Volume Title: Race and Schooling in the South, 1880-1950: An Economic History

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Volume Publisher: University of Chicago Press, 1990

Volume ISBN: 0-226-50510-3

Volume URL: http://www.nber.org/books/marg90-1

Conference Date: n/a

Publication Date: January 1990

Chapter Title: Notes

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Chapter URL: http://www.nber.org/chapters/c8799

Chapter pages in book: (p. 135 - 150)

Notes

Chapter 1

1. A skeptic might blame the Great Depression for the stability in the earnings ratio; as is evident in Figure 1.1, the ratio declined slightly in the 1930s. However, no such explanation can account for the failure of the earnings ratio to rise in the 1920s.

2. My focus throughout is on men because comparable figures for women have not been calculated; based on an analysis of occupation statistics, however, Goldin (1990) suggests that the relative economic status of black women may have deteriorated before World War Two.

3. There is considerable debate over the proper interpretation of the increase in the earnings ratio from 1940 to 1980; a useful introduction to the issues are various chapters in Shulman and Darity (1989). There is little debate, however, that the "new" black middle class is much larger in size that the "old" black middle class of forty years ago, and that middle class blacks today are employed in a broader array of jobs than in the past. Relative improvements in black status slowed after 1975; see Heckman (1990, 243).

4. By contrast, the Great Depression was a negative shock, slowing the absorption of black labor into the nonfarm economy.

5. Here "educational attainment" means highest grade of school completed, or what is commonly meant by the phrase "years of schooling." The racial difference is defined to be the white mean minus the black mean. This difference is always positive for the cohorts analyzed by Smith (1984).

6. The coefficient of the schooling gap is −0.031 with a t-statistic of −6.51. The dependent variable is the cohort-specific black-white earnings ratio (Smith 1984, 695); the independent variables are the schooling gap, white minus black (687), plus dummy variables for census years (1900, 1920–1980; data for 1910 are not available, see Smith 1984, 693).

7. In Chapter 4, however, I analyze racial differences in wages paid to teachers, and in chapters 6 and 7 present some evidence on racial differences in earnings from the 1940 and 1950 public use samples.

8. Smith's (1984) earnings ratios, shown in Figure 1.1, were derived from employment categories. Smith assigned fixed weights (using 1970 census data on earnings) to the proportions of individuals in particular occupations. The sum of the weighted pro-

portions yields an index of "occupational status," and the black-to-white ratio of occupational status is Smith's estimate of the earnings ratio. Thus changes in Smith's proxy reflect shifts in the occupations held by black men relative to white men; shifts in the structure of wages are ignored.

9. A good example involves the analysis of region of employment in Chapter 7. In an earnings function analysis of racial differences, region would be an independent variable: other things held constant (e.g., age, educational attainment), blacks living in the South had lower wages. The earnings function approach fails to address an underlying question: if living in the South was so costly to blacks in terms of earnings, why didn't more blacks move to the North? Posing the question in this manner, as I do in Chapter 7, goes to the heart of the debate between the institutionalist and human capital models.

A limitation of analyses of employment rather than earnings is that effects of schooling on earnings within occupations are ignored. This is an important criticism, but until more earnings data are found for the early twentieth century, the criticism cannot be addressed.

10. See Heckman and Payner (1989) and Butler, Heckman, and Payner (1989).

Chapter 2

1. Slave artisans residing in southern cities and towns may be an exception to the general rule. According to the 1870 census manuscripts, fully 22 percent of urban black artisans were literate, compared with only 5.7 percent of rural southern blacks (Ransom and Sutch 1977). Slave artisans made many economic decisions on their own, frequently hiring themselves out.

2. Strictly speaking, cohort here refers to blacks born in the South in a certain time period *and* remaining in the South, not to all southern blacks born in the time period.

3. In 1910 and after, a person was counted as "in school" if he attended school for at least one day during the census year. A number of adjustments, however, had to be made to the census data for 1880–1900. A question comparable to the one in 1910 was included in the 1880 census form, but the results were not compiled. Instead, the published volumes include figures derived from reports of school superintendents. Attendance rates derived from school reports generally exceed those compiled by the census, because the school reports include a substantial amount of double-counting of enrollments. I devised a procedure to estimate black school attendance in 1880 from child literacy rates (see the notes to Table 2.2). Although I believe the estimates to be plausible, they should be interpreted with care. It is important to note that the census data refer to attendance at a school of any type—public or private, vocational, and so on. For the younger age groups, however, the vast amount of attendance took place in public schools, because private school enrollments were a small fraction of total enrollments.

4. In 1940 and 1950 the specific period was shorter in length than before 1940. Had the 1940 or 1950 census question been used consistently throughout the period, the increase over time in black school attendance rates would have been larger than as measured in Table 2.2; some black children who attended school in 1910 would not have been so counted had the 1940 (or 1950) question been asked.

5. The distinction between "years of schooling" in the literal sense of time spent in school, and "years of schooling" as a measure of grades completed (i.e., a certain level of knowledge) is discussed later in the chapter.

6. Using the procedure to estimate literacy rates in 1940 and 1950, had every black male attended 4.5 years of school ca. 1890, virtually all would have been classified as literate.

7. This explanation is similar to Goldin's (1990) analysis of female labor force participation and labor market experience. After World War Two the labor force participation rate of married women increased sharply, but average years of labor market experience among women in the labor force remained low. Average experience remained low by the addition to the labor force of women with little or no work experience since marriage.

8. White years-per-grade is biased upwards because grade includes foreign whites who were less educated than native whites. Thus census data also overstate schooling for whites.

9. Calculated from figures in Jones (1917, 7).

10. There were sixty-seven high schools, if Missouri and Kansas are included; see Jones (1917, 34).

11. Computed from U.S. Bureau of Education (1893).

12. "Plantation" agriculture was a term used by the U.S. Bureau of the Census (1916) to describe the situation in which a large number of farmers operated separate units under the direction of a single landlord. Plantation counties produced staple crops (such as cotton), had high labor-to-land ratios and little mechanization, high rates of tenancy, and were predominantly black.

13. One should not conclude, however, that the equal part of separate-but-equal was a real possibility in 1954. There were still large differences in expenditures on ancillary services, such as transportation, and the value of school capital per black child was far below that per white child.

14. I have focussed my attention on public elementary and secondary schools because that is where the majority of southerners, black or white, were educated during the first half of the twentieth century, and because comprehensive data on private schools are unavailable. For further discussion of the relative quality of black schools in the South during the segregation era, see Bond (1934, 1939), Harlan (1958), and especially Anderson (1987).

15. Racial differences in class sizes would be considerably larger if expressed on the basis of enrollment rather than average daily attendance.

16. See Center for Studies in Demography and Ecology (1980, 35). Fractional months were not retained in the public use sample; it appears that the answers were rounded up. Thus the estimated average months of school attendance are biased upward, but the degree of upward bias cannot be determined from the sample.

17. This was the procedure used in the 1940 census. Census clerks in Washington were instructed that, if the school attendance question was left blank, column 25 of the census form was to be checked. Then, if an "S" (for student) appeared in column 25, a "Yes" was to be entered in the school attendance column as well.

18. The census reached a similar conclusion; see U.S. Bureau of the Census (1918, 378).

Chapter 3

1. See Jones (1917), Bond (1934, 1939), Harlan (1958), Kousser (1974, 1980a), Pritchett (1985, 1986), and Anderson (1987).

2. The discussion in this section is woven together from a number of sources. The best historical account of disenfranchisement is Kousser (1974); see also Bloom (1987). Foner (1988) is a recent synthesis of scholarship on Reconstruction.

3. Other disenfranchisement methods included the "whites-only" primary and the "Australian" or secret ballot. The introduction of the secret ballot in Arkansas, which was especially effective in eliminating illiterates, prompted the following ditty (quoted in Kousser 1974, 54):

> The Australian Ballot works like a charm
> It makes them think and scratch,
> and when a Negro gets a ballot
> He certainly has his match.

4. "Decimate" here does not mean completely eliminate. There were areas in the ex-Confederate South in which blacks voted after disenfranchisement, such as Atlanta; see Dittmer (1977). Blacks were freer to vote in border states like Kentucky, and racial differences in per pupil expenditures were smaller in the border states than elsewhere in the South (Jones 1917).

5. Black turnout was declining before suffrage restrictions were enacted; see Kousser (1974, 242–43).

6. Another way around the obstacle was private support for white public schools. It was common in the late nineteenth century South for "patrons" (parents and other interested parties) to provide supplemental funds when public funds ran out. In this manner white parents could be sure that their "tax dollars" (donations) went to support schools for white children.

7. There is considerable debate over the racial incidence of school taxes in the South ca. 1910 (Smith 1973; Pritchett 1986). However, there is little question that, even if the black schools were still being subsidized by whites in 1910, the amount of the subsidy was less *after* disenfranchisement; see Kousser (1980a).

8. The effects of disenfranchisement on school spending in North Carolina have also been investigated; see the valuable studies by Kousser (1980a) and Pritchett (1985).

9. Calculated from figures in States of Louisiana (1879, 12–15).

10. A Louisiana "parish" is the same as a county in other states.

11. A regression was also estimated in which the interaction term was allowed to have a nonzero coefficient only in the sugar parishes in which Hair (1969) claimed blacks voted freely. Consistent with Hair's argument, the coefficient of this interaction term was positive. The level of statistical significance was somewhat lower, however, than in thé equation in the text, suggesting that, in the cotton parishes, the black voter was a real threat at the height of the Populist challenge.

12. Indeed, in light of scholarly discussion of the extent of the fraud (Hair 1969; Kousser 1974), the fact that the coefficient has the "correct sign," let alone approaches conventional levels of statistical significance, is strong evidence of a disenfranchisement effect.

13. Chapter 5 shows that the reasoning was self-serving, insofar as better black schools would have led to higher black attendance rates.

14. Quoted in U.S. Bureau of Education (1893, 1079). The superintendent got his wish. Except for Atlanta, blacks were fully disenfranchised in Georgia by 1908; see Kousser (1974).

15. A subsidy from whites to blacks would have been required because per capita black demand for education was lower than per capita white demand. Consider a school district that opted to open a white high school. According to a strict interpretation of separate-but-equal, the district would have been required to open a black high school, regardless of the level of per capita black demand (the results of Chapter 5 suggest, though, that the level of black demand would not be independent of the existence of the black school). As I discuss in Chapter 5, various legal loopholes emerged to get around the strict interpretation. The required subsidy would have been even larger if compensatory doctrine (unequal schools in favor of blacks) had been put into place to offset the effects of family background on black school attendance.

16. Part of the strategy of the National Association for the Advancement of Colored

People (NAACP) initially was to force an abandonment of separate-but-equal by convincing the South that it was too expensive to maintain, if strictly construed; see Chapter 5.

17. The source of this belief, aside from Myrdal's creed may have been a social norm that blacks were entitled to public school funds equal to what they paid in school taxes; see Kousser (1980a).

18. Of course, any given school district might decide to flout the law by not opening a black school but, as I discuss below, the community then risked losing its black residents, which had economic costs.

19. For example, state courts in the South consistently ruled against statutes requiring that school taxes be segregated, that is, taxes paid by blacks would go to black schools, taxes paid by whites to white schools; see Risen (1935). By gerrymandering school districts, however, it was frequently possible to get around the restriction, as happened in North Carolina; see Pritchett (1985).

20. Following the logic to its conclusion, however, foundation contributions would have stimulated more spending on the white schools as well; some of the contributions may have substituted for public funds that would have been spent on blacks anyway. Moreover, it would make no sense at all for school officials to accept matching grants unless they already were committed to supporting the black public schools. Thus, the existence of philanthropic contributions cannot answer Myrdal's paradox in its most basic form, although it can explain why improvements in the black schools took place.

21. Anderson (1987) demonstrates that many of the philanthropic officials were racist and their motives far from pure. But there is no doubt that philanthropic contributions expanded educational opportunities for southern black children.

23. To attract aid from the Rosenwald Foundation, for example, local matching funds would have to be raised. State agents mustered such support from local school officials.

Chapter 4

1. Because the length of the black school year declined relative to the white school year between 1890 and 1910 (Chapter 2), the decline in the black-to-white ratio of *monthly* teacher salaries was somewhat smaller (see Margo 1985).

2. A variation on this argument emphasizes racial differences in grades taught. Black teachers were disproportionately employed in the elementary grades, where salaries were lower than in high schools, even after accounting for education and experience.

3. Monthly salaries are used rather than annual salaries in order to control for variation across counties in the length of the school year. The purpose of pooling by sex is to increase the sample size, because the number of counties in both states are small. The number of observations in every state is less than the number of counties (or twice the number, in the Florida and Louisiana regressions), because of missing data or because a few counties hired only female teachers.

4. The additional regressions are reported in Margo (1984b, 314–15).

5. Variations in population density across counties may also capture variations in the cost of living, which could not be included in the regressions due to data limitations.

6. To correct for heteroscedasticity, each observation was weighted by the square root of the number of teachers to which the average salary refers.

7. This conclusion is sensitive to how qualifications are measured. White teachers

appear to have received higher returns to education and experience; see Margo (1984b, 315).

8. This data base is part of a larger one constructed for a project on the social and economic history of American teachers; see Perlmann and Margo (1989).

9. The racial difference in the age-salary profile is smaller for weekly earnings. Evidently, older black teachers worked significantly more weeks (were employed in school districts with longer than average school years) than younger black teachers.

10. The South Atlantic region consists of Delaware, Maryland, the District of Columbia, Virginia, West Virginia, North and South Carolina, Georgia, and Florida. States in the East South Central region are Alabama, Mississippi, Kentucky, and Tennessee. States in the West South Central region are Arkansas, Oklahoma, Louisiana, and Texas. The greater locational disparity in black salaries suggests that the market for black teachers may have been less efficient spatially than the market for white teachers. Additional support for this conclusion is obtained if dummy variables for states are substituted for the regional dummies in the regressions; the coefficient of variation of the black state dummies exceeded the coefficient of variation of the white state dummies.

11. I will return to this point in Chapter 5, when I examine the impact of separate-but-equal.

12. See Murray (1949, 63) for a list of cases through early 1948 and their outcomes.

13. This avenue was closed off, however, with the successful resolution of *Morris v. Little Rock* in 1945, which outlawed "rating systems" as a method of discriminating against black teachers; see Marshall (1947, 47).

14. The number of males employed in high schools also fell proportionately to the number of females; see Federal Security Agency, U.S. Office of Education (1951, 83).

15. Class sizes were computed from U.S. Department of Health, Education and Welfare (1954, 98). In the elementary grades the average class size (enrollment per member of the instructional staff) was 38 in 1942 and 37.3 in 1950. In the high schools the average class size was 25.7 in 1942 and 17.4 in 1950.

16. Total black enrollment in the elementary grades fell by 3 percent between 1942 and 1950, while total enrollment in high schools rose by 28 percent; see Federal Security Agency, U.S. Office of Education (1947, 113) and U.S. Department of Health, Education, and Welfare (1954, 98). An improvement in the educational qualifications of black teachers vis-a-vis white teachers, which was associated with the increase in black high school attendance, is also part of the demand story, and helps explain the rise in the black-white salary ratio. Between 1940 and 1950 the proportion of black teachers with 2 years or less of post–high school training fell by 14 percentage points, while the proportion who were college graduates or better increased by 30 percentage points; the corresponding figures for white teachers were 1 percentage point and 12 percentage points (Welch 1973, 43). The black regression coefficients in Table 6.3 imply that the premium (in logs) for up to 2 years of college was 0.171 and for 4 or more years was 0.566 (this lumps persons with 3 years into the "2 year or less category," which will bias upward the total importance of the changes in educational background). The corresponding figures for whites are 0.136 (2 years or less) and 0.40 (4 years or more). If these figures are used to weight the changes in educational background, the implied increase in the black-white salary ratio is 0.096 = (0.133 [blacks] − 0.034 [whites]), or 27 percent of the actual change from 1940 to 1950 (0.096/0.352).

17. In log terms, the increase in black salaries (in 1950 dollars) between 1942 and 1950 was 0.59. If $e = -1$, the log of the number of black teachers employed would have declined by 0.59 which, in 1950, would have corresponded to a reduction of 33,000 in instructional staff. If black enrollment had stayed constant (which, I admit,

is doubtful, because overcrowding would have been horrendous), the pupil-staff ratio would have been 58 (compared with an actual value of 32.1).

Chapter 5

1. My discussion of *Plessy v. Ferguson* draws heavily on Lofgren's (1987) monograph; see also Woodward (1955).

2. Notwithstanding its modern notoriety, there is debate whether *Plessy* was an unexceptional ruling for its time. The conventional wisdom is that there was little in Brown's opinion to excite contemporaries and the decision could have been foreseen by the participants (Lofgren 1987). Recent work by Morgan Kousser (1986, 1988a, 1988b), however, casts doubt on this view. The opinion was a product of its era: popular support for civil rights was at a low ebb in the late nineteenth century, and there was little pressure on the justices to vote in favor of *Plessy* although, in Kousser's view, there was legal precedent to do so.

3. Mangum (1940, 79–83) discusses the various state laws regarding racial segregation in public schools.

4. See Kousser (1980b) for an excellent discussion of *Cummins*.

5. At the time there was a private black high school operating in Augusta; see Kousser (1980b).

6. The import was less extreme because it is unclear whether knowledge of the decision was widespread among school boards. Further, as pointed out earlier, the number of black public high schools in the South increased anyway after World War One.

7. See also Schmidt (1982).

8. Lloyd Gaines, who had sought to enroll in law school, could not be located after the Supreme Court reached its decision, and never directly benefited; see Tushnet (1987, 70–77).

9. Not quite the right statistics, however. The very county and state data that permitted me to write this monograph would not have been sufficient; the right detail was at the school district level, which typically would have to have been collected on site, at great expense.

10. It was legally sound because of the Supreme Court's 1886 decision *Yick Wo v. Hopkins* which, in theory, linked unconstitutionality to the administration of racially based statutes; see Tushnet (1987, 25). It was cheaper because the equalization suits would have been brought in numerous localities, most very distant from NAACP headquarters in New York.

11. That is, as long as the Court was receptive to the argument that *de jure* segregation was morally wrong, there could be no defense of compensating inequalities.

12. I do not wish to imply that no further moral indictment is possible. A case could be made, for example, that separate-but-equal, strictly enforced, would have wasted resources (unnecessary duplication of school facilities); or, alternatively, would have required so large a redistribution of income from whites to blacks that it would not have been too costly to achieve (that is, there would have been strong incentives for whites to prevent the redistribution from taking place).

13. For a good discussion of the difficulties involved, see Hanushek (1986).

14. Orazem (1987, 714) makes a similar point.

15. For a contrary view, see Kiefer and Phillips (1988) and Donohue and Heckman (1989).

16. A recent paper by Card and Krueger (1990) persuasively demonstrates that long-term improvements in the quality of schooling (which they measure by class size,

the length of the school year, and similar variables) did appreciably raise the rate of return to schooling.

17. The major exception is Bond (1934). who conducted a number of pioneering studies of racial differences in educational achievement, but who did not provide a quantitative estimate of the impact of separate-but-equal.

18. As the state-level regressions in Chapter 2 showed, compulsory schooling laws had only a minor effect on black attendance rates; see also Landes and Solmon (1972). Orazem (1987) uses a household model in his study of separate-but-equal and Maryland test scores.

19. The variable refers to attendance in any type of school, not just public schools. But for the age group in question (5–16), the majority must have been enrolled in public elementary schools, because private school enrollments were a tiny fraction of total enrollments in the South in 1900, concentrated at the high school level (that is, older children).

20. Occupational status (available from the 1900 census tape) is constructed by attaching to each occupation a weight derived from 1950 income data (the higher the average income in the occupation, the higher the weight). Thus occupational status is a proxy for income.

21. The school variables were drawn from the published reports of state superintendents of four states in 1900: North and South Carolina, Alabama, and Florida (data for a fifth state, Texas, could not be successfully matched to the 1900 census sample). Observations for the four states from the 1900 census tape make up the sample analyzed.

22. Strictly speaking, positive coefficients could be expected only for children who were at the margin between attending and not attending school. For children already in school, it is possible that an improvement in average school characteristics could lower school attendance. This is not just a theoretical quibble. Black children in northern cities could complete more grades in less time than southern children, because the schools were better. Consequently attendance rates of older urban children were sometimes lower in the North (and in some southern cities). On black education in the North at the turn of the century, see the important recent book by Perlmann (1988).

23. Because most children attended one-teacher schools, variations in class size may capture variations in the availability of schooling, thus accounting for the insignificant coefficients of the school density variable.

24. Because the regression does not control for within-county differences in school characteristics, it is also likely that the urban dummy is partially capturing the positive effect of better urban schools on school attendance.

25. In performing the calculation using the black coefficients, I ignore the negative coefficient of school density (see Table 5.2). If the true impact of school density on black attendance were negative, the calculated impact would be biased upward, but this would simply reinforce the conclusion that racial differences in school characteristics do not explain all of the attendance gap.

26. The calculation is a "partial equilibrium" one. If the equal part of separate-but-equal had been enforced, someone would have had to pay for it. Because strict enforcement would have probably increased the white share of the school tax burden (see Chapter 3), white after-tax incomes would have fallen relative to black after-tax incomes, further lowering the racial attendance gap.

27. Racial differences in child characteristics and geographic location explain only a small amount of the racial attendance gap; see Margo (1987).

28. To estimate per capita incomes at the county level, I devised a procedure that applied fixed income weights from the 1950 census to urban, rural farm, and rural nonfarm population shares; see Margo (1986a, 795). Because of the errors inherent in this

procedure, I tested the sensitivity of the results by estimating regressions including urban and rural nonfarm population shares instead of the income variable. Urban incomes were higher on average than rural nonfarm incomes, so an increase in the urban share should have a larger positive effect on the literacy rate than an increase in the proportion urban. This turned out to be the case. The additional regressions can be found in Margo (1986a, 796).

29. The fixed effects estimator corresponds to a regression with dummy variables for counties and years. On the random effects estimator, see Maddala (1977).

30. School capital was typically valued at its historical production cost; privately owned buildings used as schools (e.g., churches) were frequently not included in the valuation. Consequently the insignificant coefficients may reflect measurement error.

31. Due to the difficulties of interpreting the coefficients of the school capital and percentage one-teacher schools variables (the coefficients are either the "wrong" sign or are statistically insignificant), the calculations do not equalize the value of school capital and the percentage one-teacher schools.

32. Margo (1986a, 799) reports a regression of state-level data in 1930 showing that, holding constant school characteristics and school attendance, child literacy was positively related to adult literacy.

33. Orazem (1987) also estimated regressions combining the black and white schools together (including a race dummy), with no substantive effect on the results.

34. Orazem (1987, 720) also estimated race-specific attendance equations, concluding that equalization of school inputs completely explained the racial attendance gap. It is likely that his omission of family background variables accounts for the larger impact of separate-but-equal than in my analysis of school attendance in Section 5.2 (which did control for family background).

35. The inclusion of dummy variables for counties corresponds to the fixed effects estimator of Section 5.3.

36. If the fixed effects coefficients are used, the average reduction is 27 percent.

Chapter 6

1. Figures for adult males can be constructed for the entire country from published census volumes (as in Smith 1984), but not for regions.

2. Over two-thirds of the black professionals were schoolteachers or clergymen in the 1940 sample. In 1930 there was one black teacher for every 194 southern blacks and one black clergyman for every 495 blacks; the corresponding ratio for lawyers was one for every 21,472 blacks (U.S. Bureau of the Census 1935, 292).

3. In the sample drawn from the 1940 census tape, 87 percent of the fifty-eight black managers labored in trade, financial and business services, or personal services. Only two were employed in manufacturing, compared with 12 percent of southern white managers. Eighty-one percent of the black managers were self-employed, and 79 percent of these worked in wholesale and retail trade. None of the non-self-employed black managers worked in southern manufacturing; 15 percent of non-self-employed white managers did.

4. See Goldin (1990) for an application of this index to measure employment segregation between men and women.

5. The indices would be invariant to the number of categories (below the one-digit level) if and only if the *signs* of the racial differences within categories were the same as the sign at the one-digit level. For example, if the white proportion with skilled blue-collar jobs exceeded the black proportion, the same would have to be true for every skilled trade. Since this is unlikely, the values of the indices are lower bounds.

6. Compared with Smith's (1984) indices, Becker's are based on fewer occupational categories, a broader age grouping, and are *not* cohort specific. However, Becker's index of relative black status in the North shows an *increase* before 1950, which suggests that regional differences were fundamental to the behavior of the aggregate black-white earnings ratio prior to World War Two.

7. Goldin (1990) shows that, prior to World War Two, many large firms had "color bars" prohibiting the employment of blacks in office work and other white-collar jobs.

8. Models of "statistical" discrimination help explain the persistence of social norms involving employment; see Starrett (1976), Lundberg and Startz (1983) and Akerlof (1985). In Lundberg and Startz's model, for example, there are two population groups, W and B; firms base hiring decisions on the expected productivities of workers which, in turn, are assumed to be a weighted average of individual characteristics (e.g., human capital) and the average characteristics of the worker's racial group. It is assumed that the weight attached by firms to group characteristics is greater for the B group than for the W group. Given this assumption, individual members of the B group have less incentive to invest in human capital than do members of W; and over time, the average level of human capital in the B group will be less than the average level in the W group (which, in turn, reinforces the initial beliefs of employers about the relative productivities of the two groups).

9. Roosevelt's order was in response to a threatened march on Washington to be led by A. Philip Randolph, president of the Brotherhood of Sleeping Car Porters, the purpose of which was to demand an end to hiring discrimination in defense plants; see Vatter (1985, 132).

10. Given the categorical nature of the dependent variable, a multinomial logit or probit model might be preferred to the linear probability model. The multinomial logit and probit models, however, are impractical because of the large sample sizes and number of estimations to be performed. I did, however, estimate certain regressions (the agricultural participation regressions) using binomial logit analysis, and all of the regressions were estimated using discriminant analysis (Amemiya 1981). Discriminant function estimates of multinomial logit parameters are biased, but the biases are typically small, and discriminant analysis is much cheaper than maximum likelihood. None of the substantive findings were affected. See also Heckman and Payner (1989), who use the linear probability model in their very similar analysis of racial differences in employment in South Carolina during the twentieth century.

11. Census region and urbanization are included because, as discussed in the text, the extent of the nonfarm economy in the South varied geographically. Marital status is included to control for the possibility (suggested by Wright 1986) that certain occupations (e.g., unskilled nonfarm labor) were avoided by married men because wages in the occupation were too low to support a family. Years of schooling (1940 and 1950) is highest grade completed. Because the census did not ask a separate question about highest grade *attended* in 1940 (the 1950 census did), it is possible that some persons in 1940 reported their highest grade attended instead of highest grade completed, thus overstating their completed schooling level. Experiments with the 1950 census sample indicate that the mean racial difference in years of schooling (for adult males) was only slightly larger if measured by highest grade completed than by highest grade attended. Thus the substantive conclusions of the chapter regarding the effects of schooling on labor market outcomes are unaffected by the use of highest grade completed (as measured by the census) in the 1940 and 1950 regressions.

12. In light of the cohort differences found in the previous section, it would be better to estimate age-specific regressions, rather than include age as an independent variable. Unfortunately, once the dependent variable is disaggregated into one-digit industry and occupation groups, the sample sizes are too small to disaggregate by age.

13. Let α_i be the coefficient of the race variable in, say, the ith occupation. The segregation index is $\Sigma|\alpha_i|$ (since $\alpha_i = b_i - w_i$, controlling for other factors).

14. The regression coefficients were used to predict race-specific values of the industry or occupational probabilities, the p_i's, given the particular values assumed for the independent variables (e.g., that the mean value of years of schooling was the same for whites and blacks).

15. In the 1940 and 1950 census samples, "teachers" were classified in the "professional services" industry, which accounts for black overrepresentation in both years. Because most of the teachers were employees of local governments, had they been correctly classified, blacks would have been more underrepresented in government employment than as measured by census data.

16. Because the 1940 and 1950 regressions use a more accurate measure of educational attainment, it is likely that the estimated increase in employment segregation is biased downward. The industry and occupation regressions for 1940 and 1950 were re-estimated with a proxy for literacy instead of years of schooling. A person was deemed literate if his educational attainment was greater than two years, and illiterate otherwise. The values of the segregation indices computed from the race coefficients of these regressions were 36.4 in 1940 and 35.9 in 1950, both exceeding the values in Table 6.5.

17. Bond (1939, 339–44) reported the results of standardized tests conducted in the 1920s showing that black third and sixth graders in Alabama and Louisiana scored a full grade below national norms, controlling for the age of the pupil; not controlling for age, the gap was three years, which is the basis for the adjustment in the text. A similar adjustment for school quality was made by Heckman and Payner (1989). A three-year adjustment is not restrictive; a larger adjustment, say five years, would not alter the conclusions.

18. Calculations equating white and black urbanization and regional population shares reached a similar conclusion.

19. "Earnings" here refer to wage and salary earnings; self-employment income is excluded. Consequently the regression sample excludes a fairly large portion of agricultural employment in both 1940 and 1950. Because the decline in agricultural wage labor among blacks during the 1940s was greater than the overall decline in agricultural employment (compare the means in Table 6.6 with Table 6.1), the earnings data overstate the improvement in relative black *incomes* (including self-employment income) in the South between 1940 and 1950.

20. The sectoral differences in earnings are not adjusted for nonwage benefits. Farm laborers, however, received more nonwage benefits (e.g., food, housing) than nonfarm workers. Adjusting for such benefits would reduce the size of the sectoral gap and therefore the importance of the sectoral shift. On sectoral wage gaps before 1950, see Alston and Hatton (1989) and Williamson and Lindert (1980).

21. The age range 25 to 64 was chosen because, for these cohorts, no adjustment for relative school quality is warranted; see note 22 in this chapter.

22. No adjustment for relative (black-to-white) school quality is appropriate because, for the birth cohorts included in the regressions (1876–80 to 1921–25), no improvement in quality took place for the average black member of the 1940 and 1950 samples; the average birth year of blacks in the 1940 sample was 1900 and in the 1950 sample it was 1909. However, the impact of changing racial differences in schooling is biased downward, because no adjustment is made for "ungraded school bias" (Chapter 2). The 1940 census understated the educational attainments of blacks born in the late nineteenth century; the bias is less in 1950 because these cohorts are a smaller share of the 1950 sample. If all blacks born before 1900 were educated in ungraded schools and the adjustment for ungraded school bias reduces mean educational attainment by two

years, then the reduction in racial differences in years of schooling between 1940 and 1950 would be -0.51 years (instead of -0.26 years; see Table 6.6). Thus adjusting for ungraded school bias among blacks would approximately double the explanatory power of changing racial differences in schooling (Panel C, Table 6.6). I regard this as an upper bound to the true adjustment for ungraded school bias because many southern whites, too, attended ungraded schools.

23. Freeman (1973) argued that educational discrimination slowed the narrowing of racial income differences by reducing the supply of black employers in the South, who would have hired black workers. The results of the chapter support Freeman's hypothesis.

24. Without the public use samples, it would have been impossible to calculate segregation indices controlling for racial differences in schooling.

25. The importance I attach to social norms may be overstated. It is possible that census samples for 1920 and 1930, were they available, would show that the increase in employment segregation was primarily a consequence of setbacks during the Great Depression. On the other hand, the data in Table 6.1 show that the lag in the black shift out of agriculture predated the 1930s (see Table 6.1).

Chapter 7

1. See Epstein (1918), U.S. Department of Labor (1919), Kennedy (1930), Kiser (1932), Vickery (1969), Henri (1975), Gill (1979), Johnson and Campbell (1981), Fligstein (1981), Gottlieb (1987), and Grossman (1989).

2. A major exception is the important study by Leiberson (1978), who applied forward survival techniques to aggregate census data from 1890 to 1950 to show that net migration of southern blacks was higher among literates than illiterates. My analysis differs from Leiberson's in two ways: the measure of migration I use is gross and the data refer. to individuals. Gross measures of migration are preferable to net measures because migration theory, economic or otherwise, refers to gross flows, not net. Because the data I use refer to individuals, it is possible to control simultaneously for characteristics other than schooling, which cannot be done with aggregate data.

3. Another possibility is that there may have been a greater census undercount of less-educated blacks in northern cities than in the rural South, but the differential would have to have been very large to account for the migration-schooling relationship.

4. Some persons migrated to attend high school or college in the North and ended up staying. Middle-class black parents in the upper South, for example, sent their teenage offspring to Chicago to attend high school; see Grossman (1989).

5. The variables for 1900 and 1910 are age, age squared, gender, literacy, family size, relationship to head of household, marital status, region of birth within the South, and whether the person's parents were interstate migrants. The last variable was included to test for migration of young children with families or for an intergenerational effect, but none was found. The variables for 1940 and 1950 are the same, except for the following differences. In 1940 and 1950 the schooling variables are years of schooling and years of schooling squared, and parents' interstate migration is excluded. In 1950, veteran status was added. The substantive results are not affected if family size, relationship to head of household, and marital status (which, for migrants, are observed after the move) are excluded.

6. Schooling could be a proxy for ability or some other unobserved characteristic ("motivation"), but the census samples, because they are cross sectional, cannot address this issue (but see Sec. 7.3).

7. For a contrary view, see Fligstein (1981).

8. Consider the calculation for 1940. Suppose that, for black schooling to equal white schooling (including the quality of schooling), the black schooling distribution (as in Table 7.2) would have had to be: 0–1 years of schooling, 3.4%; 2–4, 0%; 5–8, 20%; 9–12, 60%; ≥ 13, 16.6%. This corresponds to an increase in the mean years of schooling for blacks so that it equalled the mean for whites plus two additional years. Under this assumption, the predicted black migration rate in 1940 would be 28.1% (instead of 23.9; see Table 7.5).

9. The state dummies control for geographic factors that influenced the extent of the migration (e.g., distance to the North, literacy rates, percentage nonfarm employment, wage differentials, and so forth).

10. The regression specification can be criticized because it excludes schooling (although variations in black literacy rates would be captured in the state dummies). To address the criticism, I estimated a logit regression using a sample of southern-born adult blacks from the 1910 census tape, in which the lagged outmigration from the state of birth was included as an independent variable, as well as literacy. The logit coefficient of lagged migration implied a partial probability change of 0.88 (the dp/dx, analogous to the autoregressive coefficient of the state-level regression).

11. The coefficients of the year dummies are also of interest; in particular, the value of the 1930 dummy (0.043) indicates that slightly more than half of the migration between 1910 and 1930 was due to "trend" factors (e.g., schooling, falling costs of migration, etc.).

12. In fact, the coefficient of the lagged migration rate is insignificantly different from one. This implies that the long-run effect of a shock would be to raise permanently the proportion of black migrants (if the coefficient were less than one, the black migration rate would eventually return back to its original level).

13. On the other hand, if black schooling levels had been higher earlier in the century, the autoregression implies that more blacks would have left subsequently. The autoregressive coefficient can be used to calculate how big this "feedback" effect would have been. Suppose, for example, the literacy rate of adult black males in 1910 had equalled its 1940 value. According to Table 7.5, the 1910 migration rate would have then been 1.86 percentage points higher than its actual value. This increase, in turn, would have caused an increase in the migration rate in 1920, 1930, and 1940 (through the feedback effect). The total increase in the migration rate (between 1910 and 1940) can be shown to be 2.93 percentage points, or 34.1 percent (= 2.93/8.5) of the actual increase in the migration rate of adult males between 1910 and 1940 (see Table 7.2).

14. It is also consistent with "learning" by firms; after experimenting with black workers (because of a shock like a war) and finding the experiment to be successful, the firm permanently raises its demand for black labor in the future; see Whatley (1990).

15. The cost-of-living adjustments are described in the notes to Table 7.6. No adjustment is made for nonwage payments, which were quite common among farm laborers (Alston and Hatton 1989). Including these payments, however, would narrow further the regional difference in wages at the lowest schooling levels (because farm laborers were the least schooled occupation), and, hence, would have no effect on the substantive conclusions.

16. If data for all northern blacks were used (instead of data for migrants), the substantive contrasts in Table 7.6 would be unchanged. The North-South wage gap is usually calculated in this manner; see, for example, U.S. Commission on Civil Rights (1986).

17. In Margo (1988b), I specify and estimate an econometric model of the Great Migration, using individual-level data from the 1940 census tape. The model consists of three equations:

(1) $\ln w_m = X\beta_m + \varepsilon_m$

(2) $\ln w_n = X\beta_n + \varepsilon_n$

(3) $I^* = \mu(\ln w_m - \ln w_n) + Z\alpha + \delta$

Equations (1) and (2) are earnings functions for migrants and nonmigrants. Equation (3) is an index function: if $I^* > 0$, the person migrates; if $I^* < 0$, the person stays. The X's are variables that influence earnings, and the Z's are variables that influence the costs of migration. Schooling is included in both X and Z; that is, it has an impact on migration indirectly through its effects on earnings, and it has a direct impact by affecting the costs of migration (or the nonpecuniary benefits). The model is completed by assuming that the error terms (the ε's and δ) are joint normally distributed.

Equations (1)–(3) are estimated in a three-step procedure (see Maddala 1983). First, (1) and (2) are substituted into (3) and a reduced-form probit is estimated. The probit coefficients are used to calculate selectivity-bias correction terms, which are then added to (1) and (2) in a least squares regression (selectivity bias is present unless ε_m and ε_n are uncorrelated, which is highly unlikely). The regression coefficients are used to calculate the expected earnings gains of migration ($\ln w_m - \ln w_n$). Finally, the structural probit (e.q. [3]) is estimated, included the expected earnings gain.

The model was estimated using a sample of 1,147 southern-born black males (ages 14 and over), 284 of whom were migrants. The dependent variable of the earnings functions was the person's weekly wage. In principle, the model's parameters are identified as long as one variable in X is excluded from Z, but in practice, it proved necessary to include occupational and broad sectoral (manufacturing, agriculture) dummies in the earnings functions. In effect, the specification implies that black migrants would have worked in the same occupation and economic sector in both regions, a factually incorrect assumption for many migrants. However, the estimate of μ was insignificantly different from one, and the coefficient of schooling in the structural probit was positive and statistically significant. The higher were the expected earnings gains, the more likely a black would migrate; but, holding constant the expected earnings gains, schooling had a positive effect on the propensity to leave the South. These econometric results, then, are consistent with the conclusions reached in the text.

18. The decline in the earnings gap at the high school level is larger than that reported in U.S. Commission on Civil Rights (1986, 81), but the commission's study did not adjust for regional cost-of-living differences. Another interpretation of the decline is that skill differentials were higher in the South, and, indeed, had been since the antebellum period; see Margo and Villaflor (1987).

19. It is also possible that economic opportunities for better-educated blacks were greater in the North, in ways not fully reflected in earnings; for example, the chances of finding a blue- or white-collar job.

20. A large collection of responses appear in Foner and Lewis (1980, 259–81), from which the examples in the chapter are selected.

21. For example, in the 1940 sample used in Panel C of Table 7.7 below, the average educational attainment of non-southern-born blacks was 8.4 years, compared with 7 years among southern-born black migrants.

22. It is noteworthy that the proportion of black migrants in white-collar jobs exceeded the proportion of blacks in white-collar jobs in the South (see Chapter 6), which is consistent with the schooling-migration relationship.

23. In order to ensure a large enough sample of Recent migrants, the sample definition was extended to include all black males in the labor force (ages 14 and over); see Margo (1989). The percentage differentials were calculated from the logs of the variables, so the sample definition excludes persons who reported no wage and salary earnings in 1939, and who worked zero weeks.

24. Because there were over ten times as many Lifetime migrants as there were Recent migrants, the average earnings of black migrants (Lifetime and Recent) exceeded the average earnings of non-southern blacks. A similar overall difference (not shown) was found using the 1950 census sample.

25. Poorly educated non-southern blacks, too, were atypical of the population from which they were drawn. Their earnings may have been unusually low, biasing the comparison in favor of southern black migrants.

26. The black-white earnings and wage ratios are biased upwards because no adjustment has been made for the 1939 earnings limit (which was $5,000). Proportionately more whites earned above the limit than did blacks. For such adjustments, see U.S. Commission on Civil Rights (1986, 128).

27. The result is consistent with Becker (1957, 113), who found an increase in black-to-white occupational status in the North from 1910 to 1950.

28. According to the 1910 census sample (see Chapter 2), 60 percent of southern black males (ages 20–64) were literate. According to Higgs (1971, 424), every male immigrant group except for two, Portuguese and Turkish, had a higher literacy rate. Higgs's figures pertain to 1909, but the contrast would be larger if late nineteenth century data (e.g., 1880) were used.

29. They also had lower unemployment. A regression analysis of the 1910 census sample shows that, among foreign-born adult men, literacy in any language reduced the annual frequency of unemployment (the probability of experiencing unemployment in a year's time) by 5.6 percentage points (t-statistic $= 5.6$). The ability to speak English also reduced the unemployment frequency, but by a smaller amount, 2.3 percentage points (t-statistic $= 2.6$). The regression included age, city size, region of residence, years in the United States, marital status, and a nonwhite (i.e., Asian) dummy. If one-digit industry and occupation dummies are added, the literacy coefficient remains significantly negative, but the English-speaking coefficient no longer is so.

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