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Police Interventions and Birth Outcomes

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a thesis submitted to the Faculty of Emory College of Arts and Sciences
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Abstract

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Based on existing literature that well establishes the stress of police interventions and the effects of in-utero exposure to stress on birth outcomes, I hypothesize that in utero exposure to local high profile uses of police force will affect infant birth outcomes, particularly in minority populations. Using comprehensive birth data from the National Center for Health Statistics, I perform a difference-in-difference analysis to test this hypothesis. I do not find compelling evidence for a link between the use of force by police and birth outcomes. I present three explanations for the contradiction between my expected and experimental results. The first two explanations revolve around limitations of measures of police violence incidents. Alternatively, I propose that my results may provide evidence for theories of race-related stress that understand this stress as a lifetime chronic factor, as opposed to being tied to individual events.

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1 Introduction

Over the past 40 years, and particularly the last decade, public attention toward the effects of the police's use of force, most notably lethal force directed at minority communities, has surged. Concern surrounding these cases has driven the Black Lives Matter, Defund the Police, and various other movements, which have gained prominence since 2015 (DeGue et al., 2016; Rembert et al., 2016). American police use lethal force at a rate higher than any other industrialized nation (Hirschfield, 2015), and police use of lethal force disproportionately affects Black Americans (Martin and Kposowa, 2019; DeGue et al., 2016; Rembert et al., 2016; Hirschfield, 2015; Alang et al., 2017).

Discussions of the effects of these incidents have expanded to encompass the externalities of police interventions. Public health researchers have drawn attention to the mechanisms by which police brutality can be linked to poor health outcomes among Black Americans (Alang et al., 2017). Police action that is perceived within minority communities as unfair has been linked to decreased mental health outcomes in those same communities (DeVylder et al., 2017; Geller et al., 2014; Cooper et al., 2004). Black individuals are more likely to cite encounters with police as a source of stress (Geller et al., 2014), which has been shown to increase the likelihood of chronic disease and early mortality (Chae et al., 2014). Additional studies have found that Black death due to police intervention can cause poor mental health among Black residents of the state of the incident (Bor et al., 2018), and that residents of neighborhoods with high uses of police force were at higher risk for diabetes and obesity (Sewell, 2017). In summary, it appears that lethal police interventions have significant negative externalities, particularly in the Black community, including effects on mental health, stress levels, and physical health (Association, 2018; Geller et al., 2014; Sewell et al., 2016).

There are several potential mechanisms by which maternal stress can affect an in-utero fetus (Dursun, 2019; Black et al., 2016; Persson and Rossin-Slater, 2018; Hobel et al., 1999). The effects of stress may be transferred between the mother and the fetus due to the production of certain hormones in the mother, compromises to the mother's immune system, inflammation, or effects on fetal neuro-development (Glover et al., 2018; Sosnowski et al., 2018; Hantsoo et al., 2019; Walsh et al., 2019; Abbott et al., 2018; Lima et al., 2018). Regardless of the specific biological mechanisms at play, recent economic literature has attempted to quantify the effect of various exogenous stressful shocks on birth outcomes. Sources of stress have included Arabic named women in California after 9/11 (Lauderdale, 2006), women affected by hurricanes in Texas (Currie and Rossin-Slater, 2013), mothers who experienced the death of a parent in Norway (Black et al., 2016), maternal relative death in Sweden (Persson and Rossin-Slater, 2018), and holiday stress in South Korea (Sohn, 2018). Closer in nature to my study, there have also been findings of significant negative relationships for birth outcomes to mothers exposed to violent crime in New York City (Currie et al., 2018), mass shootings in the United States (Dursun, 2019), violence in Brazil (Foureaux Koppensteiner and Manacorda, 2016), terrorism in Spain (Quintana-Domeque and Ródenas-Serrano, 2017), terrorism in Pakistan (Grossman et al., 2019), civil war in the Democratic Republic of Congo (Dagnelie et al., 2018), community violence in California (Goin et al., 2019), terrorism in Colombia (Camacho, 2008) and neighborhood violence in Sao Paulo (Santos et al., 2020). In summary, the economic literature seems to broadly support that local exogenous stressful shocks, in particular those related to violence, can have a negative effect on birth-weight, gestation, and other birth outcomes.

The literature thus provides significant evidence for two important relationships:

1. Lethal police interventions and negative perceptions of the police are related to increases in expressed and measured levels of stress in Black and minority communities

2. Exogenous stressful shocks can have negative effects on birth outcomes

Taken together these two relationships form the basis for my hypothesis:

Hypothesis 1 *Infants, particularly those born to Black mothers, exposed in-utero to local incidents of police violence will experience negative birth outcomes, such as decreased birthweight, as a result of increases in maternal stress levels.*

In this paper I test this hypothesis following a well established methodology (Dursun, 2019; Currie et al., 2018; Persson and Rossin-Slater, 2018; Black et al., 2016). Using a criteria derived list of 49 cases of police's use of force and birth data from the National Institute of Health National Vital Statistics System (NVSS), I perform a difference - in - difference analysis to estimate the effects of in-utero exposure to police violence on infant health outcomes, measured through birthweight.

I do not find evidence for a relationship between in-utero exposure to uses of police force and negative infant health outcomes, due to both statistical insignificance at conventional levels for the majority of my results, and a lack of meaningful economic interpretation for those that are statistically significant, especially when compared to the prior literature.

I provide three potential explanations for understanding my hypothesis in conjunction with my null result. The first two explanations revolve around limitations of measures of police violence incidents. Alternatively, I propose that my results may provide evidence for theories of race-related stress that understand this stress as a lifetime chronic factor, as opposed to being tied to individual events.

2 Data

2.1 Police Violence Data

Using data from the Mapping Police Violence and the Washington Post Fatal Force databases I assembled a list of 61 uses of police force that fit my criteria. Of the 61 events identified, 49 events took place during the years for which consistent birth data is available. Events were selected according to the following criteria:

- the event received significant local and/or national media attention, where significant media attention was defined as a minimum of:
  (a) publication of more than three local news articles; and
  (b) two nights of coverage on local TV news programs
- the police's use of force resulted in an internal investigation, local county investigation, federal investigation, criminal case, or civil case with repercussions (firing of officer(s) involved, criminal charge(s) or conviction(s), civil ruling or settlement with the family of the victim)
- the victim was either unarmed, or was holding something that, while the officer might have mistaken for a weapon, could be determined by most third party observers not to constitute a weapon (a toy gun, a broom, a twig, a cellphone, etc.)

All victims in my study were minorities, with the vast majority being Black Americans. Figure 1 shows all exposed counties in my benchmark estimation. The figure depicts a map of the United States with counties with incidents of police violence, as defined by the criteria above, shaded in turquoise. The map shows that my events are distributed geographically throughout the United States, with exposed counties in most major areas of the United States. My selection criteria means my study does not include any events in the Pacific Northwest or New England. This is due to there being no events in those areas, as opposed to an internal validity issue. I control for location fixed effects at the county level in my model estimation.

2.2 Birth Data

I use birth data from the U.S. National Vital Statistics System (NVSS) Natality records, which cover all U.S. births. By Federal law the Certificate of Live Birth is filled out for all live births in the United States. These records are then compiled by the Center for Disease Control (CDC) National Center for Health Statistics (NCHS) and made available through the NVSS. Between 1989-2004 public-use versions of NVSS data only include geographic identifiers for counties with a population of 100,000 and greater, and public-use datasets since 2005 do not contain geographic identifiers at the state, county, or city level. Therefore, I applied for restricted-use vital statistics data, which include county level geographic information. The NVSS data includes detailed demographic information on the mother and the condition of the newborn infant. In 2003, several changes were made to the Certificate of Live Birth, a complete summary of which can be found within the NVSS documentation. Due to this change I limit my study to the years 2004-2018, in order to maintain data comparability between years (Health Statistics).

I use variables that identify the mother's residence county, the birthdate, and the obstetric estimate of gestation in defining my exposure and treatment groups. I also use variables that provide information on mother's race, age, educational attainment, live birth order, residence county population, and residency status. My main variable of interest is birthweight, which is presented in the data set in grams.

3 Methodology

To estimate the effects of exposure to use of police force on birth outcomes, I use the following difference-in-difference models:

$$\log(BWT)_{i,c,t} = \beta_0 + \beta_1 * exposure_{i,c,t} + \chi_{i,c,t} + \lambda_c + \gamma_t + \epsilon_{i,c,t} \tag{1}$$

$$\begin{aligned} \log(BWT)_{i,c,t} = \beta_0 + \beta_1 * exposure_{i,c,t} + \beta_2 * exposure_{i,c,t} * Black_{i,c,t} \\ + \beta_3 * exposure_{i,c,t} * Hispanic_{i,c,t} + \chi_{i,c,t} + \lambda_c + \gamma_t + \epsilon_{i,c,t} \end{aligned} \tag{2}$$

where i = infant, c = county, and t = expected birth week-month-year[1]. Birthweight (BWT) is measured in grams. Alternatively, I use Very Low Birth Weight ($VLBWT$) or Low Birth Weight ($LBWT$) as my dependent variable, which are indicator variables I create of births less than 1500 grams and 2500 grams respectively. χ represents a matrix of mother and birth specific covariates, such as mother's race, age, educational attainment, live birth order, residence county population, and residency status. λ represents county specific fixed effects, and γ represents expected birth week-month-year fixed effects. ϵ is the idiosyncratic error term. $Exposure$ and $exposure * race$ are my variables of interest. In my benchmark estimation I limit my sample to White Non-Hispanic, Black, and Hispanic infants. My results are robust to relaxing this limitation. In Equation 1 β_1 captures the effect of exposure to police interventions for my entire treatment group. In Equation 2 β_1 captures the effect of exposure to police interventions for white non-hispanic members of the treatment group. β_2 captures the additional differential effect of exposure to incidents of police violence for Black members of the treatment group. β_3 captures the additional differential effect of exposure to incidents of police violence for Hispanic members of the treatment group.

As gestational length is an outcome intrinsically tied to the same in-utero factors that I expect to affect birth-weight(Matsumoto, 2018; Persson and Rossin-Slater, 2018), *exposure* is determined using expected birth date and mother's residence county. Expected birth date

[1] For confidentiality reasons the NVSS data presents only month and year for birth date. In order to subtract the Obstetric Estimate of Gestation (presented as a number of weeks in the data) from the birth date in month year format I am required to impute a day of birth, after which I can perform all calculations on the week-month-year level (Black et al., 2016). I therefore define exposure and all other variables using the week as my most granular time component. In my benchmark estimation I use the beginning of the month (1st) for the day of birth. My results are robust to imputing the day of birth as the middle of the month (15th) and the end of the month (28th). See Sections 4.2 and 6 for details.

is calculated by subtracting the Obstetric Estimate of Gestation (in weeks) from the actual birthdate and then adding 39 weeks, the length of normal pregnancy:

$$EB_i = BD_i - OBGEST_i + 39 \text{ weeks} \tag{3}$$

Exposure takes the value of 1 for births that occur to mothers' from exposed counties when the event of exposure occurs between expected conception and expected birth date. Thus my treatment group can be expressed as: $\mathbf{1}[EC_c < PoliceViolence_c < EB_c]$. My control group comprises all other births between 2004 and 2018, including counties that are never exposed. I also treat mothers who experience conception after an exposure event as part of my control group. I limit my sample to singleton births to mothers between the ages of 15 and 49. This follows standard practice in the literature (Persson and Rossin-Slater, 2018; Black et al., 2016)

I also estimate exposure by trimester to see if there are different effects for exposure in different trimesters. In this case I set first trimester as 0-12 weeks, second trimester as 13-26 weeks, and third trimester as 27-39 weeks after expected conception.

Table 1 presents summary statistics for my sample, as well as the control and treatment groups. Infants in the treatment groups have slightly lower birth weights on average compared to the control group, 3275.74 grams versus 3304.16 grams, respectively. In the treatment group 32.4% of mothers have a college degree or higher, as compared to 26.3% in the control group. Members of the treatment group are more likely to be Black (23.5%), and Hispanic (32.7%) compared to the control group (16% and 22.9%). The percentage of Very Low Birth Weight and Low Birth Weight births in the treatment and control group are almost identical.

As there are more than 3000 unique counties captured in my sample, and more than 4500 unique time periods, it would be computationally impossible within my resources to estimate county and time fixed effects as covariates. Thus, I use the PanelOLS package in Python, which uses groupwise demeaning to estimate entity and time fixed effects (Sheppard et al., 2021).

4 Results

Table 2 presents the results for the estimation of equation 1. Each column represents a different regression model. Model (1) and (2) use the natural log of $Birthweight_{i,c,t}$, model (3) and (4) use Very Low Birth Weight (<1500 grams), and model (5) and (6) use Low Birth Weight (<2500 grams) as the dependent variable. VLBWT and LBWT have been multiplied by 100 to increase readability. In model (1), (3), and (5) I estimate equation 1. In model (2), (4), and (6) I estimate equation 2 and include race interaction terms for Black Non-Hispanic (BNH) and Hispanic (Hisp) infants. In all models the sample is limited to White Non-Hispanic, Black Non-Hispanic, and Hispanic infants. In all models, county and week-month-year fixed effects are included. All models also include covariates for race, mother's age, live birth order, low educational attainment, county population, and mother's residence status. Standard errors are clustered at the county level, as treatment is determined at the county level.

In model (1) exposure is not statistically significant. In model (2) exposure is stastically signifigant at the 5% level, exposure for Black infants is statistically significant at the 10% level, and exposure for Hispanic infants is statistically significant at the 1% level. In all other models the objects of interest are not statistically significant at any conventional level. The race interaction terms in model (2) capture the additional effect of Black or Hispanic status on exposure. The exposure term captures the entire treatment group in model (1), and the members of the treatment group that are white non-Hispanic for model (2). In model (2) the race interaction terms predict effects moving in the opposite direction from that predicted by my hypothesis. When this is combined additively with the estimated effects of exposure in model (2) it results in a narrowly positive total effect for the racial groups of interest for both models (.0015 for Black*exposure and .0008 for Hispanic*exposure). The effect captured by the estimated coefficient for exposure in model (2) implies a -0.24% [95%CI -0.45%: -0.04%] change in birthweight due to exposure. For the average infant in my sample this represents a -7.93 gram [-14.87 gram:-1.32 gram] change in birth weight. The point estimate thus represents a 1/100th standard deviation decrease in birthweight. The effects of such a decrease in birthweight, in terms of elementary school test scores or lifetime wages, are practically zero (Figlio et al., 2014). These results are also significantly less than those captured in the majority of the literature on in utero effects of shocks. For example, Camacho (2008) finds a 27.76 gram birthweight decrease for infants exposed in-utero to landmine explosions, as compared to those not exposed, per explosion. Santos et al. (2020) finds a 43% increase in the odds of Low Birth Weight for infants most exposed in utero to neighborhood violence, compared to those least exposed. Dursun (2019) finds a 7.7% increase in incidents of Very Low Birth Weight as a result of exposure to mass shootings. My results are much smaller than those previously found in the literature for theoretically comparable sources of stress.

I would expect to find statistically and economically non-zero results when using VLWBT and LBWT as the dependent variables, as the literature strongly indicates that stress induced birthweight effects are highest at the extremes of the birthweight spectrum (Dursun, 2019; Persson and Rossin-Slater, 2018). However, for all of my models that use VLBWT and LBWT it is impossible to to reject the null hypothesis that my results are statistically different than zero. Additionally, the beta estimates for exposure and the exposure-race interaction terms for these dependent variables imply an almost 0 effect. In summary, my benchmark estimation implies no significant relationship between exposure to local police's use of force and infant birth outcomes.

This is contradictory to my expected results. In Section 1 it has been shown that lethal police interventions have a documented effect on the stress of Black Americans (Geller et al., 2014; Bor et al., 2018), and that this stress can extend to physical health effects (Sewell, 2017; Chae et al., 2014). It has also been shown that comparable external stressful shocks have statistically significant effects on birth outcomes (Currie et al., 2018; Dursun, 2019; Foureaux Koppensteiner and Manacorda, 2016; Quintana-Domeque and Ródenas-Serrano, 2017; Grossman et al., 2019; Dagnelie et al., 2018; Goin et al., 2019; Matoba et al., 2019; Santos et al., 2020). Therefore, my results may provide evidence that my hypothesis is incorrect.

Alternatively, I see three potential explanations for the contradiction between the results predicted by my hypothesis and those presented here. Firstly, it may be that the exposed population in my study is simply too small to capture the effects of these events. My study relied on 49 uses of police force over a 14 year period. There are 605,664 exposed births during this period, which represent only 1.07% of all births in my study during this period. While headlines about police force may feel omnipresent, it may be that these incidents simply do not occur frequently enough in the data to have a measurable effect.

This relates to a second potential explanation for my results: awareness of police violence, particularly as it impacts minority communities, has grown since the early 2000's (DeGue et al., 2016; Rembert et al., 2016). The summer of 2020 was a watershed moment, when the deaths of George Floyd, Breonna Taylor, and others catapulted the conversations surrounding the police's use of force into a new level of public awareness. These events generated far more news coverage, for a more sustained period of time, than the majority of the 49 events captured in my study. Despite my attempt to limit events via the criteria outlined in section 2.1 to those which would have enough coverage to enter the awareness of potentially exposed mothers, it is possible the events studied in this paper were not high profile in the lives of my treatment group. As a result, they may fail to generate a level of stress response in enough mothers in my treatment group to influence my results.

Lastly, it is possible that I am unable to find the link between the stress I expect to accompany these shocks and birth outcomes, particularly in minority communities, because the stress of these events is already baked into the everyday experience of these populations; therefore, the events do not pose an exogenous shock. Previous research has questioned whether incidents of police killings serve as reminders of historical racism and contribute to persistent psychological effects at a population level, rather than necessarily introducing new stressors (Alang et al., 2017). Other research on racism and health has shown that stressful events serve to reinforce pre-existing chronic health effects (Collins, 2002; Collins et al., 2004; Williams and Mohammed, 2013). Additionally, significant racial disparities in birth outcomes in the United States have been explained with measures of area racism (Chae et al., 2018), which may be correlated with incidents of police violence. Therefore, these incidents may not have compounding effects if a certain level of stress response is already present. In other words, population's stress levels may be inelastic (Collins et al., 2004; Alhusen et al., 2016; Bower et al., 2018; Hardeman et al., 2020). This explanation is challenged by some of the literature explored in section 1, which finds event driven negative psychological effects and higher stress levels after incidents of use of police force. It is also challenged by our understanding of exogenous shocks; for these events to have no effect it would have to be true that the population of interest was as stressed about these events before and after the event. This runs contrary to basic understandings of the human psyche as prioritizing recent stressors (Charmandari et al., 2012). However, I do not believe this final explanation is absolutely invalid, and think it has important ramifications for understanding the racial health gap and birth outcome gap in America. Under this explanation my paper provides further evidence for the theory that lifetime experiences, or perceived experiences, of individual or institutional racism contribute to chronic health effects.

4.1 Trimester Analysis

In Table 5 I show the results of differentiating exposure by trimester. Each column represents a different regression model. Model (1) and (2) use the natural log of $Birthweight_{i,c,t}$, model (3) and (4) use Very Low Birth Weight (<1500 grams), and model (5) and (6) use Low Birth Weight (<2500 grams) as the dependent variable. VLBWT and LBWT have been multiplied by 100 to increase readability. In model (1), (3), and (5) I estimate equation 1. In model (2), (4), and (6) I estimate equation 2 and include race interaction terms for Black Non-Hispanic (BNH) and Hispanic (Hisp) infants. In all models the sample is limited to White Non-Hispanic, Black Non-Hispanic, and Hispanic infants. In all models, county and week-month-year fixed effects are included. All models also include covariates for race, mother's age, live birth order, low educational attainment, county population, and mother's residence status. Standard errors are clustered at the county level, as treatment is determined at the county level. Model (2) is the only model with statistically significant results at conventional levels, with the exception of one result in model (4). Thus, the results follow the same patterns as the main benchmark results in statistical significance. These results are also similar to those presented in Table 2 in terms of magnitude. Thus the trimester by trimester analysis does not provide any evidence that exposure in any specific trimester is driving or depressing my benchmark results.

4.2 Robustness

I perform several robustness checks. In order to test the robustness of my benchmark analysis to changes in the date imputation when calculating expected birth date, I present results for imputing the date as the middle of the month (the 15th), and the end of the month (28th) in Table 4. All results are presented in the same format as Table 2 discussed above, and all results are no more than fractionally different from those discussed above.

In order to test whether any one event is depressing or driving my results, I estimate coefficients for *Exposure* and the Exposure-Race interaction terms for each of the 49 police violence events, following Equation 2. Histograms of these results are presented in Figure 2. All beta estimates are clustered around my benchmark results, and almost all estimates are within the 95% confidence intervals for my benchmark results.

In my benchmark estimation, I limit my sample to White Non-Hispanic, Black Non-Hispanic, and Hispanic infants. I present results for using the full sample in table 3. In this table *Exposure* captures the effect of exposure on all members of the expanded baseline group, which includes Asian or Pacific Islander and American Indian or Alaskan Native infants. The racial interaction terms thus capture the additional differential compared to this expanded baseline sample. All results are presented in the same format as Table 2 discussed above, and all results are no more than fractionally different from those discussed above.

5 Conclusion

Previous research has suggested that police violence events might have significant negative externalities with respect to minority community stress levels, and that stressful exogenous shocks may impact infant health outcomes. This paper fails to find evidence for a relationship between in-utero exposure to uses of police force and negative infant health outcomes.

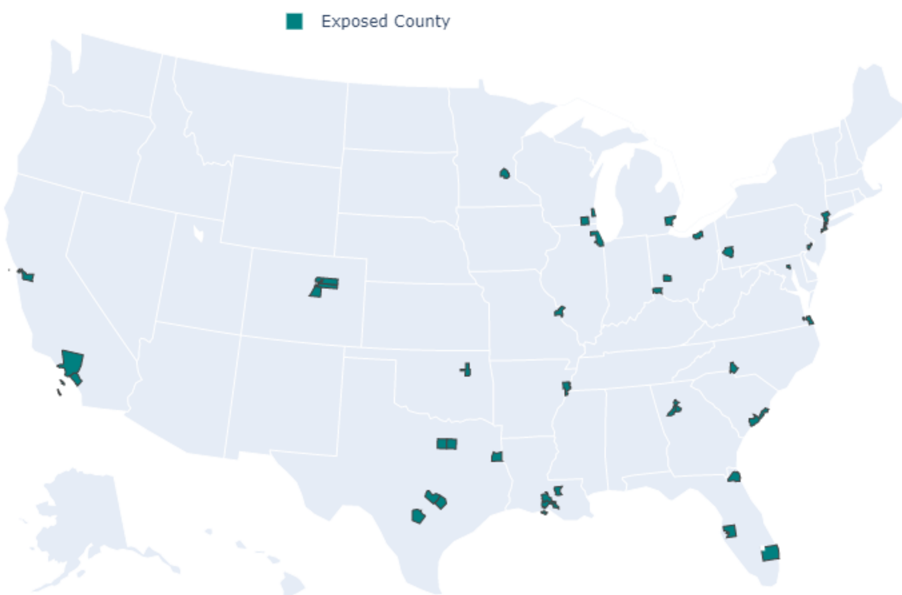
This may be due to the events studied in the primary analysis of this paper. The events I selected may not have occurred with enough frequency, or at a high enough level of news coverage, to cause measurable effects in my treatment group. Alternatively, my results may be evidence for theories of race-related stress that posit this stress as a lifetime chronic experience due to institutional factors, as opposed to being tied to individual events. In this case, my paper presents important evidence toward understanding the racial health gap in birth outcomes as a function of broad institutional driven stress, as opposed to event driven stress.

A weakness of my paper is that I compute exposure at the county level due to limitations of the data. Therefore, my calculations may include geographically relevant infants who were exposed to proximate stress from these events in the control groups, if the Mother's residence county was not the treated county. For example, for an incident of police violence occurring in Brooklyn, only infants with mothers who reside in Kings County would be in the treatment group. However, we might reasonably infer that all infants in the 5 boroughs of New York should be in the treatment group for this event. Therefore, my results may be influenced by this limitation and could potentially underestimate the true effect of exposure, by including potentially treated infants in the control group. Repeating my analysis using a radius of exposure instead of determining exposure at the county level may yeield different results.

To my knowledge, this is the first paper to address the potential link between in-utero exposure to police violence and infant health outcomes. Further work could be done with a larger pool of exposure events to encompass a wider range of police interventions. Additional work could also focus on a range of health outcomes presented in the NVSS data, such as delivery method, APGAR score, or abnormal infant health conditions. This topic also potentially merits revisiting when complete birth data is available for births through the end of 2021, in order to study the effects of more publicized police violence events that have occurred since 2018 and are not captured in this study.

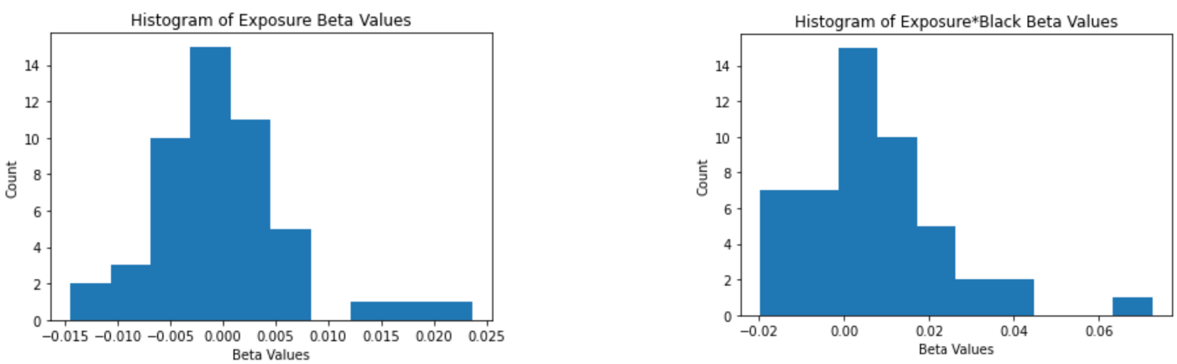
6 Appendix: Tables and Figures

Figure 1: Counties with Police Violence Episodes



Notes: Figure shows a map of the United States with selected counties where incidents of police use of force occurred (exposed counties) shaded in turquoise. Events were selected according to criteria defined in Section 2.1

Figure 2: Histogram of Exposure Estimated Beta Values



Notes: Figure shows histograms of estimated beta values for objects of interest ($exposure$ and $exposure{*}race$) for single event exposure regressions. For each event, a separate regression was estimated where the treatment group was comprised only of individuals exposed to that event.

Table 1: Mean Value for Variables of Interest

Variable	**Full Sample**	**Control**	**Treatment**
Mother's Age	27.897	27.885	28.958
	(6.01)	(6.01)	(6.02)
Live Birth Order	2.112	2.112	2.119
	(1.33)	(1.33)	(1.34)
Birth Weight	3303.861	3304.16	3275.74
	(553.86)	(553.86)	(553.18)
<College Degree	0.661	0.661	0.657
	(0.47)	(0.47)	(0.47)
>College Degree	0.263	0.263	0.324
	(0.44)	(0.44)	(0.47)
White	0.763	0.765	0.656
	(0.42)	(0.42)	(0.47)
Black	0.161	0.16	0.235
	(0.37)	(0.37)	(0.42)
American Indian or Alaskan Native	0.012	0.012	0.004
	(0.11)	(0.11)	(0.07)
Asian or Pacific Islander	0.064	0.063	0.104
	(0.24)	(0.24)	(0.31)
VLBWT	0.01	0.01	0.011
	(0.10)	(0.10)	(0.10)
LBWT	0.063	0.063	0.067
	(0.24)	(0.24)	(0.25)
White Non-Hispanic	0.544	0.547	0.344
	(0.50)	(0.50)	(0.47)
Hispanic	0.230	0.229	0.327
	(0.42)	(0.42)	(0.47)
n	56,622,192	56,016,528	605,664

Standard Deviation in Parenthesis

Notes: This table shows mean values and standard errors for key variables. Values are presented separately for the Full Sample, Control, and Treatment groups. All mean values are rounded to three decimal places, and all standard deviations are rounded to two decimal places. Infants in the treatment groups have slightly lower birth weights on average compared to the control group, 3275.74 grams versus 3304.16 grams, respectively. In the treatment group 32.4% of mothers have a college degree or higher, as compared to 26.3% in the control group. Members of the treatment group are more likely to be Black (23.5%), and Hispanic (32.7%) compared to the control group (16% and 22.9%). The percentage of Very Low Birth Weight and Low Birth Weight births in the treatment and control group are almost identical.

Table 2: Benchmark Estimation Results

	$log(BWT)$		VLBWT		LBWT	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	-0.0003	-0.0024**	0.0015	-0.0096	-0.0206	0.0093
	(0.0006)	(0.0011)	(0.0111)	(0.0142)	(0.0382)	(0.0785)
Black (non-Hispanic)*Exposure	-	0.0039*	-	-0.0005	-	-0.0460
		(0.0020)		(0.0357)		(0.1426)
Hispanic*Exposure	-	0.0032***	-	0.0306	-	-0.0507
		(0.0007)		(0.0215)		(0.0778)
Fixed Effects	✓	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓	✓
n	52,327,216	52,327,216	52,327,216	52,327,216	52,327,216	52,327,216
Dependent Variable Mean	8.087	8.087	0.998	0.998	6.278	6.278
	(0.202)	(0.202)	(9.940)	(9.940)	(24.257)	(24.257)

Standard Error in Parenthesis
* p <0.10 ** p <0.05 *** p <0.01

Notes: Each column represents a different regression model. Model (1) and (2) use the natural log of $Birthweight_{i,c,t}$, model (3) and (4) use Very Low Birth Weight (<1500 grams), and model (5) and (6) use Low Birth Weight (<2500 grams) as the dependent variable. VLBWT and LBWT have been multiplied by 100 to increase readability. In model (1), (3), and (5) I estimate equation 1. In model (2), (4), and (6) I estimate equation 2 and include race interaction terms for Black Non-Hispanic (BNH) and Hispanic (Hisp) infants. In all models the sample is limited to White Non-Hispanic, Black Non-Hispanic, and Hispanic infants. In all models, county and week-month-year fixed effects are included. All models also include covariates for race, mother's age, live birth order, low educational attainment, county population, and mother's residence status. Standard errors are clustered at the county level, as treatment is determined at the county level. In model (1) exposure is not statistically significant. In model (2) exposure is statistically significant at the 5% level, exposure for Black infants is statistically significant at the 10% level, and exposure for Hispanic infants is statistically significant at the 1% level. In all other models the objects of interest are not statistically significant at any conventional level. The race interaction terms in model (2) capture the additional effect of Black or Hispanic status on exposure. The exposure term captures the entire treatment group in model (1), and the members of the treatment group that are white non-Hispanic for model (2). In model (2) the race interaction terms predict effects moving in the opposite direction from that predicted by my hypothesis. When this is combined additively with the estimated effects of exposure in model (2) it results in a narrowly positive total effect for the racial groups of interest for both models (.0015 for Black* exposure and .0008 for Hispanic*exposure). The effect captured by the estimated coefficient for exposure in model (2) implies a -0.24% [95%CI -0.45%: -0.04%] change in birthweight due to exposure. For the average infant in my sample this represents a -7.93 gram [-14.87 gram:-1.32 gram] change in birth weight. The point estimate thus represents a 1/100th standard deviation decrease in birthweight. The effects of such a decrease in birthweight, in terms of elementary school test scores or lifetime wages, are practically zero (Figlio et al., 2014).

Table 3: Expanded Baseline Control Group Estimation Results

	log(*BWT*)		VLBWT		LBWT	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	-0.0003	-0.0041**	0.0040	0.0014	-0.0187	0.0716
	(0.0007)	(0.0014)	(0.0106)	(0.0136)	(0.0337)	(0.0569)
Black (non-Hispanic)*Exposure	-	0.0072**	-	-0.0153	-	-0.1656
		(0.0023)		(0.0357)		(0.1206)
Hispanic*Exposure	-	0.0069***	-	0.0182	-	-0.1648**
		(0.0015)		(0.0202)		(0.0682)
Fixed Effects	✓	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓	✓
n	56,622,192	56,622,192	56,622,192	56,622,192	56,622,192	56,622,192
Dependent Variable Mean	8.086	8.086	0.984	0.984	6.296	6.296
	(0.201)	(0.201)	(9.872)	(9.872)	(24.288)	(24.288)

Standard Error in Parenthesis
* p <0.10   ** p <0.05   *** p <0.01

Notes: Each column represents a different regression model. Model (1) and (2) use the natural log of $Birthweight_{i,c,t}$, model (3) and (4) use Very Low Birth Weight (<1500 grams), and model (5) and (6) use Low Birth Weight (<2500 grams) as the dependent variable. VLBWT and LBWT have been multiplied by 100 to increase readability. In model (1), (3), and (5) I estimate equation 1. In model (2), (4), and (6) I estimate equation 2 and include race interaction terms for Black Non-Hispanic (BNH) and Hispanic (Hisp) infants. In this table the sample includes all racial groups, including Asian or Pacific Islander and American Indian or Alaskan native infants. In all models, county and week-month-year fixed effects are included. All models also include covariates for race, mother's age, live birth order, low educational attainment, county population, and mother's residence status. Standard errors are clustered at the county level, as treatment is determined at the county level. Results are in line with my benchmark estimation results, presented in table 2.

Table 4: Robust Birthday Estimation Results

Panel A: Day=15 Results

	$log(BWT)$		VLBWT		LBWT	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	-0.0003	-0.0024**	0.0069	-0.0087	-0.0261	-0.0120
	(0.0006)	(0.0010)	(0.0110)	(0.0141)	(0.0345)	(0.0731)
Black (non-Hispanic)*Exposure	-	0.0037*	-	0.0152	-	0.0157
		(0.0020)		(0.0376)		(0.1324)
Hispanic*Exposure	-	0.0032***	-	0.0323	-	-0.0490
		(0.0008)		(0.0207)		(0.0778)
Fixed Effects	✓	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓	✓
n	52,327,216	52,327,216	52,327,216	52,327,216	52,327,216	52,327,216
Dependent Variable Mean	8.087	8.087	0.998	0.998	6.278	6.278
	(0.202)	(0.202)	(9.940)	(9.940)	(24.257)	(24.257)

Panel B: Day=28 Results

	$log(BWT)$		VLBWT		LBWT	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	-0.0002	-0.0023**	0.0113	-0.0033	-0.0383	-0.0162
	(0.0006)	(0.0010)	(0.0103)	(0.0133)	(0.0324)	(0.0699)
Black (non-Hispanic)*Exposure	-	0.0037*	-	0.0138	-	0.0009
		(0.0020)		(0.0367)		(0.1360)
Hispanic*Exposure	-	0.0033***	-	0.0305	-	-0.0613
		(0.0007)		(0.0203)		(0.0685)
Fixed Effects	✓	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓	✓
n	52,327,216	52,327,216	52,327,216	52,327,216	52,327,216	52,327,216
Dependent Variable Mean	8.087	8.087	0.998	0.998	6.278	6.278
	(0.202)	(0.202)	(9.940)	(9.940)	(24.257)	(24.257)

Standard Error in Parenthesis
* p <0.10 ** p <0.05 *** p <0.01

Notes: For confidentiality reasons the NVSS data presents only month and year for birth date. In order to subtract the Obstetric Estimate of Gestation (presented as a number of weeks in the data) from the birth date in month-year format, I am required to impute a day of birth, after which I can perform all calculations on the week-month-year level (Black et al., 2016). In my benchmark estimation, I use the beginning of the month (1st) for the day of birth. This table presents results for imputing day of birth as the middle of the month (Day=15) and end of the month (Day=28). Each column represents a different regression model. Model (1) and (2) use the natural log of $Birthweight_{i,c,t}$, model (3) and (4) use Very Low Birth Weight (<1500 grams), and model (5) and (6) use Low Birth Weight (<2500 grams) as the dependent variable. VLBWT and LBWT have been multiplied by 100 to increase readability. In model (1), (3), and (5) I estimate equation 1. In model (2), (4), and (6) I estimate equation 2 and include race interaction terms for Black Non-Hispanic (BNH) and Hispanic (Hisp) infants. In all models the sample is limited to White Non-Hispanic, Black Non-Hispanic, and Hispanic infants. In all models, county and week-month-year fixed effects are included. All models also include covariates for race, mother's age, live birth order, low educational attainment, county population, and mother's residence status. Standard errors are clustered at the county level, as treatment is determined at the county level. Results are in line with my benchmark estimation results, presented in table 2.

Table 5: Trimester Estimation Results

	$log(BWT)$		VLBWT		LBWT	
	(1)	(2)	(3)	(4)	(5)	(6)
Trimester 1 Exposure	-0.0005	-0.0033*	0.0011	0.0207	0.0805	0.1391
	(0.0008)	(0.0012)	(0.0180)	(0.0213)	(0.0692)	(0.0994)
Trimester 2 Exposure	-0.0001	-0.0025*	0.0102	-0.0197	-0.0746	-0.0480
	(0.0007)	(0.0010)	(0.0159)	(0.0232)	(0.0475)	(0.0707)
Trimester 3 Exposure	-0.0007	-0.0029*	-0.0038	-0.0142	-0.0204	0.0296
	(0.0005)	(0.0011)	(0.0168)	(0.0195)	(0.0351)	(0.0868)
BNH*Tri1	-	0.0047**		-0.1001*		-0.0917
		(0.0020)		(0.0592)		(0.1380)
BNH*Tri2	-	0.0035**		0.0810		0.0555
		(0.0017)		(0.0576)		(0.1509)
BNH*Tri3	-	0.0049**		-0.0083		-0.1488
		(0.0022)		(0.0453)		(0.2053)
Hisp*Tri1	-	0.0045***		0.0144		-0.0986
		(0.0008)		(0.0328)		(0.1134)
Hisp*Tri2	-	0.0043***		0.0276		-0.1094
		(0.0008)		(0.0282)		(0.0812)
Hisp*Tri3	-	0.0027**		0.0339		-0.0360
		(0.0011)		(0.0271)		(0.1194)
Fixed Effects	✓	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓	✓
n	52,327,216	52,327,216	52,327,216	52,327,216	52,327,216	52,327,216
Dependent Variable Mean	8.087	8.087	0.998	0.998	6.278	6.278
	(0.202)	(0.202)	(9.940)	(9.940)	(24.257)	(24.257)

Standard Error in Parenthesis
* p <0.10 ** p <0.05 *** p <0.01

Notes: Each column represents a different regression model. Model (1) and (2) use the natural log of $Birthweight_{i,c,t}$, model (3) and (4) use Very Low Birth Weight (<1500 grams), and model (5) and (6) use Low Birth Weight (<2500 grams) as the dependent variable. VLBWT and LBWT have been multiplied by 100 to increase readability. In model (1), (3), and (5) I estimate equation 1. In model (2), (4), and (6) I estimate equation 2 and include race interaction terms for Black Non-Hispanic (BNH) and Hispanic (Hisp) infants. In all models the sample is limited to White Non-Hispanic, Black Non-Hispanic, and Hispanic infants. In all models, county and week-month-year fixed effects are included. All models also include covariates for race, mother's age, live birth order, low educational attainment, county population, and mother's residence status. Standard errors are clustered at the county level, as treatment is determined at the county level. Model (2) is the only model with statistically significant results at conventional levels, with the exception of one result in model (4).Thus, the results follow the same patterns as the main benchmark results in statistical significance. These results are also similar to those presented in Table 2 in terms of magnitude. Thus the trimester by trimester analysis does not provide any evidence that exposure in any specific trimester is driving or depressing my benchmark results.

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