of Dep. Var.	0.263	0.700

Note: Robust standard errors clustered at the ZIP code level. Each column denotes a separate regression model. "Visits on day of assault" refers to the effect on the number of visits on the day when at least one anti-Black aggravated assault is reported in the patient's ZIP code of residence; "Visits one day after assault" refers to the effect on the number of visits one day after at least one anti-Black aggravated assault is reported in the patient's ZIP code of residence; "Visits two days after assault" refers to the effect on the number of visits two days after at least one anti-Black aggravated assault is reported in the patient's ZIP code of residence; "Visits three days after assault" refers to the effect on the number of visits three days after at least one anti-Black aggravated assault is reported in the patient's ZIP code of residence. Both models include controls for proportion of female patients, proportion of patients over 55 years old, and the total number of daily ED visits per ZIP code as well as the total and Black population in a ZIP code. Both models also include day-of-year and ZIP code fixed effects. *** indicates significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level.

Table 1.9: Heterogeneity by Availability of Local Newspapers–Effects of Anti-Black Aggravated Assaults on the Number of Chest Pain-Related Emergency Department Visits

	Black Patients		**White Patients**	
	Local news available	**Local news unavailable**	**Local news available**	**Local news unavailable**
	(1)	(2)	(3)	(4)
Visits one day of assault	0.159**	-0.246	-0.029	0.235
	(0.054)	(0.328)	(0.065)	(0.209)
Visits one day after assault	0.056	0.416***	0.038	0.037
	(0.095)	(0.144)	(0.076)	(0.303)
Visits two days after assault	-0.108*	0.969*	0.005	0.085
	(0.063)	(0.484)	(0.082)	(0.331)
Visits three days after assault	0.062	0.357	-0.041	-0.492
	(0.093)	(0.632)	(0.070)	(0.286)
Observations	7,328	1,390	10,779	2,282
Mean of Dep. Var.	0.246	0.389	0.537	0.510

Note: Robust standard errors clustered at the ZIP code level. “Local news available” refers to ZIP codes with over one daily local newspaper. “Local news unavailable” refers to ZIP codes with no daily local newspapers. “Visits on day of assault” refers to the effect on the number of visits on the day when at least one anti-Black aggravated assault is reported in the patient’s ZIP code of residence; “Visits one day after assault” refers to the effect on the number of visits one day after at least one anti-Black aggravated assault is reported in the patient’s ZIP code of residence; “Visits two days after assault” refers to the effect on the number of visits two days after at least one anti-Black aggravated assault is reported in the patient’s ZIP code of residence; “Visits three days after assault” refers to the effect on the number of visits three days after at least one anti-Black aggravated assault is reported in the patient’s ZIP code of residence. Both models include controls for proportion of female patients, proportion of patients over 55 years old, and the total number of daily ED visits per ZIP code as well as the total and Black population in a ZIP code. Both models also include day-of-year and ZIP code fixed effects. *** indicates significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level.

Table 1.10: Placebo Effects of Anti-Black Aggravated Assaults on the Number of Flu-Related Emergency Department Visits

	Number of Flu Related ED Visits	
	Black Patients	**White Patients**
	(1)	(2)
Visits on day of assault	-0.016*	-0.016
	(0.008)	(0.011)
Visits one day after assault	0.017	0.011
	(0.021)	(0.017)
Visits two days after assault	-0.011	0.003
	(0.011)	(0.019)
Visits three days after assault	0.009	-0.011
	(0.013)	(0.015)
Observations	18,181	25,637
Mean of Dep. Var.	0.021	0.054

Note: Robust standard errors clustered at the ZIP code level. Each column denotes a separate regression model. "Visits on day of assault" refers to the effect on the number of visits on the day when at least one anti-Black aggravated assault is reported in the patient's ZIP code of residence; "Visits one day after assault" refers to the effect on the number of visits one day after at least one anti-Black aggravated assault is reported in the patient's ZIP code of residence; "Visits two days after assault" refers to the effect on the number of visits two days after at least one anti-Black aggravated assault is reported in the patient's ZIP code of residence; "Visits three days after assault" refers to the effect on the number of visits three days after at least one anti-Black aggravated assault is reported in the patient's ZIP code of residence. Both models include controls for proportion of female patients, proportion of patients over 55 years old, and the total number of daily ED visits per ZIP code as well as the total and Black population in a ZIP code. Both models also include day-of-year and ZIP code fixed effects. *** indicates significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level.

Chapter 2

Gunshot Noise and Birth Outcomes

2.1 Introduction

In 2021, the United States experienced nearly 700 mass shootings, an increase of roughly 150% relative to 2014 (Gun Violence Archive, 2023). Despite the growing prevalence in gun-related violence, interest groups and lobbyists alike continue to halt any groundbreaking legislation that would prohibit gun ownership. In effect, gun violence is likely to continue indefinitely.

Although a growing body of scholarly work documents the adverse impact of large mass shooting events on birth outcomes (Dursun, 2019; Banerjee and Bharati, 2020; Currie et al., 2023), substantially less research exists demonstrating the effect of typical, every-day, gun violence on an infant's health. The mere sound of gunshots can profoundly shape a pregnant individual's perception of safety and future victimization risk, thus influencing their psychological well-being and altering their daily routines. Consequently, this added stress during in utero can significantly affect a mother's health, and in-turn, the health of their child. However, as gunshot noise remains largely under-reported (Carr and Doleac, 2016), there exists little empirical evidence demonstrating its effects on birth health.

In this study, we overcome the challenge of under-reported gunshots by leveraging novel data obtained through ShotSpotter, an automated gunshot detection technology which utilizes sensors placed on buildings and streetlamps, thereby circumventing the need for civilian reporting. We geographically map these gunshot detections to census-block locations of mothers using restricted-access information on the universe of births in nine California cities. In effect, we are able to compare the infant health of mothers that experience gunshots during pregnancy to mothers that do not, while accounting for the expected differences between census-blocks, times of the year, and mother characteristics.

The findings show that in utero exposure to gunshot noise is linked to a higher incidence of very preterm deliveries ($<$ 32 weeks) and very low birth weights ($<$ 1,500 grams). These effects are primarily concentrated among mothers with relatively low levels of formal education ($<$ bachelor's degree), underscoring the heightened vulnerability of underprivileged persons to the adverse effects of gunshot noise. Moreover, we find that the time-of-day the gunshot occurs is directly linked to detrimental birth outcomes– mothers that experience gunshots during times of day when they are likely to be home (e.g., outside 9:00am-5:00pm) are most affected, therefore implying that mothers must hear the gunshot to receive the corresponding stress it creates. These results are also robust to compositional changes in the profile of mothers giving birth.

In obtaining these findings, this research makes significant contributions to several areas of scholarly work. First, we expand on the existing body of literature studying the negative outcomes of crime exposure in utero. While previous studies in this space have largely relied on broadly defined county-level exposures (Dursun, 2019; Banerjee and Bharati, 2020), we enhance geographic precision by utilizing highly localized exposure data at the census block group level. Second, we add to the literature on the broad health impacts of gun-related violence. Existing research often focused on highly acute and rare instances of gun-related crime, such as mass shootings and school shootings (Soni and Tekin, 2020; Cabral et al., 2020). We complement this body of work by examining the impact of a relatively less acute, yet more frequent, type of gun-related stress in gunshot noise. Lastly, we add to the scholarship leveraging ShotSpotter as a unique data source to get a more accurate measure of gunshot frequency. Previous works have used ShotSpotter as a measure of underlying crime (Carr and Doleac, 2016, 2018) or as a novel way to measure police mistrust (Ang et al., 2021). Here, we utilize the unique functionality of this technology in order to get a more accurate measure of exposure to gunshots.

The sections of this paper are organized as follows: Section II discusses the relevant literature, Section III describes the data used, Section IV explains the identification strategy, Section V outlines the results, and Section VI concludes.

2.2 Literature Review

2.2.1 In Utero Stress and Birth Outcomes

Health at birth serves as the first measure of human capital accumulation. This is because health at birth affects one's health in adolescence and adulthood as well as a range of later life non-health outcomes, including educational attainment (Royer, 2009). An important mechanism explaining this connection, extensively explored in recent economic literature, is the *fetal programming hypothesis* (Almond and Currie, 2011; Almond et al., 2018). The hypothesis was initially developed by epidemiologist David Barker to connect nutritional deficiencies in utero with increased susceptibility to adverse later life outcomes. However, it has been increasingly applied in the context of psychological stress experienced by a mother as a result of exposure to events such as natural disasters, acts of terrorism, armed conflict, mass shootings, domestic violence, the loss of a family member, and even sporting events (Torche, 2011; Brown, 2018; Camacho, 2008; Quintana-Domeque and Rodenas-Serrano, 2017; Dursun, 2019; Mansour and Rees, 2012; Currie et al., 2022; Duncan et al., 2016).

It is worth noting that a handful of studies connects exposure to in utero stressors to improvements in birth outcomes (Torche and Villarreal, 2014; Lichtman-Sadot et al., 2022). Stress can lead mothers to adopt health enhancing behaviors, such as frequent prenatal visits or choosing to remain at home and rest instead of commuting to work. However, these studies report different effects across socio-economic status, suggesting that this may be a relevant dimension impacting one's vulnerability to stress as well as availability of coping strategies.

2.2.2 Effects of Crime Victimization and Exposure

The literature extensively documents the adverse impacts of crime on individuals who are directly victimized. Physical health is directly affected by victimization if the criminal act results in bodily injury. Even in the absence of bodily harm, victims often experience mental health distress (Cornaglia et al., 2014). Moreover, crime victimization has been shown to influence non-health-related outcomes, including career trajectories (Bindler and Ketel, 2022).

In addition to affecting victims, crime also affects individuals who strongly identify with the victims, such as those residing in the same area or belonging to the same racial group (Powdthavee, 2005; Bindler et al., 2020). However, it is essential to acknowledge that for a crime incident to influence the outcomes of individuals not directly victimized, there must exist a mechanism for these individuals to learn about the crime. Consequently, studies utilizing geographical variation too coarse to allow for direct witnessing of the crime employ media coverage as a pathway. To this end, Banerjee and Bharati

(2020) utilize exogenous variation in news coverage of mass shootings to illustrate that media coverage mediates the relationship between mass shootings in the mother's county of residence and birth outcomes. Similarly, Curtis et al. (2021) correlate highly publicized instances of anti-Black violence, as proxied by the number of Google searches, with an increase in worse mental health days among Black Americans.

2.3 Data

In this section, we outline the two main sources of data: restricted-access birth records in California and ShotSpotter gunshot detections from nine cities in California. As an overview, Figure 2.1 plots each city in the sample and the corresponding time frame used within the analysis. On average, a city is within the sample for approximately 5.5 years, with the oldest records dating to January 2008 and the most recent December 2020.

2.3.1 Birth Data

Restricted-access birth records are obtained from the California Department of Public Health. The data covers the universe of births in California and includes information on mothers' address of residence, demographics, as well as pregnancy and infant characteristics (California Department of Public Health, 2022). We geocode all addresses and map to the corresponding census block using the Census Bureau TIGER/Line Shapefiles for the year 2020.

Following recent scholarship on gun violence and birth outcomes (Currie et al., 2023), we define the primary birth outcomes of interest as *Very Low Birth Weight* and *Very Preterm Delivery*. *Very Low Birth Weight* is an indicator variable that takes the value of one when an infant's birth weight is less than 1,500 grams (Daysal et al., 2022), whereas *Very Preterm Delivery* is an indicator variable that takes the value of one when the gestation length is less than 32 weeks (World Health Organization, 2018). In addition to these primary measures, we also create four secondary birth outcomes: *Birth weight*, *Low birth weight*, *Gestation length*, and *Preterm delivery*. *Birth weight* captures an infant's weight at birth in grams; *Low Birth Weight* is an indicator variable that take the value of one when an infant's birth weight is less than 2,500 grams (World Health Organization, 2014); *Gestation length* captures the gestation length in weeks; *Preterm delivery* is an indicator variable that take the value of one when the gestation length is less than 37 weeks (World Health Organization, 2018).

2.3.2 Gunshot and Crime Data

We obtain gunfire data from nine cities within California that have ShotSpotter technology implemented: Bakersfield, East Palo Alto, Fresno, Oakland, Richmond, San Diego, San Francisco, San Pablo, and Stockton. These nine cities represent all cities within California that meet the following criteria: ShotSpotter must have been implemented between years 2000-2020, the city must have agreed to release location-based information on ShotSpotter detections via a California Public Record Act request, the city must reside in California.[1] The resulting sample of cities corresponding counties can be seen in Figure 2.2. Of the nine cities, 55% are located in northern California.[2]

To identify the occurrence of gunfire, we exploit each city's utilization of ShotSpotter technology. ShotSpotter is an acoustic gunfire detection software which uses an array of microphones and sensors placed on streetlamps and buildings across the city to identify, locate, and rapidly dispatch police to the sound of gunfire. The sensors are wirelessly connected to servers and are equipped with machine-learning technology in order to

[1] For some select cities, we use ShotSpotter data collected by the Justice Tech Lab whom obtain the data through California Public Record Act requests (Carr and Doleac, 2016, 2018).

[2] While ShotSpotter technology is in over 150 cities world-wide, there is a high representation within northern California where its parent company, SoundThinking, is located.

detect the sound of gunfire. Given that only 12% of gunfire is reported by civilians (Carr and Doleac, 2016), ShotSpotter represents a novel way to more closely capture the true incidence of gunfire, bypassing the reliance on human reporting.

The technology utilizes a machine-learning algorithm in order to decipher gunshot noises from other sounds such as fireworks or car-backfires. Once a sensor detects a gunshot, a recording of the sound is forwarded to ShotSpotter's 24-hour review center, where an expertly trained employee checks the recording for false positives. After confirmation, the information is sent to the police communications department where police officers are subsequently dispatched.[3] These dispatches are assigned a unique identification code which classifies the dispatch as ShotSpotter-initiated.

ShotSpotter sensors cannot capture every instance of gunfire, although the company claims that the sensors have 97% accuracy with a 0.5% false-positive rate. One field study finds a lower accuracy rate of roughly 81%, although the study was conducted in ShotSpotter's infancy (Goode, 2012; Irvin-Erickson et al., 2017; Mazerolle et al., 1998). Over the past decade, both the company and police departments claim that the technology has greatly improved. Moreover, although not specifically verifying the accuracy, studies have shown that ShotSpotter implementation results in greater numbers of gun-related dispatches (Mares and Blackburn, 2021). Hence, ShotSpotter detections represent a way to more precisely measure the presence of gunfire in comparison to 911 reports.

2.3.3 Sample Restrictions and Summary Statistics

We impose several restrictions on the sample. First, to successfully match ShotSpotter exposures to birth outcomes, we restrict to mothers residing in the nine cities for which we have access to both birth data and gunshot data: Bakersfield, East Palo Alto, Fresno,

[3]This entire process, according to SoundThinking, ShotSpotter's parent company, takes under 60 seconds.

Oakland, Richmond, San Diego, San Francisco, San Pablo, and Stockton.

As a second restriction, we exclude mothers residing in census blocks which have not adopted ShotSpotter and therefore lack information on gunshots. Within a city, ShotSpotter sensors tend to be implemented in areas that experience relatively higher incidence of gun violence than areas in which ShotSpotter is not implemented. Therefore, areas without ShotSpotter sensors may not be comparable to areas with access to ShotSpotter sensors.

Third, we include only mothers from whom the gestation period matches availability of ShotSpotter data (i.e., expected conception month falls on or after the first month when ShotSpotter data is available, and expected birth month falls before or on the last month when ShotSpotter data is available).[4] In doing so, we make certain that we have data on ShotSpotter alerts throughout the entire duration of pregnancy, as mothers whose pregnancy period only partially matches availability of ShotSpotter data in their census block of residence could be erroneously misclassified as not exposed to gunshot simply due to lack of data availability.

Finally, we focus solely on mothers between 15 and 49 years old, as well as on singleton pregnancies.[5] Low or high maternal age as well as twin, triplets and higher plurality pregnancies are typically associated with lower birth weights than otherwise comparable pregnancies.

Table 2.1 summarizes the birth outcomes and demographic characteristics of mothers in the sample. In Panel A, we create summary statistics for the birth outcomes of interest. The two main outcomes, Very Preterm and Very Low Birth Weight, are rare among mothers–roughly 1.4% and 1.1% of infants are born with such conditions. Gestation

[4]Expected conception month refers to the month obtained by subtracting gestation length from actual month of birth. Expected birth month refers to the month obtained by adding 10 months to the expected conception month.

[5]We also exclude births for which gestation length, weight at birth, birthdate, or maternal address of residence is unknown.

length is approximately 38 weeks on average, while infants are generally born weighing 3,299 grams (around 7.2 lbs).

In Panel B of Table 2.1, we present summary statistics of the main control variables describing the observable characteristics of the mothers. An average mother in the sample is approximately 29 years old, although mothers can be as young as 15 or as old as 48. Nearly 17% of mothers in the sample have completed at least a bachelor's degree, and nearly 60% of mothers identify as Hispanic. The sample is diverse with roughly 12% of mothers Asian, 15% Black, and 53% White.

2.4 Empirical Strategy

2.4.1 Baseline Specification

To evaluate the effect of gunshot noise on birth outcomes, we estimate the following regression model using OLS:

$$Y_{ict} = \alpha + \beta ShotSpotter_{ct} + \gamma X_{ict} + \pi_c + \rho_t + \upsilon_{ict} \tag{2.1}$$

where Y_{icy} denotes birth outcome of mother i residing in census block c, with a child conceived in month and year t. The primary treatment variable, $ShotSpotter_{ct}$, is a binary variable equal to one if at least one ShotSpotter alert has been reported in census block c during pregnancy that started in month and year t. This represents around 50% of mothers who experience exposure to gunshot noise during pregnancy within the sample. Pregnancy is defined in terms of expected, not actual, date of birth. We obtain conception month and year by subtracting the length of gestation from the actual birth month and year. We then calculate expected birth month and year by adding ten months to the conception date. We use expected birthdate because the actual birthdate might be endogenous to gunshot noise, as the likelihood of exposure is lower with shorter pregnancies (Currie et al., 2023). X_{ict} is a vector of observable maternal characteristics such as, age, education, sex of the infant, race, ethnicity, and the number of prior live births (see Table 2.1). We include census-block fixed effects π_c in order to account for time-invariant differences, such as crime, that may persist across neighborhoods. Additionally, year-by-month fixed effects ρ_t allow us to account for the expected differences across different times of the year. Finally, υ_{ict} is the error term, and standard errors are clustered at the census block level to account for the serial correlation that may occur within blocks. Taken together, the model compares mothers that experience gunshot noise during pregnancy to mothers that do not experience gunshot noise, while accounting for the expected differences between census blocks, times of day, and mother characteristics.

The coefficient of interest is β, which captures the average effect of in utero exposure to gunshot noise (as detected by ShotSpotter) during pregnancy on a mother's birth outcome. However, in order for β to be interpreted as casual, three main assumptions must be met; mothers that experience gunshots would have had similar birth outcomes as mothers that did not experience gunshots, the number ShotSpotter gunshot detection devices are stable throughout the time period, and the composition of births is not changing.

For the first assumption that birth outcomes would be similar without the exposure to gunshots, we conduct a placebo test wherein we estimate whether gunshots experienced *after* the delivery of a child affect birth outcomes. In particular, we test whether mothers that experience gunshots only after their birth exhibit similar changes in their birth outcomes as mothers that experience gunshots during pregnancy. As discussed further in Section 2.5.2, we find little evidence of mothers displaying worse birth outcomes without in utero exposure to gunshots.

The second assumption is that ShotSpotter gunshot detection devices are stable throughout the sample period. If cities were to increase the number of gunshot detection sensors, then it is possible that gunshot *detections* become more prevalent while the underlying number of gunshots remains constant. However, we mitigate this concern by restricting the sample within each city to only periods where there is no increase in the amount of coverage.[6]

Next, the third assumption is that there is no change in the composition of births. For instance, it is well documented that Black mothers tend to have worse birth outcomes than non-Black mothers (Office of Minority Health, 2023a). If exposure to treatment induces more Black mothers to conceive and/or carry the pregnancy to term, we could report an increase in adverse birth outcomes despite mothers not experiencing higher risk due to gunshot exposure. To test this, we show that in utero exposure to gunshot noise does not predict the sex of the infant, whether the mother is giving birth to her first infant, maternal age, whether the mother is Hispanic, Black, or has a bachelor's degree or more (Table 2.2).

Finally, it is important to note that β does not enable us to directly delineate the effects of *hearing* a gunshot noise and the effect related to the police dispatch cars that follow. Although it has been shown that increased police presence is associated adverse birth outcomes among minority mothers (Hardeman et al., 2021), we emphasize that gunshots can be heard within a large radius where a listener may not see the following police dispatch.

[6] Specifically, for the city of Fresno, we had to omit any data later than 2018 as it went through a large expansion of the technology.

2.5 Results

2.5.1 Main Results

Table 2.3 presents the effects of gunshot noise during pregnancy, estimated using Equation 2.1, on Very Low Birth Weight (birth weight <1,500 grams) in Panel A and Very Preterm Delivery (gestation length <32 weeks) in Panel B. We find that gunshot noise causes large increases in these outcomes for mothers with low education (no bachelor's degree), and they are driven by times of the day when mothers are likely to be at home.

Contrary to the initial hypothesis that gunshot noise should affect all mothers, we do not find any evidence of in utero exposure to gunshot noise affecting birth outcomes when including all mothers in the sample (Column 1). However, this does not necessarily imply that exposure to gunshot noise does not result in psychological stress. In particular, stress from gunshot noise may result in two simultaneous, and confounding effects; on one hand, some mothers may seek to mitigate their enhanced stress levels with extra prenatal care and doctors visits, thus improving their birth outcomes. On the other hand, other mothers may not be able to adopt behaviors and leverage resources to mitigate the negative consequences of resulting stress. Hence, these two confounding effects may drive the initial null result we find.

Columns 2 and 3 of Table 2.3 separate subgroups that may respond differently to the stress of gunshot noise: mothers with low levels of formal education (below bachelor's degree), and high levels of formal education (bachelor's degree and above). Consequently, the results show that at least one gunshot during pregnancy is associated with a 30% increase in the incidence of very low birth-rates and a 33% increase in very preterm deliveries relative to the mean for low education mothers only–high education mothers show little evidence of an effect. These results for low education mothers are statistically significant at the 10% and 5% level respectively. Hence, it may be that low education mothers have less access to resources that could be used to mitigate the adverse effects of stress, and in-turn, their birth outcomes are hindered. We further explore this potential mechanism in Section 2.5.3.

In Columns 4 through 7, we separate the effects of gunshot noise on high and low education mothers by the time of day in which the gunshot is detected–Working Hours (9:00am-5:00pm) and Non-Working Hours (before 9:00am or after 5:00pm). This is motivated by the notion that if a gunshot occurs and a mother is not home to hear it, then they should not receive the corresponding increase in stress. Indeed, we find no evidence of working hour gunshots affecting birth outcomes of mothers with either low or high levels of education (Columns 4 and 5). Conversely, gunshot effects on low education mothers appear to be driven by exposures to gunshots that occur outside working hours. In particular, Column 6 shows that at least one non-working hour gunshot during pregnancy is linked to an increase in the incidence of both very low birth weights and very preterm deliveries by 34% and 28% relative to the mean, respectively. These results are statistically significant at the 5% and 10% level, respectively. Hence, these results further demonstrate that salience is key to gunshot noise acting as a stressor–mothers must be in the vicinity of the gunfire in order to receive the stress it creates.

It is interesting to note the magnitude of the effects reported for mothers with low levels of education are substantial compared to previous literature. For reference, Dursun (2019) finds that a mass shooting in mother's county of residence during pregnancy is linked with an 8% increase in the incidence of very low birth weights and a 7% increase in the incidence of very preterm births. The effects we report here are approximately four times as large as the ones reported by Dursun (2019). Although gunshot noise is a less acute stressor than a mass shooting, our data enables us to capture the effects of gunshot noise occurring within a very close proximity to the mother's residence, i.e., at a census block level, as opposed to county level.

2.5.2 Placebo Test

To verify that trends in unobservable characteristics associated with gunshot noise do not affect birth outcomes, we conduct a placebo test estimating the effect of gunshots occurring after expected delivery on birth outcomes. Exposures after delivery should have no effect on birth outcomes, as birth has already taken place. We focus the placebo test on mothers with low levels of formal education and exposures to non-working hours gunshots, as this is where our results are concentrated.

To conduct the placebo test, we estimate the following model:

$$Y_{ict} = \alpha + \sum_{x=1}^{3} \beta_x ShotSpotter_{ct,x} + \gamma X_{ict} + \pi_c + \rho_t + \upsilon_{ict} \tag{2.2}$$

where $ShotSpotter_{ct,1}$ is a binary variable equal to one if at least one ShotSpotter alert has been reported in census block c during pregnancy that started in month and year t; $ShotSpotter_{ct,2}$ is a binary variable equal to one if at least one ShotSpotter alert has been reported in census block c up to 10 months after delivery; $ShotSpotter_{ct,3}$ is a binary variable equal to one if at least one ShotSpotter alert has been reported in census block c between 11 and 20 months after delivery. The model is estimated using the original sample restricted to only the mothers for whom both the expected gestation period and the period up to 20 months following expected delivery falls within the period of availability of the ShotSpotter data within their census blocks of residence.

Figures 2.4 and 2.5 document the results of the placebo test for Very Low Birth Weight and Very Preterm respectively. "Pregnancy" denotes the effect of exposure to at least one non work-time gunshot during pregnancy; "After Delivery I" denotes the effect of exposure to at least one non work-time gunshot up to 10 months after expected delivery; "After Delivery II" denotes the effect of exposure to at least one non work-time gunshot from 11 through 20 months after expected delivery. Circles represent the point estimates, and the error bars denote the 95% confidence intervals.

The results are qualitatively similar to the baseline results for exposures during pregnancy. At the same time, we find no evidence of post-trends as the effects of gunshot exposures after expected delivery on very low birth weights and very preterm deliveries.

2.5.3 Mechanism

Although the results show that gunshot detections during pregnancy result in worse birth outcomes for low-educated mothers, it is important to understand why this does not appear to be the case for high-education mothers. As briefly discussed in Section 2.5.1, it could be that mothers with high education seek care to mediate their higher stress levels. In this subsection, we test this hypothesis by testing whether exposure to gunshots results in high-education mothers experiencing more prenatal care (as measured by visits), or alternatively, whether low-education mothers exhibit a higher likelihood of risky coping behaviors (smoking).

Table 2.4 presents the results when we estimate Equation 2.1 with three new outcomes: a binary variable for whether a mother engaged in smoking behavior during pregnancy (Columns 1 and 4), the number of prenatal care visits (Columns 2 and 5), and the probability of initiating care after the first trimester (i.e., all care is received within trimester 2 or 3). In Columns 1 and 4, we test whether mothers engage in smoking more frequently when hearing gunshots. We find little evidence of such behavior for both high and low education mothersâthe point estimates are small and not statistically significant. However, it is important to note that smoking is a self-reported measure. Moreover, Columns 2 and 4 estimate the model for the number of prenatal care visits, while Columns 3 and 6 for delayed care. However, in each of the specifications, we find little evidence of an effect on prenatal visits or late visit following in utero exposure to gunshot noisesâthe marginal effects are close to zero and precisely estimated for each subgroup.

Given the results, it is notable that prenatal visits are an imperfect measure of mothers' mediating their stress. For instance, it is difficult to know whether a stressed mother will seek more care (out of fear for the child) or less care (out of inability to cope). Therefore, the null results we find may be a composition of these two effects, thus providing an unclear story on how mothers effectively cope with more stress. Moreover, the number of prenatal visits is self reported by mothers *after* their delivery–a question that can be difficult to recall and subject to considerable bunching. Indeed, as shown in Figure 2.6, the number of prenatal visits exhibits clear bunching at 10 and 12.

2.5.4 Other Birth Outcomes

Table 2.5 shows the results when equation (1) is used to predict the effects of gunshots on supplementary birth outcomes of mothers with low levels of formal education: birth weight (in grams), low birth weight, gestation length (in weeks), and preterm delivery. We find no evidence of adverse effects on these secondary birth outcomes.

2.6 Conclusion

Gun-related violence has surged in recent years, with both high-profile mass shootings and less severe gun-related crimes on the rise across the country. However, tracking the health implications of the latter has been a challenge for researchers due to underreporting. This paper overcomes this obstacle by leveraging novel data from ShotSpotter, an automatic gunshot detection technology. Through combining ShotSpotter's gunshot data with the universe of birth records in nine cities in California, we document the intergenerational impact of exposure to gunshots noise in utero.

Despite gunshots representing a relatively less acute form of stress, we document significant adverse effects on birth outcomes, particularly pronounced in infants born to mothers with lower levels of education. The magnitude of these effects is substantial, and the effects are driven by exposures during non-working hours, when mothers are more likely to be physically at home, and able to hear the corresponding noise.

One of the key strengths of this study is that we manage to identify exposures at a fine geographic level, potentially allowing for direct exposure to the gunshot noise. In contrast, much prior work in the birth outcome space has relied on county-level exposures, which likely involve mothers learning about the crime incident through the news media instead of witnessing it directly. At the same time, the study has several important limitations. First, we focus on mothers residing in a sample of California cities. This both reduces statistical power but also poses challenges for external validity. Future research should strive to collect a broader range of data, potentially spanning other cities. Furthermore, as mentioned before, the estimates capture the effect of stress associated with both gunshot noise and increased police presence. Future work should aim to collect data on police dispatches to particular locations to isolate these effects. Despite these limitations, this research provides evidence of adverse consequences of exposure to even the less severe manifestations of gun violence, emphasizing the critical need for policymakers to target efforts to mitigate it.

Figures

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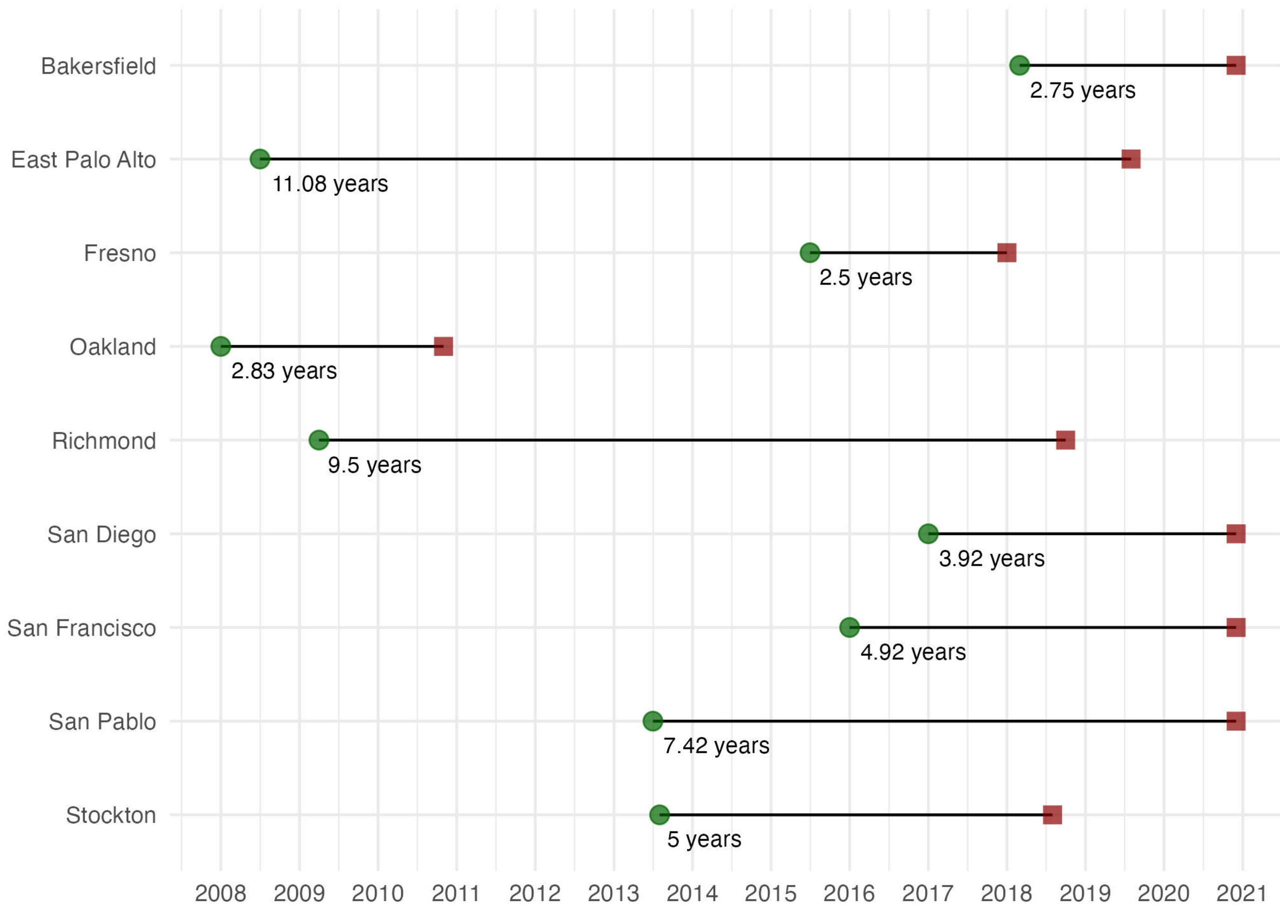

Figure 2.1: Time Period Utilized for Sample Cities

Note: This figure shows the nine sample cities and the corresponding time period that is analyzed. The entire time period covers from January 1, 2008 through December 31, 2020. While birth data exists for this entire time period, the sample is restricted due to the availability of ShotSpotter gunshot detection data. Hence, the time periods utilized reflect periods in which we have both birth data and gunshot detection data. The average coverage time per-city is 5.5 years.

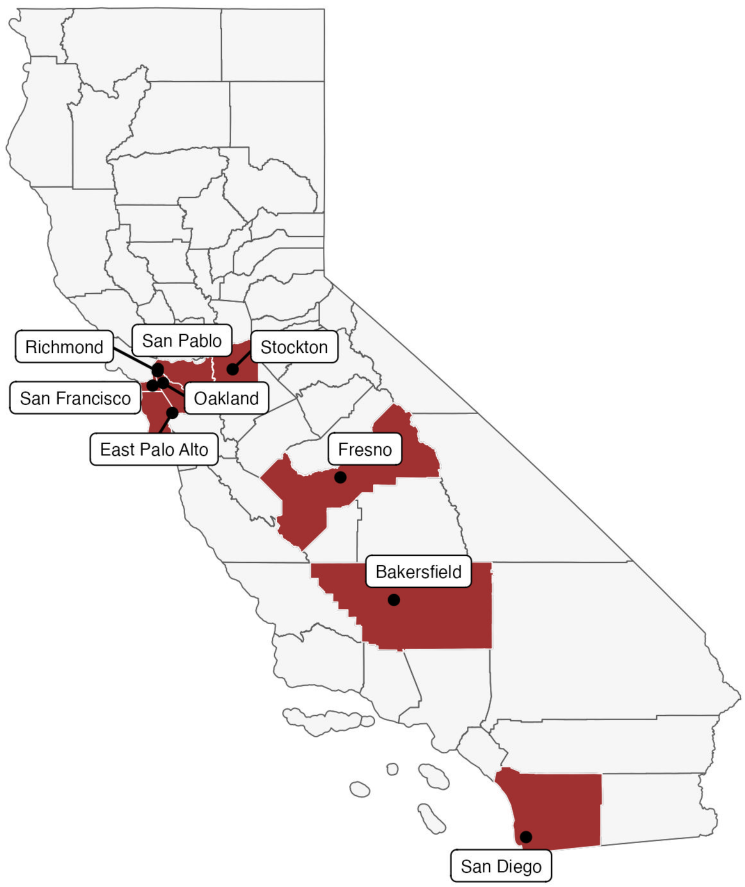

Figure 2.2: Sample City Locations

Note: This figure shows the location of the nine cities (blue points) that are in the sample. In particular, the nine cities are Bakersfield, East Palo Alto, Fresno, Oakland, Richmond San Francisco, San Diego, San Pablo, and Stockton. The red portion of the map represents the city's residing county.

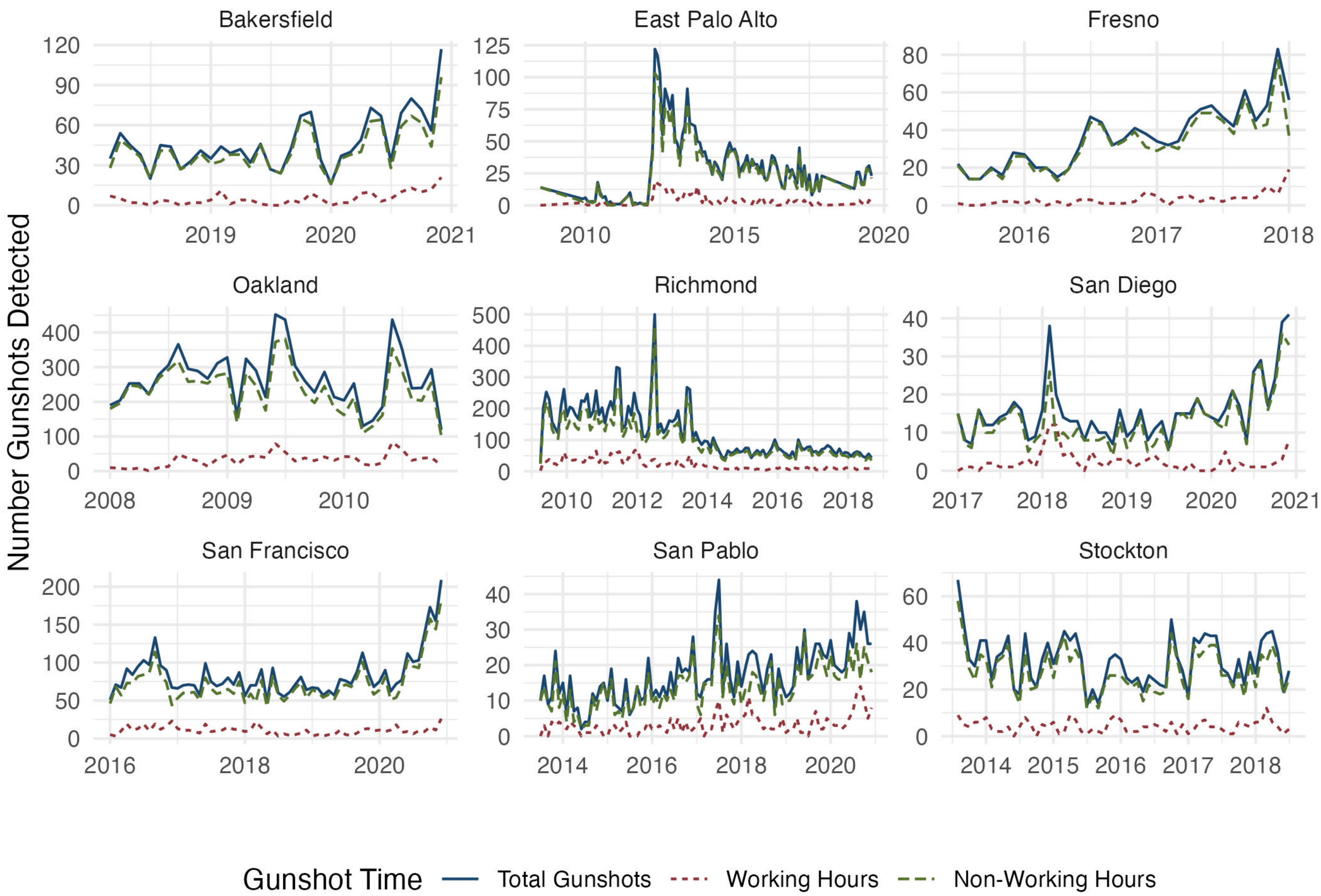

Figure 2.3: Gunshot Detections Over Time (monthly)

Note: This figure plots the number of gunshots at the monthly level (y-axis) over time. Three lines are shown: Total Gunshots, Working Hours, and Non-Working Hours. Total Gunshots refers to the total number of gunshots detected, while Working Hours and Non-Working Hours refer to the number of gunshots detected during the time periods 9:00am-5:00pm and 5:00pm-9:00am, respectively. Therefore, Total Gunshots is the sum of Working Hours and Non-Working Hours. As shown, there are far fewer Working Hours gunshots. We use this as motivation for analysis in Section 2.5.

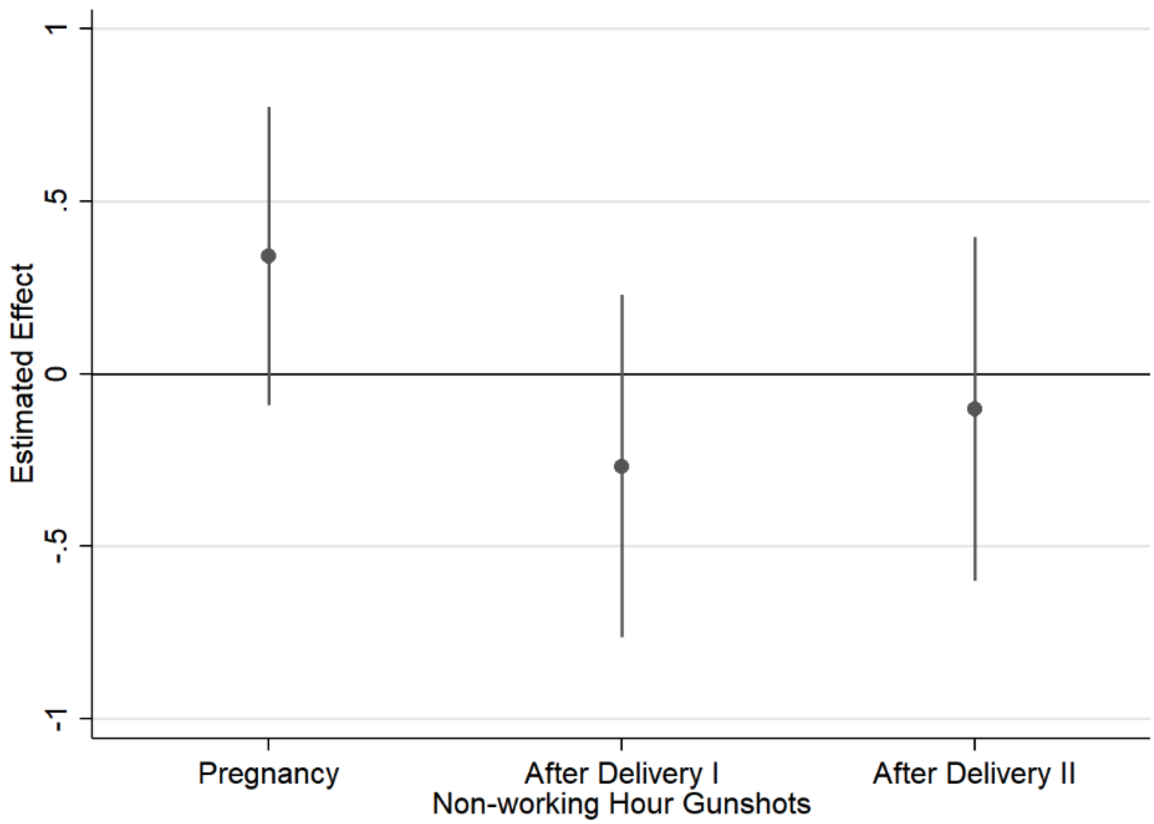

Figure 2.4: Placebo Test for Very Low Birth Weight

Note: Circles represent the point estimates, and the error bars denote the 95% confidence intervals, estimated using equation (2) and restricted to mothers with low formal education (i.e., below a bachelor's degree). "Pregnancy" denotes the effect of exposure to at least one non work-time gunshot during pregnancy; "After Delivery I" denotes the effect of exposure to at least one non work-time gunshot up to 10 months after expected delivery; "After Delivery II" denotes the effect of exposure to at least one non work-time gunshot from 11 through 20 months after expected delivery. Very Low Birth Weight is defined as a birth below 1,500 grams. Moreover, Non-Working Hours is defined as experiencing a gunshot between the hours 5:00pm-9:00am. All coefficients multiplied by 100 for ease of interpretation. All models include census-block, year-by-month fixed effects, the set of controls as defined in Table 1.

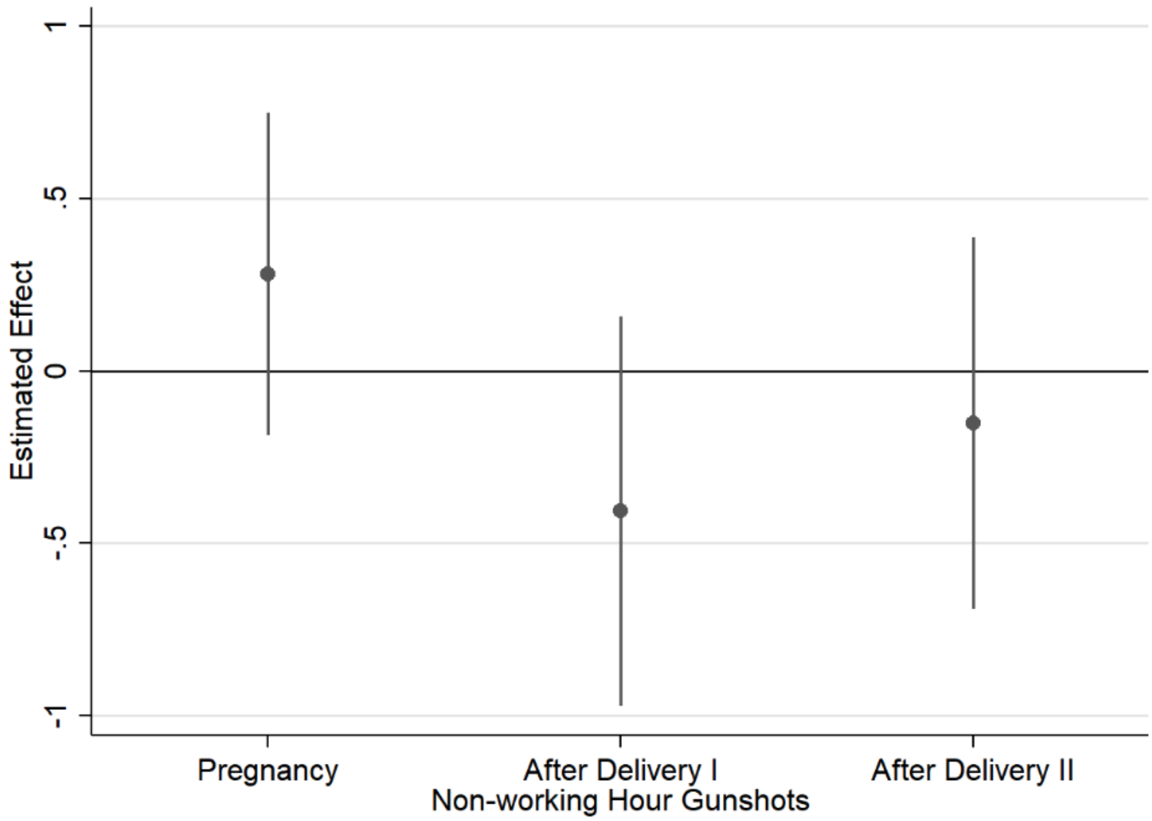

Figure 2.5: Placebo Test for Very Preterm

Note: Circles represent the point estimates, and the error bars denote the 95% confidence intervals, estimated using equation (2) and restricted to mothers with low formal education (i.e., below a bachelor's degree). "Pregnancy" denotes the effect of exposure to at least one non work-time gunshot during pregnancy; "After Delivery I" denotes the effect of exposure to at least one non work-time gunshot up to 10 months after expected delivery; "After Delivery II" denotes the effect of exposure to at least one non work-time gunshot from 11 through 20 months after expected delivery. Very Preterm is defined as a gestation length less than 32 weeks. Moreover, Non-Working Hours is defined as experiencing a gunshot between the hours 5:00pm-9:00am. All coefficients multiplied by 100 for ease of interpretation. All models include census-block, year-by-month fixed effects, the set of controls as defined in Table 1.

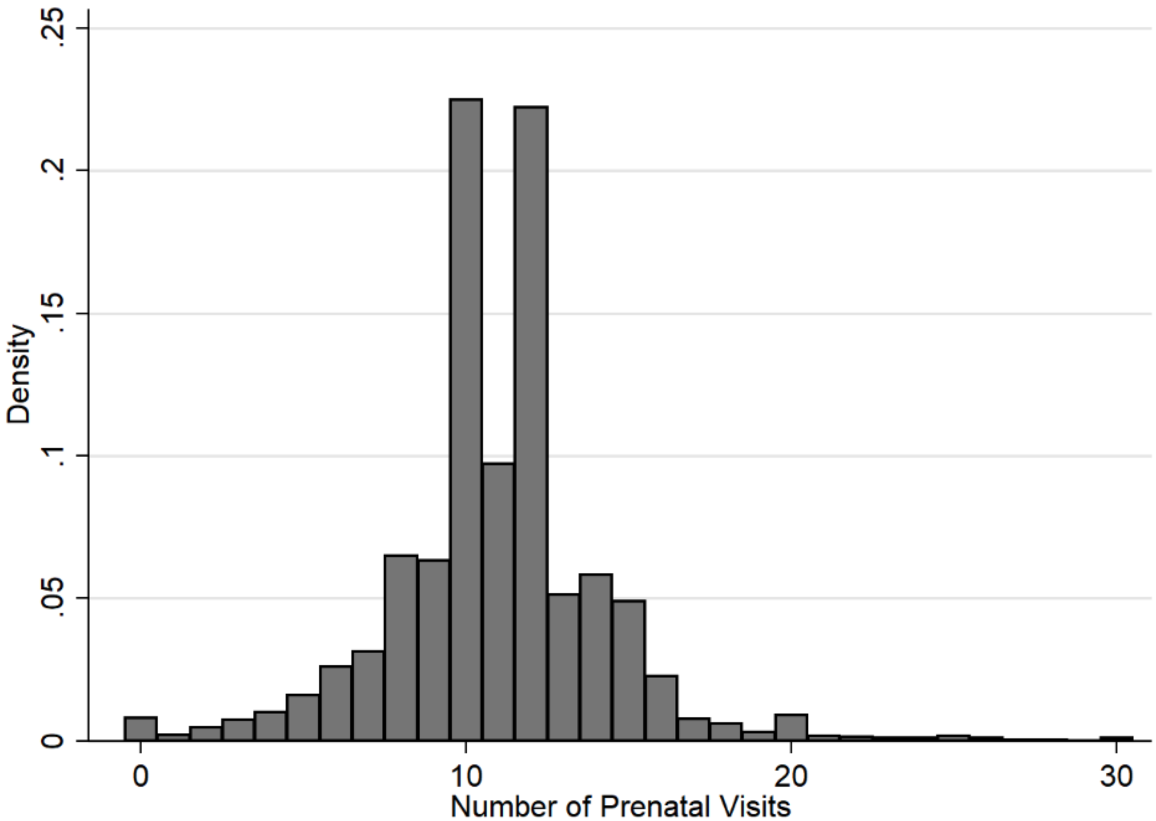

Figure 2.6: Distribution of Prenatal Visits
Note: Prenatal Visits refer to the total number of prenatal visits reported by the mother. The distribution shows 99% of the observations (top 1% of the distribution is not shown).

Tables

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Table 2.1: Summary Statistics

	Mean	Std.Dev.	Min	Max
Panel A: Birth Outcomes				
Very Preterm	0.014	0.115	0	1
Very Low Birth Weight	0.011	0.105	0	1
Preterm	0.084	0.277	0	1
Low Birth Weight	0.064	0.245	0	1
Gestation Length (weeks)	38.688	2.079	1	48
Birth Weight (grams)	3,299.841	571.560	68	6,350
Panel B: Controls				
Age	28.531	6.322	15	49
Bachelors or Higher	0.168	0.374	0	1
Hispanic	0.595	0.491	0	1
Asian	0.125	0.331	0	1
Black	0.153	0.360	0	1
White	0.539	0.499	0	1
First time Mother	0.388	0.487	0	1
Male Infant	0.509	0.500	0	1
Total Observations:	38,373			

Note: Included mothers are those that reside in the nine California cities for which locational gunshot data occurs. Mothers must reside in a census block which are known to have the ShotSpotter technology during their gestation period. Mothers of ages 15 to 49 are included, as well as only singleton pregnancies. Panel A contains the two primary outcomes of interest: Very Preterm ($<$ 32 weeks) and Very Low Birth Weight ($<$ 1,500 grams). Four other less serious birth outcomes are also provided: Preterm, Low Birth Weight, Gestation Length (weeks), and Birth Weight (grams). In Panel B, controls used in Equation 1 are presented.

Table 2.2: Effect of Gunshot Noise on Controls (OLS)

	Male Infant	First-time Mother	Mother's Age	Hispanic	Bachelors or Higher	Black
	(1)	(2)	(3)	(4)	(5)	(6)
Gunshots	0.003	-0.009	0.045	-0.006	-0.003	0.004
	(0.007)	(0.007)	(0.090)	(0.006)	(0.005)	(0.005)
Mean of Dependent Variable	0.509	0.388	28.531	0.595	0.168	0.153
Observations	38,373	38,373	38,373	38,373	38,373	38,373
FE: Census Block	X	X	X	X	X	X
FE: Year by Month	X	X	X	X	X	X

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered by census block. All models include census-block and year-by-month fixed effects. Gunshots is a binary variable equal to one if a mother experienced a gunshot as detected through ShotSpotter during their pregnancy. Male Infant is a binary variable equal to 1 if the infant is male. First-time Mother is a binary variable equal to 1 if the pregnancy corresponds to mother's first live birth. Mother's Age denotes mother's age in years. Hispanic is a binary variable equal to 1 if the mother identifies as Hispanic. Bachelor's or Higher is a binary variable equal to 1 if the mother has at least a bachelor's degree. Black is a binary variable equal to 1 if the mother identifies as Black.

Table 2.3: Effect of Gunshot Noise on Very Low Birth Weight and Very Preterm (OLS)

				Working Hours		Non-Working Hours	
	All Mothers	Low Education	High Education	Low Education	High Education	Low Education	High Education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Very Low Birth Weight							
Gunshots	0.039	0.333*	-0.144	-0.190	-0.178	0.383**	-0.257
	(0.139)	(0.176)	(0.286)	(0.213)	(0.966)	(0.181)	(0.266)
Mean of Dependent Variable	1.115	1.125	0.778	1.125	0.778	1.125	0.778
Observations	38,373	27,272	5,622	27,272	5,622	27,272	5,622
Panel B: Very Preterm							
Gunshots	0.182	0.446**	-0.243	0.008	0.872	0.377*	-0.723*
	(0.152)	(0.190)	(0.388)	(0.242)	(1.131)	(0.194)	(0.381)
Mean of Dependent Variable	1.348	1.360	0.976	1.360	0.976	1.360	0.976
Observations	38,373	27,272	5,622	27,272	5,622	27,272	5,622
FE: Census Block	X	X	X	X	X	X	X
FE: Year by Month	X	X	X	X	X	X	X

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered by census-block. Panel A estimates the effect of gunshot noise on Very Low Birth Weight, defined as a birth below 1,500 grams. Panel B, estimates the effect of gunshot noise on Very Preterm Birth, defined as a gestation length less than 32 weeks. The treatment variable, Gunshots, is an indicator equal to one if a gunshot is detected during pregnancy. All coefficients multiplied by 100 for ease of interpretation. Low Education is defined as having less than a bachelor's degree, while High Education is defined as having a bachelor's degree or higher. Moreover, Working Hours is defined as experiencing a gunshot between the hours 9:00am-5:00pm during pregnancy. All models include census-block, year-by-month fixed effects, the set of controls as defined in Table 1.

Table 2.4: Effect of Gunshot Noise on Prenatal Visits (OLS)

	Low Education			High Education		
	Smoking	Visits	Delayed Care	Smoking	Visits	Delayed Care
	(1)	(2)	(3)	(4)	(5)	(6)
Gunshots	0.003	-0.031	0.006	0.001	-0.154	0.007
	(0.002)	(0.065)	(0.007)	(0.002)	(0.141)	(0.011)
Mean of Dependent Variable	0.020	10.963	0.174	0.002	11.113	0.092
Observations	27,232	27,024	27,024	5,615	5,608	5,608
FE: Census Block	X	X	X	X	X	X
FE: Year by Month	X	X	X	X	X	X

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered by census block. Low education are mothers with less than a bachelor's degree, while High Education are mothers with a bachelor's or higher. The outcome variables are Smoking, Visits, and Delayed Care. Smoking is a binary equal to 1 if a mother engaged in smoking behavior during pregnancy. Visits is a continuous variable denoting the number of prenatal care visits a mother attended. Delayed Care is a binary variable equal to 1 if the initial visit occurs after the first trimester. Specifically, Delayed Care is equal to one if a mother receives care only during trimesters 2 and 3. Gunshots is a binary variable equal to one if a mother experienced a gunshot as detected through ShotSpotter during their pregnancy. All models include census-block, year-by-month fixed effects, the set of controls as defined in Table 1.

Table 2.5: Effect of Gunshot Noise on Other Birth Outcomes (OLS)

	Birth Weight	Low Birth Weight	Gestation Length	Preterm
	(1)	(2)	(3)	(4)
Gunshots	4.002	-0.748*	-0.040	0.063
	(10.068)	(0.442)	(0.037)	(0.518)
Mean of Dependent Variable	3,296.515	6.556	38.646	8.753
Observations	27,272	27,272	27,272	27,272
FE: Census Block	X	X	X	X
FE: Year by Month	X	X	X	X

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered by census block. Coefficients in columns (2) and (4) multiplied by 100 for ease of interpretation. The sample includes only low-education mothers (no bachelors degree attained). All models include census-block, year-by-month fixed effects, the set of controls as defined in Table 1. Gunshots is a binary variable equal to one if a mother experienced a gunshot as detected through ShotSpotter during their pregnancy. Birth Weight is measured in grams, while Low Birth Weight is an indicator equal to 1 if an infant's weight is less than 2,500 grams. Gestation Length is measured in weeks, and Preterm is a binary variable equal to 1 if the gestation length is less than 37 weeks.

Chapter 3

Motivating Academic Success: The Role of Leaderboards in Shaping Student Study Behaviors

3.1 Introduction

Leaderboards, which visually display real-time rankings and offer instant performance feedback, have been widely implemented across a variety of contexts. These include athletic competitions (golf, tennis, chess, etc.), web-based educational platforms (Khan Academy, Coursera, Duolingo, LeetCode, etc.), ride-sharing mobile applications (Ai et al., 2023) and platforms for public contributions (Chen et al., 2017). These tools are intended to motivate individuals and enhance engagement by gamifying experiences. However, leaderboards have also been found to lead to a decline in performance in other contexts, such as high school classrooms (Bursztyn and Jensen, 2015; Bursztyn et al., 2019). This has been attributed to disclosure of real names and identities on the leaderboard, which subsequently discouraged some individuals from further competition. Nonetheless, these results do not necessarily imply that leaderboards as such are an unreliable incentive tool. It may simply be that certain settings are particularly sensitive to specific leaderboard features, such as lack of anonymity.

In this paper, we explore if an anonymized leaderboard can effectively incentivize students to enhance their study behaviors and achieve greater academic success. The field experiment is conducted in the context of an undergraduate Economics course at a large public university in California. We leverage an online autograding platform through which students submit their weekly self-paced assignments. Within the treatment group, students have access to a leaderboard that ranks them based on submission time, conditional on successfully completing their assignment. Within the the control group, students do not have such access to the leaderboard.

We find that the implemented leaderboard positively shapes students' study behaviors. First, we report that exposure to the leaderboard reduces assignment completion times. These effects are substantial in magnitude, with an over 15 hour and a nearly 20 hour reduction for the first two treated assignments. The effects persist–although smaller–throughout the quarter. Second, we also find that treated transfer students and treated male students experience an improvement in their overall course grade. This is particularly relevant given that transfer students encounter unique challenges when adapting to a new academic environment, which could hinder their ability to achieve the grade they desire.

We contribute to several lines of work. First, we extend the literature directly evaluating how leaderboards as well as other ranking systems affect effort provision in various contexts. Past research in this space has largely relied on publicizing individual performance data to generate sufficient social pressure to impact behavior (Bursztyn and Jensen, 2015; Bursztyn et al., 2019; Gill et al., 2019; Hudja et al., 2022; Chetty et al., 2014). We focus on a leaderboard that is *anonymous*, i.e., it does not reveal true identities of participants and instead relies on confidential pseudonyms selected by the participants themselves.

Second, we contribute to the literature exploring how symbolic rewards affect behaviors. Prior scholarship has documented that interventions such as thank you cards are successful at increasing effort provision and improving performance (Bradler et al., 2016; Kosfeld and Neckermann, 2011). Despite the fact that in our setting one's rank is not associated with any "real" rewards, such as extra course credit, the leaderboard still induces a significant behavioral change. In consequence, achieving a high rank on a leaderboard may also act as symbolic reward.

Third, more narrowly, we contribute to the scholarship documenting the various ways of gamifying academic assessments and the effects of such gamification (Markopoulos et al., 2015; Chang and Wei, 2016; Subhash and Cudney, 2018; Dichev and Dicheva, 2017; Buckley and Doyle, 2014). While the leaderboard itself already introduces an element of gamification, we amplify this by enabling students to design their own playful pseudonyms that are displayed on the leaderboard instead of their real names.

Fourth, we extend the line of work on procrastination in the academic context (Schiming, 2012; Hen and Goroshit, 2018; Dewitte and Schouwenburg, 2002). We show that a leaderboard which ranks individuals based on not only submission accuracy but also submission speed can be an effective tool at reducing procrastination.

Finally, past research documents that interventions such as mentoring programs can help individuals from groups underrepresented in the field of Economics at relatively later stages of their careers, i.e., post graduation (Ginther et al., 2020; Ginther and Na, 2021). We show that leaderboards have heterogeneous effects across student subgroups and may thus be used as tools that potentially help foster diversity within the profession at earlier career stages (pre-graduation).

The remainder of this paper is structured as follows: Section 2 introduces the background information and outlines the general setup of the field experiment. Section 3 offers an overview of the collected data and conducts a balance check. Section 4 delineates our hypotheses, while Section 5 describes our identification strategy. Section 6 presents the main findings of our analysis. Finally, Section 7 provides a conclusion and discusses the implications of our results.

3.2 The Experiment

3.2.1 Setting

We conducted the experiment within an undergraduate Economics course taught at a large public university in California. The university is designated as Hispanic serving; it also hosts a non-trivial number of transfer students from the local community college as well as international students.

The economics course is required for all prospective Economics majors and covers topics such as probability and probability distributions, sampling and sampling distributions, and hypothesis testing. The course is offered every quarter throughout the academic year but tends to achieve its highest enrollment levels in the Fall quarter, which is when we carried out the study. The course is usually taught by graduate student instructors, and students are split into sections capped at 50.

In Fall 2023, when we carried out the experiment, there were 642 students enrolled across 15 different sections. As this is an introductory pre-major course, enrolled students predominantly consist of sophomores and juniors. Majority of enrollments are typically finalized before the first day of instruction but a small number of students tend to drop the class or join from wait list during the first week of the quarter. To account for this, we recruited participants during the second week of the quarter.

3.2.2 Excel Assignments

The final course grade is calculated based on student's performance on a number of assessments: a midterm and a final exam, short lecture quizzes, group activities performed in class, self-paced homework modules, and online Excel assignments.

Online Excel assignments are short applied projects which students solve using Microsoft Excel. Excel assignments are typically due on Friday each week of class, with a penalty-free grace period extending until Sunday at midnight. Prompts for each Excel assignment are released to students periodically throughout the quarter. Data used in each assignment is unique to each student and is generated using a custom-built R script. The data is made available to students through a website administered by the instructors (see Appendix B.1 for a screenshot of the assignment download site). The release and due times for all assignments were made available to students in advance.

Each Excel assignment is graded out of 5 points and frequently involves both computational and graphing questions. Assignments are self paced in the sense that students can spend as much time as they wish working on them as long as the grace period has not elapsed. Additionally, students can submit assignments multiple times and only the score achieved on their last submission counts towards the course grade calculation. Usually, students have around 10-14 days to work on each Excel assignment, i.e. there is 10-14 day gap between the time when a prompt is made available to students and the time when grace period finishes.

Performance on Excel assignments can substantially impact a student's course grade as seven best scores across nine assignments in total contribute 28% of the overall grade. To assist students as they work on Excel assignments, experienced peer helpers employed by the department hold weekly office hours dedicated specifically to answering questions on each week's Excel assignment. Students enrolled in the course also receive general

Excel instruction during weekly discussion sections led by graduate student teaching assistants.

3.2.3 Leaderboard

Excel assignments are submitted by students through an online autograding platform, which allows for immediate feedback on performance on the computational part of the assignment.[1] The platform allows instructors to include a leaderboard as a separate subpage feature of each assignment page (a leaderboard is by default hidden unless it is explicitly enabled).

We customize the leaderboard on the platform so that it ranks all submissions by the submission time among the class members, i.e., the earlier the submission time, the higher the rank on the leaderboard. At the same time, we constrain the leaderboard to only rank those who receive a full score on the autograded (computational) part of the assignment. For example, if Alice and Bob both receive a full score, and Alice submits earlier, then Alice will appear higher on the leaderboard than Bob. A student will appear at the bottom of the leaderboard if the student's submission scores less than a full score on the autograded part of the assignment.

In place of actual student names, the leaderboard displays student-selected leaderboard pseudonyms. This is done for two reasons: first, to preserve confidentiality of each student's performance on the assignments, and second, to further gamify the experience for students by enabling them to select playful names they enjoy using and identify with. Students get to input their leaderboard pseudonym upon submitting their Excel assignment. As leaving the leaderboard pseudonym blank would cause a submission not to be processed, students are informed that if at any point they no longer wish their

[1] Students are required to submit their solutions to computational and graphing problems separately. While the autograding platform provides instant feedback on performance on the former, the latter are graded manually after the grace period elapses.

leaderboard pseudonym to be uniquely identified, they can type Optout Otter as their leaderboard pseudonym instead.

It is important to note that after they submit their Excel assignment, students are not automatically redirected to the leaderboard page but instead need to navigate to it intentionally. Figure 3.1 presents an illustrative screenshot of the leaderboard as viewed from the student perspective.

3.2.4 Recruitment and Randomization

We conducted the study in the Fall quarter of academic year 2023/2024. The recruitment process took place during the second week of classes, to mitigate the effect of students joining the class from the wait list or dropping the class during the first week of classes. During in-person lectures, we distributed consent forms to students, along with a survey requesting basic demographic information. To encourage participation, students were informed that they would be entered into a lottery for a $25 gift card, with odds of winning estimated at approximately 1 in 50. It is important to note that apart from the lottery drawing, there were no other incentives provided to the participants, such as extra credit for class.

Of the 642 students enrolled in the course, 425 consented to participate in our study. Students were randomly divided into a treatment group and a control group. We decided to further split treated students into 50-student leaderboards to allow for relatively high mobility potential along the leaderboard and relatively high familiarity with others on the same leaderboard. Consequently, participants were randomly assigned to one of eight mutually exclusive and collectively exhaustive groups, each linked to a unique course page on the autograding platform. Of these, seven groups were designated as treatment groups and had access to the leaderboard, thereby differentiating them from the control group, which did not have access to the leaderboard.

For all Excel assignments, students assigned to the control group received standard prompts which outlined the requirements of each assignment and did not mention the leaderboard in any way. In addition to the standard prompts, students assigned to the treatment group received extra instructions which provided an overview of the leaderboard setup. In particular, the instructions explained how the leaderboard ranks individual student submissions, where to find it on the online submission platform, and how to select pseudonyms for the leaderboard.[2]

3.3 Data

Our data were sourced from the demographic survey completed by students immediately following their consent to participate in the experiment, the platform for Excel assignment submissions, and the course records. The demographic survey provided basic demographic information about the students, such as age, gender, international status, and transfer status. Information regarding the Excel assignment submissions was collected directly from the autograding platform. Lastly, the course records provided information on the overall grades students achieved in the course.

3.3.1 Balance Check

Our sample consists of 425 students, with 236 assigned to the treatment group (i.e., with access to the leaderboard) and 189 students assigned to the control group (i.e., without access to the leaderboard). Table 3.1 provides a summary of the demographic characteristics of students in both the treatment group (Column 1) and control group

[2] Appendix B.2 contains the exact text delivered to the treatment group within the instructions for their Excel assignments.

(Column 2). Slightly over 40% of the students in our sample are male, about a half are transfer students, and over 90% are either sophomores or juniors. We report no statistical difference between the treatment group and the control group across nearly all demographic characteristics, except for Hispanic/Latino and International status (marginally significant).

3.3.2 Excel Assignment Submissions

For our outcome variables, we leverage information on various aspects of students' submissions for the nine Excel assignments, including the assignment release time and submission time, assignment scores, and leaderboard ranks.

Across the nine Excel assignments, we focus on student performance on assignments 2 through 7. This is because the prompt for Excel assignment 1 is released in the very first week of classes, prior to when students are introduced to the leaderboard. Moreover, the first assignment is designed to be straightforward, primarily to acquaint students with the autograding platform. For these two reasons, we anticipate no significant difference between the treatment and control groups in terms of completion times. As such, the first assignment also serves as a placebo test to verify the comparability of the treatment and control groups at the outset of the study.

Furthermore, recall that only the best seven scores on the nine Excel assignments count towards to the final course grade. Therefore, a student may choose to delay or altogether skip the submission for the last two assignments if they manage to achieve full marks on the initial seven assignments. Consequently, we omit assignments 8 and 9 from our primary analyses to account for this behavior.

3.4 Hypotheses

In this section, we develop our hypotheses within the context of the field experiment's framework.

Hypothesis 1 (Immediate Submission Effect). Following their initial exposure to the leaderboard, students in the treatment group will submit the subsequent Excel assignment more promptly than their counterparts in the control group.

After submitting their first Excel assignment, students in the treatment group will encounter the leaderboard for the first time. We anticipate that this initial engagement with the leaderboard–as well as the novelty of the leaderboard–will motivate students to improve their rank for the second assignment (if their original position is low) or to maintain their original rank (if their original position is high). Consequently, we hypothesize that students interacting with the leaderboard will, on average, finish Excel assignment 2 more quickly than their counterparts in the control group.

Hypothesis 2 (Persistent Submission Effect). Students in the treatment group will consistently complete their Excel assignments faster than students in the control group throughout the academic quarter.

A new leaderboard is created for each Excel assignment, a deliberate design choice to provide all students in the treatment group with a clean slate for each assignment. This mechanism serves to motivate students as everyone can rank highly on a new leaderboard, regardless of their prior rank. Accordingly, we hypothesize that the leaderboard will continue to affect assignment completion times throughout the quarter, i.e., from the second through the seventh Excel assignment. In other words, we anticipate that the leaderboard will have a lasting impact on students' study behaviors, beyond just the immediate effect posited in Hypothesis 1.

Hypothesis 3 (Academic Performance Effect). The leaderboard will not only positively affect assignment completion times but will also improve the overall performance in the class, leading to a higher final course grade.

The effects of leaderboard exposure may extend beyond just Excel assignments. Students may develop a habit of initiating their work on various tasks early on. For example, students may start studying for a test earlier than they otherwise would have. Similarly to the benefits associated with starting Excel assignments ahead of time, this may provide students with more opportunities to seek (and receive) assistance from instructors and Teaching Assistants, leading to better performance overall. At the same time, because the treatment is not associated with any financial incentives or extra academic credits, we do not expect these effects to be very high in magnitude. Therefore, we expect the leaderboard intervention to exert a moderate indirect effect on students' final course grades.

Hypothesis 4 (Heterogeneous Effects on Different Demographic Groups). The leaderboard will have heterogeneous academic performance effects across different demographic groups.

Different demographic groups may respond to the leaderboard differently. We expect students who have prior experiences with various types of leaderboards, e.g. through gaming platforms or athletic events, to be more strongly affected than students who do not have such experiences. We observe in the data we collected through the demographic survey that male students more frequently report being engaged in video games and sports. Thus, we expect the effects of the leaderboard to be concentrated among male students.

In addition, our field experiment takes place at a university with a substantial population of transfer students from community colleges. Transfer students may be particularly responsive to interventions that encourage engagement. We hypothesize that the leaderboard will have stronger effects among transfer students than non-transfer students.

3.5 Identification

In this section, we specify our identification strategy.

To estimate the average treatment effect of exposure to a leaderboard on study habits, we first estimate the following OLS regression model:

$$Y_i^j = \alpha + \beta Treat_i + \gamma X_i + \epsilon_{ij} \tag{3.1}$$

where Y_i^j denotes the number of hours taken by student i to submit their Excel assignment j calculated as the difference between the time when the assignment prompt is released and the time when student i makes their last submission for assignment $j \in \{1, 2, \ldots, 7\}$. $Treat_i$ is a binary indicator equal to one if a student is assigned to the treatment group (i.e., exposed to the leaderboard) and equal to 0 if a student is assigned to the control group (i.e., not exposed to the leaderboard). Therefore, β, which is the main parameter of interest, indicates the effect of being assigned to treatment on assignment completion time. X_i is a vector of demographic controls that include student's gender, age, race, ethnicity, major, transfer and international status. Wild-bootstrap standard errors are clustered at the level of enrollment section. The model is estimated separately for assignments one through seven, with assignment one being treated as a placebo as it was released prior to initial exposure to the leaderboard.

Furthermore, to estimate the average treatment effect of the leaderboard intervention on students' academic performance, i.e., the final course grade, we then estimate the following regression model:

$$Y_i = \alpha + \beta Treat_i + \gamma X_i + \epsilon_i \tag{3.2}$$

where Y_i denotes student i's final course grade operationalized either as a z-transformation of the overall number of points accumulated by student i throughout the duration of the class or as the raw number of points accumulated by student i throughout the duration of the class.

To allow for heterogeneous effects by transfer status and gender, we also estimate supplementary regression models in which we include interaction terms between a student's treatment assignment as well as their transfer status and gender.

3.6 Results

In this section, we present the results from our field experiment. We first examine the effect of the leaderboard intervention on students' assignment completion times. We then examine the impact of exposure to a leaderboard on students' academic performance. Finally, we end with a discussion of heterogeneous effects on transfer and male students.

Figure 3.2 plots the average assignment completion times separately for the treated leaderboard group (purple line) and the non treated no leaderboard group (orange line) across assignments 1 through 7. It is worth noting here that Figure 3.2 merely shows the raw means across the treatment and control groups and does not include any demographic controls. The average completion times for students in the treatment group fall consistently below the average submission times of students in the control group. The only exception is Excel Project 1 which was released prior to the initial exposure to the leaderboard and can be therefore considered a placebo. This provides suggestive evidence in support of both Hypothesis 1 (Immediate Submission Effect) and Hypothesis 2 (Persistent Submission Effect).

Figure 3.3 provides the effects of being assigned to treatment on assignment completion time estimated using equation (1). Recall that completion time is defined as the difference between the last submission time and assignment release time. The effects are estimated separately for assignments 1 through 7. For assignment 1, we observe no statistical difference in assignment completion times between the treatment and the control group. As explained before, this is intuitive as assignment 1 can be interpreted as a placebo. In the case of subsequent Excel assignments, exposure to the leaderboard is associated with a statistically significant reduction in assignment completion time for all assignments except for assignment 6. The prompt for assignment 6 was released soon before the midterm exam. Given that the midterm exam score has a larger impact on the overall course grade than an individual Excel assignment, students might have prioritized preparing for the midterm and simultaneously postponed completing the assignment. The effects on assignments 2, 3, 4, 5, and 7 are substantial in magnitude. Notably, in the case of Excel Assignment 2, being assigned to the treatment group resulted in a over 15-hour reduction in assignment completion time, which corresponds to over half a day. The strong immediate effect of the leaderboard on assignment 2 completion time provides support for Hypothesis 1. This effect, although decreasing in magnitude, persists throughout the quarter. Even in the case of assignment 7 (the last assignment we consider, which is due almost two months after the initial exposure to the leaderboard), the reduction in completion time is still significant (over 10 hours). The persisting effect of the leaderboard on completion time provides evidence for Hypothesis 2.

Is the leaderboard also associated with a better performance in the class overall? Table 3.2 presents the effects of the treatment (leaderboard) on z-transformed final course grades, estimated using equation (2). We report no significant effects of the treatment on total course grades (Columns 1 and 2). Although going against Hypothesis 3, these results are not surprising. When the sample as a whole is considered, exposure to the leaderboard may be too light-touch of an intervention to result in a change in behavior extending beyond assignment completion times, which would subsequently improve the final course grades. It is unlikely that all students in the sample respond to the treatment in the same way, which could affect the overall treatment effect. However, certain demographic groups may be more sensitive to exposure to the leaderboard than others. Therefore, in columns 3 through 8, we explore whether the leaderboard has different effects on subgroups of students.

There appears to be substantial treatment heterogeneity for transfer/non-transfer students as well as male/female students, as put forward in Hypothesis 4. Columns 3 and 4 provide the coefficient estimates when the baseline model in equation (2) is extended by adding an interaction term between a student's transfer status and treatment assignment. Transfer students assigned to the treatment group appear to achieve higher overall course grades relative to their peers assigned to the control group.[3] This finding is particularly important given that transfer students are often considered as being at a higher risk of attrition from the major track than non-transfer students due to the additional challenges associated transitioning from another school. Similarly, columns 5 and 6 provide the coefficient estimates when the baseline model in equation (2) is extended by adding an

[3]P-values from the F-test for joint significance significant at the 10% level.

interaction term between a student's gender and treatment assignment. Male students assigned to the treatment group tend to achieve higher overall course grades as compared to their peers assigned to the control group.[4] These effects are non-trivial in magnitude. To put this is more context, we replicate the analyses presented in the previous table with raw final course grade as the outcome (as opposed to z-transformed course grades). Table 3.3 presents the effects of the treatment on the final course grade when the final course grade is operationalized as the raw number of points accumulated throughout the course across all assignments. As being assigned to the treatment increases the overall course grade by around 3 points for transfer students, the effect is large enough to move up a letter grade (e.g., from B- to B).

3.7 Conclusion

We conducted a field experiment in a large undergraduate Economics course to investigate whether gamifying online Excel assignments through implementation of leaderboards will affect assignment completion times and overall course performance. Our results indicate that students assigned to the treatment group exhibit faster assignment completion times, suggesting that the leaderboard positively alters students' study behaviors. At the same time, while we do not find significant effects of the leaderboard on performance in the class in the context of the entire sample, we report that transfer students as well as male students exposed to the leaderboard achieve higher overall course grades than their peers in the control group. These results are particularly interesting given that the intervention (leaderboard) is light-touch (i.e., students are not required to check the leaderboard) and costless (i.e., implementing the leaderboard on the assignment submission platform requires no additional fees as well as minimal effort upfront

[4]P-values from the F-test for joint significance marginally significant at the 10% level.

and virtually no effort associated with maintenance).

Our study, like all studies, has its limitations. First, we capped the leaderboards at 50 students which we hoped would create a sufficient potential for mobility along the leaderboard without alienating the low performers. However, future work should explore more closely the relationship between leaderboard size and leaderboard effects to arrive at a more precise optimum. Second, in our study we did not have the technical infrastructure to automatically redirect students to the leaderboard upon assignment submission, nor did we have access to the information on if and how frequently individual students chose to inspect the leaderboard. Future research may experiment with various leaderboard settings, while attempting to collect more data on how students interact with the leaderboard. Third, our study was limited to a single class. It would be interesting to explore if leaderboards implemented in several courses at once crowd each other out or amplify each other's effects.

Despite these limitations, our findings still provide important insights into how leaderboards can be used as incentives to shape behaviors. More narrowly, our results can be directly used to guide how instructors design their assignments with the goal of promoting engagement within digital learning spaces and fostering positive study habits among students.

Figures

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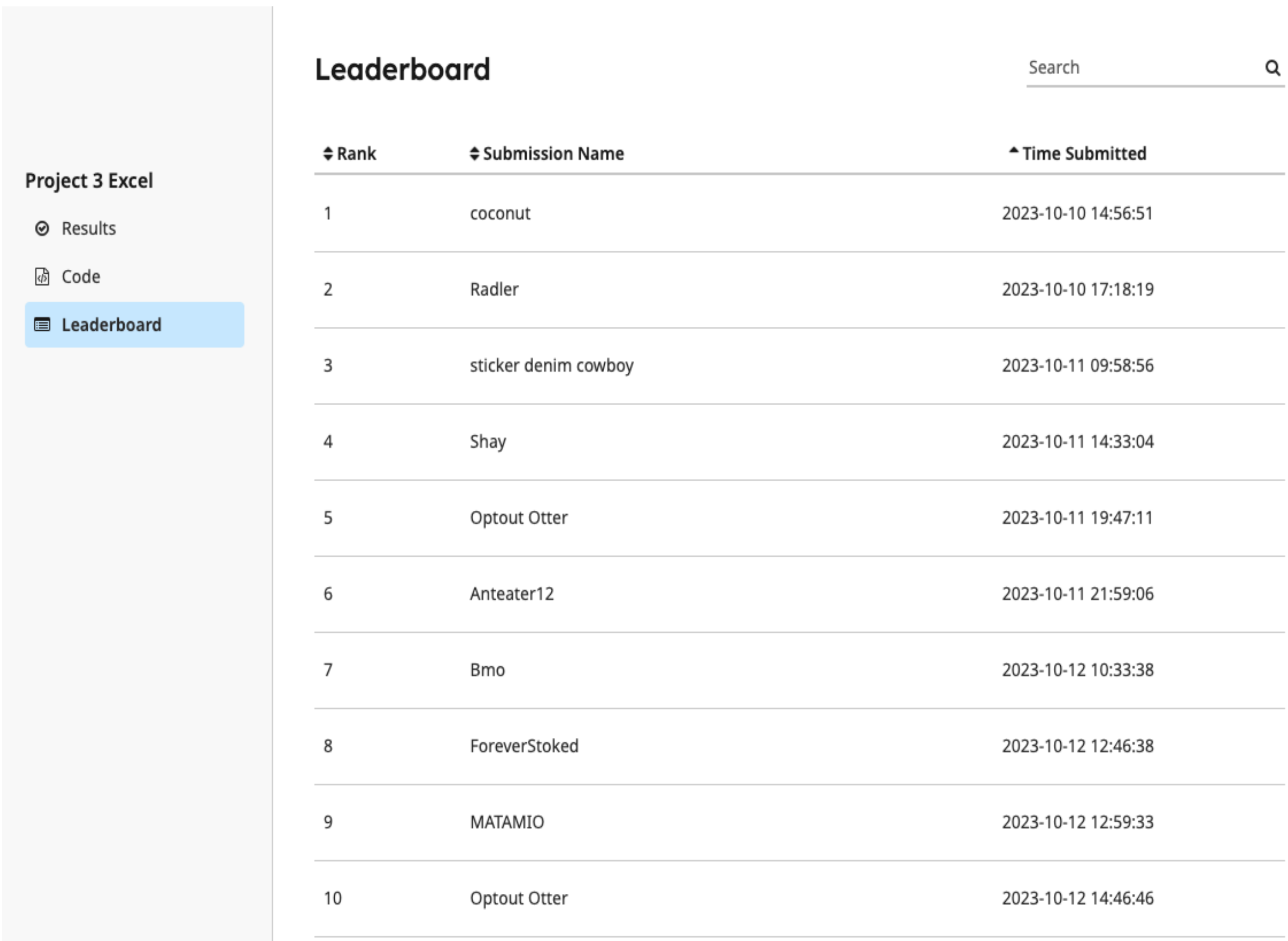

Figure 3.1: Leaderboard Viewed From a Student Perspective

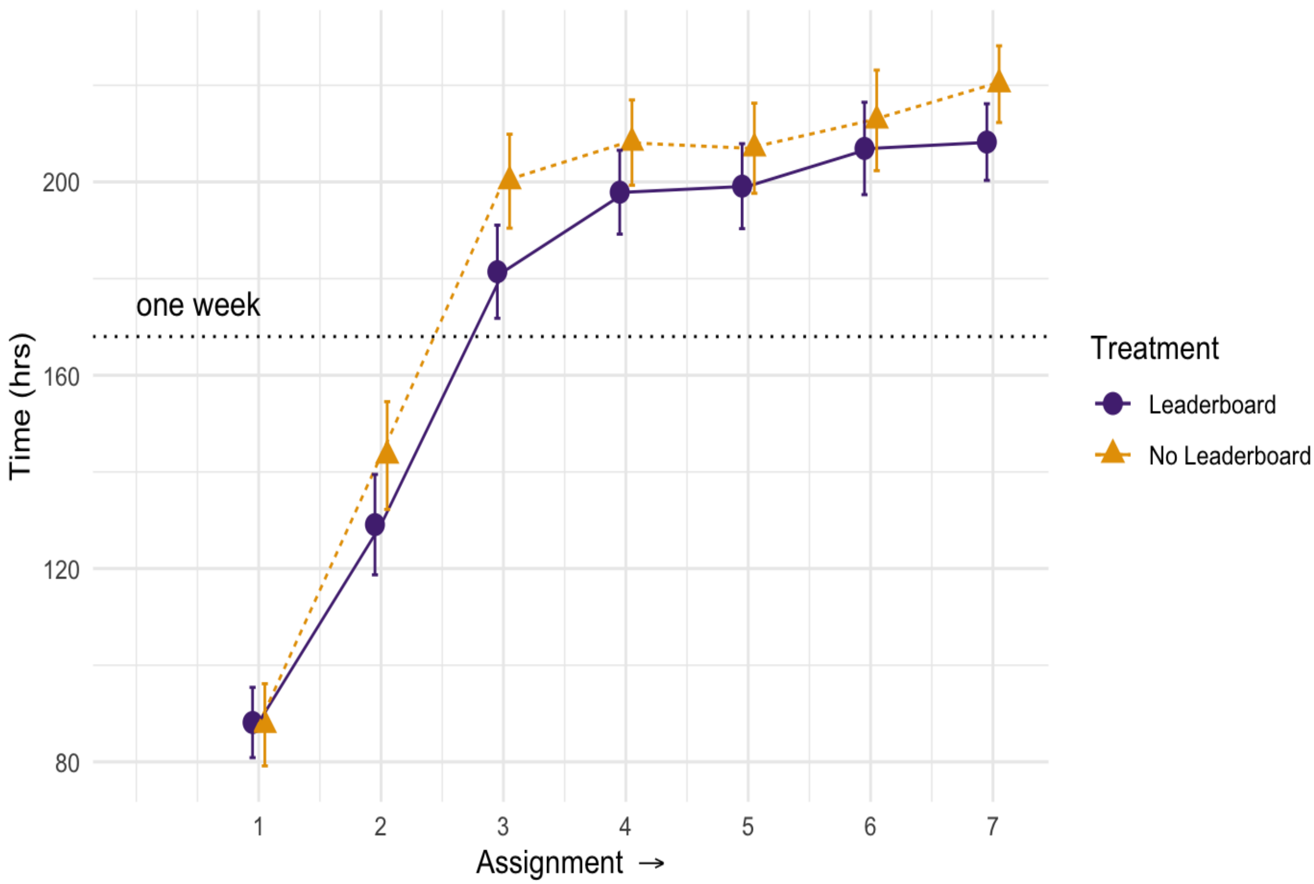

Figure 3.2: Average Completion Times for Treatment (Leaderboard) Group and Control (No Leaderboard) Group Across Assignments 1 Through 7

Note: Purple circles denote raw mean completion times for students in the treatment group (no controls included). Orange triangles denote raw mean completion times for students in the control group (no controls included). Whiskers denote 90% confidence intervals. Assignment 1 provides a placebo test (no expected difference between treatment and control group). Completion time is defined as the difference between assignment release time and the last submission made by the student.

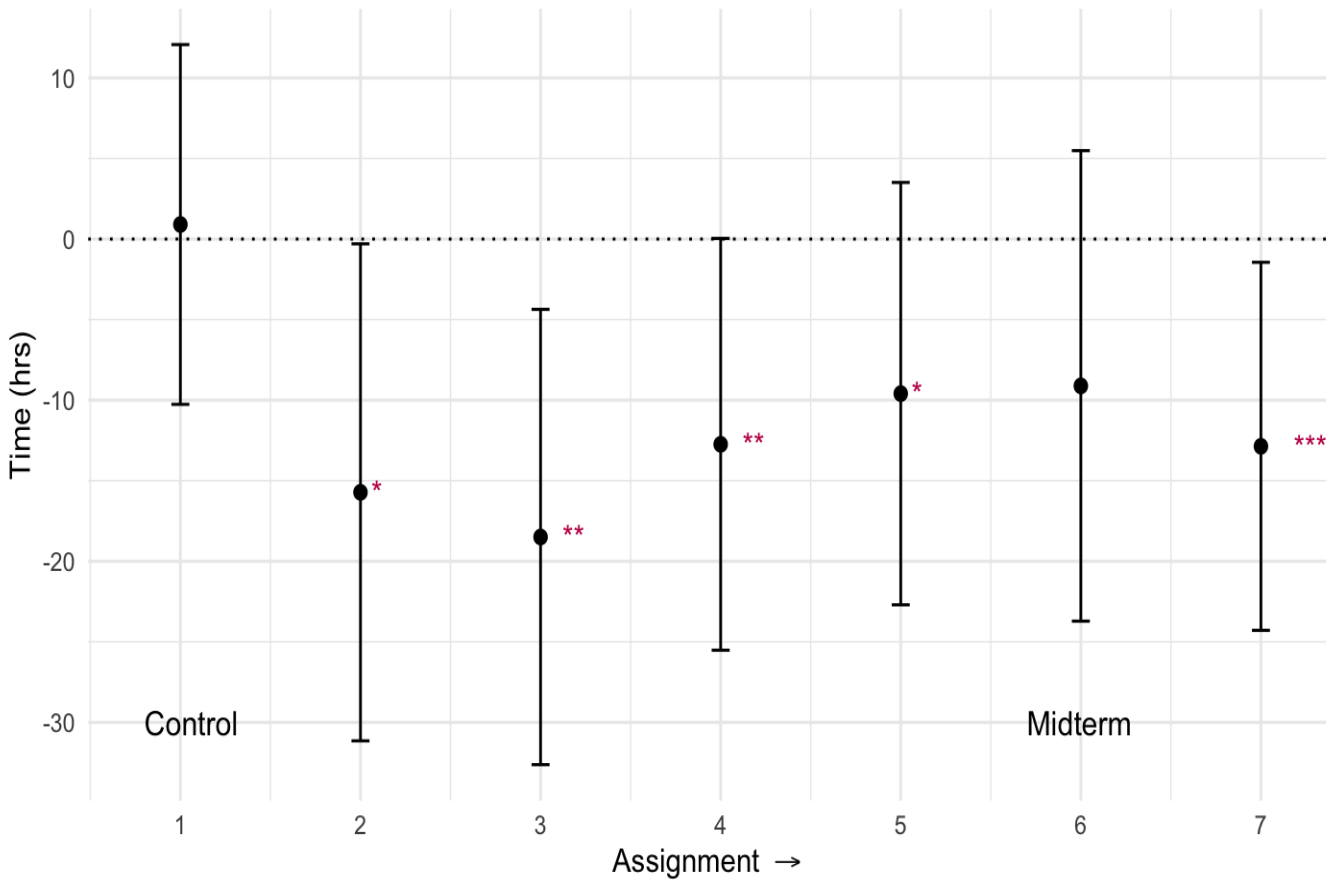

Figure 3.3: Regression Estimates of the Treatment Effect (Leaderboard Exposure) on Completion Times Across Assignments 1 Through 7

Note: Circles denote coefficients estimated using equation (1). Whiskers denote 90% confidence intervals. * denotes significance at the 10% level, ** denotes significance at the 5% level, and *** denotes significance at the 1% level. Controls include student's gender, age, race, ethnicity, major, transfer and international status. Wild-bootstrap standard errors are clustered at the level of enrollment section. Assignment 1 provides a placebo test (no expected difference between treatment and control group).

Tables

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Table 3.1: Balance Check

	(1) Leaderboard Mean (Std.)	(2) No Leaderboard Mean (Std.)	(3) Mean Diff. (1) - (2)
Male	0.413 (0.496)	0.421 (0.495)	-0.008
Transfer	0.504 (0.518)	0.481 (0.501)	0.023
Age 18 to 20	0.778 (0.416)	0.760 (0.429)	0.019
Age 21 to 24	0.209 (0.407)	0.213 (0.411)	-0.004
Age 25 or more	0.013 (0.114)	0.027 (0.164)	-0.014
Major Econ	0.565 (0.497)	0.546 (0.499)	0.019
Major Econ/Acc	0.400 (0.491)	0.383 (0.487)	0.018
Major Other	0.022 (0.146)	0.049 (0.217)	-0.028
Major Undecided	0.013 (0.114)	0.022 (0.147)	-0.009
Hispanic/Latino	0.135 (0.342)	0.208 (0.407)	-0.073*
Race White	0.509 (0.501)	0.443 (0.498)	0.066
Race Black	0.057 (0.231)	0.049 (0.217)	0.007
Race Asian	0.344 (0.476)	0.372 (0.485)	-0.028
Race Other	0.091 (0.289)	0.137 (0.344)	-0.045
Year Freshman	0.039 (0.194)	0.044 (0.205)	-0.005
Year Sophomore	0.430 (0.496)	0.432 (0.497)	-0.001
Year Junior	0.509 (0.501)	0.497 (0.501)	0.011
Year Senior	0.022 (0.146)	0.027 (0.164)	-0.006
International	0.087 (0.282)	0.153 (0.361)	-0.066*
N (Students)	236	189	

Table 3.2: Effect of Treatment (Leaderboard) on z-Transformed Final Course Grades

	(1)	(2)	(3)	(4)	(5)	(6)
Leaderboard	0.032	-0.022	-0.132	-0.198	-0.114	-0.178
	(0.104)	(0.096)	(0.116)	(0.116)	(0.152)	(0.145)
Transfer			-0.709***	-0.588***		-0.394**
			(0.136)	(0.190)		(0.180)
Leaderboard * Transfer			0.344**	0.364**		
			(0.154)	(0.157)		
Male				-0.051	-0.092	-0.250
				(0.129)	(0.158)	(0.166)
Leaderboard * Male					0.350**	0.373**
					(0.148)	(0.147)
Additional Controls	-	✓	-	✓	-	✓
Observations	413	413	413	413	413	413

Note: Significant at the *10%, **5%, and ***1% levels. Robust standard errors for clustered data in parentheses. Constants not displayed. Additional Controls: Age, Hispanic/Latino, Race, Major, International Student.

Table 3.3: Effect of Treatment (Leaderboard) on Raw Final Course Grades

	(1)	(2)	(3)	(4)	(5)	(6)
Leaderboard	0.438 (0.144)	-0.304 (1.322)	-1.820 (1.610)	-2.739 (1.604)	-1.570 (2.100)	-2.452 (1.995)
Transfer		-5.300** (2.432)	-9.780*** (1.880)	-8.119*** (2.619)		-5.432** (2.481)
Leaderboard * Transfer			4.740** (2.120)	5.026** (2.167)		
Male		-0.619 (1.782)		-0.704 (1.774)	-1.270 (2.180)	-3.457 (2.287)
Leaderboard * Male					4.830** (2.040)	5.150** (2.026)
Additional Controls	-	✓	-	✓	-	✓
Observations	413	413	413	413	413	413

Note: Significant at the *10%, **5%, and ***1% levels. Robust standard errors for clustered data in parentheses. Constants not displayed. Additional Controls: Age, Hispanic/Latino, Race, Major, International Student.

Appendix A

Appendix to Chapter 1

A.1 Additional Figures and Tables

Table A.1: ICD-9 and ICD-10 Diagnosis Codes for Chest Pain.

Description	**ICD-9 Code**	**ICD-10 Code**
Chest pain on breathing	R07.1	78652
Precordial pain	R07.2	78651
Other chest pain	R07.8	78659
Chest pain, unspecified	R07.9	78650

Source: Transition from ICD-9 to ICD-10 occured in 2015. Chest pain-related ICD-9 and ICD-10 codes taken from Aalam et al. (2020)

Appendix B

Appendix to Chapter 3

B.1 Additional Figures and Tables

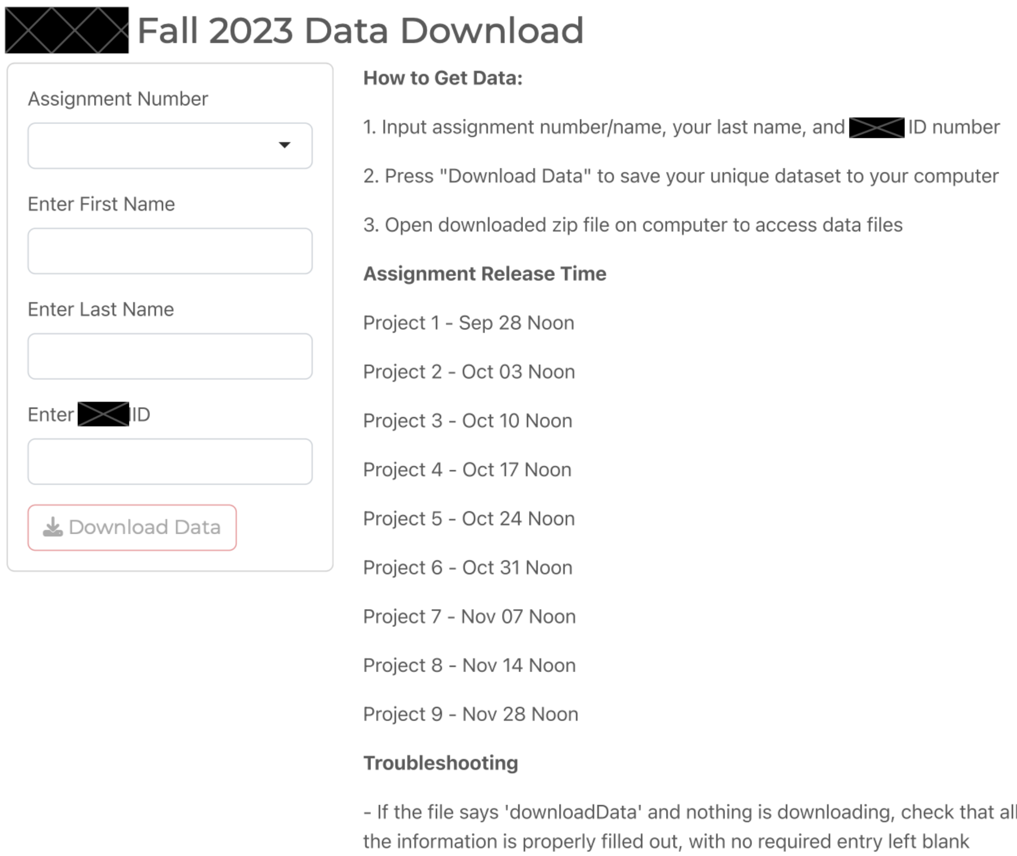

Figure B.1: View of Assignment Download Site

- When uploading the Excel spreadsheet to [the autograding platform], you will be prompted to provide a name for the leaderboard. A leaderboard is a tool that allows you to monitor accuracy and timing of your submission relative to others in the class.
- You can pick your own leaderboard name, please make sure it is appropriate and respectful, given that the leaderboard is visible to other students in the class.
- DO NOT use your real name as your leaderboard name. If you accidentally use your real name as your leaderboard name, submit again, the original leaderboard entry will be deleted.
- The leaderboard will NOT affect your course grade at all. If you don't want your leaderboard name to be uniquely identified, you can type Optout Otter as your leaderboard name.
- DO NOT leave the leaderboard name blank, your submission will not be processed.
- DO NOT share your leaderboard name with other students. A student's leaderboard name is known only to that particular student and course instructors.
- Leaderboard ranks all submissions by the SUBMISSION TIME among the class members. The earlier the submission, the higher the rank. Note though, the leaderboard ONLY ranks those who got FULL SCORE on the autograder part of the assignment. For example, if you and another student both got a full score, and you submitted earlier, your leaderboard name will appear higher on the leaderboard than the other student's. Please notice that your leaderboard name would appear at the bottom of the leaderboard if your submission scored less than FULL SCORE on the autograder part of the assignment.
- After your submission, click on "Leaderboard" on the "Autograder Results page" to check the ranking.

Figure B.2: Extra Instructions for Treatment Group

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