



## Highly public anti-Black violence and preterm birth odds for Black and White mothers

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### ABSTRACT

Highly public anti-Black violence may increase preterm birth in the general population of pregnant women via stress-mediated paths, particularly Black women exposed in early gestation. To examine spillover from racial violence in the US, we included a total of 49 high publicity incidents of the following types: police lethal force toward Black persons, legal decisions not to indict/convict officers involved, and hate crime murders of Black victims. National search interest in these incidents was measured via Google Trends to proxy for public awareness of racial violence. Timing of racial violence was coded in relation to a three-month preconception period and subsequent pregnancy trimesters, with the primary hypothesis being that first trimester exposure is associated with higher preterm birth odds. The national sample included 1.6 million singleton live births to US-born Black mothers and 6.6 million births to US-born White mothers from 2014 to 2017. Using a preregistered analysis plan, findings show that Black mothers had 5% higher preterm birth odds when exposed to any high publicity racial incidents relative to none in their first trimester, and 2–3% higher preterm birth odds with each  $\log_{10}$  increase in national interest. However, post hoc sensitivity tests that included month fixed effects attenuated these associations to null. For White mothers, associations were smaller but of a similar pattern, and were attenuated when including month fixed effects. Highly public anti-Black violence may act as a national stressor, yet whether racial violence is associated with reproductive outcomes in the population is unknown and merits further research.

### 1. Introduction

Black women in the US have elevated rates of preterm and small-for-gestational-age births relative to Whites (i.e., birth prior to 37 weeks and <10th percentile of gestation-adjusted birthweight, respectively) (Kramer, Ananth, Platt, & Joseph, 2006; Martin, Hamilton, Osterman, Driscoll, & Drake, 2018). Greater exposure to interpersonal racial discrimination is a potential contributor to these disparities in birth outcomes (Alhusen, Bower, Epstein, & Sharps, 2016; Giurgescu, McFarlin, Lomax, Craddock, & Albrecht, 2011). However, nearly all research from this literature has focused on personally experienced racism (Alhusen et al., 2016; Heard-Garris, Cale, Camaj, Hamati, &

Dominguez, 2018), whereas secondhand observations of discrimination in one's social network or through the media have rarely been examined as predictors of birth outcomes. Similar to personal experiences with racism, observing racist acts may evoke stress responses and racism-related vigilance, even when the target is a stranger (Bor, Venkataramani, Williams, & Tsai, 2018; Heard-Garris et al., 2018, 2021; McFarland, Geller, & McFarland, 2019). Due to the large scale of exposure, highly visible and well-known racist acts may result in widely dispersed effects on psychological distress and mental health for the exposed population (Bor et al., 2018; Curtis, Washburn, et al., 2021; Tynes, Willis, Stewart, & Hamilton, 2019). However, unknown is whether highly public anti-Black violence and other forms of visible

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racism in the US influence birth outcomes for Black pregnant women (Premkumar, Nseyo, & Jackson, 2017).

Police use of lethal force toward Black civilians, particularly when widely perceived as unjust and racially motivated, is one type of racial violence with potential for spillover or population-level effects (Bor et al., 2018; Sewell et al., 2020). Although many incidents end up unknown to the public, select police killings are extensively reported by traditional news media and widely discussed on social media and beyond (Lee, Weitzer, & Martínez, 2018). For instance, several high profile police killings occurred in 2014 and the following years, including Michael Brown, Eric Garner, Tamir Rice, and Freddie Gray (Freelon, McIlwain, & Clark, 2016; Lee et al., 2018). The news reporting and public discussion for these incidents was often critical of law enforcement and highlighted systemic problems in the criminal justice system while a counter-perspective justified police actions and redirected criticism to protests and social issues disproportionately present in Black communities (Freelon et al., 2016; Lee et al., 2018). Perceptions of unjust police killings accompanied by frequent discussion of race and racism may have amplified threat and stress levels for Black Americans (Curtis, Washburn, et al., 2021; McFarland et al., 2019; Tynes et al., 2019). Moreover, legal sanctioning of police lethal force through non-indictment of the involved officers may exacerbate feelings of injustice, grief, and anger. For example, after the police killing of Michael Brown and the ensuing grand jury non-indictment, depressive and post-traumatic symptoms were elevated among nearby Black residents relative to Whites, with media exposure being a risk factor (Galovski et al., 2016).

Stress-related mechanisms could link exposure to high publicity racial incidents to preterm birth (PTB) and other birth outcomes (Hoffman, Mazzoni, Wagner, Laudenslager, & Ross, 2016; Wadhwa, Culhane, Rauh, & Barve, 2001). Stress-induced elevated maternal cortisol in the first half of gestation is associated with earlier increases of placental corticotropin-releasing hormone levels, a hormone that is mechanistically involved in parturition (Hobel, Dunkel-Schetter, Roesch, Castro, & Arora, 1999; Lockwood, 1999; Sandman et al., 2006). Elevated circulating pro-inflammatory cytokines and local urogenital tract and placental-decidual infections are related inflammatory pathways that begin in early- and mid-gestation and cause PTB (Ama-bebe & Anumba, 2018; Wadhwa et al., 2001). Moreover, hypertension and cardiovascular stress reactivity are stress-related vascular pathways, which are linked with pregnancy complications, PTB, and restricted fetal growth (Giscombé & Lobel, 2005). Thus, stress from observing racial violence could lead to shortened gestational length, with the first half of gestation being a potential sensitive period, while some evidence supports restricted fetal growth as another outcome.

High publicity racial violence is unlikely to influence different groups uniformly, with spillover possibly limited to the targeted racial group. For instance, following a large immigration raid in Iowa, low birth weight term births increased among US-born and immigrant Latina mothers exposed to the raid in their first trimester but not Whites (Novak, Geronimus, & Martinez-Cardoso, 2017). Similarly, an increasingly hostile anti-Latino sociopolitical climate during the 2016 US Presidential Election was linked with a higher PTB rate nationally for Latina women independent of trends in the general population (Gemmill et al., 2019). Finally, in the six-months after the terrorist attack of September 11th, low birth weight (from prematurity) increased by 34% for Californians with Arabic surnames but not for any other racial group, possibly due to increasing discrimination experiences and anti-Arab hostility (Lauderdale, 2006); a comparable study, however, found no increase for Michigan women with Arabic surnames (El-Sayed, Hadley, & Galea, 2008). High-profile racial violence therefore may cause widely experienced distress that manifests in elevated adverse birth outcomes, but findings are mixed and it is unknown whether effects are targeted to a specific racial group and under what conditions.

Spillover effects of racial violence may depend on factors such as proximity to and awareness of incidents. A recent review of the vicarious

racism literature suggested a definition where secondhand discrimination occurs irrespective of the race of the observer, but noted that vicarious exposure is contingent on awareness of the racist act and that distress may be greater when identifying with the victim (e.g., due to shared geography or racial group) (Heard-Garris et al., 2018). Public awareness of specific police killings is likely highest in the area where violence occurred, such that the potential for spillover is larger (Bor et al., 2018). Indeed, the number of police killings of Black persons has been conceptualized as an area-level exposure for neighborhoods or states where incidents occurred (Bor et al., 2018; Goin et al., 2021; Sewell et al., 2020). This decision is presumably based on the assumption that police killings are well-known in an area and that number of killings approximates for corresponding distress among Black residents. However, this assumption has not been tested and is a major limitation of prior research, as spillover may be unlikely for the many police killings that are either unknown by the public or perceived as justified (Curtis, Washburn, et al., 2021). Health spillover from police killings also is unlikely constrained by area boundaries for incidents receiving widespread public interest and media coverage. In national surveys during 2016–2017, more than two-thirds of Black and one-third of White Americans reported police violence toward minorities is a somewhat or very significant source of stress (American Psychological Association, 2017), and protests for racial justice following select high-profile police killings were national in scale (Buchanan et al., 2020). Moreover, one study showed Black Americans nationally reported higher poor mental health days following high publicity anti-Black incidents (Curtis, Washburn, et al., 2021). Quantifying exposure to racial violence, based on spatial proximity and public interest, can thus strengthen evidence for spillover effects.

### 1.1. Current study

This study contributes to research on spillover from racial stressors to population health by exploiting the timing of 49 highly publicized incidents of anti-Black violence from 2014 through 2017 that are treated as national stress exposures for a large sample of live births in the US (Curtis, Washburn, et al., 2021). National search interest in selected incidents and state variation in interest proxy for awareness of racial violence incidents, as we expected larger spillover effects on health when awareness was higher (Curtis, Washburn, et al., 2021). This design focuses on incidents for which public awareness is plausibly sufficient for population-level impacts on health. We used national natality records for live singleton births to US-born Black and White women with conceptions from August 2013 through February 2017 to model associations between exposure to racial violence and PTB odds. Exposure was coded for a three-month preconception period and pregnancy trimesters to examine differential associations based on the timing of exposure.

Our primary hypothesis was that first trimester exposure to racial violence is associated with elevated PTB odds for Black mothers, while we were agnostic about preconception and second trimester exposure and expected no association with third trimester exposure. Expectations of the first trimester being a period of heightened sensitivity to stress were based on evidence of trimester-specific associations with PTB from natural experiments (Camacho, 2008; Glynn, Wadhwa, Dunkel-Schetter, Chiciz-DeMet, & Sandman, 2001; Torche, 2011); mixed results for preconception stress exposure (M. R. Kramer, Hogue, Dunlop, & Menon, 2011); and the potential that stress reactivity is dampened in the third trimester (de Weerth & Buitelaar, 2005). However, some research suggests worse birth outcomes following stress exposure in mid and late gestation, with evidence of PTB increasing following second and third trimester stress exposures (Bruckner, Lebreton, Perrone, Mortensen, & Blondel, 2019; Catalano & Hartig, 2001; Eskenazi, Marks, Catalano, Bruckner, & Toniolo, 2007; Hoffman et al., 2016).

Additionally, we examined whether spillover was limited to births to Black mothers and varied by geography. We expected spillover to be

absent or at least attenuated for White mothers (Bor et al., 2018; Goin et al., 2021), and that exposure to racial violence would be more strongly associated with PTB in states where racial incidents occurred and where interest was higher (Bor et al., 2018). Also, we considered SGA as a secondary outcome, as prior evidence for stress effects has been mixed (Lauderdale, 2006; Novak et al., 2017; Torche, 2011), and examined different PTB categories and low birth weight in sensitivity tests. This study advances existing literature by focusing on highly public anti-Black violence of multiple types rather than area-specific fatal police violence, examining sensitive periods for stress exposure in relation to PTB, and conducting preregistered analyses to identify how associations vary based on race, geography, and birth outcome.

## 2. Methods

Sample data came from the restricted access US national birth certificate records, 2014–2017 (National Center for Health Statistics, n.d.). We selected singleton births to US-born mothers who identified as non-Hispanic Black or non-Hispanic White. We restricted the sample to singletons due to different PTB and SGA risk factors for multiple gestation births. Maternal race and Hispanic ethnicity were available for 97.9% of live births, and nativity status was available for 99.8% of births for included ethnoracial groups. Gestational age was available for more than 99.9% of births. Records with missing data on sample criteria and gestational age were excluded. Birth records were available based on birth month from January 2014 through December 2017, such that the full cohorts of live births for conceptions prior to August 2013 and after February 2017 were not included (with PTB risk biased for partial cohorts). Thus, we restricted the sample to births with estimated conceptions from August 2013 to February 2017. To verify the full cohort of live births was included for these conception months, we used publicly available 2013 and 2018 birth records and found missingness was nearly absent (described in Supplementary Material). In total, the sample consists of 1,600,619 live births to Black mothers and 6,600,001 live births to White mothers. Mothers resided in all fifty states and the District of Columbia. The University of Utah Institutional Review Board determined the study was not human subjects research due to use of deidentified data.

Month of conception was used to identify exposure by gestational period (i.e., three-month preconception period, pregnancy trimesters). Similar to prior research (Currie & Schwandt, 2013), we estimated month of conception by transforming gestational age in weeks to months (multiplying weeks by 7/30.5) and subtracting rounded gestational months from birth month. Simulations of a similar approach found that calendar month of conception is correctly classified for 79% of births and is always within one month (Noelke, Chen, Osypuk, & Acevedo-Garcia, 2019).

### 2.1. Measures

#### 2.1.1. Adverse birth outcome variables

Gestational age at birth was based on the best obstetric estimate as recorded on the birth certificate, which has higher validity than estimates using last normal menses (Martin, Osterman, Kirmeyer, & Gregory, 2015). We coded PTB as births prior to 37 completed weeks gestation, with further specification of very or extreme preterm (prior to 32 weeks) and moderate preterm birth (32–36 weeks) in sensitivity tests (Martin et al., 2015).

Birth weight was directly assessed and is listed on nearly all records (>99.9%) (Northam & Knapp, 2006). SGA, or <10th percentile of birth weight for gestational age, was coded using published sex-specific cut points (Olsen, Groveman, Lawson, Clark, & Zemel, 2010). In sensitivity tests, implausible values for birth weight by gestational age were dropped, consistent with prior research (i.e., values of two interquartile ranges below the 25th percentile or above the 75th percentile for each gestational age) (Olsen et al., 2010). Low birth weight (<2500 g),

primarily caused by shortened gestational age and restricted fetal growth, was used as a secondary outcome because it is widely used and not dependent on estimating gestational age (Curtis, Fuller-Rowell, Carlson, Wen, & Kramer, 2021). Classifications of PTB and low birth weight from birth records have high validity (Lain et al., 2012).

#### 2.1.2. Racial violence exposure variables

Measurement of racial violence was a multi-step procedure: first, identifying high publicity incidents based on news coverage; second, quantifying public interest in individual incidents by month; and, third, creating summary measures representing maternal-level exposure by gestational period. Detailed methods are described elsewhere (Curtis, Washburn, et al., 2021). Four types of high publicity racial violence were included: police use of lethal force toward unarmed Black persons; police lethal force toward armed Black persons; legal decisions not to indict or convict officer/s involved in the police lethal force incidents; and hate crime murders of Black victims.

To identify high publicity incidents, news coverage was assessed using Proquest's US Newsstream as the number of news stories mentioning the victim's name within 60 days of the incident. News stories were required to include terms relating to incident location and law enforcement to reduce irrelevant search retrievals (Curtis, Washburn, et al., 2021). Beginning with unarmed Black victims of police killings in the Mapping Police Violence (MPV) 2013–2017 dataset ( $n = 278$ ) (MPV, 2020), incidents receiving the top decile of coverage were selected as high publicity incidents. This process resulted in 28 incidents with at least 44 stories each. The top decile was selected based on the assumption that these incidents were well-known (Curtis, Washburn, et al., 2021). For police killings of armed Black persons, 30 possible high publicity incidents were identified, of which 10 met the same threshold of 44 news stories. Hate crime murders of Black victims where the perpetrator was affiliated with white supremacist ideology came from the Anti-Defamation League (Anti-Defamation League, n.d.). Based on news coverage including the perpetrator's name, two hate crime murders met the news threshold.

National interest in each incident was measured using Google Trends, a web-based tool providing the relative share of Google search volume for individual search strings over time (Stephens-Davidowitz & Varian, 2014). Search strings were defined as the victim's first and last name (or perpetrator's for hate crime murders). Values are normalized from 0 to 100 for each data request, with 100 indicating the month when one of the strings received the highest share of conducted searches. Up to five strings were compared per request, and Michael Brown was included as a common reference for incidents. Thus, a value of 10 indicates that a string received 10% of the search interest relative to the month when Michael Brown received highest interest. To reduce irrelevant search interest (e.g., from common names), average interest in the six pre-incident months was subtracted from monthly post-incident search interest. Because interest was sustained for multiple months in some cases and to allow for flexibility in timing, incidents were treated as three-month exposures (i.e., including search interest in the incident month and following two months). Whereas reliability of Google Trends estimates is lower for less populated areas, with recommendations to average values from multiple data downloads (Tran et al., 2017), monthly estimates for national interest in incidents had very high reliability (intraclass correlation coefficients of > 0.99) and thus a single data request was sufficient.

To identify high publicity legal decisions resulting in non-indictment or non-conviction of officer/s using lethal force, incident-specific monthly search interest following the initial three months was used to detect subsequent spikes in interest. The highest subsequent spike in interest was selected for each incident that met a minimum value (i.e., 1 on the Michael Brown scale) and coincided with a legal decision not to indict or convict an officer/s involved in the killing. Among all high publicity police killings (unarmed and armed), nine spikes in interest met these criteria, each of which surpassed the news threshold in the 60

days following the legal decision. These high publicity legal decisions were treated as separate incidents from police killings because announcements fell outside the initial three-month period, yet decisions to sanction police killings may elicit renewed public attention and shape public perceptions and emotional responses (Freelon et al., 2016).

From the data collection process, 49 unique incidents were identified from which the number of racial violence incidents by month was determined. To compute national interest in racial violence incidents for each month, monthly interest was summed across individual incidents (including only the three post-incident months). Next, using three-month moving averages for national interest in racial incidents, month of conception was used to determine maternal exposure in the three-month preconception period and individual pregnancy trimesters (gestation months 1–3; 4–6; and 7–9). As a longer gestation provides added time for stress exposure, biasing third-trimester estimates, exposure was measured independent of completed gestational age (i.e., all third trimesters were defined as three-month periods); this coding ensured comparability with other trimesters and was appropriate given expectations for non-significance (Currie & Rossin-Slater, 2013). We examined sensitivity tests where third trimester exposure was defined using racial violence only in the 7th gestational month.

Using Google Trends, state-specific relative search volume was collected for each incident in the first ninety days following incident dates. Values are normalized by search string, such that 100 represents the state with the highest share of total searches that included the relevant string. Because values refer to relative share of searches, differences in population size and internet use are inherently adjusted. Google Trends estimates come from a sample of Google searches, cached daily, with recommendations to average multiple downloads (Stephens-Davidowitz & Varian, 2014). Thus, we averaged estimates of state relative search volume from five data downloads (each pulled on separate days), resulting in reliable estimates (intraclass correlation coefficient = 0.96). Missing values for state relative search interest were common for incidents receiving relatively low interest, for which we imputed the incident-specific minimum relative search volume among states with non-missing values (this approach assumes low search volume was the cause of missingness). Some states had missing values for only a portion of the five downloads; in these instances, a moderate correlation of  $r = 0.55$  between available state-incident observations and incident-specific minimum observations among other states provided support for our imputation strategy. State search interest was higher in states where the incident occurred (mean = 89.1, SD = 20.31) relative to the remaining states or Washington DC (mean = 24.1, SD = 17.7); 31 of the 49 incident states had the highest incident-specific search interest (i.e., value of 100).

To summarize state-specific search interest across incidents by gestational period, we first weighted state relative interest using national interest for corresponding incidents. Weighting state relative interest by national interest was necessary as normalization of state interest for each string (i.e., incident) gives equal weight to incidents despite substantial variation in national interest. After rescaling state relative interest to range from 0 to 1 (dividing original values by 100), we multiplied state relative interest for each incident by monthly national interest. For instance, state interest of 50 would be rescaled to 0.50 and multiplied by national interest for the three incident months, with the resulting nationally-weighted state relative interest on a scale comparable to national interest. Next, we summed nationally-weighted state monthly interest across incidents and months to determine exposure within gestational periods, and divided this value by cumulative national interest in the corresponding period (i.e., across incidents for the three months). The end product represented the total portion of state search interest in recent racial incidents relative to the theoretical state maximum interest; the variable has a potential range from 0 to 1, with 1 for a state with the maximum interest in all corresponding incidents. The expectation is that, net of stable state-specific differences (e.g., political and demographic characteristics), residents of a state with less interest

in racial incidents would experience smaller increases in PTB odds associated with racial violence incidents.

### 2.1.3. Covariates

Covariates derived from birth certificates include infant sex (Challis, Newnham, Petraglia, Yeganegi, & Bocking, 2013), maternal age ( $\leq 19$  years, 20–24, 25–29, 30–34, and  $\geq 35$  years) (Ferré, 2016), parity inclusive of birth-of-record (1, 2, 3–5, 6+) (Shah & Knowledge Synthesis Group on Determinants of LBW/PT Births, 2010), and prior PTB (1 = yes, 0 = no or unknown) (Hackney, Durie, Dozier, Suter, & Glantz, 2012). These variables are established predictors of PTB risk and are measured from birth records with at least moderate sensitivity and high specificity (Hackney et al., 2012; Lain et al., 2012; Northam & Knapp, 2006). A small share of records had missing or unknown values for prior PTB (0.16%) and were imputed as the 'no' category. When determining parity, 0.41% had unknown values for number of prior live births still living or dead and zero was assumed in the respective category.

Individual risk factors for PTB may bias estimates if rates among birthing cohorts are correlated with exposure to racial stress (e.g., differences in stress-linked fertility or pregnancy loss). Although the pre-registration called for maternal education and marital status to be examined as potential confounds, we included a fuller list of variables to account for differential selection into pregnancy or in utero based on reviewer feedback—namely, paternity acknowledgment, payment source, and maternal smoking during pregnancy. Socioeconomic and health risks were extracted from birth certificates. Maternal education was coded in five categories: no high school diploma; high school graduate or GED; some college, no Bachelor's degree; Bachelor's degree or higher; and unknown (this last category represented 0.54% of birth records). For marital status, 1.36% of values were missing, nearly all of which were in California, and so we included a distinct category for unknown (i.e., married; unmarried; unknown). Coverage and coding were similar for paternity acknowledgment. Sensitivity tests were conducted using listwise deletion to handle missingness for variables and results were identical.

To account for macro-economic trends that may influence rates of crime and police contact as well as PTB risk, we included early gestation changes in the unemployment rate (Noelke et al., 2019). Specifically, unemployment had a strong decreasing trend across the study period and was nearly collinear with time, such that we used change in unemployment from the three-month pre-conception period to the first trimester, accounting for recent economic shocks and boons that would coincide with the hypothesized timing of racial stress effects. National and state monthly unemployment rates, seasonally adjusted, came from the Bureau of Labor Statistics (US Bureau of Labor Statistics, 2020b; 2020a). The two geographic levels for unemployment are included to match the unit of exposure variables in models (i.e., national and state-level).

## 2.2. Statistical analyses

We relied on a preregistered analysis plan, and otherwise note post hoc sensitivity tests. Models were fit using logistic regression, with robust standard errors to account for clustering within states. All models were adjusted for infant sex, maternal age, parity, prior PTB, change in unemployment, season of conception (Dec–Feb; Mar–May; Jun–Aug; Sep–Nov) (Currie & Schwandt, 2013), and the linear yearly trend based on conception year (Martin et al., 2018). Treating racial incidents as national exposures, we first modeled associations between racial violence incidents per gestational period and PTB odds. Exposure was measured using dichotomous variables representing the occurrence of any racial incident relative to none (Model 1A). Results with number of racial incidents as discrete categories (i.e., 1, 2, 3, 4, and 5+) are reported in Supplementary Material. We preferred dichotomous exposure variables to number of incidents as inferences were similar while the former was more parsimonious and avoided multicollinearity problems.



In Model 1B, we added maternal sociodemographic characteristics to account for differences in conception cohorts that may bias estimates (i.e., education, marital status, paternity acknowledgment, payment source for delivery, and maternal smoking during pregnancy). Following reviewer feedback describing potential confounding due to monthly differences in PTB, we fit post hoc sensitivity tests to adjust for calendar month fixed effects (Model 1C) (Currie & Schwandt, 2013).

In place of racial incidents, we also defined exposure using national search interest by gestational period (on a  $\log_{10}$  scale), with the intent to more closely proxy for awareness of racial violence, and fit a similar series of models (Models 2A-2C). Similar models were fit using SGA as the outcome.

To examine geographic patterning, we tested whether racial violence is more strongly associated with PTB in incident relative to non-incident states and in states with greater relative to less interest. Along with covariates from Models 1A and 2A, we controlled for state-specific unemployment changes from preconception to first trimester (in place of national), month fixed effects, and state fixed effects to account for stable state-specific differences in PTB. In the first test, we added dummy variables for each gestational period representing if any of the 49 racial incidents occurred within the mother's state of residence (Model 1D); these are akin to interaction tests as state fixed effects and national incidents are included. As our second test, in Model 2D, adjusting for national interest by gestational period, we added state-specific relative interest in racial incidents (ranging from 0 [no search interest] to 1 [highest interest among states]) and interacted state relative interest with  $\log_{10}$ (national interest). For periods with no national interest in racial violence incidents, the value of 0 was assigned for state relative interest to prevent observations from being dropped and a binary variable was added to represent periods with no national interest. This approach is consistent with suggestions for testing internal moderators (where moderator variables apply only to some observations) (Mirowsky, 2013).

We included supplemental models and sensitivity tests. First, we expected that spillover from racial incidents would primarily be experienced by Black Americans. To compare racial differences in associations between racial violence and PTB odds, we fit the above models using the national sample of births to White mothers, who otherwise met the same inclusion criteria as the sample of births to Black mothers. Second, to differentiate between severity of PTB and consider whether findings are sensitive to the threshold selected, we examined very/extreme preterm (i.e., gestational length <32 weeks) and moderate preterm birth (i.e., 32–36 weeks) as alternative outcomes. Low birth weight was also examined as an outcome. Additionally, post hoc sensitivity tests were added in the peer review process (Supplementary Material, Table S8).

### 3. Results

Descriptive statistics are presented by race in Table 1. We visualize the preterm birth rate by race for each conception month and timing of high publicity racial incidents in Fig. 1. This figure demonstrates the frequent occurrence of high publicity anti-Black violence. National search interest in racial incidents is plotted on a  $\log_{10}$  scale with number of racial incidents in Supplementary Material, Fig. S1. When timing of exposure is coded by gestational period (i.e., three-month preconception period and pregnancy trimesters), only 16.9% of Black women were unexposed to a national high publicity racial incident in their first trimester, with 15.9, 16.6, 9.2, 14.2, and 27.3% exposed to 1, 2, 3, 4, and 5+ incidents, respectively. Exposure in other gestational periods was comparable, within ~5 percentage points. Considered as a state-level exposure, 10.1% of Black births had first trimester exposure to a high publicity racial incident in their state, and mean state interest in racial violence during the first trimester was 0.47 (SD = 0.23) for mothers in incident states relative to 0.29 (SD = 0.17) in non-incident states; these values weight incidents by national interest, with 0–1 representing

**Table 1**  
Descriptive statistics by self-reported maternal racial group.

Variables	Black	White
	%	%
Maternal age categories		
≤19-years-old	9.5	4.3
20–24 years	32.2	19.1
25–29 years	29.3	30.5
30–34 years	18.4	30.4
≥35 years	10.6	15.8
Parity		
Primiparous	37.3	40.6
2	28.5	33.3
3-5	30.6	24.3
6+	3.5	1.8
Prior preterm birth	4.5	2.9
Infant sex (% female)	49.2	48.7
Maternal education <sup>a</sup>		
No high school diploma	15.1	7.6
High school diploma/GED	35.5	21.6
Some college, no Bachelor's	35.0	30.9
Bachelor's degree or higher	13.6	39.4
Marital status <sup>a</sup>		
Married	22.4	68.8
Unmarried, paternity acknowledgment	43.5	22.3
Unmarried, no paternity acknowledgment	32.6	7.3
Insurance at delivery <sup>a</sup>		
Private	25.9	62.4
Medicaid	68.5	30.6
Self-pay	1.5	2.7
Other	3.5	3.6
Smoking during pregnancy	7.2	11.5
Preterm birth	12.2	7.1
Small-for-gestational-age birth	10.3	4.7

<sup>a</sup> Category 'unknown' not shown in table.

portion of maximum search interest for the state.

#### 3.1. Exposure to national racial incidents

Results when racial violence incidents are conceptualized as national exposures are shown in Table 2. Black mothers experiencing any racial incidents relative to none in their first trimester had 5% higher PTB odds (Model 1A, Table 2). Expressed as marginal effects, probability of PTB was 0.48% higher for Black women with exposure to any racial violence incident in the first trimester (95% CI: 0.24, 0.72) – representing an increase from 11.78% to 12.26%. Exposure in other periods was non-significant. Maternal sociodemographic covariates were added in Model 1B to account for potential compositional differences among conception cohorts ending in a live birth, and estimates were similar. In post hoc sensitivity tests that adjusted for calendar month of conception instead of season, first trimester exposure to racial incidents was no longer associated with PTB odds (see Model 1C). Estimates for month of conception are shown in Supplementary Material, Table S1.

In place of any racial incident as the exposure, we also modeled number of racial incidents as discrete categories. Inferences are similar in these models (see Supplementary Material, Table S2), with associations mostly between first trimester exposure and PTB odds. However, no dose-response relationship between number of incidents and PTB odds was evident in the first trimester or other periods. Adjusting for month fixed effects, all estimates for number of incidents were non-significant. These models are reported as supplemental because of potential multicollinearity problems, with variance inflation factor (VIF) scores exceeding 10.

National interest in racial incidents was modeled as an exposure, with the assumption that national interest proxies for racial stress more closely than number of incidents. See the lower panel of Table 2 for results. In Model 2A, national interest during the first trimester was associated with 2% higher PTB odds while interest during preconception and third trimester was each associated with 1% lower PTB odds. The

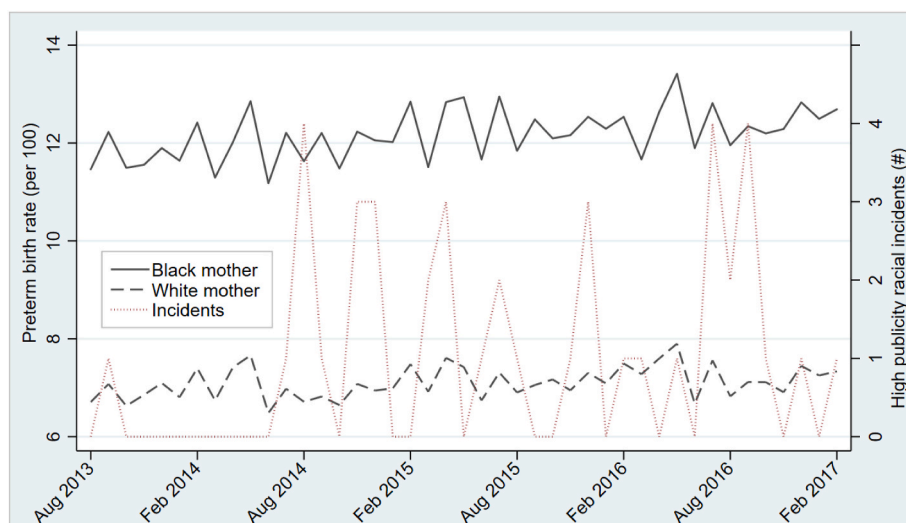


Fig. 1. Plot of preterm birth rate by month of conception for Black and White mothers and timing of high publicity racial incidents.

Table 2

Logistic regression results for exposure to racial violence incidents and national interest by gestational period as predictors of PTB odds for Black mothers ( $n = 1,600,619$ ).

Any racial incident by period (none = ref.)	Model 1A		Model 1B <sup>a</sup>		Model 1C <sup>b</sup>	
	OR	95% CI	OR	95% CI	OR	95% CI
Preconception	0.99	[0.97, 1.01]	1.00	[0.98, 1.02]	1.02	[0.99, 1.04]
First trimester	<b>1.05</b>	[1.02, 1.07]	<b>1.05</b>	[1.02, 1.07]	1.00	[0.98, 1.03]
Second trimester	0.99	[0.96, 1.02]	0.99	[0.96, 1.02]	1.00	[0.97, 1.04]
Third trimester	1.01	[0.99, 1.03]	1.02	[1.00, 1.03]	1.00	[0.98, 1.02]
National interest by period (log <sub>10</sub> scale)	Model 2A		Model 2B <sup>a</sup>		Model 2C <sup>b</sup>	
	OR	95% CI	OR	95% CI	OR	95% CI
Preconception	<b>0.99</b>	[0.98, 0.99]	<b>0.99</b>	[0.98, 1.00]	1.00	[0.99, 1.01]
First trimester	<b>1.02</b>	[1.02, 1.03]	<b>1.03</b>	[1.02, 1.03]	1.00	[0.99, 1.01]
Second trimester	0.99	[0.98, 1.00]	<b>0.99</b>	[0.98, 1.00]	1.00	[0.99, 1.01]
Third trimester	<b>0.99</b>	[0.98, 1.00]	<b>0.99</b>	[0.98, 1.00]	1.00	[0.99, 1.00]

Note. Bolded odds ratios are significant at  $p < .05$ . All models adjusted for infant sex, maternal age, parity, prior preterm birth, change in unemployment rate, season of conception, and yearly trend, unless specified.

<sup>a</sup> Further adjusted for maternal education, marital status/paternity acknowledgment, insurance coverage at delivery, and cigarette smoking in pregnancy.

<sup>b</sup> Calendar month of conception fixed effects are included in place of season of conception.

marginal effect of each log<sub>10</sub> unit increase in first trimester national interest was equivalent to 0.26% higher probability of PTB (95% CI: 0.18, 0.34); for reference, 1 SD = 0.79 for log<sub>10</sub>(national interest). Estimates were similar when adjusting for maternal sociodemographic covariates (see Model 2B). When adjusting for month fixed effects in Model 2C, estimates for national interest in each gestational period were attenuated and non-significant. Also, we considered whether associations between national interest and PTB odds were non-linear by categorizing exposure into four levels of national interest. In a model paralleling Model 2C, none of the exposure variables was significant (Supplementary Material, Table S3). Thus, across different modeling approaches, national exposure to high publicity racial violence was not associated with PTB odds when adjusting for month of conception.

Parallel models were fit with SGA as the outcome and results are depicted in Supplementary Material, Table S4. None of the exposure variables were associated with SGA odds among Black mothers.

### 3.2. Geographic patterning of associations

We tested if associations between racial violence and PTB odds were larger in states where incidents occurred or where search interest was higher (see Supplementary Material, Table S5 for regression results). Net of exposure to national incidents and irrespective of exposure period, mothers residing in states where racial incidents occurred did not experience higher PTB odds relative to mothers in non-incident states. However, preconception and first trimester state relative interest moderated associations between national interest and PTB odds. The pattern of moderation was contrary for preconception and first trimester exposure, with the association between state relative interest and PTB becoming more positive during periods of higher preconception national interest but more negative during periods of higher first trimester national interest. See Supplementary Material for probability of PTB plotted as a function of state relative interest and national interest (Fig. S2), and for a discussion of the finding.

### 3.3. Sensitivity and supplemental tests

To consider how spillover from anti-Black violence may vary by racial group, we fit models with the sample of live births to White mothers. Results are shown in Supplementary Material, Table S6. Contrary to expectations of non-significant associations, findings parallel results with Black mothers. White mothers had 3% higher PTB odds with first trimester exposure to any racial incidents (see Models 1A and 1B). First trimester national interest in racial violence was associated with 1% higher PTB odds whereas interest during preconception and third trimester periods were associated with 1% lower odds (Models 2A and 2B). Expressed as marginal effects, first trimester exposure to any racial incidents (relative to none) and each log<sub>10</sub>(national interest) were associated with 0.18% (95% CI: 0.08, 0.28) and 0.08% (95% CI: 0.04, 0.11) higher probability of PTB, respectively. These estimates are approximately one-third of the magnitude of estimates from similar models for Black mothers. Adjustment for month fixed effects attenuated associations between first trimester exposure and PTB odds to null.

We examined moderate preterm birth, very preterm birth, and low birth weight as alternative outcomes for Black mothers in supplemental models (see Supplementary Material, Table S7). Adjusting for month fixed effects and other covariates, results showed little evidence for

associations between racial violence and adverse birth outcomes, with the exception of higher very PTB odds with preconception and second trimester exposure to any racial incident relative to none.

Post hoc sensitivity tests included excluding infant sex as a covariate, alternative specifications of third trimester exposure, and modeling cumulative exposure to racial violence from preconception through seventh gestational month. Results are described in Supplementary Material, [Table S8](#).

#### 4. Discussion

Public attention to contemporary anti-Black violence and racism increased following the police killings of Eric Garner, Michael Brown, and Freddie Gray, among other well-known incidents ([Bonilla & Rosa, 2015](#); [Buchanan et al., 2020](#); [Freelon et al., 2016](#)). These publicized incidents may have widely influenced mental health in the US population ([American Psychological Association, 2017](#); [Curtis, Washburn, et al., 2021](#)), but the scale of spillover and impact on physical health (e.g., reproductive outcomes) is not well understood. Using a sample of 8.2 million live births to Black and White mothers with conceptions from August 2013 through February 2017, we examined the timing of 49 racial violence incidents and corresponding national interest as predictors of PTB. Findings from our preregistered analyses were consistent with the hypothesis that exposure to high publicity racial violence in the first trimester was associated with elevated PTB odds for Black mothers. Estimates varied across models but PTB odds were approximately 5% higher with exposure to any racial incident relative to none and with increased national interest (i.e., equal to two  $\log_{10}$  units). However, first trimester exposure was no longer associated with PTB in post hoc sensitivity tests when adding calendar month fixed effects. Moreover, results from other models also support a likely null relationship between our measure of racial violence exposure and elevated PTB odds – namely, the lack of a dose-response relationship with number of racial incidents as the exposure, and absence of moderation by geographic variables (i.e., state-of-occurrence).

Our preregistration did not specify controlling for month fixed effects because of a limited number of yearly observations (i.e., three or four occurrences of each calendar month). Rather, we initially included seasonal indicators as controls, consistent with prior research ([Goin et al., 2021](#); [Stieb et al., 2016](#)) and evidence of differences in gestational length and birth weight by season ([Currie & Schwandt, 2013](#)). Residual confounding from coarse temporal controls was possible, however, especially as May conceptions had elevated PTB odds ([Currie & Schwandt, 2013](#)) when first trimester exposure to racial violence was relatively high. Moreover, PTB rates increased during the study period, as previously shown ([Martin et al., 2018](#)). Given seasonality in racial violence and the frequent occurrence of incidents in the final 2.5 years, inclusion of the more robust set of temporal controls (i.e., month fixed effects, yearly trend) could lead to underestimates of the association between racial violence and PTB. However, the consistency across models with which estimates were attenuated due to month fixed effects suggests residual temporal influences likely biased, at least partially, results from preregistered analyses.

A few prior studies have similarly examined spillover on reproductive health from fatal police violence using temporal variation at the level of census tracts or metropolitan statistical areas ([Goin et al., 2021](#); [Jahn et al., 2021](#)). Results from [Goin et al. \(2021\)](#) indicate that fatal police violence in one's area is associated with increased hazard of moderate and late PTB. [Jahn et al. \(2021\)](#) found exposure to fatal police violence was linked to greater pregnancy loss in the general population. These studies reported estimates only adjusted for season and seasonal rhythms, respectively, such that alternative specification of sample-wide temporal patterns (e.g., monthly differences in outcomes) represent important robustness checks. Future research is needed that carefully disentangles seasonal influences (e.g., influenza, temperature extremes, pollution) from temporal variation in high publicity racial violence.

Evidence of race-specific findings was inconclusive. Contrary to expectations, we found that White mothers had higher PTB odds when exposed in their first trimester to any racial incident relative to none and as national interest increased, although estimates were smaller relative to those from models with Black births. Associations between racial violence and PTB odds for White mothers were attenuated to null when adjusting for month fixed effects. Existing research provides mixed support for race-specific associations between fatal police violence and adverse birth outcomes. In [Goin et al. \(2021\)](#), using race-concordant analyses where the race of the victim of police violence and birthing parent matched, fatal police violence was associated with moderate PTB for Black mothers with female infants and late PTB for Latina mothers with female infants, although tests with different PTB categories and for male infants were non-significant. In contrast, [Jahn et al. \(2021\)](#) found that police killings of Black persons were not associated with pregnancy loss for Black women but predicted greater pregnancy loss for White women. Research also has linked other forms of racialized societal stressors to preterm and low birth weight births for persons of the targeted ethnoracial group ([Gemmill et al., 2019](#); [Lauderdale, 2006](#); [Novak et al., 2017](#)), although null associations have been reported as well ([El-Sayed et al., 2008](#)). Stressors were disparate across these studies but involved highly visible events that likely led to an increasingly hostile or threatening sociopolitical climate for targeted racial/ethnic groups ([Gemmill et al., 2019](#); [Lauderdale, 2006](#); [Novak et al., 2017](#)). Thus, research is warranted that moves beyond interpersonal discrimination to consider racialized societal stressors as determinants of population-level reproductive outcomes, especially for targeted groups ([Alhusen et al., 2016](#); [Premkumar et al., 2017](#)).

Expectations for a link between racial violence exposure and PTB were based on stress-related pathways to premature parturition, yet psychosocial and physiological stress also could result in changes in fertility patterns and selection in utero that may confound estimates of the stress-PTB link ([Bruckner & Catalano, 2018](#)). That is, acute stress could increase individual PTB risk for pregnancies that result in a live birth while also influencing who conceives and who carries a pregnancy to live birth. One study documented greater pregnancy loss, defined using live birth identified conceptions, in areas with more fatal police violence ([Jahn et al., 2021](#)). Stress pathways could be counter-opposing and even seemingly improve population-level birth outcomes if exposed birthing cohorts are of lower social and biological risk than unexposed cohorts ([Eskenazi et al., 2007](#); [Noelke et al., 2019](#)). Prior research provides some support for these distinct pathways. For instance, birthing cohorts exposed to unemployment shocks prior to conception have lower PTB rates as women with higher pregnancy risks are more likely to delay fertility ([Noelke et al., 2019](#)). Elevated stress hormones and immunologic dysregulation increases risk of pregnancy loss, especially in early gestation ([Frazier, Hogue, Bonney, Yount, & Pearce, 2018](#)). However, existing literature is mixed concerning how the gestational timing of stress exposure influences pregnancy loss, with evidence for selection in utero at multiple points in gestation ([Bruckner & Catalano, 2018](#)). Counter-opposing pathways may account for our finding of a null association between racial violence and PTB, and even the finding that preconception and second and third trimester exposure to national interest was associated with lower PTB odds, although these latter results were attenuated to null when adjusting for month fixed effects. Thus, associations between discrete stressors and PTB are complex and may depend on the timing of stress in relation to gestation and the extent of pregnancy loss. Future studies need to simultaneously examine stress-related changes in fertility, selection in utero, and spontaneous PTB to identify time-dependent stress effects.

There were notable study limitations. First, the cutoff used to select high publicity racial incidents was arbitrary, although selected at a level to retain known incidents with variation in public interest. We also omitted high publicity incidents that did not meet our criterion (e.g., non-lethal violence), and thus did not capture all types of public anti-Black violence. Second, we considered exposure to racial violence only



as discrete stressors whereas their impact on health may primarily arise from repeated or chronic exposure. Indeed, considerable literature has documented diverse paths through which racism is associated with PTB, which includes chronic exposure to discrimination (Giurgescu et al., 2011). Third, use of estimated month of conception resulted in two limitations: error in estimating month of conception and exposure misclassification due to a relatively coarse temporal unit when defining exposure for three-month periods. However, we viewed these limitations as unlikely to affect study inferences as systematic error in exposure misclassification was improbable and because we used multiple approaches to measure exposure to racial violence. Fourth, estimates from Google Trends are aggregated measures of interest with ambiguous motivations for search behavior, and we lacked person-level measures of awareness or distress from racial incidents. Finally, we relied on racial classifications that include within-group heterogeneity (e.g., Caribbean and African American categories) and used only mother's reported race.

Study strengths include using a natural experiment and a preregistered analysis plan with specific hypotheses to improve the rigor and transparency of the design, and the inclusion of a large sample of nearly all live births to US-born Black and White mothers during the study period. In addition, we focused on the gestational timing of exposure to test whether the first trimester acts as a sensitive period for stress. By focusing on high publicity incidents and including national interest as proxies of racial stress, we addressed a limitation in prior research where fatal police violence incidents were treated equally (Bor et al., 2018; Goin et al., 2021; Sewell et al., 2020).

## 5. Conclusion

Black or African American women experience higher risk pregnancies and a disproportionate share of adverse perinatal outcomes. Chronic stress and racism experiences are presumed risk factors, but research has focused on personal life stress or direct discrimination during pregnancy. We tested whether exposure to highly public anti-Black violence is associated with preterm birth for Black women but found inconclusive evidence. Our approach for identifying high publicity racial violence and measuring public interest offers a promising direction for research on societal stressors, including anti-Black violence. Given the large scale of these stressors, the potential threat to population health is immense.

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## Data statement

The preregistration and select study materials are available at <https://osf.io/u7kbp/>. Access to natality records with geographic identifiers is restricted, and approval must be obtained through the National Center for Health Statistics (see <https://www.cdc.gov/nchs/nvss/nvss-restricted-data.htm>). Public natality records without maternal state of residence can be obtained at <https://www.nber.org/research/data/vital-statistics-natality-birth-data>.

## CRedit authorship contribution statement

**David S. Curtis:** Conceptualization, Data curation, Methodology, Investigation, Formal analysis, Visualization, Writing – original draft, Writing – review & editing, Project administration, Funding acquisition. **Ken R. Smith:** Conceptualization, Writing – original draft, Writing – review & editing, Funding acquisition. **David H. Chae:** Conceptualization, Methodology, Funding acquisition. **Tessa Washburn:** Data

curation, Investigation, Writing – original draft. **Hedwig Lee:** Writing – original draft, Writing – review & editing. **Jaewhan Kim:** Conceptualization, Validation, Funding acquisition. **Michael R. Kramer:** Conceptualization, Methodology, Writing – review & editing, Funding acquisition.

## Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

## Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.ssmph.2022.101112>.

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