Interracial Marriage in the US: The Role of Legal Barriers, Migration, and Segregation

Cameron Deal*

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Abstract

Interracial marriage offers a measure of social integration and plays a role in reducing persistent economic inequality between racial groups. This paper studies the determinants of interracial marriage in the United States. I first record the remarkable increase in the prevalence of interracial marriage—while less than 1% of marriages were interracial in 1950, that number has increased to nearly 10% in 2020 (though still less than expected under random assignment). I evaluate the role of legal barriers, segregation, internal migration, and assortative matching in explaining the prevalence of interracial marriage using decennial Census data. I find that residential segregation, the Great Migration, and assortative matching all affected the prevalence of interracial marriage, but legal barriers did not.

Keywords: Interracial Marriage, Segregation, Great Migration, Marriage Markets

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*Vanderbilt University. cameron.h.deal@vanderbilt.edu. I thank Bhash Mazumder, Daniel Aaronson, William Collins, Christopher Carpenter, Michelle Marcus, Marcus Dillender, Steven Hill, Ben Harrell, Samuel Mann, Laura Nettuno, Santiago Deambrosi, Geoffrey Kocks, Arthur Wickard, and especially Brian Beach for their comments, feedback, and guidance, as well as seminar participants at ASHEcon 2023 and the Federal Reserve Bank of Chicago. I also thank Ellora Derenoncourt for graciously sharing data.

1 Introduction

In 1950, the interracial marriage rate (IMR) between Black and white Americans was approximately 0.2%—50 years later, it was 26 times higher at 5.6%.[1] However, this is still less than half of the IMR expected under random assignment. Evidently, the US marriage market has made significant progress toward social integration but remains far from achieving it. Interracial marriage is a useful marker of social integration—perhaps the most intimate decision one makes in their life is who their partner will be. Beyond this, the segregation of the marriage market contributes to the well-documented and persistent racial gaps in economic outcomes by strengthening the intergenerational transmission of wealth within racial groups (Margo, 2016; Derenoncourt et al., 2023). As Margo (2016) highlights, low levels of racially mixed households can, in combination with high intergenerational transmission and large initial gaps of human capital, create "intergenerational drag," making racial inequality persistent.[2] While substantial research has described trends in interracial marriage rates and estimated structural models of interracial marriage markets (see, for example, Wong (2003); Fryer (2007)), little is known about the causal historical forces that shaped interracial marriage and social integration.

In this study, I explore the determinants of interracial marriage and marital integration in the United States. While previous scholarship has recorded the large increase in the interracial marriage, various explanations have been proposed (Fryer, 2007). Some claim a changing legal landscape lifted previously binding constraints, allowing interracial couples who previously desired marriage to realize it (Moran, 2003; Newbeck, 2008). Studies of access to same-sex marriage, for example, find increases in marriage for same-sex couples following expansion of legal access (Dillender, 2014). However, other contemporaneous historical trends such as decreasing residential segregation (see Figure A5) and internal migration may have also increased the social interactions and integration of Black and white Americans. I am the first (to my knowledge) to use causal empirical frameworks to assess the role of historical forces such as anti-miscegenation statute repeals, residential segregation, and the Great Migration in explaining the prevalence of interracial marriage in the United States. Additionally, I report novel descriptive statistics on interracial couples from historical Censuses.

I use data from the US Decennial Census from 1850-2000, a population enumeration every 10 years that records the demographic and economic characteristics of the American population. I use four designs to assess the causal determinants of interracial marriage; I first evaluate the role of anti-miscegenation statutes

[1] I define the interracial marriage rate (IMR) to be the share of Black marriages, Black and white marriages, and white marriages that are between Black and white spouses. For more detail, see Section 3.

[2] While economic and health disparities between Black and white Americans have reduced, they remain high—the white-to-Black wealth ratio remains at 6 to 1 and Black Americans live on average 3.6 fewer years than their white counterparts (Schwandt et al., 2021; Derenoncourt et al., 2023). Marriage can be one pathway to reducing these inequalities.

(which banned interracial marriage) using a differences-in-differences design. Then, I examine the role of residential segregation using the placement of railroads as an instrument for contemporary segregation. To evaluate the role of non-white population shares, I use a shift-share instrument constructed with the Great Migration. Finally, I use first names as proxies for socioeconomic status and conduct a matching exercise to assess the role of assortative matching and economic differences in explaining the rate of interracial marriage. In addition to evaluating the prevalence of interracial marriage, I also construct a marital integration outcome to account for differences in population racial composition (for example, comparing a city that is 50% Black and 50% white to one that is 5% Black and 95% white). This outcome can be thought of as the fraction of interracial marriages expected under random assignment that are observed in the data.

I find that the permanent repeals of anti-miscegenation statutes do not substantially affect the rate of either interracial marriage or marital integration, and I can rule out effects larger than 2.08 (0.21) interracial marriages per 1000 marriages (points of marital integration) using data from 1850-2000. This lack of substantive effects directly contradicts those who implicate a changing legal landscape as the primary driver of the increase in interracial marriage. While lifting legal barriers may be necessary for interracial marriages to occur, it is not sufficient.

However, I find more support for the role of increasing social contact. A 20-percentile increase in Black population change causes 2.64 additional interracial marriages per 1000 marriages and a 0.31 decrease in marital integration in 1990 (and these effects are substantive across 1970-2000). Additionally, a 1 standard deviation increase in the dissimilarity index (a measure of residential segregation) causes 3.47 (0.61) less interracial marriages (less points of marital integration) in 1990. Taken together, this evidence suggests that changing residential segregation and internal migration have substantively shaped interracial marriage in the United States. Finally, I find that differences in economic endowments and assortative matching explain approximately 60 percent of the gap between the rate of interracial marriage expected under random assignment and the observed rate of interracial marriage in 1900.

My findings contribute to the economic literature on interracial marriage. Previous research has focused on studying the trends in interracial marriage over time and evaluating which models of the marriage market accord with the descriptive evidence on interracial marriage, finding broad increases and support for the Becker model of marriage (Fryer, 2007). Others have used structural models of the marriage market to analyze the determinants of interracial marriage and the benefits from increasing IMRs (Koh, 2022; Wong, 2003). Some research has also examined the effects of anti-miscegenation laws on migration, and found that their repeal impacted the locational distribution of married Black males (Gevrek, 2014). Additionally, other work has analyzed the effects of school desegregation on mixed-race births, finding mixed results and no effects on interracial marriage rates (Gordon and Reber, 2017; Shen, 2018). I build on this literature by

examining the effects of several historical forces on interracial marriage and marital integration using quasi-experimental methods, and find some evidence that contrasts previous work—while others have discounted the role of social interactions in determining interracial marriage, I find that residential segregation and internal migration both affect interracial marriage and marital integration (Wong, 2003). Additionally, I build on work that broadly considers historical forces that affected the marriage market and how those effects may have translated into economic inequality by studying decisions to enter interracial marriage (Ager et al., 2021; Carlana and Tabellini, 2018).

I also contribute to the growing literatures on the effects of residential segregation and the Great Migration. Early work on segregation found strong relationships between residential segregation and adverse economic outcomes for Black residents in the later part of the 20th century (Cutler et al., 1999; Collins and Margo, 2000). Several papers have used quasi-experimental designs to evaluate residential segregation and found that higher segregation reduces upward mobility for Black children and lowers academic achievement while increasing Black poverty and overall Black-white income disparities (Ananat, 2011; Chyn et al., 2022). Additionally, recent work has found that segregation impacts crime and political efficacy for Black residents (Ananat and Washington, 2009; Cox et al., 2022). While these papers evaluate the role of segregation in explaining economic and mortality disparities between Black and white residents, I evaluate its role in limiting the amount of social integration between racial groups. Additionally, the Great Migration was a mass movement of four million African Americans from the US South to the North and West of the United States (Collins, 2021). Previous research has examined how this movement reshaped the demographics and politics of destination cities, prompting white flight and increasing support for civil rights (Boustan, 2010; Calderon et al., 2022; Shi et al., 2022). In general, the migrants themselves experienced large increases in income (Collins, 2021). However, the Great Migration also had deleterious effects on African Americans in destination communities—Derenoncourt (2022) documents how the Great Migration reduced the upward mobility of Black families in the North as community backlash limited the gains that Black migrants could achieve (see also Boustan (2009)). I show that this backlash is reflected in the social integration response—while the IMR increases in response to the Great Migration, it does so at an order of magnitude lower than the increase expected under randomization, leading to decreases in the marital integration of these destination cities.

Finally, I contribute to the literature on race and assortative matching in marriage markets (Chiappori, 2020). Using experiments and data from dating apps, several papers have recorded the existence of substantial racial preferences in dating markets and used this to estimate mate preferences (Hitsch et al., 2010; Fisman et al., 2008). Additionally, some previous literature has recorded the characteristics of interracial couples and used this to evaluate models of marriage in explaining the IMR—in general, this work has found

that status exchange theory[3] and a mating taboo (representing racial preferences) play a dominant role in determining the IMR as opposed to endowment differences or lack of social interaction (Chiappori et al., 2016; Wong, 2003). Finally, some literature has examined how assortative matching, or the tendency of marital matches to share high or low economic status, has shaped both marital outcomes and intergenerational mobility (Craig et al., 2019; Olivetti et al., 2020). I build on this literature by using a matching exercise to document that under pure assortative matching based on socio-economic status, the IMR in 1900 would be much greater than the observed rate and approximately 40% of the random IMR.

The remainder of this paper proceeds as follows. In Sections 2 and 3 I describe the setting and data, respectively. I outline my empirical strategy in Section 4 and present the main results and robustness tests in Section 5. Section 6 concludes.

2 Historical Context

In 1681, a white servant called Irish Nell wed a Black enslaved person called Negro Charles in coastal Maryland— the first recorded interracial marriage in the Americas. Nell and Charles were both of low socioeconomic status and it is likely this trait that allowed their marriage without threatening rigid racial boundaries in the colonial Chesapeake (Hodes, 1999b). Many of their elite white neighbors attended the wedding, perhaps passing judgment on Nell, but her transgression was not enough to warrant retribution. They lived more than 30 years together and had several children. There was no court case or violence—the only reason we have a record of this marriage is that the slave status of their grandchildren needed to be resolved nearly 80 years later.

Three centuries later, Edna Ferber's 1926 novel *Showboat* imagines an 1870s floating theater on the Mississippi. The best actress, Julie, passed for white but was discovered as Black. The sheriff was notified; just as he was about to arrest her, her lover, Steve, a white man, pricked her finger and swallowed some of her blood. Under the logic (or lack thereof) of hypodescent or the "one-drop rule," they were now considered a Black couple and no longer violated Mississippi's anti-miscegenation statute (Williamson, 1995). However, it was a close call—punishments for "miscegenation" or interracial marriage ranged from the annulment of marriage to decades in prison (Hodes, 1999a).

These two anecdotes offer distinct and divergent accounts of the lives of interracial couples in the United States—one interracial couple enjoyed a long and happy marriage, and another needed to resort to drastic measures to avoid arrest. Interracial relationships (and the opposition to them) have long been a topic of study for historians, legal scholars, sociologists, and more recently, economists. Some begin their study with

[3] A marriage pattern in which one spouse compensates for their disadvantage—relative to the other spouse—in one status dimension with an advantage in another status dimension.

the first recorded interracial relationships in America—those between white indentured servants and Black enslaved people in colonial Virginia and Maryland (Hodes, 1999b). Others trace backwards, examining the legal proceedings and political reception of *Loving v. Virginia*, the 1967 Supreme Court Case that declared interracial marriage bans unconstitutional, and analyzing what they can tell us about the accumulated history of the topic (Newbeck, 2008).

Much of the scholarship on race and marriage has emphasized the legal history of anti-"miscegenation" laws, a term coined by two Democrats in 1864 when they created an allegedly Republican pamphlet that advocated for "the Blending of the Races" (Newbeck, 2008; Lemire, 2002a; Romano, 2003). Some claim that the primary intent of these laws was to consolidate racial identity and prevent the emergence of a multiracial class that might threaten the social status of whites (Moran, 2003; Johnston, 1970; Spickard, 1989). Others argue that these laws primarily served as an appendage to slavery in the South and as a way of drawing racial boundaries in the North, pointing to evidence that interracial relationships were only met with violence when racial boundaries began to erode (Hodes, 1999b; Moran, 2003; Romano, 2003; Washington, 2011; Pascoe, 2009). Another explanation of anti-"miscegenation" laws points to gender dynamics, where relationships between white men and nonwhite women were tacitly accepted, while relationships that violated notions of white womens' "purity" were punished harshly (Hodes, 1999b; Pascoe, 2009). Additionally, while some portray the history of these laws as a monotonic decrease over time, there were ebbs and flows in their frequency, with peaks in legal action in the direct aftermath of the Civil War, and during the early twentieth century (Newbeck, 2008; Pascoe, 2009). There were two large bursts of action on the permanent repeal of these laws—one followed the Civil War, when states like Ohio, Rhode Island, and Illinois repealed their statutes as racial attitudes grew more progressive in the Northeast and Midwest. Another began with *Perez v. Sharp* in California in 1948, and was followed by a series of state-level legislative and judicial repeals that culminated in the *Loving v. Virginia* decision, which declared interracial marriage legal nationwide in 1967.

Beyond the intent and prevalence of their laws, scholars also disagree about the effectiveness and enforcement of anti-miscegenation statutes. Some portray these laws as widely ignored, arguing they did not affect the prevalence of interracial marriages and referring to the emergence of large multiracial populations as evidence of their inefficacy (Mills, 1981; Williamson, 1995; Hollinger, 2003; Spickard, 1989). Others point to large numbers of court cases that describe their application not only in the prosecution of interracial couples (which continued until the *Loving* decision), but also in divorce and inheritance cases (Moran, 2003; Hodes, 1999b; Berry, 1991; Newbeck, 2008). Of course, these cases include many of those that challenged anti-miscegenation law—*Pace v. Alabama, Perez v. Sharp, Loving v. Virginia*. Additionally, these cases offer an opportunity to explore how the legal system dealt with the problem of racial classification—something that became more difficult as interracial relationships and multiracial populations grew and rigid racial boundaries

became harder to craft and maintain (Moran, 2003; Pascoe, 2009).

Further complicating legal reactions to interracial marriage, popular culture and community attitudes often engaged with the topic in ways distinct from the legal system. Some popular media, including Civil War-period romance novels, imagined a post-racial future where intermarriage was common (Hodes, 1999a; Sheffer, 2013). However, drawings and cartoons would often reinforce racial boundaries in marriage by caricaturing the partners depicted with racialized physical traits (Lemire, 2002b). Some expressed widespread derogatory beliefs about the couples that entered into interracial relationships, summarized by W.E.B. Du Bois when he wrote, "It is often said that only the worst Negroes and lowest whites intermarry" (Gullickson, 2006). In contrast, there are also accounts of toleration and even anonymous support for interracial relationships, seen in letters to the editor and court attestations to the character of individuals in interracial relationships (Williamson, 1995; Berry, 1991). These mixed accounts of popular support may reflect heterogeneity in support across places or the conflict between personal desire (perhaps manifested in popular media fantasies) and social expectations of racial hierarchy, or both.

As the data landscape for studying interracial couples has improved significantly in the past 20 years, several important quantitative patterns have emerged. First, the prevalence of interracial marriages has increased rapidly since the second half of the 20th century (Fryer, 2007). It appears that interracial relationships are descriptively more likely to dissolve, but this pattern may be due to compositional differences (Zhang and Hook, 2009). Additionally, newly available data has enabled us to study the historical characteristics of this population. Appendix Figures A1, A2, A3 show previously unreported differences in economic and demographic characteristics for interracial couples in the 1880 and 1910 censuses—in general, they fell in between Black and white same-race couples on most measures of economic success and health. However, a question that remains is the forces that may decide whether someone enters an interracial relationship—and what implications that decision may have for their well-being.

3 Data

My primary data source is the US Decennial Census, conducted every 10 years by the US Census Bureau, and publicly available through IPUMS-USA at the University of Minnesota Population Center (Ruggles et al., 2021, 2023). This population enumeration measures the demographic and economic characteristics of the US population. For 1850-1940, I use the full-count data, which allows me to capture a sizeable sample of interracial couples in those years. Additionally, I use the 1950 1%, 1960 5%, 1970 2%, 1980 5%, 1990 5%, and 2000 5% samples to complete a time series from 1850-2000.

The mode of enumeration changed twice–from 1790-1870 the Census was conducted by the US Marshals,

an agency primarily tasked with law enforcement. However, from 1880-1950, the Census was conducted by a force of trained enumerators who went door-to-door methodically. Finally, starting in 1960, the Census began to be conducted by mail and then enumerators would "sweep" the area, checking on residences where no return was received. These changes to enumeration mode may have implications for the precision of my measures of interracial marriage and the frequency of error–for example, interracial couples starting in 1960 could hide aspects of their background if they feared repercussions from disclosing their status. The original data was recorded each decade, as required by the Constitution. For the historical censuses, the records were collected at the local level, and then a system of administrators passed them up to the Census Bureau near Washington, D.C. The Census has some data practices that are relevant for this project. Prior to 1960, the census enumerator was responsible for categorizing persons and was not specifically instructed to ask the individual his or her race. Throughout much of this period there was a focus on the concept of "hypodescent" or the "one-drop rule"—if an individual had even one drop of non-white blood, they were recorded as non-white, so the race classifications in this period should be viewed through this lens. In practice, this meant that enumerators would label anyone who had any "appearance" or potential suspicion of being Black as Black. Additionally, for the 1850-1880 and 1910-1920 Censuses, enumerators would classify respondents as "mulatto," indicating they were mixed race, but this was later recoded as Black during the digitization process by employees of IPUMS to improve the comparability of the race variable across Census years (and remains that way in my analysis). For example, in the 1880 Census, enumerators were given the following instructions about race:

> *It must not be assumed that, where nothing is written in this column, "white" is to be understood. The column is always to be filled. Be particularly careful in reporting the class mulatto. The word is here generic, and includes quadroons, octoroons, and all persons having any perceptible trace of African blood.*

In 1970 and later years, an individual's race was reported by someone in the household or group quarters. Two additional notes are necessary to facilitate the interpretation of Census results. First, in 1850-1860, enslaved individuals were enumerated separately and no individual information was recorded about them. Thus, the results regarding Black people from 1850 and 1860 represent only the selected sample of free individuals. Additionally, all of the time series are missing the 1890 census—most records from the 1890 Census were lost in a fire and are hence not available for this project.

I measure my primary variable of interest, the IMR, by linking the race of the spouse and estimating the fraction of Black and white marriages that are between a Black and white spouse. In this project, I focus on Black-white interracial couples, as the historical forces, legal environment, and popular reactions to this population is quite different from those of other interracial couples. In practice, I restrict to Black

and white respondents that are identified as the head of the household or the spouse of the household head and drop any respondents for whom the race of spouse is unobserved or a race other than Black or white.[4] More formally, for a geographic unit g and Census year t, the observed IMR (μ^o_{gt}) is simply the fraction of marriages that are interracial:

$$\mu^o_{gt} = \frac{m_i}{m_i + m_w + m_b}$$

where m_i is the count of interracial marriages, m_w is the count of white marriages (both spouses are white), and m_b is the count of Black marriages (both spouses are Black), all within geographic unit g and Census year t.

One danger is that my measure of interracial couples—those who were recorded in the Census as having a different race from their spouse—is picking up random noise due to the potential of recording error. If, for example, the original Census enumeration form recorded both spouses' races as white but one was unclear, it may have been miscoded as Black, leading to a false "interracial" couple and upwards biasing my results. Alternatively, in some Census years, enumerators may have only filled in the household head's race and left the race of the rest of the respondents blank, and in these cases the race of the rest of the household would have been imputed as matching that of the household head, potentially downwards biasing my results. While these transcription errors are certainly possible, I find differences in socioeconomic status across gender-race pairs (nonwhite men married to white women vs. white men married to nonwhite women), and errors are likely to be random with respect to gender, suggesting my measures are picking up something other than random error. Additionally, I run a county-level regression that examines the predictiveness of my interracial marriage measure for mixed-race ethnicity in both 1880 and 1990, displayed in Appendix Table A1. In both Census years, my IMR measure is highly predictive of county-level mixed-race ethnicity, even when controlling for the fraction of the population that is Black, suggesting that interracial relationships are more common in these areas. Another potential complication is the possibility of intentional changes in racial classification—Dahis et al. (2019) records a substantial number of African American men who changed their racial identity and "passed" for white using the 1880-1940 Censuses. These men would likely be recorded as members of interracial couples if there were no corresponding changes in race for their partners. However, Dahis et al. (2019) also suggest that these individuals lived their lives as white to escape discrimination and increase employment opportunities, so these "interracial" couples might capture valuable information about social integration and marital norms of their environs. Additionally, my primary period of analysis for segregation and the Great Migration concerns later Censuses, when the premium associated with passing for

[4] In robustness exercises for my analysis of anti-miscegenation statutes, I consider the unconditional interracial marriage rate (not using these restrictions) and my results are similar.

white was reduced considerably, though it is still possible that passing may be present in these time periods.[5]

While the above outcome captures the IMR, it may not describe meaningful variation in social integration. Imagine two geographic units where the observed IMR is 5%, but 10% of the marriage market is Black and 90% is white in the first, while 50% of the marriage market is Black and 50% is white in the second. Evidently, the first is more integrated than the second, but the observed IMR would be the same. To overcome this problem, I create a measure of expected interracial marriages—the IMR we would expect under random assignment. It takes the form of a weighted average of the outmarriage rates for four race-gender groups. The IMRs under random assignment at the level of geographic unit g at time t (μ^r_{gt}) begins with finding four key quantities among the married couples (presumably composing the marriage market) in a geographic unit g: $\text{share}_g^{\text{white, men}}$—the percent of the marriage market in geographic unit g that consists of white men, $\text{share}_g^{\text{white, women}}$—the percent of the marriage market in geographic unit g that consists of white women, $\text{share}_g^{\text{black, men}}$—the percent of the marriage market in geographic unit g that consists of Black men, and $\text{share}_g^{\text{black, women}}$—the percent of the marriage market in geographic unit g that consists of Black women. Once we have these quantities, the IMR under random assignment of marriage for a certain group is the fraction of the opposite gender that has the opposite racial identity. For Black men, for example, the predicted IMR would be $\frac{\text{share}_g^{\text{white, women}}}{\text{share}_g^{\text{white, women}}+\text{share}_g^{\text{black, women}}}$. Thus, we can calculate this predicted rate for each group and then construct a weighted average for the geographic unit:

$$\mu^r_{gt} = \text{share}_g^{\text{white, men}} \frac{\text{share}_g^{\text{black, women}}}{\text{share}_g^{\text{black, women}} + \text{share}_g^{\text{white, women}}} + \text{share}_g^{\text{white, women}} \frac{\text{share}_g^{\text{black, men}}}{\text{share}_g^{\text{black, men}} + \text{share}_g^{\text{white, men}}}$$

$$+\text{share}_g^{\text{black, men}} \frac{\text{share}_g^{\text{white, women}}}{\text{share}_g^{\text{white, women}} + \text{share}_g^{\text{black, women}}} + \text{share}_g^{\text{black, women}} \frac{\text{share}_g^{\text{white, men}}}{\text{share}_g^{\text{white, men}} + \text{share}_g^{\text{black, men}}}$$

In the numerical example above, the first scenario would have $\mu^r_{gt} = 0.18$ and the latter would have $\mu^r_{gt} = 0.5$ Finally, I construct a measure of marital integration m_{gt} for a geographic unit g and time t that is the ratio of these two measures:

$$m_{gt} = \frac{\mu^o_{gt}}{\mu^r_{gt}}$$

I plot a national time series of these three outcome measures in Figure 1. Throughout my analysis, I estimate these quantities at different levels of geography g depending on the level at that my independent variable of interest is measured. For my matching exercise, I use the full count 1880 census available through IPUMS International, which contains data on first and last names (Ruggles et al., 2021). I use this to proxy socioeconomic status pre-marriage market—more details can be found in the Empirical Strategy.

[5]Another consideration is that the racial category of "white" expanded over time. Many immigrants, for example from Italy and Ireland, were not accepted as white and could be treated as non-white in the eyes of the legal system (Roediger and Cleaver, 2022; Wilkerson, 2020).

Additionally, I use several additional datasets to estimate the effects of anti-miscegenation statutes, segregation, and the Great Migration. For segregation, I use the sample of 121 non-Southern metropolitan areas for which Ananat (2011) located 19th-century maps needed to construct the railroad placement instrumental variable. Additionally, data from Cutler et al. (1999) is used to measure metropolitan residential segregation in the years 1970-2000. For anti-miscegenation statutes, I collect data on the timing of permanent repeals of anti-miscegenation statutes from Pascoe (2009); Fryer (2007); Washington (2011), summarized in Appendix Table A24. Finally, I use the sample of 130 non-southern commuting zones (CZs) used in Derenoncourt (2022) for which data on the urban Black population in 1940 and 1970 is collected from the census and from the County and City Data Book 1944–1977 (CCDB), which is used to construct the Great Migration shift-share instrument.

4 Empirical Strategy

I use four approaches to study the determinants of interracial marriage and marital integration in the United States. One primary question is whether legal variation in access to interracial marriage affected the rates of interracial marriage (and might explain the large increase in the latter half of the 20th century). I use a differences-in-differences (DD) approach to assess the effects of anti-miscegenation statute repeals.

However, internal migration and declining residential segregation have greatly increased interaction opportunities for Black and white Americans. Thus, I use two approaches to assess the impact of plausibly exogenous variation in Black in-migration and residential segregation on interracial marriage outcomes. I use a shift-share approach with the Great Migration to explore the effects of increasing black population share and internal migration. Then, I use historical railroad placement to instrument for contemporary segregation and evaluate its relationship with interracial marriage and marital integration.

Finally, I conduct a matching exercise to assess the role of economic endowment differences and assortative matching. One could imagine that in a world absent racial preferences, there would still be lower rates of interracial marriage given large endowment differences between Black and white Americans and the tendency of couples to match assortatively. This exercise aims to capture the importance of that factor.

4.1 Anti-Miscegenation Statutes

I first estimate a standard DD design using the variation in permanent anti-miscegenation statute repeals over time. While twelve states never had bans on interracial marriage,[6] seven had bans in effect that were repealed between 1850 and 1890. Then, another wave of repeals occurred between 1948 and 1967, with

[6] And Pennsylvania repealed its ban in 1780.

fourteen states repealing their anti-miscegenation statutes. Finally, in 1967, *Loving v. Virginia* was the US Supreme Court case that overturned all remaining anti-miscegenation statutes. Because the legal status of interracial marriage in some cases changed several times, I define treatment with the permanent/final repeals, and define the first census year t after the repeal to be the first time that state g is treated. This takes the following form:

$$\mu^o_{gt} = \beta_0 + \beta_1(\text{REPEAL}_{gt}) + \beta_2\gamma + \beta_3\delta + \epsilon_{gt} \tag{1}$$

$$m_{gt} = \beta_0 + \beta_1(\text{REPEAL}_{gt}) + \beta_2\gamma + \beta_3\delta + \epsilon_{gt} \tag{2}$$

μ^o_{gt} is the IMR in state g and time t, m_{gt} is the marital integration in state g and time t, REPEAL_{gt} is an indicator variable that returns a 1 when the observation is a state g that permanently repealed their anti-miscegenation statute at or before time t, γ is state fixed effects, δ is year fixed effects, and ϵ_{gt} is an idiosyncratic error term. Hence, my coefficient of interest, β_1, is intended to capture the causal effect of anti-miscegenation statute repeals on interracial marriage and marital integration.

$$\mu^o_{gt} = \beta_0 + \sum_{j=-4}^{4} \beta_1^j(\text{REPEAL}^j_{gt}) + \beta_2\gamma + \beta_3\delta + \epsilon_{gt} \tag{3}$$

$$m_{gt} = \beta_0 + \sum_{j=-4}^{4} \beta_1^j(\text{REPEAL}^j_{gt}) + \beta_2\gamma + \beta_3\delta + \epsilon_{gt} \tag{4}$$

Additionally, I generate event study estimates of the effects of the anti-miscegenation statue repeals on the IMR and marital integration. In these models (displayed in equations 3 and 4), I replace the single anti-miscegenation statute repeals indicator with a series of event-time indicators (REPEAL^j_{gt}) where j is the time period relative to the event, created by interacting each relative event time indicator with the DD dummy. As standard in the literature, I normalize the first lead operator (T-1) to zero.

The key assumption of the differences-in-differences design is parallel trends—that states that repealed anti-miscegenation statutes would have IMRs that evolved similarly to those states that did not repeal them yet absent treatment (or never had a law on the books). While inherently untestable, this assumption does generate some testable predictions—first, prior to treatment, event study coefficients should be close to zero and insignificant, suggesting parallel pre-trends. I present evidence in my results that this is the case. Additionally, the historical contexts for many anti-miscegenation statute repeals were often court cases, which tended to have unpredictable timing and outcomes based in part on judge assignment and finding petitioners whose case could rise through the appeals system without being dismissed by a lower court, suggesting that the timing of this variation may have been exogenous to factors like racial attitudes that might bias the relationship between anti-miscegenation statute repeals and interracial marriage. For

example, *Perez v. Sharp*, the 1948 California Supreme Court Case that began the legal momentum for repealing anti-miscegenation statutes in the 20th century, began when Andrea Perez (a Mexican American woman) and Sylvester Davis (an African American man) met while working in the defense industry and applied for a marriage license with the County Clerk of Los Angeles—they were denied. They petitioned the California Supreme Court to compel the issuance of the license, setting in motion the legal arguments that would lead to *Loving v. Virginia* 19 years later.

However, because recent literature has found that using the naive DD and event study specifications in settings with differential treatment timing can generate biased estimates, I also estimate these same designs using the specifications outlined in Callaway and Sant'Anna (2021). Additionally, Marcus and Sant'Anna (2021) outlines the implications of different parallel trends assumptions depending on the comparison group, which might consist of either "not-yet-treated" units or "never-treated" units in different contexts. In my setting, I have both comparison groups—12 states never had any anti-miscegenation statutes on the books, so they had no repeal to change their treatment status. However, if states that never had anti-miscegenation statutes differ in fundamental ways from those that have them (and eventually repeal them), then perhaps this is not the desired comparison group, and a better comparison is to those states that have not yet repealed their anti-miscegenation statutes. Due to time variation in these repeals, I can additionally exploit this comparison group and my estimates are nearly identical across specifications.

4.2 Great Migration

The Great Migration was a mass migration of more than 4 million African Americans who moved North in search of opportunity outside of the heavily segregated Jim Crow South. It also offers an opportunity to examine the effects of exogenous increases in Black population share on marriage markets and marital integration in non-Southern areas. I follow Derenoncourt (2022) by measuring this population change at the Commuting Zone (CZ) level and define the Great Migration Black population change as the 1940 to 1970 increase in urban Black population as a share of initial 1940 urban population:

$$\Delta\text{Black pop}_{CZ}^{1940-1970} = \frac{b_{\text{urban},CZ}^{1970} - b_{\text{urban},CZ}^{1940}}{\text{pop}_{\text{urban},CZ}^{1940}}$$

where $b_{\text{urban},CZ}^{t}$ is the total Black population in all sample cities in commuting zone CZ in year t.

As Derenoncourt (2022) outlines, this distribution is highly right-skewed, so I instead use the percentile function of the increase (GM_{CZ}) as the key independent variable for the effects of the Great Migration. However, the relationship between this variable and interracial marriage and marital integration cannot be interpreted as causal because many of the correlates of Black population change may drive this relationship

(for example, the racial attitudes of receiving commuting zones). Thus, I instead use a shift-share approach, which has been used previously in the Great Migration context Derenoncourt (2022); Boustan (2010). The intuition with a migration shift-share is that migration decisions are often due to a combination of "push" and "pull" factors of both origin and destination locations. Additionally, Black southern migrants tended to move where previous migrants from their communities had settled. Thus, when "pushes" from Southern counties cause outmigration, some component of the migration destination can be predicted with the pre-existing loctional distribution of Black Southern migrants. These shocks to "push" factors are plausibly exogenous with respect to shocks to "pull" factors. Interacting exogenous shifts in migration from origin locations with historical migration patterns in destination locations yields a potential instrument for Black population changes in the North.

Shift-share designs can be formulated as a set of assumptions about the exogeneity of the shifts, shares, or both to yield a parameter of interest (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2021; Adão et al., 2019). In my setting, I use shocks to Southern counties (push factors) as my "shifts" and the distribution of pre-1940 Southern migrants as my "shares." Because early Black Southern migrants were not choosing their destinations at random, these shares do not yield a path to identification (Derenoncourt, 2022). However, the shocks to Southern counties are plausibly exogenous to unobserved determinants of interracial marriage rates in Northern cities, and it is this design on which I rely for identification.

More formally, following Derenoncourt (2022) and Borusyak et al. (2021), I rely on two assumptions to identify the effects of the Great Migration on interracial marriage rates:

1. Conditional on baseline characteristics, the instrument for Black population increases is orthogonal to omitted characteristics that are correlated with changes in interracial marriage after 1940:

$$\mathbb{E}[\widehat{GM}_{CZ} \times \tilde{\varepsilon}_{CZ} | X'_{CZ}] = 0$$

2. A shock-level law of large numbers applies—there are sufficient independent shocks, each with sufficiently small average exposure.

While Assumption (1) is inherently untestable, I provide corroborating evidence by testing whether my instrument is associated with pre-Great Migration interracial marriage rates (in the spirit of testing pretrends in a difference-in-differences design). Appendix Table A11 displays results from these regressions—in all cases the coefficients are insignificant and small in magnitude. Thus, the Migration does not predict interracial marriage rates 1900-1940.[7] Assumption (2) is supported by my use of shocks to over 1200 origin counties

[7] Additionally, Derenoncourt (2022) highlights that a key component of this assumption is that shocks to the South are

rather than a state-level analysis, enabled by Derenoncourt (2022)'s use of complete-count Census data. Thus, I rely on these two assumptions to identify the effect of the Great Migration on interracial marriage outcomes in the North with a shift-share approach.

The instrument is constructed as follows: I replace the numerator in the black population change measure above with the predicted, instead of observed, increase in the Black population:

$$\text{Predicted Black pop}_{CZ}^{1940-1970} = \frac{\hat{\Delta} b_{\text{urban},CZ}^{1940-1970}}{\text{pop}_{\text{urban},CZ}^{1940}}$$

where $\hat{\Delta} b_{\text{urban},CZ}^{1940-1970}$ is the predicted increase, defined as follows:

$$\hat{\Delta} b_{\text{urban},CZ}^{1940-1970} = \sum_{j \in S} \sum_{c \in CZ} \omega_{jc}^{1935-1940} \cdot \hat{m}_j^{1940-1970}$$

and $\omega_{jc}^{1935-1940}$ is the share of recently migrated pre-1940 Black southern migrants from county j living in city c in 1940. The term $\hat{m}_j^{1940-1970}$ is the predicted Black migration from southern county j, which comes from the sum of fitted values of decadal predictions of southern county net migration using lagged southern economic predictors of migration. See Derenoncourt (2022) for more details on the construction of $\omega_{jc}^{1935-1940}$ and $\hat{m}_j^{1940-1970}$. Then, after computing predicted increases in the northern CZ-level Black population, I use the percentile of predicted increases, $\widehat{GM}_{CZ}$, to instrument for observed increases in the Black population.

The estimating equations are as follows:

$$\text{First Stage: } GM_{CZ} = \gamma + \delta \widehat{GM}_{CZ} + X'_{CZ}\rho + \varepsilon_{CZ} \tag{5}$$

$$\text{Reduced Form: } y_{CZ} = \tilde{\alpha} + \tilde{\beta} \widehat{GM}_{CZ} + X'_{CZ}\tilde{\rho} + \tilde{\varepsilon}_{CZ} \tag{6}$$

The first stage equation 5 estimates the first stage relationship between the instrument, the percentile of predicted Black population change $\widehat{GM}_{CZ}$, and the percentile of actual Black population change GM_{CZ}. In equation 6, $\tilde{\beta}$ represents the reduced form impact of the Great Migration instrument on observed IMR, marital integration, and random IMR, respectively. All specifications include the control vector X'_{CZ}, which consists of census region fixed effects and the share of the urban population made up of 1935–1940 Black southern migrants. For my main results, I report the reduced form ($\tilde{\beta}$) and 2SLS ($\tilde{\beta}/\delta$) coefficients outlined

uncorrelated with shocks to the North. She provides evidence that identification with this instrument is not the result of correlated shocks to origin and destination locations by constructing alternative instruments and conducting an overidentification test.

above, as well as OLS (β) coefficients from a regression of the following form:

$$\text{OLS: } \mu^{o}_{CZ} = \alpha + \beta GM_{CZ} + X'_{CZ}\rho + \varepsilon_{CZ} \tag{7}$$

The coefficient β from equation 7 represents the OLS estimate of the effect of GM_{CZ}, the commuting zone level percentile of Black population increase 1940-1970, on my three interracial marriage outcomes, conditional on the controls outlined above.[8]

4.3 Segregation

Following prior literature, I measure segregation using the index of dissimilarity (Chyn et al., 2022; Ananat, 2011):

$$\text{Seg}_c = \frac{1}{2} \sum_{n \in c} \left| \frac{\text{Black}_n}{\text{Black}_c} - \frac{\text{White}_n}{\text{White}_c} \right|$$

where Black_n is the Black population in tract n, Black_c is the Black population in city c, and White_n and White_c are defined analogously for White population. This measure can be thought of as the share of the Black population that must relocate to achieve complete integration. It ranges between 0 and 1, indicating complete integration and segregation, respectively. Interpreting the direct effects of segregation on interracial marriage is difficult, as there are many factors that might simultaneously cause segregation and have effects on IMRs—for example, local government policies, labor market geography, or racial attitudes. To address this potential endogeneity, I build on prior work by Ananat (2011), which constructs an instrumental variable (IV) for contemporary segregation in Northern cities using the historical placement of railroads. The basic intuition is that when Black migrants arrived in a city, preexisting railroad networks facilitated the division of cities into predominantly single-race areas through coordinated behaviors by white residents.

To measure this activity, Ananat (2011) uses a railroad division index (RDI):

$$\text{RDI}_c = 1 - \sum_{r \in c} \left(\frac{\text{area}_r}{\text{area}_c} \right)^2$$

where r indexes "railroad neighborhoods" (the regions constructed by the intersection of historical railroad lines), area_r is the area of land in railroad neighborhood r, and area_c is the total area of land in city c. This captures the amount of subdivision generated by railroad track placement, so cities that had a greater number of delineated areas had more potential for segregation. This measure ranges from 0 (representing a city with 1 railroad neighborhood) to 1 (representing a city with a nearly infinite number of railroad neighborhoods).

[8] Equation 7 displays the equation estimating effects on observed interracial marriage rates.

I follow Ananat (2011) and Chyn et al. (2022) in using RDI_c as an instrument for residential segregation and estimate using two-stage least-squares (2SLS). Additionally, I test the robustness of my results to controlling for historical railroad track per square kilometer, a measure that is correlated with RDI and could affect outcomes independently. Chyn et al. (2022) choose not to control for this, noting issues with the interpretation of linear IV estimates when controlling for covariates and an outlier in their data that causes substantial uncertainty across estimates. In general, my results are substantively unchanged whether controlling for this variable or not.

The main estimating equations take the following form:

$$\text{First Stage: } \text{Seg}_c = \gamma + \delta \text{RDI}_c + X_c'\rho + \varepsilon_c \tag{8}$$

$$\text{Reduced Form: } y_c = \tilde{\alpha} + \tilde{\beta} \text{RDI}_c + X_c'\tilde{\rho} + \tilde{\varepsilon}_c \tag{9}$$

The first stage equation 8 estimates the first stage relationship between the instrument, the RDI index RDI_c, and the contemporary segregation of an area, measured with the dissimilarity index, Seg_c. In equation 9, $\tilde{\beta}$ represents the reduced form impact of the RDI instrument on the observed IMR and marital integration, respectively. Some specifications include the control vector X_{CZ}', which consists of historical railroad track length per square kilometer. For my main results, I report the reduced form ($\tilde{\beta}$) and 2SLS ($\tilde{\beta}/\delta$) coefficients outlined above, as well as OLS (β) coefficients from a regression of the following form:

$$\text{OLS: } \mu_c^o = \alpha + \beta \text{Seg}_c + X_c'\rho + \varepsilon_c \tag{10}$$

The coefficient β from equation 10 represents the OLS estimate of the effect of Seg_c, the city-level dissimilarity index, on two interracial marriage outcomes, conditional on the controls outlined above.[9]

More formally, this design relies on three crucial assumptions to identify the causal effect of segregation on interracial marriage outcomes:

1. The RDI serves as a valid instrument for the contemporary segregation, meaning it has a strong relationship with the causal variable of interest.

2. The RDI instrument is independent of potential outcomes (in this case potential rates of interracial marriage at the city level).

3. The RDI instrument only affects interracial marriage outcomes through segregation—the exclusion restriction.

[9]Equation 10 displays the equation estimating effects on observed interracial marriage rates.

Assumption (1) is equivalent to the strength of the first stage—I provide evidence in the results that the RDI instrument is strongly predictive of residential segregation, and the first-stage F-statistic for 1990 is 24.5. Other work has also verified the strength of this relationship (Ananat and Washington, 2009; Ananat, 2011; Chyn et al., 2022; Cox et al., 2022). Assumption (2) is inherently untestable, but I provide some evidence by examining the relationship between the instrument and placebo interracial marriage outcomes in 1930, before the segregation differences between high RDI and low RDI cities emerge. Appendix Table A10 displays these results for my two main outcomes with and without controls for historical railroad placement. These coefficients are all insignificant and small in magnitude, suggesting that prior to segregation differences, interracial marriage outcomes across places with high and low RDIs were similar.

Assumption (3) states formally that historical railroad placement measured via RDI_c is only related to interracial marriage outcomes through segregation and is untestable. This identification rises in part from geographic factors like hill placement and distance that may have determined both the extent of railroad track development in a city and the layout of that track Chyn et al. (2022). I present evidence that my results are robust to the inclusion of historical railroad track length as a control as support for this assumption.

Together, these assumptions enable me to identify the causal effect of segregation on interracial marriage outcomes, using the historical placement of railroad tracks as an instrument.

4.4 Assortative Matching

One possibility is that differences in economic endowments between races may contribute to the gap between expected (under random assignment) and observed interracial marriage—even if there was no taboo or social barriers to interracial relationships, we might expect that couples form based in part on matching socioeconomic status. This would result in a lower IMR than under random assignment due to well-documented income and wealth gaps between racial groups (Derenoncourt et al., 2023; Fryer, 2007). For example, Figure A3b displays average earnings scores by couple type in the 1880 Census, which measures the percentage of persons in occupations having lower standardized median earnings than the respondent's occupation. While white couples have a mean earnings score of approximately 40, Black couples have a mean earnings score of approximately 25, and interracial couples are around 35. It is difficult to isolate the role that assortative matching and economic endowments play in determining IMRs, as exogenous variation in assortative matching across marriage markets is difficult to measure or construct. Instead, I use a counterfactual simulation that relies on a result from the theory of matching models with transferable utility (which has been frequently applied to model marital matching) to make a step in the right direction toward assessing the role of assortative matching and economic endowments in interracial marriage and marital integration.

Under the assumptions that matching occurs off of a single characteristic (or a unidimensional index) and that the marital surplus function is supermodular, the only stable matching is assortative on that characteristic (Chiappori, 2020). Thus, if I consider the situation that couples are matching based purely on socioeconomic status (and importantly, not race), I can then construct an assortative matching and measure the IMR and marital integration under this counterfactual matching. First, I restrict to all members of married couples in county c—this constitutes the marriage market with which I can construct a counterfactual matching—and "separate" married couples, considering them as if they are entering the marriage market anew. Then, I rank all married women in county c on socioeconomic status, and all married men in county c on socioeconomic status separately.

I then use an algorithm to perform a one-to-one matching on this unidimensional measure of socioeconomic status between all men and women in the marriage market of county c (I am guaranteed equality across sexes because the pool is of married couples). The algorithm used in a one-to-one match starts by evaluating which target observation each base observation is closest to and vice versa for each target observations. If a base and target observation pair mutually prefer each other, then these two observations are matched. The algorithm then repeats the initial two evaluation steps, and excludes observations after they are matched, until all base observations are matched. Matched observations end up in pairs of exactly one base observation and one target observation.

Once I have constructed this counterfactual matching for each county c, I can measure my two main outcomes— the IMR (μ^o_{gt}) and marital integration measure (m_{gt})—under these pairings. Then, I plot the county-level distributions of observed and simulated outcomes to determine how much of the gap between observed and expected IMRs (under random assignment) might be attributed to assortative matching and economic endowment differences.

There is an additional complication with this exercise—I only observe equilibrium socioeconomic status for members of married couples. It may be the case, for example, that a wife of high socioeconomic status responded to marriage with a husband of high socioeconomic status by reducing labor force participation or not working at all. Thus, she would appear to be low socioeconomic status in this matching exercise when in reality she is of high socioeconomic status and would likely match with another person of high socioeconomic status in a counterfactual marriage market. Additionally, for earlier Census years, economic outcomes are often only measured at the household or husband level, making it impossible to separate socioeconomic status. Instead, what I need is a proxy for their socioeconomic status pre-marriage market. One possibility is to use parental income—valuable work has begun to link more and more Census materials across time. However, a large limitation of these links is that they rarely link fathers to daughters, who change their name upon entering marriage. To overcome this problem, I use the method introduced in Olivetti et al.

(2020); Olivetti and Paserman (2015), which constructs a pseudo-linked proxy for parental socioeconomic status using first names for all 20-35 year olds enumerated in a Census. Thus, I use this proxy as a measure of pre-marriage market socioeconomic status with the 1900 Census, which has a 100% sample available with first-name data.

The central idea of this empirical strategy is to impute father's income using the average income of all fathers of children with a given first name. This approach only works, however, if first names carry information about socioeconomic status—if, for example, high socioeconomic status parents are more likely to name their daughter Lucy and low socioeconomic status parents are more likely to name their daughter Molly. Olivetti and Paserman (2015) find that there is substantial between-name heterogeneity in parental income and use this to estimate intergenerational mobility for both daughters and sons. I use their approach, leveraging the publicly available 1880 Census 1% sample with names to construct average incomes of fathers for each son and daughter name. However, I make one change—while they focus only on white individuals, I also incorporate Black individuals into the sample. Additionally, I match based off both names and race—proxying a 1900 Black woman named Elizabeth's parental income with the average income of all Black fathers with a daughter named Elizabeth in 1880. Then, I conduct the simulation described above using these proxies for parental socioeconomic status.

5 Results

5.1 Anti-Miscegenation Statutes

Anti-miscegenation statutes sought to restrict interracial relationships and marriages through prohibitions and penalties included the annulment of the marriage, fines, and in some cases imprisonment. I use the permanent repeal of these laws across states and time to assess their impact on the prevalence of interracial marriage—we might expect that once these laws are removed, the prevalence of interracial marriage and marital integration in those states would increase. Graphically, this intuition does not appear to be true in the case of one of the largest legal changes in the status of interracial marriage—the Supreme Court Case *Loving V. Virginia*. While *Loving* legalized interracial marriage across the United States, those states affected by it do not appear to experience a larger increase in interracial marriage rates following the decision (see Figure 2).

Table 1 reports the results of a standard difference-in-differences using the permanent repeal of these laws on my two primary outcome measures. Displayed in Column 1, I find that these permanent repeals do not cause a significant increase in the IMR—the 95% confidence interval for my estimate on interracial

marriage rates is $(-3.56, 2.08)$. The upper bound of this confidence interval is equivalent to 2.08 interracial marriages per 1000 marriages of the full sample IMR, so I rule out effects larger than this magnitude. In Column 2, I report the same results for marital integration and find similar null results, though the smallest effect size I can rule out is 0.21 units of marital integration

In Figure 3, I present the results of an event study analysis using the permanent repeals of anti-miscegenation statutes. These figures show that treatment states' interracial marriage outcomes evolve similarly to control states in the decades preceding the repeal of anti-miscegenation statutes, providing support for one testable implication of the parallel trends assumption. Additionally, the coefficients succeeding the event are all nonsignificant and close to zero, again supporting the claim that anti-miscegenation statutes did not significantly impact the IMR or marital integration.

Additionally, I assess the robustness of this null finding to using new methods that address problems with differential treatment timing—a large concern in my setting, given that permanent repeals are decades apart across many states. Appendix Tables A5 and A6 display these results when using the estimation method introduced in Callaway and Sant'Anna (2021). Again, I find precise null effects on both interracial marriage outcomes.[10] I also demonstrate the robustness of my results to considering different comparison groups and parallel trend assumptions. Marcus and Sant'Anna (2021) highlight the importance of being precise about the comparison group used in difference-in-difference and event study models, as they have implications for the parallel trend assumption invoked. I use the Callaway and Sant'Anna (2021) estimator with two different comparison groups—never treated states (those that never implemented an anti-miscegenation statute and thus did not have a permanent repeal) and not yet treated states (those that will have an anti-miscegenation statute repeal but have not yet—implying they likely currently have one in effect). These comparison groups yield similar results. I also estimate the event study specifications in the Callaway and Sant'Anna (2021) setup and find similar results—if anything, these suggest a reduction in the IMR and marital integration following the permanent repeal of anti-miscegenation statutes (See Appendix Figures A7a and A7b).

Another possibility is that my results would be downward biased because I examine changes in the stock of marriages rather than the flow. However, in Appendix Table A2 and Appendix Figure A6, I replicate these results using interracial marriage rates for 16-30-year-olds, who most likely married in the past 10 years (since the enumeration of the last Census), to proxy for new marriages and find similar results. I also run similar sample restrictions with nonmigrants in Censuses after 1940 (when migration status began to be recorded) to address concerns of Black migration in response to antimiscegenation laws (Gevrek, 2014). However, I find null effects in this subsample. Finally, another concern is that over the sample period, marriage rates in

[10]These regressions restrict to a balanced panel, and results are similar in a naive difference-in-differences with the same set of state-year observations.

the US have declined, in some cases differentially across demographic groups, with Black adults in particular experiencing larger declines. Because my outcome is defined conditional on marriage, it is possible that the unconditional interracial marriage rate may differ when examined as an outcome. I define an alternative outcome of an unconditional interracial marriage rate, and these results, displayed in Appendix Table A2 and Appendix Figure A6, are also null. Finally, one could imagine that the two eras of legislative change around antimiscegenation laws (the late 19th century and the mid 20th century) might have substantively different impacts on interracial marriage rates. I address this possibility directly by estimating using only early (defined as 1851-1887) and late (1948-1967) anti-miscegenation statute repeals (Appendix Tables A3 A4. Again, results on both interracial marriage outcomes are insignificant, and in fact, the late repeals seem to have decreased interracial marriage, if anything.

In sum, I find evidence that the permanent repeals of state-level anti-miscegenation statutes did not have a significant effect on either the IMR or the marital integration of those states. This might seem implausible—if interracial marriages were previously prohibited, how can it be that their allowance does not increase the rate of interracial marriage? I find that there are many interracial marriages recorded in states where it is not legal at the time of the Census (See Appendix Figure A4). This suggests that these laws were not especially salient in discouraging interracial couples. Alternatively, another interpretation of the finding is that legal barriers were not the binding constraint keeping people from interracial marriages—who they met, whether they had similar socioeconomic status, and whether they were considered an eligible partner given racial preferences and racism may have kept the interracial marriage rate low even in the absence of legal barriers.

5.2 Great Migration

The Great Migration was a mass movement of millions of African Americans who fled the restrictive racial hierarchies and lack of economic opportunities in the South. When they moved to their new communities, they provide an opportunity to see how marriage markets respond to an influx of racial minorities—does the IMR and marital integration of these communities increase in response?

Figure 4 displays the relationship between commuting-zone-level Black population change from 1940-1970 and three interracial marriage outcomes measured in 1990. Panel A displays the relationship between ventile of Black population change and the observed IMR. There is a linear and positive relationship between the two outcomes—it appears that higher Black population change is associated with higher rates of interracial marriage. Panel B displays the relationship between ventile of Black population change and marital integration—which deflates the observed IMR by the random IMR. Strikingly, it appears that this outcome

is negatively associated with the ventile of Black population change. Panel C offers a potential explanation, showing that the random IMR is increasing in ventile of Black population change—and that the slope of this relationship is much higher than that observed in Panel A. As a result, while the observed IMR increases in Black population change, the random rate increases at a greater magnitude, and thus the marital integration of those communities that had higher Black population change is lower.

However, this variation is not necessarily exogenous—there may be factors that would bias the relationship between Black population change and 1990 interracial marriage outcomes. As a result, I use the shift share approach introduced in Derenoncourt (2022) to instrument for Black population change. Table 2 displays the first-stage relationship between the predicted Black population change and actual Black population change—there is a strong relationship between the two when controlling for region dummies and Black Southern Migration from 1935-1940 (F-statistic=43.56).[11]

Then, Tables 4, 5, and 6 display the 2SLS relationship between instrumented Black population change and interracial marriage outcomes across four decades (1970-2000). Each column reports results for a different Census year. Table 4 displays results for the observed IMRs. There is a positive and significant effect on IMRs across the four decades. In 2000, a 20-percentile increase in Black population change caused an additional 2.64 interracial marriages per 1000 marriages. Table 5 displays the effects of Black population change on the IMR expected under random assignment. Again, there is a significant positive effect across decades. However, the magnitude of this change is much larger than that of the observed increase in interracial marriage. For example, in 1990, a 1-percentile increase in Black population changes causes an increase in random IMR that is 27 times larger than the increase in observed IMR. In terms of the standard deviations of both variables, this increase is still 70% larger. Table 6 displays the effects on marital integration—there is a significant negative relationship between Great Migration induced Black population change and the marital integration of those communities, corresponding to the discrepancy in magnitudes above. In 1990, a 20-percentile increase in Black population change caused a 0.31 point decrease in marital integration.

Overall, these results reinforce that the observed patterns in Figure 4 correspond to significant causal effects on interracial marriage outcomes when instrumenting Black population change. I find that Great Migration induced Black population change increased both the observed and random IMRs, but decreased the marital integration of the communities that received these migrants. Additionally, I find that these effects are substantive and significant from 1970-2000. It should be noted that it is not necessarily the migrants themselves who are marrying in the "extra" interracial marriages formed—indeed, due to the timing of the Great Migration and when my interracial marriage outcomes are measured, it seems unlikely that this is the

[11] My first stage is stronger than that reported in Derenoncourt (2022) because I omit two controls that she includes in her specifications—education upward mobility in 1940 and the share of the labor force that is employed in manufacturing. In her setting, these controls are relevant for the primary outcome of upward mobility.

case. Instead, the effect could be driven by the children of these new migrants or less measurable changes in racial attitudes and social norms that result from Black population increases. For example, Calderon et al. (2022) find that the Great Migration increased support for Civil Rights and the Democratic Party in destination cities—they also find improvements' in racial attitudes among whites, which may have increased the probability of entering an interracial relationship.

Recent work by Goldsmith-Pinkham et al. (2020) formulates shift-share designs as a "pooled exposure design" in which the "shares measure differential exogenous exposure to shocks", which in my case are the shares of Black southern migrants living in a southern county in 1935 that report residing in a northern commuting zone in 1940. Following Goldsmith-Pinkham et al. (2020), I decompose my instrument into Rotemberg weights to assess what variation in the data is driving the estimates. Tables A22, A23 detail summary statistics about the Rotemberg weights. A majority of the weights (99%, Table A22) are positive and the weights are highly correlated with predicted migration flows from southern counties. The correlation between Rotemberg weights and predicted migration flows is 0.793 (Table A23) which means that the migration flows predicted by southern "push-factors" explain about 63% of the variation in the weights. Conversely, the weights are weakly correlated with variation in historical migration shares ($Var(z_k)$) with a low correlation coefficient of 0.158.

5.3 Segregation

One factor that may impact the IMR is the opportunity that people of different races have to interact. Residential segregation could restrict this opportunity, and has been decreasing over the past 50 years (Chyn et al., 2022). Beyond segregation in people's residences, residential segregation is also closely related with the geographic location (and segregation) of spaces like churches, schools, and social locations—where one might meet their partner. As US residential segregation has declined dramatically in the past 50 years—from a dissimilarity index of 0.73 in 1950 to 0.49 in 2000—it is possible that this shift may have affected interracial marriage rates (see Figure A5). I investigate whether the large increase in IMRs might be related to the decrease in residential segregation using plausibly exogenous cross-sectional variation.

There are many factors that might bias the relationship between residential segregation and interracial marriage, so I use the strategy introduced in Ananat (2011) to isolate exogenous variation in residential segregation using the placement of railroad tracks. Table 3 displays the first stage relationships between the RDI instrument and the dissimilarity index (a measure of residential segregation) from 1970-2000. This relationship is strongly positive and statistically significant, with similar magnitude across the four decades. The 1990 first-stage F-statistic is 24.5.[12] Additionally, Appendix Table A7 reports these first stage results

[12]The segregation data comes from Cutler et al. (1999) and this data is only available for some of the 121 cities used by

with an additional control for track length, and these results are very similar.

Table 7 displays the instrumented 2SLS relationship between the residential dissimilarity index and the observed IMR from 1970-2000. Each column reports results from a different regression equation. I find that residential segregation causes lower IMRs in 1990 and 2000. In 2000, a standard deviation increase in the dissimilarity index caused 3.47 less interracial marriages per 1000 marriages. Table 8 displays the effects of residential segregation on marital integration. I find that increased residential segregation decreases the marital integration of that community in 1980-2000, and that this effect grows over time. In 2000, a standard deviation increase in the dissimilarity index causes a 0.61 point decrease in marital integration. Appendix Tables A8 and A9 display these results when controlling for track length, and they are very similar. A back of the envelope calculation suggests that in a counterfactual perfectly integrated world, the black-white interracial marriage rate in the 2000 Census for my sample of 95 non-Southern cities would be 30 per 1000 black and white marriages, as opposed to the 18 per 1000 that is observed.

Thus, I find that residential segregation does have a negative causal effect on interracial marriage and marital integration using the placement of railroads as an instrument and cross-sectional variation. The national decrease in residential segregation from 1970 onwards may play a role in the large contemporaneous increase in interracial marriage and marital integration. There are several potential mechanisms that might explain this effect. Perhaps the most immediate is that residential segregation determines the degree of interaction between people of different races—where you go to church, walk on your block, etc. Thus, more segregated cities may have lower interracial marriage rates given that Black and white residents do not interact as often. However, there may also be less direct pathways from residential segregation to interracial marriage. Ananat (2011), for example, finds that residential segregation increases racial economic inequality, which may also decrease interracial marriage rates given patterns of assortative matching. Ananat and Washington (2009) finds that segregation decreases Black political efficacy, which may also have downstream impacts on interracial marriage and social integration.

5.4 Assortative Matching

I conduct the assortative matching exercise using a version of the full-count 1900 Census that records first names, crucial for my socioeconomic status proxy (Ruggles et al., 2021). Additionally, I restrict this sample to counties that have more than 200 20-35-year-olds in the marriage market in the full count, yielding a sample size of 1,946 counties. As discussed in the methods, I first construct a proxy for socioeconomic status entering the marriage market using first names, and these links differ by race. Then, I conduct my counterfactual simulation, matching assortatively within county and then measuring the prevalence of

Ananat (2011) in years other than 1990, so the sample size changes by year.

interracial marriage in these pairs.

In this sample, the observed interracial marriage rate is 0.4 per 1000 black and white marriages. The expected rate of interracial marriage under random assignment is 97.0 per 1000 black and white marriages. Finally, my counterfactual simulation suggests a rate of 33.2 interracial marriages per 1000 black and white marriages. This suggests that differences in economic endowments and assortative matching account for a large portion of the "missing" interracial marriages in 1900. Additionally, under the counterfactual simulation, the marital integration is 0.57, relative to 0.02 observed. Both of these results show that interracial marriage would be higher under pure assortative matching, suggesting that endowment differences cannot completely account for the gap between observed interracial marriages and random interracial marriages. However, they also show that endowment differences do play a large role, accounting for approximately 60% of the gap between observed and random interracial marriages.

Figure 5 shows county-level cumulative distributions of the interracial marriage outcomes that are observed and those that are simulated. All three distributions display significant right skew. Observed interracial marriage is very concentrated near 0, with 100% of counties having an interracial marriage rate less than 10 per 1000 Black and white marriages. However, the counterfactual simulation has a greater range of IMRs, with approximately 50% of counties having a counterfactual IMR higher than 10 per 1000 Black and white marriages. Finally, the expected IMR under complete randomization has a much larger range, varying from 0 to nearly 500 per 1000 Black and white marriages. The marital integration measure, which deflates observed and counterfactual IMR by expected IMR, shows similar results.

Overall, this simulation suggests that endowment differences and assortative matching do account for a large portion of the gap between observed and random interracial marriages. However, there is still a significant portion of the gap that remains unexplained by these forces.

6 Conclusion

Between 1950 and 2000, the Black-white IMR in the United States increased from less than half a percent to over 5 percent. Several historical events or patterns—the repeal of anti-miscegenation laws, the Great Migration, declining residential segregation, shrinking Black-white income gaps—may have played a role in this increase. While previous studies have documented this increase or used structural models to study the determinants of interracial marriage, none have evaluated the historical forces impacting the prevalence of interracial marriage and marital integration (Wong, 2003; Fryer, 2007). As a result, I evaluate the roles of these various forces with quasi-experimental research designs and a matching exercise conducted on 150 years of US Census data.

Using a difference-in-differences design, I find the permanent repeals of anti-miscegenation laws did not affect the prevalence of interracial marriage or marital integration. However, I use shift-share and IV approaches to find that the Great Migration increased the IMR but decreased marital integration, while residential segregation decreases both. This suggests that the Great Migration and the national decline in residential segregation have contributed to the large increase in interracial marriage, while an increasingly friendly legal regime did not. My results on the effects of the Great Migration also highlight that while the IMR increased, this increase was an order of magnitude lower than that expected under random assignment—while there was an increase in integration, these migrants were not fully accepted into their destination communities (Derenoncourt, 2022). I also consider the role of assortative matching and endowment differences using a matching exercise and find that these explain approximately 60% percent of the gap between the IMR expected under random assignment and the observed IMR. While there has been significant progress on this measure of social integration in the last 50 years as internal migration and residential desegregation have occurred, my results also serve as a reminder that the United States remains far from achieving equality and integration between racial groups.

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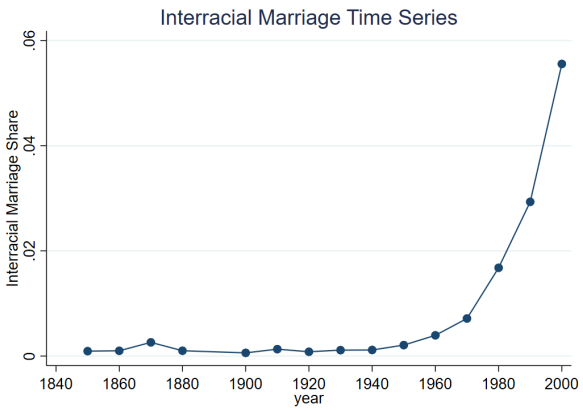
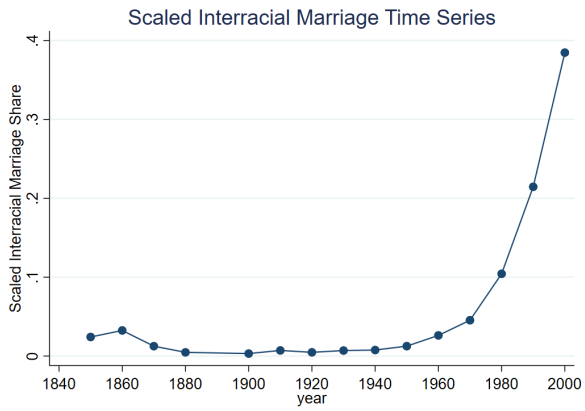
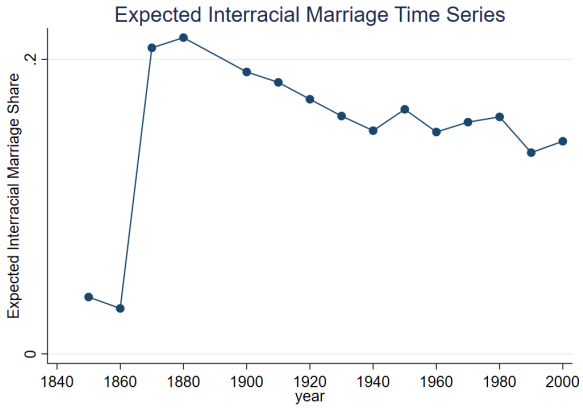
Tables and Figures



(a) Observed Interracial Marriage



(b) Marital Integration

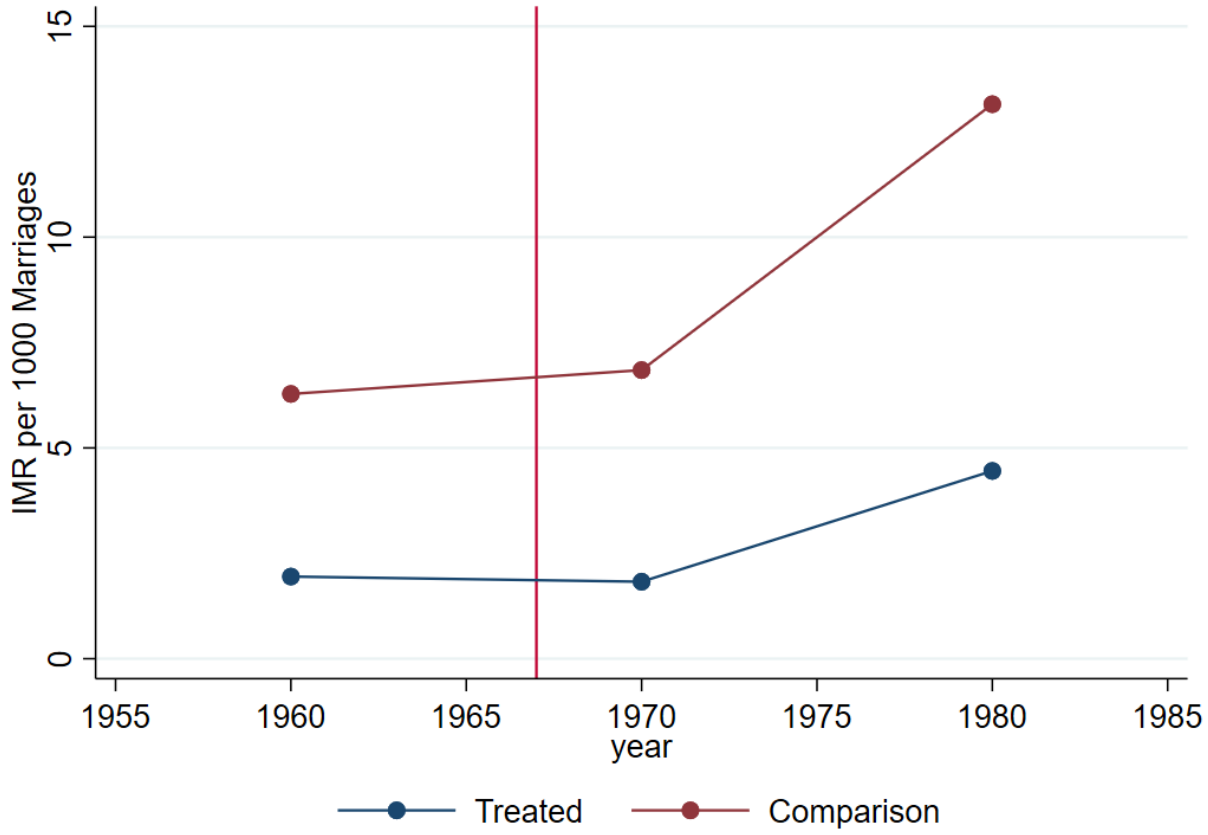


(c) Expected Interracial Marriage

Figure 1: Time Series of Interracial Marriage, Marital Integration, Expected Interracial Marriages

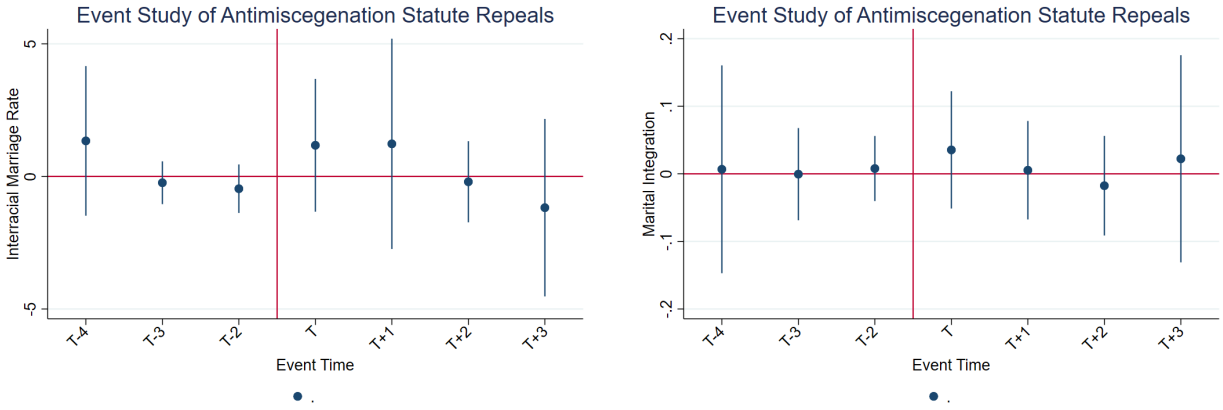
Source: IPUMS-USA Full Counts and Samples, 1850-2000; Authors' calculations. Interracial marriages as a fraction of all Black and white marriages. Marital integration is the observed interracial marriage rate scaled by the expected rate of interracial marriage.

Figure 2: Graphical Illustration of Effects of *Loving V. Virginia*



Source: 1960-1980 IPUMS-USA Samples, Author's calculations. Dependent variable is interracial marriages as a fraction of all Black and white marriages (scaled by 1000). Treated states are those that experienced antimiscegenation statute repeals from 1960-1970 (mostly through *Loving V. Virginia*), while interracial marriage was already legal in comparison states.

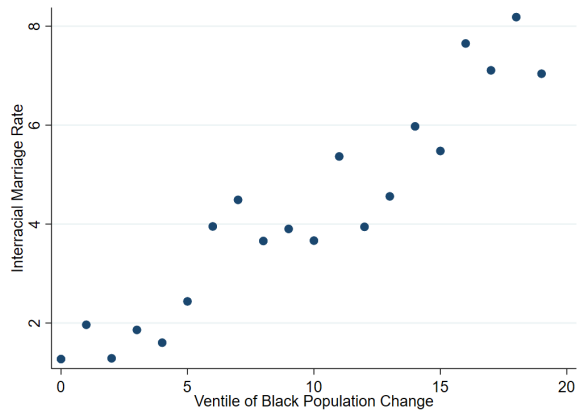
Figure 3: Event Studies of Anti-miscegenation Statute Repeals



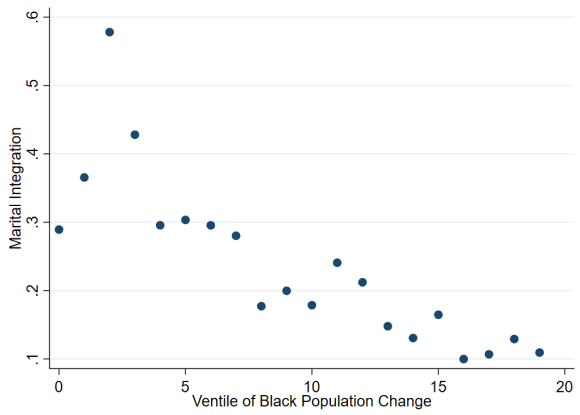
(a) Interracial Marriage Rate

(b) Marital Integration

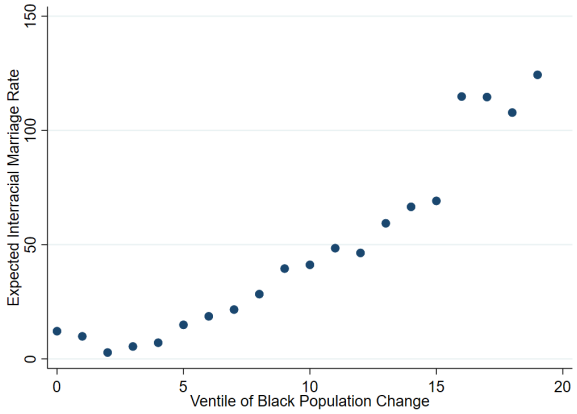
Source: Decennial Census 1850-2000, Author's calculations. Each series is an event study coefficient from a regression on a different outcome. Dependent variable is IMR per 1000 marriages in Panel A and marital integration measure in Panel B. All specifications include state and time fixed effects. Standard errors clustered at the state level were used to calculate 95% confidence intervals.



(a) Observed Interracial Marriage



(b) Marital Integration

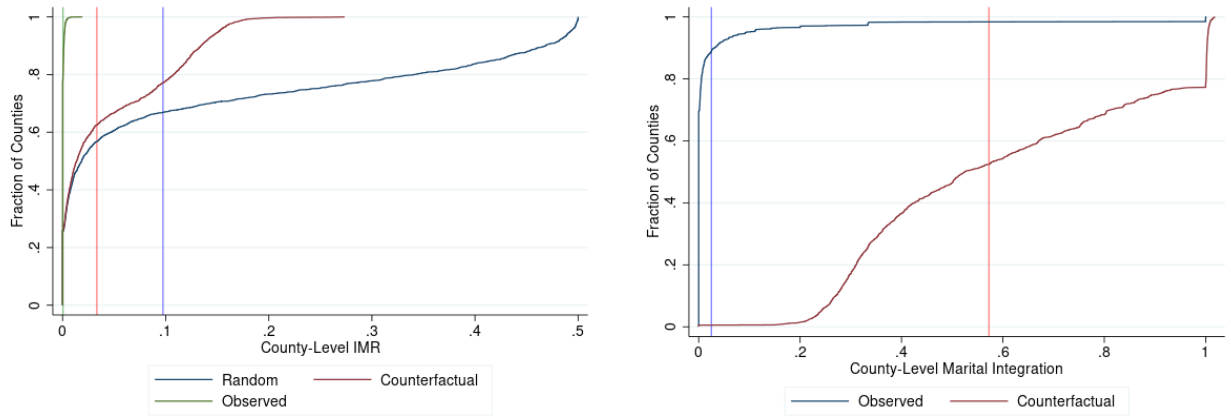


(c) Expected Interracial Marriage

Figure 4: Relationship between Black Population Change and Outcomes

Source: IPUMS-USA 1990 5% Sample; Authors' calculations. These binned scatterplots depict the relationship between interracial marriage outcomes and the percentile of actual Black population increase during the Great Migration (1940-1970) for northern CZs. The unit of observation is a CZ. The right-hand-side variable is grouped into 20 bins (5 percentiles each). Interracial marriages as a fraction of all Black and white marriages, dependent variable is IMR per 1000 marriages. Marital integration is the observed interracial marriage rate scaled by the expected rate of interracial marriage.

Figure 5: Simulated County-Level IMR Cumulative Distributions



(a) Interracial Marriage Rate

(b) Marital Integration

Source: 1900 full count, Author's calculations. Unit of observation is a county. The counterfactual distributions come from an assortative matching simulation of the marriage market among 20-35-year-olds. Dependent variable is IMR per 1000 marriages in Panel A and marital integration measure in Panel B.

Table 1: Effect of Legal Interracial Marriage on IMR

	Interracial Marriage	Marital Integration
Permanent Antimiscegenation Statute Repeal	-0.740	0.0390
	(1.441)	(0.0878)
ymean	5.23	.175
N	711	705

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Decennial Census 1850-2000, Author's calculations. Each entry is a difference-in-differences coefficient on a different outcome. All specifications include state and time fixed effects. Standard errors are clustered at the state level. Column 1 presents the effects on state-level interracial marriage rate per 1000 marriages, μ^o_{st}. Column 2 presents the effects on my state-level measure of marital integration, m_{st}. Six observations are dropped in Column 2 because the expected interracial marriage rate was 0 (as there were no Black people recorded in that state-census combination).

Table 2: First Stage on Black Population Change

	Percentile of Black Population Change
$\hat{GM}$	0.461^{***}
	(0.0699)
Observations	130

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Data from Derenoncourt (2022), Author's calculations. This table reports the first stage relationship (coefficients and heteroskedasticity-robust standard errors) between the percentile of predicted Black population change and the actual Black population change 1940-1970, conditional on 1935-1940 Black Southern migration and region indicators. Unit of observation is a commuting zone.

Table 3: First Stage on Residential Segregation

	1970	1980	1990	2000
RDI Instrument	0.355***	0.420***	0.406***	0.429***
	(0.0918)	(0.0794)	(0.0820)	(0.0956)
Observations	69	87	104	96

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Data from Ananat (2011); Cutler et al. (1999), Author's calculations. This table reports the first stage relationship (coefficients and heteroskedasticity-robust standard errors) between the railroad density instrument and the dissimilarity index segregation measure by decade. Column 1 reports the results for 1970, 2 reports 1980, 3 reports 1990, and Column 4 reports 2000. The unit of observation is non-Southern metro areas for which both segregation and RDI are available.

Table 4: Effect of Great Migration on Observed IMR

	1970	1980	1990	2000
GM	0.0125**	0.0377***	0.0602***	0.132***
	(0.00614)	(0.00852)	(0.0138)	(0.0301)
ymean	1.26	2.43	4.28	14.1
N	130	130	130	130

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Effect of Great Migration on Expected IMR

	1970	1980	1990	2000
GM	1.880***	2.018***	1.633***	1.654***
	(0.248)	(0.234)	(0.176)	(0.173)
ymean	51.7	52.7	47.9	50.1
N	130	130	130	130

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Effect of Great Migration on Marital Integration

	1970	1980	1990	2000
GM	-0.00287***	-0.00339***	-0.00316***	-0.0155***
	(0.00103)	(0.000927)	(0.00116)	(0.00375)
ymean	.0778	.109	.236	.667
N	128	129	130	130

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Data from Derenoncourt (2022) and the following IPUMS-USA samples: 1970 2% metro, 1980 5% state, 1990 5% state, and 2000 5% state. Author's calculations. These tables report the estimated impact of the Great Migration on observed interracial marriage rates, expected interracial marriage rates, and marital integration. The unit of observation is a CZ. The dependent variable in Table 4 is of the observed rate of interracial marriage per 1000 marriages. The dependent variable in Table 5 is the expected rate of interracial marriage per 1000 marriages. The dependent variable in Table 6 is marital integration. Interracial marriages as a fraction of all Black and white marriages. Marital integration is the observed interracial marriage rate scaled by the expected rate of interracial marriage. The independent variable is the percentile of Black population increase during the Great Migration. The instrument for Black population increase is the percentile of predicted Black population increase, defined as the interaction between pre-1940 Black southern migration patterns and post-1940 outflows of migrants as predicted by southern economic factors alone. Baseline 1940 controls include the share of the urban population made up of 1935–1940 Black southern migrants and census region fixed effects. Standard errors are in parentheses.

Table 7: Effect of Segregation on Observed IMR

	1970	1980	1990	2000
Dissimilarity Index	1.196	-4.539	-8.425*	-24.82**
	(3.132)	(4.626)	(5.039)	(11.73)
ymean	1.49	3.45	5.46	18
N	48	80	104	95

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Effect of Segregation on Marital Integration

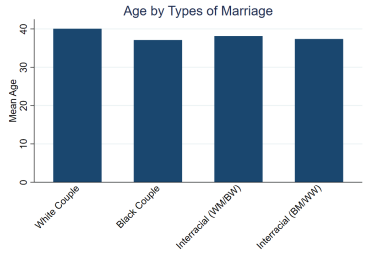
	1970	1980	1990	2000
Dissimilarity Index	-0.0409	-0.487***	-0.933***	-4.420***
	(0.0749)	(0.158)	(0.333)	(1.429)
ymean	.0306	.0762	.16	.567
N	48	80	104	95

Standard errors in parentheses

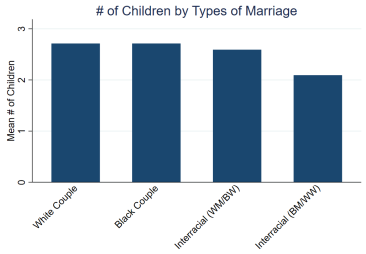
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Data from Ananat (2011); Cutler et al. (1999) and the following IPUMS-USA samples: 1970 2% metro, 1980 5% state, 1990 5% state, and 2000 5% state. Author's calculations. These tables present point estimates and heteroskedasticity-robust standard errors (in parentheses) from regression models in which the key independent variable is the dissimilarity index in that Census year, instrumented by the RDI variable. In Table 7, the dependent variable is the observed interracial marriage rate per 1000 marriages in that Census year, and in Table 8, the dependent variable is the marital integration in that Census year. Sample contains those non-Southern metro areas which can be matched to the Census MSA codes and are present in the Ananat (2011); Cutler et al. (1999) data.

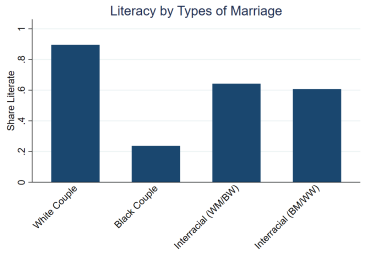
Appendix



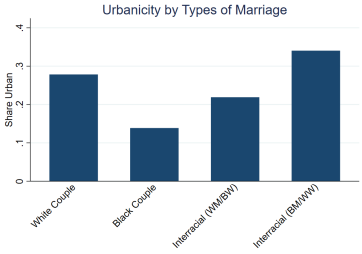
(a) Age



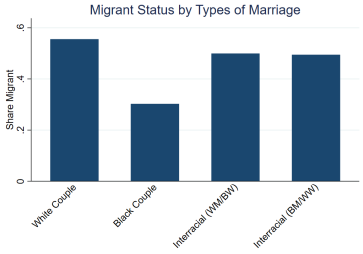
(b) Number of Children



(c) Literacy



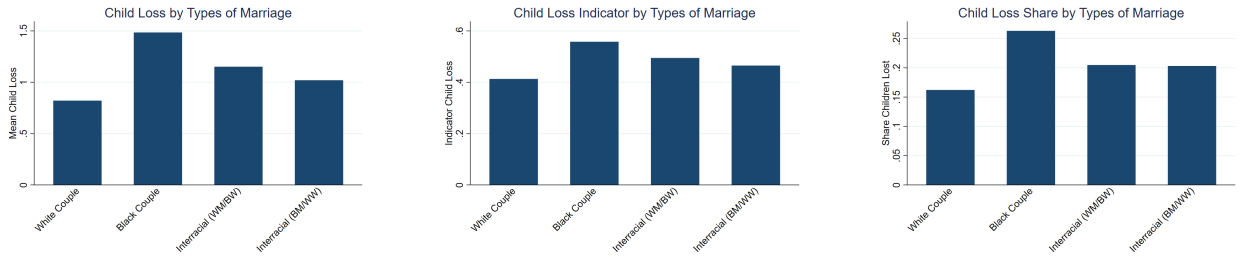
(d) Urban Status



(e) Migrant Status

Figure A1: Demographic Differences in 1880

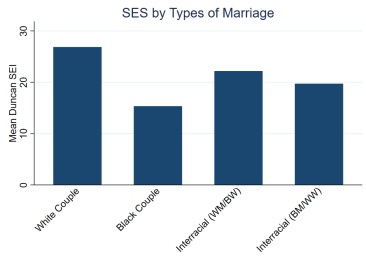
Source: IPUMS-USA Full Count 1880 Census; Authors' calculations. Migrant status is defined by discordance between birthplace and place of enumeration. Number of children measures the number of own children in the household, which may not capture children who have moved out.



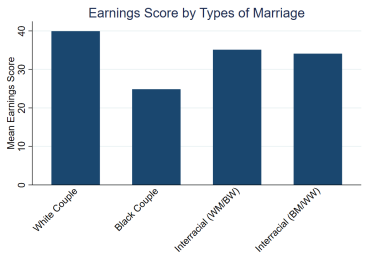
(a) Child Loss Count (b) Child Loss Indicator (c) Share of Children Lost

Figure A2: Child Loss Differences in 1910

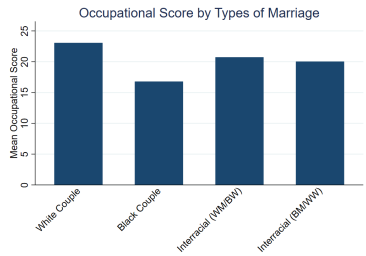
Source: IPUMS-USA Full Count 1910 Census; Authors' calculations. Child loss count is defined as the difference between children ever born and children surviving. The indicator measures whether any difference between these two values exists. The share divides the child loss count by the number of children ever born. These variables are only measured in the 1900 and 1910 census.



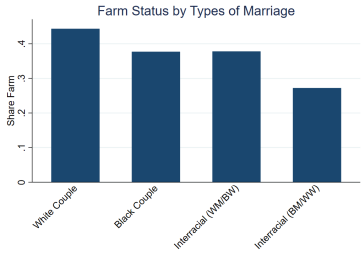
(a) Duncan Socioeconomic Index



(b) Occupational Earnings Score



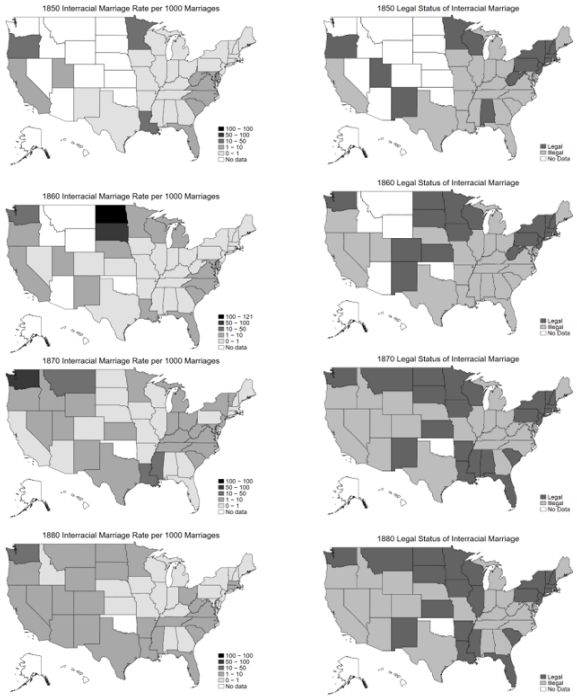
(c) Occupational Income Score



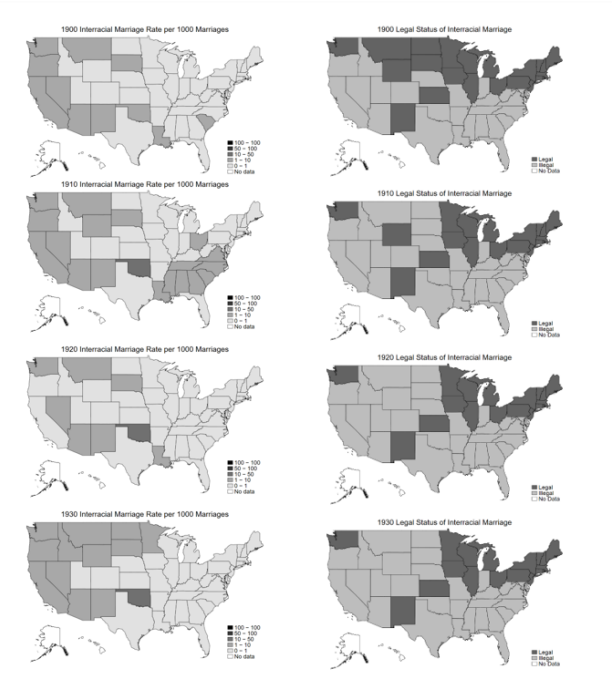
(d) Farm Status

Figure A3: Economic Differences in 1880

Source: IPUMS-USA Full Count 1880 Census; Authors' calculations. Earnings Score assigns a measure of the median earned income for each occupation using the 1950 occupational classification scheme. Occupational Score assigns each occupation in all years a value representing the median total income (in hundreds of 1950 dollars) of all persons with that particular occupation in 1950. assigns a Duncan Socioeconomic Index (SEI) score to each occupation using the 1950 occupational classification scheme. The SEI is a measure of occupational status based on the income level and educational attainment associated with each occupation in 1950.



(a) 1850-1880



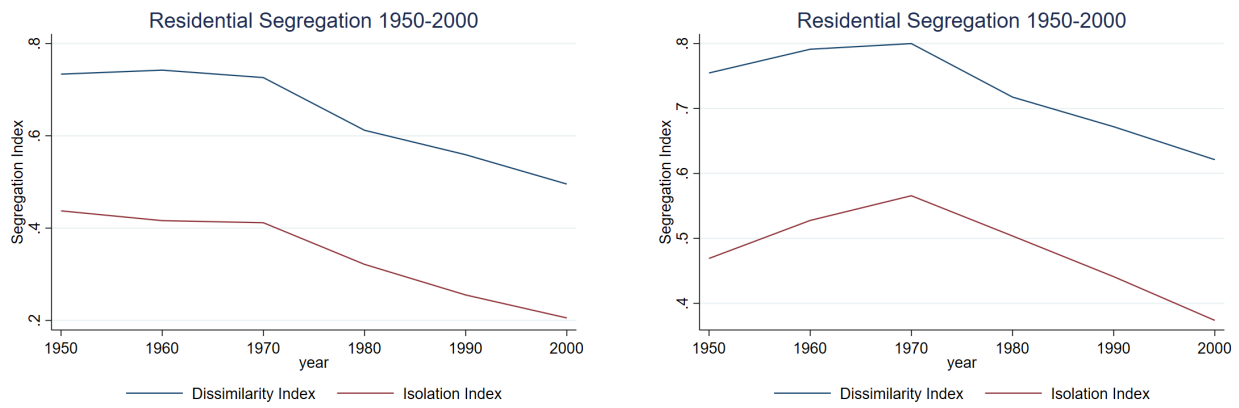
(b) 1900-1930

(c) 1940-1970

(d) 1980-2000

Figure A4: Interracial Marriage Rate and Legal Status by State

Source: IPUMS-USA Full Counts and Samples, 1850-2000; Authors' calculations. Interracial marriages as a fraction of all marriages.



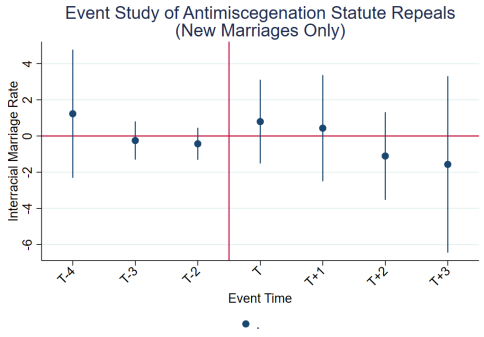
(a) Full Sample

(b) Consistent Panel 1950-2000

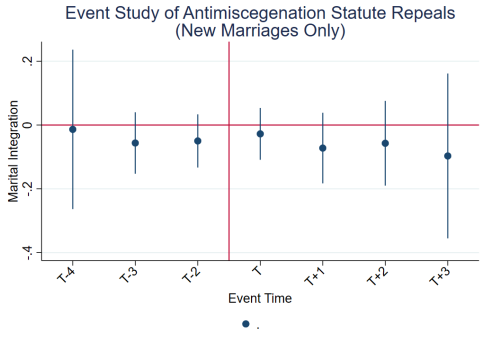
Figure A5: Time Series of Residential Segregation, 1950-2000

Source: Cutler et al. (1999); Authors' calculations. Dissimilarity index measures the percentage of a group's population that would have to change residence for each neighborhood to have the same percentage of that group as the metropolitan area overall. Isolation index measures minority-weighted average of the minority proportion in each area.

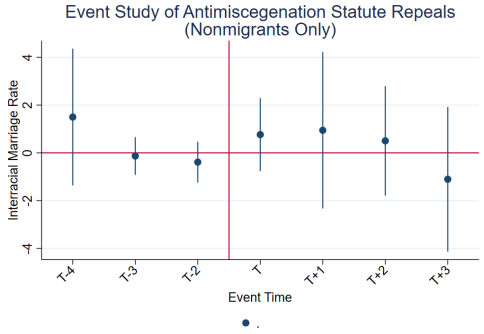
Figure A6: Event Studies of Anti-miscegenation Statute Repeals (Samples)



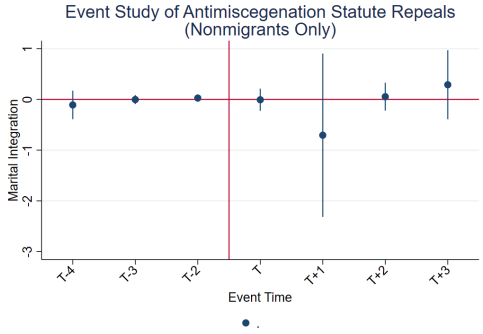
(a) Interracial Marriage Rate



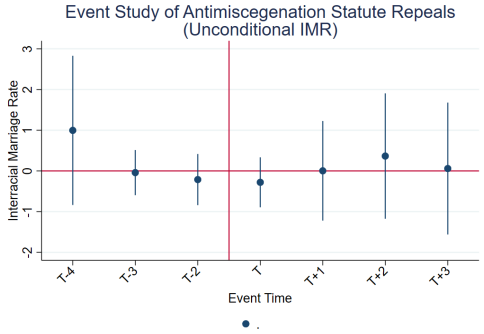
(b) Marital Integration



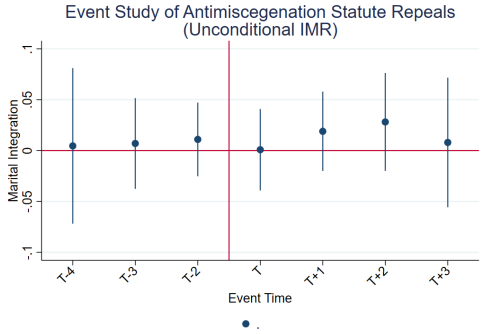
(c) Interracial Marriage Rate



(d) Marital Integration



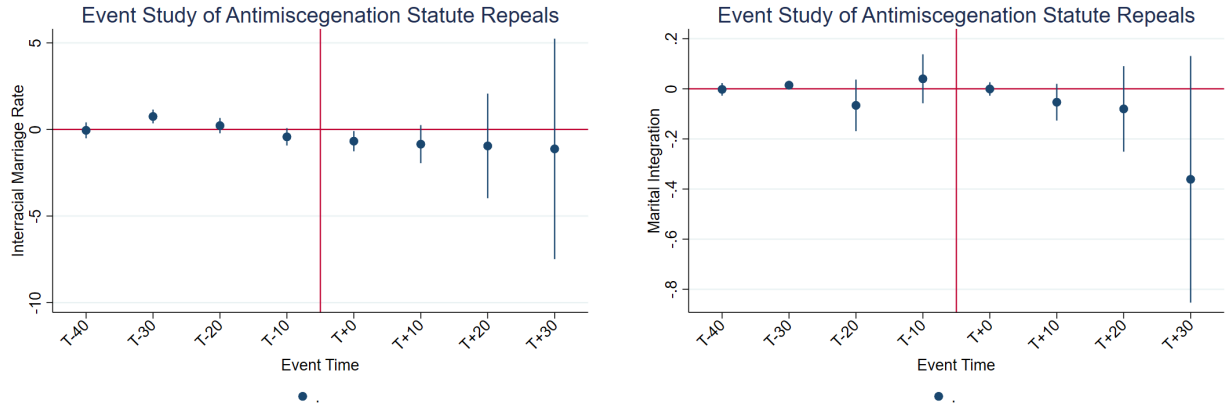
(e) Interracial Marriage Rate



(f) Marital Integration

Source: Decennial Census 1850-2000, Author's calculations. In Panels (a) and (b), sample is restricted to 16-30-year-olds to approximate the sample of new marriages since the last Census. In Panels (c) and (d), sample is restricted to nonmigrants in all samples after 1940. In Panels (e) and (f), the interracial marriage outcomes are unconditional on marriage. Each series is an event study coefficient from a regression on a different outcome. Dependent variable is of IMR per 1000 marriages in Panels a, c, e and marital integration in Panels b, d, f. All specifications include state and time fixed effects. Standard errors clustered at the state level were used to calculate 95% confidence intervals.

Figure A7: Callaway & Sant'anna (2021) Event Studies of Anti-miscegenation Statute Repeals



(a) Interracial Marriage Rate

(b) Marital Integration

Source: Decennial Census 1850-2000, Author's calculations. Each series is an event study coefficient from a regression on a different outcome, estimated using the methods outlined in Callaway and Sant'Anna (2021). Dependent variable is IMR per 1000 marriages in Panel A and marital integration measure in Panel B. All specifications include state and time fixed effects. Standard errors clustered at the state level were used to calculate 95% confidence intervals.

Table A1: Predictiveness of County-Level IMR for County-Level Mixed-Race Ethnicity

	1880 Mixed Race	1880 Mixed Race	1990 Mixed Race	1990 Mixed Race
IMR	2.843***	1.643***	1.376***	1.361***
	(0.200)	(0.102)	(0.0670)	(0.0672)
Share Black		0.115***		0.0245**
		(0.00134)		(0.0108)
ymean	.0201	.0201	.036	.036
N	2538	2538	1726	1726

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Decennial Census 1880 and 1990, Author's calculations. Unit of observation is a county. Outcome variable is fraction of county residents that identify as mixed-race. Columns 1 and 3 present bivariate regressions, while columns 2 and 4 control for share black at the county level.

Table A2: Effect of Legal Interracial Marriage on IMR (Sample Restrictions)

	New Marriages		Nonmigrants		Unconditional	
	IMR	Integration	IMR	Integration	IMR	Integration
Permanent Repeal	-1.095	-0.0144	-0.614	0.0763	-0.157	0.0286
	(2.225)	(0.142)	(1.290)	(0.106)	(0.748)	(0.0298)
ymean	7.53	.201	5.49	.301	2.53	.0622
N	711	702	711	704	711	711

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Decennial Census 1850-2000, Author's calculations. In Columns 1 and 2, sample is restricted to 16-30-year-olds to approximate the sample of new marriages since the last Census. In Columns 3 and 4, sample is restricted to nonmigrants in all samples after 1940. In Columns 5 and 6, the interracial marriage outcomes are unconditional on marriage. Each entry is a difference-in-differences coefficient on a different outcome. All specifications include state and time fixed effects. Standard errors are clustered at the state level. Columns 1, 3, and 5 present the effects on the state-level interracial marriage rate per 1000 marriages, μ^o_{st}. Columns 2, 4, and 6 present the effects on my state-level measure of marital integration, m_{st}.

Table A3: Effect of Legal Interracial Marriage on IMR (Early Repeals)

	Interracial Marriage	Marital Integration
Permanent Antimiscegenation Statute Repeal	0.226	0.0112
	(0.867)	(0.0783)
ymean	6.9	.208
N	278	278

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Effect of Legal Interracial Marriage on IMR (Late Repeals)

	Interracial Marriage	Marital Integration
Permanent Antimiscegenation Statute Repeal	-3.128	-0.0208
	(2.334)	(0.118)
ymean	5.8	.191
N	591	585

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Decennial Census 1850-2000, Author's calculations. In Columns 1 and 2, sample is restricted to 16-30-year-olds to approximate the sample of new marriages since the last Census. Each entry is a difference-in-differences coefficient on a different outcome. All specifications include state and time fixed effects. Standard errors are clustered at the state level. Column 1 presents the effects on the state-level interracial marriage rate per 1000 marriages, μ^o_{st}. Column 2 presents the effects on my state-level measure of marital integration, m_{st}.

Table A5: Effect of Legal Interracial Marriage on IMR (Callaway and Sant'anna Never Treated)

	Interracial Marriage	Marital Integration
ATT	-0.173	-0.0862
	(1.365)	(0.0844)
Observations	540	540

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Decennial Census 1850-2000, Author's calculations. Each entry is a difference-in-differences coefficient on a different outcome calculated using the specification from Callaway and Sant'Anna (2021). All specifications include state and time fixed effects. Standard errors are clustered at the state level. Column 1 presents the effects on the state-level interracial marriage rate per 1000 marriages, μ^o_{st}. Column 2 presents the effects on my state-level measure of marital integration, m_{st}. For these regressions, I restricted to a balanced panel of 36 states that are recorded in every Census from 1850-2000.

Table A6: Effect of Legal Interracial Marriage on IMR (Callaway and Sant'anna Not Yet Treated)

	Interracial Marriage	Marital Integration
ATT	-0.0918	-0.0841
	(1.370)	(0.0839)
Observations	540	540

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Decennial Census 1850-2000, Author's calculations. Each entry is a difference-in-differences coefficient on a different outcome calculated using the specification from Callaway and Sant'Anna (2021). All specifications include state and time fixed effects. Standard errors are clustered at the state level. Column 1 presents the effects on the state-level interracial marriage rate per 1000 marriages, μ^o_{st}. Column 2 presents the effects on my state-level measure of marital integration, m_{st}. For these regressions, I restricted to a balanced panel of 36 states that are recorded in every Census from 1850-2000.

Table A7: First Stage on Residential Segregation (+Track Length)

	1970	1980	1990	2000
RDI Instrument	0.343***	0.386***	0.366***	0.386***
	(0.0922)	(0.0793)	(0.0832)	(0.0962)
Track Length	5.875**	14.21***	17.84*	19.08*
	(2.541)	(5.322)	(10.31)	(11.16)
Observations	69	87	104	96

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Data from Ananat (2011); Cutler et al. (1999), Author's calculations. This table reports the first stage relationship (coefficients and heteroskedasticity-robust standard errors) between the railroad density instrument and the dissimilarity index segregation measure by decade, controlling for railroad track length. Column 1 reports the results for 1970, 2 reports 1980, 3 reports 1990, and Column 4 reports 2000. The unit of observation is non-Southern metro areas for which both segregation and RDI are available.

Table A8: Effect of Segregation on Observed IMR (+Track Length)

	1970	1980	1990	2000
Dissimilarity Index	1.026	-5.913	-9.568*	-25.76**
	(3.863)	(5.070)	(5.645)	(13.02)
ymean	1.49	3.45	5.46	18
N	48	80	104	95

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9: Effect of Segregation on Marital Integration (+Track Length)

	1970	1980	1990	2000
Dissimilarity Index	0.0408	-0.490***	-0.957**	-4.605***
	(0.124)	(0.168)	(0.381)	(1.635)
ymean	.0306	.0762	.16	.567
N	48	80	104	95

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Data from Ananat (2011); Cutler et al. (1999) and the following IPUMS-USA samples: 1970 2% metro, 1980 5% state, 1990 5% state, and 2000 5% state. Author's calculations. These tables present point estimates and heteroskedasticity-robust standard errors (in parentheses) from regression models in which the key independent variable is the dissimilarity index in that Census year, instrumented by the RDI variable. These tables also include a control for railroad track length. In Table 7, the dependent variable is the observed interracial marriage rate per 1000 marriages in that Census year, and in Table 8, the dependent variable is the marital integration in that Census year. Sample contains those non-Southern metro areas which can be matched to the Census MSA codes and are present in the Ananat (2011); Cutler et al. (1999) data.

Table A10: Placebo Test of Segregation on 1930 IMR

	IMR	IMR	Integration	Integration
RDI Instrument	-0.255	-0.243	-0.0149	0.00261
	(0.252)	(0.252)	(0.0322)	(0.0410)
Track Length		-5.739		-8.079
		(6.034)		(6.274)
ymean	.346	.346	.0344	.0344
N	113	113	113	113

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Source: Data from Ananat (2011) and 1930 Full-Count Census. Author's calculations. This table presents point estimates and heteroskedasticity-robust standard errors (in parentheses) from regression models in which the key independent variable is the RDI variable. Columns 2 and 4 also include a control for historical railroad track length per square kilometer. In Columns 1 and 2, the dependent variable is the observed interracial marriage rate per 1000 marriages in that Census year, and in Columns 3 and 4, the dependent variable is the marital integration in that Census year. Sample contains those non-Southern metro areas which can be matched to the Census city codes in 1940 and are present in the Ananat (2011) data.

Table A11: Placebo Test of Great Migration on 1900-1940 IMR

	1900	1910	1920	1930	1940
$\hat{GM}$	-0.000336	-0.000155	-0.000669	-0.00233	-0.000494
	(0.000907)	(0.000976)	(0.00108)	(0.00210)	(0.00121)
ymean	.377	.564	.321	.254	.294
N	130	130	130	130	130

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Data from Derenoncourt (2022) and 1900-1940 Full-Count Censuses. Author's calculations. This table reports the estimated impact of the Great Migration on observed interracial marriage rates in 1900-1940 (a placebo outcome). The unit of observation is a CZ. The dependent variable is the observed rate of interracial marriage per 1000 marriages. Interracial marriages as a fraction of all Black and white marriages. The independent variable is the percentile of Black population increase during the Great Migration. The instrument for Black population increase is the percentile of predicted Black population increase, defined as the interaction between pre-1940 Black southern migration patterns and post-1940 outflows of migrants as predicted by southern economic factors alone. Baseline 1940 controls include the share of the urban population made up of 1935–1940 Black southern migrants and census region fixed effects. Standard errors are in parentheses.

Table A12: Reduced Form of RDI on Observed IMR

	1970	1980	1990	2000
RDI Instrument	0.885	-0.315	-3.419	-10.70*
	(1.022)	(1.773)	(2.082)	(5.557)
Observations	50	88	104	95

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A13: Reduced Form of RDI on Marital Integration

	1970	1980	1990	2000
RDI Instrument	0.000744	-0.155**	-0.379***	-1.905***
	(0.0269)	(0.0767)	(0.116)	(0.543)
Observations	50	88	104	95

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Data from Ananat (2011) and the following IPUMS-USA samples: 1970 2% metro, 1980 5% state, 1990 5% state, and 2000 5% state. Author's calculations. These tables present point estimates and heteroskedasticity-robust standard errors (in parentheses) from regression models in which the key independent variable is the RDI variable. In Table A12, the dependent variable is the observed interracial marriage rate per 1000 marriages in that Census year, and in Table A13, the dependent variable is the marital integration in that Census year. Sample contains those non-Southern metro areas which can be matched to the Census MSA codes and are present in the Ananat (2011); Cutler et al. (1999) data.

Table A14: OLS of Segregation on Observed IMR

	1970	1980	1990	2000
Dissimilarity Index	0.895	-1.205	-2.364	-21.88***
	(1.141)	(1.765)	(2.130)	(3.924)
Observations	48	80	104	95

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A15: OLS of Segregation on Marital Integration

	1970	1980	1990	2000
Dissimilarity Index	-0.0802*	-0.329***	-0.555***	-2.825***
	(0.0465)	(0.0826)	(0.0969)	(0.520)
Observations	48	80	104	95

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Data from Cutler et al. (1999); Ananat (2011) and the following IPUMS-USA samples: 1970 2% metro, 1980 5% state, 1990 5% state, and 2000 5% state. Author's calculations. These tables present point estimates and heteroskedasticity-robust standard errors (in parentheses) from regression models in which the key independent variable is the dissimilarity index in that Census year, Seg_c. In Table A14, the dependent variable is the observed interracial marriage rate per 1000 marriagesin that Census year, and in Table A15, the dependent variable is the marital integration in that Census year. Sample contains those non-Southern metro areas which can be matched to the Census MSA codes and are present in the Ananat (2011); Cutler et al. (1999) data.

Table A16: Reduced Form of Predicted Migration on Observed IMR

	1970	1980	1990	2000
$\hat{GM}$	0.00577**	0.0174***	0.0278***	0.0609***
	(0.00269)	(0.00482)	(0.00788)	(0.0216)
Observations	130	130	130	130

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A17: Reduced Form of Predicted Migration on Expected IMR

	1970	1980	1990	2000
$\hat{GM}$	0.868***	0.931***	0.754***	0.763***
	(0.156)	(0.145)	(0.119)	(0.124)
Observations	130	130	130	130

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A18: Reduced Form of Predicted Migration on Marital Integration

	1970	1980	1990	2000
$\hat{GM}$	-0.00129***	-0.00156***	-0.00146***	-0.00714***
	(0.000454)	(0.000469)	(0.000534)	(0.00191)
Observations	128	129	130	130

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Data from Derenoncourt (2022) and the following IPUMS-USA samples: 1970 2% metro, 1980 5% state, 1990 5% state, and 2000 5% state. Author's calculations. These tables report the estimated impact of predicted migration on observed interracial marriage rates, expected interracial marriage rates, and marital integration. The unit of observation is a CZ. The dependent variable in Table A16 is the observed rate of interracial marriage per 1000 marriages. The dependent variable in Table A17 is the expected rate of interracial marriage per 1000 marriages. The dependent variable in Table A18 is marital integration. Interracial marriages as a fraction of all Black and white marriages. Marital integration is the observed interracial marriage rate scaled by the expected rate of interracial marriage. The independent variable is the percentile of predicted Black population increase during the Great Migration, defined as the interaction between pre-1940 Black southern migration patterns and post-1940 outflows of migrants as predicted by southern economic factors alone. Baseline 1940 controls include the share of the urban population made up of 1935–1940 Black southern migrants and census region fixed effects. Standard errors are in parentheses.

Table A19: OLS of Great Migration on Observed IMR

	1970	1980	1990	2000
GM	0.00996***	0.0394***	0.0676***	0.151***
	(0.00366)	(0.00501)	(0.00755)	(0.0219)
Observations	130	130	130	130

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A20: OLS of Great Migration on Expected IMR

	1970	1980	1990	2000
GM	1.299***	1.354***	1.208***	1.254***
	(0.144)	(0.134)	(0.0999)	(0.0978)
Observations	130	130	130	130

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A21: OLS of Great Migration on Marital Integration

	1970	1980	1990	2000
GM	-0.00214***	-0.00286***	-0.00339***	-0.0185***
	(0.000537)	(0.000676)	(0.000726)	(0.00252)
Observations	128	129	130	130

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Data from Derenoncourt (2022) and the following IPUMS-USA samples: 1970 2% metro, 1980 5% state, 1990 5% state, and 2000 5% state. Author's calculations. These tables report the OLS impact of the Great Migration on observed interracial marriage rates, expected interracial marriage rates, and marital integration. The unit of observation is a CZ. The dependent variable in Table A19 is the observed rate of interracial marriage per 1000 marriages. The dependent variable in Table A20 is the expected rate of interracial marriage per 1000 marriages. The dependent variable in Table A21 is marital integration. Interracial marriages as a fraction of all Black and white marriages. Marital integration is the observed interracial marriage rate scaled by the expected rate of interracial marriage. The independent variable is the percentile of Black population increase during the Great Migration. Baseline 1940 controls include the share of the urban population made up of 1935–1940 Black southern migrants and census region fixed effects. Standard errors are in parentheses.

Table A22: Rotemberg Negative and Positive Weights

	Sum	**Mean**	**Share**
Negative	-0.001	-0.000	0.001
Positive	1.001	0.001	0.999

Table A23: Rotemberg Correlations of Predicted Migration Aggregates

	α_k	g_k	β_k	F_k	Var(z_k)
α_k	1				
g_k	0.793	1			
β_k	-0.016	-0.010	1		
F_k	-0.036	-0.066	0.004	1	
Var(z_k)	0.158	-0.075	0.040	0.273	1

Notes: These tables summarize statistics about Rotemberg weights, where k indexes counties, following Goldsmith-Pinkham et al. (2020). Table A22 reports share of positive and negative Rotemberg weights. Table A23 reports correlation between the weights (α_k), predicted migration inflows into commuting zones (g_k), the just identified coefficient estimates (β_k), the first stage F-statistic of the historical settlement patterns of Black southern migrants (F_k), and the variation in the shares of Black southern migrants ($Var(z_k)$) residing in the north in 1940.

Table A24: Year of Permanent Anti-miscegenation Statute Repeals (*indicates none in effect)

State Name	Abbreviation	Year
Alabama	AL	1967
Alaska	AK	*
Arizona	AZ	1962
Arkansas	AR	1967
California	CA	1948
Colorado	CO	1957
Connecticut	CT	*
Delaware	DE	1967
Florida	FL	1967
Georgia	GA	1967
Hawaii	HI	*
Idaho	ID	1959
Illinois	IL	1874
Indiana	IN	1965
Iowa	IA	1851
Kansas	KS	*
Kentucky	KY	1967
Louisiana	LA	1967
Maine	ME	1883
Maryland	MD	1967
Massachusetts	MA	1843
Michigan	MI	1883
Minnesota	MN	*
Mississippi	MS	1967
Missouri	MO	1967
Montana	MT	1953
Nebraska	NE	1963
Nevada	NV	1959
New Hampshire	NH	*
New Jersey	NJ	*
New Mexico	NM	*
New York	NY	*
North Carolina	NC	1967
North Dakota	ND	1955
Ohio	OH	1887
Oklahoma	OK	1967
Oregon	OR	1951
Pennsylvania	PA	1780
Rhode Island	RI	1881
South Carolina	SC	1967
South Dakota	SD	1957
Tennessee	TN	1967
Texas	TX	1967
Utah	UT	1963
Vermont	VT	*
Virginia	VA	1967
Washington	WA	*
West Virginia	WV	1967
Wisconsin	WI	*
Wyoming	WY	1965