The Economic Legacy of Racial Violence in the American South*

Luke N. Condra† Daniel B. Jones‡ Randall P. Walsh§

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Abstract

How does early-life exposure to racialized violence affect later-life economic outcomes? We study this question in the context of lynchings of Black citizens in the American South between 1880 and 1920 and provide systematic evidence of long-run economic impacts of that violence for the broader community and the effects' persistence across generations. First, using data on averted lynchings and matched placebos as counterfactuals, we show that children indirectly exposed to the racial trauma of lynchings (proxied by close proximity to the victim's household location) exhibit a reduction in occupational income score and likelihood of holding a white collar occupation, in their prime earning years as adults. We also observe intergenerational effects: children of the individuals who were exposed (as children) to lynchings see, as adults observed in 1940, a reduction in their income relative to counterfactual individuals. By documenting long-run and intergenerational economic effects of exposure to lynchings, we add empirical evidence to an interdisciplinary literature that identifies racial trauma as a distinctive and durable form of psychological harm. *JEL codes*: J15, N31, J62.

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†School of Public and International Affairs, University of Pittsburgh, 230 S Bouquet St, 3601 Wesley W Posvar Hall, Pittsburgh, PA 15260. Email: lcondra@pitt.edu

‡School of Public and International Affairs, University of Pittsburgh, 230 S Bouquet St, 3601 Wesley W Posvar Hall, Pittsburgh, PA 15260. Email: daniel.jones@pitt.edu

§Department of Economics, University of Pittsburgh, 230 S Bouquet St, 4700 Wesley W Posvar Hall, Pittsburgh, PA 15260. Email: walshr@pitt.edu

Introduction

The legacy of racial violence in the United States is deeply embedded in the social, psychological, and economic fabric of the nation. Among the most brutal and public expressions of this violence were lynchings: extrajudicial killings, overwhelmingly perpetrated by white mobs against Black Americans, particularly in the post-reconstruction South. In this context, lynching served as both a mechanism of racial control and a spectacle of terror (Tolnay and Beck, 1995; Carrigan, 2004), intended to reinforce white supremacy and suppress Black agency. While a growing body of research has documented the prevalence, causes, and political effects of lynching (Wells-Barnett, 1895; Brundage, 1997; Tolnay and Beck, 1995; Cook, 2014; Jones, Troesken and Walsh, 2017; Testa and Williams, 2025), much less is known about the long-term economic and psychological consequences of such violence, particularly in the children and communities who suffered its witness.

This gap in the research is important given that there are reasons to believe that exposure to such violence could have profoundly harmful and long-lasting effects, both acutely through its coercive effects on economic choices and investments in human capital (Stelzner and Darity, 2026), and more broadly as a form of racial trauma (Howard and Hu, 2024). Further, the potential for harm is not limited to those living at the time of the lynching. Economists have clearly documented the potential for economic shocks to pass through generations (Black and Devereux, 2011; Corak, 2008) and there is increasing evidence that traumatic stress can be transmitted across generations (Yehuda et al., 2005, 2016; Yehuda and Lehrner, 2018; Conching and Thayer, 2019).

To assess these impacts, we evaluate systematic evidence of the long-run economic effects of lynchings in the American South that occurred between 1880 and 1919. To enable analysis of effects of violence at the individual-level, we link a comprehensive database of lynchings with US Census data on those individuals between 1900 and 1940. This approach has the

advantage of not only allowing analysis using different degrees of "exposure" to violence, but provides data on actual economic outcomes at the individual level.

A core challenge in this work is estimating causal effects of violence since violence is strategically produced (e.g., mobs might be more likely to target Black victims for lynching in relatively impoverished areas). To advance a causal interpretation of our estimates of various effects of violence, we employ two complementary approaches. First, we use event data on "threatened" (but failed) lynchings across the South between 1866-1975 (Beck, 2015; Beck, Tolnay and Bailey, 2016; Beck, 2022) to construct a set of counterfactuals against which we can compare those areas and individuals that suffered actual lynchings. This approach follows others who have leveraged inherent randomness in "failed" events as a counterfactual for observed violent events (Jones and Olken, 2009). Where the assumption of event "failure" randomness holds, this approach is essentially identical to a Randomized Control Trial (RCT) study. Second, we use a propensity score matching approach to construct plausible counterfactual individuals based on pre-treatment observable characteristics (Leuven and Sianesi, 2003). Results from both approaches are broadly similar, strengthening confidence in the causal claims we make as the approaches rest on different identification assumptions.

We document the effects of varying degrees of proximate indirect exposure to lynchings on long-run economic outcomes, measured when individuals are between the ages of 25 and 40. Childhood exposure (through age 18) to a nearby lynching—defined as occurring within 10 households—is associated with a 3 percent reduction in occupational income score (approximately 0.065 SDs), a 2 percent reduction in the likelihood of holding a white-collar occupation (approximately 0.099 SDs), and an increase of roughly two percentage points in the likelihood of having no occupation (approximately 0.058 SDs). These results are robust to comparing completed lynchings to averted lynchings and to a matched placebo. We then show that the effects of lynching exposure persist across generations. The children of individuals who were themselves exposed to lynchings as children suffer an income penalty

in adulthood—observed in 1940—of 10 to 13 percent relative to comparable individuals identified through either averted lynchings or propensity score matching.

We then draw on the literature and consider plausible mechanisms connecting lynching to these economic effects, including trauma from racial violence and violence as a means of economic control. To illustrate the logic of these mechanisms, we draw on rich repositories of interviews of Black Americans' memories of lynchings in the American South and the effects this violence had on them, their families, and their communities (Rogers, 2006; Sims, 2016). We use available data to test more systematically some observable implications of the mechanisms in an effort to shed further light on the ways in which this racial violence had the effects that we observe.

By highlighting the effects that lynchings had on communities and across generations, our work underscores that the economic effects of lynchings reverberate to have broader effects on aggregate Black-white inequality for decades thereafter. Our findings show that children who lived near lynching victims experienced worse economic outcomes than those who did not, as did their children when observed in 1940. In a calculation based on our estimates discussed later in the paper, we estimate that the aggregate value of lost wages for the second generation individuals (those observed in 1940) amounts to roughly $43 billion in 2025 dollars.

With regard to existing research, our results speak to several broader literatures. First, our work provides evidence for how violence contributes to an "enduring repressive tradition" (Jacobs, Kent and Carmichael, 2007, 657) marked by injustice along racial lines in the United States. In this sense, our results are consistent with the framework of stratification economics (Darity and Mason, 1998; Darity, Hamilton and Stewart, 2015), which emphasizes that group-based structural forces, including racial violence, create persistent intergenerational economic disparities unexplained by individual characteristics alone. Arguments in this literature for how structural racism embeds disadvantages in communities and families across

generations align directly with our evidence. Showing systematically that lynchings had long-run, intergenerational negative economic effects provides further empirical grounding for claims that historical racial violence and exclusion translated into persistent group disparities (Althoff and Reichardt, 2024; Cook, 2014; Darity and Mullen, 2022).

Second, we contribute to an emerging economic literature by bringing to bear insights from racial trauma theory and applying rigorous empirical methods to estimate the long-term impact of a type of racially motivated violence on children who were indirectly affected. In so doing, we build on two strands of related work. The first examines how racial trauma from violence affects outcomes, which remains an open question.[1] The second studies the effects of second-hand exposure to racism—or "vicarious" racism (Iruka et al., 2022)—about which less is known relative to the effects of direct exposure to racism (Heard-Garris et al., 2018; Howard and Hu, 2024).

Third, we contribute to work in historical political economy (Jenkins and Rubin, 2024), and specifically on political violence (Walden and Zhukov, 2020), by documenting the persistence of economic effects of violence across generations (Lupu and Peisakhin, 2017; Rozenas, Schutte and Zhukov, 2017). Our work highlights opportunities for future research on the channels through which these effects are transmitted and persist (Acharya, Blackwell and Sen, 2024). In so doing, we empirically enlarge our understanding of the impacts of these atrocities beyond the victims themselves (Bailey and Tolnay, 2015), showing evidence that even those in the community outside of the victim's family were affected by the trauma, as well as their children decades later.

[1] "How PTSD relates to other political, social and economic outcomes represents an open question, and a gap between the findings of public health scholars and other legacies of conflict work" (Walden and Zhukov, 2020).

Context and Conceptual Frameworks

In this section, we provide a preliminary overview of two key potential theoretical channels through which early-life exposure to racialized violence may impact later-life outcomes. We revisit each later in the paper, where we draw on interviews to further develop the connection between each mechanism and our own work. We also highlight other work on lynchings relevant to our work.

Regarding potential channels, first, some existing work argues and provides evidence that lynchings were a tool of economic control (Beck and Tolnay, 1990; Woodruff, 2003; Lombardi and Mriziq, 2022). Particularly relevant to our work, Stelzner and Darity (2026) argue that lynchings were used to create fear to extract labor, specifically from Black workers in occupations that were especially subject to coercion. Consistent with their argument, they find that white-on-Black homicides in the pre-harvest period across the South in 1879 were associated with an increase in the total number of hours of labor expended on Black farms engaged in sharecropping (but had no effect on labor supply where Blacks had more economic agency: Black-owned farms or where the farmer rented for cash).

This finding relates to earlier evidence documenting that during economic downturns resulting from poor returns on cotton harvests, lynchings increased, possibly through either instrumental mechanisms (e.g., labor substitution (Raper, 1933)) or psychological ones (e.g., frustration and aggression (Hovland and Sears, 1940)) resulting in racial violence. Beck and Tolnay (1992, 6) argued that landowners and planters had to exercise the most control over labor during periods of greatest need for labor, "in the early summer when the cotton had to be chopped and weeded, and the second during the fall harvest." They note that as accounts of lynchings generally reference violations of criminal codes, "we suspect that the linkage between mob violence and the labor market was, for the most part, indirect and diffuse, as opposed to being direct and specific."

In our context, if lynchings are used as a tool of economic control, Black children exposed to nearby lynchings may be more likely to be pulled into the workforce and/or their parents may be locked into work and locations that are less beneficial to the family. These effects would have impacts on children's later-life outcomes and also—through intergenerational transmission of education and income—on later generations' outcomes.

A second channel we consider is the trauma resulting from exposure to racialized violence. There is a considerable interdisciplinary literature that links exposure to violence with persistent and negative effects on individuals (Herman, 1997). This exposure need not even be experienced first-hand: negative impacts result from even second-hand exposure.[2] Exposure to violence and abuse, particularly early in life, has been studied in the context of psychopathology. Detrimental effects of various types of violence on children's development are severe and wide-ranging, and include aggression and self-destructive behavior, depression and anxiety disorders, various psychobiological effects, PTSD, delayed cognitive development, and may be long-run in nature (Margolin and Gordis, 2000; Sharkey, 2018).[3] Studies show increased risk of PTSD (Johnson and Thompson, 2008; Kilpatrick et al., 2003) and depression (McLaughlin, Weissman and Bitrán, 2019) through different neural mechanisms (Weissman et al., 2020; Conching and Thayer, 2019).

More broadly, exposure to violence has been studied for how it affects individuals' subsequent decision-making and well-being. Cabral et al. (2026) show that experiencing gun violence at public schools in Texas increases those survivors' absenteeism and grade repetition, reduces their high school graduation, college enrollment, and college completion, and reduces their employment and earnings in their early 20s. In related work, Ang (2020) studies the effect of police officer-involved killings' on measures of Los Angeles public school

[2] As Walden and Zhukov (2020) observe, "Importantly, PTSD can arise following both direct and indirect exposure to violence, as individuals learn of the harrowing experiences of others."

[3] "Long after the danger is past, traumatized people relive the event as though it were continually recurring in the present. They cannot resume the normal course of their lives, for the trauma repeatedly interrupts. It is as if time stops at the moment of trauma" (Herman, 1997, 37).

students' well-being. Exposure to such violence leads to lasting drops in grades, higher rates of emotional struggles, and lower chances of finishing high school or going to college. Importantly, these effects appear only among Black and Hispanic students, and especially when the person killed was from a minoritized group and unarmed.

Critically, the effects of trauma can be transmitted across generations. In the context of lynching, Gaston (2021, 83) writes that "These accounts of intergenerational, internalized trauma responses are consistent with research indicating that cumulative, collective historical racialized trauma and its effects can be experienced over multiple generations. This means that even descendants who did not directly experience the traumatic event can evince trauma-related symptoms and responses." In the context of enduring and persistent effects of trauma, researchers and practitioners argue that racism, especially violent or dehumanizing acts, can cause psychological injuries similar in scope to PTSD. In this frame, racial trauma is a chronic form of identity-based trauma resulting from racism, with emphasis on community-wide events including lynchings, police violence, and systemic racism.

Finally, the economics literature on the direct impacts of lynching exposure is relatively sparse. As discussed above, working at the county level, Stelzner and Darity (2026) examine how lynching served to coerce Black sharecroppers onto the land. Jones, Troesken and Walsh (2017) and Williams (2022) document the chilling effects of lynchings on voter turnout over both the short and long run, and Testa and Williams (2025) examines their political dimensions more broadly. Three papers are most closely related to our work. Gabriel et al. (2023) find that family members of lynching victims were 10 percentage points more likely to leave their county in the ensuing decade. Antman and Duncan (2024) provide suggestive evidence that childhood exposure to the lynching of Mexicans in Texas reduced home ownership rates among Mexican Americans. Finally, Vu et al. (2023) document that prenatal exposure to a within-county lynching had lasting effects on mortality.

Data and Empirical Approach

Data

Our study leverages four main sources of data. The first two of these data sources relate to lynchings, both completed and averted. We then link the individuals that appear in these lynching data to full count censuses for the years 1880 thru 1910. Finally, we leverage Census Tree linkages data to connect these individuals and their children to data in later census years, up through 1940.

Lynchings Data

For our primary source of lynching data, we draw on an updated database of lynchings produced by Tolnay and Beck.[4] The data include all events that the researchers could identify as lynchings from 1865-2020. All events are in Southern and Border states. We restrict the sample to lynchings with a named Black victim from 1880-1919, which results in 2,840 events. Our year-based restriction is a function of the Censuses that we use in the study. 70% of lynchings with Black victims in the database occur during this time period. In these data, we observe the name of the victim, the date of the lynching, and the state and county where the lynching occurred.

The second lynching-related database we use contains records of known threatened lynchings that did not ultimately end in the murder of the victim. These 'threatened' (but failed) lynchings data (Beck, 2015; Beck, Tolnay and Bailey, 2016; Beck, 2022) are collected from across the South between 1866-1975. Events are gathered from newspapers and coded to the county level for Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland,

[4]Another often used dataset in the literature is that from the HAL Project, which was based on the original data used in Tolnay and Beck (1995). The data in Tolnay and Beck (1995) has been superseded by the Beck-Tolnay lynching inventory, which is in continuous update by the authors, and which we use here. Personal correspondence with author, March 30, 2023.

Mississippi, North and South Carolina, Tennessee, Virginia, and West Virginia.

Studying the details of these events suggests a large degree of randomness in whether a lynching occurred or was narrowly averted (i.e., threatened). These events range from situations where it was known that an individual was the potential target of a lynch mob, but no lynch mob was ultimately formed; to situations where a lynch mob was formed but called off or stopped by law enforcement; and ultimately to situations where a mob was formed but the victim escaped. Local or state law enforcement sometimes stepped in to drive away mobs or protect would-be victims by moving them to other counties or penitentiaries (Beck, 2015).[5] Sometimes clergy intervened to persuade angry mobs to back down and allow the courts to handle alleged offenses (Beck, 2015). There were instances where groups of Black citizens banded together to stand up to lynching mobs, sometimes successfully (Beck, 2015; Hill, 2016, 116). Sometimes white citizens defused a potential lynching by vouching for the innocence of Black community members accused of a crime (e.g., Collins 2009; Brundage 1997, 27). In at least two bizarre incidents, the mobs themselves stopped to take a vote on whether to continue with the planned lynching and in each case, a majority voted against and the mobs disbanded on their own (Beck, 2015, 131).

In summary, existing descriptions of the incidents do not suggest a reliable predictor of failure. We investigate this issue in our data. Results of a series of regressions are presented in Table SI-1. We find that whether a lynching is completed or averted is not predicted by a variety of characteristics of a victim, their enumeration districts, or their household, including: enumeration district Black population share and total population, whether the victim is a child or adult, and the victim's household head's occupational score and literacy. There are two statistically significant predictors of whether a lynching is completed. First, lynchings are more likely to be averted in more urban areas. The inclusion of enumeration

[5]Controlling for county as we do in empirical specifications below will, to the extent sheriff jurisdictions followed county lines then as they largely do today, also control for any time-invariant sheriff-jurisdictional factors affecting likelihood of aversion.

district fixed effects in our analysis controls for these differences. Conversely, lynchings are more likely to be completed when the victim is female. Note, though, that this latter difference is based on a very small number of observations with female victims: only 45 out of roughly 1,300 completed and averted lynchings are recorded as having female victims in our data.

Throughout, we refer to these events interchangeably as threatened or averted lynchings. As we will detail below, we use these observations as one of our counterfactuals to estimate the long-run impact of exposure to lynching. As such, we exclude observations that are tagged as being threatened victims who were ultimately lynched in a later event (sometimes the same day as the first recorded event). We restrict the sample to events with Black threatened victims ranging from 1880-1919. There are 2,986 such observations. In these data, we observe the name of the threatened victim, the date of the threatened lynching, and the state and county where the threatened lynching occurred. For clarity, note that we refer to targets of completed and threatened/averted lynchings as victims; we distinguish those that ultimately ended in the murder of the target as "completed lynchings".

Census Data

By merging to census data we use the lynching datasets to identify individuals who, during childhood, lived in close proximity to lynching victims. We then link those individuals to their own adult outcomes. Doing so requires leveraging our two remaining sources of data: (1) 100% Censuses from 1880, 1900, 1910, 1920, 1930, and 1940, and (2) the Census Tree project's linkages of individuals across historical Censuses (Price et al., 2021).

As a first step, we use the restricted-use version of the 100% Censuses to identify the location of the lynching victims. The restricted-use version includes the names of the Census respondents. We use fuzzy matching to identify the Census observation of lynching victims from 1880-1899 in the 1880 Census, victims from 1900-1909 in the 1900 Census, and victims

from 1910-1919 in the 1910 Census. Doing so provides the last Census observation of an individual before the lynching. We match on first name, last name, and county. Fuzzy matching is required because names are not always spelled or formatted the same in the lynching databases and the Censuses. In our matching, we err on the side of higher certainty in matching at the expense of number of observations.[6] We ultimately match 786 victims from the completed lynchings database to the Censuses and 666 victims from the threatened lynchings database to the Censuses.

The key Census identifiers we take away from this matching process are the enumeration district and household serial number of the matched victims. Enumeration districts are the smallest geographic unit above address consistently available in the Censuses we use; they reflect the area covered by a single Census enumerator, generally on foot, and therefore function as a useful sub-county-level geographic area, in lieu of modern Census blocks and tracts. Grigoryeva and Ruef (2015) write that "the districts represent geographic areas where residents could feasibly know any of their neighbors, and interact with them on a regular basis". Within enumeration districts, households are numbered by serial number. The serial numbers are sequenced in the order that the enumerator visited a household. Thus, proximate serial numbers can largely be interpreted as proximate households. In most cases, for instance, a household with serial number s and a household with serial number $s + 1$ are next door neighbors.[7] We take advantage of these facts to identify close neighbors

[6]Specifically, we use the Stata command `reclink2` from Wasi and Flaaen (2015), which performs probabilistic record linkage. We fuzzy-matched observations across datasets based on: first name, last name, county (as numerical county codes), and sex. The county variable is the county of lynching in the lynching database, but county of residence in the Census. We imposed two requirements: that any record linkage must match on first letter of lastname and county code. Often, the procedure identifies multiple potential matches in the Census for a given lynching victim. (Johnathan Smith from County A in the lynching database may be probabilistically matched with, e.g., Jonathan Smith (likely actual match), Nathan Smith, and Jonathan Schmitt from County A in the Census data – again, because the approach searches for all roughly similar strings and then ranks the resulting potential matches with a bigram similarity score – a measure of string similarity, here across the multiple string variables used for matching.) We keep the name with the highest bigram similarity score. We further imposed a lower bound on allowable bigram similarity score; we set a relatively high lower bound (0.85) after manually reviewing candidate matches across the distribution of scores – again, with the aim of prioritizing quality matches.

[7]Grigoryeva and Ruef (2015) geocode data from Washington, DC to empirically validate that sequentially

of lynchings victims.

We focus on individuals who were born after 1865 but who were children (ages 0-18) as identified in the 1880, 1900, or 1910 census at the time of a lynching event. We take those individuals (and also similar birth cohorts from areas where there was no lynching) and use their birth year to identify the first subsequent Census where the individual is at least 25 years old. In practice, this identifies people between the ages of 25 and 40; the oldest cohort was born in 1865 and they are 15 in 1880. As there is no 1890 Census (the original enumeration documents were destroyed by fire), the first Census where we see them again beyond the age of 25 is the 1900 Census, at which point they are 40. For people observed as children in the 1900 or 1910 Census, the adult age ranges we observe run from 25-35. The oldest someone can be as a child in 1900 is 18; they would first be at least 25 in the 1910 Census (at age 28). The youngest someone can be in 1900 is 0; they are first at least 25 in the 1930 Census (at age 30). As such, this process leads us to match individuals who are children in 1880, 1900, or 1910, to their cross-Census identifiers in Census Tree data from 1900, 1910, 1920, 1930, and 1940. We can then directly attach childhood lynching exposure to the individuals' appearance as an adult of at least age 25 in one of the 100% Census Files from those years.

Our final dataset therefore consists of individuals observed as being at least age 25 in the 1900-1940 Censuses, the labor market outcomes they experience at time of observation, their location, and also their childhood lynching exposure status. The lynching exposure status includes both whether there was a completed or averted lynching in the enumeration district where they were observed as a child; for those in districts where there was an event, we record how many households they were from the victim, the observed individual's age at time of event, and information about the victim.[8]

listed households with sequentially listed serial numbers are in fact neighbors. Logan and Parman (2017) also take advantage of the fact that sequentially listed households are neighbors in order to construct a neighbor-based measure of residential segregation.

[8]In Appendix Table SI-2, we assess whether either our treatment variables or children's parents' economic

The specific labor market outcomes on which we focus are occupational income scores and also a coding of occupational category. Occupational income score is used in lieu of directly observing income and essentially captures the average income of individuals working in a particular occupation.[9] We also code occupations as falling in one of five categories: agriculture, white collar, blue collar, labor/service, and no formal occupation.[10] Finally, in some analyses, we take locational variables as outcomes to assess the role of migration in driving our labor market results. Specifically, we construct "Moved State" which equals one if an individual lives in a different state as an adult than as a child, "Moved Region" which is the same concept but for Census region, and "Lives in South" which is a dummy equal to one if the individual remains in the South as an adult. We also take the variables identifying whether someone lives in an urban area or a farm as outcomes. Table 1 provides summary statistics for variables used in our analyses.

Intergenerational Analysis

Finally, while the bulk of our analysis focuses on later-life outcomes of children exposed to nearby lynchings, we also test for intergenerational impacts: the impacts on the children of people who were exposed to a lynching as a child. With regard to data construction, we largely follow a procedure similar to that outlined above. We start from our sample of people who were initially exposed to a lynching in childhood and the dataset where

or literacy status predict successful linkage to later-life observations. In short, successful later-life linkage is unrelated to growing up in an enumeration district where there was a lynching victim, unrelated to being a close neighbor of a lynching victim (or placebo or averted victim), and also the interaction of those – which is our key treatment measure. Later-life linkage is predicted by higher parental occupational income score during childhood, parental literacy during childhood, and living on a farm in childhood, but the magnitudes of these relationships are all quite small.

[9]This is a constructed income score based on the relative economic standing of occupations in 1950 and indicates the median total income by occupation with positive income. See the IPUMS USA User Guide for more description of this variable in the Census data.

[10]We define these broad occupations based on the "OCC1950" occupational coding in IPUMS. In particular, agriculture workers have codes of 100 (farmer) or 810-840 (farm laborers); white collar workers have codes of 0-99 (professionals), 200-290 (managers, officials, and proprietors), 300-390 (clerical and kindred workers), or 400-490 (sales workers); blue collar workers have codes of 500-595 (craftsmen) or 600-690 (operatives); labor or service workers have codes of 700-790 (service workers) or 910-970 (laborers).

Table 1: Summary Statistics for Outcome Variables, Main Sample

	mean	sd	min	max	count
Panel A: Adult Labor Outcomes					
Occ. Inc. Score	15.603	6.786	3.00	80.00	123713
ln(Occ. Inc. Score)	2.650	0.460	1.10	4.38	123713
Ag. Occ.	0.497	0.500	0.00	1.00	123707
Wht. Coll. Occ.	0.034	0.182	0.00	1.00	123707
Bl. Coll. Occ.	0.111	0.315	0.00	1.00	123707
Labor Occ.	0.357	0.479	0.00	1.00	123707
No Occ.	0.138	0.344	0.00	1.00	143430
Panel B: Adult Location					
Moved State	0.216	0.411	0.00	1.00	143430
Moved Region	0.144	0.351	0.00	1.00	143430
Lives in South	0.912	0.284	0.00	1.00	143430
Urban Area	0.311	0.463	0.00	1.00	143430
Farm	0.434	0.496	0.00	1.00	143430
Observations	143430				

Notes: Sample consists of Black census respondents first observed as a child in 1880, 1900, or 1910 Census and linked to first Census that they are at least 25 years old. Outcomes reported are at adulthood. Restricted to estimation sample: namely, those individuals who lived in an enumeration district with a completed, averted, or placebo lynch victim in their early life. Location variables "Moved State" and "Moved Region" indicate that the individual lives in a different state/region than in childhood.

we observe them as adults – in the first Census that they are at least 25 years old. We then identify any biological children of those people living in the same household as those individuals when they are observed. Finally, we link those children ahead to the 1940 Census to observe their adult outcomes – 1940 being the latest year in which Census Tree linkages are available. In linking to the 1940 Census, we can take actual income as an outcome, rather than occupational score.

Table 2: Summary Statistics for Outcome Variables, Intergenerational Analysis

	mean	sd	min	max	count
Wage Income	291.660	388.373	0.00	5001.00	39516
ln(Wage Income)	5.775	0.936	0.00	8.52	24903
Weekly Earnings	8.447	8.711	0.00	80.77	32642
Weeks Worked	33.134	20.829	0.00	52.00	43694
Observations	43694				

Notes: Sample consists of children of Black census respondents who were themselves first observed as children in the 1880, 1900, or 1910 Census in close proximity to a lynching victim. Children are linked forward to the 1940 Census, the first year in which actual income was reported. Income and weeks worked outcomes are measured at adulthood in 1940.

Empirical Strategy

Our empirical approach centers on identifying the later-life effects of living in close proximity to a completed lynching victim as a child. An important piece of our strategy relies on the inherent randomness of the proximity to a lynching victim. We do not claim that counties or even enumeration districts where lynchings occurred are as-if-randomly selected; we would argue, though, that *within* an enumeration district, living within close proximity (which we primarily define as within 10 households) of a victim vs. further away (say, 50 households) is plausibly exogenous.[11] The within-enumeration district comparison will be facilitated by generally included enumeration district fixed effects. Further note that our focus on within-enumeration district effects and variation in proximity to the victim will lead to lower bound estimates of the effects of a lynching, as it is plausible that the effects spread throughout the district. We simply claim, and test empirically, that there is a particularly acute effect of a close neighbor being a victim.

Of course, it is true that there are particular individual- and family-level correlates of experiencing a lynching, as documented by Bailey and Tolnay (2015). For example, lynching victims were more likely to be single men in lower earning occupations, and were less likely to be literate. Thus, there remains some concern that individuals living in very close proximity to individuals with those characteristics are similarly distinct in some ways that could drive their later-life outcomes.

With this concern in mind, a second aspect of our empirical strategy relies on comparing individuals in close proximity to a completed lynching victim to reasonably comparable

[11] Accounts of lynchings provide evidence supportive of this argument, in that the eventual victim was sometimes not even the one originally intended (e.g., Love 1995, quoted in Rogers 2000) or there was no consistent individual trait or provocation triggering a lynching: "No single provocation led to the mob attack. Instead the combination of Thompson's political activism, his religiously inspired crusade against moonshine, his independence, and his prosperity aroused the anger of his white neighbors" (Brundage, 1997, 24); and "The mixed motives and varied targets of terrorist mobs reveal the heterogeneous and contradictory nature of some lynchings" (Brundage, 1997, 25).

observations in other enumeration districts. We adopt two distinct approaches to identify "reasonably comparable observations": (1) comparing areas with completed lynchings to areas that experienced averted lynchings and (2) comparing areas with completed lynchings to propensity score matched individuals serving as "placebo" victims. We describe each approach in turn.

Completed Lynchings vs. Averted Lynchings

Our first strategy takes advantage of the averted lynchings database. In this approach, we take neighbors of threatened victims of averted lynchings as plausible counterfactuals for neighbors of victims of completed lynchings. As we match the averted lynching victims to Census data in the same way that we did with the completed lynching victims, we can similarly consider enumeration districts where an averted lynching occurred and identify all individuals within 10 households of the averted lynching. We similarly know the year that the event occurred and characteristics of the threatened victim. As such, we can combine the within-enumeration district comparison (very close vs. less close to the victim) and the across-enumeration district comparison (close to a completed lynching vs. close to an averted lynching) into a single specification. We restrict the sample to Black individuals who lived in an enumeration district where an averted or completed lynching occurred while they are 0-18 years old and estimate:

$$\begin{aligned} y_{i,t,k,e} = \beta_1 CompletedLynch_{e,t} \times CloseNeighbor_{i,t} \\ + \beta_2 CloseNeighbor_{i,t} + \beta_3 CompletedLynch_{e,t} \\ + \delta_k + \gamma_{e,t} + \Gamma' X_i + \varepsilon_{i,t,k,e} \end{aligned} \tag{1}$$

Here, $y_{i,t,k,e}$ is the labor market outcome for individual i when they are an adult and observed in decade k; they were first observed in childhood in decade t living in enumeration

district e. $CloseNeighbor_{i,t}$ is a dummy equal to 1 if individual i was a close neighbor of an averted or completed lynching victim in decade t. We define "close neighbor" as living within 10 households of the victim, excluding individuals in the victim's household.[12] β_2 captures any average difference between very close neighbors to threatened and completed lynching victims and more distant neighbors. $CompletedLynch_{e,t}$ is a dummy equal to one if the lynching event in the enumeration district was a completed, rather than an averted, lynching. β_3 therefore captures average differences that exist between enumeration districts where lynchings were completed versus averted, which may include institutional differences across locations in administration of justice. (In reality, this variable would be omitted and subsumed into the enumeration district fixed effects that we include, but we discuss it here to illustrate how our empirical approach works.) Having differenced out the general effects of living close to someone who is targeted for a lynching and also the differences between places where averted vs. completed lynchings occur, β_1, the coefficient on the interaction of the preceding two variables, captures the differential impact of living in close proximity to a completed lynching as a child – and is therefore our key parameter of interest.

We also include decade fixed effects for the year the individual is first observed at at least the age of 25 (δ_k) and enumeration district-by-decade fixed effects ($\gamma_{e,t}$). Finally, we include a vector (X_i) of individual-level controls (birthyear fixed effects, state of residence when observed as an adult, farm residence status as a child, and sex). We emphasize that when we refer to enumeration districts, these should be interpreted as enumeration district-by-early life Census decade fixed effects, as enumeration districts are not defined in the same way across Censuses. We thus have a distinct set of fixed effects for districts in each decade. Standard errors are clustered at the county level.[13]

[12] We test other distances from the victim's household in analysis that we present in Appendix Figure SI-1.

[13] Clustering standard errors on county allows for non-independence of enumeration districts within a county. An alternative would be to cluster standard errors on enumeration district. This would treat enumeration districts within a county as independent from one another and seems a strictly worse approach.

Completed Lynchings vs. Matched Placebos

Our second identification strategy uses nearest-neighbor propensity score matching. Specifically, we match completed lynching victims to five Black individuals in counties where we observe no lynching as having occurred that decade. The variables we use to match lynching victims to placebo victims are motivated by the individual-level characteristics that Bailey and Tolnay (2015) identified as correlates of being victimized. We match on sex, age (with a quadratic in age), household head status, literacy status, occupational score, household type, and school attendance. We also match on enumeration district-level measures of the Black population share, farm employment share, total population, and urbanization. Those individuals then serve as "placebo" victims. In exactly the way that we do for actual victims, we can identify the placebo victims' locations (enumeration district and household serial number) and then identify children in close proximity to them (same enumeration district, within 10 households).

We then estimate a variation of Equation 1 that restricts the sample to Black individuals who grew up in enumeration districts where either a completed lynching occurred or where one of our placebo victims is located. The specification is estimated in exactly the manner described above, except that $CompletedLynch_{e,t}$ is now a dummy that identifies whether the enumeration district is the site of a completed lynching or a *placebo* lynching victim.

Averted vs. Alternate Matched Placebos

Finally, we can use a similar approach to empirically assess the impact of childhood exposure to a nearby *averted* lynching. This question is of independent interest but also sheds light on whether averted lynchings serve as an appropriate comparison group for completed lynchings. We implement the same matching procedure to find matched placebos for the averted lynchings; this is a different set of matched placebos, as the characteristics of averted

lynching victims differ from those of completed lynching victims. Otherwise, the procedure and regression are identical. The main variable of interest is the interaction of a dummy for "Averted Lynching" (vs. Placebo) and "within 10 households". The results of this analysis support the appropriateness of using averted lynchings as a control group.

Results

While the trauma that exposure to a lynching imposed on the Black community was almost certainly widely felt (Rogers, 2000), impacts would likely have been greatest for those who were most closely associated with the victim(s). Thus, as described above, we focus on "treated" individuals who lived within 10 households (as enumerated in the Census) to an individual lynching victim—but did not live in the victim's household. Living in close proximity to an averted lynching could also be a source of racial trauma, though we reason that impacts are likely to be greatest and longest-lasting for exposure to completed lynchings.

For identification, as a first difference, we compare the results for these proximate individuals with those who lived more than 10 households away. In order to control for potentially systemic differences in the immediate locations where lynching victims lived relative to more distant neighborhood locations, following Equation 1, we set up two separate comparison sets that yield two separate difference-in-differences estimation strategies. As a first point of comparison, we use victims of averted lynchings. The essential identifying assumption is that the selection processes underlying systemic differences in the actual location of a lynching victim within a given Census Enumeration District are also at play in the case of an averted lynching. Our second comparison set utilizes a matching algorithm to identify a set of "placebo" lynching victims whose observed characteristics match those of the victims. It is important to note that both of these strategies are identifying a differential effect on individuals in a lynching victim's community who lived very close to that individual. In many ways, one would expect these atrocities to have "treated" the entire community. Any

Table 3: Labor Market Impacts of Black Childhood Exposure to Nearby Lynching, Measured at Ages 25-40

Panel A: Completed vs. Averted Lynchings

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	ln(Occ Score)	Ag. Occ.	Wht. Coll. Occ.	Bl. Coll. Occ.	Labor Occ.	No Occ.
Lynch X Neighbor (10 HH)	-0.030*	0.024	-0.018**	-0.005	-0.001	0.020*
	(0.017)	(0.021)	(0.008)	(0.012)	(0.021)	(0.012)
Neighbor (10 HH)	0.016	-0.018	0.013**	-0.007	0.011	-0.008
	(0.013)	(0.016)	(0.007)	(0.009)	(0.016)	(0.008)
Observations	44,745	44,745	44,745	44,745	44,745	51,209
R-squared	0.286	0.285	0.056	0.101	0.162	0.198

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel B: Completed Lynchings vs. Matched Control Group

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	ln(Occ Score)	Ag. Occ.	Wht. Coll. Occ.	Bl. Coll. Occ.	Labor Occ.	No Occ.
Lynch X Neighbor (10 HH)	-0.022*	-0.000	-0.011**	-0.007	0.018	0.008
	(0.013)	(0.015)	(0.005)	(0.009)	(0.016)	(0.010)
Neighbor (10 HH)	0.007	0.006	0.006**	-0.005	-0.007	0.004
	(0.006)	(0.007)	(0.003)	(0.005)	(0.008)	(0.005)
Observations	93,694	93,688	93,688	93,688	93,688	108,936
R-squared	0.281	0.275	0.056	0.095	0.155	0.223

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Includes controls or fixed effects for: early-life enumeration district, early-life Census year, adult Census year fully interacted with birthyear, sex, early life farm status, adult state of residence, and log occupational score for early-life head-of-household. Standard errors clustered at county-level.

such community-wide impacts would be in addition to the effects that we estimate here. As such, the estimates reported here are best considered as lower-bound estimates.

Adult Outcomes

Results from analysis of adult labor market outcomes using our two approaches are presented in Panels A and B of Table 3. Here, we evaluate the differential impact of Black childhood exposure to lynchings on labor market outcomes later in life (between the ages of 25 and 40). Labor market outcomes are measured based on the first census available in which we observe an individual at an age greater than 25 years (we drop observations for individuals that we do not observe between the ages of 25 and 40). Column 1 assesses the impact of exposure on occupational attainment as measured by the log of occupational income score. The results are consistent across both identification strategies. They suggest that children

living immediately proximate to a lynching victim have occupations in their prime earning years that yield incomes 2 to 3 percent lower than those of individuals who grew up in the same neighborhood, but lived further from the victim. Columns 2 through 6 evaluate an individual's propensity to be in different types of occupation (agriculture, white collar, blue collar, laborer, no occupation). These results suggest that our occupational income score results are driven by the fact that childhood exposure to lynching lowers the probability of being in a white collar occupation.[14]

Importantly, our argument is that, in this context, there was, critically, a *racial* aspect to the violence that is consequential for outcomes. An observable implication of the argument is that Black children should exhibit effects of the racial trauma relative to appropriate counterfactual comparisons, but that white children exposed to the violence should not be similarly affected. We test this part of the argument by replicating the main analysis reported in Table 3, following Equation 1, with the crucial difference being that we estimate effects of geographic proximity to a lynching victim for *white* children, rather than black. We construct two sets of counterfactuals as before, and estimate economic effects on individuals when they are between 25-40 years of age. Results are presented in Table 4.

We see that there are no discernible later-in-life economic effects on white children resulting from living in proximity to a black victim of a lynching, using either counterfactual comparison set. This is consistent with our argument that not only was the violence understood as racially targeted, but this quality of the violence is critical for understanding how it affected people: the trauma it induced in bystanders was not universal in nature, but similarly racially targeted.[15]

[14]Appendix Figure SI-1 reports coefficient estimates for the model of Table 3 Column 1 for a variety of distance specifications. The results suggest that effects are concentrated within the 10 Household threshold that we adopt.

[15]For robustness, Appendix Table SI-3 reports analysis that matches specifications just discussed, but omits households 10-125 households away from the victim's (or control) household. That allows for a starker comparison between nearby and farther households. 125 households is chosen because that is the median distance in our estimation sample. Results are generally similar.

Table 4: Labor Market Impacts of White Childhood Exposure to Nearby Lynching, Measured at Ages 25-40

Panel A: Completed vs. Averted Lynchings

VARIABLES	(1) ln(Occ Score)	(2) Ag. Occ.	(3) Wht. Coll. Occ.	(4) Bl. Coll. Occ.	(5) Labor Occ.	(6) No Occ.
Lynch X Neighbor (10 HH)	0.003	-0.016	-0.004	0.009	0.014	0.007
	(0.022)	(0.021)	(0.021)	(0.018)	(0.014)	(0.010)
Neighbor (10 HH)	0.014	-0.005	0.004	0.001	-0.001	-0.002
	(0.014)	(0.014)	(0.014)	(0.013)	(0.010)	(0.008)
Observations	56,382	56,381	56,381	56,381	56,381	92,442
R-squared	0.193	0.260	0.182	0.097	0.042	0.595

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel B: Completed Lynchings vs. Matched Control Group

VARIABLES	(1) ln(Occ Score)	(2) Ag. Occ.	(3) Wht. Coll. Occ.	(4) Bl. Coll. Occ.	(5) Labor Occ.	(6) No Occ.
Lynch X Neighbor (10 HH)	0.008	-0.016	-0.008	0.008	0.016	0.004
	(0.018)	(0.018)	(0.017)	(0.014)	(0.010)	(0.008)
Neighbor (10 HH)	0.008	-0.005	0.008	0.001	-0.003	0.000
	(0.007)	(0.008)	(0.006)	(0.006)	(0.004)	(0.003)
Observations	160,523	160,522	160,522	160,522	160,522	268,051
R-squared	0.190	0.264	0.180	0.102	0.042	0.617

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Includes controls or fixed effects for: early-life enumeration district, early-life Census year, adult Census year fully interacted with birthyear, sex, early life farm status, adult state of residence, and log occupational score for early-life head-of-household. Standard errors clustered at county-level.

To better understand our main results, Table 5 examines two dimensions of treatment effect heterogeneity.[16] First, we split the sample by age at the time of exposure to the lynching, with results reported in Columns (1) and (2). Although point estimates are negative for both subsamples, the effects appear concentrated among older children between the ages of 12 and 18. This pattern is consistent with research suggesting that trauma experienced during pre-teen and adolescent years can have particularly strong and lasting psychological effects,[17] particularly a model in which exposure to lynching disrupted human capital accumulation in ways that diminished gradually over time. The age gradient is also consistent with the findings of Stelzner and Darity (2026), who argue that lynching functioned as a form of economic coercion that compelled southern Black workers into sharecropping—particularly if Black teenagers served as marginal laborers on these farms. Finally, an alternative interpretation is that witnessing a lynching altered risk preferences or aspirational beliefs, inducing persistent shifts in occupational choice. Under this mechanism, as the salience of the shock waned over time, its effect on outcomes would similarly diminish—a pattern we observe in the data.

Next, Columns (3) and (4) of the Table further speak to the salience of exposure to a lynching for the children in our sample. Here, we divide the sample by the age of the victim (over or under the age of 18). We find that later life outcomes are concentrated in cases where the lynching victim was himself/herself a child. In these cases, the estimated effect is much larger, with "treated" individuals having occupations in their prime earning years that yield incomes on the order of 6 percent lower than their "untreated" counterparts.

[16] Due to technical issues related to defining the "placebo" sample, we currently only include results for the averted lynching model.

[17] Research suggests that different regions of the brain are most sensitive to trauma during different time windows, including pre-teen (Pechtel et al., 2014) and teen years for some brain regions (Teicher et al., 2016, 658), though this literature has not identified definitively which windows of childhood and adolescence are most sensitive to specific types of trauma (Dunn et al., 2017). The especially strong effects of trauma experienced in adolescence for outcomes like PTSD have been documented, for example, in the case of sexual abuse (Schoedl et al., 2010): a study found the greatest effects on frontal cortex development for those victimized by sexual abuse between the ages 14 and 16 (Andersen et al., 2008).

Table 5: Labor Market Impacts of Black Childhood Exposure to Nearby Lynching, Measured at Ages 25-40, Heterogeneity

VARIABLES	(1) ln(Occ Score)	(2) ln(Occ Score)	(3) ln(Occ Score)	(4) ln(Occ Score)
Lynch X Neighbor (10 HH)	-0.017	-0.038*	-0.061**	-0.029
	(0.027)	(0.021)	(0.029)	(0.020)
Neighbor (10 HH)	0.003	0.022	0.030	0.013
	(0.020)	(0.016)	(0.019)	(0.016)
Observations	19,558	25,142	10,238	35,080
R-squared	0.303	0.295	0.293	0.287
Age at Lynch	0-11	12-18		
Victim			Child	Adult

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Includes controls or fixed effects for: early-life enumeration district, early-life Census year, adult Census year fully interacted with birthyear, sex, early life farm status, adult state of residence, and log occupational score for early-life head-of-household. Standard errors clustered at county-level.

One potential mechanism to explain our observed changes in occupational outcomes is differential opportunities arising from systemic differences in migration decisions. This potential is particularly relevant given the finding of Gabriel et al. (2023) that family members of lynching victims migrate at differentially high rates, and there is some anecdotal evidence of this in interviews (Briggs and Montgomery, 2009). To evaluate this potential channel, Table 6 repeats our main analysis, taking locational/migration-related variables as outcomes. The results suggest that there is no such migration response. Across both specifications, there is no statistical difference between our "treated" and "untreated" groups in terms of leaving the state, leaving the region, staying in the south, moving to an urban area or living on a farm.

Using the location of individuals who experienced an averted lynching to identify a control set raises the concern that exposure to an averted lynching might in and of itself be a form of treatment. If so, our models that incorporate averted lynchings will yield biased results. To test for this concern, in Appendix Tables SI-4 and SI-5, we replicate the matched placebo approach taken in Panel B of Tables 3 and 6 to test for a treatment effect associated with

Table 6: Migration Responses to Black Childhood Exposure to Nearby Lynching, by Ages 25-40

Panel A: Completed vs. Averted Lynchings

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Moved State	Moved Region	Lives in South	Urban Area	Farm
Lynch X Neighbor (10 HH)	0.002	-0.003	-0.007	-0.022	0.018
	(0.018)	(0.015)	(0.012)	(0.014)	(0.018)
Neighbor (10 HH)	-0.001	0.000	0.006	0.017	-0.012
	(0.012)	(0.011)	(0.009)	(0.011)	(0.014)
Observations	51,213	51,213	51,213	51,209	51,209
R-squared	0.111	0.102	0.127	0.359	0.242

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel B: Completed Lynchings vs. Matched Control Group

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Moved State	Moved Region	Lives in South	Urban Area	Farm
Lynch X Neighbor (10 HH)	0.008	0.005	-0.002	0.015	0.003
	(0.013)	(0.012)	(0.010)	(0.011)	(0.014)
Neighbor (10 HH)	-0.006	-0.007	0.001	-0.019***	0.002
	(0.006)	(0.005)	(0.004)	(0.006)	(0.007)
Observations	108,940	108,940	108,940	108,936	108,936
R-squared	0.100	0.094	0.118	0.342	0.234

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Includes controls or fixed effects for: early-life enumeration district, early-life Census year, adult Census year fully interacted with birthyear, sex, early life farm status, and log occupational score for early-life head-of-household. Standard errors clustered at county-level.

averted lynchings. We find no evidence of any averted lynching treatment effect on either labor market outcomes or migration decisions.

Finally, somewhat related to the prior test, we note that an important potential confound in our analysis is the broad presence of Jim Crow institutions, which – in general – shape both the presence and frequency of lynchings, but also early life conditions for Black children in the time period. While such a general relationship certainly exists, our design largely rules this out as an explanation for our results. That is because of the hyper-local nature of our treatment and control status. We compare individuals close to a lynching victim (within 10 households) to other individuals in the same enumeration district (and therefore the same county) but more than 10 households away. Jim Crow Institutions, and their across time evolution, would be held constant within this small geographic area.[18] Nonetheless, in Appendix Table SI-6 we test whether there are different effects in our main specifications for people who remained in the same county vs. those who did not. We reason this is a way to further probe whether the mechanism is parental transmission (effects should appear for both subsamples) or local institutions (effects should appear *only* for stayers).[19] The results become imprecise, but the key is that there is no difference across movers and stayers. This adds some support to a parental transmission mechanism.

Intergenerational Effects

Central to our understanding of the persistence of racial disparity is the intergenerational persistence of these disparities. Given that exposure to a lynching in childhood impacts incomes during peak child-bearing years, we might expect to find such effects. We test for these in Table 7. Results under the averted lynching model are reported in the first

[18]Note that this holds for other county-level correlates of racial violence during this period, such as the presence or racial composition of federal troops deployed in the post-Reconstruction period (Byun and Kwon, 2026; Frieden, Grossman and Lowery, 2026).

[19]We thank Jhacova Williams for suggesting this test.

two columns and results from the matched placebo model are reported in the third and fourth columns. Because all of the children in the sample are observed in the 1940 Census (the first year in which the Census reported income), we use the log of actual income as our dependent variable as opposed to the Occupational Income Scores used in the previous regressions. Our baseline estimates (Column 1) suggest a large intergenerational impact from lynching exposure. We estimate an income penalty for the children of our treated sample on the order of 11 to 13 percent. In Column 2 we add controls for the child's educational attainment to the model. If these estimated income penalties were, at least in part, driven by lower levels of educational attainment, we would expect to see attenuated coefficients in these models. This is not the case, suggesting that differential educational attainment is not the mechanism underlying our intergenerational results.

Finally, in our analysis of adult outcomes for those who were exposed to lynchings as a child, we find evidence that lynching exposure led to differences in both occupational sorting and labor force attachment. We do not find evidence of occupational sorting in the second generation (see Appendix Table SI-7). In fact, when we explicitly control for occupation in Column 3 of Table 7, our coefficient estimates actually increase slightly in magnitude. Instead, as we show in Columns 4 and 5, these income results appear to be completely driven by increased levels of unemployment, with the children of treated individuals spending, on average, roughly three and a half additional weeks out of work each year.

These effects on the next generation are considerably larger in magnitude relative to those estimated for the first generation in Table 3. We note this is due, in part, to variation in the distributions of the outcome variables. As Table 2 shows, the standard deviation of the dependent variable (ln(IncWage)) in the intergenerational sample is about two times that of the standard deviation of the dependent variable (ln(OccScore)) in the first generation sample. That is, part of the explanation for the larger coefficient in the intergenerational

Table 7: Intergenerational Effects – Adult Children (Observed in 1940) of Black People Who Were Exposed To Nearby Lynching As Children

Panel A: Completed vs. Averted Lynchings

	(1)	(2)	(3)	(4)	(5)
VARIABLES	ln(Income)	ln(Income)	ln(Income)	ln(Wk. Earn.)	Wks. Work.
Par.: Lynch X Neighbor	-0.134*	-0.158**	-0.174**	-0.037	-3.458**
	(0.079)	(0.077)	(0.076)	(0.057)	(1.360)
Par.: Neighbor (10 HH)	-0.007	0.017	0.008	0.005	0.436
	(0.052)	(0.052)	(0.051)	(0.041)	(0.949)
Observations	8,745	8,745	8,717	8,596	8,717
R-squared	0.379	0.400	0.465	0.550	0.160
Edu. FE's	No	Yes	Yes	Yes	Yes
Occ. FE's	No	No	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel B: Completed Lynchings vs. Matched Control Group

	(1)	(2)	(3)	(4)	(5)
VARIABLES	ln(Income)	ln(Income)	ln(Income)	ln(Wk. Earn.)	Wks. Work.
Par.: Lynch X Neighbor	-0.111*	-0.095	-0.120**	0.013	-3.380***
	(0.064)	(0.062)	(0.061)	(0.044)	(1.108)
Par.: Neighbor (10 HH)	-0.024	-0.028	-0.022	-0.034	0.649
	(0.028)	(0.028)	(0.026)	(0.022)	(0.498)
Observations	19,617	19,617	19,582	19,310	19,582
R-squared	0.343	0.364	0.428	0.528	0.143
Edu. FE's	No	Yes	Yes	Yes	Yes
Occ. FE's	No	No	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Outcome variable and final sample are from 1940 Census. Earlier Censuses used to identify earlier generation exposure. Includes controls or fixed effects for: parent's early-life county (and therefore lynching location), parent's early-life Census year (and therefore lynching decade), parent's adult Census year, sex, farm status, state in childhood, state in 1940, and log occupational score for early-life head-of-household. Columns 1-3 take log of past year's income as outcome. Column 4 takes log of weekly earnings as outcome, which is calculated by dividing year's income by weeks worked. Column 5 takes weeks worked as the outcome. Standard errors clustered at county-level.

analysis is that there is simply more dispersion in that outcome.[20] Part of the reason for that is that there is more variation in occupations held by Black workers in 1940 than in the earlier decades. The three most commonly held occupations in our first generation results are: "farmers (owners and tenants)", "laborers (not elsewhere classified)", and "farm laborers, wage workers". Collectively these three occupations account for 70% of our sample. In the intergenerational sample, the three most commonly held occupations are "laborers (not elsewhere classified)", "farm laborers, wage workers", and "private household workers (not elsewhere classified)", but collectively these account for only 50% of the sample. Also, OccScore accounts only for variation in earnings across occupations, but not variation within occupation.

Beyond this point, the larger effects observed in the next generation could be the result of compounding over time. We have already documented a negative economic impact of childhood exposure to lynching on the first generation. Intergenerational transmission of income/wealth alone would reproduce that to some degree for the next generation; if the mechanism driving second generation results also includes transmission of trauma, these dual effects may produce a larger effect than the first generation experienced.

In considering the broader intergenerational implications, we estimate that approximately 700,000 Black men and women belong to the cohort of second-generation individuals whose parent, as a child, resided in close proximity to a lynching victim. Combining the coefficient estimate from Column 2 of Table 7 with 1940 income data from the U.S. Census indicates that the aggregate value of lost wages for these individuals amounts to roughly $43 billion in 2025 dollars. These losses, moreover, represent only the direct intergenerational effects and do not account for the additional economic and social damages likely borne by the broader affected communities.[21]

[20] In coefficient terms, the intergenerational coefficient looks four times larger, but in standard deviation terms, it is more like two times larger.

[21] See Appendix Table SI-8 for details on these calculations.

To put this calculation in a broader context, consider two other estimates of economic loss due to racial discrimination. First, during the course of the twentieth century, the amount of agricultural land owned by Black citizens declined by roughly 90%. Francis et al. (2022) estimate the compounded value of Black-owned agricultural land lost between 1920 and 1997 at roughly $326 billion, which is almost as much as the combined market capitalization of Ford Motor Company, Starbucks, and Target in 2022 (Francis et al., 2022, 41). Second, in their seminal study of American racial inequality and its implications for reparations policy, Darity and Mullen (2022, xi-xii) estimate a racial wealth disparity between Black and White US citizens as a way to capture the economic impact of systemic discrimination against Black individuals across generations. They use this disparity as the basis for calculating that the cost of paying reparations to each of the approximately 40 million Black descendants of the 4 million persons emancipated in 1865 would equate to at least $14 trillion.

Discussion and Evidence on Mechanisms

We identify two principal mechanisms linking lynching to economic outcomes: violence as a driver of racial trauma and violence as a tool of economic control. We do not view these as mutually exclusive — both may operate simultaneously, and our primary contribution is establishing that early-life lynching exposure generates causal effects on later-life and intergenerational outcomes rather than isolating a single pathway. In what follows, we outline the theoretical logic of each mechanism and illustrate it with qualitative evidence drawn from four interview sources: Dr. Angela Sims' interviews of Black Americans, archived at the Institute for Oral History at Baylor University (Sims, 2016); Dr. Kim Lacy Rogers' interviews of residents of the Mississippi Delta (Rogers, 2006); Dr. Shytierra Gaston's interviews of descendants of lynching victims (Gaston, 2021); and the Federal Writers' Project Slave Narratives (Federal Writers' Project, 1941).

Table 8: Summary of Oral History Collections Analyzed

	Includes Discussion of		
Collection	Economic Coercion	Racial Trauma	Total Articles Analyzed
Baylor Institute	17	63	69
Slave Narratives	64	275	1,614

Note: Categories are not mutually exclusive; a single narrative may include discussion of both economic threat and racial trauma. 'Remembering Lynching' oral histories are from the Sims (2016) project. 'Slave Narratives' oral histories are from the collection published as Born in Slavery: Slave Narratives from the Federal Writers' Project, 1936-1938.

Two of these sources — Federal Writers' Project (1941) and Sims (2016) — are sufficiently large and available in digital form to permit systematic analysis using Natural Language Processing (NLP). We use NLP to tally the number of interviews in each source that connect lynching to economic coercion or racial trauma.[22] The results, presented in Table 8, indicate that both mechanisms receive substantial attention across sources. Mentions are less frequent in the Slave Narratives, which is expected: although we restrict our analysis to passages describing post-slavery events, these narratives are primarily focused on the experience of enslavement itself. Both racial trauma and economic coercion emerge as prevalent themes, with the former appearing somewhat more frequently. We now examine each mechanism in turn.

Violence as Racial Trauma

Oral histories of Black Americans provide extensive evidence that exposure to lynching — whether direct or indirect — induced profound and lasting fear. Dr. Dorsey, who grew up in Depression-era Mississippi, reflected on the dread that the "mob crew" evoked in her community:

> The violence was very clearly the mob crew, and it was identified as the mob crew. And the feeling that it evoked that I can remember from my earliest

[22]See Appendix Section A for details on the Large Language Model approach used for this task.

> memories is fear. There was a tremendous amount of fear in the community and in almost every house of this faceless group of people who arrived at your house at night, on horses and in cars, when cars got available and plentiful, to drag you out and kill you for any little infraction of rules that you didn't always know about. People worried tremendously about their sons and their menfolk in their families... What you remember about it was fear, that there was no way to be protected... I understood the fear so strongly that it wouldn't let them talk out loud. (Rogers, 2006, 37)

A key mechanism through which such trauma could generate intergenerational effects is the transmission — and suppression — of knowledge about violence across generations. Research on collective trauma suggests that "the damage inflicted on traumatized populations can persist among generations seemingly free from the originating terrors" (Rogers, 2006, 152). Strikingly, this transmission often operated through silence rather than speech. As Gaston (2021, 80) reports from interviews with descendants of lynching victims, "The most common theme related to psychological impacts was the code of silence and avoidance surrounding the lynching incident in families, a common response and coping mechanism for managing and surviving trauma. All but one descendant reported that their families avoided discussing their victimized relative and/or lynching, resulting in recent generations having minimal knowledge about the victim or the circumstances." Fear was central to this silence. Addie Bolton from Louisiana explained: "One of the reasons I believe that people don't talk about [lynchings] a lot has to do with the fear — people are afraid to talk about those things even in the twenty-first century because there are still consequences that could follow your words. There are still consequences — many of them that we are not privy to — but you don't always know. So because people try to be extra-special careful, then they are reluctant to say too much about it" (Bolton and et al., 2009).

Where accounts of lynchings *were* passed down, that transmission came freighted with

fear. Ms. Jones, the daughter of an interviewee, recalled: "we have my aunts that are not living now. They, you know, when they would get together, we could overhear, you know, those stories, and at that time, as a child, you heard it, but you wasn't experiencing it. But you could understand the fear" (Walker, 2010). In the same interview, Ms. Jones' mother recounted how her father instructed her brothers: "Whatever you do, when you see white girls or white women or anything, don't speak to them, don't look at them or nothing, because they will say that you said something, did something, and when you know anything, a mob will be here and you'll get lynched. That's just the law. Negroes don't tarry around white women" (Walker, 2010).

The results in Table 5 offer evidence consistent with this trauma mechanism. Labor market effects of childhood exposure to a nearby lynching are particularly pronounced when the victim was also a child, consistent with the argument that lethal violence directed at children generated especially acute trauma among other children in the community. L.P. Lewis, born in Louisiana in 1931, reflected on his earliest memory of lynching: "when I was, I guess, around six years old our neighbor's sister's son was ultimately lynched, and — first of all, that was an experience that I'll never be able to get out of my mind. The trauma that that involves surrounded this whole event" (Hill and Lewis, 2010). Mrs. Fletcher described the effects of the lynching of her fifth-grade classmate in St. Joseph, Missouri: "It affected me in a horrible way. I was so full of anger and hate... I hated every white face I saw. Oh, I was so full of anger and hate. Unfortunately, I didn't talk to anybody about this. I think it would have helped if I had talked about it. But after a while, I kind of buried this... So I think I just buried this hate and anger for a number of years, but I really didn't get rid of it until many, many years later" (Fletcher, 2009).

Violence as Economic Control

A second prominent channel is the use of violence as a means of economic control. Beck and Tolnay (1992) and Stelzner and Darity (2026) provide quantitative evidence linking lynchings to economic outcomes, particularly in the context of agricultural labor. Stelzner and Darity (2026) find that a white-on-Black murder within a county led to a significant increase in hours worked and in the share of family members — including children — working among Black sharecroppers, with no analogous effect among Black agricultural workers with greater economic autonomy. Beck and Tolnay (1992) conclude that the latent function of lynchings was "to tighten the reins of control over the black population, especially during times when whites most needed black labor to work fields of cotton or tobacco."

We do not have the ideal data to test whether this precise mechanism operates in our setting. We cannot, for instance, distinguish sharecroppers from other agricultural workers within our sample. We nonetheless test two observable implications. First, we examine whether our main results differ by whether a child's parents worked in agriculture versus another industry; we find no such heterogeneity (Appendix Table SI-9). Second, we test whether exposure to a nearby lynching in early childhood predicts lower school attendance or higher rates of child labor in later childhood, for children of agricultural and non-agricultural workers alike; we find no response on either margin.[23] A likely explanation for these null results is the highly local geographic scope of our study: if economic coercion effects operate at the county level, children in the same enumeration district but at varying distances from the victim would be similarly exposed, limiting our ability to detect them. We therefore do not claim that this channel is inoperative in our setting.

The interview evidence suggests that economic control extended well beyond the sharecropping context emphasized in prior work.[24] Thomas (2009) was explicit about how the

[23]Results are reported in Appendix Table SI-10.

[24]Rogers (2006, 20) writes: "I argue that many, if not a majority, of sharecropping and tenant farming

threat of violence constrained Black workers' mobility and opportunity: "So a lot of times they were forced — we were forced to take the jobs, forced to keep jobs because you belonged to the white man, practically. And if you leave, he'd kill you out there" and "Back in them days, if you worked for a white company, a white man, if you quit — if he fired you — he blackballed you. When you went to another job, he'd kill you." These accounts are consistent with our finding that nearby lynching reduced the likelihood of subsequent white-collar employment among Black workers. Mordessa Corbin situated the mechanism in terms of fear that outlasted the era of physical violence: "The culture of fear still exists — that's just the bottom line... We would rather ignore it and sweep it under the rug than talk about it. A lot has to do with the fear and the fear is not necessarily a physical kind, even though that does exist to a degree — you have a fear of retaliation by economic means. The suppression is still there and now, instead of using the physical, we use the economic sweep and it can be so effectively used on both white and black until it just makes it uncomfortable" (Bolton and et al., 2009). The daughter of another interviewee connected this fear directly to land tenure, recalling that her mother's family suppressed discussion of violence out of fear that "if something went wrong, they would get throwed off of —" [Interviewer: "The land."] "Off of the land, because of things that a kid could do, or that what was going on in their little community... it was word of mouth all about the bad things that were going to happen, and those things that, that they had a fear of" (Walker, 2010).

Conclusion

This paper documents the long-run consequences of childhood exposure to racial terror. Linking lynching records to individual-level Census data for the American South between 1880 and 1919, we show that Black children who lived in close proximity to a lynching

families lived in conditions of chronic social trauma and structural violence that compounded widespread economic exploitation and poverty with recurring vigilante, Klan, and state-sanctioned authorities' assaults on individuals and groups of African Americans."

victim suffered significant and enduring economic harm. In adulthood, these individuals were less likely to hold white-collar occupations and earned substantially lower occupational income scores during their prime working years. These effects hold when we compare completed lynchings to narrowly averted lynchings and to carefully matched counterfactuals, strengthening the causal interpretation. The findings are consistent with a growing body of interview-based evidence documenting the psychological, behavioral, and economic harms borne by those even indirectly exposed to racial violence (Gaston, 2021).

The consequences of racial terror extended across generations. Children of individuals who were themselves exposed to lynchings as minors earned 10 to 13 percent less as adults, even decades later. These intergenerational penalties are not explained by differences in educational attainment, pointing to deeper transmission mechanisms — psychosocial, behavioral, or embedded in the norms and expectations families carried across generations. Our results contribute to a growing literature on the persistence of historical trauma and its role in reproducing economic inequality.

The implications extend beyond the specific context of Southern lynching. Our design focuses on children living within 10 households of a victim — a deliberately narrow geographic window — yet the effects are large, precisely estimated, and intergenerationally persistent. This underscores a point that is easily lost in county- or state-level analyses of historical violence: trauma was not confined to victims' families, but radiated across neighborhoods and communities. In this sense, our estimates are likely a lower bound on the aggregate costs of racial terror. Accounting for the full spatial and temporal reach of this violence would only deepen the reckoning.

This study joins a wider research program on the economic and political legacies of historical conflict, repression, and collective violence. The tools of economics — causal identification, linked administrative records, intergenerational panel methods — prove well-suited to recovering the long shadow of atrocities that generated no contemporaneous legal

record and left no official accounting of harm. By documenting how lynching reverberated through Black communities across decades and generations, our findings contribute to an understanding of the deep historical roots of the racial disparities that persist in the United States today.

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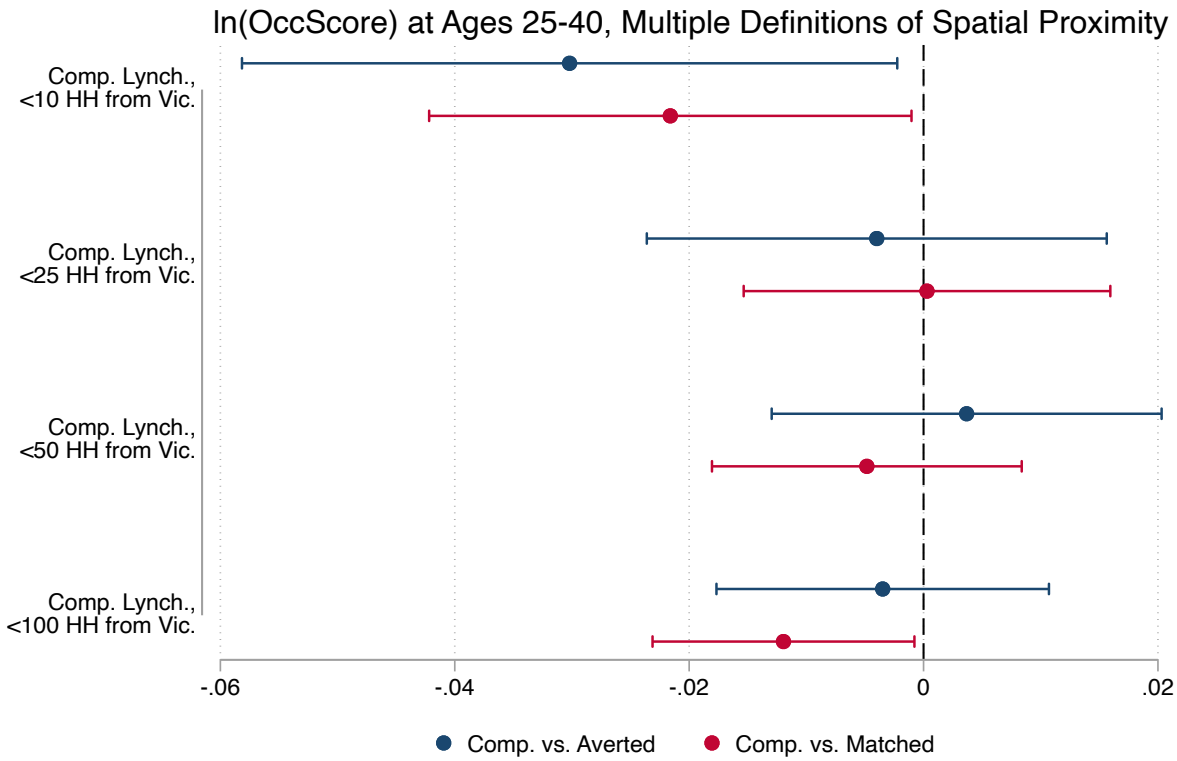
SUPPORTING INFORMATION
— For Online Publication Only —

Supplemental Figures

Supplemental Tables

Figure SI-1: ln(OccScore) at Ages 25-40, Multiple Definitions of Spatial Proximity



Notes: Includes controls or fixed effects for: early-life enumeration district, early-life Census year, adult Census year fully interacted with birthyear, sex, early life farm status, adult state of residence, and log occupational score for early-life head-of-household. Standard errors clustered at county-level. 95 percent confidence intervals are plotted.

Table SI-1: Victim-level Predictors of Completed vs. Averted Lynchings

VARIABLES	(1) Completed lynching	(2) Completed lynching	(3) Completed lynching
Black share (ED)	0.055	0.055	-0.069
	(0.068)	(0.069)	(0.072)
Total pop. (ED)	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Urban share (ED)	-0.225***	-0.223***	-0.215***
	(0.055)	(0.054)	(0.055)
Female	0.159*	0.158*	0.171**
	(0.084)	(0.085)	(0.072)
Farm residence	0.060*	0.062**	0.050
	(0.031)	(0.031)	(0.030)
Child	-0.015	-0.019	-0.012
	(0.028)	(0.028)	(0.027)
HH occ. score	-0.003	-0.003	-0.003
	(0.002)	(0.002)	(0.002)
HH literate	0.007	0.008	0.007
	(0.010)	(0.011)	(0.011)
Observations	1,267	1,267	1,267
R-squared	0.033	0.034	0.083
FE's	None	Decade	Dec. + State

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Standard errors clustered at county-level. Predictor variables marked with "ED" are enumeration district level characteristics. All others are individual level characteristics of the victim. "HH occ. score" and "HH literate" represent characteristics of victim's household head.

Table SI-2: Predictors of Successful Match of Youth to Adult Census

VARIABLES	(1) Matched	(2) Matched	(3) Matched	(4) Matched
Lynch X Neighbor (10 HH)	-0.004	-0.006	0.001	-0.001
	(0.007)	(0.007)	(0.006)	(0.006)
Neighbor (10 HH)	0.006	0.007	0.002	0.002
	(0.005)	(0.005)	(0.003)	(0.003)
Any Lynch in ED	0.004		-0.006	
	(0.004)		(0.004)	
ln(HH Occscore)	0.017***	0.020***	0.013***	0.015***
	(0.003)	(0.002)	(0.002)	(0.002)
HH Not Lit.	-0.020***	-0.020***	-0.022***	-0.022***
	(0.002)	(0.002)	(0.001)	(0.001)
Lives on farm	0.024***	0.028***	0.025***	0.028***
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	337,056	337,056	687,708	687,706
R-squared	0.008	0.012	0.007	0.012
Geo FE	County	ED	County	ED
Control Grp.	Averted	Averted	Placebo	Placebo

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Standard errors clustered at county-level. Beyond what is listed in table, includes fixed effects for early life decade of observation. Outcome "Matched" equals 1 if the childhood observation that forms this sample has been successfully linked to a later Census, to identify adult outcomes in our main analysis.

Table SI-3: Replicating Main Results, Omitting Households More than 10 but less than 125 From Focal Household

Panel A: Completed vs. Averted Lynchings

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	ln(Occ Score)	Ag. Occ.	Wht. Coll. Occ.	Bl. Coll. Occ.	Labor Occ.	No Occ.
Lynch X Neighbor (10 HH)	-0.035*	0.010	-0.018**	-0.002	0.010	0.026*
	(0.019)	(0.023)	(0.009)	(0.014)	(0.023)	(0.014)
Neighbor (10 HH)	0.020	-0.007	0.013*	-0.008	0.002	-0.008
	(0.014)	(0.017)	(0.007)	(0.010)	(0.017)	(0.009)
Observations	24,886	24,886	24,886	24,886	24,886	28,381
R-squared	0.293	0.294	0.073	0.121	0.174	0.194

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel B: Completed Lynchings vs. Matched Control Group

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	ln(Occ Score)	Ag. Occ.	Wht. Coll. Occ.	Bl. Coll. Occ.	Labor Occ.	No Occ.
Lynch X Neighbor (10 HH)	-0.020	-0.009	-0.012**	-0.008	0.029	0.011
	(0.014)	(0.018)	(0.006)	(0.011)	(0.018)	(0.011)
Neighbor (10 HH)	0.004	0.011	0.006*	-0.001	-0.016*	0.007
	(0.007)	(0.009)	(0.003)	(0.006)	(0.009)	(0.005)
Observations	50,452	50,449	50,449	50,449	50,449	58,536
R-squared	0.289	0.283	0.072	0.111	0.170	0.222

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Includes controls or fixed effects for: early-life enumeration district, early-life Census year, adult Census year fully interacted with birthyear, sex, early life farm status, adult state of residence, and log occupational score for early-life head-of-household. Standard errors clustered at county-level.

Table SI-4: Averted Lynchings vs. Averted Matched Placebos, Labor Outcomes at Ages 25-40

VARIABLES	(1) ln(Occ Score)	(2) Ag. Occ.	(3) Wht. Coll. Occ.	(4) Bl. Coll. Occ.	(5) Labor Occ.	(6) No Occ.
Av. Lynch X Neighbor (10 HH)	0.011	-0.019	0.011	-0.007	0.015	-0.010
	(0.015)	(0.017)	(0.007)	(0.010)	(0.018)	(0.010)
Neighbor (10 HH)	0.005	0.001	0.002	0.000	-0.004	0.002
	(0.007)	(0.008)	(0.003)	(0.006)	(0.007)	(0.005)
Observations	88,571	88,570	88,570	88,570	88,570	103,767
R-squared	0.288	0.288	0.063	0.104	0.152	0.224

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Includes controls or fixed effects for: early-life enumeration district, early-life Census year, adult Census year fully interacted with birthyear, sex, early life farm status, adult state of residence, and log occupational score for early-life head-of-household. Standard errors clustered at county-level.

Table SI-5: Averted Lynchings vs. Averted Matched Placebos, Location at Ages 25-40

VARIABLES	(1) Moved State	(2) Moved Region	(3) Lives in South	(4) Urban Area	(5) Farm
Av. Lynch X Neighbor (10 HH)	0.009	0.007	-0.000	0.020	-0.010
	(0.014)	(0.012)	(0.009)	(0.012)	(0.015)
Neighbor (10 HH)	-0.009	-0.006	0.007*	-0.003	-0.001
	(0.006)	(0.005)	(0.004)	(0.006)	(0.007)
Observations	103,770	103,770	103,770	103,767	103,767
R-squared	0.112	0.104	0.126	0.361	0.235

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Includes controls or fixed effects for: early-life enumeration district, early-life Census year, adult Census year fully interacted with birthyear, sex, early life farm status, and log occupational score for early-life head-of-household. Standard errors clustered at county-level.

Table SI-6: Replicating Main Results, Splitting by Whether an Individual Moves

VARIABLES	(1) ln(Occ Score)	(2) ln(Occ Score)	(3) ln(Occ Score)	(4) ln(Occ Score)
Lynch X Neighbor (10 HH)	-0.014	-0.030	-0.017	-0.020
	(0.030)	(0.021)	(0.024)	(0.015)
Observations	16,802	27,892	36,856	56,761
R-squared	0.302	0.303	0.290	0.299
Control Grp.	Averted	Averted	Matched	Matched
Sample	Stayers	Movers	Stayers	Movers

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Includes controls or fixed effects for: early-life enumeration district, early-life Census year, adult Census year fully interacted with birthyear, sex, early life farm status, adult state of residence, and log occupational score for early-life head-of-household. Standard errors clustered at county-level.

Table SI-7: Intergenerational Effects – Adult Children (Observed in 1940) of Black People Who Were Exposed To Nearby Lynching As Children – Occupations

Panel A: Completed vs. Averted Lynchings

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Ag. Occ.	Wht. Coll. Occ.	Bl. Coll. Occ.	Labor Occ.	No Occ.
Par.: Lynch X Neighbor	-0.055	0.016	0.015	0.025	0.000
	(0.039)	(0.018)	(0.030)	(0.040)	(0.026)
Par.: Neighbor (10 HH)	0.004	-0.008	-0.002	0.006	-0.000
	(0.032)	(0.014)	(0.023)	(0.032)	(0.021)
Observations	12,856	12,856	12,856	12,856	15,529
R-squared	0.287	0.095	0.097	0.149	0.275

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel B: Completed Lynchings vs. Matched Control Group

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Ag. Occ.	Wht. Coll. Occ.	Bl. Coll. Occ.	Labor Occ.	No Occ.
Par.: Lynch X Neighbor	-0.056**	-0.006	0.007	0.055**	-0.004
	(0.025)	(0.013)	(0.022)	(0.026)	(0.018)
Par.: Neighbor (10 HH)	0.007	0.007	0.002	-0.016	0.002
	(0.013)	(0.007)	(0.010)	(0.015)	(0.010)
Observations	28,455	28,455	28,455	28,455	34,629
R-squared	0.259	0.069	0.081	0.134	0.260

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Outcome variable and final sample are from 1940 Census. Earlier Censuses used to identify earlier generation exposure. Includes controls or fixed effects for: parent's early-life county (and therefore lynching location), parent's early-life Census year (and therefore lynching decade), parent's adult Census year, sex, farm status, state in childhood, state in 1940, and log occupational score for early-life head-of-household. Occupation categories are as defined in the text. Standard errors clustered at county-level.

Table SI-8: Constructing the Counterfactual

Variable	Value	Running Product	Source:
# of Lynchings	3,141		
# of Neighboring Households	20	62,820	By Construction
Avg. # of Children per Household living w/in 10 homes of a lynching victim)	2.39	150,139.8	Computed by authors based on full-count decennial censuses
Our Coefficient Estimate	0.143	21,469.9914	Taken from Column 2 of Table 4
Average Black Wage in 1940	$462	$9,919,136	Includes Both Men and Women and is conditional on working. Taken from 1940 Census
# of Earning Years	40	$396,765,441	By Assumption
Inflation Adjustment from 1940 to 2025 Dollars	23.02	$9,134,107,261	Based on Historical CPI: https://www.minneapolisfed.org/about-us/monetary-policy/inflation-calculator/consumer-price-index-1913-
Lifetime Average # of Children per Black Individual	4.678	$42,729,353,768	This is the number of lifetime children reported by ever-married black women between the ages of 65 and 74, as Reported in the "1940 Census of Population: Differential Fertility, 1940 and 1910"

Notes: Our counterfactual estimate of roughly $43 Billion dollars in lost wages to the children of those who lived proximate (within 10 houses in either direction) of a lynching victim is calculated by multiplying together the 8 values contained in the column "Value". All monetary values are rounded to the nearest dollar. The number of children exposed to the 14.3% reduction in income is computed as: $150,139.8 X 4.678 \approx 702,354$.

Table SI-9: Labor Market Impacts of Black Childhood Exposure to Nearby Lynching, Measured at Ages 25-40, by Childhood Household Head Occupation Category

Panel A: Completed vs. Averted Lynchings

	(1)	(2)
	HH Occ.	HH Occ.
VARIABLES	All	Ag.
Lynch X Neighbor (10 HH)	-0.030*	-0.039**
	(0.017)	(0.020)
Neighbor (10 HH)	0.016	0.023
	(0.013)	(0.015)
Observations	44,745	32,429
R-squared	0.286	0.289

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel B: Completed Lynchings vs. Matched Control Group

	(1)	(2)	(3)
	HH Occ.	HH Occ.	HH Occ.
VARIABLES	All	Ag.	Not Ag.
Lynch X Neighbor (10 HH)	-0.022*	-0.019	-0.059
	(0.013)	(0.014)	(0.041)
Neighbor (10 HH)	0.007	0.003	-0.007
	(0.006)	(0.007)	(0.017)
Observations	93,694	69,282	13,772
R-squared	0.281	0.281	0.335

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Includes controls or fixed effects for: early-life enumeration district, early-life Census year, adult Census year fully interacted with birthyear, sex, early life farm status, adult state of residence, and log occupational score for early-life head-of-household. Standard errors clustered at county-level.

Table SI-10: Childhood Impacts of Childhood Exposure (Decade d+1) to a Lynching (decade d), Using Averted Lynchings as Comparison

VARIABLES	(1) HH Occ. All In School	(2) HH Occ. Ag. In School	(3) HH Occ. Not Ag. In School	(4) HH Occ. All Working	(5) HH Occ. Ag. Working	(6) HH Occ. Not Ag. Working
Lynch X Neighbor (10 HH)	-0.002	0.001	-0.010	-0.017	-0.017	-0.009
	(0.018)	(0.021)	(0.035)	(0.018)	(0.020)	(0.036)
Neighbor (10 HH)	0.008	0.006	0.006	0.014	0.014	0.023
	(0.013)	(0.016)	(0.023)	(0.013)	(0.015)	(0.020)
Observations	72,897	55,219	17,558	72,898	55,219	17,559
R-squared	0.244	0.234	0.315	0.327	0.322	0.335

Robust standard errors in parentheses

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

Notes: Includes controls or fixed effects for: early-life enumeration district, early-life Census year, adult Census year fully interacted with birthyear, sex, early life farm status, adult state of residence, and log occupational score for early-life head-of-household. Standard errors clustered at county-level.

A Claude API System Prompt

The following system prompt was submitted to the Claude API (`claude-sonnet-4-20250514`) for all calls used to analyze the Federal Writers' Project *Slave Narratives* and the *Baylor University Institute for Oral History* interviews (hereafter "Baylor Interviews"). The prompt instructed the model to classify each transcript for two themes—Violence as Economic Control (`response_a`) and Racial Trauma (`response_b`)—and to return structured JSON output suitable for downstream analysis.

```
You are a qualitative research assistant analyzing interview transcripts
from the Federal Writers' Project (1936-1938) -- interviews with formerly
enslaved African Americans about their life experiences, with a focus on
lynching and racial terror.

CRITICAL SCOPE RESTRICTION -- POST-SLAVERY ONLY:
You must ONLY identify and extract content describing events, experiences,
or conditions that occurred AFTER the end of slavery (i.e., after
emancipation/surrender, roughly 1865 onward). This includes
Reconstruction, the post-Reconstruction era, Jim Crow, and any later
period up to the time of the interview. Ignore any violence, labor
coercion, trauma, or economic harm that occurred DURING slavery. If the
only relevant content in an interview describes events from the slavery
period, mark both response types as false.

You will receive one or more interview transcripts. For EACH interview,
extract the following:

1. interview_id -- The name of the person interviewed. Use exactly the
   name as it appears in the transcript header (e.g., 'Person
   interviewed' line). If no name is found, use 'Unknown'.

2. response_a_present -- true or false. Is there content consistent
   with 'Violence as Economic Control'?
   This includes any discussion of:
   - Lynching or racial violence used to control Black labor (e.g.,
     preventing workers from quitting, seeking better wages, or
     changing employers)
   - Blackballing, retaliation for seeking raises, or being forced to
     leave town after asserting economic independence
   - Violence or threats used to enforce staying 'in one's place' in
     the social/economic order
   - Destruction of Black businesses or property as economic
     intimidation
   - Systemic exclusion from certain occupations or professions
```

```
     enforced through threat of violence
   - Any connection between mob violence and labor market conditions
     or economic competition

3. response_a_quote -- If response_a_present is true, provide the
   single best direct quote from the interview that exemplifies this
   theme. Use the interviewee's exact words. Make sure to include the
   entire relevant quote. If false, set to null.

4. response_b_present -- true or false. Is there content consistent
   with the threat of violence leading to 'Racial Trauma'?
   This includes any discussion of:
   - Embodied fear, physical or emotional threats, and real dangers
     that led to physical illness, depression, early death, or
     frequent sickness
   - Deep, pervasive fear that 'robbed adults of speech' or enforced
     silence about racial violence
   - Terror being deliberately 'imprinted' on children -- parents or
     grandparents intentionally exposing younger generations to
     lynching sites or stories so the danger would be internalized
   - Families being 'scared to death' in response to civil rights
     activity or white retaliation
   - Somatic responses to racial terror (shaking, heart pounding,
     numbness)
   - Coping mechanisms such as faith, avoidance, or emotional numbing
     in response to lynching or racial violence
   - Trauma, silence, and the feeling that the world offered no
     sanctuary or protection
   - Loss of family members to racial violence and the lasting
     emotional impact of that loss

5. response_b_quote -- If response_b_present is true, provide the
   single best direct quote from the interview that exemplifies this
   theme. Use the interviewee's exact words. Make sure to include the
   entire relevant quote. If false, set to null.

Return your response as a JSON array of objects, one per interview:
[
  {
    "interview_id": "Silas Abbott",
    "response_a_present": false,
    "response_a_quote": null,
    "response_b_present": true,
    "response_b_quote": "..."
  }
```

```
]

IMPORTANT: Return ONLY the JSON array. No preamble, no markdown fences.
```
