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LABOR MARKET EFFECTS OF SCHOOL
QUALITY: THEORY AND EVIDENCE

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ABSTRACT

This paper presents an overview and interpretation of the literature relating school quality to students' subsequent labor market success. We begin with a simple theoretical model that describes the determination of schooling and earnings with varying school quality. A key insight of the model is that changes in school quality may affect the characteristics of individuals who choose each level of schooling, imparting a potential selection bias to comparisons of earnings conditional on education. We then summarize the literature that relates school resources to students' earnings and educational attainment. A variety of evidence suggests that students who were educated in schools with more resources tend to earn more and have higher schooling. We also discuss two important issues in the literature: the tradeoffs involved in using school-level versus more aggregated (district or state-level) quality measures; and the evidence on school quality effects for African Americans educated in the segregated school systems of the South.

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The effectiveness of school spending is a critical issue for public policy. Education is the single largest component of government spending in the United States, accounting for 14 percent of combined federal, state and local government expenditures.¹ In addition, the private sector makes a major financial contribution to education through private schooling and through the opportunity cost of students' time in school. Any improvement in the effectiveness of schools could have vital implications for the nation's 48 million school children, or generate huge savings for taxpayers, or both. In the past two decades the issue of school quality has taken on even greater significance, as the labor market rewards to skill have risen and U.S. schools have come under renewed criticism for failing to meet international standards.

In this paper we present an overview and evaluation of the literature on the impact of school quality on students' subsequent labor market success. School quality is measured by the level of resources available in the school, district, or state where the student grew up, such as expenditures per student or the pupil-teacher ratio. Achievement is measured by students' subsequent earnings in the labor market and by their educational attainment. This approach contrasts with much of the research on school quality conducted by psychologists, sociologists and political scientists, who tend to measure student achievement by standardized test scores.² To economists, however, the labor market is a natural yardstick for measuring the effectiveness of schools. Schools that increase the earnings of their students meet an objective "market test". In the standard economic model of schooling (e.g., Becker, 1967), education is treated as an investment: current resources are spent while the student is in school in anticipation of higher

¹See Statistical Abstract of the United States: 1994 (114th edition), Washington, DC: U.S. Bureau of the Census, Table 464.

²If standardized test scores were highly correlated with economic success, the distinction between test scores and earnings would be less important. But many studies find only a weak link between standardized tests and earnings. For example, the addition of test score information to the earnings models reported by Griliches and Mason (1972, Table 3) improves the R-squared of their models by less than one-half of a percentage point. Using more recent data, Murnane, Willet and Levy (1995, Tables 3 and 4) find that adding a math test score raises the R-squared by 1.7 percentage points for men and 4.0 percentage points for women. Furthermore, recent research on the GED test indicates that simply passing the test has no significant effect on labor market outcomes (Heckman and Cameron, 1993). These findings underscore the importance of directly examining labor market outcomes.

income later in life. This framework lends itself naturally to a consideration of the costs and benefits of expenditures on school resources. A reduction in the pupil-teacher ratio, for example, has immediate and readily measured costs. Monetary benefits, if any, are only realized over students' lifetimes.

We hasten to add, however, that raising students' earnings is not the only measure of the effectiveness of schools, or the only purpose of schooling. Schools may have other economic or non-economic effects that are as important as their effect on earnings (see Haveman and Wolfe, 1984). For example, Milton Friedman (1962) argued for compulsory schooling and government-subsidized education because of the presumed positive externalities of an educated electorate. Nevertheless, in this paper we focus narrowly on the monetary benefits of education.

We begin in Section I with a simple theoretical model of school quality, educational attainment, and earnings that provides a unifying framework for analyzing many of the issues that arise in the literature. An important intuition arises from this model: if higher school quality increases the payoff to each additional year of schooling, it may also lead some students to acquire more schooling. This extra schooling creates a potential "selection" problem, in that the characteristics of students with a given level of schooling will change with a change in school quality. Our theoretical model suggests it is important to examine the impact of school quality on both the payoff to additional years of schooling and on students' eventual educational attainment.

The second section of the paper summarizes previous studies of the connection between school resources and students' earnings and educational attainment. A variety of evidence suggests that students who were educated in schools or areas with more resources tend to earn more once they enter the labor market, holding other factors constant. For example, several studies have found that the estimated payoff to a year of education increases as the average quality of schooling in a state increases. Moreover, the earnings premium associated with higher

quality schooling appears to be greatest for those who have attended school the longest, which is consistent with the view that school quality is responsible for the earnings differences, rather than some omitted characteristic of students.

Other evidence based on the variation in school spending at a finer level of analysis also suggests that higher quality schooling is associated with higher earnings. For example, in a series of studies of army veterans, Paul Wachtel (1975, 1976) found a statistically significant relationship between district-level expenditures and students' subsequent earnings. A notable exception to this conclusion is a set of recent studies that focus on the earnings of relatively young workers from micro data sets, including the National Longitudinal Survey of Youth (NLS-Y). These studies generally find statistically insignificant effects of school resources on earnings. On this basis, some analysts (e.g. Betts, forthcoming) have concluded that school resources don't matter. We reconsider the evidence from these studies and reach a somewhat different conclusion. In particular, we argue that specific aspects of these data sets, including the young age of the individuals and the relatively small number of observations, make it very difficult to obtain precise estimates of any school quality effects. When comparable specifications are estimated with NLS-Y and Census data, the estimated school quality coefficients typically are not statistically different.

Most of the studies surveyed in Section II also suggest that greater school resources are positively associated with students' educational attainment. For example, this conclusion emerges from studies that examine the effect of high-school resources on student graduation rates, as well as from studies that relate years of educational attainment to the average quality of education in one's state or school district. As suggested by our theoretical model, higher school quality seems to lead to higher earnings by increasing the payoff per year of schooling, and by encouraging students to stay in school longer. Reduced form models, which relate earnings directly to school quality, typically show that a 10 percent increase in educational expenditures per student is

associated with a 1 to 2 percent increase in the students' eventual annual earnings.

The third section of the paper evaluates the impact of the level of aggregation at which school resources are measured. Some researchers have argued that estimates based on state-level or district-level school quality measures are biased toward finding positive effects of school quality. The tradeoffs involved in using more or less aggregated measures are considered in some detail. The conclusion of this analysis, and of the studies reviewed in Section II, is that the level of aggregation does not appear to be an important factor in determining the sign or magnitude of the relationship between school resources and either the payoff per year of education or the level of educational attainment.

In the fourth section of the paper we review the evidence on the effects of school quality for African Americans who were educated in the segregated school systems of the South. The quality of schooling available to black children in different states and across different cohorts varied widely -- mainly for political and historical reasons. Thus, some of the concerns that arise in interpreting the correlation between school quality and the subsequent earnings of white children, such as the role of family background or unmeasured ability factors, are lessened. The available evidence suggests that the effects of school quality for black workers are very similar to the effects for white workers, providing support for a causal interpretation of the link between school resources and student outcomes.

Finally, our conclusion offers some suggestions for additional research that might shed further light on the effects of school resources on labor market outcomes, and that might help reconcile the apparent conflict between studies that relate school quality to earnings and those that focus on test scores.

I. A Theoretical Model of School Quality, Education, and Earnings

As a starting point for discussing the labor market effects of school quality it is useful to outline a simple theoretical model of individual earnings and schooling determination. Let y_{is} represent the earnings of individual i who attended school system s . We defer discussion of the level of aggregation represented by s : in principle, s refers to the specific set of schools potentially attended by i . Let E_{is} represent the level of education of individual i . Assume that log earnings are determined by a simple linear equation:

$$(1) \quad \log y_{is} = a_i + b_s E_{is} + u_{is} ,$$

where a_i represents a person-specific intercept (person i 's "ability"), b_s represents the marginal productivity of schooling acquired from system s , and u_{is} represents a random term.³ It is natural to assume that higher quality leads to a higher marginal return to each additional year of schooling, e.g. $b_s = b(Q_s)$, where Q_s is a quality index and $b'(Q) \geq 0$. In a simple static model, equation (1) represents the budget constraint facing individual i prior to the choice of a specific level of education.

To close the model, assume that individual preferences are represented by a utility function of the form:

$$(2) \quad U(y_{is}, E_{is}) = \log y_{is} - f(E_{is}),$$

where f is a quadratic function with a person-specific slope:

$$f(E_{is}) = c_i E_{is} + k/2 \cdot E_{is}^2 .$$

Variation in c_i captures differences across people in the "cost" of schooling, associated with differences in access to funds (Becker, 1967), differences in tastes or family background, or differences in aptitude for schooling (Spence, 1973).

Maximization of (2) subject to the constraint (1) gives rise to the optimal schooling

³We abstract from differences in the structure of earnings across labor markets, such as higher or lower levels of earnings for all workers, or differences in the return to education.

choice:

$$(3) \quad E_{i_s} = (b_s - c_i)/k .$$

In this very simple framework, all of the variation of schooling outcomes within a particular school system is attributable to differences in the cost of schooling. A more general model would allow for idiosyncratic differences in the return to schooling, possibly in combination with decreasing returns to schooling (as in Becker, 1967). As discussed in Card (1995), however, most of the literature on earnings and schooling can be reconciled within the simplified framework of (1) and (2), ignoring person-specific components in the return to education.

Equations (1) and (3) form a simple model of schooling and earnings with several important implications for analyzing the effect of school quality. Observe that the conventional OLS estimate of the return to schooling for individuals educated in system s has the probability limit

$$(4) \quad \rho_s = b_s - k \cdot r_{ac},$$

where r_{ac} represents the (theoretical) regression coefficient of a_i on c_i . The OLS estimate of the return to schooling for people who attended school system s differs from the true productivity effect b_s by an "ability bias" component: if a_i and c_i are negatively correlated (so that people with lower costs of schooling tend to have higher levels of earnings, regardless of their schooling choice), then ρ_s will be upward-biased as an estimate of b_s . Nevertheless, if the joint distribution of a_i and c_i is the same across school systems, then the ability bias component is constant, and estimates of the relationship between b_s and Q_s can be obtained by studying the relationship between ρ_s and Q_s across school systems.

Higher school quality in this model increases the slope of the observed relationship between log earnings and schooling. If schooling and ability are positively correlated, however, then higher quality also lowers the intercept of this relationship.⁴ The reason for this is that an

⁴This point was raised by Lang (1993) in the context of a more complex model of schooling.

increase in b_s leads all individuals to increase their schooling. For example, consider people who would acquire only the minimum level of schooling when faced with a given level of school quality (i.e., a given b_s). With higher school quality, some individuals in this group with the lowest costs of schooling will acquire an additional year of schooling. If costs and ability are negatively correlated, this shift will lower the average ability of those who remain at the minimum level of schooling, causing the intercept of the earnings-schooling relationship to fall.

Formally, the conditional expectation of individual ability given observed schooling and b_s is

$$E(a_i | E_{is}, b_s) = \text{constant} + r_{ac} (b_s - k \cdot E_{is}),$$

which is a decreasing function of b_s if $r_{ac} < 0$. Thus the theoretical regression function that relates log earnings to schooling (conditional on b_s) is:

$$\begin{aligned} E(\log y_{is} | E_{is}, b_s) &= E(a_i | E_{is}, b_s) + b_s E_{is}, \\ &= \text{constant} + r_{ac} b_s + (b_s - k \cdot r_{ac}) E_{is}. \end{aligned}$$

A rise in Q_s lowers the intercept and raises the slope of this function, as illustrated in Figure 1. The cross-over point occurs at the level of education $E = -r_{ac}$.⁵ For example, if we assume that a_i and c_i are normally distributed, and calibrate the model so that the standard deviation of education is 3.3, the standard deviation of log earnings is 0.50, the average level of education is 12, and ability bias leads to a 20 percent upward bias in the OLS estimate of the return to schooling, then the cross-over occurs between 9 and 10 years of education.

Examination of Figure 1 leads to three observations regarding the measurement of the earnings gains associated with higher school quality. First, higher school quality does not necessarily lead to higher average earnings conditional on education. If schooling and ability are positively correlated, an increase in school quality may be associated with lower average earnings

⁵If $r_{ac} = 0$, schooling and ability are uncorrelated and an increase in Q_s rotates the earnings-schooling relationship at the origin.

at low levels of education, and little or no effect on average earnings at intermediate levels of schooling. A second and related observation is that evaluations of school quality based on samples that exclude highly-educated workers may understate the true effects of higher quality. Third, measures of the effect of school quality on the slope of the earnings-schooling relationship may capture only part of the benefits of higher quality. If schooling is endogenously determined, a full assessment of the effect of school quality should compare the unconditional distributions of earnings associated with high and low quality school systems.

II. Summary of the Literature

The empirical economics literature has focused on two main questions concerning education and earnings. The first, and most widely studied, concerns the interpretation of the correlation between education and earnings. The second, and less widely studied, is how factors like school spending affect the relationship between schooling and earnings. Although the focus of this paper is on the second question, it is helpful to begin with a brief overview of the literature on the first.

a. The Return to Years of Education

The finding that average earnings are higher for individuals with more schooling is one of the most strongly established facts in social science. Figure 2 illustrates the relationship between weekly earnings and years of schooling, using data on the (adjusted) mean log weekly wage of three birth cohorts of white men from the 1980 Census.⁶ The figures show that earnings rise almost linearly with years of education, for education beyond a minimal threshold (corresponding roughly to the second percentile of the education distribution). There are dips

⁶These figures are from Card and Krueger (1992a). The log earnings have been adjusted for marital status, experience, state of residence, and residence in an SMSA.

at 11 and 15 years of schooling, and a jump at the 16th year, suggesting the presence of "sheep skin" effects. Thus, a linear model of log earnings and education provides a reasonable description of the data, although formal goodness-of-fit tests will reject linearity with large enough samples.⁷ Because of its relatively good fit, and its close connection to simple theoretical models (e.g. Mincer, 1974 and Willis, 1986), the log-linear functional form has dominated the empirical literature. Most estimates indicate that the marginal return per year of schooling is in the range of 6-10 percent, with a trend toward increasing returns during the 1980s (e.g., Levy and Murnane, 1992).

A vast literature has investigated the relationship between earnings and years of schooling with the aim of understanding the forces behind the positive association between earnings and years of education. The early wave of these studies tried to explicitly control for individuals' characteristics, such as IQ and parental education (see Griliches, 1977). A second set of studies compares identical twins with different levels of education to control for family background and genetic characteristics (see Behrman et al., 1980, Ashenfelter and Krueger, 1994, and Miller, Mulvey, and Martin, 1995). Yet another strand of this literature has tried to estimate the monetary payoff to schooling by comparing workers who obtained different levels of education for reasons having little to do with innate abilities or background characteristics, such as those who are compelled to stay in school longer because of compulsory schooling laws (see Angrist and Krueger, 1991). Finally, estimates of the return to schooling and schooling choices derived from structural econometric models tend to be similar to, or greater than, the simple OLS estimates (e.g., Willis and Rosen, 1979).

Our interpretation of this literature is that additional years of schooling tend to lead to higher earnings, and that this relationship results primarily from the extra schooling itself rather

⁷Using a Box-Cox procedure, Heckman and Polachek (1974) find that earnings and schooling have an approximately log-linear relationship. See Solon and Hungerford (1987) and Park (1994) for further evidence on sheepskin effects.

than extraneous factors. Omitted factors, such as imperfectly measured ability, may bias the OLS estimate of the return to education upward, but our reading of the evidence is that the bias is on the order of just 10-20 percent. Moreover, measurement error in reported education appears to bias the OLS estimate down by roughly the same magnitude. Thus, these two potential sources of bias appear to us to approximately cancel out.⁸ Interestingly, Zvi Griliches (1977) reached essentially the same conclusion in his Presidential Address to the Econometric Society nearly two decades ago.

b. School Quality and Earnings

The literature on school quality and earnings is much less voluminous than the literature on years of education and earnings. This is partly due to data limitations. Studies of school quality must combine data on the quality of schools that workers attended, or could have attended, with data on their labor-market outcomes. In the literature, school quality is typically measured by expenditures per pupil, or by school resources such as the pupil-teacher ratio and teacher pay. In reviewing the literature, we focus on expenditures per pupil as an index of school quality. We also summarize the evidence on the effect of the pupil-teacher ratio, because this is the policy variable typically of interest to decision-makers⁹, and because variation in the pupil-teacher ratio accounts for close to one-half of the variability in total expenditures per student.¹⁰ Some of the conclusions may hold for other measures of school resources, such as average teacher pay, but we do not extensively review the evidence here.

⁸This evidence is reviewed in Card (1995).

⁹For example, the Nevada legislature passed a law in 1990 lowering the pupil-teacher ratio in the first three years of primary school for all schools in the state.

¹⁰While differences in the pupil-teacher ratio account for 43 percent of the variation in total expenditures per student across school districts, differences in average teacher salary account for only about 8 percent. These calculations pertain to New Jersey; we thank Cecilia Rouse for providing them.

The research on earnings and school quality can be divided into four general classes of studies, based on the statistical model used to link school quality to earnings.¹¹ The first class of models simply adds school quality variable(s) to the right hand side of a conventional wage equation that also controls for education, experience, and other worker characteristics. The typical model estimated in such "Class I" models is:

$$(5) \quad Y_{is} = \beta X_{is} + \rho E_{is} + \theta Q_s + \epsilon_{is},$$

where Y_{is} represents the logarithm of earnings of worker i who was educated in school system s ; X_{is} is a set of covariates such as experience, region of residence dummies, and marital status; E_{is} represents years of education, Q_s is a measure of the quality of schooling (e.g., expenditures per student) in school system s , measured at the state, school district, or school level; and ϵ_{is} is a random error term. The parameter θ measures the effect of a unit change of school quality on log earnings. Notice that this specification implies that school quality has the same impact on expected earnings, regardless of the individual's education level.

A second class of models recognizes that school quality may influence the education slope as well as the intercept of the earnings function. These specifications are more consistent with the theoretical model in the previous section, in that school quality has a potentially larger effect on individuals' earnings if they stay in school longer. A typical statistical model in such "Class II" studies is:

$$(6) \quad Y_{is} = \beta X_{is} + \rho E_{is} + \theta Q_s + \varphi E_{is} Q_s + \epsilon_{is},$$

where all variables are defined as above. In this model, the parameter θ measures the impact of school quality on the intercept of the earnings function, while the parameter φ measures the impact of school quality on the slope of the earnings-education relationship. If school quality has no effect on educational attainment, the effect of a unit change in school quality on the log wage of individuals with E years of schooling is $\theta + \varphi E$. If school quality affects educational

¹¹For a less technical presentation of this material, see Card and Krueger (1995).

attainment, the total effect of school quality is $\theta + \varphi E + (\rho + \varphi Q) \cdot \partial E / \partial Q$, where the third term reflects the marginal effect of a change in school quality on educational attainment ($\partial E / \partial Q$).

A third class of studies focuses on estimating the impact of school quality on the slope of the return to education (the parameter φ), having absorbing any level effect of school quality (θQ) by including unrestricted dummies for the areas where individuals went to school. This approach enables researchers to control for differences across labor markets in the average level of earnings (through market-specific intercepts for the place of work), as well as for differences in the rate of return to education across local labor market (through market-specific interactions with education). In these models the school quality effect φ is identified by comparing earnings of individuals who were educated in one locale and observed working in another. In Card and Krueger (1992a), for example, we implement a two step variant of such a model. In the first step we estimate the return to education for individuals from each state of birth and cohort with the following model:

$$(7) \quad Y_{ijbc} = \delta_{bc} + \mu_{jc} + \beta X_{ijbc} + (\gamma_{bc} + \rho_{rc}) \cdot E_{ijbc} + \epsilon_{ijbc},$$

where i indexes individuals, j represents the state where the individual currently lives and works, b represents the state where the individual was born (and by assumption, was educated), and c represents a birth cohort. This model includes unrestricted state-of-birth intercepts for each cohort (δ_{bc}) as well as unrestricted state-of-residence intercepts for each cohort (μ_{jc}). The model also allows the rate of return to education to differ across regions of residence (r) by cohort (the ρ_{rc} interactions). The key parameters are the state-of-birth-specific rates of return to education, estimated separately for each cohort (γ_{bc}).

In the second stage, we relate the returns to education for each state of birth and cohort to state-level school quality measures and other variables using Generalized Least Squares (GLS):

$$(8) \quad \gamma_{bc} = \alpha_b + \alpha_c + \varphi Q_{bc} + \Omega W_{bc} ,$$

where α_b represents an unrestricted state-of-birth effect in the return to education, α_c represents an unrestricted cohort effect, Q_{bc} represents average school quality at the time cohort c attended school in state b , and W_{bc} is a vector of state-of-birth-by-cohort level variables such as the average education of parents. As in equation (6), the parameter φ measures the effect of school quality on the slope of the earnings-education gradient. Unlike equation (6), however, any effect of school quality on the intercepts is absorbed by the unrestricted state-of-birth effects in the first step model. Notice that this model could also be estimated in one step by substituting equation (8) into equation (7). The two-step estimator, however, is asymptotically unbiased and efficient if proper GLS weights are used in the second step.¹² Moreover, the two-step approach is computationally attractive, and allows researchers to easily investigate the robustness of any relation between school quality and the return to education.

Finally, some studies have estimated a fourth class of models: reduced form relationships between earnings and school quality, without conditioning on educational attainment. These "Class IV" models are of the form:

$$(9) \quad Y_{is} = \beta X_{is} + \pi Q_s + \epsilon_{is}.$$

Because this model excludes educational attainment, the parameter π reflects both the direct effect of school quality on earnings, and any indirect effect of school quality on earnings via its effect on educational attainment. One limitation of these models (compared to Class III models) is that they do not control for cohort-specific unobserved factors associated with the area in which an individual was educated: unrestricted place-of-birth effects would absorb the school quality

¹²See Hanushek (1974).

variables.

Table 1 summarizes 24 estimates of the effect of school spending on earnings from 11 different studies. The estimates are derived from 8 independent data sets and 16 mutually exclusive subsamples. We selected the set of studies in the table by searching the Journal of Economic Literature index, and by examining citations in known papers and past issues of selected education journals. We included only those studies that report information on their coefficients and standard errors, in order to evaluate the power of the estimates.¹³ The table reports estimated elasticities of earnings with respect to spending per student. For example, if a study regressed log earnings on log expenditures per student, we simply report the estimated expenditure coefficient. For studies that estimate log-linear models, we convert the coefficient estimate to an elasticity using mean spending per student.¹⁴ The table also gives a description of the data and model the study used, and a designation of the class of model corresponding to the discussion above. Inevitably, there is some arbitrariness in the construction of such a summary table, but we have tried to include the estimates that were highlighted by the authors.

All 24 of the estimates in Table 1 show a positive effect of additional school spending on subsequent earnings. The odds of this happening purely by chance are less than 1 in 16 million. Of course, it is possible that many of the studies find a positive effect of school quality for spurious reasons (e.g., omitted variables), but one can clearly rule out chance as an explanation

¹³Furthermore, if an author published more than one article using the same data set to estimate a similar empirical specification, we only report results from one of the articles in Table 1, usually the most recent one. The table also is limited to studies of U.S. data. See Behrman and Birdsall (1983) for a study of the effect of school quality on students' subsequent income in Brazil.

¹⁴One study, Jud and Walker (1977), regressed the level of earnings on the level of expenditures per student. The elasticity for this study was calculated at the mean earnings in the sample, and at the mean expenditure per enrolled student for 1969-70, as reported in the Digest of Education Statistics, U.S. Department of Education, 1994.

for the consistent finding that more resources are associated with higher earnings. Most of the studies that report sufficient data to calculate a t-ratio for the estimate have statistically significant coefficients. Betts (forthcoming) similarly finds that 16 of 21 estimates have statistically significant effects of expenditures per pupil at the 10% level (see his Table 2).

The mean of the estimates in Table 1 (taking the midrange for Link, Ratledge and Lewis, 1980) is 0.152, and the median is 0.125; the interquartile range is 0.085 to 0.195.¹⁵ The consistency across studies is impressive in view of the fact that many use different data sets or samples, estimate varying specifications, and include different sets of control variables.

All of the estimates represented in Table 1 control for education, and are either Class I models or Class II models. Many of the studies also control for differences in family background, by including father's education, family income, IQ, or the Armed Forces Qualifying Test score. Questions could be raised about many of the specifications used in the literature. For example, it does not seem appropriate to us to include occupation dummies as explanatory variables, as Tremblay (1986) has done, because more and higher quality education may enable people to obtain jobs in better paying occupations. More generally, the functional forms used in some of the studies are open to question.

Despite the varying sets of control variables and specifications used in the literature, it is possible that the estimated school quality coefficients reflect the influence of omitted factors, rather than the true effect of school resources. In our view, the most important omitted variables are likely to be measures of family background and characteristics of the areas where individuals attended school. An important motivation for the Class II and Class III models is that many of

¹⁵The unweighted mean elasticity in Betts's Table 2 is 0.121.

these omitted variables are absorbed by state of birth effects (or by the quality "main effect" in the Class II models), while still identifying the effect of school quality on the rate of return to education.

Estimates of the elasticity of students' earnings with respect to the pupil-teacher ratio (or teacher-pupil ratio) are summarized in Table 2.¹⁶ The models underlying these estimates are either Class IV reduced form-models that exclude education or (for Grogger, 1994) Class I models that condition on educational attainment. For comparability across studies, log-linear coefficient estimates were converted to elasticities by assuming 17.9 students per class (or 1/17.9 for the teacher-pupil ratio).¹⁷ Specifically, if π is the coefficient on the pupil-teacher ratio in a log wage regression, a point elasticity η was calculated as $\eta = \pi (17.9)$. Notice that a log-linear specification implies a larger proportionate effect of the pupil-teacher ratio for larger class sizes. Thus, as average class sizes have shrunk in recent decades, these models would imply a lower elasticity of earnings with respect to changes in the pupil-teacher ratio.

All the studies except Grogger (1994) find that a lower pupil-teacher ratio is associated with higher earnings, although only two are statistically significant. The four negative estimates have an average elasticity of -0.053, and are all fairly close. A notable pattern that emerges from Table 2 is that the two recent studies, by Betts (1995) and Grogger (1994), find statistically insignificant effects. These studies have two features in common: they analyze relatively small micro data sets of young workers, and they use quality data from the worker's high school. Although Betts (1995) has argued that the weaker school quality effects in these two studies arise

¹⁶As in Table 1, we summarize only those studies that report both a coefficient estimate and standard error (or t-statistic) for the pupil-teacher variable.

¹⁷A pupil-teacher ratio of 17.9 was selected for comparability to the NLS-Y.

from the use of school-level quality data, we suspect that the insignificant findings are mainly a result of the age and sample sizes of the data sets.

In the NLS-Y sample used by Betts (1995), the average age of workers is 23; the average age in Grogger's HSB sample is similar. The youthfulness of the sample in studies of school quality is a potential problem for at least two reasons. First, many determinants of labor market performance are only revealed with experience. For example, it is widely acknowledged that the return to the quantity of schooling is understated among very young workers (Mincer, 1974).¹⁸ One might expect a similar understatement of the effect of school quality in the first few years of the work career. Indeed, this assumption approximately holds in Wachtel's (1976) comparison of returns to school quantity and school quality for the Thorndike-Hagen sample of veterans in 1955 and 1969. Between 1955 and 1969, as the average age of the sample rose from 32 to 46, the rate of return to education rose from 0.030 to 0.079 (163 percent), while the "return" to school quality (measured as the coefficient of school expenditures per pupil in a Class I regression model) rose from 0.291 to 0.684 (135 percent). Second, samples of young workers tend to under-represent individuals of a given age with higher education. If higher school quality leads individuals to acquire more education, such samples will contain too few earnings observations for individuals from higher-quality schools, leading to an understatement of any school quality effects.

The small sample sizes of the NLS-Y and HSB data sets make it potentially difficult to detect small but economically significant effects of school quality. Moreover, the set of models

¹⁸Using Saiger and Irwin's NLS-Y sample and a cumulative experience measure similar to the one used by Betts (1995), we estimate that the rate of return to schooling in the NLS-Y data set is 4.5 percent -- far below generally accepted estimates of the return to schooling in the 1980s. Murnane, Willet and Levy (1995) report a similarly low estimate of the return to a year of schooling for men in the 1986 High School and Beyond Survey: 4.4 percent.

that can be estimated in the NLS-Y or HSB is limited. For example, Heckman et al. estimate earnings models that include about 400 parameters, capturing subtle differences across states and cohorts in the level of earnings and the return to schooling. Models of this complexity cannot be precisely estimated with samples of only one or two thousand individuals.¹⁹

In light of these features it is interesting to compare the estimated school quality effects from the micro-level data sets to the estimates in the other literature. The reduced form models in Betts (1995, Table A-4) and Card and Krueger (1992a, Table 6) are the most comparable estimates in the two studies. Using the NLS-Y data, Betts estimates a reduced form model that excludes education and its interactions, and controls only for age, marital status, and region of residence. As shown in Table 2, this model yields an earnings elasticity of 0.043 ($t=1.70$) at the mean teacher-pupil ratio. The 95% confidence interval for this estimate runs from -0.01 to 0.09. The comparable elasticity evaluated at the same mean class room size in a similar reduced form model from our 1992a study is 0.074.²⁰ Thus, for comparable specifications, the NLS-Y data yield results that are not statistically distinguishable from estimates derived from Census data using state-level school quality data, although the confidence interval based on the NLS-Y is

¹⁹A third aspect of the NLS-Y and HSB data sets is the range of variation in measured school quality. The variance of measured quality across schools in either data set is relatively wide -- comparable to the historical variability of quality measures across states. For example, the variance in the pupil-teacher ratio for white men in the NLS-Y is 16.6, compared with a variance of 17.5 across states and cohorts in our 1992a paper. However, the observed variances of school quality in the HSB and NLS-Y may overstate the true variability of school quality across individuals since the quality measures pertain to just a single year of high school, and may also contain measurement errors.

²⁰The reduced form equations in our 1992a paper include essentially the same covariates. The reason for conditioning on age, rather than experience, is that experience is a function of educational attainment; workers with less education have more opportunity to gain work experience. Because the reduced form equations are intended to capture all direct and indirect effects of school quality (including educational attainment), we believe it is appropriate to condition only on age. When Betts controls for experience (measured by cumulative weeks of work) instead of age, the reduced form coefficient is less than half as large.

relatively large.²¹

In a replication of Betts's study, Saiger and Irwin (1995) also estimate a reduced form model with the NLS-Y data, using the pupil-teacher ratio rather than the teacher-pupil ratio. Their point estimate of the elasticity of earnings with respect to the pupil-teacher ratio evaluated at the mean is 0.05 ($t=0.52$): the associated 95% confidence interval is huge, ranging from -0.14 to 0.24. Moreover, adjusting their standard errors for the fact that the NLS-Y sample includes repeated earnings observations for the same individuals increases the width of the confidence interval.

c. School Quality and the *Return to Education*

Focusing on the impact of school quality on the slope of the earnings-education relationship may reduce the bias attributable to omitted variables, since these factors will primarily affect the estimate of the "main effect" of school quality on the level of earnings, or be absorbed by unrestricted state-of-birth effects. Table 3 summarizes the available estimates of the effect of the pupil-teacher ratio on the slope of the log earnings-education relationship. The table reports the change in the percentage return to a year of education for a proportionate change in the pupil-teacher ratio. The studies by Heckman et al. and our two studies implement a Class III two-step estimation procedure with state-level data; Betts uses a one-step Class II

²¹Betts argues that estimates from the NLS-Y can reject comparable estimates of the effect of the teacher-pupil ratio on earnings from the earlier literature. However, he reaches this conclusion by comparing Class III estimates from Card and Krueger (1992a) to estimates of a restricted Class II model in the NLS-Y data that excludes the teacher-pupil main effect. The state-of-birth effects in our model (and in Heckman et al.) absorb any direct effect of Q . The standard error of φ is over 6 times greater in Betts's model that includes the main effect for the teacher-pupil ratio than in the restricted model.

model with high-school level quality data.²²

When state-of-birth effects are omitted from the models estimated by us and by Heckman et al. (column 1), the pupil-teacher ratio tends to have a statistically insignificant effect on the return to education, but when state effects are added (column 2) the effect is statistically significant. In other words, the Census samples only show a systematic effect of the pupil-teacher ratio on the return to education when permanent differences in the rate of return to schooling across states are taken into account (through the α_b 's in equation 8). It is interesting to note that Betts's GLS estimate from the NLS-Y shows a relatively strong effect of the pupil-teacher ratio on the return to education, even though his model does not control for permanent state effects. Indeed, his GLS estimate of φ based on school-level quality data is larger than the estimates based on Census earnings data with state-level quality measures, although his OLS estimate for the same model has the opposite sign.²³

The estimates in Table 3 only pertain to the slope of the education-earnings relationship. Corresponding estimates of the effect of the pupil-teacher ratio on the intercepts are often positive, suggesting that varying class sizes are associated with a rotation of the relationship, as predicted by our theoretical model. Estimates from Census data in our 1992a study and in Heckman et al. suggest that the "cross-over" point of this rotation is about 12 years of education. The GLS estimates of the intercept effects reported by Betts (1995) imply a slightly higher point

²²Our estimates for Betts are taken from his Table 1 column 1 (for the OLS estimate), and from his Appendix Table A3, column 1, for the GLS estimate. Betts uses the teacher-pupil ratio, rather than the pupil-teacher ratio. Thus, we first convert his estimate of the effect of the teacher-pupil ratio on the return to education into an elasticity (by multiplying by $0.059 = 1/17$), and then multiply by -100 to obtain the elasticity of the percentage return to education with respect to the pupil-teacher ratio.

²³We are uncertain why the GLS estimate is so different from the OLS estimate. In principle, both estimates have the same probability limit.

of rotation (14.2 years), while Betts's OLS estimates imply a rotation in the opposite direction (although the coefficients in the OLS model are insignificant). We discuss the interpretation of this rotation effect in more detail below.

Although not included in our summary table, Lang (1993) also uses the NLS-Y data set to estimate the effect of school quality on the slope of the education-earnings relationship. His estimates, derived from a pair of non-linear equations with cross-equation restrictions, indicate that a reduction of the pupil-teacher ratio increases the education slope and lowers the intercept of the earnings function. Both of these effects are statistically significant at the 0.05 level. However, the reduction of the intercept dominates the steeper slope at all reasonable levels of education, suggesting that school quality has a negative effect on earnings conditional on any level of education.

It has been argued (e.g. Hanushek, Rivkin, and Taylor, 1995) that the use of aggregated quality data leads to an over-statement of school quality effects. In this regard, the estimates reported by Betts (1995) using school-level and state-level quality measures are informative. The OLS estimate of the elasticity of the education slope with respect to the state-average pupil-teacher ratio is 5.30, compared to 0.50 using the school-level pupil-teacher ratio. Both of these estimates are "wrong-signed" for the hypothesis that higher school quality raises the return to schooling. If anything, however, the estimate based on aggregate data under-states rather than over-states the effect of class size on the education slope relative to the estimate based on school-level data. In Section IV we present a more detailed discussion of the tradeoffs involved in using aggregated versus school-level quality measures.

Lastly, we note an interesting study that is not included in this table by Brattsberg and

Terrell (1995). These authors use 1980 Census data to estimate 58 separate returns to education for immigrants living in the U.S. based on their country of origin.²⁴ They then relate these estimated returns to characteristics of the home countries, including the pupil-teacher ratio, an index of teacher pay, educational attainment, GNP per capita, income inequality, distance from the U.S., region dummies, and political variables. The paper finds a statistically significant relationship between the pupil-teacher ratio and the return to education, and between the teacher wage and the return to education, both with their expected signs. For example, the elasticity with respect to the pupil-teacher ratio is -0.80 (standard error=0.17). Although it is unclear whether this estimate should be compared to the estimates in column 1 or 2 of Table 3, Brattsberg and Terrell's study provides suggestive evidence that immigrants from countries with higher quality schools have higher returns to education in the U.S. labor market. A potential limitation of this study is that differential selection biases may lead to differences in the returns to education for immigrants from different countries that are not attributable to school quality.

d. School Quality and Educational Attainment

The evidence in the preceding section suggests that improved school quality is associated with a steeper slope for the return to education function. As discussed in Section I, an increase in the payoff to a year of education would also be expected to induce students to stay in school longer and increase their educational attainment. We do not attempt an exhaustive survey of the empirical literature on this prediction, but our reading of the literature suggests that the evidence is consistent with this hypothesis. For example, over half of the studies in Betts's Table 10 find

²⁴To raise the likelihood that the immigrants were educated abroad, the sample is restricted to those who were at least 25 at the time they emigrated to the U.S.

a statistically significant relationship between the pupil-teacher ratio and educational attainment.²⁵

Nevertheless, Betts (forthcoming) questions the conclusion that school resources affect educational attainment. Like Hanushek, Rivkin, and Taylor (1995), Betts argues that studies of the link between school resources and schooling outcomes that use aggregated quality measures are biased toward finding a positive quality effect. While we consider this argument in more detail in the next section, we note that it runs counter to the results in Sander (1993). Sander relates high school graduation rates to the pupil-teacher ratio across 154 Illinois school districts. In 86 of the districts there is only one high school, so estimates for this sub-sample are equivalent to school-level estimates. If the level of aggregation matters, then one would expect an insignificant relationship in the subsample of single-school districts. Sander reports that the estimates are quite similar in single-school districts and the full sample: a 10 percent decrease in the pupil-teacher ratio is associated with a 1.5 percentage point increase in the graduation rate in the single-school subsample, and a 1.4 percentage point increase in the full sample.

Two other recent studies are also inconsistent with the conclusion that school quality has no effect on educational attainment if quality is measured at the school level. First, Lang (1993) finds a statistically significant relationship between educational attainment and high school quality with the 1988 wave of the NLS-Y. Second, Boozer (1993) finds generally positive effects of smaller class size on the drop out rate for whites and blacks in the National Educational Longitudinal Survey and in the High School and Beyond data sets. In three of four subsamples

²⁵Additionally, Ribar (1993) finds a positive association between state-level expenditures per student and high school graduation rates for women in the NLS-Y, and Murray, Evans and Schwab (1995) find a significant relationship with district level data for 15 states using Census data.

that he examines, a lower pupil-teacher ratio is significantly associated with a lower drop out rate.²⁶

One of the older studies using disaggregated quality data that finds an insignificant effect on educational attainment, by Wachtel (1975), deserves further comment. Individuals in this data set, the Thorndike-Hagen data, were selected in part on the basis of their education. The sample has an extremely high level of average education for the cohort of men involved, averaging over 15 years. Thus, it may not be too surprising that this sample shows an insignificant effect.²⁷

In interpreting the relationship between educational attainment and resources per student, it is worth noting that the observed correlation between these variables may give a downward-biased estimate of the true effect of per-capita spending on educational attainment. To see this point, consider an increase in enrollment in a particular district or state induced by a change in family background or tastes for education. The rise in enrollment will tend to raise the pupil-teacher ratio and depress spending per student, unless school taxes and the number of teachers are increased in proportion to the new enrollment. Thus unobserved taste factors or family income changes that lead to variation in enrollment rates across districts or states could spuriously induce a negative correlation between educational attainment and per-student expenditures. This conclusion may be reversed, of course, if a rise in interest in education leads to a rise in enrollment and a greater than proportional increase in taxation and spending.

In summary, we believe that the literature generally supports the hypothesis of a positive link between school quality and educational attainment. Although it is unclear whether this

²⁶One subsample shows a significant effect with the opposite sign. The use of sample weights that adjust for non-random attrition has an important effect on Boozer's estimates from the HSB data set.

²⁷We thank Paul Wachtel for bringing this to our attention.

relationship arises because students respond to the economic incentives created by a shift in the return to schooling, or because they find it more enjoyable to attend schools with smaller classes or better-paid teachers, the relationship seems to exist.

e. Heckman, Layne-Farrar, and Todd

In their 1995a paper, Heckman et al. extend the third class of models described above in many important directions. Table 4 summarizes the main features of their analysis and their principle findings. First, they estimate a variant of Card and Krueger's (1992a) two-step model using data from the 1970, 1980 and 1990 Censuses. Their variant allows the return-to-education-by-state-of-birth coefficients in equation (7) to also vary by region of residence, as follows:

$$(7') \quad Y_{ijbc} = \delta_{bc} + \mu_{jc} + \beta X_{ijbc} + \gamma_{rbc} \cdot E_{ijbc} + \epsilon_{ijbc}.$$

In the first step of their analysis they estimate some 1,323 (= 49 states of birth x 9 regions of residence x 3 cohorts) slope coefficients, denoted γ_{rbc} .²⁸ In the second step, they relate these coefficients to cohort effects, state-of-birth effects, region of residence effects, and measures of state-average school quality for the particular cohort and state of birth. Their findings are generally consistent with the estimates reported in our original paper (Card and Krueger, 1992a), although their estimated school quality effects tend to be slightly larger. Their estimate of the effect of school quality on the return to education is especially large in 1990, a year in which the payoff to skills is considered to be at a very high level.

Heckman et al. next relate the state-of-birth intercepts to school quality measures for each state and cohort. These results suggest that higher school quality is associated with a lower

²⁸It is impressive to note that these estimates are obtained from a model that also includes a large number of other covariates.

intercept, as in the theoretical model depicted in Figure 1. Card and Krueger (1992a, Figure 5) found a similar result, although our analysis was somewhat complicated by the 2% threshold we assumed in the education variable.

Their next extension is to include aggregate region-of-residence variables in their second-stage model, to capture the effects of aggregate supply and demand factors on the return to education. In this specification they remove the region of residence dummies (which would absorb any regional variables) and include instead the regional means of the quality variables. Once the regional quality means are included, the aggregate region-of-residence variables are essentially orthogonal to the state-and-cohort specific quality measures, so the inclusion of the aggregate variables has almost no effect on the estimated quality coefficients.

A fourth issue they address is non-random migration. If interstate mobility induces a correlation between the state-of-birth-specific return to education and the quality of education, then the second-step estimates may be biased. In Heckman et al.'s specification, biases associated with inter-regional mobility are eliminated by including features of the region of origin and region of residence in the second step equation. Specifically, Heckman et al. include a quadratic in the distance between the region of birth and the region of residence. They simultaneously free up the effect of school quality across regions by interacting the school quality variables with a set of 9 region-of-residence dummies. Although they reject the hypothesis that the pupil-teacher ratio has the same effect on the return to education in different regions, this hypothesis cannot be rejected for teacher salary.²⁹ Furthermore, in almost all regions higher school resources are associated with a higher return to education. To summarize these estimates,

²⁹Indeed, the F-test for the teacher salary restrictions are far in the left tail of the distribution, with p-values around 0.99.

in Table 5 we report unweighted averages of the pupil-teacher coefficients for the nine Census regions.³⁰ The average pupil-teacher coefficients are generally similar to the coefficients from more restrictive models that exclude the distance variables and impose constant quality effects across regions. The only exception is 1990, when the average unrestricted quality coefficient is three times larger than the restricted one. This pattern suggests that the assumptions of random migration and constant returns to school quality across regions may lead to a downward bias in the estimated effect of school quality.

Their fifth contribution is to relax the linearity assumption in the first step equation by allowing for a piece-wise linear earnings-education relationship with discrete jumps at 12 and 16 years of education. Each jump introduces an additional 76-120 parameters to be estimated in the first step equation. They then relate the returns to education from these less-restrictive models to the school quality variables. These results, which are reported in Table 12 of their paper, show a weak and inconsistent pattern of the school quality variables at grade 12, but stronger results at grade 16. Taken together, 13 of the 18 coefficients (2 grade levels x 3 Census years x 3 quality variables) have the "right sign" for the hypothesis that higher school quality raises the return to education -- a pattern that is unlikely to occur by chance. Nevertheless, the results from the non-linear specifications are probably the weakest set of findings in Heckman et al.'s study of school quality effects. In our 1992a paper we similarly found that school quality had small and inconsistent effects on earnings for those with exactly 12 years of schooling.

A critical issue in the interpretation of these results concerns the selectivity of schooling. Although it may seem intuitively obvious that higher quality public schooling has its largest

³⁰The unweighted average represents the return that a person would face who had an equal probability of moving to each region. Of course, one could use different weights.

impact on students who go no further than high school, this intuition misses the fact that students with better elementary and secondary education may be more likely to enter college.³¹ The results described in the previous subsection and Heckman et al.'s findings (see row 7 of our Table 4) suggest that higher school quality is associated with greater educational attainment. If the "ability" of individuals at each level of education falls as school quality rises, we would expect to see small or even zero effects of school quality on earnings for those with intermediate levels of education, consistent with the nonlinear specifications reported by Heckman et al. In our view, then, it is still an open question as to how to interpret the effect of school quality on the level of earnings, conditional on education.

A sixth contribution of Heckman et al. is to provide nonparametric tests of the hypothesis that average wages of individuals educated in different regions have the same rank-order across different regional labor markets. The tests are carried out by cohort, conditional on various levels of education. Taken as a whole the evidence in support of this hypothesis is weak. Heckman et al. also correlate regional average quality measures with the mean wages of individuals who were educated in different regions and observed working in a given region (by education level and cohort). Again the correlations are weak. However, we suspect that these tests have limited power, since school quality effects tend to be relatively small compared to the background noise in earnings, and because the effects of school quality, conditional on education, may be weak at middle levels of education.

As a final comment, we note that simple reduced form models of the effect of school

³¹An analogy may be helpful. It is arguable that advanced-level undergraduate courses yield the greatest benefit for students who go on to graduate school. In an evaluation of an undergraduate program these benefits would be missed if students who continued their training were excluded from the analysis.

quality (i.e. Class IV models) may be quite useful in evaluating the overall effects of school quality. Inferences about school quality effects from reduced form models do not depend on the functional form of the earnings-education relationship, and incorporate any effect of school quality on education attainment. It is also possible to estimate reduced form models for different quantiles of the earnings distribution, revealing potential differences in the effect of school quality for individuals in the lower or upper tails of the distribution. A limitation of reduced form models is the presence of unobserved state-of-birth effects (or unobserved differences across school districts) that may lead to biases in the correlation between school quality and earnings. In our view, however, estimates from reduced form models are an important part of the collage of evidence linking school quality and earnings.

III. The Measurement of School Quality: Aggregation and Measurement Error

One of the key issues that arises in comparisons across the literature on school quality is the unit of measurement for the quality variables. Several authors have argued that the use of school quality measures at a higher level of aggregation (such as the district or state level) leads to systematic upward biases in the estimated effects of school quality, relative to the use of measures at the school level (e.g. Hanushek, Rivkin, and Taylor, 1995 and Betts, forthcoming). On the other hand, the use of aggregated quality measures may overcome certain biases that arise when quality is measured at a finer level. In this section we provide an overview of the tradeoffs involved in the choice of different units of observation, and evaluate some of the evidence on the relative biases associated with the alternative choices.

Conditional on observed educational attainment, a conceptually appropriate measure of

school quality is the average quality of the schools (or classrooms) actually attended by a given individual.³² Without conditioning on observed education, however, the conceptually appropriate definition of school quality is broader. An ideal measure of quality for an unconditional analysis should take into account the quality of the schools potentially attended by a given individual, and not just the quality of the classes or schools actually attended. Thus, in reduced form (Class IV) models, or in models of educational attainment, the school quality measures should reflect the quality of the overall school system available to an individual.

To evaluate the implications of using school quality measures at different levels of aggregation, we begin with a simple reduced form (Class IV) specification of school quality effects. Consider an individual (indexed by i) who attended or potentially attended "school system" s in state b . Let Q_{sb}^* denote the true average quality of this set of schools. Suppose that log earnings of the individual (Y_{isb}) depend on a set of covariates (X_{isb}) and on the true average quality of the school system:

$$(10) \quad Y_{isb} = \beta X_{isb} + \pi Q_{sb}^* + \epsilon_{isb} .$$

Let Q_{sb} represent an observed measure of quality, and write

$$Q_{sb} = Q_{sb}^* + u_{sb} ,$$

where u_{sb} is a random measurement error satisfying the classical properties $E(u_{sb})=0$ and $E(Q_{sb}^* u_{sb})=0$. Finally, decompose the error term ϵ_{isb} as:

³²It is usually assumed that the quality of each year of schooling exerts a constant influence on labor market outcomes, so that an unweighted average of quality measures over the educational career is a valid index of quality. If early education is more important than later education for determining lifetime opportunities, the quality of lower levels of schooling should be more heavily weighted. The only systematic evidence we are aware of on this issue comes from Wachtel (1976) who includes quality measures for primary and elementary schooling, as well as for college, in a model for the earnings of men in the Thorndike-Hagen sample who completed college (Wachtel, 1976, Table 2, column 3). His estimates suggest that the effect of elementary and secondary schooling quality is substantially larger than the effect of college quality.

$$\epsilon_{isb} = v_{sb} + \mu_b + \xi_{isb},$$

where v_{sb} represents a shared error component for all individuals who attended school s in state b , μ_b represents an error component shared by all individuals from state b , and ξ_{isb} is a purely idiosyncratic error.

Consider now an OLS estimator of π that uses the observed school-level quality variable. Assuming that the school quality measurement error is orthogonal to v_{sb} , the asymptotic bias of this estimator is

$$(11) \quad -\pi(1-\lambda) + \frac{\text{cov}(v_{sb}, Q_{sb}^* | X_{sb})}{\text{var}(Q_{sb} | X_{sb})} \\ + \frac{\text{cov}(\mu_b, Q_b | X_b)}{\text{var}(Q_b | X_b)},$$

where $\lambda = \text{var}(Q_{sb}^*)/\text{var}(Q_{sb})$ represents the reliability of Q_{sb} ; X_{sb} represents the mean value of the covariates for students from school s ; $\text{cov}(v_{sb}, Q_{sb}^* | X_{sb})$ represents the covariance of the school-level error component v_{sb} and Q_{sb}^* , conditional on X_{sb} (i.e., having partialled out X_{sb} from v_{sb}); $\text{var}(Q_{sb} | X_{sb})$ represents the variance of Q_{sb} across schools, conditional on X_{sb} ; and $\text{cov}(\mu_b, Q_b | X_b)$ represents the covariance of the state-level error component μ_b with mean state-level quality, conditional on the state-level means of the covariates (i.e., having partialled out the state means X_b from μ_b).

The first term in this bias expression, $-\pi(1-\lambda)$, represents the attenuation bias in the OLS estimator attributable to the noise in the school-level quality variable. This can arise because Q_{sb} is only measured for a subset of the schools attended by an individual who remained in the same school system (e.g., only high school), or because some individuals move across school systems, or from the fact that classroom sizes and teacher characteristics vary even within schools, or from random reporting errors.

The second term in the bias expression depends on the covariance of the school-level error component in earnings with Q_{sb}^* . An important source of such a correlation is the endogeneity of school quality. If children who would otherwise perform well in the labor market (conditional on their observed X's) attend higher-quality schools, then this covariance will be positive. This is the presumption in much of the literature, including Heckman, Layne-Farrar, and Todd (1995a, section V). On the other hand, if school funding is allocated by compensatory formulas this covariance will be negative. Interestingly, two recent studies of U.S. schooling (Akerhielm, 1995 and Boozer and Rouse, 1995), and another recent study of Israeli schooling (Lavy, 1995) all conclude that school resources are disproportionately targeted at the least able students, leading to a systematic negative correlation between student outcomes and expenditure.

The third source of bias depends on the covariance between the state-level error component in y_{iab} and state average quality. If children from different states have the same distributions of unobserved productivity factors, controlling for their observable characteristics, this term will be negligible. On the other hand, if children from states with higher quality schools would tend to earn more regardless of school quality, this term will be positive.

An alternative estimator of the school quality effect π uses the mean quality of schools in each state as a proxy variable for Q_{sb}^* . Consider the projection of Q_{sb}^* on state-level average quality Q_b and the covariates included in equation (10):

$$Q_{sb}^* = \alpha Q_b + \eta X_{iab} + v_{sb}.$$

In the absence of any individual-level or school-level X's, α will equal 1. More generally, α can be bigger or less than 1, although the inclusion of micro-level X's that are correlated with school quality (such as family income or parental education) would be expected to lead to a value of

$\alpha < 1$. Substituting this equation into (11) and taking probability limits, the asymptotic bias of the school quality coefficient using state-level quality measures is

$$(12) \quad (\alpha-1)\pi + \text{cov}(\mu_b, Q_b | X_b) / \text{var}(Q_b | X_b),$$

which depends on $(\alpha-1)$, and on the covariance of the state-level error component in earnings with state-level quality. In comparison to equation (11), however, the covariance of μ_b with Q_b is divided by the conditional variance of school quality across states, rather than the conditional variance across schools. It is informative to re-write equation (12) as:

$$(12') \quad (\alpha-1)\pi + \text{cov}(\mu_b, Q_b | X_b) / \text{var}(Q_{sb} | X_{sb}) \cdot R,$$

where $R = \text{var}(Q_{sb} | X_{sb}) / \text{var}(Q_b | X_b) > 1$, represents the ratio of the variance in school quality across schools to the variance across states, suitably adjusted for the covariates included in the underlying model. For example, among white men in the NLS-Y data set we estimate that $R \approx 5$ for the pupil-teacher ratio when X consists of just an intercept.

A direct comparison of the biases in reduced-form estimators based on school-level and state-level quality variables suggests that the state-level estimator will unambiguously dominate (in terms of a smaller asymptotic bias) if $\alpha = 1$ and $\text{cov}(\mu_b, Q_b | X_b) = 0$. If the latter condition is not met, then both estimators are biased, and the relative comparison hinges on the magnitudes of $\text{cov}(\mu_b, Q_b | X_b)$ and R , on the relative magnitudes of α and λ , and on the school-level covariance between quality and the unobserved error component in labor market outcomes.³³ In particular, the estimator based on state-level data will have a smaller asymptotic bias if and only if

³³Although some researchers have referred to the asymptotic bias in the estimator based on aggregate-level school quality as "aggregation bias" this is not the conventional use of the term (e.g. Theil, 1971, p. 561). Rather, aggregation eliminates some biases that exist in the "micro-level" estimates and magnifies others.

$$\pi(\lambda - \alpha) + \frac{\text{cov}(v_{sb}, Q_b | X_b)}{\text{var}(Q_b | X_b)} > \frac{\text{cov}(\mu_b, Q_b | X_b)}{\text{var}(Q_b | X_b)} (R-1) .$$

This is more likely, the smaller is α relative to λ , the smaller is R , and the smaller is the covariance between μ_b and Q_b , conditional on the observed covariates.

Examination of the asymptotic bias in the estimator based on average quality at the state level suggests a useful "specification check" for this estimator. Since the bias depends on $\text{cov}(\mu_b, Q_b | X_b)$, the addition of other state-level variables that are correlated with the average earnings of people from the state would be expected to lower the magnitude of the bias. Thus a useful check in such models is to compare the magnitude of the estimated quality effects as other aggregate control variables are added.³⁴

A third estimator of equation (10) is an instrumental variables (IV) estimator that uses state average quality measures as instruments for the school-level variables. To see the relation between this estimator (π^{iv}), and one in which the state averages are inserted directly into the reduced form earnings equation (π^{avg}), note that $\pi^{iv} = \pi^{avg}/\alpha$. Thus the IV estimate "corrects" π^{avg} for any deviation of α from 1, but has the same sign as π^{avg} .³⁵ If $\alpha \leq 1$, as we suspect is the case for most data sets, then the IV estimator will be larger (in absolute value) than the OLS estimator using state averages. The IV estimator provides a consistent estimate of the true quality effect if (and only if) $\text{cov}(\mu_b, Q_b | X_b) = 0$.

³⁴This approach was followed in Card and Krueger (1992a, Table 4) in the second step of a Class III model.

³⁵In this light, the IV estimates reported by Betts (1995, footnote 29) are puzzling. He reports that the IV estimate of the teacher-pupil effect, using the state-level teacher-pupil ratio as an instrument, is negative, while the OLS estimate using the state average data is positive (Table 6, column 1).

More Complex School Quality Models

The preceding discussion has focussed on reduced-form school quality models. Similar issues of measurement error and bias also arise in more complex specifications, such as the Class II and Class III models discussed in Section II. For example, consider a Class II model:

$$(13) \quad Y_{iab} = \beta X_{iab} + \rho E_{iab} + \theta Q_{ab}^* + \varphi E_{iab} \cdot Q_{ab}^* + \epsilon_{iab},$$

where E_{iab} represents the years of education of individual i , educated in school system s in state b (with quality Q_{ab}^*). As before, the error term ϵ_{iab} can be decomposed into a school-level effect, a state-effect, and an idiosyncratic effect:

$$\epsilon_{iab} = v_{ab} + \mu_b + \xi_{iab}.$$

(Note that these error components do not correspond to the errors in the reduced-form model (10): we use the same notation only for simplicity). Estimation of (13) with observed quality data leads to estimates of the quality effects θ and φ that contain biases depending on the reliability of the observed quality data, and on the covariances of individual education and school quality with the unobserved error components of Y_{iab} .

As with the reduced form models, it is also possible to estimate equation (13) using state-level averages of quality. Given the biased standard errors that arise from micro-level regressions that use aggregated explanatory variables, however, it may be more attractive to use a two-step estimation method, as in Card and Krueger (1992a) and Heckman et al. The first step estimates state-specific intercepts and education slopes; the second step regresses the state-specific coefficients on state quality averages and other state-level data. Let δ_b represent a state-specific intercept, and let γ_b represent the average state-specific return to education coefficient. Then the first-stage model can be written as:

$$y_{iab} = \beta X_{iab} + \delta_b + \gamma_b E_{iab} + \theta(Q_{ab}^* - Q_b^*) + \varphi(Q_{ab}^* - Q_b^*) E_{iab} + v_{ab} + \xi_{iab},$$

where $\delta_b = \mu_b + \theta Q_b^*$ and $\gamma_b = \rho + \varphi Q_b^*$. The first step estimate of the return to education in state b has probability limit

$$\gamma_b + \text{cov}(E_{iab}, \{ \xi_{iab} + v_{ab} + \theta(Q_{ab}^* - Q_b^*) + \varphi(Q_{ab}^* - Q_b^*) E_{iab} \}) / \text{var}(E_{iab}),$$

where the variance and covariances are interpreted as "within state" and conditional on X_{iab} , and we have assumed that the measurement error in quality averages to zero within any state.

The covariance of E_{iab} with the combined error component $\xi_{iab} + v_{ab}$ is equivalent to the conventional "ability bias" component that is widely discussed in the literature on the return to schooling (see Section I). This covariance will be positive if students who get more education would tend to earn more anyway. The covariance of E_{iab} with $\theta(Q_{ab}^* - Q_b^*)$ reflects a similar effect, associated with non-random sorting of individuals to schools. If people who attended higher quality schools in a given state tend to get more education, this term will be positive. Finally, the covariance of education with $\varphi(Q_{ab}^* - Q_b^*) E_{iab}$ reflects a higher-order interaction that may arise if the people who attended higher quality schools within a state have a higher return to education and acquire more education.

In the second step of the analysis, the estimated state-specific education slopes are regressed on measured state-level average quality variables. Ignoring sampling error, this will recover a coefficient estimate equal to φ (the true effect of quality on the return to education) plus a term reflecting the covariation across states between average school quality and the state-specific bias in the estimated return to education. The asymptotic bias in the first stage estimate of the return to schooling in state b is

$$\text{cov}(E_{iab}, \{ \xi_{iab} + v_{ab} + \theta(Q_{ab}^* - Q_b^*) + \varphi(Q_{ab}^* - Q_b^*) E_{iab} \}) / \text{var}(E_{iab}).$$

If this bias is either constant across states, or uncorrelated with average school quality, the two-step estimator of φ will be asymptotically unbiased.³⁶

An important observation about the first stage estimates of the return to education is that under the null hypothesis that $\theta = \varphi = 0$ (i.e., assuming that the true effects of school quality are zero) the bias in the estimated return to schooling for people from state b is

$$\text{cov}(E_{iab}, \xi_{iab} + v_{ab}) / \text{var}(E_{iab}).$$

This expression will be orthogonal to quality differences across states under fairly plausible assumptions. For example, one sufficient condition is that the joint distribution of individual "ability" ($\xi_{iab} + v_{ab}$) with the idiosyncratic determinants of the cost of education is the same across different states. Under this assumption, even though the returns to education for each state are upward-biased, a regression of the estimated returns on state-level school quality variables provides a valid test of the null hypothesis.

More generally, estimates of the state-specific returns to education are purged of the "standard" sources of bias in the measured relationship between school quality and the level of earnings, such as unobserved differences in family background or community-level characteristics (that concern Hanushek, Rivkin, and Taylor, 1995). These factors are absorbed by the unrestricted state effects in the first-stage model and will complicate any inferences about the effect of school quality on the intercepts of the earnings-schooling relationship. But the bias in φ is determined by a higher order omitted variable. For φ to be asymptotically biased, omitted variables must be correlated with the interaction $E_{iab}Q_{ab}$ after controlling for the effects of

³⁶We are ignoring biases that might arise if there is some slippage in the identification of which state an individual attended school. As shown in Card and Krueger (1992a), this is a problem in Census data, where state of birth must be used to infer where an individual attended school. We estimate that this bias attenuates the coefficients of school quality by 5-15 percent.

education and school quality (or unrestricted state dummies). It is unclear which direction, if any, these omitted effects might bias the estimated impact of school quality on the return to years of education. Most of the intuition for this problem, and assertions in the literature, come from simpler reduced form models. However, the fact that the variables that Card and Krueger (1992a, Table 6) and Heckman et al. add to equation (8) do not substantively change the estimates of φ suggests that these higher-order bias terms may be small.

IV. Lessons from the Experiences of Southern-Born Blacks

A final aspect of the link between school quality and earnings that is worth considering in more detail is the unique experience of African American children who were schooled in the segregated school systems of the South. Racially segregated schooling led to profound differences in school resources available to black and white children, and among black children educated in different states. In 1915, for example, the 18 jurisdictions with legally segregated schools had an average pupil-teacher ratio of 61 in black schools and 38 in white schools. By the 1953-54 school year, on the eve of the Brown versus Board of Education decision, the average pupil-teacher ratio fell to 32 in black schools and 28 in white schools. The differences in school quality were also enormous across states. For example, in 1953-54 the average pupil-teacher in black schools was 39 in Mississippi and 27 in Kentucky.

A particularly revealing comparison is provided by two neighboring states: North Carolina and South Carolina. Figure 3 shows the pupil-teacher ratio for black and white schools each year in South Carolina and North Carolina for most of the century. Although these two states are similar in many respects, they differed dramatically in the resources they provided for schooling,

especially for black children. Whereas North Carolina was among the most progressive of the non-border Southern states vis-a-vis black schooling, South Carolina was among the least progressive (see Harlan, 1958). By comparison, schools for whites were actually better funded in South Carolina than in North Carolina throughout the first half of the century. In both states, the pupil-teacher ratios in black and white schools converged to about the same level by the late 1960s.

Bond (1934) and others have observed that in areas where blacks were more numerous, a greater share of school resources were diverted from the black schools to white schools. An exclusionary political system enabled this discrimination to persist until the 1960s (see Boozer, Krueger and Wolkon, 1992). South Carolina and North Carolina are consistent with this pattern, as South Carolina had a much higher proportion of blacks in its population than North Carolina.³⁷ Compared to white families, black families had much less discretion over the level of school resources in the segregated states. To the extent that the endogenous determination of school resources (i.e., a correlation between school resources and omitted variables) is a problem for studies based on samples of white workers, this is much less of a problem for estimates based on black workers who were educated during the segregation era.

In Card and Krueger (1992b), we utilize the large inter-state differences in the pupil-teacher ratio and other resources in the black and white schools in the first half of the century, as well as changes in resources across cohorts, to estimate the impact of school quality on earnings. To control for differential labor market effects, in much of our analysis we focused on workers who attended school in the South but later were observed working in a common set

³⁷This difference reflected in part the historically different cropping patterns in the two states -- see Fogel and Engerman (1974).

of Northern labor markets. This technique has the advantage of controlling for labor market differences that may be correlated with school quality differences: for example, states that discriminated in terms of school resources may be more likely to allow discrimination in terms of labor market conditions. We related the payoff to a year of schooling by racial group for individuals who were educated in different states to the quality of the educational resources available to students of that racial group at the time they attended school (i.e., Class III models). The results indicate that the payoff to education is greater for individuals (of either racial group) who were born in states that devoted more resources to education.

Figure 4 displays the relationship between the differential "return to education" between blacks and whites and the black-white difference in the pupil-teacher ratio for men born from 1910 to 1939. The downward-sloping relationship signifies that the differential payoff to a year of education was greatest for those from states where black schools lagged furthest behind white schools in terms of class size. Similar results are found when we focus just on black workers, rather than on the black-white difference. Moreover, when we estimate separate models for blacks and whites from the segregated states, we are not able to reject the hypothesis that the effect of school quality on the return to education is the same for both racial groups.

The South Carolina-North Carolina comparison provides additional evidence that the returns to education reflected the differences in quality. As noted, the quality of black schools was higher in North Carolina than in South Carolina, while the reverse was true for white schools throughout most of the century (see Figure 3). For blacks, the estimated payoff to a year of education in 1980 was 2.1 percent for those born in South Carolina and 4.0 percent for those born in North Carolina; while for whites the order of the returns was reversed: 6.6 percent for

South Carolina and 6.0 percent for North Carolina. Thus, for these two states the differences in the payoff to education accord fairly well with the differences in the resources devoted to education.³⁸

Because the analysis described so far is based on individuals who were educated (actually, born) in the South and then observed working elsewhere, the validity of our inferences depends on the migration decisions of the workers. To the extent that these decisions are correlated with the payoff to education (conditioning on the payoff to education in the labor market where the individuals work) and the quality of individuals' schooling, the estimated effects of school quality may be biased. We suspect that a consideration of the nature of black mobility from the South would lead to the conclusion that patterns of earnings and school quality among migrants tend to underestimate any school quality effects with estimated with state-level quality data. Most research suggests that Southern out-migrants were better educated than non-migrants (e.g., Margo, 1990). More generally, people with higher overall productivity (including both the level and quality of education, as well as other unobserved components of productivity) may have been more likely to leave the South. Thus, low-educated blacks who migrated from states with low average school quality -- for example, Mississippi -- are likely to have had higher levels of unobserved ability and to have attended schools with above-average school quality within the state. In this scenario, the unobserved components of ability and school quality for individuals in our sample will be negatively correlated with measured school quality, biasing downward any

³⁸A similar comparison is also possible between white and black students born in Virginia and West Virginia. White schools in the early part of this century had lower pupil-teacher ratios in Virginia than West Virginia, while the reverse was true for black schools. Consistent with the school quality hypothesis, the estimated returns to schooling are higher for whites born in Virginia than West Virginia, while the returns are higher for black born in West Virginia than Virginia.

effect of school quality.

More direct evidence that selective migration is not driving our findings comes from reduced form models estimated on samples of all workers from a given state, including those who moved out of state and those who did not. In our 1992b paper we related the gap in average weekly earnings between blacks and whites from each state to the gap in average school quality between blacks and whites that existed in the state during the years the workers would have attended school. The results are consistent with our findings based on differences in the returns to education: earnings are higher for groups from states with higher quality schooling. According to the reduced form estimates, a reduction of the pupil-teacher ratio by 10 students is associated with nearly 3 percent higher weekly earnings during each year of one's working life. Notice also that this approach reflects any effect of school quality on the level of education, on the payoff to a year of education, and on the intercept.

In summary, we believe that evidence based on patterns of school quality and earnings for Southern-born black workers provides further confirmation of a link between school resources and labor market success. Because of the wide variation in school quality available to black children in different states and at different times, and because of the arguably exogenous -- and certainly different -- nature of the resource decisions that affected black students, this evidence is especially valuable. The estimated effects of school quality on earnings of Southern-born blacks educated before the mid-1960s are consistent with the estimates obtained in the rest of the literature.

V. Conclusions

Our review of the literature reveals a high degree of consistency across studies regarding the effect of school quality on students' subsequent earnings. The literature suggests that a 10 percent increase in school spending is associated with a 1 to 2 percent increase in earnings for students later in their lives. The studies estimate a wide range of specifications, and hold constant a number of different variables, including IQ, parental income, and parental education. Nonetheless, it is possible that school quality is only spuriously correlated with earnings as a result of unobserved background factors that affect school resources and student outcomes. Class III studies, which identify the effect of education on the slope of the earnings-education relationship, are the least susceptible to such biases because the inclusion of unrestricted state effects eliminates the influence of omitted state-level characteristics. Most studies that estimate Class III models tend to find a positive effect of school quality on the payoff to a year of education, once fixed state effects are removed.

Another finding in the literature is that educational attainment is positively related to school quality. This finding holds in several data sets, including those that measure quality at the state, district and school level. It is also consistent with a simple model that predicts that students will invest in more years of education if they perceive that higher quality schooling increases the payoff to each additional year of education.

Some authors have argued that the positive effects of school quality on earnings and educational attainment estimated in much of the literature are a spurious result of using aggregated school quality data. To support this argument, they point to studies using school-level quality data that often (although not always) find a statistically insignificant relationship between school quality and earnings. Our review casts doubt on this conclusion. The small number of

data sets that have information on workers' earnings and the quality of school they attended have three features in common that limit their usefulness: they include only young workers (often in their early 20s); they have school quality for just one year (typically a year during high school); and they have relatively small samples. Wachtel (1975) finds that the return to school quality increases as workers gain experience, and similar results have been found for the return to years of education. School quality in a given year is an imprecise measure of the student's history of school quality, leading to potential attenuation biases. Perhaps most importantly, the small sample sizes of these data sets lead to imprecise estimates that cannot distinguish between small but economically important differences in school quality effects that are estimated with comparable specifications using more aggregative quality data in larger data sets. For example, reduced form estimates based on the NLS-Y do not reject even the largest estimates of comparable specifications in the previous literature. Furthermore, when state-level average quality is used in place of school-level data in these studies, the estimates are even less precise, and often have the wrong sign, suggesting that the use of aggregated quality measures does not automatically lead to upward-biased quality effects.

Our reading of the literature leads us to suggest several directions for future research that may help resolve some of the remaining puzzles regarding the link between school quality and earnings:

(1) As suggested by Wachtel (1975), we hypothesize that the labor market returns to school quality tend to be lower for younger workers and to rise with experience. This can be tested by examining young cohorts of workers (e.g., age 17-31) in Census data, or by following cohorts

in the NLS-Y, High School and Beyond, and similar data sets as they age.

(2) As Heckman et al. and Card and Krueger (1992a) find, the precise form of the earnings-school quality relationship exerts an important influence on the estimated effects of school quality. For example, these studies conclude that school quality has a weak and inconsistent effect on earnings of those with exactly 12 years of schooling. Conditioning on education is inappropriate, however, if schooling attainment is affected by school quality, and if people with higher education would tend to earn more even without more schooling. Our theoretical model suggests that such unobserved ability biases will lead the observed earnings-schooling relationship to rotate as school quality increases -- a prediction that is consistent with findings in Heckman et al. and our earlier study.

(3) Reduced form models incorporate the effects of school quality on both educational attainment and the shape of the earnings-schooling relationship. For this reason, we recommend that researchers report such models and compare the reduced form estimates with their other specifications. Nevertheless, reduced form models may be susceptible to omitted variable biases. Perhaps the only way to fully overcome the problems of inferring the net effects of school quality is through randomized experiments. In the meantime, "natural" experiments that generate large and more-or-less exogenous shifts in school resources, such as the system of segregated schooling in the South, may provide the strongest evidence on school quality and labor market outcomes.

Our conclusion that most studies of labor market outcomes show a positive effect of

school quality is in apparent conflict with the widely-held view that school resources have little or no impact on students' test scores (e.g., Hanushek, 1986). We do not think this conflict is resolved by concluding that the relationship between school quality and earnings is spurious. Instead, we believe that the following observations are relevant:

(1) Test scores are not strong predictors of students' success in the labor market, so the finding that school resources have a low association with test scores does not imply that school resources have a low association with earnings.³⁹ Test scores may be a poor indicator of what is learned in school and subsequently rewarded in the labor market. And students' performance on tests while they are in school may be a poor indicator of what is retained years later, when the students are in the labor market.

(2) The conclusion that school inputs have no effect on test scores is open to question. Many of the individual studies in the literature have low power, even though as a group the studies may have power. Indeed, Hedges's (1993) meta-analysis suggests that school quality does affect test scores. Although one can criticize the assumptions that underlie any meta-analysis, one must bear in mind that similar assumption are implicit in other quantitative summaries of the literature. A recent experiment in which students were randomly assigned to large and small classes in Tennessee found that students tended to do perform better on tests if they were assigned to small classes (e.g., Mosteller, 1995).

³⁹This point was made by Johnson and Stafford (1973) over 20 years ago, who wrote: "Are our empirical findings necessarily inconsistent with those of several authors who argue that the quality of schooling has little effect on output measures such as tests of cognitive skills? We think not. First, there is to our knowledge no precise link between test scores and earnings."

(3) Because educational attainment appears to be related to school quality, the type of selection issues that arise in the literature on earnings may also arise in the literature on test scores. In particular, marginal students who benefit from improved school quality may be less likely to drop out or be held back a grade. Therefore, studies of the relationship between test scores and school quality that condition on grade level may be biased toward finding no effect of school quality. This issue can be investigated by estimating reduced form models that condition on age, but not grade level.

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Figure 1

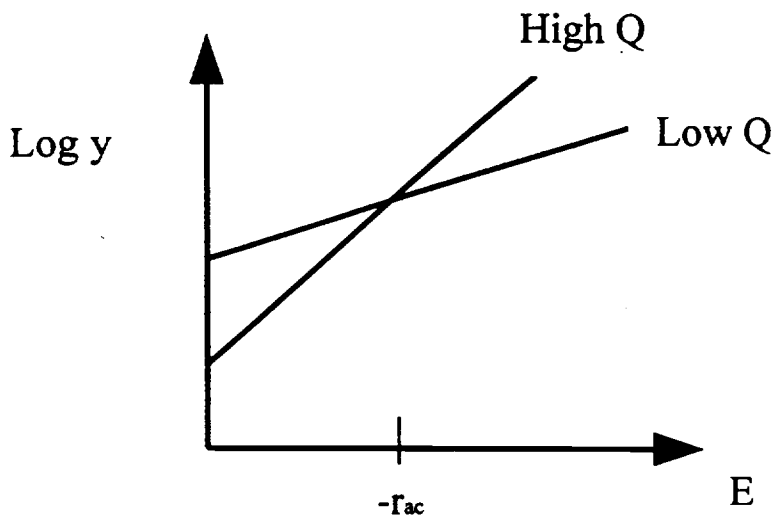
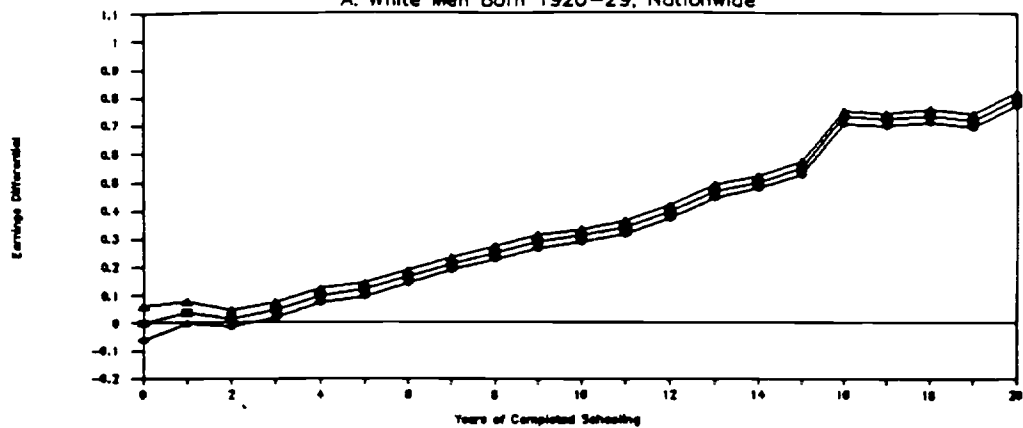
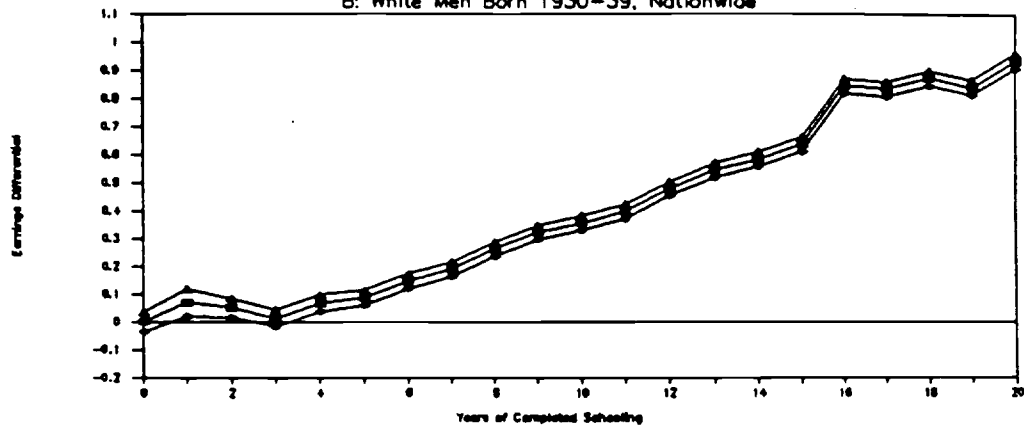


Figure 2
Return to Single Years of Education

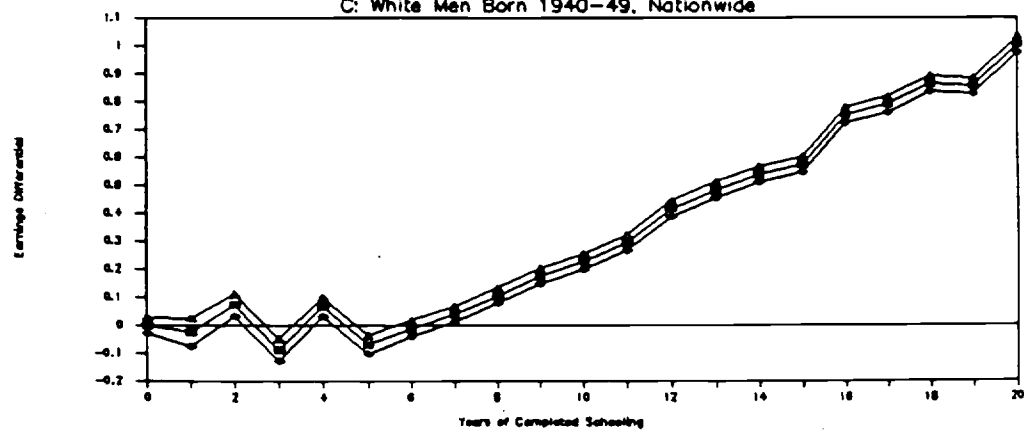
A: White Men Born 1920-29, Nationwide



B: White Men Born 1930-39, Nationwide



C: White Men Born 1940-49, Nationwide



■ Point Estimate

— -1 Std. Error

△ +1 Std. Error

Figure 3: Pupil-Teacher Ratios by Race, North Carolina vs. South Carolina

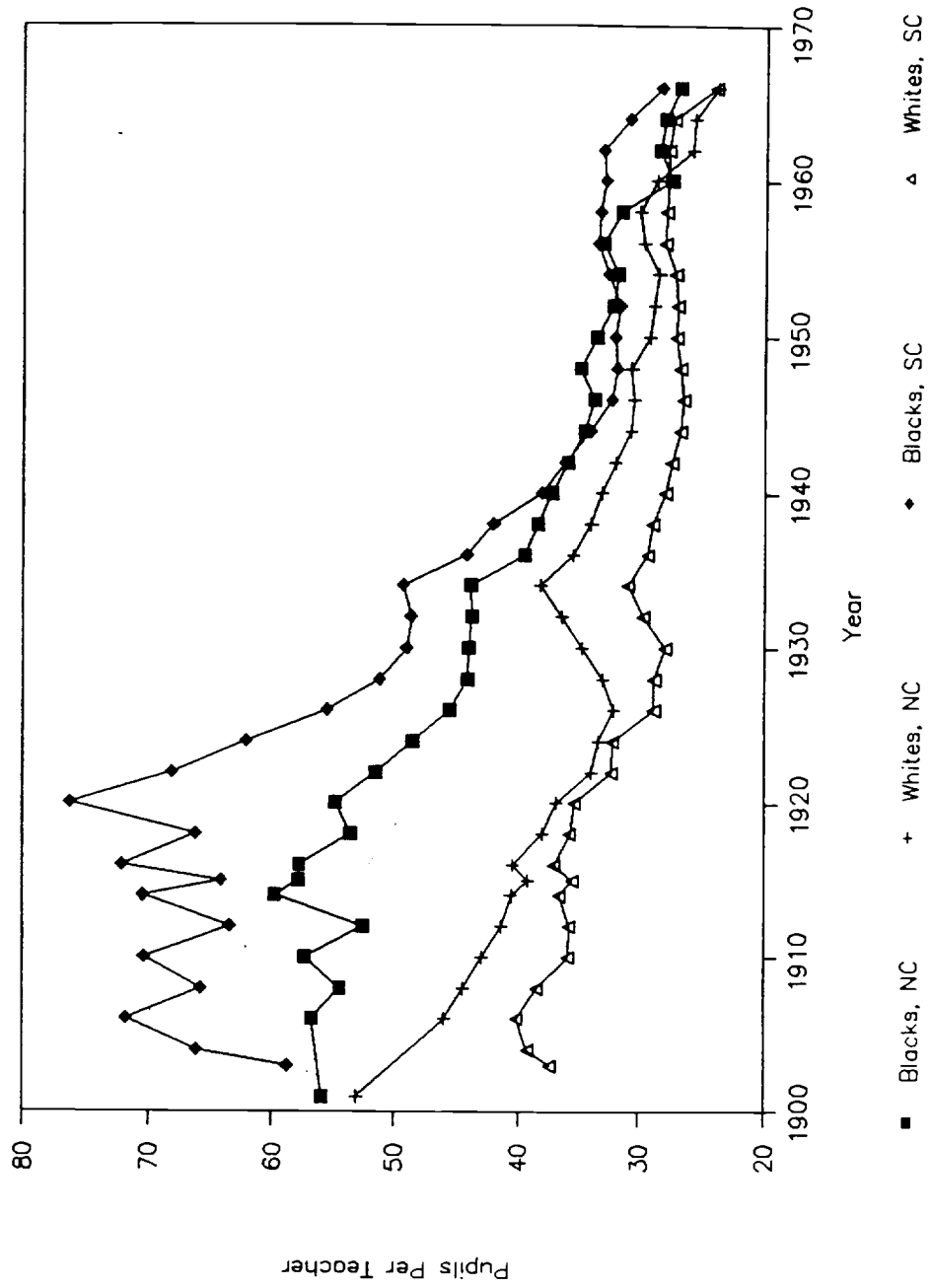


Figure 4: Difference in Black-White Returns to Schooling vs. Difference in Pupil-Teacher Ratios, By State

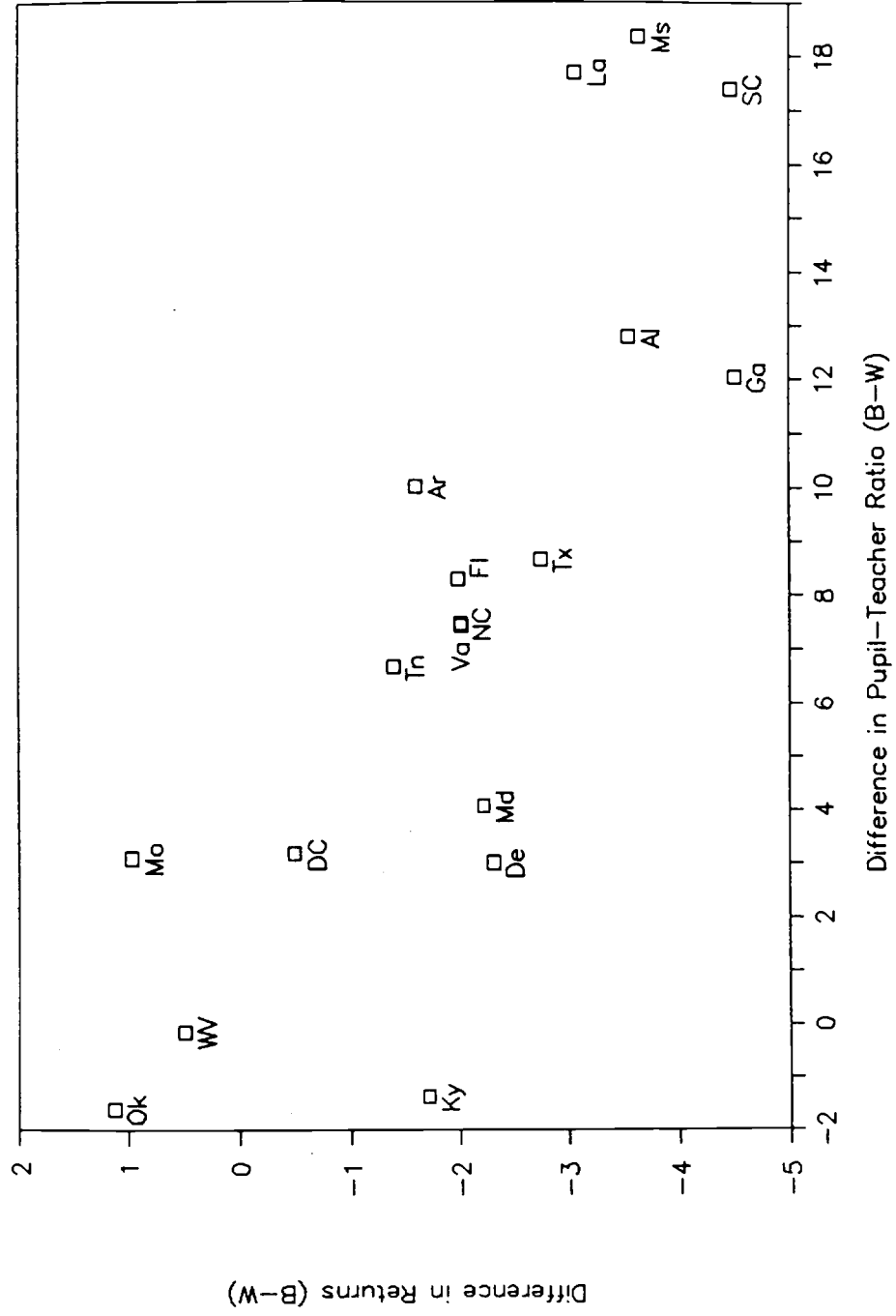


Table 1: Summary of Studies Estimating the Effect of Expenditures Per Student on Earnings

Study	Sample	Methodology and Comments	Estimated Elasticity of Earnings w.r.t. \$/pupil
1. Morgan and Sirageldin (1968)	1965 SRC Sample Family heads, not self-employed N=1,438	Class I. State expenditures (average of 1930, 1940, 1950) matched to individuals by state where they grew up. D.V.= log hourly wage in 1964. Stepwise regression. First step: remove yrs of education, age, sex, race, grew up on farm.	0.29 (0.05)
2. Johnson and Stafford (1973)	1965 SRC Sample White male non- farm heads; N=1,039	Class I. State*decade average expenditures matched to individuals by age/state where they grew up. D.V.= log hourly wage in 1964. Other controls: years of education, experience, urban residence/birthplace, father's education.	0.20 (0.05)
3. Morgenstern (1973)	1968 Urban Problems Sample, conducted by ISR. N=1,624 heads of households. Black households oversampled.	Class I. State expenditures matched to individuals by state where grew up. D.V.=average hourly wage last year. Other controls include gender, age, place of birth, experience, and parent's education.	Blacks 0.12 (0.04) Whites 0.13 (0.07)
4. Jud and Walker (1977)	1970 NLS, males age 18-28. N=1,215	Class I. District-level expenditures matched to high schools. D.V. = annual earnings in 1970 in levels. Controls: experience, education, race, SMSA, married, South, health, IQ, and work hours.	0.07 (0.07)
5. Ribich and Murphy (1975)	Project Talent, 1959 9th grade men, re- interviewed in 1968. Blacks and Southerners under- represented. N=8,466	Class I. Expenditures per pupil collected from school survey. D.V.=estimated lifetime earnings, imputed from 1967 earnings (if employed) and education data. Other controls: years of education, AFQT, parental and school-average SES.	0.01 (0.02)
6. Link and Ratledge (1975)	NLS Young Men age 16-26 in 1968, out of school \geq 1 yr N=1,157	Class I. School-district expend. (for 1968) matched by school district of school attended. D.V.=1968 log annual earnings. Other controls: years of education experience (quadratic), urban residence, IQ, hours worked.	Whites 0.15 (0.05) Blacks 0.55 (0.17)
7. Wachtel (1976)	Thorndike-Hagen sample, men age 18-26 in 1943 (army veterans) N=1,633	Class I. School-district expenditures matched to individuals by name/location of high school. (1936-38 BSE data). D.V.=1955/1969 earnings. Other controls: yrs. of education, experience, hours worked, test score, college quality, father's education.	1969: 0.20 (0.04) 1955: 0.09 (0.03)
8. Rizzuto and Wachtel (1980)	1960, 1970 Census white and black men age 14-65 not self employed N=26,204 (1960) N=27,729 (1970)	Class I. State expenditure data matched to individuals by state of birth and age interval. D.V.=1959/1969 log earnings. Other controls: years of education, experience, urban residence.	Whites 1959 0.12 (0.01) Blacks 1959 0.09 (0.03) Whites 1969 0.08 (0.01) Blacks 1969 0.11 (0.04)

Table continues

Table 1: Continued

Study	Sample	Methodology and Comments	Estimated Elasticity of Earnings w.r.t. \$/pupil
9. Akin and Garfinkel (1980)	PSID male heads age 30-55 during 1968-72. N=1,049	Class II. State*decade average expenditures matched to individuals by state of residence at age 12. D.V.=log average wage 1968-72. Expenditures interacted with years of education. Other controls: IQ, father's income, indicators for family income.	Whites 0.30 (at ed=12) Blacks 0.14 (at ed=10.5)
10. Link, Ratledge and Lewis (1980)	NLS Young Men age 19-29 in 1969 N=2,127 PSID male heads as in study 6.	Class II. See comments for studies 6 and 7. D.V. for NLS is log average wage in 1971. D.V. for PSID is log average wage 1968-72. Expenditures interacted with years of education. Other controls in NLS: IQ, father's education. Other controls in PSID as in study 7.	NLS Whites 0.13-0.17 NLS Blacks 0.15-0.23 PSID Whites 0.24-0.32 PSID Blacks 0.13-0.15 (at mean education for group)
11. Tremblay (1986)	1976 NLS Young Men, age 24-34. N=247 Southern, 496 Nonsouthern	Class II. See comments for study 6. Expenditures interacted with education. D.V=log monthly income. Other controls: occupation and industry dummies IQ, race, age, seniority, SMSA, marital status, occupational training dummy, and union status.	<u>High School:</u> South 0.12 Nonsouth 0.04 <u>College:</u> South 0.05 Nonsouth 0.03

Notes:

A positive coefficient means that higher earnings are associated with higher spending per student.

Elasticities are with respect to a percentage change in spending per student. Estimated standard errors are shown in parentheses.

Table 2: Summary of Studies Estimating the Effect of Pupil-Teacher Ratio on Earnings

Study	Sample	Methodology and Comments	Estimated Elasticity of Earnings w.r.t. pupil-teacher ratio
1. Wachtel (1975)	Thorndike-Hagen sample, men age 18-26 in 1943 (army veterans) N=1,812	Class I. School-district teacher-pupil ratio matched to individuals by name/location of high school. (1936-38 BSE data). D.V.= 1969 earnings. Other controls: yrs of education, experience, hours worked, test score, mother's education, father's education, and college quality.	-0.044 (t < 2)
2. Card and Krueger (1992a)	1980 Census white men age 31-60, not self employed. N=1,018,477	Class IV. State average school data for 1926-66 matched to individuals by state of birth and years of school attendance. D.V. = log weekly wage in 1979. Other controls: state of birth and state of residence, experience (quadratic), marital status, urban residence.	-0.074 (0.012)
3. Card and Krueger (1992b)	1960-80 Census black and white men born in South from 1910-49 N=728,284	Class IV. State average school data for 1916-66 matched to race* state-of-birth*10-year cohort. D.V. = state*cohort*Census black-white log weekly wage gap. Other controls: cohort dummies, year dummies.	-0.050 (0.018)
4. Grogger (1994)	NLS-72. High school class of 1972. N=4,685. High School and Beyond (HSB). High school class of 1979. N=2,396.	Class I. Pupil-teacher ratio for high school provided by school survey. Other controls include educational attainment dummies, experience, region, family income, achievement test scores, and high school grades.	NLS-72: 0.052 (0.027) HSB: 0.056 (0.034)
5. Betts (1995)	1979-89 NLS-Y. White men age 17-31. N=11,314 worker/year obs.	Class IV. School data from survey of latest high school attended in 1979. Other controls include teacher salary, teacher educ., region of residence, and age. Uses teacher-pupil ratio.	-0.043 (0.025)

Notes: The sign was changed for studies that used the teacher-pupil ratio. A negative coefficient means that higher earnings are associated with a lower pupil-teacher ratio. The elasticities were calculated at assuming the pupil-teacher ratio equals 17.9. Estimated standard errors are shown in parentheses.

Table 3: Impact of Pupil-Teacher Ratio on Return to a Year of Schooling

	Model	Without State Effects (α_b)	With State Effects (α_b)
Card and Krueger (1992a)	2% Threshold:	-0.40 (0.28)	-1.59 (0.54)
	Linear Model:	-0.20 (0.27)	-1.37 (0.52)
Card and Krueger (1992b)	Linear Model:	-1.44 (0.39)	-1.25 (0.54)
Heckman, et al.	Linear Model:		
	1970	-0.36 (0.12)	-1.51 (0.26)
	1980	0.23 (0.09)	-1.07 (0.25)
	1990	0.26 (0.13)	-1.81 (0.25)
Betts (1995)	OLS (White SE):	0.50 (1.24)	--
	Random Effects:	-4.06 (1.56)	--
	State Avg. Quality:	5.30 (6.79)	--

Notes:

Estimate is the change in the percentage return to one year of education for a one percent change in the pupil-teacher ratio. Estimated standard errors are shown in parentheses. The state effects (α_b) are in the return to education (see text). Because Betts uses the teacher-pupil ratio as his explanatory variable, the negative sign was taken. In this table, a negative estimate indicates that reducing the pupil-teacher ratio increases the slope of the earnings-education function. Calculations assume 17 students per class.

Table 4: Summary of Heckman, Layne-Farrar and Todd

Analysis	Findings
<p>1. Replicate CK in 1970, 1980, and 1990 Censuses, with region-of-residence x region-of-birth interactions with linear education in the first step model</p>	<p>School quality effects on the return to education are stronger than CK's 1980 findings in 1970 and 1990.</p>
<p>2. Relate state of birth intercepts to school quality variables</p>	<p>Higher school quality tends to reduce the intercept; similar to result found in CK.</p>
<p>3. Include region-of-residence effects and aggregate region-of-residence variables in second step estimator</p>	<p>School quality continues to have a beneficial and sizable effect on the rate of return to education.</p>
<p>4. Free up linearity assumption by allowing jumps in the return to education</p>	<p>School quality has a weak and inconsistent relationship with the return to education around grade 12, but a significant relationship for post-college years. CK found similar results for grade 12 in 1980 Census.</p>
<p>5. Include quadratic in distance between region of birth and region of residence and free up effect of school quality by region of residence in second step equation</p>	<p>Effects of quality are positive in most regions, and often large. Can reject that the effect of the pupil-teacher ratio is constant in all regions, but average across regions is about the same as in the unrestricted model</p>
<p>6. Calculate Kendall Coefficients of Concordance between ranks of average wages across regions of residence and regions of birth by cohort in 1980</p>	<p>Weak positive correlations. Also find weak correlations between ranks of school quality and ranks of earnings.</p>
<p>7. Relate fractions of pop. in a state with college degree, high school degree (but no college degree), and high school dropouts to school quality by cohort</p>	<p>Higher quality is strongly related to relatively more college graduates, weakly related to high school graduates, and strongly related to reduction in dropouts.</p>

**Table 5: Effect of Pupil-Teacher Ratio on Return to Education
Based on Heckman et al.**

	Restricted	Average Across Nine Regions
1970	-8.71	-7.23
1980	-5.66	-4.65
1990	-9.22	-26.62

Source: Column 1 of Tables 7 a,b,c and Tables 14 a,b,c of
Heckman et al.