In-School Work Experience and the Returns to Schooling

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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 har-vesting problem: when should a person stop school and enter the market?” This view justifies the

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I am grateful to Robert Kaestner, Kathleen McGarry, Randy Olsen, Patricia Reagan, and seminar participants at the Ohio State University for helpful comments.

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 Wickens (1954).

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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
labour 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 sub-
estantial 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 experi-
ence but not work experience gained in school. Because schooling attain-
ment 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 prom-
ience 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 recog-
nized, this latter interpretation is rarely mentioned explicitly in the em-
pirical 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 Hoxa (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 counterevidence 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 post-school 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

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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.  

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

$$W_i = \gamma_0 + \gamma_1 S_i + \gamma_2 X_i + \gamma_3 SX_i + \gamma_4 S_i \times X_i + \eta_i,$$  

(1)

where $W_i$ 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 $t$, $SX_i$ 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

3 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 ($\eta_{i0}$) 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:

$$W = \beta_0 + \beta_1 S_i + \beta_2 X_i + \eta_{i0}$$  (2)

where $X_{i0}$, 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 $\beta_S$ 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 $\beta_S$ 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 $\beta_S$ to the estimator (call it $\hat{\beta}_S$) obtained by estimating equation (2) without controlling for unobserved ability, tastes, and so forth. For each $\beta_S$ I obtain, I compute this ability bias as $100(\hat{\beta}_S - \beta_S)/\beta_S$ and compare it to my estimate of omitted in-school experience bias. Critchley (1977) warns that such measures of ability bias can be misleading, for $\beta_S$ 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

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*6* The full sample of 6,403 male respondents in the NLSY consists of a nationally representative cross section (N = 5,003), a supplemental sample of blacks, Hispanics, and economically disadvantaged nonblack, non-Hispanics (N = 1,516), 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

<table>
<thead>
<tr>
<th>Years of work experience from age 16 to school exit (SX)*</th>
<th>&lt;12</th>
<th>12</th>
<th>13-15</th>
<th>≥16</th>
<th>All</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean</td>
<td>.380</td>
<td>.723</td>
<td>1.822</td>
<td>2.825</td>
<td>1.126</td>
</tr>
<tr>
<td>Standard deviation</td>
<td>.622</td>
<td>.641</td>
<td>1.377</td>
<td>1.823</td>
<td>1.399</td>
</tr>
<tr>
<td>Median</td>
<td>.154</td>
<td>.803</td>
<td>1.560</td>
<td>2.890</td>
<td>.695</td>
</tr>
<tr>
<td>Fraction equal to zero</td>
<td>.282</td>
<td>.378</td>
<td>.155</td>
<td>.000</td>
<td>.159</td>
</tr>
<tr>
<td>Years elapsed from age 16 to school exit</td>
<td>.577</td>
<td>2.714</td>
<td>4.792</td>
<td>7.710</td>
<td>3.926</td>
</tr>
<tr>
<td>Mean</td>
<td>1.09</td>
<td></td>
<td>.99</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Standard deviation</td>
<td></td>
<td>.75</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Median</td>
<td></td>
<td>.39</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fraction equal to zero</td>
<td></td>
<td>.69</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Average hours per week worked from age 16 to school exit</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mean</td>
<td>9.345</td>
<td>10.921</td>
<td>14.308</td>
<td>14.247</td>
<td>11.599</td>
</tr>
<tr>
<td>Standard deviation</td>
<td>11.06</td>
<td>9.00</td>
<td>8.69</td>
<td>7.47</td>
<td>9.56</td>
</tr>
<tr>
<td>Number of individuals</td>
<td>577</td>
<td>867</td>
<td>377</td>
<td>346</td>
<td>2,077</td>
</tr>
<tr>
<td>Fraction of 2,077</td>
<td>.278</td>
<td>.417</td>
<td>.181</td>
<td>.152</td>
<td></td>
</tr>
</tbody>
</table>

* 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. 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 ≥ 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. 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 20,780 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 \( S + 6 \).

\[ ^5 \text{Most evidence on the causal effect of schooling comes from wage or earnings models estimated with cross-sectional data. Because the respondents in my sample were all born in 1962–64, there would be relatively little variation in postschool work experience if I were to confine the wage data to a cross section. Thus, I use all available postschool data to improve the comparability between my analysis and the literature. As it turns out, the generalized squares estimators presented in the next section are similar to what I obtain using ordinary least squares for a cross section drawn from one of the later (1991–94) interview years.} \]
Returns to Schooling

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

<table>
<thead>
<tr>
<th>Variable Name</th>
<th>Definition</th>
<th>Mean (Standard Deviation)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dependent variable:</td>
<td>Log of CPI-deflated average hourly wage (1986 dollars)</td>
<td>1.824 (3.022)</td>
</tr>
<tr>
<td>Wage</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Selected regressors:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Schooling (S)</td>
<td>Highest grade completed</td>
<td>12.167 (1.990)</td>
</tr>
<tr>
<td>Actual experience (X)</td>
<td>Years of work experience since school exit</td>
<td>4.730 (3.493)</td>
</tr>
<tr>
<td>Xs</td>
<td></td>
<td>34.478 (44.449)</td>
</tr>
<tr>
<td>Potential experience</td>
<td>Age - S - 6</td>
<td>6.201 (3.520)</td>
</tr>
<tr>
<td>Potential experience squared</td>
<td></td>
<td>30.884 (47.456)</td>
</tr>
<tr>
<td>In-school work experience (SX)</td>
<td>Years of work experience from age 16 to school exit</td>
<td>1.040 (1.174)</td>
</tr>
<tr>
<td>SX</td>
<td></td>
<td>2.359 (6.293)</td>
</tr>
<tr>
<td>SX x S</td>
<td></td>
<td>13.770 (18.342)</td>
</tr>
<tr>
<td>School ability test score (A_s)</td>
<td>Sum of raw scores for academic components of ASVAB</td>
<td>69.865 (26.314)</td>
</tr>
<tr>
<td>Work ability test score (A_w)</td>
<td>Sum of raw scores for nonacademic components of ASVAB</td>
<td>100.279 (32.681)</td>
</tr>
<tr>
<td>Number of observations</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of individuals</td>
<td></td>
<td>22,788</td>
</tr>
<tr>
<td></td>
<td></td>
<td>2,977</td>
</tr>
</tbody>
</table>

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

Schooling, in-school experience, and post-school 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. 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 (1993b) 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 (e_{it}) is the sum of two components, \( \alpha_i \) and \( \epsilon_{it} \), both of which are mean zero, random variables with constant variances \( \sigma_\alpha^2 \) and \( \sigma_\epsilon^2 \). The time-varying error component (\( \epsilon_{it} \)) is assumed to be white noise, whereas the time-constant individual effect (\( \alpha_i \)) 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 equation (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 unobserved 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 experience 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.\(^5\) For comparison with column \( a' \), I reestimate the column \( d \) model.