# Separate and Unequal in the Labor Market: Human Capital and the Jim Crow Wage Gap

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**Abstract**

We examine the role of taste-based discrimination in supporting racial wage gaps using the racially charged Jim Crow South as a case study. Combining earnings data from the 1940 Census with newly transcribed local school quality metrics, we estimate that 70% of unconditional racial wage differences were attributable to observable human capital disparities. The conditional gap is reduced further after incorporating estimated ability, and the remaining 8 to 9 log point difference is comparable to estimates from the end of the century. A counterfactual "separate but equal" school quality standard could have reduced wage inequalities by 66 to 77 percent.

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# 1 Introduction

The American labor market has long exhibited a sizable gap in wages awarded to black and white workers, motivating a large literature devoted to disentangling the role of human capital, or "pre-market," factors from more structural labor market issues. In doing so, and with many caveats, the literature identifies the portion of the gap unattributable to differences in human capital and worker productivity as the upper limit of discrimination.[1] Conclusions from this literature have important policy implications. If pre-market differences are largely responsible for pay differences, appropriate policy solutions should prioritize disparities in human capital accumulation over labor market interventions.

Racial wage separation based on taste-based discrimination has an important theoretical prerequisite: it may only exist if there is a critical mass of prejudiced or discriminatory employers in the labor market (Becker, 1957). Otherwise, black workers will sort into non-discriminatory firms, and no wage gap can be sustained in equilibrium. This theoretical prerequisite, coupled with evidence that racially discriminatory views in the United States have declined over time, has given rise to a literature highlighting the idea that discrimination plays a smaller role in the black-white wage gap than it once did (Fryer, 2011).[2]

To date, available data linking earnings to measures of human capital accumulation have limited analyses of the conditional wage gap to the post-1960 period when the Civil Rights Act (1964) was newly in force. We utilize recently-released data from 1940 U.S. census returns to examine the conditional black-white wage gap at a much earlier point in time, well before employers were expressly prohibited from discriminating on the basis of race. Additionally, we focus on the Jim Crow South, a plausible setting for the critical mass of discriminating employers necessary to sustain taste-based racial discrimination. The underlying premise of taste-based discrimination is the idea that white employers incur an additional cost from hiring a black employee, independent

[1] See Lazear (1991); Oettinger (1996); Darity Jr. & Mason (1998); Altonji & Pierret (2001); Lang & Manove (2011). Also see Lang & Lehmann (2012) for a more complete survey of the racial discrimination literature. We join Lang & Manove (2011) and others in interpreting the conditional black-white wage gap with care. In the absence of experimental variation in the race of employees or potential employees (Bertrand & Mullainathan, 2004), the conditional wage gap is consistent with racial discrimination and omitted variables.

[2] See Lang & Lehmann (2012), Figure 3, for evidence of a decline in prejudice measures after 1956. The decline continues through the racially charged 1960s. To our knowledge, no data for 1940 exist, and no regionally subdivided data are available either.

of his or her productivity. Prejudicial attitudes were not limited to the South in 1940, but the unique force of social, political, and legal constructs that blocked any meaningful integration of blacks and whites in Southern schools, workplaces, and public life is indicative of such a cost.

The unconditional difference between black and white wages in 1940 was a substantial 53 log points for young, employed Southern men. But gaps in human capital were also substantial. Black and white Southerners at this time were educated in "separate but equal" schools where the *de facto* outcome was anything but equal. As an example, in the South in 1935, black teacher salaries were 39% lower than white teacher salaries and black school years were 14% shorter than their white peers. Educational attainment differed greatly by race as well. Nationwide, 47 percent of adult whites had at least a $9^{th}$ grade education in 1940, versus one in five adult blacks. The disparity was greater in the South, where whites were three times as likely as blacks to have at least a $9^{th}$ grade education.[3]

Census respondents' years of completed schooling have always been an available human capital metric in the 1940 census. But detailed quality of schooling metrics have heretofore been missing from analyses of the wage gap prior to the civil rights movement. We generate a new panel of county-by-race school quality statistics for each year between 1920 and 1940 for ten Southern states and assign young men in the 1940 public use microsample (Ruggles et al., 2010) a school quality metric specific to their race, age, and probable county of education, relying on newly transcribed data describing county of residence and recent mobility as of the 1940 census. In addition to years of schooling and the quality of available schooling, we utilize a known oddity in the World War II enlistment records to impute Army General Classification Test (AGCT) scores for Southern males in 1940 as a third measure of human capital comparable to Armed Forces Qualification Test (AFQT) scores utilized in more recent work. With this battery of human capital metrics, we estimate conditional black-white wage gaps among young male workers.

Remarkably, we find that at this place and time there was a 16 percent conditional wage gap after controlling for years of schooling and school quality and an 8 to 9 percent gap after controlling for imputed AGCT. These estimated wage gaps are no greater than those estimated for the post-Civil Rights era. Results are robust to several variations of our main empirical strategy, including specifications with controls for unobserved state-level or county-level heterogeneity. We conduct

[3] Authors' calculations using Ruggles et al. (2010).

a simple counterfactual exercise where school quality is equalized across races within each county and years of schooling are allowed to respond to the increase in quality. We find that a binding separate-but-equal mandate would have reduced the wage gap by 66 to 77%.

We conclude that the discriminatory preferences of white Southerners were powerful in limiting black public school quality and reducing the wages of blacks through the human capital channel, but were far less powerful in affecting wages through labor market discrimination itself. We believe our results provide guidance for evaluating the role of labor market discrimination in explaining today's wage gap. If conditional wage gaps exhibit little difference between 1940 and 2000, this calls into question interpretations of $20^{th}$ century black relative wage growth as a manifestation of a decline in labor market discrimination *per se*. It would seem, rather, that the narrowing wage gap that characterized the late $20^{th}$ century is attributable largely or wholly to gains in the quality of blacks' pre-market human capital, which in turn is predominantly attributable to improvements in the quality of public schooling available to black youths.

# 2 Literature on the Black-White Wage Gap

Wage differentials may emerge for a number of reasons; competing candidates can be summarized as differences in the pre-market human capital accumulation of workers, differences in on-the-job productivity conditional on human capital, and labor market discrimination that results in differential compensation for equivalently productive individuals. Much of the literature on the black-white wage gap can be viewed as a horse race between these competing explanations. And because discrimination itself is rarely observable, the literature tends to label any remaining gap in black-white wages, after controlling for observable measures of human capital and productivity, as an estimate of the upper bound of racial discrimination.

Such strategies depend critically on the ability of the empiricist to accurately measure human capital and on-the-job productivity. In the absence of more direct measures of productivity and ability, workers' years of schooling, school quality, and labor market experience are the best available human capital proxies. Both educational attainment and school quality have been shown to drive large portions of differences in earnings across races. The measured impact of years of schooling on labor market outcomes is consistently positive and significant, and the steady increase in black educational attainment is consequently viewed as an important part of the narrowing of the wage gap after 1940.[4]

The record for school quality is more varied. A consistent feature of the literature is that estimated returns to school quality differ depending on whether data are at the state, district, or school level. *Ex ante*, a primary limitation of state-level approaches would seem to be aggregation (attenuation) bias discussed explicitly in Morgenstern (1973). More granular school quality indices, however, do not necessarily increase the estimated returns to schooling (Betts, 2010).[5] Further, the possibility of non-linearities in the returns to school quality imply that assigning individuals the average level of school quality for the state in which they were educated will generate a bias in the estimated returns, although the direction of bias is ambiguous. As a result, if the goal is to attribute some portion of any remaining gap in earnings after controlling for human capital to labor market discrimination, a disaggregated measure of human capital is of tantamount importance.

Margo (1986), Welch (1974), Smith (1984), and Smith & Welch (1986) were perhaps the first to highlight the rising quality of black education as an important driver of gains in the black-white earnings ratio, but to date, direct tests of the impact of improving black school quality have relied on state-level data. Ashenfelter et al. (2006) estimate the importance of school quality in determining the difference in black and white earnings in 1970. They assign individual school quality measures based on state of birth and find that differences in state measures of school quality are responsible for between 0.019 and 0.085 log points of wage difference for cohorts born between 1920 and 1939. Card & Krueger (1992a) and Card & Krueger (1992b) find a differential return to schooling across races which, in turn, can be attributed directly to differences in state school quality metrics. They conclude that 20 percent of the narrowing of the black/white earnings gap between cohorts born in the 1920s and those born in the 1940s (measured between 1960 and 1980) is attributable to rising school quality.[6] Nechyba (1990) estimates that improvements in teacher

[4] See Smith (1984), Smith & Welch (1986), Smith & Welch (1989).

[5] "Most of the studies that find no link or a weak link between school inputs and student outcomes measure school inputs at the level of the actual school attended; studies that do find a strong effect typically measure school resources at the level of the state."

[6] Several others have quantified the impact of school quality on earnings, *per se*, without a particular focus on the black-white gap. For our period, the most relevant of these are Johnson & Stafford (1973), Morgan & Sirageldin (1968), and Morgenstern (1973). Each use state-level data on school quality. Wachtel (1975) and Wachtel (1976) document positive returns to school quality for a selected sample of individuals likely restricted only to whites. See Betts (2010) for a summary of the literature on the effect of school quality on earnings.

salaries, again measured at the state level, can explain roughly half of the rise in the ratio of black to white earnings between 1950 and 1980.[7] Link & Ratledge (1975) and Link et al. (1976) utilize a district-level measure of school quality and find that "increases in educational quality have been an important cause of the relative gains in black earnings" over the 1960s. At the same time, they infer a negligible role for years of schooling in explaining the racial earnings convergence.

The literature on later decades of the 20th century has the advantage of access not only to measures of schooling and school quality but also scores from standardized tests taken while the individuals are still in school. An inherent weakness in the use of educational attainment and school quality to proxy human capital is the inability to measure ability directly. The benefit of standardized scores is that they, arguably, encapsulate a single measure of skill, one that can be shown to be highly correlated with eventual labor market outcomes. Even with this additional measure of ability, the literature still highlights a strong link between school quality and academic achievement.

In Table 1, we present a limited review of papers measuring the contributions of schooling, school quality, experience, ability, and family background to the overall wage gap. The second column of the table lists the data source and cohort used in each analysis. The third indicates which human capital variables are included in the study and the fourth indicates what percentage of the overall gap they explain. The fifth column reports the log conditional black-white gap. The overwhelming indication from this literature is that pre-market factors matter for determining wage differences, and, in many cases, the wage gap potentially attributable to labor market discrimination is minimal after controlling for these factors.

On the theoretical side, a number of papers discuss the prerequisites necessary to achieve an observable wage gap attributable to discrimination. For our purposes, two points are of critical importance. We posit that a wage gap attributable to taste-based discrimination has both an extensive and an intensive margin. On the extensive margin, the presence of a racially separable wage equilibrium depends on the number and size of discriminatory employers relative to the group being discriminated against. On the intensive margin, the size of the wage gap is a function of the disutility of employing workers in this group. Thus the racial wage gap is increasing in the

[7] See also Rizzuto & Wachtel (1980) for an estimate of the social rate of return to investments in school quality for whites and blacks separately in the 1960 and 1970 census.

prejudicial preferences of the general population because it both increases the number of discriminatory employers and increases the disutility of employing black workers.[8] We contend that the U.S. South of 1940, more than any other time and place studied to date, sustained prejudicial attitudes and mores that satisfied the necessary conditions for taste-based discrimination to manifest as otherwise-unexplained black-white wage gaps.

# 3 Data

As Table 1 indicates, the wage gap literature has, to date, focused on the post-Civil Rights period when overt racial discrimination in public and private sectors was prohibited. In order to estimate unexplained racial wage differences earlier in time, we link several measures of county-by-race school quality, as well as estimated AGCT scores, to individual wages reported by young men (aged 18-25) in the 1940 census.

The public-use sample of the 1940 U.S. Census (Ruggles et al. (2010)) contains the first available micro-level data on wages for a cross-section of the U.S. population.[9] Prior to 1940, labor market measures include occupation and industry of employment, but no individual earnings data.

In 1940, census enumerators recorded labor market wages but not non-wage income. Consequently, the self-employed (including a substantial number of farmers and farm tenants) do not report income in this sample. As such, we exclude individuals without recorded earnings from main results.[10] Occupational score results are robust to including these individuals, as we show in Section 7, and in a supplemental analysis discussed in the Appendix, we analyze the impact of human capital on farming employment *per se*. Another drawback of the 1940 census is that we cannot adequately measure payments-in-kind as part of wages. This limitation is problematic given that there is substantial agricultural employment in this time period, even after omitting farm owners and tenants without reported earnings. Payments-in-kind may have been more prevalent

[8] These implications are true both in Becker's original framework and in adaptations to a search model as discussed in Lang & Lehmann (2012).

[9] There are precious few sources for labor market earnings other than the Census prior to the advent of the National Longitudinal Survey of Youth. One notable example is the NBER Thorndike-Hagen sample exploited in Wachtel (1975) and Wachtel (1976), but the sample is believed to be limited to white males.

[10] For our purposes the limitation is innocuous as we are primarily interested in the wage impact of prejudiced employers and not the implications of racial discrimination for self-employed earnings. In addition, limited earnings of black farmers reflect discrimination as well as disadvantages beyond the scope of our inquiry, such as obstacles to landowning.

for agricultural workers, and in particular, for black agricultural workers. Robustness checks described in Section 7 show that findings are insensitive to the exclusion of respondents with more than $50 in non-wage income.

In addition to earnings measures in the census, we generate an occupational score for each individual in the sample based on their 1940 reported occupation. The occupation score variable provided by IPUMS is the average 1950 wage for each reported occupation in the 1940 manuscripts, mapped to a three-digit code. Since this score is measured across races, ages, genders, and geographies, it might be affected by discrimination via race-dependent occupational or regional sorting. With this in mind, we compute occupational scores using the average income reported over all *white* males in our 10 Southern states employed in a given occupation in the 1950 U.S. Census returns.[11]

The 1940 census contains measures of the highest grade completed by each individual. Census enumerators were instructed to record the "highest full grade that the person has successfully finished." Despite this instruction, there is some concern that the question was interpreted differently across races, especially for (predominantly older) black individuals who were educated in ungraded schools (Margo, 1986). In that case, the census instructions directed enumerators to record the number of years the person was in school.[12] Our focus on younger men in the data avoids much of this form of mismeasurement. And to the extent that there remains overstatement of "highest full grade" for men with an ungraded education (perhaps because grades were typically completed in more than one year), it will serve to bias *downwards* the contribution of differences in human capital to the black-white earnings gap and overstate the role of labor market discrimination.[13] We identify working men aged 18 to 25 from the public-use sample who reside in one of ten Southern states for which we have education quality data (described below): Alabama, Arkansas, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, and Texas. The age controls are designed to ensure that we can more accurately assign individuals to their county of schooling and abstract away differential on-the-job training or experience.

Although individual-level data on earnings have been available from this census for a number

[11] Again, we rely on Ruggles et al. (2010).

[12] "[I]f this cannot readily be determined, [enter] the number of years the persons attended school."

[13] See Margo (1986) for evidence that using respondents' highest grade completed as a proxy for educational attainment understates the contribution of human capital to the 1930-1970 decline in the wage gap.

of years, the specific (county) location of individuals within this sample is newly-released. This location information allows us to link individuals to their county of residence and, importantly, to their probable county of schooling for the young cohort of men that is the focus of our analysis. The ability to identify the probable county of education for individuals allows us to assign a race-specific school quality measure more proximate to the actual education experience of individuals than previously possible. Aggregation bias as well as the presence of non-linearities in the return to school quality imply that the ability to disaggregate from state-level measures is critically important for identification.

To measure the quality of schools available to each individual in our sample, we utilize transcribed county-level measures of race-specific school quality in the years leading up to 1940 for ten Southern states. Over much of the $20^{th}$ century, each U.S. state's department of education or equivalent office published an annual or biennial report containing statistics on revenues and expenditures, disaggregated by county and by race. With the exception of a small number of biennial editions, these education reports allow us to measure at least one race-specific school quality statistic for each year for each county. The data and data collection process are described more fully in the Appendix.

The school quality data can be matched to each individual in the Census data after making some assumptions about where individuals lived when they were young. In 1940, census takers inquired as to the location of respondents five years prior, in 1935. We assume this 1935 location is the county of residence during an individual's potential schooling years. Because our analysis focuses on individuals 25 and under in 1940, this amounts to assuming that individuals aged 13 to 20 in 1935 are in the same county as during their school years.[14] We undertake sensitivity tests on this assumption in Section 7.

Annual education statistics collected by county and race are used to construct indices of white

[14]To gauge the mismeasurement in this assignment, we look to a sample of death records from North Carolina generously shared with us by John Parman. The records include both county of birth and county of death for deaths reported prior to 1976. We examine a subset of males who died between the ages of 7 and 20 and were born in the relevant years (1914 to 1923). For these individuals, we find that 30% lived in a state other than their home state by age 7 as indicated on their death certificate and 38% did the same by age 20. (The numbers are 26% and 42% using a 3-year moving average.) Thus, our methodology will falsely identify the county of education for approximately 8-16% of individuals in the sample. The unfortunate assumption in this, and the only one we can reasonably make, is that individuals only move once so that the number who have relocated from their birth county by age 20 less the number who did the same by age 7 captures all migration.

and black school quality that vary by census respondents' cohort, 1935 county, and race. Depending on the year and state, school quality indices are composed of one or more of the following eight metrics: expenditures per enrolled pupil, expenditures per pupil in average daily attendance (ADA), teachers per enrolled pupil, teachers per pupil in ADA, certified teachers per enrolled pupil, certified teachers per pupil in ADA, term length, and average teacher salary. Reported measures vary by state and year, but definitions are consistent across races within a state-year (and county-year). States had some leeway in the metrics they chose to document, and many changed the format and set of data reported over time. But, importantly, if a white-specific metric is reported in a given year, a corresponding black-specific metric is as well. An inventory of the school quality statistics available for each state and year can be found in the Appendix.

Given the varying availability of quality metrics, selecting a single metric to proxy overall school quality is an untenable strategy. Instead, for each quality measure, we calculate a within-year Z-score relative to all other counties in the data who report the same quality metric. The index computation is as follows

$$Z_{jct} = \frac{M_{jctr} - \bar{M}_{jt}}{\sigma_{jt}}$$

where $M_{jct}$ is the value of metric $j$ (e.g., teachers per enrolled pupil) in county $c$ in year $t$ for race $r$. $\bar{M}_{jt}$ and $\sigma_{jt}$ are the mean and standard deviation of measure $j$ across all county-race observations reporting the same metric in year $t$. We emphasize that the conversion is relative to all county years reporting the same statistic and is across, not within, race.[15] $Z_{jct}$ converts statistic $M_{jct}$ to a scale with mean zero and unit standard deviation that can be compared across counties and races in year $t$. In state-years where more than one measure of school quality are reported, we use the average value of $Z_{jct}$ across all available $j's$.

$$Q_{ct} = \sum_{j=1}^{J} Z_{jct}/J$$

where $J$ is the total number of available metrics for county $c$ in year $t$.[16] The exception is when a single metric is available with both enrolled pupil and pupils in ADA as a denominator. In this case, we use only the measure per enrolled pupil.

[15]See Section 7 for results using pooled Z-scores across counties, races *and* cohorts.

[16]Robustness checks described in Section 7 control for the quantity of school data: i.e., $J$ and $J^2$. Additionally, results are not sensitive to the inclusion of eight controls measuring the frequency with which each metric was reported during an individual's potential years of schooling.

The index achieves two goals. First, it allows us to aggregate information about school quality across quality metrics that differ in their distribution and coverage. Second, by calculating a within-year Z-score, we reduce the influence of cyclical variation on school quality measures. This is especially important given variation in school funding over time in the 1930s attributable to changes in the macroeconomic environment. $Q_{ct}$ school quality measures are relative; the measure for each county is standardized relative to all other counties in the same year.[17]

The education panel and 1940 individual-level labor market data are merged so that school quality measures can be inferred for each individual, given our assumption about their 1935 location. In order to assign school quality measures to individuals, we must make a final assumption. We observe individuals' ages and years of schooling in 1940, but not the precise years of enrollment. We assume that all individuals are "at risk" for school enrollment between the ages of 7 and 18 and measure average school quality across those years.[18] As an example, an individual observed in the 1940 Census who is 25 years of age was a potential enrollee from the 1922/1923 school year through 1933/1934. For each individual, then, we assign a school quality measure which is the average of the school quality index in the county where he resided in 1935 over the years he could have been in school. Years for which there are missing data on school quality are excluded from both the numerator and denominator of $Q_{ct}$.[19] Therefore, the school quality metric varies across cohorts and counties and is best thought of as the typical quality of public education available to each respondent when they were ages 7 to 18.

The data linkage generates a base sample of 11,394 men aged 18-25 who report earnings, reside in 10 Southern states in 1940, report a discernible residence in 1935 for which school quality metrics are available, and report race of either "white" or "black" to the census enumerator. A critical issue for the empirical strategy described below is whether there is enough overlap in the

[17]It is also possible that cyclical fluctuations in overall school funding levels, which would be captured by age fixed effects, had an effect on labor market outcomes differently by race. If so, these effects will remain in the unexplained portion of the wage gap.

[18]In practice, across-county variation in school quality measures are far more substantial than-within county differences across cohorts and attendance years, making the county of schooling assignment more important than the years of schooling one.

[19]An alternative approach is to assign school quality for years we infer individuals were actually in school. The issue is that age-in-grade distributions varied wildly so that individuals of a given age and highest grade attended cannot be credibly assigned to specific years in school. Further, because there is evidence that school quality was a determinant of enrollment, the relationship is endogenous in any case and measuring school quality over years individuals were at-risk of being enrolled breaks this endogeneity.

school quality index of black and white respondents to justify a counterfactual exercise where blacks and whites were exposed to similar schools. Non-linearities in the returns to education are included in the analysis, but a lack of common support across black and white school quality indices would hinder our ability to infer counterfactual black outcomes under a true "separate but equal" schooling system. Figure 1 shows kernel density functions for black and white school quality indices and illustrates the distribution of black and white educational attainment. In both cases, there is considerable overlap, lending support to the empirical strategy described in the next section. In Section 7, we show that restricting our analysis to the common support of these two human capital measures has modest consequences for our estimates.

# 4 Empirical Strategy and Results

Table 2 contains summary statistics of labor market outcomes and human capital investments. Columns 1 and 2 of the table give average values for all men in the sample.[20] As noted before, a large number of men in the sample have no available income data and columns 3 and 4 give the average value of these same characteristics for the sample used in the estimation. Due to the loss of agricultural workers in the baseline sample, occupational scores for this selected sample are slightly higher, as are measures of school quality and manufacturing value-added in their county of residence. In addition, the estimation sample is more urban than the underlying population. Section 4.1 presents baseline results for this working sample and Section 4.2 examines within-occupation wage gaps. In the Appendix, we estimate the impact of pre-market factors on employment *per se*, agricultural employment, and New Deal work relief employment.

## 4.1 Baseline Results

Among the available labor market measures, it is clear that racial differences in labor force participation, employment rates, and average weeks worked from Table 2 are relatively small. Labor market wages, on the other hand, differ substantially by race with blacks trailing whites by 51 log points in the full sample. Despite small differences in weeks worked, and in order to be consistent with the existing literature, we use a weekly wage as the main outcome of interest. No measure

[20] The universe is all black and white men from the 1940 IPUMS sample aged 18 to 25 with a (discernible) county and state of residence in 1935 within our 10-state school quality region.

of hours worked is available. Weekly wages in the sample differed by 53 log points in the broad sample.

Prior literature has shown that occupational sorting was an important part of black-white labor market inequalities in this period, a statement confirmed by differences in our inferred occupational score. In Table 2, the black-white gap in average occupational score is 36 log points.

To evalute the impact of human capital measures on labor market outcomes, we estimate Equation 1 below with and without educational attainment and school quality included in the vector $X_{icra}$.[21] In addition, we include age fixed effects and proxies for the economic conditions of an individual's county of residence (percent rural, per capita manufacturing value, per capita retail sales and per-capita crop value) from the bottom panel of Table 2. Differences in the black-white gap, $\delta$, across these specifications reflect the ability of observable characteristics to account for racial differences in outcomes.

The estimating equation is:

$$lnY_{icra} = \alpha + \delta BLACK_i + \beta X_{icra} + \epsilon_{icra} \tag{1}$$

where $Y_{icra}$ is the labor market outcome of interest for individual $i$ educated in county $c$ residing currently in county $r$ of age $a$. In this setting, $Y_{icra}$ measures one of four labor market outcomes: labor market wages, weeks worked, weekly wage, or occupational score. $BLACK_i$ is a binary indicator, and the estimated wage gap will be negative if black respondents have lower labor market outcomes than whites. $X_{icra}$ is a vector of county of residence covariates, county of schooling school quality, years of schooling, and age fixed effects. Identifying variation in school quality comes from within-cohort, across-county and across-race differences in school quality. The primary threat to the internal validity of results is that of a classic omitted variable, correlated with both human capital and earnings in a way that falsely attributes labor market gaps to human capital or (implicitly) to discrimination. We describe a number of robustness checks to address this threat in Sections 5 and 7, including the addition of county fixed effects to Equation 1.[22]

[21] An alternative strategy is to use an Oaxaca-Blinder decomposition. See Elder et al. (2010) for a discussion of the relative attractiveness of an OLS regression framework with group indicators. In short, Oaxaca-style decompositions may overstate the contribution of observables to inter-group wage gaps.

[22] Essentially, this normalizes school quality within counties and estimates the impact of within-county changes in school quality on earnings. This is not our preferred specification because the idea that black and white school quality had areas of common support within counties has less merit than the cross-sectional analogue. These reservations notwithstanding, we find that conclusions are robust to county-of-residence or county-of-schooling fixed effects.

When controls for age, county covariates, and human capital are excluded from the estimation of Equation 1, the parameter $\delta$ measures an unadjusted gap in earnings or occupational scores across races, or the difference in means across racial groups, as reported in Table 3. Column 1 indicates that the unadjusted racial gap in weekly wages is 52.9 log points among the 11,394 individuals in the sample. The weekly wage gap reflects the combination of an unadjusted income gap of 51.3 log points (Column 7) and a weeks worked gap of 1.6 (log) weeks favoring blacks (Column 10), leading to a larger weekly wage gap than income gap. The two groups have an unconditional occupational score gap of 35.8 log points (Column 4).[23]

We then calculate a conditional wage gap, first conditioning only on age fixed effects and characteristics of the individual's county of residence which may have impacted wage levels. This lowers the racial gap in weekly wages, income and occupational scores, albeit very slightly, while raising the gap in weeks worked (Columns 2, 5, 8, and 11 of Table 3).

Finally, we include third-degree polynomial functions of school attainment and quality variables in $X_{icra}$ to capture non-linearities in the impact of school quality and years of schooling on labor market outcomes. Age fixed effects ensure that identification of the effects of schooling comes from within-cohort variation in school quality and educational attainment across races and counties in the South.

The addition of these human capital controls dramatically reduces the racial gap in labor market outcomes. Column 3 of Table 3 indicates that after controlling for these measures, the racial gap in weekly wages falls to 15.9 log points, 70 percent lower than the unadjusted gap. The conditional gap in weeks worked (Column 12) significantly favors black men, such that the conditional income gap (Column 9) is 82 percent lower than the unconditional gap. For occupational scores (Column 6), the addition of human capital controls reduces the gap to 16 log points, less than half of the original gap.

School quality and individual attainment are highly correlated, and it is not clear from $\delta$ estimates alone which measure of human capital is primarily responsible for attenuating the black-white earnings gap. Adding covariates sequentially is one approach to disentangling the contribu-

[23] We limit the occupational score analysis to those who also report wages to keep samples consistent, although they differ somewhat due to individuals who do not report an occupation, but do report wages. We relax the sample constraint in the robustness tests at the end of the paper to include all occupation reporters and show no change in the adjusted score gap.

tion of school quality from that of time in school. But Gelbach (2009) shows that this procedure can lead to misleading results, and that such decompositions depend on the order in which controls are added to the model. We use Gelbach's decomposition framework to estimate the relative contribution of years of schooling and school quality to wages and occupational scores.[24] In Table 3, the contribution of each term (in log points) is displayed beneath the $\delta$ coefficient in columns 3, 6, 9 and 12. Differences in years of schooling account for 17.2 log points of the difference between black and white annual income while differences in school quality account for somewhat less, 16.5 log points. The results are similar for income in column 9. For occupation scores, the contributions of years of schooling and school quality are 13.3 and 5.5 log points, respectively.

We conclude that differences in human capital measures account for the majority of the black-white wage and occupational score gap in 1940. Further, the remaining coefficients on $\delta$, 15.9 log points of weekly wages and 16 occupational score points, are not dissimilar from the range of conditional differences displayed in Table 1.

## 4.2 Within-Occupation Wage Gap Results

A conditional wage gap of 15.9 log points and an occupational score gap of 16.0 log points begs the question of whether occupational sorting itself was the primary driver of the black-white wage gap (Higgs, 1977). Occupations, used to calculate occupational scores, are reported to census enumerators in 1940 as qualitative values but have since been converted to standardized 1950-based codes by IPUMS.[25] The codes are at the 3-digit level. Because blacks did not participate in a number of occupations in 1940, we roll the categorization up to the 1-digit level giving us nine occupation categories for the fixed effects analysis.[26]

Figure 2 plots the distribution of black and white workers across broad occupation categories, along with average log income for each category. There are substantial differences in the distribution across occupation categories, and the income measures layered on the histogram indicate,

[24] Gelbach's 2009 procedure stems from the identity $\hat{\beta}_1^{base} = \hat{\beta}_1^{full}(X_1'X_1)^{-1}X_1'X_2\hat{\beta}_2$, where $\hat{\beta}_1^{base}$ ($\hat{\beta}_1^{full}$) is a vector of $X_1$ coefficients in the limited (fully conditioned) model and $\hat{\beta}_2$ is a vector of $X_2$ coefficients. In our context, $X_1$ is a race indicator, and $X_2$ factors are years of schooling and school quality, two contributing factors to race-based differences in labor outcomes.

[25] According to Higgs (1977), racially dependent sorting across high-wage and low-wage firms is another likely source of the overall black-white gap. We do not observe respondents' employers in the 1940 census.

[26] The categories are managers/officials/proprietors, clerical and kindred, sales workers, craftsmen, operatives, service workers, agricultural laborers, and general laborers.

unsurprisingly, that wages are higher in the occupations where whites are disproportionately represented.

In a regression framework, we calculate the conditional within-occupation racial gap in weekly wages, income, and weeks worked by introducing an occupation category fixed effect to Equation 1.[27] Table 4 presents the resulting estimates of the $\delta$ coefficient under this specification. Consistent with results in Table 3, we find that the conditional, within-occupation weekly wage gap is reduced to 11.5 log points and that the conditional annual income gap is an insignificant 1.8 log points. The difference between Columns 3 and 6 is reconciled by the conditional gap in weeks worked, which expands with controls for occupational fixed effects. Within occupations, black and white men of equivalent schooling had similar annual earnings, but given that blacks worked 9.7 percent more over the course of the year, the weekly earnings gap remained significant at 11.5 percent. These findings support the idea that discrimination manifested in part via occupational sorting, but not so much as to render the labor market non-discriminatory within occupations.

# 5 Additional Tests for Discrimination

Interpreting $\delta$ in Equation 1 as labor market discrimination requires that we assess the risk from omitted variable bias. Leading candidates for omitted factors in this setting are unobserved ability and local race relations. If blacks in our sample, conditional on observable human capital, have systematically different expected ability, $\delta$ is biased. Second, collinearity in racially disparate public goods provision and labor market discrimination may serve to cloud the interpretation of $\delta$. We address each in turn.

## 5.1 Unobserved Ability

A common concern in the wage gap literature is the issue of unobserved ability that would have affected an individual's labor force productivity and is correlated with the regressors in Equation 1. If average ability, conditional on observable human capital, differs across blacks and whites, the conditional wage gap does not accurately reflect the depth of labor market discrimination. The direction of the bias is ambiguous. In modern data, Lang & Manove (2011) show that controls for AFQT scores alongside years of schooling increase estimates of the pay gap because blacks tend

[27] Results do not differ in any meaningful way if we use 3-digit code fixed effects.

to have more years of schooling for a given AFQT-measured ability. Omitting ability measures – as we do in Equation 1 – may therefore understate the conditional pay gap. In our context, it may well have been the case that only the highest ability black students would have achieved higher levels of educational attainment given the pervasive impediments to attendance. On the other hand, it may be that blacks had lower total human capital than observationally equivalent white counterparts due to differences in the intergenerational transmission of human capital or other pre-market investments outside of schooling. If so, we overstate the role of labor market discrimination in Table 3.

One potential proxy for unobserved ability is parental education. Lang & Manove (2011) find that family background controls are important parts of the wage model using NLSY data. In the 1940 census, however, this information is only available for respondents who were still living with their parents (57 percent of the analytical sample of 18 to 25-year old males). Adding controls for this incomplete measure of family background along with an indicator for missing parental education yields very little change to our main results, as shown in the second column of Table 5. The conditional weekly wage gap rises from 15.9 log points to 16.7 log points, and the conditional income gap rises from 9.0 log points to 10.8 log points.

A second proxy for unobserved ability used in the modern literature is performance on a standardized exam. Several studies examining features of the wage gap in the NLSY panel utilize AFQT scores as pre-market proxies for ability (See Table 1 for examples). Through a historical fluke, standardized test scores are available for a subset of World War II enlistees for several weeks in 1943.[28]

Unfortunately, directly linking our 1940 IPUMS sample of males to the WWII enlistment data for this window of time, matching on name and county of residence, generates too small a sample for meaningful analysis. Instead, we utilize the fact that WWII records include a measure of highest grade completed, much like the 1940 census sample, as well as an exact county and state of residence at enlistment to assign human capital measures to each individual in the WWII records where AGCT is recorded. For these WWII enlistees, then, we have a measure of race, educational

[28]For a limited time, WWII enlistment cards contain AGCT scores in place of weight. We know of no evidence that this test was racially unbiased. Enlistment in the armed forces, however, was conditional on a minimum literacy standard so that the test results would not have been racially biased for literacy reason. Like the modern AGCT, the test appears to measure acquired ability rather than inherent cognition (Zeidner & Drucker, 1983). See Troesken et al. (2012) for additional details.

attainment, school quality, and ability.[29]

We can use this sample to impute AGCT scores for men in the 1940 census conditioning on educational attainment, school quality, and race. We wish to remain as agnostic as possible about the relationship between race, school quality, attainment, and AGCT scores, recognizing that black/white differences in AGCT conditional on other human capital metrics may differ across the distribution of those metrics. We limit the age range of WWII enlistees to mirror that of our census sample.[30] We then exploit the fact that race and educational attainment are binary and categorical variables, respectively, and use two different methods to control for school quality, a continuous variable.

- Method 1: We subdivide school quality into 10 deciles and calculate average AGCT scores within each race/educational attainment/school quality decile bin. We assign an imputed AGCT score to each individual in the baseline sample accordingly, with the restriction that bins with fewer than 20 observations in the WWII data are thrown out.

- Method 2: We specify that AGCT is a function of a 5th-order polynomial in school quality within each race/educational attainment bin. We then use the parameter estimates to impute AGCT for the baseline sample.

With these two methods for imputing AGCT scores, we revisit Equation 1 with imputed AGCT as an additional control variable. Results for both weekly wages and gross income are located in Table 5. The estimates indicate that, controlling for estimated ability, the black-white wage gap falls substantially. The weekly wage gap falls to between 8.0 (Column 3) and 9.1 log points (Column 4), depending on the imputation method. The standard error also increases somewhat, and the result is a racial wage gap that is only weakly significant. For total income, the remaining gaps in columns 8 and 9 are negative but insignificant.

[29] The assumption that allows us to link school quality to individuals is that their county of residence at enlistment is the county where they were educated. This is a worse assumption than that employed in the main analysis due to migration between schooling and enlistment.

[30] Neal & Johnson (1996) are careful to limit the AGCT scores in their analysis to those taken prior to entrance in the labor market arguing that "[j]ob experience and post-secondary education surely enhance human capital and will therefore increase test scores. If discrimination limits access to these human capital investments, then postentry discrimination contaminates the test scores (p.873)." Because the AGCT test was reported at enlistment, the youngest age at which we observe this score is age 18 with a large mass of observations at age 19. Seventy-five percent of the individuals in the WWII enlistment records used for ability imputation are 20 and younger.

As a falsification exercise, we repeat the second imputation method for weight values from WWII enlistment records in the same months of 1942, when the weight field should have contained actual weight and not AGCT scores (Columns 5 and 10 of Table 5). The conditional wage gap rises slightly to 17.2 log points and the gross income gap to 10.0. In addition, weight is insignificantly predictive of wages in the estimation for 1942 but is positively, significantly correlated in 1943, confirming that the 1943 "weight" data do measure ability.

A final caveat on the imputation of AGCT scores is that the sample of individuals in the 1943 enlistment may be selected in some way. Selection would impact the estimate of $\delta$ only if selection differed across blacks and whites. We do observe higher educational attainment in the WWII enlistment records for both blacks and whites, perhaps due to the literacy restriction on enlistment. The difference in (log) average educational attainment between the enlistment records and the census records is larger for blacks than whites, indicating more positive selection on observables amongst blacks. Educational attainment is explicitly accounted for in the imputation, but if the same selection pattern is true for AGCT scores as well, then we overestimate the ability of blacks in the census sample and our estimates of $\delta$ in Table 5 are too high as a result.

## 5.2 Collinearity in Public and Labor Market Discrimination

A second issue that affects the interpretation of $\delta$ is the possibility that school quality measures embodied in $X_{icra}$ in Equation 1 are proxies for county race relations in general. If so, it is not at all surprising that including school quality in a wage equation goes a long way towards explaining the racial wage gap. Black Southerners could have experienced discrimination in both the labor market and in decisions that affected school quality. If those two factors are highly correlated, the human capital inputs available to blacks may simply be proxies for overall relations. If so, Table 3 results understate the role of labor market discrimination in explaining the earnings gap and overstate the role of human capital inputs, including school quality provision.[31]

To address this, we exploit the existence of inter-county migrants in our data - those who were likely educated in counties other than their county of residence in 1940. Focusing on these black migrants breaks the coexistence between school quality and labor market discrimination in each

[31]This is different from the argument that the South was just universally discriminating. If that were true, then variation in school quality within the black sample should no change the coefficient on RACE and the conditional gap would not be affected by human capital proxies.

individual's 1940 county of residence (although it adds the question of selection into migration), and we calculate a measure of the contribution of human capital in determining the wage gap outside of this correlation. If human capital measures are much less successful at explaining the earnings gap of new resident blacks relative to all resident whites (i.e., if the conditional gap is much larger between these two groups than between blacks and whites more broadly), then we would suspect that estimates in Table 3 are driven by local race relations more so than local school quality.

We omit from our sample all black individuals living and working in the same county in which they were educated, as evidenced by their 1935 location. Practically, this involves eliminating 90% of the black sample so that identification comes from a relatively small number of blacks. Migrating blacks had somewhat higher wages than their non-migrating peers, and this generates a lower baseline wage gap between blacks and whites as evidenced in Columns 1 (weekly wage) and 7 (income) of Table 6.[32]

The remainder of Table 6 repeats the analysis reported in Table 3 with non-migrating blacks excluded. Contrary to the expected results if collinearity between school quality and labor market discrimination was driving our estimates, Column 3 indicates that human capital controls result in a somewhat *lower* conditional pay gap between new resident blacks and all resident whites, not less. The conditional black-white gap in weekly wages falls to 13.7 log points and the conditional gap in earnings (Column 9) is negligible and insignificantly different from zero. Further, the contribution of school quality differences to the overall weekly wage gap is 30% (13.2/44.3 log points), similar to the effect identified in Table 3 (33%=16.5/49.6 log points).

We do find some evidence that the occupation score gap grows after eliminating non-migrant blacks (Column 6), from 16.0 to 23.6 log points. It appears that the forces, discrimination or otherwise, that served to push blacks into lower-paying occupations relative to observationally equivalent whites were stronger for blacks who were new to their place of residence.

Still, we take the income and wage results in Columns 3 and 9 of Table 6 to indicate that school quality measures are not simply serving as a proxy for local race relations in Equation 1. The fact that human capital controls result in a lower conditional pay gap when we limit the black population to inter-county migrants is itself an interesting conclusion, with the caveat that

[32] Summary statistics for the migrating sample are available in the Appendix.

migrating blacks are very small in number and perhaps positively selected, even after controlling for observable human capital.

# 6 Counterfactual Estimates

Our results thus far have indicated that the 1940 wage gap cannot, by in large, be attributed to rampant wage discrimination. Instead, observable human capital plays a large role in the wage gap between races. In this section, we examine how far a binding separate-but-equal school quality mandate would have gone to relieve income inequality in 1940.

The implicit counterfactual exercise in our baseline results calculates the wage gap in 1940 earnings if blacks had achieved the same level of education in schools of comparable quality to whites. Columns 3 and 9 of Table 3 indicate that the remaining gap would have been roughly 15.9 log points of weekly wages and 9.0 log earnings points.[33] Equalizing school quality alone would have resulted in gaps of 36.3 and 32.7 log points, respectively.[34]

There are two reasons that another counterfactual is more relevant to the time period in question. First, we have so far ignored differences in the geographic distribution of blacks and whites across the South. Assigning all blacks the average education quality of whites presumes perfect mobility of households and education funds, when in reality, education funding was decentralized and spatial variation in school quality was substantial for whites and blacks. Table 3 counterfactuals effectively equalize school quality across races but also imply a fluid migration of blacks so that their geographic distribution mimics that of whites.

Instead, we consider a counterfactual where local governments adhered to the Supreme Court decision in *Plessy v Ferguson* and ask what the impact of a true "separate but equal" standard would have been. We re-calculate the wage gap in 1940 under the assumption that all black schools rose to the quality of white schools *in the same county*. An important caveat to this analysis is that we do not include individuals who migrate outside of the South. If school quality promoted inter-regional migration, our counterfactual estimates may overstate the conditional gap.[35]

[33]This assumes an equalization of ages and county covariates, the other components of $X_{icra}$ across blacks and whites as well.

[34]These calculations reflect reducing the unadjusted coefficients (52.9 and 51.3 log points) by the contribution of school quality from the Gelbach analysis (16.5 and 18.6 log points).

[35]Collins & Wanamaker (2014) find evidence of limited positive selection into the inter-regional migrant stream, indicating that the bias from this sample selection issue may be small.

The second refinement we make to this counterfactual exercise recognizes that time in school is a function of school quality (Margo, 1987), and that equalized school resources would have affected earnings through the years of schooling channel as well as the input quality channel.

We take two approaches to incorporate the second order effect of school quality on educational attainment. First, we use our own sample to predict years of schooling as a function of the school quality index for all black individuals:

$$School_{ic} = \tau Qual_{ic} + \epsilon_{ic}$$

The estimated value of $\tau$ is taken to be the elasticity of time in school with respect to school quality. We then estimate the full impact of a counterfactual level of school quality on earnings using both first and second-order effects.

We recognize, however, that these simple regression estimates of counterfactual years of schooling, in light of improved black school quality, likely overstate the elasticity of attainment. Our second approach to endogenizing years of schooling relies on quasi-experimental evidence of the effect of school quality from other work. Aaronson & Mazumder (2011) estimate the effect of exposure to Rosenwald schools – in terms of classroom capacity per black school-aged youth – on individual years of schooling, among other outcomes. We convert their Rosenwald exposure measure into a change in our calculated Z-score, and then convert the elasticity of educational attainment with respect to Rosenwald schools to an elasticity per unit of school quality in our sample. We then use this quasi-experimental estimate of $\hat{\tau}$ to calculate the counterfactual level of educational attainment after a "separate but equal" mandate.[36]

Counterfactual results for each of the main labor market outcomes of interest are presented in Table 7. The top, unnumbered, row of estimates repeats the baseline black-white gap observed in our 1940 data. Row 1 lists estimates of the remaining gap after school quality is equalized

[36] Aaronson & Mazumder (2011) express the effect of school quality on years of schooling with respect to their quality measure "Rosenwald exposure," the number of classrooms per 45 rural blacks aged 7 to 17 in a county. We lack access to the same population measures, but we substitute blacks aged 10 to 20 in a county, multiplied by the percent of the overall county population that is rural. Because classrooms are not one of our school quality metrics, we make the innocuous assumption that each classroom represented an additional teacher (as supported by the historical record) and convert the change from 0 to 1 in Rosenwald exposure to the number of additional black teachers in a county, based on our rural black population estimates. We then re-calculate the teachers-per-student Z-score in each county after Rosenwald exposure changes from 0 to 1. We divide the Aaronson & Mazumder (2011) reported elasticity by this change in Z-score to get the average change in years of schooling per Z-score unit and use this as our quasi-experimental estimate of $\tau$.

within counties. These numbers are *lower* than those indicated by Table 3 if the years of schooling contribution were simply subtracted from the unconditional wage gap, reflecting the fact that white school quality was higher in areas with more black residents. As a result, equalizing school quality within counties generates average school quality measures that are higher for blacks, on average, than for whites. In Row 2a, we allow years of schooling to be a function of school quality using the value of $\tau$ indicated by the regression above. The black-white wage gap is less than 6 log points, and the occupation score gap falls to 3.6 log points.

As mentioned above, interpreting the entirety of the correlation between school quality and educational attainment as causal likely overstates the role of school quality in driving school attendance. Using the quasi-experimental calculation from above, Row 2b indicates a counterfactual black-white wage gap of between 12 and 13 log points in weekly and total earnings and 7.9 log points in occupational score. We conclude that equalizing school quality across races would have had a dramatic impact on labor market inequalities in 1940, reducing unconditional gaps in teh weekly wage by 66 - 77 percent.

# 7 Robustness Checks

This section outlines the results of several sensitivity checks. Results are reported in Tables 8 and 9. Table 8 presents specification tests from a number of alternatives to Equation 1. We emphasize that our preferred models omit controls for unobserved geographic heterogeneity. Sundstrom (2007) indicates systematic variation in the black/white wage gap by characteristics of the locale, including the prevalence of antebellum plantation institutions, and the segregationist preferences of white voters. In our model, discrimination itself is unobservable but to the extent it is concentrated in certain geographic areas, introducing state and local fixed effects would partially obfuscate the effect. In an attempt to avoid this issue, we do not utilize both county and age fixed effects at the same time - in that case, identifying variation would come from within-county within-age differences in school quality, a number that is almost certainly tied up with discrimination tastes.

Baseline findings from Table 3 are repeated in Column 1 for weekly wages, Column 8 for occupation scores, Column 15 for earnings and Column 22 for weeks worked. In the following columns, we change the underlying specification to include state fixed effects, county-of-residence fixed effects and county-of-schooling fixed effects, in turn. For the county-of-residence specification, county-level covariates drop out of the specification, and for both specifications with county fixed effects we drop age fixed effects. We then control for the number of missing school quality metrics (Columns 5, 12, 19 and 26). Recall that quality indices are averages for up to eight normalized statistics. Specifically, we supplement Equation 1 with a quadratic function of the number of missing school quality statistics for each county and cohort. This accounts for the possibility that the quality of data reporting is correlated with the quality of schooling and/or unobserved labor market mechanisms dictating the black-white wage gap. Another specification includes indicator variables for the availability of each school quality metric (Columns 6, 13, 20 and 27). Finally, we re-generate the school quality Z-score as across, rather than within, age cohorts. Our preferred measure of each census respondent's available school quality is composed of eight school inputs normalized by school year, averaged over ages 7 to 18. Columns 7, 14, 21, and 28 illustrate results with an alternative school quality measure, one where each input is pooled and normalized across 1920-1940, better reflecting tremendous growth in school resources over those years.

Conditional differences in weekly wages, occupational scores, and annual incomes are generally within one standard error of baseline estimates. We are left with an estimated conditional weekly wage gap of 13.7 - 17.3 log points, an occupational score gap between 14.5 and 17.2 log points, and a somewhat wider income gap of 5.9 - 16.4 log points.

In Table 9, we check robustness of our estimates to limitations on the underlying sample. The baseline analysis limits the sample of 1940 census respondents to young men who reported non-missing earnings, and who may or may not have had substantial non-wage income. We relax these limitations and make additional changes to the analytical sample of Equation 1. Again, Columns 1, 6, 11 and 16 serve to repeat the baseline results from 3. We then drop all individuals earning more than $50 in non-wage income. We observe a slight increase in the black-white wage gap in Columns 2 and 13 when these earners are excluded, indicating that these in-kind earnings were more prevalent in white rather than black compensation packages. In Columns 3, 8, 13, and 18 we limit the sample to exclude agricultural workers and focus only on the non-farm sectors. This restriction serves to increase estimates of the black-white gap in weekly and gross earnings (to 21.4 and 15.3 log points, respectively) and increase the occupational score gap to 22.9 log points. Higher discrimination in the non-farm sector might be consistent with models of discrimination based on customer preferences and the literal sales penalty imposed on the employers of black workers. This is a version of taste-based discrimination plausibly concentrated in the Jim Crow South, although the resulting black-white gaps remain qualitatively close to those estimated for more modern eras.

Columns 4, 9, 14, and 20 show results when we restrict the samples of black and white males to a common support defined as school quality and educational attainment contained in the range from the mean to the 95th percentile of observed black values. When we restrict to this sample, our measures of wage discrimination rise. When we condition on AGCT score for this group (not shown), the estimated gap for overall wages falls to 8.0 log points and for weekly wages to 13.1 log points (not shown), indicating that conditional racial differences in AGCT differ across the support of school quality and educational attainment and can partially explain the differences in estimated wage discrimination across the distribution of observable human capital.

Columns 5, 10, 15 and 20 contain results from restricting the estimating sample to those individuals where state of birth is equivalent to state of residence in 1935, potentially reducing the error in the assignment of county of schooling. We see a reduction in measured wage discrimination and in conditional differences in occupational score for this restricted sample, although differences from baseline point estimates are slight.

Finally, we expand the sample used to estimate conditional occupational score gaps to all individuals recording an occupation, regardless of whether they reported earnings as well.[37] The expansion in sample size has limited impact on the conditional gap.

# 8 Conclusion

Recent labor market studies have highlighted the importance of human capital in explaining the black-white wage gap. We ask the same question for 1940 workers - how far can human capital disparities go in explaining the large pre-war black-white wage gap? Incorporating new data on race-specific school quality in ten southern states, we document a predominant role of school quality and educational attainment in determining wage inequality. Once we control for estimated

[37]We still exclude farmers from this analysis as the occupation category includes tenant farmers and farm owners. Blacks were more likely to be tenants and whites to be owners, and the resulting occupational score estimated from white earnings in 1950 is highly unlikely to be representative of black earnings in the category.

AGCT scores imputed from WWII enlistment records, the conditional gap falls to as low as 8 log points. Returns to school quality and years of schooling are substantial, and our estimates indicate that the residual, conditional gap in labor market earnings between blacks and whites was not dissimilar from levels observed today.

The power of education to drive labor market wages is echoed in a counterfactual exercise whereby school quality is equalized across races in the South. Under this "separate but equal" standard, we estimate a counterfactual weekly wage gap of between 6 and 18 percent, a fraction of the 53 percent gap observed in 1940. Education equality would have been a powerful tool for raising black economic standing in the South, and the lost opportunity of enforcing separate but equal in southern states, by our estimates, reduced the earnings capacity of this generation of Southerners by between 66 and 77 percent.

At the same time, our results fail to support a narrative of declining labor market wage discrimination over the course of the 20th century. Results identify a conditional weekly wage differential of between 8 and 16 log points, representing models with and without controls for unobserved ability, respectively. The minimum value among modern estimates of labor market discrimination (for males) is 6 log points, but estimates more frequently range from 7 to 12 log points.[38] It is thus difficult to see how declining discrimination could explain a decline in the unconditional gap from 52.9 log points in our sample to 38.2 in 1994. Indeed, the failure to identify a substantively higher conditional wage gap in 1940 relative to today, despite the plausible assumption of a higher number of prejudiced employers in the former period and explicit legal boundaries in the latter, gives us pause in identifying the conditional gap in either period as a consequence, in large part, of discrimination in the labor market.

The absence of large conditional wage gaps seems incompatible with what we know about the Jim Crow era South. Black Southerners were excluded from civil life through a variety of measures that effectively eviscerated their participation in the political process. One result was a denial of the provision of black education at the same level as that provided to whites and an enormous roadblock to the accumulation of human capital. Yet, as we show, blacks participated in economic life, exhibiting labor force participation and employment rates not dissimilar from those of whites and (conditional) earnings ratios not remarkably different from blacks later in the

[38] The 6 log point estimate is from Altonji & Blank (1999) analyzing the NLSY1979 data for 1994. See Table 1.

century. Employers of 1940 may have held animus or equality aversion toward black individuals, but the effect of these attitudes on black wages would have been offset to some degree by profit-maximizing objectives. These profit-maximizing values, not necessarily shared by largely white voting constituencies, explain why severe racial discrimination in the provision of public goods could coincide with a more equal (conditional) labor market.

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# Tables and Figures

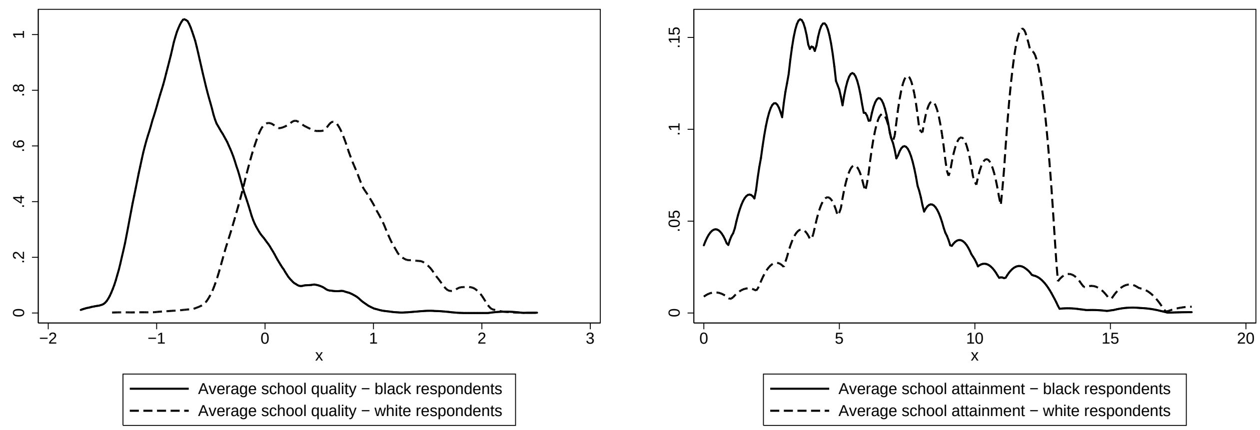


FIGURE 1: School quality and educational attainment kernel density estimates for black and white 1940 census respondents

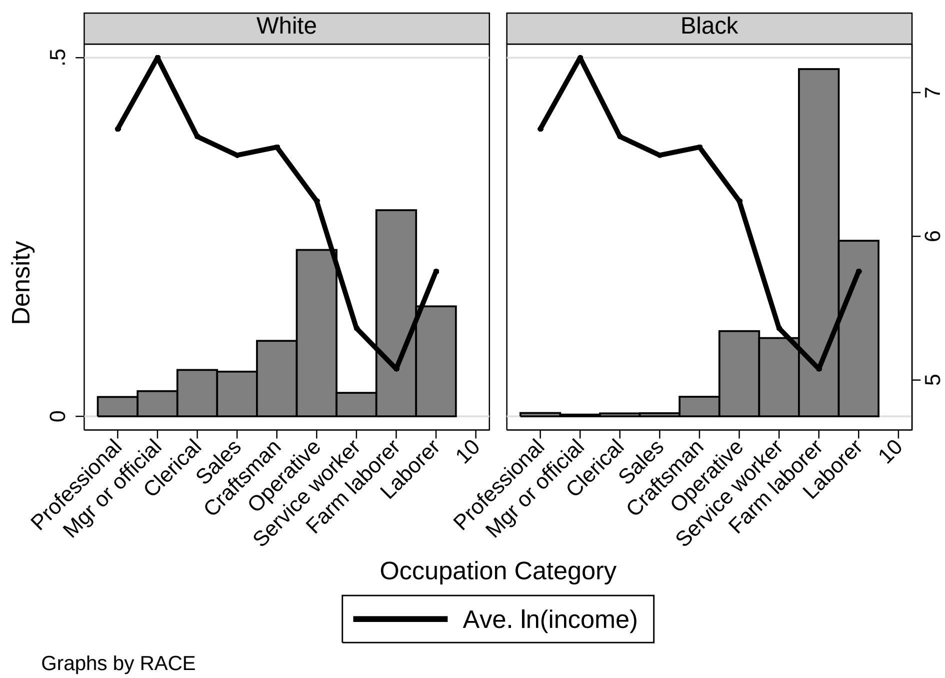


FIGURE 2: Occupation distributions for black and white 1940 census respondents. Average (log) income within those occupations, across both races, are reported on the second axis.

TABLE 1: Decomposition of the Black-White Pay Gap: The Role of Human Capital

| Study | Data | Proxy for human capital | Percentage of total wage gap explained by human capital | Log gap remaining after controlling for human capital |
|---|---|---|---|---|
| Altonji & Blank (1999) | 1980 CPS | years of schooling | 25% | -0.11 |
| Altonji & Blank (1999) | 1996 CPS | years of schooling | 43% | -0.12 |
| Altonji & Blank (1999) | NLSY1979 | years of schooling, AFQT | 61% | -0.06 |
| Altonji & Pierret (2001) | NLSY1979 | years of schooling, AFQT, father's education, labor force experience | all | *insig.* |
| Card & Krueger (1992b) | 1960-1980 Census | years of schooling, state-level school quality | 20% of gap narrowing | *n/a* |
| Carneiro et al. (2005) | NLSY; men 26-28 | years of schooling, $8^{th}$-grade equivalent AFQT | 35-50% | -0.133 - -0.241 |
| Fryer (2011) | NLSY1979 | AFQT | women: all, plus<br>men: 72% | women: 0.127<br>men: -0.109 |
| Fryer (2011) | NLSY1997 | AFQT | women: 71%<br>men: 39% | women: -0.044<br>men: -0.109 |
| Fryer (2011) | College and Beyond 1976 | SAT | women: 53%<br>men: 44% | 0.286 for women<br>-0.152 for men |
| Lang & Manove (2011) | NLSY1979 | AFQT, years of schooling, school inputs | 70% | -0.11 |
| Neal & Johnson (1996) | NLSY1979 | AFQT | 100%<br>men: 70% | women: *insig.*<br>men: -0.072 |
| Oaxaca & Ransom (1994) | 1988 CPS | years of schooling | 43% | -0.125 |
| O'Neill et al. (2006) | NLSY1979 | AFQT, father's education, non-cognitive skills | 46-114%,<br>rising with wage quantile | falls with quantile |

TABLE 2: Summary Statistics

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | ALL BLACK | ALL WHITE | BASELINE SAMPLE BLACK | BASELINE SAMPLE WHITE |
| Individual | | | | |
| Average Wage Income *in natural log* | 5.42 | 5.93 | 5.42 | 5.93 |
| Average Weekly Wage *in natural log* | 1.87 | 2.40 | 1.87 | 2.40 |
| % Reporting | 58.8 | 56.0 | 100.0 | 100.0 |
| Occupational Score *in natural log* | 6.99 | 7.35 | 6.99 | 7.35 |
| % Reporting | 87.0 | 82.2 | 97.5 | 96.4 |
| Average Weeks Worked | 40.9 | 40.8 | 39.0 | 38.7 |
| Unemployment Rate at Time of Census | 9.17 | 9.52 | 9.90 | 8.93 |
| Duration of Unemployment | 38.8 | 45.4 | 35.4 | 43.8 |
| Labor Force Participation Rate | 88.9 | 85.0 | 98.2 | 97.4 |
| Highest Grade Completed | 5.6 | 8.9 | 5.6 | 8.9 |
| School Quality Index (Standardized (0,1)) | -0.55 | 0.47 | -0.50 | 0.55 |
| State of Residence in 1940 | | | | |
| Alabama | 12.8 | 9.0 | 12.3 | 8.7 |
| Arkansas | 5.6 | 6.5 | 4.0 | 6.0 |
| Georgia | 15.5 | 9.6 | 18.1 | 9.7 |
| Kentucky | 2.7 | 12.2 | 3.0 | 10.9 |
| Louisiana | 11.4 | 7.5 | 12.4 | 7.5 |
| Mississippi | 6.6 | 2.2 | 3.8 | 2.2 |
| North Carolina | 14.3 | 12.6 | 14.3 | 12.7 |
| South Carolina | 13.9 | 5.6 | 14.0 | 6.2 |
| Tennessee | 5.8 | 11.6 | 6.4 | 11.0 |
| Texas | 11.6 | 23.3 | 11.8 | 24.5 |
| County of Residence | | | | |
| Percent Rural | 70.4 | 69.2 | 66.2 | 64.2 |
| Per Capita Manufacturing Value | 111.2 | 111.9 | 126.6 | 127.1 |
| Per Capita Retail Sales | 0.22 | 0.24 | 0.24 | 0.26 |
| Per Capita Crop Value | 87.5 | 84.0 | 78.4 | 75.5 |
| Number of observations | 5,423 | 14,849 | 3,141 | 8,260 |

*Notes:* Authors' Calculations from 1940 IPUMS data (Ruggles et al., 2010) and annual reports of state education departments. Includes all black and white males from the 1940 IPUMS sample aged 18 to 25 who lived within the 10 Southern states covered by our school quality data 1935 with reported years of schooling and school quality. Columns 3 and 4 contain only those individuals for whom earnings data are available.

## TABLE 3: Estimates of Black-White Labor Market Outcome Gaps

| Column | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Outcome | ln(Weekly Wage) | | | ln(Occupation Score) | | | ln(Income) | | | ln(Weeks Worked) | | |
| | Unadjusted | Adjusted | Human Capital Adjusted | Unadjusted | Adjusted | Human Capital Adjusted | Unadjusted | Adjusted | Human Capital Adjusted | Unadjusted | Adjusted | Human Capital Adjusted |
| BLACK-WHITE GAP | -0.529 | -0.496 | -0.159 | -0.358 | -0.348 | -0.160 | -0.513 | -0.477 | -0.090 | 0.016 | 0.019 | 0.069 |
| | (0.024) | (0.023) | (0.035) | (0.016) | (0.014) | (0.024) | (0.027) | (0.025) | (0.041) | (0.014) | (0.014) | (0.024) |
| Contribution of | – | – | -0.165 | – | – | -0.055 | – | – | -0.186 | – | – | -0.020 |
| School Quality | – | – | (0.026) | – | – | (0.018) | – | – | (0.029) | – | – | (0.018) |
| Contribution of | – | – | -0.172 | – | – | -0.133 | – | – | -0.202 | – | – | -0.030 |
| Years of Schooling | – | – | (0.010) | – | – | (0.007) | – | – | (0.013) | – | – | (0.007) |
| County Covariates? | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| Age Fixed Effects? | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| Cubic in School Quality Index? | | | ✓ | | | ✓ | | | ✓ | | | ✓ |
| Cubic in Yrs of Schooling? | | | ✓ | | | ✓ | | | ✓ | | | ✓ |
| N | 11,394 | 11,394 | 11,394 | 11,021 | 11,021 | 11,021 | 11,394 | 11,394 | 11,394 | 11,394 | 11,394 | 11,394 |
| Adjusted R-Squared | 0.10 | 0.22 | 0.29 | 0.10 | 0.17 | 0.25 | 0.06 | 0.21 | 0.27 | 0.00 | 0.05 | 0.05 |

*Notes:* Authors' Calculations from 1940 IPUMS data (Ruggles et al., 2010) and annual reports of state education departments. Columns 1, 4, 7, and 10 represent coefficients from an unadjusted regression of log weekly wages, log occupational score, log income, and log weeks worked, respectively, on race alone. Columns 2, 5, 8, and 11 include age fixed effects and county covariates in the regression. Columns 3, 6, 9 and 12 include cubic functions of years of schooling and school quality as per Equation 1. We follow Gelbach (2009) to estimate the separate contributions of school quality and years of schooling in explaining unconditional gaps (i.e., the difference between the age and location-adjusted wage gap in columns 2, 5, 8 and 11 and the human capital-adjusted model in columns 3, 6, 9, and 12). County covariates include the percent urban population, crop value per capita, retail sales per capita, and manufacturing value added per capita.

TABLE 4: Estimates of Black-White Labor Market Outcome Gaps, With Occupation Fixed Effects

| Column | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| Outcome | ln(Weekly Wage) | | | ln(Income) | | | ln(Weeks Worked) | | |
| BLACK-WHITE GAP | -0.280 | -0.287 | -0.115 | -0.207 | -0.218 | -0.018 | 0.073 | 0.069 | 0.097 |
| | (0.019) | (0.019) | (0.030) | (0.024) | (0.023) | (0.037) | (0.015) | (0.015) | (0.025) |
| Occupation Category Fixed Effects? | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| County Covariates? | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| Age Fixed Effects? | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| Cubic in School Quality Index? | | | ✓ | | | ✓ | | | ✓ |
| Cubic in Yrs of Schooling? | | | ✓ | | | ✓ | | | ✓ |
| N | 11,021 | 11,021 | 11,021 | 11,021 | 11,021 | 11,021 | 11,021 | 11,021 | 11,021 |
| Adjusted R-Squared | 0.33 | 0.36 | 0.38 | 0.26 | 0.32 | 0.34 | 0.03 | 0.06 | 0.06 |

*Notes:* Authors' Calculations from 1940 IPUMS data (Ruggles et al., 2010) and annual reports of state education departments. See discussion in Section 4.2

TABLE 5: Estimates of Black-White Labor Market Outcome Gaps, Including Unobserved Ability Estimates

| Column | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| Outcome | | | ln(Weekly Wage) | | | | | ln(Income) | | |
| BLACK-WHITE GAP | -0.159 | -0.167 | -0.080 | -0.091 | -0.172 | -0.090 | -0.108 | 0.058 | 0.036 | -0.100 |
| | (0.035) | (0.29) | (0.042) | (0.043) | (0.037) | (0.041) | (0.28) | (0.051) | (0.048) | (0.041) |
| Baseline | ✓ | | | | | ✓ | | | | |
| Parental education controls | | ✓ | | | | | ✓ | | | |
| Cognitive ability imputation method 1 | | | ✓ | | | | | ✓ | | |
| Cognitive ability imputation method 2 | | | | ✓ | | | | | ✓ | |
| Cognitive ability imputation falsification | | | | | ✓ | | | | | ✓ |
| N | 11,394 | 11,394 | 11,261 | 11,394 | 11,394 | 11,394 | 11,394 | 11,261 | 11,394 | 11,394 |
| Adjusted R-Squared | 0.29 | 0.29 | 0.29 | 0.29 | 0.29 | 0.27 | 0.28 | 0.27 | 0.27 | 0.27 |

*Notes:* Authors' Calculations from 1940 IPUMS data (Ruggles et al., 2010), World War II enlistment records, and annual reports of state education departments. See specifications and further discussion in Section 5.

TABLE 6: Estimates of Black-White Labor Market Outcome Gaps, Excluding Non-Migrant Blacks

| Column | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Outcome | ln(Weekly Wage) | | | ln(Occupation Score) | | | ln(Income) | | | ln(Weeks Worked) | | |
| | Unadjusted | Adjusted | Human Capital Adjusted | Unadjusted | Adjusted | Human Capital Adjusted | Unadjusted | Adjusted | Human Capital Adjusted | Unadjusted | Adjusted | Human Capital Adjusted |
| BLACK-WHITE GAP | -0.411 | -0.443 | -0.137 | -0.360 | -0.372 | -0.236 | -0.415 | -0.455 | 0.020 | -0.004 | -0.012 | 0.152 |
| | (0.048) | (0.055) | (0.072) | (0.038) | (0.036) | (0.049) | (0.065) | (0.072) | (0.096) | (0.039) | (0.039) | (0.059) |
| Contribution of | – | – | -0.132 | – | – | -0.003 | – | – | -0.247 | – | – | -0.115 |
| School Quality | – | – | (0.045) | – | – | (0.028) | – | – | (0.053) | – | – | (0.041) |
| Contribution of | – | – | -0.174 | – | – | -0.129 | – | – | -0.224 | – | – | -0.050 |
| Years of Schooling | – | – | (0.018) | – | – | (0.014) | – | – | (0.023) | – | – | (0.009) |
| County Covariates? | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| Age Fixed Effects? | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| Cubic in School Quality Index? | | | ✓ | | | ✓ | | | ✓ | | | ✓ |
| Cubic in Yrs of Schooling? | | | ✓ | | | ✓ | | | ✓ | | | ✓ |
| N | 8,464 | 8,464 | 8,464 | 8,166 | 8,166 | 8,166 | 8,464 | 8,464 | 8,464 | 8,464 | 8,464 | 8,464 |
| Adjusted R-Squared | 0.01 | 0.16 | 0.25 | 0.01 | 0.10 | 0.22 | 0.05 | 0.18 | 0.27 | 0.00 | 0.06 | 0.07 |

*Notes:* See notes to Table 3. Black males who *did not* migrate between counties over 1935-1940 are excluded from the estimation. See further discussion in Section 5.

TABLE 7: Counterfactual Estimates of the Black-White Earnings and Occupational Score Gap

| Column | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Outcome | ln(Weekly Wage) | ln(Occupation score) | ln(Income) | ln(Weeks Worked) |
| Actual, baseline black-white gap | -0.529 | -0.358 | -0.513 | 0.016 |
| | (0.024) | (0.016) | (0.027) | (0.014) |
| (1) Counterfactual “separate but equal” gap | -0.181 | -0.117 | -0.194 | -0.013 |
| | (0.011) | (0.006) | (0.013) | (0.004) |
| (2) Counterfactual “separate but equal” gap with endogenous years of schooling | | | | |
| (2a) With data-driven elasticity | -0.058 | -0.036 | -0.057 | 0.001 |
| | (0.011) | (0.006) | (0.014) | (0.004) |
| (2b) With quasi-experimental elasticity | -0.122 | -0.079 | -0.128 | -0.006 |
| | (0.011) | (0.006) | (0.013) | (0.004) |

*Notes:* Authors' Calculations from 1940 IPUMS data (Ruggles et al., 2010), annual reports of state education departments, and Aaronson & Mazumder (2011) results for the quasi-experimental impact of school quality on years of schooling. The table compares black-white gaps from Equation 1, with controls for individual and county covariates, to counterfactual gaps under “separate but equal” school quality within counties. For each outcome, row (1) lists the counterfactual black-white gap under equalized school quality, holding years of schooling constant. Row (2a) allows black years of schooling to increase with school quality according to the elasticity of time in school with respect to school quality observed in the analytical sample. Row (2b) allows black years of schooling to increase according to the quasi-experimental elasticity of time in school with respect to Rosenwald schools' exposure, as reported by Aaronson & Mazumder (2011), Table 5, column 1 (1.186 years per Rosenwald exposure).

TABLE 8: Specification checks: state fixed effects, county fixed effects, missing school quality data, school quality indicators, and pooled school quality

| Column | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Outcome | ln(Weekly Wage) | | | | | | | ln(Occupational Score) | | | | | | |
| BLACK-WHITE GAP | -0.159 | -0.173 | -0.161 | -0.137 | -0.159 | -0.164 | -0.159 | -0.160 | -0.163 | -0.164 | -0.145 | -0.155 | -0.166 | -0.172 |
| | (0.035) | (0.037) | (0.039) | (0.042) | (0.037) | (0.035) | (0.034) | (0.024) | (0.024) | (0.026) | (0.032) | (0.024) | (0.024) | (0.023) |
| Baseline | ✓ | | | | | | | ✓ | | | | | | |
| State fixed effects | | ✓ | | | | | | | ✓ | | | | | |
| County-of-residence fixed effects | | | ✓ | | | | | | | ✓ | | | | |
| County-of-schooling fixed effects | | | | ✓ | | | | | | | ✓ | | | |
| Missing data control | | | | | ✓ | | | | | | | ✓ | | |
| Quality metric indicators | | | | | | ✓ | | | | | | | ✓ | |
| Pooled school quality | | | | | | | ✓ | | | | | | | ✓ |
| N | 11,394 | 11,394 | 11,394 | 11,394 | 11,394 | 11,394 | 11,394 | 11,021 | 11,021 | 11,021 | 11,021 | 11,021 | 11,021 | 11,021 |
| Adjusted R-Squared | 0.29 | 0.30 | 0.30 | 0.30 | 0.29 | 0.30 | 0.29 | 0.25 | 0.26 | 0.29 | 0.28 | 0.25 | 0.25 | 0.25 |

| Column | (15) | (16) | (17) | (18) | (19) | (20) | (21) | (22) | (23) | (24) | (25) | (26) | (27) | (28) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Outcome | ln(Income) | | | | | | | ln(Weeks Worked) | | | | | | |
| BLACK-WHITE GAP | -0.090 | -0.147 | -0.164 | -0.059 | -0.097 | -0.102 | -0.098 | 0.069 | 0.026 | -0.003 | 0.078 | 0.062 | 0.062 | 0.070 |
| | (0.041) | (0.039) | (0.046) | (0.053) | (0.043) | (0.053) | (0.039) | (0.024) | (0.023) | (0.025) | (0.031) | (0.023) | (0.023) | (0.023) |
| Baseline | ✓ | | | | | | | ✓ | | | | | | |
| State fixed effects | | ✓ | | | | | | | ✓ | | | | | |
| County-of-residence fixed effects | | | ✓ | | | | | | | ✓ | | | | |
| County-of-schooling fixed effects | | | | ✓ | | | | | | | ✓ | | | |
| Missing data control | | | | | ✓ | | | | | | | ✓ | | |
| Quality metric indicators | | | | | | ✓ | | | | | | | ✓ | |
| Pooled school quality | | | | | | | ✓ | | | | | | | ✓ |
| N | 11,394 | 11,394 | 11,394 | 11,394 | 11,394 | 11,394 | 11,394 | 11,394 | 11,394 | 11,394 | 11,394 | 11,394 | 11,394 | 11,394 |
| Adjusted R-Squared | 0.27 | 0.28 | 0.23 | 0.22 | 0.27 | 0.27 | 0.26 | 0.05 | 0.06 | 0.05 | 0.04 | 0.05 | 0.06 | 0.05 |

*Notes:* Authors' Calculations from 1940 IPUMS data (Ruggles et al., 2010) and annual reports of state education departments. See discussion in Section 7.

TABLE 9: Robustness checks: exclude in-kind earners, exclude agriculture workers, restrict to common support of human capital metrics, birthplace check

| Column | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | 11 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Outcome | | | ln(Weekly Wage) | | | | | ln(Occupational Score) | | | |
| BLACK-WHITE GAP | -0.159 | -0.168 | -0.214 | -0.233 | -0.150 | -0.160 | -0.157 | -0.229 | -0.216 | -0.154 | -0.153 |
| | (0.035) | (0.035) | (0.034) | (0.040) | (0.038) | (0.024) | (0.026) | (0.021) | (0.031) | (0.026) | (0.022) |
| Baseline | ✓ | | | | | ✓ | | | | | |
| Exclude in-kind earners | | ✓ | | | | | ✓ | | | | |
| No agricultural workers | | | ✓ | | | | | ✓ | | | |
| Common support of human capital | | | | ✓ | | | | | ✓ | | |
| Birthplace check | | | | | ✓ | | | | | ✓ | |
| Individuals w missing earnings | | | | | | | | | | | ✓ |
| N | 11,394 | 10,349 | 9,272 | 3,671 | 10,141 | 11,021 | 10,004 | 8,899 | 3.548 | 9,805 | 12,275 |
| Adjusted R-Squared | 0.29 | 0.30 | 0.23 | 0.15 | 0.29 | 0.25 | 0.25 | 0.13 | 0.12 | 0.25 | 0.26 |

| Column | (12) | (13) | (14) | (15) | 16 | (17) | (18) | (19) | (20) | (21) |
|---|---|---|---|---|---|---|---|---|---|---|
| Outcome | | | ln(Income) | | | | | ln(Weeks Worked) | | |
| BLACK-WHITE GAP | -0.090 | -0.104 | -0.153 | -0.222 | -0.069 | 0.069 | 0.064 | 0.061 | 0.011 | 0.080 |
| | (0.041) | (0.043) | (0.046) | (0.043) | (0.040) | (0.024) | (0.026) | (0.026) | (0.033) | (0.026) |
| Baseline | ✓ | | | | | ✓ | | | | |
| Exclude in-kind earners | | ✓ | | | | | ✓ | | | |
| No agricultural workers | | | ✓ | | | | | ✓ | | |
| Common support of human capital | | | | ✓ | | | | | ✓ | |
| Birthplace check | | | | | ✓ | | | | | ✓ |
| N | 11,394 | 10,349 | 9,272 | 3,671 | 10,141 | 11,394 | 10,349 | 9,272 | 3,671 | 10,141 |
| Adjusted R-Squared | 0.27 | 0.27 | 0.23 | 0.18 | 0.26 | 0.05 | 0.05 | 0.06 | 0.06 | 0.05 |

*Notes:* Authors' Calculations from 1940 IPUMS data (Ruggles et al., 2010) and annual reports of state education departments. See discussion in Section 7.