The effect of school starting age on school and labor market performance*

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Abstract

In Sweden, children typically start compulsory school the year they turn seven. Individuals born just before or just after the new year have about the same date of birth but start school at different ages. We exploit this source of exogenous variation to identify the effects of age at school entry on school and labor market outcomes. Using data for the entire Swedish population born 1935-84, we find that children who start school at an older age do better in school and go on to have more education than their younger peers. However, since starting school later entails the opportunity cost of entering the labor market later, the net earnings effect over the entire life-cycle is negative.

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1 Introduction

At what age should children start school? This is a concern for most parents. Yet, there is little credible evidence on the importance of school starting age for outcomes in the longer run. The main purpose of this paper is to fill some of this void. Specifically, we ask the question: How does school starting age affect school performance, educational attainment and long-run labor market outcomes? To answer this question we exploit exogenous variation in school starting age due to month of birth and the school entry cut-off date (the 1st of January). This setting implies a regression-discontinuity design which we apply to unique Swedish administrative data. The data cover the entire Swedish population born between 1935 and 1984. The data set contains a multitude of information: earnings and educational attainment in 2000 for all birth cohorts; and for a sub-set of the cohorts (typically the youngest ones) there is information on school performance at the end of compulsory school.

Swedish data are particularly apt for examining the issues addressed in this paper. One advantage is that the number of years of compulsory education is more or less given. The compulsory schooling law requires individuals to complete 9 years of education, independently of when they start. Moreover, grade retention or advancement is rarely practiced in the Sweden. These two features facilitate the identification of the school starting age effect, since the effect is not contaminated by the variation in years of compulsory schooling.

Another advantage of the data is that they span a vast range of cohorts. Thus, we can analyze long-run earnings and education outcomes, which is the main value added of the paper.[1] And we can trace out the earnings effects of school starting age for individuals at different points in the life-cycle, thus providing a sense of when any gains and losses accrue over the life-cycle. An analysis of these issues using U.S. data is not possible since the effect of school entry age is contaminated by the state school leaving age legislation.[2]

[1] In fact, we have seen no previous analysis of the earnings effects of the variation in school starting age. Just recently a few papers (Dobkin and Ferreira, 2007, and Black et al., 2008) have appeared on this topic.

[2] The U.S. compulsory schooling laws typically require students to remain in school until their 16th or 17th birthday. Individuals who enter school at an older age have fewer years of compulsory schooling since they reach the legal dropout age at an earlier point in their schooling careers than students who enter school at a younger age. Since the possible effects of school starting age are contaminated by the effects of school leaving age legislation, estimates of season of birth on education and earnings using U.S. data (e.g. Angrist and Krueger, 1991) cannot be interpreted as the (reduced form) effects of school starting age.

The fact that we have access to long-run outcomes offers a solution to a fundamental identification problem encountered in the literature. Studies using data on in-school performance face the problem that in a given grade, age at test is the sum of age at school start and time spent in school. Therefore, such studies generally fail to separate the effect of age at school start from any direct age effect. With information on adult outcomes, observed in the cross-section, it is possible to separately identify the direct effect of age and school starting age.

A final advantage or our data is that they span cohorts who attended school in different systems. The older cohorts went to school in a selective system with early tracking; the younger cohorts attended a comprehensive system where students were held together. Thus, we can examine the importance of the selectiveness of the schooling system for the long-run effects of school starting age within a single country, rather than comparing across countries while children are still in school as in Bedard and Dhuey (2006).

The literature on the relationship between age at school and skill acquisition has emphasized two kinds of effects: one due to absolute maturity – learning in a school environment is more/less effective at certain ages – and another due to relative maturity – being the oldest in class gives an early advantage which may persist in the longer run.[3] The variation we are using captures both of these two types of effects. Note that, even if only relative maturity at school start is relevant, it is likely that the effects will persist for some time. In systems where children are tracked early on it is more likely that early advantages will persist; see Bedard and Dhuey (2006).

The earnings effects of school starting age reflect the effect on skills but also the opportunity cost of starting late. Conditional on the effect on skills, the opportunity cost of starting at an older age comes in the form of a shorter time horizon to collect the returns from human capital investments and less experience for a given age.

The results show that children who start school when they are older do better in school. Older school starters go on to have more schooling. The effects on educational attainment are more pronounced for the cohorts who attended the more selective schooling system. The earnings effects of age at

[3] The effects due to absolute maturity include: evidence from genetics suggesting that there are "critical periods" of brain development where the child is especially sensitive to specific experiences (Shonkoff and Phillips, 2000); and theories emphasizing that young children lack the maturity to learn complicated things in a school environment. The effects due to relative maturity include: a theory based on peer quality, where younger children may benefit from being surrounded by older and more able peers; and evidence from psychology where older children respond to early encouragement by pushing to perform even better in the future.

school start are negative for the youngest birth cohorts – since children who start school later also enter the labor market later – but the long run effects are positive (although fairly small). The earnings effects primarily reflect labor supply effects; the positive long-run estimates, for instance, are mostly driven by a higher probability of working among those who start late. The net earnings effect over the entire life-cycle is negative and estimated to be in the order of 1-2 percent. This suggests that the opportunity cost of starting late outweighs the earnings gains accruing later on in life.

2 Previous literature

There is an extensive educational literature on the relation between age at school entry and (early) academic performance (see Stipek, 2002 for a survey). This literature is not particularly informative about the effects of school starting age, however. There are two reasons: first, much of the literature fails to account for the endogeneity of school starting age; second, it fails to separately identify the effects of school starting age. All studies examining in-school performance face the fundamental identification problem of separating the effects of school starting age from the effects of age at test. This problem arises from the identity that for individual i at time t

$$A_{it} = A_i^s + S_{it}^c \tag{1}$$

where A denotes age at test, A^s school starting age, and S^c years of compulsory schooling. In a given grade (S^c given), children who are older when they start school are also older when they do the test.

There has been a recent surge in the economics literature on issues related to age at school start.[4] Most of these studies look at short-run effects, e.g., Strøm (2003), Datar (2005), Bedard and Dhuey (2006), and Elder and Lubotsky (2008). The studies focusing on in-school outcomes suggest that older kids do better in school. But for reasons given above, it is generally not clear whether these effects are due to school starting age or age per se.[5]

[4] Some of these studies follow the approach of Angrist and Krueger (1992); they thus ignore any direct effect of school starting age and use timing of birth to identify years of schooling (e.g., Cascio and Lewis, 2006, Del Mondo and Galindo-Rueda, 2004, and Leuven et al., 2004).

[5] Strøm (2003) fails to distinguish between the effects of A and A^s; Bedard and Dhuey (2006) have the same problem in their main analysis, based on a cross-section of countries in TIMSS; Datar (2005) relies on functional form assumptions (linearity in A) to separate the two. Elder and Lubotsky (2008) also rely on functional form in their analysis of the efficiency of starting school at different ages.

The long-run consequences of school entry age are arguably the most interesting ones. Longer-run outcomes, such as educational attainment and earnings, observed after the end of compulsory school also provide a convenient solution to the identification problem. In the cross-section of adults of different ages, A^s and A vary independently (S^c is still given); therefore, one can control for the direct effect of age.

There are only a few recent studies looking at the effects of school entry age on longer run educational attainment. Fertig and Kluve (2005) use German data covering 18-29 year olds; Plug (2001) conducts a similar analysis using data for the Netherlands. At face value, these two studies suggest that older school-starters have more schooling than those starting at a younger age. There are a couple of potential problems with these studies. First, they use the entire season of birth range to identify school starting age effects, rather than relying on the (sharp) discontinuity implied by the school entry cut-off date. The results may thus be contaminated by unobserved ability related to season of birth. Second, grade retention and advancement is commonly practiced in the Netherlands and (West) Germany. In the Netherlands, for instance, a quarter of the males repeat a grade. This implies that years of schooling at the compulsory level varies with timing of birth, which complicates the interpretation of the effects.[6]

Studies of the effects of school starting age on earnings are much less common; a recent exception, however, is the paper by Black et al. (2008).[7] The U.S. school leaving laws imply that it is hard to interpret the reduced form relationship between date of birth and earnings as the effect of school starting age (e.g. Angrist and Krueger, 1991).[8] Some studies have used quarter of birth as an instrument for estimating the return to schooling. Whether quarter of birth is excludable or not has been discussed at length in the literature (e.g., Bound and Jaeger, 2000) and we will not rely on this exclusion restriction. Instead, we estimate the reduced-form effect on earnings of school starting age (without controlling for experience and schooling). This reduced-form parameter is what we mainly should focus on if we are interested in the benefits and costs of alternative school starting ages.

[6] Grade repetition also plagues the interpretation of the findings in Elder and Lubotsky (2008). Plug (2001) attempts to "solve" the problem by controlling for grade retention and advancement. Since, retention and advancement are likely outcomes of school starting age this is not a satisfactory procedure.

[7] Their analysis of Norwegian data is in many ways similar to our analysis.

[8] Despite these difficulties, Dobkin and Ferreira (2007) attempt to estimate the school starting age effect on education and earnings using data for California and Texas. Their estimates imply that those who are older at school start do better in school but go on to have lower educational attainment. This configuration of the results suggests that they have not fully handled the problems caused by the school leaving age legislation.

3 Compulsory schooling in Sweden

Since the birth cohorts in our data span 50 years, we give a brief account of the historical development of the lower levels of schooling in Sweden.[9] While there have been some changes in the system as such, the school entry laws have not changed: throughout the relevant time-period, compulsory school has started the year the children turned 7 which implies a school entry cut-off date on the 1st of January.

Individuals in the oldest cohort (born 1935) were exposed to a rather selective schooling system, where 7 years of schooling were mandatory.[10] Compulsory schools were run locally and local authorities determined the curriculum. There was strict ability tracking starting in 5th or 7th grade. Children in different tracks went to different schools; children in the lower tracks had scant opportunities to pursue further education.

In 1950, the parliament decided to introduce a 9-year "comprehensive school" gradually across the country. The comprehensive school abolished the strict tracking system and featured a nationally determined curriculum. There was still some tracking in lower secondary school. Importantly, however, students in different tracks attended the same school. Moreover, choosing the lower track did not imply that further educational opportunities were closed. The gradual introduction of the comprehensive school mainly affected the cohorts born between 1945 and 1955 (Holmlund, 2007). The comprehensive school was fully implemented starting with the cohort born in 1961.

Since the comprehensive school reform, the basic structure of the system has not changed. After compulsory school, students may go on to upper-secondary school, offering several programs, ranging from vocational training to university-preparatory programs. The majority of students from university-preparatory programs then go on to university education.

Since 1970 there have been a fair amount of changes at the pre-primary level. These changes are relevant as they affect the alternative to starting school early. In the mid 1970s, pre-schools – starting at age 6 – were introduced. During the 1970s there was also a massive increase in the number of child care slots. This build-up implied that around 60 percent of those born 1985 attended child-care at age 5.

For successive birth cohorts, it is reasonable to expect that the contents of "treatment" and the "alternative" change; for example, curricula, pedagogical techniques, tracking ages, pre-school/child-care attendance change over time. We do not have information on all these changes. But we can

[9] We base this presentation on Gunnarsson et al. (1999).

[10] Compulsory schools were subsequently extended to 8 years on the decision of the local authorities (this is mainly relevant for the cohorts born 1938-1945).

examine if the estimates vary over birth cohorts.

4 Identification strategy

Suppose the outcome of interest (test scores, grade points, years of completed schooling or earnings) for individual i at time t is given by:

$$Y_{it} = \beta_0 + \beta_1 A_{it} + \beta_2 S_{it}^c + \beta_3 A_i^s + \mathbf{X}_{it}\boldsymbol{\beta}_4 + \varepsilon_{it}, \tag{2}$$

where $\mathbf{X}_{it}$ is a vector of individual characteristics and β_3 the parameter of primary interest. Age (A_{it}), years of compulsory schooling (S_{it}^c), and age at school start (A_i^s) are endogenous variables in a given grade. In expectation, however, S_{it}^c is the same for everyone and we subsume it in what follows.

Since parents and school administrators influence the age when children start school, we need an instrument to identify the true effect of age at school entry on the outcome. We exploit the exogenous variation in age at school entry driven by the children's date of birth and the school entry cut-off date. In particular, children born on each side of the new year have about the same date of birth but differ in their school starting age by almost a year. This is an application of Thistlethwaite and Campbell's (1960) regression-discontinutiy design, where the regressor of interest (A^s) can be expressed as a known discontinuous function of an underlying variable (date of birth). The school starting age legislation implies that expected school starting age ($f_i^{A^s}$) is given by

$$f_i^{A^s} = 7 + \frac{8 - MoB_i}{12}, \tag{3}$$

where MoB_i denotes month of birth and 8 reflects the fact that schools start in August. Children born in January ($MoB_i = 1$) typically start school at 7.6 years of age. The expected school starting age function falls monotonically to 6.7 years for children born in December. The function then makes a sharp jump back to 7.6 years at January 1st, generating a saw-teeth shaped pattern for age at school entry by month of birth.

The discontinuity of $f_i^{A^s}$ is crucial for identification. It implies that we can include any smooth function of date of birth to control for the direct effect of season of birth on outcomes. In particular, we specify birth cohort fixed effects along with a quadratic in date of birth which is allowed to shift at the break-point (January 1st). Note, that we redefine year-of-birth to run from July to June such that the discontinuity is in the middle of the "year". An obvious identifying assumption is that the direct effect of month of birth does not "jump" at the point of the discontinuity. This seems like a rather innocuous assumption to us.

A further requirement for the validity of the instrument is that the exact timing of birth should be random. In particular, the characteristics of children should be randomly distributed around the discontinuity. We address this issue in several ways in the regression analysis. First, we examine whether family background is related to expected school starting age. Second, we directly control for observed characteristics and examine if the estimate of the key parameter changes. Third we limit the analysis to individuals born between December 15 and January 14, i.e., we restrict the data to a short time frame where there is little reason to expect that parents can plan the timing of birth.

Our analysis of in-school outcomes will suffer from the same problem as previous analyses: we cannot separate the effects of A and A^s, since, in expectation, they do not vary independently.

For the long-run outcomes, observed in the cross-section of adults who have completed compulsory school, A is exogenous and varies independently of $f_i^{A^s}$. The flexible controls for date of birth then effectively remove any direct effect of age on the outcomes. If expected school starting age is a valid instrument, we can estimate the causal effect of school starting age on the long-run outcomes.

5 Data and description

5.1 Data

The data mainly come from administrative records but also from some surveys. The administrative data originate from Statistics Sweden and cover the entire population born in Sweden 1935-84 (around 4.8 million observations).[11] Information on year and month of birth originate from birth records and do not suffer from measurement error. Census information on the educational level of the biological parents have been linked to the individuals born 1941-1982. The coverage of the parental information increases with year of birth; it is 80 percent or higher for individuals born after 1960.

School starting age is unfortunately not reported in Swedish administrative records. We therefore use different data sources to construct such a measure. For individuals born 1972-84, there is information on age at compulsory school completion (in ninth grade). Therefore, we calculate school

[11] We exclude all 900,000 immigrants since they lack reliable information on date of birth, school starting age, and years of schooling. Further, individuals who have deceased or emigrated by 2000 are not covered by the data.

starting age for these cohorts as:

$$A_i^s = (\text{Age at compulsory school completion})_i - 9 + \left(\frac{8 - MoB_i}{12}\right)$$

Since grade retention/advancement is rarely practiced in Swedish schools, potential mis-classification is a very minor issue.[12]

To obtain a measure of school starting age for earlier cohorts we use data from the so called UGU project run by the Department of Education at the University of Göteborg. These data include scores on intelligence and achievements tests, as well as grade point averages in sixth grade, for representative samples of cohorts born 1948, 1953, 1967, 1972, 1977, and 1982. The UGU-data contain the age of individuals (in a given grade) which we use to calculate age at school entry.[13] School starting age in the UGU samples is then regressed on month of birth to predict school starting age for the 1935-71 cohorts in the administrative data.[14]

Final compulsory school grades are available for the 1988-2000 period. The normal graduation age is 16 years. Due to delayed entry for some individuals, we have complete compulsory school grades for the 1975-83 cohorts only. The final grades have been set according to two different systems. The cohorts graduating 1988-97 were subject to a relative system where the grades were set to obtain a given national average (and standard deviation). The cohorts graduating from 1998 and onwards were subject to an "absolute" system containing four levels. To make these different grading systems comparable, we attach a percentile rank to each grade for all subjects.[15]

National achievement tests have been undertaken in both the old and the new grading system. These results are advisory and, hence, teachers might deviate from them. As long as teachers do not systematically compensate or punish some groups, this should not be a problem. Nevertheless, we use the UGU data to assess the problem in the regression analysis.

Educational attainment data pertain to 2000. We convert the attainment level to years of education using the Swedish Level of Living Survey (SLLS) conducted in 2000. The SLLS includes register information on educational

[12] Data for the 1960s suggest that half of those finishing late (only 3.6 % of the population) were retained during compulsory school. Corman (2003) and Eide and Sholwater (2001) show that grade retention and advancement is strongly related to season of birth in the U.S. Since retention/advancement induces variation in years of schooling, this contaminates the estimates of the effects of the school entry age.

[13] The grade in which the age is reported varies somewhat over cohorts.

[14] We apply this procedure solely to get the right scaling of the IV-parameter.

[15] We have ranked the grades in the old and the new system separately. In the empirical analysis, we let year of birth fixed effects capture any cohort trends in the grades.

attainment and survey information on time spent in school for a representative sample of individuals aged 18 to 75. We predict years of education for the entire population using the regression estimates derived from the SLLS data.[16]

The earnings measure comes from the income tax registers. It is measured as of 2000 and defined as the sum of annual gross wage earnings and compensation during temporary work absence (illness or parental leave).[17]

5.2 Descriptive analysis

Average school starting age in our sample is 7.2 years.[18] Some children start school already at age five and others at age 10. But the overwhelming majority (97%) start school the year they turn seven.

Figure 1 shows the relation between age at school start and grade point average (GPA) for the 1975-83 birth cohorts. The performance in compulsory school by school starting age exhibits a stepwise negative relationship. Note the sharp drops in performance just at the minimum and maximum of the normal school starting age (6.8 and 7.7 years, respectively). Within the segment of normal school starters, there is a positive relation between age at school entry and ninth grade GPA. The highly irregular relation between age at school start and school performance is most likely due to the non-random selection of children with early or delayed school start and cannot be given a causal interpretation.

Let us instead examine how outcomes are related to expected school starting age. A first issue is the power of the instrument. Figure 2 illustrates that the "first-stage" relation is very strong. It is only around the cutoffs that actual and expected school starting age deviate slightly. Children born in January tend to start early to a greater extent than those born in December: 2.6 (0.02) percent of the children born in January (December) start school one year early. The opposite pattern is true for the delayed: 8.8 (0.8) percent of children born in December (January) are delayed.

Figure 3 shows the relationship between final GPA and expected school starting age. GPA follows the saw-teeth pattern of expected school starting age closely, with sharp jumps in student achievement just around the school entry cut-off date. Children born in the beginning of the year on average perform better than children born later. The GPA difference between Janu-

[16] We allow years of schooling associated with each level of education to vary smoothly across cohorts by estimating separate regressions for each birth cohort ($\pm$ five cohorts).

[17] The wage earnings information in the registers are based on statements of income from the employers.

[18] Appendix Table A1 reports the full set of descriptive statistics.

ary and December births is 4-5 percentile ranks. It is reasonable to believe that some of this difference is caused by the school starting age legislation. We subject this conjecture to formal tests in next section.

How do the long-run outcomes vary with expected school starting age? Figures 4 and 5 examine this question. Figure 4 presents average years of education in 2000 by date of birth for those born between 1955 and 1964, while Figure 5 contains the corresponding information but in this case for earnings in 2000.[19]

According to Figure 4, individuals born in January have more schooling on average than those born in December. Thus, the sharp discontinuity in expected school starting age is translated into an upward jump in educational attainment around the break-point. This implies that the observed relation between season of birth and GPA is not solely due to age differences. Individuals born at the end of the year or in the beginning of the next have about the same age but start school at different ages. Thus, there is a long-run positive effect of age at school start on educational attainment. According to Figure 5, however, there is no (visible) systematic pattern for earnings.[20]

6 Regression analysis

The purpose of this section is to present a collection of evidence on the importance of age at school start for schooling and labor market outcomes. Section 6.1 discusses the validity of the instrument and the remaining sections (6.2-6.4) present results for the three outcomes. Section 6.2 thus deals with compulsory school outcomes. This is the natural place to start, although we cannot separate the effect of the school entry age from the effect of age differences when the outcome is measured. Sections 6.3 and 6.4 examine the long-run effects on education attainment and earnings of differences in school starting age.

6.1 Validity of the instrument

The big concern with respect to the validity of the instrument is whether different families time their births with respect to the school entry cut-off. Table 1 addresses this concern by presenting a set of IV estimates of the relationship between parental education and school starting age. The instrument

[19] Note that we have detrended the data on attainment and earnings by subtracting off the average for each birth cohort.

[20] Notice that Figure 5 plots median log earnings by date of birth. Individuals with no earnings are included when calculating the median.

is expected school starting age and parental education is defined as years of schooling for the parent with the highest level of education.

Column (1) reports the results for a standard regression discontinuity specification, including year of birth fixed effects and a linear date of birth trend in an attempt to control for the direct effect of season of birth.[21] The parents of children who start school a year later have an additional 0.07 years of schooling. This is not much, but sample size renders the estimate statistically significant.

We proceed by adding a more flexible control function in date of birth. In column (2) we include a quadratic in date of birth which is allowed to shift at the discontinuity. This is sufficient to remove the systematic relationship between parental education and school starting age: the parents of children who start school a year later have 0.01 years of extra schooling.

We also use auxiliary data where we observe the birthday rather than birth month. These data come from the military enlistment and thus pertain to men. Using these data, we specify a discontinuity sample consisting of men born between December 15 and January 14.

For completeness, column (3) reports the estimate on the school starting age for men using the same specification as in column (2). The estimate is slightly higher than in the full population but not statistically significant. Column (4), finally, shows that family background is unrelated to expected school starting age in the discontinuity sample.

The results in Table 1 suggest that the date of birth controls included in column (2) are necessary to remove a systematic relationship between expected school starting age and parental education; it is with this specification we proceed.

6.2 Compulsory school outcomes

Table 2 examines the relationship between school starting age and grade point average. The grade point average pertains to the ninth grade and we base the estimates on the 1975-83 birth cohorts.

The first column presents the OLS estimate. The association between actual school starting age and student performance is negative. However, this estimate is driven by selection as illustrated by the two reduced forms presented in columns (2) and (3). It is no surprise that the relationship between actual and expected school starting age is very strong (and positive).

[21] To abstract from any trends and seasonalities in birth rates, we always weight the estimates by the inverse probability of being included in the sample. The a priori reason for weighting is that we want the results to apply to all individuals independently of when they were born. Weighting does not make a difference for the results, however.

Moreover, the relationship between GPA and expected school starting age is positive. These two reduced forms imply our preferred IV estimate presented in column (4). The causal effect of starting school at an older age is thus positive: increasing school starting age by a year raises GPA by 5 percentile ranks (which corresponds to 0.17 standard deviations of the transformed GPA distribution). This estimate is in the mid range of the estimates for various countries reported by Bedard and Dhuey (2006): their estimates, based on TIMSS data in grade 8, suggest that the oldest children have an advantage of 2-9 percentiles over the youngest children.[22]

Our identifying variation comes from the sharp school starting age difference between January and December kids. Table 1 suggests that the omission of other (observed and unobserved) characteristics should not bias the estimate. Nevertheless, the remaining columns of Table 2 report the results from alternative specifications. Column (5) shows that the estimate is unaffected by controlling for parental education (controlling for school fixed effects has no impact either). Column (7) is based on the discontinuity sample containing men born within 2 weeks of either side of the break-point. For purposes of comparison, column (6) contains the baseline estimates for men; this estimate (5.17 percentile ranks) is slightly higher than the estimate for both sexes. The discontinuity sample produces an estimate of 5.3 percentile ranks which is not significantly different from the estimate reported in column (6). Thus, the baseline estimate is very robust.[23]

We have also estimated separate regressions by subject and gender. The estimates of the key parameter of interest are remarkably stable across the academic subjects. The school starting age effect is marginally lower for females (5.08) than for males (5.17).[24]

Separate regressions by family background suggest that the effect on GPA is slightly higher for children who have non-academic parents (5.10) than for children whose parents have an academic education (4.88). That these estimates do not differ much is perhaps surprising, in particular if the alternative to starting school early would be to stay at home with parents whose charac-

[22] The estimate for Sweden, reported by Bedard and Dhuey (2006), is almost 6.5 percentile ranks, while the corresponding estimate for the U.S. is above 8 ranks.

[23] In fact, there is a slight source of downward bias in our estimates. Roughly 2 percent of the cohorts born 1975-83, living in Sweden in 2000, have no grades whatsoever. The probability of not being included in the data is decreasing in the school starting age, which may yield a very minor downward bias in our estimates.

[24] Fredriksson and Öckert (2006) present these results in full detail. The only instance when the school starting age effect was substantially different was for sports. Sports was also the case where we found a significant gender difference: 11.2 for males and 5.8 for females. The natural interpretation of these results is that physical development is more vital in sports than in the academic subjects.

teristics vary. But remember that, for a significant fraction of these cohorts, the alternative to a large extent would imply proceeding in some form of pre-primary education.

Above we raised some concerns about the informational value of grades. Appendix Table A2 shows that these concerns are unwarranted. The table reports school starting age effects on a variety of 6th grade outcomes for the cohorts available in the UGU-data (6 cohorts born 1948-82). These data contain information on IQ and achievement tests along with grades in Swedish, English, and Math. A drawback is that we cannot rely on the sharp discontinuity to identify the key effect of interest; thus, the estimates are most likely biased.[25] It is unlikely that this is a big issue for the relative comparison across outcomes (for example achievement scores and grades), however. Table A2 contains several messages. First, it does not matter much whether we measure outcomes in terms of grades or scores on achievement tests. Second, there are school starting age effects also for the IQ test scores: measured IQ is "malleable". Third, while there are some differences across cohorts, the effects on IQ test scores are not appreciably different when, e.g., the cohorts born 1948 and 1953 are compared to the cohorts born in 1972 or 1977. Since the same IQ test has been given to all cohorts, the qualitative nature of the results reported in Table 2 should hold for the older cohorts as well.

6.3 Educational attainment

Let us turn to the longer-term effects of variation in school starting age. As noted above, these estimates can be thought of as giving the pure (long-run) effect of variation in school starting age.

Table 3 examines whether educational attainment is affected by school starting age. We restrict attention to the cohorts above age 25, who are assumed to have completed education. The educational outcomes are represented by (imputed) years of schooling and the probability of having a college degree; the latter is defined as having at least two years of university education. From top to bottom we present separate estimates for individuals born 1935-44, 1945-54, and so on. One reason for presenting separate estimates by 10-year birth cohorts is that the characteristics of the compulsory school system has varied over time, an issue to which we return below.

The school starting age effects for all cohorts are consistent with the estimates in the previous section: individuals benefit from starting school at an older age. The effect of starting school one year later varies from 0.04

[25] With the identification approach in Table A2 we estimate the school starting age effect on GPA to 5.90 rather than 5.06. The relative bias is thus in the order of 17 percent.

years of schooling (1965-74) to 0.15 years of schooling (1935-44). The effects on the probability of having a college degree exhibit the analogous pattern, with the effects ranging from 0.5 percentage points for the 1965-74 cohorts to 1.7 points for the 1935-44 cohorts.[26] The effect for those born prior to 1955 is thus substantially greater than the effect for those born after this time point. To a limited extent, this is driven by the evolution of the standard deviation of the schooling distribution (see Table A1). But even if we convert the effects into standard deviation units – 0.05 SD (1935-44) and 0.02 SD (1955-74) – the difference across cohorts remains.

Table 3 also shows that the parameter of interest varies somewhat by gender, but there is no consistent pattern across cohorts.[27]

6.3.1 Why do the effects vary across cohorts and outcomes?

In this section we raise two questions. Why do the effects vary across cohorts? And why are the estimates smaller in Table 3 than in Table 2?

In relative terms, the effects in Table 3 are much higher for the older cohorts than for the younger ones. We interpret these differences as being due to the school system being decisively less selective after the introduction of the comprehensive school. The oldest birth cohorts (1935-44) attended the old and arguably more selective school system. The 1945-54 cohorts were in between the old system and the comprehensive school system, since the reform was gradually introduced;[28] the reform was essentially completed by the time that the cohort born in 1955 attended school (Holmlund, 2007).

To substantiate this conjecture, Figure 6 plots age at school entry effects and the share attending the selective system by birth cohort, for the cohorts born between 1943 and 1968.[29] The sharp fall in the effects coincides with the gradual introduction of the less selective school which is consistent with our interpretation.[30] Initial differences, caused by variation in school starting

[26] Given the trend increases in educational attainment – see Table A1 – the differences across cohorts is more substantial. In relative terms, the effect sizes range from 1.4% (1965-74) to 8.0% (1935-44).

[27] Note also that the estimates do not differ by family background.

[28] The gradual introduction of the comprehensive school causes some problems for us. Children born in January are more likely to face 9 years of comprehensive school while children born in December the previous year is more likely to have 8 years of compulsory school. One worry is that this would "mechanically" raise educational attainment for children born in January. We have made various attempts to correct for this mechanical effect; neither of these corrections had an impact on the estimate.

[29] Thanks to Helena Holmlund for supplying data on the share of the 1943-55 birth cohorts in the selective system. We have extrapolated this information using the fact that the reform was fully introduced starting with the 1961 cohort.

[30] We know of no other source of across-cohort variation which is consistent with this

age, are more likely to persist in school systems where students are tracked early on.

Let us turn to the second question. The school starting age effect is much smaller for the long-run schooling outcomes (0.02 SD for those born in the 1965-74) than for the compulsory school outcomes (0.17 SD). Two explanations for this difference strike us as particularly potent: (i) the estimates in Table 2 are contaminated by the fact that we cannot distinguish between the school starting age effect and the age effect; (ii) achievement gains in compulsory school are not fully translated into increases in educational attainment.

The recent work by Öckert (2008) sheds light on the first potential explanation. Using test scores from the Swedish equivalent of the Armed Forces Qualification Test (typically done around the 18th birthday), he is able to separately identify the effects of school starting age, age at measurement, and years of (upper-secondary) schooling. The age effect is identified from the fact that not everyone does the enlistment test on their 18th birthday; the schooling effect is identified from the fact that schools are not in session during the summer; the school starting age effect is identified in the same way as we do. Thus, there is independent variation in all three components. Notice that since the type/length of the upper-secondary program (if any) is held constant, some of the school starting age and age effects will be netted out.[31] This implies that the magnitudes will not be comparable to those in Table 2; still, Öckert's work sheds light on the relative importance of the two age effects.

Öckert (2008) finds that a year of upper-secondary schooling improves cognitive abilities by 0.15 SD and that starting school a year later raises performance by 0.05 SD; but age at measurement has no effect. Thus, on the basis of this evidence, we are inclined to emphasize the second potential explanation: achievement gains are not fully translated into years of schooling.

6.4 Earnings

The variation in school starting age affects many margins influencing the final earnings outcome. Most obviously, it has an effect on educational attainment. Perhaps as obviously, children who start school one year later enter the labor market one year later conditional on age and schooling. In addition, experience is lower because late school starters have more schooling. Finally,

pattern. The expansion of pre-schools, for instance, primarily affected those born in the 1970s.

[31] If school starting age affects compulsory school performance it will have an effect on the choice of upper-secondary program. Since Öckert (2008) includes upper-secondary program fixed effects this will reduce the potential impact of school starting age.

the analysis in section 6.2 shows that there are achievement differences by age at school start conditional on years of compulsory schooling. Since these differences are not fully translated into years of schooling, there will be unobserved performance differences related to school starting age conditional on years of schooling. If these unobserved performance differences are valued by the market, date of birth is not a valid instrument for schooling in an earnings regression.

In this section we present evidence on the reduced-form effect of the school starting age on earnings (without controlling for experience and schooling). This reduced-form parameter is what we should focus on if we are interested in the benefits and costs of alternative school starting ages.

The total earnings effect will be driven by a labor supply effect and a wage effect. To shed light on the effects along both of these margins, Table 4 presents estimates (by cohort and gender) for earnings in levels (including those with no earnings) and log earnings conditional on having earnings above SEK 100,000. The lower earnings limit, which excludes 32 percent of the individuals born 1935-74, is imposed since we want the estimates to resemble estimates of wage effects.[32] We view the estimates based on earnings for the full sample as the most interesting ones since they capture both labor supply and wage effects and since they are not plagued by bias caused by selection of individuals into the work force.

To facilitate interpretation, we present the estimates in percent (i.e. the levels estimates are reported relative to mean earnings in each cell defined by cohort and gender); the log earnings estimates are only reported for individuals above age 25 – the age when individuals are assumed to have finished their educational careers.

Table 4 conveys two messages: (i) the school starting age effects on earnings are mostly driven by changes in labor supply; and (ii) the estimates by birth cohort have a similar flavor as the estimates for educational attainment. There are positive earnings effects for the oldest cohorts (born prior to 1945). These effects are driven mostly by labor supply. One interpretation is that the increase in educational attainment, caused by starting school at a higher age, induces individuals to retire later. The earnings effects then turn negative for younger cohorts and become (statistically) significant for the cohorts born after 1955. The negative effects for the 1975-84 cohorts are of course driven by the fact that these cohorts have not yet finished their

[32] Since annual earnings also reflect variation in hours of work estimates of, e.g., the return to a year of schooling are higher using earnings data than wage data. Antelius and Björklund (2000) examine how much one must restrict annual earnings from the lower end to get similar estimated returns to schooling using data on annual earnings and wage rates, respectively. The limit we impose corresponds to their findings.

schooling careers. Starting school one year later entails the opportunity cost of entering the labor market one year later. Also, there is a higher probability for individuals born in the beginning of the year to still be enrolled in education (see Table 3).

The general pattern of the effects across cohorts makes sense. The estimates reflect two opposing forces. On the one hand, starting school later raises educational attainment (and potentially other skills). On the other hand, starting school later entails forgone labor market experience. Since the earnings profile is concave, the loss of experience matters a lot early on in the labor market career; the earnings loss becomes less important as the working life proceeds.

The net effect on earnings depends on, *inter alia*, the returns to schooling and experience in the Swedish labor market. To illustrate the importance of the returns to observed characteristics for the overall earnings return to starting school later, we conducted the following simple exercise. We ran a standard Mincer log earnings regression, where we introduced years of schooling linearly and included a quartic in potential experience as well a gender dummy. The estimated return to a year of schooling was 5.8 percent which is low by U.S. standards. The experience profile is flatter than in the U.S. and peaks at 41 years of experience. Now, take an individual in the 1935-44 cohorts. On average, this individual has 10.5 years of schooling; the experience lost by starting school 1 year later is irrelevant in this age range. If the only effect of school starting age is the effect on schooling (0.15 years), we would predict an earnings gain of 0.9 percent (i.e. $0.15 \times 5.8 = 0.9$), which is not far off the the estimate of 0.5 percent reported in the top panel. Suppose instead that we look at an individual born in 1970. On average this individual has 13 years of schooling and, hence, 10 years of potential experience in 2000. The return to a year of experience in this range is 1.5 percent. For this individual we predict an earnings loss of 1.3 percent (i.e. $0.04 \times 5.8 - (1 + 0.04) \times 1.5 = -1.3$), if only the changes in education and experience were the relevant effects of school starting age. This is almost identical to the estimate for the 1965-74 cohorts (-1.4%). These simple calculations are of course based on the assumption that the cross-sectional returns to experience and education are causal and abstract from the fact that age and cohort are collinear in the cross-section; we return to the latter issue below. With these caveats in mind, the calculations convey two messages: first, the pattern of the estimated net earnings returns are sensible; second, in a country with greater returns to education and experience, such as the U.S., we should expect to see greater effects on earnings.[33]

[33] We verified this conjecture using log earnings estimates based on U.S. Census data

6.4.1 To what extent is life-time income affected?

The bottom panel of Table 4 presents earnings estimates for all individuals in our sample. These estimates are interesting as they can be interpreted as the individual net earnings effect, aggregated over the life-cycle, of starting school a year later. As can be seen, the overall effects are negative: taken literally, life-time earnings are reduced by 1-2 percent.

This interpretation has all the problems associated with treating the age profile estimated in the cross-section as an approximation of the life-cycle income profile for a given cohort – age and cohort are collinear in the cross-section. A particular problem here is that Table 3 demonstrates that there are cohort effects: the school starting age effects on educational attainment are substantially higher for the individuals who attended the old, more selective, school system.[34]

Is the collinearity of age and cohort a major problem for us? Figure 7 reports evidence suggesting that the cross-section earnings profile is a reasonable approximation for the cohorts where the schooling effect is the same. The evidence is based on register data from the LINDA-database (see Edin and Fredriksson, 2000). This data set contains longitudinal data for a 3% sample of the Swedish population. The nature of the information in LINDA is the same as the one we used above, but it is available for a much longer time-period (1968-2005). We follow Black et al. (2008) and use the panel dimension of LINDA to separate the age and cohort effects.

Figure 7 contains the earnings effects by age for the cohorts born 1955-65. The message of the figure is not much different than the cross-section estimates in Table 4.[35] There are large earnings losses in the beginning of working-life since starting school a year later implies entering the labor market a year later. These losses persist over the working life, but due to the smaller sample size they are no longer statistically significant after age 30.

Having established that the estimates based on the cross-section are reasonable approximations within a given schooling system, we proceed to do some "counter-factual" simulations. We simulate the life-time earnings ef-

for 2000 (the IPUMS). All education and earnings premia are roughly twice the size in the U.S. relative to Sweden. The earnings losses due to forgone experience early on in the career are accordingly roughly twice as large in the U.S. The subsequent earnings gains are larger by a factor of 2 as well.

[34] Since schooling is more or less completed by age 25 we would argue that the collinearity of age and cohort is a minor issue in Table 3.

[35] According to estimates in Figure 7, the earnings effect for individuals aged ≤ 25 equals -18% (c.f. cohorts born 1975-84 in Table 4), for individuals aged 26-35 (c.f. cohorts born 1965-74) the effect is -3.5%, and for individuals aged 36-45 (c.f. cohorts born 1955-64) it is -1.8%.

fects under two scenarios: one where school entry age has a small impact on years of schooling (0.0476 years) and one where entry age has a big impact (0.1505 years). The basic input for these simulations are regressions where we relate annual earnings (including zero-earners) to years of schooling, a second-order polynomial in potential experience, and gender. Finally, we calculate the discounted sum of the earnings effects over the working career.

The left-hand panel of Table 5 shows that there is never a life-time earnings gain of starting school later, not even when the schooling effects are high and there is no discounting. The left-most column is arguably most relevant for the current Swedish situation. The life-time earnings loss, with no discounting, translates into a loss of 1.3 percent.

How do these estimates translate to a country characterized by greater earnings differentials? To answer this question, the right-hand panel of Table 5 shows analogous counter-factual calculations for the U.S. We estimate annual earnings regressions in a similar fashion as for Sweden using the 2000 U.S. Census 5 % sample (IPUMS, see Ruggles et al., 2004). Then we simulate the earnings effects under two scenarios. To obtain these two scenarios we transformed our Swedish estimates into standard deviation units and translated them into years of schooling using the U.S. schooling distribution. Provided there is no discounting, the higher U.S. return to schooling implies a life-time earnings gain if the school age entry effect is high. Overall, however, starting school a year later appears to entail negative life-time earnings consequences.

7 Concluding remarks

In this paper we have presented a collection of evidence suggesting that starting school at an older age is beneficial for scholastic achievement: compulsory school performance increases by 5 percentile ranks, which is in the mid-range of the estimates for several countries reported by Bedard and Dhuey (2006). Moreover, the effects persist into adulthood. Late school starters go on to have more schooling: starting school a year later raises educational attainment by 0.04–0.15 years.

School starting age has ambiguous effects on earnings. Starting school one year later raises educational attainment but it also has the direct effect of reducing potential experience by one year. In the longer run, the experience lost has no implications and therefore late school starters have a earnings advantage (almost 4%) in comparison to early starters. The loss of experience is much more important for individuals early on in their labor market careers. The effect of starting school one year later is negative (-3%) for individuals aged 26-35. The opportunity cost of school starting age is even more visible for those who are less than 25 years of age. Since the probability of still being in school (not being on the labor market) is higher, the earnings estimate is negative for this age group. Thus, starting school one year later has a positive long-run earnings effect, but the effect is negative in the shorter run. Our analysis suggests that the short-run opportunity cost outweighs the long-run gains: starting school one year later has a negative earnings return (in the order of 1-2%) over the life cycle. The earnings effects observed over the life-cycle are mostly driven by the impact on labor supply: in other words, there are long-run gains because late school starters retire later; and there are short-run losses since individuals enter the labor market later.

What can other countries learn from our analysis using Swedish data? Our evidence suggests that the negative consequences on educational attainment of starting at a young age can be ameliorated if early tracking is abolished. The time span of our data covers time periods where a strict tracking system sorted some students into tracks where future educational opportunities were scant. The impact of school starting age on educational attainment is greater in this system than in the system which featured much less tracking.

The earnings effects of school starting age depend on the labor market returns to productive characteristics. Thus, the earnings effects for each specific age group will be magnified in a country featuring higher returns to characteristics than Sweden: there will be greater losses in the beginning of the working life, but also larger long-run earnings gains. This implies that it should be possible to generalize our results concerning the age-profile of earnings gains and losses to other countries where labor markets operate differently. Indeed, we have presented some "back-of-the-envelope" calculations suggesting that our conclusions regarding the life-time earnings effects are fairly robust to varying the labor market returns.

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Appendix

Table A1 Descriptive statistics

	All	Females	Males
A. Compulsory school outcomes			
	1975-83 Birth cohorts (n = 796,330)		
GPA	50.13	54.25	46.25
	(20.47)	(20.03)	(20.12)
Actual school starting age	7.19	7.18	7.20
	(0.32)	(0.31)	(0.32)
Expected school starting age	7.17	7.17	7.17
	(0.29)	(0.29)	(0.29)
B. Educational attainment and earnings			
	1935-44 Birth cohorts (n = 787,882)		
Years of schooling	10.4561	10.4611	10.4516
	(3.0449)	(2.9599)	(3.1279)
College degree	0.2128	0.2206	0.2051
Earnings	122,618	98,207	146,933
	(157,390)	(110,653)	(189,897)
Log(earnings) | (earnings >100,000)	12.2913	12.1398	12.4201
	(0.4220)	(0.3422)	(0.4416)
	1945-54 Birth cohorts (n = 1,057,221)		
Years of schooling	12.0949	12.2344	11.9609
	(2.9426)	(2.8780)	(2.9975)
College degree	0.2963	0.3202	0.2732
Earnings	203,682	168,613	237,474
	(175,120)	(115,338)	(212,232)
Log(earnings) | (earnings >100,000)	12.3906	12.2400	12.5304
	(0.4014)	(0.3305)	(0.4107)
	1955-64 Birth cohorts (n = 964,414)		
Years of schooling	12.6050	12.8100	12.4085
	(2.4990)	(2.4409)	(2.5380)
College degree	0.3140	0.3466	0.2827
Earnings	199,734	158,958	238,767
	(179,163)	(113,371)	(217,658)
Log(earnings) | (earnings >100,000)	12.3580	12.1896	12.5037
	(0.4024)	(0.3349)	(0.3997)
	1965-74 Birth cohorts (n = 1,037,657)		
Years of schooling	12.9993	13.1930	12.8176
	(2.3397)	(2.3329)	(2.3317)
College degree	0.3418	0.3681	0.3171
Earnings	172,300	131,400	210,721
	(137,117)	(102,690)	(153,342)
Log(earnings) | (earnings >100,000)	12.2927	12.1414	12.4001
	(0.3601)	(0.3266)	(0.3441)
	1975-84 Birth cohorts (n = 944,115)		
Earnings	70,271	59,832	80,053
	(82,694)	(70,124)	(91,877)
	All cohorts (1935-84) (n = 4,791,289)		
Earnings	153,197	123,018	182,121
	(158,410)	(111,098)	(188,816)

Notes: Standard deviations are in parentheses. The GPA is percentile ranked. College degree is defined as having completed at least 2 years of university education. The overall sample size (n) refers to both genders. The log earnings sample is smaller. The probability of having earnings > 100,000 is 68 % for the cohorts born 1935-74. To abstract from trends and seasonality in birth rates, the observations are weighted with the inverse probability of being included in the sample with respect to year and month of birth.

Table A2 IV estimates of school starting age on different (percentile ranked) outcomes in 6th grade, different birth cohorts

Birth cohort:	1948	1953	1967	1972	1977	1982
IQ-test scores:						
Verbal	7.95	8.83	10.54	7.14	9.63	8.60
	(0.91)	(1.39)	(1.27)	(1.40)	(1.49)	(0.94)
Spatial	6.10	5.19	6.82	8.23	7.85	8.12
	(1.14)	(0.89)	(1.10)	(0.73)	(1.75)	(1.82)
Number series	4.29	6.00	8.26	6.25	6.37	9.98
	(0.95)	(1.02)	(1.33)	(0.84)	(1.21)	(1.06)
Achievement test scores:						
Swedish	6.77	8.23	11.66	3.92	.	.
	(0.90)	(1.21)	(1.38)	(1.33)	.	.
English	3.97	6.14	10.78	.	.	.
	(1.20)	(1.51)	(1.57)	.	.	.
Mathematics	6.36	8.50	9.42	8.50	6.37	8.63
	(0.79)	(0.46)	(1.25)	(0.98)	(1.22)	(1.36)
Grades:						
Swedish	5.95	7.46	11.50	.	.	.
	(1.38)	(1.41)	(1.17)	.	.	.
English	3.98	6.08	9.54	.	.	.
	(1.45)	(1.40)	(1.58)	.	.	.
Mathematics	6.77	8.63	10.87	.	.	.
	(1.18)	(0.87)	(1.59)	.	.	.
n	11,903	9,855	9,100	9,329	4,398	8,599

Notes: School starting age is instrumented with expected school starting age. Missing values have been imputed by regressing the outcome measure on all other test scores and grades. The IQ-tests are identical for all birth cohorts whereas the achievement tests are different. Starting with the 1967 birth cohort, the data are collected for all individuals in a given grade. Hence, individuals who have an early or a delayed school start (or who have been retained or advanced a grade) are not born the specified years. Starting with the 1972 birth cohort, no grades were given in 6th grade. Standard errors, reported in parentheses, are adjusted for clustering on year and month of birth.

Table 1 IV estimates of school starting age on parental education, 1965-82 birth cohorts

Model	(1)	(2)	(3)	(4)
School starting age	0.068	0.010	0.024	0.021
	(0.012)	(0.014)	(0.019)	(0.031)
Control variables:				
Year of birth fixed effects	X	X	X	X
Date of birth	X	X	X	
(Date of birth)^2		X	X	
Date of birth × above the break-point	X	X	X	
(Date of birth)^2 × above the break-point		X	X	
Sample:				
± 6 months from break-point	X	X	X	
± 2 weeks from break-point				X
Women	X	X		
Men	X	X	X	X
n	1,724,396	1,724,396	889,188	60,876

Notes: School starting age is instrumented with expected school starting age. Year of birth is defined as running from July to June. Date of birth is measured in months, except in the last two columns where it is measured in days. The observations are weighted with the inverse probability of being included in the sample with respect to year and month (day) of birth. Standard errors, reported in parentheses, are adjusted for clustering on year and month (day) of birth.

Table 2 OLS and IV estimates of school starting age on 9th grade percentile ranked GPA, 1975-83 birth cohorts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Model	OLS	Reduced Forms		IV	IV	IV	IV
Dependent variable	GPA	School starting age	GPA	GPA	GPA	GPA	GPA
School starting age	-6.40	.	.	5.06	4.99	5.17	5.33
	(0.94)	.	.	(0.21)	(0.22)	(0.22)	(0.32)
Expected school starting age	.	0.903	4.57	.	.	.	.
	.	(0.006)	(0.19)	.	.	.	.
Control variables:							
Year of birth fixed effects	X	X	X	X	X	X	X
Date of birth controls	X	X	X	X	X	X	
Parental education					X		
Sample:							
± 6 months from break-point	X	X	X	X	X	X	
± 2 weeks from break-point							X
Women	X	X	X	X	X		
Men	X	X	X	X	X	X	X
n	796,330	796,330	796,330	796,330	710,678	409,867	27,641

Notes: Year of birth is defined as running from July to June. The date of birth controls include a quadratic in date of birth which is interacted with a dummy for being above the break-point. Date of birth is measured in months, except in the last column where it is measured in days. The observations are weighted with the inverse probability of being included in the sample with respect to year and month (day) of birth. Standard errors, in parentheses, are adjusted for clustering on year and month (day) of birth. The model controlling for parental (mother's and father's) education is restricted to the 1975-82 period.

Table 3 IV estimates of school starting age on educational attainment in 2000

	All	Females	Males
	1935-44 Birth cohorts (n = 867,627)		
Years of schooling	0.1505	0.1695	0.1308
	(0.0277)	(0.0316)	(0.0355)
P(College)	0.0170	0.0183	0.0155
	(0.0034)	(0.0044)	(0.0042)
	1945-54 Birth cohorts (n = 1,037,462)		
Years of schooling	0.1336	0.1718[b]	0.0966[b]
	(0.0197)	(0.0188)	(0.0295)
P(College)	0.0166	0.0238[b]	0.0096[b]
	(0.0031)	(0.0035)	(0.0047)
	1955-64 Birth cohorts (n = 981,598)		
Years of schooling	0.0476	0.0699	0.0253
	(0.0137)	(0.0218)	(0.0253)
P(College)	0.0124	0.0112	0.0132
	(0.0027)	(0.0035)	(0.0046)
	1965-74 Birth cohorts (n = 1,025,285)		
Years of schooling	0.0422	0.0272	0.0561
	(0.0125)	(0.0175)	(0.0197)
P(College)	0.0049	0.0006	0.0091
	(0.0027)	(0.0035)	(0.0045)

Notes: School starting age is instrumented with expected school starting age. All models also include year of birth (July/June) dummy variables, a quadratic in month of birth which is also interacted with a dummy for being above the break-point. The probability of having attended college is estimated using a linear probability model. Sample size (n) refers to both genders. The observations are weighted with the inverse probability of being included in the sample with respect to year and month of birth. Standard errors, reported in parentheses, are adjusted for clustering on year and month of birth. a/b/c=the estimates differ significantly by gender at the 1/5/10 percent level.

Table 4 IV estimates of school starting age on earnings in 2000

Outcome (units)	All	Females	Males
	1935-44 Birth cohorts (n = 867,627)		
Earnings (percent)	3.75	5.77[b]	2.62[b]
	(0.89)	(1.11)	(1.18)
Log(earnings)|(earnings >100,000) (percent)	0.49	0.81	0.92
	(0.89)	(0.66)	(1.16)
	1945-54 Birth cohorts (n = 1,037,462)		
Earnings (percent)	-0.32	0.74[b]	-1.11[b]
	(0.56)	(0.52)	(0.84)
Log(earnings)|(earnings >100,000) (percent)	-0.24	0.06	-0.47
	(0.28)	(0.32)	(0.40)
	1955-64 Birth cohorts (n = 981,598)		
Earnings (percent)	-1.02	-0.81	-1.08
	(0.37)	(0.59)	(0.58)
Log(earnings)|(earnings >100,000) (percent)	-0.45	-0.36	-0.58
	(0.23)	(0.32)	(0.27)
	1965-74 Birth cohorts (n = 1,025,285)		
Earnings (percent)	-3.19	-3.08	-3.13
	(0.53)	(0.62)	(0.63)
Log(earnings)|(earnings >100,000) (percent)	-1.41	-0.89[b]	-1.85[b]
	(0.29)	(0.31)	(0.36)
	1975-84 Birth cohorts (n = 844,944)		
Earnings (percent)	-15.41	-15.01	-15.88
	(2.70)	(3.01)	(2.74)
	All cohorts (1935-84) (n = 4,756,916)		
Earnings (percent)	-1.91	-1.22	-2.30
	(0.50)	(0.59)	(0.54)

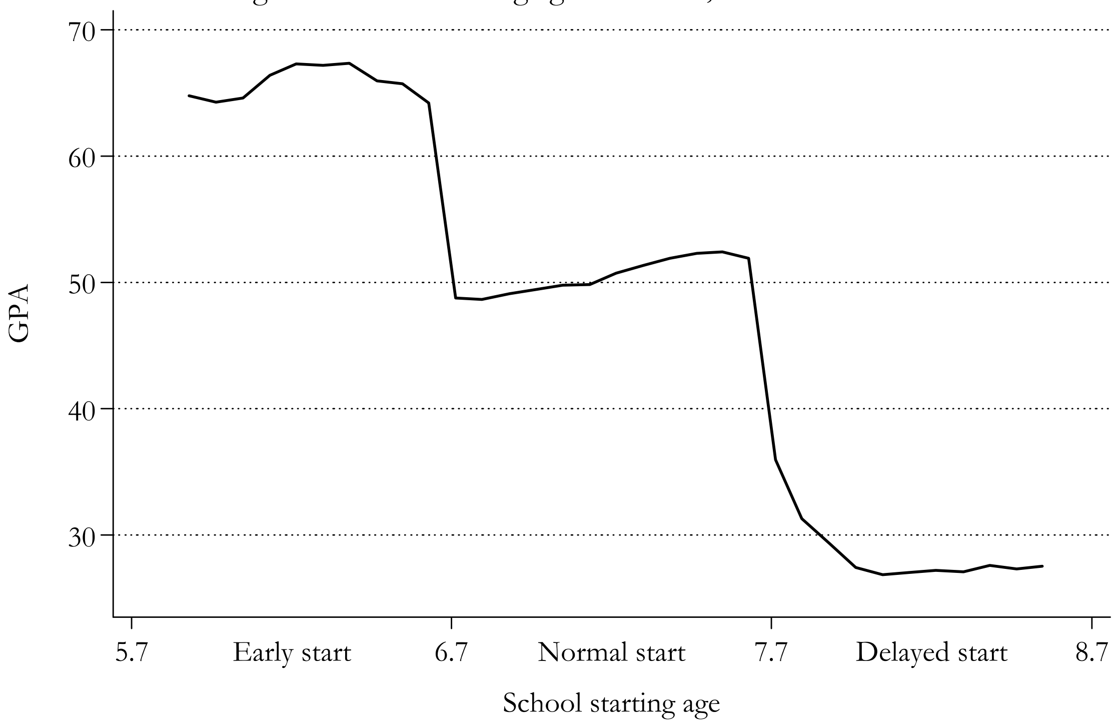
Notes: School starting age is instrumented with expected school starting age. All models also include year of birth (July/June) fixed effects, a quadratic in month of birth which is also interacted with a dummy for being above the break-point. The observations are weighted with the inverse probability of being included in the sample with respect to year and month of birth. The earnings estimates have been converted to percent by dividing the estimate by mean earnings in each cell; mean earnings are reported in Table A1. Sample size (n) refers to the earnings-sample including both genders. Standard errors, reported in parentheses, are adjusted for clustering on year and month of birth. a/b/c=the estimates differ significantly by gender at the 1/5/10 percent level.

Table 5 Present values of simulated earnings effects of the school starting age

	Sweden		U.S.	
Effect on schooling:	Low	High	Low	High
Discount rate				
0.00	-$11,099	-$842	-$10,375	$5,078
0.01	-$21,700	-$13,537	-$26,086	-$14,816
0.03	-$30,806	-$25,199	-$40,077	-$33,756
0.05	-$32,812	-$28,565	-$43,668	-$39,890

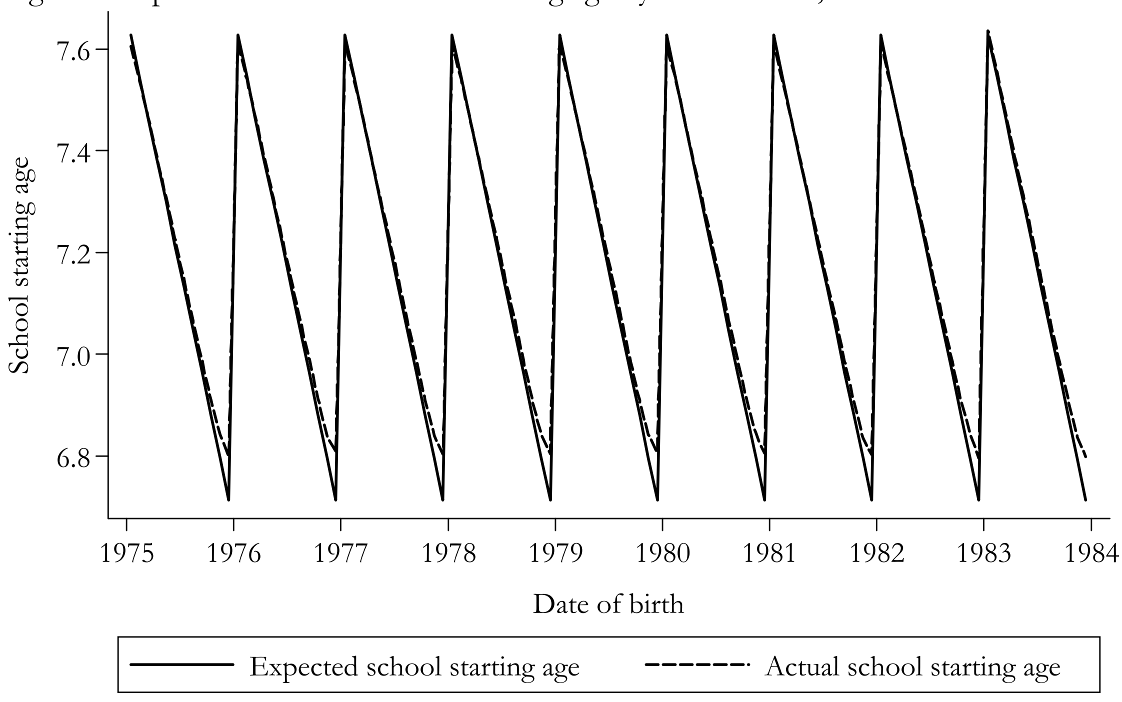
Note: The table shows the effect of age at school start on years of schooling and potential experience in Sweden, evaluated at the earnings premiums for schooling and potential experience over the life-cycle in Sweden and in the U.S., respectively. The earnings premiums have been estimated using data from Statistics Sweden and the 2000 U.S. Census 5 % sample (IPUMS). The model for annual earnings (including zero-earners) includes an intercept, schooling, potential experience, potential experience squared, gender and race (only for the U.S.). The earnings penalty from entering the labor market one year later, calculated as the earnings at one year of potential work experience, has been subtracted off the life-time earnings effects. An individual is assumed to stay on the labor market for 50 years, and the earnings effects over the life-cycle has been discounted back to the time of labor market entrance. No productivity growth is assumed. The schooling and (potential experience) effect in Sweden (0.0476 and 0.1505 years, respectively) has been calculated in standard deviation units, and converted to the corresponding years of schooling in the U.S. schooling distribution (0.0562 and 0.1552 years, respectively). The earnings in SEK have been converted to USD using the SEK/USD exchange rate for year 2000. All numbers are in 2000 USD.

Figure 1 School starting age and GPA, 1975-83 birth cohorts



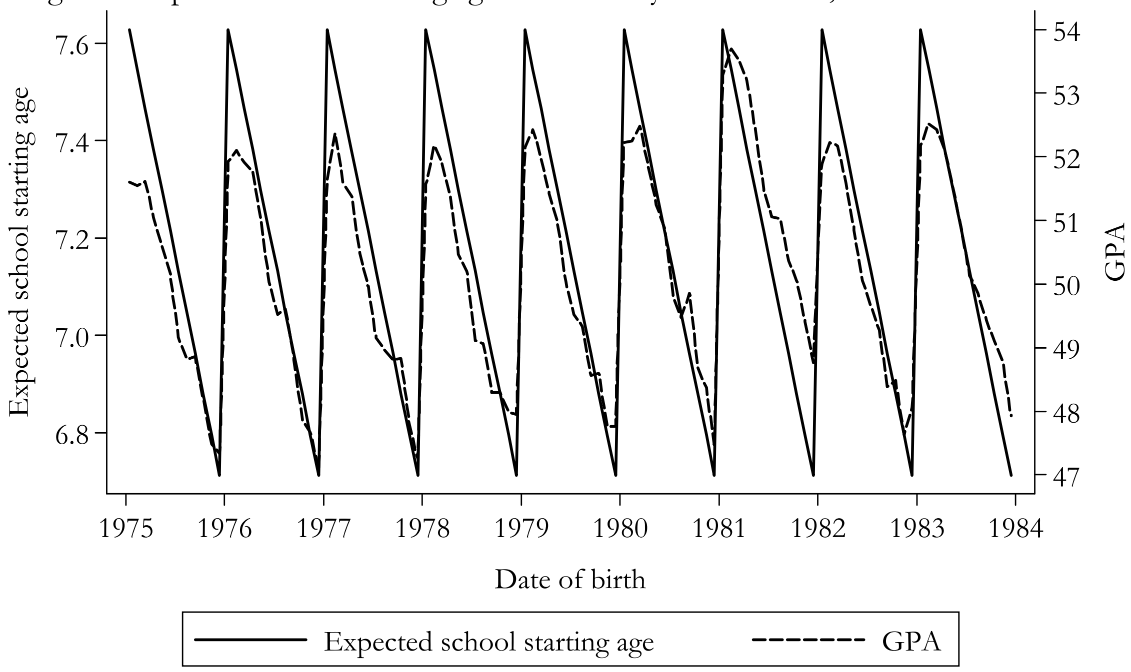
Note: Ninth grade GPA is in percentile ranks.

Figure 2 Expected and actual school starting age by date of birth, 1975-83 birth cohorts



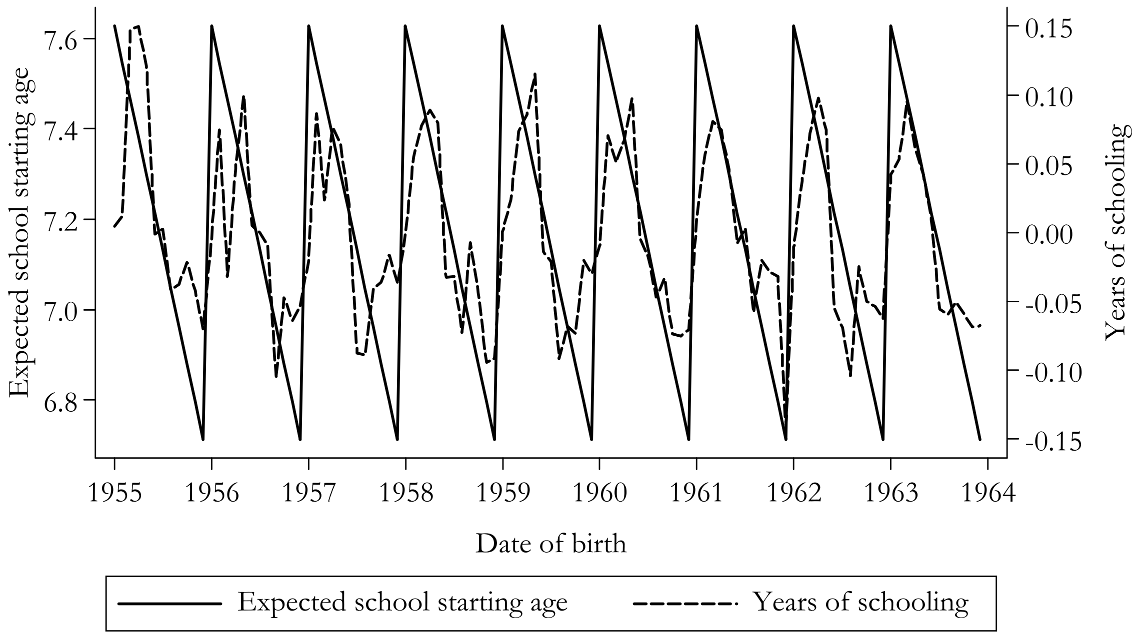
Note: Expected school starting age is the age when an individual is expected to start school according to date of birth and the school cut-off date. Date of birth is measured in months.

Figure 3 Expected school starting age and GPA by date of birth, 1975-83 birth cohorts



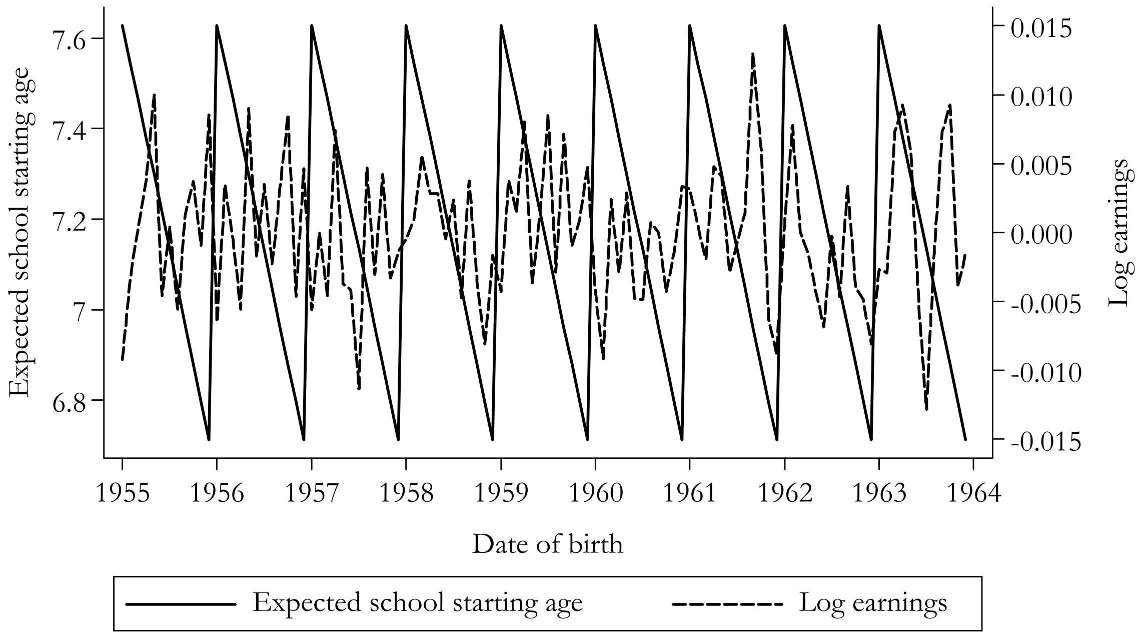
Note: Expected school starting age is the age when an individual is expected to start school according to date of birth and the school cut-off date. Date of birth is measured in months. Ninth grade GPA is in percentile ranks.

Figure 4 Expected school starting age and years of schooling by date of birth, 1955-64 birth cohorts



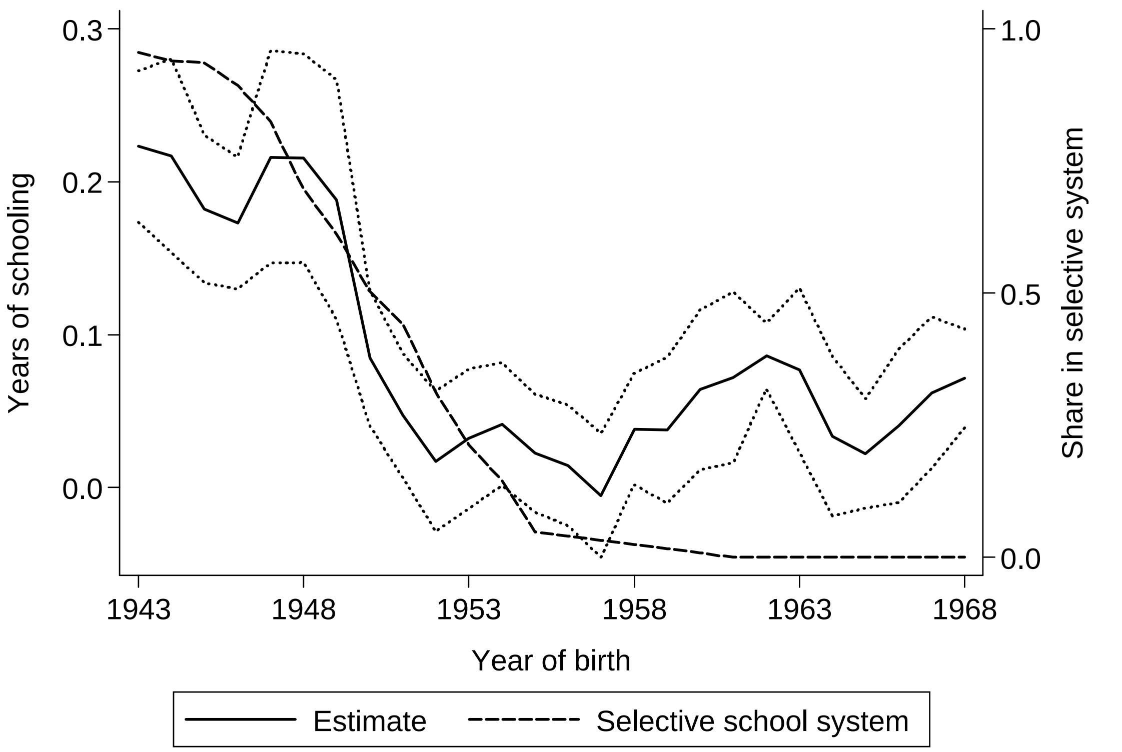
Note: Expected school starting age is the age when an individual is expected to start school according to date of birth and the school cut-off date. The years of schooling variable has been detrended by subtracting off the average years of schooling for each shifted year of birth cohort. Date of birth is measured in months.

Figure 5 Expected school starting age and log earnings by date of birth, 1955-64 birth cohorts



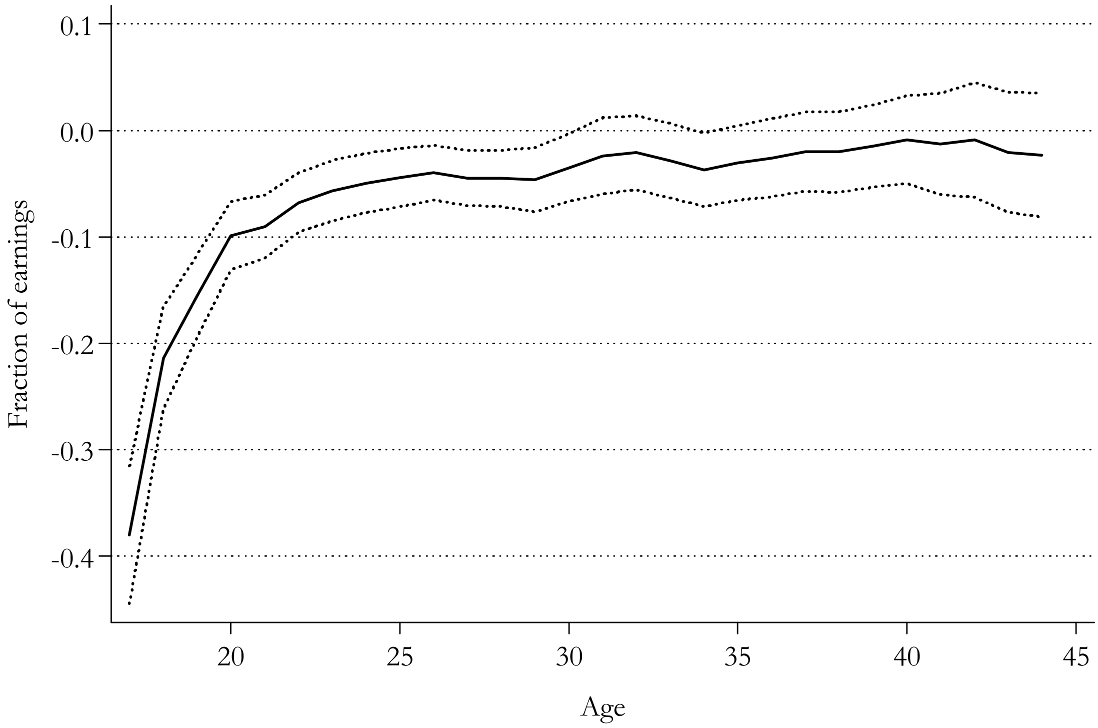
Note: Expected school starting age is the age when an individual is expected to start school according to date of birth and the school cut-off date. The median log earnings has been detrended by subtracting off the median log earnings for each shifted year of birth cohort. Date of birth is measured in months.

Figure 6 School entry age effects and the share in the selective school system by birth cohort



Note: The dotted lines show the point estimates ± 1.96 standard errors.

Figure 7 Earnings effects over the life-cycle, 1955-65 birth cohorts



Note: The estimates have been smoothed using one year moving averages. The dotted lines show the point estimates ± 1.96 standard errors.