Is Lynching Over? Long-Term Effects of In Utero Exposure to Racialized Violence in the U.S. Black Population*

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Abstract

This study explores the long-term health effects of prenatal exposure to racialized violence by analyzing administrative death records linked to historical lynching data. We exploit variation in lynching incidences to understand the impact of this type of racialized violence on old-age longevity. The results reveal a 3.5 month decrease in longevity among Black males who were exposed to the lynching of a Black victim during gestation. This exposure accounts for approximately 10% of the longevity gap between Black and White men in 1980, with no negative effects observed among White men. Further analysis suggests that diminished socioeconomic outcomes are likely explanatory factors.

Keywords: Mortality, Longevity, Lynching, Racialized Violence, Black-White life expectancy gap

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

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1. Introduction

Throughout the life course, Black Americans are more likely to experience poor health and die prematurely than their non-Black counterparts. For example, beginning in infancy, Black individuals have elevated rates of preterm birth and infant mortality (Osterman et al., 2023). These health disparities persist in later stages of life, with Black individuals experiencing higher rates of chronic diseases, including hypertension, diabetes, and heart disease, than non-Black individuals well into old age (Odlum et al., 2020).

One likely important source of racial inequities in health is the unequal burden of environmental stressors prior to birth. Decades of evidence from the fetal origins literature demonstrates that exposure to in utero stressors or "shocks" can have both more immediate (e.g., low birth weight and premature birth) and latent (e.g., chronic disease and premature mortality) health effects (Almond & Currie, 2011). Violence is a prominent example: exposure to intimate partner violence and violent conflict (e.g., terrorism and war) is linked to poor birth outcomes such as preterm birth and low birth weight (Camacho, 2008; Currie et al., 2022; Mansour & Rees, 2012; Torche & Villarreal, 2014) as well as later-life disability and mortality (Lee, 2014). Importantly, in the United States, exposure to violence is not random. Black Americans bear a disproportionate burden of exposure to various forms of violence, including both interpersonal and structural violence (e.g., excessive use of police force in the neighborhood) (O'Flaherty & Sethi, 2019). However, the presence of racialized disparities in violence exposure, combined with data limitations, has posed challenges for analyses that seek to establish whether such exposure causally influences health inequities in later life.

In this study, we examine the causal impacts of in utero exposure to a common form of 20th century violence—lynching—on racial inequities in longevity. Lynching is a form of

extrajudicial punishment in which a group of people, often a mob, kill an individual suspected of a crime without a legal trial. In the United States, lynching was wielded primarily against Black Americans (Bailey et al., 2011; Bailey & Tolnay, 2015; Price et al., 2008). During the 19th and 20th centuries, White Americans used lynching to enforce racial segregation and maintain White supremacy in the U.S. South. Black Americans were often lynched for alleged offenses such as rape, murder, and other acts that were perceived as a challenge to traditional racial order. Interviews with surviving family members of lynching victims have shown that both family members and the wider community experienced trauma, stress, and fear in the wake of this form of racial terrorism and that the resulting mental health effects persisted across generations (Gaston, 2021). Recent studies have found that counties that experienced historical lynching have lower contemporary levels of Black voter registration (Williams, 2022) and higher contemporary levels of unemployment and poverty among their Black populations (Williams et al., 2021).

Emerging evidence suggests that exposure to lynching is also associated with worse population health outcomes. One recent study found that the total number of Black victims as a share of the county-level Black population was associated with higher contemporary levels of overall infant mortality (Abbott et al. 2022). Kihlström & Kirby (2021) showed that counties that experienced lynching in the early 20th century had lower average life expectancies in 2020 than countries with no lynching record. Similarly, Prost et al. (2019) documented lower contemporary mortality rates in counties without a history of lynching. Further, Messner et al. (2016) demonstrated that counties with a history of lynching had higher contemporary rates of homicide. Yet, while these studies provide suggestive evidence of a link between exposure to lynching and poorer health, it remains unclear whether this link is causal and whether exposure to lynching has latent effects that can explain health inequities later in the life course.

Estimating the causal effects of in utero lynching exposure is challenging because there is a lack of individual data on longevity linked to the time and place of birth. Existing studies often rely on county-level life expectancy data, which can obscure vital variation in experiences within counties and overlook the long-term impact of violence during individuals' lives. Additionally, research on lynching and health typically compares counties with a history of lynching to those without, potentially failing to account for unobserved factors, such as variations in economic conditions or social attitudes, that could explain any observed differences in health outcomes.

We address these knowledge gaps by leveraging individual-level data on birth month-year and place of birth drawn from the CenSoc Project to examine long-term effects of in utero exposure to lynching on later-life longevity. We integrate the CenSoc Project data and data from the Historical American Lynching Data Collection Project (Project HAL) and Seguin & Rigby (2019), which documents lynching incidents from 1882 to 1937. We define lynching exposure as being born in a county where a lynching incident involving a Black victim occurred during gestation. We posit that such exposure may have long-lasting effects on individuals' health and well-being, potentially leading to differences in longevity outcomes. This hypothesis is grounded in the understanding that traumatic events experienced during early stages of development can have enduring impacts on individuals' physiological and psychological health (Almond & Currie, 2011).

To estimate the long-term effects of in utero exposure to lynching on old age longevity, we implement a difference-in-differences model comparing the longevity of Black births exposed to lynching in utero (born within 1–9 months after the event) with Black births born in months 10–18 after the event, relative to the analogous difference for White births. The main identification assumption is that there are no systematic differences in the selection of individuals between the treatment and control groups that could be associated with their longevity later in life. This

assumption is supported by the results of balancing tests for differences in observable parental characteristics between the treatment and control groups. We also considered whether any observed effects of lynching may be driven by changes in the sociodemographic composition of the mortality sample. The analysis reveals that there is no significant impact of in utero exposure to lynching on the sociodemographic composition of the mortality sample, indicating that factors such as migration or differential survival into adulthood are not the driving forces behind the results.

We found that Black males exposed to lynching in utero experienced a significant reduction in longevity, approximately 3.5 months. By contrast, the estimated effect for Black females is much smaller in magnitude and statistically insignificant, a pattern that echoes well-established findings on sex-differentiated vulnerability to prenatal insults, whereby male fetuses are generally more sensitive to adverse in utero environments (Eriksson et al., 2010; Kraemer, 2000; Sanders & Stoecker, 2015). The findings suggest that in utero exposure to racial violence played a significant role in exacerbating health disparities between Black men and their non-Black counterparts. In particular, the estimates suggest that such exposure contributed to about 10 percent of the Black-White life expectancy gap observed in 1980.

One potential explanation for the findings revolves around the severe stress and trauma experienced during pregnancy as a consequence of a lynching event in the community, which can have long-lasting impacts on developing fetuses via multiple mechanisms. These mechanisms can be broadly grouped into two categories: culling and scarring. First, in extreme cases, in utero exposure to lynching may result in fetal demise due to the severe stress and trauma encountered by the pregnant person. Second, the intense fear, distress, and trauma associated with being exposed to a nearby lynching during pregnancy can leave enduring ‘scars’ on the developing

fetus.[6] These scars can manifest in various aspects of life, such as educational attainment, wages, and health outcomes, including increased susceptibility to diseases in adulthood (Almond and Currie, 2011).

There is suggestive evidence that the scarring mechanism, rather than culling, appears to be the primary driver of our main results. To investigate the culling mechanism, we examined outcomes such as infant mortality rate, birth rate, and the share of births to Black and White individuals, but found no significant effects of exposure to lynching on any of these outcomes. In contrast, the models showed significant negative effects of in utero exposure to lynching on the socioeconomic scores and occupational income scores of Black (but not White) individuals, suggesting that this exposure may have left lasting scars on their economic prospects.

Prior research has established a link between shocks experienced during the fetal period and lasting declines in adult health, human capital, and labor market outcomes (Almond et al., 2018; Barker, 1990). Building upon this body of knowledge, the current study offers significant insights into the long-term consequences of in utero exposure to racialized violence, particularly lynching, for health outcomes, with a specific focus on disparities in longevity between Black and White individuals. By examining the long-term impacts of such exposure on mortality rates later in life, the results expand researchers' understanding of how early-life experiences contribute to health disparities during adulthood. Further, the findings have broader implications for comprehending the potential consequences of contemporary forms of violence for future health outcomes.

[6] Studies in contemporaneous settings provide descriptive evidence that not only direct exposure to community violence but also hearing about the use of the violence can cause mental stress, depression, and even Post Traumatic Stress Disorder (Scarpa et al., 2006; Shields et al., 2010; Thompson et al., 2019). Research further suggests that stress induced by such exposure during prenatal development may negatively impact birth outcomes (Carlson, 2015; Cozzani et al., 2022).

This paper makes a novel and significant contribution to the literature by uncovering a clear pathway through which anti-Black violence affects the later health of Black individuals who were not directly physically impacted by the violence. While the fetal origins literature is large, it predominantly focuses on stressors linked to prenatal behaviors (e.g., substance use, smoking) or factors that directly affect the birthing person's physical and material well-being (e.g., nutrition, housing, cash transfers). In contrast, research on external and psychological stressors has largely concentrated on shorter-term outcomes. This study, however, seeks to examine external factors beyond a birthing person's control such as acts of racial violence. Understanding the extent to which the overall environment that an expecting parent is exposed to, rather than their behavior, may affect the fetus and transmit what could be understood as "intergenerational trauma" into adulthood is important. While racial violence like lynching is no longer prevalent, its legacy provides a framework for understanding the potential long-term impacts of contemporary racialized stressors, such as police violence, on racial health disparities. The findings highlight a specific pathway through which historical racialized violence can contribute to persistent racial health disparities today.

2. Literature Review

2.1 Relationship between in utero exposure to violence and outcomes

Research shows that exposure to violence during pregnancy is linked to poor infant health outcomes, such as preterm birth and low birth weight. This includes different types of violence, such as terrorism (Camacho, 2008; Quintana-Domeque & Ródenas-Serrano, 2017), wars (Mansour & Rees, 2012), local homicides (Torche & Villarreal, 2014), and intimate partner

violence (Currie et al., 2022).[7] The medical literature has provided ample evidence that increased stress and insecurity act as a biological pathway linking exposure to violence to both birth and adult outcomes. Specifically, maternal stress during pregnancy triggers the hypothalamic-pituitary-adrenal (HPA) axis,[8] leading to elevated levels of corticotrophin-releasing hormone (CRH) that can pass to the placenta; high levels of CRH during pregnancy are associated with adverse birth outcomes (Steine et al., 2020; Wadhwa et al., 2004).

2.2 Racialized violence and terror and health outcomes

Prior research has also shown that racialized violence contributes to population health inequities at the time of birth. In a prominent example, people of Arab descent experienced a substantial increase in racist harassment, violence, and discrimination after the terrorist attacks of September 11 (Padela & Heisler, 2010; Poynting & Noble, 2004). Lauderdale (2006) found that infants of Arab descent born in the subsequent months had lower birth weight and higher rates of low birth weight relative to those of non-Arab decent. Similarly, a more recent study conducted by Bakhtiari (2020) revealed an increase in the incidence of low birth weight among Middle Eastern, South Asian, and Sikh birthing people in California following the events of September 11.

[7] Other factors contributing to maternal mental distress can adversely affect infant health. For example, Black et al. (2016) compare the effects of a birthing parent's parental death in utero versus just before or just after the in utero period, finding that in utero exposure to bereavement leads to more pronounced short-term impacts on birth outcomes. Similarly, Persson & Rossin-Slater (2018) document that family ruptures during pregnancy have greater negative effects on birth outcomes and perinatal complications compared to those occurring after birth. They further highlight that in utero exposure to stress is associated with medium-term impacts on children's mental health, including higher rates of depression during childhood.

[8] The HPA axis relates the hypothalamus (located above the brain stream), pituitary gland (located at the base of the brain), and adrenal glands (located on top of the kidneys). It is evolved as the primary basis for stress response and is involved in a wide array of body processes including energy storage, digestion, depression, and immune system response (Smith & Vale, 2006).

There is also evidence that Black Americans' disproportionate exposure to police violence has negative health consequences. Chegwin et al. (2023) studied the impact of police use of force on infant health outcomes in New Jersey and identified a significant association between use of force and low birth weight among Black birthing parents but not White birthing parents. In an examination of more diffuse effects, Curtis et al. (2021) found that highly public anti-Black violence has a significant negative impact on the mental health of Black Americans.

Because of data limitations and the challenges posed by studying long-ago events, prior studies have focused primarily on contemporaneous and short-term effects. For example, data on police use of force has only been collected since 2015 (by the FBI). We seek to extend this body of knowledge by examining the long-term effects of racialized violence on a crucial health outcome: longevity. While mortality and longevity may be considered extreme outcomes, they serve as accurately measured proxies for cumulative health in old age (Buchman et al., 2012; Mathers et al., 2001). By focusing on longevity, we aim to shed light on the enduring consequences of racialized violence for individuals' health over the life course.

While prior studies have demonstrated associations between lynching and contemporary outcomes, they often rely on aggregate county-level data that obscure individual experiences and fail to establish causal relationships (Abbott et al., 2022; Kihlström & Kirby, 2021; Messner et al., 2016; Probst et al., 2019; Williams, 2022; Williams et al., 2021).[9] Further, such research potentially neglects unobserved factors, such as economic conditions and social attitudes, that can contribute to observed differences in health outcomes. In contrast, our data links individuals' death

[9] Several studies in this literature document adverse effects of lynching on local economic activities or political participation of African Americans during the late 19th and early 20th centuries (Cook, 2014; Jones et al., 2017). There is also descriptive evidence that counties with higher lynching incidences experienced higher outmigration of Black population during the Great Migration (Tolnay & Beck, 1992).

records to their county of childhood residence, allowing us to explore their exposure to lynching during the critical in utero period and track their longevity later in life. By examining the long-term impacts of such exposure on longevity, the current study offers valuable insights into how early-life experiences shape health disparities during adulthood. Moreover, these findings have broader implications in that they elucidate the potential consequences of modern forms of violence for overall health outcomes.

3. Data Sources

The primary data source for this study is the Berkeley Unified Numident Mortality Database (BUNMD), which was accessed through the CenSoc Project website (Goldstein et al., 2021). The BUNMD contains death records for both females and males who died between 1988 and 2005.[10] A key advantage of the BUNMD is that it provides individuals' exact county of birth, which is critical for our analysis. Because this study examines the long-term effects of locally transmitted fear stemming from mob violence, precise information on birthplace at the county level is essential for accurately assigning exposure.

A second advantage of the BUNMD is its very large sample size, comprising millions of observations prior to sample restrictions. This allows us to focus on narrowly defined birth cohorts, such as those exposed or unexposed to mob violence in utero, and small geographic units (counties) while maintaining sufficient statistical power.

A limitation of the BUNMD is that it is not linked to historical Census records, preventing us from observing individuals' characteristics needed to assess key identification assumptions,

[10] The Censoc project provides three data products. 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 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 DMF and Numident data are linked to the 1940 census.

discussed below. To address this limitation, we complement the BUNMD with the Social Security Administration's Death Master File (DMF). The DMF includes death records for male individuals who died between 1975 and 2005 and can be linked to the full-count 1940 U.S. Census. We performed this linkage using a consistent individual-level identifier (histid) available in both datasets. [11] The linked DMF–1940 Census data provide detailed information on individuals' family background and socioeconomic characteristics in 1940, allowing us to examine potential endogeneity in in utero exposure to lynching and to explore mechanisms in subsequent analyses.

A limitation of the DMF is that it does not contain county of birth. Identifying county of birth is essential for assigning local exposure to mob violence. The 1940 Census reports county of residence in 1940 as well as county of residence five years earlier (in 1935) for individuals who had moved. To improve the accuracy of birthplace assignment, we applied cross-census linking procedures from the Census Linking Project (Abramitzky et al., 2020) to link a subset of individuals from the 1940 Census to earlier censuses (1900–1930). For linked individuals, we use the county recorded in the earliest observed census as their county of birth. For those not successfully linked to earlier censuses, we use county of residence in 1935, when available, or county of residence in 1940 as a proxy. Using this approach, we identify county of birth directly from historical census records for 76.2% of observations, while relying on the 1940 residence proxy for the remaining 23.7%. [12]

[11] The match rate between the DMF and the 1940 Census is 11% (see mean of dependent variable in Column 1 of Table 4). We discuss potential concerns regarding the differential matching rate to the DMF in Sections 5.6 and 5.8.

[12] We acknowledge that this method of assigning county of birth may introduce measurement errors that could potentially confound the estimates. However, we have four reasons that this is not likely the case. First, using BUNMD data which reports exact county of birth by the Social Security Administration, we observe almost identical point estimates. Second, as reported in section 5.7, we find little evidence that data merging process is correlated with lynching exposure. Third, even when we only use the county of residence in 1940 as a proxy for county of birth, we observe quite comparable coefficients (Appendix Table H-2). Fourth, as documented in Appendix Table H-1, we find quite small and insignificant effects of exposure to lynching on the probability of migration.

The lynching data were obtained from the Historical American Lynching Data Collection Project (Project HAL) and Seguin & Rigby (2019). This dataset documents lynching incidents spanning the years 1882 to 1937. It is important to note that the HAL dataset includes only incidents that meet the National Association for the Advancement of Colored People (NAACP)'s definition of lynching.[13] While lynching is a particularly visible and well-documented form of racial violence, other forms, such as race riots and individual acts of racially motivated violence, were also prevalent during this period. These events, though not captured in the HAL dataset, could theoretically have similar effects on socioeconomic status and longevity. We merged the lynching data with the mortality data by county-of-birth and month-year of birth.[14,15] The regression models also include county controls as well as literacy rate, average occupational income score, share of immigrants, share of females, and share of people in different age groups. These covariates were extracted from the full-count decennial censuses (Ruggles et al., 2020) and were interpolated linearly for inter-decennial years.

We derived the main estimation sample using the following criteria. First, we excluded individuals from states not covered in the Project HAL and Seguin and Rigby (2019) lynching

[13] The NAACP's definition of lynching requires that four conditions obtain: (1) there must be evidence that a person was killed; (2) the person must have met death illegally; (3) a group of three or more persons must have participated in the killing (to rule out personal vendettas, etc.); and (4) the group must have acted under the pretext of protecting justice or tradition.

[14] Birth month and birth year information are reported in the BUNMD and DMF data. One concern is the accuracy of this information. For instance, it is possible that birth month is recorded January (birth month=1) or June (birth month=6) when the information on birth month is missing. In Appendix Figure K-1, we show the distribution of birth month in the final sample. We do not observe discernible lumping or hipping in these months. Additionally, in Appendix Table K-1, we replicate the results across models that exclude each birth month separately. We observe fairly robust estimates that are similar to the baseline.

[15] One concern in the main results is potential confounding influence of spillover effects from lynching incidents in neighboring counties. To examine this concern, we include counties neighbor to lynching counties and assign two lynching exposures based on the incident in the own county and any of the neighboring counties. We then interact these exposure measures with an indicator of Black. We implement regressions that include lynching fixed effects and county controls. The results are reported in Appendix Table J-1. We observe that the only sizable and statistically significant coefficient is own county exposure. The main take away is that the results do not provide evidence of spillover effects.

data. As a result, the sample primarily consists of Southern states, as the Project HAL database includes lynching information exclusively from that region.[16] Second, we restricted the sample to those born after 1900 and before 1937.[17] Third, we limited the sample to lynchings of African Americans. Lynchings of White Americans are excluded since Whites serve as the control group in our analysis, and incidents involving other racial/ethnic groups are excluded due to their very small sample sizes. For completeness, we provide an analysis of non-Black lynching incidents in the Appendix.[18]

Fourth, we restricted the sample to lynching events with no subsequent lynching in the same county within the following 18 months. Fifth, we excluded events that lack sufficient identifying variation—specifically, those with no racial variation (i.e., only Black or only White births observed after the event) or no timing variation (i.e., no births occurring during the in utero exposure window of 1–9 months after the event or the immediate post-exposure period of 10–18 months after the event).

Sixth, to address concerns about sparse data, we drop lynching events associated with fewer than 10 observations. Finally, we restricted the sample to four birth cohorts after each event:

[16] The final sample includes male individuals from Alabama, Arkansas, California, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia.

[17] Those born in the late 19th century would have had to reach age 75 or older to appear in the DMF data. Since we only observe their mortality conditional on them reaching 75+ years of age, concerns about sample selection bias arise. To mitigate this issue, we excluded these individuals from our analyses. Moreover, because lynching data do not cover the years after 1937, we also excluded cohorts born after this period.

[18] One possible extension is to examine the effects of lynchings targeting other racial groups on the longevity of individuals within those groups. In Appendix Table M-1, we show results examining the impact of White lynchings on White longevity. We observe positive but quite small and statistically insignificant coefficients. Notably, the sample size for White longevity is about 10% of the primary sample used in the main analysis of Black lynchings and Black longevity.

We also considered including lynchings of non-Black, non-White groups, but the sample sizes are extremely limited. In the 1940 Census data, only 0.39 percent of individuals identified as a non-Black, non-White race, and 1.69 percent identified as Hispanic. When examining the effects of lynchings on these groups using non-Hispanic Whites as a comparison, we identified only 14 recorded lynching incidents with non-Black, non-White victims. Additionally, our final sample includes just 8 American Indian or Alaska Native individuals and 357 Hispanic Whites. Given these constraints, extending the analysis to smaller, less-documented racial and ethnic groups would yield highly imprecise and unreliable estimates.

Black males born in months 1–9, Black males born in months 10–18, White males born in months 1–9, and White males born in months 10–18. This approach ensures that the control group for one lynching event is not inadvertently included as the treated group for another, and vice versa.

Figure 1 shows the distribution of lynching incidents across counties in the final sample.[19] The HAL dataset identifies 1,211 counties with at least one recorded lynching.[20] Among these counties, 482 have experienced a singular lynching incident, while 729 have faced multiple incidents. As noted earlier, the sample was restricted to lynching events without a subsequent incident in the same county within the following 18 months and that provide sufficient identifying variation, resulting in a final total of 1,016 lynching incidents.

In Figure 2 and Appendix G, we present temporal and spatial distribution of lynchings in detail. Figure 2 illustrates frequency of lynching incidence across states in the final sample. Appendix Figure G-1 shows the distribution of observations around a lynching incidence within a county. Additionally, we illustrate the exact timing of each lynching incidence in each county and each state in the final sample in Appendix Figure G-2 through Appendix Figure G-16. Appendix Table G-1 shows the distribution of lynching incidences by temporal proximity within the same county, as reported in the original HAL lynching data. Finally, we show the timeseries evolution of lynching incidents across census divisions in Appendix Figure G-17.

Appendix Table A-1 reports summary statistics for the final sample. The average age at death in the final sample is 903 months (75.3 years). The average years of schooling in 1940 among these individuals is 7.6 years.[21] Non-Hispanic Blacks and Whites account for 28% and 70%

[19] As noted above, the sample primarily consists of Southern states, as the Project HAL database includes lynching information exclusively from that region. However, as shown in Appendix Table I-1, the results remain broadly consistent when lynching incidents from outside of the South are included.
[20] In Appendix C, we show that the results are robust when we drop California and Texas from the final sample.
[21] Individuals in some of these cohorts, specifically those born after 1923, had not completed their education in 1940.

percent of the sample, respectively. By design, approximately half of the individuals were exposed to a lynching incident in utero, as each 9-month treatment cohort is paired with a 9-month control cohort. Among the final sample, 14.5% of individuals were Black and were exposed to a lynching during gestation (treated observations).

4. Empirical Method

Our identification strategy can be described as pooling multiple difference-in-differences estimates across lynching events. For each lynching event, the treatment and control cohorts of Black male births are adjacent, occur in the same county, and only consist of 9 months each. The treatment cohort includes Black births born during the first 1–9 months following the event, while the control cohort comprises Black births born in months 10–18 after the event. This design ensures that the cohorts are likely to experience similar unobserved shocks, apart from the effects of the lynching itself. The primary concern with this approach is that the treatment cohort was born 9 months earlier than the control cohort. This timing difference could systematically affect the probability of having moved prior to the census, being matched to the mortality data, and truncation within the mortality data.

To address this concern, we incorporate two control cohorts of White births from the same county, born during the same two 9-month periods as the two Black birth cohorts. If the effects of being born 9 months earlier (including any related seasonal effects on birth health) are similar for the White births and the Black births, then the Black-White difference addresses the concern about the treatment cohort being born 9 months prior to the control cohort.[22] Thus, our regression can be

[22] One might be concerned about the collective "memory" of lynching incidents. Specifically, if the memory of past lynchings persists within a community, the control group of individuals born 10–18 months after a lynching could also be affected. We anticipate that this would bias our estimates toward zero if the control group is still partially treated.

seen as a method of aggregating evidence across 1,016 lynching events to estimate the long-term impacts of in utero exposure to racial violence. Specifically, we estimated models of the following form:

$$Y_{ikct} = \alpha_{1k} + \alpha_2 Lynching_i \times Black_i + \alpha_3 Lynching_i + \alpha_4 Black_i + \alpha_5 X_{ikct} + \alpha_6 Z_{ct} + \varepsilon_{ikct} \quad (1)$$

Where Y_{ik} denotes the longevity of person i who exposed to lynching event k and born in county c during month-year t. α_{1k} represents fixed effects for each separate lynching event. $Lynching_i$ equals 1 for those born 1-9 months following a lynching event in their county and 0 for those born 10-18 months after a lynching event. $Black_i$ is an indicator variable that takes on a value of 1 if the person is Black and 0 if the person is White.

The matrix X_{ict} includes parental education controls (0 years, 1-12 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 county-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, and immigrants 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 Sheppard-Towner Act, Rosenwald schools, the 1918 Flu epidemic, and the Great Depression. We clustered the standard errors at the county level to account for serial correlation in error

terms.[23] The coefficient of interest is α_2, which estimates the impact of exposure to a lynching event during pregnancy among Black individuals.

In addition to addressing concerns about cohort differences in matching rates to the mortality data and the truncation by age at death, our specification also accounts for county-specific and time-specific shocks that affect all racial groups similarly, as well as county-race-specific shocks that may impact both cohorts in the same way. We will address other potential sources of bias below, including low match rates between DMF and the 1940 census, differential effects of race and cohort on fetal deaths, and mobility since birth.

5. Results

5.1. Balancing Tests

The main assumption in the empirical strategy is that there are no systematic differences in the selection of individuals between the treatment and control groups that could be associated with longevity later in life. If there were differences in survival into adulthood by exposure to lynching between, for example, children of different socioeconomic statuses, the models would provide biased estimates that reflect (in part) endogenous survival rather than solely lynching exposure. We can empirically test this assumption using data on observable family characteristics from the 1940 census. The results in Table 1 assess the credibility of this identifying assumption by examining whether there are any observable differences in characteristics between the treatment and control groups. Specifically, we estimated parental characteristics as a function of in utero exposure to lynching, lynching incidence fixed effects, and county-level controls (as in Equation 1).

[23] The results remain robust when clustering standard errors at the lynching incident level.

The results show that parents in the treatment and control groups are broadly comparable in observable characteristics. Most coefficients are small and statistically insignificant, indicating no meaningful differences. In particular, lynching exposure is not significantly associated with maternal education, paternal education, or fathers' occupational income scores. To further support these balancing tests, we examine the correlation between exposure to a lynching incidence and parental observable characteristics in the spirit of an omnibus test, conditional on similar set of fixed effects. The main purpose of this test is to examine whether the combined set of parental characteristics has any predictive power in explaining lynching exposure. These results, presented in Appendix Table F-1, indicate that all coefficients are both economically small and statistically insignificant. Moreover, the joint significance test for the full set of these characteristics yields a small F-statistic that is statistically insignificant (p-value = 0.15).

5.2. Endogenous Fertility

Research has shown that stress, particularly racial and ethnic violence, is associated with increased rates of fetal death, miscarriage, and infant mortality (de Oliveira et al., 2021; Duncan et al., 2017; Quintana-Domeque & Ródenas-Serrano, 2017; Sanders & Stoecker, 2015). These findings raise the concern that our results might be confounded by changes in fertility and subsequent infant deaths. For example, if Black birthing people in counties where a lynching occurred experienced higher rates of fetal death and miscarriage, the infants who survived might differ systematically from their peers in ways that could affect later-life longevity.

We tested this possibility using historical county-level fertility and mortality data extracted from Bailey et al. (2016). One drawback of this test is that the data do not begin until 1915 and cover only a small fraction of U.S. counties.[24] We merged these data with the lynching database

[24] The data reports fertility for about 366 counties in 1915, 1380 counties in 1920, and 2775 counties in 1930.

based on county and year. In order to examine dynamic changes in fertility and infant mortality outcomes, we compare the differences in these measures in lynching counties versus non-lynching counties in different years relative to a lynching incident, conditional on county fixed effects, year fixed effects, and county covariates. These results are reported in Figure 3. We do not observe a significant change or a discernible pattern of change in different years around the lynching incident for infant mortality rates and share of birth to White birthing people (top left and bottom left panels). Compared with birth rates of 2-3 years prior to the incident, birth rate is lower in years following the lynching as well as prior years. For the share of births to Black birthing people, we do observe some increases in 3-6 years post-lynching, although all point estimates are statistically insignificant, which hinders further interpretations. The overall picture of this graph mitigates concerns regarding a specific pattern or contemporaneous changes in fertility or infant mortality rates.

5.3. Main Results

The main results of the regressions based on Equation (1) are reported in Table 2. Columns 1, 2, and 3 show the estimates for males, females, and the full sample, respectively. The results reveal a statistically significant 3.5-month reduction in longevity for Black males exposed in utero to a lynching event (column 1). In contrast, the estimated effect for Black females is much smaller in magnitude and statistically insignificant (column 2), a pattern that echoes well-established findings on sex-differentiated vulnerability to prenatal insults, whereby male fetuses are generally more sensitive to adverse in utero environments (Eriksson et al., 2010; Kraemer, 2000; Sanders & Stoecker, 2015). The pooled estimate in column 3 yields a statistically significant reduction of approximately 2.15 months, reflecting the composition of the two effects.

A limitation of the BUNMD is that it cannot be linked to historical Census records, precluding direct observation of individual characteristics necessary to assess the validity of our identification strategy. To address this, we supplement the BUNMD with the Social Security Administration's Death Master File (DMF), which contains death records for male individuals who died between 1975 and 2005 and can be linked to the full-count 1940 U.S. Census.

Table 3 presents the estimated results of Equation (1) using the DMF data. The model in Column 1 includes a fixed effect for each lynching event. We then added parental controls and county controls in the models reported in Columns 2 and 3, respectively. Across all three specifications, the estimated coefficients remain stable and robust, suggesting that the absence of parental characteristics in the BUNMD is unlikely to bias our main estimates in any meaningful way. The fully specified model in Column 3 indicates that Black men exposed to lynching during prenatal development experienced a statistically significant reduction in longevity of approximately 3.6 months, a finding that is remarkably consistent with the estimates obtained using the BUNMD data.

Alternative control group.— In this specification, we present results using an alternative control group. Instead of using White births in the same county as in the main specification, we compare Black births across different counties within the same state. This approach leverages a difference-in-differences framework, where the sample is restricted to Black male births. We estimate the following regression model:

$$Y_{ik} = \alpha_{1k} + \alpha_2 Lynching_i + \alpha_3 X_{ict} + \alpha_4 Z_{ct} + \xi_c + \zeta_{st} + \varepsilon_{ik} \quad (2)$$

where α_{1k} are lynching-event-specific fixed effects (so a separate FE for each lynching event k that is "on" for the 2 cohorts born in the county of the event in the 18 months following the event),

ξ_c are county fixed effects, and ζ_{st} are year-month specific fixed effects that differ by state (indexed by s). The results are presented in Table 4. Panel A revealed an estimated 4.1-month reduction in longevity for in utero exposure. This estimate is slightly larger in magnitude compared to the main specification. As a falsification test, Panel B applies the same model to White male births. Consistent with our expectations, the placebo regression yields small and statistically insignificant coefficients.[25]

To better understand the magnitude of these effects, we can compare them with the documented effects of other early-life exposures on longevity. For example, Halpern-Manners et al. (2020) implemented a twin fixed effect strategy and linked Social Security death records with historical censuses to examine the relation between education and longevity. Their results showed that one additional year of education is associated with a 3.4-month increase in longevity. Therefore, the impact of lynching exposure is roughly equivalent to the impact of one fewer year of education. Noghanibehambari & Fletcher (2023) estimated the impact of in-utero and childhood exposure to the topsoil erosion due to the Dust Bowl of the 1930s on old-age longevity. They found an intent-to-treat effect of 0.9 months and estimated an effect of 2.2 months among those whose fathers worked on farms, who were more likely to be in the treated group. Therefore, lynching exposure in utero is about 60% more harmful for longevity than exposure to the most disastrous environmental catastrophe in the 20th century.

Event-Study Style Placebo Tests.— Next, we present graphical results of the treatment effects for cohorts that were not affected, in the spirit of an event study in Appendix D. The placebo

[25] As another placebo test, we examine whether in-utero exposure to county-level executions — rather than lynchings — affects later-life longevity among Black individuals. Unlike lynchings, executions were carried out through formal legal proceedings and thus lacked the unpredictable, terror-inducing character of extrajudicial mob violence. Therefore, we would expect any effect of execution exposure on Black longevity to be smaller in magnitude than that of lynching exposure, if present at all. Appendix Table N-1 confirms this: across all specifications, the estimated coefficients are small and statistically indistinguishable from zero.

cohorts serve as a "pre-period," although in our context, they represent cohorts born after the event. Specifically, we expand the number of cohorts analyzed. In addition to the cohort born 1–9 months after the event (treatment cohort, lynching = 1) and the cohort born 10–18 months after the event (control cohort), we include cohorts born 19–27 months post-event (placebo cohort 1) and 28–36 months post-event (placebo cohort 2). We graphically present the estimated effects and confidence intervals for these cohorts, showing results sequentially from placebo cohorts (2 and 1) to the control cohort (normalized at zero) and finally to the treatment cohort. Across all three specifications, the findings indicate small and statistically insignificant effects for the placebo cohorts, bolstering the credibility of our control group.

Heterogeneity by Cohort and Trimester.— As shown in Figure 2, a large portion of lynching incidences occurred in 1900s and 1910s. Therefore, one would expect relatively larger impacts for these cohorts. We examine these heterogeneities in Appendix E. We find a considerably larger point estimate for cohorts born in 1900s while the coefficient of cohorts born in 1910s is comparable to those born in 1920s-1930s. Another potential source of heterogeneity is related to the trimester of exposure to lynching. Using distinct indicators for exposure to lynching in each trimester, we examine the effects of trimester specific exposure and report the results in Appendix Figure E-1. Although the point estimates for lynching exposure effects appear slightly larger in the third trimester compared to the first and second, the confidence intervals overlap substantially, suggesting that these differences are not statistically significant.

5.4. Age at Exposure

In the main results, we focus on in utero exposures; however, children of other age groups could also be affected by lynching incidents through various channels. For example, lynching could induce a stressful environment in households, specifically among Black families. Research

in various settings has linked childhood stress with later-life health and longevity (Birn et al., 2017; Goodman & Armelagos, 1988; Hedges & Woon, 2010; Taylor, 2010). To examine the impact across other age groups, we extend the analytic sample to include cohorts exposed to lynching between the ages of 0 and 7. Specifically, we estimate models of the following form:

$$y_{ict} = \beta_{1k} + \beta_2 Black_i + \sum_{j=-1}^{7} (\alpha_{j1} Lynching_{ct}^{j} \times Black_i + \alpha_{j2} Lynching_{ct}^{j}) + \beta_3 X_{ict} + \beta_4 Z_{ct} + \zeta_t + \varepsilon_{ict}$$

where j denotes age at exposure. For instance, $j = -1$ corresponds to in utero exposure, $j = 0$ corresponds to exposure during the first 12 months of life, and so forth. ζ_t denotes birth year-month fixed effects, and all remaining parameters are defined as in Equation (1). The comparison group consists of male births for whom the lynching incident occurred prior to conception, specifically those born 10–18 months after a lynching event.

In this specification, the coefficient α_{j1} captures the effect of in utero lynching exposure on later-life longevity among Black men relative to White men. The coefficient α_{j2}, in turn, captures the effect of lynching exposure among White men and serves as a placebo test (because Black men experienced stress and social-emotional pressure as a result of a lynching, while White men presumably did not); prior research has documented the race-specific impacts of lynching (Abbott et al., 2022; Kihlström & Kirby, 2021; J. Williams, 2022).

The results are reported in two panels in Figure 4. The top panel shows the coefficients for the interaction between age-at-exposure and Black. The bottom panel illustrates the main effects of age-at-exposure (the effects on White males). We observe fairly small and statistically insignificant coefficients across various postnatal ages. For in utero exposed cohorts, we observe a discernible and sizable reduction in longevity among Black men (top panel). Among White men

(main effects, bottom panel), we observe point estimates for both in utero exposure and postnatal age exposures that are indistinguishable from zero, economically and statistically.

5.5. Robustness Checks

Figure 5 reports the robustness of the main findings across alternative specifications. The first estimate reproduces the model in column 1 of Table 2 as a benchmark. The second regression adds census-region-of-birth by birth-year fixed effects to account for cross-region convergence in longevity across cohorts. The regression of line 3 controls for linear evolution of counties' unobserved features by including county-specific trends in birth-year. The coefficients for these two estimations are very similar to those for the baseline.

Next, we control for the malaria eradication campaign of 1920s which has been documented to affect long-term outcomes (Bleakley, 2010; Venkataramani, 2012). In so doing, we include in the regressions a state-specific malaria exposure index interacted with state-level share of Blacks interacted with birth year fixed effects. During the 1910s, the Rockefeller Sanitary Commission (RSC) implemented a hookworm eradication campaign across southern counties. Research suggests that early-life exposure to this initiative had lasting effects on later-life outcomes (Bleakley & Lange, 2009; Henderson, 2018). The control for this campaign, we use data from Henderson (2018) and include in our regressions county-specific RSC-induced treatment rate interacted with county level share of Blacks interacted with birth year fixed effects. The estimated coefficients remain robust.

Studies also highlight the impact of the Boll Weevil infestation, which triggered negative labor demand shocks that influenced child labor, as well as the establishment of County Health Departments, which aimed to provide affordable healthcare to underserved communities. Both events had significant short- and medium-term impacts on children in southern counties (Baker,

2015; Hoehn-Velasco, 2018). To account for these factors, we construct exposure measures based on the timing of the Boll Weevil arrival and the establishment of County Health Departments (separately) and include them in our regressions. Our results remain robust to the inclusion of these measures.

One concern relates to the influence of the Great Depression. We use data from Federal Deposit Insurance Corporation (1984) and calculate county level drop in bank deposits between the years 1929–1933. We then interact this measure with the share of Blacks and with birth year fixed effects. The inclusion of these additional controls leaves minimal changes to the coefficient. In the next regressions, we add controls for exposure to Rosenwald Schools and more county controls, including share of different occupation groups, occupational income score, share of children, and share of married people. The estimated effects are almost identical to those in the main results. Finally, we use two-way clustering with clusters at county and birth-year levels to account for both serial and spatial correlations in error terms. Overall, the results remain consistent across specifications.

The main results are estimated using the ordinary least squares method. In column 1 of Appendix Table B-1, we report results of models using the estimation method outlined by Sun & Abraham (2021). This approach accounts for the heterogeneity of impacts across cohorts, and we find that the effects remain robust under this method.

In column 2 of Appendix Table B-1, we implement the Heckman two-step correction method (Heckman, 1979). In so doing, we take original populations in the 1910, 1920, 1930, and 1940 censuses and implement the same sample selection as in Section 3. We then merge this with our final sample to observe which individuals appear in the final sample from their respective population. The Heckman method addresses the concern of the systematic difference between

matched individuals and unmatched observations from the original population. The method estimates selection process by regressing the successful merging on a series of observable characteristics. It then calculates the Inverse Mills Ratio (IMR) as a variable that contains information on selection bias. In the second stage, the method implements the original regression equation and adds IMR as an additional control variable to control for potential selection bias. We find a difference-in-difference coefficient comparable to the main results.

5.6. Endogenous Merging

The analysis based on the DMF sample is constructed through several steps of data merging, with the most critical step involving the linkage of Social Security Administration DMF 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-census linked sample and simultaneously have higher probability of exposure to lynching, 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 lynching are less likely to appear in the DMF-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 column 1 of Table 4, show an extremely small point estimate, suggesting a change of 0.01% with respect to the mean.

To infer county of birth in the DMF data, we link individuals in the 1940 census to the 1910, 1920, and 1930 censuses. However, endogenous data linking between the 1940 census and earlier censuses could introduce bias to our proxy of county of birth. We empirically examine this concern by regressing an indicator of successful merging between the 1940 census sample and earlier censuses on lynching exposure measures. These results, shown in columns 2-4 of Table 4, reveal that while coefficient for merging with 1930 census is statistically significant, its magnitude is negligible compared with the mean.

In addition, when using only county of residence in 1940 as a proxy for county of birth, we observe almost identical point estimates (reported in Appendix H). Furthermore, analyses using BUNMD data, which provide exact county of birth (reported in Table 2), yield quite comparable results. Taken together, these findings mitigate concerns about endogenous data merging.

5.7. Mechanisms

The stress resulting from exposure to a lynching event during pregnancy can have lasting effects on a developing fetus through various mechanisms, which can be broadly categorized as either *culling* or *scarring* mechanisms.[26] Historical narratives of lynching provide critical context to understand these mechanisms, illustrating the pervasive fear, psychological trauma, and community-wide impacts of these violent events. Accounts from survivors and contemporaneous anti-lynching campaigns highlight how these events were designed not only to punish individuals but to instill fear across entire communities, particularly targeting Black families (NAACP, 1920).

First, in extreme cases, in utero exposure to lynching may result in fetal demise due to the severe stress and trauma experienced by the pregnant person. For instance, "In 1927, after lynching

[26] We recognize that there may be alternate explanations that may not fit neatly within the culling or scarring mechanisms that we propose here (e.g., behavioral responses to lynching events). However, isolating and empirically testing these mechanisms is unfortunately beyond the scope of our data.

a Black man named John Carter, a White mob in Little Rock, Arkansas, paraded his mutilated body through the heart of the Black community. They desecrated a prominent Black church, tearing pews out to fuel a bonfire on which they burned his corpse." (Equal Justice Initiative, 2017). Such events were widely publicized through local newspapers, often with graphic details, and became spectacles of terror meant to instill fear beyond those who directly witnessed them (Equal Justice Initiative, 2018). The extreme stress and fear triggered by lynching events could result in physiological consequences, including adverse pregnancy outcomes. For example, severe emotional distress has been linked to increased heart rate, elevated blood pressure, and heightened cortisol levels, all of which contribute to pregnancy complications such as miscarriage or stillbirth (Kinsella & Monk, 2009).

Second, exposure to lynching during pregnancy can have profound and enduring effects on the developing fetus. Research suggests that fetuses can perceive and respond to external stimuli, including the birthing person's emotional state and stress (Oliverio, 1983). The cumulative impact of this trauma can have a lasting impact on the developing fetus, which can manifest in various aspects of life, such as educational attainment, wages, and health, including susceptibility to diseases later in adulthood (Almond and Currie, 2011). The narratives surrounding the anti-lynching movement also reveal the cultural and psychological scarring inflicted by these events. Prominent campaigns led by organizations such as the NAACP sought to expose the brutality of lynching through publications, exhibits, and art, which often highlighted the generational effects on Black families (Giddings, 2008; NAACP, 1920). For example, the harrowing photographs collected in the NAACP's anti-lynching campaigns vividly depict the fear and trauma lynching imposed on local communities (NAACP, 1920).

Unfortunately, the scarcity of available data has hindered researchers' ability to directly test and differentiate the culling and scarring mechanisms—there are no documented detailed surveys focused on the health of both birthing parents and infants during the early 1900s. To explore these dynamics, we turned to three individual-level measures from the 1940 census: schooling, socioeconomic score, and occupational income score. Several studies have suggested that maternal stress and mental pressure influence developmental outcomes, education, and later-life socioeconomic measures (Caruso & Miller, 2015; Noghanibehambari, 2022; Vaiserman, 2015). Because socioeconomic outcomes contribute to health and longevity, they could operate as potential mediatory channels (Chetty et al., 2016; Salm, 2011). Thus, we use schooling, socioeconomic score, and occupational income score as the outcomes in Equation (1); results are reported in Table 6. In utero exposure among Black males is associated with a 0.15-year reduction in schooling (Column 1), a 1.41-unit decline in the socioeconomic score (Column 2), and a 0.47-unit decrease in the occupational income score (Column 3). These represent declines of approximately 1.8%, 6.2%, and 2.3%, respectively, relative to the mean values of these outcomes. These findings, combined with the results in Figure 3, which show no significant effects of lynching exposure on infant mortality, birth rates, or the racial composition of births, suggest that the scarring effect may be the primary driver of our main results.

6. Conclusion

Several studies have examined the negative short-term impacts of racism and racial violence on various social and health outcomes (Jones et al., 2017; Kramer et al., 2017; Mujahid et al., 2021; Probst et al., 2019). The current study extends the scope of this literature by examining the long-term effects of racial violence. Specifically, we examined how maternal exposure to racial violence during pregnancy affects the child's longevity during adulthood and old age.

Using administrative death records linked to historical lynching data, we implement difference-in-differences models to estimate the effect of in utero exposure. Our findings indicate that Black male births whose gestation overlapped with a lynching event experienced a 3.5-month reduction in later-life longevity. Further analyses suggested that this effect is not driven by changes in the socioeconomic composition of the sample and that exposure is not correlated with other observable determinants of longevity. Placebo tests found small and statistically insignificant impacts of exposure to lynchings with Black victims on longevity among White males.

Based on the life expectancy gap between Black and White males in the CenSoc-DMF data for those whose death year occurred between 1975 and 1985, the Black-White life expectancy gap conditional on survival to age 35 during this period is approximately 38 months. Therefore, in utero exposure to a lynching accounts for roughly 10% of the life expectancy gap between Black and White males during these years.[27]

In sum, this research reveals the enduring and profound impacts of historical racial violence on the later-life longevity of Black males. The results have significant implications given the persistence of racial violence in the United States and the extent of new sources of exposure to such violent events, namely video recordings and social media. The identified reduction in longevity among Black male infants exposed to lynching suggests that high-profile contemporary incidents of violence, such as police use of excessive force against Black individuals, may also have lasting consequences.

[27] Given that the Black-White life expectancy gap has changed over time, we also compared the 3.7-month effect to the overall change in the gap from 1975 to 2005. Specifically, in 1975, the life expectancy gap for Black and White males, conditional on survival to age 35, was 40 months. By 2005, this gap had decreased to 25 months, representing a change of 15 months. As a result, the 3.7-month effect corresponds to approximately 25% of the total change in life expectancy among Black and White males between 1975 and 2005.

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Tables

Table 1 – Balancing Test: In Utero Exposure to Lynching 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 education college	Father's education missing	Father's occupational income score missing	Father's occupational income score below median	Father's occupational income score above median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
In utero exposure to lynching × Black	-.0051*	.0106*	.0143*	.0033	-.0084	.0004	.0103	.0046	-.0009	-.0045	-.0025	-.0086	.0111*
	(.0031)	(.0059)	(.0074)	(.0026)	(.0071)	(.0035)	(.0072)	(.0084)	(.0025)	(.0074)	(.0073)	(.0070)	(.0057)
Observations	56,177	56,177	56,177	56,177	56,177	56,177	56,177	56,177	56,177	56,177	56,177	56,177	56,177
R-squared	.074	.144	.272	.073	.455	.079	.176	.291	.070	.438	.445	.253	.358
Mean DV	0.020	0.902	0.713	0.024	0.584	0.024	0.892	0.743	0.024	0.650	0.641	0.180	0.179

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 lynching, while controlling for lynching incident fixed effects, following the specification outlined in Equation (1). This table uses our preferred sample, which includes only lynching events with no subsequent lynching in the same county within the following 18 months. Standard errors, clustered on county, are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

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

	Outcome: Age at death (months)		
	Males	Females	Full Sample
	(1)	(2)	(3)
In utero exposure to lynching × Black	-3.52***	-.35	-2.15***
	(1.25)	(.98)	(.75)
Observations	130,024	150,552	280,747
R-squared	.34	.48	.43
Lynching event fixed effects	✓	✓	✓
County-level controls	✓	✓	✓

Notes. This table presents the main results using the Berkeley Unified Numident Mortality Database (BUNMD), which includes exact county-of-birth information and data on both males and females. Columns 1 and 2 present estimates for males and females separately, while Column 3 provides estimates for the combined sample of males and females. Standard errors, clustered on county, are in parentheses. County covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, and literacy rate.

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

Table 3 – Replicating the Main Results Using DMF Data that Linking with 1940 Census

	Outcome: Age at death (months)		
	(1)	(2)	(3)
In utero exposure to lynching × Black	-3.72**	-3.73**	-3.61**
	(1.79)	(1.79)	(1.80)
Observations	56,177	56,177	56,177
R-squared	.32	.32	.32
Lynching event fixed effects	✓	✓	✓
Parental controls		✓	✓
County-level controls			✓

Notes. This table reports estimates from Equation (1) using the DMF data. Standard errors, clustered on county, are in parentheses. Parental controls include dummies for maternal education, paternal education, and paternal socioeconomic status. County covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, and literacy rate.

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

Table 4 – Using Black Births in Non-Lynching Counties as Alternative Control Groups

	Outcome: Age-at-Death (Months)		
	(1)	(2)	(3)
Panel A. Using Black births in different counties as an alternative control group			
In utero exposure to lynching	-4.04**	-4.03**	-4.07**
	(1.75)	(1.75)	(1.75)
Observations	99,488	99,488	99,488
R-squared	.50	.50	.50
Panel B. Placebo test using White births			
In utero exposure to lynching	.19	.20	.24
	(.98)	(.98)	(.98)
Observations	803,051	803,051	803,051
R-squared	.45	.45	.45
Lynching event FE	✓	✓	✓
County FE	✓	✓	✓
State-specific-birth-year-month FE	✓	✓	✓
Parental controls		✓	✓
County-level controls			✓

Notes. Panel A uses Black births in different counties as the control group, instead of White births in the same county as in the main specification. This analysis restricts the sample to Black male births and includes state-by-year-by-month fixed effects, lynching event-specific fixed effects, and county fixed effects.

Panel B presents a placebo test, applying the same model specification from Panel A to White male births.

Standard errors, clustered on county, are in parentheses. Parental controls include dummies for maternal education, paternal education, and paternal socioeconomic status. County covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, and literacy rate. Fixed effect matrix includes lynching event fixed effects, county fixed effects, and state by year by month fixed effects.

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

Table 5 – Exposure to Lynching and Endogenous Data Linking

	Outcome: Successful merging between 1940 census and			
	DMF Data	1910 Census	1920 Census	1930 Census
	(1)	(2)	(3)	(4)
In utero exposure to lynching × Black	-.00001	-.00049	.00299*	.00140
	(.00007)	(.00125)	(.00178)	(.00181)
Observations	575,596	575,596	575,596	575,596
R-squared	.9985	.3977	.43847	.47884
Mean DV	0.103	0.019	0.036	0.051
Lynching event FE	✓	✓	✓	✓
Parental Controls	✓	✓	✓	✓
County-level controls	✓	✓	✓	✓

Notes. This analysis tests whether individuals with higher exposure to lynching are less likely to appear in the DMF-census data. In Column 1, the dependent variable is a dummy indicating successful merging between the 1940 census and DMF data. In Columns 2, 3, and 4, the dependent variable is a dummy indicating successful merging between the 1940 census and the 1910, 1920, and 1930 censuses, respectively.

Standard errors, clustered on county, are in parentheses. Regressions include lynching incident fixed effects and controls. Parental controls include dummies for maternal education, paternal education, and paternal socioeconomic status. County covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, and literacy rate.

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

Table 6 – Mechanism Channels: In Utero Exposure to Lynching and Later-Life Education and Socioeconomics Status

	Outcomes:		
	Years of Schooling	Socioeconomic score	Occupational income score
	(1)	(2)	(3)
In utero exposure to lynching × Black	-.15**	-1.41***	-.47**
	(.07)	(.42)	(.23)
Observations	44,500	41,621	42,193
R-squared	.32	.22	.27
Mean DV	8.2	22.8	20.4
Lynching event FE	✓	✓	✓
Parental Controls	✓	✓	✓
County-level controls	✓	✓	✓

Notes. The dependent variables are three individual-level measures from the 1940 census: years of schooling, socioeconomic score, and occupational income score as outcomes to estimate Equation (1).

Standard errors, clustered on county, are in parentheses. Parental controls include dummies for maternal education, paternal education, and paternal socioeconomic status. County covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, and literacy rate. The sample is restricted to cohorts born 1900-1921.

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

Figures

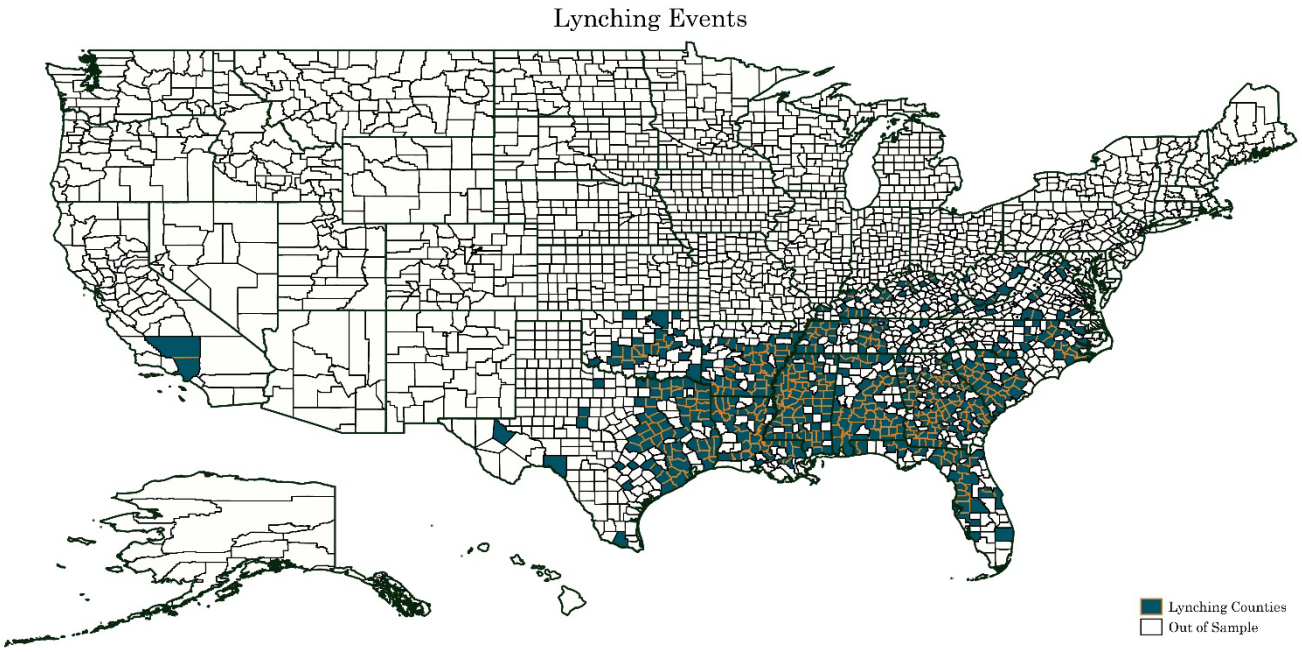


Figure 1 - Map of Lynching Events by County in the Final Sample

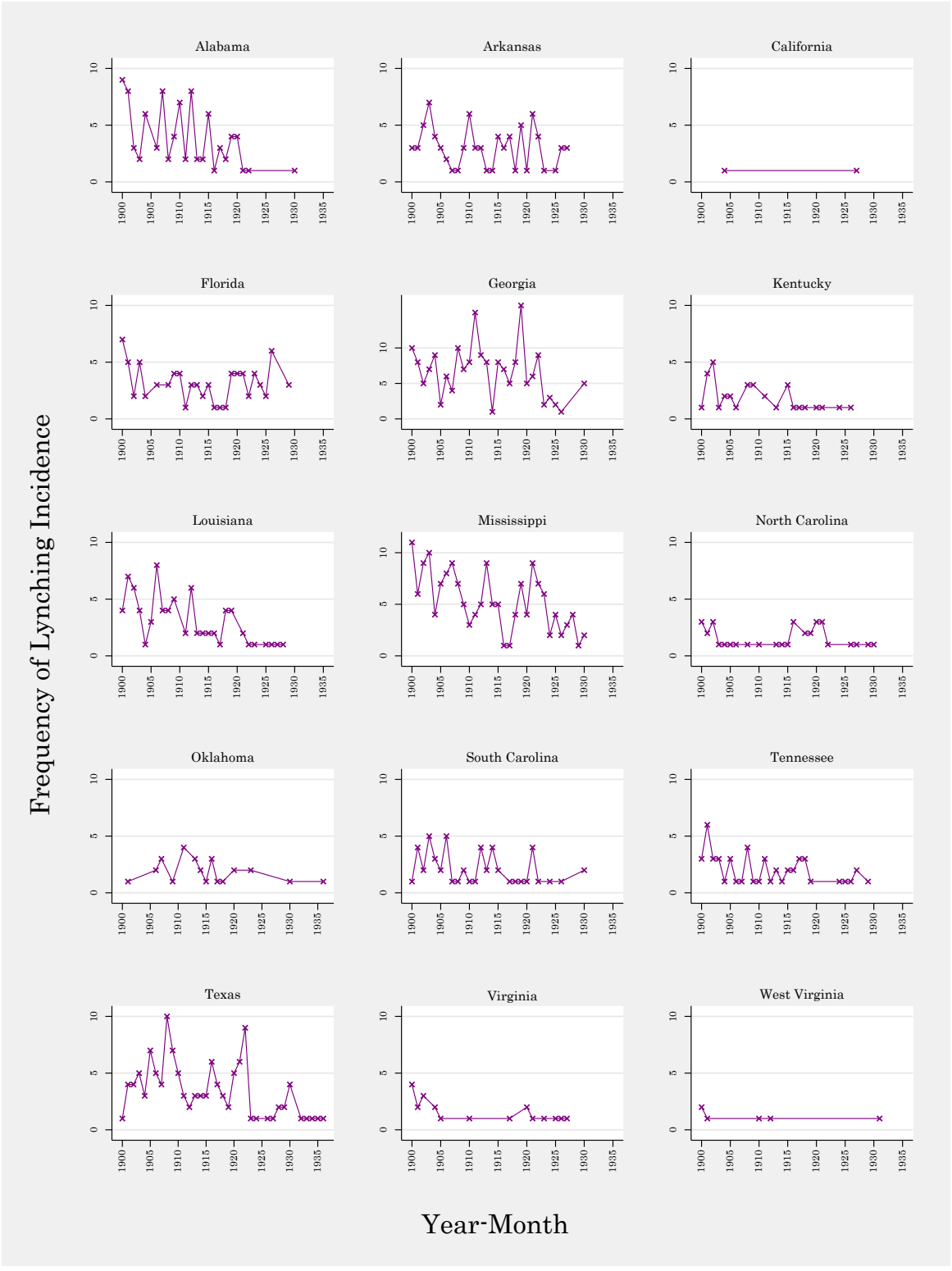


Figure 2 - Frequency of Lynching Incidence across States in the Final Sample

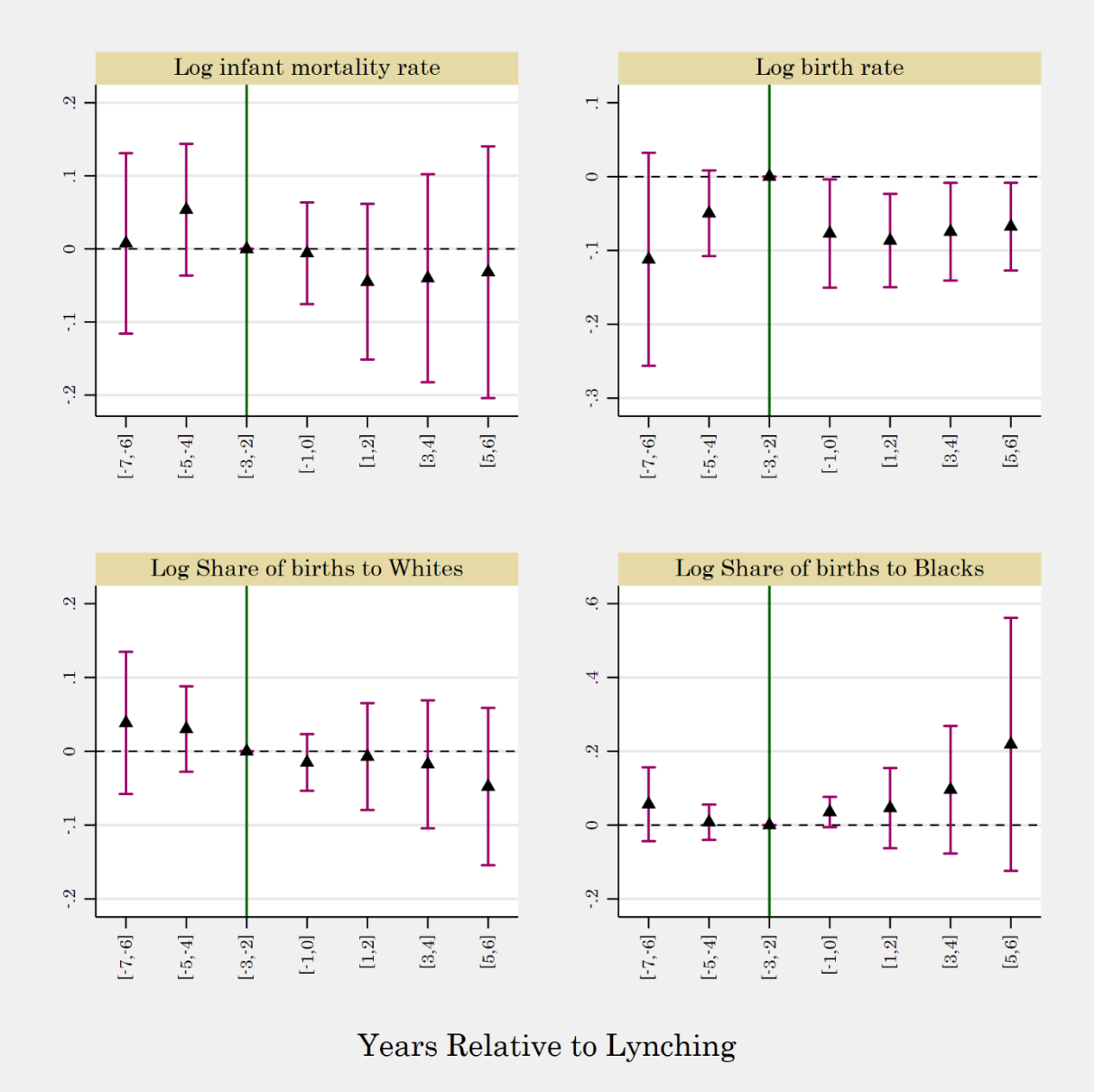


Figure 3 – Examining the Effects on Fertility and Infant Mortality

Notes. This figure presents event-study estimates of the effects of lynching on county-level infant mortality and fertility outcomes using data from Bailey et al. (2016). The figure reports point estimates with 95% confidence intervals, with standard errors clustered at the county level. The outcome variable is age at death (in months). Regressions include birth year fixed effects and lynching incident fixed effects interacted with county-level share of Blacks. County covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, and literacy rate.

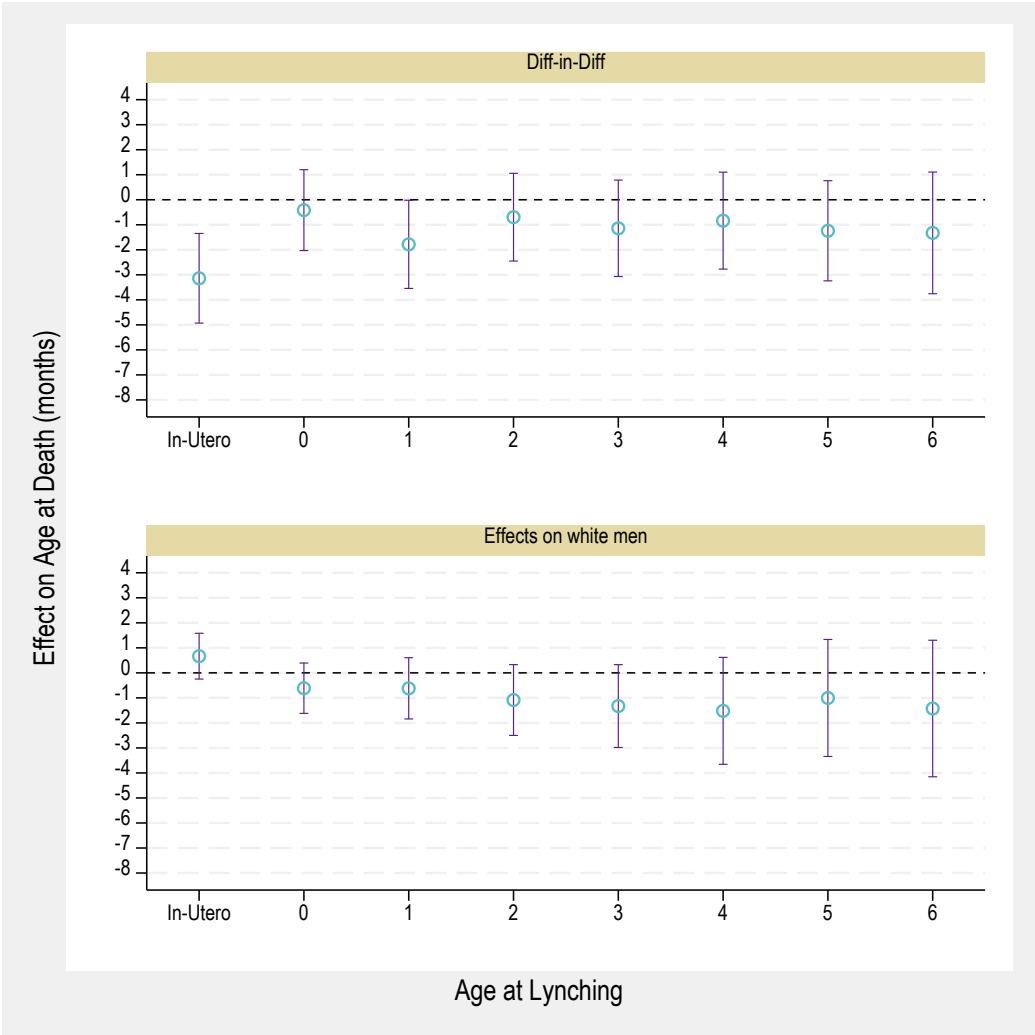


Figure 4 - Effects of Lynching by Age at Exposure

Notes. The comparison group for the estimation in this figure consists of male births for whom the lynching incident occurred before the pregnancy (i.e., born 10–18 months after a lynching incident). Point estimates and 95% confidence intervals are depicted. Standard errors are clustered on county. Regressions include lynching incident fixed effects and birth year by birth month fixed effects, and controls. Parental controls include dummies for maternal education, paternal education, and paternal socioeconomic status. County controls include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, and literacy rate. Data are from the Berkeley Unified Numident Mortality Database (BUNMD)

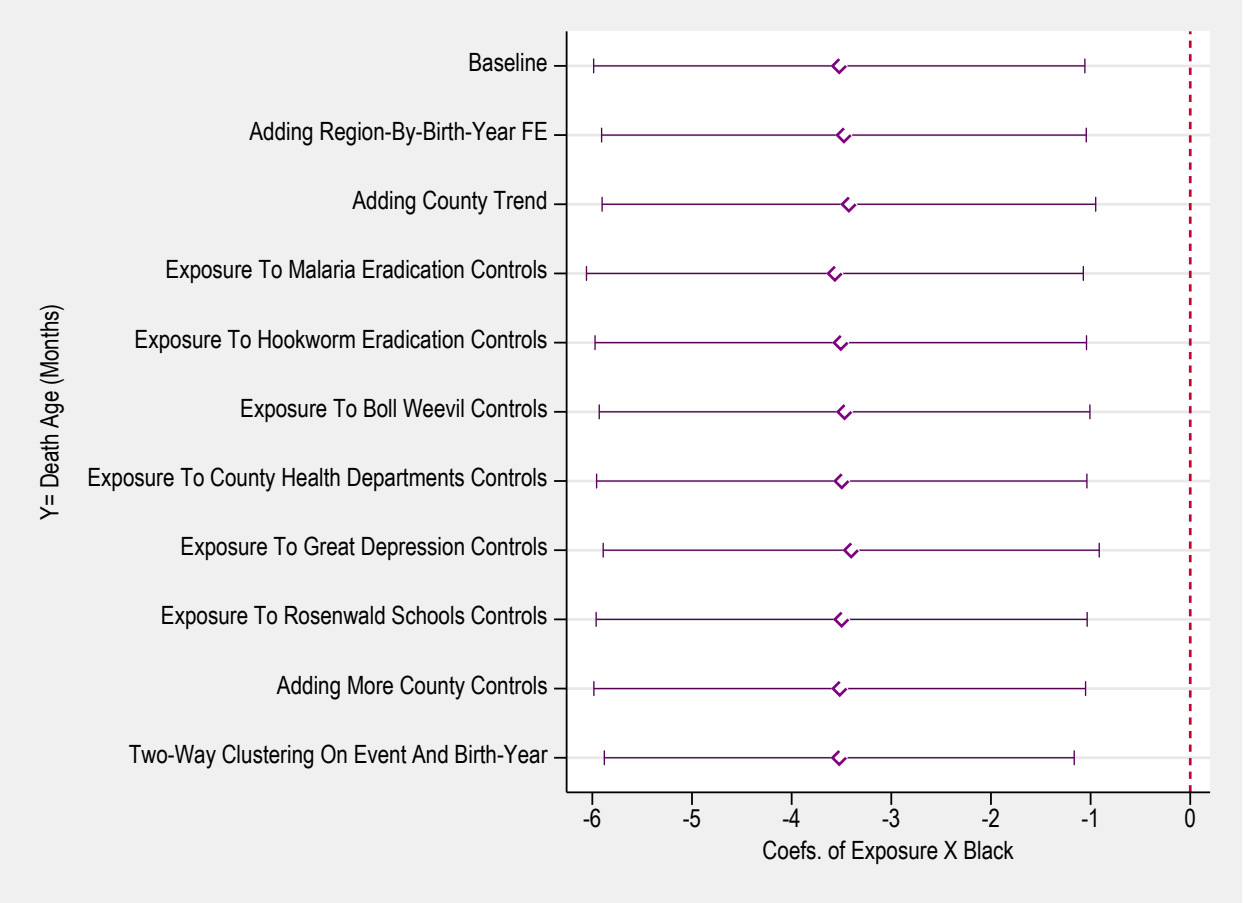


Figure 5 - Robustness Checks across Alternative Specifications

Notes. The figure presents point estimates and 95% confidence intervals for the male-only sample. The first estimate reproduces the model in column 1 of Table 2 as a benchmark. The sample includes only males. Standard errors are clustered on county. The outcome is age at death (in months). The outcome variable is age at death, measured in months. All regressions include lynching-incident fixed effects and a set of county-level controls, including the population shares by age group, occupation, gender, race (Black), immigrant status, and the literacy rate.

Online Appendix

Prenatal Exposure to Racial Violence and Later-Life Mortality among Males: Evidence from Lynching

Hoa Vu Hamid Noghanibehambari Jason Fletcher Tiffany Green

Appendix A

Appendix Table A-1 - Summary Statistics

	Mean	SD	Min	Max
Age at death (months)	902.993	114.732	475	1250
Log age at death	6.797	.132	6.163	7.131
Age at death > 70 years	.685	.464	0	1
Age at death > 75 years	.483	.5	0	1
Age at death > 80 years	.286	.452	0	1
Year of birth	1914.49	8.256	1900	1937
Year of death	1989.755	8.491	1975	2005
Black × In utero exposure to lynching	.145	.352	0	1
In utero exposure to lynching	.524	.499	0	1
Non-Hispanic Black	.279	.448	0	1
Non-Hispanic White	.703	.457	0	1
Years of schooling	7.57	3.78	0	20
Socioeconomic index	22.442	19.256	3	96
Occupational income score	19.905	10.126	3	80
Mother's education zero	.021	.142	0	1
Mother's education 5-more	.902	.297	0	1
Mother's education 9-more	.713	.452	0	1
Mother's education college	.024	.154	0	1
Mother's education missing	.584	.493	0	1
Father's education zero	.024	.154	0	1
Father's education 5-more	.892	.31	0	1
Father's education 9-more	.743	.437	0	1
Father's education college	.024	.152	0	1
Father's education missing	.65	.477	0	1
Father’s occupational score missing	.641	.48	0	1
Father’s occupational score below median	.237	.425	0	1
Father’s occupational score above median	.122	.327	0	1
Observations		56,177		

Notes. This table reports summary statistics for the main analysis sample. Data are from the Berkeley Unified Numident Mortality Database (BUNMD)

Appendix B

Appendix Table B-1 - Replication of the Main Results Using Sun and Abraham (2021) Estimate and Heckman (1979) Estimate

	Outcome: Age at death (months)	
	Sun and Abraham (2021)	**Heckman (1979)**
	(1)	(2)
In utero exposure to lynching × Black	-3.52***	-3.64*
	(1.25)	(2.06)
Observations	130,024	571,223
R-squared	.34	----
Lynching event fixed effects	✓	✓
Parental controls		✓
County-level controls	✓	✓

Notes. This table replicates the main results using two alternative estimation approaches. Column 1 reports estimates using the Sun & Abraham (2021) estimator, which accounts for heterogeneity of treatment effects across cohorts and guards against bias arising from bad comparisons in staggered difference-in-differences designs. Column 2 implements the Heckman (1979) two-step selection correction, which models the probability of appearing in the final mortality sample as a function of observable characteristics from the 1910–1940 censuses, constructs the Inverse Mills Ratio (IMR), and includes it as an additional control in the main regression to correct for potential selection bias. Standard errors, clustered on county, are in parentheses. Parental controls include dummies for maternal education, paternal education, and paternal socioeconomic status. County covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, and literacy rate.

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

Appendix C

One concern regarding the final analytic sample is the influence of larger states such as California and Texas. As shown in Figure 1, California is geographically separated from the other states in the final sample and experienced a relatively lower lynching rate than the other states. Moreover, Texas has historically had a lower share of Black individuals than the other states in the final sample. In Appendix Table C-1, we examine the robustness of the results to excluding these states. In Column 1, we replicate Column 1 of Table 2. In Columns 2-3, we drop California and Texas from the models. We observe relatively robust estimates in the subsamples relative to the full sample.

Appendix Table C-1 – Robustness to Excluding California and Texas

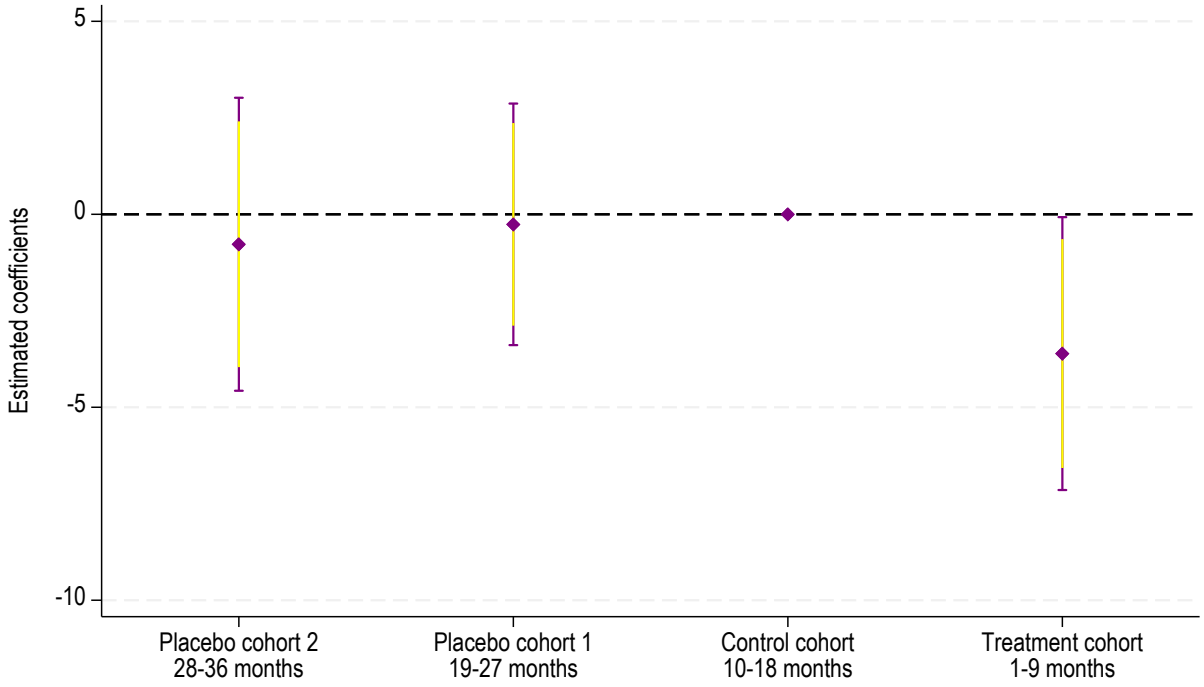
	Outcome: Age at death (months)		
	Full sample	Drop California	Drop Texas
	(1)	(2)	(3)
In utero exposure to lynching × Black	-3.5223***	-3.5502***	-4.6299***
	(1.2546)	(1.2645)	(1.3135)
Observations	130024	122060	110177
R-squared	.3416	.3322	.3341
Lynching event FE	✓	✓	✓
County-level controls	✓	✓	✓

Notes. Standard errors, clustered on county, are in parentheses. Regressions include lynching incident fixed effects and controls. Parental controls include dummies for maternal education, paternal education, and paternal socioeconomic status. County covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, and literacy rate.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix D

This appendix presents graphical results of the treatment effects for cohorts that were not affected, in the spirit of an event study. The placebo cohorts serve as a "pre-period," although in our context, they represent cohorts born after the event. Specifically, we expand the number of cohorts analyzed. In addition to the cohort born 1–9 months after the event (treatment cohort, lynching = 1) and the cohort born 10–18 months after the event (control cohort), we include cohorts born 19–27 months post-event (placebo cohort 1) and 28–36 months post-event (placebo cohort 2).

We graphically present the estimated effects and confidence intervals for these cohorts, showing results sequentially from placebo cohorts (2 and 1) to the control cohort (normalized at zero) and finally to the treatment cohort. Across all specifications, the findings indicate small and statistically insignificant effects for the placebo cohorts, bolstering the credibility of our control group.

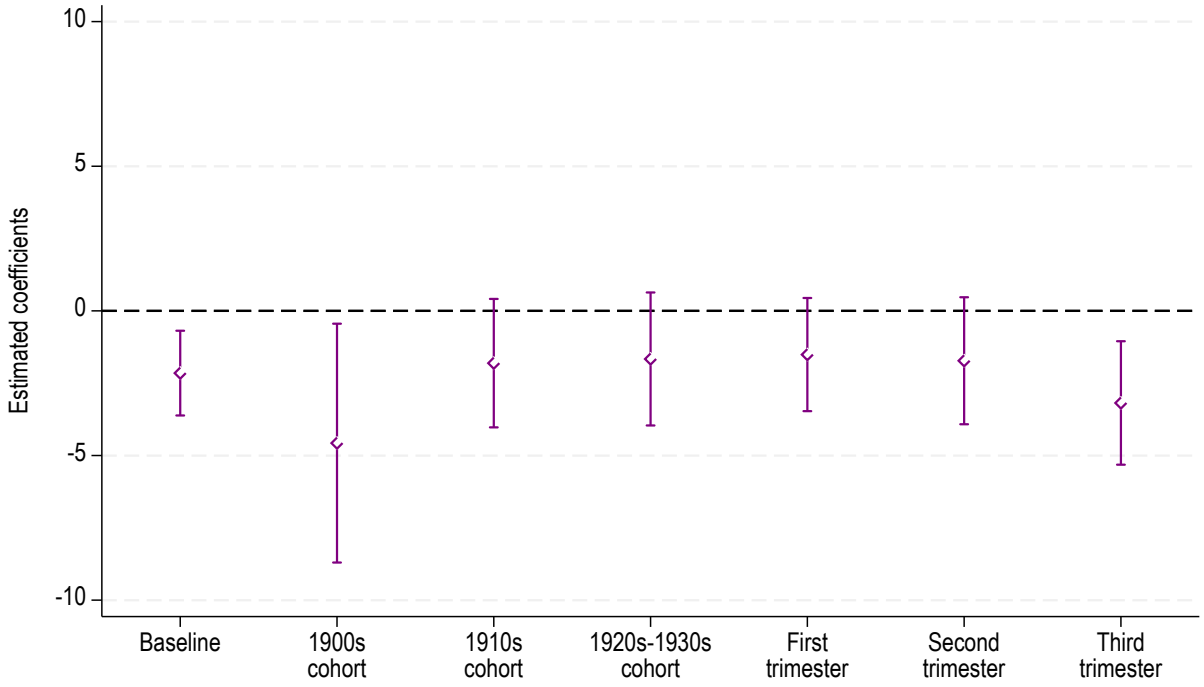


Appendix Figure D-1 - Event-Study Style Results to Examine the Effects of Lynching across Various Birth Cohorts

Notes. For each placebo cohort, we restrict the analysis to event windows without subsequent lynching incidents. For example, for placebo cohort 2 (individuals born 28–36 months after a lynching incident), we include only lynching events with no other lynchings occurring in the 36 months following the incident. The vertical axis represents the number of months born relative to a lynching incident.

Point estimates and confidence intervals are illustrated. The yellow lines indicate 90% and the purple lines indicate 95% confidence intervals. Standard errors are clustered on county. Regressions include lynching incident fixed effects and controls. Parental controls include dummies for maternal education, paternal education, and paternal socioeconomic status. County covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, and literacy rate. Data are from the Social Security Administration's Death Master File (DMF).

Appendix E



Appendix Figure E-1 – Heterogeneity by Birth Cohorts and Trimester

Notes. As shown in Figure 2, a large portion of lynching incidents occurred during the 1900s and 1910s, and we would therefore expect relatively larger impacts for cohorts born in these decades. This figure examines these heterogeneities by birth cohort and trimester of exposure. This figure also examines trimester-specific exposure using distinct indicators for each trimester. Point estimates and 95% confidence intervals are illustrated. Standard errors are clustered on county. The outcome is age at death (in months). Regressions include lynching incidence fixed effects and county-level controls. County covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, and literacy rate.

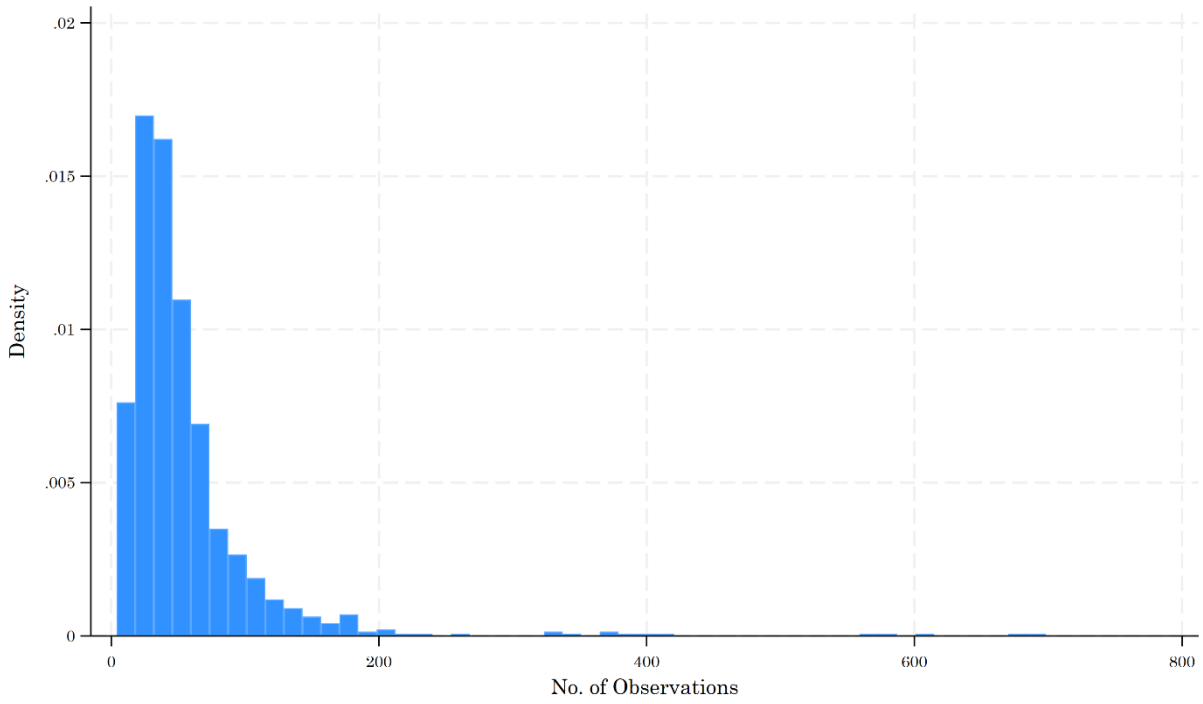
Appendix F

Appendix Table F-1 - Joint Balancing Test

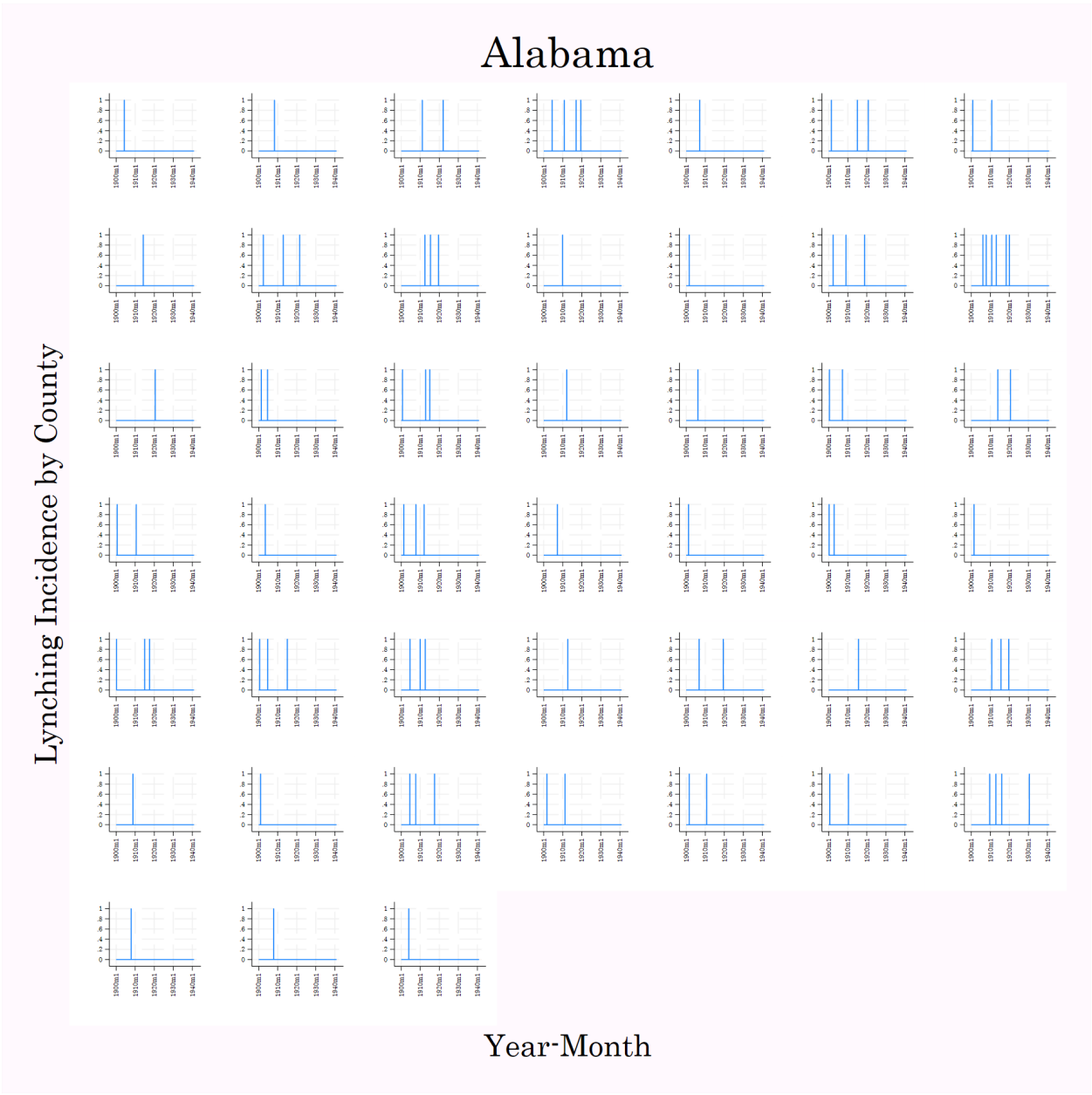
	Outcome: Lynching Exposure × Black
	(1)
Mother's education zero	-.00888
	(.0077)
Mother's education equal or more than 5 years	.00089
	(.00428)
Mother's education equal or more than 9 years	.00281
	(.00259)
Mother's education college	.00525
	(.0052)
Father's education zero	.0086
	(.00708)
Father's education equal or more than 5 years	.00634
	(.00426)
Father's education equal or more than 9 years	.00053
	(.00371)
Father's education college	-.00834
	(.00555)
Father's occupational income score below median	.00247
	(.00391)
Father's occupational income score above median	.0069*
	(.00354)
Observations	56,177
R-squared	.68
P-Value of joint significance Test	.15
F-Stat of joint significance Test	1.46

Notes. This table examines the correlation between exposure to a lynching 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 county level. Data are from the Berkeley Unified Numident Mortality Database (BUNMD).
**** p<0.01, ** p<0.05, * p<0.1*

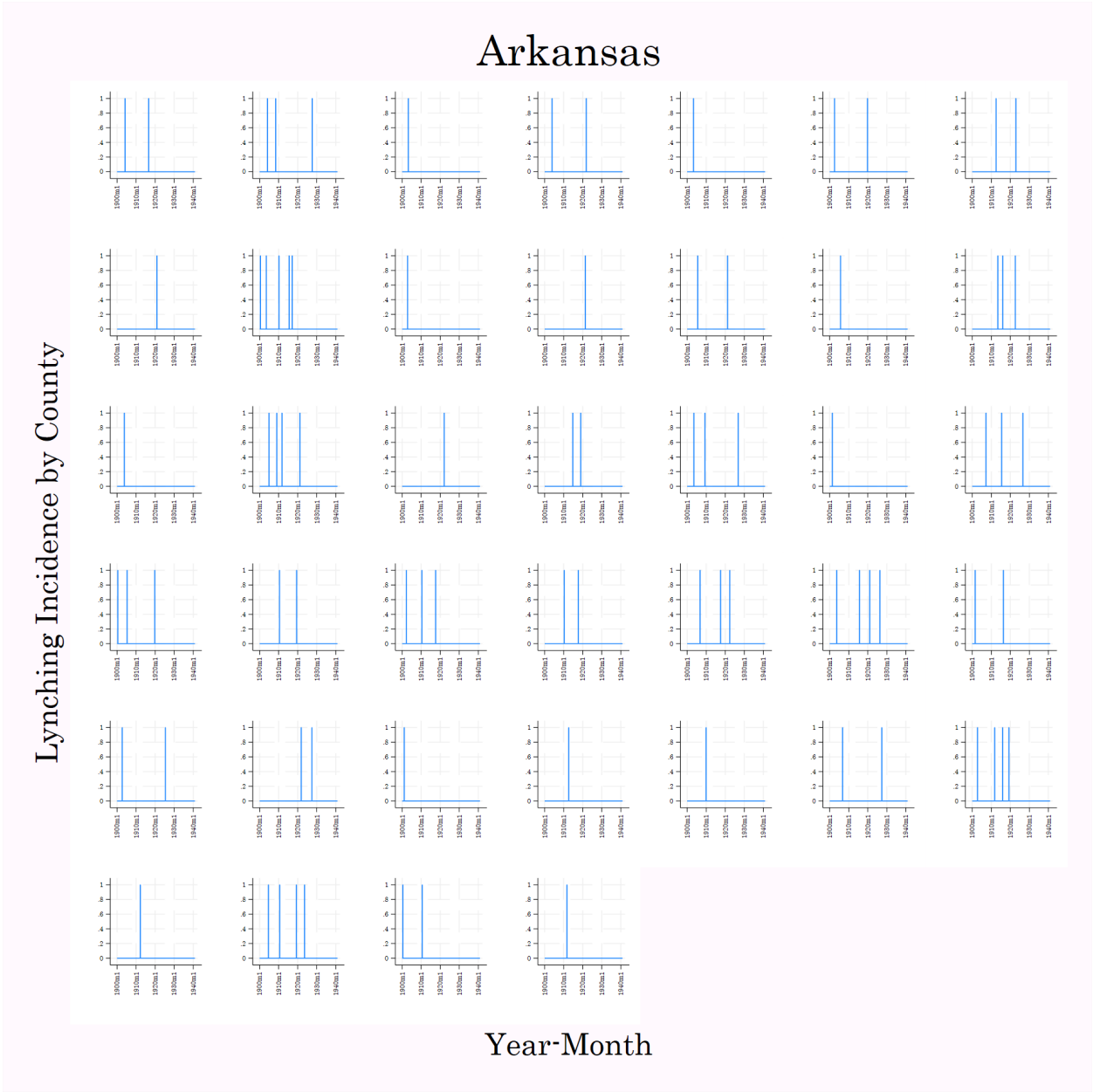
Appendix G



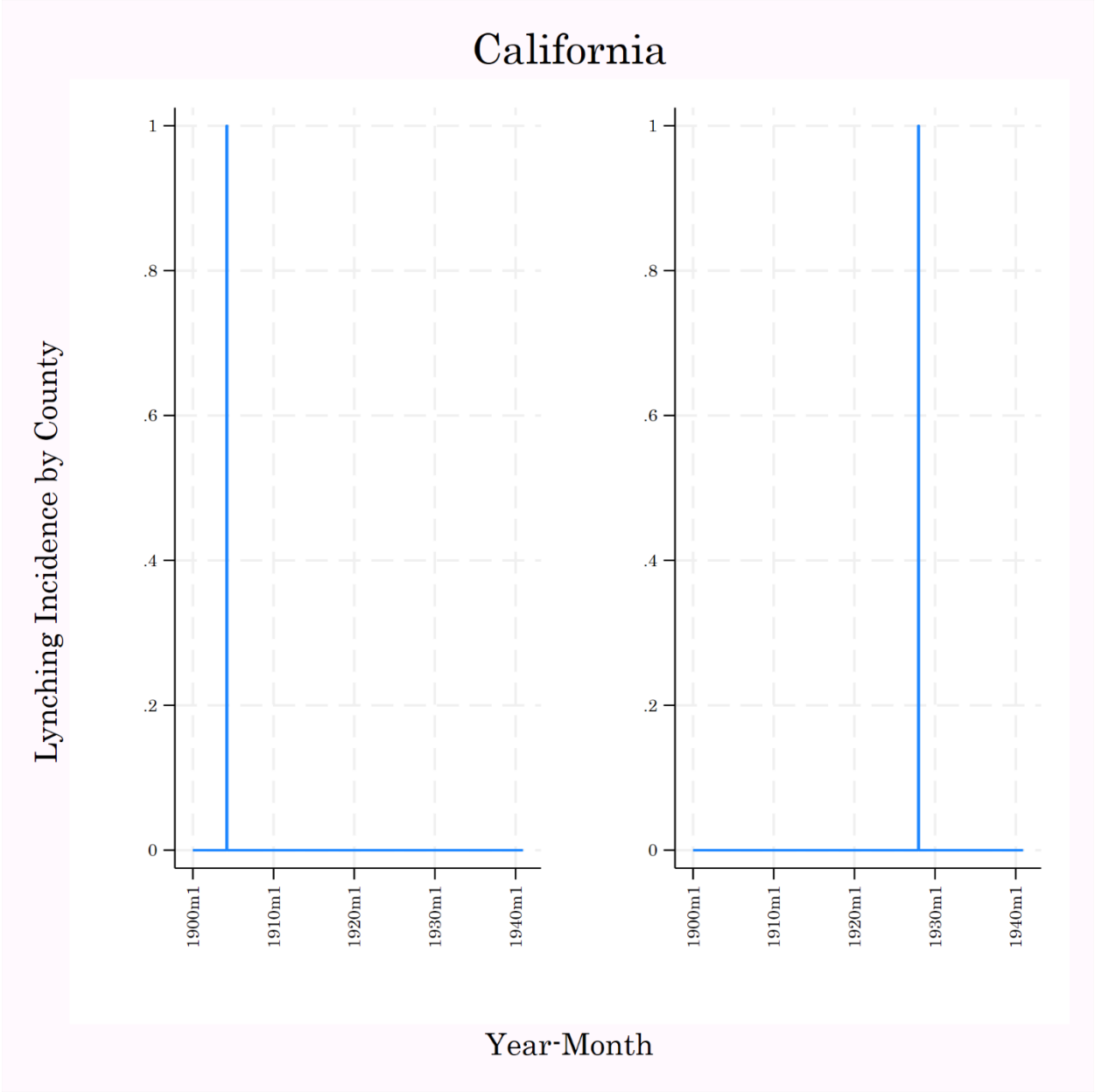
Appendix Figure G-1 - Distribution of Observations Within a Lynching Incidence Year



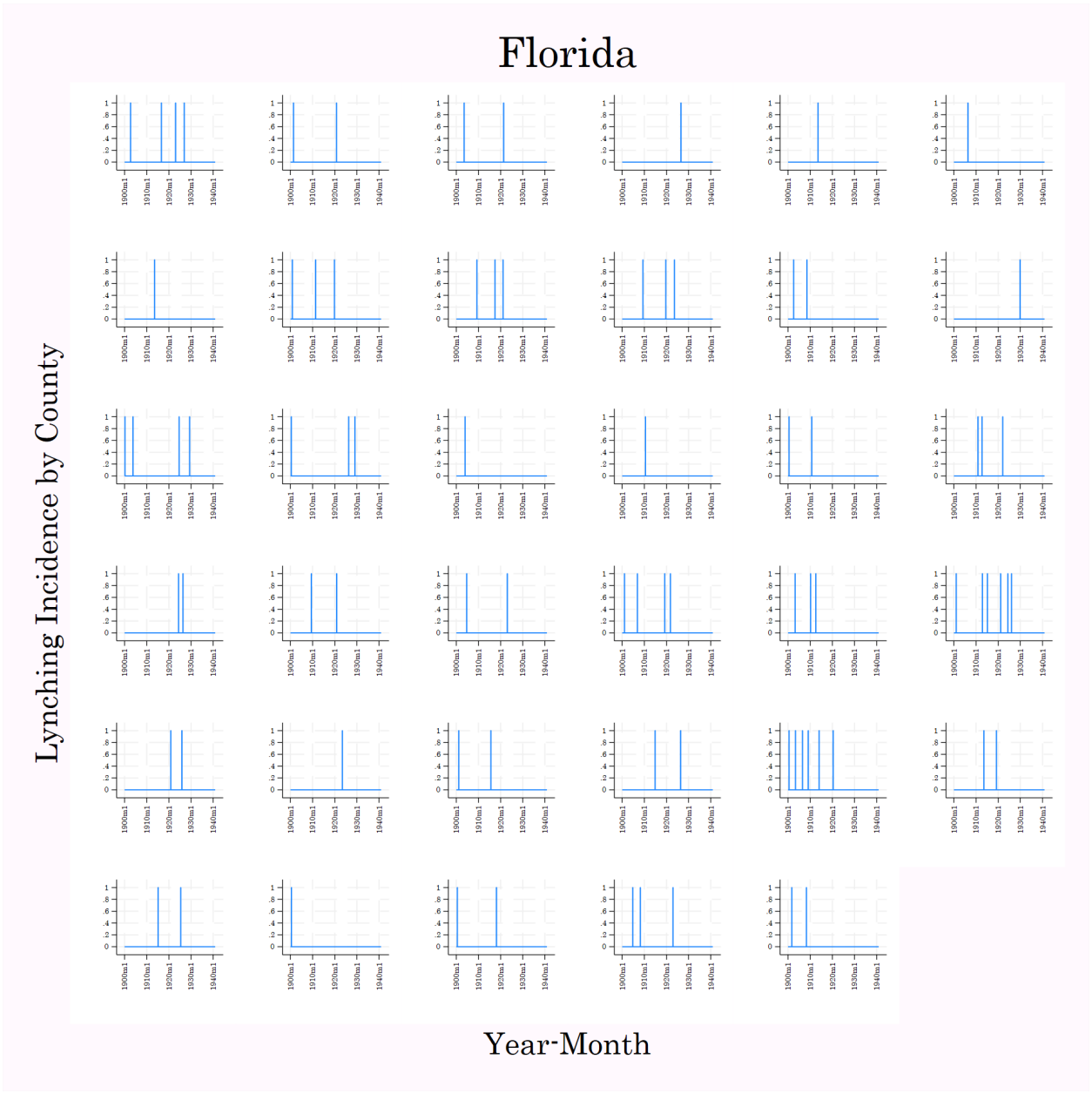
Appendix Figure G-2 - Time Series Graphs of Lynching Incidents across Counties of Alabama



Appendix Figure G-3 - Time Series Graphs of Lynching Incidents across Counties of Arkansas



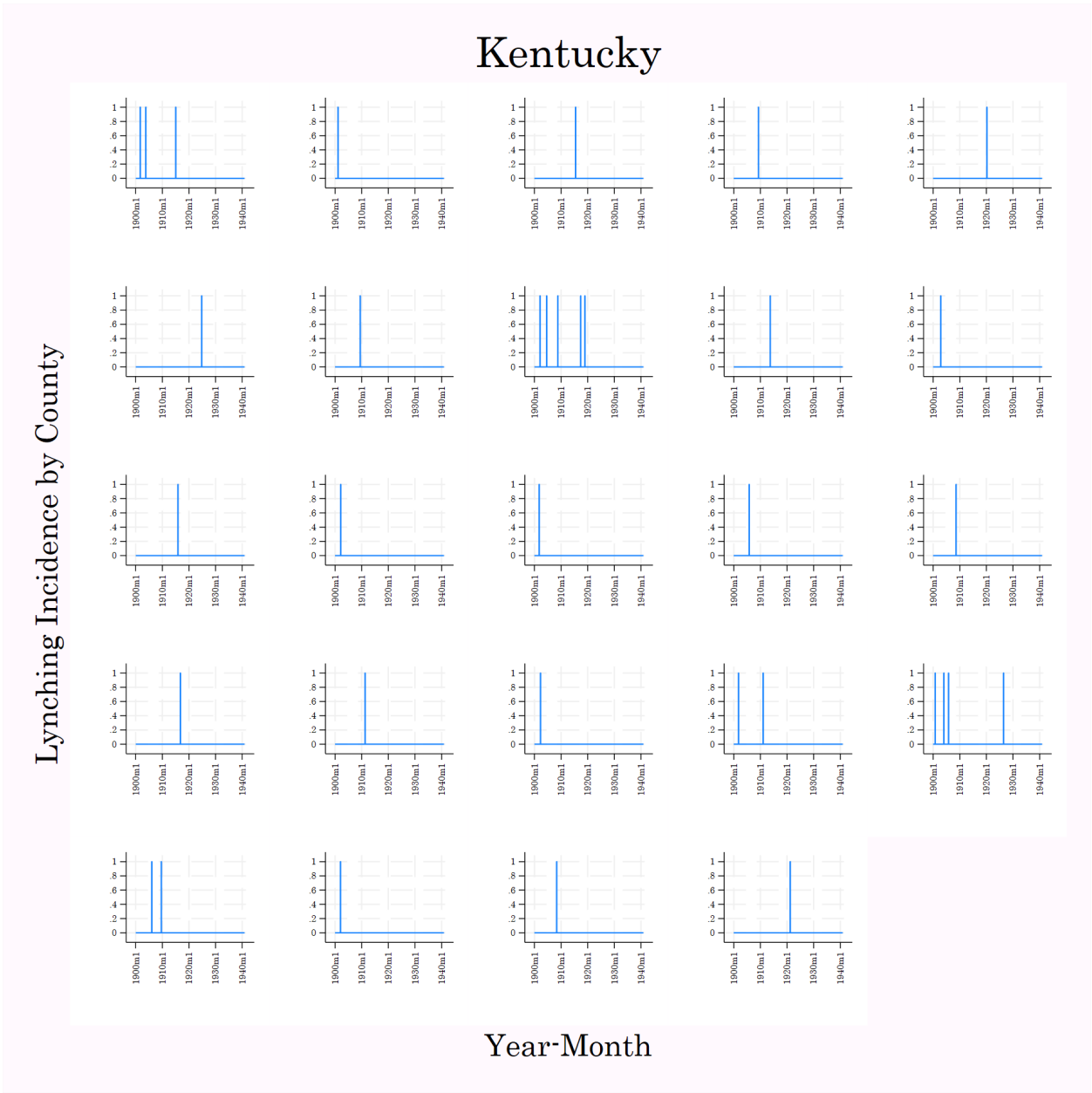
Appendix Figure G-4 - Time Series Graphs of Lynching Incidents across Counties of California



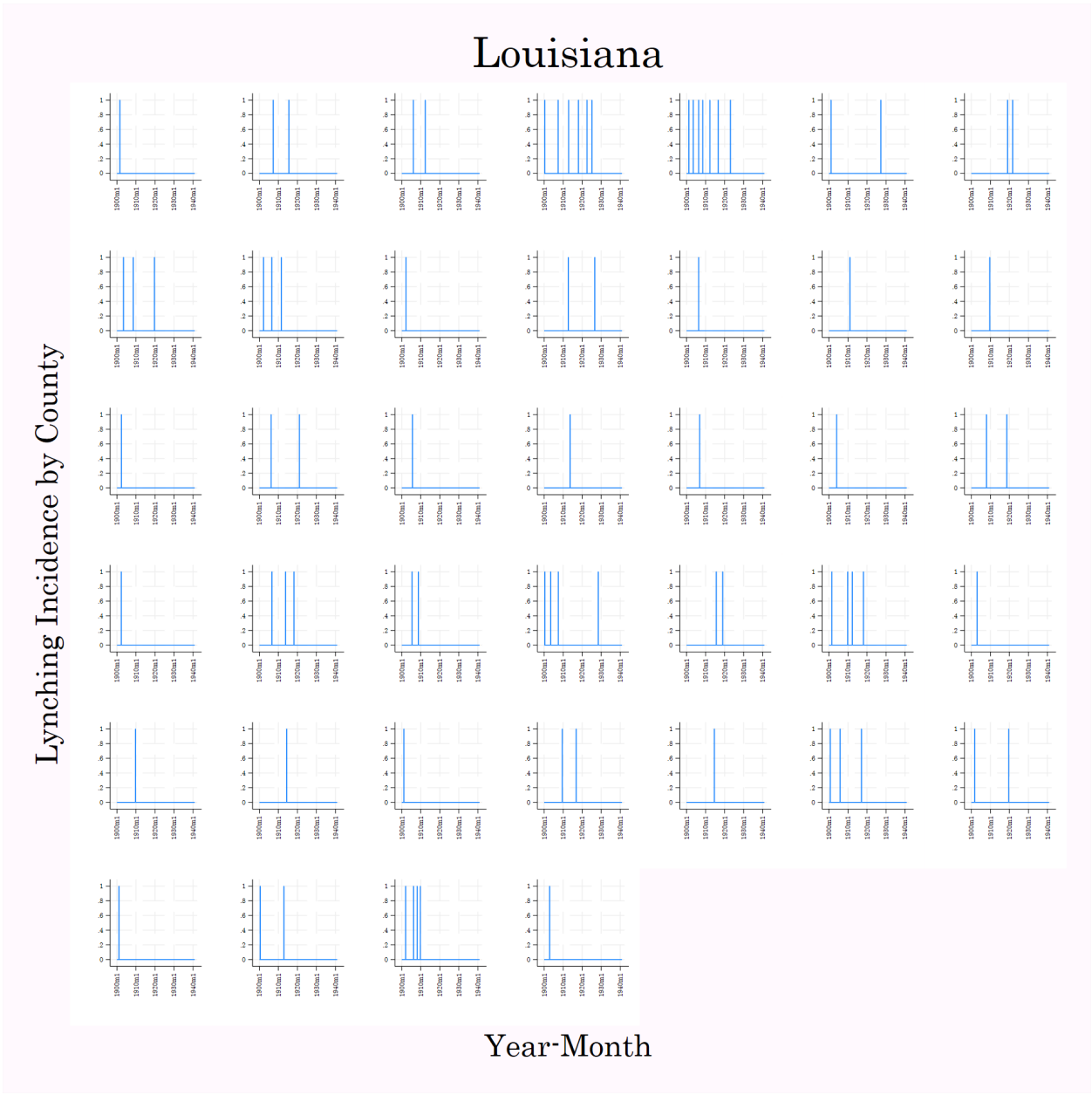
Appendix Figure G-5 - Time Series Graphs of Lynching Incidents across Counties of Florida



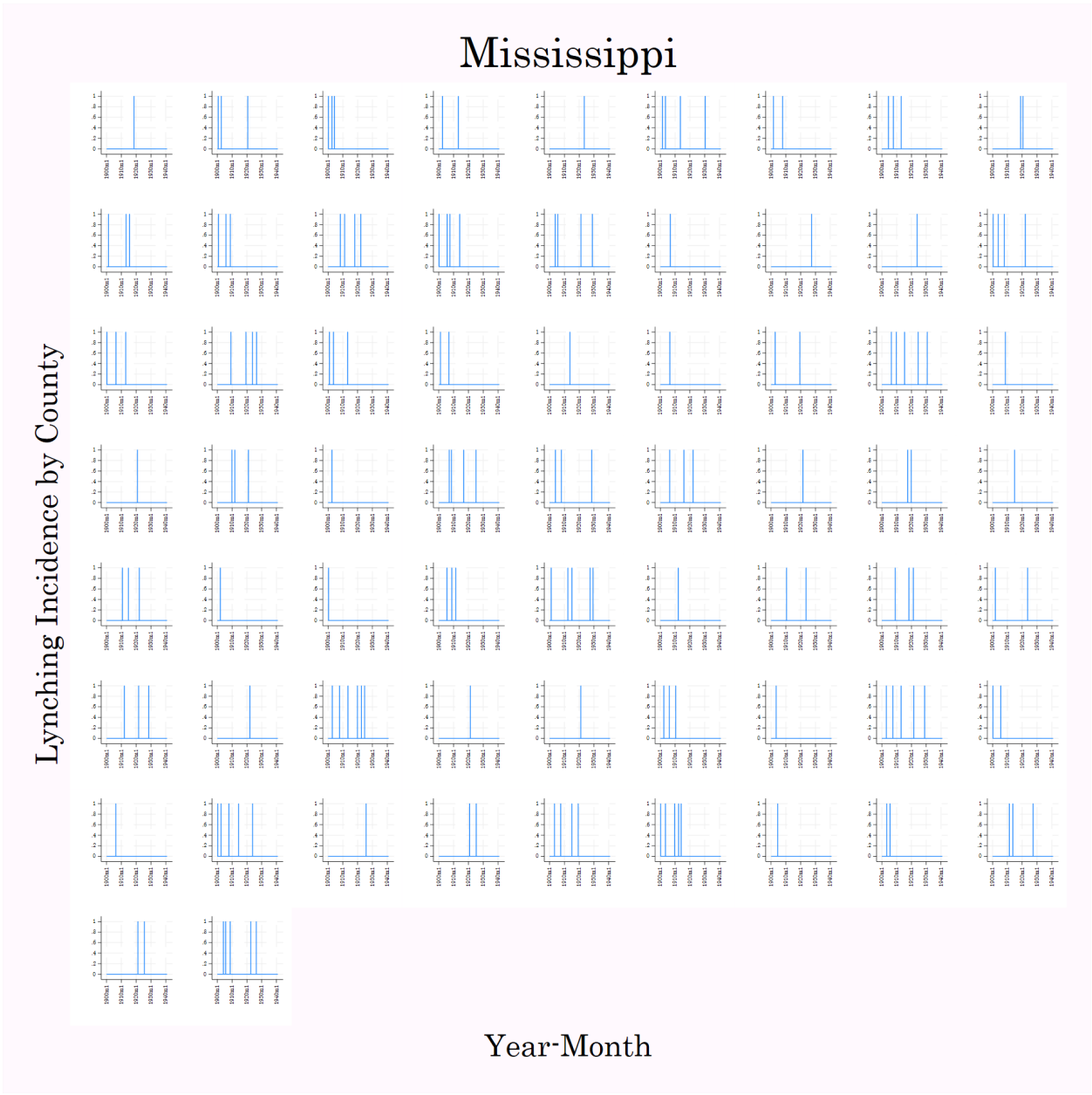
Appendix Figure G-6 - Time Series Graphs of Lynching Incidents across Counties of Georgia



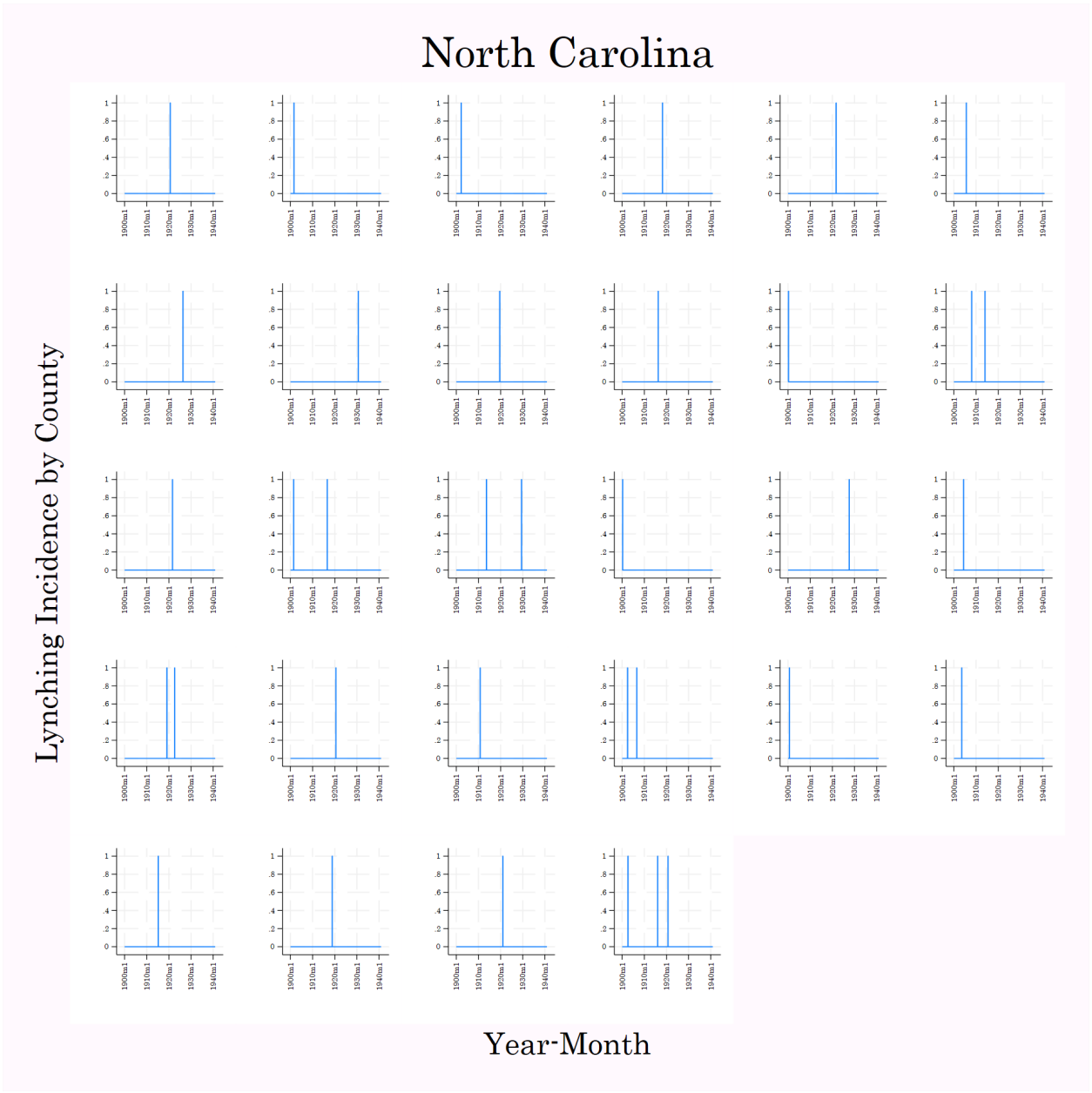
Appendix Figure G-7 - Time Series Graphs of Lynching Incidents across Counties of Kentucky



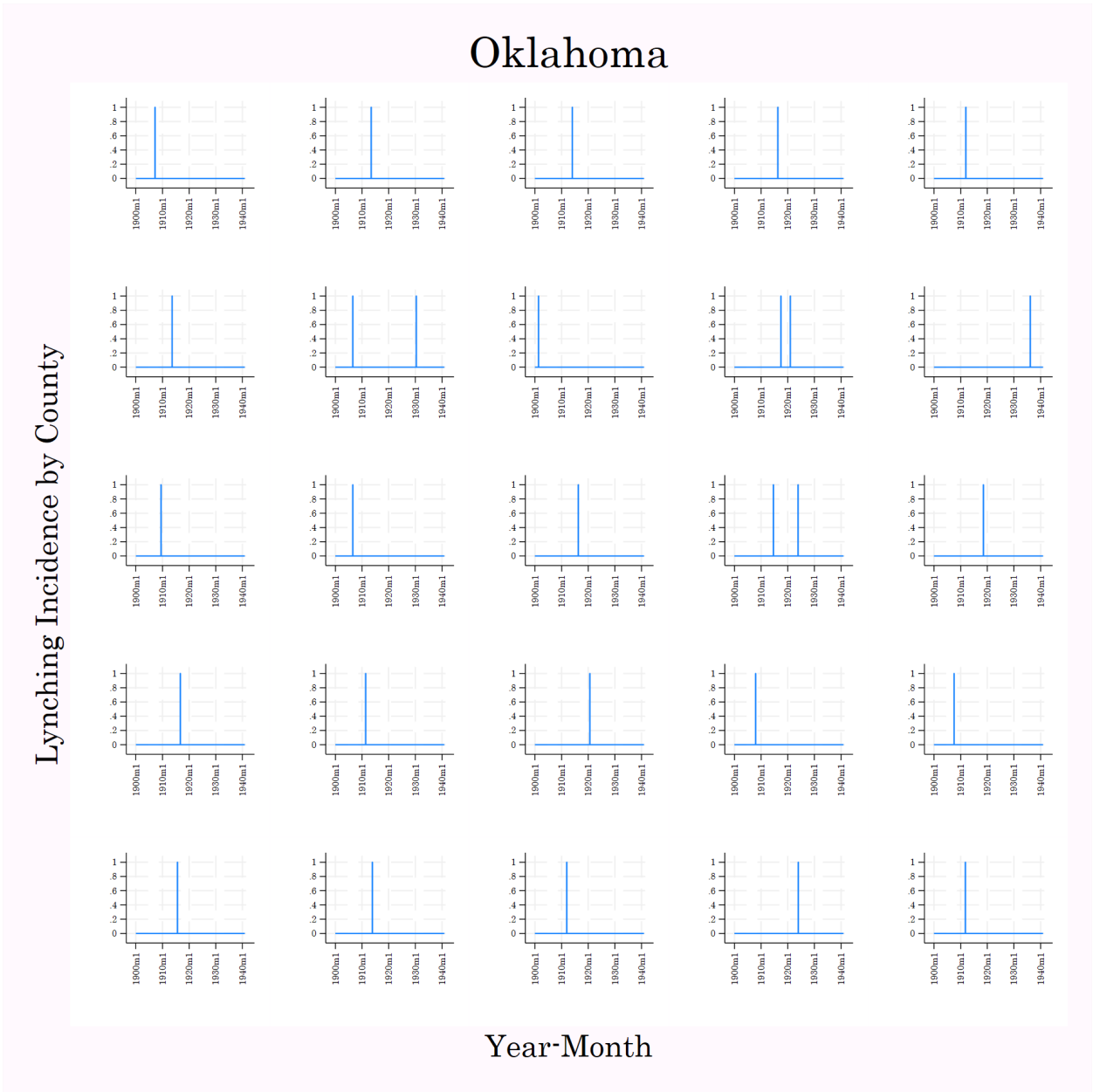
Appendix Figure G-8 - Time Series Graphs of Lynching Incidents across Counties of Louisiana



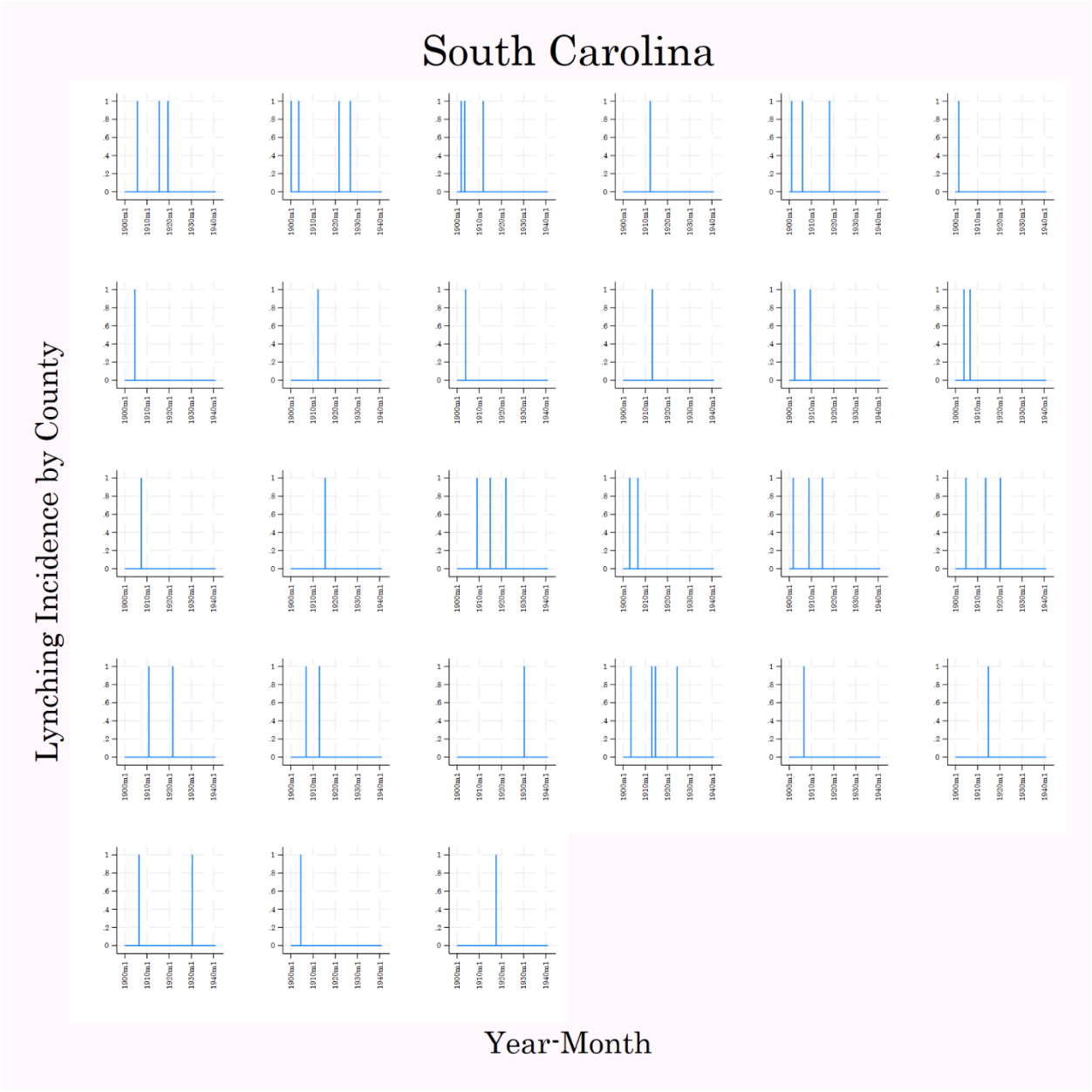
Appendix Figure G-9 - Time Series Graphs of Lynching Incidents across Counties of Mississippi



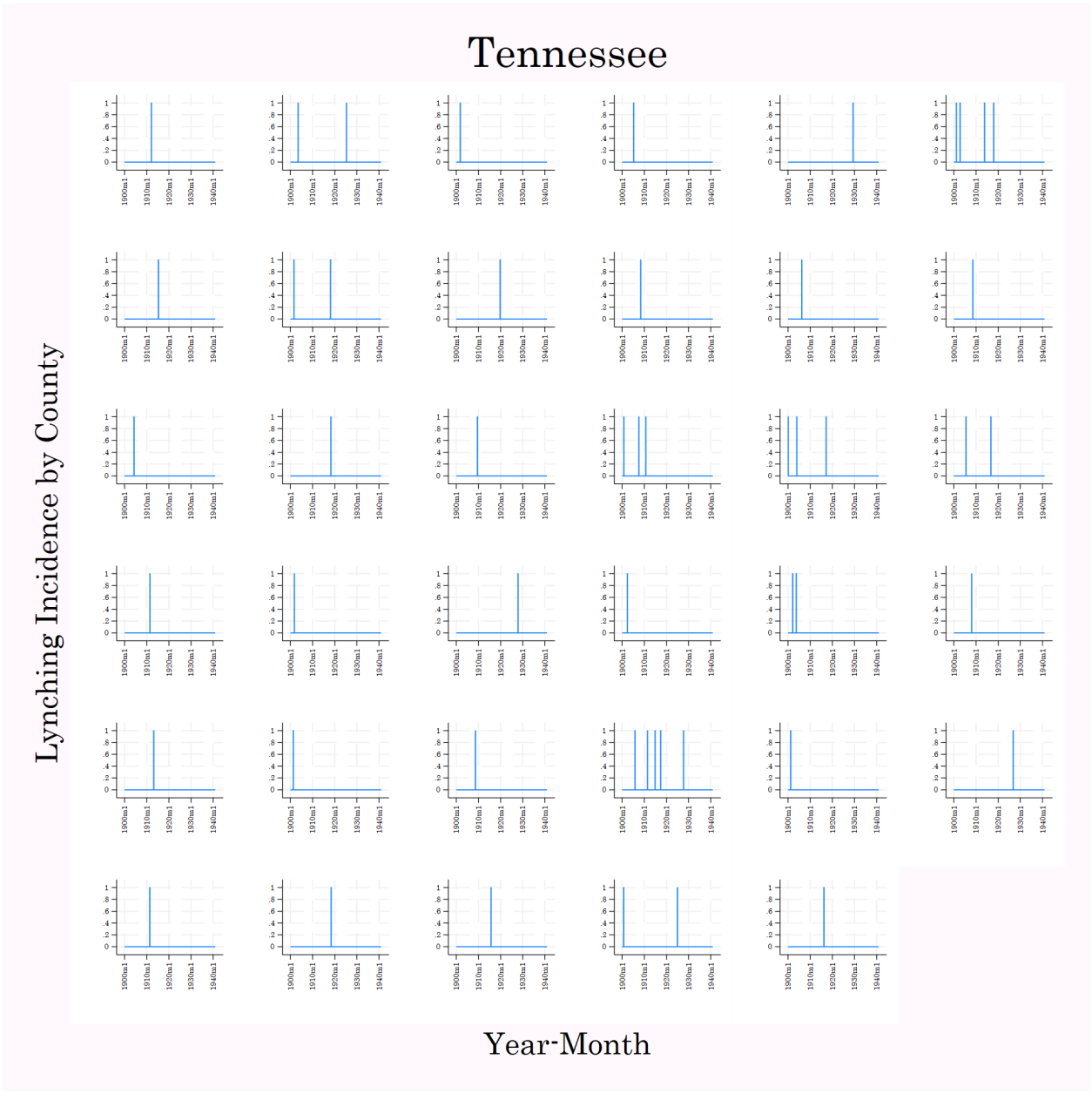
Appendix Figure G-10 - Time Series Graphs of Lynching Incidents across Counties of North Carolina



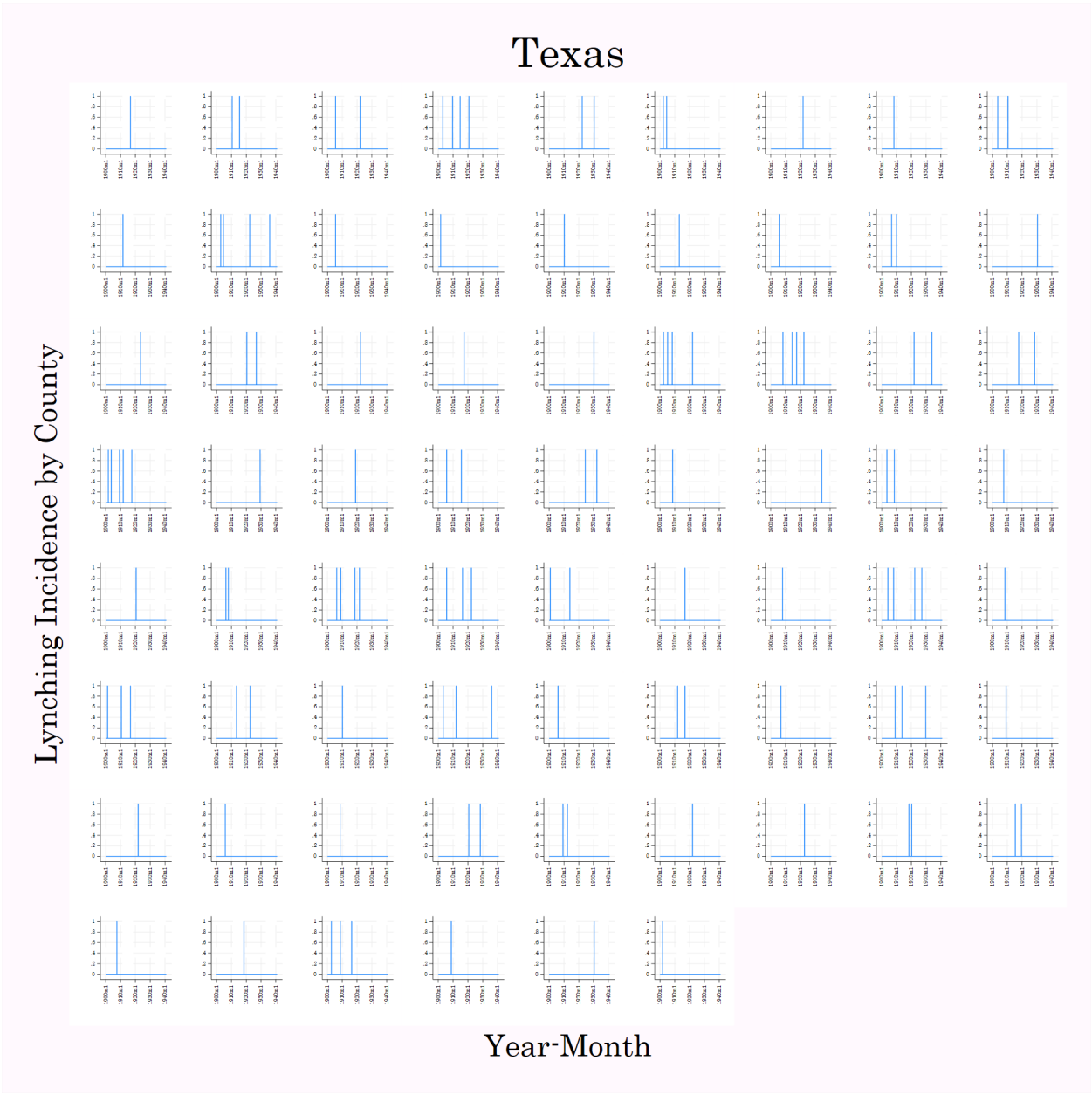
Appendix Figure G-11 - Time Series Graphs of Lynching Incidents across Counties of Oklahoma



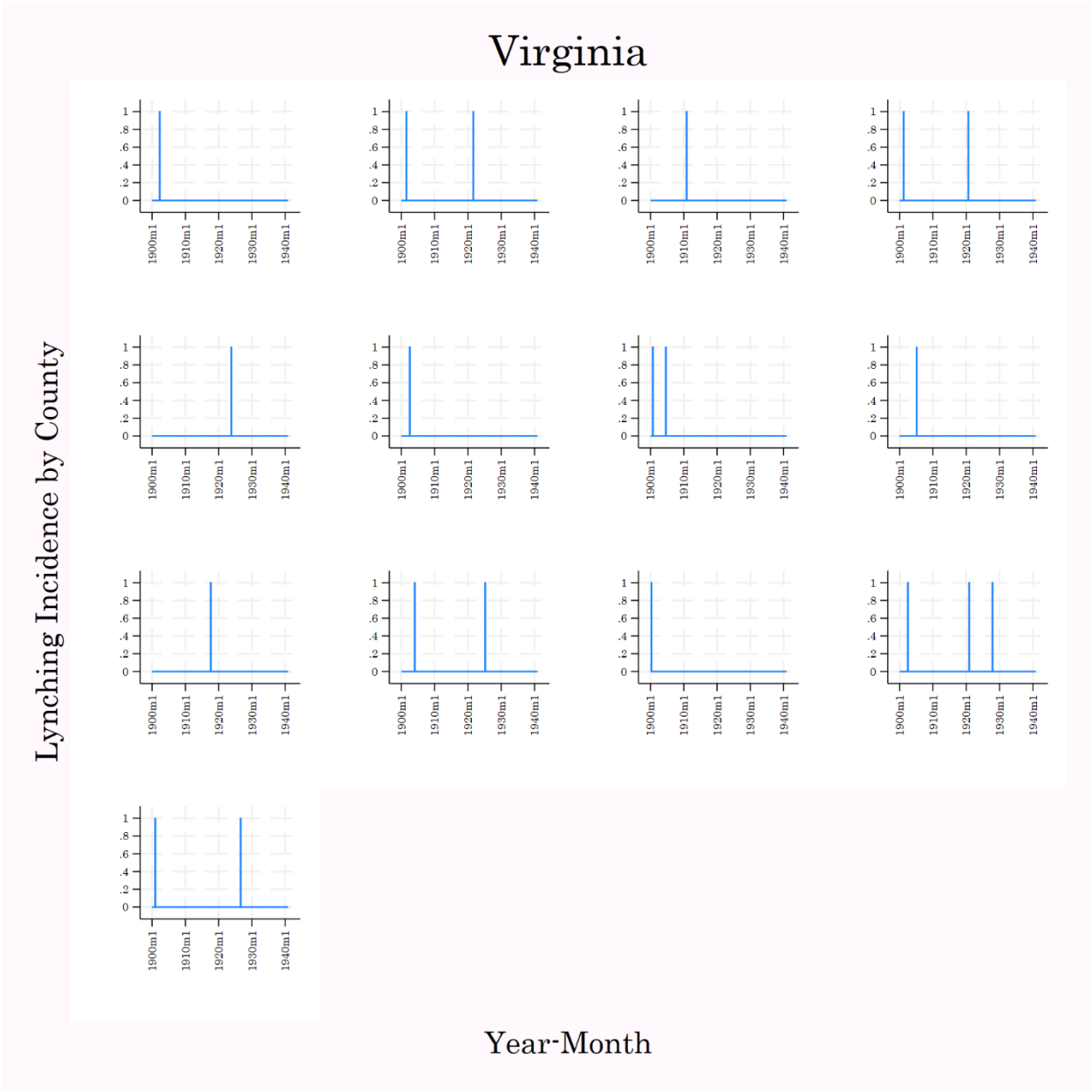
Appendix Figure G-12 - Time Series Graphs of Lynching Incidents across Counties of South Carolina



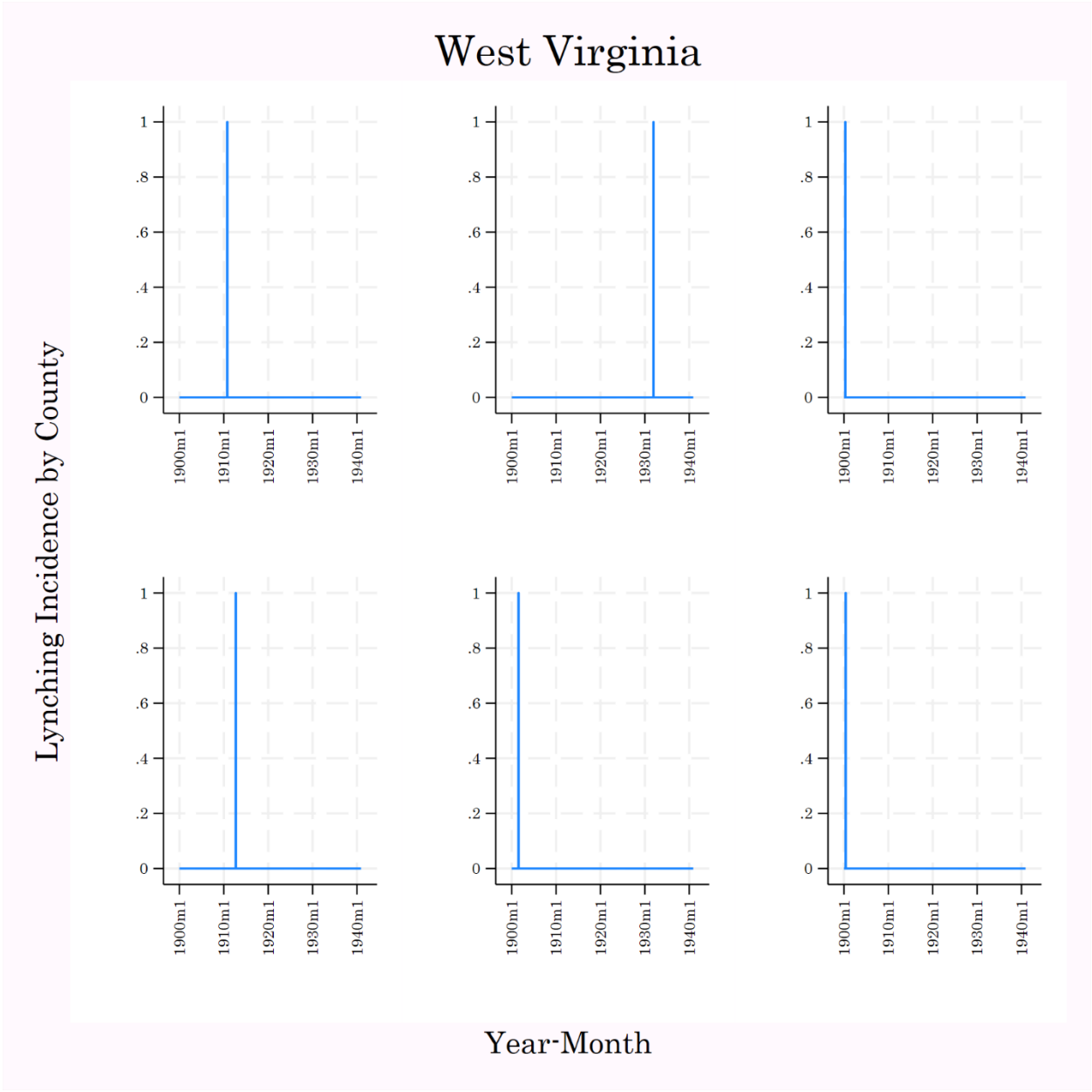
Appendix Figure G-13 - Time Series Graphs of Lynching Incidents across Counties of Tennessee



Appendix Figure G-14 - Time Series Graphs of Lynching Incidents across Counties of Texas



Appendix Figure G-15 - Time Series Graphs of Lynching Incidents across Counties of Virginia

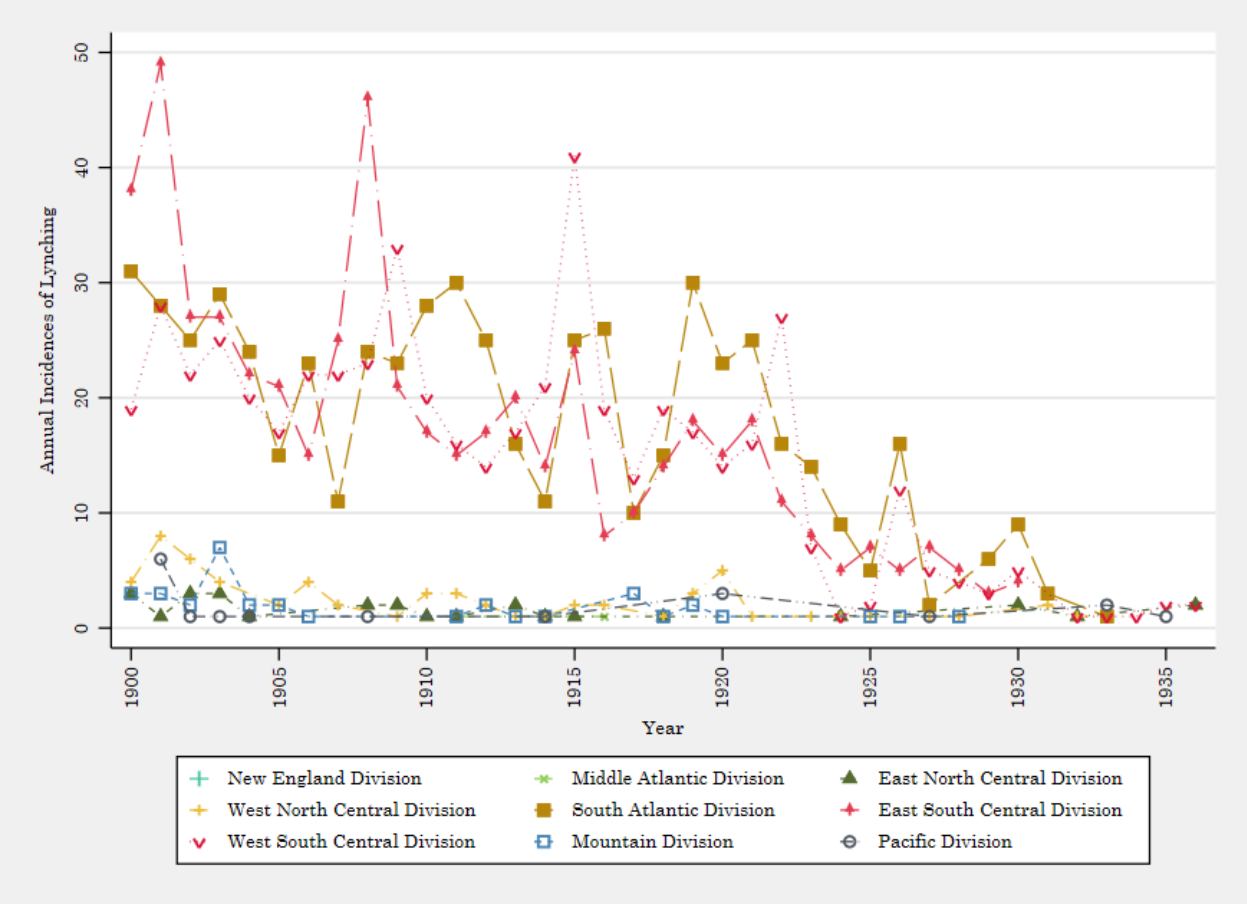


Appendix Figure G-16 - Time Series Graphs of Lynching Incidents across Counties of West Virginia

Appendix Table G-1 - Distribution of Lynching Incidents by Temporal Proximity Within the Same County in the Original HAL Data

Variable	Mean	Std. Dev.	Min	Max
Within 12 Months	3.5167	19.4844	0	400
Within 24 Months	6.8097	28.0384	0	800
Within 36 Months	9.9109	34.7677	0	720
Within 48 Months	12.8551	40.7491	0	1600
Within 60 Months	15.6113	45.7446	0	1000
Within 120 Months	27.5915	66.9782	0	1680

Notes. All numbers are multiplied by 100 so that the mean represents percentage of counties that experienced another lynching within the given time span.



Appendix Figure G-17 - Timeseries Graph of Lynching Incidence across Census Divisions in the Lynching Data

Appendix H

Appendix Table H-1 - Effects on Migration

	Outcome:	
	Cross-County Migration	Cross-State Migration
	(1)	(2)
In utero exposure to lynching × Black	.0001	.0015
	(.0078)	(.0052)
Observations	56,177	56,177
R-squared	.11	.08
Mean DV	0.243	0.095
Lynching event fixed effects	✓	✓
Parental controls	✓	✓
County-level controls	✓	✓

Notes. This table examines whether in utero exposure to lynching affected later-life migration patterns among Black men, helping to assess whether selective migration could confound our main longevity estimates. Cross-county migration is an indicator equal to one if the individual's county of residence recorded in the 1940 census differs from their county of birth. Cross-state migration is an indicator equal to one if the individual's state of residence in 1940 differs from their state of birth. Data are from the Social Security Death Master File (DMF). Standard errors, clustered on county, are in parentheses. Regressions include lynching incidence fixed effects and controls. Parental controls include dummies for maternal education, paternal education, and paternal socioeconomic status. County covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, and literacy rate.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix Table H-2 –Using Only County of Residence in 1940 as a Proxy of County of Birth

	Outcome: Age at death (months)		
	(1)	(2)	(3)
In utero exposure to lynching × Black	-3.76**	-3.76**	-3.75**
	(1.65)	(1.65)	(1.64)
Observations	55,342	55,342	55,342
R-squared	.32	.32	.32
Lynching event FE	✓	✓	✓
Parental controls		✓	✓
County-level controls			✓

Notes. This table replicates the main analysis using county of residence in 1940 as a proxy for county of birth for all observations, rather than the preferred approach of cross-census linking. In the main analysis, county of birth is assigned using cross-census linking procedures from the Census Linking Project (Abramitzky et al., 2020), which identifies county of birth directly from historical census records for 76.2% of observations; county of residence in 1940 serves as a fallback proxy for the remaining 23.7%. The robustness of the estimates across both approaches suggests that measurement error in county of birth assignment is unlikely to meaningfully bias the main results. Data are from the Social Security Death Master File (DMF). Standard errors, clustered on county, are in parentheses. Parental controls include dummies for maternal education, paternal education, and paternal socioeconomic status. County covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, and literacy rate. FE = fixed effects.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix I

Appendix Table I-1 – Robustness to Including Non-Southern Lynchings

	Outcome: Age at death (months)		
	Males	Females	Full Sample
	(1)	(2)	(3)
In utero exposure to lynching × Black	-3.59***	-.79	-2.41***
	(1.19)	(.99)	(.73)
Observations	162,953	179,777	342,916
R-squared	.33	.48	.42
Lynching event fixed effects	✓	✓	✓
County-level controls	✓	✓	✓

Notes. Our main sample primarily consists of Southern states because the Project HAL database contains lynching information only for that region. This table reports results from a specification that additionally incorporates lynching incidents occurring outside the South. Standard errors, clustered on county, are in parentheses. County covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, and literacy rate. Data are from the Berkeley Unified Numident Mortality Database (BUNMD).

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

Appendix J

To address concerns regarding potential spillovers across counties, this appendix expands our analysis to incorporate neighboring counties. Specifically, we assign lynching exposure based on incidents occurring within the county of birth as well as in any directly neighboring counties, and interact these exposure measures with an indicator for Black men. We then estimate regressions that include lynching event fixed effects and county-level controls.

The results are reported in Appendix Table J-1. Across all three columns, the only consistently sizable and statistically significant coefficient is own-county exposure — Black men exposed in utero to a lynching in their county of birth experience a reduction in longevity of 4.50 months in the full sample (Column 1), 4.21 months for counties with below-median average distance to neighboring counties (Column 2), and 4.71 months for counties with above-median average distance (Column 3). The neighboring county exposure coefficient is statistically indistinguishable from zero in the all three columns.

Appendix Table J-1 - The Effects of Own and Neighboring Counties' Lynching Incidence on Longevity

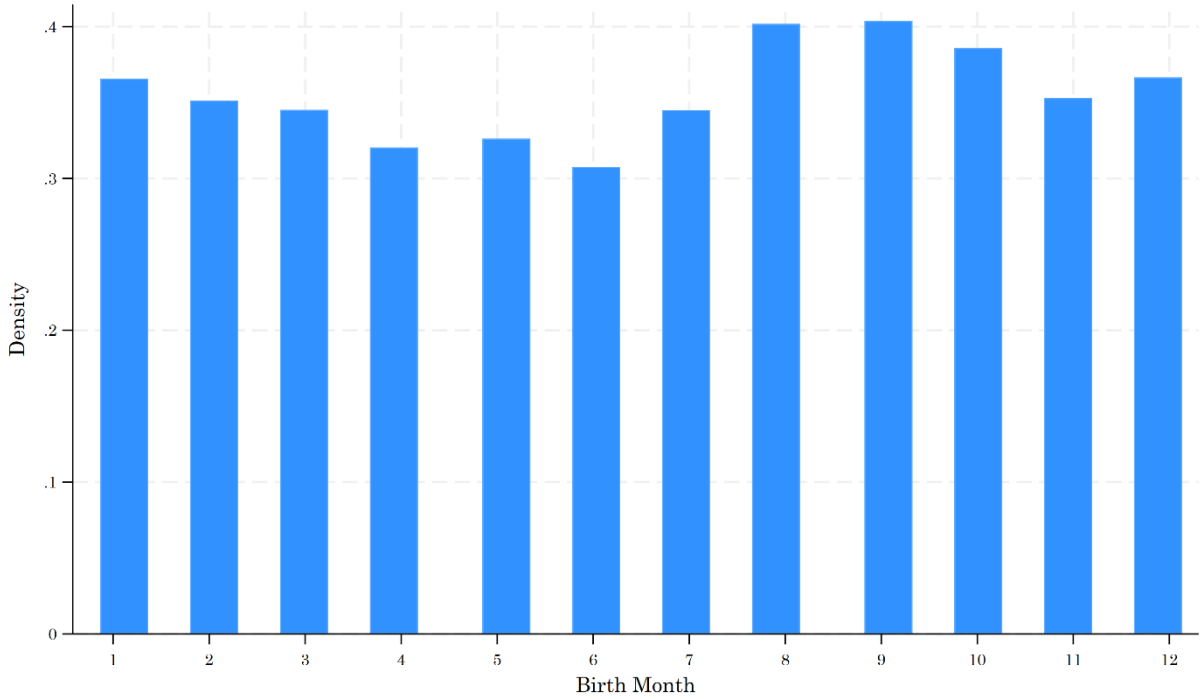
	Outcome: Age at death (months)		
	Full Sample	Below-Median Average Distance with All Neighboring Counties	Above-Median Average Distance with All Neighboring Counties
	(1)	(2)	(3)
In utero exposure to lynching (Own County) × Black	-4.50***	-4.21**	-4.71**
	(1.38)	(1.84)	(2.00)
In utero exposure to lynching (Neighboring County)	2.32	1.11	3.44*
	(1.56)	(2.37)	(2.05)
Observations	130,024	55,368	74,656
R-squared	.34	.32	.36
Lynching event fixed effects	✓	✓	✓
County-level controls	✓	✓	✓

Notes. A potential concern is that lynching events in neighboring counties may spill over into the county of birth, biasing our estimates. To address this, we expand our exposure measure to include lynching incidents occurring in both the county of birth and any directly adjoining counties. Standard errors, clustered on county, are in parentheses. Regressions include lynching event fixed effects and county-level controls. County covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, and literacy rate. Data are from the Berkeley Unified Numident Mortality Database (BUNMD).

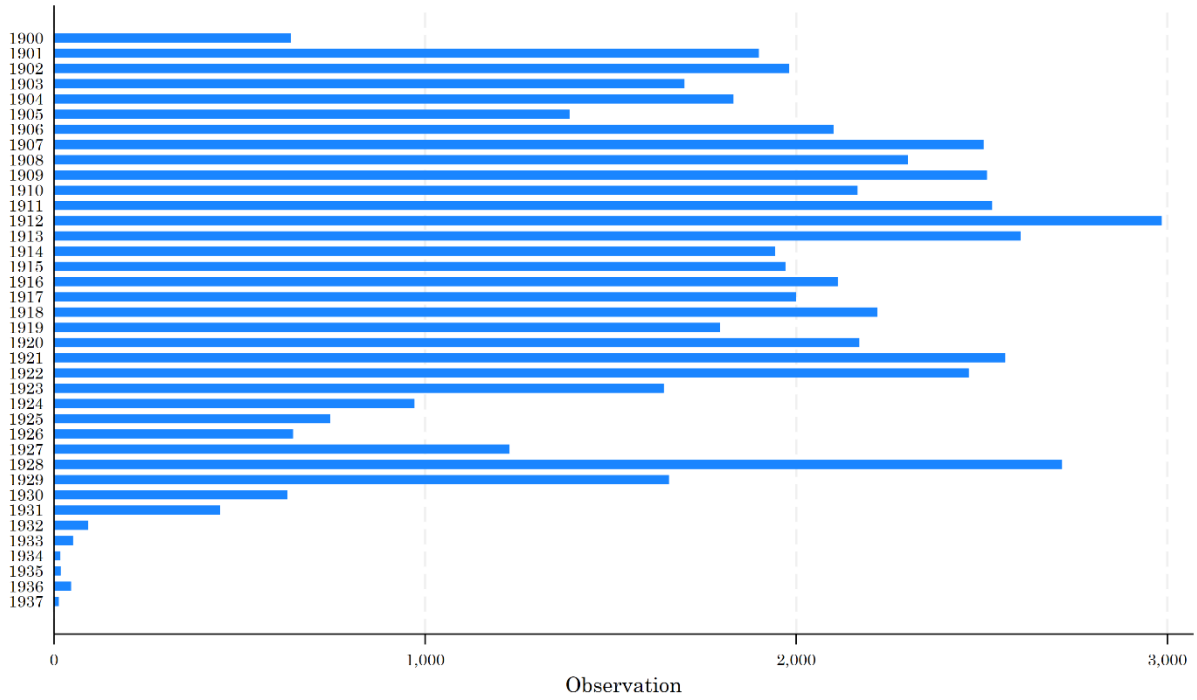
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix K

The month and year of birth in our sample are sourced from the Berkeley Unified Numident Mortality Database and Death Master File. A potential concern is that birth months might be inaccurately recorded. For instance, lumping in January could indicate that missing information is systematically assigned to this month. To address this, we include the distribution of birth months and years in this appendix. As shown in the figure below, the distribution of birth months is relatively uniform, with no notable concentration in January or any other specific month, suggesting that the data is not affected by systematic imputation or recording errors.



Appendix Figure K-1 - Distribution of Birth Months



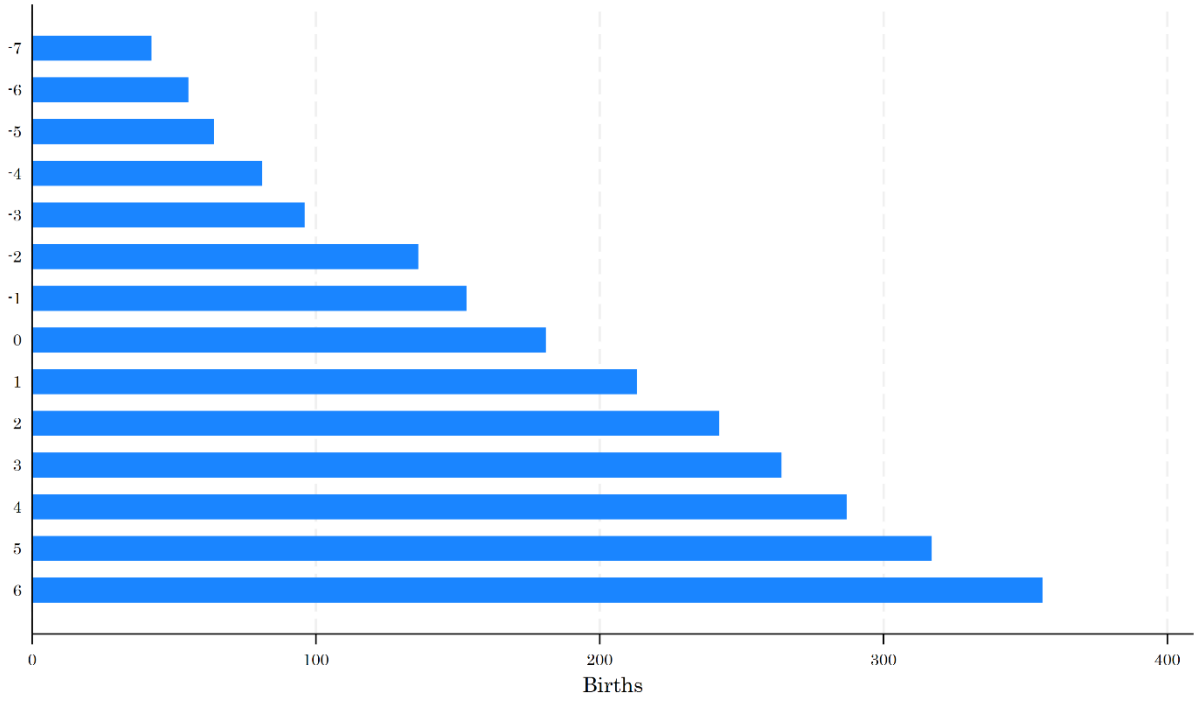
Appendix Figure K-2 - Distribution of Birth Years

Appendix Table K-1 - Robustness to Removing One Birth Month at a Time

	Outcome: Age at death (months)												
	Baseline	Removing Birth Month: January	Removing Birth Month: February	Removing Birth Month: March	Removing Birth Month: April	Removing Birth Month: May	Removing Birth Month: June	Removing Birth Month: July	Removing Birth Month: August	Removing Birth Month: September	Removing Birth Month: October	Removing Birth Month: November	Removing Birth Month: December
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
In utero exposure to lynching × Black	-3.52***	-3.57***	-3.64***	-3.56***	-2.45*	-3.54***	-3.49***	-3.52***	-4.47***	-3.37**	-3.90***	-3.57***	-3.10**
	(1.25)	(1.26)	(1.36)	(1.32)	(1.28)	(1.28)	(1.32)	(1.35)	(1.31)	(1.34)	(1.28)	(1.29)	(1.29)
Observations	130,024	118,727	119,524	118,982	119,920	119865	120,369	119,764	118,387	118,283	118,415	119,189	118,502
R-squared	.341	.344	.342	.3424	.342	.343	.342	.342	.342	.343	.342	.343	.343

Notes. A potential concern is the accuracy of reported birth months in the data. Specifically, birth months of January (month = 1) or June (month = 6) may be disproportionately recorded when birth month information is missing, reflecting common imputation patterns. To assess the sensitivity of our results to this issue, this table presents estimates from models that sequentially exclude each birth month. Regressions include lynching incidence fixed effects and county-level controls. County covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, and literacy rate. Data are from the Berkeley Unified Numident Mortality Database (BUNMD).
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix L



Appendix Figure L-1 - Number of Births in the County CDC Data across Years Relative to County-Specific Lynching

Appendix M

Appendix Table M-1 - The Effects of In-Utero Exposure to Lynchings of White Victims on White Longevity

	Outcome: Age at death (months)		
	Males	Females	Full Sample
	(1)	(2)	(3)
In utero exposure to lynching × Black	2.59	-3.64	-.33
	(3.59)	(2.91)	(2.41)
Observations	11,307	11,971	23,342
R-squared	.31	.48	.41
Lynching event fixed effects	✓	✓	✓
County-level controls	✓	✓	✓

Notes. This table shows results examining the impact of White lynchings on White longevity. Standard errors, clustered on county, are in parentheses. County covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, and literacy rate.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix N

Appendix Table N-1 - In-Utero Exposure to Public Executions and Later-Life Longevity

	Outcome: Age at death (months)		
	(1)	(2)	(3)
In utero exposure to Execution × Black	1.10	.02	.39
	(.77)	(.86)	(.58)
Observations	364,330	335,390	699,935
R-squared	.47	.61	.55
Execution event fixed effects	✓	✓	✓
County-level controls	✓	✓	✓

Notes. This table presents the results for another falsification test. The treatment variable, In Utero Exposure to Execution × Black, captures whether an individual was in utero during a county-level execution event, interacted with an indicator for race. Execution data are obtained from ICPSR. Because the ICPSR dataset does not distinguish between public and private executions, the analysis includes all executions regardless of visibility. This table serves as a falsification test: unlike lynchings — which were extrajudicial acts of mob violence carried out without due process — executions were administered through formal legal proceedings, and thus plausibly should not generate the same terror-driven effects on Black birth cohorts. Consistent with this interpretation, the estimated coefficients are small in magnitude and statistically indistinguishable from zero across all specifications, providing no evidence that legally sanctioned executions affected the longevity of in-utero Black cohorts.

Standard errors, clustered on county, are in parentheses. County covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, and literacy rate. Data are from the Berkeley Unified Numident Mortality Database (BUNMD).

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