NBER WORKING PAPER SERIES

THE EFFECTS OF SCHOOL AND FAMILY CHARACTERISTICS ON THE RETURN TO EDUCATION

Joseph G. Altonji
Thomas A. Dunn

Working Paper No. 5072

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
March 1995

This project was funded by the U.S. Department of Labor, Bureau of Labor Statistics under Contract Number J-9-J-7-0094. Opinions stated in this document do not necessarily represent the official position or policy of the U.S. Department of Labor. Research support from the Center for Urban Affairs and Policy Research, Northwestern University and the Center for Policy Research, Syracuse University is also gratefully acknowledged. Dan Aaronson, Alex Idichandy, Michele Borsetti, Yasuyo Abe, John Phillips, and James Spletzer provided skillful research assistance. We also thank John Abowd, Julian Betts, Ronald Ehrenberg, Eric Hanushek, Judith Hellerstein, Bruce Meyer, Frank Vella, and participants in seminars at the University of Michigan, Syracuse University, the Milkin Institute, and conferences at the University of Texas at Austin (March 1994), the Kennedy School (December 1994) and the Institute for Research on Poverty, University of Wisconsin (February 1995) for helpful comments. A June 1990 draft was circulated under the title "Effects of Parental Characteristics on the Return to Education and Labor Market Experience." This paper is part of NBER's research program in Labor Studies. Any opinions expressed are those of the authors and not those of the National Bureau of Economic Research.

NBER Working Paper #5072
March 1995

THE EFFECTS OF SCHOOL AND FAMILY CHARACTERISTICS ON THE RETURN TO EDUCATION

ABSTRACT

We measure the effects of parental education on the education profile of wages. The analysis uses sibling pairs from the Panel Study of Income Dynamics and the National Longitudinal Surveys of Labor Market Experience of Young Men and Young Women. We also use the variance across siblings in school characteristics to estimate the effects of school inputs on wages holding family background constant. We obtained mixed evidence on whether parental education raises the return to education. We find that teacher's salary, expenditures per pupil, and a composite index of school quality measures have a substantial positive effect on the wages of high school graduates.

Joseph G. Altonji
Department of Economics
Northwestern University
Evanston, IL 60208
and NBER

Thomas A. Dunn
Department of Economics
Syracuse University
Syracuse, NY 13244-1090

I. Introduction

In light of the extensive research on the direct effects of school quality and family background on school achievement, schooling levels and wages as well as the huge literature on the "main effect" of education on wages[1], it is surprising that only a handful of studies have examined whether the education slope of wages varies systematically with school characteristics and family background characteristics that influence the quantity and quality of formal education received.[2] Indeed, all of the studies surveyed in Griliches (1979) assume that the rate of return to education is the same for all individuals. Willis and Rosen (1979) and a small number of subsequent studies do consider heterogeneity in the returns to education, but Willis and Rosen make the identifying assumption that family background measures do not affect the return to education.

In contrast, the main purpose of this paper is to estimate the effect of family background and school characteristics on the return to education. The hypothesis that quality of the school would influence how much one learns and hence, the labor market payoff to time spent in school, is a natural one. It is also natural to hypothesize that parents have large effects on the economic value of time in school given the abundant evidence that parental education and income and other variables have large

[1] See Siebert (1985) for a discussion of this literature and detailed references. Hanushek (1986) and (1989) surveys the literature on "education production functions", of which Betts (1994) is a recent example, and argue that there is little evidence that gross inputs matter. This finding is disputed by Hedges et al (1995).

[2] Morgan and Sirageldin (1968) and Link and Ratledge (1975)) are early studies that examine the relationship between school variables and future earnings levels. The latter study used the NLS young men. Card and Krueger (1992) focus on education slopes and find substantial effects of student/teacher ratio, the relative salary of teachers, and the length of the school term on estimates of the rates of return to education. They rely on variation across states and age cohorts in these input measures and do not control for differences in parental inputs. Heckman et al (1994) and Betts (forthcoming) re-examine the evidence and find less strong support for an effect of these measures of school inputs on wage levels or slopes. See Card and Krueger (undated) and Betts (1994) for seemingly contradictory surveys of the literature. Earlier studies of the effects of family background on education slopes include Hauser (1973), who finds little evidence that father's occupation influences the return to education for men in a cross section analysis that permits the education slope and intercept to vary with occupation. Olneck (1979) finds little evidence that education slopes differ by IQ or by father's education. Altonji (1988) estimates the effects of parental education on education slopes for a limited sample of sibling pairs from the PSID using methods similar to those of the present paper. There is a large literature on race and sex differences in the rates of return to education. (See Cain (1986) and Welch (1973).) Hause (1972) finds a positive effect of an aptitude measure using the NBER-Thorndike sample, but Taubman and Wales (1975), page 11 argue that this is because he used an overly restrictive functional form for the main effect of aptitude. See also Olneck (1979). Willis and Rosen found a positive effect of aptitude on the return to some college. A number of recent papers have examined education-aptitude interactions, with mixed results. Finally, there is also a substantial literature on variation over time in the return to education, but most of this literature emphasizes the macro effects of changes in the supply and demand for educated workers rather than changes in the quality of education or family background.

effects on cognitive development in the pre-school years, quality of schooling received, and achievement in school. One would presume that these variables are related to how much a child learns during a year in school. Indeed, since a strong positive role for the family in school achievement and in labor market success is much more firmly established than the role of school characteristics, our prior belief was that in the U.S. family variables are more important than school characteristics for wage slopes. Since family background and school quality are positively correlated, it is important to control for one when studying the effects of the other.

The possibility that the family has large effects on the labor market payoff to time spent in school has implications for the issue of why the family plays such an important role in the demand for schooling. It is well known that sibling correlations in years of education are in the neighborhood of .5. [3] Most studies of educational attainment take the view that family background affects education primarily by influencing the amount of education individuals obtain holding the rate of return to education constant. However, education choice models imply that family background characteristics that raise the rate of return to schooling may induce individuals to stay in school longer. Perhaps the strong parental and sibling correlations in years of education arise in part because there is a correlation across family members in the economic value of education.

Our empirical strategy for examining the determinants of the return to education is simple. We augment a standard log wage equation with interaction terms between years of education and father's education, mother's education and a set of school quality variables. We estimate the wage equations with data from the PSID and the original NLS cohorts of young men and young women. Since education as well as the interaction between education and variables such as parents' education are likely to depend upon unobserved family characteristics that have an independent influence on wages, there is a strong possibility that omitted family variables will bias estimates of the effects of school and family

[3] In the NLS data used below the correlations of education levels are .46 for fathers and sons, .43 for mothers and sons, .40 for fathers and daughters, .41 for mothers and daughters, .58 for brothers, .50 for sisters, and .48 for brothers and sisters. In the PSID data used below the correlations of education levels are .46 for fathers and sons, .48 for mothers and sons, .35 for fathers and daughters, .33 for mothers and daughters, .53 for brothers, .49 for sisters, and .46 for brothers and sisters.

characteristics on this return. We deal with this by using a sibling fixed effect to control for unobserved variables that are common to siblings.[4]

Two potentially serious econometric problems remain that affect our study. First, controlling for the "main effect" of the family only eliminates an additive, family specific error component. It does not eliminate idiosyncratic differences among siblings in cognitive ability and motivational factors which have an independent effect on both wages and education, or random variation across families or siblings in the education slope. We use a model of wages and education choice that is in the spirit of Becker (1967) to show that coefficients on interactions between education and variables that raise education levels (such as father's education or school quality variables) are likely to be biased upward unless parents compensate in a particular way for differences among siblings in comparative advantage for education. The problem appears to be generic to studies of the determinants of education slopes. It would be a source of upward bias in the Card and Krueger (1992) study, to give one prominent example, even if their measures of educational inputs are uncorrelated with all unobservables affecting wages.

The second econometric problem is measurement error. It has been well known since Griliches (1977) that classical measurement error in education reports leads to downward bias in the main effects of education, particularly when one works with sibling differences. It is less obvious what the effect of measurement error would be on interaction coefficients, or what the combined effect of measurement error in the education reports and measurement error in the variables that shift the education slope (such father's education) would be, particularly when other variables such as IQ are controlled for. We provide a simple formula for correcting for measurement error bias in the interaction coefficients.

Our study of the main effects and interaction effects of school inputs is the first to use variation among siblings in school inputs to identify these effects. This gets around the criticism that variation across families (or even states) in school inputs is related to other characteristics that affect wage levels.

[4] While we are unable to control for the "main effects" of school quality directly, most of the unobserved variation in this variable is controlled for by the sibling fixed effects given that most siblings attain the same school.

Since the approach is invalid if the variation across siblings in the school quality variables is related to characteristics that are likely to affect wages or education slopes, we investigate this issue.

Our results may be summarized as follows. First, for men in the PSID and NLS with fixed effects included, we find a substantial positive effect of the parents' education, particularly mother's education, on the education slope. For women, the fixed effects results show a substantial effect of mother's education in the NLS but not in the PSID. However, when we pool the NLS samples of men and women and include fixed effects, we do not find much of an effect of parental education on wage slopes. The subsample involving brothers and sisters appears to behave quite differently from the subsample of brothers and the subsample of sisters. In the PSID the pooled results point to a modest positive effect of mother's education on the education slope. When we exclude family fixed effects, we find little evidence of an important education interaction term, regardless of the data source or gender composition of the sample. Thus, while we find a substantial positive effect of parental education on wage slopes for our preferred specifications (same sex samples with fixed effects) in 3 of 4 cases, and the PSID pooled results point to an economically significant effect, the evidence is not compelling if one places much weight on the specifications without fixed effects or the NLS pooled sample. The strength of the evidence is further weakened by the fact that the theoretical analysis suggests that the fixed effects estimates are probably biased upward.

Second, we find that differences between siblings in the quality of the high school attended have a substantial positive relationship with differences in the wages of high school graduates, although we do not find consistent evidence that our measures of high school inputs and quality have a positive effect on the education slope. Our estimates of the main effects of school characteristics are positive but smaller when family effects are not controlled for.

The paper is organized as follows. Section I presents the econometric framework for the study. Data issues are addressed in section II. Section III presents the empirical results, and in section IV we summarize the findings and offer some suggestions for future research.

I. Econometric Framework

Consider the following log wage equation for a young woman:

$$(1)\ w_{hkt} = Z_{hkt}B_1 + Z_hB_2 + r_{hk}S_{hk} + \epsilon_h + \epsilon_{hk} + \upsilon_{hkt}$$

In (1) w_{hkt} is the log wage of woman k from family h in year t. The elements of the vector Z_h are observed characteristics of the family and/or school. The variable S_{hk} is years of education. For expository convenience, we work with a linear specification of education. However, in most of our empirical work we include third order polynomials in education and experience and an interaction term between education and experience. The vector Z_{hkt} consists of other observed characteristics of the woman that affect wage rates. Finally, ϵ_{hk} and ϵ_h are individual specific and family specific error components, and υ_{hkt} is a time varying error component that we assume is uncorrelated with the other right hand side variables in the equation.

We wish to estimate the relationship between the education slope r_{hk} and school variables such as expenditures per pupil and family background characteristics such as father's education and mother's education. Parental education may be related to innate cognitive ability, early childhood development, primary and secondary school quality, the amount of time and energy that children devote to schooling when in primary and secondary school, and the success of parents as aides in the formal schooling process and as informal teachers at home. Specifically,

$$(2)\ r_{hk} = r_1 + r_2X_h + \eta_h + \eta_{hk}$$

where η_h and η_{hk} are mean 0 unobserved household and person specific error components affecting the rate of return to education, X_h is a vector of school and family background characteristics that influence the return to education and are defined to have mean 0, and r_1 is the average return to a year of school. We wish to estimate r_2 .

Equation (2) is taken as given by the child, who makes educational decisions given preferences and budget constraints shaped in part by the parents and schools. The coefficient r_2 and the values of η_h and η_{hk} are determined in part by parental decisions and by exogenous links between parental characteristics and the characteristics of kids. One can easily generalize (2) to include person specific variables such as IQ scores as well as school quality variables, and we do so in the empirical work below. We define r_2 so that η_h and η_{hk} are uncorrelated with X_h. The elements of the vector r_2 corresponding to particular elements on X_h depend on what other variables are included in X_h. When we exclude school quality variables and IQ scores from X_h, we are measuring the total effect of observable parental variables on the education slope, including effects that operate through a particular measure of aptitude and achievement and effects that operate through school characteristics.

Using (2) to substitute for r_{hk} in (1) leads to

(3) $w_{hkt} = Z_{hkt}B_1 + Z_hB_2 + [r_1 + r_2X_h]S_{hk} + \epsilon_h + \epsilon_{hk} + (\eta_h + \eta_{hk})\, S_{hk} + \upsilon_{hkt}$

Because S_{hk} is likely to be correlated with the additive family and person specific wage error components ϵ_h and ϵ_{hk} as well as with school slopes components η_h and η_{hk}, least squares estimation of the above equation will lead to biased parameter estimates. One can eliminate ϵ_h from the equation by differencing (3) for pairs of siblings. Let D denote the "sibling difference" operator. For siblings indexed by k and k', the differenced equation is

(4) $Dw_{hkt} = [DZ_{hkt}]B_1 + [r_1 + r_2X_h]\,[DS_{hk}] + D\epsilon_{hk} + \eta_hDS_{hk} + D(\eta_{hk}S_{hk}) + D\upsilon_{hkt}$

The error component η_h is constant within the household and so will be uncorrelated with the explanatory variables in (4). However, the sibling difference in schooling choice DS_{hk} might be correlated with $D\epsilon_{hk}$ or with $D(\eta_{hk}S_{hk})$, which capture sibling differences in productivity and the return to education. In some specifications we add IQ to (3) and the difference between the IQ scores of k and k' to (4) to control partially for these unobservables. However, the IQ measure is an imperfect control, and so in the next few paragraphs we use a simplified model of wages and the demand for education to examine whether our estimates of the effects of parental education or school characteristics on returns to education will be subject to bias.[5] The answer is that the estimate of r_2 is likely to be

[5] In addition to adding the level of IQ we report results with and without interactions between IQ and S_{hk}. However, since parental education and schooling probably influence the test scores, it is not clear that we should hold IQ constant when examing the effects of parental education.

biased upward. The econometric intuition is as follows. Suppose that X_h is parental education, and parental education increases the mean of S_{hk} and $S_{hk'}$ but leaves the distribution of DS_{hk} unaffected. Since S_{hk} and $S_{hk'}$ are the weights on the error terms η_{hk} and $\eta_{hk'}$, the fact that X_h increases S_{hk} and $S_{hk'}$ has the effect of increasing the importance of η_{hk} and $\eta_{hk'}$ in the error term. The more important these terms, the greater the bias. The coefficient r_2 on the interaction term is affected as a result.

The formal analysis draws on Becker (1967), and complements Lang (1993a and 1993b) and Card's (1994) recent analyses of bias in education slope estimates. We focus on bias in estimating the interaction term r_2 using sibling differences.[6]

Suppose that the child's utility function is

$$(5)\quad U_{hk} = \ln\left(\beta_{hk}(S_{hk})\, W_{hk}(S_{hk})\right) + V_{hk}(S_{hk}) .$$

In (5) $\beta_{hk}(S_{hk})$ is the child's discount function, which captures the fact that additional schooling both delays earnings and affects the borrowing rate. It depends on transfers and loans from the parents and the child's rate of time preference. W_{hk} is the permanent wage rate (in levels rather than logs), which is determined by (3) after taking account of the time variation in Z and υ_{hkt} in that equation and converting from logs to levels. $V_{hk}(S_{hk})$ is the direct utility from schooling net of direct costs. All three functions are influenced by autonomous parental factors and active choices made by the parents. The child chooses S to solve

$$(6)\quad \partial \ln(\beta_{hk}(S_{hk}))/\partial S_{hk} + \partial \ln(W_{hk}(S_{hk}))/\partial S_{hk} + \partial V_{hk}(S_{hk})/\partial S_{hk} = 0$$

If the equations for the above derivatives are

$$(7a)\quad \partial \ln\beta_{hk}(S_{hk})/\partial S_{hk} = a_0 + a_1 S_{hk} + a_2 X_h + u_h + u_{hk}$$

---

[6] Card focuses on estimating the main effect r_1 and considers both cross sections and sibling differences. Lang (1993a) is concerned with interaction terms as well as the main effect of education but does not focus on the use of sibling differences. Lang's analysis suggests that if X_h lowers the discount rate or raises the nonpecuniary return to education, then "discount bias" may bias OLS estimates of r_2 downward if fixed effects are excluded.

(7b) $\partial w_{hk}(S_{hk}) \;/\; \partial S_{hk} \;\equiv\; \partial \ln(W_{hk}(S_{hk})) \;/\; \partial S_{hk} \;=\; r_1 + r_2 X_h + \eta_h + \eta_{hk}$

(7c) $\partial V_{hk}(S_{hk}) \;/\; \partial S_{hk} \;=\; c_0 + c_1 S_{hk} + c_2 X_h + v_h + v_{hk}$

then the solution for S_{hk} is

$$(8)\quad S_{hk} = \frac{-1}{a_1 + c_1}\left\{\begin{matrix} a_0 + r_1 + c_0 + (a_2 + r_2 + c_2)X_h + (u_h + \eta_h + v_h) + \\ (u_{hk} + \eta_{hk} + v_{hk}) \end{matrix}\right\}.$$

We assume that the log discount factor and the direct costs of schooling are strictly concave in S_{hk}, with $a_1 + c_1 < 0$. We also asume that for favorable family background characteristics such as parental education or income $a_2 > 0$ and $c_2 > 0$

X_h and the unobserved components $\{\epsilon_h, \epsilon_{hk}, u_h, u_{hk}, \eta_h, \eta_{hk}, v_h, v_{hk}\}$ are taken as given by the child. They depend on genetic and environmental factors that are common to the children in household h and on factors specific to child k. In part, this dependence is involuntary. For example, the genetic endowments of parents and some aspects of personality and family culture are passed on to children involuntarily. The geographic location of the parents influences the school and community environment and may be affected by family history. However, the error components are also shaped by conscious investments of time and money by the parents. Specifically, parents maximize a utility function that depends on their own consumption and the utility of their children. They devote time and money to influence the additive wage components ϵ_h and ϵ_{hk}, the components u_h and u_{hk} in discount function $\beta(S_{hk})$, the components η_h and η_{hk} in the return to education function, and the components v_h and v_{hk} that affect the direct pecuniary and nonpecuniary costs of education borne by the child. They may try to compensate for differences in absolute advantage by altering ϵ_{hk} or subsidizing the education of the disadvantaged child. The optimal strategy for the parents will depend on the specific preferences and on the relative costs of influencing the components of $\{\epsilon_h, \epsilon_{hk}, u_h, u_{hk}, \eta_h, \eta_{hk}, v_h, v_{hk}\}$.[7] The means of these components are independent of X_h. The variances and covariances of the components may be related to X_h either because X_h has an autonomous influence on the higher moments of these variables or because the choices of the parents that vary with X_h influence the higher moments. Without specific assumptions, one cannot say how the wage error ϵ_{hk} will be related to u_{hk}, for example, or whether the strength of this relationship will depend on X_h.

Let us now use the schooling model to examine at the properties of OLS estimates of (4). Suppose that Z_{hkt} is constant across siblings and time and ignore $D\upsilon_{hkt}$. Then (4) reduces to

(9) $Dw_{hkt} = [r_1 + r_2X_h]\, DS_{hk} + D\epsilon_{hk} + \eta_h DS_{hk} + D(\eta_{hk}S_{hk})$.

From (8) DS_{hk} is

$$(10)\quad DS_{hk} = \frac{-1}{a_1 + c_1}\{Du_{hk} + D\eta_{hk} + Dv_{hk}\}$$

Finally, assume that X_h is a scalar and takes on only two values, 0 and 1. Suppose one estimates (9) separately for families with X_h=0 and X_h=1. The probability limit of the OLS estimator of r_1 for the X_h=0 sample will be

$$(11)\quad r_1 + \frac{\mathrm{Cov}(DS_{hk}, D\epsilon_{hk} + \eta_h DS_{hk} + D(\eta_{hk}S_{hk}) \mid X_h=0)}{\mathrm{Var}(DS_{hk} \mid X_h=0)}$$

The probability limit of the estimator of $r_1 + r_2$ for the X_h=1 sample is

[7] What parents perceive to be the lifetime utility function of their child may be different from what an adolescent or young adult perceives her lifetime utility function to be. The parents will seek to maximize their own utility function but will still be constrained by (8), which drives the decisions of their child, given the costs of altering $\{\epsilon_h, \epsilon_{hk}, u_h, u_{hk}, \eta_h, \eta_{hk}, v_h, v_{hk}\}$. For example, some parents may believe that their children underestimate the lifetime monetary and/or the nonmonetary value of education. Such parents may bribe, threaten, and cajole their children into obtaining a larger amount. At a later date, their children may be grateful.

$$(12)\quad r_1 + r_2 + \frac{\mathrm{Cov}(DS_{hk}\,,\, D\epsilon_h + \eta_h\, DS_{hk} \;+\; D(\eta_{hk}\, S_{hk}) \mid X_h{=}1)}{\mathrm{Var}(DS_{hk} \mid X_h{=}1)}$$

The difference between (11) and (12) is the probability limit of the OLS estimator of r_2 that one would obtain if one used the pooled sample of families to estimate (4). It follows almost immediately that the difference in the coefficients from the two separate regressions is a consistent estimator of r_2 if and only if the <u>ratio</u> of the conditional covariance and variance in (11) does not depend on X_h. If this condition holds then the difference between (12) and (11) is consistent for r_2 even though (11) is an inconsistent estimator of r_1 and (12) is an inconsistent estimator of $r_1 + r_2$. The argument can easily be generalized to the case in which X_h takes on a variety of values.

This invariance to X_h of the ratios in (11) and (12) is unlikely to be satisfied. To show the potential bias we consider the case in which X_h does not affect the distribution of the error vector $\{\epsilon_{hk}, \epsilon_{hk'}, u_h, u_{hk}, u_{hk'}, \eta_h, \eta_{hk}, \eta_{hk'}, v_h, v_{hk}, v_{hk'}\}$.[8] In this case (8) and (10) imply that X_h does not affect the distribution of DS_{hk} but does affect the levels S_{hk} and $S_{hk'}$. Using the fact that $D(\eta_{hk}S_{hk}) = \eta_{hk'}DS_{hk} + D\eta_{hk}\, S_{hk}$ and using (8) and (10) to evaluate the difference between (11) and (12) implies that the probability limit of the estimator of r_2 is equal to

$$(13a)\qquad \mathbf{Bias} = \frac{\partial S_{hk}}{\partial X_h}\, B_{D\eta,DS}\ ,$$

where from (8) the first term $\partial S_{hk}/\partial X_h = -(a_2 + r_2 + c_2)/(a_1 + c_1)$ is the effect of X_h on the expected value of S_{hk} and

$$(13b)\quad B_{D\eta,DS} \equiv \frac{\mathrm{Cov}\left(\dfrac{-1}{a_1 + c_1}\left(Du_{hk} + D\eta_{hk} + Dv_{hk}\right),\ D\eta_{hk}\right)}{\mathrm{Var}\left(\dfrac{-1}{a_1 + c_1}\left(Du_{hk} + D\eta_{hk} + Dv_{hk}\right)\right)}$$

[8] In the NLS we found that $\mathrm{Var}(DS_{hk})$ does not depend much on our measures of father's education or mother's education, which is consistent with this assumption given (10).

is the coefficient of the regression of $D\eta_{hk}$ on DS_{hk}. Assume that parental choices that influence the sibling difference in the marginal effect of schooling on the discount factor β (through Du_{hk}) and/or the sibling difference in the direct utility from schooling $V(S_{hk})$ (through Dv_{hk}) either reinforce or only partially offset the effects of $D\eta_{hk}$ on DS_{hk}. Then since $-1/(a_1+c_1)$ is positive the covariance term in (13b) and $B_{D\eta,DS}$ will be positive. Since our measures of X_h have a strong positive effect on the mean of S_{hk} and $S_{hk'}$, $\partial S_{hk}/\partial X_h$ is likely to be positive. We conclude that within family OLS estimates of the interaction term r_2 are likely to be positively biased <u>even if</u> X_h <u>is independent of the error term in (9)</u>.[9] A similar argument applies if X_h is a school quality measure, particularly if school quality variables that raise the return to a year in school raise S_{hk} by making school more enjoyable for students ($c_2 > 0$) or by lowering discount rate and thus raising the discount factor β ($a_2 > 0$).

The result may seem surprising given that X_h is independent of the error in the wage equation. The intuition is as follows. Note first that since we can rewrite $D(\eta_{hk}S_{hk})=D\eta_{hk}S_{hk} + \eta_{hk'}DS_{hk}$, the unobserved difference in education slopes $D\eta_{hk}$ enters with weight S_{hk} in (9). The size of the covariance between DS_{hk} and the wage error involving $D\eta_{hk}$ increases with X_h because X_h shifts out the mean of both S_{hk} and $S_{hk'}$. Under the assumptions made above, X_h does not alter $Var(DS_{hk})$. The covariance between DS_{hk} and $D\eta_{hk}$ relative to the variance of DS_{hk} is partially responsible for the bias in the return to education in a subsample with a particular value for X_h. The covariance is larger when schooling levels are high for both siblings than when they are low. Consequently, when one uses sibling differences the bias in the return to education is higher in a high X_h sample than in a low X_h sample. The interaction coefficient r_2 picks up this relationship between the bias and X_h and is biased upward as a result.[10]

Note that a similar bias may arise in studies that interact state or school specific characteristics with education slopes while controlling for the state or school, assuming that these characteristics are

---

[9] The bias is negative if $\partial S_{hk}/\partial X_h$ is negative.

[10] Given that we argue that the bias arises because the weight on the η_{hk} terms increases with X_h, one may wonder whether the analysis is affected by affine transformations of X_h.

positively related to the demand for education. To see this, re-interpret h as a state-age cohort, k as person subscript, X_h as a state and cohort specific education characteristic (such as pupil/teacher ratio), η_{hk} as heterogeneity in rates of return among students from the same state and age cohort, and D as the operator that takes differences is across pairs of individuals from the same state and age cohort. Then the wage equation (4) becomes similar to that of Card and Krueger (1992). Since they show that the school variables they use for X_h are positively associated with S_{hk}, it appears that individual and family level heterogeneity in the return to education provide a source of upward bias in their estimates. Other factors may bias their results in the other direction.[11]

To get a sense of whether the bias is likely to be important in our case, we performed the following rough calculations. In the NLS data $\partial S_{hk}/\partial X_h$ is about .35 for father's education or for mother's education. The sample estimate of $Var(DS_{hk})$ for the pooled sample of men and women is 5.9*.8=4.72, where the factor .8 is a measurement error correction (see below). If one assumes that $D\eta_{hk}$ is uncorrelated with $Du_{hk} + Dv_{hk}$, that $Var(D\eta_{hk})$ accounts for 25 percent of DS_{hk}, and that an increase in the return to schooling of .1 (e.g., from .08 to .18) will raise the expected value of S_{hk} by 4 years (which implies that $-1/(a_1+c_1)$ is 40), then the bias in r_2 is .002. The implied standard deviation of η_{hk} is .019 which does not seem wildly implausible. Since below we multiply coefficients by 100, this would translate into a bias of .20, which is not trivial relative to the coefficient estimates we report. Lowering the R^2 of η, raising $-1/(a_1 + c_1)$, or assuming that $u_{hk} + v_{hk}$ depends positively on η_{hk} would lower the bias and the implied standard deviation of η_{hk}. Given all of the assumptions underlying (13) and uncertainty about the various parameter values in the above calculation we conclude only that a modest upward bias is a serious possibility.

[11] See their paper. See Lang (1993a) for a discussion of potential biases in their estimates. One source of bias Card and Krueger consider that we ignore is the possibility that X_h is correlated with state level heterogeneity in education slopes that arises from the demand side of the labor market. However, we rely on variation in X_h across individuals rather than across regions, so we doubt if this is a major problem. Heckman et al (1994) re-examine the evidence for school quality effects in the Card and Krueger paper and obtain mixed results.

Note that the bias could be negative if parents with large values of X_h tend to offset sibling differences, leading to a reduced variance in $D\eta_{hk}$ or to a negative covariance between $D\eta_{hk}$ and some of the other terms. Also, our log wage specifications include nonlinear education terms in the wage and other explanatory variables. Many of our specifications include more than one interaction term with education, and all of our models include a measure of experience (part of the Z_{hkt} vector in (1) and (2)), which was omitted from the above discussion entirely. While mindful of these limitations of our analysis, we nevertheless tentatively conclude that the estimate of r_h (the influence of the parents' characteristics or the school's characteristics on the daughter's return to education) will be biased upward. Unfortunately, we do not have a solution for this problem.[12]

The Effects of Measurement Error in Schooling:

Griliches (1977) has emphasized the problems that measurement error may pose when one uses sibling differences to estimate the main effect of education. Ashenfelter and Krueger (1994) and Ashenfelter and Zimmerman (1993) are recent examples of studies that try to deal with the problem. Less is known about how measurement error in the education reports of children and in X_h will affect the interaction coefficient r_2. The effect is not obvious because the model contains both the sibling difference in education and the interaction between the sibling difference in education and X_h. Many of the specifications reported below include controls for sibling differences in other variables, such as IQ

[12] There are two alternative approaches to estimating the effect of X_h on the return to education that we have considered but not used. The first is instrumental variables. If one has a variable Q_h that affects the demand for education but neither the education slope of wages nor the wage level conditional on education, then one may use Q_h and interactions between Q_h and X_h as instruments for S_{hk} and X_hS_{hk} in (3). An advantage of this approach is that it would deliver consistent estimates of the main effect of education as well as the interaction term. Unfortunately, measures of family resources are likely to be correlated with the additive error component ϵ_h given the strong intergenerational correlation in earnings. This is not a problem in the differenced equation (5), but only instruments that are correlated with sibling differences in education but not with ϵ_{hk} or η_{hk} are helpful here.

An alternative approach is to correct for self selection in education choice using the approach of Willis and Rosen (1979) and Garen (1984). The problem of what to exclude from the wage equation again arises unless one is willing to achieve identification through a functional form assumption (See Heckman and Honore (1990).) As we noted earlier, Willis and Rosen's key exclusion restriction is that parental background variables do not affect education slopes (i.e., r_2=0). Consequently, we think that our approach is both the simplest and the best procedure for estimating the effects of family characteristics on the education slope given the available data and econometric methods, despite the likelihood of positive bias. Kyriazidou (1994) has recently proposed a fixed effects estimator for models with sample selection that would permit one to estimate the effect of X_h on the education slope while controlling for family fixed effects. Her estimator requires that at least one sibling specific variable affect education but not wages.

tests, that will be related to the true differences in sibling schooling but probably not the measurement error.

In Appendix 1 we analyze the effects of measurement error on the coefficient r_2 in the equation

(9') $Dw_{hkt} = DZ_{hkt}B_1 + [r_1 + r_2X^*_h] DS^*_{hk} + D\epsilon_{hk} - r_2[u^*_{Xh} DS^*_{hk} + X^*_h u^*_{Shk}]$,

where

$DS^*_{hk} = DS_{hk} + u^*_{Shk}$,

$X^*_h = X_h + u^*_{Xh}$,

and u^*_{Shk} and u^*_{Xh} are independent of each other and both DS_{hk} and X_h. We eliminated the random coefficients in (9) from the model and also assume in our analysis that the wage error component $D\epsilon_{hk}$ is independent of all other variables, so that measurement error is the only source of bias. We have re-introduced the variable DZ into the model. We show that

(14) $\text{plim}(r_1) = [1 - (\rho_{DS,DZ})^2][Var(DS_{hk})/[Var(DS_{hk}) + Var(u^*_{Shk})]]\, r_1$,

which is the same as the standard result for the model with $r_2=0$. For the case in which X_h is a scalar, we show that

(15) $\text{plim}(r_2) = r_2\{Var(DS_{hk})/[Var(D_{Shk}) + Var(u^*_{Shk})]\}\{Var(X_h)/[Var(X_h) + Var(u^*_{Xh})]\}$

Interestingly, the bias in r_2 does not depend on the correlation $\rho_{DS,DZ}$ between DS_{hk} and DZ_{hk}, while the bias in the main effect r_1 does. The assumptions that 10 percent of the cross section variance in schooling is attributable to measurement error and that the sibling correlation in schooling is .5, imply that $Var(DS_{hk})/[Var(D_{Shk}) + Var(u^*_{Shk})]$ is .8181.[13] If X^*_h is the child's report of father's education, then .90 is plausible for $Var(X_h)/[Var(X_h) + Var(u^*_{Xh})]$. This suggests that the within family OLS

---

[13] Ashenfelter and Zimmerman discuss the appropriate measurement error corrections for the return to education when interaction effects are excluded in the context of the NLS sibling data on young men used here. They use 6.7% to 20% as the range of estimates of the percentage of the variance in schooling that is due to measurement errors. We follow Card's (1994) discussion of their work and other studies and use 10%. Using the 20% figure would imply a bias correction on the interaction coefficient of 1.66.

estimates of r_2 should be inflated by the factor (1/.818)(1/.9)= 1.36, which is a substantial correction. When X_h is a school variable we assume that the school variable is measured without error.

This correction factor is not changed by dropping the assumption that DS_{hk} is independent of $D\epsilon_{hk}$, although one would be applying the correction to a biased coefficient. It may be affected by the presence of differences in the square and cube of S_{hk} in the models we estimate. We do not have analytical results in this case but use the formula anyway. If X_h is a vector then (15) can be used to reinterpret r_2 and X_h as corresponding to a particular element of X_h, provided that all elements of X_h are measured without error and/or all elements of X_h are independent. Since the latter condition is unlikely for the family background and school variables we use, we are on firm ground in using (15) only when we work with one family background or school characteristic at a time.

A Fixed Effect Approach

Since we have more than one wage observation for most individuals in the sample and the number of children differs across households, a more efficient estimation approach is to work with (3) and include a separate intercept for each family (a fixed effect) to absorb ϵ_h. This permits us to use the time variation in experience for each individual to identify the experience effects and year dummies even in the case of individuals who do not have a sibling in the sample. It is also a convenient way to use all of the data on households with more than two children.[14]

3. Data

Most of the analysis is based on the Young Men and Young Women cohorts of the NLS. The young men were 14 to 24 years old in 1966 and were followed through 1981. The young women were 14 to 24 in 1968 and continue to be followed. We use their data through 1988. Since some of the households contribute more than one person to the young men and/or young women surveys, we can match data on sibling pairs. We restrict the sample to individuals who were at least 24 years old prior to leaving the survey and who completed at least 4 years of school. We chose this age cutoff to reduce

[14] Efficiency could be improved by using a GLS estimator that accounts for fixed person specific error components. We do not do this but do correct standard errors for person specific correlation across time.

transitory variation in labor market outcomes associated with the transition between school and work. We use wage data from a particular year only if the individual was at least 24, had left school and did not return in a later year, and reported a wage greater than $1.50 in 1982-84 dollars.

The dependent variable is the log of the real reported wage for a given year. An individual may contribute more than one observation to his or her sample.[15] Education (S_{hk}) is the highest grade completed. The measures of parental education (DADED and MOMED) are based on the young men and young women's survey reports.[16] The IQ measure is obtained from the school survey. We use multivariate regressions to impute missing values for IQ, mother's education, and father's education. All equations that involve IQ include a dummy variable that is one if the IQ score was imputed and is zero otherwise.[17] Imputed data for DADED and MOMED are handled in the same way. Since missing data on DADED are likely to be related to whether the individual's father was present in the household while he or she was growing up, it is inappropriate to simply eliminate cases with missing data from the analysis.

<u>School Variables:</u>

We also report results in which a subset of the school quality variables and their interactions with S_{hk} are added to the wage regression. The variables are the student/teacher ratio (ST_RATIO), expenditures per student (EXPEND), average salary for an inexperienced teacher with a BA

---

[15]. The average number of observations is 4.5 per young man and 5.3 per young women. See the Appendix 2 for summary statistics and more detailed descriptions of the young men's and women's data sets.

[16] Using information provided by the parents themselves would limit the sample to only those young men and young women whose parents are found in the mature women and older men's cohorts. DADED is missing for 24.6 percent for young men, and 24.4 for women. MOMED is missing for 12.5 percent for young men and 9.2 percent for women. We use the average of the reports of DADED and MOMED of all siblings in the family (both young men and young women) as the measures of DADED and MOMED. One could improve upon this by using the parental education reports of the various siblings as the basis for an IV scheme of the type employed in Ashenfelter and Krueger (1994), but we doubt this would make much difference.

[17] IQ is missing for 31.8 percent of young men's sample and 31.7 percent of the young women. Since IQ is obtained from the high schools, we do not have a measure for persons who did not attend high school. The NLS identifies the specific test used by the school, but we have not made use of this information. The various tests were normalized to a common scale.

(T_SALARY), and an index of school quality constructed as part of the NLS (S_QUALITY).[18] Our analysis of the high school variables in Table 8 is limited to observations from persons for whom a valid report of a particular school variable is available.[19] Since siblings sometimes attend different high schools, there is variation among siblings in the school variables. Consequently, we are able to identify the effects of the school variables holding family fixed effects constant, although the number of cases is limited.

Hanushek (1986) and others have criticized the use of school level input measures such as T_Salary and EXPEND on the grounds that they are weak proxies for what goes on in the classroom. Furthermore, our information is limited to high schools. In using the data we are assuming that elementary school quality is strongly correlated with high school quality, and that high school and elementary school quality condition the returns to college. Since the data are available only for persons who started high school, the main effect of the variables in the models we estimate may be capturing the effects of differences in elementary school quality. Below we discuss the effect of high school quality when S_{hk} is 12 and how the effect of the school variables changes with S_{hk}. The distinction between the main effect and the interaction effects of high school quality has less theoretical appeal than the distinction between the main effect and interaction effect of family background.

Functional Form and Control Variables:

We work with cubic specifications for education and IQ and the product of education and labor market experience (more precisely, potential experience, calculated as number of years since last

---

[18] The school quality variable is a composite normalized ranking of 4 individual school variables: student/teacher ratio, teacher salary, counselor/student ratio, and books per pupil. The resulting variable ranges from 0 to 11.

[19] The school characteristics and IQ score we use were collected in 1968 from surveys completed by the respondent's most recently attended secondary school. Responses may be missing for particular variables for a variety of reasons: the respondent had not completed ninth grade by 1968, so the school was not contacted (about 17% of individuals); or the respondent denied permission for the school to be contacted (about 5%); or the school failed to complete the survey fully. Ninety five percent of the contacted schools provided at least some information and 75% provided a full set of responses. It is important to note that the school variables are reported for the year 1968, not the year the student last attended. Consequently, variation across siblings cannot be attributed to changes over time in the same school. Also, since siblings had to be living in the same household at the start of the survey to be in the sample, the family fixed effects results involving school quality measures are not likely to biased by regional variation in wages that is reflected in both teacher salaries and the wages of the NLS sample members.

enrolled in school). We parameterize the models so that the coefficient on the linear education term is the marginal rate of return to education for an individual with 12 years of education. When interactions between education and any or all of IQ, DADED, MOMED are included, the equations are parameterized so that the coefficient on education is the marginal rate of return to education for an individual with 12 years of education, 0 years of experience, an IQ of 100, and a mother and father who each have 12 years of education.[20]

Although not shown in the tables, all equations without family fixed effects include dummy variables for the year for which the wage is reported, child's race, residence in an SMSA, residence in the South, two parents in the household when the child was age 14, in addition to number of siblings, and a cubic in experience.[21]

The Panel Study of Income Dynamics.

The PSID sample is based on the 1968-1989 release of the PSID and consists of persons who were children in the original sample households in 1968 and were aged 24 or older and observed as heads of households or spouses at least once by 1989. Most of the variables are analogous to those in the NLS. However, the experience measure is based on a report of years of full time experience and counts of years in which the person worked more than 1,875 hours. We do not control for whether both parents were present when the individual was 14. Data on IQ and school quality are not available.

III. Empirical Results

In Section III.1 we discuss the effects of father's education and mother's education on the education slope based on both the NLS and the PSID samples. In Section III.2 we examine the effects of IQ on the education slope. In Section III.3 we discuss the effects of the school quality variables on the wage level and the education slope based on the NLS data.

---

[20]. All specifications that include interactions between education and any or all of IQ, DADED, MOMED also include interactions between education and the corresponding missing dummy variable(s).

[21]. In the models with family fixed effects, race and number of siblings are excluded from the control variables since they should be constant within the family.

III.1 The Effects of Father's and Mother's Education on the Education Slope

NLS Results for Young Men

Table 1 presents a set of wage equations for young men with a fixed effect included for each family. All coefficients and standard errors have been multiplied by 100 to make the tables easier to read. We report Huber-White standard errors that allow for arbitrary family specific heteroskedasticity and for correlation across siblings and over time for a given individual.

In column 1 we report a base line equation with all school and background interaction effects excluded. The coefficient on education is the marginal effect of education when education equals 12 years and experience is 0. The coefficient is 3.73 with a standard error of 1.17[22] When we add a cubic specification for IQ, the education coefficient falls to 2.99 (column 2).

In column 4 of Table 1 we add the interaction between father's education and the son's education to the equation containing IQ. The coefficient is .508 (.202), which has the expected sign and is significantly different from 0. Scaling up the coefficient by the measurement error correction factor of 1.36 leads to an estimate of .691 (.275). This is a substantial effect. The interaction between mother's education and the son's education in column 5 has a coefficient (standard error) of .807 (.284). After the measurement error correction the coefficient is 1.10, which is very large. When we include both parents' interactions in column 6 of Table 1 the coefficient on the mother's education interaction rises slightly to .826 (.328) while the coefficient on S_{hk}*DADED falls to .157 (.222). We do not have a formula for the measurement error correction in this case, but we suspect it is also substantial. Thus, the fixed effects estimates suggest that a one year increase in mother's education raises the return to education by about 1 percent, which is a very large effect. The evidence for an independent effect of father's education is weak.

[22] Taking into account the coefficients on higher powers of education, we found that when fixed effects are included, the difference in log wages (times 100) associated with increasing education from 10 years to 12, to 14, and to 16 years is estimated to be 4.7, 14.4, and 27.6 (respectively). For young women the corresponding estimates are 12.2, 24.7, and 35.9. When fixed effects are excluded, the estimates are 8.7, 18.4, and 28.0 for young men and 13.5, 28.9, and 44.0 for young women.

In an effort to get more precise estimates at the cost of bias from omitted family variables that are correlated with the young man's education and his parents' education, we report estimates without family fixed effects in Table 2. The estimate of the rate of return when only parents' education is controlled for (column 1) is 4.67 (.466). This is in the low range of estimates from other studies based on other data sets from 1968-1981 that do not contain detailed controls for family variables. The interaction between father's education and son's education has a coefficient of .101 (.039). The interaction between mother's education and son's education has a coefficient of .084 with a standard error of .043. When we include both parents' interactions, both coefficients fall somewhat. We have not worked out a correction for measurement error for this model, but believe that it is much smaller than in the fixed effect case. Lang's (1993a and b) analyses suggests that there could be a negative bias on the interaction terms if parental education is strongly correlated with tastes for education or negatively correlated with discount rates and there is heterogeneity in education slopes. We place more weight on the fixed effects estimates, which imply a substantial effect of parental education, primarily the mother's education, on the young men's education slope.

NLS Results for Women

Tables 3 and 4 report wage equations for young women that are comparable to Tables 1 and 2 for young men. Table 3 includes fixed effects for each family, while Table 4 does not. The base line specification in column 1 of the two tables indicates that the estimated rate of return to education for women is 6.33 (.995) with fixed effects and 7.40 (.414) without. In percentage terms the gap between the fixed effects estimates and the estimates without fixed effects is smaller for women than men. This is not true of the PSID.[23] When IQ is in the model in column 2 the fixed effects estimate exceeds the OLS estimate by a small amount, although the standard errors are large. Associated with this result is a

---

[23] To fully explore the issue one would have to deal with the fact that there is a time trend in the return to education that we exclude from the models and that the models also contain experience interactions, which are smaller for the women. Measurement error corrections would substantially increase the fixed effects estimates relative to no fixed effects estimates. Allowing the return to education to depend on a time trend did not have much effect on the school and parental variables and interactions, which are our main focus.

peculiar pattern in which the effect of IQ (level) actually changes from positive to negative when we control for family effects (compare column 2 of Tables 3 and 4). When fixed effects are excluded, the coefficient on IQ is a bit larger for women than men (.499 versus .367). [24]

The coefficient on the interaction of daughter's and father's education (in column 4 of Table 3) is -.041 with a standard error of .173. The corresponding interaction term with mother's education is .581 (.215). The correction for measurement error increases the coefficient to .790. These results imply a less powerful effect of parental education on the education slope for young women than for young men. (Recall that for young men the corresponding corrected estimates are .691 and 1.10.) In column 6 we report that the interaction with mother's education rises to .765 (.223) and the interaction with father's education falls to -.285 (.180) when they are both entered into the equation with family fixed effects.

The parents' education interaction terms are essentially 0 when family fixed effects are excluded from the equation. (See columns 4-7 in Table 4.) We prefer the estimates with fixed effects included. These point to a substantial positive effect of mother's education on the education slope. There is little evidence for an independent effect of father's education on the education slope.[25]

PSID Results

In Table 6 we present results for men from the PSID. In Columns 2 and 3 we obtain coefficients of .094 (.122) on S_{hk}*DADED and .328 (.146) on S_{hk}*MOMED when they are entered separately with fixed effects included. The correction for measurement error boosts these coefficients to .128 (.166)

---

[24] For young men, the main effect of IQ has a positive and statistically significant effect on the log wage in nearly all specifications. When family fixed effects are excluded, the coefficients on the linear, quadratic, and cubic terms imply that the wages of individual in the 75th percentile of IQ scores are 5.3 higher than individuals in the 25th percentile. When family fixed effects are included, the corresponding differential is 11.9 percentile. For young women we obtain a differential of 9.6% when family fixed effects are excluded. However, when we add family fixed effects we obtain a negative IQ differential equal to 3.8%. In an earlier version of the paper we explored the issue in some detail and did not arrive at a full explanation. However, it appears to be do in part to an anomaly in the sample of young women who have sisters, because the estimates of the effect of IQ when fixed effects are excluded are smaller for this sample as well. When we discard observations on women with imputed IQ stores, the coefficient on IQ is even more negative (-.556) in the fixed effects case and about the same in the no fixed effects case.

[25] For the NLS samples we examined whether the effect of parental education on the education slope is operating through school quality by adding S_QUALITY and its interaction with S_{hk} to the equation. This had little effect on the coefficients on the interactions parents' education and S_{hk}.

and .446 (.199) respectively. The estimate for DADED is well below the NLS estimate, but the estimates imply the mother's education has a substantial effect on the rate of return. Using the corrected estimate a 2 year increase in mother's education is associated with a .892 point increase in the return to education. When both interactions are entered in column 4 we obtain -.184 (.177) for DADED and .480 (.211) for MOMED. As was the case with NLS, we find little evidence that parental education raises the return to education when we estimate models without fixed effects. (See columns 5-9.)

For women we find little evidence of an effect of parental education on the education slope in the PSID sample (Table 7). The point estimates with fixed effects are all small and negative, although none are statistically significant. Removing the fixed effects has little effect on the point estimates, although some of the coefficients become significant.[26]

PSID and NLS Results for the Pooled Sample of Men and Women

In Table 5 we report estimates from a model in which the men and women are pooled and all coefficients except the interactions between S_{hk}*DADED and S_{hk}*MOMED are allowed to depend on gender. The advantage of pooling the data is that in the fixed effects case we are able to make use of variation in the education outcomes of sibling pairs, including brother-sister pairs. The disadvantage is that the family and sibling fixed effects may be different for same sex siblings than for different sex siblings, and so taking deviations from family means does not eliminate them. The fact that so many coefficients, including the coefficients on the level of mother's education and father's education in the equations without fixed effects are different is particularly worrisome. Butcher and Case (1994) is one of a number of papers that show that gender composition of the family matters for educational attainment and that boys and girls are treated differently with respect to education.

The fixed effects results on the pooled sample show little effect of parental education on the education slope. The coefficients are not significant, and the point estimates are actually negative. The results are quite surprising given the fairly strong positive coefficients on mother's education in gender

---

[26] When we restrict the PSID and NLS samples to individuals who have same sex siblings in the samples, we obtain similar point estimates on the education interaction terms but substantially larger standard errors in the no fixed effects case.

specific samples. We estimated the model on the sample of persons with at least one sibling of the opposite sex in the sample, and found a large negative interaction term on both father's education and mother's education. This term is sufficient to offset the positive coefficients on the men and women considered separately. We do not fully understand the results.

In contrast, the PSID estimates based on the pooled sample are close to an average of the separate results for men and women. In column 1 of Table 5a we report that the fixed effects estimate of the coefficient on interaction between S_{hk} and DADED is basically 0. The interaction with mother's education is .130 (.076) in column 2. The measurement error correction boosts this coefficient to .177. This implies 4 years of maternal education boosts the education slope by .71 percent, which is economically significant relative to a base of 5 percent.

III.2 The Effects of IQ on the Education Slope

Using the NLS sample we examine whether the effect of parental education on the education slope operates through the IQ variable. The results in column 7 of the Tables 1-4 indicate that adding the IQ variables has little impact on the interactions between S_{hk} and parental education. We also examine the effects of "IQ" on the return to education by adding the interaction between IQ and education to models that exclude interactions between S_{hk}*DADED and S_{hk}*MOMED (column 3). We obtain a coefficient of .052 (.059) on the IQ interaction for men and .097 (.049) for women. The point estimate is substantial in the case of women, since they imply that a 10 point increase in the test score would lead to a .9 percent increase in the rate of return. A correction for measurement error in the sibling schooling difference and the fact that the sibling differences in IQ tests are probably less reliable than the levels of the IQ test might increase this further. On the other hand, we do not wish to make too much of the result given that the main effect of IQ has the wrong sign in all of the fixed effects specifications for women. When fixed effects are excluded, the IQ interaction term is -.012 for men and -.042 for women.

In summary, the fixed effects results point to a positive effect of IQ on the return to education, but the evidence is not strong. Adding the interaction term in the variable has little effect on the interactions between S_{hk} and parents' education.

III.3 The Effects of School Characteristics on Wage Levels and the Education Slope

Table 8 explores the effects of school quality on wages. All equations include the standard controls used in Tables 1-4 as well as the interactions between S_{hk} and both mother's education and father's education. Since S_{hk} is 0 at 12 years of school, the main effects of the school quality measures are the effect on the wage for persons with 12 years of schooling. The top panel contains no controls for IQ and, except for the inclusion of the school quality variable and its interaction with S_{hk}, is the same as the specification in column 6 of Tables 1-4. The lower panel adds controls for IQ and IQ*S_{hk} and corresponds to column 7 in the Tables 1-4. The fixed effects results are most interesting, because in contrast to virtually the entire literature on school quality, family background is held constant.[27] The coefficients on the levels (main effects) of the schooling variables are identified by differences across siblings. At the bottom of each column we report the number of panel data observations, the number of persons in the sample, the number of persons with siblings, the number of persons for whom the deviation from family mean is nonzero, the mean and interquartile range of the levels of the variables for the nonzero cases, and the range of the deviations from means. For example, only 303 of the individuals have a sibling in the sample who attended a high school with a different average teacher salary. We pool men and women in this table rather than report separate estimates for each, to increase the number of individual who identify the school effects.[28] Results for men and women separately are the same basic story (not reported).

[27] This is an overstatement, since the family environment differs between siblings, particularly in the case of divorce. We do not distinguish half brother or sisters or step brothers or sisters in our analysis.

[28] In these regressions all coefficients (including the interactions between S_{hk} and parents' education) are permitted to depend on gender except the coefficients on the level and education interaction of the school quality variables.

We begin with S_QUALITY, the composite index of school quality in column 4a and 4b of Table 8. Column 4a reports fixed effects estimates. Deviations from family means are nonzero for 278 men and women. The coefficients on S_QUALITY and its interaction with S_{hk} are 1.57 (.878) and -.867 (.197) in the regressions without controls for IQ. Since the dependent variable is in log form and the mean of S_QUALITY is 5.9, the implied wage elasticity with respect to S_QUALITY quality is .093 when S_{hk} is 12. An increase in the index equal to the interquartile range of 3 is associated with a wage increase of 4.71 percent for a person with 12 years of education. The effect declines to 0 after two years of college. The results are not very sensitive to controlling for IQ.

In column 2a of the table we find a substantial effect of T_SALARY. When IQ is excluded, the main effect is .156 (.036) and the interaction effect is -.025 (.006). The implied wage elasticity at the mean of T_SALARY ($6,018 in 1968 dollars) is .950 when S_{hk} is 12. The results also imply that an increase in starting teacher's salary equal to the interquartile range of $681 in 1968 dollars would lead to a 10.6 percent increase in wages. This is a large increase that declines slowly with additional years of schooling. The estimate is not sensitive to IQ. While the estimates seem implausibly large, it should be kept in mind that the range of variation is limited. When fixed effects are excluded the sign pattern is the same but the magnitude of the main effect is much smaller.

The effects of expenditures per student (EXPEND) are similar but smaller in magnitude. (The correlation between EXPEND and T_SALARY is about .50). With IQ excluded, the fixed effect estimate of the interaction effect is .011 (.025) and of the level effect is .247 (.123). The corresponding estimates without fixed effects are -.023 (.007) and .101 (.018). The fixed effects result implies that an increase in EXPEND by the interquartile range of $228 in 1968 dollars leads to a 5.6 percent increase in the wage of a high school graduate. The implied elasticity at the mean of $578 in 1968 dollars is .132.

In columns 1a and 1b we report results for the student teacher ratio (ST_RATIO). The sample includes 382 persons who attended a high school with a different ST_RATIO than a sibling in the sample. The coefficients are small in magnitude and are not statistically significant. At the sample mean of 20.6 students per teacher they imply an elasticity of .06.

Is Sibling Variation in School Quality Endogenous in the Wage Equation?

Suppose that input measures are higher in college preparatory high schools. If more able siblings are more likely to attend college preparatory high schools in districts where more than one school is available, then school quality differences might be positively related to other factors that raise wages or education slopes. This would lead to positive bias on S_QUALITY, T_SALARY, and EXPEND and negative bias on ST_RATIO in Table 8.

To provide evidence on this issue, we re-estimated Table 8 with controls for whether the person expected help from the parent with college, whether the person liked high school, whether the person worked during high school, whether conditions in the home interfered with studies, and the number of days the student was absent from school. This made little difference. In Table 9 we report regression coefficients from univariate regressions of the deviations from family means of 4 school quality variables on deviations from family means of IQ, S_{hk}, whether there were two parents in the household at age 14, the number of days the student was absent from school, whether the student was in a vocational curriculum (which we view as a negative indicator for wages), and whether the person liked her high school experience. Note that several of the variables, including IQ and education, might be directly affected by school quality, which would bias the results in the direction of finding a positive correlation between school quality and favorable personal characteristics. In any event, the coefficients vary in sign and most are statistically insignificant, and collectively provide little evidence that sibling differences in the school quality measures are positively correlated with sibling differences in wage components or education slopes. For example, IQ is negatively related to teacher salary and is not significantly related to the other variables. The presence of two parents in the household at age 14 has a negative relationship with expenditures per pupil. Of course, these results do not rule out the possibility that there are other unobserved sibling specific components that are positively related to both school quality and wages.

To get a sense of whether there is something unusual about families with siblings who attend different schools, in Table 10 we present the means for families with exactly two children in the sample classified by whether the children attended the same school or not.[29] We show the means of individual, household and school variables. Overall, we find that there are no large or systematic differences between the two groups, with one important exception, discussed below. Siblings who went to different schools tend to have more positive characteristics (education, IQ scores and parental education) on average. However, the means of the absolute differences in these variables between paired siblings are not significantly different across the two groups of siblings. (not reported) The means of the school quality variables and other individual characteristics are similar across the two groups of siblings, as well. It is important to note that there are significant differences across the two groups in the variables that indicate changes of location during schooling years. Siblings who went to different schools are about three times more likely to have one member who moved between high school and the first NLS survey year and one who did not. (not reported.) Also these siblings are about 1.5 times as likely to have lived in different geographical residences at age 14 compared to siblings who went to the same school.[30]

The fact that family moves are associated with schooling differences suggests that one identify school quality affects by using sibling differences in school quality that are associated with differences in location. The NLS reports the size of the city in which the person lived at age 14. (There are 6 categories). We use within family variation in school quality that is associated with within family variation in the size of the city where the person lived at age 14 to construct an instrumental variables estimator for S_QUALITY and S_QUALITY*S_{hk}. We add child specific dummies for the city size categories to the models as well and continue to control for family fixed effects. The results are reported in the bottom panel of Table 8. The coefficient on ST_RATIO becomes -.906, which has the expected sign. However, the standard error rises to .94. The coefficient on the T_SALARY and

---

[29] Two-sibling pairs account for 78% of the individuals with siblings in the data set.

[30] There are also significant differences between the two groups of siblings in the gender composition of the sibling pairs. Fifty four percent of the pairs that went to the same school where brother-sister pairs, while 66% of the pairs who went to different schools were brother-sister pairs.

T_SALARY*S_{hk} increase slightly, although once again the standard error rises substantially. The coefficients on EXPEND and S_QUALITY fall substantially, but the interaction effects rise. Overall, we view the IV-fixed effects estimates as broadly consistent with the other fixed effects results in Table 8.

In summary, using a family fixed effects estimator and a family fixed effect-IV estimator we find a substantial effect on the wage rate for the school quality index, teacher's salary, and expenditures per pupil. The fixed effects have the wrong sign for student teacher ratio and are not statistically significant. Furthermore, there is little evidence that sibling variation in school quality is correlated with sibling variation in other wage determinants.

IV. Conclusion

In this paper we have explored the possibility that the education slope of wage equations are influenced by IQ, parental education, and school characteristics. In our preferred specifications we take advantage of the presence of siblings in the PSID and NLS to use family fixed effects to control for unobserved differences in family background. Our results may be summarized as follows. First, for men in the PSID and NLS we find a substantial positive effect of the parents' education, particularly mother's education, on the education slope. For women, we find a substantial effect of mother's education in the NLS but not in the PSID. However, when we pool the NLS samples of men and women, which results in a large increase in sibling pairs, we do not find much of an effect of parental education on wage slopes. In the PSID the pooled results point to a modest positive effect of mother's education on the education slope and are roughly equal to the average of the separate results for men and women. When we exclude family fixed effects, we find little evidence of an important education interaction term, regardless of the data source or gender composition of the sample. Thus, while we find a substantial positive effect of parental education on wage slopes for our preferred specifications (same sex samples with fixed effects) in three of four cases, and the PSID pooled results point to an economically significant effect, the evidence is not compelling if one places much weight on the specifications without fixed effects or the NLS pooled sample, particularly when one considers the fact that the estimates are likely to be biased upward. It should be kept in mind that there is evidence in our tables and abundant evidence elsewhere that parental characteristics, including education, have substantial effects on the wage level. Even if the interaction effect between parental education and education were 0 in a model of the log wage, an extra year of parental education would increase the dollar value of a year in school.[31]

We find that differences between siblings in high school quality have a substantial positive relationship with differences in wages. While we do not have consistent evidence on how the wage effects depend on years of schooling, we find that increases in teacher's salary and expenditures per pupil equal to the interquartile range for these variables leads to wage increases of 10.6 percent and 5.6 percent, respectively, for a student who leaves school after high school. We find qualitatively similar but much noisier estimates when we use an IV-fixed effects scheme to deal with possible endogeneity of variation across siblings in school quality. There is little evidence that the results are biased by a positive correlation between sibling differences in school inputs and sibling differences in other factors that are favorable to wages. While our results do not confirm the recent results of Card and Krueger suggesting that educational inputs have large effects on education slopes, our results are consistent with theirs in that they imply that educational inputs have strong positive effects on wages of high school graduates. On the other hand, we do not have a good understanding of why the results that control for family fixed effects are much stronger than the results without such effects. Further investigation of the use of within family variation in school quality variables to study the effects of school quality seems warranted.

---

[31]. Note also that when the return to education exceeds the discount rate or the effect of education on wages is nonlinear, the ex ante return must take account of the fact that educational outcomes are uncertain. Variables that affect years of schooling may raise the ex ante return even if they have no effect or a small negative effect on the response of wages to a given number of years of schooling. Using NLS72, Altonji (1993) finds that favorable family background raise the ex ante rate of return to starting college even though his methodology assumes that wage response to education does not depend on family background. He also finds that aptitude and achievement raises the ex ante turn to starting college both by raising the wage response to education (the ex post return) and by leading to education outcomes with higher payoffs.

References

Altonji, Joseph G. "The Effects of Family Background and School Characteristics on Education and Labor Market Outcomes." Mimeo, Northwestern University, December 1988.

Altonji, Joseph G., "The Demand for andReturn to Education When Education Outcomes Are Uncertain". Journal of Labor Economics 11 No. 1 Part 1 (January 1993): 48-83.

Ashenfelter, Orley and Alan Krueger, "Estimates of the Return to Schooling for a New Sample of Twins." American Economic Review 84 No. 5 (December 1994): 1157-1173.

Ashenfelter, Orley and David J. Zimmerman, "Estimates of the Returns to Schooling from Sibling Data: Fathers, Sons, and Brothers". NBERWorking Paper No. 4491 (1993).

Becker, Gary, "Human Capital and the Personal Distribution of Income: An Analytical Approach", Wytinsky Lecture, University of Michigan (1967). Reprinted in Gary Becker, Human Capital, third edition, University of Chicago Press (1993).

Betts, Julian, "Is There a Link Between School Inputs and Earnings? Fresh Scrutiny of an Old Literature". unpublished paper, Dept. of Economics, University of California, San Diego (November 1994).

Betts, Julian, "Does School Quality Matter? Evidence from the National Longitudinal Survey of Youth". forthcoming in Review of Economics and Statistics

Butcher, Kristin F. and Ann Case, "The Effect of Sibling Sex Composition on Women's Education and Earnings", Quarterly Journal of Economics, CIX (3) (August 1994):531-564.

Cain, Glen G., "The Economic Analysis of Labor Market Discrimination: A Survey," in O. Ashenfelter and R. Layard (eds.), Handbook of Labor Economics. Amsterdam: Elsevier Science Publishers, 1986.

Card, David, "Earnings, Schooling, and Ability Revisited", Industrial Relations Section Working Paper #331, Princeton University (May 1994).

Card, David, and Alan Krueger. "Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States." Journal of Political Economy 100 (February 1992): 1-40.

Card, David and Alan Krueger. "The Economic Return to School Quality: A Partial Survey", (1994).

Garen, John, "The Returns to Schooling: A Selectivity Bias Approach with a Continuous Choice Variable", 52 No. 5 (September 1984): 1199-1218.

Griliches, Zvi, "Sibling Models and Data in Economics: Beginnings of a Survey," Journal of Political Economy 87 (October 1979): S37- S64.

Griliches, Zvi, "Estimating the Returns to Schooling: Some Econometric Problems." Econometrica 45 (January 1977): 1-22.

Hause, John C. "Earnings Profile: Ability and Schooling", Journal of Politicial Economy 80 Part II (May/June1972): S108-S138.

Hauser, Robert M. "Socioeconomic Background and Differential Returns to Education." In Does College Matter? Some Evidence on the Impacts of Higher Education, L.C. Solmon and P.J. Taubman, eds. New York: Academic Press, 1973.

Hanushek, Eric "The Economics of Schooling: Production and Efficiency in Public Schools," Journal of Economic Literature 24 (September 1986): 1141-77

Hanushek, Eric, " Differential Expenditures and School Performance," Educational Researcher, (May 1989)

Heckman, James J. and Bo E. Honore, "The Empirical Content of the Roy Model". Econometrica 58 No. 5 (September 1990): 1121-1150.

Heckman, James J. , Anne S. Layne-Farrar, and Petra E. Todd, "Does Measured School Quality Really Matter?" unpublished paper, University of Chicago (December 1994).

Hedges, Larry V., Rob Greenwald, and Richard D. Laine, "The Effect of School Resources on Student Achievement", unpublished paper, University of Chicago (February 1995).

Lang, Kevin, "Explaining Variation in Estimated Returns to Education Across Time and Place unpublished paper, Department of Economics, Boston University (May 1993).

Lang, Kevin, "Ability Bias, Discount Rate Bias, and the Return to Education", unpublished paper, Department of Economics, Boston University (October 1993).

Link, Charles R. and Edward C. Ratledge, "Social Returns to Quantity and Quality of Education: A Further Statement", Journal of Human Resources, X(1) (1975):78-89.

Kyriazidou, Ekaterini, "Estimation of a Panel Data Sample Selection Model" unpublished paper Northwestern University (1994).

Morgan, James and Ismail Sirageldin. "A Note on the Quality Dimension in Education." Journal of Political Economy 76 (September 1968): 1069- 1077.

Olneck, Michael R. "The Effects of Education." In C. Jencks, et al., Who Gets Ahead ? New York: Basic Books, 1979.

Siebert, W. Stanley, "Developments in the Economics of Human Capital." In Labour Economics. London and New York: Longman, 1985.

Taubman, Paul and Terrence Wales, Higher Education and Earnings. New York: McGraw Hill (1974)

Welsh, Finis. "Black-White Differences in Returns to Schooling." American Economic Review 63 (December 1973): 893- 907.

Willis, Robert J., and Sherwin Rosen. "Education and Self- Selection." Journal of Political Economy 87 (October 1979): S7- S36.

Appendix 1: The effects of measurement error on r_2

For exposition we reproduce equation (9')

$$(9')\ Dw_{hkt} = [r_1 + r_2X^*_h]\, DS^*_{hk} + \beta_1 DZ_{hk} + D\epsilon_{hk} - r_2[u^*_{Xh}\, DS^*_{hk} + X^*_h\, u^*_{Shk}],$$

where $DS^*_{hk} = DS_{hk} + Du^*_{Shk}$, $X^*_h = X_h + u^*_{Xh}$.

To focus on the effects of measurement eror we assume that the wage error component $D\epsilon_{hk}$ is independent of all other variables so that OLS is unbiased in the absence of measurement error. We assume first that Du^*_{Shk} and u^*_{Xh} are independent of each other and DS_{hk}, DZ_{hk}, and X_h. Note that X_h is uncorrelated with DS_{hk} and with DZ_{hk} by construction. We add the stronger assumption that X_h is independent of DS_{hk} and DZ_{hk}. X_h is normalized to have mean 0. These assumptions imply that

$Var(DS^*_{hk})=Var(DS_{hk}) + Var(u^*_{Shk})$, $Cov(DZ_{hk},DS^*_{hk})=Cov(DZ_{hk},DS_{hk})$ and

$Var(X^*_h\, DS^*_{hk})=(Var(DS_{hk}) + Var(Du^*_{Shk}))(Var(X_h) + Var(u^*_X))$.

Since $Cov(X^*_hDS^*_{hk},DZ_{hk})=E(X^*_h(DS^*_{hk}DZ_{hk}))-E(X^*_hDS^*_{hk})E(DZ_{hk}) = 0\ E(DS^*_{hk}DZ_{hk}) - 0$ by virtue of the fact X^*_h is independent of DS^*_{hk} and DZ_{hk}, the fact that the expectation of a product of independent variables is the product of the expectations, and the fact that all three variables have mean 0. A similar argument establishes that $Cov(X^*_hDS^*_{hk},DS^*_{hk})=0$. Evaluating the covariance between the composite error term and DS^*_{hk}, $X^*_hDS^*_{hk}$, and DZ_{hk} (respectively) probability limit of the OLS estimator $\{\hat{r}1, \hat{\beta}_1, \hat{r}_2\}'$ is $\{r_1, \beta_1, r_2\}'$ plus the bias term

$$\begin{bmatrix} Var(DS_{hk}) + Var(u^*_{Shk}) & Cov(DZ_{hk},DS_{hk}) & 0 \\ Cov(DZ_{hk},DS_{hk}) & Var(DZ_{hk}) & 0 \\ 0 & 0 & (Var(DS_{hk}) + Var(u^*_{Shk}))\cdot(Var(X_h) + Var(u^*_{Xh})) \end{bmatrix}^{-1} \cdot$$

$$\begin{bmatrix} -r_1 Var(u^*_{Shk}) \\ 0 \\ -r_2(Var(u^*_{Shk})(Var(X_h) + Var(u^*_{Xh})) + Var(u^*_{Xh})Var(DS^*_{hk}) \end{bmatrix}$$

Taking advantage of the block diagonal structure to evaluate the above expression for the bias leads to equations (14) and (15) in the text. If one redefines r_1 and r_2 as the probability limits of $\hat{r}_1$ and $\hat{r}_2$ one concludes that the correction factor for measurement error is not changed by dropping the assumption that DS_{hk} is independent of $D\epsilon_{hk}$, although the corrected coefficients may be biased. If one sets $Var(u^*_{Xh})$ to 0 and re-interprets the terms involving X_h as matrices, one can use the above expression for the bias to verify that the claim in the text that (15) holds when r_2 and X_h are vectors if (1) there is no error in the measure of X_h and/or (2) the elements of X_h are independent.

Appendix 2A: Summary of NLS Young Men's and Young Women's Data Sets

There are 5225 men and 5159 women in the younger cohorts of the NLS. Young men were first surveyed in 1966 when they were between 14 and 24 years old, and were surveyed 12 times between 1966 and 1981. Young women were surveyed 15 times between 1968 (when they were between 14 and 24 years old) and 1988, the last year of data we use. We kept a particular observation if all the following conditions were met: the individual was at least 24 years old and out of school, had completed at least four years of school and reported an hourly wage of at least $1.50 (1982-4$).

<u>Young Men's Data Set:</u>

After imposing these screens we are left with 3994 young men, 3340 (91.4%) of whom have no brothers in the young men's data set, and 654 of whom do. There are 286 sets of two brothers (7.8% of the young men), 26 sets of three brothers (0.7%) and 1 set of four brothers. All together, these young men provide 18,092 observations. The number of observations per person depends on the individual's age at the beginning of the survey, time spent in school, and number of years of valid wage reports. The distribution of the number of observations per person is given below:

Number of Obs	1	2	3	4	5	6	7	8	9-12
Number of Persons	552	528	525	562	620	408	714	165	420
(%) Contributing This Many Obs	13.8	13.2	13.1	14.1	15.5	10.2	5.4	4.1	10.5

The average number of observations per person is 4.5, the median is about 4 and the mode is 5 observations. The maximum number of observations for any brother set is 22 observations, while the mean, median and mode number of observations per brother set (including singletons) is 5 observations.

<u>Young Women's Data Set:</u>

After imposing our screens we are left at least one valid observation for 3924 young women. Of these, 3269, or 91%, have no sisters in the data set. There are 273 sets of two sisters (7.6% of the young women), 35 sets of three sisters (1.0%), 1 set of four sisters and one set of 5. They contribute 20,859 observations over the course of the 15 surveys. The distribution of observations per person is listed below.

Number of Obs	1	2	3	4	5	6	7	8	9-15
Number of Persons	517	451	446	370	384	316	373	425	647
(%) Contributing This Many Obs	13.2	11.5	11.3	9.41	9.85	8.02	9.5	10.8	16.5

The average number of observations per person is 5.3, the mode is 1, and the median is about 5 observations. The average number of observations per sibling set (including singletons) is 5.8, the median is 5, the mode is 1 and the maximum number of observations per sibling set is 25.

Variable Definitions and Summary Statistics:

Education is the highest grade completed over the course of the survey. Potential experience is the number of years since last enrolled in school. If the school leaving age is unknown, then experience is age minus 14 years. The report of father's and mother's education is from the first survey year. Individuals who did not provide one of these reports, but who had siblings that did were assigned the average report of the other siblings. About 1% of the missing values were filled in this way. In other cases, DADED and MOMED were predicted from regressions on the non-missing observations. IQ scores were imputed for individuals with missing reports; separate regressions were used for men and women. Missing value indicators are included in the regression analysis whenever the particular variable is used. Means and Standard deviations of key variables are as follows.

	Men (N= 3994)		Women (N= 3929)	
Variable	Mean	Std Dev	Mean	Std Dev
Age in 1968	20.4	3.21	18.9	3.05
Education	13.1	2.72	13.0	2.51
Father's Education	9.8	3.75	10.0	3.83
(percent missing)	(.23)		(.24)	
Mother's Education	10.2	3.21	10.3	3.16
(percent missing)	(.11)		(.08)	
Black	0.25		0.28	
Other Race	0.01		0.01	
Number of Siblings at Start of Survey	3.35	2.59	3.53	2.62
Panel Mean Log Wage	2.23	0.46	1.79	0.46
Panel Mean Age	29.1	3.80	31.8	5.32
Panel Mean Experience	9.06	4.74	11.8	6.18
School Reported Variables (percent missing)				
IQ Score	101.5	15.7	102.4	15.1
	(.33)		(.14)	
ST_RATIO	20.6	5.31	18.6	8.03
	(.24)		(.20)	
PRIVATE?	0.08	0.27	0.08	0.27
	(.25)		(.25)	
TSALARY (68$)	6018	571	6014	577
	(.28)		(.28)	
EXPEND (68$)	581	171	575	171
	(.35)		(.35)	
SQUALITY	5.88	2.08	5.82	2.10
	(.31)		(.33)	

Appendix 2B: Summary of PSID Men's and Women's Data Sets

The PSID data set was constructed similarly, except that the PSID lacks IQ and school quality measures. Observations from the years 1968 to 1989 were kept for heads and wives from the PSID who were between 24 and 55 years old and were out of school and had completed at least 4 years of schooling. Sibling sets were found by looking at the relationship to head of household variable in 1968 and giving all individuals in the same household who were sons or daughters of the head the same identifier. The men and women who were heads or wives in 1968 are included in the data set, but they will have no siblings in the data set. The men and women who are counted as siblings could only have been children of the head of household in 1968, and so were not heads themselves.

Men's Data Set:

In the men's sample there are 61,427 observations contributed by 7272 men from 6434 families. There are 5888 men who have no brothers in the data set; 1384 young men belong to brother sets. There are 355 sets of two brothers, 119 sets of three, 49 sets of 4, 19 sets of five, 3 sets of six, and 1 set of eight brothers. The number of observations per person is given below.

Number of Obs	1-2	3-4	5-6	7-8	9-10	11-12	13-14	15-16	17-22
Number of Persons	1626	991	865	658	632	554	464	402	1080
(%) Contributing This Many Obs	22.3	13.6	11.9	9.0	8.7	7.7	6.3	5.5	14.9

The average number of observations per person is 8.4, the median is about 7 and the mode is 1 observation. The average number of observations per brother set is 9.5, the median is about 8 and the mode is 1 observation. The maximum contribution of any one brother set is 87 observations.

Women's Data Set:

The women's sample has 50,011 observations contributed by 7020 women from 6157 families. There are 5605 women who have no sisters in the data set and 1415 who do. There are 347 sets of two sisters, 134 sets of three, 47 sets of four, 17 sets of five, 4 sets of six, 2 sets of 7 and 1 set of eight sisters. The women generally have fewer observations per person than the men. The number of observations per woman is given below.

Number of Obs	1-2	3-4	5-6	7-8	9-10	11-12	13-14	15-16	17-22
Number of Persons	1856	1128	915	714	609	494	383	308	613
(%) Contributing This Many Obs	26.4	16.1	13.0	10.1	8.6	7.0	5.5	4.4	8.7

The average number of observations per woman is 7.1, the median is 5 and the mode is 1 observation. The average number of observations per sister set is 8.1, the median is about 6 and the mode is still 1 observation. The maximum contribution of any one sister set is 60 observations.

Variable Definitions and Summary Statistics:

The wage measure is the log of the real (1982-84$) average hourly earnings. Reports less than $1.50 were excluded. Education is the last reported highest grade completed. Observations were thrown out for any year the respondent reported being a student. Father's education and mother's education are reported by respondents. Only about 5% of these responses are missing. These bracketed responses were recoded (e.g., "0-5 years"= 4, ... "some college"= 13.) For about 2% of individuals, we assigned the average of the other siblings' reports to fill in a missing DADED or MOMED value. In the men's and women's data sets, about 93% of the individuals' DADED and MOMED reports differ by less than one year from the average of the siblings' reports.

Experience is the number of years of full time employment since age 18. This is reported starting in 1974. We kept the first valid response to this variable, then used the yearly reports of annual hours (defining full time employment as 1875 annual hours) to increment experience in the years previous to and following the first valid report. If years of experience exceeded age minus 14, experience was set to missing.

The means and standard deviations of key variables are as follows.

	Men (N= 7272)		Women (N= 7020)	
Variable	Mean	Std Dev	Mean	Std Dev
Education	12.4	2.84	12.4	2.51
Father's Education	9.8	3.47	9.1	4.34
(percent missing)	(.04)		(.08)	
Mother's Education	10.5	3.08	9.5	4.07
(percent missing)	(.13)		(.10)	
Black	0.34		0.38	
Number of Siblings at Start of Survey	2.96	3.23	2.87	3.23
(percent missing)	(.30)		(.30)	
Panel Mean Log Wage	2.03	0.46	1.66	0.55
Panel Mean Age	31.0	8.83	31.0	8.72
Panel Mean Experience	8.80	8.88	5.14	6.40

TABLE 1: The Effects of Parental Education, IQ, and School Quality on Wage Levels and the Education Slope
Results for Men from NLS with Fixed Effects

Dependent Variable: Log Wage (1982-84 $)[1]

Explanatory Variables	1	2	3	4	5	6	7
Education[2]	3.73 (1.17)	2.99 (1.29)	2.64 (1.53)	4.50 (1.52)	4.98 (1.45)	4.95 (1.54)	4.62 (1.73)
Education x Experience	.101 (.029)	.102 (.029)	.102 (.029)	.103 (.029)	.102 (.029)	.105 (.029)	.105 (.029)
Education x IQ[3]			.052 (.059)				.049 (.060)
Education x DADED[4]				.508 (.202)		.157 (.222)	.175 (.223)
Education x MOMED					.807 (.284)	.826 (.328)	.781 (.330)
IQ		.593 (.144)	.484 (.176)	.628 (.143)	.585 (.143)	.604 (.144)	.504 (.176)
DADED							
MOMED							

NOTES:

1 The mean of the dependent variable is 2.23. All coefficients and standard errors have been multiplied by 100. There are 18092 observations contributed by 3994 men from 3653 families. There are 654 men with at least one brother in the data set. The sample is limited to men with at least four years of education. These equations also include the following control variables: year dummies, potential experience (number of years since last enrolled in school), its square and cube, education squared and cubed, indicators for residence in the South and in a SMSA. The typical regression had $R^2 = .76$. White standard errors are reported in parentheses.

2 Education is defined as highest grade completed minus 12, so it's coefficient is read as the additional wage accruing to a year of education past high school. We report the level coefficient based on a cubic specification.

3 IQ is defined as the individual's school-reported IQ score minus 100. Values were imputed for those with no reports.

4 DADED is the average report over all siblings in the family, rather than the individual young man's report, of father's education minus 12. MOMED is similarly constructed.

TABLE 2: The Effects of Parental Education, IQ, and School Quality on Wage Levels and the Education Slope
Results for Men from NLS without Fixed Effects

Dependent Variable: Log Wage (1982-84 $)[1]

Explanatory Variables	1	2	3	4	5	6	7
Education[2]	4.67 (.466)	3.29 (.485)	3.29 (.520)	3.49 (.504)	3.42 (.503)	3.51 (.511)	3.48 (.535)
Education x Experience	.220 (.037)	.221 (.037)	.221 (.037)	.220 (.037)	.221 (.037)	.221 (.037)	.221 (.037)
Education x IQ[3]			-.012 (.015)				-.017 (.015)
Education x DADED[4]				.101 (.039)		.074 (.045)	.079 (.045)
Education x MOMED					.084 (.043)	.053 (.049)	.056 (.049)
IQ		.367 (.039)	.391 (.046)	.365 (.039)	.365 (.039)	.365 (.039)	.395 (.046)
DADED	-.345 (.119)	-.356 (.118)	-.356 (.118)	-.453 (.129)	-.354 (.119)	-.419 (.131)	-.425 (.131)
MOMED	.641 (.128)	.521 (.128)	.521 (.128)	.516 (.128)	.442 (.134)	.464 (.136)	.460 (.136)

NOTES:

1 The mean of the dependent variable is 2.23. All coefficients and standard errors have been multiplied by 100. There are 18092 observations contributed by 3994 men from 3653 families. The sample is limited to men with at least four years of education. These equations also include the following control variables: year dummies, potential experience (number of years since last enrolled in school), its square and cube, education squared and cubed, indicators for residence in the South and in a SMSA, race, number of siblings, and whether two parents were in the household when the child was 14 years old. The typical regression had $R^2 = .31$. White standard errors are reported in parentheses.

2 Education is defined as highest grade completed minus 12, so it's coefficient is read as the additional wage accruing to a year of education past high school. We report the level coefficient based on a cubic specification.

3 IQ is defined as the individual's school-reported IQ score minus 100. Values were imputed for those with no reports.

4 DADED is the average report over all siblings in the family, rather than the individual young man's report, of father's education minus 12. MOMED is similarly constructed.

TABLE 3: The Effects of Parental Education, IQ, and School Quality on Wage Levels and the Education Slope
Results for Women from NLS with Fixed Effects

Dependent Variable: Log Wage (1982-84 \$)[1]

Explanatory Variables	1	2	3	4	5	6	7
Education[2]	6.33 (.995)	5.92 (1.02)	5.24 (1.19)	5.49 (1.07)	7.32 (1.08)	6.65 (1.11)	5.66 (1.16)
Education x Experience	.019 (.025)	.018 (.025)	.018 (.025)	.019 (.025)	.021 (.026)	.022 (.025)	.022 (.025)
Education x IQ[3]			.097 (.049)				.071 (.056)
Education x DADED[4]				-.041 (.173)		-.285 (.180)	-.296 (.177)
Education x MOMED					.581 (.215)	.765 (.223)	.685 (.254)
IQ		-.189 (.140)	-.414 (.156)	-.209 (.142)	-.229 (.140)	-.266 (.143)	-.444 (.157)
DADED							
MOMED							

NOTES:

1 The mean of the dependent variable is 1.79. All coefficients and standard errors have been multiplied by 100. There are 20859 observations contributed by 3924 women from 3579 families. There are 655 women with at least one sister in the data set. The sample is limited to women with at least four years of education. These equations also include the following control variables: year dummies, potential experience (number of years since last enrolled in school), its square and cube, education squared and cubed, indicators for residence in the South and in a SMSA. The typical regression had $R^2 = .70$. White standard errors are reported in parentheses.

2 Education is defined as highest grade completed minus 12, so it's coefficient is read as the additional wage accruing to a year of education past high school. We report the level coefficient based on a cubic specification.

3 IQ is defined as the individual's school-reported IQ score minus 100. Values were imputed for those with no reports.

4 DADED is the average report over all siblings in the family, rather than the individual young man's report, of father's education minus 12. MOMED is similarly constructed.

TABLE 4: The Effects of Parental Education, IQ, and School Quality on Wage Levels and the Education Slope
Results for Women from NLS without Fixed Effects

Dependent Variable: Log Wage (1982-84 $)[1]

Explanatory Variables	1	2	3	4	5	6	7
Education[2]	7.40 (.414)	5.75 (.420)	5.58 (.453)	5.55 (.429)	5.68 (.436)	5.53 (.436)	5.53 (.458)
Education x Experience	-.035 (.026)	-.040 (.026)	-.037 (.026)	-.041 (.026)	-.042 (.026)	-.042 (.026)	-.039 (.026)
Education x IQ[3]			-.042 (.012)				-.046 (.013)
Education x DADED[4]				.008 (.036)		.012 (.041)	.039 (.041)
Education x MOMED					-.010 (.038)	-.010 (.043)	.021 (.044)
IQ		.499 (.039)	.539 (.041)	.496 (.039)	.499 (.039)	.496 (.039)	.540 (.041)
DADED	.354 (.106)	.096 (.106)	.104 (.106)	.141 (.111)	.110 (.107)	.139 (.111)	.106 (.111)
MOMED	.523 (.119)	.343 (.118)	.353 (.118)	.330 (.118)	.352 (.119)	.339 (.119)	.327 (.119)

NOTES:

1 The mean of the dependent variable is 1.79. All coefficients and standard errors have been multiplied by 100. There are 20859 observations contributed by 3924 women from 3579 families. The sample is limited to women with at least four years of education. These equations also include the following control variables: year dummies, potential experience (number of years since last enrolled in school), its square and cube, education squared and cubed, indicators for residence in the South and in a SMSA, race, number of siblings, and whether two parents were in the household when the child was 14 years old. The typical regression had $R^2 = .26$. White standard errors are reported in parentheses.

2 Education is defined as highest grade completed minus 12, so it's coefficient is read as the additional wage accruing to a year of education past high school. We report the level coefficient based on a cubic specification.

3 IQ is defined as the individual's school-reported IQ score minus 100. Values were imputed for those with no reports.

4 DADED is the average report over all siblings in the family, rather than the individual young man's report, of father's education minus 12. MOMED is similarly constructed.

Table 5: The Effects of Parental Education on Wages and Education Slopes
Pooled Results for Men and Women from NLS

Explanatory Variables	**With** Fixed Effects[1]			**Without** Fixed Effects[2]		
	(1)	(2)	(3)	(4)	(5)	(6)
Education * DADED	.029 (.091)		.078 (.083)	.051 (.027)		.040 (.030)
Education * MOMED		-.079 (.103)	-.088 (.096)		.035 (.029)	.021 (.032)
DADED				-.364 (.123)	-.338 (.119)	-.349 (.124)
DADED * Female				.445 (.159)	.437 (.159)	.439 (.159)
MOMED				.509 (.128)	.491 (.131)	.490 (.132)
MOMED * Female				-.179 (.175)	-.170 (.175)	-.172 (.175)

NOTES:

1 There are 38951 observations contributed by 7923 individuals from 6476 families. There are 2633 individuals with at least one sibling in the data set.

2 All equations include year dummies, experience, experience squared, experience cubed, education, education * experience, education squared, education cubed, controls for race, residence in South and in an SMSA, whether two parents in household at age 14, number of siblings, IQ, IQ squared, IQ cubed, missing indicators for all these and female interactions with all of these. The typical regression without fixed effects had R^2 = .42. Equations with fixed effects do not include race, number of siblings, or whether two parents were in the household. The R^2 for these equations is about .75. White standard errors are reported in parentheses.

Table 5a: The Effects of Parental Education on Wages and Education Slopes
Pooled Results for Men and Women from PSID

Explanatory Variables	**With** Fixed Effects[1]			**Without** Fixed Effects[2]		
	(1)	(2)	(3)	(4)	(5)	(6)
Education * DADED	.004 (.074)		-.117 (.095)	-.066 (.017)		-.047 (.021)
Education * MOMED		.130 (.094)	.222 (.121)		-.067 (.019)	-.037 (.023)
DADED				1.06 (.084)	1.01 (.083)	1.05 (.084)
DADED * Female				-.490 (.118)	-.478 (.118)	-.487 (.118)
MOMED				.253 (.091)	.290 (.091)	.275 (.091)
MOMED * Female				-.145 (.126)	-.148 (.126)	-.146 (.126)

NOTES:

1 There are 111,438 observations contributed by 14,292 persons from 11,794 families. There are 3658 individuals who have at least one sibling in the data set.

2 All equations include year dummies, experience, experience squared, experience cubed, education, education * experience, education squared, education cubed, controls for race, residence in South and in an SMSA, number of siblings, missing indicators for all these and female interactions with all of these. The typical regression without fixed effects had $R^2 = .35$. Equations with fixed effects do not include race or number of siblings. The R^2 for these equations is about .67. White standard errors are reported in parentheses.

Table 6: The Effects of Parental Education on Wages and Education Slopes

Results for Men from the PSID

Dependent Variable: Log Average Hourly Earnings (1982-4$)[1]

Explanatory Variables	With Family Fixed Effects[2]				Without Family Fixed Effects[3]				
	1	2	3	4	5	6	7	8	9
Education[4] (based on a cubic spec)	4.58	4.84	5.19	4.94	6.18	5.51	5.29	5.48	5.31
	(.513)	(.616)	(.562)	(.616)	(.190)	(.194)	(.206)	(.199)	(.206)
Education x Experience	.140	.141	.141	.141	.050	.060	.057	.059	.058
	(.014)	(.014)	(.014)	(.014)	(.008)	(.008)	(.009)	(.009)	(.009)
Education x DADED[5]		.094		-.184			-.078		-.092
		(.122)		(.177)			(.024)		(.027)
Education x MOMED			.328	.480				-.019	.032
			(.146)	(.211)				(.026)	(.030)
DADED						1.02	1.10	1.02	1.11
						(.083)	(.085)	(.083)	(.085)
MOMED						.257	.260	.268	.241
						(.091)	(.091)	(.091)	(.091)

[1] The mean of the dependent variable is 2.23. All coefficients and standard errors have been multiplied by 100. There are 61,427 observations contributed by 7272 men from 6434 families. There are 1384 men who have brothers in the data set. The sample is limited to young men with at least four years of education. White standard errors are reported in parentheses.

[2] All fixed effects equations also include year dummies, experience and its square, age, its square and cube, and yearly indicators for residence in the South and residence in an SMSA. The typical regression had an Rsquared= .68.

[3] These equations include the controls in the fixed effects equations plus number of siblings and an indicator for black. The typical regression had an Rsquared= .28.

[4] Education is defined as highest grade completed minus 12, so its coefficient is read as the additional wage accruing to a year of education past high school.

[5] In the fixed effects equations, DADED (MOMED) is the *average report over all siblings* in the family-- rather than the individual man's report-- of the father's (mother's) education minus 12. In the equations without fixed effects, the individual's reports of DADED and MOMED are used.

Table 7: The Effects of Parental Education on Wages and Education Slopes

Results for Women from the PSID

Dependent Variable: Log Average Hourly Earnings (1982-4$)[1]

Explanatory Variables	With Family Fixed Effects[2]				Without Family Fixed Effects[3]				
	1	2	3	4	5	6	7	8	9
Education[4] (based on a cubic spec)	4.21 (.502)	3.97 (.576)	3.96 (.548)	3.94 (.579)	7.17 (.184)	6.84 (.188)	6.63 (.209)	6.46 (.206)	6.51 (.212)
Education x Experience	.107 (.022)	.108 (.022)	.108 (.022)	.108 (.022)	.057 (.011)	.060 (.011)	.058 (.011)	.054 (.011)	.054 (.011)
Education x DADED[5]		-.105 (.128)		-.026 (.164)			-.050 (.025)		.037 (.032)
Education x MOMED			-.169 (.178)	-.147 (.234)				-.119 (.027)	-.146 (.035)
DADED						.478 (.083)	.525 (.085)	.510 (.084)	.484 (.085)
MOMED						.079 (.088)	.084 (.088)	.154 (.089)	.167 (.089)

[1] The mean of the dependent variable is 1.82. All coefficients and standard errors have been multiplied by 100. There are 50,011 observations contributed by 7020 women from 6157 families. The sample is limited to young women with at least four years of education. White standard errors are reported in parentheses. There are 1415 women who have sisters in the data set.

[2] All fixed effects equations also include year dummies, experience and its square, age, its square and cube, and yearly indicators for residence in the South and residence in an SMSA. The typical regression had an Rsquared= .62.

[3] These equations include the controls in the fixed effects equations plus number of siblings and race. The typical regression had an Rsquared= .26.

[4] Education is defined as highest grade completed minus 12, so its coefficient is read as the additional wage accruing to a year of education past high school.

[5] In the fixed effects equations, DADED (MOMED) is the *average report over all siblings* in the family-- rather than the individual woman's report-- of the father's (mother's) education minus 12. In the equations without fixed effects, the individual's reports of DADED and MOMED are used.

Table 8: The Effects of School Characteristics on Wage Levels and Education Slopes

Pooled Results for Men and Women from NLS

Dependent Variable: Log Wage (1982-84$)

	ST_RATIO		T_SALARY		EXPEND		S_QUALITY	
	1a	1b	2a	2b	3a	3b	4a	4b
				No IQ				
interaction	-.068	.008	-.025	-.007	.011	-.023	-.867	-.214
	(.089)	(.019)	(.006)	(.002)	(.025)	(.007)	(.197)	(.055)
level	.321	.146	.156	.034	.247	.101	1.57	.168
	(.411)	(.043)	(.036)	(.004)	(.123)	(.018)	(.878)	(.133)
				IQ Level & Interaction				
interaction	-.058	.010	-.025	-.006	.015	-.022	-.880	-.205
	(.090)	(.019)	(.006)	(.002)	(.025)	(.007)	(.199)	(.055)
level	.289	.172	.158	.032	.228	.086	1.33	.069
	(.417)	(.043)	(.036)	(.005)	(.123)	(.018)	(.879)	(.132)
				Instrumental Variables Approach				
interaction	.150		.005		.061		.320	
	(.099)		(.003)		(.034)		(.354)	
level	-.906		.173		.121		.501	
	(.936)		(.096)		(.278)		(2.35)	
# observations	29010		28159		25490		26687	
# families	4979		4807		4362		4572	
# persons	5833		5653		5127		5358	
# siblings	1579		1568		1418		1456	
# with dev ≠ 0	382		303		252		278	
Mean of School Variable	20.6		6018		578		5.9	
Interq.Range	5.1		681		228		3	
Dev. Range	33.2		3022		1039		7.4	

NOTES:

1. The school characteristics are described in Appendix 1A. Observations were only included if a valid report of the particluar school characteristic was provided. White standard errors are reported in parentheses in the top two panels.

2. The "a" columns include fixed effects. The "b" columns do not. All regressions include the same control variables reported in Tables 1 - 4 plus the interactions of child's and parents' educations. All coefficients, except those on the school characteristics level and interaction, are allowed to differ by gender. The coefficients and standard errors in the T_SALARY and EXPEND columns were multiplied by 1000 and the ST_RATIO and S_QUALITY coefficients and standard errors by 100.

3. At the base of each set of columns, we report the number of observations in the regressions, the number of persons contributing these observations, the number with siblings, and the number of those whose school variable report differs from the family mean. (These individuals identify the effect of the school variable in the fixed effects regressions.) The mean and interquartile range of the school variable and the range of the non-zero within family deviations are also reported.

4. IV technique: 2SLS was applied in deviation from the mean form using within family differences in "residence at age 14" (to capture changes in location) to instrument for differences in school characteristics among siblings. Standard errors were corrected for degrees of freedom, but not for correlation across observations from the same family.

Table 9: Univariate Regressions of School Characteristics on Individual's Characteristics

Dependent Variable: Deviation of Individual's School Variable from Family Mean of the School Variable

Coefficient
(t-statistic)

	School Characteristic			
Individual's Characteristic	ST_RATIO	T_SALARY	EXPEND	S_QUALITY
	(1)	(2)	(3)	(4)
Education	0.857 (1.82)	4.50 (0.99)	-2.04 (-1.11)	.006 (0.34)
IQ Score	-.020 (-0.25)	-1.05 (-1.47)	-.032 (-0.09)	.001 (0.40)
Two parents in hh @ age 14?	1.59 (0.32)	32.76 (0.68)	-29.16 (-1.56)	.009 (0.05)
Days absent	-.039 (-.054)	1.22 (1.83)	.042 (0.15)	.000 (0.07)
Vocational curriculum?	-8.02 (-2.54)	50.50 (1.64)	10.92 (0.88)	.357 (2.86)
Good attitude toward hs?	-3.83 (-1.48)	12.22 (0.41)	-6.51 (-0.62)	.277 (2.29)

NOTES:

Independent variables are measured as deviations of the individual's characteristic from the family mean. Separate regressions, which controlled for family fixed effects, were run for each of the independent variables.

Table 10: Comparison of Mean Characteristics of Siblings
Who Went to the Same School and
Siblings Who Went to Different Schools

Characteristics	Same School		Different Schools		p-value H0: means are equal
Level	Mean	SD	Mean	SD	
Education, S_{hk}	13.8	(2.3)	14.2	(2.4)	0.03
IQ Score	102.6	(15.2)	106.4	(15.3)	0.0
Black?	0.22	(0.41)	0.23	(0.42)	0.72
Female?	0.49	(0.5)	0.51	(0.5)	0.65
Number of Siblings	3.5	(2.3)	3.6	(2.3)	0.49
Two parents in HH @ age 14?	0.88	(0.32)	0.82	(0.39)	0.03
DADED	10.6	(3.5)	11.5	(3.3)	0.0
MOMED	11	(2.8)	11.3	(2.7)	0.07
ST_RATIO	202	(66)	192	(60)	0.05
T_SALARY	6038	(541)	6006	(640)	0.51
EXPEND	581	(159)	584	(216)	0.84
S_QUALITY	5.9	(2.1)	5.9	(2)	0.89
Conditions made it hard to study?	0.15	(0.36)	0.14	(0.35)	0.65
Good attitude towards HS?	0.93	(0.26)	0.92	(0.28)	0.65
Work during HS?	0.47	(0.5)	0.52	(0.5)	0.3
Days absent last yr HS	10.4	(13.5)	11.6	(14.8)	0.32
Expect help from parents?	0.6	(0.49)	0.63	(0.48)	0.45
Vocational curriculum?	0.06	(0.25)	0.07	(0.26)	0.76
Move between HS & 1966?	0.09	(0.28)	0.17	(0.38)	0.0

Note:
1. Results in this table are based on families with two siblings. (These families account for 78% of the individuals with siblings in the data set.)
2. There are 1036 pairs of siblings who attended the same school and 202 pairs who attended different schools. Because of missing reports on particular variables, the number of observations entering the mean calculations will be lower than these for most variables.