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DOES SCHOOL QUALITY MATTER? RETURNS TO EDUCATION AND THE CHARACTERISTICS OF
PUBLIC SCHOOLS IN THE UNITED STATES

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ABSTRACT

This paper estimates the effects of school quality -- measured by the pupil-teacher ratio, the average term length, and the relative pay of teachers -- on the rate of return to education for men born between 1920 and 1949. Using earnings data from the 1980 Census, we find that men who were educated in states with higher quality schools have a higher return to additional years of schooling, holding constant their current state of residence, their state of birth, the average return to education in the region where they currently reside, and other factors. A decrease in the pupil-teacher ratio from 30 to 25, for example, is associated with a 0.4 percentage point increase in the rate of return to education. The estimated relationship between the return to education and measures of school quality is similar for blacks and whites. Since improvements in school quality for black students were mainly driven by political and judicial pressures, we argue that the evidence for blacks reinforces a causal interpretation of the link between school quality and earnings. We also find that returns to schooling are higher for students educated in states with a higher fraction of female teachers, and in states with higher average teacher education. Holding constant school quality measures, however, we find no evidence that parental income or education affects state-level rates of return.

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Beginning with the highly influential Coleman Report (1966), researchers have found little, if any, association between the quality of schools and student achievement on standardized tests (see Hanushek (1986) for a recent survey). On the basis of these findings it is now widely argued that increases in public school funding have few important benefits for students. This conclusion, although currently politically popular, contradicts two other strands of evidence on the quality of schooling. On one hand, the small number of studies that have directly correlated school quality and earnings have found a significantly positive relationship between them (Morgan and Sirageldin (1968), Welch (1969), Johnson and Stafford (1973), Wachtel (1976)). On the other hand, a widely-cited series of studies has identified improvements in the relative quality of black schooling as an important factor in the narrowing of the black/white wage gap (Welch (1966, 1967, 1973a, 1973b), Freeman (1973), Smith and Welch (1989)).

There are several explanations for the conflicting evidence. Most studies of earnings and school quality focus on the correlation between school characteristics (typically per-capita expenditure) and the mean earnings of students educated in a school district. One can easily argue that family background variables affect both educational expenditures and labor market earnings. In this case, the correlation of school quality and earnings is potentially spurious.¹ From the opposite perspective, however, one can argue that test scores are an imperfect measure of school performance. Indeed, although earnings and test scores are correlated, they are by no means

¹An exception is Wachtel's (1976) study of the earnings of air force veterans, which controls for father's education.

identical.² The aspects of school training that affect subsequent labor market achievement (e.g., discipline) may be poorly measured by test scores. Furthermore, the relation between school quality and test scores measured when students are in the eighth or twelfth grade fails to capture any effects of school quality on subsequent schooling achievement.

This paper presents an extensive analysis of the relation between earnings and school quality for cohorts of men born between 1920 and 1949. We use the relatively large samples available from the 1980 Census to estimate rates of return to education by state-of-birth and cohort. We then relate rates of return to schooling to objective measures of school quality, including pupil-teacher ratios, relative wages of teachers, and the duration of the school term.³

Our procedures overcome at least some of the objections to earlier studies of earnings and school quality. First, our statistical models include unrestricted state-of-birth effects, and therefore control for any differences in the mean earnings of men born in different states. To the extent that differences in family background raise or lower earnings for all levels of schooling attainment, our estimated rates of return are purged of any effects of differential family background. Second, we control for systematic differences in the returns to education associated with an individual's current region of residence. We thereby eliminate relative supply or demand

²For example, the addition of test score information to the log-earnings regressions reported by Griliches and Mason (1972, Table 3) improves the explanatory power of their regressions by less than 0.5 percentage point.

³Our approach is conceptually similar to that of Behrman and Birdsall (1983), who relate the returns to schooling among young Brazilian men to the average years of education of teachers in each individual's region of residence.

effects that raise or lower the returns to education in different parts of the country. Finally, in much of our analysis we incorporate permanent state-specific effects in the return to education, and use only the within-state variation among birth cohorts to identify the effects of school quality on the returns to education.

The results indicate that there is substantial variation in the rate of return to education across individuals educated in different states and at different times. Much of this variation is related to differences in the quality of schooling. We find that rates of return are systematically higher for individuals who attended schools with lower pupil-teacher ratios and higher relative teacher salaries. For example, our estimates suggest that a decrease in the pupil-teacher ratio by 5 students is associated with a 0.4 percentage point increase in the rate of return to schooling. Similarly, a 10 percent increase in teachers' pay is associated with a 0.1 percentage point increase in the rate of return to schooling. We also find that returns are linked to higher education among teachers. Controlling for measures of school quality, however, we find no evidence that returns to education are related to the income or schooling levels of the parents' generation, or to the fraction of students who graduate from high school or college.

Our main results are obtained for samples of white men. In view of the remarkable growth in school quality for black students during the past 70 years, however, the effects of school quality are of particular interest for blacks.⁴ Comparative data for the segregated Southern school systems confirm

⁴See Welch (1967, 1973a, 1973b) and Freeman (1973), for example. Contemporary observers throughout the early 20th century consistently stressed improvements in the quality of education for blacks as a key to their economic progress. See, for example, Bond (1934).

that measures of school quality were significantly lower for black students in the 1920's and 1930's. By the early 1950's, however, most states had reduced or even eliminated the gap in measured quality. These rapid changes provide a unique and arguably exogenous experiment with which to evaluate the effects of school quality.⁵ Perhaps surprisingly, the measured relationship between school quality and returns to education is very similar for blacks and whites, providing more support for a causal interpretation of our estimates.

I. Empirical Framework

Our goal is to relate the returns to education earned by individuals educated in different states to the characteristics of the public school system during the time they attended school. To describe the empirical framework, it is useful to assume that individuals attend school in their state of birth. (Procedures that adjust for the interstate mobility of pre-school and school-age children are developed in section IIIId, below). Let y_{ijkc} represent the logarithm of weekly earnings for individual i , born in state j in cohort c and currently living in state k , and let E_{ijkc} represent the years of education attained by individual i . We assume that earnings are determined by an equation of the form:

$$(1) \quad y_{ijkc} = \delta_{jc} + \mu_{kc} + x_{ijkc} \beta_c + E_{ijkc} \cdot (\gamma_{jc} + \rho_{kc}) + \epsilon_{ijkc}$$

where δ_{jc} represents a cohort-specific effect for each state-of-birth, μ_{kc} represents a cohort-specific effect for each state-of-residence, x_{ijkc} represents a set of measured covariates (including years of labor market

⁵ Increases in spending on black education in the South during the 1940's were driven in part by the attempts of white legislators to defuse impending challenges to the 'separate but equal' doctrine. See Freeman (1976, Chapter 2), Margo (1990), and the descriptive chapters by Griffith (1969, pp. 658-659) and Kirk (1969, p. 1129).

experience and its square), and ϵ_{ijkc} represents a stochastic error term.⁶ Equation (1) assumes a linear specification of the return to education that consists of two additive components: a cohort-by-state-of-birth component (γ_{jc}), and a cohort-and-state-of-residence component (ρ_{kc}).⁷ These components allow observed rates of return to schooling to vary because of differences in the return to education across different labor markets (i.e., variation in ρ_{kc}), and because of differences in the rate of return to education earned by individuals in a given state-of-birth and cohort group in any labor market (i.e., variation in γ_{jc}).

Notice that by including an interaction between individuals' state of birth and education, and an interaction between individuals' current region of residence and education, the state-of-birth-specific contribution to the return to education is identified by individuals who are educated in one state and move to another region. It is the shift in the return to education attributable to schooling in a particular state that we seek to explain by differences in school quality across states and over time.

Specifically, we hypothesize that the state-of-birth component of the return to education depends on the quality of the education system in the state when the cohort attended school, on an aggregate cohort effect, and possibly on a set of state-specific effects that are invariant across cohorts:

⁶ Although there is a substantial literature on possible unobserved-variable (e.g., ability) bias for estimates of the return to education, we assume that unobserved variables are not correlated with years of education, conditional on the other variables in the equation. We make this assumption to keep the estimation tractable, and because we focus on school quality, which itself is an unobservable in most studies of the return to education.

⁷ In the empirical work reported in this paper we only allow rates of return to education to vary by current region of residence (indexed by r). We normalize the coefficients γ_{jc} and ρ_{rc} by assuming $\sum_r f_{rc} \rho_{rc} = 0$, where f_{rc} is the fraction of cohort c living in one of the 9 Census regions.

$$(2) \quad \gamma_{jc} = a_j + Q_{jc} b + d_c,$$

where Q_{jc} is a vector of measures of the quality of the education system in state j during the time that cohort c attended school. In this specification, any permanent differences in the returns to education, arising (for example) from differences in the distributions of ability across states, are absorbed by the state-of-birth effects, a_j , in (2).

Under these assumptions, the effects of a particular measure of education quality can be obtained in one step by estimating a conventional log-linear earnings function that includes state-of-birth dummies, state-of-residence dummies, interactions of region of residence with education, and interactions of education with the quality measures for state j and cohort c .⁸ Alternatively, one can proceed in two steps: first, estimate the average rate of return to education for individuals born in cohort c in state j , controlling for state-of-birth, state-of-residence, and any geographic differences in the return to education; and then use a second-stage regression to relate estimated rates of return to observed quality variables.

In this paper we employ both estimation strategies, although we concentrate on the two-step approach. A two-step procedure provides a convenient reduction of the data, and allows us to illustrate the diversity of returns to education and their relation to measures of school quality. It also facilitates a relatively simple correction for interstate mobility of pre-school children. Nevertheless, in a two-step procedure cohorts must be defined fairly broadly to obtain reliable estimates of the state- and cohort-specific returns to education. In the analysis below we use 10-year intervals

⁸ If the state-of-birth effects a_j in equation (2) are non-zero, the equation must also include interactions of education with state-of-birth dummies.

of births. This aggregation eliminates any within-cohort variation in either school-quality or rates of return to education, and may potentially limit our ability to estimate the effects of school quality on the returns to schooling. By comparison, the one-step estimates that we present have the advantage that cohort-groups are defined quite narrowly: by year-of-birth.

a. Functional Form

The assumption of a linear relation between schooling and (log) earnings is widely used in applied studies of earnings, and is often found to perform as well as or better than simple alternatives.⁹ However, most studies pool samples of individuals from different states and birth cohorts with no allowance for regional or cohort differences in returns. It is conceivable (and indeed roughly true) that the log earnings-schooling relation is approximately linear in pooled samples, but is non-linear for particular subsamples. It is also conceivable that changes in the quality of public schooling shift the returns to elementary or secondary education more (or less) than the returns to college. If so, then the specification of the return-to-education function should allow for kinks at 12 years of education.

In an effort to obtain some simple evidence on these issues we estimated a series of unrestricted earnings-schooling models on narrowly defined subsamples of individuals in the 1980 Census. These models include a complete set of dummy variables for 0 through 20 years of education, as well as controls for potential labor market experience, marital status, state of

⁹ Heckman and Polachek (1974), for example, test the conventional semi-logarithmic earnings-schooling model against other specifications by estimating the functional form parameter of a Box-Cox transformation. Hungerford and Solon (1987) present some evidence of non-linearities around the 12th and 16th years of completed education.

residence, and residence in an SMSA.¹⁰ Figure 1 graphs the estimated education coefficients for 9 of the sub-samples: three cohorts of white men born in Alabama or Georgia (1920-29, 1930-39, and 1940-49), three cohorts of white men born in California, and three cohorts of black men born in Alabama, Georgia, South Carolina, or Mississippi. Figure 2 graphs the estimated schooling coefficients (together with their standard error bounds) for national samples of white men in the three birth cohorts.

These figures illustrate three general findings. First, for a particular cohort and state-of-birth group, the earnings-education relation is approximately log-linear for levels of education above a minimum threshold. Although there is some evidence of a college graduation effect, departures from log-linearity above the threshold level are small. Second, the threshold education level varies widely across states and over time within states, being relatively lower for older cohorts and for individuals from states with lower average educational attainment, and lower for blacks than for whites. This phenomenon is evident even in the national samples in Figure 2: the threshold is at 2 years of education for whites born between 1920 and 1929, at 3 years of education for those born during 1930-39, and at 5 years for those born in 1940-49. Finally, the rate of return to education (for years of education above the threshold level) is higher for later cohorts: especially so for blacks.

¹⁰Specifically, the models include linear and quadratic terms in potential experience, a dummy variable for being married with spouse present, a dummy variable for residing in an SMSA, and unrestricted dummy variables for residency in each of the 50 states. Additionally, dummy variables indicating state of birth were included if the sample combined observations from more than one state. The models are estimated on subgroups of the sample described in Appendix B.

The positive correlation between the average educational attainment of a state-of-birth and cohort group and the threshold point in their return-to-education function led us to investigate the determinants of this threshold more carefully. For each of 13 larger states (or pairs of contiguous states) and each of three 10-year birth cohorts, we first estimated a non-parametric version of the return-to-education function (using 20 unrestricted dummy variables) and found the approximate threshold point in the return-to-education relation. We then compared this point to various percentiles of the education distribution in each state-cohort group. This comparison led us to a simple empirical relation: across different cohorts and states-of-birth, and different race groups, the threshold point corresponds approximately to the grade level attained by the second percentile of the education distribution of workers. For example, a simple linear regression of the estimated threshold point on the grade attained by the second percentile of the education distribution has an estimated coefficient of .88, with an estimated standard error of .13 and an R-squared of .57.¹¹

The approximate linearity of the returns functions in Figure 1 suggests that states and cohorts with higher returns to elementary and secondary schooling have higher returns to post-secondary education. Indeed, in the sub-sample of 13 states and 3 birth cohorts the earnings gap between individuals with 8 and 12 years of completed education is positively correlated with the earnings gap between individuals with 12 and 16 years of

¹¹ Further details of our investigation, including tabulations of the estimated threshold points and education percentiles, are available on request. The 13 state-groups include 11 individual states (California, New York, Ohio, Texas, Pennsylvania, Illinois, Michigan, New Jersey, Massachusetts, North Carolina, Virginia) and two pairs of states (Alabama/Georgia and Kentucky/Tennessee).

education. This suggests that improvements in the quality of elementary and secondary education increase the returns to post-secondary education as well as the returns to the first 12 years of schooling. We present further evidence on this question in section V, below.

Given the pattern of the non-parametric estimates of the return-to-education function for the larger states, in the remainder of the paper we concentrate on measuring the return to education for years of schooling above the second percentile of the education distribution of an individual's state-of-birth and cohort. Specifically, we replace an individual's completed education by

$$\max (E_{ijkc} - T_{jc}, 0),$$

where T_{jc} is the second percentile of the (race-specific) education distribution for men born in state j in cohort c . Estimates presented in Section V examine the effect of ignoring the minimum threshold and treating the earnings-schooling relationship as log-linear.

II. Measures of the Quality of Public Schooling

Since the late nineteenth century the U.S. Office of Education has regularly published a summary of the characteristics of the public school systems in each state. These data are available on a semi-annual basis from 1918 to 1958 in the Biennial Survey of Education, and annually since 1960 in the Digest of Education Statistics. The Office of Education tabulates the results of questionnaires sent to the state offices of education inquiring about state-wide enrollment, revenues, number of teaching positions, length of school term, average teacher salaries, and other variables. Prior to 1954,

data are available separately for the white and black school systems in states that operated segregated schools.

The Biennial Survey of Education is a rich source of information on the average characteristics of public schools in different states at different points in time. From the available data we have collected information on three main characteristics: the ratio of enrolled students to instructional staff in the state ("pupil-teacher ratio"), the average length of the school term ("term length"), and average annual teacher salaries.¹² We hypothesize that increases in term length increase the amount of material covered in a school year, and thereby increase the economic value of additional years of schooling. We similarly hypothesize that reductions in the pupil-teacher ratio improve the quality of classroom instruction and lead to higher returns for each year of completed education. Finally, we hypothesize that higher teacher salaries enable schools to attract and retain more-qualified teachers, leading to improved classroom instruction and higher returns to education.¹³

Several previous authors, including Morgan and Sirageldin (1968), Johnson and Stafford (1973), and Wachtel (1976), have used total expenditures per pupil as an index of school quality. We suspect that the quality of education is more directly linked to indexes of pupil-teacher ratios and teacher salaries than to total expenditures per pupil, and indeed this is suggested by the results in Welch (1966). Nevertheless, roughly 60 percent of total

¹²The data are described in detail in Appendix A.

¹³As a check on the quality of the salary data in the Biennial Survey, we compared average teacher wages for each state in the 1939-40 Biennial Survey to state-specific averages of annual salaries for teachers in the 1940 Census. We also compared average wages of teachers in the 1959-60 Biennial Survey to state-specific averages of annual earnings for teachers in the 1960 Census. In each case the correlation across states was over 0.95.

education expenditures are for instructional salaries.¹⁴ Since the per capita expenditure on instructional salaries is simply the ratio of the average teacher wage to the pupil-teacher ratio, differences in teacher salaries and pupil-teacher ratios account for much of the variation in total expenditures per pupil.

Given geographic differences in the cost of living and in the level of alternative wages available to potential teachers, it seems unlikely that the level of teacher wages is an adequate index of teacher quality in different states. We have therefore normalized teacher wages in each state by the level of average wages in the state. We use average weekly earnings of employees covered by the Social Security system to adjust wage rates from 1940 onward. Due to a paucity of state-wide wage data prior to 1940, we use a regional wage rate for workers on federal construction projects to normalize average teacher salaries. The comparison wage series are index-linked between 1940 and 1944 as described in Appendix A. In view of the changing coverage of the Social Security wage index, and given the necessity of index-linking disparate wage series, we prefer to remove the time trends in the average relative teacher salary in our sample period. We therefore divide the relative teacher wage for each state by the national average of this ratio in the same year. This second normalization eliminates any time-series variability in the average value of relative teacher salaries, while preserving the cross-sectional information on relative teacher wages at a point in time.

A summary of these three measures of school quality is presented in Table 1. We present average values of the quality measures by state for three

¹⁴ For example, in 1919-20 the proportion was 61 percent nationwide. It was 60 percent in 1939-40, 57 percent in 1969-70, and 56 percent in 1979-80. (Digest of Educational Statistics 1987 edition, Table 96, page 110).

different cohorts of students: those born between 1920 and 1929, those born between 1930 and 1939, and those born between 1940 and 1949. The averages for a cohort assume that each person attends public school for 12 years, and that the number of individuals born per year in any cohort is constant. The quality measures for each year of school attended are weighted equally. We have also computed the averages using individual-specific years of education for men born in these cohorts and obtained virtually identical averages.¹⁵

The data in Table 1 show substantial variation in education "quality" across states. For the 1920-29 birth cohort, pupil-teacher ratios range from 20 (in the Dakotas, Montana, and Wyoming) to over 35 (in Arkansas, Mississippi and North Carolina). Average term lengths for this cohort range from 139 days (in Mississippi) to over 180 days (in the mid-Atlantic states). Similarly, relative teacher wages range from 0.75 or lower (in many Southern states) to over 1.25 (in many Northeastern states). As one might expect, the interstate variation in our three measures of education quality is much lower for the later cohorts. This is particularly true of the term length variable, which falls in a very narrow range for the latest cohort.

The patterns of quality variation over time within individual states also vary widely. Most of the Southern states show uniform improvements in quality. Other states, such as Michigan and Missouri, show almost no change in the quality variables, while some states show declines in certain dimensions of quality. The differences are most pronounced for relative teacher wages. For example, teachers in Alabama and Georgia show strong

¹⁵The correlations across state and cohort observations between the average quality measures described in the text, and averages of individual-specific quality measures, based on individual-specific age and years of education, exceed 0.99.

relative wage gains, while teachers in Massachusetts, New Jersey, and New York show relative wage losses during the period.

III. Returns to Education by Cohort and State of Birth for White Males

In this section we present estimates of the average rates of return to education for white men born in the 48 mainland states and the District of Columbia between 1920 and 1949. We divide the samples of men born in these states into three 10-year birth cohorts, and estimate rates of return for 147 separate state and cohort groups. We then perform a second-stage analysis of the relation between rates of return to schooling and the measures of school quality in Table 1. We also explore the effects of additional characteristics of the school systems in each state, and contrast these to the effects of some simple measures of family background. Finally, we present the results of a simple correction for the "measurement error" induced by the interstate mobility of pre-school and school-age children.

a. Rates of Return to Education by State and Cohort

Our estimated rates of return to education are obtained from three cohort-specific regressions fitted to individual data on log weekly earnings for 1979. The data samples are taken from the 5 Percent Public Use A-Sample of the 1980 Census (see Appendix B for details). Following the specification of equation (1), the explanatory variables in each regression include a set of 50 indicator variables for an individual's current state of residence (μ_k), a set of 48 indicator variables for an individual's state of birth (δ_j), and controls for potential experience and its square, marital status, and residence within an SMSA. To control for any differences in the current rate

of return to education across different labor markets in the country, the models also include interactions between 9 current region-of-residence dummies and completed education.¹⁶ Finally, the models include state-of-birth-specific interactions with individual education, where, as described in Section I, individual education is modelled as the maximum of zero, and years of education over and above the years of schooling attained by the second percentile of the education distribution in an individual's state-of-birth and cohort. These interactions are interpreted as estimates of the rate of return to education for individuals from a particular cohort and state.

The estimated rates of return (times 100), together with their estimated standard errors, are presented in Table 2. The lower panel of the table reports the weighted means and standard deviations of the estimated returns across the 49 states, and the weighted correlations between the returns and cohort-specific quality measures. Despite the fact that the estimates are obtained from highly parameterized models (there are 158 explanatory variables in the regression equation for each cohort), the estimates are relatively precise, with standard errors in the range of .1 to .3 percent for most states. As the information in Figures 1 and 2 suggests, rates of return to education are much lower for older workers: 5.1 percent per year for the oldest cohort (age 50-59 in 1979) versus 7.4 percent for the youngest cohort

¹⁶The results are qualitatively unchanged if state of residence is used instead of region of residence. The estimated coefficients suggest that rates of return to education vary across regions of residence by as much as 2 percent per year. Returns are lowest in the Mountain and Pacific regions, and highest in the East-South Central and West-South Central regions. These patterns are consistent with those reported by Chiswick (1974) in an earlier analysis of the regional variation in returns to education.

(age 30-39 in 1979). The interstate dispersion in returns shows the opposite trend, being largest for the oldest cohort and smallest for the youngest.¹⁷

The correlations in the lower panel of Table 2 suggest that returns to education are significantly related to all three measures of school quality. The connection is illustrated in Figure 3, which plots the rate of return for each state-of-birth (for the 1920-29 cohort) against the relative teacher wage in the state. We have divided the states into three groups, based on the pupil-teacher ratio, and plotted states in each group with a different symbol. The pattern of the plot suggests that returns are higher, controlling for teacher wages, among states with lower pupil-teacher ratios.

A second illustration of the relation between school quality and returns is provided by Figure 4, which plots the change in the return to education between the 1920-29 and the 1940-49 cohort against the change in the pupil-teacher ratio for the same cohorts. Even within states, the figure suggests a systematic correlation between the return to schooling and the quality of the public schools.

b. Rates of Return and the Quality of Schools

Table 3 presents estimation results for a series of regression models fitted to the estimated rates of return presented in Table 2. The models are estimated by weighted least squares, using as weights the inverse sampling variances of the estimated returns.¹⁸ The first group of models, in columns

¹⁷The dispersion measures in Table 2 are adjusted to account for the fact that our estimated rates of return are measured with sampling error.

¹⁸An optimal second-stage estimation scheme should take account of the covariances between the estimated returns for different states. We have experimented with such a procedure and found few differences from the simpler weighting scheme described in the text. The reason for this is that the

(1) through (5), exclude any state-specific information other than the measured quality variables, while the second group, in columns (6) through (10), includes a set of 49 unrestricted state effects. The latter models are identified by changes in school quality that occurred over time within states.

The model in column (1) includes only dummy variables for the second and third cohorts. These two variables alone explain 71 percent of the (weighted) variance in the returns to education.¹⁹ The estimated coefficients show significantly higher returns for the later cohorts -- approximately 1.2 percent per decade. The three quality variables are introduced individually into the regression model in columns (2) through (4), and jointly in column (5). Individually, all three variables are strongly correlated with returns to education, with t-statistics of 3.3, 7.0, and 7.2 for the pupil-teacher ratio, term length, and the relative teacher wage, respectively. When the three quality variables are entered together, they are jointly significant, although the effects of term length and the pupil-teacher ratio are smaller and less precisely estimated, presumably as a result of the high degree of colinearity between the quality measures.

estimated returns by state-of-birth are "almost" independent. The only source of covariation between them arises from the fact that the same regression parameters are used to adjust for other control variables in the first-stage regression.

¹⁹The R-squared coefficients in row 6 can be used to form chi-squared test statistics for the hypothesis that the included explanatory variables explain all of the variation in state and cohort returns. The transformation is $(1-R^2)*TSS$, where TSS represents the weighted sum of squares of the dependent variable. For the sample in Table 3, TSS=4014.8. The resulting test statistic is distributed as chi-squared with degrees of freedom equal to the number of estimated returns (147) minus the number of estimated parameters. For example, the chi-squared test statistic for the model in column (1) is 1164, with 144 degrees of freedom. On the other hand, the chi-squared test statistic for the model in column (10) is 165.5, with 93 degrees of freedom.

The models with state-specific effects lead to broadly similar conclusions as the models in columns (1)-(5), although the estimated coefficients of the pupil-teacher ratio are larger in absolute value when state effects are included, and the estimated effects of the other two quality measures are attenuated. When the three quality variables are included jointly (in column (10)) the estimated coefficient of the term length variable falls to zero. Evidently, only two dimensions of school quality can be identified in the data once state-specific effects are included.²⁰

The quality variables have a sizeable impact on the return to education. For example, based on the estimate in Column 10, a decrease in the pupil-teacher ratio of 6 students (which is the nationwide change experienced from 1966 to 1986) is associated with a .56 percentage point increase in the return to education for years of schooling above the threshold level. If the threshold level of education is the eighth grade, this reduction in the pupil-teacher ratio would lead to about a 4.5 percent increase in the relative earnings of college graduates.

Despite the significance of the quality variables, they explain relatively little of the inter-cohort trend in returns to education. Comparing the models in columns (5) and (1), for example, the quality measures explain only about 12 percent of the increased return to education between the earliest and the middle cohorts, and 6 percent of the increase between the

²⁰One possible difficulty with the term length variable is that teachers would prefer a shorter term. This suggests that teacher quality may decline with term length, holding constant teacher wages. We have re-estimated the models in Table 3 using the teacher wage expressed in terms of days worked per year (using term length as the measure of days worked) instead of annual salary. This change has the effect of raising the coefficient on the term length variable by about 0.5, with little or no effect on the other coefficients in the model. Even with this adjustment, the term length effects in column (10) are insignificantly different from zero.

middle and latest cohorts. In the models with state effects the school quality variables explain a larger share of the inter-cohort trend in the return to education: about 20 percent of the increase in returns between the 1920-29 cohort and the 1930-39 cohort, and about 10 percent of the increase between the 1930-39 cohort and the 1940-49 cohort. Nevertheless, the cohort dummies are highly significant, and their omission would lead to a substantial overstatement of the effects of the quality variables.²¹

Of course, the higher rate of return for younger cohorts is not necessarily a consequence of higher quality education. If there is any relation between age and the returns to education, the dummies in Table 3 confound age and cohort effects. To provide some evidence on the relative importance of cohort and age effects, we used the 1970 Census to estimate rates of return to education in 1970 for two of the same birth cohorts. Based on a simple log-linear regression model with controls for linear and quadratic experience and state of residence, we estimated the following rates of return by cohort and time period:²²

<u>Cohort</u>	<u>Year</u>	
	<u>1970</u>	<u>1980</u>
1920-29	6.73	5.04
1930-39	7.44	6.25

If we assume that the 1930-39 cohort had as good (or only slightly better) quality schooling than the earlier cohort, these data indicate an

²¹For example, if the cohort dummies are excluded from the model in column (10) the coefficient of the pupil-teacher ratio is -50.1, with a standard error of 3.7.

²²To the extent possible, we have used similar sample definitions and similar variable definitions in our analysis of the 1970 and 1980 Censuses. Details of the estimation results are available on request.

overall decline in the average return to education between 1970 and 1980 of about 0.50 percent (comparing the 6.73 percent return of the 1920-29 cohort in 1970 to the 6.25 percent return of the 1930-39 cohort in 1980). Using this estimate of the period effects, the implied age effects indicate a 1.2 percent decline in the return to education between age 40-49 and age 50-59, and a 0.70 percent decline between age 30-39 and 40-49. Thus, most of the increase in returns to education for later cohorts documented in Table 3 is probably attributable to aging effects.

Finally, we note that the estimated state effects in Table 3 are also highly significant. For example, a comparison of the models in columns (1) and (6) leads to a chi-squared statistic of 963 for the joint significance of the state effects (with 48 degrees of freedom). A similar comparison of the models in columns (5) and (10) leads to a test statistic of 642. These results suggest that some important determinants of the return to education are missing from our analysis. Examination of the estimated state effects indicates that returns to education are relatively low (controlling for measured quality) for men born in the South and in the North-Central/North-West regions (Montana, the Dakotas, Oregon, Washington, Idaho), and relatively high in the Midwest and Northeast.

A finding of relatively low returns for white men in the Southern states may be somewhat surprising, given that the quality measures in our analysis refer to the entire school system in each state. States that operated segregated school systems before 1954 typically had lower pupil-teacher ratios, longer term lengths, and higher teacher salaries for white students than black students (see section IV, below). As a result, average quality measures based on total student enrollment probably understate the quality of

the schools for white students in the segregated states. Nevertheless, when a dummy variable for the segregated states is added to the model in column (5) of Table 3, it has an estimated coefficient of $-.41$ (with a standard error of 0.13). Furthermore, when the segregated states are stratified into those with 20 percent or higher black enrollment, and those with less than 20 percent black enrollment, the returns to education are even lower in the states with higher black enrollments (0.42 percent versus 0.31 percent lower in states with less than 20 percent black enrollment). These findings are not consistent with a simple mis-measurement hypothesis for the quality of white schools in the South. Rather, they suggest that other dimensions of quality, or characteristics of the state such as the degree of urbanization, affect the returns to education for individuals from the South.

c. Family Background and Additional Aspects of School Quality

In view of the substantial unexplained differences in the returns to education both within and across states, we have explored the effects of several other school and state-level characteristics on the returns to education. Table 4 summarizes our main findings. In each case we have included our three basic measures of school quality, as well as state-specific fixed effects. To preview the results, we find that the estimated coefficients of the school quality variables are largely unaffected by the addition of controls for other characteristics, including characteristics of teachers, average income in the state, and characteristics of private schools.

Columns 1-3 of Table 4 address the effect of family background characteristics on the return to education. A number of previous studies (including the Coleman Report (1964)) have found a strong association between family background factors, such as parental education and income, and student

performance on standardized tests. If these family background characteristics are correlated with school quality, and if these characteristics change substantially over time within states (so they are not partialled-out by the state fixed-effects), our estimates of the effect of school quality may be confounded by the effect of family background variables.

Although the Census lacks direct information on the education of individuals' parents, we can at least partially control for differences in parental education by including the median level of education among adults who lived in the state when the men in our sample attended school (row 4). Likewise, we include the log of real per capita income in the state at the time the cohorts in our sample entered school (row 5).²³ Regardless of whether they are included separately or jointly, each of these variables has a relatively small and statistically insignificant effect on the return to education. Moreover, the estimated effects of the three main school quality variables (pupil-teacher ratio, term length, and relative teacher wage) are unaffected by the inclusion of these family background variables.

Teacher Characteristics

Columns 4-6 explore the role of teacher characteristics on the returns to schooling. The fraction of male teachers is included because, holding constant the level of teacher salaries, one might expect the quality of the teaching staff to vary with the fraction of male teachers. For example, assuming that female teachers were paid less than otherwise identical males during the period 1926-1966, one can view the percentage of male teachers as a proxy for lower quality teachers. Alternatively, one can view the fraction of male teachers as an indicator of higher non-wage compensation or better

²³ These variables are described in greater detail in Appendix A.

working conditions within the schools, as would be necessary to attract more men into the teaching profession in a given state, holding constant wages.

Perhaps surprisingly, the results indicate that an increase in the fraction of teachers in the state who are male has a substantial negative impact on students' return to education. An increase in the fraction of male teachers from 19 to 42 percent, which is the range observed across states in 1966, is associated with a 0.77 percentage point lower return to years of education above the threshold.

Columns 5 and 6 add the mean years of education of teachers in the state to the regression equation.²⁴ The estimated coefficient of mean teacher education is positive and statistically significant, whereas the estimated effect of teachers' experience is negligible. Notice that the pupil-teacher ratio and relative teacher wage continue to be significant determinants of the return to education when these teacher quality variables are included; in fact, their estimated coefficients are hardly affected by the addition of the teacher quality variables.²⁵ Furthermore, the addition of controls for the average education and experience of teachers hardly changes the estimated coefficient of the fraction of male teachers. Whether the fraction of male teachers influence the return to education because male teachers are less

²⁴The Biennial Survey does not contain information on the training of teachers. Consequently, as described in Appendix A, we derived these variables from the 1940, 1950 and 1960 Censuses.

²⁵In results not reported here, we have also included the truancy rate, defined as one minus the ratio of average daily attendance to average daily enrollment. This variable had an insignificant effect, perhaps because it is measured with substantial error as many schools are slow to administratively eliminate dropouts from the list of enrolled students. In addition, the correctly measured truancy rate may have two opposing effects that cancel out: a higher truancy rate reduces the effective pupil-teacher ratio for students in attendance, but the effective length of the school term is shorter for those who are absent.

effective teachers, or through some other channel, is difficult to ascertain.

Estimates of the high school completion rate and the college completion rate for each state-of-birth and cohort group are included in the models in columns (8) and (9). These variables are added to control for biases that may arise as more schooling is acquired by a higher fraction of a given cohort. For example, suppose that more schools are built in a state, leading to a decrease in the pupil/teacher ratio and a decrease in the travel time for students. Suppose further that individuals differ in their expected returns to education, and that as more high schools are built, some individuals who previously dropped out after 8th grade acquire more education. In this case, one might expect increases in school quality to be correlated with lower returns to education, reflecting a negative correlation between the average rate of return to education and the fraction of individuals with higher education. In our data there is a strong positive correlation (both in cross-section and within particular states over time) between average educational attainment and measures of education quality.²⁶ Therefore, if rates of return vary systematically across the population, and if individuals with higher expected returns choose more schooling, then there is a possible downward bias in estimates of the effect of schooling quality on returns to education. This can be controlled in part by including measures of the fraction of individuals at higher education levels in each cohort.²⁷

²⁶ For example, across our 3 cohorts and 49 states of birth the correlation between the fraction who completed high school and the pupil-teacher ratio is -.71.

²⁷ Similar biases are widely acknowledged to make interstate comparisons of standardized test scores, such as the SAT, very difficult. For example, data for 1982 on average SAT scores and the fraction of high school graduates taking the test show a strong negative association across states. See Digest of Education Statistics 1987 edition, Table 82, page 95.

Perhaps surprisingly, neither the high school graduation rate nor the college graduation rate has a statistically significant effect on the return to education. Moreover, the high school graduation rate has a negative coefficient, while the college graduation rate has a small, positive coefficient. These results provide little evidence of sorting among students into higher education on the basis of different expected returns to education.

Private Schools

All of the previous estimates have measured school quality by the characteristics of the public school system. Not all students attend public schools, however. During the period 1920-1960, the fraction of students enrolled in private schools grew from 7.5 percent to 13.6 percent.²⁸ The variation is even greater in a cross section of states: in 1938, for example, the share of private enrollments ranged from less than 2 percent in many Southern states to over 20 percent in New Hampshire and Rhode Island. The presence of private schools introduces two potential sources of unobserved variation in school quality. First, private schools may be more or less effective than public schools.²⁹ Second, private schools may have different staffing levels, teacher salaries, and term lengths than the public schools.

²⁸Most private school students attend Catholic schools. For example, in 1938 the fraction of private school students in Catholic schools was 89.5 percent. In 1956 this figure was 90.9.

²⁹Coleman, Hoffer and Kilgore (1982) present data on standardized test scores that indicate higher achievement levels among students in private (mainly Catholic) schools. The interpretation of these data is an issue of some dispute: see Goldberger and Cain (1982), Murnane (1984), and San Segundo (1987).

In an effort to examine these issues, we collected information on private school enrollments and the pupil-teacher ratio in Catholic schools.³⁰

Evidence on the effects of accounting for private school enrollment is presented in columns (9) and (10) of Table 4. In column (9) we include the fraction of students enrolled in all private schools as an additional explanatory variable for the rate of return to education. Controlling for pupil-teacher ratios, term length, and relative teacher salaries in the public schools, the effect of higher private school enrollments is numerically small and statistically insignificant. These results suggest that increases in private school enrollment do not by themselves affect returns to education.

The specification in column (10) is an attempt to directly measure the biases created by using data for the public schools to proxy pupil-teacher ratios for the state as a whole. The average pupil-teacher ratio is a weighted sum of ratios in the public and private systems. Hence, the measurement error in using the public school ratio as a proxy for the overall state average is the product of the fraction of enrollment in private schools and the gap between the private and public school ratios.³¹ An estimate of this error component is included in the model in column (10). As predicted by a naive model of attenuation bias, the addition of this control variable raises the estimated coefficient of the pupil-teacher ratio. Furthermore, the

³⁰As described in Appendix A, the Biennial Survey contains information on private schools in some years. Information on the number of private school teachers, however, is limited to Catholic schools.

³¹Let p represent the pupil-teacher ratio for all students, let p_1 represent the ratio in the public school system, let p_2 represent the ratio in the private school system, and let f represent the fraction of enrollment in private schools. Then $p = (1-f) \cdot p_1 + f \cdot p_2$. Hence

$$p - p_1 = f \cdot (p_2 - p_1).$$

estimated coefficient of the error component is (roughly) equal to the estimated coefficient of the pupil-teacher ratio in the public schools. Nevertheless, the relatively modest changes in the estimated effect of the public school quality variables suggest that the biases introduced by measuring only the quality of public schools are small.

Thus far, we have proceeded by considering extensions to the basic quality variables (family background, teacher quality, private schools) individually. In column 11 we jointly include several of the additional variables. With the exception of the term length variable, the school quality variables have their expected signs and are statistically significant. In contrast, the variables measuring family background characteristics, student educational achievement, and private school attendance generally have insignificant and small effects. The data seem to accord a greater role to school quality than to other variables in determining the return to education.

d. Adjustments for Mobility of Pre-school and School-age Children

Interstate mobility of pre-school and school-age children introduces a problem similar to measurement error in the interpretation of the returns to education for individuals born in a particular state. To proceed, it is useful to concentrate on a single cohort. Let γ_j represent the estimated rate of return to education for individuals born in state j (in a particular cohort), and let γ_s^* represent the rate of return for individuals educated in state s .³² Finally, let p_{js} represent the probability that an individual attended school in state s , given that he was born in state j . Then

³²Of course, some children are educated in more than one state. We abstract from this difficulty.

$$\gamma_j = \sum_s \gamma_s^* \cdot P_{js}$$

If we define P as a matrix whose j,s element is p_{js} , then the vector of coefficients γ is related to the vector of true returns γ^* by $\gamma = P \gamma^*$. Given estimates of γ and P , one can obtain an estimate of γ^* by $\gamma^* = P^{-1} \gamma$. Notice that if individuals are always educated in their state of birth, then P is an identity matrix and $\gamma^* = \gamma$.

We obtained an estimate of the matrix P by cross-tabulating state-of-birth with current state-of-residence for white children age 6-12 (both male and female) in the Public Use Sample of the 1940 Census. The average probability that a 6-12 year old is living in his or her state of birth is around 90 percent, although this probability ranges from 62 percent for children born in the District of Columbia to 94 percent for children born in North Carolina and Pennsylvania. In principle, this estimate of the matrix P is only appropriate for children born between 1928 and 1934, and only for those with 1-6 years of schooling. Nonetheless, we used this transition matrix to transform the estimated rates of return for each of the 3 birth cohorts into estimates of the rate of return for attending school in different states. We then re-estimated the regression models in Table 3, using the corrected rates of return as dependent variables. The results are presented in Table 5.³³

The results are qualitatively similar to those in Table 3. The correction has the effect of expanding the standard deviation of the estimated

³³We also used the estimated P matrix to obtain estimated sampling variances for the corrected returns. These sampling variances are used to weight the regressions in Table 5.

returns by 10 percent.³⁴ As a consequence, the magnitudes of the estimated coefficients are typically 5 to 15 percent larger than in the uncorrected model, although the associated standard errors rise by roughly the same proportion. On balance, the results suggest that corrections for interstate mobility have a relatively minor impact on the qualitative and quantitative conclusions in Table 5. This reflects the relatively low mobility rates of pre-school and school-age children, and the absence of a strong connection between interstate mobility and the geographic pattern of the measured quality of education.

IV. Returns to Education by Cohort and State of Birth for Black Males

The quality of schooling available to blacks born between 1920 and 1949 varied even more widely across states than that for whites. In the early part of this century, black students in the segregated Southern states attended overcrowded schools staffed by poorly qualified teachers.³⁵ By comparison, most Northern states allowed black students to attend regular schools, often far superior to those attended by Southern whites.³⁶ During the 1940's, however, there was a remarkable surge in the absolute and relative quality of

³⁴The correction actually affects the estimated returns for the oldest cohort more than for the youngest cohort. The ratio of the corrected to the uncorrected standard deviation of estimated returns is 1.19 for the 1920-29 birth cohort, 1.11 for the 1930-39 birth cohort, and 1.08 for the 1940-49 cohort.

³⁵The U.S. Office of Education published an impressive series of monographs throughout the early 20th century documenting the poor quality of black schools: for example, Jones (1917). See also the classic work by Bond (1934).

³⁶Prior to World War II, some Northern states still operated segregated schools. For example, in 1938 over one-half of black high school students in Illinois and Indiana attended segregated schools (see the Biennial Survey of Education 1936-38 Edition, Chapter IV, Table 5).

black schools in the South. These changes have been attributed to the legal campaign to equalize teacher salaries initiated by the NAACP in 1936, and to the growing political influence of blacks in the Southern states (see, for example, Bullock, 1967). In any case, interstate differences in the quality of black schooling, and in the rate of change of the quality of black schools, reflected forces far beyond simple differences in family background. It is therefore useful to use differences in the characteristics of schools for black students as a check on the inferences we have drawn from the relation between school quality and returns to education for whites.

Although we have little direct information on the quality of schools for blacks educated in the North, the Biennial Survey provides relatively complete data for those who attended school in the South. Tables 6a and 6b illustrate the extent of racial inequality in school characteristics for students educated in the 18 states with segregated schools. These tables present average quality measures for three cohorts of men, assuming that each person attended six years of elementary school. The discrepancy between quality measures for white and black students is readily apparent. For example, in Mississippi blacks born between 1920 and 1929 attended schools with an average of 50 students per teacher, while whites attended schools with an average of 32 students per teacher. In most states the length of the school term and the level of teacher pay were also substantially lower in black schools during the 1920's and 1930's. Nevertheless, in some Southern states (West Virginia, Kentucky, District of Columbia) the gap in quality between white and black students was relatively small.

By the time the 1940-49 birth cohort attended school, the gap in the quality of education between white and black students had diminished, even in

the deep South. For example, in South Carolina the average school term for the 1920-29 cohort of blacks was only 60 percent as long as the corresponding school term for whites. In the 1953-54 school year (the last year for which data are available by race in the Biennial Survey) the white and black schools in South Carolina met for averages of 180 and 179 days, respectively. Similar equalization of teacher salaries and pupil-teacher ratios is evident for most states by the early 1950's.³⁷

Following the procedures outlined in Section III, we used data from the 1980 Census to estimate rates of return to education for blacks born in the 49 mainland states in each of three 10 year birth cohorts (1920-29, 1930-39, 1940-49). As for white men, these returns were estimated in three cohort-specific regressions, including state-of-birth dummy variables, state-of-residence dummy variables, controls for experience, marital status, and residence in an SMSA, and interactions of education with current region of residence. The returns to education were estimated by interacting education and state-of-birth indicators, where education is defined as the maximum of zero and the difference between completed education and the grade of the second percentile of the education distribution of black workers in the individual's state-of-birth and cohort.

Because of the relatively small numbers of blacks born in many states, the rates of return are less precisely estimated than those for whites. In fact, the 1980 Census sample contains no black men born between 1920 and 1929 in New Hampshire, and none born between 1930 and 1939 in Vermont. Table 7

³⁷The one important exception to this pattern of equalization seems to have been Mississippi. In 1953-54, the ratio of black-to-white average teacher salaries was 0.57. By 1959-60 this ratio had risen to 0.83 and by 1965-66 to 0.92. See Griffith (1969).

reports weighted means, standard deviations, and coefficients of variation of the estimated rates of returns to education by race and cohort.³⁸ These data confirm earlier findings (e.g., Welch (1973a, 1973b)) that the rate of return to education is lower for blacks than whites among individuals born before 1940. By comparison, among individuals born after 1940 the rate of return to education is slightly higher for blacks than for whites. The interstate dispersion in the rate of return to education -- whether measured by the standard deviation or the coefficient of variation -- is greater for blacks than for whites for the two earlier cohorts, but again this pattern is reversed for the cohort of men born after 1940. The dramatic reduction in the interstate dispersion in the return to education for blacks coincides with the relative improvement in the average quality of education for blacks in the Southern states, and with the corresponding reduction in the interstate dispersion in school quality for black students.

The estimated rates of return to education for black men form the dependent variables for the regression models reported in Table 8. For men born in the segregated states, the explanatory variables are the measures of school quality reported in Table 6a.³⁹ For men born in other states, we use the average quality variables for the state school system as a whole, assuming that each individual attended 6 years of public school. These measures

³⁸The standard deviations and coefficients of variation in Table 7 are adjusted for the expected contributions of sampling variation in the estimated rates of return.

³⁹The relative wage for black teachers was constructed in the same manner as the relative wage for white teachers. The small number of Southern states which did not report school quality data by race for years prior to 1954 were dropped from the sample used in these regressions because the data for the system as a whole (i.e., white and black schools combined) are a grossly inaccurate measure of school quality for blacks in these states.

presumably overstate the quality of schools actually attended by black students in the non-segregated states, although available data on pupil-teacher ratios in segregated high schools in Ohio, Illinois, and Indiana show little difference between school quality in black and white schools.⁴⁰ The models in Table 8 are estimated by weighted least squares, using the inverse sampling variances of the estimated returns as weights.⁴¹

The first column of Table 8 reports estimates of a model that includes only cohort dummy variables. The estimated cohort effects reflect the sharp increases in rates of return to education noted in Table 7. The school quality variables are introduced individually in the models in columns (2), (3) and (4), and jointly in the model in column (5). Similar specifications with 44 unrestricted state-effects are reported in columns (6)-(10). The models with individual quality measures are quite similar to those estimated for whites in Table 3. In each case, returns to education are significantly related to school quality. In row 7 of the table we report the probability value of an F-statistic that tests for equality of the estimated coefficients between the model for blacks and the corresponding model for whites.⁴² These statistics are generally favorable to the hypothesis of the same model for

⁴⁰See Biennial Survey of Education 1936-38 Edition, Chapter IV, Table 5.

⁴¹As a rough check on the effect of assigning overall school system characteristics to black students, we re-estimated the models in Table 8 on the subset of segregated Southern states. Estimates from models without individual state effects are very similar to those in columns (1)-(5) of Table 8. When individual state effects are included the models in columns (8)-(10) are again very similar. The one major discrepancy is for the model that includes only the pupil-teacher ratio. On the subset of segregated states, with state effects, the estimated effect of the pupil-teacher ratio is small and imprecise.

⁴²The models for whites are those reported in Table 3. In testing for equality between the models for whites and blacks we allow for race-specific intercepts, and race-specific cohort effects.

whites and blacks, although the estimated coefficients of the relative teacher wage are uniformly smaller for blacks.

In the two models that include all 3 quality measures (columns (5) and (10)) the individual contributions of the quality variables are poorly determined. This is particularly true of the model that includes state-specific effects. Given the rather imprecise measures of the return to education for blacks from many states, and the high degree of colinearity between the quality measures, it is understandable that the regression model has difficulty identifying the precise effects of the individual quality variables. On balance, we believe that the results for blacks provide strong support for a causal interpretation of the quality coefficients. The similarity of the estimated equations for the two groups is consistent with the view that endogenous school quality and/or omitted variables are not a major source of bias in the estimated quality effects.⁴³

An examination of the estimated cohort effects in the various columns of Table 8 indicates that, especially in the models that exclude state fixed effects, the quality variables explain much of the inter-cohort convergence of returns to education for blacks and whites. In particular, trends in the pupil-teacher ratio are able to account for most of the relative growth in returns to education for blacks between the first (1920-29) and second (1930-39) cohort, and about one-half of the relative increase between the second and

⁴³ One potentially important difference between whites and blacks is the out-migration rate of children from their state-of-birth. Following the approach in Section IIId, we tabulated the probability that a child age 6-15 was living in his or her state-of-birth at the time of the 1940 Census. This probability is slightly higher for blacks than whites (91.5 versus 89.3 percent). Interestingly, the probabilities by age show a sharp increase for blacks after age 13, and are higher for blacks than whites by age 16. These patterns suggest that blacks were more likely than whites to leave their state of birth after completing schooling.

third cohort. Whether controls for other aspects of school quality, such as teacher education, can fully explain the convergence in relative returns is an important question for further research.

V. One-Step Estimates of the Effects of School Quality on Earnings

The estimated effects of school quality presented in the previous two sections are based on a relatively simple specification of the earnings-schooling relationship. To explore the robustness of our findings to alternative specifications it is convenient to work directly with the micro-data in a one-step estimation procedure. A one-step procedure has the additional advantage of incorporating intra-cohort variation in school quality resulting from differences in the timing and duration of school attendance. In this section we present a series of models that relate individual earnings to individual-specific measures of school quality. We compare the results of alternative functional forms for the return-to schooling relationship, including models without a minimum threshold level of education, and models that permit differential effects of school quality on the returns to pre-college and post-secondary education.

Unfortunately, one-step estimation of the model in equations (1) and (2) poses a major computational burden. A one-step model equivalent to the fixed-effects estimates presented in Tables 3 and 4 includes over 350 variables! To simplify the model, we estimate:

$$(3) \quad y_{ijkc} = \delta_j + \mu_k + x_{ijkc} \beta + (r_c + r'Q_{ijc} + r_a \text{Age}_{ijkc} + \rho_k) \cdot E_{ijkc} + \epsilon_{ijkc}$$

where y_{ijkc} represents log weekly earnings for individual i , born in state j in cohort c , and currently living in state k , δ_j is a state-of-birth effect,

μ_k is a state-of-residence effect, x_{ijkc} is a vector of control variables (experience and its' square, marital status, and an indicator for residence in an SMSA), Q_{ijc} is an individual-specific vector of schooling quality measures, formed from simple averages of state-specific data during the years in which individual i attended school, and AGE_{ijc} is the age of individual i .

This model differs from the one described by equations (1) and (2) in a number of important respects. First, the state-of-residence and state-of-birth effects are constrained to be the same across all three cohorts. Similarly, the coefficients of the x vector are constrained to be the same for all three cohorts, as are the controls for differences in the rate of return to schooling by region of current residence (the ρ_k 's). Second, there are no state-of-birth interactions with years of education in (3). Consequently, one-step estimates based on (3) correspond to two-step estimates that exclude state-specific fixed effects in the second state. Finally, equation (3) uses individual-specific measures of the school quality variables, and incorporates age-specific interactions with the return to education.

The specification of equation (3) retains the assumption that measures of school quality affect earnings only insofar as they raise or lower the returns to education. Thus, levels of the school quality variables are explicitly excluded from the earnings equation. In equation (1) this is a costless restriction, since the levels of the school quality variables are absorbed by the cohort-specific state-of-birth effects. In a model such as (3) that restricts the state-of-birth effects across cohorts, and includes individual-specific education quality measures, the effects of the levels of the school quality variables are potentially identifiable.

Even the estimation of this simplified specification is a formidable task on our 1980 Census sample, which contains some 1.2 million observations. To ease the computational burden we drew a stratified random sample of white men from the 5 Percent A-Sample of the 1980 Census. The sample was drawn to contain at most 2000 observations from any particular state-of-birth and cohort. We then estimated the micro-level models by weighted least squares, using as weights the inverse sampling probabilities for each observation. The results are presented in Tables 9 and 10.⁴⁴

Columns (1)-(4) of Table 9 introduce the three quality measures individually, and then jointly, into the earnings equation as interactions with the level of education. Following our two-stage estimation approach, individual education is measured as years of schooling above the grade attained by the second percentile of the educational distribution in the individual's state of birth and cohort. (The coefficients have been scaled to be comparable to the two-step estimates.) The estimated coefficients of the education-quality interactions are very similar to those reported in columns (2)-(5) of Table 3, although the standard errors are 40-50 percent smaller. Apparently, the restrictions on the state-of-birth and state-of-residence effects in the one-step model have little impact on the signs or magnitudes of the estimated quality effects.

The models in columns (5) and (6) present even more restrictive specifications of the state-of-birth and state-of-residence effects. The specification in column (5) drops the state-of-birth effects, while the specification in column (6) drops both state-of-residence and state-of-birth

⁴⁴We have also replicated the one-step estimates using the 1970 Census, and obtained similar results.

effects. These restrictions increase the magnitudes of the estimated quality effects -- particularly the pupil-teacher ratio. Evidently, the interactions of school quality and individual education are correlated with permanent state-level characteristics that are associated with higher returns to schooling.

Our specifications so far have assumed that improvements in the quality of the public school system raise the returns to each additional year of schooling by the same amount. It may seem plausible that increases in the quality of the public schools have a bigger effect on the returns to elementary and secondary education than on the returns to college. One could easily argue, however, that improvements in the quality of elementary and secondary schooling (such as improved mathematics instruction) actually benefit students who attend college more than those who only attend high school.

In an effort to address this issue, the specifications in columns (7) and (8) allow for differential effects of school quality on the returns to post-secondary education. Column (7) includes three additional interaction terms between the measures of school quality and years of post-secondary education. The estimates suggest that higher pupil-teacher ratios do indeed affect the returns to elementary and secondary education more than the returns to post-secondary education. In fact, the net effect of higher pupil-teacher ratios on the returns to post-secondary education is positive. By comparison, changes in teacher wages and changes in term length have statistically similar effects on pre-college and post-secondary returns.

The model in column (8) adds years of post-secondary schooling and its interaction with the three age/cohort variables to the model in column (7).

Only the effects of the teacher wage variable are similar to those in the restricted model. The effects of the pupil-teacher variable are negligible, whereas increases in term length are estimated to have a negative effect on the returns to pre-college education, and a large positive effect on post-secondary returns. It is important to keep in mind that the model in column (8) includes a total of 20 education interaction terms, including the 12 displayed in the table and 9 interactions with current region. Evidently, the regression has some difficulty apportioning the variation in rates of return to education, once the returns to pre-college and post-secondary education are completely unrestricted.

The models in Table 10 address a second set of issues: What is the effect of adding the levels of the quality measures to the earnings regressions? And what is the effect of ignoring the threshold level of education? As in Table 9, the first two columns of Table 10 present models that define education as years of schooling above the threshold level attained by the second percentile of the education distribution in an individual's cohort and state-of-birth. By comparison, the third and fourth columns present models that simply use years of completed education. Column (1) is reproduced from column (4) of Table 9, and is presented for ease of comparison. The specification in column (2) adds the levels of the three quality variables to the earnings model. This addition increases the magnitudes of the estimated interaction effects of the pupil-teacher and term length variables, while having only a small effect on the interaction of teacher wages and education. The levels of the pupil-teacher and term length variables are themselves highly significant. In each case, the pattern of the level and interaction coefficients implies that an improvement in quality

rotates the earnings-schooling relationship. In the case of the pupil-teacher ratio, the point of rotation is at 14 years of education above the threshold level (which averages 7 years of schooling), while in the case of the term length variable the rotation point is at 7 years above the threshold.

Columns (3) and (4) present specifications that ignore any threshold in the earnings-schooling relationship. A comparison of the models with and without the levels of the quality variables shows the importance of including the levels of the quality variables if the education threshold is ignored. Whereas the results in column (4) are similar to those in column (2), the results in column (3) give very little evidence of school quality effects. This is apparently a result of the fact that improvements in school quality rotate the earnings-schooling relation through a point other than the zero-schooling intercept. Although the rotation point is somewhat above the threshold level of schooling, the specification in (1) is nevertheless close enough to the correct specification to obtain significant quality effects.

VI. Conclusion

The estimates presented in this paper provide new evidence that the quality of schooling affects earnings. Men who are educated in states with higher quality school systems tend to earn a higher economic return for their years of schooling. Although the evidence we have assembled is necessarily non-experimental, we believe that our findings are consistent with causal interpretation of the role of school quality. In the first place, our findings are based on statistical models that control for any differences across state-of-birth and cohort groups in the overall level of earnings. Secondly, we have controlled for differences in the rates of return to

education earned by current residents in different regions of the country. Third, we have controlled for any permanent differences across states in the return to education earned by different cohorts of men, and for differences in family background measures (education and earnings of the parents' generation) that may affect subsequent labor market performance. Finally, our analysis suggests that the relation between school quality and the returns to education is similar for whites and blacks. Since we believe that changes in the quality of black schooling were largely exogenous to individual student choices and backgrounds, the similarity of the estimated effects of school quality for the two race groups is reassuring.

Our findings underscore the paradox we noted in the introduction: school quality appears to have an important effect on earnings, but is often found to have little measurable impact on standardized achievement tests.⁴⁵ Although much further research will be needed to resolve this paradox, we believe that success in the labor market is at least as important a yardstick for measuring the performance of the education system as success on standardized tests. At a minimum, our finding of a positive link between school quality and the economic returns to education should give pause to those who argue that investments in the public school system have few benefits for students.

⁴⁵We note, however, that an important, ongoing study that uses random assignment to examine the effect of additional academic training during the summer months on student performance finds that, at least in the short run, the provision of summer school education has a substantial positive effect on disadvantaged students' math and reading test scores (see Sipe, Grossman and Milliner (1988)).

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Appendix A
Sources for Data on School Quality: 1920-66

I. Basic Data for All Students

The following series were extracted from various issues of the Biennial Survey Of Education and the Digest of Education Statistics:

- a. number of pupils enrolled in full time public elementary and secondary school.
- b. average number of days in the school term (full-time public elementary and secondary schools).
- c. average number of days attended by each enrolled student (full time public elementary and secondary schools) (1920-58 only).
- d. number of instructional staff, including supervisors and principals (full time public elementary and secondary schools).
- e. percent of teachers who are male (full time public elementary and secondary schools).
- f. average annual salary per member of the instructional staff (full time public elementary and secondary schools).

These data were collected by state for alternating years, beginning with 1919-1920. In some cases, the figures in the most recently published Biennial Survey were revised in subsequent editions. We have attempted to incorporate as many of these revisions as possible. For 1960 and later we collected a variable representing the percentage of school days attended by enrolled students. This percentage was then used to construct an estimate of average days attended from the series on the length of the school term. The data from the biennial editions of the Survey are allocated to the previous two years: for example, data from the 1937-38 edition is used for both 1937 and 1938.

II. Data for White and Black School Systems -- 18 Segregated States

With the exception of the percentage of male teachers, the above data are available for most states for the white and black school systems separately up to 1954. Data on teacher salaries are unavailable for 1933-34, and are missing for some states in other years. We used the average of salaries in 1931-32 and 1935-36 to estimate 1933-34 salaries. In some cases we had information on teacher salaries in the black schools only. In these cases we used information on the numbers of teachers in the white and black schools, together with information on average salaries for both races, to infer the average salary for teachers in the white schools. Data by race for the schools in Kentucky, Tennessee, Missouri, and West Virginia are missing in many issues of the Biennial Survey. We used information in the annual reports of the state offices of education of these four states to estimate enrollments, numbers of teachers, and average wages in some years. Data for other years are estimated by linear interpolation between the available years. In cases where data were unavailable in two consecutive Biennial Surveys, the observations were dropped from the data set.

III. Construction of the Relative Teacher Wage

Two wage series were combined to create a relative wage index for teachers in each state. For 1920-38, we used the wage paid to laborers on federal road construction projects. This wage is available on a regional basis (for 9 Census regions) in the Statistical Abstract of the United States. Data for 1920-29 are taken from the 1930 edition, Table 358. Data for 1930-56 are taken from the 1957 edition, Table 271. For 1940-66 we use the average state-level wage of workers covered by the Social Security system, from U.S. Department of Labor Employment and Training Administration Handbook Number 394. To convert the regional construction wage rates into state-level averages, we formed the average ratio of the state Social Security wage to the regional construction wage in the period 1940-44. This average ratio was then applied to the construction wage in the period 1920-38 to obtain a state-specific average.

IV. Data on Education and Experience of Teachers

a. 1940

We used the public use sample of the 1940 Census to form extracts of teachers in each of the 48 (mainland) states and District of Columbia. Teachers were identified by industry (educational services) and occupation (teachers not elsewhere classified). We sampled only those teachers who reported either white or black race, and who reported positive earnings and weeks worked and non-allocated age, sex, race, industry, occupation, and years of education. The extract contains 9161 teachers.

b. 1950

We used the public use sample of the 1950 Census to form extracts of teachers in each of the 48 (mainland) states and District of Columbia. (Owing to technical difficulties our public use sample excludes 1/8 of the available sample). Teachers were identified by industry (educational services) and occupation (teachers not elsewhere classified). We sampled only those teachers who reported either white or black race, and who reported non-allocated age, sex, race, industry, occupation, and years of education. The extract contains 3206 teachers.

c. 1960

We used the public use sample of the 1960 Census to form extracts of teachers in each of the 48 (mainland) states and District of Columbia. Teachers were identified by industry (educational services) and occupation (teachers, elementary schools and teachers, secondary schools). We sampled only those teachers who reported either white or black race, and who reported non-allocated age. The extract contains 16052 teachers.

V. Data on Family Background

a. Average Per Capita Income

We collected average personal income per capita by state for the years 1930, 1940, and 1950 from State Personal Income: Estimates for 1929-1982, Revised Estimates, Washington, D.C.: Bureau of Economic Analysis, 1984. The income data were originally derived from the National Income and Product Accounts. The consumer price index was used to convert the data into real dollars. The state-level per capita income in 1930 was assigned to the cohort of men born in the 1920s, the state-level per capita income in 1940 was assigned to the cohort of men born in the 1930s, and the state-level per capita income in 1950 was assigned to the cohort of men born in the 1940s. These years roughly correspond to the years when educational expenditures for each cohort would have been determined.

b. Median Education of Parents' Generation

As a measure of the education of each cohort's parents, we collected information on the median education of white persons age 25 or older by state in 1940, 1950, and 1960. These data are reported in Statistical Abstract of the United States (Washington, D.C.: Government Printing Office, no. 66, 75, and 85), and were originally derived from the 1940, 1950 and 1960 Censuses. In 1940, the education data are only reported for native-born individuals, while in 1950 and 1960 the data pertain to native and foreign born individuals. The median education of adults in 1940 was assigned to the 1920s cohort, the median education of adults in 1950 was assigned to the 1930s cohort, and the median education of adults in 1960 was assigned to the 1940s cohort.

VI. Private Schools

State-level data on the number of students enrolled in private schools, the number of students enrolled in Catholic schools, and the number of teachers in Catholic schools were collected from the Biennial Survey. Unfortunately, these variables are only available on an irregular basis. Data for 1937-1938 were assigned to the cohort born in the 1920s, data for 1949-1950 were assigned to the cohort born in the 1930s, and data for 1955-1956 were assigned to the cohort born in the 1940s.

Appendix B
1980 Microdata Samples

Our samples are taken from the Public Use A-sample of the 1980 Census (a 5 percent sample of the population). The samples consist of men born in the 48 mainland states or the District of Columbia between 1920 and 1949, and currently living in any of the 50 states or D.C. Year of birth is estimated from information on age and quarter of birth. We include only those individuals whose race is identified as "white" or "black". Individuals with imputed information on age, race, sex, education, weeks worked, or total annual earnings are excluded, as are individuals who report no weeks of work in 1979. In addition, individuals with wage or salary income in 1979 less than \$101, and individuals with average weekly wage and salary income of less than \$36 or greater than \$2,500 are excluded. The final sample sizes are:

Born 1920-29:	whites - 279,008	blacks - 20,258
Born 1930-39:	whites - 299,063	blacks - 26,108
Born 1940-49:	whites - 441,675	blacks - 38,659

These samples are used to form the first-stage estimates of the return to education in our 2-step procedure. For the one-step estimates presented in Tables 9 and 10, we used stratified random samples of these overall samples, drawn to yield a maximum of (approximately) 2,000 white men per cohort and state-of-birth. The sampling procedure generates a subsample of 265,618. The regression models are then estimated by weighted least squares, using as weights the inverse sampling probabilities of the various states-of-birth.

Figure 1
Wages vs. Schooling, by Cohort and State of Birth

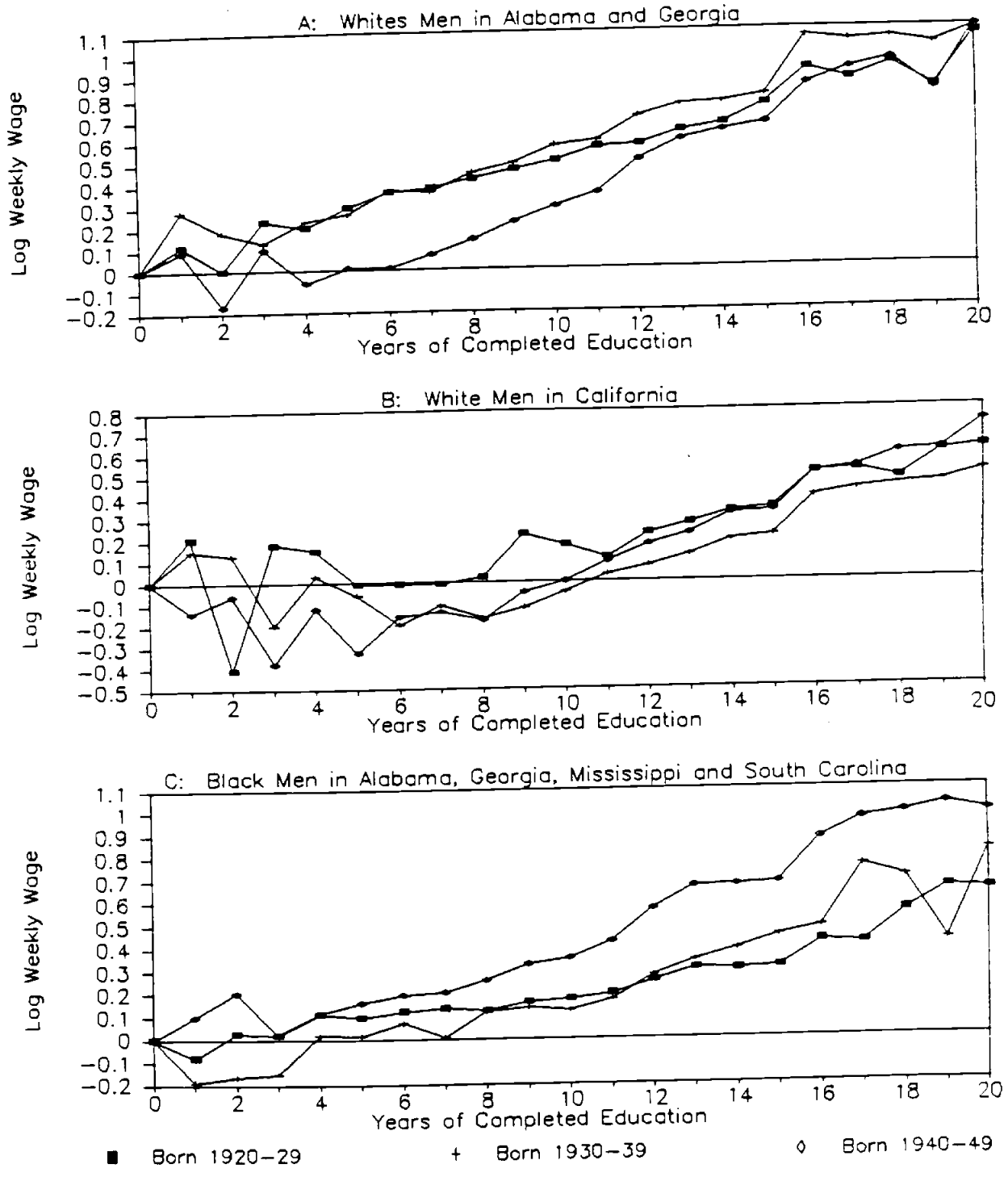
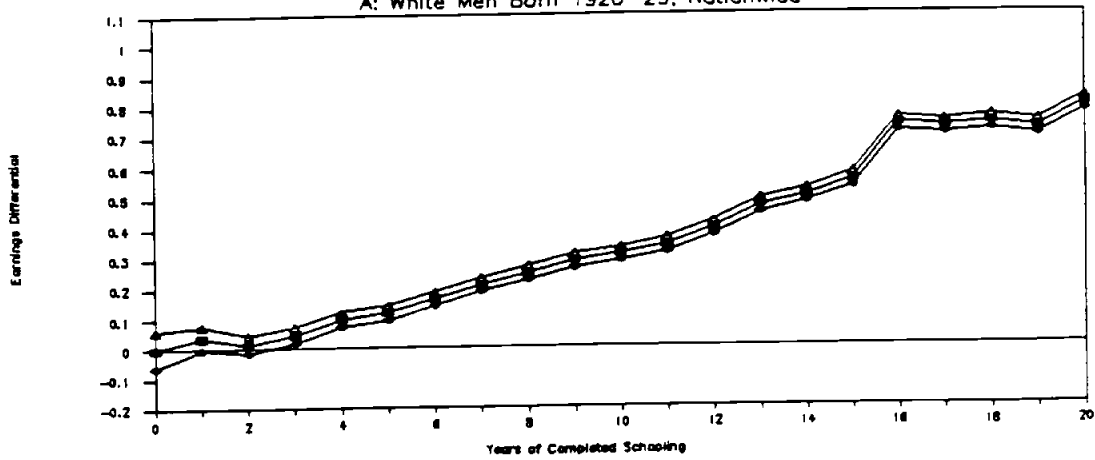
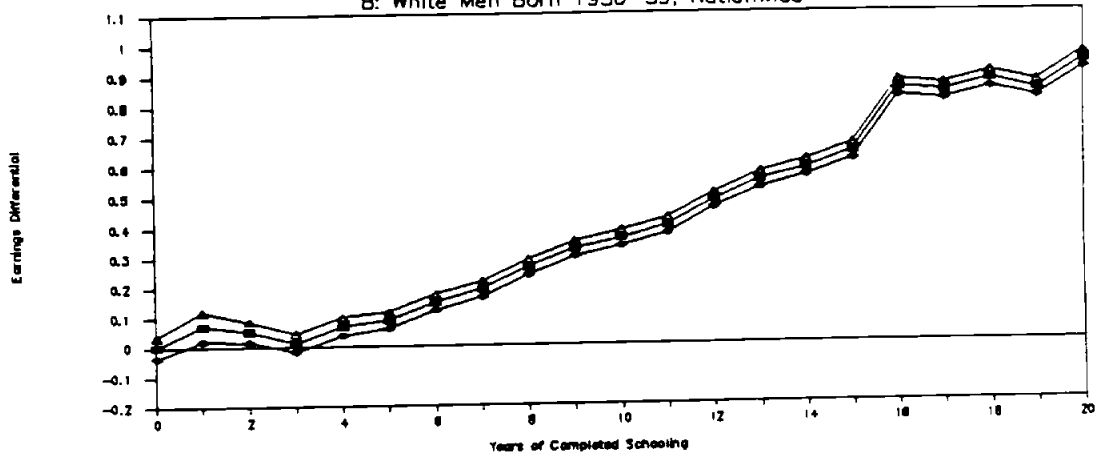


Figure 2 Return to Single Years of Education

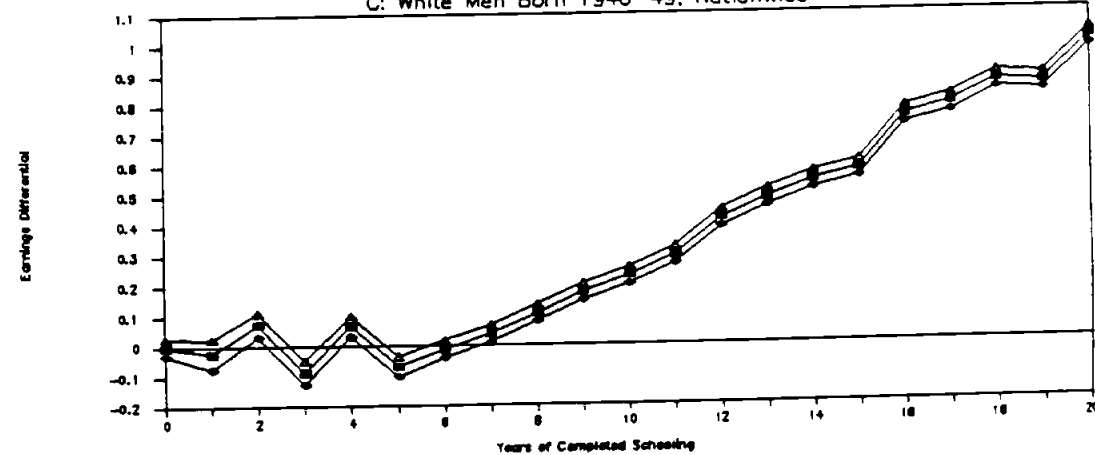
A: White Men Born 1920-29, Nationwide



B: White Men Born 1930-39, Nationwide



C: White Men Born 1940-49, Nationwide



Point Estimate
 - 1 Std. Error
 + 1 Std. Error

Figure 3

Relative Teacher Wage vs. Return to Schooling,
With High, Low and Medium Pupil-Teacher Ratios (P/T) Indicated

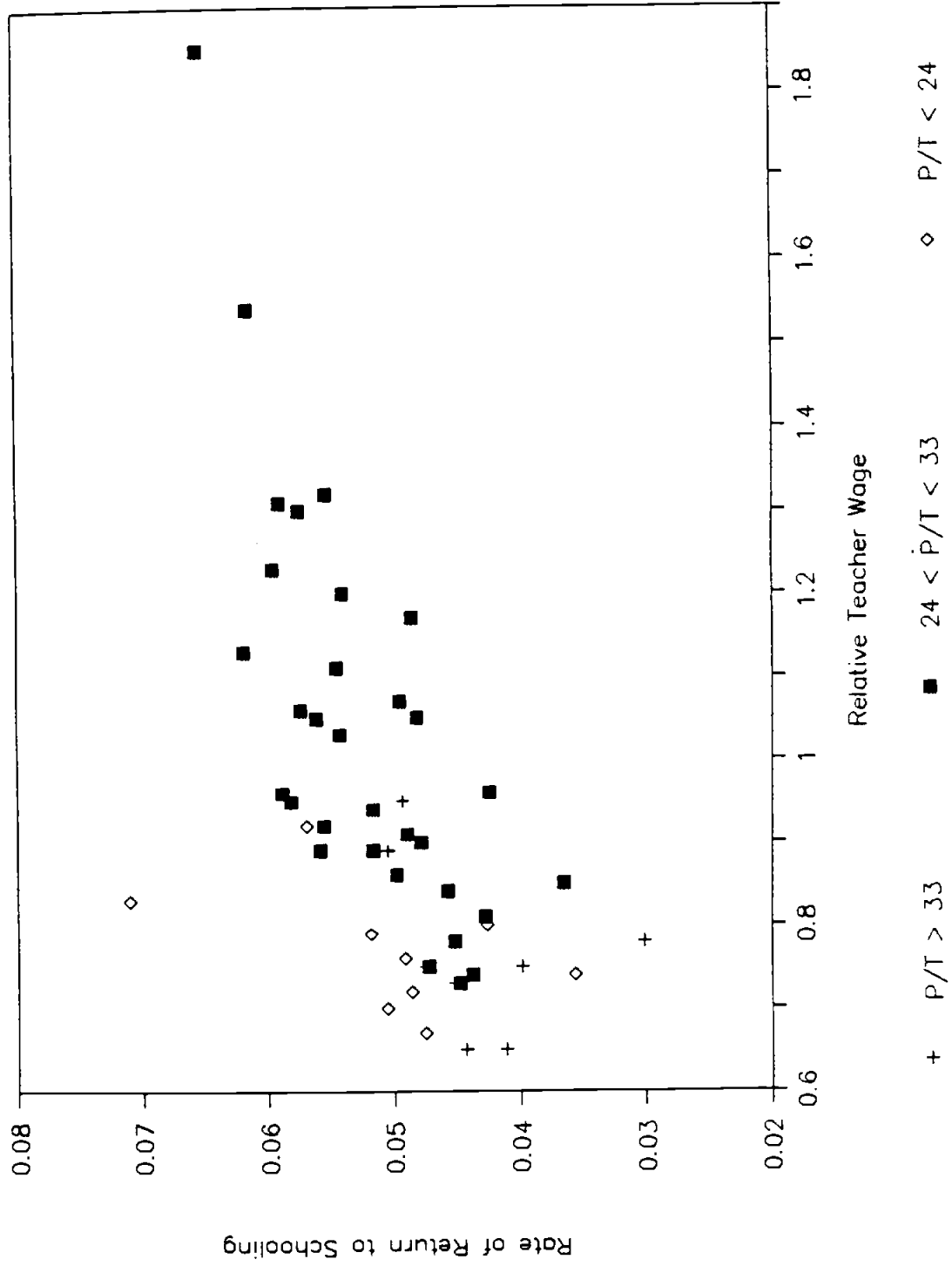
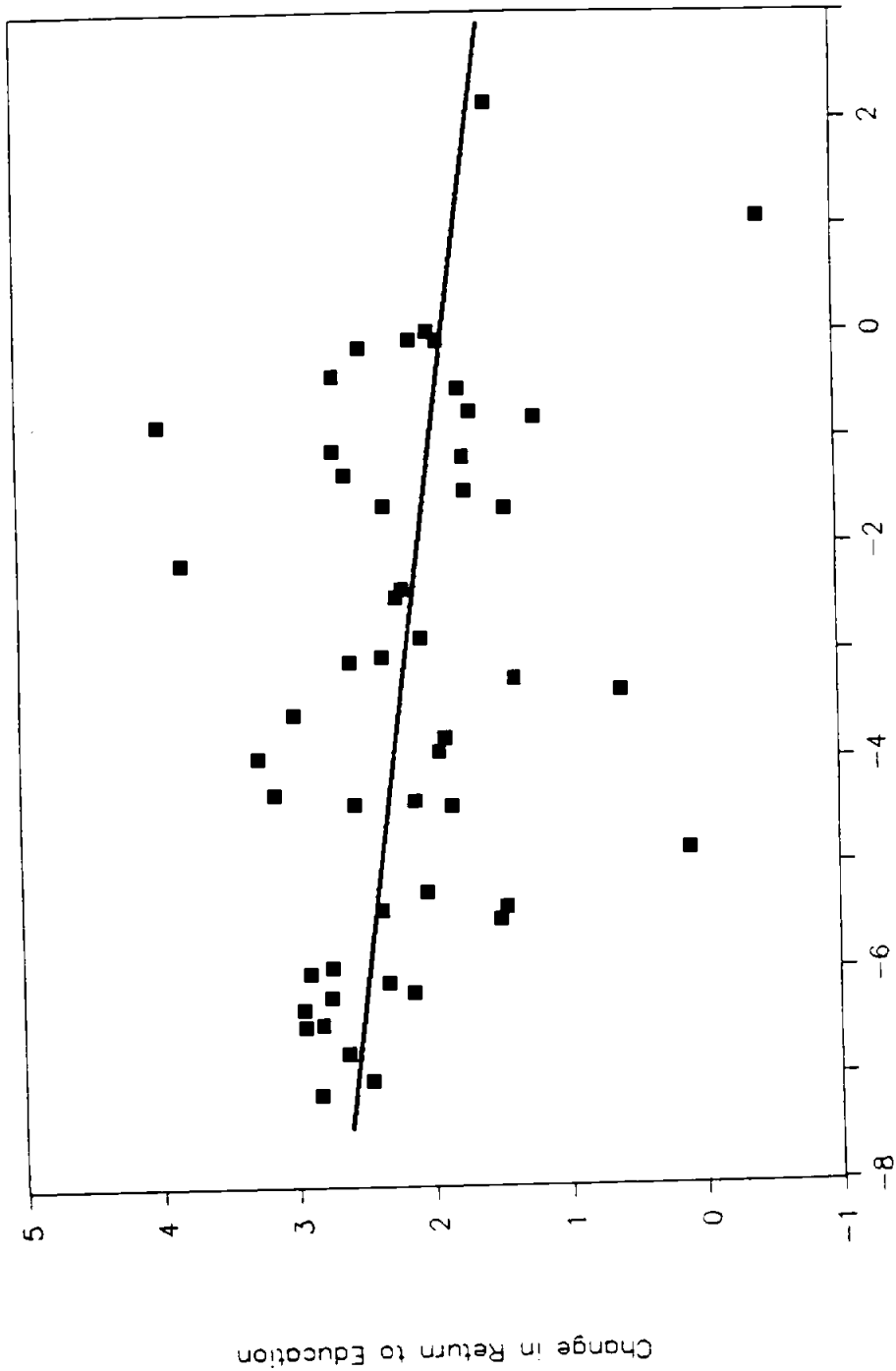


Figure 4
Change in Returns to Education vs. Change in Pupil Teacher Ratio
1940s Cohort minus 1920s Cohort



Note: The OLS line is also shown on the figure.

Table 1
 Averages of School Quality Variables For Cohorts Born 1920-29, 1930-39, 1940-49
 Black and White Students Combined

	Cohorts Born:	Pupil/Teacher Ratio			Term Length (Days)			Relative Teacher Wage		
		20-29	30-39	40-49	20-29	30-39	40-49	20-29	30-39	40-49
1.	Alabama	35.9	31.7	29.1	150	168	176	0.73	0.81	0.90
2.	Arizona	30.8	28.6	26.4	169	169	170	1.05	1.13	1.11
3.	Arkansas	36.3	32.3	29.1	151	165	173	0.65	0.75	0.86
4.	California	28.0	29.7	27.2	177	176	177	1.30	1.27	1.17
5.	Colorado	24.9	24.1	24.1	174	175	177	0.95	0.98	0.95
6.	Connecticut	29.6	25.2	24.2	182	181	180	1.11	1.09	1.07
7.	Delaware	27.3	24.4	22.5	183	182	180	1.13	1.05	1.01
8.	Florida	30.4	26.5	26.1	166	175	180	0.96	1.06	1.15
9.	Georgia	34.8	30.2	28.4	154	173	180	0.75	0.80	0.93
10.	Idaho	26.0	26.5	26.0	170	173	174	0.84	0.92	0.92
11.	Illinois	28.1	24.9	24.3	184	187	184	1.06	1.02	1.00
12.	Indiana	30.0	28.3	27.2	169	171	175	0.96	0.99	1.00
13.	Iowa	21.7	20.8	20.5	176	177	179	0.79	0.85	0.85
14.	Kansas	21.3	20.5	21.1	171	171	174	0.76	0.82	0.89
15.	Kentucky	33.4	29.7	27.4	159	165	172	0.75	0.76	0.76
16.	Louisiana	33.7	29.8	27.2	161	173	179	0.78	0.96	1.04
17.	Maine	25.9	25.0	23.8	178	179	181	0.73	0.76	0.82
18.	Maryland	32.8	30.4	26.7	187	185	182	1.20	1.22	1.19
19.	Massachusetts	27.8	25.2	23.4	179	178	177	1.32	1.26	1.12
20.	Michigan	28.6	27.5	25.6	180	180	180	0.89	0.91	0.91
21.	Minnesota	25.0	24.0	23.5	175	173	174	0.89	0.97	1.00
22.	Mississippi	37.6	34.4	31.4	139	154	168	0.65	0.66	0.78
23.	Missouri	27.7	27.1	26.6	177	180	182	0.86	0.86	0.87
24.	Montana	20.1	19.7	20.0	176	177	179	0.80	0.96	0.96

Table 1 Continued

Cohorts Born:	Pupil/Teacher Ratio			Term Length (Days)			Relative Teacher Wage		
	20-29	30-39	40-49	20-29	30-39	40-49	20-29	30-39	40-49
25. Nebraska	21.0	19.0	19.4	177	177	177	0.70	0.76	0.84
26. Nevada	22.7	24.3	24.9	175	176	178	0.92	0.97	0.96
27. New Hampshire	25.1	23.5	23.8	177	176	177	1.07	1.03	1.02
28. New Jersey	28.2	24.1	22.9	186	183	181	1.31	1.14	1.02
29. New Mexico	28.5	29.1	26.0	176	179	180	0.92	1.11	1.13
30. New York	27.6	25.7	23.2	184	182	181	1.54	1.30	1.13
31. North Carolina	35.8	31.9	28.8	161	175	180	0.95	1.09	1.12
32. North Dakota	18.4	17.7	17.9	175	172	177	0.67	0.76	0.81
33. Ohio	29.4	27.4	26.4	179	179	177	1.03	0.97	0.93
34. Oklahoma	32.1	27.0	25.6	172	176	176	0.74	0.87	0.92
35. Oregon	24.7	24.5	23.0	172	176	179	0.90	0.97	1.02
36. Pennsylvania	31.4	27.2	25.4	181	182	182	1.17	1.08	1.05
37. Rhode Island	27.8	25.0	23.9	180	180	180	1.23	1.20	1.17
38. South Carolina	34.2	29.9	28.7	154	171	179	0.89	0.89	0.92
39. South Dakota	17.7	16.6	17.6	174	175	177	0.72	0.81	0.82
40. Tennessee	32.8	29.9	28.8	164	170	176	0.78	0.80	0.88
41. Texas	29.3	27.2	25.7	164	173	175	0.75	0.92	1.01
42. Utah	31.0	29.2	27.8	172	174	176	0.91	1.07	1.06
43. Vermont	23.5	22.7	22.6	175	173	173	0.74	0.84	0.91
44. Virginia	32.9	29.3	26.7	172	179	180	0.81	0.89	0.96
45. Washington	30.8	29.0	25.4	180	178	178	1.05	1.11	1.05
46. West Virginia	27.2	27.3	26.8	171	174	174	0.85	0.84	0.77
47. Wisconsin	25.7	24.0	23.3	179	179	178	0.94	0.94	0.94
48. Wyoming	20.3	21.5	21.4	175	175	177	0.83	0.91	0.98
49. D.C.	30.8	28.5	27.4	178	176	178	1.85	1.54	1.28

Notes: Cohort averages are formed assuming 12 years of elementary and secondary education. See text for definitions of variables. See Data Appendix for sources.

Table 2

Estimated Returns to Education by State and CohortWhite Males Born 1920-1949

(standard errors in parentheses)

Estimated Return for Cohort Born:

State	1920-29	1930-29	1940-49
Alabama	4.52 (0.22)	6.08 (0.21)	7.15 (0.20)
Arizona	5.62 (0.49)	7.15 (0.50)	7.47 (0.42)
Arkansas	4.44 (0.23)	5.60 (0.23)	7.28 (0.23)
California	5.76 (0.21)	6.20 (0.19)	6.96 (0.13)
Colorado	5.82 (0.34)	6.22 (0.33)	7.49 (0.29)
Connecticut	5.46 (0.28)	7.14 (0.28)	7.83 (0.23)
Delaware	6.19 (0.76)	7.07 (0.76)	6.31 (0.58)
Florida	4.25 (0.33)	6.07 (0.30)	7.40 (0.23)
Georgia	4.76 (0.21)	6.14 (0.21)	7.71 (0.19)
Idaho	4.58 (0.50)	5.40 (0.43)	6.57 (0.39)
Illinois	5.74 (0.15)	6.91 (0.14)	7.63 (0.12)
Indiana	5.89 (0.22)	6.98 (0.20)	7.95 (0.16)
Iowa	5.19 (0.24)	6.02 (0.23)	6.92 (0.21)

Table 2, continued

<u>Estimated Return for Cohort Born:</u>			
<u>State</u>	<u>1920-29</u>	<u>1930-29</u>	<u>1940-49</u>
Kansas	4.92 (0.27)	5.90 (0.26)	7.40 (0.24)
Kentucky	3.99 (0.19)	5.59 (0.18)	6.90 (0.16)
Louisiana	3.02 (0.27)	4.78 (0.25)	5.84 (0.22)
Maine	4.49 (0.40)	6.63 (0.36)	8.29 (0.33)
Maryland	5.41 (0.28)	5.99 (0.27)	7.74 (0.22)
Massachusetts	5.54 (0.22)	7.21 (0.22)	8.10 (0.18)
Michigan	5.59 (0.18)	6.64 (0.16)	8.17 (0.14)
Minnesota	5.17 (0.23)	6.09 (0.22)	6.89 (0.20)
Mississippi	4.12 (0.29)	5.67 (0.27)	6.87 (0.24)
Missouri	4.99 (0.20)	6.32 (0.19)	7.68 (0.17)
Montana	4.27 (0.48)	4.82 (0.46)	6.39 (0.42)
Nebraska	5.06 (0.31)	6.43 (0.30)	7.38 (0.29)
Nevada	5.69 (1.20)	6.64 (1.00)	7.24 (0.75)
New Hampshire	4.96 (0.52)	6.12 (0.50)	7.57 (0.43)

Table 2, continued

<u>Estimated Return for Cohort Born:</u>			
State	1920-29	1930-29	1940-49
New Jersey	5.91 (0.20)	7.49 (0.20)	7.95 (0.16)
New Mexico	5.56 (0.47)	5.84 (0.42)	7.80 (0.38)
New York	6.16 (0.13)	7.18 (0.12)	8.28 (0.10)
North Carolina	4.94 (0.19)	5.93 (0.19)	7.40 (0.17)
North Dakota	4.76 (0.38)	5.17 (0.36)	6.53 (0.38)
Ohio	5.43 (0.16)	6.86 (0.15)	7.78 (0.12)
Oklahoma	4.39 (0.22)	5.72 (0.22)	7.34 (0.22)
Oregon	4.79 (0.42)	5.26 (0.40)	6.23 (0.29)
Pennsylvania	4.87 (0.13)	6.26 (0.13)	7.61 (0.11)
Rhode Island	5.96 (0.41)	6.62 (0.40)	7.90 (0.33)
South Carolina	5.05 (0.28)	5.76 (0.27)	6.56 (0.22)
South Dakota	4.87 (0.41)	5.68 (0.39)	6.79 (0.39)
Tennessee	4.53 (0.20)	6.62 (0.19)	7.80 (0.18)
Texas	4.73 (0.16)	5.93 (0.15)	7.72 (0.14)

Table 2, continued

<u>Estimated Return for Cohort Born:</u>			
State	1920-29	1930-29	1940-49
Utah	4.90 (0.44)	5.56 (0.39)	6.28 (0.34)
Vermont	3.57 (0.55)	6.82 (0.53)	7.54 (0.48)
Virginia	4.29 (0.21)	5.54 (0.20)	6.44 (0.17)
Washington	4.82 (0.34)	5.46 (0.30)	6.28 (0.23)
West Virginia	3.66 (0.23)	5.25 (0.21)	6.34 (0.20)
Wisconsin	5.17 (0.21)	6.10 (0.20)	7.37 (0.18)
Wyoming	7.10 (0.69)	4.99 (0.65)	6.65 (0.59)
D.C.	6.54 (0.63)	6.07 (0.54)	7.14 (0.38)
Mean over all states:	5.07	6.27	7.44
Standard Deviation:	0.65	0.58	0.56
<u>Correlation with:</u>			
Pupil/Teacher Ratio	-0.36	-0.23	-0.19
Term Length	0.62	0.51	0.35
Relative Teacher Wage	0.71	0.51	0.25

Note: Column entries are estimated rates of return to education, based on samples in the 1980 Census. See text for estimation method. Estimated standard deviation of returns is adjusted for the expected contribution of sampling variability.

Table 3

Determinants of the Return to Education: White Males

Dependent Variable: Percentage Return to Education

(standard errors in parentheses)

Independent Variable	Excluding State Effects					Including 48 State Effects				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1. Pupil/Teacher Ratio (:100)	---	-5.37 (1.62)	---	---	-2.38 (1.64)	---	-9.52 (2.81)	---	---	-9.35 (3.18)
2. Term Length (100's of days)	---	---	4.57 (.67)	---	1.93 (0.94)	---	---	2.16 (0.70)	---	-.02 (0.99)
3. Relative Teacher Wage	---	---	---	1.86 (0.26)	1.35 (0.33)	---	---	---	0.99 (0.35)	0.97 (0.44)
4. Dummy for Born 1930-39	1.21 (0.14)	1.07 (0.14)	1.03 (0.12)	1.20 (0.12)	1.07 (0.12)	1.21 (0.07)	0.98 (0.10)	1.13 (0.07)	1.21 (0.07)	0.98 (0.09)
5. Dummy for Born 1940-49	2.37 (0.13)	2.16 (0.14)	2.13 (0.12)	2.35 (0.11)	2.16 (0.12)	2.35 (0.07)	1.98 (0.12)	2.24 (0.07)	2.36 (0.06)	1.99 (0.12)
6. R ²	.71	.72	.78	.78	.80	.95	.96	.95	.95	.96

Note: Sample size is 147. Mean and standard deviation of the dependent variable are 6.421 and 1.161 respectively. Equations are weighted by the inverse sampling variance of the dependent variable. All equations include an unrestricted constant term (not reported).

Table 4
Additional Determinants of the Return to Education: White Males
 Dependent Variable: Percentage Return to Education
 Fixed Effects Estimates
 (standard errors in parentheses)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
1. Pupil/Teacher Ratio (+ 100)	-9.67 (3.16)	-9.36 (3.21)	-9.77 (3.20)	-9.33 (3.10)	-9.81 (3.06)	-9.80 (2.98)	-8.42 (3.54)	-10.62 (3.22)	-9.23 (3.23)	-9.77 (3.20)	-8.82 (3.51)
2. Term Length (100's of Days)	-0.51 (1.03)	-0.04 (1.12)	-0.65 (1.17)	-0.98 (1.05)	-0.40 (0.97)	-1.26 (1.01)	-0.08 (1.00)	-1.04 (1.13)	0.06 (1.04)	-0.35 (1.04)	-1.09 (1.25)
3. Relative Teacher Wage	1.22 (0.47)	0.95 (0.57)	1.13 (0.58)	0.86 (0.43)	1.05 (0.44)	1.05 (0.43)	0.86 (0.48)	1.12 (0.44)	1.00 (0.47)	0.80 (0.47)	1.52 (0.61)
4. Median Education of Parents Generation	-0.18 (0.11)	---	-0.18 (0.11)	---	---	---	---	---	---	---	0.38 (0.13)
5. Log Real Per Capita Income of Parents Generation	---	0.18 (0.52)	0.13 (0.51)	---	---	---	---	---	---	---	-0.50 (0.53)
6. Fraction Male Teachers	---	---	---	-3.36 (1.39)	---	-3.46 (1.43)	---	---	---	---	-2.98 (1.73)
7. Mean Years of Education of Teachers	---	---	---	---	0.33 (0.13)	0.38 (0.13)	---	---	---	---	0.38 (0.13)
8. Mean Years of Experience of Teachers	---	---	---	---	0.03 (0.02)	0.02 (0.02)	---	---	---	---	0.02 (0.02)
9. Fraction HS Grads in Cohort	---	---	---	---	---	---	0.89 (1.48)	---	---	---	0.93 (1.52)
10. Fraction College Grads in Cohort	---	---	---	---	---	---	---	-3.86 (2.11)	---	---	-1.38 (2.34)
11. Fraction of Enrollment in Private Schools	---	---	---	---	---	---	---	---	0.71 (2.69)	---	1.51 (2.62)
12. Weighted Gap Between Pupil-Teacher Ratio in Catholic and Public Schools	---	---	---	---	---	---	---	---	---	-6.06 (5.80)	---
13. Dummy for Born 1930-39	1.12 (0.11)	0.98 (0.10)	1.12 (0.13)	1.01 (0.09)	0.88 (0.10)	0.97 (0.11)	0.90 (0.16)	1.26 (0.18)	0.96 (0.12)	1.04 (0.11)	1.01 (0.24)
14. Dummy for Born 1940-49	2.32 (0.24)	1.98 (0.26)	2.27 (0.31)	2.22 (0.16)	1.59 (0.18)	1.84 (0.20)	1.84 (0.27)	2.72 (0.42)	1.96 (0.16)	1.09 (0.16)	2.20 (0.54)
15. R ²	0.96	0.96	0.96	0.96	0.96	0.97	0.96	0.96	0.96	0.96	0.97

Note: See note to Table 3. All equations include 48 state effects. See text for definitions of explanatory variables.

Table 5

Determinants of the Return to Education: White Males

Dependent Variable: Percentage Return to Education, Adjusted for Pre-School Mobility

(standard errors in parentheses)

Independent Variable	Excluding State Effects					Including 48 State Effects				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1. Pupil/Teacher Ratio (:100)	---	-6.41 (1.79)	---	---	-3.03 (1.84)	---	-9.77 (3.12)	---	---	-10.24 (3.53)
2. Term Length (100's of days)	---	---	4.83 (0.72)	---	1.79 (1.03)	---	---	2.11 (0.76)	---	-.41 (1.09)
3. Relative Teacher Wage	---	---	---	2.01 (0.28)	1.49 (0.35)	---	---	---	1.08 (0.37)	1.17 (0.49)
4. Dummy for Born 1930-39	1.22 (0.15)	1.07 (0.15)	1.05 (0.13)	1.23 (0.13)	1.09 (0.13)	1.23 (0.07)	0.99 (0.11)	1.12 (0.08)	1.23 (0.07)	0.99 (0.10)
5. Dummy for Born 1940-49	2.39 (0.14)	2.14 (0.15)	2.14 (0.13)	2.39 (0.12)	2.17 (0.13)	2.37 (0.07)	1.98 (0.14)	2.27 (0.08)	2.39 (0.07)	1.99 (0.14)
6. R ²	.68	.70	.75	.76	.78	.94	.95	.95	.95	.95

Note: Sample size is 147. Mean and standard deviation of the dependent variable are 6.435 and 1.19 respectively. Equations are weighted by the inverse sampling variance of the dependent variable. All equations include an unrestricted constant term (not reported).

Table 6a

Averages of School Quality Variables For Cohorts Born 1920-29, 1930-39, 1940-49

Black Men Born In Segregated States

State	Pupil/Teacher Ratio			Term Length			Average Teacher Salary		
	20-29	30-39	40-49	20-29	30-39	40-49	20-29	30-39	40-49
1 Alabama	47.3	38.5	30.8	132	159	176	351	766	2603
2 Arkansas	45.6	39.8	33.4	129	148	171	348	523	1375
3 Delaware	31.8	28.0	24.8	183	182	182	1493	1865	4054
4 Florida	38.1	29.9	26.9	155	171	180	468	1090	3451
5 Georgia	45.5	36.5	31.4	135	164	179	297	637	1557
6 Kentucky	34.1	27.6	26.3	NA	171	175	NA	NA	NA
7 Louisiana	49.4	39.1	31.5	126	156	177	441	377	1153
8 Maryland	35.7	33.7	29.2	179	185	183	1185	1366	4047
9 Mississippi	49.5	43.1	36.6	116	130	159	293	351	1554
10 Missouri	31.3	31.9	30.1	NA	188	188	NA	NA	NA
11 North Carolina	42.3	35.6	31.4	152	173	180	522	1265	3132
12 Oklahoma	32.5	25.2	24.2	164	175	175	824	1479	1006
13 South Carolina	47.4	35.6	30.9	122	156	177	302	693	2032
14 Tennessee	40.9	34.9	30.7	158	169	177	NA	NA	NA
15 Texas	40.0	31.5	27.0	145	166	174	658	1171	1451
16 Virginia	39.7	34.0	29.1	162	178	180	518	1133	2525
17 West Virginia	27.7	26.7	26.4	172	174	173	NA	NA	NA
18 D.C.	33.9	31.9	29.5	179	177	177	NA	NA	4655

Notes: Cohort averages are formed assuming 6 years of public education. See text for definition of variables. See Data Appendix for sources.

Table 6b

Averages of School Quality Variables For Cohorts Born 1920-29, 1930-39, 1940-49

White Men Born In Segregated States

State	Pupil/Teacher Ratio			Term Length			Average Teacher Wage		
	20-29	30-39	40-49	20-39	30-39	40-40	20-29	30-39	40-49
1 Alabama	33.5	30.3	28.9	154	165	176	802	1246	2321
2 Arkansas	35.4	31.6	29.2	152	166	173	628	985	2245
3 Delaware	27.8	24.6	22.4	183	182	180	1481	2045	4065
4 Florida	30.4	25.8	26.3	168	174	180	1016	1661	3698
5 Georgia	33.0	28.4	27.7	155	173	180	810	1194	2733
6 Kentucky	35.8	30.4	28.5	NA	161	172	NA	NA	NA
7 Louisiana	30.4	27.0	26.3	175	178	179	1034	1566	3537
8 Maryland	32.3	30.8	27.4	188	185	182	1556	2155	4110
9 Mississippi	31.5	29.8	29.1	154	167	169	739	1048	2133
10 Missouri	28.0	26.8	26.8	NA	179	182	NA	NA	NA
11 North Carolina	35.4	31.7	29.0	159	173	180	872	1407	3152
12 Oklahoma	34.2	27.8	26.0	169	175	177	973	1493	3237
13 South Carolina	29.4	27.3	27.6	172	176	180	896	1271	2733
14 Tennessee	32.6	29.2	28.9	165	167	175	NA	NA	NA
15 Texas	29.2	26.9	26.3	164	174	175	988	1551	3575
16 Virginia	32.8	29.2	27.0	170	179	180	937	1400	2951
17 West Virginia	27.1	27.3	27.3	171	174	173	NA	NA	NA
18 D.C.	30.9	27.1	25.0	179	175	177	NA	NA	4783

Notes: Cohort averages are formed assuming 6 years of public education. See text for definition of variables. See Data Appendix for sources.

Table 7

Summary Statistics for Percentage Return to Education by State of Birth:
Black and White Men

Birth Cohort	Black Men			White Men		
	Mean	Std. Dev.	C.V	Mean	Std. Dev.	C.V
1920-29	3.63	0.71	0.20	5.07	0.65	0.13
1930-39	5.25	0.71	0.14	6.27	0.58	0.09
1940-49	7.52	0.38	0.05	7.44	0.56	0.08

Notes: Summary statistics are weighted by the inverse sampling variance of the estimated coefficients. The summary statistics for all cohorts of whites are based on 49 jurisdictions; the summary statistics for blacks are based on 48 jurisdictions for the 1920-29 cohort, 48 jurisdictions for the 1930-39 cohort, and 49 jurisdictions for the 1940-49 cohort. The standard deviations and coefficient of variations have been corrected for sampling variance in the estimated returns.

Table 8

Determinants of the Return to Education: Black Males

(Dependent Variable: Percentage Return to Education)

(standard errors in parentheses)

Independent Variable	Excluding State Effects					Including 43 State Effects				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1. Pupil/Teacher Ratio (:100)	---	-9.91 (1.39)	---	---	-10.30 (3.01)	---	-6.75 (3.75)	---	---	-0.41 (9.03)
2. Term Length (100's of days)	---	---	2.90 (0.50)	---	-0.29 (1.10)	---	---	1.70 (0.89)	---	0.96 (2.23)
3. Relative Teacher Wage	---	---	---	1.07 (0.20)	0.06 (0.36)	---	---	---	0.65 (0.35)	0.33 (0.58)
4. Dummy for Born 1930-39	1.69 (0.21)	0.97 (0.21)	1.14 (0.21)	1.71 (0.19)	1.00 (0.27)	1.68 (0.18)	1.19 (0.32)	1.35 (0.24)	1.69 (0.17)	1.47 (0.47)
5. Dummy for Born 1940-49	3.97 (0.21)	2.73 (0.25)	3.05 (0.25)	3.95 (0.19)	2.77 (0.37)	3.90 (0.18)	3.12 (0.47)	3.39 (0.32)	3.94 (0.18)	3.59 (0.73)
6. R ²	0.73	0.81	0.79	0.79	0.81	0.88	0.89	0.89	0.89	0.89
7. P-Value of F-Test of Different Quality Effects for Blacks and Whites	---	0.138	0.101	0.045	0.001	---	0.738	0.943	0.107	0.051

Note: Sample size is 131. Mean and standard deviation of dependent variable are 5.53 and 1.89, respectively. Equations are weighted by the inverse sampling variance of the dependent variable. Sample consists of all states with nonsegregated school systems, and segregated states which report separate data for black schools. One observation for both New Hampshire and Vermont was excluded because there were no black men in the 1980 census sample born in these states in 1920-29. All equations include an unrestricted constant term (not reported).

Table 9

One-Step Estimates of the Effect of School Quality on Earnings:
White Males, 1980 CensusDependent Variable: Log Weekly Earnings
(standard errors in parentheses)

Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1. Education (-100)	3.98 (0.38)	-4.07 (0.69)	1.08 (0.38)	-0.72 (0.92)	-1.64 (0.73)	-5.34 (0.71)	0.30 (0.98)	9.01 (1.45)
2. Education x Pupil/Teacher Ratio (-10,000)	-5.10 (0.91)	--	--	-2.47 (1.05)	-7.94 (0.65)	-13.02 (0.60)	-3.53 (1.39)	0.06 (1.71)
3. Education x Term Length (-10,000)	--	3.62 (0.30)	--	1.30 (0.43)	1.14 (0.33)	3.13 (0.31)	1.07 (0.45)	-1.67 (0.64)
4. Education x Relative Teacher Wage (-100)	--	--	1.85 (0.13)	1.50 (0.16)	1.54 (0.12)	2.09 (0.11)	0.85 (0.25)	0.81 (0.26)
5. Education x Age (-100)	0.03 (0.01)	0.04 (0.01)	0.03 (0.01)	0.04 (0.01)	0.06 (0.01)	0.12 (0.01)	0.03 (0.01)	-0.07 (0.01)
6. Education x Born 1930-39 (-100)	0.89 (0.06)	0.92 (0.06)	0.97 (0.06)	0.95 (0.07)	0.87 (0.06)	1.06 (0.07)	0.76 (0.07)	0.32 (0.11)
7. Education x Born 1940-49 (-100)	1.96 (0.11)	1.99 (0.10)	2.06 (0.11)	2.04 (0.11)	1.63 (0.06)	2.11 (0.10)	1.75 (0.11)	2.58 (0.19)
8. Post-HS Educ. (-100)	--	--	--	--	--	--	--	-12.79 (2.84)
9. Post-HS Educ. x Pupil/teacher Ratio (-10,000)	--	--	--	--	--	--	6.15 (1.83)	0.89 (2.75)
10. Post-HS Educ. x Term Length (-10,000)	--	--	--	--	--	--	-0.38 (0.39)	5.68 (1.37)
11. Post-HS Educ. x Relative Teacher Wage (-100)	--	--	--	--	--	--	0.66 (0.43)	0.12 (0.45)
12. Post-HS Educ. x Age (-100)	--	--	--	--	--	--	--	0.09 (0.02)
13. Post-HS Educ. x Born 1930-39 (-100)	--	--	--	--	--	--	--	1.17 (0.25)
14. Post-HS Educ. x Born 1940-49 (-100)	--	--	--	--	--	--	--	-1.65 (0.44)
15. 50 State-of-Residence Dums.	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes
16. 48 State-of-Birth Dums.	Yes	Yes	Yes	Yes	No	No	Yes	Yes
17. R ²	0.181	0.182	0.182	0.182	0.178	0.161	0.183	0.184

Notes: Education is defined as the maximum of zero, and years of completed schooling minus the number of years of schooling of the second percentile of the education distribution for the individual's state of birth and cohort. Each equation also includes potential experience and its square, a dummy indicating current marital status, a dummy indicating residence in an SMSA, 9 region of residence dummies interacted with years of education, and an intercept. Sample size is 265,618.

Table 10

One Step Estimates of the Effect of School Quality on Earnings:
White Males, 1980 Census

Dependent Variable: Log Weekly Earnings
(standard errors in parentheses)

Independent Variable	Education Spline at Threshold		Linear Education	
	(1)	(2)	(3)	(4)
1. Education (+100)	-0.72 (0.92)	-3.40 (1.27)	7.88 (0.63)	0.33 (1.23)
2. Education x Pupil/Teacher Ratio (+10,000)	-2.47 (1.05)	-8.08 (1.29)	1.46 (0.74)	-3.59 (1.29)
3. Education x Term Length (+10,000)	1.30 (0.43)	3.60 (0.64)	0.47 (0.27)	4.34 (0.62)
4. Education x Relative Teacher Wage (+10,000)	1.50 (0.16)	1.23 (0.23)	0.30 (0.11)	1.16 (0.23)
5. Education x Age (+100)	0.04 (0.01)	0.04 (0.01)	-0.06 (0.01)	-0.06 (0.01)
6. Education x Born 1930-39 (+100)	0.95 (0.07)	0.87 (0.07)	0.21 (0.04)	0.19 (0.04)
7. Education x Born 1940-49 (+100)	2.04 (0.11)	1.90 (0.11)	0.04 (0.05)	0.38 (0.05)
8. Pupil/Teacher Ratio (+100)	--	1.15 (0.13)	--	1.15 (0.18)
9. Term Length (+100)	--	-0.25 (0.05)	--	-0.45 (0.08)
10. Relative Teacher Wage	--	-0.01 (0.02)	--	-0.16 (0.03)
11. R ²	.182	.182	.182	.182

Notes: See notes to Table 9. In columns (1) and (2) education is defined as in Table 9. In columns (3) and (4) education is defined as years of completed education.