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LABOUR MARKET POLICY
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**Is early learning really more
productive?
The effect of school starting age
on school and labor market
performance**

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WORKING PAPER 2006:12

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Is early learning really more productive? The effect of school starting age on school and labor market performance*

Peter Fredriksson[†] Björn Öckert[‡]

3 November, 2006

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. The long-run earnings effects are positive but small. 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. Exploiting within-school variation in peer age composition, we find that the school starting age effect primarily is due to absolute maturity rather than to the relative age in the class.

KEYWORDS: School starting age, School performance, Labor market outcomes, Regression-discontinuity design

JEL-CLASSIFICATION: J24, I21

*We thank Olof Åslund, Torsten Persson, Roope Uusitalo, seminar participants at IUI, SOFI, IFAU, FIEF, NTNU, IIES, the Universities of Copenhagen, Gothenburg, Helsinki, Montreal, and Växjö, participants at EALE/SOLE 2005, the CEPR conference on "Education and Inequality", and the first EEEPE workshop for valuable comments.

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1 Introduction

Evidence from small-scale early childhood education experiments suggests substantial gains in both scholastic achievement and later outcomes for the participating children (see Barnett, 1992; Currie, 2001 for two surveys). The relative magnitudes of the returns involved have led some authors to propose a reallocation of public spending from older to younger persons (for example Carneiro and Heckman, 2003). However, the existing preschool evidence reflects the net effects of multidimensional programs. These early interventions involve (i) more schooling, (ii) formal learning at a young age and (iii) targeted training towards disadvantaged children. Thus, the available preschool evidence is not very informative about the virtues of early learning for the average individual.

In this paper we attempt to isolate the effect of early learning. In particular we ask the question: How does the school entry age affect school performance, educational attainment and long-run labor market outcomes? The empirical strategy is to exploit exogenous variation in the school starting age due to month of birth and the school cut-off date (the 1st of January). We thus compare outcomes for children born on either side of the school cut-off (December and January born kids) who are expected to start school almost a full year apart. The expected age at school entry – implied by the school cut-off and birth month – is used as an instrument for the actual school starting age.

We apply this strategy 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) we have information on school performance at the end of compulsory school.

Swedish data are particularly apt for examining the issue addressed in this paper. The main 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 Swedish system. 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. A particularly intriguing feature of the data is that we can trace out the earnings effects of the school start-

ing age for individuals at different points in the life-cycle. Thus, we can estimate the long-run earnings effects of variations in the school starting age; moreover, if the effects persist into adulthood we can provide a sense of when any gains and losses accrue over the life-cycle. The analysis of long-run earnings and education outcomes is the main value-added of this paper. In fact, we have seen no previous analysis of the earnings effects of variations in the school entry age.¹ An analysis of this issue using US data is not possible since the effects of the school entry age is contaminated by the state school leaving age legislations (Angrist and Krueger, 1991).²

We also address some other problems encountered in the literature. Studies using data on performance in compulsory school face a severe identification problem. Since the age at test in compulsory school is an exact linear function of age at school start and time spent in school, these studies generally fail to separate the effect of age at school start from any direct age effect. This problem is typically not well-recognized in the literature.³ However, after the completion of compulsory school, it is possible to separately identify the direct effect of age and the school starting age. Thus, using longer-run information on earnings and educational attainment we can credibly estimate the effect of the school starting age.

Another problem in the literature concerns the interpretation of the estimated effects. Are achievement differences by age at school start due to differences in absolute maturity or relative maturity? Again, one can think of this as a fundamental identification problem: Children who start early are also among the youngest in class, while children who start late are among the oldest in class. From a policy perspective it is obviously paramount to disentangle the two. If the start of formal class room training is pushed one year forward, such a change will not alter the age distribution

¹The paper by Plug (2001) has the potential of doing this. However, he uses season of birth in an instrumental variables strategy for estimating the returns to schooling. It is not clear that the exclusions restrictions necessary for this approach are fulfilled.

²American compulsory schooling laws typically require students to remain in school until their 16th or 17th birthday. Individuals who enter school at an older age reach the legal dropout age at an earlier point in their educational careers than students who enter school later. Thus, the possible effects of school starting age are contaminated by the effects of school leaving age legislations. Estimates of, e.g., quarter of birth on education and earnings using American data are hard to interpret as the (reduced form) effects of school starting age (e.g. Angrist and Krueger, 1991; Bound et al., 1995; Mayer and Knutson, 1999).

³Hansen et al. (2004) is an exception.

within class. In the extreme case when being the oldest/youngest in class is all that matters, the policy change will not affect outcomes. Thus, we attempt to separate absolute maturity from relative maturity using idiosyncratic variations in the age composition in schools over time.

What are the possible effects of an earlier school start on skill acquisition? The evidence from developmental behavioral genetics and molecular genetics may provide a case for an early school start. This evidence suggest that some age ranges of child development (critical periods) are especially sensitive to the impact of specific types of experiences (Shonkoff and Phillips, 2000). An extremist interpretation of these results is that many important abilities are fairly set by the age of six, and, hence, it is crucial that children start school early. Child developmentalists, on the other hand, have traditionally stressed the importance of school readiness: young children might not be mature enough to learn complicated things in a school environment. Developmental psychologists typically focus on relative maturity. The oldest in class tend to be encouraged more – in particular early on when age is a strong predictor of performance – and are therefore pushed to perform even better. On the other hand, a theory based on peer quality may suggest that younger children benefit from being surrounded by older and more able peers. All in all, the longer run effects on skill acquisition is an open question.

The earnings effects conditional on skills are more straightforward. Basic human capital theory suggests that children should start formal learning as soon as possible. The reasons are threefold. First, individuals who start school earlier also enter the labor market earlier, and can collect the returns from their human capital investments over a longer time horizon. Second, the opportunity cost for going to school is lower if they enter the labor market earlier, since labor market productivity can be assumed to increase with age. Third, early school starters have more work experience conditional on age.

Before offering a preview of the results we also want to make clear that we are examining the effects of early learning in a given school environment. In other words we are considering alternative school starting ages for a given curriculum. This is conceptually distinct from a policy changing the age of school entry, because such a policy change will introduce early learning and, most likely, a new curriculum. The effect of such a reform will thus reflect the combined effect of early learning and a change in the curriculum.

The results show that children who start school when they are older do better in all subjects in school. In particular, children with weaker educational backgrounds have more to win from starting school later. Individuals who are older when they start school also go on to have more schooling and are more likely to graduate from college than other individuals. Our analysis also suggests that the school starting age effect is mainly driven by absolute – rather than relative – maturity. The earnings effects of age at 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 small). The net effect over the entire life-cycle is negative. Taken at face value, this suggests that the opportunity cost of starting late outweighs the earnings gains accruing later on in life.

The outline of this paper is as follows. The next section gives a brief account of the previous literature. Section 3 provides some basic facts about compulsory schooling in Sweden. Section 4 describes the data and reports some descriptive statistics. Section 5 provides a graphical analysis. This is followed by empirical considerations in section 6 and section 7 contains the results. Section 8 concludes.

2 Previous literature

The empirical educational literature on the relation between age at school entry and (early) academic performance is extensive (see Stipek, 2002 for a survey). One group of studies compares the outcomes of children who have delayed entry with children who entered school when they were eligible. They typically find that children who have been held back perform less well than their same-age peers (Byrd et al., 1997; Graue and DiPerna, 2000; May et al., 1995). These results are most likely misleading, however, since the suspicion of low academic performance is likely to be a source of delayed entry. Another group of studies compares the outcomes of children who entered school when they were eligible, but who differ in birth dates within the year. The evidence from this literature suggests that the youngest children in a class score slightly below their older peers, but that the differences tend to be small and transitory (Langer et al., 1984; Jones and Mandeville, 1990; Mayer and Knutson, 1999; Cahan and Cohen, 1989; Cahan and Davis, 1987). However, since low-performing children born just before the school cut-off date (youngest in class) probably are more likely

to be held back than low-performing children born just after the cut-off date (oldest in class), these studies tend to underestimate the effect of school starting age.⁴ The common practice of retaining weaker children in many countries, might also explain the declining school starting age effects with grade level (Corman, 2003).

Economists have typically shown less interest in the effects of age at school start on performance. The first paper in this vein is the one by Angrist and Krueger (1992). They use quarter of birth as an instrument for the school starting age. Their idea is that the school entry age affects outcomes because American compulsory schooling laws typically require students to remain in school until their 16th or 17th birthday. Individuals who enter school at an older age thus reach the legal dropout age at an earlier point than students who enter school later. They find that children who start school at an older age have less schooling than children who start school when they are younger. But this should not be interpreted as the direct effect of the school starting age since this effect is contaminated by the school leaving age legislations. For the same reason, estimates of quarter of birth on education and earnings using American data are hard to interpret as the (reduced form) effects of school starting age (e.g. Angrist and Krueger, 1991; Bound et al., 1995; Mayer and Knutson, 1999).

There has been a recent surge in the economics literature on issues related to the age at school start.⁵ Most of these studies look only at short-run effects, e.g., Strøm (2003), Bedard and Dhuey (2005), and Datar (2005). Strøm (2003) fails to distinguish between the effect of the school starting age and the direct effect of age. Datar (2005) recognizes this identification problem, but the separate identification of these two effects relies on functional form assumptions. Bedard and Dhuey (2005) are presumably aware of the problem but in their main analysis – using a cross-section of countries in TIMSS – they cannot separate the two effects.⁶ The stud-

⁴For this reason Cahan and Cohen (1989) exclude the months preceding and following the school cut-off date.

⁵Some of these new studies are similar to Angrist and Krueger (1992) in the sense that they abstract from any direct effect of the school starting age and use timing of birth to identify variations in compulsory schooling (e.g., Cascio and Lewis, 2004, Del Mondo and Galindo-Rueda, 2004, and Leuven et al., 2004).

⁶In fairness it should be said that they also study the longer term effects of age at school entry using data for England and New Zealand. The data are at the aggregate cohort level, however; hence it is not possible to separate the direct effect of age from age at school entry.

ies focusing on age at school entry all suggest that older kids do better in school. As noted above, however, it is generally not clear whether these effects are to the school starting age or age per se.

There are only a few recent studies looking at the effects of the school entry age on longer run educational attainment. Fertig and Kluge (2005) examine this issue using individual data covering 18-29 year olds in East and West Germany. Plug (2001) conducts a similar analysis using data for the Netherlands. Taken at face value, these two studies suggest that children who are born just after the cut-off have more schooling than those born before the school cut-off. There are a couple of potential problems with these studies. First, they use the entire season of birth range to identify the school starting age effects. In other words they are not using the (sharp) discontinuity implied by the school cut-off. It is thus possible that the results are 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. Plug (2001) attempts to "solve" the problem by controlling for grade retention and advancement. Since, retention and advancement are likely outcomes of the school starting age this is not a satisfactory procedure.

Plug (2001) goes on to use 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 the 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.

3 Compulsory schooling in Sweden

Since the birth cohorts in our data span some 50 years it is necessary to give a brief account of historical development of the lower levels of schooling in Sweden.⁷ Table 1 shows that compulsory schools were introduced as early

⁷We base this presentation on Gunnarsson et al. (1999).

as 1842. To start off with, the school starting age was varying between 5 and 9 years-of-age and the minimum school requirement was 4 years of education. However, compulsory education was far from “compulsory”. It was not until the late 1920s, that the vast majority (92 percent) fulfilled the minimum school requirement of six years.

The school system during the first half of the 1900s was rather selective. Moreover, 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 attending the lower tracks essentially did not have the opportunity to pursue further education.

However, the system was changed by a parliamentary decision in 1950. A comprehensive school, with a nationally determined curriculum that abolished the strict tracking system, was introduced gradually across the country. 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, 2006). The comprehensive school was fully implemented all across the country in 1968.

Table 1 Development of Swedish compulsory education

Year(s)	Description	Starting age	Length	Leaving age
1842	Introduction of basic compulsory school	5-9	4	9-13
1882	Extension of basic compulsory school	7	6	13
1936	Extension of basic compulsory schooling	7	7	14
1945-52	Gradual extension of basic compulsory schooling	7	8	15
1950-67	Gradual introduction of comprehensive compulsory school	7	9	16
1968	Comprehensive compulsory school fully introduced	7	9	16

Since 1968 the basic structure of compulsory schools has been intact. After compulsory school, students may go on to upper-secondary school. Upper-secondary school is voluntary and offers several programs, ranging from vocational training to programs that prepare for studies at the university level. Today, the choice of an academic or a vocational program is the crucial stage of selection. A minor share of those completing

vocational training go on to university education.

Since 1970 there have been a fair amount of changes at the pre-primary level, however. 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. Also, during the 1970s there was 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 5 years-of-age.

Our analysis covers roughly 50 birth cohorts. It is reasonable to expect that the contents of “treatment” (starting school at a younger age) and the “alternative” (starting at an older age) is changing over time. Changes in pedagogical techniques and the selectiveness of the entire school system influence the treatment. The alternative is also changing for successive birth cohorts. The alternative usually meant staying at home with one parent for the cohorts born in the 1940s, 1950s, and most of the 1960s. But for cohorts born in the 1970s it has increasingly become attending child care. We do not have information on all these changes. But we can examine if the estimates varies over birth cohorts.

4 Data and descriptive statistics

The data have mainly been collected 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. All in all, there are around 4.8 million observations.⁸ Information on year and month of birth originates from birth records, and should in principle not suffer from measurement errors. Individuals born 1941-1982 have been linked to their biological parents, and information on parental highest education have been obtained from the censuses. The coverage of the parental information increases with year of birth and is 80 per cent or higher for individuals born after 1960.

The school starting age is unfortunately not reported in Swedish administrative records. We will therefore use different data sources to construct such a measure. For individuals born 1972-84 there is information on the year of compulsory school completion (in ninth grade). Since grade

⁸We exclude all 900,000 immigrants (this may include native-born who have spent at least a year abroad), since they lack reliable information on date of birth, school starting age and years of schooling. Further, individuals who have deceased or emigrated by the year of 2000 are not covered by the data.

retention or advancement rarely is practiced in Swedish schools, potential mis-classification is a very minor issue.⁹ Therefore, we calculate the school starting age for these cohorts as:

$$A_i^S = (YOC_i - YOB_i) - 9 + \left(\frac{8 - MOB_i}{12} \right), \quad (1)$$

where A_i^S is school starting age, YOC_i is the year of compulsory school completion, YOB_i is the year of birth (9 is the number of years in compulsory school), and MOB_i is the month of birth (ranging from 1 to 12, and 8 reflects the fact that schools start in August). Thus, A_i^S is measured in yearly units but varies by month of birth.¹⁰

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. The first two surveys cover all individuals born in Sweden on the 5th, 15th or 25th of any month in 1948 or 1953, respectively. We also use data for cohorts born in 1967 and 1972; these data have been sampled using a two step procedure, where municipalities first have been selected from different strata and then a number of compulsory school classes in grade 3 have been randomly sampled from these strata. These data include intelligence test scores and achievement test scores in sixth grade. For the 1948 cohort there is information on the year the pupils entered grade one and in the 1953 cohort there is data on the grade the individuals attended in the 1965/66 school year. For the 1967 and 1972 cohorts we have information on the age in grade 3. We use this information to calculate age at school entry (using equation (1) with fewer years of compulsory schooling). The school starting age in the UGU samples is then regressed on month of birth to predict the school starting age for the 1935-71 cohorts in the administrative data.

Final grades from compulsory school is collected by Statistics Sweden for the 1988-2000 period. The normal graduation age is 16 years, so this information is in principle available for the 1972-1984 cohorts. However,

⁹Corman (2003) and Eide and Sholwater (2001) show that grade retention and advancement is strongly related to season of birth in the U.S. In Sweden, however, children rarely repeat or skip grades. Data for the 1960s suggest that half of those finishing late (only 3.6 % of the population) were retended during compulsory school.

¹⁰Rather than calculating the school starting age from the school leaving age for individuals born 1972-84, we can use the UGU data for cohorts born in 1977 and 1982, and apply the procedure described below. Notice that this has no implications for our results.

due to delayed entry for some individuals, we have complete compulsory school grades for the 1975-83 cohorts only.¹¹ The register contains information on grades for all subjects. In most of the period studied, the grading system was relative. The grades ranged from 1 to 5 (with 5 being the highest grade) and were set such that the national average was 3 (with a unit standard deviation). With the implementation of a new curriculum – which affected those graduating in 1998 – the relative grading system was replaced with an absolute (or goal-oriented) system. There are four levels of grades in the new system: fail, pass, pass with distinction and pass with special distinction. To make these different grading systems comparable, we attach a percentile rank to each grade for all subjects.¹²

To guide teachers in their grading, national achievement test have been undertaken in both the old and the new grading system. These results are, however, only advisory, and teachers might deviate from them. As long as teachers do not systematically overcompensate or punish some groups, this should not be a problem. To assess the information value contained in the grades we use the UGU data, which, as already noted, contain information on, *inter alia*, achievement test scores.

Statistics Sweden collects information on the highest level of education completed for all individuals; our educational attainment data pertain to 2000. We convert the highest educational level attained into years of education using the Swedish Level of Living Survey (SLLS) conducted in 2000. The SLLS contains information on educational attainment and labor market success for a representative sample of individuals aged 18 to 75. What is convenient for our purposes is that it includes both register information on highest level of education and survey information on time spent in school. We predict years of education for the entire population using the estimates from a regression of years of education in the SLLS on the educational levels according to the register information. To allow years of schooling associated with each level of education to vary smoothly across cohorts, we estimate separate regressions for each birth cohort (\pm five cohorts).

¹¹To get complete data for a full birth cohort, compulsory school grades for individuals who have an early (delayed) school start or who have been advanced (retained), are collected from the preceding (subsequent) school years.

¹²Hence, 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.

Table 2 Month of birth and timing of school start (percentage points), 1975-1983 birth cohorts

	January	February	March	April	May	June
Earlier school start	2.58	1.28	0.81	0.61	0.40	0.32
Normal school start	96.64	97.87	98.22	98.31	98.24	98.06
Delayed school start	0.79	0.86	0.97	1.08	1.36	1.63
n	66,361	65,816	77,092	75,932	72,516	66,912
	July	August	September	October	November	December
Earlier school start	0.25	0.15	0.14	0.10	0.06	0.02
Normal school start	97.94	97.72	97.38	96.88	95.41	91.14
Delayed school start	1.81	2.13	2.49	3.02	4.52	8.84
n	67,394	65,036	64,987	61,507	56,215	56,560

Note: Normal school starting age is between 6.8 and 7.7 years depending on month of birth.

Our earnings measure is based on register information. It is measured as of 2000 and defined as the sum of annual gross wage earnings and compensation during temporary work absence (basically due to illness or parental leave) in SEK.¹³

The average school starting age in our sample is 7.2 years.¹⁴ Children typically start school between the ages 6.8 and 7.7, but some children start school already at the age of five and others not until they turn ten years old. Table 2 shows the share of children with an early, normal or delayed school start by month of birth for the 1975-83 birth cohorts. Even though parents and school administrators can affect the timing of the children's school start to some extent, roughly 97 percent of all children start school the year they turn seven. The probability of a delayed or an early school start varies with month of birth. About 2.6 percent of the children born in January starts school one year early. The probability of an early school start falls monotonically for later months of birth, and is negligible for children born in December. The opposite pattern is found for delayed school start, where as much as 8.8 percent of children born in December begins school one year late. The share of children with a delayed school start falls monotonically for children born earlier in the year.

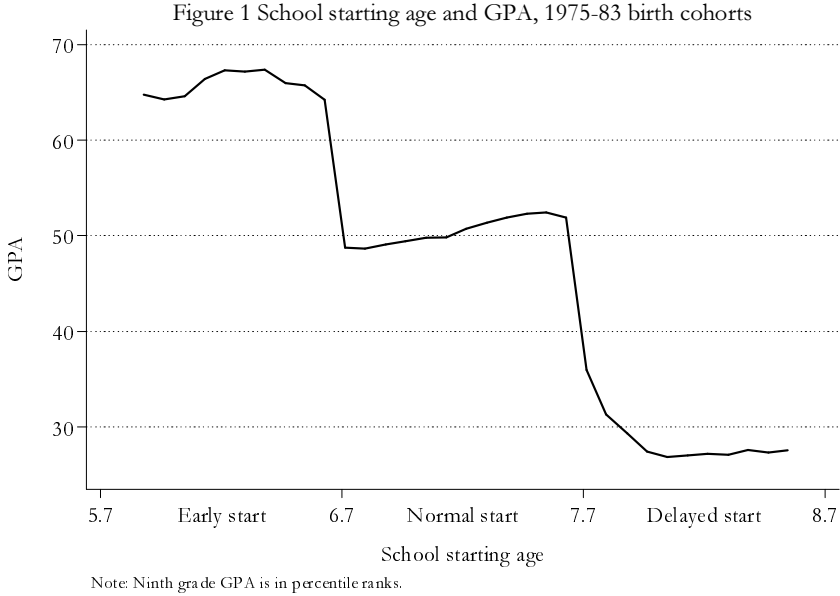
5 Graphical analysis

In Sweden, only 3 percent of the children start school earlier or later than the year when they turn seven. Such a small share might seem negligible, but might still be enough to produce a misleading correlation between school starting age and later performance. Figure 1 shows the relation between age at school start and the ninth grade grade point average (GPA) for the 1975-83 birth cohorts. The performance in compulsory school by school starting age exhibit a stepwise negative relationship. Taken at face value, this would suggest that children who start school earlier do better than children who start school later. Note, however, that the downward trend, is driven by sharp drops in the performance just at the minimum and maximum normal school starting age (6.8 and 7.7 years, respectively).

¹³The information comes from the tax assessment as well as the employers' and authorities' statements of income and allowances.

¹⁴Tables A1-A3 in the Appendix report descriptive statistics for the 1935-84 birth cohorts.

Within each segment of early, normal and delayed school starters, there seems to be no or a positive relation – in particular for the group of normal school starters – between the age at school entry and ninth grade GPA. This highly irregular relation between age at school start and compulsory school performance is, thus, likely to be due to the non-random selection of children with early or delayed school start and cannot be given a causal interpretation.



The empirical strategy in this paper is to exploit the exogenous variation in school starting age caused by the children’s month of birth and the school cut-off date. The school starting age legislation induces the following expected school starting age function:

$$f_i^{AS} = 7.7 - \frac{MOB_i - 1}{12}, \quad (2)$$

where f_i^{AS} is the age child i is expected to start school. Children born in January ($MOB_i = 1$) typically start school at 7.7 years. For individuals born later in the year, the expected school starting age function falls monotonically to 6.8 years for children born in December. The function then makes a sharp jump back to 7.7 years at January 1st, generating a saw-teeth shaped pattern for age at school entry by month of birth.

Earlier studies of the effect of school starting age typically focus on the performance in compulsory school. Usually, the data is collected for all pupils born a specific year who are in the same grade. Although we use data on early academic achievement for all individuals in a birth cohort (and not only those who have a normal school start), our study of early outcomes is not an exception. There is, however, an exact linear dependence between age, age at school start and time spent in school for children still enrolled in compulsory school:

$$A_{it} = A_i^S + S_{it}^C, \quad (3)$$

where A_{it} is age at t (the time of data collection), A_i^S is school starting age and S_{it}^C is compulsory schooling at t . Children who start school when they are somewhat older, are, thus, somewhat older also when they do the test (or get the grades) in a given grade. Again, using the school starting age legislations, expressions (2) and (3) can be used to derive an expected age function at time t for children in a given grade:

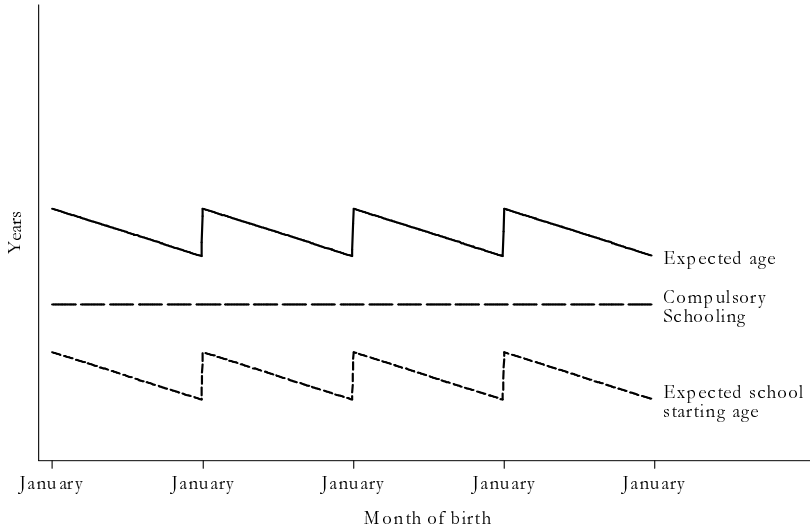
$$f_{it}^A = f_i^{A^S} + s_t^C = 7.7 - \frac{MOB_i - 1}{12} + s_t^C, \quad (4)$$

where f_{it}^A is the expected age function and s_t^C is a given grade in compulsory school. The only difference between the expected school starting age and the expected age functions is the constant grade level. It is, thus, not possible to freely vary expected age and expected age at school entry for children enrolled in the same grade of compulsory school. So differences in compulsory school performance by month of birth may reflect not only school starting age effects but also age effects.

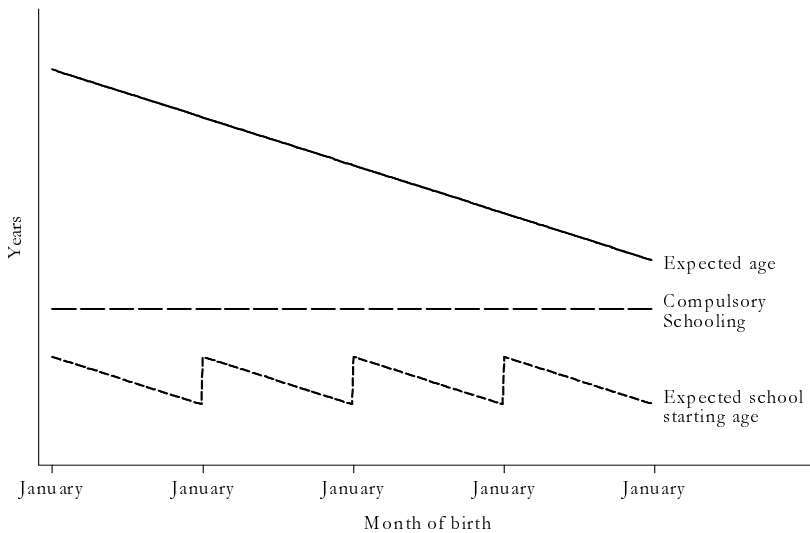
Figure 2 illustrates the expected age and expected school starting age profiles induced by the school starting age legislations for different data collection strategies. When the data are collected for all individuals in the same grade of compulsory school, the saw-teeth shaped pattern for the expected school starting age function is transmitted to the expected age function. This is illustrated in Panel A of Figure 2. Children born just after the school cut-off date, start school one year later than children born just before the cut-off. Since the data is collected in the same grade, the outcomes of children born in January are collected one year later than for children born in December. Consequently, children born just after the break-point are older when they start school *and* when they do the test (or get the grade) than children born just before the break-point.

Figure 2 Age and school starting age profiles induced by the school starting age legislation and different data collection strategies

Panel A Repeated cross-sectional data collected *in the same grade of compulsory school at different points of time.*



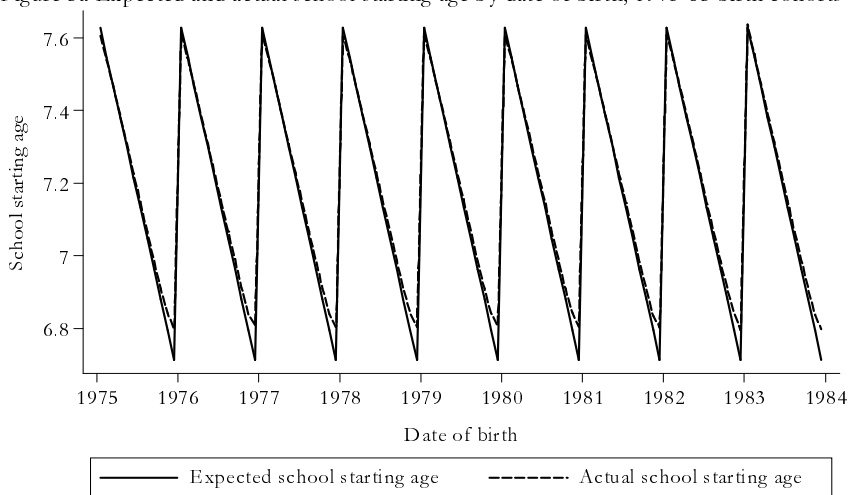
Panel B Cross-sectional data collected *after compulsory school at the same point of time.*



Note: The figure shows prototypical age and school starting age profiles induced by the school starting age legislations and different data collection strategies. Panel A shows the profiles for repeated cross-sectional data collected for all individuals in a given grade of compulsory school at different points of time. Panel B shows the profiles for cross-sectional data collected for all individuals after compulsory school at the same point of time, respectively.

To break the exact linear dependence between age, age at school entry and time spent in compulsory school, we use data collected at the same point in time for individuals who have completed their compulsory schooling. The expected age function then corresponds to the individuals' actual age, and is not affected by the age at school entry. This is illustrated in Panel B of Figure 2. In particular, children who are born just before or just after January 1st have approximately the same age at the time of observation, the same amount of compulsory schooling but start school at different ages. For this population, the only difference is their expected age at school entry. The timing of the data collection is, thus, crucial for separately identifying the effect of the school starting age; this point is also made by Hansen et al. (2004).

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



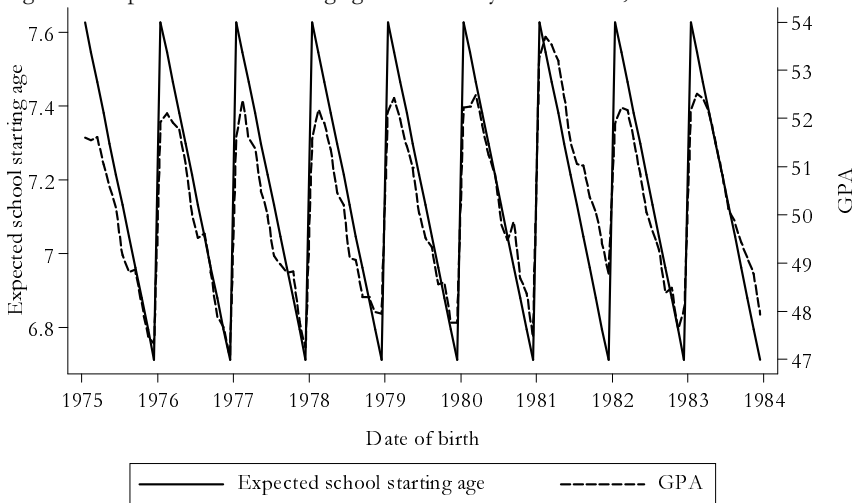
Note: Expected school starting age is the age when an individual is expected to start school according to date of birth and the school cut-off date. Date of birth is measured in months.

Figure 3a shows the "first-stage" relation between expected and actual age at school entry by year and month of birth. Clearly, the actual age at school start follows the expected school starting age closely. Children born early in the year on average start school at the age of 7.7 years. The age at school entry then falls monotonically by month of birth to about 6.8 years for children born in December. It then jumps back to an average school starting age of 7.7 years for children born the following January, generating a saw-teeth pattern between age at school entry and date of birth. This suggests that children born on different sides of the new year

typically start school with an age difference of almost a year.

In addition to exhibiting a strong association with the actual school starting age, the expected school starting age is also correlated with the average ninth grade GPA. This is shown in Figure 3b. The average GPA follows closely the saw-teeth pattern of the average school starting age, with sharp jumps in student achievement just around the school cut-off date. Hence, children born in the beginning of the year on average perform better in school than children born later. The difference in average GPA between January and December births is about 5 percentile ranks. It seems reasonable to believe that the variation in age at school entry driven by month of birth and the school cut-off date is exogenous, and that the better performance of children born in January relative to those born in December is caused by the differences in the school starting age. We subject this conjecture to formal tests in section 7.

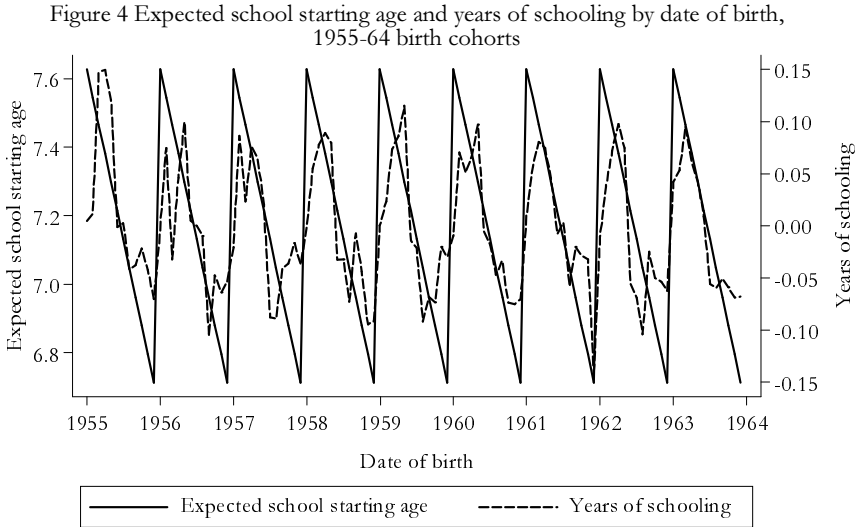
Figure 3b Expected school starting age and GPA by date of birth, 1975-83 birth cohorts



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.

The observed relation between month of birth and ninth grade GPA might reflect differences in school starting age and/or differences in age. To separate the effect of age at school entry from any general age effect – and to study the long-run effects of school starting age – we next present data on schooling and earnings by month of birth. There is a strong

positive trend in educational attainment for cohorts born 1935-75; see Table A2. Also, we expect the standard concave age-earnings profiles to produce a negative trend in earnings by year of birth. To abstract from these issues, we detrend the data on schooling and earnings by month of birth by subtracting off the average for each birth cohort.

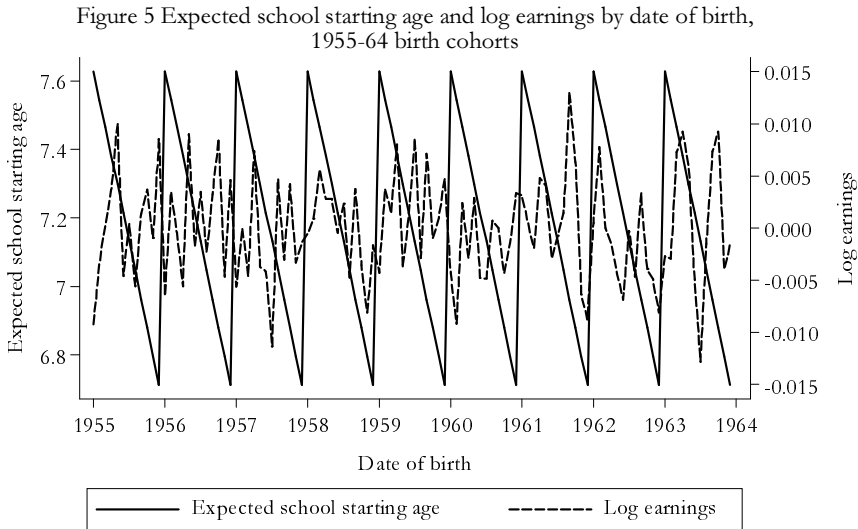


Note: Expected school starting age is the age when an individual is expected to start school according to date of birth and the school cut-off date. The years of schooling variable has been detrended by subtracting off the average years of schooling for each shifted year of birth cohort. Date of birth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

Figure 4 depicts the relationship between average years of education in 2000 and date of birth for those born between 1955 and 1964. The figure shows that season of birth is strongly related to educational attainment. Contrary to what Angrist and Krueger (1991) find, however, individuals born in January have more schooling on average than those born in December. Thus the sharp discontinuity in the expected school starting age is translated into an upward jump in educational attainment around the break-point.¹⁵ This implies that the observed relation between season of birth and ninth grade GPA cannot solely be explained by 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.

¹⁵We show later that this pattern is strongest for the oldest cohorts and that the regularity holds for all birth cohorts born before 1975.

Figure 5 shows the expected school starting age and detrended median log earnings in 2000 for the cohorts born between 1955 and 1964.¹⁶ The pattern is considerably more erratic for earnings than for education. For the birth cohorts shown in the figure, there is effectively no relationship between the expected school starting age and earnings.



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.

All in all, date of birth and the school cut-off date generate potentially exogenous variation in age at school entry. Actual school starting age by month of birth follows a saw-teeth shaped pattern, with those being born in the beginning of the year being more likely to be older when they start school. The relationship between ninth grade GPA and month of birth follows closely the pattern found for the school starting age, suggesting that children who start school when they are somewhat older do better than children who start school when they are younger. Using data on later outcomes, we find that individuals born just before the school cut-off date do better in school. But the relationship between the expected school start and earnings appears to be substantially weaker.

¹⁶Notice that individuals with no earnings are included in the sample and assigned the lowest value of log earnings.

6 Empirical considerations

The graphical analysis suggests a clear link between the variation in age at school entry induced by the school starting age legislations and educational outcomes. However, to be able draw some inference there is need for a more formal analysis. Assume that the outcome (e.g. test scores, grades, years of schooling or earnings) for individual i at time t (the time of data collection) can be written as:

$$Y_{it} = \beta_0 + A_{it}\beta_1 + S_{it}^C\beta_2 + A_i^S\beta_3 + \mathbf{X}_{it}\boldsymbol{\beta}_4 + \varepsilon_{it}, \quad (5)$$

where A_{it} denotes age, S_{it}^C compulsory schooling, A_i^S the school starting age, \mathbf{X}_{it} a vector of individual characteristics, and ε_{it} an individual specific error component. The school starting age coefficient β_3 is the parameter of primary interest.

There are two main empirical problems with estimating (5) using OLS. First, since parents and school administrators can affect the age when the children start school, age at school entry is endogenous ($E[A_i^S\varepsilon_{it}] \neq 0$). Hence, OLS estimates of β_3 might suffer from bias.¹⁷ Second, for individuals still enrolled in compulsory school, there is an exact linear dependence between age, age at school start and time spent in school ($A_{it} = A_i^S + S_{it}^C$). It is, thus, not possible to separately identify β_1 , β_2 and β_3 for this population.

Our empirical strategy is to exploit the exogenous variation in age at school entry driven by the children's date of birth and the school cut-off date. In particular, children born on each side of the new year, are born at about the same time but differ in their school starting age. This is an application of Thistlethwaite and Cambell's (1960) regression-discontinuity design, where the regressor of interest (school starting age) can be expressed as a known discontinuous function of an underlying variable (date of birth). To exploit the exogenous variation induced by the school starting age legislations, we will use the expected school starting age function ($f_i^{A^S}$) as an instrument for actual age at school entry (A_i^S). The "first-stage" relation can, thus, be expressed as:

$$A_i^S = \pi_0 + A_{it}\pi_1 + S_{it}^C\pi_2 + f_i^{A^S}\pi_3 + \mathbf{X}_{it}\boldsymbol{\pi}_4 + \eta_{it}. \quad (6)$$

¹⁷Since retention and advancement is so rare in Sweden, we do not treat compulsory schooling as an endogenous variable.

Since f_i^{AS} is a deterministic discontinuous function of date of birth, and since age (date of birth) is related both to earnings and educational attainment, it is crucial to account for any direct effect of age on the outcome. Our main approach is to add flexible specifications of date of birth to the model. 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 (December 31). Note, however, that adding year-of-birth dummy variables would divide time into calendar years, which is exactly what the school starting age legislation does. To be able to exploit the variation in the school starting age around the new year, we therefore shift the year-of-birth fixed effects; time is thus divided into one-year-long intervals ranging from July 1st to June 30th the following year. The identifying assumption in this model is, thus, that these flexible controls for time of birth remove any direct effect of date of birth on the outcome. For the long-run outcomes (collected at the same point in time), including these controls removes any direct effect of age on the outcomes. As a specification test, we will also restrict the sample to individuals born close to the school cut-off date.

All outcome measures are collected at a given amount of compulsory schooling; either in the same grade of compulsory school or when compulsory school is completed. Hence, our data implicitly hold time spent in compulsory school constant and we can drop S_{it}^C from the model. As noted above, the timing of the data collection is crucial for what the estimates reflect. For outcomes collected in the same grade of compulsory school, there is no independent variation in age and age at school start. These estimates may, thus, reflect the combined effect of age and age at school entry. For outcomes collected after compulsory schooling, the exact linear dependence between age, age at school start and compulsory schooling is no longer binding. These estimates will, thus, separately identify the effect of school starting age.

7 Regression results

The purpose of this section is to present a collection of evidence pertaining to the importance of age at school start for schooling and labor market outcomes. We begin in section 7.1 with compulsory school outcomes. Outcomes in school is the natural place to start, although it is then generally not possible to separate the effect of the school starting age from the effect

of age differences when the outcome is measured. However, the compulsory school outcomes are interesting, in particular since the descriptive analysis in section 5 showed that there are long-run effects of differences in the school starting age.

In section 7.2 we address a question of great policy significance. Is the school starting age effect due to the fact that it is more productive to start class room training (earlier or) later or is it due to the fact that those starting school at an older age are *relatively* older within class? If it is primarily a relative age effect, then our estimates may have little to say about the fundamental policy question: Should formal learning start at an earlier or a later age? To address these issues, we make use of the arguably stochastic variation across cohorts within schools in the age composition of the individuals' peers. This strategy enables us to obtain a measure of the individuals' relative position in the age distribution. To precede the results, we show in section 7.2 that the relative age of the kids sometimes matters but when it matters the effect is minor.

Section 7.3 examines the long-run effects on education attainment and earnings (both observed in 2000) of differences in the school starting age. As hinted at earlier, the effects of the school starting age persist into adulthood.

7.1 Compulsory school outcomes

In Table 3 we examine the relationship between the school starting age and the 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 simple OLS estimates.¹⁸ The association between the actual school starting age and student performance is negative. However, this should not be interpreted as the causal effect of the school starting age. Instead, it is driven by the fact that early starters is selected from the pool of well-performing pupils while those with a delayed school start is selected from the pool of under-achievers (see Figure 1).

That selection biases the OLS estimate is illustrated by the two reduced form relationships presented in columns 2 and 3. It comes as no

¹⁸To abstract from any trends and seasonalities in birth rates, we always weight the estimates by the inverse probability of being included in the sample; notice, though, that the estimates are more or less invariant to weighting. We also present standard errors which are adjusted for clustering on school and year.

surprise that the relationship between the actual school starting age and the expected school starting age is very strong (and positive). Moreover, the relationship between the GPA and the expected school starting age is *positive*. These two reduced forms imply our preferred IV estimate presented in column 4. Relative to the OLS estimate, the IV approach reverses the relationship between the school starting age and student performance. The causal effect of starting school at an older age is thus positive. The result of increasing the school starting age by a year is that the GPA increases by 5 percentile ranks (which corresponds to 0.25 standard deviations of the transformed GPA distribution).

As illustrated in section 5, our identifying variation comes from the sharp school starting age difference between January and December kids. Whether children are born in December or January would appear to be largely random and therefore, our baseline specification includes a very limited set of controls; the baseline specification only includes the flexible date of birth controls (adding higher order polynomials in date of birth has no impact on the estimates). A priori, we could see little reason for such factors as family background, gender, and school take-up area to bias our estimates.

Nevertheless, this is a conjecture which should be subjected to formal scrutiny. Therefore, we devote the remaining three columns to some specification checks. In column 5 we control for parental education, which basically has no impact on the estimate. Moreover, if we include gender and school fixed effects the estimate is unchanged.

Columns 7 and 8 are devoted to examining whether our controls for birth date are sufficiently flexible to capture any direct effect of birth month on the outcomes. To conduct this exercise we use auxiliary data where we observe the birthday (rather than the birth month). Using these data, we limit the sample to those born within 2 weeks of either side of the break-point. Unfortunately, these data pertain to men only. Column 6, therefore, contains the baseline estimates for men only; the estimate for men (5.17 percentile ranks) is slightly higher than the estimate for both sexes. This baseline estimate should be compared to the estimate presented in column 7, where we have narrowed the sample down to those born within two weeks of either side of the break-point. The estimate is virtually identical to the one reported in the previous column. Finally, column 8 adds the control for parental education which has no effect on the estimate.

Table 3 OLS and IV estimates of school starting age on ninth grade GPA, 1975-83 birth cohorts

Model	OLS	Reduced Forms		IV	IV	IV	IV	IV
Dependent variable	GPA	School starting age	GPA	GPA	GPA	GPA	GPA	GPA
School starting age	-6.40 (0.11)	.	.	5.06 (0.14)	4.98 (0.14)	5.17 (0.19)	5.20 (0.30)	5.18 (0.29)
Expected school starting age	.	0.903 (0.001)	4.57 (0.12)
<i>Control variables:</i>								
Shifted year of birth dummies	X	X	X	X	X	X	X	X
Date of birth	X	X	X	X	X	X		
(Date of birth) ²	X	X	X	X	X	X		
Date of birth × above the break-point	X	X	X	X	X	X		
(Date of birth) ² × above the break-point	X	X	X	X	X	X		
Parental education					X			X
<i>Sample:</i>								
± 6 months from break-point	X	X	X	X	X	X		
± 2 weeks from break-point							X	X
n	796,328	796,328	796,328	796,328	710,676	409,865	30,902	27,622

Notes: GPA is in percentile ranks. Expected school starting age is the age when the individual is expected to start school according to date of birth and the school cut-off date. Date of birth is measured in months. 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. Standard errors adjusted for clustering (school and year) are in parentheses. The models that control for parental (mother's and father's) education are restricted to the 1975-82 period. The last three columns pertain to men only.

All in all, these specification tests suggest that parental education is unrelated to date of birth conditional on the flexible set of date of birth controls that we have added to the model. In fact, a sufficient amount of flexibility is key to arrive at this conclusion. We illustrate this in Table A4 in the Appendix where we, *inter alia*, experiment with alternative sets of date of birth controls.¹⁹ Thus, there appears to be no omitted variables bias in our baseline specification.²⁰ Starting school one year later increases school performance by around 5 percentile ranks.

Having subjected the baseline specification to a number of specification tests we proceed to Table 4. In Table 4, we report separate estimates by subject, gender, and family background using the baseline specification. Column 1 contains the estimates by subject for the entire sample. Prior to the introduction of the new grading system, students could opt for general and advanced classes in English and Math. The grades from a general and an advanced class are not comparable. Therefore, for the cohorts born 1975-1980, we regress the probability of attending an advanced class for these two subjects.²¹

Overall, the estimates of the key parameter of interest are remarkably stable across subjects. When student performance is measured by grades, the effect of starting a year later hovers around 5 percentile ranks (around 0.20 standard deviations). The exception from this rule is sports. To some extent this comes as no surprise. Physical development is of course a key ingredient in sports. Sporting activities are also conducted outside the school to a greater extent than for the academic subjects. Therefore, one would think that early advantages tend to persist in sports because sporting activities outside schools tend to be more selective. There is also a substantial literature documenting the importance of season of birth for

¹⁹The careful reader will have noticed that conditioning on parental education reduces the sample somewhat in Table 3. However, the reduction in sample size has no implications for our conclusion as shown in Table A4 where we stick to the same sample.

²⁰In 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. These individuals are not in our data on grades and, hence, we miss some key information. However, the probability of not being included in the data on grades is decreasing in the school starting age. So, if we think that missing grades data is an indication of poor school achievement, there is a slight downward bias in our estimates.

²¹At face value, it is somewhat hard to compare the magnitudes of the estimates across the two definitions of the outcome variables in Table 4. Note, though, that the estimate pertaining to the probability of attending an advanced class in Math corresponds (again) to an effect size of 0.17 standard deviations.

success in sports; see Musch and Grondin (2001) for a survey.

Turning to the gender differences presented in columns 2 and 3 of Table 4, the school starting age effects do not appear to vary much across gender. Again, there is one exception – sports. In sports, the starting age is almost twice as important for boys in comparison to girls. This may be due to the fact that they participate in sporting activities outside the school to a greater extent than girls, and as argued above, the total amount of selection in sports is greater than in any other subject.

Table 4 IV estimates of school starting age on ninth grade outcomes, 1975-83 birth cohorts

	All	Females	Males	Academic parents	Non-academic parents
<i>Grades</i>					
GPA	5.06 (0.14)	5.08 (0.19)	5.17 (0.19)	4.88 (0.19)	5.11 (0.20)
Swedish	5.52 (0.18)	5.83 (0.24)	5.48 (0.25)	5.27 (0.27)	5.61 (0.26)
Science	4.62 (0.17)	4.86 (0.24)	4.47 (0.25)	4.25 ^c (0.25)	4.88 ^c (0.25)
Social science	5.16 (0.17)	5.50 (0.24)	4.96 (0.24)	4.80 (0.24)	5.36 (0.25)
Sports	8.57 (0.18)	5.77 ^a (0.25)	11.22 ^a (0.27)	9.07 ^b (0.26)	8.14 ^b (0.28)
n	796,328	386,463	409,865	357,187	341,977
<i>Advanced class</i>					
English	0.064 (0.004)	0.058 ^c (0.005)	0.071 ^c (0.006)	0.044 ^a (0.005)	0.081 ^a (0.006)
Mathematics	0.086 (0.004)	0.087 (0.006)	0.085 (0.006)	0.074 ^b (0.005)	0.094 ^b (0.006)
n	536,924	260,329	276,595	268,606	259,956

Notes: All grades are in percentile ranks. School starting age is instrumented with expected school starting age. All models also include an intercept, shifted year of birth dummy variables, date of birth, date of birth squared, date of birth interacted with a dummy for being above the break-point and date of birth squared interacted with a dummy for being above the break-point. Date of birth is measured in months. 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. Academic parents means that at least one parent have a long high school degree or higher, and is restricted to the 1975-82 birth cohorts. The probability of attending an advanced class is estimated using a linear probability model, and restricted to the 1975-80 birth cohorts. Standard errors adjusted for clustering (school and year) are in parentheses. a/b/c=the estimates are significantly different from zero at the 1/5/10 per cent level of confidence using a two-sided t-test for females/males and academic/non-academic parents, respectively.

Finally, columns 4 and 5 report separate estimates by parental education. Kids having at least one parent with at least three years of upper secondary education are defined as having "Academic parents". As shown by these two columns, this is the point where there is some coefficient het-

erogeneity. For all academic subjects, the starting age tends to be more important for children who have parents that are relatively less skilled. The most striking difference across the two groups pertains to the probability of attending advanced classes in Math and English. For instance, in English the effect for kids with relatively less skilled parents is almost twice the size of the effect for kids with more skilled parents. We think it makes sense to see the greatest differences when it comes to these two outcomes. The choice of attending an advanced class probably reflects family background to a greater extent than grades. Having an initial advantage due to a later school start appears to be more important for outcomes that tend to be more "constrained" by family background.

It is difficult to determine whether one should be surprised by the general pattern of the estimates by parental education in Table 4. On the one hand, the pattern is consistent with findings from other educational interventions. For instance, according to the evidence from STAR, class size interventions have greater effects for kids from disadvantaged backgrounds; see Krueger (1999) for example. On the other hand, proponents of early intervention argue that such policies will have particularly beneficial effects for children from disadvantaged backgrounds.²² However, it is not clear that the argument in favor of early intervention is so pertinent in this context. The alternative to starting school early (in part staying at home with their less skilled parents but also proceeding in child care) may (or may not) be less advantageous for kids from disadvantaged background. But presumably they are not ready for the treatment (formal classroom training) to the same extent as kids from advantaged backgrounds. Some indication that this is what is going on can be obtained by looking at the estimate for sports. Clearly sports does not have a class-room component and for sports the pattern is reversed; children with academic parents gain more from starting later when it comes to sports. All in all, we think the pattern of the estimates in columns 4 and 5 make sense.

²²Some qualifications are probably due here. Proponents of early intervention presumably have other treatments in mind when presenting their case. Early childhood programs may involve learning social skills and behavior to a greater extent than formal class room training. Moreover, while the family background characteristic we consider is a sensible measure of family skills, it is not a direct indicator of troubled families.

Table 5 IV estimates of school starting age on different outcomes in 6th grade, different birth cohorts

Birth cohort:	1948	1953	1967	1972	1977	1982
<i>IQ-test scores:</i>						
Verbal	7.71 (1.03)	8.71 (1.22)	9.70 (1.25)	7.98 (1.13)	10.24 (1.79)	7.37 (1.27)
Spatial	5.87 (1.01)	5.08 (1.21)	6.19 (1.21)	8.79 (1.12)	7.45 (1.81)	7.12 (1.27)
Number series	4.18 (1.03)	5.94 (1.21)	7.54 (1.23)	7.38 (1.12)	6.60 (1.80)	8.71 (1.28)
<i>Achievement test scores:</i>						
Swedish	6.34 (0.99)	8.57 (1.21)	11.21 (1.21)	4.24 (1.05)	.	.
English	3.94 (1.02)	6.24 (1.20)	10.11 (1.18)	.	.	.
Mathematics	6.35 (1.03)	8.35 (1.21)	9.96 (1.20)	8.21 (1.10)	6.70 (1.77)	8.87 (1.25)
<i>Grades:</i>						
Swedish	5.94 (0.99)	7.38 (1.16)	10.08 (1.10)	.	.	.
English	4.04 (1.00)	6.10 (1.16)	8.85 (1.08)	.	.	.
Mathematics	6.75 (1.01)	8.51 (1.17)	9.96 (1.09)	.	.	.
n	11,905	9,855	8,711	9,035	4,049	7,829

Notes: All outcome measures are in percentile ranks. 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 is 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.

Above we raised some concerns about the informational value of grades. Table 5 shows that these concerns are unwarranted. We report school starting age effects on 6th grade outcomes for a selection of cohorts born between 1948 and 1982, using data from the UGU project. The virtue of these data is that there is information on scores on IQ and achievement tests along with grades for Swedish, English, and Math. A drawback with these data is that we cannot rely on the sharp discontinuity to identify the key effect of interest – so the estimates are probably biased. We see no reason for this problem to bias the relative comparison across outcomes (e.g. achievement scores and grades), however.

Table 5 contains several interesting messages. First, the results show that it does not make much difference 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, which is consistent with the view that IQ is "malleable". Third, the effects on IQ test scores are remarkably consistent over time. Since the same IQ test has been given to all cohorts in the UGU-project, this implies that the results we report in Tables 3 and 4 should apply to the older cohorts as well.

As a final sensitivity check we have estimated the effects of the school starting age separately by birth cohort for those born 1975-83. These estimates are not reported, since they vary across birth cohorts only to a limited extent.

7.2 Is it a relative age effect?

The question that we are framing this paper with (Is it more productive to start school earlier?) is a question about the effect of the absolute school starting age. But the effects may also be due to the relative age of kids, since, e.g., those born in December are also the youngest in the class. The estimates on the school starting age in, e.g., Table 4 capture both of these effects.

But for policy purposes one would most often like to free the estimates of relative age effects. A key policy question is what would happen to school performance if the school start is pushed forward by, say, one year for all children. Clearly, such a policy will not change the relative age distribution of kids.

The estimates we have reported so far can be viewed as non-parametric estimates of the total effect of the school starting age (c.f. col. 7, Table

3). We now want to separate the relative age effect from the absolute age difference. To do this we impose some additional assumptions. We make the assumption that the relative age effect is captured by the rank order in the age distribution within school and exploit the within school variation in the age composition across cohorts to estimate the relative age effect.²³ The individuals' position in the age distribution is simply the percentile rank in the age distribution in the school. The identifying variation thus comes from the fact that the rank order of individuals born in, e.g., December in a given school changes along with the age distribution across cohorts within schools. We instrument the age rank with the rank according to individuals' expected school starting age within the expected school starting age distribution. We choose units such that the percentile rank is defined on the unit interval. This implies that there will be roughly a full year difference between the top ranked individual (assigned a rank of 1 and born in January) and the lowest ranked individual (assigned a rank of 0 and born in December). The magnitudes of the estimates on the absolute age effect and the relative age effect should thus be approximately comparable. Notice, finally, that we use the entire range of birth dates (not just December and January kids) to estimate the relative age effect.

To be able to interpret the estimates causally, we require that individuals are not sorted on the basis of their position in the age distribution. It seems to us that such sorting is highly unlikely. In Sweden, children were allocated to schools based on proximity.²⁴ Thus, the only way to change schools was to move to another school district. The incidence of school switchers would, however, only threaten our identifying strategy if parents based their residential choice on the changes in the expected age distribution of the schools. We doubt that this is the case, since informa-

²³Here, the school refers to the school in ninth grade. Ideally we would have liked to rank the individuals within class and then used the within school variation in the expected school starting age distribution as an instrument. However, the class information is unavailable to us. Notice, that this preferred strategy would have no effect on the precision of the estimates but may affect the relative size of the estimates. We have also performed some sensitivity checks restricting the analysis to pupils from small schools. The idea is that that the within school variation will be a better representation of the within class variation in small schools. As it turned out, the relative age effects were never significant in the sample restricted to small schools.

²⁴A limited amount of school choice was introduced in 1992. However, those living in the school district are still given preferential treatment in case of over-subscription. Moreover, the youngest individuals in our sample started first class in 1990. Therefore, we believe that this reform is a very minor issue for the individuals in our sample.

tion on month of birth among the pupils in different schools hardly was available to them. Of course, we can also substantiate these claims by examining whether parental education is related to our instruments. This is done in column 7 of Table A4. It turns out that there is no correlation between our instruments and parental education; both instruments enter the equation with t-values lower than 0.7.

Having established that the instruments appear to be valid, we proceed to the estimates reported in Table 6. The general outline is identical to Table 4, i.e., we present separate estimates by subject, gender and family background. Each cell, however, presents two sets of estimates. The top one is the effect of the absolute age difference while the bottom one in italics is the relative age effect. The overall message conveyed by these results is that the absolute age effect is substantially more important than the relative age effect; taking the estimates at face value, some 20 percent of the overall effect appears to be due to the relative age within class. Notice, further that the relative age effects are as precisely determined as the effects of the absolute age differences.

There is interesting gender pattern in Table 6. Absolute maturity appears to be more important for boys than for girls; the mirror image is that relative age effects are significant (economically as well as statistically) in a greater number of instances for girls. To us this pattern makes sense since boys, on average, mature later than girls; consequently an absolute age difference of a year should have a greater effect for boys than for girls.

Table 6 IV estimates of absolute and relative school starting age on ninth grade outcomes, 1975-83 birth cohorts

	All	Females	Males	Academic parents	Non-academic parents
<i>Grades</i>					
GPA	3.92 (0.69) <i>1.22</i> (0.70)	3.50 (1.02) <i>1.66</i> (1.03)	4.95 (1.01) <i>0.21</i> (1.02)	3.50 (0.94) <i>1.42</i> (0.94)	3.96 (0.93) <i>1.22</i> (0.94)
Swedish	4.51 (0.95) <i>1.08</i> (0.96)	3.47 (1.34) <i>2.46^c</i> (1.35)	6.52 (1.31) <i>-1.13^c</i> (1.32)	4.51 (1.30) <i>0.79</i> (1.31)	4.33 (1.25) <i>1.35</i> (1.27)
Science	2.70 (0.88) <i>2.08</i> (0.89)	2.33 (1.29) <i>2.75</i> (1.31)	3.68 (1.31) <i>0.83</i> (1.32)	1.82 (1.19) <i>2.51</i> (1.20)	3.00 (1.18) <i>2.02</i> (1.20)
Social science	4.22 (0.90) <i>1.00</i> (0.90)	3.94 (1.30) <i>1.61</i> (1.31)	5.15 (1.30) <i>-0.19</i> (1.32)	4.06 (1.21) <i>0.75</i> (1.21)	4.34 (1.18) <i>1.08</i> (1.20)
Sports	5.23 (1.00) <i>3.57</i> (1.01)	3.03 ^b (1.34) <i>2.94</i> (1.36)	7.51 ^b (1.38) <i>3.96</i> (1.39)	4.57 (1.32) <i>4.70</i> (1.33)	5.46 (1.37) <i>2.79</i> (1.39)
n	796,328	386,463	409,865	357,187	341,977
<i>Advanced class</i>					
English	0.019 (0.028) <i>0.045</i> (0.028)	0.009 (0.033) <i>0.048</i> (0.033)	0.038 (0.033) <i>0.035</i> (0.034)	-0.010 (0.032) <i>0.053</i> (0.033)	0.051 (0.033) <i>0.033</i> (0.034)
Mathematics	0.082 (0.024) <i>0.004</i> (0.025)	0.074 (0.033) <i>0.010</i> (0.033)	0.094 (0.031) <i>-0.007</i> (0.032)	0.081 (0.030) <i>-0.009</i> (0.030)	0.081 (0.031) <i>0.016</i> (0.032)
n	536,924	260,329	276,595	268,606	259,956

Notes: All grades are in percentile ranks. The effect of (absolute) age at school start is in normal fonts; the effect of relative age in class is in italics. School starting age is instrumented with expected school starting age. Relative school starting age is the percentile rank (divided by 100) of the individual's school starting age in the school and year, and is instrumented with the expected school starting age percentile rank. All models also include an intercept, shifted year of birth dummy variables, date of birth, date of birth squared, date of birth interacted with a dummy for being above the break-point and date of birth squared interacted with a dummy for being above the break-point. Date of birth is measured in months. 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. Academic parents means that at least one parent have a long high school degree or higher, and is restricted to the 1975-82 birth cohorts. The probability of attending an advanced class is estimated using a linear probability model, and restricted to the 1975-80 birth cohorts. Robust standard errors are in parentheses. a/b/c=the estimates are significantly different from zero at the 1/5/10 per cent level of confidence using a two-sided t-test for females/males and academic/non-academic parents, respectively.

7.3 Educational attainment

Let us turn to the longer term effects of variations in the school starting age. In this case, the age when measuring outcomes and the school starting age vary independently when comparing January and December kids; see Figure 2. Therefore, these estimates can be thought of as giving the pure (long-run) effect of variations in the school starting age.

In Table 7 we examine whether educational attainment is affected by the school starting age. The educational outcomes are represented by (imputed) years of schooling and the probability of attending college. The probability of attending college is, in turn, defined as having a degree from university education which is at least two years long. From top to bottom we present separate estimates for individuals born 1935-44, 1945-54, and so on. The bottom panel contains results for all birth cohorts – born 1935-84 – pooled together. One reason for presenting separate estimates by 10-year birth cohorts is that the characteristics of the compulsory school system has varied over time; see Table 1. 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 there was a gradual introduction of the comprehensive school reform (see Table 1); the comprehensive school reform was essentially completed by the time that the cohort born in 1955 started school (Holmlund, 2006).²⁵ Since then children have gone to school within the same basic structure.

The school starting age effects for all cohorts are consistent with the estimates presented in section 7.1. Except for the cohorts born during 1975-84, all individuals benefit from starting school at an older age. The reason for the negative effect for those born after 1975 is that a substantial portion of these individuals have not yet finished their schooling careers; individuals born in the beginning of the year, start school one year later

²⁵ Because of the gradual introduction of the comprehensive school, there is an issue about the interpretation of the estimate for the 1945-54 cohorts. Children born in January are more likely to attend the new 9-year comprehensive school while the old system (generally 8 years of compulsory school) is more likely to apply to children born in December the previous year. One worry is that this feature would "mechanically" raise educational attainment for children born in January. We have made various attempts to correct for this problem; neither of these corrections had an impact on the estimate. In any event, the size of this "mechanical effect" is likely to be very small. The evidence in Meghir and Palme (2005) suggests that it is in the order of 0.02 years.

and are thus more likely to still be enrolled in school or college than individuals born at the end of the year. For the remaining cohorts, the effect of starting school one year later varies from 0.08 years of schooling (1955-74) to 0.19 years of schooling (1945-54).

The effect for those born prior to 1955 is thus substantially bigger 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 A2). But even if we convert the effects into standard deviation units – 0.06 SD (1935-54) and 0.03-0.04 SD (1955-74) – the difference across cohorts remains. This difference across the cohorts is interesting. We are inclined to interpret the difference as having to do with the schooling system being decisively less selective after the introduction of the comprehensive school. In particular the key selection stage is pushed forward from age 10 in the old system to age 13 in the comprehensive school.²⁶

The school starting age effect seems to be smaller when it comes to the long-run schooling outcomes considered here (0.04 SD for those born in the 1965-74) in comparison with the compulsory school outcomes considered in section 7.1 (0.20-0.25 SD). That the long-run effects are smaller seems sensible given depreciation. Moreover, achievement gains are presumably not translated fully into increases in years of schooling. But it also seems likely that it reflects the fact that we cannot distinguish between the school starting age and the age effect for the compulsory school outcomes.

Table 7 also shows that the parameter of interest varies somewhat by gender, but there is no consistent pattern across cohorts. The coefficient of interest does not vary by family background, as evidenced by the estimates for the 1965-74 cohorts. Thus, unlike the compulsory school outcomes, the school starting age effects do not seem to vary by groups for these long-run schooling outcomes.

²⁶Bedard and Dhuey (2005) have a model where they show that it is more likely that initial difference will persist if children are tracked early on.

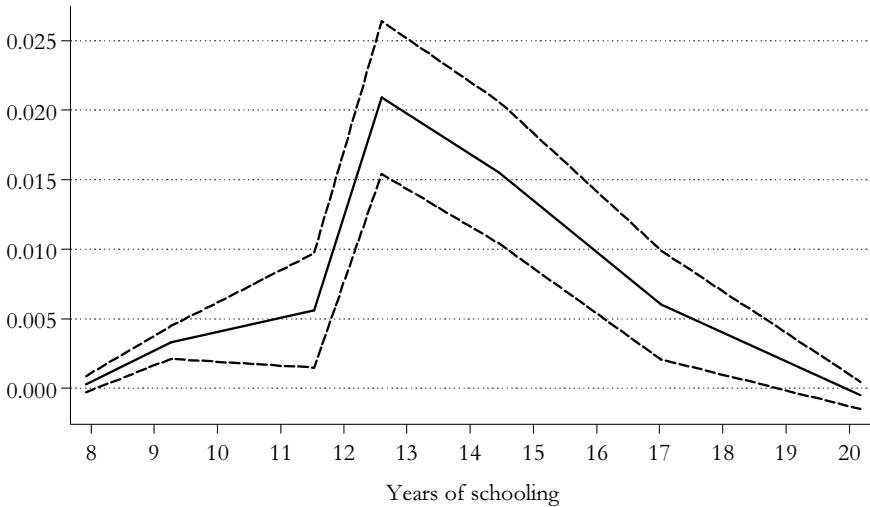
Table 7 IV estimates of school starting age on various educational outcomes in 2000

	All	Females	Males	Academic parents	Non-academic parents
1935-44 Birth cohorts					
Schooling	0.1721 (0.0184)	0.1760 (0.0249)	0.1683 (0.0271)	.	.
P(College)	0.0190 (0.0025)	0.0186 (0.0035)	0.0193 (0.0035)	.	.
n	787,882	393,694	394,188		
1945-54 Birth cohorts					
Schooling	0.1910 (0.0153)	0.2004 (0.0210)	0.1828 (0.0221)	.	.
P(College)	0.0246 (0.0024)	0.0274 (0.0035)	0.0221 (0.0033)	.	.
n	1,057,221	518,866	538,355		
1955-64 Birth cohorts					
Schooling	0.0822 (0.0140)	0.1128 ^a (0.0195)	0.0510 ^a (0.0198)	.	.
P(College)	0.0155 (0.0026)	0.0185 (0.0038)	0.0123 (0.0035)	.	.
n	964,414	471,704	492,710		
1965-74 Birth cohorts					
Schooling	0.0837 (0.0120)	0.0699 (0.0170)	0.0979 (0.0169)	0.0820 (0.0198)	0.0858 (0.0139)
P(College)	0.0142 (0.0024)	0.0088 ^b (0.0035)	0.0195 ^b (0.0034)	0.0149 (0.0041)	0.0136 (0.0028)
n	1,037,657	502,475	535,182	419,484	592,051
1975-84 Birth cohorts					
Schooling	-0.3721 (0.0094)	-0.4081 ^a (0.0133)	-0.3343 ^a (0.0132)	-0.5414 ^a (0.0159)	-0.3245 ^a (0.0161)
P(College)	-0.0271 (0.0018)	-0.0281 (0.0027)	-0.0256 (0.0024)	-0.0566 ^a (0.0037)	-0.0137 ^a (0.0028)
n	944,115	457,486	486,629	362,666	350,195
All birth cohorts (1935-84)					
Schooling	0.0283 (0.0062)	0.0261 (0.0086)	0.0308 (0.0089)	.	.
P(College)	0.0088 (0.0010)	0.0086 (0.0015)	0.0090 (0.0014)	.	.
n	4,791,289	2,344,225	2,447,064		

Notes: School starting age is instrumented with expected school starting age. All models also include an intercept, shifted year of birth dummy variables, date of birth, date of birth squared, date of birth interacted with a dummy for being above the break-point and date of birth squared interacted with a dummy for being above the break-point. Date of birth is measured in months. Academic parents means that at least one parent have a long high school degree or higher. 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. Only individuals born 1965-82 have sufficiently high share of non-missing information on parental education to be used in the analysis. The probability of having attended college is estimated using a linear probability model. Robust standard errors are in parenthesis. a/b/c=the estimates are significantly different from zero at the 1/5/10 per cent level of confidence using a two-sided t-test for females/males and academic/non-academic parents, respectively.

Where in the educational distribution does the school starting age have an effect? Figure 6 examines this question by plotting the difference in the probability to attain different levels for the 1955-64 birth cohorts. The main message given by the figure is that those who start school one year later are more likely to choose academic tracks at upper-secondary school (12 years of schooling) rather than vocational tracks (11 years of schooling). Thus the major part of the total effect occurs around the median of the schooling distribution.²⁷

Figure 6 The effect of school starting age over the education distribution, 1955-64 birth cohorts



Note: The solid line shows the effect of age at school start on the probability of having schooling equal to or exceeding the values on the x-axis. The dashed lines show the mean \pm 1.96 standard deviations.

7.4 Earnings

Variations in the school starting age affect many margins influencing the final earnings outcome. Most obviously, it has an effect on the amount of schooling, as shown in the previous section. Perhaps as obviously, children who start school one year later, enter the labor market one year later con-

²⁷In this age span, 85 % of the native-born population has more than 9 years of schooling and the median individual has a vocational education from upper-secondary school.

ditional on age and schooling. In addition, experience is lower because late school starters have more schooling. Finally, the analysis in section 7.1 shows that there are achievement differences by age at school start conditional on years of compulsory schooling. If these achievement differences are not translated fully into years of schooling, there will be unobserved performance differences related to the 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.²⁸

Here we estimate the reduced-form effect on earnings of the school-starting age (without controlling for experience and schooling). This reduced-form parameter is what we mainly should focus on if the policy-interest is in the benefits and costs of alternative school-starting ages.

Table 8 reports the estimated school starting age effects for earnings. The table has an identical outline as the previous one; from top to bottom we thus present estimates for successively younger cohorts. In each panel we report estimates for earnings (which includes those with no earnings) and estimates for log earnings where we have imposed a lower earnings limit of 100,000 SEK. We impose the lower earnings limit since we want the estimates to resemble estimates of wage effects.

The estimates by birth cohort have a similar flavor as the estimates for educational attainment. There is a positive earnings effect of starting school at an older age for the oldest cohorts (born prior to 1955); this effect turns negative and becomes negative and significant for the cohorts born after 1965.

The earnings estimate for the 1975-84 cohorts reveals a sizable negative effects. Again, this negative estimate is driven by the fact that these cohorts have not yet finished their 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 school or college (see Table 7).

The general pattern of the earnings estimates makes sense. The net earnings 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. Because the earnings profile is concave, the loss of experience

²⁸There are also other reasons to treat IV-estimates of the return to schooling using date of birth as an instrument with some skepticism; see Bound and Jaeger (2000).

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 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 fairly low by US standards. The experience profile is flat by US standards 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 the school starting age is the effect on schooling (0.17 years), we would predict an earnings gain of 1 percent (i.e. $0.17 \times 5.8 = 1$). This is very close to the estimate of 1.3 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 1.2 percent (i.e. $0.08 \times 5.8 - (1 + 0.08) \times 1.5 = -1.2$), if only the changes in education and experience were the relevant effects of the school starting age. Again, this is not far off the estimate reported for the 1965-74 cohorts (-0.8 %). These simple calculations are of course based on the assumption that the cross-sectional returns to experience and education are causal. With this caveat in mind, the calculations convey two messages: first, the pattern of the estimated net earnings returns is sensible; second, in a country with greater returns to education and experience, such as the US, we should expect to see greater effects on earnings.²⁹

The bottom panel presents earnings estimates for all individuals in our sample. This estimate is interesting since it may be interpreted as the individual net earnings effect over the life-cycle of starting school a year later. As can be seen, this overall effect is negative.

²⁹We verified this conjecture using log earnings estimates based on US Census data for 2000 (the IPUMS). All education and earnings premia are roughly twice the size in the US relative to Sweden. The earnings losses due to forgone experience early on in the career are accordingly roughly twice as large in the US. The subsequent earnings gains are larger by a factor of 2 as well.

Table 8 IV estimates of school starting age on earnings in 2000

	All	Females	Males	Academic parents	Non-academic parents
1935-44 Birth cohorts					
Earnings	4041 (921)	5035 (867)	3515 (1618)	.	.
Log(Earnings)	0.0134 (0.0047)	0.0146 (0.0061)	0.0177 (0.0065)	.	.
n	787,882	393,694	394,188		
1945-54 Birth cohorts					
Earnings	1245 (881)	1158 (847)	1100 (1495)	.	.
Log(Earnings)	0.0010 (0.0024)	-0.0007 (0.0028)	0.0019 (0.0035)	.	.
n	1,057,221	518,866	538,355		
1955-64 Birth cohorts					
Earnings	-982 (973)	-209 (904)	-1424 (1638)	.	.
Log(Earnings)	-0.0018 (0.0026)	-0.0011 (0.0032)	-0.0016 (0.0035)	.	.
n	964,414	471,704	492,710		
1965-74 Birth cohorts					
Earnings	-2949 (706)	-2427 (751)	-3499 (1111)	-4373 ^b (1385)	-1237 ^b (771)
Log(Earnings)	-0.0075 (0.0022)	-0.0037 ^b (0.0031)	-0.0124 ^b (0.0028)	-0.0133 ^b (0.0038)	-0.0027 ^b (0.0027)
n	1,037,657	502,475	535,182	419,484	592,051
1975-84 Birth cohorts					
Earnings	-10695 (361)	-8839 (438)	-12677 (554)	-13649 ^b (668)	-11344 ^b (669)
Log(Earnings)	-0.0333 (0.0254)	-0.0995 ^b (0.0242)	-0.0019 ^b (0.0404)	-0.0437 (0.0066)	-0.0417 (0.0046)
n	944,115	457,486	486,629	362,666	350,195
All birth cohorts (1935-84)					
Earnings	-2153 (350)	-1314 (342)	-2923 (589)	.	.
Log(Earnings)	-0.0063 (0.0056)	-0.0190 ^c (0.0061)	0.0003 ^c (0.0088)	.	.
n	4,791,246	2,344,210	2,447,036		

Notes: School starting age is instrumented with expected school starting age. All models also include an intercept, shifted year of birth dummy variables, date of birth, date of birth squared, date of birth interacted with a dummy for being above the break-point and date of birth squared interacted with a dummy for being above the break-point. Date of birth is measured in months. Academic parents means that at least one parent have a long high school degree or higher. 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. Only individuals born 1965-82 have sufficiently high share of non-missing information on parental education to be used in the analysis. The log earnings are restricted to individuals earning more than 100,000 SEK, which reduces the sample sizes. Robust standard errors are in parenthesis. a/b/c=the estimates are significantly different from zero at the 1/5/10 per cent level of confidence using a two-sided t-test for females/males and academic/non-academic parents, respectively.

The interpretation of the estimate in the bottom panel is complicated by the fact we are aggregating over two different schooling systems. As shown in Table 7, the school starting age effects are greater for individuals who attended the old – more selective – system than for individuals who attended the new – more egalitarian – comprehensive school. To deal with these concerns we simulate the school starting effects on earnings in a similar fashion as above. The basic input for these calculations are regressions where we relate annual earnings (including zero-earners) to years of schooling, a second-order polynomial in potential experience, and gender. We then simulate the earnings effects under two scenarios – one where the school entry age has a small impact on years of schooling (0.08 years) and one where the entry age has a big impact (0.19 years). Finally, we calculate the discounted sum of the earnings effects over the working career.

Table 9 Present values of simulated earnings effects of the school starting age

Effect on schooling:	Sweden		U.S.	
	Low	High	Low	High
<i>Discount rate</i>				
0.00	-\$7,905	\$2,970	-\$3,669	\$13,174
0.01	-\$19,318	-\$10,668	-\$21,039	-\$8,693
0.03	-\$29,383	-\$23,443	-\$37,012	-\$29,983
0.05	-\$31,881	-\$27,380	-\$41,630	-\$37,336

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 (0.0822 and 0.1910 years, respectively) has been calculated in standard deviation units, and converted to the corresponding years of schooling in the U.S. schooling distribution (0.0972 and 0.2039 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.

The left-hand panel of Table 9 presents the results of these calculations. The first row shows that there is a life-time earnings gain of starting school later only when the schooling effects are high and there is no discounting of the future. As soon as we start discounting the subsequent gains, the present values become negative.

How do these estimates translate to a country characterized by greater earnings differentials? To answer this question, the right-hand panel of Table 9 shows analogous "counter-factual" calculations for the US. We estimate annual earnings regressions in a similar fashion as for Sweden using the 2000 US Census 5 % sample (IPUMS, see Ruggles et al., 2004). Then we simulate the earnings effects under two assumptions about the size of the schooling effects. To obtain these two scenarios we transformed our Swedish estimates into standard deviation units and translated them into years of schooling using the US schooling distribution. These calculations convey the same message as the one for Sweden: it is only when the schooling effects are high (0.20 years) and we apply no discounting that there is an life-time earnings gain of starting school later. When we start to discount the future gains, the present values turn negative. In sum, it is reasonable to conclude from these calculations that starting school one year later has a negative effect on life-time earnings

8 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. Starting school one year later increases compulsory school performance by 0.20-0.25 standard deviations (SD). Moreover, the effects persist into adulthood. Late school starters go on to have more schooling; starting school one year later has the effect of raising educational attainment by 0.03–0.06 SD. The 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 experienced lost has no implications and therefore late school starters have a slight earnings advantage (0.03 SD) 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 (-0.02 SD) for individuals aged 26-35. The opportunity cost of starting school later 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. Taken seriously, our analysis suggests that the short-run opportunity cost outweighs the long-

run gains: starting school one year later has a negative earnings return over the life cycle.

Our evidence also suggests that the effects we measure are primarily due to absolute maturity (in particular for boys) rather than to the relative age in the class. Taken at face value, this implies that pushing the start of classroom training, e.g., one year earlier will be detrimental for the children's scholastic achievement.

In sum, there appears to be a trade-off: if one is concerned with the scholastic achievement of children, our estimates suggest a slightly higher school starting age; while if one is concerned with their life-time earnings the conclusion appears to be the opposite. Are there any ways to mitigate this apparent trade-off? Ameliorating the consequences of early tracking seems to be one option. 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 negative impact on schooling outcomes of an early school start are greater in this system than in the system which featured much less tracking.

Another way to reduce the detrimental effects on school performance of an early school start is to adapt the pedagogical techniques. We have analysed the virtues of an early school start for a given curricula (involving classroom training). But as argued by Stipek (2002): The appropriate question is not whether kids are ready for school – it is whether schools are ready for children. Older kids appear to be better prepared for classroom training. Younger kids may well be more receptive to other forms of learning.

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Appendix

Table A1 Descriptive statistics of ninth grade outcomes, 1975-83 birth cohorts

	All	Females	Males	Academic parents	Non-academic parents
<i>Grades</i>					
GPA	50.13 (20.47)	54.25 (20.03)	46.25 (20.12)	57.09 (19.09)	43.00 (19.50)
Swedish	50.26 (27.07)	58.87 (25.63)	42.14 (25.85)	58.22 (25.98)	42.04 (25.84)
Science	50.23 (25.59)	53.16 (24.90)	47.46 (25.92)	58.55 (24.22)	41.61 (24.35)
Social science	50.15 (25.42)	54.71 (24.72)	45.85 (25.32)	58.46 (23.90)	41.64 (24.31)
Sports	49.70 (27.02)	46.97 (26.35)	52.27 (27.39)	54.33 (26.08)	45.04 (27.20)
<i>Attend advanced class</i>					
English	0.69	0.75	0.63	0.81	0.57
Mathematics	0.57	0.57	0.56	0.71	0.42
<i>School starting age</i>					
Actual	7.19 (0.32)	7.18 (0.31)	7.20 (0.32)	7.18 (0.31)	7.20 (0.32)
Expected	7.17 (0.29)	7.17 (0.29)	7.17 (0.29)	7.17 (0.29)	7.17 (0.29)
n	796,328	386,463	409,865	357,187	341,977

Notes: Standard deviations are in paranthesis. All grades are in percentile ranks. 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. Academic parents means that at least one parent have a long high school degree or higher, and the sample is restricted to those born 1975-82. The share of individuals attending an advanced class is restricted to the 1975-80 birth cohorts.

Table A2 Descriptive statistics of various educational outcomes

	All	Females	Males	Academic parents	Non-academic parents
1935-44 Birth cohorts					
Schooling	10.4561 (3.0449)	10.4611 (2.9599)	10.4516 (3.1279)	.	.
College degree	0.2128	0.2206	0.2051	.	.
n	787,882	393,694	394,188		
1945-54 Birth cohorts					
Schooling	12.0949 (2.9426)	12.2344 (2.8780)	11.9609 (2.9975)	.	.
College degree	0.2963	0.3202	0.2732	.	.
n	1,057,221	518,866	538,355		
1955-64 Birth cohorts					
Schooling	12.6050 (2.4990)	12.8100 (2.4409)	12.4085 (2.5380)	.	.
College degree	0.3140	0.3466	0.2827	.	.
n	964,414	471,704	492,710		
1965-74 Birth cohorts					
Schooling	12.9993 (2.3397)	13.1930 (2.3329)	12.8176 (2.3317)	13.9172 (2.4068)	12.3597 (2.0444)
College degree	0.3418	0.3681	0.3171	0.5153	0.2195
n	1,037,657	502,475	535,182	419,484	592,051
1975-84 Birth cohorts					
Schooling	11.8416 (2.3831)	11.9899 (2.4492)	11.7023 (2.3111)	13.0680 (2.1558)	12.1746 (2.0148)
College degree	0.1701	0.1981	0.1438	0.3177	0.1292
n	944,115	457,486	486,629	362,666	350,195
All birth cohorts (1935-84)					
Schooling	12.0132 (2.7861)	12.1530 (2.7775)	11.8807 (2.7894)	.	.
College degree	0.2666	0.2905	0.2438	.	.
n	4,791,289	2,344,225	2,447,064		

Notes: Standard deviations are in paranthesis. Academic parents means that at least one parent have a long high school degree or higher. 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. Only individuals born 1965-82 have sufficiently high share of non-missing information on parental education to be used in the analysis.

Table A3 Descriptive statistics of labour market outcomes

	All	Females	Males	Academic parents	Non-academic parents
1935-44 Birth cohorts					
Earnings	122,618 (157,390)	98,207 (110,653)	146,933 (189,897)	.	.
Log(Earnings)	12.2913 (0.4220)	12.1398 (0.3422)	12.4201 (0.4416)	.	.
n	787,882	393,694	394,188		
1945-54 Birth cohorts					
Earnings	203,682 (175,120)	168,613 (115,338)	237,474 (212,232)	.	.
Log(Earnings)	12.3906 (0.4014)	12.2400 (0.3305)	12.5304 (0.4107)	.	.
n	1,057,221	518,866	538,355		
1955-64 Birth cohorts					
Earnings	199,734 (179,163)	158,958 (113,371)	238,767 (217,658)	.	.
Log(Earnings)	12.3580 (0.4024)	12.1896 (0.3349)	12.5037 (0.3997)	.	.
n	964,414	471,704	492,710		
1965-74 Birth cohorts					
Earnings	172,300 (137,117)	131,400 (102,690)	210,721 (153,342)	191,465 (164,078)	160,063 (114,425)
Log(Earnings)	12.2927 (0.3601)	12.1414 (0.3266)	12.4001 (0.3441)	12.3628 (0.3923)	12.2429 (0.3271)
n	1,037,657	502,475	535,182	419,484	592,051
1975-84 Birth cohorts					
Earnings	70,271 (82,694)	59,832 (70,124)	80,053 (91,877)	82,235 (85,116)	98,368 (85,165)
Log(Earnings)	11.9510 (0.3056)	11.8959 (0.2660)	12.0030 (0.3261)	12.0019 (0.3161)	12.0094 (0.2929)
n	944,115	457,486	486,629	362,666	350,195
All birth cohorts (1935-84)					
Earnings	153,197 (158,410)	123,018 (111,098)	182,121 (188,816)	.	.
Log(Earnings)	12.2544 (0.4119)	12.1208 (0.3424)	12.3716 (0.4310)	.	.
n	4,791,289	2,344,225	2,447,064		

Notes: Standard deviations are in paranthesis. Academic parents means that at least one parent have a long high school degree or higher. 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. Only individuals born 1965-82 have sufficiently high share of non-missing information on parental education to be used in the analysis. The log earnings are restricted to individuals earning more than 100,000 SEK, which reduces the sample sizes.

Table A4 Specification test: IV estimates of absolute and relative school starting age on parental education, 1975-82 birth cohorts

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
School starting age	0.123 (0.014)	0.089 (0.015)	0.032 (0.019)	0.024 (0.026)	-0.022 (0.042)	0.117 (0.083)	0.058 (0.084)
Relative school starting age						-0.037 (0.085)	-0.033 (0.085)
<i>Control variables:</i>							
Shifted year of birth dummies	X	X	X	X	X	X	X
Date of birth		X	X	X		X	X
(Date of birth) ²			X	X			
Date of birth × above the break-point		X	X	X		X	X
(Date of birth) ² × above the break-point			X	X			X
School fixed effects						X	X
<i>Sample:</i>							
± 6 months from break-point	X	X	X	X		X	X
± 2 weeks from break-point					X		
n	710,676	710,676	710,676	365,962	27,783	710,676	710,676

Notes: School starting age is instrumented with expected school starting age. Relative school starting age is the percentile rank (divided by 100) of the individual's school starting age in the school and year, and is instrumented with the expected school starting age percentile rank. Date of birth is measured in months. 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. Standard errors adjusted for clustering (school and year) are in parentheses. The fourth and fifth columns pertain to men only.

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