# JIM CROW AND BLACK ECONOMIC PROGRESS AFTER SLAVERY *

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This article studies the long-run effects of slavery and restrictive Jim Crow institutions on Black Americans' economic outcomes. We track individual-level census records of each Black family from 1850 to 1940 and extend our analysis to neighborhood-level outcomes in 2000 and surname-based outcomes in 2023. We show that Black families whose ancestors were enslaved until the Civil War have considerably lower education, income, and wealth than Black families whose ancestors were free before the Civil War. The disparities between the two groups have persisted substantially because most families enslaved until the Civil War lived in states with strict Jim Crow regimes after slavery ended. In a regression discontinuity design based on ancestors' enslavement locations, we show that Jim Crow institutions sharply reduced Black families' economic progress in the long run. *JEL codes:* N3, H7, J15, J7, O15, P16.

## I. INTRODUCTION

Black Americans have faced a long history of economic oppression in the United States. Throughout the country's early history, slavery was legal—until around 1800 in the Northern states and until the end of the Civil War (1861–1865) in the

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South. Soon after slavery ended, Southern states created racially oppressive regimes that limited the economic progress of newly freed Black families—a set of institutions collectively known as Jim Crow. States' Jim Crow regimes instituted racial segregation, Black voter disenfranchisement, and restrictions on Black Americans' economic and geographic mobility.[1] The Jim Crow era persisted for almost 100 years and only ended with the passage of civil rights legislation in the 1960s, which outlawed racial discrimination.

This article studies the extent to which Black Americans' economic status continues to be shaped by their ancestors' historical exposure to racial oppression. Our results reveal that such exposure continues to affect Black families, primarily because it increased their likelihood of facing continued oppression under subsequent regimes. Specifically, we find that Black families whose ancestors were enslaved until the Civil War still have far lower economic status than those who were free before the Civil War. However, the importance of differential exposure to slavery in contributing to these disparities dissipated over the early twentieth century.[2] Instead, the gap faced by families formerly enslaved until the Civil War persists due to their disproportionate exposure to continued oppression under Jim Crow. The rapid southern expansion of the U.S. plantation economy meant that the longer a family was enslaved, the more likely they were to be concentrated in the southernmost states—later the epicenter of Jim Crow. The severe and long-lasting impact of Jim Crow institutions thus perpetuated the economic disadvantage faced by formerly enslaved families into the twenty-first century.

We develop new methods to overcome the challenge of measuring families' historical exposure to slavery and Jim Crow. First, we infer if a family was free before the Civil War based on their ancestors' presence in the 1850 or 1860 census, which only enumerated free Black people. We then trace enslavement status across generations using (i) automated record-linkage

1. Throughout this article, we use the term "Jim Crow" to refer to state-level institutions that limited Black Americans' civil rights. Examples include school segregation, vagrancy laws, and poll taxes.

2. To quantify differences in exposure to slavery, we estimate that the average free Black family was free 50 to 65 years before the Civil War. We do so by using aggregate counts of the Black population starting in 1790 and assuming that free Black families' fertility equaled that of white families (see Online Appendix B.5).

(Abramitzky et al. 2021) and (ii) a new surname-based approach (Ager, Boustan, and Eriksson 2021). Second, we measure a family's exposure to Jim Crow by combining their ancestors' location, traced through automated record-linkage, with proxies for each state's Jim Crow intensity. Finally, we relate our exposure measures to the outcomes of Black prime-age men. Our linking-based approach uses individual-level census data (1850–1940) and neighborhood-level proxies for the late-life economic status of individuals who experienced both the Jim Crow era and its aftermath, derived from mortality records (1988–2007) linked to the 1940 census. The surname-based approach extends the coverage from the linked sample to the entire historical census population and real-time credit bureau data (2023).[3]

Our first result is that today, Black families enslaved until the Civil War continue to have 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 the Free-Enslaved gaps were even larger immediately after slavery, their narrowing has been much slower than one would expect under standard rates of intergenerational mobility. We demonstrate the robustness of our results to measurement error in ancestors' enslavement status by combining our surname- and linking-based measures in an instrumental variable strategy.

Second, we find that the Free-Enslaved gap persisted because families enslaved until the Civil War were disproportionately concentrated in states that harmed Black economic progress after slavery. We use plausibly exogenous variation from enslavement locations to estimate each Southern state's effect on the descendants of those freed from slavery there. We find that these effects were large and drive the Free-Enslaved gap's persistence. Conditional on their ancestor's location, the economic status of Black Americans ceased to depend on their ancestor's enslavement status by 1940. Importantly, our results capture only the additional disadvantage faced by those enslaved until the Civil War, not the broader impact of slavery on all Black Americans regardless of when they gained freedom.

Third, Jim Crow institutions underlie the severely limiting effects of certain states on Black economic progress. To isolate

3. Due to data-sharing agreements, we cannot disclose the name of the credit bureau.

the effect of these state institutions from other factors, such as economic activity, culture, or climate, we use a regression discontinuity design that compares the outcomes of Black families freed across state borders. We find that with the onset of the Jim Crow era, Black economic progress began to diverge sharply across state borders. For example, families freed in Louisiana attained 1.2 fewer years of education by 1940 compared with families freed just a few miles away in Texas. Notably, the long-run border discontinuity estimates, which capture the effects of institutions, are nearly identical in magnitude to the overall long-run state effects, which encompass both institutional and noninstitutional factors. Moreover, these border differences increase with the difference in the intensity of states' Jim Crow regimes. These findings implicate state-level Jim Crow institutions as a central factor shaping the geography of Black economic progress and perpetuating the disadvantages faced by families enslaved until the Civil War.

We extensively validate our empirical strategy. For the border discontinuity design, we show that (i) gaps in the economic status of formerly enslaved people only arise with the beginning of Jim Crow (circa 1880); (ii) those gaps only exist for borders where states' Jim Crow regimes differ and increase with those differences; (iii) before Jim Crow there are no border gaps in counties' economic, agricultural, political, or demographic characteristics; (iv) with the beginning of Jim Crow, large border gaps emerge in key county-level outcomes targeted by those regimes, including votes cast per adult male and Black school quality; and (v) Jim Crow regimes did not harm white families' economic outcomes. 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. Both historical and new empirical evidence support our main identifying assumption that an enslaved person's birthplace is exogenous to future generations' potential economic outcomes. Because of high migration costs, partly due to Jim Crow's institutional barriers to mobility, a family's enslavement location is a strong indicator of their exposure to Jim Crow. However, as many families did migrate despite those barriers, we assess the role of migration in shaping place effects using a standard framework of random assignment with imperfect compliance.

We explore potential mechanisms of how Jim Crow regimes slowed Black economic progress using a newly compiled data set

on state-level Jim Crow laws. We classify Jim Crow laws by topic and find that the largest number pertains to education. Education is the target of 283 laws—one-third of all Jim Crow laws passed throughout the South. 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. Indeed, we find that the quality of Black schools drops sharply across borders with states that have more oppressive Jim Crow regimes. In addition, our main regression discontinuity estimates are similar when using educational Jim Crow laws or Black school quality, rather than more comprehensive measures of Jim Crow intensity. Statements from leading historians confirm that educational restrictions were likely a key factor in Jim Crow's negative effect on Black economic progress.

This article makes several contributions. First, leveraging new methods to link families' data across generations (Abramitzky, Boustan, and Rashid 2020), we generate new evidence on the mechanisms behind institutions' persistent effects (Acemoglu, Johnson, and Robinson 2002; Dell 2010; Donaldson 2018; Dell and Olken 2020). Second, we design methods to identify descendants of enslaved people, uncovering important economic differences among Black Americans based on ancestral enslavement status. Third, by analyzing exposure to Jim Crow, we find that systemic discrimination—the higher exposure to ongoing discrimination because of past discrimination (Cain 1986; Loury 2002; Darity 2005, 2022; Darity et al. 2017)—is central to the enduring legacy of racial oppression in the United States. We find that Black economic progress was rapid where conditions allowed, consistent with seminal works (Du Bois 1935; Woodward 1955; Ransom and Sutch 2001; Aaronson and Mazumder 2011; Naidu 2012; Wright 2013). Last, despite the recognized impact of location on upward mobility, its underlying causal mechanisms remain unclear (Chetty et al. 2014; Olivetti and Paserman 2015; Chetty and Hendren 2018). Our results show that institutions can play a key role in shaping upward mobility.

## II. Historical Context

This section provides historical context for the evolution of anti-Black institutions in the United States—from slavery to Jim Crow and beyond.

### *II.A. 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 had existed on U.S. 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 twenty 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 (Galenson 1981). From 1662 onward, the law 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, both the British and American leadership promised freedom to enslaved people willing and able to serve. 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 United States as free persons. As a result, the free Black population in some states increased dramatically.

The Revolutionary War also spread a spirit of egalitarianism, challenging the institution of slavery in some regions. In the North, the abolitionist movement grew 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.[4] All 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 antislavery beliefs, most manumitters freed all

4. The Lower South comprises Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, South Carolina, 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.

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%). 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, 31). 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%).

The legal and economic status of free Black Americans 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 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).

By the beginning of the Civil War (1861–1865), the enslaved population was concentrated in the Lower South (see Figure I). 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.

### *II.B. Freedom of All Black Americans after 1865*

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. 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; Frieden, Grossman, and Lowery 2024).

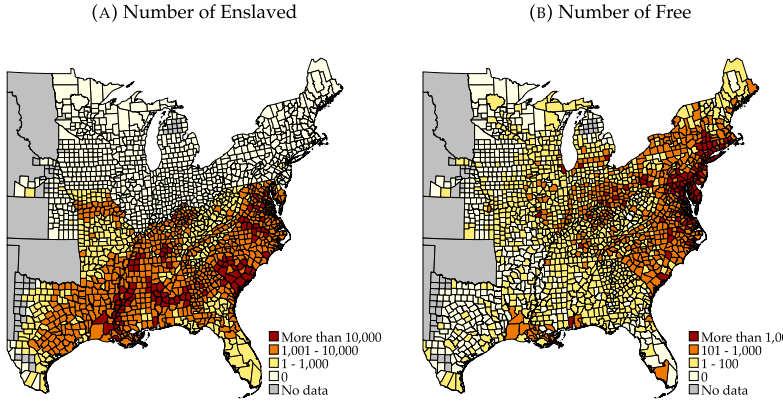


FIGURE I

Population by County in 1860

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 sparsely populated. Online Appendix Figure B.9 shows the maps for 1790.

New schools and 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 2008).

In 1877, the Union Army left the South, abandoning the project of Reconstruction. The disenfranchisement of Black people through legal and extralegal means led to massive declines in Black political participation (Kousser 1974; Wright 1986; Perman 2001; Naidu 2012). Many free Black Americans lost their higher social status and some 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 regimes 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, Puckett, and Deskins 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 2008). In addition to legal factors, various extralegal 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 participated less in this first wave, both because Jim Crow limited their geographic mobility and because migration was more costly for them (Roback 1984; Carrington, Detragiache, and Vishwanath 1996; Naidu 2010).

After almost 100 years, the civil rights movement successfully fought oppression starting in the mid-1950s and eventually ended Jim Crow—"one of the most significant legislative achievements in American history" (U.S. Senate 2019). The Great Migration continued until the end of the movement in the late 1960s. By then, 6 million Black Americans had left the South (Boustan 2016). However, many Black families still faced challenges in capitalizing on available opportunities in the North (Collins 1997; Akbar et al. forthcoming; Derenoncourt 2022). In addition, even after the achievements of the 1960s, old forms of racial oppression persisted, and new forms—such as mass incarceration and "color-blind" voter suppression—have arisen since (Western 2006; Alexander 2010; Bonilla-Silva 2015; Zaw, Hamilton, and Darity 2016). The narrowing of racial disparities has slowed substantially since the 1960s (Bayer and Charles 2018; Althoff 2021; Derenoncourt et al. 2024).

## III. Data and New Methods to Measure a Family's Exposure to Slavery and Jim Crow

A major empirical challenge we overcome in this article is to measure a Black family's exposure to slavery and Jim Crow. We construct family histories for Black Americans in the historical censuses and develop new methods to measure 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.

### *III.A. Measuring How Long a Family Was Enslaved*

To measure how long a family was enslaved, we leverage that the pre–Civil War censuses of 1850 and 1860 did not record enslaved people.

1. *Main Method Based on Census Linking.* We identify Black Americans free before 1865 (Free) as those who were (i) recorded in the 1850 or 1860 census or (ii) 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 (Enslaved).[5] We carry this information forward to their descendants. To do so, we build family trees using the census's information on family interrelationships for members of the same household and by linking individuals' records across time.

This classification strategy accurately identifies whether a Black family's ancestor was 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 a census. Hence, in the South, we inevitably misclassify some Black families who were free before 1865. However, census records show that only 6% of the Southern Black population were free in 1860. Therefore, our comparison involves a group almost certainly free in 1860 against a group where at least 94% were enslaved until the Civil War, minimizing the potential for attenuation bias due to imperfect linking rates (see also Online Appendix A.1). Record linkage helped us identify around 20% of free Black Americans in the 1870 census, 10% of whom we trace to descendants in 1940.

Our classification method has two critical 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 effects of slavery cannot be studied beyond the second

5. 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.

generation in the census cross section. Our panel allows us to follow individual Black families' records until 2000. Second and most important, relying on a person's birthplace can only identify free Black families born in the North. However, 50% of all Black families free before 1865 lived in the South. Our method 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.[6]

The Free-Enslaved gap quantifies disparities based on a family's male ancestry. Due to women's surname changes upon marriage, accurately linking female ancestry is challenging. Focusing on the male lineage minimizes bias that could arise from selective marriage patterns, allowing us to accurately estimate the Free-Enslaved gap as we define it. However, this approach limits our ability to estimate another important measure: the variation in economic status based on the proportion of Free versus Enslaved ancestors across both maternal and paternal lines. Given the vast geographic and socioeconomic divides between Free and Enslaved families, intermarriage between these groups was likely limited by 1940. This is corroborated by quantitative evidence and historical narratives (see Online Appendix A.2). However, we show that in the presence of intermarriage, even if limited, the Free-Enslaved gap serves as a lower bound for the disparities between families with exclusively versus no enslaved ancestors.[7]

2. *Alternative Method Based on Surnames.* We develop a second strategy to identify descendants of the Free and Enslaved based solely on surnames, without requiring census linkage. We use the change in the distribution over surnames from before 1865 (pooling the 1850 and 1860 censuses), when the census included only free Black Americans, to after 1865 (pooling the 1870 and 1880 censuses), when it included all Black Americans.[8]

6. See Online Appendix Figure B.1 for average socioeconomic outcomes among descendants of the Enslaved and the Free by region of origin.

7. In Online Appendix A.2, we derive this result theoretically. We estimate that for the first generation born after 1865, the gaps between Black Americans whose ancestors only descend from Enslaved versus free Black ancestors could be 15% larger than the Free-Enslaved gap.

8. Census pooling reduces the impact of imperfect coverage in any given decade.

While some surnames were common among the Free and the Enslaved, others were characteristic of one group (see Online Appendix Table B.1). For example, the surname 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 10 times less frequent—an indication that having this surname meant a person likely descended from the Free. In contrast, the surname Freedman did not exist in the 1860 census but appeared in the 1870 census after many newly freed families chose it as their new surname. Thus, Black families called Freedman were likely enslaved until 1865.

This surname-based approach allows us to measure the likelihood that one's ancestors were enslaved until the Civil War in any data set that includes surnames, such as the full (not just the linked) sample of Black Americans in the historical censuses as well as real-time credit bureau data. The linking-based and the surname-based approaches yield highly correlated Free-Enslaved classifications (see Online Appendix Figure B.2). We also provide evidence that the surname-based measure predicts outcomes only through Free-Enslaved status, ruling out other surname-related channels (see Online Appendix B.5).

### *III.B. 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 southern expansion of the U.S. 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 likely exposure to Jim Crow, we use record linkage to observe the birthplace of their formerly enslaved ancestors. A family's enslavement location is generally a strong indicator of their exposure to Jim Crow over the subsequent 75 years. Black Americans whose ancestors were enslaved in the Lower South were likely exposed to the strict Jim Crow regimes in the region for decades. Online Appendix Figure B.3 shows that prior to 1930, the share of Black families originat-

ing from the Lower South who migrated out of the region was less than 10%—significantly lower than the mobility rates experienced by Black families from the Upper South. Among families enslaved until the Civil War, the propensity to migrate North was especially low compared with Black families free earlier. However, it is worth noting that many families migrated despite Jim Crow's institutional barriers to mobility (Roback 1984; Wright 1986; Naidu 2010) and high migration costs (Carrington, Detragiache, and Vishwanath 1996). We formally account for migration in our econometric analysis.

Our primary measure of the intensity of states' anti-Black institutions, including their Jim Crow regime, is a composite index of persistent state-level racial oppression—the Historical Racial Regime (HRR) index (Baker 2022). This index is derived from four key components: a state's population share enslaved in 1860; its share of sharecroppers who were Black in 1930; its number of Jim Crow disfranchisement devices; and its share of congressional delegates that signed the Southern Manifesto.

To complement our analysis and validate our main findings, we consider alternative Jim Crow intensity measures. First, we create a new composite index that, in contrast to the HRR index, focuses on institutional factors and the Jim Crow era specifically. We derive this new Jim Crow index from five factors frequently referred to in the historical literature as reflections of Jim Crow regimes: (i) the anti-Black discriminatory share of a state's laws specific to race, (ii) a state's number of disenfranchisement devices, (iii) the share of congressional delegates who signed the Southern Manifesto, (iv) the Black-white disparity in schools' term lengths, and (v) the year minimum pay for teachers was introduced—legislation central to narrowing the large wage penalty historically suffered by Black teachers (Card, Domnisoru, and Taylor 2022; Cascio and Lewis 2024). This Jim Crow index is highly correlated with the HRR index ($\rho = 0.99$).

In addition, we consider a state's total number of Jim Crow laws. We analyzed over 800 laws from multiple sources, including newly digitized data from "States' Laws on Race and Color," which aimed to document all race-related state laws in 1950 (Murray 1950). We categorized each law as discriminatory (Jim Crow) or not based on its content and context provided by the authors. We incorporated additional laws on employment and suffrage not covered in the primary source (Roback 1984; Cohen 1991; Walton,

Puckett, and Deskins 2012). The number of Jim Crow laws correlates with the HRR index ($\rho = 0.74$).

Another measure we consider is a new composite index of Black school quality, derived from three factors: teacher salaries, student-to-teacher ratios, and term lengths for Black children in 1940—sourced from (Card and Krueger 1992). Black school quality negatively correlates with the HRR index ($\rho = -0.94$).

We acknowledge the challenge in quantifying the severity of Jim Crow regimes, which used both legal methods (e.g., literacy tests) and extralegal methods (e.g., voter intimidation) to marginalize Black Americans. As Woodward noted, "there [was] more Jim Crowism practiced in the South than there [were] Jim Crow laws on the books" (Woodward 1955, 102). While no single measure can fully capture this complexity, all of our different proxies are highly correlated (see Online Appendix Figure B.4). We argue that a collective analysis of our proposed measures offers valuable insights into the nature and extent of Jim Crow institutions in different states.

### *III.C. 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 nonadjacent decades using the automated linking methodology provided by Abramitzky, Boustan, and Rashid (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). Allowing for misspellings tends to be a more conservative approach because it treats phonetically similar names as equivalent, yet maintains the requirement for uniqueness in establishing a match. Because women tend to change their surname upon marriage, only men can be linked over time (Althoff, Brookes Gray, and Reichardt 2024).

The census also contains information on the relationship between individuals in the same household. By observing a person in their parents' household during child- or adulthood, we can build family trees based on this information. We transfer parental data, such as Free-Enslaved status and county of residence, to subsequent census records of the individual and their descendants. These family trees allow us to study the evolution

of a family's social, economic, and geographic mobility across generations. We study individuals' outcomes in census records between 1870 and 1940 (from the first census to include all Black Americans to the most recent full-count census available). Our primary outcomes include education, income, and wealth (Online Appendix B.1 describes all outcome variables in detail). Over time, the census data provide increasingly rich information on those outcomes. Therefore, we focus particular attention on the 1940 census.

To extend our analysis to the twenty-first century, we link the 1940 census to administrative mortality records from the Social Security Administration (Goldstein et al. 2021).[9] Effectively, this sample contains individuals born before 1940 and deceased between 1988 and 2007. The mortality records contain a person's last neighborhood of residence (nine-digit ZIP code) at the time of death. We use National Historical Geographic Information System (NHGIS) data on each neighborhood's distribution of education, income, and wealth by race to proxy for a person's economic status (see Online Appendix B.2 for details).

To extend our results to the present day, we combine the surname-based Free-Enslaved classification with real-time data from one of the primary U.S. credit bureaus. The credit bureau merged our probabilistic classification with their universe of credit reports before removing personally identifying information. The main outcomes include predicted total income, predicted disposable income, and credit score. Because those predictions are based on data and models proprietary to the credit bureau, our ability to validate the accuracy of these predictions is limited. However, recent work using similar credit bureau data validate the accuracy of these predictions using payroll records (Mello 2023). We subset the data to focus on Black prime-age men. The credit bureau does not observe a person's race directly and instead predicts it based on the person's first and last name as well as their neighborhood (nine-digit ZIP code).[10] We access a snap-

9. The linkage from 1940 to 2000 leverages automated methods based on a person's name, year of birth, and state of birth (Abramitzky, Boustan, and Rashid 2020), analogous to the linkage between 1850 and 1940.

10. Using a separate data set—our Social Security mortality records—we find that surnames and nine-digit ZIP codes combined capture 90% of the variation in whether a person is Black.

shot of this anonymous data from March 2023 through a secure server (see Online Appendix B.3 for further details).

### *III.D. Sample*

For our analysis, we focus on Black men aged 20 to 54 and limit the linked sample to individuals who can be traced back to their ancestors in 1880 or earlier. The latter restriction serves two purposes. First, our method for identifying families who gained freedom before 1865 requires linking them to their ancestors in 1850 or 1860. This requirement may introduce bias in the Free-Enslaved gap resulting from comparing families who can be linked back in time with those who cannot. By restricting the sample to Black Americans linkable to 1880 or earlier, we minimize this potential bias. Second, this restriction excludes families who immigrated to the United States after 1880, as they may have experienced significantly different institutional contexts prior to their arrival, which could confound our analysis. Our results are not sensitive to this restriction.

For 1940, 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. Linking a large number of descendants in 1940 to their Civil War–era ancestors is feasible for several reasons. First, to track an individual over time, we use links between both adjacent and nonadjacent census years. Second, we establish links between fathers and sons through their cohabitation. Third, the likelihood of establishing at least one link to a male descendant increases if an ancestor has multiple male descendants. On average, we make 3.7 links across different census decades to establish an 1870–1940 family tree. We link 10% of families in 1870 to at least one descendant in 1940 (see Online Appendix Table B.2). This statistic is essential because those links allow us to observe the state in which ancestors were freed from slavery via their birthplace in the 1870 census. Our data show a marginally higher match rate for free Black men compared with formerly enslaved men (18.5% versus 17.1%, respectively, from 1870 to 1880).[11] From the 1940 census

11. To evaluate linking rates by Free-Enslaved status, we contrast Black Americans born in the North (Free) with those from the South (mostly Enslaved), rather than basing the Free-Enslaved status on linkability in earlier decades. The relatively lower linking rates for Southern-born Black Americans may stem partly

to administrative records in 2000, we link 21,059 descendants of enslaved and 1,591 descendants of free Black families.

Our sample is highly balanced on observable characteristics (see Online Appendix Table A.1). For example, the literacy rate (20.4%) of those who we classify as formerly enslaved in our linked sample of 1870 matches the literacy rate of the 1870 Black census population—the vast majority of whom were enslaved until the Civil War. For free Black families in our linked sample of 1860, the literacy rate (65.1%) is also close to that of the 1860 Black census population (66.8%)—all of whom were free by definition of who was included in the census prior to 1865. The sample of individuals in 1940 linked to ancestors between 1850 and 1880 is also highly balanced compared to all Black men with U.S.-born parents in 1940.

1. *Potential Linking Bias.* 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 economic status depends on how many generations or decades they can be linked backward.

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 II). 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 economic status. To err on the side of caution, we limit our sample to individuals who can be linked back to 1880 or earlier.

## IV. A Simple Model of Black Economic Progress after Slavery

We propose a simple econometric model of Black economic progress to guide our interpretation of the forces that shape

from their larger population sizes, which decrease the likelihood of having unique names within their birth states.

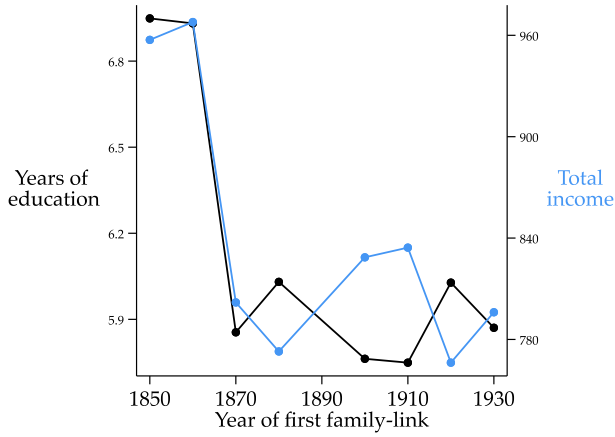


FIGURE II

Average Outcomes in 1940

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 line (left $y$-axis) shows the years of education; the light 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 Online Appendix B for details on the sample and data.

the Free-Enslaved gap's long-run persistence. Our framework incorporates intergenerational mobility, the effects of exposure to location-specific factors, (selective) migration, and the effect of delayed freedom. We use this model to answer the following questions. What factors determine the gap's long-run persistence? 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?

### *IV.A. 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 $\gamma_\ell^t$ as the effect of being exposed to location $\ell$ 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 $\delta \geqslant 0$ captures any direct advantage that free Black Americans had relative to the Enslaved, such as access to education during slavery.[12]

### *IV.B. 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 article, 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

12. 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 transmission of "ability" ($\lambda$) and through intergenerational advantage derived from "ability" in previous generations ($\rho$). 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 (i) how previous generations were affected by where they lived ($\gamma^0_{\ell(i,0)}$), (ii) whether their ancestors were enslaved until 1865 ($\delta$), and (iii) their ancestors' idiosyncratic human capital shocks ($\varepsilon_{i,0}$).

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).

## V. Economic Gaps between Descendants of Free and Enslaved Families

This section documents the gaps in education, income, and wealth from 1870 to 2023 between descendants of families enslaved until the Civil War and those freed earlier. We find that these gaps are large and persist to today.

### *V.A. Evolution of the Free-Enslaved Gap until 1940*

We estimate the Free-Enslaved gap ($\beta_t$) in economic outcomes ($y_{i,t}$) separately for each decade $t$ in our linked sample 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 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.[13]

We find that the economic differences between descendants of the Free and Enslaved are large and persistent. In 1870, the formerly Enslaved were two times (over 40 percentage points) more likely to be illiterate than free Black Americans (see Figure III). By 1940, the gap was still 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 lower incomes and are significantly less likely to own their homes (see Online Appendix Figure C.1). Overall, we estimate the Free-Enslaved gap to be smaller than the gap between Black Americans born in the North versus South before 1865—a comparison that Sacerdote (2005) uses as a proxy for the Free-Enslaved

13. We define a family as a group of individuals with a common 1870 ancestor. In 1940, our linked sample comprises 49,876 families with an average of 1.6 prime-age male descendants each.

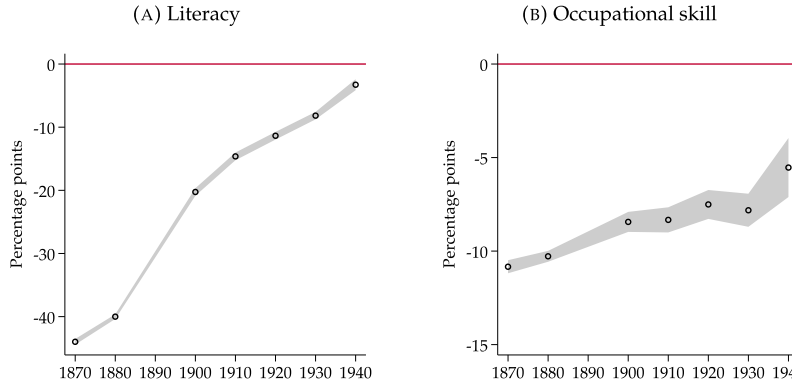


FIGURE III

Free-Enslaved Gap (1870–1940)

This figure shows the gaps in literacy and occupation skill among prime-age (20–54) male descendants of enslaved versus free Black Americans in each census decade. The sample includes both the South and North of the United States. In the 1940 census, instead of literacy, we observe the highest year of school or degree completed. We classify individuals who have completed at least two grades of school as literate; others we classify as illiterate. We assign "skilled" to occupations classified as "medium skilled workers" or above by the HISCLASS scheme (Van Leeuwen and Maas 2011) and "unskilled" to others. 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% confidence bands clustered at the family level. See Online Appendix B for details on the sample and data.

gap (see Online Appendix Figure C.2). Our estimates capture the important fact that free Black Americans fared far worse in the South than in the North after slavery.

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. Using those outcomes, we find that descendants of the Enslaved are less educated, earn lower incomes, and have accumulated less wealth than descendants of free Black Americans in 1940 (see Table I).[14] The gap in education amounts to 1.6 years—more than one-quarter of the average years of education among Black men in 1940. The likelihood that a descendant of the Enslaved earned a high school or college degree was only half compared with descendants of the Free (see Online Appendix Table C.2).

14. Online Appendix Table C.1 compares the Free-Enslaved gap across different income measures.

TABLE I
FREE-ENSLAVED GAP (1940)

| | Education (years) Mean: 5.99 | Wage income (US$) Mean: 381.20 | Homeownership (%) Mean: 29.25 | House value (US$) Mean: 1,371.95 |
|---|---|---|---|---|
| Ancestor enslaved until Civil War | −1.59*** (0.05) | −145.92*** (6.13) | −7.24*** (0.62) | −694.69*** (65.85) |
| Controls (age, age$^2$) | Y | Y | Y | Y |
| % of Black-white gap | 42 | 29 | 36 | 37 |
| 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 enslaved versus free Black Americans in 1940. The sample includes both the South and North of the United States. 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 Enslaved. See Online Appendix B for details on the sample and data. Standard errors are clustered at the family level and are shown in parentheses. *** $p < .01$, ** $p < .05$, * $p < .1$.

The narrowing of the Free-Enslaved gap from 1870 to 1940 is slow relative to benchmark rates of intergenerational mobility among white Americans. To compare the convergence speed, we estimate economic gaps from 1870 to 1940 between white families whose ancestors had no measurable physical or human capital in 1870 and all other white families (see Online Appendix Figure A.1). In only 30 years, the gap in literacy between those two groups of white 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 was similar to the respective Free-Enslaved gap in 1870 but closed by 1900—while the Free-Enslaved gap changed very little until then.

1. *Robustness.* We reestimate the Free-Enslaved gap based on the full population (rather than the linked sample) of Black Americans in 1940 using our surname-based approach, yielding results very similar to our preferred approach based on record linking (see Online Appendix Table A.2). The gaps between Black families with surnames that convey high versus low likelihoods of having been enslaved until the Civil War are −1.40 (0.09, $p < .01$) in years of education, −113.15 (25.50, $p < .01$) in wage income, −2.31 (1.05, $p < .05$) in homeownership, and −1,098.68 (282.83, $p < .01$) in house values.

Next, to mitigate misclassification bias, we use our surname-based measure as an instrumental variable (IV) for the linking-based measure. The resulting IV estimates offer an unbiased assessment of the Free-Enslaved gap if the errors in the linking-based measure are uncorrelated with the errors in the surname-based measure (Ashenfelter and Krueger 1994; Angrist and Pischke 2008). This assumption is supported by the surname-based measure's independence from census-linking methods. These IV estimates suggest that measurement error reduces our initial estimates of the Free-Enslaved gap by an average of 9% across various outcomes (see Online Appendix A.1). For example, based on our IV estimates, descendants of the Enslaved attained 1.67 (0.15, $p < .01$) years less in education in 1940 than descendants of the Free, compared with 1.59 (0.05, $p < .01$) via OLS.

We also conduct an array of placebo exercises to validate our empirical strategy (see Online Appendix A.3). First, we use 1875 as a placebo year of Emancipation. Specifically, we classify Black families as descending from the Free or the Enslaved based on whether we can link them back to ancestors in 1870 (rather than 1860). This placebo exercise yields no economically significant gaps. For example, a small gap of less than 1% in education emerges (compared with 25% in our baseline). Second, we use white Americans as a placebo group. Specifically, we divide white families into two groups depending on whether we can link them back to ancestors in the 1860 census, similar to our Free-Enslaved classification. Again, this placebo exercise yields no economically significant gaps (at most 1.7% across all outcomes, most of them not statistically significant).

### *V.B. The Free-Enslaved Gap in the Twenty-First Century*

The civil rights movement (1954–1968) ended Jim Crow, thereby instigating institutional change that held the promise to accelerate Black economic progress. Existing evidence suggests that in the decade following the passage of the Civil Rights Act of 1964, Black Americans experienced a temporary surge in economic mobility, especially in the South (Freeman 1981; Donohue and Heckman 1991; Wright 2013; Margo 2016). How has the Free-Enslaved gap evolved since the end of Jim Crow?

We extend our analysis past 1940 using two methods. First, we merge data from a major U.S. credit bureau with our surname-based probabilities of descending from ancestors enslaved until

TABLE II
FREE-ENSLAVED GAP (2023)

| | Total income (US$) Mean: 92,068.48 | Disposable income (US$) Mean: 52,773.74 | Credit score (300–850) Mean: 630.41 | Hourly job Mean: 0.72 |
|---|---|---|---|---|
| Ancestor enslaved until Civil War | −12, 487.72*** (1,147.08) | −11, 623.44*** (920.12) | −33.15*** (2.07) | 0.05*** (0.01) |
| Controls (age group–FE) | Y | Y | Y | Y |
| % of Black-white gap | 23 | 26 | 40 | 69 |
| Adjusted $R^2$ | 0.001 | 0.001 | 0.003 | 0.000 |
| Observations | 547,189 | 547,189 | 547,189 | 459,889 |

*Notes*. This table shows the Free-Enslaved gap in predicted total income, predicted disposable income, credit score, and hourly wage employment among Americans as of March 2023. We estimate a person's likelihood to descend from free Black Americans via their surname, not requiring record linkage. We reweight the sample to hold the distribution of surnames constant at the 1870 level. The sample's average likelihood of a person's ancestor being free before the Civil War based on their surname is 9.6%—very close to the factual fraction. The sample includes both the South and North of the United States. Credit scores (VantageScore® 3.0) reflect a person's credit health, ranging from 300 to 850 (scores above 700 are considered "good" and scores below 550 "very poor"). See Online Appendix B for details on the sample and data. Standard errors are clustered at the family level and are shown in parentheses. *** $p < .01$, ** $p < .05$, * $p < .1$.

the Civil War. This approach lets us estimate the Free-Enslaved gap in real time without needing record linkage. We use a snapshot of this data from March 2023, limiting the main sample to Black Americans as identified by the credit bureau through names and nine-digit ZIP codes. Second, we link 1940 census records for Black Americans to administrative mortality data, covering birth cohorts from 1910 to 1940. These records include a person's last residential nine-digit ZIP code, allowing us to infer neighborhood proxies for their income, wealth, and education circa 2000.

Using U.S. credit bureau data from 2023, we find that descendants of the Enslaved have vastly lower predicted incomes and worse credit health than descendants of free Black Americans (see Table II). The Free-Enslaved gap in predicted disposable incomes is $11,620 (22% of Black Americans' average). The Free-Enslaved gap in credit scores is 33 points (one-fifth of the difference between "good" and "very poor" credit). Descendants of the Enslaved are also more likely to work in hourly wage jobs, presumably leading to higher uncertainty in earnings compared with salaried jobs. These Free-Enslaved gaps amount to 23% to 69% of the corresponding Black-white gaps.

Using neighborhood-level data from mortality records linked to the 1940 census, we find that around 2000, descendants of the Enslaved resided in neighborhoods with lower education, income, and wealth than those of the Free descendants (see Online Appendix Table C.3). Descendants of the Enslaved lived in neighborhoods where Black residents were 3.9 percentage points less likely to hold a high school degree and 2.6 percentage points less likely to hold a college degree. Black residents' expected incomes in those neighborhoods were $5,100 lower (17% of the median). Owner-occupied houses in those neighborhoods were worth $17,500 less (19% of the median).

In sum, our two strategies suggest that the present-day Free-Enslaved gaps in various economic outcomes amount to at least one-fifth of the corresponding Black-white gaps. This finding highlights the enduring impact of historical oppression on present racial disparities. Importantly, the Free-Enslaved gap only quantifies the additional disadvantage faced by those whose ancestors were enslaved until 1865 compared with those who gained freedom earlier. Most Black families, even those who were free before the Civil War, were enslaved in earlier periods, and all Black Americans faced discrimination due to slavery and Jim Crow, regardless of their specific family history. The sheer difference in intensity of their experiences yields economic gaps of such enormous magnitude. Next, we turn to the drivers of this persistence.

### *V.C. Interpreting the Free-Enslaved Gap*

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

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

Intuitively, the Free-Enslaved gap therefore reflects (i) any potential differences in "ability" between the two groups transmitted over generations, (ii) different exposure to locations over time (as a result of slavery and potential selection), and (iii) 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.

## VI. The Importance of Geography in Shaping Black Economic Progress after Slavery

In this section, we use ancestors' enslavement locations as plausibly exogenous variation in where Black families lived to identify what fraction of the Free-Enslaved gap is caused by differential exposure to place-specific factors. We limit our sample to Black Americans whose ancestors were enslaved until the Civil War. We find that state-specific factors are the leading cause of the Free-Enslaved gap's persistence after 1940.

### *VI.A. 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 (excluding free Black Americans and their descendants). Our empirical strategy to identify the importance of exposure to location-specific factors builds on the following assumption, which we discuss in detail in Section VI.C.

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

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

where $s_i$ is a dummy variable equal to one 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 the 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 economic 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 IV,

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

where $\gamma_\ell^0$ and $\gamma_\ell^1$ are the effects that location $\ell$ had on Black families during and after slavery, respectively. Thus, $\eta_\ell$ reflects both the (inherited) effect the state of birth $\ell$ had on the ancestor during slavery and the expected effects of future locations of their descendants given the 1865 location. One can interpret $\eta_\ell$ 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.

1. *The Effect of Being Freed in Each State in 1865.* We find a distinct geography of Black economic progress after slavery (see Online Appendix Figure C.3). Gaining freedom in a state further south negatively affected Black families' economic 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 Kentucky.[15] States affect other outcomes, such as literacy and income, with similarly large magnitudes. States' effects are substantial even in 2000 when, for example, families freed in Louisiana live in neighborhoods with average incomes lower by over one-quarter of the average income among Black Americans compared with those rooted in the Upper South.

2. *Accounting for Migration: The Effect of Living in Each State between 1865 and 1940.* Our estimates of the effect of being freed in each state in 1865 may partly reflect differences in migration opportunities. We formally assess the importance of post-slavery migration and recover the effect of living in each location $\ell$ between 1865 and 1940 on Black economic progress absent migration ($\gamma_\ell^1$). We do so based on Assumption 1 and the additional assumption that place-specific experiences during slavery ceased to affect descendants in 1940 directly ($\rho\gamma_\ell^0 = 0$); we formalize this decomposition in Online Appendix A.4. This problem is a standard case of multiple instruments (location assignment) and imperfect compliance (migration). Specifically, the ITT effect of initial location $\ell$, $\eta_\ell$, is the average of all potential future locations' treatment effects, $\gamma_{\ell'}^1$, weighted by the probability of migrating

15. Being freed in Louisiana has the strongest negative impact on education by 1940 (−0.84 years less than the average across Southern Black Americans)—followed by Georgia and South Carolina (−0.47 years). Missouri has the strongest positive impact (2.28 years), followed by Kentucky (1.66 years).

from $\ell$ to $\ell'$:

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

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 in 1865 did not differ across enslavement locations.

Our results indicate that the effect of being freed in location $\ell$ closely approximates the treatment effect of living in $\ell$ from 1865 to 1940. The recovered treatment effects are almost identical to the ITT effects estimated using equation (6), except for the border states of the Upper South. The effect of living in the border states is more negative than the effect of being freed there, suggesting that the relatively better conditions for Black Americans were partly due to greater migration opportunities. For those freed in the Lower South, benefits from Northern opportunities were more limited due to lower migration rates and a reduced likelihood of the North being their destination conditional on migration.

Early Black migration mostly consisted of movement within the South, often between states offering similarly limited opportunities for economic advancement. North-South migration was rare due to the isolation of the Southern labor market, particularly in the Deep South, which experienced "nearly complete isolation . . . before 1916" (Wright 1986, 108). Within the South, migration flowed mainly from the low-wage Southeast to the high-wage Southwest. Southwestern states such as Mississippi, Louisiana, and Arkansas attracted many Black migrants in the early postslavery era, as they offered the potential for landownership and political participation. However, the intensification of Jim Crow around 1890 ultimately reversed the fortunes of these migrants.

With Black families freed in the Lower South faring so much worse than those freed elsewhere, it may seem puzzling that the region did not experience a larger exodus than the Upper South. For example, 75% of Black families enslaved in Louisiana still lived there in 1940; less than 10% reached the North (see Online Appendix Figures B.5 and B.6). Lower Southern white families were almost 30% more likely to migrate. Institutional and economic factors partly resolve this puzzle.

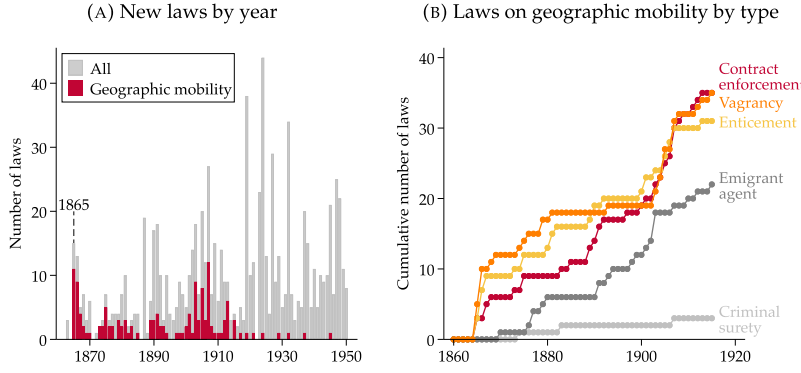


FIGURE IV

Number of Jim Crow Laws across the South

The 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 Online Appendix B 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, and criminal surety laws created the possibility of involuntary servitude on arrests for minor charges (see also Blackmon 2008). These laws began emerging immediately after slavery (see Figure IV).

Second, moving to the North was costly, especially from the Lower South. Among families enslaved until the Civil War, the propensity to migrate North was especially low compared with Black families free earlier—some of whom may have used the resources they had accumulated by the end of the Civil War to leave the South. The region's geographic distance to the North limited the potential of social networks to lower the cost of migration (Carrington, Detragiache, and Vishwanath 1996). Moreover, despite successful migration to the North, many Black families still faced challenges in capitalizing on available opportunities (Collins 1997; Akbar et al. forthcoming; Derenoncourt 2022).

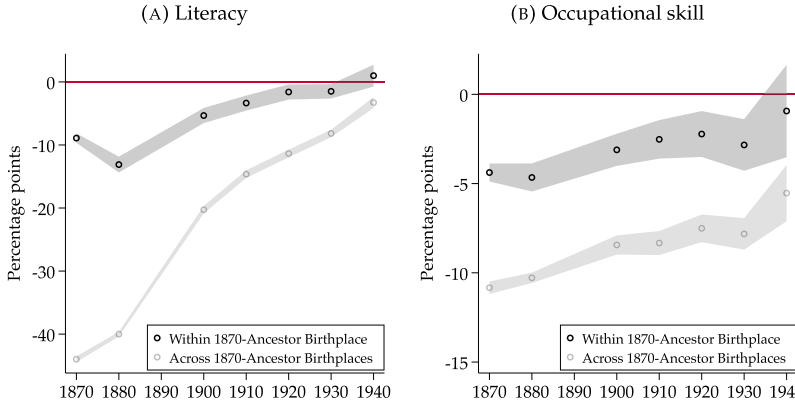


FIGURE V

The Free-Enslaved Gap Conditional on Ancestor's State (1870–1940)

This figure shows the gaps in literacy and occupational skill before (light) and after (dark) including fixed effects for 1870 ancestor state of birth. The sample includes both the South and North of the United States. The comparison is made between prime-age (20–54 years) male descendants of enslaved versus free Black Americans in each census decade. In the 1940 census, instead of literacy, we observe the highest year of school or degree completed. We classify individuals who have completed at least two grades of school as literate; others we classify as illiterate. We assign "skilled" to occupations classified as "medium skilled workers" or above by the HISCLASS scheme (Van Leeuwen and Maas 2011) and "unskilled" to others. Both panels control for age and include 95% confidence bands clustered at the family level. See Online Appendix B for details on the sample and data.

### *VI.B. The Free-Enslaved Gap is Driven by Geography*

To explore the importance of differential exposure to state-specific factors, we compute the Free-Enslaved gap conditional on ancestor location. We add fixed effects for the state of birth $\ell$ 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. It does not account for free Black Americans' potential selection into states before 1865.

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 V).[16] The 1940 gap in literacy, for

16. The 1940 gaps in almost any other outcome also shrink to zero after conditioning on the 1870 state of origin (see Online Appendix Figure C.4 and Online Appendix Table C.4).

example, fully closes after accounting for variation across ancestor states. Similarly, the conditional Free-Enslaved gap in 2000 is insignificant for all outcomes (see Online Appendix Table C.5). These results suggest that the Free-Enslaved gap persists mainly because the two groups were exposed to different state-specific factors after slavery.

We also assess the causal importance of state-specific factors (robust to free Black Americans' potential selection into states before 1865). Two counterfactual analyses (see Online Appendix D) show that (i) had the Enslaved ancestors been distributed as the Free within the South, the Free-Enslaved gap would have been at least 67% smaller (lower bound),[17] and (ii) had the Enslaved ancestors been distributed as the Free within both the South and North, the gap would have closed entirely by 1940 (see Online Appendix Table D.1). Overall, our results show that group differences in initial location were the primary driver of the persistent Free-Enslaved gap.

In addition, we show that it is ancestor states that explain the Free-Enslaved gap, not other levels of ancestor geography (see Online Appendix Figure C.5). 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 the Free-Enslaved gap, suggesting that it is not geographic granularity that makes states an important explanation.

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 advantage of being free earlier—to drive the gap after 1940. State-specific factors compressed the economic status of Black Americans in states irrespective of their ancestors' enslavement status (see Online Appendix Figure C.6). Their exposure to states that slowed Black economic progress after slavery placed descendants of the Enslaved at a disproportionate disadvantage.

Two exercises provide additional evidence in support of this interpretation. First, we consider free Black Americans who had no measured physical or human capital by the end of slavery. We find that even this group of free Black Americans had higher so-

17. We argue that the Enslaved's geographic disadvantage within the South provides a lower bound for the importance of group differences in location, as the Free in the North faced more favorable post-slavery conditions.

cioeconomic status than descendants of the Enslaved by 1940 (see Online Appendix Table C.6). This result further supports the conclusion that the Free-Enslaved gap's persistence is unlikely to be driven by selection into freedom or the inherent advantage of being free earlier. Second, we estimate the Free-Enslaved gap controlling for skin tones. We find that the Free-Enslaved gap is almost identical with or without this control (see Online Appendix Figure A.2). This result suggests that potential differences in discrimination of descendants of the Free and the Enslaved based on their skin tones is not a key driver of the gap's persistence (see also Abramitzky et al. 2023).

### *VI.C. Location of Freedom and the Question of Exogeneity*

Estimating the causal effect of place-specific factors requires that a person's location is orthogonal to their potential outcomes. Our empirical strategy relies on the immobility of the enslaved population. In particular, we build on the circumstance that the Enslaved did not have freedom of movement before 1865, leaving no room for self-selection into location. In contrast, past research typically relied on "mover designs" (e.g., Chetty, Hendren, and Katz 2016). In those studies, places' effects are estimated from the outcomes of families who move between them. Assumptions on the nature of their moves allow for a causal interpretation.

The lack of free movement among enslaved people lends plausibility to the key identifying assumption of an enslaved person's birthplace to be orthogonal to the potential outcomes of their (third-generation) descendants. The main threat to our identification assumption is the possibility of selective forced migration of enslaved people. Even though the Enslaved did not choose where they lived, owners' or traders' decisions may have induced selection into enslavement locations.

Slaveholder migration and the domestic slave trade contributed equally to the forced migration before 1865 (Fogel and Engerman 1974; Tadman 1979; Pritchett 2001; Steckel and Ziebarth 2013). Slaveholders were generally nonselective in moving all their enslaved people with them (Fogel and Engerman 1974; Pritchett 2001; Tadman 2008; Pritchett 2017). 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. The domestic slave trade accounts for the remaining interregional slave mobility. Selective slave trade is only evident in the small sugar cultivation areas.[18] Sugar cultivation accounted for 6% of the rural enslaved population (Tadman 1977, 1979).[19]

If anything, one can hypothesize that the selection into location based on physical traits has biased upward the estimates of states that supposedly selected positively on height and strength. In contrast, we find that such states—those in the Lower South in general and those in the sugar region of Louisiana in particular—were especially detrimental to Black economic progress.

The results from the following section strongly support our key identifying assumption. Because our estimated place effects vary sharply across state borders (and less within states), 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 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.

18. In contrast to the sugar industry, the cotton and tobacco industries (accounting for around 87% of enslaved agricultural workers) were generally nonselective on age and sex (Tadman 1977).

19. By the nature of the work required, enslaved people there tended to be physically stronger and more likely to be male (Phillips 1918). Traded enslaved people were found to be disproportionately likely to be young adults (e.g., Pritchett 2017) and more likely to be male (Fogel and Engerman 1974), but some of this evidence is nuanced by Tadman (1977, 1979). Pritchett (2001) finds that traded enslaved people were marginally taller than the average enslaved population, conditional on age and sex, but Steckel and Ziebarth (2016) contest this finding. Physical characteristics were also co-determined by environmental influences such as nutrition, illness, or stress (Steckel 1979; Carson 2008). There is no evidence that traders selected enslaved people on anything other than such basic physical characteristics. This is consistent with the dehumanization of Black people that characterized the slave trade, which "reduced people to the sum of their biological parts" (Smallwood 2008, 43).

## VII. The Jim Crow Effect

Our analysis so far attributes the Free-Enslaved gap's persistence primarily to the two groups' differential exposure to place-specific factors. This section assesses whether state institutions, particularly Jim Crow regimes, underlie the importance of those place-specific factors. We find evidence that implicates state institutions as the main drivers: (i) places' effects on Black economic progress differ sharply across state borders and (ii) observed non-institutional factors do not differ across state borders. Furthermore, our evidence suggests that Jim Crow regimes are key state institutions responsible: (i) the negative impact of state institutions was race-specific, largely leaving the economic status of white families unaffected; (ii) the effect of state institutions can be statistically explained by various measures of states' Jim Crow intensity; and (iii) the impact of state institutions emerged with the onset of the Jim Crow era.

### *VII.A. State Institutions and Black Economic Progress after Slavery*

Places may affect families' economic status 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 $\ell$, and $s(\ell)$ as the state that location $\ell$ is in.[20] In the next section, we propose a border discontinuity design to separate the effect of institutions, $\gamma_{s(\ell)}^t$, from the effect of noninstitutional factors, $\gamma_{\boldsymbol{\epsilon}(\ell)}^t$.

20. 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 $\ell$ is on, not on the precise coordinates $\boldsymbol{\epsilon}(\ell)$: $\gamma_{s(\ell)}^t = \gamma_s^t$.

### *VII.B. Border Discontinuity Design*

Our border discontinuity design compares the economic 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. 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 the South (see Online Appendix Figure A.3), where $y_{i,b}^{1940}$ is the economic status 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. The main coefficient of interest, $\beta_b$, captures the long-run effect of being freed on the more oppressive side of border $b$ on a Black family's economic status.

To assess the extent to which institutions shaped the geography of Black economic progress, we compare the sharp differences in progress that emerge at state borders with the overall differences between states' effects (see Figure VI). We find large border discontinuities, indicating that Black families freed in close proximity to each other but on opposite sides of state borders experienced vastly different economic trajectories. These border discontinuities account for a significant portion of states' overall long-run effects ($R^2 = 0.77$), suggesting that institutional factors, rather than factors that vary continuously across borders, are the primary drivers shaping the geography of Black economic progress. While institutional factors play a predominant role, there is residual variation that may be attributable to differences in economic activity, culture, or climate.

Having established the importance of state institutions, we examine whether it was Jim Crow institutions specifically that slowed Black economic progress. To do so, we correlate our border discontinuity estimates $\widehat{\beta}_b$ with differences in Jim Crow intensity, using that Jim Crow regimes differ more drastically across some borders than others. To quantify Jim Crow severity—which encompasses both de jure and de facto tactics (Woodward 1955;

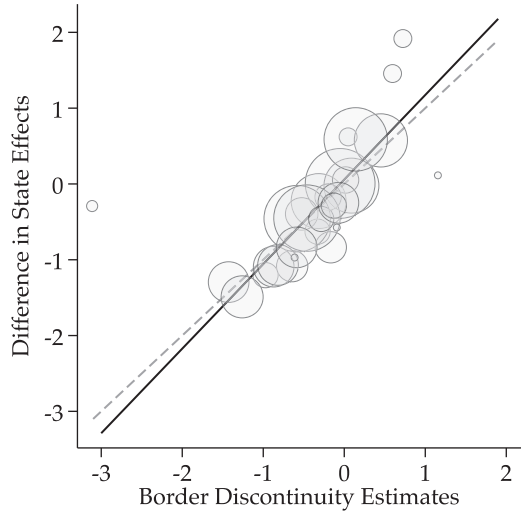


FIGURE VI

Differences in Black Economic Progress Arise Sharply at State Borders

This figure relates each regression discontinuity estimate (as shown in Figure VII) to the difference in the two states' overall causal effect on 1940 years of education (as shown in Online Appendix Figure C.3, Panel A). Estimates are weighted by the minimum sample size underlying the difference in state effects. A dashed 45-degree line shows the benchmark of equal differences across two states and across the border counties of two states. The solid line shows the best weighted linear fit ($\widehat{\beta} = 1.12, p < .01, R^2 = 0.77$). Findings are robust to excluding Louisiana and Virginia (results available on request). See Online Appendix B for details on the sample and data.

Acemoglu and Robinson 2008)—we employ a range of proxies that despite their differing natures, are highly correlated. For example, the HRR index and the Jim Crow index have a correlation of $\rho = 0.99$; the HRR index and Black school quality have a correlation of $\rho = -0.94$ (see Online Appendix Figure B.4). Across these measures, we consistently arrive at the same key finding.

We find that states' intensity of Jim Crow regimes predicts border discontinuities in Black economic progress. Specifically, families freed in states with more severe regimes experienced significantly lower rates of progress, starting from the Jim Crow era (see Figure VII, Panel A). These gaps widen as the difference in Jim Crow severity increases across a border. For example, consistent with Louisiana's more severe Jim Crow regime compared to Texas's, families freed in Louisiana attained 1.2 fewer years

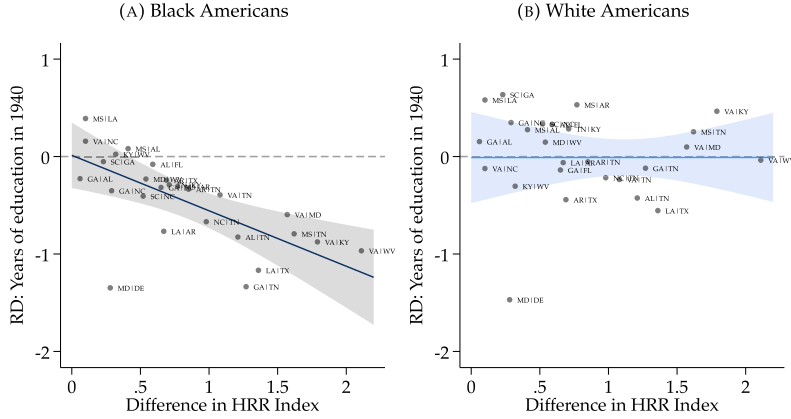


FIGURE VII

Regression Discontinuity Estimates and Jim Crow

Panel A shows each separate regression discontinuity estimate in 1940 years of education for Black families whose ancestors were freed on different sides of state borders in 1865. Panel B shows the same for white families depending on where their ancestors lived in 1870. Each label shows the more oppressive before the less oppressive state. Jim Crow intensity is measured via the Historical Racial Regime (HRR) index (Baker 2022). Negative estimates reflect lower education in more oppressive states. Lines show the best linear fit between regression discontinuity estimates and the differences in Jim Crow intensity, weighted by the inverse of the estimates' standard error. Shaded areas represent robust 95% confidence bands. For point estimates, we use a 350 km bandwidth and empirical Bayesian shrinkage as described in Online Appendix A.5. See Online Appendix B for details on the sample and data.

of education by 1940 than those freed just miles away in Texas. Similarly, residing in states with more severe Jim Crow regimes led to a greater likelihood of working as a farmer in 1940 but did not significantly affect wage incomes (see Online Appendix Figure C.7). No differences emerge for families freed across borders where states have comparable institutions. Incorporating extensive controls for 1860 local demographics, characteristics of slaves, crop suitability, and economic activity further strengthens these findings (see Online Appendix Figure A.4).

We also find that, as expected, families who left their enslavement state before the Jim Crow era were unaffected by their origin state's Jim Crow regime (see Online Appendix Figure C.8). 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 severe Jim Crow regimes who stayed there until 1920 were still strongly affected by their pre-1920 experiences in 1940. The longer a family was exposed, the larger the effect on their economic status.

In principle, Jim Crow could also have affected white Americans, not just Black Americans. First, some Jim Crow laws may have directly harmed poor white Americans. For example, poll taxes aimed at disenfranchising Black voters also 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 landowning families' profits.

We find that in contrast to Black families, the economic status of white families was not negatively affected by the Jim Crow intensity of the state in which their ancestors lived in 1870 (see Figure VII, Panel B). The same is true even for poor white Americans whose ancestors had no measurable human or physical capital in 1870 (see Online Appendix Figure C.9, Panel A). 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).

We do, however, find positive effects for the white landowning elite. We find that the more oppressive a Jim Crow regime, the more economically significant the gains by the border region's wealthiest 10% of white families (see Online Appendix Figure C.9, Panel B). 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 most economic harm.

The end of slavery led to a dramatic change in the geography of racially oppressive institutions in the United States. State governments took the leading role in instituting Jim Crow regimes to limit the economic progress of newly freed enslaved families. Our results show that state institutions became a crucial determinant of 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 provide further evidence that our border discontinuity design isolates the Jim Crow effect without being confounded by other factors.

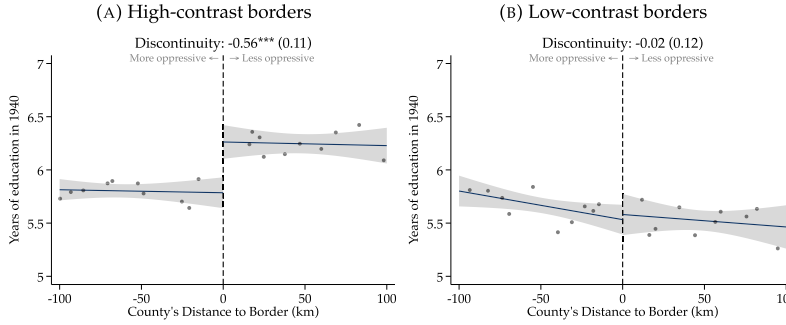


FIGURE VIII

Pooled Regression Discontinuity Estimates

This figure shows the regression discontinuity estimates in 1940 years of education for Black families freed across state borders with different Jim Crow intensity in 1865. Jim Crow intensity is measured via the Historical Racial Regime (HRR) index (Baker 2022). Panel A shows "high-contrast borders" where Jim Crow intensity differs more than across the median border (above 0.71 HRR index points, with differences averaging 1.30 HRR index points); Panel B shows "low-contrast borders" where it differs less than the median (below 0.71 HRR index points, with differences averaging 0.32 HRR index points). 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% confidence bands clustered at the 1870 county level. See Online Appendix B for details on the sample and data.

### *VII.C. Validation of the Border Discontinuity Design*

To validate our border discontinuity design, we pool all borders, rather than estimating discontinuities for each border separately. The pooled regression equation closely follows equation (9). We equally divide our sample into two types of borders: "high-contrast borders" between states that strongly differ in their Jim Crow intensity (more than the median border difference in the HRR index), and "low-contrast borders" between states that differ less in their Jim Crow intensity (less than the median border difference).

Consistent with our main estimates, sharp educational differences only arise for Black families freed across borders where institutions differ substantially (see Figure VIII).[21] Being freed on the more oppressive side of such a high-contrast border sharply

21. Online Appendix Figure C.10 shows the pooled regression discontinuity estimate for all borders—both high- and low-contrast.

reduced the years of education in 1940 by 0.6 years—10% of the average among Black men.

We confirm that differences across high-contrast borders only arise after the onset of Jim Crow (see Figure IX). Before Jim Crow, there were no differences in literacy among families freed in states that would become more oppressive during Jim Crow.[22] In 1880, the literacy rates of families began to differ. By 1900, Black families attained almost five percentage points lower literacy rates in more oppressive states. These differences grow over time in absolute terms but even more so in relative terms. By 1930, while almost 90% of all Southern Black men were literate, families freed in more oppressive states were still 4.6 percentage points less likely to be able to read and write.

We also confirm that before Jim Crow, location characteristics evolved smoothly across state borders. In 1860, none of a large array of observable characteristics differed discontinuously across state borders in the South: the number of enslaved people relative to a county's overall population, the share of its Black population, the share of plantation crops (cotton, sugar, tobacco, and rice) of total agricultural output, total agricultural output per capita, cotton output per capita, farm values, white wealth inequality, migration costs to the North, population density, incomes, or the age of enslaved people (see Online Appendix Figure C.12). Our validation exercises focus on high-contrast borders where differences in Black economic progress emerged, but the results generalize to low-contrast borders.

We further present evidence that Jim Crow institutions varied sharply across state borders. We find significant gaps in key outcomes directly targeted by Jim Crow across state borders with differing Jim Crow intensities (see Online Appendix Figures C.13, C.14, and B.7). Specifically, counties in states with more severe Jim Crow regimes have sharply lower voter participation, Black school attendance, Black teacher education, and Black teacher wages, plausibly reflecting the direct impact of suffrage restrictions and reduced school funding instituted in those states. Importantly, neither voter participation nor Black school attendance differ sharply across borders before the Jim Crow era (the other outcomes are not observed pre–Jim Crow). We also find that the number of lynchings between 1883 and 1941 does not vary

22. Online Appendix Figure C.11 shows regression discontinuity estimates in literacy rates over time, separately by border.

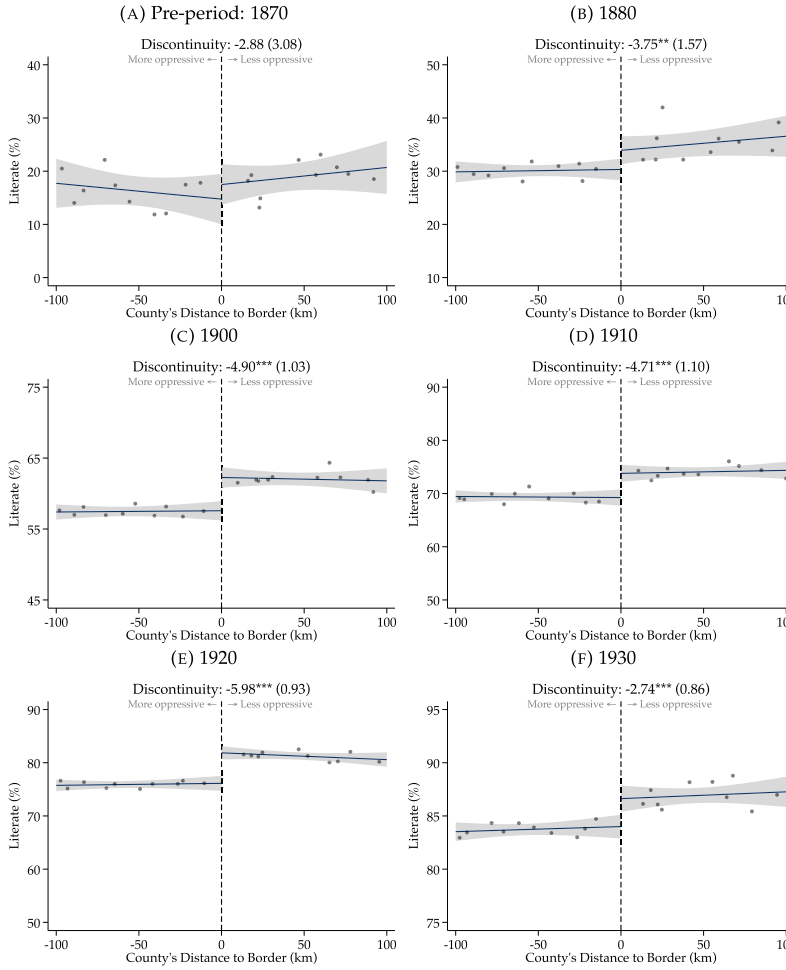


FIGURE IX

Regression Discontinuities in Literacy (High-Contrast Borders)

This figure shows the regression discontinuity estimate in literacy for Black families freed across state borders with different Jim Crow intensity in 1865. Jim Crow intensity is measured via the Historical Racial Regime (HRR) index (Baker 2022). The sample is restricted to high-contrast borders (above 0.71 HRR index points, with differences averaging 1.30 HRR index points). 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% confidence bands clustered at the 1870 county level. See Online Appendix B for details on the sample and data.

sharply across borders, supporting the assumption that border differences in economic progress capture the effect of state institutions (see Online Appendix Figure C.15).

Our results are also robust to using alternative measures for the intensity of states' Jim Crow regimes. We consider both the Jim Crow index and a state's number of Jim Crow laws (see Online Appendix Figure A.5).

Last, we show that our results are robust to different cutoffs for the distance between a county's centroid and a state border between 100 and 350 km (see Online Appendix Figure A.6). The pooled regression discontinuity estimates across high-contrast borders (as shown in Figure VIII, Panel A) for those cutoffs all range between −0.61 and −0.46 and are all highly significant. Our baseline bandwidth is 100 km in pooled estimations—close to the mean squared error optimum—and 350 km when separately estimating discontinuities by state pair to reduce the impact of smaller sample sizes.

The results from our regression discontinuity design also strongly support our key identifying assumption—that the birthplace of an enslaved person is orthogonal to their innate "ability." Specifically, we find that the differences in the causal effects of states sharply and fully arise at state borders. Therefore, the main potential threat of selection bias remains the selection of enslaved people into states sharply around borders. However, any plausible selection into the destination of forced migration was based on the crop cultivated in an area that, as we confirm, transcends state borders (along with many other characteristics of border areas). Therefore, it is implausible that the selection of enslaved people into locations affects our results. In addition, we directly rule out selection based on observable characteristics, showing that the characteristics of enslaved people, such as their age during or their literacy immediately after slavery, do not differ across borders.

In sum, our evidence suggests that states' Jim Crow regimes played a critical role in shaping the South's detrimental effect on Black economic progress. The estimates are a lower bound for Jim Crow's importance because all Southern states adopted Jim Crow regimes. Our estimates only isolate the additional effect of more oppressive institutions rather than their aggregate effects.

## VIII. The Mechanism of Limited Access to Education

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 1901, 22). 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, 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, 25)

We use our newly built database on laws and their content to explore the relative importance of different domains that Jim Crow regimes affected. We document that the most significant number of laws pertained to education, accounting for one-third of all Jim Crow laws passed across the South until 1950 (see Online Appendix Figure B.8).[23]

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, we find a robust negative correlation between a state's number of education-specific Jim Crow laws and the quality of Black schools ($\rho = -0.70$).

23. 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 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 further analysis can one conclude that they were a crucial part of states' Jim Crow regimes.

Our analysis of Black teacher wages confirms that disparities in school quality are pronounced right at states' borders, underscoring the critical role of institutional factors in shaping the quality of Black schools (see Online Appendix Figure B.7 and Margo 1982, 1990; Naidu 2012; Card, Domnisoru, and Taylor 2022). We also explore the importance of education-specific Jim Crow regimes for Black economic progress by repeating our regression discontinuity design based on the number of education-specific Jim Crow laws and the quality of Black schools (Card and Krueger 1992; Carruthers and Wanamaker 2017). Both measures capture the sharp differences in Black economic progress across Jim Crow regimes (see Online Appendix Figure C.16). These findings are consistent with Card and Krueger (1996) and Card, Domnisoru, and Taylor (2022, 42) who show that state institutions induced critical differences in school quality and educational outcomes among Black children, "helping to explain the persistence of the human capital gap between Blacks and whites."

## IX. Conclusion

This article provides new evidence on the long-run effects of racially oppressive institutions, finding that Black Americans' economic status today depends strongly on their ancestors' exposure to those institutions. First, we document that Black families enslaved until the Civil War continue to have considerably lower education, income, and wealth today. Second, we show that this persistence is mostly driven by post-slavery oppression under Jim Crow. We discuss Black Americans' limited access to education as a critical mechanism.

We 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 the state where a family was freed determined the Jim Crow regime they likely faced over the subsequent decades. While Jim Crow compressed the economic status of Black Americans within states, differences in Jim Crow intensity led to pronounced disparities across states, thereby placing descendants of those enslaved until the Civil War at a disproportionate disadvantage. After 1940, the main reason descendants of families enslaved until the Civil War have lower economic 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 (Cain 1986; Loury 2002; Darity 2005, 2022; Darity et al. 2017)—is thus a central aspect of slavery's persisting legacy.

Despite the end of Jim Crow, today's geography of Black economic progress has similarities with that of the past. States that impeded Black economic progress post-slavery also limit intergenerational mobility for low-income children today (see Online Appendix Figure C.17 and Berger 2018). However, different from the Jim Crow era, those differences do not arise sharply across state borders. Future research should investigate why places' capacity to generate upward mobility has persisted despite drastic institutional change. Part of the answer may lie in anti-Black resentment, which remains high in places with historical prevalence of slavery and Jim Crow (Acharya, Blackwell, and Sen 2018).

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. For example, while only 13% of 18- to 19-year-old Black Americans have an immigration background, 41% of Black Ivy League students do. Affirmative action increases racial diversity on campuses but may be less effective in alleviating disadvantages faced by descendants of the Enslaved.

Second, there has been renewed interest in the specific policy of reparations, that is, wealth transfers to descendants of the Enslaved (e.g., Darity 2008; Craemer et al. 2020; Albuquerque and Ifergane 2023; Boerma and Karabarbounis 2023). We argue that any assessment of the legacy of slavery should incorporate both when and where a family was freed—that is, 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 affected drastically by when and where their ancestors were freed. 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. 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 with 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 regardless of their specific family history.

This article 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 surnames upon marriage, making it impossible for conventional methods to link them across census records (Althoff, Brookes Gray, and Reichardt 2024). Second, we emphasize the significance of educational Jim Crow institutions as a crucial mechanism; however, institutions related to other aspects may have further impeded Black economic advancement. Although several of these institutions have been thoroughly investigated (e.g., restrictions on Black suffrage—see Naidu 2012), numerous others remain relatively unexplored (e.g., constraints on interracial marriage). Third, while this study quantifies the impact of Jim Crow, future work should explore the political economy underlying the rise of states' different institutional regimes.

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## SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

## DATA AVAILABILITY

The data underlying this article are available in the Harvard Dataverse, https://doi.org/10.7910/DVN/CH0BFC (Althoff and Reichardt 2024).

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