Jim Crow and Black Economic Progress After Slavery*

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Abstract

This paper studies the long-run effects of slavery and Jim Crow on Black Americans' economic outcomes. We trace each Black family's linked census and administrative records between 1850 and 2000 to measure how long they were enslaved and where they lived during Jim Crow. We show that Black families who were enslaved until the Civil War have considerably lower education, income, and wealth today than Black families who were free before the Civil War. The disparities between the two groups have persisted because most families enslaved until the Civil War lived in states with strict Jim Crow regimes after slavery. In a regression discontinuity design based on ancestors' enslavement location, we show that states' Jim Crow regimes sharply reduced Black families' economic progress in the long run, largely by limiting their access to education. Using quasi-experimental variation, we show that gaining school access closed 80 percent of the gap in human capital caused by exposure to strict Jim Crow regimes.

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1. Introduction

The socioeconomic gap between Black and white Americans is one of the most persistent features of US society. For example, Black Americans today own over 80 percent less wealth than white Americans on average (Derenoncourt et al., 2022). Black Americans are also 40 percent less likely to hold a college degree than white Americans (US Department of Education, 2019), and their median incomes are 50 percent lower (Bayer and Charles, 2018). Although racial disparities have narrowed considerably over the past two centuries, the progress has been slow.

One possible explanation for the lower socioeconomic status of Black Americans is the US's particular history of institutionalized racial oppression. Throughout the country's early history, slavery was legal—until around 1800 in the North and until the end of the Civil War (1861–1865) in the South. However, slavery was not the end of institutionalized oppression. Soon after slavery, Southern state governments passed a mounting number of racially oppressive laws designed to limit the economic progress of newly freed Black families—a regime called Jim Crow. States' Jim Crow regimes instituted racial segregation (e.g., of schools and public transport), Black voter disenfranchisement (e.g., literacy requirements and poll taxes), and restricted the geographic mobility of Black Americans (e.g., vagrancy laws and enticement laws).[1] After almost 100 years, the Civil Rights legislation of the 1960s outlawed racial discrimination and ended Jim Crow, making it "one of the most significant legislative achievements in American history" (U.S. Senate, 2019).

This paper studies whether and to what extent Black families' historical exposure to slavery and Jim Crow continues to shape US racial inequality. In sum, we find that the socioeconomic status of black families today depends strongly on their historical exposure to racially oppressive institutions. Black families left slavery with little or no measurable physical or human capital. We show that after slavery, Black families' economic progress critically depended on the state in which they were freed. Most families enslaved until the Civil War were freed in the southernmost states. After slavery ended, those states implemented the most severe forms of Jim Crow institutions. Our results suggest that the economic progress of families enslaved until the Civil War would have been substantially faster between 1865 and today if it had not been for their high exposure to Jim Crow. We highlight the denial of equal access to education as a critical factor that made Jim Crow detrimental to Black economic progress.

We overcome the challenge of measuring each individual family's historical exposure to slavery and Jim Crow by tracing their census and administrative records from 1850 to

[1] Throughout this paper, we use the term "Jim Crow" to refer to institutions that limited Black Americans' civil rights. Extralegal factors—such as lynchings or employer discrimination that often went above and beyond the letter of the law—do not fall under our definition of Jim Crow. In terms of measurement, we focus on state institutions, ignoring less prevalent instances of local Jim Crow-like ordinances.

2000 using automated record-linking methods (Abramitzky et al., 2019).[2] First, to measure a family's exposure to slavery, we leverage that the 1850 and 1860 censuses did not record enslaved people. Therefore, we argue that we can identify families freed before the Civil War as those having ancestors recorded in the 1850 or 1860 census; others are classified as enslaved until the Civil War.[3] We validate this method by developing a new surname-based approach to determine how likely a family was to have been enslaved until the Civil War (Ager et al., 2021).[4] Second, to measure a family's exposure to Jim Crow, we use our linked sample to observe where a family's ancestors were freed from slavery.[5] Where a family was freed is a good reflection of their exposure to state-level Jim Crow institutions over the subsequent 75 years because Black Americans' geographic mobility was low before 1940 (Boustan, 2016), especially for those under intense Jim Crow regimes.[6] We measure a state's Jim Crow intensity using a newly constructed dataset of 800 Jim Crow laws and a preexisting composite index of state-level racial oppression.[7]

While exposure to oppression under slavery and Jim Crow was correlated, the two institutions' different geographies allow us to disentangle their effects. As a result of the rapid southern expansion of the US plantation economy, the longer a family was enslaved, the more likely they were to be concentrated in the southernmost states—which would become the epicenter of Jim Crow. State-specific laws formed Jim Crow regimes; in contrast, slavery was an institution that transcended state borders. Jim Crow restrictions on geographic mobility made it difficult to escape those regimes, even in state border regions. Therefore, families who had been enslaved close to each other sometimes began to experience drastically different institutions of racial oppression under Jim Crow.

We proceed in three steps to assess and disentangle the long-run effects of slavery and Jim Crow. First, we divide our sample into two groups and document socioeconomic gaps between them: Black families who had higher exposure to both slavery *and*

[2]Using their name, year and place of birth, and race, we follow individuals across full-count census records from 1850 to 1940. We use the census's information on interrelationships among individuals in the same household to build family trees based on those linked records. We also link families to Social Security mortality records that allow us to measure socioeconomic outcomes at the neighborhood level.

[3]Linking the historical records of women remains difficult, allowing us to follow only the paternal line of ancestry. We estimate that intermarriage between families freed before 1865 and families freed in 1865 likely attenuates our estimates of the socioeconomic gaps between them in 1940 by one-third.

[4]This approach leverages changes in the distribution of last names in the census from 1860 to 1870—before and after the inclusion of newly freed Black families—assigning a probability of having been enslaved until 1865 to each last name. For example, the last name "Freedman" did not exist in 1860, but many newly freed families chose it in 1865. In contrast, the last name "Du Bois" became ten times less frequent in the census after it included the formerly Enslaved in 1870.

[5]As a family's location of freedom, we use their ancestor's state of birth or county of residence as observed in the 1870 census. We only use this information for families who were enslaved until 1865.

[6]Mobility was low due to institutional factors—such as Jim Crow laws that limited labor mobility (Roback, 1984; Naidu, 2010)—and economic factors—such as high migration costs (Carrington et al., 1996) or the elusiveness of opportunities in potential destinations (Akbar et al., 2020; Derenoncourt, 2022).

[7]The composite index is based on a state's enslaved population share in 1860; its share of sharecroppers who were Black in 1930; its number of disfranchisement devices; and its share of congressional delegates that signed the Southern Manifesto (Baker, 2022).

Jim Crow because they were enslaved until the Civil War ("Enslaved"); and families who were less exposed to both institutions because they were free before the Civil War ("Free").[8] Second, we assess the importance of state-specific factors—such as Jim Crow regimes—by decomposing this "Free-Enslaved gap" into variation in Black economic progress within and across ancestor states.[9] Last, using a border discontinuity design, we isolate the effect that states' Jim Crow regimes had on Black economic progress from other factors that may vary across states, such as economic activity, culture, or climate.

Our first key result is that today, Black families enslaved until the Civil War continue to have considerably lower education, income, and wealth than Black families freed before the Civil War. These Free-Enslaved gaps are almost half as large as the corresponding Black-white gaps. While immediately after slavery, the Free-Enslaved gaps were even larger, their narrowing has been much slower than one would expect under standard levels of intergenerational mobility.[10]

Our second key result is that state-specific factors drive the long-run persistence of the Free-Enslaved gap. First, gaps due to direct exposure to slavery itself dissipated by 1940. In 1870, five years after the end of slavery, the socioeconomic status of recently freed families was far below that of families freed earlier, even for individuals from the same state. By 1940, those large Free-Enslaved gaps vanished conditional on the state in which their ancestors lived during slavery. Second, families enslaved until the Civil War were concentrated in the states where Black Americans fared worse after slavery. The difference in the two groups' geographic distribution fully explains the persistently lower socioeconomic status of families enslaved until the Civil War. In sum, state-specific factors were the critical force that perpetuated the socioeconomic disparities that slavery had created among Black families in the long run.

To identify the likely mechanism behind the importance of state-specific factors—namely, the effect of Jim Crow regimes—we use a regression discontinuity design that compares the socioeconomic outcomes of Black families freed across state borders with more or less stringent Jim Crow regimes. By focusing on counties close to state borders, we isolate the role of institutions from factors that transcend those borders.

Our third key result is that Black families freed in states with more oppressive regimes

[8] Using aggregate counts of the Black population starting in 1790 and assuming that free Black families' population growth equaled that of white families, we approximate that the average free Black family was freed 50 years before the Civil War—around 1815.

[9] If the main reason for the long-run persistence of the Free-Enslaved gap were differential exposure to slavery, we would expect this gap to largely reflect within-ancestor state differences between families freed before versus during the Civil War. In contrast, if the Free-Enslaved gap were driven by differential exposure to the state-specific factors, we would expect the gap to largely reflect across-state differences between families, irrespective of when they were free.

[10] We find that social upward mobility is far lower for Black than white Americans, especially among descendants of the Enslaved (see Appendix Figure A.2). We also show that there is a much faster convergence between the economic status of white families without any measurable physical or human capital in 1870 and families with average outcomes in 1870 (see Appendix Figure B.34).

experienced sharply lower rates of economic progress starting in the Jim Crow era (1877–1964). The resulting differences in socioeconomic status are increasing in the differences in Jim Crow intensity across a border. For example, consistent with Louisiana's Jim Crow legislation being far more extensive than Texas's, we find that families freed in Louisiana attained 1.2 fewer years of education by 1940 than families freed only a few miles away in Texas. The magnitudes of those border discontinuities are virtually identical to the general state differences in how families fared after slavery, suggesting that Jim Crow single-handedly shaped the geography of Black economic progress.

We validate this border discontinuity design by showing that 1) differences in the socioeconomic status of formerly enslaved people only arise with the beginning of Jim Crow (circa 1880), 2) those differences are increasing in the intensity of states' Jim Crow regimes, 3) before Jim Crow there are no cross-border differences in economic, agricultural, or demographic characteristics, and 4) Jim Crow regimes did not negatively affect white families. Basing our design on ancestor location before 1865—rather than the current location—leaves little room for selection, given that enslaved people had no say in their place of residence.

Our main identifying assumption is that an enslaved person's birthplace is exogenous to future generations' potential socioeconomic outcomes.[11] Historical evidence supports this assumption. Enslaved people had no freedom of movement before the Civil War, leaving no room for self-selection into location. Selection could only have occurred through forced migration, to which slaveholder migration and the domestic slave trade equally contributed (Steckel and Ziebarth, 2013). Slaveholders were generally non-selective in moving all their enslaved people with them (Fogel and Engerman, 1974; Pritchett, 2001; Tadman, 2008; Pritchett, 2019).[12] Selective slave trade is only evident in the small sugar cultivation areas.[13] However, the physical characteristics that led to selection into slave trade are likely unrelated to human capital today.[14] The evidence from our regression discontinuity design offers strong support for this assumption.[15]

To understand how Jim Crow regimes slowed Black economic progress, we classify

[11] We limit our sample to families enslaved until 1865.

[12] In principle, selection could also arise through differences in the slaveholders who choose to migrate. However, for selection to arise, the slaveholder's decision would need to be correlated with the potential outcomes of their enslaved people—a scenario we cannot rule out but deem unlikely.

[13] Sugar cultivation accounted for only 6 percent of the rural enslaved population (Tadman, 1977, 1979). By the nature of the work required, enslaved people there tended to be physically stronger and more likely to be male (Phillips, 1918).

[14] Contrary to the most plausible scenario for positive selection, we find that families freed in the Louisiana sugar areas achieved lower socioeconomic status by 1940 than families freed in other areas.

[15] Because our RDD estimates fully capture the differences in the causal state effects, any relevant selection would need to occur sharply at the border. Such forms of selection are implausible given that enslaved people were—if anything—selectively forced to migrate to specific locations based on the crops cultivated there. We verify that crops do not discontinuously change across state borders. We also verify that the observable characteristics of enslaved people—such as their age in 1860 or their literacy in 1870—did not discontinuously vary across borders, ruling out selection on observable characteristics directly.

Jim Crow laws by topic and find that the largest number pertain to education. Education is the target of 227 laws—over one-quarter of all Jim Crow laws passed throughout the South.[16] Those laws racially segregated schools, reduced educational resources allocated to Black children, shortened term lengths for Black schools, and prevented Black Americans from participating in the local bodies that governed education. Our analysis of the content of these laws suggests that Jim Crow directly restricted Black Americans' *access* to education, motivating our exploration of access to education as an essential mechanism in the persistent effect of Jim Crow.

We assess whether access to education mediated the effect of Jim Crow on outcomes in the long run by leveraging a natural experiment in school provision in the early 20th century. Specifically, we compare the education of children depending on whether their ancestors were freed in a county that would receive one of 5,000 schools built by the Rosenwald program (1914–1931) by the time the children were of school age (Aaronson and Mazumder, 2011). We find that the supply of schools had persistent positive effects on the economic progress of Black families, especially in the most oppressive states.[17] Gaining access to a Rosenwald school closed 80 percent of the education gap caused by exposure to an intense Jim Crow regime. We find that the schools not only increased the education of those who had access but also improved the economic conditions of their children in the long run—for example, college completion increased by 40 percent.

This paper contributes to our understanding of whether and how historical institutions affect economic outcomes in the long run. Acemoglu et al. (2002), Dell (2010), Donaldson (2018), and Dell and Olken (2019) show that institutions can lastingly transform regions. In this paper, we develop innovative methods to study the impact of institutions on *individual families* rather than regions and apply them in the context of US historical racial oppression. Such individual-level evidence allows for the geographic mobility of families, which attenuates regional differences in the long run. Tracing the effect of institutions on families can also generate novel insights into the mechanisms that drive various forms of persistence. We leverage quasi-experimental variation in school construction in the South to show that mere access to a school closes 80 percent of the education gap caused by Jim Crow.

This paper further contributes to the evidence of the long-run effects that oppressive institutions can have on racial inequality. Within and outside of the US, regions that relied on slave labor continue to have lower and more unequally distributed incomes (Nunn, 2008), lower upward mobility (Berger, 2018), larger racial disparities (O'Connell, 2012; Bertocchi and Dimico, 2014), and higher levels of racial resentment against the for-

[16] Education as the main target is followed by public transport (150 laws), employment (138 laws), public facilities (106 laws), marriage (85 laws), and suffrage (29 laws). We pool the remaining 55 laws that do not fall into either of those categories under "other."

[17] A Black child with access to a Rosenwald school attained 0.3 years more education than a child without access. In the most oppressive states, the effect was 0.9 years.

merly enslaved (Acharya et al., 2018).[18] Evidence of how racially oppressive institutions affect individual Black families in the long run is scarce. Sacerdote (2005) uses Southern place of birth as a proxy for being enslaved until the Civil War and shows that Black descendants of this group continued to have lower socioeconomic status than those freed earlier. By combining newly available linked records, exogenous variation in ancestor location, and new details on state institutions, we assess why Black families whose ancestors were enslaved until the Civil War still experience lower socioeconomic outcomes. We show that after 1940, the single reason those differences persist is that the most strict Jim Crow regimes arose in the states where Black families enslaved until the Civil War were concentrated. This result implies that systemic discrimination—the higher exposure to ongoing discrimination *because of past discrimination* (Bohren et al., 2022)—is at the core of slavery's persisting legacy.

This paper also enhances our understanding of geographic disparities in intergenerational mobility. Historically and in recent decades, upward mobility has been lower in the South than in any other US region (Olivetti and Paserman, 2015; Chetty et al., 2014). Chetty and Hendren (2018) analyze children whose families move across places to show that locations *caused* low upward mobility in the South rather than being a result of selection. This paper shows that institutions can be a crucial force underlying places' causal effect on intergenerational mobility. We show that a key channel through which the institution of Jim Crow reduced Black Americans' economic mobility is the restricted access to education (see also Card et al., 2022). In addition, we develop a novel empirical strategy to assess a place's effect on upward mobility based on the geographic *immobility* of a specific population rather than their mobility. We find that a state's capacity to generate upward mobility is highly persistent: our estimates (1865–1940) have a strong correlation $(0.617, p = 0.000)$ with states' causal effects on the intergenerational mobility of low-income groups in recent decades (Chetty and Hendren, 2018). This high persistence is consistent with rigid cultural, economic, and institutional factors that set boundaries to economic opportunity (Acharya et al., 2018).

Lastly, this paper contributes to the historical literature on the evolution of Black economic progress after the end of slavery in the US. Margo (1991) argues that beyond market forces such as the supply of and demand for educated Black workers, one of the main reasons for the persistence of Black-white gaps in education was the barriers faced by Black parents, which in turn diminished the economic opportunities of their children. Our results show that this "intergenerational drag" of slavery itself was quantitatively important for around three generations but diminished over time. We extend Margo's model of Black economic progress to encompass the racially oppressive institutions after slavery, which we show to be decisive in the long run. The dependence of Black economic

[18] Those locations also have lower productivity (Mitchener and McLean, 2003). In Brazil, a location's past reliance on slavery caused weaker institutions and higher inequality until today (Fujiwara et al., 2019).

progress on institutional factors is consistent with the seminal work of Du Bois (1935), Woodward (1955), Ransom and Sutch (2001), and Wright (2013) who highlight that when and where their environment allowed for it, Black families did make rapid progress—such as in the Reconstruction era (1865–1877). Consistent with that, our evidence from the Rosenwald schools suggests that it was not a lack of demand for education among Black children in the Jim Crow South but a lack of *access* to education that slowed their human capital accumulation (see also Aaronson and Mazumder, 2011).

2. Historical Context

This section provides some historical context for the evolution of racially oppressive institutions in the US—from slavery to Jim Crow and beyond.

2.1 Free Black Americans before 1865

In 1860, just before the Civil War (1861–1865) that led to the abolition of slavery, 4 million enslaved and 0.4 million free Black people lived in America. Enslaved people have existed on American soil since the country's colonial origins (Sowell, 1978). The roots of the free Black population may trace back to 1619 when settlers in Virginia purchased the first 20 Black people. Little is known about their fate, but it is likely that some of them were treated as servants who had to work for a fixed term and gained freedom afterward (Frazier, 1949). Around 1660, both law and practice had changed, implying that virtually all Black individuals who arrived in the colonies were enslaved for life. From 1662 onwards, the law also mandated that a child would inherit their legal (i.e., free or enslaved) status from their mother regardless of race.

For some enslaved people, the Revolutionary War (1775–1783) provided a road to freedom. Responding to a need for troops and laborers, the British governor promised freedom to all enslaved willing and able to serve the British. It is estimated that up to 100,000 enslaved people ran away from plantations to do so (Schama, 2006). After the war, many remained in the US as free persons. As a result, the free Black population in some states increased dramatically.

The Revolutionary War also brought a spirit of egalitarianism, challenging the institution of slavery in some regions. In the North, the abolitionist movement spread quickly after the war. While only a few Black people lived free of slavery before the Revolutionary War, most Northern states adopted gradual Emancipation laws after the war. New Jersey was the last Northern state to do so in 1804.

In the South, the path to freedom was narrow, especially in the Lower South.[19] All

[19]The Lower South comprises Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, South Car-

Southern states except North Carolina allowed masters to free ("manumit") their enslaved people by 1790, but the practice was employed to different degrees across regions. In the Upper South, the first wave of manumissions occurred between 1783 and 1793, the first decade after the Revolutionary War. Motivated by anti-slavery beliefs, most manumitters freed all their enslaved people at once. However, manumission gradually became more selective and turned into a reward system designed to uphold slavery (Wolf, 2006). By 1860, 0.2 million of the 1.8 million Black Americans in the Upper South were free (11.1 percent). The Lower South did not see a similar manumission wave after the war, as manumissions there were usually limited to masters' "illicit offspring, special favorites, or least productive slaves" (Berlin, 1974). The free Black population of the Lower South mainly originated from refugees who fled from Saint-Domingue (now Haiti) and the purchase of Louisiana from France, which had a sizable free Black population. By 1860, 40,000 of the 2.5 million Black Americans in the Lower South were free (1.6 percent).

The legal and socioeconomic status of the Free varied greatly across locations and over time before 1865 (Sowell, 1978). In most states, free Black Americans were deprived of the right to vote and to hold political office. However, their legally protected property rights were respected in most cases. With the limited freedom they enjoyed, some free Black families could accumulate modest wealth and social status. Most of them, however, lived in poverty "under conditions barely distinguishable from those of the mass of slaves" (Berlin, 1974). Their economic status varied considerably across the country and, perhaps surprisingly, tended to be better further South (Berlin, 1976). In the North, free Black families were concentrated in cities where they suffered from competition with and hostility from white laborers (Frazier, 1949). Most free Black families in the South lived in rural areas, working as farmhands and casual laborers (Berlin, 1974).

2.2 Freedom of All Black Americans after 1865

By the beginning of the Civil War (1861–1865), the enslaved population was concentrated in the Lower South (see Figure 1). The free Black population, in contrast, was concentrated in the North and the Upper South. These differences in geographic location exposed them to different institutional regimes after slavery.

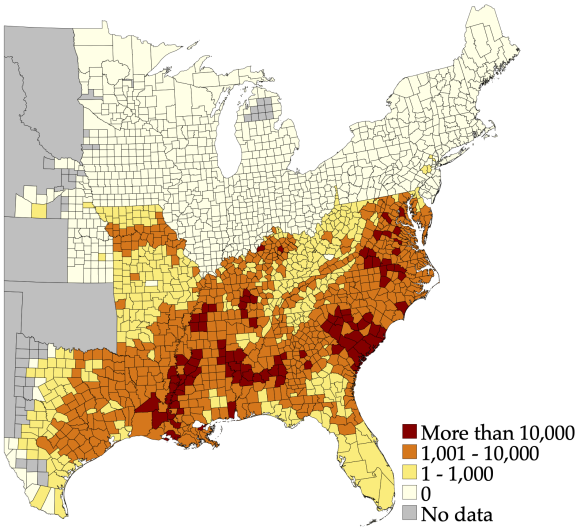
The Civil War led to the emancipation of enslaved families, giving all Black Americans the same legal status. The average free Black family had likely already been free for around 50 years.[20] For the first 12 years after the Civil War—the Reconstruction era (1865–1877)—the Union Army occupied the South. Black Americans experienced unprecedented economic progress under Reconstruction (Foner, 2014). New schools and

olina, and Texas. The Upper South comprises Delaware, Washington, DC, Kentucky, Maryland, Missouri, North Carolina, Tennessee, Virginia, and West Virginia. The North comprises all other states.

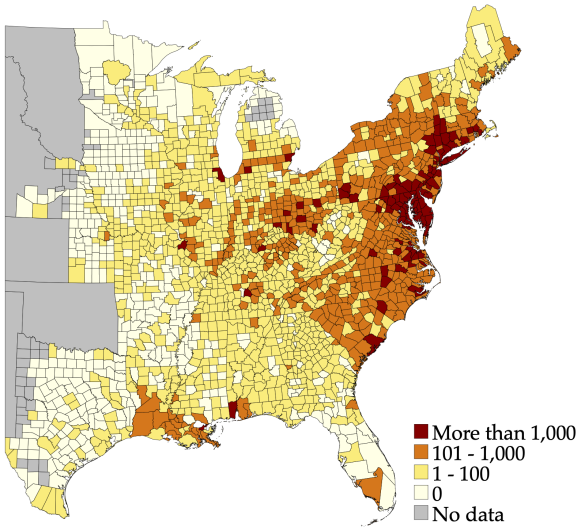
[20] Fifty years is an estimate derived from aggregate counts of the Black population starting in 1790 and assuming that free Black families' population growth equaled that of white families.

FIGURE 1: Population by County in 1860

(A) Number of Enslaved



(B) Number of Free



Notes: This figure shows the population sizes of enslaved Black Americans (Panel A) and free Black Americans (Panel B) in the 1860 census. The maps are truncated to omit the western half of the country, which at the time was only sparsely populated. Appendix Figure C.44 shows the full maps for 1790 and 1860.

colleges were built to educate Black Americans throughout the South. Black men participated politically, casting their votes in high numbers and serving in public office (Logan, 2020). Throughout Reconstruction, Black economic and political progress was met with violent opposition from white Southerners (Du Bois, 1935; Foner, 1963; Blackmon, 2009).

In 1877, the Union troops left the South, abandoning the project of Reconstruction. The disenfranchisement of Black people through informal and legal means led to massive declines in Black political participation (Kousser, 1974; Naidu, 2012). Many free Black Americans lost their higher social status and left the South (Woodson, 1918).

Black Americans who remained in the South after Reconstruction faced increasing oppression through the rise of Jim Crow (1877–1964). Jim Crow laws governed almost every aspect of Black life. Schools, workplaces, public transport, medical facilities, and parks were racially segregated (Murray, 1950). Poll taxes, literacy tests, and other rules limited Black suffrage (Naidu, 2012; Walton et al., 2012). Enticement laws, contract enforcement laws, and emigrant-agent laws prevented Black workers from seeking economic opportunities with new employers or in states outside the South (Roback, 1984; Naidu, 2010). Vagrancy laws criminalized the unemployment of Black people (Blackmon, 2009). In addition to institutionalized oppression, various informal means of excluding Black Americans spread through the South and beyond.

From 1910 to 1940, many Black Americans started to leave the (Upper) South in the first wave of the Great Migration. Black families from the Lower South only participated

in this migration in small numbers before 1940, both because Jim Crow limited their geographic mobility and because migration was more costly for them (Roback, 1984; Naidu, 2010; Carrington et al., 1996). While the Civil Rights Movement successfully fought oppression starting in the mid-1950s, the Great Migration continued until the end of the movement in the late 1960s. By then, six million Black Americans had left the South (Boustan, 2016). However, opportunities in the North proved elusive to Black families (Akbar et al., 2020; Derenoncourt, 2022). In addition, even after the achievements of the 1960s, old forms of racial oppression have persisted, and new forms—such as mass incarceration and "color-blind" voter suppression—have arisen since (Western, 2006; Alexander, 2010; Bonilla-Silva, 2015; Darity et al., 2016). Progress in narrowing racial gaps in socioeconomic status has largely stalled since the 1960s (Bayer and Charles, 2018; Althoff, 2021; Derenoncourt et al., 2022).

3. Data and a New Method to Measure a Family's Exposure to Slavery and Jim Crow

A major empirical challenge we overcome in this paper is to measure a Black family's exposure to slavery and Jim Crow. We construct family histories for Black Americans in the census between 1850 to 2000 and develop new methods to measure the two critical components of a family's historical exposure to institutionalized oppression: how long a family was enslaved and where they were freed, determining the intensity of the Jim Crow regime under which they likely lived.

3.1 Measuring How Long a Family Was Enslaved

To measure how long a family was enslaved, we leverage that the 1850 and 1860 censuses did not record enslaved people.[21]

Main method. We identify Black Americans free before 1865 ("the Free") as those who were (1) recorded in the 1850 or 1860 census or (2) born in a state that had already abolished slavery; Black Americans who were born in slave states before 1865 and cannot be traced back to ancestors in the 1850 or 1860 census are classified as enslaved until 1865 ("the Enslaved").[22] We then carry this information forward to their descendants. To do so, we build family trees using the information on family interrelationships for

[21] These are the only pre-1865 census decades with individual-level data.

[22] We refer to Black families free before 1865 as "the Free" even though they or their ancestors may have been enslaved in previous decades. We refer to those enslaved until 1865 as "the (formerly) Enslaved." We choose this terminology to avoid confusion engendered by the sometimes-used terms "Freemen" (Free) and "Freedmen" (formerly Enslaved). We avoid the term "slave" and capitalize "Free" and "Enslaved" when used as nouns to be respectful of the people we study.

members of the same household from the census and by linking individuals' census and administrative records from 1850 and 2000.

This classification strategy accurately identifies whether a Black family's ancestors were enslaved until 1865. In principle, if a family cannot be linked back to the 1850 or 1860 census, this could either mean that they were enslaved until 1865 or that they could not be linked using automated methods—for example, because their name was misspelled in one census. Hence, in the South, we inevitably misclassify some Black families who were free before 1865. However, census records show that only 6 percent of the Southern Black population were free in 1860, many of whom we correctly classify as such. As a result, we compare a group that was free with almost certainty in 1860 with a group of which at least 94 percent were enslaved. Therefore, the potential for attenuation bias due to imperfect linking rates is minimal.[23]

Our classification method has two significant advantages over previous research, which typically relied on birthplaces to identify how long a family was likely enslaved. First, because the census only provides information on birthplaces for a person and their parents, the intergenerational effects of slavery beyond the second generation cannot be studied in the census cross-section. Our panels allow us to follow families until today.[24] Second and more importantly, relying on a person's birthplace can only identify free Black families born in the North. However, 50 percent of all Black families freed before 1865 lived in the South. Our method correctly identifies a large number of those families. Measuring how long a family was enslaved and where it was freed is crucial to determining what role slavery, Jim Crow, and their interaction play in shaping the persistent effects of institutionalized racial oppression.[25]

While the Free-Enslaved gap accurately captures differences based on the enslavement status of a family's *male* ancestry line, we show that it is also a lower bound for differences that would arise if the entire family tree had the same enslavement status. Because women tended to change their last names upon marriage, automated linking methods do not allow us to directly follow a family's female ancestry line. An advantage of considering only male ancestry is that it avoids bias through selective marriage. A disadvantage is that we do not know which fraction of one's ancestors were enslaved until 1865 and which fraction was freed earlier. We show that the male ancestry line provides a valuable proxy for a family's share of ancestors enslaved, making the Free-Enslaved gap a lower bound for the actual group differences between families with high vs. low shares of ancestors enslaved.[26]

[23] In Appendix B.6, we show that our results are robust to 1) correcting for state-specific rates of misclassification and 2) excluding the (small) Southern states where a large share of Black Americans was free before the Civil War (Delaware, DC, and Maryland).

[24] This extension also lets us use the rich data on education, income, and wealth in the 1940 census.

[25] See Appendix Figure C.43 for average socioeconomic outcomes among descendants of the Enslaved and the Free by region of origin.

[26] In Appendix B.5, we derive this result theoretically and empirically approximate the difference be-

Alternative method. We develop a second strategy to identify descendants of the Free and the Enslaved based solely on last names. We use the change in the distribution over last names before 1865, when the census included only free Black Americans, to after 1865, when it included all Black Americans. This approach allows us to use the full (rather than only the linked) sample of Black Americans in the census. The two approaches yield Free-Enslaved classifications that are highly correlated.[27]

While some last names were common among the Free and the Enslaved, others were characteristic of one group. For example, the last name "Du Bois" was relatively frequent among free Black families in the 1860 census. However, with the inclusion of the families newly freed in 1865 in the 1870 census, Du Bois became ten times less frequent—an indication that having this last name meant a person likely descended from the Free. In contrast, the last name "Freedman" did not exist in the 1860 census but appeared in the 1870 census after some newly freed families chose it as their new last name. Thus, Black families called Freedman were likely enslaved until 1865.

This alternative classification method trades off accuracy in favor of coverage. Based on this probabilistic measure, we expect the resulting Free-Enslaved gaps to be subject to attenuation bias, making them appear smaller than they are in truth. In contrast to our main method, however, this classification can be applied to the entire population of Black Americans rather than being limited to those we can link back in time.

This classification also allows us to identify descendants of the Enslaved in non-census data that include last names. We are collaborating with one of the primary credit score providers in the US to extend our results to 2022.

3.2 Measuring the Exposure to State-Led Oppression During Jim Crow

Black families' exposure to slavery and Jim Crow is highly correlated. Families enslaved until 1865 were also geographically concentrated in states that would become the epicenter of Jim Crow. In contrast, families freed earlier were concentrated in states that would adopt less intensive Jim Crow regimes. These different geographic distributions result from the rapid South expansion of the US plantation economy. The longer a family was enslaved, the more likely they were to be freed in the Lower South.

To measure a family's exposure to Jim Crow, we use that record linkage allows us to observe the birthplace of its formerly enslaved ancestors. The state where a family was freed is a good proxy for exposure to state-level Jim Crow institutions over the subse-

tween the two measures. Intermarriage was likely rare before 1940 due to the differences in the two groups' geographic concentration. We estimate that for the first generation born after 1865, the socioeconomic gaps between Black Americans whose ancestors only descend from Enslaved vs. Free Black ancestors is around 15 percent larger than the Free-Enslaved gap. In later generations, this difference is almost certainly more significant.

[27] See Appendix Figure C.41.

quent 75 years. Their geographic mobility across states was low before 1940, especially for those in the most oppressive Jim Crow states.

We use three different measures of a state's Jim Crow intensity. Our first measure is the number of Jim Crow laws that each state passed until 1950. For this measure, we collected data on 800 Jim Crow laws. Specifically, we digitized the laws recorded in "States' Laws on Race and Color" (Murray, 1950)—a resource aiming to document all state laws that regulated race and color in 1950.[28] We classify each law as discriminatory, not discriminatory, or anti-discriminatory based on the legislative text and sometimes the context provided by the author. We use discriminatory laws to proxy for Jim Crow laws. We also determine each law's domain, such as education, marriage, or public transport. We complete our dataset on Jim Crow laws by digitizing two crucial types of laws that Murray (1950) largely omitted: Laws on employment from Roback (1984); Cohen (1991) and laws on suffrage from Walton et al. (2012).

Our second measure is a composite index of state-level racial oppression—the Historical Racial Regime (HRR) index. This index is a data-driven proxy of a state's intensity of racial oppression from slavery to Jim Crow (Baker, 2022). The measure is a factor extracted from four components: a state's population share enslaved in 1860; its share of sharecroppers who were Black in 1930; its number of disfranchisement devices; and its share of congressional delegates that signed the Southern Manifesto. A state's HRR index is highly correlated with its number of Jim Crow laws ($\rho = 0.71$).

As our third measure we construct a composite index of a state's Black school quality from data collected by Card and Krueger (1992). Specifically, we isolate the principle component of three state-specific school quality measures: teacher salaries, student-to-teacher ratios, and term lengths (all specific to Black children). Our index of Black school quality is highly correlated with the state's HRR index ($\rho = -0.94$) and its number of Jim Crow laws ($\rho = -0.58$).

It is important to stress that Jim Crow regimes comprised de jure and de facto tactics, both of which critically contributed to the political exclusion of Black Americans (Acemoglu and Robinson, 2008). For example, Black suffrage was limited through laws, such as literacy tests or poll taxes, and non-legal means, such as Black voter intimidation and violence against Black Americans. No single measure perfectly captures those factors, in part because many cannot be observed in historical records. "There [was] more Jim Crowism practiced in the South than there [were] Jim Crow laws on the books" (p. 102 Woodward, 1955). Therefore, one should view the number of Jim Crow laws not as the golden truth of a state's Jim Crow intensity but as an imperfect proxy. The outcome-based measures of the HRR index and Black school quality fill some gaps in measuring Jim Crow intensity that the number of Jim Crow laws likely leaves open.

[28] In an effort independent of ours, Cook et al. (2022) have also collected this data from (Murray, 1950).

3.3 Linked Data

We use full-count census data for all available decades between 1850 and 1940 (Ruggles et al., 2020) and link observations across adjacent and non-adjacent decades using the automated linking methodology provided by Abramitzky et al. (2020). A person is linked from one census to another if their name, year of birth, and state of birth match and if the match is *unique* conditional on race. We use a method that allows for misspellings by matching names based on their phonetic sound (NYSIIS). Because women tend to change their last name upon marriage, only men can be linked over time.

The census also contains information on the relationship between individuals in the same household. Most importantly, by observing a person in their parents' household during childhood, we can build family trees based on this information. These family trees allow us to study the evolution of a family's social, economic, and geographic mobility across generations. We study families' outcomes in census records between 1870 (the first census to include all Black Americans) and 1940 (currently the most recent full-count census available). Our primary outcomes include education, income, and wealth. Over time, the census data provide increasingly rich information on those outcomes. Therefore, we focus particular attention on the rich information in the 1940 census.

To extend our analysis to the 21st century, we link the 1940 census to administrative death records between 1988 and 2005 (Goldstein et al., 2021). These records cover the near-universe of deaths among American citizens and contain the nine-digit ZIP code of the decedent's residence at the time of death. Nine-digit ZIP codes are highly granular indicators of location, which refer to a "segment or one side of a street" (USPS, 2021), allowing us to obtain rich information on the socioeconomic characteristics of a person's neighborhood.[29] We use National Historical Geographic Information System (NHGIS) data on the distribution of education, income, and wealth by race within these areas to extend our analysis to the present day. We also examine broader measures of well-being such as health, mortality, residential segregation, evictions, and fatal police encounters. Last, we build a database of historical location characteristics, including agriculture, demographics, lynchings, segregation, railroad networks, and transportation costs.[30]

3.4 Sample

For our analysis, we focus on Black men aged 20 to 54. For two reasons, we also limit our sample to individuals who can be linked to their ancestors in 1880 or earlier. First, to identify a family who gained freedom before 1865 in a state that had not abolished slav-

[29] We map nine-digit ZIP codes into statistical areas, such as census blocks, which are small and designed to have socioeconomically homogeneous populations (Census Bureau, 2017). In Appendix C.9, we describe the procedure for linking nine-digit ZIP codes to statistical areas.

[30] We describe this dataset in detail in Appendices C.6–C.8.

ery, it must be linkable to their ancestors in 1850 or 1860. Restricting the sample to Black Americans linkable to 1880 or earlier minimizes the bias that may result from comparing families who can be linked back in time easily (e.g., because they have unique names) with those who cannot. Second, this restriction excludes families who immigrated to the US after 1880 who might have experienced very different sets of oppressive institutions historically. Our results are not sensitive to this restriction.

For 1940, the latest year available, our sample of Black prime-age men consists of 155,813 descendants of families enslaved until 1865 and 9,325 descendants of families freed before 1865. We achieve a linking rate of 10 percent from 1870 to 1940—an essential benchmark because those links allow us to observe the state in which a Black family's ancestors were freed from slavery via their birthplace in the 1870 census.[31] Such imperfect linking rates are standard in the literature, especially for Black individuals. Despite the imperfect linking rate, our sample is highly balanced on observables (see Appendix Table C.15 for balance checks). For example, the literacy rate of formerly enslaved families in our linked sample matches that of the 1870 census population: 20.4 percent. For free Black families in our linked sample, literacy is very close to that of the 1860 census population: 65.1 compared to 66.8 percent. From the 1940 census to administrative records in 2000, we can link 21,059 descendants of enslaved families and 1,591 descendants of free Black families.

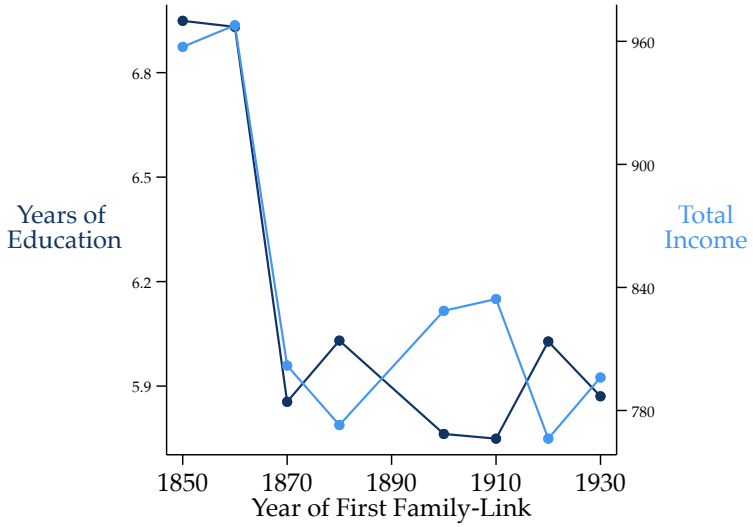
Potential Linking Bias. In constructing our main sample, we rely on linking families across census records. One may be concerned that linking procedures introduce mechanical differences between families enslaved until 1865 and those freed earlier. The most plausible concern is that a person's socioeconomic status depends on how many generations or decades they can be linked backward.[32]

To examine the quantitative importance of this concern, we group Black Americans in 1940 by the earliest decade in which we can link them back to one of their ancestors and plot their average outcomes by group (see Figure 2). In 1870, Black families enslaved until 1865 were included in the census for the first time. Consistent with that change in sample composition, we observe a significant drop in average income and education for people who can be linked to ancestors in 1870 but not 1860 or 1850. Aside from this drop, there are no trends in income or education, suggesting that individuals who can be linked further do not have a mechanically higher socioeconomic status. The evidence suggests that the group differences estimated here are not affected by such a mechanical bias. To err on the side of caution, we limit our sample to individuals who can be linked back to 1880 or earlier throughout this paper.

[31] Linking rates are lower for Black than for white individuals. For example, for white men, Ager et al. (2021) achieve a linking rate of 20 percent from 1860 to 1870 or 1900. Our average linking rates are 12 percent between adjacent census decades and 20 percent from one census to any other census.

[32] For example, children of single mothers typically cannot be linked to their grandparents in earlier censuses because existing methods exclude women.

FIGURE 2: Average Outcomes in 1940



Notes: This figure shows the average outcomes of Black Americans in 1940 by the earliest year to which we can link them back to one of their ancestors. The dark blue line (left y-axis) shows the years of education; the light blue line (right y-axis) shows the total predicted income. The lines suggest no trend in outcomes outside of the break from 1860 to 1870. See Data Appendix C for details on the sample and data.

4. A SIMPLE MODEL OF BLACK ECONOMIC PROGRESS AFTER SLAVERY

We propose a simple model of Black economic progress to guide our interpretation of which forces shape the Free-Enslaved gap's persistence in the long run. The framework incorporates intergenerational mobility, the effects of exposure to location-specific factors, (selective) migration, and the effect of delayed freedom. Throughout this paper, we use this model to answer the following questions: What factors determine the long-run persistence in the gap? How important was the differential exposure to location-specific factors among the Enslaved and the Free in shaping the gap? Is the persisting disadvantage faced by descendants of the Enslaved a causal effect of slavery or Jim Crow?

4.1 Model setup

Let $y_{i,t}$ denote the human capital—or any other outcome of interest—for person i at time t. For simplicity, let there be two time periods, $t \in \{0, 1\}$; the model is easily extendable to more time periods. We think of $t = 0$ as reflecting 1865, the year of Emancipation, and $t = 1$ as reflecting 1940, the last census year to which we can link families. We model $y_{i,t}$ to be determined by

$$y_{i,t} = \alpha_{i,t} + \gamma^{t}_{\ell(i,t)} + \rho y_{i,t-1} + \varepsilon_{i,t} \tag{1}$$

such that it depends on four factors: a factor capturing innate ability $\alpha_{i,t}$ with c.d.f. $F(\cdot)$, the family's previous human capital $y_{i,t-1}$, their location $\ell(i,t) \in \mathcal{L}$, and a random error term $\varepsilon_{i,t}$ that satisfies $\mathbb{E}[\varepsilon_{i,t} \mid s_i, \alpha_{i,t}, \ell(i,t)] = 0$. Last, we define γ_ℓ^t as the effect of being exposed to location ℓ at time t. We model $y_{i,0}$ (the starting condition) as

$$y_{i,0} = \alpha_{i,0} + \gamma^0_{\ell(i,0)} - \delta s_i + \varepsilon_{i,0}, \tag{2}$$

where s_i is an indicator for whether the family was enslaved until 1865. That is, in 1865, the outcomes depend on ability, location, and whether a person had been free before the Civil War. The parameter $\delta \geq 0$ captures any direct advantage that free Black Americans had relative to the Enslaved, such as access to education during slavery.[33]

4.2 The Intergenerational Effect of Being Enslaved Until the Civil War

We define the effect of descending from ancestors who were enslaved until the Civil War ($s_i = 1$) as the expected difference between the two groups in the absence of differences in ability ($\alpha_{i,0}$). That is, we define the average treatment effect as

$$ATE \equiv \int \left(\mathbb{E}[y_{i,1} \mid s_i = 1, \alpha_{i,0}] - \mathbb{E}[y_{i,1} \mid s_i = 0, \alpha_{i,0}]\right) dF(\alpha_{i,0}). \tag{4}$$

Throughout the paper, this definition will guide the interpretation of our estimates.

In conceptual contrast to prior work (e.g., Sacerdote, 2005), we argue that one should not think of slavery's average treatment effect merely as an effect *conditional on location*. Descending from an enslaved person made a person much more likely to come from (and still live in) environments that were relatively harmful to their economic progress. Their enslavement status directly caused the location of enslavement, and the treatment effect should include its impact. From an econometric perspective, geographic location can be interpreted as a *bad control* since it is a mediating variable through which slave status affects future descendants (Angrist and Pischke, 2008).

[33] At time $t = 1$, the outcomes then become

$$y_{i,1} = (\lambda + \rho)\,\alpha_{i,0} + \rho\gamma^0_{\ell(i,0)} + \gamma^1_{\ell(i,1)} - s_i\rho\delta + \rho\varepsilon_{i,0} + \varepsilon_{i,1}, \tag{3}$$

where $\alpha_{i,1} = \lambda\alpha_{i,0}$ allows for transmission of ability over multiple generations. Thus, outcomes are determined by the ability of the initial generation through direct inheritance of ability (λ) and through intergenerational advantage derived from ability in previous generations (ρ). The current location ($\gamma^1_{\ell(i,1)}$) shifts the level of a person's human capital. Through intergenerational transmission, human capital is also affected by (1) how previous generations were affected by where they lived ($\gamma^0_{\ell(i,0)}$), (2) whether their ancestors were enslaved until 1865 (δ), and (3) their ancestors' idiosyncratic human capital shocks ($\varepsilon_{i,0}$).

5. Socioeconomic Gaps between Descendants of Free and Enslaved Families

This section documents the gaps in education, income, and wealth from 1870 to 2000 between descendants of families enslaved until the Civil War and those freed earlier. We find that these gaps are large and persist until today.

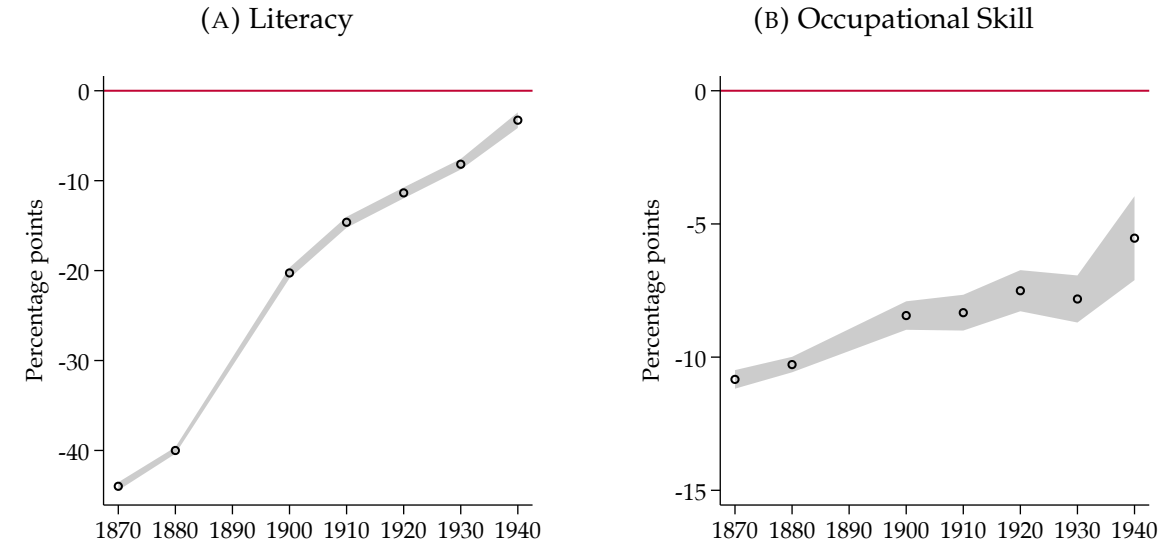
5.1 Evolution of the Free-Enslaved Gap until 1940

We estimate the Free-Enslaved gap (β_t) in socioeconomic outcomes ($y_{i,t}$) separately for each decade t from 1870 to 1940:

$$y_{i,t} = \alpha_t + \beta_t s_i + \phi_t' X_{i,t} + \varepsilon_{i,t}, \tag{5}$$

where s_i is equal to one if person i is classified as a descendant of the Enslaved and is zero otherwise. $X_{i,t}$ is a vector of controls that includes a quadratic term of age in our baseline specification. We cluster standard errors at the family level.[34]

Figure 3: Free-Enslaved Gap (1870–1940)



Notes: This figure shows the gaps in literacy and occupation skill (HISCLASS) among prime-age (20-54) male descendants of free vs. enslaved Black Americans in each census decade. We restrict the sample to observations linked to ancestors in 1850, 1860, 1870, or 1880. We control for a quadratic function in age and include 95 percent confidence bands clustered at the family level. See Data Appendix C for details on the sample and data.

We find that the socioeconomic differences between descendants of the Free and the Enslaved are large and persistent. In 1870, the formerly Enslaved were two times (over 40

[34] We define a family as a group of individuals with a common 1870 ancestor.

percentage points) more likely to be illiterate than free Black Americans (see Figure 3). By 1940, the gap was 1.8 times (5 percentage points). Descendants of the Enslaved worked in less skill-intensive occupations than descendants of the Free from 1870 to 1940. Consistent with this skill gap, descendants of the Enslaved earn substantially lower incomes and are significantly less likely to own their homes.[35]

The rich information on education, income, and wealth provided by the 1940 census allows us to get a detailed picture of the Free-Enslaved gap 75 years after slavery ended. Consistent with our previous results, we find that descendants of the Enslaved are substantially less educated, earn lower incomes, and have accumulated less wealth than descendants of free Black Americans (see Table 1). The gap in education amounts to 1.6 years—more than one-quarter of the average years of education among Black men in 1940.[36] The likelihood that a descendant of the Enslaved earned a high school or college degree was only half compared to descendants of the Free.[37] Consistent with the educational gap, the income and wealth of the Enslaved are substantially lower.[38]

TABLE 1: Free-Enslaved Gap (1940)

	Education (Years) Mean: 5.99	Wage Income (USD) Mean: 381.20	Homeownership (%) Mean: 29.25	House Value (USD) Mean: 1,371.95
Ancestor Enslaved	**-1.59***** (0.05)	**-145.92***** (6.13)	**-7.24***** (0.62)	**-694.69***** (65.85)
Controls (age, age^2)	Y	Y	Y	Y
Adjusted R^2	0.04	0.05	0.01	0.01
Observations	163,549	154,463	164,357	46,971
Ancestor Free	*9,078*	*8,551*	*9,070*	*3,227*

Notes: This table shows the gap in years of education, wage income, homeownership, and house value (conditional on ownership) among prime-age (20–54) male descendants of free vs. enslaved Black Americans in 1940. Only observations that can be linked to the 1850, 1860, 1870, or 1880 census are included. Sample means are computed for the combined sample of the Free and the Enslaved. See Data Appendix C for details on the sample and data. Standard errors are clustered at the family level and are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The narrowing of the Free-Enslaved gap from 1870 to 1940 is slow relative to benchmark rates of intergenerational mobility among white Americans. To benchmark the convergence speed, we estimate socioeconomic gaps between white families whose ancestors had no measurable physical or human capital in 1870 and all white families between 1870 and 1940.[39] In only 30 years, the gap in literacy between those two groups of white

[35] See Appendix Figure A.1.

[36] This gap is at the lower end of the range that Sacerdote (2005) estimates based on comparing Southern- and Northern-born Black men older than 54.

[37] See Appendix Table A.2.

[38] Appendix Table A.1 compares the Free-Enslaved gap across different income measures. The relative gap in (imputed) *total* income is similar to the relative gap in years of education: 25 percent. In Appendix A.2, we document the Free-Enslaved gap in intergenerational mobility.

[39] See Appendix Figure B.34.

Americans rapidly shrunk from over 90 percentage points to less than 10 (from twice the Free-Enslaved gap in 1870 to half the Free-Enslaved gap in 1900). The homeownership gap for the two groups of white Americans was similar to the respective Free-Enslaved gap in 1870 but closed by 1900—while the Free-Enslaved changed very little until then. Consistent with the comparatively slow narrowing of the Free-Enslaved gap, we show that intergenerational upward mobility is not only lower for Black than for white Americans throughout US history (Collins and Wanamaker, 2022; Chetty et al., 2020), but that it is particularly low for descendants of the Enslaved.[40]

5.2 The Free-Enslaved Gap in the 21st Century

We extend our results beyond 1940 by linking Black families from the 1940 census to administrative death records around 2000. Those records include the nine-digit ZIP code of a person's residence at the time of death, from which we derive neighborhood-level information on the income, wealth, and education distribution.[41] It is reasonable to suspect that the Free-Enslaved gap's narrowing accelerated over the Civil Rights Movement (1954–1968). Existing evidence suggests that the social mobility of Black Americans temporarily increased around 1970 (Clark, 2014).[42]

TABLE 2: Free-Enslaved Gap (2000)

	HS Degree (%) Mean: 68.85	College Degree (%) Mean: 12.31	Income (USD) Mean: 29,875.58	House Value (USD) Mean: 87,921.78
Ancestor Enslaved	**-3.02***** (0.51)	**-2.45***** (0.55)	**-4,795.93***** (636.79)	**-15,755.30***** (2,462.82)
Level of outcome	Tract×Race×Sex	Tract×Race×Sex	Tract×Race	Tract×Race
Controls (age, age^2)	Y	Y	Y	Y
Adjusted R^2	0.01	0.00	0.01	0.00
Observations	26,765	26,765	26,803	25,787
Ancestor Free	*1,713*	*1,713*	*1,715*	*1,634*

Notes: This table shows the Free-Enslaved gap in 2000 neighborhood-level outcomes: high school and college degrees, median incomes, and median house values (conditional on ownership). A neighborhood is a census tract. Each person is assigned the value of the census tract in which they last lived according to administrative death records. Sample means are computed for the combined sample of the Free and the Enslaved. See Data Appendix C for details on the sample and data. Standard errors are clustered at the family level and are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

We find that in 2000, the Enslaved's descendants resided in neighborhoods with substantially lower education, income, and wealth than those of the Free descendants (see Table 2). Descendants of the Enslaved are 3.9 percentage points less likely to hold a high

[40] See Appendix Figure A.2.

[41] Our sample primarily covers 1910–1940 birth cohorts.

[42] Clark (2014) uses the last names of "underclass" Black Americans to study their representation among licensed physicians and attorneys, two "elite" occupations.

school degree and 2.6 percentage points less likely to hold a college degree. Their expected income is lower by $5,100—17 percent of the Black median income. Conditional on owning their home, their houses are worth $17,500 less—19 percent of the Black median house value. We also show that descendants of the Enslaved live in neighborhoods with one year less in life expectancy and a 16 percent higher chance of a fatal police encounter.[43] Because those estimates ignore within-neighborhood differences, they should be considered an underestimate of the actual Free-Enslaved gap today.[44]

These Free-Enslaved gaps are equal to almost half of the corresponding Black-white gaps today, yielding a lower bound for the importance of historical oppression for modern racial disparities. The Free-Enslaved gap only quantifies the *additional* disadvantage faced by those whose ancestors were enslaved until 1865 compared to those who gained freedom earlier. Many free Black families were enslaved in earlier periods, and all Black Americans faced discrimination due to slavery and Jim Crow, regardless of their specific family history. The mere difference in intensity of their experiences yields socioeconomic gaps of such enormous magnitude. Next, we turn to the drivers of this persistence.

5.3 Interpreting the Free-Enslaved Gap

Using our model from Section 4, the Free-Enslaved gap measured as $\hat{\beta}_{1940}$ in equation (5), is a consistent estimator of

$$\begin{aligned}\mathbb{E}[y_{i,1} \mid s_i = 1, X_{i,t}] - \mathbb{E}\left[y_{i,1} \mid s_i = 0, X_{i,t}\right] = \\ (\lambda + \rho)\left(\mathbb{E}\left[\alpha_{i,0} \mid s_i = 1, X_{i,t}\right] - \mathbb{E}\left[\alpha_{i,0} \mid s_i = 0, X_{i,t}\right]\right) + \\ \mathbb{E}\left[\rho\gamma^0_{\ell(i,0)} + \gamma^1_{\ell(i,1)} \mid s_i = 1, X_{i,t}\right] - \mathbb{E}\left[\rho\gamma^0_{\ell(i,0)} + \gamma^1_{\ell(i,1)} \mid s_i = 0, X_{i,t}\right] - \rho\delta.\end{aligned}$$

Intuitively, the Free-Enslaved gap, therefore, reflects 1) any potential differences in ability between the two groups transmitted over generations, 2) different exposure to locations over time (as a result of slavery and potential selection), and 3) the inherited disadvantage of descending from an enslaved person conditional on environment and ability. In the next section, we show that the two groups' differential exposure to locations due to slavery—not selection—accounts for virtually all of the Free-Enslaved gap.

[43] See Appendix Table A.7.

[44] While we show that the distribution of deaths across space in our data highly correlates with population density ($\rho = 0.91$), we confirm that our results are robust to dropping neighborhoods with excess deaths (see Table B.10).

6. The Importance of Geography in Shaping Black Economic Progress After Slavery

In this section, we use Black Americans' enslavement location as a plausibly exogenous variation to identify what fraction of the Free-Enslaved gap can be accounted for by differences in exposure to place-specific factors. We find that differential exposure to state-specific factors accounts for virtually all of the Free-Enslaved gap after 1940.

6.1 States' Effect on Black Economic Progress After Slavery

We estimate each state's causal effect on the long-run economic progress of Black families freed there in 1865. Our empirical strategy to identify the importance of geography builds on the following assumption, which we discuss in detail in the full version of the paper.

Assumption 1 (Exogeneity of enslavement location)**.** *The enslaved population was not selected into location. That is,*

$$\alpha_{i,0} \perp\!\!\!\perp \ell(i,0) \;\; \textit{if} \;\; s_i = 1$$

where s_i is a dummy variable equal to 1 if one's ancestor was enslaved up to 1865, $\ell(i,0)$ is the birthplace of one's enslaved ancestor, and $\alpha_{i,0}$ is the innate "ability" of one's enslaved ancestor.

We limit our sample to families whose ancestors were enslaved until the Civil War and estimate the causal effect that the geographic distribution of formerly enslaved ancestors had on the Black economic progress of their descendants:

$$y_i = \eta_{\ell(i,1865)} + \phi' X_i + \epsilon_i, \tag{6}$$

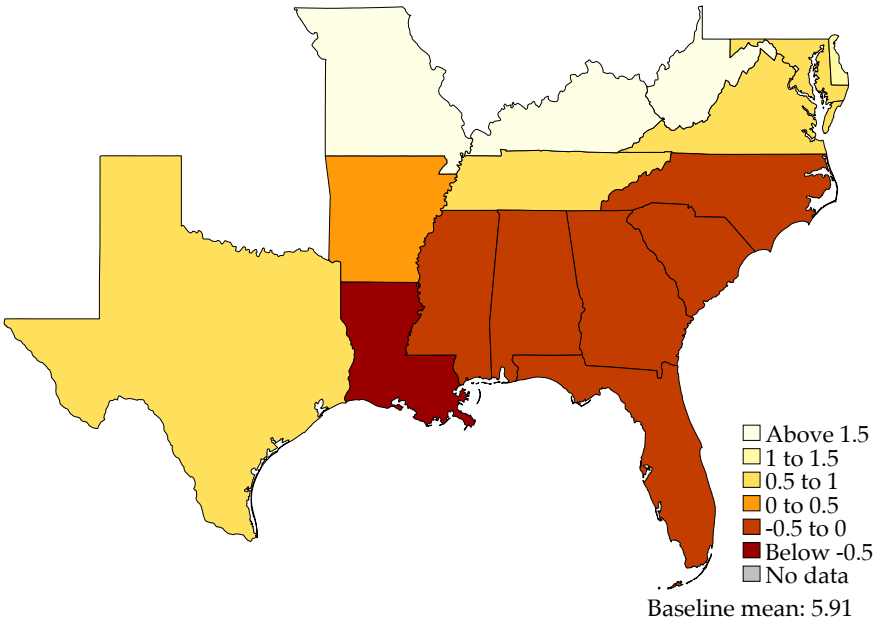
where y_i are socioeconomic outcomes in 1940 and X_i is a vector of controls as defined in equation (5). In the context of the model introduced in Section 4,

$$\eta_\ell = \rho\gamma^0_\ell + \mathbb{E}[\gamma^1_{\ell(i,1)} \mid s_i = 1, \ell(i,0) = \ell, X_i], \tag{7}$$

where γ^0_ℓ and γ^1_ℓ are the effects that location ℓ had on Black families during and after slavery respectively. Thus, η_ℓ reflects both the (inherited) effect the state of birth ℓ had on the ancestor during slavery and the expected effects of future locations of their descendants given the 1865 location. One can interpret η_ℓ as an intent-to-treat (ITT) effect of living in location l from before the Civil War to 1940, where the initial location is plausibly randomly assigned, but the post-1865 location is a result of endogenous (and potentially selective) migration decisions.

The effect of being freed in each state in 1865. We find a distinct geography of Black economic progress after slavery (see Figure 4). Gaining freedom in a state further south

FIGURE 4: Effect of Ancestor's State of Emancipation on 1940 Years of Education



Notes: This figure shows the 1870 ancestor state of birth fixed effect (FE) estimates on 1940 years of education for descendants of the Enslaved. A state's FE is the deviation from the population-weighted average across all states (baseline mean) after controlling for a quadratic function of age. The sample includes Black prime-age (20–54) men. See Data Appendix C for details on the sample and data.

substantially negatively affected Black families' socioeconomic outcomes in the long run. For example, a family freed in Louisiana would attain over two years more education had they instead been freed in, say, Kentucky. States affect other outcomes, such as literacy, income, or wealth, with similar magnitudes.[45] States' effects are substantial even in 2000 when, for example, families freed in Louisiana have average incomes lower by over one-quarter of the average income among Black Americans compared to those rooted in states along the border to the North.[46]

We also estimate the causal effect of counties rather than states, suggesting that the factors that shape the geography of Black economic progress are *race*- and *state*-specific. We estimate those more granular effects based on the county in which a formerly enslaved family's ancestors lived in 1870—five years after the end of slavery. We find that the county-level effects are highly clustered in the shape of states, yielding very similar spatial patterns to our state-level effects.[47] We also compare those causal estimates with the non-causal counterpart for white Americans and descendants of free Black Americans to assess whether race-neutral or race-specific factors likely drive the geography of Black economic progress. We find that the effects of formerly enslaved and white Americans are uncorrelated ($\rho = 0.05$), even for poor white Americans ($-0.04 \leq \rho \leq 0.15$).[48] We use

[45] See Appendix Figure A.6.
[46] See Appendix Figure A.7.
[47] See Appendix Figure A.8.
[48] See Appendix Figure A.9.

Bayesian shrinkage to show that noise does not drive the lack of correlation.[49] In contrast, estimates for the Free are highly correlated with our causal estimates ($\rho = 0.84$).[50] We also explore a large array of county-specific correlates—from Black school access and Union Army presence during Reconstruction to the number of lynchings and the prevalence of sharecropping.[51] Black school access stands out as a strong and robust predictor of Black economic progress.

The effect of living in each state between 1865 and 1940. Based on Assumption 1 and the mild additional assumption that place-specific experiences during slavery ceased to affect descendants in 1940 directly ($\rho\gamma_\ell^0 = 0$), we can recover the effect that living in each location ℓ between 1865 and 1940 had on the economic progress of Black families (γ_ℓ^1).[52] This exercise applies standard methods in the context of multiple instruments (location assignment) and imperfect compliance (migration). Specifically, the intent-to-treat effect of initial location ℓ, η_ℓ, is the average of all potential future locations' treatment effects, $\gamma_{\ell'}^1$, weighted by the probability of migrating from ℓ to ℓ':

$$\eta_\ell = \sum_{\ell' \in \mathcal{L}} p_{\ell,\ell'} \cdot \gamma_{\ell'}^1.$$

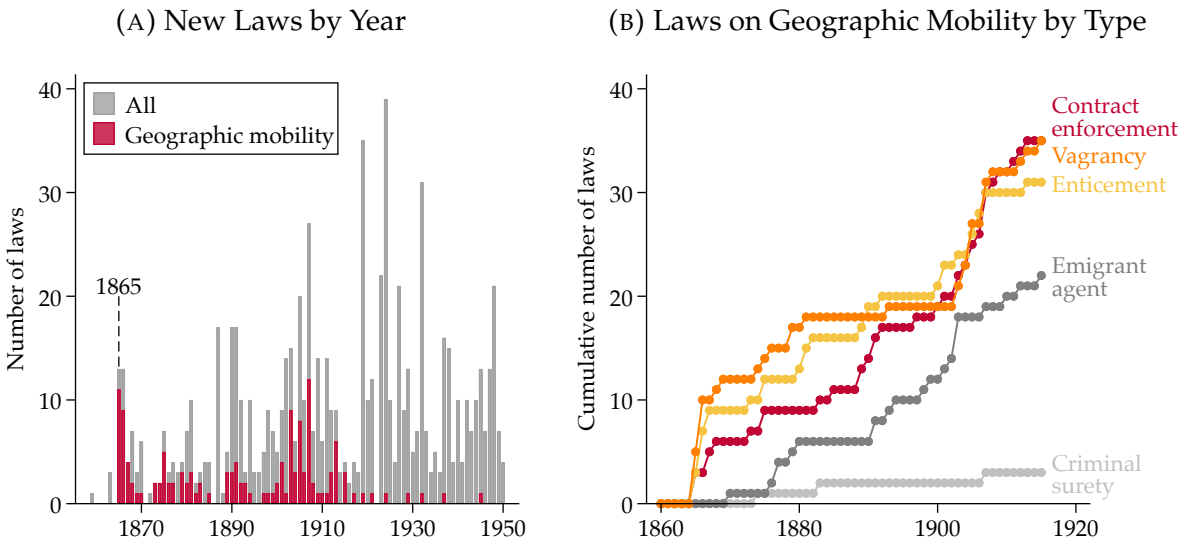
We invert the migration probability matrix to recover the effect of living in each state until 1940, which is unaffected by selective migration under the assumption that the average innate ability of Black Americans did not differ across enslavement locations.

We find that the effect of being freed in location ℓ is a very good proxy for the treatment effect of living in ℓ from 1865 to 1940.[53] Due to low geographic mobility, the recovered estimates are almost identical to the intent-to-treat effects estimated using equation (6).

With Black families freed in the Lower South faring so much worse than those freed elsewhere, it may seem puzzling why so few left the region while large fractions of those freed in the Upper South migrated to the North. For example, 75 percent of Black families enslaved in Louisiana still lived there in 1940; less than 10 percent reached the North.[54] Institutional and economic factors partly resolve this puzzle.

[49] See Appendix B.8.
[50] See Appendix Figure A.9.
[51] See Appendix Figure A.10.
[52] We formalize this decomposition in Appendix B.7.
[53] See Appendix B.7.
[54] See Appendix Figures C.45 and C.46. Southern white families were almost 30% more likely to migrate.

FIGURE 5: Number of Jim Crow Laws Across the South



Notes: This figure shows the number of new Jim Crow laws passed across all Southern states each year (Panel A) and the cumulative number of laws pertaining to the geographic mobility or employment of Black Americans by type (Panel B). See Data Appendix C for details on the data.

First, Jim Crow directly targeted the geographic mobility of Black people (Roback, 1984; Cohen, 1991; Naidu, 2010): Enticement laws and contract enforcement laws limited Black workers' ability to terminate their employment contracts; Vagrancy laws criminalized being out of employment; Emigrant-agent laws prevented employers from seeking workers from other states; Criminal surety laws created the possibility of involuntary servitude upon arrests for minor charges (see also Blackmon, 2009). These laws began emerging immediately after slavery (see Figure 5).

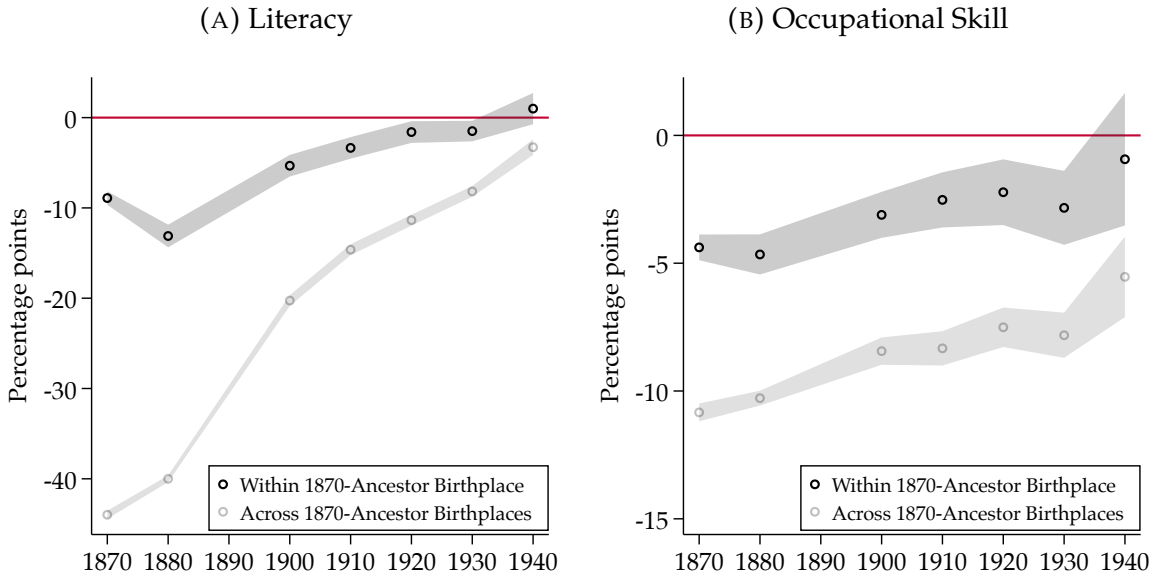
Second, moving to the North was costly, especially from the Lower South. Their geographic distance to the North limited the potential for social networks that would lower the cost of migration (Carrington et al., 1996). Even for Black families who did migrate to the North, opportunities were elusive (Collins, 1997; Akbar et al., 2020; Derenoncourt, 2022).

6.2 The Free-Enslaved Gap is Driven by Geography

To assess the importance of differential exposure of states with different effects on Black economic progress, we compute the Free-Enslaved gap conditional on ancestor location. To do so, we add fixed effects for the state of birth ℓ of a family's ancestor before 1865 to our baseline specification in equation (5). This exercise provides a back-of-the-envelope assessment of how important geography was in shaping the Free-Enslaved gap's long-run persistence, as it does not account for free Black Americans' potential selection into states before 1865. In the next section, we provide a lower bound for the importance

of state-specific factors, which is robust to potentially selective selection among the Free before 1865 and therefore has a causal interpretation.

FIGURE 6: Free-Enslaved Gap Conditional on Ancestor State (1870-1940)



Notes: This figure shows the gaps in literacy and skill before (light) and after (dark) including fixed effects for 1870 ancestor state of birth. The comparison is made between prime-age (20-54 years) male descendants of free vs. enslaved Black Americans in each census decade. Only observations that can be linked to the 1850, 1860, 1870, or 1880 census are included, minimizing bias due to the fact that the Free by definition have a link to 1850 or 1860. Both panels control for age and include 95 percent confidence bands clustered at the family level. See Data Appendix C for details on the sample and data.

We find that in contrast to the unconditional Free-Enslaved gap, the conditional gap was large in 1870 but shrunk to virtually zero after 1940 (see Figure 6).[55] The 1940 gap in literacy, for example, fully closes after accounting for variation across ancestor states. Similarly, the conditional Free-Enslaved gap in 2000 is insignificant for all outcomes.[56] These results suggest that the Free-Enslaved gap persists solely because the two groups were exposed to different state-specific factors after slavery.

With the ancestor state accounting for the vast majority of the Free-Enslaved gap, there is little room for other factors—such as differences in ability or the inherent disadvantage of being enslaved longer conditional on place and ability—to drive this within-race disparity after 1940. It is ancestor *states* that explain the Free-Enslaved gap, not other levels of ancestor geography.[57] We find that the gap conditional on ancestor *region* is still large after 1940, suggesting that the Free-Enslaved gap is not merely a result of North-South differences. Adding ancestor *county* fixed effects does not further explain

[55]The 1940 gaps in almost any other outcome also shrink to zero after conditioning on the 1870 state of origin (see Appendix Figure A.3 and Appendix Tables A.4). Individual states do not drive these findings (see Appendix Figure A.5).

[56]By 2000, the conditional gap is zero in all outcomes—again in stark contrast to the unconditional gap (see Appendix Table A.5).

[57]See Appendix Figure A.4.

the Free-Enslaved gap, suggesting that it is not geographic granularity that makes states an important explanation.

To assess the potential bias that may result from selection into freedom on observable characteristics, we re-estimate the Free-Enslaved gap using only the Free who had not accumulated any measurable physical or human capital by the end of slavery. Even for this subsample, we find large Free-Enslaved gaps in 1940.[58] This result provides additional evidence against the importance of selection into freedom for the Free-Enslaved gap's persistence.

7. The Jim Crow Effect

Thus far, we have shown that the Free-Enslaved gap is driven almost exclusively by the two groups' differential exposure to state-specific factors. This section assesses to what degree Jim Crow shapes those state-specific factors. To identify the effect of Jim Crow, we use a border discontinuity design that compares the socioeconomic outcomes of Black families freed across state borders with more or less stringent Jim Crow regimes. We find that Jim Crow single-handedly shaped the geography of Black economic progress after slavery.

7.1 State Institutions and Black Economic Progress After Slavery

Places may affect families' human capital (and other outcomes) for many reasons, be it cultural, climatic, economic, or institutional. We argue that only institutions change sharply at state borders, while other factors vary continuously. Therefore, to distinguish the effects of institutions from those of other factors, we decompose the location-specific parameters in equation (1):

$$\gamma_\ell^t = \gamma_{\boldsymbol{\epsilon}(\ell)}^t + \gamma_{s(\ell)}^t, \tag{8}$$

where $\gamma_{\boldsymbol{\epsilon}(\ell)}^t$ captures factors that vary continuously across state borders and $\gamma_{s(\ell)}^t$ captures factors that vary discontinuously across state borders. We can think of $\boldsymbol{\epsilon}(\ell)$ as the geographic coordinates of location ℓ; and $s(\ell)$ as the side of any relevant border that ℓ is on.[59] In the next section, we propose a border discontinuity design to separate the effect of institutions, $\gamma_{s(\ell)}^t$, from the effect of non-institutional factors, $\gamma_{\boldsymbol{\epsilon}(\ell)}^t$.

[58] See Appendix Table A.3.

[59] Formally, $||\boldsymbol{\epsilon}(\ell) - \boldsymbol{\epsilon}(\ell')|| \to 0 \;\Rightarrow\; |\gamma_{\boldsymbol{\epsilon}(\ell)}^t - \gamma_{\boldsymbol{\epsilon}(\ell')}^t| \to 0$, whereas $\gamma_{s(\ell)}^t$ only depends on which side of a border ℓ is on, not on the precise coordinates $\boldsymbol{\epsilon}(\ell)$: $\gamma_{s(\ell)}^t = \gamma_s^t$.

7.2 Border Discontinuity Design

Our border discontinuity design compares the socioeconomic status of families in 1940 whose ancestors were freed on different sides of (but in close proximity to) state borders within the South in 1865.[60] We provide quantitative evidence that culture, climate, and economic activity do not vary discontinuously across state borders. We therefore argue that our border discontinuity design isolates the effect of institutions.

The border discontinuity design takes the following form:

$$y_{i,b}^{1940} = \alpha_b + \beta_b \cdot \text{High}_{i,b}^{1870} + \upsilon_b \cdot dist_{i,b}^{1870} + \psi_b \cdot dist_{i,b}^{1870} \cdot \text{High}_{i,b}^{1870} + \varepsilon_{i,b}, \tag{9}$$

separately for each border $b \in B^{South}$, where $y_{i,b}^{1940}$ is the years of education of Black person i in 1940 whose ancestors were freed close to state-border b, $\text{High}_{i,b}^{1870}$ indicates whether i's 1870 ancestors lived on the side of border b that had a more intensive Jim Crow regime than the state on the other side of the border, and $dist_{i,b}^{1870}$ is the distance between border b and the county's centroid in which i's ancestors lived in 1870.[61] The main coefficient of interest, β_b, captures the long-run effect of being freed on the more oppressive side of border b on a Black family's human capital.

Jim Crow regimes differ more drastically across some state borders than others. We therefore next relate each border discontinuity estimate $\widehat{\beta}_b$ to the states' difference in Jim Crow intensity. Importantly, Jim Crow comprised de jure and de facto tactics, both of which critically contributed to the political exclusion of Black Americans (Woodward, 1955; Acemoglu and Robinson, 2008). We approach the challenge of measuring Jim Crow regimes' intensity from different angles by using both de facto and de jure proxies: the number of Jim Crow laws, the HRR index, and Black school quality. The measures are highly correlated, despite their different natures.[62]

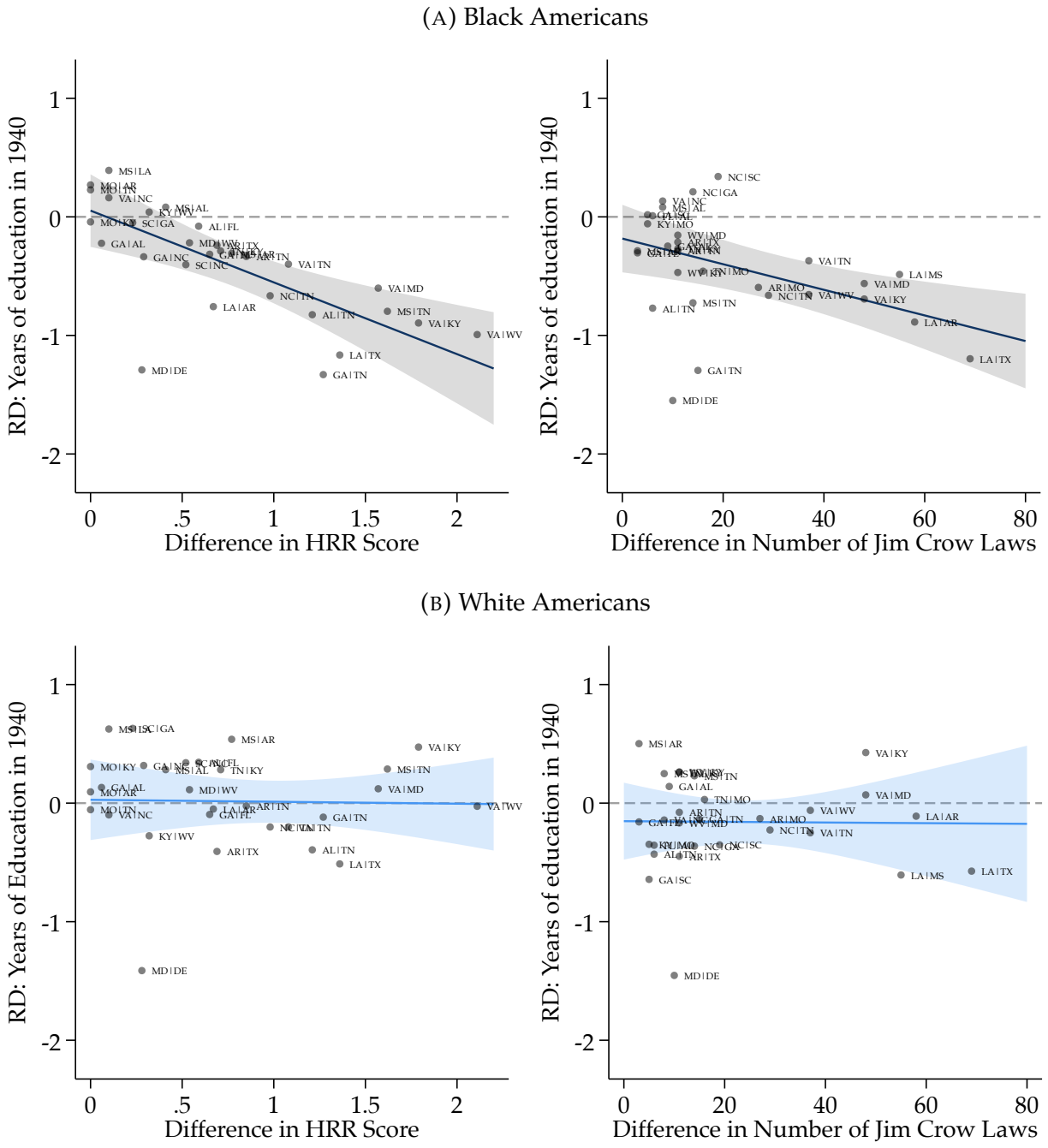
We find that Black families freed in states with more oppressive regimes experienced sharply lower rates of economic progress starting in the Jim Crow era (see Panel A of Figure 7). The resulting differences in socioeconomic status are increasing in the differences in Jim Crow intensity across a border. For example, consistent with Louisiana's Jim Crow regime being far stricter than Texas's, we find that families freed in Louisiana attained 1.2 years less education by 1940 than families freed only a few miles away in Texas. Consistent with our results for human capital, we find that living under more oppressive Jim Crow regimes reduced Black families' incomes and increased their likelihood of being a

[60] See Appendix Figure B.24.

[61] We thereby allow the slope coefficients of the distance to the border to flexibly vary for each state-border pair.

[62] See Appendix Figure C.37. For example, the number of Jim Crow laws and the HRR score have a correlation of $\rho = 0.71$; the HRR score and our composite measure of Black school quality have a correlation of $\rho = -0.94$.

FIGURE 7: Regression Discontinuity Estimates and Jim Crow



Notes: Panel (A) of this figures shows each separate RD estimate in 1940 years of education for Black families freed on different sides of state borders in 1865. Panel (B) shows the same for white families depending on where there ancestors lived in 1870. Each label shows the more oppressive before the less oppressive state. Negative estimates reflect lower education in the more oppressive state. Lines show the best linear fit between RD estimates and the differences in Jim Crow intensity, weighted by the inverse of the estimates' standard error. Shaded areas represent robust 95 percent confidence bands. See Data Appendix C for details on the sample and data.

farmer in 1940.[63] No differences emerge for families freed across borders between states with similar institutions. Adding a large set of controls for local demographics, slave characteristics, crop suitability, and economic activity in 1860—if anything—sharpens these results.[64]

We also find that, as expected, the longer a family was exposed to a Jim Crow regime, the larger the effect on their socioeconomic status.[65] Families who left their state of origin early after slavery were not exposed to the Jim Crow regime there and thus were unaffected by its intensity. However, if a family stayed and became exposed to the Jim Crow regime, the exposure had a persistent effect even for families who migrated in later decades. For instance, families freed in states with strict Jim Crow regimes and who stayed there until 1920 were still strongly impacted by their pre-1920 experiences in 1940.

In principle, Jim Crow may have not only harmed Black Americans, but it could also have affected white Americans. First, some Jim Crow laws likely directly affect some poor white Americans negatively. For example, poll taxes aimed at disenfranchising Black voters may also have disenfranchised some poor white voters. Second, Jim Crow may have benefited white elites. For example, vagrancy and emigrant-agent laws depressed farm workers' wages, potentially increasing land-owning families' profits.

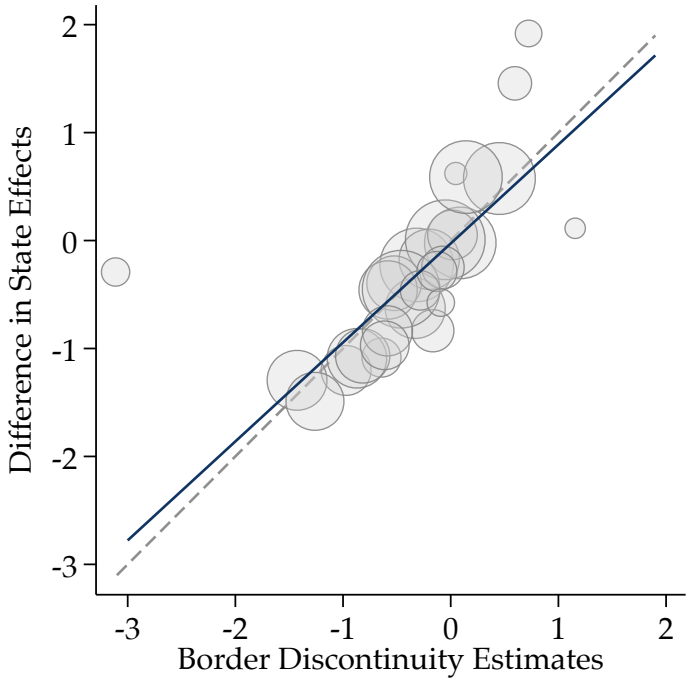
We find that in contrast to Black families, the socioeconomic status of white families was not negatively affected by the Jim Crow intensity of the state in which their ancestors lived in 1870 (see Panel B of Figure 7). The same is true even for poor white Americas whose ancestors had no measurable human or physical capital in 1870.[66] Our findings are consistent with existing evidence of Black Americans being the main beneficiaries of ending Jim Crow through the Civil Rights legislation (Wright, 2013). While there is no systematic relationship between state-led Jim Crow institutions and the education of white Americans, a few individual states led to worse outcomes for both Black and white Americans. For example, both groups fared substantially worse in Maryland compared to Delaware. These gaps may point to differences in their school system or other state institutions that are orthogonal to Jim Crow.

We do, however, find *positive* effects for the white land-owning elite. We find that the more oppressive a Jim Crow regime is, the more economically significant the gains by the border region's wealthiest ten percent of white families.[67] In sum, our results suggest that Jim Crow was an extractive institution that benefited the wealthiest white families at the cost of Black families while shielding poor white families from direct harm.

To assess to which degree Jim Crow institutions shaped the geography of Black eco-

[63] See Appendix Figure A.17.
[64] See Appendix Figures B.22 and B.23.
[65] See Appendix Figure A.20.
[66] See Appendix Figure A.18.
[67] See Appendix Figure A.19.

FIGURE 8: Jim Crow Single-Handedly Shaped State Effects



Notes: This figures relates each RD estimate to the difference in the two states' overall causal effect on 1940 years of education. Estimates are weighted by the population of ancestors born close to the respective border. A gray dashed 45 degree line shows the benchmark of equal differences across two states and across the border counties of two states. The blue line shows the best weighted linear fit. See Data Appendix C for details on the sample and data.

nomic progress, we compare the differences in progress that arise sharply at the border with the overall differences between states' effects (as documented in Figure 4). We find that the border discontinuity estimates are almost identical to differences in neighboring states' overall long-run effects on Black economic progress (see Figure 8). This finding suggests that the geography of Black economic progress is entirely driven by institutional factors, not by factors that vary continuously—such as economic activity, culture, agriculture, or population density.

The end of slavery led to a drastic change in the geography of racially oppressive institutions in the US. State governments took the leading role in passing Jim Crow laws designed to limit the economic progress of newly freed enslaved families. Our results show that state institutions became crucial in determining how likely a Black family was to experience severe forms of oppression over the next century, shaping Black families long-run economic progress. In the next section, we confirm that our border discontinuity design isolates the Jim Crow effect without being confounded by other factors.

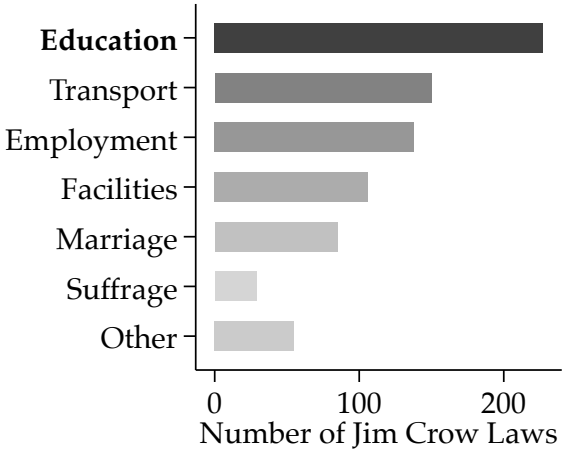
8. The Mechanism of Limited Access to Education

This section assesses restrictions to human capital as a mechanism of how Jim Crow reduced Black economic progress in the long run.

8.1 Jim Crow Regulated Black Family's Access to Education

We use our newly built database on laws and their content to gauge the relative importance of different domains that Jim Crow laws affected. We document that the most significant number of laws pertains to education, accounting for one-third of all Jim Crow laws passed across the South until 1950 (see Figure 9).[68]

FIGURE 9: Jim Crow Laws by Type



Notes: This figure shows the number of Jim Crow laws across Southern states that pertain to each category.

Jim Crow laws on education established the provision of resources for new schools or colleges for white Americans only. They also required the racial segregation of existing schools or local school boards to comprise only white people. Even school books were regulated, stipulating that once a Black or white child had used a book, children of the other race were not allowed to use the same book. Those laws likely created drastic differences in the educational resources available to Black and white children. Indeed, suggestive of this link, we find a robust negative correlation between a state's number of education-specific Jim Crow laws and the quality of Black schools ($\rho = -0.70$).[69]

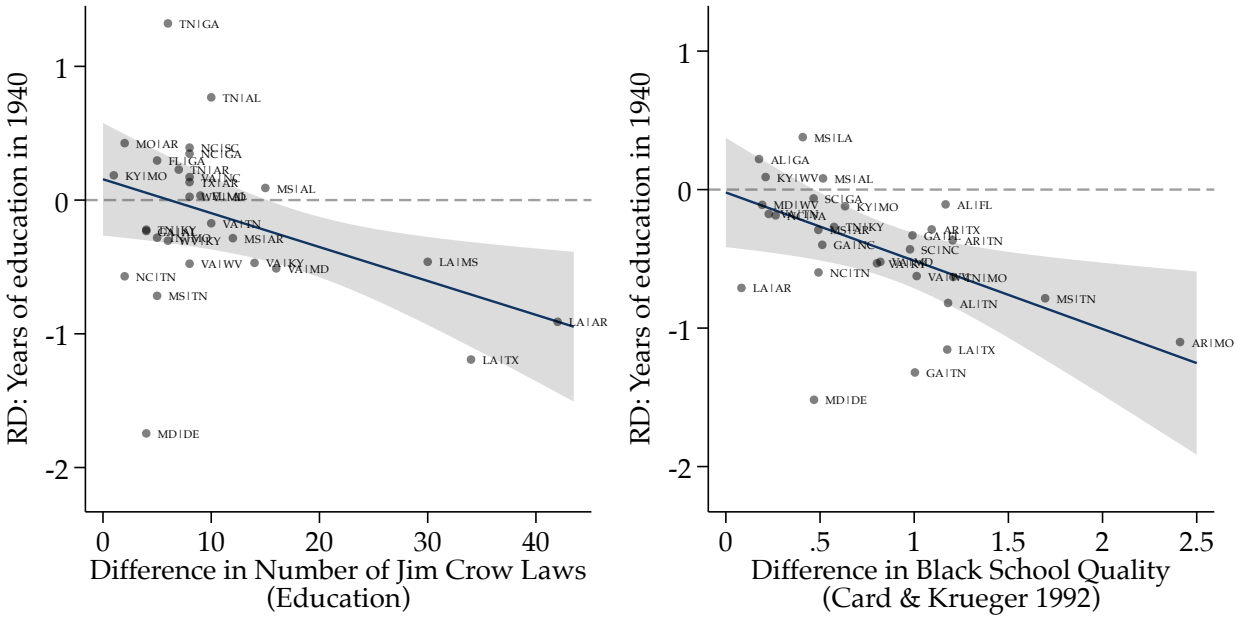
[68] A category's number of Jim Crow laws is not a conclusive measure of its importance; suffrage laws are a prime example. Suffrage laws are low in number, but their effects are massive (see e.g., Naidu, 2012). Laws in other categories are likely a downstream outcome of Black voter disenfranchisement (Engerman and Sokoloff, 2011). Therefore, while the number of Jim Crow laws on education is extensive, only through the following analysis can we conclude that they were a crucial part of states' Jim Crow regimes.

[69] We extract a principal component from three measures of Black school quality by the state prior to 1940: student-teacher ratios, term lengths, and teacher wages (Card and Krueger, 1992).

Leading scholars have pointed out the importance of Jim Crow in limiting Black families' long-run human capital accumulation. Booker T. Washington writes that "few people [have an] idea of the intensive desire which [Black people] showed for education. It was a whole race trying to go to school" (Washington, 1907). However, Black people's desire for education was met with resistance. "[Black Americans'] attempts at education provoked the most intense and bitter hostilities as evincing a desire to render themselves equal to the whites" (Freedmen's Commission Report cited in Du Bois, 1935, p. 645). Robert Higgs argues that governments were the leading force of this resistance:

> *"Most damaging of all [racial discrimination after slavery] was the discriminatory behavior of the southern state and local governments. By providing only scant resources for black education, public school boards helped to perpetuate illiteracy [...], and they thereby set in motion a variety of adverse effects."* (Higgs, 1989, p. 25)

FIGURE 10: Regression Discontinuity Estimates and Education under Jim Crow



Notes: This figures shows each separate RD estimate in 1940 years of education for Black families freed on different sides of state borders in 1865. Each label shows the more oppressive before the less oppressive state. Negative estimates reflect lower education in the more oppressive state. Lines show the best linear fit, weighted by the inverse of each estimate's standard error. Shaded areas represent robust 95 percent confidence bands. See Data Appendix C for details on the sample and data.

We empirically confirm the importance of education-specific Jim Crow regimes for Black economic progress by repeating our regression discontinuity design based on the number of Jim Crow laws that pertain to education and the quality of Black schools (Card and Krueger, 1992). Both measures capture the sharp differences in Black economic progress across Jim Crow regimes (see Figure 10). The finding is consistent with Card et al. (2022), who show that state-level factors induced critical differences in school

quality, especially for Black children, "helping to explain the persistence of the human capital gap between Blacks and whites."

8.2 Access to Schools Mediated Jim Crow's Negative Effects

A crucial question for the design of policies is whether access to education can successfully mediate the adverse effects of oppressive institutions. On the one hand, oppressive institutions may limit the economic opportunities, reducing the oppressed's returns to education and rendering school provision ineffective. On the other hand, oppressive institutions may limit school access despite existing demand among the oppressed, potentially making school provision an effective policy to increase economic progress.

We leverage quasi-experimental variation from the Rosenwald school program to identify the lack of resources to accumulate human capital as a mechanism through which Jim Crow slowed the long-run economic progress of Black families (Aaronson and Mazumder, 2011). This program built 5,000 new schools for Black children throughout rural communities in the South between 1914 and 1931.[70]

Relative to Aaronson and Mazumder (2011), we make three empirical innovations. First, we use the county in which a Black child's enslaved ancestors were freed, not the child's county of birth, to determine the likelihood it attended a Rosenwald school. Holding the place of treatment constant at the ancestor level addresses the concern that parents may sort into locations based on educational opportunities there.[71] Second, we use our measures of Jim Crow intensity to explore the heterogeneity of treatment effects across different institutional environments. Third, we analyze the program's intergenerational effects by following the children of potential Rosenwald school students from the 1940 census to administrative records in 2000.

In particular, we estimate

$$y_{i,1940} = \beta \cdot Rosenwald_{c(i,1870),b} + \mu_{c(i,1870)} + \mu_{s(i,1870),b} + \varepsilon_{i,1940},$$

where $y_{i,1940}$ is the 1940-outcome of child i born in year b, $c(i,1870)$ and $s(i,1870)$ are the county and state in which i's ancestors lived in 1870, $Rosenwald_{c,b}$ is the fraction of Black children born in year b who attended a Rosenwald school in county c, and μ_c and $\mu_{s,b}$ are county and state-cohort fixed effects.[72] We limit our sample to rural areas that were

[70] See Appendix Figure C.42 for a map of where those schools were built.

[71] We thereby also broaden the sample to all men, rather than men who served in WWII—Aaronson and Mazumder (2011) link the census to enlistment records which contain enlistees' county of birth.

[72] Our continuous treatment measure reflects the fraction of Black children in county c who was able to attend a Rosenwald school in the years in which child i from c was of school age (6–16):

$$Rosenwald_{c(i,1870),b} = \frac{1}{10} \sum_{t=b+6}^{b+16} \frac{RosenwaldTeachers_{c(i,1870),t} \times 45}{BlackChildren_{c(i,1870),t}},$$

the program's target. Our primary outcome is years of education, but we also consider literacy, income, and wealth.

TABLE 3: Rosenwald Schools and Education

	Years of Education in 1940 Sample mean: 5.26						
	Baseline	Placebo		Heterogeneity			
		Movers	Stayers	Few Laws	Many Laws	Low HRR	High HRR
Rosenwald exposure	**0.30*****	**0.16**	**0.61*****	**0.24**	**0.53*****	**0.20**	**0.85*****
	(0.11)	(0.13)	(0.19)	(0.27)	(0.20)	(0.15)	(0.23)
Controls	Y	Y	Y	Y	Y	Y	Y
Adjusted R^2	0.16	0.15	0.18	0.15	0.15	0.17	0.12
Observations	107,141	74,287	32,799	43,061	32,639	37,516	34,475

Notes: This table shows the effect of Rosenwald school exposure (0 to 1) on a person's years of education in 1940. Regressions control for ancestor-county FEs, and year-of-birth×ancestor-state FEs. The sample is limited to families who lived in rural areas in 1870. Columns 2 and 3 compare individuals who remained in the county of their ancestors until 1920 ("Stayers") to those who left ("Movers"). "Few Laws" considers bottom-quartile states in terms of their number of educational Jim Crow laws; "Many Laws" the top quartile. "Low HRR" considers bottom-quartile states in terms of their HRR index (least oppressive); "High HRR" the top quartile (most oppressive). Standard errors are clustered at both the county and cohort level. See Data Appendix C for details on the sample and data. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

We find that the Rosenwald school program had persistent positive effects on the economic progress of Black families, especially in the most oppressive states (see Table 3). A Black child able to attend a Rosenwald school attained 0.3 years more education than a child unable to attend.[73] The effect was more than twice as large in the most oppressive states. Our results imply that gaining access to a Rosenwald school closed 80 percent of the education gap caused by exposure to a highly intensive Jim Crow regime.[74] These findings suggest that the supply of education, not the demand for education, limited Black economic progress in the Jim Crow South.

We confirm that only families who lived in the county where their ancestors were freed until at least 1920 benefited from new Rosenwald schools built there after 1919. For families who out-migrated before 1920, a new Rosenwald school in their place of origin had, as expected, no effect on their education. For those who stayed, the effect was 0.6 years of education.

where b is a child's birth year, $c(i, 1870)$ is their 1870 ancestor county, $RosenwaldTeachers_{c(i,1870),t}$ and $BlackChildren_{c(i,1870),t}$ are the numbers of Rosenwald teachers and Black children of school age in county c at time t; 45 approximates average class size at the time.

[73] An increase of the cohort×county-specific Rosenwald exposure from 0 to 1 yields an increase in the cohort×county-specific share of Black children in school by 0.65 (F-statistic: 1,736.2).

[74] The difference in years of education between states with high and low HRR indexes is 0.8, while the difference in the effect of Rosenwald schools across those states is $0.85 - 0.20 = 0.65$.

TABLE 4: Intergenerational Effects of the Rosenwald Schools

	Children's Neighborhood Level Outcomes in 2000			
	HS Degree (%) Mean: 69.33	College Degree (%) Mean: 12.15	Income (USD) Mean: 28,831.25	House Value (USD) Mean: 95,986.10
Father's Rosenwald exposure	**2.78** (3.06)	**4.95**** (2.10)	**2,277.22**** (1,120.35)	**35,471.16**** (16,917.63)
Level	ZIP×Race×Sex	ZIP×Race×Sex	ZIP×Race	ZIP×Race
Adjusted R^2	0.03	0.03	0.03	0.03
Observations	6,420	6,420	6,434	6,434

Notes: This table shows the effect of a father's Rosenwald school exposure (0 to 1) on the residential neighborhood quality of the their children in 2000. All regressions control for ancestor-county FEs, father's year-of-birth×ancestor-state FEs, and the within-county rural/urban status of each family in 1870. Standard errors are clustered at both the county and father's cohort level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

While the educational benefits of Rosenwald school access accrued to the treated generation directly, wages did not increase—evidence consistent with low returns to education for Black Americans during the Jim Crow era (see also Wright, 2013; Mohammed and Mohnen, 2022).[75]

We find that having access to a Rosenwald school not only increased the education of the benefiting individuals but also improved the economic conditions of their children today (see Table 4). We find that the children of Black men who had access to a Rosenwald school today are 40 percent more likely to have a college degree. Consistent with the long-run gains in human capital, those descendants also earn higher incomes and own more valuable homes. Thus, the Rosenwald program's massive positive effects have persisted beyond the Jim Crow era.

9. CONCLUSION

This paper provides new evidence on the historical roots of modern racial disparities, finding that Black Americans' socioeconomic status today is lower than that of white Americans in large part due to the US's history of slavery and Jim Crow. First, we document that Black families enslaved until 1865 continue to have considerably lower socioeconomic status today. Second, we show that this persistence is driven by post-slavery oppression under Jim Crow and highlight the limited access to human capital as a critical mechanism.

We have put forward a new framework for slavery's legacy to incorporate systemic discrimination of the formerly Enslaved and their descendants under Jim Crow. The institution of slavery determined *where* a Black family was freed from slavery. We show that

[75] See Appendix Table A.8.

the state where a family was freed determined the Jim Crow regime they likely faced over the subsequent decades. After 1940, the single reason descendants of families enslaved until the Civil War have lower socioeconomic status is their concentration in the states that adopted the most strict Jim Crow regimes starting in 1877. Systemic discrimination—the higher exposure to ongoing discrimination *because of past discrimination* (Bohren et al., 2022)—is at the core of slavery's persisting legacy.

Our findings have important implications for policies that aim to reduce the disadvantage faced by descendants of the Enslaved. First, our results highlight the importance of *within-race* disparities that race-specific policies may not address. College affirmative action is a prime example. Massey et al. (2007) show that the more selective a college, the less likely Black students are to descend from the Enslaved. While only 13 percent of 18- to 19-year-old Black Americans have an immigration background, 41 percent of Black Ivy League students do. Affirmative action increases racial diversity on campuses but is less effective in alleviating disadvantages faced by descendants of the Enslaved.

Second, our results highlight that increasing the supply of educational resources to Black children can have long-lasting positive effects on their families' economic progress. We find that providing such resources is particularly effective in areas where Black children are most deprived of them, closing 80 percent of the education gap caused by Jim Crow. Development economists have documented similar evidence (e.g., Duflo, 2001; Glewwe and Kremer, 2006). Our evidence also suggests that those policies can have significant intergenerational spillovers. Ignoring those effects may lead to the design of policies below their optimal scale.

Third, there has been renewed interest in the specific policy of reparations, i.e., wealth transfers to descendants of the Enslaved (e.g., Darity, 2008; Craemer et al., 2020; Boerma and Karabarbounis, 2021). We argue that any assessment of the legacy of slavery should incorporate both when and where a family was freed—i.e., how long they were enslaved and how intensively they were exposed to Jim Crow after slavery. Our empirical evidence suggests that Black families today are impacted drastically by when and where their ancestors were freed. For that matter, we must stress again that we only quantify the *additional* disadvantage faced by those whose ancestors were enslaved until 1865 and concentrated in the Lower South compared to those who gained freedom earlier, mainly in the Upper South and North. Many free Black Americans had been enslaved in earlier periods, and all Black Americans faced discrimination due to slavery regardless of their specific family history. Note that while some argue that reparations should only be received by those who can prove their ancestors were enslaved, our results suggest that post-slavery institutions also harmed Black Americans who descended from the Free—a group that may find it harder to prove their ancestors had been enslaved decades before the Civil War.

This paper has limitations that future work may be able to overcome. First, we limit

our analysis to men because automated census-linking methods are unavailable or have poor coverage for women. Women have historically tended to change their last names upon marriage, making it impossible for conventional methods to link them across census records. While not within the scope of this paper, we link millions of women across census records in a separate project by using the information on their maiden and married names from social security applications (Althoff et al., 2022). Second, while we highlight Jim Crow laws that regulate Black families' education as a cruicial mechanism, laws pertaining to other domains may have additionally limited Black economic progress. Some of those laws have been studied more (e.g., limits to Black suffrage—see Naidu, 2012) than others (e.g., limits to interracial marriage).

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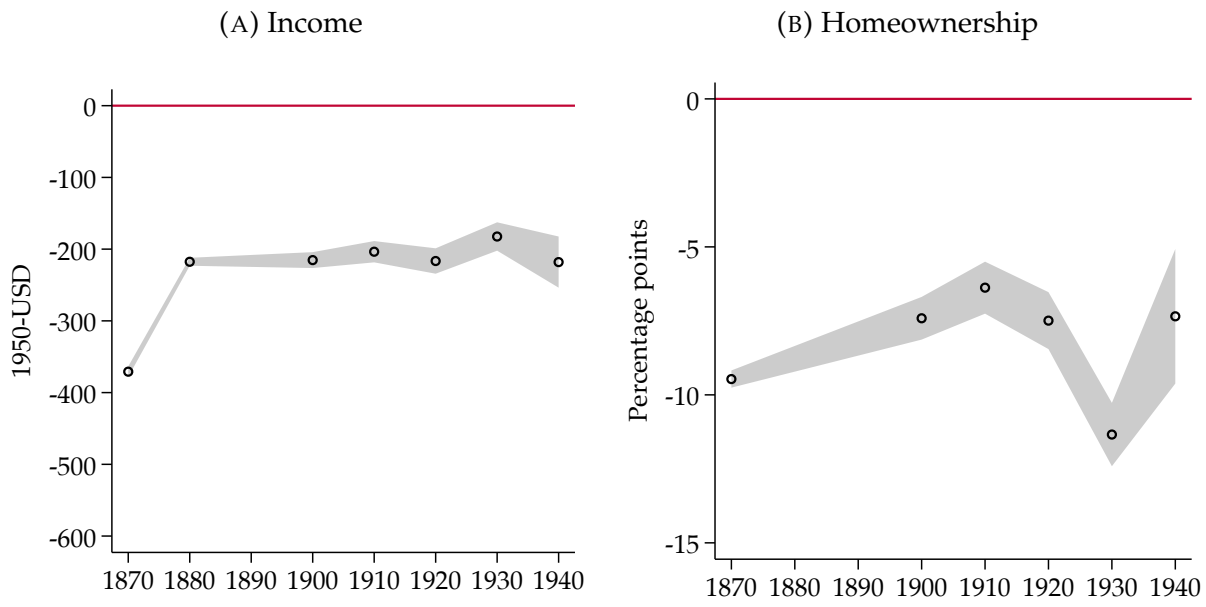
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A. Additional Results

A.1 Free-Enslaved Gap in Alternative Measures

FIGURE A.1: Free-Enslaved Gap (1870–1940)



Notes: This figure shows the gaps in income (occupational income score) and homeownership among prime-age (20-54) male descendants of free vs. enslaved Black Americans in each census decade. We restrict the sample to observations linked to ancestors in 1850, 1860, 1870, or 1880. We control for a quadratic function in age and include 95 percent confidence bands clustered at the family level. See Data Appendix C for details on the sample and data.

TABLE A.1: Free-Enslaved Gap (1940) in Different Income Measures

	OCCSCORE (1950-USD) Mean: 1,604.09	LIDO Score (1950-USD) Mean: 1,161.69	Wage Income (1940-USD) Mean: 381.20	Total Income (1940-USD) Mean: 793.47
Ancestor Enslaved	**-148.39*****	**-279.00*****	**-145.92*****	**-204.29*****
	(10.86)	(8.59)	(6.13)	(10.29)
Controls (age, age^2)	Y	Y	Y	Y
Adjusted R^2	0.04	0.04	0.05	0.09
Observations	168,138	142,743	154,463	146,871
Ancestor Free	*9,325*	*7,517*	*8,551*	*8,100*

Notes: This table shows the Free-Enslaved gap in income across different measures: Occupational income score (OCCSCORE), a refined occupational income score (Lido), wage income, and total predicted income. All estimates are for Black prime-age men in 1940. Sample means are computed for the combined sample of the Free and the Enslaved. See Data Appendix C for details on the sample and data. Standard errors are clustered at the family level and are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A.2: Free-Enslaved Gap (1940) in Different Education Measures

	Literacy (%) Mean: 91.49	Education (Years) Mean: 5.99	High School (%) Mean: 9.28	College (%) Mean: 1.70	Graduate (%) Mean: 0.46
Ancestor Enslaved	**-4.25*****	**-1.59*****	**-7.86*****	**-1.86*****	**-0.74*****
	(0.26)	(0.05)	(0.45)	(0.21)	(0.12)
Controls (age, age^2)	Y	Y	Y	Y	Y
Adjusted R^2	0.01	0.04	0.01	0.00	0.00
Observations	163,549	163,549	163,549	163,549	163,549
Ancestor Free	*9,078*	*9,078*	*9,078*	*9,078*	*9,078*

Notes: This table shows the Free-Enslaved gap in education across different measures: Literacy, years of education, and the probability of holding a high school, college, or graduate degree. As literacy is reported only until 1930, we use educational attainment beyond second grade as a proxy in 1940. All estimates are for Black prime-age men in 1940. Sample means are computed for the combined sample of the Free and the Enslaved. See Data Appendix C for details on the sample and data. Standard errors are clustered at the family level and are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A.2 Free-Enslaved Gap in Intergenerational Mobility

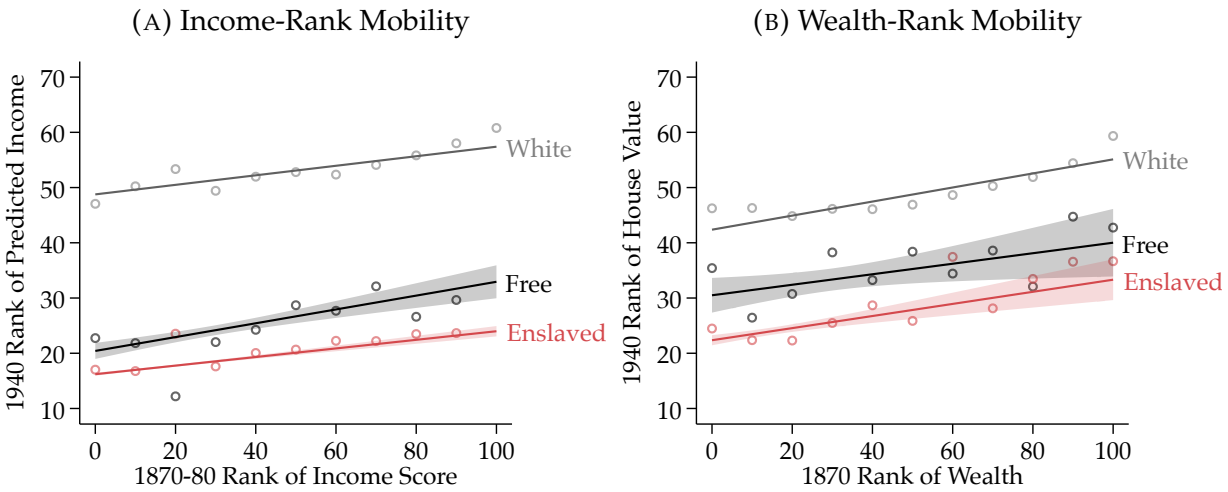
To determine if the persisting Free-Enslaved gap is purely the result of large initial differences or if are there also differences in intergenerational mobility, we estimate the intergenerational mobility of person i between 1870 and 1940 as the following:

$$r_{i,1940} = \alpha + \beta_1 s_i + \beta_2 r_{i,1870} + \beta_3 s_i \times r_{i,1870} + \varepsilon_i, \tag{10}$$

where $r_{i,1940}$ is the percentile rank of i's 1940 income or wealth in the Black distribution and $r_{i,1870}$ is the percentile rank of the income or wealth occupied by the prime-age male ancestor of i in 1870. Upward mobility is defined as the increase in percentile ranks from one generation to the next. To reduce the impact of measurement error, we use the average across 1870 and 1880 for ancestors if possible (Ward, 2021).

Figure A.2 shows that upward mobility is significantly lower for descendants of the Enslaved. Conditional on one's ancestor having the same income or wealth, descendants of the Enslaved reach lower outcomes than descendants of the Free. Both groups reach far lower outcomes than white Americans conditional own having the same socioeconomic background. This is true across all parts of the 1870–1880 income distribution and the 1870 wealth distribution.

FIGURE A.2: Gaps in Intergenerational Mobility (1870-1940)



Notes: This figure shows the estimated intergenerational mobility of free Black (in black), formerly enslaved (in red), and white prime-age (20-54) men. The left panel shows income-rank mobility conditional on the average rank of an ancestor's occupational income score (OCCSCORE) in 1870 and 1880 (Ward, 2021). The right panel shows wealth-rank mobility conditional on the rank of an ancestor's wealth (personal property and real property) in 1870. 1940 total income is predicted (see Appendix C.6). 1940 house values are measured conditional on ownership. The lines reflect the coefficients estimated in equation 10, including robust confidence bands at the 95 percent level. Bubbles reflect binned averages. Percentile ranks reflect rank that each income occupies in the year-specific national income distribution of Black prime-age men. See Data Appendix C for details on the sample and data.

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A.3 Free-Enslaved Gap for Free Without Physical or Human Capital

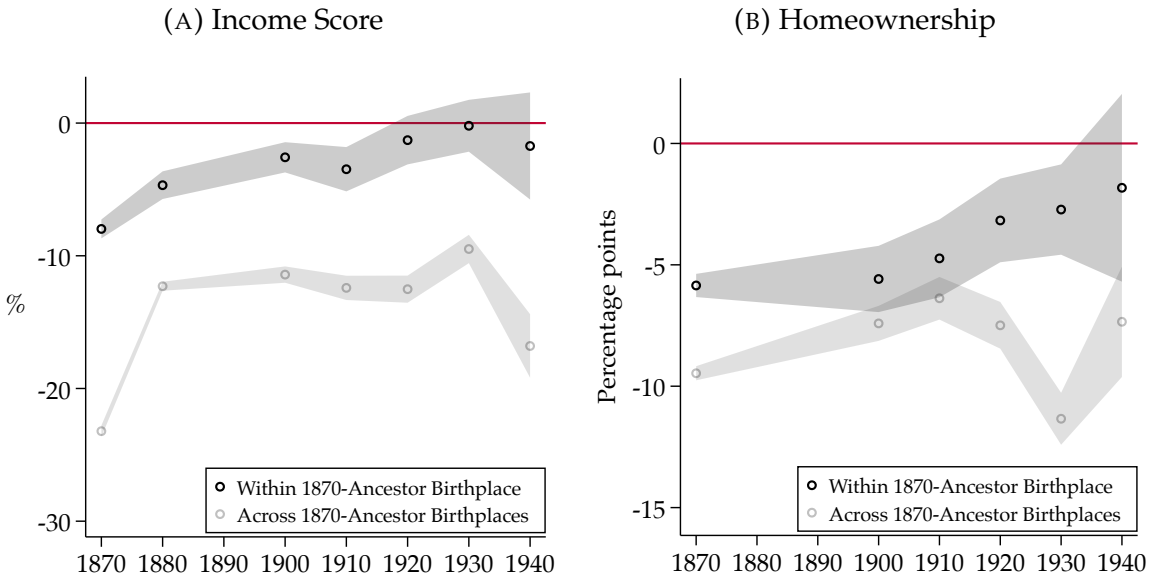
TABLE A.3: Free-Enslaved Gap (1940) for Free Without Physical or Human Capital in 1860

	Education (Years) Mean: 5.83		Wage Income (USD) Mean: 381.64		Homeownership (%) Mean: 29.08		House Value (USD) Mean: 1,380.43	
Ancestor Enslaved	**-1.00*****	**-0.12**	**-90.43*****	**26.85**	**-6.16*****	**-1.42**	**-343.74****	**440.28****
	(0.15)	(0.15)	(21.13)	(21.44)	(1.95)	(2.00)	(159.58)	(184.15)
1870 State of Birth-FE	N	Y	N	Y	N	Y	N	Y
Controls (age, age^2)	Y	Y	Y	Y	Y	Y	Y	Y
Adjusted R^2	0.03	0.07	0.04	0.07	0.01	0.02	0.00	0.03
Observations	71,574	71,574	67,672	67,672	72,013	72,013	20,455	20,455
Ancestor Free	*608*	*608*	*569*	*569*	*605*	*605*	*206*	*206*

Notes: This table shows the gap in years of education, total income, homeownership rate, and house value among prime-age (20-54) male descendants of a subset of the free vs. enslaved Black Americans in 1940. Among the Free, we only include those whose ancestors had no measurable physical capital (real and personal property) or human capital (literacy) in 1860. Only observations that can be linked to the 1850, 1860, 1870, or 1880 census are included. Columns 1, 3, 5, and 7 repeat Table 1 but hold the sample constant to the other columns. Columns 2, 4, 6, and 8 add fixed effects for 1870 ancestor state of birth. House values are measured conditional on ownership. Sample means are computed for the combined sample of the Free and the Enslaved. Appendix Figure A.3 shows the evolution of the conditional Free-Enslaved gap over time. See Data Appendix C for details. Standard errors are clustered at the family level and are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

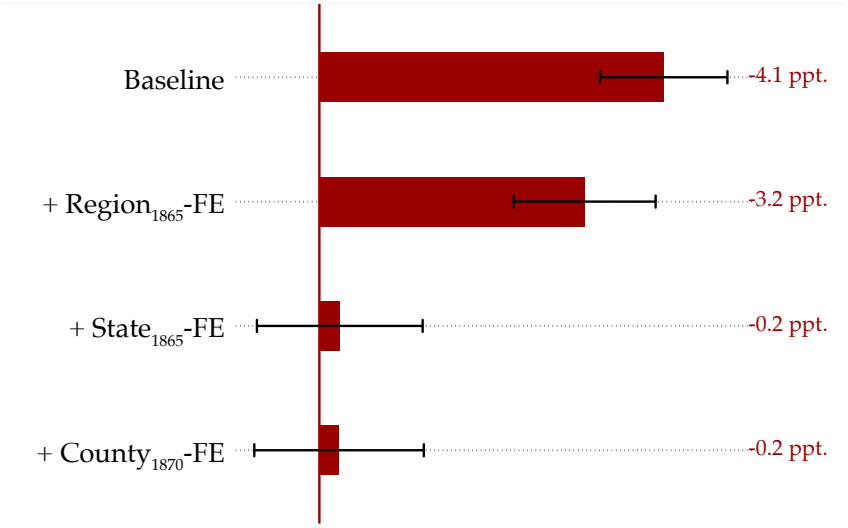
A.4 Free-Enslaved Gap between and within Ancestor's Birthplace

FIGURE A.3: Free-Enslaved Gap Conditional on Ancestor State (1870-1940)



Notes: This figure shows the gaps in literacy, income, skill, and homeownership before (light) and after (dark) including fixed effects for 1870 ancestor state of birth. The comparison is made between prime-age (20-54 years) male descendants of free vs. enslaved Black Americans in each census decade. Only observations that can be linked to the 1850, 1860, 1870, or 1880 census are included, minimizing bias due to the fact that the Free by definition have a link to 1850 or 1860. Both panels control for age and include 95 percent confidence bands clustered at the family level. See Data Appendix C for details on the sample and data.

FIGURE A.4: Free-Enslaved Gap in Literacy Conditional on Ancestor Location (1940)



Notes: This figure shows the 1940 Free-Enslaved gap in literacy before and after including different levels of origin location fixed effects. We successively add fixed effects for the region (South or North) and state a family's 1870 ancestor were born, and the county in which their 1870 ancestors lived. The sample includes only Black prime-age (20–54) men whose ancestors can be located in 1870. See Data Appendix C for details on the sample and data.

TABLE A.4: Free-Enslaved Gap (1940) between and within Ancestor's Birthplace

	Education (Years) Mean: 5.91		Wage Income (USD) Mean: 388.01		Home Ownership (%) Mean: 29.48		House Value (USD) Mean: 1,412.17	
Ancestor Enslaved	-1.49***	**-0.41*****	-137.00***	**-20.22****	-6.76***	**-1.61**	-574.06***	**8.40**
	(0.07)	(0.08)	(8.51)	(9.84)	(0.86)	(1.04)	(90.08)	(115.61)
1870 State of Birth-FE	N	Y	N	Y	N	Y	N	Y
Controls (age, age^2)	Y	Y	Y	Y	Y	Y	Y	Y
Adjusted R^2	0.04	0.08	0.04	0.07	0.01	0.03	0.01	0.03
Observations	75,583	75,583	71,474	71,474	76,048	76,048	21,873	21,873
Ancestor Free	*4,617*	*4,617*	*4,371*	*4,371*	*4,640*	*4,640*	*1,624*	*1,624*

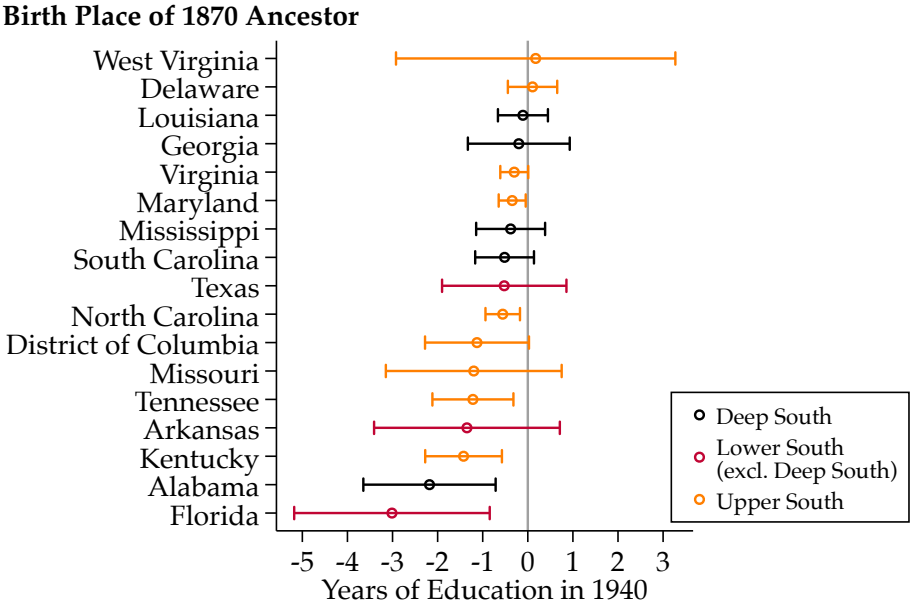
Notes: This table shows the gap in years of education, total income, homeownership rate, and house value among prime-age (20-54) male descendants of free vs. enslaved Black Americans in 1940. Only observations that can be linked to the 1850, 1860, 1870, or 1880 census are included. Columns 1, 3, 5, and 7 repeat Table 1 but hold the sample constant to the other columns. Columns 2, 4, 6, and 8 add fixed effects for 1870 ancestor state of birth. House values are measured conditional on ownership. Sample means are computed for the combined sample of the Free and the Enslaved. Figure 6 and Appendix Figure A.3 show the evolution of the conditional Free-Enslaved gap over time. See Data Appendix C for details. Standard errors are clustered at the family level and are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A.5: Free-Enslaved Gap (2000) at Census Block Level between and within Ancestor's Birthplace

	HS Degree (%) Mean: 69.20		College Degree (%) Mean: 12.32		Income (USD) Mean: 30,143.90		House Value (USD) Mean: 88,830.12	
Ancestor Enslaved	-2.57***	**-0.89**	-2.07***	**-0.29**	-5,032.50***	**-1,014.92**	-13,391.02***	**-780.04**
	(0.74)	(0.82)	(0.78)	(0.78)	(921.89)	(1,005.32)	(3,498.95)	(3,829.19)
Level	Tract×Race×Sex		Tract×Race×Sex		Tract×Race		Tract×Race	
1870 State of Birth-FE	N	Y	N	Y	N	Y	N	Y
Controls (age, age^2)	Y	Y	Y	Y	Y	Y	Y	Y
Adjusted R^2	0.00	0.01	0.00	0.01	0.01	0.03	0.00	0.02
Observations	11,931	11,931	11,931	11,931	11,932	11,932	11,500	11,500
Ancestor Free	*863*	*863*	*863*	*863*	*861*	*861*	*830*	*830*

Notes: This table shows the Free-Enslaved gap in the fraction of people who hold a high school degree, the fraction of people who hold a college degree, the median income earned, and the median house value in 2000. Columns 1, 3, 5, and 7 repeat Table 2 but hold the sample constant to the other columns. Columns 2, 4, 6, and 8 add fixed effects for 1870 ancestor state of birth. House values are measured conditional on ownership and therefore exclude zeros. Each person is assigned the respective value of the census block in which they lived at the time of death. Sample means are computed for the combined sample of the Free and the Enslaved. See Data Appendix C for details on the sample and data. Standard errors are clustered at the family level and are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

FIGURE A.5: Free-Enslaved Gap in 1940 Years of Education by 1870 Ancestor Birthplace



Notes: This figure shows the gaps between descendants of Free and Enslaved in 1940 years of education by 1870 ancestor state of birth. The comparison is made between prime-age (20-54 years) male descendants in each census decade. Only observations that can be linked to the 1850, 1860, 1870, or 1880 census are included, minimizing bias due to the fact that the Free by definition have a link to 1850 or 1860. Both panels control for age and include 95 percent confidence bands that are clustered at the family level.

TABLE A.6: Decomposition of the Free-Enslaved Gap in 1940

	Free-Enslaved gap & ancestor location			Geography's effect as % of gap	
	National	Within South	Within state	Preferred	Lower bound
Literacy (%)	-4.2	-3.2	-0.2	95%	67%
Years of education	-1.6	-1.2	-0.4	75%	50%

Notes: This table decomposes the 1940 Free-Enslaved gaps in literacy and years of education. We successively add fixed effects for the region (South or North) and state a family's 1870 ancestor were born, and the county in which their 1870 ancestors lived. Column 4 shows the fraction of the national Free-Enslaved gap (column 1) that can be accounted for by state variation (column 3). Column 5 shows the result of our formal decomposition in Section **??**. The sample includes only Black prime-age (20–54) men whose ancestors can be located in 1870. See Data Appendix C for details on the sample and data.

A.5 Free-Enslaved Gap in Mortality

TABLE A.7: Mortality Gaps between Descendants of the Free and the Enslaved (2000)

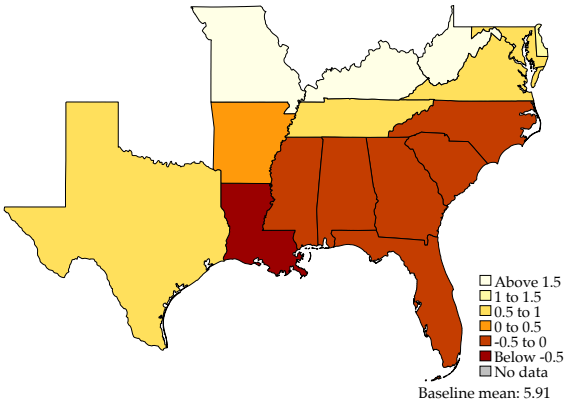
	Life Expectancy (Years) Mean: 74.94	Fatal Police Encounters (per 100K) Mean: 21.31
Ancestor Enslaved	**-1.08*****	**3.40*****
	(0.14)	(1.19)
Level	Tract	ZIP
Controls (age, age^2)	Y	Y
Adjusted R^2	0.00	0.00
Observations	26,181	25,246
Ancestor Free	*1,686*	*1,423*

Notes: This table shows the Free-Enslaved gap in life expectancy and fatal police encounters per 100,000 residents. Each person is assigned the respective value of the census tract or five-digit ZIP code in which they lived at the time of death. Sample means are computed for the combined sample of the Free and the Enslaved. See Data Appendix C for details on the sample and data. Standard errors are clustered at the family level and are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

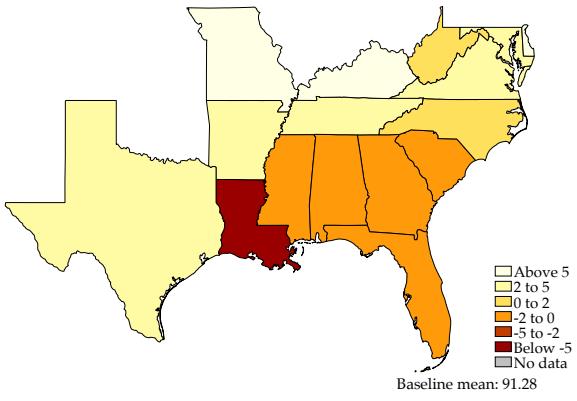
A.6 State-Level Place Effects

FIGURE A.6: Effect of 1870 Ancestor's State of Birth on 1940 Outcomes

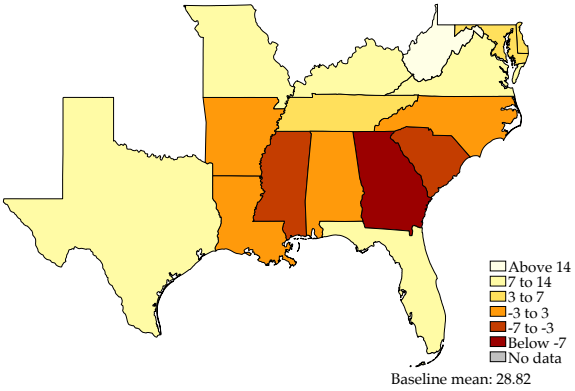
(A) Years of Education



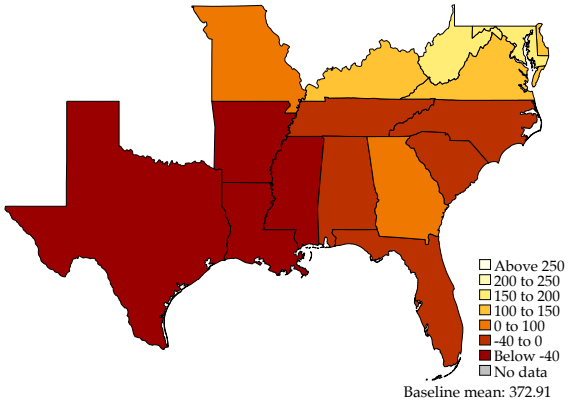
(B) Literacy Rate



(C) Homeownership Rate



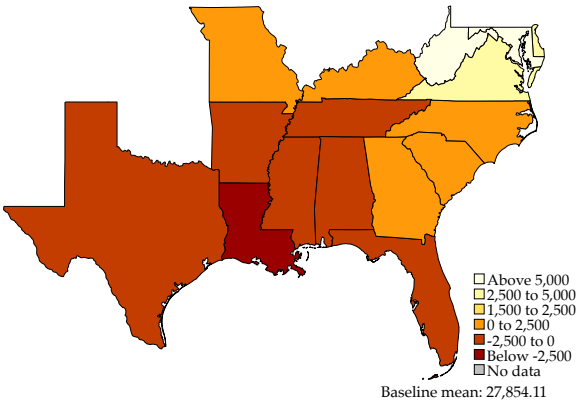
(D) Wage Income



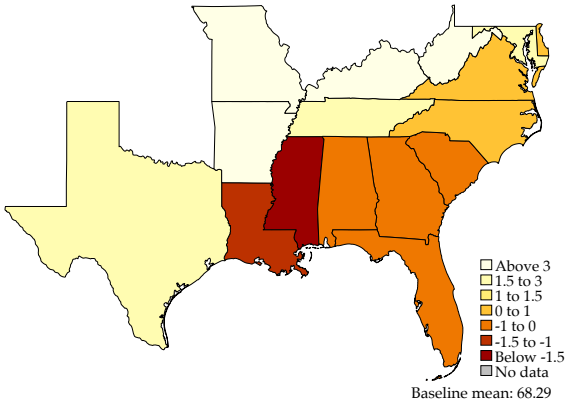
Notes: This figure shows the 1870 ancestor state of birth fixed effect estimates on years of education, literacy rate, homeownership rate, and wage income in 1940. A state's fixed effect is the deviation from the population-weighted average across all states after controlling for a quadratic function of age. The sample includes only Black prime-age men whose ancestors can be located in 1870.

FIGURE A.7: Effect of Ancestor's State of Birth of Enslavement on 2000 Outcomes

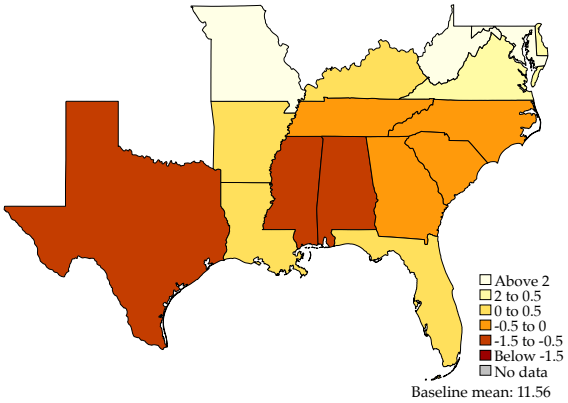
(A) Income (2000 USD)



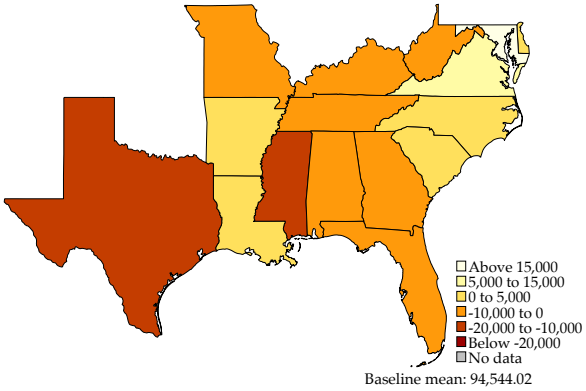
(B) High School Degree (%)



(C) College Degree (%)



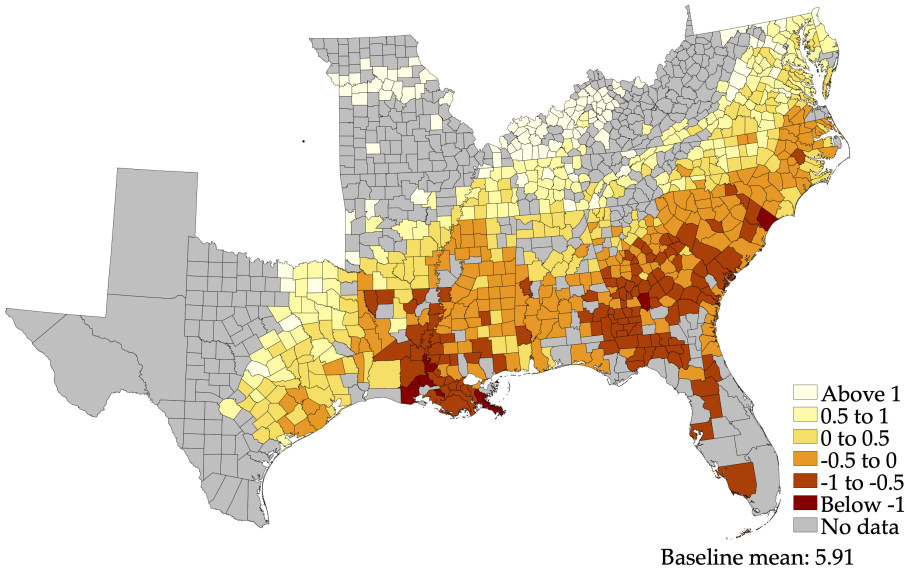
(D) House Value (2000 USD)



Notes: This figure shows the 1870 ancestor state of birth fixed effect estimates on income, the likelihood of holding a high school degree or college degree, and house values in 2000 for descendants of the Enslaved. A state's fixed effect is the deviation from the population-weighted average across all states (baseline mean) after controlling for a quadratic function of age. The sample includes men and women whose ancestors can be located in 1870.

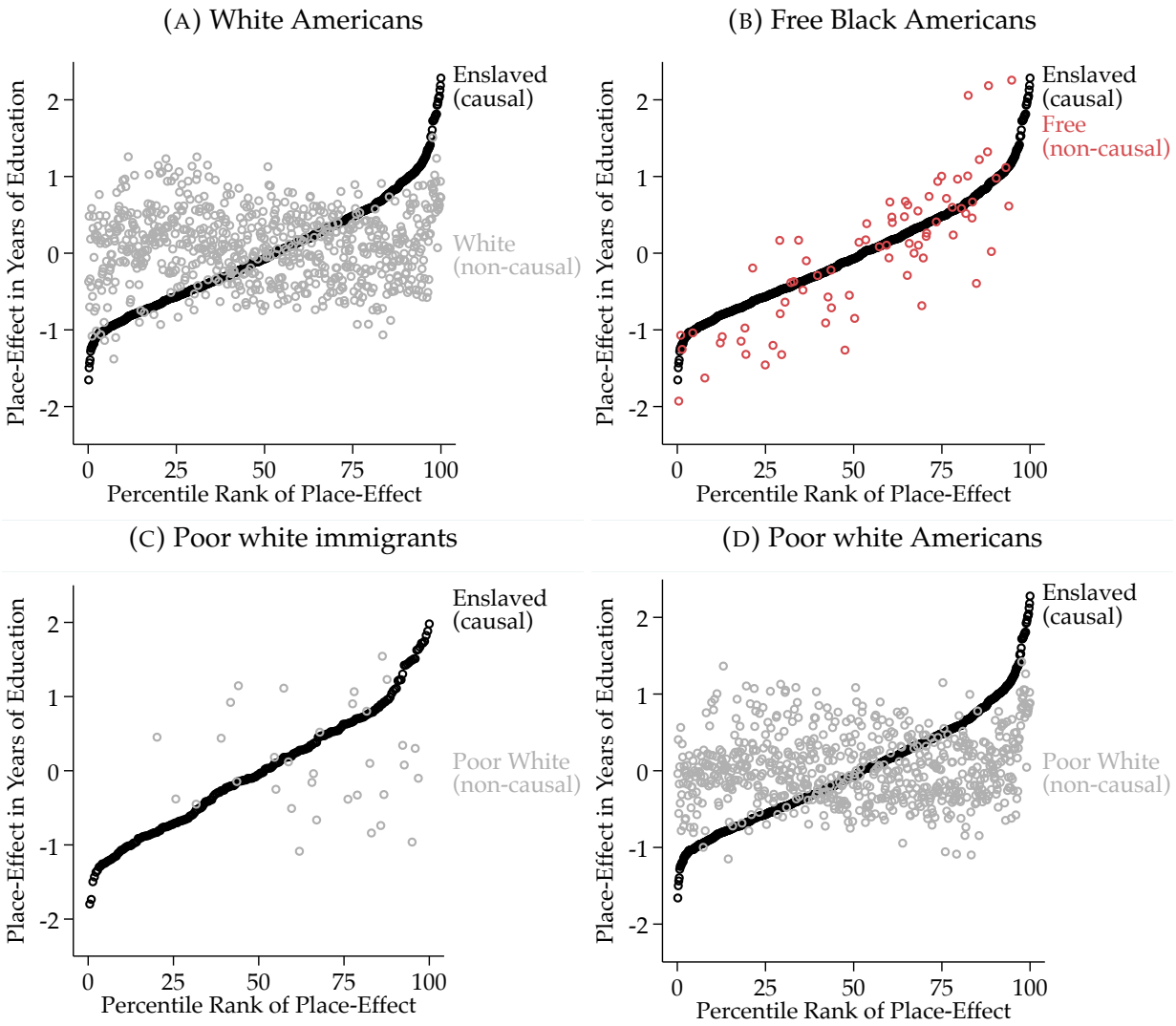
A.7 County-Level Place Effects

FIGURE A.8: Effect of 1870 Ancestor's County of Residence on 1940 Outcomes



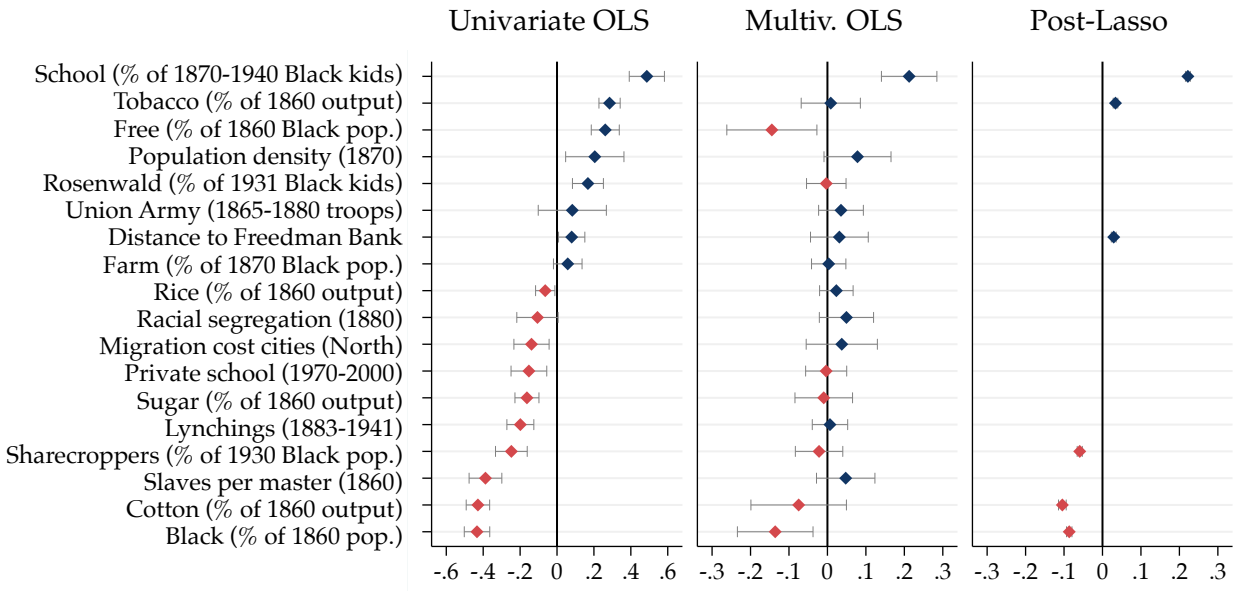
Notes: This figure shows the 1870 county of residence fixed effect estimates on years of education in 1940. A state's fixed effect is the deviation from the population-weighted average across all states after controlling for a quadratic function of age. All county fixed effects based on ten observations or less are discarded. The sample includes only prime-age (20–54) men whose ancestors can be located in 1870. We use empirical Bayesian shrinkage as described in Appendix B.8. See Data Appendix C for details on the sample and data.

FIGURE A.9: Ancestor County Effects by Race



Notes: This figure compares the 1870 ancestor county fixed effects on years of education in 1940 for descendants of the Enslaved (causal) with those of white Americans and the Free (both non-causal). The correlation of the causal place effects and the (non-causal) effects is high for free Black Americans ($\rho = 0.84$) but low for white Americans ($\rho = 0.05$), poor white immigrants ($\rho = -0.04$), and white Americans whose ancestors were illiterate and had zero wealth in 1870 ($\rho = 0.15$). A county's fixed effect is the deviation from the race-specific population-weighted average across all Southern counties after controlling for a quadratic function of age. The sample includes only prime-age (20–54) men whose ancestors can be located in 1870. We use empirical Bayesian shrinkage as described in Appendix B.8. See Data Appendix C for details on the sample and data.

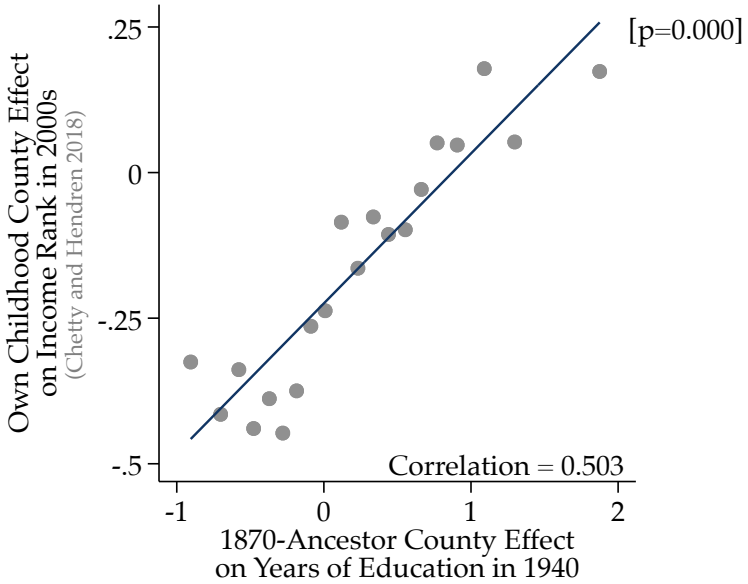
FIGURE A.10: Horse Race in Predicting Counties' Effect on Black Economic Progress



Notes: This figure runs a horse race between county-level characteristics to predict a county's causal effect on Black economic progress after slavery. Counties' effects are 1870 ancestor county fixed effects on years of education in 1940 for descendants of the Enslaved. The sample includes only prime-age (20–54) men whose ancestors can be located in 1870. See Data Appendix C for details on the sample and data.

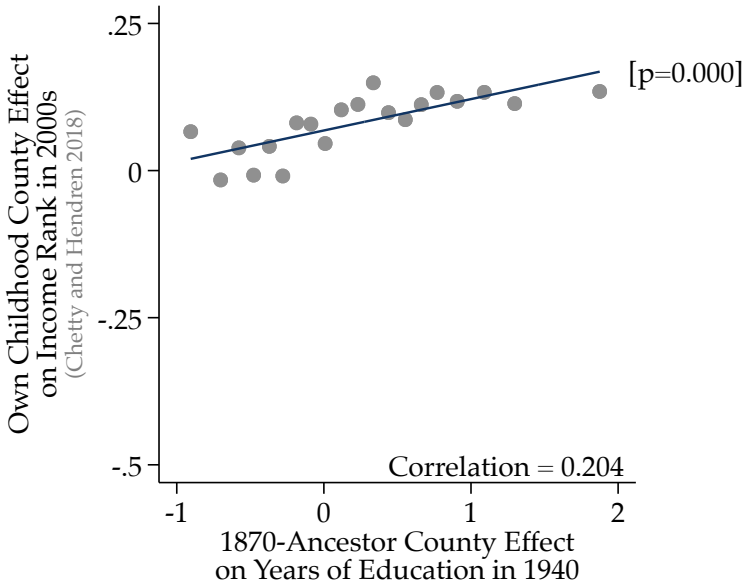
A.8 Persistence of Place Effects

FIGURE A.11: Persistence of a County's Capacity to Generate Upward Mobility



Notes: This figure repeats Figure **??** at the county level. We use empirical Bayesian shrinkage as described in Appendix B.8. See Data Appendix C for details on the sample and data.

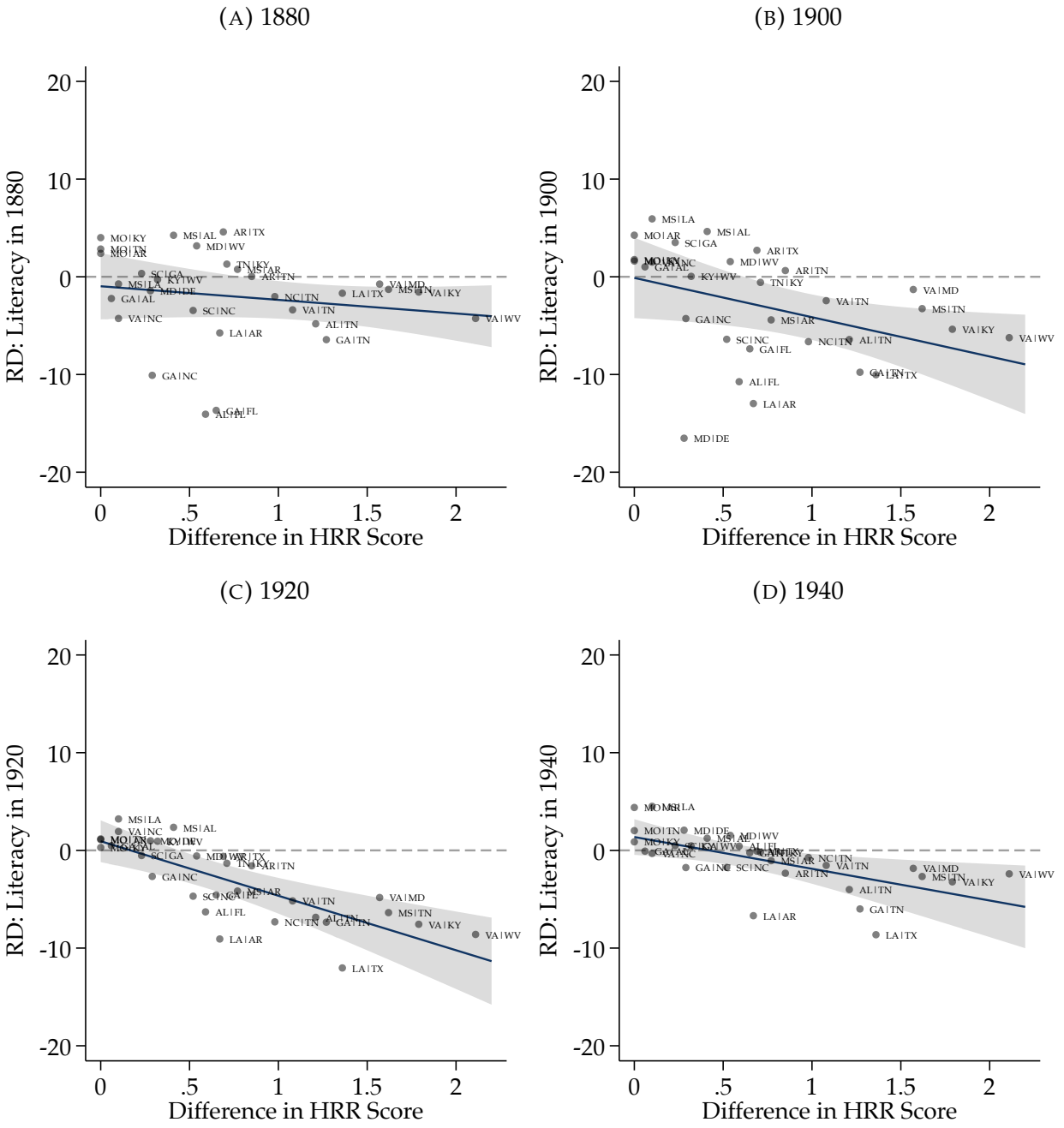
FIGURE A.12: Persistence of a County's Capacity to Generate Upward Mobility



Notes: This figure repeats Figure **??** using estimates of a county's effect on intergenerational mobility conditional on a child growing up with parents at the 75th (rather than the 25th) percentile of the national household income distribution. See Data Appendix C for details on the sample and data.

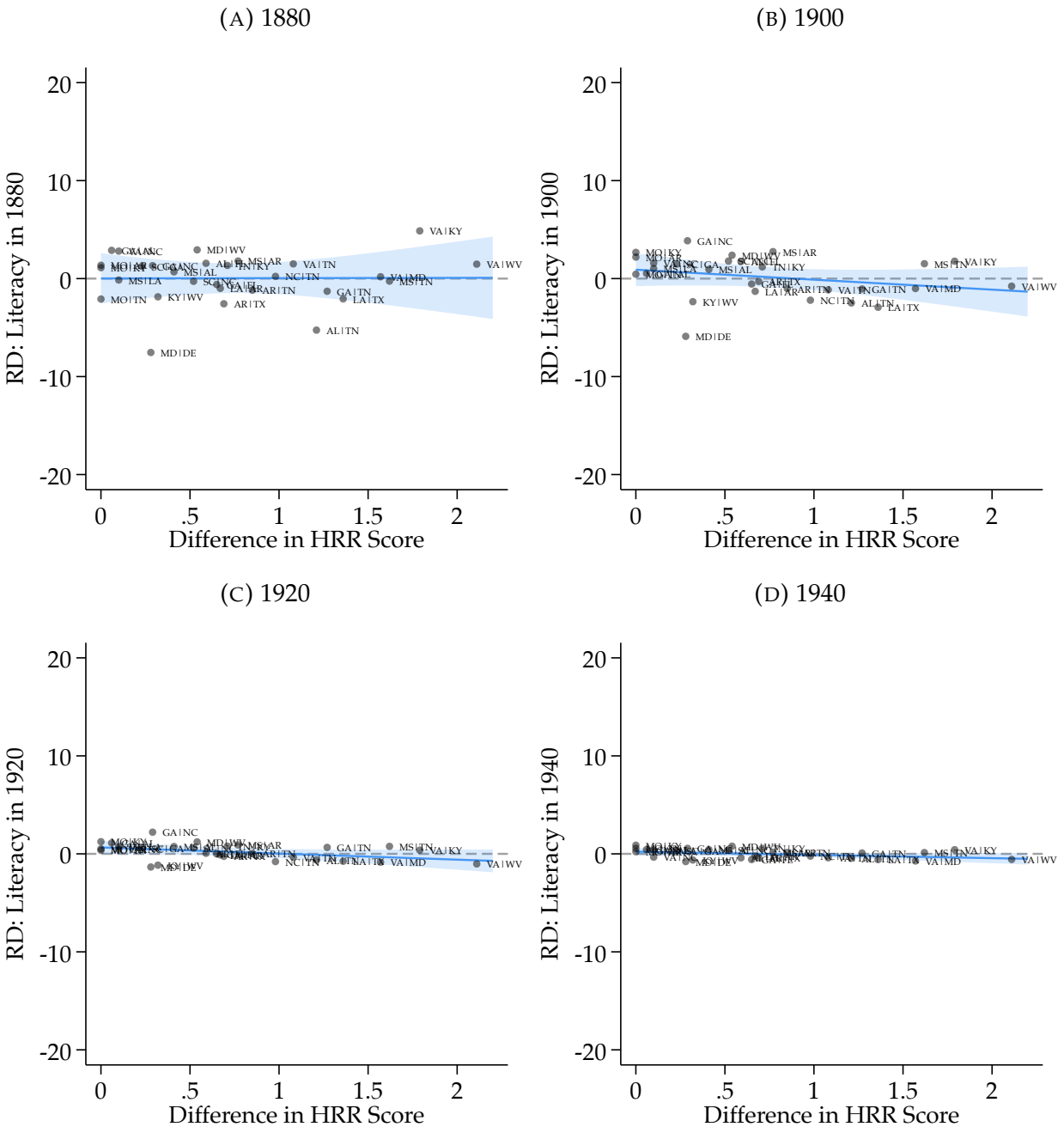
A.9 Border Discontinuities

FIGURE A.13: RD Estimates in Black Americans' Literacy over Time



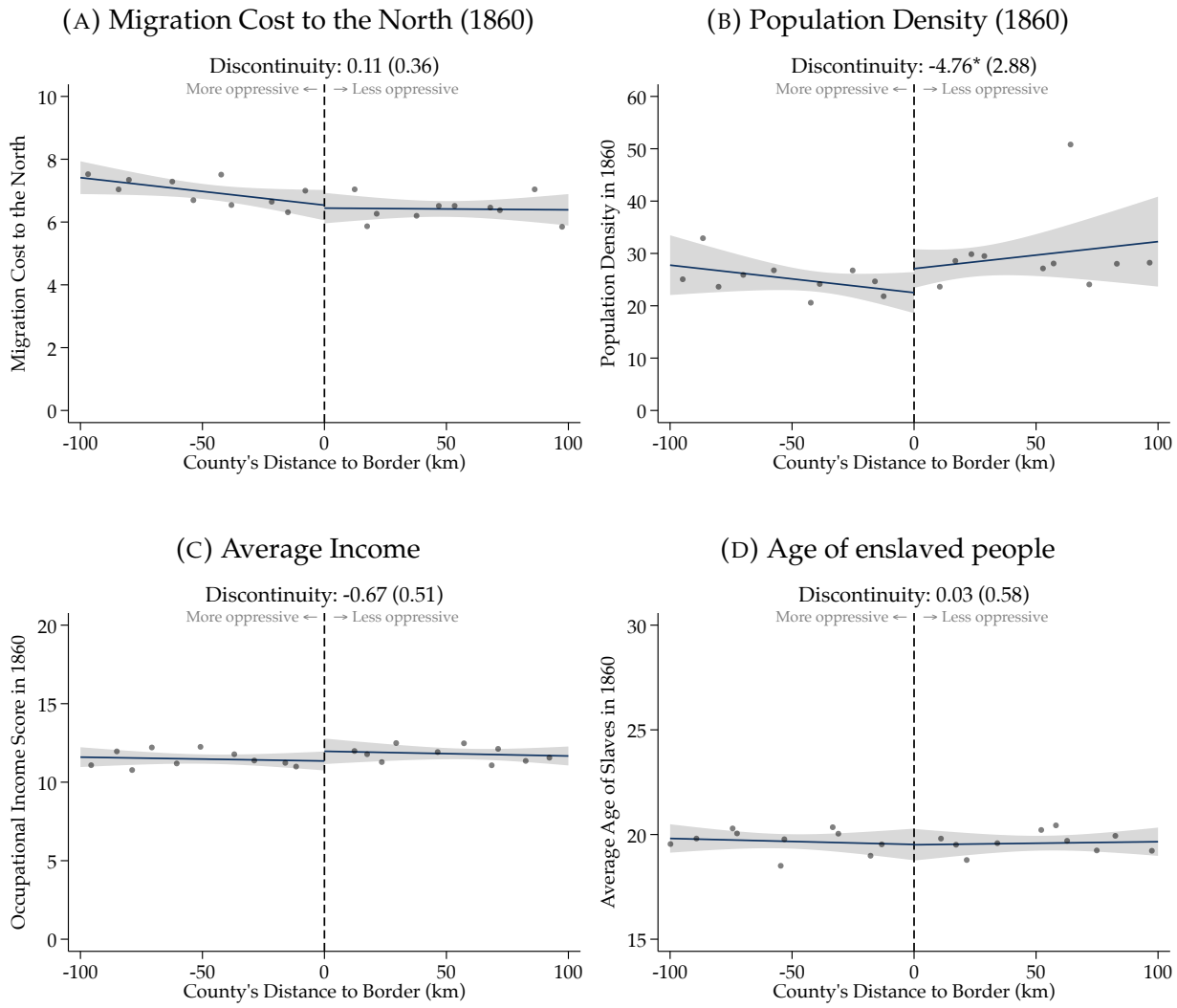
Notes: This Figure shows each separate RD estimate in literacy in 1880, 1900, 1920, and 1940 for Black families freed on different sides of state borders in 1865. Each label shows the more oppressive before the less oppressive state. Negative estimates reflect lower literacy in the more oppressive state. Lines show the best linear fit between RD estimates and the differences in Jim Crow intensity, weighted by the inverse of the estimates' standard error. Shaded areas represent robust 95 percent confidence bands. See Data Appendix C for details on the sample and data.

FIGURE A.14: RD Estimates in White Americans' Literacy over Time



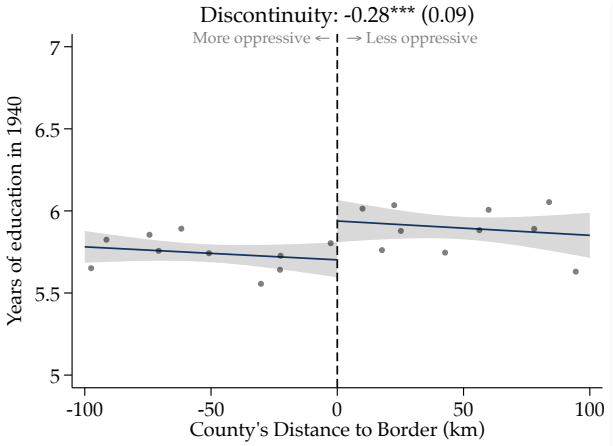
Notes: This Figure shows each separate RD estimate in literacy in 1880, 1900, 1920, and 1940 for white families who lived on different sides of state borders in 1865. Each label shows the more oppressive before the less oppressive state. Negative estimates reflect lower literacy in the more oppressive state. Lines show the best linear fit between RD estimates and the differences in Jim Crow intensity, weighted by the inverse of the estimates' standard error. Shaded areas represent robust 95 percent confidence bands. See Data Appendix C for details on the sample and data.

FIGURE A.15: No Border Discontinuities in Additional Location Characteristics



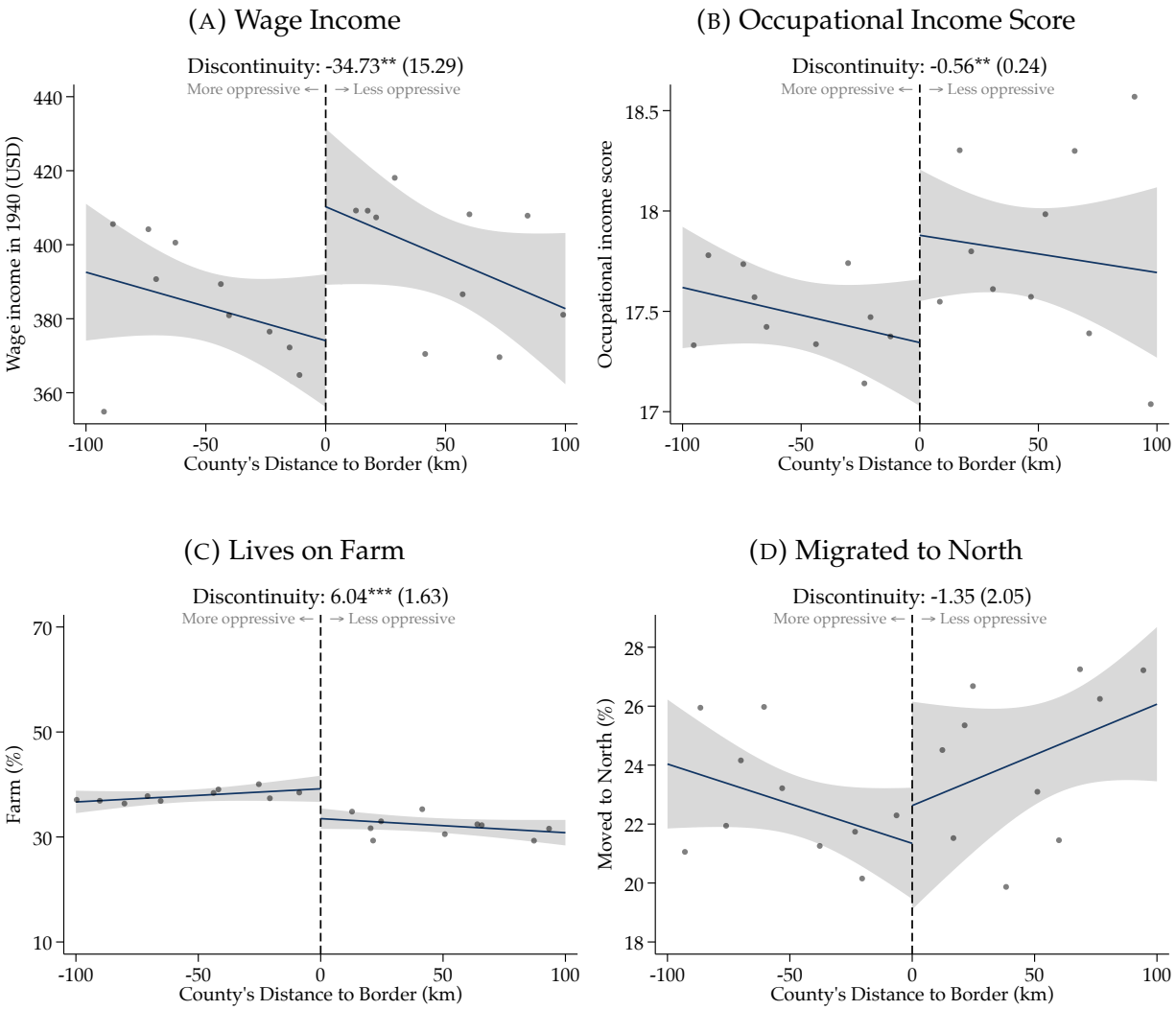
Notes: This figures shows the RD estimate in additional counties' characteristics in 1860 across state borders with different Jim Crow intensity in 1865. Migration costs reflect estimates by Donaldson and Hornbeck (2016). Average income is calculated based on occupational income scores. The sample is restricted to policy borders. The left half of each panel represents more oppressive states; the right half less oppressive states. Each dot is the average across a decile of the border population. Lines show the best linear fit. Shaded areas represent 95 percent confidence bands clustered at the 1870 county level. See Data Appendix C for details on the sample and data.

FIGURE A.16: Pooled Regression Discontinuity (RD) Estimate



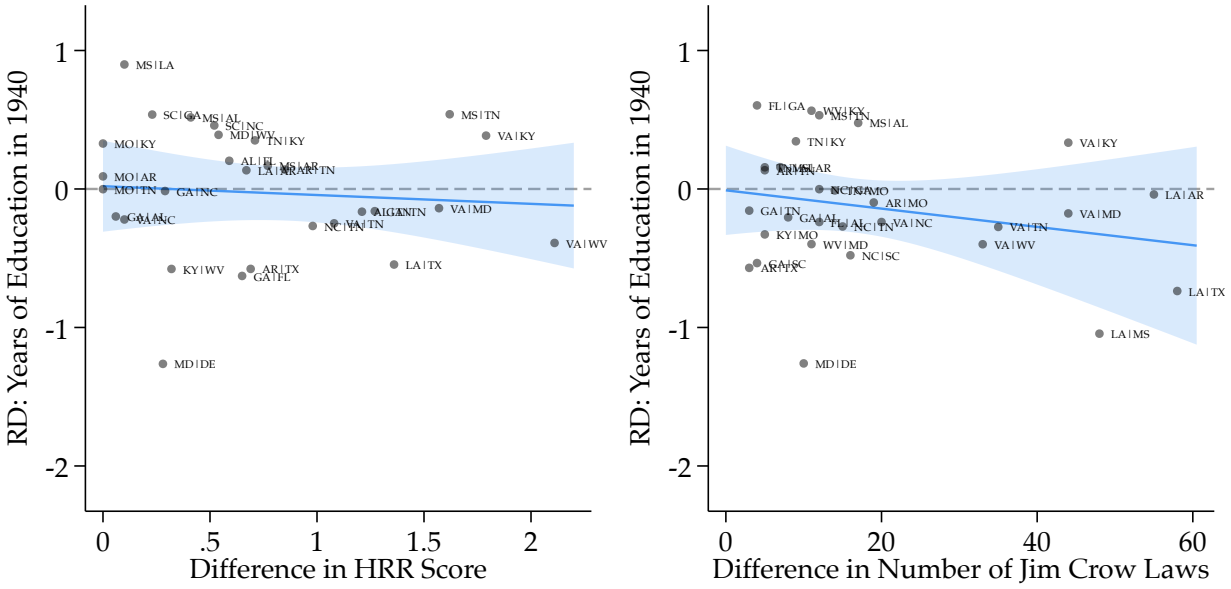
Notes: This figures shows the RD estimate in 1940 years of education for Black families freed across state borders with different Jim Crow intensity in 1865. The left half of the figure represents more oppressive states; the right half less oppressive states. Each dot is the average across a decile of the border population. Lines show the best linear fit. Shaded areas represent 95 percent confidence bands clustered at the 1870 county level.

FIGURE A.17: Border Discontinuities in Additional 1940 Outcomes



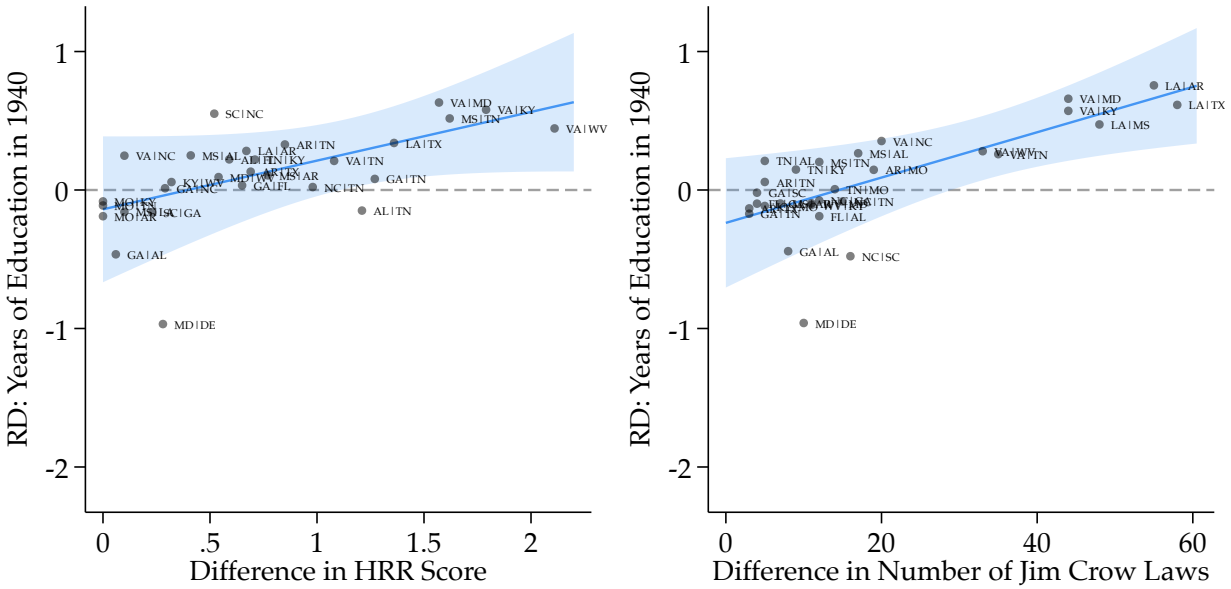
Notes: This figures shows the RD estimate in additional 1940 outcomes for Black families freed across state borders with different Jim Crow intensity in 1865. The sample is restricted to "policy borders" where Jim Crow intensity differed more than across the median border. The left half of each panel represents more oppressive states; the right half less oppressive states. Each dot is the average across a decile of the border population. Lines show the best linear fit. Shaded areas represent 95 percent confidence bands clustered at the 1870 county level. See Data Appendix C for details on the sample and data.

FIGURE A.18: RD Estimates for Poor White Americans



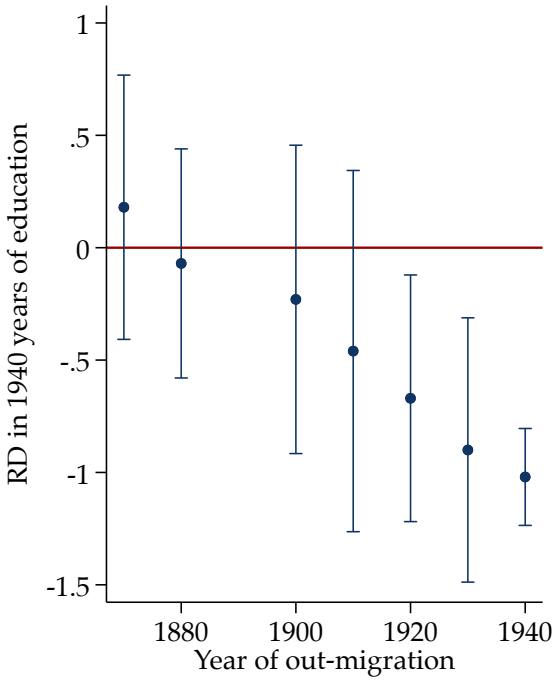
Notes: This figures shows each separate RD estimate in 1940 years of education for white families who had no physical or human capital in 1870 (illiterate and zero wealth) and lived on different sides of state borders. Each label shows the more oppressive before the less oppressive state. Negative estimates reflect lower education in the more oppressive state. Lines show the best linear fit between RD estimates and the differences in Jim Crow intensity, weighted by the inverse of each estimate's standard error. Shaded areas represent robust 95 percent confidence bands. See Data Appendix C for details on the sample and data.

FIGURE A.19: RD Estimates for Wealthy White Americans



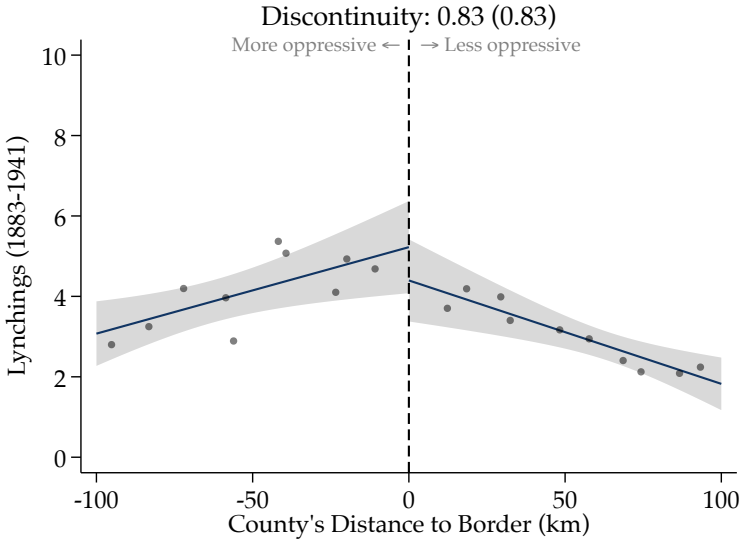
Notes: This figures shows each separate RD estimate in 1940 years of education for white families who were in the top decile in terms of real property in 1870 and lived on different sides of state borders. Each label shows the more oppressive before the less oppressive state. Positive estimates reflect higher education in the more oppressive state. Lines show the best linear fit between RD estimates and the differences in Jim Crow intensity, weighted by the inverse of each estimate's standard error. Shaded areas represent robust 95 percent confidence bands. See Data Appendix C for details on the sample and data.

FIGURE A.20: RD Estimates by Share of Decades Between 1870 and 1940 that a Family Lived in Their Ancestor's Enslavement State



Notes: This figures shows RD estimates in 1940 years of education for Black families freed on different sides of state borders in 1865 and stayed there for different amounts of time. Each estimate shows the pooled RD estimate for families who stayed in the state where their ancestors were freed from slavery until a given year (x-axis). Negative estimates reflect lower education in the more oppressive state. Bars represent 95 percent confidence intervals. See Data Appendix C for details on the sample and data.

FIGURE A.21: No Border Discontinuities in Lynchings between 1883 and 1941 (Policy Borders)



Notes: This figures shows the RD estimate in counties' number of lynchings of Black Americans between 1883 and 1941 (Seguin and Rigby, 2019). The sample is restricted to policy borders. The left half of each panel represents more oppressive states; the right half less oppressive states. Each dot is the average across a decile of counties. Lines show the best linear fit. Shaded areas represent 95 percent confidence bands. See Data Appendix C for details on the sample and data.

A.10 Rosenwald School Exposure

TABLE A.8: Rosenwald Schools and Socioeconomic Outcomes

	Literate (%) Mean: 84.91	Skilled (%) Mean: 12.16	Wages (USD) Mean: 497.64	Left South (%) Mean: 18.40	Homeownership (%) Mean: 32.83	House Value (USD) Mean: 175.42
Rosenwald exposure	**1.38****	**1.42**	**-17.40**	**2.27****	**4.27*****	**8.16**
	(0.62)	(1.07)	(11.85)	(1.14)	(1.44)	(10.95)
Controls	Y	Y	Y	Y	Y	Y
Adjusted R^2	0.14	0.02	0.11	0.05	0.04	0.04
Observations	107,141	89,709	57,816	110,375	108,299	110,375

Notes: This table shows the effect of Rosenwald school exposure (0 to 1) on a person's socioeconomic status in 1940. Regressions control for ancestor-county FEs, and year-of-birth×ancestor-state FEs. The sample is limited to families who lived in rural areas in 1870. Standard errors are clustered at both the county and cohort level. See Data Appendix C for details on the sample and data. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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DONALDSON, D. AND R. HORNBECK (2016): "Railroads and American Economic Growth: A Market Access Approach," *Quarterly Journal of Economics*, 131, 799–858.

SEGUIN, C. AND D. RIGBY (2019): "National crimes: A new national data set of lynchings in the United States, 1883 to 1941," *Socius*, 5, 2378023119841780.

B. Robustness Checks

B.1 The Free-Enslaved Gap Based on the Distribution of Last Names

TABLE B.9: Free-Enslaved Gap Based on the Distribution of Last Names (1940)

PANEL (A): Re-weighted to 1870-level of last names' relative frequency

	Education (Years)		Wage Income (USD)		Homeownership (%)		House Value (USD)	
	Mean: 5.70		Mean: 588.60		Mean: 21.53		Mean: 1,616.81	
P(Ancestor Enslaved)	**-1.25*****	**-1.40*****	**-88.36*****	**-113.15*****	**-1.95****	**-2.31****	**-1,098.68*****	**-1,194.53*****
	(0.07)	(0.09)	(21.22)	(25.50)	(0.87)	(1.05)	(237.09)	(282.83)
Name-measure	Exact	NYSIIS	Exact	NYSIIS	Exact	NYSIIS	Exact	NYSIIS
Controls (age, age^2)	Y	Y	Y	Y	Y	Y	Y	Y
Adjusted R^2	0.03	0.03	0.01	0.01	0.01	0.01	0.00	0.00
Observations	2,598,739		2,842,572		2,618,795		556,422	

PANEL (B): Not re-weighted

	Education (Years)		Wage Income (USD)		Homeownership (%)		House Value (USD)	
	Mean: 5.71		Mean: 598.74		Mean: 21.89		Mean: 1,599.75	
P(Ancestor Enslaved)	**-0.47*****	**-0.54*****	**-13.73*****	**-29.89*****	**-2.43*****	**-2.43*****	**-630.53****	**-970.17***
	(0.02)	(0.02)	(5.26)	(7.17)	(0.21)	(0.29)	(277.72)	(506.45)
Name-measure	Exact	NYSIIS	Exact	NYSIIS	Exact	NYSIIS	Exact	NYSIIS
Controls (age, age^2)	Y	Y	Y	Y	Y	Y	Y	Y
Adjusted R^2	0.03	0.03	0.00	0.00	0.01	0.01	0.00	0.00
Observations	2,859,747		2,821,235		2,842,572		601,789	

Notes: This table repeats Table 1 showing the gap in years of education, total income, homeownership, and house value among prime-age (20-54) male descendants of free vs. enslaved Black Americans in 1940. The sample includes the entire universe of prime-age Black men, not just those linkable. The coefficients can be interpreted as a 100 percentage point increase in the likelihood of descending from the Enslaved based on their (exact) last name. House values are measured conditional on ownership. Sample means are computed for the combined sample of the Free and the Enslaved. See Data Appendix C for details on the sample and data. Robust standard errors are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.2 The Free-Enslaved Gap Excluding Neighborhoods with Excess Deaths

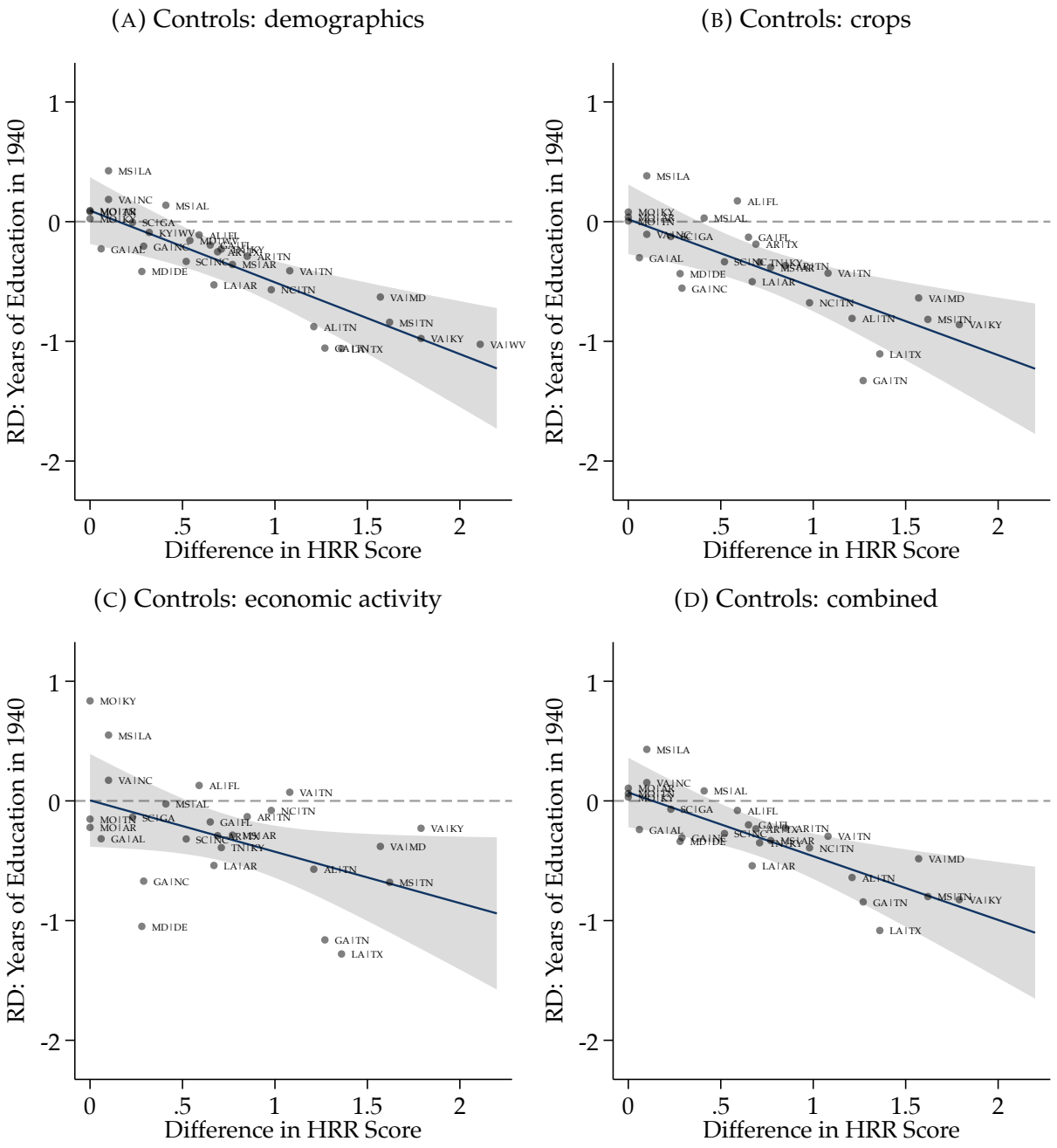
TABLE B.10: Free-Enslaved Gap Excluding Neighborhoods with Excess Deaths (2000)

	HS Degree (%) Mean: 67.68	College Degree (%) Mean: 11.27	Income (USD) Mean: 27,452.51	House Value (USD) Mean: 93,948.30
Ancestor Enslaved	**-3.59*****	**-2.45*****	**-4,794.24*****	**-16,242.52*****
	(0.39)	(0.32)	(432.35)	(1,862.48)
Level	ZIP×Race×Sex	ZIP×Race×Sex	ZIP×Race	ZIP
Controls (age, age^2)	Y	Y	Y	Y
Adjusted R^2	0.01	0.01	0.01	0.00
Observations	33,932	33,932	33,951	34,274
Ancestor Free	*2,196*	*2,196*	*2,196*	*2,219*

Notes: This table repeats Table 2 excluding ZIP codes that have deaths more than twice as high as predicted based on their population density. Each person is assigned the respective value of the ZIP code in which they last lived according to administrative death records. Only observations that can be linked to the 1850, 1860, 1870, or 1880 census are included. House values are measured conditional on ownership. Sample means are computed for the combined sample of the Free and the Enslaved. See Data Appendix C for details on the sample and data. Standard errors are clustered at the family level and are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

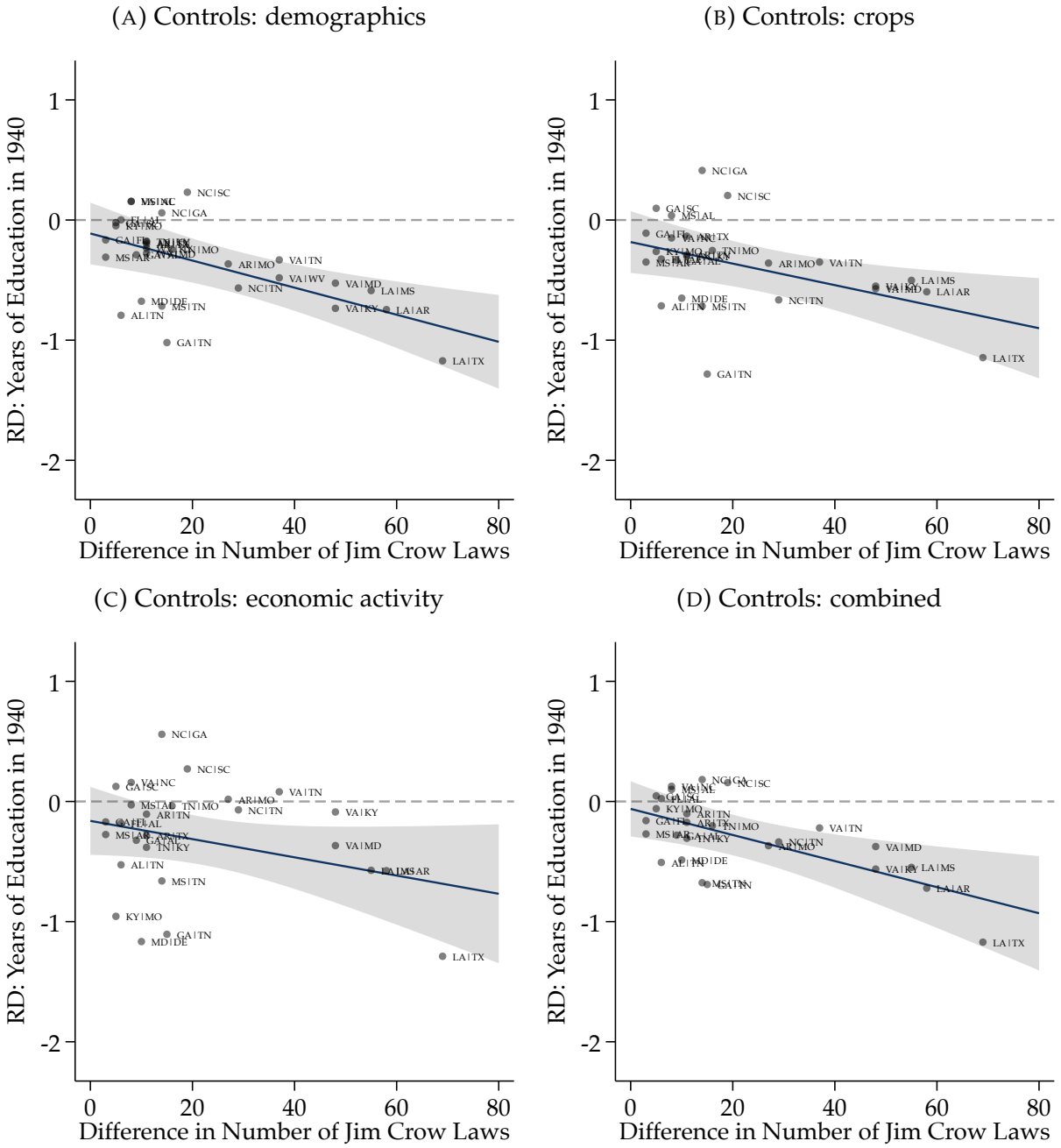
B.3 Border Discontinuities Controlling for Location Characteristics

FIGURE B.22: RD Estimates Using Different Sets of Control Variables (HRR)



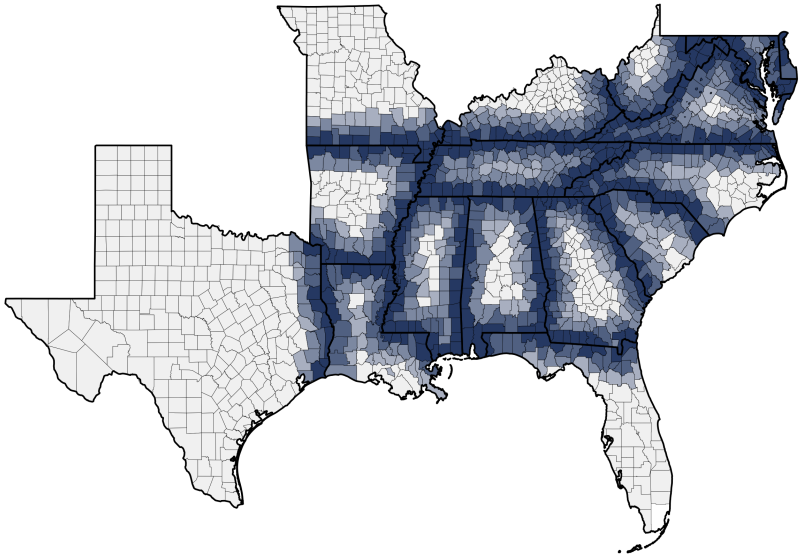
Notes: This figures shows each separate RD estimate in 1940 years of education for Black families freed across state borders with different Jim Crow intensity in 1865 after controlling for different sets of county-level variables in 1860. Panel (A) includes controls for the fraction Black, the fraction free among Black persons, and the age and sex of enslaved persons. Panel (B) includes controls for per-capita wheat, corn, rice, tobacco, cotton, wool, cane sugar, and total output. Panel (C) includes controls for the farm share, wealth, population density, share Black, and migration cost o the North. Panel (D) includes controls for the farm share, wealth, population density, share Black, migration cost to the North, per-capita tobacco, cotton, and cane sugar output, farm values, and share slaveholders. Each label shows the more oppressive before the less oppressive state. See Data Appendix C for details on the sample and data.

FIGURE B.23: RD Estimates Using Different Sets of Control Variables (Number of Laws)



Notes: This figures shows each separate RD estimate in 1940 years of education for Black families freed across state borders with different Jim Crow intensity in 1865 after controlling for different sets of county-level variables in 1860. Panel (A) includes controls for the fraction Black, the fraction free among Black persons, and the age and sex of enslaved persons. Panel (B) includes controls for per-capita wheat, corn, rice, tobacco, cotton, wool, cane sugar, and total output. Panel (C) includes controls for the farm share, wealth, population density, share Black, and migration cost o the North. Panel (D) includes controls for the farm share, wealth, population density, share Black, migration cost to the North, per-capita tobacco, cotton, and cane sugar output, farm values, and share slaveholders. Each label shows the more oppressive before the less oppressive state. See Data Appendix C for details on the sample and data.

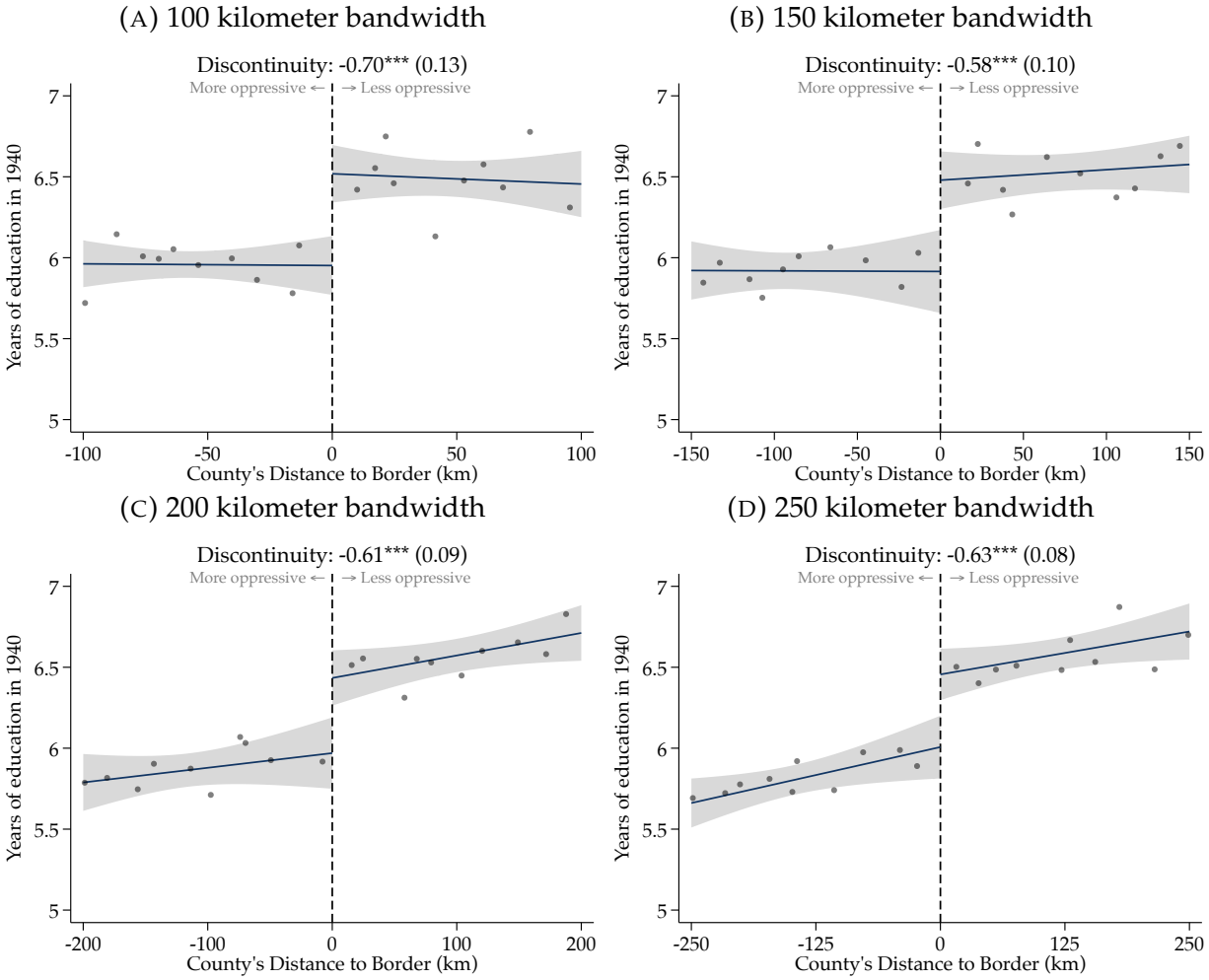
FIGURE B.24: Southern Counties' Distance to State Borders



Notes: This map shows each county's distance to the closest state border within the South. Darker shades correspond to closer proximity to a border. Distances are measured from a county's centroid to the border. We limit our analysis to counties within 100 kilometers (62 miles) of any border but show that our results are robust to other cutoffs.

B.4 Border Discontinuities Using Different Bandwidths

FIGURE B.25: Different Bandwidths for Pooled RD Estimates



Notes: This figures shows the RD estimate in 1940 years of education for Black families freed across state borders with different Jim Crow intensity in 1865. The analysis is limited to "policy borders" where Jim Crow intensity differed more than across the median border. Panels (A) to (D) show 100, 150, 200, and 250 kilometer bandwidths respectively. The left half of each panel represents more oppressive states; the right half less oppressive states. Each dot is the average across a decile of the border population. Lines show the best linear fit. Shaded areas represent 95 percent confidence bands clustered at the 1870 county level. See Data Appendix C for details on the sample and data.

B.5 Adjusting Estimates for Intermarriage

The Free-Enslaved gap accurately captures differences between Black Americans whose male ancestry line goes back to people enslaved until the Civil War vs. Black Americans whose male ancestry line goes back to people free before the Civil War. Because women tended to change their last names upon marriage, automated linking methods do not allow us to follow a family's female ancestry line directly. Therefore, some individuals who are classified as descendants of one group, may in truth also partly descend from the other group. This type of misclassification is a form of non-classical measurement

error, which we show leads to attenuation.

For the Free-Enslaved gap, we estimate

$$y_i = \alpha + \beta \cdot s_i + \varepsilon_i,$$

while we may also be interested in

$$y_i = a + b \cdot share_i + e_i,$$

where $share_i$ is the share of i's ancestors who were slave until the Civil War. For our estimate of the Free-enslaved gap, we have

$$\hat{\beta} \xrightarrow{p} \mathbb{E}[y|s=1] - \mathbb{E}[y|s=0] = b \cdot (\mathbb{E}[share_i|s=1] - \mathbb{E}[share_i|s=0]). \tag{11}$$

We use this expression to derive the attenuation bias that makes the Free-Enslaved gap a lower bound for the true group differences between families with high vs. low shares of ancestors enslaved.

B.5.1 First generation after slavery

For the first generation of descendants, we know that

$$\begin{aligned}
\mathbb{E}[share_{i,1}|s=1] &= 1 \cdot \mathbb{P}(share_{i,1}=1|s_i=1) + 0.5 \cdot \mathbb{P}(share_{i,1}=0.5|s_i=1) + 0 \\
&= 1 \cdot \mathbb{P}(\text{mother slave}|\text{father slave}) + 0.5 \cdot \mathbb{P}(\text{mother free}|\text{father slave}) \\
\mathbb{E}[share_{i,1}|s=0] &= 1 \cdot \mathbb{P}(share_{i,1}=1|s_i=0) + 0.5 \cdot \mathbb{P}(share_{i,1}=0.5|s_i=0) + 0 \\
&= 0.5 \cdot \mathbb{P}(\text{mother slave}|\text{father free})
\end{aligned}$$

Therefore, we have

$$\hat{\beta} \xrightarrow{p} b_1 \cdot [0.5 + 0.5 \cdot \mathbb{P}(\text{mother slave}|\text{father slave}) - 0.5 \cdot \mathbb{P}(\text{mother slave}|\text{father free})].$$

If there was no intermarriage, we would have $\hat{\beta} \xrightarrow{p} b_1$.[76] If marriage between formerly enslaved families and free Black families were random—in the sense that free and enslaved fathers have an equal probability of marrying an enslaved mother—we would have $\hat{\beta} \xrightarrow{p} 0.5 \cdot b_1$.[77] Given that it is implausible that free Black men were more likely than formerly enslaved Black men to marry formerly enslaved women, it seems reasonable that $b_1 \in [\hat{\beta}, 2 \cdot \hat{\beta}]$.

[76] Under no intermarriage, we have $\mathbb{P}(\text{mother slave}|\text{father slave}) = 1$ and $\mathbb{P}(\text{mother slave}|\text{father free}) = 0$.

[77] Under random intermarriage, we have $\mathbb{P}(\text{mother slave}|\text{father free}) = \mathbb{P}(\text{mother slave}|\text{father slave}) = \mathbb{P}(\text{mother slave})$.

We empirically assess this bias by analyzing the likelihood that a Black person descends from one parent born in a slave state and another parent born in a free state for 20-40 year old Americans in the 1910 census (whose parents were likely born towards the end of slavery). We are not able to quantify intermarriage between the formerly Enslaved and Free within state of origin because we do not have information on women's enslavement status beyond her birthplace. True intermarriage was therefore likely somewhat higher than the proxy we derive here.

We find that in 1910,

$$\begin{aligned}\widehat{\mathbb{P}}(\text{mother slave}|\text{father slave}) &= 0.99\\ \widehat{\mathbb{P}}(\text{mother slave}|\text{father free}) &= 0.20,\end{aligned}$$

suggesting that the the gap between individuals whose grandparents are either all formerly Enslaved or all Free is 1.1 times as large as the Free-Enslaved gap.

B.5.2 Second generation after slavery

For the second generation of descendants, we know that

$$\begin{aligned}\mathbb{E}[share_{i,1}|s=1] &= 1\cdot\mathbb{P}(\text{fm, mf, mm slave}|\text{ff slave})\\ &\quad +0.75\cdot[\mathbb{P}(\text{fm, mf slave \& mm free}|\text{ff slave})\\ &\qquad +\mathbb{P}(\text{fm, mm slave \& mf free}|\text{ff slave})\\ &\qquad +\mathbb{P}(\text{mf, mm slave \& fm free}|\text{ff slave})]\\ &\quad +0.5\cdot[\mathbb{P}(\text{fm slave \& mf, mm free}|\text{ff slave})\\ &\qquad +\mathbb{P}(\text{mf slave \& fm, mm free}|\text{ff slave})\\ &\qquad +\mathbb{P}(\text{mm slave \& fm, mf free}|\text{ff slave})]\\ &\quad +0.25\cdot\mathbb{P}(\text{fm, mf, mm free}|\text{ff slave})\\ \mathbb{E}[share_{i,1}|s=0] &= 0.75\cdot\mathbb{P}(\text{fm, mf, mm slave}|\text{ff free})\\ &\quad +0.5\cdot[\mathbb{P}(\text{fm, mf slave \& mm free}|\text{ff free})\\ &\qquad +\mathbb{P}(\text{fm, mm slave \& mf free}|\text{ff free})\\ &\qquad +\mathbb{P}(\text{mf, mm slave \& fm free}|\text{ff free})]\\ &\quad +0.25\cdot[\mathbb{P}(\text{fm slave \& mf, mm free}|\text{ff free})\\ &\qquad +\mathbb{P}(\text{mf slave \& fm, mm free}|\text{ff free})\\ &\qquad +\mathbb{P}(\text{mm slave \& fm, mf free}|\text{ff free})]\end{aligned}$$

where ff is i's father's father, fm father's mother, mf mother's father, and mm mother's mother. We can slightly simplify the first expression using that the sum over all possible

events sums to 1:

$$\begin{aligned}\mathbb{E}[share_{i,1}|s=1] &= 0.25 + 0.75 \cdot \mathbb{P}(\text{fm, mf, mm slave}|\text{ff slave}) \\ &\quad +0.5 \cdot [\mathbb{P}(\text{fm, mf slave \& mm free}|\text{ff slave}) \\ &\qquad +\mathbb{P}(\text{fm, mm slave \& mf free}|\text{ff slave}) \\ &\qquad + \mathbb{P}(\text{mf, mm slave \& fm free}|\text{ff slave})] \\ &\quad +0.25 \cdot [\mathbb{P}(\text{fm slave \& mf, mm free}|\text{ff slave}) \\ &\qquad +\mathbb{P}(\text{mf slave \& fm, mm free}|\text{ff slave}) \\ &\qquad +\mathbb{P}(\text{mm slave \& fm, mf free}|\text{ff slave})] \,.\end{aligned}$$

If there was no intermarriage, we would have $\hat{\beta} \xrightarrow{p} b_2$. If marriage between formerly enslaved families and free Black families were random we would have $\hat{\beta} \xrightarrow{p} 0.25 \cdot b_2$. Thus, $b_2 \in [\hat{\beta}, 4 \cdot \hat{\beta}]$.

We empirically assess this bias by analyzing the likelihood of having parents born in slave or free states for married couples between 20 and 40 years old in the 1910 census (whose parents were likely born towards the end of slavery). Our estimates suggest that the the gap between individuals whose grandparents are either all formerly Enslaved or all Free is 1.5 times as large as the Free-Enslaved gap.

B.5.3 n^{th} generation after slavery

Generally, if there was no intermarriage, we would have $\hat{\beta} \xrightarrow{p} b_n$. If marriage between formerly enslaved families and free Black families were random we would have $\hat{\beta} \xrightarrow{p} 2^{-n} \cdot b_n$. Thus, $b_n \in [\hat{\beta}, 2^n \cdot \hat{\beta}]$.

B.6 Adjusting Estimates for Misclassification Bias

We may incorrectly classify a substantial number of Black families as descendants of the Enslaved if they are born in a slave state where a large share of Black Americans was free before 1860. For example, among Black Americans from Maryland, around 50 percent are free in 1860. Due to imperfect linking rates, many of those free Black families will be misclassified as Enslaved by our method.

We adjust our estimates for bias that may arise from this type of misclassification. We make use that our original estimates are a weighted average of the (unknown) unbiased estimate and the non-causal estimate for free Black Americans:

$$\widehat{\beta}_{\text{original}} = \frac{\text{Enslaved}_{s,\text{links}}}{\text{Enslaved}_{s,1860}} \cdot \widehat{\beta}_{\text{unbiased}} + \left(1 - \frac{\text{Enslaved}_{s,\text{links}}}{\text{Enslaved}_{s,1860}}\right) \cdot \widehat{\beta}_{\text{free}}, \tag{12}$$

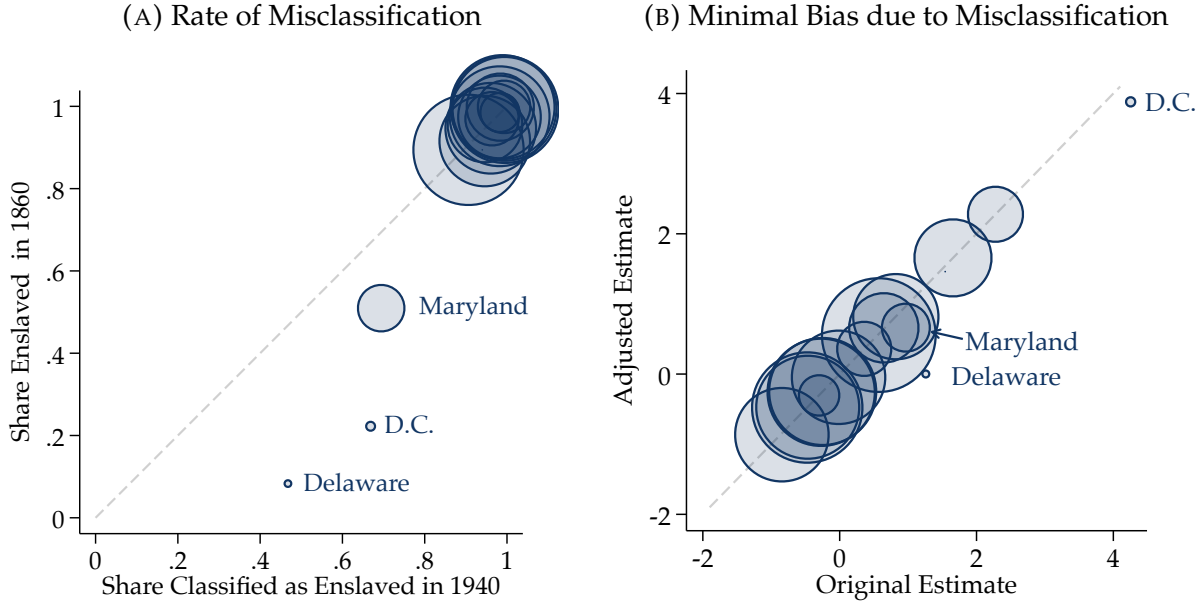
where $\text{Enslaved}_{s,\text{links}}$ is the share of Black Americans who descend from the Enslaved

of state s according to our classification in 1940, $\text{Enslaved}_{s,1860}$ is the true share of Black Americans who descend from the Enslaved of state s according to the 1860 census, and $\widehat{\beta}_{\text{free}}$ is the non-causal estimate for outcomes of free Black families with ancestors from state s.

In our sample, for instance, 70 percent of Black Americans with ancestors from Maryland are classified as descendants of the Enslaved in 1940—almost 20 percentage points more than the true share. Comparing the two groups suggests, unsurprisingly, that the free families of Maryland achieved somewhat higher levels of education by 1940 than descendants of the Enslaved. As a consequence, our original estimates (that incorrectly include some free families) are upward biased estimates of the true effect living in Maryland had on descendants of the Enslaved. Using the equation above, we can adjust our estimates.

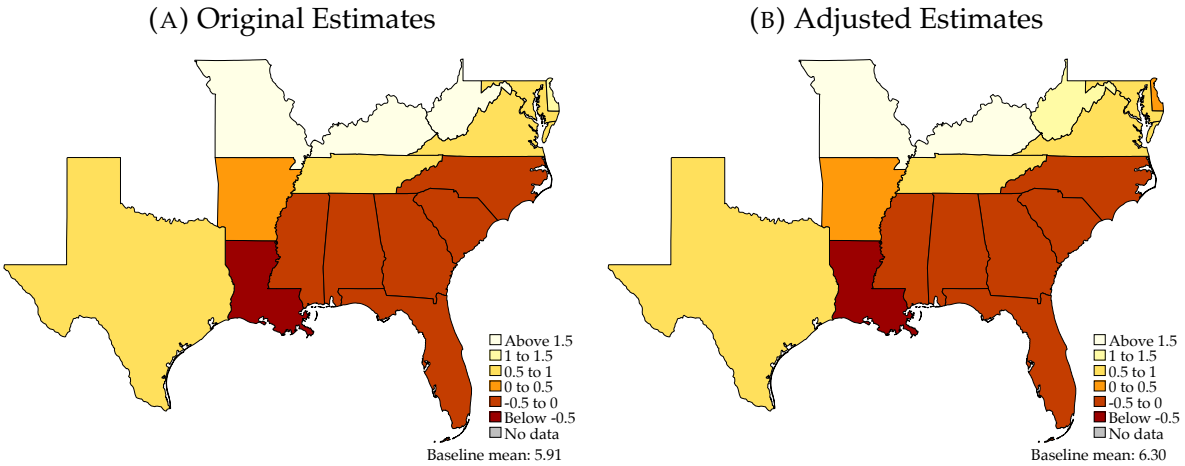
Adjusting our estimates for bias that may result from this form of misclassification turns out not to quantitatively matter. Figure B.26 shows that the share of Black Americans that descend from the Enslaved only deviates substantially from our classification for three small slave states. Accordingly, adjusting our original estimates of the causal effect of each state barely affects our estimates. Panel (A) of Figure B.27 shows that the original estimates tend to almost perfectly coincide with the adjusted estimates. Panel (B) shows the map of states' effects before and after the adjustment. Similarly, the Free-Enslaved gap is robust to excluding Delaware, DC, and Maryland (see Figure B.28).

FIGURE B.26: Misclassification and Bias



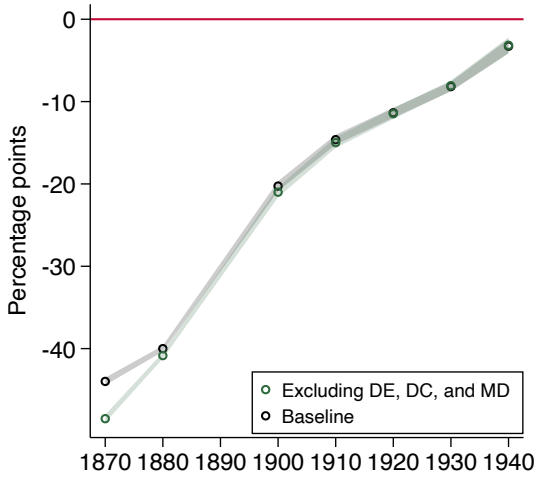
Notes: This figure assesses on misclassification of the Free-Enslaved status and the impact misclassification has on our estimates. Panel (A) shows the extent of misclassification as descendants of the Enslaved or the Free among Black Americans in 1940 with ancestors born in a given state before 1870. Panel (B) shows our causal estimates of living in each state before and after adjusting for misclassification bias. See Data Appendix C for details on the sample and data.

FIGURE B.27: Original and Adjusted Estimates



Notes: This figure compares the geographic patterns in our original estimates of states' effect on Black economic progress after slavery with the adjusted estimates. See Data Appendix C for details on the sample and data.

FIGURE B.28: Free-Enslaved Gap in Literacy (1870–1940)



Notes: This figure shows the gaps in literacy among prime-age (20-54) male descendants of free vs. enslaved Black Americans in each census decade before and after excluding Delaware, DC, and Maryland. We restrict the sample to observations linked to ancestors in 1850, 1860, 1870, or 1880. We control for a quadratic function in age and include 95 percent confidence bands clustered at the family level. See Data Appendix C for details on the sample and data.

B.7 The Direct Effect of Locations After Accounting for Migration

Our estimates of how being freed in a given location affected the economic progress of Black families reflects both the effect the original location and the expected effects of future locations conditional on the 1870 location. Under a mild assumption, we can recover the treatment effect of each destination location.

Assumption 2 (No direct long-run effect of enslavement location)**.** *The pre-1865 effect of enslavement location ℓ ceases to directly affect a family's descendants by 1940. That is,*

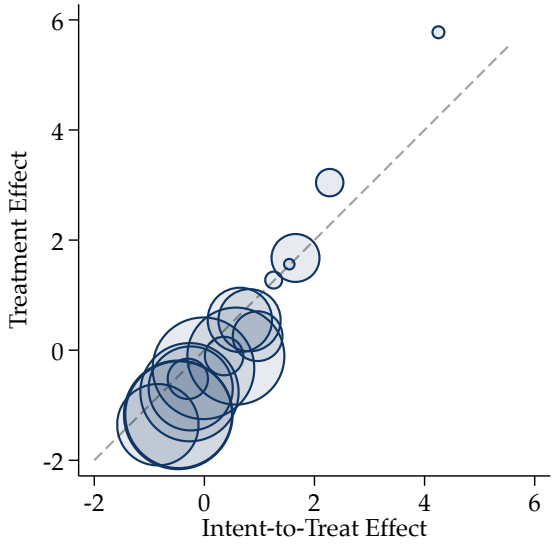
$$\rho\gamma_c^0 = 0$$

where ρ is the intergenerational elasticity from 1865 to 1940 and γ_ℓ^0 is the effect that location ℓ had on Black families who lived there.

This assumption is plausible for two reasons. First, the vast majority of enslaved people were freed from slavery with little to no measurable physical or human capital with little variation across locations. Second, plausible values for ρ are likely small given the high intergenerational mobility of Black Americans following the end of slavery and the amount of time that elapsed until 1940.[78]

Under this assumption, we can recover a state's treatment effect from the originally estimated intent-to-treat (ITT) using standard instrumental variable methods in settings with multiple treatments under imperfect compliance—each treatment being a potential state of birth and non-compliance arising through migration. As described in Section 6.1, the ITT effect of location ℓ, η_ℓ, is the average of all potential future locations' treatment effects, $\gamma_{\ell'}^1$, weighted by the probability of migrating from ℓ to ℓ'. We invert the migration probability matrix to recover the effect of living in each state until 1940.

FIGURE B.29: ITT Effect and Treatment Effect of Living in Each Southern State (1870–1940) on Years of Education in 1940



Notes: This figure compares our original (ITT) estimates of how being freed in a given state affected a Black family's economic progress to the direct treatment effect that living in that state had. The estimates are in years of education in 1940. See Data Appendix C for details on the sample and data.

We find that the original ITT effect of living in a state after 1865, estimated as the causal

[78] With an intergenerational elasticity of 0.25 for Black Americans (Althoff et al., 2022) and around three generations elapsing between 1865 and 1940: $\rho = 0.25^3 \approx 0.02$.

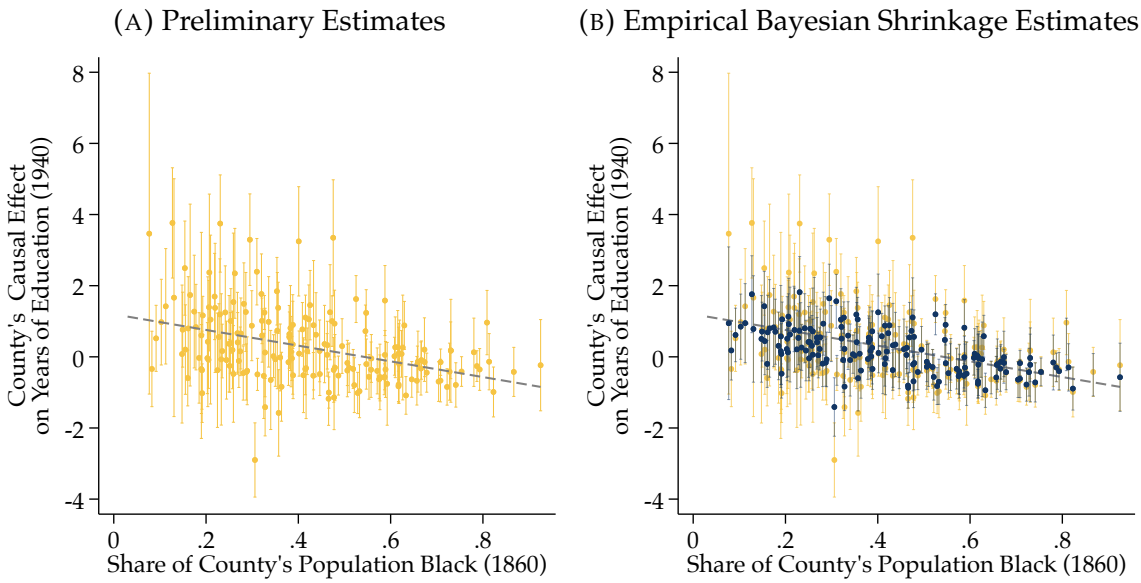
effect of being born into slavery in that state, is almost identical to the treatment effect of living in the state after 1865 (see Figure B.29). In essence, this finding results from high "compliance rates" due to low geographic mobility in the Deep South before 1940.

B.8 Empirical Bayes Shrinkage of Place Effects

When estimating the place effects with many geographic units (counties), a common problem is that some estimates may be very noisy. While these estimates are unbiased, they are on average further from the truth—in a total squared error sense—than optimal (Efron, 2010). Shrinkage techniques address this problem.

Empirical Bayes methods have become a popular means to shrink noisy estimates (e.g., Angrist et al., 2017; Chetty and Hendren, 2018). The method is motivated by the fact that under the assumption of place effects resulting from a common (unknown) distribution, the optimal point estimator has the from of a Bayesian posterior mean (Armstrong et al., 2021). One does not need to make any assumptions on the specific distribution that the place effects result from.

FIGURE B.30: Example of Empirical Bayes Shrinkage With a Single Covariate



Notes: This figure shows a random subsample of the 1870 ancestor county fixed effect (FE) estimates on 1940 years of education for descendants of the Enslaved. Panel (A) shows the preliminary estimates and 95 percent confidence bands clustered at the family level. Panel (B) shows the estimates after shrinking them toward the regression line based on the county's Black population share and robust 95 percent empirical Bayes confidence intervals (Armstrong et al., 2021). County FEs based on ten observations or less are discarded. See Data Appendix C for details on the sample and data.

Figure B.30 provides a simple example of empirical Bayes shrinkage with a single covariate. Panel (A) shows the negative correlation between the preliminary estimates of a county's causal effect on years of education in 1940 and county's share of the population that is Black in 1860. Panel (B) shows the empirical Bayesian shrinkage estimates. Noisier

preliminary estimates are pulled toward the regression line more drastically than more precise preliminary estimates.

We employ a empirical Bayes shrinkage to our baseline county-effects. We provide two forms of shrinkage estimates. The first set does not use covariates, shrinking the baseline estimates toward a common mean. The second set includes covariates, shrinking the baseline estimates toward the place effect predicted by the covariates. We prefer the second set of estimates because they leverage a larger set of information in the shrinkage process.

The empirical Bayes estimate for county c including covariates takes the form

$$\widehat{\theta}_c \quad = \quad X_c'\widehat{\delta} + \frac{\widehat{s}}{\widehat{s} + \widehat{\sigma}_c^2} \cdot (\eta_c - X_c'\widehat{\delta}),$$

where $\widehat{\eta}_c$ is the preliminary estimate of county c's effect,

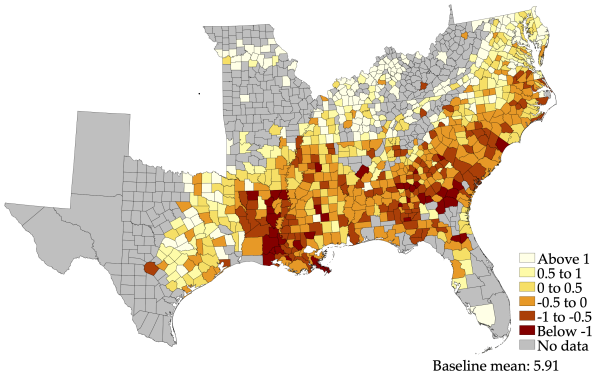
$$\widehat{\delta} = \left(\sum_{c=1}^{N} \widehat{\sigma}_c^{-2} X_c X_c' \right)^{-1} \sum_{c=1}^{N} \widehat{\sigma}_c^{-2} X_c \widehat{\eta}_c$$

is the ordinary least squares estimate of $\widehat{\eta}_c$ on the county covariates X_c, $\widehat{\sigma}_c^2$ is the standard error of $\widehat{\eta}_c$, and $\widehat{s} = \max \left\{ \frac{-N + \sum_{c=1}^{N} \widehat{\sigma}_c^{-2} \widehat{\varepsilon}_c^2}{\sum_{c=1}^{N} \widehat{\sigma}_c^{-2}}, \frac{2}{\sum_{c=1}^{N} \widehat{\sigma}_c^{-2}} \right\}$ with $\widehat{\varepsilon}_c = \widehat{\eta}_c - X_c'\widehat{\delta}$. The shrinkage estimate is therefore a weighted average of the preliminary county fixed effect and the predicted effect based on the county's characteristics, with greater weight assigned to a preliminary fixed effects when it is more precisely estimated.

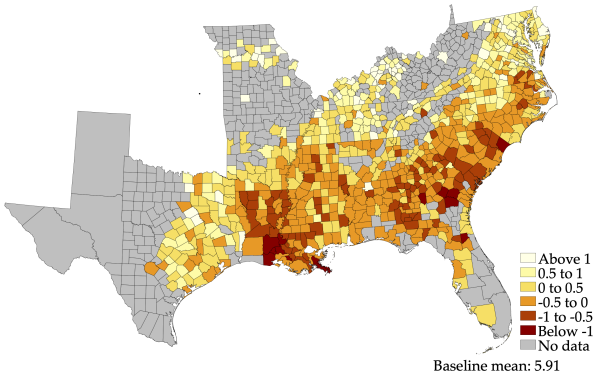
Figure B.31 shows a comparison of the place effects before and after empirical Bayes shrinkage. While the general pattern of negative effects being concentrated in the Lower South holds before and after, the shrunk estimates are more spatially correlated. Figure B.32 shows the correlation of causal place-effects on Black economic progress with the same places' (non-causal) effects on the outcomes of white and free Black Americans. Before and after shrinkage, there is no correlation between the effects for descendants of the Enslaved and white Americans, but a strong positive correlation between those for descendants of the Enslaved and the Free.

FIGURE B.31: Causal Place Effects on 1940 Years of Education

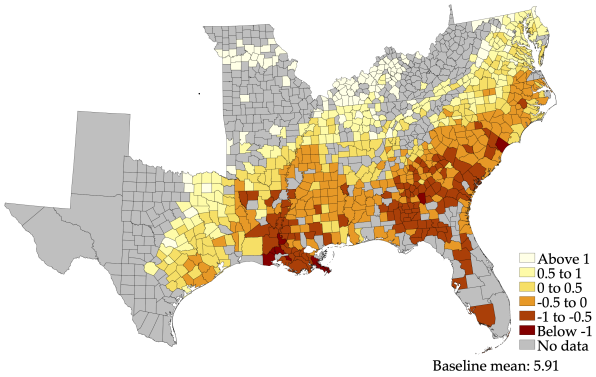
(A) Preliminary Estimates



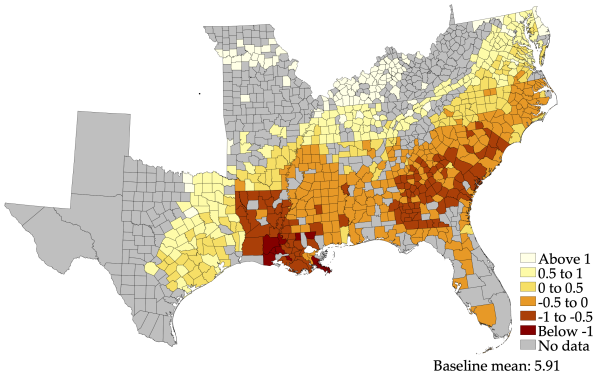
(B) Shrinkage Estimates (No Covariates)



(C) Shrinkage Estimates (Covariates)

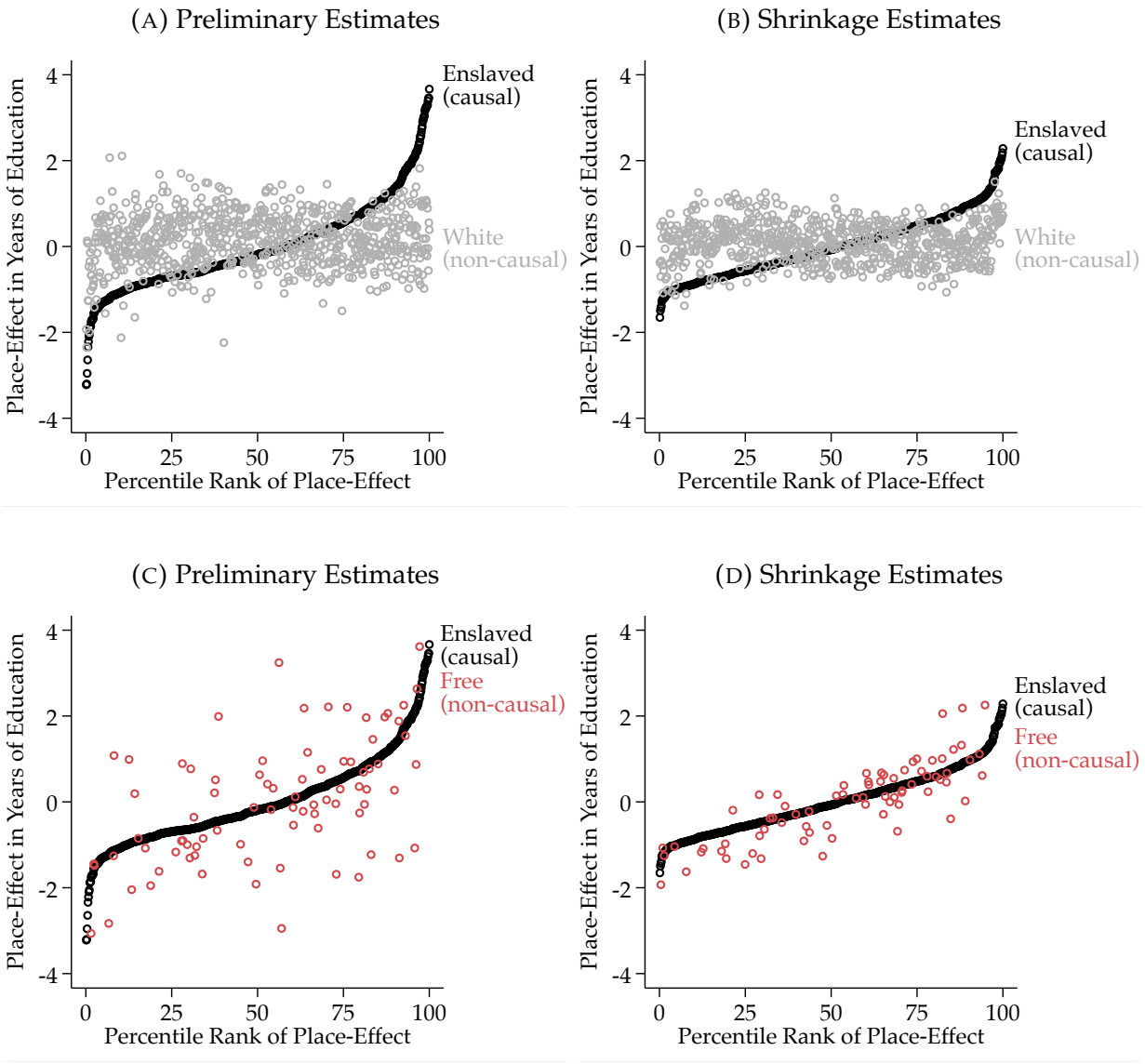


(D) Shrinkage Estimates (Covariates & State FEs)



Notes: This figure shows the 1870 ancestor county fixed effect (FE) estimates on 1940 years of education for descendants of the Enslaved. Panel (A) shows the preliminary estimates. Panel (B) shows the estimates after shrinking them to their common mean. Panel (C) shows the estimates after shrinking them to the regression line based on various covariates. Panel (D) repeats Panel (C) adding state FEs. County FEs based on ten observations or less are discarded. See Data Appendix C for details on the sample and data.

FIGURE B.32: Place Effects Across Groups Before and After Shrinkage



Notes: This figure compares the 1870 ancestor county fixed effect estimates on years of education in 1940 for descendants of the Enslaved (causal) with those of white Americans and descendants of free Black Americans (both non-causal). Panels (A) and (C) show the estimates before shrinkage, Panels (B) and (D) show the estimates after shrinkage. The shrinkage does not preserve a county's original rank. County fixed effects based on ten observations or less are discarded. See Data Appendix C for details on the sample and data.

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B.9 Placebo Exercises

In two types of placebo exercises, we test our method of quantifying the Free-Enslaved gap. First, we estimate the placebo Free-Enslaved gap for white Americans. White families that cannot be linked to the 1850 or 1860 censuses are classified as (placebo) descendants of the Enslaved. This exercise may not yield pure placebo estimates because families immigrating after 1860 may be different from those who immigrated earlier. Table B.11 shows that the composition of white Americans indeed experienced some changes after 1860. White Americans grew more likely to be first-generation immigrants and among those immigrants fewer came from the United Kingdom and more from Northern, Central, and Eastern Europe. When evaluating the placebo estimates, we should bare in mind that these changes contaminate a pure placebo.

Second, we estimate the Free-Enslaved gap using 1875 as the (placebo) end of slavery. Figure 2 already suggests that there is no gap between Black Americans who can be linked back to 1880 (but not 1870 or earlier) and those who can be linked back to 1870 or earlier. In this section, we also estimate the placebo Free-Enslaved gap based on the change in the distribution of last names from 1870 to 1880.

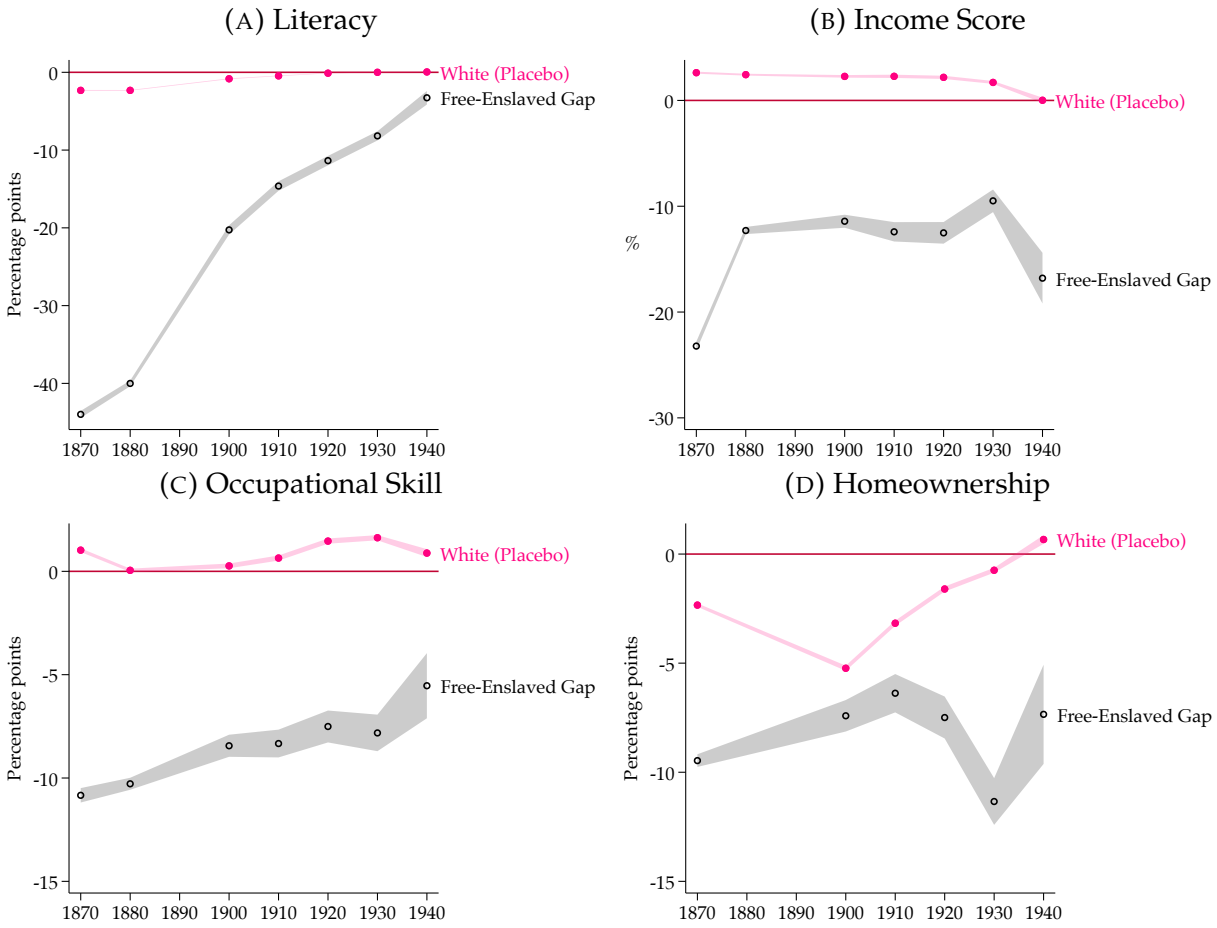
Beyond the two placebo exercises, we also estimate the effect that the 1870 ancestor birthplace has on white Americans' socioeconomic status in 1940. Because we do not have exogenous variation in the ancestor birthplace for free people such as white Americans, these estimates are non-causal. We also trace the speed of convergence in the socioeconomic status of white families whose 1870 ancestors did or did not have physical or human capital. This exercise yields a benchmark for the speed of convergence between descendants of the Enslaved and the Free. Lastly, we estimate the intergenerational effect that the Lower South's institutions had on white Americans using our border discontinuity design.

TABLE B.11: White Americans Before and After 1865

Region of Birth	Share in 1860 (%)	Share in 1870 (%)	Change (Ppts.)	Change (%)
North America	86.5	85.3	-1.2	-1.4
Central America	0.2	0.2	0.0	25.3
Northern Europe	0.2	1.0	0.8	337.3
UK	8.1	7.0	-1.0	-12.8
Western Europe	0.7	0.8	0.1	10.8
Southern Europe	0.1	0.2	0.1	76.6
Central/Eastern Europe	4.2	5.4	1.3	30.7

Notes: This table shows the share of white American men born in each region in the 1850–1860 and the 1870–1880 censuses. Regions with shares below 0.1 percent in any year are omitted.

FIGURE B.33: Free-Enslaved Gap (1870-1940) vs. Placebo for White Americans



Notes: This figure shows the true and placebo gaps in literacy rates, income scores (OCCSCORE), occupation skill levels (HISCLASS), and homeownership rates among prime-age (20-54) male descendants of free vs. enslaved Black Americans in each census decade. The placebo applies the exact same procedure to the sample of white Americans. The comparison shows that some linking bias may affect results in early periods, but all of it vanishes over time. Only observations that can be linked to the 1850, 1860, 1870, or 1880 census are included. All estimates control for a quadratic function in age and include 95 percent confidence bands that are clustered at the family level. While literacy is reported directly in the census decades only until 1930, we use schooling in second grade as a proxy in 1940. See Data Appendix C for details on the sample and data.

TABLE B.12: Placebo Free-Enslaved Gap (1940) for White Americans

	Education (Years)	Wage Income (USD)	Home Ownership (%)	House Value (USD)
	Mean: 9.76	Mean: 892.68	Mean: 49.74	Mean: 3,284.56
Placebo	**-0.17*****	**-1.68**	**0.09**	**12.17**
	(0.00)	(1.04)	(0.05)	(9.63)
Controls (age, age^2)	Y	Y	Y	Y
Adjusted R^2	0.03	0.06	0.01	0.00
Observations	5,015,270	4,770,969	5,012,884	2,425,204
Ancestor Free	*3,158,604*	*3,001,138*	*3,155,980*	*1,536,909*

Notes: This table shows the placebo gaps in years of education, total income, homeownership, and house value among prime-age (20-54) male Black Americans in 1940. The placebo applies the exact same procedure to the sample of white Americans. Only observations that can be linked to the 1850, 1860, 1870, or 1880 census are included. House values are measured conditional on ownership. Sample means are computed for the combined sample of the Free and the Enslaved. See Data Appendix C for details on the sample and data. Standard errors are clustered at the family level and are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE B.13: Placebo Free-Enslaved Gap (2000) at Census Tract Level

	HS Degree (%)	College Degree (%)	Income (USD)	House Value (USD)
	Mean: 83.52	Mean: 25.52	Mean: 46,123.73	Mean: 133,616.06
Placebo	**-0.11****	**0.22*****	**133.37**	**908.63***
	(0.05)	(0.08)	(91.00)	(499.87)
Level	Tract×Race×Sex	Tract×Race×Sex	Tract×Race	Tract×Race
Controls (age, age^2)	Y	Y	Y	Y
Adjusted R^2	0.00	0.01	0.00	0.00
Observations	437,099	437,099	437,076	435,809
Ancestor Free	*383,221*	*383,221*	*383,213*	*382,049*

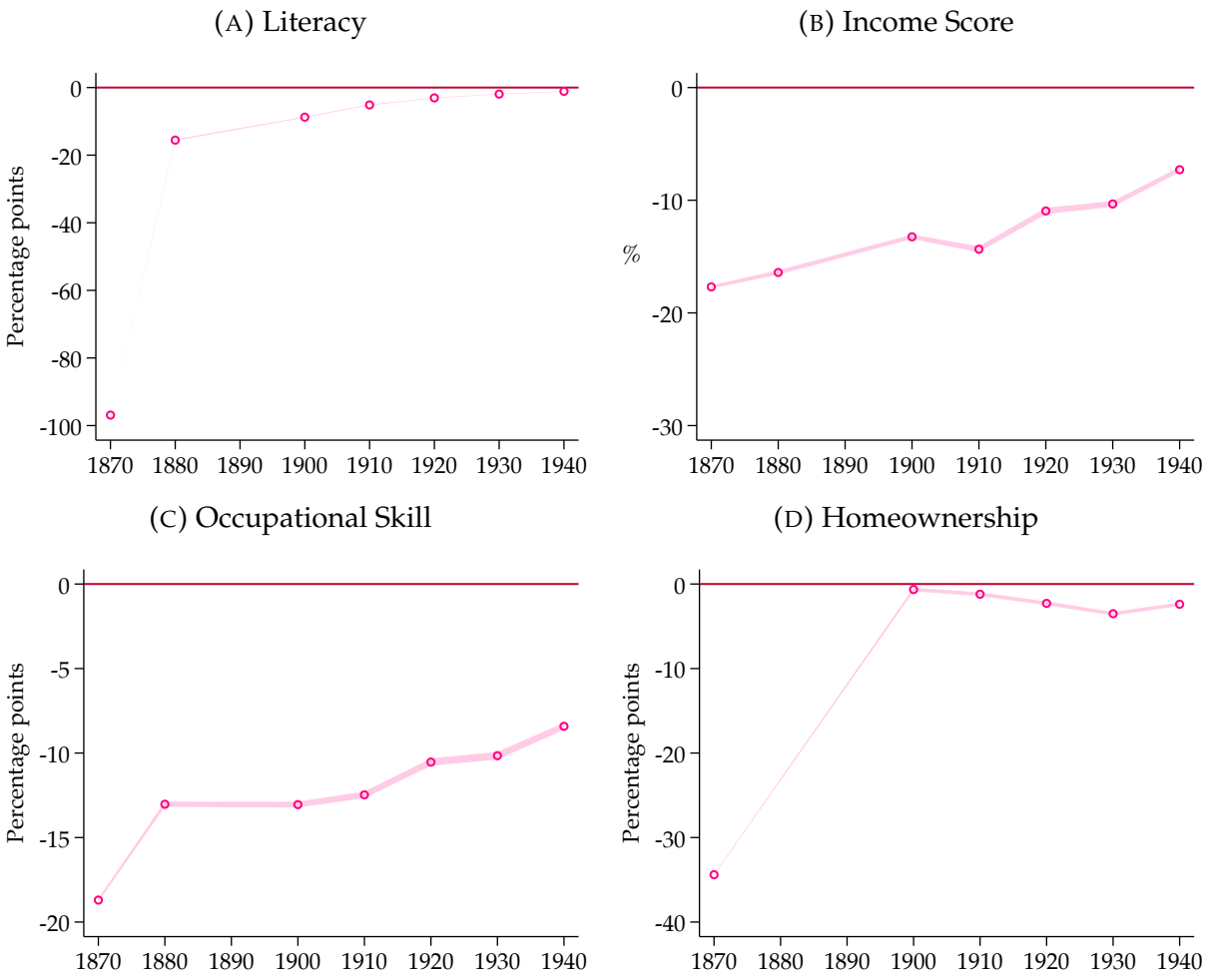
Notes: This table shows the placebo gap in neighborhood-level outcomes among male white Americans around 2000. Only observations that can be linked to the 1850, 1860, 1870, or 1880 census are included. We estimate the gap in terms of the fraction of people who hold a high school or college degree, the median income earned, and the median house value. House values are measured conditional on ownership. Each person is assigned the respective value of the census tract in which they lived at the time of death. See Data Appendix C for details on the sample and data. Standard errors are clustered at the family level and are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE B.14: Placebo vs. Free-Enslaved Gap Based on the Distribution of Last Names (1940)

	Education (Years)				
P(Ancestor Enslaved)			**-0.47*****	**-0.43*****	**-0.54*****
			(0.02)	(0.02)	(0.02)
Placebo: 1875	-0.05***		-0.03***		0.21***
	(0.01)		(0.01)		(0.01)
Placebo: White		-0.04***		-0.04***	0.07***
		(0.00)		(0.00)	(0.00)
Placebo: 1875×White					-0.24***
					(0.00)
Name-measure	Exact	Exact	Exact	Exact	Exact
Controls (race, race×age, race×age^2)	Y	Y	Y	Y	Y
Adjusted R^2	0.03	0.03	0.03	0.14	0.14
Observations	2,859,747	24,267,079	2,859,747	27,126,826	27,126,826

Notes: This table repeats Table 1 showing the gap in years of education, total income, homeownership, and house value among prime-age (20-54) male descendants of free vs. enslaved Black Americans in 1940. The sample includes the entire universe of prime-age Black men, not just those linkable. The coefficients can be interpreted as a 100 percentage point increase in the likelihood of descending from the Enslaved based on their (exact) last name. House values are measured conditional on ownership. Sample means are computed for the combined sample of the Free and the Enslaved. See Data Appendix C for details on the sample and data. Robust standard errors are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

FIGURE B.34: Benchmark for Speed of Convergence—White Americans Whose Ancestors Did vs. Did Not Have Any Physical or Human Capital



Notes: This figure shows the gaps in literacy, income, skill, and homeownership among white prime-age (20-54) male descendants of ancestors with vs. without any physical or human capital in 1870. Physical capital is measured in terms of real and personal property; Human capital is measured in terms of literacy. The comparison yields a benchmark for the convergence of large socioeconomic gaps from 1870 to 1940. Only observations that can be linked to the 1850, 1860, 1870, or 1880 census are included. All estimates control for a quadratic function in age and include 95 percent confidence bands that are clustered at the family level. While literacy is reported directly in the census decades only until 1930, we use schooling in second grade as a proxy in 1940. See Data Appendix C for details on the sample and data.

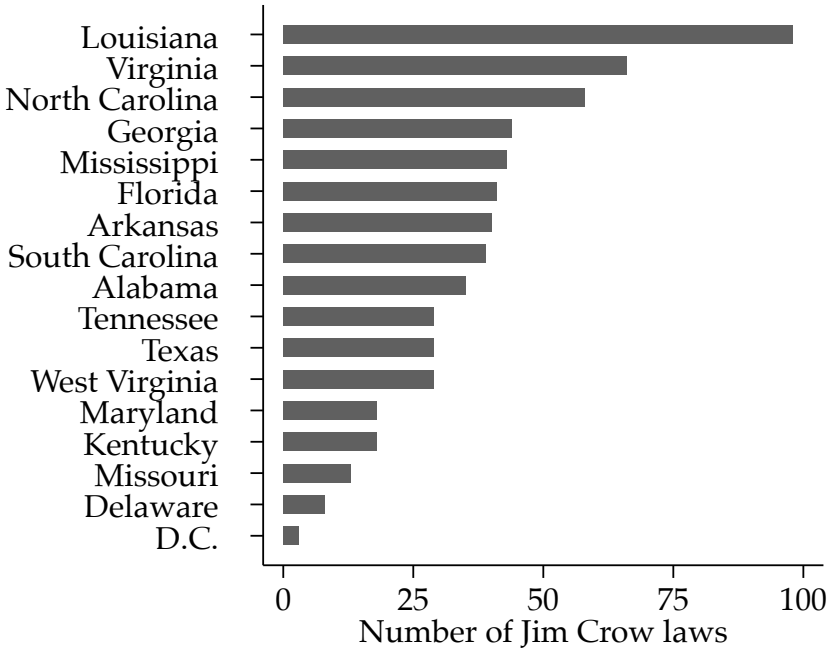
C. Data Appendix

C.1 Jim Crow Database

C.1.1 New Database on Jim Crow Laws

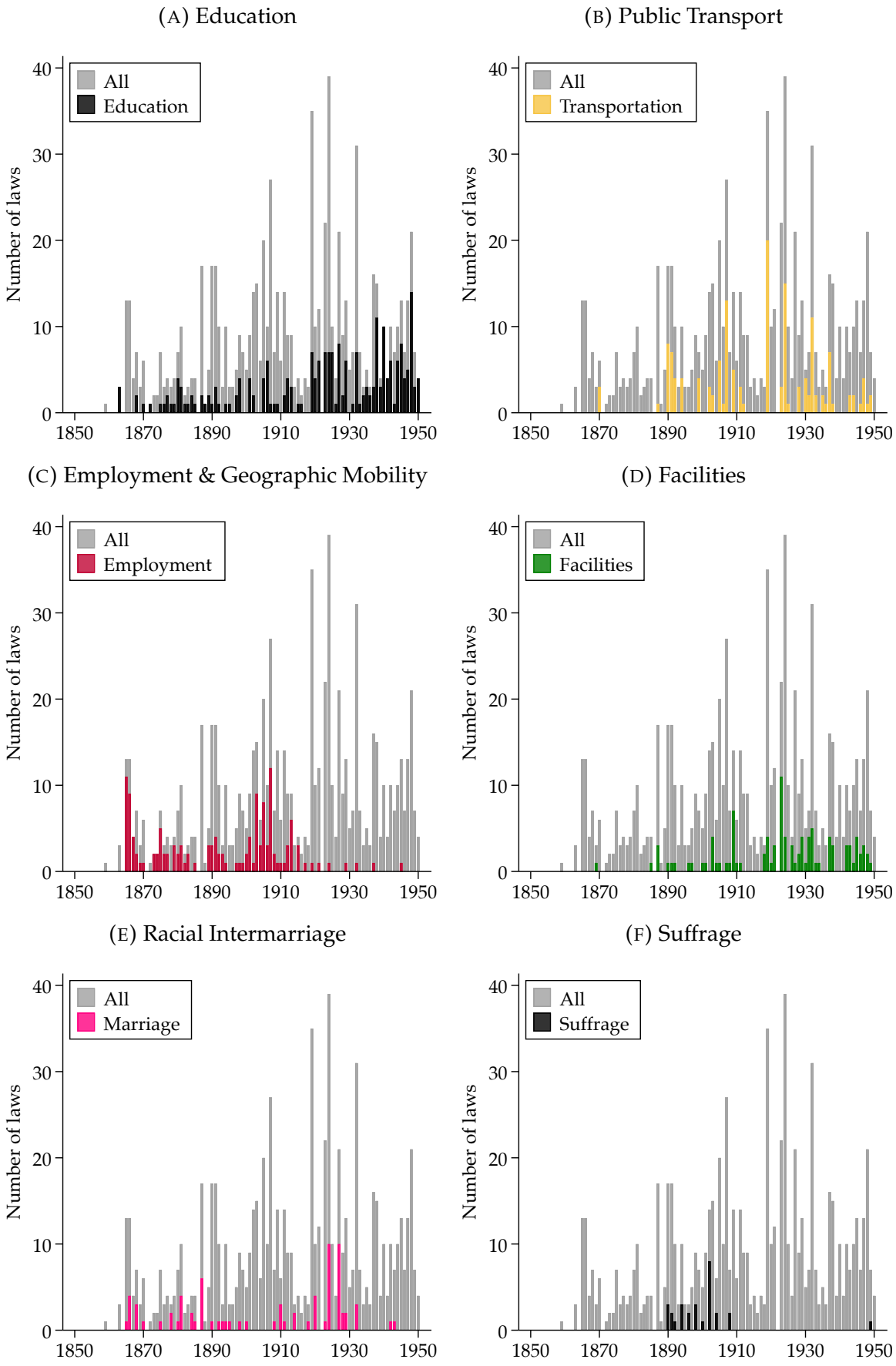
We collect information from 800 Jim Crow laws from four sources, covering both race-specific and "race-blind" Jim Crow laws. We digitize a comprehensive collection of laws that refer to race and color by state in 1950 Murray (1950). We digitize those laws and categorize them as discriminatory, anti-discriminatory, or neutral. We restrict our sample to discriminatory laws and further categorize the domain they pertain to, such as education, suffrage, or employment. Our remaining sources add Jim Crow laws that made no explicit mention of race. We collect laws that limited the geographic mobility of Black Americans and regulated their employment arrangements from Roback (1984) and Cohen (1991). We further collect laws that restricted Black suffrage from Walton et al. (2012). Figure C.35 shows the number of total Jim Crow laws passed by each state until 1950. Figure C.36 shows the distribution over years in which Southern governments passed laws of different types.

Figure C.35: Number of Jim Crow laws by State



Notes: This figure shows the cumulative number of Jim Crow laws passed by state until 1950.

FIGURE C.36: Number of Jim Crow Laws Across the South by Type



Notes: This figure shows the number of Jim Crow laws passed by type across all Southern states and years.

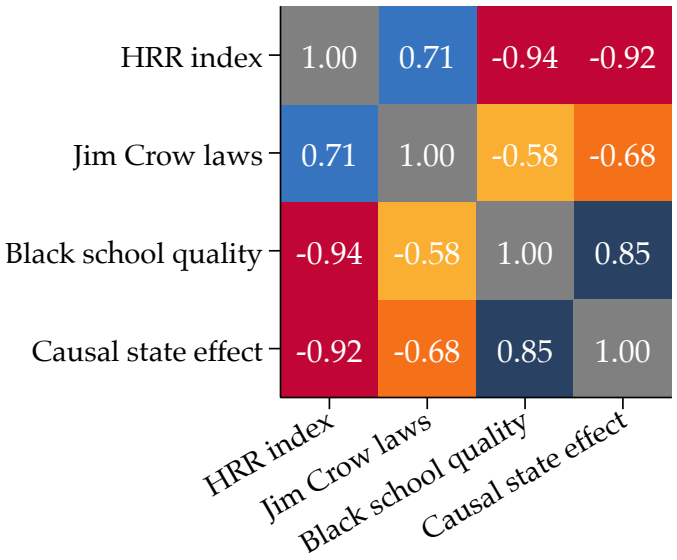
C.1.2 Other Data on Jim Crow Regimes

Historical Racial Regime index. As an alternative measure of a state's Jim Crow intensity, we use the Historical Racial Regime index (Baker, 2022). This index "measures different manifestations of the US racial regime across different historical periods—slavery and Jim Crow—and is based on state-level institutions including slavery, sharecropping, disfranchisement, and segregation."

Black school quality index. Second, as a second proxy for the intensity of Jim Crow regimes specific to education, we construct an aggregate measure of Black school quality in the South (Card and Krueger, 1992). We extract a principal component from three measures of Black school quality by state prior to 1940: student-teacher ratios, term lengths, and teacher wages.

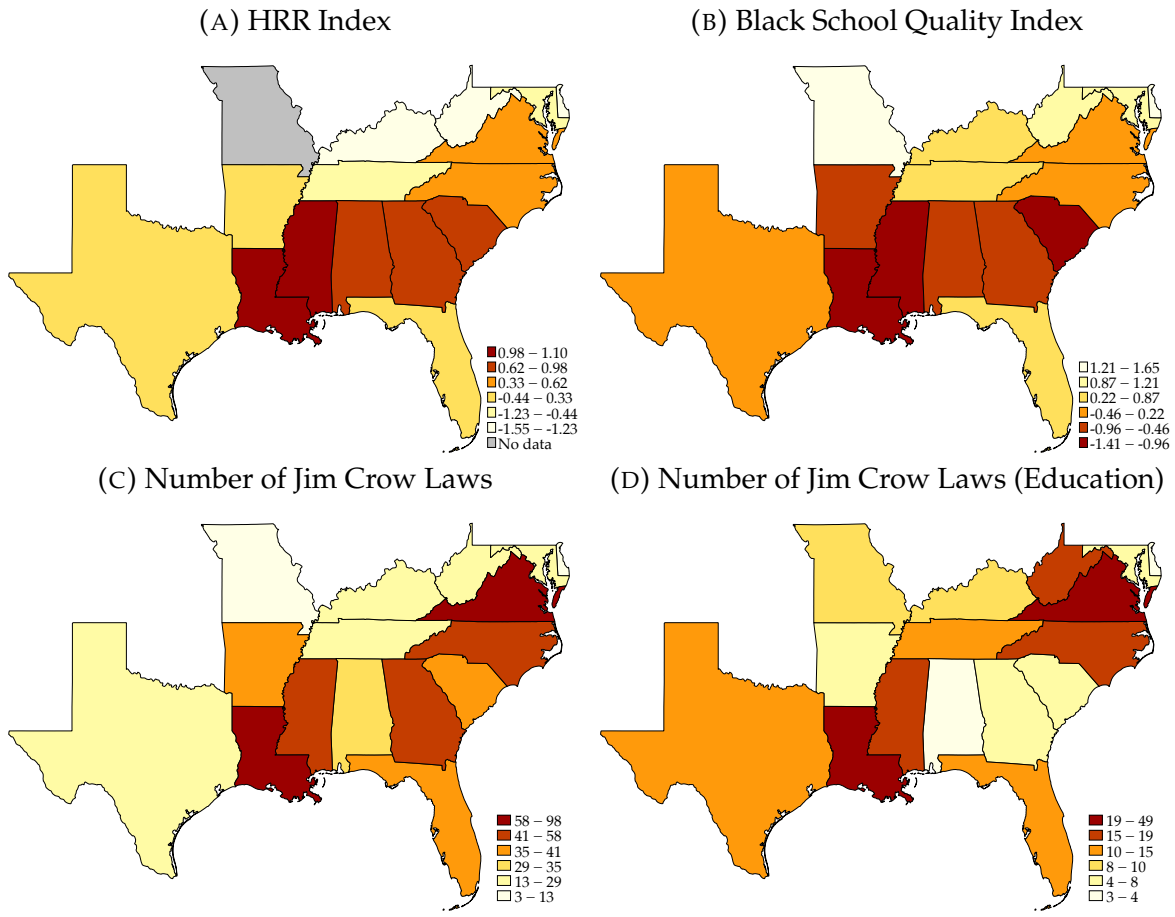
Figure C.37 shows the correlation between our different proxies of Jim Crow regimes' intensity. Figure C.38 shows maps of the Southern states and their Jim Crow intensity based on those proxies.

FIGURE C.37: Correlations Between Proxies of Jim Crow Intensity



Notes: This figure shows the correlation between a state's Historical Racial Regime index (Baker, 2022), number of Jim Crow laws, quality of Black schools (Card and Krueger, 1992), and causal effect on long-run economic progress of Black families as presented in Figure 4.

FIGURE C.38: Jim Crow Intensity by State



Notes: This figure shows each state's Jim Crow intensity across four different proxies: a state's Historical Racial Regime index (Baker, 2022), number of Jim Crow laws, number of Jim Crow laws pertaining to education, and the quality of Black schools (Card and Krueger, 1992).

C.2 Census Data

We use the publicly available full-count US Census Bureau data at the individual level for Black Americans for all decades between 1850 and 1940. The 1850 census is the first to include individual-level data; the 1940 census is the latest for which restricted access to the names of individuals is available. We use linking methods that rely on those names. The data from 1890 were destroyed by a fire and are therefore not included.

C.3 Census Linking

We use crosswalks for individuals across census decades provided by Abramitzky et al. (2020), publicly available here at `https://censuslinkingproject.org`. The crosswalks can be merged into the public version of the census data using the histid identifier. We do so, linking all adjacent and non-adjacent census decades in our sample period.

There are multiple crosswalks available, each based on different linking techniques.

Our main results use the "abe_race_nysiis_standard" link, which matches observations based on first name, last name, and age. It requires each name to be unique within a five-year window for each race but allows some names to be matched even if their spelling differs. We also provide results for "abe_nysiis_conservative," which requires names to be unique within and across races and allows for fewer deviations in matching characteristics.

To study intergenerational dynamics, we inherit parents' characteristics to the children in their household. Certain characteristics, such as occupation or education, are only inherited from prime-age male (ages 20–54) ancestors to ensure comparability over time.

C.4 Identifying Descendants of the Free and the Enslaved

C.4.1 Main Method: Linking Historical Census Records

Figure C.39 illustrates our new method to identify descendants of the Free and descendants of the Enslaved in census records between 1870 and 1940. It mainly relies on census-linking methods (Abramitzky et al., 2019) but also uses information on place and year of birth.

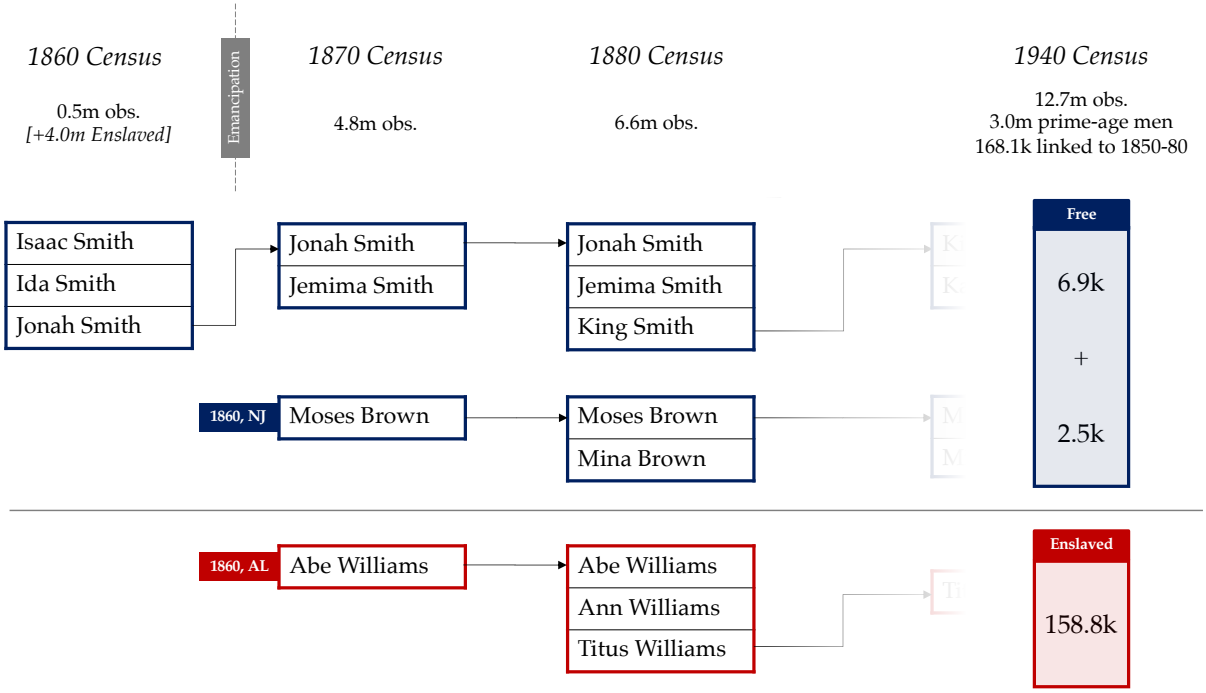
The method consists of three steps. First, we identify the Free themselves before identifying their descendants. In 1850 and 1860, the enslaved population was excluded from the individual-level censuses. By definition, every Black American included in the census was therefore free before 1865. We link the 1850 and 1860 censuses forward to all census decades between 1870 and 1940 and then classify every Black American who can be linked to 1850 or 1860 as free.

In addition to linking, we use information on place and year of birth in our classification algorithm. All Northern states had begun banning or restricting slavery by 1804—some of them decades earlier. Any Black person born in those states was either free upon birth or would be emancipated by a certain age (typically in their 20s). While the latter case opens up the possibility of a Northern-born Black person being sold into slavery in other states before their emancipation, this possibility was ruled out by law.

In Appendix Table C.16, we compare the de jure to the de facto status of slavery in the North. As a de facto measure, we show the number of slaves in the state in absolute numbers and as a fraction of the state's Black population. Based on this evidence, we classify any Black American born outside of the slave states after 1804 and before 1865 as Free. In addition, we use the state-specific years in which slavery was abolished or restricted in non-slave states to go even further back in time.

Second, we identify the *descendants* of the Free by using information on the relationship between individuals within census households. Specifically, we classify Black people with a free Black American *ancestor* as being descendants of the Free. Any person

FIGURE C.39: Illustration of Our Free-Enslaved Classification Algorithm



Notes: This figure illustrates our new method to identify descendants of the Free and the Enslaved in census records 1870-1940. The names are chosen are arbitrary examples and do not reflect real data. Jonah Smith is identified as a descendant of the Free because he can be linked back to the 1860 census; Moses Brown because he was born in a state (New Jersey) that had abolished slavery by the time of his birth (1860). Abe Williams does not fall into either category and is therefore classified as formerly enslaved or a descendant of the Enslaved. The Free-Enslaved status is assigned to descendants based on their male ancestor. In 1940, the final year of our sample, we identify 9,400 descendants of the Free (6,800 through direct linking to 1850–1860 and 2,600 through their ancestor's birthplace) and 155,800 descendants of the Enslaved. While not comprehensively illustrated here, we do link across all adjacent and non-adjacent census records of 1850, 1860, 1870, 1880, 1900, 1910, 1920, 1930, and 1940.

without a free ancestor is classified as a descendant of the Enslaved. In 1940, the final year of our sample, we identify 9,400 descendants of the Free and 155,800 descendants of the Enslaved.

Attenuation bias. Because we can only link men, the descendant classification is determined exclusively through male ancestors. This data limitation prevents us from assessing inter-marriage between the Free and the formerly Enslaved or their descendants. The fact that some Black Americans will be both descendants of the Free and descendants of the Enslaved potentially biases our estimates of the Free-Enslaved gap toward zero. This attenuation bias is one reason why our results should be interpreted as a lower bound for the true Free-Enslaved gap.

Linking bias. Any study that uses automated linking methods faces the problem that individuals who can be linked across decades may not represent the overall population. For example, families with a high socioeconomic status may choose more unique names for their children, making it easier to create a unique match across census records. A

socioeconomic gap between two sub-populations is only biased if the linking procedure differentially selects them into the sample. Table C.15 shows that, if anything, the linking procedure biases the Free-Enslaved gap toward zero.

TABLE C.15: Assessing Linking Bias

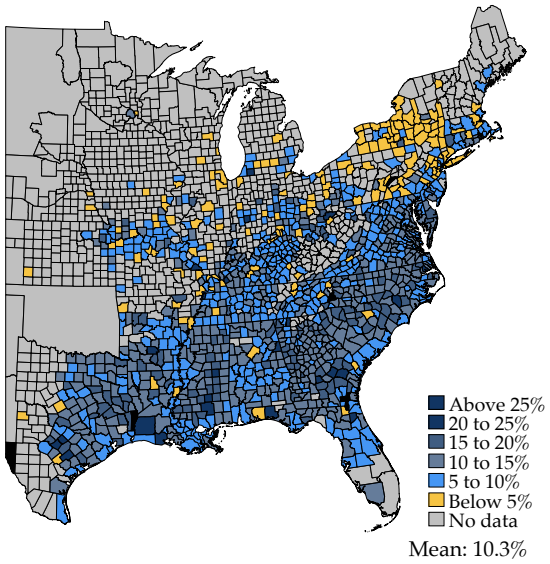
	Free (1860)			Enslaved (1870)		
	Linked	Population	Δ	Linked	Population	Δ
Literacy (%)	65.1	66.8	-3%	20.4	20.4	0%
Occupation Score	6.0	6.1	-1%	3.7	3.8	-1%
Real property ($)	1,217	1,230	-1%	1,400	1,270	10%
Personal property ($)	312	316	-1%	312	293	6%
Lives in North (%)	45.1	52.1	-13%	7.8	8.2	-4%
Lives on Farm (%)	21.2	18.2	17%	23.8	23.2	3%
Observations	20,994	79,374		190,676	726,667	

Notes: This table suggests that sample selection is small and negative among the Free and more positive among the formerly Enslaved. If anything, this kind of selection biases the Free-Enslaved gap toward zero. The left panel compares the Free who can be linked to any future decade to the entire population (which only consists of free Black Americans) in 1860. The right panel compares the formerly Enslaved (anyone not classified as Free by our algorithm) in 1870 who can be linked to any future decade to the 1860 population of the formerly Enslaved.

In addition, a family's socioeconomic status may affect not only *whether* they can be linked across decades but also *over how many decades* they can be linked. For example, children who grow up with single mothers can typically not be linked to their grandparents because women cannot be linked by these methods due to name changes at marriage. Our classification algorithm identifies descendants of the Free mainly through whether they can be linked back to 1850 or 1860, which could lead to an almost mechanically higher socioeconomic status. We addressed this concern in Section 3.4 (see Figure 2).

Last, one may be concerned that the effect of place in 1870 on outcomes in 1940 may be biased by differences in linking rates across those locations. In particular, areas with large Black populations may have lower linking rates because the linking relies on the uniqueness of a person's name, state of birth, and age. Lower linking rates may imply that only individuals with particularly rare names—and therefore potentially different socioeconomic statuses—are selected into the sample. Figure C.40 addresses this concern by showing counties' average likelihood of a resident in 1870 being linkable to the 1940 census. Linking rates are similar across the country except for the most sparsely populated counties in the North (which do not contribute to most of our main results).

FIGURE C.40: Linking Rates by County from 1870 to 1940



Notes: This figure shows the average linking rate for Black prime-age (20–54) men in 1870 to 1940. Only counties with a Black population of at least 50 prime-age men in 1870 are included.

TABLE C.16: Abolition of Slavery in the North

Year	State	*De Jure* Abolition of Slavery	*De Facto* Number of Slaves	
			Year	Total
1777	Vermont	Slavery was banned immediately upon founding of Vermont (Constitution of Vermont, 1777).	1790	0[79]
			1800	0
			1810	0
			1820	0
			1830	0
			1840	0
			1850	0
1780	Pennsylvania	Law of gradual emancipation passed in 1780 (Pennsylvania General Assembly, 1780). Black Americans born to enslaved mothers after 1780 would be freed at age 28. Slavery was ended in 1847.	1790	3,737 (36%)
			1800	1,706 (10%)
			1810	795 (3%)
			1820	211 (1%)
			1830	403 (1%)
			1840	64 (0%)
			1850	0
1781	Maine Massachusetts	Slavery was abolished by Supreme Judicial Court rulings in three related court cases, collectively known as the "Quock Walker case" (Cushing, 1961; Zilversmit, 1968). Slavery was ruled incompatible with the new state constitution of 1780.	1790	0
			1800	0
			1810	0
			1820	0
			1830	3 (0%)
			1840	0
			1850	0
1783	New Hampshire	Similar to Massachusetts, New Hampshire's constitution essentially abolished slavery by stating "all men are born equal and independent" (Constitution of the State of New Hampshire, 1783). However, it is not clear whether court rulings indeed interpreted the constitution as being at odds with slavery or not.	1790	158 (20%)
			1800	8 (1%)
			1810	0
			1820	0
			1830	3 (0%)
			1840	1 (0%)
			1850	0
1784	Rhode Island	Law for gradual emancipation passed in 1784 (General Assembly of Rhode Island, 1784). Black Americans born to enslaved mothers after 1784 would be freed at age 18 (women) or 21 (men).	1790	952 (22%)
			1800	381 (10%)
			1810	108 (3%)
			1820	48 (1%)
			1830	17 (0%)
			1840	5 (0%)
			1850	0

TABLE C.16: Abolition of Slavery in the North

Year	State	*De Jure* Abolition of Slavery	*De Facto* Number of Slaves	
			Year	Total
1784	Connecticut	Law for gradual emancipation passed in 1784 (Connecticut General Assembly, 1784). Black Americans born to enslaved mothers after 1784 would be freed at age 25. This age was lowered to 21 in 1797. Slavery was abolished in 1848.	1790	2,759 (50%)
			1800	951 (15%)
			1810	310 (5%)
			1820	97 (1%)
			1830	25 (0%)
			1840	17 (0%)
			1850	0
1787	Ohio Indiana Illinois Michigan Wisconsin Minnesota	The Confederation Congress's Northwest Ordinance of 1787 both banned and enforced slavery (Confederation Congress, 1787). A clause allowed Northerners to capture and enslave runaway slaves. Slavery was abolished by Ohio in 1802, Indiana in 1816, and Illinois in 1818.	1790	–
			1800	135 (21%)
			1810	429 (28%)
			1820	1,106 (40%)
			1830	788 (5%)
			1840	348 (1%)
			1850	0
1799	New York	Law for gradual emancipation passed in 1799 (New York State Legislature, 1799). Black Americans born to enslaved mothers after 1799 would be freed at age 25 (women) or 28 (men). In 1817, state decided to free all slaves born before 1799 (but not their children) in 1827 (New York State Legislature, 1817).	1790	21,324 (82%)
			1800	20,343 (66%)
			1810	15,017 (37%)
			1820	10,088 (26%)
			1830	75 (0%)
			1840	4 (0%)
			1850	0
1804	New Jersey	Law for gradual emancipation passed in 1804 (New Jersey State Legislature, 1804). While not freeing living slaves, Black Americans born to enslaved mothers after 1804 would be freed at age 21 (women) or 25 (men).[80]	1790	11,423 (81%)
			1800	12,422 (74%)
			1810	10,851 (58%)
			1820	7,557 (38%)
			1830	2,254 (11%)
			1840	674 (3%)
			1850	236 (1%)

Notes: This table provides a timeline for the abolition of slavery in the North. The first column indicates the year which we choose as the states' final year of slavery. We classify any Black American born in the state after this cutoff as free. The third column shows the laws that abolished slavery. In many cases, slavery was not abolished outright, but rather it was restricted in ways that would imply a person is free before 1865 in all likelihood. The final column shows the actual number of slaves who reside in the state and the percentage of the state's Black population being enslaved in parentheses. The number of slaves is taken from aggregate counts in census records (1790–1850).

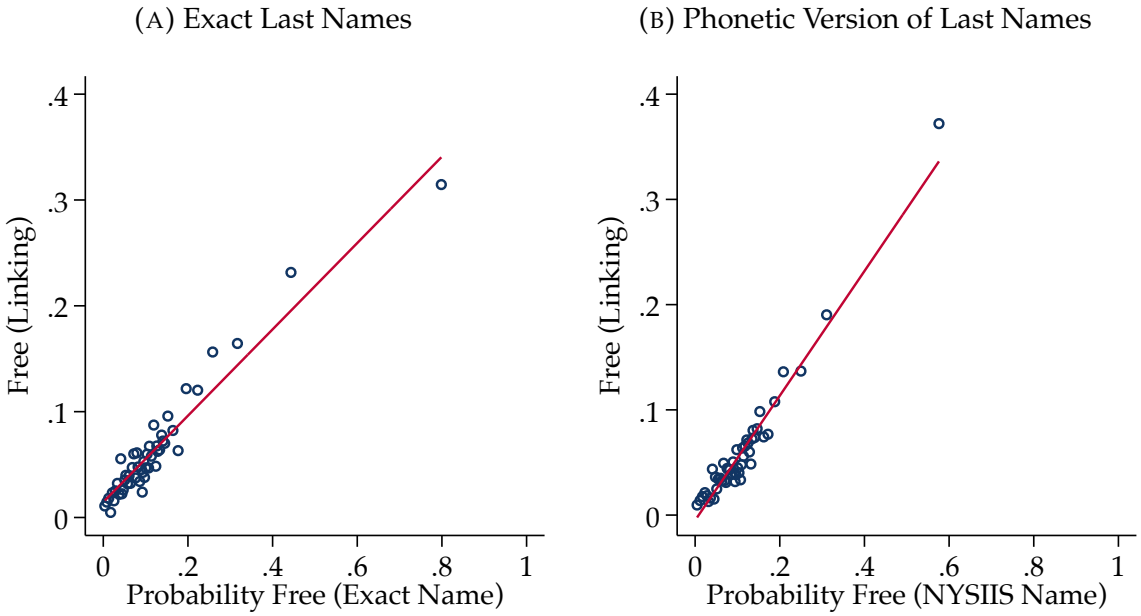
[80] While the 1790 census states that 16 slaves were in Vermont that year, this is likely an error.

[80] There is some evidence that after 1804, some Black Americans were sold to slave states before they

C.4.2 Alternative Method of Free-Enslaved Classification: Distribution of Last Names

While our main method provides a high-accuracy classification of descendants of the Free and the Enslaved, accuracy comes at the cost of reduced sample sizes due to imperfect linking rates across the decades. To use the full census sample of Black Americans after 1870, rather than a linked sub-sample thereof, we develop an additional strategy for identifying descendants of the Free and the Enslaved based on last names. Figure C.41 shows that the name-based measures are highly correlated with the free status based on our preferred measure, though they are attenuated as expected.

FIGURE C.41: Comparing Name-Based and Linking-Based Measures



Notes: This figure compares the probabilistic measures of descending from free Black Americans with our preferred measure based mainly on census linking. This binned scatter plot shows that among Black prime-age men in the 1940 census, the fraction of people classified as Free closely coincides with the predicted probability based on the people's last names.

Our alternative classification algorithm uses changes in the distribution over last names from 1850–1860 to 1870–1880. Before 1865, the census only includes free Black Americans—after, it also includes the formerly Enslaved and their descendants.

We compute the relative frequency of each last name before and after 1865. We then create a measure of how likely a person is to descend from the Free by dividing their last name's relative frequency before 1865 by its relative frequency after 1865. For example, the last name Du Bois appears with relatively high frequency in the 1850 and 1860 censuses, while Freedman does not appear at all. After the four million formerly enslaved individuals enter the census sample in 1870 and 1880, the name Du Bois is far less (one-tenth) frequent, whereas a substantial number of individuals entered the sample with

reached the age to be emancipated (Armstead et al., 2016, p.104).

the last name Freedman for the first time. These changes suggest that anyone named Du Bois after 1865 likely descends from the Free, whereas anyone named Freedman likely descends from the Enslaved. Note that not all names give us a good idea of whether a person descends from the Enslaved or not. Especially names very common among Black Americans before 1865, such as Johnson, Brown, or Smith, remain very common after 1865. Other names such as Washington did exist among Black Americans before 1865, but became substantially more common after many newly freed enslaved people chose this name in honor of the country's first president.

Formally, using the example of the last name Du Bois, we estimate the name-specific likelihood of descending from free Black Americans defined as

$$\begin{aligned} P(Free_{it} = 1 | Name_i = \#DuBois_t) &= \frac{P(Free_{it} = 1, Name_{it} = \#DuBois_t)}{P(Name_{it} = \#DuBois_t)} \\ &= \frac{P(Free_{i,1860} = 1, Name_{i,1860} = \#DuBois_t)}{P(Name_{i,1870} = \#DuBois_t)} \\ &= \frac{P(Name_{i,1860} = \#DuBois_t)}{P(Name_{i,1870} = \#DuBois_t),} \end{aligned}$$

where the second equation follows from assuming that a last name conveys a constant probability of descending from free Black Americans. The last equation follows from the fact that the 1860 census only contained free Black Americans. This equation can be approximated by

$$\widehat{P}(Free_{it} = 1 | Name_{it} = \#DuBois_t) = \frac{\#(\#DuBois_t)_{1860} / BlackPop_{1860}}{\#(\#DuBois_t)_{1870} / BlackPop_{1870}},$$

where $\#DuBois_t$ is the number of individuals with the last name Du Bois in a given year and $BlackPop_t$ is the population of all Black Americans (free and enslaved). To reduce noise, we combine the names from the 1850 and 1860 censuses as a pre-1865 count and the 1870 and 1880 censuses as a post-1865 count. Before 1865, we compute the population by adding up the census sample size (the Free) and the number of the Enslaved (Berlin, 1974). We truncate our estimated probability by 0 and 1. Names that only appear pre-1865 but not post-1865 are assigned probability 1; those that only appear post-1865 are assigned probability 0.

To allow for misspellings, we also compute this measure based on the phonetics of last names. Specifically, we transform last names using the New York State Identification and Intelligence System (NYSIIS) phonetic code. For example, the last names "Browne" and "Brown" both become "Bran." For placebo exercises, we also compute the above measure as a pseudo-probability of being free for white Americans as well as for 1875 as a time placebo for Emancipation.

C.5 Main Sample

For our main sample, we focus on prime-age (20–54) Black men who can be linked to ancestors in 1880 or before. Our focus on prime-age individuals provides a certain form of comparability, limiting the possibility that an individual is in school or retired. We focus on men because we rely on automated census-linking techniques that are either unavailable or have notoriously low coverage for women. We restrict the sample to individuals who can be linked back to 1880 or before for two reasons. First, this requirement excludes families who migrated to the US after 1880. Any comparison made in our analysis will thus be for individuals whose families have lived in the country for at least 60 years. Second, it reduces the potential for linking bias as discussed in Section C.4.

C.6 Individual-Level Outcome Variables

Our main outcomes variables can be categorized as (proxies of) income, education, or wealth. Most individual-level data draw on census records provided through IPUMS (Ruggles et al., 2020).

Income

- **Occupational income scores, 1850–1940 (census).** Because the census does not include any continuous measure of income before 1940, researchers have instead relied on occupational income scores. The most popular version, "occscore," reflects the median total income of a person in that occupation in 1950.

- **Lido income scores, 1850–1940 (Saavedra and Twinam, 2020).** Occupational income scores do not contain any age-, sex-, or race-specific information. The recent literature has used regression and machine learning techniques to improve on the traditional occupational income score (e.g., Saavedra and Twinam, 2020; Abramitzky et al., 2021). We use the Lido score constructed by Saavedra and Twinam (2020). The authors constructed it using machine learning techniques using 1950 and 2000 census data to validate their results against occscore in the 1915 Iowa census. According to Abramitzky et al. (2021), the Lido score has a correlation of 0.99 with their own measure.

- **Occupational skill, 1850–1940 (Leeuwen and Maas, 2011).** We use HISCLASS, a classification to compare occupations based on the skill they typically required. The classification ranges from "higher managers" to "unskilled farm workers." We coarsen this classification by assigning "skilled" to every occupation classified as "medium skilled workers" or above and "unskilled" to everyone else.

- **Wage income, 1940 (census).** We use wage income for 1940, the only year it is available for in our sample period.

Education

- **Literacy, 1850–1940 (census).** We use literacy for all years. In 1940, literacy becomes unavailable, and instead the census starts to include educational attainment. We proxy for literacy by having completed at least the second grade.
- **Years of education, 1940 (census).** We impute years of education from the highest educational level attained ("educd").
- **High school, 1940 (census).** We impute whether a person holds a high school degree based on whether they completed at least 12 years of schooling ("educd").
- **College, 1940 (census).** We impute whether a person holds a college degree based on whether they completed at least 16 years of schooling ("educd").
- **Graduate, 1940 (census).** We impute whether a person holds a graduate degree based on whether they completed at least 17 years of schooling ("educd").

Wealth

- **Personal property, 1860–1870 (census).** Measures "the contemporary dollar value of all stocks, bonds, mortgages, notes, livestock, plate, jewels, and furniture" as reported to the census. It is not clear whether zeros indicate missing values or true zero personal property, and therefore we replace zeros with "missing."
- **Real property, 1850–1870 (census).** Measures "the contemporary dollar value of any real estate owned by the respondent" as reported to the census. It is not clear whether zeros indicate missing values or true zero personal property, and therefore we replace zeros with "missing."
- **Homeownership, 1850–1940 (census).** Measures whether the individual rents or owns their home. For 1900 to 1940, the census reports homeownership directly. For 1850 to 1870, we follow Collins and Margo (2011) in imputing homeownership status using information on wealth, where every household with positive real property is classified as owner-occupied. Collins and Margo (2011) exempt households who live in multi-family homes from this classification but the information necessary to follow them in doing so is not included in the full-count version of the census we use. However, creating homeownership proxies using their and our method yields a correlation of 0.9733 in the 1 percent sample.

- **House value, 1930–1940 (census).** Measures the house value conditional on owning the house.

C.7 Neighborhood-Level Outcome Variables

While we cannot link our data to censuses after 1940, we can link the 1940 census to administrative death records from 1988 and 2005 using the CenSoc-Numident file (Goldstein et al., 2021). Importantly, the death records contain the nine-digit ZIP codes of residence at the time of death. We link these codes to statistical census geographic areas, i.e., census tracts, block groups, and blocks (see Section C.9 for more detail on the procedure). Census tracts contain between 1,200 and 8,000 people and are designed to be "relatively homogeneous units with respect to population characteristics, economic status, and living conditions" (Census Bureau, 2017). Block groups (between 600 and 3,000 people) and blocks are subdivisions of a census tract.

We assigned to each decedent various socioeconomic characteristics based on these statistical areas at the time of death. Since the sample is about evenly split between deaths before 2000 and deaths after 2000, we used the aggregated census data for the year 2000 from the NHGIS database. For variables from other sources, we selected the data to refer to a period as close to 2000 as availability allowed.

One potential concern with this data may be that many people live in retirement homes, possibly making the neighborhood a less precise proxy of a person's socioeconomic status. To assess this potential issue, we compare the density of deaths with a ZIP code's population density and find that the two are highly correlated ($\rho = 0.91$). We show that our results are robust to dropping ZIP codes that have far higher rates of deaths than predicted by their population density (see Table B.10).

Income

- **Income, 2000 (NHGIS).** The median household income by race of householder. Available by ZCTA, census tracts, and block groups.

Wealth

- **House value, 2000 (NHGIS).** The median value of owner-occupied housing units by race of householder. Available by ZCTA and census tracts.
- **Homeownership, 2000 (NHGIS).** The share of occupied housing units that is occupied by the owner (relative to a renter) by race. Available by ZCTA, census tracts, block groups, and blocks.

Education

- **High school degree, 2000 (NHGIS).** The share of the population over 25 years old by race and sex who hold a high school degree. Available by ZCTA, census tracts, and block groups.
- **College degree, 2000 (NHGIS).** The share of the population over 25 years old by race and sex who hold a college degree. Available by ZCTA, census tracts, and block groups.

Health

- **Age at death, 1988–2005 (BUNMD, Goldstein et al., 2021).** The median age at death by race and sex. Available by five-digit ZIP code, census tracts, block groups, and block.
- **Life expectancy, 2010–2015 (National Center for Health Statistics, 2021).** Estimates of life expectancy at birth. Available by census tracts.
- **Physical health, 2017–2018 (CDC, 2020).** Prevalence of poor physical health in the last 14 days among individuals over 18 years. Available by ZCTA and census tracts.
- **Mental health, 2017–2018 (CDC, 2020).** Prevalence of poor mental health in the last 14 days among individuals over 18 years. Available by ZCTA and census tracts.

Miscellaneous

- **Evictions, 2000–2005 (Desmond et al., 2018).** Number of eviction filings and evictions per 100 renter-occupied households. Available by census tracts and block groups.
- **Fatal police encounters, 2000–2021** (`fatalencounters.org`). "Fatal Encounters documents non-police deaths that occur when police are present or are precipitated by police action or presence. Officer deaths are included when caused by another officer, including friendly fire incidents, and criminal actions—like domestic violence—and suicides that occur when other officers are present. Officer vehicle-related deaths are included when they are caused by another officer. Homicides of officers by felons or deaths in the regular course of duties are not generally documented in the database." The data cover the entire US from 2000 to September 09, 2021, but the database is continuously updated by journalist D. Brian Burghart. Available by five-digit ZIP code.

- **Racial segregation, 2000 (NHGIS)**. The Theil Index of racial segregation using the racial composition of a census tract or block group relative to the block that it contains (Theil, 1972). For the exact formula, see equation (4) in Chetty et al. (2014). Whereas Chetty et al. (2014) compute the measure on the level of the commuting zone relative to the census tracts it contains, we compute the measure on the level of a census tract and block group relative to the block. Available by census tracts and block groups.

- **Percentage Black, 2000 (NHGIS)**. The share of the population that is Black. Available by ZCTA, census tracts, block groups, and blocks.

C.8 County Characteristics

To assess the characteristics of "good" and "bad" counties and the persistence over time, we compile a dataset on the county level. (Manson et al., 2021).

- **Distance to the North, East (NHGIS).** A county's distance to the North and the East is proxied by its centroid's latitude and longitude.

- **Free, 1860 (NHGIS).** Measures the percentage of a county's 1860 Black population that is free.

- **Black, 1860 (NHGIS).** Measures the percentage of a county's 1860 population that is Black.

- **Tobacco, cotton, rice, and sugar, 1860 (NHGIS).** Measures the value of a county's tobacco, cotton, rice, or sugar output as a percentage of the total agricultural output in 1860.

- **Population density, 1870 (NHGIS).** Measures a county's 1870 population per square kilometer area.

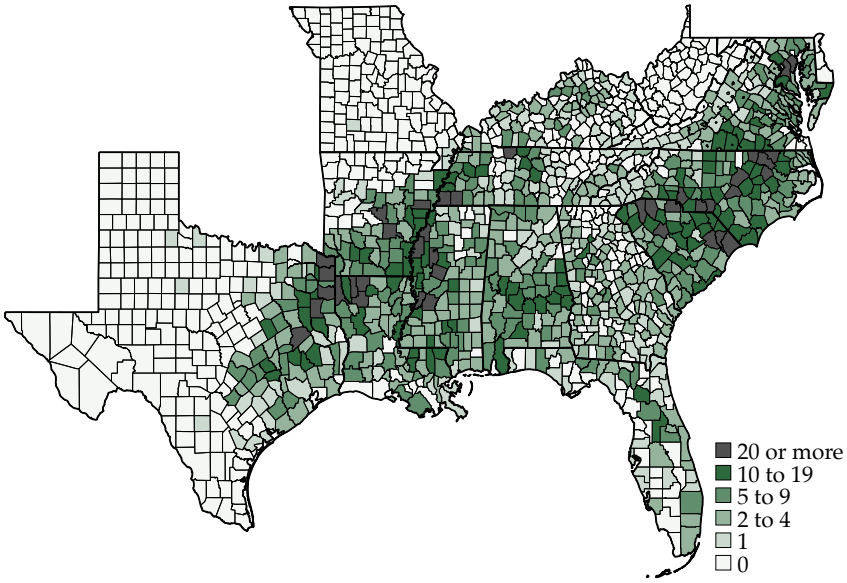
- **School, 1870 (NHGIS).** Measures the fraction of a county's Black children (ages 6–16) attending school in 1870.

- **Farm, 1870 (NHGIS).** Measures the fraction of a county's population living on a farm in 1870.

- **Migration cost North, 1870 (Donaldson and Hornbeck, 2016).** Measures the transportation cost through land and water ways from a given county to the Northern cities that were the main destinations of the Great Migration: Chicago, Detroit, Pittsburgh, and New York. The migration cost estimates are based on the 1870 railroad network.

- **Lynchings, 1883–1941 (Seguin and Rigby, 2019).** Measures the number of lynchings that occurred in a county between 1883 and 1941.
- **Racial segregation, 1880 and 1940 (Logan and Parman, 2017).** Measures racial segregation based on a comparison of the probability of different-race neighbors in a county relative to the counterfactual probability had the population been randomly distributed across the county.
- **Slaves per slaveholder, 1860 (NHGIS)**. The average number of enslaved people per slaveholder.
- **Intergenerational mobility, 1996–2012 (Chetty and Hendren, 2018).** Measures the causal effect of a county on the expected rank in the national income distribution conditional on one's parents' income ranking at the 25th percentile during childhood.
- **Rosenwald schools, 1919–1931 (Aaronson and Mazumder, 2011).** Number of Rosenwald schools and teachers in each county and year.

FIGURE C.42: Number of Rosenwald Schools in 1931



Notes: This figure shows the cumulative number of Rosenwald schools that were built in each county until 1931 using data from Aaronson and Mazumder (2011).

C.9 Nine-Digit ZIP to Census 2000 Crosswalks

The administrative death records contain nine-digit ZIP codes ("ZIP9") of the place of residence at the time of death. We use the Census Bureau's TIGER/Line ASCII files (1994, 1995, 1997, 1998, 1999, 2000, 2002, 2003, 2005, and 2006) to link ZIP9s to 2000 census statistical areas (i.e., census blocks, block groups, and census tracts). A ZIP9 is a

characteristic of a range of addresses, usually a side or segment of a street. The relevant records in the TIGER/Line files for our purpose are record types 1, 6, and Z.[81] Each entry in record type 1 represents a complete chain (a street segment) and contains the five-digit ZIP ("ZIP5") for the main address range of the complete chain. It also contains the census block number of the polygon on either side of the complete chain.

Record type 6 provides remaining address ranges and their ZIP5s in case the relevant segment of a street is associated with multiple address ranges. Record type Z provides ZIP+4 add-on codes for each address range in record types 1 and 6. Merging the three record types, we obtain a database of ZIP9s and corresponding blocks. The TIGER/Line versions before 2000 linked ZIP9s to 1990 census areas. For those versions, we extract the ZIP9 and longitude and latitude of the beginning and end of the street segment that the complete chain corresponds with. Then, we map the street segment's midpoint onto the 2000 census shape files.

In most cases, a ZIP9 maps into a unique block (and hence maps into a unique block group and census tract). For instance, in 2000, 81 percent of ZIP9s were matched to a unique block. For block groups and census tracts, 96 percent and 97 percent of the ZIP9 matches were unique, respectively. In cases where a ZIP9 occurs in more than one statistical area, we assign the area that has the largest number of matches with the relevant ZIP9. This yields a one-to-one mapping of ZIP9s to blocks for each TIGER/Line year between 2000 and 2006. However, not all ZIP9s in the Censoc-Numident death records occur in the TIGER/Line files. To improve the coverage, we sort the data by ZIP9 for each version and interpolate the census statistical areas in case the next non-missing census area is exactly equal to the previous non-missing area (using that the ZIP9s are ordered geographically).

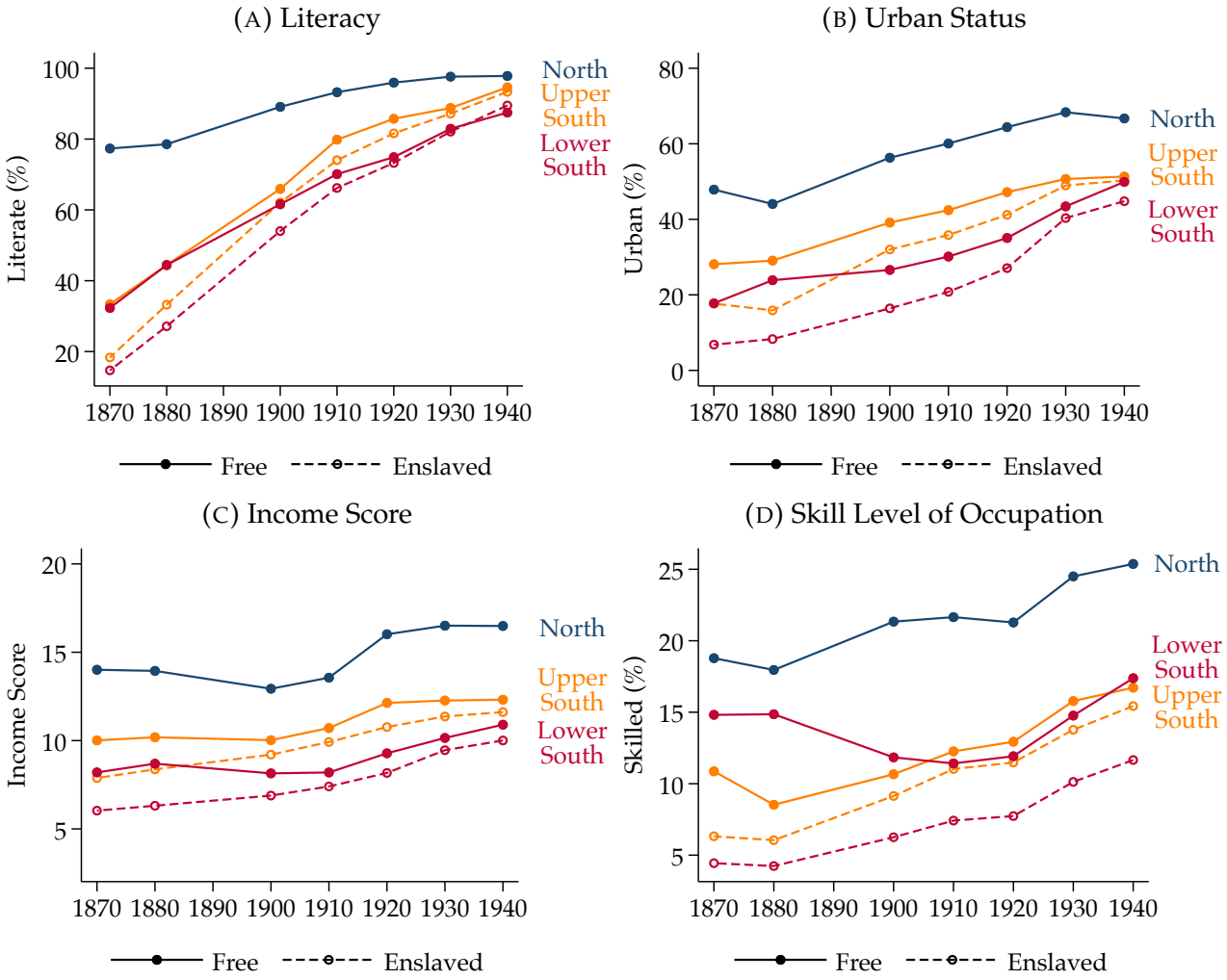
Last, for each decedent, we assign the census area corresponding to their ZIP9 derived from a TIGER/Line version before and after their year of death (if available). For instance, if someone was born in 1996, we first try to assign the census area based on the TIGER/Line in 1995 and in 1997. If either of them is not available, we try to match using the next proximate version. Using this procedure, we link around 84 percent of the decedents with ZIP9s to a census tract, 82 percent to a block group, and 77 percent to a block. For decedents for which we can find the census area corresponding to their ZIP9 both before and after their death, the agreement rate between the different versions is high (98 percent for census tracts, 96 percent for block groups, and 88 percent for blocks).

[81] Extensive documentation is available online for each TIGER/Line version.

C.10 Descriptive Statistics

C.10.1 Socioeconomic Status of Descendants of the Free and the Enslaved

FIGURE C.43: Socioeconomic Characteristics of Family by Region of Origin (1870-1940)

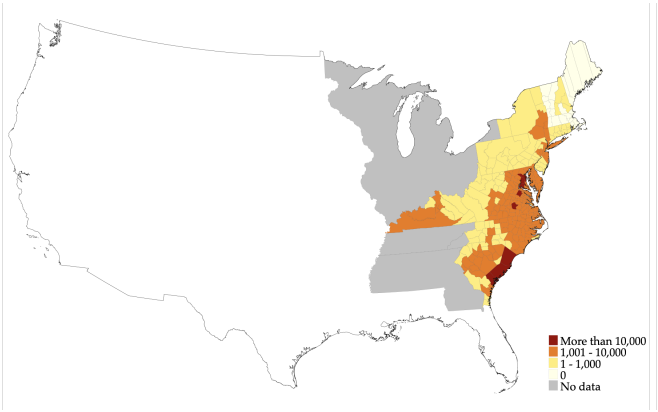


Notes: This figure shows the averages of characteristics in the cross-section of prime-age male descendants of the Free and the Enslaved by their ancestor's region (family's residence pre-1880). Incomes Score uses the Lido score developed by Saavedra and Twinam (2020). Skill level of occupations is inferred from the classification by Leeuwen and Maas (2011). See Data Appendix C for details on the sample and data.

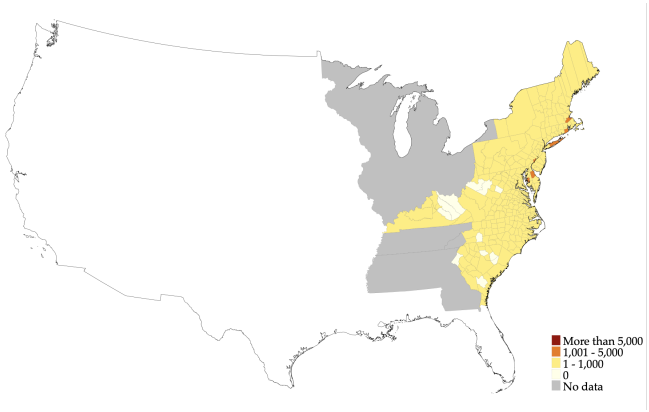
C.10.2 Migration among Descendants of the Free and the Enslaved

FIGURE C.44: County Population of Enslaved and Free (1790 & 1860)

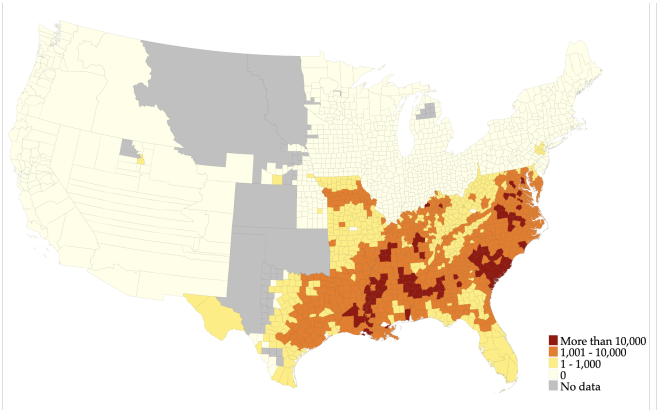
(A) The Enslaved in 1790



(B) The Free in 1790



(C) The Enslaved in 1860



(D) The Free in 1860

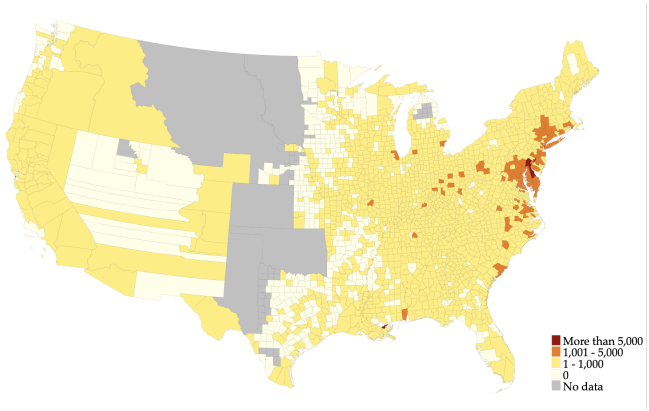
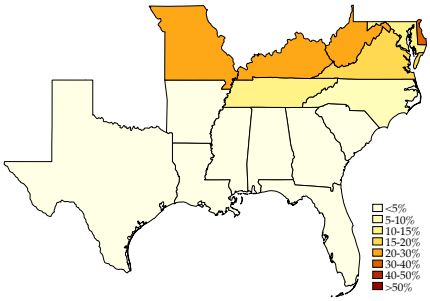
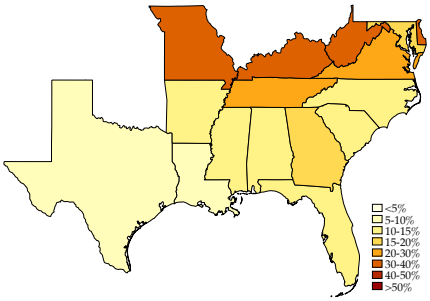


FIGURE C.45: Black Families Leaving the Slave States by 1870 State of Origin

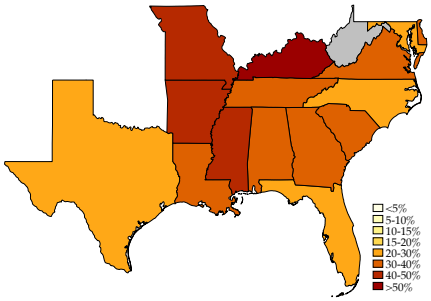
(A) By 1920



(B) By 1940

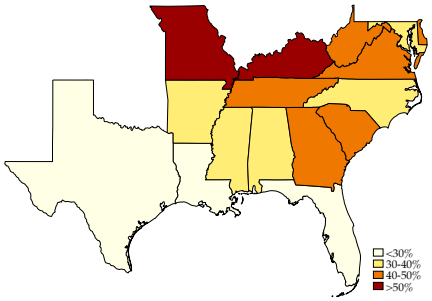


(C) By 2000



Notes: This figure shows the cumulative fraction of Black families who live outside the slave states, by the state their 1870 ancestor was born. The figure highlights that the first wave of the Great Migration from 1910 to 1940 was mainly an Upper Southern phenomenon (see Panels A and B). Black families with roots to the Lower South only caught up with those rates of migration to the North after 1940 (see Panel C).

FIGURE C.46: Black Families Leaving their 1870 State of Origin



Notes: This figure shows the fraction of Black families who in 1940 live outside the state in which their ancestors were enslaved. As the state of enslavement, we use the state of birth of formerly enslaved ancestors in the 1870 census.

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D. Model Appendix

We think of the effect of being enslaved until 1865 as the expected difference between the descendants of the Enslaved and descendants of the Free holding constant any confounding factors (i.e., ability). That is, we define the average "treatment" effect (ATE) at $t = 1$ as

$$\begin{aligned} ATE \equiv \int \left(\mathbb{E}[y_{i,1} \mid s_i = 1, \alpha_{i,0}] - \mathbb{E}[y_{i,1} \mid s_i = 0, \alpha_{i,0}]\right) dF(a_{i,0}) = \\ = \int \mathbb{E}\left[\rho\left(\gamma^0_{\ell(i,0)} - \delta\right) + \gamma^1_{\ell(i,1)} \middle| s_i = 1, \alpha_{i,0}\right] dF(a_{i,0}) - \\ \int \mathbb{E}\left[\rho\gamma^0_{\ell(i,0)} + \gamma^1_{\ell(i,1)} \middle| s_i = 0, \alpha_{i,0}\right] dF(a_{i,0}). \quad (13) \end{aligned}$$

Importantly, in our definition, the effect of descending from an enslaved person includes not just the effect of delayed freedom but also any potential effect operating through differential exposure to location-specific factors. Combining (2), (3), and (13), the observed Free-Enslaved gap is equal to

$$\mathbb{E}[y_{i,1} \mid s_i = 1] - \mathbb{E}[y_{i,1} \mid s_i = 0] = ATE - B, \quad (14)$$

where the (negative of) the selection bias B, arising from (1) potential selection into being free, (2) potential selection into location by (descendants of) the Free, and (3) potential selection into location by (descendants of) the Enslaved:

$$\begin{aligned} B = \underbrace{\mathbb{E}\left[(\lambda + \rho)\,\alpha_{i,0} \mid s_i = 0\right] - \mathbb{E}\left[(\lambda + \rho)\,\alpha_{i,0} \mid s_i = 1\right]}_{\text{Potential selection into being free}} + \\ \underbrace{\left(\mathbb{E}[\rho\gamma^0_{\ell(i,0)} + \gamma^1_{\ell(i,1)} \mid s_i = 0] - \int \mathbb{E}[\rho\gamma^0_{\ell(i,0)} + \gamma^1_{\ell(i,1)} \mid s_i = 0, \alpha_{i,0}] dF(\alpha_{i,0})\right)}_{\text{Potential selection into location by (descendants of) the Free}} - \\ \underbrace{\left(\mathbb{E}\left[\rho\left(\gamma^0_{\ell(i,0)} - \delta\right) + \gamma^1_{\ell(i,1)} \mid s_i = 1\right] - \int \mathbb{E}\left[\rho\left(\gamma^0_{\ell(i,0)} - \delta\right) + \gamma^1_{\ell(i,1)} \mid s_i = 1, \alpha_{i,0}\right] dF(\alpha_{i,0})\right)}_{\text{Potential selection into location by (descendants of) the Enslaved}}. \end{aligned}$$

Naturally, if being free before the Civil War was a matter of pure chance, the differences between the Free and the Enslaved have a causal interpretation. A priori, this assumption is strong. However, the plausibility of the assumption depends crucially on the conditions under which freedom was attained.

There were five main channels into freedom between the Revolutionary War (1775–1783) and the abolition of slavery in 1865: 1) by emancipation through abolition of slavery in the North in the late 18th and early 19th century, 2) by manumission through one's master, 3) by manumission through self-purchase, 4) by manumission through purchase

by a third party, or 5) by running away. A person born to a free mother inherited their mother's freedom. In rare occasions, enslaved people were unintentionally freed by accompanying their masters on a trip to a free state. Setting foot on free soil freed enslaved people by law and some sued to enforce their rights (see, e.g., Rose, 2009).

In 1860, around half of the free population was born in the North, which we argue is a reasonable approximation of the share of the free families freed through general emancipation in the North. Within the remaining half, it is hard to estimate the share of people freed "legally" and those who ran away. While the 1850 and 1860 censuses suggest the number of runaway slaves is less than 2,000 (out of a total population of around 4 million), the true number is likely much higher (Franklin and Schweninger, 2000).

Dittmar and Naidu (2012) use runaway slave advertisements placed in Southern newspapers between 1840 and 1860 and suggest that such advertisements were placed for around 8,000 runaway slaves throughout those two decades. However, the authors also point out that "it is clear that among the many absconders only a small fraction remained at large for a lengthy period." The odds of a successful escape were especially small in the Lower South. This is corroborated by the fact that in a Pennsylvania census of Free Black Americans, only 2 out of 314 people who were not born free indicated that they attained freedom through escape.[82] It is therefore safe to conclude that the vast majority of those who became free in the South did so through manumission (as opposed to escape).

Since slavery had been de facto abolished in the North by 1850 (see Table C.16), the enslaved people there were freed non-selectively. That is, as long as one is willing to assume that those enslaved in the North were not inherently different from those enslaved in the South, those in the North were freed entirely independent of any observed or unobserved characteristics. In the South, the degree of selection into manumission varied largely across time and locations. Around the 1780s, the early years after the Revolutionary War, there was a stream of manumissions motivated by morality or religion. In later antebellum years, manumission turned into an instrument to uphold slavery (Berlin, 1974). It did not, in most cases, arise from anti-slavery sentiments. On the contrary, many owners manumitted their slaves as a reward for loyalty and by doing so "reinforced rather than challenged the values, assumptions, and discipline of slavery" (Wolf, 2006, p. 44).

One could imagine that the practice of manumission induced a degree of selection into being free. Indeed, some quantitative evidence on the presence of selection into manumission exists. Cole (2005) finds that in Louisiana, manumitted people were 62.5 percent female (43.6 percent in the enslaved population) and much more likely to be "mulatto" (38.5 percent) than the slave population (5.8 percent). This is consistent with the observation that manumission in the Lower South was reserved for "illicit offspring, special favorites, or least productive slaves" (Berlin, 1974). Bodenhorn (2011), too, finds evidence

[82] Pennsylvania Abolition Society and Society of Friends Manuscript Census Schedules, 1838. Available in machine-readable form through https://doi.org/10.3886/ICPSR03805.v1.

of preferential manumission for people of mixed race in Virginia. Similarly, Berlin (1974) argues that skilled slaves had a larger chance of accumulating enough wealth to be manumitted through self-purchase. Little is known about selection into being manumitted through purchase by other people (usually other free Black people). Runaways, however, "as a group, had always been more skilled, sophisticated, and aggressive than the mass of slaves" (Berlin, 1974, p. 160). Table D.17 summarizes the discussion.

TABLE D.17: Relative prevalence of and selectivity in different roads to freedom

	%	Degree of selection
Emancipation in North	≈ 50	None
Manumission by master	30-40	Varied across time and locations
Manumission by self-purchase	5-10	Potentially high
Manumission by a third buyer	5-10	Unknown
Escape	< 5	Potentially high

Notes: This table indicates a rough breakdown of the relative probability of attaining freedom in various ways. The percentage emancipated in the North is estimated by the fraction of free Black people born in the North in the 1860 census. The fraction that escaped is a conservative upper bound given the observations mentioned in the text. The remaining probability is attributed to manumissions. The distribution within manumissions is derived from (Bodenhorn, 2011): 10-20 percent through self-purchase, 10-20 percent through a third buyer, and the remaining 60-80 percent by the master.

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