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High School Graduation, Performance, and Wages

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Using data from the Panel Study of Income Dynamics and a proprietary sample of semiskilled production workers, this paper investigates the reasons for the discontinuous increase in wages associated with graduation from high school. I find a discontinuous decrease in workers' propensities to quit or be absent. However, I do not find that high school graduates have a comparative advantage in production jobs requiring more training, nor in either sample is there a discontinuous increase in required training associated with the jobs held by high school graduates. The wage premium associated with graduation from high school appears to be procyclical: falling during slumps, periods in which employers are likely to be hoarding labor and in which quits and absences are least important to firms. There is also some evidence suggesting that prior quits have a larger effect on the wages of high school graduates than on the wages of high school dropouts.

I. Introduction

It has often been noticed that graduation from high school induces a discontinuous upward shift in the relationship between schooling and

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wages. For example, Hashimoto and Raisian (1985) find that completion of high school increases the earnings of men by between five and six times as much as does completion of a year of education that was not associated with completion of high school. This effect held regardless of whether workers were employed at small, medium, or large firms.[1]

Using data from the Panel Study of Income Dynamics (PSID), I found that even when education squared and education cubed were included as additional independent variables in a wage equation, the increase in wages associated with completion of high school was more than three times as great as the increase in wages associated with completion of eleventh grade (see table 9 below). Hence the discontinuity in wages associated with completion of high school does not appear to be due to nonlinearities in the relationship between education and wages.

I investigated possible reasons for this discontinuity. Using a proprietary data set of semiskilled manufacturing workers, I found that high school graduation was associated with a discontinuous fall in quit propensity and absenteeism. On the other hand, there is not a discontinuous increase in output associated with completion of high school, nor did I find that high school graduates had a comparative advantage in the more complex jobs in this study (job assignment was random).

This evidence suggests that at least part of the relationship between secondary education and wages may be due to the discontinuous decrease in quit and absenteeism rates associated with completion of high school.[2]

[1] The following data, from the *Current Population Survey*, appear in Hashimoto and Raisian (1985, p. 730) (*t*-statistics are in parentheses):

REGRESSION RESULTS OF MALE EARNINGS

	U.S. Firm Size		
	Small	Medium	Large
Years of schooling	.0367	.0362	.0296
	(5.6)	(4.2)	(5.0)
High school graduate	.1630	.1735	.1286
	(4.9)	(3.9)	(4.5)
University graduate	.0630	.0808	.1590
	(1.7)	(1.8)	(5.6)

[2] In two-stage wage equations estimated using the PSID, the predicted probability of a worker's quitting had a significant negative effect on wages. In particular, when high school graduation was not included directly in the wage equation, I estimated that workers with a 10 percent higher predicted probability of quitting received between 7 percent and 10 percent lower wages, depending on the form of the wage equation

The PSID data support this view. In the data high school graduates have lower quit rates than would be expected from a continuous relationship between quits and education. On the other hand, there is not a discontinuous increase in required training for jobs held by high school graduates. Finally, it appears that the wage premium associated with high school graduation is procyclical: rising during booms, when quits and absences are most harmful to employers, and falling during slumps. (This last finding is especially tentative since problems of collinearity hindered me in separating procyclical effects of high school graduation on wages from procyclical effects of education on wages.)

Because there are unlikely to be demographic characteristics that affect quit propensity (or the propensity to be absent) and do not directly affect wages, it is difficult to use standard data sets to estimate the impact of differences in quit propensity on wages. However, Mirvis and Lawler (1977) directly calculated the cost of quits and absenteeism for a sample of bank tellers. Their measurements suggest that a considerable portion of the wage premium associated with graduation from high school can be explained by the lower propensities to quit or be absent of high school graduates.[3]

being estimated. Of course, the negative correlation between wages and quit propensity is due in part to the positive correlation between high school graduation and quit propensity already mentioned. This problem would be avoided if high school graduation was included not only indirectly in the wage equation as one of the instrumental variables predicting quit propensity but also directly as one of the independent variables. When both predicted quit propensity and high school graduation were included in a wage equation, both were statistically significant; however, in that case there were no variables affecting quit propensity that did not directly affect wages. Hence, the model was identified only by functional form restrictions. Consequently, I did not pursue this approach further. The reader may obtain copies of the two-stage estimates by writing directly to the author.

[3] Mirvis and Lawler (1977) calculated that the total cost of turnover of bank tellers was 85 times as large as their daily earnings plus benefits. Hence, if graduation from high school decreases a worker's probability of quitting during his or her first year on the job from 20 percent to 10 percent (assuming the average work year was 240 days), high school graduation would increase earnings of newly hired bank tellers by 4.4 percent. This calculation was made by assuming a constant quit rate and 240 workdays per year so that a worker with a 10 percent per year probability of quitting has a daily quit probability of .000439 and a worker with a 20 percent per year quit probability has a daily quit probability of .000929. Let the wages of the low-quit-rate and high-quit-rate worker be denoted w_0 and w_1, respectively. Let the cost of a quit be $85w_0$. Then in equilibrium

$$\begin{aligned} w_0 + 85 \times .000439w_0 &= w_1 + 85 \times .000929w_0, \\ 1.0373w_0 &= w_1 + .0790w_0, \\ w_0 &= 1.044w_1. \end{aligned}$$

Mirvis and Lawler also estimated that the total cost of absenteeism for a sample of 160 bank tellers was more than twice the cost in salaries and benefits. Thus if, as estimated below, high school graduation results in a 14 percent decrease in the percentage of days

I conclude from this research that standard estimates of rates of return to education are capturing, in part, the higher earnings of high school graduates associated with their lower quit rates and lower rates of absenteeism. To the extent that these traits were not learned in secondary school, the standard models are overestimating rates of return to secondary schooling. To the extent that these traits were selectively learned in primary school, standard models are underestimating the rates of return to primary education.[4]

These biases obtain in both signaling and human capital theoretic models of schooling and wages.[5] In the signaling model, individuals choose their length of secondary schooling to signal these traits to potential employers (see Spence 1974). Schooling is an effective signal of these traits because of the same attributes that give workers low quit propensities and low rates of absenteeism are likely to give them low nonpecuniary costs of schooling. It is unlikely that a low quit propensity can be directly observed for new entrants into the labor force. High school transcripts generally do not contain information on absenteeism or tardiness, and high schools generally either fail to respond to requests by firms for transcripts or respond too slowly to affect hiring decisions (see Bishop 1986). Even if low quit and absenteeism propensities were directly observed by firms but not by the researcher (as in some human capital models), standard estimates of rates of return to education would still be biased upward. However, schooling decisions would not be distorted (see Griliches [1977] and Hausman and Taylor [1981] for analyses of the role of unobserved attributes in human capital models).

Alternatively, one could consider a human capital model in which the same unobserved traits that cause individuals to have low quit and absenteeism rates increase the efficiency with which they learn in school—increasing the returns to schooling and making it more likely that students complete high school. Then the increase in wages associated with graduation from high school would be due to the superior

absent, then one would expect that at a 6 percent absence rate the negative correlation between absenteeism and education leads to roughly a 2 percent higher wage for high school graduates working as bank tellers. (This calculation is made by noting that a worker with a 6 percent absence rate is paid 12 percent less than a [hypothetical] worker with zero expected absences, while a worker with a 6.84 percent absence rate is paid 13.68 percent less. Hence decreasing one's expected absence rate from 6.84 to 6 percent would increase one's earnings by 1.95 percent.)

[4] Studies of rates of return to education in countries in which there are many early school leavers typically estimate very high rates of return to primary schooling; these estimates are much higher than estimated returns to secondary schooling (see Psacharopoulos 1981).

[5] Note that in both the sorting models and some versions of the human capital model the error term in an estimated earnings equation is correlated with the education variable, causing the estimated coefficient to be biased.

cognitive skills of high school graduates. However, I find this last explanation unpersuasive and not supported by the data.

II. Overview of the Data

The data for this study come from the PSID and from a proprietary data set that I assembled consisting of the personnel files of 2,920 newly hired, semiskilled production workers employed by a high-wage, unionized firm at three widely separated geographic locations.

These data sets have different advantages and drawbacks. Consequently, I am encouraged that the overall story is consistent with the evidence in both data sets. The PSID data have two major disadvantages. First, they do not contain a direct measure of output or a good measure of absenteeism. Second, since education typically affects job assignments and promotional opportunities, it would be difficult, if not impossible, using the PSID or other standard data sets to determine whether lower quit rates of high school graduates are due to the jobs they have or to attributes of high school graduates.

The main drawback from using data derived from the personnel files of a single firm is that they may be subject to serious sample selection biases. That issue is addressed later in this section, where I argue that the effects of sample selection bias are likely to be small. In Section VI, I show that, to the extent that sample selection biases are present, they are likely to lead to underestimates of the negative correlation between high school graduation and propensities to quit or be absent.

On the other hand, using data on workers in similar jobs at a single firm has several important advantages over standard survey data. First, I was able to obtain detailed records of the physical output of a large number of workers and expert evaluation of the complexity of the jobs to which they were assigned. Second, by limiting the sample to workers on similar jobs at the same firm, I was able to focus on the effects of individual attributes on output, absenteeism, and quit propensity, holding constant firm and job effects. Third, since these data were copied from personnel records, they are likely to be more accurate than those obtained in surveys. The difference in accuracy is especially relevant for data on absences and job characteristics. (Mellow and Sider [1983] found that 42.3 percent of the workers surveyed reported for themselves a three-digit occupation different from the one reported by their employer.) Fourth, because this data set contains direct measures of three aspects of productivity—output per hour, absenteeism, and quits—it is possible to measure the effects of education separately for each of those aspects of productivity. Fifth, because the data include a measure of the complexity of the job to which the worker was assigned and because job assignment was (nearly) random, I could estimate whether better-educated workers have a comparative advantage on more complex jobs.[6] Finally, because in this sample the wage schedule faced by a worker was independent of his actual or predicted performance or absenteeism and I was able to partially control for alternative opportunities, I avoided some of the problems present in standard data basis in which wage differences bias estimates of the effects of demographic characteristics on quits, absences, or productivity.

Although all workers at each location in the sample received the same wage, they obviously did not have the same alternative opportunities. The effect of differences in alternative opportunities on quits and absences is discussed in Section III. A detailed description of this data set is presented in Appendix A.

Before investigating the effect of education, and particularly graduation from high school, on the performance of workers in the sample, I first examine the effect of education and other observable characteristics on previous wages for some of the workers in the sample.

Job applicants at one of the plants used in constructing the data base (referred to as plant A) were asked their wage at their most recent job and whether or not they were currently employed at that job. Using those data, I estimated the effect of an additional year of education on the previous pay of workers who were employed when they applied for their current job.[7] Column 2 of table 1 contains

[6] In general, studies that rely on productivity measures of experienced workers within a given job classification are subject to important sample truncation biases. Landau and Weiss (1985) have shown that, in a model with heterogeneous labor, if all workers have to meet a given productivity standard before being assigned to a given job and are promoted if they exceed some higher standard, then even if output Q were equal to education (or experience) times ability (i), on any given job, productivity could be uncorrelated or negatively correlated with education (or experience). For example, if output $Q = ix$, where x could be experience or education and the normalized density of unobserved ability $f(i) = ki^{\beta}$, then average productivity would be uncorrelated with the observed characteristic x. If promotion criteria are less stringent for the better educated, as would appear from table 4 of Medoff and Abraham (1981), a negative correlation between productivity and education could obtain for a wide range of distributions of unobserved ability. This problem does not arise in the data here since none of the workers was promoted, job assignments were (nearly) random, and sample selection bias was likely to be small.

[7] I excluded from the sample workers that were unemployed when hired since the wage on their previous job, one that they either could not keep or found less desirable than unemployment, does not seem representative of their expected earnings. Wages at the previous job were used because at the firm studied wages for experienced workers are a function solely of seniority and the output of their pay group. Obviously this is a biased estimate of the effect of education on earnings for workers in the population: the workers in the sample chose to leave the jobs for which I have recorded their earnings. In Sec. VI, I analyze the effect of this bias.

TABLE 1

NATURAL LOG OF HOURLY EARNINGS IN 1979
(*Current Population Survey* Sample versus Weiss Sample)

Intercept	CPS* (1)	Plant A in Weiss Sample† (2)
Nonwhite	−.065	−.051
	(−5.14)	(−2.11)
Female	−.247	−.253
	(−35.47)	(−13.06)
Nonwhite × female	.050	.052
	(2.81)	(1.43)
Education	.018	.166
	(2.28)	(2.25)
$(\text{Education}/10)^2$	.098	−.519
	(3.61)	(−1.86)
Job tenure	.013	.034
	(7.15)	(.90)
$(\text{Job tenure}/10)^2$	−.036	−.184
	(−11.61)	(−2.64)
Other experience	.014	.042
	(8.01)	(3.57)
$(\text{Other experience}/10)^2$	−.021	−.056
	(−11.13)	(−4.84)
Education × job tenure	.00060	.0015
	(5.15)	(.50)
Education × other experience	−.00033	−.0017
	(−3.29)	(−1.99)
Sample size	18,551	1,272
R^2	.537	.204

NOTE.—The smaller R^2 in col. 2 compared with col. 1 is due, in part, to the smaller range of the independent variables among workers at plant A.

* The other independent variables in the col. 1 regression were whether the worker belonged to a union, the percentage of workers unionized in the worker's industry, interactions between union membership and firm size, and plant size.

† The data in col. 2 are from the only plant in this study for which wage data on the previous job are available. The dependent variable is the logarithm of the wage at the most recent job divided by the mean wage in the economy at the time the job was held, or ln(most recent previous pay ÷ average pay in the United States at that date). On the present job the lifetime wages of all the workers are approximately identical.

estimates of the effect of various demographic variables on the logarithm of the previous hourly wage of workers at that plant. Column 1 reproduces the coefficients estimated by Mellow (1982) for workers in the 1979 *Current Population Survey* (CPS). When one evaluates the marginal effect of education at the mean values in each sample for education, tenure, and experience,[8] $\partial \ln \text{wage}/\partial \text{education} = .043$ for the CPS sample and .037 for this sample.

Table 1 can be used in various ways depending on how ambitious one wishes to be. First, the estimates of $\partial \ln \text{wage}/\partial \text{education}$ provide a

[8] The relevant mean values for the 1979 CPS are mean(education) = 12.64, mean(tenure) = 6.51, and mean(other experience) = 11.57, where other experience is measured by age − education − tenure − 6.

measure of the effect of education on earnings (in their previous job) for the subsample of workers for whom there are data on previous wages. One can then estimate how much of that effect can be explained by the partial correlation between education and various aspects of performance for workers in that limited sample. This approach does not make any assumptions that the sample is representative of a larger population but simply seeks to find those factors that contributed to earnings differences on previous jobs for workers within this subsample. It does assume that performance on current jobs is correlated with performance on previous jobs. I shall, however, also assume that workers for whom usable pay data were available are representative of workers in the sample, so that a year of education has approximately a 4 percent effect on the hourly wage on other jobs for the workers in the larger sample.[9]

A second use of the data in table 1 is to provide a partial check of the importance of sample selection bias for the sample if one wishes to generalize the results outside the sample. Since the major concern here is in explaining the relationship between education and wages, it is useful to check to see if the return to education in the sample is biased. This is a potentially serious problem because one would expect that better-educated individuals who apply for these jobs are less representative of their schooling cohort than less well educated workers. However, the estimated value of the return on education for workers for whom there are wage data is roughly the same as returns estimated using the CPS sample. Thus it does not seem that this sample differs from the CPS sample in ways that grossly distort the effect of education on earnings. In addition, the sign of the effect of race, sex, education, tenure, and experience on the logarithm of the (previous) wage is the same for this sample as for the randomly selected CPS sample.

The personnel practices at these plants also provide grounds for believing that the sample is representative. Only 22 percent of applicants whose applications were reviewed were rejected. Of those, 85 percent were rejected because of a low score on the Crawford Physical Dexterity Test.[10] The estimation procedures included the worker's score on the dexterity test as an independent variable, thus eliminat-

[9] I had usable data on previous pay for 77 percent of the workers employed at plant A (those workers constituted 43 percent of the entire sample).

[10] Of course some residual sample selection bias occurs if unobserved attributes that lead workers to wait in line longer, such as the ability to withstand the cold, are correlated with performance and with observed attributes studied here. Applications were reviewed in order of the applicant's place in line with some adjustments to ensure racial and sexual balance. At the location used for table 1 the individual's place in line is known. It was uncorrelated with any of the measures of performance. Consequently, I have assumed that this source of bias is small and have disregarded it.

ing that potential source of sample selection bias. In addition, the average pay increase for workers for whom there were wage data was 103 percent, and workers waited for more than 36 hours in freezing temperatures to receive job application forms. This evidence indicates that the firm was paying above-market wages. These high wages are likely to reduce the sample selection problems derived from more "able" applicants not applying to the firm, where ability refers to unobserved worker characteristics that are correlated both with observed characteristics and with productivity. These high wages and the concomitant excess supply of job applicants were routine features at the manufacturing locations of this unionized firm. At another location of this firm, when recall notices were mailed to former employees, 90 percent of those who had found alternative work quit their jobs to return to work for the firm.

I was also able to test for possible biases introduced by the job assignment decisions of the firm. The personnel officers of the firm maintained that newly hired workers were randomly assigned to entry-level jobs. I tested whether this policy was followed in practice by regressing the measure of the complexity of the job to which a worker was assigned against all the observed demographic characteristics of the workers, including schooling, previous work experience, and scores on each part of the physical dexterity test. Those explanatory variables were neither economically nor statistically significant, nor were they jointly significant.[11] Consequently I have assumed that sample selection and job assignment biases were small and have not corrected for them.[12]

Sections III and IV present evidence linking the wage premium received by high school graduates to their low propensities to quit or be absent. In Section V, I present corroborating evidence suggesting that the wage premium received by high school graduates is unlikely to be due solely to skills learned in high school. Section VI discusses the effects of selection bias on the results. Section VII contains some concluding remarks.

III. Models

Typically an individual's choice of a level of education is a function of both observable and unobservable traits. The traits that are observ-

[11] I did find, however, that males were assigned to simpler jobs.

[12] Alternatively I could have used the Bloom and Killingsworth (1984) procedure to correct for sample selection bias. (Because the characteristics of excluded observations are unknown, the standard Heckit correction cannot be used.) However, their procedure is extremely sensitive to assumptions about the distribution of the error term in the selection equation. Indeed, as they point out, the equation being estimated is identified only if the assumed distribution of the error term of the selection equation is nonlinear. See Muthén and Jöreskog (1983) for a discussion of this issue.

able for this sample and that affect the schooling decision include age, race, sex, and manual dexterity. I grouped together under the rubric "stick-to-itiveness" all the unobserved attributes that affect an individual's choice of a level of schooling. Stick-to-itiveness represents the combined effect on schooling level of characteristics such as self-discipline, desire for variety, and susceptibility to illness or to the urge to "take a day off." Years of schooling and high school graduation were used as proxies for the traits referred to as stick-to-itiveness.

In addition to representing these unobserved characteristics, education also indicates a level of training. One skill that is likely to have been acquired in school is the ability to learn complex tasks. Of course, that skill may have been acquired prior to the years of schooling across which the sample differs (almost the entire sample had at least 9 years of education) and influenced the individual's choice of a level of education. I assume that skills such as the ability to learn complex tasks that affect the productivity of workers and that may plausibly have been learned in secondary school were learned there. That is, I intentionally biased the analysis in favor of a learning explanation for the correlation between education and wages.

The performance equations estimated are whether or not the worker quit during his first 6 months on the job, absenteeism (both days absent and occasions of absenteeism), and output per hour during the first month on the job as a fraction of expected output given the complexity of the job. (These normalizations are routinely performed by the industrial engineering staff as part of their efforts to compute the piece rate for different jobs.) The critical independent variables for the analysis are years of education, high school graduation, job complexity, and a measure of the "match" between education level and job complexity: match $\equiv$ [education $-$ mean(education)] $\times$ [job complexity $-$ mean(job complexity)]. If better-educated individuals have a comparative advantage in more complex production jobs, the coefficient on the match term in an output equation would be positive.

Job complexity was defined as the logarithm of the number of weeks the plant's industrial engineering staff estimates it should take a new employee to learn the job (achieve the expected productivity rating for an experienced worker on that job). The main component in the industrial engineers' calculation is the number of times per week an experienced worker performs the task (see fig. 1). Mean job complexity is the average level of job complexity for the subsample of workers at each location.

Although there is a large literature examining the effect of job enrichment and job complexity on job satisfaction and performance, there is considerable controversy in interpreting these results. There

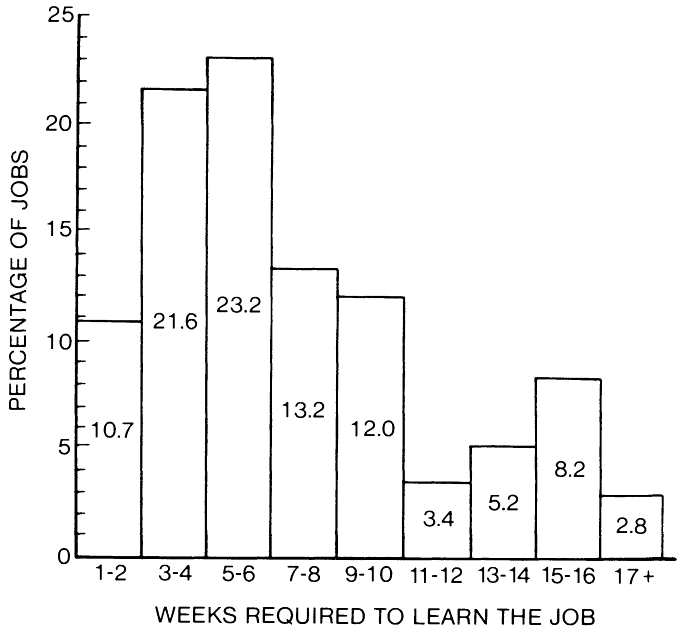

FIG. 1

are both biases in experimental design—often the studies rely on screened volunteers who are given pay supplements (see Fein 1975)—and biases in the reporting of results: Hackman (1974) points out that papers describing the results of job enrichment programs are typically written by the consultants that implemented them. Consultants are more likely to publicize their successes than their failures.

In general one would expect stick-to-itiveness (those unobservable traits that lead individuals to complete high school) to have its greatest effect on quit propensity and to have a lesser effect on other aspects of behavior such as absenteeism. On the other hand, one direct effect of education and of high school graduation is to improve alternative opportunities, increasing the probability of a quit.

A. *Quits*

Let $\overline{S}_i$ denote worker i's present value of his current job, $\overline{V}_i$ denote i's present value of his best opportunity elsewhere, and M_i denote i's mobility costs (both real and psychic) of a job change (housework and leisure are counted as jobs). Assume that worker i is risk neutral and quits if and only if

$$\overline{V}_i - \overline{S}_i > M_i. \tag{1}$$

Denote the time discount rate of workers by r, worker i's age at the start of the observation period by a_i, and the value of an individual's alternative and present job at time t by $V_i(t)$ and $S_i(t)$, respectively. Then, assuming that male workers anticipate working until they are 65 years old and normalizing $t = 0$ for the time when the quit decision is made, we get

$$\overline{V}_i - \overline{S}_i = \int_0^{65 - a_i + \beta_0 F} e^{-rt}[V_i(t) - S_i(t)]dt, \tag{2}$$

where F is a dummy variable indicating whether a worker is a female, and β_0 is an estimated parameter of the problem. If females anticipate working fewer years, β_0 is negative. Next, let $p_i(t)$ represent the probability that individual i changes jobs after the observation period and before period t, and let the value of that new job be a weighted average of the value of the previous job and some constant term B_i.

Assume that the values of the alternative and the present job grow at an exponential rate so that

$$V_i(t) = e^{ut}\{[1 - p_i(t)]V_i(0) + p_i(t)[\gamma V_i(0) + (1 - \gamma)B_i(0)]\}, \tag{3a}$$

$$S_i(t) = e^{ut}\{[1 - p_i(t)]S_i(0) + p_i(t)[\gamma S_i(0) + (1 - \gamma)B_i(0)]\}, \tag{3b}$$

where $0 < \gamma < 1$. Then, with $\delta = (r - u)$ and $\alpha_i = 1 - (1 - \gamma)p_i(t)$, worker i quits if and only if

$$\int_0^{65 - a_i + \beta_0 F} \alpha_i e^{-\delta t}[V_i(0) - S_i(0)]dt > M_i. \tag{4}$$

To obtain an estimable quit equation from (4), assume

$$\alpha V(0) = X_1\beta_1 \tag{5}$$

$$\alpha S(0) = X_2\beta_2 \tag{6}$$

$$M = X_3\beta_3. \tag{7}$$

Denoting $[e^{\delta(65 - a + \beta_0 F)} - 1]/\delta$ by $g(a, \delta, \beta_0 F)$ and substituting (5)–(7) into (4), we get

$$Q = \begin{cases} 1 & \text{if } g(a, \delta, \beta_0 F)(X_1\beta_1 - X_2\beta_2) - X_3\beta_3 > 0 \\ 0 & \text{otherwise.} \end{cases} \tag{8}$$

Clearly, the net gain from changing jobs, the left-hand side of (8), will be measured with error. Assume that this error term, denoted μ_Q, is distributed $N(0, \sigma^2)$. Therefore, the quit equation estimated is

$$Q = \begin{cases} 1 & \text{if } g(a, \delta, \beta_0 F)(X_1\beta_1 - X_2\beta_2) - X_3\beta_3 > \mu_Q \\ 0 & \text{otherwise.} \end{cases} \tag{9}$$

The worker characteristics that are measured and that affect alternative opportunities include education, race, sex, age, geographic location, dexterity (as measured by scores on the physical dexterity test), and employment status when hired (a worker who was on a temporary layoff from a desirable previous job might be likely to quit when recalled from the layoff).[13] The factors affecting alternative opportunities could also affect job satisfaction. For example, one would expect workers that were employed at the time of application to have a significantly higher level of job satisfaction (one reason they left the previous job was the anticipation of increased job satisfaction) and hence to be less likely to quit. There are also job-related characteristics, such as job complexity, that affect job satisfaction but not alternative opportunities.[14]

Finally, one would expect stick-to-itiveness (as measured by high school and college graduation) and the worker's marital status to impose additional mobility costs.[15] Those variables are not multiplied by $g(a, \delta, \beta_0 F)$. They enter directly into the quit equation as elements of X_3. I also included education as an element of X_3. Thus education was allowed to directly affect quit propensity if students learn not to quit during their postprimary years of schooling (the years over which schooling levels differed in the sample). Since education may affect alternative opportunities and job satisfaction, I also included education times $g(a, \delta, \beta_0 F)$ as a right-hand variable in the estimated quit equation.

As can be seen in table 2, the major hypothesis is confirmed. High school graduation has a strong negative effect on a worker's probability of quitting. Because this effect is independent of the direct effect of schooling, we can reject the hypothesis that the reason better-educated individuals have lower quit propensities is that they learned not to quit in secondary school.

Equation (9) was estimated by the method of maximum likelihood, using a pooled sample from plants A and B. Individuals were omitted if they were laid off before being with the firm for 6 months, and selection bias was avoided by also omitting workers who would have been laid off before 6 months had passed had they not already quit.

[13] Of course employers are concerned with the total effect of education on the worker's probability of quitting, taking into account the better alternative opportunities available to the better-educated workers. However, we are concerned with explaining wage differences in the market, not at the firm being studied, where there are no wage differences due to education differences. In the market equilibrium, better-educated workers have higher wages as well as better alternative opportunities. It is differences in quit propensities, not of the effect of alternative opportunities on quits, that contribute to differences in the equilibrium wage at different education levels.

[14] Contemporaneous wage was not included as an element of X_2 because all workers had substantively the same expected lifetime wage on their current job.

[15] See Mincer (1978) for an analysis of the effect of marital status on worker mobility.

TABLE 2

PROBABILITY OF QUITTING WITHIN FIRST 6 MONTHS ON THE JOB
(Maximum Likelihood Estimation Procedure)

Independent Variable*	Estimated Coefficient or Value	Standard Errors: Gradient Method	Hessian Method	White's Method
δ	.26	2.04	.61	.28
		(−.13)	(−.42)	(−.91)
$\hat{\beta}$ (effect of being female on anticipated work life)	−6.81	42.3	10.6	4.19
		(−.16)	(−.63)	(−1.62)
High school graduate	**−.342**	**.140**	**.135**	**.136**
		(−2.44)	**(−2.54)**	**(−2.61)**
College graduate	−.242	.367	.397	.444
		(−.66)	(−.62)	(−.59)
Education	−.07	.171	.057	.035
		(−.44)	(−.67)	(−.76)
$g(a, \delta, \hat{\beta}F) \times$ education	.015	.143	.044	.029
		(.11)	(.34)	(.53)
Married	−.091	.078	.077	.077
		(−1.16)	(−1.18)	(−1.17)
$g(a, \delta, \hat{\beta}F) \times$ employed at application	−.098	.774	.231	.107
		(−.13)	(−.43)	(−.92)
Number of observations	2,146			
Log likelihood	−741.99			

NOTE.—t-statistics are in parentheses. Throughout this paper I loosely use the term "t" to refer to the estimated coefficient divided by the standard error.

* Other independent variables included scores on each half of the dexterity test, race-location interactions, location effects, age, and an intercept term. I used three different methods for calculating the standard errors because there is no consensus as to which is the correct technique. If the model is correctly specified, the three methods give the same asymptotic estimates. The estimates obtained are sufficiently similar to provide some confidence that the model is not grossly misspecified. Computational costs precluded performing the model specification test suggested by White (1982).

(All layoffs were made strictly by seniority.) Because the likelihood function is almost flat with respect to changes in δ, offset by compensating changes in the vector of variables multiplied by δ, the standard errors of δ and of the coefficients of variables multiplied by $g(a, \delta, \beta_0 F)$ are high. Although statistically insignificant, they have values consistent with the model. Individuals such as white males who have better alternative opportunities are more likely to quit these jobs; workers who were employed when they applied for these jobs are less likely to quit. Similar confirmation was provided by the negative value of β_0. The estimates suggest that females in the sample anticipate spending 7 fewer years in the labor force than males. This finding is consistent with unpublished research by Jacob Mincer. Using the National Longitudinal Study sample, he finds that women spend roughly 25 percent less time in the labor force than men. Finally, the similarity in the standard errors obtained using the gradient, Hessian, and White (1982) methods suggests that the model is not grossly misspecified.

To obtain more precise estimates of the coefficients and to make

TABLE 3

PROBABILITY OF QUITTING WITHIN FIRST 6 MONTHS

Independent Variable	Coefficient	t-Statistic	Δ Quit Probability from a One-Unit Change in the Variable Evaluated at Quit Probability of 10%	20%
Intercept	−.922	−1.54	...	...
High school graduate	**−.341**	**−2.54**	**−.059**	**−.096**
College graduate	−.280	−.72	−.049	−.078
Education	−.106	−1.32	−.019	−.030
Married	−.097	−1.26	−.017	−.027
$h(a, \delta, \hat{\gamma}F) \times$ education	.0055	1.45	.00097	.0015
$h(a, \delta, \hat{\gamma}F) \times$ age	−.00029	−.32	−.000052	−.000083
$h(a, \delta, \hat{\gamma}F) \times$ male	.0012	.16	.00022	.00034
$h(a, \delta, \hat{\gamma}F) \times$ white $\times$ South	.068	4.58	.012	.019
$h(a, \delta, \hat{\gamma}F) \times$ South	−.036	−2.34	−.0062	−.0099
$h(a, \delta, \hat{\gamma}F) \times$ white $\times$ Midwest	.0061	1.00	.0011	.0017
$h(a, \delta, \hat{\gamma}F) \times$ employed at application	−.0023	−4.95	−.0040	−.0064
$h(a, \delta, \hat{\gamma}F) \times$ pins section of dexterity test	.00020	.34	.000035	.000056
$h(a, \delta, \hat{\gamma}F) \times$ screws section of dexterity test	.00094	1.94	.00016	.00026
Number of observations	2,146			

NOTE.—$\hat{\gamma}$ set equal to −.25, δ set equal to .05; 0.124 of observations had $Q = 1$; the mean of $h(a, \delta, \hat{\gamma}F)$ is 16.12.

use of Mincer's findings on the shorter work life of women, we can reformulate equation (9) as

$$Q = \begin{cases} 1 & \text{if } h(a, \delta, \hat{\gamma}F)(X_1\beta_1 - X_2\beta_2) - X_3\beta_3 > \mu_Q \\ 0 & \text{otherwise,} \end{cases} \tag{9'}$$

where $h(a, \delta, \hat{\gamma}F) = [e^{-.05(1-.25F)(65-\text{age})} - 1]/-.05$. That is, we can impose the restrictions that $r - \mu = .05$ and that the effective work life of women is 25 percent shorter than that of men.

Equation (9′) was estimated using a probit estimation procedure. The estimated coefficients and the effect of a change in each independent variable on a worker's probability of quitting are presented in table 3.[16]

[16] Note that in table 3 job complexity and match were not included as independent variables. If they are included as independent variables when (9′) is estimated, the sample size falls to 1,532 and the absolute value of all the t-statistics also falls. However, none of the results in table 3 is significantly affected: high school graduation is estimated to correspond to a reduction in the worker's probability of quitting of −.059 at a 10 percent quit probability and −.096 at a 20 percent quit probability. Similarly, if quit propensity is estimated separately for men and women in the sample, the qualitative effects in table 3 hold for each subsample.

High school graduation has a large and statistically significant effect on a worker's probability of quitting. Evaluated at a 10 percent probability of quitting, high school graduation decreases a worker's probability of quitting by almost 6 percent. At a 20 percent probability of quitting the decrease is 9.6 percent. The continuous effect of education on quit propensity is also negative, suggesting that workers with more education have more stick-to-itiveness, while when education is interacted with the $h(\,)$ function its coefficient is positive, as would be predicted from the better alternative opportunities available to better-educated workers.

The other estimated variables in table 3 are also consistent with the model. Being employed when hired by your current employer has a negative effect on a worker's probability of quitting. This is consistent with our model since those workers incurred greater costs in taking this job and hence were likely to have a higher anticipated level of job satisfaction. Southern blacks are far less likely to quit than southern whites, perhaps reflecting differences in their alternative opportunities. On the other hand, males do not seem more likely to quit than females, suggesting that sex discrimination by other firms may be of relatively low magnitude compared with race discrimination in the South.

These results were not sensitive to the particular specification of the quit equation. When I estimated either a probit, logit, or linear probability model with no interactions between the $h(\,)$ function and the explanatory variable, the results were substantially unaffected. The results from the probit model with no interactions between the $h(\,)$ function and the other explanatory variables are presented in table 4. In this table, high school graduation continues to have a strong negative relationship with predicted quit propensity, while the continuous relationship between years of education and quit propensity is weak and statistically insignificant. Apparently the greater stick-to-itiveness of better-educated workers is offsetting their superior alternative opportunities, eliminating any discernible relationship between education (other than high school graduation) and quits.

To provide a rough check of whether peculiarities in the distribution of education were generating the results in tables 2, 3, and 4, I plotted in figure 2 histograms of the proportions of quitters and nonquitters at each education level. I also checked to see if the dummy variable for high school graduation was capturing nonlinearities in the relation between education and quits. When the dummy variable for high school graduation, in the model estimated in table 3, was replaced by a dummy variable for completion of eleventh grade or completion of thirteenth grade, the coefficients of these latter terms were statistically insignificant. As an alternate means of

TABLE 4

PROBABILITY OF QUITTING WITHIN FIRST 6 MONTHS ON THE JOB
(Probit Model with Repressed Interaction Terms)

INDEPENDENT VARIABLE	COEFFICIENT (1)	COEFFICIENT (2)
Intercept	−.561	−.664
	(−1.00)	(−.36)
High school graduate	**−.329**	**−.314**
	(−2.51)	**(−7.27)**
College graduate	−.227	. . .
	(−.59)	
Education	−.024	.0025
	(−.53)	(.008)
Education squared	. . .	−.0016
		(−.14)
Married	−.082	−.083
	(−1.10)	(−1.11)
Age	−.021	−.021
	(−3.64)	(−3.61)
Male	.173	.173
	(2.05)	(3.61)
White-South	.956	.954
	(5.09)	(5.08)
South	−.354	−.352
	(−1.77)	(−1.76)
White-Midwest	.088	.083
	(.861)	(.822)
Employed when hired	−.392	−.392
	(−5.20)	(−5.18)
Score on pins part of dexterity test	.0026	.0026
	(.280)	(.27)
Score on screws part of dexterity test	.018	.018
	(2.26)	(2.26)
Number of observations	2,236	2,236
Log likelihood	−788.8	−788.99

NOTE.—t-statistics are in parentheses.

capturing nonlinearities, I added education squared and education cubed to the independent variables listed in table 3. With that cubic specification, I found that at a 10 percent quit probability the discontinuous effect on quits from graduation from high school reduces a worker's probability of quitting by 5.1 percent; at a 20 percent quit probability the discontinuous effect of graduation from high school reduces a worker's probability of quitting by 8.1 percent. The t-statistic on high school graduation given the cubic specification for the effect of education is −1.84.[17]

[17] This fall in the t-statistic from including higher-order polynomial terms is, of course, not surprising. There are several reasons why I expected the coefficient on high school graduation to be sensitive to the addition of (education)^2 and (education)^3 as independent variables: (1) there were relatively few quits, (2) the sample clustered

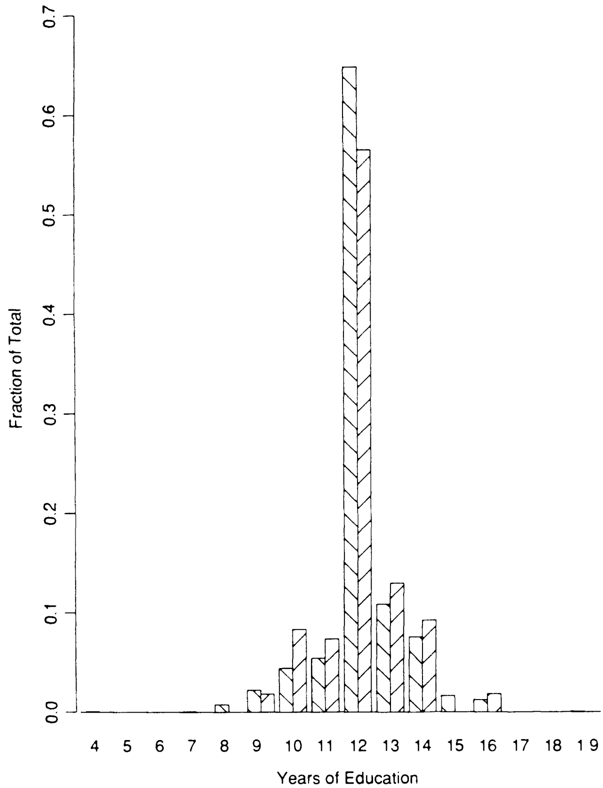

FIG. 2.—Years of education for quitters and nonquitters. Left columns of pairs refer to nonquitters, right columns to quitters.

I checked the finding that high school graduation significantly affects quit propensities using the 1968–82 PSID sample. As in table 4, I suppressed the interactions between the $h(\ )$ function and the other explanatory variables. The results in table 5 are consistent with those in tables 2, 3, and 4. Completion of high school is associated with a reduction in the individual's quit rate of roughly one-third, while postprimary schooling (aside from completion of twelfth grade) has an insignificant effect on quit rates. These results are also robust to changes in the specification of the quit function and of the error term.

around an education level of 12.1 (the standard deviation was 1.2), and (3) with the cubic specification the independent variables included six different measures of education. Given these difficulties, I was surprised to find that the discontinuous relationship between graduation from high school and quit propensity was still observable with the cubic specification for the continuous effect of education on quit probabilities.

TABLE 5

MALE PRIVATE-SECTOR EMPLOYEES IN PSID SAMPLE, 1968–82

A. QUITS PER YEAR

Independent Variable	Coefficient
High school graduate	**−.030**
	(−3.58)
Education	−.024
	(−2.26)
Education squared	.0030
	(2.53)
Education cubed	−.00011
	(−2.76)
ln(mean experience while in sample)	−.025
	(−3.67)
New entrant	.014
	(1.62)
Nonwhite	−.030
	(−5.05)
Age	−.0023
	(−5.30)
Mean city unemployment rate minus mean national unemployment rate	−.003
	(−1.79)
Number of observations	3,781
R^2	.16
Mean quits per year	.092

B. ANNUAL PROBABILITY OF QUITTING (Logistic Model)

Independent Variable*	Coefficient	Standard Error
Intercept	1.48	.590
Education	−.59	.180
Education squared	.072	.020
Education cubed	−.0025	.00065
Work experience	−.111	.0118
Work experience squared	.0012	.00033
Disabled	.172	.119
Nonwhite	−.429	.087
Married	−.408	.083
Union member	−.744	.086
High school graduate	**−.338**	**.132**
Part-time worker	.263	.094
National unemployment rate	−.084	.027
County unemployment rate	.016	.062
County unemployment rate squared	−.0033	.004

NOTE.—Each observation was weighted by the square root of the number of years the individual was in the sample. *t*-statistics are in parentheses in pt. A. I did not include wages as an independent variable in these regressions since I was concerned with the indirect effect of various demographic characteristics on wages through their effect on quit propensities.

* Eight location dummies were also included.

B. *Absenteeism*

In Appendix B I show why differences in expected absenteeism rates can significantly affect the wages of workers. In this subsection, I estimate the discontinuities in expected absenteeism rates associated with high school graduation for workers in the sample.

In estimating the determinants of absenteeism I assume that a worker is more likely to be absent the higher is his level of job dissatisfaction and the lower his stick-to-itiveness. As in the quit equation, job satisfaction is measured by the elements of X_2. Education and high school graduation are used as proxies for persistence: an individual who had insufficient persistence to complete high school is likely to have poorer than expected attendance habits. I assume that both days and occasions of absenteeism as percentages of possible days worked are linear functions of X_2, the proxies for stick-to-itiveness, and normally distributed error terms (consecutive days absent are referred to as a single occasion).

In the quit equation we can separate the effect of education on job satisfaction from its effect on stick-to-itiveness by arguing that job satisfaction has a greater effect on the quit rates of workers with longer expected future work lives. (The education $\times$ $g(a, \delta, \beta_0 F)$ term captured the effect of education on quits that was caused by its effect on either alternative opportunities or job satisfaction.) This option is not available in the absenteeism equations: workers are trading off one-period gains and losses from absences. Consequently, the estimated coefficients on education and high school graduation reported in tables 6 and 7 could be estimating the effect of lower (or higher) levels of job satisfaction of better-educated workers on their absence rates rather than the effect of stick-to-itiveness on absenteeism. However, the results from the previously estimated quit equations suggest that this is not happening.

Percentage of days absent was estimated using a Tobit procedure.[18] I calculated the percentage of days workers were absent during their first 6 months on the job. However, because not all the workers in the sample completed 6 months of work, those observations were weighted by the square root of the number of days for which they were observed. I also used occasions of absenteeism as a dependent variable. In those calculations I restricted the sample to individuals who worked for 6 months. I assumed that occasions of absenteeism

[18] The Tobit procedure implicitly assumes that discrete absenteeism data can be approximated by a continuous distribution and that the error term in the estimation equation is normally distributed but truncated, so that absenteeism rates that the model predicts would be negative are recorded as zeros. Of course, theoretically absences are also truncated from above: no one can be absent more than 100 percent of the time. In this sample the problem did not arise.

TABLE 6

Days Absent as a Percentage of Days Worked
(Tobit Estimation Procedures)

Independent Variable	Coefficient
Intercept	7.38
	(2.54)
Male	.398
	(−.95)
Age	−.143
	(−5.89)
Education	.133
	(.58)
High school graduate	**−2.35**
	(−4.04)
College graduate	−1.34
	(−.69)
Married	−.046
	(−.13)
Job complexity	−.12
	(−.45)
Match	−.21
	(−1.10)
Employed at application	−1.23
	(−3.56)
Number of observations	1,890
Log likelihood	−4,781.2

Note.—The data for tables 6–8 come from three locations of the firm, rather than only the two included in the quit equations. Each observation was weighted by the square root of the number of days worked. *t*-statistics are in parentheses.

are generated from a Poisson arrival process and that the λ parameter that describes the Poisson process has a gamma distribution across the population. The resulting distribution is negative binomial.[19] I computed maximum likelihood estimates for that distribution. (Estimation using Tobit is inappropriate for this problem because the continuity assumption of the Tobit procedure is grossly violated when computing occasions of absences.)

Whether days or occasions are used as a measure of absenteeism, we find that high school graduation has a negative effect on absenteeism. This effect is statistically significant regardless of the estimation procedure used and holds even when we attempt to capture nonlinearities in the relationship between education and absenteeism by allowing for a cubic specification. The continuous effect of secondary education on absenteeism is small relative to the discontinuous effect of graduation from high school. It seems unlikely that high school graduates learned in secondary school the traits that gave them

[19] This result comes from Greenwood and Yule (1920). A derivation appears in Johnson and Kotz (1969, p. 124).

TABLE 7

OCCASIONS ABSENT DURING THE FIRST 6 MONTHS ON THE JOB
(Negative Binomial Model)

Independent Variable*	Coefficient
Intercept	4.46
	(1.03)
Male	−.113
	(−2.33)
Age	−.031
	(−8.03)
Education	−.945
	(−.786)
High school graduate	**−.362**
	(−2.94)
College graduate	.389
	(.998)
Married	.0344
	(.716)
Match	−.00736
	(−.238)
Job complexity	.00819
	(1.52)
Employed at application	−.181
	(−3.84)
Number of observations	1,740
Log likelihood	1,533.78

NOTE.—Standard errors were calculated using the Hessian method. t-statistics are in parentheses.

* Other independent variables were location, score on the screws section of the dexterity test, and race-location interactions for the two plants at which race data were available.

low rates of absenteeism. Instead, the same unobserved characteristics that lead to successful completion of high school seem to lead to low rates of absenteeism.

C. *Output per Hour*

The final indicator of performance explored was the logarithm of normalized output per hour during the worker's first month at the manufacturing facilities studied (see table 8). As previously noted, job assignments of newly hired workers were independent of their education, scores on either part of the physical dexterity test, and prior work experience. They are paid piece rate during that month. I assume that output per hour is a linear function of observed characteristics, including the match term described above. The distribution of jobs was described in figure 1.

Assigning high school graduates to more complex jobs has neither

TABLE 8

NORMALIZED FIRST-MONTH OUTPUT ESTIMATED BY ORDINARY LEAST SQUARES

	COEFFICIENT		
INDEPENDENT VARIABLE*	(1)	(2)	(3)
Intercept	67.36	69.05	71.63
	(5.80)	(6.24)	(7.14)
Male	8.91	8.87	8.87
	(5.81)	(5.80)	(5.80)
Age	−.11	−.11	−.11
	(−1.15)	(−1.16)	(−1.16)
Education	1.36	1.34	1.33
	(1.63)	(1.61)	(1.59)
High school graduate	**4.44**	**2.54**	**−.21**
	(.64)	**(.45)**	**(−.08)**
Match	.49	...	.03
	(.48)		(.04)
Job complexity × high school graduate	−2.64	−1.55	...
	(−.73)	(−.55)	
College graduate	−12.96	−13.12	−13.03
	(−2.09)	(−2.11)	(−2.10)
Married	4.01	4.02	4.01
	(2.98)	(2.98)	(2.98)
Score on dexterity test	.22	.22	.21
	(1.52)	(1.53)	(1.53)
Job complexity†	7.82	6.98	5.63
	(2.45)	(2.60)	(5.26)
Employed at application	1.48	1.45	1.48
	(1.08)	(1.06)	(1.08)
Number of observations	1,859	1,859	1,859
Multiple R^2	.382	.382	.382

NOTE.—*t*-statistics are in parentheses.
* Other independent variables were location dummies.
† The statistically significant coefficients for job complexity suggest that the industrial engineers overestimate the difficulty of performing complex jobs.

a statistically nor economically significant effect on output, nor does matching better-educated workers with the more complex jobs significantly affect output. These findings suggest that the pecuniary reward to education for the workers in the sample is not due to skills learned in school that help those workers learn complex production tasks.

The direct effect of education on output is marginally significant but does not seem large enough to explain the correlation between education and wages. In table 1 I computed the effect of education on the logarithm of their real wages on their previous job for workers in plant A. The best estimate was that each year of secondary education had roughly a 3.7 percent effect on the previous wage of the workers in that plant. On the other hand, I find that each year of secondary education has roughly a 1.3 percent effect on the output of workers in that plant. (This estimated effect is not statistically different from zero. The mean value of job complexity for this subsample was 1.76. Consequently, for the average job, high school graduation does not affect output.)

IV. Effects on Wages

Although one should be cautious about generalizing the results in Section II beyond the sample of semiskilled production workers, the PSID provides evidence that the wage premium received by high school graduates is due, at least in part, to their low quit propensity.

There are both theoretical and empirical grounds for believing that during business slumps firms hoard workers: pay wages above the value of the product of the workers. (Fay and Medoff [1985] estimate excessive staffing levels of 4 percent for a typical manufacturing plant during its most recent trough quarter.) Consequently, during business slumps one would expect firms to benefit from (or be hurt less by) quits and absences. To obtain quantitative estimates of the effect of the low quit propensity and low absenteeism of high school graduates on their wages, I interacted county unemployment rates with high school graduation while estimating a wage equation for privately employed males in the PSID data set between 1976 and 1982.[20] I included education, education squared, and education cubed as independent variables to reduce the possible role of high school graduation in approximating a precluded degree of curvature in the relationship between wages and education.

As a further check on possible nonlinearities generating these results, I replaced the dummy variable for high school graduation first with a dummy variable for completion of eleventh grade and then with a dummy variable for completion of the thirteenth year of schooling. Neither of these dummy variables was statistically significant. Thus the results do not seem due to the dummy variable for completion of high school capturing additional nonlinearities in the relationship between education and wages.

In table 9, I estimate that at the mean county unemployment rate of

[20] Complete wage data are not available in the PSID data set for years prior to 1976. For those years it is possible to construct wages using reported earnings and hours worked. Duncan and Hill (1985, pp. 519–20) verified reported earnings calculated in this way with employer records and found that "errors in interview reports of average hourly earnings (defined as the ratio of interview reports of annual earnings to annual hours) were enormous." Consequently, I used only years in which wages were directly reported. Because wages reported were truncated for 1976 and 1977 (no one could report a wage above $9.99), I inferred wages for workers that reported a wage of $9.99 by extrapolating back from their reported wage in 1978. I assumed that their percentage wage increase was equal to the average in the sample, except that if the extrapolated wage was less than $9.99 I assumed that it was equal to $9.99.

TABLE 9

NATURAL LOG OF REAL WAGES OF MALE PRIVATE-SECTOR EMPLOYEES IN PSID SAMPLE, 1976–81

INDEPENDENT VARIABLE*	ESTIMATED COEFFICIENT (1)	ESTIMATED COEFFICIENT (2)
Intercept	.051	.054
	(5.62)	(5.93)
Education	.078	.077
	(2.87)	(2.84)
Education squared	−.0062	−.0062
	(−2.16)	(−2.16)
Education cubed	.00029	.00029
	(3.22)	(3.23)
Reported experience	.028	.028
	(13.71)	(13.52)
Reported experience squared	−.00040	−.00039
	(−12.09)	(−11.86)
Age	−.0031	−.003
	(−2.14)	(−2.34)
Disabled	−.087	−.085
	(−6.44)	(−6.35)
Married	.084	.082
	(8.25)	(8.11)
Part-time work	...	−.081
		(−6.75)
Nonwhite	−.156	−.159
	(−17.79)	(−18.02)
High school graduate	**.133**	**.140**
	(5.03)	**(5.12)**
High school graduate × county employment rate	**−.010**	**−.010**
	(−3.03)	**(−2.99)**
New hire (less than 1 year of tenure)	−.110	−.099
	(−11.88)	(−9.48)
Union member	.169	.167
	(21.08)	(20.77)
County unemployment rate	.0015	.0014
	(.51)	(.51)
Average quits per year in the sample	...	−.126
		(−2.28)
(Average quits per year in the sample)^2	...	.118
		(1.96)
High school graduate × average quits	...	−.070
		(−1.63)
Number of degrees of freedom	9,517	9,514

NOTE.—*t*-statistics are in parentheses.

* Other independent variables were eight location dummy variables, 5-year dummy variable for each year, and an intercept term. This model was also estimated with tenure and tenure squared as independent variables. The coefficients on high school graduate and high school times unemployment were unchanged to two decimal places.

6 percent, going from 11 to 12 years of schooling has three times the effect on wages as going from 10 to 11 years. At a zero unemployment rate, completion of twelfth grade has more than four times as large an effect on wages as completion of eleventh grade.[21] At a 15 percent unemployment rate, completion of twelfth grade has the same effect on wages as completion of eleventh grade.

The procyclical behavior of the wage premium for completion of high school is especially surprising given the occupational distributions of high school dropouts and high school graduates. High school dropouts are overrepresented among blue-collar workers: 54 percent of the blue-collar workers in the PSID sample were high school dropouts, while only 10 percent of the white-collar workers were high school dropouts. Raisian (1983) finds that the wages of blue-collar workers relative to those of white-collar workers are procyclical.[22] Hence the different occupations chosen by high school graduates and high school dropouts would lead the wage premium for high school graduation to behave countercyclically.[23]

Of course the procyclical behavior of the wage premium associated with graduation from high school may be due to a procyclical effect of education on wages. Unfortunately the data were inadequate to obtain meaningful estimates of the cyclical effects of both high school graduation and education on wages.

Table 9 also shows that average quits per year that a worker was in the sample, when evaluated at the mean average quit rate of 0.11, has a negative effect on wages. This effect appears stronger for high school graduates than for high school dropouts. However, the statistical significance is weak. The estimate obtained of the effect of the interaction between high school graduation and previous quits on real wages was sensitive to the particular independent variables included and the number of observations. When the model was estimated including constructed wage data from 1968 to 1976, the estimated coefficient (which as argued in n. 20 is likely to be subject to serious measurement error) was -0.080 with a t-statistic of -2.60 (the number of observations was 13,604). On the other hand, when average

[21] The effect of high school graduation on wages includes the effect calculated from the continuous education variables for completion of twelfth grade as well as the coefficient on the dummy variable for high school graduates.

[22] I used the same classification scheme as Raisian (1983) in determining which two-digit occupations were blue-collar and which were white-collar.

[23] Note that the formulation I have chosen has the form $\ln W_{it} = X_{it}\beta + \eta_{it}$, where $\eta_{it} \approx N(0, \sigma^2)$. Coleman (1984) has argued against assuming that η_{it} is distributed independently and identically if one wishes to study cyclical effects. He argues persuasively that a more reasonable model is $\ln W_{it} = X_{it}\beta + \eta_{it} + \epsilon_t$. Consequently, the t-statistics for the ordinary least squares estimates of the cyclical variables should be viewed with caution. (However, most of the variance in county unemployment rates is due to cross-sectional rather than cyclical effects.)

quits was replaced by ln(average quits + 1), the coefficient on high school graduates times ln(average quits + 1) fell to −0.072 with a *t*-statistic of −1.31 while the coefficient of ln(average quits + 1) was −0.081 with a *t*-statistic of −1.63.

V. Corroborative Evidence

The PSID provides some evidence suggesting that the discontinuous increase in earnings associated with graduation from high school is not due to a discontinuous increase in the cognitive skills of high school graduates associated with their superior learning ability. This would be an implication of human capital models in which choices of levels of education are determined by the efficiency with which individuals learn.

In 1976 and 1978 respondents to the PSID were asked the training required for an average worker to learn their job. Better-educated individuals reported having jobs requiring more training. However, after correcting for the continuous effect of education, I did not find that high school graduates had jobs requiring more training. Since I had previously found a large negative effect of high school graduation on quit propensities and quit rates (see tables 2–5), I would have expected that, if high school graduates and dropouts derive the same benefit from training, the former would have jobs requiring more training. The data in table 10 suggest that while employers may believe that better-educated workers have a comparative advantage in acquiring job training, they do not believe that there are special skills in assimilating training associated with high school graduation.[24]

One problem with this interpretation of the data in table 10 is that individuals were asked how long it would take the average worker to become fully trained on their job. It is not clear how the respondents interpreted the phrase "average person." They might interpret the average worker as someone like themselves. In that case high school graduates could be assigned to more complex jobs, but if they learn those jobs more rapidly, the two effects could cancel one another.

In table 11, I took a different approach to this question. I estimated the effect on wages of the interaction between high school graduation and on-the-job training. In column 1 the effect was estimated for all workers, while in columns 2 and 3 I considered only workers who had

[24] An alternative explanation for the data in table 10 is that skills learned in high school by high school graduates are substitutes for skills taught on the job. However, that explanation seems inconsistent with better-educated workers' being assigned to jobs requiring more training unless it is only the skills associated with high school graduation that are substitutes for training, while the other traits learned in school complement the skills learned on jobs.

TABLE 10

YEARS OF TRAINING REQUIRED FOR JOB IN 1976–78
(PSID Sample, Ordinary Least Squares Estimation)

Independent Variable*	Coefficient
Intercept	−2.46 (−5.36)
Education	.310 (2.13)
Education squared	−.24 (−1.46)
Education cubed	.00119 (2.10)
Work experience	.058 (4.63)
Work experience squared	−.0010 (−5.05)
Age	.033 (3.80)
Race = nonwhite	−.87 (−16.29)
High school graduate	**−.02** **(−.23)**
College graduate	−.37 (−.197)
Married	.12 (1.69)
Disabled	.012 (.88)
R^2	.1638
Number of observations	9,941

NOTE.—*t*-statistics are in parentheses.
* The other independent variables were eight dummy variables for different areas of the country.

completed the training needed to fully learn their job. When I estimated the coefficients listed in columns 1 and 2, I obtained the surprising (astonishing?) result that the continuous effect of education on the impact of on-the-job training on wages has the opposite sign as the effect of graduation from high school. (When I omitted the interaction between education and completed training, I estimated a *negative* coefficient for the effect of the interaction between high school graduation and completed training on wages.)

On the other hand, the estimated coefficients in column 3 suggest that these results may be due to nonlinearities in the interaction between education and returns to on-the-job training. In column 3 I allowed for a cubic specification of the interaction between education and on-the-job training and found that, for relevant levels of education, differences in education levels do not affect the impact of on-

TABLE 11

EFFECT OF TRAINING ON LN WAGES OF MALE PRIVATE-SECTOR EMPLOYEES IN PSID SAMPLE, 1976–81

Independent Variable*	Coefficient (1)	(2)	(3)
Education	.073 (2.78)	.059 (2.09)	.057 (20.83)
Education squared	−.0064 (−2.33)	−.0056 (−1.87)	...
Education cubed	.00030 (3.48)	.00030 (3.12)	...
Required training for job currently held	.025 (8.00)	...	...
Attained training (includes workers still in training)	.056 (5.57)	...	...
High school graduate × attained training	.013 (2.07)	...	...
Education × attained training	−.0036 (−3.98)	...	...
High school graduate	.116 (4.43)	.105 (3.49)	.057 (2.01)
High school graduate × county unemployment	−.0093 (−2.86)	−.010 (−2.82)	−.0087 (−2.36)
Years of completed training	...	.099 (10.01)	.527 (7.49)
High school graduate × years of completed training	...	.017 (2.53)	.0045 (.48)
Education × years of completed training	...	−.0049 (−5.08)	−.121 (−6.15)
Education squared × years of completed training	...	...	.00975 (5.39)
Education cubed × years of completed training	...	...	−.00026 (−.487)
Number of degrees of freedom	9,510	6,526	6,526
R^2	.522	.548	.543

NOTE.—t-statistics are in parentheses.
* The other independent variables were the same as those in col. 2 of table 9, except the interaction between high school graduation and previous quits was omitted. In col. 3 education squared and cubed were also omitted. The sample size falls in cols. 2 and 3 because I included only workers who had completed their training.

the-job training on wages, nor does there appear to be a discontinuous change in the impact of on-the-job training on wages associated with graduation from high school.

Note that the coefficient of required training in column 1 is 0.025 with a t-statistic of 8.00. This coefficient is consistent with the evidence presented in Weiss (1984) that workers on more complex semiskilled production jobs have higher quit rates, suggesting that workers dislike those jobs and hence need to be rewarded with a compensating wage differential. It is also consistent with efficiency wage models that predict that firms pay higher wages in jobs for which the returns to effort or to ability are highest.

There are several reasons why the estimated coefficients obtained in all three columns of table 11 should be viewed with particular caution. First, the range of realized education levels is fairly small, and education is allowed to affect wages in a wide variety of ways leading to serious problems of multicollinearity. Second, the process by which workers choose or are assigned to jobs is not known. The resulting simultaneous equation bias could be seriously affecting the estimated coefficients. Third, as noted in the discussion of the results in table 10, it is not clear what the training requirements reported by workers are actually measuring. These problems are added to the usual problems of model misspecification, misreporting of wages, and omission of fringe benefits in measures of reported wages that are likely to affect the estimated coefficients in table 11.

VI. Sample Selection Bias[25]

As discussed above, there are persuasive reasons for believing that sample selection bias is not a serious problem for the analysis. However, to the extent sample selection bias is a problem, the results obtained above are stronger than would be indicated from the estimated coefficients and *t*-statistics.

Since only a trivial number of applicants were rejected for reasons other than low scores on the dexterity test, I shall restrict the discussion in this section to the biases introduced from the application decision of workers. Thus we shall consider a model in which the sorting effect of wages differs across different groups of workers (see Weiss 1980).

Consider an unobserved application equation. A worker applies for a job with this firm if and only if

$$X_0\gamma_0 > \epsilon_1, \tag{10}$$

where X_0 includes all observable characteristics of the worker and ϵ_1 includes unobserved characteristics of a worker as well as random noise. Having applied and been accepted for this job, the worker subsequently quits if and only if

$$X_0\gamma_1 > \epsilon_2 | (X_0\gamma_0 > \epsilon_1) \tag{11}$$

and is absent if and only if

$$X_0\gamma_2 > \epsilon_3 | (X_0\gamma_0 > \epsilon_1). \tag{12}$$

Because better-educated workers had higher wages on their previous jobs and generally have higher reservation wages when unem-

[25] The material in this section is a direct application of the seminal treatment of these issues in Heckman (1976).

ployed, we would expect the coefficients on education and high school graduation in (10) to be negative. Since (10) is not estimated, within the sample we would expect ϵ_1 and education to be negatively correlated. It seems reasonable to expect that, at least for employed applicants, ϵ_1 is positively correlated with ϵ_2 and ϵ_3: A worker who was easily induced to leave his previous job is likely to be easily induced to leave his current job, and a low level of job commitment might also be expected to lead to a higher than expected probability of being absent. Therefore, we expect ϵ_2 and ϵ_3 to be negatively correlated with education in equations (11) and (12).

Hence, for this sample there is positive bias on the estimated coefficients on education and high school graduation in the quit and absenteeism equations, resulting in an underestimate of the magnitude of the negative correlation between high school graduation and the propensity to be absent or to quit. (In the market equilibrium it is the unbiased correlation between high school graduation and quit or absenteeism propensity that is relevant for explaining the positive correlation between high school graduation and wages.)

VII. Review of Results

Recent large-sample studies have shown returns to education in the region of 4–5 percent. There is a not dissimilar rate of return for the pay on their previous job for workers in the proprietary sample I assembled. A large component of this rate of return is the discontinuous increase in wages associated with high school graduation.

Presumably the higher earnings of high school graduates are due to their better performance relative to high school dropouts. I investigated four components of performance: output per hour, comparative advantage in more complex jobs, propensity to quit, and propensity to be absent.

For the sample of semiskilled production workers, high school graduation appears to be uncorrelated with output per hour, and high school graduates did not appear to have a comparative advantage in more complex production jobs. On the other hand, high school graduates were significantly less likely to quit or to be absent. The wage premium received by high school graduates in the PSID sample appears to be procyclical. Consequently, if a low quit propensity and a low rate of absenteeism are more valuable during booms than slumps, these data suggest that a considerable fraction of the estimated return to high school graduation is due to unobserved traits that are associated with that credential.

Finally, I found that high school graduates in the PSID do not have jobs requiring more training than would be expected from a continuous relationship between education and required training.

Appendix A

A Description of the Proprietary Data Set

Each of the workers in this sample was initially paid according to the same nonlinear piece rate. The form of this pay schedule was such that for newly hired workers wages were a convex function of output. When a worker achieved 83 percent of expected output for 1 month, he or she was assigned to a pay group. All the members of a pay group received the same pay. Pay was proportional to the output of the group—the average group size was 126 members—and promotion opportunities were insignificant. Thus there was little financial incentive for group members to achieve high levels of output. Consequently, I used the output of workers only during their first month on the job in this study. (Among experienced workers, the range in output from the bottom to top deciles was less than 20 percent of the mean.)

Almost all workers were assigned to a pay group within their first 3 months on the job. Therefore, expected lifetime earnings differences among the newly hired workers at each location were trivial.

For each worker data were available on sex, age, marital status, education, employment status when he applied for work, an estimate of the time required for an average employee to learn the worker's job as well as his output per hour (measured in physical units and normalized by the industrial engineering force to be equivalent across jobs), number of days absent, number of occasions absent (consecutive days absent are a single occasion), whether or not the worker quit, and the date a quit occurred. The workers did not know that they were going to be subjects of an empirical study. All the data were routinely collected either by the personnel office, industrial engineers, or foremen at the three locations. Although the total sample contained 2,920 individuals, complete data were not available for all workers. In some cases workers were assigned jobs for which there were no measures of job complexity; in other cases the worker failed to answer all the questions on the application form. Also at each location different information was available in the personnel records. In location C, race data were not available and only the screws half of the Crawford Physical Dexterity Test was administered; in the other two locations both the pins and screws sections of the dexterity test were administered before a worker was hired. The scores on this test were used by the firm in deciding whether to hire the worker but were not used in assigning workers to jobs. Job assignments were random.

An additional problem arose at location C: the plant was divided into two halves, with significantly different promotional opportunities. Unfortunately the data did not reveal to which half of the plant particular workers were assigned, that initial assignment may not have been random. At that plant, workers assigned to more complex jobs had better promotional opportunities. Because of these unobserved differences in promotion opportunities, the relatively small sample size at that location, and the missing data on race, prior experience, employment status when they applied for this job, and scores on the pins section of the dexterity test, I did not use data from that location in estimating quit equations.

Table A1 presents some summary statistics that provide a context within which to evaluate the data.

TABLE A1

MEAN VALUES OF THE RELEVANT VARIABLES

	Plant A	Plant B	Plant C
Percentage white	.72	.76	NA
Percentage black	.24	.20	NA
Age	24.6	25.3	26.7
	(7.52)	(7.07)	(8.23)
Education	12.1	12.2	11.9
	(1.20)	(1.26)	(.98)
Percentage employed at time of application (1 if employed, 0 otherwise)	.64	.29	.38
Percentage male	.57	.19	.41
Percentage married	.43	.48	.42
Tenure on previous job (in years)	1.56	NA	NA
	(2.04)		
Previous work experience (in years)	3.54	NA	NA
	(7.74)		
Weeks to learn job	7.5	5.1	11.7
	(4.9)	(2.4)	(4.4)
Score on screws test	22.6	21.3	24.0
	(5.08)	(4.20)	(4.02)
Score on pins test	22.4	23.5	NA
	(4.10)	(3.64)	
First-month output	111	63	105
	(19.7)	(22.2)	(29.2)
Percentage days absent	2.96	2.3	3.0
	(6.53)	(4.23)	(4.38)
Percentage occasions absent	1.2	1.1	1.12
	(4.5)	(1.4)	(1.1)
Quit rate in first 6 months on job (%)	9.8	18.2	12.3

NOTE.—Standard errors are in parentheses below the relevant variables. NA means data were not available. For the subsample used to estimate the output equation, 56 percent were in plant A, 34 percent in plant B, and 10 percent in plant C.

Appendix B

To evaluate the cost of absenteeism to firms, the nature of the production process is critical. Traditional economic analyses of absenteeism assume that a worker's marginal product is equal to his wage and the cost of absenteeism is the wage of the worker. In this Appendix, we will consider the case in which the production process used by all firms requires k workers to operate. If more than k workers are present, the extra workers are redundant; they do not increase output. If fewer than k workers appear, output is zero.

To simplify the notation, assume output is linear in the number of workers and normalize the value of output to be equal to k. The number of workers hired is denoted by n, the wage of each worker by ω, the probability that a worker appears for work by p, and the expected profit of a firm employing n workers by $\pi(n)$. Let $P(S_n \geq k)$ denote the probability that at least k workers are present when n workers are hired. Finally, assume that the absenteeism rate of each worker is common knowledge and that workers are not paid if

TABLE B1

RELATIONSHIP BETWEEN ABSENTEEISM AND WAGES

Probability of Being Absent	Profit-maximizing Number of Employees	Equilibrium Daily Wage
.20	30	197.66
.19	30	198.42
.18	29	199.51
.17	29	200.43
.16	28	201.46
.15	28	202.64
.14	27	203.47
.13	27	205.07
.12	27	205.56
.11	26	207.70
.10	26	208.60
.09	25	210.53
.08	25	212.05
.07	24	213.59
.06	24	216.02
.05	23	217.02
.04	23	220.87
.03	23	222.08
.02	22	227.61
.01	21	231.60
.00	20	250.00

they are absent (this assumption is made so that we can focus on the indirect costs of absenteeism rather than the direct cost of paying for an absent worker): $\pi(n) = \tilde{k}P(S_n \geq k) - np\omega$. The expected marginal product of the $n + 1$ worker is $\tilde{k}[P(S_{n+1} \geq k) - P(S_n \geq k)]$. The net expected value of the marginal worker is

$$\begin{aligned}\pi(n+1) - \pi(n) &= \tilde{k}P(S_{n+1} \geq k) - (n+1)p\omega - \tilde{k}P(S_n \geq k) + np\omega \\ &= p\tilde{k}P(S_n \geq k-1) + (1-p)\tilde{k}P(S_n \geq k) \\ &\quad - \tilde{k}P(S_n \geq k) - p\omega \qquad \text{(B1)} \\ &= p\tilde{k}[P(S_n \geq k-1) - P(S_n \geq k)] - p\omega \\ &= p[\tilde{k}P(S_n = k-1) - \omega].\end{aligned}$$

The expected value of the marginal worker is the probability that the worker is decisive (enables the plant to operate) times the value of the production process, minus that worker's expected cost to the firm.

A firm increases the number of workers it employs as long as (B1) is positive; it chooses the smallest n such that

$$p\tilde{k}\frac{n}{(n-j)j}p^j(1-p)^{n-j} - p\omega < 0, \qquad \text{(B2)}$$

where $j = k - 1$.

If we consider the case in which workers are paid even if they are absent, the usual policy in U.S. firms for routine levels of absenteeism, (B1) is rewritten as $p\tilde{k}P(S_n = k - 1) - \omega$ and (B2) as

$$p\tilde{k}\frac{n}{(n - j)j}p^j(1 - p)^{n-j} - \omega < 0.$$

To illustrate the effect of absenteeism on the equilibrium value of a worker to the firm, let us consider a production process that requires 20 workers to operate, generates \$10,000 of income per day if it operates, and has a fixed cost of \$5,000 per day whether it operates or not. We shall assume that workers are not paid if they are absent. In equilibrium, if the probability of a worker's being absent is known by all employers, firms compete for workers so that wages are bid up to the level at which each production process earns zero profits, and all the workers at a given plant have the same absenteeism rate, the relationship between absenteeism and wages shown in table B1 follows. These calculations suggest that, even if employees are not paid when they are absent, in competitive markets employers might pay significantly higher wages to workers with lower probabilities of being absent.

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