Separate and Unequal in the Labor Market: Human Capital and the Jim Crow Wage Gap Appendix: For Online Publication

1 The Importance of County-Level Data

A key contribution of this paper is a county-level dataset to study how differences in school quality across locations and races in the South ultimately affected labor market outcomes. Prior to the construction of this data, investigation of this question would have been limited to the use of state-level averages of school quality metrics.

These state-level averages are suboptimal for two reasons. First, as discussed in Section 2 of the main text, the level of aggregation for school quality data matters in the modern literature (Betts, 2010), and the use of aggregate data flattens important variation in school quality within states. Second, because multiple measures of school quality are reported (term length, teacher salaries, class size, and so forth), and because states did not unanimously report any particular statistic, relying on state-level data would limit the scope of our study to uniformly available metrics or a smaller set of states that reported most of the same metrics. With county-level annual data on several domains of school quality we have the breadth and depth of information necessary to produce a standardized index of school quality for all ten states, as described in Section 3 of the main text.

Table 1 quantifies the advances from utilizing county-level versus state-level data. First, we highlight the aggregation impact coming from state-level data. In Column 1, we regress the outcomes of interest (here limited to weekly and annual wages) on a cubic of county-level measures of two statistics with close to universal coverage in the data: average teacher salary and teachers per pupil. We continue to include a cubic function of educational attainment, age fixed effects, and a vector of local characteristics, but do not interact these values given the number of covariates now entering the estimating equation. Although estimation of Equation 1 in the main text utilizes a summary Z-score of these (and other) measures, the conditional black-white wage gap under this disaggregated specification matches closely what is observed in Table 3 of the main text (17.2 versus 17.1 log points of weekly wage and 11.1 versus 11.3 of annual wage). We then run the same regression with state-level aggregates of salaries and class sizes and present the results in Column 2 of Table 1. The estimated conditional gap increases by between 11 and 19 log points for the two outcomes of interest, a sizable change. Essentially, state-level quality data contribute less information to the wage model than county-level data, widening the residual wage gap. An important point to emphasize is that, by the same logic, more granular data at the school level may yield an even smaller conditional wage gap.

In Columns 3-8 we show that restricting the measure of school quality to a particular metric (average teacher salary, term length, or teachers per pupil) and then estimating the earnings function not only demonstrates differences between county-level and state-level data but also demonstrates how our results change when we depart from an index of multiple measures and characterize school quality with a single metric. Again, measurement error is a likely factor, as a single domain of school quality (at any level of aggregation) would be expected to do an inferior job of profiling individual human capital. As before, we include age fixed effects in these regressions, so identification comes from within-year variation in the school quality metric of interest across counties (odd columns) or states (even columns). Note also that we would not necessarily expect conditional wage gap estimates with term length controls to match baseline results, as the sample is restricted to a smaller set of states.

Columns 3 and 4 contain county and state-level school quality results, respectively, for average teacher salary. The county-level school quality data generate an estimated conditional wage gap similar to that in the baseline: 15.8 log points of weekly wage and 10.9 log points of annual wages. But the state-level data generate higher conditional gaps:

29.2 and 30.0 log points, respectively. For term length, the state-level metrics in Column 6 produce *lower* estimates of the conditional wage gap than the county-level data results in Column 5. For teachers per pupil, we find that the state-level data introduce a bias of ambiguous direction.

Thus, we conclude that profiling school quality with finely measured detail is preferred both because the level of aggregation matters and because the use of state-level data can generate sample selection or measurement issues that cloud inference.

2 A Decomposition of the Wage Gap

Conclusions in the main text regarding the role of race in determining labor market outcomes were restricted to the measured δ coefficient in the wage regression, In practice, race may interact with other elements of X to determine wages in ways we do not incorporate in the baseline specification. In this section, we use Oaxaca decompositions to value racial differences in the returns to human capital, racial differences in endowments, and the interactions of these two in determining the overall wage gap.

We begin by estimating Equation 1 from the main text by race:

$$lnY_{icra} = \alpha + \beta X_{icra} + \epsilon_{icra},$$

where X_{icra}, as before, controls for school quality, years of schooling, the interaction of quality and years of schooling, age fixed effects, and local characteristics.

The black-white wage gap is decomposed as follows:

$$\bar{X_W}\beta_W - \bar{X_B}\beta_B = (\bar{X_W} - \bar{X_B})\beta_B + \bar{X_B}(\beta_W - \beta_B) + (\bar{X_W} - \bar{X_B})(\beta_W - \beta_B)$$

The first right-hand-side term is the contribution of endowments, the second is the contribution of coefficients (i.e., race-specific differences in returns to X_{icra} elements including human capital), and the third is the contribution of the interaction of the two.[1] The value of each is reported in Table 2 for both weekly and annual wages, broken down further by the contribution of school quality, educational attainment, and the remaining covariates.

[1] For more discussion, see the detail in Biewen (2014).

Table 2 indicates that differences in endowments are the predominant determinant of racial differences in weekly and annual wages, accounting for 58 and 59 percent of the total gap, respectively. That leaves 22.1 and 20.9 log points to be explained by coefficients and the interaction of gaps in endowments and coefficients, or 5-10 log points on top of baseline conditional wage gaps reported in Table 3 of the main text. Results for coefficient differences, however, leave us with little guidance as to which (if any) endowments are valued for blacks differently than for whites. Standard errors are large enough to render point estimates statistically insignificant at conventional levels, and coefficient differences for some covariates (e.g., school quality with regards to annual wages, local characteristics with regards to both outcomes), if they were precise, would indicate that returns to these variables work against the wage gap. Beyond endowment gaps *per se*, much of the wage gap is explained by the interaction of endowment and coefficient gaps.

Because differences in the returns to human capital appear to be a minor portion of the overall decomposition, we do not expect allowances for differences in returns to be critical for inference in the main paper. Indeed, when we evaluate the counterfactual "separate but equal" scenario and allow returns to school quality and educational attainment to differ by race (see Table 7 in the main text), our results are not much changed relative to inference from pooled coefficients.

3 Robustness Checks

This section outlines the results of several sensitivity checks. Results are reported in Tables 3 and 4. In addition to these checks, unreported specification checks using quartic and linear controls for school quality and educational attainment indicate results not dissimilar from those reported in the baseline.

Table 3 presents specification tests from a number of alternatives to Equation 1 in the main text. Baseline findings from Table 3 in the main text 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 three columns within each stratification of specification checks, we change the underlying specification to include state fixed effects, county-of-residence fixed effects and county-of-schooling fixed effects,[2] 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 five normalized statistics. Specifically, we supplement our primary specification (Equation 1 from the main text) with a quadratic function of the number of missing school quality statistics for each county and cohort. This accounts for classic measurement error as well as 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 to take advantage of tremendous intercohort gains in school quality.

Conditional differences in weekly wages, occupational scores, and annual wages are generally within one standard error of baseline estimates. We are left with an estimated conditional weekly wage gap of 13.4 - 18.2 log points, an occupational score gap between 14.4 - 17.7 log points, and a somewhat wider annual gap of 5.6 - 16.8 log points.

In Table 4, 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. Again, Columns 1, 6, 12 and 17 serve to repeat the baseline results from 3 of the main text. We then drop all individuals earning more than $50 in non-wage income. We observe a

[2] We emphasize that our preferred model omits controls for unobserved geographic heterogeneity in labor market outcomes. Sundstrom (2007) highlights 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.

slight increase in the black-white wage gap in Columns 2 and 13 when these earners are excluded, indicating that these non-wage earnings were more prevalent in white rather than black compensation packages. In Columns 3, 8, 14, and 19 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 annual earnings (to 23.0 and 18.3 log points, respectively) and increase the occupational score gap to 23.8 log points. Higher discrimination in the non-farm sector is consistent with recent work on discrimination among skilled versus unskilled men (Wright, 2013) and may 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 using more recent data.

Columns 4, 9, 15, 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. Much like the non-farm sector, the region of common support is expected to include higher skilled black men, who have been shown to be more affected by labor market discrimination. Indeed, when we restrict our attention to this sample, measures of wage discrimination rise by 6 - 12 log points. When we condition on AGCT score for this group (not shown), the estimated gap for annual 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, 16, and 21 contain results from restricting the estimating sample to those individuals where state of birth is equivalent to state of residence in 1935, potentially reducing 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. 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. The expansion in sample size has limited impact on the conditional gap with regards to occupational standing.

4 Other Labor Market Outcomes

In addition to wage earnings and occupational score outcomes, human capital measures may have been influential in determining differences in other labor market outcomes : unemployment, farm employment, and work relief employment.[3] For these results, we expand the sample to include individuals who did not report income. The unemployment rate difference between blacks and whites in this group is minimal, but blacks were 14.5% more likely to be employed in a farming occupation and were 2.8% less likely to be on federal work relief.

In Appendix Table 5 we find a conditional unemployment gap of 3.2 percentage points favoring blacks. Blacks were less likely to be unemployed than whites in the unconditional view (Column 1), but because unemployment was higher among more advantaged white men, controlling for human capital reverses this difference (Column 3). Columns 4-6 indicate that blacks were more likely to be employed in farming but, conditional on human capital measures, were far less likely to be so-employed.

Our main analysis includes a small share of men who were employed via New Deal work relief at the time of the census (about 6 percent of the analytical sample), which was not market-based employment. Work relief employment (which we cannot observe over the months preceding the census) could bias our wage gap estimates downward to the extent

[3]We exclude farm owners and tenants from the farm employment analysis, as in other parts of the study. Since we cannot distinguish farm tenants from owners, grouping all self-employed farm workers together would likely understate the occupational score gap.

that it was concentrated among black men with favorable human capital measures. We find that blacks were 2.8 percentage points less likely to be on work relief than whites, and that human capital measures narrowed that gap to 2.1 percentage points.

5 Data Appendix

5.1 School Finance and Resource Data

This study makes use of data on Southern public school districts between 1910 and 1940 in 10 states (Alabama, Arkansas, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Texas, Kentucky, and Tennessee), including statistics on schools, teachers, students, and expenditures. These states were selected for their consistency in reporting the educational resources of interest. While several researchers have used portions of these data for specific projects, to our knowledge, the assembled dataset is unprecedented in its size, scope, and depth. We have already used these data to estimate the impact of philanthropically-funded Rosenwald schools on public support for segregated schools (Carruthers & Wanamaker, 2013) and to estimate the effect of women's suffrage on local school spending (Carruthers & Wanamaker, forthcoming). This section describes the data and data collection process in more detail.

Our primary sources of education input data are annual reports from state Superintendent Offices, Departments of Education, or equivalent governmental units. Measures of schooling resources reported separately for white and African-American schools typically include:

1. Enrollment, average daily attendance
2. Number of teachers overall
3. Expenditures
4. Teacher salaries
5. Number of schools
6. Average term lengths
7. School revenues drawn from local taxes

See Appendix Table 6 for the distribution of data availability across states and time. We transcribed all available statistics for these ten states and assembled county-by-race panels for the years 1910-1940. Data availability is remarkably consistent across states and years. We conducted an informed 0.5 percent audit of each transcribed variable. Specifically, for each school statistic and each state, we regressed transcribed data against county fixed effects and a quadratic function of time, generating predicted values and residuals. We flagged cells in the top 99.5 percentile of residuals, in absolute value. Then, our research assistant verified the accuracy of each flagged cell by consulting the original scanned reports and fixed any transcription errors. The realized error rate from these flags was 14.9 percent. We believe this to be an encouraging signal of the underlying fidelity of these data, considering that our audit focused on the top 0.5 percent of outliers within counties' time series. We did not correct the rare items that we thought to be errors in the original documents because we have no way of discerning computation and recording errors from year-to-year variation during what was a tumultuous time for public schools in the South.

Other researchers have used portions of these data or statewide aggregations of historic education data to characterize pre-war school resources. We construct summary statistics comparable to those that have been reported in earlier work. See the Carruthers & Wanamaker (forthcoming) (Appendix) for details.

5.2 Census Data

Census data are the 1% micro sample available from IPUMS-USA. We converted top-coded earnings to 140% of the top-coded value. (0.04% of our baseline sample was top-coded.) All individuals reporting a race other than "black" or "white" were deleted from the sample. All individuals with occupation codes corresponding to "unpaid family workers," "farmers," and "farm managers" were eliminated from the working sample, as most individuals in these categories reported no wage income. Educational attainment was top-coded at 17 years of schooling ("5th year of college or more"), and we assume all individuals in this category have 18 years of schooling, reflecting two years of post-college education.

5.3 World War II Enlistment Data

World War II enlistment records are available from the National Archives via their website at archives.gov. We downloaded the raw data files (where the weight field is transcribed) for use in this paper. The files contain a large number of errors, and we deleted all records where fields could not have contained the correct information (for example, weight fields containing non-numeric values or with values greater than 301 or less than 1). We eliminated all females from the sample.

For each individual listed March 1943 through the first week of June (the window during which the weight field was actually each enlistee's AGCT score), we assigned an average school quality metric based on county of residence at enlistment according to the method described in the main text for the census sample. We eliminated all individuals younger than 18 or older than 25 and all individuals where reported race was something other than "black" or "white." The remaining sample contains 66,684 individuals. We estimate AGCT-proxied aptitude as a function of enlistees' observable characteristics that have counterparts in the matched sample of census responses and school quality and then map predicted AGCT to the analytical census sample. See the main text, Section 5.1, for additional details.

References

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TABLE 1: Estimates of Black-White Labor Market Outcome Gaps, Comparing County and State Data

Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Level of Measurement	County	State	County	State	County	State	County	State
Outcome	Teachers per pupil + Average teacher salary		Average teacher salary		Term length		Teachers per pupil	
ln(Weekly Wage)	-0.172	-0.286	-0.158	-0.292	-0.233	-0.174	-0.321	-0.294
	(0.036)	(0.064)	(0.034)	(0.062)	(0.029)	(0.070)	(0.035)	(0.070)
ln(Annual Wage)	-0.111	-0.300	-0.109	-0.300	-0.173	-0.123	-0.249	-0.183
	(0.045)	(0.067)	(0.039)	(0.065)	(0.039)	(0.076)	(0.033)	(0.063)
N	11,130	11,130	11,164	11,164	8,054	8,054	11,182	11,182

Notes: Estimates of the conditional gap, δ, from Equation 1 in the main text using county- or state-level measures of school quality. Standard errors in parentheses. Specific school quality metrics included are the column headings. See text for specification details.

TABLE 2: Decompositions of the Black-White Wage Gap

Column	(1)	(2)
Outcome	ln(Weekly Wage)	ln(Annual Wage)
Black-White Gap		
Baseline Difference	-0.529	-0.513
Oaxaca Decomposition		
Difference due to endowments	-0.308	-0.304
	(0.044)	(0.053)
Difference due to β's	-0.078	0.295
	(0.100)	(0.129)
On Cubic in Educational Attainment	-0.142	-0.549
	(0.296)	(0.382)
On Cubic in School Quality	-0.065	0.762
	(0.394)	(0.509)
All Other Covariates	0.129	0.082
	(0.139)	(0.172)
Difference due to interaction	-0.142	-0.503
	(0.108)	(0.139)

Notes: See text for further discussion.

TABLE 3: 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	-0.171 (0.036)	-0.181 (0.038)	-0.160 (0.039)	-0.134 (0.041)	-0.171 (0.038)	-0.182 (0.035)	-0.180 (0.036)	-0.168 (0.025)	-0.167 (0.025)	-0.164 (0.027)	-0.144 (0.032)	-0.162 (0.025)	-0.177 (0.024)	-0.173 (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	0.31	0.30	0.29	0.29	0.30	0.25	0.26	0.29	0.29	0.25	0.25	0.25

Column	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)	(27)	(28)
Outcome				ln(Annual Wage)							ln(Weeks Worked)			
BLACK-WHITE GAP	-0.113 (0.041)	-0.163 (0.040)	-0.168 (0.047)	-0.056 (0.053)	-0.120 (0.043)	-0.130 (0.040)	-0.133 (0.041)	0.058 (0.024)	0.019 (0.023)	-0.008 (0.026)	0.078 (0.031)	0.051 (0.024)	0.051 (0.023)	0.047 (0.024)
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	0.23	0.27	0.27	0.28	0.06	0.07	0.05	0.05	0.06	0.06	0.07

Notes: Authors' Calculations from 1940 IPUMS data (Ruggles et al., 2010) and annual reports of state education departments. See discussion in Section 3.

TABLE 4: 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	-0.171	-0.179	-0.230	-0.233	-0.160	-0.168	-0.164	-0.238	-0.213	-0.162	-0.162
	(0.036)	(0.037)	(0.034)	(0.040)	(0.039)	(0.025)	(0.027)	(0.022)	(0.031)	(0.027)	(0.023)
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	0.31	0.24	0.15	0.29	0.25	0.25	0.14	0.12	0.25	0.25

Column	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Outcome	ln(Annual Wage)					ln(Weeks Worked)				
BLACK-WHITE GAP	-0.113	-0.125	-0.183	-0.228	-0.086	0.058	0.054	0.047	0.004	0.074
	(0.041)	(0.043)	(0.040)	(0.049)	(0.046)	(0.024)	(0.026)	(0.023)	(0.031)	(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	0.24	0.18	0.27	0.06	0.06	0.06	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 3.

TABLE 5: Estimates of Additional Black-White Labor Market Outcome Gaps

Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome	Unemployment			Farming Employment			Federal Work Relief		
BLACK-WHITE GAP	-0.004	3.1E-04	0.032	0.145	0.125	-0.124	-0.028	-0.029	-0.021
	(0.007)	(0.006)	(0.012)	(0.017)	(0.013)	(0.025)	(0.005)	(0.005)	(0.008)
County Covariates?		✓	✓		✓	✓		✓	✓
Age Fixed Effects?		✓	✓		✓	✓		✓	✓
Human Capital Controls?									
Interacted HC Controls?			✓			✓			✓
N	17,439	17,439	17,439	16,914	16,914	16,914	17,439	17,439	17,439
Adjusted R-Squared	0.00	0.02	0.03	0.02	0.20	0.26	0.00	0.01	0.02

Notes: Authors' Calculations from 1940 IPUMS data (Ruggles et al., 2010) and annual reports of state education departments. Columns 1, 4, and 7 represent coefficients from an unadjusted regression of binary unemployment, binary employment in farming occupations, excluding farm owners and family farm workers, and federal work relief participation, respectively, on race alone. For the second set of regression (farm employment), farmers and farm managers are dropped from the sample. Columns 2, 5, and 8 include age fixed effects and county covariates in the regression. Columns 3, 6, and 9 include cubic functions of years of schooling and school quality, and their interaction, as per Equation 1 in the main text. County covariates include the percent urban population, crop value per capita, retail sales per capita, and manufacturing value added per capita.

TABLE 6: Availability of Education Quality Variables

year	AL	AR	GA	KY	LA	MS	NC	SC	TN	TX
1920	5 6 7 8	3 4 5 6 7	1 2 3 4 5 6 7 8		3 4 5 6 7 8	3 4 8	8	1 2 3 4 7 8	3 4 8	
1921	1 2 3 4 5 6	3 4 5 6 7 8	1 2 3 4 5 6 7 8		3 4 5 6 7 8	3 4 8	3 4 7 8	1 2 3 4 7 8	3 4 8	
1922	1 2 3 4 5 6	3 4 5 6 7 8	1 2 3 4 5 6 7 8		3 4 5 6 7 8	3 4 8	3 4 7 8	1 2 3 4 7 8		
1923	1 2 3 4 5 6	3 4 5 6 7 8			3 4 5 6 7	3 4 8	3 4 7 8	1 2 3 4 5 6 7 8		
1924	1 2 3 4 5 6	3 4 5 6 7 8	1 2 3 4 5 6 7 8		3 4 5 6 7	3 4 8	3 4 7 8	1 2 3 4 5 6 7 8		
1925	1 2 3 4 5 6				3 4 5 6 7 8	3 4 8	3 4 7 8	1 2 3 4 5 6 7 8		3 8
1926	1 2 3 4 5 6		1 2 3 4 5 6 7 8		1 2 3 4		3 4 7 8	1 2 3 4 7 8		3 8
1927	1 2 3 4 5 6	3 4 8			1 2 3 4 7		3 4 7 8	1 2 3 4 5 6 7 8	3 4 7	3 8
1928	1 2 3 4 5 6	3 4 5 6 7 8	1 2 3 4 5 6 7 8		1 2 3 4 7 8		3 4 7 8	1 2 3 4 5 6 7 8	3 4 7	8
1929	1 2 3 4				1 2 3 4 7 8		3 4 7 8	1 2 3 4 5 6 7 8	3 4 7	3 8
1930	1 2 3 4	3 4 5 6 7 8	1 2 3 4 5 6 7 8		1 2 3 4 7 8		3 4 7 8	1 2 3 4 5 6 7 8	3 4 7 8	3 8
1931	1 2 3 4	3 4 7		3 4 7 8	1 2 3 4 7 8		3 4 7 8	1 2 3 4 5 6 7 8	3 4 7 8	3 4 8
1932	1 2 3 4	3 4 5 6 7 8	1 2 3 4 5 6 7 8	3 4 7	1 2 3 4 7 8		3 4 7 8	1 2 3 4 5 6 7 8	3 4 8	3 4 8
1933	1 2 3 4			3 4 7 8	1 2 3 4 7 8		3 4 7 8	1 2 3 4 5 6 7 8	3 4 7 8	3 4 8
1934	1 2 3 4	3 4 7 8	1 2 3 4 5 6 7 8	3 4 7 8	1 2 3 4 7 8		3 4 7 8	1 2 3 4 5 6 7 8	3 4 8	3 4 8
1935	1 2 3 4 5 6			3 4 7 8	1 2 3 4 7 8		3 4 7 8	1 2 3 4 5 6 7 8	3 4 7 8	3 4 8
1936	1 2 3 4 5 6	3 4 7 8	3 4 5 6 7 8	3 4 7 8	1 2 3 4 7 8		3 4 7 8	1 2 3 4 5 6 7 8		3 4 8
1937	1 2 3 4 5 6			3 4 7 8	1 2 3 4 7 8		3 4 7 8	1 2 3 4 5 6 7 8	3 4 7 8	3 4 8
1938	1 2 3 4 5 6	3 4 7 8	3 4 7 8	3 4 7 8	1 2 3 4 7 8		3 4 7 8	1 2 3 4 5 6 7 8	3 4 7 8	3 4 8
1939	1 2 3 4 5 6			3 4 7	1 2 3 4 7 8		3 4 7 8	1 2 3 4 5 6 7 8	3 4 7 8	3 4 8
1940	1 2 3 4 5 6	7	1 2 3 4 7 8	3 4 7	1 2 3 4 7 8		3 4 7 8	1 2 3 4 5 6 7 8	3 4 7 8	3 4 8

Notes: Data available in annual reports of state education departments. Coding 1: Expenditures per enrolled pupil; 2: Expenditures per pupil in ADA; 3: Teachers per enrolled pupil; 4: Teachers per pupil in ADA; 5: Certified teachers per enrolled pupil; 6: Certified teachers per pupil in ADA; 7: Term length; 8: Average teacher salary

TABLE 7: Summary of Variables for Migrant and Non-Migrant Blacks

	Black Non-Migrants	Black Migrants
Average Annual Wage Income *in natural log*	5.41	5.51
Occupational Score *in natural log*	7.0	7.0
Average Weeks Worked	39.0	38.4
Highest Grade Completed	5.5	6.1
School Quality Index	-0.49	-0.58
County of 1940 Residence		
Percent Rural	66.6	61.5
Per Capita Manufacturing Value	125.5	141.0
Per Capita Retail Sales	0.23	0.27
Per Capita Crop Value	78.7	73.7
Number of observations	2,928	211

Notes: See discussion in Section 5.2 of the main text. Migrants include those who crossed county lines within the South. Non-migrants did not change county of residence between 1935 and 1940.