NORC at the University of Chicago

In-School Work Experience and the Returns to Schooling

Author(s): Audrey Light

In-School Work Experience and the Returns to Schooling

Audrey Light, *Ohio State University*

Students often accumulate substantial work experience before leaving school. Because conventional earnings functions do not control for in-school work experience, their estimates of the return to schooling include the benefit of work experience gained along the way. Using data from the National Longitudinal Survey of Youth, I estimate wage models with and without controls for in-school work experience. The estimated schooling coefficients are 25%–44% higher (depending on how I control for ability bias) when in-school work experience is omitted than when it is included. These findings indicate that conventional models significantly overstate the wage effects of "school only."

I. Introduction

Models of educational investment decisions typically make the simplifying assumption that the life cycle is neatly divided into a period of full-time schooling followed by full-time employment. In recasting the behavioral model of Becker (1967), Rosen (1977) provides a succinct statement of the prevailing view when he writes, "At heart then the theory of human capital is a generalized harvesting problem: when should a person stop school and enter the market?"[1] This view justifies the

[1] This statement is made in the context of showing that the decision to terminate one's schooling reduces to a comparison of the marginal, internal rate of return to additional schooling and the marginal cost, as in the capital investment models of Jevons (1871) and Wicksell (1934).

empirical earnings function originally proposed by Mincer (1974) and now in widespread use, in which measures of completed schooling and postschool work experience control for skill accumulated before and after labor market entry.

As useful as the orthodox models have been in helping us understand the relationship between skill acquisition and earnings, they mask a potentially important difference between human capital investment and traditional harvesting problems: many young people accumulate a substantial amount of work experience before completing their schooling. They do so by working during school vacations, holding jobs during the academic year, and working during schooling interruptions that last anywhere from a few months to a few years. Using a sample of male respondents in the National Longitudinal Survey of Youth (NLSY), I find that the typical high-school graduate accumulates almost fifteen hundred hours of work experience between his sixteenth birthday and his graduation from high school, while the typical college graduate gains over five thousand hours of work experience by the time he leaves school. Moreover, every college graduate in my sample acquires some work experience prior to leaving school.

In this article, I examine the implications of student employment for estimating the causal relationship between schooling and postschool wages. Analysts seeking to identify this effect typically estimate a Mincer-type earnings function in which the covariates include measures of schooling attainment and (actual or potential) postschool work experience but not work experience gained in school. Because schooling attainment is influenced by such factors as innate ability and family background that also affect earnings, analysts often use proxy, instrumental variables or fixed-effect techniques to identify the causal effect of schooling. One interpretation of the resulting parameter estimate is that it represents the labor market return to classroom learning. This view, while extremely narrow, is consistent with models of human capital investment (e.g., Ben Porath 1967) that assume "learning" is the only skill-enhancing activity to take place during the school phase of the life cycle. However, the prominence of in-school work experience seen in the data suggests that we should interpret the causal schooling effect as the wage benefit of skills gained in the classroom plus skills gained concurrently via on-the-job training.[2] Although the extent of student employment is widely recognized, this latter interpretation is rarely mentioned explicitly in the empirical literature. It appears most analysts interpret estimated schooling

_____

[2] Estimated schooling coefficients might also capture the skill-enhancing effects of extracurricular activities, social interaction, and other experiences that coincide with schooling.

coefficients as the return to time spent acquiring human capital via school enrollment.

The goal of this article is to identify the separate, causal effects on postschool wages of schooling (time spent in school) and in-school work experience (time spent working while in school). Just as we determine how the estimated schooling coefficient changes when we control for the confounding effects of innate ability and related unobservables, I ask how the causal effect of schooling changes further when the wage-enhancing role of in-school work experience is "netted out." To address this question, I use data from the NLSY to estimate wage models with and without controls for in-school work experience. I begin with an orthodox specification that omits in-school work experience from the covariates, and I contend with ability bias by using a variety of techniques proposed in the literature. Depending on which technique is used, the estimated wage benefit of 4 years of school ranges from 30% to 60%. The estimated ability bias—defined as the percent change in the estimated schooling coefficient when the endogeneity of schooling is ignored—ranges from −37% to 31%. When I reestimate each model after adding measures of in-school work experience, the estimated schooling coefficient declines substantially. In this second series of specifications, the estimated wage effect of 4 years of school (when in-school work experience is zero) ranges from 21% to 43%. The schooling coefficients in the series of specifications that exclude controls for in-school work experience are 25%–44% larger than these values. In light of this finding, I caution against interpreting estimates from specifications that ignore in-school work experience as the causal return to "school only," for they dramatically overstate that effect. At the same time, such specifications understate by 4%–20% the gap in starting wages between a college graduate and high-school graduate who begin their careers with the mean amount of in-school work experience for their schooling level.

My findings should come as no surprise, for the prevalence of student employment and its positive relationship to postschool wages are well documented. Coleman (1984); Michael and Tuma (1984); Ahituv, Tienda, Xu, and Hotz (1994); and Light (1998) are among the studies that describe the timing and extent of youth employment, while Ehrenberg and Sherman (1987) document the job-holding behavior of college students. These studies reveal that school-to-work transitions are far more heterogeneous than the "harvesting" model suggests and that a large segment of the population enters the labor market prior to completing formal schooling. Of course, there would be no need to control for in-school work experience in wage models if it was not skill enhancing, but an extensive literature suggests that it is. Meyer and Wise (1982), Coleman (1984), Ruhm (1995, 1997), and Light (1999) are among the studies that examine the link between high-school employment and subsequent earnings.

These studies differ in whether and how they contend with the endogeneity of in-school work experience, but each concludes that student employment has a subsequent labor market payoff. Compelling counter-evidence appears in a recent study by Hotz, Xu, Tienda, and Ahituv (1998), who find that the positive relationship between in-school employment and subsequent wages is eliminated when unobserved factors are taken into account. Hotz et al. (1998) conclude that "school only" often has a bigger impact on wages than school combined with work. Despite the disagreement on whether in-school employment "pays," the literature consistently indicates that efforts to identify the causal effect of schooling will be confounded by unmeasured in-school work experience, just as they are confounded by unobserved ability.[3]

In interpreting my results, a primary concern is whether mismeasurement of schooling accounts for the decrease in the schooling coefficient when measures of in-school work experience are added as covariates. In-school experience and schooling are highly correlated so, to paraphrase Griliches (1977), this may be a case of "killing the patient" in an attempt to cure the problem of omitted variable bias. Measurement error is likely to be less severe in my sample than in cross-sectional data because I use the wealth of schooling-related information in the NLSY to reconcile inconsistencies in self-reported schooling attainment. Nonetheless, I make alternative assumptions about the degree of remaining error and assess its impact on my results. I find that if 5% of the variation in schooling is due to random error, then the percentage decline in the estimated schooling coefficient resulting from the inclusion of in-school work experience falls by roughly one-third. This finding does not reverse my contention that orthodox estimates of the return to schooling reflect the skill-enhancing effects of classroom training plus job skills acquired along the way.

II. The Decision to Work While in School

An individual may choose to enter the labor market prior to leaving school for many reasons. He may need money to pay for additional schooling or to finance current consumption. Alternatively, or perhaps additionally, he may view employment as a chance to invest in income-enhancing skills not provided in the classroom.[4] In addition to acquiring

---

[3] Hotz et al. (1998) treat the endogeneity of in-school experience more formally than other studies, but their estimation procedure only allows them to distinguish between students who work during a given year and students who do not work at all. Their finding that student employment has no labor market payoff may be due to their inability to control for the intensity of work effort.

[4] Hanoch (1967) and the studies assessed by Parsons (1974) are among the first to consider empirically the role of in-school employment. These studies view

marketable skills, student workers are likely to gain a sense of responsibility and improve their job search and interpersonal skills, all of which may have a payoff during their postschool careers. In many cases, work experience may complement the student's formal classroom education—for example, a student of restaurant management will invariably find that experience gained working in a restaurant enhances his school work, and he may even be required to hold a job as part of his schooling.

As easy as it is to explain informally the decision to combine school and work, extending familiar models of schooling investment to incorporate the employment option is far from trivial. One approach suggested by Haley (1973) involves modifying Ben-Porath's (1967) model of optimal life cycle human capital accumulation. In the Ben-Porath model, there is no inherent difference between school and work—"school" is simply the name for the initial phase of the life cycle when an individual forgoes earnings in order to invest full-time in skill acquisition. When a sufficient stock of human capital is acquired, complete specialization becomes undesirable. At that point, the individual leaves school and divides his effort between earning and undertaking additional human capital investments in the workplace.

Haley (1973) observes that the time when the individual stops specializing in human capital production need not coincide with his departure from school. Instead, the individual may choose to decrease his investment intensity by cutting back on his classroom time and adding a part-time job. A similar revision of the Ben-Porath model is developed by Southwick and Zionts (1974) who derive an optimal human capital investment path in which a period of part-time schooling is chosen as the bridge between full-time schooling and no schooling. The idea behind the extensions of Haley and Southwick and Zionts was recognized by Becker (1962), who notes that "a sharp distinction between schools and firms is not always necessary: for some purposes schools can be treated as a special kind of firm and students as a special kind of trainee" (p. 26).

The models presented by Haley (1973) and Southwick and Zionts (1974) demonstrate that the school-plus-work option can be derived from standard human capital theory. However, these models are limited in their ability to inform empirical analyses because they assume students take jobs to decrease their investment intensity. The models do not recognize that students might take jobs because work experience provides them with different skills than classroom training. I believe an approach that allows for heterogeneous human capital (along the lines of Willis 1986) will conform to observed behavior more closely than existing

---

student employment strictly as a means of offsetting the direct costs of school and not as a skill-enhancing activity.

models and will be better able to justify my empirical assessment of the separate wage returns to schooling and in-school work experience. We can assume individuals allocate their time between acquiring school skills and job skills on the basis of their "school ability" and "work ability." Although discount rates (tastes, access to funds, etc.) and potential complementarities in the investment processes will come into play, individuals may be driven to combine school and work simply because they lack a strong comparative advantage in acquiring one type of skill or the other. This approach implies that the same person-specific factors that influence the familiar "school versus work" decision (ability, tastes, and access to funds) also play a role in the decision to combine school and work. As such, it suggests that the same methods used to assess the relationships between schooling, earnings, and ability can be extended in a straightforward manner to include in-school work experience.[5]

III. Estimation and Data Issues

A. Wage Model

In the previous section I suggest that students might choose to work in order to acquire marketable skills that are different than the skills gained through classroom study. With that view in mind, standard empirical wage functions can be easily extended to incorporate the role of in-school work experience. Just as we typically regress log wages on schooling and postschool labor market experience to assess the labor market value of in-school and postschool skills, I can add controls for in-school work experience to identify the value of skills acquired by students outside the classroom; I can interact in-school work experience with schooling to allow for complementarities in these alternative skill investments. Moreover, I can treat schooling and in-school experience similarly when dealing with ability bias, for these factors have similar influences on both the decision to attend school and the decision to work while in school.

To incorporate these extensions, I estimate the wage equation

$$W_{it} = \gamma_0 + \gamma_S S_i + \gamma_1 X_{it} + \gamma_2 SX_i + \gamma_3 SX_i \times S_i + \eta_{it}, \tag{1}$$

where W_{it} is the natural logarithm of the average hourly wage earned by individual i at time t (following school exit), S_i is years of schooling completed by i, SX_i is years of in-school work experience, and X_{it} is years of postschool work experience. In estimating equation (1), I also control for SX_i^2, X_{it}^2, and a number of additional, standard regressors. It

---

[5] A simple income-maximizing model of the joint school-work investment decision appears in an unpublished version of this article, which is available on request.

would be trivial to estimate equation (1) and obtain estimates of the causal effects of S_i and SX_i if the unobserved factors that affect wages (η_{it}) did not include ability, access to funds, and other characteristics that influence i's choice of S and SX. Because these factors are difficult to observe, the familiar problem of ability bias arises not only with respect to the causal effect of schooling but also with respect to in-school work experience. As I explain in detail below, I contend with these biases by using several alternative strategies that have been proposed in the literature.

In order to compare the estimated return to schooling implied by equation (1) to a more conventional estimate, I use the following wage model:

$$W_{it} = \beta_0 + \beta_S S_i + \beta_1 X_{it} + \eta_{it}, \tag{2}$$

where X_{it}^2, and the additional controls included in equation (1) are also among the covariates. In estimating equation (2), I use the same strategies applied to the estimation of equation (1) to correct for ability bias in the schooling coefficient. For each estimation method used, I obtain a pair of estimators $\hat{\gamma}_S$ and $\hat{\beta}_S$—the former from equation (1) that includes in-school work experience, and the latter from the more conventional equation (2) that excludes SX_i. For each pair of estimators, I use $100(\hat{\beta}_S - \hat{\gamma}_S)/\hat{\gamma}_S$ to assess the magnitude of the change in the estimated schooling coefficient when in-school work experience is omitted from the model. I refer to this percent change as the "omitted in-school experience bias" inherent in equation (2).

The expression defined above does not identify a "bias" in the traditional sense, but I argue that each $\hat{\beta}_S$ is a potentially misleading estimate of the return to "school only" by virtue of the exclusion of SX_i from equation (2). However, parameter estimates like $\hat{\beta}_S$ are typically presented as unbiased estimates of the causal effect of schooling; as I discuss in the introduction, most analysts appear to interpret this effect as the return to "school only"—that is, the return to skill investments undertaken while enrolled in school. It is common practice to compare $\hat{\beta}_S$ to the estimator (call it $\hat{\beta}'_S$) obtained by estimating equation (2) without controlling for unobserved ability, tastes, and so forth. For each $\hat{\beta}_S$ I obtain, I compute this ability bias as $100(\hat{\beta}'_S - \hat{\beta}_S)/\hat{\beta}_S$ and compare it to my estimate of omitted in-school experience bias. Griliches (1977) warns that such measures of ability bias can be misleading, for $\hat{\beta}_S$ varies as sample characteristics and/or model specifications change. I am not claiming there is an absolute ability bias or an absolute in-school experience bias—and, in fact, I demonstrate the extent to which these biases are sensitive to the proxy or instrumental variables method used for estimation. Nonetheless, I believe the comparisons made for my particular sample are illuminating.

B. NLSY Data

Equations (1) and (2) are estimated with data from the National Longitudinal Survey of Youth. The NLSY began in 1979 with a sample of 12,686 men and women born between 1957 and 1964; respondents were interviewed annually from 1979 to 1994 and biennially thereafter. To obtain the sample used for my empirical analysis, I impose the following selection rules. First, I eliminate the 6,283 female respondents in the original sample to skirt the special issues surrounding the labor market experiences of women and also for comparability with most existing studies of the returns to schooling. Second, I eliminate the 4,141 men who were not born in 1962–64. The NLSY provides a week-by-week event history of each respondent's employment experiences (labor market status, usual hours worked, etc.) from the first week of 1978 onward, so this stringent selection rule allows me to measure in-school work experience from each respondent's sixteenth birthday to his date of school exit. Third, I eliminate 150 respondents who leave school before their sixteenth birthday or who fail to leave school prior to their last interview date, and, fourth, I eliminate 35 men who do not report a spell of paid employment after leaving school. Together, these four selection rules produce a sample of 2,077 men.[6]

Each young man in my sample is observed from his sixteenth birthday until the date of his 1994 interview or an earlier date if he leaves the survey prior to 1994. A key step in the construction of variables for the wage models is to determine where to "draw the line" between each respondent's in-school and postschool activities. Because schooling attainment and enrollment status might be reported with error at any given interview, I use virtually all the schooling data available in the NLSY to reconcile inconsistencies in each respondent's schooling history. During each annual interview, respondents are asked if they attended "regular" school since the last interview, which is defined as attendance at an elementary school, middle school, high school, or college that can be counted toward a diploma or degree. Respondents who answer affirmatively are asked a number of questions about their dates of enrollment, grades attended, and dates/types of diplomas and degrees received. In addition, the high schools attended by over 75% of my sample members reported their high-school exit dates and graduation status as part of a 1980 mail-in school survey. I use this information to determine precisely

---

[6] The full sample of 6,403 male respondents in the NLSY consists of a nationally representative cross section ($N = 3,003$), a supplemental sample of blacks, Hispanics, and economically disadvantaged nonblack, non-Hispanics ($N = 2,576$), and a group of military enlistees ($N = 824$). Members of the military subsample were among the older respondents, so my first selection criterion eliminates all of them.

Table 1
Summary of In-School Work Experience, by Schooling Level

	Schooling Level				
	<12	12	13–15	≥16	All
Years of work experience from age 16 to school exit (SX)*:					
Mean	.380	.723	1.802	2.825	1.126
Standard deviation	(.62)	(.61)	(1.37)	(1.82)	(1.39)
Median	.154	.605	1.560	2.580	.695
Fraction equal to zero	.282	.075	.015	.000	.109
Years elapsed from age 16 to school exit:					
Mean	1.577	2.714	4.792	7.710	3.506
Standard deviation	(1.09)	(.75)	(1.59)	(1.69)	(2.37)
Average hours per week worked from age 16 to school exit:					
Mean	9.345	10.921	14.598	14.247	11.599
Standard deviation	(11.06)	(9.00)	(8.69)	(7.47)	(9.56)
Median	5.560	9.282	14.252	13.723	10.036
Number of individuals	577	867	377	316	2,077
Fraction of 2,077	(.278)	(.417)	(.181)	(.152)	

* Cumulative hours worked divided by 2,000.

when each respondent is enrolled in school and what his schooling attainment is at all times. I then define the date of school exit as the first month that he leaves school for a period of 12 months or longer.[7]

Having determined when each respondent leaves school, I define in-school work experience (SX) as the number of cumulative hours worked from the week of his sixteenth birthday to the midpoint of the month corresponding to his exit from school. This variable is divided by 2,000 to convert it to a full-time, full-year equivalent. I define schooling attainment (S) as the highest grade completed at the time of school exit.

Table 1 summarizes the distributions of schooling and in-school experience. Among the 2,077 men in my sample, the mean level of in-school experience is 1.1 years or about 2,200 hours. High school dropouts ($S < 12$) gain barely more than one-third of a year of experience during the relatively short time between their sixteenth birthdays and their exits from school, while college graduates ($S \geq 16$) accumulate more than 2.8 years of experience or 5,600 hours. The fraction of individuals acquiring some amount of work experience before leaving school is also closely tied to completed schooling levels: 28% of high-school dropouts leave school without having been employed in the labor market, but not a single college graduate in the sample does so. As one would expect, the positive

[7] In Light (1998) I assess the extent to which estimated schooling coefficients are sensitive to where one "draws the line."

relationship between in-school work experience and schooling is largely due to the fact that the interval over which experience is measured necessarily lengthens with schooling attainment. Table 1 reveals that college graduates have an average of 7.7 years during which they can combine school and work, which is considerably longer than the average interval length for their less schooled counterparts. At the same time, the tendency to increase average weekly work effort with age also contributes to the positive relationship between in-school work experience and schooling. High-school dropouts average about 9 hours of work effort per week while they are in school, while high school graduates average almost 11 hours per week, and men who attend college average over 14 hours per week. In summary, table 1 demonstrates that many young men acquire a substantial amount of in-school work experience and that their cumulative experience is highly correlated with schooling attainment.

In constructing the postschool variables used to estimate equations (1) and (2), I allow individuals to contribute one observation for every wage reported between their dates of school exit and last interview.[8] During each annual interview, NLSY respondents describe their earnings, weekly work effort, and other characteristics for their current job(s), if any, and for each job held since their last interview. Hence, continuously employed respondents report a minimum of one wage per interview, and mobile workers report at least one wage per job; the 2,077 respondents in my sample report 20,788 wage observations for 15,889 jobs. Although respondents report wages in units of their choosing, the NLSY computes average, hourly wages for each observation. The dependent variable used for equations (1) and (2) is the natural logarithm of these computed hourly wages, deflated by the consumer price index in 1986 dollars.

For most of the analysis, I define postschool experience (X) as cumulative hours worked between the week following school exit and the week when the reported wage was earned; I divide this measure by 2,000 to convert it to a full-time, full-year equivalent. Because much of the previous research on causal effects of schooling lacks data on actual work experience, I also estimate versions of equations (1) and (2) using a measure of potential work experience for X. In these alternative specifications, I define X as the respondent's age at time t minus $S + 6$.

---

[8] Most evidence on the causal effect of schooling comes from wage or earnings models estimated with cross-sectional data. Because the respondents in my sample were all born in 1962–64, there would be relatively little variation in postschool work experience if I were to confine the wage data to a cross section. Thus, I use all available postschool data to improve the comparability between my analysis and the literature. As it turns out, the generalized squares estimators presented in the next section are similar to what I obtain using ordinary least squares for a cross section drawn from one of the later (1991–94) interview years.

Table 2
Definitions and Summary Statistics for Selected Variables Used in Wage Models

Variable Name	Definition	Mean (Standard Deviation)
Dependent variable:		
Wage	Log of CPI-deflated average hourly wage (1986 dollars)	1.824 (.502)
Selected regressors:		
Schooling (S)	Highest grade completed	12.167 (1.990)
Actual experience (X)	Years of work experience since school exit*	4.730 (3.493)
X^2		34.578 (44.419)
Potential experience	Age $- S - 6$	6.201 (3.520)
Potential experience squared		50.844 (47.456)
In-school work experience (SX)	Years of work experience from age 16 to school exit*	1.010 (1.174)
SX^2		2.399 (6.265)
$SX \times S$		13.770 (18.342)
School ability test score (A_S)	Sum of raw scores for academic components of ASVAB	69.865 (26.314)
Work ability test score (A_W)	Sum of raw scores for nonacademic components of ASVAB	100.279 (32.681)
Number of observations		20,788
Number of individuals		2,077

NOTE.—ASVAB = Armed Services Vocational Aptitude Battery.
* Cumulative hours worked divided by 2,000.

Schooling, in-school experience, and postschool experience are the key explanatory variables used in the wage models; the means and standard deviations for these variables, plus their higher-order terms and the dependent variable, are reported in table 2. In addition to the regressors shown in table 2, a uniform set of baseline controls is included in every specification. These include dummy variables indicating whether the respondent is black, Hispanic, working part-time (less than 35 hours/week), unionized, working in the public sector, and residing in the South. They also include continuous measures of the unemployment rate and percent of the population that is urban in the respondent's county of residence at time t, plus a set of dummy variables indicating the calendar year (1978–94) in which the wage was earned and an additional dummy variable indicating that the respondent's union status is un-

known. Summary statistics for these regressors are reported in appendix table A1.[9]

C. Estimation Procedures

In estimating wage equations (1) and (2), I use three proxy and three instrumental variables (IV) methods to control for ability bias in the schooling coefficient. I use a range of procedures—each of which has been used elsewhere in the literature—to demonstrate that my estimate of omitted in-school experience bias is not very sensitive to how the causal effect of schooling is estimated. The literature does not propose as many solutions for identifying the causal effect of in-school work experience; in estimating equation (1) I use two methods proposed by Ruhm (1997) and Light (1999) for handling the endogeneity of in-school work experience.

My first approach to correcting the estimated schooling coefficient for ability bias is to add a set of family background controls. These controls, which are summarized in table A1, include per capita family income in 1979, father's and mother's highest grade completed, number of siblings, and dummy variables indicating religion (Baptist, Catholic, or Jewish), whether the respondent is foreign born, household composition at age 14 (both parents present or mother only present), and whether any member of the respondent's household at age 14 regularly received magazines or newspapers or had a library card. In addition, I include three dummy variables to indicate when family income, mother's schooling, and father's schooling are not reported; I set the variable equal to the sample mean in these cases. Including family background measures is not among the most common ways to contend with ability (or family background) bias, but Griliches and Mason (1972) and Lam and Schoeni (1993) use limited sets of background variables for this purpose. The rationale is that an array of background measures might absorb some of the sample heterogeneity in tastes for schooling, individual ability, and/or access to funds.

My second method for contending with ability bias is to include two test scores as regressors, and, as a third approach, I include the test scores along with the array of family background controls. In the summer of 1980, the Armed Services Vocational Aptitude Battery was administered to NLSY respondents. I use the scores from the 10 components of this test to construct measures of "school ability" and "work ability." The former is the sum of the raw scores for the general science, arithmetic

---

[9] My estimated coefficients for schooling and in-school experience are not unduly sensitive to the choice of covariates. For example, the addition of a tenure variable or the replacement of year dummies with a wage index changes the estimated coefficients for postschool work experience but does not affect the key coefficients significantly. Similarly, the exclusion of government jobs affects the estimated intercept but not the coefficients of interest.

reasoning, word knowledge, paragraph comprehension, and mathematics knowledge tests. The "work ability" score is obtained by summing raw scores for the numerical operations, coding speed, auto and shop information, mechanical comprehension, and electronics information tests. Table 2 reports the means and standard deviations for these two composite test scores. Test scores have been used as proxies for ability by Griliches and Mason (1972), Griliches (1977), and Blackburn and Neumark (1993, 1995), among others.

In the first IV method, I include the two ability measures among the covariates and use the set of family background variables as instruments for schooling and ability. A similar approach is used by Griliches and Mason (1972), Griliches (1977), and Blackburn and Neumark (1993, 1995). These authors argue that both schooling and observed ability will continue to be correlated with unobserved factors if test scores are imperfect proxies for the true ability affecting schooling decisions. Family background variables explain much of the variation in observed schooling attainment, presumably because they reflect heterogeneity in tastes, access to funds, and other factors that affect the demand for schooling.

My second IV procedure involves using sibling composition and sibling schooling attainment as instrumental variables for schooling. The instruments are the respondent's number of siblings and its square, dummy variables indicating whether he has an older brother and whether he has an older sister, and the highest grade completed by his oldest sibling (which is set to zero if he has no older sibling). Butcher and Case (1994) present evidence that women who have sisters receive less schooling than do women with only brothers, holding family size constant. Because sibling composition is unlikely to be related to unobserved factors that explain wages, they argue that it is a valid instrument for women's schooling. Although they do not find a significant correlation between sibling composition and men's schooling attainment, my sample reveals a slight, negative correlation between highest grade completed and the presence of older sisters. I also find a pronounced, negative relationship between highest grade completed and oldest siblings' schooling attainments, perhaps because family resources are substituted from one sibling to another. Following Butcher and Case (1994), I also include the array of family background controls described above (excluding the number of siblings) as covariates.

My final IV method uses characteristics of respondents' environments at age 17 as instruments for schooling. These include the average tuition for all public, postsecondary institutions in the respondent's state of residence, the percent of the population with a college education, the percent of the population living in an urban area, the unemployment rate, and the ratio of population to land area; the latter four variables are all

based on the respondent's county of residence.[10] The tuition variable is similar to an instrumental variable used by Kane and Rouse (1995), while the measures of countywide population density, urbanization, and college-going behavior are similar to the measures of "distance to nearest college" used by Card (1995*b*) and Kane and Rouse (1995). All five variables help explain observed schooling levels insofar as they reflect college-going costs.

In the specification that includes controls for in-school work experience (SX), I am concerned with ability bias in the estimated SX coefficient as well as the schooling coefficient. Following Ruhm (1997), I argue that the inclusion of family background measures and test scores absorbs much of the heterogeneity in ability, access to funds, and other factors that affect the decision to work while in school. Thus, the proxy methods described above are potentially valid ways to control for ability bias in both schooling and in-school experience. In addition, I use an alternative, IV approach similar to ones used by Ruhm (1997) and Light (1999). As instrumental variables for SX, I use a dummy variable indicating whether the respondent's high school offers a distributive education program designed to combine classroom training with workplace exposure and another dummy indicating whether this information, which is obtained through the NLSY survey of high schools, is missing. I also use three measures of the health of the respondent's local labor market (county of residence) while he is in school: the unemployment rate, the percent of the population that is urban, and the median, per capita family income. In constructing the latter three variables, which are generally known on an annual basis, I compute the average value during the respondent's last 4 years of school.

Because my data contain multiple observations for each respondent (see n. 8), I estimate equations (1) and (2) using feasible generalized least squares (GLS). I assume the residual (η_{it}) is the sum of two components, α_i and ε_{it}, both of which are mean zero, random variables with constant variances σ_α^2 and σ_ε^2. The time-varying error component (ε_{it}) is assumed to be white noise, whereas the time-constant individual effect (α_i) contains factors such as innate ability and, as discussed, is likely to be correlated with S_i and SX_i. The instrumental variables methods discussed above are obtained via two-stage, GLS estimation; I refer to the resulting estimators as IV/GLS estimators.

---

[10] With the exception of tuition data, these variables are contained in the NLSY geocode file. I obtain state-by-year tuition data from Halstead (1993).

IV. Findings

Selected GLS and IV/GLS estimates from alternative versions of equa-
tion (2), which excludes controls for in-school work experience, are
presented in table 3; additional parameter estimates appear in appendix
table A1. In estimating the column a version of equation (2), I ignore
potential correlations between respondents' schooling levels and unob-
served factors that affect wages—I simply regress log wages on schooling,
actual experience and its square, and the additional controls listed in table
A1. The estimated schooling coefficient is 0.096, which implies that an
additional 4 years of school raises starting wages by 38.5%. The estimated
coefficients for X and X^2 indicate that 5 years of full-time, year-round,
postschool work effort lead to 33.7% wage growth. The column a' model
is identical to a except I replace actual experience with "age minus
schooling minus 6." This substitution causes the estimated effect of
experience to decline slightly (in keeping with the fact that it is now
measured with considerable error) but has no effect on the estimated
schooling coefficient.

Most analysts would argue that the estimated schooling coefficient
reported in column a of table 3 does not represent the causal effect of
schooling because the specification fails to account for the relationship
between schooling and innate ability or family background. In column b
I control for family background characteristics, in column c I add two test
scores as proxies for innate ability, and in column d I control for both
family background and ability. Each successive change in the specification
causes the estimated intercept to increase relative to column $a;$ the expe-
rience coefficients to change slightly; and, most important, the estimated
schooling coefficient to decline. Taking specifications $b, c,$ and $d,$ in turn,
as the correct way to control for the endogeneity of schooling, I infer that
the column a model leads to an upward ability bias of 12%, 22%, and
31%, respectively; these computations are reported in the bottom row of
table 3.[11] For comparison with column $a',$ I reestimate the column d
model after replacing actual experience with potential experience. The
estimates for this version appear in column d' and indicate that failure to
control for ability and family background causes a 50% upward bias in
the estimated schooling coefficient when an error-ridden experience mea-
sure is used.

---

[11] Other analysts obtain estimates that are qualitatively similar to those re-
ported in cols. a–$d.$ For example, Blackburn and Neumark (1995) use a cross
section from the NLSY to estimate wage models that are comparable to those
summarized in columns a and $c.$ Their estimated schooling coefficients (standard
errors) are 0.058 (.005) when test scores are omitted and 0.042 (.006) when they
are included, which implies an ability bias in the "uncorrected" version of 38% (in
contrast to the 22% I report).

Table 3
GLS and IV/GLS Estimates for Selected Coefficients from Wage Models That Exclude In-School Work Experience

	GLS						IV/GLS		
	a	a'	b	c	d	d'	e	f	g
Schooling (S)	.096	.096	.086	.079	.073	.064	.112	.153	.108
	(.004)	(.007)	(.004)	(.004)	(.005)	(.007)	(.023)	(.026)	(.052)
Experience (X)*	.080	.077	.080	.077	.077	.071	.056	.095	.084
	(.004)	(.007)	(.004)	(.004)	(.004)	(.007)	(.004)	(.007)	(.013)
$X^2/100$*	−.259	−.337	−.260	−.253	−.255	−.341	−.210	−.292	−.269
	(.022)	(.030)	(.022)	(.022)	(.022)	(.030)	(.022)	(.026)	(.035)
School ability score (A_S)/100	…	…	…	.096	.069	.023	.100	.048	.051
				(.045)	(.045)	(.046)	(.078)	(.038)	(.039)
Work ability score (A_S)/100	…	…	…	.140	.146	.233	.185	.096	.100
				(.034)	(.034)	(.035)	(.088)	(.040)	(.044)
Family background variables included	No	No	Yes	No	Yes	Yes	No	Yes	No
Endogenous variables†	…	…	…	…	…	…	A_S, A_W, S	S	S
P-value from Hausman test:‡									
A_S, A_W							.055		
S							.044	.029	.030
R^2 from first stage regression for S							.368	.111	.099
Estimated effect on log wage of 5 years of X	.337	.302	.337	.321	.324	.270	.231	.404	.354
	(.015)	(.029)	(.015)	(.015)	(.015)	(.029)	(.014)	(.032)	(.059)
Estimated effect on log wage of 4 years of S	.385	.384	.344	.315	.293	.256	.447	.614	.433
	(.016)	(.026)	(.017)	(.018)	(.019)	(.028)	(.092)	(.104)	(.207)
Ability bias in col. a S coefficient (%)§	…	…	12	22	31	50	−14	−37	−11

NOTE.—Standard errors are in parentheses. Additional estimates for selected specifications appear in table A1. The sample contains 20,788 observations.

* In columns a' and d', X is "age − S − 6." In all other columns, X is cumulative hours worked since school exit, divided by 2,000.

† The instrumental variables are family background measures (col. e), sibling composition and schooling (col. f) and college cost-related factors for the place of residence at age 17 (col. g). See text for details.

‡ Test of the null hypothesis that the variables listed as endogenous are uncorrelated with the error term.

§ The calculation is 100 (0.096 − β_S)/β_S where β_S is the estimated schooling coefficient for the particular column.

In columns e–g of table 3, I take an alternative approach to controlling for ability bias by using instrumental variables for schooling. In column e I use family background measures as instruments (for the test scores as well as schooling), in column f I use sibling-related variables, and in column g I use measures related to college-going costs. All three IV methods yield estimated schooling coefficients that are larger than what is seen in column a. Taking each, in turn, as an unbiased estimate of the causal effect of schooling, columns e–g imply that the column a estimate is biased downward by 14%, 37%, and 11%, respectively.

Given the large differences between the IV/GLS estimates in column e–g and the GLS estimates in columns a–$d,$ it is worth investigating the validity of the instrumental variables although, in general, my IV/GLS estimates are consistent with other analysts' findings.[12] The R^2 from each first stage regression for schooling is reported in table 3; these statistics indicate that each set of instrumental variables explains a sizable portion of the variation in schooling. I also report p-values for a test of endogeneity proposed by Hausman (1978). For all three IV specifications, I reject at a 5% significance level the null hypothesis that schooling is exogenous. Card (1995a) explains why an uncorrected estimate of the schooling coefficient (e.g., my column a estimate) might be smaller than an IV estimate (e.g., my column g estimate) in which the instrumental variables can be viewed as measuring interventions that cause individuals with high discount rates to increase their schooling levels. The IV estimates in this case identify the marginal returns to schooling for individuals affected by the interventions who, by virtue of having high discount rates, have marginal returns that exceed the population-wide returns identified by non-IV estimation techniques.

Thus far, I have used a variety of conventional methods to identify the causal effect of schooling in a model that ignores in-school work experience, and I have determined the amount by which each causal effect is over- or understated when ability bias is ignored. Table 3 reveals these estimates to vary considerably with the method used to correct for ability bias, but such discrepancies are to be expected; in general, my findings closely mirror what other analysts have reported. Having established these benchmarks, I now ask how the estimated schooling coefficients change when controls for in-school work experience are added to the

---

[12] As noted in Sec. III, Blackburn and Neumark (1995), Butcher and Case (1994), and Card (1995b) use IV methods that are comparable to the ones I use in cols. $e,$ $f,$ and $g,$ respectively. Blackburn and Neumark report ordinary least squares and IV estimates for the schooling coefficient equal to 0.042 (.006) and 0.096 (.073), respectively, with standard errors in parentheses. Butcher and Case report 0.091 (.007) and 0.182 (.055), and Card reports 0.073 (.004) and 0.132 (.049). The ability biases implied by these estimates are -56%, -50%, and -45%.

models. I repeat the estimation underlying table 3 after adding three regressors to each model: in-school work experience (SX), the square of SX, and an interaction between SX and schooling ($SX \times S$). As detailed in Section III, I contend with potential correlation between SX and unobserved factors by using proxy and IV methods that parallel those used to treat the endogeneity of schooling.

Estimates for the specification that includes in-school work experience are reported in tables 4 and A1. When each estimated schooling coefficient in table 4 is compared to its counterpart in table 3, a clear contrast emerges: the addition of in-school work experience leads to a dramatic decline in the estimated schooling effect, regardless of which method is used to estimate the model. In the column a model, for example, the estimated schooling coefficient falls from 0.096 to 0.075. The latter estimate implies that an individual who leaves school with $S = 16$ and $SX = 0$ begins his postschool career earning 29.8% more than an individual for whom $S = 12$ and $SX = 0$. The former (table 3) estimate indicates that this wage gap is 38.5%, or 29% higher. For the column a model, I conclude that omitted in-school experience bias is 29%.

When I introduce various controls for ability bias in subsequent columns of table 4, the estimate of in-school experience bias generally increases, reaching 44% in column f and 55% in column d'. It is important to recognize that this bias, which I report in the second to last row of table 4, is simply the percent by which the estimated schooling coefficient in table 3 exceeds the corresponding estimate in table 4. If we interpret each schooling coefficient as the return to "school only," the bias is the amount by which we overstate this return by failing to account for the wage effects of in-school work experience. Whereas the estimated schooling coefficients in tables 3 and 4 and the estimates of ability bias are highly sensitive to the estimation method used, the estimates of in-school experience bias are fairly robust. Moreover, they are always larger than the absolute value of the corresponding estimate of ability bias. For example, model d', which yields the largest estimate of ability bias of any model at 50%, yields a 55% estimate of in-school experience bias. If we use the orthodox models underlying table 3 to assess the return to "school only," the bias due to omitting in-school work experience is at least as large as the bias due to ignoring unobserved factors (such as ability) that affect schooling.

Failure to control for in-school work experience causes the estimated schooling coefficients to increase in magnitude because student employment has a nontrivial effect on postschool wages. The GLS estimates in table 4 indicate that an individual who accumulates 2 years of work experience while completing 16 years of school begins his postschool career earning about 10% more than his counterpart who gains no in-school experience. The IV/GLS estimates in table 4, which use mea-

sures of labor market health as instruments for in-school experience, imply that this wage boost is 14%–18%.[13]

Conventional estimates of the causal effect of schooling reflect the return to school plus the return to work experience gained while in school. I have shown that these estimates are considerably higher than the estimated return to "school only" identified by a model that controls separately for in-school work experience. I now compare them to the estimated return to "school plus average in-school work." Specifically, I use the estimates reported in table 4 to compute the difference in the predicted log wage for an individual who leaves school with $S = 16$ and $SX = 2.8$ and the predicted log wage for an otherwise identical individual for whom $S = 12$ and $SX = 0.7$; these are the mean levels of SX for sample members in the $S = 16+$ and $S = 12$ schooling categories. For the GLS estimates (cols. a–d) in table 4, the estimated return to 4 years of school combined with mean levels of in-school work experience is 10–15 percentage points higher than the return to 4 years of school only. In column a, for example, the former estimate is 0.421, while the latter is 0.298. The IV/GLS estimates in columns e–g of table 4 imply returns to school-plus-work that are 17–22 percentage points higher than the estimated returns to school only. Using the column e specification, the estimated return to 4 years of "school only" is 0.342, while the estimated return to 4 years of school combined with mean levels of in-school work experience is 0.530.

The estimated returns to schooling shown in table 3 are 4%–20% lower than the returns to school-plus-work reported in table 4. In column e of table 3, for example, 4 years of schooling is estimated to increase log wages by 0.447; this estimated return is 16% smaller than the 0.530 increase due to school-plus-work reported in column e of table 4. (I report this alternative formulation of "omitted in-school experience bias" in the bottom row of table 4.) I have already shown that the orthodox wage models underlying table 3 necessarily overstate the return to "school only" because their schooling coefficients absorb the wage effects of in-school work experience. The computations reported here reveal that they understate slightly the wage benefit associated with incrementing work experience from 0.7 years to 2.8 years while completing 4 years of college.

The conclusions I have drawn are based on the assumption that schooling is not measured with error. If my schooling variable contains a

---

[13] The Hausman test p-values reported in table 4 exceed 0.2, which suggests there is no need to treat in-school experience as endogenous. Nonetheless, the SX-related estimates appear to be reasonably well identified and are consistent with Ruhm's (1997) and Light's (1999) findings that IV estimates of the returns to student employment exceed non-IV estimates.

Table 4
GLS and IV/GLS Estimates for Selected Coefficients from Wage Models That Include In-School Work Experience

	GLS						IV/GLS		
	a	a'	b	c	d	d'	e	f	g
Schooling (S)	.075	.072	.066	.055	.052	.041	.086	.107	.080
	(.007)	(.009)	(.007)	(.008)	(.008)	(.010)	(.030)	(.029)	(.049)
Experience (X)*	.075	.061	.076	.072	.072	.056	.058	.087	.084
	(.004)	(.007)	(.004)	(.004)	(.004)	(.007)	(.003)	(.006)	(.010)
$X^2/100$*	−.250	−.265	−.251	−.244	−.245	−.270	−.216	−.278	−.271
	(.022)	(.032)	(.022)	(.022)	(.022)	(.032)	(.020)	(.022)	(.029)
In-school experience (SX)	.026	.042	.018	.024	.020	.025	.030	.027	.034
	(.003)	(.004)	(.005)	(.004)	(.005)	(.006)	(.011)	(.010)	(.012)
$SX^2/100$	−.134	−.162	−.938	−.131	−.155	−.272	−.169	−.171	−.165
	(.213)	(.242)	(.244)	(.258)	(.226)	(.256)	(.301)	(.256)	(.267)
$SX \times S$	.002	.002	.003	.002	.002	.003	.004	.003	.004
	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)	(.005)	(.005)	(.005)
School ability score (A_S)/100				.069	.044	.070	.088	.049	.052
				(.047)	(.048)	(.050)	(.080)	(.038)	(.041)
Work ability score (A_S)/100				.180	.185	.264	.169	.090	.097
				(.037)	(.037)	(.038)	(.091)	(.045)	(.046)
Family background variables included	No	No	Yes	No	Yes	Yes	No	Yes	No
Endogenous variables†	…	…	…	…	…	…	A_S, A_W, S, SX	S, SX	S, S
P-value from Hausman test:‡	…	…	…	…	…	…			
A_S, A_W							.063		
S							.049	.040	.039
SX							.211	.271	.210

84

R^2 from first stage regression for SX							.089	.089	.089
Estimated effect on log wage of 5 years of X	.314 (.017)	.239 (.031)	.315 (.017)	.310 (.013)	.301 (.017)	.210 (.031)	.234 (.013)	.365 (.024)	.351 (.046)
Estimated effect on log wage of 4 years of S evaluated at:									
$\quad SX = 0$	.298 (.029)	.288 (.037)	.265 (.030)	.252 (.015)	.208 (.031)	.165 (.038)	.342 (.112)	.426 (.118)	.325 (.195)
$\quad SX$ = sample mean§	.421 (.125)	.437 (.143)	.360 (.123)	.384 (.126)	.311 (.131)	.295 (.153)	.530 (.283)	.596 (.218)	.544 (.356)
Omitted in-school experience bias evaluated at (%):‖									
$\quad SX = 0$	29	33	30	25	41	55	31	44	33
$\quad SX$ = sample mean	−9	−12	−4	−18	−6	−11	−16	−3	−20

NOTE.—Standard errors are in parentheses. Additional estimates for selected specifications appear in table A1. The sample contains 20,788 observations.

  * In columns a' and d', X is "age − S − 6." In all other columns, X is cumulative hours worked since school exit, divided by 2,000.

  † The instrumental variables for SX are the unemployment rate, median family income, and percent of population that is urban in the respondent's county of residence during his last 4 years of school, plus a dummy variable indicating whether his high school offers a distributive education program. Instruments for the remaining endogenous regressors are summarized in n. † of table 3. See text for details.

  ‡ Test of the null hypothesis that the variables listed as endogenous are uncorrelated with the error term.

  § Estimated effect on the log wage of incrementing S from 12 to 16 and incrementing SX from 0.7 to 2.8.

  ‖ The calculation is $100(4\hat{\beta}_S − 4\hat{\gamma}_{YS})/4\hat{\gamma}_{YS}$, where $4\hat{\beta}_S$ is the estimated return to 4 years of schooling shown in table 3, and $4\hat{\gamma}_{YS}$ is the estimated return shown in the preceding rows of this table.

85

substantial amount of classical measurement error, its estimated effect on log wages would necessarily decrease when I add controls for in-school work experience. In-school experience and schooling are highly correlated, so I would be introducing a downward bias in the estimates in table 4 that, depending on the extent of the correlation and measurement error, may exceed (in absolute magnitude) what I term the (upward) omitted variables bias in the table 3 estimates.[14] More precisely, I am arguing that in the absence of measurement error the estimated schooling coefficients reported in table 4 have the property plim $\hat{\gamma}_S = \gamma_S$, where γ_S is the true, causal effect of schooling described by equation (1); the estimated schooling coefficients reported in table 3 exceed $\hat{\gamma}_S$ because of omitted in-school experience bias. If S is measured with error, then—abstracting from the presence of covariates other than S and SX—plim $\hat{\gamma}_S = \gamma_S - \lambda_S \gamma_S (1 - R^2_{S \times SX})$, where λ_S is the fraction of the variance of observed S that is due to measurement error and $R^2_{S \times SX}$ is the R^2 from a regression of schooling on in-school work experience. It could be the case that the estimated schooling coefficients in table 3 exceed those in table 4 because the latter estimates are biased downward by measurement error in reported schooling.

Measurement error bias is unlikely to be extreme in my analysis because my schooling variable is almost certain to have substantially less random error than variables drawn from cross-sectional surveys. As noted in Section III, I use all schooling-related data available in the NLSY to identify and correct apparent errors in each respondent's sequence of self-reports of schooling attainment. The cleaned schooling measure that I use throughout the analysis has a variance that is 19% less than the variance of the alternative measure obtained by simply using the highest grade completed reported by each respondent at the time of school exit.

Despite my efforts to exploit the longitudinal nature of the NLSY to reduce measurement error, it is worth asking how large the remaining error would have to be to explain the discrepancies between the GLS estimates in tables 3 and 4. The estimates I use for this investigation are from column d in table 3—that is, I include test scores and family background measures among the regressors but do not use instrumental variables. When I estimate the model without controls for in-school work experience, the estimated schooling coefficient is 0.073 (see table 3). When I add SX to the model (but not SX^2 or $SX \times S$), the estimated coefficient falls to 0.056. If I assume schooling and every other covariate are measured without error, I infer that the failure to control for in-school

---

[14] It is less clear how measurement error would affect IV estimates of the schooling coefficient, but the GLS estimates—which are very close to unreported OLS estimates, as noted in n. 8—would suffer from this problem.

experience produces an omitted variable bias of 30% (cf. 41% for the slightly different specification reported in table 4). These calculations are summarized in the top of table 5.

To obtain the estimates shown in the bottom of table 5, I make alternative assumptions about the degree of random error that remains in my schooling measure and "back out" estimates of omitted in-school experience bias after accounting for measurement error bias. If I assume that error accounts for, in turn, 1%, 5%, 10%, and 15% of the total variance in the schooling measure, I infer that the true schooling coefficient should range from 0.057 to 0.074, which, of course, is somewhat higher than my actual estimate of 0.056. Replacing my estimated schooling coefficient ($\hat{\gamma}_S$) with these estimates of the true coefficient, I calculate the true omitted in-school experience bias. These estimates range from 29% when $\lambda_S = 0.01$ to -0.7% when $\lambda_S = 0.15$. If as much as 15% of the variance of my schooling measure is due to noise, the decline in the schooling coefficient that comes about when I add controls for in-school work experience is wholly attributable to downward measurement error bias. If the ratio of noise to total variance is only 5%, however, I conclude that two-thirds of the estimated omitted in-school experience bias is "real," and the rest is due to measurement error bias. Five percent is likely to be an upward bound on the degree of measurement error in my data, given that 10% is often found to exist in uncleaned, cross-sectional data. If this assumption is correct, I conclude that adding controls for in-school experience to the wage model improves our ability to identify the causal wage effects of "school only."

V. Conclusions

Rather than make a clean transition from school to work, many young people gain substantial amounts of work experience before leaving school. This phenomenon is widely recognized and has received considerable attention from analysts seeking to learn whether student employment enhances or detracts from subsequent career outcomes. However, most analysts have no direct interest in the returns to in-school experience and do not control for it in their wage models. In the current study, I argue that the returns to "school only" are overestimated by wage models that fail to control for in-school work experience because estimated schooling coefficients represent the labor market returns to schooling plus work experience gained while in school. Just as unobserved ability leads to biased estimates of the causal effect of schooling, work experience gained while in school causes an "in-school experience bias" if left unmeasured.

To assess the magnitude of in-school experience bias—and compare it to more familiar estimates of ability bias—I estimate a series of wage models with data from the National Longitudinal Survey of Youth. I

Table 5
Effect of Measurement Error on the Estimated Bias Due to Omitting In-School Work Experience

Definition	Estimate
Coefficient for S when SX is excluded ($\hat{\beta}_S$)	.073
Coefficient for S when SX is included ($\hat{\gamma}_S$)	.056
Omitted in-school experience bias ($100(\hat{\beta}_S - \hat{\gamma}_S)/\hat{\gamma}_S$) (%)	30.4
R^2 from regression of S on SX	.385

Variance of S Due to Measurement Error (λ_S)	True Coefficient for $S(\gamma_S^*)$[a]	True Omitted In-School Experience Bias[b] (%)
.01	.057	29
.05	.061	21
.10	.067	10
.15	.074	−.7

[a] Calculated as $\hat{\gamma}_S(1 - R^2)/(1 - R^2 - \lambda_S)$.
[b] Calculated as $100(\hat{\beta}_S - \gamma_S^*)/\gamma_S^*$.

begin with an orthodox wage model that excludes measures of in-school work experience and use a range of procedures proposed in the literature to correct for ability bias. My estimates of ability bias range from -37% to 31%. I then reestimate each model after adding controls for in-school work experience; I continue to correct for correlations between schooling and unobserved factors such as ability, and I also treat in-school experience as endogenous. The new estimates reveal that school alone has a smaller effect on postschool wages than the orthodox models indicate, while student employment has an additional, positive effect. Models that fail to control for in-school work effort overstate the return to "school only" by 25%–44%. For each estimation procedure I use, my estimate of in-school experience bias is larger than the absolute value of the corresponding estimate of ability bias.

On the basis of these findings, analysts may wish to control for student employment in their wage or earnings models even if its labor market effect is not the focus of their research. Without controls for in-school work experience, it is incorrect to interpret estimated schooling coefficients as the return to skill acquired strictly via time spent in school. The difficulty, of course, is that relatively few data sources provide information on in-school work experience. The NLSY is ideally suited to measuring both in-school and postschool work experience because workers' employment histories are tracked in detail starting in 1978, when respondents were 14–21 years old. By confining my sample to the younger respondents in the survey, I am able to measure actual work experience from age 16 onward. Cross-sectional surveys and even longitudinal surveys that focus on older respondents are unable to collect similarly detailed data. However, they may be able to collect retrospective information on adult workers' cumulative, in-school work effort at relatively low cost. The reliability of such information would have to be assessed, but it may have considerable value in improving the specification and interpretation of earnings functions.

Appendix

Table A1

Summary Statistics and Estimates for Covariates Not Reported in Tables 3–4

Variable	Mean (SD)	Table 3				Table 4			
		a	d	d'	e	a	d	d'	e
Intercept	···	.353 (.060)	.297 (.079)	.328 (.119)	.322 (.060)	.594 (.094)	.548 (.106)	.631 (.142)	.592 (.093)
1 if black	.273	−.135 (.016)	−.025 (.021)	−.048 (.021)	−.063 (.018)	−.123 (.016)	−.022 (.020)	−.044 (.021)	−.053 (.018)
1 if Hispanic	.153	−.036 (.019)	−.016 (.023)	−.021 (.023)	−.001 (.020)	−.035 (.019)	−.016 (.023)	−.022 (.023)	−.000 (.019)
1 if work less than 35 hours per week	.175	−.058 (.007)	−.059 (.007)	−.069 (.007)	−.059 (.007)	−.059 (.007)	−.060 (.007)	−.070 (.007)	−.060 (.007)
1 if union worker	.121	.165 (.010)	.164 (.010)	.167 (.010)	.165 (.010)	.164 (.010)	.164 (.010)	.166 (.010)	.165 (.010)
1 if government worker	.051	.047 (.014)	.044 (.014)	.043 (.014)	.046 (.014)	.047 (.014)	.045 (.014)	.042 (.014)	.047 (.015)
1 if live in South	.358	−.015 (.011)	−.001 (.012)	.011 (.012)	−.009 (.011)	−.014 (.011)	−.001 (.012)	.010 (.012)	−.008 (.011)
Percent of labor market that is urban	79.230 (26.50)	.001 (.000)	.001 (.000)	.001 (.000)	.001 (.000)	.001 (.000)	.001 (.000)	.001 (.000)	.001 (.001)
Local unemployment rate	7.370 (3.01)	−.007 (.001)	−.007 (.001)	−.007 (.001)	−.007 (.001)	−.007 (.001)	−.007 (.001)	−.007 (.001)	−.007 (.001)
Family background: Per capita family income/ 1,000	3.618 (2.75)		.007 (.003)	.008 (.003)			.007 (.004)	.009 (.004)	
Father's highest grade completed	10.922 (3.39)		.003 (.003)	.003 (.003)			.003 (.002)	.003 (.003)	

	Mean (SD)	(1)	(2)	(3)	(4)	(5)	(6)
Mother's highest grade completed	10.829 (2.95)	−.002 (.003)	−.001 (.003)			−.002 (.003)	−.001 (.003)
Number of siblings	3.715 (2.52)	.001 (.003)	−.001 (.003)			−.001 (.003)	−.002 (.003)
1 if foreign born	.070	.108 (.027)	.087 (.028)			.095 (.027)	.074 (.028)
1 if Baptist	.265	.004 (.018)	.004 (.018)			.001 (.018)	.002 (.018)
1 if Catholic	.333	.032 (.017)	.032 (.017)			.028 (.017)	.029 (.017)
1 if Jewish	.005	.264 (.078)	.211 (.080)			.260 (.077)	.220 (.079)
1 if both parents present at age 14	.651	.003 (.028)	.014 (.029)			.001 (.028)	.012 (.028)
1 if mother only present at age 14	.268	−.011 (.030)	−.007 (.031)			−.017 (.030)	−.012 (.030)
1 if member of household at age 14: Received magazines regularly	.548	.023 (.015)	.032 (.016)			.028 (.015)	.035 (.016)
Received newspaper regularly	.713	−.017 (.017)	−.016 (.017)			−.017 (.016)	−.017 (.017)
Had a library card	.647	.048 (.015)	.038 (.016)			.042 (.015)	.034 (.015)
Root MSE	.359	.358	.361	.364	.358	.350	.360
σ^2_α	.063	.060	.063	.060	.062	.058	.062
σ^2_ϵ	.130	.130	.131	.130	.132	.130	.131

Note.— All specifications include dummy variables indicating the calendar year in which the wage was earned (1979–94, with 1986 the omitted year) and a variable indicating that union status is unknown. The specifications that include family background controls also include three indicators that mother's schooling, father's schooling, and family income are unknown.

References

Ahituv, Avner; Tienda, Marta; Xu, Lixin; and Hotz, V. Joseph. "Initial Labor Market Experiences of Black, Hispanic, and White Men." *Industrial Relations Research Association Forty-Sixth Annual Proceeding* (1994): 256–65.

Becker, Gary S. "Investment in Human Capital: A Theoretical Analysis." *Journal of Political Economy* 70 (October 1962): 9–49.

———. *Human Capital and the Personal Distribution of Income: An Analytical Approach.* Ann Arbor: University of Michigan Press, 1967.

Ben-Porath, Yoram. "The Production of Human Capital and the Life Cycle of Earnings." *Journal of Political Economy* 75 (August 1967): 352–65.

Blackburn, McKinley L., and Neumark, David. "Omitted-Ability Bias and the Increase in the Return to Schooling." *Journal of Labor Economics* 11 (July 1993): 521–44.

———. "Are OLS Estimates of the Returns to Schooling Biased Downward? Another Look." *Review of Economics and Statistics* 77 (May 1995): 217–30.

Butcher, Kristin F., and Case, Anne. "The Effect of Sibling Sex Composition on Women's Education and Earnings." *Quarterly Journal of Economics* 109 (August 1994): 531–64.

Card, David. "Earnings, Schooling, and Ability Revisited." In *Research in Labor Economics,* vol. 14, edited by Solomon W. Polachek, pp. 23–48. Greenwich, CT: JAI Press, 1995. (*a*)

———. "Using Geographic Variation in College Proximity to Estimate the Return to Schooling." In *Aspects of Labour Market Behavior: Essays in Honour of John Vanderkamp,* edited by Louis N. Christofides, E. Kenneth Grant, and Robert Swidinsky, pp. 201–22. Toronto: University of Toronto Press, 1995. (*b*)

Coleman, James S. "The Transition from School to Work." In *Research in Social Stratification and Mobility,* vol. 3, edited by Donald J. Treiman and Robert V. Robinson, pp. 27–59. Greenwich, CT: JAI Press, 1984.

Ehrenberg, Ronald G., and Sherman, Daniel R. "Employment While in College, Academic Achievement and Postcollege Outcomes." *Journal of Human Resources* 23 (Winter 1987): 1–23.

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

Griliches, Zvi, and Mason, William M. "Education, Income, and Ability." *Journal of Political Economy* 80, pt. 2 (May/June 1972): S74–S103.

Haley, William J. "Human Capital: The Choice between Investment and Income." *American Economic Review* 63 (December 1973): 929–44.

Halstead, Kent. *State Profiles: Financing Public Higher Education, 1979 to 1993.* Washington, DC: Research Associates of Washington, 1993.

Hanoch, Giora. "An Economic Analysis of Earnings and Schooling." *Journal of Human Resources* 2 (Summer 1967): 310–29.

Hausman, Jerry A. "Specification Tests in Econometrics." *Econometrica* 46 (November 1978): 1251–78.

Hotz, V. Joseph; Xu, Lixin; Tienda, Marta; and Ahituv, Avner. "Are There Returns to the Wages of Young Men from Working While in School?" Unpublished manuscript. Los Angeles: University of California, Los Angeles, Department of Economics, March 1998.

Jevons, W. S. *The Theory of Political Economy.* London: Macmillan, 1871.

Kane, Thomas J., and Rouse, Cecilia E. "Labor Market Returns to Two- and Four-Year College." *American Economic Review* 85 (June 1995): 600–614.

Lam, David, and Schoeni, Robert F. "Effects of Family Background on Earnings and Returns to Schooling: Evidence from Brazil." *Journal of Political Economy* 101 (August 1993): 710–40.

Light, Audrey. "Estimating the Returns to Schooling: When Does the Career Begin?" *Economics of Education Review* 17 (February 1998): 31–45.

———. "High School Employment, High School Curriculum, and Post-School Wages." *Economics of Education Review* 18 (May 1999): 291–309.

Meyer, Robert H., and Wise, David A. "High School Preparation and Early Labor Force Experience." In *The Youth Labor Market Problem: Its Nature, Causes and Consequences,* edited by Richard B. Freeman and David A. Wise, pp. 277–344. Chicago: University of Chicago Press for National Bureau of Economic Research, 1982.

Michael Robert T., and Tuma, Nancy Brandon. "Youth Employment: Does Life Begin at 16?" *Journal of Labor Economics* 2 (October 1984): 464–76.

Mincer, Jacob. *Schooling, Experience, and Earnings.* New York: Columbia University Press for National Bureau of Economic Research, 1974.

Parsons, Donald O. "The Cost of School Time, Foregone Earnings, and Human Capital Formation." *Journal of Political Economy* 82 (March/April 1974): 251–66.

Rosen, Sherwin. "Human Capital: A Survey of Empirical Research." In *Research in Labor Economics,* vol. 1, edited by Ronald G. Ehrenberg, pp. 3–37. Greenwich, CT: JAI Press, 1977.

Ruhm, Christopher. "The Extent and Consequences of High School Employment." *Journal of Labor Research* 16 (Summer 1995): 293–303.

———. "Is High School Employment Consumption or Investment?" *Journal of Labor Economics* 15 (October 1997): 735–76.

Southwick, Lawrence, Jr., and Zionts, Stanley. "An Optimal Control Theory Approach to the Education-Investment Decision." *Operations Research* 22 (November/December 1974): 1156–74.

Wicksell, Knut. *Lectures in Political Economy.* London: Routledge & Kegan Paul, 1934.

Willis, Robert. "Wage Determinants: A Survey and Reinterpretation of Human Capital Earnings Functions." In *Handbook of Labor Economics,* vol. 1, edited by Orley Ashenfelter and Richard Layard, pp. 525–602. Amsterdam: Elsevier-Science, 1986.