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COLORISM AND AFRICAN-AMERICAN WEALTH: EVIDENCE FROM THE NINETEENTH-CENTURY SOUTH

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ABSTRACT

Black is not always black. Subtle distinctions in skin tone translate into significant differences in outcomes. Data on more than 15,000 households interviewed during the 1860 federal census exhibit sharp differences in wealth holdings between white, mulatto, and black households in the urban South. We document these differences, investigate the relationships between wealth and the recorded household characteristics, and decompose the wealth gaps into treatment and characteristic effects. In addition to higher wealth holdings of white households as compared to free African-Americans in general, there are distinct differences between both the characteristics of and wealth of free mulatto and black households, whether male- or female-headed. While black-headed households' mean predicted log wealth was only 20% of white-headed households', mulatto-headed households' was nearly 50% that of whites'. The difference between light- and dark-complexion is highly significant in semi-log wealth regressions. In the decomposition of this wealth differential, treatment effects play a large role in explaining the wealth gap between all subpopulation pairs.

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1. Introduction

The first documented case of a white man being punished for engaging in miscegenation occurred in Virginia in 1630, just 10 years after the arrival of the first Africans into the colony (Mumford 1999). Interracial sex is not new and neither are the questions of the relationship between identity and race. Throughout the nineteenth and twentieth centuries, both whites and blacks were uncomfortable with miscegenation and mixed-race people (Bynum 1998). Although the number of interracial unions in the United States is currently increasing, the desire of mixed-race individuals to seek a racial identity separate from their constituent racial ancestries is not new. For many of African descent, black is not black—both in terms of how they view themselves and how others view them. There are meaningful subtleties of shade, differences that have socially and politically meaningful distinctions today—just as they have in the past.[1]

Despite the complex history of miscegenation, researchers have been complacent about the content of federally defined racial categories (Snipp 2003). With a few recent exceptions (Bodenhorn and Ruebeck 2003; Darity, Mason, and Stewart Forthcoming), economists have used the blunt instrument of binary racial classifications to categorize individuals as either black or white. Researchers consider all individuals within a category to be subject to similar treatment. Relatively little attention, if any, is paid to the concordance between social constructions of race and official categorizations. What may appear to be self-evident—whether an individual is black

[1] Modern studies of the social consequences of skin shade differences include Russell, Wilson and Hall (1992), Root (1996), Daniel (2002), Tizard and Phoenix (2002), Herman (2002), and Doyle (undated).

or white—actually masks a complex reality and likely does not capture the subtleties of racial interactions. If different skin tones are treated unequally, the continued practice of scholarly racial categorization based on anachronistic notions developed in the Jim Crow South may not lead to useful or relevant insights into modern phenomena. As Trina Jones (2000, p. 1499) notes, to understand "the nuanced ways in which discrimination operates and differentially impacts similarly but not identically situated people, we must examine and attempt to understand color. Such an understanding begins with history."

Embracing Jones' call for historical understanding, our paper turns to history to document the economic consequences of colorism, thereby providing fresh insights into an increasingly modern concern. Using data on more than 15,000 households interviewed during the 1860 federal census, we find sharp differences in wealth holdings between white, mulatto and black households in the urban South.[2]

This study advances the literature in two ways. First, we consider the operation and effects of colorism closer to its historical origins. Historians attribute the origins of light-complexion preferences to slaveholders. By investigating data on complexion-based differences in attainment of free African-Americans in the pre-Civil War South, our study provides a more objective basis for such beliefs and provides a baseline against which to consider modern outcomes. Second, recent economic and sociological research has stressed the importance of household wealth, rather than income, for the determination of socioeconomic status (Conley 1999; Shapiro 2004). Accumulations of household wealth have been shown to insulate middle-

[2] Teo (2003) notes how the use of terms such as mulatto and colored, as well as the conception of mixed-blood, are terms heavily laden with racial, often racist, connotations. This is not the place to develop a new terminology. We will use the term mulatto in the contemporary context to describe mixed-race and, generally, light-complected African Americans. We recognize that some readers dislike the word, and ask for their indulgence. Some have suggested the terms biracial or multiracial be used instead, but we resist because the modern use of these terms is politically charged and does not capture the meaning of mulatto as it was used in the mid-nineteenth century. We will also use the terms light-complected and dark-complected to recognize the likely factor that determined the "mulatto" and "black" identifications and to refer to modern studies of colorism.

and working-class families from falling into poverty when they face even short-term labor market disruption or other temporary crises (Oliver and Shapiro 1995). Previous studies of the economic consequences of modern colorism have focused on income (Goldsmith, Hamilton, and Darity (2004); Darity, Dietrich and Hamilton (Forthcoming)), which incompletely captures the consequences of differential treatment according to skin shade. With its twin focus on wealth holdings and complexion, this paper demonstrates that the economic consequences of colorism were (and likely are) far reaching.

Section 2 summarizes the history and modern experience with colorism in the African American community, including just a handful of recent economic studies. We then proceed through an empirical analysis in three steps. Section 3 describes the data and takes a first look at the three sub-populations' differences evident in the 1860 census manuscripts. In Section 4 we present several regressions in which we show that bivariate racial classification does not capture the subtleties of race even after controlling for a wide variety of individual and household influences. By separating African-Americans into blacks and mulattoes, we find meaningful differences in outcomes between households with a black or a mulatto head. Section 5 presents decompositions of the complexion wealth gap to identify how the wealth gaps are influenced by differences in wealth-generating characteristics between the groups and by differences in treatment between the groups not explained by the observed characteristics other than skin color. The decompositions show that this treatment effect explains a substantial fraction of the observed complexion wealth gaps among all sub-populations. Section 6 summarizes and offers some concluding remarks.

2. Colorism and socioeconomic outcomes

History provides compelling evidence of a consistent pattern of preferences shown, by both whites and blacks, toward light-complected African Americans. The economic advantages of a light complexion date back to slavery. Reuter (1918) and Frazier (1957), among others, argue that a complexion advantage appeared early in the slavery era when slaveholders disproportionately selected light-complected blacks to work as house servants and field foremen. Indeed, some slaveholders contended that "no man would buy a mulatto for field work" (Johnson 1996, p. 111), while others reported that light-complected slaves resisted field work by arguing that their physical constitutions could not stand up to the demands of hard labor (Kemble 1863, p. 193). Although few slaveholders balked at setting light-complected slaves to field work, they preferentially selected light-complected slaves for craft training and apprenticeship.

According to Frazier (1957) a fair complexion improved a slave's life chances by significantly reducing his or her toil and drudgery, by improving his or her access to food and shelter, and by exposure to the culture, as well as the manners, dress, and linguistic conventions of whites. Light-complected, mixed-race slaves were also more likely to be educated and manumitted. Moreover, despite legal prohibitions in many states against slaveholders providing for slaves in their wills, masters did occasionally provide for their mixed-race offspring at their passing, sometimes at the expense of white offspring (Horowitz 1973).

In the antebellum South, free light-complected blacks were more likely than dark-complected blacks to be literate. Horton and Horton (1997, p. 154), for example, report general white opposition to black school attendance except for those of "very light complexion." With greater access to education, mulattoes labored at more remunerative occupations than blacks. Neither mulattoes nor blacks labored in high prestige occupations but, among African Americans, mulattoes dominated positions at the top of the employment ladder—merchants,

shopkeepers, and skilled craftsmen. Those with fair complexions were also overrepresented among skilled workers and professionals. Among rural free African Americans, Bodenhorn (2003) finds that mulattoes were more likely than blacks to work as tenants or to own their own farms.

Gatewood (2000) shows that social preferences for light-skinned blacks persisted through the post-Civil War period, but the increasing severity of Jim Crow and the movement toward the one-drop rule[3] worked to politically and socially align light and dark blacks, groups that were often at odds prior to the Civil War. Nevertheless, color-based differences persisted and by 1920, colorism and complexion-based stratification in the African American community gained the attention of sociologists, notably E. B. Reuter (1918, 1928). A number of studies conducted through the 1930s and 1940s, including Myrdal's (1944) magnum opus, found support for Reuter's contention that light skin tones and perceptible traces of non-African heritage were associated with material advantages for African Americans (Hill 2000). A rhetoric of complexion egalitarianism, however, seemingly reigned through the Civil Rights era and into the 1970s, which led to fewer studies of colorism during the 1960s and 1970s (Freeman et al. 1966 is a notable exception). Since the early 1990s, however, sociologists have become interested in complexion-based differences in non-white communities. Modern evidence documents the continuing, perhaps growing, significance of colorism.

[3] In 1787 Virginia defined anyone with one or more black grandparents as mulatto, a definition that was reiterated in 1792 and 1819. In 1833 the Virginia legislature conceded that the one-fourth definition created a class of people that were neither black nor mulatto nor white. An act defined such people with less than one-fourth black ancestry to petition the local court for certificates of freedom that would establish their legal "whiteness." It was not until 1866 that the term mulatto was eliminated from the civil code and replaced with the word colored. In 1910 the statutory limit was changed so that anyone with one-sixteenth black ancestry was considered colored. Finally, in 1930 the law was changed so that anyone with any ascertainable amount of black ancestry was colored (Wadlington 1966, pp. 1196, 1201). Mississippi's state constitution in 1911 classified individuals with one-fourth black ancestry as legally colored (Anonymous 1911). By 1948, the constitution had been amended so that individuals with one-eighth black ancestry were colored (Bynum 1998). Legal definitions in other southern states followed similar trajectories.

Using data from the National Survey of Black Americans conducted in 1980, Hughes and Hertel (1990), Keith and Herring (1991), and Seltzer and Smith (1991) find that dark-skinned blacks acquire less education, work in less prestigious occupations, and earn lower incomes than lighter complected blacks, even after controlling for other factors believed to be correlated with achievement. Using longitudinal analysis, Hill (2000) finds that biracial men enjoyed *modestly* advantaged backgrounds compared to blacks, but multivariate analysis reveals that family background variables account for only a small portion of complexion-based differences in outcomes. Color is the decisive factor. Light-complected men are more than twice as likely as dark men to find high-prestige employment. Research has also documented skin-tone differences among Latinos, which suggests that colorism characterizes a host of minority communities (Murguia and Telles 1996; Telles and Murguia 1990; Mason 2004; Darity and Boza 2003). Gullickson's (2003) interpretation that the significance of skin tone differentials are declining stands in sharp contrast to an emergent literature that finds complexion differences still loom large in the life experiences of mixed-race youth (Russell, Wilson and Hall (1992), Root (1996), Daniel (2002), Tizard and Phoenix (2002), Herman (2002), and Doyle (undated)).

Despite the attention that colorism has garnered among sociologists, economists have not accounted for intraracial differences in economic outcomes that may result from colorism. One recent exception that uses nationally representative samples of African Americans from 1979 and 1992 finds that the wages of light-complected blacks exceed those of dark-complected blacks by about eight to ten percent (Goldsmith, Hamilton, and Darity 2004). By comparison, their regression decompositions suggest color-based wage differentials (treatment effects) between whites and light-complected blacks of about five percent. Thus, colorism accounts for about twice the income gap between light- and dark-complected blacks as between whites and light-

complected blacks. Thus, subtle distinctions in skin tone seemingly translate into significant outcome differences, including educational attainment, occupational achievement and earnings, among others. We now turn to a description of the data source that allows us to contribute to this emergent and important discussion.

3. Data

This study uses a large, regionally representative sample of free African Americans residing in the urban South, constructed from information in the original population manuscripts of the 1860 census. In discussing the outcomes of free African Americans we are, by definition, excluding the experience of slaves, who represented the majority of blacks in the antebellum South and held little or no wealth.[4] The cities that are included in the sample are listed along with their relative frequencies in the lower part of Table 1's summary statistics. Williamson (1980) notes that the border states of the Old South formed a sort of *mulatto belt*, which contained a high proportion of mulattoes relative to blacks. The Lower South cities of Charleston, Mobile, and New Orleans also had substantial populations of *gens de couleur libre*. The size or proportions of their respective mulatto and black populations did not drive the choice of cities for this study. Rather, cities were selected to provide a representative sample of all southern, urban free African Americans in 1860.

Every household headed by an African American (black or mulatto) from these cities was recorded while collecting data, along with an equal number of randomly selected white households. Some non-white households were later dropped because the census failed to report information on one or more of the variables of interest or because the values reported were

[4] Kenzer (1997) provides a discussion of wealth accumulation among free blacks, slaves and former slaves for North Carolina.

clearly erroneous. The resulting sample includes information on 15,861 households. Of these, 5,774 households were headed by white males, 2,121 were headed by mulatto males, and 3,697 by black males. The sample of 4,269 female-headed households includes 842 headed by whites, 1,451 headed by mulattoes, and 1,976 headed by blacks.

The 1860 census is a particularly valuable source for the study of the economic consequences of colorism because census marshals were instructed to classify individuals as white, black or mulatto. Specifically, marshals were directed to leave the "Color" column blank if the individual was white, to insert the letter "B" if the person was black without admixture, and to insert the letter "M" if the person was a mulatto or of mixed heritage. It is unclear whether marshals inquired into a person's racial heritage or classified him or her based on physical appearance, but Gross (1998) contends that local residents, including census takers, used locally-accepted notions of race and admixture to draw conclusions about one's racial heritage. Most southerners believed the differences between blacks and mulattoes were obvious and involved complexion and physiology, as well as the complexion and physiology of those with whom one associated.[5] There is no reason to believe that a census taker's assignations of color would have deviated from accepted community standards of racial classification. A second concern is whether a person classified as a mulatto in Baltimore would have been similarly classified in New Orleans. Even if there were inter-city differences in classification standard, to the extent

[5] Of tangential concern to us is the possibility (in fact, the likelihood) that the identity of one's associates also played a part in the census taker's determination of color. This connection between an individual's choices and his or her identity is important but outside the scope of this paper. It is an active area of research (see Bodenhorn and Ruebeck (2003) for references), but here we seek to document the effects of colorism, with less concern for questions of its source and its links to individuals' identity.

that they existed at all, they do not vitiate the results presented here. What ultimately determined whether an individual received differential treatment was whether he or she met the *local* physiological or heritage standards, because that individual interacted with members of the *local* community. Inclusion of city dummy variables in the regressions reported below also controls, in part, for local differences in the relationship between treatment and racial categorizations.

In addition to the valuable information on complexion, federal census takers from the mid-nineteenth century collected and recorded information on a number of demographic and economic variables. Household structure is easily inferred from the manuscripts because the head of each household is listed first, identified by both given name and surname. Only first names are recorded for spouses and children. Members of the extended family were listed next, followed by unrelated members of the household (typically servants or apprentices), both identified by given name and surname. The ages and genders of all household members were recorded, along with their state or nation of birth, and whether the individuals were literate or had attended school in the past year. The census marshals also recorded economic data, including the occupations of household heads. In a few instances, marshals recorded the occupations of the wives of male-headed households, but no use is made of this information because it was inconsistently reported.

Finally, census marshals were asked to provide estimates of the dollar value of each household's real and personal property. The value of household real estate was to be obtained by asking the householder for his or her valuation of the real property owned by the household ignoring any lien or encumbrance on the property. Thus, the reported figure is gross, not net, real estate wealth and does not include the value of rental property. Marshals were also instructed to obtain the householder's valuation of all other property (personal estate), including financial

assets, slaves, livestock, jewelry, fixtures and furniture. The instructions recognized that an accurate valuation may not be possible, but marshals were encouraged to obtain as "near and prompt" an estimate as they could. The Census Bureau anticipated the reluctance of many householders to divulge information about their wealth and instructed marshals to cajole and reassure respondents that the information was confidential and would not be used for private gain or any other public (i.e., tax) purposes.

It is clear that some marshals were better at cajoling and reassuring householders than others. It was not uncommon for marshals to return a blank (nonresponse) when reporting real and personal estate in the manuscripts. An empty cell in the real estate column is generally taken to represent zero wealth, reflecting that the family rented rather than owned its current habitation, but historians have long debated the meaning of nonresponses for personal wealth, and the debate is far from resolved. Some contend that marshals left the cell blank rather than recording zeroes. Others contend that marshals failed to report modest or hard-to-value property holdings, so that nonresponses likely represent small but nonzero personal wealth. Conley and Galenson (1998) and Bodenhorn (2003) review the debate and the data and conclude that marshals had idiosyncratic, nonzero censoring points for personal wealth below which they returned a blank.

Just as there is no consensus concerning the interpretation of unreported personal wealth for a household, there is no consensus among economic historians concerning a preferred empirical technique. Conley and Galenson, and Bodenhorn, estimate quantile or median regressions; others estimate Tobit models. Given the common assertion that nonresponses are typically indicative of small, but nonzero personal property values, we attribute to each nonresponse one-half the minimum value recorded by any marshal in each city ward, and then correct the standard errors for ward-level clustering. We conducted a number of robustness

checks (available by request): we estimated models that excluded households with unreported wealth; we excluded wards if nonresponses exceeded 10, or 25, or 45 percent of sampled households; we replaced the natural log of wealth as the dependent variable with the inverse hyperbolic sine, among other procedures. In every instance, the basic results reported below hold. We prefer our procedure because estimating the models using Ordinary Least Squares allows us to report the full results of standard Oaxaca-Blinder regression decompositions. Thus, although the wealth data drawn from the 1860 U.S. census are imperfect, they are rich enough to generate meaningful comparisons of wealth accumulation among white, black, and mulatto households.

In addition to age and complexion, we employ a number of variables expected to explain differences in household wealth. We label household heads *Migrants* if they migrated across state lines.[6] We also label as *Immigrants* any head of household born outside the United States. Migrants and immigrants are often thought to be self-selected, highly motivated individuals, suggesting that, all else equal, movers will accumulate more wealth than nonmovers (Borjas 1994). On the other hand, movers may have financed their migration by consuming previously accumulated wealth, which may imply lower current wealth. There is no reason to expect, *a priori*, one effect to dominate. Summary statistics reported in Table 1 show that while a little over 10 percent of all male heads of households migrated across state lines, another 25 percent immigrated from outside the United States. Women were as likely as their male counterparts to have migrated across state lines, but a much smaller proportion of female heads of household were immigrants. Given the restrictions on black in-migration in most southern states, it is not surprising that blacks and mulattoes were less likely than whites to migrate across state lines

[6] Given the proximity of Baltimore and Washington, D.C., individuals who moved from Washington to Baltimore were not considered interstate migrants.

(Guild, 1969). Despite such restrictions, about 7 percent of African-American male household heads and nearly 9 percent of African-American female heads had migrated across state lines. Further, 1.5 percent of African-American male heads of households had emigrated from foreign, mostly Caribbean, countries, as had 2.7 percent of African-American female heads of households.

Another set of variables is included to control for the household's human capital. We include a dummy variable equal to one if the head of the household was literate, a variable equal to the number of literate males 20 years of age or older (including the head of the household), and a variable equal to the number of literate females 20 years of age or older in the household. Table 1 shows that about 82 percent of male heads of household and 74 percent of female heads were literate. Both male- and female-headed households on average had more than one literate female (1.109 and 1.280, respectively), but female-headed households generally had far less than one literate adult male while the mean male-headed household had more than one. Given the absence of publicly funded education for African Americans in the antebellum South, it is not surprising that African American households had fewer literate adults than white households. Indeed, the level of literacy among African Americans is notable given the outright animosity among many whites toward the education of blacks and mulattoes (Woodson 1919).

Two additional variables are constructed to capture possible education or training effects. The first (SEI) is a Duncan-style socioeconomic index (Reiss 1961), intended to indicate occupational prestige. It is based on wages and educational levels associated with several hundred occupations in the 1950s. For occupations recorded in the 1860 census with the same title (e.g., barber, blacksmith, carpenter, bricklayer, etc.) the occupational prestige index value was taken directly from the 1950s index. For some occupations recorded in the 1860 census,

assigning a corresponding Duncan occupational prestige score required some ingenuity. Duncan's index has no entry for carriage drivers (ignoring only those few still found in Central Park and other tourist locations), but fortunately Duncan did list a prestige score for the modern counterpart of the carriage driver, namely the taxi driver. Similarly, modern bus drivers are roughly the equivalent of stagecoach drivers. Because Duncan's classification includes several hundred occupations—from bootblacks (SEI = 8) and charwoman (SEI = 10) to lawyers (93) and dentists (96)—and nearly every major industrial category, it is possible to assign a reasonable occupational prestige value to every occupation listed in the 1860 census.[7] When a specific occupation listed in the 1860 census had no reasonable corresponding Duncan value, that occupation was given Duncan's general prestige score for job class (foreman, operative, or laborer) and industry (textiles, metals, machinery, services, etc.). Finally, just as marshals failed to report wealth holdings for some households, they also failed to report occupations for every household head. These observations are also not deleted from the sample. We attribute to each of these households the average occupational prestige value by the gender of the household head (15 for males, 14 for females), and we include a dummy variable to indicate these observations. The dummy variable should capture any fundamental differences between reporting and non-reporting households, but we can see from Table 1 that there may be much less concern for this issue in the men (6% of the male sample) than females (46% of the female sample, with representation decreasing significantly from white to mulatto to black women).

It is likely that the labor supply decisions of women and men could be very different, as they often can be today, and as a result the determinants of wealth accumulation may have been quite different across gender as well. This leads us, as is the usual practice, to present separate results for male- and female-headed households. Historians and labor economists have noted

[7] A detailed appendix of occupational classifications is available from the authors on request.

differential economic and occupational opportunities for women, and have discussed the negative consequences of single parenthood (Moehling 2004). We also report our full model regressions both with and without the occupational variables because occupations may be endogenous: to the extent that family wealth persists across generations, greater wealth in previous generations may imply both greater wealth and a more prestigious occupation for the current head of household. Because we cannot account for intergenerational transfers and because we lack any reasonable instruments to perform two-stage least squares, we ask the reader to interpret those regressions including the occupational variables with care.

Before moving on to the regressions, we call attention to the substantial differences in means across same-gender subpopulations (comparing among white, mulatto, and black as well as comparing white to African-Americans as a whole). Nearly all these differences were highly statistically significant (see the comments below Table 1 for exceptions). Note especially the significant differences in characteristics between blacks and mulattoes. In every category mulatto-headed households were better endowed than their black counterparts, but mulattoes were still not as well endowed as heads of white households.

4. The determinants of household wealth

Our empirical strategy in this section is to estimate household-level wealth regressions that include several explanatory economic and demographic characteristics as well as the complexion of the household head, Black and Mulatto (omitting White). The parameter vectors β and γ in the equation

(1) $$\ln(w_j) = \beta_0 + \beta_1 X_j + \beta_2 Y_j + \beta_3 Z_j + \gamma_1 \text{Black}_j + \gamma_2 \text{Mulatto}_j + \varepsilon_j$$

are estimated using ordinary least squares regression. The dependent variable $\ln(w_j)$ is the natural logarithm of the j^{th} household's wealth (following the usual semi-log regression form due to the skewed nature of the distribution of wealth in levels), X_j is a vector of individual characteristics of the household head (age and its square, immigrant and interstate migrant status, literacy, and occupational prestige score), Y_j is a vector of household characteristics (the number of literate males, the number of literate females, and the size of the household), and Z_j is a vector of city dummy variables (excluding Baltimore) to capture any systematic regional differences in the ability of households to accumulate wealth. ε_j is the error term associated with the j^{th} household. We also estimated the equation

$$(2) \qquad \ln(w_j) = \beta_0 + \beta_1 X_j + \beta_2 Y_j + \beta_3 Z_j + \gamma \mathrm{AfAm_j} + \varepsilon_j \ ,$$

where both subpopulations with African-American heritage are grouped into the same category, AfAm. We only present the value of this shift variable's coefficient in our results because the other parameters did not differ substantively from those estimated using (1).

Table 2 reports the regressions. Probability weights are applied to account for the sampling method described in the Section 3.[8] The columns labeled (3) and (4) are our preferred specifications because they offer the largest number of exogenous controls. A mentioned above, we include regression (4) to account for occupational differences but recognize that occupations may be endogenous to wealth. Before turning to the issue of colorism, we summarize and interpret the other individual and household-level correlates and the overall performance of the regressions.

[8] These weights take into account the undersampling of white-headed households. We took the assertion that the census included all free households at face value and used the probability weights to adjust for the relative probability of sampling from each household subpopulation.

Household wealth for both male- and female-headed households increased with age at a decreasing rate. The maximum for men occurs at about 65 years, an age which little more than five percent of our sample exceeded, although a handful of ages in the 100's were recorded for both genders.

Households headed by male immigrants had about half ($e^{-0.56} = 0.57$) of the wealth of households headed by native-born men, indicating that the benefits of initiative were outweighed by the costs of moving (at least for the current generation), although the results in column (4) indicate that immigrants' advantage may be explained by their occupations. The results in columns (1)-(3) are consistent with the economic studies of immigration that show that first-generation immigrants are slow to assimilate often because immigrants tend to congregate in ethnic enclaves (Borjas 1994; Borjas 2000). We have no evidence on the ethnic or racial compositions of the neighborhoods in which southern urban African Americans resided, but relying on crude data at the level of census wards Curry (1981) found that blacks were considerably less likely to be crowded into proto-ghettoes in southern than in northern cities in the antebellum era. It may also be the case that these free blacks' forebears had not passed significant wealth on to them. Female-headed household wealth did not have a significant relationship with immigrant status. Unlike international immigration, internal migration has no statistically or economically meaningful effect on household wealth once the city controls are added.

Household size was significantly related to household wealth. The addition of one person to a male-headed household was associated with more than a 10 percent increase in male-headed household wealth, while the increase was somewhat smaller in a female-headed household. The results are consistent with the hypothesis that larger extended households, which were relatively

common in 1860, brought together larger agglomerations of wealth than smaller nuclear households.

Literacy, as expected, had an economically meaningful association with household wealth. Households in which the male head was literate had more than double ($e^{0.83} = 2.3$) the wealth of households with an illiterate male head (about 50% more if we control for occupational status). There was little relationship between wealth and female heads' literacy. Interestingly, the addition of a literate female was associated with an increase in male-headed household wealth of about 20 percent, and an increase in female-headed household wealth by about 25 percent, but the addition of a literate adult male did not add significantly to household wealth. We do not have an explanation for this phenomenon.

Turning now to the issue of colorism, we consider the estimated coefficients on the Black and Mulatto variables, as well as that on the AfAm variable in the incompletely reported companion regressions. (See the first row of coefficients in Table 2 for the latter, and the second and third rows for the former pair of variables.) It is immediately apparent that standard specifications of a single race identifier do not fully capture the subtleties of race and racial wealth differences. Simply comparing the means of the white, mulatto, and black sub-populations implies that a household headed by an African-American male had just 30 to 50 percent ($e^{-1.26}$ to $e^{-0.76}$)—or less—of the wealth of the average household headed by a white male. But when we separate African-American households into those with mulatto and black heads of household, there are significant and meaningful differences between them. Households headed by a black male had on average less than 40 percent ($e^{-0.95}$)—probably less than 25 percent ($e^{-1.46}$)—of the wealth of households headed by white males, while the mean household with male mulatto head had *at least* 40 percent – and perhaps as much as 63 percent ($e^{-0.46}$) -- of white male

wealth. Even more distinct bounds can be placed on female-headed households' wealth differentials by racial ancestry. The fact that the separate male and female regressions generate such similar results increases our confidence that we are identifying a real phenomenon. Further evidence is provided in the following section by estimates of separate wealth equations for each subpopulation where we decompose the differences into characteristics and treatment effects.

We recognize that, if colorism was as powerful a social force as we contend, ancestry and skin shade may have influenced wealth accumulation because lighter-complected individuals received preferential treatment at younger ages that created occupational opportunities not available to those with darker complexions. Dark individuals may have been discriminated against through the apprenticeship system or through other types of occupational training. Columns (4) in Table 2 address this concern by including the Occupational Prestige (SEI) and No Occupation variables. As we recognize above, the coefficients on the colorism variables are smaller than in columns (3), but the colorism effect does not disappear. It remains a powerful force even after controlling for occupation. In every specification, we reject the null hypothesis of Black and Mulatto coefficient equality at confidence level $p \leq 0.001$.

The shortcoming of the analysis in Table 2 is that it imposes equality restrictions on the non-race (non-colorism) coefficients across the subpopulations. Thus the analysis above ignores the possibility that, in controlling for the household's characteristics, we have missed the differing return to characteristics across subpopulations. In the next section, we report the results of standard Oaxaca-Blinder regression decompositions to investigate the extent to which the colorism effect was attributable to differences in characteristics across groups versus arising from the treatment afforded those of different complexions.

Before moving on to the decompositions, we should also note that the regressions' adjusted-R^2 values indicate that there is much variation in household wealth remaining to be explained. Because the decomposition techniques we use below rely on an analysis of residuals, the results must still be considered only imperfect insights into the treatment effects that follow from colorism.

5. Did light-complected households receive preferential treatment? Evidence from regression decompositions

Wealth differences between any two groups may be due to differences in the average features of that group's members (characteristic effects), or to systematic differences in how each group is rewarded for those features by the marketplace (treatment effects). The premise of this article is that colorism influences economic outcomes, generating greater household wealth in light-complected households for a given set of observable characteristics. In order to better understand the contributions of these two effects of colorism, we use the following methodology.

For each gender, separate coefficients are estimated for the white, mulatto, and black subpopulations. Because there are some concerns that the occupational prestige variable may be endogenous, regressions and decompositions are reported with and without the occupational variables, as in columns 3 and 4 of Table 2. We omit the regression results themselves to conserve space and because they do not meaningfully differ from those in Table 2. We then use these regressions to further investigate the differences between subpopulations' wealth using the decomposition method of Oaxaca (1973) and Blinder (1973), in addition to Cotton's (1988) modified form.

The goal of the decomposition is to consider the counterfactual implied by the assertion that different groups have different wealth levels, due perhaps to colorism: what wealth would the members of each group expect to have in the absence of their identification as a member of their group? Although we will construct a decomposition for all four pairs of sub-populations, consider for concreteness the pair of subpopulations whose wealth differences may illustrate colorism: mulattoes and blacks. Label the associated pair of regressions $\ln w_{Mulatto} = \alpha_{Mulatto} + X_{Mulatto}\beta_{Mulatto} + \varepsilon$ and $\ln w_{Black} = \alpha_{Black} + X_{Black}\beta_{Black} + \varepsilon.$ To decompose the advantages accrued by those with lighter skin, we consider the difference in predicted log wealth evaluated at the explanatory variables' means (indicated by "$\bar{}$" notation) and parameter estimates (indicated by "$\hat{}$" notation)

$$(3) \qquad \ln w_{Mulatto} - \ln w_{Black} = \left[\hat{\alpha}_{Mulatto} - \hat{\alpha}_{Black} + \overline{X}_{Black}(\hat{\beta}_{Mulatto} - \hat{\beta}_{Black})\right] + \left[(\overline{X}_{Mulatto} - \overline{X}_{Black})\hat{\beta}_{Mulatto}\right],$$

where the first bracketed term is the unexplained, or treatment, effect and is presumed to be due, at least in part, to colorism. The second term is the characteristic effect, the effect on mean log wealth due to differences between the two groups. With the structure of (3) we are taking the perspective that in the absence of colorism the advantaged group's wealth structure would prevail. The opposite assumption is to presume that all groups would receive the disadvantaged group's wealth structure in the absence of colorism, or

$$(4) \qquad \ln w_{Mulatto} - \ln w_{Black} = \left[\hat{\alpha}_{Mulatto} - \hat{\alpha}_{Black} + \overline{X}_{Mulatto}(\hat{\beta}_{Mulatto} - \hat{\beta}_{Black})\right] + \left[(\overline{X}_{Mulatto} - \overline{X}_{Black})\hat{\beta}_{Black}\right].$$

These two decompositions provide a range within which we expect the actual effect of colorism to lie. Many alternative decompositions have been proposed to predict a particular value inside this range. Because we feel, along with other authors, that the counterfactual wealth is likely to lie closer to the larger group's prevailing wealth, we calculate Cotton's (1988) decomposition as well. That is,

$$
\begin{aligned}
&\ln w_{Mulatto} - \ln w_{Black} \\
&= \left[\hat{\alpha}_{Mulatto} - \alpha^* + \overline{X}_{Mulatto}(\hat{\beta}_{Mulatto} - \beta^*)\right] + \left[\alpha^* - \hat{\alpha}_{Black} + \overline{X}_{Black}(\beta^* - \hat{\beta}_{Black})\right] \\
&\quad + \left[(\overline{X}_{Mulatto} - \overline{X}_{Black})\beta^*\right]
\end{aligned} \tag{5}
$$

The vector of parameters (α^*, β^*) generalizes the previous two cases: $(\alpha^*, \beta^*) = (\alpha_{Light}, \beta_{Light})$ in (3), while $(\alpha^*, \beta^*) = (\alpha_{Dark}, \beta_{Dark})$ in (4). The parameter vector that we actually use for (α^*, β^*) is a weighted sum of the previous parameters, where the weights are the relative frequencies of the two populations[9]. This decomposition has the added feature that it apportions the treatment effect into components that indicate the gain of the advantaged subpopulation and the loss of the disadvantaged population (respectively the first and second bracketed terms above), but we do not report these separately because our focus is instead on the total treatment effect, the addition of those first two bracketed terms. It is also worth noting that, because the weights are the subpopulations' relative frequencies, when comparing the treatment of people of color (blacks, mulattoes, or African-Americans as a whole) to the white subpopulation this decomposition weights the advantaged population (the majority whites) more heavily, but when considering the existence of *colorism* it is the disadvantaged population (the majority blacks) that receives the higher weight.

The two panels of Table 3 first provide estimates of the subpopulation pairs' relative predicted wealth and the resulting raw wealth differentials, as well as the subpopulations' predicted geometric mean wealth (the exponential of the arithmetic log mean). Note that our discussion of the raw wealth ratios compared across subgroups from the previous section's results with pooled regression is similar to the results presented here from separate regressions. Households headed by black men hold on average 13% of white wealth, while mulatto-headed

[9] Due to our sampling method, the relative frequencies are actually calculated as the sum of each subpopulation's probability weights.

households' average wealth is 35% of white-headed households. The results for female-headed households are quite similar to those for male-headed households.

Next in Table 3's two panels, the raw differential is decomposed into two parts using the method described above and estimation of the model from either column 3 (no occupation variables) or column 4 (with occupation variables) of Table 2. The raw differential is decomposed into both the treatment and characteristic effects, but to save space Table 3 presents only the treatment effect. This effect is the (imperfect) estimate of discrimination or colorism. As well, the characteristic effect (to which we return later in the table) need not be listed explicitly because it is simply the remainder of the raw differential. Our discussion focuses on the third set of treatment effect estimates, those typeset in boldface and listing the characteristic effect as $C = \beta^*(X_H - X_L)$, using Cotton's (1988) decomposition. For example, on the right side of Table 3 where the results of pairing the white and African-American subpopulations are presented, we see that for male-headed households the treatment effect is estimated at 69% of the raw differential with model (3) and 42% with model (4). Thus the characteristic effect is 31% and 58%, respectively, for the two models' application to this pair of subpopulations. These percentages are the focus of this portion of the table: how much of the raw wealth differential is explained by treatment effects?

For the moment, compare the boldface treatment effects to those presented in the rows above them for each gender. We see—as should have been expected—that Cotton's estimate is always between those of the traditional Oaxaca and Blinder decompositions. Its counterfactual assumption is a weighting of the two groups' returns to characteristics, while the Blinder-Oaxaca decompositions take two possible extreme counterfactual assumptions: that either the advantaged

or the disadvantaged group's returns to characteristics would obtain in the absence of differential treatment across groups.

Our first evidence of colorism in Table 3 is simply to note the large estimate of the treatment effect in the first column's decomposition of the Mulatto and Black subpopulation pair of regressions. This treatment effect is about 40 percent of the raw wealth differential, with little difference dependent on including occupational status. Considering all sub-population pairs, the differences between models 3 and 4 illustrate another result we should expect: controlling for occupation dampens the treatment effect; more of the raw wealth differential can be explained by including this characteristic of the household head to in the regressions. Duncan's SEI is one way in which all subpopulations are different from each other, it has a positive relationship with wealth, and the more advantaged subpopulations have higher SEI.

On the other hand the larger white-mulatto treatment effect (85%) as compared to the white-black treatment effect (66%), without controlling for occupation (model 3), can be explained by mulattoes' characteristics having greater similarity to whites' characteristics than blacks' characteristics have to whites' characteristics. Even with a higher return to mulattoes' characteristics as compared to blacks', more of the raw wealth differential is left unexplained by any difference in mulattoes' characteristics as compared to whites precisely because their characteristics are less different. The resulting smaller characteristics effect leads to a larger treatment effect. Once occupation is included (model 4), the difference in treatment effects is small (41% and 43%, for males). The treatment effect for the mulatto-black pair of subpopulations shows little change whether we include occupation or not (38% versus 42%).

The final portion of each of Table 3's panels then uses the boldface estimate of the treatment effect (Cotton's method) to estimate two more comparisons of the results for the

subpopulation pairs. First is the estimated return to characteristics, the wealth ratio. It is the ratio of the advantaged group's wealth to the disadvantaged group's wealth. The values are all greater than 1, reflecting the inherent advantage in characteristics that the lighter-skinned group has, regardless of differential treatment. Loury (1998) focuses on this as an issue that remains even after dealing with discriminatory practices: the fact that a subpopulation has a disadvantage in characteristics should still be of concern even if there is no treatment effect due to discrimination. First note that including another explanatory variable (in particular, occupation) in the regression will always increase the return to characteristics. These comparisons confirm our earlier observation that whites had a much greater advantage in characteristics over blacks (a factor of 2 to 3) than they did over mulattoes (always less than 2). Likewise, the advantage of mulattoes in characteristics as compared to blacks (remarkably similar for males and females) is about a factor of 1.8. The final comparison in each panel is Oaxaca's (1973) adaptation of Becker's (1971) discrimination coefficient, the relative benefit to the advantaged group from discrimination, assuming that the entire treatment residual can be attributed to discrimination (as measured by Cotton's residual). It is measured as the percentage gain in the wealth ratio as compared to what it would be without differential treatment. Similar patterns are seen in these comparisons as those above. The gains from treatment are large, and the differences within African-Americans are also large.

So we see that, although differences in underlying characteristics were a driving force behind the mulatto-black wealth gap, treatment disadvantages were substantial. The magnitude of the treatment disadvantage estimates supports the colorism hypothesis, and reflecting on the characteristic effect further strengthens that hypothesis. Further research needs to uncover the extent to which pre-market discrimination led to differences in observable wealth-generating

characteristics. It is well known that light-complected slaves were more likely to receive skill training (Margo 1992), for example. It may have been that mixed-race free African Americans, too, received more or better education and job skills than monoracial blacks. Uncovering whether and why this conjecture was true represents a potentially fruitful line of further inquiry.

6. Concluding Remarks

Does black always signify Black? In matters of race economists have assumed it does. Nearly every economic investigation into the consequences of race uncritically and without pause separates Whites and Blacks into distinct and mutually exclusive categories. For the purposes of empirical work done during the past 50 years such a dichotomous racial classification scheme was probably appropriate, an intellectual and academic recognition of a broad social acceptance of Jim Crow's "one-drop" rule. In the future, however, dichotomous racial categories may not capture the increasing subtleties of race as race mixing spreads and becomes increasingly socially acceptable. Empirical economics cannot continue to model race as a binary variable and expect the resulting coefficient to meaningfully capture the subtleties of race.

Our paper shows that mulattoes were treated differently than blacks in the mid-nineteenth century. As a percentage of white households' log wealth, the predicted log wealth of black-headed households was 20 percentage points lower than that of mulatto-headed households. Regression decompositions reveal that treatment effects played a large role in explaining the white-mulatto wealth gap, even after including a measure of occupation; treatment effects played a comparable role for other subpopulation pairs as well. Our results are consistent with those from a sociological literature dating to Reuter (1917) and an emergent economics literature that finds that colorism remains a powerful social phenomenon in minority communities. An

important line of further inquiry will be to understand how color-based identities are established and reinforced in historical and modern populations. We intend to contribute to that developing literature as well.

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Table 1: Summary statistics

	Male-headed households					Female-headed households				
	Full sample	White	Af-Am	Black	Mulatto	Full sample	White	AfAm	Black	Mulatto
Af-Am	0.502					0.803				
Black	0.319					0.463				
Mulatto	0.183					0.340				
Wealth	3,380	6,328	455	231	846	1,023	3,707	363	177	617
	(18839)	(26285)	(2079)	(1009)	(3138)	(6130)	(13111)	(1556)	(842)	(2154)
ln(Wealth)	5.074	5.924	4.230	3.876	4.846	4.176	5.424	3.869	3.450	4.439
	(2.280)	(2.440)	(1.738)	(1.565)	(1.849)	(1.971)	(2.415)	(1.711)	(1.519)	(1.793)
Age	40.354	40.089	40.617	40.951	40.034	42.923	45.008	42.411	43.174	41.371
	(11.817)	(11.358)	(12.250)	(12.409)	(11.948)	(14.054)	(13.440)	(14.155)	(14.212)	(14.016)
Immigrant	0.252	0.490	0.015	0.008	0.029	0.080	0.296	0.027	0.014	0.044
Migrant	0.105	0.139	0.072	0.058	0.096	0.098	0.147	0.086	0.070	0.108
SEI	22.320	29.756	14.940	13.446	17.545	14.892	18.487	14.008	12.895	15.524
	(17.907)	(20.495)	(10.617)	(9.377)	(12.054)	(8.524)	(12.154)	(7.093)	(6.521)	(7.548)
No occupation	0.058	0.050	0.065	0.057	0.079	0.456	0.684	0.400	0.332	0.492
Literate head	0.821	0.971	0.672	0.606	0.788	0.736	0.944	0.684	0.626	0.764
Literate males	1.209	1.506	0.914	0.796	1.119	0.485	0.787	0.411	0.361	0.478
	(0.961)	(1.009)	(0.810)	(0.749)	(0.869)	(0.879)	(1.094)	(0.800)	(0.700)	(0.914)
Literate females	1.109	1.383	0.837	0.723	1.034	1.280	1.776	1.158	1.039	1.320
	(0.950)	(0.954)	(0.864)	(0.805)	(0.926)	(1.076)	(1.125)	(1.028)	(0.980)	(1.070)
Adults in hhold	5.381	5.965	4.801	4.608	5.137	4.174	5.001	3.971	3.799	4.205
	(2.662)	(2.706)	(2.485)	(2.398)	(2.597)	(2.436)	(2.539)	(2.366)	(2.265)	(2.479)
Baltimore, Md	0.454	0.331	0.576	0.722	0.321	0.319	0.232	0.340	0.461	0.175
Baton Rouge, La	0.009	0.012	0.007	0.001	0.017	0.012	0.021	0.009	0.004	0.017
Charleston, SC	0.077	0.105	0.049	0.022	0.097	0.121	0.138	0.117	0.069	0.182
Frederick, Md	0.030	0.032	0.028	0.025	0.033	0.027	0.050	0.021	0.019	0.024
Louisville, Ky	0.046	0.055	0.037	0.037	0.038	0.039	0.049	0.037	0.042	0.030
Mobile, Al	0.025	0.032	0.018	0.004	0.044	0.024	0.038	0.020	0.008	0.037
Nashville, Ky	0.019	0.027	0.012	0.011	0.013	0.024	0.036	0.021	0.016	0.027
New Orleans, La	0.203	0.238	0.169	0.063	0.355	0.223	0.238	0.220	0.119	0.357
Petersburg, Va	0.085	0.101	0.069	0.089	0.034	0.132	0.121	0.135	0.186	0.064
Richmond, Va	0.051	0.068	0.035	0.027	0.049	0.080	0.078	0.081	0.076	0.087
Observations	11,592	5,774	2,121	3,697	5,818	4,269	842	1,451	1,976	3,427

Means, with standard deviations (except for dichotomous variables) in parentheses.
As discussed in Section 3, we have imputed wealth values at the ward level.
All same-gender subpopulation pairs of variables have means significantly different at $p \leq 0.01$ ($p \leq 0.001$ in almost all cases), except men's age and the no-occupation indicator for white versus black men. Due to undersampling of whites, the city indicators can only be compared for black and mulatto subsamples; those other than Fredericksburg, Louisville, and Richmond consistently differ across African-American classification with $p \leq 0.001$.
Sources: U.S. Bureau of the Census, Eighth Census (1860), population census manuscripts.

Table 2: Determinants of ln (household wealth) in the urban south: ordinary least squares regression

Dependent variable	Male-headed households				Female-headed households			
is ln(wealth)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
AfAm†	*-1.731****	*-1.553****	*-1.257****	*-0.759****	*-1.481****	*-1.335****	*-1.187****	*-1.092****
	(0.247)	*(0.250)*	*(0.186)*	*(0.169)*	*(0.240)*	*(0.233)*	*(0.193)*	*(0.199)*
Black	-2.052***	-1.842***	-1.456***	-0.949***	-1.908***	-1.738***	-1.452***	-1.344***
	(0.271)	(0.278)	(0.220)	(0.202)	(0.247)	(0.239)	(0.199)	(0.213)
Mulatto	-1.215***	-1.091***	-0.945***	-0.462***	-0.958***	-0.855***	-0.875***	-0.814***
	(0.213)	(0.215)	(0.152)	(0.131)	(0.219)	(0.218)	(0.198)	(0.200)
Age	0.159***	0.133***	0.136***	0.124***	0.090**	0.081**	0.078**	0.077**
	(0.023)	(0.020)	(0.019)	(0.019)	(0.027)	(0.027)	(0.026)	(0.027)
Age squared	-0.001***	-0.001***	-0.001***	-0.001***	-0.001**	-0.001*	-0.001*	-0.001*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Immigrant	-0.714***	-0.618**	-0.559***	-0.228^	-0.239	-0.18	-0.503*	-0.480^
	(0.181)	(0.184)	(0.138)	(0.119)	(0.318)	(0.321)	(0.245)	(0.250)
Migrant	0.313	0.374^	0.172	0.091	0.282	0.346	-0.059	-0.088
	(0.199)	(0.193)	(0.142)	(0.123)	(0.236)	(0.232)	(0.249)	(0.262)
Literate	0.991***	0.838**	0.830***	0.501*	0.584***	0.344^	0.322	0.278
	(0.240)	(0.253)	(0.223)	(0.215)	(0.149)	(0.201)	(0.224)	(0.232)
Adults in hhold		0.105***	0.135***	0.123***		0.104*	0.088*	0.086*
		(0.028)	(0.024)	(0.023)		(0.040)	(0.036)	(0.036)
Literate males		-0.011	-0.047	-0.059		-0.09	-0.094	-0.105
		(0.048)	(0.042)	(0.039)		(0.094)	(0.078)	(0.077)
Literate females		0.162*	0.197***	0.115*		0.205*	0.251*	0.254**
		(0.066)	(0.052)	(0.053)		(0.102)	(0.099)	(0.091)
SEI				0.038***				0.007
				(0.002)				(0.012)
No Occupation				0.15				0.306
				(0.182)				(0.185)
City controls	No	No	Yes	Yes	No	No	Yes	Yes
Constant	0.980^	0.958^	0.184	-0.2	2.627***	2.290**	1.863*	1.653*
	(0.564)	(0.526)	(0.432)	(0.405)	(0.734)	(0.765)	(0.733)	(0.812)
Adj. R-squared	0.163	0.185	0.226	0.310	0.156	0.180	0.228	0.231
N	11592	11592	11592	11592	4269	4269	4269	4269
prob F>0 for Mulatto==Black	0.000	0.000	0.001	0.001	0.000	0.000	0.000	0.000

†The first row (italicized) reports only the African-American indicator variable estimated in equations that also included the variables in the associated columns but did not include the Black and Mulatto variables.
Other notes: Robust standard errors (allowing for correlation within wards, Stata®'s `cluster` option) are in parentheses. Probability weighting was used to adjust for undersampling of white-headed households. Significance is indicated by ^ $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

Table 3: Complexion-based wealth decompositions' treatment (T) and characteristics (C) effects

Male-headed	Advantaged group (H): / Disadvantaged (L):	Mulatto / Black		White / Mulatto		White / Black		White / African-American	
	mean log wealth: $\ln(w_H)$, $\ln(w_L)$	4.846, 3.876		5.888, 4.846		5.888, 3.876		5.888, 4.230	
	geometric mean wealth ($): w_H, w_L	127.2, 48.2		360.7, 127.2		360.7, 48.2		360.7, 68.7	
	raw differential: $R = \ln(w_H) - \ln(w_L)$	0.970		1.042		2.012		1.658	
	raw ratio (%): $w_L/w_H = e^{-R}$	38%		35%		13%		19%	
	Model From Table 2:	(3)	(4)	(3)	(4)	(3)	(4)	(3)	(4)
		treatment: T = R – C, (as % of R)				treatment: T = R – C, (as % of R)			
Assumed return to characteristics in absence of discrimination	Advantaged group, $C = b_H(x_H - x_L)$	0.444	0.402	0.888	0.431	1.354	0.900	1.184	0.729
		(46%)	(41%)	(85%)	(41%)	(67%)	(45%)	(71%)	(44%)
	Disadvantaged group, $C = b_L(x_H - x_L)$	0.391	0.344	0.686	0.404	0.664	0.308	0.701	0.378
		(40%)	(35%)	(66%)	(39%)	(33%)	(15%)	(42%)	(23%)
	Population-weighted, $C = b^*(x_H - x_L)$	**0.411**	**0.365**	**0.882**	**0.431**	**1.321**	**0.873**	**1.149**	**0.704**
		(42%)	**(38%)**	**(85%)**	**(41%)**	**(66%)**	**(43%)**	**(69%)**	**(42%)**
	Frequency of advantaged group	0.365		0.972		0.953		0.928	
	W_H/W_L, estimated wealth due to characteristics ratio = $e^C = e^{R-T}$	1.75	1.83	1.17	1.84	2.00	3.13	1.66	2.60
	$D = e^T - 1$ — see notes	51%	44%	142%	54%	275%	139%	215%	102%

Female-headed	Advantaged (H): / Disadvantaged (L):	Mulatto / Black		White / Mulatto		White / Black		White / African-American	
	mean log wealth: $\ln(w_H)$, $\ln(w_L)$	4.439, 3.450		5.538, 4.439		5.538, 3.450		5.538, 3.869	
	geometric mean wealth ($): w_H, w_L	84.7, 31.5		254.2, 84.7		254.2, 31.5		254.2, 47.9	
	raw differential: $R = \ln(w_H) - \ln(w_L)$	0.989		1.099		2.088		1.669	
	raw ratio (%): $w_L/w_H = e^{-R}$	37%		33%		12%		19%	
	Model from Table 2:	(3)	(4)	(3)	(4)	(3)	(4)	(3)	(4)
		treatment: T = R – C, (as % of R)				treatment: T = R – C, (as % of R)			
Assumed return to characteristics in absence of discrimination	Advantaged group, $C = b_H(x_H - x_L)$	0.422	0.380	0.761	0.716	1.325	1.245	1.086	1.021
		(43%)	(38%)	(69%)	(65%)	(63%)	(60%)	(65%)	(61%)
	Disadvantaged group, $C = b_L(x_H - x_L)$	0.421	0.387	0.587	0.475	1.122	1.043	0.833	0.729
		(43%)	(39%)	(53%)	(43%)	(54%)	(50%)	(50%)	(44%)
	Population-weighted, $C = b^*(x_H - x_L)$	**0.421**	**0.384**	**0.739**	**0.686**	**1.291**	**1.212**	**1.022**	**0.947**
		(43%)	**(39%)**	**(67%)**	**(62%)**	**(62%)**	**(58%)**	**(61%)**	**(57%)**
	Frequency of advantaged group	0.423		0.874		0.836		0.746	
	W_H/W_L, estimated wealth due to characteristics ratio = $e^C = e^{R-T}$	1.76	1.83	1.43	1.51	2.22	2.40	1.91	2.06
	$D = e^T - 1$ — see notes	52%	47%	109%	99%	264%	236%	178%	158%

Notes: Calculated from regressions specified as those reported in columns 3 and 4 of Table 2, but run separately for each color and gender subpopulation. Oaxaca's (1973) adaptation of Becker's (1971) discrimination coefficient, $D = ((w_H/w_L)-(w_H/w_L)) / (w_H/w_L) = e^T - 1$, is the increase in wealth due to unmeasured differences, using the boldface (population-weighted) counterfactual estimate of the treatment residual, $C = b^*(x_H - x_L)$.