The Gendered Impacts of Perceived Skin Tone: Evidence from African American Siblings in 1870–1940

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Abstract

We study differences in economic outcomes by perceived skin tone among African Americans using full-count U.S. decennial census data from the late-19$^{\text{th}}$ and early-20$^{\text{th}}$ centuries. Comparing children coded as "Black" or "Mulatto" by census enumerators and linking these children across population censuses, we first document large gaps in educational attainment and income among African Americans with darker and lighter perceived skin tones. To disentangle the drivers of these gaps, we identify all 36,329 families in which enumerators assigned same-gender siblings different Black/Mulatto classifications. Relative to sisters coded as Mulatto, sisters coded as Black had lower educational attainment, were less likely to marry, and had lower-earning, less-educated husbands. These patterns are consistent with more severe contemporaneous discrimination against African American women with darker perceived skin tones. In contrast, we find similar educational attainment, marital outcomes, and incomes among differently-classified brothers. Men perceived as African Americans of any skin tone faced similar contemporaneous discrimination, consistent with the "one-drop" racial classification rule that grouped together individuals with *any* known Black ancestry. Lower incomes for African American men perceived as having darker skin tone in the general population were driven by differences in opportunities and resources that varied across families, likely reflecting the impacts of historical or family-level discrimination.

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1 Introduction

It is well known that African Americans have faced severe discrimination relative to White Americans throughout U.S. history. This discrimination has led to large racial gaps in economic outcomes.[1] However, despite the importance of colorism and skin tone, less is known about discrimination and gaps in economic outcomes between African Americans perceived as having darker or lighter skin tones, and about the drivers of these gaps.[2] As Monk (2021) concludes: "future research on colorism will be essential to understand comprehensively the significance of race/ethnicity in a demographically shifting United States."

This paper starts by using full-count U.S. decennial census data from the late-19th and early-20th centuries to systematically document large gaps in educational attainment and income between African Americans classified by census enumerators as having lighter and darker skin tones, as well as between African American and White individuals. Next, we focus on the degree to which African American men perceived as having darker skin faced heightened labor market discrimination, and African American women perceived as having darker skin suffered worse marriage prospects.

Inferring the role of contemporaneous discrimination varying by perceived skin tone in outcomes among African Americans is challenging because perceived skin tone is typically correlated with other factors that affect outcomes, including access to opportunities as well as characteristics like education and occupation which can themselves influence perceived race (e.g., Saperstein and Gullickson, 2013). Differences in access to opportunities and resources that vary across families themselves likely reflect systematic, longstanding differences in racism and discrimination. Indeed we find that African American children were more likely to be perceived as lighter skinned in more urbanized and "whiter" counties, as well as in non-farming households, reflecting differences in available resources that may trace back across generations. For example, Bodenhorn (2015) shows that enslaved people with lighter complexions were more likely to have been assigned less onerous tasks that led to the accumulation of skills, were likely to have been freed earlier, and had better access to nutrition and suffered less stunted growth.

To study the causal effect of contemporaneous discrimination, a "controlled experiment" might randomize

[1] Differences in economic outcomes by race have generated a great deal of attention from economic historians, economists, and other social scientists. For studies by economic historians see, for example, Margo (1990); Whatley and Wright (1994); Vedder and Gallaway (1992); Maloney (2002); Boustan (2012); Wright (2013); Althoff and Reichardt (2022); Collins and Wanamaker (2022); Derenoncourt et al. (2022). For studies by other economists see, for example, Myrdal (1944); Card and Krueger (1993); Heckman et al. (2000); Becker (1971); Altonji and Pierret (2001); Carneiro et al. (2005); Neal and Johnson (1996); Bertrand and Mullainathan (2004); List (2004); Chetty et al. (2020). For studies by other social scientists, see, for example Allen and Farley (1986); Smith (1997); Hunt (2007); McConnell and Leibold (2001); Vera and Feagin (2007).

[2] We discuss this literature extensively in Section 3.2.

only the perceived skin tone of children at birth and then compare siblings' economic outcomes later in life. While such controlled randomization obviously does not occur, we use a rare historical setting and newly constructed data from 1870–1940—a period during which race was explicitly coded in U.S. population censuses as "Black," "Mulatto," or "White"—to conduct an observational study related to this "controlled experiment."[3] In particular, we compare siblings who vary in their perceived skin tone, reflecting differences in a bundle of physical features, social cues, and in others' interpretation of these features and cues.[4]

To construct our data, we started with all 99 million children classified as Black, Mulatto, or White by U.S. decennial census enumerators in 1870–1920.[5] Enumerators went door-to-door to all households, coding the perceived race of respondents. Enumerators were instructed to consider individuals as Mulatto if they observed "any perceptible trace of African blood" and as Black if the respondent was "evidently full-blooded." We then group these children into families with the same set of parents, and link children to their adult selves in future census years. Constructing these links is a non-trivial task given the lack of Social Security Numbers or other unique identifiers, relying instead on the similarity of names, birth years, and other identifying information (see, for example, Abramitzky et al., 2014, 2021). Women are typically more difficult to link (and are often excluded from linked historical studies) due to their more frequent name changes at marriage. The primary linking method we use (Price et al., 2021) successfully links a large number of women, even across name changes at marriage, by incorporating information from genealogy links and from additional records (including birth, marriage, and death records) that often include maiden or parents' names for women. Because linking across historical censuses is inevitably imperfect, we present results based on a variety of automated and hand-linking methods.

When comparing childhood outcomes of the approximately $54,000$ differently-classified sisters, we find

[3]This paper uses a number of terms (for example the word "Mulatto") that were widespread in the time period we study, but can carry negative connotations. To the extent that any readers find our inclusion of such terminology offensive, we apologize. Fully motivated by good will, we feel our subject is sufficiently important as to justify careful research and discussion. We will often refer to individuals being classified as White, Black, or Mulatto, in line with the classification codes used by census enumerators at the time. We use the term "African American" to designate Americans with known African ancestry, that is inclusive of people categorized as either Black or Mulatto by census enumerators during the period of our study.

[4]Our estimates capture differences by *perceived* skin tone. A large literature spanning sociology, economics, and other related fields (discussed in more detail in Section 3) has robustly demonstrated that perceptions of race respond not only to physical attributes (e.g., skin tone and hair texture) but also to a wide variety of contextual and social cues such as the way a person dresses or the language they speak. In both the general population and among differently-classified siblings, we find that Mulatto-coded children were more likely than Black-coded children to be classified as White adults in future census years (in which the Mulatto category no longer existed) and when self-reporting race on Social Security Number applications. These findings suggests that the bundle of physical and/or social characteristics that differed between Mulatto-coded and Black-coded children persisted even 20 years later and predicted the perceptions of other census enumerators, as well as self-reported race.

[5]Only the population censuses of 1870, 1880, 1910, and 1920 both contain the Black vs. Mulatto distinction and provide data for all people (see Footnote 19 for detail). In most specifications, our sample restricts to children age 3–18 in these four censuses.

that sisters coded as Black were about 2% less likely to attend school and had lower levels of literacy. Linking across censuses, we find that sisters with perceived darker childhood skin tones were 4.8% less likely to be married as adults and, when they did marry, had less educated and likely lower-earning spouses.[6] This suggests that African American women with perceived darker skin tones faced significantly greater levels of discrimination on the marriage market, consistent with a literature showing marriage-market penalties for darker-skinned women due to discriminatory feminine beauty standards (Hill, 2002b; Hamilton et al., 2009).

In contrast, when analyzing around 51,000 differently-classified brothers, we find small and generally statistically insignificant differences in educational, marital, and labor market outcomes between brothers coded as Black and Mulatto. There are, however, large differences for men in the general population: African American sons with lighter perceived skin tones were more literate and completed 0.9 more years of schooling, their incomes were 13.8% higher, and they married spouses with 0.8 more years of schooling on average.[7] These differences are generally reduced by about a third when comparing African Americans who grew up as children in the same area and by another third when controlling for observable family characteristics (such as parents' literacy and income scores, whether the household head is listed as a parent, and whether the household owns their home), themselves likely affected by historical discrimination. When comparing brothers, these differences largely disappear. These results may reflect the dominance of a "one-drop" racial classification rule, under which individuals with *any* known Black ancestry were grouped together. In this historical setting, differences in discrimination due to an individual's perceived skin tone explain less than about a third (34%) of the *Black–Mulatto* income gap observed in the general population, even at the upper bound of a 95% confidence interval.

The majority of income gaps by perceived skin tone among African American men were likely driven by differences in families' access to opportunities and resources, such as differences in geography, educational access, and family wealth, which have been shown to meaningfully affect outcomes (Carruthers and Wanamaker, 2017; Althoff and Reichardt, 2022; Derenoncourt et al., 2022). Since discrimination in

[6]The spouses of Black-coded sisters had 0.6 fewer years of schooling and earned 1.8% less than the spouses of Mulatto-coded sisters on average. Relative to sisters, in the general population we find larger perceived skin tone gaps in educational attainment and similar magnitude gaps in marital outcomes: By adulthood, Black-coded sisters had received 0.3 fewer years of schooling than their Mulatto-coded sisters on average (smaller than the analogous general population gap of 0.7 years), and their 4.8% lower marriage rate is not significantly different from the 3.0% analogous general population gap.

[7]Our analysis also highlights that while *Black–Mulatto* gaps in the general population represent economically and statistically meaningful differences, they are much smaller than analogous *White–Mulatto* differences (2.9 more years of education and 68.8% higher earnings in 1940). In other words, we show that throughout the late 19th and early 20th centuries, the White vs. Mulatto/Black gaps in all outcomes studied were much larger than the Mulatto vs. Black gaps, consistent with the abundant qualitative and quantitative evidence of discrimination against African Americans.

previous generations likely created these differences in access by perceived skin tone in the first place, family differences included the effect of perceived skin tone discrimination suffered by previous generations; this analysis of siblings should thus be understood as measuring the effect of perceived individual skin tone. Given our finding that unequal opportunity and resources were the main drivers of observed outcome gaps for men, reducing outcome gaps may require policies that seek to remedy the underlying inequality rather than simply moderating contemporaneous discrimination. Such policies might include affirmative action or reparations that seek to address the differences in access to opportunities and resources, which themselves have resulted from systematic racism and past discrimination.

While our data do not allow us to fully explain different patterns between brothers and sisters, we discuss possible reasons such as differential parental investment in girls (Landor et al., 2013) and higher skin tone discrimination against women with darker perceived skin tones. Our analysis corroborates the literature on the gendered nature of skin tone dynamics, which suggests that African American women face harsher penalties for darker skin tone than African American men (see Monk, 2014 for a summary). We show that these penalties existed even when controlling for family background differences by making comparisons among sisters.

There are a number of potential concerns with our identification strategy and interpretation of our findings. First, we cannot know with certainty that siblings shared the same biological parents. Our analysis partially addresses this concern by defining two children as siblings only if neither is listed as a stepchild or with an absent parent. Second, perceived skin tone differences between Black-coded and Mulatto-coded siblings could have been less stark than such perceived differences in the population. We are reassured by our finding in Section 6.2 that gaps in the likelihood of future classification as White were similar for differently-classified siblings and for the general population, suggesting that this concern is likely not driving our main results. Third, attenuation bias in estimates of the effect of perceived skin tone may be exacerbated by the use of fixed effects in some specifications (Griliches, 1979). We use an empirical specification comparing the weighted populations of same-gender Black vs. Mulatto siblings from differently-classified families, which helps limit this concern and produces estimates similar to specifications including family fixed effects when the latter have sufficient statistical power.

Fourth, we cannot know the precise bundle of unobservable characteristics that differed between children coded as Black and Mulatto, including differences in aspects of a child's physical and social presentation as perceived by census enumerators. For this reason, our paper focuses on the overall effects of *perceived* skin tone rather than of physical skin tone. At some level, the effects of perceived skin tone are more interesting than the effects of physical skin tone, as perceptions are what determine discrimination. We do, however, discuss and analyze issues of presentation to census enumerators and endogenous classification. For example, we show that Black- and Mulatto-coded siblings were similar along other observable characteristics including racially distinctive names, and motivate our focus on comparisons by the perceived skin tone of children, who had not settled into occupations. These results are consistent with physical features (e.g., skin tone) having been a key driver of within-family variation in enumerators' classification as Black or Mulatto. Finally, parents whose children varied substantially in perceived skin tone may have reallocated resources within the family to reinforce or counteract different social attitudes towards their children. To the extent that this occurred, our estimates would still correctly identify the causal effect of perceived individual skin tone within differently-classified families (including the impact of this family allocation effect) but might not extrapolate to the effect in families with uniform classifications. We discuss these concerns in more detail in Section 7. Although our ability to extrapolate the effects of skin tone from within families to the population necessarily relies on assumptions typically required in studies of siblings or twins (e.g., Ashenfelter and Krueger, 1994), we argue that these assumptions are reasonable in our setting.

The remainder of the paper proceeds as follows. Sections 2 and 3 put our paper in the broader historical context and in the context of several related branches of literature on race and economic outcomes, respectively. Section 4 describes our data sources, samples, and the process of assembling the data set. Section 5 lays out our conceptual framework and empirical strategies. Section 6 presents our main results, while Section 7 discusses the interpretation of these results. Section 8 concludes.

2 Historical background on skin tones in a racially segregated society

To help motivate and interpret our estimated gaps in economic outcomes by perceived skin tone classification, we now briefly describe some historical context faced by individuals during our analysis period. For more detailed discussion of segregation and African American economic history, see Boustan (2012), Wright (2013), Bodenhorn (2015), and Boustan (2016) among others.

Segregation

African Americans around the turn of the twentieth century inhabited largely separate public realms due to the White majority's anti-Black racism and reluctance to interact with African Americans in public places. In the South, where 89% of African Americans lived in 1910 (Bureau of the Census, 1913, Ch. 2, Table 14), segregation was set by "Jim Crow" laws pertaining to the separate treatment of African Americans. In the North, segregation was mostly *de facto*, but was nevertheless practiced extensively by private individuals and companies. Surveying legal cases of segregation throughout the U.S. between the abolition of slavery in 1865 and the reintroduction of Jim Crow laws in the South in 1881, Stephenson (1910) summarizes: "In the absence of legislative authority, many of the public conveyance companies had regulations of their own separating the races. The 'Jim Crow' laws [. . .] did scarcely more than to legalize an existing and widespread custom."

White American society actively excluded African Americans from public institutions such as schools, courts, and churches, as well as venues such as restaurants, hotels, and theaters (Stephenson, 1910; Margo, 1990; Carruthers and Wanamaker, 2017; Cook et al., 2023); means of public transportation were regulated so that African Americans had to ride separate cars or occupy separate sections (Stephenson, 1910). At the same time, disenfranchisement excluded African Americans from the political arena (Kousser, 1974; Naidu, 2012). Racial segregation was endemic in markets as well, whether the result of individual or collective action.

In particular, labor markets were largely segregated. Maloney and Whatley (1995) and Foote et al. (2003) describe how the Ford Motor Company exploited discrimination against African Americans by other companies, but channeled its own African American workers to more demanding and dangerous manufacturing jobs. Sundstrom (1994) and Fishback (1984) highlight that even though junior roles were not segregated in some occupations, African Americans were not allowed to supervise White workers. Labor unions were another source of discrimination, as most did not accept African Americans into their ranks.

African Americans faced discrimination in housing markets as well, even if they migrated from the South to the industrialized North. Cutler et al. (1999) find evidence that ". . . variation in the level of segregation in 1940 is due to collective action racism on the part of Whites rather than a desire among Blacks to live in Black areas." White individuals' desire not to share their neighborhoods with African Americans was important in determining the patterns of suburbanization and urban development that occurred in the second half of the twentieth century, even after the Civil Rights Acts of the 1960s (Boustan, 2010).

Intermediate skin tones

Whenever racial segregation emerged, the question of defining distinct racial categories had to arise (Stephenson, 1910). While approximately one-fifth of African Americans were classified as Mulatto in the 1910 census, the true proportion with some European ancestry may have been as high as three-quarters (Cummings and Hill, 1918). Marriages between African American and White individuals were very rare in the early twentieth century (Fryer, 2007), with much of the variation in the degree of European ancestry originating during the era of slavery (Williamson, 1980).[8]

In the late nineteenth and early twentieth centuries, some states considered those with only small proportions of Black ancestry to be White. However, the standard approach to racial classification shifted during the early twentieth century to the "one-drop rule," which grouped together individuals with *any* known Black ancestry (Bodenhorn, 2015). This shift was embodied both in *de facto* norms and through *de jure* political and legal changes; between 1910 and 1930, seventeen states adopted or amended their laws to define African American ancestry using one-drop rule equivalents (Murray, 1997). Thus, even people of African American descent who looked "fully European" were considered African American and were excluded from the White public sphere if their ancestry was known.

Nevertheless, economic differences between lighter- and darker-skinned African Americans date back at least to antebellum times, when free African Americans were more likely to be light skinned, and among free African Americans, those with light skin were on average more educated and richer (Bodenhorn and Ruebeck, 2007; Bodenhorn, 2015). While mixed-race individuals sometimes tried to set themselves apart culturally, this distinction faded somewhat during the first decades of the twentieth century (Davenport, 2020). Some of the most prominent African American leaders such as Booker T. Washington, W. E. B. Du Bois, and Walter Francis White were of mixed ancestry. Horace Mann Bond, another prominent figure of mixed race, noted the shift towards a binary treatment of skin tone and racial identity:

> "Time was when there were blue-vein societies [social clubs admitting only light-skinned African Americans] [. . .] among Negroes in this country, but they seem largely to have disintegrated, owing to two happy chances of fortune: The first has been that those who were so much like the dominant group [. . .] have in great part folded their tents and crept quietly into the ranks of the whites. The other [. . .] has been the unyielding refusal of the dominant group to accept any

[8] Bodenhorn (2006) shows that even among African Americans, individuals were likely to marry other African Americans with relatively similar skin tones.

> of its hybrid progeny, if known as such [. . .]. [The One Drop Rule] has done countless good for the Negro, as it has served to focus his energy and that of all his potential leaders upon the immediate task of racial survival." (Bond, 1931)

The attempt to distinguish between light- and dark-skinned African Americans in the U.S. census dates back to 1850; Hochschild and Powell (2008) study why the Mulatto category was first introduced. All censuses from 1850 to 1920 (except 1900) asked enumerators to distinguish between people of full and mixed African ancestry, despite contemporaneous observers' recognition that such ancestry-based classification was questionable and subjective (Cummings and Hill, 1918, p. 209).

Census enumerators were instructed to classify African Americans according to their appearance, distinguishing "persons who are evidently fullblooded negroes" from those merely "having some proportion or perceptible trace of negro blood" (Gauthier, 2002, p. 48).[9] It appears that enumerators indeed applied an appearance-based (rather than a "blood-based") standard: Table 1 shows that when one parent was classified as Mulatto and the other as Black (so that a "one-drop" blood-based standard would categorize *all* children as Mulatto), between 34% and 53% of a family's children were classified as Black.

Passing as White

In a segregated public sphere, light-skinned African Americans could sometimes "pass for White." As long as nobody identified passers as such, it was the choice of potential passers whether to present themselves as White or Black. Passing could have been very casual if it was partial: every bus ride or play at a theater offered an opportunity to pass temporarily, only later to return to the African American world.

The types of passing more interesting for this paper are professional and complete passing (Myrdal, 1944, ch. 31, sec. 4). Professional passing was the representation of oneself as White to colleagues, supervisors, and clients, for professional purposes. Under segregated arrangements where many White people were unwilling or unable to hire or to trade with African Americans, professional passing opened many doors.[10] However, professional passing was not complete if the person returned at the end of the day to a family or community that did not identify him or her as White. Complete passing required both professional and social passing. In

[9] Complete instructions to census enumerators on racial classification can be found in Appendix B.

[10] The presumed disutility to White individuals of doing business with African Americans was evident in the legal treatment of racial misidentification as defamation *per se* under the category of "words disparaging to a person *in his trade, business, office, or profession*" [emphasis added] (Stephenson, 1910). (Defamation *per se* meant that a White person accused of being African American could sue the accuser for the recovery of damages even without proving actual harm, but simply by proving that the false accusation was made.)

many cases, professional passing was a step on the way to complete passing.

African Americans considering passing needed not only sufficiently light skin and the ability to convincingly “act White,” but also that the potential benefits outweigh a variety of personal costs. For example, Myrdal (1944) quotes an African American who passed for White in college but then reclaimed his African American identity to become a teacher. He mentions four reasons not to pass: the psychological burden of fearing that someone would reveal he was Black, the higher relative social status he could enjoy in the African American community versus the White community, the economic stability he gained from the relative scarcity of skilled African American workers, and his richer social life among the higher ranks of African Americans. Hobbs (2014) offers a rich history of the evolution of these factors, and the decisions faced by African Americans on whether, when, and how to pass.

While census enumerators may have accidentally classified some African Americans as White even if they did not intentionally pass, those who did pass completely were very likely to have been accepted by the enumerator—as by the community—as White. While the distinction between Mulatto and Black had been eliminated from the censuses by 1930, the line between White and African American, however, did not change: individuals who would have been classified as either Mulatto or Black in earlier censuses were presumably later classified as Black unless they passed. For more analysis of the economics of passing, see Bodenhorn and Ruebeck (2003), Mill and Stein (2016), and Dahis et al. (2020).

3 Related literature

3.1 Determinants of perceived race and skin tone

To help interpret our comparisons of differences in outcomes by perceived race and skin tone, we draw insights from a rich literature theorizing about racial categorizations and analyzing the determinants of race and skin tone perceptions.

Debates over the concept of race in the social sciences revolve around two primary theories of race: essentialism and constructivism. Essentialists argue that humans are naturally divided into races that share inherent or immutable defining qualities. This classification assigns different biological and genetic traits to individuals from different ancestries and phenotypes, and typically views race as an intrinsically defined and immutable characteristic (for a discussion, see Morning, 2011 and Sen and Wasow, 2016).

On the contrary, constructivism argues that racial categories are created products of social life. Constructivists point to the lack of scientific basis for racial categories: rather than being based on inherent differences, racial classifications and the relevance assigned to certain traits respond to malleable social structures. These structures reinforce hierarchies that provide socioeconomic advantages to dominant groups (Morning, 2011; Sen and Wasow, 2016; Rose, 2023).

Theories of race are also a source of debate in economics. This is the case for individualist and structuralist approaches to the relationship between race and economic outcomes. Under an individualist approach, differences between races are ascribed to individual behaviors and cultural differences (Francis et al., 2022). Structuralist approaches, such as stratification economics, suggest that intergroup differences in outcomes between dominant and subordinate groups are due to the uneven intergenerational transmission of resources and advantage over time. In this case, which group dominates and which is subordinated arises from differences in power, rather than culture (Darity Jr, 2022; Francis et al., 2022). By comparing differently-classified siblings within families, our paper resonates with this literature in studying the role of differences in the transmission of resources and advantages in explaining intra-group gaps.

Constructivism has challenged how race is often measured in the social sciences. If race is taken as an exogenous characteristic, comparisons of outcomes between racial groups reflect historically accumulated disadvantages that may be misinterpreted as cultural differences. This often ends up presuming inferiority for the underprivileged group (Francis et al., 2022). Sen and Wasow (2016) argue that the use of race as a treatment can prove redundant when measuring differences in characteristics that are already affected by race, such as education or wages. Instead, they propose the understanding of race as a "bundle of sticks," with components ranging from societal values and institutional power relationships to physical attributes and skin tone. Under this setting, race is a fluid concept that can only be partially explained or proxied by each of its components. Among these components, some exhibit a higher degree of stability than others (e.g., skin tone) (Sen and Wasow, 2016).

The understanding of race as a mutable feature suggests that racial categorizations are not determined only by stable components, such as skin tone. As a whole, race is flexible and changes across time and context (Davenport, 2020). However, any understanding of race must account for the beliefs, accurate or not, that shape how race is perceived (Morning, 2011).

These conceptions of race have practical implications for how race is perceived. In the words of Morning (2011), "racial concepts—that is, the way we think about what race is and how racial difference is demarcated—have the potential to shape our evaluations of others and the courses of action we take with respect to them." Research in sociology documents the relationship between stereotypic traits and racial perception. For instance, Penner and Saperstein (2008) suggest that racial perceptions are associated with perceptions of social status. Using longitudinal data, they find that unemployed, incarcerated, and poor individuals are more likely to be classified as Black, conditional on being previously classified as White. Even when taking skin tone into account, social status cues, such as a low-status attire, can lead people to consider a light-skinned person as Black (Freeman et al., 2011). Most closely related to our own time period and sample, Saperstein and Gullickson (2013) analyze U.S. census data from 1870–1920 and show that Black vs. Mulatto racial classification by census enumerators is associated with an individual's occupational status.

Any analysis of perceived skin tone can suffer from a resulting statistical bias, although this is partially mitigated in our data by a focus on enumerators' perceptions of race and skin tone among children. However, we are aware that social and physical cues of other family members likely affected these perceptions. Perceptions of race can also be driven by observer characteristics. Rather than being objective, physical traits can be perceived differently if observers belong to a different group. Using data from the early 1990s, Hill (2002a) finds that White interviewers reported African American respondents' skin tones as being darker than did African American interviewers. African American interviewers show the opposite pattern when interviewing White respondents. This analysis offers empirical support for viewing perceived race and skin tone as flexible characteristics that depend on individual and social contexts.

3.2 Empirical analyses of differences by race and skin tone

Across racial categories

Numerous studies document differences in economic outcomes between African American and White workers in American labor markets.[11] For instance, 2010 data from the Bureau of Labor Statistics show that White men earned 33% higher wages than Black men on average (2011, Table 14).

To identify the role of contemporaneous discrimination in wage gaps, a common approach in observational studies is to control for observable measures of productivity and ascribe remaining differences by race to contemporaneous discrimination (e.g., Altonji and Blank, 1999; Altonji and Pierret, 2001; Hughes and Hertel, 1990; Keith and Herring, 1991; Kreisman and Rangel, 2015).[12] It is nevertheless unclear whether remaining

[11] See Lang and Kahn-Lang Spitzer (2020) for a review of the economic literature on race discrimination. See also Logan (2022) for a discussion of the importance of qualitative and contextual information in understanding racial discrimination and disparities.

[12] See also Woo-Mora (2022) for similar analysis of racial disparities by skin tone in Latin America. This analysis shows that income disparities by skin tone persist (but decrease by roughly two thirds) even when controlling for both observed demographic

differences represent discrimination, unobservable differences in productivity, or whether some of the controls themselves capture aspects of discrimination and thus underestimate discrimination; productivity differences themselves may reflect historical or systemic discrimination.

One way to compare individuals who differ only by race is to conduct experiments—specifically, correspondence and audit studies—where researchers create fake agents who interact indirectly with real agents in the market. Such studies allow researchers to manipulate the perceived identity of the imaginary agent. For example, Bertrand and Mullainathan (2004) submit similar (fictitious) resumes under names that are either typically African American or typically White, and find that candidates with “White-sounding” names get more calls back than similar candidates with “Black-sounding” names. Other examples of this type of study include Zussman (2013), who finds “Arab-sounding” names disadvantage sellers in an Israeli online used car market, and Doleac and Stein (2013), who find a visual cue that the seller is Black decreases responses to online classified advertisements. Observational studies complement such experiments: they are perhaps less able to cleanly identify discrimination, but can follow people over time, analyzing more comprehensive consequences of discrimination and other factors that may affect racial inequality.

A key identification challenge in such observational studies is that families differ in their ability to access opportunities and resources, and much of this variation is driven by historical or family-level discrimination. Althoff and Reichardt (2022) and Derenoncourt et al. (2022) show that historical racial wealth gaps have been and continue to be driven in large part by the vastly different starting positions of African American and White individuals under slavery. We are able to control for differences among African American families’ access to opportunities and resources by comparing siblings of varying perceived skin tones. Our sample is nearly unique in that it allows perceived skin tone to vary within a family, whereas race or ethnicity is often thought of as fully shared by all members of the family. Rangel (2015) uses a similar methodology of comparing siblings in a cross-section, looking at self-reported skin tone classifications in the 1991 Brazilian Census of Population and the 1987 Brazilian Survey of Nutrition. He finds that lighter-skinned children received slightly more education. Francis-Tan (2016) similarly compares siblings using the 2010 Brazilian census and finds that skin tone correlates contemporaneously with education and labor market outcomes.

Our analysis is complemented by recent work by Dahis et al. (2020), who investigate the determinants

differences and unobserved geographic heterogeneity (via a comparison to a respondent’s immediate neighbor). These penalties are estimated to be nonlinear in skin tone, with sharp differences among light-to-medium skin tones coupled with strong but less varied penalties among medium-dark to very dark skin tones. These nonlinear penalties are consistent with our own results, in which we find much larger gaps when comparing African American to White outcomes than when comparing Black-coded to Mulatto-coded individuals.

of and possible returns to racial passing in a large sample that links across several nineteenth and twentieth century censuses. They document that many individuals formerly classified as African American were later classified as White, and that the rates of passing were correlated with factors that plausibly relate to the returns to passing. Additional evidence on the economic importance of changing racial affiliation comes from Cornwell et al. (2017), who show that in Brazil, the same worker receives higher pay from an employer who classifies him as White than from another employer who classifies him as non-White.

Colorism and skin tone penalties among African Americans

Related to the analyses of inequalities across racial groups, a literature in sociology and economics analyzes differences in outcomes by skin tone, often within a given coarser racial group.

The study of skin tone penalties, a key component of colorism,[13] offers an additional layer of analysis within racial gaps in the US (see Monk, 2021 for an illuminating review).[14] During recent decades, a large body of literature has shown that skin tone differentials play an important role in explaining inter- and intra-racial inequalities (Goldsmith et al., 2006), and that skin tone racism persists as other explicit forms of racism are condemned by society (Monk, 2021).

The literature provides robust evidence of differential labor market and related outcomes by skin tone dating back to at least antebellum times. Skin tone gaps are documented within a perceived racial category, usually among African Americans (Keith and Herring, 1991). Most of the research has focused on socioeconomic skin tone penalties, although these also extend to the physiological, health, and legal realms (Adams et al., 2016; Monk, 2021). Howard Bodenhorn and co-authors find skin tone gaps in several outcomes (including occupational status, wealth, freedom, nutrition, and height) among African Americans in the 19th century (Bodenhorn, 2002, 2003, 2006, 2015; Bodenhorn and Ruebeck, 2007). Gullickson (2005) documents large skin tone gaps in occupational status and education for cohorts of African Americans born between 1900 and 1940. Hughes and Hertel (1990) find that, during the 1950–1980 period, the differences in socioeconomic status between light- and dark-skinned African Americans were as high as between African Americans and White Americans overall. Using survey data for 1980, Keith and Herring (1991) suggest that among Black respondents, skin tone can be more predictive of occupation or income than parental socioeconomic status.[15]

[13] Monk (2021) defines colorism as "as a discriminatory practice by which lighter skin tones, straight hair, and relatively more Eurocentric facial features are preferred over darker skin tones, kinky hair, and more stereotypically Afrocentric facial features."

[14] See also Dixon and Telles (2017) for a review of colorism with a focus outside of the U.S.

[15] These findings were questioned by Gullickson (2005), who highlighted that educational and labor market skin tone gaps actually declined for cohorts born after 1950. The results of this study were later contested because of its small sample of light-skinned

Goldsmith et al. (2007) show that, by the early 1990s, light-skinned African Americans earned 23% higher hourly wages than dark-skinned African Americans.

These skin tone gaps extend to different measures of labor market success, such as household income and occupational status, as observed by Monk (2014) for data from the early 2000s. Furthermore, Monk (2014) finds that skin tone does not correlate with employment itself, suggesting skin tone discrimination may restrict access to better jobs rather than to jobs overall. This robust literature leads us to reasonably expect gaps in economic outcomes by perceived skin tone among African Americans, which we confirm in the general population with our census data.

The literature also highlights the intersectional (and in particular often gendered) nature of colorism. Monk (2014) finds that skin tone penalties among men in the labor market are driven by differences among African Americans: dark-skinned African American men are almost twice as likely to exhibit a lower occupational status than their light-skinned counterparts. The paper points out that this could be the product of stereotypes of criminality and troublemaking that dark-skinned African American men face in a racially integrated labor market in which White gatekeepers control their access to employment.

In contrast, African American women face important skin tone penalties in the marriage market due to light-skinned preferences, usually associated with femininity and discriminatory beauty standards (Hill, 2002b; Hamilton et al., 2009).[16] Although there is not an overall strong relationship between skin tone and marital status among African Americans in more recent years,[17] young light-skinned African American women exhibit a marriage premium in that they marry African American men with higher average educational attainment (Monk, 2014; Hamilton et al., 2009; Hunter, 2013). Also, Bodenhorn (2006) and Monk (2014) find assortative marital matching among African Americans of similar skin tone. Our analysis of marital differences confirms and builds on this important literature by showing that a significant part of these skin tone effects holds even when controlling for unobserved family background differences by comparing among differently-classified sisters.

Skin tone penalties for African American women extend to their physical and psychological health.

Black respondents in the later years (Goldsmith et al., 2006).

[16] See also Banerjee et al. (2013), which estimates a marriage market penalty for women with darker skin tones in the context of India based on responses to matrimonial advertisements. Fisman et al. (2008) also estimate that African American women are on average less preferred as potential partners than White women in a U.S. experimental speed dating market, and that this racial penalty appears to be driven by a gap in perceived attractiveness. In contrast, African American men and White men are perceived as similarly attractive on average in this speed dating market.

[17] There are large gaps in marriage rates between White and African American adults in recent years (United States Census Bureau, 2022). Among U.S. individuals 15 years and older in 2022, White men and women were each more likely to be currently married (54% and 53%, respectively) than were Black men and women (36% and 32%).

Using longitudinal health data, Hargrove (2019) finds that dark-skinned African American women exhibit higher cumulative biological risk than light-skinned African American women. The results are partially explained by socioeconomic characteristics and stressors. In a similar spirit, Diette et al. (2015) find that among unemployed African American women, those with darker skin tones are more likely to experience depression.

Skin tone gaps are also traced to childhood and youth experiences, and in particular to educational attainment. Branigan et al. (2013) find that, among African Americans between 1985 and 2000, a one standard deviation increase in skin tone lightness was associated with a three-month increase in educational attainment. Monk (2014) suggests that educational inequality among African Americans with different skin tones is as large as that between White and African Americans. Adams et al. (2016) argue that skin tone penalties affect boys to a larger extent. Stereotypic perceptions of the intellectual abilities and aggressiveness of dark-skinned boys can truncate their academic achievement (Maddox and Gray, 2002). For instance, Hannon et al. (2013) find that dark-skinned African American children face a higher probability of school suspension. Finally, skin tone biases translate into legal consequences for African Americans. Levinson and Young (2010) find that dark-skinned African Americans are more likely to be considered guilty than lighter-skinned African Americans and that skin tone cues become more important in the absence of clear evidence. Analyzing court records, Blair et al. (2004) find that feature-based stereotypes about inmates, such as darker skin and other Afrocentric physical features, are correlated with longer sentences. White (2015) and Monk (2019) find similar results among other dimensions of the criminal justice system. Branigan et al. (2017) find that skin tone penalties also exist among lighter- and darker-skinned White Americans in quick, low information decisions, such as arrests.

There is a large body of literature that studies *how* contemporary skin tone discrimination operates. Goldsmith et al. (2006) highlight that preferences and biases for light skin tones (colorism) have persisted in the U.S. over centuries. They suggest that light-skinned African American workers are favored because of their physical proximity to White workers. This skin-tone-based proximity activates racialized stereotypes and beliefs about worker competence (Adams et al., 2016; Goldsmith et al., 2007; Maddox and Gray, 2002). The conflation between light-skin tones and desirable characteristics, when internalized, can bias how people infer the socioeconomic status of others. Paul et al. (2022) find that, although people can accurately gauge wealth and income from facial cues for light-skinned individuals, they perform poorly when doing so with high-income African American and Latino populations. These patterns extend to perceptions of beauty, even among African Americans (Hill, 2002b; Landor et al., 2013).

Skin tone affects social outcomes in direct and indirect ways. For the former, the effect of colorism on outcomes is mediated by how the additional skin-tone-related discrimination changes their experiences in school, marriage, the labor market, etc. (Branigan et al., 2013). For instance, stereotypes about skin tone can end up as self-fulfilling prophecies: positive or negative beliefs about a given group's ability can affect their performance, regardless of the belief's accuracy (Darity Jr, 2022).

Adams et al. (2016) point out that discrimination is interwoven with individual and contextual factors. For instance, a cohesive racial identity at home can prevent dark-skinned African Americans from being largely affected by colorism. Also, if there is skin tone variation among children in the household, families can tailor their parenting practices to the level and type of discrimination they expect the children to face (Adams et al., 2016). By comparing variation in skin tones within families, we can examine how families might respond to expected discrimination with differential investments in their children. However, as Branigan et al. (2013) note, linking discrimination to skin tone biases, leaving aside other components of racism, proves challenging.

On the other hand, skin tone can affect social outcomes in indirect ways when it becomes a proxy for characteristics that are historically associated with certain gradients of skin tone (Branigan et al., 2013). In this case, inequalities from skin tone accumulate over time and are intergenerationally transmitted, even if the discrimination that caused those gaps in the past has waned (Branigan et al., 2013). We point to this indirect mechanism by comparing skin tone gaps among differently-classified brothers. We find *Black–Mulatto* perceived skin tone gaps in earnings and educational attainment among siblings that are significantly and economically smaller than those observed in the general population. This points to the explanatory power of differences in families' access to opportunity that accumulates over time, driving intra-group skin tone inequality.

Finally, the literature on skin tone penalties has shed light on the experience of other racial and ethnic groups where skin tone discrimination is salient. Among Latin American immigrants, dark-skinned immigrants face higher penalties in the labor market (Hersch, 2008; Rosenblum et al., 2016; Bonilla-Silva, 2006). These patterns even extend, in some cases, to Asian Americans (Monk, 2021).

To summarize the key takeaways from this literature as it pertains to our analysis, we first note that there is ample evidence of skin tone socioeconomic differences among African Americans. Higher penalties for darker-skinned African Americans start at school and persist over the life-cycle through limited labor market opportunities and, in the case of women, reduced marriage prospects and health.

We also highlight the two key mechanisms proposed by the literature through which skin tone penalties operate among African Americans: (a) through contemporaneous discrimination in many contexts (e.g., in labor markets, marriage markets, and other bias-ridden interactions) in which individuals are perceived, treated, and stereotyped differently based on the tone of their skin (which can in turn affect individuals' own identity and investment decisions); and (b) through the intergenerational transmission of stratification and disadvantage, due to differential access to resources and opportunities correlated with skin tone due to discrimination at the family-level or in the past (Monk, 2021). Our paper seeks to decompose the relative effects of these two mechanisms through our analysis of differently-classified siblings, who share the same family-level access to opportunities but who differ in their perceived skin tone.[18] We also study the potentially gendered nature of perceived skin tone effects (as suggested by the literature) by assessing these effects separately for men and women.

4 Data and linking

4.1 Child cohorts

We analyze complete-count, decennial U.S. census data from 1870–1940 (Ruggles et al., 2021). Our primary analysis focuses on cohorts of children age 3–18 and coded as White, Black, or Mulatto in 1870, 1880, 1910, or 1920.[19] We exclude children below the age of 3 and above the age of 18 since very young children have typically not reached their adult skin tone, and older children who remain in their parents' household are highly selected. This gives a full sample of $49,621,196$ sons and $49,021,904$ daughters. We enrich data on the children themselves with characteristics of their household head and parents (when present within the household).[20] We then group children within a household into families. Given our interest in comparing siblings with identical family opportunity access, we define two sons to be members of the same family only

[18] Our analysis is related to the decomposition of "direct" and "systemic" discrimination analyzed theoretically and experimentally by Bohren et al. (2022).

[19] This is the full set of available census years including all African Americans and a Black and Mulatto classification distinction. Data from the 1890 census is no longer extant. The 1900, 1930, and 1940 census years do not distinguish between Black-coded and Mulatto-coded individuals, and so we use this data only when defining linked adult outcomes or in analysis of White vs. Non-White children. We exclude the 1850 and 1860 census years from our analysis, as only free individuals (an unrepresentative and small portion of African Americans, 12% in 1850) are enumerated in this pre-Civil War data. Digitized data has not yet been released for census years after 1940.

[20] We locate a child's father and mother when present within the household using the *poploc* and *momloc* variables constructed by IPUMS.

if they shared the same general race[21] and if we are reasonably certain that they shared the same pair of biological parents. Specifically, we define children to have been members of the same family only if they had: the same household; the same general race (coded as White vs. Black/Mulatto); the same parents listed; both parents present within the household; no parent listed as a step-parent; and a nonzero number of siblings. The possible consequences of illegitimacy, remarriage, adoption, and relationship misclassification are discussed in Section 7.1.

Given this family definition, two key subsets we use in our analysis are the set of sons from families that had both a Black-coded and a Mulatto-coded son age 3–18, and an analogous sample for daughters from differently-classified families. We refer to these samples throughout as BMS ("Black-and-Mulatto-sons") and BMD ("Black-and-Mulatto-daughters").[22] We find $50,737$ sons in $17,475$ families that had some sons classified as Black and the others as Mulatto (BMS), and $54,240$ daughters in $18,854$ families that met the equivalent condition for girls (BMD).[23]

Figure 1 presents one observation that illustrates our empirical exercise. Panel A shows the 1910 census record of John and Florence Spencer and their four sons, two of whom were coded as Black (Maurice, age 9; and John, 4) and two as Mulatto (Isaac, 11; and Edward, 7). Panel B shows the Mulatto-coded son Isaac and his Black-coded brother John thirty years later in the 1940 census. Isaac is now 41 years old, living in Pennsylvania with his wife and child. He works as an operator in a steel mill and earns an annual salary of $1,800 (roughly $38,000 in current dollars). His Black-coded brother John lives in South Carolina and works as a simple laborer for the much lower annual salary of $480 (roughly $10,000 in current dollars).[24]

Table 1 shows the distribution of skin tone classifications of African American couples overall and in the BMS and BMD samples. While 79% of all African American couples had both members classified as Black, these couples represented only 29% of BMS households, while 70% of BMS households had at

[21] We initially considered also analyzing the small number of households containing both White and Mulatto-coded children. However, examination of the original census records for such households in 1910 suggested that virtually all of them contained either enumeration errors (e.g., household code corrections were visible on the original census record) or data entry errors. The latter were frequently driven by the similarity of handwritten codes used to distinguish White from Mulatto: "W" and "M." (Many enumerators ignored the instruction to identify Mulatto-coded individuals with the more distinctive "Mu.") Because of the small sample and high error rate, we decided to limit analysis of differently-classified siblings to households with both Black-coded and Mulatto-coded children.

[22] Another sample that may be of interest ignores the gender of children when looking for families with both Mulatto or Black siblings. This sample would include the union of the BMS and BMD samples, along with additional families where race varies only across the gender of siblings. We proceed only with the BMS and BMD samples as the considerations in investments in sons and daughters during childhood in this era were very different.

[23] We note that the same family could be counted multiple times if it contains at least one Black-coded and one Mulatto-coded same-gender sibling both age 3–18 in multiple distinct decennial census years.

[24] The other two brothers, Maurice and Edward, could not be linked to 1940 census records.

least one parent classified as Mulatto. (The remaining 1% primarily reflect households classified as Black–White couples.)[25] Differently-classified Black- and Mulatto-coded parents were especially overrepresented, accounting for 59% and 55% of parents in our BMS and BMD families, respectively, but only 10% of African American couples overall. This higher frequency of differently-classified couples likely reflects either greater variation in perceived skin tone among the children of differently-classified parents, and/or enumerators who were particularly responsive to this variation.

Although it was rare for brothers to receive different racial classifications, Appendix Figure A1 shows that these families were not highly geographically concentrated. We know, therefore, that many enumerators gave different racial classifications to siblings in the same household, which also increases our confidence that the existence of such households represented genuine differences in perceived skin tone. This is also consistent with the fact that Mulatto-coded siblings were more likely than Black-coded siblings to be classified as White adults in future census years (in which the Mulatto category no longer existed), as discussed in Section 6.2.

Census records do not indicate which household members were physically present when enumerators visited, and we cannot know how exactly enumerators assessed "color or race," especially for individuals they did not observe (see Appendix B for complete racial classification instructions to enumerators).[26] When spouses or siblings *were* coded differently, this was presumably either because both were present and the enumerator perceived them differently, or because the enumerator was told by a respondent about differences in household members' skin tone. In both cases, coding reflected skin tone as perceived by a third party, and our estimates capture the effects of perceptions rather than physical skin tone. Regardless of how enumerators assessed skin tone, we note that differences in perceived skin tone persisted across decades and third-party observers (as shown by differences in future classification as White) and therefore allow us to consistently estimate the effects of contemporaneous discrimination based on perceived skin tone.

Table 2 Panel A provides a variety of summary statistics for the BMS sample and for households with multiple all-Black- or all-Mulatto-coded sons. The characteristic that varied the most across the samples

[25] One might be concerned that some census enumerators could have classified multiracial children as Mulatto if their parents came from at least two different perceived races, even when neither parent was perceived as African American. Relatedly, one might be concerned that Native American or other individuals not necessarily of African ancestry might have been misclassified as Mulatto. Parent race frequencies for our BMS and BMD samples (Table 1) and among all families with at least one Mulatto-coded child (available upon request) both show a vanishingly small fraction of families in which neither parent is coded as Black/Mulatto, suggesting that consistent with instructions, enumerators generally did not code non-African American but multiracial individuals as Mulatto. Individuals classified as Native American also represent a much smaller share of the general population than individuals coded as Mulatto or Black, limiting the scope for misclassification of entire families as African American to impact our estimates or interpretation.

[26] The 1940 census—in which a Black/Mulatto distinction no longer exists—does include information about "the respondent who provided most of the information for that household."

was the number of sons, which varied in part because of the way we constructed the samples: The greater the number of sons in a household, the more chances there were for one of them to have a different skin tone classification, and indeed households in the siblings sample had the highest average number of sons. Even though we restrict the same-race sample to families with at least two sons, the number of sons was still lower than in the siblings sample. The average age of children followed the same pattern, perhaps since having more sons in the household also meant that younger or older households (with fewer children, or only youngest son present) were less likely to appear, and the younger households were more numerous because of population growth. Besides these two mechanical differences in characteristics, the sibling sample was not selected evenly across geographic locations. Children in the siblings sample came from more rural counties than the general population. This geographic selection contributed to differences in other characteristics such as literacy and occupational structure. Panel B shows similar results for the samples of daughters.

4.2 Linking across censuses

To assess outcomes later in life, we link individuals from the child cohorts described above to their records as adults in future census years.[27] We link all cohorts to their adult selves 20 and/or 30 years later, depending on data availability. All cohorts are linked at least to their adult selves 20 years later, with the exception of the 1870 cohort (which is instead linked to the 1900 census); we refer to these as *"t+20* links."[28] We also link the 1910 cohort to their adult selves in 1940 (as well as 1930), since 1940 is the only currently available full-count census including individual income.[29] We refer to the union of 1910–1940 and 1920–1940 links as "1940 links."

Given the lack of Social Security Numbers or other unique identifiers, linking children to their adult selves is a non-trivial task; we use links identified by the Census Tree data set (Price et al., 2021) as our baseline specification, and show robustness to using links identified by users of a genealogy platform (FamilySearch), by the Census Linking Project (Abramitzky et al., 2020), and by information from Social Security Number applications (Althoff et al., 2022).

[27]Focusing our analysis on perceived racial and skin tone gaps by childhood (rather than adult) race lessens the extent to which census enumerator racial classifications responded endogenously to an individual's occupation, income, or other outcomes of interest, as these characteristics had not yet been realized for children.

[28]We link children from the 1870 cohort to the 1900 census because the 1890 census is no longer extant. Our main analyses include age fixed effects which are allowed to vary flexibly by cohort and comparisons are made only within a cohort, thus accounting for potential life cycle differences after 20 vs. 30 years.

[29]We do not analyze 1940 outcomes among the 1870 and 1880 child cohorts, as most of these individuals would no longer have been of typical working age in 1940.

The Census Tree data set combines links from a variety of methods and sources. These include links from a genealogy platform (FamilySearch), from machine learning predictions trained on these genealogy links, from the method used in the Census Linking Project (Abramitzky et al., 2014), from the IPUMS Multigenerational Longitudinal Panel (Helgertz et al., 2020), and from hand-linking by a large team of research assistant and volunteers. Importantly for our analysis of daughters' adult outcomes, the Census Tree data set is able to form links for women. The Census Tree data set successfully links a large number of women across name changes at marriage by incorporating information from genealogy links and additional records (including birth, marriage, and death records) that often include maiden or parents' names for women.

The Census Linking Project is a research effort which uses a variety of automated algorithms to identify records in different census years which represent the same individual. Two records are linked if they have the same state/country of birth, if they have similar birth years, first names, and last names, and if these records better align on these criteria than all other possible links for these records (Abramitzky et al., 2014).[30] Since most women changed their last names upon marriage, the Census Linking Project does not attempt to link daughters to their adult selves.

Althoff et al. (2022) similarly links individuals across census years by using a version of the linking algorithm from Abramitzky et al. (2014) that has been modified to incorporate additional information from individuals' applications for a Social Security Number (SSN). This additional information includes applicants' maiden names, as well as the names and maiden names of applicants' parents. This additional information helps to improve the match rate for applicants and their parents, and importantly links women across potential name changes at marriage. Relative to the other linking methods we use, it also reduces linking rates and statistical power by restricting to individuals (and their parents) that applied for SSNs and, for data privacy reasons, that were known to have died before 2007.

Table 3 reports the results of linking sons from our child cohorts to their adult selves. Two linking rules are considered. The first set of links, reported in columns (2)–(5), comes from the Census Tree and links sons at a relatively high 51% linking rate (35% for daughters[31]). A more conservative method, reported in columns (6)–(9), uses the Census Linking Project's "ABE Exact Conservative" links and results in a linking

[30] See Abramitzky et al. (2020) for more detail on the specifics of the linking algorithms implemented in the Census Linking Project and see Abramitzky et al. (2021) for discussion of linking considerations, these algorithms, and their relative performance. We use the "ABE Exact Conservative" links provided by the Census Linking Project. See Price et al. (2021) for more detail on the Census Tree data set.

[31] See Appendix Table A1 for linking statistics for daughters.

rate of 17% for sons (and does not link daughters), relying only on information determined at birth.[32] We use the more complete (Census Tree) links in our baseline specification, and show robustness of our key analyses to alternative linking algorithms in Appendix Figures A2 and A3.[33]

Individuals are not randomly selected into the linked sample. Table 3 columns (2)–(3) and (6)–(7) report attributes of linked sons, and differences in those attributes between linked and unlinked sons. Linking rates are higher among sons who were lighter-skinned, literate, and those in households with literate heads or more children.[34] Literate and richer households may have used more unique names which make an individual more likely to be successfully linked; most of the observations that we fail to link have more than one potential link that we cannot sufficiently distinguish from the best potential match, rather than lacking even a single sufficiently close match.

To make the linked sample more representative of the baseline sample, we reweight observations according to how likely they are to be linked. As suggested in the linking literature (e.g., Pérez, 2017; Zimran, 2019; Bailey et al., 2020), we predict the probability of an observation being linked given a set of background variables.[35] We then assign weights equal to the reciprocal of this probability, representing the average number of observations each linked observation represents. The resulting weighted characteristics of the linked samples, and the differences between linked and unlinked observations, are presented in columns (4)–(5) and (8)–(9). The linked sample is now much closer to the unlinked sample. In our baseline specification, we weight all observations of linked outcomes by these inverse link probability weights.[36]

[32]These linkage rates of 17 to 51% exceed or are comparable with other papers using historical individual-level data (Abramitzky et al., 2012, 2014, 2021; Ferrie and Long, 2013; Price et al., 2021).

[33]We observe lower rates of linking children to their *t+20* outcomes when using genealogical links from FamilySearch (9% and 5% of sons and daughters, respectively) or the SSN application-based method (4% and 2%). As a result, while replicating our analyses with these alternative linking methods generally produces similar point estimates and qualitative conclusions, there is far less precision (especially when comparing differently-classified siblings).

[34]Appendix Table A1 shows similar results when linking daughters.

[35]We predict a link using a probit regression. We include the following controls as indicator or factor variables: the numbers of same-gender children in the family; household head is child's parent; head home ownership; head, father, and mother literacy; head, father, and mother presence in the household; head, father, and mother 1-digit occupation; child age; child birth order; child is Black- or Mulatto-coded. We also include continuous controls for the head, father, and mother's income score (see Section 4.3). We include missing values as a distinct outcome level for each of these variables.

[36]We also winsorize weights at the 99th percentile to limit the influence of outliers. When analyzing linked specifications that include family fixed effects (Equation 2), only cases in which both an individual and their same-gender sibling are linked will inform our coefficient estimates. As such, for these specifications we weight by the inverse of the predicted probability of successfully linking an individual *and* at least one of their same-gender siblings.

4.3 Measuring income scores in adulthood

Data on individual incomes was collected as part of the decennial census starting in 1940, by which point our 1870 and 1880 cohorts would no longer have been of working age. To enable an analysis of individual earnings which covers these child cohorts, we follow Abramitzky et al. (2021) in constructing proxies of individual income (which we refer to as "income scores"). To do so, we first predict log wage income among men age 20–50 in 1940 census data based on an individual's 3-digit occupation, state, and age.[37] As the 1940 income variable excludes income from self-employment (a category which includes most farmers), we adjust this income prediction for farmers using a method developed by Collins and Wanamaker (2022). This adjustment makes use of the facts that the 1940 census records the income of farm laborers, while the 1960 census records the incomes of both farmers and farm laborers. We thus impute the (non-wage, unrecorded) income of farm laborers in 1940 by multiplying the income of farm laborers in 1940 with the ratio of earnings for farmers relative to farm laborers in the 1960 census, by region and home-ownership (when available, starting in 1900). We also follow Abramitzky et al. (2021) and Collins and Wanamaker (2022) in scaling up the incomes of farm managers and farm laborers to account for in-kind transfers.

We also show the robustness of our income score-based results to alternative constructions of this income score in Appendix Figure A4. In these alternatives, we either include race as an income predictor,[38] predict log total income (as defined in Section 6.4 to include imputed non-wage income, rather than predicting log wage income), use the 1900 Census of Agriculture to adjust incomes for farm managers and farmer laborers (rather than the 1960-decennial-census-based adjustment suggested by Collins and Wanamaker, 2022), or use the 1901 Cost of Living survey (rather than decennial census data) as our income data source.[39]

5 Empirical strategy

A naïve assessment of the effects of discrimination might simply compare the outcomes of individuals in different groups: for example, did individuals perceived as being darker-skinned earn more than those

[37]The precise controls included when predicting an individual's log wage income in 1940 are: occupation; age; squared age; state; and (1-digit occupation)×(region). We predict and analyze only income earned by men due to lower female labor force participation rates during this time period.

[38]The precise controls added when predicting an individual's log wage income in 1940 are: race indicators (Black/Mulatto vs. White); (race)×(1-digit occupation); (race)×(region).

[39]We follow Abramitzky et al. (2021) in the construction of these latter two alternative income scores, where this construction is described in greater detail. Relative to the 1940–1960 decennial censuses, the 1900 Census of Agriculture and the 1901 Cost of Living surveys occurred closer in time to when adult outcomes are measured for our earlier cohorts, but cover fewer individuals or contain less granular information on occupations and wages.

perceived as lighter? In Table 4, we report average characteristics by perceived child race and gender. We see that children perceived as darker-skinned had lower levels of literacy and school attendance as children on average coupled with lower educational attainment. We also see gaps in earnings and predicted income as adults among sons, along with gaps among daughters in marital rates and in the spouse's education and earnings. However, we also see that a child's perceived skin tone was correlated with myriad confounding factors, including home geography, parental literacy, and other family characteristics.

A more informative assessment of contemporaneous discrimination will attempt to control econometrically for (some of) these factors. We therefore first estimate an equation of the form:

$$y_i = \alpha + \beta_1 Black_i + \beta_2 (Black \cup Mulatto)_i + \mathbf{x}_i \gamma + u_i \tag{1}$$

In this specification, we regress an outcome y_i, on indicators of individual i's perceived skin tone and on $\mathbf{x_i}$, a vector of observable attributes that are expected to affect the outcome in question and which may be correlated with race (e.g., age, geography, or family characteristics). Our estimated β coefficients will then measure mean differences in y_i between racial groups conditional on these other controls, with β_1 measuring the "*Black–Mulatto*" gap and β_2 measuring the "*Mulatto–White*" gap. We use this approach to measure the initial gap in outcomes by child race in the general population, along with the extent that this gap is explained by observable characteristics. We pool observations across years in our baseline specification, including cohort fixed effects and allowing all non-race controls to vary flexibly by year (via interactions with cohort fixed effects). In alternative specifications, we also separately estimate race and perceived skin tone gaps by child cohort.

Even an extensive set of controls cannot control for the unobservable factors that might vary systematically with race. In particular, there are many attributes of the environment in which an individual grows up that affect their adult outcomes. When comparing Black vs. Mulatto children, we can control for even unobservable differences in children's access to resources and opportunities varying across families by comparing Black-coded and Mulatto-coded children from differently-classified (BMS/BMD) families. These siblings grew up in the same family but differ in their perceived skin tone to the extent that the enumerator classified them differently.

One natural way to measure the gap across African American skin tones within the BMS sample would be to reformulate Equation 1 by decomposing the error term for individual i (raised in family f) into a family and an individual-specific component:[40]

$$y_i = \alpha + \beta_1 Black_i + \mathbf{x}_i\gamma + \underbrace{\phi_f + \varepsilon_{if}}_{u_i}, \tag{2}$$

In this family fixed effect specification, sons coded as Black are compared only to their Mulatto-coded brothers.[41] This requirement compounds the considerable difficulty of linking individuals across census years, as two differently-classified brothers will contribute to our "*Black–Mulatto*" gap estimate only if *both* brothers are linked across census years. This results in a much smaller sample size and lower power when analyzing linked outcomes.

Our preferred specification for estimating the *Black–Mulatto* outcome gap among BMS families compares the weighted mean of y_i among Black-coded BMS sons to that of Mulatto-coded BMS sons, conditional on other controls (with an analogous specification for BMD daughters). We implement this approach by adding race indicators interacted with BMS family indicators to Equation 1:

$$\begin{aligned} y_i = \alpha + \beta_1 Black_i + \beta_2 (Black) \times (Not\,BMS)_i + \beta_3 (Black \cup Mulatto)_i \times (BMS)_i \\ + \beta_4 (Black \cup Mulatto)_i \times (Not\,BMS)_i + \mathbf{x}_i\gamma + u_i \end{aligned} \tag{3}$$

In this specification, β_1 measures the *Black–Mulatto* gap among sons in differently-classified families. In the BMS sample, a given family may have contained an unequal number of Black-coded and Mulatto-coded sons. We therefore weight observations within a BMS family by the inverse proportion of their racial classification within a family.[42] For example, if a given BMS family contained one Black-coded and two Mulatto-coded sons, the Black-coded son is assigned twice the weight of each of the Mulatto-coded sons, so that this family contributes equally to the weighted Black and Mulatto averages within the BMS sample. This ensures that our initial sample of Black-coded and Mulatto-coded children in BMS families have identical

[40] See Abramitzky et al. (2020) and Kreisman and Smith (2022) for papers that use a similar identification strategy to disentangle family circumstances from within-household differences. Both use household fixed effects to compare the outcomes of siblings with more distinctively foreign or Black names, respectively. Similar to most of our results for men, both papers find much smaller gaps in outcomes among siblings than in the general population.

[41] Our family definition (see Section 4.1) precludes White and Non-White siblings, so our "*Mulatto–White*" comparison is subsumed by our family fixed effects.

[42] BMS individuals of racial classification $j \in \{Black, Mulatto\}$ are weighted proportionally to $\frac{min\{N_{Black}, N_{Mulatto}\}}{N_j}$, where N_j denotes the number of sons of classification j within an individual's family. Weights in our BMS sample are then scaled to have mean 1, so that BMS and non-BMS individuals receive the same average weight.

(weighted) distributions of family characteristics, including access to opportunities and resources that vary across families. This approach is closely related to controlling for differences in the perceived skin tone distribution across families by using inverse propensity score weights for racial group membership when predicting race using family fixed effects. It is also conceptually similar to randomly sampling matched pairs of Black- and Mulatto-coded siblings from within differently-classified families.

By comparing these two groups of individuals with identical family opportunity access distributions but different perceived individual skin tones, we are able to plausibly estimate the causal effect of skin tone perceptions themselves on economic outcomes. In Appendix Table A2, we show that Black- and Mulatto-coded siblings in our BMS and BMD samples are linked to their adult selves at similar rates by our preferred Census Tree links, which suggests that differential attrition is unlikely to significantly influence our estimates.[43] To account for any small differences in these populations which occur due to minor differences in selection into our regression sample, we also include our full set of granular geographic and family characteristic controls in this weighted difference specification. In Sections 6.1–6.3 we also confirm that our weighted difference and family fixed effect specifications produce quantitatively similar results when analyzing child outcomes and both estimate no statistically significant differences in adult economic outcomes among Black vs. Mulatto siblings in our BMS and BMD samples.

Relative to the family fixed effect specification, our preferred BMS/BMD weighted specification dramatically increases (generally by a factor ranging from 2–12) the number of differently-classified siblings used in the analysis by relaxing the requirement that at least two siblings are successfully linked. This significantly increases the precision of these estimates, and Appendix Figure A2 shows that our weighted BMS/BMD specification is also more robust than is the family fixed effect specification to our choice of linking algorithm.

6 Perceived skin tone and economic outcomes

6.1 Gaps in child education

Table 5 shows race and perceived skin tone gaps in school attendance among children age 5–18, defining y_i as an indicator for reported school attendance within a recent specified period. Note that analysis of this subsection does not rely on linking, because childhood race classification, education, and siblings are

[43] When using our alternative genealogical or SSN application-based links, we do observe small but statistically significant differences in linking rates among differently-classified daughters; daughters coded as Black are 0.3% more likely to be linked by these methods than are daughters coded as Mulatto.

observed in the same census. Comparing sons in Panel A, we see large initial gaps in the general population correlated with perceived skin tone. 67.3% of White-coded children attended school, which is larger than the corresponding share among Mulatto-coded (49.7%) and Black-coded (39.8%) children. Controlling only for age and birth order fixed effects (interacted with child cohort) in column (1), we observe a *Black–Mulatto* school attendance gap of −7.8% and a *Mulatto–White* school attendance gap of −18.5%. These gaps shrink to −5.6% and −10.2%, respectively, when controlling for state fixed effects, to −4.9% and −10.7% after adding granular geographic controls, and shrink further to −3.1% and −5.5% when adding controls for a rich set of family characteristics.[44] While smaller in magnitude than the White vs. non-White gap, the gap between perceived lighter- and darker-skinned African Americans was economically and statistically significant in the general population and across observably similar families.

In contrast, column (5) shows that among BMS families the weighted *Black–Mulatto* gap was only −0.6%. This estimate is very close to that of our specification with family fixed effects (column 6), which estimates a gap of −0.5%. Both of these estimates are not different from zero at a standard (95%) significance level and are an order of magnitude smaller than the *Black–Mulatto* school attendance gap in the general population. The 95% confidence intervals on these differently-classified brother estimates have lower bounds of −1.3% and −1.2%, respectively. This suggests that the vast majority of the *Black–Mulatto* school attendance gap in the population was driven by differences across families, rather than by skin tone discrimination itself. We visualize our estimated racial gaps in child education in Figure 2.

Table 5 Panel B similarly estimates gaps in school attendance among *daughters* by child race. *Black–Mulatto* gaps were similar to those among sons in columns (1)–(4), ranging from −7.9% when controlling only for age and birth order to −3.3% when controlling for granular geographic and family characteristics. Our *Mulatto–White* estimates in these same regressions range from −14.9% to −3.1%. In contrast to our analysis of sons, a smaller but significant *Black–Mulatto* gap in school attendance remains among differently-classified families in columns (5)–(6); we estimate gaps of −2.1% and −1.8% using our weighted and family fixed effect specifications respectively, which are both statistically significant at standard significance levels. Even within the same African American family, daughters coded as lighter-skinned were about 2 percentage

[44] Controls for family characteristics include the following predictors: fixed effects for the number of same-gender children within the family; an indicator for the household head being the child's parent; and indicator for home ownership by the household head (available starting in 1900). The remaining family characteristic controls include the following predictors associated with a child's household head, father, and mother: predicted log wage income score; indicators for literacy; indicators for presence within the home; fixed effects for general (1-digit) occupation. We include missing values as a distinct outcome level for each of these controls. All non-race controls, including family characteristics, are allowed to vary flexibly by child cohort.

points more likely to attend school than their darker-skinned sisters.

Table 6 similarly estimates racial gaps in literacy (defined as the ability to read and write) among children age 10 to 18. Panels A and B report results for sons and daughters, respectively, which largely parallel our findings regarding school attendance. We again find similar initial *Black–Mulatto* gaps in literacy among sons and daughters (-12.6% and -12.7%, respectively), which largely then disappear among differently-classified brothers (-0.04% among BMS families) and which shrink but significantly persist among differently-classified daughters (-2.3% among BMD families). Our 95% confidence interval on the *Black–Mulatto* gap among BMS sons has a lower bound of -1.0%, again suggesting that most of the *Black–Mulatto* literacy gap in the general population was driven by factors which varied across families.

It is impossible to tell with certainty why racial gaps in child education persisted to a significant degree among differently-classified daughters but not among differently-classified sons. Much of the research on colorism and our own analysis suggests that African American women faced harsher penalties for darker skin tone than African American men, especially on the marriage market (see Section 6.3 and Monk, 2014).[45] Higher investment, then, may have yielded higher returns in the marriage or labor markets for lighter-skinned girls and led to this gap in educational investment (see Goldin, 1992 for discussion of the historical returns to educational investment for women).[46]

We show educational gaps by child cohort in Figure 3 (*Black–Mulatto* gaps), Appendix Figure A5 (*Mulatto–White* gaps), and Appendix Figure A6 (*Black/Mulatto–White* gaps). We see that gaps among perceived darker-skinned children in the general population partially closed over time, with a particularly dramatic decrease in gap magnitudes relative to White children (as in Collins and Margo, 2006). In contrast, we do not observe clear time trends in the (much smaller) *Black–Mulatto* gap among differently-classified siblings.

6.2 Gaps in classification as White adult

We next analyze adult outcomes, which requires linking children forward to their adult census records as discussed in Section 4.2. In Table 7 and Figure 4 Panel A, we first analyze differences (by perceived child race

[45] See Hamilton et al. (2009) and Arceo-Gomez and Campos-Vazquez (2014) for empirical evidence of this phenomenon in more recent times. Kotlikoff (1979) also shows light skin was associated with the price for female but not male enslaved people. For contemporaneous cultural references see, for example, Hurston (1937).

[46] An alternative explanation is that reverse causality may have driven some of this educational gap if census enumerators were more likely to classify African American girls as Mulatto (rather than Black) if they were more educated. For this effect to drive our observed differences among differently-classified daughters but not among differently-classified sons, enumerator classification would need to have responded to the education of daughters but not of sons. This explanation seems somewhat unlikely.

and gender) in an individual's likelihood of being classified as a White adult by future census enumerators. To do so, we define y_i to be 1 if a child from a cohort in census year t was classified as White in $t+20$ and 0 if this child was identified as Black in $t+20$, noting that Mulatto was not an available racial category in any of the linked census years from which we draw adult outcomes (1900, 1930, and 1940).[47] Unsurprisingly, we find that Black- and Mulatto-coded children were much less likely than White-coded children to be classified as White adults.

Importantly, we also find that Mulatto-coded sons were significantly more likely than Black-coded sons to be classified as a White adult in the general population (2.9% more likely), conditional on geography and family characteristics (2.3% more likely), among sons in our weighted BMS specification (1.8% more likely), and conditional on family fixed effects (1.9% more likely).[48] We see similar differences for daughters, with Mulatto-coded daughters having been more likely than Black-coded daughters to be classified as a White adult in the general population (3.1% more likely), conditional on geography and family characteristics (2.6% more likely), among daughters in our weighted BMD specification (2.8% more likely), and conditional on family fixed effects (2.8% more likely). This difference in future classification as White provides strong empirical support for the claim that Black vs. Mulatto classification by a census enumerator reflected real differences in skin tone or other features influencing one's perceived race and skin tone, with likely similar difference magnitudes in the general population and among differently-classified siblings.[49]

In Appendix Table A3, we similarly analyze gaps (by childhood race) in the likelihood that an individual self-reports their race as White when applying for a Social Security Number (SSN). To do so, we start with our child cohorts linked to their adult selves in 1940. We then use data and links provided by the CenSoc Project (Goldstein et al., 2021) to link adults in the 1940 Census to their SSN application information. We again find that individuals classified as Mulatto during childhood are significantly more likely than Black-coded children to be listed as White (rather than Black) on their SSN applications in the general population (1.7%

[47] We exclude the rare cases in which a child was later classified as a race other than White or Black in adulthood (0.04% and 0.03% of linked sons and daughters, respectively).

[48] Dahis et al. (2020, Table 4) similarly find that individuals (including adults) coded by census enumerators as Mulatto in 1910 were more likely to be coded as White in the following 1920 census, relative to individuals originally coded as Black. In related work, Saperstein and Gullickson (2013) analyze transitions in racial classifications among Black- and Mulatto-coded individuals in U.S. census data from 1870–1920. They find that Mulatto-coded individuals are more likely to appear as Mulatto in future census years, relative to individuals originally coded as Black.

[49] The difference between the gaps in the general population vs. among differently-classified siblings varies in direction and magnitude across different linking methods, and is generally not significantly different at standard 95% significance levels. Relative to other linking methods, the SSN application-based links produce future classification as White gaps in the general population which are unusually small among sons and unusually large among daughters. See Appendix Figures A2 and A3 for details.

and 1.6% more likely among sons and daughters, respectively).[50] Among differently-classified siblings, we see even larger gaps (3.4% and 6.3% among brothers and sisters, respectively), although these gaps are less precisely estimated and statistically insignificant due to the difficulty of linking children to both their adult census records and to their SSN applications.

6.3 Gaps in adult educational attainment and marital outcomes

Table 8 and Figure 4 Panel B report differences in the total number of years of schooling attained by adulthood. Sons identified as White in 1910 or 1920 had on average attended 2.9 more years of schooling by 1940 than Mulatto-coded children. As we add controls for geography and family characteristics, this *White–Mulatto* gap among sons decreases to 1.2 years. Comparing differences by child skin tone within the African American population, we see that perceived darker-skinned children attended 0.9 fewer years of education than lighter-skinned African American children. This gap gradually decreases in magnitude to 0.8, 0.7, and 0.4 years as we add controls for a child's state, enumeration district, and family characteristics, respectively.[51] Analyzing differences among differently-classified siblings in column (5), we see that Black-coded and Mulatto-coded brothers had similar educational attainment as adults. This *Black–Mulatto* gap of -0.1 years among siblings is statistically insignificant, and is an order of magnitude smaller and in the opposite direction of the general population gap. Adding family fixed effects in column (6) shows a similarly small and insignificant gap of -0.05 between differently classified brothers. This finding regarding adult educational attainment is consistent with our analysis of child literacy and school attendance in Section 6.1, where we also found insignificant and very small differences between Black- and Mulatto-coded sons within our sample of differently-classified families.

Comparing daughters in this same figure and table, we see more persistent educational differences as we add controls, consistent with our analysis of childhood educational outcomes. We initially see a *Black–Mulatto* gap among daughters of 0.7 years in the general population, which decreases in magnitude to 0.7, 0.6, and 0.3 years as we add controls for a child's state, enumeration district, and family characteristics, respectively. This gap persists at 0.3 years even among sisters in our preferred weighted BMD specification

[50] We note that in general, Black/Mulatto-coded daughters are much more likely to be classified as White on their linked SSN applications than in their adult census records, and also more likely to be classified as White than are Black/Mulatto-coded sons. This may reflect a higher rate of false positive matches for women in the CenSoc Project links between adult census records and SSN applications.

[51] Enumeration districts are small, sub-county geographies that could be completely covered by a single enumerator within two weeks in cities and within four weeks in rural areas. Enumeration district data are not available in the 1870 cohort, for which we therefore compare within counties rather than enumeration districts.

and remains statistically significant. Adding family fixed effects decreases the magnitude of this gap to a much smaller 0.03 but also decreases the precision noticeably (se=0.3), as our estimate in this specification is not statistically different from either that of our preferred weighted BMD specification or from zero. As discussed in Section 5, specifications including family fixed effects rely on many fewer observations because of the need to link both an individual and at least one of their same-gender siblings. When combined with our more precisely estimated gaps in childhood educational outcomes that do not rely on imperfect linking, we can reasonably conclude that there were significant gaps in education among sisters from differently-classified (BMD) families with perceived lighter and darker skin tones, but that the magnitude of these gaps was smaller than in the general population.

We next turn to comparisons of marital outcomes in Tables 9 and 10 and in Figure 5. First analyzing differences among African American sons, we find that individuals with perceived darker skin tones (Black-coded) as children were more likely than Mulatto-coded sons to be married as adults in the general population (2.6% more likely) or conditional on controls for geography and family characteristics (0.9%). This gap becomes near-zero and statistically insignificant in our preferred weighted BMS specification (−0.02%), and remains statistically insignificant conditional on family fixed effects (with a point estimate of 0.7%). Next analyzing differences in the total number of years of schooling attained by the sons' spouses (conditional on marriage), we estimate initially significant *Black–Mulatto* gaps in the general population (0.8 more years on average for the spouses of Mulatto-coded sons) and conditional on geography and family characteristics (0.4 more years). This gap also shrinks dramatically and become insignificant when comparing siblings in our weighted BMS (0.05 more years) and family fixed effect (0.03 more years) specifications. Together, these results suggest that African American men with darker perceived skin tones did not face significantly greater contemporaneous discrimination on the marriage market than did African American men with lighter perceived skin tones.

We find much starker perceived skin tone penalties on the marriage market when repeating this same analysis for women in Panel B of these same tables and in the same figure. We find that daughters with darker perceived skin tones were less likely than Mulatto-coded daughters to be married as adults in the general population (3.0% less likely) or conditional on controls for geography and family characteristics (4.4%). This gap actually grows slightly and remains highly statistically significant among differently classified sisters in our preferred weighted BMD specification (4.8%, se=1.4%). This gap becomes less precise and shrinks conditional on family fixed effects (0.9%, se=2.4%), and is not statistically significantly different from our weighted BMD gap, population gap, or an estimate of zero given this decreased precision. Next analyzing differences in the total number of years of schooling attained by the daughters' spouses (conditional on marriage), we see significant *Black–Mulatto* gaps in the general population (0.6 more years on average for the spouses of Mulatto-coded daughters), conditional on geography and family characteristics (0.2 more years), and when comparing siblings in our preferred weighted BMS specification (0.6 more years). This gap has a similar magnitude (0.4 more years) conditional on family fixed effects, but with less precision (se=0.5 years). In sharp contrast to our results for men, these results suggest that African American women with perceived darker skin tones faced substantially more discrimination than lighter-skinned African American women on the marriage market. These results resonate strongly with the existing literature, which often finds that skin tone discrimination is gendered and more prevalent against women, particularly on the marriage market due to discriminatory beauty standards (Hill, 2002b; Hamilton et al., 2009). Our findings of significant differences in marital outcomes among sisters, and of differences in the likelihood of future classification as White among both brothers and sisters, also suggest that our data and weighted BMS/BMD method are able to detect significant differences when such gaps existed, supporting our interpretation of estimates of otherwise similar outcomes among brothers as ruling out large effects of contemporaneous discrimination.

In Appendix Table A4 and Appendix Figure A7 Panel B, we show that African American children with lighter perceived skin tones (both sons and daughters) were more likely to be married to spouses classified as White adults (conditional on marriage). This result is not particularly surprising given the differences in the future racial classification of these children themselves as adults and the historical context, in which interracial marriages were often particular targets of discrimination and were illegal in some states.

6.4 Gaps in adult income

We now turn to our analysis of income differences by perceived child race and gender. To do so, we first analyze differences in actual log wage income among sons in 1940 in Table 11 Panel A and Figure 6 Panel A. Darker-skinned African American men earned 13.8% less than lighter skinned ones in the general population (column 1). This gap decreases to 10.8%, 9.2%, and 5.1% as we control for childhood state, enumeration district, and family characteristics respectively, but remains economically and statistically significant. In contrast, Black-coded brothers in our BMS sample earned similar amounts relative to Mulatto-coded brothers in our preferred weighted BMS specification (−0.6%, se=2.7%) or conditional on family fixed effects (0.1%). In other words, controlling for childhood access to opportunities at the family-level by comparing brothers, we find similar incomes among African American brothers with lighter vs. darker perceived skin tones. While there is significant uncertainty in this estimate due to the difficulty of linking children to their adult selves, this gap among differently-classified siblings is different from the general population gap from both an economic and statistical perspective. With a standard error of 2.7% on our preferred weighted BMS estimate, our 95% confidence interval rules out a gap larger in magnitude than 4.7%. This suggests that most of the *Black–Mulatto* income score gap in the general population was driven by opportunity access that varies across families, rather than by contemporaneous discrimination that differentiates among African American skin tones. The *Mulatto–White* gap in earnings is measured as −68.8% in the general population and −45.0% conditional on geography and family characteristics, much larger than the income gaps between lighter- and darker-skinned African Americans.[52]

One concern with an analysis of labor-market income (recorded on the census as "amount of money wages or salary received (including commissions)") is that the reported values exclude income from one's farm, business, or other sources. Appendix Table A5 therefore repeats the analysis in Table 11, but adds in non-wage income. This is imputed using data from the 1950 census, in which a sample of respondents separately reported their income from labor and other sources. We calculate the median non-wage income for each occupation in the South and outside of the South from the IPUMS sample of the 1950 census, and use the deflated values to approximate each individual's non-wage income in 1940 given his occupation and location.[53] Imputing income for farmers and others who did not receive income from wages or salaries also adds observations to our analysis. Overall, the results for total income are qualitatively similar to those for labor income.

While this income analysis uses actual wage income in 1940, this restricts our analysis to our 1910 and 1920 child cohorts as our 1870 and 1880 cohorts are unlikely to still be working in 1940. In Table 12 Panel A and Figure 6 Panel B, we now analyze differences in log wage income scores for all four cohorts in census year *t+20*. As described in Section 4.3, log wage income scores are predicted by occupation, location, and other characteristics. Our conclusions are qualitatively similar using income scores and all four cohorts. We again find *Mulatto–White* gaps among sons which shrink as we add controls (from a −45.5% difference in wages to −21.1%) and which are larger than the *Black–Mulatto* gaps (which range from −9.1% in the

[52] See Chetty et al. (2020), Collins and Wanamaker (2022), Jácome et al. (2021), and Ward (2021) for related analysis showing lower rates of intergenerational mobility among African American relative to White individuals. Racial gaps in income due to discrimination also contributed to important differences in other important outcomes such as health (Eli et al., 2019).

[53] We note that significant wage compression occurred between the 1940 and 1950 censuses (Goldin and Margo, 1992).

general population to -2.6% when controlling for enumeration districts and family characteristics). Among our differently-classified brothers sample, we again find a small (but now marginally significant) gap of -1.6% (se=0.8%) in our preferred weighted BMS specification and a statistically insignificant gap in the opposite direction conditional on family fixed effects (0.6%, se=1.2%). Taken together, these results suggest that any gaps in earned or predicted income among differently-classified brothers were small relative to the overall population gaps.

We next analyze these same income and income score outcomes for daughters' spouses, conditional on marriage. We focus on analyzing the earnings of spouses (men) rather than of the women themselves because female labor force participation rates were low during this period. These results are reported in Tables 11–12 Panel B and in Figure 6. We find a *Black–Mulatto* gap in the 1940 income of daughters' spouses in the general population (the spouses of Black-coded daughters earned 11.8% less on average). This gap decreases to 7.9%, 7.2%, and 2.6% as we control for childhood state, enumeration district, and family characteristics, respectively. When comparing among differently-classified sisters in our weighted BMD specification, this gap mostly persists in magnitude but becomes less precisely estimated (1.8%, se=6.8%). Adding family fixed effects dramatically decreases the precision and changes the sign of this estimate, as it ultimately relies on a comparison of just 72 differently-classified sisters who are both linked and have spousal income.[54]

When similarly analyzing the predicted log wage income of daughters' spouses, we see similar gaps in the general population (the spouses of Black-coded daughters earned 9.3% less on average). This gap decreases to 5.2%, 4.8%, and 2.6% as we control for childhood state, enumeration district, and family characteristics respectively, and remains significant (and is somewhat larger) in our preferred weighted BMD specification (7.1%, se=2.0%). Adding family fixed effects leads to a somewhat smaller and less precisely estimated gap (6.0%, se=4.4%).[55] Together, these results are somewhat underpowered but suggest that differently-classified sisters with perceived darker skin tones may have married lower-earning men on average (conditional on marriage) than did their sisters with lighter perceived skin tones. These results provide additional evidence consistent with a marriage market penalty for African American women with darker perceived skin tones, though less definitively than in the case of marriage rates or spouse's education.

[54] Our estimates are similar when incorporating imputed non-wage income in Appendix Table A5 Panel B.

[55] Appendix Figure A4 shows that our conclusions are fairly robust to alternative income score constructions.

6.5 Gaps in migration

African Americans with different perceived skin tones might also have been differentially likely to move and/or to choose different destinations given differences across geographies that interact with perceived skin tone, potentially including variation in discrimination, the feasibility of passing as White, or in employment or social opportunities. Appendix Tables A6–A7 and Appendix Figure A8 report differences by perceived skin tone in migration patterns, considering two outcome measures: the propensity to have moved to a different state between a child's cohort year and *t+20*, and whether individuals lived in the South in *t+20*.[56] In the general population and conditional on child geography and family characteristics, Mulatto-coded children were more likely to migrate to a different state than were Black-coded or White-coded children. This *Black–Mulatto* gap in migration in the general population shrinks dramatically and becomes statistically insignificant when we restrict this analysis to differently-classified siblings using either our weighted BMS/ BMD specification or family fixed effect specification. Black-coded sons were more likely than Mulatto-coded sons to live in a southern state in *t+20*, with a marginally significant gap remaining among differently classified siblings in our weighted BMS specification and with a much smaller and insignificant gap in our family fixed effect specification. Black-coded daughters were also more likely than Mulatto-coded daughters to live in a southern state in *t+20* in the general population, but among differently classified sisters this gap significantly changes direction with Black-coded sisters being less likely than Mulatto-coded sisters to be in a southern state in *t+20*. We see no such gap among differently classified-sisters conditional on family fixed effects. Together these results suggest that perceived skin tone differences didn't significantly affect whether siblings migrated away from their initial state, and provide suggestive and somewhat mixed evidence regarding effects on which region individuals move to when migrating.

7 Discussion of identification and interpretation

7.1 Did siblings share the same biological parents?

We use a stringent family definition for which we can be fairly confident that two siblings within a family shared the same set of biological parents. We do so because we want to compare siblings who shared the same family-level access to resources and opportunities, and might be concerned that this access potentially differed

[56] Southern states are defined as: Texas, Louisiana, Mississippi, Alabama, Georgia, South Carolina, Florida, North Carolina, Tennessee, Virginia, Maryland, Arkansas, or the District of Columbia.

for half, step, or otherwise non-biological siblings. The high frequency of differently-classified parents in our BMS/BMD samples (discussed in Section 4.1) is also consistent with real differences in perceived skin tone among biological siblings; we might reasonably expect that households with differently-classified parents contained siblings with relatively large variation in perceived skin tone and/or that these households were coded by enumerators who were particularly responsive to variation in their racial classifications. Still, the possibility remains that at least some siblings in our sample might not have shared the same biological parents despite this observed overrepresentation and despite the sample-selection steps described in Section 4.1.

The possibility that some individuals whom we identify as siblings in fact had different parents does not in itself constitute a threat to our differently-classified-family identification strategy. As a thought exercise, one can imagine replicating our analysis if random children in the population had been allocated to families after they were born. For the observed within-family skin tone differences actually to be driven by different parentage, this would require both that parents treated non-biological children differently because they were not biological children (as in Case et al., 2000), and that having a different biological parent was correlated with skin tone. If parents did not change their behavior towards non-biological children or if non-biological children were equally likely to be classified Mulatto or Black, it should not have affected the differences by skin tone.

7.2 Inferring the role of families' opportunity access

By comparing outcome differences by perceived skin tone in the general population to those among differently-classified siblings, we can infer the role that systematic differences in families' access to opportunities and resources played in explaining the population gaps. Such inference, however, requires that the effects of being perceived as Mulatto versus Black, conditional on perfect measures of family opportunity access, were the same in the general population as they were between differently-classified siblings. This may have been true under certain assumptions that we discuss below. Under these assumptions, if contemporaneous skin tone discrimination drove outcome differences rather than systematic differences in family opportunity access, we would expect perceived skin tone stratification among siblings to have been roughly equal to stratification in the general population: $\hat{\beta}^f \approx \hat{\beta}^p$. In contrast, if systematic differences in families' opportunity and resource access drove outcome differences in the population, we would expect $\hat{\beta}^f \approx 0$. Our results for sons are broadly consistent with the latter case. We therefore consider three conditions that might have caused skin tone to operate differently across families rather than within our BMS/BMD samples.

Differences in perceived skin tone between Black- and Mulatto-coded siblings not equal to those in the population The difference in perceived skin tone between the average Black-coded and Mulatto-coded siblings may have differed from the perceived skin tone difference between the average Black-coded and Mulatto-coded children in the population. If Black- and Mulatto-coded siblings had smaller perceived skin tone differences (presumably because siblings tended to look alike), we should expect smaller outcome differences than in the population, where perceived skin tone differences were larger. Small skin tone differences between siblings would also exacerbate a concern, as laid out in Griliches (1979), that comparisons among siblings take away most of the true variation in the independent variable of interest (schooling in Griliches, 1979, perceived skin tone here), leaving the remaining total variation with a higher share of measurement error. However, enumerators had a strong tendency to classify all sons or daughters in a household with the same skin tone. This suggests that large differences in physical appearance or other social cues may have been required for siblings to be classified differently, in which case Black-coded and Mulatto-coded siblings in our sample may actually have had quite distinct skin tones.

Consistent with the idea of similar perceived skin tone differences among Black- vs. Mulatto-coded children within differently-classified families and in the general population, we show in Table 7 and discussed in Section 6.2 that the *Black–Mulatto* gap in future classification as White was similar in magnitude in our BMS/BMD samples and in the general population. Similar differences in Black- vs. Mulatto-coded perceived skin tone variation within differently-classified families offer support for our differently-classified family estimates as reasonable estimates for the causal effect of contemporaneous perceived skin tone discrimination on economic outcomes in the general population among African Americans.

We also use an empirical specification comparing the weighted populations of Black- vs. Mulatto-coded brothers from differently-classified families. This helps limit the attenuation concern associated with fixed effects described in Griliches (1979), and also generally produces estimates similar to specifications including family fixed effects when the latter have sufficient statistical power in our analysis.

Endogenous skin tone classification exacerbated within families Penner and Saperstein (2008), Freeman et al. (2011), Saperstein and Gullickson (2013), and Woo-Mora (2022) show that racial classification may depend on signals such as incarceration, unemployment, the way a person dresses, occupational status, and language. Skin tones themselves could also have in part reflected whether an individual worked outdoors or their clothing. If enumerators' racial classification was partially determined by factors that correlate with our outcomes of interest, estimates of the effect of perceived skin tone on outcomes can be biased. Our focus on comparisons by *childhood* perceived skin tone partially remedies this concern as children had not settled into occupations.

In line with this logic, in Appendix Figures A9 (for sons) and A10 (for daughters) we compare adult outcome gaps estimated in our baseline samples to gaps in subsamples based on the age at which childhood race was enumerated (separating children age 3–10 and 11–18). If an important part of enumerators' classifications were driven endogenously by aspects of a child's social presentation that developed over time and which may be correlated with future outcomes (such as perceptions of intelligence or markers of perceived future attractiveness), we might expect to see systematically larger gaps among the subsample classified after having had more time to develop these characteristics. We find similar patterns across all three samples, without systematic evidence of larger gaps among those classified at older ages (in either the general population or among differently-classified siblings).

Of course, we are unable to determine exactly which characteristics drove differences in racial classification among differently-classified siblings, as actual skin tone pigmentation and many other characteristics are unobservable to researchers. We can however sharpen our interpretation by testing for significant differences among differently-classified siblings in observable, *non-physical* characteristics that might drive persistent differences in perceived race, such as in the racial distinctiveness of a child's first name (following Cook et al., 2014). Defining names' racial distinctiveness as the share of children coded as Black or Mulatto among all same-gender children in a child's cohort, Appendix Table A8 shows that the first names of Black-coded and Mulatto-coded siblings are similarly distinctive on average in both our BMS and BMD samples.

One key factor that does systematically affect physical skin tone, even conditional on family, is age. Skin pigment typically continues to develop during the early months (and sometimes years) of life. Appendix Figure A11 Panel A illustrates that among families with Black- and/or Mulatto-coded sons, the youngest children are disproportionately likely to be classified as Mulatto. We therefore restrict our analysis throughout to children whose racial classification was conducted between the ages of 3 and 18. In Appendix Figure A11 Panel B, we show the share of Mulatto-coded sons by age among families with both a Black- and Mulatto-coded son age 3–18, where skin tone does not appear to have been strongly correlated with age. Regardless, we include age and birth order fixed effects in all baseline regressions. We also note that endogenous skin tone classification would (if anything) likely bias our estimates towards finding significant differences by perceived skin tone among differently-classified siblings, and we find no such differences among differently-classified brothers and only small childhood differences among differently-classified sisters. We also show qualitatively similar results across distinct age groups in Appendix Figures A9 and A10.

Systematic reallocation of resources or peer effects in the family Having a sibling of a different skin tone may have given rise to differential treatment that would not have arisen if there were no lighter- or darker-skinned sibling in the same household. If parents reallocated familial resources from darker-skinned children to lighter-skinned ones (perhaps perceiving higher returns on their investments), our within-family estimates of the effect of perceived skin tone would overstate the effects of discrimination as it operated in the general population. The observed gaps among sisters in educational attainment and marital outcomes could plausibly be explained by such a mechanism, with parents potentially investing less in the education of their daughters with perceived darker skin due to anticipation of discrimination toward these daughters on the marriage market. On the other hand, if parents wished to equalize the economic welfare of their children, they might have tried to counteract discrimination outside of the family by reallocating resources from lighter- to darker-skinned children, and our within-family estimates would understate the effect of perceived skin tone. To the extent that either response occurred, our estimates would still correctly identify the causal effect of perceived individual skin tone within differently-classified families (including the impact of this family allocation effect) but might not extrapolate to the effect in families with uniform classifications.

Peer effects between siblings may be another channel through which skin tone had a different effect on outcomes inside the family vs. in the population. The specific family trait of having children with different skin tone cannot be extrapolated to the general population. Children who grew up with a sibling of different perceived skin tone could have changed their own behavior, and/or their siblings'.

Both reallocation by parents and peer effects across siblings are unobservable to us as researchers. They are common potential confounders, however, in this type of sibling study.

8 Conclusion

We use full-count U.S. decennial census data from 1870–1940 to analyze differences in economic outcomes by race and perceived skin tone. To do so, we construct a panel data set that follows children from their childhood households to their adult outcomes and we compare children coded as White, Black, or Mulatto by census enumerators. We first document large educational and income gaps between African American and White individuals that continued throughout the period, as would be expected given the severe discrimination

African Americans have faced. The paper then documents smaller but economically significant gaps in these outcomes among African Americans with darker skin tones relative to lighter-skinned African Americans (as perceived by enumerators).

We next focus on estimating the extent to which these economic gaps among African Americans can be explained by contemporaneous racism and discrimination that differentiates across perceived skin tone differences, or instead by differences in families' access to opportunities and resources. These differences in access themselves likely reflect racism and discrimination in earlier generations or at the family-level. Our primary analysis answering this question compares differently-classified siblings from families in which census enumerators classified at least one child as Black while coding their same-gender sibling as Mulatto. Comparing the outcomes of differently-classified siblings allows us to estimate the causal impact of being perceived as Black relative to Mulatto during this time period, holding family opportunity and resource access constant so that we can separately account for the relative role of these two mechanisms. As discussed throughout the paper, these differences in perceived skin tone likely reflect a bundle of not only physical characteristics but also social and contextual cues. We find (in both the general population and among differently-classified siblings) that children coded as Mulatto were more likely than Black-coded children to be classified as White adults in future census years (in which the Mulatto category no longer existed). This difference in future classification as White suggests that whatever the bundle of physical and/or social characteristics that differed on average between Mulatto-coded and Black-coded children, this difference persisted across different time periods and census enumerators.

We find significantly lower educational attainment, marriage rates, and spouse's education among African American daughters perceived as having darker skin tones in differently-classified families relative to their Mulatto-coded sisters. The educational gaps are much smaller in magnitude than *Black–Mulatto* differences in the general population, suggesting that differences in families' opportunity and resource access explained the majority of the educational gap among African American girls with darker perceived skin tones (relative to African Americans perceived as lighter-skinned) but that ongoing discrimination still played a significant role in determining this gap. Importantly, the differences in marital outcomes show that Black-coded sisters faced a large penalty on the marriage market relative to their sisters with perceived lighter skin tones. This suggests different anticipated returns to educational investment as a potential mechanism for the observed gap in educational attainment among sisters. The gap in marital outcomes among differently-classified sisters is similar in magnitude to the general population gap, suggesting contemporaneous perceived skin tone discrimination as the primary driver of these marriage market gaps, due perhaps to discriminatory beauty standards (Hill, 2002b; Hamilton et al., 2009).

In contrast, we find that African American brothers who differed in perceived skin tone had similar outcomes on average in terms of education, marriage prospects, and earnings. Our point estimates of this gap are generally near zero, and their 95% confidence intervals can rule out that the *Black–Mulatto* income gap among differently-classified brothers was more than about a third as large as the analogous *Black–Mulatto* gap in the population. These results suggest that conditional on being perceived as African American, the differences in 1870–1940 outcomes among men with different third-party categorization of skin tones were likely driven primarily by variation in families' access to resources and opportunities, rather than resulting from perceptions of an individual's skin tone. Differences in families' opportunity access likely reflect variation in discrimination faced by earlier generations or at the family-level, and alleviating these gaps in outcomes may require policies specifically targeting these underlying disparities in access to opportunities and resources.

We note that skin tone variation among African Americans could have larger impacts during other time periods or in other societal contexts less beholden to "one-drop" racial classification rules, or through the long-term accumulation of even small effects over generations. Our analysis also shows that throughout the late 19th and early 20th centuries, White vs. Mulatto/Black gaps in all outcomes studied were much larger than Mulatto vs. Black gaps, consistent with the abundant qualitative and quantitative evidence of discrimination towards African Americans.

Our findings regarding the gendered and intersectional nature of perceived skin tone effects among African Americans resonate with a rich literature, which we contribute to by quantifying differences in economic outcomes across perceived skin tones while controlling for family background differences by comparing differently-classified siblings. The gendered nature of these perceived skin tone effects could reflect different stereotypical gender roles in society during the time period we study. More broadly, our paper contributes to our understanding of racial equality, and of the important and understudied topics of colorism and skin tone. As Monk (2021) writes: "in a society with increasing intermarriage and 'multiracial' children—in which everyone will look mixed—the importance of skin tone should only increase over time as ethnoracial categories become even more heterogenous with respect to phenotype."

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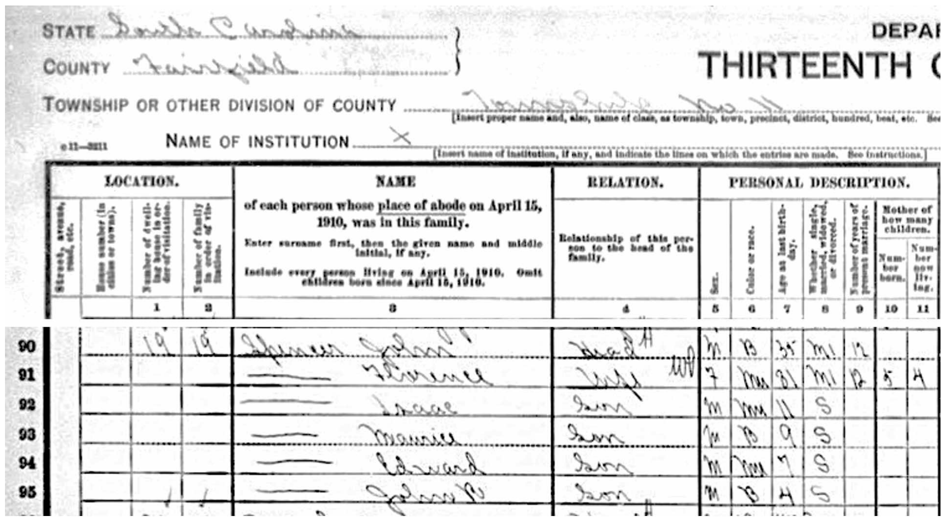
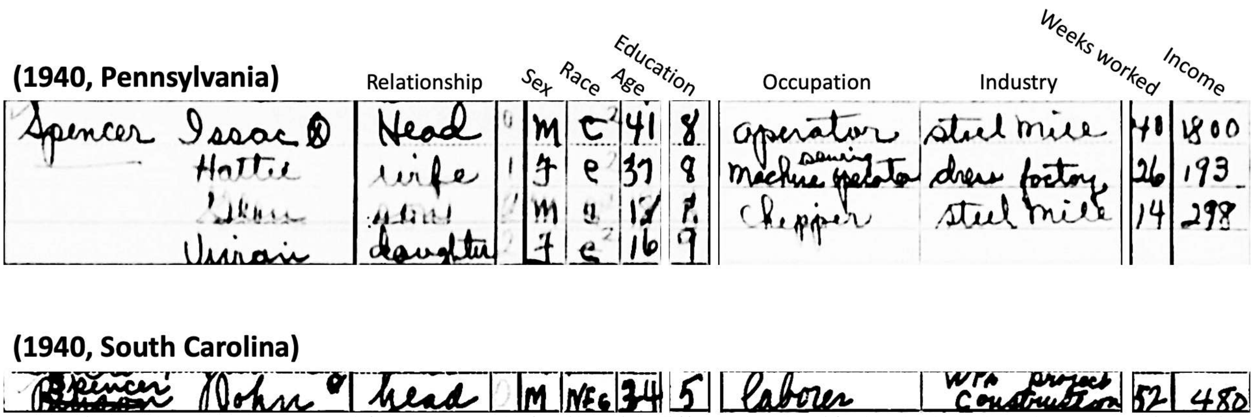
9 Figures and tables

Figure 1: Brothers Isaac and John Spencer in 1910 and 1940

(a) The Spencer family in 1910



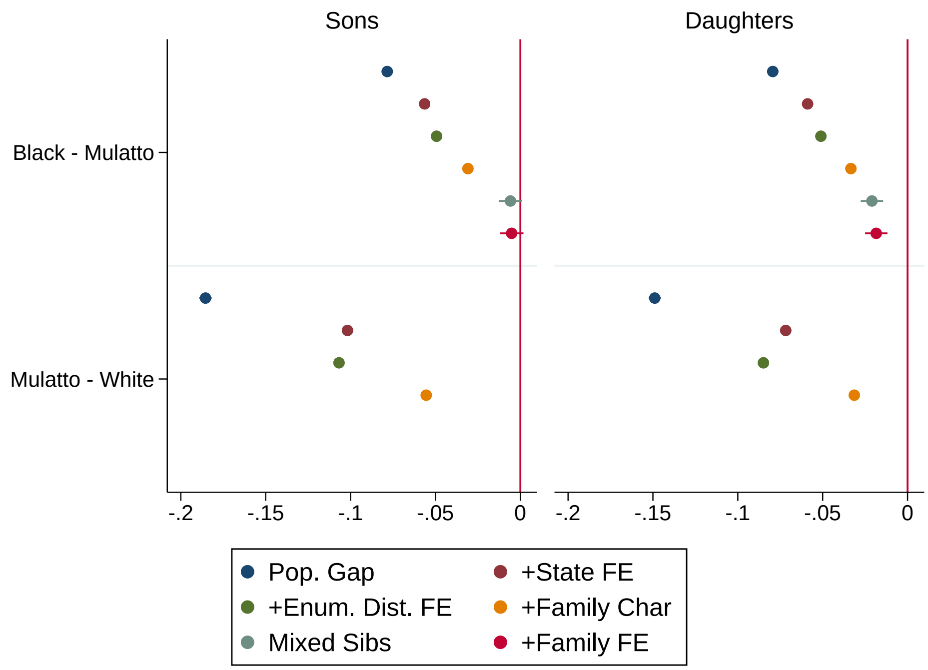
(b) Isaac and John Spencer in 1940



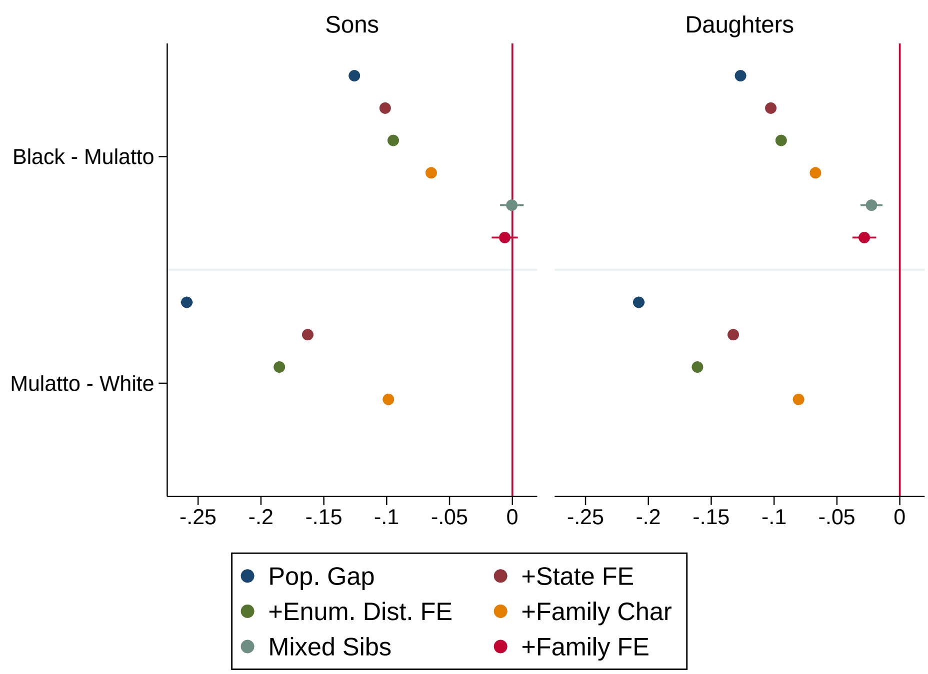
Note: Figure shows census record for an example family with differently-classified (Black and Mulatto) sons, including their 1910 records as children (Panel A) and their 1940 census records as adults (Panel B).

Figure 2: Gaps in childhood education, by perceived child race and gender

(a) School attendance



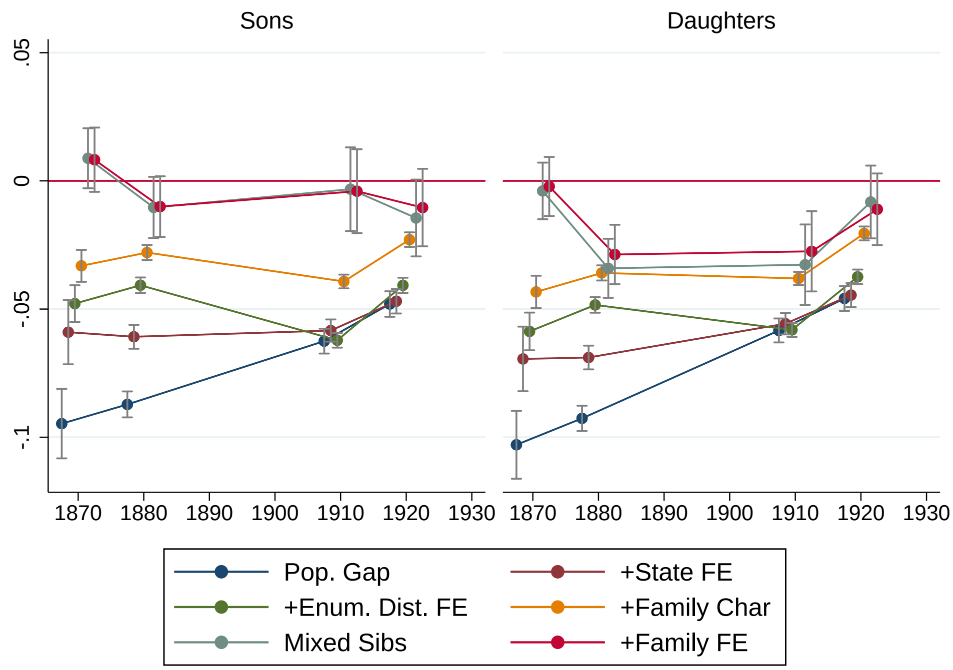
(b) Literacy



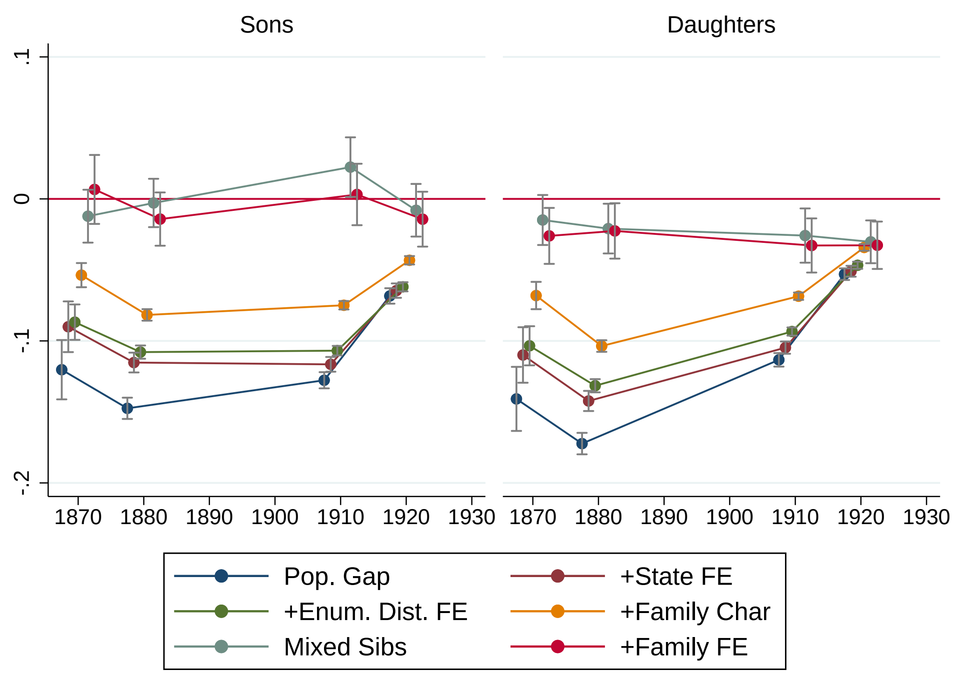
Note: Figure shows regression coefficients and 95% confidence intervals documenting racial gaps in childhood school attendance (Panel A) and literacy (Panel B). See Section 6.1 for detail and Tables 5 and 6 for corresponding regression tables.

Figure 3: *Black–Mulatto* gaps in childhood education, by child cohort and gender

(a) School attendance



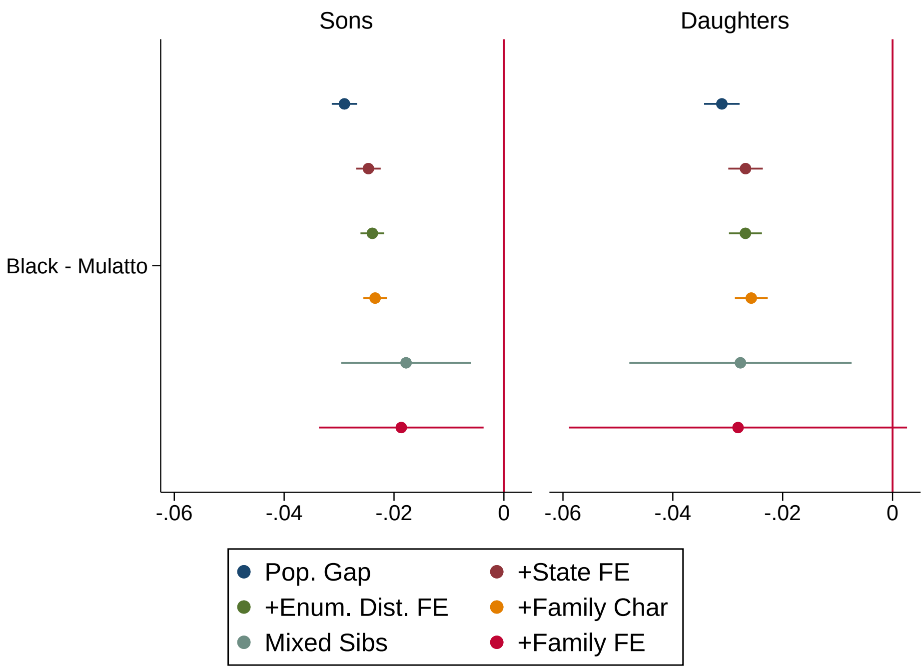
(b) Literacy



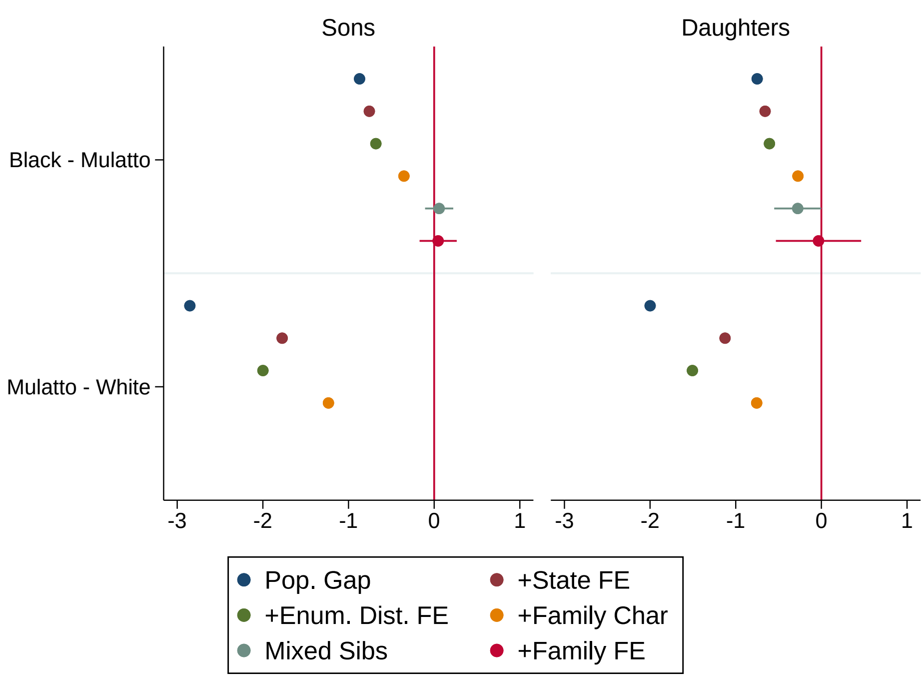
Note: Figure shows regression coefficients and 95% confidence intervals documenting Black–Mulatto gaps in childhood school attendance (Panel A) and literacy (Panel B) by gender and child cohort. See Section 6.1 for detail.

Figure 4: Gaps in perceived race and educational attainment as an adult, by perceived child race and gender

(a) Classification as White in *t+20*



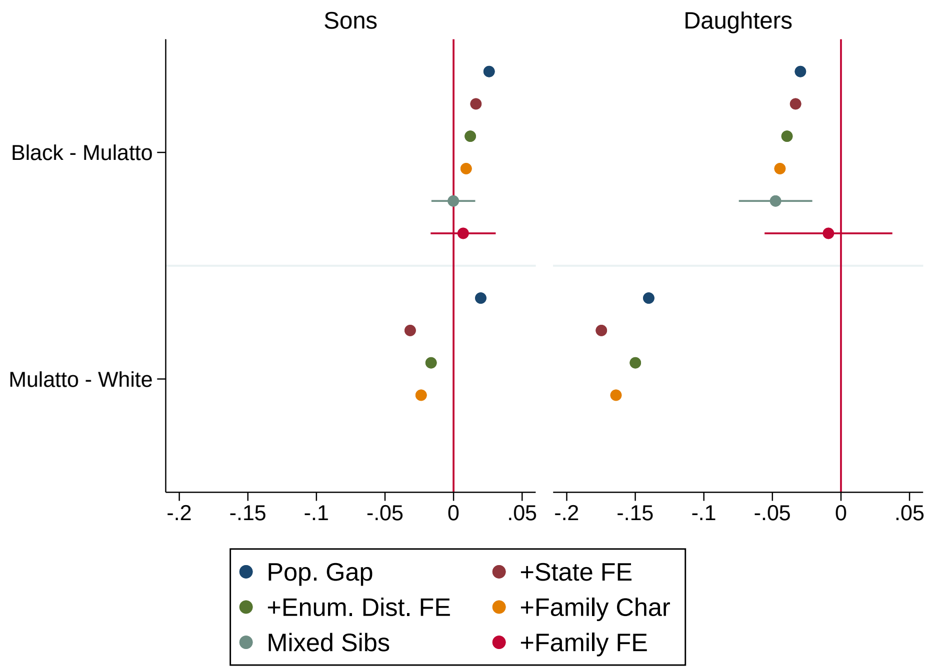
(b) Years of education by 1940



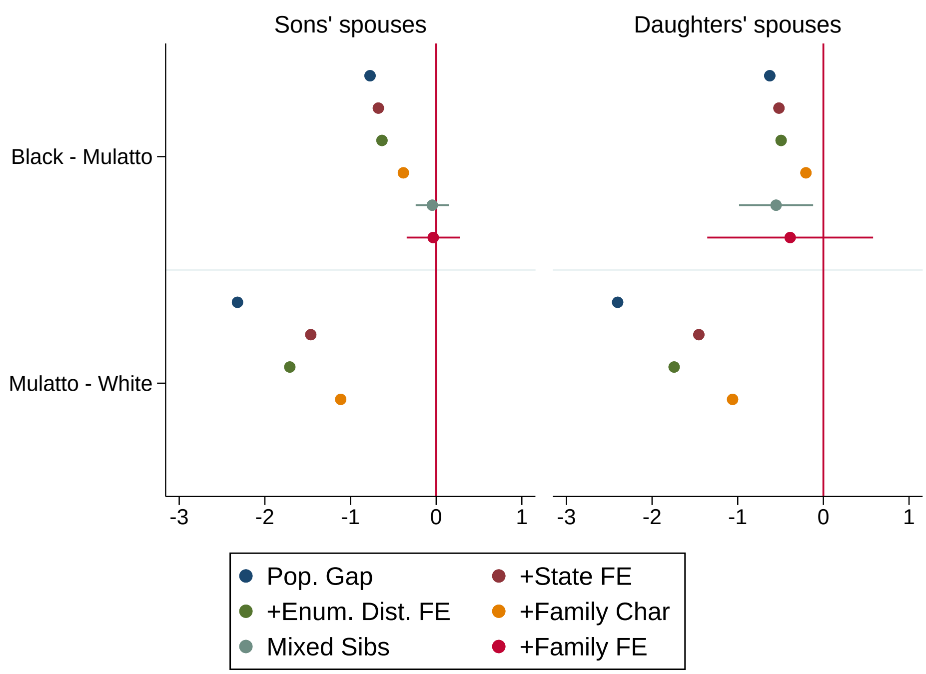
Note: Figure shows regression coefficients and 95% confidence intervals documenting gaps by perceived childhood race and gender in an indicator for classification as a White adult in *t+20* (Panel A) and for years of education attained by 1940 (Panel B). See Sections 6.2 and 6.3 for detail, and see Tables 7 and 8 for corresponding regression tables.

Figure 5: Gaps in marital outcomes as an adult, by perceived child race and gender

(a) Married in *t+20*



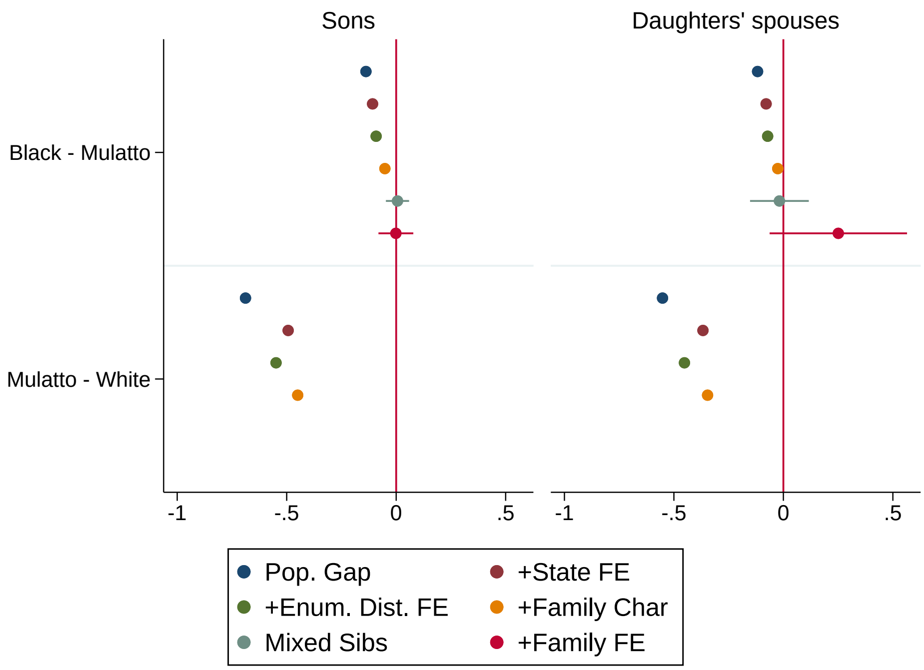
(b) Spouses' years of education by 1940



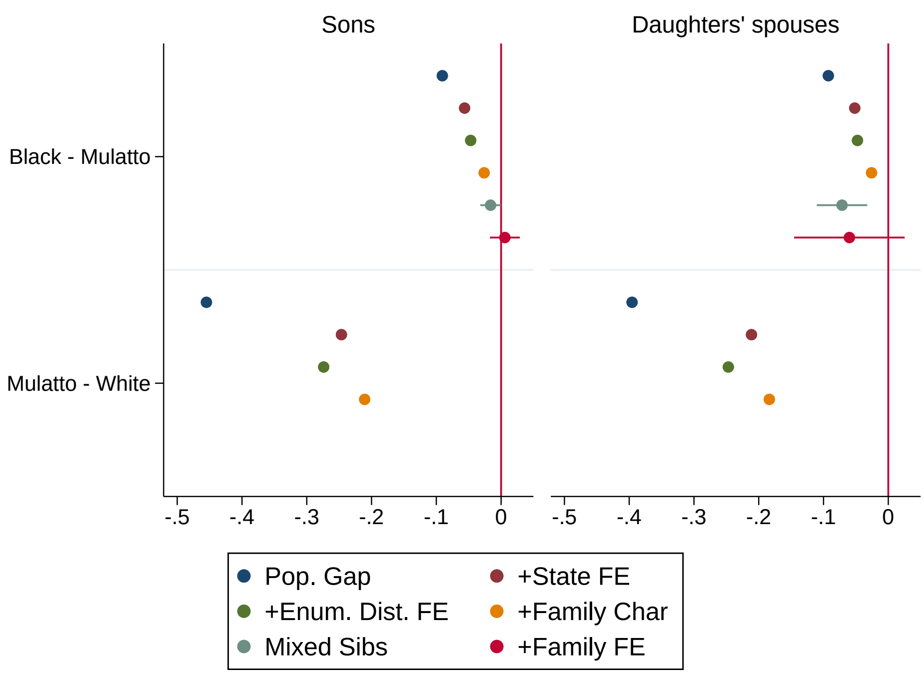
Note: Figure shows regression coefficients and 95% confidence intervals documenting gaps by perceived childhood race and gender in an indicator for being married twenty years later (Panel A) and in the years of education attained by one's *spouse* by 1940 (Panel B). See Section 6.3 for detail, and see Tables 9 and 10 for corresponding regression tables.

Figure 6: Gaps in income and income scores, by perceived child race and gender

(a) Log wage income in 1940



(b) Log wage income score in *t+20*



Note: Figure shows regression coefficients and 95% confidence intervals documenting gaps by perceived childhood race and gender in actual log wage income in 1940 and in log wage income scores in *t+20*. See Section 6.4 for detail and Tables 11 and 12 for corresponding regression tables.

Table 1: Parents' perceived races in the population and among differently-classified families

(a) Attributes of African American couples, by perceived race

Parents' Race					
Husband	Wife	% of Couples	N Children	% Black	% Female
Black	Black	78.6%	2.20	98.7%	49.7%
Black	Mulatto	6.6%	2.19	34.2%	49.6%
Mulatto	Black	3.5%	2.34	53.0%	49.2%
Mulatto	Mulatto	11.3%	2.15	3.0%	49.6%

(b) Parent perceived race frequencies among families with differently-classified Black and Mulatto sons (BMS sample)

	Mother				
Father	Black	Mulatto	White	Other	Total
Black	29.2%	38.2%	0.3%	-	67.7%
Mulatto	20.8%	10.6%	0.0%	0.0%	31.4%
White	0.4%	0.2%	0.3%	-	0.9%
Other	0.0%	-	-	-	0.0%
Total	50.4%	49.0%	0.6%	0.0%	100.0%

(c) Parent perceived race frequencies among families with differently-classified Black and Mulatto daughters (BMD sample)

	Mother				
Father	Black	Mulatto	White	Other	Total
Black	35.2%	35.7%	0.3%	-	71.2%
Mulatto	19.1%	8.9%	0.0%	-	28.0%
White	0.4%	0.1%	0.4%	-	0.9%
Other	-	-	-	-	-
Total	54.6%	44.7%	0.7%	-	100.0%

Note: Panel A shows differences in perceived race frequency and characteristics among African-American couples in the general population in years 1870, 1880, 1910, and 1920. African-American couples are defined as two spouses who were both classified as Black/Mulatto. Panel B shows parent perceived race frequencies among families containing at least one Black-coded and at least one Mulatto-coded son (BMS sample) both age 3–18, while Panel C shows the analogous panel for differently-classified Black and Mulatto daughters (BMD sample). These panels again cover the 1870, 1880, 1910, and 1920 census years.

Table 2: Attributes of families with same vs. differently-classified siblings

(a) Sons

Sample:	Single-Classification Families			BMS
Household classification:	Black	Mulatto		Both
	(1)	(2)		(3)
Sons in family	3.35	3.34	$<$	3.60
Age	9.53	9.51	$<$	9.72
Able to write	0.54	0.70	$>$	0.49
Head is literate	0.44	0.60	$>$	0.40
Head's occupation:				
- Farm laborer	0.19	0.11	$<$	0.19
- Farmer	0.54	0.52	$\ni$	0.53
- Laborer (not farm)	0.14	0.13	$\ni$	0.13
- In home services	0.00	0.00	$<$	0.00
County urban	0.15	0.20	$>$	0.12
County Afr. Americans	0.47	0.40	$\ni$	0.46
Observations	2,229,561	432,910		50,737

(b) Daughters

Sample:	Single-Classification Families			BMD
Household classification:	Black	Mulatto		Both
	(1)	(2)		(3)
Daughters in family	3.32	3.31	$<$	3.56
Age	9.47	9.47	$<$	9.76
Able to write	0.60	0.76	$>$	0.55
Head is literate	0.45	0.61	$>$	0.39
Head's occupation:				
- Farm laborer	0.19	0.11	$<$	0.20
- Farmer	0.53	0.51	$>$	0.50
- Laborer (not farm)	0.14	0.13	$<$	0.15
- In home services	0.00	0.00	$<$	0.00
County urban	0.16	0.21	$>$	0.13
County Afr. Americans	0.47	0.40	$\ni$	0.46
Observations	2,187,818	443,615		54,240

Note: Panel A shows mean characteristics for the sample of sons in households with at least two sons age 3–18 all Black-coded or all Mulatto-coded (columns 1–2) and for sons in BMS households with at least one Black- and one Mulatto-coded son age 3–18 (column 3). Panel B shows analogous results for daughters.

Table 3: Characteristics of linked vs. all searched sons

	All obs	Census Tree				ABE Exact Conservative			
Observations (count)	49,621,196	25,215,551				8,473,348			
Black+Mulatto in county	6,241,828	1,505,360				417,070			
Black+Mulatto in family	50,737	5,661				622			
		Unweighted		Weighted		Unweighted		Weighted	
Child characteristics	Mean	Mean	Difference	Mean	Difference	Mean	Difference	Mean	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Black	.105	.0481	-.0568***	.0993	-.00556***	.0398	-.065***	.101	-.00352***
			(.0000605)		(.0000865)		(.000105)		(.000167)
Mulatto	.0212	.012	-.00921***	.0231	.00183***	.0104	-.0108***	.0237	.00247***
			(.0000287)		(.000043)		(.0000495)		(.0000823)
Age	10.1	10.1	-.0481***	10.1	-.00925***	10.1	-.0206***	10.1	-.00348**
			(.000917)		(.000985)		(.00158)		(.00177)
Able to write	.888	.931	.043***	.897	.00825***	.948	.0591***	.899	.011***
			(.0000861)		(.00011)		(.000148)		(.00021)
Sons in household	2.17	2.24	.0747***	2.17	.00836***	2.21	.0387***	2.18	.00894***
			(.000268)		(.000287)		(.000463)		(.000523)
Head is literate	.852	.902	.05***	.854	.00216***	.923	.0707***	.852	.000228
			(.0000705)		(.000091)		(.000122)		(.000177)
Head's occupation:									
- Farm laborer	.0475	.0336	-.0139***	.0461	-.00146***	.031	-.0165***	.0471	-.000449***
			(.0000424)		(.0000545)		(.0000732)		(.000104)
- Farmer	.383	.402	.019***	.384	.000569***	.388	.00515***	.384	.000511***
			(.0000969)		(.000103)		(.000167)		(.000186)
- Laborer (not farm)	.0896	.0756	-.014***	.0892	-.00046***	.0686	-.021***	.0895	-.000152
			(.0000569)		(.0000664)		(.0000983)		(.000124)
County urban	.368	.379	.0103***	.364	-.00445***	.394	.0257***	.363	-.00506***
			(.0000724)		(.0000762)		(.000125)		(.000135)
Census Year	1902	1904	2.78***	1902	.177***	1906	4.06***	1902	.053***
			(.00387)		(.00443)		(.00669)		(.008)

Note: Table shows differences in mean characteristics for our initial and linked samples of sons. Column (1) reports counts and mean statistics among our initial sample prior to linking, including children from 1870, 1880, 1910, and 1920. Columns (2) and (4) denote mean characteristics under our Census Tree links when linking to *t+20*, without and with inverse probability link weights respectively (see Section 4.2 for detail on linking and weights). Columns (3) and (5) report differences between our initial and linked means, without and with weights respectively. Columns (6)–(9) repeats the above for the ABE Exact Conservative linking algorithm. Standard errors are clustered by (enumeration district)×(year). See Appendix Table A1 for an analogous table comparing the characteristics of linked vs. all searched daughters.

Table 4: Average characteristics, by perceived child race and gender

(a) Sons

Perceived Race:	White (1)	Black (2)	Mulatto (3)
Observations	43,366,331	5,201,963	1,052,902
Age	10.14	10.02	9.95
In school	0.67	0.40	0.50
Literate	0.93	0.54	0.68
In South	0.22	0.88	0.82
Father present	0.86	0.71	0.72
Mother present	0.91	0.82	0.83
Sons in family	2.18	2.06	2.06
Head is literate	0.91	0.45	0.58
Head's occupation:			
- Farm laborer	0.03	0.18	0.11
- Farmer	0.37	0.46	0.43
- Laborer (not farm)	0.08	0.13	0.12
- In home services	0.02	0.02	0.03
County urban	0.40	0.17	0.23
County Afr. Americans	0.08	0.46	0.40
Is White in t+20	0.99	0.12	0.15
Years of education in 1940	9.54	5.91	6.74
Married in t+20	0.68	0.71	0.68
Spouse years of educ. in 1940	9.75	6.72	7.46
Is employed in 1940	0.93	0.92	0.92
Log wage income in 1940	6.85	6.03	6.19
Log wage income score in t+20	6.74	6.18	6.27

(b) Daughters

Perceived Race:	White (1)	Black (2)	Mulatto (3)
Observations	42,679,300	5,209,213	1,133,391
Age	10.19	10.10	10.18
In school	0.67	0.43	0.53
Literate	0.95	0.61	0.74
In South	0.22	0.88	0.82
Father present	0.85	0.70	0.69
Mother present	0.89	0.81	0.81
Daughters in family	2.12	2.03	2.00
Head is literate	0.91	0.46	0.59
Head's occupation:			
- Farm laborer	0.03	0.18	0.11
- Farmer	0.35	0.45	0.41
- Laborer (not farm)	0.08	0.14	0.13
- In home services	0.02	0.02	0.03
County urban	0.40	0.18	0.24
County Afr. Americans	0.08	0.46	0.40
Is White in t+20	0.99	0.18	0.21
Years of education in 1940	9.86	7.20	7.90
Married in t+20	0.67	0.48	0.51
Spouse years of educ. in 1940	9.09	6.04	6.65
Spouse is employed in 1940	0.96	0.95	0.95
Spouse log wg. inc. in 1940	6.95	6.28	6.41
Spouse log wg. inc. score in t+20	6.82	6.34	6.44

Note: Table shows counts and mean characteristics by perceived child race and gender in 1870, 1880, 1910, and 1920. The bottom portion of each panel includes adult outcomes identified by the Census Tree links. Panels A and B analyze means among sons and daughters respectively.

Table 5: Gaps in childhood school attendance, by perceived child race and gender

(a) Sons

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.078***	-0.056***	-0.049***	-0.031***	-0.006*	-0.005
	(0.002)	(0.001)	(0.001)	(0.001)	(0.004)	(0.004)
Mulatto - White	-0.185***	-0.102***	-0.107***	-0.055***		
	(0.002)	(0.001)	(0.001)	(0.001)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMS Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	42,581,534	42,581,534	42,581,338	42,581,338	42,581,338	20,834,013
R-Squared	0.296	0.328	0.390	0.401	0.401	0.734
Count Black	4,462,185	4,462,185	4,462,183	4,462,183	4,462,183	1,795,483
Count Mulatto	898,376	898,376	898,374	898,374	898,374	363,150
Count White	37,220,973	37,220,973	37,220,781	37,220,781	37,220,781	18,675,380
Mean[Outcome | Black]	0.398	0.398	0.398	0.398	0.398	0.429
Mean[Outcome | Mulatto]	0.497	0.497	0.497	0.497	0.497	0.542
Mean[Outcome | White]	0.673	0.673	0.673	0.673	0.673	0.708
Count Mixed Siblings	43,551	43,551	43,551	43,551	43,551	38,914

(b) Daughters

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.079***	-0.059***	-0.051***	-0.033***	-0.021***	-0.018***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.003)	(0.003)
Mulatto - White	-0.149***	-0.072***	-0.085***	-0.031***		
	(0.002)	(0.001)	(0.001)	(0.001)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMD Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	42,135,574	42,135,574	42,135,319	42,135,319	42,135,319	19,834,933
R-Squared	0.300	0.330	0.390	0.403	0.403	0.733
Count Black	4,471,866	4,471,866	4,471,851	4,471,851	4,471,851	1,753,715
Count Mulatto	973,631	973,631	973,629	973,629	973,629	372,353
Count White	36,690,077	36,690,077	36,689,839	36,689,839	36,689,839	17,708,865
Mean[Outcome | Black]	0.428	0.428	0.428	0.428	0.428	0.464
Mean[Outcome | Mulatto]	0.525	0.525	0.525	0.525	0.525	0.581
Mean[Outcome | White]	0.670	0.670	0.670	0.670	0.670	0.716
Count Mixed Siblings	46,616	46,616	46,616	46,616	46,616	41,594

Standard errors in parentheses

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

Note: Table shows regressions measuring gaps in an indicator for childhood school attendance, by perceived child race among children age 5–18 in 1870, 1880, 1910, and 1920. Columns (1)–(4) follow Equation 1, starting with age and birth order fixed effects and adding controls for state fixed effects, enumeration district fixed effects, and family characteristics in columns (2)–(4), respectively. See Footnote 44 for detail regarding the set of family characteristics included as controls. Column (5) implements Equation 3 by further adding BMS/BMD race interaction terms and weights such that our *Black–Mulatto* coefficient measures differences among differently-classified siblings with approximately equal weighted distributions of family characteristics. Column (6) adds family fixed effects to the specification in column (4). Panels A and B analyze differences among sons and daughters respectively. The bottom part of each panel presents observation counts and mean outcome variables by perceived child race. All non-race controls are interacted with cohort fixed effects. Standard errors are clustered by (enumeration district)×(year).

Table 6: Gaps in childhood literacy, by perceived child race and gender

(a) Sons

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.126***	-0.101***	-0.095***	-0.065***	-0.000	-0.006
	(0.002)	(0.002)	(0.001)	(0.001)	(0.005)	(0.005)
Mulatto - White	-0.259***	-0.163***	-0.185***	-0.099***		
	(0.002)	(0.002)	(0.002)	(0.001)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMS Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	26,231,042	26,231,042	26,230,773	26,230,773	26,230,773	9,889,376
R-Squared	0.209	0.297	0.390	0.447	0.447	0.834
Count Black	2,715,359	2,715,359	2,715,353	2,715,353	2,715,353	812,061
Count Mulatto	541,312	541,312	541,311	541,311	541,311	164,481
Count White	22,974,371	22,974,371	22,974,109	22,974,109	22,974,109	8,912,834
Mean[Outcome | Black]	0.542	0.542	0.542	0.542	0.542	0.552
Mean[Outcome | Mulatto]	0.677	0.677	0.677	0.677	0.677	0.698
Mean[Outcome | White]	0.934	0.934	0.934	0.934	0.934	0.934
Count Mixed Siblings	25,233	25,233	25,233	25,233	25,233	15,551

(b) Daughters

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.127***	-0.103***	-0.094***	-0.067***	-0.023***	-0.028***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.004)	(0.005)
Mulatto - White	-0.208***	-0.132***	-0.161***	-0.081***		
	(0.002)	(0.002)	(0.002)	(0.001)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMD Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	26,099,609	26,099,609	26,099,283	26,099,283	26,099,283	9,215,171
R-Squared	0.199	0.293	0.384	0.445	0.445	0.840
Count Black	2,734,160	2,734,160	2,734,143	2,734,143	2,734,143	776,931
Count Mulatto	601,325	601,325	601,323	601,323	601,323	166,904
Count White	22,764,124	22,764,124	22,763,817	22,763,817	22,763,817	8,271,336
Mean[Outcome | Black]	0.606	0.606	0.606	0.606	0.606	0.618
Mean[Outcome | Mulatto]	0.742	0.742	0.742	0.742	0.742	0.765
Mean[Outcome | White]	0.947	0.947	0.947	0.947	0.947	0.948
Count Mixed Siblings	27,246	27,246	27,246	27,246	27,246	16,476

Standard errors in parentheses

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

Note: Table shows regressions measuring gaps in an indicator for a child's ability to read and write, by perceived child race among children age 10-18 in 1870, 1880, 1910, and 1920. Columns (1)–(4) follow Equation 1, starting with age and birth order fixed effects and adding controls for state fixed effects, enumeration district fixed effects, and family characteristics in columns (2)–(4) respectively. See Footnote 44 for detail regarding the set of family characteristics included as controls. Column (5) implements Equation 3 by further adding BMS/BMD race interaction terms and weights such that our *Black–Mulatto* coefficient measures differences among differently-classified siblings with approximately equal weighted distributions of family characteristics. Column (6) adds family fixed effects to the specification in column (4). Panels A and B analyze differences among sons and daughters respectively. The bottom part of each panel presents observation counts and mean outcome variables by perceived child race. All non-race controls are interacted with cohort fixed effects. Standard errors are clustered by (enumeration district)×(year).

Table 7: Gaps in classification as White adult in *t+20*, by perceived child race and gender

(a) Sons

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.029***	-0.025***	-0.024***	-0.023***	-0.018***	-0.019**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.006)	(0.008)
Mulatto - White	-0.839***	-0.816***	-0.803***	-0.800***		
	(0.001)	(0.001)	(0.001)	(0.001)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMS Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	25,096,715	25,096,715	25,096,331	25,096,331	25,096,331	10,891,807
R-Squared	0.797	0.799	0.803	0.804	0.804	0.930
Count Black	1,205,578	1,205,578	1,205,567	1,205,567	1,205,567	329,765
Count Mulatto	300,498	300,498	300,494	300,494	300,494	91,804
Count White	23,590,639	23,590,639	23,590,270	23,590,270	23,590,270	10,470,238
Mean[Outcome | Black]	0.124	0.124	0.124	0.124	0.124	0.084
Mean[Outcome | Mulatto]	0.153	0.153	0.153	0.153	0.153	0.129
Mean[Outcome | White]	0.993	0.993	0.993	0.993	0.993	0.995
Count Mixed Siblings	13,370	13,370	13,370	13,370	13,370	5,621

(b) Daughters

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.031***	-0.027***	-0.027***	-0.026***	-0.028***	-0.028*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.010)	(0.016)
Mulatto - White	-0.783***	-0.759***	-0.751***	-0.746***		
	(0.002)	(0.002)	(0.002)	(0.002)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMD Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	17,084,221	17,084,221	17,083,674	17,083,673	17,083,673	6,244,781
R-Squared	0.735	0.737	0.747	0.747	0.747	0.915
Count Black	540,021	540,021	540,000	540,000	540,000	90,408
Count Mulatto	153,423	153,423	153,417	153,417	153,417	30,626
Count White	16,390,777	16,390,777	16,390,257	16,390,256	16,390,256	6,123,747
Mean[Outcome | Black]	0.178	0.178	0.178	0.178	0.178	0.119
Mean[Outcome | Mulatto]	0.209	0.209	0.209	0.209	0.209	0.192
Mean[Outcome | White]	0.992	0.992	0.992	0.992	0.992	0.995
Count Mixed Siblings	5,862	5,862	5,862	5,862	5,862	1,470

Standard errors in parentheses

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

Note: Table shows regressions measuring gaps in an indicator for future classification as White two decades later, by perceived child race among children age 3–18 in 1870, 1880, 1910, and 1920. Columns (1)–(4) follow Equation 1, starting with age and birth order fixed effects and adding controls for state fixed effects, enumeration district fixed effects, and family characteristics in columns (2)–(4) respectively. See Footnote 44 for detail regarding the set of family characteristics included as controls. Column (5) implements Equation 3 by further adding BMS/BMD race interaction terms and weights such that our *Black–Mulatto* coefficient measures differences among differently-classified siblings with approximately equal weighted distributions of family characteristics. Panels A and B analyze differences among sons and daughters respectively. The bottom part of each panel presents observation counts and mean outcome variables by perceived child race. All non-race controls are interacted with cohort fixed effects. Observations are weighted by their inverse link probabilities as described in Section 4.2. Standard errors are clustered by (enumeration district)×(year). Observations in which a child was identified as a race other than White or Black as an adult are dropped. All controls come from the child's census year, rather than from their linked adult self.

Table 8: Gaps in years of educational attainment by 1940, by perceived child race and gender

(a) Sons

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.871***	-0.757***	-0.681***	-0.353***	0.058	0.046
	(0.018)	(0.016)	(0.012)	(0.011)	(0.084)	(0.111)
Mulatto - White	-2.852***	-1.775***	-1.999***	-1.234***		
	(0.019)	(0.017)	(0.013)	(0.012)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMS Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	16,916,541	16,916,541	16,916,179	16,916,179	16,916,179	7,067,969
R-Squared	0.127	0.179	0.292	0.349	0.349	0.770
Count Black	766,020	766,020	766,008	766,008	766,008	209,067
Count Mulatto	206,522	206,522	206,519	206,519	206,519	62,126
Count White	15,943,999	15,943,999	15,943,652	15,943,652	15,943,652	6,796,776
Mean[Outcome | Black]	5.753	5.753	5.753	5.753	5.753	5.817
Mean[Outcome | Mulatto]	6.565	6.565	6.565	6.565	6.565	6.680
Mean[Outcome | White]	9.474	9.474	9.474	9.474	9.474	9.348
Count Mixed Siblings	6,667	6,667	6,667	6,667	6,667	2,916

(b) Daughters

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.750***	-0.657***	-0.606***	-0.274***	-0.276**	-0.034
	(0.021)	(0.019)	(0.016)	(0.015)	(0.140)	(0.254)
Mulatto - White	-1.999***	-1.125***	-1.506***	-0.755***		
	(0.021)	(0.019)	(0.017)	(0.015)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMD Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	11,066,989	11,066,989	11,066,496	11,066,496	11,066,496	3,645,889
R-Squared	0.091	0.138	0.257	0.316	0.316	0.774
Count Black	353,569	353,569	353,550	353,550	353,550	54,232
Count Mulatto	103,177	103,177	103,170	103,170	103,170	18,588
Count White	10,610,243	10,610,243	10,609,776	10,609,776	10,609,776	3,573,069
Mean[Outcome | Black]	7.043	7.043	7.043	7.043	7.043	7.525
Mean[Outcome | Mulatto]	7.712	7.712	7.712	7.712	7.713	8.292
Mean[Outcome | White]	9.780	9.780	9.780	9.780	9.780	9.776
Count Mixed Siblings	2,975	2,975	2,975	2,975	2,975	648

Standard errors in parentheses

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

Note: Table shows regressions measuring gaps in a child's years of education by 1940, by perceived child race among children age 3–18 in 1910 and 1920. Columns (1)–(4) follow Equation 1, starting with age and birth order fixed effects and adding controls for state fixed effects, enumeration district fixed effects, and family characteristics in columns (2)–(4) respectively. See Footnote 44 for detail regarding the set of family characteristics included as controls. Column (5) implements Equation 3 by further adding BMS/BMD race interaction terms and weights such that our *Black–Mulatto* coefficient measures differences among differently-classified siblings with approximately equal weighted distributions of family characteristics. Panels A and B analyze differences among sons and daughters respectively. The bottom part of each panel presents observation counts and mean outcome variables by perceived child race. All non-race controls are interacted with cohort fixed effects. Observations are weighted by their inverse link probabilities as described in Section 4.2. Standard errors are clustered by (enumeration district)×(year). All controls come from the child's census year, rather than from their linked adult self.

Table 9: Gaps in marriage rates in *t+20*, by perceived child race and gender

(a) Sons

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	0.026***	0.016***	0.012***	0.009***	-0.000	0.007
	(0.001)	(0.001)	(0.001)	(0.001)	(0.008)	(0.012)
Mulatto - White	0.020***	-0.032***	-0.016***	-0.024***		
	(0.001)	(0.001)	(0.001)	(0.001)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMS Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	25,106,397	25,106,397	25,106,012	25,106,012	25,106,012	10,896,899
R-Squared	0.116	0.129	0.149	0.150	0.150	0.595
Count Black	1,206,427	1,206,427	1,206,416	1,206,416	1,206,416	330,125
Count Mulatto	301,405	301,405	301,401	301,401	301,401	92,259
Count White	23,598,565	23,598,565	23,598,195	23,598,195	23,598,195	10,474,515
Mean[Outcome | Black]	0.723	0.723	0.723	0.723	0.723	0.688
Mean[Outcome | Mulatto]	0.694	0.694	0.694	0.694	0.694	0.657
Mean[Outcome | White]	0.689	0.689	0.689	0.689	0.689	0.676
Count Mixed Siblings	13,376	13,376	13,376	13,376	13,376	5,625

(b) Daughters

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.030***	-0.033***	-0.039***	-0.044***	-0.048***	-0.009
	(0.002)	(0.002)	(0.002)	(0.002)	(0.014)	(0.024)
Mulatto - White	-0.140***	-0.175***	-0.150***	-0.164***		
	(0.002)	(0.002)	(0.002)	(0.002)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMD Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	17,090,136	17,090,136	17,089,585	17,089,584	17,089,584	6,247,274
R-Squared	0.077	0.108	0.150	0.155	0.155	0.647
Count Black	540,466	540,466	540,445	540,445	540,445	90,536
Count Mulatto	153,984	153,984	153,977	153,977	153,977	30,810
Count White	16,395,686	16,395,686	16,395,163	16,395,162	16,395,162	6,125,928
Mean[Outcome | Black]	0.483	0.483	0.483	0.483	0.483	0.418
Mean[Outcome | Mulatto]	0.516	0.516	0.516	0.516	0.516	0.480
Mean[Outcome | White]	0.676	0.676	0.676	0.676	0.676	0.671
Count Mixed Siblings	5,866	5,866	5,866	5,866	5,866	1,470

Standard errors in parentheses

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

Note: Table shows regressions measuring gaps in an indicator for being married two decades later, by perceived child race among children age 3–18 in 1910 and 1920. Columns (1)–(4) follow Equation 1, starting with age and birth order fixed effects and adding controls for state fixed effects, enumeration district fixed effects, and family characteristics in columns (2)–(4) respectively. See Footnote 44 for detail regarding the set of family characteristics included as controls. Column (5) implements Equation 3 by further adding BMS/BMD race interaction terms and weights such that our *Black–Mulatto* coefficient measures differences among differently-classified siblings with approximately equal weighted distributions of family characteristics. Panels A and B analyze differences among sons and daughters respectively. The bottom part of each panel presents observation counts and mean outcome variables by perceived child race. All non-race controls are interacted with cohort fixed effects. Observations are weighted by their inverse link probabilities as described in Section 4.2. Standard errors are clustered by (enumeration district)×(year). All controls come from the child's census year, rather than from their linked adult self.

Table 10: Gaps in spouses' years of educational attainment by 1940, by perceived child race and gender

(a) Sons

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.772***	-0.675***	-0.633***	-0.383***	-0.045	-0.034
	(0.018)	(0.016)	(0.013)	(0.012)	(0.099)	(0.158)
Mulatto - White	-2.319***	-1.464***	-1.709***	-1.115***		
	(0.019)	(0.017)	(0.014)	(0.013)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMS Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	12,241,668	12,241,668	12,241,178	12,241,178	12,241,178	4,270,518
R-Squared	0.102	0.151	0.252	0.289	0.289	0.698
Count Black	525,143	525,143	525,120	525,120	525,120	108,949
Count Mulatto	140,253	140,253	140,252	140,252	140,252	32,269
Count White	11,576,272	11,576,272	11,575,806	11,575,806	11,575,806	4,129,300
Mean[Outcome | Black]	6.611	6.611	6.611	6.611	6.611	6.511
Mean[Outcome | Mulatto]	7.334	7.334	7.334	7.334	7.335	7.273
Mean[Outcome | White]	9.691	9.691	9.691	9.691	9.691	9.589
Count Mixed Siblings	4,575	4,575	4,575	4,575	4,575	1,406

(b) Daughters

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.626***	-0.519***	-0.494***	-0.203***	-0.552**	-0.388
	(0.026)	(0.024)	(0.022)	(0.022)	(0.221)	(0.494)
Mulatto - White	-2.402***	-1.454***	-1.743***	-1.060***		
	(0.025)	(0.023)	(0.021)	(0.020)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMD Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	7,432,333	7,432,333	7,431,550	7,431,550	7,431,550	2,071,106
R-Squared	0.064	0.119	0.232	0.274	0.274	0.692
Count Black	162,994	162,994	162,961	162,961	162,961	11,981
Count Mulatto	51,271	51,271	51,258	51,258	51,258	5,793
Count White	7,218,068	7,218,068	7,217,331	7,217,331	7,217,331	2,053,332
Mean[Outcome | Black]	5.997	5.997	5.996	5.996	5.996	5.834
Mean[Outcome | Mulatto]	6.568	6.568	6.569	6.569	6.570	6.750
Mean[Outcome | White]	9.043	9.043	9.043	9.043	9.043	8.874
Count Mixed Siblings	1,346	1,346	1,346	1,346	1,346	174

Standard errors in parentheses

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

Note: Table shows regressions measuring gaps in the years of education a child's future spouse had attained by 1940, by perceived child race among children age 3–18 in 1910 and 1920. Columns (1)–(4) follow Equation 1, starting with age and birth order fixed effects and adding controls for state fixed effects, enumeration district fixed effects, and family characteristics in columns (2)–(4) respectively. See Footnote 44 for detail regarding the set of family characteristics included as controls. Column (5) implements Equation 3 by further adding BMS/BMD race interaction terms and weights such that our *Black–Mulatto* coefficient measures differences among differently-classified siblings with approximately equal weighted distributions of family characteristics. Panels A and B analyze differences among sons and daughters respectively. The bottom part of each panel presents observation counts and mean outcome variables by perceived child race. All non-race controls are interacted with cohort fixed effects. Observations are weighted by their inverse link probabilities as described in Section 4.2. Standard errors are clustered by (enumeration district)×(year). All controls come from the child's census year, rather than from their linked adult self. Restricts to individuals married in 1940.

Table 11: Gaps in log wage income in 1940, by perceived child race and gender

(a) Sons

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.138***	-0.108***	-0.092***	-0.051***	0.006	-0.001
	(0.004)	(0.003)	(0.003)	(0.003)	(0.027)	(0.041)
Mulatto - White	-0.688***	-0.493***	-0.549***	-0.450***		
	(0.004)	(0.004)	(0.003)	(0.003)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMS Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	12,399,329	12,399,329	12,398,872	12,398,872	12,398,872	4,309,563
R-Squared	0.112	0.157	0.238	0.251	0.251	0.671
Count Black	528,280	528,280	528,265	528,265	528,265	112,153
Count Mulatto	144,940	144,940	144,937	144,937	144,937	34,313
Count White	11,726,109	11,726,109	11,725,670	11,725,670	11,725,670	4,163,097
Mean[Outcome | Black]	6.032	6.032	6.032	6.032	6.032	5.994
Mean[Outcome | Mulatto]	6.188	6.188	6.188	6.188	6.188	6.170
Mean[Outcome | White]	6.852	6.852	6.852	6.852	6.852	6.823
Count Mixed Siblings	4,506	4,506	4,506	4,506	4,506	1,410

(b) Daughters' spouses

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.118***	-0.079***	-0.072***	-0.026***	-0.018	0.251
	(0.007)	(0.006)	(0.007)	(0.006)	(0.068)	(0.160)
Mulatto - White	-0.552***	-0.367***	-0.452***	-0.346***		
	(0.006)	(0.006)	(0.006)	(0.006)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMD Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	5,232,460	5,232,460	5,231,481	5,231,481	5,231,481	1,114,811
R-Squared	0.052	0.110	0.220	0.237	0.237	0.644
Count Black	107,993	107,993	107,938	107,938	107,938	5,538
Count Mulatto	34,700	34,700	34,681	34,681	34,681	2,963
Count White	5,089,767	5,089,767	5,088,862	5,088,862	5,088,862	1,106,310
Mean[Outcome | Black]	6.284	6.284	6.284	6.284	6.284	6.230
Mean[Outcome | Mulatto]	6.416	6.416	6.416	6.416	6.416	6.461
Mean[Outcome | White]	6.956	6.956	6.956	6.956	6.956	6.916
Count Mixed Siblings	875	875	875	875	875	72

Standard errors in parentheses

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

Note: Table shows regressions measuring gaps in the log wage income of a son or of a daughter's spouse as an adult in 1940, by perceived child race among children age 3–18 in 1870, 1880, 1910, and 1920. Columns (1)–(4) follow Equation 1, starting with age and birth order fixed effects and adding controls for state fixed effects, enumeration district fixed effects, and family characteristics in columns (2)–(4) respectively. See Footnote 44 for detail regarding the set of family characteristics included as controls. Column (5) implements Equation 3 by further adding BMS/BMD race interaction terms and weights such that our *Black–Mulatto* coefficient measures differences among differently-classified siblings with approximately equal weighted distributions of family characteristics. Panels A and B analyze differences among sons and among daughters' spouses respectively. The bottom part of each panel presents observation counts and mean outcome variables by perceived child race. All non-race controls are interacted with cohort fixed effects. Observations are weighted by their inverse link probabilities as described in Section 4.2. Standard errors are clustered by (enumeration district)×(year). All controls come from the child's census year, rather than from their linked adult self. Panel B restricts to daughters married in 1940.

Table 12: Gaps in predicted log wage in *t+20*, by perceived child race and gender

(a) Sons

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.091***	-0.056***	-0.047***	-0.026***	-0.016**	0.006
	(0.002)	(0.001)	(0.001)	(0.001)	(0.008)	(0.012)
Mulatto - White	-0.455***	-0.246***	-0.274***	-0.211***		
	(0.002)	(0.001)	(0.001)	(0.001)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMS Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	21,738,645	21,738,645	21,738,205	21,738,205	21,738,205	8,835,748
R-Squared	0.230	0.368	0.425	0.444	0.444	0.754
Count Black	1,078,695	1,078,695	1,078,680	1,078,680	1,078,680	279,131
Count Mulatto	263,437	263,437	263,434	263,434	263,434	75,559
Count White	20,396,513	20,396,513	20,396,091	20,396,091	20,396,091	8,481,058
Mean[Outcome | Black]	6.184	6.184	6.184	6.184	6.184	6.134
Mean[Outcome | Mulatto]	6.272	6.272	6.272	6.272	6.272	6.235
Mean[Outcome | White]	6.742	6.742	6.742	6.742	6.742	6.698
Count Mixed Siblings	11,917	11,917	11,917	11,917	11,917	4,630

(b) Daughters' spouses

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.093***	-0.052***	-0.048***	-0.026***	-0.071***	-0.060
	(0.003)	(0.002)	(0.002)	(0.002)	(0.020)	(0.044)
Mulatto - White	-0.396***	-0.211***	-0.247***	-0.184***		
	(0.003)	(0.002)	(0.002)	(0.002)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMD Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	9,487,992	9,487,992	9,487,021	9,487,019	9,487,019	2,676,260
R-Squared	0.114	0.285	0.356	0.374	0.374	0.696
Count Black	199,176	199,176	199,132	199,132	199,132	14,681
Count Mulatto	60,198	60,198	60,185	60,184	60,184	6,808
Count White	9,228,618	9,228,618	9,227,704	9,227,703	9,227,703	2,654,771
Mean[Outcome | Black]	6.338	6.338	6.338	6.338	6.338	6.293
Mean[Outcome | Mulatto]	6.433	6.433	6.433	6.433	6.433	6.443
Mean[Outcome | White]	6.826	6.826	6.826	6.826	6.826	6.770
Count Mixed Siblings	2,033	2,033	2,033	2,033	2,033	268

Standard errors in parentheses

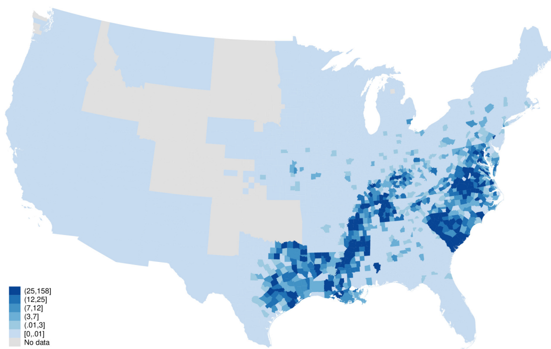
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows regressions measuring gaps in the predicted log wage income score of a son or a daughter's spouse, by perceived child race among children age 3–18 in 1870, 1880, 1910, and 1920. Columns (1)–(4) follow Equation 1, starting with age and birth order fixed effects and adding controls for state fixed effects, enumeration district fixed effects, and family characteristics in columns (2)–(4) respectively. See Footnote 44 for detail regarding the set of family characteristics included as controls. Column (5) implements Equation 3 by further adding BMS/BMD race interaction terms and weights such that our *Black–Mulatto* coefficient measures differences among differently-classified siblings with approximately equal weighted distributions of family characteristics. Panels A and B analyze differences among sons and daughters' spouses respectively. The bottom part of each panel presents observation counts and mean outcome variables by perceived child race. All non-race controls are interacted with cohort fixed effects. Observations are weighted by their inverse link probabilities as described in Section 4.2. Standard errors are clustered by (enumeration district)×(year). Predictions made based on adult occupation, geography, and age (see Section 4.3). All controls come from the child's census year, rather than from their linked adult self. Panel B restricts to married daughters.

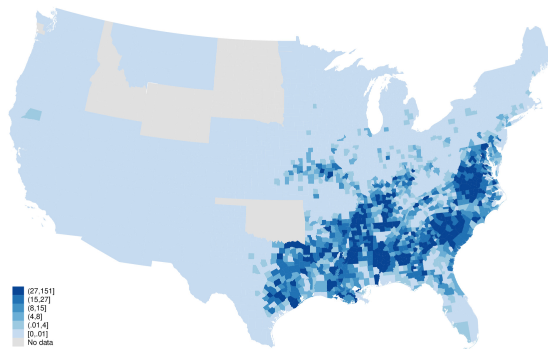
A Additional figures and tables

Appendix Figure A1: Number of differently-classified brothers, by census cohort and county

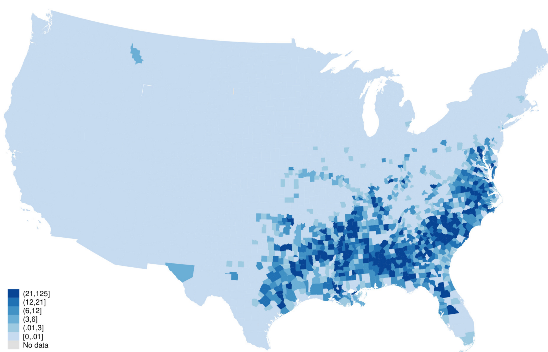
(a) 1870 cohort



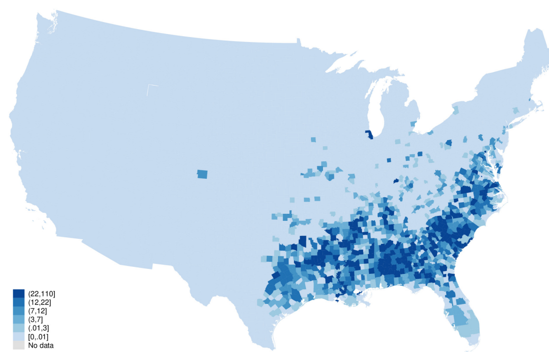
(b) 1880 cohort



(c) 1910 cohort

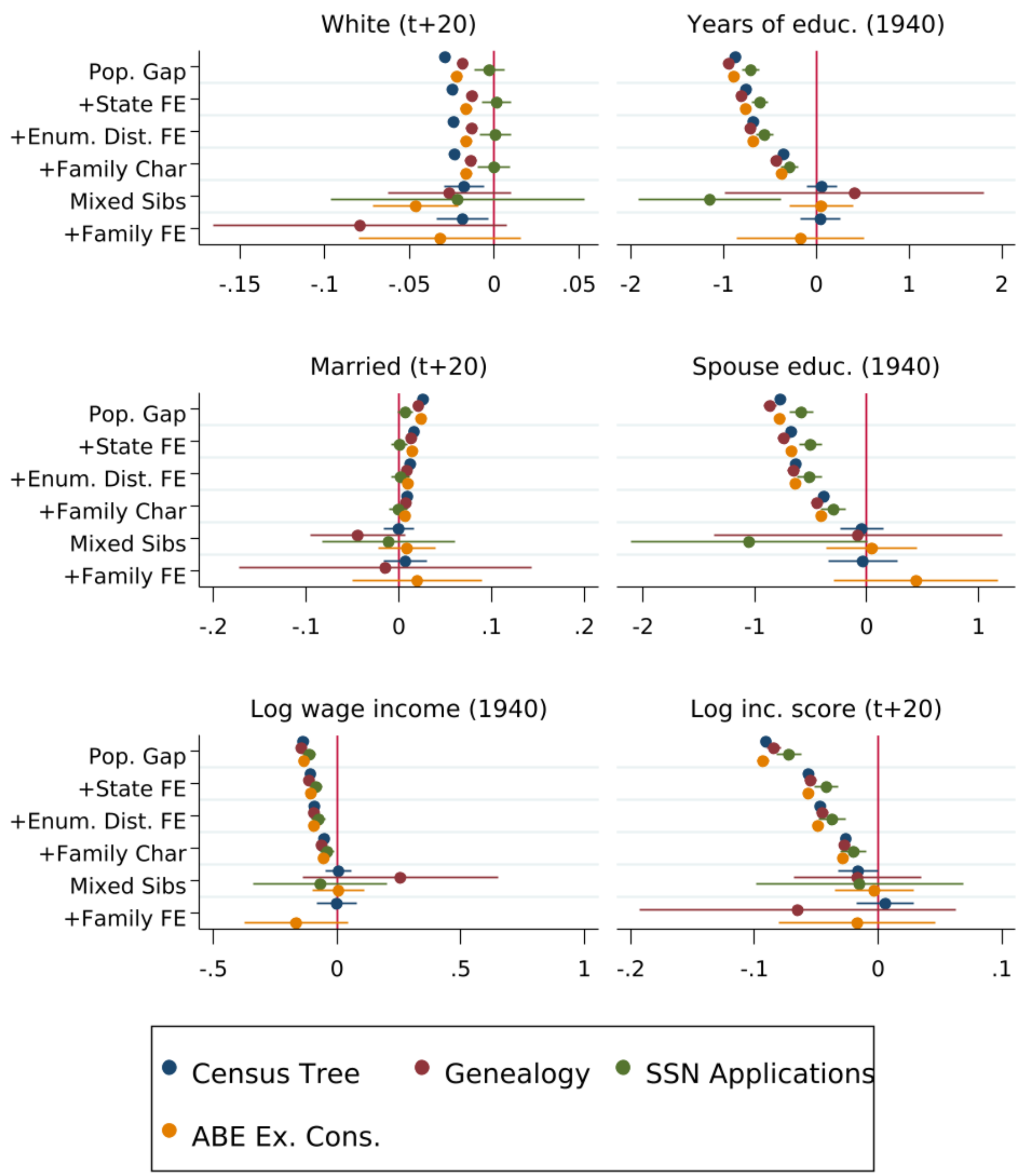


(d) 1920 cohort



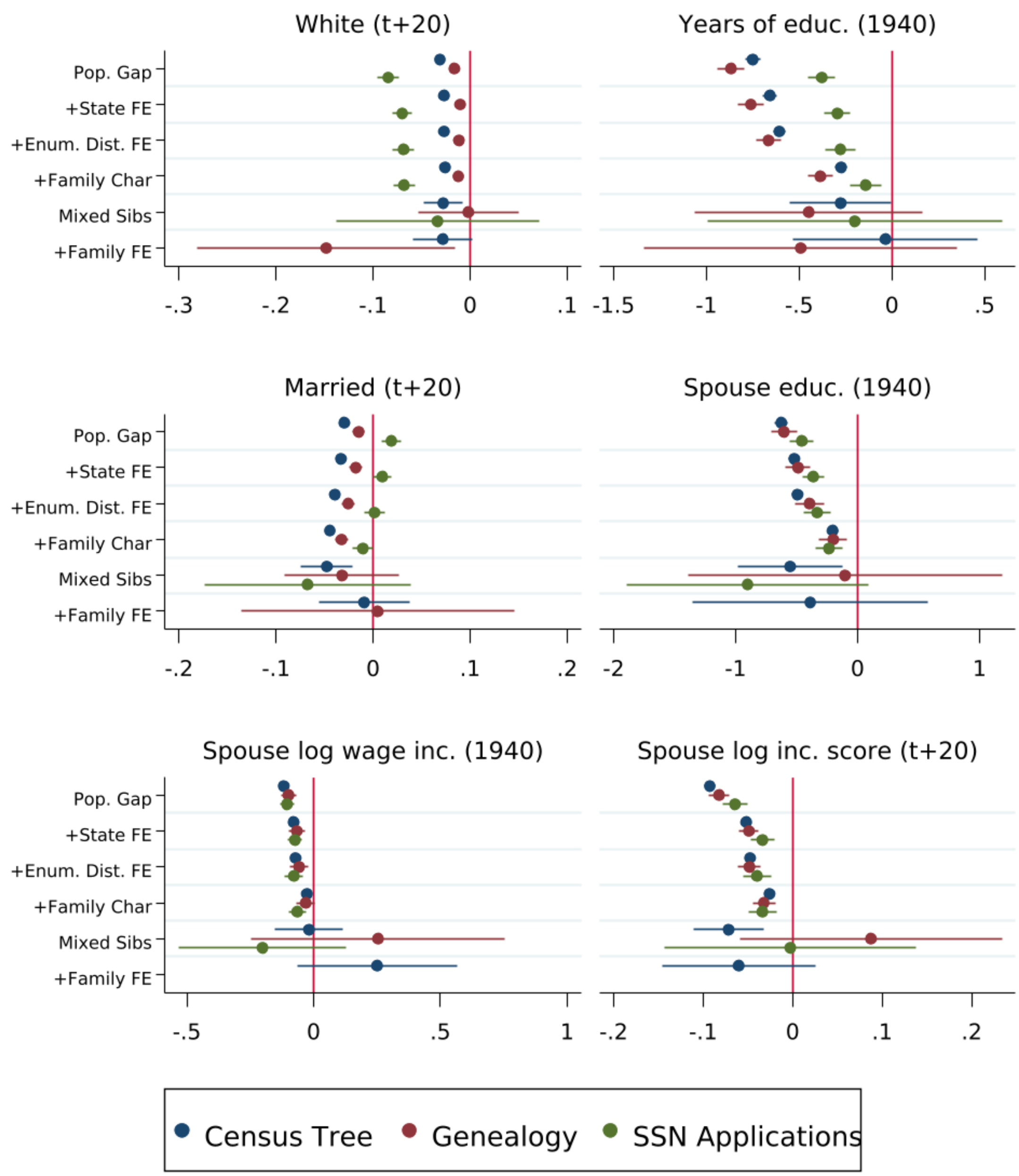
Note: Figure maps the number of differently-classified sons by county and census cohort. Our Black-and-Mulatto-sons (BMS) sample consists of households where some son aged 3–18 was classified Mulatto and another Black. We restrict to cases where these sons shared two parents who were both present within the household. Sample construction is described in Section 4.1.

Appendix Figure A2: Robustness of *Black–Mulatto* gaps in sons' linked adult outcomes across linking algorithms



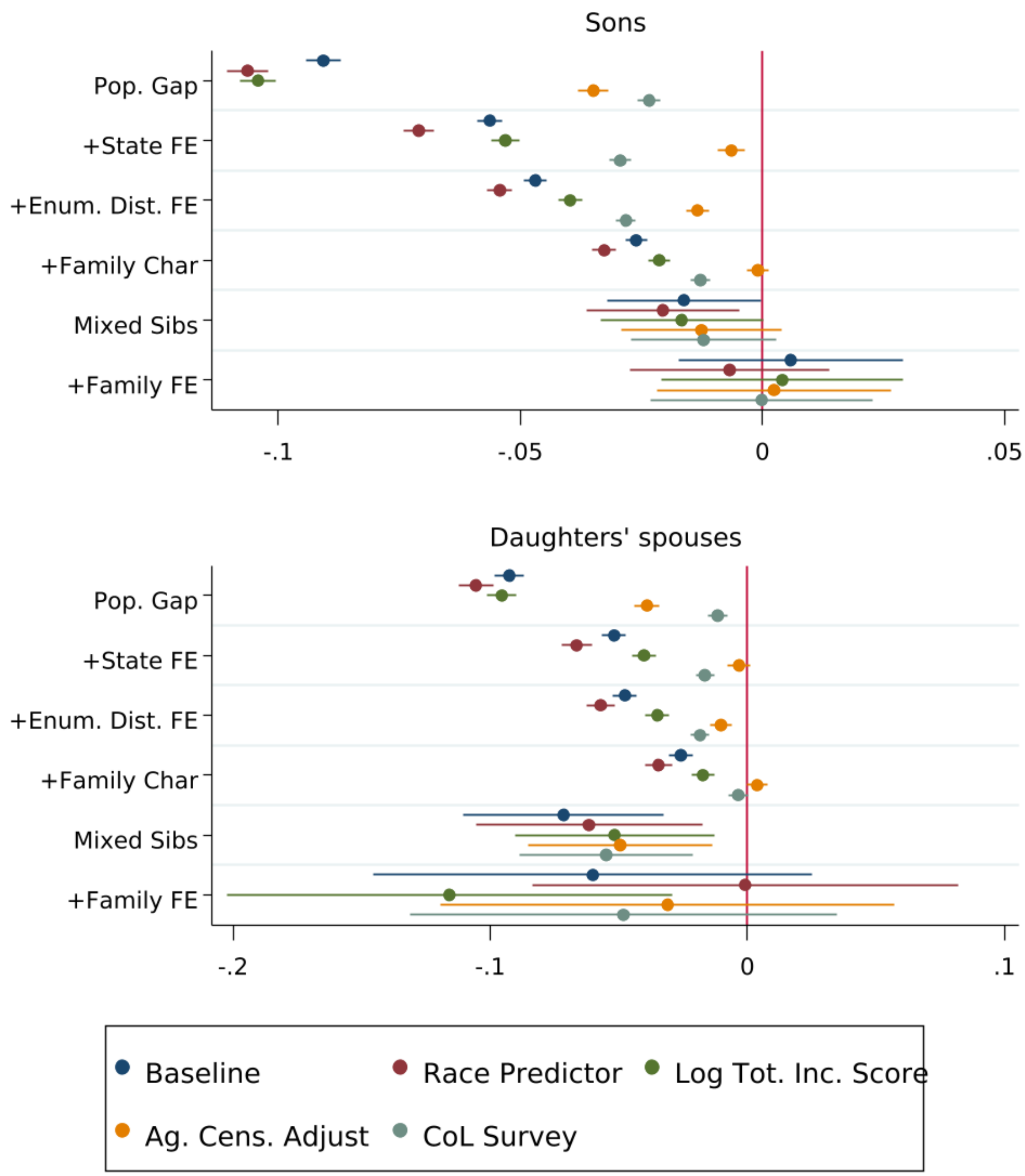
Note: Figure shows robustness of our linked adult outcome results to our choice of matching algorithm by showing regression coefficients and 95% confidence intervals documenting sons' *Black–Mulatto* gaps for each of our main linked outcomes, for each set of controls, and for four different linking algorithms. We repeat results using our preferred Census Tree links (as shown in Figures 4–6 and as used throughout the paper unless noted otherwise), which have a linking rate to *t+20* of 51%. We now also show analogous results when individuals are linked by either genealogical links from FamilySearch users (9% linking rate), Social Security Number application-based links (4%), or the ABE Exact Conservative method (17%). See Section 4.2 for further discussion of these linking methods. We omit coefficients that would be based on a comparison of fewer than 70 individuals.

Appendix Figure A3: Robustness of *Black–Mulatto* gaps in daughters' linked adult outcomes across linking algorithms



Note: Figure shows robustness of our linked adult outcome results to our choice of matching algorithm by showing regression coefficients and 95% confidence intervals documenting daughters' *Black–Mulatto* gaps for each of our main linked outcomes, for each set of controls, and for three different matching algorithms. We repeat results using our preferred Census Tree links (as shown in Figures 4–6 and as used throughout the paper unless noted otherwise), which have a linking rate to *t+20* of 35%. We now also show analogous results when individuals are linked by either genealogical links from FamilySearch users (5% linking rate) and Social Security Number application-based links (2%). See Section 4.2 for further discussion of these linking methods. We omit coefficients that would be based on a comparison of fewer than 70 individuals.

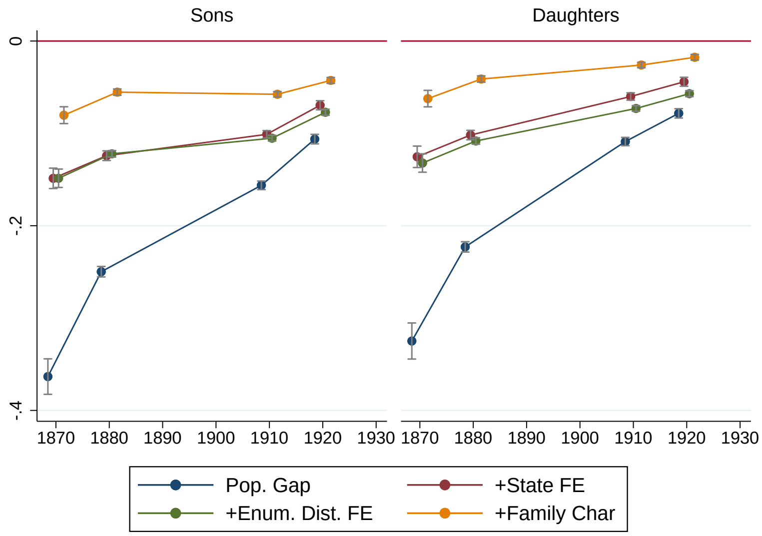
Appendix Figure A4: Robustness of *Black–Mulatto* gaps across construction of income scores



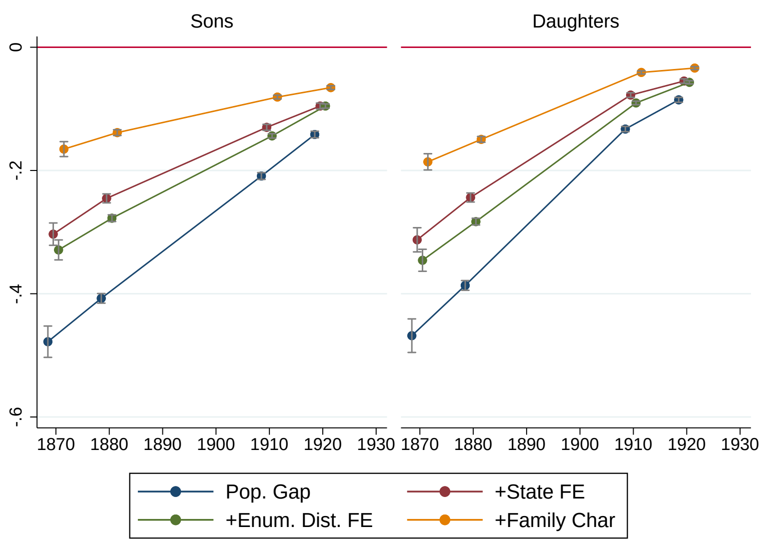
Note: Figure shows robustness of our linked adult income score outcomes to the construction of income scores. We do so by showing regression coefficients and 95% confidence intervals documenting *Black–Mulatto* gaps in the income scores of sons and of daughters' spouses, for each set of controls and for different construction methods. We repeat results using our baseline income scores (as shown in Figure 6 Panel B), while now also showing analogous results when income scores are modified to either include race as an income predictor, predict log total income (as defined in Section 6.4, rather than predicting log wage income), use the 1900 Census of Agriculture to adjust incomes for farm managers and farmer laborers (rather than the 1960-decennial-census-based adjustment suggested by Collins and Wanamaker, 2022), or use the 1901 Cost of Living survey (rather than decennial census data) as our income data source. See Section 4.3 for further discussion of these construction methods.

Appendix Figure A5: *Mulatto–White* gap in childhood education, by child cohort and gender

(a) School attendance



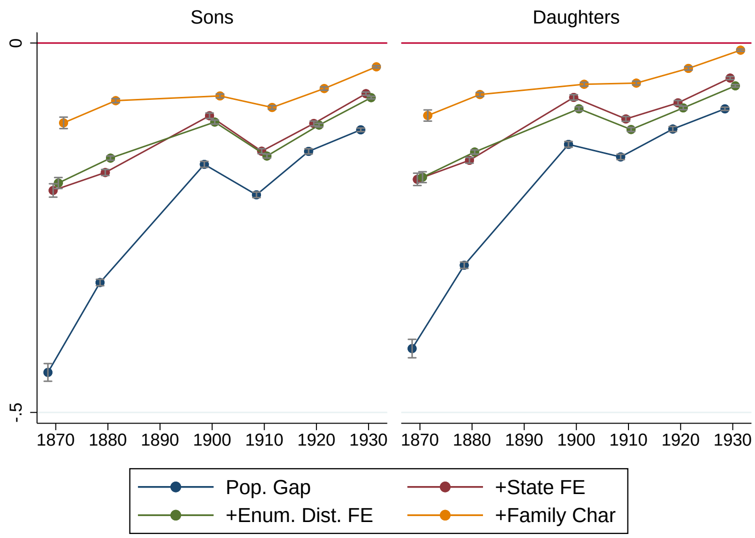
(b) Literacy



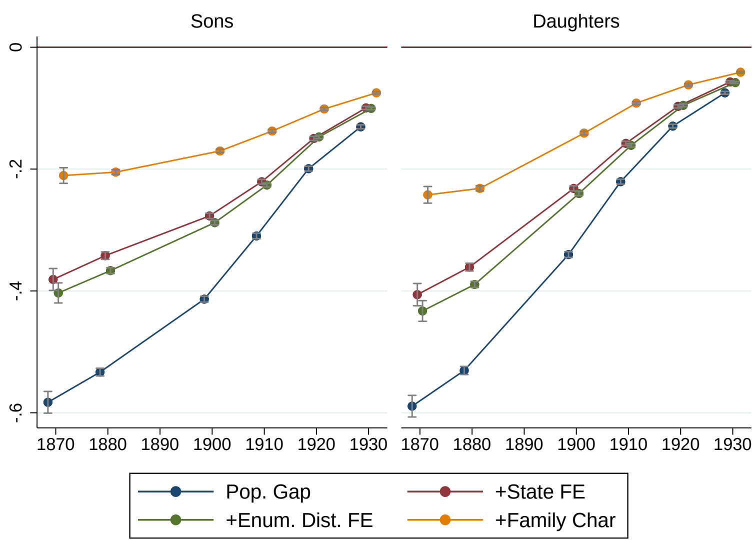
Note: Figure shows regression coefficients and 95% confidence intervals documenting *Mulatto–White* gaps in childhood school attendance (Panel A) and literacy (Panel B) by gender and child cohort. See Section 6.1 for detail.

Appendix Figure A6: *Black/Mulatto–White* gap in childhood education, by child cohort and gender

(a) School attendance



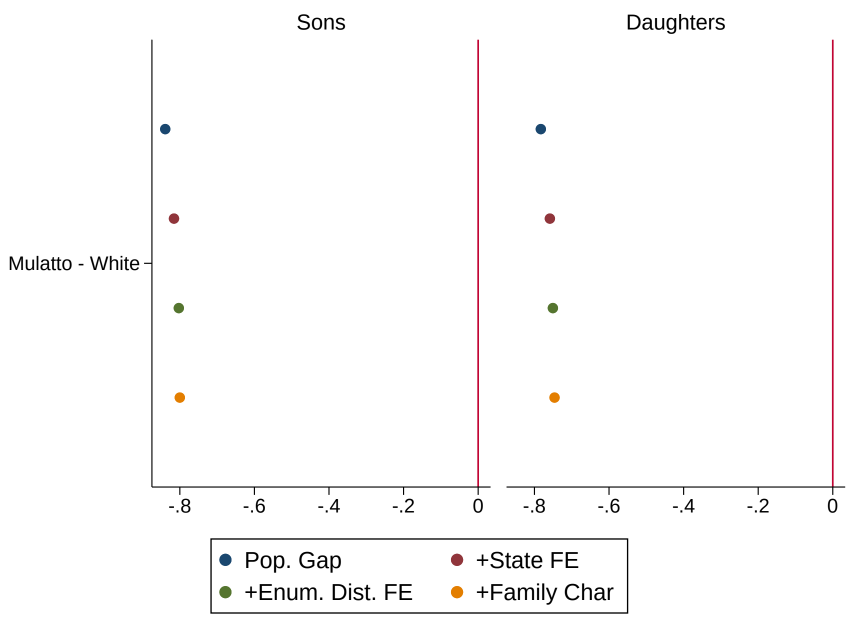
(b) Literacy



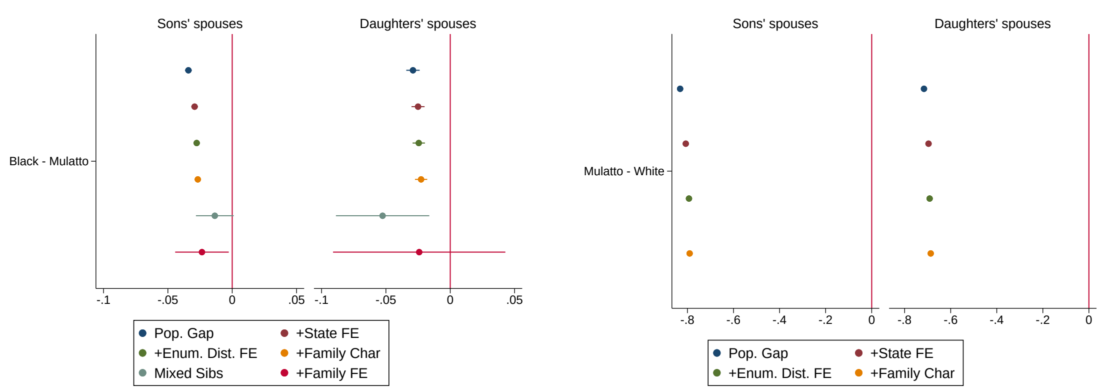
Note: Figure shows regression coefficients and 95% confidence intervals documenting *Mulatto–White* gaps in childhood school attendance (Panel A) and literacy (Panel B) by gender and child cohort. The regression specification modifies Equation 1 by pooling Mulatto- and Black-coded children together into a single Black/Mulatto category, which allows us to also include 1900 and 1930 child cohorts (where the Black vs. Mulatto distinction was not made by census enumerators).

Appendix Figure A7: Additional gaps in classification as White, by perceived child race and gender

(a) Classification as White in *t+20* (*Mulatto–White* gap)



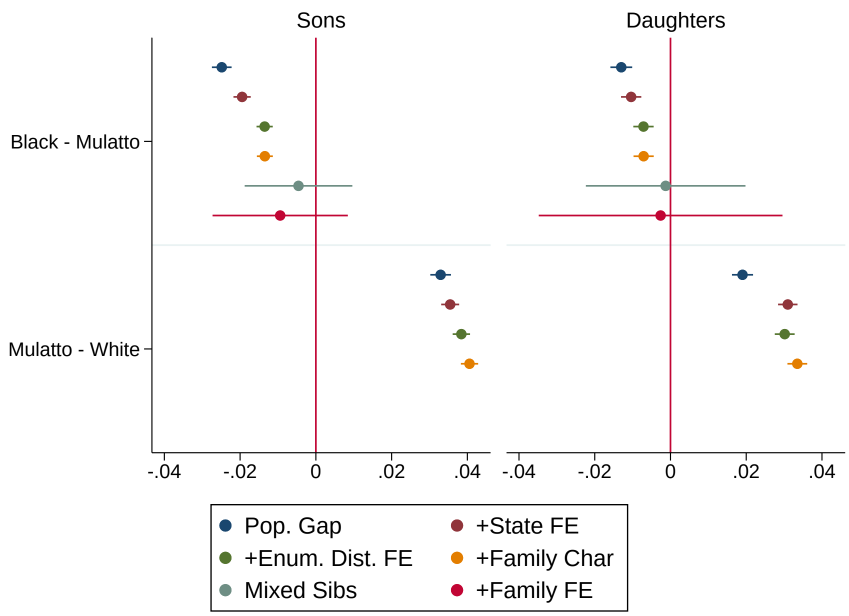
(b) Spouses' classification as White in *t+20*



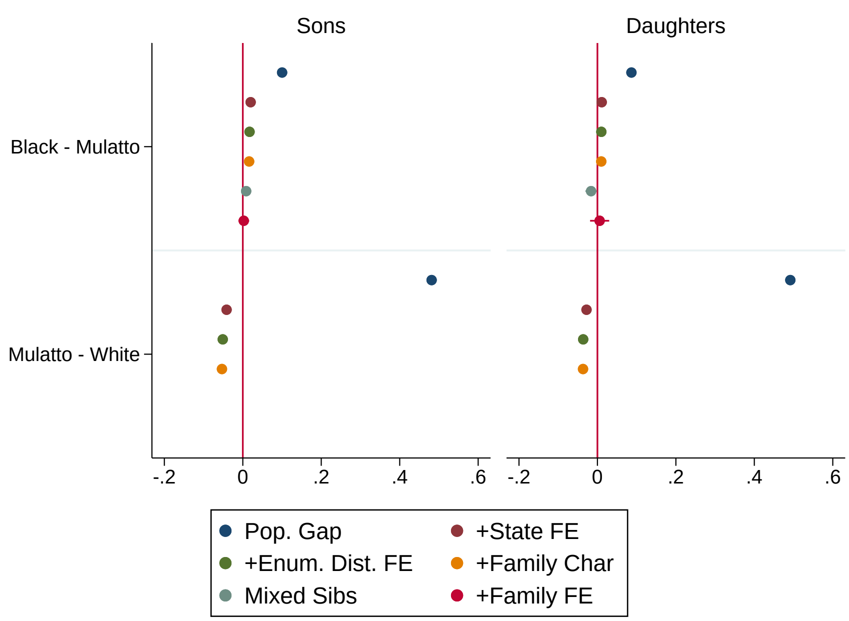
Note: Figure shows regression coefficients and 95% confidence intervals documenting gaps by perceived childhood race in an indicator for being classified as a White adult twenty years later. Panel A shows the *Mulatto–White* gap in the likelihood that a child is him- or herself classified as White as an adult (see Table 7 for the analogous *Black–Mulatto* gap). Panel B shows differences in the rate at which a child's spouse is classified as White as an adult (showing both the *Black–Mulatto* and *Mulatto–White* gaps in side-by-side plots on different x-scales). See Sections 6.2 and 6.3 for detail and Tables 7 and A4 for corresponding regression tables.

Appendix Figure A8: Gaps in migration by *t+20*, by perceived child race and gender

(a) Different state in *t+20* vs. *t*

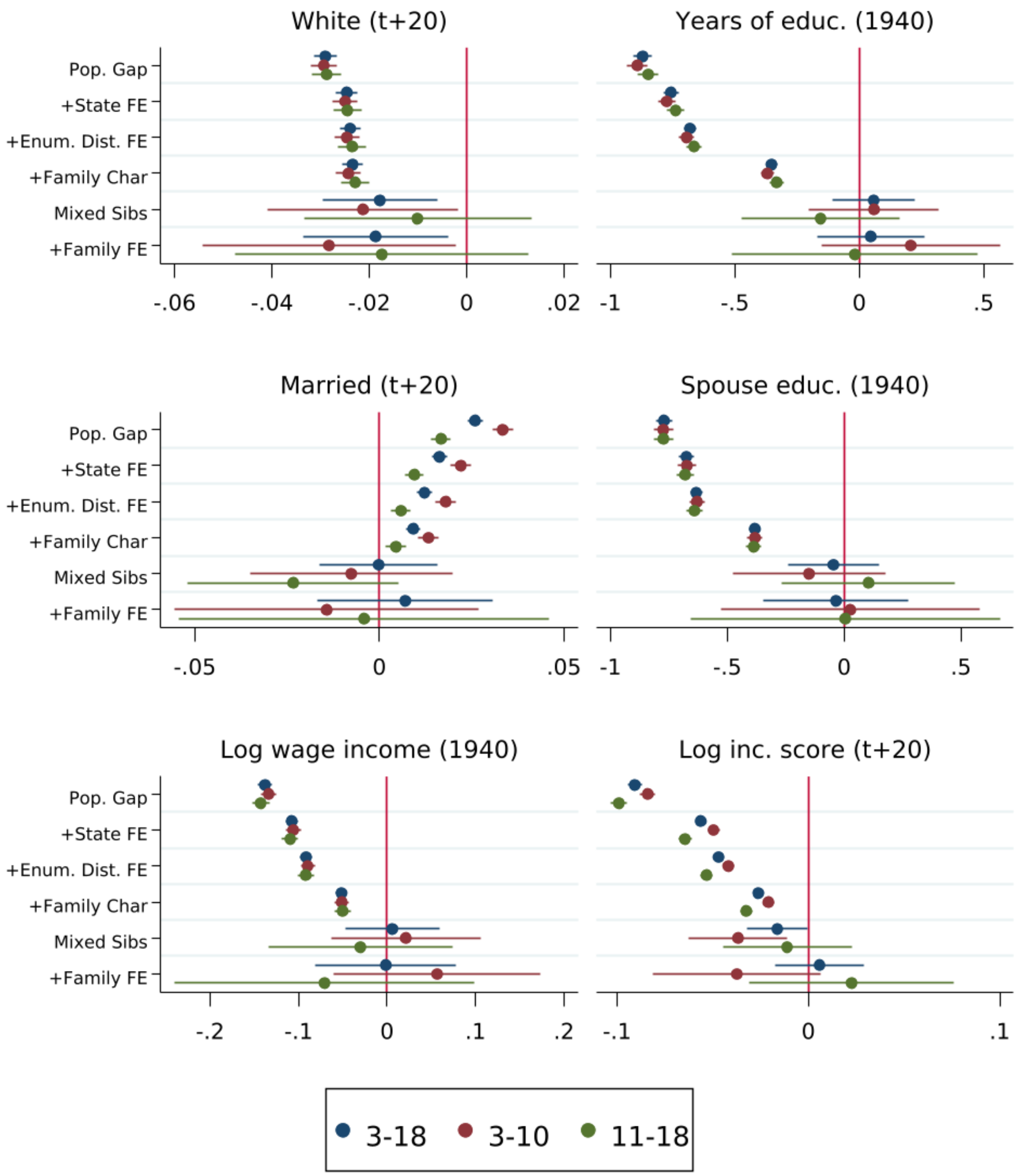


(b) Lives in southern state in *t+20*



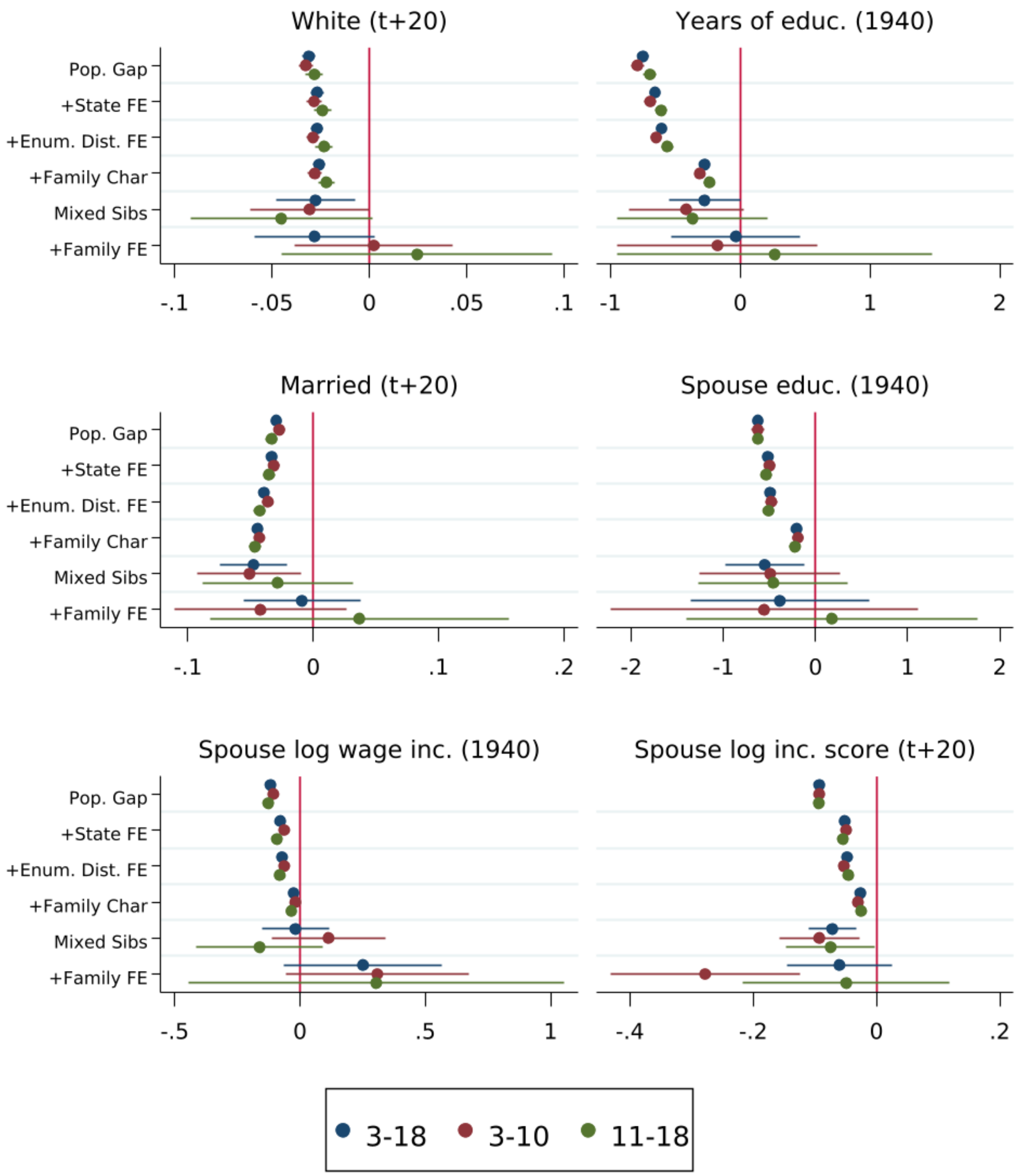
Note: Figure shows regression coefficients and 95% confidence intervals documenting gaps by perceived childhood race in migration by *t+20*. Panel A shows differences in an indicator for living in a state as an adult in year *t+20* that is different from the child's state in *t*. Panel B shows differences in an indicator for living in a southern state in *t+20*. See Section 6.5 for detail and Tables A6 and A7 for corresponding regression tables.

Appendix Figure A9: Comparison of *Black–Mulatto* gaps among sons, across child age groups



Note: Figure shows regression coefficients and 95% confidence intervals documenting sons' *Black–Mulatto* gaps for each of our main adult outcomes, for each set of controls, and for three samples. We repeat results using our baseline sample (ages 3–18 at classification, as shown in Figures 2–6 and as used throughout the paper unless otherwise noted), while adding analogous results restricting to sons age 3–10 or to 11–18 at the time of classification. To be included in our differently-classified (BMS) sample, two brothers must both be within the age restriction.

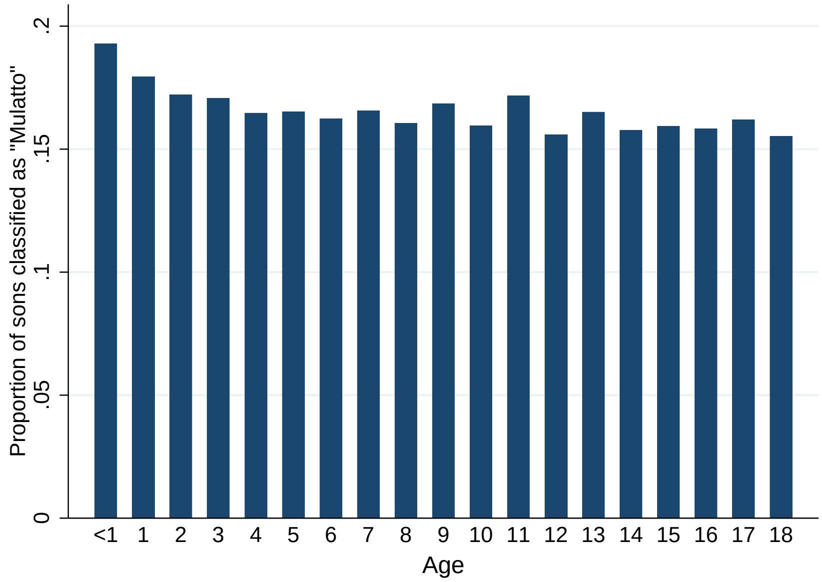
Appendix Figure A10: Comparison of *Black–Mulatto* gaps among daughters, across child age groups



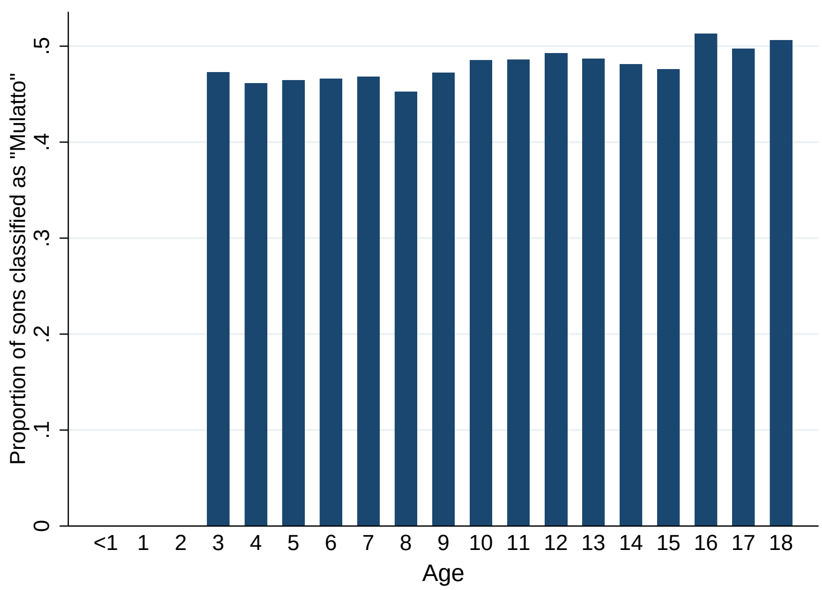
Note: Figure shows regression coefficients and 95% confidence intervals documenting daughters' *Black–Mulatto* gaps for each of our main adult outcomes, for each set of controls, and for three samples (as in Figure A9). We repeat results using our baseline sample (ages 3–18 at classification, as shown in Figures 2–6 and as used throughout the paper unless otherwise noted), while adding analogous results restricting to daughters age 3–10 or to 11–18 at the time of classification. To be included in our differently-classified (BMD) sample, two sisters must both be within the age restriction.

Appendix Figure A11: Proportion of sons coded as Mulatto, by age

(a) Proportion of sons coded as Mulatto by age, among all sons coded as Black or Mulatto



(b) Proportion of sons coded as Mulatto by age, among Black-and-Mulatto-sons (BMS) sample



Note: Figure shows the share of African American sons classified as Mulatto (vs. Black) by age. Panel A shows these proportions among all Black- and/or Mulatto-coded sons. Panel B shows these proportions among our sample of differently-classified sons (BMS) sample, where at least one son in a family was classified as Black while his brother was classified as Mulatto. See Section 4.1 for the complete definition of this sample.

Appendix Table A1: Characteristics of linked vs. all searched daughters

	All obs	Census Tree				SSN Applications			
Observations (count)	49,021,904	17,231,696				913,704			
Black+Mulatto in county	6,330,433	696,715				99,579			
Black+Mulatto in family	54,240	1,498				27			
		Unweighted		Weighted		Unweighted		Weighted	
Child characteristics	Mean	Mean	Difference	Mean	Difference	Mean	Difference	Mean	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Black	.106	.0318	-.0744***	.0914	-.0149***	.0975	-.00878***	.109	.00283***
			(.0000735)		(.000119)		(.000322)		(.000495)
Mulatto	.0231	.00906	-.0141***	.0244	.00127***	.0185	-.00464***	.0201	-.00302***
			(.0000361)		(.0000634)		(.000157)		(.000215)
Age	10.2	10	-.149***	10.2	-.0189***	8.3	-1.88***	9.83	-.35***
			(.00112)		(.00128)		(.00484)		(.00676)
Able to write	.906	.952	.0461***	.921	.0152***	.955	.049***	.937	.0306***
			(.000097)		(.000134)		(.000516)		(.00072)
Daughters in household	2.11	2.19	.0779***	2.12	.0128***	2.11	.000775	2.11	-.000533
			(.000316)		(.00036)		(.00137)		(.00202)
Head is literate	.853	.918	.0649***	.863	.0101***	.901	.0481***	.867	.0132***
			(.0000848)		(.000121)		(.000371)		(.00059)
Head's occupation:									
- Farm laborer	.0481	.0313	-.0168***	.0429	-.00521***	.0347	-.0134***	.0421	-.00601***
			(.0000515)		(.0000684)		(.000224)		(.000343)
- Farmer	.365	.405	.0406***	.366	.00126***	.31	-.0546***	.359	-.00617***
			(.000116)		(.000129)		(.000504)		(.00075)
- Laborer (not farm)	.0908	.0717	-.0191***	.0902	-.000613***	.0866	-.00422***	.0917	.000948**
			(.0000692)		(.000089)		(.000301)		(.000457)
County urban	.375	.365	-.00957***	.361	-.0141***	.462	.087***	.404	.0292***
			(.000088)		(.0000969)		(.000382)		(.000547)
Census Year	1902	1904	2.37***	1902	.587***	1915	13.5***	1907	5.63***
			(.00469)		(.00566)		(.0203)		(.0357)

Note: Table shows differences in mean characteristics for our initial and linked samples of daughters. Column (1) reports counts and mean statistics among our initial sample prior to linking, including children from 1870, 1880, 1910, and 1920. Columns (2) and (4) denote mean characteristics under our Census Tree links when linking to *t+20*, without and with inverse probability match weights, respectively (see Section 4.2 for detail on linking and weights). Columns (3) and (5) report differences between our initial and linked means, without and with weights, respectively. Columns (6)–(9) repeats the above for the Social Security Number application-based linking algorithm. Standard errors are clustered by (enumeration district)×(year). See Table 3 for an analogous table comparing the characteristics of linked vs. all searched sons.

Appendix Table A2: Differences in match rate among differently-classified siblings, by perceived child race and gender

(a) Sons

	(1) Census Tree	(2) Genealogy	(3) SSN-Apps	(4) ABE Ex. Cons.
Black - Mulatto	-0.001	-0.001	0.002	0.002
	(0.004)	(0.002)	(0.001)	(0.002)
Constant	0.263***	0.036***	0.022***	0.072***
	(0.003)	(0.001)	(0.001)	(0.002)
Observations	50,737	50,737	50,737	50,737
R-Squared	0.000	0.000	0.000	0.000
Count Black	26,612	26,612	26,612	26,612
Count Mulatto	24,125	24,125	24,125	24,125
Mean[Outcome | Black]	0.262	0.035	0.024	0.074
Mean[Outcome | Mulatto]	0.263	0.036	0.022	0.072

(b) Daughters

	(1) Census Tree	(2) Genealogy	(3) SSN-Apps
Black - Mulatto	0.002	0.003**	0.003***
	(0.003)	(0.001)	(0.001)
Constant	0.107***	0.022***	0.011***
	(0.002)	(0.001)	(0.001)
Observations	54,240	54,240	54,240
R-Squared	0.000	0.000	0.000
Count Black	28,194	28,194	28,194
Count Mulatto	26,046	26,046	26,046
Mean[Outcome | Black]	0.109	0.025	0.014
Mean[Outcome | Mulatto]	0.107	0.022	0.011

Standard errors in parentheses

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

Note: Table shows differences in linking rates among Black- vs. Mulatto-coded siblings in our differently-classified (BMS/BMD) samples to *t+20*, by linking algorithm. Observations are weighted proportional to the inverse share of their perceived race within a family (see Sections 5 and 4.2 for detail on these weights and linking procedures, respectively). Standard errors are clustered by (enumeration district)×(year). Panels A and B analyze differences among sons and daughters, respectively.

Appendix Table A3: Gaps in classification as White in SSN application, by perceived child race and gender

(a) Sons

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.017***	-0.011*	-0.010	-0.012	-0.034	-0.007
	(0.006)	(0.006)	(0.007)	(0.007)	(0.060)	(0.007)
Mulatto - White	-0.816***	-0.757***	-0.752***	-0.752***		
	(0.006)	(0.006)	(0.007)	(0.007)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMS Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	712,888	712,888	688,193	688,191	688,191	42,201
R-Squared	0.699	0.710	0.792	0.793	0.793	0.928
Count Black	30,042	30,042	28,488	28,488	28,488	1,099
Count Mulatto	7,409	7,409	6,975	6,975	6,975	317
Count White	675,437	675,437	652,730	652,728	652,728	40,785
Mean[Outcome | Black]	0.142	0.142	0.142	0.142	0.142	0.092
Mean[Outcome | Mulatto]	0.159	0.159	0.158	0.158	0.159	0.108
Mean[Outcome | White]	0.979	0.979	0.980	0.980	0.980	0.987
Count Mixed Siblings	268	268	256	256	256	9

(b) Daughters

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.016**	-0.009	-0.001	-0.001	-0.063	-0.240
	(0.008)	(0.008)	(0.009)	(0.009)	(0.087)	(0.226)
Mulatto - White	-0.558***	-0.505***	-0.514***	-0.515***		
	(0.007)	(0.007)	(0.008)	(0.008)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMD Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	825,055	825,055	806,530	806,530	806,530	41,057
R-Squared	0.402	0.421	0.566	0.567	0.567	0.766
Count Black	23,698	23,698	22,817	22,817	22,817	570
Count Mulatto	6,327	6,327	6,083	6,083	6,083	177
Count White	795,030	795,030	777,630	777,630	777,630	40,310
Mean[Outcome | Black]	0.403	0.403	0.402	0.402	0.402	0.330
Mean[Outcome | Mulatto]	0.420	0.420	0.414	0.414	0.414	0.395
Mean[Outcome | White]	0.978	0.978	0.978	0.978	0.978	0.980
Count Mixed Siblings	197	197	191	191	191	6

Standard errors in parentheses

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

Note: Table shows regressions measuring gaps in an indicator for (self-reported) classification as White when an individual first applied for a Social Security Number, by perceived child race among children age 3–18 in 1870, 1880, 1910, and 1920. See Table 7 for detail regarding the empirical specification. Social Security Number application data is provided and linked to 1940 U.S. decennial census individuals by Goldstein et al. (2021). Observations are weighted by their inverse link probabilities, multiplying together the inverse probability of linking an individual from their child cohort to their 1940 adult self (as described in Section 4.2) with the inverse probability of linking their 1940 adult self to a Social Security Number application (as calculated by Goldstein et al., 2021). Observations in which a child was identified as a race other than White or Black as an adult are dropped. All controls come from the child's census year, rather than from their linked adult self.

Appendix Table A4: Gaps in spouses' classification as White in *t+20*, by perceived child race and gender

(a) Sons' spouses

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.034***	-0.029***	-0.028***	-0.027***	-0.014*	-0.024**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.007)	(0.011)
Mulatto - White	-0.832***	-0.807***	-0.793***	-0.790***		
	(0.001)	(0.001)	(0.001)	(0.002)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMS Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	16,502,694	16,502,694	16,502,126	16,502,126	16,502,126	5,703,194
R-Squared	0.796	0.798	0.803	0.804	0.804	0.930
Count Black	792,703	792,703	792,681	792,681	792,681	159,423
Count Mulatto	188,117	188,117	188,112	188,112	188,112	41,356
Count White	15,521,874	15,521,874	15,521,333	15,521,333	15,521,333	5,502,415
Mean[Outcome | Black]	0.127	0.127	0.127	0.127	0.127	0.084
Mean[Outcome | Mulatto]	0.161	0.161	0.161	0.161	0.161	0.133
Mean[Outcome | White]	0.993	0.993	0.993	0.993	0.993	0.995
Count Mixed Siblings	8,699	8,699	8,699	8,699	8,699	2,537

(b) Daughters' spouses

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.029***	-0.025***	-0.024***	-0.023***	-0.053***	-0.024
	(0.003)	(0.003)	(0.002)	(0.002)	(0.019)	(0.034)
Mulatto - White	-0.716***	-0.696***	-0.691***	-0.687***		
	(0.003)	(0.003)	(0.002)	(0.003)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMD Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	10,857,394	10,857,394	10,856,526	10,856,524	10,856,524	3,294,488
R-Squared	0.665	0.669	0.690	0.692	0.692	0.879
Count Black	219,016	219,016	218,979	218,979	218,979	17,016
Count Mulatto	67,496	67,496	67,482	67,481	67,481	8,189
Count White	10,570,882	10,570,882	10,570,065	10,570,064	10,570,064	3,269,283
Mean[Outcome | Black]	0.249	0.249	0.249	0.249	0.249	0.185
Mean[Outcome | Mulatto]	0.278	0.278	0.278	0.278	0.278	0.296
Mean[Outcome | White]	0.995	0.995	0.995	0.995	0.995	0.996
Count Mixed Siblings	2,257	2,257	2,257	2,257	2,257	321

Standard errors in parentheses

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

Note: Table shows regressions measuring gaps in a child's spouse being classified as White in *t+20*, by perceived child race among children age 3–18 in 1870, 1880, 1910, and 1920. Columns (1)–(4) follow Equation 1, starting with age and birth order fixed effects and adding controls for state fixed effects, enumeration district fixed effects, and family characteristics in columns (2)–(4), respectively. See Footnote 44 for detail regarding the set of family characteristics included as controls. Column (5) implements Equation 3 by further adding BMS/BMD race interaction terms and weights such that our *Black–Mulatto* coefficient measures differences among differently-classified siblings with approximately equal weighted distributions of family characteristics. Panels A and B analyze differences among sons' and daughters' spouses, respectively. The bottom part of each panel presents observation counts and mean outcome variables by perceived child race. All non-race controls are interacted with cohort fixed effects. Observations are weighted by their inverse link probabilities as described in Section 4.2. Standard errors are clustered by (enumeration district)×(year). Observations in which a child's spouse was identified as a race other than White or Black as an adult are dropped. All controls come from the child's census year, rather than from their linked adult self. Restricts to married individuals.

Appendix Table A5: Gaps in log total income in 1940, by perceived child race

(a) Sons

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.115***	-0.081***	-0.068***	-0.036***	0.010	0.031
	(0.003)	(0.003)	(0.003)	(0.003)	(0.021)	(0.032)
Mulatto - White	-0.638***	-0.383***	-0.424***	-0.344***		
	(0.003)	(0.003)	(0.003)	(0.003)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMS Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	14,264,463	14,264,463	14,264,034	14,264,034	14,264,034	5,400,782
R-Squared	0.098	0.153	0.214	0.224	0.224	0.639
Count Black	629,412	629,412	629,396	629,396	629,396	150,381
Count Mulatto	168,536	168,536	168,533	168,533	168,533	44,372
Count White	13,466,515	13,466,515	13,466,105	13,466,105	13,466,105	5,206,029
Mean[Outcome | Black]	6.068	6.068	6.068	6.068	6.068	6.024
Mean[Outcome | Mulatto]	6.195	6.195	6.195	6.195	6.195	6.166
Mean[Outcome | White]	6.817	6.817	6.817	6.817	6.817	6.782
Count Mixed Siblings	5,448	5,448	5,448	5,448	5,448	1,962

(b) Daughters' spouses

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.102***	-0.054***	-0.047***	-0.014***	-0.023	0.165
	(0.005)	(0.005)	(0.005)	(0.005)	(0.050)	(0.126)
Mulatto - White	-0.523***	-0.259***	-0.316***	-0.238***		
	(0.005)	(0.004)	(0.004)	(0.004)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMD Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	6,447,196	6,447,196	6,446,298	6,446,298	6,446,298	1,630,848
R-Squared	0.047	0.125	0.205	0.216	0.216	0.610
Count Black	139,065	139,065	139,027	139,027	139,027	9,015
Count Mulatto	43,672	43,672	43,657	43,657	43,657	4,419
Count White	6,264,459	6,264,459	6,263,614	6,263,614	6,263,614	1,617,414
Mean[Outcome | Black]	6.253	6.253	6.253	6.253	6.253	6.194
Mean[Outcome | Mulatto]	6.361	6.361	6.361	6.361	6.361	6.378
Mean[Outcome | White]	6.879	6.879	6.878	6.878	6.878	6.829
Count Mixed Siblings	1,147	1,147	1,147	1,147	1,147	119

Standard errors in parentheses

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

Note: Table modifies Table 11 by combining 1940 wage income (as in the prior table) with estimated non-wage income (not included in the prior table). Non-wage income is estimated as described in Section 6.4.

Appendix Table A6: Gaps in living in different state in *t+20* vs. *t*, by perceived child race and gender

(a) Sons

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.025***	-0.019***	-0.014***	-0.013***	-0.005	-0.009
	(0.001)	(0.001)	(0.001)	(0.001)	(0.007)	(0.009)
Mulatto - White	0.033***	0.035***	0.038***	0.041***		
	(0.001)	(0.001)	(0.001)	(0.001)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMS Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	25,106,397	25,106,397	25,106,012	25,106,012	25,106,012	10,896,899
R-Squared	0.007	0.043	0.088	0.094	0.094	0.699
Count Black	1,206,427	1,206,427	1,206,416	1,206,416	1,206,416	330,125
Count Mulatto	301,405	301,405	301,401	301,401	301,401	92,259
Count White	23,598,565	23,598,565	23,598,195	23,598,195	23,598,195	10,474,515
Mean[Outcome | Black]	0.225	0.225	0.225	0.225	0.225	0.179
Mean[Outcome | Mulatto]	0.251	0.251	0.251	0.251	0.251	0.211
Mean[Outcome | White]	0.219	0.219	0.219	0.219	0.219	0.199
Count Mixed Siblings	13,376	13,376	13,376	13,376	13,376	5,625

(b) Daughters

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	-0.013***	-0.010***	-0.007***	-0.007***	-0.001	-0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.011)	(0.016)
Mulatto - White	0.019***	0.031***	0.030***	0.033***		
	(0.001)	(0.001)	(0.001)	(0.001)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMD Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	17,090,136	17,090,136	17,089,585	17,089,584	17,089,584	6,247,274
R-Squared	0.003	0.044	0.092	0.096	0.096	0.701
Count Black	540,466	540,466	540,445	540,445	540,445	90,536
Count Mulatto	153,984	153,984	153,977	153,977	153,977	30,810
Count White	16,395,686	16,395,686	16,395,163	16,395,162	16,395,162	6,125,928
Mean[Outcome | Black]	0.203	0.203	0.203	0.203	0.203	0.148
Mean[Outcome | Mulatto]	0.219	0.219	0.219	0.219	0.219	0.169
Mean[Outcome | White]	0.202	0.202	0.202	0.202	0.202	0.187
Count Mixed Siblings	5,866	5,866	5,866	5,866	5,866	1,470

Standard errors in parentheses

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

Note: Table shows regressions measuring gaps in an indicator for living in a different state twenty years later, by perceived child race among children age 3–18 in 1870, 1880, 1910, and 1920. Columns (1)–(4) follow Equation 1, starting with age and birth order fixed effects and adding controls for state fixed effects, enumeration district fixed effects, and family characteristics in columns (2)–(4), respectively. See Footnote 44 for detail regarding the set of family characteristics included as controls. Column (5) implements Equation 3 by further adding BMS/BMD race interaction terms and weights such that our *Black–Mulatto* coefficient measures differences among differently-classified siblings with approximately equal weighted distributions of family characteristics. Panels A and B analyze differences among sons and daughters, respectively. The bottom part of each panel presents observation counts and mean outcome variables by perceived child race. All non-race controls are interacted with cohort fixed effects. Observations are weighted by their inverse link probabilities as described in Section 4.2. Standard errors are clustered by (enumeration district)×(year). All controls come from the child's census year, rather than from their linked adult self.

Appendix Table A7: Gaps in living in a southern state in *t+20*, by perceived child race and gender

(a) Sons

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	0.100***	0.020***	0.017***	0.016***	0.009*	0.002
	(0.002)	(0.001)	(0.001)	(0.001)	(0.005)	(0.007)
Mulatto - White	0.481***	-0.041***	-0.051***	-0.053***		
	(0.003)	(0.001)	(0.001)	(0.001)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMS Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	25,106,397	25,106,397	25,106,012	25,106,012	25,106,012	10,896,899
R-Squared	0.173	0.799	0.808	0.808	0.808	0.938
Count Black	1,206,427	1,206,427	1,206,416	1,206,416	1,206,416	330,125
Count Mulatto	301,405	301,405	301,401	301,401	301,401	92,259
Count White	23,598,565	23,598,565	23,598,195	23,598,195	23,598,195	10,474,515
Mean[Outcome | Black]	0.788	0.788	0.788	0.788	0.787	0.799
Mean[Outcome | Mulatto]	0.686	0.686	0.686	0.686	0.686	0.685
Mean[Outcome | White]	0.205	0.205	0.205	0.205	0.205	0.208
Count Mixed Siblings	13,376	13,376	13,376	13,376	13,376	5,625

(b) Daughters

	(1)	(2)	(3)	(4)	(5)	(6)
Black - Mulatto	0.087***	0.011***	0.010***	0.010***	-0.016**	0.006
	(0.003)	(0.001)	(0.001)	(0.001)	(0.007)	(0.012)
Mulatto - White	0.492***	-0.028***	-0.036***	-0.037***		
	(0.003)	(0.001)	(0.001)	(0.001)		
(Year)X(Age+Birth Order) FE	Yes	Yes	Yes	Yes	Yes	Yes
(Year)X(State) FE	No	Yes	No	No	No	No
(Year)X(Enum. Dist.) FE	No	No	Yes	Yes	Yes	No
(Year)X(Fam. Char.)	No	No	No	Yes	Yes	No
BMD Interactions	No	No	No	No	Yes	No
(Year)X(Fam.) FE	No	No	No	No	No	Yes
Observations	17,090,136	17,090,136	17,089,585	17,089,584	17,089,584	6,247,274
R-Squared	0.160	0.823	0.831	0.831	0.831	0.947
Count Black	540,466	540,466	540,445	540,445	540,445	90,536
Count Mulatto	153,984	153,984	153,977	153,977	153,977	30,810
Count White	16,395,686	16,395,686	16,395,163	16,395,162	16,395,162	6,125,928
Mean[Outcome | Black]	0.794	0.794	0.794	0.794	0.794	0.787
Mean[Outcome | Mulatto]	0.707	0.707	0.707	0.707	0.707	0.681
Mean[Outcome | White]	0.214	0.214	0.214	0.214	0.214	0.217
Count Mixed Siblings	5,866	5,866	5,866	5,866	5,866	1,470

Standard errors in parentheses

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

Note: Table shows regressions measuring gaps in an indicator for living in a southern state twenty years later, by perceived child race among children age 3–18 in 1870, 1880, 1910, and 1920. Columns (1)–(4) follow Equation 1, starting with age and birth order fixed effects and adding controls for state fixed effects, enumeration district fixed effects, and family characteristics in columns (2)–(4), respectively. See Footnote 44 for detail regarding the set of family characteristics included as controls. Column (5) implements Equation 3 by further adding BMS/BMD race interaction terms and weights such that our *Black–Mulatto* coefficient measures differences among differently-classified siblings with approximately equal weighted distributions of family characteristics. Panels A and B analyze differences among sons and daughters, respectively. The bottom part of each panel presents observation counts and mean outcome variables by perceived child race. All non-race controls are interacted with cohort fixed effects. Observations are weighted by their inverse link probabilities as described in Section 4.2. Standard errors are clustered by (enumeration district)×(year). All controls come from the child's census year, rather than from their linked adult self. Southern states are defined as: Texas, Louisiana, Mississippi, Alabama, Georgia, South Carolina, Florida, North Carolina, Tennessee, Virginia, Maryland, Arkansas, and the District of Columbia.

Appendix Table A8: Differences in distinctively African American names among differently-classified siblings, by perceived child race and gender

	General Population		Differently-Classified Siblings	
	(1) Sons	(2) Daughters	(3) Brothers	(4) Sisters
Black - Mulatto	0.018***	0.014***	0.002	0.001
	(0.000)	(0.000)	(0.002)	(0.002)
Constant	0.211***	0.210***	0.229***	0.222***
	(0.000)	(0.000)	(0.001)	(0.001)
Observations	6,254,865	6,342,604	50,737	54,240
R-Squared	0.001	0.001	0.000	0.000
Count Black	5,201,963	5,209,213	26,612	28,194
Count Mulatto	1,052,902	1,133,391	24,125	26,046
Mean[Outcome | Black]	0.228	0.224	0.231	0.223
Mean[Outcome | Mulatto]	0.211	0.210	0.229	0.222

Note: Table shows differences in $\mathbb{P}[Black \cup Mulatto | FirstName, Gender]$ among Black- vs. Mulatto-coded children in the general population and in our differently-classified siblings (BMS/BMD) samples. The dependent variable is calculated as the share of children who are coded as Black or Mulatto among all same-gender children age 3–18 with the same first name in a child's census cohort (restricting to those coded as Black, White, or Mulatto). Columns (1) and (2) show differences among children in the general population. Columns (3) and (4) show differences among BMS/BMD families, with observations weighted proportional to the inverse share of their perceived race within a family (see Section 5 for detail on these weights). Differences for sons are shown in columns (1) and (3) while differences for daughters are shown in columns (2) and (4). Standard errors are clustered by (enumeration district)×(year).

B Race instructions to census enumerators, by year

- **1870:** *"Color* White (W), black (B), mulatto (M), Chinese (C), Indian (I)... 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. Important scientific results depend upon the correct determination of this class in schedules 1 and 2."
- **1880:** *"Color* 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. Important scientific results depend upon the correct determination of this class in schedules 1 and 5."
- **1900:** *"Color or race* Write "W" for white; "B" for black (negro or of negro descent); "Ch" for Chinese; "JP" for Japanese, and "In" for Indian, as the case may be."
- **1910:** "*Color or race* Write "W" for white; "B" for black; "Mu" for mulatto; "Ch" for Chinese; "Jp" for Japanese; "In" for Indian. For all persons not falling within one of these classes, write "Ot" (for other), and write on the left-hand margin of the schedule the race of the person so indicated... For census purposes, the term "black" (B) includes all persons who are evidently full-blooded negroes, while the term "mulatto" (Mu) includes all other persons having some proportion or perceptible trace of negro blood."
- **1920:** "*Color or race* Write "W" for white, "B" for black; "Mu" for mulatto; "In" for Indian; "Ch" for Chinese; "Jp" for Japanese; "Fil" for Filipino; "Hin" for Hindu; "Kor" for Korean. for all persons not falling within one of these classes, write "Ot" (for other), and write on the left-hand margin of the schedule the race of the person so indicated... For census purposes the term "black" (B) includes all Negroes of full blood, while the term "mulatto" (Mu) includes all Negroes having some proportion of white blood."
- **1930:** "*Color or race* Write "W" for white, "B" for black; "Mex" for Mexican; "In" for Indian; "Ch" for Chinese; "Jp" for Japanese; "Fil" for Filipino; "Hin" for Hindu; "Kor" for Korean. For a person of any other race, write the race in full.
  - *Negroes.* A person of mixed white and Negro blood should be returned as a Negro, no

matter how small the percentage of Negro blood. Both black and mulatto persons are to be returned as Negroes, without distinction. A person of mixed Indian and Negro blood should be returned a Negro, unless the Indian blood predominates and the status as an Indian is generally accepted in the community.

- *Indians.* A person of mixed white and Indian blood should be returned as Indian, except where the percentage of Indian blood is very small, or where he is regarded as a white person by those in the community where he lives. (See par. 151 for mixed Indian and Negro.) For a person reported as Indian in column 12, report is to be made in column 19 as to whether "full blood" or "mixed blood," and in column 20 the name of the tribe is to be reported. For Indians, columns 19 and 20 are thus to be used to indicate the degree of Indian blood and the tribe, instead of the birthplace of father and mother.
- *Mexicans.* Practically all Mexican laborers are of a racial mixture difficult to classify, though usually well recognized in the localities where they are found. In order to obtain separate figures for this racial group, it has been decided that all person born in Mexico, or having parents born in Mexico, who are not definitely white, Negro, Indian, Chinese, or Japanese, should be returned as Mexican ("Mex").
- *Other mixed races.* Any mixture of white and nonwhite should be reported according to the nonwhite parent. Mixtures of colored races should be reported according to the race of the father, except Negro-Indian (see par. 151)."

• **1940:** *Color or Race* Write "W" for white; "Neg" for Negro; "In" for Indian; "Chi" for Chinese; "Jp" for Japanese; "Fil" for Filipino; "Hi" for Hindu; and "Kor" for Korean. For a person of any other race, write the race in full.

- *Mexicans.* Mexicans are to be regarded as white unless definitely of Indian or other nonwhite race.
- *Negroes.* A person of mixed white and Negro blood should be returned as Negro, no matter how small a percentage of Negro blood. Both black and mulatto persons are to be returned as Negroes, without distinction. A person of mixed Indian and Negro blood should be returned as a Negro, unless the Indian blood very definitely predominates and he is universally accepted in the community as an Indian.
- *Indians.* A person of mixed white and Indian blood should be returned as an Indian, if

enrolled on an Indian agency or reservation roll, or if not so enrolled, if the proportion of Indian blood is one-fourth or more, or if the person is regarded as an Indian in the community where he lives.

– *Mixed Races.* Any mixture of white and nonwhite should be reported according to the nonwhite parent. Mixtures of nonwhite races should be reported according to the race of the father, except that Negro-Indian should be reported as Negro."