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IDENTIFYING THE ROLE OF COGNITIVE ABILITY IN EXPLAINING THE LEVEL OF AND CHANGE IN THE RETURN TO SCHOOLING

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ABSTRACT

This paper considers two problems that arise in determining the role of ability in explaining the level of and change in the rate of return to schooling. (1) Ability and schooling are so strongly dependent that it is not possible, over a wide range of variation in schooling and ability, to independently vary these two variables and estimate their separate impacts. (2) The structure of panel data makes it difficult to identify main age and time effects or to isolate crucial education-ability-time interactions needed to assess the role of ability in explaining the rise in the return to education.

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Introduction

This paper examines the contribution of ability to the rise in the economic return to education. A common view in both the popular and professional literatures 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 economic return to education is concentrated among those with high ability, a different proposition from the one stated by Herrnstein and Murray, but not necessarily inconsistent with it. In a similar vein, Murnane, Levy and Willett (1995) conclude that a substantial fraction of the rise in the return to education between 1978 and 1986 for young workers can be attributed to a rise in the return to ability. When they condition on ability, the rise in the economic return to education is diminished.

The implicit assumptions that govern much of 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, and (3) that ability is correlated with education. As a consequence of these assumptions, failure to control for ability leads to an upward bias in the estimated economic return to education, and the bias is greater in periods when the return to ability is greater. This is one possible explanation for a positive interaction of education, time and ability. Other explanations are (a) that the correlation between ability and schooling is increasing over time, due to increasing application of the meritocratic principle in educational enrollment, even if the return to ability remains constant (Herrnstein and Murray, 1994) or (b) that ability-education bundles produce skills that are more valued in the new economy, the skills are superadditive functions of ability and education (Rubinstein and Tsiddon, 1999) and the demand for the highest skills has increased disproportionately.

The small ability bias reported in Chamberlain and Griliches (1975) may be a consequence of the low economic return to ability in the time period of their samples. Ability bias will be greater in an era with greater return to ability or a more meritocratic relationship between schooling and ability. Herrnstein and Murray (1994) argue that both of these factors are at work in the modern economy.

Ability bias is usually discussed as a problem of omitted variables (see, *e.g.* Griliches, 1977 or Chamberlain and Griliches 1975). Include the missing ability variable and, except for problems of measurement error, there will be no bias. The conventional formulation of the ability bias problem ignores the strong dependence between education and ability which Herrnstein and Murray (1994) argue has become stronger in recent years. If the dependence between ability and education becomes too strong, it is impossible to isolate the effect of education from ability even when the latter is perfectly observed. This gives rise to the logically prior problem of *sorting bias*, discussed in this paper.

Table 1 shows that there are very few white male college graduates with low ability in the NLSY. Further, there are no white men with postgraduate education in the lowest ability quartile, so for that ability quartile, no 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 and schooling effects, and making main effects of ability and education difficult, if not impossible, to identify. In the limit, if ability and education are perfectly stratified, there is no way to isolate returns to education from returns to ability, even if ability is perfectly measured. Empirically, the two are indistinguishable.[1]

Missing data also complicate attempts to separate the effects of age and time. Estimates of the role of ability in explaining the increasing return to schooling that are reported in the recent literature 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. The econometric problem created by such samples is more severe than the usual age-period-cohort effect problem.[2] Figure 1 is a Lexis diagram for a single cohort of a specified initial age followed over time. Darkened cells indicate the data that exist for each age and time period. If panel data or repeated cross section data consist of only a single age cohort, age and time are hopelessly confounded. It is impossible to identify separate age and time effects. Even with multiple age cohorts (see, *e.g.*, Figure 2 for the data structure of 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. (In the age-period-cohort problem these averages can be identified if cohort effects are suppressed.) Some of the components required to form these means are missing. It is also impossible to identify interactions associated with the empty cells without imposing parametric structure (*e.g.*, that age and time effects are linear so that trends fit on nonempty data cells apply to the empty ones).

The current literature on ability bias ignores the first problem (strong dependence between education and ability) and implicitly solves the second problem in two distinct ways. Some authors impose linearity of time and/or age effects (*e.g.* Blackburn and Neumark, 1993; Bishop, 1991; Grogger and Eide, 1995) and arbitrarily suppress certain interactions.[3] Although a fully nonparametric model is not identified, the hypothesis of linearity is testable. We demonstrate that the NLSY data are at odds with the widely-used assumptions that time and age effects are linear. Invoking linearity solves the identification problem but imposes unjustified restrictions across time periods and ages. Murnane, Levy and Willet (1995) solve the second problem in a different way by estimating the contribution of ability to eliminating the rise in the return to education measured at one age in two different years. This procedure leaves open the question of whether their results are special to the age they choose.

This paper is organized into two sections. The first section discusses the identification problems that arise from using panel data or repeated cross section data to separate time and age effects that arise from the strong stratification of ability and education. There we present 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 2 we test and reject the widely-used specification that age and time effects are linear. Estimates from nonparametric procedures indicate mild support for the point of view that in the mid-80s there was an increase in the college-high school wage differential for the most able. This pattern is not found for other ability and schooling groups for which nonparametric estimates can be obtained. This produces a more nuanced interpretation of the ability - schooling - time interaction than that reported in the recent literature.

1 Estimating Interactions and Main Effects From Incomplete Data

Assume that the log 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 $\alpha(a)$ is the age main effect, $\beta(t)$ is the time main effect and $\gamma(a,t)$ is the interaction of age and time. To simplify the exposition, we implicitly condition on education and ability.

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

$$(1.1) \qquad \ell n\ w(a,t) = \alpha(a) + \beta(t), \qquad a = 1,..,A,\ t = 1,..,T$$

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.[4]

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 is achieved by imposing 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 the times observed, and time effects conditional on the ages observed. This problem is distinct from the linear dependence that arises in the standard age-period-cohort effect problem. That problem arises even when all the cells of Figures 1 and 2 are available. The problem discussed in this paper arises even if there are no cohort effects. 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, interactions associated with empty data cells obviously cannot 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 Cawley, Heckman, Lochner and Vytlacil (1999), considerable attention has been devoted to interactions for the youngest age groups in the NLSY.

The absence of identifiable interactions outside the black band displayed in Figure 2 means that any test for the absence of interactions is actually tests whether or not linear combinations of the identified interactions are zero. The distinction is important because even if there are 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.[5] The combinations of interactions that can be identified and tested are characterized in Appendix B.

The literature copes with the identification problem in various ways. Different strategies lead to very different empirical results. Bishop (1991) assumes linear time and age effects. Blackburn and Neumark (1993) assume linear age effects and linear time effects in the interactions they estimate. Grogger and Eide (1995) assume linear experience effects and but no age effects. None of the studies summarized in Cawley, Heckman, Lochner and Vytlacil (1999) fits a model with time and age effects estimated for each education-ability cell. Studies differ in which interactions 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. The problem of missing data on age and time 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 addition, some ability-education cells are missing and others are sparse (see Table 1). This means that it is impossible to identify all education-ability interactions. Main effects for education are formed only over a subset of the ability cells. In the limit, with perfect stratification of education with ability, the main effects are interaction effects. The inability to identify main effects attributable to either ability or education is the problem of *sorting bias.*

The next section of the paper reexamines the wage returns to ability and education. We nonparametrically estimate conditional time and age main effects and the identified combinations of interactions. In order to conduct a nonparametric analysis, we necessarily must limit the number of variables we include in the model. This means that our models contain fewer regressors than previous models that investigate the returns to ability over time and the education-ability-time interaction.

2 Nonparametric Estimates of Main Effects and Interactions

To address these identification problems, we use extracts from the NLSY data documented in Appendix A. The NLSY is a panel data set with unusually rich information on measures of cognitive ability. Table 2 presents the components of the ASVAB test reported in the NLSY.

For our measure of ability, we use general intelligence, or g, which we take as the first principal component of the ASVAB test scores.[6] There has been considerable debate about what represents the best measure of cognitive ability. General intelligence, which reflects the ability to perform well on the tests used to estimate it, is commonly used in psychometrics, though it is often supplemented with more specific ability measures (see, *e.g.* the review in Carroll 1997). In Cawley, Conneely, Heckman and Vytlacil (1997) we show that there is little difference between general intelligence, (Armed Forces Qualifying Test), or averages of the ASVAB test of the sort used by Blackburn and Neumark (1993), in terms of explanatory power in log wage regressions. In parallel analyses of the sort we conduct in this paper, using the measure employed by Blackburn and Neumark, and for each of the ASVAB test scores separately, we find qualitatively similar results for each measure with the exception of Paragraph Completion.[7]

We have already presented our evidence on sorting bias and it is summarized in Table 1. Figure 3 shows that there was a rise in the return to college education in the mid-80s for white males in the NLSY. However, as Murnane, Levy and Willet (1995) claim, this may largely be a consequence of a rise in the return to cognitive ability over time. Figure 4 suggests that the wage gap between individuals in the upper and lower quartile of ability rose over this period.

Many hypotheses are consistent with the data, including: 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.

We address two questions. (1) Is the rising return to education concentrated among the most able? We investigate this question using a nonparametric approach. We estimate time effects within education-ability-age cells. (2) The second question addressed in this paper 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. Relaxing linearity substantially qualifies the interpretation of interactions previously reported in the literature.

All of our analysis in this section is for white males. Sparse data within cells prevent us from estimating our nonparametric models for all other groups. We cannot pool these groups because, as we have shown elsewhere (Cawley, Conneely, Heckman and Vytlacil, 1997), the wage returns to ability and education differ significantly across race and gender. A cost of adopting a nonparametric approach is that we are forced to adopt a simpler model, with fewer regressors, than has typically been estimated in this literature. We use nonparametric methods to clarify the two stated questions. With a data set the size of the NLSY, we cannot be fully nonparametric in using the full array of variables presented in other studies in this literature.

2.1 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 empirical 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. We define these education levels by highest grade completed less than 12, equal to 12, and equal to 16, respectively. This results in twelve education-ability cells.[8]

Figure 5 plots the time trends from a specification that does not allow for age-time interactions. Within each education level, we run a spline regression of log wage on ability with knots at the 25th, 50th, and 75th percentiles of ability, where the coefficients of the spline regression are allowed to depend on time and age in an additively separable manner. In particular, letting a denote age, t denote time, e denote education level, c denote cognitive ability, and q_c denote quartile of cognitive ability, the specification is:

$$\ell n\ w = \Big(\alpha(a, e, q_c) + \beta(t, e, q_c)\Big) + \Big(\gamma(a, e, q_c) + \delta(t, e, q_c)\Big)c + \epsilon \tag{2.1}$$

where ϵ is mean independent of the a, t, e and c, and where the regression equation is constrained to be continuous in the cognitive ability score, c, and linear in c within the ability quartiles.[9] No functional form assumption is imposed on the coefficients besides those required to constrain the equation to be continuous in c for each age, time and education level. The coefficients may vary with age, time, education, or ability quartile. The plotted point estimates are fitted values with the ability level evaluated at the midpoint of each ability quartile.[10] The plotted confidence bands are plus and minus two standard errors, with the standard errors estimated by a robust Eicher-White procedure allowing for correlation in log wages across time for a given individual. Because of the strong association between ability and education, estimates could only be obtained 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 for college graduates in the two highest ability quartiles.[11]

In addition to 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.[12] In particular, we estimate the following spline regression:

$$\ell n\ w = \alpha(a, t, e, q_c) + \gamma(a, t, e, q_c)c + \epsilon \tag{2.2}$$

where the regression equation is again constrained to be continuous in the cognitive ability score, c, within quartiles and no functional form assumption is imposed on how the coefficients vary with age, time or education.[13] This analysis is not without cost; by looking within smaller data cells, we obtain noisy estimates.

From this 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-20s. Figure 6 presents the most interesting of these estimated wage premia.[14] Similar analysis, for the third quartile, is reported in Figure 7. We find no increase in the wage premium for college graduation for those in the third quartile of ability, a result essentially in agreement with the interaction of education, ability and time reported in Blackburn and Neumark (1993). Their finding of an interaction among ability, education and time is supported but it is isolated in the highest g quartile group. The effect of ability on the education-time interaction is not continuous. At lower ability or education levels, increases in ability do not increase the education-time trend.

Figure 7 should be treated very cautiously due to small sample sizes. There are more than twenty observations in each reported age-time cell for fourth quartile college graduates and high school graduates, but there are less than twenty observations in many reported age-time cells for third quartile college graduates. Insufficient data prevent us from performing a parallel analysis for the bottom two ability quartiles. For the high school graduate - high school drop out wage differential, there is little evidence of a rise in the return to education for the ability cohorts where usable cells are available.[15] Among the estimable cells, the rise in the wage differentials among schooling groups is only found among younger 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 80s but only for workers with the least work experience.[16]

2.2 Parameterizing Age and Time Effects

The nonparametric stance we take in this paper is very conservative. With a little additional structure, a clearer story might 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 perform a series of tests.[17]

We first test whether time effects are equal across education cells, in particular, in the notation of equation (2.2) whether[18]

$$\alpha(a, t+1, e, q_c) - \alpha(a, t, e, q_c) = \alpha(a, t+1, e', q_c) - \alpha(a, t, e', q_c)$$

$$\gamma(a, t+1, e, q_c) - \gamma(a, t, e, q_c) = \gamma(a, t+1, e', q_c) - \gamma(a, t, e', q_c)$$

for all available (a, t, q_c, e), (a, t, q_c, e') cells with $e \neq e'$. We also test whether time effects are equal across ability quartiles, and whether age effects are equal across education and ability cells. We reject each of these four hypotheses, which implies that age and time effects should be estimated within education-ability cells.[19]

Next, within each education-ability cell, we test whether all identified age-time interactions are zero. In particular, we conduct a score test with the unrestricted model given by equation (2.2) and the restricted model given by equation (2.1). We reject the hypothesis of zero age-time interactions. Combining the inferences from these tests, we conclude that in order to test for the linearity of time effects we must condition on age, and to test for linearity of age effects we must condition on time. We follow this strategy. Specifically, for each age, we consider whether the age-specific time trend is linear for each ability-education-age cell with data. The same approach is used for testing whether the time-specific age trend is linear.[20] We 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 entire 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.[21]

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. This is a consequence of the problem of sorting bias previously discussed. We find that the college graduate-high school graduate wage differential rose in the mid-80s for those in the highest quartile of ability but only for young workers, (those with the least amount of work experience). High school graduate-high school dropout wage differentials are stagnant over time for the lowest two quartiles of ability whether age or experience is used to control for life cycle wage growth.

The second question asked 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 solving the identification problems posed in Section 1 by imposing linear effects of time and age. When this assumption is relaxed, we find that an education-ability-time interaction only holds for high ability college graduates.

3 Conclusions

This paper examines the role of ability in accounting for the recent rise in the economic return to education. Estimates of this effect are often 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 different authors cope with in different ways.[22]

In addition to the identification problems raised by the panel structure of the data used to isolate the effect of ability, there is additional stratification of persons by ability into schooling strata. This gives rise to the problem of *sorting bias* which is logically prior to the problem of ability bias that has occupied the attention of empirical labor economists. If ability and education are perfectly stratified, separate effects of ability or schooling on earnings cannot be identified. With the levels of stratification in Table 1, separate ability and education effects are estimable only by imposing arbitrary parametric assumptions like linearity in age and education in an earnings equation. In the literature, the ability bias problem is usually formulated as a problem of omitted variables. The evidence reported in this paper suggests that the real problem is that ability and schooling appear to be inseparable — all interaction and no main effects — even if ability is perfectly observed. Sorting bias creates empty cells which compound the usual problems of identifying interactions. Different strategies for coping with these problems have led to different interpretations of the role of ability in explaining the rising return to schooling. It would be fruitful to conduct additional investigations of sorting bias for data from earlier periods. Herrnstein and Murray (1994) claim that strong sorting of ability and education is a recent phenomenon.

We show that a common method of "solving" the identification problem, assuming linear effects of age and time, is not supported by the NLSY data. 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 1980s for young persons of the fourth quartile of ability but not for young persons in the third quartile of ability. Because of the strong sorting of ability by schooling, the college-high school differential cannot be identified for other quartiles and the estimated pattern is very fragile for the third quartile of ability. 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 men with 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.

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Notes

[1]The evidence reported in Table 1 may be called into question because education may increase ability. However, the level of sorting (as reported in Table 1) is only slightly weaker if we consider only 14-16 year olds in 1979 in the NLSY whose ability is measured before they complete their schooling. This table is available on request from the authors.

[2]See the essays in Mason and Fienberg (1983) for discussions of the classical age-period-cohort effect problem.

[3]Cawley, Heckman, Lochner and Vytlacil (1999) summarize the literature and demonstrate the sensitivity of estimates of ability-education-time effects to exclusion and inclusion of other variables, and suppression of certain interactions.

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

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

[6]Because age at the time of test influences test performance, we standardize each of the ASVAB subtests to mean zero and variance one by age. We calculate g as the first principal component of the standardized test scores. For a more complete description of our measure of g and its characteristics, see Cawley, Conneely, Heckman and Vytlacil (1997).

[7]When using Paragraph Completion as the measure of ability, we found

the time trends in the return to education to be qualitatively the same in the third and fourth quartiles and could not reject the hypothesis that the time trends were the same.

[8]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.

[9]Experimentation with higher order splines produced similar though noisier empirical results.

[10]Similar results are obtained using medians within quartiles.

[11]Our estimate of rising wages college educated individuals in the third quartile of ability is fragile to the specification used. The rising wage in the third quartile for college graduates is not found with the alternative specification which conditions on ability quartile instead of using the linear spline specification. These results are available from the authors on request.

[12]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 are the two methods equivalent.

[13]Use of higher order splines within ability quartiles does not affect the estimates.

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

[15]Parallel analyses comparing the some college - high school graduate wage differential shows no rise in the wage differential for the ability cohorts where usable cells are available.

[16]These graphs are available from the authors upon request.

[17]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. We use a robust Eicher-White procedure for all tests.

[18]For the test of equality of age and time trends across education and ability cells, we estimate equation (2.2) unrestricted and run a Wald test of the given linear restrictions on the model.

[19]Details of these tests are available on request from the authors.

[20]For the linearity tests, we estimate equation (2.2) unrestricted and run a Wald test of the appropriate linear restrictions on the model.

[21]Details of these tests are available from the authors on request.

[22]Cawley, Heckman, Lochner and Vytlacil (1999) demonstrate that small changes in conventional specifications (adding and suppressing interactions based on linear measures of age and time) produce very different estimates of age - period - education effects on wages.

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). Parallel analysis using \$1 and \$100 as the cut-off points resulted in similar results. 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 (ASVAB). Table 2 lists the ten tests. See Cawley, Conneely, Heckman and Vytlacil (1997) for a more complete description.

Appendix B: Identifying Interactions In Incomplete Data

This appendix presents a formal analysis of identification of interactions when there are 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. Assume A age groups and T time periods.

The problem of empty off-diagonal cells 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

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

and

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

Since we lack the data for every time and age, which are required to form these sums, we cannot 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, *i.e.* there are $\bar{A}$ cohorts in the panel. For any t, the youngest and oldest ages observed in any year are $A_f(t) = t$ and $A_\ell(t) = t + \bar{A} - 1$.

The conditional main time effect is:

$$\beta(t, A_f(t), A_\ell(t)) = \tfrac{1}{\bar{A}} \sum_{a=A_f(t)}^{A_\ell(t)} \mu(a,t)$$

equivalently,

$$\beta(t, A_f(t), A_\ell(t)) = \beta(t) + \tfrac{1}{\bar{A}} \sum_{a=A_f(t)}^{A_\ell(t)} (\alpha(a) + \gamma(a,t)).$$

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

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

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

equivalently,

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

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

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

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 $a < \bar{A}$ or if

$a > T$. For ages on the border of the panel, $T_f(a) = \max\{1, a - (\bar{A} - 1)\}$ and $T_\ell(a) = \min\{a, T\}$.

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 the panel structure. 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 without assuming zero interactions. 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 additive age effect and the difference in differences removes the common additive time effect. One can then test whether the residuals for the set of all pairs $((t,a),(t',a'))$ jointly equal zero.

Table 1 -- Percent of Highest Grade Completed by Ability Quartile
Age 30, White Males
Number of Observations: 1621

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	1937	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

Notes:
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: The Armed Services Vocational Aptitude Battery

Subtest	Minutes	Description (A subtest of ASVAB measuring...)
General Science	11	Knowledge measuring the physical and biological sciences.
Arithmetic Reasoning	36	Ability to solve arithmetic word problems.
Word Knowledge	11	Ability to select the correct meaning of words presented in context and to identify the best synonym for a given word.
Paragraph Comprehension	13	Ability to obtain information from written passages.
Numerical Operations	3	Ability to perform arithmetic computations (speeded).
Coding Speed	7	Ability to use a key in assigning code numbers to words (speeded).
Auto and Shop Information	11	Knowledge of automobiles, tools, and shop terminology and practices.
Mathematics Knowledge	24	Knowledge of high school mathematics principles.
Mechanical Comprehension	19	Knowledge of mechanical and physical principles and ability to visualize how illustrated objects work.
Electronics Information	9	Knowledge of electricity and electronics.
ASVAB Testing Time	144	

Figure 1: Lexis Diagram With a Single Age Cohort

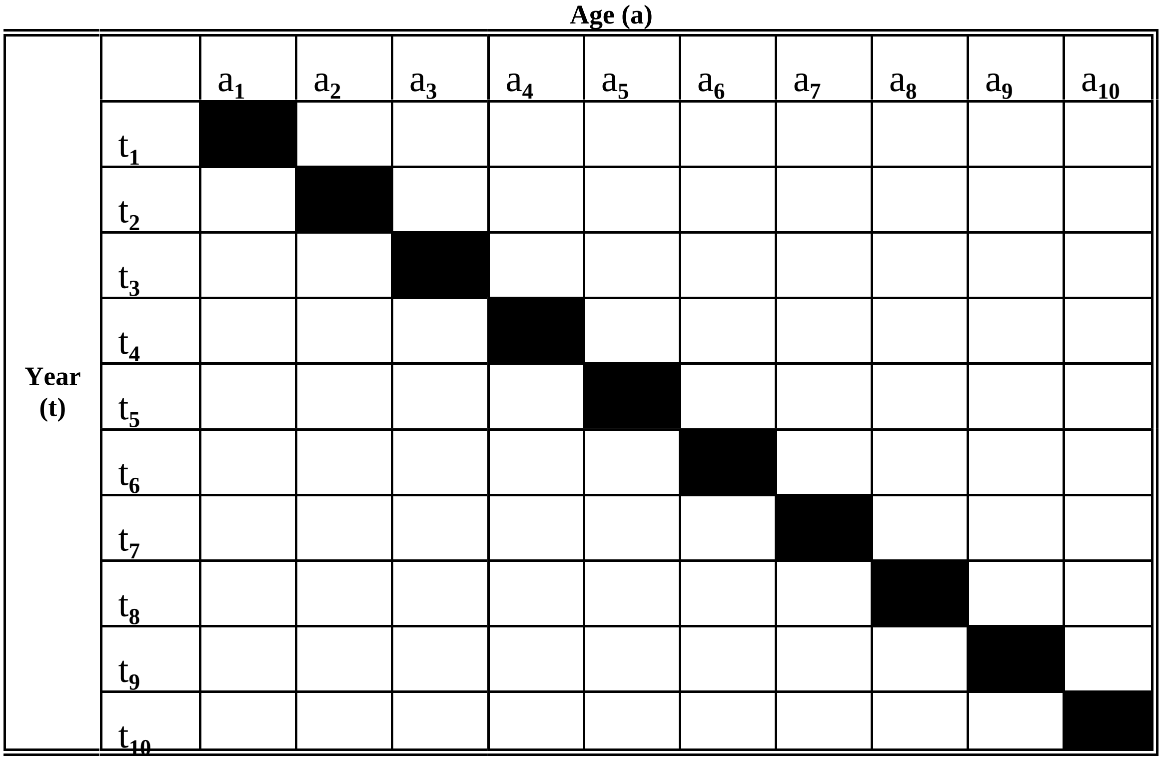


Figure 2: Lexis Diagram of NLSY Panel 1979-94

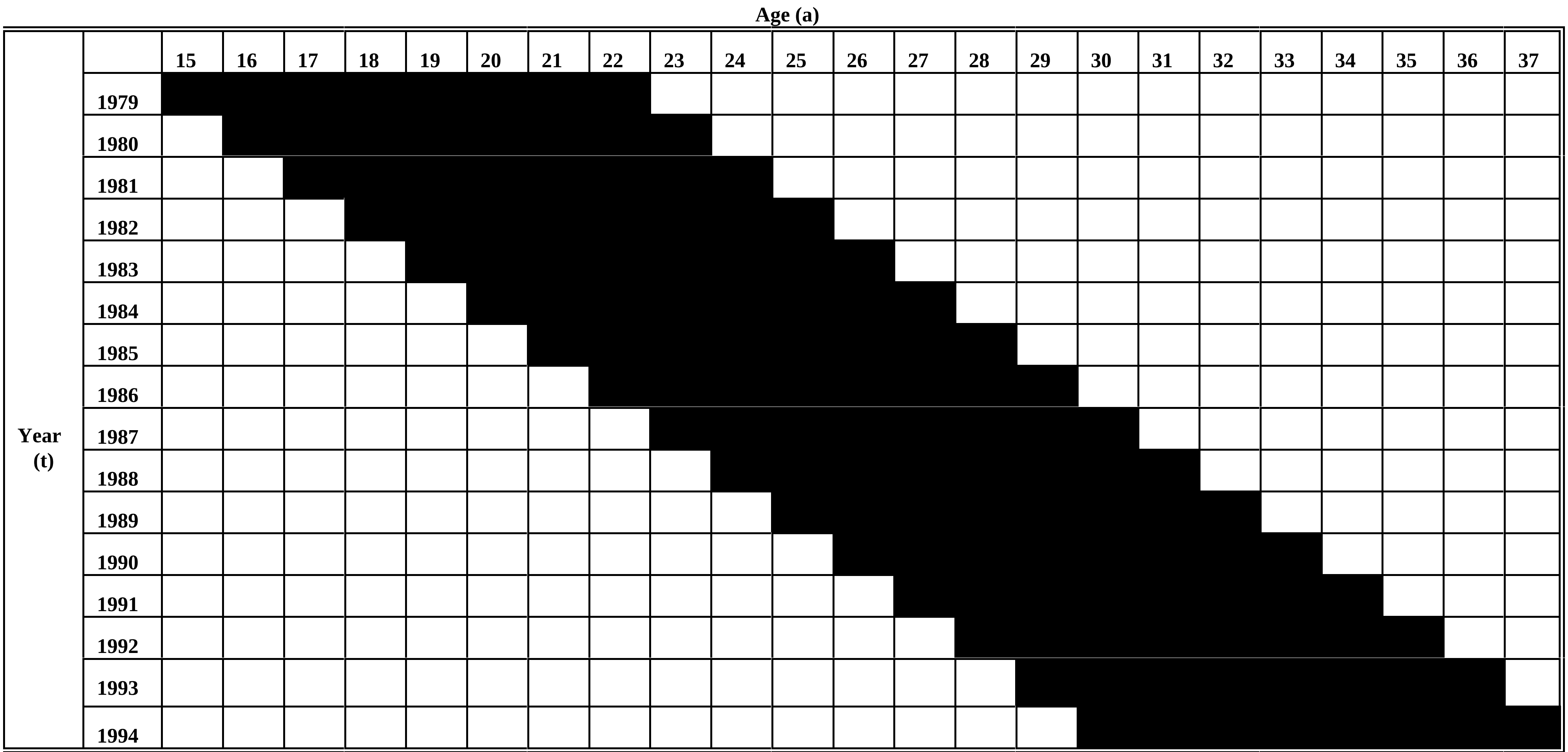


Figure 3: Return to Education over Time

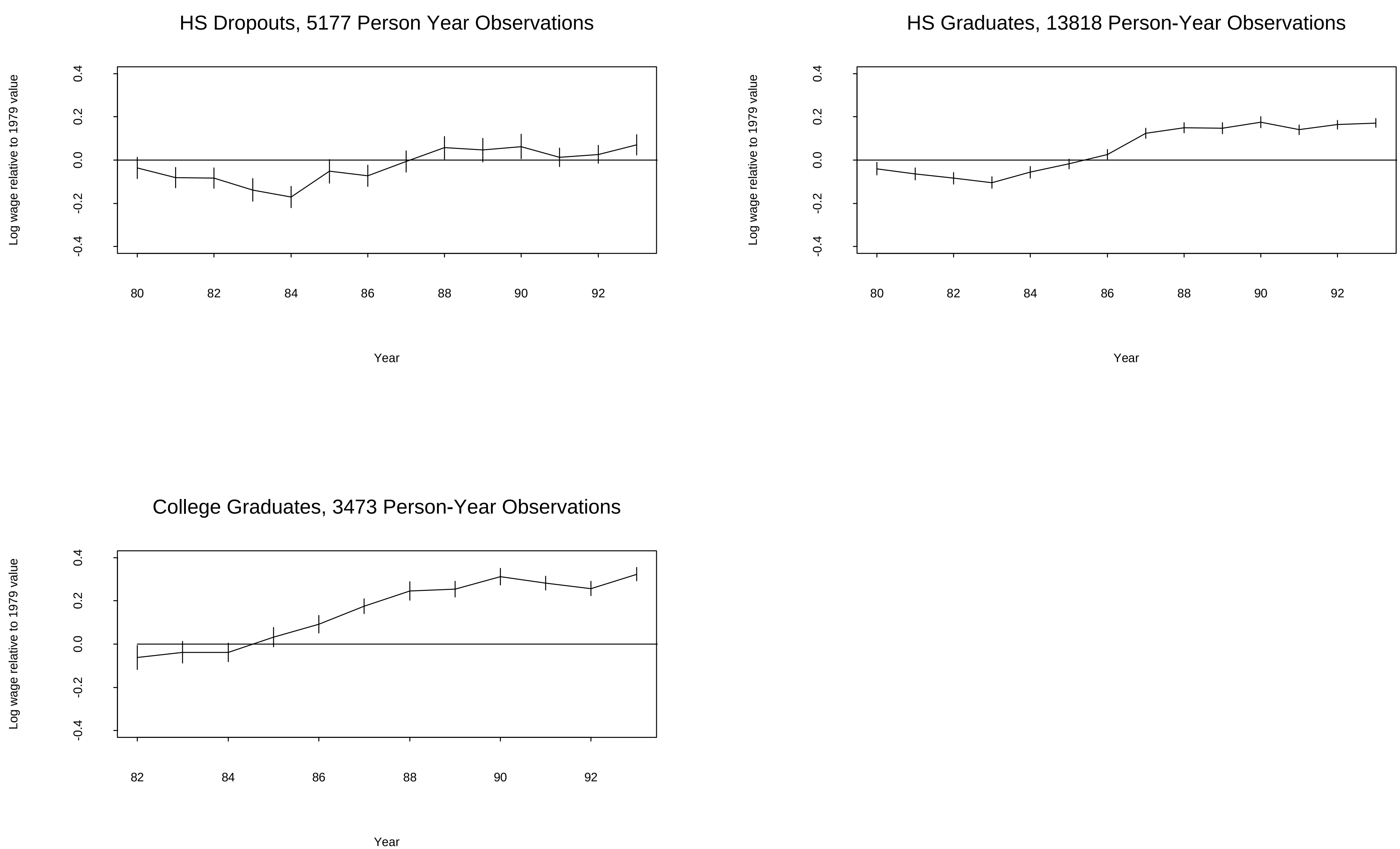


Figure 4: Return to Ability over Time

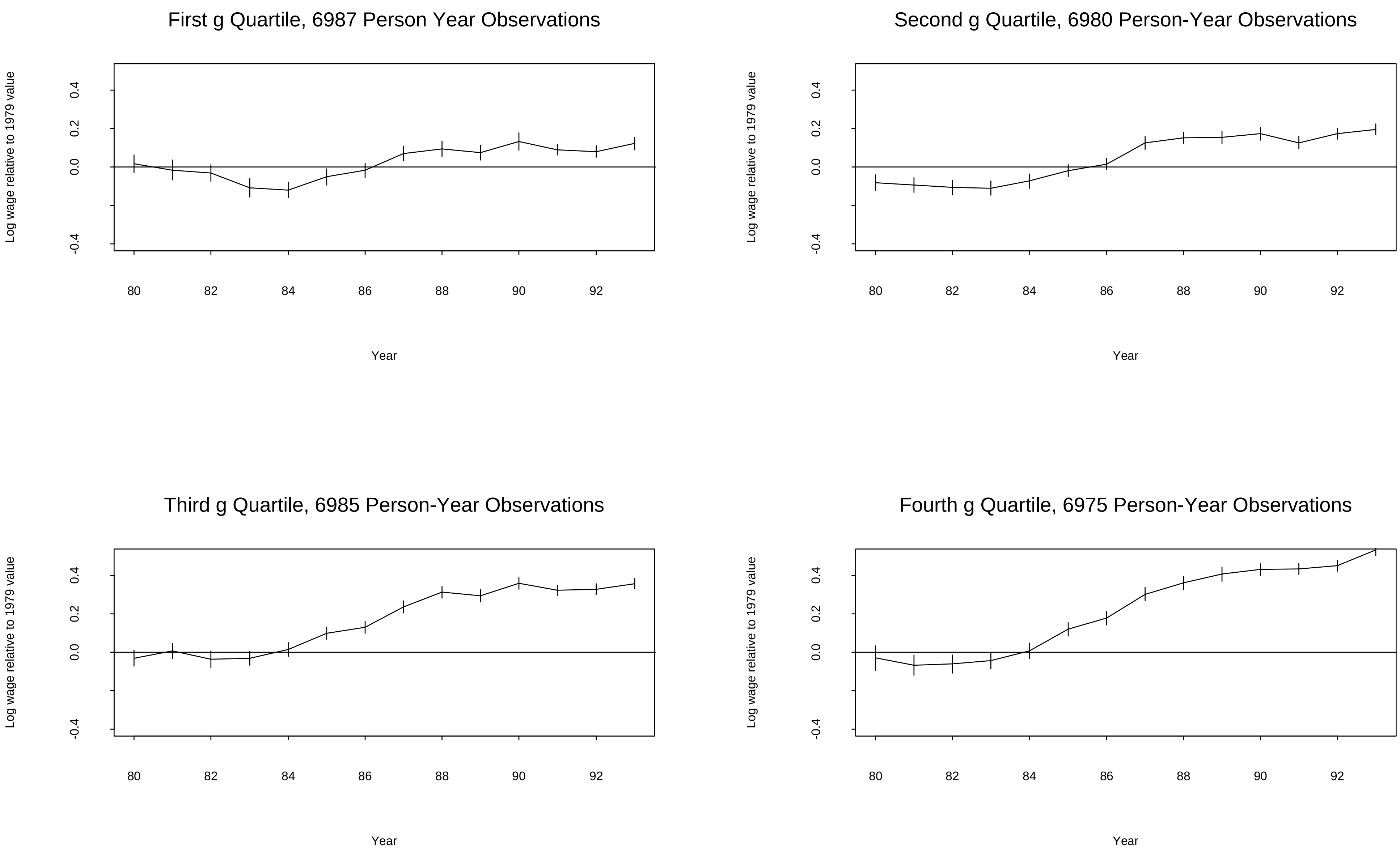
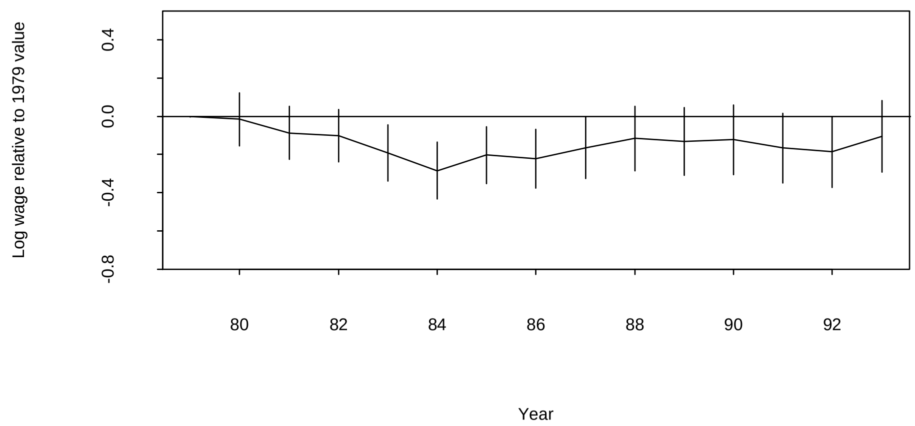


Figure 5: High School Dropouts

Based on spline regression of log wage on ability, with coefficients allowed to vary freely with education, age and time, subject to age and time having additively separable effects

First g Quartile



Second g Quartile

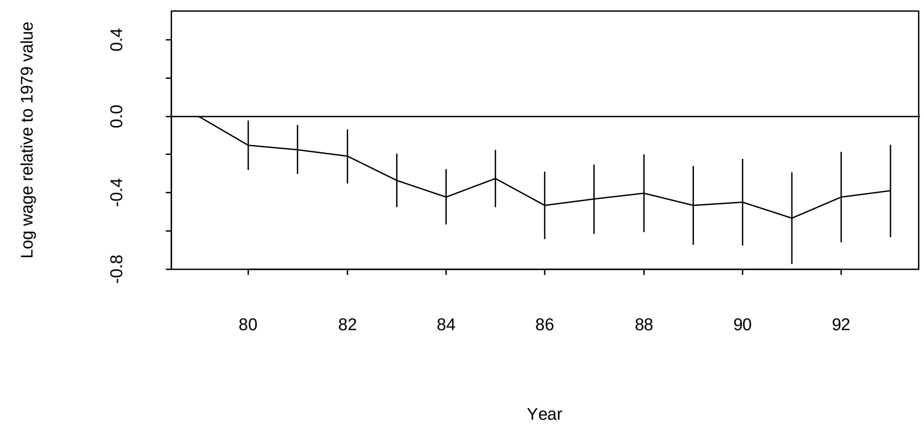


Figure 5: High School Graduates

Based on spline regression of log wage on ability, with coefficients allowed to vary freely with education, age and time, subject to age and time having additively separable effects

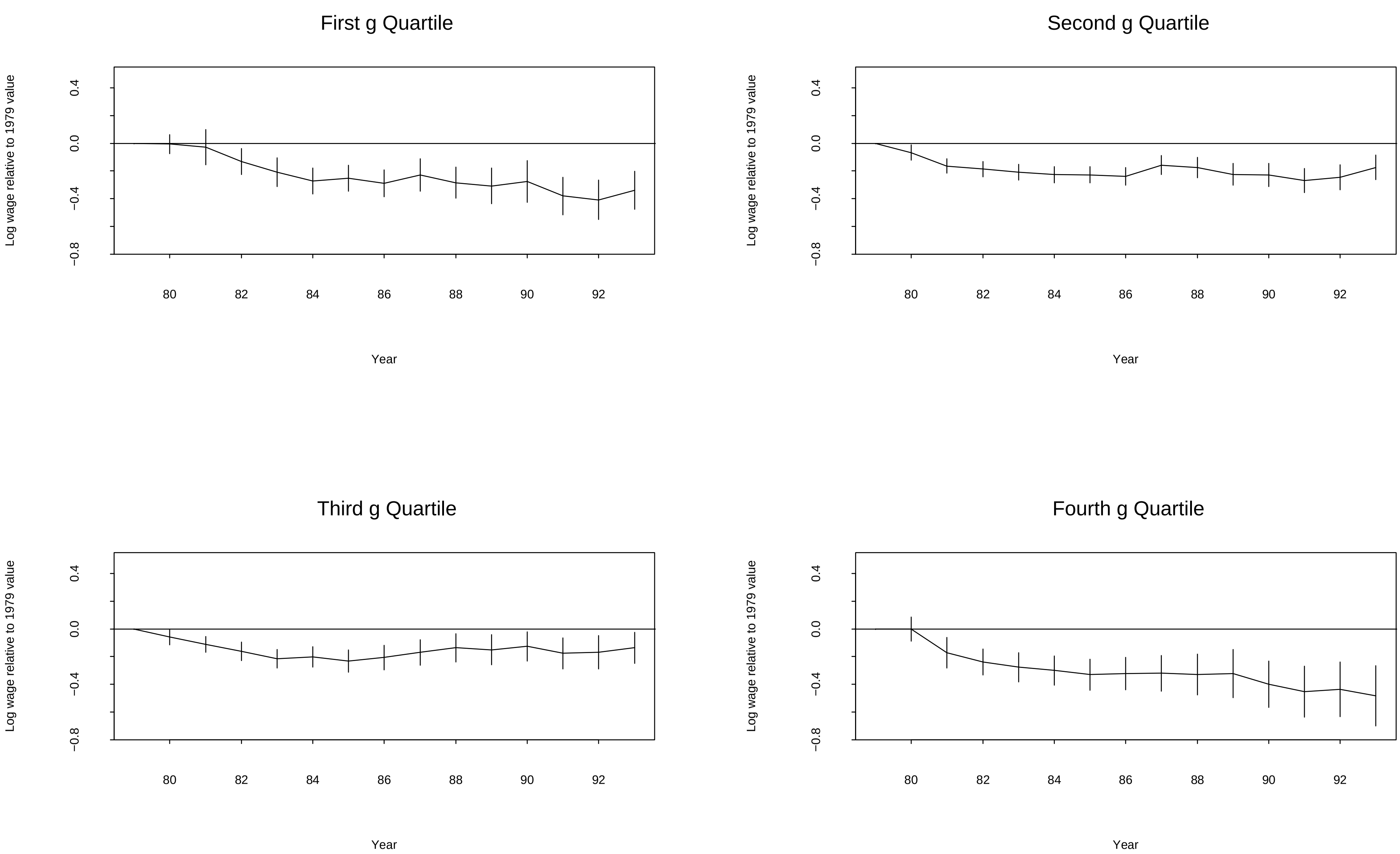


Figure 5: Some College

Based on spline regression of log wage on ability, with coefficients allowed to vary freely with education, age and time

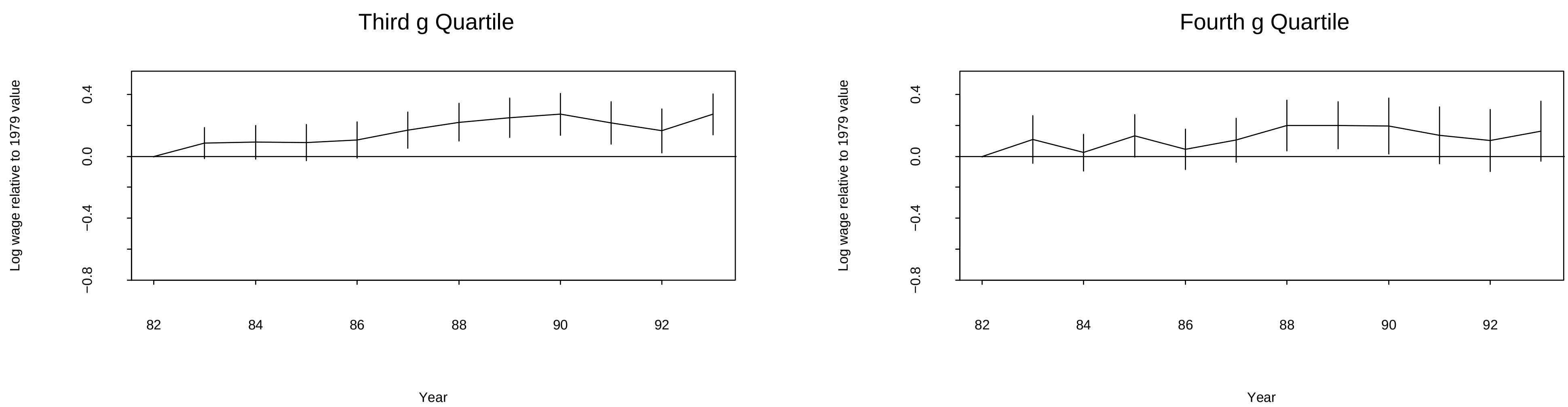


Figure 5: College Graduates

Based on spline regression of log wage on ability, with coefficients allowed to vary freely with education, age and time, subject to age and time having additively separable effects

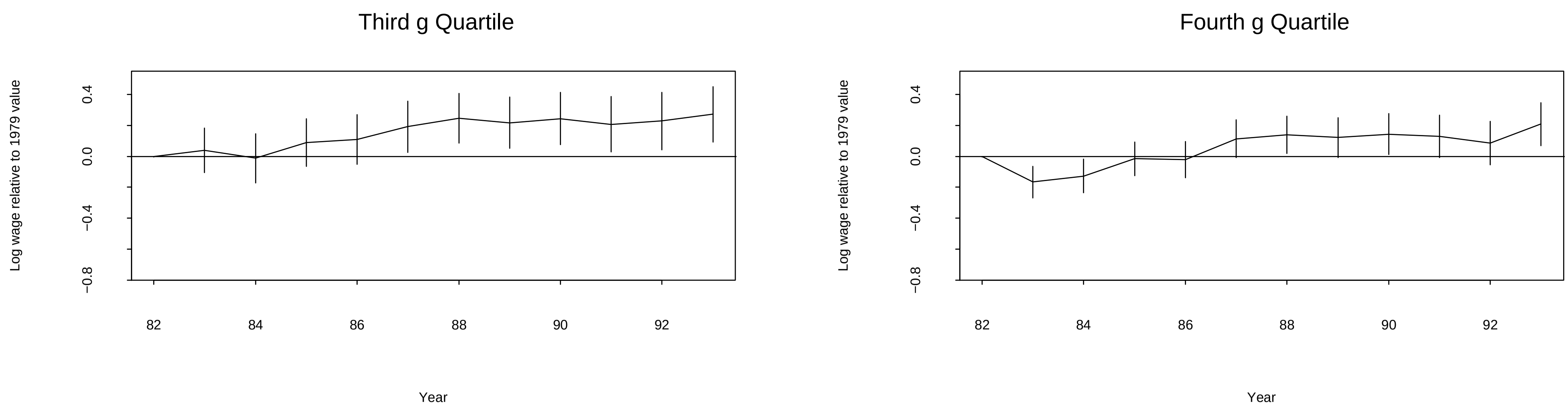


Figure 6: HS Grads vs. College Grads, Fourth g Quartile

Based on spline regression of log wage on ability, with coefficients allowed to vary freely with education, age and time

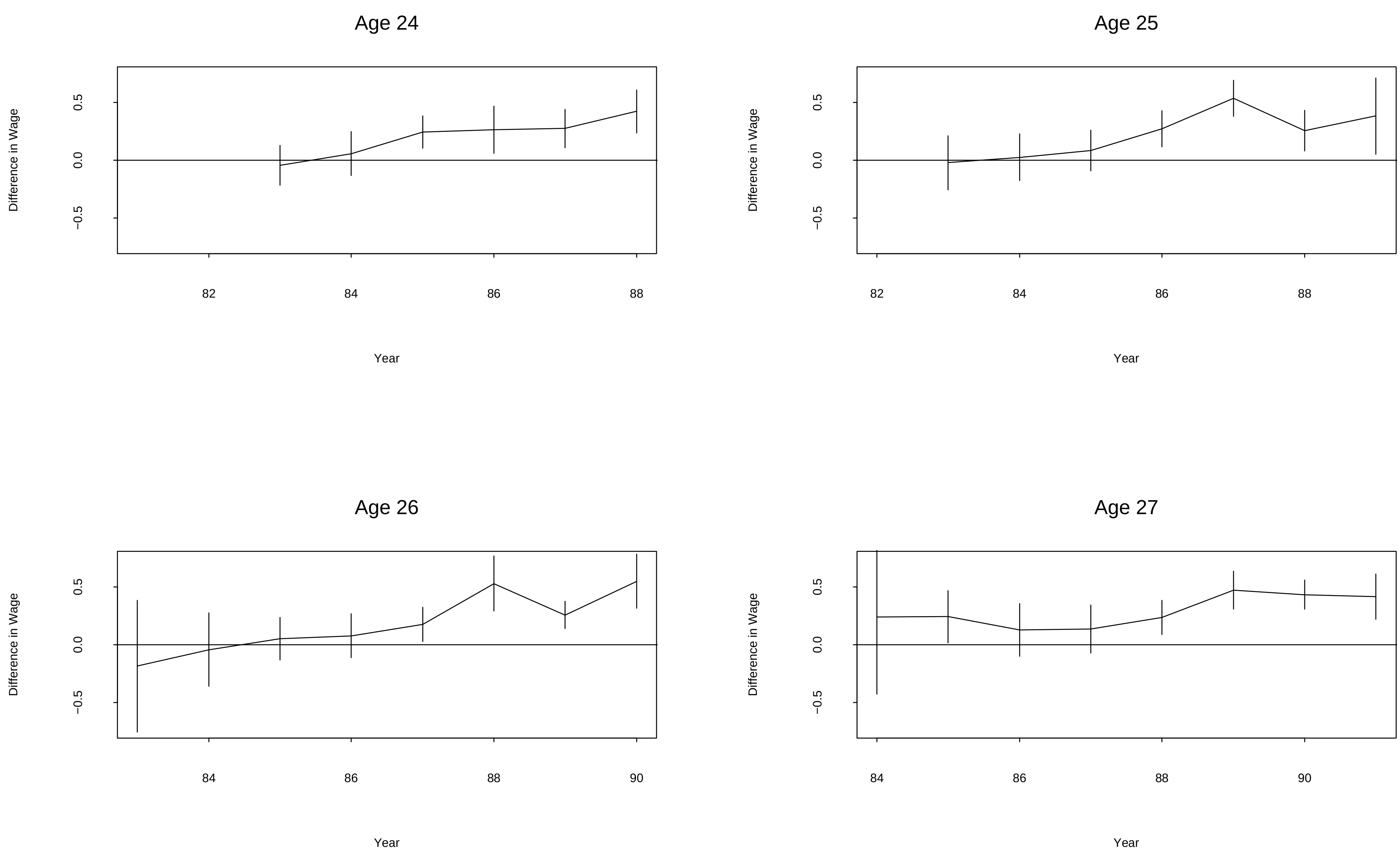


Figure 6: HS Grads vs. College Grads, Fourth g Quartile

Based on spline regression of log wage on ability, with coefficients allowed to vary freely with education, age and time

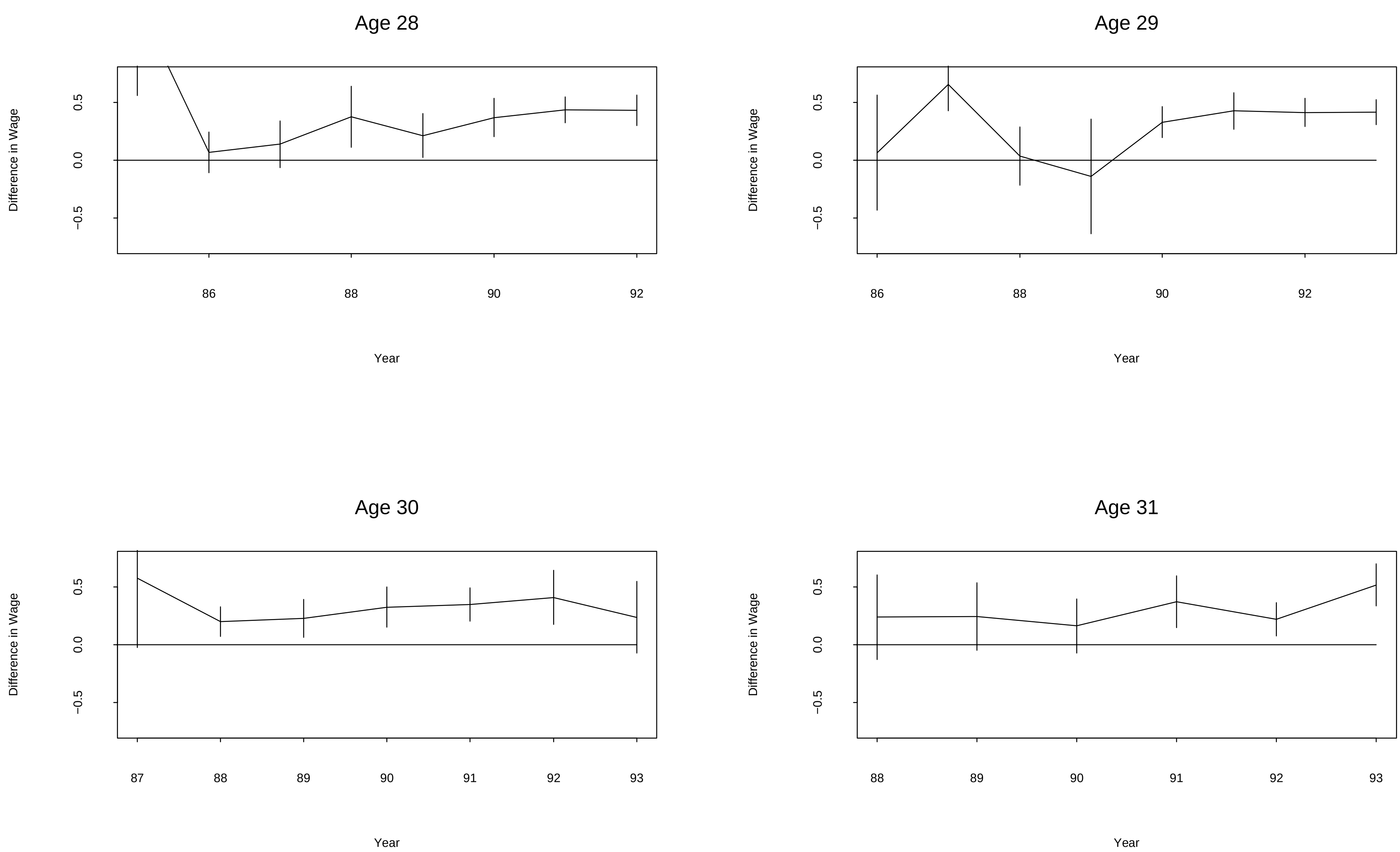


Figure 7: HS Grads vs. College Grads, Third g Quartile

Based on spline regression of log wage on ability, with coefficients allowed to vary freely with education, age and time

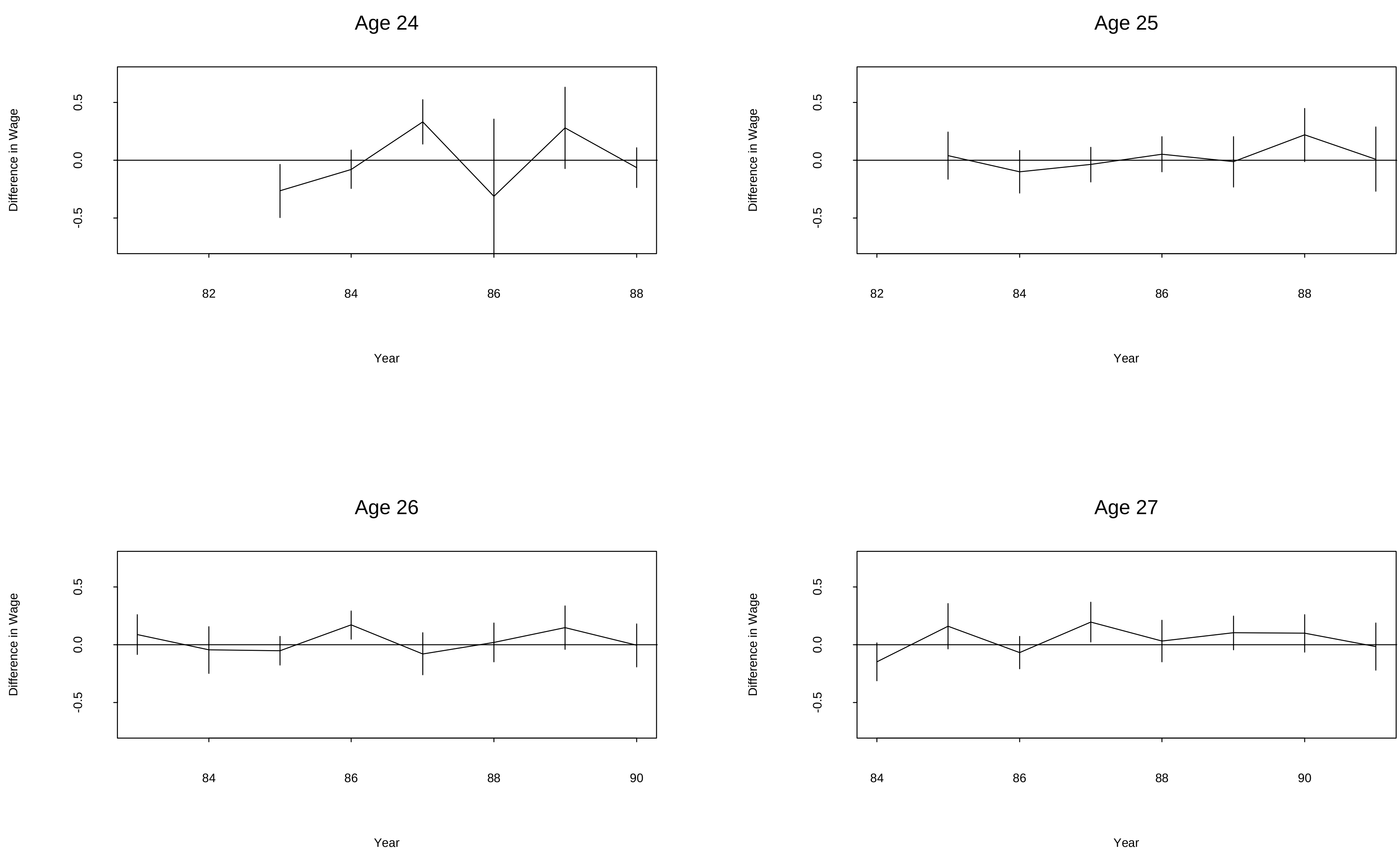


Figure 7: HS Grads vs. College Grads, Third g Quartile

Based on spline regression of log wage on ability, with coefficients allowed to vary freely with education, age and time