

Race, Skin Color, and Economic Outcomes in Early Twentieth-Century America

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We study the effect of race on economic outcomes using unique data from the first half of the twentieth century, a period in which skin color was explicitly coded in population censuses as “White,” “Black,” or “Mulatto.” We construct a panel of siblings by digitizing and matching records across the 1910 and 1940 censuses and identifying all 12,000 African-American families in which enumerators classified some children as light-skinned (“Mulatto”) and others as dark-skinned (“Black”). Siblings coded “Mulatto” when they were children (in 1910) earned similar wages as adults (in 1940) relative to their Black siblings. This within-family earnings difference is substantially lower than the Black-Mulatto earnings difference in the general population, suggesting that skin color in itself played only a small role in the racial earnings gap. To explore the role of the more social aspect that might be associated with being Black, we then focus on individuals who “passed for White,” an important social phenomenon at the time. To do so, we identify individuals coded “Mulatto” as children but “White” as adults. Passing for White meant that individuals changed their racial affiliation by changing their social ties, while skin color remained unchanged. We compare passers to their siblings who did not pass. Passing was associated with substantially higher earnings, suggesting that race in its social form could have significant consequences for economic outcomes. We discuss how our findings shed light on the roles of discrimination and identity in driving economic outcomes.

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This paper addresses a central question in the social sciences: how does race affect economic outcomes?¹ We study the effects of a visual form of race—one’s skin color—and of a social form of race—one’s racial identity.

The first half of the paper studies the effect of race in the form of skin color on economic outcomes. This question is challenging to address because skin color is typically correlated with other factors that affect economic outcomes such as family background. To study the causal effect of skin color on economic outcomes, an ideal experiment would randomize only the skin color of children at birth and then compare siblings’ economic outcomes by color later on in life. While such controlled randomization of skin color at birth does not occur, we use a unique historical setting and unique data from the first half of the 20th century, a period in which skin color was explicitly coded in population censuses as “White,” “Black,” or “Mulatto.”²

We exploit the fact that skin color could vary within African-American families by genetic accident. Figure 1 presents one observation that illustrates our empirical exercise and how we constructed our data. Panel A of this figure shows the 1910 census record of John and Florence Spencer, who had four sons, two of whom were coded by the enumerator as Black (9-year-old Maurice and 4-year-old John) and two as Mulatto (11-year-old Isaac and 7-year-old Edward).³ Panel B shows the Mulatto son Isaac and his Black 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 (\$72,000 in today’s dollars). His Black brother, John, lives in South Carolina and works as a simple laborer for the much lower annual

¹This question has 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). 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). 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).

²This paper uses a number of terms that were widespread in the time period we study, but 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 oftentimes refer to individuals as Mulatto or Black to be loyal to the classification used by the census and to the decision made by the enumerator at the time.

³The classification of Isaac is unusual because it uses the letter “C” (for Colored) instead of the required “Neg.” This term was used by the enumerator for all others who were classified as African-American.

salary of \$480 (\$19,000 in today's dollars). Both of them are classified as African-American.

We construct the rest of the data in a similar way, identifying and linking across censuses all such families with siblings of different color. Specifically, we start the construction of the data by identifying siblings who in 1910 lived in all 12,000 such African-American families in which census enumerators classified some children as light-skinned ("Mulatto") and others as dark-skinned ("Black"). We then construct a panel dataset of individuals who were children in 1910 and adults in 1940 by digitizing and linking records (based on name, age, and state of birth) across population censuses.⁴ The panel nature of our data allows us to track individuals and their siblings from their childhood households in 1910 to their locations and outcomes as adults in 1940.

Our panel of siblings who vary in color allows us to disentangle the direct effect of skin color on economic outcomes from differences in human capital that are correlated with color but driven by differences in family background. Specifically, comparing siblings who vary in skin color allows us to hold family background constant and directly measure the effect of skin color. Moreover, a comparison of the Black-Mulatto differences in outcomes within family vs. in the population allows us to infer the role of family background in the educational and economic gaps observed in the population. This inference is possible because the Black-Mulatto differences in the population are driven by both family background differences and discrimination, but family background differences cannot explain Black-Mulatto differences between siblings, meaning the gaps within families are driven solely by discrimination.

We find small and statistically insignificant differences in outcomes between the Black and Mulatto siblings, both in childhood (education attainment) and as adults (earnings and migration). In contrast, we document large differences in outcomes of Blacks and Mulattoes in the general population.⁵ These findings suggest that skin color in itself played only a small role in

⁴We note that an inherent limitation of such linking across historical censuses is that it results in low linkage rates, which stands at 15 to 30% in our case, depending on how strict the match is set to be. These match rates are comparable with other papers using historical individual-level data (Abramitzky et al., 2012a,b; Long and Ferrie, 2012). Section 3.3 and Appendix A detail our linking procedure.

⁵In the population, we find that lighter-skinned African-Americans completed 1.1 more years of schooling on average, and their weekly salaries were 11.4% higher. These differences in income are fully explained by the

the Black-Mulatto earnings gap, and that family background drove most of this gap.⁶

There are a number of potential concerns with our identification strategy and interpretation of the findings. First, we cannot know for sure that siblings shared the same biological parents (although children explicitly listed as stepchildren are not part of our sample). Second, differences between Black and Mulatto siblings in underlying “true” skin color could be smaller than such differences in color in the population. Third, parents who have children who substantially vary in color may reinforce or counteract social attitudes towards color by reallocating resources that would not have been reallocated if skin color did not vary within the family. Fourth, attenuation bias in estimates of the effect of color may be exacerbated by the use of family fixed effects (Griliches, 1979). We discuss and address these concerns in more detail in sections 4.2 and 4.3. We use various robustness exercises to show that the within-family comparisons are unlikely to be biased by misclassification of non-siblings as siblings. Still, our ability to extrapolate the effects of color from the local setting of families that vary in color to the general population depends on further assumptions that are usually necessary in studies of siblings or twins. Although our ability to extrapolate the effects of color from within families to the population in general necessarily relies on the standard assumptions required in studies of siblings or twins (e.g., Ashenfelter and Krueger, 1994), we argue that these assumptions are not unreasonable in our setting.

The second half of the paper explores the role of a more social aspect that might be associated with being Black. While we find that skin color in itself did not play a large role among African-Americans at the time, segregation and discrimination against all African-Americans was explicit, sometimes official, and widespread during this era. It was the boundary between Whites and all others that defined who was to be discriminated against.⁷

differences in educational attainment. We also record different migratory patterns between lighter- and darker-skinned children hailing from the same county and even the same neighborhood: those classified as “Mulatto” are more likely to be in a different state and out of the South in 1940. Similarly to the case of earnings, differences in migration patterns are explained by differences educational attainment.

⁶We note that discrimination in previous generations may have created differences in family background in the first place. If that is the case, family background perpetuates discrimination from one generation to the next.

⁷The decision of where to put this boundary between the races was a social choice in its essence. While in Latin America and in the Caribbean Islands, racial distinctions were less binary, the U.S. converged by the early 1910’s

We focus on individuals who “passed for White,” a term used to describe African-Americans who redefined themselves as White by erasing any connections to their African-American community and heritage.⁸ For most African-Americans, the distinction between the visual and the social was irrelevant because they looked distinctively Black and could not be mistaken for being White. However, for the minority who could be considered White merely by their physical appearance, staying a member of African-American community was a matter of choice. We ask, what was the earnings premium associated with passing as White?

To address this question, we identify individuals coded as “Mulatto” when they were children, but as “White” when they were adults. Specifically, we restrict our attention to light-skinned African-American children whom enumerators coded as Mulatto in 1910, and we define “passers” as those whom enumerators coded as “White” in the 1940 census.⁹

The earnings difference between those who passed and those who did not reflects both the returns to passing for White and selection into passing. Selection into passing could occur both across families and within families. To control for selection across families we compare passers with their siblings who did not pass. We find that about 10%-13% of 1910 Mulattoes were classified as White in 1940, and Mulattoes who passed for White earned 31%-42% more than non-passers. Because passers tended to be more educated than non-passers, the increase in earnings from being classified as White decreases when we control for education, though it remains considerable at 20%-31%. When comparing siblings we find that passers earned 14%-21% more than their brothers who did not pass. These figures are lower, suggesting that passers were more likely to come from more productive families. Nevertheless, differences between siblings persist

to the One Drop rule which defined an individual as Black if he was known to have even the slightest African ancestry (Williamson, 1980).

⁸One famous example of passing was the case of Anatole Broyard (Gates, 1997). Broyard was born to an African-American family and grew up in Brooklyn. He wanted to be a writer but did not want to be labeled as a “Black writer” so he passed for White. He moved to Manhattan, left his first Black wife, eventually married a White woman, and did not acknowledge his sisters in public. His children learned that he was African-American only after he died (Broyard, 2007). In contrast, (Myrdal, 1944) quotes an African-American teacher who decided against passing and explained that he did not want to give up his African-American culture, social ties and status, or to bear the psychological burden of fearing someone would find out he was actually African-American.

⁹We note that unlike in the 1910 census, in the 1940 census the “Mulatto” option was no longer available, and enumerators could code individuals as either “White” or “Negro”, the latter term being used to label both Blacks and Mulattoes.

when we compare across the social divide between Whites and African-Americans.

Overall, the findings of this paper suggest that lighter-skinned African-Americans did not enjoy a wage premium in excess of the return to their higher level of schooling. Siblings show remarkably similar educational outcomes by skin color on average, suggesting that family background is driving the overall differences in educational attainment between dark- and light-skinned African-Americans. In contrast, we find that the race in its social form was associated with much higher earnings gaps. Thus one way in which dark skin did matter was in ruling out the option of assimilating into the dominant group for most African-Americans who could not pass simply because of their skin color.

The remainder of the paper proceeds as follows. Section 1 puts our paper in the broader historical context. Section 2 places the paper in the context of several related branches of literature on race and economic outcomes. Section 3 describes our data sources, samples, and the process of assembling the dataset. Sections 4 and 5 lay out the conceptual framework, empirical strategy, and main results. Section 6 concludes.

[Figure 1 about here.]

1 Historical context: Race and skin color in early 20th century America

“Me and a man was workin’ side by side
This is what it meant
They was paying him a dollar an hour, and they was paying me fifty cent
They said, “if you was white, ’t should be all right,
If you was brown, could stick around,
But as you black, hmm boy, get back, get back, get back”

—Big Bill Broonzy in “Get Back”

1.1 Segregation

African-Americans in the first half of the 20th century lived in a separate public realm due to the Caucasian majority's refusal to interact with them in public places. In the South, where 89% of African-Americans lived in 1910 (Bureau of the Census, 1913, Ch. 2, T. 14), segregation was set by law. The laws pertaining to the separate treatment of African-Americans were referred to as Jim Crow laws. 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 end 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 took great efforts to exclude 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); means of public transportation were regulated so that Blacks had to ride separate cars or occupy separate sections of the vessel (Stephenson, 1910). At the same time, disenfranchisement of African-Americans excluded them 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) show how the Ford Motor Company arbitrated discrimination against African-Americans by other companies, but still channeled Black workers to more demanding and dangerous manufacturing jobs. Sundstrom (1994) and Fishback (1984) highlight that even though in lower roles some occupations were not segregated, Blacks were not allowed to supervise Whites. Labor unions were another source of discrimination, as most did not accept Blacks 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.” Whites’ desire not to share their neighborhoods with Blacks is considered an important determinant of the suburbanization and urban development that occurred in the second half of the 20th century, even after the Civil Rights acts of the 1960’s Boustan (2010).

1.2 Intermediate skin tones in a racially segregated society

Whenever racial segregation emerged, the question of “What is a Negro?” had to arise (Stephenson, 1910). One-fifth of all African-Americans were classified as Mulatto in the 1910 census, and the true proportion of those with some European ancestry may have been as high as three-quarters of all African-Americans at the time (Cummings and Hill, 1918). Note, however, that Black-White marriages were very rare in the early 20th century (Fryer, 2007), and much of the variation in the degree of European ancestry originated during the era of slavery (Williamson, 1980).

Until 1915, some states still considered those with only small proportions of African ancestry to be White. However, by 1915 all states had converged to the One Drop Rule, which treated any individual with any African ancestry as a “Negro.” Thus, even people of African-American descent who looked “fully European” were considered Black if their ancestry was known.

The One Drop Rule treatment excluded all African-Americans from the White public sphere regardless of their skin tone. Nevertheless, differences between light- and dark-skinned African-Americans date back to antebellum times when Free Blacks were more likely to be light skinned, more educated, and richer than darker-skinned Free Blacks. Light-skinned Free Blacks in 1860 had twice as much wealth as dark-skinned Free Blacks (Bodenhorn and Ruebeck, 2007). Culturally, while mixed-race individuals sometimes tried to set themselves apart, this distinction faded during the first decades of the 20th century. Some of the most prominent African-American leaders such as Booker T. Washington, W. E. B. Du Bois, and Walter Francis White were actually of mixed ancestry. Horace Mann Bond, another prominent figure of mixed race, explains in a

1931 paper how the African-American community reached this pooling equilibrium over skin colors:¹⁰

“Time was when there were blue-vein societies [...] 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 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.”

Mulattoes in the census The distinction 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 introduced into the census. With the exception of the 1900 census, all censuses until 1920 asked enumerators to distinguish between people of full and mixed African ancestry. Cummings and Hill (1918, p. 209) admit that such ancestry-based classification is questionable and subjective.

By 1910, 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). It appears that enumerators indeed applied an appearance-based (rather than a “blood-based”) standard. Table a shows that when one of the parents was classified as Mulatto and the other as Black, still between 27% and 45% of the children were classified as Black.¹¹

¹⁰See Bond (1931). Blue-vein societies, which he mentions in the quotation, were African-American social clubs that accepted only members whose skin was light enough so that their veins could be seen through it.

¹¹The stark difference between the share of children classified as Black between Black-Mulatto and Mulatto-Black couples has not been verified but is likely to be the outcome of some context-based classification that is more likely to classify children in households of higher status as Mulattoes. Note that the number of children is lower than the other three categories. Family status does not vary between siblings, though, and this is another advantage of the siblings' sample.

Genetically, skin color is determined mainly by genes that affect the production of melanin, which is the primary pigment giving human skin, hair, and eyes their color. Many genes have been found to affect melanin production. The most prominent and probably most studied one is melanocortin 1 receptor (MC1R),¹² but other genes affect melanin production (Graf et al., 2005; Bonilla et al., 2005). Pertinent for our purposes is the fact that skin color genes are not completely dominant. Thus, for example, homozygous parents of different skin colors may generate varying intermediate levels of skin color and heterozygous parents may bear children who are either lighter or darker than both parents

1.3 Passing

In a segregated public sphere, light-skinned African-Americans could sometimes “pass for White.” As long as nobody noticed passers as such and took them as any other Caucasians, 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 to later 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 the segregated arrangements, agents in White markets were not willing to hire or to trade with African-Americans in many cases, so professional passing opened many doors.¹³ However, professional passing was not complete if the person returned at the end of the day to his family or community that did not identify as White. Complete passing required both professional and social passing. In many cases, professional passing was a step on the way to complete passing.

¹²See Rees (2003) for further review of MC1R's role in the determination of skin color, Gerstenblith et al. (2007) for a survey of different phenotypes associated with different variants of the MC1R gene.

¹³The disutility of Whites from doing business with African-Americans was evident in the legal treatment of racial misidentification (blaming a White person for actually being Black) as defamation *per se* under the category of “words disparaging to a person in his trade, business, office, or profession.” Defamation *per se* meant that a White person who has been accused of being “Negro,” could sue the accuser for the recovery of damages even without proving damages but simply by proving that the accusation was made. (Stephenson, 1910)

Looking at the potential economic benefits—Whites were earning twice as much as Blacks for each level of schooling in 1940—one may ask why all African Americans didn't pass. First, not everyone could; both light skin color and sufficient skills to “act White” were necessary. More surprisingly, even those who could pass did not always choose to do so. Myrdal (1944) quotes an African-American teacher 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 from fearing someone would reveal he was Black, the higher social status he could enjoy in the African-American community relative to in 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.

While census enumerators may have accidentally classified some African-Americans as White even if the latter did not intentionally pass, those who did pass completely were very likely to have been accepted by the census enumerator, as by the community, as White. By 1930, the distinction between Mulattoes and Blacks had been removed from the censuses. The line between Whites and African-Americans, however, did not change, and individuals who would have been classified as either “Mulatto” or “Black” in earlier censuses became “Negro.”

2 Related literature on racial inequality

Numerous studies document differences in economic outcomes between Blacks and Whites in American labor markets. For instance, 2010 data from the US Bureau of Labor Statistics show that white males earned 33% higher wages than black males (2011, Table 14). Large differences have also been documented between African-Americans with different skin tones: by the early 1990s, light-skinned African-Americans earned 23% higher hourly wages than dark-skinned African-Americans (Goldsmith et al., 2007).

Studies of these wage gaps typically try to disentangle differences in productivity from discrimination that implies unequal pay by race despite equal productivity. To identify the role of 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 discrimination. Wage gaps have been shown to decrease by roughly half when such controls are added (e.g., Altonji and Blank, 1999; Altonji and Pierret, 2001; Hughes and Hertel, 1990; Keith and Herring, 1991). It is nevertheless unclear whether remaining differences represent discrimination or merely additional unobservable differences in productivity, and whether some of the controls themselves capture aspects of discrimination.

One way to compare individuals who differ only by race is to conduct experiments—specifically, correspondence studies—where the researcher creates fake agents who interact indirectly with real agents in the market. Such studies allow the researchers to manipulate the perceived identity of the imaginary agent. For example, Bertrand and Mullainathan (2004) submit similar resumes under names that are either typically African-American or typically Caucasian, and find that (made-up) 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 (2012), who finds “Arab-sounding” names disadvantage sellers in an Israeli online used car market, and Doleac and Stein (2012), who find a visual cue that the seller is black decreases responses to an online advertisement for a music player. Observational studies complement such correspondence studies; they are less able to cleanly identify discrimination, but they can follow people over time, analyzing more comprehensive consequences of discrimination and other factors that may affect racial inequality.

In the above studies, race is signaled through skin color or through names. Other studies have investigated the additional cues people use in assigning race to individuals. Freeman et al. (2011) show that the clothes worn by computer-generated images of individuals of intermediate color affected how respondents classified them racially.

Akerlof and Kranton (2000) suggests that such priors are created in the first place because members of a group adhere to a set of actions that are considered acceptable among the group’s members. Austen-Smith and Fryer (2005) takes a similar approach and apply it to the decision African-American children make over their schooling level, where schooling is both a signal of

“acting White” and economically desirable. The phenomenon of “passing for White” offered light-skinned African-Americans the opportunity to choose the group to affiliate with and behave like. This paper is the first study to analyze this change of racial affiliation in a time when some labor markets were explicitly segregated.

Our finding that family background has an important role in explaining differences by skin color has a strong backing in previous literature. Neal and Johnson (1996) shows that family background can significantly affect AFQT scores, which are in turn a strong predictor of earnings; Cunha and Heckman (2008) suggests ways in which cognitive and non-cognitive skills are acquired in early childhood through parental investment.

We are able to control for differences in family background by comparing siblings of varying skin tones. Our sample from the 1910 census is unique in that it allows perception of race to vary within a family, whereas race or ethnicity is generally thought to be fully shared by all members of the family. Rangel (2007) uses a similar methodology of comparing siblings in a cross-section, looking at self-reported color classifications in the 1991 Brazilian Census of Population and the 1987 Brazilian Survey of Nutrition. It finds that lighter-skinned children receive slightly more education, and are more likely to host their darker-skinned sibling than vice versa during adulthood.

3 Data and matching

Our main data sources are the U.S. Censuses of 1910 and 1940. According to the 72-years rule, individual census records are made public 72 years after the census took place. These censuses are, nevertheless, comprised of handwritten forms saved in microfilms in the National Archives. Thanks to recent interest in family history, some organizations have digitized all names, ages, and other identifying variables from these records to enable individuals to search for their ancestors. FamilySearch.org, one such organization, kindly agreed to share its digitized version of the 1910 and 1940 censuses with us, which enabled us to construct our basic samples in 1910 and to link them to 1940. For these samples, we digitized additional variables that were previously available

only in handwritten form, enabling us to study outcomes such as literacy, years of schooling, employment status, and income. We complement these data with IPUMS and NHGIS data as described below.

3.1 Families with both Black and Mulatto children

Our main sample is families in which some children were classified as Black and others as Mulatto. Our data collection therefore starts with finding all families with differently-classified children in the 1910 U.S. census. Children are individuals identified as a “son” or a “daughter” of the head of their household.¹⁴ We drop children below the age of 3 and above the age of 18 since very young children have not reached their adult skin tone and older children who stay in the household are highly selected. We then construct a sample of all sons in families that have both a Black and a Mulatto son, and an equivalent sample for daughters.¹⁵ Not that although we use the term “sample”, these are 100% of all such families in 1910. We also record details of the other family members, and especially characteristics of the household’s head. We refer to these samples throughout as BMS (“Black-Mulatto-sons”) and BMD (“Black-Mulatto-daughters”) respectively.¹⁶

We find 19,910 sons in 6,047 families that have some sons classified as Black and the others as Mulatto, and 20,834 daughters in 6,413 families that meet the equivalent color condition for girls.

¹⁴Enumerators were supposed to identify stepchildren as such, so two sons of the same household head should be at least half brothers; no census data explicitly addresses whether these sons have the same mother. The possible consequences of illegitimacy, remarriage, adoption, and relationship misclassification are discussed in section 4.2. We also possibly miss some siblings that are not sons of the household head, such as grandchildren when households are headed by a grandparent, or children of lodgers.

¹⁵We initially considered also applying our identification strategy to the small number of households containing both “white” and “mulatto” children. However, examination of the census records for these households 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 the handwritten codes used to distinguish “White” from “Mulatto”: “W” and “M.” (Many enumerators ignored the instruction to identify “Mulattoes” with the more distinctive “Mu.”) Because of the small sample and high error rate, we decided to ignore these households altogether, focusing entirely on households with both “Black” and “Mulatto” sons.

¹⁶Another 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 and would also contain families that enter neither: families where race varies only across the gender of siblings. We proceed only with the BMS and BMD, and analyze them separately as we can match only males to their 1940 records, and the considerations in investments in sons and daughters during childhood in this era were very different.

Table 1 shows the distribution of color classifications of sons and parents in the BMS sample.¹⁷ Note that slightly over two thirds of the households have at least one parent classified as “Mulatto”, and 16% of the households have both parents classified as “Black.” Such cases are consistent with an appearance-based classification, since children could have received their skin color from recessive genes carried by their parents. About one quarter of households in our sample have either a missing father (20%) or mother (4%).¹⁸ We drop these families for robustness specifications in section 4.2.

[Table 1 about here.]

Although it is rare for brothers to receive different racial classifications, the phenomenon is not limited geographically. As Figure 2 shows, these families are not concentrated in just a few counties. We know, therefore, that many different enumerators gave different racial classifications to siblings in the same household, which also increases confidence that such households represent a genuine physical phenomenon.

[Figure 2 about here.]

3.2 Benchmark samples: a random sample and a sample of Black-and-Mulatto-siblings’ neighbors

We compare differences in outcomes between siblings to differences in the African-American population as a whole and to neighboring African-American families.

For the population as a whole, we combine a 1% sample of families in the 1910 census with Black children only from IPUMS (Ruggles et al., 2010) with a 5% sample of families with Mulatto children only from FamilySearch. Data from IPUMS have the advantage that all information has

¹⁷The terms “father” and “mother” are used somewhat imprecisely in this table and throughout. “Father” refers to the household head in the typical male-headed household. In the case of female-headed households, “father” refers to the husband (if any) of the female head, or much more often, is listed as absent. “Mother” refers either to a female household head, or to the male head’s wife (if any; otherwise she is listed as absent).

¹⁸These rates of single-parenthood are higher than those found in a random sample of all African-American households (from IPUMS), where 12.7% have absent fathers and 4.3% absent mothers.

already been digitized, but the 1% sample yields too small a number of Mulatto-only families, which are rarer in the population. Our random sample for the population is thus representative separately of Black-only and Mulatto-only families in the population, but oversamples Mulatto families relative to Black. Note that we exclude mixed-color families from this sample. To make the random sample comparable to the siblings sample, we restrict our attention to children between the ages of 3 and 18 in families that have at least two children in this age range.

Because siblings share a neighborhood as well as a family background, but the Mulatto and Black families in our population sample have different geographic distributions, differences between estimates using the sibling sample and the population sample could be driven by the effect of neighborhood as well as by family background. To investigate the importance of neighborhood relative to family background, we therefore construct a second comparison sample consisting of Mulatto-only and Black-only families who live near BMS or BMD families. We use two methods to find neighbors, and pool the individuals we find using each. First, for each son or daughter in a BMS or BMD family, we took the child of the same gender who was closest on the census rolls (up or down) and no more than two years different in age. Second, for each BMS or BMD family we took the Mulatto family and Black family within the same enumeration district that had the most similar household structure in terms of number of children, and their genders and ages.¹⁹ Being in the same enumeration district meant that individuals were also color-coded by the same enumerator, which removes the effects of any differences enumerators may have had over who they considered to be Mulatto. Households in the same neighborhood were also close geographically, and had access to similar local public goods.²⁰

Table 2 provides a variety of summary statistics for the BMS sample and for the random and neighbors samples. The random and neighbors samples' means are presented separately for households with Black and Mulatto sons. The characteristic that varies the most between the sample is the number of sons in the household, and it varies by sample because of the way we

¹⁹We weight distances in different attributes of a family to assess how close they are to each other overall.

²⁰Enumeration districts vary in geographic and population size. In 1910, the lowest quartile of districts had no more than 800 individuals while the highest quartile had at least 1,800 individuals listed.

constructed the samples. The siblings sample has the highest number of sons in the household on average because the greater the number of sons in a household the higher are the chances that one of them will have a different color classification. Even though we restricted the random sample to have only families with at least two sons, the number of sons is still lower in the random sample than the siblings sample. In the neighbors sample, since some of the neighbors were selected only because they were of a close age and the same gender, some of them came from households with only one son and therefore the number of sons in the household there is even lower than in the random sample. The average age of children follows the same pattern since having more sons in the household also means that younger or older households (with only the oldest or youngest son present) are less likely to be selected as we have more children in the household and the younger households are more numerous because of population growth.

Besides these two mechanical differences in characteristics, the sibling sample is not selected evenly across geographic locations. Children in the siblings sample come from more rural counties with a higher share of African-Americans relative to the general population. This geographic selection creates differences in other characteristics such as literacy and occupational structure. The sample of neighbors has more similar characteristics. Both literacy and occupational structure are closer to the ones of the siblings sample. Neighbors still come from more African-American counties because of the requirement to have both Mulatto and Black neighbors.

[Table 2 about here.]

3.3 Matching across censuses

To record outcomes later in life we match the children in the samples described above from the 1910 census to their records as adults in 1940. To avoid selecting our matched sample on outcomes, we rely only on predetermined information and match by name, age, and birth state. We do not use current location or race to avoid biasing the sample towards those who didn't migrate or didn't pass for White. We do not attempt to match girls, since most women changed

their last names upon marriage.

We use a novel record linkage method described in detail in Appendix A. In summary, our method combines several existing tools and allows us to link individuals with varying levels of confidence. The matching algorithm considers whether a pair of records with the same birth state are a correct match by measuring the distance between the name in the 1910 record and in the 1940 record, along with the distance between the birth years. From these distances it outputs a single “link score.” The link score is equal to 1 for a pair of records where name, birth year and birth state match exactly, and decreases to 0 as the distances between names and birth years increase. Birth states are required to match exactly. Since people may misremember their ages and enumerators may mishear or misspell names, some leeway should be given to how names and ages match. However, as leeway increases so too does the probability of false matches. Whether we consider a potential match as true (and by definition unique) depends on the link score that the best and second-best potential matches get. A match rule in our context consists of a link score threshold that the best potential match must exceed, and a distance by which the link score of the best potential match must exceed that of the next-best match. Both the threshold and distance requirements must be met for the best potential match to be considered the true match. A strict match rule imposes a high threshold for the best match and a large distance link scores.

The main challenge in choosing the threshold and distance is maximizing the “signal-to-noise” ratio while minimizing any selection bias into the sample. As we make the rule for matching stricter, the sample changes in two ways. On the one hand, false matches are less likely to occur. On the other hand, as we filter out false matches we filter out true matches along with them. This may increase selection bias because individuals with uncommon names or who were born in less populous states are more likely to be unique in these details and are therefore more likely to be matched.

Table 3 reports the number of matched records and various characteristics of those who matched relative to the whole population that is searched *in 1910*. Three matching rules are

considered. The first match, reported in columns (2) to (5), is a relatively lenient match and yields a matching rate of 34% which is equivalent to matching rates in other papers matching records across censuses (see Abramitzky et al., 2012a; Long and Ferrie, 2012). This match, nevertheless, shows some features indicating a possibly relatively high rate of false matches.²¹ A stricter match (columns (6) and (9)) results in a matching rate of 11%. This match is used for the analysis reported for all 1940 results.

Individuals are not randomly selected to the matched sample. Columns (2)-(3) and (6)-(7) show that literate children in households with literate heads of households are more likely to be matched. Children from richer households in which the home is owned by the family and there are more children are also more likely to be matched. Literate and richer households may have used more unique names which make an individual more likely to be uniquely matched. Most of the observations that we fail to match are not matched because they have more than one potential match that we can not distinguish from the best potential match.

[Table 3 about here.]

To make the matched sample more representative of the baseline sample we can assign varying weights to different observations according to how likely they are to be matched. In a similar fashion to survey data with a biased response rate, we calculate the probability of an observation to be matched given a set of background variables.²² We then give each weight the inverse of this probability, representing the average number of observations each matched observation represents. The resulting weighted characteristics of the matched samples are presented in

²¹False matches cannot be directly marked because if we could mark them we would not have included them in the sample in the first place. To get a sense of possible false match rates we look at the percentage of individuals who appear to have migrated or changed their race as an indicator that is correlated with false matches. While some people truly migrate or change racial classification, false matches are going to increase the incidence of such changes: matching falsely is likely to find individuals who happen to live in another state, for example.

²²

We estimate the probability to be matched by calculating the proportion of matched records within each combination of the following set of binary variables: Black, head literacy, head home ownership, whether a county urban share is greater than 0.25, whether the number of sons in the household is greater than four, and the set of the head's occupations shown in Table 3. We do not weight by children's literacy since it is missing for all children under 10 years of age.

columns (4)-(5) and (8)-(9). The matched sample is now much closer to the baseline sample that we tried to match. Literacy in 1910 is still higher among matched individuals since we do not take it into account in our re-weighting, but otherwise the statistics are quite similar. Our analyses in the remainder of the paper are nevertheless unweighted since the weighted analyses yield similar estimates to the unweighted ones but the unweighted analysis is more straightforward and does not require us to take into account additional statistical noise in the estimation of the weights.

3.4 Data entry

The digitized census records available through FamilySearch.org include only a limited selection of variables. In particular, only those items most useful for genealogical research were entered; fortunately, this includes key fields necessary for identifying our sample in 1910, and matching in 1940. Our analysis depends on a richer set of data, so we hired about 30 data entry clerks to digitize all other variables from scans of the original, handwritten census records. A fraction of all records were entered by a second clerk.²³ By digitizing the records we are able to see schooling (attendance, reading, writing, and completed years of schooling by 1940), income, employment, and occupation. The clerks also corrected a number of data entry errors present in the FamilySearch.org data. Overall, more than 200,000 observations were digitized for individuals in the sample and their family members in 1910 and 1940.

3.5 Non-wage income imputations

The 1940 census asked all individuals aged 14 or above to report their income from wages and salaries.²⁴ While this was a significant improvement over previous censuses this figure still lacked income from one's business, farm or other source. Having a non-negligible proportion of farmers even in 1940 we impute non-wage income using the census of 1950 where a sample

²³When clerks disagreed, a third clerk was given the record to enter. A majority rule was then applied to decide which value to take. If no two clerks agreed on a value we took the value of the clerk with the highest record of agreement with other clerks.

²⁴The exact definition was "Amount of money wages or salary received (including commissions)."

of respondents reported their income from labor and other sources of income separately. 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. We use a similar measure for wage income in order to find the deflator from 1950 to 1940 incomes that will allow to sum up wage and non-wage income. Results for total (non-wage-imputed) income and labor income will be presented separately.

4 Skin color and economic outcomes

4.1 Outcome differences by color in the population and between siblings

A naïve assessment of the effects of discrimination might simply compare the outcomes of individuals in different groups: for example, do darker-skinned individuals earn more than lighter-skinned ones? Since skin tone can be correlated with myriad confounding factors, a more informative assessment will attempt to control econometrically for (some of) these factors. We might therefore estimate an equation of the form:

$$y_i = \alpha + \beta \text{darkskin}_i + \mathbf{x}_i \gamma + u_i \quad (4.1)$$

for an outcome y_i , where darkskin_i is an indicator of individual i 's skin tone and \mathbf{x}_i is vector of observable attributes that are expected to affect the outcome in question and that may be correlated with race.

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 his adult outcomes. We control for differences in childhood environment by comparing siblings who grew up in the same family but differ in their skin tone to the extent that the enumerator classified them differently. We can reformulate equation 4.1 by decomposing the error term for individual i (raised in family f) into a family and an

individual-specific component:

$$y_{if} = \alpha + \beta \text{darkskin}_{if} + \mathbf{x}_{if}\gamma + \underbrace{\phi_f + \varepsilon_{if}}_{u_i}, \quad (4.2)$$

In our main within-family specification, we identify siblings based on their classification as either the sons or daughters of same household head. We have no direct information on their relationship to each other: illegitimacy, remarriage, adoption, or the misclassification of stepchildren as biological children could result in a failure to correctly identify biological siblings. Our identification of the effect of color is threatened by the existence of non-biological siblings only if they are systematically lighter or darker and get special treatment because of the fact that they have different biological parents. Our maintained assumption for this section is that this is not the case. We provide evidence for the robustness of our analysis to more restrictive definitions of siblings in section 4.2.

Since the sample of siblings is not random, it is not representative of the African-American community. As discussed in section 3.2, we collected information on similar children in the same enumeration districts as the children in the Black-and-Mulatto-siblings sample. The sample of neighbors provides a better benchmark for the results we observe for siblings because neighbors are collected from the same geographical units as siblings. They were also all coded by the same enumerator so any differences in color between children in the same enumeration district cannot be attributed to differences in enumerators' subjective perception of color.²⁵ We therefore also estimate a similar equation to 4.2 with enumeration district fixed effects instead of family fixed effects:

$$y_{id} = \alpha + \beta \text{darkskin}_{id} + \mathbf{x}_{id}\gamma + \phi_d + \varepsilon_{id}, \quad (4.3)$$

Table 4 presents differences in educational investments in childhood. The 1910 census asked

²⁵Digitization of the neighbors sample has not been digitized fully yet. Digitization is under progress and updated numbers should be available in future versions.

all Americans aged 10 or older whether they could read and whether they could write. Here we define y_i to be an indicator for whether the child was able to write. Columns (1)-(5) reports results for boys and Columns (6)-(10) reports results for girls. Columns (1) and (6) present the overall difference in literacy in percentage points. Illiteracy levels are higher for children classified as Black in the census: 37.8% relative to 28.2% among boys and 29.8% relative to 16.8% among girls.

It is unclear why gaps are larger for girls. One reason may be their slightly lighter skin on average, and higher tendency to be classified as Mulatto, which makes girls classified as Black more conspicuous as the targets for discrimination. Alternatively, light skin seems to have been relatively more valued for females in the marriage market.²⁶ Higher investment, then, may have yielded higher returns in the marriage markets for lighter-skinned girls. Unfortunately, we cannot study these girls' outcomes in 1940 because most girls changed their last names upon marriage, leaving us with insufficient information to match them between censuses.

[Table 4 about here.]

One way to control for family background is to include observable characteristics of the childhood household into the regression. Columns (2) and (7) report the difference by color controlling for several household characteristics. The remaining color gap that is not explained by these measures of family background is 4.8 percentage points for boys and 7.9 percentage points for girls. The contribution of the family characteristics to literacy all work in the direction we expect: if the head is literate then children are much more likely to be literate; if the family has more financial resources and owns their house then children are more likely to be literate. Farm laborers were both rural and low-skilled and therefore their children less likely to be able to read. Family characteristics seem to have smaller effects on girls' literacy and to decrease the effect of color by less.

We next turn to the sample of siblings who vary by color. Differences in literacy are significantly

²⁶Kotlikoff (1979) shows light skinned was associated with the price for slaves for female but not male slaves. For contemporary cultural references see, for example, Hurston (1937).

smaller between siblings, with a 1.3 percentage point gap among brothers and a 2.2 percentage point gap among sisters (columns (3) and (8)). Even though we can reject the null hypothesis that there is no difference between light- and dark-skinned sisters, both of the differences are significantly smaller than the gaps measured in the population, suggesting that color was not driving large differences in parental investment or differential treatment by teachers and peers.

Finally, we turn to the sample of neighboring children. This removes the variation between Blacks and Mulattoes that is correlated with geography and also allows us to compare Blacks and Mulattoes who were classified by the same enumerator. The construction of the neighbors sample is described in section 3.2.

We see large differences between neighbors by color, both unconditionally and conditional on indicators of family background. Again, they are larger for girls, and are much higher than the differences observed for the siblings, who live in the same enumeration districts. This finding rules out that differences in the population stem from Blacks and Mulattoes having different geographic distributions, or from varying subjective judgments on who is Black and who is Mulatto.

We next analyze the data that matches children forwards to their census records in 1940. As discussed in section 3.3 in more detail, we lose a significant number of observations, but gain longitudinal information. We include in the regressions of the matched sample only individuals who were matched and had at least one sibling or neighbor of the opposite color who was matched as well.²⁷

Table 5 reports differences in the total number of years in school attained by adulthood. In the population, those with darker skin attain 1.1 fewer years of schooling on average; within enumeration districts in 1910, the difference is also considerable at 1.3 fewer years. Siblings, on the other hand, differ by only 0.2 years.²⁸ These results are consistent with the differences in literacy observed during childhood in 1910.

²⁷Note this means we lose more observations from the within-household and within-district samples.

²⁸Statistical tests for the hypothesis that differences are the same between siblings and in the population are in progress and will be available in an upcoming updated version.

[Table 5 about here.]

In Table 6 we move to studying the differences in weekly labor income by color. Darker-skinned African-Americans earn 10.9% less than lighter skinned ones in the general population (column 1). Siblings, in contrast, exhibit only a small premium that is even opposite in its sign (column 4): siblings classified as darker in 1910 earn 3.5% more than their lighter-skinned siblings, even though this estimate is not precisely measured. Standard errors are quite large because of the smaller sample size; we cannot reject the hypothesis that color premium for siblings is 10.9% as well. The premium among neighbors (column 7) stands at 15.5%, which is closer to the premium in the general population.

Controlling for years of schooling—which is informative despite potential positive selection bias—we estimate the returns to one year of schooling at 8-9% across samples (columns 2, 5, and 8).²⁹ In the general population, adding this control decreases the color premium dramatically and renders it not statistically different from zero (column 2).

[Table 6 about here.]

Table 7 repeats the analysis in Table 6 adding the imputed median non-wage income for the occupation and South/non-South location of the individual in 1940, as described in section 3.5. This way farmers and others whose main income does not come in the form of a wage or a salary are entered into the analysis, even if not as accurately as in the analysis of the income from labor. Adding observations to the analysis is especially critical for the within-group analysis since missing the income of one sibling or neighbor is very likely to have the others dropped from the analysis as well. Overall, results are quite close to the ones presented in Table 6.

²⁹Margo (1990, Table 6.6) reports the returns to one year of schooling at 3.2% for Black males in 1940. However, the regression controls for economic sector, urban status, U.S. region and marital status, which are all partly determined by educational attainment, and it has weekly rather than annual earnings. We present the unconditional return to schooling for annual income to capture the total returns to schooling, including stabler employment, migration and choice of sector. Regressing the log of weekly earnings just on years of schooling for Black males aged 33–48 puts the return to schooling at 6.7% for the IPUMS 1940 sample and 8.5% for our panel of random Blacks and Mulattoes in 1910 matched to 1940. Restricting our sample to those who are still classified as Black in 1940, which reduces false matches but also rules out passing for White, puts the estimate of the returns to schooling at 7.3% with a confidence interval between 6% and 8.7% which includes the unconditional 6.7% we get from the IPUMS sample that Margo (1990) used.

[Table 7 about here.]

Table 8 shows how African-Americans sorted geographically by color through the differences in education. In the top panel of the table those classified as Black in the 1910 census were less likely to live in a different state by 1940 relative to those classified as Mulattoes in their county or even enumeration district. They were also more likely to be in the South in 1940 (bottom panel). Controlling for education, however, differences in migration patterns by color disappear. The more educated were more likely to move to a different state and less likely to be in the South by 1940. Siblings show no significant differences in migratory patterns. A few of them actually end up in the same household in 1940.

[Table 8 about here.]

4.2 Do siblings share the same biological parents?

If some individuals whom we identify as siblings in fact have different parents, this does not in itself constitute a threat to our within-family identification of the effects of color. As a thought exercise we could have done the same analysis if random children in the population were allocated to families after they were born. For the color differences we find between “siblings” to be actually driven by them not being true biological siblings, it would require both that parents treat their non-biological children differently *because* they are not their biological children, *and* that having a different biological parent is correlated with having either light or dark skin. If parents do not change their behavior towards non-biological children then it is not an issue to begin with, and if non-biological children are as likely to be Mulatto as they are to be Black then even though they may be treated better or worse, it should not affect the differences between the colors.

We can be more confident that mis-classification of siblings is not affecting our results if the estimates are unaltered by restricting our sample in ways that increase the probability that included siblings are truly full biological siblings. Roughly 25% of the households in our sample

are headed by a single parent. First, we drop these households from the analysis. Alternatively, we drop children whose last names do not match the last names of their household heads. In addition, we repeat our analysis retaining only children who were born during the current marriage (of the head of the household and his wife).³⁰ Lastly, we keep only children who were born after their mothers were 30 years old, increasing the chances that by this time they were already long settled in a marriage.

Table 9 reports the literacy color difference in each of the proposed sample restrictions. Differences by color are unaltered by the restriction of the sample to siblings who are more likely to be full siblings.

[Table 9 about here.]

4.3 Inferring the role of family background

We can infer the role of family background from comparing color differences in the general population to color differences within families. Such inference requires color to operate in the same way and it operates in the general population, which may be true under certain assumptions that we discuss below. Under these assumptions, if discrimination is driving the color gaps and family background differences do not contribute much to the gaps, then we expect the color gap among siblings to be roughly equal to the color gap in the general population: $\hat{\beta}^f \approx \hat{\beta}^p$. If family background is driving the color gap in the population, then we expect $\hat{\beta}^f \approx 0$ because once it cannot drive differences between siblings. We find that the latter is true, so under the following assumptions:

Differences in true color between Black, Mulatto siblings equal to the population The distance in true skin tone between the average Black and Mulatto siblings may be different than the distance in skin color between the average Black and Mulatto in the population. If the distance in true color is shorter among siblings because siblings tend to look

³⁰For this analysis, we use the question on the length of the current marriage.

alike, we should take it into account when we wish to extrapolate from our sample to the general African-American population. However, there are two main reasons to believe that Black and Mulatto siblings in our sample actually have quite dissimilar skin tones. First, enumerators had a strong tendency to classify all sons or daughters in a household with the same color, suggesting large differences in appearance were required for siblings to be classified differently.

Second, while it is natural to claim that siblings resemble one another physically, this notion is based in the fact that (full) siblings share on average 50% of their alleles. These shared alleles give rise to shared physical traits which contribute to overall resemblance.. But our sample is different by construction. Siblings in our sample must have received different alleles for skin tone or because if they had the same alleles they would have had dissimilar similar skin color and would not have had different color classification. Conditioning on not sharing the same alleles, the siblings in our sample have different alleles that could be as different as those produced by any pair of parents.

This problem, which arises from the crudeness of the categorical measure of color or race, is not limited to within-family exercises. In the usual study of Black-White earning gaps, if lighter-skinned Blacks are more likely to earn a college degree, adding an indicator for having a college degree in a White-Black wage differential regression decreases the variation in skin tone captured by the indicator for “Black.” The resulting coefficient on Black thus captures the wage difference caused by a smaller difference in skin tone. More generally, if the actual skin tone matters, controlling for other variables in the regression that are correlated with skin tone changes the true skin tone difference that is captured by the category of race or color that is used.

It is impossible to quantify the size of the color difference between differently-classified siblings relative to the size of this color difference in the population.³¹ In the absence of a measure of the relative color distance between siblings the maintained assumption will be that they are as far from each other in color as the average Black and Mulatto in the population.

It is impossible to tell just how much of the distance in color in the population is covered

³¹Note that acquiring such measure from current studies can not incorporate how the decision of enumerators to classify siblings differently changed the distribution of color distances among siblings classified differently.

by the distance in color between differently-classified siblings. If one assumes that skin-color discrimination is linear in skin color then we need to inflate the differences between siblings by the inverse of the relative color difference among siblings in our sample. In other words, if one maintains that the distance in color between Mulatto and Black siblings is 0.5 the distance in color between Blacks and Mulattoes in the population, then we need to multiply β^f by 2. In absence of a measure for the relative color distance between siblings the maintained assumption will be that they are as far from each other in color as the average Black and Mulatto in the population.³²

Another way to look at the question of how far is siblings' color from each other is in the context of measurement error bias that is exacerbated by within-family estimation. The basic argument, laid out in Griliches (1979), is that a within-family comparison takes away most of the true variation in the variable of interest (schooling in the case of Griliches (1979) and color in our case) and leaves the remaining total variation with a higher share of measurement error variation. However, in our case enumerators were inclined to classify siblings as the same color, and since the family environment may have affected classification, measurement error may have been greater across families than within families. Thus, it is unclear whether attenuation bias will be stronger when comparing siblings.

No systematic reallocation of resources or peer effects in the family

Having a sibling of a different color 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. In other words, having the “treated” (dark) and “control” (light) siblings in the same family may contaminate the experiment. If a child gets fewer resources only because they are reallocated to her lighter sister then we may overestimate the effect of color because in the general population such reallocation would not have happened to families in which such variation in skin color did not exist. If parents reallocate resources from darker children to lighter ones, either to maximize

³²Note that acquiring such measure from current studies can not incorporate how the decision of enumerators to classify siblings differently changed the distribution of color distances among siblings classified differently.

the return on their investment or because they have discriminatory preferences, then we will overstate the effect of effect of color as it operates in the general population. On the other hand, if parents wish to equalize the economic welfare of their children, they may try to counteract discrimination outside of the family by reallocating resources from lighter- to the darker-skinned children, and we will understate the effect of color.

Peer effects between siblings may be another channel that may change how color is translated into outcomes. The specific family background trait of having children with different color cannot be extrapolated to the general population. Darker-skinned children who grew up with lighter-skinned siblings could be changing their own or their siblings' behavior. In both cases, whether by parents or siblings, these are forces that are unobservable to us as researchers. They are common, however, to this type of a siblings' study.

Endogenous color classification not exacerbated within families Penner and Saperstein (2008) and Freeman et al. (2011) show that racial classification may depend on signals such as incarceration, unemployment, and the way a person dresses. If skin color classification is partially determined by factors that directly affect outcomes of interest, our estimates of the effect of skin color on outcomes will be biased by this endogeneity.

One key factor that does systematically affect skin color, even conditional on family, is age. Skin pigment typically continues to develop during the early months (and sometimes years) of life. Figure 3a illustrates that among families with both "Black" and "Mulatto" sons, the youngest children are disproportionately likely to be classified as "Mulatto." We therefore restrict our analysis to children whose racial classification was conducted between the ages of 3 and 18. For this restricted sample, a set of age-year indicator variables fails to explain variation in "Black"/"Mulatto" classification ($p = 0.28$). The variation at young ages provides additional evidence that enumerators' racial classification was indeed based on appearance rather than ancestry. The lack of variation at older ages could also be interpreted as weak evidence that classification was not affected by individual behavior, or at least not by behavior that varies

systematically by age.

[Figure 3 about here.]

Moreover, if enumerators coded children as Mulatto because they were attending school and hence both smart and perhaps less exposed to the sun then the causality could be running from education outcomes to color classification. To test whether this was the case, we break down school attendance by color and age. Suppose that a child's intellectual character increased the probability of the enumerator to classify the child as a Mulatto. We expect personality to vary more for older more than younger children Daniel et al. (2012), so we expect to see that as age increases, school-goers are more likely to be Mulatto, or that Mulattoes are more likely to go to school. However, we find the contrary: older children classified as Black are actually slightly more likely to attend school than children classified as Mulatto in 1910.

Finally, variation *across* families in color may be affected by status, though this will not be an issue within families. If this is the case, then within-household estimations of color differences will better capture the effect of skin tone on economic outcomes.

5 Racial affiliation and economic outcomes: “passing for White”

Race can also be perceived as a social category into which individuals fit themselves and others, based on perceived geographic ancestry. As with ethnicity, a set of cultural practices and perspectives may be associated with racial identity Akerlof and Kranton (2000) but unlike the concept of ethnicity more generally, races have associated typical physical appearances.³³ As the first part of the paper showed, people can vary in how close they look to the stereotypical racial look. Those who looked sufficiently White could have presented themselves as White. The name given to such a change in affiliation is “passing for White” Myrdal (1944).

³³Although societies' reasons for deciding what constitutes a race may vary, racial categories are widely noted by individuals today and even more so in the early 20th century. For a description of leading definitions of race and an examination of them in light of genetic variation within and between races see Long and Kittles (2009) which claims that there are no biologically distinct human races.

In this part of the paper we will assess the economic returns of changing one's racial affiliation. In a segregated society and economy, being White meant better jobs, better schools, and better public amenities. A simple difference between the incomes of Whites and Blacks will miss important differences in pre-market conditions between Whites and Blacks.³⁴ The experience of those who passed for White poses a better measure of the increased opportunity embodied in being White since passers started their lives as Blacks. Besides the returns to passing, selection into who is passing and who is staying Black is interesting in itself. Even if all the difference between passers and non-passers arise from the higher ability of passers (whether Black or White), the racial sorting according to economic outcomes mattered for the long-run development of African-Americans. Nevertheless, we will try to decompose the measured income differences between passers and non-passers to returns and selection.

Not everyone could become White. Passing for White required both looking and acting White. According to the One-Drop rule, even people who had even very low proportions of African ancestry and very light skin were still considered Black. Some African-Americans could have appeared White to contemporary observers and they could choose whether to present themselves as Whites or Black. Some may have passively passed for White simply by having others assume they are White. But to fully pass for White and enjoy the economic opportunities that were open only to Whites, passers had to remove any doubt about their background and family history. Their social ties, use of language, and cultural references could not have revealed their African-American past.

Depending on our matching rule, between 10% and 13% of sons classified as Mulatto in the 1910 census appear as White in the 1940 census. What was the economic benefit of passing. Were individuals of higher earning potential more likely to pass? Selection of individuals into passing, while interesting in itself, introduces a challenge for estimating the returns to passing. Following the literature on average treatment effects (Imbens and Angrist, 1994; Rubin, 1974,

³⁴In this section we will use Black and African-American interchangeably. Since for all purposes but the census the social distinction that mattered was between Whites and African-Americans no matter what their skin tone was, the term Black will be used to refer to any African-American as is usually the case in the literature.

among others) we would like to measure returns as:

$$\mathbb{E} \left[y_i^w - y_i^b \right] \tag{5.1}$$

where y_i^w is the income of individual i when he is White and y_i^b is the income of the same individual when he is Black. The problem is that income is observed only once in 1940 when individuals are either White or Black. Thus, estimating a difference between passers and non-passers the following difference

$$\begin{aligned} \overbrace{\mathbb{E} \left[y_i^w | p \right] - \mathbb{E} \left[y_j^b | np \right]}^{\text{measured difference}} &= \mathbb{E} \left[y_i^w | p \right] - \mathbb{E} \left[y_i^b | p \right] + \mathbb{E} \left[y_i^b | p \right] - \mathbb{E} \left[y_j^b | np \right] = \tag{5.2} \\ &= \underbrace{\mathbb{E} \left[y_i^w - y_i^b | p \right]}_{\text{returns (5.1)}} + \underbrace{\mathbb{E} \left[y_i^b | p \right] - \mathbb{E} \left[y_j^b | np \right]}_{\text{selection}} \end{aligned}$$

where $\mathbb{E} \left[y_i^w | p \right]$ is the expected income of individual i who passes for White and $\mathbb{E} \left[y_j^b | np \right]$ is the expected income of individual k who does not pass, as Black. In the right hand side we subtract and add the hypothetical income of passers if they do not pass: $\mathbb{E} \left[y_i^b | p \right]$. We then rewrite the terms to express the measured difference of the left-hand-side as a sum of the returns to passing and the selection effect: the potential of passers to earn even without passing relative to the actual earnings of non-passers.

If passing was randomly assigned, passers and non-passers would have been similar on average. But unlike in a randomized trial or when comparing across skin colors which are predetermined, the decision to pass is far from being random. Those who passed could have been those who had the most to benefit from passing, perhaps because they had higher earning potential and they would have made more money even if they did not pass. The relative importance of the returns and the selection changes the way we interpret the overall income differences between passers and non-passers.

We will control for selection in three ways: controlling for the level of education, comparing passers to their siblings, and combining the two by controlling for education in the comparison

of siblings. All these exercises are aimed at reducing the difference between the actual income of non-passers and the hypothetical income of passers had they not passed ($\mathbb{E} [y_j^b | np, \mathbf{X}]$ and $\mathbb{E} [y_i^b | p, \mathbf{X}]$). We also restrict our attention to Mulattoes in 1910 to reduce the role of skin color and isolate the social aspect of race.³⁵

Figure 4 compares income of Whites and Blacks in the cross-section in 1940 and sheds light on two points that are important for the analysis. First, it shows that incomes of Whites were much higher than incomes of Blacks, especially in high-skilled occupations. However, such comparison suffers from even greater problems of selection as the backgrounds of the average White and Black at the time were very different. Second, according to the Roy model (Borjas, 1987), we should expect those who had higher earning potential to be more likely to pass because the variance of income among Whites was higher. Another way to see the higher benefits for the higher-skilled is by breaking down racial gaps by occupation for selected occupations. As the skill of level increase, wage gaps increase.

[Figure 4 about here.]

Table 10 reports the results on income differences between passers and non-passers. Columns (1)-(3) report the following equation for the whole population that we successfully matched. We control for age (quadratic) and birth order in the 1910 household in all columns. In columns (2) we add education level to \mathbf{x}_i and then an interaction between passing and education in column (3). Columns (4)-(6) repeat the exercise for a sample of siblings where some passed while others did not pass.

$$y_i = \alpha + \beta \text{white1940}_i + \mathbf{x}_i \gamma + u_i \quad (5.3)$$

[Table 10 about here.]

Differences between passers and non-passers are much larger than differences by skin color. Overall differences in income between those who become White and those who stayed Black

³⁵Our assumption is that all Mulattoes were light-skinned enough to be able to pass. However, even if this assumption is false, we do not expect any differences in color between passers and non-passers to drive significant differences in economic outcomes since the relative size of the differences by color estimated in the previous part of the paper is small.

are estimated at 31%. Restricting attention to income from labor passers earn 42% more than non-passers. How much of these differences can be attributed to passers being more productive in the first place? In column (2) we report that passers earn 20% and 31% more than non-passers with the same level of schooling. The tendency of passers to have better schooling accounts for about 10 percentage points of the difference between passers and non-passers measured in column (1). If we can maintain that the hypothetical income of passers as Black is equal to the actual income of non-passers *with the same level of schooling* then according to (5.3) the coefficient in column (2) would measure the return to passing. There are two problems with this comparison, though. First, we may think that other dimensions of productivity that are not captured by the number of years of schooling still make passers' hypothetical income higher than non-passers actual income on average. Second, even if we measure returns cleanly (given the number of years of schooling), part of the returns to passing are the ability to increase the number of years of schooling.³⁶ Controlling for the number of years of schooling can not count for the returns to passing that were working through the ability to study more.

A better counterfactual for a passer is his non-passing brother. Since family background plays a large role in the acquisition of human capital, as we showed in the first part of the paper, brothers are quite similar to each other on average. Thus, in a similar fashion to 4.2, we will estimate:

$$y_{if} = \alpha + \beta \text{white1940}_{if} + \mathbf{x}_{if}\gamma + \phi_f + u_{if} \quad (5.4)$$

Column (4) reports returns of 21% in total income and 14% when we restrict attention to siblings' weekly salaries when both have positive income from labor. These estimates are lower than differences in the general population, as we expected. Looking at the mean weekly earnings at the bottom of each panel and comparing them to the means in the general population we see

³⁶Even though Black colleges and universities were dedicated to African-Americans, passing for White allowed access to more and better schools. Myrdal (1944)'s impressions on the costs of passing for White came from a conversation he had with an African-American teacher who passed for White in college but came back to his community after graduating.

that siblings of passers indeed earn more than the average non-passer in the population and this is why the estimated returns are lower. Yet returns are still quite high between siblings, especially if we compare them to differences in income by skin color between siblings that we measured in the first part of the paper. Selection across families accounts for 1/3-2/3 of the differences between passers and non-passers in the general population.

We control for education in column (5) to account for selection within the family, but the two problems of schooling as a control persist: schooling may not capture enough (additional unobservables) or it may capture too much (eliminate the returns from passing in terms of education from the analysis). In either case, the returns measured show minor differences between passers and their non-passing brothers if their brothers have the same level of schooling. Finally, besides the average premium for being classified as White in 1940, columns (3) and (6) show that the returns to schooling are higher for those who are White. The returns to schooling are higher by more than 50% for Whites in the general population of Mulattoes in 1910. Between siblings the differentiation by education is even higher.

6 Conclusion

We estimate how race affected economic outcomes in the US in the first half of the 20th century. We use two alternative measures of race, namely skin color and social affiliation, and construct a panel dataset that follows differently-colored siblings from their childhood households to their adult outcomes, allowing us to estimate racial differences holding family background constant. We find that the social boundary between what is considered White and what is considered Black mattered more economically than differences in skin tone between African-Americans.

We find that siblings who differ in skin color—who share a family background but could face differential discrimination—actually have very similar outcomes on average in terms of both education and earnings. In contrast, lighter-skinned African-Americans in the population are considerably more educated than darker-skinned African-Americans, and this educational gap drives a large difference in wages. Further, differences in family background are not merely driven

by neighborhood differences such as the supply or quality of schools; even neighbors of different colors have large differences in educational attainment. These results suggest differences in outcomes between African-Americans with different skin tones are largely the result of differing family backgrounds, rather than resulting from discrimination.

In the second part of the paper we turn to the effect of the social affiliation aspect of race. A small minority of those classified as light-skinned African-Americans in the 1910 census appear as White in the 1940 census. In the highly segregated labor market of the era, such a change of identity could have opened many doors, but also entailed high psychological and cultural costs. In fact, light-skinned African-Americans who passed as white earned 30%-40% higher incomes than those who did not. We expect those who had more to gain from passing to have been more likely to pass, and indeed we see that passers were positively selected on education. We also see that comparing between passers and their siblings reduces the premium to 14%-21% and that passers' siblings who did not pass earned higher wages than the average non-passers, meaning that passers came from better families (in terms of these families' children having higher earning potential). The larger differences by race (White vs Black) relative to the differences by skin color suggest that the behavioral and social factors associated with racial affiliation have been more important than purely physical or biological perception of race. Skin color was not unimportant. It precluded most African-Americans from assimilating into the White community and reaping the associated economic benefits. While other groups such as European immigrants suffered from prejudice and discrimination they could have assimilated quite easily in the course of a generation at most if they wanted to. The vast majority of African-Americans could not make the same choice to assimilate because their physical appearance always marked them as socially different.

Although markets are no longer formally segregated as they were in the period of study, racial differences in social norms persist to the present, and many African-Americans today still face the choice of whether to "act White" with the economic benefits and cultural costs of doing so (Akerlof and Kranton, 2000; Fryer et al., 2012; Fryer Jr and Torelli, 2010). In a broader context,

this paper demonstrates that the economic value of assimilating into the dominant culture can be considerable, and suggests that the way in which a society chooses to draw its racial boundaries can have a greater economic impact than how racially stereotypical is one's physical appearance. Finally, initial discrimination in one period can be perpetuated through differing family backgrounds that make the children of those who were discriminated against in one generation worse off even if discrimination was no longer present in their generation, as the results on skin color within and between families suggest.

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A Linking Appendix

To be added.

DEPARTMENT OF COMMERCE AND LABOR—BUREAU OF THE CENSUS
THIRTEENTH CENSUS OF THE UNITED STATES: 1910—POPULATION

STATE South Carolina SUPERVISOR'S DISTRICT No. 5 SHEET No. 1
 COUNTY Fairfield ENUMERATION DISTRICT No. 61 **B**
 ENUMERATOR Eric Chapell

CATION.	NAME of each person whose place of abode on April 15, 1910, was in this family. Enter surname first, then the given name and middle initial, if any. Include every person living on April 15, 1910. Omit children born since April 15, 1910.	RELATION. Relationship of this per- son to the head of the family.	PERSONAL DESCRIPTION.						
			Sex.	Color or race.	Age at last birth- day.	Whether already married, widowed, or divorced.	Number of years of marriage.	Number born.	Number now living.
19	Spencer John	Head	M	W	35	M	12		
	— Florence	Wife	W	W	31	M	12	5	4
	— Isaac	Son	M	W	11	S			
	— Maurice	Son	M	W	9	S			
	— Edward	Son	M	W	7	S			
	— John	Son	M	W	4	S			

(a) The Spencers in 1910

(1940, Pennsylvania)

		Race	Schooling	Occupation	Industry	Weeks worked	Income
Spencer Isaac	Head	M	C 41 8	operator	steel mill	40	1800
Hattie	wife	W	E 37 8	machine operator	dress factory	26	193
Blair	son	M	E 15 8	clerk	steel mill	14	298

(1940, South Carolina)

Spencer John	head	M	NEG 34 5	laborer	WPA project construction	52	480
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(b) Isaac and John Spencer in 1940

Figure 1: The Spencer brothers in 1910 and 1940

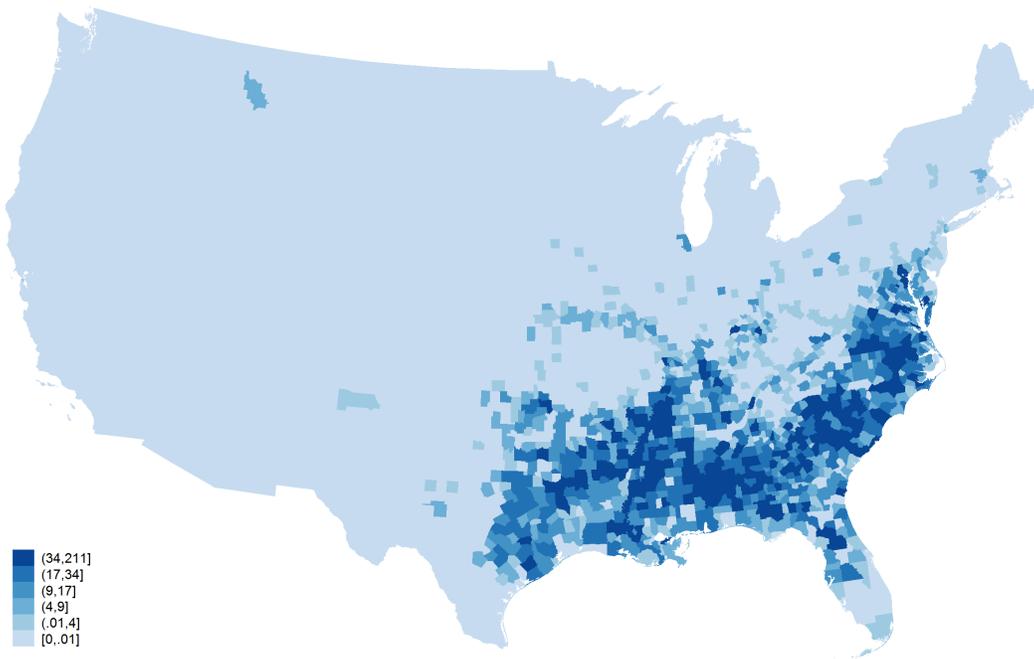
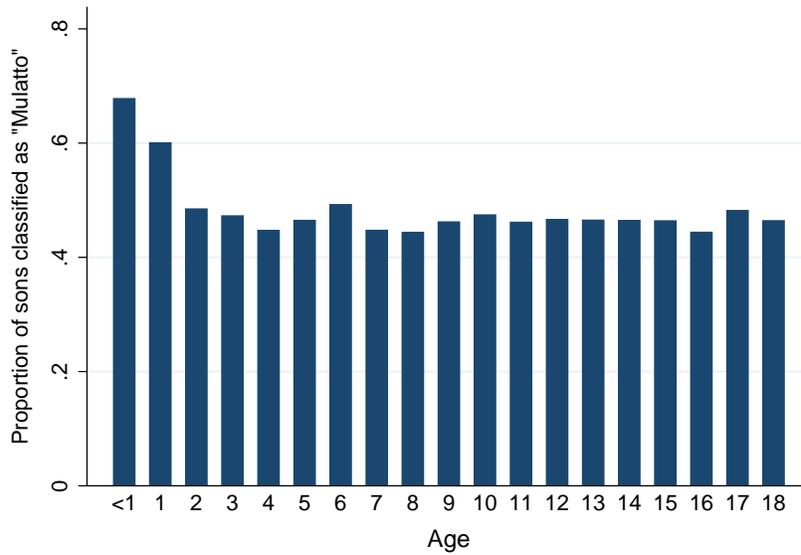


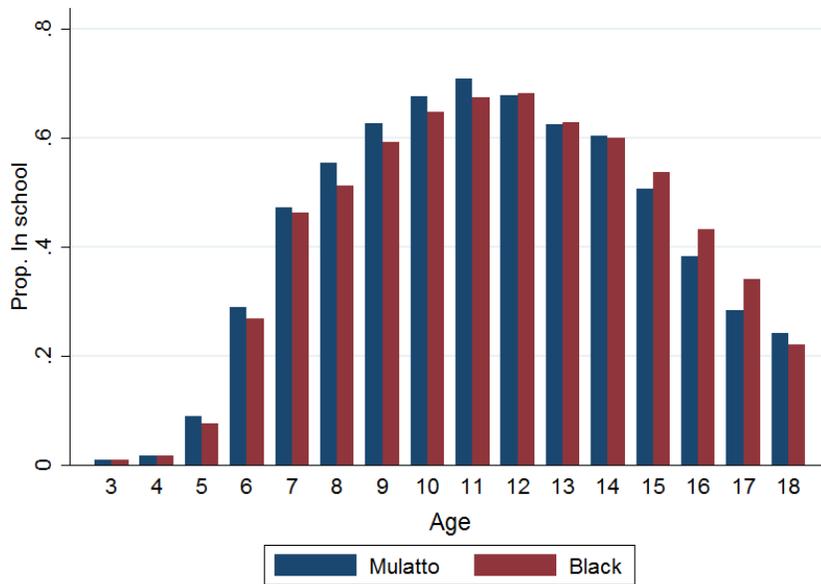
Figure 2: Geographic distribution of BMS families, by county

Note: BMS are households with at least two sons aged 3–18 in 1910 where at least one classified “Black” and at least one classified “Mulatto.”



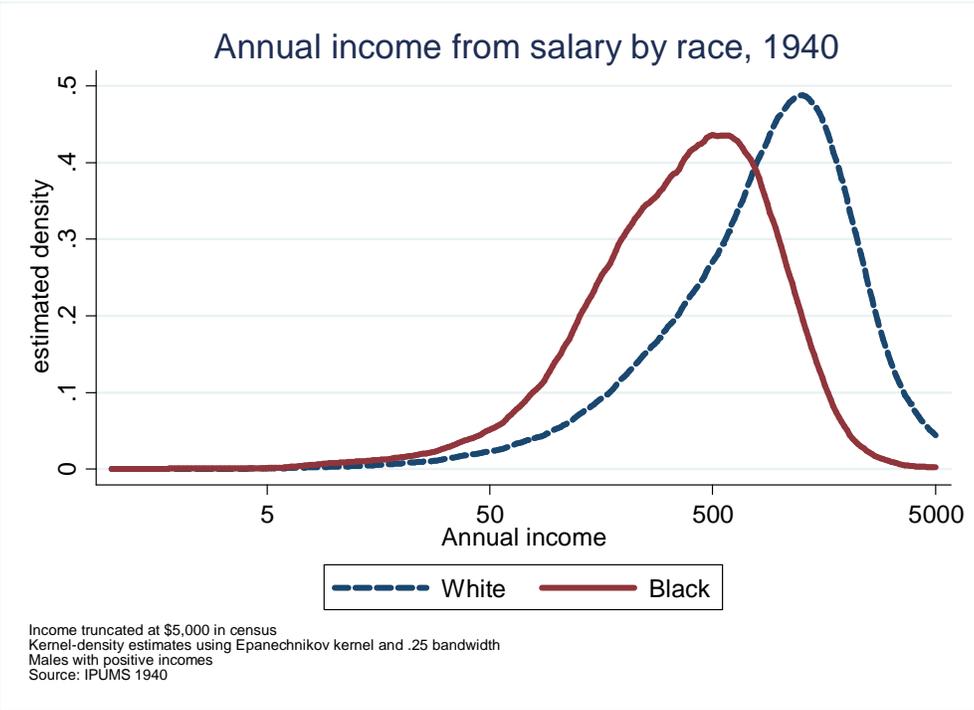
Sample is all "black" or "mulatto" sons 18 years old or younger that live in a household with at least one "mulatto" and one "black" son in 1910

(a) Proportion of "Mulattoes" in households with both "black" and "mulatto" sons, by age

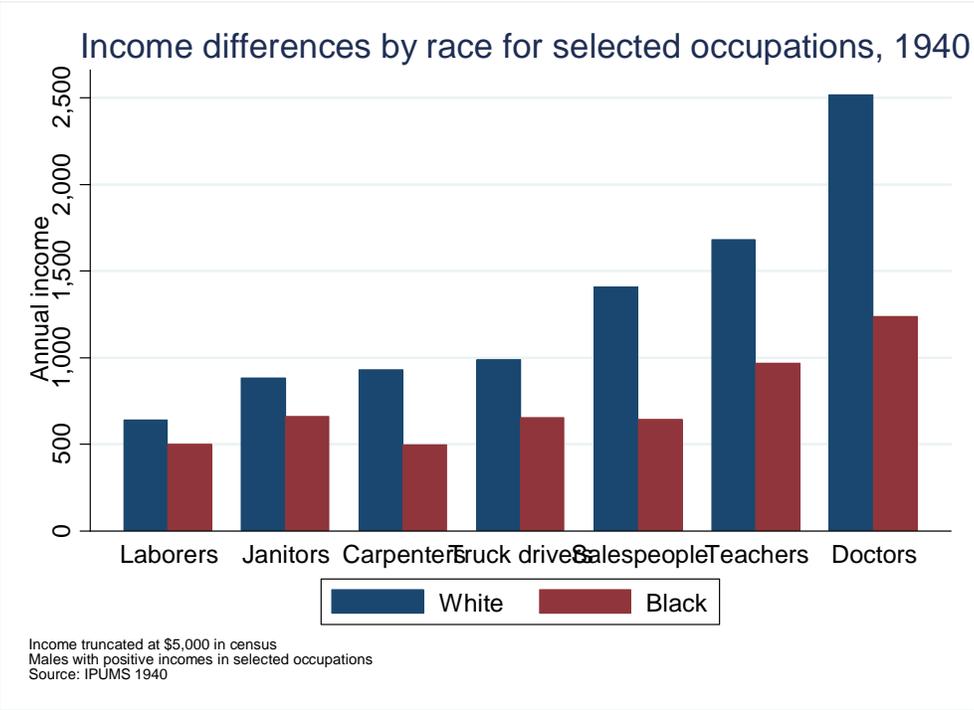


(b) Older siblings who are Black more likely to attend school

Figure 3: Evidence consistent with skin-color based classifications



(a) Income distribution



(b) Income from labor by occupation and race

Figure 4: Income from labor by race, 1940

Table 1: Racial compositions of African-American couples and parents in Black-and-Mulatto-sons (BMS) families

(a) In population, proportion Black children in mixed families consistent with color coding

Parents' color			Children's composition		
Head	Wife	% of couples	N children	% Black	% Female
Black	Black	74%	2.11	99.2%	50.0%
Black	Mulatto	8%	2.02	27.5%	49.7%
Mulatto	Black	4%	2.17	44.2%	49.7%
Mulatto	Mulatto	14%	2.12	1.6%	49.8%

(b) Families with both Black and Mulatto sons, by parents' race (percent of 6,047 families)

17

		Mother is					Total
		Black	Mulatto	White	Other	Absent	
Father is	Black	15.77	32.21	0.08	0.02	2.47	50.55
	Mulatto	18.69	9.1	0.03	0.03	1.6	29.47
	White	0.1	0.07	0.05		0.02	0.23
	Other						
	Absent	13.01	6.63	0.1			19.75
		47.57	48.01	0.27	0.05	4.09	100

Notes:

1. Numbers in 1a are reported for the whole population of couples where the head and his wife were classified as either Black or Mulatto in 1910.
2. Numbers in 1b are percentages of the 6,047 households where some sons were classified as Mulatto and the other sons were classified as Black.
3. Blank cells contain no households.
4. Single-parent households in 1b show the missing parent as "Absent"

Table 2: Attributes of African-American households (1910)

Differences in background characteristics between samples, 1910

Sample:	<u>Random</u>		<u>Siblings</u>		<u>Neighbors</u>		
	<u>Black</u>	<u>Mulatto</u>	<u>Both</u>		<u>Black</u>	<u>Mulatto</u>	
Household color:	(1)	(2)	(3)		(4)	(5)	
Sons in household	3.54	3.54	<	3.82	>	2.80	2.89
Age	9.91	10.11	<	10.19	>	9.91	9.90
Able to write	0.622	0.718	>	0.595	€	0.549	0.676
Head is literate	0.562	0.664	∃	0.568	€	0.510	0.636
Head's occupation:							
- Farm laborer	0.046	0.038	<	0.049	€	0.058	0.042
- Farmer	0.590	0.513	<	0.619	€	0.619	0.580
- Laborer (not farm)	0.163	0.151	>	0.135	<	0.148	0.135
- In home services	0.047	0.071	∃	0.066	>	0.051	0.059
Head owns home	0.244	0.380	∃	0.278	€	0.247	0.367
County urban	0.191	0.236	>	0.161	€	0.152	0.161
County Afr. Americans	0.460	0.415	<	0.487	<	0.535	0.511
Observations	8,939	7,345		19,910		16,165	10,225

Note: Mean characteristics are reported for the sample of random sons in households with at least two sons all Black or all Mulatto (columns (1) and (2)), sons in households with at least one Black and one Mulatto son (column (3)), and sons that are neighbors of sons in the Black-and-Mulatto sons households.

Table 3: Characteristics of matched vs. all searched individuals**Differences in 1910 char's between searched sample and matched subsample, pooled samples***

	All obs	Match 1				Match 2 (stricter)			
	Mean	Unweighted		Weighted		Unweighted		Weighted	
		Mean	Difference	Mean	Difference	Mean	Difference	Mean	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Observations (count)	61,055		--- 20,574 ---				--- 6,613 ---		
Both colors in district	51,511		--- 14,867 ---				--- 3,234 ---		
Both colors in family	20,061		--- 2,925 ---				--- 409 ---		
Characteristics in 1910									
Black	0.597	0.584	-0.018 *** (0.004)	-0.005	-0.006 (0.004)	0.560	-0.041 *** (0.007)	0.597	-0.003 (0.007)
Age	10.0	10.0	0.022 (0.039)	10.0	0.021 (0.040)	10.1	0.054 (0.060)	10.1	0.063 (0.063)
Able to write	0.614	0.631	0.027 *** (0.007)	0.624	0.020 *** (0.007)	0.672	0.065 *** (0.010)	0.652	0.045 *** (0.010)
# sons in household	3.26	3.29	0.052 *** (0.014)	3.27	0.033 ** (0.014)	3.34	0.088 *** (0.023)	3.28	0.031 (0.022)
Head is literate	0.573	0.586	0.020 *** (0.005)	0.573	0.007 (0.005)	0.627	0.060 *** (0.008)	0.573	0.007 (0.008)
Head's occupation:									
- Farm laborer	0.048	0.046	-0.003 (0.002)	0.047	-0.001 (0.002)	0.043	-0.005 * (0.003)	0.047	-0.002 (0.004)
- Farmer	0.591	0.590	-0.002 (0.005)	0.592	0.000 (0.005)	0.588	-0.004 (0.008)	0.595	0.003 (0.008)
- Laborer (not farm)	0.147	0.146	-0.001 (0.004)	0.147	-0.001 (0.004)	0.139	-0.009 * (0.005)	0.147	-0.001 (0.006)
- In home services	0.061	0.058	-0.005 ** (0.002)	0.061	-0.002 (0.002)	0.048	-0.015 *** (0.003)	0.059	-0.003 (0.004)
Head owns home	0.287	0.312	0.037 *** (0.005)	0.287	0.012 *** (0.005)	0.345	0.066 *** (0.008)	0.286	0.006 (0.007)
County's share urban	0.173	0.173	0.001 (0.002)	0.173	0.001 (0.002)	0.182	0.011 *** (0.004)	0.175	0.004 (0.004)

- Notes:
- Column (1) reports statistics of background characteristics for our samples in 1910. Columns (2)-(5) reports the characteristics for sons matched to the 1940 census using one matching rule (Match 1) and columns (6)-(9) reports them for sons matched to the 1940 using a second matching rule (Match 2).
 - The top panel of the table reports the number of observations in the baseline 1910 sample and in the matched samples. The first row reports the overall number while the following rows report the number of observations that are in a district (row 2) or households (row 3) where both Black and Mulatto sons are found.
 - Columns (4) and (5) repeat the results of columns (2) and (3) re-weighting matched observations according to how likely they were to be matched. See footnote 22 for the list of variables used to calculate these weights. Similarly, columns (8) and (9) show re-weighted statistics for columns (6) and (7). Each pair of columns reports the mean characteristic in the matched sample and the result of a t-test for the difference in the characteristic between the matched and unmatched groups.
 - Match 1 requires each observation's best potential match to have a link score of at least 0.5 and the second best potential match to have a score that is lower by at least 0.1. Match 2 requires the best match's link score to be at least 0.75 and the second best match to be at least 0.3 lower or to be at least 0.15 lower if only the best match's first and last names' Soundex codes match. See Appendix A for further details.

Table 4: Literacy higher for children classified as Mulatto in the population; family background differences drive most differences in population

Comparing between:	Dependent variable: Literacy, 1910									
	Boys					Girls				
	General population (1)	(2)	Brothers (3)	Neighbors of brothers (4)	(5)	General population (6)	(7)	Sisters (8)	Neighbors of sisters (9)	(10)
Black (vs. Mulatto)	-0.093 *** (0.010)	-0.039 *** (0.010)	-0.013 (0.008)	-0.094 *** (0.017)	-0.069 *** (0.017)	-0.128 *** (0.009)	-0.079 *** (0.009)	-0.022 *** (0.008)	-0.116 *** (0.025)	-0.087 *** (0.026)
Head is literate		0.238 *** (0.010)			0.141 *** (0.019)		0.178 *** (0.009)			0.146 *** (0.035)
Family owns home		0.098 *** (0.011)			0.148 *** (0.023)		0.116 *** (0.010)			0.130 *** (0.042)
Household head occupation: Farm laborer		-0.263 *** (0.027)			-0.076 (0.057)		-0.149 *** (0.026)			-0.209 *** (0.106)
Farmer		-0.206 *** (0.019)			-0.005 (0.041)		-0.120 *** (0.017)			-0.100 (0.084)
Laborer		-0.110 *** (0.018)			-0.029 (0.041)		-0.054 *** (0.016)			-0.118 (0.073)
Home services		-0.042 * (0.023)			-0.011 (0.045)		-0.022 (0.020)			-0.103 (0.071)
Head is employer		0.006 (0.018)			0.003 (0.037)		0.003 (0.017)			-0.057 (0.061)
Head works on own account		0.008 (0.018)			-0.032 (0.036)		0.029 * (0.016)			-0.023 (0.058)
Main fixed effects:	None	None	Families	Enum. districts	Enum. districts	None	None	Families	Enum. districts	Enum. districts
Observations	8,480	8,078	6,875	4,545	4,290	8,256	7,919	7,220	1,777	1,684
R-squared	0.042	0.163	0.094	0.058	0.105	0.070	0.153	0.118	0.077	0.131
Mean[Literate Mulatto]	0.718	0.718	0.614	0.667	0.667	0.832	0.832	0.752	0.732	0.732
Mean[Literate Black]	0.622	0.622	0.605	0.555	0.555	0.702	0.702	0.706	0.563	0.563

Notes:

- Literacy is taken from the question on whether an individual can write. Enumerators were instructed to ask each household member over 10 years old whether they could read or write.
 - All samples taken from the 1910 Census. "General population" columns (1),(2),(6),(7) report results for a 1% random sample of children classified as either Black or Mulatto aged 3–18 in families that had at least two sons aged 3–18 classified in the same color. Siblings columns (3),(8) report results for the universe of either sons or daughters aged 3–18 in households that have at least one more son or daughter color-coded differently. Neighbors of siblings columns (4),(5),(9),(10) report results for children close in age and of equal gender who also live in the same enumeration district in 1910.
 - In columns (3)-(5) and (8)-(10) only children that have other children of the opposite color from the same group (family or enumeration district) with non-missing literacy are included.
 - The omitted category for household head occupation is all other occupations. The omitted category for the employment type (head employer or works on own account) is those employed by others. All regressions control for a set of age and birth order (among sons) dummies.
 - Standard errors are clustered at the county level for the general population and enumeration district level for the brothers and neighbors regression.
- * p<0.10, ** p<0.05, *** p<0.01

Table 5: Lighter-skinned attain more years in school in population and between neighbors; less so within families

Dependent variable: Years of schooling, 1940			
	General population (1)	Brothers (2)	Neighbors (3)
Black (vs. Mulatto), 1910	-1.115 *** (0.175)	-0.214 (0.320)	-1.270 *** (0.447)
Observations	1,743	374	281
Mean[Education Mulatto]	7.02	6.03	6.50
Mean[Education Black]	5.92	5.87	5.43

Notes:

1. The dependent variables are the eventual number of years in school as reported in the 1940 census. The variable of interest, Black, is taken from the 1910 census classification.
 2. “General population” in column (1) reports difference in schooling for the of children aged 3–18 in 1910 who had siblings only of the same color classification and were in families randomly selected from the population. The sample is further restricted to those who were successfully matched to 1940 records.
 3. “Brothers” in column (2) reports difference in schooling for the universe of sons aged 3–18 in households that have at least one more son color-coded differently with at least one son of each color classification successfully matched to 1940.
 4. “Neighbors” in columns (3) report difference in schooling for sons who, in 1910, were close in age and lived in the same enumeration district as children from the “Brothers” sample . Only neighbors who were successfully matched with at least one other neighbor of the opposite color classification successfully matched entered the sample.
 5. All regressions also control for age and birth order. Standard errors are clustered at the county level for the general population and enumeration district level for the brothers and neighbors regression.
- * p<0.10, ** p<0.05, *** p<0.01

Table 6: Income premium for light skin explained by differences in educational attainment

Comparing between:	Dependent variable: weekly labor income in 1939 (natural log)								
	General population			Brothers			Neighbors		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Black (vs. Mulatto)	-0.116 ** (0.047)	-0.028 (0.045)	-0.157 * (0.095)	0.035 (0.087)	0.046 (0.074)	0.222 (0.158)	-0.168 (0.130)	-0.107 (0.119)	-0.670 * (0.376)
Years of schooling		0.087 *** (0.006)	0.079 *** (0.008)		0.091 *** (0.020)	0.106 *** (0.021)		0.092 *** (0.021)	0.047 (0.030)
Years of schooling X Black			0.019 (0.013)			-0.029 (0.024)			0.083 (0.052)
Main fixed effects:		None			Families			Enumeration districts	
Observations	1,188	1,173	1,173	206	203	203	111	110	110
R-squared	0.008	0.148	0.150	0.012	0.216	0.225	0.063	0.279	0.307
Mean[wkly labor income mulatto, >0]		\$19.7			\$15.4			\$17.8	
Mean[wkly labor income black, >0]		\$17.6			\$15.7			\$16.2	

Notes:

1. The dependent variable, $\ln(\text{weekly salary})$, was computed as the natural log of the reported annual income from salary divided by the reported number of weeks worked. Only individuals with positive weekly income enter the analysis but no differences by color were found in whether income was positive or zero.
2. "General population" in columns (1)-(3) report results for the of children aged 3-18 in 1910 who had siblings only of the same color classification and were in families randomly selected from the population. The sample is further restricted to those who were successfully matched to 1940 records.
3. "Brothers" in columns (4)-(6) report results for the universe of either sons aged 3-18 in households that have at least one more son color-coded differently with at least one son of each color classification successfully matched to 1940.
4. "Neighbors of siblings" in columns (7)-(9) report difference in schooling for sons who, in 1910, were close in age and lived in the same enumeration district as children from the "Brothers" sample. Only neighbors who were successfully matched with at least one other neighbor of the opposite color classification successfully matched entered the sample.
5. Regressions also control for age (quadratic) and birth order among sons in the childhood households.
6. Standard errors are clustered at the county level for the general population and enumeration district level for the brothers and neighbors regression.

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

Table 7: Income premiums robust to the imputation of non-wage income for occupations where wage income is relatively small

Comparing between:	Dependent variable: weekly income ¹ in 1939 (natural log)								
	General population			Brothers			Neighbors		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Black (vs. Mulatto)	-0.092 ** (0.038)	-0.014 (0.036)	-0.115 (0.072)	0.032 (0.071)	0.073 (0.061)	0.054 (0.138)	-0.108 (0.092)	-0.000 (0.090)	-0.077 (0.235)
Years of schooling	0.077 *** (0.005)	0.077 *** (0.006)	0.070 *** (0.006)	0.092 *** (0.015)	0.092 *** (0.015)	0.090 *** (0.019)	0.082 *** (0.016)	0.082 *** (0.016)	0.076 *** (0.024)
Years of schooling X Black		0.016 (0.010)		0.003 (0.022)					0.013 (0.037)
Main fixed effects:	None			Families			Enumeration districts		
Observations	1,542	1,519	1,519	318	313	313	241	238	238
R-squared	0.009	0.147	0.149	0.004	0.200	0.200	0.032	0.197	0.198
Mean[wkly income mulatto, >0]		\$17.0			\$13.9			\$14.2	
Mean[wkly income black, >0]		\$15.3			\$13.8			\$13.7	

Notes:

1. The dependent variable – weekly income (in natural log) – is the natural log of the sum of the weekly income from labor as reported directly in the census and the median non-labor income for the occupation and region (South or non-South) of the individual. Median incomes calculated from the IPUMS sample of the 1950 census, and adjusted for 1940 prices (multiplied by 0.4).
2. The dependent variable, ln(weekly salary), was computed as the natural log of the reported annual income from salary divided by the reported number of weeks worked. Only individuals with positive weekly income enter the analysis but no differences by color were found in whether income was positive or zero.
3. “General population” in columns (1)-(3) report results for the of children aged 3–18 in 1910 who had siblings only of the same color classification and were in families randomly selected from the population. The sample is further restricted to those who were successfully matched to 1940 records.
4. “Brothers” in columns (4)-(6) report results for the universe of either sons aged 3–18 in households that have at least one more son color-coded differently with at least one son of each color classification successfully matched to 1940.
5. “Neighbors of siblings” in columns (7)-(9) report difference in schooling for sons who, in 1910, were close in age and lived in the same enumeration district as children from the “Brothers” sample. Only neighbors who were successfully matched with at least one other neighbor of the opposite color classification successfully matched entered the sample.
6. Regressions also control for age (quadratic) and birth order among sons in the childhood households.
7. Standard errors are clustered at the county level for the general population and enumeration district level for the brothers and neighbors regression.

- * p<0.10, ** p<0.05, *** p<0.01

Table 8: Lighter-skinned more likely to migrate and leave South by 1940; Education gap can explain differences

	Skin color and migration 1910-1940							
	General population (1)	(2)	Gen. pop. within county (3)	(4)	Siblings (5)	(6)	(7)	Neighbors (8)
	<i>Dependent variable: Whether state in 1940 different than state in 1910</i>							
Black (vs. Mulatto)	-0.023 (0.027)	0.007 (0.054)	-0.075 * (0.040)	-0.014 (0.078)	0.035 (0.042)	0.101 (0.097)	-0.116 *** (0.031)	-0.147 (0.140)
Years of schooling		0.023 *** (0.005)		0.029 *** (0.007)		0.019 * (0.011)		0.015 (0.015)
Black X schooling		-0.000 (0.007)		-0.004 (0.010)		-0.008 (0.014)		0.008 (0.021)
Mean[dep. Mulatto]	0.399		0.389		0.368			0.436
Mean[dep. Black]	0.376		0.367		0.392			0.347
	<i>Dependent variable: Lives in southern state in 1940</i>							
Black (vs. Mulatto)	0.112 *** (0.028)	-0.024 (0.046)	0.093 *** (0.034)	-0.022 (0.062)	-0.041 (0.036)	-0.049 (0.082)	0.085 *** (0.027)	-0.045 (0.113)
Years of schooling		-0.041 *** (0.005)		-0.025 *** (0.006)		-0.011 (0.011)		-0.041 (0.011)
Black X schooling		0.015 ** (0.007)		0.015 * (0.009)		-0.001 (0.012)		0.013 (0.017)
Mean[dep. Mulatto]	0.575		0.656		0.670			0.656
Mean[dep. Black]	0.669		0.716		0.652			0.729
Observations	1,779	1,741	951	932	392	381	1,285	493

Notes:

- The dependent variable in the top panel is an indicator for whether an individual's residence in 1940 was in a different state than his residence in 1910. The dependent variable in the bottom panel is an indicator for whether a person resided in the South in 1940 where the South is defined as Texas, Louisiana, Mississippi, Alabama, Georgia, South Carolina, Florida, North Carolina, Tennessee, Virginia, Maryland, Arkansas, and the District of Columbia.
- "General population" in columns (1)-(4) report results for the of children aged 3-18 in 1910 who had siblings only of the same color classification and were in families randomly selected from the population. The sample is further restricted to those who were successfully matched to 1940 records. Columns (3) and (4) adds county fixed-effects to avoid differences in initial location that are correlated with color to drive differences in 1940 location.
- "Siblings" in columns (5)-(6) report results for the universe of sons aged 3-18 in households that have at least one more son color-coded differently with at least one son of each color successfully matched to 1940.
- "Neighbors of siblings" in columns (7)-(8) report results for children who, in 1910, were close in age and of the same gender who also live in the same enumeration district as children from the "Siblings" sample. Only neighbors who were successfully matched with at least one other neighbor of the opposite color classification successfully matched entered the sample.

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

Table 9: When samples restricted to siblings with higher likelihood to be share same parents, differences between siblings are unchanged

Dependent variable: Literacy, 1910					
Comparing between:	<u>All siblings</u>	<u>In hhods w. both parents</u>	<u>Same last name as head</u>	<u>Born current marriage</u>	<u>Born to moms 30+</u>
	(1)	(2)	(3)	(4)	(5)
<i>Boys</i>					
Black (vs. Mulatto)	-0.013 (0.008)	-0.015 (0.009)	-0.014 * (0.008)	-0.012 (0.011)	-0.010 (0.016)
Observations	6,875	5,143	6,211	3,757	1,692
<i>Girls</i>					
Black (vs. Mulatto)	-0.022 *** (0.008)	-0.018 ** (0.009)	-0.025 *** (0.008)	-0.010 (0.010)	-0.008 (0.016)
Observations	7,220	5,278	6,393	3,906	1,598

Notes:

1. The dependent variable is literacy (being able to write), as recorded for individuals aged 10 years or older.
 2. Column (1) has the full sample of siblings, and matches columns 3 (boys) and 8 (girls) in Table 4.
 3. Column (2) limits the sample only to siblings in families where both parents were present.
 4. Column (3) limits the sample to siblings who had the same last name as the household head.
 5. Column (4) limits the sample to siblings whose age is strictly smaller than the length of current marriage of the head of household and the spouse.
 6. Column (5) limits the sample to siblings born after the top female in the household (head or spouse) reached the age of 30.
- * p<0.10, ** p<0.05, *** p<0.01

Table 10: Returns to passing-for-White in are high; even between siblings

	Differences in income, 1939 (natural log)					
	All observations			Between siblings		
	(1)	(2)	(3)	(4)	(5)	(6)
	<i>A. Weekly income¹</i>					
White in 1940 (passed)	0.273 *** (0.040)	0.185 *** (0.037)	-0.041 (0.079)	0.191 * (0.097)	0.054 (0.089)	-0.442 ** (0.216)
Years of schooling		0.068 *** (0.004)	0.064 *** (0.004)		0.098 *** (0.016)	0.065 *** (0.022)
White 1940 X years of schc			0.029 *** (0.009)			0.065 ** (0.026)
Observations		--- 3,948 ---			--- 293 ---	
Mean[wkly income black] ⁶		--- 15.0 ---			--- 17.0 ---	
Mean[wkly income white] ⁶		--- 22.3 ---			--- 23.5 ---	
	<i>B. Weekly labor income</i>					
White in 1940 (passed)	0.351 *** (0.045)	0.269 *** (0.042)	-0.063 (0.096)	0.130 (0.125)	0.001 (0.116)	-0.772 ** (0.313)
Years of schooling		0.066 *** (0.006)	0.061 *** (0.006)		0.120 *** (0.020)	0.063 ** (0.027)
White 1940 X years of schc			0.042 *** (0.010)			0.100 *** (0.032)
Observations		--- 3,104 ---			--- 184 ---	
Mean[wkly labor income black] ⁶		--- 16.9 ---			--- 19.6 ---	
Mean[wkly labor income white] ⁶		--- 26.3 ---			--- 26.0 ---	

Notes:

1. Weekly income in Panel A is the sum of the weekly income from labor as reported directly in the census and the median non-labor income for the occupation and region (South or non-South) of the individual. Median incomes calculated from the IPUMS sample of the 1950 census, and adjusted for 1940 prices (multiplied by 0.4).

2. Columns (1) through (3) report results for all Mulatto children who have brothers, all aged 3-18, in 1910, matched to 1940. Columns (4)-(6) report results for children classified in 1910 as Mulatto who were classified as White in 1940 and their siblings who were classified as "Negro" in 1940. The latter category represented what would be classified in 1910 as either Black or Mulatto. Fixed effects at the household level were used in columns (4)-(6).

3. All regressions additionally control for age, squared age, and birth order among sons in the household in 1910.

4. The omitted category for White in 1940 has individuals classified as "Negro" in the 1940 census. All were classified as Mulatto in 1910.

5. All standard errors are clustered at the enumeration-district level.

6. Means of the income levels for the samples of columns (1) and (4) are presented at the bottom of each panel. Means are calculated only for strictly positive incomes.

- * p<0.10, ** p<0.05, *** p<0.01