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OF THE RETURN TO SCHOOLING
BIASED DOWNWARD?
ANOTHER LOOK

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ABSTRACT

We examine evidence on omitted-ability bias in estimates of the economic return to schooling, using proxies for unobserved ability. We consider measurement error in these ability proxies and the potential endogeneity of both experience and schooling, and examine wages at labor market entry and later. Including ability proxies reduces the estimate of the return to schooling, and instrumenting for these proxies reduces the estimated return still further. Instrumenting for schooling leads to considerably higher estimates of the return to schooling, although only for wages at labor market entry. This estimated return generally reverts to being near (although still above) the OLS estimate if we allow experience to be endogenous. In contrast, for observations at least a few years after labor market entry, the evidence indicates that OLS estimates of the return to schooling that ignore omitted ability are, if anything, biased upward rather than downward.

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I. Introduction

One of the longest-running debates in empirical labor economics regards omitted-ability bias in estimates of the economic return to schooling. The concern is that ability is positively associated with both wages and schooling. Thus, if the return to schooling is estimated with no account taken of the role of ability, the estimate is biased upwards. This concern has led to three general classes of approaches intended to remove this bias.

One, exemplified in Griliches and Mason (1972), includes explicit measures of ability, while recognizing that these measures are at best proxies for unobserved ability. Including such measures in cross-sectional regressions tends to reduce the estimated return to schooling, especially when measurement error in them is taken into account. A second, exemplified in Behrman, *et al.* (1980), attempts to eliminate omitted-ability bias by estimating the return to schooling from differences across twins in levels of schooling and wages (on the assumption that much of the unobserved ability is common across twins and is therefore differenced out). Roughly speaking, this approach has tended to lead to larger reductions in the estimated return to schooling than the first approach. A related literature (*e.g.*, Griliches, 1979) uses differences across siblings. A third, newer approach, exemplified in the papers by Angrist and Krueger (1991, 1992), exploits natural variation in factors affecting schooling decisions (such as the interaction between quarter of birth and compulsory schooling laws) to create instruments for schooling that are uncorrelated with ability. This approach tends to find no omitted-ability bias in the estimated return to schooling, or, if anything, a *downward* rather than an upward bias.[1]

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[1]Angrist and Newey (1991) use fixed effects estimation with panel data to

The differences between the results from twin studies and these "natural experiments" have prompted a reexamination of evidence from twins by Ashenfelter and Krueger (1992). There are two components to this reexamination. First, Ashenfelter and Krueger (hereafter AK) constructed a new sample of twins by collecting data directly at a Twins Day Festival. Second, and more important, AK collected data in a form that lets them address a potential problem with twin (or sibling) studies that was originally raised by Griliches (1979): in forming differences across twins, bias from measurement error in schooling may increase, as the signal-to-noise ratio in the differenced schooling variable falls relative to that in schooling levels. Griliches argued that this measurement-error problem can explain the fact that twin and sibling studies tend to find stronger evidence of upward bias in schooling return estimates than studies using proxies for ability.

The empirical analysis in AK suggests that the measurement-error problem is important. OLS estimates of a differenced equation lead to an estimate of the return to schooling of .09, whereas IV estimates of the differenced equation, using the twins' reports of their siblings' schooling levels as instruments, lead to an estimated return of .17.[2] Thus, these results suggest that the OLS return to schooling is in fact biased downward, rather than upward.[3] AK's final estimate of the return to schooling is striking, nearly

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remove the effects of fixed ability components in the wage error, and also find that the schooling coefficient estimate is biased downward.

[2]Although of less importance, AK also report results in which there is downward (rather than upward) omitted-ability bias in the schooling coefficient in a wage-level equation. However, the opposite result holds for a wage regression including standard control variables (union status, marital status, and tenure).

[3]Behrman, Rosenzweig, and Taubman (1992) find qualitatively similar results, using data on reports of parents' schooling from the survey of offspring of the NAS-NRC twins sample. However, the return to schooling increases by less

double most existing estimates.

The purpose of the present paper is to consider the same sources of bias studied by AK using the first approach outlined above. We estimate wage equations using cross-sectional data including proxies for unobserved ability, and variables to use as instruments both for these proxies and for schooling. In our view, a study such as this one is useful in assessing the strength of AK's findings which are, by all accounts, surprising.[4] In addition, this study may provide additional information, because our method allows us to remove potential endogeneity bias in the schooling coefficient estimate, a problem that AK's estimation design does not take into account.

We are not the first to consider omitted-ability bias and measurement error in schooling (as well as ability proxies) jointly in a cross-sectional data set. Griliches (1977) instruments for schooling while also dealing with unobserved ability using test scores, and interprets the evidence as suggesting that schooling is negatively correlated with the wage equation residual. He points out that this may be because of measurement error, or because of endogeneity such that those with high wage draws are induced to leave school earlier. While some of the procedures we follow parallel Griliches (1977), we do a number of things differently, in addition to using a more recent data set. First, we treat the problem of unobserved ability and error-ridden proxies using a more consistent set of test scores--the scores from the Armed Services Vocational Aptitude Battery--than the amalgamation

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upon instrumenting, from .035 to .05. Both of these estimates are lower than the OLS estimate of .053, for a specification including age and its square.

[4]There is also clearly a role for studies that attempt to replicate their results on similar twin or sibling data sets. However, we know of no such data sets. For example, in none of the NLS surveys are respondents in multiple-respondent households asked about the schooling of brothers or sisters.

of IQ test scores (and a short test of the "knowledge of the world of work") available to Griliches. These test scores also allow for the potential control of separate academic and nonacademic ability components. Second, we analyze data on the individual's first wage after leaving school, as well as a later observation, to provide information on whether any bias in the schooling coefficient that we find is more likely to arise from measurement error or endogeneity. Because the first wage (residual) should be more correlated with schooling because of endogeneity, if only the return to schooling for the early wage is affected by instrumenting for schooling, we would believe that endogeneity is the source of the bias, rather than measurement error.

In addition, we treat experience as potentially endogenous. As pointed out by Griliches (1977)--but ignored in much subsequent research on the return to schooling--if there is endogeneity bias in the estimate of the return to schooling because high wage draws induce individuals to leave school earlier, then this source of bias should also affect estimates of the return to experience (see Griliches, 1977). This suggests that instrumenting for schooling while treating experience as exogenous results in a misspecification that could bias IV estimates of the return to schooling.[5]

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[5]AK use age instead of actual or potential experience. Since age should be uncorrelated with the wage equation error, this suggests that their IV estimates of the return to schooling should not be biased by the endogeneity of experience. However, if actual experience is correlated with schooling (holding constant age), their schooling coefficient estimate will partly reflect experience effects, a bias that their IV estimation would not be expected to remove (nor to exacerbate).

It is also true that using age instead of experience can lead to the schooling coefficient reflecting both schooling and experience effects, if the specification using potential experience is the correct specification. For example, if the "true" model has only a linear term in potential experience, an estimated model that only includes linear age would provide a schooling coefficient estimate that is actually an estimate of the return to schooling minus the return to experience.

II. Theoretical Framework

In this section we briefly highlight the alternative sources of bias in estimates of the return to schooling suggested by the human capital model, abstracting from the role of labor market experience. In the basic human-capital model of the relationship between schooling and earnings (Mincer, 1974, Ch. 1), it is assumed that all workers have identical opportunities for human-capital investment, and that the rewards that they receive from those investments are the same for all workers. In contrast, Becker's (1975) model allows ability to affect the rate of return to human capital investment, with the assumption that workers with higher levels of innate ability also receive a higher return to their human capital. Following Becker, we assume that for each individual there are functions representing the marginal benefit and marginal cost of education that take the form:

$$MB(A_i, S_i) = \exp(kA_i)S_i^b$$

$$MC(P_i, S_i) = \exp(-P_i)S_i^d \quad ,$$

where S_i is years of schooling, A_i is ability, P_i is the individual's opportunities for investing in human capital, and b and d are parameters. It is assumed that d>0 and b<d. The optimal level of investment in schooling is equal to:

$$S_i^* = \exp\left[(kA_i+P_i)/(d-b)\right] \quad .$$

so that workers with higher ability (or higher opportunity) will invest more in education. The wage is given by $W_i = \int_0^{S_i^*} MB(A_i, x_i)dx_i$, which leads to the log-linear wage equation:

$$w_i = \log(W_i) = -\log(1+b) + (1+b)\log(S_i^*) + kA_i \quad .$$

This form for the wage equation shows that if ability is held constant, then the empirical relationship between w_i and S_i^* reflects the (logarithmic) slope of the marginal benefit function for schooling.[6] However, if ability is omitted, the correlation between S_i^* and A_i will lead to bias in the estimated return to schooling.

The general presumption that omitted ability should lead to an upward-biased estimate of the marginal benefit of schooling does not automatically follow when ability and opportunity are negatively correlated. The covariance of log schooling and ability is:

$$\sigma_{\log(S^*),A} = \frac{1}{d-b}\left[k\sigma_A^2 + \sigma_{P,A}\right] \quad ,$$

where $\sigma_{P,A}$ is the covariance of ability and opportunity. If ability and opportunity are uncorrelated, the schooling/ability covariance is clearly positive; however, if A and P are negatively correlated, the sign of the ability/schooling covariance is less clear. Since opportunity represents anything that shifts the marginal cost curve for schooling, and since higher-ability individuals are likely to face higher foregone-earnings costs at the margin, P and A may be negatively correlated. To consider a particular example, if we allow wages to be a direct determinant of the marginal cost of

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[6] It is not possible, in this framework, to derive a marginal benefit function for schooling that leads to the commonly-used semi-log specification for the wage equation. To see this, note that the expression for W_i would have to be:

$$W_i = \exp(f + gS_i^* + hA_i) \quad ,$$

which implies the marginal benefit function:

$$MB = dW_i/dS_i^* = g\cdot\exp(f + gS_i^* + hA_i) \quad .$$

But this marginal benefit function, when integrated from 0 to S_i^*, does not yield the required form for W_i. Nonetheless, we follow common practice and enter schooling linearly in the log wage equations that we estimate. However, the qualitative nature of the conclusions we draw from our empirical section are unchanged if we use log schooling in place of schooling in our wage equations.

education, we have:

$$MC = \exp(C_i)W_i = \exp(C_i)\int_0^{S_i} MB(A_i,x_i)dx_i$$
$$= \frac{1}{(1+b)} \exp(kA_i+C_i)S_i^{(1+b)} \quad ,$$

where C represents anything (besides the current wage) that shifts marginal costs. In terms of the earlier expression for marginal cost, we have $d=(1+b)$ and $P_i=-kA_i-C_i+\log(1+b)$. With this restriction, $\sigma_{\log(S^*),A} = -\sigma_{C,A}$, so the sign is indeterminate. If more able individuals have lower costs of education (exclusive of opportunity-wage costs), then the ability/schooling covariance will be positive, and the OLS schooling coefficient estimate will still be an upward-biased estimate of the marginal benefit of education. On the other hand, it also theoretically possible that omitted ability could bias downward the schooling coefficient estimate.[7]

In this framework we can also introduce the endogeneity problem considered by Griliches. Let W_i now be given by

$$W_i = \exp(\epsilon_{it})\cdot\int_0^{S_i^*} MB(A_i,x_i)dx_i \quad ,$$

where ϵ_{it} parallels the error term in the wage equation. It is a component of the wage that affects the marginal cost of schooling, without affecting the marginal benefit.[8] Given the expression for MC defined above, the optimal level of schooling is

$$S_i^* = (1+b)/\exp(C_i+\epsilon_{it}) \quad ,$$

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[7] This discussion suggests that AK's finding of a larger estimate from the differenced data than from the levels (in one specification) is not necessarily anomalous, from the perspective of the human capital model.

[8] Such a component could be related to, for example, physical strength that affects earnings, without affecting the benefit of schooling, or a "lucky" wage offer that is unlikely to be available later if an individual remains in school.

and the log wage equation is

$$w_i = \log(W_i) = -\log(1+b) + (1+b)\log(S_i^*) + kA_i + \epsilon_{it} \quad .$$

Clearly, schooling is negatively correlated with the wage equation residual.[9] This causes a downward bias in the coefficient estimate for schooling (as would classical measurement error in schooling), though the size of this bias compared to any upward omitted-ability bias is theoretically indeterminate.

III. Empirical Estimation

A. Data

Our theoretical discussion stressed the importance of including ability controls in estimating wage differences across schooling groups. We use data from the Youth Cohort of the National Longitudinal Survey, which allows us to use indicators of ability that are not generally available in labor market surveys. These data also allow us to consider the potential endogeneity of schooling in wage-equation estimates for workers at the start of their working careers, when this relationship is likely to be most relevant.

The National Longitudinal Survey Youth Cohort consists of a nationally representative sample of men and women between the ages of 14 and 22 in 1979, and an oversample of hispanics, blacks, and economically-disadvantaged youths in that same age group. The initial survey was performed in 1979, and reinterviews have been conducted in each subsequent year. For our estimation, we restrict our sample to white males who were still responding to the survey in 1985.[10] The NLS provides information on pay status at the job held at the

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[9] Griliches (1977) obtains this negative correlation in a different framework.

[10] Attrition rates have been considerably lower in the Youth Cohort than in most other longitudinal surveys. For example, 93 percent of the initial sample of white males in 1979 were successfully interviewed in 1985.

time of each survey, from which it is possible to construct an hourly rate of pay at that job. We further restricted our sample to those workers for which we could find a post-schooling wage in an interview year before 1985, and who also had a valid wage observation in 1985. This post-schooling wage--which we call the "early wage" variable--was the wage from the earliest year in which: one, the individual's reported level of schooling did not change from the previous year; and, two, the individual's reported level of schooling never changed after that year.

The NLS also collects information on weeks worked since the previous interview, as well as weeks worked in the three years prior to the 1979 interview. From this information, it is possible to construct a measure of actual experience in the labor market. Another advantage of the NLS data is that several measures of the family-background environment of each individual are available; these variables play a major role in our analysis.

The indicators of ability that we use are a set of test scores on each of the ten components of the Armed Services Vocational Aptitude Battery (ASVAB) tests. These components are: paragraph comprehension; general science; arithmetic reasoning; mathematics knowledge; word knowledge; paragraph comprehension; mechanical comprehension; numerical operations; electronic information; auto and shop information; and coding speed. These tests are regularly administered to assist in placement of U.S. military recruits, and a subset of these test scores (known as the Armed Forces Qualifications Test) is used as a primary criterion of enlistment eligibility (see NLS Users' Guide, 1992). These tests were administered to the NLS Youth Cohort sample between the 1979 and 1980 interviews, with a completion rate of 94 percent.[11]

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[11] We omit individuals who did not take the ASVAB tests.

Since sample respondents were of different ages when the test was taken, we removed age effects from the test scores by regressing each test score on a set of seven age dummies, and using the individual's standardized residual as their test-score measure for each test.[12]

B. Basic Wage Equations

We first report estimates of conventional log-wage equations using the NLS Youth Cohort data. We estimate early-wage and 1985 wage equations of the form (omitting individual subscripts):

$$w_E = \alpha_E S + \beta_E \mathbf{X}_E + \epsilon_{w_E}$$

$$w_{85} = \alpha_{85} S + \beta_{85} \mathbf{X}_{85} + \epsilon_{w_{85}}$$

where w is the log of the wage and S is years of schooling. The vector **X** includes controls for experience, union coverage, marital status, residential location (South or SMSA), the local unemployment rate, a dummy for working night shifts, ten industry dummies, eight occupation dummies, and five year dummies (in the early-wage equation).[13] The values of these variables may differ in the two years, though the value of schooling--by definition of the early-wage variable --will be the same in the two equations for any individual. Means for the variables in the two years are presented in the first two columns of Table 1.

OLS estimates of the two equations are reported in the middle two columns

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[12] We might also be concerned with the effects of education at the time of the test. However, regressing test scores on education at the time of the test is inappropriate, since education may be endogenous in this relationship. We might instead instrument for education with the age dummies (as in Farber and Gibbons, 1990), but this would require the assumption that age does not affect the test scores independently of education.

[13] We include experience squared as a regressor in the 1985 wage equation, but not in the early-wage equation. OLS estimations including experience squared in the early wage equation provided small and statistically insignificant estimates for its coefficient.

Table 1: Wage Equation Estimates without Controls for Ability[1]

Indep. Var.	Means (S.D.)[2] Early Year	1985	OLS *Actual Experience* Early Wage	1985 Wage	OLS *Age Controls* Early Wage	1985 Wage
Years of Schooling	12.2 (2.2)	12.2 (2.2)	.052 (.006)	.056 (.006)	.032 (.007)	.056 (.007)
Experience	0.3 (0.6)	5.3 (1.9)	.059 (.020)	.135 (.026)	--	--
Experience Squared	--	--	--	-.007 (.002)	--	--
Age	20.8 (2.2)	23.7 (2.2)	--	--	.045 (.008)	.219 (.117)
Age Squared	--	--	--	--	--	-.004 (.002)
Union Coverage	0.18	0.18	.243 (.030)	.231 (.031)	.244 (.029)	.250 (.032)
Married	0.14	0.39	.121 (.032)	.103 (.026)	.095 (.032)	.126 (.027)
Divorced or Separated	0.01	0.07	.158 (.099)	.071 (.048)	.090 (.099)	.066 (.049)
SMSA	0.71	0.74	.086 (.026)	.129 (.029)	.085 (.026)	.141 (.030)
South	0.33	0.33	-.055 (.025)	-.039 (.026)	-.049 (.024)	-.030 (.026)
Night Shift	0.06	0.05	.047 (.048)	.147 (.056)	.047 (.047)	.122 (.058)
Local Unem. Rate	3.3 (1.2)	3.4 (1.2)	-.020 (.012)	-.031 (.011)	-.017 (.012)	-.036 (.011)
Stand. Dev. of Industry Coeffs.	--	--	.179	.163	.179	.168
Stand. Dev. of Occup. Coeffs.	--	--	.126	.098	.123	.110
VAR(ϵ)			.175	.194	.172	.208

Table 1 (continued)

1. The regressions include a constant, ten industry dummies, and eight occupation dummies as independent variables. The early-wage equations also include five year dummies as independent variables.

2. The mean (standard deviation) of the early (log) wage variable is 1.50 (.49), and of the 1985 log wage is 1.89 (.54).

of Table 1. The schooling coefficient estimate is between 5 and 6 percent in both years. These estimates are slightly lower than what is generally found in estimations with Census data, but are similar to estimates obtained using the NLS Young Men's Cohort of 1966 (see Blackburn and Neumark, 1992). It may seem that the estimates contradict other research (using other data) that suggests the schooling return was significantly higher in 1985 than earlier in the 1980s (*e.g.*, see Katz and Murphy, 1991), but our particular regressions are not well-suited for studying this trend. The comparison of our early and 1985 regression estimates may reflect both an increasing underlying trend in the schooling coefficient, and a tendency for the importance of schooling to wages to decline as workers get older. In other estimations that use the early-wage variable only, we did find evidence that the schooling coefficient was increasing over this time period (see Blackburn and Neumark, forthcoming).

The experience variable used in the regressions reported in the middle two columns of Table 1 is actual experience, *i.e.*, cumulative weeks worked (divided by 52) at the time of the reported wage. Much of the recent research on schooling returns (AK, and the Angrist and Newey papers) does not use data with information on actual experience, but includes age controls instead. Therefore, we also estimated wage equations that omit the actual experience variables but include linear and quadratic terms in age as regressors. The final two columns of Table 1 present these estimates. The schooling return is lower in the early-wage regression when using age rather than actual experience, but is unchanged in the 1985 equation.

C. Estimates with Ability Controls

As mentioned in section II, schooling-return estimates are potentially biased if the estimation fails to control for kinds of ability that may raise the marginal benefit of schooling. We attempt to control for at least some variation in this "unobserved" ability by using the ASVAB test scores to proxy for ability. Initially, we estimate our two wage equations incorporating a time-invariant vector of ability variables (A):

$$w_E = \alpha_E S + \beta_E X_E + \tau_E A + \epsilon_{w_E}$$
$$w_{85} = \alpha_{85} S + \beta_{85} X_{85} + \tau_{85} A + \epsilon_{w_{85}} \qquad (1),$$

in which we set A=T, where T is the vector of test scores. This amounts to simply including the ten test scores as regressors and estimating each equation by OLS. These estimates--which use actual experience rather than age as a regressor--are reported in the first two columns of Table 2. The schooling coefficient estimate for 1985 is about 20 percent lower than the estimate without the ability controls; a smaller decline occurs in the early-wage schooling coefficient. The individual test score coefficients (not reported) tend to be very imprecisely estimated, which is not surprising given that the test scores are highly correlated.

A potential problem with simply adding the test scores as regressors is that they would seem to be, at best, only rough proxies for an individual's true ability. This could cause measurement-error bias in the wage equation estimates, especially if this measurement error follows the classical errors-in-variables setup. We follow earlier research in using family-background variables as potential determinants of ability in a model that allows the test scores to be error-ridden measures of ability (*e.g.*, see Griliches and Mason, 1972; Griliches, 1977). To (1), we add

$$T_k = \lambda A + \nu_k \quad , \; k = 1, \ldots, K$$
$$A = \vartheta_A F + \epsilon_A \qquad (2)$$

where F is a vector of family-background variables, ν_k and ϵ_A are error terms,

Table 2: Wage Equation Estimates with Controls for Ability

Indep. Var.	OLS[1] Early Wage	OLS[1] 1985 Wage	IV[2] Early Wage	IV[2] 1985 Wage	IV[3] Early Wage	IV[3] 1985 Wage
Years of Schooling	.047 (.007)	.046 (.007)	.014 (.010)	.043 (.009)	.023 (.012)	.049 (.012)
Experience	.059 (.020)	.121 (.026)	.087 (.021)	.124 (.026)	.074 (.023)	.125 (.026)
Experience Squared	--	-.005 (.002)	--	-.005 (.003)	--	-.006 (.003)
Union Coverage	.246 (.030)	.230 (.031)	.271 (.031)	.238 (.031)	.266 (.031)	.229 (.033)
Married	.120 (.032)	.107 (.026)	.135 (.033)	.109 (.026)	.125 (.034)	.104 (.027)
Divorced or Separated	.174 (.099)	.081 (.048)	.206 (.103)	.078 (.048)	.204 (.103)	.078 (.047)
SMSA	.091 (.027)	.134 (.029)	.094 (.027)	.131 (.029)	.101 (.028)	.138 (.030)
South	-.051 (.025)	-.032 (.026)	-.040 (.026)	-.036 (.026)	-.031 (.026)	-.031 (.026)
Night Shift	.050 (.048)	.143 (.056)	.053 (.050)	.141 (.056)	.044 (.050)	.144 (.056)
Local Unem. Rate	-.020 (.012)	-.032 (.011)	-.016 (.012)	-.030 (.011)	-.018 (.012)	-.032 (.011)
Ability[4]						
Test Average	--	--	.166 (.031)	.056 (.030)	--	--
Academic Test Average	--	--	--	--	-.029 (.093)	-.046 (.111)
Nonacademic Test Average	--	--	--	--	.177 (.074)	.094 (.096)
Hausman test P-value	--	--	.000	.642	.000	.854
Stand. Dev. of:						
Indus. Coeffs.	.175	.161	.161	.161	.163	.159
Occup. Coeffs.	.126	.093	.112	.092	.122	.095

Table 2 (continued)

VAR(ϵ)	.175	.195	.188	.196	.187	.195

1. These estimations include each of the ten test scores as independent variables, without correction for measurement error. Other independent variables are the same as in Table 1.

2. These estimations use the standardized average of nine test scores as the measure of ability. The test score variable is instrumented for using, as instruments, number of siblings, number of younger siblings, birth order percentile among siblings, mother's high grade, father's high grade, dummies for the presence of magazines or newspapers in the home while growing up, a dummy for living with both parents at age 14, a dummy for living with one parent and a step-parent at age 14, and dummies for missing data on siblings, birth order, mother's high grade and father's high grade (missing values for the corresponding variables for these observations are set to zero).

3. The standardized average of the nonacademic test scores, and the standardized average of the academic test scores, are included as separate regressors. The same set of instruments (see note 2) are used for both test scores.

4. The test score coefficients for the OLS equations are not reported here.

and K is the number of test scores.[14] A is now a scalar ability variable, assumed to be reflected in each of the test scores.[15] The test scores are considered to be error-ridden measures of the same true quantity, rather than measures of nine separate true quantities.

The average of the test scores for any individual can be written

$$\bar{T} = \frac{T'\iota}{n} = \left[\frac{\lambda'\iota}{n}\right] A + \frac{\nu'\iota}{n} = \left[\frac{\lambda'\iota}{n}\right] \vartheta_A F + \left[\frac{\lambda'\iota}{n}\right]\epsilon_A + \frac{\nu'\iota}{n} ,$$

where ι is a vector of ones, and T, λ, and ν are vectors containing T_k, λ_k, and ν_k. Assuming that the measurement errors in the test scores (ν) are uncorrelated with ability, we essentially have the classical errors-in-variables model when $\bar{T}$ is used as a regressor in the wage equation. The one modification is that we will be estimating τ_j divided by $\lambda'\iota/n$ rather than τ_j (in equation (1)), but since the scale for ability is not identified we can normalize $\lambda'\iota/n=1$.[16] Since $\bar{T}$ can also be expressed as a function of the observable vector F, we can use F as a set of instrumental variables for $\bar{T}$, assuming that F is uncorrelated with ν and the wage equation errors.[17]

Estimates of this model are reported in the two middle columns of Table 2. The schooling coefficient for the 1985 wage equation is still about 20

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[14]We assume that $V(\nu)=\Omega$, where $\nu = (\nu_1,\ldots,\nu_n)'$, is a full, symmetric matrix, so that the measurement errors can be correlated across test scores. However, we also assume that ν is independent of ϵ_{w_E}, $\epsilon_{w_{85}}$ and ϵ_A, and ϵ_{w_E} and $\epsilon_{w_{85}}$ are independent of ϵ_A.

[15]Following Bishop (1990), we drop the coding speed test score from the analysis.

[16]Note that we do not require that λ in each of the nine test score equations be the same. This implies that using the test average does not necessarily restrict each test score to be an equally reliable indicator of ability.

[17]We also include exogenous wage characteristics in the other wage equation in constructing our set of instrumental variables for $\bar{T}$.

percent below the original OLS estimate. However, the schooling coefficient in the early-wage equation is about 80 percent below the original estimate, and insignificantly different from zero. The coefficients on ability in the two wage equations are positive and significantly greater than zero.

It seems quite plausible that there is more than one kind of ability reflected in our test-score measures. In particular, some of the test scores may reflect communicative and reasoning ability, while others reflect knowledge relevant to performing well on particular jobs. To this end, we classify all of the test scores as either representing academic or nonacademic ability.[18] We then assume that A in equations (1) and (2) represents two ability variables for each individual, and that each individual's average test score among the academic (and nonacademic) tests are error-ridden measures of academic (and nonacademic) ability. Estimates of this model are reported in the final two columns of Table 2. For the most part, the wage-equation estimates are not affected by this extension, although the schooling coefficients are somewhat higher. The point estimates suggest that only nonacademic ability is awarded in the labor market, with the academic-ability coefficient estimates negative but insignificant. However, the ability coefficients in the wage equations are very imprecisely estimated, and the null hypothesis that the two abilities have the same coefficient in each of the two wage equations cannot be rejected at conventional levels.[19]

Using the NLS Young Men's Cohort of 1966, Blackburn and Neumark (1992)

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[18] The academic tests are assumed to be paragraph comprehension, word knowledge, mathematics, arithmetic, and general science; the nonacademic tests are mechanical knowledge, numerical operations, electronics, and auto and shop knowledge. This classification loosely follows that used by Bishop (1991).

[19] The F-statistic in the early-wage equation is 1.6 (p-value of .21); in the 1985 equation, it is .47 (p-value of .49).

considered the effect that including test-score measures had on industry and occupation coefficients in wage equations. In that paper, we found that including ability controls reduced the standard deviation of industry coefficients by 10 to 15 percent, and of occupation coefficients by 15 to 30 percent. Standard deviations of these coefficients are reported for the NLS Youth Cohort data in Tables 1 and 2. The impact on industry and occupation coefficients is somewhat smaller in the Youth Cohort data, which may be surprising given that the test score information available for the Youth is more complete than that available for the Young Men.

D. Estimates with Endogenous Schooling

The estimates in Table 2 are consistent under the assumption that schooling is not correlated with the wage equation errors. There are at least two reasons to expect this assumption to be invalid. First, human capital models of schooling attainment (as in Section II) suggest that, other things equal, individuals with low wage draws (at the time of the schooling decision) will be more likely to invest in an additional year of schooling than high-wage individuals. If there are persistent (but not fixed) individual-specific components to the wage equation error, the level of schooling is likely to be correlated with the errors in the post-schooling wage equations, more strongly so with that of the early-wage equation.[20] Second, classical measurement error in the schooling variable could cause measured schooling to be correlated with the wage equation error.[21]

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[20] For example, this will hold if the part of the wage equation error that is correlated with the schooling error is $\rho^{s}\epsilon_{w_E}$, $0 < \rho < 1$, where s is the number of years following the early observation.

[21] In contrast, Griliches and Mason (1972) assume that the measurement error in schooling represents quality only (*i.e.*, no misreporting), and that this

Endogenous schooling choices and measurement error in the schooling variable would both be expected to bias downward the schooling coefficient estimate. However, both problems can be addressed in a similar fashion. We add a reduced-form equation for years of schooling to our model, *i.e.*,

$$S = \vartheta_s F + \epsilon_s \quad . \qquad (3)$$

This suggests using the family background variables (and exogenous variables in the other wage equation) to instrument for the level of schooling.[22]

The first two columns of Table 3 present estimates instrumenting for schooling but excluding any ability controls. Compared to the middle columns of Table 1, instrumenting for schooling raises the estimated return to schooling considerably in the early-wage equation, but only slightly in the 1985 wage equation. The coefficient estimate for experience in the early-wage equation falls below zero, and is statistically insignificant.[23] The middle two columns of Table 3 report estimates obtained by adding the test average variables, and instrumenting for both schooling and the test average. Again, the schooling coefficient estimates fall slightly. Splitting the tests into academic and nonacademic tests (the final two columns of Table 3) again causes the schooling coefficients to increase slightly.

For the early-wage equation, all three specifications suggest that there

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measurement error is uncorrelated with years of schooling. Under these assumptions measurement error bias is only present when ability controls are included in the regression, since the estimated ability effects will partially reflect quality of schooling effects. In this framework, instrumenting for test scores is sufficient to correct for the bias.

[22]We are uncertain about a correct specification for a structural-form equation for schooling. Therefore, we do not use any systems estimators (such as three stage least squares). The consistency of our estimates, then, depends only on correctly specifying the two wage equations.

[23]The other coefficient estimates are not reported in Table 3; they were essentially unaffected by instrumenting for schooling.

Table 3: Wage Equation Estimates With Endogenous Schooling[1]

	IV^2		IV^3		IV^3	
Indep. Var.	Early Wage	1985 Wage	Early Wage	1985 Wage	Early Wage	1985 Wage
Years of Schooling	.124 (.012)	.062 (.010)	.116 (.025)	.040 (.017)	.148 (.029)	.048 (.021)
Experience	-.017 (.023)	.133 (.026)	-.010 (.030)	.124 (.026)	-.047 (.035)	.125 (.026)
Experience Squared	--	-.006 (.002)	--	-.005 (.003)	--	-.006 (.003)
Ability Test Average	--	--	.017 (.046)	.062 (.037)	--	--
Academic Test Average	--	--	--	--	-.242 (.106)	-.043 (.121)
Nonacademic Test Average	--	--	--	--	.190 (.077)	.093 (.098)
Hausman test P-values:						
Schooling	.000	.665	.001	.463	.001	.461
Ability	--	--	.560	.467	.028	.732
VAR(ϵ)	.191	.198	.189	.196	.204	.195

1. These estimations include the same independent variables in the wage equations are used in Table 2. The same family background variables used as instruments for the test score averages are also used as instruments for the schooling variable.

2. This estimation instruments for schooling, but excludes the test score variables as regressors in the wage equation.

3. These estimations instrument for both schooling and the test score(s).

is a sizable downward bias in the OLS estimate of the schooling coefficient ignoring omitted ability.[24] On the other hand, for the 1985 wage equation the evidence suggests some upward bias. The Hausman test results suggest that the schooling variable is correlated with the error term only in the early-wage equation. This provides some support for the idea that endogeneity bias in the schooling coefficient estimate is more important than measurement error bias, since the size of the measurement error bias should be similar in both years. Of course, the validity of these results depends on labor market experience being exogenous in the wage equation.

E. <u>Endogeneity of Experience</u>

The wage equation estimates in Table 3 may still be inconsistent if labor market experience is correlated with the wage equation errors. Since labor supply is expected to depend on wages, accumulated labor supply will depend on an individual's history of wages. As with schooling, a persistent component in the wage equation error can lead to current experience being correlated with the current wage equation error, since current wages would be correlated with past wages. As years of schooling and experience tend to be correlated, inconsistency in the experience coefficient estimate can carry over to the schooling coefficient estimate.

We first incorporate endogenous experience by adding an equation similar to the reduced-form schooling equation, *i.e.*,

$$E = \vartheta_E F + \epsilon_E \quad . \qquad (4)$$

This suggests using the family background variables (and the "other wage" controls) as instruments for the test average, schooling, and experience. To

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[24]A higher return to schooling for earlier labor market observations is consistent with signaling and learning models (see Farber and Gibbons, 1990).

avoid instrumenting for both experience and its square, we use linear experience only in these estimations.[25]

The estimates reported in specification (1) of Table 4 instrument for all three suspects. In the early-wage equation, instrumenting for experience increases the experience coefficient--the opposite of what we expected--and causes the schooling coefficient to fall. In fact, the schooling coefficient in the early-wage equation (.060) is now close to the schooling coefficient in the 1985 wage equation (.053), and both are close to the original OLS estimates (.052 and .056 for the early and 1985 observations, respectively).

The Hausman test statistics provide strong evidence of endogeneity only for experience in the 1985 wage equation. If we re-estimate the two wage equations instrumenting for experience only (and only in the 1985 equation), the results do not change drastically (see specification (2) in Table 4), although the schooling coefficient estimates are now slightly smaller than in Table 1, suggesting a slight upward bias in the schooling coefficient estimates ignoring these sources of bias.

The evidence for endogeneity of schooling, or of experience, in the early wage equation is weak when instrumenting for both (and the test average). One possible interpretation of the evidence is that the Hausman tests in Tables 3 and 4 suggest that neither experience nor the test average are endogenous in the early-wage equation, but that there is some evidence that schooling is endogenous in this equation. If so, the range of estimates reported in Table 3 for the early-wage equation may be more reasonable than the estimates in

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[25] OLS estimates of the 1985 wage equation with linear experience (excluding the test average) provide an experience coefficient (standard error) of .068 (.007). The education coefficient estimate is higher by .001 when the quadratic term for experience is dropped.

Table 4: Wage Equation Estimates With Endogenous Schooling and Experience[1]

Specification:[2]	(1)		(2)		(3)		(4)	
	Actual Experience				Age Controls			
Indep. Var.	Early Wage	1985 Wage	Early Wage	1985 Wage	Early Wage	1985 Wage	Early Wage	1985 Wage
Years of Schooling	.060 (.033)	.053 (.023)	.046 (.007)	.040 (.007)	.067 (.040)	-.036 (.019)	.019 (.008)	-.036 (.019)
Experience	.104 (.127)	.106 (.011)	.063 (.020)	.085 (.010)	--	--	--	--
Test Average	.068 (.048)	.050 (.046)	.025 (.013)	.061 (.014)	.061 (.055)	.231 (.041)	.044 (.014)	.231 (.041)
Age	--	--	--	--	.022 (.023)	.181 (.120)	.051 (.008)	.181 (.120)
Age Squared	--	--	--	--	--	-.002 (.003)	--	-.002 (.003)
VAR(ϵ)	.179	.202	.175	.198	.177	.217	.171	.217
Hausman test P-values:								
Schooling	.485	.743	--	--	.151	.000	--	.000
Experience	.695	.000	--	.000	--	--	--	--
Test Average	.174	.835	--	--	.454	.000	--	.000

1. These estimations include the same independent variables in the wage equations are used in Table 1. The average of the nine test scores is the only ability control. Instrumental variables are the same as in Table 2.

2. Specification (1) instruments for schooling, experience, and the test average. Specification (2) only instruments for experience in the 1985 wage equation. Specification (3) instruments for schooling and the test average. Specification (4) instruments for schooling and the test average in the 1985 wage equation only.

specification (2) of Table 4, which would imply that there is a downward bias in the OLS estimate of the return to schooling in this equation.[26]

The Hausman tests in specification (3) may have low power, since the predicted values of schooling, experience, and the test average may be highly correlated. An alternative method for handling the potential endogeneity of experience is to drop experience from the equation, and use age controls instead. Then it is only necessary to instrument for two variables, schooling and the test average.[27] These results are presented in specification (3) of Table 4. The surprise in these estimates is the negative coefficient for the schooling return in the 1985 wage equation. Instrumenting only when the Hausman tests suggest--for 1985--we also find that the early-wage schooling coefficient is lower than its initial OLS estimate. These results do suggest a larger upward bias in the schooling coefficient estimates than the results using experience; however, we find the specifications using actual experience more plausible and theoretically more appropriate.

F. Over-identification Tests and Robustness to Identifying Assumptions

The consistency of our estimates in Tables 3 and 4 relies upon the assumption that the excluded variables used as instruments for the test scores, schooling, and experience are uncorrelated with the wage equation error terms. Since our models are overidentified, it is possible to test this assumption for a subset of our set of instruments. Unfortunately, the validity of this test depends on our correctly selecting a complementary subset of instruments that we know can be excluded, and the proper selection

[26] An early-wage equation that includes the test average, but instruments only for schooling, has a schooling coefficient estimate (standard error) of .149 (.018).

[27] It is also more similar to the models estimated in the recent literature on biases in schooling coefficient estimates.

of this subset is not testable.

Chamberlain and Griliches (1977) argue that the family background variables that they use in their estimation are uncorrelated with the errors in their wage equations.[28] To test this assumption, they use a sample of brothers from the NLS Young Men's Cohort. Using the brother's IQ test score as an instrument for IQ, they do not reject the hypothesis that the family background variables should not be added to their (expected) income equation.[29]

We perform a similar analysis by matching brothers in our NLS Youth Cohort sample. Since the original NLS survey sampled all individuals between the ages of 14 and 22 in any given household, we are able to construct a number of such brother pairs.[30] Restricting ourselves to matches between brothers that are both in our sample used for Tables 1-4, we are able to find a valid brother match for 314 individuals. We then estimate wage equations that instrument for schooling, experience, and test average using the brother's schooling, test average, and two experience variables as instruments.[31] These equations also include, *as regressors*, all of the instruments used in our estimations in Tables 2-4. Table 5 reports p-values from F-tests that all coefficients are equal to zero, for five mutually exclusive and exhaustive subsets of our previous instrument list.

The results (see the "siblings" column under actual experience) do cast

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[28] These variables include: median income in father's occupation when respondent was 14; an index of the availability of newspapers, magazines, and library cards while growing up; number of siblings; and race.

[29] Griliches (1979) cites other evidence in favor of excluding family background variables from wage equations.

[30] For individuals with two brothers in the sample, we randomly choose one of the brothers for this match. For individuals with three brothers in the sample, we randomly construct two matched pairs.

[31] We also include the dummy variables for the brother's "early" year as instruments.

Table 5: Over-Identification Tests for Instrumental Variables

	Actual Experience[1]				Age Controls[2]			
Sample:	Siblings		Full		Siblings		Full	
Indep. Vars.[3]	Early Wage	1985 Wage	Early Wage	1985 Wage	Early Wage	1985 Wage	Early Wage	1985 Wage
P-value of Test for:								
"Other Wage" Controls[4]	.07	.07	.80	.97	.02	.04	.37	.43
Siblings/ Birth Order	.09	.26	.98	.44	.02	.32	.95	.21
Magazines/ Newspapers	.15	.08	.23	.61	.11	.08	.15	.69
Parents' Education	.14	.70	--	--	.15	.46	--	--
Family Status at Age 14	.61	.32	--	--	.64	.29	--	--

1. These specifications instrument for the test average, schooling, and experience. In the sibling sample, the sibling's values of those variables (and the set of early year dummies for the sibling) are the only instruments. In the full sample, parents' education and living arrangements at 14 are the only instruments.

2. These specifications instrument for the test average and schooling, using the sibling's variables as instruments in the sibling sample and parents' education and living arrangements in the full sample.

3. The sibling sample consists of all individuals for which there is a sibling also in the dataset. Individuals with more than one sibling are randomly paired with a sibling. The sample size is 314 (or 157 pairs).

4. The other wage controls are the exogenous regressors in the other wage equation for that individual (*e.g.*, the 1985 wage equation regressors when estimating the early wage equation).

some doubt on the validity of using the regressors from the other wage equation as instruments. They also cast doubt on the exclusion of the siblings and birth order variables, and of the magazines and newspapers dummies.[32] In contrast, the results do seem to support the exclusion of parents' education and family status at age 14. Similar results were found in models that excluded experience from the wage equations (using age instead) and instrumented for the test average and schooling only (see the "siblings" column under age controls).

Following the suggestion from the over-identification tests that only parents' education and family status should be used as instruments, we re-estimated our wage equations (using our full sample) treating schooling, experience, and the test average as potentially correlated with the wage equation error. These results are presented in specification (1) of Table 6. The point estimates are largely nonsensical--the schooling coefficient estimate is 21 percent for the early wage, but minus 8 percent for the 1985 wage--and are very imprecisely estimated. The Hausman tests do not provide strong evidence of endogeneity of any of the three variables. The nature of the results is similar using age in place of experience (specification (3)).

The results of the over-identification tests in Table 5 are only valid if the other coefficients in the wage equation are consistently estimated when using the brother's variables as instruments. There may be some reason to doubt that the brother's schooling and experience are uncorrelated with the wage error.[33] An alternative approach is to perform the over-identification

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[32]Similar equations that excluded the other wage controls from the wage regressions provided lower p-values for these two sets of variables (especially in the 1985 equation) but not for the parents' education and family status variables.

[33]For example, Becker (1981) suggests that intrafamily transfers may depend on

Table 6: Wage Equation Estimates With Parents' Education and Living Arrangements as Identifying Instruments[1]

Specification:[2]	(1)		(2)		(3)		(4)	
	Actual Experience				Age Controls			
Indep. Var.	Early Wage	1985 Wage	Early Wage	1985 Wage	Early Wage	1985 Wage	Early Wage	1985 Wage
Years of Schooling	.206 (.170)	-.079 (.128)	.046 (.007)	.043 (.007)	.146 (.141)	-.037 (.128)	.019 (.008)	.030 (.008)
Experience	-.337 (.723)	.200 (.115)	.063 (.020)	.069 (.007)	--	--	--	--
Test Average	-.117 (.194)	.236 (.189)	.025 (.013)	.061 (.014)	-.055 (.189)	.259 (.207)	.044 (.014)	.092 (.015)
Age	--	--	--	--	-.022 (.080)	.181 (.132)	.051 (.008)	.208 (.116)
Age Squared	--	--	--	--	--	-.002 (.003)	--	-.003 (.002)
VAR(ϵ)	.257	.280	.175	.197	.198	.221	.171	.203
Hausman test P-values:								
Schooling	.234	.266	--	--	.318	.605	--	--
Experience	.505	.169	--	--	--	--	--	--
Test Average	.423	.243	--	--	.626	.365	--	--

1. The only instrumental variables used in this table are the two parents' education variables, the two living arrangements dummies, and the two missing-value dummies for parent's education.

2. Specification (1) instruments for schooling, experience, and the test average. Specification (3) instruments for schooling and the test average. Specifications (2) and (4) are OLS.

tests assuming parents' education and family status are valid instruments. In doing so, we are able to use the full sample in performing the test. Theoretically, these tests should provide the same result as the earlier over-identification tests, if the brother's variables are valid instruments.[34] However, these tests provide virtually no evidence that the other potential instruments are actually correlated with the wage equation errors (see the "full sample" columns in Table 5). Given these results, we regard the estimations using the full set of instruments as more informative than the instrumental-variable estimates reported in Table 6.

IV. Conclusion

Many of our findings concerning bias in OLS estimates of the return to schooling accord with previous research. Including measures of ability reduces the estimate of the return to schooling, and instrumenting for these measures reduces the estimated return still further. Furthermore, instrumenting for schooling can lead to considerably higher estimates of the return to schooling. However, unlike previous research, we find evidence of a downward bias in the schooling return only in initial post-schooling wages; there is no evidence of a downward bias in schooling using wages of individuals who have been out of school for a few years. We interpret this difference in results for the early and later observations as implying that the downward bias in the early-wage equation reflects endogeneity bias that becomes less important over time.

We also consider the potential endogeneity of experience in wage

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labor market outcomes of children, if family heads redistribute resources to low-income children.

[34] The large sample size should increase the power of the tests, while a lower correlation between endogenous variables and instruments could reduce the power.

equations. While we do find strong evidence that experience is correlated with the wage equation error in the later-wage equation, this bias does not greatly affect the schooling return in that equation. Interestingly, instrumenting for experience actually increases the estimate of the return to experience in the later-wage equation.

Our findings do contrast with some of the results in Ashenfelter and Krueger (1992). Our results using the later wage would be expected to be more comparable to AK's, but it is with this wage variable that we find no evidence of upward bias in the schooling return. Using a specification more closely akin to AK--replacing experience with age--does not change this result. There is some evidence of endogeneity (though not measurement error) of schooling in the early-wage equation, but any such bias is not likely to be removed by AK's instruments.

This does not mean that our results are necessarily correct, and Ashenfelter and Krueger's incorrect. They use different data representative of a different population, and control for unobserved ability in a different manner. Neither they nor we have instruments for schooling that are unambiguously valid, in contrast (arguably) to work by Angrist and Krueger (1991, 1992). But our results show that one can address issues of omitted-ability bias, measurement-error bias, and endogeneity bias, and still conclude that OLS estimation ignoring unobserved ability *overstates* the economic return to schooling.

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