The Enduring Impact of Prenatal Exposure to Racial Riots on the Black Population*

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Abstract

This paper provides causal evidence on the long-term health consequences of prenatal exposure to racial riots in the United States. We construct a novel database linking historical riot events to individual-level mortality records and Census data, and estimate difference-in-differences models that exploit quasi-random variation in birth timing relative to riot events. Our design compares changes in longevity across adjacent nine-month cohorts exposed to anti-Black versus non-Black riots. We find that prenatal exposure to anti-Black riots reduces Black Americans' life expectancy by about 7.1 months. This effect is nearly twice the magnitude of the longevity reduction associated with prenatal exposure to lynchings documented in prior research, suggesting that the broader community disruption caused by riots—including housing loss, resource deprivation, and social dislocation—may have more severe long-run health consequences than individualized acts of racial terror. Our findings underscore the enduring biological imprint of structural racial violence and contribute to a growing literature documenting how early-life exposure to social shocks can shape long-term human capital and health outcomes.

Keywords: Racial Riots, Prenatal Exposure, Mortality, Longevity, Structural Violence, Early Life Shocks

JEL Codes: I14, I18, J15, J18, N31, N32, Z13

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1. Introduction

Prior research demonstrates that in utero exposure to violent conflict, such as terrorism, civil unrest, or war, can have lasting effects on health, including increased risks of low birth weight, preterm birth, disability, and premature mortality (Camacho, 2008; Currie et al., 2022; Lee, 2014; Mansour & Rees, 2012; Torche & Villarreal, 2014). Black Americans are disproportionately exposed to structural and community-level violence, including excessive policing, residential segregation and displacement, and environmental injustice (Boyd et al., 2020; O'Flaherty & Sethi, 2019). Among the most extreme historical instances of such violence are racial riots, which involved large-scale destruction of Black neighborhoods, mass displacement, and lasting economic and social dislocation. Events like the 1919 Red Summer and the 1921 Tulsa Massacre were not isolated or spontaneous acts of disorder but organized and racially motivated assaults that devastated entire communities. While their historical significance is well documented, the long-term health consequences of these events remain largely unexplored in empirical research.

Recent work by Vu et al. (2023) offers compelling evidence that racial violence can have enduring biological effects. Their study found that prenatal exposure to lynchings—more localized and individualized acts of racial terror—reduced longevity among Black males by approximately 3.7 months. Building on this research, our study investigates whether prenatal exposure to racial riots—one of the most pervasive forms of racially motivated violence—has long-term consequences for individual health and longevity. While lynchings represented targeted acts of terror with symbolic significance for the broader community, riots directly affected far more individuals through physical violence, property destruction, and community displacement. These differences suggest that the mechanisms linking racial violence to long-term health outcomes might differ in important ways between lynchings and riots. Prenatal exposure to riots could affect fetal development not only through heightened maternal stress but also through reduced access to essential resources such as housing and nutrition, as well as the erosion of community support systems. These compounded disruptions may produce more severe and lasting effects on health across the life course.

To examine these relationships empirically, we construct a novel database by linking historical riot events to individual-level mortality outcomes and Census data. In particular, our dataset combines information on 186 racial riots that occurred across the United States between 1900 and 1935 with mortality datasets from the CenSoc project and 1940 Census. This allows us to identify individuals born in riot-affected communities during specific time windows and track their mortality outcomes later in life. We exploit variation in the timing of riots relative to individual birthdates to implement a difference-in-differences design. The first difference compares outcomes between individuals born 1–9 months after a riot event (exposed in utero to riots) and those born 10–18 months after a riot event. The second difference compares this temporal exposure gap across cohorts exposed to riots targeting Black communities versus those exposed to riots targeting other racial or ethnic groups.

Our main finding is that prenatal exposure to anti-Black riots is associated with a reduction in longevity of approximately 7.1 months among Black Americans. This effect persists across multiple specifications with varying sets of controls and fixed effects. Critically, this mortality impact is nearly twice the magnitude of the 3.7-month reduction in longevity that Vu et al. (2023) found for in utero exposure to lynchings. This substantially larger effect aligns with our hypothesis that the more pervasive community disruption caused by riots would generate stronger health consequences than exposure to lynchings.

Our identification strategy relies on quasi-random variation in the timing of conception relative to riot events. We conduct extensive balance tests examining observable characteristics of parents across treatment and control groups, finding little evidence of systematic differences that might confound our estimates. We also conduct an omnibus test of whether the full set of parental characteristics jointly predicts riot exposure, conditioning on the set of fixed effects. The results further support the idea that our empirical strategy, based on adjacent nine-month cohorts, plausibly produced treated and control groups that are uncorrelated with baseline characteristics after accounting for our set of fixed effects.

This paper contributes to the growing literature on the long-run consequences of racial violence in the U.S. Prior work has shown that historical racial violence affected a wide range of political, economic, and social outcomes—from reduced patenting activity (Cook, 2014) and depressed voter registration (Williams, 2022) to diminished tax revenues (Logan, 2019) and economic outcomes following the 1960s riots (Collins & Margo, 2004, 2007). Our study contributes to this literature by providing causal evidence that in utero exposure to racial violence—specifically, anti-Black riots—reduced life expectancy among affected Black cohorts. In doing so, we extend recent work by Vu et al. (2023), who document mortality effects of prenatal exposure to lynchings, by showing that riots, as a more disruptive form of racial violence, had even more severe long-term health consequences.

2. Background

From 1900 through the 1930s, African American communities across the United States faced widespread White supremacist violence and "race riots" that caused mass death and destruction. This era, often called the "nadir" of American race relations, saw thousands of Black Americans lynched, burned, shot, and mutilated by white mobs (*Racial Violence and the Red*

Summer, 2020). The wave of violence culminated in episodic riots and massacres, such as the 1919 "Red Summer," when White mobs attacked Black neighborhoods in more than two dozen cities. For example, Chicago's 1919 race riot (the deadliest of that summer) left 38 people dead (23 African American, 15 white), injured over 500, and rendered roughly 1,000 Black families homeless (*Chicago Race Riot of 1919 | Summary, History, & Facts | Britannica*, 2025). Other events were even more brutal: in Elaine, Arkansas (September 1919) an estimated 100 Black men and women were killed (*Racial Violence and the Red Summer*, 2020), and in the 1921 Tulsa Race Massacre Tulsa's prosperous Greenwood district ("Black Wall Street") was destroyed over 18 hours – some 35 city blocks burned and up to 300 Black residents killed *(Tulsa Race Massacre | The Encyclopedia of Oklahoma History and Culture, n.d.).* These incidents occurred against the backdrop of Jim Crow segregation and a resurgent Ku Klux Klan, and they helped drive the Great Migration of Southern Blacks northward.

The immediate impacts of these riots were profound – homes, businesses, and lives were lost – and the long-run scars lingered for decades. Recent economic history research on specific events highlights lasting harm. For example, the 1921 Tulsa massacre devastated Black wealth: one study finds that Black household heads exposed to the riot were about 4.2 percentage points less likely to own homes in the ensuing decades, and that their average occupational status remained depressed even into the late 20th century (Albright et al., 2021). Vu et al. (2023) show that in utero exposure to the lynching of a Black victim led to a 3.7 month decrease in longevity among Black males. Taken together, these findings underscore how racial violence was not only a tool of terror in its time but also a structural force that shaped racial inequality in the United States.

3. Data

***Mortality data*.**— The main data source for this study is the Social Security Administration's Death Master File (DMF) and Numident data, which was accessed through the CenSoc Project website (Goldstein et al., 2021). The Censoc project provides three data products. The DMF data reports deaths to male individuals over the years 1975 – 2005. The Numident data reports deaths to both males and females over the years 1988 – 2005. The Berkeley Unified Numident Mortality Database (BUNMD) reports deaths to both males and females with a high coverage for the years 1988 – 2007 and thin coverage for the years prior to 1988. The DMF and Numident data are linked to the 1940 census make them the ideal for this project. This linkage was conducted by us using consistent individual-level unique identifier *histid*, which is available in both datasets and the 1940 census. This linkage enables us to infer individuals' county of birth, a critical variable for our analysis. We pooled the DMF and Numident data together to increase our power through increase sample size.

An advantage of our data is that they contain millions of observations before sample selection. Therefore, we can restrict the sample to narrowly defined cohorts (exposure or no exposure to racialized riot during gestation) while still retaining sufficient statistical power.[3] Another advantage of using the 1940-census-DMF-Numident linked sample is that it includes information on individuals' family characteristics and socioeconomic outcomes in 1940. This data allows us to explore possible mechanisms in later analyses.

As mentioned above, it is important to identify the county-of-birth to assign the local area a specific type of in utero exposure. The 1940 census contains information on county-of-residence

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[3] While other data sets can be linked to the 1940 census, they have very small sample sizes. For example, the Health and Retirement Study provides an initial linked sample of slightly more than 9,000 individuals, which is too small to divide into narrower geographical, cohort, and specifically racial groups.

in 1940 as well as the county-of-residence five years prior (i.e., in 1935) if the individual had moved. To increase the accuracy of the county-of-birth measure, we used the cross-census linking rules provided by the Census Linking Project (Abramitzky et al., 2020) to link data from a portion of individuals from the 1940 census back to their data from earlier censuses (1900-1930). We used the county information in the first census in which individuals were observed as the county-of-birth. For those who were not linked to any of the earlier censuses, we used county-of-residence in 1935 if applicable (if they had moved between 1935 and 1940) or the county-of-residence in 1940 as a proxy for county-of-birth. We are able to identify 76.2% observations in historical censuses and use the 1940 county as a proxy for only 23.7% observations. This method of assigning county of birth may introduce measurement errors that could potentially confound the estimates. However, two factors suggest it does not bias our estimates. First, as reported in Section 5.3, we find little evidence that successful data linkage is correlated with riot exposure. Second, we account for potential cross-state mobility by including birth-state–by–state-of-residence (1940) fixed effects, which allows us to compare migrants and non-migrants separately. Results remain stable, indicating that cross-state mobility does not drive our findings (Figure 2).

Riot data.— Our main source of data on racial riots is the American Riots database. Historian Paul A. Gilge originally constructed this database for his book, Rioting in America (Gilje, 1999). Ryan Jones created a digital repository of these data, which Turchin (2012) used for analysis; we also use these digitized data for our study. Gilge defines a riot as “any group of twelve or more people attempting to assert their will immediately through the use of force outside the normal bounds of law.” Through an extensive review of news media and other sources, Gilje documents the circumstances of each riot, including the date, location (i.e., city), a description of the precipitating event, whether race played a role in the violence, and the outcome.[4] We cross reference these data with other relevant sources on race riots , including Rucker and Upton (2006)'s data on race riots and the ICPSR political violence database (Levy, 1991). We focus on the period 1900-1935 because individuals born before 1900 are likely to be too old to appear in the 1975 death records.

We derived the main estimation sample using the following criteria. First, we excluded non-Black individuals. Second, we restricted the sample to four birth cohorts: Black births born in months 1–9 after each riot event against Black individuals, Black births born in months 10–18 after each riot event against Black individuals, Black births born in months 1–9 after each riot event against non-Black individuals, and Black births born in months 10–18 after each riot event against non-Black individuals.

Appendix Table A-1 reports summary statistics for the final sample. The average age at death in the final sample is 912.8 months (76 years). The average years of schooling in 1940 among these individuals is 8.3 years.[5] By design, approximately half of the individuals were exposed to a riot incident in utero, as each 9-month treatment cohort is paired with a 9-month control cohort. Also, by design, 100% of individuals were Black. In our final sample, 55% out of the riots were targeting Black communities, the rest are against other race/ethnicity. 25.3% of individuals were exposed to a riot event targets Black during gestation (treated observations).

[4] In one such example, Gilje describes a 1917 event in Chester, PA where "July: Race riot. Began with the killing of a National Guardsman by a black man because the Guardsman insulted two black girls. Two days later rioting broke out that lasted three days. Three whites and three blacks killed. Many other injured. Many blacks left town.". Sources: Crisis. October, 1917, p.313; Downey and Hyser. No Crooked Death, p.157; NYT, July 26, 1917

[5] Individuals in some of these cohorts, specifically those born after 1923, had not completed their education in 1940.

4. Empirical Strategy

Our identification strategy aggregates multiple difference-in-differences estimates to tease out the long-term impacts of in utero exposure to racialized civil unrest. For each riot event, we define narrowly spaced treatment and control cohorts of Black births within the same city. The treatment cohort comprises individuals born during the first to ninth month following a riot (months 1-9 post-event), while the control cohort includes those born in the subsequent nine-month window (months 10–18 post-event). By construction, these cohorts are locally defined in time and space, ensuring that they are likely to be subject to similar unobserved shocks and demographic trends, apart from the direct effects of the riot itself.

The primary threat to identification in this design is the systematic difference in birth timing across cohorts. Specifically, individuals in the treatment group are born nine months earlier than those in the control group, which may influence key outcomes such as residential mobility before the decennial census, match rates to the Censoc mortality data, and truncation within the Censoc mortality data. To address this concern, we construct an additional layer of comparison using births from the same states and same riot time periods but exposed to events not targeting Black communities. These "non-Black riot" events generate analogous treatment and control birth cohorts, allowing us to difference out any effects related to the nine-month timing gap that are common across similarly timed cohorts exposed to civil unrest more broadly.

To implement this strategy, our empirical specification includes fixed effects for state-by-riot timing. The identifying variation thus comes from within-state comparisons between similarly timed Black cohorts differentially exposed to riots targeting Black individuals versus those exposed to riots targeting non-Black individuals. This approach relies on the assumption that Black births in these parallel cohorts are comparable along unobserved dimensions apart from the nature of riot targeting. In the subsequent section, we provide evidence on the plausibility of this assumption by examining observable parental characteristics.

If the effects of being born nine months earlier—including any associated seasonal variations in birth outcomes—are similar for Black births exposed to anti-Black riots and those exposed to riots not targeting Black communities, then the difference between these two groups effectively addresses concerns related to the treatment cohort being born nine months before the control cohort. Thus, our empirical strategy can be interpreted as a method of pooling evidence across 186 riot events to estimate the long-term impacts of in-utero exposure to racially targeted unrest. Specifically, we estimate models of the following form:

$$Y_{irct} = \alpha_1 + \alpha_2 ExposedInUtero_t \times RiotTargetsBlack_r + \alpha_3 ExposedInUtero_t + \alpha_4 RiotTargetsBlack_r + \alpha_5 X_{irct} + \alpha_6 Z_{ct} + \xi_c + \zeta_t + \eta + \varepsilon_{irct} \quad (1)$$

Where Y_{irt} denotes the longevity of person i who exposed to riot event r and born in city c during month-year t. $ExposedInUtero_t$ equals 1 for those born 1-9 months following a riot event in their city and 0 for those born 10-18 months after a riot event. $RiotTargetsBlack_r$ is an indicator variable that takes on a value of 1 if the riot is against Black and 0 if it is against other race/ethnicity.

The matrix X_{irct} includes parental education controls (0 years, equal or more than 5 years, equal or more than 9 years, some college, and missing) and paternal occupational score dummies (below the sample median, above the sample median, and missing). However, these controls are missing for a substantial portion of the sample. The city-level controls (represented by Z_{ct}) comprise several factors, including the proportion of the population in different age groups (11-18, 19-25, 26-55, >56 years old); the percentages of women, Black people, immigrants, homeowners in the population; literacy rate, average occupational score, and proportion of families with children below age 5. In robustness checks, we control for state-by-cohort exposure to significant historical events affecting mortality, such as the Rosenwald school program and the Great Depression. We clustered the standard errors at the city level to account for serial correlation in error terms. The coefficient of interest is α_2, which estimates the impact of exposure to a riot event during pregnancy among Black American.

City fixed effects (represented by ξ_c) control for both the observable and unobservable characteristics of each city that remain constant over time. Birth year-month fixed effects (represented by ζ_t) are included to capture time-invariant unobserved heterogeneity that might affect birth cohorts. As mentioned above, we include fixed effects for state-by-riot timing (represented by η), so identification relies on within-state comparisons of similarly timed Black cohorts who were differentially exposed to riots targeted against Black individuals versus those not.

In addition to mitigating concerns about differential matching rates to the mortality data and potential truncation due to age at death, our empirical specification is designed to account for both location-specific and time-specific shocks that are likely to affect Black communities similarly. By comparing adjacent cohorts within the same city, we also control for local shocks—such as economic or environmental changes—that may influence both cohorts similarly. In section 5.3, we address additional potential sources of bias, including the relatively low match rates between the DMF/Numident data and the 1940 Census.

It is important to note that our design exploits short-lived in-utero exposure to local riot events to estimate causal effects on later-life outcomes. For each riot, we compare children born 1–9 months afterward (exposed in utero; "treated") with those born 10–18 months afterward (not exposed; “controls”). Because exposure is transient—limited to a single birth cohort window—this setting differs from the staggered-rollout difference-in-differences frameworks emphasized in recent work (De Chaisemartin & D’Haultfœuille, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2021). Instead, it is an aggregation of multiple difference-in-differences estimates.

4.1. Identification Assumption

As discussed above, a key identifying assumption underlying our empirical strategy is that there are no systematic differences in the selection of individuals between the treatment and control groups. If there were differences in survival into adulthood by exposure to riots between, for example, children of different socioeconomic statuses, the models would provide biased estimates that reflect (in part) endogenous survival rather than solely riot exposure. To assess the plausibility of this assumption, we leverage data on observable family characteristics from the 1940 Census.

Table 1 reports estimate from regressions of parental characteristics on an indicator for in utero exposure to a riot, controlling for city-level covariates and fixed effects as specified in Equation 1. Across most outcomes, we find that the estimated coefficients are small in magnitude and statistically indistinguishable from zero. Two exceptions emerge: Column 5 shows a higher rate of missing data on maternal education, and Column 12 shows a lower proportion of fathers with below-median occupational income among Black children exposed in utero to riots targeting Black communities. The latter pattern suggests potential downward bias in our estimates, as higher parental socioeconomic status is generally associated with improved health and longevity outcomes later in life (Almond et al., 2018; Currie, 2009; Hayward & Gorman, 2004; Montez & Hayward, 2011). However, these imbalances are not consistent across outcomes, weakening concerns about systematic selection.

To further support these balancing tests, we conduct an omnibus test of whether the full set of parental characteristics jointly predicts riot exposure, conditioning on the same set of fixed effects. The results, presented in Appendix Table A-2, reveal that nearly all coefficients are both economically negligible and statistically insignificant. The joint F-test confirms this lack of association (F-statistic = 1.398; p-value = 0.204). Overall, the results in this section support the idea that our empirical strategy, based on adjacent nine-month cohorts, plausibly produced treated and control groups that are uncorrelated with baseline characteristics after accounting for our set of fixed effects.

5. Results

5.1. Main Results

The main results of the regressions based on Equation (1) are reported in Panel A of Table 2. The model in Column 1 includes city fixed effects, birth-year-month fixed effects, and birth-state-by-riot-time fixed effects. We then added parental controls and city-level controls in the models reported in Columns 2 and 3, respectively. Across specifications, the estimated effect of prenatal exposure to riots remains stable, providing evidence of robustness to the inclusion of additional covariates. The fully parametrized model in Column 3 indicates a 7.1-month shorter life span among Black individuals exposed to riots targets Black during prenatal development.

To contextualize the magnitude of this effect, it is instructive to compare it with other well-documented early-life exposures. For example, Vu et al., (2023) examine the long-term effects of racialized violence in the form of lynchings and estimate that in utero exposure to a lynching event reduces life expectancy among Black men by 3.7 months. Thus, the effect of riot exposure on longevity is approximately 90% larger than the estimated impact of lynching. This larger effect is consistent with the hypothesis that riots represent more widespread, community-level disruptions that may generate a broader set of stressors and material hardships during gestation, thereby compounding the adverse consequences for fetal development and long-term health.

Placebo Test.— Next, we repeat our analysis including only White births, who are excluded in the main analysis, and who we assume were unaffected by the riots targeting Black communities. This placebo check helps address potential concerns about unobserved cohort- or city-specific changes in longevity that might coincidentally align with riot exposure. As anticipated, the results, presented in Panel B of Table 2, show no detectable changes in longevity among White individuals that correlate with the timing of anti-Black riots. This strengthens our confidence that the observed negative effects on longevity are indeed specific to the Black population exposed to these riots, rather than being driven by broader, unobserved environmental factors.

5.2. Placebo Treatment Effects

We next present graphical evidence of treatment effects for cohorts not exposed to the event in utero, in the spirit of an event study in Figure 1. While these cohorts were born after the focal event, they function analogously to a "pre-period" in standard event studies. We expand the analysis beyond the original treatment and control cohorts by including additional post-event birth cohorts. Specifically, we define the treatment cohort as individuals born 1–9 months after the event (i.e., those exposed in utero), and the control cohort as those born 10–18 months post-event. We then introduce four placebo cohorts: those born 19–27 months (placebo 1), 28–36 months (placebo 2), 37–45 months (placebo 3), and 46–54 months (placebo 4) following the event. Figure 1 plots the estimated coefficients and 95% confidence intervals for each cohort, with the control group normalized to zero. The estimates for all placebo cohorts are smaller in magnitude and statistically insignificant from zero. The insignificant effects on placebo cohorts mitigates concerns about differential attrition, mobility since birth, and truncation in the linked mortality and census data.

5.3. Endogenous Merging

The final sample is constructed through several steps of data merging, with the most critical step involving the linkage of Social Security Administration DMF/Numident data to the full-count 1940 census. A potential concern is the endogeneity introduced by this linking process. For instance, if people of lower socioeconomic status are less likely to be in the DMF/Numident-census linked sample and simultaneously have higher probability of exposure to riots, the results may understate the true effects as these people usually have on average, lower longevity for other reasons.

To address this concern, we empirically test whether individuals with higher exposure to riot are less likely to appear in the DMF/Numident-census data. In so doing, we start by the original population from the full-count 1940 census and implement the same sample selection criteria as in Section 3. We then merge this dataset with our final sample at the individual level and create a dummy variable that indicates successful merging. We then regress this successful merging dummy on the exposure measures of Equation (1), conditional on covariates and fixed effects. The results, reported in Table 3, show an extremely small point estimate, suggesting a change of 0.2% with respect to the mean (column 3). These results suggest that endogenous merging between the DMF/Numident data and the 1940 Census is unlikely to bias our main estimates in any meaningful way.

5.4. Robustness Checks

Figure 2 presents robustness checks of our main findings across a range of alternative specifications. The first estimate reproduces the benchmark model from column 3, Panel A of

Table 2. We begin by addressing concerns about selective migration. To account for potential cross-state mobility, we compare migrants and non-migrants separately by including birth-state–by–state-of-residence in 1940 fixed effects. The results remain stable, suggesting that cross-state mobility does not bias our estimates.

Next, we add controls for exposure to Rosenwald Schools, a large-scale philanthropic initiative that expanded educational opportunities for Black children in the early 20th century. The estimated effects remain similar to the main results.

We then account for the exposure to the Great Depression. Using county-level data from the Federal Deposit Insurance Corporation (1984), we calculate the decline in bank deposits between 1929 and 1933 and interact this measure with the Black population share and birth-year fixed effects. The coefficients show little change once these controls are added.

We also account for major public health campaigns. For the malaria eradication efforts of the 1920s (Bleakley, 2010; Venkataramani, 2012), we incorporate a state-specific malaria exposure index interacted with the Black population share and birth-year fixed effects. For the Rockefeller Sanitary Commission's hookworm eradication campaign of the 1910s (Bleakley & Lange, 2009; Henderson, 2018), we include county-specific treatment rates from Henderson (2018) interacted with the Black population share and birth-year fixed effects. In both cases, our estimates remain robust.

Finally, we address two additional historical shocks that may have affected child health and labor markets in the South. The Boll Weevil infestation reduced labor demand and altered child labor dynamics (Baker, 2015; Hoehn-Velasco, 2018), while the establishment of County Health Departments expanded access to affordable care. We construct exposure measures for each and include them separately in our regressions. The inclusion of these controls leaves our main results essentially unchanged.

Taken together, these exercises demonstrate that the estimated effects are robust to a wide set of historical events related to education, economic shocks, health campaigns, and local labor market conditions that could have affected the life expectancy of people in our sample.

6. Conclusion

This paper provides new causal evidence on the long-term effects of racialized collective violence in the United States. By combining a dataset of racialized riots between 1900 and 1935 with individual-level mortality records from the CenSoc project and 1940 Census data, we estimate the effect of prenatal exposure to riots on longevity among Black Americans. Our difference-in-differences strategy exploits quasi-random variation in birth timing relative to riot events and compares changes in longevity across adjacent nine-month cohorts exposed to anti-Black versus non-Black riots. Our findings indicate that Black individuals exposed in utero to anti-Black riots experienced a reduction in life expectancy of approximately 7.1 months, nearly double the longevity loss recently estimated for prenatal exposure to lynchings (Vu et al., 2023). This pattern is consistent with the view that riots—through their broader geographic reach, greater material destruction, and large-scale disruption of community networks—produced more severe and enduring consequences than more targeted acts of racial terror.

More broadly, our findings highlight that the legacy of racialized violence is not limited to immediate economic or political disruption but may extend into the biological lives of the next generation. Understanding these long-term impacts is critical for assessing the full cost of racial violence and its contribution to persistent racial disparities in health and longevity.

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Tables

Table 1 – Balancing Test: In Utero Exposure to Racial Riot and Observable Parental Characteristics

	Outcomes:												
	Mother's education zero	Mother's education equal or more than 5 years	Mother's education equal or more than 9 years	Mother's education college	Mother's education missing	Father's education zero	Father's education equal or more than 5 years	Father's education equal or more than 9 years	Father's educatio n college	Father's education missing	Father's occupation al income score missing	Father's occupationa l income score below median	Father's occupation al income score above median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Exposed in Utero × Riot Targets Black	.0054 (.0061)	.023 (.0139)	.0171 (.0177)	-.0121 (.0078)	.0327** (.0163)	.0086 (.0058)	.023 (.0139)	.0171 (.0177)	-.0184* (.0099)	.0176 (.0199)	.0168 (.0183)	-.0429** (.0196)	.0261 (.0184)
Observations	12,352	12,352	12,352	12,352	12,352	12,352	12,352	12,352	12,352	12,352	12,352	12,352	12,352
R-squared	.0627	.1188	.1548	.0694	.3011	.0782	.1188	.1548	.08	.2276	.1521	.113	.2294
Mean DV	0.015	0.899	0.608	0.026	0.428	0.021	0.899	0.608	0.025	0.588	0.246	0.171	0.583

Notes. This table examines whether there are observable differences in parental characteristics between the treatment and control groups. Specifically, we estimate parental characteristics as a function of in utero exposure to riots, while controlling for city fixed effects, year-month of birth fixed effects, and birth-state-by-riot-time fixed effects, following the specification outlined in Equation (1). Standard errors, clustered on city level, are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 2 – In Utero Exposure to Racial Riot and Later-Life Longevity

	Outcome: Age at death (months)		
	(1)	(2)	(3)
Panel A. Black births			
Exposed in Utero × Riot Targets Black	-7.4588**	-7.25**	-7.0575**
	(3.0798)	(2.9393)	(3.2499)
Observations	12,352	12,352	12,352
R-squared	.4125	.4138	.415
Panel B. Placebo test using White births			
Exposed in Utero × Riot Targets Black	.0704	.0745	.2968
	(.7788)	(.781)	(.8626)
Observations	303,120	303,120	303,120
R-squared	.2249	.2254	.2254
City fixed effects	✓	✓	✓
Birth-year-month fixed effects	✓	✓	✓
Birth-state-by-riot-time fixed effects	✓	✓	✓
Parental Controls		✓	✓
City-level controls			✓

Notes. Panel A reports estimates from Equation (1) among Black births. Panel B reports estimates Equation (1) among White births as a placebo test. Standard errors, clustered on city level, are in parentheses. Parental controls include dummies for maternal education, paternal education, and paternal socioeconomic status. City covariates include share of population in different age groups, share of females, average number of children less than 5, share of Blacks, share of immigrants, share of home owners, and literacy rate. FE = fixed effects.

*** p<0.01, ** p<0.05, * p<0.1

Table 3 - Exposure to Riot and Endogenous Data Linking

	Outcome: Successful merging between 1940 census and DMF data		
	(1)	(2)	(3)
Exposed in Utero × Riot Targets Black	-.00439	-.00416	.00161
	(.00443)	(.0042)	(.00305)
Observations	19,065	19,065	19,065
R-squared	.99632	.99634	.99976
Mean DV	0.652	0.652	0.652
Birth-state-by-riot-time fixed effects	✓	✓	✓
Parental Controls		✓	✓
City-level controls			✓

Notes. This analysis tests whether individuals with higher exposure to riot are less likely to appear in the DMF-1940 census linked data. The dependent variable is a dummy indicating successful merging between the 1940 census and DMF data. Standard errors, clustered on city level, are in parentheses. Regressions include birth-state-by-riot-time fixed effects and controls. Parental controls include dummies for maternal education, paternal education, and paternal socioeconomic status. City covariates include share of population in different age groups, share of females, average number of children less than 5, share of Blacks, share of immigrants, share of home owners, and literacy rate.

*** p<0.01, ** p<0.05, * p<0.1

Figures

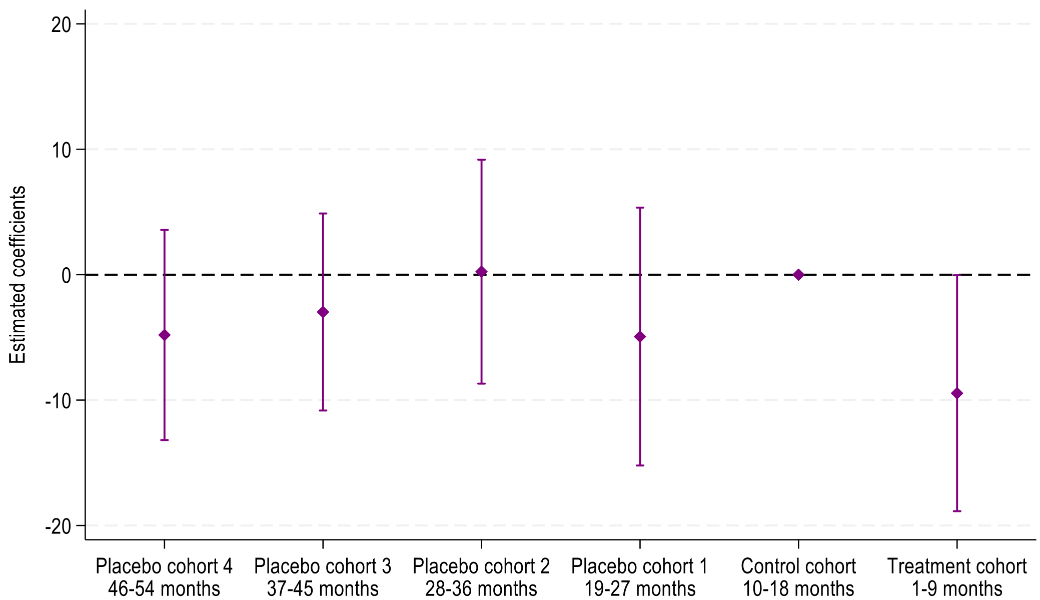

Figure 1 – Event Study Style Results

Notes. This figure presents graphical evidence of treatment effects for cohorts not exposed to the riot event in utero, in the spirit of an event study. Specifically, we expand the analysis beyond the original treatment and control cohorts by including additional post-event birth cohorts. Specifically, we define the treatment cohort as individuals born 1–9 months after the event (i.e., those exposed in utero), and the control cohort as those born 10–18 months post-event. We then introduce four placebo cohorts: those born 19–27 months (placebo 1), 28–36 months (placebo 2), 37–45 months (placebo 3), and 46–54 months (placebo 4) following the event. The vertical axis represents the number of months born relative to a riot incident. Point estimates and 95% confidence intervals for each cohort are illustrated, with the control group normalized to zero. Standard errors are clustered on city. The outcome is age at death (in months). Regressions include city fixed effects, birth-year-month fixed effects, birth-state-by-riot-time fixed effects, and controls. Parental controls include dummies for maternal education, paternal education, and paternal socioeconomic status. City covariates include share of population in different age groups, share of females, average number of children less than 5, share of Blacks, share of immigrants, share of home owners, and literacy rate.

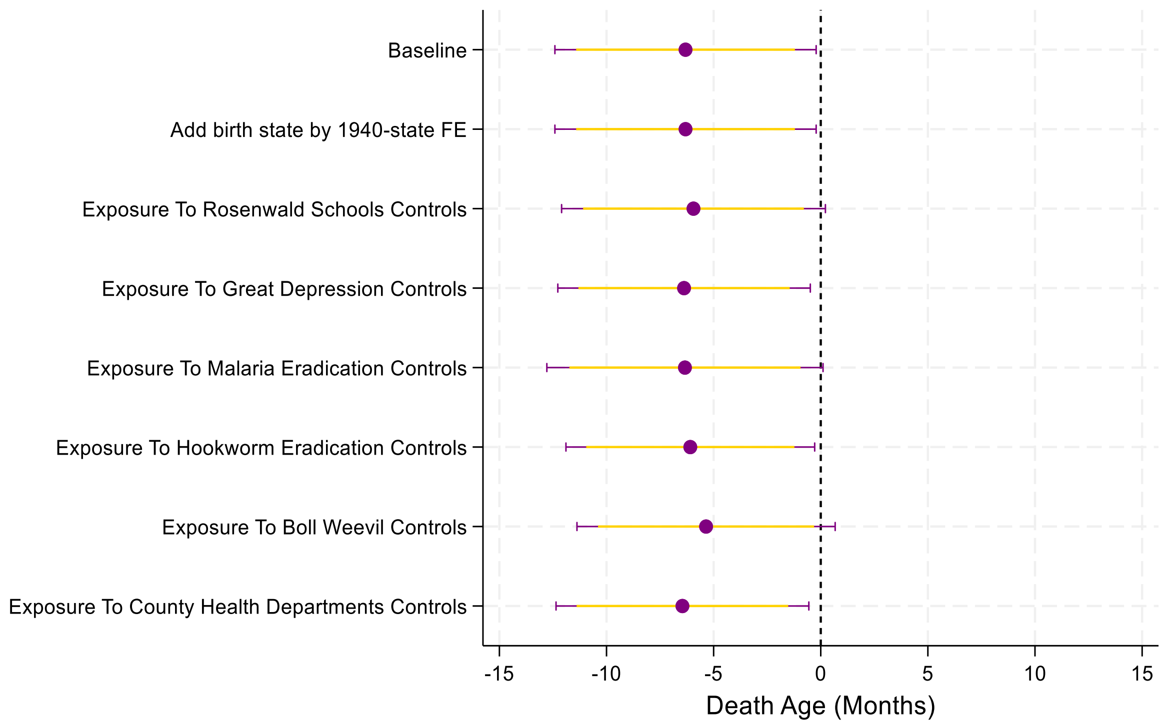

Figure 2 - Robustness Checks across Alternative Specifications

Notes. This figure presents robustness checks across alternative specifications. Point estimates and confidence intervals are illustrated. The whisker lines indicate 95% and the purple lines indicate 90% confidence intervals. Standard errors are clustered at city-level. The outcome is age at death (in months). Regressions include city fixed effects, birth-year-month fixed effects, birth-state-by-riot-time fixed effects, and controls. Parental controls include dummies for maternal education, paternal education, and paternal socioeconomic status. City covariates include share of population in different age groups, share of females, average number of children less than 5, share of Blacks, share of immigrants, share of home owners, and literacy rate.

Online Appendix

Appendix Table A-1 - Summary Statistics

	Mean	SD	Min	Max
Age at death (months)	912.807	102.935	493	1228
Log age at death	4.318	.119	3.714	4.625
Age at death > 70 years	.735	.441	0	1
Year of birth	1918.837	6.723	1900	1938
Year of death	1994.928	7.08	1975	2005
Exposed in Utero × Riot Targets Black	.253	.435	0	1
Exposed in Utero	.471	.499	0	1
Riot Targets Black	.554	.497	0	1
Non-Hispanic Black	1	0	1	1
Years of schooling	8.337	3.777	0	20
Socioeconomic index	17.335	16.098	3	92
Occupational income score	18.034	7.784	3	80
Mother's education zero	.015	.122	0	1
Mother's education 1-12 years	.531	.499	0	1
Mother's education college	.026	.158	0	1
Mother's education missing	.428	.495	0	1
Father's education zero	.021	.143	0	1
Father's education 1-12 years	.366	.482	0	1
Father's education college	.025	.156	0	1
Father's education missing	.588	.492	0	1
Father's occupational score missing	.246	.431	0	1
Father's occupational score below median	.171	.377	0	1
Father's occupational score above median	.583	.493	0	1
Observations		12,352		

Appendix Table A-2 - Joint Balancing Test

	Outcome: Exposed in Utero × Riot Targets Black
	(1)
Mother's education zero	-.00642
	(.01454)
Mother's education 1-12 years	-.00338
	(.00658)
Mother's education college	-.00896
	(.01233)
Father's education zero	.08162**
	(.03951)
Father's education 1-12 years	.05492
	(.03562)
Father's education college	.03841
	(.0388)
Father's occupational income score below median	-.05681*
	(.03244)
Father's occupational income score above median	-.0613*
	(.0353)
Observations	12,352
R-squared	.76
P-Value of joint significance Test	.204
F-Stat of joint significance Test	1.398

Notes. This table examines the correlation between exposure to a riot incidence and parental observable characteristics in the spirit of an omnibus test, conditional on a set of fixed effects. Standard errors are reported in parentheses and clustered at the city level. Regressions include city fixed effects, birth-year-month fixed effects, and birth-state-by-riot-time fixed effects.

**** p<0.01, ** p<0.05, * p<0.1*