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INCOME, SCHOOLING, AND ABILITY: EVIDENCE FROM A NEW SAMPLE OF IDENTICAL TWINS

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ABSTRACT

We develop a model of optimal schooling investments and estimate it using new data on approximately 700 identical twins. We estimate an average return to schooling of 9 percent for identical twins, but estimated returns appear to be slightly higher for less able individuals. Simple cross-section estimates are marginally upward biased. These empirical results imply that more able individuals attain more schooling because they face lower marginal costs of schooling, not because of higher marginal benefits.

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I. Introduction

Understanding the role of genetic and family background factors is a critical component of most public policy discussions about the effectiveness of investments in education. Attempts to understand the influence of schooling on the increased income inequality observed in recent years have heightened the concern with these issues.[1] Publication of the influential Bell Curve [1994], with its claim that genetic endowments predetermine income, has made attempts to understand these issues even more urgent.

In this paper we set out a formal model of schooling investments and income determination that emphasizes the potential role of unobservable ability in the determination of both schooling and income. We estimate this model with a fresh sample of data we have collected on approximately 700 twins who are genetically identical. In the model ability may influence both desired schooling levels and the rate of return to schooling. Our goal is to estimate the return to schooling for genetically identical individuals, the correlation of ability with schooling levels, and the extent to which the return to schooling varies with ability level. It is only with such information that the role of schooling as a potential equalizer of intergenerational correlations in income can be assessed.

The model we estimate implies that the schooling investments of genetically equivalent individuals should be the same, apart from random deviations that are not related to the determinants of schooling choices. We provide a variety of direct and indirect tests of this hypothesis, and therefore of the extent to which contrasts of twins' education and income levels provide causal estimates of the economic return to schooling.

The structure of the remainder of the paper is as follows: in the second and third sections of the paper we develop a simple optimizing model of schooling and show how the correlation between ability (or family background) and schooling level may be estimated. We next describe our data collection, show how we use multiple measurements of schooling to treat the important problem of measurement error,

[1] See, for example, Bound and Johnson [1992], Katz and Murphy [1992], and Levy and Murnane [1992].

and compare our sample of twins with data from a random sample of the population. The empirical results, including a variety of direct and indirect tests of the determinants of twins schooling differences, are contained in Section V. Section VI concludes.

II. Theoretical Framework

In order to provide a structure for our empirical analysis, we set out the simplest optimizing model of school choice that is consistent with the known stylized facts about the determinants of schooling choice and the relationship of earnings to schooling. The two key stylized facts any optimizing theory must capture are: 1) the determinants of school attainment are strongly influenced by the family background of students[2], and 2) the relationship of log earnings to schooling is essentially linear [Mincer 1974].

We assume that individuals (or their parents) seek to maximize utility, which is a function of income and schooling[3]:

$$\begin{aligned} U(y,S) &= \ln(y) - f(S) \\ &= \ln[g(S)] - f(S) \,, \end{aligned} \tag{1}$$

where $y = g(s)$ represents the observable relationship of earnings (y) to schooling (S), and $\ln[g(s)]$ and $f(s)$ are increasing convex functions that represent the (log) benefits and costs of schooling. Maximizing utility in equation (1) requires that optimal schooling (S^*) satisfy the first-order condition,

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[2] See, for example, Hauser and Featherman [1976].

[3] See Becker [1993], Card [1995], or Lang [1993] for a similar approach. The antilog of a special case of equation (1) is $[e^{-rS}/r]g(S)$, which is familiar from the work of Rosen [1973], and the survey by Willis [1986].

$$\frac{g'(S)}{g(S)} = f'(S) , \tag{2}$$

where marginal benefits (g'(S)/g(S)) equal marginal costs (f'(S)).

In order to implement this model empirically, we must choose functional forms for the marginal (proportional) benefits and costs of schooling. To capture the well-known stylized fact that (log) earnings is a (nearly) linear function of schooling[4] that may vary across individuals, it must be the case that for individual i, in family, j, the marginal benefit (MB_{ij}) of schooling is represented by,

$$MB_{ij} = \frac{g'(S_{ij})}{g(S_{ij})} = b_j + \theta A_{ij} , \tag{3}$$

where A_{ij} is unobserved "ability" of the individual.[5] To generate an interior solution for the choice of schooling, we assume that the marginal cost (MC_{ij}) of schooling has the simple form,

$$MC_{ij} = f'(S_{ij}) = r_j + r_0 S_{ij} . \tag{4}$$

It follows that the optimal level of schooling is,

$$S^*_{ij} = \frac{b_j - r_j}{r_0} + \frac{\theta}{r_0}A_{ij} = S^*_j + \frac{\theta}{r_0}A_{ij} , \tag{5}$$

[4] See, for example, Card and Krueger [1992], Heckman and Polachek [1974], Hungerford and Solon [1987], and Park [1994].

[5] In principle, we may also assume that the marginal benefit of schooling depends on the level of schooling attained. This would not alter the linear form of equation (5) describing the optimal level of schooling. It would, however, lead to intractable estimating equations for earnings because of the problems associated with estimation of a quadratic equation in a variable (schooling) measured with error (see Amemiya [1985] and Hausman, Newey, Ichimura, and Powell [1991]).

which varies across families (S_j^*), and may also vary by individual ability if Θ differs from zero. It is clear from equation (5) that schooling levels within the family vary only to the extent that the marginal benefit of schooling varies within the family and is correlated with within-family differences in ability. It follows that the key assumption identifying the return to schooling from within-family variability in schooling levels is that $\Theta=0$; that is, any differences in schooling are determined by differences in tastes or other characteristics that are uncorrelated with the unobserved determinants of earnings, i.e., "optimization errors".[6] The model we fit below, where we assume $\Theta=0$, has the (theoretically) testable proposition that individuals in the same families will have the same desired schooling levels. That is, observed differences in schooling levels, S_{ij}, are due to measurement or optimization errors (ξ_{ij}) so that

$$S_{ij} = S_j^* + \xi_{ij} \, , \tag{6}$$

where the errors ξ_{ij} have mean zero and must be independent of the optimal desired schooling level, S_j^*. Below we discuss the effects on our estimates of relaxing the assumption that $\Theta=0$.

III. Empirical Framework

A. The Average Return to Schooling

Integrating (3) with respect to S_{ij} and assuming $\Theta=0$ gives the well-known log wage equation:

[6] It is important to emphasize that ability here is defined entirely by its effect on earning income in the market, which may not be what some often associate with academic achievement.

$$y_{1j} = A_j + b_j S_{1j} + dX_j + \varepsilon_{1j}$$

(7) and

$$y_{2j} = A_j + b_j S_{2j} + dX_j + \varepsilon_{2j}$$

where y_{1j} and y_{2j} are the logarithms of the wage rates of the first and second twins in a pair, S_{1j} and S_{2j} are the schooling levels of the twins (or, more generally, all attributes that vary within families), X_j are other observable determinants of wages that vary across families, but not within twins (such as race and age), and ϵ_{1j} and ϵ_{2j} are unobservable individual components. A_j is an unobserved family component that represents an unspecified combination of innate (inherited) "ability", family environment, or general unobserved skills, and may be correlated with attained schooling levels. The return to schooling is b_j. According to this model, there may be two types of ability: A_j, which confers higher earnings at all levels of schooling ("absolute advantage"), and b_j, which confers higher net returns to schooling and may also be correlated with ability ("comparative advantage").[7]

First, consider the case in which there is no heterogeneity in the return to schooling, i.e., $b_j=b$ and does not vary by family. In this case, $S_j^* = (b-r_j)/r_0$, and schooling varies across families only because the marginal cost (e.g. psychic costs, access to funds) varies across families. If A_j is correlated with S_j^*, the optimal schooling level, we may write,

$$(8) \qquad \begin{aligned} A_j &= \gamma S_j^* + \upsilon_j \\ &= \gamma\left[\frac{b-r_j}{r_0}\right] + \upsilon_j , \end{aligned}$$

[7] See Willis and Rosen [1979].

where γ indicates whether the marginal cost of schooling (r_j) is positively or negatively related to ability levels. Writing $(S^*_{1j}+S^*_{2j})/2=S^*_j$, and substituting from (6) leads to

$$A_j = \gamma[(S_{1j}+S_{2j})/2] + \upsilon_j - \gamma[(\xi_{1j}+\xi_{2j})/2] \quad (9)$$

In this model, the correlations between the family's absolute ability level and each twin's schooling are identical, and γ indicates the size of that correlation.

Substituting (9) into (7) gives the reduced-form correlated random effects [Chamberlain 1982] estimating equations:

$$y_{1j} = bS_{1j} + \gamma[(S_{1j}+S_{2j})/2] + dX_j + (\nu_j-\gamma u_j) + \varepsilon_{1j}$$

(10) and

$$y_{2j} = bS_{2j} + \gamma[(S_{1j}+S_{2j})/2] + dX_j + (\nu_j-\gamma u_j) + \varepsilon_{2j}$$

where $u_j = (\xi_{1j}+\xi_{2j})/2$. In effect, including the mean schooling level of the family in each wage equation eliminates any "absolute ability" bias in the estimated economic return to schooling, and also provides an estimate of the correlation between schooling and ability, γ. If more able families have more schooling (because the marginal cost of schooling is lower for them), then γ will be positive, and the failure to address this family effect will bias upward the estimated economic return to schooling. Since twins are labeled randomly, we stack the wage equations in (10) and estimate them using generalized least squares (GLS).

An alternative way to estimate the return to schooling (and to other twin-specific characteristics) is to difference the equations in (10) to eliminate the ability effect, obtaining the fixed-effects estimator,

$$y_{2j} - y_{1j} = b[(S_{2j} - S_{1j})/2] + \varepsilon_{2j} - \varepsilon_{1j} \, . \quad (11)$$

Note, however, that the fixed-effects estimator does not allow us to estimate the correlation between ability and schooling directly.

It is worth considering the consequences for the fixed-effects estimator of relaxing the assumption that there are no within-twin ability differences (that is, the assumption that $\Theta=0$). In this case, the fixed-effects estimator may be biased. As in Griliches [1979], the key question then becomes whether the fixed-effects estimator leads to less bias in the estimated return to schooling than the simple cross-section regression estimator. As Griliches [1979] shows, the bias in the fixed-effects estimator will be smaller than the bias in the cross-section estimator if the regression coefficient of ability on schooling is lower in the within-twins regression than in the across-twins regression. Although it is not possible to provide direct estimates of the relevant regression coefficients to evaluate the nature of any bias (because ability is unobserved) we do provide some indirect evidence on this issue in Section V.C below.

B. Returns to Schooling by Ability and Social Class

Next, suppose the marginal return to schooling, b_j, varies by family and is correlated with the family's unobservable "ability". Then we may write,

(12) $$b_j = b_0 + b_1 A_j \, ,$$

where the parameter, b_1, reflects the degree of heterogeneity in the return to schooling that results from the distribution of "abilities" or "learning environments" across families. If families with higher levels of innate "ability" or more enriching learning environments for their children benefit more from schooling, then b_1 should be positive.

Substituting (9) into (12) and the result into (7) gives the reduced-form estimating equations where schooling returns vary with ability:

(13)
$$y_{1j} = b_0 S_{1j} + b_1\gamma[(S_{1j}+S_{2j})/2]S_{1j} + \gamma[(S_{1j}+S_{2j})/2] + dX_j + \varepsilon'_{1j}$$
$$y_{2j} = b_0 S_{2j} + b_1\gamma[(S_{1j}+S_{2j})/2]S_{2j} + \gamma[(S_{1j}+S_{2j})/2] + dX_j + \varepsilon'_{2j}$$

where ε'_{ij} is the error term. The equations in (13) include an interaction term between the individual's schooling level and the family's average schooling level. The coefficient on this interaction term is the product of the two types of ability bias: the correlation between the level of ability and schooling, γ; and the correlation of the marginal benefits of schooling with ability, b_1. As equation (13) indicates, we are able to identify both types of ability effects, assuming there are no intra-twin differences in ability that are correlated with the optimal schooling level (i.e., $\Theta=0$). The first-differenced, or fixed-effects, estimator, is based on fitting

(14)
$$y_{2j}-y_{1j} = b_0(S_{2j}-S_{1j}) + b_1\gamma[(S_{1j}+S_{2j})/2](S_{2j}-S_{1j}) + \varepsilon'_{2j}-\varepsilon'_{1j}.$$

An alternative empirical strategy is to focus on heterogeneity in the return to schooling due to variability in family background. To do this, we stratify families using measures of the education level of the parents. To implement this strategy, suppose family background is related to ability as,

(15)
$$A_j = \delta F_j + \upsilon'_j$$

where F_j represents father's and mother's education. The corresponding reduced-form and fixed-effects equations are,

(16)
$$y_{1j} = b_0 S_{1j} + (b_1 \delta) F_j S_{1j} + \delta F_j + dX_j + \varepsilon''_{1j}$$
$$y_{2j} = b_0 S_{2j} + (b_1 \delta) F_j S_{2j} + \delta F_j + dX_j + \varepsilon''_{2j}$$

where ε''_{ij} is the error term and,

(17)
$$y_{2j} - y_{1j} = b_0 (S_{2j} - S_{1j}) + b_1 \delta F_j (S_{2j} - S_{1j}) + \varepsilon''_{2j} - \varepsilon''_{1j} \ .$$

IV. The Data and Sample

Our data on twin pairs were obtained from over 700 interviews we conducted during the summers of 1991, 1992 and 1993 at the Twinsburg Twins Festival, which is held annually in Twinsburg, Ohio. The festival, the largest of its kind in the world, is much like any other midwestern carnival. An added attraction for attenders is the presence of about 3,000 sets of twins, triplets, and, occasionally, quadruplets.[8]

Since the festival celebrates the attractive features of twinning, attenders appear in similar clothing and hairstyles. We suspect that attenders may be more similar than a random sample of twins because of the emphasis on similarity that is a part of the culture of the festival. We separate the twins in our interview so that respondents will not hear the answers given by their siblings. Interview questions are based primarily on the survey instruments used by the Census and the Current Population Survey, although we have added several questions that are relevant only for twins. (Copies of all three interview schedules are available from the authors upon request.)

[8] Ashenfelter and Krueger [1994], which contains a more complete description of the survey methodology, is based on the much smaller sample of data collected in 1991, the first year of the survey.

Our sample consists of identical twins both of whom have held a job at some point in the previous two years and are not currently living outside of the United States. (Thus, the wages reflect the years 1989-1993 and are inflated to 1993 dollars.) For the 25 percent of the twins in our sample who were interviewed more than once, we average their responses to most questions across the years. We consider a set of twins "identical" if both twins responded that they were identical.[9] In the few cases where a respondent was interviewed more than once and gave conflicting answers to whether she and her twin are identical, we average the responses and consider the pair identical if the average over the years and over the twins is more than 0.5 (i.e., the twins answered that they were identical more often than not). We investigate the influence of misclassifying twins as identical on the estimated returns to schooling in section IV.

Table I provides a comparison of some of the characteristics of our sample of twins with data from the Current Population Survey (CPS) and from the General Social Survey (GSS) for the period during which we conducted our interviews. The characteristics of our sample of twins may differ from the CPS and GSS because our data are not a random sample of twins or because twins do not share identical characteristics with the general population.

It is apparent from Table I that the twins (and their parents) in our sample are better educated and have a higher hourly wage than the general population. It is also apparent that our sample of twins contains relatively more white workers than the general population. We suspect that all of these differences arise from the way twins select themselves into the pool of Twins Festival attenders. It is also apparent that our sample of twins contains relatively fewer married people than the general population. To see what effect, if any, the differences between the CPS data and the twins data imply for inferences about the return to schooling, we report the results from a simple regression of the (natural) logarithm

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[9] The question is, "Is your twin brother/sister an identical twin? That is, are you monozygotic twins?"

of the wage on education, age, and indicators for the race and sex of the respondent for the CPS and our sample of twins in Table II. The comparison in this table is probably the most useful indicator of the extent to which our estimates of economic returns are generalizable. What is apparent from Table II is that every coefficient for the sample of twins in the table (with the exception of the coefficient indicating a respondent's race) is of the same sign, but of a slightly larger magnitude, than in the CPS sample.[10] This pattern would be consistent with the hypothesis that the CPS data contain more measurement error in the independent variables than is the case in our sample of twins. This hypothesis does not explain the anomalous results for the indicator of a respondent's race, however. Because of this anomaly, we have repeated all of our computations deleting non-white respondents, but this has only a small effect on the results. In general, the comparison of our sample of twins with the CPS data shows only small differences between the primary characteristics of the two samples.[11]

In order to deal with measurement error in the schooling levels, which is well-known to be exacerbated in first-differenced equations [Griliches 1977], we asked each twin we interviewed to report on her own schooling level and on her sibling's.[12] Writing S_{ij}^{k} for twin k's report on twin i's schooling implies there are two different ways to use the auxiliary schooling information as an instrumental variable. The clearest way to see this is to consider estimating the wage equation in differenced form. There are four different ways to estimate the schooling difference ΔS_j:

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[10] Note that the estimates of the return to a year of schooling presented in this table are likely to be slightly lower than those estimated using traditional Mincerian models since we control for age rather than (potential) experience. We use age rather than potential experience so that we do not have to instrument for potential experience in the analyses where we correct for measurement error in schooling.

[11] In addition, the educational distribution in our data is extremely close to that reported in Lykken, Bouchard, McGue, and Tellegen [1990] from the Minnesota Twins Registry.

[12] The correlation matrix for the alternative measures is in Appendix 1.

(18) $$\Delta S'_j = S^1_{1,j} - S^2_{2,j} = \Delta S_j + \Delta v'_j$$

(19) $$\Delta S''_j = S^2_{1,j} - S^1_{2,j} = \Delta S_j + \Delta v''_j$$

(20) $$\Delta S^+_j = S^1_{1,j} - S^1_{2,j} = \Delta S_j + \Delta v^+_j$$

(21) $$\Delta S^{++}_j = S^2_{1,j} - S^2_{2,j} = \Delta S_j + \Delta v^{++}_j ,$$

where ΔS_j indicates the true schooling difference and the Δv_j terms represent measurement error. First, one can use, $\Delta S'$, the difference in the self-reported education levels, as the independent variable, and $\Delta S''$, the difference between the sibling reported estimates of the schooling levels, as an instrumental variable for $\Delta S'$. The instrumental variables estimates using the self-reported measures of schooling assume that the measurement error in $\Delta S''$ is uncorrelated with the true level of schooling, and with the measurement error in $\Delta S'$. $\Delta S''$ will be uncorrelated with $\Delta S'$ even if there is a family effect in the measurement error because the family effect is subtracted from both $\Delta S'$ and $\Delta S''$. However, $\Delta S'$ and $\Delta S''$ will be correlated if there is a person-specific component of the measurement error. To eliminate the person-specific component of the measurement error it is sufficient to estimate the schooling differences using the definitions in equations (20) and (21), which amounts to calculating the schooling difference reported by each sibling and using one as an instrument for the other. Throughout this paper we estimate returns to schooling assuming "correlated measurement errors" as it is clear from Ashenfelter and Krueger [1994] that the data reject the assumption that the measurement errors are independent.[13]

V. Empirical Results

A. <u>The Average Return to Schooling</u>

[13] Estimates from the instrumental variables procedure that assumes independent measurement errors are nonetheless very similar and are available from the authors upon request.

The first five columns of Table III show estimates of equations (10) with age, race, and sex as additional covariates, while columns (6) through (10) also control for union and marital status, and job tenure. The generalized least squares (GLS) estimate of equation (10) that sets $\gamma = 0$ and ignores the family effect is contained in the first column.[14] The estimate of the economic return to schooling is 0.102. The coefficients of the other variables in the regression are similar to other estimates in the literature. Column (2) reports the fit of equation (10). The estimated correlation between ability and the mean education level across families (γ) is a positive and statistically significant 0.05. The resulting smaller estimate of the economic return to schooling is approximately 0.07. However, once we use the three-stage-least-squares (3SLS) instrumental variables procedure to deal with measurement error in schooling, the estimate of γ, in column (3), remains positive but is statistically insignificant. These results contrast sharply with those in Ashenfelter and Krueger [1994] who found for a much smaller sample from these data that $\gamma < 0$.[15]

A fixed effects (or "first-differenced") estimate is provided in column (4) of the table.[16] The

[14] The GLS estimates use the seemingly unrelated regression method [Zellner 1962]; the three-stage-least-squares (3SLS) estimates are the analogous method for instrumental variables [Zellner and Theil 1962]. We use GLS and 3SLS methods because by exploiting cross-equation restrictions they are more efficient than ordinary least squares (OLS) (and instrumental variables (IV)), and to ensure correct computation of sampling errors. OLS (and IV) results differ only slightly, and for the sake of completeness we report these for the fixed-effects estimator in Table III. Since we have no explicit measures of lifetime experience, we control for age since the more commonly used "potential experience" (age-education-6) would require further use of instrumental variables to correct for measurement error.

[15] The difference between the results presented here and those reported by Ashenfelter and Krueger [1994] are due to sampling error as the cross-sectional and within-twin estimates of the return to schooling using the smaller sample are not statistically significantly different from those estimated using only later waves.

[16] With two observations per family the fixed-effects estimator is equivalent to a first-differenced estimator. Putting a constant into the first-differenced equation is equivalent to putting a dummy variable into the fixed-effects equation that indicates which of the twins is ordered first. A random ordering will, on average, lead to a coefficient of zero for the constant. However, the regression estimator of the schooling coefficient will be different for each different ordering (or sorting) of the twins, even though

within-twin estimate of the return to schooling is lower than the cross-sectional estimate (in column (1)) by about 30 percent suggesting a positive and significant correlation between ability and the level of schooling. Once we allow for correlated measurement errors in column (5), however, the estimated return increases to about 9 percent, close to the 10 percent return estimated using the cross-section.[17] These estimates are somewhat larger than other results for twins (e.g., Behrman, Rosenzweig, and Taubman 1994, Miller, Mulvey, and Martin 1995, for two recent examples), perhaps because of the widely varying time periods and countries covered. However, all of these studies find that once the estimates are adjusted for measurement error, the fixed-effects estimates are insignificantly different from the OLS estimates.

In columns (6) through (10) we control for other observable differences between twins. The estimates of the return to schooling rise slightly when we control for these covariates, although the estimates of the ability bias in columns (7) and (8) are almost identical to those estimated in columns (2) and (3). Overall, the estimates in Table III suggest that the return to schooling is about 9 percent. Schooling and ability are sufficiently positively correlated to result in ordinary least squares estimates of the return to schooling that are slightly more biased upward than measurement error biases the return downward.

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the average will equal the coefficient obtained when the constant (in the first-differenced equation) is constrained to equal zero. As a result, we run the first-differenced equations without a constant term.

[17] As another approach to dealing with measurement error we restricted the sample to twins who agreed on the difference in their schooling levels. Without other covariates in the regrèssion the coefficient on schooling differences is 0.067, which may be compared with a coefficient of 0.070 in column (4) of Table III. With other covariates in the regression the coefficient is 0.083, which may be compared with a coefficient of 0.078 in column (9) of Table III. It is apparent that the differences in these coefficients are very small and well within the sampling errors of the coefficients. We also get similar results when we interact age and age^2 with the female dummy variable.

B. Returns to Schooling by Ability and Social Class

Table IVa reports estimates of equations (13) where an interaction term with the average level of schooling is included.[18] Note that in our instrumental variables (IV) estimates, we also instrument for measurement error in the interaction term. It is apparent from the table that the interaction term never approaches statistical significance in any of the specifications reported.[19] Table IVb uses the coefficient estimates in Table IVa to provide estimates and standard errors of the return to schooling at different levels of mean schooling. It is apparent that in the actual range of observed schooling levels (roughly 12-16 years) estimated returns are quite similar, although they are lower at higher schooling levels.

Equation (13) indicates that the coefficient on the interaction term is the product of the correlation between ability and schooling (γ) and the heterogeneity in the return to schooling (b_1). Because our GLS and 3SLS estimates allow us to identify separately the correlation between ability and schooling, we can also separately identify the effect of ability on the marginal benefit of schooling, b_1. Estimates of b_1 corresponding to the GLS and 3SLS results are in Table IVc, below:

[18] Others who have attempted to estimate the heterogeneity in the return to schooling have relied on observable measures of ability, such as IQ scores and ability tests, to control for both self-selection and the ability/social class of the individual. For example, see Wolfle and Smith [1956], Weisbrod and Karpoff [1968], Hause [1972], Hauser [1973], Taubman and Wales [1973], Jencks, et. al. [1979], Willis and Rosen [1979], and Garen [1985]. Much of this literature is inconclusive, and the sample of identical twins provides us with a better control for genetic and environmental endowments. Estimates controlling for union and marital status and job tenure are available from the authors upon request.

[19] We also tried estimating these equations separately for men and women, but the differences between them were never statistically different from zero.

Table IVc
Estimates of Heterogeneity in the Return to Schooling

	Column of Table IVa upon which estimates are based	
	GLS (1)	3SLS (2)
Estimate of the heterogeneity in the return to schooling (b_1)	-0.133 (0.112)	-0.041 (0.040)

Estimated asymptotic standard errors are in parentheses.[20]

These estimates indicate that individuals from higher ability families receive a lower marginal benefit from their human capital investment suggesting that schooling is compensatory. The estimates are not statistically significantly different from zero, however.

In Table V we report estimates of equations (16) where the difference in schooling between twins is interacted with an indicator for whether the parents' average education is less than high school, equal to high school, or greater than high school.[21] As the results in the table indicate, these interaction terms are not jointly significantly different from zero. The pattern of returns implied by the interaction terms in the top panel of Table V is contained in the middle panel. This pattern suggests that the highest total returns are attained by the families in the middle and upper portions of the education distribution, although the standard errors of the coefficients are relatively large.

The bottom panel of Table V shows the estimates for the heterogeneity in the marginal return to schooling that takes out the correlation between ability and schooling levels. Again, the estimates suggest

[20] The standard errors are calculated using the delta method. The covariance between the estimated coefficient on the ability bias term (γ) and the interaction between the measure of family background and schooling is -0.000517 in column (1) and -0.000843 in column (2).

[21] We address measurement error in the parents' education by averaging the twins' reports before creating the categories. We have also used continuous measures of family background and instrumented one twin's report using that of the other twin with similar results.

that individuals from families with higher levels of ability receive lower returns to their schooling investments. In addition, this difference is generally statistically significant.

In related work, Altonji and Dunn [1996] have estimated similar earnings equations interacting measures of family background with the differences in schooling levels between siblings using the larger samples in the Panel Study of Income Dynamics (PSID) and the National Longitudinal Surveys of Labor Market Experience of Young Men and Young Women (NLS). Their estimated coefficients on the interaction term produce mixed results. In light of the differences in these estimates, a reasonable interpretation of our results at this time may be that the measured return to schooling is not likely greater for more able individuals.

C. Why Do Twins Have Different Schooling Levels?

Under our interpretation of the determinants of twins schooling decisions, differences in the observed schooling levels of twins in the same family result from either measurement error or random deviations from the optimum schooling level. This interpretation permits us to identify the economic returns to schooling from within-family schooling variability independent of genetic ability. Virtually any model of optimum schooling decisions will predict identical desired schooling levels so long as there are no ability differences among twins in the same family.[22] Since the assumption that any within-family ability differences are either negligible or uncorrelated with schooling decisions is essential to our interpretation of the empirical results, it is natural to ask whether there is any evidence to support our interpretation.

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[22] This result is quite general, despite the explicit functional forms we have chosen for the marginal benefit and costs of schooling, because any monotone transformation of equation (1) leads to the same optimal schooling level. If parents optimize (1) for their children, any monotone transformation of the sum of their utility functions will also lead to identical optimal schooling levels for each child.

Despite the considerable difficulty associated with any attempt to document exogeneity with non-experimental data, we think there are six interesting findings that bear on the credibility of our interpretation. First, it is important to recognize that identical twins are formed only when a fertilized egg is accidentally split. There can, therefore, be no genetic differences between identical twins, as they have precisely the same DNA. The only way that genetic differences might appear in our data is from measurement error, where twins are incorrectly classified as identical when they are not. (Fraternal twins, who also attend the Twinsburg Festival, are formed when more than one egg is fertilized, and they are no more alike genetically than ordinary brothers or sisters.) To explore this possibility, in Table VI we report several alternative estimates of the economic returns to schooling that examine the sensitivity of our results to possible misclassification. The results in the table provide no evidence that better determination of identical twinning alters the results. This suggests that measurement error in twin classification has not contaminated our findings.

Second, twins may be treated differently for non-economic reasons, so that they end up with different non-genetic abilities. It is difficult to measure directly the way that twins were treated while growing up, but the casual evidence we have observed during interviews suggests that parents find it extremely difficult to treat identical twins any other way than identically. For example, one of the earliest decisions parents face, where they must treat twins differently, is in the assignment of first names. Table VII contains some of the (almost humorous) data on the first names of the twins in our sample that indicates how parents cope with the difficulty. As the table indicates, 27 percent of our identical twins had first names that rhyme, and 12 percent had names that differ by only one letter! Likewise, 50 percent of the twins in our sample had names that begin with the same letter, although we would have expected this to happen in only 8 percent of twin pairs if names were selected randomly from the population.[23]

[23] If the first letter in first names were distributed uniformly throughout the alphabet two randomly selected individuals would have names that begin with the same letter in 1/26, or about 4% of cases. Since names are not uniformly distributed, however, we have used the actual distribution

As to individual names, Ronald and Donald, and Karen and Sharon, are the most common names in our sample for males and females, respectively. As the table indicates, the parents of twins are actually selecting these names at a rate nearly five times their incidence in the general population. This statistically significant difference in the selection of names from the population at large is a strong indication that parents are selecting these names in an attempt to treat twins as similarly as possible.

Third, in the second year of our survey, we asked the twins why one twin had received more schooling than the other. Among female twins the modal response was that the less educated twin "got married" (or the converse, "got divorced and needed to get a job"). Among males, the modal response was that the two twins had different career interests. Only 11 percent of our unstructured responses included such explanations of schooling differences as, "one twin was better at books," which might (obviously) be interpreted as ability differences.

Fourth, it is sometimes argued that the first born twin is the heavier, and that this reflects different pre-birth environmental influences in the womb. Since it has been established that early birth weight may affect early childhood development and thus perhaps schooling level attained, it is possible that the first born is both more able and has more schooling.[24] Since we were not aware of any explicit tests of this hypothesis, we collected data from the twins we interviewed on which of the pair was born first. The results of a regression of the schooling difference on a dummy variable indicating which twin was first born are contained in Table VIII, below.

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of first letters to compute Σp_i^2, where p_i is the actual fraction of first names associated with the i^{th} letter in the alphabet, which is the probability of observing two randomly selected individuals with names that begin with the same letter, given the actual distribution of names in the twins sample.

[24] See Behrman, Rosenzweig, and Taubman [1994] for a discussion of the role of birth weight and twinning.

Table VIII
The Effect of Birth Order on Educational Attainment

	Dependent variable: ΔS_j	
	All identical twins who agree on birth order	Identical twins who agree on birth order and on the difference in schooling levels
First-born	0.048 (0.112)	0.193 (0.156)
R^2	0.001	0.010
No. of Obs.	241	152

As the table indicates, the difference in schooling levels attained is at most 0.2 of a year and not statistically significant. As a result, when we include birth order as a determinant of earnings in a fixed-effects (IV) regression, its coefficient is a statistically insignificant -0.013 (with a standard error of 0.044) and the coefficient on schooling does not change. These results show little evidence that ability differences are influencing our findings.

Fifth, using our data on twins it is straightforward to estimate the extent to which variables associated with family background account for variability in educational attainment. If we are correct in our interpretation that the within-family schooling variability of identical twins represents primarily optimization errors, it seems reasonable to suppose that family background variables will explain a significant fraction of the variance in schooling levels. In principle, our measure of the variability in schooling levels due to family background provides an upper limit to the explained variance (that is, R^2) that could be attained by a regression of schooling levels on family background variables. This upper limit should, in principle, be larger than that obtained in empirical studies where schooling levels are regressed on observable family background variables.

The data on total and within-family variability in schooling necessary to calculate the theoretical upper bound on the variance in schooling attainment explained by family background are contained in Table IX.

Table IX
Variances of Schooling

Total variance in schooling		Within-family variance in schooling	
$Var(S_{ij})$ (1)	$Var(S^*_{ij})^\dagger$ (2)	$Var(\Delta S^\dagger_j)$ (3)	$Var(\Delta S^*_j)^\dagger$ (4)
4.299	4.030	2.129	1.583

† These variances are corrected for correlated measurement error.

We adjust the variances in Table IX for measurement error by replacing the variance of the measured schooling level (or difference) with the covariance between the two independent measures of the level (or difference). As the table indicates, measurement error represents about 6 percent [((4.299-4.030)/4.299)×100] of the total variance in measured schooling, while measurement error represents a much larger 26 percent [((2.129-1.583)/2.129)×100] of the within-family variance. A comparison of the results in columns (2) and (4) in the table indicates that, in theory, family background explains about 60 percent of the variance in schooling attainment.[25] In short, our data indicate that much of the variability in schooling decisions is due to differences across families, which is consistent with our interpretation of the origin and nature of schooling differences, and indicates that the within-twin estimates of the return to schooling are less biased than the across-twin estimates.

Finally, it is possible to test whether there is a twin-specific component to ability by using ancillary data on the other observable characteristics that differ across and within families. To do so,

25 This theoretical fraction of variance explained by family background factors may be compared with the estimated R^2 in Hauser and Featherman [1976] of about 0.33.

assume that (in the spirit of Griliches 1979) the observed schooling level, S_{ij}, is a function of family ability (A_j) as well as independent individual-specific ability (A_{ij}) so that,

(22) $$S_{ij} = \gamma_1 A_j + \gamma_2 A_{ij} + \omega_{ij}$$

where ω_{ij} is independent of A_j and A_{ij}.[26] The family schooling level, the average of the twins' schooling levels, S_j, is then,

(23) $$S_j = \gamma_1 A_j + \omega_j .$$

Now suppose that other observable characteristics (X) that vary within twins and across twins are correlated with ability as well, so that

(24) $$X_{ij} = \pi_1 A_j + \pi_2 A_{ij} + \omega'_{ij} \quad \text{and}$$

(25) $$X_j = \pi_1 A_j + \omega'_j .$$

The correlation coefficient of the across family measures of schooling and other characteristics is $\mathrm{Corr}(S_j,X_j)=[\gamma_1\pi_1\ \mathrm{Var}(A_j)]/[\mathrm{Var}(S_j)\mathrm{Var}(X_j)]^{1/2}$. This correlation will be significant only if there is significant variability across families in ability ($\mathrm{var}(A_j)>0$), and if ability is related to schooling ($\gamma_1 \neq 0$), and the characteristic, X ($\pi_1 \neq 0$). Differencing equations (22) and (24) shows that the correlation between the within-family measures of schooling and other measurable characteristics is $\mathrm{Corr}(\Delta S_j,\Delta X_j)=[\gamma_2\pi_2\ \mathrm{Var}(\Delta A_j)]/[\mathrm{Var}(\Delta S_j)\mathrm{Var}(\Delta X_j)]^{1/2}$. It is reasonable to suppose that if the family's ability level is correlated with an observable X (so that $\pi_1 \neq 0$), then an individual's ability level will also be correlated

[26] We normalize A_j and A_{ij} so that $[(A_j+A_{ij})/2]=0$.

with the same observable X (so that $\pi_2 \neq 0$). Thus, we may test for the presence of within-family ability differences by comparing across-family correlations of schooling and measurable characteristics with within-family correlations. If the former are significant for some characteristics, we may conclude that the characteristic is correlated with unmeasured ability and that families differ in ability. If the within-family ability differences are also significant, then we have evidence that there is within-family variation in ability that may bias our results. If the within-twin correlations are negligible, however, we can infer that unobserved ability differences between twins are not an important source of bias for our estimates.

In Table X we present correlations between schooling differences and several other observable characteristics of twins. The top panel shows correlations based on the across-family variation, that is the average of the twins' characteristics. In the bottom panel, we estimate the correlations based on the within-twin differences. The asterisks indicate whether the correlation is significant at the 1 percent, 5 percent, or 10 percent level. In the top panel, many of the characteristics, such as union status and job tenure, are significantly correlated with average education, and with each other, indicating that the correlation between ability and these characteristics is strong. On the other hand, the correlations of schooling differences within-twins and differences in their self-employment, union status, and job tenure are negligible. This provides strong evidence that within-twin ability differences are negligible compared to across-family ability differences. This, in turn, implies that any ability bias in returns to schooling estimates is smaller in the within-twin estimator than in the cross-section estimator.

A particularly compelling example is spouse's education. A growing literature examines the extent to which the characteristics of individuals in a marital union are complements or substitutes in household production. Much data supports findings of positive assortative mating with respect to education. We also estimate a correlation of 0.552 between the average spouse education and the average education of the twins. If the schooling differences between twins also reflect differences in their abilities, then the correlation between the schooling differences of the twins' spouses will also be correlated with the schooling differences of the twins.[27] However, we find that, for those twins who were married, the difference in their spouses' education levels is essentially uncorrelated with the difference in the twins' schooling levels.

In order to explore the extent to which the difference between the across-family and within-twin correlations with spousal education is due to measurement error, we repeated the exercise in Table X in a regression framework. First we regressed the average of the spouses' education on the average of the twin's education (to obtain the across-family estimate); we then regressed the within-twin difference in the spouses' education on the difference in the twins' education levels. In both cases we instrumented for schooling assuming correlated measurement errors. The results are presented in Table XI below.

Table XI
IV Estimates of the Effect of Own Education on Spouse's Education

	Dependent variable: Spouses' education (1)	Dependent variable: Δ Spouses' education (2)
Education (across-family)	0.502 (0.084)	
Δ Education		0.039 (0.242)
Number of observations	91	91

As with the correlations, the twins' schooling is significantly correlated with their spouses' education using the across-family variation, while the coefficient decreases to near zero using the within-twin variation. Thus the difference is not entirely due to measurement error. These results provide further evidence that the within-twin schooling differences are not solely determined by within-twin ability differences and that within-twin estimates of the return to schooling contain less ability bias than cross-sectional estimates.

[27] See Behrman, Rosenzweig, and Taubman [1994] for a discussion of the literature on assortative mating and the extent among identical twins.

D. Implications of the Empirical Results

Taken as a whole, these results have a straightforward interpretation within the theoretical framework set out above. They suggest that individuals with higher levels of ability receive slightly higher levels of schooling. As a result, cross-sectional estimates of the return to schooling are marginally upward biased by an omitted ability variable. At the same time, the higher ability individuals may receive a slightly lower marginal benefit to schooling. Assuming that the marginal benefit schedule is independent of the level of schooling (as in equation (3)), Figure I shows the resulting implied marginal benefit and marginal cost curves for individuals with "high" and "low" abilities. Because the lower ability individuals receive a higher marginal benefit from their schooling, we would expect that their optimal schooling level would be higher than that for the higher ability individuals if the two groups face the same marginal cost curves. However, the fact that higher ability individuals receive more schooling implies that higher ability individuals do, in fact, face a lower cost of funds. As a result, the optimal schooling levels of high ability people are slightly higher than those of lower ability individuals despite their lower returns. These results imply that higher ability individuals have higher wages, in general, but the slope of their wage-schooling profile is flatter than that for lower ability individuals.

The finding that the slope of the relationship between wages and schooling is flatter for high ability people than for low ability people implies that the two wage schedules may cross at a very high level of schooling. Some may find this implication implausible. Of course, these results are only approximations to some underlying functional form and may be inaccurate outside the range of approximation. An alternative model, also consistent with these findings, is one where the marginal benefit of schooling decreases with the schooling level and where marginal costs are negatively correlated with ability level, as in Card [1995] and Lang [1993].

Under either assumption about the slope of the marginal benefit schedule, these results are consistent with recent instrumental variables estimates of the return to schooling that are at least as large as the equivalent ordinary least squares estimates.[28] Since the instruments used in most studies affect the schooling of individuals for whom the marginal cost of schooling is high, the instruments are likely to have a disproportionate affect on the schooling of individuals with high marginal benefits, such as those from disadvantaged families. Because these instrumental variables estimates tend to be at least as great as the OLS estimates, they suggest that individuals from more advantaged backgrounds have lower marginal returns to schooling.[29]

VI. Conclusion

This paper contains a simple model of schooling investments and income determination that emphasizes the potential role of unobservable ability in the determination of both schooling and income. Under some assumptions, this model implies that the schooling investments of genetically equivalent individuals should also be the same, apart from random deviations that are not related to the determinants of schooling choices. Using data we have collected for identical twins, who have the same genes and family background, we find that a variety of direct and indirect tests provides little evidence inconsistent with this hypothesis. Although these results are only suggestive, it follows that contrasts of the wage differences of identical twins with their education differences may provide a particularly useful way to isolate the causal effect of schooling on earnings.

From our analysis of the wage rates of twins we estimate that the average return to schooling is about 9 percent per year attained for genetically identical individuals. Our estimates also suggest that the returns to schooling may be slightly lower for high ability individuals, so that schooling compensates for

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[28] See, for example, Angrist and Krueger [1991], Card [1993], and Kane and Rouse [1993]. Typical instruments are: compulsory schooling laws or distance from the nearest college, which are likely to have their greatest impact on high school dropouts and those for whom transportation costs could prove prohibitive.

[29] This explanation is developed in Card [1995] and Lang [1993].

genetic differences. However, unlike the results from the smaller sample in Ashenfelter and Krueger [1994], we find that typical cross-section estimates of the return to schooling are slightly upward biased. In our theoretical framework, these findings taken together imply that high ability individuals invest more in schooling because they face lower marginal costs of doing so. These results stand in sharp contrast to recent claims that genetic factors predetermine education and income, and that such differences are not amenable to alteration by public or private choices.

Appendix 1: Correlation Matrix for Identical Twins

	Log wage$_1$	Log wage$_2$	Educ$_1^1$	Educ$_1^2$	Educ$_2^2$	Educ$_2^1$	Father's educ^1	Father's educ^2	Mother's educ^1	Mother's educ^2
Log wage$_1$	1.000									
Log wage$_2$	0.666	1.000								
Education$_1^1$	0.322	0.346	1.000							
Education$_1^2$	0.294	0.307	0.933	1.000						
Education$_2^2$	0.231	0.364	0.752	0.769	1.000					
Education$_2^1$	0.219	0.363	0.756	0.739	0.913	1.000				
Father's education^1	0.068	0.094	0.369	0.363	0.326	0.344	1.000			
Father's education^2	0.081	0.112	0.375	0.374	0.346	0.369	0.871	1.000		
Mother's education^1	0.079	0.055	0.290	0.282	0.239	0.254	0.581	0.558	1.000	
Mother's education^2	0.080	0.051	0.325	0.307	0.233	0.257	0.574	0.604	0.820	1.000

There are 320 observations. "Log wage$_i$" is twin "i"'s report of his/her own wage, "Education$_i^j$" is twin "j"'s report of twin "i"'s education, and "Parent's education^j" is twin j's report of the parent's education.

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Table I

Descriptive Statistics: Means and Standard Deviations

	Identical Twins	CPS	GSS
Self-reported education	14.03 [2.07]	13.16 [2.59]	13.57 [2.69]
Sibling-reported education	13.95 [2.10]		
Hourly wage	14.44 [13.02]	12.04 [7.61]	
Age	38.07 [11.55]	37.61 [11.40]	38.86 [11.31]
White	0.92 [0.27]	0.77 [0.42]	0.83 [0.37]
Female	0.59 [0.49]	0.47 [0.50]	0.49 [0.50]
Father's education	12.10 [3.04]		11.37 [3.98]
Mother's education	12.09 [2.49]		11.39 [3.20]
Number of siblings	3.56 [2.21]		3.53 [2.83]
Covered by union	0.23 [0.41]	0.19 [0.39]	
Job tenure (years)	8.34 [8.71]		
Married	0.64 [0.48]	0.61 [0.49]	0.77 [0.42]
Interviewed more than once	0.24 [0.43]		
Sample size	680	476,851	4,836

The Current Population Survey (CPS) sample is drawn from the 1991-1993 Outgoing Rotation Group files; the sample includes workers age 18-65 with an hourly wage greater than $1.00 per hour in 1993 dollars and the means are weighted using the earnings weight. The General Social Survey (GSS) sample is from the 1990, 1991, 1993, and 1994 surveys. The sample includes workers age 18-65.

Table II

OLS Estimates of the (Mean) Return to Schooling using the CPS and Twins Data

	CPS[a]	Identical Twins
	OLS (1)	OLS (2)
Own education	0.085	0.110
	(0.0003)	(0.009)
Age	0.071	0.104
	(0.0004)	(0.010)
Age^2 ($\div$100)	-0.074	-0.106
	(0.0005)	(0.013)
Female	-0.253	-0.318
	(0.001)	(0.040)
White	0.087	-0.100
	(0.002)	(0.072)
Sample size	476,851	680
R^2	0.332	0.339

Standard errors are in parentheses. All regressions include a constant.

[a] The Current Population Survey (CPS) sample is drawn from the 1991-1993 Outgoing Rotation Group files. The sample includes workers age 18-65 with an hourly wage greater than $1.00 per hour in 1993 dollars; the regression is weighted using the earnings weight. We converted the 1992 and 1993 education categories into a continuous measure according to the categorization suggested by Park [1994].

Table III
GLS, 3SLS, and Fixed-Effects Estimates of the (Mean) Return to Schooling for Identical Twins

	Without Other Covariates					Controlling for Other Covariates				
	GLS (1)	GLS (2)	3SLS (3)	First-difference (4)	First-diff. by IV (5)	GLS (6)	GLS (7)	3SLS (8)	First-difference (9)	First-diff. by IV (10)
Own education	0.102	0.066	0.091	0.070	0.088	0.113	0.074	0.106	0.078	0.100
	(0.010)	(0.018)	(0.024)	(0.019)	(0.025)	(0.010)	(0.017)	(0.022)	(0.018)	(0.023)
Avg. education		0.051	0.033				0.055	0.031		
$[(S_1+S_2)/2]$		(0.022)	(0.028)				(0.021)	(0.025)		
Age	0.104	0.103	0.103			0.092	0.088	0.087		
	(0.013)	(0.013)	(0.013)			(0.013)	(0.013)	(0.013)		
Age^2 ($\div$100)	-0.107	-0.104	-0.104			-0.106	-0.101	-0.100		
	(0.015)	(0.015)	(0.015)			(0.015)	(0.015)	(0.015)		
Female	-0.315	-0.309	-0.306			-0.239	-0.228	-0.226		
	(0.049)	(0.049)	(0.049)			(0.048)	(0.048)	(0.049)		
White	-0.106	-0.105	-0.101			-0.094	-0.096	-0.094		
	(0.090)	(0.091)	(0.091)			(0.086)	(0.087)	(0.087)		
Covered by a						0.109	0.106	0.109	0.085	0.087
union						(0.044)	(0.043)	(0.043)	(0.055)	(0.055)
Married						0.066	0.087	0.101	0.044	0.052
						(0.050)	(0.050)	(0.050)	(0.073)	(0.073)
Tenure (years)						0.022	0.023	0.023	0.024	0.024
						(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Sample size	680	680	680	340	340	666	666	666	333	333
R^2	0.262	0.264	0.267	0.039		0.352	0.351		0.177	

Standard errors are in parentheses. Cols. (1)-(3) and (6)-(8) include a constant; cols. (2)-(5) and (7)-(10) assume correlated measurement errors. For the GLS and IV estimates, one twin's reports of twin 1 and twin 2's education are used as regressors and the other twin's reports of the two measures are used as instruments. For the fixed-effects estimates, the difference in education is the difference between twin 1's report of twin 1's own education and twin 1's report of twin 2's education; the instrument used is the difference between twin 2's report of twin 1's education and twin 2's report of twin 2's own education.

Table IVa

GLS, 3SLS, and Fixed-Effects Estimates of the Returns to Schooling by the Average of the Twins' Schooling Levels for Identical Twins

	GLS (1)	3SLS (2)	First-difference (3)	First-diff. by IV (4)
Own education	-0.041	0.141	-0.030	0.167
$[S_1]$	(0.088)	(0.113)	(0.196)	(0.329)
Avg. education	-0.056	0.080		
$[(S_1+S_2)/2]$	(0.089)	(0.114)		
Own*avg. educ.	0.007	-0.003	0.007	-0.005
$[S_1*((S_1+S_2)/2)]$	(0.006)	(0.008)	(0.013)	(0.023)
Age	0.102	0.103		
	(0.013)	(0.013)		
Age^2 (÷100)	-0.104	-0.105		
	(0.015)	(0.015)		
Female	-0.312	-0.305		
	(0.049)	(0.049)		
White	-0.111	-0.098		
	(0.091)	(0.091)		
Sample size	680	680	340	340
R^2	0.266		0.040	

Standard errors are in parentheses. Estimates assume correlated measurement errors. Cols. (1) and (2) include a constant. For the GLS and IV estimates, one twin's reports of twin 1 and twin 2's education are used as the regressors and the other twin's reports of the two measures are used as instruments. For the fixed-effects estimates, the difference in education is the difference between twin 1's report of twin 1's own education and twin 1's report of twin 2's education; the instrument used is the difference between twin 2's report of twin 1's education and twin 2's report of twin 2's own education.

Table IVb

Estimated Return to Schooling for Different Average Twin Schooling Levels for Identical Twins

	Estimated return to schooling			
Avg. of twins' schooling levels	GLS (1)	3SLS (2)	First-difference (3)	First-diff. by IV (4)
9	0.062	0.111	0.032	0.118
	(0.037)	(0.048)	(0.076)	(0.127)
12	0.085	0.101	0.053	0.101
	(0.023)	(0.031)	(0.038)	(0.061)
14	0.099	0.094	0.067	0.090
	(0.019)	(0.025)	(0.020)	(0.027)
16	0.114	0.087	0.081	0.079
	(0.021)	(0.027)	(0.028)	(0.043)
18	0.129	0.081	0.094	0.068
	(0.028)	(0.036)	(0.052)	(0.084)

Standard errors are in parentheses. Based on estimates in Table IVa.

Table V

GLS, 3SLS, and Fixed-Effects Estimates of the Returns to Schooling by Family Background for Identical Twins

	GLS (1)	3SLS (2)	First-difference (3)	First-diff. by IV (4)
Own education	0.076 (0.021)	0.092 (0.022)	0.029 (0.032)	0.052 (0.039)
Parents' education[a]= high school	-0.190 (0.367)	-0.372 (0.384)		
Parents' education= more than high school	-0.574 (0.444)	-0.457 (0.460)		
Own educ*(parents' educ=HS)	0.021 (0.027)	0.033 (0.028)	0.068 (0.043)	0.092 (0.056)
Own educ*(parents' educ=HS+)	0.046 (0.031)	0.036 (0.032)	0.061 (0.052)	0.040 (0.066)
p-Value of difference between own educ*parents' educ=HS+ and own educ*parents' educ=HS	0.349	0.844	0.884	0.444
Sample size	656	656	328	328
R^2	0.263		0.049	
		Estimated return to schooling		
Less than high school	0.076 (0.021)	0.092 (0.022)	0.029 (0.032)	0.052 (0.039)
High school	0.097 (0.017)	0.125 (0.019)	0.097 (0.029)	0.144 (0.040)
More than high school	0.122 (0.022)	0.128 (0.023)	0.090 (0.040)	0.093 (0.053)
		Estimated heterogeneity in the return to schooling (b_1)		
High school[a]	-0.108 (0.073)	-0.089 (0.021)		
More than high school[a]	-0.080 (0.013)	-0.079 (0.016)		

Standard errors are in parentheses. Estimates assume correlated measurement errors. Cols. (1) and (2) include a constant, age, age squared, and dummy variables for female and white.

[a] "Parents' average education" is the average of father's and mother's education as reported by both twins. The base group is parents' education less than high school.

Table VI

The Effect of Measurement Error in the Identification of Identical Twins on the Returns to Schooling in OLS and IV First-Differenced Log Wage Equations

	One twin reported as alike as "two peas in a pod" (1)	One twin reported identical (2)	Both reported as alike as "two peas in a pod" (3)	Both reported identical (4)	Both reported as alike as "two peas in a pod" and identical (5)	Both reported as alike as "two peas in a pod", identical, and tested (6)
				OLS		
Education	0.066 (0.018)	0.064 (0.019)	0.058 (0.019)	0.066 (0.019)	0.067 (0.020)	0.089 (0.033)
R^2	0.034	0.032	0.026	0.036	0.036	0.121
				IV		
Education	0.085 (0.023)	0.086 (0.025)	0.077 (0.026)	0.083 (0.025)	0.083 (0.027)	0.163 (0.047)
No. of obs.	395	353	342	335	297	52

Estimated assuming correlated measurement errors. Standard errors are in parentheses.

The samples are:

(1) At least one twin reported that as children they looked as alike as "two peas in a pod".
(2) At least one twin identified them as identical.
(3) Both twins reported that as children they looked as alike as "two peas in a pod".
(4) Both twins identified them as identical.
(5) Both twins reported that as children they looked as alike as "two peas in a pod" and that they are identical.
(6) Both twins reported that as children they looked as alike as "two peas in a pod", they are identical, and they were tested.

Table VII

Characteristics of Identical Twin First Names

I. Percentage of First Names that Rhyme: 26.259

Percentage of First Names that Differ by Only One Letter: 12.050

II. Percentage of First Names that Begin with the Same Letter

Twins	If Assigned Independently
50.2%	8.2%

III. Most Common First Names Among Identical Twins

	Twins	U.S. Population
	Men	
Ronald	4.728%	0.797%
Donald	5.201%	1.076%
	Women	
Karen	3.066%	0.667%
Sharon	1.314%	0.522%

Twins based on the twins surveys. U.S. Population based on the top 90% of names from the U.S. Census.

Table X

Correlation Matrix of Family Characteristics of Identical Twins

	Education	Married	Self-employed	Covered by a union	Years of job tenure	Father's education	Mother's education	Spouse's education
Education	1.000							
Married	-0.319***	1.000						
Self-employed	0.033	0.115**	1.000					
Covered by a union	-0.091*	0.137**	-0.197***	1.000				
Years of job tenure	-0.147***	0.381***	0.215***	0.277***	1.000			
Father's education	0.399***	-0.261***	-0.046	-0.173***	-0.138**	1.000		
Mother's education	0.303***	-0.225***	0.049	-0.149***	-0.179***	0.628***	1.000	
Spouse's education[a]	0.552***	--	-0.022	0.054	-0.132	0.283***	0.099	1.000

Correlation Matrix of Differences Between Twins for Identical Twins

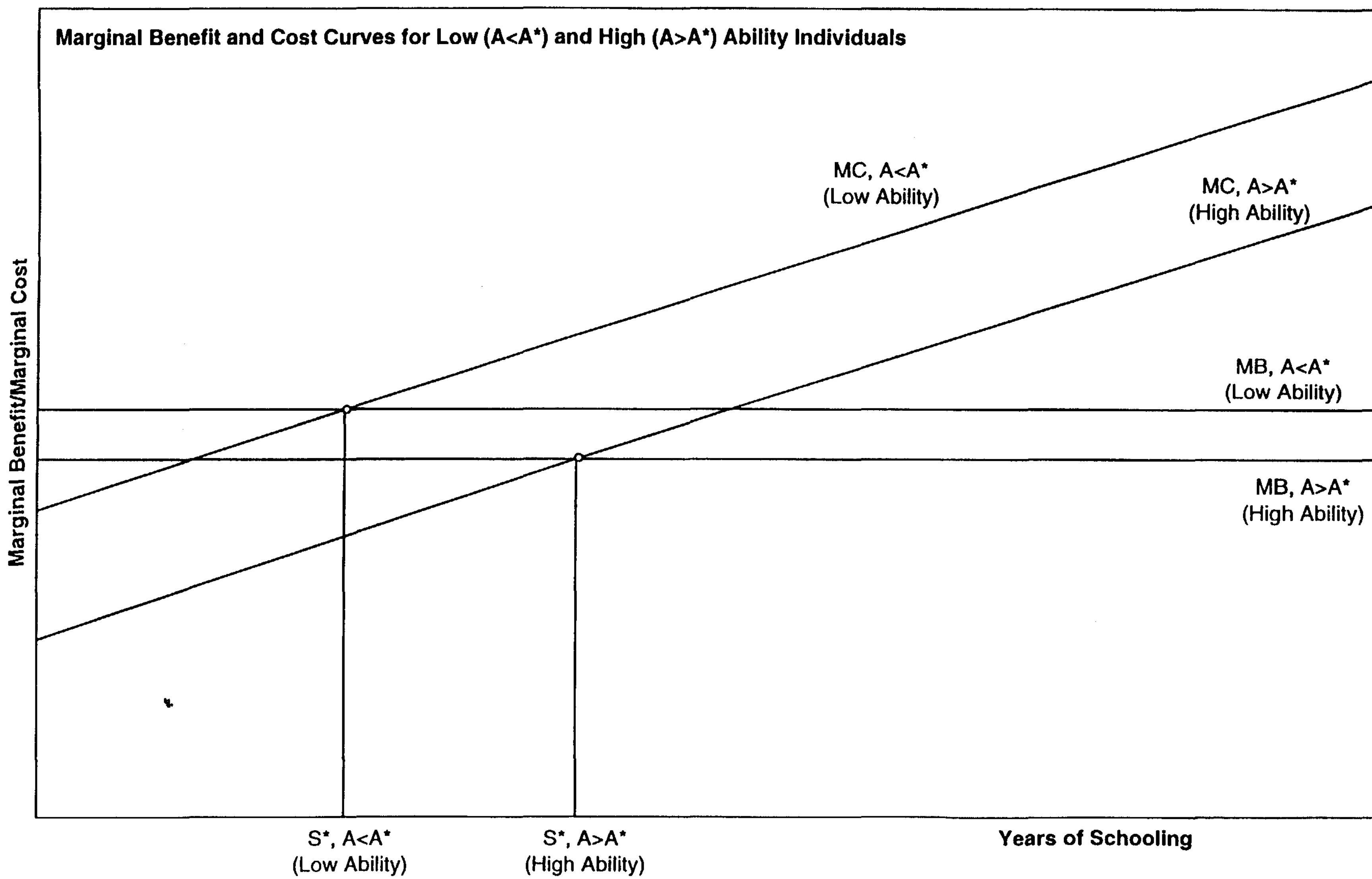
	Δ Education	Δ Married	Δ Self-employed	Δ Union	Δ Tenure	Δ Spouse's educ
Δ Education	1.000					
Δ Married	-0.113*	1.000				
Δ Self-employed	-0.046	0.031	1.000			
Δ Covered by union	-0.055	-0.026	-0.148**	1.000		
Δ Tenure	-0.027	0.023	-0.074	0.123*	1.000	
Δ Spouses' education[a]	0.006	--	-0.071	0.050	-0.011	1.000

In the top panel, all characteristics are measured as the twin average. In the bottom panel, Δ X is the first-difference for the twins' characteristic, X. Δ Education is the first-difference of the average education for each twin. There are a maximum of 313 observations.

* Significant at the 10% level; ** Significant at the 5% level; *** Significant at the 1% level.

[a] Based on the sub-sample of married twins. There are a maximum of 91 observations.

Figure I


Marginal Benefit and Cost Curves for Low (A<A*) and High (A>A*) Ability Individuals
MC, A<A*
(Low Ability)
MC, A>A*
(High Ability)
MB, A<A*
(Low Ability)
MB, A>A*
(High Ability)
Marginal Benefit/Marginal Cost
S*, A<A*
(Low Ability)
S*, A>A*
(High Ability)
Years of Schooling