The Geography of Wage Discrimination in the Pre-Civil Rights South

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ABSTRACT

Prior to the modern civil rights movement of the 1960s, the pay gap between African-American and white workers in the South was large overall, but also quite variable across location. Using 1940 census data, I estimate the white-black earnings gap of men for separate county groups called state economic areas, adjusting for individual differences in schooling and experience. I show that the gap was significantly greater in areas where blacks were a larger proportion of the workforce, plantation institutions were more prevalent, more of the population was urban, and white voters exhibited segregationist preferences. These results are consistent with descriptive evidence that discrimination in southern labor markets operated through discrimination in job assignments, which prevented black workers from acquiring skills and also depressed their wages through a crowding effect.

Throughout the twentieth century, but especially before the 1960s, the average pay of African-Americans was substantially below that of white workers. Part of this difference has been attributed to racial differences in skills, arising at least in part from racial differences in the quantity and quality of schooling. In this sense "pre-market" discrimination helped produce earnings differences. But even controlling for measured individual differences in human capital, a significant residual earnings gap between the races remained, most notably in the South prior to the 1960s. The evidence points to an important role for labor-market discrimination in producing the earnings gap. Relative black progress in the labor market was episodic, rather than continuous, suggesting the importance of factors beyond the slow-moving convergence of average human capital levels. The period of most rapid progress, roughly between 1965 and 1975, coincided with major federal initiatives in the areas of civil rights and equal employment opportunity that broke down discriminatory practices. The lion's share of the relative gains occurred within the South.[1]

The idea that labor-market discrimination contributed significantly to the racial wage gap has had to overcome some resistance from economists who, having read their Becker (1971), incline toward the view that most markets are competitive and that competition undermines discrimination. But Becker himself did not deny that discrimination– even by otherwise profit-maximizing employers– was part of the explanation for the large racial wage gap in the American South. And subsequent theoretical work on the economics of discrimination has suggested a number of mechanisms whereby discrimination could persist in equilibrium.

Accepting that discrimination happens, a logical next step is to ask why measured discrimination was so much worse in some times and places than others: this is the question I take up in the current paper. A striking feature of the racial wage gap in the South prior to the 1960s is not only that it was large

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[1] On racial differences in schooling and the role of human capital, see Smith and Welch (1989); Margo (1990); Collins and Margo (2003). On the role of declining discrimination in wage convergence in the South, see Donohue and Heckman (1991); Wright (1999); Chandra (2003); Vigdor (2005). On the effects of federal antidiscrimination laws, see Chay (1998) and Chay and Honore (1998). Collins (2003) analyzes the impact of state-level antidiscrimination laws in northern states prior to the national legislation.

overall, but that it varied substantially across location within the South. I document this variation using individual-level data on weekly earnings derived from the 1940 Census, for county groups called state economic areas (SEAs). An SEA consists of a county or group of counties within a state that had "similar economic and social characteristics" (Bogue 1951, p. 1); it is the smallest geographical unit identified in the individual-level IPUMS 1940 census sample (Ruggles et al 2004). There are 169 such areas in my full estimating sample for southern states.

Cross-SEA variation in the wage gap can be exploited to identify the correlates of labor-market discrimination. I draw upon economic theory as well as specific historical and institutional features of the southern economy to identify potential correlates of the residual wage gap, and then estimate their effects using the census data. I estimate the empirical model in two stages. The first is an individual earnings regression controlling for age, education, marital status, and SEA, all interacted with race. The coefficients on the interaction of white race and SEA are estimates of the adjusted racial earnings gap in each SEA. These coefficients are then used as the dependent variable in a second-stage, SEA-level regression, controlling for characteristics of the area predicted to influence discrimination.

The empirical results indicate that, *ceteris paribus*, the residual white-black wage gap was larger in SEAs where blacks were a larger share of the workforce, in more urbanized areas, in plantation farming areas (as proxied by sharecropping) and– in some specifications– where more voters expressed segregationist preferences in the 1948 presidential election by voting for Strom Thurmond. The estimated effect of racial composition (proportion black) confirms the predictions of discrimination models based on crowding effects (e.g., Becker 1971; Bergmann 1971), but it may also proxy for unobserved variation in attitudes or culture. The geographical distribution of the races in the South was remarkably persistent, a legacy of slave times: the correlation across southern counties between percentage black among adult men in 1940 and percentage slave in the 1860 population is 0.87. In fact, the proportion slave in 1860 is a stronger predictor of the 1940 racial wage gap than is the racial composition of the workforce in 1940. This finding is what one would expect if black workers had migrated from areas with a large adverse

wage gap to those with less discrimination, which would tend to undermine the racial crowding effect over time. The magnitude and significance of the plantation and Thurmond vote coefficients suggest that ideology and culture mattered: areas with a strong tradition of paternalistic and hierarchical race relations, as well as a potentially tight-knit white employing class, did exhibit greater wage discrimination.

THE RACIAL WAGE GAP ACROSS THE SOUTH, 1939

The substantial wage gap between whites and blacks that existed in the South before the modern Civil Rights movement is well known. In my census sample of southern male wage earners with less than 12 years of schooling (discussed in detail below), the median of average weekly earnings in 1939 was $9.62 for blacks, compared with $17.50 for whites. This gap reflected the combined effects of racial differences in pre-market characteristics affecting productivity– such as education– and differential treatment in the labor market. That unequal treatment in the market may have played a substantial role is suggested by the fact that a quite large residual white-black gap (0.36 log points or roughly 36%) remains even after controlling for schooling, age, marital status, and farm industry with a log earnings regression. Donohue and Heckman (1991) obtain a similar residual wage gap for the South in the early 1960s and show that it converged rapidly toward the smaller non-South gap after 1965.

Perhaps less widely appreciated than the size of the overall racial wage gap is the considerable geographic variation in the gap within the South. Map 1 exhibits the pattern of the gap in mean log weekly earnings between white and black men across southern SEAs in 1939.[2] Darker areas on the map had a larger gap between white and black wages. The spatial variation is substantial: the SEA at the 75th percentile had a racial pay gap some 31 percentage (log) points greater than the gap at the 25th percentile.

Racial inequality in schooling played a role in the spatial pattern of the wage gap, just as it did in

[2] The data used in Maps 1-3 are based on the 1940 IPUMS sample, restricted to adult male workers with less than 12 years of education. In these maps the geographical unit is the SEA, so all counties in the same SEA receive the same shading. There are 169 different SEAs represented in these maps. All maps were generated using present-day (c. 2000) county boundaries and Mapland 10 software. Completely unshaded counties had missing or insufficient data (e.g., West Texas and parts of Oklahoma in Maps 1-3).

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the aggregate. Map 2 shows the difference in average years of schooling between white and black adult male workers across SEAs.[3] Comparing it with Map 1, it would indeed appear that the wage gap tended to be greater where the schooling deficit was greater, a hunch confirmed by the correlation of 0.43 between these variables. Because the principal objective of this paper is to shed light on patterns of discrimination in labor markets, not pre-market skills, I focus on explaining the geographical pattern of the racial wage gap after controlling for measurable human capital. Map 3 shows the residual or adjusted earnings gap by SEA, net of race-specific controls for age, education, marital status, farm industry, and interactions of education with birth state and race as controls for school quality.[4] Map 3 is overall a little paler than Map 1, a reflection of the fact that controlling for schooling tends to reduce the size of the estimated racial wage gap, but the spatial variation remains quite substantial.[5]

What can account for the patchwork quilt exhibited in Map 3? If the adjusted wage gap does represent labor-market discrimination against African-Americans, what made discrimination relatively severe in some locations and mild in others? To explain these patterns, I begin with a description of how labor-market discrimination functioned in the South generally. This discussion points to potential factors influencing equilibrium wage discrimination that varied across the South; these form the independent variables in the empirical analysis that follows.

EXPLAINING THE WAGE GAP AND ITS SPATIAL VARIATION

Wage discrimination in the South was largely the product of a racial division of labor limiting the

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[3] The sample is restricted to individuals with less than 12 years of school to be consistent with the sampling criteria used below. The map looks very similar if high-school graduates are included.

[4] Details of the individual log earnings regression used to identify the adjusted wage gap are provided below. For this map, the earnings gap in each SEA is calculated for a hypothetical comparison between a white and a black worker, each 35 years old, married, with 8 years of schooling, working in a nonfarm industry, and born in Georgia. Standardizing on a worker with different characteristics would change the levels but not the relative values of the wage gap between SEAs.

[5] In fact, the general spatial pattern of the wage gap is not much altered by controlling for human capital– the correlation between the unadjusted and adjusted racial wage gaps across SEAs is 0.84.

kinds of jobs open to black workers. These constraints were enforced both by white workers, who resisted competition for preferred jobs and at least some forms of interracial contact, and by white employers' and customers' views regarding the suitability or acceptability of African-Americans for various types of work. Blacks were commonly excluded from supervisory jobs– especially in cases that might involve supervision of whites, which would violate accepted norms of the racial hierarchy (Dewey 1952; Sundstrom 1994); jobs involving substantial interaction with white customers were also often taboo. In industries with job ladders, black workers were frequently blocked from advancing beyond a certain level or restricted to separate "dead-end" occupational clusters.[6] White-dominated unions further limited black entry into organized skilled trades (Spero and Harris 1931; Higgs 1989). The result was that racial job restrictions were not skill-neutral, but biased in the direction of reserving higher-paying work to whites.

Black workers' inferior occupational status cannot be fully explained by black deficits in the quantity and quality of schooling (Margo 1990). In agricultural and blue-collar labor markets, many skills were learned on the job and required little formal education; access to higher pay was determined by job assignments and skills acquired once employed (Wright 1986). Consigned to lower-skilled jobs, black workers ended up with lower average wages than whites who had started their careers with the same basic qualifications. Southern labor markets in the early twentieth century may often have provided "equal pay for equal work" (Higgs 1977; Wright 1986), but this was not inconsistent with pay discrimination arising from *unequal* work assignments.

Labor mobility and competition among employers placed limits on discrimination in wages or job assignments. That southern labor was mobile across employers, at least locally, is well known (Wright 1986). Markets for wage labor in many parts of the South were closely tied to the agricultural sector; farm tenancy was an alternative to employment outside agriculture, and in many areas workers shifted seasonally between farm and nonfarm employments (Jones 1992). The racial wage differential in these

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[6] Examples include obstacles to job promotions in the railroad industry (Sundstrom 1990) and racially separate job groupings in the pulp and paper industry (Northrup 1970). Also see Johnson (1943).

areas could not have much exceeded the racial differential in farm incomes. Alston and Kauffman (2001) show that in 1910, black and white sharecroppers actually earned roughly equal incomes, ceteris paribus, although blacks tended to occupy a lower average position on the agricultural ladder. Competition between employers should also have mitigated wage discrimination, because hiring lower-paid black workers into "white" jobs would have increased a firm's competitiveness by reducing costs (Becker 1971).

Despite these market pressures, there is evidence that by the 1920s a racial wage gap had emerged even in low-skilled markets, where one might expect competition and alternative employment in farming to have had their strongest equalizing effect. Wright (1986, p. 197) argues that decades of occupational and industrial segregation had resulted in black and white workers occupying "noncompeting jobs," even at the entry level. With job mobility constrained by racial norms and restrictions, black jobs may have been subject to a crowding effect that reduced the wage below wages in white jobs of similar skill (Bergmann 1971).[7] Social pressures from white workers and the white community at large imposed costs on employers who deviated from entrenched racial norms governing employment and pay, offsetting the profit incentive for nondiscrimination (Johnson 1943; Donohue and Heckman 1991; Wright 1999). Even when there were employers willing to cross the color line, black workers had to find them; with costly job search, many black workers may have settled for a lower wage instead.[8]

These general observations suggest four sets of factors that should have determined the degree of wage discrimination in a local labor market: (1) the strength of the racial norms or beliefs that restricted job assignments by race; (2) the relative supply of black versus white workers; (3) the structure of the local job market, in terms of the heterogeneity of demand for skills and the degree of competition; and (4)

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[7] In principle "crowding" could adversely affect the wages of either of the noncompeting groups, but in the case of racial discrimination, mobility was probably asymmetric, with whites able to move into black jobs if increased demand was making the wage attractive, but the reverse less common.

[8] Job search costs give rise to monopsony power in the labor market and the opportunity for employers to wage discriminate against workers with less elastic supply (fewer alternatives): see Manning 1993; Black 1995; Sasaki 1999.

government interventions in markets that may have constrained wage setting or created temporary

disequilibrium. These factors, or observable proxies for them, constitute the explanatory variables I use to

predict the adjusted racial wage gap across SEAs.

Racial attitudes and norms. White racial attitudes and unwritten social rules governing black-

white relations varied considerably across the South. As Johnson (1943, p. 6) put it, "Within the

framework and policy of racial segregation are wide variations in local practice." Race relations tended to

be more traditional and rigid in the deep South and rural areas than in border states and urban areas.

Racial stratification remained particularly potent in the rural plantation districts, with their white

landowning elite and traditions of paternalism and black deference (Alston 1986; Alston and Ferrie 1993;

Wright 2001). The discussion to this point suggests that general social restrictions on black behavior

should have been correlated with restrictions on black job assignments and therefore wage discrimination.

For the period under study, direct evidence on racial attitudes, such as might be derived from

social surveys, cannot be obtained at a useful level of disaggregation.[9] But plausible proxies are available

at the SEA level. Plantation districts can be identified using the prevalence of sharecropping in agriculture

(Virts 2006). In addition, I use two variables measuring white behavior that seems to have been animated

by racial prejudice: the strength of the local white vote for Strom Thurmond in the 1948 presidential

election and the relative frequency of lynchings of blacks during the early twentieth century.

Relative supply of black workers. Discrimination in job assignments shaped the relative demand

for black versus white labor, which interacted with the relative supply of black and white workers to

determine the racial wage differential. Studies going back at least to Blalock (1957) tend to find a positive

correlation between percent black and the racial income gap, although not always.[10] In Becker's (1971)

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[9] In his study of race etiquette and tenancy in the South, Alston (1986) uses data from a survey of African-Americans from 1961-62. But the timing and small sample sizes of such surveys render them of little utility in the present study.

[10] See recent studies by sociologists, such as Fossett and Seibert (1997); Cohen (1998); and Huffman and Cohen (2004). Using state-level data for different occupational groupings, Flanagan (1973) found no significant effect of relative supply on relative wages for most occupations.

discrimination model, an increase in the relative supply of blacks (proportion black) decreases the black-white relative wage (increases the racial pay gap), given the distribution of tastes for discrimination. But in a more realistic model of job assignment, the net effect of relative supply is ambiguous. On the one hand, given binding restrictions on the jobs open to black workers, the crowding of more blacks into those jobs would depress their marginal product, resulting in lower average pay for black workers and widening the overall racial pay gap (Bergmann 1971). On the other hand, greater black representation in the workforce could lead employers to upgrade black workers to skilled positions once they had exhausted the supply of qualified whites (Thurow 1975; Wright 1986). The resulting compositional shift would then tend to raise the average wage of blacks relative to whites, diminishing the wage gap.[11] By increasing the proportion of jobs potentially open to blacks, upgrading might also reduce the expected search time of blacks, increasing their equilibrium wage relative to whites.[12]

Racial composition may also have affected racial attitudes or taste for discrimination. A common argument posits that where blacks constituted a larger share of the local population, they were perceived by whites as a greater social, economic, or political threat (e.g., Allport 1954; Becker 1971; Huffman and Cohen 2004). This increased threat in turn elicited greater hostility from whites and more efforts to exert social control, for example through lynchings or voting behavior (Myrdal 1944; Heer 1959; Tolnay and Beck 1995). Of course one cannot rule out the reverse effect– that racial hostility might be mitigated by greater exposure to the minority group. As an example of the latter phenomenon one might cite cases of relative racial cooperation in some southern labor unions where white workers had to form coalitions with a large incumbent black workforce, such as in dock work (Spero and Harris 1931). Greater exposure to black workers could presumably also lead to a learning process that mitigated white misperceptions about their abilities (Whatley 1990).

[11] Sociologists refer to the same idea as the "overflow" or "spillover" hypothesis (e.g., see Fossett and Seibert 1997).

[12] This is a prediction of search models of discrimination, such as Black (1995) and Sasaki (1999). See also Akerlof (1985). Empirical support for such models can be found in Barth and Dale-Olsen (1999) and Bowlus and Eckstein (2002).

Finally, concentration of African-Americans in impoverished southern communities might have operated like residential segregation to create adverse neighborhood or social network effects, as in Cutler and Glaeser (1997). It is noteworthy, however, that the adverse "ghetto" effects identified by Cutler and Glaeser using 1990 data appear to be a relatively recent phenomenon; using a similar methodology, Collins and Margo (2000) find little evidence of an adverse residential segregation effect for the period under study here.

Assessing the impact of relative supply or proportion black on the relative wage empirically requires dealing with the likely endogeneity of relative supply. To the extent that black workers were mobile and responsive to wage incentives, they would have tended to move from areas with more severe wage discrimination to areas with less. In the presence of a crowding effect, migration that reduced the relative supply of blacks in the place of origin would narrow the wage gap there and increase it in the place of destination. An estimate of the crowding effect based on the correlation between relative supply and relative wages would then be biased toward zero. The evidence summarized in Map 3 provides strong circumstantial evidence that considerable spatial variation in the wage gap survived such labor flows. But mobility between state economic areas in the South was not uncommon: In my core sample of male workers in 1940, 6.3% of blacks and 10.7% of whites had changed SEA of residence between 1935 and 1940.[13]

Ideally, to identify the impact of proportion black on the wage gap one would like to have a natural experiment, a la the Mariel boatlift, that exogenously altered the racial composition of some SEAs (Card 1990). In lieu of such an event, I exploit the persistence of racial composition across southern SEAs between the era of slavery and 1940. It seems safe to assume that the historical distribution of slaves across location did not reflect voluntary migration decisions by black workers, and therefore that the proportion of the population who were slaves in 1860 was not caused by the postbellum wage gap.

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[13] These numbers come from responses to the Census question on residence in 1935, for those individuals in the IPUMS with known SEA in both 1935 and 1940.

Unfortunately, although predetermined, proportion slave in 1860 may still not be a valid instrument, to the extent that it is correlated with omitted variables that affected the wage gap. For example, the racial difference in unobserved dimensions of school quality during the twentieth century may have been greater in former slave districts. Below, I discuss the results of using proportion slaves in 1860 as an instrument, and present some results for a reduced-form equation in which the 1940 racial composition is replaced by the proportion slaves in 1860.

Job market structure. In a homogeneous job market with limited opportunities for skill acquisition, racial job restrictions would have had relatively limited impact on black-white relative wages. Competition among employers for workers would likely have mitigated the effects of taste for discrimination. At the SEA level, available data are inadequate to calculate any sophisticated measures of job heterogeneity or market concentration. Instead, I rely on a few basic controls for economic characteristics of the SEA: proportion of population urban and the proportions of workers employed in manufacturing and in agriculture. Compared with labor markets in rural, agriculture areas, labor demand in urban and industrial areas was likely more diversified in terms of occupations and skills, with a greater potential for restrictions on skill acquisition; more differentiated markets may also have created opportunities for the exercise of market power against black workers by employers or organized white workers with specialized skills. In rural markets, discrimination was also limited by alternative opportunities for low-skilled black workers on the agricultural tenancy ladder.

Government policy. Measuring the impact of labor-market discrimination within the South in 1940 is complicated by federal policies of the period that may have generated long-run or short-run disequilibrium conditions in the labor market: namely, the minimum wage and agricultural adjustment policies. To the extent that the legal minimum wage established under the Fair Labor Standards Act of 1938 was enforced and binding, it would have forced racial wage equality in the lower tail of the distribution in covered sectors of the economy. In this case, employers with a taste for discrimination would simply have failed to employ black workers, forcing them into nonemployment or the uncovered

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sector (predominantly agriculture). The measured racial wage gap among employed workers in nonfarm industries would then be biased toward zero in low-wage areas.

Studies by the Bureau of Labor Statistics at the time suggested that the minimum wage was much more likely to be binding in the low-wage South than elsewhere in the country (U.S. Dept. of Labor 1947; Schulman 1991, p. 66; see also Cogan 1982). Figures 1a-1c present corroborating evidence from the distribution of average weekly earnings in 1939 using my census samples. Through October 24, 1939, the federal minimum wage was 25 cents per hour, or $10 for a 40-hour workweek. Figure 1a shows evidence of considerable heaping at $10 per week for black workers in nonagricultural industries.[14] Black workers employed in agriculture (Figure 1b) were generally uncovered, and there is no evidence of heaping there. Figure 1c, which shows earnings for southern whites in nonfarm industries, indicates little evidence of wage heaping at the legal minimum. A comparison of Figures 1a and 1c suggests that the legal minimum was more likely to bind for blacks than for whites.

Because the legal minimum was of course a uniform federal policy across locations, its impact on the wage racial gap cannot be estimated directly. Rather, in results discussed below, I examine the potential impact of the minimum wage on my results by estimating the model for a sample restricted to workers with above-minimum earnings, correcting for the selection bias. The main results are robust to this specification.

New Deal farm policy may also have had at least a short-run effect on the geographical pattern of the racial wage gap. The shift from sharecropping to wage labor and the mechanization of preharvest activities in response to incentives created by the Agricultural Adjustment Act resulted in a release of tenants into the wage-labor market, but with little immediate out-migration until the wartime increase in demand for industrial labor (Whatley 1983). If black former sharecroppers were disproportionately

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[14] Industries that could claim not to be engaged in interstate commerce– such as some sawmills, for example– were not covered by the FLSA, which along with lax enforcement may help explain the presence of sub-minimum pay in the figure (see Schulman 1991) The minimum was raised to 30 cents per hour effective October 24, 1939..

affected by these changes, one might expect to see a short-run widening of the white-black pay gap in the

hardest hit, plantation districts. This suggests another mechanism whereby the wage gap might be

expected to be positively associated with plantation agriculture.

EMPIRICAL MODEL OF THE RACIAL EARNINGS GAP

To identify empirically the covariates of wage discrimination across southern SEAs, I employ a

two-stage regression procedure. The first stage is a log earnings regression using individual-level census

data, with controls for individual characteristics, including age, education, and marital status, a set of state

economic area (SEA) dummy variables, and interactions of those SEA variables with a dummy for white

race. The coefficients on the SEA-race interactions represent estimates of the white earnings premium or

racial earnings gap (in percent) within each SEA, adjusted for racial differences in skills and returns to

skill.[15] That is, the adjusted racial earnings gap by SEA j is a set of coefficients (φ_j) on interactions

between race and SEA dummy variables:

$$y_i = \beta X_i + \gamma X_i d_i + \sum_j \left(\lambda_j Z_{ji} + \varphi_j Z_{ji} d_i \right) + u_i$$

where y_i is log earnings for individual i, X_i is a vector of individual characteristics, d_i is a dummy variable

equal to 1 for whites and 0 for blacks, and Z_j is a dummy variable = 1 for SEA j. By including a full set of

SEA dummy variables Z_{ji}, the regression controls for any SEA-specific characteristics that affected the

general local wage level, including local cost of living. The black wage level in SEA j is captured by the

coefficient λ_j, while the white wage level is captured by $\lambda_j + \varphi_j$.

In the second stage, the estimated interaction coefficients $\hat{\varphi}_j$ are regressed on characteristics of

the SEA, S_j:

$$\hat{\varphi}_j = \alpha S_j + e_j$$

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[15] Throughout the paper I refer to log-point changes as percentage changes, which is approximately true if
we define percentage changes relative to the mean of before and after levels.

Because of sampling error in the estimation of φ_j, the errors of the second-stage regression e_j are heteroskedastic, so I use the estimated standard errors from the first stage to form weights for weighted least squares.[16]

Census earnings data and first-stage regressions

Any effort to analyze earnings across local labor markets in the South prior to the modern Civil Rights movement of the 1960s is hampered by data limitations. Blalock (1957) used 1950 Census data on black-white income differences at the county level to show that the racial income gap was greater in counties with a larger proportion black. But his estimate of the racial income gap did not control for individual characteristics that might have varied by race and location, such as human capital. If blacks happen to have been concentrated in counties where the black-white educational gap was relatively large, for instance, the unadjusted racial income gap would have been greater in such counties because of unequal education rather than unequal treatment within the labor market. For this reason it is desirable to use individual-level data to control for pre-market skills to the extent possible. The tradeoff is that geographical detail is often limited in individual-level data, and sample sizes are often small.

The federal census did not collect information on educational attainment or earnings before 1940, so standard controls for individual skills cannot be employed until that date. Earnings and education were only asked of a subsample of respondents to the 1950 Census (sample line individuals), limiting the number of observations available within small geographical units. The microsample of the 1960 Census does not identify any geographical unit smaller than the state. This leaves one with the 1940 Census, which includes earnings and education information, and for which the smallest identifiable geographical unit is the SEA (Ruggles et al, 2004). Past examinations of spatial variation in discrimination have often

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[16] Following a similar strategy employed by Card and Krueger (1992), observations are weighted by the inverse of the squared standard error. This is only approximately correct, because the second-stage error variances should include not only a measurement error but also a model error component, and are in principle correlated with each other off the diagonal.

used metropolitan areas as the geographical unit, but SEAs offer a couple of important advantages over metropolitan areas. First, obviously, they permit the inclusion of nonmetropolitan areas in the analysis– especially important in a relatively rural region such as the South. Second, because SEAs are strictly defined by their constituent counties, any data available at the county level can be aggregated to the SEA level.

One potential concern about relying on the 1940 census is that 1940 was not a very typical year because of the lingering impact of the Great Depression. Nationwide, substantial numbers of workers remained unemployed or on government-sponsored work relief programs, such as the WPA, and therefore those workers reporting earnings may have been a selective group.[17] This problem, however, is probably less severe for southern labor markets than northern ones. The overall unemployment rate was lower in the South, and racial differences in unemployment and work relief were actually smaller in the South, suggesting less potential for sample selection bias affecting racial earnings comparisons (see Sundstrom 1997). As a robustness check, I estimate the model for the smaller 1950 census sample.

For the first-stage (log earnings) regression, the sample consisted of all individuals in the 1940 IPUMS satisfying the following criteria:

- white or black male, age 18-64
- less than 12 years of education completed
- wage or salary worker
- holding civilian job
- worked at least 26 weeks during 1939
- positive earnings in 1939
- residing in southern SEA with at least 5 white and 5 black workers satisfying the above criteria.

This sample contained 11,560 blacks and 27,585 whites.

In 1940, only a small fraction of southern blacks had completed high school (6.7 percent of the black workers satisfying my other sampling criteria), whereas a substantially larger proportion (33.3 percent) of whites had. To the extent that a high-school diploma opened up a range of job opportunities generally unavailable to less-educated workers, high-school graduates occupied a distinct segment of the

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[17] Unemployed and emergency workers are not included in the regression samples discussed below.

labor market from which most blacks were excluded. For the purposes of identifying labor-market

discrimination, then, restricting the sample to workers who had not finished high school yields a sample

of whites who are more comparable with the vast majority of black workers.[18] The restriction to SEAs

with some minimal representation of workers assures that SEA-specific earnings can be estimated– the

results are robust to changes in these thresholds.

The dependent variable for the earnings regression is the natural log of average weekly earnings

in 1939, calculated as wage and salary income for 1939 divided by weeks worked in 1939. The 1940

census only reported total wage and salary earnings and total weeks worked for the preceding year, not

total hours or hourly wages. However, the weeks worked question in the 1940 census asked respondents

to report their estimated 1939 *full-time equivalent* workweeks, typically normed to a 40-hour workweek.

Thus an individual who worked 50 weeks at 20 hours per week was expected to report 25 weeks. This

procedure implies that the weeks worked are really an estimate of total hours/40, so the average weekly

earnings measure used here is roughly proportional to average hourly earnings.

Table 1 shows the proportion of individuals satisfying the various work-related sample

restrictions, conditional on less than high school completion, by race. Altogether, only about two-fifths of

the observations satisfy all the restrictions, but the degree of selectivity is very similar for blacks and

whites. The sampling criteria for valid weekly earnings disproportionately exclude individuals working in

farming, because a large fraction of farmers and farm workers were owners or tenants rather than wage or

salary workers. Among whites, the sampling criteria exclude 46 percent of individuals in nonfarm

industries, but fully 86 percent of those in farm industries. The corresponding numbers for blacks are 43

and 74 percent.

Southern farm workers often received some of their payment in kind (e.g. Alston and Ferrie

1985). The census earnings data relate only to cash income, and thus full agricultural wages may be

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[18] The core results presented below are robust to including high-school grads in the sample. This and other
sensitivity checks reported in the paper are available from the author upon request.

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underestimated. I deal with this problem in two ways. First, all earnings regressions include a dummy variable for working in agriculture, interacted with race, which should control for the in-kind component of payments to farm workers if it is roughly a constant proportion of pay for each race across locations. Second, I estimate the regressions for a sample restricted to nonagricultural workers.

In the earnings regressions, the basic individual controls (X_i) are for age, education, agricultural industry, and marital status (married with spouse present). All are fully interacted with race to allow for racial differences in returns. Age is entered as a quadratic and education is linear in years of schooling completed.[19] The results, not reported in detail here, indicate positive returns to age and education, with significant racial differences in returns.[20] Following Card and Krueger (1992), I control for variation in the quality of schooling by interacting the race and education variables with state-of-birth dummies. The second-stage results are similar with or without these interactions in the first stage. An alternative approach to controlling for school quality is to interact years of schooling with various proxies for school quality. For residents of Alabama, Florida, Georgia, North Carolina, South Carolina, and Virginia, I am able to interact years of schooling with race-specific average term length and teacher-pupil ratios for 1910 at the SEA level.[21] When SEA dummy variables are included in the regression, the coefficients on these school-quality interactions are not individually or collectively significant.

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[19] This specification closely follows the standard Mincer earnings regression format. Heckman et al (2003) have shown that the key assumptions of the Mincer specification hold up fairly well for the 1940 and 1950 census data (although not so well in more recent years). I use age and its square in place of the conventional measure of potential experience (equal to age minus years of school minus 6) and its square. The standard experience measure seems suspect in my sample, in which a large percentage of workers have very few years of schooling (the median for blacks is 5 years). According to the conventional experience calculation this would count work at age 11 or even younger as work experience for a good many individuals. No doubt many children were working in the South, but whether their experience contributed to human capital acquisition in the same way as a young adult's is open to question. At any rate, figures in Heckman et al (2003) show that the age and experience profiles of earnings, stratified by schooling, actually have quite similar shapes in 1940. Using potential experience in place of age does not substantively change my results.

[20] Complete first-stage regression results are available from the author upon request.

[21] These data were kindly provided by Carolyn Moehling. Because it is impossible to know where an individual was living when he received his education, each was assigned the school quality proxies for his 1940 SEA of residence. The 1940 Census did record state of birth, and I restricted this sample to those individuals residing in their state of birth to reduce potential measurement error introduced by migration between schooling and 1940.

The earnings regressions do not include controls for occupation. To the extent that wage discrimination operated through differential job assignments that created obstacles to black advancement, controlling for occupational status would simply remove a key component of wage discrimination from the SEA wage gap estimates. Racial differences in occupational assignments that resulted from black deficits in the quantity and quality of schooling, on the other hand, should be captured by the educational controls in the earnings regression.

SEA-level data

The objective of the second-stage regressions is to estimate the impact of local labor-market (SEA) characteristics (S_j) on the SEA white-black earnings premium estimated in the first stage. Regressors were chosen based on the theoretical and historical factors discussed in the preceding section and were in each case generated by aggregating county data to the SEA level. Summary statistics for the independent and dependent variables in the SEA sample are given in Table 2. The first two variables represent the adjusted racial wage gap as estimated by the coefficient on the white-SEA interaction in the first-stage log earnings regression, relative to the excluded SEA category.[22] Only the relative values of these coefficients are of interest, so the mean is not informative. The standard deviation indicates considerable variation in the adjusted racial earnings gap across SEAs– on the order of 20 percentage points.

The relative supply of black workers in the SEA is measured as the proportion black among adult males (21 and over). The sign of the coefficient on proportion black should depend on the offsetting effects of crowding and upgrading on black wages, as discussed above. The mean of proportion black indicates that about a quarter of the adult male population was black in the average southern SEA. The spatial distribution of proportion black (by county) is illustrated in Map 4; it exhibits the expected

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[22] The excluded SEA is ICPSR SEA#1, consisting of the Alabama counties of Colbert, Lauderdale, Lawrence, Limestone, Madison, and Morgan.

concentration of blacks in the southern "black belt" areas. Comparing Maps 3 and 4, the spatial

correlation between the earnings gap and relative supply appears to be positive, but is far from perfect.

Two variables are included as measures of pro-segregationist or anti-black behavior by whites:

the number of votes for Strom Thurmond in the 1948 presidential election per white adult population, and

the number of black victims of known lynchings during the period 1910-1930 per 1000 black population.

Thurmond was the candidate of the explicitly segregationist "Dixiecrat" movement in the South (Key

1949, ch. 15). Heer (1959) showed that voting for Thurmond in South Carolina counties was correlated

with proportion black, which he interpreted as evidence of a racial threat effect. Because it is impossible

to distinguish white from black votes, I assume that only whites voted for Thurmond and use the white

adult population as a proxy for the number of white voters. The coefficients on both the Thurmond and

lynchings variables are expected to be positive if these aspects of racially motivated white behavior are

indicative of underlying attitudes and norms that spilled over into employment practices.

An additional variable that may proxy for local racial attitudes is the prevalence of plantations,

measured here as the proportion of farms worked by sharecroppers or share tenants. Virts (2006) suggests

that sharecropping is probably the best proxy for plantation agriculture for this period, although I obtain

similar results using plausible alternative measures, such as the value of the cotton crop per capita.

Plantation districts are expected to have exhibited greater discrimination, but they may also have been

subject to the temporary disequilibrium effects of federal agricultural policies noted above.

Finally, some basic controls for economic characteristics of the SEA are included, which are

intended to pick up some of the impact of the local job structure and competition on measured

discrimination: the proportion of population urban and the proportions of workers employed in

manufacturing and in agriculture. I use the census definition of urban places– namely places with

population at least 2500– but the results are not substantively different if an urban control for population

in places of 25,000 or greater is added.

Table 3 presents correlations between the various SEA-level variables used here. Notable are the

positive correlations between the racial earnings gap on the one hand and the proportion black, the

proportion of farms operated on shares, and the Thurmond vote on the other. These correlations suggest

the potential roles of relative supply, plantation institutions, and white racial attitudes in generating wage

inequality. But these variables are also correlated with one another. For example, comparing Map 4 with

Map 5, which shows the proportion of farms on shares, indicates unsurprisingly that the "black belt"

coincided to some degree with the "plantation belt." A multiple regression approach is needed to sort out

the distinct partial effects.

RESULTS

The second-stage regression results for 1940 are reported in Table 4. In the core specification,

presented in column (1), the coefficients on proportion black, proportion urban, sharecropping, and

proportion of votes for Thurmond are all positive and statistically significant. Among these variables, the

urbanization effect is strongest: a one standard deviation increase in proportion urban increases the

adjusted log racial wage gap by about .09, or nearly half a standard deviation of the dependent variable.

The effects of proportion black and sharecropping are more modest but still substantial: a one standard

deviation increase widens the wage gap by about .04 and .06 respectively. The results are not

substantively changed when the first-stage regression is restricted to nonfarm workers (not reported

here).[23]

As noted above, the coefficient on proportion black is likely to be biased due to endogenous

migration, which would be expected to mitigate the crowding effect on wage discrimination. Column (2)

reports the results of a reduced-form equation replacing the proportion black variable with the proportion

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[23] The core results presented in Table 4 are robust to a variety of alternative specifications. For example, adding variables for the proportion of population in larger cities (25,000 plus) and the value of the cotton crop per capita to provide a richer specification of urbanization and patterns of agriculture, does not alter the main results (neither of these additional variables is significant). In another specification, I interacted the Thurmond variable with state dummies for Alabama, Louisiana, Mississippi, and South Carolina, the states where Thurmond was listed on the ballot as the official candidate of the Democratic Party and might therefore have received more votes from party-line voters. This did not alter the basic results.

of the SEA population in 1860 who were slaves, which is highly correlated with proportion black in 1940 but should be exogenous to the 1940 wage gap. The coefficient on the slavery variable in column (2) is large and significant: a one standard deviation increase in the proportion slave in 1860 is associated with an increase in the 1940 racial wage gap of nearly one-half standard deviation.[24] Notably, the coefficient on the Thurmond vote in column (2) is much reduced and no longer statistically significant. When the slavery variable is used as an instrument for proportion black in 1940, the estimated coefficient on proportion black increases to 0.82.[25] One interpretation of these results is that endogeneity of the SEA racial composition variable may indeed bias the unadjusted coefficient estimate downward. Alternatively, the historical legacy of slavery may simply be a better proxy for the racial and institutional factors that contributed to a wider racial wage differential. This interpretation is suggested by the weakening of the Thurmond vote variable: Thurmond voters were presumably concentrated in the heart of the Confederacy, the former slave belt.

The positive effect of proportion black suggests that the job-market crowding and threat effects of increased relative supply of black workers in a local market dominated any offsetting tendency toward occupational upgrading. Of the two fairly direct proxies for racial attitudes– the Thurmond vote and lynching variables– only the Thurmond variable is significantly different from zero, and not in every specification. The significant coefficient on the plantation proxy (sharecropping), which is robust even after controlling for the overall importance of agriculture in the local economy, may be a better indication that local social relations, including white racial attitudes, affected black labor-market opportunity. Finally, the positive coefficient on urbanization is consistent with the hypothesis that the more heterogeneous structure of urban job markets created more opportunities for discrimination in job assignments.

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[24] The sample used in estimating column (2) is restricted to SEAs in which the counties with valid 1860 data make up at least half the SEA population in 1940. When the regression specification from column (1) is estimated for this restricted sample, the coefficient on proportion black in 1940 rises to 0.328.

[25] IV results are available from the author upon request. The 1860 slavery variable is a strong instrument, with an F statistic in the first-stage regression of 131.7.

Alternative specifications and samples

Column (3) repeats the specification of column (1) but adds a set of state dummies as a crude control for unmeasured factors across larger geographic units, such as the potentially more relaxed race relations in the border states. Unsurprisingly, inclusion of state dummies tends to weaken some of the estimated coefficients, but the results are overall qualitatively similar.

The results in column (4) attempt to correct for potential bias created by the federal minimum wage. As noted above, if the minimum wage were binding on workers of both races, it would tend to obscure the racial wage differential in low-wage markets. This could bias toward zero the estimated coefficients on any SEA characteristics that were correlated with both the racial wage gap and low wages overall. To address this problem, I have estimated the first-stage log earnings equation as a tobit, censoring all observations with weekly earnings at or below $10.50, a figure slightly above the average weekly earnings a full-time worker would have earned at the minimum wage in 1939, and just above the observed spike in the black earnings distribution in Figure 1a. In this case, the parameters of the earnings equation are estimated from the observations with earnings above the minimum, correcting for the potential bias due to censoring. The data are restricted to nonfarm workers, since agriculture was not generally covered by the minimum.[26] The results for the second stage using the tobit coefficients are presented in column (4) of Table 4. For comparison, column (5) reports the results for the same sample of SEAs using an OLS first-stage earnings regression on all wage observations. The tobit and OLS results are mostly similar, although using the tobit increases somewhat the estimated effect of proportion black and decreases the Thurmond vote coefficient.

Another potential source of bias in the results is the possibility that the SEA characteristics associated with a larger racial wage gap are merely picking up the indirect effect of unobserved worker productivity differences. For example, perhaps my controls for school quality fail to capture fully the

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[26] To achieve convergence of the tobit model required limiting the number of SEA dummies estimated, which I accomplished by restricting the sample to those SEAs with at least 25 observations of each race.

extent to which schooling for blacks was particularly bad in areas of concentrated black population. If unobserved school quality were driving the results, one would expect to find that the same variables found to influence the racial gap in earnings would also significantly affect the estimated *returns to schooling* across SEAs. In regression results not presented here, I find little evidence that this is the case.[27]

As a further check on the robustness of the results presented in Table 4, I have estimated the same model using census data from 1950, to make sure the 1940 results are not merely the product of an abnormal census year. The second-stage results are presented in Table 5. The sample is somewhat smaller than for 1940, because earnings and education information is available for only a subset of the 1950 IPUMS sample, so fewer SEAs meet the minimum sample threshold. In column (1), all the regressors that can be are updated to 1950 values (the exceptions being the Thurmond vote and lynching variables). The coefficient on proportion black is similar in magnitude to its value for the 1940 sample, although it is only marginally statistically significant (p-value of 0.09). The proportion urban coefficient is again positive, but smaller and not statistically significant. In comparison with the 1940 results, the impact of the sharecropping variable in 1950 is smaller and weakly significant. Sharecropping was in decline during the 1940s, spurred by the AAA and mechanization: between 1940 and 1950, the percentage of farms operating on shares in the average SEA in my samples fell from 27 to 20. But the former plantation districts apparently retained their tendency toward a larger wage gap. This can be seen in column (2) of Table 5, which replaces the 1950 values of the sharecropping variable with the 1940 values. Here, the estimated coefficient on sharecropping is quite similar to its value in Table 4 (positive and significant). Clearly the 1940 sharecropping variable proxies for some more persistent underlying characteristic of these plantation areas.

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[27] The racial gap in returns to schooling by SEA can be estimated by interacting years of schooling with SEA and race in the first-stage regression. The second-stage regression of these estimates on the regressors used in Table 4 fits quite poorly; only the Thurmond vote variable has a significant (positive) coefficient. Results are available from the author.

Variation in black and white wage levels across SEAs

The primary focus of this paper has been on the adjusted racial earnings *gap,* interpreted as a measure of labor-market discrimination. Of course the wage gap could be larger as a consequence of white wages being higher, black wages being lower, or some combination of the two. Using the first-stage regression results, we can examine geographic variation in the adjusted earnings *levels* of black and white workers, controlling for individual measured human capital and the racial wage gap. The estimated coefficient on a specific SEA dummy variable captures the black earnings level in that SEA relative to the excluded SEA category, while the analogous relative white earnings level is the sum of the coefficients on SEA and the SEA*white interaction.[28] It must be stressed that the earnings variable is nominal, so geographic variation in the wage levels could reflect local price differences as well as real wage differences. The results of second-stage regressions analogous to those in Table 4 but using the estimated SEA relative earnings levels are presented in Table 6.[29]

Comparing the effect of proportion black on the earnings levels of blacks versus whites, it can be seen that an increase in the relative supply of black workers tended to have little effect on black wages and to increase white wages. Urbanization depressed black wages, with no significant impact on white wages. For both races, earnings were negatively related to agricultural and manufacturing employment and to the lynching rate, but in each case the difference between the black and white coefficients was not large enough to have a significant impact on the racial wage gap in Table 4. These results suggest that the local factors correlated with general poverty, as measured by low wages for both whites and blacks, were not necessarily the same factors affecting the racial pay gap.

Columns (3) and (4) of the table replace the contemporaneous racial composition with the proportion slaves in 1860. Blacks residing in former slave areas earned lower wages in 1940, while whites

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[28] For convenience in obtaining the estimated standard errors for the white earnings levels I estimated the first-stage regression again with whites as the excluded category and used the SEA coefficients as estimates of the white wage levels.

[29] Estimates for nonfarm workers only were similar to those reported in Table 7.

in the same areas earned significantly higher wages. These results indicate that, *ceteris paribus*, white

workers could have earned more by moving into traditionally black areas, whereas black workers could

have earned more by leaving them. That migration within the South did not redistribute population in

such a way as to equalize returns for each race echoes the familiar North-South regional wage gap; but

obviously some of the important explanations for the latter, such as the general isolation of southern labor

markets (Wright 1986) or the competition that blacks faced from low-skilled European immigrants

(Collins 1997), are not plausible for the within-South case. Other impediments to mobility must have

been involved: perhaps poverty (capital constraints) or tastes for residential segregation. Clearly this issue

warrants further study.

CONCLUSIONS

Black workers in the South were paid substantially lower wages than their white counterparts,

even after controlling for a range of individual characteristics. Furthermore, this wage differential varied

considerably across location within the South. A robust and striking finding of this paper is that the racial

pay gap was substantially greater in local labor markets with a greater relative supply of black workers

(proportion black). This geographical pattern was apparently a long-run legacy of the geographical

distribution of slavery: the distribution of slaves per capita in 1860 is an even better predictor of the 1940

wage gap than is the contemporaneous distribution of blacks. The pay gap also tended to be greater in

more urbanized SEAs, in plantation districts, and in areas where whites revealed segregationist

preferences in the 1948 presidential election. Overall, the results are consistent with the picture that wage

discrimination was generated by restrictions on the labor-market opportunities open to blacks. In

historically black areas of the South, labor-market crowding then reinforced traditional racial norms and

white hostility, to the disadvantage of black workers.

At first glance the regression results seem to suggest a potential factor contributing to declining

discrimination in the South after 1940: out-migration of blacks reduced their relative numbers and thus

may have relieved some of the market and social pressures that exacerbated wage discrimination. The problem with such a hypothesis is the timing. The net migration of blacks out of the South was largely over by the mid-1960s, whereas rapid wage convergence within the South was a product of the post-1965 period. But perhaps the adjustments to the changes in relative supply that accompanied the Great Migration– particularly those relating to racial norms and perceptions of threat or competition– occurred with a lag.

Southern society confronted African-Americans with a web of social and economic restrictions. In such an environment, increased relative numbers could actually make matters worse by increasing the competition for limited opportunities. In another context, increased numbers might have offered an alternative avenue toward improvements in status through the exercise of political power; but this route was blocked until the advances in black political rights of the 1960s.

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Table 1. Proportion of individuals satisfying various first-stage sample restrictions, by race

	Black	White
Civilian job	.841	.838
Wage or salary worker	.659	.612
Worked at least 26 weeks in 1939	.750	.751
Positive earnings in 1939	.629	.604
Satisfies all four of above	.438	.409

Note: Based on 1940 IPUMS sample already satisfying the following: male, age 18-64, residing in the South, less than 12 years of schooling completed.
Source: Ruggles et al (2004).

Table 2. Variables used in second-stage (SEA level) regressions

Variable	N	Mean	Std. Dev.	Min	Max
White-black earnings gap relative to SEA #1, 1939, all workers[a]	168	-0.095	0.203	-0.725	0.548
White-black earnings gap relative to SEA #1, 1939, nonfarm workers[a]	168	-0.057	0.224	-0.997	0.626
Black wage level relative to SEA #1, 1939, all workers[a]	168	0.252	0.280	-0.722	1.137
White wage level relative to SEA #1, 1939, all workers[a]	168	0.156	0.212	-0.277	0.640
Proportion of adult (21+) males black, 1940[b]	168	0.236	0.151	0.007	0.718
Proportion of population slave, 1860[b]	140	0.336	0.196	0.005	0.835
Proportion of population urban, 1940[b]	168	0.350	0.249	0.000	1.000
Proportion of employed male workers in manufacturing, 1940[b]	168	0.156	0.136	0.008	0.671
Proportion of employed male workers in agriculture, 1940[b]	168	0.381	0.228	0.001	0.780
Proportion of farms worked by sharecropper or share tenant, 1940[b]	168	0.272	0.170	0	0.827
Votes for Thurmond in 1948, per white adult population in 1940[c]	168	0.083	0.089	0	0.348
Number of black victims of lynchings, 1910-1930, per 1000 black population in 1940[d]	168	0.063	0.092	0	0.498

Note: Statistics give equal weight to each SEA.
Sources:
a. Coefficient estimates from earnings regressions using Ruggles et al (2004).
b. Haines (2004).
c. Votes: Inter-university Consortium for Political and Social Research (1999), supplemented by McGillivray and Scammon (1994, pp. 742-746) for 38 Texas counties with missing data in ICPSR (of 254 total Texas counties in the data set). The latter source includes Thurmond votes in the "other" category. Statewide, 93.4% of "other" votes went to Thurmond, so I apply this percentage to the "other" total in each missing county to obtain estimated Thurmond votes.
Population, 1940: Haines (2004).
d. Lynchings: Historical American Lynching Data Collection Project (2005).
Population, 1940: Haines (2004).

Table 3. Pairwise correlations of variables used in second-stage (SEA level) regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(1) White-black earnings gap, all workers	1.00									
(2) White-black earnings gap, nonfarm workers	0.93	1.00								
(3) Prop. of adult males black 1940	0.39	0.31	1.00							
(4) Prop. of population slave 1860	0.46	0.36	0.86	1.00						
(5) Proportion urban	0.17	0.14	-0.08	-0.22	1.00					
(6) Prop. employed manufacturing	-0.01	-0.07	-0.00	-0.15	0.28	1.00				
(7) Prop. employed agriculture	0.03	0.05	0.18	0.29	-0.86	-0.43	1.00			
(8) Prop. of farms on share	0.30	0.28	0.33	0.40	-0.45	-0.22	0.68	1.00		
(9) Thurmond vote per adult white population	0.34	0.26	0.70	0.62	-0.10	0.01	0.17	0.19	1.00	
(10) Lynchings per 1000 black population	-0.04	-0.05	0.16	0.15	-0.29	-0.16	0.36	0.19	0.26	1.00

Sources: See Table 2.

Table 4. Racial earnings gap regressions, southern SEAs, 1940

Dependent variable: Estimated coefficient on interaction of SEA and white race from first-stage earnings regressions

	(1)	(2)	(3)	(4)	(5)
Proportion black among adult males, 1940	.241* (.119)		.274* (.136)	.522* (.168)	.318* (.160)
Proportion of population slaves, 1860		.442* (.092)			
Proportion urban, 1940	.379* (.037)	.386* (.112)	.265* (.106)	.299* (.110)	.302* (.107)
Proportion employed in manufacturing, 1940	.036 (.097)	.077 (.102)	-.054 (.113)	.062 (.112)	-.007 (.108)
Proportion employed in agriculture, 1940	.144 (.145)	.078 (.157)	.009 (.152)	-.149 (.172)	-.151 (.166)
Proportion of farms on share, 1940	.350* (.104)	.215* (.107)	.243* (.118)	.454* (.123)	.497* (.118)
Thurmond vote (1948) per adult white population	.465* (.186)	.189 (.175)	.455 (.257)	.322 (.213)	.604* (.201)
Lynchings of blacks (1910-1930) per 1000 black population	-.132 (.150)	-.106 (.151)	-.009 (.181)	.236 (.202)	.081 (.188)
State dummies?	no	no	yes	no	no
R-squared	.354	.421	.459	.432	.445
Observations	168	140	168	107	107

* = significant at 5% level.
Notes: Standard errors in parentheses. Weighted least squares regressions using inverse of estimated variance of coefficients from first stage as weights. First-stage regressions (results not reported here) regress log of average weekly earnings in 1939 on age, education, marital status, farm industry, state of birth, education interacted with state of birth, and SEA dummies, all interacted with race. Sample for column (2) restricted to SEAs with 1860 data on counties that accounted for at least half the 1940 SEA population. For column (3), DC, DE, and MD are combined into one state dummy due to small sample sizes. First-stage regressions for columns (4) and (5) use a sample restricted to nonfarm workers in SEAs with at least 25 workers of each race. First-stage earnings regression for column (5) is a tobit left-censoring observations to include only those with weekly earnings exceeding $10.50.
Sources: See Table 2.

Table 5. Racial earnings gap regressions, southern SEAs, 1950

Dependent variable: Coefficient on interaction of SEA and white race from first-stage earnings regression

	(1)	(2)
Proportion black among adult males, 1950	.290 (.169)	.411* (.159)
Proportion urban, 1950	.157 (.118)	.149 (.111)
Proportion employed in manufacturing, 1950	-.200 (.175)	-.192 (.169)
Proportion employed in agriculture, 1950	.185 (.252)	.068 (.232)
Proportion of farms on share, 1950	.225 (.134)	
Proportion of farms on share, 1940		.292* (.107)
Thurmond vote (1948) per adult white population	.646* (.218)	.591* (.207)
Lynchings of blacks (1910-1930) per 1000 black population	.233 (.198)	.283 (.192)
R-squared	.347	.384
Observations	138	137

* = significant at 5% level.
Notes: Standard errors in parentheses. Weighted least squares regressions using inverse of estimated variance of coefficients from first stage as weights. First-stage regression (results not reported here) regresses log of average weekly earnings in 1949 on age, education, marital status, farm industry, state of birth, education interacted with state of birth, and SEA dummies, all interacted with race. Proportion employed in manufacturing and in agriculture are for males and females.
Sources: Earnings regression estimated using Ruggles et al (2004); Thurmond vote and lynchings variables are same as those used for 1940 regressions; other variables derived from Haines (2004).

Table 6. Explaining variation in adjusted black and white earnings across southern SEAs, 1940

Dependent variable: Coefficient on SEA dummies from first-stage earnings regression, estimated separately by race

	Black (1)	White (2)	Black (3)	White (4)
Proportion black among adult males, 1940	-.021 (.136)	.247* (.090)		
Proportion of population slaves, 1860			-.270* (.107)	.177* (.076)
Proportion urban, 1940	-.397* (.115)	-.028 (.068)	-.371* (.132)	.009 (.086)
Proportion employed in manufacturing, 1940	-.366* (.115)	-.272* (.067)	-.371* (.122)	-.282* (.079)
Proportion employed in agriculture, 1940	-1.027* (.170)	-.849* (.101)	-.909* (.185)	-.793* (.122)
Proportion of farms on share, 1940	-.120 (.118)	.122 (.079)	.028 (.023)	.136 (.091)
Thurmond vote (1948) per adult white population	-.623* (.208)	-.227 (.152)	-.274 (.199)	-.142 (.155)
Lynchings of blacks (1910-1930) per 1000 black population	-.310 (.173)	-.459* (.108)	-.374* (.176)	-.523* (.116)
R-squared	.572	.744	.534	.720
Observations	168	168	140	140

* = significant at 5% level.
Notes: Standard errors in parentheses. Weighted least squares regressions using inverse of estimated variance of coefficients from first stage as weights. First-stage regressions (results not reported here) regress log of average weekly earnings in 1939 on age, education, marital status, farm industry, state of birth, education interacted with state of birth, and SEA dummies, all interacted with race. Samples for columns (3) and (4) restricted to SEAs with 1860 data on counties that accounted for at least half the 1940 SEA population.
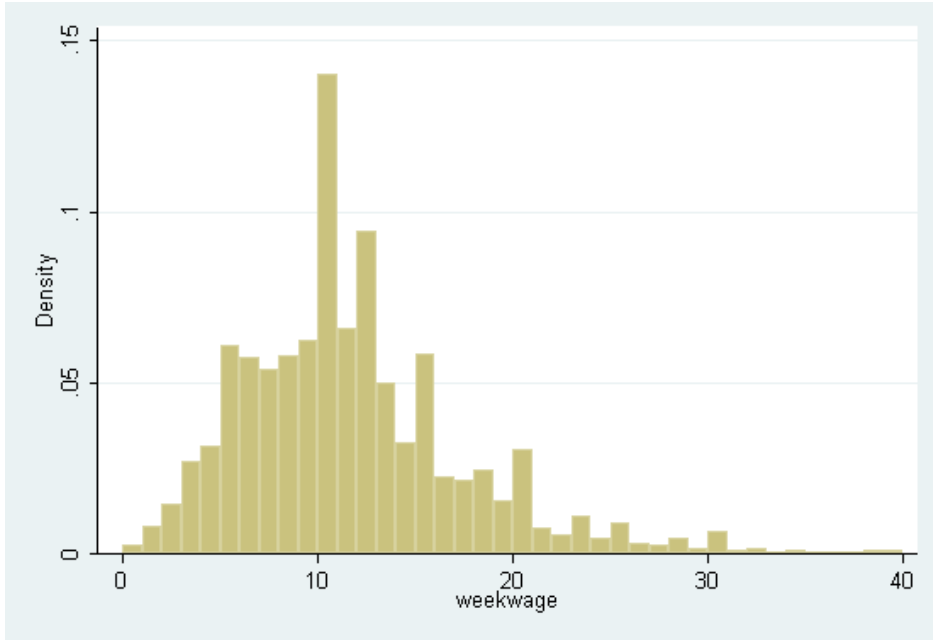*Sources:* See Table 2.

Figure 1a. Histogram of average weekly wage, black males in nonfarm industries in the South, 1939
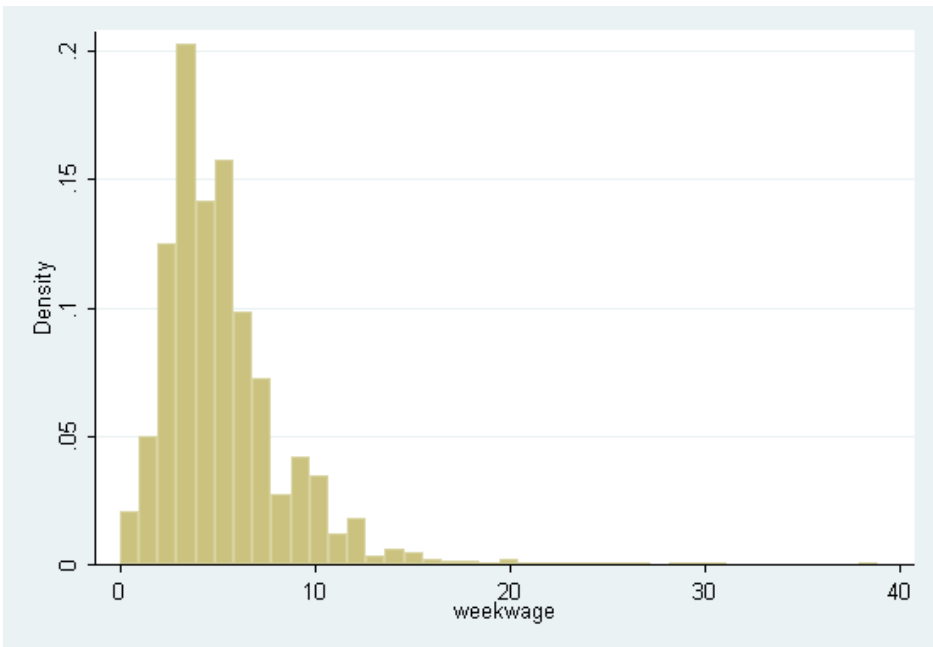

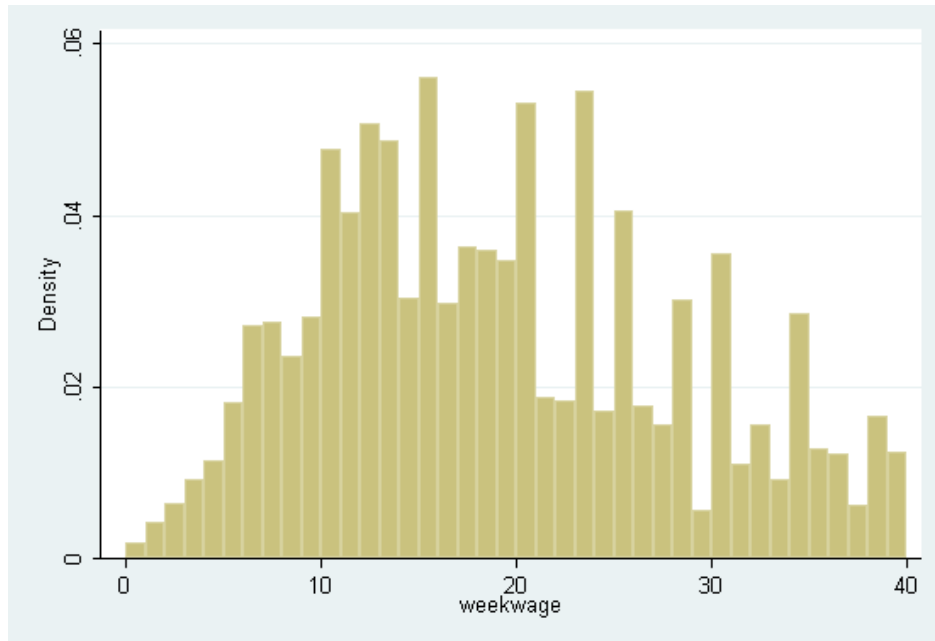Figure 1b. Histogram of average weekly wage, black males in farm industries in the South, 1939

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Figure 1c. Histogram of average weekly wage, white males in nonfarm industries in the South, 1939

Note: Samples restricted to workers with less than 12 years of schooling and average weekly earnings less than $40.
Source: Ruggles et al (2004).

Titles and source notes for Maps 1-5:

Map 1: White-black log earnings gap, 1939
Note: All counties in same SEA receive same shading. Unshaded areas are missing data.
Source: 1940 IPUMS sample of male workers (Ruggles et al 2004); see text.

Map 2: White-black schooling gap in years, 1940
Note: All counties in same SEA receive same shading. Unshaded areas are missing data.
Source: 1940 IPUMS sample of male workers (Ruggles et al 2004); see text.

Map 3: Adjusted white-black log earnings gap, 1939
Note: Residual racial earnings gap after controlling for individual characteristics (see text). All counties in same SEA receive same shading. Unshaded areas are missing data.
Source: 1940 IPUMS sample of male workers (Ruggles et al 2004); see text.

Map 4: Proportion black of adult males (21+), 1940
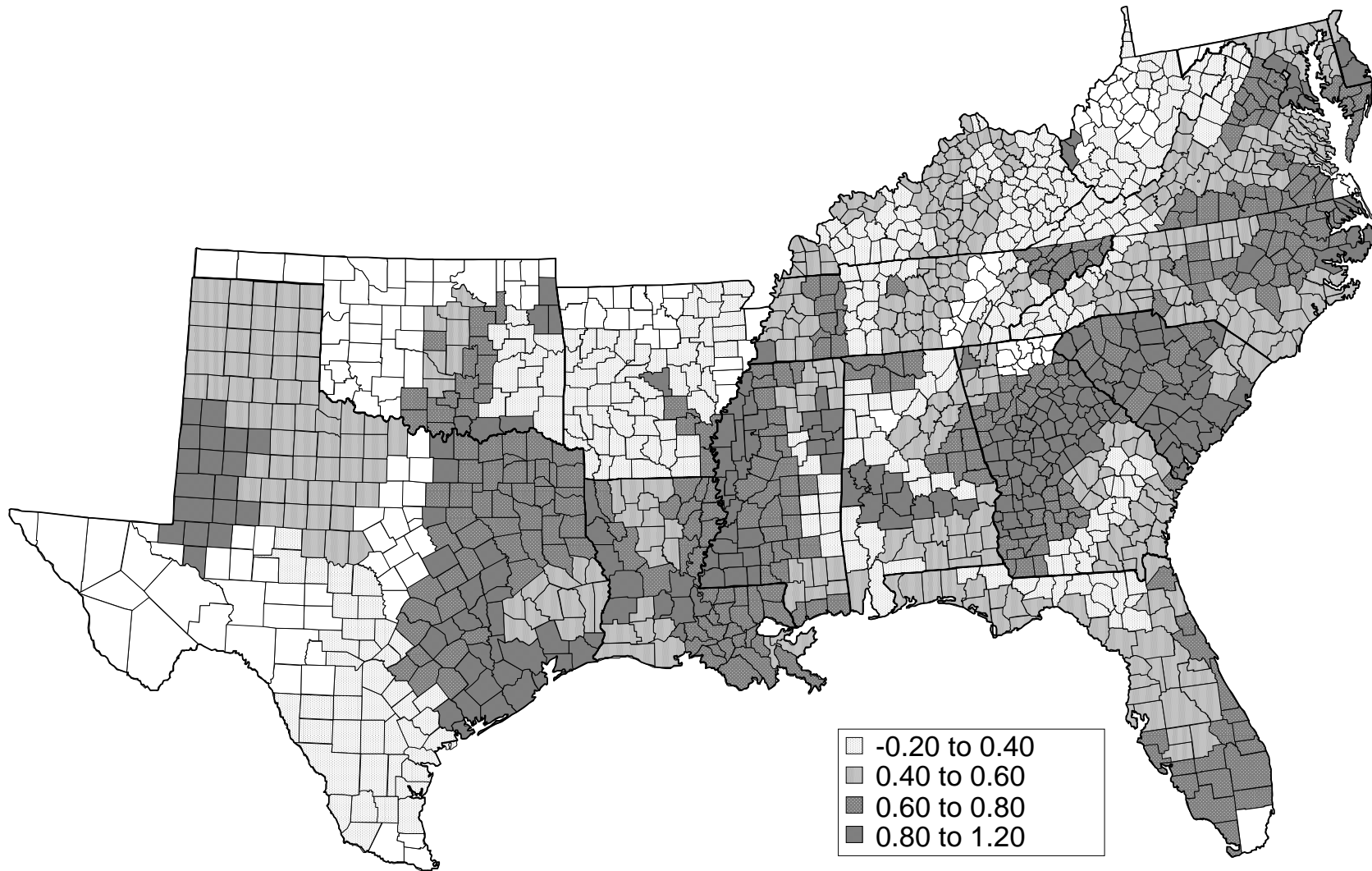*Note:* County-level data.
Source: Haines (2004).
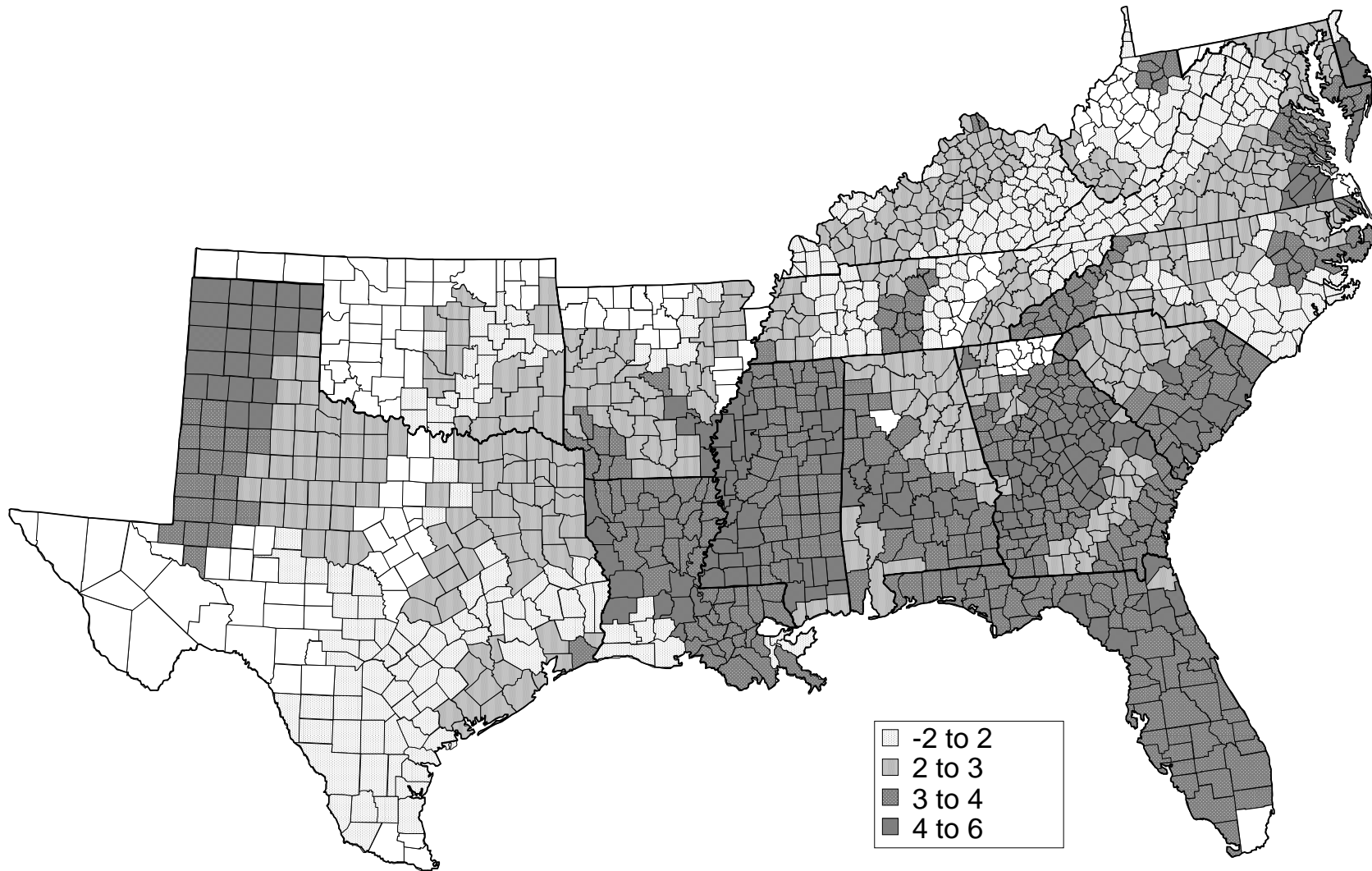
Map 5: Proportion of farms on shares, 1940
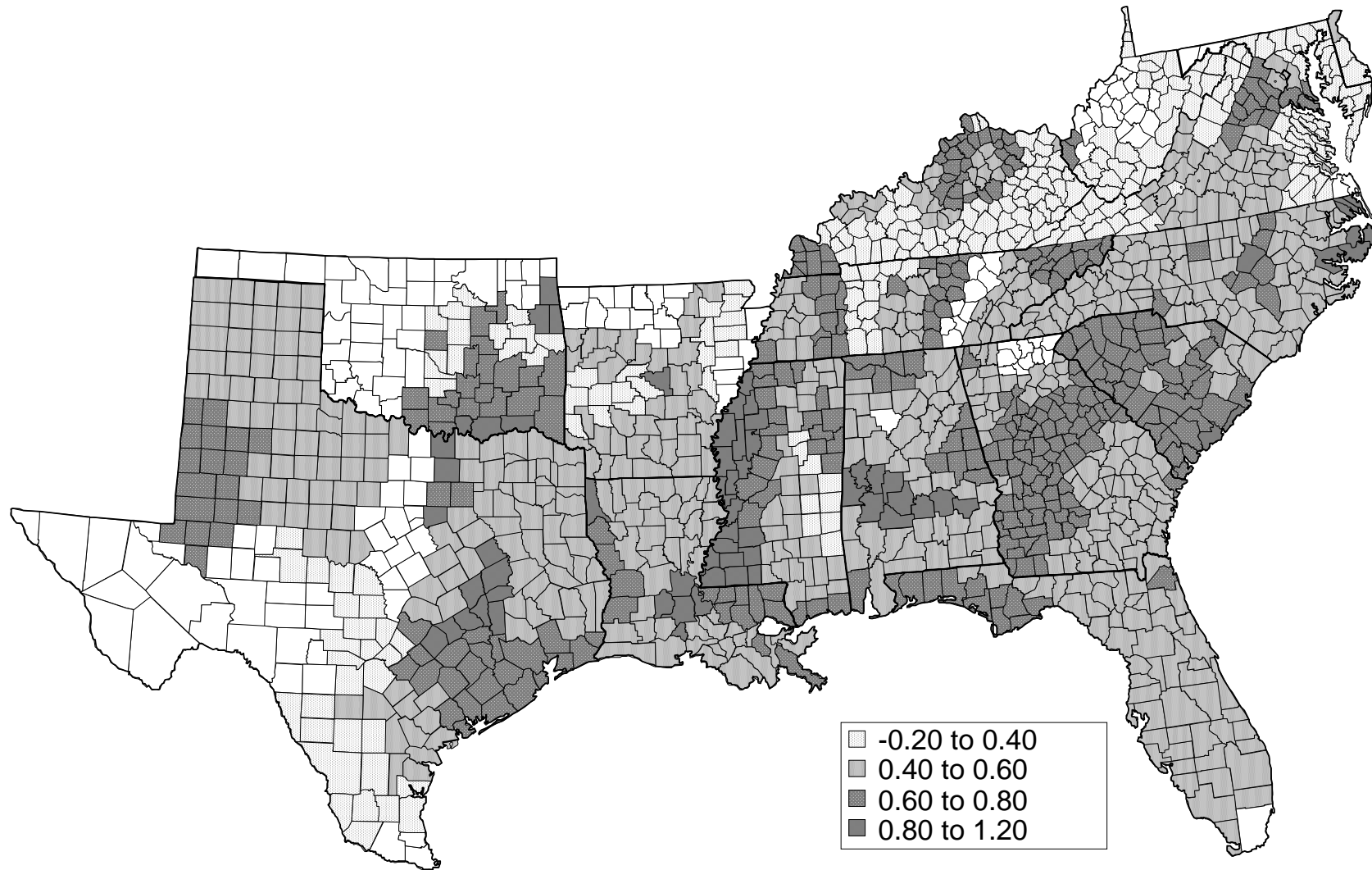*Note:* County-level data.
Source: Haines (2004).
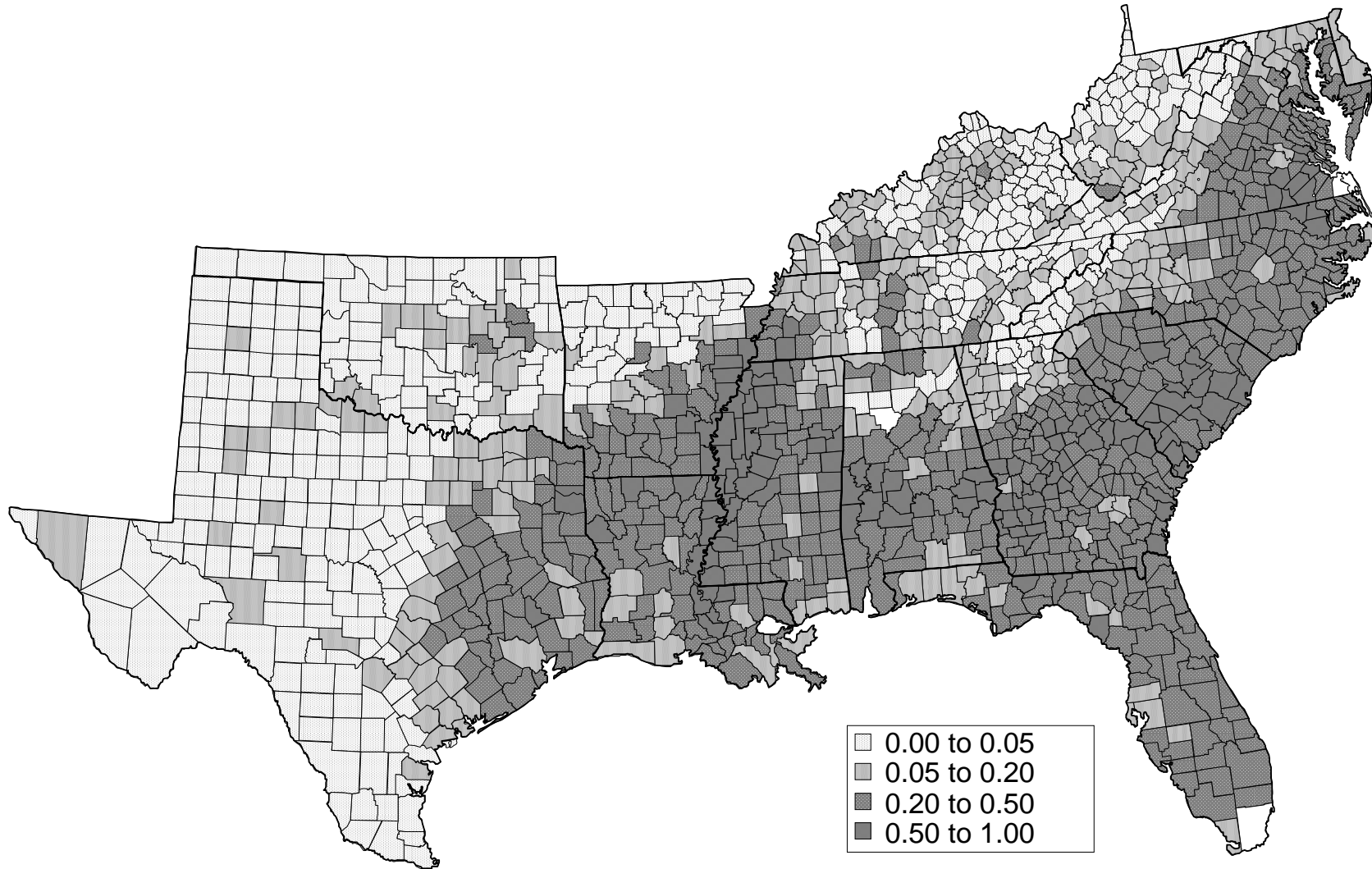
Map 1. White-black log earnings gap, 1939

Legend:
- -0.20 to 0.40
- 0.40 to 0.60
- 0.60 to 0.80
- 0.80 to 1.20

Map 2. White-black schooling gap in years, 1940

Legend:
- -2 to 2
- 2 to 3
- 3 to 4
- 4 to 6

Map 3. Adjusted white-black log earnings gap

Legend:
- -0.20 to 0.40
- 0.40 to 0.60
- 0.60 to 0.80
- 0.80 to 1.20

Map 4. Proportion black of adult males (21+), 1940

☐	0.00 to 0.05
▨	0.05 to 0.20
▨	0.20 to 0.50
▨	0.50 to 1.00

Map 5. Proportion of farms on shares, 1940

☐	0.00 to 0.15
☐	0.15 to 0.30
■	0.30 to 0.50
■	0.50 to 1.00