DEPARTMENT OF ECONOMICS
JOHANNES KEPLER UNIVERSITY OF LINZ

Early tracking and the misfortune of being young

by

Nicole Schneeweis
Martina Zweimüller*)

Working Paper No. 0911
October 2009

Johannes Kepler University of Linz
Department of Economics
Altenberger Strasse 69
A-4040 Linz - Auhof, Austria
www.econ.jku.at

martina.zweimueller@jku.at
phone +43 (0)70 2468 -5393, -8209 (fax)

Early tracking and the misfortune of being young*

Nicole Schneeweis
University of Linz
NRN Labor & Welfare State

Martina Zweimüller
University of Linz
NRN Labor & Welfare State

September 10, 2012

Abstract

Empirical evidence suggests that the relative age of a student within a grade has a causal effect on educational achievement, and that this effect fades with the duration of schooling. However, if students are separated into different educational tracks early in their schooling career, relative age effects may be preserved. In this paper we estimate the causal effect of relative age on track choice in Austria, a country where students are tracked at the age of ten. We exploit the exogenous variation in month of birth to identify the causal effect of relative age. We find a strong positive effect of relative age on track choice in grades 5–8. We also look beyond grade 8, where students again have to choose between different educational tracks. We find a persistent age effect for students from less favorable socioeconomic backgrounds and students in urban areas.

JEL Classification: I21, I24, I28
Keywords: Educational tracking, school choice, relative age effect, socioeconomic gradient

*Corresponding author: Martina Zweimüller, Johannes Kepler University of Linz, Department of Economics, Altenbergerstr. 69, 4040 Linz, Ph.: +43 70 2468 5393, Fax: +43 70 2468 8209, Email: martina.zweimueller@jku.at. We are grateful to Ludger Wößmann, Guido Schwerdt, Rudolf Winter-Ebmer, Martin Halla, two anonymous referees and (seminar) participants in Stockholm, Munich, Linz, 2008 ESPE, 2009 EALE and 2009 ECER annual meetings for helpful comments and suggestions. The usual disclaimer applies. Financial support by the Austrian Science Fund (NRN S103) is gratefully acknowledged. We thank the municipality of Linz for providing the student register data and funding.

1 Introduction

Although all European countries allocate their students to different educational tracks at some stage of secondary education, in some countries, the decision about which track to attend has to be made at a relatively early stage of the education process, e.g. at the age of ten in Austria and Germany.[1] The early segregation of students into academic and vocational schools is controversial since arguments in favor and against early tracking can be brought forward. One concern is that early tracking may be inefficient because some students end up in the wrong track when track choice is based on a noisy signal of ability (Brunello et al., 2007).

The literature on relative age effects in educational achievement shows that the size of the noise is related to the age at which students are graded or take a test.[2] Younger students perform significantly worse compared to their older peers in the same grade (Bedard and Duhey, 2006; Datar, 2006; Elder and Lubotsky, 2009; Fredriksson and Öckert, 2006; McEwan and Shapiro, 2008). The position of a student in the age distribution within a grade is determined by the school enrollment cutoff date and the birth month of a student. Therefore, the noise can be considered as randomly allocated via birth month. The existing evidence suggests that relative age effects fade with the duration of schooling (Black et al., 2011; Elder and Lubotsky, 2009), but this evidence is based on countries where tracking is postponed to higher grades or students are not tracked into different schools at all.[3] If students are separated into different educational tracks early in their schooling career, relative age effects may be preserved or even reinforced through differences in the rate of human capital accumulation between tracks or if human capital investments exhibit self-productivity and complementarity as suggested by Cunha et al. (2006).

[1]Hungary, the Czech Republic, Slovakia and Turkey track at the age of 11, Belgium and Netherlands at the age of 12 and Luxembourg at the age of 13. All other European countries track their students at the age of 14 to 16 (Brunello and Checchi, 2007).

[2]The concept of "relative age" was first introduced by Allen and Barnsley (1993) and refers to age differences between individuals that are grouped by cohort (based on a specific cutoff date).

[3]U.S. high school students are divided into ability groups within schools based on their prior scholastic performance.

In this paper, we estimate the relative age effect on track choice in Austria, a country where students are first tracked at the age of 10 (in grade 5) and again tracked at the age of 14 (in grade 9). The second tracking may mitigate the relative age effect if students who were assigned to the wrong track because of their age-related disadvantage manage to upgrade to the high track in grade 9. We use register data from a major Austrian city for the period 1984 to 2006 and data from PISA (Programme for International Student Assessment) 2003 and 2006 to estimate whether older students are more likely to attend the high track than their younger peers. Since the observed age of a student is endogenous due to grade retention and the possibility to enroll late or early, we exploit the exogenous variation in birth month to identify the relative age effect on track choice.

After the first tracking (grade 5), the youngest students are almost 40 percent less likely to attend the high track than the oldest students. In grade 9, when the second tracking occurs, we do not find a significant effect on average. These results are in line with recent research on the effects of school entry age on track choice in Germany (Mühlenweg and Puhani, 2010). We contribute to this literature by studying differences between socioeconomic groups and between rural and urban areas. We find that relative age effects differ between groups. The conclusion that younger students fully catch up after the second tracking is not true for all groups. While the relative age effect disappears for children with a more favorable parental background, it even increases for children with a less favorable parental background. Thus, relative age effects reinforce existing socioeconomic inequalities in a system of early tracking.

Comparing urban and rural areas, we find a significant age effect in urban areas, both in grades 8 and 9. In contrast, relative age has no effect in rural areas, neither in grade 8 nor in grade 9. The heterogeneity in the estimated effect between rural and urban areas may be explained by differences in the supply of academic schools for grades 5–8. Because the supply of academic schools is rather low in rural areas, the majority of students (about 80 percent) attend vocational schools until the end of grade 8. In contrast, only about 45 percent of students attend vocational schools in urban areas. If relative age matters less for students at the top and the bottom of the ability distribution and most for students at the middle of the ability distribution, a low supply of academic schools for grades 5–8 may explain the absence of relative age effects in rural areas. Furthermore, the almost comprehensive nature of vocational schools in rural areas implies that the first tracking actually occurs in grade 9. This postponement may be responsible for the absence of relative age effects in grade 9 in rural areas.

The structure of the paper is as follows: Section 2 gives an overview of the related literature. Information on the relevant aspects of the Austrian education system and on the data is given in sections 3 and 4, respectively. In section 5 we outline our identification strategy and discuss potential threats to identification. Section 6 presents our estimates of the relative age effect on track choice after the first and the second tracking, highlights the heterogeneity of the effects between socioeconomic groups and urban and rural areas and adds some sensitivity checks. We also provide some descriptive evidence on potential labor market consequences. Finally, section 7 concludes and discusses the policy implications of our findings.

2 Related literature

This paper is related to the literature on the effects of early tracking on efficiency and equality of opportunity and the literature on relative age effects on school performance and track choice.

In recent work, economists have shown that early tracking reinforces the role of parental background, thereby limiting intergenerational mobility in educational attainment and income (e.g. Bauer and Riphahn, 2006; Brunello and Checchi, 2007; Dustmann, 2004; Hanushek and Wößmann, 2006; Pekkarinen et al., 2009b). Apart from concerns about equality of opportunities, economists have stressed the effects of tracking on overall efficiency. Proponents of educational tracking emphasize that all students benefit from homogenous classrooms, which result from the placement of students into differing-ability schools or classes. They argue that heterogenous classrooms harm gifted students and less talented students alike because teachers may either divide attention among both groups or may adjust teaching to the proficiency level of the median ability student. In such a situation gifted students are not able to unfold their potential while less talented students get discouraged, resulting in lower aggregate achievement. In contrast, tracking students by ability may induce a teacher effect, i.e. teachers are more effective in teaching homogenous classes.

Exploiting a randomized experiment in Kenya, Duflo et al. (2011) provide evidence that tracking primary school students by prior achievement increased test scores of students in high-achievement and low-achievement classes because homogenous classrooms allowed teachers to focus their teaching. However, the authors admit that these results may only be obtained in developing countries, where students are very heterogenous and classes are large. In contrast, developed countries are characterized by smaller classes, lower achievement differences and a higher level of resources. Galindo-Rueda and Vignoles (2004) analyze the gradual abolition of selective grammar schools in the UK. Using political affiliation of the county as an instrument of comprehensive school attendance they find some evidence that high ability students do worse under the comprehensive schooling system and low/middle ability students were not hurt by ability tracking.[4] To the best of our knowledge, there is no other direct evidence for efficiency gains through early tracking in developed countries.

On the contrary, there are studies showing that early tracking is inefficient. Meghir and Palme (1995) find that the introduction of compulsory comprehensive schooling in Sweden induced on average an increase in schooling beyond the compulsory level

[4]These results have been challenged by Pischke and Manning (2006) who show that this identification strategy is not able to remove the selection bias between students attending comprehensive and selective schools.

and an increase in earnings for students with unskilled fathers. The mean effect on earnings for all students is positive but not significant. In a recent study, Pekkarinen et al. (2009a) investigate the impact of the Finnish comprehensive school reform in the 1970s on cognitive skills. The authors find small positive effects on mean achievement in verbal tests as well as positive effects in math for students with low parental education.

The literature on peer effects gives indirect evidence on the optimal allocation of students. If peer effects are non-linear, such that weak students benefit from high-ability students whereas the latter are less or not affected by less favorable peers, heterogenous classrooms should be more efficient as they lead to higher aggregate achievement. In contrast, if high-ability students are more sensitive to peers, aggregate achievement is maximized when classrooms are homogenous. There is mixed evidence from the literature on peer effects, in particular with respect to non-linearities. While some studies show that students from less favorable social backgrounds and low achieving students are most affected by their peers (e.g. Gould et al., 2009; Lavy et al., 2011; Schindler-Rangvid, 2003; Schneeweis and Winter-Ebmer, 2007), other authors did not find any non-linearities (e.g. Ammermüller and Pischke, 2009; Hanushek et al., 2003). Carrell and Hoekstra (2010), on the other hand, found that peers from troubled families strongly impair the cognitive achievement of high income kids and the disciplinary behavior of low income kids.[5]

Overall, there are two channels how tracking could enhance efficiency, either through non-linear peer effects (in favor of high-ability students) or through teacher effects. Both channels suggest that tracking should occur as soon as possible. However, tracking also comes at a cost: Ideally, track choice should be based on a student's innate ability. Actually, a student's ability is unobserved and track choice is based on an imperfect measure of ability, i.e. prior educational achievement (e.g. grades or test scores). Psychologists argue that the correlation between childhood

[5]There is also some evidence for other models of peer effects (e.g. Hoxby and Weingarth, 2006).

and adult intelligence scores is low before grade 4 (Hopkins, 1990), and that at the age of 10 cognitive skills are still developing (Petersen, 1983). Therefore, the cost of tracking is the potential misallocation of students to tracks, and these cost are expected to be higher, the earlier the track choice has to be made.[6] Allen and Barnsley (1993) argue that the misallocation effect stems from the "impossibility of observing ability independent of maturity ..." (p. 649), resulting in achievement differences that are related to birth month.

Since school enrollment is always based on a certain cutoff date, the birth month of a student determines his or her position in the age distribution within a grade or class. Recent research has shown that this position is related to a student's achievement. For example, Bedard and Duhey (2006) show for a number of OECD countries that younger students perform significantly worse than their older peers in grade 4 and 8.[7] However, the estimated effect is a combination of an age-at-test effect and a school-entry-age effect. Using IQ scores at the age of 18, Black et al. (2011) are able to disentangle these effects and find a strong positive age-at-test effect and a small negative effect of starting school one year later. Elder and Lubotsky (2009) show that the age effect tends to be smaller in higher grades.

The main point of our paper is the following: If students are separated into different educational tracks very early, age-related achievement differences probably translate into age-related differences in track choice — irrespective of the exact origin of the age effect. Moreover, if age-related achievement differences are less important in higher grades, early tracking may contribute to their persistence whereas later selection could increase educational attainment and earnings.

Related research has been done by Fertig and Kluve (2005), Jürges and Schneider (2011) and Mühlenweg and Puhani (2010) for Germany. Jürges and Schneider (2011) use data from the German PISA 2000 extension study and show that age at track

[6] Brunello et al. (2007) describe this trade-off in a theoretical model and denote the counteracting effects as "specialization" and "noise" effect.

[7] Similar results were obtained by McEwan and Shapiro (2008) for Chile.

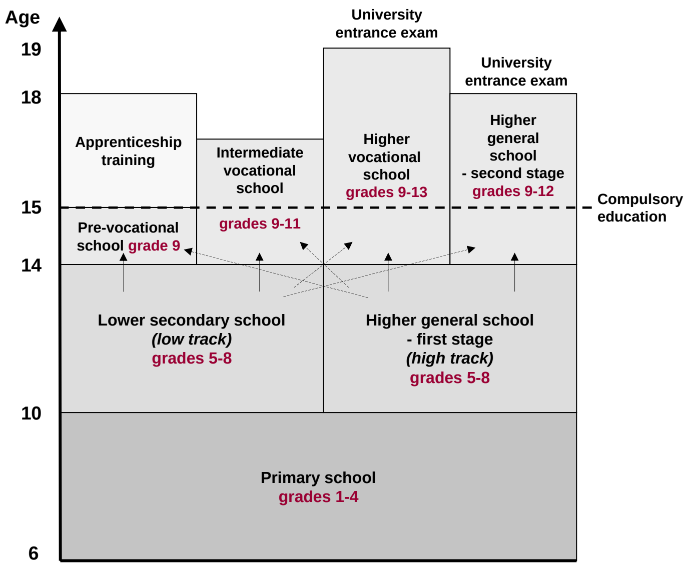
choice has a sizeable positive effect on the probability to attend a high track school in grades 5, 7 and 9. Fertig and Kluve (2005) use survey data and find no significant effect of enrollment age on track choice for students enrolled in the late 1960s and 70s. The study most closely related to our research is the paper by Mühlenweg and Puhani (2010). The authors estimate relative age effects using register data for the state of Hessen. They find that students who are relatively young at school entry are more likely to choose the low track in grade 5, and that this effect partly disappears due to the possibility of track revision after grade 10. Actually, they show that the younger students are more likely to upgrade to the higher secondary but not to the highest secondary track. While the highest track schools provide unrestricted access to all forms of tertiary education, the higher track schools provide unrestricted access to "Universities of Applied Science" only and restricted access (by subject of specialization) to universities.

Our contribution highlights the heterogeneity in the relative age effect between socioeconomic groups and between urban and rural areas. We find that only students with high parental socioeconomic status are able to benefit from the opportunity to revise the initial track choice. In this group younger students are more likely than their older peers to upgrade to a high track school. In contrast, younger students with low parental socioeconomic status are not able to catch up. The age effect even increases because older students are more likely than their younger peers to change from the low track to the high track.

3 Institutional background

The Austrian education system is characterized by early tracking, a multitude of different educational tracks and a strong vocational orientation. Figure 1 gives an overview.

Figure 1: The Austrian education system



Primary school starts at the age of six and takes four years. School enrollment is based on a cutoff date, children are enrolled in a given year if they turn six before September 1 of that year. Children turning six thereafter must delay enrollment by one year. Since children may differ in maturity, these enrollment rules are not strictly enforced, for example children who turn six between September 1 and December 31 may enroll early, if their parents apply for early enrollment, the health officer of the school confirms that the child is mature enough and the primary school principal agrees (*early enrollment*). On the other hand, if it turns out that a six-year-old child is not mature enough, he or she has to attend the pre-primary class instead of the first grade of primary school (*late enrollment*). Furthermore, if a student's achievement is insufficient in more than two subjects he or she has to repeat the grade (*grade retention*).

After primary school, i.e. at the age of 10, students can choose between two types of secondary education. Lower secondary (*low track*) schools comprise grades 5–8, provide basic general education and prepare students for vocational education and training. Higher general (*high track*) schools comprise a first stage (grades 5–8) and a second stage (grades 9–12), provide advanced general education and conclude with a university entrance exam. High track schools offer an academically preferable curriculum, employ teachers with higher qualification and pay higher wages.

Admission to a high track school requires grades of ”very good” or ”good” in the core subjects of the primary school (German writing and reading, mathematics).[8] If these requirements are not met, students have to sit an admission exam. Apart from that, track choice depends on parental choice and non-binding recommendations of primary school teachers. In principle, there is the possibility to switch from the low track to the high track, but depending on their performance, students may have to pass an admission exam. In grades 5–8 upward mobility from the low track to the high track is virtually non-existent due to the more academic curriculum in the high track, but some downward mobility exists.

In grade 9, students again have the possibility to choose between different types of schools: a pre-vocational school, a range of intermediate and higher vocational schools and the second stage of a higher general school. Pre-vocational schools provide the last year of compulsory schooling for those students who intend to pursue an apprenticeship training. Intermediate vocational schools provide professional training and conclude with a final exam after three years. Higher vocational schools additionally provide advanced general education and university entrance qualifications. There are several types of intermediate and higher vocational schools with different professional orientations (e.g. business, technical, teacher training).

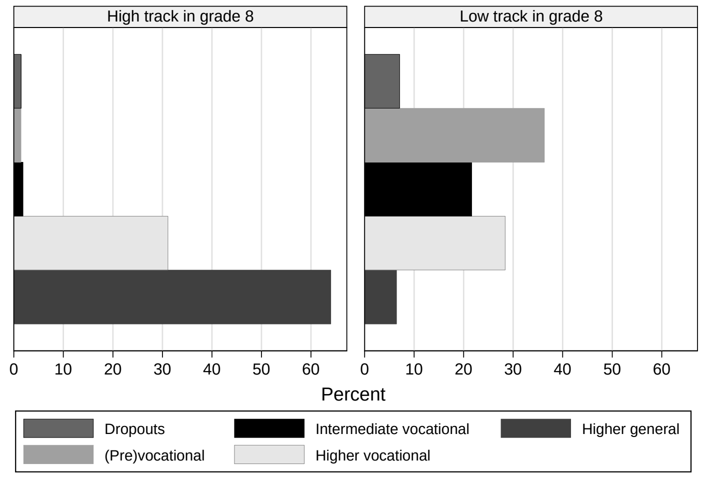
Although first tracking occurs very early, the education system provides some flexibility by allowing students to revise their track decision in grade 9. For example, students from low track schools have the possibility to go for a university entrance qualification by choosing a higher vocational or higher general school (*high track*) in grade 9. Depending on their grades, these students may have to pass an exam to be admitted to a high track school. However, the difference in the quality of education

[8] In Austria, marks from 1 to 5 are used, where 1 means ”very good”, 2 means ”good”, 3 means ”satisfactory”, 4 means ”sufficient” and 5 means ”insufficient”.

between high and low track schools in grades 5–8 hampers the transition to a high track school in grade 9 for low track students.

The majority of Austrian students attend a low track school in grades 5–8, e.g. in the school year 2005/06 about 71 percent of Austrian students attended a low track school in grade 8 (Statistics Austria, various years). Figure 2 shows the transition of those students after grade 8 separately for students from high and low track schools. The school choices of high and low track students are very different. While about 96 percent of high track students choose a track that provides university entrance qualifications, either a higher general school (64 percent) or a higher vocational school (32 percent), only about 36 percent of low track students change to one of these two tracks in grade 9.

Figure 2: Transition of high and low track students after grade 8



Data source: Statistics Austria, Education documentation (Schulstatistik) 2008, Grade 8 students in 2005/06.

4 Data and samples

We analyze two sources of data: administrative student-level data from the city of Linz, the third largest city of Austria with about 190,000 inhabitants and data from two PISA waves.

Student register data. The administrative student-level data cover all resident students who attended grade 5 in public or private (mostly confessional) schools in Linz in the school years 1984/85 to 2001/02.[9] We observe each student until grade 8.[10] The data provides information on some basic individual characteristics of the students (year and month of birth, sex, language) and their school career (school type, school, grade).

Summary statistics are provided in the appendix (panel A of table A.1). Over the whole period, about 45 percent of students attended the high track in grade 5. Because some students changed to the low track, the percentage is somewhat lower in grade 8, indicating that downward mobility is greater than upward mobility. About 81 percent of students enrolled according to the rule, 1 percent enrolled early and 18 percent enrolled late. Late enrollment implies that children attend the pre-primary class of the primary school and enroll in first grade one year later. On average, students are about 10.67 years old (*observed age*) when they make their track choice.[11] According to the school enrollment rule and without grade repetition, students' average age at track choice should be 10.46 years (*assigned age*). The difference between observed and assigned age is due to late enrollment on the one hand and grade repetition on the other hand. Figure 3 shows that the share of students who have enrolled late was highest among students born in August, i.e. children born closely before the cutoff date (1st September). About 50% of all August-born children delay enrollment by at least one year. In contrast, early enrollment is only observed for children born closely after the cutoff date.

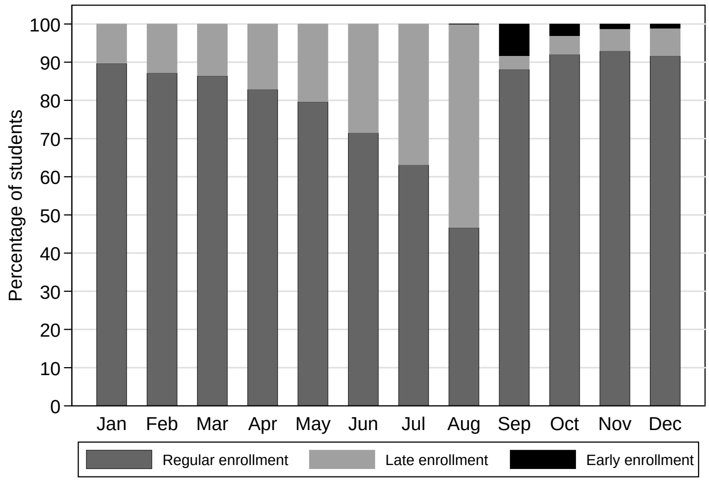
PISA (Programme for International Student Assessment). We use survey data from PISA 2003 and 2006 to estimate the relative age effect in grade 9. PISA does

[9]Our sample consists of 27 public schools and 6 private schools.

[10]For some students we also observe grade 9, but only for those who have not repeated a grade or have not attended a pre-primary class. This is because the single purpose of the data collection is to report 9 years of compulsory schooling which includes repeated grades and pre-primary education.

[11]Actually, students are half a year younger because the track choice is made after the first term of grade 4 because then admission for grade 5 starts.

Figure 3: Regular, late and early enrollment by birth month



Data source: Administrative student-level data for Linz, Grade 5 students from 1984/85 to 2001/02

not sample whole grades, but students born in a certain year. PISA waves 2003 and 2006 cover the birth cohorts 1987 and 1990. Due to birth cohort sampling, students are observed in grade 8, 9 or 10 depending on their birth month, their compliance to the cutoff date rule and whether they have repeated a grade or not. Table 1 shows the composition of our sample. Students born between January and August are predominantly observed in grade 10 (79 percent). They are observed in grade 9 (20 percent) or grade 8 (1 percent) only if they have delayed enrollment and/or have repeated a grade. In contrast, students born between September and December have to delay enrollment from the year they turn six to the year they turn seven. Therefore, the majority of these students is observed in grade 9 (90 percent). 6 percent attend grade 8 because they have either repeated a grade or delayed enrollment and 4 percent attend grade 10 because they have enrolled early.

The Austrian PISA study contains a number of questions on the school career of the students. Students are asked about the school types they have attended, whether they attended pre-primary education, the age at which they entered primary education, the number of years in education so far and how often they have repeated a grade in primary or secondary school. This information allows us to reconstruct the attended school tracks in grades 8 and 9 as well as the students' age at the

Table 1: Composition of the PISA sample

		Grade 8	**Grade 9**	**Grade 10**	**Total**
Jan-Aug born	Percent	1.1	20.2	78.6	100
	N	63	1,118	4,345	5,526
		(late/repeat)	*(late/repeat)*	*(regular)*	
Sep-Dec born	Percent	5.8	89.8	4.4	100
	N	152	2,343	115	2,610
		(late/repeat)	*(regular)*	*(early)*	
Total	Percent	2.6	42.5	54.8	100
	N	215	3,461	4,460	8,136

NOTES: Data from PISA waves 2003 and 2006.

first and second track choice (after grade 4 and 8). Our estimation sample consists of all students observed in grade 8, 9 and 10 for which we can reconstruct these variables.[12]

We do not observe the second track choice for students who are still in grade 8. This group of students is presumably negatively selected with respect to educational achievement, as they either repeated a grade or delayed enrollment (see table 1). Since the majority of these students is born between September and December, excluding those observations from our analysis would lead to a upward bias in the estimated relative age effect. Therefore, we assume that grade 8 students would choose a low track school in grade 9 and keep this group in our estimation sample. By assigning the low track we pursue a conservative strategy and may underestimate the true effect.

A further complication arises because PISA only samples students in educational programmes. Our sample of grade 10 students does not include students who leave the education system after grade 9.[13] The omission of this group probably introduces

[12]We do not have enough information to reconstruct the school career for 2.9 percent of the sample. These observations are excluded from the analysis. A comparison of the share of students in low and high track schools in grade 8 and the transition probabilities in our estimation sample with the official numbers from the federal bureau of statistics shows that our estimations sample is representative for the population of students. Results are available upon request.

[13]There are no official statistics on the percentage of students who leave the educations system after grade 9. Using data from the Austrian Mikrozensus (1-percent sample of Austrian households) for the 1990s, Steiner and Lassnigg (2000) estimate that about 6–8 percent of all grade 9 students did not pursue any further education after grade 9.

a downward bias in our estimates because these students are more likely to be among the younger students (born between January and August) and more likely to have attended the low track in grade 8.

Summary statistics for the PISA sample are presented in the appendix (panel B of table A.1). About 29 percent of Austrian students attend the high track in grade 8. The percentage is lower than in the sample for the city of Linz because of differences in the supply of high track schools for grade 5–8 between urban and rural areas. In grade 9, 56 percent of all students attend a high track school, implying that a considerable number of low track students upgrade to the high track after grade 8.

5 Identification strategy

We want to estimate whether older students are more likely to attend a high track school and whether the effect of relative age is different after the first and the second tracking. A simple econometric model would describe the track choice of a student as follows:

$$\textit{High track}_{ig} = \alpha_{1g} + \alpha_{2g}\,\textit{Observed age}_i \;+ \alpha_{3g}\,X_i + \nu_{ig}$$

$$\textit{High track}_{ig} = \begin{cases} 1 & \text{if } \textit{High track}^*_{ig} > 0 \\ 0 & \text{otherwise} \end{cases}$$

where $\textit{High track}^*_{ig}$ is the latent probability of student i to attend a high track school in grade $g = \{5, 6, 7, 8, 9\}$. We estimate separate regressions for each grade. $\textit{Observed age}_i$ is either the observed age of student i (measured in years) at track choice in grade 5 for $g = \{5, 6, 7, 8\}$ or the observed age at track choice in grade 9 for $g = \{9\}$. We use observed age in grade 5 in the regressions for grades 5–8 because the track choice is actually made after grade 4 and not after each grade. X_i is a vector of student characteristics, ν_{ig} is the error term and α_{2g} measures the effect of relative age on track choice in grade g.

Variation in observed age arises from the following three sources: First, the distribution of births over the calendar year, second, the non-compliance of some students to the school enrollment cutoff date, and third, grade retention. As our data suggest, some students enroll early and thus, are among the youngest within grade, whereas students who enroll late or have repeated a grade are among the oldest within grade. Since we cannot assume that grade retention and non-compliance with the school enrollment rule are exogenous with respect to track choice, a simple probit or linear probability model will give us a biased estimate of the relative age effect (α_{2g}). The estimate is expected to be downward-biased if children who defer enrollment or repeat a grade tend to be negatively selected with respect to cognitive and non-cognitive skills, whereas children who start school early tend to be particularly skilled.

To identify the causal relative age effect, we use only the exogenous variation in observed age at track choice coming from the variation in birth month and the school enrollment cutoff date; i.e. we use the assigned age at track choice as an instrument for observed age.[14] The assigned age is the age a student would have if he or she had not deferred enrollment, had not started school early and had not repeated a grade. The first stage equation for observed age at track choice is the following:

$$Observed\ age_i = \delta_{1g} + \delta_{2g}\, Assigned\ age_i \ + \delta_{3g}\, X_i + u_{ig}$$

where $Assigned\ age_i$ is equal to 0 for students born in August and equal to 1 for students born in September. The difference in assigned age between August-born

[14]This identification strategy was first used by Bedard and Duhey (2006) in their study of relative age effects on test scores for several countries.

and September-born students corresponds to 11 months. The relationship between birth month and assigned age is given by

$$Assigned\ age_i = \begin{cases} \frac{8-b_i}{11} & \text{if} \quad 1 \leq b_i \leq 8 \\ \frac{20-b_i}{11} & \text{if} \quad 9 \leq b_i \leq 12 \end{cases}$$

where b_i is the birth month of student i.

From a policy point of view, we are also interested in the reduced-form relationship between assigned age and track choice. The reduced form or intention-to-treat effect (θ_{2g}) is the effect of the cutoff date rule net of grade retention and late/early enrollment. The equation can be written as:

$$High\ track_{ig} = \theta_{1g} + \theta_{2g}\, Assigned\ age_i \ + \theta_{3g}\, X_i + \epsilon_{ig}$$

We interpret our results in a heterogenous treatment effects framework (Angrist et al., 1996), implying that we can only identify the causal effect of relative age for compliers (LATE). Compliers are students who are among the oldest within grade only because they are born after the cutoff date (e.g. in September) and would be among the youngest if they were born before the cutoff date (e.g. in August).

Identification is based on the following assumptions. First, the instrument must be randomly assigned. The random assignment assumption requires that a student's birth month is random and not related to other (unobserved) determinants of academic achievement, in particular student ability and parents' socioeconomic background. At least, we have to assume that parents do not schedule births to fall either before or after the cutoff date. If, for example, high-ability parents are more likely to have their children in September than in August, the estimated age effect would be upward-biased. Second, the instrument must not have any other direct effect on track choice (exclusion restriction).

Since our administrative data contains no information on parental characteristics, we cannot check whether there are any parental differences between August-born and September-born students. However, we are confident that birth month is a valid instrument because the need for birth timing is less obvious in a system where the cutoff date rule is not strictly enforced (late and early enrollment is possible). As a robustness check, we include quarter of birth in our regressions and show results for a restricted sample including only students born in August and September. In the PISA sample we control for an index of parental socioeconomic status and highest parental education. The distribution of parental education and the index of parental socioeconomic status across birth months (see figure A.1 in the appendix) does not indicate a clear pattern of seasonality. Furthermore, table A.2 in the appendix presents estimates from separate regressions of the instrument on background characteristics, such as student's sex, immigrant background, residential neighborhood or socioeconomic background. Overall, we do not find significant evidence for a violation of the random assignment assumption.

Third, while the instrument may not affect some students (always-takers and never-takers), all students who are affected by the instrument must be affected in the same direction (monotonicity). The monotonicity assumption implies that no one does the opposite of his or her assignment, i.e. there are no defiers. If monotonicity is violated, IV estimates an average of the effect for compliers and the effect for defiers. If those effects differ, IV yields a biased estimate of the causal effect for compliers. The bias depends on the proportion of defiers and the difference between the effect for compliers and the effect for defiers (Angrist et al., 1996).

In our context, the monotonicity assumption is not fulfilled because our treatment variable is multi-valued and the instrument shifts the age of all students.[15] This implies that always-takers and never-takers do not exist and the instrument does not affect all students in the same way. According to the school enrollment rule,

[15]See Barua and Lang (2009) for a discussion. Most previous studies on relative age effects on track choice have not acknowledged this problem.

being born in September should make a student 11 months older at track choice than she would be if she were born in August. However, some August-born students enroll one year later than they should, which makes them 1 month older than they would be if they were born in September and had enrolled according to the rule. While these students would be always-takers if we had a binary treatment, in our setting they are defiers. A similar argument can be made with respect to grade repeaters and students who enroll early. The violation of the monotonicity assumption implies that IV yields an unbiased estimate of the LATE for compliers only if the relative age effect is linear and homogenous for compliers and defiers.

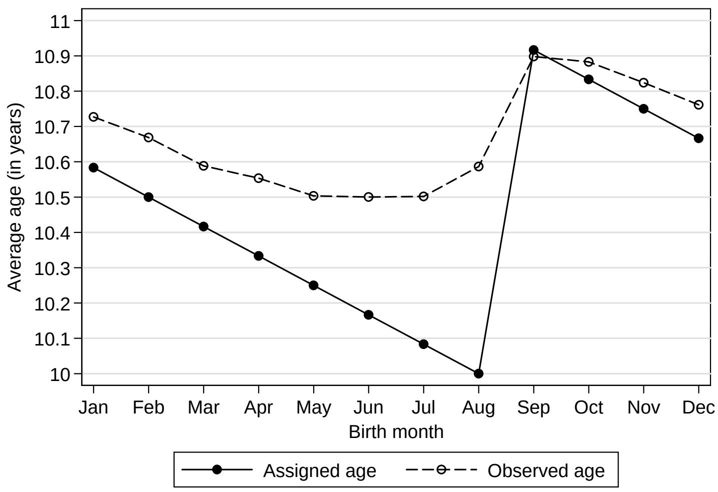
We propose the following solution to this problem: We provide a sensitivity test where we redefine our treatment and assignment variables such that the monotonicity assumption is fulfilled. More details are provided in section 6.3.

6 Results

Based on our administrative data for Linz, we first present some descriptive evidence for the existence of the first stage, i.e. the relationship between assigned and observed age and for a positive relationship between assigned age and the probability to attend a high track school (reduced form).

Figure 4 plots the assigned age (solid line) and the observed age (dashed line) at track choice in grade 5 by birth month. Students born in August should be 10 years old when making their track choice, whereas students born in September should be 11 months older. The deviation of observed age from assigned age is due to non-compliance with the cutoff date rule and grade retention in primary school. As expected, the highest deviation is found for students born closely before the cutoff date because these students are more likely to enroll late than students born after the cutoff date. Nevertheless, there is still a clear discontinuity in observed age at track choice between August-born and September-born students.

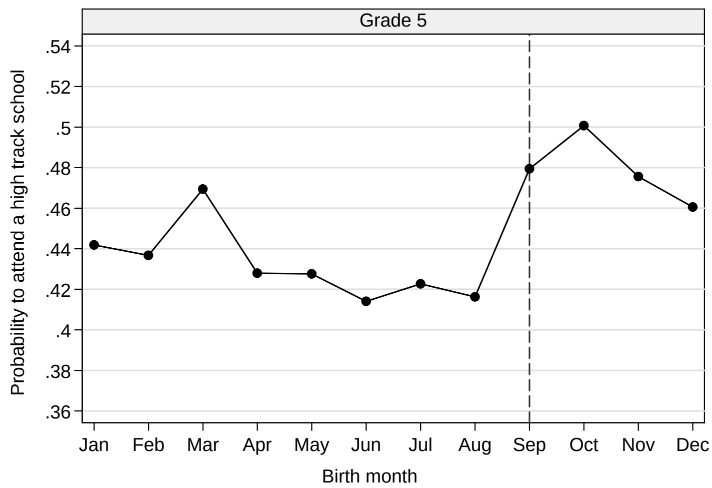
Figure 4: Assigned age and observed age by birth month



Data source: Administrative student-level data for Linz, Grade 5 students from 1984/85 to 2001/02

The reduced-form relationship between birth month (assigned age) and the probability to attend a high track school, displayed in figure 5, shows a similar pattern. September-born students are not only older when they make their track choice, they are also more likely to attend a high track school in grade 5. Even though a considerable number of August-born students enroll late, which increases their age at track choice, these students are still significantly less likely to choose a high track school in grade 5.

Figure 5: Attendance of high track school by birth month



Data source: Administrative student-level data for Linz, Grade 5 students from 1984/85 to 2001/02

6.1 Baseline results: Administrative data

Table 2 presents our baseline IV-Probit estimates of the relative age effect on the probability to attend a high track school in grades 5–8. The table also shows results from first-stage, reduced-form and probit estimations for each grade. Each number represents a single regression including binary indicators for female and foreign students and school year dummies as control variables.[16]

The first-stage estimates and the F-statistics show that assigned age seems to be a strong instrument, in the sense that it is sufficiently correlated with observed age. The IV-Probit estimate for grade 5 suggests that being 11 months older at track choice increases a student's probability to attend a high track school by 17.5 percentage points, which is a substantial effect given that on average about 45 percent of students attend a high track school in grade 5.[17] Table 2 also reports results for grades 6–8 to show how the relative age effect evolves over grades. Given that mobility between school tracks is possible, we would expect that the effect diminishes over grades if small differences in age become less and less important as the school career progresses. Due to the more academic curriculum and formal admission requirements in high track schools, upgrading from the low track to the high track is more difficult and more unusual than downgrading from the high track to the low track. If students perform poorly in the high track they can change to the low track without any formal requirements. The point estimate of the relative age effect for grade 8 is only 2 percentage points lower than the estimate for grade 5, suggesting that the education system provides no "self-correcting mechanism" that leads to a correct allocation of talents until grade 8. In the last column of table 2 we restrict our sample to students born between 1987 and 1990. The estimates for this sample are more comparable to the estimates for the PISA sample (presented below) which contains only students born in 1987 and 1990.

[16]The results are not sensitive to the exclusion of these variables.

[17]The estimated coefficients from linear probability models are similar to the coefficients presented above.

Table 2: Administrative data: Track choice in grades 5–8

	All cohorts				Cohorts 1987-1990
	Grade 5	**Grade 6**	**Grade 7**	**Grade 8**	**Grade 8**
IV-Probit[a]	0.175***	0.165***	0.160***	0.154***	0.234***
	(0.021)	(0.021)	(0.021)	(0.021)	(0.054)
First stage[b]	0.423***	0.423***	0.423***	0.423***	0.325***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.018)
	[2319]	[2319]	[2319]	[2319]	[310]
Reduced form[c]	0.077***	0.072***	0.068***	0.065***	0.081***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.021)
Probit[d]	-0.237***	-0.241***	-0.240***	-0.238***	-0.231***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.015)
Observations	25,248	25,248	25,248	25,248	5,591
% in high track	45%	44%	42%	41%	41%

NOTES: Administrative student-level data for the city of Linz. The "all cohorts" sample consists of all students observed in grade 5 between 1984-2001. The same students are observed in grades 6–8. These students are born between 1972 and 1992. The 1987-1990 cohort sample includes only students born between 1987 and 1990. Robust standard error in parentheses. ***, ** and * indicate significance at the 1-percent, 5-percent and 10-percent level. Control variables are: sex, immigrant background and year dummies.
[a]Probit instrumental variables regressions of a binary indicator for attending a high track school on observed age at track choice (grade 5), instrumented by assigned age.
[b]Ordinary least squares regressions of observed age at track choice on assigned age. F-statistics on the excluded instrument in brackets. The first stage is the same for all grade-level estimations.
[c]Probit regressions of a binary indicator for attending a high track school on assigned age.
[d]Probit regressions of a binary indicator for attending a high track school on observed age at track choice.

The reduced-form estimates show the net impact of the cutoff date rule on the probability to attend a high track school. Being born in September instead of August leads to a 7.7 (6.5) percentage points difference in the probability to attend a high track school in grade 5 (8). The reduced-form estimates are lower because non-compliance partly offsets the disadvantage created by the education system for children born closely before the cutoff date; for example delaying enrollment by one year and attending a pre-primary class instead may help those students to compensate for their initial disadvantage.

As expected, the estimated parameter from a simple probit model that ignores the endogeneity of observed age is downward-biased. Actually, the estimates for all grades are even negative, suggesting that students who are older because they enrolled late or repeated a grade in primary school are negatively selected with respect to cognitive skills.

6.2 Baseline results: PISA data

Do students who were assigned to the wrong track because of their age-related disadvantage manage to upgrade to a high track school in grade 9? Since upward mobility is common after grade 8 (see figure 2), the second tracking may lead to a correct allocation of talents. We would expect that the relative age effect disappears, if students who ended up in the low track because they were young at track choice can compensate for their initial disadvantage and are more likely to change to the high track in grade 9 than their older peers. We use survey data from PISA 2003 and 2006 to test whether such a “self-correcting mechanism” exists after grade 8.[18]

Table 3 presents our estimates of the relative age effect on the probability to attend a high track school in grade 8 and grade 9. Each number represents a single regression including binary indicators for female and foreign students and a pisa wave dummy as control variables.[19] Based on the whole sample of Austrian students (columns (1) and (2)) we find that the probability to attend a high track school in grade 8 is 12.4 percentage points higher for students being 11 months older at track choice in grade 5. After the second tracking in grade 9, we find a relative age effect of 7.2 percentage points which is significant only at the 13 percent level. These results are in line with recent research on the effects of relative age on track choice in Germany (Jürges and Schneider, 2011; Mühlenweg and Puhani, 2010). However, as columns (3)-(10) of table 3 show we also find that the relative age effect differs between socioeconomic groups and between urban and rural areas.

We use the ISEI score (International Socio-Economic Index of Occupational Status, Ganzeboom et al. (1992)) to divide our sample by the socioeconomic status of the students’ parents. The score varies between 16 and 90 and has a mean of 48 (median: 46). The high SES sample includes all students who have at least one parent

[18]Our register data do not cover the transition after grade 8 for students who have repeated a grade or attended a pre-primary class. We cannot use these data to investigate grade 9 because whether we observe a student in grade 9 is correlated with age and birth month.

[19]The results are not sensitive to the exclusion of these variables.

with a score of 49 or higher. The IV estimate of the relative age effect in grade 8 is remarkably similar for students from different socioeconomic backgrounds. In both groups, younger students are about 12 percentage points less likely to attend a high track school than their older peers. After the second tracking in grade 9, the relative age effect disappears for students from more favorable socioeconomic backgrounds. For students from less favorable backgrounds, in contrast, it seems that the second tracking reinforces the relative age effect.

Table 4 shows whether it is track upgrading or track downgrading that explains the observed changes in the relative age effects from grade 8 to grade 9. We differentiate between upgrading to (downgrading from) the high track (including higher general and higher vocational schools) and upgrading to (downgrading from) the higher general school in order to show which school type is responsible for the observed changes. The absence of a relative age effect for students with more favorable parental backgrounds in grade 9 is mainly due to disproportional upgrading of younger low track students to high track schools (13.4 percentage points) and a somewhat higher likelihood of older high track students to downgrade to the low track (1.7 percentage points). Since we do not find any effect when only higher general schools are considered as high track schools, we can conclude that these students almost exclusively upgrade to higher vocational schools and not to higher general schools.[20] This result is similar to Mühlenweg and Puhani (2010), however holds only for students with high parental socioeconomic status. The increase in the relative age effect for students from less favorable parental backgrounds is due to older students being 13.5 percentage points more likely to change from the low track to the high track in grade 9. Again, the change is due to upgrading to higher vocational schools. In addition to having a lower likelihood of attending the high track in grades 5–8, younger students with low parental socioeconomic status also have a lower likelihood of upgrading to the high track in grade 9.

[20]Regressions based on these different definitions of high track schools (including and excluding higher vocational schools) confirm this finding. These results are available upon request.

Table 3: PISA sample: Track choice in grade 8 and 9

	All students		**Low SES**		**High SES**		**Urban areas**		**Rural areas**	
	Grade 8	**Grade 9**	**Grade 8**	**Grade 9**	**Grade 8**	**Grade 9**	**Grade 8**	**Grade 9**	**Grade 8**	**Grade 9**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
IV-Probit[a]	0.124***	0.072	0.120**	0.212***	0.122**	-0.033	0.244***	0.184***	0.020	-0.029
	(0.039)	(0.047)	(0.054)	(0.061)	(0.050)	(0.045)	(0.053)	(0.059)	(0.046)	(0.073)
First stage[b]	0.450***	0.473***	0.392***	0.414***	0.508***	0.531***	0.420***	0.446***	0.486***	0.506***
	(0.019)	(0.019)	(0.030)	(0.031)	(0.024)	(0.024)	(0.025)	(0.025)	(0.031)	(0.030)
	[521]	[556]	[161]	172.7	[447]	[481]	[272]	[301]	[246]	[265]
Reduced form[c]	0.056***	0.036	0.046**	0.093***	0.063**	-0.014	0.109***	0.092***	0.009	-0.018
	(0.018)	(0.023)	(0.021)	(0.030)	(0.026)	(0.024)	(0.026)	(0.030)	(0.022)	(0.037)
Probit[d]	-0.072***	-0.241***	-0.023*	-0.207***	-0.072***	-0.221***	-0.100***	-0.235***	-0.039***	-0.255***
	(0.013)	(0.016)	(0.013)	(0.019)	(0.020)	(0.018)	(0.018)	(0.020)	(0.013)	(0.026)
Observations	8,136	8,136	3,964	3,964	3,940	3,940	4,508	4,508	3,628	3,628
% in high track	29%	56%	15%	40%	44%	73%	39%	63%	17%	47%
Mean SES	48.13	48.13	34.85	34.85	61.35	61.35	50.05	50.05	45.82	45.82
St.Dev. SES	0.32	0.32	0.15	0.15	0.21	0.21	0.43	0.43	0.49	0.49

NOTES: Data from PISA waves 2003 and 2006. Robust standard error (in parentheses) account for complex survey design. ***, ** and * indicate significance at the 1-percent, 5-percent and 10-percent level. Control variables are: sex, immigrant background and a pisa wave dummy. The urban (rural) sample includes only students whose school is located in a municipality with more (less) than 15.000 inhabitants. The low (high) SES sample includes students with an index of parental socioeconomic status that is lower (higher) than the sample mean. The index is missing for 3% of the sample.

[a]Probit instrumental variables regressions of a binary indicator for attending a high track school on observed age at track choice, instrumented by assigned age.

[b]Ordinary least squares regressions of observed age at track choice on assigned age. F-statistics on the excluded instrument in brackets. The first stage is the same for all grade-level estimations.

[c]Probit regressions of a binary indicator for attending a high track school on assigned age.

[d]Probit regressions of a binary indicator for attending a high track school on observed age at track choice.

Table 4: Upgrading and downgrading after grade 8

	All students	**Low SES**	**High SES**	**Urban areas**	**Rural areas**
Panel A: Upgrading					
to high track[a]	-0.024	0.135**	-0.134***	0.004	-0.057
	(0.042)	(0.063)	(0.047)	(0.054)	(0.064)
to higher general school[b]	-0.014	-0.001	-0.015	-0.013	-0.007
	(0.014)	(0.024)	(0.020)	(0.021)	(0.021)
Panel B: Downgrading					
to low track[c]	0.022**	0.021	0.017**	0.046**	-0.002
	(0.009)	(0.016)	(0.008)	(0.020)	(0.014)
to low track or	0.073**	0.039	0.089**	0.138***	0.010
higher vocational school[d]	(0.030)	(0.042)	(0.038)	(0.044)	(0.034)
Observations	8136	3964	3940	4508	3628

Notes: See notes in table 3 for description of estimation sample. All estimations are probit instrumental variables regressions of different binary indicators for upgrading and downgrading in grade 9 on observed age at track choice in grade 5, instrumented by assigned age. The high track includes higher general and higher vocational schools, the low track includes prevocational and intermediate vocational schools.
[a]Binary indicator that is one for students who change from the low track to the high track, and zero otherwise. High track students in grade 8 are included although they cannot upgrade.
[b]Binary indicator that is one for students who change from the low track to the higher general school, and zero otherwise. High track students in grade 8 are included although they cannot upgrade.
[c]Binary indicator that is one for students who change from the high track to the low track, and zero otherwise. Low track students in grade 8 are included although they cannot downgrade.
[d]Binary indicator that is one for students who change from the high track to the higher vocational school or the low track, and zero otherwise. Low track students in grade 8 are included although they cannot downgrade.

Columns (7)-(10) of table 3 present results from separate regressions for schools located in urban and rural areas. The classification is based on the location of the school attended in grade 9, i.e. we assume that students who attended a rural (urban) school in grade 9 also attended a rural (urban) school in grade 8. Urban areas are defined as municipalities with more than 15.000 inhabitants.[21] We find no relative age effect on the probability to attend a high track school for students who attend schools in rural areas, neither in grade 8 nor in grade 9. In urban areas, older students are 18.4 percentage points more likely to attend the high track than younger students in grade 9. In grade 8, the relative age effect amounts to 24.4 percentage points in urban areas. Note that the result for grade 8 in the urban sample is comparable to the estimated effect for the city of Linz presented in the last column of table 2. As table 4 shows, the decrease in the relative age effect between

[21]In 2001, Austria was divided into 2,359 municipalities, 32 of which had more than 15,000 inhabitants.

grade 8 and grade 9 in urban areas is due to older students being 4.6 percentage points more likely than younger students to downgrade from the high track to the low track. These students are also more likely to change from the higher general to the higher vocational track.

The heterogeneity in the estimated effect between rural and urban areas can be explained by a considerable lower supply of high track schools for grades 5–8 in rural areas. While about 80 percent of students in rural areas attend low track schools until grade 8, only about 45 percent of students in urban areas attend the low track.[22] Given that high ability students are more likely to attend the high track, a low supply of high track schools implies that most students at the middle and the bottom of the ability distribution will attend a low track school. If relative age matters more for students at the middle of the ability distribution than for students at the top or the bottom, a low supply of high track schools may be the reason for the absence of the relative age effect in grade 8 in rural areas.

A low supply of high track schools also implies that the average ability of low track students in grades 5–8 is higher and that the students' peer groups are more heterogenous compared to a situation were the supply of high track schools is high. Rural low track schools are almost similar to comprehensive schools which implies that the first tracking actually occurs in grade 9. This postponement may be responsible for the absence of relative age effects in grade 9 in rural areas. In contrast, in urban areas, where almost 50 percent of all students attend a high track school until grade 8, early tracking leads to a persistent age effect.[23]

While there is some concern that we misclassify some of the students because we only know the location of the school a student attended in grade 9, we are confident that the classification error cannot explain the magnitude of our estimates,

[22]Statistics Austria (various years). Own calculations for the school year 2009/2010.

[23]One may object that comprehensive schools are more efficient only in rural areas because students are more homogenous there. In fact, we find the opposite pattern; the standard deviation of the index of parental socioeconomic status is higher for students in rural than in urban areas (see table 3).

in particular the difference in the effects for urban and rural areas. Our results would be biased if either older rural students were more likely than younger rural students to change to high track schools in urban areas, or younger rural students were more likely than older rural students to change to low track schools in urban areas. If one of these hypotheses were true, we would expect differences in the distribution of students across birth months between the urban and the rural sample. We find no evidence that the proportion of older students is higher in the urban sample.

6.3 Sensitivity analysis

This section provides sensitivity tests with respect to the random assignment assumption and the monotonicity assumption.

Random assignment. Based on our administrative data we perform two sensitivity tests to show that we are not confounding the causal effect of relative age with season of birth effects. Recent research suggests that season of birth might be correlated with family background. Buckles and Hungerman (2008) find for the U.S. that children born in the first quarter are more likely to have a less favorable family background. To meet these concerns, table 5 presents results for a restricted sample and from regressions with quarter of birth dummies as additional control variables. The restricted sample includes only students born closely before and after the cutoff date, i.e. students born in August or September. The estimated coefficients from the restricted sample are very similar to the coefficients from the sample including all birth months. Furthermore, including quarter of birth dummies does not significantly change our results. Since our PISA data provides information on parental characteristics, we re-estimate our baseline model augmented by control variables for the highest parental socioeconomic status (ISEI, Ganzeboom et al., 1992) and the parents' highest education level (measured in ISCED categories, UNESCO, 2006).

Results do not differ significantly, indicating that our instrument is not correlated with parental background.[24]

Table 5: Sensitivity checks: Random assignment

	Administrative data[a]				*Pisa data*[b]	
	Restricted sample		**+ Quarter of birth**		**+ SES & educ**	
	Grade 5	**Grade 8**	**Grade 5**	**Grade 8**	**Grade 8**	**Grade 9**
IV-Probit[c]	0.196***	0.169***	0.155***	0.126***	0.140***	0.088*
	(0.043)	(0.044)	(0.031)	(0.032)	(0.040)	(0.047)
First stage[d]	0.309***	0.309***	0.380***	0.380***	0.448***	0.471***
	(0.015)	(0.015)	(0.012)	(0.012)	(0.019)	(0.019)
	[457.5]	[457.5]	[2319]	[2319]	[521]	[556]
Reduced form[e]	0.065***	0.055***	0.061***	0.047***	0.062***	0.042*
	(0.015)	(0.015)	(0.013)	(0.013)	(0.018)	(0.024)
Observations	4,263	4,263	25,248	25,248	8,136	8,136

NOTES: Robust standard error in parentheses. ***, ** and * indicate significance at the 1-percent, 5-percent and 10-percent level.
[a]Administrative student-level data for the city of Linz. The sample consists of students observed in grade 5 between 1984-2001. The restricted sample includes only students born in August or September. The same students are observed in grades 6–8. Control variables are: sex, immigrant background and year dummies.
[b]Data from PISA waves 2003 and 2006. Control variables are: sex, immigrant background, pisa wave dummy, an index of parental socioeconomic status (SES) and parents' highest education level (educ).
[c]IV-Probit estimation of a binary indicator for attending a high track school on observed age at track choice, instrumented by assigned age.
[d]Ordinary least squares estimation of observed age at track choice on assigned age. F-statistics on the excluded instrument in brackets.
[e]Probit estimation of a binary indicator for attending a high track school on assigned age.

Monotonicity. As discussed in section 5, our instrument does not affect all students in the same way, i.e. the monotonicity assumption is violated. We suggest the following sensitivity check: We redefine our treatment and assignment variable such that the monotonicity assumption is fulfilled and analyze whether our results show the same patterns as those from our baseline regressions.

Our redefined assignment variable (*old*) is a binary variable that is equal to one if a student is born within six months after the cutoff date (between September and February) and zero otherwise. The first group includes students who would be among the older students within grade if they followed the school enrollment rule. In contrast, students born between March and August would be among the younger students. Similarly, we recode our treatment variable (*among the older*) to a binary

[24]See als table A.2 in the appendix for results from regressions of background characteristics on the instrument.

variable that equals one for students who actually are among the older students, i.e. students who are at least 10.5 years at the first tracking and at least 14.5. years old at the second tracking. For the redefined variables the monotonicity assumption is fulfilled if there is no single student who would enroll early if born in September, but would enroll late (or repeat a grade) if born in August. In other words, defiers are students who would make their track choice for grade 5 in the year they turn 9 if born in September, but would delay their track choice to the year they turn 11 if born in August. Since this behavior seems unlikely, we are confident that the binary instrument fulfills the monotonicity assumption.

Table 6 presents IV, reduced form and first stage estimates from linear probability models. The interpretation of the coefficients is different compared to our baseline estimates that use assigned age as an instrument for observed age.[25] Our baseline estimates show the effect of an age difference of 11 months on the probability to attend the high track. In contrast, the coefficients presented below are based on the comparison of the group of older students with the group of younger students. Thus, the estimated age effect corresponds to an average age difference of 6 months.

Although the magnitude of the estimated effects is not comparable to our baseline results, the conclusions are the same. Older students are 5.3 percentage points more likely than younger students to choose a high track school in grade 5. In the sample of all Austrian students, the relative age effect amounts to 6.3 percentage points in grade 8 and 3.6 percentage points in grade 9 (significant at the 10-percent level). Comparing urban and rural areas and students from more favorable and less favorable parental backgrounds, we find the same patterns as in our baseline regressions presented in tables 2 and 3.[26]

[25]Note that our baseline estimates are obtained from probit instrumental variables models. Results from linear probability models are similar.

[26]Results from regressions that use *assigned age* as an instrument for being among the older students are somewhat larger but show the same pattern as the estimates presented in table 6.

Table 6: Sensitivity checks: Monotonicity

	Administrative data[a]		*PISA data*[b]									
	Linz		**All students**		**Low SES**		**High SES**		**Urban areas**		**Rural areas**	
	Grade 5	**Grade 8**	**Grade 8**	**Grade 9**	**Grade 8**	**Grade 9**	**Grade 8**	**Grade 9**	**Grade 8**	**Grade 9**	**Grade 8**	**Grade 9**
IV[c]	0.053***	0.045***	0.063***	0.036*	0.056***	0.087***	0.066***	-0.007	0.109***	0.084***	0.020	-0.012
	(0.009)	(0.009)	(0.017)	(0.020)	(0.020)	(0.028)	(0.023)	(0.021)	(0.024)	(0.026)	(0.020)	(0.030)
FS[d]	0.669***	0.669***	0.668***	0.665***	0.627***	0.627***	0.716***	0.710***	0.644***	0.637***	0.698***	0.701***
	(0.004)	(0.004)	(0.010)	(0.010)	(0.014)	(0.014)	(0.012)	(0.012)	(0.012)	(0.012)	(0.015)	(0.015)
	[24786]	[24806]	[4880]	[4673]	[1936]	[1895]	[3567]	[3490]	[2685]	[2732]	[2058]	[1997]
RF[e]	0.035***	0.030***	0.042***	0.024*	0.035***	0.055***	0.047***	-0.005	0.070***	0.053***	0.014	-0.008
	(0.006)	(0.006)	(0.011)	(0.013)	(0.013)	(0.018)	(0.017)	(0.015)	(0.015)	(0.017)	(0.014)	(0.021)
Obs.	25,248	25,248	8,136	8,136	3,964	3,964	3,940	3,940	4,508	4,508	3,628	3,628

NOTES: Robust standard error in parentheses. ***, ** and * indicate significance at the 1-percent, 5-percent and 10-percent level.
[a]Administrative student-level data for the city of Linz. The sample consists of students observed in grade 5 between 1984-2001. The same students are observed until grade 8. Control variables are: sex, immigrant background and year dummies.
[b]Data from PISA waves 2003 and 2006. Control variables are: sex, immigrant background and a pisa wave dummy. The urban (rural) sample includes only students whose school is located in a city with more (less) than 15.000 inhabitants. The low (high) SES sample includes students with an index of parental socioeconomic status that is lower (higher) than the sample mean.
[c]Two-stage least squares regression of a binary indicator for attending a high track school on a binary indicator (*'among the older'*) that is one for students who are among the older students and zero otherwise, instrumented by a binary indicator (*'old'*) that is one for students who are born within six months after the cutoff date and zero otherwise.
[d]Ordinary least squares regression of a binary indicator (*'among the older'*) that is one for students who are among the older students and zero otherwise on a binary indicator (*'old'*) that is one for students who are born within six months after the cutoff date and zero otherwise. F-statistics on the excluded instrument in brackets.
[e]Ordinary least squares regression of a binary indicator for attending a high track school on a binary indicator (*'old'*) that is one for students who are born within six months after the cutoff date and zero otherwise.

6.4 School tracks and labor market outcomes

We have shown that relative age is an important predictor of track choice in Austria. Relatively younger students are more likely to end up in low track schools during grades 5–8. After the second tracking in grade 9, younger students do not fully catch up. We find substantial age effects for students from lower socioeconomic backgrounds and for urban areas.

The educational disadvantage of relatively younger students most likely perpetuates to the labor market. To the best of our knowledge, there are no studies investigating the causal effect of holding a certificate of a high track school or a university education on labor market outcomes in Austria or Germany. However, descriptive evidence shows that higher school certificates are associated with higher earnings on the labor market in Austria (Fersterer and Winter-Ebmer, 2003) and Germany (Dustmann, 2004; Mühlenweg and Puhani, 2010).

We present additional descriptive evidence for Austria in table 7. We run weighted OLS regressions for 25–55 year-old individuals, using consistent cross-sections of the Mikrozensus 1991, 1993, 1995 and 1997. The Mikrozensus is a 1-percent representative sample of the Austrian population and includes information on education, labor market status and net earnings. Education is measured by the highest educational qualifications (compulsory schooling, apprenticeship training, intermediate vocational school, higher vocational school, higher general school and university education).

Table 7 presents regression results of labor market outcomes on educational attainment. First, we construct a dummy variable for all high track schools (panel A)[27], second, we investigate the different types of high track education separately (panel B) and finally, we distinguish between individuals living in rural and urban areas (panel C).

[27]Since only the high track schools (higher vocational and general schools) conclude with a university entrance exam, we assume that individuals holding a university education have attended a high track school.

High track schools are positively associated with labor market outcomes, such as increased employment, a lower risk of unemployment and higher wages. On average, the earnings of workers with high track education are roughly 30 percent higher compared to workers with low track education. As can be seen in panel B, most of these results are found for all types of high track education, although largest for individuals holding a university education, their wages are 36–40 percent higher. When comparing rural and urban areas, we find the returns to high track education to be significantly higher in urban areas.

Overall, the returns to high track education are very high in Austria. The educational disadvantage of younger students in urban areas and from lower socioeconomic backgrounds are likely to have major consequences for their life time earnings.

Table 7: School tracks and labor market outcomes

	Employed		**Unemployed**		**Hourly earnings**[a]		**Monthly earnings**[b]	
	Males	Females	Males	Females	Males	Females	Males	Females
Panel A								
High track school[c]	0.038***	0.137***	-0.014***	-0.012***	0.301***	0.307***	0.316***	0.315***
	(0.003)	(0.007)	(0.002)	(0.002)	(0.008)	(0.009)	(0.008)	(0.008)
Part-time							-0.481***	-0.529***
							(0.033)	(0.008)
Panel B								
Higher voc school	0.039***	0.133***	-0.013***	-0.014***	0.278***	0.275***	0.281***	0.281***
	(0.005)	(0.010)	(0.003)	(0.003)	(0.012)	(0.014)	(0.011)	(0.012)
Higher general school	0.021***	0.080***	-0.007	-0.010**	0.219***	0.259***	0.223***	0.253***
	(0.007)	(0.012)	(0.005)	(0.004)	(0.018)	(0.015)	(0.017)	(0.015)
University	0.045***	0.189***	-0.018***	-0.011***	0.370***	0.369***	0.402***	0.392***
	(0.004)	(0.009)	(0.003)	(0.003)	(0.012)	(0.013)	(0.013)	(0.012)
Part-time							-0.484***	-0.526***
							(0.033)	(0.008)
Observations	48,480	50,188	48,480	50,188	25,088	17,225	25,088	17,225
Panel C								
High track*rural[d]	0.044***	0.125***	-0.022***	-0.021***	0.273***	0.258***	0.289***	0.270***
	(0.004)	(0.009)	(0.002)	(0.003)	(0.011)	(0.012)	(0.011)	(0.011)
High track*urban	0.032***	0.144***	-0.008***	-0.006**	0.320***	0.336***	0.334***	0.343***
	(0.004)	(0.008)	(0.003)	(0.003)	(0.011)	(0.011)	(0.011)	(0.011)
Part-time							-0.483***	-0.529***
							(0.034)	(0.008)
Observations	48,312	50,085	48,312	50,085	25,088	17,225	25,088	17,225

NOTES: Data are drawn from the Austrian Mikrozensus of the years 1991, 1993, 1995 and 1997. The sample consists of all 25-55 year-old individuals who are not in school or in an apprenticeship training and who reported their employment status/hourly wages. Coefficients of weighted linear probability models for Employed and Unemployed and weighted least squares regressions for Earnings reported (sampling weights used). Controls for age and age squared included in all regressions, an indicator for part-time work (<35 hours per week) included in the monthly earnings regressions. The base group always consists of individuals with compulsory schooling, apprenticeship training or intermediate vocational school. Robust standard errors in parentheses. ***, ** and * indicate significance at the 1-percent, 5-percent and 10-percent level.

[a] The natural logarithm of hourly net earnings: based on the monthly net income divided by 4.3 times weekly working hours, adjusted to changes in the consumer price index (base 1986).

[b] The natural logarithm of monthly net earnings, adjusted to changes in the consumer price index (base 1986).

[c] High track schools consist of higher vocational schools, higher general schools and university education.

[d] The distinction between rural and urban areas is based on the size of the municipality, with urban areas comprising more than 10.000 inhabitants (37.75 percent live in urban ares).

7 Conclusions

In this paper, we study the secondary school track choice of Austrian students. By international standards, educational tracking occurs very early in Austria. Students have to choose between an academic track and a vocational track at the age of 10. We argue that in education systems where first tracking occurs early, track choice is strongly influenced by factors other than innate ability and provide evidence that the relative age at track choice is one such factor.

Combining administrative student-level data from the city of Linz with data from two PISA waves, we are able to study relative age effects on track choice in grades 5–9, thus providing a comprehensive picture of the relative age effect on early and later track choices. Furthermore, we contribute to the existing literature not only by providing evidence for an additional country with early tracking but also by investigating heterogeneous age effects for different groups of students.

Our study shows that younger students are significantly less likely to attend a high track school than their older peers in grades 5–8. Beyond grade 8, students again have to choose between different educational tracks. The second tracking may mitigate the relative age effect if students who were assigned to the wrong track because of their age-related disadvantage manage to upgrade to the high track in grade 9. On average, we do not find a significant effect of relative age in grade 9. However, we find evidence for important heterogeneities in the relative age effect with respect to parental background and the location of the school.

While the relative age effect disappears for children from more favorable parental backgrounds, the second tracking reinforces the effects for children from less favorable parental backgrounds. In this group, younger students are about 21 percentage points less likely to choose a high track school than their older peers in grade 9. The magnitudes of the estimated effects are substantial given that 56 percent of all students and 40 percent of students from lower socioeconomic backgrounds attend a high track school in grade 9. We conclude that relative age effects reinforce existing socioeconomic inequalities in a system of early tracking.

Furthermore, we find no relative age effect on the probability to attend a high track school for students who attend schools in rural areas, neither in grade 8 nor in grade 9. In urban areas on the other hand, younger students are less likely to attend the high track than older students in grade 8 and grade 9. Again, the magnitude of the relative age effect is substantial. Students who are 11 months younger at the time of track choice are about 24 percentage points less likely to choose a high track school in grade 8 and about 18 percentage points less likely to choose a high track school in grade 9.

We suppose that the absence of a relative age effect in rural areas is related to the low supply of high track schools for grades 5–8 in rural areas. Since 80 percent of students in rural areas attend the local low track school until grade 8, these schools are almost like comprehensive schools; in fact, students are first tracked in grade 9. This postponement may be responsible for the absence of relative age effects in grade 9 in rural areas. Although we cannot directly show whether the allocation of students to tracks would be more efficient if the first tracking occurred later, the difference in the relative age effect between urban and rural areas gives some indirect evidence that this would be the case.

In contrast to related studies (e.g. Jürges and Schneider, 2011; Mühlenweg and Puhani, 2010), we provide a sensitivity check with respect to the monotonicity assumption. Recent research (Barua and Lang, 2009) suggests that this assumption is violated because the instrument does not affect all students in the same way. We redefine our treatment and assignment variable such that the monotonicity assumption is fulfilled and show that our conclusions do not change.

Appendix

Figure A.1: Distribution of parental socioeconomic status across birth months

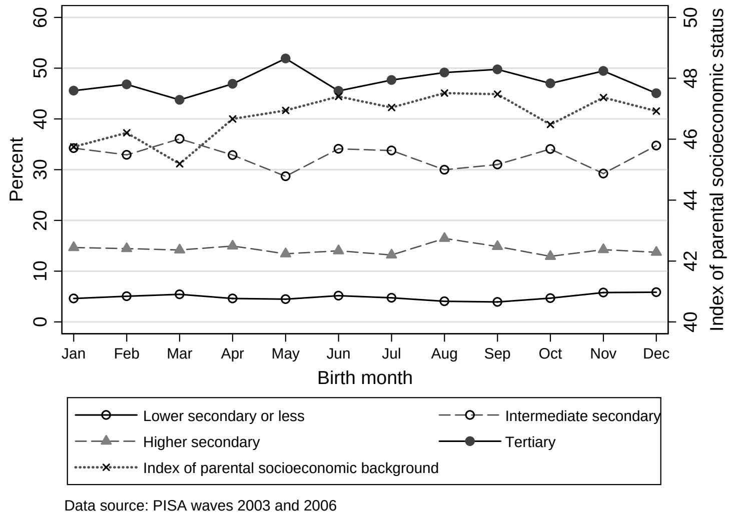


Table A.1: Summary statistics

	Mean	St.Dev.
Panel A: Administrative student-level data (Linz)[a]		
High track (grade 5)	0.45	
High track (grade 8)	0.41	
Enrollment:		
Regular	0.80	
Late	0.18	
Early	0.02	
Repeated class (grade 2-4)	0.04	
Repeated class (grade 5-8)	0.06	
Observed age at track choice (grade 5)	10.67	0.46
Assigned age at track choice (grade 5)	10.46	0.29
Female	0.49	
Immigrant background	0.05	
Observations	25,248	
Panel B: PISA waves 2003 and 2006 (Austria)[b]		
High track (grade 8)	0.29	
High track (grade 9)	0.56	
Enrollment:		
Regular	0.79	
Late	0.17	
Early	0.04	
Repeated class (grade 2-4)	0.02	
Repeated class (grade 5-8)	0.03	
Repeated class (grade 9)	0.04	
Observed age at track choice (grade 5)	10.62	0.64
Observed age at track choice (grade 9)	14.65	0.63
Female	0.49	
Immigrant background	0.10	
Urban area	0.55	
Index of parental socioeconomic status[c]	48.13	0.32
Parents' highest education:[d]		
Isced 0 or 1 (Primary)	0.01	
Isced 2 (Lower secondary)	0.04	
Isced 3b or 3c (Intermediate secondary)	0.33	
Isced 3a or 4 (Higher secondary)	0.14	
Isced 5a or 5b or 6 (Tertiary)	0.47	
Observations	8,136	

NOTES: [a]The sample consists of students observed in grade 5 between 1984-2001 and excludes students in special education. [b]Data from PISA waves 2003 and 2006. The sample excludes students in special education (0.83 percent) and students with missing values in our main variables: attendance of high track in grade 8/9 and observed age in grade 5/9 (2.87 percent). [c]The higher ISEI score (International Socio-Economic Index of Occupational Status, Ganzeboom et al. (1992)) of either parent or the only available parent's ISEI score and varies between 16 and 90. The index is missing for 3 percent of the sample. [d]Highest parental education derived from ISCED categories (UNESCO, 2006).

Table A.2: Regressions of background characteristics on instrument (*assigned age*)

	Coeff.	SE	Mean
Panel A: Administrative student-level data (Linz)			
Immigrant background	-0.004	(0.004)	0.05
Female	-0.017*	(0.010)	0.49
Residential area in grade 1:[a]			
1	-0.001	(0.002)	0.01
2	-0.008	(0.005)	0.08
3	-0.002	(0.005)	0.06
4	0.001	(0.003)	0.03
5	-0.002	(0.003)	0.02
6	-0.004	(0.004)	0.04
7	-0.002	(0.003)	0.02
8	0.007	(0.005)	0.06
9	0.000	(0.003)	0.02
10	0.000	(0.004)	0.04
11	0.001	(0.005)	0.05
12	-0.003	(0.004)	0.04
13	0.002	(0.005)	0.07
14	0.009*	(0.005)	0.06
15	0.004	(0.004)	0.05
16	0.006	(0.004)	0.04
17	0.001	(0.005)	0.06
18	-0.007*	(0.004)	0.04
19	-0.004	(0.003)	0.03
20	-0.004	(0.004)	0.05
21	-0.001	(0.002)	0.01
22	0.002	(0.003)	0.02
23	-0.000	(0.003)	0.03
24	0.003	(0.006)	0.08
Panel B: PISA waves 2003 and 2006 (Austria)			
Immigrant background	-0.000	(0.011)	0.10
Female	0.009	(0.021)	0.49
Index of parental socio-economic status	0.284	(0.640)	48.13
ISEI below sample mean	-0.002	(0.019)	0.50
Urban area	-0.021	(0.025)	0.55
Highest parental education:			
Primary or less	0.006	(0.004)	0.01
Lower secondary	-0.002	(0.007)	0.04
Intermediate secondary	0.005	(0.017)	0.33
Higher secondary	-0.007	(0.013)	0.14
Tertiary	-0.003	(0.018)	0.47
Lower secondary or less	0.003	(0.009)	0.05
Higher secondary or more	-0.010	(0.018)	0.62

NOTES: Ordinary least squares regression of observed characteristics on assigned age. See table A.1 and Section 4 for a description of the samples and variables. ***, ** and * indicate significance at the 1-percent, 5-percent and 10-percent level. [a]Results are similar when we aggregate the 24 statistical districts to 7 quarters.

References

Allen, J., Barnsley, R., 1993. Streams and tiers: The interaction of ability, maturity, and training in systems with age-dependent recursive selection. Journal of Human Resources 28 (3), 649–659.

Ammermüller, A., Pischke, J.-S., 2009. Peer effects in European primary schools: Evidence from PIRLS. Journal of Labour Economics 27 (3), 315–348.

Angrist, J. D., Imbens, G. W., Rubin, D. B., 1996. Identification of causal effects using instrumental variabels. Journal of the American Statistical Association 91, 444–472.

Barua, R., Lang, K., 2009. School entry, educational attainment and quarter of birth: A cautionary tale of LATE. National Bureau of Economic Research (Cambridge, MA) Working Paper No. 15236.

Bauer, P., Riphahn, R. T., 2006. Timing of school tracking as a determinant of intergenerational transmission of education. Economics Letters 91, 90–97.

Bedard, K., Duhey, E., 2006. The persistence of early childhood maturity: International evidence of long-run age effects. The Quarterly Journal of Economics 121 (4), 1437–1472.

Black, S. E., Devereux, P. J., Salvanes, K. G., 2011. Too young to leave the nest? The effects of school starting age. The Review of Economics and Statistics 92 (2), 455–467.

Brunello, G., Checchi, D., 2007. Does school tracking affect equality of opportunity? New international evidence. Economic Policy 22 (52), 781–861.

Brunello, G., Giannini, M., Ariga, K., 2007. The optimal timing of school tracking. In: Wößmann, L., Petersen, P. (Eds.), Schools and the Equal Opportunity Problem. MIT Press, pp. 129–156.

Buckles, K., Hungerman, D. M., 2008. Season of birth and later outcomes: Old questions, new answers. National Bureau of Economic Research (Cambridge, MA) Working Paper No. 14573.

Carrell, S. E., Hoekstra, M. L., 2010. Externalities in the classroom: How children exposed to domestic violence affect everyone's kids. American Economic Journal: Applied Economics 2 (1), 211–228.

Cunha, F., Heckman, J. J., Lochner, L., Masterov, D. V., 2006. Interpreting the evidence on life cycle skill formation. In: Hanushek, E. A., Welch, F. (Eds.), Handbook of the Economics of Education. Vol. 1. Amsterdam: Elsevier, pp. 697–812.

Datar, A., 2006. Does delaying kindergarten entrance give children a head start? Economics of Education Review 25 (1), 43–62.

Duflo, E., Dupas, P., Kremer, M., 2011. Peer effects and the impact of tracking: Evidence from a randomized evaluation in Kenya. American Economic Review 101 (5), 1739–1774.

Dustmann, C., 2004. Parental background, secondary school track choice, and wages. Oxford Economic Papers 56 (1), 209–230.

Elder, T. E., Lubotsky, D. H., 2009. Kindergarten entrance age and children's achievement: Impacts of state policies, family background, and peers. Journal of Human Resources 44 (3), 641–683.

Fersterer, J., Winter-Ebmer, R., 2003. Are Austrian returns to education falling over time? Labour Economics 10, 73–89.

Fertig, M., Kluve, J., 2005. The effect of age at school entry on educational attainment in Germany. Institute for the Study of Labor (IZA, Bonn) Discussion Paper No. 1507.

Fredriksson, P., Öckert, B., 2006. Is early learning really more productive? The effect of school starting age on school and labor market performance. Institute for Labour Market Policy Evaluation (IFAU, Uppsala) Working Paper 2006:12.

Galindo-Rueda, F., Vignoles, A., 2004. The heterogeneous effect of selection in secondary schools: Understanding the changing role of ability. Institute for the Study of Labor (IZA, Bonn) Discussion Paper No. 1245.

Ganzeboom, H. B. G., de Graaf, P. M., Treiman, D. J., 1992. A standard international socioeconomic index of occupational status. Social Science Research 21, 1–56.

Gould, E. D., Lavy, V., Paserman, M. D., 2009. Does immigration affect the long-term educational outcomes of natives? Quasi-experimental evidence. The Economic Journal 119 (540), 1243–1269.

Hanushek, E. A., Kain, J. F., Markman, J. M., Rivkin, S. G., 2003. Does peer ability affect student achievement? Journal of Applied Econometrics 18, 527–544.

Hanushek, E. A., Wößmann, L., 2006. Does educational tracking affect performance and inequality? Differences-in-differences evidence across countries. The Economic Journal 160 (510), C63–C76.

Hopkins, K. D., 1990. Educational and Psychological Measurement and Evaluation. Englewood Cliffs, NJ: Prentice-Hall.

Hoxby, C. M., Weingarth, G., 2006. Taking race out of the equation: School reassignment and the structure of peers effects. Unpublished manuscript, Harvard University.

Jürges, H., Schneider, K., 2011. Why young boys stumble: Early tracking, age and gender bias in the German school system. German Economic Review 12 (4), 371–394.

Lavy, V., Paserman, M. D., Schlosser, A., 2011. Inside the black of box of ability peer effects: Evidence from variation in high and low achievers in the classroom. The Economic Journal Doi: 10.1111/j.1468-0297.2011.02463.x.

McEwan, P. J., Shapiro, J. S., 2008. The benefits of delayed primary school enrollment. Discontinuity estimates using exact birth dates. Journal of Human Resources 43 (1), 1–29.

Meghir, C., Palme, M., 1995. Educational reform, ability, and family background. American Economic Review 95 (1), 414–424.

Mühlenweg, A. M., Puhani, P. A., 2010. The evolution of the school-entry age effect in a school tracking system. Journal of Human Resources 45 (2), 407–438.

Pekkarinen, T., Uusitalo, R., Kerr, S., 2009a. School tracking and development of cognitive skills. Institute for the Study of Labor (IZA, Bonn) Discussion Paper No. 4058.

Pekkarinen, T., Uusitalo, R., Pekkala, S., 2009b. School tracking and intergenerational income mobility: Evidence from the Finnish comprehensive school reform. Journal of Public Economics 93 (7-8), 965–973.

Petersen, A. C., 1983. Pubertal change and cognition. In: Brooks-Gunn, J., Petersen, A. (Eds.), Girls at puberty: Biological and psychosocial perspectives. New York: Plenum Press, pp. 179–198.

Pischke, J.-S., Manning, A., 2006. Comprehensive versus selective schooling in England and Wales: What do we know? National Bureau of Economic Research (Cambridge, MA) Working Paper No. 12176.

Schindler-Rangvid, B., 2003. Educational peer effects. Quantile regression evidence from Denmark with PISA 2000 data. Chapter 3. Do schools matter? Ph.D. thesis, Aarhus School of Business, Denmark.

Schneeweis, N., Winter-Ebmer, R., 2007. Peer effects in Austrian schools. Empirical Economics 32 (2), 387–409.

Statistics Austria, various years. Education documentation (Schulstatistik). Statistics Austria, Vienna.

Steiner, M., Lassnigg, L., 2000. Schnittstellenproblematik in der Sekundarstufe. Erziehung und Unterricht 9/10, 1063–1070.

UNESCO, 2006. The International Standard Classification of Education (ISCED) 1997. United Nations Educational, Scientific and Cultural Organization. Re-edition, May 2006.