

JIM CROW AND BLACK ECONOMIC PROGRESS AFTER SLAVERY*

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Abstract

This paper 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

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1. 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 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 to Black Americans’ economic and geographic mobility.¹ The Jim Crow era persisted for almost 100 years and only ended with the passage of the Civil Rights legislation in the 1960s, which outlawed racial discrimination.

This paper 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 impact 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 per se in contributing to these disparities dissipated over the early 20th 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 US 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 to the 21st 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 1) automated record-linkage ([Abramitzky et al., 2021](#)) and 2) a new surname-based approach ([Ager et al., 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 individ-

¹Throughout this paper, 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.

²To 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 Appendix B.5).

uals 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.

Our third result is that Jim Crow institutions underlie the severely limiting effects of certain states on Black economic progress. To isolate the impact 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 to 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 non-institutional 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,

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

we show that 1) gaps in the economic status of formerly enslaved people only arise with the beginning of Jim Crow (circa 1880), 2) those gaps only exist for borders where states' Jim Crow regimes differ and increase with those differences, 3) before Jim Crow there are no border gaps in counties' economic, agricultural, political, or demographic characteristics, 4) 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 5) 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 dataset on state-level Jim Crow laws. We first 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 impact on Black economic progress.

This paper makes several contributions. First, leveraging new methods to link families' data across generations ([Abramitzky et al., 2020](#)), we generate new evidence on the mechanisms behind institutions' persistent effects ([Acemoglu et al., 2002](#); [Dell, 2010](#); [Donaldson, 2018](#); [Dell and Olken, 2019](#)). 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 Jr., 2005](#); [Darity Jr. et al., 2017](#); [Darity Jr., 2022](#))—is central to the enduring legacy of racial oppression in the US. We find that Black economic progress was rapid where conditions allowed, consistent with sem-

inal 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 (Olivetti and Paserman, 2015; Chetty et al., 2014; Chetty and Hendren, 2018). Our results show that institutions can play a key role in shaping upward mobility.

2. HISTORICAL CONTEXT

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

2.1 Free Black Americans before 1865

In 1860, just before the Civil War (1861–1865) that led to the abolition of slavery, 4 million enslaved and 0.4 million free Black people lived in America. Enslaved people had existed on American soil since the country’s colonial origins (Sowell, 1978). The roots of the free Black population may trace back to 1619 when settlers in Virginia purchased the first 20 Black people. Little is known about their fate, but it is likely that some of them were treated as servants who had to work for a fixed term and gained freedom afterward (Frazier, 1949). Around 1660, both law and practice had changed, implying that virtually all Black individuals who arrived in the colonies were enslaved for life (Galenson, 1981). From 1662 onwards, the law also mandated that a child would inherit their legal (i.e., free or enslaved) status from their mother regardless of race.

For some enslaved people, the Revolutionary War (1775–1783) provided a road to freedom. Responding to a need for troops and laborers, 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 US as free persons. As a result, the free Black population in some states increased dramatically.

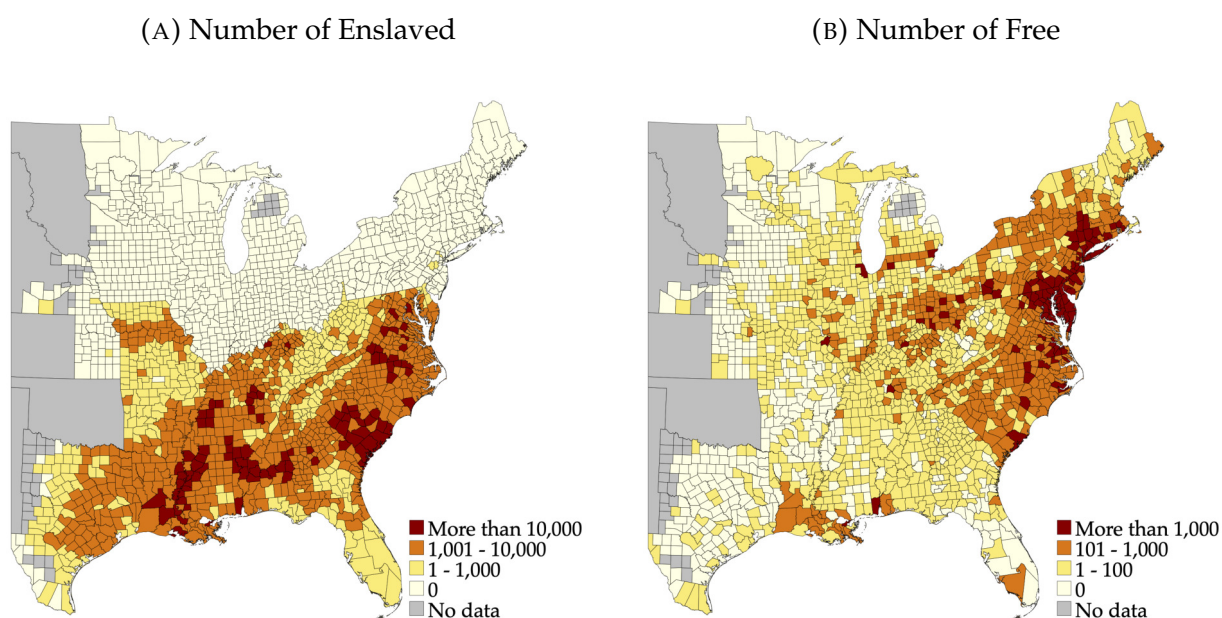
The Revolutionary War also spread a spirit of egalitarianism, challenging the institution of slavery in some regions. In the North, the abolitionist movement grew quickly after the war. While only a few Black people lived free of slavery before the Revolutionary War, most Northern states adopted gradual 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 en-

⁴The 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.

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

FIGURE 1: Population by County in 1860



Notes: This figure shows the population sizes of enslaved Black Americans (panel A) and free Black Americans (panel B) in the 1860 census. The maps are truncated to omit the western half of the country, which at the time was only sparsely populated. Appendix Figure B.9 shows the maps for 1790.

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 to hold political office. However, their legally protected property rights were respected in most cases. With the limited freedom they enjoyed, some free Black families could accumulate modest wealth and social status. Most of them, however, lived in poverty "under conditions barely distinguishable from those of the mass of slaves" (Berlin, 1974). Their economic status varied considerably across the country and, perhaps surprisingly, tended to be better further South (Berlin, 1976). In the North, free Black families were concentrated in cities where they suffered from competi-

tion 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 1). The free Black population, in contrast, was concentrated in the North and the Upper South. These differences in geographic location exposed them to different institutional regimes after slavery.

2.2 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 et al., 2024). 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 extra-legal 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 et al., 2012). Enticement laws, contract enforcement laws, and emigrant-agent laws prevented Black workers from seeking economic opportunities with new employers or in states outside the South (Roback, 1984; Naidu, 2010). Vagrancy laws criminalized the unemployment of Black people (Blackmon, 2008). In addition to legal factors, various extra-legal 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; Naidu, 2010; Carrington et al., 1996).

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, six 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., 2022; 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; Darity Jr. et al., 2016). The narrowing of racial disparities has slowed substantially since the 1960s (Bayer and Charles, 2018; Althoff, 2021; Derenoncourt et al., 2024).

3. DATA AND NEW METHODS TO MEASURE A FAMILY’S EXPOSURE TO SLAVERY AND JIM CROW

A major empirical challenge we overcome in this paper is to measure a Black family’s exposure to slavery and Jim Crow. We construct family histories for Black Americans in the 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.

3.1 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.

Main method based on census linking. We identify Black Americans free before 1865 (“the Free”) as those who were 1) recorded in the 1850 or 1860 census or 2) born in a state that had already abolished slavery; Black Americans who were born in slave states before 1865 and cannot be traced back to ancestors in the 1850 or 1860 census are classified as enslaved until 1865 (“the Enslaved”).⁵ We then 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

⁵We 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.

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 one census. Hence, in the South, we inevitably misclassify some Black families who were free before 1865. However, census records show that only 6 percent of the Southern Black population were free in 1860. Therefore, our comparison involves a group almost certainly free in 1860 against a group where at least 94 percent were enslaved until the Civil War, minimizing the potential for attenuation bias due to imperfect linking rates (see also Appendix A.1). Record linkage helped us identify around 20 percent of free Black Americans in the 1870 census, 10 percent 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 generation in the census cross-section. Our panel allows us to follow individual Black families' records until 2000. Second and most importantly, relying on a person's birthplace can only identify free Black families born in the North. However, 50 percent 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 vs. 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 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

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

⁷In 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 vs. free Black ancestors could be 15 percent larger than the Free-Enslaved gap.

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

While some surnames were common among the Free and the Enslaved, others were characteristic of one group (see 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 ten 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 dataset that includes surnames, such as the full (not only 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 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 Appendix B.5).

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

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

To measure a family’s likely exposure to Jim Crow, we use that record linkage allows us 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. Appendix Figure B.3 shows that prior to 1930, the share of Black families originating from the Lower South who migrated out of the region was less than 10 percent—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

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

to 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 et al., 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: 1) the anti-Black discriminatory share of a state’s laws specific to race; 2) a state’s number of disfranchisement devices; 3) the share of congressional delegates who signed the Southern Manifesto; 4) the Black-white disparity in schools’ term lengths; and 5) the year minimum pay for teachers was introduced—legislation central to narrowing the large wage penalty historically suffered by Black teachers (Card et al., 2022; Cascio and Lewis, 2024). This Jim Crow index is highly correlated with the HRR index ($\rho = 0.99$).

Additionally, 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 also incorporated additional laws on employment and suffrage not covered in the primary source (Roback, 1984; Cohen, 1991; Walton et al., 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 employed both legal methods (e.g., literacy tests) and extra-legal methods (e.g., voter intimidation) to marginalize Black Americans. As Woodward noted, “[t]here [was] more Jim Crowism practiced in the South than there [were] Jim Crow laws on the books” (p. 102 Woodward, 1955). While no single measure can fully capture this complexity, all of our different proxies are highly correlated (see 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.

3.3 Linked Data

We use full-count census data for all available decades between 1850 and 1940 ([Ruggles et al., 2020](#)) and link observations across adjacent and non-adjacent decades using the automated linking methodology provided by [Abramitzky et al. \(2020\)](#). A person is linked from one census to another if their name, year of birth, and state of birth match and if the match is *unique* conditional on race. We use a method that allows for misspellings by matching names based on their phonetic sound (NYSIIS). 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 et al., 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 (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 21st 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 Appendix B.2 for details).

To extend our results to the present day, we combine our surname-based Free-Enslaved classification with real-time data from one of the primary US 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 to-

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

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

3.4 Sample

For our analysis, we focus on Black men aged 20 to 54 and limit our 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 US 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 non-adjacent 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 a 1870–1940 family tree. We link 10 percent of families in 1870 to at least one ancestor in 1940 (see 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 to formerly enslaved men (18.5 vs. 17.1 percent, respectively, from 1870 to 1880).¹¹ From the 1940 census to admin-

¹⁰Using a separate dataset—our Social Security mortality records—we find that surnames and nine-digit ZIP codes combined capture 90 percent of the variation in whether a person is Black or not.

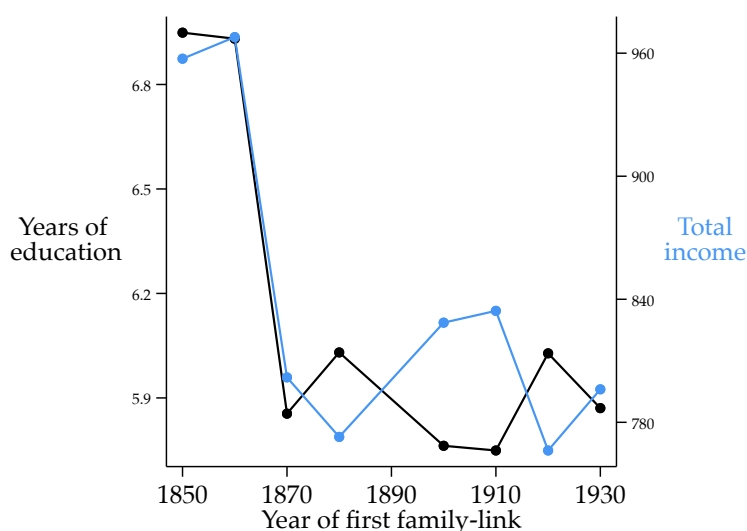
¹¹To evaluate linking rates by Free-Enslaved status, we contrast Black Americans born in the North (Free) with those from the South (mostly Enslaved), rather than basing the Free-Enslaved status on linkability in earlier decades. The relatively lower linking rates for Southern-born Black Americans may stem partly from their larger population sizes, which decrease the likelihood of having unique names within their

istrative 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 Appendix Table A.1). For example, the literacy rate (20.4 percent) 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 percent) is also close to that of the 1860 Black census population (66.8 percent)—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 US-born parents in 1940.

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.

FIGURE 2: Average Outcomes in 1940



Notes: This figure shows the average outcomes of Black Americans in 1940 by the earliest year to which we can link them back to one of their ancestors. The dark 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 Data Appendix B for details on the sample and data.

To examine the quantitative importance of this concern, we group Black Americans in 1940 by the earliest decade in which we can link them back to one of their ancestors and plot their average outcomes by group (see Figure 2). In 1870, Black families enslaved until 1865 were included in the census for the first time. Consistent with that change in sample composition, we observe a significant drop in average income and education birth states.

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 throughout this paper.

4. 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 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?

4.1 Model setup

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

$$y_{i,t} = \alpha_{i,t} + \gamma_{\ell(i,t)}^t + \rho y_{i,t-1} + \varepsilon_{i,t} \quad (1)$$

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

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

where s_i is an indicator for whether the family was enslaved until 1865. That is, in 1865, the outcomes depend on “ability,” location, and whether a person had been free before the Civil War. The parameter $\delta \geq 0$ captures any direct advantage that free Black Ameri-

cans had relative to the Enslaved, such as access to education during slavery.¹²

4.2 The Intergenerational Effect of Being Enslaved Until the Civil War

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

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

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

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

5. 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 until today.

¹²At time $t = 1$, the outcomes then become

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

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” (λ) and through intergenerational advantage derived from “ability” in previous generations (ρ). The current location ($\gamma_{\ell(i,1)}^1$) shifts the level of a person’s human capital. Through intergenerational transmission, human capital is also affected by 1) how previous generations were impacted by where they lived ($\gamma_{\ell(i,0)}^0$), 2) whether their ancestors were enslaved until 1865 (δ), and 3) their ancestors’ idiosyncratic human capital shocks ($\varepsilon_{i,0}$).

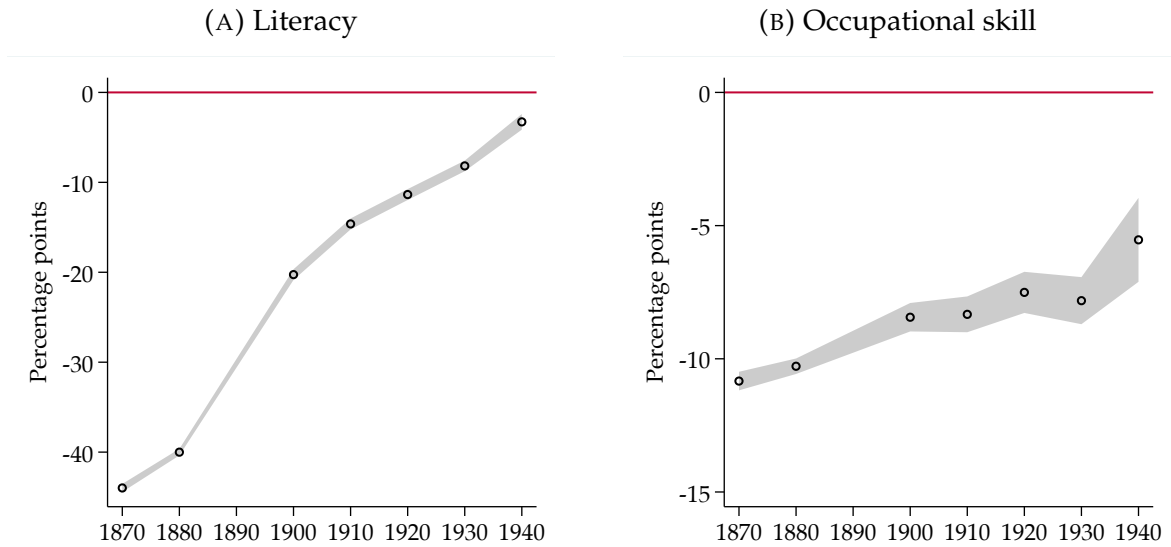
5.1 Evolution of the Free-Enslaved Gap until 1940

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

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

where s_i is equal to one if person i is classified as a descendant of the Enslaved and is zero otherwise. $X_{i,t}$ is a vector of controls that includes a quadratic term of age in our baseline specification. We cluster standard errors at the family level.¹³

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



Notes: This figure shows the gaps in literacy and occupation skill among prime-age (20–54) male descendants of enslaved vs. free Black Americans in each census decade. The sample includes both the South and North of the US. 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 percent confidence bands clustered at the family level. See Data Appendix B for details on the sample and data.

We find that the economic differences between descendants of the Free and Enslaved are large and persistent. In 1870, the formerly Enslaved were 2 times (over 40 percentage points) more likely to be illiterate than free Black Americans (see Figure 3). 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

¹³We 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.

less likely to own their homes (see Appendix Figure C.1). Overall, we estimate the Free-Enslaved gap to be *smaller* than the gap between Black Americans born in the North vs. South before 1865—a comparison that [Sacerdote \(2005\)](#) uses as a proxy for the Free-Enslaved gap (see 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 1).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 to descendants of the Free (see Appendix Table C.2).

TABLE 1: Free-Enslaved Gap (1940)

	Education (Years) Mean: 5.99	Wage Income (USD) Mean: 381.20	Homeownership (%) Mean: 29.25	House Value (USD) Mean: 1,371.95
Ancestor Enslaved 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
<i>Ancestor Free</i>	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 vs. free Black Americans in 1940. The sample includes both the South and North of the US. 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 Data Appendix B for details on the sample and data. Standard errors are clustered at the family level and are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The narrowing of the Free-Enslaved gap from 1870 to 1940 is slow relative to benchmark rates of intergenerational mobility among white Americans. To 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 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

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

gap in 1870 but closed by 1900—while the Free-Enslaved gap changed very little until then.

Robustness. We re-estimate 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 Appendix Table A.2). The gaps between Black families with surnames that convey high vs. low likelihoods of having been enslaved until the Civil War are -1.40^{***} (0.09) in years of education, -113.15^{***} (25.50) in wage income, -2.31^{**} (1.05) in homeownership, and $-1,098.68^{***}$ (282.83) 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 percent across various outcomes (see Appendix A.1). For example, based on our IV estimates, descendants of the Enslaved attained 1.67^{***} (0.15) years less in education in 1940 than descendants of the Free, compared to 1.59^{***} (0.05) via OLS.

We also conduct an array of placebo exercises to validate our empirical strategy (see 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 or not 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 percent in education emerges (compared to 25 percent in our baseline). Second, we use white Americans as a placebo group. Specifically, we divide white families into two groups depending on whether or not 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 percent across all outcomes, most of them not statistically significant).

5.2 The Free-Enslaved Gap in the 21st 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 US credit bureau with our surname-based probabilities of descending from ancestors enslaved until 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.

TABLE 2: Free-Enslaved Gap (2023)

	Total income (USD) Mean: 92,068.48	Disposable income (USD) Mean: 52,773.74	Credit Score (from 300 to 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 re-weight the sample to hold the distribution of surnames constant at the 1870 level. The sample’s average likelihood of a person’s ancestor to be free before the Civil War based on their surname is 9.6 percent—very close to the factual fraction. The sample includes both the South and North of the US. 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 Data Appendix B for details on the sample and data. Standard errors are clustered at the family level and are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Using US 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 2). The Free-Enslaved gap in predicted disposable incomes is \$11,620 (22 percent 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 to salaried jobs. These Free-Enslaved gaps amount to 23 to 69 percent 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 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 percent of the median). Owner-occupied houses in those neighborhoods were worth \$17,500 less (19 percent 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 to 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.

5.3 Interpreting the Free-Enslaved Gap

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

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

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

6. 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.

6.1 States’ Effect on Black Economic Progress After Slavery

We estimate each state’s causal effect on the long-run economic progress of Black families freed there in 1865 (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 6.3.

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 1 if one’s ancestor was enslaved up to 1865, $\ell(i,0)$ is the birthplace of one’s enslaved ancestor, and $\alpha_{i,0}$ is the innate “ability” of one’s enslaved ancestor.

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

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

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 4,

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

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

The effect of being freed in each state in 1865. We find a distinct geography of Black economic progress after slavery (see 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 liter-

¹⁵Being freed in Louisiana has the strongest negative impact on education by 1940 (−0.84 years less than

acy 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 to those rooted in the Upper South.

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 ℓ between 1865 and 1940 on Black economic progress absent migration (γ_ℓ^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 Appendix A.4. This problem is a standard case of multiple instruments (location assignment) and imperfect compliance (migration). Specifically, the intent-to-treat effect of initial location ℓ , η_ℓ , is the average of all potential future locations' treatment effects, $\gamma_{\ell'}^1$, weighted by the probability of migrating from ℓ to ℓ' :

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

We invert the migration probability matrix to recover the effect of living in each state until 1940, which is unaffected by selective migration under the assumption that the average innate “ability” of Black Americans in 1865 did not differ across enslavement locations.

Our results indicate that the effect of being freed in location ℓ closely approximates the treatment effect of living in ℓ from 1865 to 1940. The recovered treatment effects are almost identical to the intent-to-treat 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). 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 post-slavery era, as they offered the potential

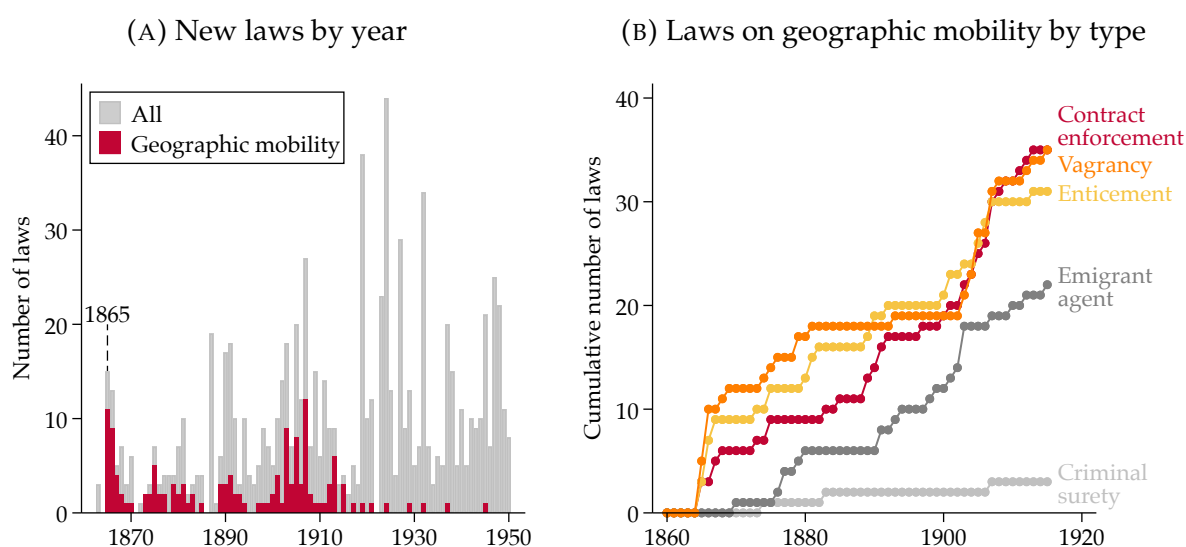
the average across Southern Black Americans)—followed by Georgia and South Carolina (−0.47 years). Missouri has the strongest positive impact (2.28 years), followed by Kentucky (1.66 years).

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 why the region did not experience a larger exodus than the Upper South. For example, 75 percent of Black families enslaved in Louisiana still lived there in 1940; less than 10 percent reached the North (see 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.

First, Jim Crow directly targeted the geographic mobility of Black people (Roback, 1984; Cohen, 1991; Naidu, 2010): enticement laws and contract enforcement laws limited Black workers' ability to terminate their employment contracts; vagrancy laws criminalized being out of employment; emigrant-agent laws prevented employers from seeking workers from other states; criminal surety laws created the possibility of involuntary servitude upon arrests for minor charges (see also Blackmon, 2008). These laws began emerging immediately after slavery (see Figure 4).

FIGURE 4: Number of Jim Crow Laws Across the South



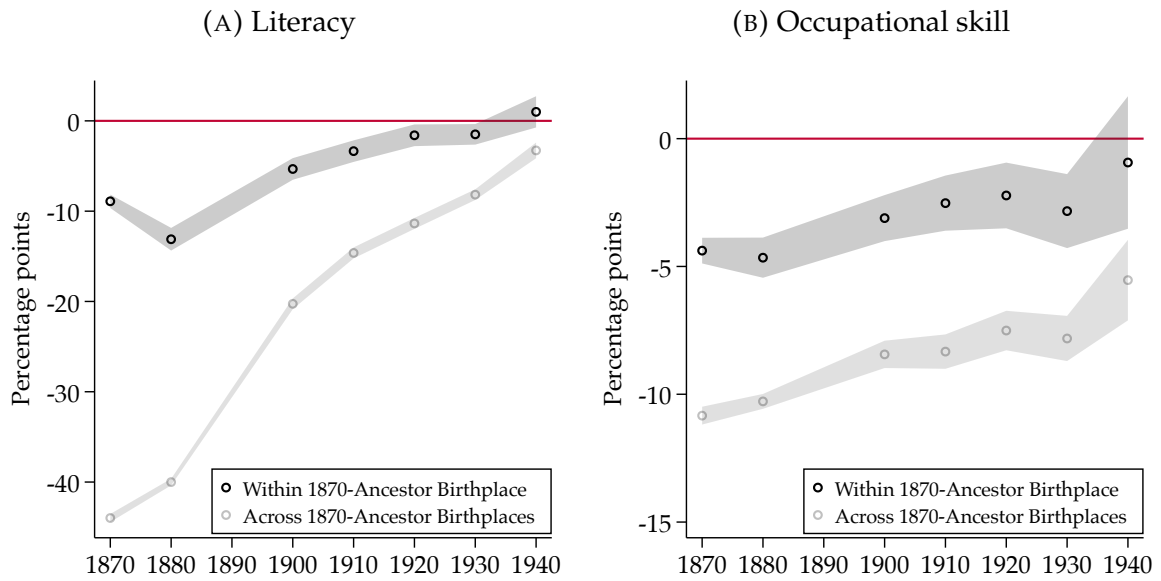
Notes: This figure shows the number of new Jim Crow laws passed across all Southern states each year (panel A) and the cumulative number of laws pertaining to the geographic mobility or employment of Black Americans by type (panel B). See Data Appendix B for details on the data.

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 to 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 et al., 1996). Moreover, despite successful migration to the North, many Black families still faced challenges in capitalizing on available opportunities (Collins,

6.2 The Free-Enslaved Gap is Driven by Geography

To explore the importance of differential exposure to state-specific factors, we first compute the Free-Enslaved gap conditional on ancestor location. To do so, we add fixed effects for the state of birth ℓ of a family's ancestor before 1865 to our baseline specification in equation (5). This exercise provides a back-of-the-envelope assessment of how important geography was in shaping the Free-Enslaved gap's long-run persistence. It does not account for free Black Americans' potential selection into states before 1865.

FIGURE 5: Free-Enslaved Gap Conditional on Ancestor State (1870–1940)



Notes: 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 US. The comparison is made between prime-age (20–54 years) male descendants of enslaved vs. 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 percent confidence bands clustered at the family level. See Data Appendix B for details on the sample and data.

We find that in contrast to the unconditional Free-Enslaved gap, the conditional gap was large in 1870 but shrunk to virtually zero after 1940 (see Figure 5).¹⁶ The 1940 gap in literacy, for example, fully closes after accounting for variation across ancestor states. Similarly, the conditional Free-Enslaved gap in 2000 is insignificant for all outcomes (see 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.

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

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 Appendix D) show that 1) had the Enslaved ancestors been distributed as the Free *within the South*, the Free-Enslaved gap would have been at least 67 percent smaller (lower bound),¹⁷ and 2) had the Enslaved ancestors been distributed as the Free *within both the South and North*, the gap would have closed entirely by 1940 (see 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 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 within states irrespective of their ancestors’ enslavement status (see 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 socioeconomic status than descendants of the Enslaved by 1940 (see 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 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](#)).

6.3 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

¹⁷We 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.

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 et al., 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 non-selective 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 inter-regional slave mobility. Selective slave trade is only evident in the small sugar cultivation areas.¹⁸ Sugar cultivation accounted for 6 percent 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

¹⁸In contrast to the sugar industry, the cotton and tobacco industries (accounting for around 87 percent of enslaved agricultural workers) were generally non-selective on age and sex ([Tadman, 1977](#)).

¹⁹By 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](#), p. 43).

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 also verify that the observable characteristics of enslaved people—such as their age in 1860 or their literacy in 1870—did not discontinuously vary across borders, ruling out selection on observable characteristics directly.

7. 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: 1) places’ effects on Black economic progress differ sharply across state borders and 2) observed non-institutional factors do not differ across state borders. Furthermore, our evidence suggests that Jim Crow regimes are key state institutions responsible: 1) the negative impact of state institutions was race-specific, largely leaving the economic status of white families unaffected, 2) the impact of state institutions can be statistically explained by various measures of states’ Jim Crow intensity, and 3) the impact of state institutions emerged with the onset of the Jim Crow era.

7.1 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):

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

where $\gamma_{\epsilon(\ell)}^t$ 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 $\epsilon(\ell)$ as the geographic coordinates of location ℓ , and $s(\ell)$ as the state that location ℓ 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 non-institutional factors, $\gamma_{\epsilon(\ell)}^t$.

²⁰Formally, $\|\epsilon(\ell) - \epsilon(\ell')\| \rightarrow 0 \Rightarrow |\gamma_{\epsilon(\ell)}^t - \gamma_{\epsilon(\ell')}^t| \rightarrow 0$, whereas $\gamma_{s(\ell)}^t$ only depends on which side of a border ℓ is on, not on the precise coordinates $\epsilon(\ell)$: $\gamma_{s(\ell)}^t = \gamma_s^t$.

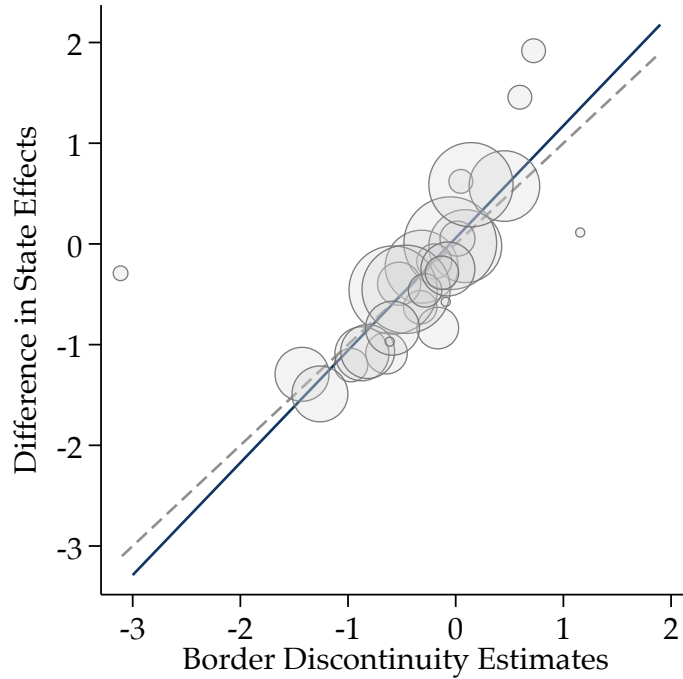
7.2 Border Discontinuity Design

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:

$$y_{i,b}^{1940} = \alpha_b + \beta_b \cdot \text{High}_{i,b}^{1870} + v_b \cdot \text{dist}_{i,b}^{1870} + \psi_b \cdot \text{dist}_{i,b}^{1870} \cdot \text{High}_{i,b}^{1870} + \varepsilon_{i,b}, \quad (9)$$

separately for each border b in the South (see 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 $\text{dist}_{i,b}^{1870}$ is the distance between border b and the county's centroid in which i 's ancestors lived in 1870. The main coefficient of interest, β_b , captures the long-run effect of being freed on the more oppressive side of border b on a Black family's economic status.

FIGURE 6: Differences in Black Economic Progress Arise Sharply at State Borders

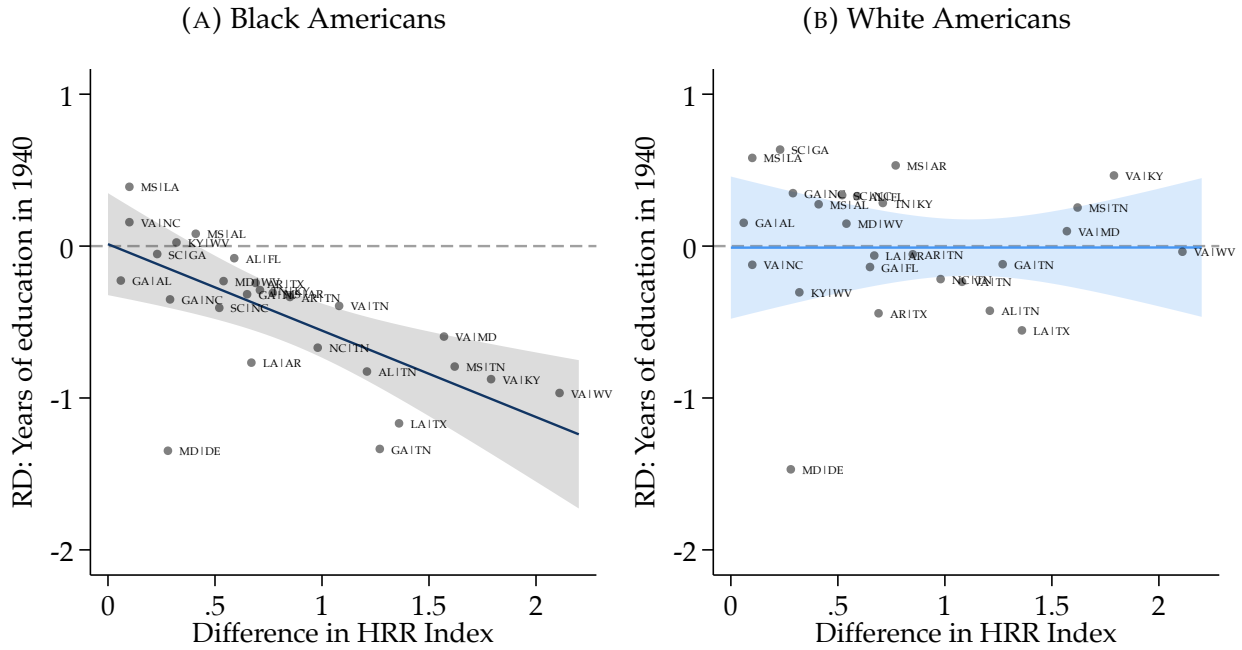


Notes: This figure relates each RD estimate (as shown in Figure 7) to the difference in the two states' overall causal effect on 1940 years of education (as shown in panel A of Appendix Figure C.3). Estimates are weighted by the minimum sample size underlying the difference in state effects. A dashed 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 ($\hat{\beta} = 1.12^{***}$, $R^2 = 0.77$). Findings are robust to excluding Louisiana and Virginia (results available upon request). See Data Appendix B for details on the sample and data.

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 6). We find large border dis-

continuities, 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.

FIGURE 7: Regression Discontinuity Estimates and Jim Crow



Notes: Panel A of this figure shows each separate RD 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 RD estimates and the differences in Jim Crow intensity, weighted by the inverse of the estimates' standard error. Shaded areas represent robust 95 percent confidence bands. For point estimates, we use a 350km bandwidth and empirical Bayesian shrinkage as described in Appendix A.5. See Data Appendix B for details on the sample and data.

Having established the importance of state institutions, we next examine whether it was Jim Crow institutions specifically that slowed Black economic progress. To do so, we correlate our border discontinuity estimates $\hat{\beta}_b$ 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; 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 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 panel A of Figure 7). 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 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 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 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 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 impacted 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 only 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 land-owning 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 panel B of Figure 7). The same is true even for poor white Americans whose ancestors had no measurable human or physical capital in 1870 (see panel A of Appendix Figure C.9). 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 land-owning elite. We find that the more oppressive a Jim Crow regime, the more economically significant the gains by the border region's wealthiest ten percent of white families (see panel B of Appendix Figure C.9). In sum, our results suggest that Jim Crow was an extractive institution that

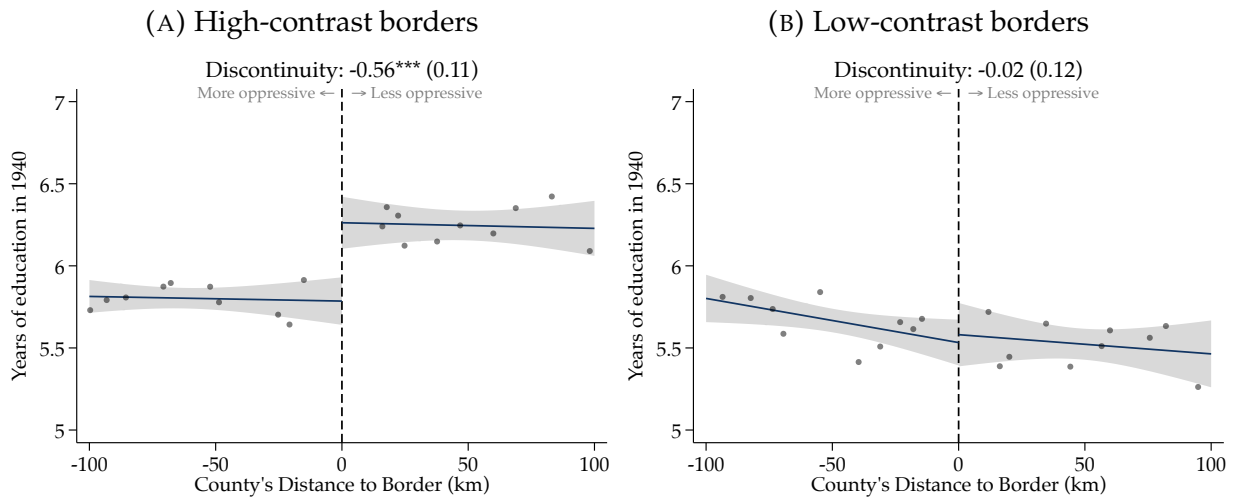
benefited the wealthiest white families at the cost of Black families while shielding poor white families from most economic harm.

The end of slavery led to a drastic change in the geography of racially oppressive institutions in the US. 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.

7.3 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).

FIGURE 8: Pooled Regression Discontinuity Estimates



Notes: This figure shows the RD 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 percent confidence bands clustered at the 1870 county level. See Data Appendix B for details on the sample and data.

Consistent with our main estimates, sharp educational differences only arise for Black

families freed across borders where institutions differ substantially (see Figure 8).²¹ Being freed on the more oppressive side of such a high-contrast border sharply reduced the years of education in 1940 by 0.6 years—10 percent of the average among Black men.

We confirm that differences across high-contrast borders only arise after the onset of Jim Crow (see Figure 9). 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 percent 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 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.

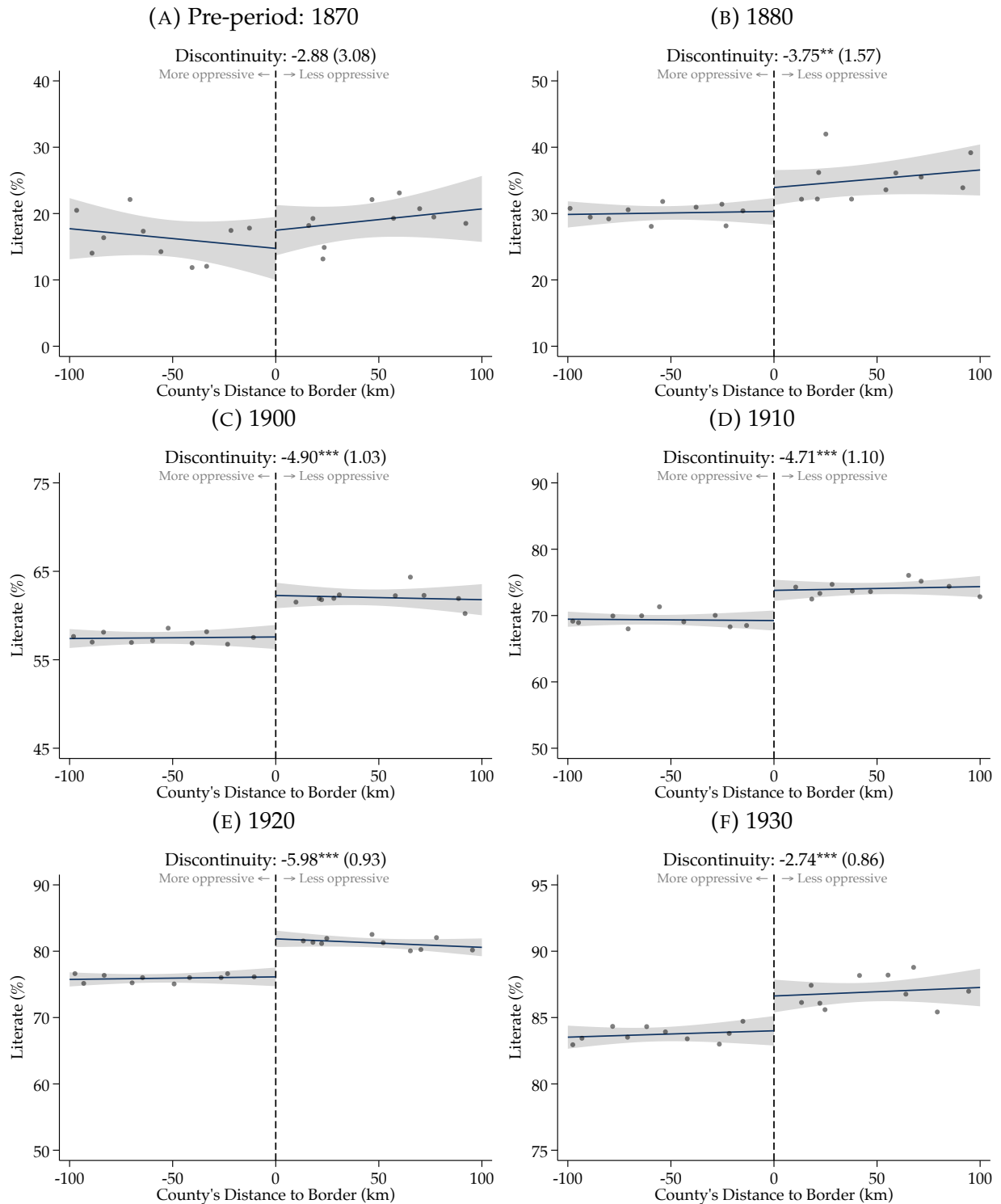
We further present evidence that Jim Crow institutions varied sharply across state borders. We find significant gaps in key outcomes directly targeted by Jim Crow across state borders with differing Jim Crow intensities (see Appendix Figures C.13, C.14, and B.7). Specifically, counties in states with 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 sharply across borders, supporting the assumption that border differences in economic progress capture the effect of state institutions (see 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 Appendix Figure A.5).

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

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

FIGURE 9: Regression Discontinuities in Literacy (High-Contrast Borders)



Notes: This figure shows the RD 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 percent confidence bands clustered at the 1870 county level. See Data Appendix B for details on the sample and data.

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 kilometers (see Appendix Figure A.6). The pooled RD estimates across high-contrast borders (as shown in panel A of Figure 8) for those cutoffs all range between -0.61 and -0.46 and are all highly significant. Our baseline bandwidth is 100 kilometers in pooled estimations—close to the mean squared error optimum—and 350 kilometers 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, the selection of enslaved people into location is implausible to affect 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.

8. 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). However, Black people's desire for education was met with resistance. “[Black Americans'] attempts at education provoked the most intense and bitter hostilities as evincing a desire to render themselves equal to the whites” (Freedmen's Commission Report cited in Du Bois, 1935, p. 645). Robert Higgs argues that governments were the leading force of this resistance:

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

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 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 ($\rho = -0.70$).

Our analysis of Black teacher wages confirms that disparities in school quality are pronounced right at states’ borders, underlining the critical role of institutional factors in shaping the quality of Black schools (see Appendix Figure B.7 and Margo, 1982, 1990; Naidu, 2012; Card et al., 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 Appendix Figure C.16). These findings are consistent with Card and Krueger (1996) and Card et al. (2022) 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.”

9. CONCLUSION

This paper provides new evidence on the long-run impact of racially oppressive institutions, finding that Black Americans’ economic status today depends strongly on their

²³A category’s number of Jim Crow laws is not a conclusive measure of its importance; suffrage laws are a prime example. Suffrage laws are low in number, but their effects are massive (see e.g., Naidu, 2012). Laws in other categories are likely a downstream outcome of Black voter disenfranchisement (Engerman and Sokoloff, 2011). Therefore, while the number of Jim Crow laws on education is extensive, only through further analysis can one conclude that they were a crucial part of states’ Jim Crow regimes.

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 Jr., 2005; Darity Jr. et al., 2017; Darity Jr., 2022)—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 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 et al., 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 percent of 18- to 19-year-old Black Americans have an immigration background, 41 percent of Black Ivy League students do. Affirmative action increases racial diversity on campuses but 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, i.e., wealth transfers to descendants of the Enslaved (e.g., Darity Jr., 2008; Craemer et al., 2020; Boerma and Karabarbounis, 2023; Albuquerque and Ifergane, 2023). We argue that any assessment of the legacy of slavery should incorporate both *when* and *where* a family

was freed—i.e., how long they were enslaved and how intensively they were exposed to Jim Crow after slavery. Our empirical evidence suggests that Black families today are impacted drastically by when and where their ancestors were freed. 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 to those who gained freedom earlier, mainly in the Upper South and North. Many free Black Americans had been enslaved in earlier periods, and all Black Americans faced discrimination regardless of their specific family history.

This paper has limitations that future work may be able to overcome. First, we limit our analysis to men because automated census-linking methods are unavailable or have poor coverage for women. Women have historically tended to change their surnames upon marriage, making it impossible for conventional methods to link them across census records (Althoff et al., 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 paper 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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