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HIGH SCHOOL GRADUATION, PERFORMANCE AND EARNINGS

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ABSTRACT

Using data from the Panel Study of Income Dynamics and a proprietary sample of semi-skilled production workers, this paper investigates the reasons for the discontinuous increase in wages associated with graduation from high school.

Associated with graduation from high school, we find a discontinuous decrease in a worker's propensities to quit or be absent. However, we do not find that high school graduates have a comparative advantage on production jobs requiring more training, nor, in the PSID sample, are high school graduates assigned to jobs requiring more training. Finally, we find that the wage premium associated with graduation from high school vanishes during severe slumps, periods in which employers are likely to be hoarding labor and in which quits and absences are least important to firms.

We conclude from this evidence that the sorting model of education provides a better explanation for the higher wages of high school graduates than does the human capital model.

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HIGH SCHOOL GRADUATION, PERFORMANCE AND EARNINGS*

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Using data from the PSID and from a proprietary data set of newly hired production workers, we investigate whether the discontinuous increase in earnings associated with graduation from high school is due to unobserved attributes that are correlated with both completion of high school and performance on jobs. This correlation is a basic assumption of sorting models of education.

We find that high school graduates have lower quit propensities and rates of absenteeism than would be expected from their other demographic characteristics

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(including any continuous effects of education).[1] [2] On the other hand, they do not have jobs requiring more training than would be expected from their demographic characteristics, nor do they appear to have a comparative advantage on more complex jobs.

Thus it appears that the discontinuous increase in earnings associated with graduation from high school is due to traits such as stick-to-itiveness or a low time discount rate that were acquired prior to 12th grade and that increased the likelihood of the individual completing high school. We found no evidence that the higher earnings of high school graduates were due to their superior cognitive skills.

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1. P. H. Mirves and E. E. Lawler 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 workers probability of quitting during their first year on the job by 10%, and assuming the average work year was 240 days, H.S. graduation would increase earnings of newly hired bank tellers by 3.5%.

2. Mirves 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 we estimate, H.S. graduation results in a 14% decrease in the percentage of days absent, then we would expect the negative correlation between absenteeism and education to lead to a 2% higher wage for H.S. graduates working as bank tellers.

   Allen (1981) used the 1972-73 Quality of Employment Survey to calculate the effect of wages on absenteeism. He found that the elasticity of the absence rate with respect to marginal wage rate varies from -.35 to -.48. Since these elasticities stem from partial derivatives we can not directly impute the elasticity of wages with respect to absenteeism from the inverse of Allen's estimates. However his results suggest that if, as we estimate, high school graduates are absent 14% less that would be expected from their other demographic characteristics, then these differences in absenteeism could explain the entire wage premium received by high school graduates. On the other hand, Allen's estimated coefficients are likely to be biased toward zero, leading to overestimates of the effect of absenteeism on wages.

   Allen also finds a negative, albeit minor, effect of high school graduation on absenteeism. However, since he includes wages as an independent variable in his absenteeism equation, the model being estimated is substantially different from ours-in which we are investigating the effect of education on wages. (If high school graduates are paid more because they are absent less, one might find that then including wages as an independent variable in the absence equation would obscure the relationship between high school graduation and wages).

Consequently, we conclude that some of the conventional estimates of rates of return to secondary education (those that do not include dummy variables for high school graduation) are capturing the effect of traits that were unlikely to have been learned in secondary school.

1. THE DATA

The data for this study comes from the Panel Study of Income Dynamics (PSID), with which the reader is presumably familiar, and from a proprietary data set consisting of the personnel files of 2,920 newly hired semi-skilled production workers employed by a high wage, unionized firm at three widely separated geographic locations: labeled A, B and C.

Each of these data sets have advantages and drawbacks. The PSID has two disadvantages. First, it does not have 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, it not impossible, using the PSID to determine whether lower quit rates of high school graduates are due to their job assignments or to unobserved attributes particular to the individual.

The main drawback of the data derived from personnel files is that it is suspect to serious sample selection biases. That issue shall be addressed on pages 10 and 11 of this section and at greater length in section 4. Let us first describe in detail the proprietary data used in this study.

Each of the workers in this sample was initially paid the same non-linear piece-rate. When a worker achieved 83% of expected output for one month, the worker 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 is 126 members — and promotion opportunities were insignificant, so there was little financial incentive for group members to achieve high levels of output. Consequently, we only used the output of workers 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% of the mean).

Almost all workers were assigned to a pay group within their first three months on the job, and the lower bound on earnings of newly hired workers was 65% of the wage in a pay group, with earnings rising less than proportionately to output up to a ceiling at 83% of the pay group wage. Therefore, expected lifetime earnings differences among the newly hired workers at each location were trivial.[3]

For each worker data were available on sex, age, marital status, education, employment status when the worker applied for work, and an estimate of the time required for an average employee to learn the worker's job as well as the worker's

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3. Because in our sample wages are independent of performance, we were able to avoid the sample selection biases present if a worker's wage were included as an independent variable. If employers value a low propensity to be absent or to quit or a high level of productivity, then workers who have those qualities (or are perceived by employers as more likely to have those qualities) will receive higher wages and more training. Hence if the wage were an independent variable the error term in a quit, absenteeism, or productivity equation would be correlated with the wage variable and hence with all other independent variables that are correlated with wages.

   Although all the workers at each location in our sample received the same wage, they obviously did not have the same alternative opportunities: the effect of differences in alternative opportunities is discussed below.

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,[4] whether or not the worker quit and the date a quit occurred. The workers did not know that they were the 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 was 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.[5] 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,

4. Consecutive days absent are recorded as a single occasion of absenteeism.

5. The Crawford Physical Dexterity Test is a two-part test. In the first part an individual uses a tweezers to place pins into sleeves in a pegboard. In the second part small screws are screwed into sleeves in the pegboard. Each part of the test takes three minutes and the number of operations performed in that time is tabulated.

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, we did not use data from that location in estimating the quit equations below.

Table 1 presents some summary statistics which provide a context within which to evaluate the data. NA denotes data that were not available.

Insert Table 1 about here

Because this data set contains direct measures of three aspects of productivity: output per hour, absenteeism (both percentage of days absent and occasions absent), and quit propensity, it is possible to separately measure the effects of education for each of those aspects of productivity. In addition, because the data includes a measure of the complexity of the job to which the worker was assigned, one can estimate whether better educated workers have a comparative advantage on more complex jobs.[6]

Insert Figure 1 about here

Before showing how the data provides information on the relative contributions of the sorting and learning aspects of education to earnings, we first examine the effect of education on earnings for some of the workers in our sample, and discuss whether conclusions drawn from this subsample are applicable to a broader population.

At plant A job applicants were asked their wage at their most recent job and whether or not they were currently employed at that job. Using that data, we estimated the effect of an additional year of education on the previous pay of those workers who were employed when they applied for their current job.[7] [8] Column 3

6. In general, studies that rely on productivity measures of experienced workers within a given job classification are subject to important sample truncation biases. For example, suppose that education has a positive effect on performance within a given job, and that, as Medoff and Abraham find, education (and performance) have positive effects on a worker's probability of being promoted. Then among workers within a job classification, (i.e., workers who have not been promoted) education could be negatively correlated with performance even though education increases the performance of workers on the job. These effects can be seen in Figure 1. The downward sloping line is the criterion for promotion from job 1 to job 2. If the distribution of perceived productivities were the same at all education levels, and were a symmetric increasing hazard rate distribution, such as the normal or uniform, then on both jobs the better educated workers would be the less able. This problem does not arise for our data since none of the workers were promoted. (Note that this sample selection bias does not affect the substantive point made by Medoff and Abraham that within a sample of workers on the same job wages rise with experience despite productivities falling with experience. We are simply pointing out that in jobs in which there are significant promotional opportunities it is difficult to infer the direct effect of experience or education on productivity.)

7. We excluded unemployed workers from the sample since the wage on their previous job, one which they either couldn't keep or chose to leave, does not seem representative of their expected earnings. In each case wages at the previous job were used since wages for experienced workers on their current job are a function solely of seniority and the output of the relevant pay group.

8. Obviously this is a biased estimate of the effect of education on earnings for workers in the population — the workers in our sample chose to leave the jobs for which we have recorded their

of Table 2, contains estimates of the effect of various demographic variables on the logarithm of the previous hourly wage of workers at that plant. Columns 1 and 2 reproduce the coefficients estimated by Wesley Mellow using the 1979 current population survey.

Insert Table 2 about here

Table 2 can be used in various ways depending on how ambitious we wish to be. First, the estimates of $\frac{\partial \ln \text{wage}}{\partial \text{ educ}}$ provide a measure of the effect of education on the earnings (in their previous job) for the subsample of workers for whom we have earnings data. We used that estimate as a standard, and then calculated 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 about the sample being 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. We assume that performance on current jobs is correlated with wages on previous jobs. We 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% effect on the hourly wage on other jobs for the workers in this

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earnings. In section 3, we analyze the effect of this bias.

sample.[9] Including high school graduation as an independent variable we found that for the workers in our sample completion of high school had 2 to 3 times as great an effect on wages as did completion of 11^{th} or 13^{th} grade.

A second use of the data in Table 2 is to provide a partial check of the importance of sample selection bias for our sample if one wishes to generalize our results outside our sample. Since our major concern is in explaining the positive correlation between education and earnings, it is useful to check to see if the return of education in our 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 are less well educated workers. However, the estimated value of the return on education for workers for whom we had wage data is roughly the same as returns estimated using the C.P.S. sample. Thus it does not seem that our sample differs from the C.P.S. 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 wage is the same for our sample as for the randomly selected C.P.S. sample.

The personnel practices at these plants also provide grounds for believing that our sample is representative. Only 22% of applicants whose applications were reviewed were rejected. Of those, 85% were rejected because of a low score on the Crawford Physical Dexterity Test. Our estimation procedures included the

9. We had usable data on previous pay for 77% of the workers employed at the midwest location (those workers comprised 43% of the entire sample).

worker's score on the dexterity test as an independent variable, thus eliminating that potential source of sample selection bias. In addition, the average pay increase for workers for whom we had wage data was 103%, and workers waited more than 36 hours in freezing temperatures to receive job application forms. This evidence indicates that the firm was paying above market wages, reducing 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.[10] 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 recall notices were recently mailed to former employees: 90% of those who had found alternative work quit their jobs to return to work for the firm. Consequently we have assumed that sample selection biases were small and have not corrected for them.[11]

10. Of course some residual sample selection bias occurs if unobserved attributes that lead workers to wait on line longer, such as the ability to withstand the cold, are correlated with both performance measures and with observed attributes we are studying. At the location A we know the individual's place on line. It was uncorrelated with any of our measures of performance. Consequently, we have assumed that this source of bias is small and have disregarded it.

11. Alternatively we could have used the Bloom and Killingsworth (B-K) procedure to correct for sample selection bias. (Because the characteristics of excluded observations are unknown, the standard Heckit correction can't be used.) However, the B-K 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 only identified if the assumed distribution of the error term of the selection equation is nonlinear. See Olsen (1982) and Muthén and Jöreskog (1983) for discussions of this issue.

2. MODELS

Typically an individual's choice of a level of education is a function of both observable and unobservable traits. The traits that are observable for our sample, and that affect the schooling decision, include age, race, sex and manual dexterity. We 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 are 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. We would expect that one skill that is 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 our sample differs (almost the entire sample had at least nine years of education) and influenced the individual's choice of a level of education. We assume that skills such as 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, we intentionally biased our analysis in favor of a learning explanation for the correlation between education and earnings.

The performance equations we estimate are: whether or not the worker quit during his first six 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 his 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 our analysis are years of education, H.S. graduation, job complexity, and a measure of the "match" between education level and job complexity.

$$\text{Match} \equiv [\text{education - mean (education)}] \text{ times } [\text{job complexity - mean (job complexity)}].$$

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 average productivity rating for the factory on that job). The main component in the industrial engineers' calculation is the number of times per week an experienced worker performs the task.

In general we would expect stick-to-itiveness (those unobservable traits which lead individuals to choose higher levels of schooling) 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 is to improve alternative opportunities, increasing the probability of a quit.

2.1 Quits

Let $\bar{S}_i$ denote worker i's present value of his current job, $\bar{V}_i$ 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 worker i is risk neutral and quits if and only if

(1) $$\bar{V}_i - \bar{S}_i > M_i.$$

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 male workers anticipate working until they are 65 years old, and normalizing $t = 0$ for the time when the quit decision is made

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

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, $\beta_0 < 0$. Next, let $p_i(t)$ represent the probability 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

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

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

where $0 < \gamma < 1$. Then, defining $\delta = (r-u)$ 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 $\frac{(e^{\delta(65-a+\beta_0 F)}-1}{\delta}$ by $g(a,\delta,\beta_0 F)$, and substituting (5)-(7) into (4),

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

Clearly, the net gain from changing jobs, the left hand side of (8), will be measured with error. We assume this error term, denoted μ_Q, is distributed normally with zero mean and is i.i.d. Therefore, the quit equation we estimate 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 we 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 lay-off from a desirable previous job might be likely to quit when recalled from the layoff).[12] The factors affecting

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12. One might argue that employers should be concerned with the total effect of education on the probability of a worker 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

alternative opportunities could also affect job satisfaction. For example, we 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.

Finally we would expect stick-to-itiveness (as measured by years of education and high school graduation), and whether the worker is married to impose additional mobility costs. Those variables are not multiplied by $g(a,\delta,\beta_0 F)$. They enter directly into the quit equation as elements of X_3.[13]

As can be seen in Table 3A our major hypothesis is confirmed. High school graduation has a strong negative effect on the probability of a worker quitting. 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 school.

Insert Table 3A about here

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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, which contributes to differences in the equilibrium wage at different education levels.

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

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 six months and selection bias was avoided by also omitting workers who would have been laid-off before six months had passed had they not already quit. (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 $g(a,\delta,\beta_0F)$ are high. Although statistically insignificant, they have values consistent with the model. Individuals such as white males that 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. Our estimates suggest that females in our sample anticipate spending 7 fewer years in the labor force than do males. This finding is consistent with unpublished research by Jacob Mincer. Using the PSID sample Mincer finds that women spend roughly 25% less time in the labor force than do men. Finally the similarity in the "t" statistics obtained using the gradient, hessian and White methods suggests that the model is not grossly misspecified.

To obtain more precise estimates of the coefficients, and to make use of Mincer's findings on the shorter work-life of women, we reformulated equation (9) as

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

where $h(a,\delta,\hat{\gamma}F) = \dfrac{e^{-.05(1-.25F)(65-\text{age})} - 1}{-.05}$

That is, we impose the restrictions that $r - \mu = .05$ and that the effective work-life of women is 25% shorter than that of men.

Equation (9a) was estimated using a probit estimation procedure. The estimated coefficients. and the effect of a change in each independent variable on the probability of a worker quitting are presented in Table 3B.

Insert Table 3B about here

Note that in Table 3B job complexity and match were not included as independent variables. If job complexity and match are included as independent variables when (9a) is estimated, the sample size falls to 1532 and the absolute value of all the "t" statistics also falls. However, none of the results shown in Table 3B are significantly affected: high school graduation is estimated to correspond to a reduction in the probability of the worker quitting of -.052 at a 10% quit probability and -.083 at a 20% quit probability. Similarly if quit propensity is estimated separately for men and women in our sample, the qualitative effects in Table 3B hold for each subsample.

We checked the finding that high school graduation significantly affects quit propensities using the 1968-1982 PSID sample. We estimate that each year of post—primary education has neither a statistically nor economically significant effect on a worker's quit rate, while completion of high school is associated with a reduction in the individual's quit rate of roughly one-third.

Insert Table 4 about here

2.2 Absenteeism

In Appendix A, we present a theoretical model that shows why differences in expected absenteeism rates can significantly affect the wages of workers. In this section, we estimate the discontinuities in expected absenteeism rates associated with H.S. graduation for workers in our sample.

In estimating the determinants of absenteeism we assume that a worker is more likely to be absent the higher is his level of job dissatisfaction, and the lower his persistence. As in the quit equation, job satisfaction is measured by the elements of X_2. We use education and high school graduation as proxies for persistence—an individual who had insufficient persistence to complete high school is likely to have poorer than expected attendance habits. We shall assume that absenteeism (both days and occasions of absenteeism as a percentage of possible days worked) is a linear function of X_2, our proxies for diligence, and a normally distributed error term.

In the quit equation we were able to 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 × $g(a,\delta,\beta_0 F)$ 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 to us 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 could be estimating the effect of lower (or higher) levels of job satisfaction of better educated workers on their absence rates. However, the results from the previously estimated quit equations suggest this is not happening.

As mentioned, two measures of absenteeism are used as dependent variables.

Percentage of days absent is estimated using a Tobit procedure.[14]

We calculated the percentage of days workers were absent during their first six months on the job; however, because not all the workers in our sample completed 6 months of work those observations were weighted by the square root of the number of days for which they were observed.

14. We are implicitly assuming 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.

Insert Table 5 about here

We also used occasions of absent as a dependent variable. In those calculations we restricted ourselves to individuals who worked for 6 months, and used the negative binomial procedure described by Hausman, Griliches and Hall (HGH).[15]

Insert Table 6 about here

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. On the other hand education has a positive, albeit statistically insignificant, effect on absenteeism. Thus, it seems unlikely that high school graduates learned in secondary school the traits that gave them 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.

15. The implicit assumptions made by using the negative binomial model are that incidents of absenteeism (several consecutive days absent are recorded as one incident) are generated from a binomial distribution which, because of the large number of observations for each individual, can be approximated by a Poisson arrival process; and that the λ parameter which describes the Poisson process has a gamma distribution across the population. HGH describe a maximum likelihood procedure to compute the effect of various independent variables on λ. (Because the probability of being absent is a function of whether the worker was absent on the previous day, the independence assumption required by the binomial distribution is inappropriate for modeling the percentage of days absent. It seems more reasonable to expect incidents of absenteeism to be independently distributed.)

2.3 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 plant. As previously noted, newly hired workers are randomly assigned to jobs and paid piece-rate. We assume that output per hour is a linear function of observed characteristics, including the match term described above.

Insert Table 7 about here

From Table 7 we see that matching better educated people, or high school graduates, to more complex jobs (the match term) has neither a statistically, nor economically, significant effort on output. This finding suggests 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 2 we computed the effect of education on the logarithm of their real wages on their previous job for workers in Plant A. Our best estimate was that each year of secondary education had a 4-5% effect on the pay of the workers in our sample. On the other hand, restricting the sample to workers in that plant we find that each year of education has only a 1.3% effect on the output of workers in that plant. This estimated effect is not statistically different from zero.

Figure 2 describes the distribution of jobs in our sample according to the number of weeks required to learn each job.

Insert Figure 2 about here

3. EFFECTS ON WAGES

Although one should be extremely cautious about generalizing the results in Section 2 beyond our sample of semi-skilled production workers, the PSID data provides evidence of the impact of a low quit propensity on the wage premium received by high school graduates.

There is considerable evidence that during business slumps firms hoard workers: pay wages above the value of the product of the workers. Implicit contract theory maintains that these high levels of wages and employment are consequences of optimal risk sharing between firms and workers. Consequently during business slumps we would expect firms to benefit from (or be hurt less by) quits and unpaid absences. Consequently, to obtain quantitative estimates of the effect of the low quit propensity and low absenteeism of high school graduates on their wages, we interacted county unemployment rates with high school graduation while estimating a wage equation for privately employed males in the PSID data set between 1968 and 1982. We 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 non-linearities generating these results we replaced the dummy variable for high school graduation first with, a dummy variable for completion of 11^{th} grade and then with a dummy variable for completion of 13^{th} grade.

Neither of these dummy variables were statistically significant. Thus the results do not seem due to the dummy variable for completion of high school capturing some non-linearities in the relationship between calculation and wages.[16]

Insert Table 8 about here

As can be seen in Table 8, if the county unemployment rate is zero, graduation from high school has roughly 4.4 times as great an effect on wages as does completion of 11th grade. While, if the county unemployment rate is over 15%, the wage premium received by high school graduates disappears. At the mean county unemployment rate, completion of 12^{th} grade has roughly four times the effect on wages as does completion of 11^{th} grade.

This pro-cyclical behavior of the wage premium for completion of high school is especially surprising given the occupational distributions of high school dropouts

16. Nominal wages were used as a dependent variable with the natural logarithm of the C.P.I. as an independent variable to avoid restricting the coefficient of the ln C.P.I. to 1.

and high school graduates. High school dropouts are over represented among blue collar workers: 54% of the blue collar workers in the PSID sample were high school dropouts, while only 10% of the white collar workers were high school dropouts. Raisian finds that the wages of blue collar workers relative to those of white collar workers are procyclical.[17] Hence the different occupations chosen by high school graduates and high school dropouts would lead the wage premium for high school graduation to behave counter-cyclically.[18]

3.1 Corroborative Evidence

If our analysis were wrong and the effect of high school graduation on earning had nothing to do with the lower quite propensities of high school graduates. then previous quits would have the same effect on the wages of high school graduates as on the wages of high school dropouts. Previous quits have no discernable effect on the wages of high school dropouts but a large negative effect on the wages of high school graduates.

Insert Table 9 about here

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17. We used the same classification scheme as Raisian in determining which 2 digit occupations were blue collar and which were white collar.

18. Note that the formulation we have chosen has the form $\ln W_{it} - X_{it}\beta + \eta_{it}$ where $\eta_{it} - N(0,\sigma^2)$. Coleman has argued against assuming that η_{it} is distributed i.i.d. 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 our OLS estimates of the cyclical variables should be viewed with caution. (Most of the variance in county unemployment rates are due to cross sectional rather than cyclical effects).

The evidence in Table 9 suggests that high school graduation is used as a screen for jobs in which a low quit propensity is highly valued. However, graduation from high school may be only necessary, but not sufficient, for obtaining those jobs. In particular among experienced workers a low realized quit rate may also be required.

The PSID also provides corroborative evidence 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.

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, we did not find that high school graduates were matched with jobs requiring more training. Since we previously found a large effect of high school graduation on quit propensities and quit rates, it would appear that the major determinant of whether individuals take jobs requiring significant amounts of training is how well they benefit from training, *not* their quit propensities (this interpretation is consistent with training being largely the acquisition of general, not firm specific, human capital).

Insert Table 10 about here

Thus the discontinuous increase in earnings associated with high school graduation does not seem to be due to the acquisition of skills that are complementary with on-the-job training.

On the other hand, the evidence presented in Table 10 suggests that the earnings gains associated with education are due, in part, to better educated individuals having attributes that enhance their benefit from training.

Thus far we have not directly addressed the question of whether the evidence we have presented supports a sorting rather than human capital theoretic explanation for the higher earnings of high school graduates. One of the reasons that it is so difficult to distinguish between the two explanations is that both could be based on unobserved (to the econometrician) attributes of individuals. In a pure sorting model, although no learning occurs during the relevant course of schooling, the same unobserved attributes that affect productivity also affect the private return to schooling. In a human capital model, unobserved attributes could indirectly affect productivity and affect the private return to schooling by affecting the efficiency with which the individual learns. In that version of human capital theory, some individuals go to school longer than others because they learn more in school.

A low rate of absenteeism and a low quit propensity could directly be affecting productivity (as in a sorting model). Or they could be correlated with the efficiency with which an individual learns (as in a human capital model).

However, if high school graduates went to school longer than dropouts because they learn more rapidly, and effectiveness of learning in school and learning on the job are positively correlated, then we would expect the high school graduates to have a comparative advantage at jobs requiring more training; and, in the PSID, to have jobs requiring more training.

As we've seen, high school graduates do *not* have a comparative advantage on production jobs and they do not report having jobs requiring more training. Thus, it seems unlikely that the discontinuity in earnings associated with graduation from high school is due to a discontinuous increase in the learning acquired by the high school graduates, as would be implied by human capital theory.

4. SAMPLE SELECTION BIAS

As we discussed above, we have strong reasons for believing that sample selection bias is not a serious problem for our analysis. However, to the extent sample selection is a problem our results 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, we shall restrict the discussion in this section to the biases introduced from the application decision of workers.

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

$$X\gamma > \epsilon_1 ; \tag{10}$$

where ϵ_1 includes unobserved characteristics of a worker. Having applied and been accepted at this job, the worker subsequently quits if and only if

(11) $$X_1\beta_1 > \epsilon_2 | (X\gamma > \epsilon_1),$$

and is absent if and only if

(13) $$X_2\beta_2 > \epsilon_3 | (X\gamma > \epsilon_1).$$

Because better educated workers had higher wages on their previous jobs, and generally have higher reservation wages when unemployed, we would expect the coefficients on education and high school graduation in (10) to be negative. Since (10) is not estimated, ϵ_1 and education are 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 commitment to his job 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 (13) and (14). Hence, for our sample there is an upward bias on the estimated coefficients on education and H.S. graduation in the quit and absenteeism equations, resulting in an underestimate of the magnitude of the negative correlation between H.S. graduation and the propensity to be absent or to quit. (Because most firms pay higher wages to better educated workers, they are not subject to this form of sample selection bias. In the market equilibrium it is the unbiased correlation between education and performance that determines the rate of return to education.)

5. REVIEW OF RESULTS

Recent large sample studies have shown returns to education in the region of 5%. We find a similar rate of return for the pay on their previous job for workers in our two samples. 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 higher productivity relative to high school dropouts. We investigated four components of productivity: output per hour, comparative advantage on more complex task, propensity to quit, and propensity to be absent.

High school graduation appeared to be uncorrelated with output per hour, and high school graduates did not appear to have a comparative advantage on more complex 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 is pro-cyclical. We estimated that when the county unemployment rate reaches 15% this wage premium vanishes. 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 portion of the estimated return to high school graduation is due to unobserved traits that are associated with that credential.

We also found that previous quits have a strong adverse effect on the earnings of high school graduates, but not on the earnings of high school dropouts. This difference in the impact of quits on earnings is consistent with the high earnings of high school graduates being due to their lower quit propensities.

Finally, we found that high school graduates were not assigned to jobs requiring more training than would be expected from a continuous relationship between education and required training. If the same skills that enable individuals to learn effectively in school also improve their learning ability on the job (as is indicated by better educated workers having jobs that do require more training), these data suggest that graduation from high school is not associated with more efficient acquisition of human capital.

Consequently, the discontinuous increase in earnings associated with graduation from high school seems due to unobserved attributes that are negatively correlated with absenteeism and quit propensity, and are positively correlated with the probability of the individual graduating high school.

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TABLE 1

Mean Value of the Relevant Variable

	Plant A	Plant B	Plant C
% White	.72	.76	NA
% Black	.24	.20	NA
Age	24.6	25.3	26.7
Education	12.1	12.2	11.9
% Employed at time of application (1 if employed, 0 otherwise)	.64	.29	.38
% Male	.57	.19	.41
% Married	.43	.48	.42
Previous work experience (in years)	3.54	NA	NA
Weeks to learn job	7.5	5.1	11.7
Score on screws test	22.6	21.3	24.0
Score on pins test	22.4	23.5	NA
1st month output	111	63	105
% days absent	2.96	2.3	3.0
% occasions absent	1.2	1.1	1.12
Quit rate in first six months on job	9.8%	18.2%	12.3%

TABLE 2

LN HOURLY EARNINGS IN 1979

Current Population Survey Sample Versus Weiss Sample
("t" statistics in parentheses)

	CPS With Size of Firm*	CPS With Unionization**	Plant A
Non-White	-.070 (-7.14)	-.073 (-5.24)	-.027 (-1.52)
Female	-.343 (-57.44)	-.262 (-21.86)	-.239 (-14.17)
Education	.047 (5.73)	.045 (3.46)	.185 (2.48)
$(\text{Education}/10)^2$	.084 (2.98)	.035 (.69)	-.570 (-2.03)
Tenure	.021 (10.60)	.012 (4.42)	.027 (.707)
$(\text{Tenure}/10)^2$	-.043 (-12.73)	-.029 (-6.26)	-.120 (-1.70)
Age-Educ.-6	.022 (11.96)	.014 (4.88)	.049 (4.03)
$((\text{Age-Educ.-6})/10)^2$	-.033 (-15.84)	-.042 (-6.73)	-0.60 (-5.45)
Educ. Times Tenure	.038 (2.98)	.062 (5.25)	.0006 (.19)
Educ. Times (Age-Educ.-6)	-.069 (-6.31)	-.039 (-3.89)	-.002 (2.30)
Married Dummy	yes	yes	yes
Number of Observations	18,551	18,551	1,251
R^2	.436	.524	.207

(The smaller R^2 in column 3 compared with columns 1 and 2 is due, in part, to less variance in earnings within the Weiss sample.)

Evaluating the effect of additional year of education, tenure and experience at the mean values in each sample for education, tenure and experience:

$\dfrac{\partial \ln (\text{wage})}{\partial \text{ educ.}}$	.064	.041	.044
$\dfrac{\partial \ln (wage)}{\partial \text{ tenure}}$	.016	.012	.031
$\dfrac{\partial \ln (\text{wage})}{\partial \text{ experience(proxy)}}$	.0056	.0040	.061

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* CPS data also has dummy variables for SMSA, region, occupation, industry, and either firm size or union membership, whether or not the worker was employed part-time, and unionization of industry.

** Weiss data is from the only plant in this study for which wage data on the previous job is available. The dependent variable is the logarithm of the wage their most recent job divided by the mean wage in the economy at the time they held that job, or $\ln\left(\dfrac{\text{most recent previous pay}}{\text{average pay in U.S. at that date}}\right)$; on their present job the life time wages of all the workers of all the workers are approximately identical.

TABLE 3A

PROBABILITY OF QUITTING WITHIN FIRST SIX MONTHS ON THE JOB MAXIMUM LIKELIHOOD ESTIMATION PROCEDURE

Independent Variable**	Estimated Coefficient or Value	Standard Errors ("t" statistics below)* Gradient Method	Hessian Method	White's Method
δ	.26	2.04	.61	.28
		(-.13)	(-.42)	(-.91)
β_0 (effect of being female on anticipated work-life)	-6.81	42.3	10.6	4.19
		(-.16)	(-.63)	(-1.62)
H. S. 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)
$g(a,\delta,\beta_0 F) \times$ education	.015	.143	.044	.029
		(.11)	(.34)	(.53)
Married	-.091	.078	.077	.077
		(-1.16)	(-1.18)	(-1.17)
$g(a,\delta,\beta_0 F) \times$ employed at application	-.098	.774	.231	.107
		(-.13)	(-.43)	(-.92)
Number of observations	2146			
Log Likelihood	-741.99			

Other independent variables included scores on each half of the dexterity test, race-location interactions, location effects, age and an intercept term. We 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 we obtain are sufficiently similar to provide some confidence that the model is not grossly misspecified. Computational costs precluded performirg the model specification test suggested by White.

* We are using the term "t" statistic loosely, to refer to the estimated coefficient divided by the standard error.

TABLE 3B

PROBABILITY OF QUITTING WITHIN FIRST SIX MONTHS*

Independent Variable	Coefficient	"t" statistic	Δ Quit Probability from a 1 unit change in the Variable Evaluated at Quit Probability of 10%	20%
Intercept	-.922	-1.54		
H.S. 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	2146			

$\hat{\gamma}$ set equal to -.25, δ set equal to .05.

.124 of observations had $Q = 1$; the mean of $h(a,\delta,\hat{\gamma}F)$ is 16.12

* Estimated using Hessian method.

TABLE 4

QUITS PER YEAR FOR MALE PRIVATE SECTOR EMPLOYEES IN PSID SAMPLE 1968-82*

("t" statistics in parentheses)

Independent Variable*	Coefficient
H.S. 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)
Non-white	-.030 (-5.05)
Age	-.0023 (-5.30)
Mean Cty Unemp. minus Mean Nat. Unemp	-.003 (-1.79)
Number of Observations	3781
R^2	.16
Mean Quits per year	.092

* Each observation was weighted by the square root of the number of years the individual was in the sample. Other independent variables were an intercept term, and 8 locational dummy variables.

TABLE 5

DAYS ABSENT AS A PERCENTAGE OF DAYS WORKED *

Tobit Estimation Procedures **
(t statistics in parentheses)

Independent Variable	Coefficient
Intercept	7.38 (2.54)
Male	.398 (-.95)
Age	-.143 (-5.89)
Education	.133 (.58)
H.S. 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	1890
Log Likelihood	-4781.2

* The data for Tables 6 and 7 come from all locations of the firm, rather than the two included in the quit equations.

** Each observation was weighted by the square root of the number of days worked. Dummy variables for the locations of the plants, the scores on the screws section of the dexterity test and race-location interactions at the two plants at which we had race data were also included as independent variable.

TABLE 6

OCCASIONS ABSENT DURING THE FIRST SIX MONTH ON THE JOB

Negative Binomial Model
(*t* statistics in parentheses) *

Independent Variable **	Coefficient
Intercept	2.70 (5.68)
Male	-.11 (-2.10)
Age	-.036 (-9.20)
Education	4.43 (1.43)
H.S. Graduate	**-.239** **(-2.59)**
College Graduate	-.028 (-.11)
Married	.0085 (.18)
Match	.011 (.35)
Job Complexity	-.0011 (-.027)
Employed at Application	-.14 (-2.72)
Number of Observations	1565
Log Likelihood	1333.5

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

** Standard errors were calculated using the Hessian method.

TABLE 7

NORMALIZED FIRST MONTH OUTPUT

Estimated by Ordinary Least Squares

("t" statistics in parentheses)

Independent Variables*	Coefficient	Coefficient	Coefficient
Intercept	67.36 (5.80)	69.05 (6.24)	71.63 (7.14)
Male	8.91 (5.81)	8.87 (5.80)	8.87 (5.80)
Age	-.11 (-1.15)	-.11 (-1.16)	-.11 (-1.16)
Education	1.36 (1.63)	1.34 (1.61)	1.33 (1.59)
H.S. Graduate	**4.44** **(.64)**	**2.54** **(.45)**	**-.21** **(-.08)**
Match	**.49** **(.48)**		**.03** **(.04)**
Job Complex x H.S. grad.	**-2.64** **(-.73)**	**-1.55** **(-.55)**	
College Graduate	-12.96 (-2.09)	-13.12 (-2.11)	-13.03 (-2.10)
Married	4.01 (2.98)	4.02 (2.98)	4.01 (2.98)
Score on Dexterity Test	.22 (1.52)	.22 (1.53)	.21 (1.53)
Job Complexity**	7.82 (2.45)	6.98 (2.60)	5.63 (5.26)
Employed at Application	1.48 (1.08)	1.45 (1.06)	1.48 (1.08)
Number of Observations	1859	1859	1959
Multiple R-Square	.382	.382	.382

* 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.

TABLE 8

LN WAGES OF MALE PRIVATE SECTOR EMPLOYEES IN PSID SAMPLE 1968-82
("t" statistics in parenthesis)

Independent Variable*	Estimated Coefficient
Education	.087 (3.98)
Education squared	-.007 (2.83)
Education cubed	.00029 (3.40)
Reported Experience	.03 (16.22)
Reported Experience squared	-.00041 (13.37)
Age	-.0054 (-4.06)
Disabled	-.083 (-6.62)
Married	.065 (6.27)
Time Trend	.013 (1.34)
Race = White	-.14 (-17.76)
H.S. Graduate	**.125** **(6.34)**
H.S. Graduate times county unemployment rate	**-.0084** **(-3.86)**
College Graduate	.0082 (.29)
Ln of C.P.I.	1.03 (7.51)
Mean Cty Unemp. minus Mean Nat. Unemp.**	.0087 (2.92)
Number of observations	9942

* Other independent variables were 8 location dummy variables and an intercept term.

** Note the estimated coefficient here is consistent with Robert Hall's observation that areas with high unemployment rates have higher than average wages.

TABLE 9

LN WAGES OF MALE PRIVATE SECTOR EMPLOYEES IN PSID SAMPLE 1968-82

("t" statistics in parenthesis)

Independent Variables*	Coefficient
Education	.083 (4.93)
Education squared	-.0070 (-3.78)
Education cubed	.0003 (5.16)
H.S. graduation	**.119** **(6.04)**
H.S. x county unemployment rate	**-.0088** **(-3.62)**
Quits per year observed	-.013 (-.49)
H.S. graduate times Number Quits per year in sample	**-.080** **(-2.60)**
Number of observations	13,604

* Other independent variables included an intercept term , the county and national unemployment rates. In C.P.I., marital status, race, union membership, disabled, part time, and a dummy variable for 1st year on the job.

TABLE 10

YEARS OF TRAINING REQUIRED FOR JOB IN 1976-1978 PSID SAMPLE,

("t" statistics in parentheses)

Independent Variable*	Coefficient
Intercept	-2.46 (-5.36)
Education	.310 (2.13)
(Education)^2	-0.24 (-1.46)
(Education)^3	.00119 (2.10)
Work Experience	.058 (4.63)
(Work Experience)^2	-.0010 (-5.05)
Age	.033 (3.80)
Race = Non-white	-.87 (-16.29)
High School Graduate	**-.02 (-.23)**
College Graduate	-.37 (-.197)
Married	.12 (1.69)
Disabled	.012 (.88)
R^2	.1638
Numbers of observations	9941

* The other independent variables were 8 dummy variables for different areas of the country.

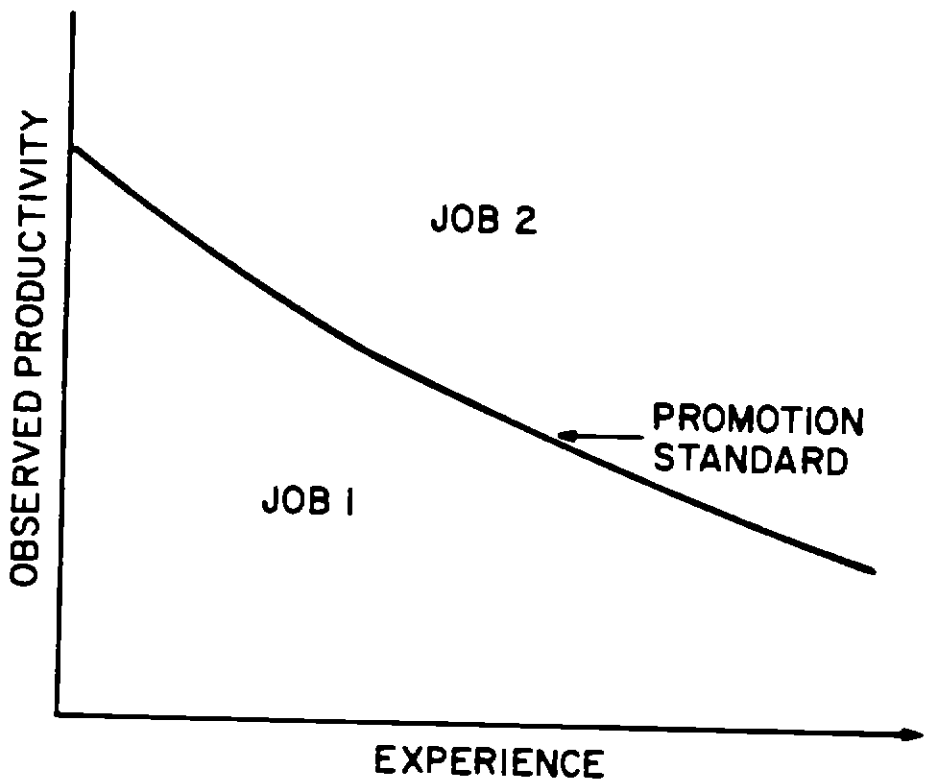
OBSERVED PRODUCTIVITY
JOB 2
PROMOTION
STANDARD
JOB I
EXPERIENCE

FIGURE 1

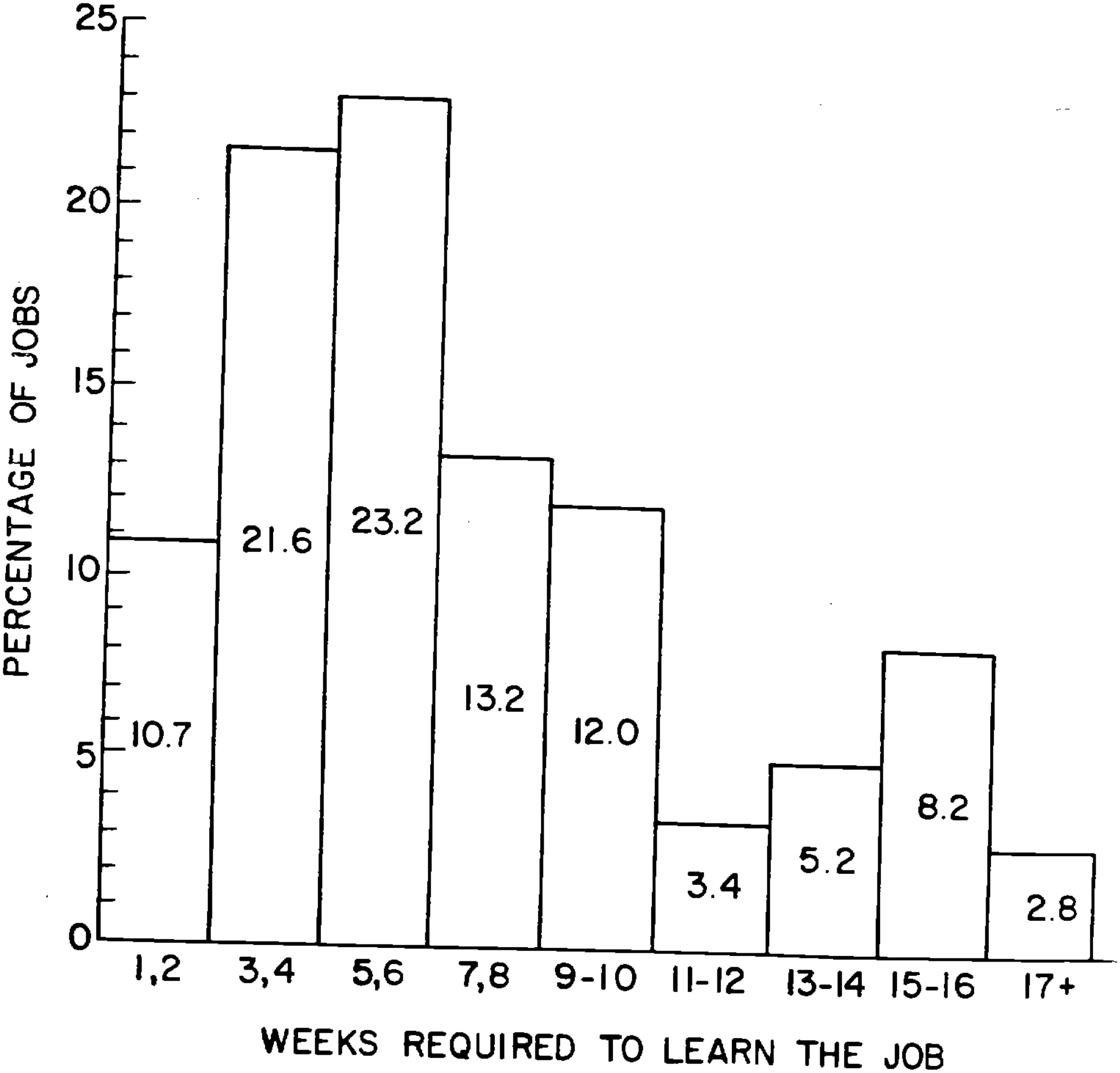
25
20
15
10
5
0
PERCENTAGE OF JOBS
10.7
21.6
23.2
13.2
12.0
3.4
5.2
8.2
2.8
1,2
3,4
5,6
7,8
9-10
11-12
13-14
15-16
17+
WEEKS REQUIRED TO LEARN THE JOB

FIGURE 2

Appendix A

In evaluating the cost of absenteeism to firms, the nature of the production process is critical. Traditional economic analyses of absenteeism assume a worker's marginal product is equal to his wage and the cost of absenteeism is the wage of the worker. In this section we consider the opposite polar case where the production process 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, we assume output is linear in the number of workers, and normalize the value of output to be equal to $\tilde{k}$. 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 $\pi(n)$. We let $P(S_n \geqslant k)$ denote the probability that at least k workers are present when n workers are hired. Finally, we assume that workers are not paid if 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).

$$\pi(n) = \tilde{k}\ P(S_n \geqslant k) - np\omega.$$

The expected marginal product of the $n+1$ worker is

$$\tilde{k}\left[P(S_{n+1} \geqslant k) - P(S_n \geqslant k)\right].$$

The net expected value of the marginal worker is

$$\pi(n+1) - \pi(n) = \bar{k}P(S_{n+1} \geqslant k) - (n+1)p\omega - \bar{k}P(S_n \geqslant k) + np\omega,$$

$$= p\bar{k}P(S_n \geqslant k-1) + (1-p)\bar{k}P(S_n \geqslant k) - \bar{k}P(S_n \geqslant k) - p\omega,$$

$$= p\bar{k}\Big[P(S_n \geqslant k-1) - P(S_n \geqslant k)\Big] - p\omega,$$

$$= p\Big[\bar{k}P(S_n = k-1) - \omega\Big]. \tag{A1}$$

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 so long as (A1) is positive; it chooses the smallest n such that

$$p\bar{k}\ \frac{n}{(n-j)j}\ p^j(1-p)^{n-j} - p\omega < 0, \text{ where } j = k-1 \tag{A2}$$

If we consider the case where workers are paid even if they are absent, the usual policy in U.S. firms for routine levels of absenteeism, (A1) is rewritten as

$$p\bar{k}\ P(S_n = k-1) - \omega$$

and (A2) as

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

Below, we present an example showing the effect of changes in (1-p), the probability of a worker being absent, on the profitability of the firm. As that example illustrates, even if firms do not pay workers when they are absent, a given change in the probability of a worker being present can have a more than proportional effect on the equilibrium wage of the worker. Consequently traits that are correlated with low absenteeism rates may be significantly rewarded in the labor market.

To illustrate the effect of absenteeism on the equilibrium value of a worker to the firm let us consider a production process which 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 workers are not paid if they are absent. In equilibrium, if the probability of a worker being absent is known by all employers, firms compete for workers so that wages are bid up to the level where each production process earns zero profits, and all the workers at a given plant have the same absenteeism rate, we find the following 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

Suggesting that even if employees are not paid when they are absent, in competitive markets employers would pay higher wages to workers with lower probabilities of being absent.

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