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ABSTRACT

This paper examines the contribution of the rise in the return to ability to the rise in the economic return to education. All of the evidence on this question comes from panel data sets in which a small collection of adjacent birth cohorts is followed over time. The structure of the data creates an identification problem that makes it impossible to identify main age and time effects and to isolate all possible age-time interactions. In addition, many education-ability cells are empty due to the stratification of ability with educational attainment. These empty cells or identification problems are "solved" in various ways in the literature and produce a variety of different estimates. We test and reject widely used linearity assumptions invoked to identify the contribution of the return to ability on the return to schooling. Using nonparametric methods, we find little evidence that the rise in the return to education is centered among the most able.

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1 Introduction

This paper examines the contribution of ability to the rise in the economic return to education. The consensus view in both the popular and professional literature is that much of the increase in the return to education can be attributed to an increase in the return to ability. Herrnstein and Murray (1994) make this a cornerstone of their analysis. They refer to the research of Blackburn and Neumark (1993) who report that the rise in the return to education is concentrated among those with both high education and high ability. In a similar vein, Murnane et al. (1995) conclude that 38-100% of the rise in the return to education for 24 year olds from 1978-1986 can be attributed as a rise in the return to ability.

The implicit assumptions in this literature are (1) that ability is valued in the market (or is a proxy for characteristics that are valued), (2) that the price of ability (or the proxied characteristics) is rising in the new market for skills, (3) that ability is correlated with education, and hence that failure to control for ability will lead to an upward bias in the estimated economic return to education.

In examining the empirical evidence on the effect of ability on the increase in the return to education, we encounter an important practical problem of interest its own right: estimating interactions in the presence of missing data. Missing data cells limit the inferences that can be made in separating the effects of ability and schooling and separating the effects of age and time.

Estimating separate effects of ability and schooling on earnings is complicated by the fact that certain ability-education combinations are rarely observed. Table 1 shows that there are very few white males in the NLSY who are low ability college graduates. Further, there are no white men in the lowest ability quartile with postgraduate education, so for that ability quartile, not even a rough estimate of the wage gain of such education is possible. For many schooling-ability pairs, the cells are empty or nearly so, making it difficult to isolate separate ability effects or schooling effects, and making interactions difficult, if not impossible to identify.

Missing data also complicate attempts to separate the effects of age and time. Estimates of the importance of ability in explaining the increasing return to schooling necessarily follow the same people, or repeated cross section samples of the same cohorts, over time. To follow the same people or cohort over time is also to follow them as they age. Figure 1 is a Lexis diagram of a single cohort of a specified initial age followed over time. Darkened cells indicate that data exist for that age and time. If panel data or repeated cross section data consist of only a single age cohort, age and time are confounded; it is impossible to identify separate age and time effects. Even with multiple age cohorts (see *e.g.* Figure 2 for the NLSY panel) there are many empty cells. The "main effects" for time or age, defined as averages over entire rows and columns cannot be computed; some of the required components are missing. It is also impossible to identify interactions associated with the empty cells without imposing parametric structure (for example, that age and time effects are linear; *i.e.* trends that fit the nonempty data cells apply to the empty ones).

The current literature copes with this problem in two distinct ways. Some authors impose linearity of time and/or age effects (Blackburn and Neumark, 1993) and arbitrarily suppress certain interactions. Although the fully nonparametric model is not identified, the hypothesis of linearity is testable. We demonstrate that the data are at odds with the widely-used assumptions that time and age effects are linear so that invoking linearity is a dangerous practice.

Other authors (e.g. Murnane, Levy and Willett, 1995) focus only on time effects measured at certain ages, typically young ones. This, too, is dangerous because it fails to account for differences in the life cycle effects of ability on wage due to investment. Our major conclusion is that the evidence in support of the claim that the rise in the return to education is a rise in the return to ability is fragile. It rests on arbitrary identifying assumptions, which when relaxed, cause the "finding" to disappear.

This paper is organized into three sections. Section 1 summarizes the literature that seeks to determine the role of ability in the recent rise in the return to education. We demonstrate that the empirical estimates reported in this literature are sensitive to *ad hoc* solutions to the problem of missing data cells.

The second section discusses the identification problem that arises from using panel data or repeated cross section data to separate time and age effects. It also defines the combinations of parameters that can be estimated from panel data. An appendix derives the precise combinations of interactions that can be identified when cells are missing.

In Section 3 we test and reject the hypothesis that age and time effects are linear. Our nonparametric estimates indicate mild support for the point of view that in the mid 80^s there was a jump in the college-high school wage differential for the most able. However, this pattern is not found for other ability and schooling groups for which nonparametric estimates can be obtained.

2 The Received Literature

Almost all of the research that tries to estimate the effect of ability on the rise in the economic return to education appeals to some version of Mincer's (1974) model of earnings.[1] The wage at time t for person of age a or work experience level x with E years of schooling and ability g is written as a regression with ℓn wages as the dependent variable and with the main effects and interactions of schooling and ability as regressors. An increase in the returns to education over time is estimated by an interaction between time and education in such a model. The goal of most papers we survey is to

[1] In the Mincer model, $\ell n\ w = \alpha_0 + \alpha_1 E + \alpha_2 x$ where E is schooling and x is experience. "α_1" is a rate of return under special conditions; see Willis (1986) or Heckman and Klenow (1997) who present a more general analysis. A hedonic model interprets the right hand side variables as characteristics, or proxies for characteristics, valued in the labor market. Thus the coefficient on ability is sometimes interpreted as the "price" of ability. For evidence against the pure characteristics model see Heckman and Scheinkman (1987).

determine if "controlling for" ability reduces the rise in the return to schooling measured by this interaction. The most influential papers are by Blackburn and Neumark (1993) and Murnane, Willett, and Levy (1995) (which we refer to as BN and MWL respectively).

2.1 A Review of the Literature

Table 2 summarizes recent studies of the role of ability in the rising return to schooling. For each study, we describe the sample, the age of the sample, the measure of wages, the regressors in the wage regression, the parameterization of time and age effects, and the measure of ability. We also describe the main findings about (a) the time trend on the wage return to education not controlling for ability, (b) the time trend on the wage return to education controlling for ability, (c) the experience trend on the return to education controlling for ability, (d) the time trend on the wage return to ability, (e) the age trend on the wage return to ability, (f) the experience trend on the wage return to ability, and (g) the education-ability interaction. Because white males have received the most attention in the empirical literature, we only survey empirical results for that group, noting that many authors fit models for women and obtain results that are *not* consistent with the results they obtain for males. (See, *e.g.* Murnane, et al. (1995) and our discussion below.)

There are several features common to all of these studies. For example, most use the NLSY and all of the samples consist of young men (the oldest in any sample is 31). There are, however, important differences across papers in the literature, especially in the specification of age and time variables. MWL are fully nonparametric in time and age variables; they estimate their model for a single age group at two different times. However, we will show that correlations of ability and wages vary with age - so that the results of MWL do not apply to workers of other ages. In contrast to MWL, most studies include workers of many ages in their samples and parameterize interactions as linear functions of age and time.

Some studies use age rather than work experience as a "control variable", ignoring Mincer's crucial distinction between the two. For example, MWL compare the college-high school wage differential at an age which implies six years of work experience for high school graduates and two years for college graduates. BN report estimates from a sample of wages observed immediately after the conclusion of schooling, when workers have almost no work experience.

All studies show an increase in the return to schooling over time but they differ in how much it is moderated by controlling for cognitive ability. MWL eliminate 38-100% of the increase. Grogger and Eide report that controlling for ability reduces, but does not eliminate, the rising return to schooling. BN, by controlling for the interaction between ability, education, and time, eliminate the increase entirely. [2]

The literature contains mixed results on the wage return to ability over

[2] Herrnstein and Murray (1994) use the BN evidence to argue that the price of ability is rising and that society is becoming more meritocratic.

time. MWL find that it nearly doubles for women and nearly triples for men between 1978 to 1986. Grogger and Eide also find statistically significant, albeit smaller, growth over the same period. The studies that include an age-ability interaction find no evidence for a rise in the return to ability.

2.2 The Literature's Results Are Not Robust

In an effort to examine the robustness of the BN specification, we attempt to reproduce their results using a different measure of cognitive ability: instead of an average of five ASVAB subtests, we use g, the first principle component of the ten ASVAB tests.[3]

Our estimates, reported in Table 3, indicate that the main result of BN cannot be reproduced. Their key finding of a statistically-significant interaction between ability, education, and time, is not statistically significant.[4]

[3]General intelligence, or g, is estimated by principal components analysis. Because age at the time of test influences test performance, we residualize each of the ASVAB subtests on age at the time of the test, separately by race and gender. The residuals were standardized to mean zero and variance one. Principal components were estimated from the standardized residuals. We calculated g by multiplying the test score vector by the eigenvector associated with the largest eigenvalue of the matrix of correlations among standardized ASVAB scores in the NLSY. For a more complete description of our measure of g and its characteristics, see Cawley et al. (1997).

[4]The results in Table 3 were produced using the first available wage after the last disenrollment from school (the same as used by BN). We also reestimated their model using the wage after first disenrollment from school; these results are consistent with those for the first wage measure and so are not printed in this paper but are available upon request.

Young people constitute virtually all of the samples used to estimate the impact of ability on earnings. Of the literature surveyed in Table 2, the oldest person in any sample is 31. This has strong implications for the estimates, because the correlation of ability with earnings and wage rates varies over the life cycle; see Jencks (1972), Griliches and Mason (1972) and Lillard (1977). In the early years, ability is associated with lower earnings and wage rates as people of high ability forego earnings to acquire skills on the job. Estimated relationships between earnings and cognitive ability, and wages and cognitive ability are weak or even perverse at the very ages at which the empirical work in Table 2 has been conducted.

MWL found that when they controlled for cognitive ability, the change from 1979 to 1986 in the education coefficient in a ℓn wage regression for 24 year olds was reduced by 38% for men and 100% for women. We examine whether the MWL findings hold for ages other than 24. Our evidence on this question is presented in Tables 4a and 4b. MWL used two data sources: The High School and Beyond, and the NLS-72. We use the NLSY and test their model with the following changes. We do not include as a regressor an indicator variable for a part-time wage, and we do not differentiate between part-time and full-time work experience. Also, our cognitive ability measure is g while MWL use a math score. Table 4a lists the change in the education coefficient over seven years when cognitive ability is excluded and then included as a regressor in the ℓn wage regression for samples of males ages 22 through 29. We find a similar pattern only for ages 23 and 25; that is, only at those ages is the change in the education coefficient statistically significant before controlling for cognitive ability and considerably lower after controlling for cognitive ability.

By performing an analysis for 24 year olds, MWL compare college graduates with roughly two years of work experience to high school graduates with roughly six years of work experience. To compare these two groups at a uniform level of experience, we re-estimated the MWL model conditioning on years of work experience (calculated from reported weeks of work experience) instead of age; results are presented in Table 4b. For those with 3, 5, 6, 8, and 10 years of work experience, the change in the education coefficient is statistically significant, and decreases when cognitive ability is entered as a regressor. However, for those with 7 or 9 years of work experience, the change in the education coefficient *rises* when ability is included as a regressor. For persons with 2 and 4 years of work experience, the change in the education coefficient is not statistically significant.

Estimated interactions are not robust across specifications. No consensus view emerges from these tables. What is the source of this empirical discord? Part of the explanation is that the literature copes in different, *ad hoc*, ways with a fundamental identification problem. We now turn to that problem.

3 Estimating Interactions From Incomplete Data

A basic identification problem plagues all of these studies. Consider Figure 1, where the problem arises in a particularly stark form. A single age cohort is followed over time. Each year the cohort ages and faces a different economic environment. It is impossible in this case to separate the effects of time and age.

Assume that the ℓn wage at age a and time t can be decomposed into main effects and interaction:

$$\ell n W(a,t) = \alpha(a) + \beta(t) + \gamma(a,t), \qquad a = 1,..,A,\ t = 1,..,T$$

where α is the age main effect, β is the time main effect and $\gamma(a,t)$ is the interaction of age and time.

The benefit of observing two age cohorts facing common year effects is that we observe the same age in two different years (except for certain ages in the first and last years), and two different ages in the same year. With access to such data, we can estimate a nonparametric additive model

$$(2.1) \qquad \ell n W(a,t) = \alpha(a) + \beta(t), \qquad a = 1,..,A,\ t = 1,..,T$$

if we suppress the interaction $\gamma(a,t)$. First, we make one normalization, *e.g.* $\alpha(1) = 0$. With this normalization, $\beta(1)$ is identified. Using this knowledge of $\beta(1)$, we can identify $\alpha(2)$ since

$$\ell n W(2,1) = \alpha(2) + \beta(1)$$

Proceeding in this fashion the main time and age effects are identified.[5]

It is also possible in this case to identify an interaction between age and time if we assume, as does much of the literature, that age and time effects are linear *i.e.* if we assume that

$$\begin{aligned} \beta(t) - \beta(t-\Delta) &= b\Delta \\ \alpha(a) - \alpha(a-\varphi) &= c\varphi \end{aligned}$$

where b and c are scalars and Δ and φ are integers. Under the assumption of linearity, it is possible to identify interaction $\gamma(a,t)$ and hence term d in $\gamma(a,t) = dat$, provided that T $\geq$ 2 and A $\geq$ 2. There are only three parameters, and they can be identified from four or more cells.

It is important to observe that identification arises from arbitrary conventions. In the NLSY, only the blackened cells in Figure 2 are available. The problem of empty off-diagonal cells substantially restricts what can be learned in two major ways. First, it prevents identification of unconditional age and time main effects. The unconditional time effect is the average time-specific effect for every age cohort, not just those observed in the data. Likewise, the unconditional age effect is the average effect for persons of a given age, across all time periods, not simply those observed in the data. Since we do not observe every age in every year (*i.e.* since we have empty off-diagonal cells), it is impossible to estimate these unconditional effects. Instead, we can estimate conditional main effects: age effects conditional on

[5] A similar identification strategy entails normalizing $\beta(1) = 0$, and subsequently identifying $\alpha(1)$ and the rest of the main effects.

the times observed, and time effects conditional on the ages observed. A formal comparison of unconditional and conditional effects is presented in Appendix B.

The second major effect of empty data cells is to limit the number of identifiable interactions. Specifically, only interactions associated with nonempty data cells can be identified. If only one age cohort is observed, (as in Figure 1), no main effects or interactions are identified; they are hopelessly confounded as the single age cohort simultaneously ages and enters a new economic environment. Given two age cohorts, all main effects are identified if interactions are assumed to be zero. For three or more age cohorts, certain combinations of interactions are identified. Individual interactions cannot be identified. The problem is more severe at the boundary ages (for the youngest and oldest workers) where certain ages are observed for the first or last time. This feature of the identification problem is unfortunate because, as noted in Section 1, considerable attention has been devoted to interactions at the youngest age groups in the NLSY.

The absence of identifiable interactions outside the blackened band displayed in Figure 2 means that any test for the absence of interactions is actually a test that linear combinations of the identified interactions are zero. The distinction is important because even if there exist nonzero interactions, it is possible that the combination of interactions that can be estimated will be zero. Any test will have zero power against such an alternative.[6] The combinations of interactions which may be tested are described in Appendix

[6]See *e.g.* Searle (1987).

B.

The literature copes with the identification problem in various ways, and it is not surprising that different strategies lead to very different empirical results. Bishop (1991) assumes linear time and age effects. BN assume linear age effects, and linear time effects in the interactions they estimate. Grogger and Eide assume linear experience effects and not age effects. No author in Table 2 fits a model with time and age effects estimated for each education-ability cell. Studies differ in the interactions that are estimated and suppressed.

We have outlined the limitations that stem from empty data cells. However, there is an additional estimation problem that is tantamount in practice, to an identification problem: data cells that are nonempty but contain little data. So far in this section we have considered the problem of missing data with respect to age and time only. In practice, this problem is compounded because estimates are often conditional on ability and education, making the problem one of missing and sparse data in a four-dimensional grid: age, time, ability, and education.

In the next section of the paper, we reexamine the wage returns to ability and education. This time, we nonparametrically estimate the conditional main time and age effects, and the identified combinations of interactions.

4 Nonparametric Estimates of Main Effects and Interactions

Figure 3 shows that there was a rise in the return to education in the mid 80^s for white males in the NLSY. Most previous research concludes that this increase is largely attributable to an increasing premium for ability over time. Figure 4 suggests that wages for upper quartile individuals increased more over time than did wages for lower quartile individuals. However, many other hypotheses are consistent with the data: a rising return to education with age, a rising return to ability with age, a rising return to education with work experience, and a rising return to ability with work experience.

This section considers two questions. The first is: is the rising return to education attributable to a rising return to cognitive ability? We investigate this question using a nonparametric approach: we estimate time effects within education-ability-age cells.

The second question is whether we need to be so agnostic about the parameterization of time and age. We test whether the assumption of linear trends in time and age is justified, so that the simple methods used in the previous literature can be vindicated. Unfortunately, they cannot.

All of our analysis in this section is for white males. Sparse data within cells prevents us from estimating our fully nonparametric models for all other groups. We cannot pool these groups because, as we have shown elsewhere (Cawley et al., 1997), the wage returns to ability and education differ significantly across race and gender.

4.1 Is the Return to Ability or Education Rising?

The first question we investigate in this section is whether the rising return to education should be attributed to a rising return to ability. We present results for the case when ability is divided into quartiles and education is broken down into three categories: high school dropout, high school graduate, and college graduate. This results in twelve education-ability cells.[7]

Figures 5 plot the time coefficients from a regression of ℓn wage on indicator variables for time and age within different education-ability cells. These estimates are produced under the assumption that there is no age-time interaction. Thus equation (2.1) is assumed to characterize wage data. Because of the correlation between ability and education, estimates could only be made for high school dropouts in the bottom two ability quartiles, high school graduates in all four quartiles, and college graduates in the top two quartiles. The plots indicate falling wages for men with less than a college education, and rising wages only for college graduates of the highest ability quartile. This confirms a finding of BN under the assumption of no age-time interactions.

Instead of the additive specification (2.1), we also control for age in a different way. We estimate time coefficients within each age cell, which permits interactions between age and time.[8] This analysis is not without cost; by

[7]We choose these divisions because they achieve a balance between differentiating ability and education groups while still retaining enough observations in each cell to generate meaningful estimates.

[8]The effects of these two methods of "controlling" for a variable are often confused in the literature, but only under the null hypothesis of no interactions between age and time

looking within smaller data cells, we can no longer say with confidence how the college premium has changed for persons below the highest quartile of ability.

From a fully nonparametric analysis, we conclude that the wage premium for college graduation (over high school graduation) rose in the mid-1980s for white males of the highest g quartile in their mid 20^s. Figures 6 present the most interesting of these estimated wage premia.[9] Insufficient data prevent us from performing a parallel analysis for the other quartiles. For the high school graduate - high school drop out wage differential, there is little evidence of a rise for the ability cohorts where usable cells are available. Among the estimable cells, the rise in the wage differentials by schooling groups is only found among some fourth quartile college graduates. In a parallel analysis that controls for work experience instead of age, we find a significant time trend in the college graduate-high school graduate wage differential again in the mid 80^s but only for workers with the least work experience.[10]

4.2 A Step Towards Parameterizing Age and Time

Our nonparametric stance is very conservative. With a little additional structure, perhaps a clearer story would emerge. The second question considered in this section is whether we need to be fully nonparametric in age and time. To answer this question we first test whether age and time effects are equal

are the two methods equivalent.

[9] A full set of results is available from the authors upon request.

[10] from the authors upon request.

across ability and education cells; we reject that hypothesis, which implies that such effects should be estimated within education-ability cells.[11]

Next, within each education-ability cell, we test whether all age-time interactions are zero.[12] We also rejected this hypothesis, which implies that in order to test for the linearity of time effects, we must condition on age, and vice-versa. We follow this strategy[13] and reject the hypothesis that time effects are linear across education-ability-age cells and that age effects are linear across education-ability-time cells. From this series of tests, we conclude that there is no empirical justification for the widespread practice of assuming that the effects of time and age are linear.

At the beginning of this section, we asked two questions. The first was: how should attribution for the wage gain be divided between education and ability? We have shown that education and cognitive ability are so strongly associated that the wage effects of the two cannot be separated for all groups. We find that the college graduate-high school graduate wage differential rose in the mid 80^sability but only for young workers, or those with the least amount of work experience. The high school graduate-high school dropout wage differentials are stagnant over time for the lowest two quartiles of ability

[11]We chose a significance level of 1% for our hypothesis tests in this section. Tables of p values associated with all hypotheses tested in this section are available upon request.

[12]Specifically, we perform a Rao test of the residuals from a main effects model on indicator variables for the identified combinations of interactions (see Appendix B).

[13]Specifically, for each age, we consider whether the age-specific time trend is linear for that ability-education-age cell. The same approach is used for testing whether the time-specific age trend is linear.

whether age or experience is used to control for life cycle wage growth.

The second question was: do we need to be nonparametric when estimating the effects of age and time? The answer is yes. We find no support for the widely-accepted practice in the empirical literature of imposing linear effects of time and age.

5 Conclusion

This paper examines the role of ability in accounting for the rise in the economic return to education in the past 15 years or so. Estimates of this effect are obtained from panel data sets that follow a small range of birth cohorts over time. The design of these data sets creates a serious identification problem that is "solved" in different ways by different authors. We demonstrate that the estimates of the effect of the rise in return to ability on generating the rise in return to education that are reported in the literature are very sensitive to small changes in identifying assumptions. In addition to the panel structure of the data used to isolate the effect of ability, there is additional stratification of persons by ability into schooling strata. These problems create empty cells which compound the usual problems of identifying interactions. Different strategies for coping with these problems have led to different estimates of the role of ability in explaining the rising return to schooling.

We present nonparametric estimates of the identified parameters in the data. We find evidence that, within age groups, the college-high school premium has increased in the mid 80^s for young persons of high ability. Because of the sorting of ability by schooling, the college-high school differential can not be identified for other quartiles. When the stratification is made on the basis of measured work experience, there is mild evidence of an increase in the college-high school wage differential for the most able people with the low levels of work experience. Few sturdy conclusions emerge about ability and its effect on the trend in the return to education for other groups.

We show that a common method of "solving" with the identification problem, assuming linear effects of age and time, is not supported by the data. We conclude that there is little evidence that the rise in the return to schooling is generated by a rise in the return to ability.

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Appendix A: Data

This paper uses the data from The National Longitudinal Survey of Youth (NLSY). The NLSY, designed to represent the entire population of American youth, consists of a randomly chosen sample of 6,111 U.S. civilian youths, a supplemental sample of 5,295 randomly chosen minority and economically disadvantaged civilian youths, and a sample of 1,280 youths on active duty in the military. All youths were between fourteen and twenty-two years of age when the first of annual interviews was conducted in 1979. The data set includes equal numbers of males and females. 16 % of respondents are Hispanic and 25% are black. For our analysis, we restricted the sample to those not currently enrolled in school and receiving an hourly wage between \$.50 and \$1000 in 1990 dollars (all results of this paper are reported in 1990 dollars). This paper uses the NLSY weights for each year to produce a nationally representative sample. However, our sample is not nationally representative in age; we only observe a nine year range of ages in any given year, and the oldest person in our 1994 sample is only 37.

In 1980, NLSY respondents were administered a battery of ten intelligence tests referred to as the Armed Services Vocational Aptitude Battery. See Cawley, *et al.* 1997 for a more complete description.

Appendix B: Identifying Interactions In Incomplete Data

This appendix presents a formal analysis of identification for the case of estimating interactions from missing cells. First, we define unconditional and conditional main time and age effects. Second, we describe the identified combinations of interactions in the presence of incomplete data with a pattern illustrated in Figure 2.

The problem of empty off-diagonal cells substantially restricts what can be learned in two major ways. First, it prevents identification of unconditional main time and age effects. Let $E(\ell n\ W(a,t)) = \mu(a,t)$. Unconditional main effects are defined as

$$\alpha(a) = \tfrac{1}{T}\textstyle\sum_{t=1}^{T}\ \mu(a,t), \qquad a = 1, ..., A$$

and

$$\beta(t) = \tfrac{1}{A}\textstyle\sum_{a=1}^{A}\ \mu(a,t), \qquad t = 1, ..., T.$$

Since we lack the data for every time and age, which is required to form these sums, we can not identify these parameters.

Without invoking further assumptions, we can only identify main time effects conditional on the ages observed. Assume that $\bar{A}$ ages are observed in each time period t. For any t, the youngest and oldest ages observed in any year are $A_\ell(t) = t$ and $A_u(t) = t + \bar{A} - 1$.

The conditional main time effect is:

$$\beta(t, A_\ell(t), A_u(t)) = \frac{1}{A_u(t) - A_\ell(t)} \sum_{a=A_\ell(t)}^{A_u(t)} \mu(a,t)$$

equivalently,

$$\beta(t, A_\ell(t), A_u(t)) = \beta(t) + \frac{1}{A_u(a) - A_\ell(a)} \sum_{a=A_\ell(t)}^{A_u(t)} (\alpha(a) + \gamma(a,t)).$$

Estimated time effects obtained by summing over available ages depend on the interactions over the interval $[A_\ell(t), A_u(t)]$.

Similarly, without making further assumptions, we can only identify the main age effect conditional on times observed. Let $T_\ell(a)$ and $T_u(a)$ represent the first and last years that age a is sampled. The conditional main age effect is:

$$\alpha(a, T_\ell(a), T_u(a)) = \frac{1}{T_u(a) - T_\ell(a)} \sum_{t=T_\ell(a)}^{T_u(a)} \mu(a,t)$$

equivalently,

$$\alpha(a, T_\ell(a), T_u(a)) = \alpha(a) + \frac{1}{T_u(a) - T_\ell(a)} \sum_{t=T_\ell(a)}^{T_u(a)} (\beta(t) + \gamma(a,t))$$

Estimated age effects obtained by summing over available times depend on the interactions over the interval $[T_\ell(a), T_u(a)]$.

$T_\ell(a)$ and $T_u(a)$ can easily be related to the other parameters. If every birth cohort in the panel is observed passing through age a (*i.e.* $\bar{A} \leq a \leq T$), then the age a is in the *interior* of the panel and $T_\ell(a) = a - (\bar{A} - 1)$ and $T_u(a) = a$.

If not every birth cohort in the panel is observed passing through age a, then age a is on the *border* of the panel. This is the case if $a \leq \bar{A}$ (*i.e.* the age a is observed in the first year of the panel) or if $a \geq T$ (*i.e.* the age a is observed in the last year of the panel). For ages on the border of the panel, $T_\ell(a) = \max\{1, a - (\bar{A} - 1)\}$ and $T_u(a) = \min\{a, T\}$.

The second major effect of empty data cells is to limit the number of identifiable interactions. In a complete table, $T(T+(\bar{A}-1))$ cells are defined but only $\bar{A}T$ are observed. For each t, only the cells $(t, a = t), ..., (t, a = t + \bar{A} - 1)$ on or near the diagonal are observed. In principle, no interaction for a (t, a) pair with width $|t - a| > \bar{A}$ can be nonparametrically identified; *i.e.* only interactions associated with nonempty data cells can be identified. If only one age cohort is observed (*i.e.* $\bar{A} = 1$, as in Figure 1), no main effects or interactions are identified; they are hopelessly confounded as the single age cohort simultaneously ages and enters a new economic environment. For $\bar{A} = 2$, all main effects are identified if all interactions are assumed to be zero. For $\bar{A} \geq 3$, certain *combinations* of the interactions are identified. Individual interactions cannot be identified.

The absence of identifiable interactions outside the blackened band displayed in Figure 2 means that any test for the absence of interactions is actually a test that linear combinations of the *identified* interactions are zero. More precisely, we can always identify the combination of interactions

$$[\gamma(a,t) - \gamma(a,t')] - [\gamma(a',t) - \gamma(a',t')]$$

for the set of all pairs $((t,a),(t',a')) \in \{(t,a),(t',a') \mid \ell \leq a, a' \leq \ell + \bar{A}$, for $\ell = t, t'; t, t' = 1, ..., T\}$. The difference within brackets removes the common age effect and the difference in differences removes the common time effect. One can then test whether the residuals for the set of all pairs $((t,a),(t',a'))$ jointly equal zero.

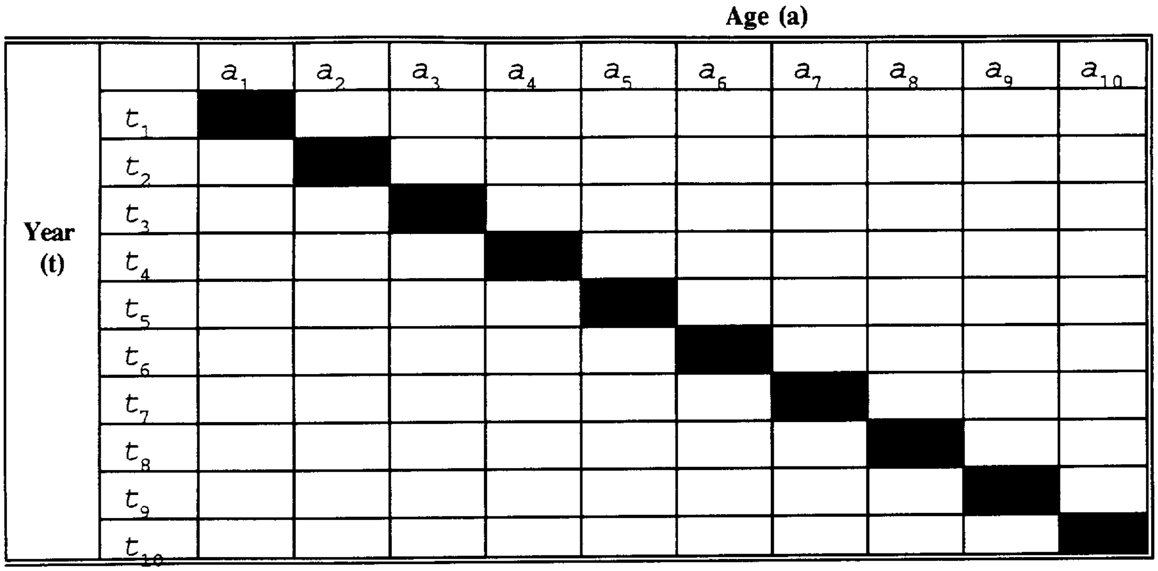


Figure 1: Lexis Diagram With a Single Age Cohort

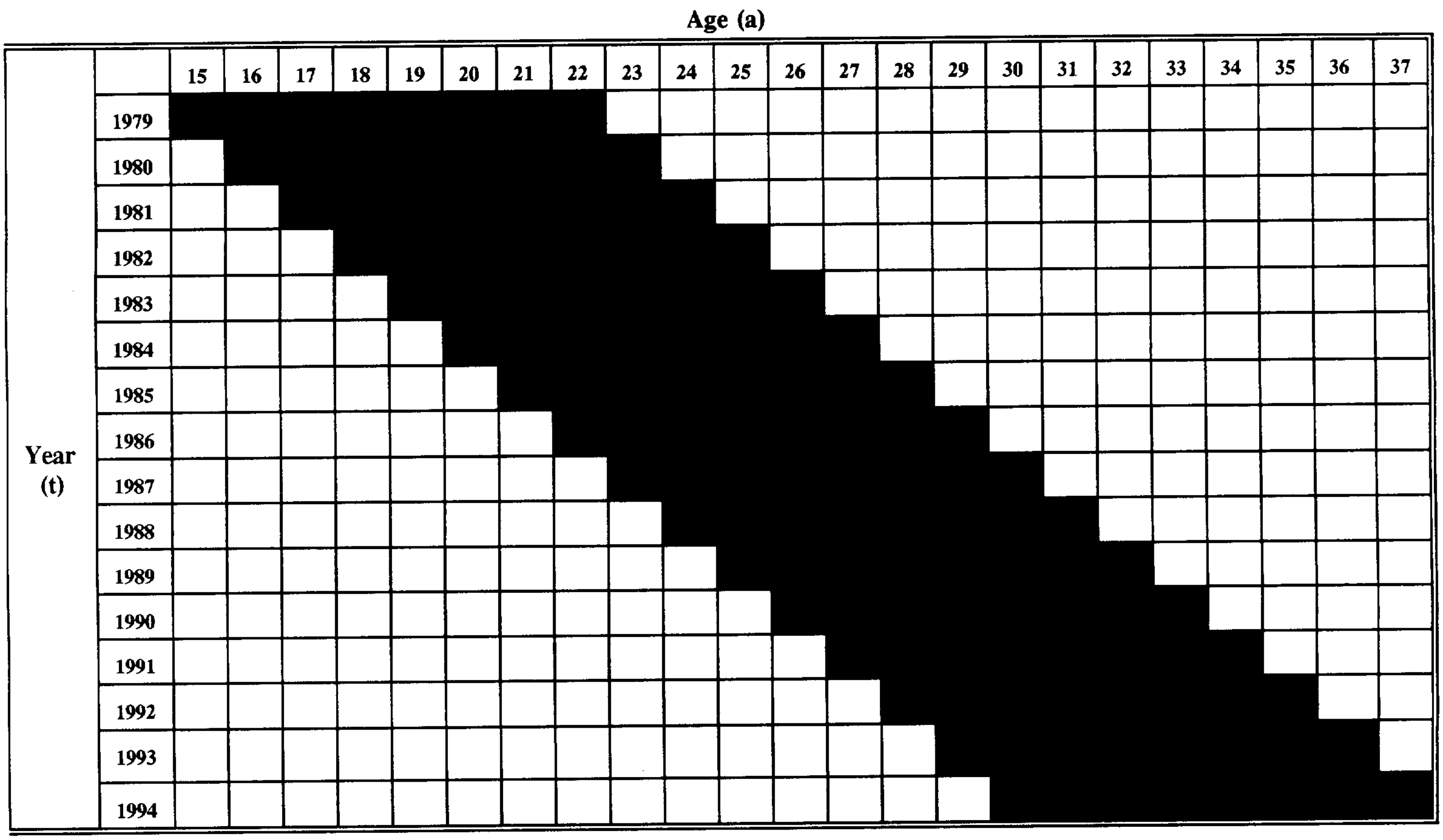
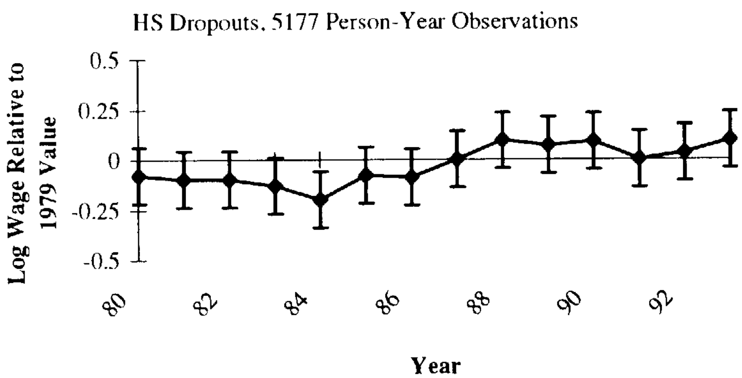
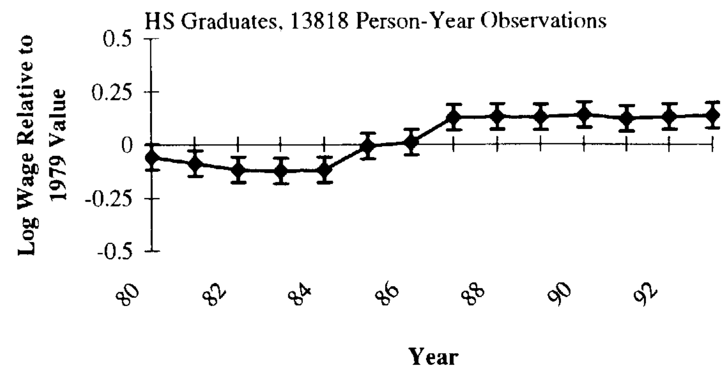
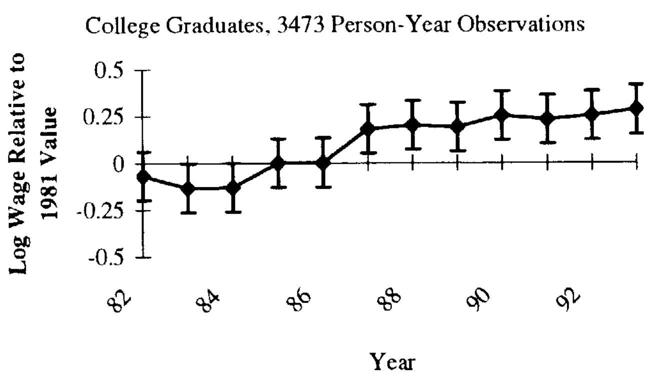


Figure 2: Lexis Diagram of NLSY Panel 1979-94

Figure 3: Return to Education over Time

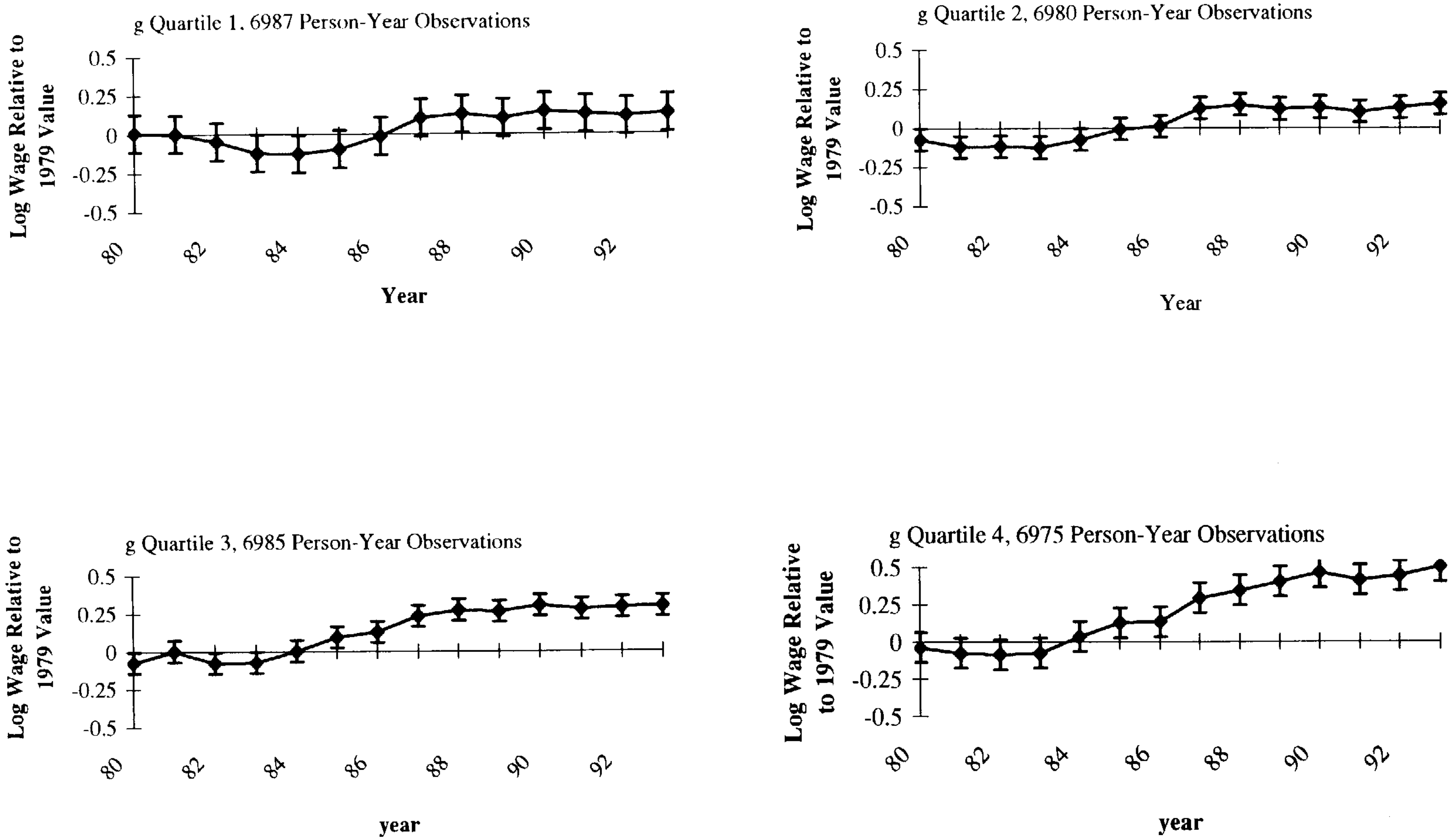






Reported standard errors are Eicher-White robust standard errors, shown as brackets. First year coefficients are standardized to 0, not plotted.

Figure 4: Return to Ability over Time

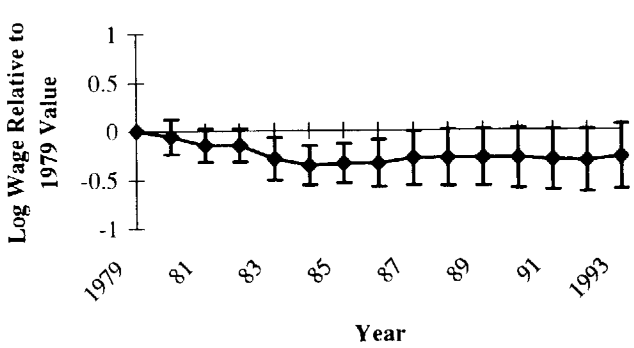


Reported standard errors are Eicher-White robust standard errors.
Standard errors shown as brackets. 1979 coefficient standardized to 0, not plotted.

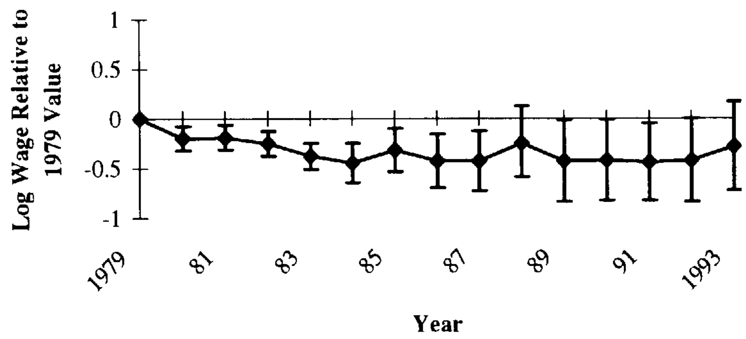
Figures 5: HS Dropouts

Based on OLS regression of log wages on age and year indicator variables

HS Dropouts, g Quartile 1, 3351 Observations



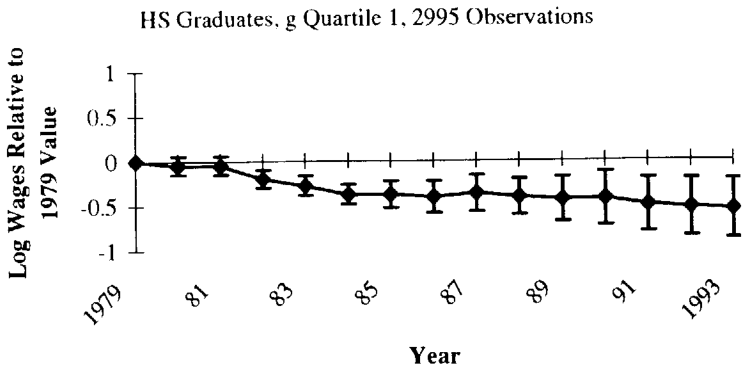
HS Dropouts, g Quartile 2, 1154 Observations

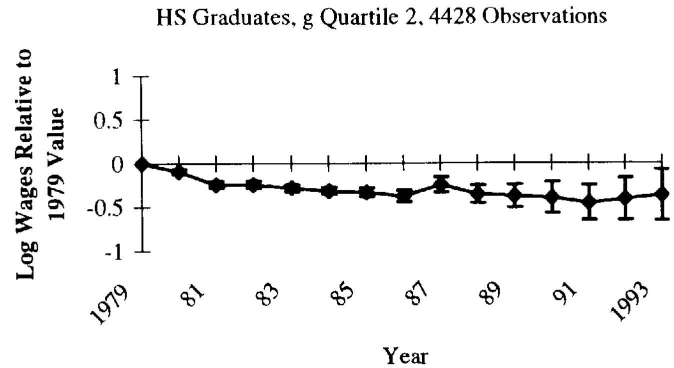


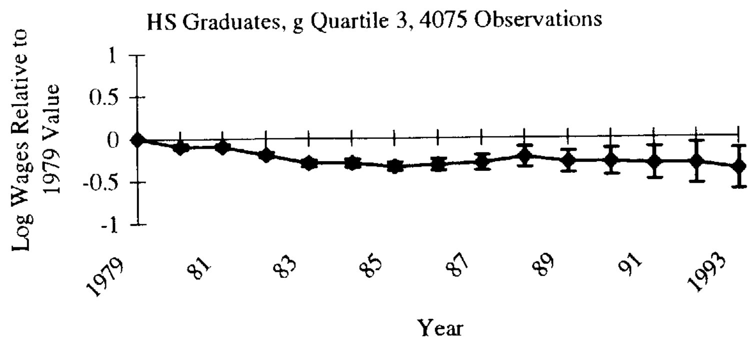
Standard errors are Eicker-White robust standard errors, reported as brackets.

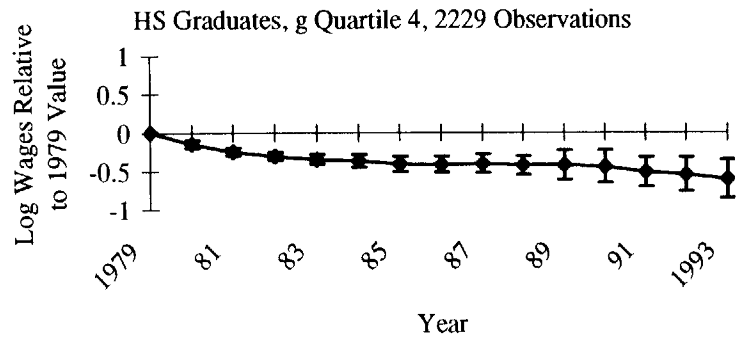
Figures 5: HS Graduates

Based on OLS regression of log wages on age and year indicator variables





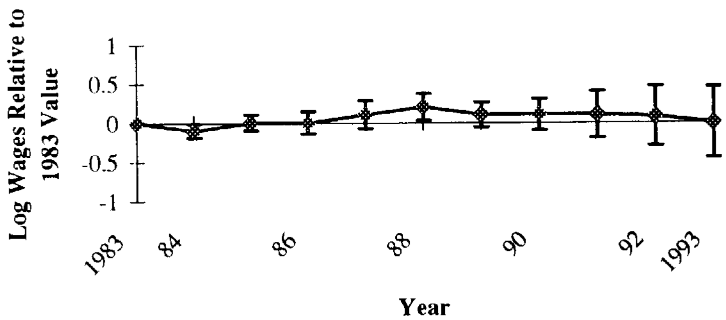




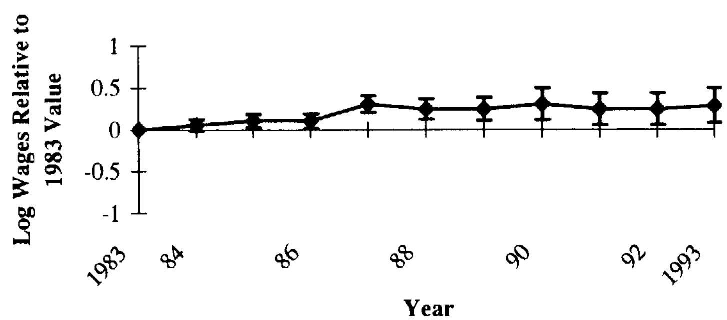
Reported standard errors are Eicher-White robust standard errors, shown as brackets.

Figures 5: College Graduates
Based on OLS regression of log wages on age and year indicator variables

College Graduates, g Quartile 3, 899 Observations



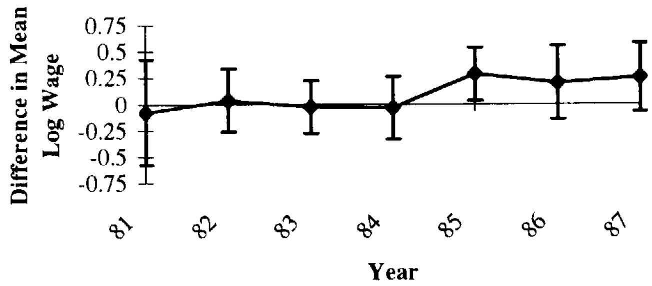
College Graduates, g Quartile 4, 1932 Observations



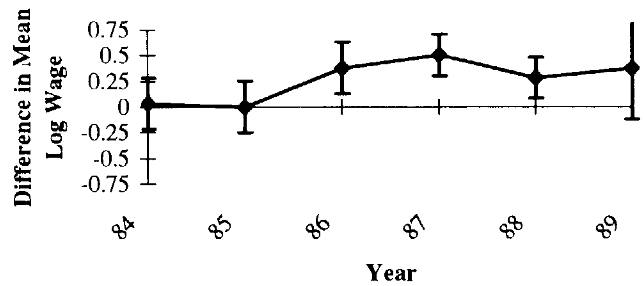
Reported standard errors are Eicher-White robust standard errors, shown as brackets.

Figures 6: Differences in Mean Log Wages Across Education Groups
g Quartile 4, Ages 24-27

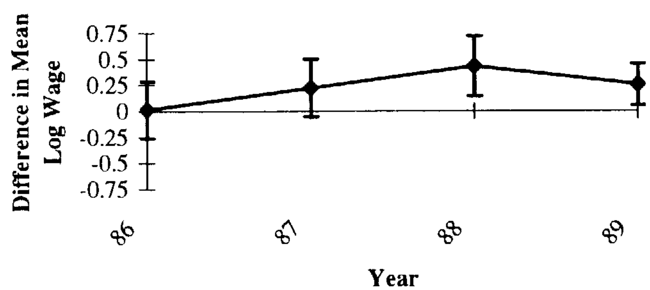
194 College Graduates, 180 HS Graduates
Age 24



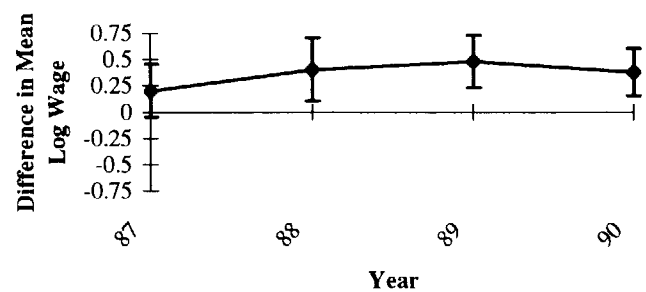
178 College Graduates, 150 HS Graduates
Age 25



128 College Graduates, 107 HS Graduates
Age 26



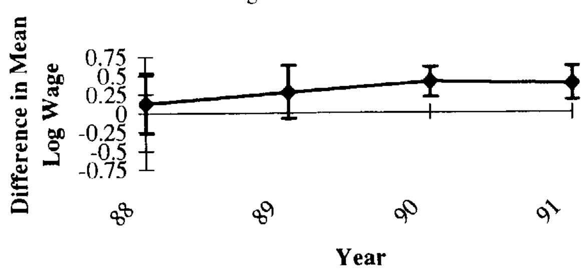
116 College Graduates, 110 HS Graduates
Age 27



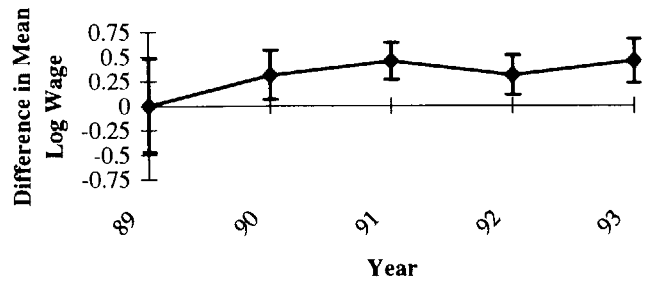
Reported standard errors are Eicher-White robust standard errors.

Figures 6: Differences in Mean Log Wages Across Education Groups
g Quartile 4, Ages 28-31

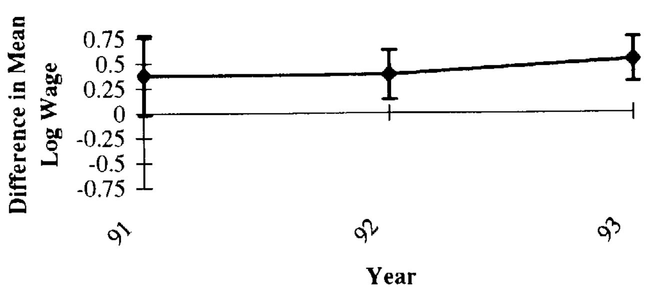
111 College Graduates, 102 HS Graduates
Age 28



130 College Graduates, 121 HS Graduates
Age 29



86 College Graduates, 78 HS Graduates
Age 31



Reported standard errors are Eicher-White robust standard errors.

TABLE 1
Percent of Highest Grade Completed by Ability Quartile
Age 30
White Males
1621 observations

Highest Grade Completed	Quartile 1	Quartile 2	Quartile 3	Quartile 4
7	2.0	0.0	0.0	0.0
8	6.7	0.5	0.0	0.0
9	10.4	1.7	0.2	0.0
10	7.9	3.2	0.0	0.0
11	9.6	2.7	1.0	0.0
12	54.0	63.2	46.9	22.5
13	3.9	7.2	11.1	4.4
14	3.0	7.9	10.1	10.6
15	0.5	1.7	3.9	4.9
16	2.2	9.6	19.7	33.6
17	0.0	1.0	1.7	5.2
18	0.0	0.5	3.0	8.4
19	0.0	0.5	1.2	5.4
20	0.0	0.2	1.0	4.9

1) Here ability is defined as general intelligence or 'g'. We compute 'g' as the ASVAB test score vector times the eigenvector associated with the largest eigenvalue in the test score covariance matrix.
2) Sample includes all respondents who were employed, out-of-school, and had valid observations each year from age 24 to age 30. Anyone receiving more schooling after age 30 was excluded

Table 2 (page 1 of 2): Comparison of the Literature on Trends in Returns to Education and Cognitive Ability		
	Bishop (1991) Table 5-8	Blackburn/Neumark (1993) Table 4
Sample	NLSY 1981-86; includes those in school.	NLSY white males, 1979-87.
Age Restriction	17 to 29	14 to 30
Dependent Variable	Log of hourly wage reported for each year.	Log of first available hourly wage after completion of schooling.
Regressors	Academic, computational, and technical ability (all drawn from ASVAB); years of schooling; years of college; actual experience; actual experience squared; potential experience; potential experience squared; interactions of date (year-1983) with the three ability measures, the two schooling measures, and actual experience; interactions of (age minus 22) with the three ability measures; interactions of actual experience with years of college and academic ability; interaction of academic ability and years of college; interactions of student status dummy with years of schooling and academic ability. Indicator variables for: school attendance, minority status, military, marital status, local unemployment rate, region, urban residence, children.	Years of education; interactions of time with education, experience, age, and union status dummy; dummy variables for urban residence, married with spouse present, and year. Academic ability and interactions of ability with education, education *trend, age, trend, and experience. Nonacademic ability and its interactions (with the same variables interacted with academic ability).
Parameterization of Time, Age, and Work Experience	Linear age, linear time, linear years of actual work experience.	Linear time in interactions. Dummy variables for each year are also included as regressors. Linear age, linear years of actual experience.
Academic Ability Measure(s)	Average of 5 normalized ASVAB tests: AR, MK, PC, WK, GS. Renormalized to have a standard deviation of 1.	Average of 5 ASVAB tests de-meaned by year of birth: AR, MK, PC, WK, GS. Standard deviation of .857.
Time Trend of Education Not Controlling for Ability	N.A.	Years of schooling trend coefficient is .0034.
Time Trend of Education Controlling for Ability.	Rising payoff to years of college (.0184) and falling payoff to years of education (-.0055) for men For women, trend on years of schooling is not statistically significant and return to years of college (.0101) rises.	Controlling for ability, there is no statistically significant change over time in payoff to years of education.
Experience Trend of Education Controlling for Ability	-.0139 for men and -.0072 for women.	N.A.
Time Trend of Ability	Not statistically significant for either men or women.	Not statistically significant.
Age Trend of Ability	Not significant but return to computational ability (Numerical Operations score) rises (.0097) for men.	Not significant if nonacademic ability (average of AS, EI, MC, NO demeaned by year of birth) included. .02 otherwise.
Experience Trend of Ability	Not significant for men or women.	-.012; however, when nonacademic ability and its interactions are included, it is not statistically significant.
Education-Ability Interaction	Years of college-ability interaction coefficient is .0124 for men and .0148 for women	Years of education-ability interaction coefficient is .015.
Time Trend of Education-Ability Interaction	N.A.	Coefficient is .0029.

Note: N.A. stands for Not Applicable; that particular interaction was not tested. All reported coefficients are statistically significant unless explicitly reported otherwise.

Table 2 (page 2 of 2): Comparison of the Literature on Trends in Returns to Education and Cognitive Ability

	Grogger/Eide (1995) Tables 4 and 5	Murnane/Willett/Levy (1995) Tables 3, 4, and 5
Sample	NLS 1972 and HS&B 1980 (pooled) full-time workers who were not full-time students with hourly wages between $1. and $100.	NLS 1972 HS&B 1980 persons out-of-school. Unweighted sample that overrepresents blacks and hispanics.
Age Restriction	23 to 25, 31	Six years after high school graduation; roughly age 24.
Dependent Variable	Log of hourly wage for NLS 1977, 1978, 1979, and 1986 and HS&B 1986.	Log of hourly wage in sixth year after HS graduation (1978 and 1986).
Regressors	Ability, work experience, and dummy variables for educational attainment (post-graduate degree, college degree, some college), race, cohort, part-time school enrollment, year, and for missing data. Education dummies are interacted with time trend and linear experience, separately.	Dummy variables for black, hispanic, attended high school in South, wage is for part-time work, and single-parent household. Years of education; mother's highest grade completed; father's highest grade completed; number of siblings; years of full-time work experience; years of part-time work experience; ability; interaction of ability and schooling.
Parameterization of Time, Age, and Work Experience	Linear time, no age variables, linear years of actual work experience.	Nonparametric time (regressions estimated separately for 1978 and 1986), age (all respondents are roughly 24 years of age), and work experience (all observations are six years after high school, which implies roughly six years of work experience for high school graduates and roughly two years of work experience for college graduates). Linear years of full-time and part-time potential work experience are entered separately as regressors.
Academic Ability Measure(s)	Scores on ETS tests of math (standard deviation=7.3 in NLS and 6.8 in HS&B), vocabulary, and mosaic, and dummy variables for high school grades.	Scaled ETS math test score. Standard deviation of 7.12 in 1972 and 7.21 in 1980
Time Trend on Education Not Controlling for Ability	For men, .0135 for those with post-graduate degrees, .0169 for college graduates, and .0087 for those with some college. For women, the trend is not statistically significant for those with post-graduate degrees, and is .0096 for college graduates and .0075 for those with some college.	For men, years of schooling coefficient is .022 in 1978 and .044 in 1986 for men. For women, the schooling coefficient is .054 in 1978 and .065 in 1986 (all results conditional on age 24).
Time Trend of Education Controlling for Ability	Statistically insignificant for men and women with post-graduate degrees, .0131 for male college graduates, .0043 for female college graduates, .0066 for men with some college, and .004 for women with some college.	Inclusion of ability eliminates the rise in the college premium for women and reduces it from 100% to 62% for men. Specifically, including ability reduces the total education trend over eight years from .011 to zero for women, and from .022 to .008 for men (conditional on roughly 24 years of age).
Experience Trend of Education Controlling for Ability	Not statistically significant for women. For men: with postgraduate degrees: .0262; with college degrees: .0192; with some college: .0068.	N.A.
Time Trend of Ability	Trend on math test score is .0006 for males and .0008 for females.	For men, the ability coefficient almost triples from .004 in 1978 to .011 in 1986. For women, the ability coefficient almost doubles, from .009 in 1978, to .017 in 1986 (conditional on roughly 24 years of age).
Age Trend of Ability	N.A.	N.A.
Experience Trend of Ability	-.0002 for both men and women.	N.A.
Education-Ability Interaction	N.A.	For men, .0034 in 1986 but statistically insignificant (with a coefficient of .0016) in 1978. The opposite pattern holds for women: .0016 in 1978 and statistically insignificant (with a coefficient of -.001) in 1986 (conditional on roughly 24 years of age)
Time Trend of Education-Ability Interaction	N.A.	The coefficients on education*ability in two separate years imply a trend coefficient of .0002 for men and -.003 for women (conditional on roughly 24 years of age).

Note: N.A. stands for Not Applicable; that particular interaction was not tested. All reported coefficients are statistically significant unless explicitly reported otherwise.

TABLE 3
Blackburn and Neumark Specification 1979-87
NLSY White Males
OLS Regression
Dependent Variable: Ln Wage
Number of Observations: 2124
R^2: .188

Variable	Coeff	Std Error	T-stat	P-val
Intercept	1.1939	.1597	7.4754	0
Years of Completed Schooling	.0644	.0136	4.7165	0
Education*Time	-.0079	.0035	-2.2456	.0247
General Intelligence (G)	-.4492	.2428	-1.8502	.0643
G*Education	.0131	.0103	1.2742	.2026
G*Education*Time	.0001	.0029	.0391	.9688
G*Time	.0095	.0365	.2607	.7943
G*Work Experience	-.0232	.0143	-1.6151	.1063
G*Age	.0166	.0121	1.368	.1713
Work Experience*Time	.0068	.0031	2.2036	.0276
Age*Time	.0026	.0023	1.1039	.2696
Urban Resident	.1511	.0277	5.4486	0
Married, Spouse Present	.138	.0363	3.8011	.0001
year==80	-.1508	.0664	-2.2702	.0232
year==81	-.1514	.1085	-1.3961	.1627
year==82	-.2499	.1605	-1.5573	.1194
year==83	-.2796	.2122	-1.3176	.1876
year==84	-.1511	.2711	-.5576	.5771
year==85	-.0933	.3374	-.2766	.7821
year==86	-.1234	.3958	-.3119	.7552
year==87	.0862	.4589	.1878	.851

1) The reported standard errors are Eicker-White robust standard errors.
2) Regression uses first wage after the completion of schooling.

TABLE 4a: Males
Murnane-Willett-Levy Specification For Different Ages And Years
NLSY Males

OLS Regression

Dependent Variable: Ln Wage

Change in Education Coefficients	Change	Std Error	T-stat	P Value
Age 22: 1979-1986	.032	.025	1.28	.201
Age 22: 1979-1986 Controlling for G	.017	.031	.548	.583
Age 23: 1980-1987	.054	.027	2	.046
Age 23: 1980-1987 Controlling for G	.023	.028	.821	.411
Age 24: 1981-1988	-.002	.023	-.087	.931
Age 24: 1981-1988 Controlling for G	-.025	.027	-.926	.354
Age 25: 1982-1989	.048	.023	2.087	.037
Age 25: 1982-1989 Controlling for G	.024	.028	.857	.391
Age 26: 1983-1990	.024	.026	.923	.356
Age 26: 1983-1990 Controlling for G	.024	.028	.857	.391
Age 27: 1984-1991	-.002	.021	-.095	.924
Age 27: 1984-1991 Controlling for G	0	.025	0	1
Age 28: 1985-1992	.025	.022	1.136	.256
Age 28: 1985-1992 Controlling for G	.017	.025	.68	.497
Age 29: 1986-1993	.012	.02	.6	.549
Age 29: 1986-1993 Controlling for G	-.005	.023	-.217	.828
Age 30: 1987-1994	-.007	.022	-.318	.75
Age 30: 1987-1994 Controlling for G	-.021	.026	-.808	.419

1) Change Represents the difference between the latter and earlier coefficient on education in a regression of ln wage on education, g, a dummy for residing in a single-parent household at age 14, number of siblings, mother and father's highest grade completed, dummy variables for residing in the South at age 14, black, and hispanic, and years (units of 50 weeks) of work experience. This regression was estimated for persons of different ages, and for persons of (n-1 to n) years of experience. This regression was estimated separately in each year. Standard errors, T-statistics, and P values apply to the change in coefficients between those two years.

2) Eicker-White robust standard errors are reported.

3) The dependent variable is log hourly wage in 1990 dollars.

4) Sample includes all valid employed out-of-school person-year observa tions.

5) NLSY sample weights are used.

TABLE 4b
Murnane-Willett-Levy Specification For Different Work Experience And Years
NLSY Males
OLS Regression
Dependent Variable: Ln Wage

Change in Education Coefficients	Change	Std Error	T-stat	P Value
2 Yrs Work Experience 1979-1986	.013	.038	.342	.732
2 Yrs Work Experience 1979-1986 Controlling for G	-.021	.037	-.568	.57
3 Yrs Work Experience 1980-1987	.095	.029	3.276	.001
3 Yrs Work Experience 1980-1987 Controlling for G	.058	.034	1.706	.088
4 Yrs Work Experience 1981-1988	.028	.024	1.167	.243
4 Yrs Work Experience 1981-1988 Controlling for G	-.003	.03	-.1	.92
5 Yrs Work Experience 1982-1989	.055	.024	2.292	.022
5 Yrs Work Experience 1982-1989 Controlling for G	.049	.029	1.69	.091
6 Yrs Work Experience 1983-1990	.075	.025	3	.003
6 Yrs Work Experience 1983-1990 Controlling for G	.066	.031	2.129	.033
7 Yrs Work Experience 1984-1991	.046	.024	1.917	.055
7 Yrs Work Experience 1984-1991 Controlling for G	.05	.03	1.667	.096
8 Yrs Work Experience 1985-1992	.078	.028	2.786	.005
8 Yrs Work Experience 1985-1992 Controlling for G	.075	.033	2.273	.023
9 Yrs Work Experience 1986-1993	.067	.03	2.233	.026
9 Yrs Work Experience 1986-1993 Controlling for G	.069	.038	1.816	.069
10 Yrs Work Experience 1987-1994	.046	.026	1.769	.077
10 Yrs Work Experience 1987-1994 Controlling for G	.013	.032	.406	.685

1) Change Represents the difference between the latter and earlier coefficient on education in a regression of ln wage on education, g, a dummy for residing in a single-parent household at age 14, number of siblings, mother and father's highest grade completed, dummy variables for residing in the South at age 14, black, and hispanic, and years (units of 50 weeks) of work experience. This regression was estimated for persons of different ages, and for persons of (n-1 to n) years of experience. Standard errors, T-statistics, and P values apply to the change in coefficients.
2) Eicker-White robust standard errors are reported.
3) The dependent variable is log hourly wage in 1990 dollars.
4) Sample includes all valid employed out-of-school person-year observations.
5) NLSY sample weights are used.